CLASS PEERS AS COMPETITORS AND EDUCATORS: THE CONSEQUENCES OF RANK-BASED REWARDS IN US HIGH SCHOOLS

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Working Paper 31135
http://www.nber.org/papers/w31135

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
April 2023, revised September 2024

This paper previously circulated as 'Classroom Competition, Student Effort, and Peer Effects.' Support from the China National Natural Science Foundation (Grants #71874144 and #72150610505) is gratefully acknowledged. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 31135
April 2023, revised September 2034
JEL No. I21,I24,J15

ABSTRACT

In U.S. high-schools important academic rewards such as admission to select colleges, financial aid, and honors, are based on ranks in grade. We examine theoretically and empirically the consequences of rank-based competition in schools using game-theoretic tournament models with heterogeneous peer competitors and peer learning effects. We use the models to derive testable implications for how changes in the ability composition of students in the same grade affect both a student's exam outcomes and effort by ability type. To test the models, we use data from a nationally-representative panel of high-ability U.S. high school students with information on homework time, class attendance and test scores along with information on the grade-competitiveness of their schools merged with administrative data on the initial location assignments of Southeast Asian refugees. We find evidence in accord with the models that student peers are both competitors and educators, with student effort and peer assistance reduced in schools emphasizing grade competition and an increase in the number of strong refugee students decreasing effort but increasing the test scores among native-born strong-student incumbents.

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A appendices is available at http://www.nber.org/data-appendix/w31139

1. Introduction

In recent years there has been important progress in understanding the determinants of early childhood development (e.g., Cunha and Heckman, 2007; Cunha *et al*., 2013; Del Bono *et al*., 2016; Heckman *et al*., forthcoming). The focus in these studies is appropriately on the early home environment and parents as educators of children, with children having little or no agency. For adolescents, however, the locus of human capital investment is the school, and while parents still play a role, the educators now include teachers and potentially class peers. Moreover, the students themselves have agency, make effort choices, and these are influenced by the rules in the educational systems determining the rewards to effort.

A salient feature of schools is that rewards are based on student ranking, usually at the grade level. In high schools, among the most important rewards are financial aid and admission to colleges and universities, many of which explicitly use class rank as a determinant of admission and financial assistance.[1] When ranks are used as reward criteria, student effort is likely affected. Indeed, Fang *et al*. (2020) demonstrate that with convex effort costs, any system that allocates prizes based on one's relative standing will reduce effort.[2] In the school context, rank-based rewards thus may not only reduce student effort but may also turn class peers from educators into competitors (Drago and Garvey, 1998).[3] As pointed out in Chen and Hu (forthcoming), the large number of empirical studies examining class peer effects assume that student peers are either educators who aid other students in learning or are disrupters, thereby

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[1] The latest (2002) administrative data on admissions and test scores for 1,991 US colleges and universities from the Integrated Postsecondary Education Data System (IPEDS) of the National Center for Education Statistics [https://nces.ed.gov/ipeds/datacenter/DataFiles.aspx?year=2022&sid=90c08118-40e7-4c3a-8049-53d2324eb18b&rtid=7] indicate that 61% of students are enrolled in a post-secondary institution that considers class rank for admission (48% of all post-secondary schools). Of the schools that admit less than 50% of applicants, 65% use class rank as an admission criterion, weighted by enrollment (55% of these schools). All of the private colleges and universities ranked in 2024 by Forbes to be in the top 15 of all public and private colleges and universities in the United States used class rank as a criterion for admission (all eight Ivy League schools plus Stanford, MIT, Chicago, Johns Hopkins, Rice and Vanderbilt) [https://www.forbes.com/top-colleges/]. Based on our search of the web sites of the state university systems in all 50 US states for the academic year 2023-24, nineteen systems explicitly indicated they used class rank for either admission or financial aid, covering more than 50% of all US high-school students. For example, 2009 legislation in Texas allocated automatic admission for 75% of available slots in the flagship University of Texas at Austin to all students based on class rank. In 2023-24, the criterion was top 6%.

[2] Xu and Pak (2021) analyzed the comparative statics of a contest model with symmetric input functions and resource constraints but in a context in which losing is so catastrophic that an interior equilibrium does not exist, and the equilibrium effort is always increasing.

[3] In courses that grade on a curve, fellow students in the classroom are also transformed into competitors such that better peers mechanically lower a student's grade even in the absence of effort effects (Calsamiglia and Laviglio, 2019).

ignoring the competitive aspects of schools.[4] In general, while most empirical studies of peer effects are successful in establishing causation, as pointed out by Kremer and Levy (2008) many studies cannot identify the specific mechanisms by which peers affect student achievement. This is particularly true in the absence of information on student behavior. Chen and Hu make some headway in identifying one of the mechanisms from their quasi-experimental evidence that among students who are more likely to be in competition, there is less mutual assistance and thus smaller positive peer effects on academic outcomes.

While there is a growing consensus in the peer effects literature that peer effects depend on context (Sacerdote, 2014), almost all peer effects studies, including Chen and Hu, also ignore that the characteristics of peers may affect a student's effort when class rank matters, as implied by Fang *et al*. Studies of peer effects that focus on student effort when class rank is extrinsically rewarding, however, are few and hampered by the absence of direct measures of student effort.[5] Important contributions include Grau (2019), who develops and structurally estimates a rank-order tournament model to explicitly assess how college admission policies affect student effort. However, in that model student strategic behavior is ignored. Tincani (2017, 2024) uses the status-good model of Hopkins and Korienko (2004), which, adopted to schools, assumes that students intrinsically value both their rank and their absolute performance to explain her empirical results showing that exogenous changes in the relative effort costs of students stratified by ability affect both test scores and class rank, but her data do not have measures of student effort. These studies of the role of ranks in schools, however, ignore the role of peers as educators and how competition affects their role as found by Chen and Hu.

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[4] They cite Sacerdote (2001); Zimmerman (2003); Stinebrickner and Stinebrickner (2006); Lyle (2007); Carrell, Fullerton and West (2009); Imberman, Kugler and Sacerdote (2012); Abdulkadiroglu *et al*. (2014); Booij, Leuven and Oosterbeek (2017); Feld and Zölitz (2017); Zarate (2023) as examples of studies where peers are educators and Figlio (2007); Kling *et al*. (2007); Carrell *et al*. (2008); Gould *et al*. (2009); Carrell and Hoekstra (2010); Lavy and Schlosser (2011); Carrell *et al*. (2018) as examples of showing peers as disrupters.

[5] Some empirical studies of sports events show that rankings-based rewards affect effort. For example, Genakos and Pagliero (2012) examine weight-lifting contests to show how revealing interim ranks affects both risk-taking and performance. Brown (2011) used the results of Stein's Nash tournament model to inform her empirical work on the effect of superstars on the effort levels of the competitors in golf tournaments. Guryan *et al*. (2009), however, find no peer effects in golf tournaments based on the random assignments of players differentiated by ability. The findings in Hickman and Metz (2018) from golf tournament data suggest that this may be because there are offsetting positive learning and negative competition effects.

In this paper we examine theoretically and empirically the consequences of competition in schools for student effort and performance while allowing class peers to also directly affect learning. Peers are thus both competitors, due to the reward rules, and educators augmenting the educational returns to effort by their fellow students. We use game-theoretic tournament models with heterogeneous competitors and peer learning effects to derive testable implications for how changes in the ability composition of students in the same grade or class cohort affect both a student's exam outcomes and effort by ability type.[6] The theoretical part of our work relates to the literature on contests with asymmetric competitors, most closely with Stein (2002) and Cornes and Hartley (2005). As noted, tournament models generally indicate that competition, which arises when rewards depend on one's performance relative to others, reduces individual performance. Stein derived the Nash equilibrium for linear input functions and showed that the equilibrium effort of all the contestants, except possibly the strongest one, weakly decreases when one of the competitors becomes stronger.

In our model, we provide for the first time comparative statics results for the general input functions considered in Cornes and Hartley (2005), and with a weaker assumption about cost curves than in Fang *et al*., but we also consider a model with more sophisticated heterogeneous agents who anticipate their peers' responses. In this model students take into account that their alike peers will always match their own effort, reducing even more the marginal return to effort relative to that in Nash-equilibrium games in which agents assume that their peer's effort remains fixed. The key point is that in a context in which rewards are based on relative performance increasing effort has little return if one knows that that peer students will also match that effort increase. In this non-myopic model, changes in class composition have differential effects on students depending on their own capability as found in most of the newer

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[6] Fang *et al*. (2020) obtains their general result from a model with homogeneous competitors. In contrast to the model of Hopkins and Korienko (2004) developed by Tincani (2024) to describe student responses to changes in the composition of the effort costs of students, in tournament models rank is not a status good but matters instrumentally because it affects the probability of rewards that are valued. There is evidence that even if rank is not instrumental, however, it is intrinsically valued by students (Tran and Zeckhauser, 2012). And, outside of schools, there is evidence that personal relative standing unrelated to extrinsic rewards can affect effort, risk-taking and outcomes, e.g., Ager *et al*. (2002). But we show that in addition to our finding that being exposed to more close competitors decreases effort as predicted by tournament models with extrinsic rewards, variation in the degree of competition matters, and not all students behave as if valuing rank.

empirical studies of school peer effects but unlike in the predictions of the Nash-equilibrium model.

We focus on the effort response of high-ability students, who we think are likely to believe they can win rewards in a class competition, to changes in the ability composition of students in the same grade in a school.[7] The key predictions of the model in which students anticipate the matching of effort is that an increase in the number of strong students decreases the effort of the strong incumbent students, similar to the Nash equilibrium. This result, however, is in contrast to the status good model of Hopkins and Korienko in which, as shown by Tincani (2024), the returns to effort rise when there is a higher proportion of stronger students because a unit increase in effort improves class rank more strongly. The non-myopic or effort-matching tournament model also predicts that an increase in the number of weaker students increases the effort levels of the strong incumbent students, in contrast to the Nash equilibrium, because the effort of weaker students does not fully match those of the strong students, and they decrease winning probabilities less than does an increase in the number of strong students.

We also extend the model to allow higher-ability peers to directly increase a student's achievement, especially among those who are higher ability (as found by Chen and Hu) and assess the robustness of the comparative static results for strong-student effort when weak students do not compete and drop out. When we add to the model the educator role of students – the possibility of strong peers augmenting directly the returns to students' effort particularly among those who are stronger, as is consistent with Chen and Hu's empirical findings of who helps whom - we find that the effort comparative statics for strong students are unchanged. However, the test scores of strong incumbents may rise or fall when new entrants are high-ability depending on whether the positive learning (educator) peer effect on test scores dominates the negative competition effect on effort. This then establishes a test for peers as educators and competitors. The dual roles are exposed when among the stronger incumbent students test scores rise while effort levels fall when there is an increase in the number of high-ability students.

To test the model, we use as our main data set the Restricted-use U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of

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[7] We show empirically that only the effort of strong students, defined by their parental background, responds to the composition of same-grade peers.

1988 (NELS:88).[8] The NELS:88 is a nationally-representative survey of U.S. students starting with a sample of over 25,000 8th-graders in 1988 a subset of whom were additionally surveyed in 1990 and 1992, the high school years. The key advantage of this survey is that it includes information in all three rounds on students' weekly homework hours and frequency of class skipping in addition to nationally standardized test scores in mathematics and reading. Thus, we have, unlike in almost all data based on school administrative records, direct measures of both student effort and individual scholastic achievement. In addition, the survey data, based on responses of teachers in the schools, identifies the degree to which high schools encourage grade competition, which allows us to assess directly how competition affects both student effort and peer assistance.

We first use all three rounds of the panel sample to estimate a parameterized version of the effort function that is the core of tournament and status good models. We do this first to identify if there are indeed payoffs to our two measures of student effort in terms of test scores and test score value-added. We find, taking into account measurement error in the effort variables and effort being affected by unobserved ability and shocks to previous test score outcomes, that both homework time (positive) and class skipping (negative) have significant effects on test scores. While this is an unsurprising result, there is in fact scant prior credible evidence on how or whether effort affects test scores.[9] Moreover, our findings provide evidence that because test scores endogenously reflect effort, stratification of students by test scores or lagged test scores to assess behavioral effects by ability can result in choice-based sample bias.[10]

Based on the structural estimates of the achievement function we also derive estimates of individual student ability net of effort, which we first use to demonstrate that our assumption that more able students expend more effort is evident in the data. Leveraging the variation across schools in the degree to which grade competition is encouraged, we also verify (i) the fundamental prediction of tournament models that increased competition reduces student effort particularly among high-ability students and (ii) increased grade competition also reduces the

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[8] A condition of the use of these data is that reported sample sizes must be rounded so that the last digit is zero. All sample sizes in the tables in this paper conform to this restriction.

[9] We discuss the prior literature below.

[10] Conley *et al*. (2022) find that homework habits persist over time. Thus, lagged test scores are also endogenous to current effort.

amount of assistance from classmates to high-ability students, consistent with the findings in Chen and Hu.

A weakness of the NELS:88 data is that is does not have information on the composition of students at the school grade level. To test the main comparative static predictions of the models therefore we append to the restricted-use NELS:88 data at the county level information from the newly-available data (Dreher *et al*., 2020) on the assigned initial county locations of all refugees who entered the United States between 1975 and 2008 from the Office of Refugee Resettlement (ORR) and data from the 1990 Public-Use Census micro data. From both data sets we extract the number of Southeast Asian Refugees by education, age and county location. We chose Southeast Asian (SEA) refugees because there are large numbers of these refugees in the years prior to and overlapping the NELS:88 panel. Despite the aggregation of the treatment variables to the county level, we are able to replicate the findings from studies using well-identified peer treatment effects at the school-grade level (Tincani (2024) on test score effects) and in the dorm room (Chen and Hu (forthcoming) on the effect of competition on peer assistance).

Our main identification strategy that takes into account the endogeneity of the location of SEA refugees uses the panel of native-born students followed from 10th grade to 12th grade and who did not change schools, employing first-differenced instrumental variables. Less than 3% of high-ability students changed schools between those grades, so this is not a selective sample biased by school-flight effects.[11] With this sample, differencing not only eliminates all time-invariant characteristics of the students but also those of the schools that might influence both SEA refugee location assignments and student behavior. Tests of the model are derived from how the change in the number of assigned high-and low-ability refugee students affects changes in the two effort variables of the native-born students and in their test scores.

A key challenge is to stratify the students by ability without using test scores, which we have found are endogenous to effort. We first divide up the students by whether or not both parents achieved schooling above high school. Strong students, both native-born and SEA refugees, defined in this way, have significantly higher test scores than weak students, spend

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[11] Boustan *et al*. (2024) find that school enrollment declined in higher-income areas of California due to the entry of Asian immigrants which the authors attribute to parents fearing increased competition.

more hours doing homework and skip classes less. All of our tests of the models use this stratification criterion, which we also assess using semi-nonparametric methods that do not impose a threshold.

The main results are obtained using both the NELS:88 panel data on high-ability native-born students combined with ORR refugee data and cross-sectional data for 10th-grade native-born high-ability students using the combined 1990 Census and the NELS:88 data. For both merged data sets we use the past cumulative county assignments of SEA refugees as instruments to predict the 1990 distribution of 10th-grade SEA students and the change in number of SEA refugee students from 1990 to 1992 (10th- to 12th-grade). For both sets of merged data we find, in accordance with the competition model, that an increase in strong SEA refuge students significantly reduces the weekly homework time of the strong native-born students, while an increase in the weak SEA refugee students increases homework time among the strong native-born students. We also find that the effort levels of weak native-born students do not respond to the changes in the composition and numbers of SEA refugee students.

To verify that our ability stratification criterion is sensible, we then use a nonparametric estimator based on the sum of the schooling years of the parents across all students to show that the predictions of the competition model indeed only hold for students at high values of parental schooling. That is, only at high values of parental schooling do we see significant responses of native-born student effort to the changes in the composition of refugee students in accord with the competition model. At low values of the parental schooling measure native-born students do not respond at all, in accord with our original stratification. The absence of response at the low end of the parental schooling distribution is consistent with the selectivity of most educational awards, in which not all students have a reasonable chance of winning (e.g., top 10%, top 5%). Our estimates suggest that between 14% and 36% of student behave as if they are competing.

We then use the same estimation procedure applied to the panel data to identify the effect of the SEA refugee students by ability on standardized test scores. Here we find that additional high-ability refugee students increase the test scores of the high-ability native-born. This result is in accord with Tincani (2024). However, our finding that additional high-ability refugee students reduce the effort levels of the native-born high-ability students rejects the rank-as-status model used by Tincani for describing rank-based behavior in U.S. high schools, where competition for extrinsic rewards may be more salient than in the Chilean grade schools.[12]

Our test score finding also suggests that the strong SEA students act as educators in addition to being rivals - the educational effect of strong peers on average dominates the negative competition effect. Consistent with this we find that the positive test score effect is only observed in schools that do not or only weakly encourage grade competition. There is, in contrast, no positive effect of the presence of higher-ability peers on test scores in schools that strongly encourage grade competition. The results are again in accord with Chen and Hu's finding that competition reduces peer learning, but our finding on effort suggests an additional mechanism by which competition that makes strong students rivals rather than educators, the reduction in own student effort.

To additionally confirm the existence of peer learning effects, we re-estimated the achievement function adding strong SEA students as a TFP variable. The estimates of homework time and class skipping on test scores and test score value-added are unchanged, but we find that, net of a student's effort, adding strong SEA refugees significantly increase her test score. Finally, we find that the addition of SEA refugee students of either ability does not increase class disruptions and that parents attempt, but only in the strong grade competition schools, to offset the lower effort levels of their children when faced with more competitors by providing more homework assistance.

Our findings that classmates are both competitors, induced by rank-based rewards, and educators with positive learning externalities has implication for educational policy as well for why there are mixed findings on the effects of peers on school performance. For example, the use of tracking in schools, separating students by ability, while perhaps reducing learning among lower-ability students (Duflo *et al*., 2011), would enhance learning among high-ability students, who benefit most from strong peers apart from a more focused-curriculum. Tracking does not reduce effort due to competition effects because most rank-based rewards are based on all

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[12] Tincani (2017, 2024) could not assess effort responses because, as in many studies of peer effects, the data used are from school administrative records, which do not provide information on student effort.

students in a grade, not within courses or classrooms. But these potential gains would be lost due to additional negative competition effects if courses are graded on a curve.

Our results also have implications for the impact on students who change schools.[13] For example, Abdulkadiroglu *et al*. (2014) find that shifting a student from a school with lower-ability students to one with high-ability students did not increase the test scores of the migrant student. Hoekstra *et al*. (2018) also find no evidence of peer effects from a person attending a school with more able peers. Our findings suggest that these null results do not imply that there are no positive learning effects from stronger peers in such schools, because, as our results show, when there is competition, the increase in the strength of peers would also reduce own effort. Finally, that competition based on ranks blunts peer learning also has implications for the assessment of grade inflation and in which schools this phenomenon is likely to occur, which we discuss in the conclusion.

