

Ability Tracking, School and Parental Effort, and Student Achievement: A Structural Model and Estimation*

Chao Fu [†] Nirav Mehta [‡]

December 11, 2016

Abstract

We develop and estimate an equilibrium model of ability tracking in which schools decide how to allocate students into ability tracks and choose track-specific teacher effort; parents choose effort in response. The model is estimated using data from the ECLS-K. Our model suggests that a counterfactual ban on tracking would benefit low-ability students but hurt high-ability students. Ignoring effort adjustments would significantly overstate the impacts. We then illustrate the tradeoffs involved when considering policies that affect schools' tracking decisions. Setting proficiency standards to maximize average achievement would lead schools to redistribute their inputs from low-ability students to high-ability students.

*We thank Yuseob Lee, George Orlov, and Atsuko Tanaka for excellent research assistance. We thank John Bound, Betsy Caucutt, Tim Conley, Steven Durlauf, John Kennan, Lance Lochner, Michael Lovenheim, Jesse Rothstein, Jeffrey Smith, Steven Stern, Todd Stinebrickner, Chris Taber, and Kenneth Wolpin for insightful discussions. We have benefited from the comments from participants at the AEA 2014 winter meeting, Labo(u)r Day, and the CESifo Area Conference on the Economics of Education, and seminars at Amherst, ASU, Berkeley, Brock, Queen's, Rochester, St. Louis, and UWO.

[†]Department of Economics, University of Wisconsin. Email: cfu@ssc.wisc.edu.

[‡]Department of Economics, University of Western Ontario. Email: nirav.mehta@uwo.ca

1 Introduction

Ability tracking, the practice of allocating students into different classrooms based on prior performance, is pervasive.¹ It is also controversial, because it may benefit students of certain ability levels while hurting others. There is considerable policy interest in learning how ability tracking affects different types of students, and how policy changes, such as changing proficiency standards, would affect schools' tracking choices and the distribution of student achievement. However, these questions are also very hard to answer. Indeed, the determinants and effects of tracking remain largely open questions, as evidenced by the mixed findings in the literature.²

Carefully-designed experiments can potentially answer these questions. However, to learn the effects of tracking, experiments would have to be conducted on a wide selection of schools, because, as Gamoran and Hallinan (1995) point out, the effectiveness of tracking depends on how it is implemented and on the school climate in which it is implemented. Similarly, to the extent that different policies may have very different impacts, one would need to run experiments under an extensive range of potential policies. The cost of doing so can become formidable very fast.

As a feasible alternative, we adopt a structural approach. Specifically, we develop and estimate a model that treats a school's tracking regime and track-specific effort, parental effort, and student achievement as joint equilibrium outcomes. The model is designed to address three interrelated components that have yet to be considered in a cohesive framework. First, changing peer composition in one classroom re-allocates students, hence changes peers in some other classroom(s). It is therefore important not to treat classrooms in isolation when studying the treatment effect of changing peers, especially for policy making purposes, which require a balance between trade-offs. Second, by explicitly modeling school and parental effort inputs, we can then infer what these input levels and student achievement would be if tracking regimes, hence peer compositions, were changed. This allows us to decompose the effects of tracking into the direct effect of changes in peers and the indirect effects caused by the behavioral responses of schools and parents. As argued by Becker and Tomes (1976), ignoring behavioral responses may bias estimated impacts of policy changes.

¹Loveless (2013) reports that 75% of US 8th-graders in the National Assessment of Educational Progress data were in tracked classes.

²We review these studies below.

Finally, our explicit modeling of tracking regime choices allows us to predict how tracking regimes, which determine classroom-level peer composition, and subsequent school and parent inputs would change in response to a policy change.

In the model, each school is attended by children from different types of households, the distribution of which may differ between schools. A household type is defined by the child's ability and by how costly it is for the parent to help her child learn. A child's achievement depends on her own ability, effort inputs invested by the school and by her parent, and the quality of her peers.³ A parent optimizes her child's achievement by choosing costly parental effort given both her child's track assignment, which determines peer quality, and the track-specific teacher effort.⁴ Taking into account responses by parents, the school chooses both a tracking regime, which assigns students to classrooms based on their ability, and track-specific teacher effort inputs to maximize its own objective, which increases in the average achievement of its students and the fraction of students satisfying a proficiency standard.

This framework naturally allows policies to produce winners and losers because changes in tracking regimes at a school may differentially affect students of different ability levels and parental backgrounds. Moreover, given that the model allows schools to base tracking decisions on the composition of households it serves, policies that affect schools' tracking decisions will necessarily create heterogeneous effects at the school level. That is, there will be a distribution of treatment effects within each school, and this distribution may itself differ across schools.

We estimate the model using data from the nationwide Early Childhood Longitudinal Study (ECLS-K), which are rich enough to allow us to model the interactions between schools and parents. Students are linked to their parents and teachers. For students, we observe prior test scores, class membership, and end-of-the-year test scores. Parents report the frequency with which they help their children with homework (parental effort). Teachers report the class-specific workload (school effort), and teachers also report the overall level of ability among students in each of their classes relative to other students in the same school. This rare last piece of informa-

³See Epple and Romano (2011) for a recent review of the literature on peer effects in education.

⁴The importance of these types of input adjustments has been supported by evidence found in the literature. For example, Pop-Eleches and Urquiola (2013) and Das et al. (2013) both document changes in home inputs in response to quasi-experimental variation in school inputs.

tion, which is available for multiple classes within a school, allows us to essentially “observe”, i.e., compute, two of the key components of the model. The first is the tracking regime used by each school. The second is the composition of peer ability in each track. Each track comprises a section of the school-specific ability distribution; the section is specified by the tracking regime. Such information directly reveals peer quality, despite the fact that, as discussed in Betts (2011), a student’s ability is not directly observable and that the grouping of students into different tracks is endogenous. Therefore, we are able to separate the roles of own ability and peer quality in the technology, a common identification concern in this literature.

We use the estimated model to conduct two policy evaluations. First, we quantify the effects of allowing ability tracking on the distribution of student achievement by comparing outcomes from the baseline model, where schools choose tracking regimes, with counterfactual outcomes where no schools are allowed to track students, i.e. class composition is equal across classrooms in a school. Over 95% of schools in our sample practice ability tracking under the baseline and, therefore, are affected by this policy. A tracking ban increases peer quality for low-ability students and decreases peer quality for high-ability students. Our results suggest that, in response, schools increase teacher effort on average, but parents react differently depending on their child’s ability. The increase in the peer quality for low-ability students is effectively an increase in these households’ endowments, causing their parents to reduce their provision of costly effort. Conversely, parents of high-ability students increase their effort because their children now have lower quality peers. Altogether, our results suggest that banning tracking increases the achievement of students with below-median prior scores by 2.2% of a standard deviation (sd) in outcome test score and reduces the achievement of students with above-median prior scores by 4.2% sd. We find the average treatment effect from banning tracking is small, that is, the effects of tracking are mostly distributional in nature.⁵

It is worth noting, however, that peer quality is estimated to be a significant determinant of achievement: holding all other inputs constant and increasing peer quality by one standard deviation (sd) causes a 20% sd increase in the outcome test score, on average. The composite effect of increasing peer quality, which allows for

⁵Bond and Lang (2013) note that plausible monotonic transformations of test scores can mitigate or even reverse estimated differences in gains for subgroups. Thus, we emphasize the distributional effects of ability tracking rather than cross-group comparisons.

behavioral responses by schools and parents, is less than half the size. For example, if the effort adjustments by schools and parents in response to the ban on tracking were ignored, one would overstate the loss for students with above-median prior scores by 121%, and overstate the gain for students with below-median prior scores by 147%. That is, the effect of banning tracking would appear larger were we to hold school and parental effort constant at their levels under the status quo. The equilibrium nature of our framework helps to cast light on the open question of why estimated peer effects are typically small, as reviewed by Sacerdote (2011). It is exactly the fact that peer quality is an important input that induces substantial behavioral responses by schools and parents, which in turn mitigate the direct effect of peer quality.

Because policies that affect schools' tracking decisions will necessarily lead to increases of peer quality for some students and decreases for some other students, our second counterfactual experiment highlights the trade-offs involved in achieving academic policy goals. In the model, schools value both average achievement and the fraction of students testing above a proficiency standard, which policymakers may have control over. In this counterfactual, we search for region-specific proficiency standards that would maximize average student test scores in each Census region. Achievement-maximizing standards would be higher than their baseline levels in every region, but do not increase without bound because eventually the density of student ability decreases, reducing the gain in average achievement to increasing standards. Under these higher proficiency standards, schools would adjust their effort inputs and tracking regimes such that resources are moved away from low-ability students toward high-ability students, leading to decreases in achievement for low-ability students and increases in achievement for high-ability students.

The rest of the paper is organized as follows. The next section gives a brief review of the related literature. Section 3 describes the model. Section 4 describes the data. Section 5 explains our estimation and identification strategy. Section 6 presents the estimation results. Section 7 conducts counterfactual experiments. The last section concludes. The appendix contains additional details about the data and model.

2 Related Literature

This paper contributes to and brings together two strands of studies on ability tracking: one that measures how tracking affects student achievement and another that studies the determinants of tracking decisions.⁶ We take a first step in the literature towards creating a comprehensive framework to understand tracking, by building and estimating a model where tracking regimes and track-specific inputs, parental effort, and student achievement are viewed as joint equilibrium outcomes.

There is considerable heterogeneity in results from empirical work assessing the effect of ability tracking on both the level and distribution of achievement. Argys et al. (1996) find that tracking reduces the performance of low ability students, while Betts and Shkolnik (2000b) and Figlio and Page (2002) find no significant differences in outcomes for US high school students of the same ability level at tracked and untracked schools. Duflo et al. (2011) run an experiment in Kenya and find that students of all abilities gain from a tracking regime where students were assigned to high- and low-ability classrooms, relative to a control group where students were randomly assigned to the two classrooms. Gamoran (1992) finds that the effects of ability tracking on high school students vary across schools. The heterogeneous findings from these studies indicate that there is no one “causal effect” of ability tracking that generalizes to all contexts, and therefore highlight the importance of explicitly taking into account the fact that households differ within a school and that the distributions of households differ across schools, both of which are fundamental to our model and empirical analyses.⁷

The primary focus of the tracking literature is on how changes in peer characteristics affect one’s academic outcomes. In this sense, the tracking literature reviewed above is very similar to a separate but closely related literature studying academic peer effects, which has been extensively reviewed in Sacerdote (2011). The majority of these papers estimate a reduced-form relationship between peer quality and own achievement. As in the tracking literature, most of these studies use either of two methods: 1) exploit random variation in peer group composition, which allows the

⁶See Slavin (1987), Slavin (1990), Gamoran and Berends (1987), Hallinan (1990), Hallinan (1994) and Betts (2011) for reviews of studies in the first strand. Examples in the second strand include Gamoran (1992) and Hallinan (1992).

⁷There is also a literature studying between-school tracking, e.g. Hanushek and Woessmann (2006).

researcher to study outcomes for the affected subset of students or 2) include fixed effects, the idea being that residual variation will be exogenous to own peer quality. Sacerdote (2011) notes that many studies find modest effects of peer quality using reduced-form linear-in-means models relating own outcomes to peer quality, but there is also a large degree of heterogeneity in terms of findings.⁸ Sacerdote (2011) suggests that nonlinearities in the relationship between own and peer characteristics may help explain such heterogeneity, noting that several studies have found evidence of nonlinear peer effects in the reduced-form, wherein higher ability students gain more from increases in peer quality than do lower ability students (see, e.g., Hoxby and Weingarth (2005), Imberman et al. (2012), Burke and Sass (2013), and Lavy and Schlosser (2011).)

Our findings reinforce the importance of allowing for nonlinearity in such reduced-form regressions. Consistent with the previous literature, we find that the structural test score production function is such that higher-ability students gain more from peer quality than do lower-ability students. However, we also show that restricting the production function itself to be linear does not significantly affect this paper's policy implications. This is because even if the production function were linear, a nonlinear reduced-form relationship between peer quality and own outcomes would arise naturally from the nonlinear nature of school and parental responses to changes in peer quality.

