

NBER WORKING PAPER SERIES

AFFIRMATIVE ACTION AND HUMAN CAPITAL INVESTMENT:
EVIDENCE FROM A RANDOMIZED FIELD EXPERIMENT

Christopher Cotton
Brent R. Hickman
Joseph P. Price

Working Paper 20397
<http://www.nber.org/papers/w20397>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2014

We greatly appreciate comments from John List, Andrew Sweeting, and participants of seminars at the University of Chicago, the 2013 meeting of the American Economic Association, the 2013 North American summer meetings of the Econometric Society, 2014 Canadian Public Economic Group conference, the 2015 Econometric Society World Congress, the 2015 European Meetings of the Economic Science Association, Brigham Young University, University of Missouri-Columbia, Queen's University, Ryerson University, SUNY Binghamton, University of British Columbia-Okanagan, University of Guelph, and London School of Economics. We also wish to acknowledge the outstanding research assistance of Joe Patten, who played a crucial role in executing this project. Hickman acknowledges support for the financial incentives in this experiment provided by a grant from the Social Sciences Division at the University of Chicago. Cotton is grateful for financial support provided by his position as the Jarislowsky-Deutsch Chair in Economic and Financial Policy at Queen's University. Cotton and Price gratefully acknowledge partial funding from the Spencer Foundation which supported an initial pilot study that lead to this research. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2014 by Christopher Cotton, Brent R. Hickman, and Joseph P. Price. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Affirmative Action and Human Capital Investment: Evidence from a Randomized Field Experiment
Christopher Cotton, Brent R. Hickman, and Joseph P. Price
NBER Working Paper No. 20397
August 2014, Revised September 2015
JEL No. C93,D44,D82,J15,J24

ABSTRACT

Pre-College human capital investment occurs within a competitive environment and depends on market incentives created by Affirmative Action (AA) in college admissions. These policies affect mechanisms for rank-order allocation of college seats, and alter the relative competition between blacks and whites. We present a theory of AA in university admissions, showing how the effects of AA on human capital investment differ by student ability and demographic group. We then conduct a field experiment designed to mimic important aspects of competitive investment prior to the college market. We pay students based on relative performance on a mathematics exam in order to test the incentive effects of AA, and track study efforts on an online mathematics website. Consistent with theory, AA increases average human capital investment and exam performance for the majority of disadvantaged students targeted by the policy, by mitigating so-called "discouragement effects." The experimental evidence suggests that AA can promote greater equality of market outcomes and narrow achievement gaps at the same time.

Christopher Cotton
Queen's University
Department of Economics
Kingston, Ontario K7L 3N6
cotton@econ.queensu.ca

Brent R. Hickman
Department of Economics
University of Chicago
1126 E. 59th Street
Chicago, IL 60637
hickmanbr@uchicago.edu

Joseph P. Price
Department of Economics
Brigham Young University
162 FOB
Provo, UT 84602
and NBER
joseph_price@byu.edu

1. INTRODUCTION

Affirmative Action (AA) is the practice of granting preferential treatment to under-represented demographic groups when allocating contractual, employment, or educational opportunities. In the US it was first mandated by the Kennedy Administration in the 1960s, and has since been widely implemented in public procurement, education, and hiring. Today, AA is a pervasive fixture of American university admissions, though it has generated much controversy.¹ AA is also widely implemented outside the United States, from Malaysia to Northern Ireland, and in India where *Reservation Law*, a set of racial and ethnic quotas, is imposed by constitutional edict.

In the US university context, the rationale for AA does not stem from concerns over proactive discrimination. Rather, the rationale is that the university market is effectively a competition where black and Hispanic children are at a fundamental disadvantage to whites and Asians due to residual effects of past institutionalized discrimination. That is, they attend lower quality schools, are less affluent, have less educated parents, and have less access to other developmental inputs such as health care and tutoring.² In turn, AA is an attempt at compensating for the competitive disadvantage by giving special consideration for race. There is a substantial empirical literature studying the effects of AA, focusing mainly on its direct impact at the point when university placement outcomes are determined.³

However, the literature has largely ignored the implications of AA for student behavior *prior* to admissions, as they study, participate in extra curricular activities, and otherwise invest in human capital before applying to university. For example, consider a set of students vying for admission to universities which vary by quality. Each student has 24

¹The US Supreme Court has deliberated on the legality of racial considerations in university admissions at least five times, including the cases of *Schuetz v. Coalition to Defend Affirmative Action* (2014), *Fisher v. Texas* (2013), *Grutter v. Bollinger* (2003), *Gratz v. Bollinger* (2003), and *Regents of the University of California v. Bakke* (1978). At the time of this writing, *Fisher v. Texas* (2013) had been scheduled for a second hearing before the Court after having been remanded back to the US 5th Circuit Court.

²Lyndon B. Johnson, Kennedy's successor, was the first American president to implement AA. In his 1965 commencement address at Howard University, Johnson articulated this idea as a motivation for AA: "You do not take a person who, for years, has been hobbled by chains and liberate him, bring him up to the starting line of a race and then say, 'you are free to compete with all the others,' and still justly believe that you have been completely fair. Thus it is not enough just to open the gates of opportunity. All our citizens must have the ability to walk through those gates... To this end equal opportunity is essential, but not enough, not enough. Men and women of all races are born with the same range of abilities. But ability is not just the product of birth. Ability is stretched or stunted by the family that you live with, and the neighborhood you live in—by the school you go to and the poverty or the richness of your surroundings. It is the product of a hundred unseen forces playing upon the little infant, the child, and finally the man."

³William G. Bowen and Derek Bok [1998], Peter Arcidiacono [2005] and Jessica S. Howell [2010], have attempted estimation of counterfactual racial admissions profiles in a color-blind world. Linda Datcher Lounsbury and David Garman [1995], Richard H. Sander [2004], Mark C. Long [2008], Jesse Rothstein and Albert H. Yoon [2008], and David L. Chambers, Timothy T. Clydesdale, William C. Kidder and Richard O. Lempert [2005], have estimated the impact of AA on graduation rates among UR minority groups.

hours in a day and would prefer to consume time as leisure, but time is a required input for human capital production. Since university seats are allocated according to measured human capital (HC) output (*e.g.*, through an exam score), each student will choose to invest some fraction of her leisure time based on her own ability, the distribution of her competitors, and her prospects for winning a favorable outcome. Since AA alters academic cutoffs required for admittance depending upon one's demographic status, it in turn shapes a student's labor-leisure decision. Thus, some important questions arise: is there a trade-off between fostering diversity on university campuses and human capital investment during middle and high school? Does AA increase or decrease incentives for its beneficiaries (under-represented minorities)? How does it impact incentives for other students? Finally, how does AA shape the racial achievement gap?

We begin the paper by presenting a model of human capital investment which is tailored toward capturing important aspects of competitive university admissions. The model allows for predictions about the direction in which AA should impact effort and performance distributions within each demographic group. There are two demographic groups of students, whose learning costs systematically differ, in a competitive investment period running up to a matching game. A fixed distribution of university seats is awarded in rank-order fashion by a centralized admissions board. The model predicts that HC production effort will depend on a student's relative ability. Moreover, if the admissions board bases placement partially on demographic status, then, conditional on a fixed ability level, investment will also differ across groups. Specifically, AA tends to decrease HC accumulation by the highest ability minority students, while increasing investment in HC by intermediate and low ability individuals. Students in the non-minority group respond in a largely opposite way, with the highest ability students in this group increasing their effort and the lowest ability students decreasing it.

Overall, we find that AA has the potential to largely *increase* minority investment activity by mitigating so-called "discouragement effects" which arise from cost asymmetry across demographic groups. Discouragement is a well-known phenomenon in the literature on rank-order competition. The idea is fairly simple: holding fixed gross payoffs and one's own productivity type, if a dominating shift in the distribution of one's competitors occurs so that one's own type falls further behind, eventually effort/output will fall. This logic is independent of one's fixed productivity type, being an artifact of the competition one faces instead. Thus, if AA works to dampen discouragement effects for minorities by shifting their effective competition group, it may actually increase their willingness to re-allocate leisure time toward building HC instead. Moreover, this increase in effort happens on the region of the minority distribution where students are

most concentrated, meaning it generally leads to an increase in average investment activity. The sign of the average effect within the non-minority group is less clear-cut.

After presenting the theoretical framework, we develop a field experiment to investigate how AA shapes the labor-leisure trade-off in mathematics learning, as well as measured proficiency following a period of competitive HC investment. Our experimental design, modeled after the theoretical framework, structures a set of short-run incentives so as to mimic important aspects of university admissions. In our setup, cash prizes stand in for university placement outcomes to motivate test subjects to spend leisure time learning math.⁴ Students are individually randomized into two treatments for our math competition, a “color-blind” control treatment, and an AA treatment. Our experiment involves paying middle-school-aged students based on their relative performance on the American Mathematics Competition 8 (AMC8), a national mathematics exam, in similar fashion as outcomes are determined on the university matching market using college entrance exams and high school grades. In order to create a clean test of theory on investment and discouragement effects, we use grade cohort as our demographic delimiter. This distinction mirrors some important racial disparities—our disadvantaged students (*e.g.*, 7th graders) have received, on average, fewer inputs into their math education relative to their counterparts (*e.g.*, 8th graders) with substantial overlap in the ability distribution notwithstanding—while filtering out other factors such as cultural differences or stereotype threat that could confound the effects we seek to investigate.

In the color blind treatment, students compete against others in both grade levels for prizes, with AMC8 score rankings determining each student’s pay-off. In the AA (or “representative quota”) treatment, we proportionally split the overall distribution of prizes and earmark subsets for each group, after which students compete only with other students from their same grade. In other words, a representative quota reserved a more favorable set of prizes for students in the lower grade, relative to what they would receive when competing with higher grade students. And, of course, the opposite is true for the set of prizes allocated to the higher grade students.

Because we are interested in how AA changes incentives to invest real effort into developing HC, a typical classroom experiment in which students are assigned to a treatment immediately before completing some task is not ideal. Instead, we worked with teachers to incorporate the experiment into the students’ schooling over a course of two weeks.

⁴Our experiment covers short-run incentives spanning a matter of weeks. The longer pre-university investment period, which spans years, is of ultimate interest to policy makers, but is difficult to study in an environment where the researcher can experimentally create exogenous identifying variation. Thus, the current study focuses on how AA shapes competitive market forces which drive individual labor-leisure decisions and short-run human capital accumulation. What the magnitude of these forces might be when students are discounting over longer periods of time, with payoffs in mind that are much larger than those we can create, is beyond the scope of current research.

We assessed two in-class AMC8 exams separated by 2 weeks—a pre-exam to measure baseline proficiency and a post-exam to allocate prizes and measure progress—and we provided the students with a 10-day interim period in which they could study math problems ahead of the final AMC8 exam. During this period we provided the students access to a website with practice materials, and tracked their use of this site. By showing that students in the color blind and AA treatments access the site at different rates, we are the first experimental study to identify an effect of AA on real learning effort. We did this in a natural classroom and home learning environment, where the free-time/study-time trade-offs were familiar to test subjects, and mimic scenarios they face when preparing for the competitive university admissions process during high school.

We find strong evidence that AA favorably alters labor-leisure trade-offs and measured proficiency by our disadvantaged group. Although the highest ability disadvantaged students did decrease performance as predicted by theory, this drop was very modest, with the majority of disadvantaged students increased learning time and test performance under AA. On average, the AA policy increased test scores among the disadvantaged group by $1/5$ of a standard deviation over the course of the experiment. While some portion of this increase may be due to greater focus or mental effort on the day of the exam, we also show that disadvantaged students in the AA treatment are more than twice as likely to use the practice website to prepare for the exam, spend almost triple the amount of total time, and attempt more practice questions relative to their counterparts in the control group. In this way, we are the first experimental paper to show that AA policy can increase real effort exerted by its beneficiaries to acquire HC.

One might worry that these gains within the demographic group targeted by the policy come at the cost of commensurate weakening in incentives for other students, but the data do not support this concern. We find no evidence that these gains come at the expense of lower average investment or test scores for higher grade students. Consistent with the theoretical model, we find suggestive evidence that the highest ability advantaged students increase performance, and lower ability advantaged students decrease performance under AA, though these shifts are small and not statistically significant. This suggests that, despite their gross payoffs declining due to AA, advantaged students as a group had low enough learning costs to maintain roughly the same performance as under color blind allocations.

The remainder of this paper has the following structure. Section 2 gives an overview of the previous literature and explains how our contribution relates to what has been done before. Section 3 presents a theoretical model of HC investment and university admissions to motivate our experimental design. We also explore model predictions to be tested in our study. Section 4 describes the structure of our field experiment in more

detail. Section 5 presents and discusses our experimental results. Section 6 concludes, and the Appendix covers additional technical details of our study, potentially relevant theory model extensions, and presents additional tables and graphs.

2. RELATED LITERATURE

The model which guides our experimental study is based on the more general theoretical framework in Aaron Bodoh-Creed and Brent R. Hickman [2015]. Both papers depict a model of the university admissions market as an all-pay auction, in which students' bids are investments in HC, and prizes are seats at different universities. We present a simple model focusing on aspects of the market that are most readily testable using experimental methods. Bodoh-Creed and Hickman [2015], on the other hand, provide a more general theoretical model and analysis, establishing conditions under which a rich college assignment model with AA can be represented in a more tractable form. They begin by fleshing out a finite model, exploring how, when the number of agents and seats is large, the analytically unruly equilibrium is well approximated by a simpler setting where agents and university seats belong to a continuum set. For our purposes in this paper, it is sufficient to focus only on the continuum setting; interested readers are directed Bodoh-Creed and Hickman [2015] for the technical foundations behind this modeling choice.

They derive general results concerning the properties and design of market mechanisms for college placement. In this paper, our focus is more narrow in one way and more broad in another. First, we restrict attention to experimental comparisons between two very simple, canonical mechanisms: a pure rank-order (color-blind) rule, and a demographically representative quota. This allows us to assess the real-world applicability of the theoretical predictions on academic outcomes, and measure their associated magnitudes. Second, we use our novel experimental design with on-line monitoring of study effort to explore background phenomena—*e.g.*, labor-leisure trade-offs—on which the theory is silent. This allows us to confirm whether observed movements in outcomes induced by our treatment are associated with actual learning activities (*i.e.*, HC investment as the general framework assumes), or something less socially valuable such as exam-day effort or purely wasteful signaling. In that sense, this paper and Bodoh-Creed and Hickman [2015] may be seen as complementary to one another.

Our theoretical framework is also related to Stephen Coate and Glenn Loury [1993], which shows how an AA policy can make high value jobs more-easily attainable and can therefore reduce the incentives for minorities to undertake costly investment that make them more qualified for such jobs. The key difference between our model—with many-to-many matching—and theirs—with one-to-one matching—is that there is no

scarcity of high value positions in Coate and Loury's framework. In our framework, students compete with one another for a fixed set of heterogeneous university seats. We show that, although AA will decrease incentives for the highest ability minorities to invest, the opposite effect will predominate for most of them. Although our results are consistent with Coate and Loury [1993] for the highest ability minorities, our predictions are quite different for the overall minority population. We also test our predictions with the experiment, and find evidence largely consistent with our theoretical model.

Previous experimental work has also tested the link between AA and effort. Examples in the laboratory include Andrew Schotter and Keith Weigelt [1992], where asymmetry was exogenously imposed by researchers assigning cost functions to subjects, and Anat Bracha, Alma Cohen and Lynn Conell-Price [2015] which focused on gender-based asymmetry in quantitative problem solving. Caterina Calsamiglia, Jorg Franke and Pedro Rey-Biel [2013] conducted a related field experiment in which 10-13 year old children compete in Sudoku puzzles in a pair-wise (one-to-one) tournament, with asymmetry stemming from previous exposure to Sudoku. Each of these studies found that AA can increase average performance by disadvantaged players.