2. Modeling In-school Class Competition

a. *General setup.* The models we employ are based on Tullock (1980)'s model of imperfectly discriminating competition, in which a population of N individuals, $i = 1,\dots,N$, compete with each other for a prize by exerting effort, $e_i \geq 0$. An individual's (expected) utility is

$$u_i(e_1,\dots,e_N) = v_i P_i(e_1,\dots,e_N) - c_i(e_i),$$

where $v_i > 0$ is individual i's valuation of the prize, $P_i(e_1,\dots,e_N)$ is the probability of obtaining the prize, and the twice-differentiable function $c_i(e_i)$, with $c_i(0) = 0$ and $c'_i > 0$, is the cost of effort.

Effort translates into individual achievement, as described by the individual achievement function $f_i(e_i)$, with $f(0) = 0$ and $f'_i > 0$, and twice-differentiable. In the contest literature, $P_i(e_1,\dots,e_N)$ is often called the contest success function and takes the following general form:

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[13] Cullen *et al*. (2013) find that the introduction of the use of class rank as admission criterion at the University of Texas led to stronger students shifting to schools with under-performing students. They examined the impact of school shifting on the composition of the admitted college class but did not assess the impact on the learning of students who changed schools or on the incumbent students in those schools.

$$P_i(e_1, \ldots, e_N) = \frac{f_i(e_i)}{f_1(e_1) + \cdots + f_N(e_N)} \text{ if } \sum_k f_k(e_k) > 0.$$

We assume that $P_i(e_1, \ldots, e_N) = 0$ if $\sum_k f_k\,(e_k) = 0$ so that competitors cannot succeed without exerting some effort. The key point is that the probability of success is increasing in one's own effort and decreasing in others. The traditional interpretation of this model is that of a winner-take-all contest, in which contestants compete for a single prize. However, our application of the model, which is that of students striving to achieve an educational success in a competitive environment, does not require such an interpretation, and we show in Section 2.c that our non-myopic model can be interpreted as a competition with $M \geq 1$ expected number of winners, provided that M is not unreasonably large.

A key component of our models is that students are heterogeneous – some students are stronger than other students, which we precisely define. To keep the model simple and directly related to our empirical work, we assume that the population of students is divided into two groups, with N_1 individuals in group 1 and N_2 individuals in group 2, and that members of the same group have identical achievement and cost functions. In particular, we say that a student is stronger than another student if she values success more, has a weakly higher marginal productivity of effort, and has a weakly lower marginal cost of effort. More precisely, we have the following definition.

Definition 1. Student i is said to be stronger than student j if $v_i > v_j$, $f'_i \geq f'_j$, and $c'_i \geq c'_j$. Group i is said to be stronger than group j if all members of group i are stronger than members of group j.

Because there are only two groups and the members of a group are identical, we assume without loss of generality that group 1 is stronger than group 2 and refer to group 1 and its members as strong and group 2 and its members as weak.

We will also assume that the members of the same group exert the same effort level in "equilibrium." It is easy to show that in equilibrium a strong student exerts a greater effort than a weak student as expected. Thus, the strong group consists of high achievers who have a greater chance of succeeding than weak students.

There are, however, multiple senses in which an equilibrium can be defined, and we consider two. The equilibrium we briefly consider arises from supposing that individuals take their competitors' efforts as fixed when they are setting their effort level. That is, the individuals do not think that their competitors will react to their effort level. The equilibrium that arises from this assumption is the classic Nash equilibrium. The second one arises from supposing that individuals have enough foresight to think that their effort level will have an influence on their competitors. In particular, we suppose that individuals think that their effort levels will be matched by the individuals in their own group (but not by the members of the other group). The equilibrium that arises from such an assumption is similar to the "collusive" equilibrium in the IO literature. Since we are not proposing that the students are consciously colluding, we call this equilibrium the "non-myopic equilibrium."

b. Nash equilibrium. A Nash equilibrium is an effort profile $(e_1^*, \dots e_N^*)$, where e_i^* maximizes $u_i(e_i, e_{-i}^*)$ for all i. In our setting, we say that it is symmetric if $e_i^* = e_k^*$ for all i and k in the same group. To characterize the Nash equilibrium, we start with the individual's optimal effort problem. The first and the second order conditions for the problem are:

$$\text{FOC:} \quad \frac{\partial u_i(e_1,\dots,e_N)}{\partial e_i} = v_i \frac{\partial P_i(e_1,\dots,e_N)}{\partial e_i} - c_i'(e_i) \text{ is } \begin{cases} \le 0 & \\ = 0 & \text{if } e_i > 0 \end{cases}$$

$$\text{SOC:} \quad \frac{\partial^2 u_i(e_1,\dots,e_N)}{\partial e_i^2} = v_i \frac{\partial^2 P_i(e_1,\dots,e_N)}{\partial e_i^2} - c_i''(e_i) \text{ is } \begin{cases} \le 0 & \text{(necessary)} \\ < 0 & \text{(sufficient).} \end{cases}$$

In a Nash equilibrium, we are assuming that a student treats her competitors' efforts as fixed when she is choosing her own effort level. Thus, the derivative of the contest success function with respect to e_i is:

$$\begin{aligned} \frac{\partial P_i(e_1,\dots,e_N)}{\partial e_i} &= \frac{\partial}{\partial e_i}\left(\frac{f_i(e_i)}{\sum_k f_k(e_k)}\right) = \frac{f'_i(e_i)\sum_{k\neq i} f_k(e_k)}{\left(\sum_k f_k(e_k)\right)^2} \\ \frac{\partial^2 P_i(e_1,\dots,e_N)}{\partial e_i^2} &= \frac{f''_i(e_i)\left(\sum_{k\neq i} f_k(e_k)\right)}{\left(\sum_k f_k(e_k)\right)^2} - \frac{2f'_i(e_i)f'_i(e_i)\left(\sum_{k\neq i} f_k(e_k)\right)}{\left(\sum_k f_k(e_k)\right)^3}. \end{aligned}$$

Because we are concerned with the symmetric equilibrium, we only need to consider the representative individual in each group. Let $i, j \in \{0,1\}$, where $i \neq j$, index the two groups, and let e_i^* and e_j^* be the equilibrium effort level of the representative individual in the two groups, respectively. Let

$$\tilde{P}_i(e_i^*, e_j^*, N_i, N_j) = v_i \frac{\partial P_i(e_1, \dots, e_N)}{\partial e_i}|_{(e_i^*, e_j^*)} = \frac{v_i f_i'(e_i^*)\big((N_i - 1) f_i(e_i^*) + N_j f_j(e_j^*)\big)}{\big(N_i f_i(e_i^*) + N_j f_j(e_j^*)\big)^2}. \tag{1}$$

denote the marginal benefit of effort at the symmetric equilibrium. The second-order condition for the optimal effort problem is satisfied when $\frac{f''_i}{f'_i} \leq \frac{c''_i}{c'_i}$ (that is, the achievement function is weakly more concave than the cost function), and we will assume that this condition holds.[14] Then an equilibrium exists and is unique by Cornes and Hartley (2005).[15] Thus, assuming that valuations are sufficiently high so that students exert positive efforts in equilibrium, the equilibrium(e_1^*, e_2^*) is characterized by the first-order condition:

$$F(e_1^*, e_2^*, N_1, N_2) = \begin{bmatrix} \tilde{P}_1(e_1^*, e_2^*, N_1, N_2) - c_1'(e_1^*) \\ \tilde{P}_2(e_1^*, e_2^*, N_1, N_2) - c_2'(e_2^*) \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \end{bmatrix}. \tag{2}$$

We now consider how effort levels of the two groups change when new competitors enter the competition. That is, whether the incumbents will exert a higher effort to compensate for the increased competition or be discouraged by it and reduce their effort. Since the equilibrium effort levels and the group sizes form an implicit relationship, such comparative statics can be made by implicitly differentiating (2):

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[14] This condition is less restrictive than assuming that achievement is concave and cost is convex in effort.

[15] The essential requirement in Cornes and Hartley's existence proof is that the cost is an increasing and convex function of the input to the contest. That is, the cost of generating input $y_i = f_i(e_i)$ is given by $g_i(y_i)$, where $g(0) = 0$, $g'_i > 0$, and $g_i'' \geq 0$. To see that our assumptions satisfy these conditions, note that $f'_i > 0$, $c'_i > 0$, and $\frac{f_i''}{f_i'} \leq \frac{c_i''}{c'_i}$ imply that f_i is weakly more concave than c_i. Thus, there is an increasing and concave function ϕ_i such that $f_i = \phi_i(c_i)$. Moreover, ϕ_i has a convex inverse function. That is, $c_i = \phi_i^{-1}(f_i)$, where $\phi_i^{-1}$is a convex function. Therefore, $g_i(y_i) = c_i(f_i^{-1}(y_i)) = \phi_i^{-1}\big(f_i(f_i^{-1}(y_i))\big) = \phi_i^{-1}(y_i)$.

$$\begin{bmatrix} \frac{\partial e_1^*}{\partial N_1} & \frac{\partial e_1^*}{\partial N_2} \\ \frac{\partial e_2^*}{\partial N_1} & \frac{\partial e_2^*}{\partial N_2} \end{bmatrix} = -\left[D_{(e_1^*,e_2^*)}F(e_1^*,e_2^*,N_1,N_2)\right]^{-1}\left[D_{(N_1,N_2)}F(e_1^*,e_2^*,N_1,N_2)\right]$$

$$= \frac{-1}{\text{DET}}\begin{bmatrix} \left(\frac{\partial \tilde{P}_2}{\partial e_2^*} - c_2''\right)\left(\frac{\partial \tilde{P}_1}{\partial N_1}\right) - \left(\frac{\partial \tilde{P}_1}{\partial e_2^*}\right)\left(\frac{\partial \tilde{P}_2}{\partial N_1}\right) & \left(\frac{\partial \tilde{P}_2}{\partial e_2^*} - c_2''\right)\left(\frac{\partial \tilde{P}_1}{\partial N_2}\right) - \left(\frac{\partial \tilde{P}_1}{\partial e_2^*}\right)\left(\frac{\partial \tilde{P}_2}{\partial N_2}\right) \\ \left(\frac{\partial \tilde{P}_1}{\partial e_1^*} - c_1''\right)\left(\frac{\partial \tilde{P}_2}{\partial N_1}\right) - \left(\frac{\partial \tilde{P}_2}{\partial e_1^*}\right)\left(\frac{\partial \tilde{P}_1}{\partial N_1}\right) & \left(\frac{\partial \tilde{P}_1}{\partial e_1^*} - c_1''\right)\left(\frac{\partial \tilde{P}_2}{\partial N_2}\right) - \left(\frac{\partial \tilde{P}_2}{\partial e_1^*}\right)\left(\frac{\partial \tilde{P}_1}{\partial N_2}\right) \end{bmatrix}$$

Where $\text{DET} = \left(\frac{\partial \tilde{P}_1}{\partial e_1^*} - c_1''\right)\left(\frac{\partial \tilde{P}_2}{\partial e_2^*} - c_2''\right) - \left(\frac{\partial \tilde{P}_1}{\partial e_2^*}\right)\left(\frac{\partial \tilde{P}_2}{\partial e_1^*}\right)$.

Taking the required derivatives and determining their signs yield the results given in the following theorem.

Theorem 2. *Suppose* $N_i \geq 2$ *for all* i. *Then we have* $\frac{\partial e_i^*}{\partial N_j} < 0$ *for all* $i = 1,2$ *and* $j = 1,2$.

Theorem 2 states that in a Nash equilibrium framework, students do not respond by increasing their effort when new competitors enter the competition.[16] Everyone's equilibrium effort decreases no matter which group's size increases. Moreover, the result is robust and does not rely on any assumption on the value of success, the achievement function, or the cost function beyond the ones used to guarantee that a non-trivial equilibrium exists. The proof is in Appendix A4.

c. Non-myopic equilibrium. The underlying assumption in the non-myopic equilibrium is that students anticipate that their effort choice will be matched by their competitors. That is, if a student adjusts her effort from e_i to e', every other e_k, where k and i are in the same group, will change to e' as well. Formally, this means we can treat the contest success function as

$$P_i(e_i, e_j, N_i, N_j) = \frac{f_i(e_i)}{N_i f_i(e_i) + N_j f_j(e_j)},$$

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[16] The comparative statics in this paper are obtained in marginal terms, whereas group sizes change by a whole number. The comparative static for a whole number change can be found by integrating the marginal effect, which would not change the sign of the effect because the marginal effect has a constant sign.

where e_i and e_j, $i \neq j$, are the effort levels of the representative members of the two groups. The first and the second order condition for the optimal effort problems are:

$$\text{FOC: } \frac{\partial u_i(e_i,e_j,N_i,N_j)}{\partial e_i} = v_i \frac{\partial P_i(e_i,e_j,N_i,N_j)}{\partial e_i} - c_i'(e_i) \text{ is } \begin{cases} \leq 0 & \\ = 0 & \text{if } e_i > 0 \end{cases}$$

$$\text{SOC: } \frac{\partial^2 u_i(e_i,e_j,N_i,N_j)}{\partial e_i^2} = v_i \frac{\partial^2 P_i(e_i,e_j,N_i,N_j)}{\partial e_i^2} - c_i''(e_i) \text{ is } \begin{cases} \leq 0 & \text{(necessary)} \\ < 0 & \text{(sufficient)} \end{cases}.$$

The above expressions are similar to those in the Nash equilibrium setting. The crucial difference is in the derivatives of the contest success function:

$$\begin{aligned} \frac{\partial P_i(e_i,e_j,N_i,N_j)}{\partial e_i} &= \frac{f'_i(e_i)N_j f_j(e_j)}{\left(N_i f_i(e_i) + N_j f_j(e_j)\right)^2}. \\ \frac{\partial^2 P_i(e_i,e_j,N_i,N_j)}{\partial e_i^2} &= \frac{f''_i(e_i)N_j f_j(e_j)}{\left(N_i f_i(e_i) + N_j f_j(e_j)\right)^2} - \frac{2f'_i(e_i)N_j f_j(e_j)N_i f'_i(e_i)}{\left(N_i f_i(e_i) + N_j f_j(e_j)\right)^3}. \end{aligned}$$

Letting e_i^* and e_j^* be the equilibrium effort levels, let

$$\tilde{P}_i(e_i^*, e_j^*, N_i, N_j) = v_i \frac{\partial P_i(e_i,e_j,N_i,N_j)}{\partial e_i}\Big|_{(e_i^*,e_j^*)} = \frac{v_i f'_i(e_i^*) N_j f_j(e_j^*)}{\left(N_i f_i(e_i^*) + N_j f_j(e_j^*)\right)^2}, \tag{3}$$

denote the marginal benefit of effort at the equilibrium. As in the Nash equilibrium setting, the second order condition is satisfied when $\frac{f''_i}{f'_i} \leq \frac{c''_i}{c'_i}$, which we assume. Then, assuming that valuations are sufficiently high so that students exert positive efforts in equilibrium, the equilibrium (e_1^*, e_2^*) is characterized by the first order conditions:

$$F(e_1^*, e_2^*, N_1, N_2) = \begin{bmatrix} \tilde{P}_1(e_1^*, e_2^*, N_1, N_2) - c_1'(e_1^*) \\ \tilde{P}_2(e_1^*, e_2^*, N_1, N_2) - c_2'(e_2^*) \end{bmatrix} = \begin{bmatrix} 0 \\ 0 \end{bmatrix}. \tag{4}$$

As in the Nash equilibrium framework, comparative statics results can be obtained by implicitly differentiating the equilibrium condition (4). The results, however, are different. Unlike the Nash equilibrium where increasing the size of any group reduces the equilibrium effort of all groups, the cross effect in the non-myopic equilibrium is not always negative and depends on the relative strength (in the sense defined below) of the two groups.

Before presenting this result formally, we first show that our non-myopic model can be interpreted as a competition in which more than one student can succeed, provided that the expected number of successful students, M, is smaller than the group sizes. To see this, let $1 \leq M \leq \min\{N_1, N_2\}$, $\hat{v}_i = \frac{v_i}{M}$, and $\hat{u}_i(e_i, e_j, N_i, N_j) = \hat{v}_i \hat{P}_i(e_i, e_j, N_i, N_j) - c_i(e_i)$, where the success probability $\hat{P}_i$ is given by

$$\hat{P}_i(e_i, e_j, N_i, N_j) = \frac{M f_i(e_i)}{N_i f_i(e_i) + N_j f_j(e_j)}.$$

Then the expected number of successful students under $\hat{P}$ is $\sum_{i=1}^{N} \text{Prob}\ (i \text{ succeeds}) = \sum_{i=1}^{N} \hat{P}_i = M$.[17] Moreover, $\hat{u}_i(e_i, e_j, N_i, N_j) = u_i(e_i, e_j, N_i, N_j)$ and $\hat{P}_i$ is a constant multiple of P_i. Thus, the two specifications generate the same equilibrium and the comparative statics. With this interpretation in mind, we slightly modify the definition of a stronger competitor for the non-myopic model.

Definition 3. Group i and its members are said to be *stronger* than group j and its members, respectively, if $\hat{v}_i > \hat{v}_j$, $N_i \hat{v}_i > N_j \hat{v}_j$, $f_i{}' \geq f_j$, and $c'_i \leq c'_j$.

Because $N_i \hat{v}_i$ is the aggregate value of success to group i, group i is stronger if it values success more at both individual and group levels, has a weakly higher marginal productivity of effort, and has a weakly lower marginal cost of effort. As before, we assume that group 1 is stronger than group 2 and refer to group 1 and its members as strong and group 2 and its members as weak. It can be shown that as expected, strong students have higher achievement and expend more effort than weak students both individually, and in the aggregate enjoy a higher expected utility in equilibrium.

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[17] Note that $\sum_{i=1}^{N} \text{Prob}\ (i \text{ succeeds})$ need not equal one because one student succeeding does not necessarily preclude another student from succeeding.

The comparative statics results are formalized in Theorem 4 below, which shows that an entry of strong students discourages everyone, while an entry of weak students induces the strong incumbents to increase their effort and discourages the weak.

Theorem 4. With $\frac{f''_i(e_i)}{f'_i(e_i)} < \frac{c''_i(e_i)}{c'_i(e_i)}$ for all i, which guarantees that the second order condition for the optimal effort problem is satisfied, and with group 1 stronger than group 2, we have $\frac{\partial e_1^*}{\partial N_1} < 0$, $\frac{\partial e_2^*}{\partial N_1} < 0$, $\frac{\partial e_1^*}{\partial N_2} > 0$, and $\frac{\partial e_2^*}{\partial N_2} < 0$.

The proof is in Appendix A5.

The key difference between the Nash and non-myopic equilibria is the positive effort response of strong incumbents to the entry of additional weak students. First, note that as seen in expressions characterizing the marginal benefits to effort in the two models (expressions (1) and (3)), the marginal benefit of effort is lower in the non-myopic equilibrium than the Nash equilibrium. This is because non-myopic individuals anticipate that some of their effort will be matched by their competitors and be wasted. As a result, a student in the non-myopic model withholds some of her effort in comparison to the Nash model. The withholding rate depends on the fraction of effort that will be wasted by matching and turns out to be roughly proportional to the fraction of her group's aggregate effort relative to the total effort. Thus, when a non-matching (weak) student enters the competition, both the base amount of effort she wants to exert and the withholding rate decrease, and she may end up decreasing or increasing her effort depending on which of the two effects dominate.[18]

d. Non-myopic model with a minimum-effort requirement and dropouts. We have assumed that weak students participate in the competition and always remain in school. Suppose instead, that staying in school requires at a minimum level of effort $\underline{e} > 0$ and that this minimum is binding for the weak students, while strong students always exert effort above the minimum. We are able to show that the non-myopic model still has an equilibrium even when such a

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[18] We also show in Appendix A3 that the comparative statics results are identical when a strong student replaces a weak student or vide versa (swapping).

minimum-effort requirement is imposed and is binding for weak students who exert minimal effort while strong students exert greater effort.

Theorem 5. Assume that for all i, f'_i is bounded above and c'_i is bounded below by a positive constant. Then equilibrium exists in the non-myopic model with a minimum-effort requirement. The equilibrium is $(\hat{e}_1, \hat{e}_2)$, where $\hat{e}_1 > \underline{e}$ while $\hat{e}_2 \geq \underline{e}$.

The proof is in Appendix A1.

What about the comparative statics results? The following theorem states that in the equilibrium where the weak students are exerting minimal effort, strong students decrease their effort when strong competitors enter the competition and increase their effort when weak students enter, the same predictions for the strong students as in the model in which all students compete. In this case, however, weak students do not react to any entries (they would otherwise reduce their effort) because they are constrained by the minimum-effort requirement.

Theorem 6. Suppose the equilibrium in the non-myopic model with a minimum-effort constraint is $(\hat{e}_1, \hat{e}_2)$, where $\hat{e}_1 > \underline{e}$ and $\hat{e}_2 = \underline{e}$. Then we have $\frac{\partial \hat{e}_1}{\partial N_1} < 0$, $\frac{\partial \hat{e}_1}{\partial N_2} > 0$, $\frac{\partial \hat{e}_2}{\partial N_1} = 0$, and $\frac{\partial \hat{e}_2}{\partial N_2} = 0$.

The proof is in Appendix A2.

We can also show that our comparative statics results for strong students are robust to the possibility of some students giving up and dropping out of school ($e_i^* = 0$) when their expected utilities from remaining in school fall below a reservation utility, for example, the utility from the average earning of high school dropouts. Assuming that the reservation utility is not lower for strong students, strong students will not drop out in an equilibrium, except possibly in the extreme case where all the weak students drop out, because strong students receive a higher utility than weak students. Thus, as long as the rank order of the two groups' strengths remain the same, the comparative statics results for the strong students remain valid even when the possibility of dropping out is considered.[19] In contrast, the equilibrium number of weak students

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[19] When some of the weak students drop out, it may raise the achievement of the remaining students because, for example, they receive more individual attention or have less classroom disruptions. The comparative statics on the

must be determined endogenously. This makes the comparative statics results for the weak students more complex.

3. Adding Peer Learning Effects into the Non-myopic Competition Model

In the non-myopic equilibrium model we have considered so far, an individual's achievement depends only on her own effort level. Peers influence achievement only via their effects on effort. We now modify the non-myopic model by assuming that a student's achievement increases as the size of the strong group increases. That is, we allow for the existence of direct peer learning effects. Formally, the achievement function f_i is now $f_i(e_i, N_1) = a_i(N_1)b_i(e_i)$, with $a_i(N_1) > 0$ and $a_i{}'(N_1) > 0$. For given effort levels, adding strong students to the class directly increases individual achievement, raising the returns to effort. We further assume that peer learning effects are characterized by complementarity with respect to ability. That is, the peer effect is stronger on strong students than weak students, or more precisely, the rate of improvement in the achievement from peer effect $\left(\frac{a'_i}{a_i}\right)$ is greater for the strong students than the weak students.[20]

Given peer learning complementarity, it can be shown that comparative static predictions for effort remain the same – in particular, when a strong competitor enters – even if providing direct positive peer effects - strong incumbents decrease their effort. However, whether a student's achievement increases or decreases depends on additional details of the achievement function. Thus, it is possible to have an achievement increase even when student effort decreases when a strong student is added to the class if there are peer learning effects. This means that without direct information on student effort it may not be possible to identify either peer learning or effort effects from changes in test scores or grade-point averages.

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strong students will remain valid under such cases as long as the weak students do not benefit so much that they become stronger than the (original) strong students.

[20] This assumption is consistent with the findings in Chen and Hu (forthcoming), who find that when students are not competing in the same major, high-ability students improve the grades of other high-ability students but have no effect on lower-ability students. We also find evidence that high-ability students are more likely to receive homework help from other students in less competitive environments.

Theorem 7. *Suppose* $\frac{f''_i(e_i,N_1)}{f'_i(e_i,N_1)} < \frac{c''_i(e_i)}{c'_i(e_i)}$ *for all i and* $\frac{a'_1(N_1)}{a_1(N_1)} > \frac{a'_2(N_1)}{a_2(N_1)}$. *Then* $\frac{\partial e_1^*}{\partial N_1} < 0$, $\frac{\partial e_1^*}{\partial N_2} > 0$, $\frac{\partial e_2^*}{\partial N_1} < 0$, and $\frac{\partial e_2^*}{\partial N_2} < 0$. *In addition, the signs of* $\frac{df_1(e_1^*,N_1)}{dN_1}$ *and* $\frac{df_2(e_2^*,N_1)}{dN_1}$ *are ambiguous.*

These results provide an indirect test of peer learning effects when effort and test scores are observed – if effort decreases by strong students when there is an increase in the number of strong students, due to competition effects, but test scores improve, that is evidence of peer learning.

The proof is straightforward, with details in Appendix A6. In this case, the contest success functions are:

$$P_1(e_1,e_2,N_1,N_2) = \frac{f_1(e_1,N_1)}{N_1 f_1(e_1,N_1)+N_2 f_2(e_2,N_1)} \text{ and } P_2(e_1,e_2,N_1,N_2) = \frac{f_2(e_2,N_1)}{N_1 f_1(e_1,N_1)+N_2 f_2(e_2,N_1)}.$$

These changes do not affect the equilibrium condition, apart from the inclusion of N_1 as an argument in f_i. Thus, the marginal benefits at the equilibrium remain the same:

$$\tilde{P}_1(e_1^*,e_2^*,N_1,N_2) = v_1 \frac{\partial P_1(e_1,e_2,N_1,N_2)}{\partial e_1}|_{(e_1^*,e_2^*)} = \frac{v_1 f'_1(e_1^*,N_1) N_2 f_2(e_2^*,N_1)}{\left(N_1 f_1(e_1^*,N_1) + N_2 f_2(e_2^*,N_1)\right)^2}$$
$$\tilde{P}_2(e_1^*,e_2^*,N_1,N_2) = v_2 \frac{\partial P_2(e_1,e_2,N_1,N_2)}{\partial e_2}|_{\left(e_i^*,e_j^*\right)} = \frac{v_2 f'_2(e_2^*,N_1) N_1 f_1(e_1^*,N_1)}{\left(N_1 f_1(e_1^*,N_1) + N_2 f_2(e_2^*,N_1)\right)^2}.$$

Since the second-order condition is satisfied under our assumption, the equilibrium condition at an interior equilibrium (e_1^*,e_2^*) is:

$$F(e_1^*,e_2^*,N_1,N_2) = \begin{bmatrix} \tilde{P}_1(e_1^*,e_2^*,N_1,N_2) - c_1'(e_1^*) \\ \\ \tilde{P}_2(e_1^*,e_2^*,N_1,N_2) - c_2'(e_2^*) \end{bmatrix} = \begin{bmatrix} 0 \\ \\ 0 \end{bmatrix}. \qquad (5)$$

As before, comparative statics are analyzed by implicitly differentiating the equilibrium condition (5). The only difference is that N_1 now affects the equilibrium through students' achievements as well.[21]

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[21] A useful extension of the model would be to add helping others as a choice along with own effort, as in Drago and Garvey (1998). In our data, however, while there is multi-round information on own student effort there is no information on student time spent helping others, only information from one round on whether a student received

4. Data

There are two principal data sets that we use to test for the effects of competition within grades in schools.

a. *Restricted-use National Education Longitudinal Study of 1988 (NELS:88) data*. The NELS:88 is a nationally-representative panel survey of students starting with a sample of over 25,000 8th-graders in 1988 who were additionally surveyed in 1990 and 1992. There are several key features of this data set that enable tests of the implications of our model of student competition. First, unlike school administrative data, the survey provides information on the academic effort of students as well as their test scores. Two key effort variables available in the data for all three survey rounds are the student's weekly homework hours and the frequency of class skipping. The data set also provides information in multiple rounds on classroom disruption, including in-class robberies and drug-transactions.

A second key feature of the data is that there is information elicited from parents and teachers on their inputs to student achievement and on their characteristics. The availability of parent characteristics along with characteristics of the students permits us to stratify students based on their nativity as well as on their pre-school human capital. In particular, we exploit the availability of parent information and use parental schooling attainment to stratify students by ability. We do not use test scores for student stratification, as is commonly done, because as we show empirically, and is a central feature of our model, test scores are endogenous equilibrium outcomes, reflecting both student effort and peer effects.[22]

The third feature of the NELS:88 data that we exploit is the panel – a random subset of students is followed from 8th grade through high school. We use the panel data for three purposes. First, using all three rounds, we estimate whether and by how much effort matters, examining the effects of changes across rounds in the two (endogenous) student effort variables on their standardized test score outcomes and the inter-round gains in test scores. Second, we test the implications of the model by estimating how, for a given native-born student, changes in the composition of student peers, from the county-level assignment of newly-arrived student-age

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homework assistance. We regard this model extension as valuable for future theoretical work along with more extensive data collection.

[22] Lagged test scores cannot be used to sort students by ability and examine the determinants of academic effort in any period to the extent that study habits – effort – are serially correlated. Conley *et al*. (2022) find using unique time-use data on college students that high school study time and study time in college are "strongly" correlated (p.4). We show that lagged test scores are endogenous to current effort in our panel data.

refugees by academic strength, affect her academic effort, as measured by homework time and class attendance. Third, we test for the existence of direct peer learning effects by assessing whether, for given own effort levels, an increase in the number of strong refugee students augments the test scores of native-born students.

Our main sample for analysis is from the NELS:88 panel data providing information on the same student while in 10th and 12th grades. One key advantage of this panel of high school students is that although many students in the NELS:88 changed schools between 8th grade and 10th grade, less than 3.7% of the native-born students changed schools after 10th grade. We thus focus on this sample of school stayers between 10th and 12th grade, eliminating both the influence of unmeasured time-invariant student and school characteristics using student fixed effects with minimal selectivity bias from any school flight.[23]

A fourth feature of the data set that we exploit is a variable characterizing the degree of grade competitiveness of the schools. While we believe that schools are inherently competitive, given class rank-based rewards, this variable provides an additional contrast enabling a direct tests of whether the degree of competitiveness reduces own student effort, by student ability, and reduces the academic advantages of higher-ability peers including classmates assisting other classmates. Teachers in the 10th and 12th grades were asked whether the statement "students are encouraged to compete for grades" is accurate, with response categories ranging from "not at all accurate" to "very accurate." As an indication of how pervasive competition in schools is, only 12.4% of 10th-grade teachers thought that the statement was less than "somewhat accurate." However, there is variation in school competitiveness even at this higher level, with 14.4% of teachers reporting that they thought the statement was "very accurate."

b. *Office of Refugee Resettlement data*. A limitation of the NELS:88 is that it does not provide information on the classmates of the surveyed students. We thus cannot characterize the composition of student cohorts (classes) by ability at the school level. However, in addition to information on students, teachers, parents, and schools, the restricted-use version of the NELS:88 data identifies the locations of the schools and residences of the students at the county level. To test the competition models we estimate the impact of variation across counties and over time in

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[23] School switching rates are lower for the strong native-born students that we define below (3.3%) and are the focus of our analysis. Thus, our results are unlikely to reflect selective school flight by strong native-born students in response to changes in the ability composition of classmates (e.g., Boustan *et al*., 2023; Figlio *et al*., 2021).

the number of Southeast Asian (SEA) student-age refugees (Cambodian, Laotian, Vietnamese), divided into two groups by parental schooling, on native-born student performance and effort, also divided into two groups based on family background. Our main data source for the SEA refugees is the newly-released micro data set describing all refugees who re-settled in the United States from 1975 through 2008 from the US Office of Refugee Resettlement (Dreher, *et al.* 2020). The data provide information on country of origin, month and year of resettlement, the county of initial location, age, gender and schooling attainment for the refugees.

We focus on SEA refugees for three reasons. First, the magnitudes of SEA refugee flows into the United States since 1975 through the periods covered by our main data set describing students are very large: 919,144 SEA refugees re-settled in the United States from 1975, the initial year of re-settlement, through 1990, the second round of the NELS:88. And another 102,090 SEA refugees were added between the second and third NELS:88 panel rounds, of whom 5,201 were of high-school age.

The second reason we focus on SEA refugees for our treatment variables is that the *initial* locations of refugees are not the choices of the refugees themselves. Unlike the locations of all non-refugee immigrants at a point in time, which reflect both their own initial location choices and subsequent internal migration, refugee initial locations are assigned by the US Department of State in co-operation with a set of non-profit resettlement agencies working with approximately 200 affiliates located across the United States. As Figure 1, which maps the initial locations of the SEA refugees in the period 1987-1990, shows, refugee location assignments are spread across the country. Our key treatment effects on effort by native-born students are identified from the variation in the *assignments* of additional student-age refugees, by ability type, across counties over time - between 1990 and 1992 - using the second and third rounds of the NELS:88 panel data. We discuss the power of the analysis and tests of the potential endogeneity of non-random refugee location assignments below.

c. *5% public-use samples from the decennial 1990 Census.* We believe that use of the panel data and panel data methods provide the most credible estimates of effort effects because they are based on an individual student's change in exposure to SEA refugee students derived from intertemporal variation in initial refugee location assignments. A shortcoming of the ORR refugee records, however, is that family members are not linked. Thus, it is not possible to stratify school-age SEA refugees by parental schooling directly. We take advantage of the coincidence of the 1990 Census with the second round of the NELS:88 in 1990 to remedy this shortcoming. It also enables us to assess the robustness of our panel estimates based on ORR refugee records. In particular, we use the 1990 Census data, which enable direct stratification of students by parental schooling, to divide the student-age SEA refugees from the ORR records into strong and weak. We also assess whether the relationships we find from the NELS:88 panel, using the approximations to the ability composition of the SEA refugee students, are also evident using the cross-sectional variation in 10th-grade SEA students in the Census stratified by parental schooling.

The Public-Use 1990 Census samples enable the identification of SEA refugees based on country-of-birthplace information. Because most 10th-grade school-age refugees co-reside with parents, and co-residing family members are linked to parents in the Census samples, we can stratify the SEA student-age refugees in the Census using the same parental schooling criterion as that of the native-born students in the NELS:88 data based on the personal and household identifiers in the Census data. Thus, we can carry out a cross-sectional analysis of variation in SEA students by ability type across counties on 10th-grade NELS:88 native-born student effort.

There are two limitations of the Census data for our analyses. First, the public-use samples only provide county location information for counties with a population size of 250,000 or more. Although the Census-identified counties make up only 36% of the counties represented in the NELS:88 1990 round, they account for 61.4% of the student sample and represent 45.3% of all counties that have any 10th-grade age SEA refugees based on the ORR location assignments since 1975. All states are represented, so we multiply the fraction of 10th-grade SEA refugees whose parents had more than a high school education at the state level to stratify the ORR SEA school-age refugees into strong and weak. This introduces error into the ORR refugee student variables, and we use an instrumental variables strategy for the panel estimates to both minimize bias from the errors and the potential bias arising from endogenous changes in refugee locations assignments.

The Census information on strong and weak SEA 10th-grade refugees do not contain approximation error. However, a second limitation of the Census Public-Use data is that the locations of the refugees in 1990 are due not only to their initial assignments but also reflect post-entry internal migration. We discuss our identification strategy in the presence of endogenous refugee location using the history of SEA refugee location assignments from the

ORR data below. Given the validity of our instruments, we indeed reject the exogeneity of cross-sectional refugee locations, however, the untestable identification assumptions for the cross-sectional Census analyses are stronger than they are for the panel. Nevertheless, the statistically-preferred cross-sectional Census-based estimates and the ORR-based panel estimates both support the non-myopic competition model.

d. *United States Department of Education, National Center for Education Statistics on public and private school enrollments for 1989-90 and 1991-92*. As an additional control variable in all our specifications we computed the total number of private and public 10th- and 12th-grade students in 1989-90 and 1991-92 at the county level for each US county to match up with the Census and NEL:88 panel rounds.

5. Estimating the Achievement Function

A key component of the school competition model is the individual achievement function that maps a student's own effort to her individual achievement. In this section we start our empirical analyses by estimating a parameterized version of the effort function. We do this to identify if there are indeed payoffs to our two measures of effort that are the focus of our empirical tests - homework time and class skipping. We also use the estimates from the effort function to derive estimates of individual student ability net of effort, which we use to test our assumption that more able students expend more effort. Leveraging the variation across schools in the degree to which grade competition is encouraged, we also test (i) the fundamental prediction of tournament models that rankings-based competition reduces student effort particularly among high-ability students, and (ii) grade competition reduces peer learning among high-ability students, examining the amount of assistance from classmates by student ability, as found by Chen and Hu (forthcoming) using quasi-experimental data from Chinese universities.

Our achievement measures are the student's standardized combined math and reading test score and her test score improvement across rounds (value-added). We assume a Cobb-Douglas form for the achievement function:

$$O_{ijtk} = \delta_j \mu_{ij} H_{ijt}^{\alpha_{1k}} e^{\alpha_{2k} S_{ijt}} C_{ijt}^{\alpha_{3k}} \varepsilon_{ijt}, \tag{6}$$

where $O_{ijtk} = T_{ijt}$, the standardized composite reading and math test score of student *i* attending school *j* at the end of school year *t*, or $T_{ijt} - T_{ijt-1}$, the test score improvement from the previous period's test score (value-added); H_{ijt} = weekly hours of homework by student i in school j during year t; S_{ijt} = whether student i in school j skipped any classes during year t; C_{ijt} = cohort (class) size for i in school j in year t; δ_j = school fixed effect, μ_{ij} = student fixed effect ("ability"); ε_{ijt} = *iid* error, including measurement error.