A closely related paper is Fruehwirth (2013), which identifies peer achievement spillovers using the introduction of a student accountability policy as an instrument.⁹ She uses this instrumental variable to eliminate the problem of nonrandom assignment in identifying peer achievement spillovers, which is a valid approach as long as students are not reassigned in response to the policy.¹⁰ Our paper complements

⁸Where possible, we have converted coefficient estimates from the literature to be in terms of changes in standard deviations of outcome scores, in terms of changes in standard deviation in peer quality (measured as the mean of their prior achievement). We were able to do so for Burke and Sass (2013), Hanushek et al. (2003), Kiss (2013), Lefgren (2004), and Vigdor and Nechyba (2007); the findings range from 0 to 0.10 sd increase in achievement resulting from a 1 sd increase in peer quality.

⁹See Manski (1993), Moffitt (2001), and Brock and Durlauf (2001) for methodological contributions concerning the identification of peer effects and Betts and Shkolnik (2000a) for a review on empirical work in this respect.

¹⁰The author provides support for this assumption by showing that observable compositions of peer groups do not appear to change in response to student accountability. Assuming random assignment to classrooms within schools, Fruehwirth (2014) finds positive effects of peer parental

her work. Unlike Fruehwirth (2013), we assume that a student’s achievement does not depend directly on the effort choices by other students (households), hence abstracting from direct social interactions within a class.¹¹ Instead, we model schools’ decisions about student assignment and track-specific inputs in order to study the nonrandom assignment of students across classrooms, and how parents respond.

Consistent with our finding, researchers have shown that parental investment in children responds to changes in other inputs. For example, Das et al. (2013) find evidence from India and Zambia that student test scores improve when schools receive unanticipated grants but not when grants are anticipated, i.e., households offset their own spending in response to anticipated grants. Using a regression discontinuity design to study the Romanian secondary school system, Pop-Eleches and Urquiola (2013) find that parents reduce their effort when their children attend a better school. Using data from the NELS, Houtenville and Conway (2008) find evidence suggesting decreases in parental effort in response to increased school resources. Liu et al. (2010) analyze the roles of and interrelationships among school inputs and household migration and maternal employment decisions. Their findings from the NLSY suggest that when parental responses are taken into account, changes in school quality may only have minor impacts on child test scores. Sacerdote (2011) writes that, given evidence supporting existence of peer effects, an important next step is to quantify the relative importance of peers, school, and home inputs. Our paper takes a first step towards filling this gap.

While our work focuses on how peer groups are determined within a school, a different literature studies how households sort themselves into different schools. Epple et al. (2002) study how ability tracking by public schools may affect student sorting between private and public schools. They find that when public schools track by ability, they may attract higher ability students who otherwise would have attended private schools. This is consistent with our finding that high-ability students benefit from tracking. Caucutt (2002), Epple and Romano (1998), Ferreyra (2007), Mehta (Forthcoming), and Nechyba (2000) develop equilibrium models to study sorting between schools and its effects on peer composition.¹² Our work com-

education on student achievement.

¹¹See Blume et al. (2011) for a comprehensive review of the social interactions literature.

¹²Mehta (Forthcoming) also endogenizes school input choices. Walters (2014) studies the demand for charter schools.

plements this literature by taking a first step toward studying schools' tracking decisions, which determine class-level peer groups faced by households within a school, and by emphasizing the interactions between a school and attendant households in the determination of student outcomes.

3 Model

In this section, we first introduce our theoretical framework, followed by more detailed model specifications, and defer discussions of our modeling choices until the end of the section. Each school is treated in isolation. A school makes decisions about ability tracking and track-specific inputs, knowing that parents will respond by adjusting parental effort.

3.1 The Environment

A school s is endowed with a continuum of households of measure one. Households are of different types in that students have different ability levels (a) and parents have different parental effort costs ($z \in \{low, high\}$). Student ability a is known to the household and the school, but z is a household's private information, which implies that all students with the same ability level are treated identically by a school. Let $g_s(a, z)$, $g_s(a)$ and $g_s(z|a)$ denote, respectively, the school- s specific joint distribution of household types, marginal distribution of ability, and conditional distribution of z given a . In the following, we suppress the school subscript s when it is not confusing to do so.

3.1.1 Timing

The timing of the model is as follows:

Stage 1: The school chooses a tracking regime and track-specific effort inputs.

Stage 2: Observing the school's choices, parents choose their own parental effort.

Stage 3: Student test scores are realized.

3.1.2 Production Function

The achievement of a student i in track j depends on the student's ability (a_i), the average ability of students in the same track (q_j), the coefficient of variation of ability of students in the same track (q_j^{cv}), track-specific school effort (e_j^s), parental effort (e_i^p), and a school-specific shifter (α_{0s}).¹³ The test score y_{ji} measures student achievement with noise $\epsilon_{ji} \sim F_\epsilon(\cdot)$, which has density f_ϵ , such that

$$y_{ji} = Y(a_i, q_j, q_j^{cv}, e_j^s, e_i^p, \alpha_{0s}) + \epsilon_{ji}. \quad (1)$$

The school-specific shifter α_{0s} captures the idea that the production processes may differ across schools in ways that are not observed by the researcher. The vector $\{\alpha_{0s}\}_{s=1}^S$ is treated as a set of free parameters to be estimated, which is a very flexible way to introduce unobserved heterogeneity without imposing any distributional assumptions.¹⁴ Such heterogeneity allows the model to capture *any* type of matching between households and schools, as the correlation between α_{0s} and the characteristics of households attending school s are entirely determined by the data.

3.2 Parent's Problem

A parent derives utility from her child's achievement and disutility from exerting parental effort. Given the track-specific school input (e_j^s), the peer quality (q_j), and the coefficient of variation of peer ability (q_j^{cv}) of her child's track, a parent i chooses her own effort to maximize the utility from her child's achievement, net of her effort cost $C^p(e_i^p, z_i)$:

$$u(e_j^s, q_j, q_j^{cv}, a_i, z_i, \alpha_{0s}) = \max_{e_i^p \geq 0} \left\{ \ln \left(Y(a_i, q_j, q_j^{cv}, e_j^s, e_i^p, \alpha_{0s}) \right) - C^p(e_i^p, z_i) \right\},$$

where $u(\cdot)$ denotes the parent's value function. Denote the optimal parental choice $e^{p*}(e_j^s, q_j, q_j^{cv}, a_i, z_i, \alpha_{0s})$. Notice that a parent's optimal choice depends not only on

¹³Arguably, the entire distribution of peer ability may matter. Following the literature, for feasibility reasons we have allowed only moments of the ability distribution, in our case the average and the variance of peer quality, to enter the production function.

¹⁴Though in principle identified, a more flexible technology, in which all production function parameters are allowed to differ between schools, would be beyond the limits of the data.

her child’s ability a_i , her cost type z_i , and school intercept α_{0s} , but also on the other inputs in the test score production, including the ones chosen by the school (q_j , q_j^{cv} , and e_j^s).

3.3 School’s Problem

A school cares about the average test score of its students and *may* also care about the fraction of students who can pass a proficiency standard y^* .¹⁵ It chooses a tracking regime, which specifies how students are allocated across classrooms based on student ability, and track-specific inputs. Formally, a tracking regime is defined as follows.¹⁶

Definition 1 Let $\mu_j(a) \in [0, 1]$ denote the fraction of ability- a students assigned to track j , such that $\sum_j \mu_j(a) = 1$. A tracking regime is defined as $\mu = \{\mu_j(\cdot)\}_j$.¹⁷

If no student of ability a is allocated to track j , then $\mu_j(a) = 0$. If track j does not exist, then $\mu_j(\cdot) = 0$. Because all students with the same ability level are treated identically, $\mu_j(a)$ is also the probability that a student of ability a is allocated to track j . The school’s problem can be viewed in two steps: 1) choose a tracking regime; 2) choose track-specific inputs given the chosen regime. The problem can be solved backwards.

3.3.1 Optimal Track-Specific School Effort

Let $e^s \equiv \{e_j^s\}_j$ denote a school effort vector across the j tracks at a school. Given a tracking regime μ , the optimal choice of track-specific effort solves the following

¹⁵Recall that the test score is an unbiased measure of student achievement and that there is a continuum of students in the school, which means the noise integrates out at the school level. Therefore, a school’s objective can be viewed as it caring about either average *test score* and the fraction of students who *test* above the standard, or average *achievement* and the fraction of students whose *achievement* is above the standard.

¹⁶Another type of tracking, which we do not model, may happen within a class. We choose to focus on between-class tracking because it is prevalent in the data (95% of schools track students across classes). In contrast, when asked how often they split students within a class by ability levels, the majority of teachers report either never doing so or doing so less than once per week.

¹⁷Note that the number of tracks can be computed from μ : $\sum_j \mathbf{1}\{\sum_a \mu_j(a) > 0\}$.

problem:

$$V_s(\mu) = \max_{e^s \geq 0} \left\{ \int_i \left\{ \sum_j [E_{(z_i, \epsilon_{ji})} ((y_{ji} + \omega I(y_{ji} > y^*)) | a_i) - C^s(e_j^s)] \mu_j(a_i) \right\} di \right. \\ \left. \begin{array}{l} \text{s.t. } y_{ji} = Y(a_i, q_j, q_j^{cv}, e_j^s, e_i^p, \alpha_{0s}) + \epsilon_{ji} \\ e_i^p = e^{p*}(e_j^s, q_j, q_j^{cv}, a_i, z_i, \alpha_{0s}) \\ n_j = \sum_a \mu_j(a) g_s(a) \\ q_j = \frac{1}{n_j} \sum_a \mu_j(a) g_s(a) a. \end{array} \right\}, \quad (2)$$

where $V_s(\cdot)$ denotes the school's value function given regime μ . The parameter $\omega \geq 0$ measures the additional valuation a school may derive from a student passing the proficiency standard (y^*). $C^s(e_j^s)$ is the per-student effort cost in track j . The terms in the square brackets comprise student i 's expected net contribution conditional on her being in track j (denominated in units of test scores), where the expectation is taken over both the test score shock ϵ_{ji} and the distribution of parent type z_i given student ability a_i , which is $g_s(z|a)$. In particular, a student contributes by her test score y_{ji} and an additional ω if y_{ji} is above y^* . Student i 's total contribution to the school's objective is a weighted sum of her track-specific contributions, where the weights are given by her probabilities of being assigned to each track, $\{\mu_j(a_i)\}_j$. The overall objective of a school is thus the integral of individual students' contributions. There are four constraints a school faces, listed in (2). The first two are the test score technology and the optimal response of the parent. The last two identity constraints show how the tracking regime μ defines the size (n_j) and student quality (q_j) of a track. Let $e^{s*}(\mu)$ be the optimal solution to (2).

3.3.2 Optimal Tracking Regime

A school's cost may vary with tracking regimes, captured by the function $D(\mu)$. Balancing benefits and costs of tracking, a school solves the following problem:¹⁸

$$\max_{\mu \in M_s} \{V_s(\mu) - D(\mu) + \eta_\mu\},$$

¹⁸We assume the tracking decision is made by the school. In reality, it is possible that some parents may request that their child be placed in a certain track. We abstract from this in the model and instead focus on parental effort responses.

where η_μ is the school-specific idiosyncratic shifter associated with regime μ , which is i.i.d. across schools. M_s is the support of tracking regime for school s , which is specified in Section 3.5.2.

3.4 Equilibrium

Definition 2 *A subgame perfect Nash equilibrium in school s consists of $\{e^{p*}(\cdot), e^{s*}(\cdot), \mu^*\}$, such that*

- 1) *For each $(e_j^s, q_j, q_j^{cv}, a_i, z_i, \alpha_{0s})$, $e^{p*}(\cdot)$ solves the parent's problem;*
- 2) *$(e^{s*}(\mu^*), \mu^*)$ solves the school's problem.¹⁹*

We solve the model using backward induction. First, solve the parent's problem for any given $(e_j^s, q_j, q_j^{cv}, a_i, z_i, \alpha_{0s})$. Second, for a given μ , solve for the track-specific school inputs e^s . Finally, optimize over tracking regimes to obtain the optimal μ^* and the associated effort inputs $(e^{p*}(\cdot), e^{s*}(\cdot))$.²⁰

3.5 Further Empirical Specifications

We present the final empirical specifications we have chosen after estimating a set of nested models, as will be further discussed at the end of this subsection.

3.5.1 Household Types

There are six types of households in a school: two types of parents (low and high effort cost) interacted with three school-specific student ability levels (a_1^s, a_2^s, a_3^s) .²¹ Household types are unobservable to the researcher but may be correlated with observable household characteristics x , which include a noisy measure of student ability (x^a) and parental characteristics (x^p) containing parental education and an indicator of single parenthood. Let $\Pr((a, z) | x, s)$ be the distribution of (a, z) conditional on x in school s . The joint distribution take the following form, the details

¹⁹The equilibrium at a particular school also depends on the distribution of households $g_s(a, z)$ and η_μ . We suppress this dependence for notational convenience.

²⁰This involves evaluating a school's objective function at all possible tracking regimes.

²¹Specifically, we use a three-point discrete approximation of a normal distribution for each school, as described in Appendix A.1.

of which are in Appendix A.1:

$$\Pr((a_i^s, z) | x, s) = \Pr(a = a_i^s | x^a, s) \Pr(z | x^p, a_i^s).$$

This specification has three features worth commenting on. First, ability distributions are school-specific and discrete.²² This assumption allows us to tractably model unobserved student heterogeneity in a manner that allows ability distributions to substantially vary between schools, which is important to our understanding why schools make different tracking decisions. Second, given that the pre-determined student ability is itself an outcome of previous parental inputs, the latter being affected by parental types, the two unobserved characteristics of the households are inherently correlated with each other. We capture this correlation by allowing the distribution of parental types to depend not only on parental characteristics but also on student ability. Third, the noisy measure of student ability (previous test score) is assumed to be a sufficient statistic for ability, i.e., conditional on the prior test score, the ability distribution does not depend on parental characteristics. This means that parental characteristics may serve as shifters for parental effort, as they can be excluded from the production technology. We are not, however, assuming that parental characteristics are not informative about student ability, rather that the previous test scores summarize all the information. Indeed, pre-determined ability is itself likely affected by prior parental investments, as can be seen from the joint distribution of (x^a, x^p) .

3.5.2 Tracking Regime

The support of tracking regimes (M_s) is finite and school-specific, and is subject to two constraints. First, the choice of tracking regimes in each school is constrained by the number of classrooms. Let K_s be the number of classrooms in school s ; we assume that the size of a particular track can only take values from $\left\{0, \frac{1}{K_s}, \frac{2}{K_s}, \dots, 1\right\}$. Schools with more classrooms have finer grids of M_s . Second, the ability distribu-

²²We assume that the school-level distribution of households depends only on observables, which abstracts from the possibility that households sort across schools along unobservable dimensions. This is consistent with most of the literature studying household-school sorting. Most studies, for example Epple and Romano (1998), Caucutt (2002), Epple et al. (2002) and Ferreyra (2007), consider sorting only on observables; Mehta (Forthcoming) finds no evidence of sorting, on average, based on unobservable student characteristics.

tion within a track cannot be “disjoint” in the sense that a track cannot mix low- and high-ability students while excluding middle-ability students. Subject to these two constraints, M_s contains all possible ways to allocate students across the K_s classrooms. If a track contains multiple classrooms ($n_j > \frac{1}{K_s}$), the composition of students is identical across classrooms in the same track.²³

The cost of a tracking regime depends only on the number of tracks, and is given by

$$D(\mu) = \gamma_{|\mu|},$$

where $|\mu| \in \{1, 2, 3, 4\}$ is the number of tracks in regime μ . $\gamma = [\gamma_1, \gamma_2, \gamma_3, \gamma_4]$ is the vector of tracking costs, with γ_1 normalized to 0.

The permanent idiosyncratic shifter in the school objective function, η_μ , follows an extreme-value distribution. From the researcher’s point of view, conditional on a set of parameter values Θ , the probability of observing a particular tracking regime $\tilde{\mu}$ in school s is given by

$$\frac{\exp(V_s(\tilde{\mu}|\Theta))}{\sum_{\mu'} \exp(V_s(\mu'|\Theta))}. \quad (3)$$

3.5.3 Production Function

Student achievement is governed by

$$Y(a, q, q^{cv}, e^s, e^p, \alpha_{0s}) = \alpha_{0s} + a + \alpha_1 e^s + \alpha_2 e^p + \alpha_3 q + \alpha_4 e^s a + \alpha_5 e^s e^p + \alpha_6 \mathbf{1}\{a > q\} q + \alpha_7 e^s q^{cv}, \quad (4)$$

where the coefficient on one’s own ability a is set to one.²⁴ The interaction terms α_4 and α_5 allow the marginal effect of school effort on student achievement to depend on student ability and parental effort, respectively. The interaction term α_6 allows the marginal product of peer quality to differ for students whose ability is higher than track mean ability. Finally, α_7 allows the coefficient of variation of peer ability q^{cv} to affect the marginal product of school effort. This allows teaching to be be

²³Notice that track sizes can be different, i.e., more students can be in one track than in another. However, we have abstracted from flexibility in the size of classes within tracks. There are mixed findings in the literature about the effect of class size on achievement, see, for example, Mishel et al. (2002) for a discussion.

²⁴In other specifications where the coefficient on own ability is free, the estimate is not significantly different from one. Therefore, we choose to restrict it to be one in this final specification.

more or less effective in classes with widely-heterogeneous students.²⁵

3.5.4 Cost Functions

The cost functions for both parent and school effort are assumed to be quadratic. The cost of parental effort is type-specific, where types differ in the linear coefficient $z \in \{z_1, z_2\}$, but are assumed to share the same quadratic coefficient (c^p), such that

$$C^P(e^p, z) = ze^p + c^p(e^p)^2.$$

The cost of school effort is given by

$$C^s(e^s) = c_1^s e^s + c_2^s (e^s)^2.$$

3.5.5 Measurement Errors

We assume that both the school effort e^s and the parent effort e^p are measured with idiosyncratic errors.²⁶ The observed school effort in track j (\tilde{e}_j^s) is given by

$$\tilde{e}_j^s = e_j^{s*} + \zeta_j^s, \tag{5}$$

where $\zeta_j^s \sim N(0, \sigma_{\zeta^s}^2)$.

For parental effort, which is reported in discrete categories, we use an ordered probit model to map model effort into a probability distribution over observed effort, as specified in Appendix A.2.1. Let $\Pr(\tilde{e}^p | e^{p*})$ denote the probability of observing \tilde{e}^p when model effort is e^{p*} .

3.5.6 Further Discussion of Model Specification

Nonlinear Peer Effects Although findings on peer effects have been mixed in the reduced-form literature, a recent common theme is that it is important to allow for a nonlinear relationship between peer quality and student achievement. As we

²⁵We also tried a specification where the coefficient of variation of peer ability also entered by itself, but dropped it because we estimated it to be zero.

²⁶Todd and Wolpin (2003) discuss the implications of mismeasuring inputs when estimating production functions for cognitive achievement and Betts (2011) discusses this problem in the context of ability tracking. Stinebrickner and Stinebrickner (2004) show that it is important to account for measurement error in the study time of college students.

show in our results, we do find the nonlinear terms in the production function (4) to be significant and this specification matches the data the best among various alternatives we have tried, as shown in Appendix F. However, using our estimated model with alternative specifications, including one with a linear production technology, we find robust counterfactual policy implications. This is because even if the production technology itself is linear, a reduced-form nonlinear relationship will arise naturally from the endogenous input responses, as illustrated in Appendix D. Therefore, our findings reinforce the theme that no matter whether the production technology itself is linear or not, to measure peer effects, it is important to account for nonlinearity in a reduced-form specification.

More General Model Alternatives We have estimated a set of nested models including versions that are more general than the one presented to incorporate additional potentially important features.²⁷ We have chosen the relatively parsimonious specifications presented here because, in likelihood ratio tests, we cannot reject that the simpler model fits our data as well as the more complicated versions.²⁸ To be specific, on the household side, where we have allowed for additional heterogeneity in the effectiveness of parental effort, on top of the existing heterogeneity in parental effort costs. On the school side, we have allowed for 1) a nested logit counterpart of equation (3) in the school’s regime choice, and 2) an additional payoff in the school’s objective function that increases with the fraction of students achieving outstanding test scores.

Student Ability We focus on one dimension of what may be multidimensional ability. Accordingly, our analysis focuses on one subject: reading. Admittedly, other dimensions of a student’s ability, e.g., math and non-cognitive skills, may also affect her performance, and hence a school’s tracking decision, in reading.²⁹ However, introducing multidimensional ability would substantially complicate the model and

²⁷The final choice of the detailed specification of the model is, of course, data-dependent. The more general models we have considered do not significantly complicate estimation or simulation. We view this as good news for future work using different data because one could feasibly extend our current specification.

²⁸The conclusions from our counterfactual experiments are robust to the inclusion of these additional features (results available upon request).

²⁹There is a growing literature on multidimensional human capital, which may encompass different dimensions of cognitive and non-cognitive skills, e.g. Cunha et al. (2010).

make computation intractable, because tracking in this case would involve dividing a multidimensional space (all students) into subspaces (tracks). We therefore follow most of the studies on peer effects, where one’s achievement in one subject may be affected by peer ability in the same subject, but not by that in other subjects.³⁰ In a regression of reading test score on the past reading scores of one’s own and one’s peers, parental and school effort inputs in reading, and parental characteristics (see the data section for the exact measures), the R squared is 0.71. The R squared increases marginally, to 0.72, when we add one’s own past math score, the average past math score of one’s peers, and parental and school effort inputs in math. This finding suggests that our results may not be significantly affected by our omission of math from the production of reading skills. However, it would still be an important extension to consider tracking in a multidimensional ability setting.

4 Data

We use data from the Early Childhood Longitudinal Study, Kindergarten Class of 1998-99 (ECLS-K). The ECLS-K is a national cohort-based study of children from kindergarten entry through middle school. Information was collected from children, parents, teachers, and schools in the fall and spring of children’s kindergarten year (1998) and 1st grade, as well as the spring of 3rd, 5th, and 8th grade (2007). Schools were probabilistically sampled to be nationally representative. More than 20 students were targeted at each school for the kindergarten survey round. This results in a student panel which also serves as a repeated cross section for each school. The ECLS-K assessed student skills that are typically taught and developmentally important, such as math and reading skills. We focus on 5th grade reading classes.³¹

The data are rich enough to allow us to model the interactions between schools and parents. For students, we observe their prior (3rd grade) test scores (which are related to their ability), class membership (to identify their ability track), and end-of-the-year test scores, where test scores are results from the ECLS-K assessment.³²

³⁰See Sacerdote (2011) for a survey of studies on peer effects. This assumption is also maintained in structural work, such as Epple et al. (2002)’s study of ability tracking.

³¹We focus on reading instead of math because the former has a much larger sample size.

³²Notice that the noisy measure of student ability in our model is a student’s test score in Grade 3, which could be viewed an outcome arising from decisions made in previous periods. Given our assumptions that the Grade-3 test score is sufficient statistic for student ability and that schools

Students are linked to parents, for whom we have a measure of parental inputs to educational production (frequency with which parents help their child with homework), and parental characteristics such as education and single-parenthood (which may affect parental effort costs).³³ Assuming that homework loads on students increase teachers' effort cost, we use homework loads reported by the teacher to measure the school's effort invested in each class.³⁴

For the tracking regime, we exploit survey data containing teachers' reports on the ability level of their classes, which are available for several classes in the same school.³⁵ The question for reading classes is: "What is the reading ability level of this child's reading class, relative to the children in your school at this child's grade?" A teacher chooses one of the following four answers: a) Primarily high ability, b) Primarily average ability, c) Primarily low ability and d) Widely mixed ability.³⁶ We use the number of distinct answers given by teachers in different classes as the number of tracks in a school. Classes with identical teacher answers to this question are viewed as in the same track. The size of each track is calculated as the number of classes in that track divided by the total number of classes. Although the relative ability ranking is a priori obvious between answers a), c) and b) or d), the relative ranking between b) and d) is less so. We use the average student prior test scores within each of the two types of classes to determine the ranking between them in schools where both tracks exist. As a result, higher tracks have students with higher mean prior scores and, hence, higher mean ability.³⁷

Finally, the data indicate the Census region in which each school is located.

track Grade-5 students based only on ability, treating the Grade-3 score as pre-determined is consistent with our framework.

³³Ferreira and Liang (2012) use time spent doing homework as a measure of student effort. Due to data limitations and computational complexity, we use one measure of school effort and one of parental effort, which are both presumably the most direct effort inputs in the production of reading skills. Admittedly, effort inputs could be multidimensional, which could be investigated with an extension of the model estimated on richer data.

³⁴The same measure has been used in the study of the relationship between child, parent, and school effort by De Fraja et al. (2010). Admittedly, the use of homework loads as school effort is largely due to data limitations, yet it is not an unreasonable measure. Homework loads increase teachers' effort, who have to create and/or grade homework problem sets. Moreover, a teacher may face complaints from students for assigning too much homework. See Appendix A.2 for the survey questions.

³⁵The ECLS-K follows many students at the same school. As such, we have the above information for several classes at each school.

³⁶This variable was also used by Figlio and Page (2002) in their study of tracking.

³⁷See Table 10 in Appendix B.

We set proficiency cutoff y^* per Census region to match the proficiency rate in the data with that in the Achievement Results for State Assessments data. This data contains state-specific proficiency rates, which we aggregate to Census region.³⁸

There are 8,853 fifth grade students in 1,772 schools in the ECLS-K sample. We delete observations missing key information, such as prior test scores, parental characteristics and track identity, leaving 7,332 students in 1,551 schools. Then, we exclude schools in which fewer than four classes or ten students are observed. The final sample includes 2,789 students in 205 schools. The last sample selection criterion costs us a significant number of observations. Given our purpose of studying the equilibrium within each school, this costly cut is necessary to guarantee that we have a reasonably well-represented sample for each school. Despite this sample restriction, Table 14 shows that, among the observed 5th-graders, the summary statistics for the entire ECLS-K sample and our final sample are not very different. A separate issue is the sample attrition over survey rounds. The survey started with a nationally-representative sample of 16,665 kindergartners, among whom 8,853 remained in the sample by Grade 5. Statistics for the first survey round are compared between the whole sample and the sample of stayers in Table 15, which shows that the sample attrition is largely random.³⁹

Like most datasets, the ECLS-K has both strengths and weaknesses compared with other data. Administrative data sets typically contain test score information for all students in a school, which has spurred a large literature estimating reduced-form social interactions models wherein one's own outcome is affected by both own and peer characteristics, as well as peer outcomes.⁴⁰ However, such datasets typically lack detailed measures of school and parental inputs, due to the fact that they are not based on surveys. It is precisely these input data that we use to study the endogenous interactions between the school and the parents. The tradeoff from using survey data is that it does not contain a large number of students per school. However, as far as we know, no currently available data set, other than the ECLS-K,

³⁸<https://inventory.data.gov>. See Table 9 in this paper for regional cutoffs. Details are available upon request.

³⁹The only slight non-randomness is that students who are more likely to remain in the sample are those with both parents (75% in the whole sample vs. 79% in the final sample) and/or college-educated parents (49% vs. 51%). Although it is comforting to see that the attrition is largely random, the slight non-randomness leads us to make some caveats, which are in Appendix E.

⁴⁰This literature is discussed by Manski (1993), Moffitt (2001), and Sacerdote (2011).

contains the information that is essential for estimating a model like ours. Moreover, Tables 14 and 15 suggest that our final estimation sample is largely representative of the ECLS-K sample, which itself is nationally representative.

4.1 Descriptive Statistics

The first row of Table 1 shows that over 95% of schools practice ability tracking, i.e., they group students into more than one track. The most common number of tracks is three, which accounts for 46% of all schools. About 13% of schools have four tracks.

To summarize the distribution of students across schools, for each school we calculate the mean and the coefficient of variation (CV) of student prior test scores and the fraction of students in the school whose prior scores were below the sample median. Rows 2-4 of Table 1 present the mean of these summary statistics across schools, by the number of tracks in the school. On average, schools with more tracks have higher dispersion and lower prior scores. For example, the average prior test score in schools with only one track is 53.4 with a CV of 0.139 and fewer than 45% of students below the median. In contrast, the average prior test score in schools with four tracks is 50.0 with a CV of 0.173 and more than 57% of students below the median.

Table 1: Student Prior Test Scores in Schools by Numbers of Tracks

	1 Track	2 Tracks	3 Tracks	4 Tracks	All Schools
% of schools	4.39	37.1	45.8	12.7	100.0
Mean	53.4	51.6	51.6	50.0	51.5
CV	0.139	0.165	0.160	0.173	0.163
% below median	44.1	48.3	49.8	57.1	50.0

Tables 2 and 3 present summary statistics by the number of tracks in the school and the identity of a track. For example, entries in Columns 7-8, Row 3 of Table 2 refer to students who belong to the third track in a school with four tracks. Table 2 shows that students in higher ability tracks have both higher average outcome test scores and a higher probability of passing the regional proficiency cutoff. Comparing average student outcome scores and pass rates in schools with one track to those in other schools, they are similar to those of the middle-track students in schools with

three or four tracks. That is, the average student allocated to the lower (higher) track in a multiple-track school has poorer (better) outcomes than an average student attending a single-track school.

It is important to note that, in our setting, a track is defined as a group of students from a particular section of student ability distribution within a school. The difference between tracks may be small or large, depending on how a school chooses to divide its student ability distribution. As such, one needs to look beyond the existence/non-existence of tracking or the number of tracks, and look into the characteristics of each track in order to see the extent to which students are tracked.⁴¹

Table 2: Average outcome score and percent of students passing the cutoff

	1 track		2 tracks		3 tracks		4 tracks	
Track	Score	% pass	Score	% pass	Score	% pass	Score	% pass
1	51.84	69.42	45.95	50.88	44.92	42.85	45.40	33.38
2			51.98	75.75	51.38	68.47	51.44	61.00
3					55.62	84.39	51.45	64.54
4							57.99	97.87
All	51.84	69.42	49.73	66.83	50.81	66.88	52.08	65.70

The top panel of Table 3 shows average school effort by track. With the exception of Track 4 in schools with four tracks, the average school effort (expected hours of homework done by students per week) increases with track level, or track-specific average ability, in schools with more than one track. At the school level, the average effort level stays roughly constant with the number of tracks. The bottom panel of Table 3 shows that parental effort shows the opposite pattern compared to school effort. Average parental effort (frequency of helping child with homework in reading) decreases with student ability or track level. For example, in schools with three tracks, while parents of lowest-track students on average help their children 2.57 times per week, parents of highest-track students do so only 2.11 times.

Table 4 summarizes parental effort and student outcomes by household characteristics. Compared to their counterpart, parents without college education, single

⁴¹Evidence presented in Appendix C shows that, while the mean range in prior test scores (i.e., mean prior test score in a school's highest track minus that of the school's lowest track) is one standard deviation, this range varies quite a bit among schools that track.

parents and parents with lower-prior-achievement children exert more parental effort. Nevertheless, average student outcome test scores are lower in these households.

Table 3: Average teacher and parent effort by track and number of tracks

Teacher effort				
Track	1 track	2 tracks	3 tracks	4 tracks
1	1.86	1.75	1.75	1.82
2		1.90	1.88	1.84
3			1.96	1.93
4				1.68
All	1.86	1.83	1.86	1.82

Parent effort				
Track	1 track	2 tracks	3 tracks	4 tracks
1	2.07	2.31	2.57	2.29
2		2.03	2.37	2.71
3			2.11	2.78
4				2.08
All	2.07	2.11	2.27	2.38

Table 4: Parent effort and outcome test score by observed characteristics

	Parent effort	Outcome test score
Less than college	2.35	48.00
Parent college	2.12	54.24
Single parent hh	2.37	48.76
Two-parent hh	2.18	52.37
Grade 3 score below median	2.61	45.35
Grade 3 score above median	1.82	57.96

5 Estimation and Identification

5.1 Estimation

We estimate the model using maximum likelihood, where parameter estimates maximize the probability of observing the joint endogenous outcomes, given the observed

distributions of household characteristics across schools.

The parameters Θ to be estimated include model parameters Θ^0 and parameters Θ^ζ that govern the distribution of effort measurement errors. The former (Θ^0) consists of the following seven groups: 1) Θ_y governing student achievement production function $Y(\cdot)$, which includes the vector of school-specific technology shifters $\{\alpha_{0s}\}_{s=1}^S$, 2) Θ_ϵ governing the distribution of shocks to test score ϵ , 3) Θ_{c^s} governing school effort cost, 4) Θ_{c^p} governing parental effort cost, 5) Θ_D governing the cost of tracking regimes, 6) ω , the weight associated with proficiency in school's objective function, 7) Θ_T governing the distribution $\Pr((a, z) | x, s)$ of household type given observables.⁴²

The endogenous outcomes observed for school s (O_s) include the tracking regime $\tilde{\mu}_s$, track-specific school effort $\{\tilde{e}_{sj}^s\}_j$ and household-level outcomes: parental effort \tilde{e}_{si}^p , the track to which the student is assigned τ_{si} , and student final test score y_{si} . Let $X_s = \{x_{si}\}_i$ be the observed household characteristics in school s . The vector X_s enters the likelihood through its relationship with household types (a, z) , which in turn affect all of O_s .

The Likelihood The likelihood for school s is

$$L_s(\Theta) = l_{\tilde{\mu}_s}(\Theta^0) \prod_j l_{sj}(\Theta \setminus \Theta_D) \prod_i l_{si}(\Theta_y, \Theta_\epsilon, \Theta_T, \Theta_{c^p}, \Theta^\zeta),$$

where each part of the likelihood is as follows:

A. $l_{\tilde{\mu}_s}(\Theta^0)$ is the probability of observing the tracking regime, which depends on all model parameters Θ^0 , since every part of Θ^0 affects a school's tracking decision, but not on Θ^ζ . It is given by (3).

B. $l_{sj}(\Theta \setminus \Theta_D)$ is the contribution of the observed school effort \tilde{e}_{sj}^s in track j given the tracking regime $\tilde{\mu}_s$. It depends on all Θ^0 but Θ_D since the latter does not affect school effort decision given the tracking regime. It also depends on Θ^ζ because

⁴²The distribution that enters the model directly, i.e., $g_s(a, z)$, does not involve additional parameters, because

$$g_s(a, z) = \int \Pr((a, z) | x, s) dF_s(x),$$

where $F_s(x)$ is the distribution of x in school s .

school effort is measured with error:

$$l_{sj}(\Theta \setminus \Theta_D) = \frac{1}{\sigma_{\zeta^s}} \phi \left(\frac{\tilde{e}_{sj}^s - e_j^{s*}(\tilde{\mu}_s | X_s, \Theta^0 \setminus \Theta_D)}{\sigma_{\zeta^s}} \right).$$

where ϕ denotes the standard normal density.

C. $l_{si}(\Theta_y, \Theta_\epsilon, \Theta_T, \Theta_{cp}, \Theta^\zeta)$ is the contribution of household i , which involves integrating type-specific contributions to the likelihood over the distribution of household types:

$$l_{si}(\Theta_y, \Theta_\epsilon, \Theta_T, \Theta_{cp}, \Theta^\zeta) = \sum_{a,z} \Pr((a, z) | x_i, s, \Theta_T) l_{si}((a, z) | \Theta_y, \Theta_\epsilon, \Theta_{cp}, \Theta^\zeta),$$

where $l_{si}((a, z) | \Theta_y, \Theta_\epsilon, \Theta_{cp}, \Theta^\zeta)$ is the contribution of household i if it were type (a, z) :

$$l_{si}((a, z) | \Theta_y, \Theta_\epsilon, \Theta_{cp}, \Theta^\zeta) = \left[\begin{array}{l} \Pr\{track = \tilde{\tau}_{si} | a, \tilde{\mu}_s\} \times \\ \Pr(\tilde{e}_{si}^p | e^{p*}(e_{\tau_{si}}^s, q_{\tau_{si}}, a, z | \Theta_y, \Theta_{cp}, \Theta^\zeta)) \times \\ f_\epsilon[(y_{si} - Y(a, q_{\tau_{si}}, e_{\tau_{si}}^s, e^p(\cdot) | \Theta_y)) | \Theta_\epsilon] \end{array} \right].$$

The three components of $l_{si}((a, z) | \cdot)$ are

- 1) the probability of being assigned to $\tilde{\tau}_{si}$ given tracking regime $\tilde{\mu}_s$ and ability a , which is implied by $\tilde{\mu}_s$ and $g_s(a)$;
- 2) the contribution of the observed parental effort \tilde{e}_{si}^p given peer quality and the model predicted school effort $e_{\tau_{si}}^s$ in track τ_{si} , which depends on parental cost parameters, the achievement parameters and the measurement error parameters; and
- 3) the contribution of test score given all model predicted inputs, which depends on achievement parameters and the test score distribution parameter.

5.2 Identification

Manski (1993) distinguishes between three components in social interactions models: endogenous effects (the direct effect of peer outcomes on one's own outcome), exogenous contextual effects (the effect of exogenous peer characteristics on one's own outcome), and correlated effects (shocks that are common to both one's peers and

oneself). Separating these channels has been the focus of much empirical work.⁴³ The focus of this paper is to understand the interrelated nature of school tracking decisions, track input choices, and parental effort choices, not to push the frontier of identification of social interactions models. However, it does take these inferential problems seriously and addresses them by utilizing two arguments. First, similar to other work in this area (Caucutt (2002), Epple and Romano (1998), Epple et al. (2002)), the mechanism through which tracking operates is through peer quality. Although peer quality and one’s own ability depend on the history of inputs, they are pre-determined at the beginning of the game. Therefore, as in this other work, the “reflection problem” of separating endogenous effects from exogenous contextual effects is not relevant here.⁴⁴ Although endogenous social interactions do not pose an identification problem in our context, we must address the problem of separating exogenous contextual effects, which operate through unobserved peer quality, and correlated effects. In our context, this is the same as accounting for selection into peer groups (Moffitt (2001)).

Intuitively, if we do not account for the correlation between own ability and peer quality when students are tracked, what looks like the effect of peers may instead be correlated unobservables (i.e., own ability and peer quality are correlated), creating an inferential problem. This concern may arise from two layers of unobservables: at the school level and within a school, at the track level. School-level unobservables can bias our findings if not controlled for because, for example, households with two working parents may buy into a better district with a more productive school but may have less time available for parenting. To address this concern, we introduce school-specific shifters α_{0s} in the production function, which also allow for any type of matching between households and schools. Given school-specific shifters, our identification of the production function then relies mainly on within-school variation.

To account for the correlation between own ability and peer quality within a school, we exploit two key pieces of information available in the ECLS-K survey. The first is student prior test scores, which are assumed to be sufficient statistics for

⁴³For example, Bramoullé et al. (2009) circumvents the reflection problem by exploiting sample finiteness. See Betts and Shkolnik (2000a) for a review of other work in this literature.

⁴⁴This also means we can estimate a linear-in-means technology without having to exploit non-linearities in peer effects, a strategy identified by Blume et al. (2006).

student abilities. The distribution of prior student test scores in each school therefore maps into the school-specific ability distribution. The second piece of information comes from teacher surveys, which directly classify each student’s track quality. This additional information, available for multiple classes, allows us to essentially “observe”, or compute, two key components of model.⁴⁵ The first is the tracking regime used by each school. The second is the composition of peer ability in each track. Each track comprises a section of the school-specific ability distribution; the section is specified by the tracking regime. Such information directly reveals peer quality, despite the fact that, as discussed in Betts (2011), a student’s ability is not directly observable. Therefore, we are able to separate the roles of own ability and peer quality in the technology, a common identification concern in this literature.

It may seem to be a strong assumption that prior test scores are sufficient statistics for ability. For example, suppose a low-ability student, who happened to have a very high prior score, was observed in a low track and received a low outcome test score; if such an observation was not adjusted in some manner one could overestimate the effect of tracking. However, note that the likelihood also considers the track assignment probability, $\Pr\{track = 1|a, \tilde{\mu}_s\}$, which would be low for a high-ability student. Therefore, when the likelihood integrates over the ability distribution of this given student, it naturally downweights the unlikely case where the student was of high ability (as indicated by their high prior score) but assigned to a low track.

6 Results

6.1 Parameters

In this section, we present estimates of key parameters. Other parameter estimates can be found in Appendix B. The top panel of Table 5 presents the production technology parameter estimates.⁴⁶ We find that school effort and parental effort complement each other, while the interaction between a student’s own ability and school effort is negative. To understand the magnitudes of these parameters, first we calculate outcome test scores according to the estimated production function by

⁴⁵ Assuming we observe a representative sample of classes per school.

⁴⁶ Recall that the coefficient of student’s own ability has been set to one.

increasing one input at a time while keeping other inputs at their baseline equilibrium values—that is, not taking into account behavioral responses. The average marginal effect of increasing ability by one standard deviation (sd) above the model average causes a 72% sd increase in the outcome test score. On average, increasing peer quality by one sd causes a 20% sd increase in the outcome test score. We also find evidence of a heterogeneity in the marginal product of increasing peer quality: Increasing peer quality for a student whose ability is above the track average results in a 17% larger increase than doing so for a student with ability below the track average. On average, increasing school effort by one sd causes a 5% sd increase in the outcome test score; increasing parental effort by one sd increases the outcome test score by 61% sd. We do not estimate there to be a strong interaction between the track coefficient of variation in peer quality and the marginal product of school effort.

The *ceteris paribus* estimate of the effect of peer quality may seem larger than some of the reduced-form estimates of peer effects found in the literature. However, reduced-form estimates correspond to the *composite effect* of peer quality, which is a combination of the direct effect of a change in peer quality and the indirect effect of input changes. Based on our estimates, this composite effect is such that 1 sd increase in peer quality would, on average, increase the outcome test score by 9% sd, which lies in the range of findings in the literature (see footnote 8 and Sacerdote (2011)). We discuss this result in more detail in Section 7.1.1.

Instead of presenting the estimates for all the school-specific shifters, we summarize the correlation between the shifters and attendant household characteristics via an OLS regression. The results, shown in Table 13 in Appendix B, show that school-specific shifters are positively correlated with student prior test score, parental education and the presence of both parents, suggesting positive assortative matching.

The middle panel of Table 5 presents estimates of school-side parameters. We find a low value of ω , suggesting that schools do not care much about how many students are proficient, compared to their concerns about average achievement. This finding may be due to the fact that the test score we use is an ECLS-K survey instrument, not a high-stakes test. This is consistent with findings from the school accountability literature, which finds that pressure, such as No Child Left Behind (NCLB), leads to large gains on high-stakes tests, but much smaller gains on low-

Table 5: Key parameter estimates

Production technology parameters			
Par.	Estimate	SE	
α_1	9.27	0.66	school effort
α_2	17.68	0.39	parent effort
α_3	0.28	0.04	track peer quality
α_4	-0.05	0.01	interaction: ability and school effort
α_5	1.71	0.30	interaction: school and parent effort
α_6	0.05	0.01	interaction: track peer quality and above track peer quality
α_7	-5.35	9.14	interaction: school effort and CV of track peer quality
School parameters			
Par.	Estimate	SE	
ω	2.45	1.82	weight on passing proficiency standard
c_2^s	2.06	0.04	quadratic school effort cost
γ_2	-1.03	0.16	regime cost, 2 tracks
γ_3	-0.73	0.16	regime cost, 3 tracks
γ_4	0.41	0.23	regime cost, 4 tracks
Household cost parameters			
Par.	Estimate	SE	
c^p	0.10	0.003	quadratic parent effort cost
z_1	0.08	0.01	linear parent effort cost, low cost type
z_2	0.11	0.01	linear parent effort cost, high cost type
θ_0^c	-44.02	2.19	cost type intercept
θ_1^c	0.85	0.02	cost type, ability
θ_2^c	-10.74	3.38	cost type, single parent indicator
θ_3^c	3.62	2.45	cost type, college indicator

stakes exams.⁴⁷ The second row shows that the cost of school effort is convex in effort levels.⁴⁸ The last three rows show that the cost of tracking regimes is non-monotone in the number of tracks. The most costly tracking regimes are those with four tracks (the highest possible number), followed by those with only one track (the cost of which normalized to zero). Without further information, our model is unable to distinguish between various components of the cost associated with

⁴⁷See, for example, Koretz and Barron (1998), Linn (2000), Klein et al. (2000), Carnoy and Loeb (2002), Hanushek and Raymond (2005), Jacob (2005), Wong et al. (2009), Dee and Jacob (2011), and Reback et al. (2014).

⁴⁸We cannot reject that the linear cost term c_1^s is zero, so we set it to zero.

different tracking regimes. However, we think these estimates are not unreasonable. On the one hand, increasing the number of tracks may involve developing more types of curricula as well as incur higher resistance from parents. On the other hand, pooling all students into one track may make the classroom too heterogeneous and thus difficult for the teacher to handle. If these competing costs are both convex in the number of tracks, one would expect the total cost to be higher for the one- and four-track cases. governing parental effort cost and the probability of being a high-cost parent. We find that the cost of parental effort is convex. The linear type-specific cost term is 43% higher for the high-cost type than for the low-cost type. Evaluated at the baseline levels of parental effort, the high-cost type incurs a 13% higher cost than the low-cost type. The last four rows show the relationship between household characteristics with parental cost types. Parents with higher education and higher-ability children are more likely to have higher effort costs, as are two-parent households. This is consistent with the data in Table 4, where parents without a college education and parents of children with lower prior achievement exert more parental effort, yet average student outcome test scores are lower in these households.

6.2 Model Fit

Overall, the model fits the data well. Table 6 shows model fit in terms of tracking patterns. The first two columns show that the model closely matches the distribution of tracking regimes across all schools. The next two columns show the fit for schools with lower spread of prior test scores, where a school is called a low-spread school if it has a coefficient of variation (CV) of prior scores below the median CV. The model slightly overpredicts the fraction of two-track schools and underpredicts that of three-track schools. The next two columns focus on schools with higher-achieving students. In particular, we rank schools by the fraction of lower-prior-score (below median score) students from high to low: the higher the ranking of a school, the higher fraction of its students are of low initial achievement. We report the tracking regime distribution among schools that are ranked below the median in this ranking, i.e., schools with relatively better students. Overall, the model captures the pattern that schools with less variation and/or higher prior test scores are more likely to have only one track and less likely to have four tracks. The top panel of Table 7

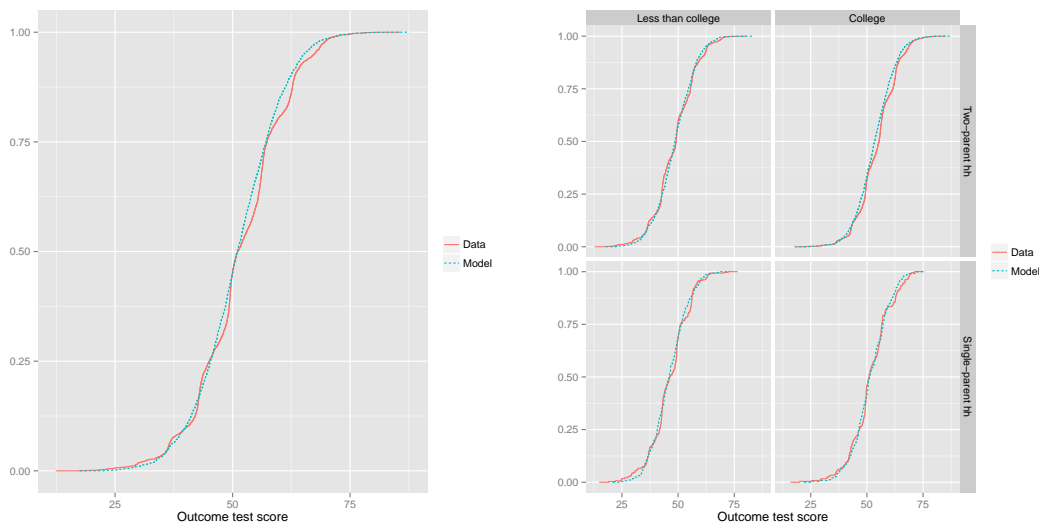
Table 6: Tracking regimes

	All schools		Low Spread*		Low fraction of low ability**	
	Data	Model	Data	Model	Data	Model
% 1 track	4.39	4.96	4.85	5.26	4.55	5.16
% 2 tracks	37.07	36.96	36.89	38.86	40.91	38.58
% 3 tracks	45.85	46.62	47.57	44.56	47.27	44.88
% 4 tracks	12.68	12.46	10.68	11.32	7.27	11.38

* “Low spread” schools have a below-median coefficient of variation in prior score.

** “Low fraction of low ability” schools have a below-median fraction of schools with below-median prior score.

Figure 1: Fit: Outcome test scores



shows that the model fits average outcome scores by track. The exception is that the model over-predicts the average score in the second track within two-track schools and that in the third track within four-track schools. Figure 1 shows the model predicted CDF of outcome test scores contrasted with the data counterpart. The left panel shows the case for all students. The right panel splits the distribution by parental education and by single-parenthood. As seen, the model-predicted score distributions match well with the ones in the data.

The middle panel of Table 7 shows the model fit for school effort. Compared to the data, the model predicts a flatter profile for school effort across tracks. The bottom panel of Table 7 shows the model fit for mean parental effort: the model

Table 7: Outcomes by track and number of tracks

Mean outcome test score								
	1 Track		2 Tracks		3 Tracks		4 Tracks	
Track	Data	Model	Data	Model	Data	Model	Data	Model
1	51.84	50.97	45.95	47.70	44.92	46.10	45.40	46.48
2			51.98	54.95	51.38	51.22	51.44	50.55
3					55.62	56.27	51.45	54.65
4							57.99	58.14

Mean school effort								
	1 Track		2 Tracks		3 Tracks		4 Tracks	
Track	Data	Model	Data	Model	Data	Model	Data	Model
1	1.86	1.87	1.75	1.88	1.75	1.87	1.82	1.87
2			1.90	1.83	1.88	1.87	1.84	1.89
3					1.96	1.81	1.93	1.85
4							1.68	1.81

Mean parent effort								
	1 Track		2 Tracks		3 Tracks		4 Tracks	
Track	Data	Model	Data	Model	Data	Model	Data	Model
1	2.07	2.21	2.31	2.46	2.57	2.63	2.29	2.76
2			2.03	1.96	2.37	2.26	2.71	2.46
3					2.11	1.93	2.78	2.17
4							2.08	1.95

matches the fact that parent effort decreases with track level, although the gradient is over-predicted in the four-track case (last two columns). Finally, Table 8 shows that the model fits well the level of parental effort and outcome scores by household characteristics. Parents with less education, single parents, and students with lower prior score all have higher parent effort levels and lower outcome test scores.

7 Counterfactual Simulations

We use the estimated model to simulate two policy-relevant counterfactual scenarios. We contrast the outcomes between the baseline and each of the counterfactual cases. In particular, we present Average Treatment Effects (ATE) for different endogenous outcomes (some of which are inputs to achievement), for subgroups of students

Table 8: Means of parent effort and outcome score, by household characteristics

	Parent effort		Outcome score	
	Data	Model	Data	Model
Less than college	2.35	2.39	48.00	48.28
College	2.12	2.17	54.24	53.37
Single parent	2.37	2.38	48.76	48.89
Two parents	2.18	2.23	52.37	51.84
Low prior score	2.52	2.50	45.35	47.46
High prior score	1.78	2.02	57.96	55.09

defined by their characteristics, such as prior test scores.⁴⁹

In the first counterfactual simulation, we quantify the effect of allowing tracking by solving the model when tracking is banned (hence all schools have only one track). We compare the changes in school effort, parental effort and student achievement. Our results indicate that failing to account for the equilibrium interactions between schools and parents could substantially bias the results. In the second counterfactual simulation, we examine the equilibrium effects of prospective changes in proficiency standards. In particular, we solve for optimal region-specific proficiency standards that would maximize average achievement. Unlike the tracking-ban counterfactual, here a school re-optimizes its tracking decision in response to this policy change.⁵⁰

It is worth noting that the first counterfactual simulation enables one to understand the effects of any tracking, regardless of how intensely different schools choose to track when allowed to do so under the baseline. To implement such a policy, a technology that detects and monitors tracking practice would be required. Such a technology is not necessary for the second counterfactual policy.

7.1 Heterogeneous Effects of Tracking

We compare the outcomes under the baseline with those when tracking is banned and every school has to put all of its students into a single track. Over 95% of the

⁴⁹When a continuous variable is used to define subgroups, as they are in Figures 3 and 4(a), ATEs are calculated by non-parametric smoothing.

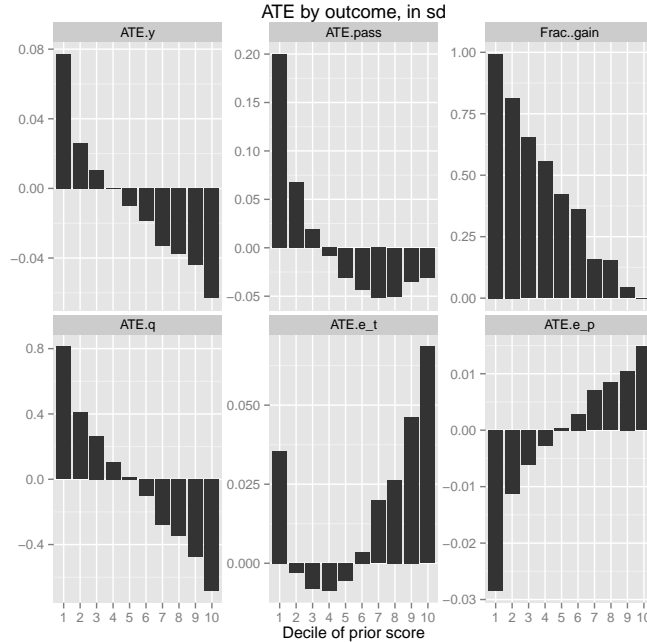
⁵⁰Results from our counterfactual experiments are subject to the caveat that household distributions across schools are fixed. Incorporating households' school choices into our framework is an important extension we leave for the future work.

simulated schools practice ability tracking to some degree under the baseline, hence are affected by this counterfactual and experience an exogenously imposed change in peer quality within classrooms.

The ATE of banning tracking, over all students, is -0.8% sd in test scores. Our result that tracking (in the short run) hurts lower-ability students and benefits higher-ability students is consistent with findings from some previous studies, e.g., Betts and Shkolnik (2000b) and Hoffer (1992). Notice that in calculating the ATE, we have followed the practice of most studies in this literature and treated test scores as cardinal measures of ability. However, as noted by Bond and Lang (2013), test scores should most naturally be treated as ordinal. Caveats should be taken when one makes cross-group comparisons or average calculations of the effects on test scores, the magnitudes of which can be sensitive to the particular scale used in the test. Therefore, we view the distribution of outcomes reported in Figure 2 as more informative. The first two panels in the top row show average results for outcome test scores and pass rates, by decile of prior test score. The effects of a tracking ban are positive for students with lower prior test scores and negative for those with higher prior test scores, as measured in both the level of the final test scores and the pass rates. In particular, students with below-median prior scores gain 2.2% sd when ability tracking is banned, while those with above-median prior scores lose 4.2% sd when tracking is banned. Consistent with the first two panels, the third panel shows that the fraction of students who gain from a tracking ban declines with prior test scores.

Underlying the changes in student outcomes are the changes in the inputs, i.e., peer quality, school effort and parental effort, which are plotted in the bottom three panels of Figure 2, by decile of prior test scores. The tracking ban places all students in a school in one track, which means that lower ability students are on average placed with better students, and are made better off through the technology, *ceteris paribus*. The opposite holds for higher ability students. However, that is not the entire story. Both school effort and parental effort adjust to the change in peer composition imposed by this policy. Without the freedom to optimize over tracking regimes, schools that used to track students can only optimize over their effort inputs. As there is only one track, a school can only choose one effort level for all students. On average, schools increase their effort for students in most deciles (the second panel on the bottom). This change is most obvious for students with very

Figure 2: Change in outcomes and inputs due to banning tracking, by decile prior score



high or very low prior test scores, who are more likely to have been tracked under the baseline. Unlike schools, which choose one effort level for all students in one track, parents can always adjust their effort levels for their own children. Indeed, the last panel shows that changes in parental effort are not only quantitatively but also qualitatively different across students with different prior test scores. Average parental effort decreases for students below the median and increases for those above the median. In particular, parents of students in the highest decile increase their inputs by the largest amount when tracking is banned, by about 1.5% sd.

Figure 2 highlights the trade-offs a school faces when choosing a tracking regime. Improving peer quality in one track necessarily involves reducing it in another, which will in turn lead to changes in parental effort. When low-ability students are grouped with higher-ability students, peer quality increases (decreases) for students with low (high) ability. For parents with low-ability students, who have been exerting much higher effort than those with high-ability students (Table 8), the exogenous increase in peer quality provides strong incentives for them to reduce their own effort. For parents with high-ability students, the exogenous decrease in peer quality pushes them to increase their own effort as a remedy. However, given the concavity of

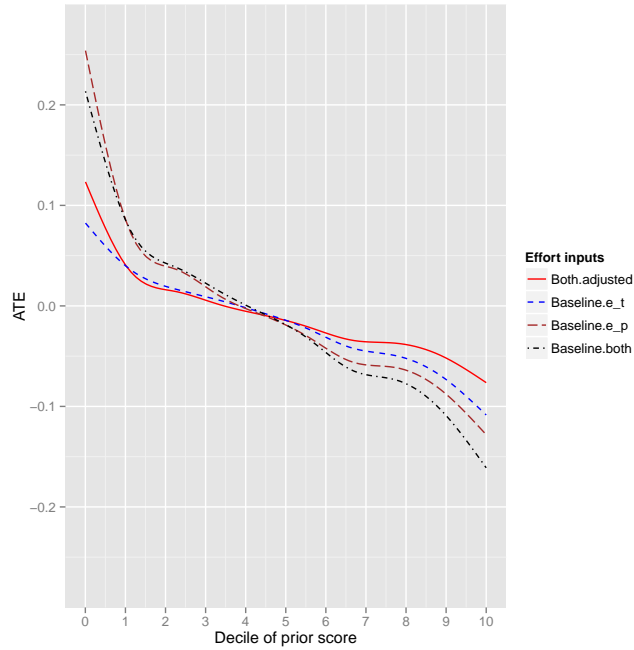
the parents' net payoff with respect to their own effort, the reduction in effort by the parents of low ability children is larger than the increase in effort by the parents of high ability children, especially among parents with very low-achieving children. To curb the reduction in parental effort among low-achieving households and encourage more effort in high-achieving households, the school increases its own effort, utilizing the fact that school effort and parental efforts are complementary to each other. Depending on the different sets of households they face, schools differ in how much effort they need to exert and how their students perform when they do not track versus when they track. These differences drive schools' different tracking decisions.

7.1.1 The Importance of Accounting for Behavioral Changes: Effort Inputs

The test score technology plays an important role in evaluating the effect of tracking on student outcomes. This might prompt one to ask whether estimates of parameters governing the technology alone would adequately characterize outcomes for students were tracking banned. To illustrate the value of estimating an equilibrium model where schools and parents may respond to changes in track peer quality, we contrast the ATE of banning tracking from our model prediction with the ATE ignoring endogenous effort responses. Let (q^*, e^{s*}, e^{p*}) denote inputs under the base-line scenario where tracking is endogenous, and $(q_{CF}^*, e_{CF}^{s*}, e_{CF}^{p*})$ denote counterfactual equilibrium inputs when tracking is banned.

Figure 3 graphs the ATE for test scores (y-axis) against student prior test scores (x-axis). The red (solid) line is the model-predicted ATE, taking into account the change in peer quality induced by a tracking ban, as well as school and parent effort responses, i.e. $Y(a, q_{CF}^*, e_{CF}^{s*}, e_{CF}^{p*}) - Y(a, q^*, e^{s*}, e^{p*})$ (recall $Y(\cdot)$ is the test score technology). The blue (short dashed) line is the ATE, ignoring school effort adjustments, i.e. $Y(a, q_{CF}^*, e^{s*}, e_{CF}^{p*}) - Y(a, q^*, e^{s*}, e^{p*})$. Banning tracking pushes schools to increase their effort; ignoring this biases the effect of tracking ban downwards, although not by much. The bias from ignoring parental responses is much larger. The brown (long dashed) line is the ATE for test scores ignoring parental effort adjustment, i.e., $Y(a, q_{CF}^*, e_{CF}^{s*}, e^{p*}) - Y(a, q^*, e^{s*}, e^{p*})$. When tracking is banned, lower-achieving students receive more inputs from the school, in terms of both peer

Figure 3: ATE of banning tracking : equilibrium vs fixed effort inputs



quality and school effort. In response, parents of these students reduce their own effort. Failing to take into account this reduction drastically overstates the ATE of banning tracking for these students. The brown line lies far above the red line for students with the lower prior scores, especially those at the end of the distribution. The opposite is true for students with higher prior scores, whose parents increase their provision of costly effort in response to the lower peer quality. The black (dotted-dashed) line is the ATE for test scores ignoring both school and parent effort adjustments, i.e., $Y(a, q_{CF}^*, e^{s*}, e^{p*}) - Y(a, q^*, e^{s*}, e^{p*})$. On average, ignoring effort changes would cause one to overstate the gains from banning tracking by 147% for students with below-median prior scores, and overstate the loss by 121% for students with above-median prior scores.