Our analysis offers a number of advantages over previous experimental studies. First, we include an investment period between assignment to a treatment group and our final exam. During this period we monitor student time usage at home in a non-invasive way. Ours is the first paper to do so, and it provides us with a window into individuals' labor-leisure trade-offs in actual learning, rather than focusing solely on in-class effort during a task. Our experimental design allows us to assess the impact of AA on both interim investment *and* final outcomes. Math learning in preparation for the AMC8 exam is close to learning that enables higher performance on college entrance exams, and therefore provides a relevant measure of HC investment. Second, our experimental competition more closely mirrors important aspects of actual college admissions markets. It is a many-to-many competitive matching contest (with participants numbering in the hundreds) having wide heterogeneity among both competitors and prize values. This heterogeneity is communicated to our test subjects in simple terms, similar to how US university applicants can compare their own standardized test score to mean scores of students who typically enroll in their most preferred universities. By working with test subjects' regular teachers, using materials that were already being used as teaching tools in their schools, and allowing for study choices at home, we are the first experiment to create a natural setting in which test subjects are making decisions similar to those which will lead to their ultimate college placement outcomes. Third, we intentionally chose an experimental design to eliminate concerns about stereotype threat—our test subjects were fairly homogeneous culturally and socioeconomically—so as to be able to

measure magnitudes of incentive effects in a clean environment. Fourth, our format also allows for overlap in ability distributions across demographic groups, rather than assuming no overlap as previous experimental work has done. This allows us to compare the performance distributions of students from asymmetric demographic groups to see whether different qualitative effects at different quantiles appear, as predicted by theory.

There is also a substantial empirical literature studying AA and its impact on college admissions. Bowen and Bok [1998] used student-level applications data to estimate the preference given to minority students by admissions officers at elite schools. Arcidiacono [2005] and Howell [2010] estimated structural models which adjust counterfactuals for changes in minority application behavior induced by policy shifts. A related vein of the literature focuses on mismatching, or the idea that AA may cause damage to black students by assigning them to academically demanding environments for which they are unprepared. Loury and Garman [1995] and Sander [2004] present evidence supporting mismatching. Other empirical work, such as Long [2008], Rothstein and Yoon [2008], and Chambers et al. [2005], suggest that some mismatching may occur, but its magnitude is relatively small and is outweighed by the benefits of placing blacks into higher quality institutions. Throughout this literature SAT—“Scholastic Aptitude Test”—scores are used as a proxy for student ability, and assumed to be fixed. However, students’ incentives to invest in HC during middle and high school depend on admission policies they expect to face when applying to college. Test scores are therefore a function of student ability *and* market incentives induced by AA. Assuming SAT scores are independent of the prevailing admissions policy may bias counterfactual estimates. Our objective is to test this theory in a field experimental study of how students adjust their effort (influencing their test scores) in response to changes in AA policy.

This paper contributes to a small but growing literature recognizing that admissions policies shape incentives for pre-college HC accumulation. Bruno Ferman and Juliano Assuncao [2011] found evidence that test scores among black Brazilian high-school students decreased in response to an admissions quota at elite universities in Rio de Janeiro.⁵ Hickman [2015] estimated a structural empirical model of the U.S. college admissions market based on the theoretical foundation of Bodoh-Creed and Hickman [2015]. His counterfactual analysis of admissions, investment, and welfare under alternative AA policies found evidence that AA increases the stock of minority HC overall,

⁵ While seemingly contradictory to our findings, our model predicts that a quota which affects all points in the quality spectrum of universities will lead to a widening of the demographic achievement gap among the most talented students. Ferman and Assuncao [2011] estimated a negative overall effect of a policy change which applied only to top universities in Rio De Janeiro. Thus, the policy they study would have predominantly impacted high-ability black students who were at less of a disadvantage to begin with. Likewise, Brent R. Hickman [2015] estimated that AA negatively affects investment among a small mass of the highest ability minorities in the US, even though on average the effect is positive.

and provided further comparisons between color-blind admissions, representative quotas, and American-style preference-based AA (score bonuses).

Finally, our study is also related to others concerning performance pay in primary and secondary schools. Recent studies including Eric Bettinger [2012], Edwin Leuven, Hessel Oosterbeek and Bas van der Klaauw [2010] and Roland Fryer [2011] found mixed results about whether paying students can improve their effort and outcomes. Our experimental incentives are different from these in that we paid participants based on their *relative performance* rather than offering a fixed wage contract. Michael Kremer, Edward Miguel and Rebecca Thornton [2009] also studied a program awarding merit scholarships based on relative performance, and found evidence that students respond to changing financial incentives in a competitive environment. Our experimental design has two main differences from these studies: first, we were able to monitor outcomes *and* interim time inputs; and second, our focus is not on the effectiveness of pay-for-performance *per se*, but rather on *differences* between common relative performance incentive schemes. Finally, in a competitive setting some of a subject's behavior may be driven by an intrinsic desire to win, as in Christopher Cotton, Frank McIntyre and Joseph Price [2013] who presented evidence that simply framing a task as a contest can lead to better performance by some participants. In our study, we frame a common task as *two different contests*, which allows us to pick up on differences across alternative allocation mechanisms.

3. THEORY

3.1. MODEL. Here we present a model of AA in competitive HC investment which will form the basis of our experimental design. It is a special case of Bodoh-Creed and Hickman [2015] who developed the technical foundations of the framework presented here. We model university admissions as a Bayesian game with a continuum of heterogeneous students of mass 1. Each student exerts effort developing human capital (HC). A student's relative HC output affects the quality of university she attends. The competition between students for higher-valued outcomes is a many-player all-pay contest for a continuum of heterogeneous university seats of mass 1. Students differ in their cost of HC accumulation, and university seats differ by quality.

Each student simultaneously chooses a level of HC accumulation, $h_i \geq \underline{h}$, where the quantity \underline{h} represents a minimum pre-requisite for market participation, as when university applicants must first earn a high school diploma in order to be considered. Acquiring HC requires time—which could otherwise be consumed as leisure—and effort, and is therefore thought to be costly. Students differ by their background, abilities, access to help, and other resources that affect the rate at which time is converted into new HC. At the individual level, these factors are summarized by a parameter θ_i , which determines

the marginal costs of student i 's HC accumulation. Investment entails a utility cost $c(h_i; \theta_i)$, which is strictly increasing and convex in the amount of output, or $c'(h_i; \theta_i) > 0$ and $c''(h_i; \theta_i) > 0$ for all $h_i \geq \underline{h}$. We also assume $\frac{\partial c}{\partial \theta} > 0$ and $\frac{\partial^2 c}{\partial h \partial \theta} > 0$ so that smaller θ implies a more productive student having lower costs for a given level h , and also a lower marginal cost of increasing output from h to $h + \Delta$. θ_i is privately known to each student, who views costs of her competitors as realizations of a random variable Θ .

There is a continuum of university seats of mass 1. We denote an individual seat by quality level s , and model the relative masses of different seats as following distribution F_S . Throughout the paper, we shall simplify discussion by assuming $F_S = \text{Uniform}[\underline{s}, \bar{s}]$ and $[\underline{s}, \bar{s}] = [0, 1]$ so that seat quality index and quantile rank are the same, with higher s indicating a more valuable seat. A student placing at seat s_i experiences benefit $u(s_i) = s_i$. Net utility of student i is

$$U(s_i, h_i; \theta_i) = s_i - c(h_i; \theta_i).^6 \quad (1)$$

For simplicity here, a student's gross match utility $u(\cdot)$ depends only indirectly on her HC, through determining the quality of university to which she is admitted. In the appendix, we explore an extension where HC directly enters match utility and students with higher HC benefit more from admittance to high quality universities. We show that the extended model still admits the qualitative patterns predicted by the version described here, which is simpler and isolates phenomena that we can directly test with our experimental design.⁷

3.1.1. Incorporating Demographics. Each student observably belongs to one of two mutually exclusive demographic subgroups, \mathcal{A} and \mathcal{D} , with $\delta \in (0, 1)$ being the mass of the latter. Since costs are privately known to each individual, students view their competitors' types in group $j = \mathcal{A}, \mathcal{D}$ as realizations of a random variable Θ_j with distribution $F_j(\theta)$ and density $f_j(\theta)$ which is strictly positive on a common support $[\underline{\theta}, \bar{\theta}]$. As convenient shorthand, we define the overall distribution and density as $F(\theta) \equiv \delta F_{\mathcal{D}}(\theta) + (1 - \delta)F_{\mathcal{A}}(\theta)$ and $f(\theta) \equiv \delta f_{\mathcal{D}}(\theta) + (1 - \delta)f_{\mathcal{A}}(\theta)$.

⁶We restrict attention to the case where cost type $\bar{\theta}$ chooses to acquire \underline{h} units of human capital. In other words, we assume $U(\underline{s}, \underline{h}; \bar{\theta}) = U(\emptyset, 0; \bar{\theta})$, which guarantees that all students in our game weakly prefer to acquire the minimum HC and attend university. Those with $h_i \geq \underline{h}$ are allocated a seat based upon their relative HC. This means that the model is one of decisions on the intensive margin, being conditional on market participation. The question of how AA may affect university attendance decisions on the extensive margin is left for future research.

⁷Since it is difficult to directly experimentally test how h factors into gross utility, we instead focus on a simpler model in this section of the paper. We study HC accumulation in our experiment through direct observations of time investment into the learning process. For illustrative purposes though, in the appendix we present some examples where gross utility is Cobb-Douglas in s and h , $U(s_i, h_i; \theta_i) = s_i^\alpha h_i^\beta - \theta_i h_i$. Similar qualitative patterns emerge in response to AA, relative to color-blind admissions, as when $u = s_i$.

We assume group \mathcal{A} is “advantaged” and group \mathcal{D} is “disadvantaged” in that students from \mathcal{D} tend to find HC production more costly, on average. Formally, we assume the distributions of $\Theta_{\mathcal{A}}$ and $\Theta_{\mathcal{D}}$ are ordered by *likelihood ratio (LR) dominance*, where

$$\frac{\partial (f_{\mathcal{D}}(\theta)/f_{\mathcal{A}}(\theta))}{\partial \theta} > 0 \quad \text{for all } \theta \in [\underline{\theta}, \bar{\theta}].$$

Intuitively, LR dominance is a strong form of first-order stochastic dominance which implies, among other things, that cost quantiles within \mathcal{A} are all lower than the corresponding quantiles within \mathcal{D} . Although some “disadvantaged” students have low costs of acquiring HC, and some “advantaged” students have high costs, the average and median students in \mathcal{D} have higher costs of producing new HC than their counterparts in \mathcal{A} , *etc.* This is how we operationalize the idea that, on average, disadvantaged students must invest more time and effort to overcome obstacles which are (imperfectly) correlated with their demographic status. In the context of race, for example, it is well-known that Black and Hispanic children in the United States tend to be less affluent and have less access to crucial childhood inputs like healthcare and high-quality public education; however, some still grow up in affluent environments which are more advantageous to childhood learning. Note also that LR dominance implies there exists a unique point, $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$, at which the densities $f_{\mathcal{A}}$ and $f_{\mathcal{D}}$ cross. This fact will become useful later when we explore comparative statics under different allocation mechanisms.

3.1.2. Seat Allocation Rules. We consider two distinct university seat allocation rules. Let $H \sim G(H)$ denote the human capital output of a randomly selected student, and let $H_j \sim G_j(H_j)$ be similarly defined for a student from group $j = \mathcal{A}, \mathcal{D}$. The first seat allocation rule is a **color-blind (CB)** policy, which ignores demographics when allocating university seats. This rule assigns students to seats in pure rank-order fashion. The mechanism determines the quantile rank of h_i within the overall HC distribution and then matches student i to a seat at the corresponding quantile rank. For example, the 75th percentile student matches with the 75th percentile university, *etc.* Formally, under the color-blind rule, student i from group j receives seat assignment

$$S_j^{cb}(h_i) = S^{cb}(h_i) \equiv G(h_i), \quad j = \mathcal{A}, \mathcal{D}. \quad (2)$$

A student’s allocation under the CB rule depends on her performance relative to *all* other students; thus, investment behavior will not vary by demographic status.

The second seat allocation rule is a **representative quota (RQ)** AA policy, which reserves a similar distribution of university seats for each group, \mathcal{A} and \mathcal{D} , and then allocates the seats *within* each group by rank ordering. By “similar distribution” we mean that fraction δ of all seats at each point in the quality spectrum are earmarked *ex*

ante for group \mathcal{D} , thus splitting the set of university seats into two subsets having mass δ and $(1 - \delta)$, but with both subsets still following the original quality distribution F_S .⁸ Formally, under the *RQ* rule, student i receives seat

$$S_j^{rq}(h_i) \equiv G_j(h_i), \quad j = \mathcal{A}, \mathcal{D}. \quad (3)$$

A student's seat assignment under the *RQ* policy depends only on her performance relative to other members of her own demographic group. It splits the university admissions market into two separate contests, with identical distributions of prizes, but different distributions of player ability. From a student's perspective, the distinguishing characteristic of the *RQ* mechanism is that it alters the distribution of one's competitors, while leaving all other aspects of the contest the same as under a *CB* rule.

3.1.3. *Solution Concept:* The strategic environment presented above will form the basis for our experimental design in the next section. Bodoh-Creed and Hickman [2015] develop a general model of competitive HC investment which nests the framework presented here as a special case. They begin with a finite set of players and university seats, and demonstrate existence of a unique symmetric Bayes-Nash equilibrium. They then establish general conditions under which equilibria of the unweildy finite model can be well approximated by the solution to a decision problem where the finite sets on both sides of the matching market are replaced with continua.⁹ This solution they refer to as an *approximate equilibrium*, and it lends analytic tractability to an otherwise complicated model. For the understanding we wish to develop in this paper, it suffices to limit discussion to the continuum representation of the model.

Our main goal in this section is to illustrate qualitative model predictions which are testable through experimental methods. The primary contribution of this paper derives from empirical comparisons of the distributions of effort and outcomes generated under the alternative policies *CB* and *RQ*. Thus, under each of these mechanisms we now characterize the approximate Bayes-Nash equilibrium of the game played between students who each choose how much HC to acquire prior to execution of the school assignment mechanism. To avoid tedious repetition, we abstract from the intricacies of the finite

⁸A subtle but crucial detail to note here is that our *RQ* policy calibrates δ to the *fraction of group \mathcal{D} market participants*, which may not be the same as the mass of that group within the population at large. For example, South Africa mandates employment quotas for skilled professions, where quotas are pegged to the fraction of blacks in the overall population. One problem in the implementation of this law has been that not enough blacks exist within the skilled labor market (*e.g.*, individuals with pre-requisite post-secondary degrees) to fill mandated quotas. Such a rule would be a more extreme version of the AA policy studied in this paper, and may therefore entail different consequences.

⁹Alternatively, one may also interpret the continuum simplification as the basis of a behavioral strategy employed by a cognitively constrained agent. Rather than tracking probabilities over all the order statistics of large, complicated sets of competitors and seats, the agent may drastically reduce computational burden at little cost by pretending as if she was operating within a continuous world instead of a discrete one.

game and simply refer to our derivation of the approximate equilibrium as “the equilibrium.” The interested reader is directed to Bodoh-Creed and Hickman [2015] for a full exposition of the technical foundations underlying the continuum model approximation.

3.2. ANALYSIS. Let $h^*(\theta)$ denote the common equilibrium investment function under the benchmark *CB* mechanism, and let $h_{\mathcal{A}}^*(\theta)$ and $h_{\mathcal{D}}^*(\theta)$ denote the group-specific investment functions under the alternative *RQ* mechanism. Moreover, let θ^* , $\theta_{\mathcal{A}}^*$, and $\theta_{\mathcal{D}}^*$ denote the relevant inverses, so that $\theta^* \equiv h^{*-1}$ and $\theta_j^* \equiv h_j^{*-1}$, $j = \mathcal{A}, \mathcal{D}$.

