The Production of Human Capital in Developed Countries: Evidence from 196 Randomized Field Experiments*

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Abstract

Randomized field experiments designed to better understand the production of human capital have increased exponentially over the past several decades. This chapter summarizes what we have learned about various partial derivatives of the human capital production function, what important partial derivatives are left to be estimated, and what – together – our collective efforts have taught us about how to produce human capital in developed countries. The chapter concludes with a back the envelope simulation of how much of the racial wage gap in America might be accounted for if human capital policy focused on best practices gleaned from randomized field experiments.

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"The True Method of Knowledge is Experiment" – William Blake

1 Introduction

Racial and ethnic inequality is a stubborn empirical reality across the developed world. Blacks in the United States earn twenty-four percent less, live five fewer years, and are six times more likely to be incarcerated on any given day (Fryer 2010). Black men in the United Kingdom are three times more likely to be unemployed and as full-time workers, earn twenty percent less (Hatton 2011). The Roma in Hungary are over two years less educated, have worse self-reported health, and earn twenty-eight percent less (Kántor 2011). Turkish immigrants in Germany are almost twice as likely to be unemployed and earn thirty-eight percent less (von Loeffelholz 2011). African immigrants in Spain are less educated than natives, have a 4.9 percentage point higher unemployment rate, and earn thirty-five percent less (de la Rica 2011). The income difference between natives and second generation immigrants in Sweden is 11% (Nordin and Rooth 2007).

Gaining a better understanding of the underlying causes of such stark racial and ethnic inequality is of tremendous importance for public policy. Using data from the U.S., O'Neill (1990) and Neal and Johnson (1996) demonstrate that blacks, Hispanics, and whites are paid similar prices for similar pre-market skill bundles – yet, there are large differences in skills. Similarly, Nordin and Rooth (2007) show that differences in income between natives and second generation immigrants in Sweden depend strongly on a skill gap – when controlling for scores on the Swedish Military Enlistment Test, the income gap decreases by more than seventy percent.

An important question then, is what obstacles preclude the acquisition of productive skills. Using ten large datasets which together, include students that range in age from eight months to 17 years old, Fryer and Levitt (2013) show that the racial achievement gap is remarkably robust across time, samples, and assessments. The achievement gap does not exist in the first year of life, but black students in the U.S. fall behind by age two (in the raw data) and these racial differences in academic achievement after kindergarten cannot be explained by including standard controls. Similarly, controls cannot explain differences between children of natives and children of immigrants on international standardized tests such as the Programme for International Student Assessment (PISA), Trends in International Mathematics and Science Study (TIMSS), and Progress in International Reading Literacy Study (PIRLS) in other developed countries such as France, Switzerland, Netherlands, and Sweden (Parsons and Smeeding 2008).

If the deleterious effects of labor market discrimination are in decline and the importance of productive skills are on the rise, an important public policy question is how to increase human capital – particularly for

those who, due to accident of birth, begin life disadvantaged. A fuller understanding would allow policy makers to use basic economic principles (e.g. equating marginal return to marginal costs) in their decision making. Is it more cost-effective to decrease class size or provide parents financial incentives to increase student achievement? Should school districts increase the management skills of principals or increase early childhood programs? What is a better use of resources – early childhood investments or providing high-dosage tutoring to adolescents?

In an effort to answer questions like these, education researchers have spent decades trying to infer causal relationships from non-experimental data by examining large data sets and invoking various assumptions, many of which are not verifiable. Prior to the late 1970s, research on the relationship between class size and academic achievement was widely considered inconclusive (Porwell 1978; Glass and Smith 1978). In fact, some studies, including the famous Coleman Report, suggested there were greater gains in classrooms with *more* students (Nelson 1959; Coleman et al. 1966). These studies did not adequately account for the fact that school districts commonly bundled better students and teachers in classrooms with more students. A well-designed randomized experiment would enable researchers to avoid such confounding factors and help settle the debate among non-experimental estimates. Using the random assignment of students to small classes in Project STAR, Krueger (1999) showed that students assigned to small classrooms indeed do score higher than students in regular sized classrooms. The effect sizes for the K-3 students in Project STAR are in the range of 0.19-0.28 standard deviations and represent 64 to 82 percent of the white-black test score gap in the data

Similarly, a large body of non-experimental studies have found significant positive correlations between neighborhood socioeconomic status and students' academic achievement (Aaronson 1998; Ainsworth 2002; Chase-Lansdale and Gordon 1996; Chase-Lansdale et al. 1997; Duncan, Brooks-Gunn, and Klebanov 1994; Halpern-Felsher et al. 1997; Kohen et al. 2002). However, randomized and quasi-experimental studies have failed to establish a causal link. Although Rosenbuam (1995) found that suburban students from Chicago's Gautreaux program outperformed urban students, Jacob (2004) found no effects on students' test scores from switching neighborhoods due to housing demolitions. Further, Oreopoulous (2003) found no evidence of long-term impacts of neighborhood quality on labor market outcomes in a quasi-experimental analysis. More importantly, in the short-run, the Moving to Opportunity randomized housing mobility experiment (Ludwig et al. 2012; Kling, Liebman and Katz 2007; Sanbonmatsu et al. 2011) produced no sustained improvements in academic achievement, educational attainment, risky behaviors, or labor market outcomes for either female or male children, including those who were below school age at the time of random assignment. Interestingly though, Chetty et al. (2016) show that the Moving to Opportunity experiment had large

impacts on early-adulthood outcomes for children who were younger than 13 years old at randomization. In their mid-twenties, these individuals have 31% higher income, have higher college attendance rates, are less likely to be single parents, and live in better neighborhoods relative to similar individuals in the control group. For children who were older than 13 years old at randomization, the experiment had no positive long-term impacts.

In the 1920s, William McCall, an education psychologist at Columbia University, was one of the first supporters of using randomization to investigate the validity of education programs. His 1923 book, "How to Experiment in Education", developed a method for gathering data by randomly determining treatment and control groups. His work provided the framework for the experimental designs we see in educational field experiments today. Many of the early influential education field experiments came decades after McCall's book with the wave of large-scale social experiments in the latter half of the 20th century.[1] In the 1960s we saw the Perry preschool experiment and the income maintenance experiments, in the 1970s the Abecedarian project was initiated, and in the 1980s there was Project STAR, the Tennessee class size experiment. The data from these randomized experiments alone were used for decades to investigate many interesting questions about how to best produce human capital.

The inherent power of randomized field experiments is in the ability to estimate partial derivatives of the educational production function. That is, holding other variables constant, one can alter the amount of time students spend in school or the salary of their teachers, or whether or not the students receive financial incentives. One's imagination is the only real bound.

To see the advantages of this approach, imagine the following simple production process.[2] Let Y_{ij} denote a measure of an academic achievement j for individual i, where j might represent state test scores or other norm-referenced tests such as the Peabody Picture Vocabulary Tests or the Woodcock-Johnson Tests of Achievement. For each j, assume a simple Education Production Function (EPF) of the following form:

$$Y_i = f(E_i, S_i, H_i, M_i, P)$$

where, E_i = denotes student $i's$ early childhood experience, S_i captures various school inputs, H_i represents household and neighborhood inputs, M_i, captures "social skills" such as grit, resilience, or what psychologists often refer to as "the Big 5." Let P be a vector of relevant prices.

We assume that f is smooth and continuously differentiable in its arguments. Imagine that we want

[1] See Levitt and List (2009) for a brief history of field experiments.

[2] The model is meant to illuminate, clarify, and contrast estimates in the literature. It is not meant to be "realistic" or to be directly estimable. There is a rich literature designed to better understand and empirically estimate the education production function (Cunha and Heckman 2007; Hanushek 1979; Krueger 1999; Todd and Wolpin 2003).

to understand the impact of important changes in home environment on student test scores, holding school quality, mindset, and early childhood experience fixed. This is equivalent to estimating $\frac{\partial Y}{\partial H}$. On the other hand, we may want to understand the impact of investments in school-based reform on human capital holding all else equal by estimating $\frac{\partial Y}{\partial S}$. Or, the impact of instilling more "grit" or a "growth mindset" into students, all else equal. This is equivalent to $\frac{\partial Y}{\partial M}$.

Perhaps recognizing the net benefits of randomized field experiments and because of a desire to avoid past miscues due to biased estimates, federal and local governments, early childhood centers, entrepreneurs, and school districts have become laboratories for randomized field experiments. Forty-five years after the famous Perry Preschool experiment, families in Chicago Heights were rewarded for teaching their own children a similar curriculum (Fryer, Levitt, and List 2015). Thirty years after the seminal class size experiment in public elementary schools of Tennessee, school districts in both America and Europe have implemented various tutoring experiments, management best practices, and programs designed to increase the human capital of the adults in school buildings (e.g. Fryer 2014; Cook et al. 2014; Clark et al. 2013; Garet et al. 2008; Carlson et al. 2011; May et al. 2013; Blachman et al. 2004). Forty years after the income maintenance experiments, public policy across the developed world is being influenced by researchers investigating the impacts of welfare-to-work programs, earnings supplements, and parental involvement (e.g. Hamilton et al. 2001; Michalopoulos et al. 2002; Avvisati et al. 2014).

Indeed, randomized control trials in education have increased exponentially over the past 50 years. In 2000, 14 percent of reviewed education publications on What Works Clearinghouse met their standards without reservations, a distinction given only to well-designed studies that have comparison groups determined through a random process. By 2010, that number had tripled to over 46 percent. Figure 1 provides a time series of studies in education. Throughout the 1980s, these randomized education studies were sparse. But in the 1990s, we start seeing a steady flow of approximately 10 publications a year that utilize a random design and then this number increases all the way up to a high of 49 randomized experiments in 2009.

Given the remarkable increase in the use of randomized field trials over the past 50 years and the robust correlation between human capital and other economic outcomes such as income and employment, it's time to take stock and summarize what we have learned about various partial derivatives of the human capital production function, what important partial derivatives are left to be estimated, and what – together– our collective effort over the past several decades has taught us about how to produce human capital in developed countries.[3]

[3]To be clear, randomized trials are not a panacea. There are important limitations to randomized controlled trials, which have been documented in Deaton (2010), Mosteller and Boruch (2002), Worrall (2007), and Rothstein and von Wachter (2016), the latter in this volume. We describe a few here. First, many questions that are potentially interesting to economists may not be answerable

This chapter attempts to do three things.

First, We conducted a relatively exhaustive search of all randomized field experiments in education. We define a field experiment as any intervention that uses a *verifiably* random procedure to assign participants to treatment and control groups in a non-laboratory environment. This definition, while restrictive, is consistent with the definition of a field experiment described in Harrison and List (2004) and the US Department of Education's What Works Clearinghouse "without reservation" standard. Using this definition, we sourced almost one thousand field experiments to be included in our analysis. We further limited the sample of studies to be included to studies conducted in "highly developed" countries with standardized reading or math outcomes.[4] These restrictions eliminated almost three-quarters of the experiments, leaving a sample of 199.

We divide our sample of studies into three main categories of intervention – early childhood, school-based interventions, and home-based interventions – and provide a summary of the literature within each category.[5] Early childhood experiments investigate the impacts of preschool attendance, home-based initiatives that target pre-kindergarten children, and different preschool models on early achievement. Indeed, any experiment with outcomes measured before kids enter school is categorized as early childhood – inde-

with a randomized trial. For instance, how much of the variance in achievement is explained by genetic endowment? Given we are not likely to alter genetics by means of a field experiment, if one is wed to randomized controlled trials (RCTs) then this question is unanswerable. Second, as with all statistics – the evaluation of field experiments has implications for the mean of the population and may have little value in predicting individual behavior. With large enough RCTs, one can alleviate some of these concerns by estimating heterogeneous treatment effects. Third, and likely most constraining, are a host of important caveats which center on external validity. One cannot always generalize the results from a local RCT to other contexts. An obvious example of this is if an RCT finds a program has large impacts using a sample of poverty-stricken minority children, one cannot assume the program will have similar impacts on the universe of students in the U.S. However, even if the RCT uses a representative sample of the target population, there are still concerns of external validity. For example, when implementing a large-scale policy, there could possibly be general equilibrium effects that a pilot RCT did not detect. Fourth, Deaton (2010) expresses many concerns about the analyses and implementations of RCTs – exploring heterogeneous treatment effects can be viewed as data-mining and researchers should explore the implications of testing a large number of hypotheses in their studies; researchers rarely use appropriate standard errors when reporting results; exploring different combinations of baseline variables to include in regressions is another potential form of data-mining; including baseline variables can lead to substantial biases in small samples; attrition from the study must be addressed; and it is not uncommon for RCTs to have implementation and operational issues that threaten the validity of the experiment. Fifth, spillover effects could lead one to misstate a program's overall effect. The example that Rothstein and von Wachter (2016) give is a labor market program that attempts to increase the search effort of individuals in treatment. This program may lower the chances of finding jobs for the control group and thus overstate the impact of the program's total effect. Sixth, RCTs evaluating programs are considered "black boxes" that do not reveal the true mechanisms of interest. Although one can use randomized admission lotteries to estimate the causal impact of pre-existing charter schools, the causal relation between specific school inputs cannot be determined from such a study. Finally, Deaton (2010) and others argue that in an effort to overcome the above issues, RCTs can become prohibitively expensive. Still, with these important limitations in mind, the conventional wisdom is: if you *can* do a randomized field experiment, you should. Of the above seven issues which are commonly discussed with RCTs, five of them can be sidestepped by running more, larger, and better designed RCTs. Moreover, if one designs the RCT in a way that helps validate a model of selection for observational data, then the only limitation appears to the budget of the researcher.

[4]We consider countries as highly developed if they received a classification of "Very High Human Development" in United Nations Development Programme (2010). A country is classified as very high if they score in the top quartile on an index of human development that includes life expectancy, mean years of schooling, expected years of schooling, and gross national income per capita.

[5]We don't focus on mindset experiments (M_i in the production function above) due to very few of these experiments passing the inclusion restrictions of our meta-analysis discussed below.

pendent of the nature of the treatment.

School-based experiments target K-12 curricula, teachers, management practices, students in classroom settings, principals, and other school resources. Any experiment where the dosage is applied in a school setting – such as offering families vouchers to attend private schools or after-school programs – We categorize as a school-based intervention. Even experiments in which K-12 resources are given at home – for instance tutors from the school tutor students in their living rooms – We code as a school-based experiment. Home-based experiments focus on parenting, income constraints, neighborhood environment, and a student's access to educational resources in their household. Similar to above, if an experiment takes place at home and focuses on these inputs, then it is considered a home-based experiment. For example, parenting classes that take place in a school auditorium are considered a home intervention.

While the above categories are mutually exclusive, collectively exhaustive, and internally consistent – which categories to sort experiments into is a bit arbitrary. For example, in Sumi et al. (2012), both teachers and parents received training on how to teach students replacement behaviors. This is potentially important because when we combine estimates within categories across the set of experiments using the DerSimonian-Laird meta-analysis coefficient (see DerSimonian and Laird 1986) – the labels on categories become a "lazy man's" way of deciding what works and what doesn't. If the meta-analysis coefficient for early childhood studies is greater than the coefficient for home studies, this is evidence that early childhood studies have a higher impact on average. In an attempt to avoid interactions of the categories in our analysis, studies that have characteristics of more than one category are excluded from our analysis (but are still included in the tables).

With these caveats in mind, the results of this inquiry are interesting and, in some cases, quite surprising. There is substantial heterogeneity in treatment effects across and within various categories of field experiments. Experiments in early childhood and schools can be particularly effective at producing human capital. The random effects meta-coefficients for early childhood experiments are 0.111σ (0.031) for standardized math scores and 0.165σ (0.032) for reading. The estimates for schools are 0.052σ (0.008) and 0.068σ (0.009) for math and reading scores, respectively. Within school-based field experiments, those that alter the management practices of schools, or implement "high-dosage" tutoring tend to demonstrate large effects. Having pooled impacts in the range of 0.507-0.655σ, the three most successful early childhood experiments were the famous Ypsilanti Perry Preschool Project (Weikart et al. 1970) and evaluations of the Breakthrough to Literacy and Ready, Set, Leap! curricula (Layzer et al. 2007). In schools, with evaluations producing pooled impacts ranging from 0.779-1.582σ, the most successful programs appear to be Reading Recovery (Center et al. 1995; Schwartz 2005) and Peer-Assisted Learning Strategies (Mathes and Babyak

2001).

Interventions that attempt to lower poverty, change neighborhoods, or otherwise alter the home environment in which children are reared have produced surprisingly consistent and precisely estimated "zero" results. Avvisati et al. (2014) show that a comprehensive parent training program in France had large behavioral impacts that spilled over to students whose parents did not participate. However, the study found no impacts on academic outcomes. The famous negative income tax experiment – which provided low-income families with more money while incentivizing them to work less – had no impact on children's test scores (Maynard and Murnane 1979). As with Avvisati et al. (2014) and Maynard and Murnane (1979), the average home or neighborhood experiment that our search returned has math and reading impacts that are statistically indistinguishable from zero.

The literature – all 196 randomized field experiments discovered through our search process – is summarized in a large set of tables at the end of the chapter. This was the most laborius part of the process. For each study, we collected data on sample demographics, key aspects of the research design, and effect sizes. The typical study published in a top economics journal has this information readily available and collecting the data only took a few minutes. But, some studies published in older journals, less technical journals, or government reports required an exhaustive search of the publication to estimate effect sizes from the information given. The large collection of tables provide a bird's eye view of the set of randomized field experiments that have been conducted and evaluated. We include a large set of studies that vary curriculum choices. These are included in the tables but not described in the text, as they don't align with traditional economic choice variables in a concise way and because of the potential effects of publication bias on these types of studies.

Second, for every randomized field experiment found, we calculated the impact (in standard deviations) of the intervention on standardized math and reading outcomes and collected data on features of the experiment. These data include over forty potential explanatory variables, including length of intervention, grade/age of subjects, location, if the sample was a majority English Language Learner (ELL), disadvantaged, black, Hispanic, or of low ability, and so on. This provides us with a novel dataset to investigate the correlation of important sample demographics and treatment effects of experiments designed to increase human capital.

An important pattern that arises in the data is the correlation between effectiveness of treatment and age of subjects at the time of intervention. It has also been observed that some interventions tend to be more effective at increasing math achievement relative to reading achievement (Fryer 2014; Abdulkadiroglu et al. 2011; Angrist et al. 2011; Dobbie and Fryer 2011; Hoxby and Murarka 2009; Gleason et al. 2010). There

are many theories that may explain the disparity in treatment effects by subject area. A leading explanation posits that reading scores are influenced by the language spoken when students are outside of the classroom (Charity et al. 2004; Rickford 1999). Charity et al. (2004) argue that if students speak non-standard English at home and in their communities, increasing reading scores might be especially difficult. Research in developmental psychology has suggested a second possibility – that the critical period for language development occurs early in life, while the critical period for developing higher cognitive functions extends into adolescence (Hopkins and Bracht 1975; Newport 1990; Pinker 1994; Nelson 2000; Knudsen et al. 2006).

Our data suggests that the age theory has merit. In math, the treatment effect is not strongly related to the age of the student at the time of intervention. The correlation coefficient is 0.0679. In stark contrast, early in life, many reforms increase reading performance. Later in life, very few treatments have any effect on reading, save "high dosage" tutoring. Put precisely – there is a negative relationship between age and reading treatment effects. The correlation coefficient is -0.2069. To put this number in perspective, the average effect on reading of interventions targeting children with an average age less than 5 is 0.177σ. The average effect of reading interventions targeting students with an average age greater than 14 is 0.039σ.

Third, We conclude this chapter by simulating a life-cycle model that enables us to make an educated guess about how much of racial and ethnic wage inequality in America might be accounted for if we simply used the best practices gleaned from an exhaustive review of what works in the literature. Although a majority of the randomized field trials discussed in this chapter do not report impacts on adult outcomes, we are able to use correlations from the National Longitudinal Survey of Youth 1979 (NLSY79) and NLSY79 Children and Young Adults (CNLSY) datasets to simulate how shocks in a given life-stage will impact later outcomes. Specifically, we follow the methods described in Winship and Owen (2013) and construct a model similar to the Social Genome Model (SGM). Using this model, we estimate that if children were given a successful early childhood intervention and then received successful school-based interventions in mid-childhood and again in adolescence, one might dramatically reduce, and under some assumptions eliminate, wage inequality. Obviously, these types of educated guesses vis-a-vis simulations must be taken with a proverbial grain of salt.

The chapter proceeds as follows. Section 2 describes our method for culling and standardizing field experiments from the education literature. Section 3 describes evidence from randomized field experiments across the three categories: early childhood, home-based interventions, and school-based interventions. Section 4 uses the estimates from the literature to simulate a life-cycle model and provide a sense of how much of racial wage inequality in America might be accounted for if government policy focused on the best practices gleaned from the literature. Section 5 concludes. There are 100 pages worth of Appendix Tables

that summarize the literature in a concise and consistent way.

2 A Method for Finding and Evaluating Field Experiments

In deciding which field experiments to include in our analysis, we first culled a reasonably exhaustive list of field experiments and then narrowed our focus to studies that satisfied certain criteria. We began by searching all "quick reviews" and "single study reviews" in the What Works Clearinghouse (WWC). WWC was created by the U.S. Department of Education's Institute of Education Sciences in 2002. Its goal is to provide reviews of education studies, policies, and interventions in order for researchers to determine "what works" in education. Currently, WWC has over 10,500 reviews available in an online searchable database. Eligible studies are reviewed by a team of WWC's certified staff against WWC standards and assigned a rating. The highest rating of the Clearinghouse is reserved for studies that met standards without reservations. This implies that groups compared in the study were determined through a random process, there was low overall attrition from the sample, the differential attrition across groups was low, and there were no confounding factors (U.S. Department of Education 2015).[6] Our search of WWC produced 115 randomized field experiments that met standards without reservations.

We augmented the WWC search by looking through recent education literature reviews (e.g. Almond and Currie 2011; Fryer 2010; Heckman and Kautz 2013; Nye et al. 2006; Yeager and Walton 2011) to ensure that all these potential studies had been included. In most all cases, the randomized studies were already in the What Works Clearhinghouse, but this process produced important additions.

Finally, we conducted relatively broad searches of known databases – such as ERIC, JSTOR, EconLit – that include education papers to augment our sample of studies. In each database, we searched for all phases generated by concatenating one element from the set of strings ("early childhood", "education", "housing", "neighborhood", "parent", "school", "student", "teacher") with one element from ("experiment", "random assignment", "randomization"). For each database, we collected all hits that searching for these 24 unique phrases returned.[7] These searches provided us with over 10,000 citations to check. To conduct this laborious task, we had a team of five research assistants skim every article and select papers that explicitly mention a random process determining the experimental sample.

Using these approaches, we found 859 potential studies. Table 1 describes how we narrowed our set

[6] That is, no factor other than the intervention itself is present that all treatment students in one group are exposed to and no students in the comparison group are exposed to. If a confounding factor is present, it would be impossible to distinguish between the effect of the intervention and the effect of the factor.

[7] JSTOR's search algorithm occasionally returned thousands of results. Due to resource and time constraints, we decided to only collect the top 200 (as determined by "relevance") results for each phrase in JSTOR.

of studies from 859 to 196. As discussed, we only included experiments that had samples determined by a verifiably random process that were pre-college; that took place in a highly developed country (as determined by the Human Development Index constructed by the United Nations Development Programme); and that reported standardized reading or mathematics test scores as an outcome measure at posttest. The random process is important for causal inference – though the rise of strong quasi-experimental analyses makes this quite restrictive. Some experiments use non-norm referenced tests designed by the experimenter for the purpose of the experiment. These evaluations are not comparable across experiments and were omitted. In general, the restrictions are important for comparability and allow one to synthesize the estimates from the studies in the analysis below.

Unfortunately, these restrictions lead to us not including some influential experimental studies. For example, our screening excluded the exploration of the impact of teacher value-added on students in Chetty et al. (2014) because their research design is non-experimental. Housing demolitions in Jacob (2004) and the famous brown and blue eye experiments performed by Jane Elliot in her classrooms in the late 1960s also did not utilize verifiably random processes. A well-known incentive experiment in Israel (Angrist and Lavy 2009) was excluded because the main outcome was receipt of matriculation certificates. Similarly, many important social-psychological, behavioral, and "mindset" experiments (e.g. Mischel et al. 1972; Cohen et al. 2006; Cohen et al. 2009; Wilson and Linville 1982; Aronson et al. 2002; Miyake et al. 2010; Duckworth et al. 2013) were excluded because they did not report results for standardized math or reading outcomes or the sample was post high school.

For each experiment that passed our screening, we report estimates of the annual pooled effect sizes on reading and math outcomes, in standard deviations. If papers did not report results in this manner, we attempted to use the information given to calculate standardized effect sizes. For example, if impacts were presented as scale score points on a test, we would divide the coefficient by the standard deviation given in the summary statistics. The most common calculation we performed was using the average treatment and control posttest scores (or changes between pretest and posttest) as well as the corresponding standard deviations to calculate the standardized difference between the two groups.

Specifically, we used this information to calculate a statistic known as Hedge's g and its corresponding standard error (see Hedges 1981 and Lipsey and Wilson 2000). Since this measure is just the difference between the average test scores of treatment and control groups, point estimates obtained from this method are identical to intent-to-treat (ITT) estimates that do not include controls and use the same standard deviation to standardize the test scores. Note that since all studies included in this paper used a random procedure to assign treatment and control groups, point estimates from multivariable ITT regressions should not differ

significantly from the raw differences. If possible, when necessary information for this statistic was missing we would make assumptions (e.g. equal number of students assigned to treatment and control or use the standard deviation from the national sample of the standardized test).[8] If there was not enough information presented in the paper for us to make credible assumptions, the study was excluded.

One common issue we encountered was the calculation of standard errors. Unfortunately, without having access to the micro-data, it was not possible to calculate the appropriate standard errors for every effect size. In an attempt to not overstate the significance of an effect size, when calculating Hedge's *g*, we erred on the conservative side and used the number of units randomized to calculate the standard errors. For example, although Slavin et al. (1984) had a sample of 504 students, randomization was done at the school level (N = 6).

3 Evidence from 196 Randomized Field Trials

3.1 Early Childhood Experiments

In the past five decades there have been many field experiments designed to increase achievement before kids enter school.[9] Appendix Table 1 provides an overview of 44 randomized field experiments (from 24 papers), the ages they serve, and their treatment effects on standardized math and reading outcomes. Here, we partition the literature into interventions that are early childhood center-based and others that are more home-based.

3.1.1 Center-Based Experiments

Perhaps the most famous early intervention program for children involved 123 students in Ypsilanti, Michigan, who attended the Perry Preschool program in 1962 (58 were randomly assigned to treatment). The program consisted of a 2.5-hour daily preschool program and weekly home visits by teachers, and targeted children from disadvantaged socioeconomic backgrounds with IQ scores in the range of 70-85. An active learning curriculum - High/Scope - was used in the preschool program in order to support both the cognitive and non-cognitive development of the children over the course of two years beginning when the children were three years old. Schweinhart, Barnes, and Weikart (1993) find that students in the Perry Preschool program had higher test scores between the ages of 5 and 27, 21 percent less grade retention or special services required, 21 percent higher graduation rates, and half the number of lifetime arrests in comparison to chil-

[8] We documented all assumptions that were made for each study and these can be obtained from the author upon request.

[9] See Carneiro and Heckman (2003) or Almond and Currie (2010) for extensive reviews.

dren in the control group. Considering the financial benefits that are associated with the positive outcomes of the Perry Preschool, Heckman et al. (2010) estimated that the rate of return on the program is between 7 and 10 percent, passing a traditional cost-benefit analysis.

Although an influential experiment, Heckman et al. (2009) argues that the randomization protocol for the Perry Preschool experiment was compromised. Post-randomization, some children initially assigned to treatment whose parents were employed were swapped with control children whose parents were unemployed. The researchers' rationale for this swap was that employed mothers would find it difficult to participate in the home visits that treatment families received. Heckman et al. (2010) investigates the implications of these swaps and other potential issues with previously reported Perry results. Even after accounting for the compromised randomization (by correcting for the imbalance in preprogram variables and matching students), multiple-hypothesis testing, and small sample sizes of the original analysis, Heckman et al. (2010) still find statistically and economically significant impacts.

Another important center-based intervention, which was initiated three years after the Perry Preschool program is Head Start. Head Start is a preschool program funded by federal matching grants that is designed to serve 3- to 5-year-old children living at or below the federal poverty level.[10] The program varies across states in terms of the scope of services provided, with some centers providing full-day programs and others only half-day. In 2007, Head Start served over 900,000 children at an average annual cost of about $7,300 per child.

Evaluations of Head Start have often been difficult to perform due to the typical non-random nature of enrollment in the program.[11] Puma et al. (2010), in response to the 1998 reauthorization of Head Start, conduct an evaluation using randomized admission into Head Start.[12] The impact of being offered admission into Head Start for 3- and 4-year-olds is 0.10 to 0.34 standard deviations in the areas of early language and literacy. For 3-year-olds, there were also small positive effects in the social-emotional domain (0.13 to 0.18 standard deviations) and on overall health status (0.12 standard deviations). Yet, by the time the children who received Head Start services had completed first grade, almost all of the positive impact on

[10] Local Head Start agencies are able to extend coverage to those meeting other eligibility criteria, such as those with disabilities and those whose families report income between 100 and 130 percent of the federal poverty level.

[11] Currie and Thomas (1995) use a national sample of children and compare children who attended a Head Start program with siblings who did not attend Head Start, based on the assumption that examining effects within the family unit will reduce selection bias. They find that those children who attended Head Start scored higher on preschool vocabulary tests but that for black students, these gains were lost by age ten. Using the same analysis method with updated data, Garces et al. (2002) find several positive outcomes associated with Head Start attendance. They conclude that there is a positive effect from Head Start on the probability of attending college and - for whites - the probability of graduating from high school. For black children, Head Start led to a lower likelihood of being arrested or charged with a crime later in life.

[12] Students not chosen by lottery to participate in Head Start were not precluded from attending other high-quality early childhood centers. Roughly ninety percent of the treatment sample and forty-three percent of the control sample attended center-based care.

initial school readiness had faded. The only remaining impacts in the cognitive domain are a 0.08 standard deviation increase in oral comprehension for 3-year-old participants and a 0.09 standard deviation increase in receptive vocabulary for the 4-year-old cohort (Puma et al. 2010).[13]

Other early childhood interventions – many based on the early success of Perry Preschool and Head Start – include the Abecedarian Project, the Early Training Project, the Milwaukee Project, and Tulsa's universal pre-kindergarten program. The Abecedarian Project provided full-time, high-quality center-based childcare services for four cohorts of children from low-income families from infancy through age five between 1971 and 1977. Campbell and Ramey (1994) find that at age 12, those children who were randomly assigned to the project scored 5 points higher on the Wechsler Intelligence Scale and 5-7 points higher on various subscales of the Woodcock-Johnson Psycho-Educational Battery achievement test.

3.1.2 Home-Based Experiments

The most well known home-based field experiment in the early childhood years is the Nurse-Family Partnership. Through this program, low-income first-time mothers received home visits from a registered nurse beginning early in the pregnancy and continued until the child is two years old – a total of fifty visits over the first two years. The program aimed to encourage preventive health practices, reduce risky health behaviors, foster positive parenting practices, and improve the economic self-sufficiency of the family. In a study of the program in Denver in 1994-95, Olds et al. (2002) found that those children whose mothers had received home visits from nurses (but not those who received home visits from paraprofessionals) were less likely to display language delays and had superior mental development at age two. In a long-term evaluation of the program, Olds et al. (1998) found that children born to women who received nurse home visits between 1978 and 1980 had fewer juvenile arrests, convictions, and violations of probation by age fifteen than those whose mothers had not received treatment.

The Early Training Project provided children from low-income homes with summertime experiences and weekly home visits during the three summers before entering first grade in an attempt to improve the children's school readiness. Gray and Klaus (1970) report that children who received these intervention services maintained higher Stanford-Binet IQ scores (2-5 points) at the end of fourth grade. The Infant Health and Development Program specifically targeted families with low-birth-weight preterm infants and provided them with weekly home visits during the child's first year and biweekly visits through age three, as well as enhanced early childhood educational care and bimonthly parent group meetings. Brooks-Gunn,

[13]The Early Head Start program, established in 1995 to provide community-based supplemental services to low-income families with infants and toddlers, had similar effects (Administration for Children and Families 2006).

Liaw, and Klebanov (1992) report that this program had positive effects on language development at the end of first grade, with participant children scoring 0.09 standard deviations higher on receptive vocabulary and 0.08 standard deviations higher on oral comprehension. The Milwaukee Project targeted newborns born to women with IQs lower than 80; mothers received education, vocational rehabilitation, and child care training while their children received high-quality educational programming and three balanced meals daily at "infant stimulation centers" for seven hours a day, five days a week until the children were six years old. Garber (1988) finds that this program resulted in an increase of 23 points on the Stanford-Binet IQ test at age six for treatment children compared to control children.

Although the above parenting programs have shown promise, they are not widely accessible due to the time demands they place on parents and high implementation costs. York and Loeb (2014) investigate the impact of READY4K!, a low-cost text message program that targets parents of preschoolers. The program helps these parents support their children's literacy development by sending parents three text messages per week for an entire school year. These texts were designed to provide parents with information on the importance of their children developing particular skills, tips on how to support their children's development in a cost-effective manner, and encouragement. York and Loeb (2014) recruited parents from 31 preschool sites run by the San Francisco Unified School District's Early Education Department. Of the 874 eligible families, 440 enrolled and were randomly assigned to treatment group that participated in READY4K! or a control group.

At the end of the school year, York and Loeb (2014) collected survey responses from parents and teachers to investigate the intervention's impact on parental involvement. They found that treatment parents engaged in literacy activities at home with their child 0.22 to 0.34 standard deviations more than control parents and were 0.13 to 0.19 standard deviations more involved at preschool. To investigate the impact the intervention had on children's literacy development, York and Loeb (2014) collected scores from the Phonological Awareness Literacy Screening (PALS), a criterion-referenced test the school district administers to its early education students every spring.[14] They found that children in treatment families score 0.344 standard deviations higher on the letter sounds subtest and 0.205 standard deviations higher on a measure of lower-case alphabet knowledge. However, there were no significant impacts on measures of name writing, upper-case letter knowledge, beginning word sounds, print and word awareness, rhyme awareness, and a summed score of all the PALS subtests. For the sample of students that progressed to higher level subtests of PALS, there were significant impacts on the upper-case letter subtest and the summed score. There was limited evidence that READY4K! had differential impacts across family characteristics.

[14]Note that since this study did not report results from a norm-referenced outcome, it was not included in our tables and analysis.

Fryer, Levitt, and List (2015) conducted a parental incentive experiment in Chicago Heights – a prototypical low performing urban school district – by starting a parent academy that distributed nearly $1 million to 257 families (these numbers include treatment and control). There were two treatment groups, which differed only in when families were rewarded, and a control group. Parents in the two treatment groups were paid for attendance at Parent Academy sessions which were designed as information sessions to aid parents in educating their children, proof of homework completion, and the performance of their children on benchmark assessments. The only difference between the two treatment groups is that parents in one group were paid in cash or via direct deposits (hereafter the "cash" condition) and parents in the second group received the majority of their incentive payments via deposits into a trust account which can only be accessed if and when the child enrolls in college (the "college" incentive condition). Eleven project managers and staff worked together to ensure that parents understood the particulars of the treatment; that the parent academy program was implemented with high fidelity; and that payments were distributed on time and accurately.

Across the entire sample, the impact on cognitive test scores of being offered a chance to participate in the parental incentive is 0.119σ (with a standard error of 0.094). These estimates are non-trivial, but smaller in magnitude than some classroom based interventions. For instance, the impact of Head Start on test scores is approximately 0.145σ. The impact of the Perry Preschool intervention on achievement at 14 years old is 0.203σ. Given the imprecision of the estimates, however, our results are statistically indistinguishable both from these programs and from zero. The impact of the "college" and "cash" incentive schemes are nearly identical.

Fryer, Levitt, and List (2015) report that the impact of being offered a chance to participate in our parental incentive scheme on non-cognitive skills is large and statistically significant (0.203σ (0.083)). These results are consistent with Kautz et al. (2014), who argue that parental investment is an important contributor to non-cognitive development. Again, the "cash" and "college" schemes yield identical results.

They complement our main statistical analysis by estimating heterogeneous treatment effects across a variety of pre-determined subsamples that we blocked on experimentally. Two stark patterns appear in the data. The first pattern is along racial lines: Hispanics (48 percent of the sample) and whites (8 percent of the sample) demonstrate large and significant increases in both cognitive and non-cognitive domains. For instance, the impact of the parent academy for Hispanic children is 0.367σ (0.133) on our cognitive score and 0.428σ (0.122) on our non-cognitive score. Among the small sample of whites, the impacts are 0.932σ (0.353) on cognitive and 0.821σ (0.181) on non-cognitive. The identical estimates for blacks are actually negative but statistically insignificant on both cognitive and non-cognitive dimensions: -0.234σ (0.134) and -0.059σ (0.129), respectively. Importantly, p-values on the differences between races are statistically

significant at conventional levels. We explore a range of possible hypotheses regarding the source of the racial differences (extent of engagement with the program, demographics, English proficiency, pre-treatment scores), but none provide a convincing explanation of the complete effect.

The second pattern of heterogeneity in treatment that we observe in the data relates to pre-treatment test scores. Students who enter our program below the median on non-cognitive skills see no benefits from our intervention in either the cognitive or non-cognitive domain. In stark contrast, students who enter our parent academy above the median in non-cognitive skills experience treatment effects of roughly 0.3 standard deviations on both cognitive and non-cognitive dimensions. If we segment children by both cognitive and non-cognitive pre-treatment scores, the greatest gains are made on both the cognitive and non-cognitive dimension by students who start the program above the median on non-cognitive skills and below the median on cognitive skills.

3.1.3 Meta-Analysis

Early childhood interventions have amassed considerable popular and political support. Yet, like other initiatives to improve human capital, they are not a panacea. For example, St. Pierre et al. (1997) find no positive effects in a national evaluation of the Comprehensive Child Development Program (CCDP). The CCDP delivers early and comprehensive services to low-income families with the aim of enhancing the development of the children in these families and helping the parents achieve economic self-sufficiency. The CCDP model revolves around the ideas that one should intervene as early as possible in children's lives, involve the entire family in an intervention, deliver comprehensive services to address the needs of young children, enhance parents' ability to contribute to their child's development, help parents achieve economic and social self-sufficiency, and ensure that families have access to all of these resources until their children enter elementary school. In their evaluation, St. Pierre et al. (1997) found no significant differences between families that were randomly assigned to a CCDP treatment group or a control group. CCDP had no impacts on measures of mothers' economic self-sufficiency or their parenting skills and CCDP had no effects on the cognitive or social emotional development of the children included in the study.

Still, early childhood investments are considered to be one of the least risky ways to increase academic achievement (Heckman 2008). Combining the 44 randomized studies in early childhood over the past 50 years, the random effects coefficients are 0.111σ (0.031) for math interventions and 0.189σ (0.027) for reading. Of the 64 treatment effects recorded in these randomized studies, 21 were statistically positive; zero were statistically negative and 43 were statistically indistinguishable from zero.[15]

[15] We consider an effect size statistically positive or negative if it is statistically significant at the 10% level.

3.2 Home Environment

There is an ongoing debate as to whether efficient production of human capital should focus on improving the environment in which a child lives or the environment in which they learn. Proponents of the school-centered approach refer to anecdotes of excellence in particular schools or examples of other countries where poor children in superior schools outperform average Americans (Chenoweth 2007). Advocates of the community-focused approach argue that teachers and school administrators are dealing with issues that originate outside the classroom, citing research that shows racial and socioeconomic achievement gaps are formed before children ever enter school (Fryer and Levitt 2004; 2006), that mother's IQ is highly correlated with child achievement (Fryer and Levitt 2013; Wilson and Matheny 1983; Yeates et al. 1983) and that one-third to one-half of the racial achievement gap can be explained by family-environment indicators (Phillips et al. 1998; Fryer and Levitt 2004). In this scenario, combating poverty and having more constructive out-of-school time may lead to better and more-focused instruction in school. Indeed, Coleman et al. (1966), in their famous report on the equality of educational opportunity, argue that schools alone cannot treat the problem of chronic underachievement in urban schools.

In this subsection, we describe several attempts to provide households with more resources and to combat poverty, in an effort to increase student achievement. We organize this strand of literature in rough approximation to the "intensity" of treatment received – which ranges from providing parents with information to poverty reduction though welfare-to-work programs and tax reform to moving families to better neighborhoods. The literature is summarized in Appendix Table 2.

3.2.1 Parental Involvement

Parents matter. Using data from a national, cross-sectional study of children aged 8-12, Davis-Kean (2005) found significant correlations between parents' characteristics and parenting practices and students' math and reading achievement. Specifically, Davis-Kean (2005) found that strong correlations with students' achievement existed for parents' education levels, income, parental expectations, number of books owned, and many parental behaviors such as being warm and affectionate, responding positively, and giving praise. Jeynes (2005) conducts a meta-analysis of 41 studies that investigate the impact of parental involvement on the academic achievement of elementary students. He found that increases in parental involvement have an effect size on elementary students' academic outcomes of about 0.7 standard deviations. Jeynes (2007) conducts a similar meta-analysis using 52 studies that focus on secondary school students and finds the effect size of parent involvement to be about 0.5 standard deviations.

Although these results are interesting, they are are not causal estimates of the impact of parental involvement on students' outcomes. Levels of parental involvement are most likely correlated with many observable and unobservable characteristics of the parents and it is exceedingly difficult to rid these estimates of thorny issues of selection. Moreover, even if these estimates were causal, it is not obvious that it is possible for interventions to change parents' involvement to achieve these positive impacts on child outcomes.

In what follows, we summarize the literature on experiments to increase parental involvement using information treatments and incentive treatments.

A. Information

To better understand the impact of parental attitudes and school involvement on student achievement, Avvisati et al. (2014) conducted an experimental study on middle school students and parents in the educational district of Creteil, an eastern suburb of Paris, France. Classrooms randomly selected from 34 middle schools were offered a parental education program that taught parents how they can assist in their child's educational process.

This paper was motivated by a strong perception that disadvantaged parents have inadequate knowledge and confidence to be effective advocates for their children. The experiment sought to test if this could be improved by a simple intervention. The experimental program consisted of three afterschool meetings with parents, conducted by the school head. The first two sessions focused on how parents can help their children's education by participating at home and at school. The final session, which took place after the end-of-term report card, focused on how parents can adapt to their children's first term results. Of the 352 state-run middle schools in the Creteil district, 34 schools volunteered to participate in the program. Around two-thirds of schools in the study were "priority education", a label indicating a historically disadvantaged area.

Parents of 6th graders in the participating middle schools were asked, over a 6-week period, if they would like to sign up for the informational meetings. After the sign-up period closed, the list of registered families constituted the "volunteer families," creating two populations within each class in each school. There were no strong observable pre-treatment differences between volunteer and non-volunteer families. After registration closed, randomization began at the class-level of each school (meaning that roughly half of all classes were treated within each school). The randomization process defined four basic groups of families within each school: volunteers in treatment classes, non-volunteers in treatment classes, volunteers in control classes, and non-volunteers in control classes.

The study was interested in addressing three outcomes: (1) parental involvement attitudes and behav-

ior; (2) children's behavior as reflected by truancy, disciplinary record and work effort; and (3) children's academic results. To measure parental involvement attitudes and behavior, all families received a questionnaire on school-based involvement, home-based involvement, and parents' perception of the school. Student outcomes were reported by teachers and academic reports. Main subject teachers were also given a questionnaire regarding both parental attitudes and child's behavior/school performance.

The evidence found that the program was successful in significantly improving volunteer parent attitudes. Based on parents' and teachers' questionnaires, parental involvement by volunteer parents in treatment classes increased. Children of volunteer parents in treatment classes saw a vast improvement in school attitudes and discipline compared to control classes: truancy was lower by 1.1 half-days, treatment students were 4.6 percentage points less likely to be punished for disciplinary reasons (6.4% versus 11.0%), more likely to earn top marks for conduct, and, according to teacher questionnaire answers, were more likely to be agreeable in class and work diligently. In addition to having a direct impact on the students whose parents volunteered to participate, there were also spillover effects on students in treatment classrooms whose parents did not participate. Treatment had a statistically significant impact on non-volunteer students' absenteeism, probability of disciplinary sanctions, and marks for conduct. For students of volunteer parents, treatment increased average grades across all subjects by 0.08 standard deviations and increased academic performance as measured by the teacher survey. However, the intervention had no impact on grades for students whose parents did not volunteer and the intervention had no impacts on standardized test scores for any students. The findings overall suggested that parental involvement can be a significant input in student achievement—mostly through an impact on behavioral outcomes.

Evidence from Avvisati et al. (2014) and other studies suggest that it may be difficult to increase students' academic outcomes using parental interventions. Other parental experiments that focus on improving students' academic outcomes through parental tutoring also tend to have insignificant impacts on academic standardized measures (Warren 2009; Powell-Smith et al. 2000; Fantuzzo et al. 1995; Hirst 1972; Ryan 1964).

On average, parental information experiments increased student achievement by -0.001σ (0.021) on math scores and 0.034σ (0.050) on reading scores. Note that our search did not return any parental incentive experiments that focused solely on parents of K-12 students. Therefore, the estimates from our meta-analysis for parental involvement and parental information are identical.

B. INCENTIVES

The most well-known and well-analyzed incentive program for parents is PROGRESA. PROGRESA

was an experiment conducted in Mexico in 1998, which provided cash incentives linked to health, nutrition and education. The largest component of PROGRESA was linked to school attendance and enrollment. The program provided cash payments to mothers in targeted households to keep their children in school (Skoufias 2005). Programs based on the PROGRESA model have been replicated in New York City, Nicaragua, and Columbia.

Beginning in 1997, the Mexican government identified 506 rural communities on the basis of a "marginality index" gleaned from census data. Socio-economic data was collected from households within these communities to target households living in extreme poverty. In 1998, about two thirds of the identified localities were randomly selected to receive financial incentives under PROGRESA; the remaining localities served as controls. As a part of the program, households could receive up to $62.50 per month if children attended school regularly. The amount of incentive was higher for older children who had to attend 85% of all school days. The average amount of incentives received by any treatment household in the first two years of treatment was $34.80, which was 21% of an average household's income. Besides school attendance, PROGRESA also emphasized actual student achievement by making a child ineligible for the program if she failed a grade more than once (Skoufias 2005; Slavin 2010).

Schultz (2000) reports that PROGRESA had a positive impact on school enrollment for both boys and girls in primary and secondary school. For primary school children, PROGRESA increased school enrollment for boys by 1.1 percentage points and 1.5 percentage points for girls from a baseline level of approximately 90 percent. For secondary school students, enrollment increased by 7.2 to 9.3 percentage points for boys and 3.5 to 5.8 percentage points for girls, from a baseline level of approximately 70%. The author also reports that PROGRESA had an accumulated effect of 0.66 years additional schooling for a student from the average poor household. Taking the baseline level of schooling at face value, PROGRESA's 0.66 years accumulated effect translates into a 10% increase in schooling attainment.

Behrman, Sengupta, and Todd (2001) also analyze the data and report that PROGRESA children entered school at an earlier age, had less grade repetition and better grade progression. Treatment children also had lower dropout rates and once dropped out, they had a higher chance of re-entry into high school.

Opportunity NYC – based on PROGRESA – was an experimental conditional cash transfer program that was conducted in New York City. The program had three components: the Family Rewards component that gave incentives for to parents to fulfill responsibilities towards their children; the Work Rewards component that gave incentives for families to work; and the Spark component that gave incentives to students to increase achievement scores in classes. The program began in August 2007 and ended in August 2010 (see Morais de Sá e Silva 2008).

Riccio et al., 2013 analyze data from the Family Rewards component of the program during the first two years of treatment. Their analysis is based on 4,800 families with 11,000 children out of which half were assigned to treatment and the other half to control. Opportunity NYC spent $8,700 per family in treatment over three years. The experiment had an insignificant impact on every school outcome measured (Riccio et al. 2013).

3.2.2 Home Educational Resources

Education, like other industries, has evolved over the past few decades – due, in part, to technological change. With the introduction of computers, the internet, mobile wifi, and smart phones, teaching strategies have changed to utilize these technologies in the classroom. However, many children still lack access to these resources in their homes. One could imagine that the returns to household computers are quite high. Students can use them as a tool to efficiently complete assignments, learn new information, study, and use for other educational purposes. Despite these potential returns, it is also possible that households face constraints (e.g. credit or information) that prevent them from investing in household technology. This is supported by the fact that ownership of household computers and access to household internet is correlated with income (National Telecommunication and Information Administration 2011). Studies examining the impact of home computers on poor families using observational or quasi-experimental data have generated mixed results. Some studies find large positive effects (Attewell and Battle 1999; Fiorini 2010; Schmitt and Wadsworth 2006; Fairlie 2005; Fairlie, Beltran and Das 2010; Malamud and Pop-Eleches 2011) and some find evidence of small or even negative impacts (Fuchs and Woessmann 2004; Vigdor and Ladd 2010; Malamud and Pop-Eleches 2011). Fairlie and Robinson (2013) present causal estimates from the first ever randomized control experiment that investigates the impact of home computers.

In their experiment, Fairlie and Robinson investigate the educational impacts of randomly giving home computers to 1,123 students in grades 6 through 10 in California over the 2008-2009 and 2009-2010 school years. No students who participated in the study had home computers at baseline. Half of these students were randomly selected to receive free computers without any training or technological assistance. Fairlie and Robinson collected administrative data on student academic outcomes and demographics pretreatment and at the end of the school year (posttreatment). In addition, they conducted baseline and posttreatment surveys that included questions about computer usage, knowledge, homework time, and other important outcomes. Using this data, Fairlie and Robinson found that the experiment had large first-stage impacts. They found that treatment students were 55 percentage points more likely to have a computer at follow-up, 25 percentage points more likely to have Internet service, they reported using a computer 2.5 hours more

per week than control students' average of 4.2 hours, and almost all of this additional usage came from a computer at home. However, not all of the computer usage was for educational purposes. Relative to control students, treatment students used computers 0.80 hours more per week for schoolwork (control mean (CM) was 1.89 hours), 0.42 hours more for e-mail (CM = 0.25 hours), 0.80 hours more for games (CM = 0.84 hours), and 0.57 hours more for social networking (CM = 0.57 hours).

Despite these large first-stage impacts, Fairlie and Robinson find minimal evidence for impacts on educational outcomes. ITT estimates for the impact of home computers on grades in math, English/reading, social studies, and science classes are all close to zero and precisely estimated. With standard errors of approximately 0.04, they can rule out effect sizes on the scale of one-fourth of the difference between a "B+" or "B" with 95 percent confidence. Using quantile regressions, they show that these null effects exist across the entire posttreatment achievement distribution. Similarly, they find no evidence of impact on students' test scores or proficiency statuses from the California Standardized Testing and Reporting (STAR) program, total credits taken in the third quarter of the school year, total credits in the fourth quarter, unexcused absences, number of tardies, and if a student was still enrolled at the end of the school year. These zero effects are consistent with survey results that show treatment students did not change intermediate inputs and outcomes such as school effort, computer knowledge, and usage of important educational software.

There are other randomized field experiments that investigate the impact of providing additional resources to families – such as giving students books to read during the summer. Numerous studies suggest that summer vacation is a critical time for forming and widening achievement gaps in reading, particularly for the income-achievement gap.[16] Kim (2005) conducted an experimental study to examine the causal effects of a voluntary summer reading intervention on the reading skills of fourth-grade students in the Lake County Public School District, a large multi-ethnic school district located in a mid-Atlantic state. The district contains more than 100 elementary schools and is therefore organized into small subdistricts, each with its own superintendent. To be included in the sample, the subdistrict needed to contain high-poverty schools that administered Title I school-wide programs and contain multi-racial schools in which reading scores for black and Latino students contributed to the federal adequate yearly progress rating. The final sample included four Title I schools and the six non-Title I schools with the largest percentage of minority students.

This paper was motivated by inefficiencies in current voluntary reading policies and the little evidence in support of these programs. Additionally, finding a cost-effective reading intervention was important for policymakers and practitioners given the goals of federal education policy and mandates under the No Child

[16] See Heyns (1978), Cooper et al. (1996), Alexander et al. (2001), Broh (2004), Heyns (1987), Klibanoff & Haggart (1981), Murnane (1975), and Phillips et al. (1998). Fryer and Levitt (2004) is a notable example of a nationally representative sample that does not find "summer setback".

Left Behind Act.[17] The intervention addressed three main factors – access to books, students' reading levels, and students' reading preferences – that are likely to shape opportunities to read in the summer and affect reading outcomes. To increase access to books, each student in the treatment group received eight free books to read during the summer. Students' reading levels were based on performance on the reading section of the Iowa Test of Basic Skills and preferences were obtained through a survey distributed before the summer. A text-leveling system, the Lexile Framework, was used to provide books that were within each student's independent reading level using information about each student's reading level and reading preferences. With each book, students also received a postcard that asked students to check comprehension strategies used while reading the book and to obtain a signature from a parent or family member after reading a portion of the book aloud to the adult. Parents were instructed to mail each postcard back to the schools, regardless of if their student completed the book or not.

A total of 552 students received consent to participate in the study and took pretests in June 2005. These students were randomly assigned to treatment and control groups (282 treatment and 270 control) within their English Language Arts (ELA) classroom, and the author reports no statistically significant differences between the two groups at the beginning of the experiment on numerous demographic and achievement characteristics. Because of attrition, the final sample included 486 students (252 treatment and 234 control) at the beginning of the Fall in 2005. The intervention attempts to improve reading skills by increasing children's access to books, matching books to children's reading levels and preferences, and encouraging children to read orally with a parent/family member to practice.

To investigate if the intervention increased children's access to books at home and literacy-related activities during summer vacation, the author used a two-way ANOVA on both self-reported measures of book ownership and on literacy habits gathered from a survey conducted at the end of the summer. The results suggest that the intervention did not increase children's access to books nor the amount of silent reading. However, children in the treatment group reported significantly more oral-reading at home with family members than the control group children. For fall reading outcomes, ITT regressions showed no significant differences between the treatment and control groups on a grade level measure of oral-reading fluency. However, treatment had a 0.08σ (0.04) impact on students' standardized reading test scores and there were differential effects by race. Treatment increased test scores by 0.22σ (0.09) for black students, 0.14σ (0.08) for Latino students, and 0.17σ (0.11) for Asian students. Further, the magnitude of the treatment effect was largest among lower performing students, and there were no significant interactions between the treatment and measures of reading ability or ownership of books

[17] Note that the No Child Left Behind Act was superseded by the Every Student Succeeds Act in December 2015.

Similarly, Allington et al. (2010) conducted a randomized trial in 17 high-poverty schools in Florida where treatment students selected 12 books from a book fair to receive for summer reading. Allington et al. (2010) found a 0.046σ (0.033) annual impact over the three years of the experiment.

The meta-coefficients for home educational resource experiment were -0.060σ (0.050) for math scores and 0.015σ (0.014) for reading scores.

3.2.3 Poverty Reduction Experiments

One of the most often articulated explanations for the racial and ethnic achievement gaps that exist across developed countries is poverty. For families with higher income, it is easier to provide their children with resources and raise them in environments that are conducive for learning. Poverty places constraints on key factors of achievement such as health care, nutrition, child care, in-home educational resources, safe neighborhoods, good schools, and college education (Brooks-Gunn and Duncan 1997; Evans 2004; Magnuson and Duncan 2002; McLoyd 1998). In America, 42 percent of black children and 37 percent of Hispanic children experience poverty while only 10 percent of white children are exposed to these hardships (Duncan and Magnuson 2005). Studies suggest that this racial income gap is an important source of variation that can account for large proportions of raw racial achievement gaps (Duncan and Magnuson 2005; Fryer and Levitt 2004; Phillips et al. 1998; Brooks-Gunn et al. 2003).

This subsection discusses the impact on student achievement of experiments that attempted to reduce poverty through tax reform and work programs.

A. Tax Reform

Maynard and Murnane (1979) discuss two mechanisms by which welfare reform could affect children's educational achievement by altering home environment: product inputs and time inputs. Product inputs are things such as food, health care, and books. Examples of time inputs are time parents spend talking to, playing with, and reading to their children. They assume that product inputs are positively related to family income and that time inputs are positively related to time not working. Maynard and Murnane investigate the educational impacts of a program that affects both of these mechanisms in unison by increasing families' income and incentivizing them not to work.

In the early 1970s, the Gary Income Maintenance Experiment was conducted by Indiana University under contracts with the U.S. Department of Health, Education, and Welfare and the Indiana State Department of Public Welfare. Families who voluntarily enrolled and had at least one child under the age of 18 were randomly assigned to negative income tax conditions or control. Of the 1,799 eligible families, 57 percent

were assigned to one of four negative income tax plans for three years. These tax plans were a combination of two tax rates (40 or 60 percent) and two guaranteed income levels (about three fourths of the poverty level or equal to the poverty level). The lower guarantee level was about $1,000 a year more than the support level of the Indiana Aid to Families with Dependent Children program.[18] The tax rate is the amount by which the negative income tax payment is reduced for each dollar of income that a family earns.

The sample for the Gary Income Maintenance Experiment was not nationally representative. All children were black and three-fifths of them lived in female-headed households. In addition, the average family had a much lower income compared to the national average (the average annual income of families in the Gary experiment was only $5,200 and the national average at that time was $9,433) and over 40 percent of the Gary families were living below the poverty line.

Maynard and Murnane investigate the impact of a Gary family's assignment to any one of the treatment arms on educational outcomes of students in grades 4-10 at the end of the experiments (three years after randomization). They found that treatment increased students' standardized reading test scores by 0.23 standard deviations on the Iowa Test of Basic Skills in grades 4-6 but had no significant impact for students in grades 7 -10. They found no evidence that treatment had an effect on grade point average of the younger students, but found that it significantly decreased grade point average for the older students. They also found no evidence that treatment had an impact on the number of days absent for either group of students.

To better understand these results, Maynard and Murnane also investigate the mechanisms by which the experiment might have affected school performance. They found that the Gary experiment had a significant first stage impact on total family income, but caused minimal change in the number of hours worked. Treatment families on average had their incomes increased by $2,000 per year (approximately a 50 percent increase). For married mothers, there was no change in hours worked per week. For female family heads, there was a decrease of about two hours per week. Additionally, experimental families that lived in public housing before randomization were more likely to move to private dwellings than control families that lived in public housing prior to randomization. However, there was no statistical difference in mobility in the pooled sample.

B. Work Programs

Michalopoulos et al. (2002) evaluated another poverty reduction program, called the Self-Sufficiency Project (SSP), that attempted to make work more appealing than welfare to long-time welfare recipients in the Canadian provinces of British Columbia and New Brunswick by providing them with wage subsidies. New Brunswick is located in eastern Canada and is bordered by the U.S. state of Maine on it's western

[18] In 1972, the official poverty level for a four-person non-farm family was $4,275.

boundary. New Brunswick has a population of 750,000, a majority of its inhabitants speak English as their first language, and has a per capita GDP of 42,600 Canadian dollars. British Columbia is located in western Canada and is bordered by the U.S. states of Alaska, Washington, Idaho, and Montana. British Columbia has a population of 4,400,000, an official language of English, and a per capita GDP of 47,500 Canadian dollars.[19]

The study randomly assigned 6,000 single parents from British Columbia and New Brunswick, who had been on income assistance for at least one year, to a treatment and control group. Treatment parents were eligible to participate in SSP and control parents were not. Parents enrolled in SSP received a monthly earnings supplement conditional on starting a full-time job and leaving income assistance. The earnings supplement was in addition to earnings from employment for up three years, as long as the parent continued to be employed full-time and remained off of income assistance. After random assignment, treatment parents had one year to find full-time employment (at least 30 hours per week) and leave income assistance to enroll in SSP. After enrollment, the supplement participants received was half of the difference between their earnings and an earnings benchmark (the benchmark varied by location and year, but was $30,000 in New Brunswick and $37,000 in British Columbia for the first year of the experiment). This supplement was not affected by unearned income, earnings of other family members, and number of children. This supplement would essentially double the wage of many low-wage workers.

Michalopoulos et al. (2002) found significant first-stage impacts. Thirty-six percent of single parents that were offered participation found full-time employment and took-up the supplement during the year long eligibility window. Of those that participated in SSP, the average parent received the supplement for 22 months over the three years of the program and received more than $18,000 in supplements over that that time. SSP increased treatment parents' probability of employment throughout the duration of the program and reduced income assistance payments received by these families. As a result, treatment parents earned nearly $3,400 more than control members. Total income (supplements, earnings, and income assistance) increased by $6,300 for the average treatment family. These impacts reduced the proportion of treatment parents below Canada's low income cut-offs by 10 percentage points. Although these large impacts were observed during the program, these impacts did not persist after the completion of SSP. By six years after random assignment (two years after all treatment parents would have stopped receiving supplements), treatment and control parents were equally likely to be employed and had similar average earnings.

Michalopoulos et al. (2002) also investigated the impact of SSP on the outcomes of the parents' children. They found differential treatment effects by the age of the child at the beginning of treatment. For children

[19] Statistics come from the 2011 Canadian census (Statistics Canada 2013).

who were 1 or 2 years old at the time of random assignment, SSP had no effects on their performance on a standardized test of vocabulary skills (Peabody Picture Vocabulary Test) and achievement as reported by parents. For children who were 3 or 4, SSP increased students' scores on a math skills test and parental-reported achievement. Treatment children who were 13, 14, or 15 at the time of random assignment reported doing worse in school and committing more minor acts of delinquency during the program, but these effects faded away after parents were no longer eligible for the supplement. Finally, for older adolescents, SSP had no impacts on educational, crime, or work related outcomes, but these students were significantly more likely to have babies. Other than the effects stated above for the young adolescents, there was no evidence of SSP having any impacts on health, behavior, and the emotional well-being of students in the study.

In a large analysis of welfare-to-work programs in the U.S., Hamilton et al. (2001) conduct a national evaluation of the long-term effects of 11 welfare-to-work programs on the recipients and their children. The evaluation investigates the effectiveness of two different types of pre-employment strategies, Labor Force Attachment (LFA) and Human Capital Development (HCD). LFA welfare-to-work programs typically consist of short-term job search and encourage welfare participants to find employment quickly. HCD programs emphasize investment in longer-term skills and typically encourage participants to enroll in training or basic education programs. Hamilton et al. (2001) use data on over 40,000 single parents (mostly female) and their children who were randomly assigned to these programs in sites across the nation to investigate the impact of LFA and HCD programs

Over the course of the five-year follow-up period, a majority of control group members worked at some point. For example, 88 percent of the control parents from the Grand Rapids site were employed at some point. In Oklahoma City, 79 percent worked at some point during that time and 66 percent worked in Riverside. Although there was a high percentage of control parents that ever worked, treatment parents still worked during more calendar quarters on average than control parents in 9 of 11 programs. Similarly, in 9 of 11 programs, treatment parents on average had higher total earnings. Typically, Hamilton et al. (2001) found that employment-focused programs produced employment and earnings effects almost immediately while education focused programs did not have effects until a year or more after randomization. However, when directly comparing the LFA and HCD programs in the sites where they were run side by side, employment and earnings levels over the five years were very similar.

By the end of the follow-up period, almost all control families were off of welfare and the average control group member remained on assistance for only 2 to 3 years. However, both treatment types still reduced months on welfare relative to the control averages and there is some evidence that LFA treatment members left welfare assistance at a faster pace than HCD participants. These reductions in welfare usage

appear to directly offset the increase in salary. Despite increasing earnings, treatment largely had no impact on total combined income (earnings, welfare and Food Stamp payments, and Earned Income Tax Credits).

Hamilton et al. (2001) also investigate if the welfare-to-work programs had effects on family circumstances and children's well-being. They found that there was no evidence of impacts on health care coverage, marriage rates, and few impacts on household composition and living arrangements. However, adults assigned to a welfare-to-work program were less likely to report recent physical abuse at the end of the experiment.

To investigate impacts on children, the researchers conducted a Child Outcomes Study in six of the programs (three different sites that each offered LFA and HCD programs). These studies included almost 50 measures of children's academic functioning, health, social skills, and behavior for children who were preschool age at randomization. The authors report that 15 percent of these tests produced statistically significant differences, but the sign and magnitudes were rarely consistent across sites. For example, the estimates from the Atlanta LFA and HCD programs suggested favorable impacts on social skills and behavior for young children, but the Grand Rapids programs revealed negative effects. For older children, the programs led to few significant results. However, whenever results for these students were significant, they tended to be unfavorable. For example, a HCD program at one site increased the likelihood of dropping out, increased percentage of adolescents who had a physical, emotional, or mental condition that impeded their mother's ability to go to work, and increased teenage pregnancies among families with lower levels of education. Note again that no effects varied consistently by program approach or site for adolescents.

Summarizing the literature on poverty reduction attempts to increase student achievement, the meta-coefficients for this strand of literature are 0.008σ (0.029) and 0.016σ (0.024). And, perhaps more telling, there is not one experiment that generates statistically significant positive effects on standardized test scores.[20]

3.2.4 Neighborhood Quality

A more nuanced version of the "poverty is first-order" argument is that the mechanism by which disadvantage affects achievement is not directly through income – hence, addressing the income problem has no real impact – but through what sociologists refer to as "a culture of poverty." This theory argues that the poor are not simply lacking resources, but are also immersed in a culture that develops mechanisms or has social institutions that perpetuate poverty (Moynihan 1969; Harrignton 1982). Taking the culture of poverty paradigm at face value, the randomized field experiment that one would ideally conduct would be to move

[20]However, note that some of these studies found impacts for sub samples of the participants or on non-cognitive outcomes.

families from high-poverty to low-poverty neighborhoods – particularly when children are young. This is precisely what the Moving to Opportunity (MTO) randomized housing mobility experiment did – one of the most pathbreaking experiments of our generation.

From 1994 to 1998, MTO enrolled 4,604 poor families with children residing in public housing in high-poverty neighborhoods of Baltimore, Boston, Chicago, Los Angeles, and New York City. Families were randomly assigned to three groups: (1) the experimental voucher group, which received a restricted housing voucher that could be used to pay for private rental housing initially restricted to be in a low-poverty area (a census tract with under a 10 percent poverty rate in 1990) and some housing-mobility counseling; (2) the Section-8 only voucher group, which received regular Section 8 housing vouchers with no MTO relocation constraint; and (3) a control group, which received no assistance through MTO. Across the MTO treatment sites, 61 percent of household heads were non-Hispanic blacks, 31 percent were Hispanic, and nearly all households were female-headed at baseline. About half of the experimental voucher group and 63 percent of the Section 8-only voucher group were able to obtain leases and move with an MTO voucher (the compliance rate). The MTO families were tracked for 15 years using administrative data as well as major interim (4 to 7 years after random assignment) and long-term (10 to 15 years after random assignment) follow-up surveys and analyses (Kling, Liebman, and Katz 2007; Sanbonmatsu et al. 2011). MTO generated large and persistent improvements in residential neighborhoods for the treatment groups (especially the experimental voucher group) relative to the control group but only modest changes in school quality. The average MTO family lived at baseline in a neighborhood with a 53 percent poverty rate. MTO led to a 9 percentage point decline in the duration-weighted average tract poverty rate over the 10-15 year follow-up period for the experimental voucher group relative to the control group.

In stark contrast, MTO only modestly improved school quality for the MTO treatment groups. From the time of random assignment until the long-term follow-up, the experimental voucher group children attended schools that outperformed their control group peers by only 3 percentile points on state exams, and the Section-8 only voucher group children attended schools that performed just 1 percentile point higher. MTO treatment group students also typically remained in schools where the majority of the students were low-income and minority. MTO reduced the share of students eligible for free or reduced-price lunch by 4 percentage points for the experimental voucher group. Although it is difficult to compare the size of the neighborhood quality change to that of the school quality change, MTO appears to have a larger improvement on neighborhood quality. The MTO treatment groups experienced more than twice as large a reduction in the share of poor residential peers as compared to poor school peers and more than three times as large an improvement in percentile rank in the national Census-tract poverty distribution for their neighborhoods than

in the state test score distribution for their schools. Many of the MTO movers remained in the same school districts and very similar schools. MTO also had no significant impact on adult economic self-sufficiency or family income at the interim or long-run follow-ups. Thus, an analysis of the impacts of MTO treatments on child outcomes comes close to getting at the pure effects of changes in home and neighborhood conditions for disadvantaged kids (with little change in schools or family economic resources): $\frac{\partial Y}{\partial H}$ in our framework.

The MTO voucher treatments did not detectably impact parent's economic outcomes, but they did significantly and persistently improve key aspects of mother's (adult female's) mental and physical health including substantial reductions in psychological distress, extreme obesity, and diabetes (Ludwig et al. 2011; Sanbonmatsu et al. 2011). MTO movers also experienced significant increases in adult subjective well-being with larger gains for adults from sites where treatment induced larger reductions in neighborhood poverty (Ludwig et al. 2012). For female youth, MTO treatments similarly led to persistent and significant improvements in mental health (including substantial reductions in psychological distress) and marginally significant improvements in physical health, but there were no long-term detectable health impacts for male youth (Kling, Liebman and Katz 2007; Sanbonmatsu et al. 2011). Analyses 4 to 7 and 10 to 15 years after randomization found that MTO produced no sustained improvements in academic achievement, educational attainment, risky behaviors, or labor market outcomes for either female or male children, including those who were below school age at the time of random assignment. Interestingly though, using administrative data from tax returns through 2012, Chetty et al. (2016) show that the Moving to Opportunity experiment has had large impacts on early-adulthood outcomes for children who were younger than 13 years old at randomization. In their mid-twenties, these individuals have 31% higher incomes, have higher college attendance rates, are less likely to be single parents, and live in better neighborhoods relative to similar individuals in the control group. For children who were older than 13 years old at randomization, MTO had no positive long-term impacts.

The MTO findings imply that large improvements in neighborhood conditions for poor families (at least in the range feasible with Section 8 housing vouchers) alone do not produce noticeable gains in children's short-term socioeconomic and educational outcomes but can have substantial impacts on important long-term outcomes for children who were exposed to these environment changes before the age of 13. The lack of school quality changes induced by treatment are suggestive of a key role for schools in children's short-term educational outcomes and risky behaviors.

3.2.5 Meta-Analysis

Combining all the randomized studies for home environment, the random effects coefficients are -0.004σ (0.008) for math interventions and 0.010σ (0.007) for reading. Astonishingly, the only study that had a statistically positive pooled impact was an unpublished dissertation. These results show that interventions that directly impact parents and households have struggled to have immediate effects on students' achievement outcomes.

3.3 Randomized Field Experiments in K-12 Schools

Thus far, the literature suggests that early childhood experiments yield strong effects, but policies designed to reduce poverty, increase work opportunities, or increase neighborhood quality do little to effect the production of human capital of school children. In this section, we explore 105 randomized field experiments conducted in K-12 schools. The literature is summarized in Appendix Table 3.

We categorize experiments into four buckets: student-based interventions, teacher-based interventions, management reforms, and "market-based" reforms.

3.3.1 Student-Based Interventions

A. FINANCIAL INCENTIVES

Perhaps the most natural way to increase human capital production – at least to an economist – is to change the incentives of school children to exert effort. Of course, rational agents – even little ones – internalize the returns to education that accrue in the labor market. Yet, if agents discount the future or are otherwise "boundedly rational", individual effort may be below the optimum. Financial incentives offer a chance to bridge the gap and thereby increase effort.

There is a nascent but growing body of scholarship on the role of incentives in primary, secondary, and post-secondary education around the globe (Angrist et al. 2002; Angrist and Lavy 2009; Kremer, Miguel, and Thornton 2009; Behrman, Sengupta, and Todd 2005; Angrist, Bettinger, and Kremer 2006; Angrist, Lang, and Oreopoulos 2009; Fryer 2011; Fryer and Holden 2013; Barrera-Osorio et al. 2011; Bettinger 2012; Hahn, Leavitt, and Aaron 1994; Jackson 2010). We describe a subset of the literature below.

Incentives in Primary Schools

Psychologists argue that children understand the concept of money as a medium of exchange at a very young age (Marshall and MacGruder 1960), but the use of financial incentives to motivate primary school

students is exceedingly rare.[21] Bettinger (2012), who evaluates a pay-for-performance program for students in grades three through six in Coshocton, Ohio, is one notable exception. Coshocton is ninety-four percent white and fifty-five percent free/reduced-price lunch. Students in grades three through six took achievement tests in five different subjects: math, reading, writing, science, and social studies. Bettinger (2012) reports a 0.13σ increase in math scores and no significant effects on reading, social science, or science. Pooling subjects produces an insignificant effect.

Fryer (2011) and Fryer and Holden (2013) also describe student financial incentive experiments that target primary students that were conducted during the 2007-2008 and 2010-2011 school year in Dallas and Houston, respectively. In Dallas, Fryer (2011) paid second graders $2 per book to read and pass a short computer-based comprehension quiz on the book in Accelerated Reader (AR), a software program that has quizzes for 80,000 trade books, all major reading textbooks, and leading children's magazines. Students were allowed to select and read books of their choice at the appropriate reading level and at their leisure, not as a classroom assignment. The books came from the existing stock available at their school (in the library or in the classroom). To reduce the possibility of cheating, quizzes were taken in the library on a computer and students were only allowed one chance to take a quiz. Data on the number of books read for students in control schools in Dallas was not available because control schools did not have consistent access to (AR). In total, the experiment distributed $42,800 (21,400 quizzes passed) to 1,777 children across the 21 treatment schools.

Paying students to read books yielded a treatment effect of 0.012σ (0.069) in reading and 0.079σ (0.086) in math. The key result from this analysis emerges when one partitions students in Dallas into two groups based on whether they took the exam administered to students in bilingual classes (Logramos) or the exam administered to students in regular classes (Iowa Test of Basic Skills). Splitting the data in this way reveals that there is a 0.173σ (0.069) increase in reading achievement among English speaking students and a 0.118σ (0.104) decrease in reading achievement among students in bilingual classes. When we aggregate the results in our main analysis this heterogeneity cancels itself out. Similarly, the treatment effect for students who are not English Language Learners is 0.221σ (0.068) and -0.164σ (0.095) for students who are English Language Learners.

Fryer and Holden (2013) conducted a randomized field experiment in fifty traditionally low-performing public schools in Houston, Texas – providing financial incentives to fifth grade students, their parents, and

[21]The use of non-financial incentives – gold stars, aromatic stickers, certificates, and so on – are a more common form of incentive for young children. Perhaps the most famous national incentive program is the Pizza Hut Book It! Program which provides one-topping personal pan pizzas for student readers. This program has been in existence for 25 years, but never credibly evaluated.

their teachers in twenty-five treatment schools. Students received $2 per math objective mastered in Accelerated Math (AM), a software program that provides practice and assessment of leveled math objectives to complement a primary math curriculum. Students practice AM objectives independently or with assistance on paper worksheets that are scored electronically and verify mastery by taking a computerized test independently at school. Parents also received $2 for each objective their child mastered and $20 per parent-teacher conference attended to discuss their student's math performance. Teachers earned $6 for each parent-teacher conference held and up to $10,100 in performance bonuses for student achievement on standardized tests. In total, the experiment distributed $51,358 to 46 teachers, $430,986 to 1,821 parents, and $393,038 to 1,734 students across the 25 treatment schools.

The experimental results raise a number of questions. On outcomes for which direct incentives were provided, there were very large and statistically significant treatment effects. Students in treatment schools mastered 1.087 σ (0.031) more math objectives than control students. On average, treatment parents attended almost twice as many parent-teacher conferences as control group parents. And, perhaps most important, these behaviors translated into a 0.081σ (0.025) increase in math achievement on Texas's statewide student assessment. The impact of our incentive scheme on reading achievement (which was not incentivized) is -0.077σ (0.027), however, offsetting the positive math effect. These results are consistent with the classic multitasking and job design work of Holmstrom and Milgrom (1991).

Interestingly, there is significant heterogeneity in treatment effects as a function of pretreatment test scores. Higher-achieving students (measured from pretreatment test scores) master 1.66σ more objectives, have parents who attend two more parent-teacher conferences, have 0.228σ higher standardized math test scores and equal reading scores relative to high-achieving students in control schools. Conversely, lower-achieving students master 0.686σ more objectives, have parents who attend 1.5 more parent-teacher conferences, have equal math test scores and 0.165σ *lower* reading scores. Put differently, higher-achieving students put in significant effort and were rewarded for that effort in math without a deleterious impact in reading. Lower-achieving students also increased effort on the incentivized task, but did not increase their math scores and their reading scores decreased significantly. These data suggest that the classic "substitution effect" may depend on baseline ability.

Two years after removing the incentives, the treatment effect for high-achieving students is large and statistically significant in math [0.271σ (0.110)] and is small and statistically insignificant in reading. In stark contrast, low-achieving students have no treatment effect in math but a large, negative, and statistically significant treatment effect on reading [-0.219σ (0.084)]. These data suggests that there may be long-run impacts of multitasking through learning, dynamic complementarities, or both.

Incentives in Secondary Schools

Fryer (2011) and Fryer (2010) describe the results of a series of randomized field experiments on financial incentives and secondary student achievement. In NYC, seventh grade students were paid for performance on a series of ten interim assessments administered by the NYC Department of Education to all students. In Chicago, ninth graders were paid every five weeks for grades in their core courses. In Washington, DC, sixth, seventh, and eighth grade students were paid for their performance on a metric that included attendance, behavior, and three inputs to the production function chosen by each school individually.

The results reported in Fryer (2011) and Fryer (2010) are surprising. The impact of financial incentives on state test scores is statistically zero in each city. In NYC, paying students for performance on standardized tests yielded treatment effects of 0.004σ (0.017) in reading and -0.031σ (0.037) in mathematics in seventh grade and similar results for fourth graders. In Chicago, rewarding ninth graders for their grades had no effect on achievement test scores in math or reading. In Washington, DC, where students were paid for various inputs to the educational production function, we observed an impact of 0.152σ (0.092) in reading and 0.114σ (0.106) in mathematics.

Overall, these estimates suggest that incentives are not a panacea – but we cannot rule out small to modest effects (e.g., 0.10σ) which, given the relatively low cost of providing financial incentives to students, have a positive return on investment.

Perhaps even more surprisingly, financial incentives had little or no effect on the outcomes for which students received direct incentives, self-reported effort, or intrinsic motivation. In NYC, the effect of student incentives on the interim assessments is, if anything, negative. In Chicago, where we rewarded students for grades in five core subjects, the grade point average in these subjects increased 0.093σ (0.057) and treatment students earned 1.979 (1.169) more credits (half a class) than control students. Both of these impacts are marginally significant. Incentives in Washington D.C. had no significant impacts on attendance rates, report card grades, or behavioral incidents.

Treatment effects on an index of "effort," which aggregates responses to survey questions such as how often students complete their homework or asks their teacher for help, are small and statistically insignificant across all cities, though there may have been substitution between tasks. Finally, using the Intrinsic Motivation Inventory developed in Ryan (1982), Fryer (2011), Fryer (2010), and Fryer and Holden (2013) find little evidence that incentives decrease intrinsic motivation.

Taken together, the randomized field experiments involving financial incentives for students have generated a rich set of facts. Paying second grade students to read books significantly increases reading achieve-

ment for students who take the English tests or those who are not English Language Learners, and is detrimental to non-English speakers. Paying fifth graders for completing math homework significantly increases their math achievement and significantly decreases their reading achievement. All other incentive schemes had, at best, small to modest effects – none of which were statistically significant.

B. Non-Financial Incentives and Returns to Schooling

Fryer (2013) describes a large and innovative randomized field experiment which grew out of a partnership between three large organizations: Tracphone – the largest pre-paid mobile phone provider in the US, Droga5 – an internationally recognized advertising firm, and the Oklahoma City Public Schools. The experiment, entitled "The Million", was designed to provide accurate information to students about the importance of education on future outcomes such as unemployment, incarceration, and wages and to provide incentives to read books through free cell phones and minutes to talk and text.

Students in three treatment groups were given cellular phones free of charge, which came pre-loaded with 300 credits that could be used to make calls or send text messages. Students in the main treatment arm received 200 credits per month to use as they wanted and received one text message per day on the link between human capital and future outcomes delivered at approximately 6:00 P.M. A second treatment arm provided the same text messages as well as non-financial incentives – credits to talk and text were earned by reading books outside of school. A third treatment arm allowed students to earn credits by reading books and included no information. There was also a pure control group that received neither free cellular phones, information, nor incentives.

On direct outcomes for students in the informational treatments, Fryer (2013) reports students' ability to answer specific questions about the information provided in the text messages. Treatment effects were uniformly positive. Pooling across both informational treatments, treatment students were 4.9 (2.7) percentage points more likely to correctly identify the wage gap between college graduates and college dropouts, 17.9 (3.8) percentage points more likely to correctly identify the relationship between schooling and incarceration, and 17.8 (3.8) percentage points more likely to answer both questions correctly. As a robustness test, we included a "placebo" question on the unemployment rate of college graduates, about which students never received information. The difference in the probability of answering this question correctly between informational treatments and the control group was trivial and statistically insignificant. Moreover, 54 percent of control students believe that incarceration rates for high school graduates and dropouts are "no[t] differen[t]" or "really close", suggesting that students in Oklahoma Public Schools do not have accurate knowledge of the returns to schooling.

Results are mixed for indirect outcomes such as self-reported effort, state test scores, and attendance. Across the treatment arms, ITT estimates of the effect of treatment on self-reported effort are positive and statistically significant for both incentives and information arms. For instance, students in the information treatment were 15.1 (3.7) percentage points more likely to report feeling more focused or excited about doing well in school and 7.0 (3.7) percentage points more likely to believe that students were working harder in school.

In stark contrast, on all administrative outcomes – math or ELA test scores, student attendance, or behavioral incidence – there was no evidence that any treatment had a statistically significant impact, though due to imprecise estimates one cannot rule out small to moderate effects which might have a positive return on investment.

Another potentially powerful incentive is offering students a chance to earn college credit or college degrees while still in high school. The idea is that offering college credit will increase student incentives to exert effort and increase access to college for some students. Over 240 schools nationwide, called Early Colleges, have already adopted this model. Early Colleges combine a rigorous high school curriculum along with the potential to earn two years of college credit or a two-year degree during high school. Most Early Colleges target underserved students and team up with colleges to offer this opportunity at no or low cost to the students. Berger et al. (2013) utilize the random lottery admission process of some Early Colleges to investigate the causal impact of Early Colleges on students' outcomes.

In their study, Berger et al. (2013) used administrative and survey data from ten Early Colleges that conducted random admission lotteries for the 2005-06, 2006-07, or 2007-08 school years. Comparing lottery winners to lottery losers, they were able to estimate causal impacts on high school completion, college enrollment, college degrees earned, standardized test scores, and high school and college experiences. High school outcome and student demographic data were obtained directly from the administrative records of the schools involved in the study; for college outcomes, students were matched to records in National Student Clearinghouse; data on high school and college experiences as well as college credits obtained while in high school came from a student survey that the researchers administered to students in eight of the Early Colleges. The final sample included 2,458 students for the administrative outcomes and 1,294 students for the survey outcomes.

Using this data, Berger et al. (2013) found that students offered admission to Early Colleges were significantly more likely to graduate high school and ever attend college than students who lost the lottery. Eighty-six percent of Early College students graduated high school compared to 81 percent of lottery losers and 80 percent of Early College students ever enrolled in college whereas only 71 percent of comparison

students did. Note that these numbers only reflect enrollment observed during the study period, 2005-2011, and that the gap in enrollment rates between lottery winners and lottery losers was decreasing as time went on. For example, for cohorts with six years of data available, the gap four years after 9th grade was 39.2 percentage points and this gap had decreased to 9.8 percentage points six years out. Further, when restricting the sample to only students that enrolled in college after high school graduation, lottery students were only 5.7 percentage points more likely to attend a four-year college and they find no significant differences for any college or two-year college enrollment. Similarly, during the study period, Early College students were 20 percentage points more likely to obtain a college degree (control mean was 2 percent). These degrees were typically associate's degrees and approximately 20 percent of Early College students earned a degree before the end of high school

Berger et al. (2013) found no impact on GPA and math standardized test scores. However, they found that Early College students scored 0.14 standard deviations higher than lottery losers on standardized ELA tests. The survey results showed that Early College students were 45.1 percentage points more likely to earn college credit in high school than comparison students and that comparison students were 33.7 percentage points more likely to take at least one advanced placement exam in high school. In addition, Early College students reported engaging in rigorous learning activities in school more frequently, being exposed to higher expectations of college attendance from teachers, principals, and their peers, and reported receiving more help for completing college applications and financial aid forms.

The findings overall suggest that Early Colleges can successfully impact students' college enrollment and attainment during the four years that the students are enrolled in an Early College – but that these impacts might not spillover to the years following high school graduation.

The meta-analysis coefficients for student incentives experiments are 0.024σ (0.018) for math achievement and 0.021σ (0.017) for reading.

C. TUTORING

Throughout recorded history, the children of the elites were taught in a manner that would now be referred to as tutoring. In ancient Greece, children from wealthy families received their primary education individually or in small groups from masters or tutors (Dunstan 2010). This practice continued for children of the rich and nobles throughout the Middle Ages (Nelson-Royes 2015). As late as the 17th century, schooling was thought to be a social, not academic, activity with primary human capital produced in small groups at home. Yet, the term, "tutoring", has in more recent history become synonymous with remediation and fallen out of favor.

There is substantial heterogeneity in how schools implement various programs that fall under the general umbrella of "tutoring." Some schools place students in one-on-one settings with a trained tutor, other schools place eight students with a volunteer. Some students receive tutoring 30 minutes per week, others are provided 5 hours of intense instruction in the same time period. This heterogeneity leads, naturally, to large differences in treatment effects. Dobbie and Fryer (2013), define "high-dosage" tutoring as being tutored in groups of 6 or fewer for 4 or more days per week. Moreover, they demonstrate that tutoring itself is not correlated with charter school effectiveness. However, schools who implement "high-dosage" tutoring demonstrate marked treatment effects.

Following Dobbie and Fryer (2013), we divide the randomized field experiments in tutoring into these two groups: low-dosage and high-dosage tutoring. They are discussed in turn. In this exposition, high-dosage tutoring is defined as being tutored in groups of 6 or fewer for more than three days per week or being tutored at a rate that would equate to 50 hours or more over a 36-week period.[22]

High-Dosage Tutoring

Blachman et al. (2004) report results from a study of a high-dosage tutoring program that targeted struggling second and third grade readers. Their study specifically focused on these young readers in an attempt to increase the growth trajectories of these students and possibly combat the negative adolescent and adult outcomes that have been associated with poor early reading skills. The study uses data from two cohorts of students drawn from eleven schools in the spring of 1997 and the spring of 1998. The researchers sent letters home to the parents of 723 students that teachers identified as being in the lowest 20% of readers in their classroom. Of these, 295 students were screened using standardized reading and IQ tests. In order to be eligible for the study, students had to obtain a standard score below 90 on either the Word Identification or the Word Attack subtest of the Woodcock Reading Mastery Tests, obtain a standard score below 90 on a composite of these two subtests, and have a Verbal IQ of at least 80. After screening and balancing for gender, 89 students were randomly assigned to treatment or control (48 to treatment and 41 to control). The study also contained a neuroimaging component that required an additional health screening post-randomization, thus, the researchers contacted parents again to gain consent for both the neuroimaging and tutoring aspects of the experiment. This resulted in a final sample of 37 students in treatment and 32 students in control. Balance tests revealed no significant differences on observables between the final set of treatment and controls students at baseline.

Treatment students received one-on-one tutoring instruction for 50 minutes a day, five days a week, from September to June. This resulted in the average treatment student attending 126 sessions or 105 hours

[22] We add to the Dobbie and Fryer (2013) definition because not all studies report days and group size.

of tutoring. This instruction replaced the typical remedial instruction that the schools offered and that the control students participated in. The instruction was carried out by 12 tutors who were certified in reading or special education. Prior to the intervention, each tutor received 45 hours of training on early childhood interventions, early reading acquisition, and teaching strategies. Additionally, tutors received 2 hours of training each month for the duration of the experiment. The instruction focused on developing fluency and comprehension strategies, and teaching students to read for pleasure. To do this, tutors incorporated a five-step plan that was featured in previous published studies into each session (Blachman 1987; Blachman et al. 1999). In over 90% of classroom observations and audiotapes of the tutor sessions, tutors included all five steps of the instruction. Control students continued business-as-usual – nine control students received no remedial instruction outside of their reading class and the rest participated in small group tutoring that met for 3-5 times a week. On average, control students that received remedial instruction attended 104 sessions and received 77 hours of additional instruction.

To test the impact of treatment, Blachman et al. (2004) administered a battery of tests pretreatment, immediately following treatment, and one year after treatment. The test battery included the Woodcock Reading Mastery Tests—Revised (WRMT), Gray Oral Reading Tests—Third Edition (GORT), Wide Range Achievement Test 3—Spelling (WRAT), and the Calculation and Applied Problems subtests from the Woodcock-Johnson Psycho-Educational Battery—Revised (WJ-R). In addition, the researchers administered subtests of the non-normed Comprehensive Test of Phonological Processes (CTOPP) four times during the treatment year and four times during the follow-up year. At posttest, the researchers found large and statistically significant impacts on all standardized reading measures. These impacts ranged from 0.55σ on the comprehension subtest of the GORT to 1.69σ on the WRMT basic skills cluster. Furthermore, 6 of these 8 impacts were still large and significant a year after the completion of the experiment (the two insignificant impacts were 0.30σ and 0.24σ on the GORT accuracy and GORT comprehension subtests, respectively). The authors saw similar reading results for the non-standardized subtests from the CTOPP. As expected, treatment had no significant impacts on standardized measures of mathematics. If anything, at posttest, the math results suggest negative impacts with effect sizes of -0.33σ and -0.37σ on the WJ-R calculations and applied problems subtests, respectively.

The findings overall suggest that one-on-one high dosage tutoring with research-proven instruction can increase the growth rates of low-ability students. Although treatment and control students have statistically indistinguishable growth rates in the follow-up year, the large impact on reading scores from one year of treatment remains.

Another randomized study that investigates the impacts of high-dosage tutoring on low-ability students

is Cook et al. (2014). In this study, we implemented an academic and behavioral intervention for 106 male 9th and 10th grade students from a public school in the south side of Chicago. Over 90% of the sample were both black and eligible for free or reduced-price lunch. The intervention consisted of providing students with non-academic supports that teach the students social cognitive skills through the "Becoming a Man" (BAM) program while also providing students intensive individualized tutoring. The BAM program used principles of cognitive behavioral therapy to deliver a curriculum that focused on values education. The program sought to develop specific social or social cognitive skills such as generating new solutions to problems, learning new ways to behave, and identifying consequences ahead of time. BAM was conducted in small groups that met once a week for one hour each time. Over the course of the year, students had the chance to participate in 27 different group sessions and typically had to skip an academic class in order to participate. For the academic portion of the intervention, students met in groups of two with a math tutor one hour a day, every day. The tutors were hired following the methodology of Match Corps and were paid $16,000 plus benefits for the nine-month academic year.[23] Control students were not eligible to participate in BAM or the intensive tutoring but could participate in other academic supports available at the high school.

Students were selected to participate in the study based on an academic risk index that was a function of the number of prior-year course failures, unexcused absences, and being previously held back. The 106 male 9th and 10th grade students with the highest risk index score were then randomly assigned to three groups: Control (N=34), BAM only (N=24), and BAM plus high-dosage tutoring (N=48). In order to investigate the impact of assignment to one of these two treatment arms, we obtained student-level records from Chicago Public Schools that contained demographic information and scores from the EXPLORE and PLAN tests for the year prior to and year of the intervention.

We found that the ITT effect of assigning students to either one of the treatment arms was large and statistically significant for math achievement and math GPA. Assignment to either treatment arm increased math achievement by 0.51σ and increased math GPA by 0.425 grade points on a four-point scale. We found no spillovers to reading achievement and no significant impacts on discipline incidents or number of days suspended. However, treatment students were absent 10.272 fewer days throughout the school year. Mostly due to the relatively modest size of our sample, when separated by treatment arm, we found no significant difference between the impacts of the two groups.

[23] Match Corps is an AmeriCorps program in which members spend a year attempting to close the achievement gap by tutoring small groups of students in the various Match Charter Schools in Boston. Match Corps seeks to employ tutors who are dedicated to constant improvement, who possess strong communication and writing skills, and who are committed to spending a year working with children. Adopting their hiring best practices, tutors in Chicago were required to pass a math assessment, conduct a mock tutorial session with actual high school students, and interview with the principal or principal's designee.

Summarizing, the meta-coefficient on high-dosage tutoring is 0.309σ (0.106) for math achievement and 0.229σ (0.033) for reading achievement. Indeed, 54.3% of coefficients demonstrate statistically significant positive treatment effects; 0% yield statistically significant negative effects. Surprisingly, the fraction of statistically positive treatments is larger than early childhood interventions.

Low-Dosage Tutoring

The Early Start to Emancipation Preparation (ESTEP)-Tutoring program of Los Angeles County was created in 1998. The program targets foster children aged 14 to 15 who are three or more years behind in math or reading ability. ESTEP-Tutoring aims to improve the math and reading skills of these students and encourage them to take advantage of educational resources of which they may have been previously unaware. Tutoring is provided in the home of the students by college student tutors drawn from the surrounding twelve community colleges. Tutors are trained to teach the students in math, reading, and spelling and are provided with curriculum materials that fit a student's skill level. In addition to tutoring, the program hopes to foster a mentorship relationship between the tutor and the student. Once assigned to the program, each student is eligible for 50 hours of tutoring and tutors are allotted additional time for preparation, mentoring, or other activities. Courtney et al. (2008) take advantage of the high demand of ESTEP-Tutoring and conduct an evaluation of the program using its oversubscribed application pool.

For the study, all students referred to the program were screened to ensure their math or reading ability was indeed three years behind grade level. Eligible students were then randomly assigned to a group that could participate in ESTEP-Tutoring or a control group that could not. This resulted in a study sample of 445 students, 246 assigned to treatment and 219 assigned to control. On average, approximately four months passed between assignment and a student's first meeting with a tutor. Throughout the two years of the study researchers found that 61.8 percent of treatment students eventually participated in ESTEP-Tutoring and the average treatment student received 18 hours of math tutoring and 17 hours of reading tutoring. The relatively low take-up rate is attributed to the high mobility of foster children and the length of time that passed between assignment and receipt of tutoring. By the time tutors attempted to deliver the first tutoring session, a majority of the non-participants were no longer in the foster home listed on their application. Once the tutoring program was initiated, students were eligible for 50 hours of tutoring delivered through 2 hour sessions twice a week.

In order to investigate the impact of ESTEP-Tutoring on these students, Courtney et al. (2008) conducted three interviews over the two years after randomization (baseline, one year out, and two years out). At each of these interviews, the researchers administered the letter-word identification, calculation, and

passage comprehension subtests of the Woodcock-Johnson Tests of Achievement III as well as a student survey. The survey combined questions from The Midwest Evaluation of Adult Functioning of Former Foster Youth, The National Survey of Child Adolescent Well-Being, the National Longitudinal Survey of Youth, and the National Longitudinal Survey of Adolescent Health. The survey collected data on demographics, prior experiences in care, prior victimization, relationships, social support, employment, education, health behaviors, and physical health.

Courtney et al. (2008) found evidence of a first-stage impact in that treatment students were more likely to report having been tutored at home. However, control students were more likely to report that they had received tutoring at school and the total number of tutoring hours reported by treatment and control students were not statistically different. The authors limit their impact analysis to the second follow-up interview (two years after random assignment) due to the fact that participation in ESTEP was still ongoing for many students one year after random assignment and they find no evidence of impacts on any outcome measure. The difference between control and treatment groups on Woodcock-Johnson achievement scores, school grades, educational attainment, and school behavior are all statistically indistinguishable from zero.

Putting all low-dosage tutoring experiments together, the meta-coefficient on low-dosage tutoring is 0.015σ (0.013) for math achievement and 0.015σ (0.015) for reading achievement.

3.3.2 Teacher-Based Interventions

Great teachers matter. A one-standard deviation improvement in teacher quality translates into annual student achievement gains of 0.15σ to 0.24σ in math and 0.15σ to 0.20σ in reading (Rockoff 2004; Rivkin et al. 2005; Aaronson et al. 2007; Kane and Staiger 2008). These effects are comparable to reducing class size by about one-third (Krueger, 1999). Using quasi-experimental methods, Chetty et al. (2011) estimate that a one-standard deviation increase in teacher quality in a single grade increases earnings by about 1% per year; students assigned to these better teachers are also more likely to attend college and save for retirement, and less likely to have children when teenagers.

How to select or produce great teachers is one of the most important open questions in human capital research. Observable characteristics such as college-entrance test scores, grade point averages, or major choice are not highly correlated with teacher value-added on standardized test scores (Aaronson et al. 2007; Rivkin et al. 2005; Kane and Staiger 2008; Rockoff et al. 2008). And, many programs that aim to make teachers more effective have shown little impact on teacher quality (see e.g., Boyd et al. 2007 for a review). Some argue that these two facts, coupled with the inherent costs of removing low performing teachers due

to collective bargaining agreements along with increased job market opportunities for women, contributes to the fact that teacher quality and aptitude has declined significantly in the past 40 years (Corcoran et al. 2004; Hoxby and Leigh 2004).

We group the set of teacher-based random assignment studies into three subcategories: increasing teacher supply, providing teachers incentives, or increasing human capital through professional development.

A. Increasing Teacher Supply

Perhaps the most obvious way to increase teacher supply is to lower the barriers into the teaching profession by allowing alternative routes for teachers to obtain necessary certifications. Due to the teacher shortages and the No Child Left Behind Act, which required every classroom to be staffed with a certified teacher or a teacher actively pursuing a certification through an approved program, there has been an increase in teachers who enter teaching through alternative paths.Traditionally, teachers have completed all of their certification requirements at an accredited university or program before starting to teach in a classroom. In comparison, alternatively certified (AC) teachers start teaching before completing their requirements and earn their certification while teaching. Well-known examples of AC programs include Teach for America (TFA) and the New York City Teaching Fellows (NYCTF) program. Both of these programs attract extremely qualified uncertified individuals and place them in schools that are in dire need of good teachers. The potential benefits and advantages of these different routes to certification have been debated by many. For example, some argue that the coursework required for traditionally certified (TC) teachers is an unnecessary burden that discourages some from pursuing teaching and AC programs are a way to circumvent that. In contrast, others argue that without that coursework, AC teachers enter classrooms underprepared and will be less effective.

In order to better understand the effectiveness of AC teachers relative to TC teachers, Constantine et al. (2009) conducted a randomized study in elementary schools around the nation in the 2004-2005 and 2005-2006 school years. Their study included 63 schools from 20 districts in 7 states across the nation. Within these schools, 2,610 K-5 students were randomly assigned to be taught by an AC teacher or a TC teacher for one school year. Schools were only allowed to participate if they had at least one eligible AC teacher and one eligible TC teacher in the same grade. In order for a teacher to be eligible to participate, teachers had to be relative novices, had to teach in a regular classroom, and had to deliver both reading and math instruction to all their students. Researchers collected data on student achievement by administering the math and reading sections of the California Achievement Test, 5th Edition (CAT). The researchers

also collected data on the classroom practices of teachers through classroom observations and principals' ratings. In addition, all teachers completed a survey in the spring that collected information on teachers' professional and personal backgrounds, experience in the school as a full-time teacher, and SAT/ACT scores. Finally, they also collected data on the details of each program a teacher attended for certification/alternative placement.

Constantine et al. (2009) found that students of AC teachers did not perform statistically different than students of TC teachers. Furthermore, there were no statistically significant differences when comparing low grade-level (K-1) teachers to high grade-level (2-5) teachers or low-experience teachers to high-experience teachers. When exploring heterogeneous effect sizes across amount of coursework teachers were required to do while teaching, there is some evidence that AC teachers who had high-levels of course work had negative impacts on student achievement. Similarly, there is no statistically significant difference in classroom observation scores between AC and TC teachers. However, when restricting the sample to teachers that had high-levels of coursework, there is evidence that AC teachers' classroom practices were worse than TC teachers' practices.

In addition to the experimental results, Constantine et al. (2009) also present non-experimental results that explore the relationship between teacher characteristics and program details with the impacts on students' achievement. Overall, they found that teacher characteristics and training experiences only explained 5 percent of the variation in effects on math test scores and 1 percent of the variation in effects on reading test scores. The only significant correlations they found were that AC teachers with master's degrees were less effective in improving student achievement in reading than TC teachers without a master's degree and that students in classrooms taught by AC teachers who were taking coursework towards a degree or certification did worse in reading than students taught by TC teachers who weren't taking coursework.

Some argue that schools don't just need access to more teachers, but specifically need access to different and potentially better talent pools. Proponents of this argument often point to successful foreign education systems, such as Hong Kong or Finland, that draw their teachers from the uppermost ranks of their universities (Tucker 2011). In contrast, it is a well-documented fact that the talent pool of American teachers has been declining since 1960 (see Corcoran et al. 2004). Hoxby and Leigh (2004) attribute a large part of this decline to opportunities outside of teaching drawing high-aptitude women from the profession. Over the past couple decades, we have seen an increase of programs designed to combat this decline and get more and better college students to enter into teaching. One such program is Teach for America.

Teach For America, a non-profit organization that recruits recent college graduates to teach for two years in low-income communities, is one of the nation's most prominent service programs. Based on founder

Wendy Kopp's undergraduate thesis at Princeton University, TFA's mission is to create a movement that will eliminate educational inequity by enlisting our nation's most promising future leaders as teachers. In 1990, TFA's first year in operation, Kopp raised $2.5 million and attracted 2,500 applicants for 500 teaching slots in New York, North Carolina, Louisiana, Georgia, and Los Angeles.

Since its founding, TFA corps members have taught more than three million students. Today, there are 8,200 TFA corps members in 125 "high-need" districts across the country, including 13 of the 20 districts with the lowest graduation rates. Roughly 80 percent of the students reached by TFA qualify for free or reduced-price lunch and more than 90 percent are black or Hispanic.

Entry into TFA is highly competitive; in 2010, more than 46,000 individuals applied for just over 4,000 spots. Twelve percent of all Ivy League seniors applied. In its recruitment efforts, TFA focuses on individuals who possess strong academic records and leadership capabilities, regardless of whether or not they have had prior exposure to teaching. To apply, candidates complete an online application, which includes a letter of intent and a resume. After a phone interview, the most promising applicants are invited to participate in an in-person interview, which includes a sample teaching lesson, a group discussion, a written exercise, and a personal interview. Applicants who are invited to interview are also required to provide transcripts, obtain two online recommendations, and provide one additional reference.

Using information collected through the application and interview, TFA bases their candidate selection on a model that accounts for multiple criteria that they believe are linked to success in the classroom. These criteria include achievement, perseverance, critical thinking, organizational ability, motivational ability, respect for others, and commitment to the TFA mission. TFA conducts ongoing research on their selection criteria, focusing on the link between these criteria and observed single-year gains in student achievement in TFA classrooms.

TFA teachers are required to take part in a five-week TFA summer institute to prepare them for placement in the classroom at the end of the summer. The TFA summer institute includes courses covering teaching practice, classroom management, diversity, learning theory, literacy development, and leadership. During the institute, groups of participants also take full teaching responsibility for a class of summer school students.

At the time of their interview, applicants submit their subject, grade, and location preferences. TFA works to balance these preferences with the needs and requirements of districts. With respect to location, applicants rank each TFA region as highly preferred, preferred, or less preferred and indicate any special considerations, such as the need to coordinate with a spouse. Over 90 percent of the TFA applicants accepted are matched to one of their "highly preferred" regions (Glazerman et al. 2006).

TFA also attempts to match applicants to their preferred grade levels and subjects, depending on applicants' academic backgrounds, district needs, and state and district certification requirements. As requirements vary by region, applicants may not be qualified to teach the same subjects and grade levels in all areas. It is also difficult for school regions to predict the exact openings they will have in the fall, and late changes in subject or grade-level assignments are not uncommon. Predicted effectiveness scores are not used to determine the placement region, grade, or school, and the scores are not available to districts.

TFA corps members are hired to teach in local school districts through alternative routes to certification. Typically, they must take and pass exams required by their districts before they begin teaching and may also be required to take additional courses to meet state certification requirements.

TFA corps members are employed and paid directly by the school districts for which they work, and generally receive the same salaries and health benefits as other first year teachers. Most districts pay a $1,500 per corps member fee to TFA to offset screening and recruiting costs. TFA gives corps members various additional financial benefits, including "education awards" of $4,725 for each year of service that can be used for past or future educational expenses, and transitional grants and no-interest loans to help corps members make it to their first paycheck.

To date, there have been a couple randomized evaluations of the impact of TFA teachers. Glazerman et al. (2006) report findings from a national evaluation of TFA. The experiment involved approximately 100 elementary classrooms from 17 schools drawn from Baltimore, Chicago, Compton, Houston, New Orleans, and the Mississippi Delta. Students were stratified by grade and school and assigned randomly to either a TFA or a non-TFA teacher. At the end of school year, Glazerman et al. (2006) found that students assigned to TFA teachers score about 0.12σ higher in math and 0.03σ higher in reading than students assigned to traditionally certified teachers.They found no impacts on other student outcomes such as attendance, promotion, or disciplinary incidents, but TFA teachers were more likely to report problems with student behavior than were their peers.

An even bigger study analyzed by Clark et al. (2013) uses a sample drawn from almost 100 schools across eight states to investigate the effectiveness of middle school math teachers from TFA and a similar program called The New Teacher Project (TNTP). In each participating school, students were randomly assigned to math classrooms taught by a program teacher (TFA or TNTP) or a teacher that did not enter teacher through either of these programs. Similar to Glazerman et al. (2006), Clark et al. (2013) find a significant impact of TFA on students' math test scores. Students assigned to TFA teachers scored 0.07σ higher on state standardized testing whereas students assigned to TNTP teachers had test scores that were indistinguishable from students in control classrooms. Note that this study was not designed to investigate

the difference between TFA and TNTP teachers. Students were not randomly assigned between TFA and TNTP teachers, so differences between the effectiveness of the teachers could be due to differences in the students they taught, the comparison teachers, or the schools they were in. Indeed, TFA and TNTP teachers included in the study largely taught in different schools and districts. With this in mind, there are still some major differences between the two programs worth noting. TFA requires its teachers to commit to two years of teacher whereas TNTP expects their recruits to teach for many years. Also, TFA recruits heavily from college campuses while TNTP recruits professionals that want to switch careers.

Several other programs similar – in spirit – to TFA are Boston Teaching Residency, Match Teaching Residency, NYC Teaching Fellowships, Inner-City Teaching Corps of Chicago, and Harvard Teaching Fellows. Although these programs all differ in length, training procedures, and credentials earned through the program, they all recruit college graduates with strong academic backgrounds and place them in struggling school districts. To the best of our knowledge, no randomized evaluations exist yet for these programs.

B. Teacher Incentives

To increase teacher productivity, there is growing enthusiasm among policy makers for initiatives that tie teacher incentives to the achievement of their students. Since 2006, the U.S. Department of Education has provided over $1 billion to incentive programs through the Teacher Incentive Fund – a program designed specifically to support efforts developing and implementing performance-based compensation systems in schools. At least seven states and many more school districts have implemented teacher incentive programs in an effort to increase student achievement (Fryer 2013).

Yet, the empirical evidence on the effectiveness of teacher incentive programs is mixed. In developing countries where the degree of teacher professionalism is extremely low and absenteeism is rampant, field experiments that link pay to teacher performance are associated with substantial improvements in student test scores (Duflo et al. 2012; Glewwe et al. 2010; Muralidharan and Sundararaman 2011). Conversely, the few field experiments conducted in the United States have had, at best, mixed results.

Theoretically, it is unclear how to design optimal teacher incentives when the objective is to improve student achievement. Much depends on the characteristics of the education production function. If, for instance, the production function is additively separable, then individual incentives may dominate group incentives, as the latter encourages free-riding. If, however, the production function has important complementarities between teachers in the production of student achievement, group incentives may be more effective at increasing achievement (Baker 2002).

Group Incentives

In the 2007-2008 through the 2009-2010 school year, the United Federation of Teachers (UFT) and the New York City Department of Education (DOE) implemented a teacher incentive program in over 200 high-need schools, distributing a total of roughly $75 million to over 20,000 teachers.[24] The experiment was a randomized school-based trial. Each participating school could earn $3,000 for every UFT-represented staff member if the school met the annual performance target set by the DOE based on school report cards, which the school could distribute at its own discretion. Each participating school was given $1,500 per UFT staff member if it met at least 75% of the target but not the full target. Note that the average New York City public school has roughly sixty teachers; this implies a transfer of $180,000 to schools on average if they met their annual targets and a transfer of $90,000 if they met at least 75% of, but not the full target. In elementary and middle schools, school report card scores hinge on student performance and progress on state assessments, student attendance, and learning environment survey results. High schools are evaluated similarly, with graduation rates, Regents exams, and credits earned replacing state assessment results as proxies for performance and progress.

An important feature of the experiment is that schools had discretion over their incentive plans. As mentioned above, if a participating school met all of the annual targets, it received a lump sum equivalent to $3,000 per full-time unionized teacher. Each school had the power to decide whether all of the rewards would be given to a small subset of teachers with the highest value-added, whether the winners of the rewards would be decided by lottery, or virtually anything in-between. The only restriction was that schools were not allowed to distribute rewards based on seniority.

An overwhelming majority of the schools decided on a group incentive scheme that varied the individual bonus amount only by the position held in the school. This could be because teachers have superior knowledge of education production and believe the production function to have important complementarities, because they feared retribution from other teachers if they supported individual rewards, or simply because this was as close to pay based on seniority (the UFT's official view as to why schools typically settled on this scheme) as they could do.

The results from this incentive experiment are informative. Providing incentives to teachers based on a school's performance on metrics involving student achievement, improvement, and the learning environment did not increase student achievement in any statistically meaningful way. If anything, student achievement declined. ITT estimates yield treatment effects of -0.018σ (0.024) in mathematics and -0.014σ (0.020) in reading for elementary schools, and -0.046σ (0.018) in math and -0.030σ (0.011) in reading for middle

[24]The details of the program were negotiated by Chancellor Joel Klein and Randi Weingarten, along with their staffs. At the time of the negotiation, I was serving as an advisor to Chancellor Klein and convinced both parties to agree to include random assignment to ensure a proper evaluation.

schools, *per year*. Thus, if an elementary school student attended schools that implemented the teacher incentive program for three years, her test scores would decline by -0.054σ in math and by -0.042σ in reading, neither of which is statistically significant. For middle school students, however, the negative impacts are more sizeable: -0.138σ in math and -0.090σ in reading over a three-year period.

Consistent with Fryer (2013), Springer et al. (2012) evaluated another group incentive experiment that took place in the Round Rock Independent School District in Texas. The study used random assignment to investigate the impacts of a program that awarded teams of middle school teachers bonuses based on their collective contribution to students' test score gains. Two years after the initial randomization, Springer et al. (2012) found no significant impacts on the attitudes and practices of teachers or on the academic achievement of students.

Individual Incentives

Springer et al. (2010) evaluated Tennessee's POINT program – a three-year pilot initiative on teacher incentives conducted in the Metropolitan Nashville School System from the 2006-07 school year through the 2008-09 school year. 296 middle school mathematics teachers who volunteered to participate in the program were randomly assigned to the treatment or the control group, and those assigned to the treatment group could earn up to $15,000 as a bonus if their students made gains in state mathematics test scores equivalent to the 95th percentile in the district. They were awarded $5,000 and $10,000 if their students made gains equivalent to the 80th and the 90th percentiles, respectively. Springer et al. (2010) found there was no significant treatment effect on student achievement and on measures of teachers' response such as teaching practices.

In an important observation, Neal (2011) discusses how group incentives (e.g. Fryer 2013; Springer et al. 2012) or sufficiently obtuse (e.g. Springer et al. 2010) pay schemes lead to problems when trying to calculate the incentive effect at the individual teacher level and could be the reason these experiments observed little to no incentive effects. For instance, calculating the expected value of a one standard deviation increase in teacher effort when the incentive scheme depends on where a teacher lies in the overall district distribution is a non-trivial calculation for an econometrician with loads of data and sophisticated techniques. It would be exceedingly difficult for a teacher to perform this calculation and understand how their efforts could translate into rewards. To circumvent this and competition issues between teachers, Barlevy and Neal (2012) develop a "pay for percentile" method that rewards teachers according to how highly their students' test score improvement ranks among other students from other schools with similar baseline achievement and demographic characteristics.

Although not fully using the method recommended by Barlevy and Neal (2012), Glazerman et al. (2009) present results from a randomized experiment that ties individual teacher incentives to value-added measures. This incentive scheme is more in line with the insights in Neal (2011) than Fryer (2013) or Springer et al (2010, 2012).

In 2007, Chicago Public Schools implemented its own version of the national Teacher Advancement Program (TAP). The national version of TAP was developed in the late 1990s by the Milken Family Foundation as an incentive program to increase teacher quality. Teachers could earn extra pay by being promoted to Mentor or Lead Teacher and receive annual performance bonuses based on their value-added and classroom observations. Chicago adopted this model with some minor alterations. For example, the Chicago TAP added principal bonuses tied to implementation benchmarks and school-wide value-added. Teacher incentives had an expected payout of $2,000 per teacher and teachers could earn an additional $7,000 by becoming a Mentor or an additional $15,000 by becoming a Lead Teacher. As Mentors, teachers were expected to provide ongoing classroom support to other teachers in the school. Lead Teachers served on the leadership team responsible for implementing TAP, analyzing student data, and developing achievement plans. In addition, Mentors and Lead Teachers conducted weekly group meetings to foster collaboration between teachers and provide additional professional development.

Glazerman et al. (2009) conducted a randomized evaluation of the first year of Chicago TAP. Of the sixteen K-8 schools that volunteered to participate in the program, eight were randomly assigned to start treatment in the 2007-2008 school year and the other eight would delay the start of the program until the 2008-2009 school year. Glazerman et al. (2009) compared the outcomes for teachers and students in schools randomly assigned to the two groups for the 2007-2008 school year to determine causal impacts of exposure to one year of Chicago TAP. For their analysis, the researchers collected student achievement data and teachers' classroom assignments from Chicago Public Schools as well as administered surveys to teachers and principals to collect important information that was not present in the administrative data.

The evaluation suggests that TAP increased retention in treatment schools. Teachers in TAP schools had a retention rate of 87.9 percent while teachers in control schools had a retention rate of 82.8 percent, a statistically significant difference. However, teacher satisfaction and teachers' positive attitudes toward their principals were not statistically different between TAP and control schools.

More importantly, the introduction of TAP did not produce any measurable impacts on student standardized test scores. The effect size for reading was -0.04σ (0.05) and the effect size for math was -0.04σ (0.06). The test sore impacts were insignificant across all grade levels and were robust to various sensitivity analyses.

Enhancing the Efficacy of Teacher Incentives Through Framing

During the 2010-2011 and the 2011-2012 school years, Fryer et al. (2015) conducted an experiment in nine schools in Chicago Heights, IL. At the beginning of each school year, teachers were randomly selected to participate in a pay-for-performance program. Among those who were selected, the timing and framing of the reward payment varied. One set of teachers – whom we label the "Gain" treatment – received "traditional" financial incentives in the form of bonuses at the end of the year linked to student achievement.[25] Other teachers – the "Loss" treatment – were given a lump sum payment at the beginning of the school year and informed that they would have to return some or all of it if their students did not meet performance targets. Teachers in the "Gain" and "Loss" groups with the same performance received the same final bonus. Within the "Loss" and "Gain" groups, we additionally tested whether there are heterogeneous effects for individual teacher rewards compared to awarding incentives to teams of teachers.

In all groups, performance was incentivized according to the "pay for percentile" method developed by Barlevy and Neal (2011), in which teachers are rewarded according to how highly their students' test score improvement ranks among peers from other schools with similar baseline achievement and demographic characteristics. As Neal (2011) describes, pay for percentile schemes separate incentives and performance measurements for teachers since this method only uses information on relative ranks of the students. Thus, motivation for teachers to engage in behaviors (e.g. coaching or cheating) that would contaminate performance measures of the students are minimized.

The first year ITT results of our experiment are consistent with over three decades of psychological and economic research on the power of framing to motivate individual behavior, though other models may also be consistent with the data. Students who were assigned to teachers in the "Loss" treatment show large and statistically significant gains in year one math test scores (0.455σ (0.097)). Teacher incentives that are framed as gains demonstrate less success. In the first year of the experiment, students in the "Gain" treatment increased their math test scores 0.245σ (0.094). Importantly, the difference between the "Loss" and "Gain" treatments in math improvement is statistically significant at conventional levels. More generally, these results support the view in Barlevy and Neal (2011) and Neal (2011) that properly designed incentives can have significant effects.

Interestingly, when looking at the sample of all students, we find little evidence of treatment effects in the second year of the experiment. The ITT estimates for "Loss" are 0.087σ (0.088) and for "Gain" are 0.115σ (0.109). The pooled estimates for both years of the experiment are 0.210σ (0.069) and 0.116σ

[25] Note that although math, reading, and science test scores were incentivized (the latter only for fourth and seventh grade science teachers), the main analysis of the paper focuses on math achievement due to most students having multiple reading teachers and the science sample being so small.

(0.075) for "Loss" and "Gain", respectively. The difference between the "Loss" and "Gain" treatments for the pooled estimates has a p-value of 0.099.

Although incentivizing teachers had differential impacts across both years when looking at the entire sample, we found that kindergartners had large gains in both years of the experiment regardless of whether their teachers were in the "Loss" or "Gain" group. In the first year of the experiment, the ITT estimates for kindergarten students were 0.796σ (0.209) for "Loss" and 0.376σ (0.168) for "Gain". In the second year, the estimates were 0.574σ (0.176) and 0.714σ (0.144) for "Loss" and "Gain" respectively. Therefore, the pooled effect size for both years and both treatments was 0.568σ (0.121) for kindergarten math scores.

Talent Transfers

In America, inexperienced teachers are more likely to be assigned to high-minority and high-poverty classrooms (Feng 2010). As a result, novice teachers are taking on tougher school assignments, teaching multiple grades, and teaching out-of-field classes (Donaldson and Johnson 2010). To counteract this trend, several school districts – such as Houston ISD – provide effective teachers incentives to teach in the most troubled schools. The theory is that the marginal return for an additional effective teacher in a well-functioning school is less than the marginal return of that teacher in a less well-functioning school. Good teachers have the potential to change the culture of a school and provide effective pedagogical tools and mentorship to struggling colleagues. If true, providing incentives for talented teachers to teach in troubled schools will increase total productivity.

Glazerman et al. (2013) used a randomized experiment in 10 districts across the nation to investigate the impact of filling vacancies with high-achieving teachers through the Talent Transfer Initiative (TTI). In each district, the TTI offered teachers with consistently high value-added (ranking in the top 20 percent within their subject and grade) $20,000, paid over two years, to teach at low-achieving schools selected through a random process. Principals of schools with low average test scores volunteered to fill vacancies at their school using the TTI. Schools that volunteered were matched based on the grade-level and subject of the vacancy as well as school demographics. Teacher teams (teachers grouped by grade and subject) within each block of schools that had at least one vacancy were then randomly assigned to treatment or control. Teacher teams assigned to treatment status were eligible to fill their vacancy with a TTI teacher and vacancies in control teacher teams were filled using the typical process of the given school. Note that high-performing teachers were not randomly assigned to these vacancies. After a teacher team is assigned to treatment, TTI teachers must interview for the position, principals must extend an offer to a TTI teacher, and then a TTI teacher must accept and voluntarily move to fill this vacancy. In order to receive the full financial incentive,

high-performing teachers must remain in the low-achieving school for a full two years.

Glazerman et al. (2013) investigated the impact of teacher teams being eligible to fill their vacancies using the TTI. Across the 10 districts included in the study, 165 teacher teams from 114 schools were randomly assigned to treatment (N= 85) or control (N=80). The transfer incentive was able to successfully attract high-achieving teachers. Eighty-eight percent of treatment vacancies were filled with teachers through the TTI. In order to achieve this high rate of transfer, over 1,500 high-achieving teachers were invited to participate in TTI. The teachers hired to fill treatment vacancies were significantly more experienced than teachers hired for control spots. Treatment teachers had on average four years more experience and were 11 percentage points more likely to have a National Board Certification than control teachers (CM = 9 percent). Interestingly, there was evidence that principals reacted to the hiring of a TTI teacher by reallocating weak teachers to be in the same team as the incoming TTI teacher. Teachers from elsewhere in the school that joined a treatment teacher team after the hiring of a TTI teacher had five years less experience than teachers that moved into a control teacher team. In addition, treatment teachers were more likely to provide mentoring to their peers (15 compared to 5 percent of the control teachers) and were less likely to receive mentoring (39 compared to 59 percent of control teachers).

Glazerman et al. (2013) found that the above first-stage and intermediate impacts translated into large effects on student achievement tests for elementary schools but that there were no significant impacts on the achievement of middle school students. TTI eligible elementary classrooms increased students' math scores by 0.18σ and students' reading scores by 0.10σ in the first year after randomization. In the second year, the cumulative impacts for treatment students were 0.22σ and 0.25σ for math and reading test scores, respectively. Although treatment elementary teachers had large impacts on their students, there were no spillover effects for other teachers in their team. Elementary student achievement outcomes were not significantly different between students assigned to other teachers in the treatment team and students assigned to other teachers in the control team.

Finally, there was evidence that the TTI was effective at keeping these high-performing teachers in the low-performing schools. At the halfway point of the program, retention rates were higher for teachers that filled TTI vacancies. Treatment teachers were 23 percentage points more likely to remain in a school after the first year than their control counterparts (93 percent compared to 70 percent). In addition, retention rates after the completion of the second year of the experiment were not statistically significant. Approximately 60 percent of treatment teachers returned to the low-achieving school for a third, non-incentivized school year. In comparison, a statistically indistinguishable 51 percent of control teachers remained in the fall of the third school year.

The meta-coefficient on teacher incentives is 0.022σ (0.022) for math achievement and -0.006σ (0.012) for reading achievement. Yet, that number seems particularly misleading in this context as many of the schemes were quite ad hoc and inconsistent with economic theory. More experiments are needed before one can better hazard a guess on the efficacy of teacher incentives. Future randomized trials ought to take the insights in Barlevy and Neal (2011) and Neal (2011) seriously when designing teacher incentive schemes.

C. Teacher Professional Development

General Professional Development

The Gates Foundation states that the American education system spends $18 billion annually on professional development (Bill and Melinda Gates Foundation 2014). For 2014, Title II of the Elementary and Secondary Education Act, a program mostly devoted to professional development, was appropriated $2.3 billion (U.S. Department of Education 2014). More than $450 million (approximately half) of the Department of Education's Investing in Innovation (i3) grant money funded professional development programs from 2010-2012 (U.S. Government Accountability Office 2014). A new report released by TNTP estimates that three large public districts included in their study spent nearly $18,000 per teacher per year for professional development (TNTP 2015).

Professional development (PD) is viewed as a vital tool to increase teachers' human capital and improve school effectiveness (Hill 2007). However, experts have expressed concern that teachers are not receiving enough professional development to have meaningful impacts on teachers' practices and that the little professional development they receive does not focus enough on subject-matter knowledge (Cohen and Hill 2001; Fletcher and Lyon 1998; Foorman and Moats 2004; Garet et al. 2001).[26] Another often articulated concern is that professional development tends to be one-time workshops scheduled on "professional development days" or in the summer months with little relevant follow-up (Joyce and Showers 1988; Parsad et al. 2001; Loucks-Horsley et al. 1998).

The U.S. Department of Education commissioned two PD interventions to provide states and districts with further information of the potential of PD programs to improve reading instruction (Garet et al. 2008). The first intervention provided second grade teachers with a year-long research-based institute series and the second intervention provided the same institute series plus in-school coaching. Garet et al. (2008) presented results from the randomized evaluation of these two interventions. In their study, 90 schools across six districts from four states were randomly assigned to one of the two treatment groups or a control group

[26] A national study revealed that over 80% of elementary and secondary teachers reported participating in 24 hours or less of professional development over the 2005-2006 school year and summer of 2006 (U.S. Department of Education 2009).

such that each district had an equal number of elementary schools allocated to the three groups. Garet et al. (2008) collected data on teacher knowledge, teacher practices, and student achievement at the completion of the intervention and two years after randomization as a follow-up.

On average, teachers in the first intervention reported attending 39 hours of PD and teachers in the second intervention reported attending 47 hours of PD. In comparison, control teachers only reported attending 13 hours of PD. Garet et al. (2008) found that this exposure to PD lead to significant impacts on teachers' knowledge and practices for both groups. Both interventions had positive impacts on second grade teachers' knowledge of early reading content and instructional knowledge at posttest and a year after the PD programs had completed. For both years, teachers in both interventions used explicit instruction to a much greater extent than teachers assigned to control. However, there was no significant difference in the amount of independent student activity incorporated into the classroom and the use of differential instruction between either of the two treatment groups and control teachers. Although there were large impacts on teacher knowledge and practices, there was no evidence that these changes had an impact on students' test scores. Test scores from the implementation year and the follow-up year revealed no statistically significant impacts on standardized math and reading outcomes.

Another widely used PD program is Classroom Assessment of Student Learning (CASL). The program set consists of a primary text, DVDs, ancillary books, and an implementation handbook. CASL is designed to be a self-executing PD program where teachers learn from the textbook and use CASL assessments to better understand their own and their students' progress. The program mostly emphasizes formative assessments, but also includes lessons on how to utilize other forms of classroom assessments such as standardized test scores. The program is typically implemented via teacher learning teams, in which teachers can discuss and receive feedback from other teachers who are also using the program.

In order to better understand the effects of CASL on students' achievement, motivation to learn, and teachers' classroom assessment practices, Randel et al. (2011) conducted a large randomized experiment. Due to regional needs, they decided to focus the study in mathematics classrooms.

Almost 70 schools from 32 districts from across Colorado participated in the study. Schools volunteered to participate and were eligible if they were large enough to have at least one fourth grade and one fifth grade teacher. The 67 eligible schools were then randomly assigned to a treatment group (N = 33) and a control group (N= 34). In November of 2007, treatment schools received one set of CASL PD materials for each math teacher in fourth or fifth grade. In total, there were 178 such teachers in treatment schools and 231 teachers in control schools. Treatment teachers participated in an introductory video conference with the author of CASL and had access to a facilitator who had received training in the CASL program,

but other than this, the experiment was completely hands off. The teachers were asked to use the PD naturally without any input or requirements from the research team. The 2007-2008 school year was used as a training year during which teachers studied the CASL material and started integrating CASL practices into their classrooms. The 2008-2009 year was the actual intervention year. Fidelity of treatment was assessed using self-reported logs that 90 percent of teachers returned to the research team. In order to combat the alternative hypothesis that any impact was just the result of the intervention schools having more resources, control schools were given $1,000.

In order to assess the impact of the intervention on student and teacher outcomes, Randel et al. (2011) collected administrative and survey data from both the training and implementation year. The administrative data came directly from the Colorado Department of Education and contained state achievement test scores and student demographics. In order to quantify students' motivation to learn, researchers administered a student survey. Further, teachers' knowledge of classroom assessments, their classroom assessment practices, and teachers' involvement of students in assessments were all measured through self-reported teacher surveys.

Randel et al. (2011) found evidence that treatment had a significant impact on teacher knowledge of class assessments. Intervention teachers on average answered 2.78 questions more (0.42σ) correctly on a 60-item test about teachers' knowledge of classroom assessments. However, there was no evidence that this knowledge influenced their classroom practices. There were no significant differences in classroom practices and the extent to which they involved their students in formative assessments. Intervention teachers were given an average rating of 1.61 for classroom assessment and control teachers had an average rating of 1.60 (where 1 represents low quality and 4 represents high quality). For student involvement, intervention teachers self-reported average score was a 0.39 and control teachers' average score was 0.34 (where 1 indicates that all students were involved in formative assessments everyday and 0 represents no students were involved).

As one would suspect from the similar practices of control and treatment teachers, average student mathematics achievement was not statistically different between treatment and control students. The adjusted mean scale score for students in a treatment classroom was 502.49(2.52) and the mean scale score for students in control classrooms was 501.91(2.44). The impacts remain statistically insignificant when looking at effect sizes by grade level.

The meta-coefficient for general PD is 0.019σ (0.024) for math achievement and 0.022σ (0.023) for reading achievement. In fact, there is not a single study with significant annual pooled impacts. Ironically –

and perhaps sadly – Erik Hanushek argues school districts are overspending on ineffective and unmanaged professional development and these districts refuse to veer away from practices that fail time and time again (Layton 2015).

'Managed' Professional Development

Another form of PD is one that has precise training and curriculum materials that schools and districts can implement to increase teacher effectiveness. These programs are significantly more prescriptive. They don't abstractly discuss issues such as "classroom management" or endeavor to increase "rigor." Consider two well known examples of this approach to professional development: Success for All and Reading Recovery.

Success for All is a school-level elementary school intervention that focuses on improving literacy outcomes for all students in order to improve overall student achievement and is currently used in 1,200 schools across the country (Borman et al. 2007). The program is designed to identify and address deficiencies in reading skills at a young age using a variety of specific instruction strategies, ranging from cooperative learning to data-driven instruction. Success for All is purchased as a comprehensive package, which includes materials, training, ongoing PD, and a well-specified "blueprint" for delivering and sustaining the model. Schools that elect to adopt Success for All implement a program that organizes resources to attempt to ensure that every child will reach the third grade on time with adequate basic skills and will continue to build on those skills throughout the later elementary grades.

Borman et al. (2007) use a cluster randomized trial design to evaluate the impacts of the Success for All model on student achievement. Forty-one schools from eleven states volunteered and were randomly assigned to either the treatment or control groups. Treatment schools implemented Success for All in grades K-2 and control schools implemented the program in grades 3-5. Borman et al. (2007) present results three years after randomization for the baseline cohort of kindergarten students. Although forty-one schools were initially randomized, only thirty-five schools were included in the analysis due to six schools dropping out over the years for various reasons. The authors conclude that these thirty-five schools are still balanced and attrition is not a threat to their results. Using standardized test scores from three subtests of the Woodcock Reading Mastery Test—Revised, Borman et al. (2007) find that Success for All increased student achievement by 0.36σ (0.11) on phonemic awareness, 0.24σ (0.11) on word identification, and 0.21σ (0.09) on passage comprehension.

Another similar professional development program is Reading Recovery (RR). RR is a short-term intervention designed to help struggling readers in first grade catch up to their peers. The program consists

of students meeting one-on-one with a specially trained teacher every day for a 30-minute lesson over 12 to 20 weeks. The lessons are individualized by the RR teacher to fit to a student's strengths and needs and follow the RR model—focusing on phonemic awareness, phonics, vocabulary, fluency, and comprehension. RR teachers undergo a year-long training procedure that takes place at designated training facilities and the schools where they are assigned. Through this training, they learn how to design and deliver daily lesson plans, document lessons, and to collect and effectively use different types of student progress data. All RR teachers are overseen by a teacher leader who has attended an intensive post-graduate program where they are expected to emerge as literary experts. Literature on RR reports that approximately 75 percent of students enrolled in RR typically reach grade-level proficiency after participating in RR for the program's intended length of 12-20 weeks and that these students go on to maintain their progress through the remainder of elementary school (May et al. 2013).

Schwartz (2005) conducted the first randomized evaluation of RR in the United States. In his study, thirty-seven first-grade teachers from across the nation identified two at-risk students in their classroom. One student from each pair was randomly assigned to a treatment condition that received RR in the fall and the other student was assigned to a control condition that received RR in the spring. The participating teachers were all certified RR teachers and the program was active in their schools. The teachers gave up one of their four 30-minute RR slots to whichever student was randomly assigned to treatment. At the end of the first semester, Schwartz (2005) found that treatment students had large and significant impacts on various observation survey and standardized reading measures. Effect sizes on the text level, letter identification, concepts about print, and hearing and recording sounds in words tasks on the observation survey ranged from 0.9σ to 2.02σ and treatment had an impact of 0.94σ on scores from the Slosson Oral Reading Test—Revised.

In 2010, the U.S. Department of Education awarded RR a $45 million i3 grant along with $10.1 million from private sources to fund a scale-up of RR across the nation. The scale-up intends to reach over 2,000 schools and provide literary assistance to over 88,000 students. May et al. (2013) report the findings from the first two years of this scale-up.

628 schools from across the nation were enrolled in the i3 scale-up of Reading Recovery. These schools were randomly assigned to three blocks and one of these blocks was randomly chosen to participate in a RCT of Reading Recovery during the 2011-2012 school year. Of the 209 schools in this block, only 156 schools actually carried out the randomization process described below and were included in the evaluation. Each school that participated in the RCT identified the eight lowest scoring students in their school using the Observation Survey of Early Literacy. These eight students were matched according to their scores and ELL status and then one student from each pair was randomly assigned to treatment and the other to control.

This process resulted in 628 students in the treatment group and 625 students in the control group (when there were less than eight eligible students, odd number students were automatically assigned to treatment. These students were omitted from the impact analysis).

Using standardized test scores from the Iowa Test of Basic Skills and baseline demographics, May et al. (2013) investigate the causal impact of being assigned to the RR program. They find that RR increased student achievement by 0.60σ on the reading words subtest and 0.61σ on the reading comprehension subtest.

The effects of these 'managed' PD experiments for both subjects are statistically significant and for reading, quite large. The meta-coefficient is 0.052σ (0.016) for math achievement and 0.403σ (0.120) for reading achievement.

Teacher Feedback

The modernization of teacher evaluation systems, an increasingly common component of teacher professional development, promises to reveal new, systematic information about the performance of individual classroom teachers. Yet while states and districts race to design new systems, most discussion of how the information might be used has focused on traditional human resource–management tasks, namely, hiring, firing, and compensation. By contrast, very little is known about how the availability of new information, or the experience of being evaluated, might change teacher effort and effectiveness. Dobbie and Fryer (2013) report that teacher feedback is one of the variables most correlated with charter school success.

In the research reported here, we study one approach to teacher feedback: practice-based assessment that relies on multiple, highly structured classroom observations conducted by experienced peer teachers and administrators. While this approach contrasts with principal walk-through styles of class observation, its use is on the rise in new and proposed evaluation systems in which rigorous classroom observation is often combined with other measures, such as teacher value-added based on student test scores.

Proponents of evaluation systems that include high-quality classroom observations point to their potential value for improving instruction (see "Capturing the Dimensions of Effective Teaching," Features, Fall 2012). Individualized, specific information about performance is especially scarce in the teaching profession, suggesting that a lack of information on how to improve could be a substantial barrier to individual improvement among teachers. Well-designed evaluations might fill that knowledge gap in several ways. First, teachers could gain information through the formal scoring and feedback routines of an evaluation program. Second, evaluation could encourage teachers to be generally more self-reflective, regardless of the evaluative criteria. Third, the evaluation process could create more opportunities for conversations with other teachers and administrators about effective practices.

Taylor and Tyler (2012), using a quasi-experimental design, find that teachers are more effective at raising student achievement during the school year when they are being evaluated as opposed to previous years, and even more effective in the years after evaluation. A student instructed by a teacher after that teacher has been through an evaluation scored about eleven percent of a standard deviation (4.5 percentile points for a median student) higher in math than a similar student taught by the same teacher before the teacher was evaluated.

3.3.3 School Management

Bloom et al. (2015) identify an interesting relationship between management quality of 1,800 high schools from eight countries and student achievement in those schools. They find a strong correlation between higher management quality and better educational outcomes. Dobbie and Fryer (2013) use variation in the management practices of New York City charter schools to investigate the characteristics that differentiate those that increase student achievement (as measured by standardized test scores) and those that do not increase achievement. Using survey and administrative data from 39 New York City charter schools, they correlated the policies of each school with the school's individual impact on math and reading achievement. Dobbie and Fryer (2013) report that traditional inputs (i.e. class size, per pupil expenditure, the fraction of teachers with no certification, and the fraction of teachers with an advanced degree) are not correlated with school effectiveness. Instead, they found that frequent teacher feedback, the use of data to guide instruction, high-dosage tutoring, increased instructional time, and high expectations are highly correlated with schools' impacts on math and reading. In this section, we present studies that explore causal impacts of some of the management practices discussed in Bloom et al. (2015) and Dobbie and Fryer (2013).

A. Using Data to Drive Instruction

Carlson et al. (2011) present results from a large randomized study that investigates the impacts of data-driven reform on student achievement in mathematics and reading. The study included over 500 schools from 59 school districts across seven states. Districts randomly assigned to treatment implemented a 3-year data-driven reform initiative with the support of consultants from the Johns Hopkins Center for Data-Driven Reform in Education (CDDRE). Control districts implemented the same initiative, but one year after random assignment. Carlson et al. (2011) utilize the delayed start to investigate the causal impacts of the first year of the CDDRE initiative on student achievement outcomes. The first year of the CDDRE initiative focuses on developing and evaluating quarterly benchmark assessments, reviewing all available data to better understand the needs of the district, and conducting leadership and data interpretation training for district

and school leaders.

The participating districts were selected through an extensive recruitment process. The Department of Education of each state nominated districts with a large number of low performing schools to participate in the study. District officials of the nominated districts were contacted and those that agreed to participate were included in the randomization procedure. Further, for each participating district, the district officials specified which schools in their district they wanted to participate in the experiment. Generally, low performing schools were selected. Following the selection of schools, districts were stratified by recruitment wave and state and randomly assigned to treatment or control. Treatment schools implemented CDDRE data-driven initiative and control schools continued business-as-usual for one year and then implemented the same initiative.

In order to assess the impact of the intervention, results from state-administered achievement tests were collected for each participating school. Carlson et al. (2011) found treatment had a significant impact on student math scores but found no significant effect for reading scores – treatment schools increased students' math scores by 0.059σ (0.029) and increased students' reading scores by 0.033σ (0.020).

In addition to providing constructive feedback for teachers, collecting teacher data could also be a useful tool for leaders in managing their schools. Rockoff et al. (2012) investigate the impact of giving over 200 New York City principals objective performance evaluations of the teachers in their schools. All schools in NYC that contained any grades four through eight were eligible to participate (over 1,000 schools); 223 signed up and completed the necessary survey to be included in the experiment. Participating principals were stratified by grade configuration and assigned randomly to treatment or control. Treatment principals received reports detailing the value-added of the teachers in their school relative to similar teachers in NYC and training on how to use and interpret this data. Rockoff et al. (2012) find evidence that principals do use this information to update their beliefs of the teachers in the school. Using baseline and post-intervention surveys that solicited principals' evaluation of their teachers, they find that treatment principals update their beliefs in the direction of the teacher value-added detailed in the report. Moreover, consistent with a Bayesian learning model, principals put more weight on the teacher value-added information when that information is more precise than their prior beliefs and they put more weight on their prior beliefs when the relative precision is reversed. Providing this information to principals led to an increase in turnover for teachers with low performance estimates and had a positive impact on students' math achievement for students assigned to teachers that remained in the intervention throughout its entirety.

B. Class Size

Project STAR was an experiment carried out in 79 Tennessee schools from 1985 to 1989 where 11,600 students in grades K to 3 were randomly assigned to small classes (13-17 students), regular classes (22-25 students), or regular classes with a full-time aide. At the time, the statewide pupil-to-teacher ratio was 22.3, so regular classes represented close to the average classroom size in the state. At the time of the experiment, kindergarten was not compulsory in Tennessee, so many new students entered schools in first grade. Students who entered a participating school after the 1985-1986 school year were randomly assigned to one of the three types of classes. Additionally, students in regular classes and in regular classes with an aide were randomly reassigned between these two types of classes at the end of kindergarten. However, kindergartners initially assigned to small classes remained in small classes throughout the entire experiment.

Using a student's initial assignment to one of the three groups, Krueger (1999) estimated the impact of reduced class size and teacher aides on an index of scores from the math, reading, and word subtests of the Stanford Achievement Test. Krueger (1999) found that for grades K-3, students scored about five to seven percentile points higher on the index than students assigned to a regular class without an aide. These results correspond to effect sizes in the range of 0.19σ-0.28σ and represent 64 to 82 percent of the white-black test score gap in the data. Additionally, there was some evidence that regular classrooms with aides outperformed regular classrooms without aides—the estimates for aide classrooms tended to be small and positive, but only the first grade results were statistically significant with an impact of 1.48 percentile points. When exploring heterogeneous treatment effects, Krueger (1999) found that smaller class sizes were more effective for students on free lunch and black students.

C. Extended Time

There are very few randomized trials that expose students to higher quantities of schooling. Zvoch and Stevens (2012) show that a summer literacy program has enormous impacts on kindergarten and first grade reading test scores. In this study, the researchers invited students to a five-week summer program that lasted for 3.5 hours a day, four days a week. In the program, students received classroom instruction on fundamental literacy topics, were assigned homework, completed in-class work packets, and practiced literacy skills in small groups with students of a similar skill level. The summer program was typically reserved for struggling students that scored below a cutoff point on the spring standardized tests. However, for this study, the district established upper bounds so that approximately 50 kindergartners and 50 first graders fell in the range between the cutoff scores and the upper bound scores. These students were considered the experimental sample and half were randomly invited to participate in the program. At posttest, Zvoch and Stevens (2012) found that the summer program on average increased reading test scores by 0.69σ for the

kindergarten and first grade students.

However, Holmes and McConnell (1990) utilized a larger sample of students to investigate the impact of full-day versus half-day kindergarten instruction and found no significantly positive impacts. In fact, their study provided evidence that half-day kindergarten students perform better on math achievement tests than full-day kindergartners. The experiment randomly assigned twenty elementary schools to either a full or half-day schedule. Holmes and McConnell found that full-day kindergartners had math scores that were 0.29σ lower and reading scores that were 0.11σ higher than the half-day students.

An experiment that investigated extended day impacts in a slightly older sample was Meyer and Van Klaveren (2013). This experiment randomly invited Dutch 5th, 6th, and 7th grade students to participate in an extended school day program. The program consisted of a classroom of approximately ten students receiving an additional two hours of language instruction, two hours of math instruction, and one hour of excursions per week. Meyer and Van Klaveren found that assignment to treatment increased math scores by 0.087σ (0.067) and increased reading scores by 0.005σ (0.081). Neither of these effects are significant.

Taking the treatment effects at face value, one potential explanation for the patterns in the experimental data is that increasing the amount of time students spend in class per day is not as effective as extending the school year. Put differently, if there are concavities in human capital production as a function of time and students are at the point of diminishing marginal returns for a given day but not for a given year, this can rationalize the findings.

3.3.4 Market-Based Approaches

In recent years, developed countries across the globe have increased the scope of schooling alternatives available to students—an approach long advocated by leading economists (Friedman 1955; Becker 1995; Hoxby 2002). Creating a competitive and active marketplace has the potential to improve educational outcomes because schools would have more incentive to improve in response to increased market pressure. To the extent that match quality between a school and a student is important, school choice programs may also yield benefits simply by increasing the set of schools over which a student is able to choose.

For these approaches to be an effective means of reform, however, it is necessary that students benefit from the opportunity to attend sought-after schools, and that these improvements are apparent to students and parents. The goal of this subsection is to understand the measurable achievement benefits accrued to students when there is more flexibility and school choice.

A. VOUCHERS

There have been a series of important studies that exploit randomized voucher lotteries to estimate the effect of attending a private school for youth at various ages. The Milwaukee voucher program, offering vouchers to a limited number of low-income students to attend one of three private nonsectarian schools in the district, is the most prominent of these. Analyses of this program obtain sharply conflicting estimates of the impact on achievement depending upon the assumptions made to deal with selective attrition of lottery losers from the sample (Witte et al. 1995; Greene et al. 1999; Witte 1997; Rouse 1998). Although in theory randomization provides an ideal context for evaluating the benefits of expanding parental choice sets, in the Milwaukee case, less than half of the unsuccessful applicants returned to the public schools and those who did return were from less educated, lower income families (Witte 1997).

Rouse (1998) used a typical ITT specification to evaluate the Milwaukee voucher program. Comparing lottery winners to lottery losers, she found that being selected for the choice program had significant impacts on math achievement but insignificant impacts on reading. Students who won the lottery scored approximately 1.5-2.3 percentile points (0.08σ-0.12σ) more per year in math compared to lottery losers. This suggests effect sizes on the order of 0.32σ-0.48σ for four years of school. The results in Rouse (1998) are robust to various methods of imputing missing data and attrition from the sample – when imputing missing observations, estimates remained in the range of 1.38 to 2.31 percentile points.

The DC Opportunity Scholarship Program (OSP) is another voucher program that provides up to $7,500 to low-income families in the District of Columbia to send their children to participating private schools. Wolf et al. (2010) use 2,300 applicants to a series of lotteries in 2004 and 2005 to evaluate the impact of the OSP. The study found that the OSP had no impact on student achievement but increased students' chance of graduation. Additionally, parents of students who were offered a scholarship had a higher satisfaction with schools and rated schools as safer. This result is significant regardless of whether a student actually used the offered scholarship or not.

Mayer et al. (2002) present results from the third year of a randomized evaluation of the School Choice Scholarships Foundation Program in NYC. In 1997, the program provided scholarships of up to $1,400 annually for up to four years via lottery to low-income families with students in grades K-4. The scholarship could be used to pay tuition at a religious or secular school of the family's choosing. Fifty-three percent of students who were offered scholarships used the scholarship for at least three full years. The families that did not utilize the offered scholarship claimed they were unable to do so because they were unable to afford the tuition and expenses that the scholarship did not cover or were unable to find a school in a convenient location.

Through parent and student surveys, Mayer et al. (2002) found that the private schools these students

elected to attend were indeed different from the public schools non-participants remained in. Parents with students who switched to private schools reported that the schools had smaller class sizes; were more likely to have computer laboratories, after-school programs, and tutor programs; had less incidents of students destroying property, fighting, cheating, and racial conflict; communicated more with parents; allowed parents to spend less time helping their children with homework; and this resulted in an overall higher level of satisfaction with their students' school. Students who switched reported that students in private school were more likely to get along with teachers, were more proud of their school, were less likely to be put down by teachers, and were asked to complete more homework. Additinoally, students reported that the private schools had stricter behavior rules, and there was a lower prevalence of cheating.

Although there was evidence that students offered a scholarship switched to better school environments, Mayer et al. (2002) found that three years after random assignment, there was no average treatment effect on students' performance. Moreover, students who ever attended a private school and students who attended for all three years did no better than students who never attended a private school. These results are robust across grade levels, but there is evidence of heterogeneous treatment effects across races – Mayer et al. (2002) found positive effects on the standardized test scores of black students.

The meta-coefficient on voucher experiments is 0.024σ (0.021) for math achievement and 0.030σ (0.024) for reading achievement. Relative to their popularity with politicians, the lack of effectiveness of voucher programs is surprising. Rather than focusing on achievement, many use a revealed preference argument to conclude families who make active choices – even if achievement is unaffected – are better off.

Before one dismisses them entirely, there are two key pieces of data missing on voucher experiments. First, in the average voucher experiment a student enrolls in a private school between grades K and 8. There is no experiment that tests the full pre-K through high school graduation treatment. This seems essential.

Second, although vouchers are a market-based reform, we do not know what happens if there are enough vouchers in a concentrated area to allow the market to respond by altering the supply (and scope) of schools available to educate disadvantaged children. Because all the experiments have been relatively small, one cannot assess the potential general equilibrium effects.[27]

[27] A notable counter example is a recent experiment implemented in India. Muralidharan and Sundararaman (2015) conducted an experiment using 180 villages from the Indian state of Andhra Pradesh in which they randomly assigned villages to treatment or control and then awarded private school vouchers to public school applicants through random lotteries in the treatment villages. Two and four years after randomization, they found that winning a voucher had no impact on Telugu (native language), math, English, and science/social studies achievement. However, the program had large impacts on Hindi test scores, a subject not taught in public schools. Since private schools are approximately a third of the cost of public schools, Muralidharan and Sundararaman (2015) conclude that private schools are a much more cost-effective way of teaching students. Further, they found no evidence of spillovers (negative or positive) on the achievement of public school students that did not apply to the voucher program or on non-voucher private students. This suggests that vouchers are a cost-effective way to potentially increase student achievement without any negative externalities.

An ideal voucher experiment might take a large state with multiple school districts and randomly implement voucher programs or Education Savings Accounts in half of the districts and analyze both student achievement and the market response. The vouchers could be risk adjusted – more disadvantaged children receive more school funding – or contain location preferences that would induce a more aggressive supply response in blighted communities. These ideas only scratch the surface of what is possible and have not been evaluated in a compelling way. Thus, whether the Friedman (1955) vision for public schools is effective at producing human capital is still unknown.

B. School Choice

Cullen, Jacob, and Levitt (2006) present causal estimates of the impact of school choice on a variety of student outcomes. Specifically, they utilize the random lotteries of oversubscribed schools in Chicago's open enrollment system. This system allows students to apply to public magnet schools and programs outside of their neighborhood school.[28] When oversubscribed, many Chicago Public Schools (CPS) use random lotteries to offer admission to students. The authors obtained the results of 194 such lotteries from 19 high schools in CPS. The final sample consisted of 14,434 students who applied to these 19 choice schools in the spring of 2000 and 2001.

The analysis in Cullen, Jacob, and Levitt (2006) finds little evidence that winning a lottery has any impact on traditional achievement measures such as test scores, graduation rates, attendance rates, or courses taken. These results are robust to a variety of sensitivity analyses and are similar across student subgroups. In an attempt to better understand the findings, the authors explored potential mechanisms that could explain the zero-impact on academic outcomes. They found little evidence of lottery winners and losers attending similar schools (lottery winners attended schools with higher average achievement, lower poverty rates, and higher graduation rates), of choice schools substituting for parental involvement, or of travel costs and disruption of peer groups interfering with academic success. Therefore, the results in Cullen, Jacob, and Levitt (2006) seem to suggest that the measurable school inputs of these choice schools have little causal impact on students' academic outcomes.

Another possibility is that students and parents apply to choice schools for non-academic reasons. Using survey data collected by the Consortium on Chicago School Research for CPS students in grades 6-10 in spring 2001, the authors investigated this possibility. They found evidence of some positive effects on non-traditional outcomes, possibly supporting the hypothesis that students and parents choose choice schools for non-academic reasons. Cullen, Jacob, and Levitt (2006) found that lottery winners report fewer incidents of

[28] Magnet schools are different from traditional public schools in that each magnet school tends to have a specific educational theme and students can choose to enroll in a school based on their interest in a school's theme.

disciplinary action, fewer arrests, and lower incarceration rates. However, lottery winners are not statistically different from lottery losers for other outcomes such as liking school, trusting their teachers, and having high expectations for the future.

Another example of a school choice experiment is Connecticut's interdistrict magnet school program. In 1996, the Connecticut Supreme Court ruled that students in Hartford public schools were denied equal educational opportunities due to racial and economic isolation. One of the state's many responses was to foster the growth of interdistrict magnet schools. A decade after the the Connecticut Supreme Court's ruling, there were 54 magnet schools in operation in Connecticut and 41 of these served students residing in Hartford, New Haven, or Waterbury. Additionally, interdistrict magnets serve two or more districts and all students residing in these districts are eligible to enroll in the school. Urban students that elect to attend magnet schools are typically moving to schools where there are fewer students eligible for free lunch, more white students, and higher average scores on standardized mathematics and reading tests.

Bifulco et al. (2009) evaluated the impact of Connecticut's interdistrict magnet schools using the random admission lotteries of two oversubscribed magnets serving Hartford and four surrounding suburban districts. One of these schools served grades 6-8 and the other served grades 6-12. The authors obtained admission data for the 2003 and 2004 sixth grade lotteries at these schools as well as student-level test scores from the Connecticut State Department of Education for the 2001-2002 to 2006-2007 school years. The final sample for these two schools consisted of 553 students in 12 oversubscribed lotteries (both schools conducted lotteries by district for each year), 164 of which were eventually offered admission to one of the two magnets. Comparing the eighth grade outcomes of lottery winners to lottery losers, Bifulco et al. (2009) find that students offered admission to the magnet schools scored 0.109σ higher on math and 0.252σ higher on reading tests, of which only the latter was statistically significant at conventional levels.

C. Charter Schools

A charter school is a school that receives public funding but operates independently of the established public school system in which it is located. They exist (and are increasing in demand) across the developed world – from Australia to England and Wales. Figure 2 shows the increase in the number of students attending charter schools in the United States and England.

When originally conceived, charter schools offered two distinct promises: First, they were to serve as an escape hatch for students in failing schools. Second, they were to use their legal and financial freedoms to create and incubate new educational practices that could be used to inform traditional public schools with new ideas and fresh approaches.

In America, charter schools currently enroll almost four percent of all students. Some of these schools have shown remarkable success in increasing test scores – closing the racial achievement gap in just a few years. For example, schools such as the Success Academy Charter Schools in New York City, YES Prep in Houston, and charter schools in the Harlem Children's Zone have become beacons of hope, demonstrating the enormous potential to improve student achievement in the most blighted communities. Others, however, have failed to increase achievement and have actually performed substantially worse than their traditional counterparts. In this scenario, students would have been better off not attending a charter school.

Evaluating Charter Schools

The method for evaluating charter schools is remarkably consistent across the literature.[29] The literature estimates two empirical models – ITT effects and Local Average Treatment Effects (LATEs) – which provide a set of causal estimates of the impact of attending a charter school on outcomes. The ITT estimates measure the causal effect of winning a charter lottery by comparing the average outcomes of students who 'won' the lottery to the average outcomes of students who 'lost' the lottery:

$$outcome_i = \mu + \gamma X_i + \pi Z_i + \sum_j \nu_j Lottery_{ij} + \sum_j \phi_j Lottery_{ij} * 1(sibling_i) + \eta_i \quad (1)$$

where Z_i is an indicator for winning an admissions lottery, and X_i includes controls for student-level demographics such as gender, race, special education status, eligibility for free or reduced-price lunch, receipt of Limited English Proficiency (LEP) services, and a quadratic in two prior years of math and ELA test scores. $Lottery_{ij}$ is an indicator for entering the lottery in year j, and $1(sibling_i)$ indicates whether student i had a sibling enter the lottery in the same year.[30] Equation (1) identifies the impact of *being offered a chance* to attend a charter school, π, where the lottery losers form the control group corresponding to the counterfactual state that would have occurred for students in the treatment group if they had not been offered a spot in the charter school. Using this approach, the literature on charter effectiveness has quickly amassed an interesting set of facts.

First, the typical charter school is no more effective at increasing test scores than the typical traditional public school (Gleason et al. 2010). Evaluations that encompass the most representative samples of charter schools show little impact. Using lottery admissions data for 36 charter schools from around the nation, Gleason et al. (2010) investigated the impact of charter schools on student outcomes. They found that two

[29]The national charter school studies released by the Center for Research on Education Outcomes (CREDO) are anomalous in that they use observational data instead of randomized admissions lotteries (Center for Research on Education Outcomes 2013).

[30]In typical charter lotteries, an offer is extended to all siblings when multiple siblings enter the same lottery and one sibling wins.

years after the random lotteries, students who won the lotteries scored, if anything, lower on standardized test scores than students who lost the lotteries.[31] In addition, this national sample of charter schools had no impact on students' math and reading proficiency levels, number of days absent, and grade promotion. Gleason et al. (2010) found no impact of charter schools on student behavior and school disciplinary action, but a higher fraction of lottery winners showed up late to school five or more days. Although the average charter school included in their study did not have any positive impacts on student outcomes, Gleason et al. (2010) found large, positive, and statistically significant impacts – ranging from 0.07σ to 0.94σ – on every measure of students' and parents' satisfaction with and perceptions of school.

Second, an emerging body of research suggests that high-performing charter schools can significantly increase the achievement of poor urban students. Students attending over-subscribed Boston-area charter schools score approximately 0.4σ higher per year in math and 0.2σ higher per year in reading (Abdulkadiroglu et al. 2011). Promise Academy students in the Harlem Children's Zone (HCZ) score 0.229σ higher per year in math and 0.047σ higher per year in reading (Dobbie and Fryer 2011). Students in the Knowledge is Power Program (KIPP) schools – America's largest network of charter schools – score 0.180σ higher per year in math and 0.075σ higher per year in reading (Tuttle et al. 2013; Angrist et al. 2011). The SEED urban boarding school in Washington D.C., demonstrates similar test score gains (Curto and Fryer 2014).

Third, charter schools are more effective at increasing math scores than reading scores. Abdulkadiroglu et al. (2011) and Angrist et al. (2011) find that the treatment effect of attending an oversubscribed charter school is four times as large for math as reading. Dobbie and Fryer (2011) demonstrate effects that are almost 5 times as large in middle school and 1.6 times as large in elementary school in favor of math. In larger samples, Hoxby and Murarka (2009) report an effect size 2.5 times as large in New York City charters, and Gleason et al. (2010) show that an average urban charter school increases math scores by 0.16σ with statistically 0 effect on reading.

According to the National Alliance for Public Charter Schools, the median grade served by charter schools in the U.S. is sixth grade (usually students are 11-12 years old). However, the achievement data necessary to conduct evaluations of charter schools is typically not available for kindergarten through second grade students, so the average grade evaluated is most likely even higher. The theory and empirical findings discussed above suggest that the relatively late timing of charter school "interventions" might be an important factor in the observed differential impacts by subject.

[31] The two-year ITT impact for the pooled sample is -0.08σ (p-value = 0.032) for reading scores and -0.06σ (p-value = 0.136) for math test scores. Note that pooling results together masks heterogeneous treatment effects described in the paper. For example, charter schools in large urban areas had a 0.16σ impact on math scores while schools outside of large urban areas had a -0.14σ impact. Both of these impacts were statistically significant.

Another leading theory posits that reading scores are influenced by the language spoken when students are outside of the classroom (Rickford 1999; Charity, Scarborough, and Griffin 2004). Charity, Scarborough, and Griffin (2004) argue that if students speak nonstandard English at home and in their communities, increasing reading scores might be especially difficult. This theory is consistent with the data and could explain why students at an urban boarding school make similar progress on reading and math (Curto and Fryer 2014).

Fourth, there are important features of charter schools that seem to be correlated with their level of student achievement. It is important to note that these analyses are non-experimental. Angrist et al. (2013) argue that both the urbanicity of charter schools and whether they adopt the so called "No Excuses" approach to culture and discipline are positive predictors of charter treatment effects.

Dobbie and Fryer (2013) provide evidence on the determinants of charter school effectiveness by collecting data on the inner-workings of 29 charter schools in New York City and correlating these data with lottery-based estimates of each school's effectiveness. Information on school practices were collected from a variety of sources. Principal interviews asked about teacher development, instructional time, data driven instruction, parent outreach, and school culture. Teacher interviews asked about professional development, school policies, school culture, and student assessment. Student interviews asked about school environment, school disciplinary policy, and future aspirations. Lesson plans were used to measure curricular rigor. Videotaped classroom observations were used to calculate the fraction of students on task throughout the school day.

School effectiveness is estimated by exploiting the fact that oversubscribed charter schools in New York City are required to admit students via random lottery. The variability inherent in the set of NYC charter schools, combined with rich measures of school inputs and lottery-based estimates of each school's impact on student achievement, provides an ideal opportunity to understand which inputs best explain school effectiveness. This, coupled with some of the best practices of our meta-analysis, provide the intellectual backbone of the randomized field trial discussed below.

Dobbie and Fryer (2013) find that input measures associated with a traditional resource-based model of education – class size, per pupil expenditure, the fraction of teachers with no teaching certification, and the fraction of teachers with an advanced degree – are not correlated with school effectiveness in our sample. Indeed, our data suggest that increasing resource-based inputs may marginally lower school effectiveness. On the surface, this evidence may seem inconsistent with the important results reported in Krueger (1999). There are a few ways to reconcile this. First, Dobbie and Fryer (2013) analyzes charter schools in NYC, whereas Krueger (1999) uses a sample of traditional public schools in Tennessee. Second, the variation in

Dobbie and Fryer (2013) comes from only 39 charter schools with relatively similar class sizes, whereas the thousands of treatment students in Krueger (1999) are placed in classrooms that are almost 40% smaller than control classrooms. Third, Krueger's analysis focused on students in grades kindergarten through third whereas the correlations in Dobbie and Fryer (2013) used third through eighth grade test scores. Fourth, and most important, the analysis in Krueger (1999) is experimental.

In stark contrast, Dobbie and Fryer (2013) demonstrate that an index of five policies suggested by forty years of human capital research – frequent teacher feedback, data-driven instruction, high-dosage tutoring, increased instructional time, and a relentless focus on academic achievement – explains roughly half of the variation in school effectiveness in both math and reading.

4 Combining What Works: Evidence from a Randomized Field Experiment in Houston

Improving the efficiency of the production of human capital is of great importance across the developed world. The United States spends $10,768 per pupil on primary and secondary education, ranking it fourth among OECD countries (Aud et al. 2011). Yet, among these same countries, American fifteen year-olds rank twenty-fifth in math achievement, seventeenth in science, and fourteenth in reading (Fleischman 2010). This is not a phenomenon that is unique to the United States. Other OECD countries are unable to translate large amounts of educational spending into educational success. For example, the two countries ranking directly behind the United States with per pupil primary and secondary spending of $9,959 and $9,448 are, respectively, Austria and Denmark (Aud et al. 2011). However, Austrian fifteen year-olds rank eighteenth in math achievement, twenty-fourth in science, and thirty-first in reading and Danish fifteen year-olds rank thirteenth in math, twentieth in science, and nineteenth in reading (Fleischman 2010).

Traditionally, there have been two approaches to increasing educational efficiency: (1) expand the scope of available educational options in the hope that the market will drive out ineffective schools, or (2) directly manipulate inputs to the educational production function.[32]

As our meta-analysis demonstrates, market-based reforms such as school choice or school vouchers have, at best, a modest impact on student achievement. This suggests that these approaches – implemented in their current form – are unlikely to significantly increase the efficiency of the public school system, subject to the important caveats discussed in the previous section.

[32] Increasing standards and accountability reflect a third approach to education reform. There is evidence that increased accountability via the No Child Left Behind Act had a positive impact on math test scores (though not reading test scores) and on wages (Dee and Jacob 2011; Deming et al. 2013).

Another approach is to inject the best practices known from the set of randomized field experiments completed to date – along with the correlates gleaned from analyzing the inner-workings of successful charter schools – in an experiment in traditional public schools. This is precisely the goal of Fryer (2014).

Between the 2010-2011 and 2012-2013 school years, Fryer (2014) implemented important elements of the above education best-practices in twenty of the lowest performing schools (containing more than 12,000 students) in Houston, Texas.

To increase time on task, the school day was lengthened by one hour and the school year was lengthened by ten days in the nine secondary (middle and high) schools. This was 21 percent more time in school than students in these schools spent in the pre-treatment year and roughly the same as achievement-increasing charter schools in New York City. In addition, students were strongly encouraged and even incentivized to attend classes on Saturday. In the eleven elementary schools, the length of the day and the year were not changed, but non-instructional activities (e.g. twenty-minute bathroom breaks) were reduced. This is consistent with the correlations in Dobbie and Fryer (2013) and the randomized field trial reported in Meyer and Van Klaveren (2013).

In an effort to improve the human capital available to teach students and lead schools, nineteen out of twenty principals were removed and 46 percent of teachers left or were removed before the experiment began. Some teachers left because they believed the program was too disruptive. Others were removed because they were too resistant to the changes. Any teacher, independent of skill level, who demonstrated a desire to implement the proposed changes with fidelity was retained. As part of the turnaround efforts, teachers received both managed professional development and frequent feedback as a part of a more holistic evaluation system. The managed professional development was similar to the Success for All treatment described in Borman et al. (2007). The frequent feedback was similar to the quasi-experimental program evaluated in Taylor and Tyler (2012).

To enhance student-level differentiation, all fourth, sixth and ninth graders received high-dosage math tutoring and extra reading or math instruction was provided to students in other grades who had previously performed below grade level. Similar to the Chicago BAM experiment described above, the tutoring model was adapted from the MATCH school in Boston – a charter school that largely adheres to the methods described in Dobbie and Fryer (2013).

In order to help teachers use interim data on student performance to guide and inform instructional practice, schools were required to administer interim assessments every three to four weeks and provided with three cumulative benchmark assessments, as well as assistance in analyzing and presenting student performance data on these assessments. Yet, as Rockoff et al. (2012) and Dobbie and Fryer (2013) demonstrate,

data alone is not enough. Dobbie and Fryer (2013) argue that the use of interim assessment data is only correlated with achievement for schools who can articulate a precise plan of how they will change student grouping or pedagogy or some other strategy in response to the data.

Finally, to instill a culture of high expectations and college access, we started by setting clear expectations for school leadership. Schools were provided with a rubric for the school and classroom environment and were expected to implement school-parent-student contracts. Specific student performance goals were set for each school and the principal was held accountable and provided with financial incentives based on these goals.

Such invasive changes were possible, in part, because eleven of the twenty schools (nine secondary and two elementary) were either "chronically low performing" or on the verge of being labeled as such and subject to takeover by the state of Texas. Thus, despite our best efforts, random assignment was not a feasible option for these schools. To round out our sample of twenty schools and provide a way to choose between alternative quasi-experimental specifications, we randomly selected nine additional elementary schools (vis-à-vis matched-pairs) from eighteen low – but not chronically low – performing schools. One of the randomly selected treatment elementary schools closed before the start of the experiment so we had to drop it and its matched pair from our experimental sample. Thus, our final experimental sample consists of sixteen schools.

In the sample of sixteen elementary schools in which treatment and control were chosen by random assignment, providing estimates of the impact of injecting charter school best practices in traditional public schools is straightforward. In the remaining set of schools, we use three separate statistical approaches to understand the impact of the intervention. Treatment is defined as being zoned to attend a treatment school for entering grade levels (e.g. sixth and ninth) or having attended a treatment school in the pre-treatment year for returning grade levels. "Comparison school" attendees are all other students in Houston. We begin by using district administrative data on student demographics and, most importantly, previous years' achievement, to fit least squares models. We then present two empirical models that instrument for a student's attendance in a treatment school with original treatment assignment.

All statistical approaches lead to the same basic conclusions. Injecting best practices from charter schools into low performing traditional public schools can significantly increase student achievement in math and has marginal, if any, effect on English Language Arts (hereafter known simply as "reading") achievement. Students in treatment elementary schools gain around 0.184σ in math per year, relative to comparison samples. Taken at face value, this is enough to eliminate the racial achievement gap in math in Houston elementary schools in approximately three years. Students in treatment secondary schools gain 0.146σ per year in math, decreasing the gap by one-half over the length of the demonstration project. The

impacts on reading for both elementary and secondary schools are small and statistically zero.

In the grade/subject areas in which we implemented all five policies described in Dobbie and Fryer (2013) – fourth, sixth, and ninth grade math – the increase in student achievement is substantially larger than the increase in other grades. Relative to students who attended control schools, fourth graders in treatment schools scored 0.331σ (0.104) higher in math, per year. Similarly, sixth and ninth grade math scores increased 0.608σ (0.093), per year, relative to students in comparison schools.

4.1 Simulating the Potential Impact of Implementing Best Practices in Education on Wage Inequality

An important question is how much of the initial gaps described in the introduction to this chapter might be eliminated if state, local, and federal governments focused on the experiments proven most effective through randomized trials. Answering this question is, by definition, speculative – as it relies on extrapolations from cross-sectional relationships and assumptions on how human capital propagates through an individual's life. Still, the exercise may be informative and we include it here as an illustrative exercise.

Data on long-term follow-ups is sparse. Perry Preschool, the Abecedarian Project, and the Moving to Opportunity experiments are notable exceptions. As described above, MTO revealed that despite having no significant impacts on children's academic outcomes, better neighborhoods had important impacts on the adulthood outcomes of children – treatment MTO children who were younger than 13 years old at randomization had 31% higher income, had higher college attendance rates, were less likely to be single parents, and lived in better neighborhoods relative to similar individuals in the control group. At posttest, the famous early childhood programs Perry Preschool and the Abecedarian Project had large impacts on children's achievement scores. At age 40, treatment students from Perry Preschool had higher high school completion rates (77% vs. 60%), were more like to be employed (76% vs. 62%), had higher median annual earnings ($20,800 vs. $15,300), were more likely to own a house (37% vs. 28%), were more likely to have a savings account (76% vs. 50%), and had better crime outcomes, self-reported health, and family-outcomes compared to the control group (Schweinhart et al. 2005). Similarly, at the age 30 follow-up, treatment students from the Abecedarian Project had significantly higher levels of educational attainment (13.46 years versus 12.31 years), were 17 percentage points more likely to hold a bachelor's degree (CM = 6%), were 22 percentage points more likely to work full-time (CM = 53%), and were six times less likely to receive public assistance for more than 10% of the preceding seven years than students who were assigned to control.

In the absence of more long term outcomes for the vast majority of randomized field trials, we follow the methods described in Winship and Owen (2013) and simulate a life-cycle model similar to the Social

Genome Model (SGM).[33] The SGM is a useful tool to simulate how shocks in a given life-stage may carry over to later life outcomes. For example, one can simulate how much increasing reading test scores in early childhood by 0.4σ would impact income at age 40. We can thus use this simulation – coupled with data on treatment effects from the meta-analysis – to investigate what sort of income benefits might accrue if we simply implement best practices. Winship and Owen (2013) provide evidence that the SGM reasonably replicates key adult impacts of the Perry Preschool experiment, the Abecedarian Project, and the Chicago Child-Parent Centers program. We find similar results.

4.1.1 Interpreting the Literature Through A Simple Life-Cycle Model

The model draws from the vast literature of human capital formation and assumes that cognitive and non-cognitive skill formation varies across an individual's lifetime and is dependent on the stock of skills in previous stages of life. Specifically, Winship and Owen (2013) define six different life-stages: circumstances at birth (CAB), early childhood (EC), middle childhood (MC), adolescence (AD), transition to adulthood (TTA), and adulthood (AH). The empirical model uses linear structural equations to describe the dependencies between the outcomes in a given stage and all revealed outcomes from the stages preceding it. Formally, given a vector of circumstances at birth, *CAB*, for individual *i*, each outcome in the vector of early childhood outcomes, *EC*, is modeled as

$$EC\ Outcome_i = \beta_0^{ec} + \beta_{cab}^{ec} CAB_i + \varepsilon_i^{ec}.$$

Similarly, each of the *MC* outcomes is given by

$$MC\ Outcome_i = \beta_0^{mc} + \beta_{cab}^{mc} CAB_i + \beta_{ec}^{mc} EC_i + \varepsilon_i^{mc}.$$

For the adolescent life-stage we have

$$AD\ Outcome_i = \beta_0^{ad} + \beta_{cab}^{ad} CAB_i + \beta_{ec}^{ad} EC_i + \beta_{mc}^{ad} MC_i + \varepsilon_i^{ad}.$$

Outcomes when transitioning to adulthood would be

$$TTA\ Outcome_i = \beta_0^{tta} + \beta_{cab}^{tta} CAB_i + \beta_{ec}^{tta} EC_i + \beta_{mc}^{tta} MC_i + \beta_{ad}^{tta} AD_i + \varepsilon_i^{tta}.$$

[33] Due to there being no source code available – even upon request – and limited description in the SGM guide, we constructed the model using our own assumptions about the cleaning, creation, and merging of the data. This leads to a final dataset used for the simulations that is different from the one described in Winship and Owen (2013). However, when comparing the simulated impacts reported in published papers using SGM to estimated impacts of the simulations, they are quite similar. We provide the code and data in an online appendix.

And finally, adult outcomes are modeled as

$$AH\ Outcome_i = \beta_0^{ah} + \beta_{cab}^{ah} CAB_i + \beta_{ec}^{ah} EC_i + \beta_{mc}^{ah} MC_i + \beta_{ad}^{ah} AD_i + \beta_{tta}^{ah} TTA_i + \varepsilon_i^{ah}.$$

Where β_ψ^λ are the partial correlations of realized outcomes from the ψ life-stage ("0" represents an intercept) with the given LHS outcome in the λ life-stage.

With a rich enough dataset, one can obtain the correlations linking all CAB, EC, MC, AD, TTA, and AH outcomes together and investigate the indirect and direct impacts of varying one outcome on another. Importantly, we could then use the structural equations of this model to predict how a shock in earlier life-stages will propagate to outcomes in adulthood.

4.1.2 Simulating the Social Genome Model

Unfortunately, as discussed by Winship and Owen (2013), there is not yet a reliable dataset that follows an individual from birth through adult outcomes. Therefore, in order to conduct simulations using the above model, we combine two well known public datasets: the National Longitudinal Survey of Youth 1979 (NLSY79) and the NLSY79 Child and Young Adult survey (CNLSY). From the CNLSY, we observe CAB, EC, MC and AD outcomes. From the NLSY79, we observe TTA and AH outcomes. See Table 3 for a list of the specific variables that were used for each life-stage. The variables include a mix of cognitive skills (e.g. standardized test scores), non-cognitive skills (e.g. self esteem and hyperactivity indices), and important life outcomes (e.g. teen birth, drug use, and graduation).

Using these two datasets and the equations above, we are able to estimate the coefficients for each outcome in a life-stage. However, an issue arises in linking the life-stages across these two data sources. Due to the age of respondents at first interview in the NLSY79, the data from earlier life stages is not as rich as in the CNLSY. Therefore, the NLSY79 does not contain all of the CAB, EC, MC, and AD variables that the CNLSY has. In order to overcome this, we define a set of linking variables, *LINK*, that contains all outcomes that are available in both the NLSY79 and the CNLSY. We can then estimate the following two equations in the NLSY79 dataset to obtain coefficients for each TTA and AH outcome:

$$TTA\ Outcome_i = \beta_0^{tta} + \beta_{link}^{tta} LINK_i + \varepsilon_i^{tta}$$

$$AH\ Outcome_i = \beta_0^{ah} + \beta_{link}^{ah} LINK_i + \beta_{tta}^{ah} TTA_i + \varepsilon_i^{ah}.$$

Using all of the coefficients generated from these estimations and the CNLSY data, we can then build a synthetic baseline dataset of birth to age 40 outcomes for the CNLSY sample. Given the impact of

an intervention at some life-stage, we can then use the same coefficients to propagate the effects of the intervention through the life-stages of these individuals. Comparing a post-intervention estimation of an outcome to the baseline estimation would then provide us with an estimated impact of the intervention on the given outcome. See Winship and Owen (2013) and our Online Appendix B for a more in depth discussion of the estimation process.

4.1.3 Simulating Impacts on Income

As mentioned, our simulations are, at best, illustrative. Relying on cross-sectional correlations, making important (untestable) assumptions on the law of motion of human capital development, and assuming that the variation induced by experiments would largely be consistent across groups and time are necessary for our exercise. If, as Cunha and Heckman (2010) argue, "skills beget skills and abilities beget abilities" these assumptions are overly restrictive and will bias our estimates downward. If on the other hand, as many economists might find natural, there are diminishing marginal returns to interventions, then the forthcoming estimates are too large.

If public policy were to implement the most successful math and reading interventions when children are in early childhood, middle childhood, and adolescence, the expected test score increase would be 1.192σ in math and 2.449σ in reading.[34] [35] Using the model, the math impact would translate into a 8.28% increase in income at age 40 and the reading impact would translate into a 25.06% increase.[36] Table 4 presents the average successful impact for each category-life-stage and the estimated impact on income at age 40 if only an intervention with that effect was implemented.

Whether or not the cumulative impact is enough to eliminate racial wage inequality depends on one's ability to "tag" (in the sense of Akerlof 1978) minorities among other things. We won't hazard a quantitative guess, but qualitatively it seems clear that adhering to the best practices gleaned from the literature on randomized field trials discussed in this chapter would significantly reduce, if not eliminate, much of the gap between racial groups in wages and other important economic and social outcomes.

[34] For the time being, there are no RCTs that estimate effects on children's math and reading abilities during the circumstances at birth life-stage. Potential studies that focused on infants that our search returned were mostly excluded from our meta-analysis for not using standardized math or reading measures.

[35] As to not give too much weight in this exercise to any one study, we approximate the impact of the "most successful" intervention for each life-stage as the average of the three largest statistically significant impacts from each category. If there were no significant studies for a given category-life-stage, we assign an impact of zero. The cumulative impacts stated are the sum of the five averages from the category-life-stages – early childhood, home (middle childhood), school (middle childhood), home (adolescence), and school (adolescence). This simple approximation assumes impacts are linearly additive one-time shocks and experiments are externally valid.

[36] Using the cross-sectional estimates generated by Chetty et al. (2014), the expected income gain at age 28 from a 1.192σ increase in standardized math scores is 15.62% and from a 2.449σ increase in standardized reading scores is 32.08%.

5 Conclusion

The review of 196 randomized field experiments designed to increase human capital production unearthed several facts. Early childhood investments, on average, significantly increase achievement. Yet, experiments that attempt to alter the home environment in which children are reared in have shown very little success at increasing student achievement. Among school experiments, high-dosage tutoring and 'managed' professional development for teachers have shown to be effective. Ironically, high-dosage tutoring of adolescents seems to be as effective – if not more effective – than early childhood investments. This argues against the growing view that there is a point at which investments in youth are unlikely to yield significant returns (Carniero and Heckman 2003; Cullen et al. 2013). Lastly, charter schools can be effective avenues of achievement-increasing reform, though the evidence on other market-based approaches such as vouchers or school choice have less demonstrated success.

These facts provide reason for optimism. Through the systematic implementation of randomized field experiments designed to increase human capital of school-aged children, we have substantially increased our knowledge of how to produce human capital and have assembled a canon of best practices. And, in an illustrative simulation exercise, we demonstrate that focusing on what we know has the potential to increase income and reduce racial wage inequality.

The question is: do we have the courage to implement, at scale, human capital policies based on best practices developed from these randomized experiments?

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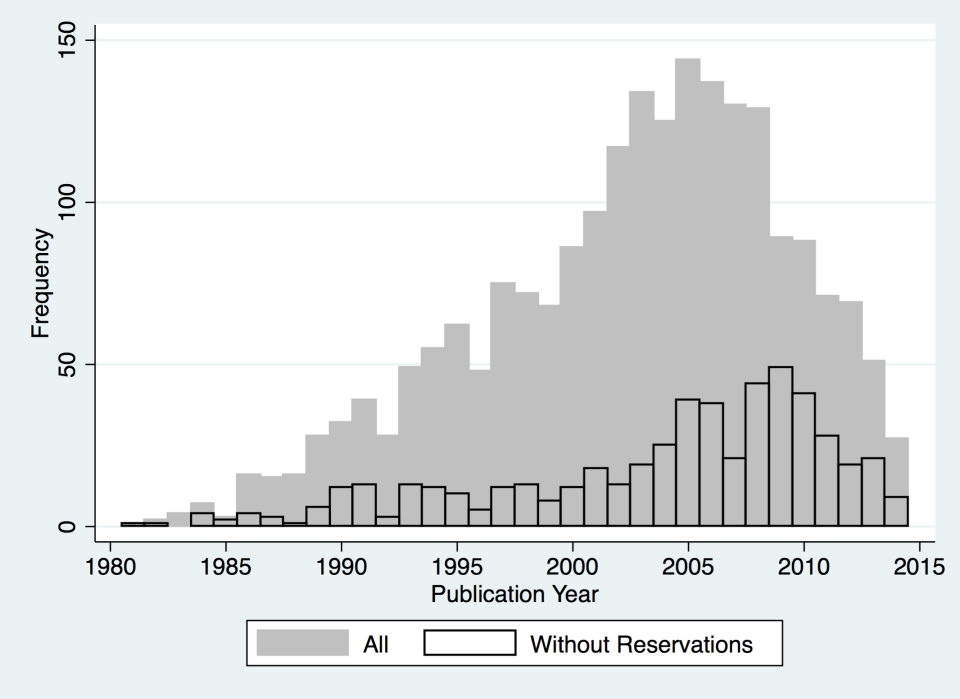
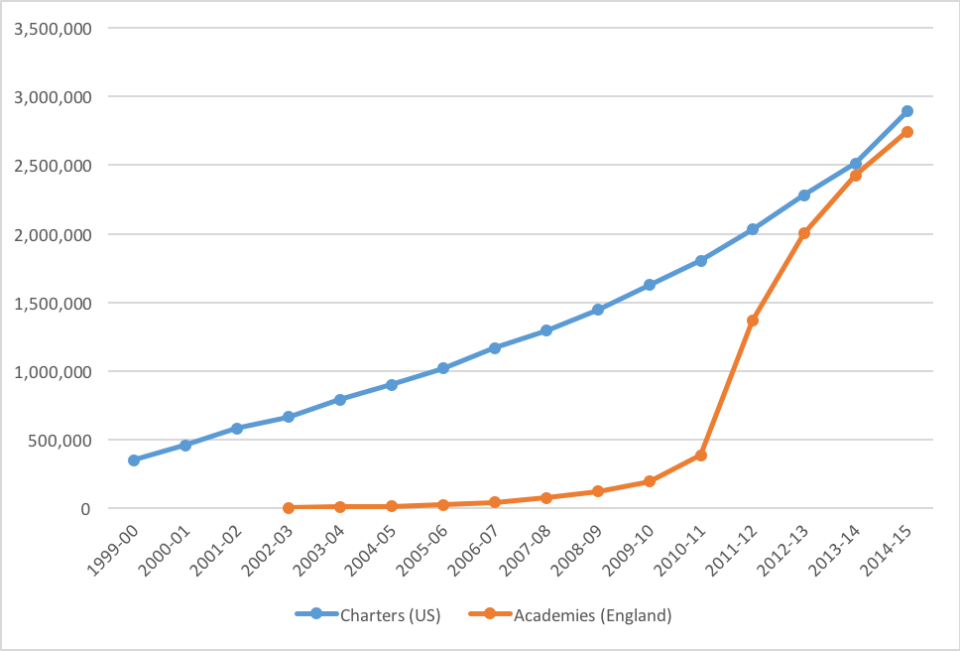
Figure 1
Reviewed WWC Studies



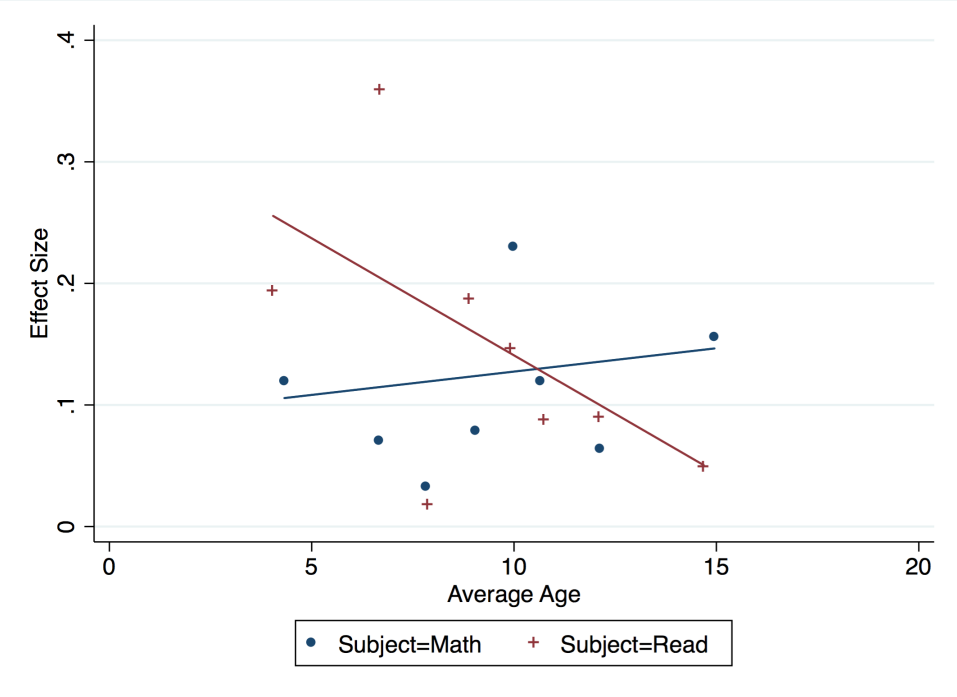
Notes: This figure presents the number of reviewed studies in the What Works Clearinghouse (WWC) by publication year of the studies. The shaded histogram is the sample of all studies in WWC. The clear histogram is the sample of studies that met WWC's standards without reservation.

Figure 2
Number of Students in "Charters"



Notes: This figure presents the number of students enrolled in charter schools (U.S.) and academies (England) for the 1999-00 to 2014-15 school years. Academies are publicly funded independent schools. Similar to the U.S. charter schools, academies don't have to follow the national curriculum and term lengths. The U.S. data comes from the National Alliance for Public Charter Schools (NAPCS) and the data for England comes from the U.K. Department of Education. Note that the U.S. 2014-2015 number is estimated (NAPCS 2015).

Figure 3
Correlation of Effect Size and Average Age of Intervention



Notes: This figures plots annual effect sizes versus average age of students in an intervention by subject. The sample includes all studies that passed our selection criteria for the meta-analysis. Each binned scatter plot was created by separating the data for the given subject into 8 equal-sized bins, computing the mean of the average age and effect sizes within each bin, then creating a scatter plot of these data points. The solid lines shows the best linear fit estimated on the underlying unbinned data estimated using a simple OLS regression.

Table 1: Paper Accounting

	Number of Papers
Panel A: Titles Found	(1)
From Broad Search	$\approx$ 8,000
Selected for Further Review	859
TOTAL INCLUDED	196
Panel B: Reason For Exclusion	
College Sample/Outcomes	42
Design Issues	96
Countries w/o Very High HDI	57
Insufficient Info	24
Paper Not Located	10
No Standardized Reading or Math	356
Repeat Paper	70
Sample Issues	8

Notes: This table summarizes our search procedure for selecting papers for inclusion. Panel A displays the approximate number of titles our initial broad search returned, the number selected for further review, and the final sample of papers. Of the titles selected for further review, Panel B reports the number of papers that were excluded for the given reason. See Online Appendix A for details on each exclusion restriction.

Table 2: Meta-Analysis

	Math			Reading		
	Unweighted Average	Fixed Effects	Random Effects	Unweighted Average	Fixed Effects	Random Effects
Panel A: Early Childhood	(1)	(2)	(3)	(4)	(5)	(6)
ALL	0.120	0.111	0.111	0.202	0.106	0.189
	(0.028)	(0.031)	(0.031)	(0.027)	(0.012)	(0.027)
		20			44	
Panel B: Home						
ALL	0.039	-0.004	-0.004	0.078	0.010	0.010
	(0.045)	(0.008)	(0.008)	(0.052)	(0.007)	(0.007)
		8			22	
Parental Involvement	0.122	-0.001	-0.001	0.143	0.009	0.034
	(0.115)	(0.021)	(0.021)	(0.103)	(0.021)	(0.050)
		3			11	
Educational Resources	-0.060	-0.060	-0.060	0.072	0.015	0.015
	(0.000)	(0.050)	(0.050)	(0.063)	(0.014)	(0.014)
		1			7	
Poverty Reduction	0.008	0.008	0.008	0.022	0.016	0.016
	(0.001)	(0.029)	(0.029)	(0.011)	(0.024)	(0.024)
		2			4	
Panel C: School						
ALL	0.135	0.035	0.053	0.203	0.023	0.069
	(0.022)	(0.004)	(0.009)	(0.028)	(0.004)	(0.011)
		72			98	
Student Incentives	0.039	0.016	0.024	0.097	0.016	0.021
	(0.026)	(0.011)	(0.018)	(0.072)	(0.011)	(0.017)
		5			8	
High Dosage Tutoring	0.393	0.309	0.309	0.405	0.217	0.229
	(0.095)	(0.106)	(0.106)	(0.047)	(0.030)	(0.033)
		6			25	
Low Dosage Tutoring	0.074	0.015	0.015	0.050	0.015	0.015
	(0.045)	(0.013)	(0.013)	(0.045)	(0.015)	(0.015)
		3			4	
Teacher Certification	0.031	0.028	0.030	0.000	0.007	0.007
	(0.036)	(0.012)	(0.030)	(0.015)	(0.028)	(0.028)
		5			3	
Teacher Incentives	0.052	0.002	0.022	-0.000	-0.006	-0.006
	(0.033)	(0.011)	(0.022)	(0.021)	(0.012)	(0.012)
		7			4	
General PD	0.173	0.019	0.019	0.153	0.022	0.022
	(0.075)	(0.024)	(0.024)	(0.060)	(0.023)	(0.023)
		7			9	
Managed PD	0.059	0.052	0.052	0.493	0.217	0.403
	(0.009)	(0.016)	(0.016)	(0.187)	(0.029)	(0.120)

		2			8	
Data-Driven	0.107	0.043	0.057	0.071	0.009	0.030
	(0.041)	(0.014)	(0.024)	(0.040)	(0.011)	(0.024)
		4			4	
Extended Time	-0.033	0.019	-0.019	0.155	0.012	0.032
	(0.089)	(0.026)	(0.068)	(0.136)	(0.029)	(0.048)
		4			5	
School Choice/Vouchers	0.076	0.024	0.024	0.070	-0.010	0.023
	(0.035)	(0.018)	(0.018)	(0.040)	(0.012)	(0.025)
		6			7	
Charters	0.121	0.088	0.110	0.072	0.038	0.048
	(0.039)	(0.011)	(0.030)	(0.026)	(0.010)	(0.018)
		9			9	
No Excuse Charters	0.170	0.124	0.153	0.104	0.055	0.077
	(0.048)	(0.022)	(0.042)	(0.040)	(0.018)	(0.031)
		5			5	

Notes: This table reports average effects for categories of papers discussed in the main text. Columns (1)-(3) report reults for math estimates and columns (4)-(6) report results for reading estimates. Columns (1) and (4) report the unweighted average for the studies in a given category. Columns (2) and (5) report estimates from a fixed-effects meta-analysis. Columns (3) and (6) report estimates from a random-effects meta-analysis using the DerSimonian-Laird model (see DerSimonian and Laird 1986). Panel A reports results for early childhood experiments. Panel B reports results for home experiments. Panel C reports results for school experiments. The first row of each panel reports the results for all studies included in the given panel. The sample includes all studies found that meet our inclusion restrictions and have annual impact estimates for the given subject. See the main text and Online Appendix A for details on our search procedure, inclusion restrictions, and the categories of papers. Standard errors are reported in parentheses. The number of observations is reported below the standard error.

Table 3: Variables Across Life Stages

Variable	Dataset	Description
(1)	(2)	(3)
Panel A: Circumstances At Birth		
Gender	CNLSY, NLSY79	A binary variable indicating if a respondent is male or female.
Race	CNLSY, NLSY79	A mutually exclusive and exhaustive set of binary variables indicating whether a respondent is black, Hispanic, white, or other.
Maternal Educational Attainment	CNLSY, NLSY79	A mutually exclusive and exhaustive set of binary variables indicating whether a respondent's mother had not completed high school, graduated from high school, attended some college, or obtained a Bachelor's degree or higher at the time of the respondent's birth.
Maternal Age (at Birth)	CNLSY, NLSY79	The age of a respondent's mother at the time of the respondent's birth.
Maternal Age (at First Birth)	CNLSY, NLSY79	The age of a respondent's mother at the time of the mother's first birth.
Marital Status of Parents	CNLSY	A binary variable indicating if a respondent's mother was married at the time of the respondent's birth.
Family Income	CNLSY	The income of the respondent's family as a fraction of the federal poverty level for that family at the time of the respondent's birth.
Low Birth Weight	CNLSY	A binary variable indicating if a respondent was 5.5 pounds or less at birth.

Mother's AFQT Score	CNLSY	The percentile score of a respondent's mother on an unofficial version of the Armed Forces Qualification Test (AFQT). The scores were normalized in three-month age groups and calculated from the Mathematical Knowledge, Arithmetic Reasoning, Word Knowledge, and Paragraph Comprehension tests from the Armed Services Vocational Aptitude Battery (ASVAB).
Cognitive Stimulation Score	CNLSY	Score on the Home Observation Measurement of the Environment (HOME) Inventory Cognitive Stimulation subscale. Scores are taken from the first reported administration of the HOME Inventory for each respondent (ages 0-6) and standardized by age at testing.
Emotional Support Score	CNLSY	Score on the Home Observation Measurement of the Environment (HOME) Inventory Emotional Support subscale. Scores are taken from the first reported administration of the HOME Inventory for each respondent (ages 0-6) and standardized by age at testing.
PPVT Score	CNLSY	A respondent's score on the Peabody Picture Vocabulary Test (PPVT). Scores are taken from the first reported administration of the PPVT for each respondent (ages 3-4) and standardized by age at testing.
Panel B: Early Childhood (≈ Age 5)		
Math Achievement	CNLSY	A respondent's score on the math subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 4 and 8 and standardized by age at testing.

Reading Achievement	CNLSY	A respondent's score on the reading recognition subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 4 and 8 and standardized by age at testing.
Antisocial Behavior	CNLSY	A respondent's score on the antisocial behavior subscale from the Behavior Problems Index (BPI). Scores are taken from tests administered between the ages of 4 and 8 and standardized by age at testing.
Hyperactivity	CNLSY	A respondent's score on the hyperactivity subscale from the Behavior Problems Index (BPI). Scores are taken from tests administered between the ages of 4 and 8 and standardized by age at testing.
Panel C: Middle Childhood (≈ Age 11)		
Math Achievement	CNLSY	A respondent's score on the math subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 9 and 11 and standardized by age at testing.
Reading Achievement	CNLSY	A respondent's score on the reading recognition subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 9 and 11 and standardized by age at testing.
Antisocial Behavior	CNLSY	A respondent's score on the antisocial behavior subscale from the Behavior Problems Index (BPI). Scores are taken from tests administered between the ages of 9 and 11 and standardized by age at testing.

Hyperactivity	CNLSY	A respondent's score on the hyperactivity subscale from the Behavior Problems Index (BPI). Scores are taken from tests administered between the ages of 9 and 11 and standardized by age at testing.
Panel D: Adolescence (≈ Age 13 - 19)		
High School Grad Status	CNLSY, NLSY79	A binary variable indicating if a respondent graduated high school by age 19. Note that obtaining a GED does not count as graduating in this analysis.
GPA	CNLSY, NLSY79	A respondent's average GPA in their last year of high school. CNLSY: This variable is reported by the respondent. NLSY79: Grades are gathered from official transcripts for all classes a respondent took. We then calculate the average grade for the last year in which more than two graded classes were reported.
Criminal Conviction	CNLSY, NLSY79	A binary variable indicating if a respondent was ever convicted or ever on probation before the age of 19.
Teen Parent	CNLSY, NLSY79	A binary variable indicating if a respondent reported having a child before the age of 19.
Lives Independently from Parents	CNLSY, NLSY79	A binary variable indicating if a respondent reported living independently from their parents by the age of 19.
Math Achievement	CNLSY, NLSY79	CNLSY: A respondent's score on the math subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 12 and 16 and standardized by age at testing. NLSY79: A respondent's raw score on the Arithmetic Reasoning test from the Armed Services Vocational Aptitude Battery (ASVAB). Scores are taken from tests administered between the ages of 15 and 23 and standardized by age at testing.

Reading Achievement	CNLSY, NLSY79	A respondent's score on the reading recognition subtest of the Peabody Individual Achievement Test (PIAT). Scores are taken from tests administered between the ages of 12 and 16 and standardized by age at testing. NLSY79: A respondent's raw score on the Word Knowledge test from the Armed Services Vocational Aptitude Battery (ASVAB). Scores are taken from tests administered between the ages of 15 and 23 and standardized by age at testing.
Family Income	CNLSY, NLSY79	CNLSY: The income of the respondent's family reported between the ages of 12 and 15. NLSY79: The income of the respondent's family reported between the ages of 13 and 22.
Marijuana Use	CNLSY, NLSY79	CNLSY: A binary variable indicating if a respondent reported ever using marijuana by age 19. NLSY79: A binary variable indicating if a respondent reported using marijuana in the past year. Responses are taken from surveys administered between the ages of 16 and 23.
Other Drug Use	CNLSY, NLSY79	CNLSY: A binary variable indicating if a respondent reported yes to "Have you ever used any drugs other than marijuana or amphetamines, such as cocaine, "crack" ("rock") cocaine, hallucinogens, downers, sniffing glue, or something else?" NLSY79: A binary variable indicating if a respondent reported using a drug other than marijuana in the past year. Responses are taken from surveys administered between the ages of 15 and 23
Early Sex	CNLSY, NLSY79	A binary variable indicating if a respondent reported having sex before the age of 15.
Suspension	CNLSY, NLSY79	CNLSY: A binary variable indicating if a respondent ever reported being suspended or expelled from school. NLSY79: A binary variable indicating if a respondent ever reported being suspended from school.

Fighting	CNLSY, NLSY79	CNLSY: A binary variable indicating if a respondent reported getting in a fight at school in the past year. Responses are taken from surveys administered between the ages of 18 and 20. NLSY79: A binary variable indicating if a respondent reported getting in a fight at school or work in the past year. Responses are taken from surveys administered between the ages of 16 and 23.
Hitting	CNLSY, NLSY79	A binary variable indicating if a respondent reported hitting or seriously threatening to hit someone in the past year. CNLSY: Responses are taken from surveys administered between the ages of 18 and 20. NLSY79: Responses are taken from surveys administered between the ages of 16 and 23.
Damaging Property	CNLSY, NLSY79	A binary variable indicating if a respondent reported intentionally damaging property that did not belong to them in the past year. CNLSY: Responses are taken from surveys administered between the ages of 18 and 20. NLSY79: Responses are taken from surveys administered between the ages of 16 and 23.
Self-Esteem Index	CNLSY, NLSY79	A respondent's score on the Rosenberg Self-Esteem Scale. Raw scores are calculated from responses to the 10 Rosenberg Self-Esteem questions on tests administered between the ages of 16 and 19 and then standardized by age at testing.
Religious Service Attendance	CNLSY, NLSY79	A categorical variable indicating the frequency a respondent attends religious services. 0 = Not at all, 1 = Several times a year or less, 2 = About once a month, 3 = Two or three times a month, 4 = About once a week, 5 = More than once a week. CNLSY: Responses are taken from surveys administered between the ages of 19 and 20. NLSY79: Responses are taken from surveys administered between the ages of 17 and 22.

Gender Role Attitudes	CNLSY, NLSY79	The average score across five questions on how respondents view women. CNLSY: Responses are taken from surveys administered between the ages of 16 and 20. NLSY79: Responses are taken from surveys administered between the ages of 17 and 22.
School Clubs	CNLSY, NLSY79	A binary variable indicating if a respondent reported belonging to any clubs, teams, or activities in high school. CNLSY: Responses are taken from surveys administered between the ages of 15 and 19. NLSY79: Responses are taken from surveys administered between the ages of 19 and 27.
Panel E: Transition to Adulthood (≈ Age 29)		
Family Income	NLSY79	The income of the respondent's family reported between the ages of 27 and 31.
College Completion	NLSY79	A binary variable indicating if a respondent received a received a Bachelor's degree or higher by age 29.
Lives Independently from Parents	NLSY79	A binary variable indicating if a respondent reported living independently from their parents. Responses are taken from surveys administered between the ages of 26 and 31.
Panel F: Adulthood (≈ Age 40)		
Family Income	NLSY79	The income of the respondent's family reported between the ages of 39 and 44.

Notes: This table presents the variables that are included in the each life-stage of our adaptation of the Social Genome Model. Column (2) reports which datasets a given variable comes from. Variables that are reported as coming from both the CNLSY and the NLSY79 are used as linking variables in the simulation. See Winship and Owen (2013) and Online Appendix B for more information.

Table 4: Life-Cycle Model

	Math		Reading	
	Average Impact Top Three	Percent Change Income at 40	Average Impact Top Three	Percent Change Income at 40
Panel A: Early Childhood (≈ Age 5)	(1)	(2)	(3)	(4)
Early Childhood	0.413σ	2.27%	0.973σ	5.58%
Panel B: Middle Childhood (≈ Age 11)				
Home	0.000σ	0.00%	0.138σ	1.53%
School	0.521σ	3.66%	1.123σ	13.37%
Panel C: Adolescence (≈ Age 13 - 19)				
Home	0.000σ	0.00%	0.000σ	0.00%
School	0.258σ	2.10%	0.215σ	2.84%
Panel D: Cumulative				
Cumulative		8.28%		25.06%
Baseline Average		$60,752		$60,752

Notes: This table reports results from a life-cycle simulation using data from the National Longitudinal Surveys of Youth that follows methods described in Winship and Owen (2013). Panel A reports results for the early childhood life stage. Panel B reports results for the middle childhood life stage. Panel C reports results for the adolescence life stage. Columns (1) and (3) report the average of the three largest statistically significant effect sizes from interventions within the given life stage, category, and subject. If there were less than three significant effect sizes, we either report the average of the one or two significant impacts or report an impact of zero if there were no significant effect sizes. The sample includes all studies found that meet our inclusion restrictions and have impact estimates for the given subject. Note that we use cummulative impacts for each intervention instead of annual impacts. Columns (2) and (4) report the simulated impact that the given increase in test scores (in columns (1) and (3), respectively) at the given life stage would have on an individual's income at age 40. Panel D reports the simulated impact on age-40 income of increasing the scores of an individual in each life stage by all amounts specified in Panels A, B, and C for a given subject. The average age-40 income in the baseline sample is reported at the bottom of the table. See the main text and Online Appendix A for details on our search procedure, inclusion restrictions, and the categories of papers. See the main text and Online Appendix B for details on the life-cycle simulation.

Appendix Table 1 - Early Childhood

Study	Study Design	Results
An Evaluation of Curriculum, Setting, and Mentoring on the Performance of Children Enrolled in Pre-Kindergarten (Assel et al., 2006). N schools = 32, N classrooms = 76, N students = 603, Ages = 4 - 5, Location = Houston, TX. **Treatment Groups** = Two treatment conditions: condition one administered the *Let's Begin with the Letter People* curriculum; condition two administered the *Doors to Discovery* curriculum. The control group continued with the normal curriculum.	**Treatment Defined** = Two language and literacy curricula - *Let's Begin with the Letter People* and *Doors to Discovery*. Additionally, some of the treatment classroom teachers received monitoring that aided in the implementation of the curricula. **Randomization** = Twenty-six Title 1 pre-kindergarten classrooms, 19 universal pre-kindergarten classrooms and 31 Head Start Center classrooms were identified in Houston, TX across 32 schools sites. School sites were randomly assigned to one of the two curricula or control. The classrooms in sites that were randomly assigned to one of the treatment curricula were further randomized into receiving monitoring or no monitoring.	**Test Score** = Pre-school Language Scale-IV edition: Auditory Comprehension subtest; Expressive Vocabulary Test. **Regression Specification** = Effect sizes were calculated using the average posttest scores. We report the average impact across site types and outcome measures. **Results** = The *Let's Begin with the Letter People* curriculum with mentoring treatment had a -0.055σ (0.607) impact on reading test scores. The *Let's Begin with the Letter People* curriculum with no mentoring treatment had a -0.059σ (0.674) impact on reading test scores. The *Doors to Discovery* curriculum with mentoring treatment had a 0.045σ (0.605) impact on reading test scores. The *Doors to Discovery* curriculum with no mentoring treatment had a 0.184σ (0.597) impact on reading test scores.
Beyond the Pages of a Book: Interactive Reading and Language Development in Preschool Classrooms (Wasik and Bond, 2001). N teachers = 4, N students = 127, Age = 4, Location = Baltimore, MD. **Treatment Groups** = Treatment classrooms incorporated book-reading into their classroom curriculum. Control classrooms continued with their regular curriculum. Ninety-five percent of the sample is eligible for free or reduced lunch and 94 percent of the sample is African American.	**Treatment Defined** = Teachers in the treatment condition were trained in interactive reading techniques designed to teach new vocabulary and prompt a classroom discussion of the material. Books, vocabulary lists, and reading-related activities like arts and crafts were provided. Teachers read approximately two books per week. **Randomization** = Four teachers agreed to the study; half were randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Effect sizes were calculated using the average growth between post and pretest scores. **Results** = Treatment had a 0.499σ (1.015) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Children At-Risk for Poor School Readiness: The Effect of an Early Intervention Home Visiting Program on Children and Parents (Necoechea, 2007). N families = 52, Ages = 3 - 4. **Treatment Groups** = The treatment group participated in the Home Instruction for Parents of Preschool Youngsters (HIPPY) program. The control group received no such intervention. Sample composed entirely of low-income families.	**Treatment Defined** = Treatment families received a 15-week reading curriculum to implement at home. This curriculum was augmented by approximately seven 30 to 60 minute home visits and eight 2 to 3 hour group meetings, the goal of which was to train treatment mothers in effective curriculum implementation techniques. **Randomization** = Families were stratified by child's age and preschool enrollment, then randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test; Expressive One-Word Picture Vocabulary Test-Revised. **Regression Specification** = Effect sizes were calculated using the means of posttest scores adjusted for pretest scores. We report the average effect size across all outcome measures. **Results** = Treatment had a 0.159σ (0.281) impact on reading test scores.
CSRP's Impact on Low-Income Preschoolers' Preacademic Skills: Self-Regulation as a Mediating Mechanism (Raver et al., 2011). N recruitment sites = 18, N classrooms = 35, N students = 543, Ages = 3 - 4. **Treatment Groups** = Treatment sites implemented the Chicago School Readines Project (CSRP) for the entire academic year. Control sites received no such intervention. Sample composed entirely of Head Start classrooms.	**Treatment Defined** = The CSRP intervention was a professional development program designed to promote self-regulation skills among low-income preschoolers. Treatment teachers learned how to curb anti-social and dominant behaviors while promoting pro-social behaviors. **Randomization** = Recruitment sites were paired on the basis of similar demographic characteristics and one site from each pairing was randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Hierarchical linear model (student, classroom, site) controlling for gender, race/ethnicity, primary language, family size, whether the child came from a single-parent household, mother's education, income-to-needs ratio, hours worked by the mother in the previous week, teacher's education, teacher's age, teacher's psychological status, availability of a full-time family worker at the Head Start site, size of the Head Start program, proportion of teachers with a bachelor's degree, proportion of teaching assistants with at least some college education, proportion of families with at least one parent employed, and the proportion of families receiving Temporary Assistance for Needy Families. **Results** = Treatment had a 0.34σ (0.14) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Early Intervention in Low-Birth-Weight Premature Infants: Results Through Age 5 Years From the Infant Health and Development Program (Brooks-Gunn et al., 1994). N infants = 985, N years = 3. **Treatment Groups** = The treatment group received home visits and schooling for three years. The control group received no such intervention. Sample composed entirely of premature infants (born at or before 37 weeks gestational age) weighing under 2,500 grams at birth.	**Treatment Defined** = Treatment parents received home visits to provide information on child health and development, as well as social support and management strategies for self-identified problems. Parents received home visits an average of three times per month during the first year, and then an average of 1.5 times per month in the two years to follow. Beginning at age 1, treatment children were expected to attend four hours of school per day, which reinforced the material introduced in the home visits. **Randomization** = Children were stratified by birth weight and randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test-Revised. **Regression Specification** = Effect sizes were calculated using posttest means. We report the average annual impact. **Results** = Treatment had a 0.142σ (0.042) impact on reading test scores.
Educational Effects of the *Tools of the Mind* Curriculum: A Randomized Trial (Barnett et al., 2008). N teachers = 18, N students = 274, Age = 3 - 4. **Treatment Groups** = Treatment classroom utilized the *Tools of the Mind* curriculum. The control group continued with their normal curricula.	**Treatment Defined** = The *Tools of the Mind* curriculum focuses on the development of broad foundational skills in reading and mathematics, including self-regulation of social and cognitive behaviors, purposeful recollection, symbolic representation, phonemic awareness, knowledge of letters, familiarity with print, counting, one-to-one correspondence, pattern recognition, and numerical recognition. Teachers in the treatment condition received four days of training prior to the start of the school year. **Randomization** = Teachers were stratified into four groups: teachers with a preschool-grade three license; teachers with a K-8 license; teachers with a N-8 license; and teachers who transferred from another school within the district. Teachers within these groups were then randomly assigned to treatment.	**Test Score** = Woodcock-Johnson: Applied Math Problems and Letter Word Identification subtests; Peabody Picture Vocabulary Test; Expressive One-Word Picture Vocabulary Test. **Regression Specification** = Two-level hierarchical linear model (student, classroom) controlling for pretest scores, primary language, and age. The average (weighted by number of observations) effect across all outcome measures is reported. **Results** = Treatment had a 0.105σ (0.125) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Effective Early Literacy Skill Development for Young Spanish-Speaking English Language Learners: An Experimental Study of Two Methods (Farver et al., 2009). N students = 94, Ages = 3 - 5, Location = Los Angeles, CA. **Treatment Groups** = Students in the first treatment group received the Literacy Express Preschool Curriculum in English-only (English treatment). Students in the second treatment group received the Literacy Express Curriculum initially in Spanish, but transitioned to English over the course of the intervention (Transitional treatment). Control students received the High/Scope Curriculum.	**Treatment Defined** = Both treatment groups utilized the Literacy Express Preschool Curriculum. This curriculum focuses on oral language, emergent literacy, basic math and science, and socio-emotional development. It is structured around ten thematic units that are sequenced in order of complexity and the literacy demands placed upon children. The curriculum lasted for approximately 21 weeks. Students in the English treatment were taught in English for the entire 21 weeks. Students in the Transitional treatment were taught in Spanish for the first nine weeks, transitioned to English over the next four weeks, and then taught in English for the remainder of the time. **Randomization** = Balancing for gender, students were randomly assigned to one of the three groups.	**Test Score** = Test of Preschool Early Literacy: Definitional Vocabulary, Phonological Awareness, and Print Knowledge subtests. **Regression Specification** = Effect sizes were calculated using the average growth between pre and posttest scores. We report the average effect size across subtests. **Results** = The English treatment had a 0.239σ (0.084) impact on reading test scores. The Transitional treatment had a 0.326σ (0.085) impact on reading test scores.
Effects of an Early Literacy Professional Development Intervention on Head Start Teachers and Children (Powell et al., 2010). N teachers = 88, N children = 759, Ages = 4 - 5. **Treatment Groups** = The treatment group implemented a professional development intervention, and the control group was placed on a wait list to receive the same professional development the following semester.	**Treatment Defined** = Treatment teachers received a 1-semester professional development intervention designed specifically for Head Start teachers. Some teachers received on-site coaching and others received remote coaching. The goal of the professional development was to improve teachers' use of evidence-based literacy instruction (data-driven instruction). The intervention comprised of a 2-day workshop followed by expert coaching. The intervention used two different cohorts of teachers and students across two years. **Randomization** = Random assignment occurred at the teacher level and was stratified by whether or not the teacher's classroom was in an urban or not urban area. First, teachers were randomly assigned to an intervention semester (fall or spring) and a participation year (first or second). Next, teachers were randomly assigned to on-site or remote coaching condition.	**Test Score** = Peabody Picture Vocabulary Test III: Receptive Language; Woodcock-Johnson III: Letter Word Identification subtest; Concepts About Print; Test of Preschool Early Literacy: Blending subtest. **Regression Specification** = Hierarchical linear model analysis (student, classroom, Head Start center) controlling for child race-ethnicity, child gender, and year of participation. **Results** = Treatment had a 0.16σ (0.09) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). N preschools = 208, N classrooms = 315, N children = 2,911, Ages = 4 - 5. **Treatment Groups** = Treatment classrooms implemented one of 14 possible curricula. The control classrooms continued their usual curricula. Eighty-eight percent of the sample came from low-income families.	**Treatment Defined** = Treatment entailed implementation of one of the following curricula: *Bright Beginnings* (BB), *Creative Curriculum* (CC), *Creative Curriculum with Ladders to Literacy* (CCwL), *Curiosity Corner* (CCorn), *DLM Early Childhood Express supplemented with Open Court Reading Pre-K* (DLM), *Doors to Discovery* (DD), *Early Literacy and Learning Model* (ELLM), *Language-Focused Curriculum* (LFC), *Let's Begin with the Letter People* (LB), *Literacy Express* (LE), *Pre-K Mathematics supplemented with DLM Early Childhood Express Software* (Pre-K Math), *Project Approach* (PA), *Project Construct* (PC), or *Ready, Set, Leap!* (RSL). **Randomization** = Sample teachers were stratified first by recruitment-site and then either by classroom or preschool (different recruitment sites implemented different stratification criteria) and randomly assigned for treatment. Children were randomly assigned to classes.	**Test Score** = Test of Early Reading Ability; Woodcock-Johnson: Letter Word Identification, Spelling, and Applied Problems subtests; Peabody Picture Vocabulary Test; Test of Language and Development: Grammatic Understanding subtest; Child Math assessment: Composite score; Preschool Comprehension Test of Phonological and Print Processing: Elision subtest. **Regression Specification** = Three-level hierarchical linear model (student, classroom, teacher), controlling for age, gender, race/ethnicity, maternal education, disability status indicator, curriculum, and recruitment-site fixed effects. We reported the average effect across outcome measures. **Results** = The impacts for each treatment were as follows: BB math = 0.182σ (0.159) and BB read = 0.182σ (0.147). CC math = 0.102σ (0.163) and CC read = 0.028σ (0.164). CCwL math = 0.014σ (0.258) and CCwL read = -0.163σ (0.267). CCorn math = 0.041σ (0.183) and CCorn read = 0.021σ (0.170). DLM math = 0.007σ (0.138) and DLM read = 0.133σ (0.158). DD math = 0.069σ (0.149) and DD read = 0.130σ (0.208). ELLM math = 0.044σ (0.181) and ELLM read = 0.107σ (0.176). LFC math = 0.118σ (0.141) and LFC read = 0.129σ (0.159). LB math = 0.025σ (0.148) and LB read = 0.022σ (0.207). LE math = 0.274σ (0.136) and LE read = 0.458σ (0.154). Pre-K Math math = 0.309σ (0.131) and Pre-K Math read = 0.121σ (0.166). PA math = 0.122σ (0.214) and PA read = 0.154σ (0.275). PC math = -0.0168σ (0.133) and PC read = -0.050σ (0.166). RSL math = 0.087σ (0.119) and RSL read = 0.049σ.

Appendix Table 1 (continued)

Study	Study Design	Results
Efficacy of a Direct Instruction Approach to Promote Early Learning (Salaway, 2008). N students = 61, Age = 3 - 5. **Treatment Groups** = Students assigned to treatment classrooms used the *Language for Learning* curriculum. The control group continued with its normal curriculum. The sample was drawn primarily from low-income families.	**Treatment Defined** = The *Language for Learning* curriculum is a form of direct instruction that uses small group and individualized instruction to develop literary skills. Teachers give numerous, fast-paced presentations with frequent opportunities for child response. Instruction was implemented three days per week. **Randomization** = Children were randomly assigned to treatment or control classrooms.	**Test Score** = Kaufman Survey of Early Academic Language Skills: vocabulary, and numbers, letters, and words subtests; Dynamic Indicators of Basic Early Literacy Skills: the initial sounds fluency and letter-naming fluency subtests. **Regression Specification** = Effect sizes for each subtest were calculated using the average growth between post and pretest scores. Effect sizes were averaged by subject to estimate a total math and reading impact. **Results** = Treatment had a 0.468σ (0.262) impact on math test scores and a 0.448σ (0.262) impact on reading test scores.
Evaluation of Child Care Subsidies: Findings from Project Upgrade in Miami (Layzer et al., 2007). N child care centers = 180, N students = 1,523, Ages = 4, Location = Miami, FL. **Treatment Groups** = Three treatment groups: classrooms in the first group utilized the *Ready, Set, Leap!* (*RSL*) curriculum; classrooms in the second group used the *Building Early Language and Literacy* (*BELL*) curriculum; classrooms in the third group implemented the *Breakthrough to Literacy* (*BTL*) curriculum. The control group continued with their normal curricula.	**Treatment defined** = The *RSL* curriculum uses interactive software to develop oral language development, phonological knowledge, and print knowledge. The *BELL* curriculum entails two daily 15 - 20 minute lessons designed to promote language proficiency, phonological awareness, shared reading skills, and print awareness. The *BTL* curriculum builds phonological knowledge through a series of exercises and examinations for one book per week. **Randomization** = Eligible child care centers were randomly assigned to one of the four groups. To be eligible for the study, a child care center had to have a full class of four-year-olds predominantly from families receiving subsidies to pay for child care.	**Test Score** = Test of Preschool Early Literacy: definitional vocabulary, phonological awareness, and print knowledge subtests. **Regression Specification** = Three-level hierarchical linear model (student, classroom, randomization block), controlling for age, gender, language spoken at home, classroom-mean pretest score, and dominant language of teacher. Effects are reported for an index of the three subtests. **Results** = The RSL treatment had a 0.507σ (0.118) impact on reading test scores. The BELL treatment had a 0.061σ (0.127) impact on reading test scores. The BTL treatment had a 0.544σ (0.119) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Evaluation of Curricular Approaches to Enhance Preschool Early Literacy Skills (Fischel et al., 2007). N preschools = 6, N classrooms = 35, N students = 507, Age = 4, Location = Southeastern NY. **Treatment Groups** = All classrooms used the *High/Scope Education Approach* curriculum. Treatment classrooms implemented either the *Let's Begin with the Letter People* curriculum or the *Waterford Early Reading Program* curriculum. Sample drawn entirely from Head Start programs.	**Treatment Defined** = The *Let's Begin with the Letter People* curriculum utilized play-centered instruction to motivate students. It introduced early literacy, math, art, music, science, and social skills via a series of games, songs, and stories. Instruction spanned a three-day period. The *Waterford Early Reading Program Level 1* used computer software to provide individualized instruction and feedback in letter knowledge, print concepts, vocabulary, and story structure. Instruction lasted for 15 minutes daily. **Randomization** = Classrooms randomly assigned to one of three groups.	**Test Score** = Woodcock-Johnson Revised Tests of Achievement: Letter Word Identification and Dictation subtests; the Peabody Picture Vocabulary Test III. **Regression Specification** = Effect sizes for each outcome were calculated using the average growth between pre and posttest scores. Effect sizes were averaged to estimate a total impact for each treatment. **Results** = The *Let's Begin with the Letter People* curriculum had a 0.252σ (0.420) impact on reading test scores. The *Waterford Early Reading Program* curriculum had a 0.079σ (0.418) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Head Start Impact Study: Final Report (Puma et al., 2010). N children = 4,667, Ages = 3 - 4. **Treatment Groups** = Treatment children were offered enrollment for one to two years of the Head Start program. Control children applied to the Head Start program, but were not offered enrollment.	**Treatment Defined** = Head Start provides comprehensive services (preschool education, medical, dental, mental health care, and nutrition services) to low-income children in hopes of boosting their school readiness. Researchers investigate the impacts on two different cohorts, a 3-year old cohort and a 4-year old cohort. The 3-year old cohort was exposed to two years of the Head Start program and the 4-year-old cohort was exposed to just one. **Randomization** = Researchers first recruited 163 Head Start grantee/delegate agencies from across the nation. They then stratified eligible Head Start centers in these grantee/delegate agencies by program and student characteristics and then randomly selected three centers from each grantee/delegate agency (note that small centers were combined with nearby centers to create "center groups" that were randomized together as one unit). For the 2002-2003 application process, these centers continued with their typical procedure, reviewing applications and selecting students that they thought would be a good fit. However, the centers selected approximately 40% more students than they had spots for. From the pool of students that each center selected, the researchers then randomly selected students to be offered a spot at that Head Start center.	**Test Score** = Peabody Picture Vocabulary Test III; Woodcock-Johnson III: Letter Word Identification, and Applied Problems subtests. **Regression Specification** = Student outcome regressions control for student pretest scores, gender, age at time of assessment, race/ethnicity, primary language at baseline, number of weeks elapsed between 9/1/2002 and fall testing, primary language spoke at home, primary care giver's age, indicator for if both biological parents live with child, indicator for if biological mother is a recent immigrant, mother's highest level of educational attainment, mother's marital status, and an indicator for if mother gave birth to child as a teenager. **Results** = Winning a lottery to attend Head Start had a 0.135σ (0.071) impact on math test scores and a 0.188σ (0.064) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Longitudinal Results of the Ypsilanti Perry Preschool Project: Final Report (Weikart et al., 1970). N children = 123, Location = Ypsilanti, MI. **Treatment Groups** = Treatment students were assigned to an early childhood program lasting from age 3 to age 5 and control students were not.	**Treatment Defined** = The Perry Preschool Program consisted of children attending 2.5 hours of preschool on weekdays during the school year and teachers making weekly home visits. The program practiced an active learning curriculum where students were encouraged to plan, carry out, and reflect on their activities. Participants were drawn from the community served by the Perry Elementary School in Ypsilanti, MI. Families were recruited through surveys, neighborhood referrals, and door-to-door searches. The study focused on disadvantaged children living in adverse situations. In addition, the study only included students in the IQ range 70-85 and students with mental illness were excluded. The intervention was conducted on five different cohorts in the mid-1960s. **Randomization** = The randomization for this study was as follows: 1) For later cohorts, if a child had an older sibling already participating in the study, they were assigned to the same experimental status as their sibling; 2) The remaining students were ranked by their IQ scores. Odd and even ranked students were then assigned to different groups; 3) Some students were manually swapped to balance gender and socioeconomic status between the two groups; 4) A coin was flipped to determine which group would be treated and which group would be control; 5) Some children initially assigned to treatment, who had employed mothers, were swapped with control, who had unemployed mothers. This was done because the researchers believed it would be hard for working mothers to participate in weekly home visits with teachers.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Effect sizes were calculated from posttest means. We report the average annual impact. **Results** = Assignment to the Perry Preschool Program had a 0.655σ (0.162) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
National Impact Evaluation of the Comprehensive Child Development Program: Final Report (St. Pierre et al., 1997). N recruitment sites = 21, N families = 4,410. **Treatment Groups** = Treatment group took part in Comprehensive Child Development Program (CCDP) for five years; the control group continued as usual. Sample drawn from families with income below the poverty line.	**Treatment Defined** = The CCDP provides physical, social, emotional, and intellectual support to impoverished families, for the purpose of promoting stable childhood development and economic self-sufficiency among families. Recruited families were either expecting a child or had a child under the age of one. The study analyzed one child per family, termed the "focus child." **Randomization** = