

NBER WORKING PAPER SERIES

HOW DOES YOUR KINDERGARTEN CLASSROOM AFFECT YOUR EARNINGS?
EVIDENCE FROM PROJECT STAR

Raj Chetty
John N. Friedman
Nathaniel Hilger
Emmanuel Saez
Diane Whitmore Schanzenbach
Danny Yagan

Working Paper 16381
<http://www.nber.org/papers/w16381>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
September 2010

We thank Josh Angrist, Lisa Barrow, David Card, Gary Chamberlain, Elizabeth Cascio, Janet Currie, Jeremy Finn, Edward Glaeser, Bryan Graham, James Heckman, Caroline Hoxby, Guido Imbens, Thomas Kane, Lawrence Katz, Alan Krueger, Derek Neal, Jonah Rockoff, Douglas Staiger, numerous seminar participants, and anonymous referees for helpful discussions and comments. We thank Helen Bain and Jayne Zaharias at HEROS for access to the Project STAR data. The tax data were accessed through contract TIRNO-09-R-00007 with the Statistics of Income (SOI) Division at the US Internal Revenue Service. We thank the SOI staff, and in particular Nicholas Greenia and Barry W. Johnson, for their invaluable help and guidance in this process. Gregory Bruich, Jane Choi, Jessica Laird, Keli Liu, Laszlo Sandor, and Patrick Turley provided outstanding research assistance. Financial support from the Lab for Economic Applications and Policy at Harvard, the Center for Equitable Growth at UC Berkeley, and the National Science Foundation 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.

© 2010 by Raj Chetty, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach, and Danny Yagan. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

How Does Your Kindergarten Classroom Affect Your Earnings? Evidence From Project STAR
Raj Chetty, John N. Friedman, Nathaniel Hilger, Emmanuel Saez, Diane Whitmore Schanzenbach,
and Danny Yagan

NBER Working Paper No. 16381

September 2010, Revised January 2011

JEL No. H0,J0

ABSTRACT

In Project STAR, 11,571 students in Tennessee and their teachers were randomly assigned to different classrooms within their schools from kindergarten to third grade. This paper evaluates the long-term impacts of STAR using administrative records. We obtain five results. First, kindergarten test scores are highly correlated with outcomes such as earnings at age 27, college attendance, home ownership, and retirement savings. Second, students in small classes are significantly more likely to attend college and exhibit improvements on other outcomes. Class size does not have a significant effect on earnings at age 27, but this effect is imprecisely estimated. Third, students who had a more experienced teacher in kindergarten have higher earnings. Fourth, an analysis of variance reveals significant classroom effects on earnings. Students who were randomly assigned to higher quality classrooms in grades K-3--as measured by classmates' end-of-class test scores--have higher earnings, college attendance rates, and other outcomes. Finally, the effects of class quality fade out on test scores in later grades but gains in non-cognitive measures persist.

Raj Chetty
Department of Economics
Harvard University
1805 Cambridge St.
Cambridge, MA 02138
and NBER
chetty@fas.harvard.edu

Emmanuel Saez
Department of Economics
University of California, Berkeley
549 Evans Hall #3880
Berkeley, CA 94720
and NBER
saez@econ.berkeley.edu

John N. Friedman
Harvard Kennedy School
Taubman 356
79 JFK St.
Cambridge, MA 02138
and NBER
jfriedman@post.harvard.edu

Diane Whitmore Schanzenbach
School of Education and Social Policy
Northwestern University
Annenberg Hall, Room 205
2120 Campus Drive
Evanston, IL 60208
and NBER
dws@northwestern.edu

Nathaniel Hilger
Department of Economics
Harvard University
1805 Cambridge St.
Cambridge, MA 02138
nhilger@fas.harvard.edu

Danny Yagan
Department of Economics
Littauer 200, North Yard
Harvard University
Cambridge, MA 02138
yagan@fas.harvard.edu

I Introduction

What are the long-term impacts of early childhood education? Evidence on this important policy question remains scarce because of a lack of data linking childhood education and adult outcomes. This paper analyzes the long-term impacts of Project STAR, one of the most widely studied education experiments in the United States. The Student/Teacher Achievement Ratio (STAR) experiment randomly assigned one cohort of 11,571 students and their teachers to different classrooms within their schools in grades K-3. Some students were assigned to small classes (15 students on average) in grades K-3, while others were assigned to large classes (22 students on average). The experiment was implemented across 79 schools in Tennessee from 1985 to 1989. Numerous studies have used the STAR experiment to show that class size, teacher quality, and peers have significant causal impacts on test scores (see Schanzenbach 2006 for a review). Whether these gains in achievement on standardized tests translate into improvements in adult outcomes such as earnings remains an open question.

We link the original STAR data to administrative data from tax returns, allowing us to follow 95% of the STAR participants into adulthood.¹ We use these data to analyze the impacts of STAR on outcomes ranging from college attendance and earnings to retirement savings, home ownership, and marriage. We begin by documenting the strong correlation between kindergarten test scores and adult outcomes. A one percentile increase in end-of-kindergarten (KG) test scores is associated with a \$132 increase in wage earnings at age 27 in the raw data, and a \$94 increase after controlling for parental characteristics. Several other adult outcomes – such as college attendance rates, quality of college attended, home ownership, and 401(k) savings – are also all highly correlated with kindergarten test scores. These strong correlations motivate the main question of the paper: do interventions that raise test scores – such as smaller classes and better teachers – cause analogous improvements in adult outcomes?

Our analysis of the experimental impacts consists of two empirical strategies. First, we study the impacts of observable classroom characteristics. We analyze the impacts of class size using the same intent-to-treat specifications as Krueger (1999), who showed that students in small classes scored higher on standardized tests. We find that students assigned to small classes are 1.8 percentage points more likely to be enrolled in college at age 20, a significant improvement relative

¹The data for this project were analyzed through a program developed by the Statistics of Income (SOI) Division at the U.S. Internal Revenue Service to support research into the effects of tax policy on economic and social outcomes and improve the administration of the tax system.

to the mean college attendance rate of 26.4% at age 20 in the sample. We do not find significant differences in earnings at age 27 between students who were in small and large classes, although these earnings impacts are imprecisely estimated. Students in small classes also exhibit statistically significant improvements on a summary index of the other outcomes we examine (home ownership, 401(k) savings, mobility rates, percent college graduate in ZIP code, and marital status).

We study variation across classrooms along other observable dimensions, such as teacher and peer characteristics, using a similar approach. Prior studies (e.g. Krueger 1999) have shown that STAR students with more experienced teachers score higher on tests. We find similar impacts on earnings. Students randomly assigned to a KG teacher with more than 10 years of experience earn an extra \$1,093 (6.9% of mean income) on average at age 27 relative to students with less experienced teachers.² We also test whether observable peer characteristics have long-term impacts by regressing earnings on the fraction of low-income, female, and black peers in KG. These peer impacts are not significant, but are very imprecisely estimated because of the limited variation in peer characteristics across classrooms.

Because we have few measures of observable classroom characteristics, we turn to a second empirical strategy that captures both observed and unobserved aspects of classrooms. We use an analysis of variance approach analogous to that in the teacher effects literature (summarized in Rockoff and Staiger 2010) to test whether earnings are clustered by kindergarten classroom. Because we observe each teacher only once in our data, we can only estimate “class effects” – the combined effect of teachers, peers, and any class-level shock – by exploiting random assignment to KG classrooms of both students and teachers. Intuitively, we test whether earnings vary across KG classes by more than what would be predicted by random variation in student abilities. An F test rejects the null hypothesis that KG classroom assignment has no effect on earnings. The standard deviation of class effects on annual earnings is approximately 10% of mean earnings, highlighting the large stakes at play in early childhood education.

The analysis of variance shows that kindergarten classroom assignment has significant impacts on earnings, but it does not tell us whether interventions that improve scores also generate earnings gains. That is, are class effects on earnings correlated with class effects on scores? To analyze this question, we proxy for each student’s KG “class quality” by the average test scores of his classmates at the end of kindergarten. End-of-class peer test scores are an omnibus measure of class quality

²Because teacher experience is correlated with many other unobserved attributes – such as attachment to the teaching profession – we cannot conclude that increasing teacher experience would improve student outcomes. This evidence simply establishes that a student’s KG teacher has effects on his or her earnings as an adult.

because they capture both ex-ante variation in peer abilities and the effects of the teacher on students. Using this measure, we find that kindergarten class quality has significant impacts on both test scores and earnings. Students randomly assigned to a classroom that is one standard deviation higher in quality earn 3% more at age 27. Students assigned to higher quality classes are also significantly more likely to attend college, enroll in higher quality colleges, and exhibit improvements in the summary index of other outcomes. The class quality impacts are similar for students who entered the experiment late (in grades 1-3) and were randomized into classes at that point. Hence, the findings of this paper should be viewed as evidence on the long-term impacts of early childhood education rather than kindergarten in particular.

Our analysis of “class quality” must be interpreted very carefully. The purpose of this analysis is simply to detect clustering in outcomes at the classroom level: are a child’s outcomes correlated with his peer’s outcomes? Although we test for such clustering by regressing own scores and earnings on peer test scores, we emphasize that such regressions are *not* intended to detect peer effects. Because we use post-intervention peer scores as the regressor, these scores incorporate the impacts of peer quality, teacher quality, and any random class-level shock (such as a jackhammer making noise outside the classroom). The correlation between own outcomes and peer scores could be due to any of these factors. Because we cannot pinpoint the sources of clustering, our analysis does not shed light on which specific factors should be manipulated to improve adult outcomes.³ Our analysis shows that which classroom you are assigned to in kindergarten matters for outcomes 20 years later; it does not tell us why it matters. Further research on which factors contribute to high “class quality” would be extremely valuable in light of the results reported here.

The impacts of early childhood class assignment on outcomes more than twenty years later may be particularly surprising because the impacts on test scores “fade out” rapidly. The impacts of class size on test scores become insignificant by grade 8 (Krueger and Whitmore 2001), as do the impacts of class quality on test scores. Why do the impacts of early childhood education fade out on test scores but re-emerge in adulthood? We find some suggestive evidence that part of the explanation may be non-cognitive skills. We find that KG class quality has significant impacts on non-cognitive measures in 4th and 8th grade such as effort, initiative, and lack of disruptive behavior. These non-cognitive measures are highly correlated with earnings even conditional on test scores but are not significant predictors of future standardized test scores. These results

³From Donald Rubin and Paul Holland’s perspective of “no causation without manipulation” (Holland 1986), our findings on the impacts of class quality should not be interpreted as causal effects.

suggest that high quality KG classrooms may build non-cognitive skills that have returns in the labor market but do not improve performance on standardized tests. While this evidence is far from conclusive, it highlights the value of further empirical research on non-cognitive skills.

In addition to the extensive literature on the impacts of STAR on test scores, our study builds on and contributes to a recent literature investigating selected long-term impacts of class size in the STAR experiment. These studies have shown that students assigned to small classes are more likely to complete high school (Finn, Gerber, and Boyd-Zaharias 2005) and take the SAT or ACT college entrance exams (Krueger and Whitmore 2001) and are less likely to be arrested for crime (Krueger and Whitmore 2001). Most recently, Muennig et al. (2010) report that students in small classes have higher mortality rates, a finding that we do not obtain in our data as we discuss below. We contribute to this literature by providing a unified evaluation of several outcomes, including the first analysis of earnings, and by examining the impacts of teachers, peers, and other attributes of the classroom in addition to class size.

Our results also complement the findings of studies on the long-term impacts of other early childhood interventions, such as the Perry and Abecedarian preschool demonstrations and the Head Start program, which also find lasting impacts on adult outcomes despite fade-out on test scores (see Almond and Currie 2010 for a review). We show that a better classroom environment from ages 5-8 can have substantial long-term benefits even without intervention at earlier ages.

Finally, the findings in this paper have important implications for tax policy and administration. The cost-benefit analyses conducted below provide inputs for the evaluation of a broad range of existing and proposed tax policies related to education finance. Chetty and Friedman (2011) use the estimates from the present paper to show that federal tax credits for primary school education in disadvantaged areas could substantially reduce the inter-generational transmission of inequality. More generally, our analysis demonstrates that tax data are extremely valuable for analyzing the effects of tax policy interventions on long-term outcomes such as earnings, home ownership, savings, and other outcomes that are critical for tax revenue. Our analysis also shows that the administration of federal tax credits for higher education provides comprehensive and reliable information on college enrollment in the United States. The federal tax system and the tax data it generates can therefore play an important role in the evaluation of US education policy.

The paper is organized as follows. In section II, we review the STAR experimental design and address potential threats to the validity of the experiment. Section III documents the cross-sectional correlation between test scores and adult outcomes. Section IV analyzes the impacts of

observable characteristics of classrooms – size, teacher characteristics, and peer characteristics – on adult outcomes. In Section V, we study class effects more broadly, incorporating unobservable aspects of class quality. Section VI documents the fade-out and re-emergence effects and the potential role of non-cognitive skills in explaining this pattern. Section VI concludes.

II Experimental Design and Data

II.A Background on Project STAR

Word et al. (1990), Krueger (1999), and Finn et al. (2007) provide a comprehensive summary of Project STAR; here, we briefly review the features of the STAR experiment most relevant for our analysis. The STAR experiment was conducted at 79 schools across the state of Tennessee over four years. The program oversampled lower-income schools, and thus the STAR sample exhibits lower socioeconomic characteristics than the state of Tennessee and the U.S. population as a whole.

In the 1985-86 school year, 6,323 kindergarten students in participating schools were randomly assigned to a small (target size 13-17 students) or regular-sized (20-25 students) class within their schools.⁴ Students were intended to remain in the same class type (small vs. large) through 3rd grade, at which point all students would return to regular classes for 4th grade and subsequent years. As the initial cohort of kindergarten students advanced across grade levels, there was substantial attrition because students who moved away from a participating school or were retained in grade no longer received treatment. In addition, because kindergarten was not mandatory and due to normal residential mobility, many children joined the initial cohort at the participating schools after kindergarten. A total of 5,248 students entered the participating schools in grades 1-3. These new entrants were randomly assigned to classrooms within school upon entry. Thus all students were randomized to classrooms within school upon entry, regardless of the entry grade. As a result, the randomization pool is school-by-entry-grade, and we include school-by-entry-grade fixed effects in all experimental analyzes below.

Upon entry into one of the 79 schools, the study design randomly assigned students not only to class type (small vs. large) but also to a classroom within each type (if there were multiple classrooms per type, as was the case in 50 of the 79 schools). Teachers were also randomly assigned to classrooms. Unfortunately, the exact protocol of randomization into specific classrooms was

⁴There was also a third treatment group: regular sized class with a full-time teacher's aide. This was a relatively minor intervention, since all regular classes were already assigned a 1/3 time teacher's aide. Prior studies of STAR find no impact of a full-time teacher's aide on test scores. We follow the convention in the literature and group the regular and regular plus aide class treatments together.

not clearly documented in any of the official STAR reports, where the emphasis was instead the random assignment into class type rather than classroom (Word et al. 1990). We present statistical evidence confirming that both students and teachers indeed appear to be randomly assigned directly to classrooms upon entry into the STAR project, as the original designers attest.

As in any field experiment, there were some deviations from the experimental protocol. In particular, some students moved from large to small classes and vice versa. To account for such potentially non-random sorting, we adopt the standard approach taken in the literature and assign treatment status based on initial random assignment (intent-to-treat).

In each year, students were administered the grade-appropriate Stanford Achievement Test, a multiple choice test that measures performance in math and reading. These tests were given only to students participating in STAR, as the regular statewide testing program did not extend to the early grades.⁵ Following Krueger (1999), we standardize the math and reading scale scores in each grade by computing the scale score’s corresponding percentile rank in the distribution for students in large classes. We then assign the appropriate percentile rank to students in small classes and take the average across math and reading percentile ranks. Note that this percentile measure is a ranking of students *within* the STAR sample.

II.B Variable Definitions and Summary Statistics

We measure adult outcomes of Project STAR participants using administrative data from United States tax records. 95.0% of STAR records were linked to the tax data using an algorithm based on standard identifiers (SSN, date of birth, gender, and names) that is described in Appendix A.⁶

We obtain data on students and their parents from federal tax forms such as 1040 individual income tax returns. Information from 1040’s is available from 1996-2008. Approximately 10% of adults do not file individual income tax returns in a given year. We use third-party reports to obtain information such as wage earnings (form W-2) and college attendance (form 1098-T) for all individuals, including those who do not file 1040s. Data from these third-party reports are available since 1999. The year always refers to the tax year (i.e., the calendar year in which the income is earned or the college expense incurred). In most cases, tax returns for tax year t are filed during the calendar year $t+1$. The analysis dataset combines selected variables from individual tax

⁵These K-3 test scores contain considerable predictive content. As reported in Krueger Whitmore (2001), the correlation between test scores in grades g and $g+1$ is 0.65 for KG and 0.80 for each grade 1-3. The values for grades 4-7 lie between 0.83 and 0.88, suggesting that the K-3 test scores contain similar predictive content.

⁶The matching algorithm was sufficiently precise that it uncovered 28 cases in the original STAR dataset that were a single split observation or duplicate records. After consolidating these records, we are left with 11,571 students.

returns, third party reports, and information from the STAR database, with individual identifiers removed to protect confidentiality.

We now describe how each of the adult outcome measures and control variables used in the empirical analysis is constructed. Table 1 reports summary statistics for these variables for the STAR sample as well as a random 0.25% sample of the US population born in the same years (1979-1980).

Earnings. The individual earnings data come from W-2 forms, yielding information on earnings for both filers and non-filers.⁷ We define earnings in each year as the sum of earnings on all W-2 forms filed on an individual's behalf. We express all monetary variables in 2009 dollars, adjusting for inflation using the Consumer Price Index. We cap earnings in each year at \$100,000 to reduce the influence of outliers; fewer than 1% of individuals in the STAR sample report earnings above \$100,000 in a given year. To increase precision, we typically use average (inflation indexed) earnings from year 2005 to 2007 as an outcome measure. The mean individual earnings for the STAR sample in 2005-2007 (when the STAR students are 25-27 years old) is \$15,912. This earnings measure includes zeros for the 13.9% of STAR students who report no income 2005-2007. The mean level of earnings in the STAR sample is lower than in the same cohort in the U.S. population, as expected given that Project STAR targeted more disadvantaged schools.

College Attendance. Higher education institutions eligible for federal financial aid – Title IV institutions – are required to file 1098-T forms that report tuition payments or scholarships received for every student.⁸ Title IV institutions include all colleges and universities as well as vocational schools and other postsecondary institutions. Comparisons to other data sources indicate that 1098-T forms accurately capture US college enrollment.⁹ We have data on college attendance from 1098-T forms for all students in our sample since 1999, when the STAR students were 19 years old. We define college attendance as an indicator for having one or more 1098-T forms filed on one's behalf in a given year. In the STAR sample, 26.4% of students are enrolled in college at age

⁷We obtain similar results using household adjusted gross income reported on individual tax returns. We focus on the W-2 measure because it provides a consistent definition of individual wage earnings for both filers and non-filers. One limitation of the W-2 measure is that it does not include self-employment income.

⁸These forms are used to administer the Hope and Lifetime Learning education tax credits created by the Taxpayer Relief Act of 1997. Colleges are not required to file 1098-T forms for students whose qualified tuition and related expenses are waived or paid entirely with scholarships or grants; however, in many instances the forms are available even for such cases, perhaps because of automation at the university level.

⁹In 2009, 27.4 million 1098-T forms were issued (Internal Revenue Service, 2010). According to the Current Population Survey (US Census Bureau, 2010, Tables 5 and 6), in October 2008, there were 22.6 million students in the U.S. (13.2 million full time, 5.4 million part-time, and 4 million vocational). As an individual can be a student at some point during the year but not in October and can receive a 1098-T form from more than one institution, the number of 1098-T forms for the calendar year should indeed be higher than the number of students as of October.

20 (year 2000). 45.5% of students are enrolled in college at some point between 1999 and 2007, compared with 57.1% in the same cohort of the U.S. population. Because the data are based purely on tuition payments, we have no information about college completion or degree attainment.

College Quality. Using the institutional identifiers on the 1098-T forms, we construct an earnings-based index of college quality as follows. First, using the full population of all individuals in the United States aged 20 on 12/31/1999 and all 1098-T forms for year 1999, we group individuals by the higher education institution they attended in 1999. This sample contains over 1.4 million individuals.¹⁰ We take a 1% sample of those not attending a higher education institution in 1999, comprising another 27,733 individuals, and pool them together in a separate “no college” category. Next, we compute average earnings of the students in 2007 when they are aged 28 by grouping students according to the educational institution they attended in 1999. This earnings-based index of college quality is highly correlated with the US News ranking of the best 125 colleges and universities: the correlation coefficient of our measure and the log US news rank is 0.75. The advantages of our index are that while the US News ranking only covers the top 125 institutions, ours covers all higher education institutions in the U.S. and provides a simple cardinal metric for college quality. Among colleges attended by STAR students, the average value of our earnings index is \$35,080 for four-year colleges and \$26,920 for two-year colleges.¹¹ For students who did not attend college, the imputed mean wage is \$16,475.

Other Outcomes. We identify spouses using information from 1040 forms. For individuals who file tax returns, we define an indicator for marriage based on whether the tax return is filed jointly. We code non-filers as single because most non-filers in the U.S. who are not receiving Social Security benefits are single (Cilke 1998, Table 1). We define a measure of ever being married by age 27 as an indicator for ever filing a joint tax return in any year between 1999 and 2007. By this measure, 43.2% of individuals are married at some point before age 27.

We measure retirement savings using contributions to 401(k) accounts reported on W-2 forms from 1999-2007. 28.2% of individuals in the sample make a 401(k) contribution at some point during this period. We measure home ownership using data from the 1098 form, a third party report filed by lenders to report mortgage interest payments. We include the few individuals who report a mortgage deduction on their 1040 forms but do not have 1098’s as homeowners. We define

¹⁰Individuals who attended more than one institution in 1999 are counted as students at all institutions they attended.

¹¹For the small fraction of STAR students who attend more than one college in a single year, we define college quality based on the college that received the largest tuition payments on behalf of the student.

any individual who has a mortgage interest deduction at any point between 1999 and 2007 as a homeowner. Note that this measure of home ownership does not cover individuals who own homes without a mortgage, which is rare among individuals younger than 27. By our measure, 30.8% of individuals own a home by age 27. We use data from 1040 forms to identify each household's ZIP code of residence in each year. For non-filers, we use the ZIP code of the address to which the W-2 form was mailed. If an individual did not file and has no W-2 in a given year, we impute current ZIP code as the last observed ZIP code. We define a measure of cross-state mobility by an indicator for whether the individual ever lived outside Tennessee between 1999 and 2007. 27.5% of STAR students lived outside Tennessee at some point between age 19 and 27. We construct a measure of neighborhood quality using data on the percentage of college graduates in the individual's 2007 ZIP code from the 2000 Census. On average, STAR students lived in 2007 in neighborhoods with 17.6% college graduates.

We observe dates of birth and death until the end of 2009 as recorded by the Social Security Administration. We define each STAR participant's age at kindergarten entry as the student's age (in days divided by 365.25) as of September 1, 1985. Virtually all students in STAR were born in the years 1979-1980. To simplify the exposition, we say that the cohort of STAR children is aged a in year $1980 + a$ (e.g., STAR children are 27 in 2007). Approximately 1.7% of the STAR sample is deceased by 2009.

Parent Characteristics. We link STAR children to their parents by finding the earliest 1040 form from 1996-2008 on which the STAR student was claimed as dependents. Most matches were found on 1040 forms for the tax year 1996, when the STAR children were 16. We identify parents for 86% of the STAR students in our linked dataset. The remaining students are likely to have parents who did not file tax returns in the early years of the sample when they could have claimed their child as a dependent, making it impossible to link the children to their parents. Note that this definition of parents is based on who claims the child as a dependent, and thus may not reflect the biological parent of the child.