3.2.1. CB Allocations. Student i chooses HC to maximize her net payoff, given that other students play according to h^* . Equilibrium HC investment is strictly decreasing in θ , so equation (2) can be re-written as $S^{cb}(h) = 1 - F[\theta^*(h)]$, and student i 's objective as

$$\max_{h_i \geq \underline{h}} \{(1 - F[\theta^*(h_i)]) - c(h_i; \theta_i)\}. \quad (4)$$

Taking a first-order condition (FOC), we get $-f(\theta_i)\theta^{*'}(h_i) = c'(h_i; \theta_i)$. In equilibrium, $\theta^*(h_i) = \theta_i$ for all i , and since h^* is the inverse of θ^* , it follows that $h^{*'}(\theta_i) = 1/\theta^{*'}(h_i)$. Therefore, through a change of variables we can rearrange the FOC to get

$$h^{*'}(\theta_i) = \frac{-f(\theta_i)}{c'[h^*(\theta_i); \theta_i]}, \text{ with boundary condition } h^*(\bar{\theta}) = \underline{h}. \quad (5)$$

Given the assumptions on f and c , it is easy to see that h^* is strictly decreasing in θ . Equation (5) allows us to compare investment under two alternative cost distributions.

Theorem 1. *Consider two color blind contests, 1 and 2, which differ only by their cost distributions, and assume competition is more fierce under contest 2 in the sense that F_1 LR dominates F_2 . Let $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$ denote the unique crossing point of the density functions where $f_1(\tilde{\theta}) = f_2(\tilde{\theta})$. There exists a unique interior crossing point $\check{\theta} \in (\underline{\theta}, \tilde{\theta})$, such that $h_1^*(\theta) < h_2^*(\theta)$ for all $\theta < \check{\theta}$ and $h_1^*(\theta) > h_2^*(\theta)$ for all $\theta > \check{\theta}$.*

A formal proof is left to the Appendix. The theorem provides useful insight into how competition shapes incentives. It says that, holding fixed the set of all university seats, an increase in the degree of competition will cause the most able students to invest more aggressively, increasing their HC output, and less talented students to withdraw somewhat, decreasing their HC output. This second shift is a common feature of contests known as the *discouragement effect*. The intuitive idea is that since investment costs must be sunk before assignments are made to a set of fixed outcomes, then, holding one's own cost type θ fixed, if the distribution of competitors shifts so that one's quantile rank falls low enough, investment incentives fall. As we will see below, there is much insight to be had from Theorem 1 when comparing alternative allocation mechanisms.

3.2.2. *RQ Allocations.* Similarly as before, under a *RQ* equation (3) can be re-written as $S_j^{rq}(h) = 1 - F_j \left[\theta_j^*(h) \right]$, $j = \mathcal{A}, \mathcal{D}$, and the objective for student i from group j is now

$$\max_{h_i \geq \underline{h}} \left\{ \left(1 - F_j \left[\theta_j^*(h_i) \right] \right) - c(h_i; \theta_i) \right\}. \quad (6)$$

Note the main difference here is that the group-specific distribution enters decision making rather than the unconditional one. That leads to the following FOC

$$h_j^{*'}(\theta_i) = \frac{-f_j(\theta_i)}{c' \left[h_j^*(\theta_i); \theta_i \right]}, \text{ with boundary condition } h_j^*(\bar{\theta}) = \underline{h}, \quad j = \mathcal{D}, \mathcal{A}. \quad (7)$$

Recall our assumption that the random variable $\Theta_{\mathcal{D}}$ LR dominates $\Theta_{\mathcal{A}}$, or in other words, the ratio $f_{\mathcal{D}}(\theta)/f_{\mathcal{A}}(\theta)$ is strictly increasing in θ . This implies that Θ LR dominates $\Theta_{\mathcal{A}}$, and that $\Theta_{\mathcal{D}}$ LR dominates Θ as well. To see why, note that

$$\frac{f(\theta)}{f_{\mathcal{A}}(\theta)} = \frac{\delta f_{\mathcal{D}}(\theta) + (1 - \delta)f_{\mathcal{A}}(\theta)}{f_{\mathcal{A}}(\theta)} = \delta \frac{f_{\mathcal{D}}(\theta)}{f_{\mathcal{A}}(\theta)} + (1 - \delta),$$

from which it follows that $f(\theta)/f_{\mathcal{A}}(\theta)$ is strictly increasing in θ . Likewise,

$$\left(\frac{f_{\mathcal{D}}(\theta)}{f(\theta)} \right)^{-1} = \frac{\delta f_{\mathcal{D}}(\theta) + (1 - \delta)f_{\mathcal{A}}(\theta)}{f_{\mathcal{D}}(\theta)} = \delta + (1 - \delta) \frac{f_{\mathcal{A}}(\theta)}{f_{\mathcal{D}}(\theta)},$$

so $f_{\mathcal{D}}(\theta)/f(\theta)$ is strictly increasing in θ as well. Therefore, switching between allocation mechanism *CB*—where the competition group is all students—and mechanism *RQ*—where competition occurs only within one's own group—entails an effective LR dominance shift in the distribution of competitors while holding the distribution of seats fixed. Thus, Theorem 1 directly leads to the following corollary on HC accumulation under an *RQ* rule, relative to *CB*.

Corollary 2. *Assume $F_{\mathcal{D}}$ LR dominates $F_{\mathcal{A}}$ and let $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$ denote the unique crossing point of the cost densities where $f(\tilde{\theta}) = f_{\mathcal{A}}(\tilde{\theta}) = f_{\mathcal{D}}(\tilde{\theta})$. Then there exist crossing points $\check{\theta}_{\mathcal{A}}, \check{\theta}_{\mathcal{D}} \in (\underline{\theta}, \tilde{\theta})$, such that*

- (i) $h_{\mathcal{D}}^*(\theta) < h^*(\theta)$ for all $\theta < \check{\theta}_{\mathcal{D}}$ and $h_{\mathcal{D}}^*(\theta) > h^*(\theta)$ for all $\theta > \check{\theta}_{\mathcal{D}}$, and
- (ii) $h_{\mathcal{A}}^*(\theta) > h^*(\theta)$ for all $\theta < \check{\theta}_{\mathcal{A}}$ and $h_{\mathcal{A}}^*(\theta) < h^*(\theta)$ for all $\theta > \check{\theta}_{\mathcal{A}}$.

In words, the best and brightest students within group \mathcal{D} decrease HC investment, as competition for the top university seats becomes less intense. At the same time, higher-cost individuals exert greater effort and increase HC accumulation, as *RQ* mitigates discouragement effects by placing them in a competition group where they are less far behind the curve. For the same reasoning, the opposite effects apply to group \mathcal{A} . The best and brightest increase HC investment under the *RQ*, as competition for the

top university seats becomes more intense, and others become discouraged as they find themselves further behind the curve in their new competition group.

Since theory predicts behavioral responses in opposite directions for students of different abilities within each group, the result above begs the question of which effect will dominate. Once again, Theorem 1 can shed some light on the answer. Using the density crossing point $\tilde{\theta}$, we can partition the cost support into two subsets,

$$T_{\mathcal{A}} \equiv (\underline{\theta}, \tilde{\theta}) \quad \text{and} \quad T_{\mathcal{D}} \equiv (\tilde{\theta}, \bar{\theta}),$$

which we refer to as the *typical cost sets* for each group. Intuitively, T_j is the region of the support where group j 's density is strictly higher, and therefore it is overrepresented relative to its share in the overall population. LR dominance implies some interesting properties for these sets. Since both densities integrate to 1, it follows that

$$\int_{T_{\mathcal{A}}} [f_{\mathcal{A}}(\theta) - f_{\mathcal{D}}(\theta)] = \int_{T_{\mathcal{D}}} [f_{\mathcal{D}}(\theta) - f_{\mathcal{A}}(\theta)].$$

In words, the degree of over-representation of group \mathcal{D} in the high-cost set $T_{\mathcal{D}}$ is the same as the degree of over-representation of group \mathcal{A} within the low-cost set $T_{\mathcal{A}}$.

Corollary 3. *Assume the same conditions as in Corollary 2. Then under the RQ policy (relative to the CB policy), typical disadvantaged students—that is, group \mathcal{D} students with costs $\theta \in \{(\tilde{\theta}_{\mathcal{D}}, \tilde{\theta}] \cup T_{\mathcal{D}}\}$ —exert higher effort and accumulate more HC. Moreover, if we define $d : (\tilde{\theta}_{\mathcal{D}}, \tilde{\theta}] \cup T_{\mathcal{D}} \rightarrow \mathbb{R}$ as the difference on this set between group \mathcal{D} investment under RQ versus CB, or*

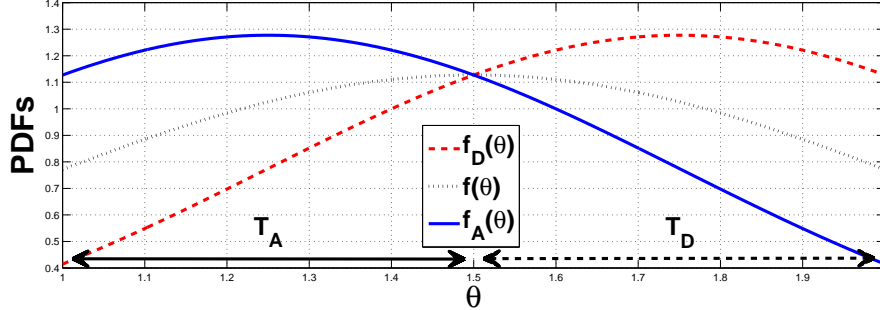
$$d(\theta) \equiv (h_{\mathcal{D}}^*(\theta) - h^*(\theta)),$$

then $d(\theta)$ is strictly positive and attains a maximum on the interval $(\tilde{\theta}_{\mathcal{D}}, \tilde{\theta}]$. Moreover, if investment costs are strictly convex in h , then $d(\theta)$ attains its maximum on the open interval $(\tilde{\theta}_{\mathcal{D}}, \tilde{\theta})$.

Proof: The reasoning behind this result is simple. Corollary 2 directly implies the first part and that d tends toward zero at its endpoints but is strictly positive everywhere else. Moreover, equation (A.2) in the proof of Theorem 1 (see appendix) establishes that $d'(\theta) < 0$ for all $\theta \in T_{\mathcal{D}}$, meaning the difference between the two investment functions becomes steadily wider as one moves toward the density crossing $\tilde{\theta}$ from the left. If costs are strictly convex, then (A.2) shows that $d'(\theta) < 0$ for all $\theta \in T_{\mathcal{D}} \cup \tilde{\theta}$, so the maximum cannot occur at $\tilde{\theta}$. ■

Corollary 3 implies that, relative to their disadvantage in the overall population, a disproportionate share of group \mathcal{D} students increase HC investment. To gain an appreciation for the strength of this result, the corollary also shows that, not only do we see a positive effect for all group \mathcal{D} cost types in their typical set, but the improved incentives

FIGURE 1. NUMERICAL EXAMPLE:
Truncated Normal Cost Densities



extend well beyond T_D as well. In fact, the largest improvement of investment incentives by type (*i.e.*, where d attains its maximum) actually occurs on the interval $(\bar{\theta}_D, \bar{\theta}]$. Thus, the result implies an increase of investment activity for a large fraction of the disadvantaged group. It is worth noting that analogous results to the three above can be proven in an extension of the model where h directly enters a student’s gross match utility; these are Theorem 4 and Corollaries 5 and 6 in the Appendix.

On the other hand, the situation is less clear for the advantaged group. Although an analogous statement can be made—that all group \mathcal{A} cost types $\theta \in \{(\bar{\theta}_A, \bar{\theta}] \cup T_D\}$ will reduce HC output under RQ —the statement is less informative, because investment also increases on part of the typical set T_A as well. To further illustrate the intuition of model predictions, we present three numerical examples, one below and two additional ones in the Appendix.

3.3. NUMERICAL EXAMPLE. Here, we explicitly solve the model using functional forms to illustrate the qualitative patterns predicted by theory. In the example, $\delta = 0.5$ and both Θ_D and Θ_A follow normal distributions truncated to a common support $[\underline{\theta}, \bar{\theta}] = [1, 2]$ with variance parameter $\sigma_A = \sigma_D = 0.5$. The mean parameters differ, with $\mu_D = 1.75$ and $\mu_A = 1.25$, which ensures that the distributions are ordered by LR dominance (see Figure 1). We specify costs as a linear function $c(h; \theta) = \theta h$, so that the maximum distance between investment under RQ and CB occurs at the boundary between T_A and T_D . Finally, we set $\underline{h} = 0.5$; this makes type $\bar{\theta}$ indifferent to market participation when the outside option is normalized to a value of zero.

Figures 2 and 3 depict a comparison of the RQ investment functions for each group (solid and dashed lines), as well as the common investment function for both groups under a CB allocation rule (dotted line). For each group there is crossing point of the investment functions, with the upper bounds of the HC distributions being different by seat allocation rule. For the disadvantaged group, a positive mass of the top students

FIGURE 2. NUMERICAL EXAMPLE:
Investment, CB vs AA

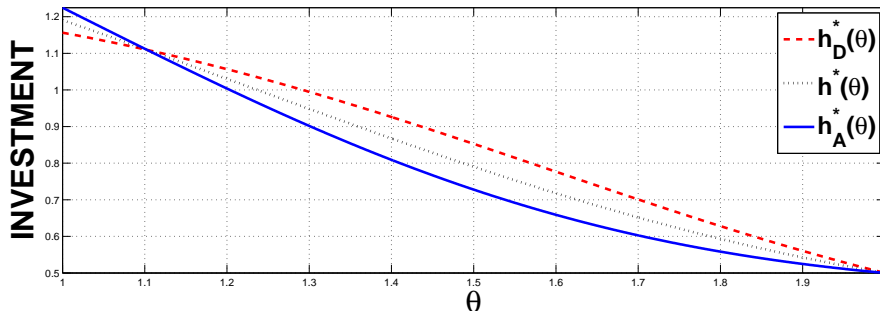
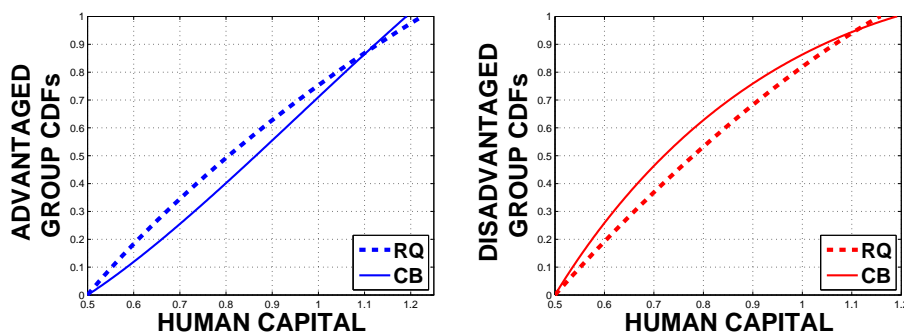


FIGURE 3. NUMERICAL EXAMPLE:
HC Distributions, CB vs AA



reduce investment, while middle- and high-cost students increase it. Intuitively, the policy aids the top students from \mathcal{D} , but since they were already placing close to the upper bound their outcomes cannot be commensurately improved and they rationally reduce effort. For other students in \mathcal{D} , the policy alleviates discouragement effects by placing them in a competition group where their own type is not as far behind, making them more competitive for higher quality outcomes. In turn, they respond with an increased willingness to engage in costly investment. Similar logic holds for group \mathcal{A} , but in reverse. A key aspect of our experiment will be to engineer this sort of counterfactual scenario, where similar ability students find themselves facing different levels of competition for the same set of outcomes. Within this experimental counterfactual we test whether AA leads to changes in performance distributions. We also investigate intermediate inputs behind investment costs depicted in the model: time and effort.

4. EXPERIMENTAL DESIGN

We built our incentives and learning exercise around the American Mathematics Competition 8 (AMC8) exam, sponsored by the Mathematical Association of America, for

students in 8th grade and below. It consists of 25 multiple choice questions (five choices each) in 40 minutes, and the questions become progressively more difficult from start to finish. The AMC8 is an early precursor to math competitions such as the International Mathematical Olympiad and the Putnam. The AMC8 website explains, “[it] provides an opportunity to apply concepts [to] high level to problems which... are designed to challenge and offer problem solving experiences beyond those provided in... junior high school.”

4.1. SAMPLE POPULATION. Our total sample includes 992 middle school and junior high students from 10 schools in Utah County, Utah, including both charter schools and regular public schools. Within these schools, participation in our study was at the classroom level. Academically and socioeconomically, our sample population was fairly average within the US.¹⁰ Most of our partner schools had previously participated in the AMC8 before partnering with us for this study.

Our partner schools exhibited a high degree of racial and cultural homogeneity. Less than 1% of students were black and only 7.5% were Hispanic, compared to nationwide averages of 15.25% and 22.2% for blacks and Hispanics, respectively. All schools in this study serve suburban populations. However, given that the goal of this paper is to cleanly test theory of incentives, the demographic homogeneity in our sample may be an advantage. We chose grade level as our demographic delimiter for groups \mathcal{A} and \mathcal{D} , which ensures that they only differ in observable ways (*i.e.*, age and grade) but are otherwise similar. Focusing on a relatively homogeneous subject pool allows us to largely rule out other cultural/behavioral phenomena (such as stereotype threat) which might confound the pure incentive effects arising from our exogenous policy variation. At the same time, however, we must recognize that AA policies typically apply in real world settings with a number of confounding factors which we minimize in our experiment. We discuss this point further in Section 6 where we compare achievement gaps within our sample pool to race gaps within the US population.

¹⁰Our test subjects are drawn from a population which is somewhat more affluent than the rest of the country, but academically comparable. Based on figures from the National Center for Education Statistics, we estimate a median household income of \$59,800 for the sample population, compared to a nationwide median of \$53,046 (information downloaded from <http://nces.ed.gov/surveys/sdds/framework/tables.aspx>). In 2012, approximately 33% of test subjects were eligible for free or reduced-price lunch, compared to a national average of 48%. That year our partner schools housing 5th and 6th grade subjects (20% of our sample pool) performed significantly better than other Utah schools in terms of meeting state math standards (approximately 91% vs 76%), while schools housing our 7th and 8th graders (80% of our sample pool) performed slightly worse than other Utah schools (81% vs 83% meeting state standards). Utah is a state which typically ranks at or near the median for nationally measured academic outcomes such as NAEP scores and enrollment rates in Advanced Placement programs.

4.2. TREATMENT GROUPS AND INCENTIVES. Participants in our study first took a practice AMC8 test from a previous year. We used this as a baseline measure of each student’s ability. Individuals were randomized into either a control group—with a “color-blind” competition—or an AA treatment—with a representative quota competition. Following the notational convention from the previous section, we will henceforth refer to these treatments as CB and RQ , respectively. For treatment CB , we ran competitions involving students in two adjacent grades; that is, 7th and 8th graders competing together, and 5th and 6th graders competing together. For each of the two age cohort pairings, students in the lower grades (5th or 7th) are henceforth referred to as the “disadvantaged” demographic \mathcal{D} , and students in the higher grades (6th or 8th) are referred to as the “advantaged” demographic group \mathcal{A} , since the latter are one year older and have received one more year of mathematics education on average.¹¹ For treatment Q , students competed only within their own grade level, but for a proportionally equivalent set of prizes (relative to CB), as described below. We ran separate competitions for 5th, 6th, 7th, and 8th graders.

The top 30 percent of students within each competition group received cash prizes, which were uniformly distributed between \$4 and \$34 in \$2 increments. Prizes were awarded assortatively within competition groups, according to final exam scores. For example, 7th grade students in treatment CB needed to score within the top 30 percent of all 7th and 8th grade students in their treatment to receive a prize. In treatment Q , students competed against others in their own grade only, but for a representative set of prizes. More specifically, we began with the same aggregate prize distribution as for treatment CB , and then earmarked prizes at each different level in proportion to the mass of lower-grade students in each age cohort pairing (note that the mass of groups \mathcal{A} and \mathcal{D} were also identical across treatments CB and Q). This ensured that the moments of the prize distribution (including the 70% mass of zeros) were the same across all competition groups, with each one vying for the same number and variety of prizes on a per capita basis. For example, 7th grade students in treatment Q only had to score within the top 30 percent of 7th graders in their treatment to receive a prize. Moreover, their distribution of prizes, conditional on winning something, was the same as for 8th graders in treatment Q , and also the same as for all 7th/8th grade students in treatment CB . Thus, for an advantaged or disadvantaged student of a given ability level, the only difference across the two treatments is the distribution of one’s competitors.

¹¹As we discuss in Section 6, the difference in average preparation between our disadvantaged and advantaged groups likely understates difference between black and white students at the same age in the US. Using national figures from the NAEP exam, we estimate that the 7th–8th grade gap in our sample pool is roughly 60% of the gap between black and white 8th graders in terms of math proficiency.

Each student received an information sheet describing their assigned group, how many students from which grade(s) they would be competing against, and the score distribution within their group based on the practice test. Students received their own practice score back at the same time so they could see where they fit within their competition group. The sheet also contained a table describing the prize structure. We printed information relative to each competition group on a different color of paper so that students could visually see in their classroom that roughly half of the students were assigned to each treatment. Altogether, there were six different groups: four groups for the quota treatment (one for each grade) and two groups for the neutral treatment (one for 5th/6th grade and one for 7th/8th grade). In a web appendix, we provide an example of the information sheet given to each group.¹²

4.3. MATH LEARNING WEBSITE. At the bottom of the information sheet was the url of a website we set up with practice problems drawn from five past AMC8 exams. At 25 questions each, this made for 125 total practice problems covering six different math subjects: Arithmetic, Algebra, Combinatorics, Geometry, Logic, and Probability. Problems were divided into a set of 31 total quizzes. Each year, the 25 AMC8 exam questions are numbered in increasing order of difficulty. For each of the previous five year's exams, the website included one quiz covering problems 1-10, a second quiz covering problems 11-20, and a third covering problems 21-25. Test subjects were notified that each grouping of 3 same-year quizzes were ordered by their difficulty level. We also arranged this same battery of math problems into an additional set of 16 quizzes, each containing 5 subject-specific math problems. These subject quizzes were also ordered by their difficulty level.

Students could attempt each quiz as many times as they liked, or move on to additional materials they had not yet tried. After completing each quiz, our software displayed an instructional page which reported to each student her score, the correct answers for each problem, and step-by-step solutions published by the developers of the AMC8. Students were provided with a web page that contained links for all of the quizzes we offered, but in order to access the quizzes, they had to input their name, grade, and school on the first page of the web form. This allowed us to track online activities for each quiz session, including which students visited the website, how many different subjects they tried, how much time they spent, how many questions they attempted, what they answered on each attempt at each question, and how much time they spent viewing the instructional page.

¹²Copies of the information sheets given to test subjects are available for download at http://home.uchicago.edu/~hickmanbr/uploads/CHP2014_WEB_APPENDIX.zip

Within each quiz, questions were separated on different web pages in blocks of 3, 4, or 5 questions per page, and the instructional page at the end displayed feedback for all questions on a single page.¹³ Time on our website was measured at the page level, meaning that we got a time measure for blocks of either 3, 4, or 5 questions. In order to convert this information into a time spent per question measure, we divided each block-level time observation by the number of questions within that block. Instructional page times for 10-question quizzes were split into two observations a piece by dividing by two in order to make them comparable to 5-question instructional page view times.

4.3.1. *Time Measurement.* One difficulty arose in that there were clear instances where students left the website in the middle of a quiz for several hours or more. To adjust for this problem we chose truncation points on the domains of time per question and instructional page view time, and we replaced each observation above that point with the appropriate student-specific censored mean.¹⁴ In selecting our truncation point we looked for occurrences of “holes” in the support of the distribution of times per question.¹⁵ For our time per question data, this leads to a truncation point of 26.14 min/question (the 99.35th percentile), and for instructional page views, 108.39 min/page view (*i.e.*, 21.68 min/solution, or the 98th percentile). In the Appendix we display a histogram of (uncensored) time per question and instructional page view times.

At the end of the day, the time monitoring capability on our website is not perfect, and it is impossible to directly observe work stoppages in the middle of a quiz question. In particular, it may still be the case that smaller work stoppages occur below the truncation points. Therefore, in terms of time per question we are effectively interpreting work stoppages of less than 27 minutes as time which comes at a positive cost to the child. We argue that 27 minutes is a reasonable truncation point for several reasons. First, work stoppages for our uncensored time observations (most of which were less than 10 minutes) would serve as a poor substitute for longer, unbroken leisure spells. Second,

¹³Each 10-question quiz was broken into three pages with 3 questions on page one, 3 questions on page two and 4 questions on page three. Each 5-question quiz displayed all 5 problems on a single page.

¹⁴To illustrate this rule, suppose that Tommy attempted three 5-question quizzes for a total of 15 questions. Suppose further that we observed times of 5 minutes each for seven questions, 15 minutes each for another seven questions, and 2000 minutes for the last one. Then if the truncation point were, say 30 minutes, the last observation of 2000 is replaced by Tommy’s idiosyncratic censored mean time of 10 minutes (for all other questions he attempted). As a robustness check, we also ran our analysis by simply dropping truncated observations instead, and results are very similar to those we present below.

¹⁵More specifically, a hole in the distribution support was defined as the minimum point at which a full-support condition fails, which we estimated as a point where a kernel-smoothed density estimate hit zero. The idea behind this rule is that if the type distribution has full support, then the distribution of times per question should have full support as well since the choice of how much time to spend is continuous. For a more complete description of our truncation point selection rule, see the Appendix.

since this potential problem is the same across both treatment groups, there is no reason to believe that our results are being aided by it.

Third, the AMC8 contains fairly challenging material that may require significant time inputs for some students. Table 2 in the next section displays the mean and variance of time spent per question attempt, using the censored sample of times. The most difficult subject appears to be combinatorics, with a mean time of 2.839 minutes and a standard deviation of 3.532. Given that the censored distribution of time per question is right skewed, and 10 minutes (the 98th percentile of the un-censored sample) is roughly two standard deviations above the mean for combinatorics, it is plausible that roughly 1.5% of our sample could exist on the interval between 10 minutes and 26 minutes.

As for instructional page view times, it is informative to consider a particular student whom we will rename “Kate” to protect her identity. Kate, a 7th grader in the quota treatment, spent more time than anyone else on the website (after time adjustments), averaging roughly 53 minutes per day during the study period. At 55 question attempts, Kate was also above the 94th percentile on that dimension as well. She attempted 11 quizzes of 5 questions each, averaging 2.24 minutes per question attempt and 7.49 minutes per question on the instructional page with solutions. She spent an hour or more on 5 of her quiz attempts, each time spending the majority of her time on the instructional page. For each of Kate’s 11 quiz attempts we see that she clicked through to the quiz termination screen herself. Kate is an example of a student who displayed consistent patterns of substantial time inputs into many of the quizzes she took—particularly on the instructional page—while never having left a quiz session open overnight.

4.4. TESTING. Students took the actual AMC8 test in their regular classrooms, under all of the normal conditions in which students around the country take the AMC8. Most of the students in our study attended schools where participation in the AMC8 was already being offered to students by their teachers, but on a voluntary basis. The schools that cooperated in this study administered the test to all students within each participating classroom on an opt-out basis, so that all students participated in the study, except those whose parents proactively signed and returned an opt-out form. The study involved two in-class exam sessions: the practice test was the AMC8 exam for the previous year, and the final exam was the AMC8 for the current year. The cash prizes were delivered to each school shortly after the final exam, and handed out to each student in an envelope. The outcome measures that we use in the next section include both the effort-based measures with website data, as well as a performance-based measure using students’ scores on the AMC8.

5. EXPERIMENTAL RESULTS

The theoretical model predicts that some students in each demographic group will increase their effort and HC accumulation under AA, while others in the same group will decrease their efforts. Our empirical investigation takes the next step by testing for the average effects of AA on effort and performance by demographic group and estimating magnitudes. We also use a non-parametric analysis to consider the effects of AA on the performance distributions and achievement gaps.

5.1. DESCRIPTIVE STATISTICS.

5.1.1. *Pre- and Post-Exams.* Table 1 contains descriptive statistics on AMC8 exam scores. Roughly three quarters of our sample were 7th/8th graders. The difference between these and our 5th/6th grade test subjects is that the latter all came from accelerated classes, whereas the former are representative of the overall student body within their schools. This difference is born out in the data: while 8th grade students did best on the pre-test with an average score of 9.04, 6th graders as a group came in second at 8.12 on average. 7th and 5th grade average pre-test scores are close, at 7.58 and 7.19, respectively.¹⁶

We have also broken down test scores by two groups that we refer to as *investors*—students who logged on to our website at least once during the study period—and *non-investors*—those who did not. Students who did better on the pre-test were more likely to be investors (*i.e.*, they were more likely to use the math practice website), although some students who did not do as well also chose to be investors, and many students who did quite well on the pre-test chose to be non-investors. For the group of investors, we also present summary statistics concerning their activities on the website. Investors' times ranged between a few minutes and 8.92 hours, or an average of about 53 minutes per day over the study period. Number of questions attempted ranged between 1 and 120, with mean and standard deviation of roughly 19 and 23, respectively. *Subjects* represents the number of different subject categories a student attempted, using the subject-specific quizzes, being about two on average.

5.1.2. *Subject-Specific Practice Question times.* Table 2 displays the mean and variance of time spent per question attempt on our website, using the censored sample of non-truncated times (once again, for investors only). Some math subjects appeared more challenging in terms of the time students took to solve problems. The most difficult subject was combinatorics, with a mean time per question attempt of 2.839 minutes and

¹⁶The national AMC8 population in 2013 (see <https://amc-reg.maa.org/reports/generalreports.aspx>) had mean and median of 10.69 and 10 out of 25, with standard deviation of 4.44. These figures illustrate the difficulty of the exam. The AMC8 is predominantly administered through opt-in participation, whereas our experiment was on an opt-out basis. This accounts for the lower mean among our sample pool.

TABLE 1. STUDENT DESCRIPTIVE STATISTICS

	Mean	Median	Std. Dev.	N
Pre-Exam Scores				
All	8.45	8	2.90	992
5 th Grade	7.19	7	2.39	48
6 th Grade	8.12	8	2.47	155
7 th Grade	7.58	7	2.84	275
8 th Grade	9.04	9	2.82	396
Investors	9.46	10	3.19	118
Non-Investors	8.32	8	2.83	874
Final Exam Scores				
All	8.64	8	2.88	895
5 th Grade	7.40	7	2.22	42
6 th Grade	9.17	9	2.82	133
7 th Grade	8.12	8	2.90	233
8 th Grade	8.75	9	2.80	374
Investors	9.20	9	3.06	113
Non-Investors	8.56	8	2.84	782
Human Capital Investment (Investors Only)				
Total Time	43.65	26.85	64.65	118
Problem Solving Time	32.99	19.31	41.43	118
Instructional Time	10.66	3.37	38.85	118
Questions	18.89	10.00	22.53	118
Subjects	1.94	1.00	1.43	118

Notes: All time figures are post-censoring as described in Section 4.3 and quoted in minute units. *Investors* are defined as students who logged on to the math learning website at least once during the investment period. *Non-Investors* are those who did not.

TABLE 2. Time Per Question Attempt by Subject

Subject	Censored Mean Minutes Per Question	Std. Dev. (minutes)	Mean + 2×Std. Dev.
Algebra	2.422	2.563	7.548
Arithmetic	1.398	1.238	3.874
Combinatorics	2.839	3.532	9.903
Geometry	2.183	2.577	7.337
Logic	1.807	1.742	5.291
Probability	1.996	1.137	4.27

TABLE 3. EFFORT AND PERFORMANCE BY TREATMENT

	Investment				Performance
	Used Website	# Subjects Attempted	Total Time	# Questions Attempted	Final Exam Score
Quota Std. Err.	0.154 (0.015)	0.284 (0.037)	6.634 (1.216)	2.729 (0.456)	8.680 (0.139)
Neutral Std. Err.	0.088 (0.014)	0.189 (0.035)	3.932 (1.149)	1.817 (0.431)	8.604 (0.133)
<i>N</i>	992	992	992	992	895

Notes: Each cell provides the mean of the measure listed in each column. Standard errors are provided in brackets. Estimates under each of the four effort variables are intended to capture the effect of a treatment on human capital investment for the total study population, and are therefore averaged over both investors and non-investors.

a standard deviation of 3.532. The least difficult subject appeared to be arithmetic, with mean and standard deviation of 1.398 and 1.238, respectively.

5.2. EMPIRICAL ANALYSIS.

5.2.1. *Testing Overall Differences by Treatment.* Tables 3 and 4 investigate the effect of a quota on the overall population, including both advantaged *and* disadvantaged groups. The first column of Table 3 displays the mean of a binary variable, being the fraction of investors from each treatment group (*i.e.*, subjects who logged on to our website at least once to practice math). As for the other investment variables, the reader should keep in mind that Tables 3 – 5 aim to measure a treatment effect of a policy on an entire group, including both the intensive and extensive margins of investment. This is why the effort

TABLE 4. TESTING DIFFERENCES BY TREATMENT

	Investment			Performance	
	Used Website	# Subjects Attempted	Total Time	# Questions Attempted	Final Exam Score
<i>Quota – Neutral</i>	0.066***	0.095*	2.701	0.912	0.076
<i>P-Value:</i>	<i>[0.001]</i>	<i>[0.061]</i>	<i>[0.107]</i>	<i>[0.146]</i>	<i>[0.693]</i>
<i>(Controls: none)</i>					
<i>Quota – Neutral</i>	0.065***	0.093*	2.650	0.884	0.097
<i>P-Value:</i>	<i>[0.002]</i>	<i>[0.067]</i>	<i>[0.113]</i>	<i>[0.158]</i>	<i>[0.576]</i>
<i>(Controls: pre-test scores)</i>					
<i>Quota – Neutral</i>	0.058***	0.078	2.404	0.773	0.164
<i>P-Value:</i>	<i>[0.005]</i>	<i>[0.130]</i>	<i>[0.158]</i>	<i>[0.224]</i>	<i>[0.346]</i>
<i>(Controls: pre-test scores, school FEs)</i>					
<i>N</i>	992	992	992	992	895

Notes: Each cell represents a separate regression. The number reported is the coefficient for the quota treatment. Row 1 includes no controls and provides a statistical test of the differences in Table 1. Row 2 includes control for practice test score. Row 3 includes school fixed effects. P-values for a two-sided test of the null hypothesis of zero difference are italicized and in brackets. Estimates under each of the four effort variables are intended to capture the effect of a treatment on human capital investment for the total study population, and are therefore averaged over both investors and non-investors.

numbers in Table 3 and afterward appear small: they are averaged over both investors *and* non-investors. The results indicate that subjects in the quota treatment, including students from all age groups, were 75% more likely to have visited the website than students in the color-blind treatment. They also tried out more subjects, spent more time on the website and answered more questions. Table 3 indicates that students in both treatments scored roughly the same on the final exam. This is allowed for by the theory, where predictions for the overall population are qualitatively ambiguous, but later on we will see a different story when we separate these measures by demographic group.

Table 4 provides statistical tests for the raw differences displayed in table 3. In the first row we run a simple regression using a dummy for the quota treatment, meaning it represents the experimental difference between an *RQ* rule and *CB* allocations at the

population level (*i.e.*, including both demographic groups). Each cell in the table represents a separate regression with the outcome variable labeled in the column header. We report the point estimate and p-value for a test of the hypothesis that there is no difference by treatment group. From the table we see strong evidence that AA increases the fraction of students willing to invest at least some time. We also see evidence that it induces them to experiment with more subjects, as well as increase the total time invested and number of questions attempted. Although these last two differences are only marginally significant, the estimated magnitudes are large, with quota students logging an estimated 57% and 70% more inputs of time and question attempts, respectively. Additional controls (pre-test score and/or school fixed effects) are added in the bottom two rows as a check on the effectiveness of our randomization. It seems to have worked well, as adding these additional 11 variables caused no significant shifts in point estimates.

5.2.2. Testing Differences by Treatment Within Demographic Groups. Recall that the theory allows for AA to have differential effects by ability and demographic group. In Table 5, we add a demographic dummy to investigate this claim. Each column presents estimates for a regression equation of the form

$$Outcome = \beta_0 + \beta_1 Quota + \beta_2 Advantaged * Quota + \beta_3 Advantaged + \beta_4 Ability + \varepsilon,$$

where *Quota* is a dummy for treatment status, *Advantaged* is a demographic dummy, *Ability* is a student's standardized pre-test score, and the specific *Outcome* variable is labeled in the column header. With the inclusion of the interaction term *Advantaged * Quota*, the coefficient β_1 represents the average effect of AA specifically on the disadvantaged group. The effect of the policy on the advantaged group is represented by the sum $\beta_1 + \beta_2$. For completeness, all regressions include controls for school-level fixed effects, and for the primary effects of interest we report p-values in brackets.

For disadvantaged group students we find evidence of large and positive effects across all four investment measures. First, we see a highly significant 8.7 percentage point increase in disadvantaged students' willingness to spend at least some time on the website, under AA. To put this in perspective, we can compute a within-demographic percent change for the disadvantaged group by $100 \times (\beta_1 / \beta_0)\%$, which amounts to an increase of 119% on the extensive margin, relative to their disadvantaged counterparts under the color-blind treatment. We also see a significant and even larger increase in terms of time investment: disadvantaged students under treatment *RQ* increased investment by 181%. The other two measures capture specific tasks done during time spent on the website: number of subjects attempted and number of questions attempted. Although the latter is only marginally significant, both render large point estimates for increases of 101% and 100%, respectively.

TABLE 5. TESTING DIFFERENCES BY DEMOGRAPHICS AND TREATMENT

	Investment				Performance
	Used Website	# Subjects Attempted	Total Time	# Questions Attempted	Final Exam Score
<i>Constant</i> ($\hat{\beta}_0$)	0.073***	0.144**	3.033	1.316*	8.147***
Std. Err.	(0.024)	(0.059)	(1.982)	(0.741)	(0.209)
<i>Quota</i> ($\hat{\beta}_1$)	0.087***	0.146*	5.517**	1.312	0.624**
Std. Err.	(0.033)	(0.083)	(2.757)	(1.030)	(0.287)
<i>P-Value:</i>	<i>[0.009]</i>	<i>[0.077]</i>	<i>[0.046]</i>	<i>[0.203]</i>	<i>[0.030]</i>
<i>Advantaged * Quota</i> ($\hat{\beta}_2$)	-0.047	-0.111	-5.034	-0.866	-0.712**
Std. Err.	(0.042)	(0.105)	(3.506)	(1.310)	(0.360)
<i>Advantaged</i> ($\hat{\beta}_3$)	0.028	0.083	1.613	0.877	0.488*
Std. Err.	(0.030)	(0.076)	(2.545)	(0.951)	(0.264)
<i>Ability</i> ($\hat{\beta}_4$)	0.029***	0.045*	1.009	0.572*	1.280***
Std. Err.	(0.011)	(0.027)	(0.893)	(0.334)	(0.092)
School Fixed Effects	yes	yes	yes	yes	yes
<i>N</i>	992	992	992	992	895
Additional Test: Effect of Quota on Advantaged Group					
$\hat{\beta}_1 + \hat{\beta}_2$	0.040	0.035	0.483	0.446	-0.089
<i>P-Value:</i>	<i>[0.123]</i>	<i>[0.586]</i>	<i>[0.823]</i>	<i>[0.581]</i>	<i>[0.684]</i>

Notes: Each column is a separate regression. Advantaged is an indicator variable for whether the student is a 6th or 8th grader (the older group in each school type). Ability is the standardized pre-test score, where standardization is based on the mean and variance within each school type (*i.e.*, 5th/6th or 7th/8th). Standard errors are in parentheses; p-values for a two-sided test of the null hypothesis of zero effect are italicized and in brackets. Estimates under each of the four effort variables are intended to capture the effect of a treatment on human capital investment for the total study population, and are therefore averaged over both investors and non-investors.

Another striking feature of the table is the performance measure. We find a large and significant difference in final exam scores for disadvantaged students under AA: they are estimated to have lifted their scores by 0.624 AMC8 points, or a remarkable 21.7% of a standard deviation over their disadvantaged counterparts in the control group. Although some portion of this effect may also be due to increased effort and concentration

on the day of the final exam, we interpret this and the other columns in Table 5 as evidence that treatment RQ altered labor-leisure trade-offs to induce additional study effort at home for \mathcal{D} students, which in turn lead to more learning and increased math proficiency.

One concern is that this strengthening of incentives for disadvantaged students may come at the cost of weakening incentives for advantaged students. However, Table 5 shows evidence that this concern is not economically significant. For 4 out of 5 outcome measures, point estimates for the effect on \mathcal{A} , given by $\beta_1 + \beta_2$, was actually positive, but insignificant. For the final exam outcome, the sum of the two coefficients is slightly negative (representing about 3% of a standard deviation) but with a large p-value. The outcome measure under which $\beta_1 + \beta_2$ is most significant is the binary measure of investment, with a p-value of 0.123. This implies an estimated percent change of $100 \times (\beta_1 + \beta_2) / (\beta_0 + \beta_3) = +39.6\%$ on the extensive margin for advantaged students under a quota. Thus, we do not find evidence that there is a trade-off between average human capital investment across demographic groups; if anything the data seem to slightly favor a small increase of investment for the advantaged demographic as well. Figures 11 – 14 in the Appendix contain graphical depictions of the distributional shifts of inputs and outputs by demographic and treatment group.

5.2.3. *Selective Attrition.* One potential source of bias in our results concerning the performance measure (final exam score) is that 97 of the students who took the practice test and were randomly assigned to a competition group (9.8%) did not show up on the day of the final test.¹⁷ We find that among the disadvantaged students, those assigned to the quota group were less likely to miss the final exam (10.6% vs. 16.7%). We also find that among the students who did not show up for the final test, the disadvantaged students assigned to the quota group had higher practice scores than the disadvantaged students not assigned to the quota group (7.16 vs 6.35). However, the practice scores among the students who did show up for the final test were nearly the same across these two groups (7.91 vs 7.79). These comparisons all point in a direction opposite of our main results and suggest that the effect of the quota on final performance for disadvantaged students may have been greater in the absence of this selective attrition.

5.2.4. *Policy Responses by Ability Level.* The theoretical model predicts that if the underlying cost types for the disadvantaged group stochastically dominate those in the advantaged group, then qualitative patterns like those displayed in Figure 3 should appear. Namely, for group \mathcal{D} the test score distributions under a quota and color-blind

¹⁷This problem does not arise with the four investment measures, which did not require observing a final score for us to estimate them. In Table 5 the sample size for the first four columns represent the full sample of test subjects.

mechanism should have an interior crossing point, with the former strictly above the latter to the right of the crossing point, and strictly below to the left. In other words, there should be a positive mass of the best group \mathcal{D} students who decrease output, while students of medium and low ability from that group increase output. The theory makes opposite predictions for group \mathcal{A} .¹⁸

While it is impossible to directly observe the distributions of cost types, we can take queues from the distribution of pre-test scores by demographic group, since they reflect initial math proficiency, as well as how much progress each student will need to increase her payout. We can then examine the distributions of final exam scores within demographic groups under different treatments to see whether our experimental data seem to be consistent with the theory of incentive effects under AA. Figures 4 – 6 depict these comparisons in three plots of empirical cumulative distribution functions (CDFs) for pre-test and final exam scores for grades 7 and 8. For the sake of comparability, we have limited our sample in these figures to include only students for whom we have both test scores. Therefore, Figure 4 plots empirical pre-test CDFs only for 7th and 8th graders who took the final exam.

Figure 4 strongly supports stochastic dominance of initial math proficiency levels across demographic groups. A two-sample Kolmogorov-Smirnov (KS) test rejects the null hypothesis that the 7th and 8th grade distributions are the same, against a one-sided alternative that the latter stochastically dominates (in the first order sense) with a p-value of 1.03×10^{-5} . This means group \mathcal{D} on average had to achieve more progress in order to be competitive for a prize. This is not the same as observing costs, but the two are certainly related and the idea of stochastic dominance in cost types appears plausible.

We find evidence in Figure 5 that by the end of the study period the score distribution within the 7th grade subsample had diverged by treatment status. A two-sided KS test for disadvantaged group final exams across treatments results in a p-value of 0.1047, providing marginally significant evidence that the two distributions were not the same.¹⁹ There appears to be a hint of divergence by treatment group among 8th graders

¹⁸Although the figures we present in this paper explore distributions of both inputs and outputs, it is important to remember that the predictions of the theory only directly apply to exam score, as this is the variable on which prize allocations are based. The mapping from inputs to outputs may vary by student if each one differs by raw math talent and leisure preference.

¹⁹Figures 13 and Figure 14 in the Appendix contain additional plots comparing pre-test scores by treatment within demographic groups. The differences in the pre-test distributions are due to selective attrition after we omitted students for whom we have no final exam. The figures suggest that in general selective attrition is working against our results presented here. For group \mathcal{D} the pre-test color-blind distribution is below the pre-test quota distribution for values at or below the median, and the upper bound of the pre-test distribution for 7th grade quota students is highest. Both characteristics of these distributions are substantially reversed by the final exam. Figure 14 suggests that selective attrition within

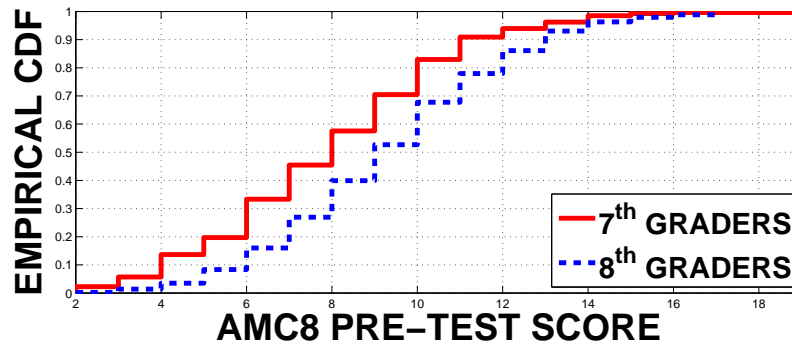
FIGURE 4. PRE-TEST SCORES: 7th GRADE VS 8th GRADE

FIGURE 5. SEVENTH GRADE FINAL EXAM SCORES

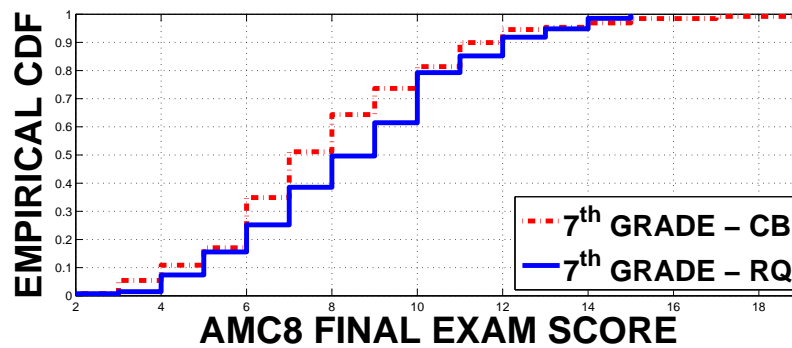
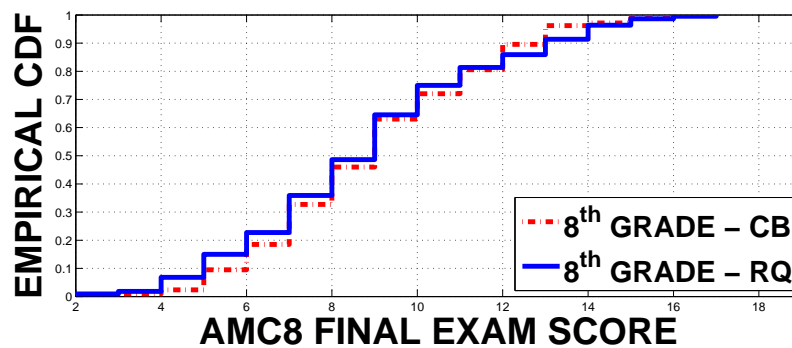


FIGURE 6. EIGHTH GRADE FINAL EXAM SCORES



Notes: For the sake of comparability, Figures 4 – 6 above use only data for 7th and 8th graders who took both the pre-test *and* final exam. See figures 13 and 14 in the appendix for a sense of the role played by selective attrition.

in Figure 6 as well, though the effect is weak and we lack sufficient power for a KS test to distinguish the two distributions (the p-value for a two-sided test is 0.889).

the 8th grade is also working slightly against finding a significant difference in the direction predicted by theory.

In interpreting the figures, one caveat should be kept in mind: the KS test can only indicate that two distributions are not the same, but it does not provide for a test of the specific ordering of two distributions with an interior crossing point on different subsets of the support. Therefore, the qualitative patterns displayed in Figures 5 and 6 are suggestive in nature. However, a striking feature of the plots is the remarkable degree to which they conform to the qualitative patterns predicted by theory and displayed in the numerical example from Section 3. Both sets of CDFs have a single interior crossing point, with the upper bound on group \mathcal{D} test scores under AA leading by four AMC8 points—about one and a third standard deviations—relative to that under color-blind allocations (15 vs 19). Below the crossing point, we see a substantial increase in measured 7th grade math proficiency under a quota. This pattern is reversed for 8th graders (once again consistent with theoretical predictions), though the effects are much smaller in magnitude.

5.2.5. *Narrowing Achievement Gaps.* We now conclude analysis of our experimental data with a look at the tendency for AA to narrow achievement gaps across demographic groups. Table 6 displays summary statistics on standardized test scores for the pre-test and final exam, for grades 7 and 8. In the top panel of the table scores were standardized within each exam by subtracting the mean and dividing by the standard deviation for all grade 7 and 8 students.²⁰ Therefore, the means indicate distance between the population average and grade cohort average, in standard deviation units. Without accounting for treatment status we see that 7th grade students were roughly half of a standard deviation behind their 8th grade counterparts on average—or $-0.295 - 0.181 = -0.476$ standard deviations to be exact—but by the final exam, the gap between 7th and 8th graders had narrowed by about half—to $-0.138 - 0.085 = -0.223$ standard deviations.

In the lower two panels of Table 6 we break out this effect by treatment group, displaying the same numbers within treatments, but where score standardization now happens within each exam-treatment cell. Part of the test score convergence had to do with differences in the conditions of the pre-test and final exams (likely due to incentives or slightly different content): within the color-blind treatment, about a quarter of the gap disappeared but remained relatively high at 0.367 standard deviations on the final exam. However, the achievement gap under AA closed substantially more, by about 80%, beginning at 0.442 standard deviations, and ending at only 0.085 standard deviations on the final exam.

The medians tell a slightly stronger story, with the median gap beginning about the same within both treatments, closing virtually to zero under AA, and closing only

²⁰Once again, in order to make the pre-test and final exam figures comparable, we excluded from the analysis any students whose final scores were missing due to attrition.

TABLE 6. NARROWING GAPS

	Mean	Median	Std. Dev.	N
Achievement Gaps for All Treatments				
Standardized Pre-Score (GRADE 7)	-0.295	-0.267	0.996	264
Standardized Pre-Score (GRADE 8)	0.181	0.071	0.960	431

Standardized Final Score (GRADE 7)	-0.138	-0.205	1.005	264
Standardized Final Score (GRADE 8)	0.085	0.142	0.987	431
Achievement Gaps for Quota Treatment				
Standardized Pre-Score (GRADE 7)	-0.211	-0.199	1.061	135
Standardized Pre-Score (GRADE 8)	0.231	0.135	0.920	220

Standardized Final Score (GRADE 7)	-0.030	0.142	0.961	135
Standardized Final Score (GRADE 8)	0.055	0.142	1.050	220
Achievement Gaps for Color-Blind Treatment				
Standardized Pre-Score (GRADE 7)	-0.243	-0.199	0.904	129
Standardized Pre-Score (GRADE 8)	0.259	0.135	0.982	211

Standardized Final Score (GRADE 7)	-0.251	-0.552	1.041	129
Standardized Final Score (GRADE 8)	0.115	0.142	0.921	211

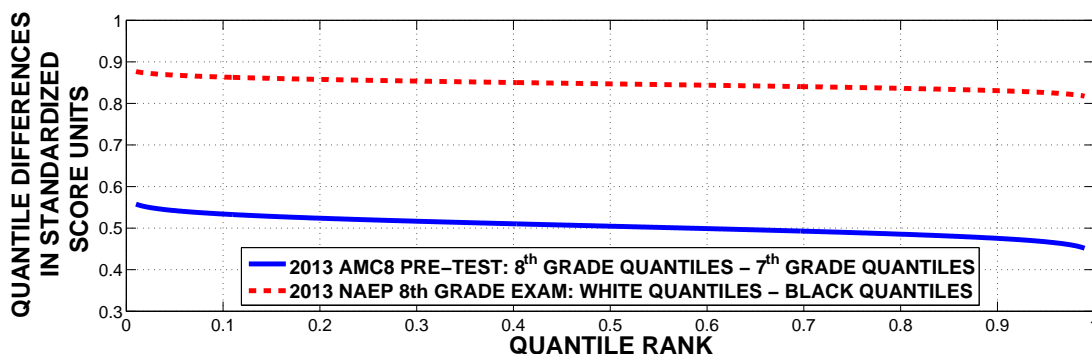
Notes: There are three separate panels in the table, each containing standardized scores on the pre-test and post-test. Standardization was performed within each panel-test grouping, excluding scores for students who missed the final exam. For example, pre-test scores for the quota treatment were standardized using the mean and standard deviation of pre-test scores for all 7th and 8th graders in the quota treatment who took both the pre-test and post-test.

slightly otherwise. Finally, observing that the within-treatment-demographic standard deviations are all close to one suggests that the narrowing of gaps within the two treatments was due predominantly to mean/median shifts in test scores. We interpret these findings as evidence that AA can actually help to narrow achievement gaps by catching up its target demographic to the rest of the population.

6. DISCUSSION AND CONCLUSION

6.1. EXTERNAL VALIDITY AND DIRECTIONS FOR FUTURE RESEARCH. While interpreting the results from this study, it is important to keep in mind what it can and

FIGURE 7. GRADE-LEVEL DIFFERENCES VS BLACK/WHITE DIFFERENCES



(A) This figure first standardizes AMC8 and NAEP exam scores by subtracting the mean and dividing by the standard deviation for the relevant sample population. Then, the distribution of standardized test scores for each sub-sample—*i.e.*, AMC8 7th grade, AMC8 8th grade, NAEP 8th grade whites, and NAEP 8th grade blacks—is approximated by a normal distribution, which appears to fit the data quite well. The resulting quantile functions are subtracted from each other in order to illustrate relative disparities between groups.

cannot say. The ultimate question of interest is the effect that real world AA has on the human capital incentives of American black and Hispanic youth and their families. There are several reasons why this study cannot fully address this policy question. First, this experiment involved a fairly homogeneous population to produce a clean exploration of theoretical predictions, using grade level as a proxy for race. This proxy is obviously imperfect. As Figure 7 illustrates, it likely understates the competitive difference in math proficiency between blacks and whites vying for actual college seats. The graph compares the difference between black and white 8th grade NAEP quantiles—expressed in standardized NAEP score units (dashed line)—to the difference in 7th and 8th grade quantiles from our sample—expressed in standardized AMC8 score units (solid line). At most NAEP quantiles, black 8th graders trail their white counterparts by about 0.85 standard deviations, whereas the gap among our test subjects is closer to 0.5 standard deviations. On the one hand, this suggests that real world discouragement effects minorities face may be even larger than those found here, suggesting that AA may have a larger impact. On the other hand, our homogeneous subject pool does not account for important differences in home environments, school quality, or cultural factors which may push the results in other, less predictable directions. Certainly though, in order to understand how/why a policy works, it is important to cleanly understand the incentive dimension in addition to other sociocultural factors. The results here demonstrate that AA can be used as an effective tool to improve performance incentives and promote investment activity among systematically disadvantaged populations.

The second major limitation in the current analysis is that, although it creates a set of incentives which mirror human capital competition in key ways, due to feasibility constraints said incentives are only engineered on a small scale and measured over a short-run horizon. We tracked our students over a period of 10-days during which they were given the opportunity to invest in their math proficiency. For students in our sample who logged positive amounts of study time, their average expected wage was \$10.73/hour, measured as the mean of actual earnings divided by observed time spent studying. Junior high and high school students preparing for college optimize labor-leisure division over a much longer horizon (4-5 years until college applications are due), with payoffs that are many orders of magnitude larger.²¹ How these two forces balance out in the long run is a challenging question. Although the larger picture may pose feasibility constraints at present, our experimental results at least inform us that students who are about to enter high school are capable of the kind of complex optimization that would imply AA having a positive impact toward increasing the minority human capital stock and narrowing achievement gaps. Once again though, this short-run evidence is suggestive in nature, and further research is needed to provide a final, complete answer.

6.2. CONCLUSION. We designed a field experiment in which middle school and junior high students compete for heterogeneous cash prizes and are paid by their relative performance on a nationwide math exam. Our experimental design is intended to give insight into students' pre-college human capital investment followed by the college admissions market; namely, voluntary labor-leisure decisions, mathematics learning, and affirmative action. Within this context, the rank-order prize allocation rule approximated either a color-blind system, or a representative quota AA policy. We tracked student study effort during a 10-day investment period prior to the final exam. Although our experiment involves relatively low-value prizes and a relatively low-stakes exam, we still find sizable and significant effects of AA on motivation and learning.

Our results are clearly suggestive that students adjust their labor supply in response to AA considerations. Although some of this effect may come through changes in effort or focus on the day of the exam, we also find evidence that the disadvantaged students are more likely to study for the exam under AA. To the extent that our experiment is informative about American university admissions, the findings suggest two things: (i) that AA may improve a minority student's real effort to build human capital in preparation for college, and (ii) that it can actually *narrow* achievement gaps while promoting diversity in higher education.

²¹A large literature has focused on the wage return to higher college quality, including Stacy Berg Dale and Alan B. Krueger [2002], Dan A. Black and Jeffrey A. Smith [2006], and Long [2008]. Although there is some disagreement as to the magnitude for all students, there is broad consensus that the return to attending higher quality colleges is economically significant for poor students.

REFERENCES

- Arcidiacono, Peter.** 2005. "Affirmative Action in Higher Education: How do Admission and Financial Aid Rules Affect Future Earnings?" *Econometrica*, 73(5): 1477–1524.
- Bettinger, Eric.** 2012. "Paying to Learn: The Effect of Financial Incentives on Elementary School Test Scores." *Review of Economics and Statistics*, 94: 686–698.
- Black, Dan A., and Jeffrey A. Smith.** 2006. "Estimating the Returns to College Quality Using Multiple Proxies for Quality." *Journal of Labor Economics*, 24(3): 701–728.
- Bodoh-Creed, Aaron, and Brent R. Hickman.** 2015. "Using Auction Theory to Study Human Capital Investment in Assortative Matching Markets: a Look at Affirmative Action in College Admissions." *working paper, University of Chicago*.
- Bowen, William G., and Derek Bok.** 1998. *The Shape of the River: Long-Term Consequences of Considering Race in College and University Admissions*. Princeton, NJ: Princeton University Press.
- Bracha, Anat, Alma Cohen, and Lynn Conell-Price.** 2015. "Affirmative Action and Stereotype Threat." *working paper*.
- Calsamiglia, Caterina, Jorg Franke, and Pedro Rey-Biel.** 2013. "The incentive effects of affirmative action in a real-effort tournament." *Journal of Public Economics*, 98: 15–31.
- Chambers, David L., Timothy T. Clydesdale, William C. Kidder, and Richard O. Lempert.** 2005. "The Real Impact of Eliminating Affirmative Action in American Law Schools: An Empirical Critique of Richard Sander's Study." *Stanford Law Review*, 57: 1855–1897.
- Coate, Stephen, and Glenn Loury.** 1993. "Anti-Discrimination Enforcement and the Problem of Patronization." *American Economic Review*, 83(2): 92–98.
- Cotton, Christopher, Frank McIntyre, and Joseph Price.** 2013. "Gender differences in repeated competition: Evidence from school math contests." *Journal of Economic Behavior and Organization*, 86: 52–66.
- Dale, Stacy Berg, and Alan B. Krueger.** 2002. "Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables." *Quarterly Journal of Economics*, 117: 1491–1528.
- Ferman, Bruno, and Juliano Assuncao.** 2011. "Does Affirmative Action Enhance or Undercut Investment Incentives? Evidence from Quotas in Brazilian Public Universities." *Typescript, Massachusetts Institute of Technology Department of Economics*.
- Fryer, Roland.** 2011. "Financial Incentives and Student Achievement: Evidence From Randomized Trials." *Quarterly Journal of Economics*, 126: 1755–1798.
- Hickman, Brent R.** 2015. "Human Capital Investment and Affirmative Action: A Structural Policy Analysis of US College Admissions." *Typescript, University of Chicago Department of Economics*.

- Howell, Jessica S.** 2010. "Assessing the Impact of Eliminating Affirmative Action in Higher Education." *Journal of Labor Economics*, 28(1): 113–66.
- Kremer, Michael, Edward Miguel, and Rebecca Thornton.** 2009. "Incentives to Learn." *Review of Economics and Statistics*, 91: 437–456.
- Leuven, Edwin, Hessel Oosterbeek, and Bas van der Klaauw.** 2010. "The Effect of Financial Rewards on Students' Achievement: Evidence from a Randomized Experiment." *Journal of the European Economic Association*, 8: 1243–1265.
- Long, Mark C.** 2008. "College Quality and Early Adult Outcomes." *Economics of Education Review*, 27: 588–602.
- Loury, Linda Datcher, and David Garman.** 1995. "Selectivity and Earnings." *Journal of Labor Economics*, 13(2): 289–308.
- Rothstein, Jesse, and Albert H. Yoon.** 2008. "Affirmative Action in Law School Admissions: What do Racial Preferences Do?" *University of Chicago Law Review*, 75(2): 649–714.
- Sander, Richard H.** 2004. "A Systemic Analysis of Affirmative Action in American Law Schools." *Stanford Law Review*, 57: 367–483.
- Schotter, Andrew, and Keith Weigelt.** 1992. "Asymmetric Tournaments, Equal Opportunity Laws and Affirmative Action: Some Experimental Results." *Quarterly Journal of Economics*, 107(2): 511–539.

7. APPENDIX

7.1. THEORETICAL APPENDIX.

7.1.1. *Proof of Theorem 1.* The result of the theorem follows from Bodoh-Creed and Hickman [2015, Theorems 3 and 4], which establish a claim that is both broader (it allows for shifts in F_S as well) and holds under a more general set of conditions. Because of this their proof is quite complicated, so for completeness we provide a proof for the simplified version of the model from Section 3. For the reader's convenience Theorem 1 is restated below:

Theorem 1: Consider two color blind contests, 1 and 2, which differ only by their cost distributions, and assume competition is more fierce under contest 2 in the sense that F_1 LR dominates F_2 . Let $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$ denote the unique crossing point of the density functions where $f_1(\tilde{\theta}) = f_2(\tilde{\theta})$. There exists a unique interior crossing point $\check{\theta} \in (\underline{\theta}, \tilde{\theta})$, such that $h_1^*(\theta) < h_2^*(\theta)$ for all $\theta < \check{\theta}$ and $h_1^*(\theta) > h_2^*(\theta)$ for all $\theta > \check{\theta}$.

Proof: As mentioned above, the LR dominance property implies first-order stochastic dominance. Therefore, not only do the densities have a unique crossing point, but it must also be true that $f_1(\theta) > f_2(\theta)$ for $\theta > \tilde{\theta}$, and $f_1(\theta) < f_2(\theta)$ for $\theta < \tilde{\theta}$. Since the

same boundary condition applies to both contests, $h_1^*(\bar{\theta}) = h_2^*(\bar{\theta}) = \underline{h}$, then equation (5) implies the initial trajectories at the boundary point are ordered in the following way:

$$h_1^{*'}(\bar{\theta}) = \frac{-f_1(\bar{\theta})}{c'(\underline{h}; \bar{\theta})} < \frac{-f_2(\bar{\theta})}{c'(\underline{h}; \bar{\theta})} = h_2^{*'}(\bar{\theta}). \quad (\text{A.1})$$

This in turn means that $h_1^*(\theta) > h_2^*(\theta)$ within a neighborhood of $\bar{\theta}$ since the investment functions are continuous and everywhere differentiable. Note that because slopes are negative $h_1^{*'}(\theta) < h_2^{*'}(\theta)$ means h_1^* rises in the leftward direction and is *more steep* at θ .

Now suppose there exists at least one point where h_1^* and h_2^* cross, and let $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$ denote the maximum of all such possible points, with $\tilde{h} \equiv h_1^*(\tilde{\theta}) = h_2^*(\tilde{\theta})$, if any exist. Since h_1^* crosses h_2^* from above at $\tilde{\theta}$, it must be that $h_1^{*'}(\tilde{\theta}) \geq h_2^{*'}(\tilde{\theta})$ (i.e., h_1^* is *less steep* at the crossing point). However, since $f_1(\theta) > f_2(\theta)$ on $(\tilde{\theta}, \bar{\theta}]$ by LR dominance, and since $h < h'$ implies $c'(h'; \theta) \geq c'(h; \theta)$ by convexity, the following must be true for any $\theta \in (\tilde{\theta}, \bar{\theta})$:

$$h_1^{*'}(\theta) = \frac{-f_1(\theta)}{c' [h_1^*(\theta); \theta]} < \frac{-f_2(\theta)}{c' [h_2^*(\theta); \theta]} = h_2^{*'}(\theta) \quad \Rightarrow \quad h_1^*(\theta) > h_2^*(\theta). \quad (\text{A.2})$$

Therefore, $\tilde{\theta} < \bar{\theta}$, if such a point exists. Similarly, since $f_1(\theta) < f_2(\theta)$ on $[\underline{\theta}, \tilde{\theta})$, then any crossing point would have to obey $h_1^{*'}(\tilde{\theta}) = -f_1(\tilde{\theta})/c'(\tilde{h}; \tilde{\theta}) > -f_2(\tilde{\theta})/c'(\tilde{h}; \tilde{\theta}) = h_2^{*'}(\tilde{\theta})$. This means that h_1^* can only cross h_2^* from above (i.e., at points where it is *less steep*) and so there can be at most one such crossing.

Finally, to see why a crossing point must exist, suppose for a contradiction that for all $\theta \in (\underline{\theta}, \bar{\theta})$ we have $h_1^*(\theta) > h_2^*(\theta)$. In that case, it follows that

$$\begin{aligned} h_1^*(\underline{\theta}) &= \int_{\underline{\theta}}^{\bar{\theta}} \frac{1}{c' [h_1^*(u); u]} f_1(u) du + \underline{h} \\ &< \int_{\underline{\theta}}^{\bar{\theta}} \frac{1}{c' [h_1^*(u); u]} f_2(u) du + \underline{h} \\ &\leq \int_{\underline{\theta}}^{\bar{\theta}} \frac{1}{c' [h_2^*(u); u]} f_2(u) du + \underline{h} = h_2^*(\underline{\theta}), \quad \rightarrow \leftarrow . \end{aligned}$$

The strict inequality follows because the first and second line depict expectations over the decreasing function $1/c' [h_1^*(u); u]$ and f_2 places more weight on strictly higher values of u (or lower values of the function). The weak inequality follows from the supposition and from c being convex and having a positive cross-partial derivative. Thus we have a contradiction, so a unique crossing $\tilde{\theta}$ exists on the open interval $(\bar{\theta}, \tilde{\theta})$ and the theorem is proved. ■

7.2. THEORY EXTENSION: INTRINSICALLY VALUED HC. In the body of the paper, we assume that students only indirectly benefit from their HC accumulation. Here, we consider an extension to the model where h is intrinsically valued to the student, and not just a means of securing a high-quality seat assignment. Naturally, with a more flexible model comparative statics become more difficult to prove. Therefore, we explore some simple examples based on a Cobb-Douglas utility form $u(s, h) = s^\alpha h^\beta$, in order to argue that the qualitative model predictions explored in Section 3 are non-pathological.

7.2.1. Example 1: Linear Utility. If $\alpha = \beta = 1$ and costs are linear so that $u(s, h) = sh$, $c(h; \theta) = \theta(h - \underline{h})$, and $[\underline{\theta}, \bar{\theta}] \subset (1, \infty)$, then proving statements analogous to Theorem 1, Corollary 2, and Corollary 3 is relatively straightforward.²² In this scenario, under CB student i 's objective is now

$$\max_{h_i \geq \underline{h}} \{h_i (1 - F[\theta^*(h_i)]) - \theta_i(h_i - \underline{h})\}. \quad (\text{A.3})$$

Taking an FOC, we get $-h_i f[\theta_i^*(h_i)] \theta_i'^*(h_i) + (1 - F[\theta_i^*(h_i)]) = \theta_i$. In this world, student i can be thought to choose HC production in two parts. First, she raises investment to the level where she optimally benefits from complementarity of her own input h_i and the university's input s ; this calculation involves the direct marginal benefit of a unit of HC, or $(1 - F[\theta_i^*(h_i)])$, and Bodoh-Creed and Hickman [2015] refer to it as the *productive channel* of incentives. Above that base level of investment, competitive incentives play an additional role: in a monotone equilibrium i invests further so as to make her higher-cost competitors prefer not to try and jump her. This calculation involves the indirect placement benefit of more HC, or $-h_i f[\theta_i^*(h_i)] \theta_i'^*(h_i)$, and it produces a tendency for over-investment which is referred to as the *competitive channel* of incentives.

If we now define $\eta(\theta) \equiv \log(h^*(\theta))$ then through a similar change of variables as before we can rearrange the FOC to get a differential equation

$$\eta'(\theta_i) = -\frac{f(\theta_i)}{\theta_i + F(\theta_i) - 1}, \text{ with boundary condition } \eta(\bar{\theta}) = \log(\underline{h}). \quad (\text{A.4})$$

Once again, η is strictly decreasing in θ and we can now prove the analog of Theorem 1 above:

Theorem 4. Assume HC production costs and gross utility are linear in h , so that $c(h; \theta) = \theta(h - \underline{h})$, and $u(s, h) = sh$. Moreover, consider two cost distributions, $F_1(\theta)$ and $F_2(\theta)$, where competition is more intense under F_2 in the sense that F_1 LR dominates F_2 (i.e., the ratio $\frac{f_1(\theta)}{f_2(\theta)}$ is

²²The assumption of $[\underline{\theta}, \bar{\theta}] \subset (1, \infty)$ is required because the gross utility and cost function are both linear in h . Since the direct marginal benefit of more human capital is $u_2(s, h) = s$ and the direct marginal cost is θ , we must have $\underline{\theta} > \bar{s}$, in order to rationalize all students choosing finite HC production.

strictly increasing). Then, letting $\tilde{\theta}$ denote the unique crossing point of f_1 and f_2 , there exists a unique interior crossing point $\check{\theta} \in (\underline{\theta}, \tilde{\theta})$ such that $h_1^*(\theta) < h_2^*(\theta)$ for $\theta < \check{\theta}$ and $h_1^*(\theta) > h_2^*(\theta)$ for $\theta > \check{\theta}$.

Proof: Recall that strict LR dominance implies $f_1(\theta) \geq f_2(\theta)$ and $F_1(\theta) < F_2(\theta)$, for $\theta \in [\tilde{\theta}, \bar{\theta})$. This with $\eta_1(\bar{\theta}) = \eta_2(\bar{\theta})$ and equation (A.4) together mean that $\eta_1'(\theta) < \eta_2'(\theta)$ and $\eta_1(\theta) > \eta_2(\theta)$, for each $\theta \in [\tilde{\theta}, \bar{\theta})$. Thus, if η_1 and η_2 cross, the crossing must be on the interval $[\underline{\theta}, \tilde{\theta})$.

Now, equation (A.4) can be expressed in integral form by $\eta_j(\theta) = \int_{\underline{\theta}}^{\bar{\theta}} \frac{f_j(x)}{x + F_j(x) - 1} dx + \log(\underline{h})$, $j = 1, 2$. Moreover, if we impose a change of variables $y = F_j(\theta)$ within the integral, we get

$$\eta_1(\underline{\theta}) = \int_0^1 \frac{1}{F_1^{-1}(y) + y - 1} dy + \log(\underline{h}) < \int_0^1 \frac{1}{F_2^{-1}(y) + y - 1} dy + \log(\underline{h}) = \eta_2(\underline{\theta}),$$

where the inequality follows from LR dominance. Therefore, by continuity at least one crossing point exists on the open interval $(\underline{\theta}, \tilde{\theta})$. Let $\check{\theta}$ denote the maximum point at which $\eta_1(\check{\theta}) = \eta_2(\check{\theta})$, and note that since the two functions are negatively sloped and η_1 crosses η_2 from above (moving in the leftward direction) at $\check{\theta}$, the following must be true when $\theta = \check{\theta}$:

$$\begin{aligned} \eta_1'(\check{\theta}) &= -\frac{f_1(\check{\theta})}{\check{\theta} + F_1(\check{\theta}) - 1} > -\frac{f_2(\check{\theta})}{\check{\theta} + F_2(\check{\theta}) - 1} = \eta_2'(\check{\theta}) \\ \Leftrightarrow f_1(\check{\theta}) [\check{\theta} + F_2(\check{\theta}) - 1] &< f_2(\check{\theta}) [\check{\theta} + F_1(\check{\theta}) - 1] \end{aligned} \quad (\text{A.5})$$

Now consider approaching $\underline{\theta}$ from above, beginning at $\check{\theta}$ and once again moving leftward. Since $\check{\theta}$ is to the left of the density crossing $\tilde{\theta}$, then by the LR dominance property, we know that if we begin at $\check{\theta}$ and approach $\underline{\theta}$ from above, then $f_2(\theta)$ becomes steadily larger relative to $f_1(\theta)$ as we move leftward. This also implies that $[\theta + F_1(\theta) - 1]$ becomes steadily larger relative to $[\theta + F_2(\theta) - 1]$ in the leftward direction (beginning from $\check{\theta}$) as well.²³ Therefore, the ordering between the right-hand and left-hand sides of inequality A.5 only becomes more pronounced as we move leftward from $\check{\theta}$. From this fact it follows that $\eta_1'(\theta) > \eta_2'(\theta)$ for each $\theta \in [\underline{\theta}, \check{\theta}]$, and the crossing point $\check{\theta}$ is therefore unique. ■

We can build on the above result to prove comparative statics for different seat allocation rules just as we did before. Corollary 5 and Corollary 6 below build on Theorem 4

²³To see why, recall that $F_1(\underline{\theta}) = F_2(\underline{\theta}) = 0$ and $f_1(\theta) < f_2(\theta)$ for each $\theta < \tilde{\theta}$. Thus, F_2 becomes steadily larger relative to F_1 when moving in the rightward direction from $\underline{\theta}$ to $\tilde{\theta}$, which is the same as saying that F_1 becomes steadily larger relative to F_2 when moving in the leftward direction from $\tilde{\theta}$.

in precisely the same way that Corollary 2 and Corollary 3 build on Theorem 1 above. In particular, Corollary 6 follows naturally from the proof of Theorem 4: we now know that $\eta'_{\mathcal{D}}(\theta) < \eta'(\theta) < \eta'_A(\theta)$ for each $\theta \in [\tilde{\theta}, \bar{\theta}]$, and since the log transformation preserves ordering, it follows that $h^*_{\mathcal{D}}(\theta) < \eta^{*'}(\theta) < \eta^{*'}_A(\theta)$ on that same interval as well. This also produces a slight strengthening of the result since the derivatives are strictly ordered at the density crossing $\tilde{\theta}$. Together, the final two results demonstrate that the model still predicts a large fraction of the disadvantaged group increasing investment under RQ:

Corollary 5. *Let HC production costs and gross utility be linear in h , so that $c(h; \theta) = \theta(h - \underline{h})$, and $u(s, h) = sh$. Moreover, assume $F_{\mathcal{D}}$ LR dominates F_A and let $\tilde{\theta} \in (\underline{\theta}, \bar{\theta})$ denote the unique crossing of the cost densities where $f(\tilde{\theta}) = f_A(\tilde{\theta}) = f_{\mathcal{D}}(\tilde{\theta})$. Then there exist crossing points $\check{\theta}_A, \check{\theta}_{\mathcal{D}} \in (\underline{\theta}, \tilde{\theta})$, such that*

- (i) $h^*_{\mathcal{D}}(\theta) < h^*(\theta)$ for all $\theta < \check{\theta}_{\mathcal{D}}$ and $h^*_{\mathcal{D}}(\theta) > h^*(\theta)$ for all $\theta > \check{\theta}_{\mathcal{D}}$, and
- (ii) $h^*_A(\theta) > h^*(\theta)$ for all $\theta < \check{\theta}_A$ and $h^*_A(\theta) < h^*(\theta)$ for all $\theta > \check{\theta}_A$.

Corollary 6. *Assume the same conditions as in Corollary 5. Then under the RQ policy (relative to the CB policy), typical disadvantaged students—that is, group \mathcal{D} students with costs $\theta \in \{(\check{\theta}_{\mathcal{D}}, \tilde{\theta}] \cup T_{\mathcal{D}}\}$ —exert higher effort and accumulate more HC. Moreover, if we define $d : (\check{\theta}_{\mathcal{D}}, \tilde{\theta}] \cup T_{\mathcal{D}} \rightarrow \mathbb{R}$ as the difference on this set between group \mathcal{D} investment under RQ versus CB, or*

$$d(\theta) \equiv (h^*_{\mathcal{D}}(\theta) - h^*(\theta)),$$

then $d(\theta)$ is strictly positive and attains a maximum on the interval $(\check{\theta}_{\mathcal{D}}, \tilde{\theta})$.

7.2.2. Example 2: Curved Utility. Proving qualitative predictions for the entire Cobb-Douglas utility family is more difficult, so in this section we present some simple numerical examples to further illustrate the model. $\Theta_{\mathcal{D}}$ and Θ_A once again follow normal distributions truncated to a common support $[\underline{\theta}, \bar{\theta}] = [1, 2]$. They both have the same variance parameter, $\sigma_A = \sigma_{\mathcal{D}} = 0.25$, but different mean parameters, $\mu_A = 1.1$ and $\mu_{\mathcal{D}} = 1.5$, which ensures LR dominance (see Figure 8). The group \mathcal{D} mass is $\delta = 0.5$.

We numerically solve for equilibria in two examples. In the first (Example 2), HC factors relatively heavily into match utility, with $\alpha = 0.15$ and $\beta = 0.75$. In the second (Example 3), students care less about their own HC and more about the quality of the institution they attend, with $\alpha = 0.75$ and $\beta = 0.15$. Equilibrium outcomes are summarized in figure 9.

The patterns arising from these examples are similar to predictions under the linear model. There continues to exist a single crossing point between the investment functions of the two groups under AA vs. CB. Under AA, the highest ability disadvantaged students and the lower ability advantaged students decrease HC investment, while the

FIGURE 8. NUMERICAL EXAMPLES:
Cost Densities

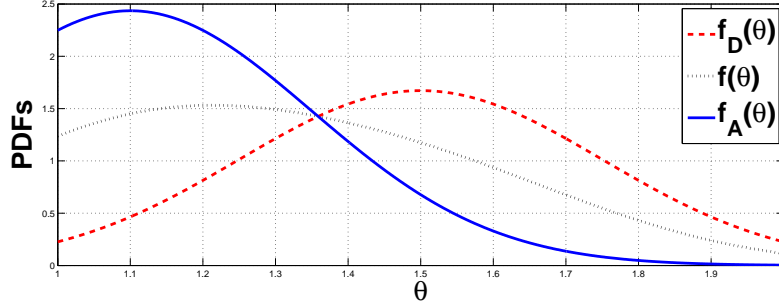
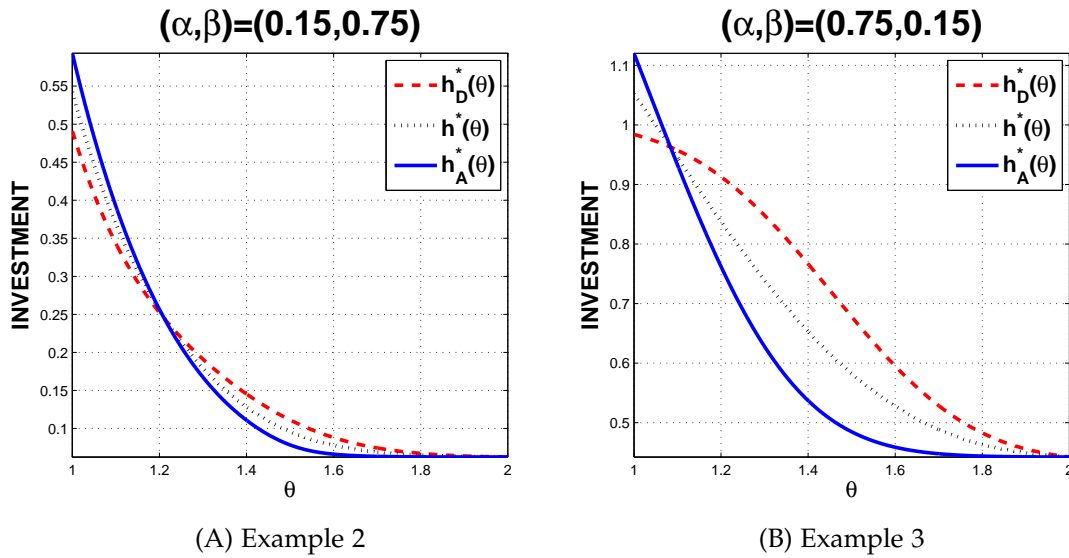


FIGURE 9. NUMERICAL EXAMPLES:
Investment, CB vs AA



lower ability disadvantaged students and the higher ability advantaged students increase HC investment. In both examples average investment in group \mathcal{D} rises; for group \mathcal{A} it rises in Example 2 and falls in example 3.

7.3. TIME TRUNCATION RULE. Time on our website was measured at the page level for each attempt of a quiz by each student. Pages contain blocks of either 3, 4, or 5 questions, so we divided each block-level time observation by the number of questions in order to get a measure of time spent per question. One difficulty arose in that there were a small number of clear instances where students left the website in the middle of a quiz for several hours or more. For example, the largest recorded time spent on a single question was 2,801 minutes, or roughly 47 hours. In order to correct this problem, a small number of implausibly large time observations needed to be corrected. After selecting a

truncation point on the time-per-question domain, we replaced each observation above that point with the student-specific censored mean of time per question. For example, suppose that Tommy attempted 11 questions with observed times of 5 minutes for the first five, 15 minutes for the next five, and 300 minutes for the last, and suppose that the truncation point were 30 minutes per question. Then the eleventh observation of 300 minutes is replaced by Tommy’s idiosyncratic censored mean of 10 minutes.

In order to select an appropriate truncation point we looked for occurrences of “holes” in the support of the distribution of times per question, or in other words, points at which a full support condition fails. We began with a natural assumption on the student type distribution that there are no interval subsets of the support where the type density assigns zero mass to the entire interval. If this condition holds, then since time spent on a question is a continuous choice related to one’s type, that distribution should also have full support too. That is, unless some observations reflect a different data generating process, say time elapsed outside of learning activity due to work stoppages. Thus, a straightforward way to search for spurious time observations is to sort the data and look for points at which a kernel smoothed density estimate (KDE) equals zero for some interval of positive length. This idea gives rise to the following data-driven algorithm for selecting a truncation point:

- (1) Sort all time observations from least to greatest, so that the j^{th} and $(j + 1)^{\text{st}}$ observations are ordered by $t_j \leq t_{j+1}$ for all $j = 1, \dots, J$.
- (2) Using the sample $\{t_j\}_{j=1}^J$, compute an appropriately chosen bandwidth b_1 for a KDE based on a kernel function with support on $[-1, 1]$.²⁴ Then find the smallest $j_1^* < J$ such that $t_{j_1^*+1} - t_{j_1^*} > 2b_1$. If no such j_1^* exists, then stop; no truncation is needed.
- (3) Define initial truncation point $\tau_1 \equiv t_{j_1^*} + b_1$, and compute bandwidth b_2 for the KDE based on the censored sample $\{t_j\}_{j=1}^{j_1^*}$.
- (4) In each subsequent iteration $k = 2, 3, \dots$, if there exists j_k^* defined by

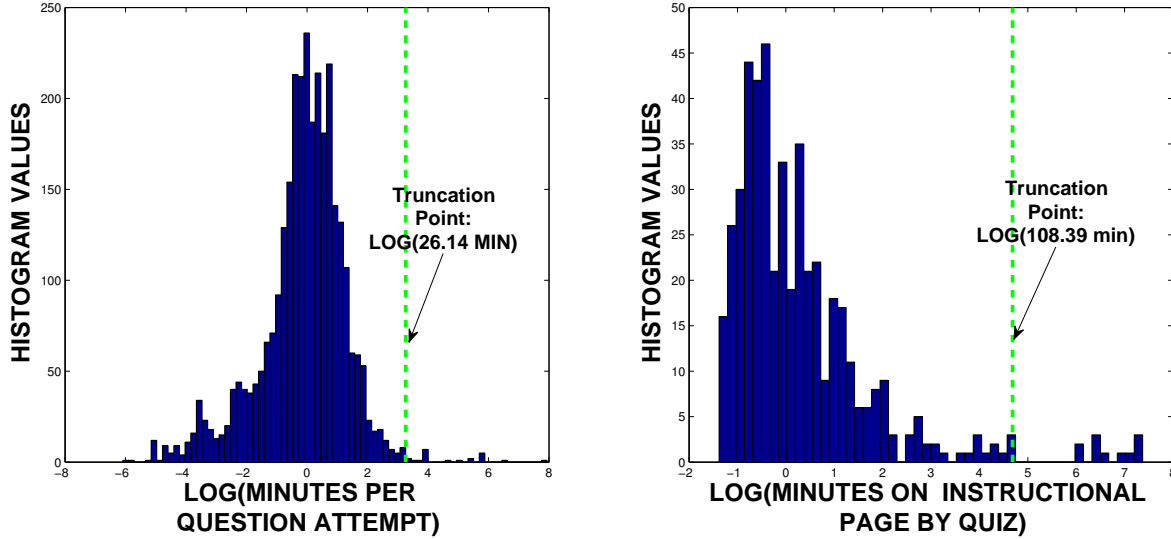
$$j_k^* \equiv \min\{j : t_{j+1} - t_j > 2b_k; j < j_{k-1}^*\},$$

then update the truncation point by $\tau_k \equiv t_{j_k^*} + b_k$, and re-compute bandwidth b_{k+1} for the KDE based on the censored sample $\{t_j\}_{j=1}^{j_k^*}$.

- (5) Stop once k is found such that j_{k+1}^* does not exist (meaning that for the censored sample $\{t_j\}_{j=1}^{j_k^*}$ a KDE is strictly positive everywhere).

²⁴Actually, the only crucial condition here is that the kernel function have bounded support. For example, in this context a Gaussian kernel would not do, as it places positive mass on the entire real line for any dataset. This would be equivalent to assuming full support *ex ante*.

FIGURE 10. TIME TRUNCATION RULE



(A) This panel displays a histogram of observed time spent on each question. Each datum in the histogram is a student-question-attempt observation.

(B) This panel displays a histogram of time per instructional page view. Each datum in the histogram is a student-quiz-attempt observation.

We chose a KDE based on the Epanechnikov kernel, which is known to be marginally more efficient than other kernel functions. This choice, in combination with Silverman's automatic bandwidth selection rule, implies a bandwidth formula of $b_1 = 2.345\hat{\sigma}_1 J^{-1/5}$ in the first iteration, and $b_k = 2.345\hat{\sigma}_k(j_{k-1}^*)^{-1/5}$ in the k^{th} iteration ($k \geq 2$), where $\hat{\sigma}_k$ is the sample standard deviation within the k^{th} iteration. Notice that the algorithm does not actually require computation of a KDE at each iteration, only a bandwidth, though choice of the specific kernel is needed to pin down the leading constant on the bandwidth selection rule.

Executing this process on our data leads to a final truncation point of $\tau_2 = 27.81$ minutes per question (the 99.35th percentile of the un-censored sample), after 2 iterations. Figure 10 displays a histogram of time spent per question, including observations above and below the truncation point. Time units are depicted in logs rather than levels for ease of visualization since the largest and smallest observations differ by several orders of magnitude.

7.4. ADDITIONAL FIGURES. Here we present some additional figures depicting the empirical distributions of investment activities by group and treatment status. In interpreting these figures, one caveat should be kept in mind. Corollary 2 only directly applies to the plots in Figures 5 – 6, since these depict CDFs of exam scores, the variable

FIGURE 11. TIME SPENT:
CB vs. *RQ*

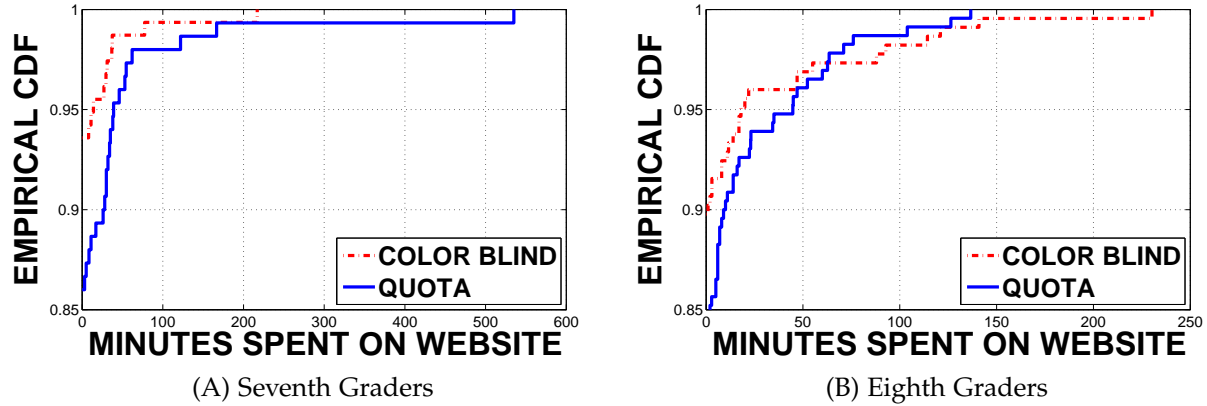
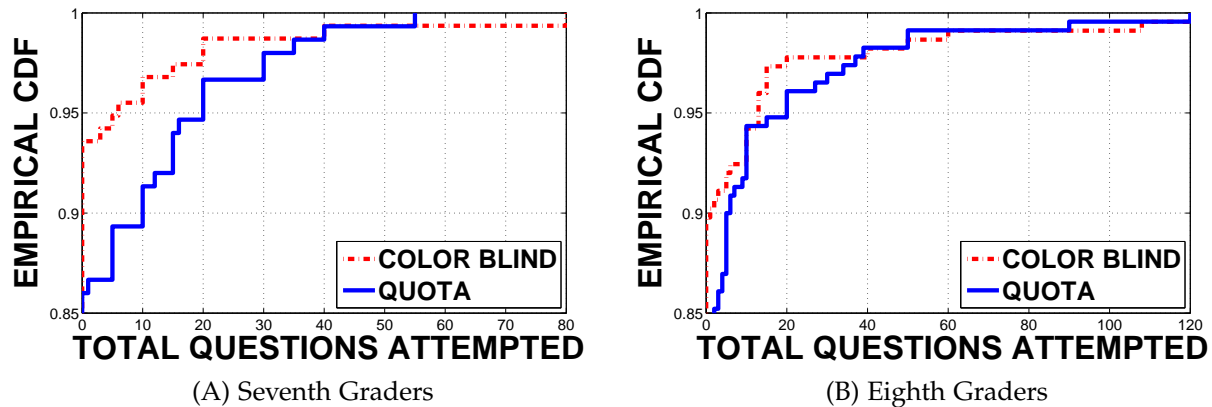


FIGURE 12. QUESTION ATTEMPTS:
CB vs. *RQ*



being directly incentivized within the experimental study. Thus, theory predicts that those plots should qualitatively resemble the patterns in Figures 3 and ???. It has nothing directly to say about other intermediate variables such as time spent on the website, or number of questions attempted, as these may combine in different ways for different agents to produce exam scores. However, for illustrative purposes, we present additional CDF plots in Figures 11 – 14 here.

FIGURE 13. 7th GRADE BY TREATMENT:
Pre-Test vs. Final Exam

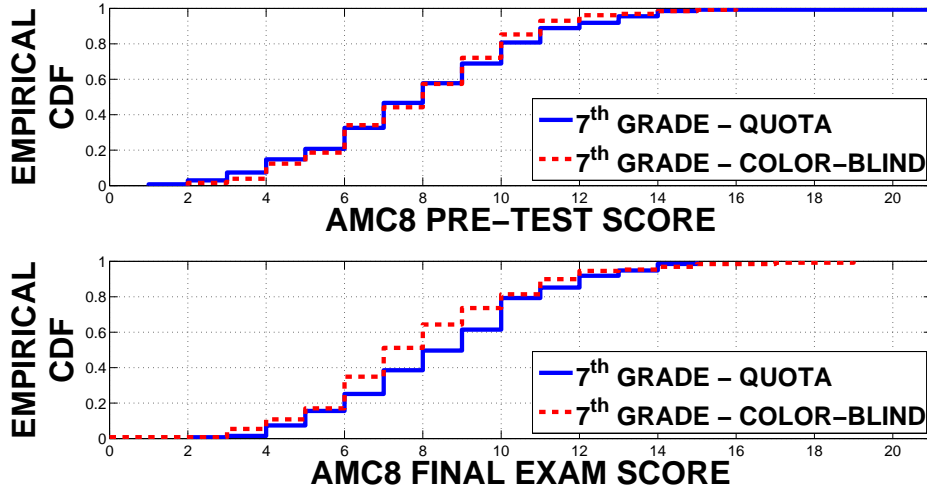


FIGURE 14. 8th GRADE BY TREATMENT:
Pre-Test vs. Final Exam