Identifying the parameters of (A) faces three challenges. First, as highlighted in the model, individual students differ in ability μ_{ij}, which affects test score performance directly and affects effort choice. Second, there are unmeasured school-specific variables (e.g., computers, teacher standards) that may affect a student's test score net of effort, as embodied in the school fixed effect δ_j, and may affect a student's individual effort. To eliminate the bias due to omitted time-invariant student and school characteristics embodied in their respective fixed effects we choose as our sample from the NELS:88 panel students who attended the same high school in 10th and 12th grades. Although many students in the NELS:88 changed schools between 8th grade and 10th grade, less than 3.7% of the native-born students with test scores in the 10th and 12th grade changed schools, so this sample of high-school stayers is not especially selective but permits us to eliminate any influence of time-invariant school as well as student characteristics.

After taking logs of both sides of equation (A), we difference across the two rounds for the same student so that the estimating equations are: [24]

$$\Delta \log O_{ijt} = \alpha_{1k} \log H_{ijt} + \alpha_{2k} S_{ijt} + \alpha_{3k} \log C_{ijt} + \Delta \log \varepsilon_{ijtk}\,, \qquad (7)$$

where Δ = difference operator. This eliminates the influence of time-invariant school characteristics and student ability. A remaining issue is that retrospectively-reported homework hours and class skipping may contain measurement errors, and biases in measurement errors are amplified when identification is obtained using fixed effects methods such as first differences.[25]

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[24] We ignore here the direct effect of changing class composition on test scores via peer learning effects. Below, when we test for peer effects, we add our SEA refugee variable to equation (A). The addition of this variable, while statistically significant, does not alter the estimates of the effort variable effects on test scores.

[25] We could find little prior evidence on the payoffs to student effort. Stinebrickner and Stinebrickner (2004) use data on homework time for college students with different retrospective periods to quantify the measurement error in homework time collected retrospectively for an entire school year. Their estimates of the effects of homework time on grades, which are large, do not, however, take into account the endogeneity of homework time, as they recognize. In Stinebrickner and Stinebrickner (2008) they use the same data to attempt to identify the casual effect of homework time on student achievement using the randomized characteristics of dorm peers as instruments This strategy, however, rules out direct peer learning effects on individual achievement. De Fraja *et al*. (2010) employ a more sophisticated econometric model to identify student effort on achievement, but the data they use do not have

In addition, shocks to the previous period's test score or test score improvement, $\varepsilon_{ijtk\text{-}1}$, may influence subsequent period effort. Measurement error will bias the α coefficients to zero, but the dynamic behavioral bias may be positive or negative (subsequent effort may decline or increase after a student experiences an unusually low test score, for example).

To eliminate the biases emanating from measurement error and dynamic effort behavior, we additionally employ instrumental variables estimation. To do this we need to obtain a set of variables that predict the changes in effort variables across the two time periods. We employ LASSO to select a parsimonious set of variables that predict best the changes in the two effort measures in the interval between grade 10 and 12 from the rich a set of home, parent and student characteristics available in the survey data.[26] The identification assumption is that these traits and home variables, although affecting how their child changes its behavior over time, are orthogonal to the intertemporal shocks to test scores.

From the candidate list, the LASSO-chosen variables for parsimoniously predicting the change in homework hours across the two survey years included dummy variables for whether the student was age 17 or 20 in 12th grade, whether the mother had completed 12 or 14 years of schooling, whether the father had completed 10 or 13 years of schooling, whether the father hoped the student attended college, whether the father resided in the student's household, whether grandparents resided in the student's household and whether the student was Black. For class skipping, the only LASSO-selected variable was whether the student was male. We employ all the LASSO-selected variables in the first stage. The use of LASSO does not guarantee that the selected instrumental variables will satisfy both the rank and order conditions for identification, nor guarantee the instruments are not weak. We employ tests of the validity (and

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direct information on homework time so they use proxies for effort based on attitudinal measures toward schooling and work. Grau (2018) takes into account the measurement error in individual indicators of student effort by employing factor analysis using multiple effort measures, but the estimates of the effort latent variable on test scores are identified under the strong assumption that effort is orthogonal to the test score error term.

[26] The candidate variables we used were a full set of dummy variables corresponding to the student's age in 12th grade and the father's and mother's years of schooling attainment, whether the student had his own study place, whether the student had his own bedroom, whether the mother was native-born, whether the father was native-born, whether the mother worked, whether the mother was a teacher, whether the mother hoped the student would attend college, whether the father hoped the student would attend college, whether the father co-resided with the student, whether the mother co-resided with the student, whether any grandparents lived with the student, whether the parents imposed homework rules, family income in 1991 and 1987, and whether the student was male, whether the student was Black, and whether the student was Asian.

necessity) of the instrumental variables estimates, including test statistics for under-identification, overidentification tests, and weak instrument tests.

Table 1 provides the descriptive statistics for the NELS:88 sample of the native-born students with test scores in all three rounds and who remained in the same school in 10th and 12th grades. The test scores are standardized for the entire population of students by grade and have a mean of 50 with a standard deviation of 10. Average test scores for this group are higher than the population mean, reflecting in part the selectivity by nativity and also sampling stratification (the descriptive statistics presented in the table do not use population sampling weights). While for this group test scores did not appreciably change across the two years, reflecting the test score standardization by grade, both homework time and class skipping increased between the panel rounds, the former by 34.1% and the latter by 58.7%. The size of the cohort declined over the period, reflecting dropouts.[27]

Table 2 reports random effects (GLS), first-differenced, and first-differenced IV (FD-IV) estimates of the effort function for the test score level and for test score improvement (value-added) for native-born students. For all estimation methods, homework time increases test scores and accelerates improvements in test scores, while skipping classes reduces both academic achievement measures. All estimates are significant at least at the 5% level except for the class skipping effect estimated by IV for the log test score (column three), which is statistically significant at the 10% level. Thus, individual student effort, measured by the two variables, matters for individual school performance.

The test statistics favor the first differenced estimates over the random effects estimates and the IV estimates over the first differenced estimates. Consistent with measurement error afflicting the first difference estimates, all coefficients move away from zero when the instruments are employed. The preferred IV estimates in columns three and six, respectively, indicate that an increase in weekly homework hours by one standard deviation (an 82% increase=5.79 hours per week, or about an hour more per weekday) increases the standardized test score by 2.6%, corresponding to an increase of 0.26 of the composite test score standard deviation. The same increase in weekly homework hours increases test score value-added by

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[27] We discuss dropouts by ability group and sample selectivity below.

4.7%. Skipping classes reduces the test score by 8.9% or 0.89 standard deviations and decreases test score improvement by16.5%.

6. Student ability, school competitiveness and student effort

The estimates in Table 2 provide evidence that effort choices are an important component of test scores and suggest that stratifying students by test score outcomes, whether contemporaneous or from the past, can lead to misclassification of students by ability and erroneous estimates of how student ability affects behavior. The achievement function estimates also, however, permit the extraction of student fixed effects and thus allow estimation of the relationship between student ability and effort that is less contaminated by endogeneity and reverse causation. Moreover, using the residual-based ability measures we can also see within schools that emphasize grade competitiveness which students along the ability distribution are most affected, if at all, by this school culture.

We cannot use the achievement function residuals directly as measure of ability. This is because for each survey round the residuals, from the estimates of equation (7) in the third column of Table 2, contain student ability fixed effects, school characteristics fixed effects, and noise, including noise due to measurement error in the test scores and the effort variables. Even if we estimate the effect of the residuals on effort, net of school fixed effects, for any given year, we will get a downward biased estimate of ability effects due to the measurement errors and other shocks also embedded in the residual. Nor can we use, say, student test scores from earlier years to represent ability, because test scores are not only affected endogenously by student effort as we have seen in Table 2, but they also contain measurement error.

To eliminate these biases, we use the initial test scores in 1988 as our ability measure but instrumented by the residuals from the estimates of (7) from the subsequent years. Variation in the 1988 test score using the residual instrument isolates the variation in test scores not due to effort. To obtain consistent estimates we need to assume the time-varying test score measurement errors are iid. This assumes that the year-specific measurement errors in the 1988 test scores are uncorrelated with the measurement errors in the effort and test score variables embedded in the fixed effect, as these are measured in the years subsequent to 1988. A further issue, however, is that we cannot use the residuals from the same year that we measure effort as a valid instrument for ability because the errors in the residual contain the measurement errors in the contemporaneous effort variables. Instead, we use the residuals from a different year.

The econometric model we estimate for the two effort variables in the third year of the panel (1992, 12th grade) is thus[28]:

$$E_{ijtk} = \beta_{1k} \log T_{ij88} + \beta_{2k} \log T_{ij88} \times c_j + Z_{jk} + \xi_{ijtk}$$

$$\log T_{ij88} = \gamma \widehat{\log \mu}_{ijt-1} + \lambda Z_j + v_{ij88}$$

$$\widehat{\log \mu}_{ijt-1} = \log \mu_{ij} + \omega_{ijt-1}$$

where t=1992 and t-1 = 1990, E_{ijtk} = log weekly homework time in year t or class skipped in year t, c_j = 1 if the school encourages grade competition, the Z_j = school fixed effects, ζ_{ijt} = iid error, and we assume that cov(ζ_{ijt}, v_{ijt}), cov(ζ_{ijt}, $\omega_{ijt\text{-}1}$), cov(ζ_{ijt}, v_{ijt}) = 0. The specifications also include the LASSO variables as controls, which are also allowed to have different effects across the two school types. We expect that β_1>0 for k = homework time and β_1<0 for k = class skipping. If the higher-ability students are the (only) competitors, which we test below, based on our model we also expect that in schools that encourage competitiveness, effort is reduced among the more able relative to the more able in less competitive schools.

Table 3 reports the estimates of the effects of ability on the two effort variables first for all schools and second by school type, with all specifications including school fixed effects and the LASSO control variables, using least squares and IV. As expected, the OLS estimates in columns 1 and 4, appear to reflect reverse causation – the coefficient on the 1988 test score is substantially more positive in column 1 than the corresponding IV estimate in column 2 for homework hours and the OLS coefficient for class skipping in column 4 is substantially more negative than its IV counterpart in column 5. These differences are statistically significant, as indicated by the endogeneity test. The other test statistics indicate that the residual ability measure (from 1990) is a strong instrument for the 1988 test score.

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[28] We focus on the third panel year, separated from the 1988 test scores by four years, so that we can rely on a weaker assumption that there exists some serial correlation in errors but the correlations decay relatively fast.

The statistically-preferred IV point estimates in columns 2 and 5 indicate that, consistent with the competition model assumption, the higher ability students in all schools spend significantly more hours doing homework, with an elasticity just below 0.5. On the other hand, on average across all schools, there is no statistically significant association between ability and the propensity to skip classes when the endogeneity of test scores is taken into account.

Splitting the sample into those schools that strongly encourage grade competition (teachers strongly agree that students are encouraged to compete for grades) and those that do not or provide less encouragement to compete reveals, in columns 3 and 6, that in the majority of schools in which grade competition is less or not encouraged the stronger students spend more time doing homework, with an estimated ability elasticity of 0.8, and do not skip classes any more than lower-ability students. However, higher-ability students in schools that strongly encouraging grade competition spend less time doing homework and are more likely to skip classes compared to both lower ability students in their own schools and to their high-ability counterparts in less competitive schools.

The results indicate that competition discourages own effort among the stronger students, the key implication of competition models. Is it possible that the competition also reduces the extent to which students help each other, as found by Chen and Hu (forthcoming)? The NELS:88 provides information in the second round (10th grade) of the survey on whether the student received homework assistance from a classmate. Peer homework help is evidently relatively common, with over 80% of students reporting that they had received such assistance. In the last three columns of the Table 3 we report the effects of ability on receiving peer homework help for all students and for students in schools differentiated by whether grade competition was encouraged. The results indicate that higher ability students overall are more likely to receive help from peers – a one standard deviation in ability measured by the 1988 test score increases the probability of receiving peer help by a statistically significant 3.7%. This finding is also consistent with the assumption we made about peer learning effects in the competition model in which the returns to peer help are higher for more able students. Moreover, within schools that do not or only weakly encourage grade-competition, this effect is over 50% higher while in the high grade-competition schools the relationship is reduced by over 60% and is statistically insignificant. Our results thus are consistent with the finding in Chen and Hu (forthcoming) that competition discourages peer assistance. But we also find that competition also decreases own effort so that test score or grade changes cannot all be attributed to the holding back of peer assistance.

Of course, as noted, we do not think that the schools where teachers do not strongly agree with the statement that the school encourages grade competition means that there is no competition in those schools (for example, competition exists if grades are determined by a curve or class rank determines school honors and better admission prospects even if competition is not directly encouraged).[29] We now turn to the testing the predictions of the model that assumes competition in all schools to test for the consequences of school competition for higher ability students that does not rely on differentiating schools by the degree of competition.

7. Empirical Strategy using the NELS:88 High-School Panel to Test the Competition Models

The key prediction of the model in which students compete for rewards and consider the reactions of their fellow students is that student effort will respond differentially to changes in the numbers of strong and weak students in their classes and that these responses differ by ability type. Our strategy for testing the model is to use the panel component of the NELS:88 to estimate the effort responses of the surveyed strong native-born high-school students to changes in the numbers of strong and weak student-age Southeast Asia (SEA) refugees, whose numbers are both large and time-varying over the course of the three rounds of the NELS:88. Specifically, we use the NELS:88 high-school panel, which provides information for the same student in 10th- and 12th-grades.

We focus on native-born high-ability students as the treated because we assume that these students believe they can likely win in a competition and therefore it is these students for whom the competition model provides unambiguous predictions. We will also test whether the responses of weak native-born students to changes in student composition differ from those of the strong or are unresponsive, as the model implies. To test the model empirically thus requires selecting the criteria for dividing the set of surveyed native-born students and the SEA refugee

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[29] There may also be other characteristics of schools correlated with the encouragement of grades that are driving the results. We test below whether the grade competition variable merely reflect whether the school is private or selective when we look at peer effects on test scores.

students into those who are strong and weak. We cannot use test scores to stratify students by ability because, as shown, test scores from any period reflect effort. Using test score criteria would result in selecting samples by the outcome variables whose variation we are interested in. In addition, we do not have test scores from the administrative records of the SEA refugees.

We define strong students as those whose father and mother both had more than a high school education. Table 4 shows the test scores, homework time, and class skipping of 12th-grade native-born students stratified by this parental education criterion. As expected, the strong students defined in this way have significantly higher standardized test scores than the "weak" students - on average, the test scores of the strong native-born exceed those of the weak students by 65% of a standard deviation. The strong native-born students also spend 28% more hours on homework per week and skip less classes, so some of the test score gap by family background is indeed due to differences in effort choice as the model implies.

We are assuming that strong foreign-born SEA refugee students are a threat to the strong native-born students. There are foreign-born SEA refugee students represented in the NELS:88.[30] Applying the same ability stratification criterion, we see from Table 4 that the strong SEA refugee students have test scores that are actually higher than those of the strong native-born, although the difference is not statistically significant.[31] High-ability SEA refugee students also spend more time doing homework than the strong native-born and skip classes less. The strong SEA students thus appear to be comparable to, and thus competitors with, the strong native-born students. Interestingly, the weak SEA refugee students exhibit comparable test scores to the weak native-born, but spend more time doing homework, although not more than the strong students of either nativity.

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[30] The SEA refugees in the NELS:88 are representative of all SEA refugees in the United States, but are not representative at the school, grade or county level. The statistics in Table 4 are derived using sample weights.

[31] A concern is that the non-native-born refugees may be substantially less capable than native-born students because of lower ability to speak or understand English. However, the standardized reading component of the composite test scores are almost identical between the SEA refugee students and the native-born – the mean standardized reading score for the refugee students is 64.1, that for the native-born is 64.4 (standard deviations are 20). The SEA refugee students have advantages in mathematics – the mean math scores are 66.8 for them compared with 65.1 for the native-born students.

Combining at the county level the NELS:88 survey data and the ORR refugee assignment data, we estimate the following equations for native-born students *i* in school *j* in group *g* (strong and weak) located in county *k* at time *t*:

$$E_{ijgkt} = \beta_{1g} R^{L}_{jkt} + \beta_{2g} R^{H}_{jkt} + \mathbb{Z}_{ijgkt}\gamma_{1g} + Q_{jgkt}\gamma_{2g} + c_{jgk} + \mu_{ijgk} + e_{ijgkt} \qquad (8)$$

where *E* = weekly homework hours, any classes skipped, or the frequency of skipped classes in the school year, test score; R^T and R^H = the number of assigned weak and SEA refugee students in the county, respectively; $\mathbb{Z}$ = vector of time-varying student, county and school characteristics; μ_{ijgk} = student fixed effect; and c_{kn} = school fixed effect. e_{ijgkt} = time-varying iid error.

In this equation β_{1g} = the effect on group-*g* native-born student effort from an increase in the number of weak SEA students, β_{2g} = the effect on group-*g* student effort from an increase in the number of strong SEA students. Table 5 provides the sign predictions for the β coefficients for each of the dependent variables that correspond to the comparative statics of the non-myopic competition model.

The principal threat to identification is that the presence of SEA foreign-born in a county may be related to the unobservable characteristics of the students and/or schools that affect student behavior, as embodied in the school and student fixed effects. We thus difference equation (8) across the two panel years to obtain:

$$\Delta E_{ijgkt} = \beta_{1g}\Delta R^{L}_{jkt} + \beta_{2g}\Delta R^{H}_{jkt} + \Delta\mathbb{Z}_{ijgkt}\gamma_{1g} + \Delta Q_{jgkt}\gamma_{2g} + \Delta e_{ijgkt}\,, \qquad (9)$$

where Δ is a time-difference operator. Because we use a panel that consists only of students who remain at the same school (over 96% of all students who remain in school), differencing eliminates all time-invariant school quality measures as well as fixed student abilities that may affect refugee placement assignments. ΔR^{L}_{jkt} and ΔR^{H}_{jkt} now represent the county assignments of new, relevant grade-age SEA refugees between the years 1990 and 1992 that add to the senior classes in the 1992 round. We also include in (9) the change in the size of the student cohort in the school, the change in the total number of students between 10th and 12th grades in the county, and the LASSO variables used to predict the changes in the effort variables used in the estimation of the achievement production function.[32]

Identification of the β's - the responses of effort to the changes in the number of strong and weak SEA refugees – using first-differencing, with county-clustered standard errors, requires that the county assignments of the new refugees in the interval between 1990 and 1991 are orthogonal to the *time-varying* shocks to student effort Δe_{ijkgt}. This seems *a priori* to be a weak assumption. However, because the strong and weak refugee students in the ORR data are stratified using state-level Census data on SEA refugee student backgrounds, as noted, the SEA variables may have measurement error. To test the orthogonality assumption and to eliminate bias due to measurement error we also estimate (9) using instrumental variables.

Labor market and school criteria are explicitly used as criteria by the US State Department for the initial placement assignments of refugees, but the agencies also tend to place new refugees where they have located refugees from the same countries in the past.[33] Accordingly, our instruments to predict the assignments of new weak and strong SEA (student-age) refugees after 1990 are a complete set of commuting-zone fixed effects and the cumulated projected total number up to 1988 of 8th-grade-age SEA refugees, the total number of SEA refugee women aged 35-60, the total number of SEA refugee women aged 35-60 with greater than a high-school education, and the total number of SEA refugees who arrived at least 10 years ago in the counties in 1987 based on all of the county placement assignments made by the ORR since 1975. We assume these histories of location assignments are orthogonal to the effort shocks between 1990 and 1992 and to the misassignments of refugee students into the strong and weak categories but are important determinants of where the agencies placed strong and weak SEA refugees subsequent to1987 along with labor-market characteristics absorbed in the commuting zone fixed effects.[34]

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[32] The total number of 10th-grade and 12th-grade students in public and private schools in 1989-90 and 1991-92 in each county are taken from the Private and Public-School Universe Surveys from the Institute of Education Science, National Center for Education statistics.

[33] The US State Department Bureau of Population, Refugees, and Migration (USBPRM) supervises the initial placement of refugees. The stated criteria on the web site of the USBPRM for selecting refugee locations are: "availability of affordable and safe housing, school capacity, medical care, and employment opportunities."

[34] A remaining issue is that there are omitted effects of changes across years by grade and student other than changes in own effort. Hanushek *et al*. (2003), who use panel data for multiple birth cohorts to estimate peer effects on

Identification of the effects of student composition on effort using panel first-differences emanates solely from the subsample of students residing in counties that experienced a change in the county-level number of refugees. The estimation sample thus includes only native-born, strong students residing in counties that received at least one new refugee student between 1990 and 1992. Table 6 presents the descriptive statistics for the same-school panel of strong native-born students experiencing new SEA refugee students between 10th- and 12th-grade. As can be seen, standardized test scores remained essentially constant across the two years for these students, but weekly homework time increased by 1.8 hours per week, an increase of 25%, and class skipping also increased, with more than half of the strong students skipping at least one class in their senior year, up from 34% in 10th grade. The number of students in the cohort in the school and in the counties fell between 1990 and 1992, reflecting dropouts. Note, however, that drop-outs are not significant among the strong native-born students, so that attrition bias is insignificant for our main subsample. The NELS:88 survey kept track of students who dropped out. Less than 3% of the strong native-born students in 1990 became dropouts by 1992. In contrast, 14.3% of weak student dropped out of school between 1990 and 1992.[35]

A concern is the power of our empirical strategy to identify significant effects on individual effort by students from the county-level changes in the SEA refugee student population between 1990 and 1992.[36] The average increase per county in the total number of SEA student-age refugees is only 43. On average, however, in counties that received at least one SEA refugee of relevant student age in the interval, the additional refugees represented 8.6% of the change in the total county student populations from 10th to 12-grade between 1990 and 1992, with the county refugee additions as high as 5.3X the change in the total number of county students. Whether we have power to reject nulls will be seen in the estimates.

8. Estimates of Effort Responses from the NELS:88 1990-92 Panel

Table 7 reports first-difference and first-difference estimates using IV of equation (9) for the log of homework time, the incidence of class skipping and class skipping frequency for the

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student achievement and are thus able to include school- and student-by-grade fixed effects find that the omission of these effects results in only minor changes in coefficients, as opposed to leaving out school and student fixed effects.

[35] Dropping out may in part be a response to competition by weaker students. In current work we are examining the determinants of dropping out in a competitive environment and in the presence of peer effects.

[36] The advantage of possibly low power is that the likelihood of falsely rejecting the null (no effect) is low.

set of strong native-born students.[37] Standard errors are clustered at the county level and all estimates use the population sample weights. All of the signs of the estimated coefficients on the student refugee variables are in accord with the comparative-static predictions of the non-myopic competition model – an increase in the number of strong-student SEA refugees reduces homework time and increases class skipping by the strong native-born students, while an increase in the number of weak SEA refugee students does the opposite. For all three effort measures, we cannot reject the null that the refugee variables, in first differences, are orthogonal to the error changes. The test statistics indicate the inability to reject exogeneity is not because of under-identification or weak instruments.

For homework hours, the refugee effects are relatively precise and non-trivial. We can successfully reject the hypothesis that both refugee effects are non-zero at the 5% level, with the statistically-preferred first-difference point estimates indicating that a one standard deviation increase in the number of strong SEA refugee students in the county reduces weekly homework time by 9.8% (about ½ hour per week) among the strong native-born students while a one standard deviation increase in the number of weak SEA refugee students increases their homework time by 11.2%. The first-difference estimates for the incidence of class skipping are not precise, but the corresponding point estimates of the refugee effects on the frequency of class skipping, statistically significant at the 10% level at a minimum for the appropriate one-tailed test, indicate that a one standard deviation increase in the number of strong (weak) SEA refugee students increases (decreases) the frequency of class skipping by16.9% (17.7%) among the strong native-born students.

For the coefficients associated with the refugee variables to be interpreted as resulting from competition effects, we showed that it is also necessary that the native-born weak students either do not respond at all to the changes in the student composition or respond differently compared to the strong native-born. Table 8 provides estimate of equation (9) jointly estimated over both the weak and strong native-born students allowing the coefficients to differ across the two groups so that effort responses can be compared. For homework time, the coefficients differ

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[37] The distribution of homework hours in skewed to the right. See the 10th- and 12th-grade homework histograms in Appendix Figures A and B. Results are not importantly different if homework hours in levels are used instead of log homework hours.

in sign across the two native-born ability groups, and these differences are statically significant. Moreover, the test statistics indicate that while we can strongly reject the null of no effort responses by the strong native-born students to the changes in class composition, the test statistics indicate that the responses of the weak native-born students to the changes in student composition are not statistically significant at conventional levels of significance.[38]

The class skipping results are less strong and mixed – for the incidence of class skipping, we can strongly reject the null of no response by the strong native-born students to changes in the composition of students, but the joint response by the weak native-born is marginally significant. The precision of the individual coefficients for both groups is also too low to infer significant differences. For class skipping frequency, the individual coefficients are relatively precise and similar for the two groups, but we cannot strongly reject the hypothesis that the joint effects are not significantly different from zero for either native-born group.

The homework time results provide the strongest evidence in favor of the competition model. One concern, however, is that the stratification criterion for weak and strong native-born students based on the schooling attainment cutoffs for both mothers and fathers may be imposing the results or are sensitive to the cutoffs chosen, permitting p-hacking. To agnostically assess if there indeed exists in the data a differential response by weak and strong native-born students to the refugee variables and to identify what fraction of a cohort are behaving as if they are competing, we estimated equation (9) semi nonparametrically using the locally-weighted functional coefficient model (LWFCM) of Cai *et al*. (2006). We allow all the coefficients to differ by a continuous measure of parental schooling attainment – the sum of the schooling years of the father and mother. We expect that the coefficients on each of the two SEA refugee variables should change signs as they cross some threshold that is determined by the data, and above that threshold should be statistically significant at all the higher values of the running variable.

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[38] This does not mean that the weaker students' academic success is not influenced by the ability composition of student peers. In preliminary work we find that increases in the number of strong and weak refugee students have significant effects on the probability that the weaker native-born students drop out of school between the 10th and 12th grades. There is no peer composition effect on dropout rates of strong students out because the number of strong students dropping outs is trivial. We also find evidence that while the selectivity of dropping out does not affect the estimates of student composition effects on weak-student effort, as found here, estimates of peer learning effects on the test scores of weak students are biased negatively and significantly.

Figures 2 and 3 display the plots of LWFCM estimates of the coefficient on the strong and weak SEA refugee variables, respectively, along with their 95% confidence intervals, for the log of homework time for all native-born students by the total schooling of their parents. As can be seen, for both SEA refugee variables, the coefficients change sign in accord with the non-myopic competition model at some threshold and attain statistical significance only at levels of combined parents' schooling above 30-32 years. The purely data-driven results are consistent with the estimates in Table 7 and 8 in which we imposed a threshold of parental schooling in which both parents have attained levels of schooling above 12 years but suggest we might have used a more stringent cutoff.

The graphs identify who among the cohort of students are competing. Both sets of estimates imply that only those students with high-achievement parents in terms of human capital display effort behavior consistent with the non-myopic competition model – they are in the competition - while students from more modest human capital households are not. Based on the combined parental schooling cutoff where the coefficients are all statistically significant for both SEA refugee variables, approximately 30 years, the school class competitors represent about 14% of the students. Using our criterion of greater than a high school education for both parents 36% of all native-born students are competing.

9. Estimates of Effort Responses from the NELS:88/Census Matched Sample

A concern with respect to the results based on the ORR SEA refugee data is that the division of SEA refugee students into strong and weak categories is approximated. While the panel estimates are robust to using instruments to predict the two refugee student types, we further explore the robustness of the results by using the Census data on SEA refugee students. The Census data, as noted, permit stratification of the SEA refugee students by parental education using exactly the same criteria as we used to divide the native-born students in the NELS:88 into the two ability groups. The 1990 Census, moreover, matches the 1990 round of the NELS:88 surveys that characterizes a representative sample of 10th-graders. We thus computed the number of strong and weak SEA refugee students in 10th grade by county, using the Census person weights, and merged the two Census SEA 10th-grade student refugee counts to the NELS:88 1990 round.

Because using the cross-sectional data we cannot automatically eliminate all time-invariant student, school and labor market characteristics, we estimate an augmented form of equation (8) applied to the matched Census-NELS:88 data, adding to the specification a complete set of commuting zone dummy variables, to control for differences in labor markets that may both affect effort levels of the students and the choice of refugee locations by the settlement agencies, and three school characteristics – whether the school is private, whether it has selective admissions, and whether grade competition is encouraged in the school (the top category). We also include the age of the student, student gender, and the educational attainment of the parents.

Identification of the SEA student refugee effects by ability type on native-born student effort using the matched Census-NELS88 data emanates from variation across counties within labor markets in 1990. The county distribution of refugee in the 1990 Census, unlike in the ORR refugee data, reflects the post-assignment migration choices of the refugees and thus may be correlated with unobserved variables characterizing students and schools that influence student effort. To address this use, we construct from the ORR refugee data a set of instruments to predict SEA student location - the projected number of SEA refugees in 1989 who would be ages 16-18 in 1989, the projected number of SEA female refugees with schooling greater than high school in the county who would be aged 35-50 in 1989, and the total number of SEA refugees in the county in 1989 with at least 10 years of residence in the United States based on the county assignments of the SEA refugees from 1975 through 1989 by the Office of Refugee Resettlement. These instrumental variables by construction do not reflect post-immigration location choices, but they do predict well the distribution of SEA refugee students by county, as seen in Figure 4.

Table 9 reports the descriptive statistics for the matched Census and NELS:88 data for 10th-graders. Table 10 reports the OLS and IV estimates for the three effort variables. The instrumental variables based on the histories of initial re-settlements, as expected, predict well where SEA refugee students are located in 1990. We can reject the exogeneity of refugee locations to both homework time and class skipping frequency – evidently the post-arrival locations choices by the refugees are determined in part by unobserved variables associated with the effort levels of native-born students. Of course, identification using the cross-section variation requires the assumption that the historical initial county assignments from 1975 to 1989, within labor markets, by the refugee agencies are orthogonal to the unobservables affecting effort by the native-born students in 1990. This is a stronger identification assumption than is required for the panel estimates.

Conditional on the assumption of the orthogonality of initial refugee location assignments prior to 1989 to the cross-sectional variation in unobservables in 1990, all of the estimates, including the statistical preferred IV estimates, in the table based on the Census-based measures of SEA students by parental background mimic those obtained from the panel estimates using the new ORR student-age refugee assignments between rounds. They are thus consistent with the myopic competition model – an increase in the number of strong SEA refugee students decreases homework time and increases class skipping frequency while an increase in the number of weak SEA refugee students increases homework time and reduces class skipping frequency, although not all of the coefficients are statistically significant. The point estimates that are statistically significant indicate that a one standard deviation in the number of strong SEA refugee students decreases native-born student weekly homework time by 14%, or by roughly one hour per week, while an increase in the number of weak SEA refugee students increases homework time by 21%, or 1.4 hours per week, and decreases the incidence of class skipping by 26%. We thus see consistent evidence in favor of competition effects on effort that are robust to estimation procedure and to measurement.

10. Peer Effects and Test Score Performance

In this section we address the question of how the change in the composition of students affects learning, the test scores of the high-ability native-born students. We would expect based on the empirical findings in Tables 7 and 10 that an increase in the number of strong SEA refugees would lower test scores, because such increases significantly lower native-born student effort and because effort positively and significantly affects test scores (Table 2). To estimate the reduced-form effect of changes in the number of SEA refugee students by ability type on test scores we employ the same specification and estimation procedures we used to estimate effort responses.

The first two columns of Table 11 report first-difference and FD-IV estimates of the effects of the two SEA refugee student variables on the standardized composite math and reading test scores of the high-ability native-born students using the panel data describing strong native-born students attending the same school in the 10th- and 12th-grades. The estimates indicate that an increase in the number of strong SEA refugees statistically significantly increases test scores, despite lowering homework time and increasing class skipping – the statistically-preferred first-difference point estimates indicate that a one standard deviation increase in the number of strong SEA refugee students increases test scores by 0.022 standard deviations.[39] The increase in the number of strong SEA refugee students also does not lower test score value added (the last two columns of the table), despite lowering effort; the coefficients are not statistically significant from zero.

These results appear to suggest the existence of a positive peer learning effect that dominates the negative competition effect on effort. To explore more directly whether competition nullifies positive peer learning effects we estimate separately the effects of changes in the number of strong and weak SEA refugee students on test scores across schools differentiated by whether grade competition is encouraged, using again the category of schools in which teachers strongly agreed with the statement that there is grade competition encouragement to stratify school type. Column three of Table 11 reports the first-differenced estimates by school type. These estimates indicate that the positive strong SEA refugee effect on native-born student test scores is only statistically significant in the schools that do not or only weakly encourage grade competition – the point estimate for the strong grade competition schools is 1/6th the size of that for the other schools and is not statistically significantly different from zero. Competition appears to nullify peer learning effects, as found by Chen and Hu (forthcoming) both by reducing assistance of peers but also because of direct effects on own effort. Estimates from the value-added specification, however, are not precise.

Do these positive effects from adding more high-ability students on test scores in the less-competitive schools reflect peer learning effects? To further pin down whether adding strong

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[39] The estimates of Table 11 indicate that a one standard deviation increase in the number of strong SEA students decrease weekly homework time by one hour. The one-hour decrease, based on the effort function point estimates would lower test scores by 0.13 standard deviations.

peers aids learning directly - net of effort effects - we re-estimate the individual achievement function allowing the number of strong SEA students to amplify effort effects. That is, we specify peer effects as a TFP booster, augmenting test scores for given effort levels, consistent with how we modeled peer learning effects in the model. The modified Cobb-Douglas achievement function thus is:

$$O_{ijtk} = (R_k^H)^{\alpha_0} \delta_j \mu_{ij} H_{ijt}^{\alpha_{1k}} e^{\alpha_{2k} S_{ijt}} C_{ijt}^{\alpha_{3k}} \varepsilon_{ijt} ,$$

where we expect $\alpha_0 > 0$ if there are positive peer learning effects. Note that this specification of the achievement function builds in peer learning complementarity – more able (higher μ_{ij}) students benefit more from more able student presence.

We estimate the augmented achievement function using FD-IV employing again the LASSO variables as instruments but also adding the instruments used to predict the changes in SEA refugee students across rounds, namely the projected number of school-age SEA refugees, the projected number of SEA women ages 40-55, and the projected number of women in that age group with at least a high-school education in the county in 1989 based on the initial county placements of SEA refugees by the Office of Refugee Resettlement from 1975 through 1989.

The first and third columns of Table 12 report the estimates of the augmented achievement function, but with the change in the number of strong SEA refugee students assumed to be orthogonal to the change in test score shocks. The estimates of homework time and class skipping effects on the two test score measures remain statistically significant, are similar in magnitude to those estimates in Table 2 obtained without the added peer effect, and are again evidently endogenously related to test score shocks. However, the test statistics in columns two and four indicate that the hypothesis that the added SEA student refugee variable is orthogonal to the changes in test score shocks cannot be rejected, and the coefficient estimates are similar by estimation procedure. The key new result is that, for given effort levels, adding strong SEA refugee students statistically significantly increases the test scores and test score value-added of the native-born students. The point estimates are small – a one standard deviation in strong SEA refugee students at the county level increases the native-born composite test score level by 0.03 standard deviations and test score value-added by 0.9%.

11. Are There Other Mechanisms?

The influx of refugees may have other effects on performance and test scores than via peer learning. The NELS:88 survey elicited information on classroom disruptions from the student respondents in both the 10th and 12th grades. Such disruptions are not trivial. Students were asked if they thought that classroom disruptions were such as to impede learning, with 33% (28%) of the strong native-born students responding affirmatively in the 10th (12th) grade. 44% (32%) of the strong native-born students reported they had experienced a theft in school in the 10th (12th) grade.

Parents and teachers may also react to the change in student composition. While there is no direct information in the survey data on teachers altering what they teach to suit particular ability groups, there is information obtained from them on the percentage of classroom time devoted to group, individual and whole-class instruction in the second and third survey rounds. And parents were asked in the same rounds if they helped with the student's homework – 52% said they did in the 10th grade and 66% in the 12th grade.[40]

Table 13 reports first differenced estimates (equation (9)) using the panel sample of high-ability native-born high-school students stratified by whether the school strongly encouraged grade competition of the effects of strong and weak SEA refugee students on the four alternative mechanisms: whether the parents helped with homework, the percent of classroom time teachers devoted to individual instruction, students' reporting on whether classroom disruption was a problem, and whether a student had personally experienced classroom theft.

The results in the third and fourth column indicate that the numbers of strong and weak SEA refugee students have no effects on classroom disruptions or on classroom theft in either type of school, or at least we do not have enough power to identify such effects. In contrast, the numbers of SEA refugee students by ability do have statistically significant effects on teacher and parent behavior, but only in schools in which grade competition is strongly encouraged. In the less or non-competitive schools where we found evidence of positive peer effects, such effects are evidently not proxying either teacher or parent behavior.

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[40] We are not cherry-picking variables – these are the only plausible alternative mechanisms for which there is information in both rounds of the high-school panel so that we can employ the first-difference estimation method.

In the strongly competitive schools parents appear to attempt to offset the effort effects on students induced by competition, with parents more likely to provide homework assistance when there is an increase in the number of strong SEA refugee students, when students are reducing their effort, and less likely to help when there are more weak SEA refugee students, when students are increasing their effort. Our test score results, showing that there are no positive effects from increasing the number high-ability peers on test scores in the strongly competitive schools indicates that these parent offsets do not fully overcome the direct negative effort effects on students.

Teachers also respond to the changes in student composition only in the competitive schools; the responses indicate they devote more time to individual instruction in the classroom when there are more strong SEA refugee students and less when there are more weak SEA refugee students. It is possible if the individuals that are receiving the extra attention from teachers are the weaker students that this change in teacher behavior could explain why high-ability native-born students in the more competitive schools do not benefit from the addition of high-ability students. However, the quantitative effect is small – a one standard deviation increase in the number of strong SEA refugee students increases the percentage of classroom time devoted to individuals by only 2.2 percentage points.

12. Conclusion and Implications for School Grade Compression

In this paper, we explored how the contest-nature of schools affects the acquisition of human capital focusing on the effects of school peers on both student effort choices and the returns to student effort. When academic rewards are based on student rankings, student peers thus play dual roles – as competitors who discourage effort and as educators who assist learning. We employed two Tullock-type tournament models incorporating direct peer effects to assess to what extent basing rewards on ranking alters student effort and affects peer learning. We derived the implications from a model in which students heterogeneous in ability anticipate that their own actions will be matched by students like themselves. This further reduces the returns to effort compared to the Nash case. In this setting we considered a student cohort consisting of two ability types and show that the entrance of additional high-ability students reduces effort levels by all students, but the addition of a low-ability student can increase effort levels of high-ability types, since there is less matching of effort among low-ability types. We showed that these predictions for the effort behavior of high-ability types is robust to low-ability types not competing and to the presence of direct peer learning effects that exhibit peer ability complementarity – greater benefits to peer learning for high-ability students. We also showed how it is possible to identify positive peer learning effects when adding high-ability peers reduces student effort in a setting in which student ranks determine rewards.

We tested the implications of the competition models exploiting two sources of variation in a nationally-representative US panel survey of students in high school that provides both student standardized test scores and measure of student effort – variation over time in the composition of students due to the additions of large numbers of Southeast Asian (SEA) refugees and variation across schools in the extent to which the schools encourage grade competition. We first showed using the panel survey data for students who remain in the same school between 10th and 12th grade that our two measures of effort – homework time and class skipping - significantly affect test scores and test score value-added and that higher-ability students carry out less homework where the encouragement of grade competition is strongest, a key implication of competition models.

Based on the change in the numbers of SEA refugees by ability, we then found that native-born student effort measured by homework time and class skipping intensity responds to the change in the numbers of refugee students by ability in conformity to the non-myopic tournament model. We also found that low-ability native-born students, classified by parental schooling, do not appear to compete. Based on ability criteria using the schooling of the parents, we find that only 14% to 36% of the top students ranked by ability behave as if in a competition.

We also found evidence of peer learning effects. First, despite the addition of high-ability refugee students reducing the effort of high-ability native-born students, on average additions of high-ability refugee students increase the test scores of the high-ability native-born. Second, more directly, we find that net of effort levels, adding high-ability refugee students directly increases the test scores of the native-born students. However, a key finding is that in schools that strongly encourage grade competition, the net effects of adding a high-ability students on test scores is not significantly different from zero – only in the schools in which grade competition is less or not encouraged do we observe the positive peer learning affect. This evidence is supported by the additional finding that high-ability students are significantly less likely to receive help from other students in the schools that strongly encourage grade competition.

The key takeaway from our findings is that student competition induced by rankings-based rewards reduces learning by both lowering student effort and by blunting peer learning effects, as peers become competitors. It is not likely that the search for the "best and brightest" by schools and employers will ever be curtailed.[41] However, there is another trend in both high schools and colleges that reduces student competition – the increase in grade compression (grade inflation). That is, the fraction of students receiving A grades has increased substantially, thus reducing distinctions among students. In elite universities, grade compression has increased as admission selectivity has also increased. For example, at Yale University, which recently issued a report based on administrative data on grades, the fraction of students receiving A grades rose from 41% in 2011 to 58% in 2022 as seen in Figure 5.

At the same time, admission selectivity also rose substantially at Yale, as also seen in Figure 5, such that between 2011 and 2022 the faction of students with mathematics SAT's between 700 and 800 rose from 79% to over 93%. This increase in the quality of students means that the potential for peer learning effects has also increased at Yale. Thus, the reduction in competition from increasing grade compression, our findings suggest, may enhance learning. Most educators, however, have bemoaned this trend, pointing to the fact that students get less feedback and have less incentive to work hard.[42]

Indeed, the association between student body quality and grade compression, as seen in Figure 6 from data compiled in Rojstaczer and Healy (2012), also is evident across universities. Moreover, the NELS:88 data indicate that grade competition is less empathized in selective, private high schools, compared with public and private, non-selective schools, as seen in Figure 7. While we do not know the reasons for grade inflation, our findings imply that grade inflation and compression may not be all bad in educational settings in which competition appears to

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[41] Indeed, the use of class rank to distribute rewards is increasing. The state of Ohio in 2023 introduced the Governor's Merit Scholarship (GMS), which provides up to a $20,000 scholarship to Ohio high school students in the top 5% of their class if they attend college in Ohio. https://highered.ohio.gov/educators/financial-aid/sgs/gms/.

[42] For example, "Easy A's signal to students they don't need to work hard to succeed…" https://www.forbes.com/sites/frederickhess/2023/09/05/grade-inflation-is-not-a-victimless-crime/.

inhibit effort and peer learning, especially among higher-ability student groups where peer learning is most prevalent and has the highest payoffs.

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Figure 1. Initial FIPS Locations of Southeast Asian Refugees, 1987-90

Data Source: Office of Refugee Resettlement

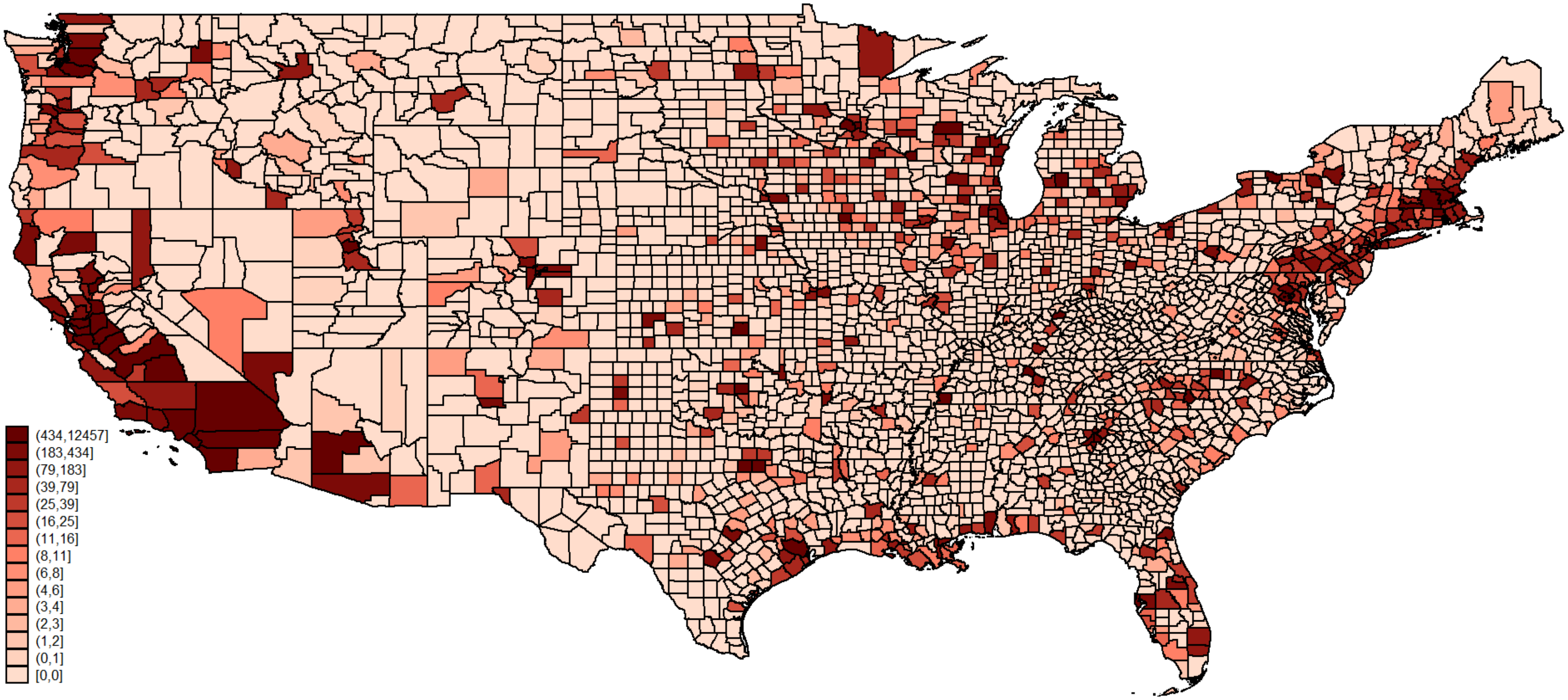
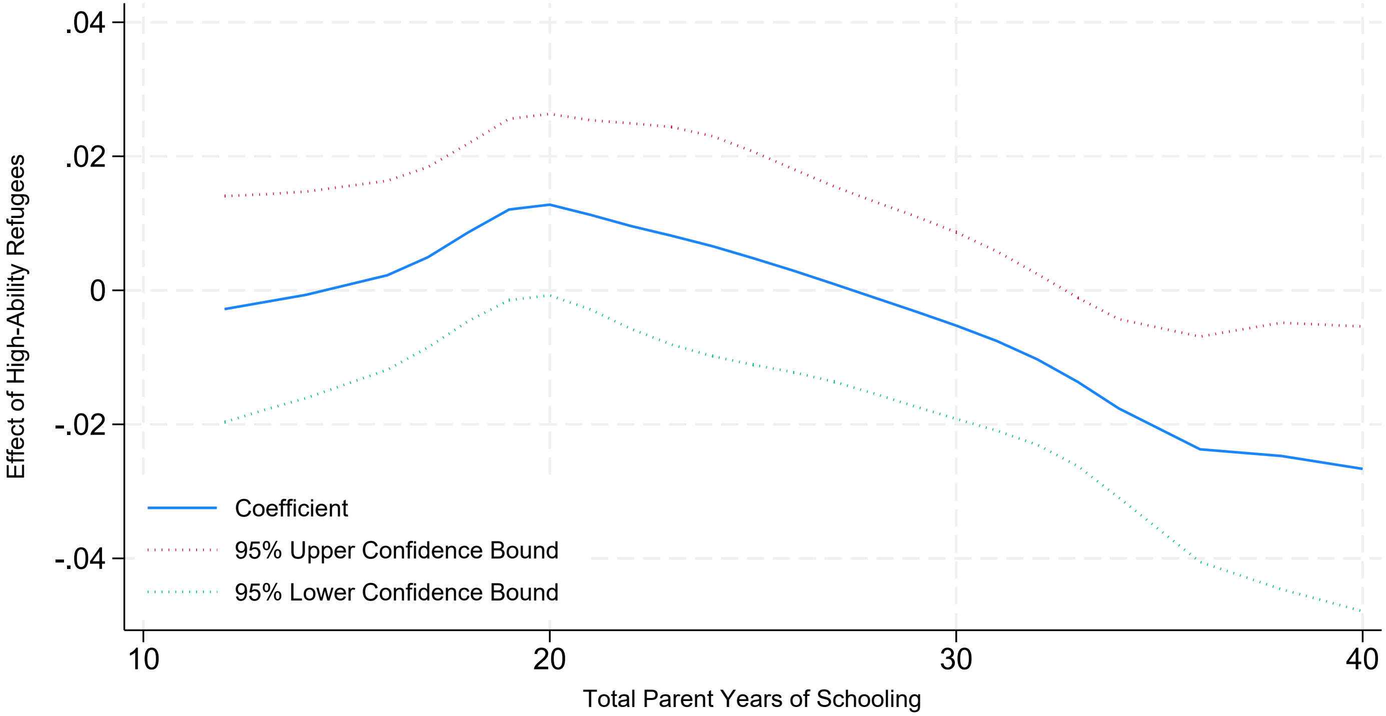


Figure 2: Estimated Effect of Increasing the Number of High-Ability SEA Refugee Students on Log Homework Hours of Native-Born Students, by Total Parent Years of Schooling

SOURCE: US Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988



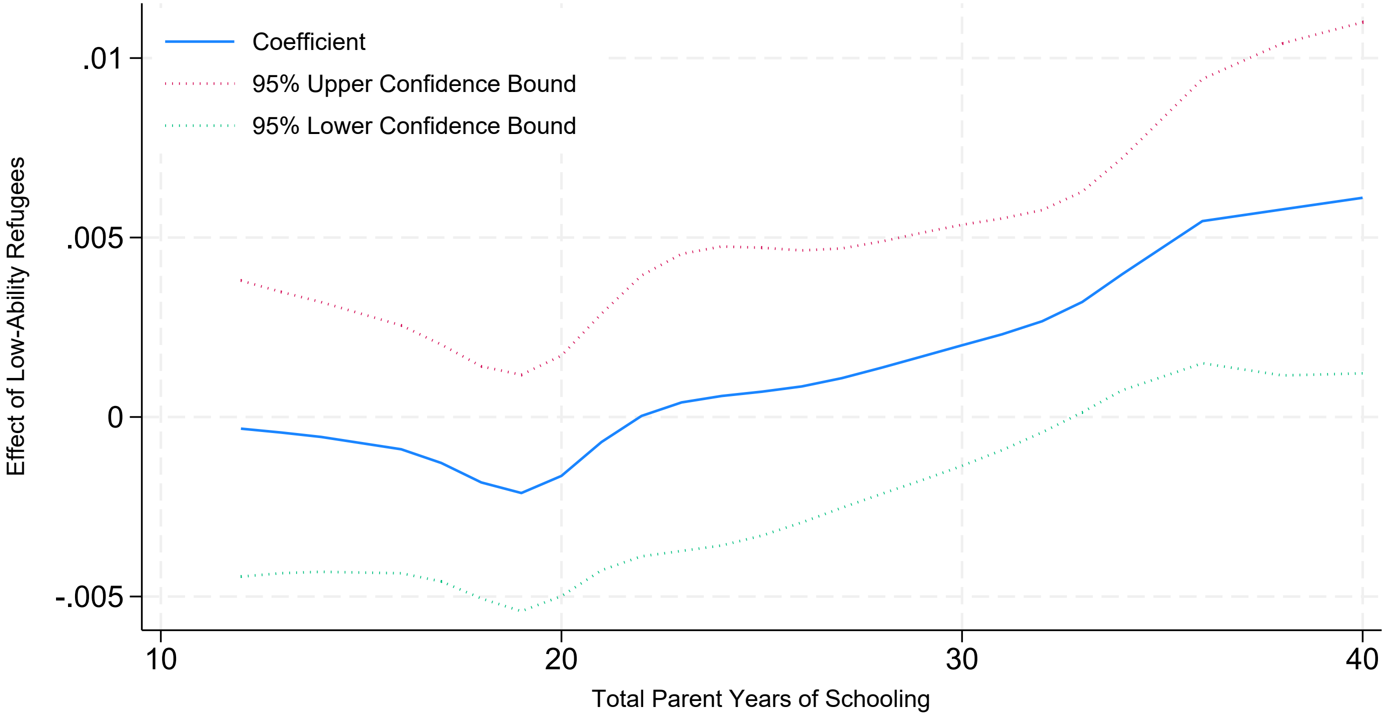


Figure 3: Estimated Effect of Increasing the Number of Low-Ability SEA Refugee Students on Log Homework Hours of Native-Born Students, by Total Parent Years of Schooling

SOURCE: US Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988

Figure 4: Relationship Between Projected 10th-Grade Age SEA Refugees from ORR Records and Census Counts in 1990

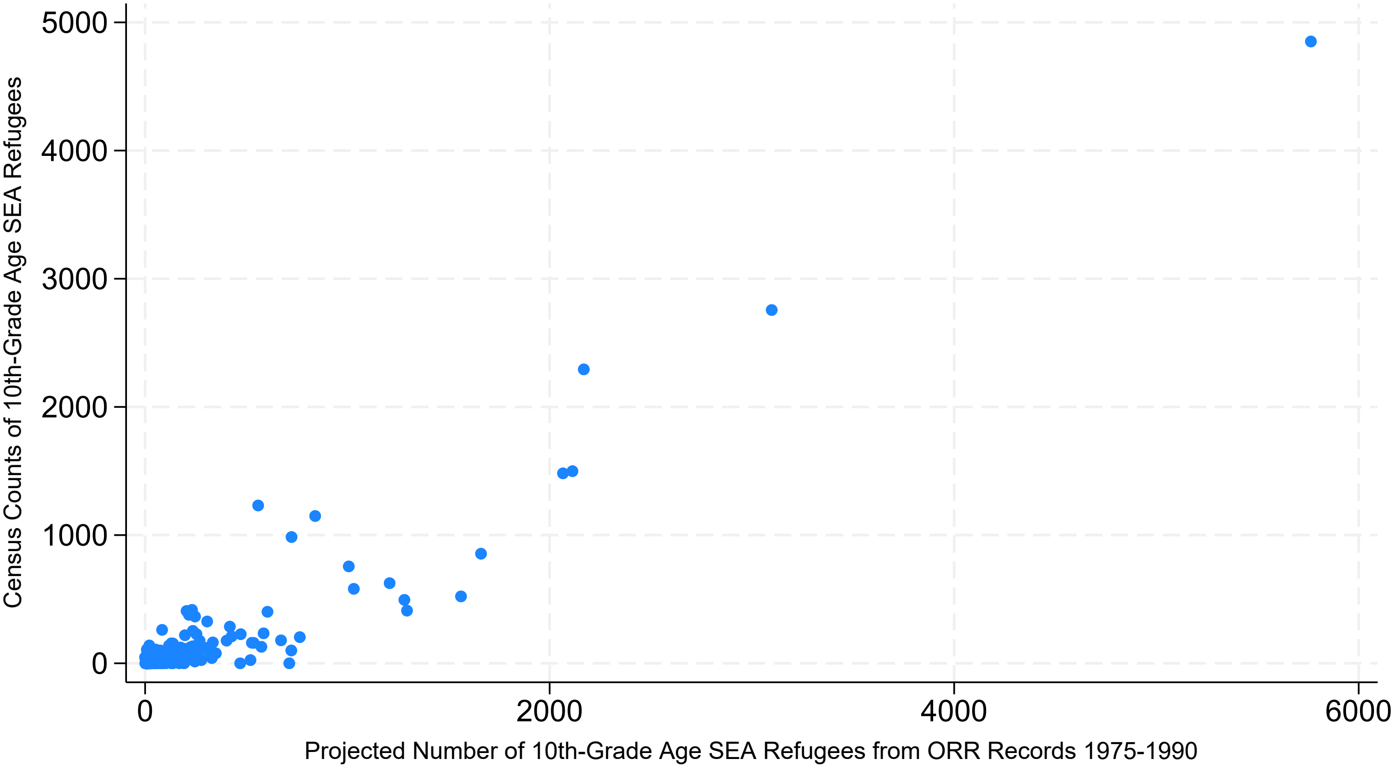


Figure 5: Student Quality and Grade Compression of Yale College Students, 2011-2022

SOURCE: Yale Office of Instructional Research

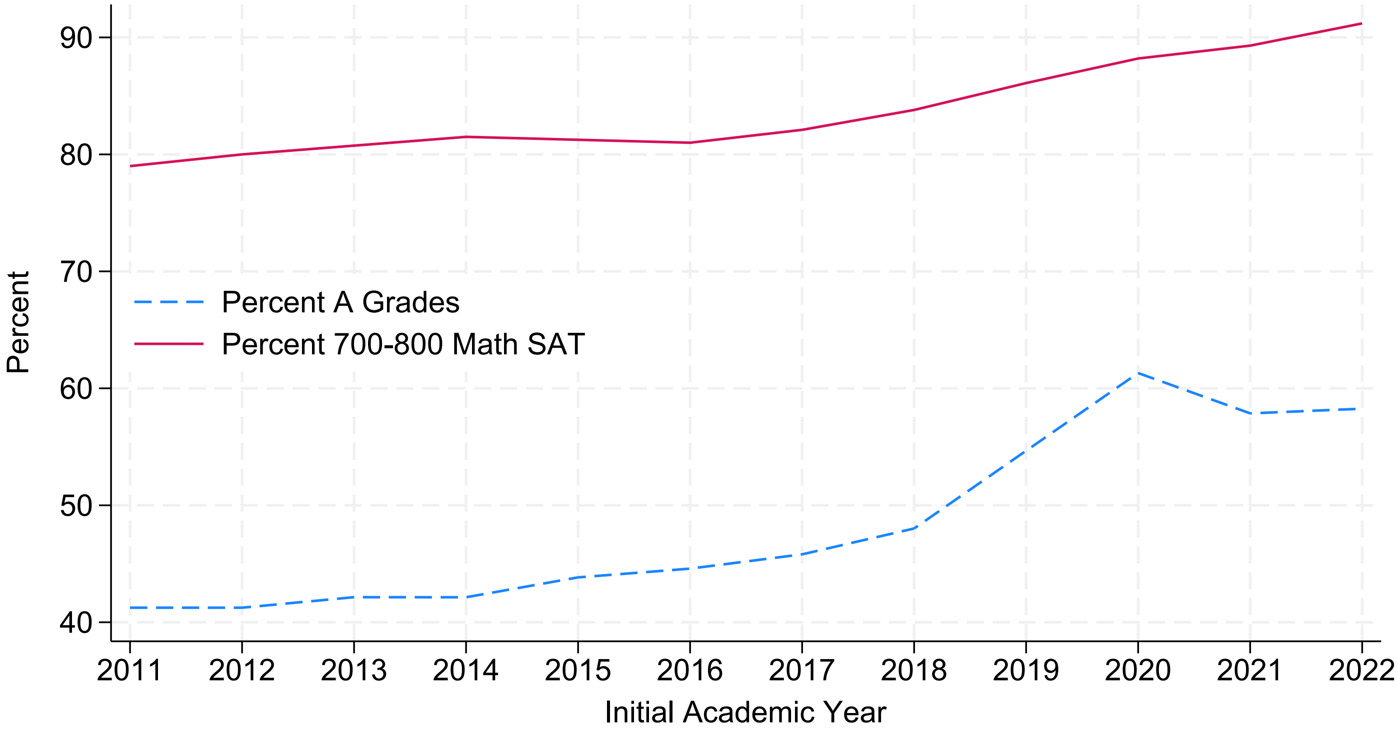
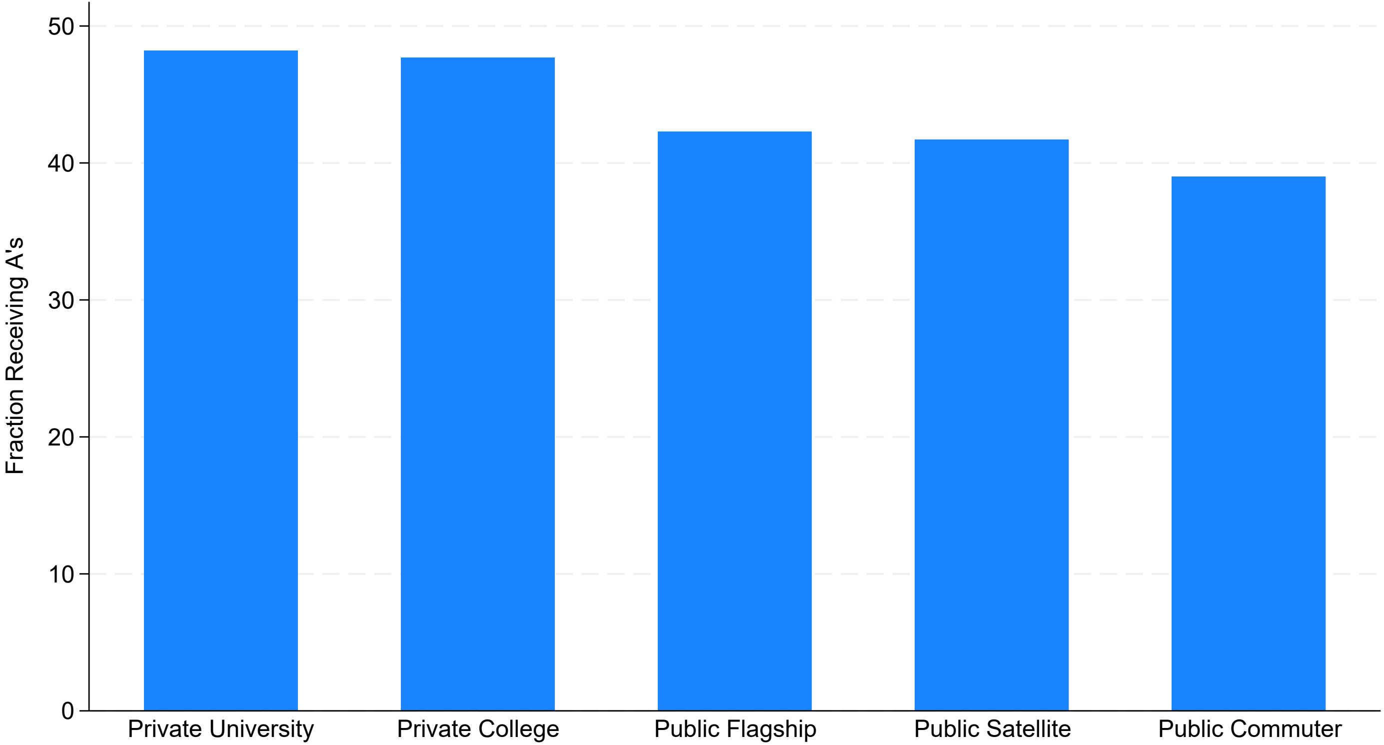


Figure 7: Fraction of Students Receiving A Grades by Tertiary Institution Type in 2009
SOURCE: Rojstaczer and Healy, 2012



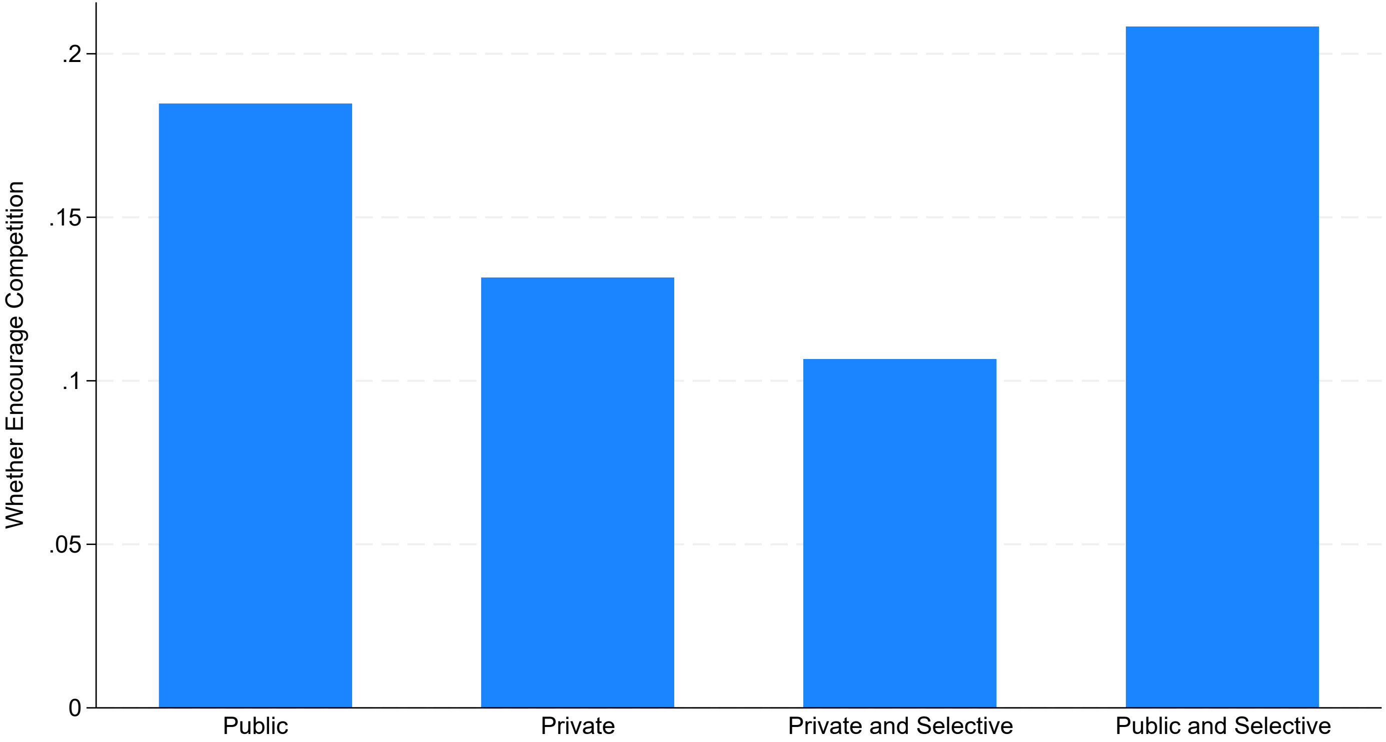


Figure 7: Whether Encourage Grade Competition, by School Type

SOURCE: US Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988

Table 1

Descriptive Statistics for Panel Sample of Native-Born Students Staying in the Same School, 1990-92 and Present in All Three Rounds

Variable/year	1990 (10th grade)		1992 (12th grade)
Standardized composite math and English test score	54.8 (9.39)		54.5 (9.24)
Weekly homework hours	5.39 (5.03)		7.23 (5.86)
Skipped any classes	0.322 (0.467)		0.511 (0.500)
School cohort size	335.6 (229.1)		280.5 (177.6)
Age	16.2 (0.485)		18.2 (0.485)
1988 standardized composite math and reading test score		55.0 (10.0)	
Mother's years of schooling		13.7 (2.51)	
Father's years of schooling		14.3 (3.09)	
Mother a teacher		0.087	
Father hopes student attends college		0.780	
Father co-resides with student		0.977	
Grandparent co-resides with student		0.040	
Black		0.054	
Male		0.488	
N		3,290	

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1988, 1990, and 1992." Means (standard deviations) in the columns.

Table 2

Achievement Function Panel Estimates: the Effects of Log Homework Time and Class Skipping on Log Test Score Performance and Log Test Score Improvement for Native-Born High-School Students, by Estimation Procedure

Dependent variable	Log Standardized Composite Test Score			Log Composite Test Score Improvement		
Variable/estimation procedure	GLS	First-difference	FD-IV	GLS	First-difference	FD-IV
Log weekly homework hours	0.00839** (0.00081)	0.00326** (0.00085)	0.0321** (0.0154)	0.00316** (0.00069)	0.00619** (0.00112)	0.0578** (0.0228)
Skip any classes	-0.0140** (0.00242)	-0.00602** (0.00255)	-0.0890* (0.0523)	-0.00945** (0.00216)	-0.00869** (0.00350)	-0.165** (0.0759)
Log school cohort size	-0.0149** (0.003209)	0.0139** (0.00615)	0.00419 (0.0122)	0.00499** (0.00133)	0.0318** (0.00975)	0.0346* (0.0209)
N	6,590	3,290	3,280	6,590	3,290	3,200
Hausman test $\chi^2(3)$ [p]	616.8 [0.000]		-	12.54 [0.0057]		-
Kleinbergen-Paap under-identification test $\chi^2(5)$ [p]	-	-	25.8 [0.0069]	-	-	25.8 [0.0069]
Anderson-Rubin Wald test of weak instruments $\chi^2(6)$ [p]	-	-	23.27 [0.0255]	-	-	28.63 [0.0045]
Hansen *J*-statistic over-identification test $\chi^2(4)$ [p]	-	-	7.62 [0.666]	-	-	5.52 [0.85]
Endogeneity test $\chi^2(2)$ [p]	-	-	6.35 [0.0419]	-	-	12.96 [0.0015]

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey 1988, 1990 and 1992." Log test score improvement = Log test score in year *t* - log test score in year *t* - 2. Standard errors in parentheses clustered by school. Log weekly hours and class skipping are treated as endogenous variables in columns 3 and 6. Instruments for the IV estimates selected by LASSO include whether the mother is a teacher, whether the father resides in the household, whether the father hopes the student attends college, the presence of grandparents, whether the student is male, whether the student is Black, whether the student is age 17, whether the student is age 20, whether the mother is a high-school graduate, whether the mother has 13 years of schooling, whether the father has 10 years of schooling, and whether the father has 13 years of schooling. The sample consists of native-born students attending the same school with test scores in both the 10th and 12th grades. Only 3.7% of native-born students in the panel sample did not attend the same school in both survey years.

* 10% significance, two-tailed test; **5% significance, two-tailed test.

Table 3

School Fixed-Effect Estimates of the Effects of Ability, Measured by the Log 8th-Grade Test Score using the 10th-Grade Log Test Score Fixed Effect as an Instrument, on Log Weekly Homework Hours, Class Skipping, and Classmate Help for High School Native-born (12th-Grade) Seniors, and by Whether Grade Competition is Encouraged in the School

12th-Grade Behavior	Log Weekly Homework Hours			Skipped Classes			Classmate Helped with Homework		
Variable/Estimation procedure	OLS	IV	IV	OLS	IV	IV	OLS	IV	OLS
Log 8th-grade test score, all schools	1.36** (0.164)	0.487** (0.217)	-	-0.162** (0.045)	0.0881 (0.0588)	-	0.185** (0.0379)	0.166** (0.048)	-
Log 8th-grade test score, schools in which grade competition is less or not encouraged	-	-	0.836** (0.268)	-	-	-0.0199 (0.0736)	-	-	0.252** (0.0453)
Log 8th-grade test score, schools in which grade competition is encouraged	-	-	-0.275 (0.425)	-	-	0.319** (0.118)	-	-	0.0978 (0.0689)
N	3,940	3,940	3,610	3,960	3,960	3,620	3,920	3,940	3,940
Test statistic H_0: ability effect on effort equal across grade-competitive and grade non-competitive schools, $\chi^2(1)$ [p]	-	-	4.89 [0.0271]	-	-	5.96 [0.0146]	-	-	3.48 [0.0620]
Kleinbergen-Paap under-identification test $\chi^2(5)$ [p]	-	1122 [0.000]	262.98 [0.000]	-	1119 [0.000]	257.3 [0.000]	-	1113 [0.000]	-
Anderson-Rubin Wald test of weak instruments $\chi^2(1,2)$ [p]	-	4.96 [0.0259]	10.0 [0.0068]	-	2.26 [0.133]	7.66 [0.0218]	-	11.66 [0.001]	-
Endogeneity test $\chi^2(2)$ [p]	-	37.1 [0.000]	31.9 [0.000]	-	45.7 [0.000]	42.0 [0.000]	-	0.392 [0.531]	-

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey 1988, 1990, and 1992." Robust standard errors in parentheses clustered at the school level. The IV instrument is the 1990 log test score fixed effect computed from the third-column IV estimates in Table W. All specifications include school fixed effects and the LASSO controls: whether the mother is a teacher, whether the father resides in the household, whether the father hopes the student attends college, the presence of grandparents, whether the student is male, whether the student is Black, whether the student is age 17, whether the student is age 20, whether the mother is a high-school graduate, whether the mother has 13 years of schooling, whether the father has 10 years of schooling, and whether the father has 13 years of schooling.

* 10% significance, two-tailed test; **5% significance, two-tailed test.

Table 4
Comparison of Strong and Weak Students Using Parental Schooling Criterion for the Cutoff,
for Native-Born and SEA Students in the 12th Grade

Outcome	Standardized Test Score		Weekly Homework Hours		Number of Classes Skipped		Took College Entrance Exam	
Student type	Native-born	SEA Refugee	Native-born	SEA Refugee	Native-born	SEA Refugee	Native-born	SEA Refugee
Strong	56.6	58.3	8.10	9.66	2.10	1.79	0.878	0.929
	(8.27)	(9.62)	(0.123)	(5.81)	(3.18)	(3.33)	(0.327)	(0.267)
Weak	50.1	51.3	6.35	7.56	2.44	2.41	0.661	0.865
	(9.25)	(8.61)	(0.091)	(5.71)	(3.56)	(3.87)	(0.473)	(0.344)
t-statistic: within-group strong = weak[p]	25.9 [0.000]	1.87 [0.065]	11.7 [0.000]	0.25 [0.798]	4.95 [0.000]	0.557 [0.579]	19.6 [0.000]	0.655 [0.514]
t-statistic: strong SEA= strong native-born [p]	0.341 [0.733]		0.618 [0.537]		0.505 [0.614]		0.577 [0.564]	
Sample N	5,160	70	6,110	90	6,140	90	6,050	90

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1992." Strong students are defined by whether both the mother's and father's schooling exceed high school. Sample weights are used.

Table 5
Predicted Signs of Coefficients in the Estimating Equation Based on the Non-myopic Tournament Model with a Direct Peer Effect

Outcome variable	Homework Time		Skipping Class		Test Score
Coefficient/Student *g*	Strong	Weak	Strong	Weak	Strong
β_{1g}	+	- or 0	-	+ or 0	0
β_{2g}	-	- or 0	+	+ or 0	?

Table 6
Descriptive Statistics for Panel Sample of Strong Native-Born Students Staying in the Same School Experiencing a Change in Student-Age SEA Refugees in the County, 1990-92

Variable/year	1990 (10th grade)	1992 (12th grade)
Standardized composite math and English test score	57.4 (8.76)	57.3 (8.57)
Weekly homework hours	6.68 (5.27)	8.48 (6.56)
Skipped any classes	0.335	0.523
Number of times skipped classes	1.06 (2.30)	2.25 (3.44)
School cohort size	329.2 (235.0)	283.6 (188.7)
Number of students in the county	14,752 (21,959)	10,206 (14,027)
Percentage class time teacher devotes to individual students	12.6 (10.1)	13.1 (11.7)
Parents helped with homework	0.517	0.644
Student was robbed in class	0.434	0.330
Student thought class disruptions were a problem	0.306	0.264
Number of new assigned weak school-age SEA refugees in the county 1991-92 (ORR)	35.4 (71.3)	
Number of new assigned strong school-age SEA refugees in the county 1991-92 (ORR)	7.18 (16.7)	
Mother's years of schooling	15.1 (1.98)	
Father's years of schooling	16.1 (2.42)	
Mother a teacher	0.113	
Father hopes student attends college	0.862	
Father co-resides with student	0.973	
Grandparent co-resides with student	0.036	
Black	0.055	
Number of counties	185	
Number of students	2,170	

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1988, 1990, and 1992" and the Office of Refugee Resettlement monthly refugee assignments by county in 1991 and 1992.

Table 7
Panel Estimates of the Effects of the Numbers of Student-Age SEA Refugees in the County by Type
on the Log Homework Time of and Class Skipping by Strong Native-Born High-School Students, by Estimation Procedure

Dependent variable	Log Weekly Homework Hours		Skipped Any Classes		Class Skipping Frequency	
Variable/estimation procedure	First-difference	FD-IV	First-difference	FD-IV	First-difference	FD-IV
Number of assigned weak school-age SEA refugees in the county 1991-1992	0.00505** (0.00153)	0.00548** (0.00170)	-0.00047 (0.00085)	-0.00116 (0.00101)	-0.0118** (0.00713)	-0.0119* (0.00754)
Number of assigned strong school-age SEA refugees in the county 1991-1992	-0.0193** (0.00658)	-0.0207** (0.00730)	0.00385 (0.00361)	0.00718** (0.00428)	0.0500* (0.0314)	0.0528* (0.0333)
School cohort size	-0.000643**a (0.000326)	-0.000643**a (0.000326)	-0.000432**a (0.000169)	-0.000441**a (0.000169)	-0.00274**a (0.00109)	-0.00276**a (0.00108)
Size of cohort in the county	0.0000116**a (0.0000029)	0.0000122**a (0.0000031)	0.0000015 (0.0000015)	0.0000020 (0.0000016)	-0.0000322**a (0.0000153)	-0.0000292**a (0.0000164)
Includes LASSO control variables	Y	Y	Y	Y	Y	Y
N	2,170	2,170	2,170	2,170	2,170	2,170
Kleinbergen-Paap under-identification test $\chi^2(117)$ [p]	-	146.3 [0.035]	-	146.3 [0.035]	-	146.3 [0.035]
Cragg-Donald Wald F test of weak instruments	-	68.5	-	68.5	-	68.5
Hansen *J*-statistic over-identification test $\chi^2(116)$ [p]	-	126.8 [0.232]	-	112.9 [0.563]	-	113.3 [0.554]
Endogeneity test $\chi^2(2)$ [p]	-	0.298 [0.862]	-	3.088 [0.214]	-	4.27 [0.118]

SOURCEs: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1990 and 1992" and the Office of Refugee Resettlement. Standard errors in parentheses clustered by county. The LASSO control variables include whether the mother is a teacher, whether the father resides in the household, whether the father hopes the student attends college, the presence of grandparents, whether the student is male, whether the student is Black, whether the student is age 17, whether the student is age 20, whether the mother is a high-school graduate, whether the mother has 13 years of schooling, whether the father has 10 years of schooling, and whether the father has 13 years of schooling. The sample includes native-born students whose mother and father have schooling above high school, who attended the same school in 10th and 12th grade, and who resided in a county that experienced new assignments of school-age SEA refugees between 1990 and 1992. The excluded instruments in columns 3 and 6 are county fixed effects, the projected total number of SEA refugees in 1989 who would be ages 16-18 in 1989, the projected number of SEA female refugees with schooling greater than high school in the county who would be aged 35-50 in 1989, and the total number of SEA refugees in the county in 1989 with at least 10 years of residence in the United States based on the county assignments of the SEA refugees from 1980 through 1989 by ORR. *a10% significance, two-tailed test; **a5% significance, two-tailed test. *10% significance, one-tailed test; **5% significance, one-tailed test.

Table 8
Panel Fixed-effect Estimates: Comparison of the Effects of the Numbers of Student-Age SEA Refugees in the County by Type on Log Homework Time and Class Skipping, by Native-born Student Type

Variable	Log Homework Time	Skipped Any Classes	Class Skipping Frequency
Treated: Strong native-born students			
Number of weak school-age SEA refugees in the county	0.00477**	-0.00040	-0.0113**
	(0.00145)	(0.00084)	(0.00664)
Number of strong school-age SEA refugees in the county	-0.0179**	0.00354	0.0478**
	(0.00616)	(0.00357)	(0.0290)
Treated: Weak native-born students			
Number of weak school-age SEA refugees in the county	-0.00182*	-0.000518	-0.00483*
	(0.00146)	(0.000433)	(0.00350)
Number of strong school-age SEA refugees in the county	0.00877*	0.00327**	0.0282**
	(0.00586)	(0.00155)	(0.0140)
N	4,710	4,710	4,710
H_0 test: Strong SEA refugee effect = by native-born ability type $\chi(1)$ [p]	16.4	0.005	0.36
	[0.000]	[0.945]	[0.550]
H_0 test: Weak SEA refugee effect = by native-born ability type $\chi(1)$ [p]	17.5	0.01	0.72
	[0.000]	[0.903]	[0.396]
H_0 test: No native-born weak student response $\chi(2)$ [p]	2.75	4.60	4.49
	[0.253]	[0.100]	[0.106]
H_0 test: No native-born strong student response $\chi(2)$ [p]	12.4	7.53	2.90
	[0.002]	[0.023]	[0.235]

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1990 and 1992" and the Office of Refugee Resettlement monthly refugee assignments by county. Standard errors in parentheses clustered by county. All specifications include the LASSO control variables listed in Table Z. All coefficients are allowed to differ by the ability group of the native-born students. Strong native-born students are those whose mother and father has schooling above high school; weak native-born students are the remainder. All treated students in the panel sample attended the same school in 10th and 12th grade and resided in a county in which new school-age SEA refugees were assigned between 1990 and 1992. County-level strong and weak SEA refugee students are from the ORR administrative records for the panel.

*10% significance, one-tailed test; **5% significance, one-tailed test.

Table 9

Descriptive Statistics for Census-Matched Sample of Strong 10th-grade Native-Born Students in 1990

Variable/year	Mean (SD)
Weekly homework hours	6.47 (5.13)
Skipped any classes	0.335
Number of times skipped classes	1.08 (2.33)
School 10th-grade cohort size	332.3 (231.7)
Number of 10th-grade students in the county	13,248 (21,636)
Number of weak 10th-grade school-age SEA refugees in the county (Census)	289.0 (791.2)
Number of strong 10th-grade school-age SEA refugees in the county (Census)	69.8 (185.0)
Number of projected 10th-grade school-age SEA refugees in the county based on initial location assignments (ORR)	527.7 (1154)
Number of projected SEA refugee women 35-50 in the county based on initial location assignments 1975-1989 (ORR)	687.6 (1569)
Number of projected SEA refugee women 35-50 with greater than a high-school education in the county based on initial location assignments 1975-1989 (ORR)	265.8 (654.8)
Number of projected SEA refugees in the county since entry based on initial location assignments 1975-1989 (ORR)	2764.6 (6442,.6)
Student age	16.2 (0.449)
Private school	0.300
Selective admissions school	0.225
Private and selective school	0.213
Grade competition encouraged in the school	0.151
Male	0.492
Number of schools	618
Number of counties	247
Number of students	2,950

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Student, School, and Parent Survey, 1990," 1990 Census 1% and 5% PUMS, and the Office of Refugee Resettlement monthly refugee assignments by county, 1975-1989.

Table 10

Cross-section Estimates of the Effects of the Numbers of Student-Age SEA Refugees in the County by Type
on the Log Homework Time and Class Skipping of Strong 10th-grade Native-Born High-School Students, by Estimation Procedure

Dependent variable	Log Weekly Homework Hours		Skipped Any Classes		Class Skipping Frequency	
Variable/estimation procedure	OLS	IV	OLS	IV	OLS	IV
Number of weak school-age SEA refugees in the county in 1990 (Census)	0.0000927 (0.0000766)	0.000276** (0.000122)	-0.000122** (0.0000368)	-0.000107** (0.0000452)	-0.000793** (0.000284)	-0.000323 (0.000307)
Number of strong school-age SEA refugees in the county in 1990 (Census)	-0.000397** (0.000186)	-0.000755** (0.000268)	0.00014* (0.000104)	0.000115 (0.000105)	0.00124** (0.000567)	0.000399 (0.000623)
Size of the 10th-grade class in the school	-0.0000154 (0.000117)	-0.0000197 (0.00012)	0.000087 (0.000077)	0.000087 (0.000077)	0.000659*a (0.000361)	0.000648*a (0.000367)
Number of 10th-grade students in the county	0.0000002 (0.0000022)	-0.0000038 (0.0000032)	0.0000037**a (0.0000010)	0.0000034**a (0.0000012)	0.0000152**a (0.0000070)	-0.0000048 (0.0000083)
Includes commuting-zone fixed effects	Y	Y	Y	Y	Y	Y
Includes school control variables and parent's schooling	Y	Y	Y	Y	Y	Y
N	2,900	2,900	2,920	2,920	2,920	2,920
Kleinbergen-Paap under-identification test $\chi^2(3)$ [p]	-	13.0 [0.005]	-	12.9 [0.005]	-	12.9 [0.005]
Cragg-Donald Wald F test of weak instruments	-	948.8	-	953.2	-	733.4
Hansen *J*-statistic over-identification test $\chi^2(4)$ [p]	-	5.24 [0.07]	-	3.50 [0.174]	-	1.31 [0.519]
Endogeneity test $\chi^2(2)$ [p]	-	4.26 [0.039]	-	0.800 [0.371]	-	3.78 [0.052]

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Student, School, and Parent Survey, 1990," the 1990 Census Public-Use 1% and 5% samples, and the Office of Refugee Resettlement monthly refugee assignments. Standard errors in parentheses clustered by county (165) and school (496). The sample includes 10th-grade native-born students whose mother and father have schooling above high school and who were residing in counties that are identified in the Public-Use 1990 Census. Strong SEA refugee students are those whose mother and father had more than a high-school education and weak SEA refugee students are those in the complement. The school control variables include whether the school is a public or private school, whether school admissions are selective, and whether the school encourages grade competition. The excluded instruments in columns 2 and 4 are the projected total number of SEA refugees in 1989 who would be ages 16-18 in 1989, the projected number of SEA female refugees with schooling greater than high school in the county who would be aged 35-50 in 1989, and the total number of SEA refugees in the county in 1989 with at least 10 years of residence in the United States based on the county assignments of the SEA refugees from 1980 through 1989 by ORR. *a10% significance, two-tailed test; **a5% significance, two-tailed test. *10% significance, one-tailed test; **5% significance, one-tailed test.

Table 11

Comparison of the Estimates of the Effects of the Number of Strong and Weak SEA School-age Refugees in the County on Test Score Performance and Test Score Improvement for Strong Native-Born High-School Students, by Estimation Procedure and School Grade Competition

Dependent variable	Standardized Composite Test Score			Composite Test Score Improvement		
Variable/estimation procedure	First-difference	FD-IV	First-difference	First-difference	FD-IV	First-difference
Number of assigned weak school-age SEA refugees in the county: all schools	-0.00523 (0.00544)	-0.00844 (0.00587)	-	-0.00986 (0.0122)	-0.0188 (0.0165)	-
Number of assigned strong school-age SEA refugees in the county: all schools	0.0460** (0.0233)	0.0582** (0.0247)	-	0.0688 (0.0501)	0.105 (0.0694)	-
Number of assigned strong school-age SEA refugees in the county: grade-competition encouraged	-	-	0.00800 (0.1245)	-	-	0.287 (0.277)
Number of assigned strong school-age SEA refugees in the county: grade competition less/not encouraged	-	-	0.0505** (0.0212)	-	-	0.0512 (0.0402)
Number of assigned weak school-age SEA refugees in the county: grade-competition encouraged	-	-	-0.00852 (0.0283)	-	-	-.0817 (0.0633)
Number of assigned weak school-age SEA refugees in the county: grade competition less/not encouraged	-	-	-0.00452 (0.00511)	-	-	-0.00231 (0.00998)
N	1,840	1,840	1,840	1,800	1,800	1,800
Kleinbergen-Paap under-identification test $\chi^2(116)$ [p]	-	141.2 [0.056]	-	-	134.2 [0.084]	-
Cragg-Donald Wald F statistic	-	67.9	-	-	65.2	-
Hansen *J*-statistic over-identification test $\chi^2(115)$ [p]	-	118.6 [0.390]	-	-	114.8 [0.409]	-
Endogeneity test $\chi^2(2)$ [p]	-	0.438 [0.803]	-	-	1.26 [0.533]	-

SOURCE: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1988, 1990 and 1992” and the Office of Refugee Resettlement. Test score improvement = test score in year *t* - test score in year *t* - 2. Standard errors in parentheses clustered by county. All specifications include school cohort size and the total number of students in the same grade in the county weighted by the survey sample weights. The sample consists of native-born students attending the same school in the same county with test scores in both the 10th and 12th grades both of whose parents had schooling beyond high school. Only 3.7% of these native-born students in the 1990 NELS:88 round did not attend the same school in 1992. The number of strong and weak school-age SEA refugees are treated as endogenous variables in columns 2 and 4 (FD-IV). Instruments for the IV estimates include the projected number of school-age SEA refugees, the projected number of SEA women ages 40-55, and the projected number of women in that age group with at least a high-school education in the county in 1989 based on the initial county placements of SEA refugees, by age and gender, by ORR from 1980 through 1989, and commuting-zone fixed effects.

* 10% significance, two-tailed test; **5% significance, two-tailed test.

Table 12

First-difference IV Achievement Function Estimates: the Effects of Log Homework Time and Class Skipping and Strong SEA Student-Age Refugees in the County on Log Test Score Performance and Log Test Score Improvement for Native-Born High-School Students, by Estimation Procedure

Dependent variable	Log Standardized Composite Test Score		Log Test Score Improvement	
Variable/specification	Effort variables endogenous	Effort variables + SEA refugees endogenous	Effort variables endogenous	Effort variables + SEA refugees endogenous
Log weekly homework hours	0.0285* (0.0150)	0.0270* (0.0143)	0.0539** (0.0225)	0.0480** (0.0240)
Skip classes	-0.0886* (0.0542)	-0.0934* (0.0568)	-0.168** (0.0781)	-0.186** (0.0902)
Log school cohort size	0.00536 (0.0117)	0.00460 (0.0118)	0.0354* (0.0203)	0.0326 (0.0202)
Number of assigned strong school-age SEA refugees in the county	0.000393** (0.000149)	0.000393** (0.000160)	0.000527** (0.000218)	0.000550** (0.000246)
N	3,260	3,260	3,260	3,260
Kleinbergen-Paap under-identification test $\chi^2(5)$ [p]	25.7 [0.0073]	26.9 [0.0127]	25.7 [0.0073]	26.9 [0.0127]
Anderson-Rubin Wald test of weak instruments $\chi^2(12)$ [p]	21.7 [0.0416]	-	27.1 [0.0074]	-
Anderson-Rubin Wald test of weak instruments $\chi^2(15)$ [p]	-	33.09 [0.0046]	-	33.2 [0.0044]
Hansen *J*-statistic over-identification test $\chi^2(4)$ [p]	8.00 [0.629]	8.41 [0.752]	5.72 [0.838]	7.05 [0.854]
Endogeneity test: effort variables $\chi^2(2)$ [p]	5.42 [0.067]	5.27 [0.0719]	11.8 [0.0027]	11.0 [0.0040]
Endogeneity test: SEA refugees $\chi^2(1)$ [p]	-	0.004 [0.953]	-	0.011 [0.916]

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Panel, Student, School, and Parent Survey, 1988, 1990, and 1992" and the Office of Refugee Resettlement. The sample consists of native-born students attending the same school with test scores in the 8th, 10th and 12th grades. Only 3.7% of native-born students in the panel sample did not attend the same school in both survey years. Log test score improvement = Log test score in year *t* - log test score in year *t* - 2. Standard errors in parentheses clustered by school. Instruments for the estimates in columns 1 and 3 include the LASSO variables: whether the mother is a teacher, whether the father resides in the household, whether the father hopes the student attends college, the presence of grandparents, whether the student is male, whether the student is Black, whether the student is age 17, whether the student is age 20, whether the mother is a high-school graduate, whether the mother has 13 years of schooling, whether the father has 10 years of schooling, and whether the father has 13 years of schooling. Instruments for the estimates in columns 2 and 4 add to the LASSO variables the projected number of school-age SEA refugees, the projected number of SEA women ages 40-55, and the projected number of women in that age group with at least a high-school education in the county in 1989 based on the initial county placements of SEA refugees from the ORR records, by age and gender, from 1975 through 1989. * 10% significance, two-tailed test; **5% significance, two-tailed test.

Table 13

First Difference Panel Estimates: Comparison of the Effects of the Number of Student-Age SEA Refugees in the County by Type on Parent and Teacher Behavior and Classroom Disruptions for Native-born Strong High-School Students, by whether Grade Competition is Encouraged in the School

Variable/dependent variable	Parents Help with Homework	% Classroom Time Individual Instruction	Classroom Disruption a Problem	Robbed in Classroom
Grade competition is encouraged in the school				
Number of assigned weak school-age SEA refugees in the county	-0.00770** (0.00378)	-0.159** (0.0733)	0.00173 (0.00198)	-0.00114 (0.00281)
Number of assigned strong school-age SEA refugees in the county	0.0383** (0.01698)	0.461* (0.258)	-0.00579 (0.00815)	0.00441 (0.0121)
Grade competition is less or not encouraged in the school				
Number of assigned weak school-age SEA refugees in the county	0.000696 (0.000958)	0.00552 (0.0237)	0.000362 (0.000582)	0.000260 (0.00111)
Number of assigned strong school-age SEA refugees in the county	-0.000262 (0.00404)	0.0228 (0.0949)	-0.000358 (0.00245)	-0.00134 (0.00468)
H_0: Weak SEA refugee effect = by grade competition $\chi(1)$ [p]	4.71 [0.030]	3.92 [0.048]	0.43 [0.51]	0.23 [0.63]
H_0: Strong SEA refugee effect = by grade competition $\chi(1)$ [p]	4.92 [0.027]	2.07 [0.15]	0.39 [0.53]	0.21 [0.65]
H_0: No SEA refugee effect when grade competition $\chi(2)$ [p]	6.48 [0.039]	4.79 [0.09]	0.99 [0.61]	0.17 [0.92]
H_0: No SEA refugee effect when no grade competition $\chi(2)$ [p]	5.55 [0.062]	1.46 [0.66]	2.80 [0.25]	0.13 [0.94]

SOURCES: U.S. Department of Education, National Center for Education Statistics, National Education Longitudinal Study of 1988 (NELS:88), "Baseline, Student, School, and Parent Survey, 1990 and 1992" and the Office of Refugee Resettlement monthly records. Standard errors in parentheses clustered by county. Specification also include class cohort size and the number of students in the cohort in the county. All coefficients are allowed to differ by grade competition. The sample includes native-born students whose mother and father have schooling above high school and attended the same school in 10th and 12th grade. * 10% significance, two-tailed test; **5% significance, two-tailed test.