We define parental household income as average Adjusted Gross Income (capped at \$252,000, the 99th percentile in our sample) from 1996-1998, when the children were 16-18 years old. For years in which parents did not file, we define parental household income as zero.¹² For divorced parents, this income measure captures the total resources available to the household claiming the

¹² Alternative definitions of income for non-filers – such as income reported on W-2's starting in 1999 – yield very similar results to those reported below.

child as a dependent (including any alimony payments), rather than the sum of the individual incomes of the two parents. By this measure, mean parent income is \$48,010 (in 2009 dollars) for STAR students whom we are able to link to parents. We define marital status, home ownership, and 401(k) saving as indicators for whether the parent who claims the STAR child ever files a joint tax return, has a mortgage interest payment, or makes a 401(k) contribution over the period for which relevant data are available. We define mother’s age at child’s birth using data from Social Security Administration records on birth dates for parents and children. For single parents, we define the mother’s age at child’s birth using the age of the filer who claimed the child, who is typically the mother but is sometimes the father or another relative.¹³ By this measure, mothers are on average 25.0 years old when they give birth to a child in the STAR sample. When a child cannot be matched to a parent, we define all parental characteristics as zero, and we always include a dummy for missing parents in regressions that include parent characteristics.

Background Variables from STAR. In addition to classroom assignment and test score variables, we use some demographic information from the STAR database in our analysis. This includes gender, race (an indicator for being black), and whether the student ever received free or reduced price lunch during the experiment. 36% of the STAR sample are black and 60% are eligible for free or reduced-price lunches. Finally, we use data on teacher characteristics – experience, race, and highest degree – from the STAR database. The average student has a teacher with 10.8 years of experience. 19.5% of kindergarten students have a black teacher, and 35.9% have a teacher with a master’s degree or higher.

Our analysis dataset contains one observation for each of the 10,992 STAR students we link to the tax data. Each observation contains information on the student’s adult outcomes, parent characteristics, and classroom characteristics in the grade the student *entered* the STAR project and was randomly assigned to a classroom. Hence, when we pool students across grades, we include test score and classroom data only from the entry grade.

II.C Validity of the Experimental Design

The validity of the causal inferences that follow rests on two assumptions: successful randomization of students into classrooms and no differences in attrition (match rates) across classrooms. We now evaluate each of these issues.

¹³We define the mother’s age at child’s birth as missing for 471 observations in which the implied mother’s age at birth based on the claiming parent’s date of birth is below 13 or above 65. These are typically cases where the parent does not have an accurate birth date recorded in the SSA file.

Randomization into Classrooms. To evaluate whether the randomization protocol was implemented as designed, we test for balance in pre-determined variables across classrooms. The original STAR dataset contains only a few pre-determined variables: age, gender, race, and free-lunch status. Although the data are balanced on these characteristics, some skepticism naturally has remained because of the coarseness of the variables (Hanushek 2003).

The tax data allow us to improve upon the prior evidence on the validity of randomization by investigating a wider variety of family background characteristics. In particular, we check for balance in the following five parental characteristics: household income, 401(k) savings, home ownership, marital status, and mother’s age at child’s birth. Although most of these characteristics are not measured prior to random assignment in 1985, they are measured prior to the STAR cohort’s expected graduation from high school and are unlikely to be impacted by the child’s classroom assignment in grades K-3.¹⁴ We first establish that these parental characteristics are in fact strong predictors of student outcomes. In column 1 of Table 2, we regress the child’s earnings on the five parent characteristics, the student’s age, gender, race, and free-lunch status, and school-by-entry-grade fixed effects. We also include indicators for missing data on certain variables (parents’ characteristics, mother’s age, student’s free lunch status, and student’s race). The student and parent demographic characteristics are highly significant predictors of earnings.

Having identified a set of pre-determined characteristics that predict children’s future earnings, we test for balance in these covariates across classrooms. We first evaluate randomization into the small class treatment by regressing an indicator for being assigned to a small class upon entry on the same variables as in column 1. As shown in column 2 of Table 2, none of the demographic characteristics predict the likelihood that a child is assigned to a small class. An F test for the joint significance of all the pre-determined demographic variables is insignificant ($p = 0.26$), showing that students in small and large classes have similar demographic characteristics.

Columns 3-5 of Table 2 evaluate the random assignment of teachers to classes by regressing teacher characteristics – experience, bachelor’s degree, and race – on the same student and parent characteristics. Again, none of the pre-determined variables predict the type of teacher a student is assigned, consistent with random assignment of teachers to classrooms.

Finally, we evaluate whether students were randomly assigned into classrooms within small or large class types. If students were randomly assigned to classrooms, then conditional on school

¹⁴ As a caveat, note that since small and large class size had a statistically significant difference in attrition (Krueger 1999), class size may have affected the probability that a family moves, which could affect parental income or home ownership.

fixed effects, classroom indicator variables should not predict any pre-determined characteristics of the students. Column 6 of Table 2 reports p values from F tests for the significance of kindergarten classroom indicators in regressions of each pre-determined characteristic on class and school fixed effects. None of the F tests is significant, showing that each of the parental and child characteristics is balanced across classrooms. To test whether the pre-determined variables jointly predict classroom assignment, we predict earnings using the specification in column 1 of Table 2. We then regress predicted earnings on KG classroom indicators and school fixed effects and run an F test for the significance of the classroom indicators. The p value of this F test is 0.92, confirming that one would not predict clustering of earnings by KG classroom based on pre-determined variables. We use only kindergarten entrants for the F tests in column 6 because F tests for class effects are not powerful in grades 1-3 as only a few students enter each class in those grades. In Appendix Table 2, we extend these randomization tests to include students who entered in grades 1-3 using the technique developed in Section V below and show that covariates are balanced across classrooms in later entry grades as well.

Selective Attrition. Another threat to the experimental design is differential attrition across classrooms (Hanushek 2003). Attrition is a much less serious concern in the present study than in past evaluations of STAR because we are able to locate 95% of the students in the tax data. Nevertheless, we investigate whether the likelihood of being matched to the tax data varies by classroom assignment within schools. In columns 1 and 2 of Table 3, we test whether the match rate varies significantly with class size by regressing an indicator for being matched on the small class dummy. Column 1 includes no controls other than school-by-entry-grade fixed effects. It shows that, eliminating the between-school variation, the match rate in small and large classes differs by less than 0.02 percentage points. Column 2 shows that controlling for the full set of demographic characteristics used in Table 2 does not uncover any significant difference in the match rate across class types. The p values reported at the bottom of columns 1 and 2 are for F tests of the significance of classroom indicators in predicting match rates in regression specifications analogous to those in column 6 of Table 2. The p values are approximately 0.9, showing that there are no significant differences in match rates across classrooms within schools.

Another potential source of attrition from the sample is through death. Columns 3 and 4 replicate the first two columns, replacing the dependent variable in the regressions with an indicator for death before January 1, 2010. We find no evidence that mortality rates vary with class size or across classrooms. The difference in death rates between small and large classes is approximately

0.01 percentage points. This finding is inconsistent with recent results reported by Muennig et al. (2010), who find that students in small classes and regular classes with a certified teaching assistant are slightly more likely to die using data from the National Death Index. We find that 154 STAR students have died by 2007 while Muennig et al. (2010) only find 141 deaths in their data. The discrepancy between the findings might be due to differences in match quality.¹⁵

III Test Scores and Adult Outcomes in the Cross-Section

We begin by documenting the correlations between test scores and adult outcomes in the cross-section to provide a benchmark for assessing the impacts of the randomized interventions. Figure 1a documents the association between end-of-kindergarten test scores and mean earnings from age 25-27.¹⁶ To construct this figure, we bin individuals into twenty equal-width bins (vingtiles) and plot mean earnings in each bin. A one percentile point increase in KG test score is associated with a \$132 (0.83%) increase in earnings twenty years later. If one codes the x-axis using national percentiles on the standardized KG tests instead of within-sample percentiles, the earnings increase is \$154 per percentile. The correlation between KG test score percentiles and earnings is linear and remains significant even in the tails of the distribution of test scores. However, KG test scores explain only a small share of the variation in adult earnings: the adjusted R^2 of the regression of earnings on scores is 5%.¹⁷

Figures 1b and 1c show that KG test scores are highly predictive of college attendance rates and the quality of the college the student attends, as measured by our earnings-based index of college quality. To analyze the other adult outcomes in a compact manner, we construct a summary index of five outcomes: ever owning a home by 2007, 401(k) savings by 2007, ever married by 2007, ever living outside Tennessee by 2007, and living in a higher SES neighborhood in 2007 as measured by the percent of college graduates living in the ZIP code. Following Kling, Liebman, and Katz (2007), we first standardize each outcome by subtracting its mean and dividing it by its standard deviation. We then sum the five standardized outcomes and divide by the standard deviation

¹⁵ As 95% of STAR students are matched to the our data and have a valid Social Security Number, we believe that deaths are recorded accurately in our sample. It is unclear why a lower match rate would lead to a systematic difference in death rates by class size. However, given the small number of deaths, slight imbalances might generate marginally significant differences in death rates across class types.

¹⁶ Although individuals' earnings trajectories remain quite steep at age 27, earnings levels from ages 25-27 are highly correlated with earnings at later ages (Haider and Solon 2006), a finding we have confirmed with our population wide longitudinal data (see Appendix Table 1).

¹⁷ These cross-sectional estimates are consistent with those obtained by Currie and Thomas (2001) using the British National Child Development Survey and Currie (2010) using the National Longitudinal Survey of Youth.

of the sum to obtain an index that has a standard deviation of 1. A higher value of the index represents more desirable outcomes. Students with higher entry-year test scores have stronger adult outcomes as measured by the summary index, as shown in Figure 1d.

Table 4 quantifies the correlations between test scores and adult outcomes. We report standard errors clustered by school in this and all subsequent tables. Column 1 replicates Figure 1a by regressing earnings on KG test scores without any additional controls. Column 2 controls for classroom fixed effects and a vector of parent and student demographic characteristics. The parent characteristics are a quartic in parent’s household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother’s age at child’s birth, and indicators for parent’s 401(k) savings and home ownership. The student characteristics are gender, race, age at entry-year entry, and free lunch status.¹⁸ We use this vector of demographic characteristics in most specifications below. When the class fixed effects and demographic controls are included, the coefficient on kindergarten percentile scores falls to \$94, showing that part of the raw correlation in Figure 1a is driven by these characteristics. Equivalently, a one standard deviation (SD) increase in test scores is associated with an 18% increase in earnings conditional on demographic characteristics.

Columns 1 and 2 use only kindergarten entrants. 55% of students entered STAR in Kindergarten, with 20%, 14% and 11% entering in grades 1 through 3, respectively. In column 3, we also include students who entered in grades 1-3 in order to obtain estimates consistent with the experimental analysis below, which pools all entrants. To do so, we define a student’s “entry-grade” test score as her score at the end of the grade in which she entered the experiment. Column 3 shows that a 1 percentile increase in entry-grade scores is associated with a \$90 increase in earnings conditional on demographic controls. This \$90 coefficient is a weighted average of the correlations between grade K-3 test scores and earnings, with the weights given by the entry rates in each grade.

In column 4, we include both 8th grade scores (the last point at which data from standardized tests are available for most students in the STAR sample) and entry-grade scores in the regression. The entire effect of entry-grade test score is absorbed by the 8th grade score, but the adjusted R^2 is essentially unchanged. In column 5, we compare the relative importance of parent characteristics and cognitive ability as measured by test scores. We calculate the parent’s income percentile rank using the tax data for the U.S. population. We regress earnings on test scores, parents’ income

¹⁸We code all parental characteristics as 0 for students whose parents are missing, and include an indicator for missing parents as a control. We also include indicators for missing data on certain variables (mother’s age, student’s free lunch status, and student’s race) and code these variables as zero when missing.

percentile, and controls for the student’s race, gender, age, and class fixed effects. A one percentile point increase in parental income is associated with approximately a \$148 increase in earnings, suggesting that parental background affects earnings as much as or more than cognitive ability in the cross section.¹⁹

Columns 6-9 of Table 4 show the correlations between entry-grade test scores and the other outcomes we study. Conditional on demographic characteristics, a one percentile point increase in entry-grade score is associated with a 0.36 percentage point increase in the probability of attending college at age 20 and a 0.51 percentage point increase in the probability of attending college at some point before age 27. A one percentile point increase in score is associated with \$32 higher predicted earnings based on the college the student attends and a 0.5% of a standard deviation improvement in the summary index of other outcomes.

We report additional cross-sectional correlations in the appendix. Appendix Table 3 reports the cross-sectional association between test scores and each of the five elements of the summary index. Appendix Table 4 replicates Table 4 for each entry grade separately. Appendix Table 5 documents the correlation between test scores and earnings from grades K-8 for a fixed sample of students, while Appendix Table 6 reports the heterogeneity of the correlations by race, gender, and free lunch status. Throughout, we find very strong correlations between test scores and adult outcomes, which motivates the central question of the paper: do interventions that raise early childhood test scores also yield improvements in adult outcomes?

IV Impacts of Observable Classroom Characteristics

In this section, we analyze the impacts of three features of classrooms that we can observe in our data – class size, teacher characteristics, and peer characteristics.

IV.A Class Size

We estimate the effects of class size on adult outcomes using an intent-to-treat regression specification analogous to Krueger (1999):

$$(1) \quad y_{icnw} = \alpha_{nw} + \beta \text{SMALL}_{cnw} + X_{icnw} \delta + \varepsilon_{icnw}$$

where y_{icnw} is an outcome such as earnings for student i randomly assigned to classroom c at school n in entry grade (wave) w . The variable SMALL_{cnw} is an indicator for whether the student

¹⁹Moreover, this \$148 coefficient is an underestimate if parental income directly affects entry-grade test scores.

was assigned to a small class upon entry. Because children were randomly assigned to classrooms within schools in the first year they joined the STAR cohort, we include school-by-entry-grade fixed effects (α_{nw}) in all specifications. The vector X_{icnw} includes the student and parent demographic characteristics described above: a quartic in household income interacted with an indicator for whether the parents are ever married, 401(k) savings, home ownership, mother’s age at child’s birth, and the student’s gender, race, age (in days), and free lunch status (along with indicators for missing data). To examine the robustness of our results, we report the coefficient both with and without this vector of controls. The inclusion of these controls does not significantly affect the estimates, as expected given that the covariates are balanced across classrooms. We cluster standard errors by school.

We report estimates of (1) for various outcomes in Table 5 using the full sample of STAR students; we show in Appendix Table 7 that similar results are obtained for the subsample of students who entered in kindergarten. As a reference, in column 1 of Table 5, we estimate (1) with the entry grade test score as the outcome. Consistent with Krueger (1999), we find that students assigned to small classes score 4.8 percentile points higher on tests in the year they enter a participating school. Note that the average student assigned to a small class spent 2.27 years in a small class, while those assigned to a large class spent 0.13 years in a small class. On average, large classes had 22.6 students while small classes had 15.1 students. Hence, the impacts on adult outcomes below should be interpreted as effects of attending a class that is 33% smaller for 2.14 years.

College Attendance. We begin by analyzing the impacts of class size on college attendance. Figure 2a plots the fraction of students who attend college in each year from 1999 to 2007 by class size. In this and all subsequent figures, we adjust for school-by-entry-grade effects to isolate the random variation of interest. To do so, we regress the outcome variable on school-by-entry-grade dummies and the small class indicator in each tax year. We then construct the two series shown in the figure by setting the difference between the two lines equal to the regression coefficient on the small class indicator in the corresponding year and the weighted average of the lines equal to the sample average in that year.

Figure 2a shows that students assigned to a small class are more likely to attend college, particularly before age 25. As the cohort ages from 19 (in 1999) to 27 (in 2007), the attendance rate of both treatment and control students declines, consistent with patterns in the broader U.S. population. Because our measure of college attendance is based on tuition payments, it includes

students who attend higher education institutions both part-time and full-time. Measures of college attendance around age 20 (two years after the expected date of high school graduation) are most likely to pick up full-time attendance to two-year and four-year colleges, while college attendance in later years may be more likely to reflect part-time enrollment. This could explain why the effect of class size becomes much smaller after age 25. We therefore analyze two measures of college attendance below: college attendance at age 20 and attendance at any point before age 27.

The regression estimates reported in Column 2 of Table 5 are consistent with the results in Figure 2a. Controlling for demographic characteristics, students assigned to a small class are 1.8 percentage points (6.7%) more likely to attend college in 2000. This effect is marginally significant with $p = 0.06$. Column 3 shows that students in small classes are 1.6 percentage points more likely to attend college at some point before age 27.

Next, we investigate how class size affects the quality of colleges that students attend. Using the earnings-based college quality measure described above, we plot the distribution of college quality attended in 2000 by small and large class assignment in Figure 2b. We compute residual college mean earnings from a regression on school-by-entry-grade effects and plot the distribution of the residuals within small and large classes, adding back the sample mean to facilitate interpretation of units. To show where the excess density in the small class group lies, the densities are scaled to integrate to the total college attendance rates for small and large classes. The excess density in the small class group lies primarily among the lower quality colleges, suggesting that the marginal students who were induced to attend college because of reduced class size enrolled in relatively low quality colleges.

Column 4 of Table 5 shows that students assigned to a small class attend colleges whose students have mean earnings that are \$109 higher. That is, based on the cross-sectional relationship between earnings and attendance at each college, we predict that students in small classes will be earning approximately \$109 more per year at age 28. This earnings increase incorporates the extensive-margin of higher college attendance rates, because students who do not attend college are assigned the mean earnings of individuals who do not attend college in our index.²⁰ Conditional on attending college, students in small classes attend *lower* quality colleges on average because of the selection effect shown in Figure 2b.²¹

²⁰Alternative earnings imputation procedures for those who do not attend college yield similar results. For example, assigning these students the mean earnings of Tennessee residents or STAR participants who do not attend college generates larger estimates.

²¹Because of the selection effect, we are unable to determine whether there was an intensive-margin improvement in quality of college attended. Quantifying the effect of reduced class size on college quality for those who were

Earnings. Figure 2c shows the analog of Figure 2a for wage earnings. Earnings rise rapidly over time because many students are in college in the early years of the sample. Individuals in small classes have slightly higher earnings than those in large classes in most years. Column 5 of Table 5 shows that without controls, students who were assigned to small classes are estimated to earn \$4 more per year on average between 2005 and 2007. With controls for demographic characteristics, the point estimate of the earnings impact becomes -\$124 (with a standard error of \$336). Though the point estimate is negative, the upper bound of the 95% confidence interval is an earnings gain of \$535 (3.4%) gain per year. If we were to predict the expected earnings gain from being assigned to a small class from the cross-sectional correlation between test scores and earnings reported in column 4 of Table 4, we obtain an expected earnings effect of 4.8 percentiles \times \$90 = \$432. This prediction lies within the 95% confidence interval for the impact of class size on earnings. In Appendix Table 8, we consider several alternative measures of earnings, such as total household income and an indicator for positive wage earnings. We find qualitatively similar impacts – point estimates close to zero with confidence intervals that include the predicted value from cross-sectional estimates – for all of these measures. We conclude that the class size intervention, which raises test scores by 4.8 percentiles, is unfortunately not powerful enough to detect earnings increases of a plausible magnitude as of age 27. Because class size has impacts on college attendance, earnings effects might emerge in subsequent years, especially since college graduates have much steeper earnings profiles than non college graduates.

Other Outcomes. Column 6 of Table 5 shows that students assigned to small classes score 4.6 percent of a standard deviation higher in the summary outcome index defined in Section III, an effect that is statistically significant with $p < 0.05$. This index combines information on savings behavior, home ownership, marriage rates, mobility rates, and residential neighborhood quality. In Appendix Table 9, we analyze the impacts of class size on each of the five outcomes separately. We find particularly large and significant impacts on the probability of having a 401(k), which can be thought of as a proxy for having a good job. This result is consistent with the view that students in small classes may have higher permanent income that could emerge in wage earnings measures later in their lifecycles. We also find positive effects on all the other components of the summary index, though these effects are not individually significant.²²

already planning to attend college would require additional assumptions such as rank preservation.

²²In Appendix Table 9, we also analyze an alternative summary index that weights each of the five components by their impacts on wage earnings. We construct this index by regressing wage earnings on the five components in the cross-section and predicting wage earnings for each individual. We find significant impacts of class size on this predicted-earnings summary index, confirming that our results are robust to the way in which the components of the

In Appendix Table 10, we document the heterogeneity of class size impacts across subgroups. We replicate the analysis of class size impacts in Table 5 for six groups: black and white students, males and females, and lower- and higher-income students (based on free lunch status). The point estimates of the impacts of class size are positive for most of the groups and outcomes. The impacts on adult outcomes are somewhat larger for groups that exhibit larger test scores increases. For instance, black students assigned to small classes score 6.9 percentile points higher on their entry-grade test, are 5.3 percentage points more likely to ever attend college, and have an earnings increase of \$250 (with a standard error of \$540). There is some evidence that reductions in class size may have more positive effects for men than women and for higher income than lower income (free-lunch eligible) students. Overall, however, the STAR experiment is not powerful enough to detect heterogeneity in the impacts of class size on adult outcomes with precision.

IV.B Observable Teacher and Peer Effects

We estimate the impacts of observable characteristics of teachers and peers using specifications analogous to (1):

$$(2) \quad y_{icnw} = \alpha_{nw} + \beta_1 \text{SMALL}_{cnw} + \beta_2 z_{cnw} + X_{icnw} \delta + \varepsilon_{icnw}$$

where z_{cnw} denotes a vector of teacher or peer characteristics for student i assigned to classroom c at school n in entry grade w . Because students and teachers were randomly assigned to classrooms, β_2 can be interpreted as the effect of the relevant teacher or peer characteristics on the outcome y . Note that we control for class size in these regressions, so the variation identifying teacher and peer effects is orthogonal to that used above.

Teachers. We begin by examining the impact of teacher experience on scores and earnings. Figure 3a plots KG scores vs. the numbers of years of experience that the student’s KG teacher had at the time she taught his class. We exclude students who entered the experiment in grades 1 to 3 in these graphs for reasons we discuss below. We adjust for school effects by regressing the outcome and dependent variables on these fixed effects and computing residuals. The figure is a scatter plot of the residuals, with the sample means added back in to facilitate interpretation of the axes. Figure 3a shows that students randomly assigned to more experienced KG teachers have higher test scores. The effect of experience on KG scores is roughly linear in the STAR experimental data, in contrast with other studies which find that the returns to experience drop

summary index are weighted.

sharply after the first few years.

Figure 3b replicates 3a for the earnings outcome. It shows that students who were randomly assigned to more experienced KG teachers have higher earnings at age 27. As with scores, the impact of experience on earnings in these data appear roughly linear. Figure 3c characterizes the time path of the earnings impact. We divide teachers in two groups – those with experience above and below 10 years (since mean years of experience is 9.3 years). We then plot mean earnings for the students in the low- and high-experience groups by year, adjusting for school fixed effects as in Figure 3b. From 2000 to 2004 (when students are aged 20 to 24), there is little difference in earnings between the two curves. A gap opens starting in 2005; by 2007, students who had high-experience teachers in kindergarten are earning \$1,104 more on average.

Columns 1-2 of Table 6 quantify the impacts of teacher experience on scores and earnings, conditioning on the standard vector of student and parent demographic characteristics as well as whether the teacher has a master’s degree or higher and the small class indicator. Column 1 shows that students assigned to a teacher with more than 10 years of experience score 3.2 percentile points higher on KG tests. Column 2 shows that these same students earn \$1,093 more on average between ages 25 and 27 ($p < 0.05$).²³

Columns 3-4 show that teacher experience has a much reduced effect for children entering the experiment in grades 1 to 3 on both test scores and earnings. The effect of teacher experience on test scores is no longer statistically significant in grades 1-3. Consistent with this result, teacher experience in grades 1-3 also does not have a statistically significant effect on wage earnings. Unfortunately, the STAR dataset includes very few teacher characteristics, so we are unable to provide definitive evidence on why the effect of teacher experience varies across grades.

The impact of kindergarten teacher experience on earnings must be interpreted very carefully. Our results show that placing a child in a kindergarten class taught by a more experienced teacher yields improved outcomes. This finding does *not* imply that increasing a given teacher’s experience will improve student outcomes. The reason is that while teachers were randomly assigned to classrooms, experience was not randomly assigned to teachers. The difference in earnings of students with experienced teachers could be due to the intrinsic characteristics of experienced teachers rather than experience of teachers per se. For instance, teachers with more experience have selected

²³In Appendix Table 11, we replicate columns 1 and 2 for small and large classes separately to evaluate whether teacher experience is more important in managing classrooms with many students. We find some evidence that teacher experience has a larger impact on earnings in large classes, but the difference in impacts is not statistically significant.

to stay in the profession and may be more passionate or more skilled at teaching. Alternatively, teachers from older cohorts may have been more skilled (Corcoran, Evans, and Schwab 2004, Hoxby and Leigh 2004, Bacolod 2007). These factors may explain the difference between the effect of teacher experience in Kindergarten and later grades. For instance, the selection of teachers may vary across grades or cohort effects may differ for Kindergarten teachers.

The linear relationship between kindergarten teacher experience and scores in the STAR data stands in contrast to earlier studies that track teachers over time in a panel and find that teacher performance improves with the first few years of experience and then plateaus. This further suggests that other factors correlated with experience may drive the observed impacts on scores and earnings. We therefore conclude that early childhood teaching has a causal impact on long term outcomes but we cannot isolate the characteristics of teachers responsible for this effect.

The few other observable teacher characteristics in the STAR data (degrees, race, and progress on a career ladder) have no significant impact on scores or earnings. For instance, columns 1-4 of Table 6 show that the effect of teachers' degrees on scores and earnings is statistically insignificant. The finding that experience is the only observable measure that predicts teacher quality matches earlier studies of teacher effects (Hanushek 2010, Rockoff and Staiger 2010).

Peers. Better classmates could create an environment more conducive to learning, leading to improvements in adult outcomes. To test for such peer effects, we follow the standard approach in the recent literature by using linear-in-means regressions specifications. We include students who enter in all grades and measure peer characteristics in their first, randomly assigned classroom, and condition on school-by-entry-grade effects. We proxy for peer abilities (z) in (2) with the following exogenous peer characteristics: fraction black, fraction female, fraction eligible for free or reduced-price lunch (a proxy for low income), and mean age. Replicating previous studies, we show in column 5 of Table 6 that the fraction of female and low-income peers significantly predict test scores. Column 6 replicates column 5 with earnings as the dependent variable. The estimates on all four peer characteristics are very imprecise. For instance, the estimated effect of increasing the fraction of low-income peers by 10 percentage points is an earnings loss of \$28, but with a standard error of \$173. In an attempt to obtain more power, we construct a single index of peer abilities by first regressing scores on the full set of parent and student demographic characteristics described above and then predicting peers' scores using this regression. However, as column 7 shows, even the predicted peer score measure does not yield a precise estimate of peer effects on earnings; the 95% confidence interval for a 1 percentile point improvement in peers' predicted test scores ranges

from -\$207 to \$160.²⁴

The STAR experiment lacks the power to measure the effects of observable peer characteristics on earnings precisely because the experimental design randomized students across classrooms. As a result, it does not generate significant variation in mean peer abilities across classes. The standard deviation of mean predicted peer test scores (removing variation across schools and waves) is less than two percentile points. This small degree of variation in peer abilities is adequate to identify some contemporaneous effects on test scores but proves to be insufficient to identify effects on outcomes twenty years later, which are presumably subject to much higher levels of idiosyncratic noise.

V Impacts of Unobservable Classroom Characteristics

Many unobserved aspects of teachers and peers could impact student achievement and adult outcomes. For instance, some teachers may generate greater enthusiasm among students or some peers might be particularly disruptive. To test whether such unobservable aspects of class quality have long-term impacts, we use an analysis of variance approach. In particular, we test for “class effects” on scores and earnings by exploiting random assignment to classrooms. These class effects include the effects of teachers, peers, and any class-level shocks. We formalize our estimation strategy using a simple empirical model.

V.A A Model of Class Effects

For simplicity, we analyze a model in which all students enter in the same grade and suppress the entry grade index (w); we discuss below how our estimator can be applied to the case with multiple entry grades. Consider the following model for test scores (s_{icn}) at the end of the class and earnings or other adult outcomes (y_{icn}) for student i in class c at school n :

$$(3) \quad s_{icn} = d_n + z_{cn} + a_{icn}$$

$$(4) \quad y_{icn} = \delta_n + \beta z_{cn} + \gamma z_{cn}^Y + \rho a_{icn} + \nu_{icn},$$

where a_{icn} denotes intrinsic academic ability. The error term ν_{icn} represents the component of intrinsic earnings ability that is uncorrelated with academic ability. The parameter ρ controls the correlation between intrinsic academic ability and intrinsic earnings ability. The school fixed

²⁴We find positive but insignificant impacts of teacher and peer characteristics on the other outcomes above, consistent with a general lack of power in observable characteristics (not reported).

effects d_n and δ_n capture school-level differences in achievement on tests and earnings outcomes, e.g. due to variation in socioeconomic characteristics across school areas.

The variables z_{cn} and z_{cn}^Y denote class-level interventions, such as better teaching or smaller class size. We first consider a case without peer effects and then show how peer effects affect our analysis below. Intervention z_{cn} affects both test scores and earnings, while intervention z_{cn}^Y affects only earnings without affecting test scores. For example, a teacher whose students learn to read well could improve both test scores and earnings, while an inspiring teacher who does not teach to the test might raise earnings without improving test scores.²⁵ We assume without loss of generality that z_{cn} is orthogonal to z_{cn}^Y , as any component of z_{cn}^Y that is correlated with z_{cn} could be incorporated into z_{cn} by definition. Because of random assignment to classrooms, students' intrinsic abilities a_{icn} and ν_{icn} are orthogonal to z_{cn} and z_{cn}^Y . With this notation, class effects on earnings are determined by $\beta + \gamma$. The parameter β measures the covariance of class effects on scores and class effects on earnings.²⁶

For interventions that are directly observable (such as small class size or teacher experience), one can estimate (3) and (4) directly using OLS, as we did in using (1) and (2) to analyze the impacts of class size and observable teacher and peer attributes. We use two techniques to analyze class-level interventions that are not observable: an analysis of variance to test for class effects on earnings ($\beta + \gamma > 0$) and a regression method to test for covariance of class effects on scores and earnings ($\beta > 0$).

Analysis of Variance: class effects on scores and earnings. We decompose the variation in y_{icn} into individual and class-level components and test for the significance of class-level variation using an ANOVA. Intuitively, the ANOVA tests whether the outcome y varies across classes by more than what would be predicted by random variation in students across classrooms. We measure the magnitude of the class effects on earnings using a random class effects specification for (4) to estimate the standard deviation of class effects.

Although the ANOVA is useful for estimating the magnitude of class effects on earnings, it has two limitations. First, it does not tell us whether class effects on scores are correlated with class effects on earnings (i.e., whether $\beta > 0$). Hence, it does not answer our main question: do interventions that raise test scores also improve adult outcomes? This is an important question

²⁵For expositional simplicity, we assume that both z_{cn} and z_{cn}^Y are one dimensional. The estimator we derive remains valid in the case of a vector of interventions.

²⁶Different interventions may affect scores and earnings in different ways, so β will generally vary across interventions. Our method of estimating β below recovers the mean impact of all interventions that occur at the classroom level.

because the impacts of most educational interventions can be measured only by test scores in the short run. Second, in the STAR data, roughly half the students enter in grades 1-3 and are randomly assigned to classrooms at that point. Because only a small number of students enter each school in each of these later grades, we do not have the power to detect class effects in later grades and therefore do not include these students in the ANOVA.

Covariance between class effects on scores and earnings. Motivated by these limitations, our second strategy measures the covariance between class effects on scores and class effects on earnings (β). As the class effect on scores z_{cn} is unobserved, we proxy for it using end-of-class peer test scores. Let s_{cn} denote the mean test score in class c (in school n) and s_n denote the mean test score in school n . Let I denote the number of students per class, C the number of classes per school, and N the number of schools.²⁷ The mean test score in class c is

$$s_{cn} = \frac{1}{I} \sum_{i=1}^I s_{icn} = d_n + z_{cn} + \frac{1}{I} \sum_{i=1}^I a_{icn}$$

To simplify notation, assume that the mean value of z_{cn} across classes within a school is 0 ($z_n = 0$).

Then the difference between mean test scores in class c and mean scores in the school is

$$(5) \quad \Delta s_{cn} = s_{cn} - s_n = z_{cn} + \left[\frac{1}{I} \sum_{j=1}^I a_{jcn} - \frac{1}{IC} \sum_{c=1}^C \sum_{j=1}^I a_{jcn} \right].$$

Equation (5) shows that Δs_{cn} is a (noisy) observable measure of class quality z_{cn} . The noise arises from variation in student abilities across classes. As the number of students grows large ($I \rightarrow \infty$), Δs_{cn} converges to the true underlying class quality z_{cn} if all students are randomly assigned to classrooms.

Equation (5) motivates substituting Δs_{cn} for z_{cn} in (4) and estimating a regression of the form:

$$(6) \quad y_{icn} = \alpha_n + b^M \Delta s_{cn} + \varepsilon_{icn}.$$

The OLS estimate \hat{b}^M is a consistent estimate of β as the number of students $I \rightarrow \infty$, but it is upward-biased with finite class size because a high ability student raises the average class score and also has high earnings himself. Because of this “own observation” problem, $\mathbb{E}b^M > 0$ even when $\beta = 0$ (see Appendix B). An intuitive solution to eliminate the upward bias due to the own-observation problem is to omit the own score s_{icn} from the measure of class quality for individual

²⁷We assume that I and C do not vary across classes and schools for presentational simplicity. Our empirical analysis accounts for variation in I and C across classrooms and schools, and the analytical results below are unaffected by such variation.

i. Hence, we proxy for class quality using a leave-out mean (or jackknife) peer score measure

$$(7) \quad \Delta s_{cn}^{-i} = s_{cn}^{-i} - s_n^{-i},$$

where

$$s_{cn}^{-i} = \frac{1}{I-1} \sum_{j=1, j \neq i}^I s_{jcn}$$

is classmates' mean test scores and

$$s_n^{-i} = \frac{1}{IC-1} \sum_{k=1}^C \sum_{j=1, j \neq i}^I s_{jkn}$$

is schoolmates' mean scores. Intuitively, the measure Δs_{cn}^{-i} answers the question: “How good are your classmates' scores compared with the classmates you could have had in your school?”

Replacing Δs_{cn} by Δs_{cn}^{-i} , we estimate regressions of the following form:

$$(8) \quad y_{icn} = \alpha_n + b^{LM} \Delta s_{cn}^{-i} + \varepsilon_{icn}.$$

We show in Appendix B that the coefficient on class quality is positive if and only if class quality has an impact on adult outcomes.²⁸ Formally, $\mathbb{E} \hat{b}^{LM} > 0$ if and only if $\beta > 0$. However, b^{LM} is downward biased relative to β because Δs_{cn}^{-i} is a noisy measure of class quality. In Appendix B, we use the sample variance of test scores to estimate the degree of this attenuation bias at 23%.

Our preceding analysis ignores variation in class quality due to peer effects. With peer effects, a high ability student may raise his peers' scores, violating the assumption made above that $z_{cn} \perp a_{icn}$. Such peer effects bias b^{LM} upward (generating $\mathbb{E} \hat{b}^{LM} > \beta$) because of the reflection problem (Manski 1993). Even if there is no effect of class quality on earnings, that student's higher earnings (due solely to her own ability) will generate a positive correlation between peer scores and own earnings. While we cannot purge our leave-out-mean estimator of this bias, we show below that we can tightly bound the degree of reflection bias in a linear-in-means model. The reflection bias turns out to be relatively small in our application because it is of order $\frac{1}{I}$ and classes have 20 students on average.

We refer to peer-score measure Δs_{cn}^{-i} as “class quality” and the coefficient b^{LM} as the effect of class quality on earnings (or other outcomes). Although we regress outcomes on peer scores in (8), the coefficient b^{LM} should *not* be interpreted as an estimate of peer effects. Because class quality Δs_{cn}^{-i} is defined based on end-of-class peer scores, it captures teacher quality, peer

²⁸We use the difference between peer scores in the class and the school (rather than simply using classmates' scores) to address the finite-sample bias in small peer groups identified by Guryan, Kroft, and Notowidigdo (2009).

quality, and any other class-level shocks that may have affected students systematically. The regression in (8) is simply a method of detecting clustering in outcomes by kindergarten classroom and evaluating whether the classrooms that generate the largest test-score gains are also those that generate improvements in adult outcomes.

We include students who enter STAR in later grades when estimating (8) by defining Δs_{cn}^{-i} as the difference between mean end-of-year test scores for classmates and schoolmates in the student's grade in the year she entered a STAR school. To maximize precision, we include all peers (including those who had entered in earlier grades) when defining Δs_{cn}^{-i} for new entrants. Importantly, Δs_{cn}^{-i} varies randomly within schools for new entrants – who are randomly assigned to their first classroom – as it does for kindergarten entrants.²⁹ With this definition of Δs_{cn}^{-i} , b^{LM} measures the extent to which class quality in the initial class of entry (weighted by the entry rates across the four grades) affects outcomes.

An alternative approach to measuring the covariance between class effects on scores and earnings is to use an instrumental variables strategy, regressing earnings on test scores and instrumenting for scores with classroom fixed effects. Because the fitted values from the first stage regression are just mean test scores by classroom, the coefficient obtained from this TSLS regression coincides with b^M when we run (6) weighted by class size. The TSLS estimate of β is upward biased because the own observation is included in both mean scores and mean earnings, which is the well known weak instruments problem. The weak instruments literature has developed various techniques to deal with this bias, including (1) jackknife IV (Angrist, Imbens, Krueger 1999), which solves the problem by omitting the own observation when forming the instrument; (2) split-sample IV (Angrist and Krueger 1995), which randomly splits classes into two and only uses mean scores in the other half of the class as an instrument; and (3) limited information maximum likelihood (LIML), which collapses the parameter space and uses maximum likelihood to obtain a consistent estimate of β . The estimator for b^{LM} in (8) is essentially the reduced-form of the first technique, the jackknife IV regression. We present estimates using the instrumental variable strategies in Appendix Table 12 to evaluate the robustness of our results.

²⁹For entrants in grades 1-3, there can be additional noise in the class quality measure because students who had entered in earlier grades were not re-randomized across classrooms. Because such noise is orthogonal to entering student ability, it generates only additional attenuation bias.

V.B Analysis of Variance

We implement the analysis of variance using regression specifications of the following form for students who enter the experiment in kindergarten:

$$(9) \quad y_{icn} = \alpha_n + \gamma_{cn} + X_{icn}\delta + \varepsilon_{icn}$$

where y_{icn} is an outcome for student i who enters class c in school n in kindergarten and γ_{cn} is the class effect on the outcome, and X_{icn} a vector of pre-determined individual background characteristics.³⁰

We first estimate (9) using a fixed-effects specification for the class effects γ_{cn} . Under the null hypothesis of no class effects, the class dummies should not be significant because of random assignment of students to classrooms. We test this null hypothesis using an F test for whether $\gamma_{cn} = 0$ for all c, n . To quantify the magnitude of the class effects, we compute the variance of γ_{cn} by estimating (9) using a random-effects specification. In particular, we assume that $\gamma_{cn} \sim N(0, \sigma_c^2)$ and estimate the standard deviation of class effects σ_c .

Table 7 reports p values from F tests and estimates of σ_c for test scores and earnings. Consistent with Nye, Konstantopoulos, and Hedges (2004) – who use an ANOVA to test for class effects on scores in the STAR data – we find highly significant class effects on KG test scores. Column 1 rejects the null hypothesis of no class effects on KG scores with $p < 0.001$. The estimated standard deviation of class effects on test scores is $\sigma_c = 8.77$, implying that a one standard deviation improvement in class quality raises student test scores by 8.77 percentiles (0.32 standard deviations). Note that this measure represents the impact of improving class quality by one SD of the *within-school* distribution because the regression specification includes school fixed effects.

Column 2 of Table 7 replicates the analysis in column 1 with 8th grade test scores as the outcome. We find no evidence that kindergarten classroom assignment has any lasting impact on achievement in 8th grade as measured by standardized test scores ($p = 0.42$). As a result, the estimated standard deviation of class effects on 8th grade scores is $\sigma_c = 0.00$. This evidence suggests that KG class effects fade out by grade 8, a finding that we revisit and explore in detail in Section VI.

Columns 3-6 of Table 7 implement the ANOVA for earnings (averaged over ages 25-27). Column 3 implements the analysis without any controls besides school fixed effects. Column 4 introduces

³⁰We omit γ_{cn} for one class in each school to avoid collinearity with the school effects α_n .

the full vector of parental and student demographic characteristics. Both specifications show statistically significant class effects on earnings ($p < 0.05$). Recall that the same specification revealed no significant differences in *predicted* earnings (based on pre-determined variables) across classrooms ($p = 0.92$, as shown in column 6 of Table 2). Hence, the clustering in actual earnings by classroom is the consequence of treatments or common shocks experienced by students after random assignment to a KG classroom. The standard deviation of KG class effects on earnings in column 4 (with controls) is $\sigma_c = \$1,520$. Assigning students to a classroom that is one standard deviation better than average in kindergarten generates an increase in earnings at ages 25-27 of \$1,520 (9.6%) per year for each student. Although one cannot manipulate “class quality” directly, this substantial impact of class quality on adult earnings highlights the stakes at play in early childhood education.

Column 5 of Table 7 restricts the sample to students assigned to large classes, to test for class effects purely within large classrooms. This specification is of interest for two reasons. First, it isolates variation in class quality orthogonal to class size. Second, students in large classes were randomly reassigned to classrooms in first grade. Hence, column 5 specifically identifies clustering by kindergarten classrooms rather than a string of teachers and peers experienced over several years by a group of children who all started in the same KG class. Class quality continues to have a significant impact on earnings within large classes, showing that components of kindergarten class quality beyond size matter for earnings.

Column 6 expands upon this approach by controlling for all observable classroom characteristics: indicators for small class, teacher experience above 10 years, teacher race, teacher with degree higher than a BA, and classmates’ mean predicted score, constructed as in column 6 of Table 6. The estimated σ_c falls by only \$66 relative to the specification in column 4, implying that most of the class effects are driven by features of the classroom that we cannot observe in our data.

The F tests in Table 7 rely on parametric assumptions to test the null of no class effects. As a robustness check, we run permutation tests in which we randomly permute students between classes within each school. For each random permutation, we calculate the F statistic on the class dummies. Using the empirical distribution of F statistics from 1,000 within-school permutations of students, we calculate a non-parametric p value based on where the true F statistic (from row 1) falls in the empirical distribution. Reassuringly, these non-parametric p values are quite similar to those produced from the parametric F test, as shown in the second row of Table 7.

V.C Covariance between Class Effects on Scores and Earnings

Having established class effects on both scores and earnings, we estimate the covariance of these class effects using regression specifications of the form

$$(10) \quad y_{icnw} = \alpha_{nw} + \beta \Delta s_{cnw}^{-i} + X_{icnw} \delta + \varepsilon_{icnw},$$

where y_{icnw} represents an outcome for student i who enters class c in school n in entry grade (wave) w . The regressor of interest Δs_{cnw}^{-i} is our leave-out mean measure of peer test scores for student i at the end of entry grade w , as defined in (7).³¹ In the baseline specifications, we include students in all entry grades to analyze how the quality of the student’s randomly assigned first class affects long-term outcomes. We then test for differences in the impacts of class quality across grades K-3 by estimating (10) for separate entry grades. As above, we cluster standard errors at the school level to adjust for the fact that outcomes are correlated across students within classrooms and possibly within schools.

We begin by characterizing the impact of class quality on test scores. Figure 4a plots each student’s end-of-grade test scores vs. his entry-grade class quality, as measured by his classmates’ test scores minus his schoolmates’ test scores. The graph adjusts for school-by-entry-grade effects to isolate the random variation in class quality using the technique in Figure 3a; it does not adjust for parent and student controls. Figure 4a shows that children randomly assigned to higher quality classes upon entry – i.e., classes where their peers score higher on the end of year test – have higher test scores at the end of the year. A one percentile increase in entry-year class quality is estimated to raise own test scores by 0.68 percentiles, confirming that test scores are highly correlated across students within a classroom. Figure 4b replicates Figure 4a, changing the dependent variable to 8th grade test score. Consistent with the earlier ANOVA results, the impact fades out by grade 8. A one percentile increase in the quality of the student’s entry-year classroom raises 8th grade test scores by only 0.08 percentiles. Figure 4c uses the same design to evaluate the effects of class quality on adult wage earnings. Students assigned to a one percentile higher quality class have \$56.6 (0.4%) higher earnings on average over ages 25-27.

We verify that our method of measuring class quality does not generate a mechanical correlation between peers scores and own outcomes using permutation tests. We randomly permute students across classrooms within schools and replicate (10). We use the t statistics on β from the random

³¹Sacerdote (2001) employs analogous regression specifications to detect clustering in randomly assigned roommates’ ex-post test scores.

permutations to form an empirical cdf of t statistics under the null hypothesis of no class effects. We find that fewer than 0.001% of the t statistics from the random permutations are larger than the actual t statistic on kindergarten test score in Figure 4a of 22.7. For the earnings outcome, fewer than 0.1% of the t statistics from the random permutations are larger than the actual t statistic of 3.55. These non-parametric permutation tests confirm that the p values obtained using parametric t -tests are accurate in our application.

As noted above, part of the relationship between earnings and peers' test scores may be driven by reflection bias: high ability students raise their peers' scores and themselves have high earnings. This could generate a correlation between peer scores and own earnings even if class quality has no causal impact on earnings. However, the fact that end-of-kindergarten peer scores are not highly correlated with 8th grade test scores (Figure 4b) places a tight upper bound on the degree of this bias. In the presence of reflection bias, a high ability student (who raises her classroom peers' scores in the year she enters) should also score highly on 8th grade tests, creating a spurious correlation between first-classroom peer scores and own 8th grade scores. Therefore, if first-classroom peer scores have zero correlation with 8th grade scores, there cannot be any reflection bias. In Appendix B, we formalize this argument by deriving a bound on the degree of reflection bias in a linear-in-means model as a function of the empirical estimates in Table 8 and the cross-sectional correlations between test scores and earnings. If class quality has no causal impact on earnings ($\beta = 0$), the upper bound on the regression coefficient of earnings on class quality is \$9, less than 20% of our empirical estimate of \$56.6. Although this quantitative bound relies on the parametric assumptions of a linear-in-means model, it captures a more general intuition: the rapid fade out of class quality effects on test scores rules out significant reflection bias in impacts of peer scores on later adult outcomes. Recall that the class quality estimates also suffer from a downward attenuation bias of 23%, the same magnitude as the upper bound on the reflection bias. We therefore proceed by using end-of-year peer scores as a simple proxy for class quality.

Figure 5a characterizes the time path of the impact of class quality on earnings, dividing classrooms in two groups – those with class quality above and below the median. The time pattern of the total class quality impact is similar to the impact of teacher experience shown in Figure 3c. Prior to 2004, there is little difference in earnings between the two curves, but the gap noticeably widens beginning in 2003. By 2007, students who were assigned to classes of above-median quality are earning \$875 (5.5%) more on average. Figure 5b shows the time path of the impacts on college attendance. Students in higher quality classes are more likely to be attending college in their early

20's, consistent with their higher earnings and steeper earnings trajectories in later years.

Table 8 quantifies the impacts of class quality on wage earnings using regressions with the standard vector of parent and student controls used above. Column 1 shows that conditional on the demographic characteristics, a one percentile point increase in class quality increases a student's own test score by 0.66 percentile points. This effect is very precisely estimated, with a t statistic of 27.6, because the intra-class correlation of test scores among students is very large. Column 2 of Table 8 shows the effect of class quality on earnings. Conditional on demographic characteristics, a one percentile point increase in class quality increases earnings (averaged from 2005 to 2007) by \$50.6 per year, with a t statistic of 2.9 ($p < 0.01$). To interpret the magnitude of this effect, note that a one standard deviation increase in class quality as measured by peer scores leads to a \$455 (2.9%) increase in earnings at age 27.^{32,33}

Column 3 of Table 8 isolates the variation in class quality that is orthogonal to observable classroom characteristics by controlling for class size, teacher characteristics, and peer characteristics as in column 6 of Table 7. Class quality continues to have a significant impact on earnings conditional on these observables, confirming that components of class quality orthogonal to observables matter for earnings.

The preceding specifications pool grades K-3. Column 4 restricts the sample to kindergarten entrants and shows that a one percentile increase in KG class quality raises earnings by \$53.4. Column 5 includes only those who entered STAR after kindergarten. This point estimate is similar to that in column 4, showing that class quality in grades 1-3 matters as much for earnings as class quality in kindergarten.

Columns 6-9 show the impacts of class quality on other adult outcomes. These columns replicate the baseline specification for the full sample in column 2. Columns 6 and 7 show that a 1 percentile improvement in class quality raises college attendance rates by 0.1 percentage points, both at age 20 and before age 27 ($p < 0.05$). Column 8 shows that a one percentile increase in class quality generates an \$9.3 increase in the college quality index ($p < 0.05$). Finally, column 9 shows that a one percentile point improvement in class quality leads to an improvement of 0.25% of a standard deviation in our outcome summary index ($p < 0.05$). Appendix Table 9 reports the impacts of

³²Panel C of Appendix Table 8 replicates the specification in column 2 to show that class quality has positive impacts on all five alternative measures of wage earnings described above.

³³Part of the impact of being randomly assigned to a higher quality class in grade w may come from being placed in higher quality classes in subsequent grades. A 1 percentile increase in KG class quality (peer scores) is associated with a 0.15 percentile increase in class quality (peer scores) in grade 1. The analogous effect of grade 1 class quality on grade 2 class quality is 0.37 percentiles.

class quality on each of the five outcomes separately and shows that the point estimates of the impacts are positive for all of the outcomes. Appendix Table 10 documents the heterogeneity of class quality impacts across subgroups. The point estimates of the impacts of class quality are positive for all the groups and outcomes.

Finally, we check the robustness of our results by implementing instrumental-variable methods of detecting covariance between class effects on scores and earnings. The effects of class quality on test scores and earnings in columns 1 and 2 of Table 8 can be combined to produce a jackknife IV estimate of the earnings gain associated with an increase in test scores: $\$50.61/0.662 = \76.48 . That is, class-level factors that raise test scores by one percentile point raise earnings by \$76.48 on average. In Appendix Table 12, we show that other IV estimators yield very similar estimates.

While class effects on scores and earnings are highly correlated, a substantial portion of class effects on earnings is orthogonal to our measure of class quality. Using a random effects estimator as in Column 4 of Table 7, we find that the standard deviation of class effects on earnings falls from \$1520 to \$1372 after we control for our peer-score class quality measure Δs_{cnw}^{-i} . Hence, roughly $1 - (\frac{1372}{1520})^2 \approx 1/5$ of the variance of the class effect on earnings comes through class effects on test scores.

VI Fade-Out, Re-Emergence, and Non-Cognitive Skills

In this section, we explore why the impacts of class size and class quality in early childhood fade out on tests administered in later grades but re-emerge in adulthood. In order to have a fixed benchmark to document fade-out, we use only kindergarten entrants throughout this section and analyze the impacts of KG class quality on test scores and other outcomes in later grades.

We first document the fade-out effect using the class quality measure by estimating (10) with test scores in each grade as the dependent variable and with the standard vector of parent and student controls as well as school fixed effects. Figure 6a plots the estimated impacts on test scores in grades K-8 of increasing KG class quality by one (within-school) standard deviation. A one (within school) SD increase in KG class quality increases end-of-kindergarten test scores by 6.27 percentiles, consistent with our findings above. In grade 1, students who were in a 1 SD better KG class score approximately 1.50 percentile points higher on end-of-year tests, an effect that is significant with $p < 0.001$. The effect gradually fades over time, and by grade 4 students who were in a better KG class no longer score significantly higher on tests.³⁴

³⁴This fade-out effect is consistent with the rapid fade-out of teacher effects documented by Jacob, Lefgren, and

If a one percentile increase in 8th grade test scores is more valuable than a one percentile increase in KG test scores, then the evidence in Figure 6a would not necessarily imply that the effects of early childhood education fade out. To evaluate this possibility, we convert the test score impacts to predicted earnings gains. We run separate OLS regressions of earnings on the test scores for each grade from K-8 to estimate the cross-sectional relationship between each grade’s test score and earnings (see Appendix Table 5, column 1 for these coefficients). We then multiply the class quality effect on scores shown in Figure 6a by the corresponding coefficient on scores from the OLS earnings regression. Figure 6b plots the earnings impacts predicted by the test score gains in each grade that arise from attending a better KG class. The pattern in Figure 6b looks very similar to that in Figure 6a, showing that there is indeed substantial fade-out of the KG class quality effect on predicted earnings. By 4th grade, one would predict less than a \$50 per year gain in earnings from a better KG class based on observed test score impacts.

The final point in Figure 6b shows the actual observed earnings impact of a one SD improvement in KG class quality. The actual impact of \$483 is similar to what one would have predicted based on the improvement in KG test scores (\$588). The impacts of early childhood education re-emerge in adulthood despite fading out on test scores in later grades.

Non-Cognitive Skills. One potential explanation for fade-out and re-emergence is the acquisition of non-cognitive skills (e.g. Heckman 2000, Heckman, Stixrud, and Urzua 2006, Lindqvist and Vestman 2011). We evaluate whether non-cognitive skills could explain our findings using data on non-cognitive measures collected for a subset of STAR students in grades 4 and 8.³⁵

Finn et al. (2007) and Dee and West (2008) describe the non-cognitive measures in the STAR data in detail; we provide a brief summary here. In grade 4, teachers in the STAR schools were asked to evaluate a random subset of their students on a scale of 1-5 on several behavioral measures, such as whether the student “annoys others.” These responses were consolidated into four standardized scales measuring each student’s effort, initiative, nonparticipatory behavior, and how the student is seen to “value” the class. In grade 8, math and English teachers were asked to rate a subset of their students on a similar set of questions, which were again consolidated into the same four standardized scales. To obtain a measure analogous to our percentile measure of test scores, we construct percentile measures for these four scales and compute the average percentile score for

Sims (2008), Kane and Staiger (2008), and others.

³⁵Previous studies have used the STAR data to investigate whether class size affects non-cognitive skills (Finn et al. 1989, Dee and West 2008). They find mixed evidence on the impact of class size on non-cognitive skills: statistically significant impacts are detected in grade 4, but not in grade 8. Here, we analyze the impacts of our broader class quality measure.

each student. For 8th grade, we then take the average of the math and English teacher ratings.

Among the 6,025 students who entered Project STAR in KG and whom we match in the IRS data, we have data on non-cognitive skills for 1,671 (28%) in grade 4 and 1,780 (30%) in grade 8. The availability of non-cognitive measures for only a subset of the students who could be tracked until grade 8 naturally raises concerns about selective attrition. Dee and West (2008) investigate this issue in detail, and we replicate their findings with our expanded set of parental characteristics. In grade 8, we find no significant differences in the probability of having non-cognitive data by KG classrooms or class types (small vs. large), and confirm that in this sample the observable background characteristics are balanced across classrooms and class types. In grade 4, non-cognitive data are significantly more likely to be available for students assigned to small classes, but among the sample with non-cognitive data there are no significant differences in background characteristics across classrooms or class types. Hence, the sample for whom we have non-cognitive data appear to be balanced across classrooms at least on observable characteristics.

We begin by estimating the cross-sectional correlation between non-cognitive outcomes and earnings. Column 1 of Table 9 shows that a 1 percentile improvement in non-cognitive measures in grade 4 is associated with a \$106 gain in earnings conditional on the standard vector of demographic characteristics used above and school-by-entry-grade fixed effects. Column 2 shows that controlling for math and reading test scores in grade 4 reduces the predictive power of non-cognitive scores only slightly, to \$88 per percentile. In contrast, column 3 shows that non-cognitive skills in grade 4 are relatively weak predictors of 8th grade test scores when compared with math and reading scores in 4th grade. Because non-cognitive skills appear to be correlated with earnings through channels that are not picked up by subsequent standardized tests, they could explain fade-out and re-emergence.

To further evaluate this mechanism, we investigate the effects of KG class quality on non-cognitive skills in grade 4 and 8. As a reference, column 4 shows that a 1 percentile improvement in KG class quality increases a student's test scores in grade 4 by a statistically insignificant 0.05 percentiles. In contrast, column 5 shows that the same improvement in KG class quality generates a statistically significant increase of 0.15 percentiles in the index of non-cognitive measures in grade 4. Columns 6 and 7 replicate columns 4 and 5 for grade 8.³⁶ Again, KG class quality does not have a significant impact on 8th grade test scores but has a significant impact on non-cognitive measures.

³⁶We use all KG entrants for whom test scores are available in columns 4 and 6 to increase precision. The point estimates on test score impacts are similar for the subsample of students for whom non-cognitive data are available.

Finally, columns 8 and 9 show that the experience of the student’s teacher in kindergarten – which we showed above also impacts earnings – has a small and statistically insignificant impact on test scores but a substantially larger impact on non-cognitive measures in 8th grade ($p = 0.07$).³⁷

We can translate the impacts on non-cognitive skills into predicted impacts on earnings following the method in Figure 6b. We regress earnings on the non-cognitive measure in grade 4, conditioning on demographic characteristics, and obtain an OLS coefficient of \$101 per percentile. Multiplying this OLS coefficient by the estimated impact of class quality on non-cognitive skills in grade 4, we predict that a 1 SD improvement in KG class quality will increase earnings by \$139. The same exercise for 4th grade math+reading test scores yields a predicted earnings gain of \$40, suggesting that improvements in non-cognitive skills explain a larger share of actual earnings gains than improvements in cognitive performance. In contrast, a one standard deviation increase in class quality is predicted to raise 8th grade test scores by only 0.47 percentiles based on its observed impacts on non-cognitive skills in grade 4 and the cross-sectional correlation between grade 4 non-cognitive skills and grade 8 test scores. This predicted impact is quite close to the actual impact of class quality on 8th grade scores of 0.57 percentiles. Hence, the impacts of class quality on non-cognitive skills is consistent with both fade-out on scores and re-emergence on adult outcomes.

Intuitively, a better kindergarten classroom might simultaneously increase performance on end-of-year tests and improve untested non-cognitive skills. For instance, a KG teacher who is able to make her students memorize vocabulary words may instill social skills in the process of managing her classroom successfully. These non-cognitive skills may not be well measured by standardized tests, leading to very rapid fade-out immediately after KG. However, these skills could still have returns in the labor market.

Although non-cognitive skills provide one plausible explanation of the data, our analysis is far from definitive proof of the importance of non-cognitive skills. The estimates of non-cognitive impacts could suffer from attrition bias and are somewhat imprecisely estimated. Moreover, our analysis does not show that manipulating non-cognitive skills directly has causal impacts on adult outcomes. We have shown that high quality KG classes improve both non-cognitive skills and adult outcomes, but the mechanism through which adult outcomes are improved could run through another channel that is correlated with the acquisition of non-cognitive skills. It would be valuable to analyze interventions that target non-cognitive skills directly in future work.

³⁷Appendix Table 13 decomposes the relationships described in Table 9 into the four constituent components of non-cognitive skill.

VII Conclusion

The impacts of education have traditionally been measured by achievement on standardized tests. This paper has shown that many of the interventions that raise test scores also improve long-term outcomes. Students who were randomly assigned to higher quality classrooms in grades K-3 earn more, are more likely to attend college, save more for retirement, and live in better neighborhoods. Yet the same students do not do much better on standardized tests in later grades. These results suggest that policy makers may wish to rethink the objective of raising test scores and evaluating interventions via long-term test score gains. Researchers who had examined only the impacts of STAR on test scores would have incorrectly concluded that early childhood education does not have long-lasting impacts. While the quality of education is best judged by directly measuring its impacts on adult outcomes, our analysis suggests that *contemporaneous* (end-of-year) test scores are a reasonably good short-run measure of the quality of a classroom.

We conclude by using our empirical estimates to provide rough calculations of the benefits of various policy interventions (see Appendix C for details). These cost-benefit calculations rely on several strong assumptions, including the assumption that the percentage gain in earnings observed at age 27 remains constant over the lifecycle. We ignore non-monetary returns to education (such as reduced crime) as well as general equilibrium effects and discount earnings gains at a 3% annual rate back to age 6, the point of the intervention.

(1) Class Quality. The random-effects estimate reported in column 4 of Table 7 implies that increasing class quality by one standard deviation of the distribution within schools raises earnings by \$1,520 (9.6%) at age 27. Under the preceding assumptions, this translates into a lifetime earnings gain of approximately \$39,100 for the average individual. For a classroom of twenty students, this implies a present-value benefit of \$782,000 for improving class quality for a single year by one (within-school) standard deviation. This large figure includes all potential benefits from an improved classroom environment, including better peers, teachers, and random shocks, and hence is useful primarily for understanding the stakes at play in early childhood education. It is less helpful from a policy perspective because one cannot implement interventions that directly improve classroom quality. This motivates the analysis of class size and better teachers, two factors that contribute to classroom quality.

(2) Class Size. We predict the gains from class size reduction using the estimated impact of classroom quality on scores and earnings. We estimate that a 1 percentile increase in class quality

raises test scores by 0.66 percentiles and earnings by \$50.6, implying an earnings gain of \$76.7 per percentile increase in test scores. Next, we make the strong assumption that the ratio of earnings gains to test score gains is the same for changes in class size as it is for improvements in class quality more generally. Under this assumption, a 33% class size reduction in grades K-3 (which raised test scores by 4.8 percentiles) is predicted to raise earnings by $4.8 \times \$76.7 = \368 (2.3%) at age 27. This calculation implies a present value earnings gain from class size reduction of \$9,460 per student and \$189,000 for the classroom.

(3) Teachers. Rockoff (2004), Rivkin, Hanushek, and Kain (2005), and Kane and Staiger (2008) use datasets with repeated teacher observations to estimate that a one standard deviation increase in teacher quality raises test scores by approximately 0.2 standard deviations (5.4 percentiles).³⁸ Under the strong assumption that the ratio of earnings gains to test score gains is the same for changes in teacher quality and class quality more broadly, this translates into an earnings gain of $5.4 \times \$76.7 = \416 (2.6%) at age 27 and hence a present-value earnings gain of \$10,700 per student. Hence, a one standard deviation improvement in teacher quality in a single year generates earnings gains of \$214,000 for a classroom of twenty students.

The magnitude of the estimated impacts of teachers on earnings suggests that good teachers can create great social value, perhaps several times larger than current teacher salaries.³⁹ However, our results do not have direct implications for optimal teacher salaries or merit pay policies as we do not know whether higher salaries or merit pay would improve teacher quality.⁴⁰ Relative to efforts that seek to improve the quality of teachers, class size reductions have the important advantage of being more well-defined and straightforward to implement. Our findings on the importance of teacher quality caution that reductions in class size must be implemented carefully to generate improvements in outcomes. If schools are forced to reduce teacher and class quality along other dimensions when reducing class size, the net gains from class size reduction may be diminished (Jepsen and Rivkin 2009, Sims 2009).

While further research is needed to determine the best ways to improve classroom quality, the broad message of these calculations is that children who attend higher quality schools – as measured

³⁸We use estimates of the impacts of teacher quality on scores from other studies to predict earnings gains because we do not have repeat observations on teachers in our data. In future work, it would be extremely valuable to link datasets with repeat observations on teachers to administrative data on students in order to measure teachers' impacts on earnings directly.

³⁹According to calculations from the 2006-2008 American Community Survey, the mean salary for elementary and middle school teachers in the U.S. was \$39,164 (in 2009 dollars).

⁴⁰An analogy with executive compensation might be helpful in understanding this point. CEOs' decisions have large impacts on the firms they run, and hence can create or destroy large amounts of economic value. But this does not necessarily imply that increasing CEO compensation or pay-for-performance would improve CEO decisions.

by performance on contemporaneous standardized tests – fare substantially better as adults. In the United States, the current property-tax system of school finance gives higher income families access to better public schools on average. This system could amplify inequality, as disadvantaged children generally attend lower quality, resource-constrained schools. Our analysis suggests that improving early childhood education in disadvantaged areas – perhaps through federal tax credits or tax policy reforms – may significantly reduce poverty and inequality in the long run (Chetty and Friedman 2011).

References

1. Aaronson, Daniel, Lisa Barrow, and William Sander, "Teachers and Student Achievement in Chicago Public High Schools," *Journal of Labor Economics* 24:1 (2007), 95-135.
2. Almond, Douglas, and Janet Currie, "Human Capital Development Before Age Five," forthcoming, *Handbook of Labor Economics*, Volume 4 (2010).
3. *American Community Survey*, (<http://www.census.gov>, U.S. Census Bureau), 2006-2008 ACS 3-year data.
4. Angrist, Joshua D., Guido W. Imbens, and Alan B. Krueger. "Jackknife Instrumental Variables Estimation," *Journal of Applied Econometrics* 14:1 (1999), 57-67.
5. Angrist, Joshua D. and Alan B. Krueger, "Split-Sample Instrumental Variables Estimates of the Return to Schooling," *Journal of Business and Economic Statistics*, American Statistical Association, 13:2 (1995), 225-235.
6. Bacolod, Marigee P, "Do Alternative Opportunities Matter? The Role of Female Labor Markets in the Decline of Teacher Quality," *Review of Economics and Statistics*, 89:4 (2007), 737-751.
7. Chetty, Raj, John N. Friedman, "Does Local Tax Financing of Public Schools Perpetuate Inequality?" *National Tax Association Proceedings, 103rd Annual Conference on Taxation* 2011.
8. Cilke, James "A Profile of Non-Filers," U.S. Department of the Treasury, Office of Tax Analysis Working Paper No. 78, July, 1998.
9. Corcoran, Sean P., William N. Evans, Robert M. Schwab, "Changing Labor-market Opportunities for Women and the Quality of Teachers, 1957-2000," *American Economic Review*, 94 (2004), 230-235.
10. Currie, Janet. "Inequality at Birth: Some Causes and Consequences." Columbia University Working Paper, 2010.
11. Currie, Janet, and Duncan Thomas, "Early Test Scores, School Quality and SES: Longrun Effects of Wage and Employment Outcomes," *Worker Wellbeing in a Changing Labor Market*, 20 (2001), 103-132.
12. Dee, Thomas S., "Teachers, Race, and Student Achievement in a Randomized Experiment," *Review of Economics and Statistics*, 86 (2004), 195-210.
13. Dee, Thomas S., and Martin West, "The Non-Cognitive Returns to Class Size," NBER Working Paper No. 13994, 2008.
14. Finn, Jeremy D., DeWayne Fulton, Jayne Zaharias, and Barbara A. Nye, "Carry-Over Effects of Small Classes," *Peabody Journal of Education*, 67 (1989) 75-84.
15. Finn, Jeremy D., Jayne Boyd-Zaharias, Reva M. Fish, and Susan B. Gerber, "Project STAR and Beyond: Database User's Guide," Lebanon: Heros, inc., 2007.

16. Guryan, Jonathan, Kory Kroft and Matthew J. Notowidigdo, "Peer Effects in the Workplace: Evidence from Random Groupings in Professional Golf Tournaments," *American Economic Journal: Applied Economics*, 1 (2009), 34-68.
17. Haider, Steven, and Gary Solon, "Life-cycle variation in the Association Between Current and Lifetime Earnings," *The American Economic Review*, 96 (2006), 1308-1320.
18. Hanushek, Eric A., "The Failure of Input-Based Schooling Policies." *Economic Journal* 113(1): F64-F98, 2003.
19. Hanushek, Eric A., "Economic Aspects of the Demand for Teacher Quality," prepared for the *Economics of Education Review*, 2010.
20. Heckman, James J., "Policies to Foster Human Capital," *Research in Economics*, 54:1 (2000), 3-56.
21. Heckman, James J., Jora Stixrud, and Sergio Urzua, "The Effects of Cognitive and Non-cognitive Abilities on Labor Market Outcomes and Social Behaviors." *Journal of Labor Economics* 24:3 (2006), 411-482.
22. Holland, Paul W. "Statistics and Causal Inference," *Journal of the American Statistical Association*, 81 (1986), 945-960
23. Hoxby, Caroline M. and Andrew Leigh, "Pulled away or pushed out? Explaining the decline of teacher aptitude in the United States," *American Economic Review*, 94 (2004) 236-240.
24. Internal Revenue Service. *Document 6961: Calendar Year Projections of Information and Withholding Documents for the United States and IRS Campuses 2010-2018*, IRS Office of Research, Analysis, and Statistics, Washington, D.C, 2010.
25. Jacob, Brian A., Lars Lefgren and David Sims, "The Persistence of Teacher-Induced Learning Gains," NBER Working Paper No. 14065, 2008.
26. Jepsen, Christopher and Steven Rivkin, "Class Size Reduction and Student Achievement: The Potential Tradeoff between Teacher Quality and Class Size," *Journal of Human Resources*, 44:1 (2009) 223-250.
27. Kane, Thomas, and Douglas O. Staiger, "Estimating Teacher Impacts on Student Achievement: An Experimental Evaluation," NBER Working Paper No. 14607, 2008.
28. Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz, "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75 (2007), 83-119.
29. Krueger, Alan B, "Experimental Estimates of Education Production Functions," *Quarterly Journal of Economics*, 114 (1999), 497-532.
30. Krueger, Alan B., and Diane M. Whitmore, "The Effect of Attending a Small Class in the Early Grades on College-Test Taking and Middle School Test Results: Evidence from Project STAR," *The Economic Journal*, 111 (2001), 1-28.
31. Lindqvist, Erik and Roine Vestman. "The Labor Market Returns to Cognitive and Noncognitive Ability: Evidence from the Swedish Enlistment." *American Economic Journal: Applied Economics*, 3 (2011), 101-28.

32. Manski, Charles, "Identification of Exogenous Social Effects: The Reflection Problem," *Review of Economic Studies*, 60 (1993), 531-542.
33. Muennig, Peter, Gretchen Johnson, Jeremy Finn, and Elizabeth Ty Wilde, The Effect of Small Class Sizes on Mortality Through Age 29: Evidence From a Multi-Center Randomized Controlled Trial, unpublished mimeo, 2010.
34. Nye, Barbara, Spyros Konstantopoulos, and Larry V. Hedges, "How Large are Teacher Effects?" *Educational Evaluation and Policy Analysis*, 26 (2004), 237-257.
35. Rivkin, Steven. G., Eric. A. Hanushek, and John F. Kain, "Teachers, Schools and Academic Achievement," *Econometrica*, 73 (2005), 417-458.
36. Rockoff, Jonah E., "The Impact of Individual Teachers on Student Achievement: Evidence from Panel Data," *American Economics Review*, 94 (2004), 247-252.
37. Rockoff, Jonah E., and Douglas Staiger, "Searching for Effective Teachers with Imperfect Information," *Journal of Economic Perspectives*, 24 (2010), 97-117.
38. Sacerdote, Bruce, "Peer Effects with Random Assignment: Results for Dartmouth Roommates," *Quarterly Journal of Economics*, 116 (2001), 681-704.
39. Schanzenbach, Diane W., "What Have Researchers Learned From Project STAR?" *Brookings Papers on Education Policy*, (2006), 205-228.
40. Sims, David, "Crowding Peter to Educate Paul: Lessons From a Class Size Reduction Externality," *Economics of Education Review* 28:4 (2009), 465-473.
41. US Census Bureau. "School Enrollment—Social and Economic Characteristics of Students: October 2008, Detailed Tables," Washington, D.C., 2010 (<http://www.census.gov/population/www/socdemo/school.html>).
42. Word, Elizabeth., John. Johnston, Helen. P. Bain, B. Dewayne Fulton, Charles M. Achilles, Martha N. Lintz, John Folger, and Carolyn Breda, "The State of Tennessee's Student/Teacher Achievement Ratio (STAR) Project: Technical Report 1985-1990," Tennessee State Department of Education, 1990.

Appendix A: Algorithm for Matching STAR Records to Tax Data

The tax data were accessed through contract TIRNO-09-R-00007 with the Statistics of Income (SOI) Division at the US Internal Revenue Service. Requests for research contracts by SOI are posted online at the Federal Business Opportunities <https://www.fbo.gov/>. SOI also welcomes research partnerships between outside academics and internal researchers at SOI.

STAR records were matched to tax data using social security number (SSN), date of birth, gender, name, and STAR elementary school ZIP code. Note that STAR records do not contain all the same information. Almost every STAR record contains date of birth, gender, and last name. Some records contain no SSN while others contain multiple possible SSNs. Some records contain no first name. A missing field yielded a non-match unless otherwise specified.

We first discuss the general logic of the match algorithm and then document the routines in detail. The match algorithm was designed to match as many records as possible using variables that are *not* contingent on ex post outcomes. SSN, date of birth, gender, and last name in the tax data are populated by the Social Security Administration using information that is not contingent on ex post outcomes. First name and ZIP code in tax data are contingent on observing some ex post outcome. First name data derive from information returns, which are typically generated after an adult outcome like employment (W-2 forms), college attendance (1098-T forms), and mortgage interest payment (1098 forms). The ZIP code on the claiming parent’s 1040 return is typically from 1996 and is thus contingent on the ex post outcome of the STAR subject not having moved far from her elementary school by age 16.

89.8% of STAR records were matched using only ex ante information. The algorithm first matched as many records as possible using only SSN, date of birth, gender, and last name. It then used first name only to *exclude* candidate matches based on date of birth, gender, and last name, often leaving only one candidate record remaining. Because that exclusion did not condition on an information return having been filed on behalf of that remaining candidate, these matches also did not condition on ex post outcomes.

The match algorithm proceeded as follows, generating seven match types denoted A through G. The matches generated purely through ex-ante information are denoted A through E below and account for 89.8% of STAR records. Matches based on ex-post-information are denoted F and G below and constitute an additional 5.4% of STAR records. The paper reports results using the full 95.0% matched sample, but all the qualitative results hold in the 89.8% sample matched using only ex ante information.

1. Match STAR records to tax records by SSN. For STAR records with multiple possible SSNs, match on all of these SSNs to obtain a set of candidate tax record matches for each STAR record with SSN information. Each candidate tax record contains date of birth, gender, and first four letters of every last name ever assigned to the SSN.
 - Match Type A. Keep unique matches after matching on first four letters of last name, date of birth, and gender.
 - Match Type B. Refine non-unique matches by matching on either first four letters of last name or on “fuzzy” date of birth. Then keep unique matches. Fuzzy date of birth requires the absolute value of the difference between STAR record and tax record dates of birth to be in the set $\{0,1,2,3,4,5,9,10,18,27\}$ in days, in the set $\{1,2\}$ in months, or in the set $\{1\}$ in years. These sets were chosen to reflect common mistakes in recorded dates of birth, such as being off by one day (e.g. 12 vs. 13) or inversion of digits (e.g. 12 vs. 21).

2. Match residual unmatched STAR records to tax records by first four letters of last name, date of birth, and gender.
 - Match Type C. Keep unique matches.
 - Match Type D. Refine non-unique matches by excluding candidates who have a first name issued on information returns (e.g. W-2 forms, 1098-T forms, and various 1099 forms) that does not match the STAR first name on first four letters when the STAR first name is available. Then keep unique matches.
 - Match Type E. Refine residual non-unique matches by excluding candidates who have SSNs that, based on SSN area number, were issued from outside the STAR region (Tennessee and neighboring environs). Then keep unique matches.
 - Match Type F. Refine residual non-unique matches by keeping unique matches after each of the following additional criteria is applied: require a first name match when STAR first name is available, require the candidate tax record’s SSN to have been issued from the STAR region, and require the first three digits of the STAR elementary school ZIP code to match the first three digits of the ZIP code on the earliest 1040 return on which the candidate tax record was claimed as a dependent.

3. Match residual unmatched STAR records to tax records by first four letters of last name and fuzzy date of birth.
 - Match Type G. Keep unique matches after each of several criteria is sequentially applied. These criteria include matches on first name, last name, and middle initial using the candidate tax record’s information returns; on STAR region using the candidate tax record’s SSN area number; and between STAR elementary school ZIP code and ZIP code on the earliest 1040 return on which the candidate tax record was claimed as a dependent.

The seven match types cumulatively yielded a 95.0% match rate:

Match type	Frequency	Percent	Cumulative percent
A	7036	60.8%	60.8%
B	271	2.3%	63.1%
C	699	6.0%	69.2%
D	1391	12.0%	81.2%
E	992	8.6%	89.8%
F	299	2.6%	92.4%
G	304	2.6%	95.0%

Identifiers such as names and SSN’s were used solely for the matching procedure. After the match was completed, the data were de-identified (i.e., individual identifiers such as names and SSNs were stripped) and the statistical analysis was conducted using the de-identified dataset.

Appendix B: Derivations for Measurement of Unobserved Class Quality

This appendix derives the estimators discussed in the empirical model in Section V and quantifies the degree of attenuation and reflection bias. We first use equations (3) and (4) to define

average of test scores and earnings within each class c and school n :

$$\begin{aligned} s_{cn} &= d_n + z_{cn} + a_{cn} \\ y_{cn} &= \delta_n + \beta z_{cn} + \gamma z_{cn}^Y + \rho a_{cn} + \nu_{cn} \\ s_n &= d_n + z_n + a_n \\ y_n &= \delta_n + \beta z_n + \gamma z_n^Y + \rho a_n + \nu_n. \end{aligned}$$

We define variables demeaned within schools as

$$\begin{aligned} s_{icn} - s_n &= z_{cn} - z_n + a_{icn} - a_n \\ \Delta s_{cn} \equiv s_{cn} - s_n &= z_{cn} - z_n + a_{cn} - a_n, \\ y_{icn} - y_n &= \beta(z_{cn} - z_n) + \gamma(z_{cn}^Y - z_n^Y) + \rho(a_{icn} - a_n) + \nu_{icn} - \nu_n \\ y_{cn} - y_n &= \beta(z_{cn} - z_n) + \gamma(z_{cn}^Y - z_n^Y) + \rho(a_{cn} - a_n) + \nu_{cn} - \nu_n. \end{aligned}$$

Recall that a_{icn} and ν_{icn} are independent of each other and z_{cn} . Let $\sigma^2 = \text{var}(a_{icn})$. We assume in parts 1 and 2 below that $z_{cn}, z_{cn}^Y \perp a_{icn}$, ruling out peer effects. Note also that, as $z_{cn} \perp z_{cn}^Y$, the interventions that affect only test scores drop out entirely of the covariance analysis below.

1. Mean score estimator. The simplest proxy for class quality is the average test score within a class. Since we include school fixed effects in all specifications, s_{cn} is equivalent to Δs_{cn} as defined above. Therefore, consider the following (school) fixed effects OLS regression:

$$(11) \quad y_{icn} = \alpha_n + b^M \Delta s_{cn} + \varepsilon_{icn}.$$

The coefficient estimate has expectation

$$\mathbb{E} \hat{b}^M = \frac{\text{cov}(y_{icn} - y_n, s_{cn} - s_n)}{\text{var}(s_{cn} - s_n)},$$

which we can rewrite as

$$\begin{aligned} \mathbb{E} \hat{b}^M &= \frac{\text{cov}\left(\beta(z_{cn} - z_n) + \rho\left(a_{icn} - \frac{\sum_k \sum_j a_{jkn}}{I \cdot C}\right), z_{cn} - z_n + \frac{\sum_j a_{jcn}}{I} - \frac{\sum_k \sum_j a_{jkn}}{I \cdot C}\right)}{\text{var}\left(z_{cn} - z_n + \frac{\sum_j a_{jcn}}{I} - \frac{\sum_k \sum_j a_{jkn}}{I \cdot C}\right)} \\ &= \frac{\beta \text{var}(z_{cn} - z_n) + \rho \sigma^2 \frac{C-1}{IC}}{\text{var}(z_{cn} - z_n) + \sigma^2 \frac{C-1}{IC}}. \end{aligned}$$

Even absent class effects ($\beta = 0$), we have $\mathbb{E} \hat{b}^M > 0$ if I is finite and $\rho > 0$. With finite class size, b^M is upward-biased due to the correlation between wages and own-score, which is included within the class quality measure.

2. Leave-out mean estimator. We address the upward bias due to the own observation problem using a leave-out mean estimator. Consider the OLS regression with school fixed effects

$$(12) \quad y_{icn} = \alpha_n + b^{LM} \Delta s_{cn}^{-i} + \varepsilon_{icn}.$$

where $\Delta s_{cn}^{-i} = s_{cn}^{-i} - s_n^{-i}$ is defined as in (7). The coefficient b^{LM} has expectation

$$\mathbb{E} \hat{b}^{LM} = \frac{\text{cov}(y_{icn} - y_n, s_{cn}^{-i} - s_n^{-i})}{\text{var}(s_{cn}^{-i} - s_n^{-i})},$$

which we can rewrite as

$$\begin{aligned}\mathbb{E}\hat{b}^{LM} &= \frac{\text{cov}\left(\beta(z_{cn} - z_n) + \rho(a_{icn} - a_n), \frac{IC}{IC-1}(z_{cn} - z_n) + \frac{1}{I-1}\sum_{j\neq i} a_{jcn} - \frac{1}{IC-1}\sum_k \sum_{j\neq i} a_{jkn}\right)}{\text{var}\left(\frac{IC}{IC-1}(z_{cn} - z_n) + \frac{1}{I-1}\sum_{j\neq i} a_{jcn} - \frac{1}{IC-1}\sum_k \sum_{j\neq i} a_{jkn}\right)} \\ &= \beta \times \frac{\frac{IC}{IC-1}\text{var}(z_{cn} - z_n)}{\frac{(IC)^2}{(IC-1)^2}\text{var}(z_{cn} - z_n) + \frac{\sigma^2}{I-1} - \frac{\sigma^2}{IC-1}}\end{aligned}$$

Hence, $\mathbb{E}\hat{b}^{LM} = 0$ if and only if $\beta\text{var}(z_{cn} - z_n) = 0$ (no class effects) even when I and C are finite.⁴¹ However, b^{LM} is attenuated relative to β because peer scores are a noisy measure of class quality.

Quantifying the degree of attenuation bias. We can quantify the degree of attenuation bias by using the within-class variance of test scores as an estimate of $\sigma^2 = \text{var}(a_{icn})$. First, note that:

$$\begin{aligned}\widehat{\text{var}}(z_{cn} - z_n) &= \frac{(IC-1)^2}{(IC)^2} \left[\widehat{\text{var}}(s_{cn}^{-i} - s_n^{-i}) - \left(\frac{\hat{\sigma}^2}{I-1} - \frac{\hat{\sigma}^2}{I \cdot C - 1} \right) \right] \\ &= \frac{(83.63)^2}{(84.73)^2} \left[81.75 - \left(\frac{437.4}{19.07} - \frac{437.4}{83.63} \right) \right] \\ &= 62.39\end{aligned}$$

where we use the sample harmonic means for IC , $IC-1$, and $I-1$ because the number of students in each class and school varies across the sample. This implies an estimate of bias of

$$\frac{\frac{83.63}{84.73}62.39}{\frac{(83.63)^2}{(84.73)^2}62.39 + \frac{437.4}{19.07} - \frac{437.4}{83.63}} = 0.773.$$

That is, b^{LM} is attenuated relative to β by 22.7%. Note that this bias calculation assumes that all students in the class were randomly assigned, which is true only in KG. In later grades, the degree of attenuation in b^{LM} when (12) is estimated using new entrants is larger than 22.7%, because existing students were not necessarily re-randomized.

3. Peer effects and reflection bias. With peer effects, the assumption $z_{cn} \perp a_{icn}$ does not hold. We expect z_{cn} and a_{icn} to be positively correlated with peer effects as a higher ability student has a positive impact on the class. This leads to an upward bias in both b^{LM} and b^{SS} due to the reflection problem. To characterize the magnitude of this bias, consider a standard linear-in-the-means model of peer effects, in which

$$z_{cn} = t_{cn} + \frac{\theta}{I} \sum_j a_{jcn}$$

with $t_{cn} \perp a_{jcn}$ for all j . Here t_{cn} represents the component of class effects independent of peer

⁴¹Equation (12) is similar to specifications used to estimate peer effects, but uses ex-post peer outcomes on the right hand side. Regression specifications used to estimate peer effects typically control for the individual's characteristic (e.g. age) and examine the effect of mean ex-ante peer characteristics (e.g. mean age) on outcomes. We do not control for own scores because end-of-year scores are endogenous: the effect of peer scores (which measures class quality) on outcomes runs through impacts on own scores. Without controlling for own scores, there is a downward bias in proxying for class quality purely using classmates' scores (s_{cn}^{-i}), because above-average students mechanically have below-average classmates, as noted by Guryan, Kroft, and Notowidigdo (2009). The difference $s_{cn}^{-i} - s_n^{-i}$ eliminates this mechanical bias.

effects (e.g., a pure teacher effect). The parameter $\theta > 0$ captures the strength of peer effects. Averaging across classrooms within a school implies that

$$z_n = t_n + \frac{\theta}{IC} \sum_k \sum_j a_{jkn}.$$

In this model, the leave-out mean proxy of class quality is

$$\Delta s_{cn}^{-i} = s_{cn}^{-i} - s_n^i = \frac{IC}{IC-1}(t_{cn} - t_n) + \theta \frac{IC}{IC-1}(a_{cn} - a_n) + \frac{1}{I-1} \sum_{j \neq i} a_{jcn} - \frac{1}{IC-1} \sum_k \sum_{j \neq i} a_{jkn}$$

and the expectation of the coefficient b^{LM} is

$$\begin{aligned} \mathbb{E} \hat{b}^{LM} &= \frac{\text{cov}(y_{icn} - y_n, s_{cn}^{-i} - s_n^{-i})}{\text{var}(s_{cn}^{-i} - s_n^{-i})} \\ &= \frac{\beta \cdot \left[\frac{IC}{IC-1} \text{var}(t_{cn} - t_n) + (\theta + \theta^2) \sigma^2 \frac{C-1}{IC-1} \right] + \rho \theta \sigma^2 \frac{C-1}{IC-1}}{\frac{(IC)^2}{(IC-1)^2} \text{var}(t_{cn} - t_n) + (2\theta + \theta^2) \sigma^2 \frac{IC(C-1)}{(IC-1)^2} + \frac{\sigma^2}{I-1} - \frac{\sigma^2}{I \cdot C-1}} \end{aligned}$$

The last term in the numerator is the reflection bias that arises because a high ability student has both high earnings (through ρ) and a positive impact on peers' scores (through θ). Because of this term, we can again obtain $\mathbb{E} \hat{b}^{LM} > 0$ even when $\beta = 0$. This bias occurs iff $\theta > 0$ (i.e., we estimate $b^{LM} > 0$ only if there are peer effects on test scores). This bias is of order $\frac{1}{I}$ since any given student is only one of I students in a class that affects class quality.

Bounding the degree of reflection bias. We use the estimated impact of KG class quality on 8th grade test scores to bound the degree of reflection bias in our estimate of the impact of class quality on earnings. Recall that the reflection bias arises because a high ability student has better long-term outcomes and also has a positive impact on peers' kindergarten test scores. Therefore, the same reflection bias is present when estimating \hat{b}^{LM} using eighth grade test scores as the outcome instead of earnings.

Denote by \hat{b}_e^{LM} the estimated coefficient on Δs_{cn}^{-i} when the outcome y is earnings and \hat{b}_s^{LM} the same coefficient when the outcome y is grade 8 test scores.⁴² Similarly, denote by ρ_e and ρ_s the (within class) correlation between individual kindergarten test score and earnings or eighth grade test score. Under our parametric assumptions, these two parameters can be estimated by an OLS regression $y_{icn} = \alpha_{cn} + \rho s_{icn} + \varepsilon_{icn}$ that includes class fixed effects.

To obtain an upper bound on the degree of reflection bias, we make the extreme assumption that the effect of kindergarten class quality on eighth grade test scores (\hat{b}_s^{LM}) is due entirely to the reflection bias. If there are no pure class effects ($\text{var}(t_{cn} - t_n) = 0$) and peers do not affect earnings ($\beta = 0$),

$$(13) \quad \mathbb{E} \hat{b}^{LM} = \frac{\rho \theta}{\frac{1}{1-\frac{1}{I}} + \frac{2\theta + \theta^2}{1-\frac{1}{IC}}} \simeq \frac{\rho \theta}{(1 + \theta)^2}$$

Using equation (13) for \hat{b}_s^{LM} and the estimate of $\hat{\rho}_s$, we obtain an estimate of the reflection bias parameter $\frac{\theta}{(1+\theta)^2} = \hat{b}_s^{LM} / \hat{\rho}_s$. Combining this estimate and the estimate $\hat{\rho}_e$, we can then use equation

⁴²The latest test score we have in our data is in grade 8. We find similar results if we use other grades, such as fourth grade test scores.

(13) for \hat{b}_e^{LM} to obtain an upper bound on the \hat{b}_e^{LM} that could arise solely from reflection bias.

We implement the bound empirically by estimating the relevant parameters conditional on the vector of parent and student demographics, using regression specifications that parallel those used in column 3 of Table 4 and column 2 of Table 8. For eighth grade scores, we estimate $\hat{b}_s^{LM} = 0.057$ (SE = 0.036) and $\rho_s = 0.597$ (SE = 0.016), and hence

$$\frac{\theta}{(1 + \theta)^2} = \frac{0.057}{0.597} = 0.0955.$$

For earnings, we estimate $\rho_e = \$90.04$ (SE = \$8.65) in Table 4. Hence, if the entire effect of class quality on earnings were due to reflection bias, we would obtain

$$\hat{b}_e^{LM} = \frac{\rho_e \theta}{(1 + \theta)^2} = \$90.04 \cdot 0.0955 = \$8.60 \text{ (SE = \$5.49)}$$

where the standard error is computed using the delta method under the assumption that the estimates of \hat{b}_s^{LM} , ρ_s , and ρ_e are uncorrelated. This upper bound of \$8.60 due to reflection bias is only 17% of the estimate of $\hat{b}_e^{LM} = \$50.61$ (SE = \$17.45) in Table 8. Note that the degree of reflection bias would be smaller in the presence of class quality effects ($\beta > 0$); hence, 17% is an upper bound on the degree of reflection bias in a linear-in-means model of peer effects.

Appendix C: Cost-Benefit Analysis

We make the following assumptions to calculate the benefits of the policies considered in the conclusion. First, following Krueger (1999), we assume a 3% annual discount rate and discount all earnings streams back to age 6, the point of the intervention. Second, we use the mean wage earnings of a random sample of the U.S. population in 2007 as a baseline earnings profile over the lifecycle. Third, because we can observe earnings impacts only up to age 27, we must make an assumption about the impacts after that point. We assume that the percentage gain observed at age 27 remains constant over the lifecycle. This assumption may understate the total benefits because the earnings impacts appear to grow over time, for example as college graduates have steeper earnings profiles. Finally, our calculations ignore non-monetary returns to education such as reduced crime. They also ignore general equilibrium effects: increasing the education of the population at large would increase the supply of skilled labor and may depress wage rates for more educated individuals, reducing total social benefits. Under these assumptions, we calculate the present-value earnings gains for a classroom of 20 students from three interventions: improvements in classroom quality, reductions in class size, and improvements in teacher quality.

(1) **Class Quality.** The random-effects estimate reported in column 4 of Table 7 implies that increasing class quality by one standard deviation of the distribution within schools raises earnings by \$1,520 (9.6%) at age 27. Under the preceding assumptions, this translates into a lifetime earnings gain of approximately \$39,100 for the average individual. This implies a present-value benefit of \$782,000 for improving class quality by one within-school standard deviation.

(2) **Class Size.** We calculate the benefits of reducing class size by 33% in two ways. The first method uses the estimated earnings gain from being assigned to a small class reported in column 5 of Table 5. The point estimate of \$4 in Table 5 translates into a lifetime earnings gain from reducing class size by 33% for one year of \$103 in present value per student, or \$2,057 for a class that originally had twenty students. But this estimate is imprecise: the 95% confidence interval for the lifetime earnings gain of reducing class size by 33% for one year ranges from -\$17,500 to \$17,700 per child. Moreover, the results for other measures such as college attendance suggest that the earnings impact may be larger in the long run.

To obtain more precise estimates, we predict the gains from class size reduction using the estimated impact of classroom quality on scores and earnings. We estimate that a 1 percentile increase in class quality raises test scores by 0.66 percentiles and earnings by \$50.6. This implies an earnings gain of \$76.67 per percentile (or 13.1% per standard deviation) increase in test scores. We make the strong assumption that the ratio of earnings gains to test score gains is the same for changes in class size as it is for improvements in class quality more generally.⁴³ Under this assumption, smaller classes (which raised test scores by 4.8 percentiles) are predicted to raise earnings by $4.8 \times \$76.7 = \368 (2.3%) at age 27. This calculation implies a present value earnings gain from class size reduction of \$9,460 per student and \$189,000 for the classroom.

Calculations analogous to those in Krueger (1999) imply that the average cost per child of reducing class size by 33% for 2.14 years (the mean treatment duration for STAR students) is \$9,355 in 2009 dollars.⁴⁴ Our second calculation suggests that the benefit of reducing class size might outweigh the costs. However, we must wait for more time to elapse before we can determine whether the predicted earnings gains based on the class quality estimates are in fact realized by those who attended smaller classes.

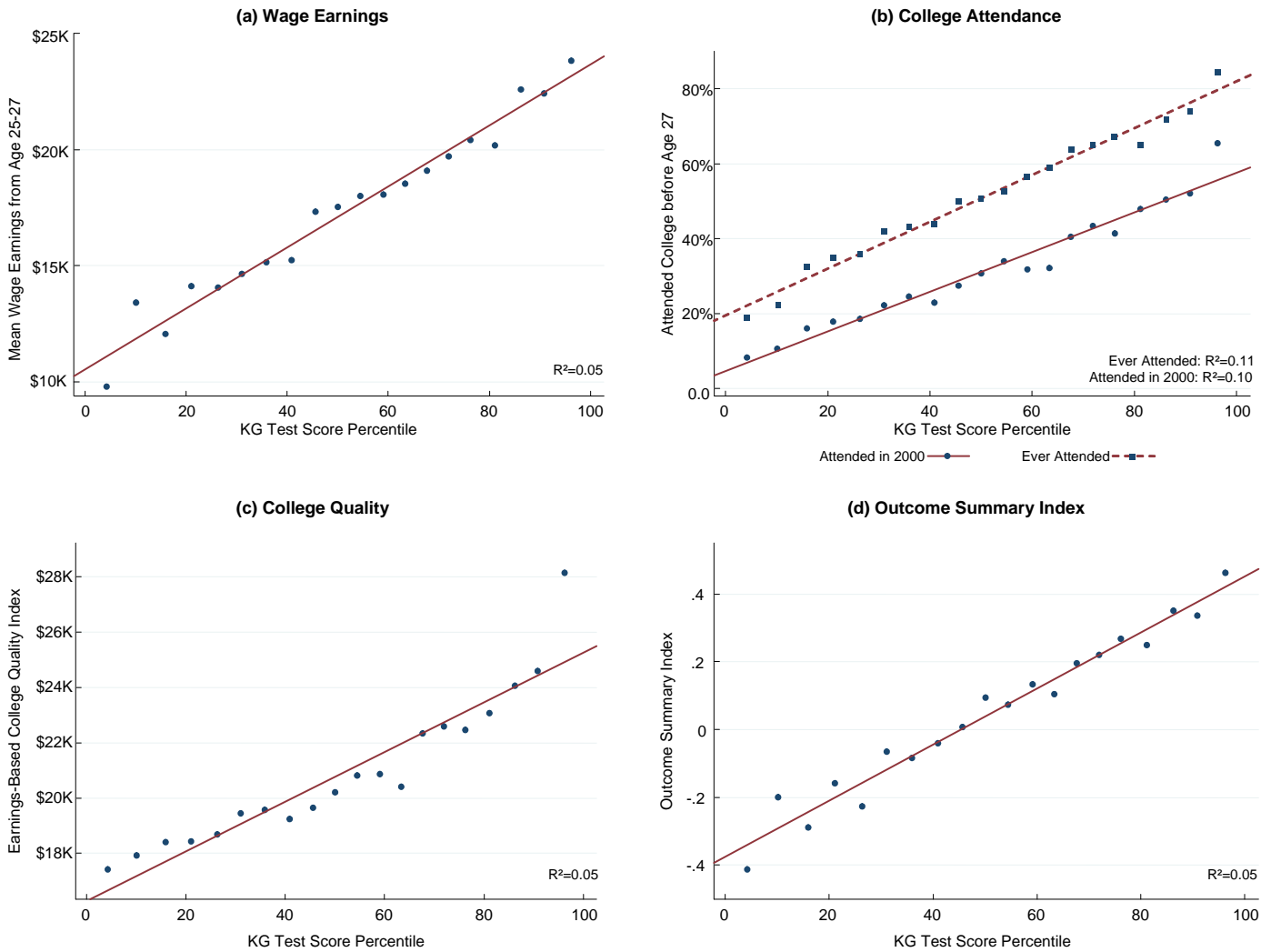
(3) Teachers. We calculate the benefits of improving teacher quality in two ways. The first method uses the estimated earnings gain of \$57 from being assigned to a kindergarten teacher with one year of extra experience, reported in Figure 3b. The standard deviation of teacher experience in our sample is 5.8 years. Hence, a one standard deviation increase in teacher experience raises earnings by \$331 (2.1%) at age 27. This translates into a lifetime earnings gain of \$8,500 in present value, or \$170,000 for a class of twenty students.

The limitation of the preceding calculation is that it is based upon only one observable aspect of teacher quality. To incorporate other aspects of teacher quality, we again develop a prediction based on the impacts of class quality on scores and earnings. Rockoff (2004), Rivkin, Hanushek, and Kain (2005), and Kane and Staiger (2008) use datasets with repeated teacher observations to estimate that a one standard deviation increase in teacher quality raises test scores by approximately 0.2 standard deviations (5.4 percentiles). Under the strong assumption that the ratio of earnings gains to test score gains is the same for changes in teacher quality and class quality more broadly, this translates into an earnings gain of $5.4 \times \$76.7 = \416 (2.6%) at age 27. This implies a present-value earnings gain of \$10,700 per student. A one standard deviation improvement in teacher quality in a single year generates earnings gains of \$214,000 for a class of twenty students.

⁴³This assumption clearly does not hold for all types of interventions. As an extreme example, raising test scores by cheating would be unlikely to yield an earnings gain of \$77 per percentile improvement in test scores. The \$77 per percentile measure should be viewed as a prior estimate of the expected gain when evaluating interventions such as class size or teacher quality for which precise estimates of earnings impacts are not yet available.

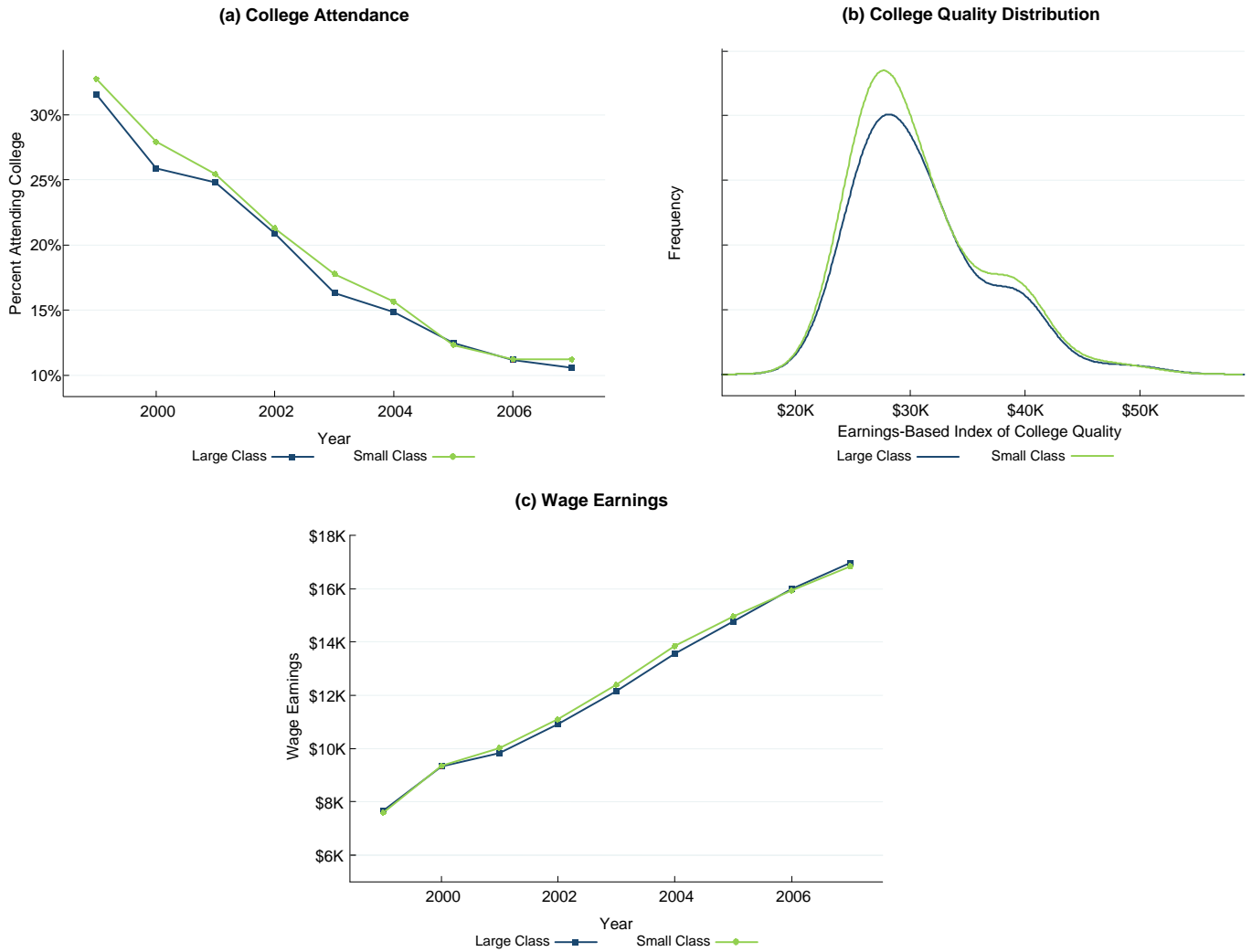
⁴⁴This cost is obtained as follows. The annual cost of school for a child is \$8,848 per year. Small classes had 15.1 students on average, while large classes had 22.56 students on average. The average small class treatment lasted 2.14 years. Hence, the cost per student of reducing class size is $(22.56/15.1-1) \times 2.14 \times 8848 = \$9,355$.

FIGURE 1
Correlation between Kindergarten Test Scores and Adult Outcomes



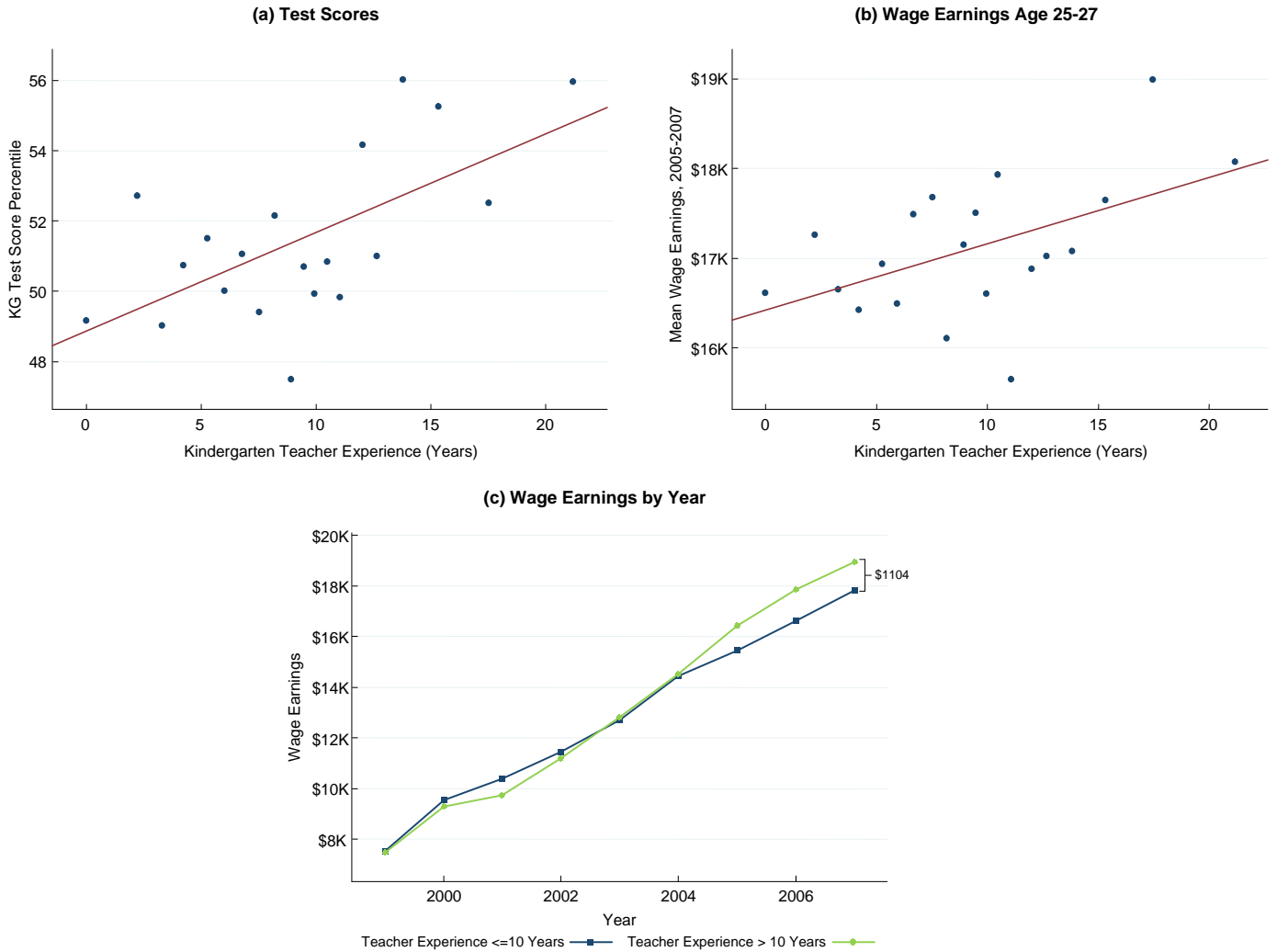
Notes: These figures plot the raw correlations between adult outcomes and kindergarten average test scores in math and reading (measured by within-sample percentile ranks). To construct these figures, we bin test scores into twenty equal sized (5 percentile point) bins and plot the mean of the adult outcome within each bin. The solid or dashed line shows the best linear fit estimated on the underlying student-level data using OLS. The R^2 from this regression, listed in each panel, shows how much of the variance in the outcome is explained by KG test scores. Earnings are mean annual earnings over years 2005-2007, measured by wage earnings on W-2 forms; those with no W-2 earnings are coded as zeros. College attendance is measured by receipt of a 1098-T form, issued by higher education institutions to report tuition payments or scholarships, at some point between 1999 and 2007. The earnings-based index of college quality is a measure of the mean earnings of all former attendees of each college in the U.S. population at age 28, as described in the text. For individuals who did not attend college, college quality is defined by mean earnings at age 28 of those who did not attend college in the U.S. population. The summary index is the standardized sum of five measures, each standardized on its own before the sum: home ownership, 401(k) retirement savings, marital status, cross-state mobility, and percent of college graduates in the individual's 2007 ZIP code of residence. Thus the summary index has mean 0 and standard deviation of 1. All monetary values are expressed in real 2009 dollars.

FIGURE 2
Effects of Class Size



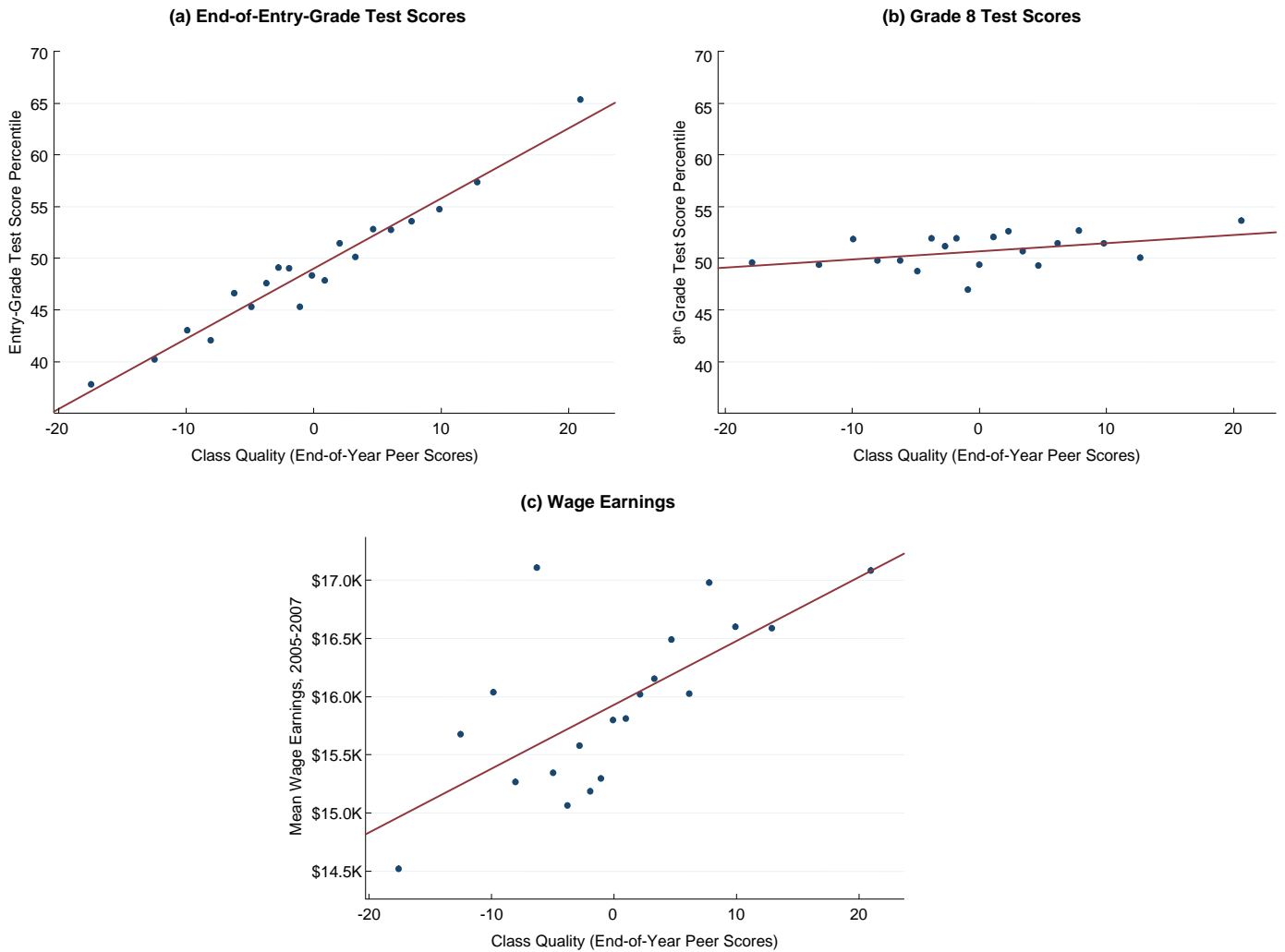
Notes: Panels (a) and (c) show college attendance rates and mean wage earnings by year (from ages 19 to 27) for students randomly assigned to small and large classes. Panel (b) plots the distribution of college quality attended in 2000 using the earnings-based college quality index described in Figure 1. Individuals who did not attend college are included in panel (b) with college quality defined as mean earnings in the U.S. population for those who did not attend college. Kernel-smoothed densities in panel (b) are scaled to integrate to total attendance rates for both small and large classes. All figures adjust for school-by-entry-grade effects to isolate the random variation in class size. In (a) and (c), we adjust for school-by-entry-grade effects by regressing the outcome variable on school-by-entry-grade dummies and the small class indicator in each tax year. We then construct the two series shown in the figure by requiring that the difference between the two lines equals the regression coefficient on the small class indicator in the corresponding year and that the weighted average of the lines equals the sample average in that year. In (b), we compute residual college mean earnings from a regression on school-by-entry-grade effects and plot the distribution of the residual within small and large classes, adding back the sample mean to facilitate interpretation of units. See notes to Figure 1 for definitions of wage earnings and college variables.

FIGURE 3
Effects of Teacher Experience



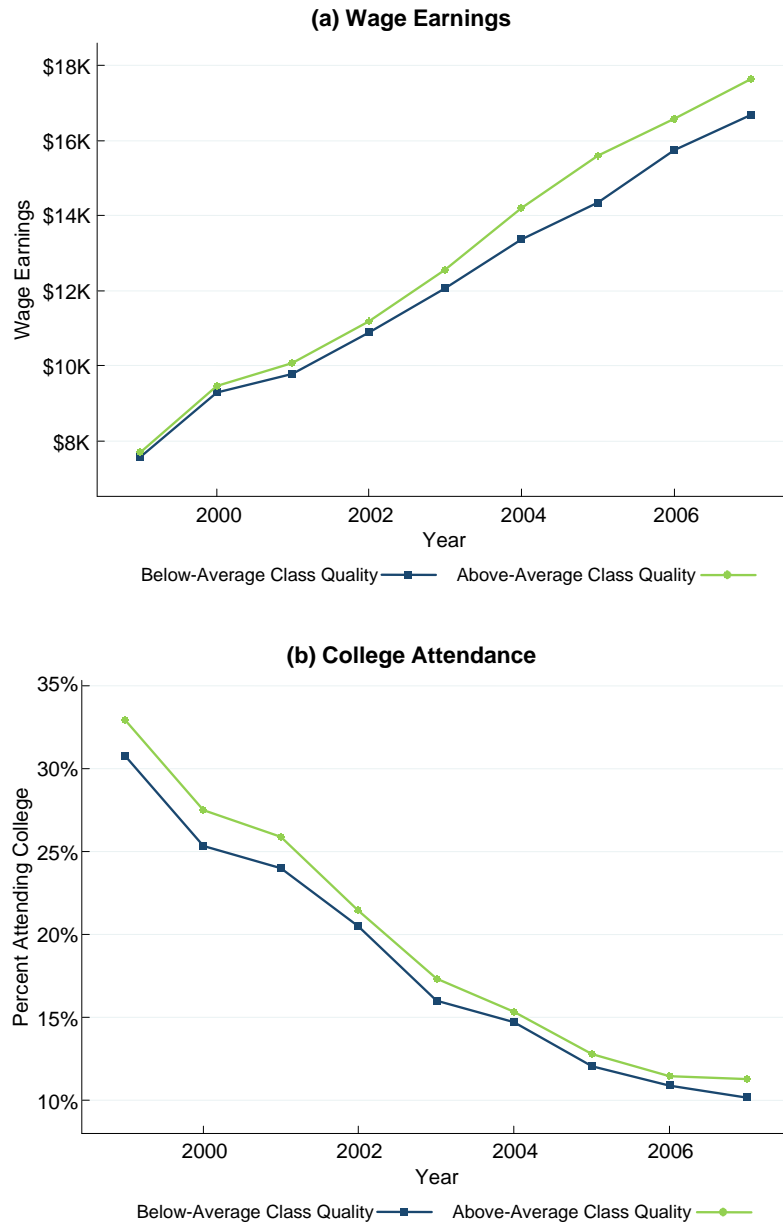
Notes: Panel (a) plots kindergarten average test scores in math and reading (measured by within-sample percentile ranks) vs. kindergarten teacher's years of prior experience. Panel (b) plots mean wage earnings over years 2005-2007 vs. kindergarten teacher's years of prior experience. In both panels (a) and (b), we bin teacher experience into twenty equal sized (5 percentile point) bins and plot the mean of the outcome variable within each bin. The solid line shows the best linear fit estimated on the underlying student-level data using OLS. Panel (c) plots mean wage earnings by year (from ages 19 to 27) for individuals who had a teacher with fewer than 10 or more than 10 years of experience in kindergarten. All figures adjust for school-by-entry-grade effects to isolate the random variation in teacher experience. In (a) and (b), we adjust for school-by-entry-grade effects by regressing both the dependent and independent variables on school-by-entry-grade dummies. We then plot the residuals, adding back the sample means to facilitate interpretation of units. The solid line shows the best linear fit estimated on the underlying data using OLS. In (c), we follow the same procedure used to construct Figure 2c. See notes to Figure 1 for definition of wage earnings.

FIGURE 4
Effects of Class Quality



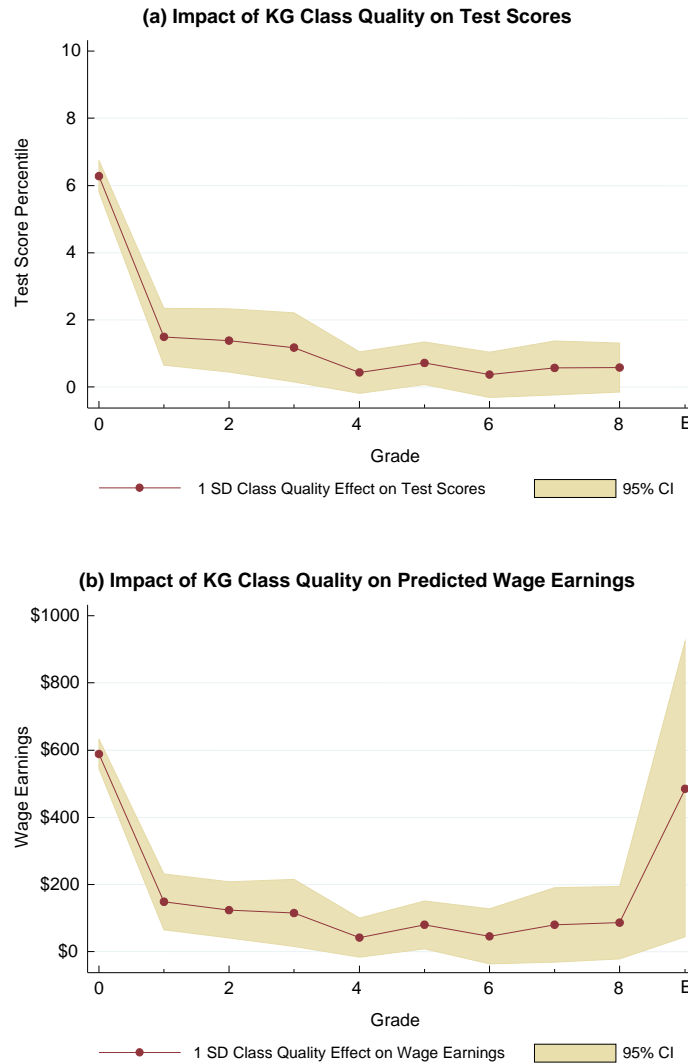
Notes: The x axis in all panels is class quality, defined as the difference between the mean end-of-entry-grade test scores of a student's classmates and (grade-specific) schoolmates. Class quality is defined based on the first, randomly assigned STAR classroom (i.e., KG classroom for KG entrants, 1st grade classroom for 1st grade entrants, etc.). In all panels, we bin class quality into twenty equal sized (5 percentile point) bins and plot the mean of the outcome variable within each bin. The solid line shows the best linear fit estimated on the underlying student-level data using OLS. The dependent variable in panel (a) is the student's own test score at the end of the grade in which he entered STAR. The coefficient of end-of-entry-grade test scores on class quality is 0.68 (s.e. = 0.03), implying that a 1 percentile improvement in class quality is associated with a 0.68 percentile improvement in test scores. The dependent variable in panel (b) is a student's test score at the end of 8th grade. The coefficient of 8th grade test scores on class quality is 0.08 (s.e. = 0.03). The dependent variable in panel (c) is a student's mean wage earnings over years 2005-2007. The coefficient of wage earnings on class quality is \$57.6 (s.e. = \$16.2), implying that a 1 percentile improvement in class quality leads to a \$57.6 increase in a student's annual earnings. All panels adjust for school-by-entry-grade effects to isolate the random variation in class quality using the technique in Figure 3a. See notes to Figure 1 for definition of wage earnings.

FIGURE 5
Effects of Class Quality by Year



Notes: These figures show college attendance rates and mean wage earnings by year (from ages 19 to 27) for students in two groups of classes: those that were above the class quality median and those that were below. Class quality is defined as the difference between the mean end-of-entry-grade test scores of a student's classmates and (grade-specific) schoolmates. Class quality is defined based on the first, randomly assigned STAR classroom (i.e., KG classroom for KG entrants, 1st grade classroom for 1st grade entrants, etc.). Both panels adjust for school-by-entry-grade effects to isolate the random variation in class quality using the procedure in Figure 2c. See notes to Figure 1 for definitions of wage earnings and college attendance.

FIGURE 6
Fade-out and Re-Emergence of Class Effects



Notes: Panel (a) shows the impact of a 1 standard deviation improvement in class quality in kindergarten on test scores from kindergarten through grade 8, estimated using specifications analogous to column 1 of Table 8. Class quality is defined as the difference between the mean end-of-kindergarten test scores of a student’s classmates and (grade-specific) schoolmates. Panel (b) shows the effect of a 1 standard deviation improvement in KG class quality on predicted earnings. To construct this figure, we first run separate cross-sectional regressions of earnings on test scores in each grade (see column 1 of Appendix Table 5). We then multiply these OLS coefficients by the corresponding estimated impacts of a 1 SD improvement in KG class quality on test scores in each grade shown in Panel (a). The last point in panel (b) shows the actual earnings impact of a 1 SD improvement in KG class quality, estimated using a specification analogous to column 4 of Table 8. All regressions used to construct these figures are run on the sample of KG entrants and control for school fixed effects and the student and parent demographic characteristics used in Table 8: a quartic in parent’s household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother’s age at child’s birth, indicators for parent’s 401(k) savings and home ownership, student’s race, gender, free lunch status, and age at kindergarten, and indicators for missing variables. See notes to Figure 1 for definition of wage earnings.

TABLE 1
Summary Statistics

Variable	STAR Sample		U.S. 1979-80 cohort	
	Mean	Standard Deviation	Mean	Standard Deviation
	(1)	(2)	(3)	(4)
Adult Outcomes:				
Average wage earnings (2005-2007)	\$15,912	\$15,558	\$20,500	\$19,541
Zero wage earnings (2005-2007)	13.9%	34.5%	15.6%	36.3%
Attended college in 2000 (age 20)	26.4%	44.1%	34.7%	47.6%
Attended college by age 27	45.5%	49.8%	57.1%	49.5%
Owned a house by age 27	30.8%	46.2%	28.4%	45.1%
Made 401 (k) contribution by age 27	28.2%	45.0%	31.0%	46.2%
Married by age 27	43.2%	49.5%	39.8%	48.9%
Moved out of TN by age 27	27.5%	44.7%		
Percent college graduates in 2007 ZIP code	17.6%	11.7%	24.2%	15.1%
Deceased before 2010	1.70%	12.9%	1.02%	10.1%
Parent Characteristics:				
Average household income (1996-98)	\$48,010	\$41,622	\$65,660	\$53,844
Mother's age at child's birth	24.98	6.53	26.29	6.17
Married between 1996 and 2008	64.8%	47.8%	75.7%	42.9%
Owned a house between 1996 and 2008	64.5%	47.8%	53.7%	49.9%
Made a 401(k) contribution between 1996 and 2008	45.9%	49.8%	50.5%	50.0%
Missing (no parent found)	13.9%	34.6%	23.9%	42.6%
Student Background Variables:				
Female	47.2%	49.9%	48.7%	50.0%
Black	35.9%	48.0%		
Eligible for free or reduced-price lunch	60.3%	48.9%		
Age at kindergarten entry	5.65	0.56		
Teacher Characteristics (Entry-Grade)				
Experience (years)	10.8	7.7		
Post-BA degree	35.9%	48.0%		
Black	19.5%	39.6%		
Number of Observations	10,992		22,568	

Notes: Adult outcomes, parent characteristics, and student age at KG entry are from 1996-2008 tax data; other student background variables and teacher characteristics are from STAR database. Columns 1 and 2 are based on the sample of STAR students who were successfully linked to U.S. tax data. Columns 3 and 4 are based on a 0.25% random sample of the U.S. population born in the same years as the STAR cohort (1979-80). All available variables are defined identically in the STAR and U.S. samples. Earnings are average individual earnings in years 2005-2007, measured by wage earnings on W-2 forms; those with no W-2 earnings are coded as zeros. College attendance is measured by receipt of a 1098-T form, issued by higher education institutions to report tuition payments or scholarships. Home ownership is measured as those who report mortgage interest payments on a 1040 or 1098 tax form. 401(k) contributions are reported on W-2 forms. Marital status is measured by whether an individual files a joint tax return. State and ZIP code of residence are taken from the most recent 1040 form or W-2 form. Percent college graduates in the student's 2007 ZIP code is based on data on percent college graduates by ZIP code from the 2000 Census. Birth and death information are as recorded by the Social Security Administration. We link STAR participants to their parents by finding the earliest 1040 form in years 1996-2008 on which the STAR student is claimed as a dependent. We are unable to link 13.9% of the STAR children (and 23.9% of the U.S. cohort) to their parents; the summary statistics reported for parents exclude these observations. Parent household income is average adjusted gross income (AGI) in years 1996-1998, when STAR participants are aged 16-18. For years in which parents did not file, household income is defined as zero. For joint-filing parents, mother's age at child's birth uses the birth date of the female parent; for single-filing parents, the variable uses the birth date of the single parent, who is usually female. Other parent variables are defined in the same manner as student variables. Free or reduced-price lunch eligibility is an indicator for whether the student was ever eligible during the experiment. Student's age at kindergarten entry is defined as age (in days, divided by 365.25) on Sep. 1, 1985. Teacher experience is the number of years taught at any school before the student's year of entry into a STAR school. All monetary values are expressed in real 2009 dollars.

TABLE 2
Randomization Tests

Dependent Variable:	Wage Earnings	Small Class	Teacher Experience	Teacher Has Post BA Deg.	Teacher is Black	
	(%)	(%)	(Years)	(%)	(%)	p value
	(1)	(2)	(3)	(4)	(5)	(6)
Parent's income (\$1000s)	65.47 (6.634) [9.87]	-0.003 (0.015) [-0.231]	-0.001 (0.002) [-0.509]	0.016 (0.012) [1.265]	-0.003 (0.007) [-0.494]	0.848
Mother's age at STAR birth	53.96 (24.95) [2.162]	0.029 (0.076) [0.384]	0.022 (0.012) [1.863]	0.008 (0.061) [0.132]	0.060 (0.050) [1.191]	0.654
Parents have 401 (k)	2273 (348.3) [6.526]	1.455 (1.063) [1.368]	0.111 (0.146) [0.761]	0.431 (0.917) [0.469]	-1.398 (0.736) [-1.901]	0.501
Parents own home	390.9 (308.1) [1.269]	-0.007 (0.946) [-0.008]	-0.023 (0.159) [-0.144]	-2.817 (0.933) [-3.018]	0.347 (0.598) [0.58]	0.435
Parents married	968.3 (384.2) [2.52]	0.803 (1.077) [0.746]	0.166 (0.165) [1.008]	-0.306 (1.101) [-0.277]	-0.120 (0.852) [-0.14]	0.820
Student female	-2317 (425.0) [-5.451]	-0.226 (0.864) [-0.261]	0.236 (0.111) [2.129]	-0.057 (0.782) [-0.072]	-0.523 (0.521) [-1.003]	0.502
Student black	-620.8 (492.0) [-1.262]	0.204 (1.449) [0.141]	0.432 (0.207) [2.089]	2.477 (1.698) [1.459]	1.922 (1.075) [1.788]	0.995
Student free-lunch	-3829 (346.2) [-11.06]	-0.291 (1.110) [-0.262]	0.051 (0.149) [0.344]	-0.116 (0.969) [-0.12]	-0.461 (0.648) [-0.712]	0.350
Student's age at KG entry	-2001 (281.4) [-7.109]	-0.828 (0.885) [-0.935]	-0.034 (0.131) [-0.257]	0.140 (0.738) [0.19]	-0.364 (0.633) [-0.575]	0.567
Student predicted earnings						0.916
p value of F test	0.000	0.261	0.190	0.258	0.133	
Observations	10,992	10,992	10,914	10,938	10,916	

Notes: Columns 1-5 each report estimates from an OLS regression of the dependent variable listed in the column on the variables listed in the rows and school-by-entry-grade fixed effects. The regressions include one observation per student, pooling across all entry grades. Standard errors clustered by school are reported in parentheses and t-statistics in square brackets. Small class is an indicator for assignment to a small class upon entry. Teacher characteristics are for teachers in the entry-grade. Independent variables are pre-determined parent and student characteristics. See notes to Table 1 for definitions of these variables. The p value reported at bottom of columns 1-5 is for an F test of the joint significance of the variables listed in the rows. Each row of column 6 reports a p value from a separate OLS regression of the pre-determined variable listed in the corresponding row on school and class fixed effects (omitting one class per school). The p value is for an F test of the joint significance of the class fixed effects. The F tests in column 6 use the subsample of students who entered in kindergarten. Student predicted earnings is formed using the specification in column 1, excluding the school-by-entry-grade fixed effects. Some observations have missing data on parent characteristics, free-lunch status, race, or mother's age at STAR birth. Columns 1-5 include these observations along with four indicators for missing data on these variables. In column 6, observations with missing data are excluded from the regressions with the corresponding dependent variables.

TABLE 3
Tests for Differential Match and Death Rates

Dependent Variable:	Matched		Deceased	
	(%)	(%)	(%)	(%)
	(1)	(2)	(3)	(4)
Small class	-0.019 (0.467)	0.079 (0.407)	-0.010 (0.286)	-0.006 (0.286)
p value on F test on class effects	0.951	0.888	0.388	0.382
Demographic controls		x		x
Mean of dep. var.	95.0	95.0	1.70	1.70

Notes: The first row of each column reports coefficients from OLS regressions on a small class indicator and school-by-entry-grade fixed effects, with standard errors clustered by school in parentheses. The second row reports a p value from a separate OLS regression of the dependent variable on school and class fixed effects (omitting one class per school). The p value is for an F test of the joint significance of the class fixed effects. Matched is an indicator for whether the STAR student was located in the tax data using the algorithm described in Appendix A. Deceased is an indicator for whether the student died before 2010 as recorded by the Social Security Administration. Columns 1-2 are estimated on the full sample of students in the STAR database; columns 3 and 4 are estimated on the sample of STAR students linked to the tax data. Specifications 2 and 4 control for the following demographic characteristics: student gender, free-lunch status, age, and race, and a quartic in the claiming parent's household income interacted with parent's marital status, mother's age at child's birth, whether the parents own a home, and whether the parents make a 401 (k) contribution between 1996 and 2008. Some observations have missing data on parent characteristics, free-lunch status, race, and mother's age at STAR birth; these observations are included along with four indicators for missing data on these variables.

TABLE 4
Cross-Sectional Correlation between Test Scores and Adult Outcomes

Dependent Variable:	Wage Earnings					College in 2000	College by Age 27	College Quality	Summary Index
	(\$)	(\$)	(\$)	(\$)	(\$)	(%)	(%)	(%)	(% SD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Entry-grade test percentile	131.7 (12.24)	93.79 (11.63)	90.04 (8.65)	0.102 (12.87)	97.7 (8.47)	0.364 (0.022)	0.510 (0.021)	32.04 (3.40)	0.551 (0.048)
8th grade test percentile				148.2 (11.95)					
Parental income percentile					145.5 (8.15)				
Entry grade	KG	KG	All	All	All	All	All	All	All
Class fixed effects		x	x	x	x	x	x	x	x
Student controls		x	x	x	x	x	x	x	x
Parent controls		x	x	x		x	x	x	x
Adjusted R ²	0.05	0.17	0.17	0.17	0.16	0.26	0.28	0.19	0.23
Observations	5,621	5,621	9,939	7,069	9,939	9,939	9,939	9,939	9,939

Notes: Each column reports coefficients from an OLS regression, with standard errors clustered by school in parentheses. In columns 1-2, the sample includes only kindergarten entrants; in columns 3-9, the sample includes all entry grades. Test percentile is the within-sample percentile rank of the student's average score in math and reading. Entry grade is the grade (kindergarten, 1, 2, or 3) when the student entered a STAR school. Entry-grade test percentile refers to the test score from the end of the student's first year at a STAR school. Grade 8 scores are available for students who remained in Tennessee public schools and took the 8th grade standardized test any time between 1990 and 1997. Parental income percentile is the parent's percentile rank in the U.S. population household income distribution. Columns with class fixed effects isolate non-experimental variation in test scores. Columns 2-9 all control for the following student characteristics: race, gender, and age at kindergarten. Parent controls comprise the following: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's free lunch status. The dependent variable in columns 1-5 is mean wage earnings over years 2005-2007 (including zeros for people with no wage earnings). College attendance is measured by receipt of a 1098-T form, issued by higher education institutions to report tuition payments or scholarships. The earnings-based index of college quality is a measure of the mean earnings of all former attendees of each college in the U.S. population at age 28, as described in the text. For individuals who did not attend college, college quality is defined by mean earnings at age 28 of those who did not attend college in the U.S. population. Summary index is the standardized sum of five measures, each standardized on its own before the sum: home ownership, 401(k) retirement savings, marital status, cross-state mobility, and percent of college graduates in the individual's 2007 ZIP code of residence.

TABLE 5
Effects of Class Size on Adult Outcomes

Dependent Variable:	Test Score	College in 2000	College by Age 27	College Quality	Wage Earnings	Summary Index
	(%)	(%)	(%)	(\$)	(\$)	(% of SD)
	(1)	(2)	(3)	(4)	(5)	(6)
Small class (no controls)	4.81 (1.05)	2.02 (1.10)	1.91 (1.19)	119 (96.8)	4.09 (327)	5.06 (2.16)
Small class (with controls)	4.76 (0.99)	1.78 (0.95)	1.57 (1.07)	109 (92.6)	-124 (336)	4.61 (2.09)
Observations	9,939	10,992	10,992	10,992	10,992	10,992
Mean of dep. var.	48.67	26.44	45.50	27,115	15,912	0.00

Notes: Each column reports the coefficient on an indicator for initial small class assignment from two separate OLS regressions, with standard errors clustered by school in parentheses. All specifications include school-by-entry-grade fixed effects to isolate random variation in class assignment. The estimates in the second row (with controls) are from specifications that additionally control for the full vector of demographic characteristics used first in Table 4: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's race, gender, free lunch status, and age at kindergarten. College attendance is measured by receipt of a 1098-T form, issued by higher education institutions to report tuition payments or scholarships. The earnings-based index of college quality is a measure of the mean earnings of all former attendees of each college in the U.S. population at age 28, as described in the text. For individuals who did not attend college, college quality is defined by mean earnings at age 28 of those who did not attend college in the U.S. population. Summary index is the standardized sum of five measures, each standardized on its own before the sum: home ownership, 401(k) retirement savings, marital status, cross-state mobility, and percent of college graduates in the individual's 2007 ZIP code of residence.

TABLE 6
Observable Teacher and Peer Effects

Dependent Variable:	Test Score	Wage Earnings	Test Score	Wage Earnings	Test Score	Wage Earnings	
	(%)	(\$)	(%)	(\$)	(%)	(\$)	(\$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Teacher with >10 years of experience	3.18 (1.26)	1093 (545.5)	1.61 (1.21)	-536.1 (619.3)			
Teacher has post-BA deg.	-0.848 (1.15)	-261.1 (449.4)	0.95 (0.90)	-359.4 (500.1)			
Fraction black classmates					-6.97 (9.92)	-1,757 (2692)	
Fraction female classmates					9.74 (4.26)	-67.53 (1539)	
Fraction free-lunch classmates					-7.53 (4.40)	-284.6 (1731)	
Classmates' mean age					-3.24 (3.33)	-25.78 (1359)	
Classmates' mean predicted score							-23.27 (93.69)
Small class	5.19 (1.19)	-8.158 (448.4)	3.77 (1.17)	-284.2 (536.4)	4.63 (0.99)	-132.2 (342.3)	-119.7 (331.4)
Entry grade	KG	KG	Grade ≥1	Grade ≥1	All	All	All
Observations	5,601	6,005	4,270	4,909	9,939	10,992	10,992

Notes: Each column reports coefficients from an OLS regression, with standard errors clustered by school in parentheses. All specifications control for school-by-entry-grade fixed effects, an indicator for initial assignment to a small class, and the vector of demographic characteristics used first in Table 4: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's race, gender, free lunch status, and age at kindergarten. Columns 1-2 include only students who entered a STAR school in kindergarten. Columns 3-4 include only students who enter STAR after kindergarten. Columns 5-7 pool all students, regardless of their entry grade. Test score is the average math and reading test score at the end of the year in which the student enters a STAR school (measured in percentiles). Wage earnings is the individual's mean wage earnings over years 2005-2007 (including zeros for people with no wage earnings). Teacher experience is the number of years the teacher taught at any school before the student's year of entry into a STAR school. Classmates' characteristics are defined based on the classroom that the student enters in the first year he is in a STAR school and omit the own student. Classmates' mean predicted score is constructed by regressing test scores on school-by-entry-grade fixed effects and the demographic characteristics listed above and then taking the mean of the predicted scores. Variables labeled "fraction" are in units between zero and one. Free-lunch status is a proxy for having low household income.

TABLE 7
Kindergarten Class Effects: Analysis of Variance

Dependent Variable:	Grade	Grade	Wage Earnings			
	K Scores	8 Scores	(3)	(4)	(5)	(6)
	(1)	(2)				
p value of F test on KG class fixed effects	0.000	0.419	0.047	0.026	0.020	0.042
p value from permutation test	0.000	0.355	0.054	0.029	0.023	0.056
SD of class effects (RE estimate)	8.77%	0.000%	\$1,497	\$1,520	\$1,703	\$1,454
Demographic controls	x	x		x	x	x
Large classes only					x	
Observable class chars.						x
Observations	5,621	4,448	6,025	6,025	4,208	5,983

Notes: Each column reports estimates from an OLS regression of the dependent variable on school and class fixed effects, omitting one class fixed effect per school. The p value in the first row is for an F test of the joint significance of the class fixed effects. The second row reports the p value from a permutation test, calculated as follows: we randomly permute students between classes within each school, calculate the F statistic on the class dummies, repeat the previous two steps 1000 times, and locate the true F statistic in this distribution. The third row reports the estimated standard deviation of class effects from a model with random class effects and school fixed effects. Grade 8 scores are available for students who remained in Tennessee public schools and took the 8th grade standardized test any time between 1990 and 1997. Both KG and 8th grade scores are coded using within-sample percentile ranks. Wage earnings is the individual's mean wage earnings over years 2005-2007 (including zeros for people with no wage earnings). All specifications are estimated on the subsample of students who entered a STAR school in kindergarten. All specifications except 3 control for the vector of demographic characteristics used in Table 4: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's race, gender, free lunch status, and age at kindergarten. Column 5 limits the sample to large classes only; this column identifies pure KG class effects because students who were in large classes were re-randomized into different classes after KG. Column 6 replicates column 4, adding controls for the following observable classroom characteristics: indicators for small class, above-median teacher experience, black teacher, and teacher with degree higher than a BA, and classmates' mean predicted score. Classmates' mean predicted score is constructed by regressing test scores on school-by-entry-grade fixed effects and the vector of demographic characteristics listed above and then taking the mean of the predicted scores.

TABLE 8
Effects of Class Quality on Wage Earnings

Dependent Variable:	Test Score	Wage Earnings				College in 2000	College by Age 27	College Quality	Summary Index
	(%)	(\$)	(\$)	(\$)	(\$)	(%)	(%)	(\$)	(% of SD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Class Quality (peer scores)	0.662 (0.024)	50.61 (17.45)	61.31 (20.21)	53.44 (24.84)	47.70 (18.63)	0.096 (0.046)	0.108 (0.053)	9.328 (4.573)	0.250 (0.098)
Entry grade	All	All	All	KG	Grade ≥1	All	All	All	All
Observable class chars.			x						
Observations	9,939	10,959	10,859	6,025	4,934	10,959	10,959	10,959	10,959

Notes: Each column reports coefficients from an OLS regression, with standard errors clustered by school in parentheses. Class quality is measured as the difference (in percentiles) between mean end-of-year test scores of the student's classmates and (grade-specific) schoolmates. Class quality is defined based on the first, randomly assigned STAR classroom (i.e. KG class for KG entrants, 1st grade class for 1st grade entrants, etc.). All specifications control for school-by-entry-grade fixed effects and the vector of demographic characteristics used first in Table 4: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's race, gender, free-lunch status, and age at kindergarten. Column 3 includes controls for observable classroom characteristics as in Column 6 of Table 7. Column 4 restricts the sample to kindergarten entrants; column 5 includes only those who enter in grades 1-3. Test score is the average math and reading test score at the end of the year in which the student enters STAR (measured in percentiles). Wage earnings is the individual's mean wage earnings over years 2005-2007 (including zeros for people with no wage earnings). College attendance is measured by receipt of a 1098-T form, issued by higher education institutions to report tuition payments or scholarships. The earnings-based index of college quality is a measure of the mean earnings of all former attendees of each college in the U.S. population at age 28, as described in the text. For individuals who did not attend college, college quality is defined by mean earnings at age 28 of those who did not attend college in the U.S. population. Summary index is the standardized sum of five measures, each standardized on its own before the sum: home ownership, 401(k) retirement savings, marital status, cross-state mobility, and percent of college graduates in the individual's 2007 ZIP code of residence.

TABLE 9
Effects of KG Class Quality on Non-Cognitive Skills

Dependent Variable:	Wage Earnings		Grade 8	Grade 4		Grade 8			
			Math + Reading	Math + Reading	Non-Cog	Math + Reading	Non-Cog	Math + Reading	Non-Cog
	(\$)	(\$)	(%)	(%)	(%)	(%)	(%)	(%)	(%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Grade 4 non-cog. score	106 (16.0)	87.7 (20.4)	0.059 (0.017)						
Grade 4 math + reading score		36.4 (24.7)	0.671 (0.023)						
Class quality (peer scores)				0.047 (0.035)	0.153 (0.065)	0.064 (0.041)	0.128 (0.054)		
Teacher with >10 years experience								0.292 (0.878)	2.60 (1.41)
Observations	1,671	1,360	1,254	4,023	1,671	4,448	1,780	4,432	1,772

Notes: Each column reports coefficients from an OLS regression, with standard errors clustered by school in parentheses. All specifications include only the subsample of students who entered a STAR school in kindergarten, and control for school fixed effects and the vector of demographic characteristics used first in Table 4: a quartic in parent's household income interacted with an indicator for whether the filing parent is ever married between 1996 and 2008, mother's age at child's birth, indicators for parent's 401(k) savings and home ownership, and student's race, gender, free lunch status, and age at kindergarten. Wage earnings is the individual's mean wage earnings over years 2005-2007 (including zeros for people with no wage earnings). Grades 4 and 8 non-cognitive scores are based on teacher surveys of student behavior across four areas: effort, initiative, engagement in class, and whether the student values school. We average the four component scores and convert them into within-sample percentile ranks. Math + reading scores are average math and reading test scores (measured in percentiles) at the end of the relevant year. Class quality is measured as the difference (in percentiles) between mean end-of-year test scores of the student's classmates and (grade-specific) schoolmates in kindergarten. Teacher experience is the number of years the KG teacher taught at any school before the student's year of entry into a STAR school.

APPENDIX TABLE 1
Correlations of Earnings Over the Life Cycle

Age	Correlation between Wage Earnings at Age x and x+6
18	0.36
19	0.36
20	0.37
21	0.41
22	0.47
23	0.55
24	0.60
25	0.62
26	0.65
27	0.67
28	0.69
29	0.70
30	0.71
31	0.72
32	0.74
33	0.75
34	0.75
35	0.77
36	0.77
37	0.78
38	0.79
39	0.79
40	0.80
41	0.80
42	0.81
43	0.81
44	0.81
45	0.81
46	0.80
47	0.80
48	0.80
49	0.79
50	0.78

Notes: This table presents correlations between individual mean wage earnings 1999-2001 and individual mean wage earnings 2005-2007 (including zeros for people with no wage earnings) for different ages in a 3% random sample of the US population. Age is defined as age on December 31, 2000. Individuals with mean wage earnings greater than \$200,000 over years 1999-2001 or 2005-2007 are omitted. The earnings outcome most commonly used in the tables is STAR subject mean wage earnings 2005-2007. The typical STAR subject was 26 on December 31, 2006. The row in bold implies that STAR subjects' mean wage earnings 2005-2007 are predicted to correlate with their mean wage earnings 2011-2013 (when STAR subjects are approximately aged 31-33) with a coefficient of 0.65.

APPENDIX TABLE 2
Randomization Tests by Entry Grade

	p-value				
	Entry Grade:	Grade K	Grade 1	Grade 2	Grade 3
	(1)	(2)	(3)	(4)	(5)
Parent's income (\$1000s)	0.848	0.081	0.127	0.117	0.412
Mother's age at STAR birth	0.654	0.082	0.874	0.555	0.165
Parents have 401 (k)	0.501	0.427	0.634	0.567	0.957
Parents married	0.820	0.921	0.981	0.280	0.116
Parents own home	0.435	0.075	0.158	0.879	0.929
Student black	0.995	1.000	0.939	0.997	0.040
Student free-lunch	0.350	0.060	0.159	0.798	0.469
Student's age at KG entry	0.567	0.008	0.251	0.972	0.304
Student female	0.502	0.413	0.625	0.069	0.052
Predicted earnings	0.916	0.674	0.035	0.280	0.645

Notes: Each cell in columns 1-4 reports the p value on an F test for joint significance of classroom fixed effects in a separate OLS regression. The row indicates the dependent variable. The column indicates the sample of entrants used. Each regression includes school fixed effects, so one classroom fixed effect per school is omitted. In column 5, we pool all entry grades by regressing a student's own characteristic on the difference between his classmates' and grade-specific schoolmates' mean values of that characteristic. Each row of Column 6 reports p values from a separate regression that includes school-by-entry-grade fixed effects. The p values are from a t test for the significance of the coefficient on peer characteristics, i.e. a test for significant intra-class correlation in the variable listed in each row.

APPENDIX TABLE 3
Correlation Between Test Scores and Components of Summary Index

Dependent Variable:	Home Owner	Have 401 K	Married	Moved Out of State	College Grads in 2007 Zip
	(%)	(%)	(%)	(%)	(%)
<i>A. All Entrants</i>	(1)	(2)	(3)	(4)	(5)
Entry-grade test percentile	0.159 (0.022)	0.109 (0.025)	0.057 (0.025)	0.179 (0.025)	0.053 (0.006)
Observations	9,939	9,939	9,939	9,301	9,424
<i>B. KG Entrants</i>					
KG test percentile	0.136 (0.028)	0.100 (0.035)	0.048 (0.035)	0.145 (0.021)	0.053 (0.007)
Observations	5,621	5,621	5,621	5,354	5,367
<i>C. 1st Grade Entrants</i>					
1st grade test percentile	0.205 (0.050)	0.113 (0.047)	0.076 (0.050)	0.282 (0.053)	0.046 (0.012)
Observations	2,124	2,124	2,124	1,934	2,002
<i>D. 2nd Grade Entrants</i>					
2nd grade test percentile	0.089 (0.072)	0.072 (0.080)	0.080 (0.082)	0.226 (0.105)	0.059 (0.024)
Observations	1,215	1,215	1,215	1,112	1,147
<i>E. 3rd Grade Entrants</i>					
3rd grade test percentile	0.231 (0.105)	0.196 (0.085)	0.022 (0.101)	0.070 (0.098)	0.056 (0.021)
Observations	979	979	979	901	908

Notes: This table replicates the specification of column 9 of Table 4 for various subgroups and the five constituent components of summary index. Each row specifies the sample restriction according to the year that the student entered a STAR school. Each column specifies which of the five components of the summary index is used as the dependent variable. See Table 1 for definitions of each outcome variable. See notes to Table 4 for the regression specification. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 4
Correlation between Test Scores and Adult Outcomes by Entry Grade

Dependent Variable:	Wage Earnings				College	College	College	Summary
	(\$)	(\$)	(\$)	(\$)	in 2000	by Age 27	Quality	Index
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. KG Entrants								
KG test percentile	131.7 (12.24)	93.79 (11.63)	-8.529 (15.30)	105.5 (11.39)	0.398 (0.031)	0.527 (0.029)	36.21 (4.08)	0.492 (0.058)
8th grade test percentile			156.4 (12.33)					
Parental income percentile				157.7 (9.57)				
B. 1st Grade Entrants								
1st grade test percentile	134.8 (15.09)	80.38 (14.81)	12.70 (23.82)	82.34 (14.81)	0.292 (0.041)	0.449 (0.047)	17.61 (4.71)	0.654 (0.096)
8th grade test percentile			124.8 (27.92)					
Parental income percentile				136.7 (18.15)				
C. 2nd Grade Entrants								
2nd grade test percentile	150.3 (19.18)	67.29 (25.56)	-42.16 (43.88)	65.70 (25.23)	0.308 (0.064)	0.459 (0.076)	42.02 (11.59)	0.568 (0.153)
8th grade test percentile			183.7 (47.40)					
Parental income percentile				112.9 (22.97)				
D. 3rd Grade Entrants								
3rd grade test percentile	146.2 (19.80)	99.03 (31.34)	87.60 (50.63)	102.2 (30.00)	0.347 (0.070)	0.534 (0.088)	28.09 (7.48)	0.589 (0.183)
8th grade test percentile			58.54 (56.81)					
Parental income percentile				99.08 (26.84)				
Class fixed effects		x	x	x	x	x	x	x
Demographic controls		x	x		x	x	x	x

Notes: This table replicates the specifications of columns 1 and 3-9 of Table 4 for four subgroups, one for each year of entering students. See notes to that table for definitions of variables and regression specifications. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 5
Correlation Between Test Scores and Earnings by Grade

Dependent Variable:	Wage Earnings (\$)	
	(1)	(2)
Grade K scores	93.79 (11.63)	39.46 (30.08)
Grade 1 scores	98.40 (11.83)	71.21 (23.53)
Grade 2 scores	88.72 (14.15)	83.32 (24.66)
Grade 3 scores	97.43 (12.78)	103.5 (20.08)
Grade 4 scores	94.71 (12.40)	91.40 (20.69)
Grade 5 scores	110.8 (10.81)	113.2 (19.24)
Grade 6 scores	121.6 (11.37)	139.5 (21.53)
Grade 7 scores	138.2 (11.53)	158.7 (21.82)
Grade 8 scores	148.9 (11.11)	155.2 (21.47)
Sample	All KG Entrants	Constant Sample of KG Entrants

Notes: Each row of this table reports the coefficient on test score from a separate regression that replicates column 2 of Table 4, replacing KG test score with the test score from the listed grade. The STAR data do not contain test scores for all students in all grades. Regressions underlying column 1 use all students who entered a STAR school in kindergarten and who have a test score for the listed grade. Regressions underlying column 2 use only these KG entrants who have a test score for every grade. The coefficients in Column 1 are used to construct Figure 6b. See notes to Table 4 for other variable definitions and the regression specification. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 6
Correlation between Test Scores and Adult Outcomes: Heterogeneity Analysis

Dependent Variable:	Wage Earnings	College in 2000	College by Age 27	College Quality	Summary Index
	(\$)	(%)	(%)	(\$)	(% of SD)
	(1)	(2)	(3)	(4)	(5)
Blacks	105.8 (14.06)	0.300 (0.044)	0.526 (0.036)	27.39 (4.89)	0.553 (0.091)
Whites	83.06 (12.24)	0.392 (0.028)	0.504 (0.030)	34.21 (4.19)	0.537 (0.061)
Males	77.16 (11.48)	0.323 (0.034)	0.480 (0.034)	28.17 (3.87)	0.415 (0.074)
Females	114.6 (12.44)	0.404 (0.040)	0.539 (0.041)	35.72 (4.98)	0.663 (0.086)
Free-lunch eligible	87.28 (9.06)	0.255 (0.031)	0.429 (0.032)	17.13 (2.78)	0.513 (0.064)
Not elig. for free lunch	94.70 (20.01)	0.544 (0.041)	0.618 (0.041)	58.02 (6.70)	0.631 (0.091)

Notes: This table replicates selected specifications of Table 4 for various subgroups of students. Each cell reports the coefficient on entry-grade test score percentile from a separate OLS regression limited to the sub-group defined in the row with the dependent variable defined in the column header. Each column 1-5 uses the specification from the following column of Table 4, respectively: 3, 6, 7, 8, and 9. Free-lunch eligible is an indicator for whether the student was ever eligible for free or reduced-price lunch during the experiment. See notes to Table 4 for regressions specifications and other variable definitions. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 7
Effects of Class Size on Adult Outcomes: Kindergarten Entrants Only

Dependent Variable: Test Score	College in	College by	College	Wage	Summary	
	2000	Age 27	Quality	Earnings	Index	
	(%)	(%)	(\$)	(\$)	(% of SD)	
	(1)	(2)	(4)	(5)	(6)	
Small class (no controls)	5.37 (1.26)	1.56 (1.50)	1.57 (1.29)	165 (145)	-3.23 (431)	6.07 (2.63)
Small class (with controls)	5.16 (1.21)	1.70 (1.35)	1.64 (1.22)	185 (143)	-57.6 (440)	5.64 (2.60)
Observations	5,621	6,025	6,025	6,025	6,025	6,025
Mean of dep. var.	51.44	31.47	51.45	27,422	17,111	4.88

Notes: This table replicates Table 5, restricting the sample to students who entered a STAR school in kindergarten. See notes to Table 5 for regression specifications and variable definitions. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 8
Results for Alternative Measures of Wage Earnings

Dependent Variable:	Positive Mean Earnings	Above Median Earnings	Percentile Earnings	Household Income	2007 Wages
	(%)	(%)	(%)	(\$)	(\$)
<i>A. Cross-Sectional Correlations</i>	(1)	(2)	(3)	(4)	(5)
Entry-grade test percentile	0.055 (0.021)	0.222 (0.028)	0.156 (0.017)	126.9 (11.75)	101.0 (9.735)
<i>B. Class Size Impacts</i>					
Small class	0.123 (0.741)	0.482 (1.213)	-0.117 (0.610)	241.5 (457.2)	-263.3 (404.7)
<i>C. Class Quality Impacts</i>					
Class quality (peer scores)	0.062 (0.032)	0.176 (0.052)	0.098 (0.031)	52.40 (20.19)	45.14 (19.05)
Mean of dep. var.	86.14	50.00	50.00	23,883	16,946

Notes: This table replicates certain specifications using alternative measures of earnings outcomes. Panel A replicates the specification of column 3 of Table 4. Panel B replicates the "with controls" specification of row 2 of Table 5. Panel C replicates the specification of column 2 of Table 8. Each of the five columns denotes a different earnings variable used in that column's regressions: (1) an indicator for having positive wage earnings in any year 2005-2007, (2) an indicator for having average wage earnings over years 2005-2007 greater than the sample median (\$12,553), (3) the within-sample percentile of a student's average wage earnings over years 2005-2007, (4) total household income for each student over years 2005-2007, defined as adjusted gross income adjusted for tax-exempt Social Security and interest payments, and (5) wage earnings in 2007, winsorized at \$100,000. See notes to Tables 4, 5, and 8 for regression specifications and other variable definitions. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 9
Impacts of Class Size and Quality on Components of Summary Outcome Index

Dependent Variable:	Home Owner	Have 401 K	Married	Moved Out of State	College Grads in 2007 Zip	Predicted Earnings Summary Index
	(%)	(%)	(%)	(%)	(%)	(\$)
<i>A. Class Size</i>	(1)	(2)	(3)	(4)	(5)	(6)
Small class	0.712 (1.039)	2.888 (1.042)	1.872 (1.082)	1.105 (0.930)	-0.038 (0.223)	438.9 (209.8)
Observations	10,992	10,992	10,404	10,268	10,404	10,992
<i>B. Class Quality</i>						
Class quality (peer scores)	0.080 (0.047)	0.053 (0.058)	0.056 (0.051)	0.029 (0.045)	0.019 (0.012)	16.48 (12.16)
Observations	10,959	10,959	10,375	10,238	10,375	10,959
Mean of dep. var.	30.80	28.18	44.83	27.53	17.60	15,912

Notes: Columns 1-5 decompose the impacts of class size and quality on the summary index into impacts on each of the summary index's five constituent components. Panel A replicates the "with controls" specification of row 2 of Table 5 for each component. Panel B replicates column 9 of Table 8 for each component. See notes to those tables for regression specifications and sample definitions. See Table 1 for definitions of the dependent variables used in columns 1-5, each of which is a component of summary index. Column 6 reports impacts on an alternative "predicted earnings" summary index. This index is constructed by predicting earnings from a regression of mean wage earnings over years 2005-2007 on the five dependent variables in columns 1-5. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 10
Impacts of Class Size and Quality: Heterogeneity Analysis

Dependent Variable:	Test Score	College in 2000	College by Age 27	College Quality	Wage Earnings	Summary Index
	(%)	(%)	(%)	(\$)	(\$)	(% of SD)
	(1)	(2)	(3)	(4)	(5)	(6)
<i>A. Effect of Small Class</i>						
Blacks	6.871 (1.825)	2.722 (2.036)	5.312 (2.417)	249.0 (134.1)	250.0 (540.0)	6.308 (3.343)
Whites	3.699 (1.109)	1.065 (1.099)	-0.177 (1.120)	38.94 (140.3)	-348.1 (413.6)	4.388 (2.649)
Males	4.883 (1.103)	2.594 (1.278)	2.279 (1.414)	244.5 (127.0)	798.3 (497.7)	10.89 (3.188)
Females	4.360 (1.226)	0.716 (1.611)	0.454 (1.621)	6.638 (163.3)	-1130 (434.8)	-2.599 (3.069)
Free-lunch eligible	5.767 (1.299)	0.837 (1.242)	3.908 (1.560)	-2.517 (88.34)	-251.7 (394.7)	3.162 (2.790)
Not elig. for free lunch	3.376 (1.288)	3.592 (1.691)	-0.914 (1.480)	296.6 (222.8)	293.0 (595.5)	7.292 (3.390)
<i>B. Effect of Class Quality (peer scores)</i>						
Blacks	0.732 (0.027)	0.081 (0.066)	0.089 (0.086)	3.197 (7.735)	36.22 (18.60)	0.236 (0.099)
Whites	0.582 (0.041)	0.069 (0.070)	0.075 (0.068)	12.16 (6.794)	68.17 (29.02)	0.180 (0.181)
Males	0.654 (0.033)	0.016 (0.061)	0.067 (0.068)	8.257 (7.548)	66.03 (25.48)	0.357 (0.158)
Females	0.654 (0.039)	0.151 (0.060)	0.131 (0.065)	7.845 (6.162)	32.37 (19.53)	0.032 (0.163)
Free-lunch eligible	0.650 (0.036)	0.058 (0.052)	0.089 (0.068)	2.319 (3.906)	53.14 (17.55)	0.077 (0.120)
Not elig. for free lunch	0.652 (0.043)	0.149 (0.103)	0.104 (0.093)	22.34 (11.02)	47.11 (39.64)	0.442 (0.213)

Notes: Each cell reports a coefficient estimate from a separate OLS regression. Panel A replicates the "with controls" specification of row 2 of Table 5, for various subgroups and dependent variables. Panel B replicates the specification of column 2 of Table 8, for various subgroups and dependent variables. Free-lunch eligible is an indicator for whether the student was ever eligible for free or reduced-price lunch during the experiment. See notes to Tables 5 and 8 for regression specifications and other variable definitions. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 11
Impacts of Teacher Experience: Small vs. Large Classes

Dependent Variable:	Test Score	Wage Earnings	Test Score	Wage Earnings
	(%)	(\$)	(%)	(\$)
	(1)	(2)	(3)	(4)
<i>A. Small Classes</i>				
Teacher with >10 years of experience	2.91 (3.15)	405.4 (1326)	0.187 (3.54)	-2244 (1750)
Observations	1,690	1,817	854	1,047
<i>B. Large Classes</i>				
Teacher with >10 years of experience	2.95 (1.68)	1471 (714.2)	0.695 (1.47)	-540.2 (702.3)
Observations	3,911	4,188	3,416	3,862
Entry grade	KG	KG	Grade ≥1	Grade ≥1

Notes: This table replicates the specifications of columns 1-4 of Table 6. Panel A includes students assigned to small classes upon entry; Panel B includes those assigned to large classes. See notes to Table 6 for regression specifications and variable definitions. Standard errors, reported in parentheses, are clustered by school.

APPENDIX TABLE 12
Clustering by Classroom in Test Scores and Earnings: IV Estimates

Dependent Variable:	Wage Earnings (\$)				
<i>A. All Entrants</i>	(1)	(2)	(3)	(4)	(5)
Entry-grade test percentile	82.21 (23.63)	96.39 (31.16)			90.04 (8.65)
<i>B. KG Entrants</i>					
KG test percentile	78.71 (35.09)	89.28 (39.70)	74.85 (26.50)	80.33 (26.40)	93.79 (9.56)
Estimation method	Leave-Out Mean	Split- Sample	LIML	2SLS	OLS

Notes: The effects of class quality on test scores and earnings reported in columns 1 and 2 of Table 8 can be combined to produce a reduced-form IV estimate of the earnings effect associated with an increase in test scores: $\$50.61/0.662=\76.48 . Including only those observations with both test score and wages in the data changes the coefficient slightly to \$82.21 in Column 1. To test the robustness of our leave-out mean estimator, we report three alternative IV estimates of the impact of test scores on earnings, controlling for school-by-entry-grade fixed effects and the demographic controls used in Column 2 of Table 8. Column 2 instead uses a split-sample definition of peer scores, in which we randomly split classes into two groups and proxy for class quality using peer scores in the other group. Column 3 estimates the model using limited information maximum likelihood, using kindergarten class dummies as instruments. Column 4 instruments for test score with classroom dummies using two-stage least squares. Column 5 reports an OLS estimate of the correlation between test scores and earnings as a reference. Panel A pools all entry grades and reports estimates for three of the specifications. Instrumenting with entry class dummies is ill defined in later grades as it would require defining class quality based purely on the new entrants, who constitute a small fraction of each class. Panel B replicates the specifications in A using the subsample of kindergarten entrants. The leave-out mean IV estimator in Panel B column 1 coincides with the jackknife IV of Angrist, Imbens, and Krueger (1995), when we use only KG entrants. When we pool entry grades, our leave-out mean estimator differs from jackknife IV because we form the leave-out mean measure of class quality using all peers (including previous entrants), not just those who entered in the current entry grade. Standard errors, reported in parentheses, are clustered by school in all columns except 3.

APPENDIX TABLE 13
Effects of Class Quality on Components of Non-Cognitive Measures

A. Cross-Sectional Correlations

Dependent Variable:	Wage Earnings							
	Grade 4 Non-Cognitive Measure				Grade 8 Non-Cognitive Measure			
	(\$)	(\$)	(\$)	(\$)	(\$)	(\$)	(\$)	(\$)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Effort	79.38 (16.63)				128.0 (15.47)			
Initiative		56.82 (18.28)				92.61 (16.87)		
Value			56.40 (15.74)				116.4 (20.01)	
Participation				35.80 (18.47)				59.48 (18.16)

B. Class Quality Impacts

Dependent Variable:	Grade 4 Non-Cognitive Measure				Grade 8 Non-Cognitive Measure			
	Effort	Initiative	Value	Particip	Effort	Initiative	Value	Particip
	(%)	(%)	(%)	(%)	(%)	(%)	(%)	(%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Class quality (peer scores)	0.152 (0.066)	0.165 (0.070)	0.083 (0.072)	0.121 (0.082)	0.142 (0.071)	0.071 (0.056)	0.134 (0.070)	0.194 (0.063)

Notes: This table decomposes relationships described in Table 9 into the four constituent components of non-cognitive skill. These four non-cognitive measures are constructed from a series of questions asked of the student's teacher(s) and are intended to measure, respectively: student effort in class, initiative, whether a student perceives school/class as "valuable", and participatory behavior. The measures were reported twice, once by the student's regular 4th grade teacher (columns 1-4) and the second time the scores are the average of the reports by the 8th grade math and English teachers (columns 5-8). Each of the four variables is scaled as a within-sample percentile rank. Panel A replicates column 1 of Table 9, using only one of the four non-cognitive measures as a covariate in each regression. Panel B replicates column 5 of Table 9, using one of the four non-cognitive measures as the dependent variable in each regression. See notes to Table 9 for regression specifications and other variable definitions. Standard errors, reported in parentheses, are clustered by school.