We have repeated the above counterfactual experiments using the estimated model with alternative production technology specifications, including one with a linear technology; the detailed results are shown in Appendix Table 16. Across all these specifications, the message remains the same: it is important to account for effort responses. For students with below-median prior scores, the bias from ignoring effort responses ranges from 70%-121%. For students with above-median

prior scores, the bias from ignoring effort responses ranges from 102%-147%.

Two points are worth noting. First, although the effect of a tracking ban on test scores is significantly attenuated by parental effort responses, its effect on household welfare is likely to be larger. In particular, banning tracking benefits households with lower-prior-score children both by increasing student achievement and by saving parental effort; while it hurts households with higher-prior-score children both by lowering student achievement and by increasing parental effort. Second, we study the effect of (non)tracking while holding the distribution of households across schools constant. It is plausible that some households will react by changing schools. For example, some households with high-ability children may change schools to reduce their losses from a ban on tracking, which will in turn decrease the gain for low-ability students from the ban. That is, taking this additional layer of household response into account may further attenuate the policy effect.

7.2 Optimal Proficiency Standards: Trade-offs

Policy changes will often create winners and losers. This is especially true in the case of educational policies that may affect ability tracking, which has qualitatively different effects on students of varying ability levels. This is because changing peer quality for some students necessarily involves changing peer quality for some other students, which is accompanied by adjustment in the effort choices by the school and by parents. By incorporating tracking regimes, school effort, and parental effort inputs into one framework, our work lends itself to a better understanding of the effects of education policy, especially with respect to the trade-offs they involve. Our second counterfactual experiment highlights this point by examining how changes in proficiency standards would affect the distribution of student achievement. Because schools care about the fraction of students above the proficiency standard, changes in these standards can guide the new equilibrium toward certain policy goals.⁵¹ As an illustration, we search for region-specific proficiency standards that maximize the region-specific averages of student test scores.⁵²

⁵¹As mentioned earlier in the paper, our estimate of a school's preference for the lower-tail of the score distribution does not reflect the pressure it faces from high-stakes tests; thus, this experiment should not be interpreted as increasing the bar for a high-stakes test.

⁵²This experiment is an illustration of the trade-offs. One could also use our framework to study the effects of other policy changes, which may generate different distributions of winners and losers.

Figure 4: Proficiency standards by region, data and counterfactual

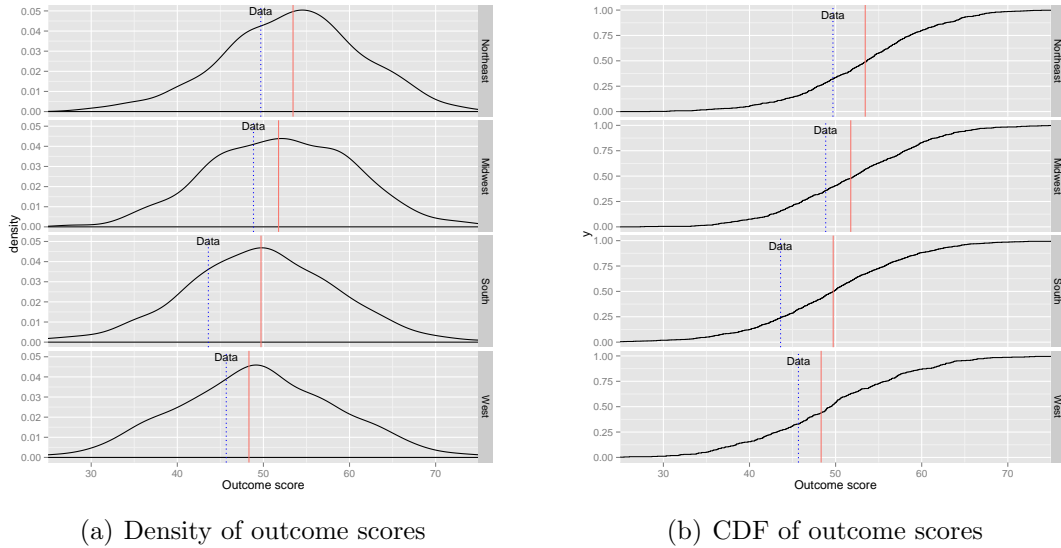
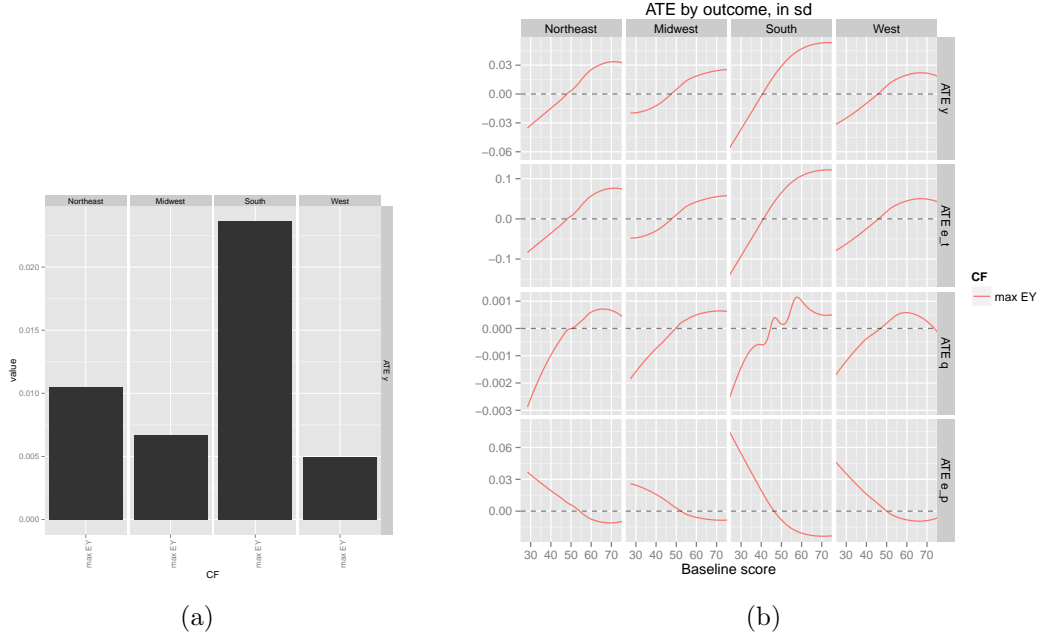


Figure 4 places proficiency standards in relation to the distribution of baseline outcome test scores, by region, where the left panel (4(a)) shows the density and the right panel (4(b)) shows the CDF. Each panel overlays the distribution of baseline outcome scores with the baseline proficiency standards (blue dotted line). Loosely speaking, the ranking of the regional distributions of student baseline achievement from high to low (in the sense of first order stochastic dominance) is the Northeast, the Midwest, the West, and lastly the South. This ranking lines up with that of regional proficiency standards, with the Northeast having the highest standard and the South having the lowest. In all four regions, the baseline (data) standard is approximately located at the 30th percentile of the regional distribution of the outcome scores.

The regional standards that maximize region-specific average achievement are the red solid lines in Figure 4, which are higher in all four regions than the baseline standards. When standards are lower (as in the baseline), schools have the incentive to improve outcomes near the lower end of the distribution, which would sacrifice a much larger measure of students near the middle and top of the distribution of baseline scores. The same argument applies for the case if standards are too high. The new standards maximize the average performance by moving schools' attention away from the low-performing students toward a location that rewards schools for

Figure 5: The effect of increasing in proficiency standards to maximize average achievement



improving mean test scores. In fact, the new standards are located at around the median of each region’s baseline distribution of test scores, which is also where the density of outcome scores is highest, as seen in Figure 4(a). Figure 5(a) summarizes the ATE on outcome scores by region due to the change in standards. Setting standards to maximize average achievement has the biggest ATE in the South, followed by the Northeast, the Midwest and finally the West. Intuitively, the ATE is increasing in the difference between the baseline and achievement-maximizing standards.

Figure 5(b) plots the ATE on the outcome score and inputs (school effort, peer quality, and parental effort) by baseline score and region. The top panel shows that in all four regions, the ATE on outcome test scores increases with student baseline outcome scores. In fact, the ATE is negative for students with low baseline scores and positive for students with high baseline scores. Underlying the pattern of the ATE on outcome test scores is schools’ redistribution of their inputs away from low-achieving students toward high-achieving students, as shown in the second and third panels of Figure 5(b). Schools decrease (increase) their effort inputs for

students with lower (higher) baseline scores. In addition, schools also change their tracking regimes such that peer quality decreases (increases) for low-achieving (high-achieving) students. Finally, the bottom panel shows that the ATE on parental effort moves in the opposite direction to school inputs, which mitigates but does not reverse the effects of school input adjustment on student outcomes.

8 Conclusion

We have developed and estimated a model of ability tracking, in which a school's tracking regime, track-specific inputs, parental effort and student achievement are joint equilibrium outcomes. The estimated model has been shown to fit the data well.

Using the estimated model, we have shown that the effects of tracking are heterogeneous across students with different prior test scores. In response to the exogenous changes in peer composition under a ban on tracking, schools on average increase their effort inputs for all students. Parents with low-ability students decrease their effort, while those with high-ability students increase theirs. As a result, a ban on tracking would increase the performance for students with below-median prior scores by 2.2% sd, while decrease that for those with above-median prior scores by 4.2% sd.

These seemingly small effects confound the change in peer quality and that in effort inputs, both of which have *significant impacts* on achievement. In fact, ignoring endogenous effort changes would cause one to overstate the gains from banning tracking by over 100% for students with below-median prior scores, and overstate the loss by over 100% for students with above-median prior scores. Our work, therefore, re-emphasizes the importance of taking into account behavioral responses when evaluating policy changes, as argued in Becker and Tomes (1976). It is worth noting that this general principle applies regardless of whether the estimation is done on observational or experimental data.⁵³

Because policies that affect schools' tracking decisions will lead to increases of peer quality for some students and decreases for some other students, our study highlights the trade-offs involved in achieving certain educational policy goals. In

⁵³Carrell et al. (2013) provides a good example of this principle in the context of experimental data.

particular, we have shown that a change in proficiency standards that maximizes average achievement would lead schools to redistribute inputs away from low-ability students toward high-ability ones, in terms of both peer quality and track-specific effort. As a result, the performance of high-ability students increases at the cost of low-ability students.

Our work is promising for future research. Researchers have been expanding the depth and width of datasets, so it is not overly optimistic to think that a dataset containing similar information as the ECLS-K, but at a larger scale per school, will be available in the near future. Our methodology can be easily applied to such a dataset, and, due to its tractability, would remain computationally feasible even when the dataset is large. Moreover, our work also admits potentially interesting extensions for future research. One extension of particular interest would combine studies on the matching between schools and households and this paper into a single framework. This extension would form a more comprehensive view of how peer composition is determined both between and within schools. An even more ambitious project may take into account residential sorting (Epple and Romano (1998), Ferreyra (2007)) or even the responses in housing prices as discussed in Avery and Pathak (2015). Another important extension would allow teachers to vary in quality and examine how schools match students with teachers, within a tracking framework.

Appendix

A Functional Forms

A.1 Type Distribution

Denote observable characteristics $x = (x^a, x^p)$, where x^a is the prior test score and x^p includes parent education level and whether or not it is a single-parent household.

Each school has three ability levels $(a_l^s, l = 1, 2, 3)$. Let T_l^s be the l^{th} tercile of $F_{s,a}(\cdot)$, which is the normal distribution approximation of prior test scores of all students in school s , $(\{x_{si}^a\}_i)$. A level a_l^s is defined as the expectation of prior score within the l^{th} tercile in school s computed using $F_{s,a}(\cdot)$, i.e.,

$$\begin{aligned} a_1^s &= \int_{-\infty}^{T_1^s} a dF_{s,a}(a|a \leq T_1^s), \\ a_2^s &= \int_{T_1^s}^{T_2^s} a dF_{s,a}(a|T_1^s < a \leq T_2^s), \\ a_3^s &= \int_{T_2^s}^{\infty} a dF_{s,a}(a|T_2^s < a). \end{aligned}$$

The distribution of type conditional on x is assumed to take the form

$$\Pr((a_l^s, z) | x, s) = \Pr(a = a_l^s | x^a, s) \Pr(z | x^p, a_l^s),$$

$$\Pr(a = a_1^s | x^a, s) = 1 - \Phi\left(\frac{x^a - T_1^s}{\sigma_a}\right)$$

$$\Pr(a = a_3^s | x^a, s) = \Phi\left(\frac{x^a - T_2^s}{\sigma_a}\right)$$

$$\Pr(a = a_2^s | x^a, s) = 1 - \Pr(a = a_1^s | x^a) - \Pr(a = a_3^s | x^a),$$

where σ_a is a parameter to be estimated. The probability that a parent is of a

high-cost type is given by

$$\Pr(z = z_2 | x^p, a_l^s) = \Phi(\theta_0^c + \theta_1^c a_l^s + \theta_2^c \mathbf{1}\{x_2^p = \text{single parent}\} + \theta_3^c \mathbf{1}\{x_1^p \geq \text{college}\}). \quad (6)$$

A.2 Effort measurement system

A.2.1 Parental effort

Observed parental effort \tilde{e}^p is discrete and ordered in 5 categories, as in Question HEQ.095 in the Spring 6 Parent Questionnaire.

“During this school year, how often did someone help CHILD with his/her reading, language arts or spelling homework? Would you say...

A. Never; B. Less than once a week; C. 1 to 2 times a week; D. 3 to 4 times a week; E. 5 or more times a week; F. REFUSED; G. DON'T KNOW.”

Model parental effort is unobserved, but the measurement system below maps e^{p*} and an i.i.d. error term $\zeta^p \sim N(0, 1)$ into discrete ordered levels:

$$\tilde{e}^p = \begin{cases} \text{never} & \Leftrightarrow -\infty < e^{p*} + \zeta^p \leq \kappa_0 \\ < 1 & \Leftrightarrow \kappa_0 < e^{p*} + \zeta^p \leq \kappa_1 \\ \in [1, 2] & \Leftrightarrow \kappa_1 < e^{p*} + \zeta^p \leq \kappa_2 \\ \in [3, 4] & \Leftrightarrow \kappa_2 < e^{p*} + \zeta^p \leq \kappa_3 \\ \geq 5 & \Leftrightarrow \kappa_3 < e^{p*} + \zeta^p \leq \infty, \end{cases} \quad (7)$$

resulting in $\Pr(\tilde{e}^p | e^{p*})$.

A.2.2 School effort

School effort is measured in hours per week, according to Question 2 in the Spring 6 Teacher Questionnaire.

“For subjects you teach, about how much time do you expect children to spend on homework in each of the following area (Reading and Language Arts) on a typical evening?

A. I don't teach this subject; B. None; C. 10 min; D. 20 min; E. 30 min; F. More

than 30 min.”⁵⁴

The observed effort is given by $\tilde{e}_j^s = e_j^{s*} + \zeta_j^s$, where $\zeta_j^s \sim N(0, \sigma_{\zeta^s}^2)$.

B Additional Tables

Table 9: Data: Proficiency cutoffs by Census region

Region name	Proficiency cutoff	Corresponding sample percentile
Northeast	49.69	42.34
Midwest	48.85	34.21
South	43.61	21.41
West	45.69	26.25

Table 10: Data: Prior Test Scores By Track

Track	Mean	Standard deviation	N
1	48.28	11.02	886
2	52.28	8.44	1,227
3	53.99	8.27	563
4	54.99	6.54	113

Table 11: Data: Standardized¹ Prior Test Scores By Track

Track	Mean	Standard deviation	N
1	0.93	0.18	886
2	1.02*	0.15	1,227
3	1.05*	0.14	563
4	1.09*	0.12	113

Note 1: Prior scores are standardized through division by school average prior score.

Note *: Significantly greater than previous standardized track mean (p-value < 0.002).

⁵⁴We treat both E and F as 30 minutes per day.

Table 12: Other Parameter Estimates

Parameter	Estimate	Standard error	
σ_a	8.94	0.55	sd of shock in ability distribution
σ_ϵ	6.72	0.16	sd test score measurement error
σ_{ζ^s}	0.56	0.05	sd school effort measurement error
κ_0	-0.17	0.05	cut-point 1, parent effort measurement
κ_1	0.78	0.03	cut-point 2, parent effort measurement
κ_2	1.98	0.03	cut-point 3, parent effort measurement
κ_3	2.78	0.04	cut-point 4, parent effort measurement

Table 13: Regression of School Intercepts on Household Characteristics and Prior Score

Variable	Coefficient	(Std. Err.)
College	1.491	(0.163)
Single-parent household	-0.633	(0.196)
Prior score	0.097	(0.008)
Intercept	-69.413	(0.430)

Note: "College" means highest education level of parents is college or higher.

Table 14: Summary Statistics: Full Sample vs Selected Sample (5th Grade)

Variable	Whole Sample			Selected Sample		
	N	Mean	Standard Deviation	N	Mean	Standard Deviation
Sample Size	8,853			2,789		
Parental Education	8,047	0.55	0.50	2,789	0.59	0.49
Single Parent	7,953	0.23	0.42	2,789	0.19	0.39
Prior Test Score	8,562	50.17	9.72	2,789	51.47	9.52
Outcome Test Score	8,751	50.15	9.65	2,777	51.68	9.40
Parental Effort	7,788	2.29	1.55	2,703	2.22	1.53
Teacher Effort	2,784	2.03	0.58	533	1.90	0.59
Student Obs. in the School	1,772	5.00	5.54	205	13.61	3.38

Note: “Whole Sample”: all fifth graders observed in the ECLS-K.

“Selected Sample”: the sample used for our empirical analysis.

Table 15: Summary Statistics: Whole Sample vs Stayers (Kindergarten)

Variable	All Kindergartners ^{1a}			Stayers ^{1b}		
	N	Mean	Standard Deviation	N	Mean	Standard Deviation
Sample Size	16,665			8,494		
Parental Education	15,705	0.49	0.50	8,185	0.51	0.50
Single Parent	14,329	0.25	0.43	7,772	0.21	0.41
Prior Test Score	13,734	49.19	9.83	7,039	50.00	9.69
Outcome Test Score	14,579	49.56	9.87	7,831	50.24	9.60

Note 1a: All kindergartners observed in the ECLS-K.

Note 1b: Kindergartners who are observed in the fifth grade whole sample.

Note 2: The stats are measured in the first survey round, where parental and teacher effort are both unreported.

C Tracking Intensity

This section characterizes the extent to which tracks within a school differ from one another, i.e., tracking intensity, and how tracking intensity differs between schools. To do this, we computed, for each school that tracks students, the mean prior test score for each track (track mean), and then computed the range in track means

within each school. We find that the difference between the mean prior score in the highest versus lowest ability tracks varies considerably between schools: the 25th percentile difference is 0.29 (sample) sd of prior test scores, the median and mean differences are 0.98 and 1.02 sd, respectively, and the 75th percentile difference is 1.59 sd. That is, conditional on it being implemented, tracking still means different things across schools. Our model is designed to be flexible enough to accommodate this type of heterogeneity.

D Nonlinear Peer Effects

There is recent interest in nonlinear peer effects, where the marginal effect of a change in peer quality is not uniform across students (Sacerdote (2011)). It is important to note that, even in our preferred specification which excludes an interaction term between own ability and peer quality in the technology, the reduced form mapping between peer quality and achievement is inherently nonlinear (i.e. depends on own ability), because we endogenize school and parental effort decisions.

To see this, consider a student with ability a_i in a track with peer quality q_j . To derive the reduced-form achievement function, substitute her parents' optimal effort e_i^{p*} and optimal track effort e_j^{s*} into (4), and differentiate with respect to peer quality:

$$\frac{\partial Y}{\partial q} = \alpha_3 + \alpha_1 \frac{\partial e_j^{s*}}{\partial q} + \alpha_2 \frac{\partial e_i^{p*}}{\partial q} + \alpha_5 \left(e_i^{p*} \frac{\partial e_j^{s*}}{\partial q} + e_j^{s*} \frac{\partial e_i^{p*}}{\partial q} \right).$$

To ease exposition, suppose $\frac{\partial e_j^{s*}}{\partial q} = 0$, which results in the expression

$$\frac{\partial Y}{\partial q} = \alpha_3 + \frac{\partial e_i^{p*}}{\partial q} \underbrace{(\alpha_2 + \alpha_5 e_j^{s*})}_{>0}.$$

Due to curvature of the parents' objective, parents will have different responses to changes in peer quality depending on their children's ability levels, i.e., $\frac{\partial^2 e_i^{p*}}{\partial q \partial a} \neq 0$. This implies that $\frac{\partial^2 Y}{\partial q \partial a} \neq 0$, which is what we set out to show.

E Caveats Arising From Sample Attrition

Although the attrition is largely random, as shown in Table 15, the slight non-randomness leads us to make the following caveats. Case 1: If the non-random attrition reflects the sorting of students across different schools, our sample will represent (hence our results will apply to) only a subset of schools in the U.S.: those with students from relatively better family backgrounds. In this case, our parameter estimates will not be biased. Case 2: If the attrition reflects higher frequency of school switches among a subgroup of students, and if the out-flow of such students from one school is associated with an in-flow of similar students into the same school, then students with better family backgrounds will be over-represented in our sample within a school. This may bias our parameter estimates. Given that the sorting of students into different schools has been widely documented in the literature,⁵⁵ we feel that concerns arising from Case 1 may be more relevant, i.e., our results may not apply to all schools in the U.S., but a subset of them.

F Alternative Production Function Specifications

In each specification, student achievement is governed by a different production function $Y(a, q, q^{cv}, e^s, e^p, \alpha_{0s})$. In particular,

$$\text{Specification 1: } Y(\cdot) = \alpha_{0s} + a + \alpha_1 e^s + \alpha_2 e^p + \alpha_3 q + \alpha_4 e^s a + \alpha_5 e^s e^p$$

$$\text{Specification 2: } Y(\cdot) = \alpha_{0s} + a + \alpha_1 e^s + \alpha_2 e^p + \alpha_3 q + \alpha_4 e^s a + \alpha_5 e^s e^p + \alpha_6 \mathbf{1}\{a > q\}q$$

$$\text{Specification 3: } Y(\cdot) = \alpha_{0s} + a + \alpha_1 e^s + \alpha_2 e^p + \alpha_3 q + \alpha_4 e^s a + \alpha_5 e^s e^p + \alpha_7 e^s q^{cv}$$

$$\text{Specification 4: } Y(\cdot) = \alpha_{0s} + a + \alpha_1 e^s + \alpha_2 e^p + \alpha_3 q + \alpha_4 e^s a + \alpha_5 e^s e^p + \alpha_6 \mathbf{1}\{a > q\}q + \alpha_7 e^s q^{cv}$$

The first specification includes school-specific intercepts. The second specification adds a nonlinear effect of peer quality, through $\alpha_6 \mathbf{1}\{a > q\}q$, allowing the effect of peer quality to differ based on one’s own ability. In the terminology of Hoxby and Weingarth (2005), this allows for a “single-crossing” model of peer effects. The third specification adds to the first one, adding an interaction between teacher effort and the track-level coefficient of variation, $\alpha_7 e^s q^{cv}$. In the terminology of Hoxby and Weingarth (2005), this allows for a “boutique” model of peer effects, wherein

⁵⁵See, for example, Caucutt (2002) and Epple and Romano (1998).

Table 16: Summary of Main Results, Across Production Function Specifications
Specification

	1	2	3	4
ATE				
All students	-0.005	-0.009	0.001	-0.008
By prior score				
Below-median	0.039	0.024	0.032	0.022
Above-median	-0.055	-0.049	-0.034	-0.042
Marginal effect of 1 sd increase in peer quality				
Ceteris paribus	0.24	0.20	0.19	0.20
Composite	0.13	0.10	0.08	0.09
Bias from ignoring effort adjustment (%)				
Below-median y0	112	137	102	147
Above-median y0	70	85	114	121
log likelihood	-16459.5	-16440.1	-16432.7	-16426.7

Note: Values are expressed in sd of outcome score, unless otherwise noted.

student homogeneity may facilitate designing relevant lesson plans and classroom activities. The fourth specification is the focal specification presented in this paper, and nests specifications 1-3.

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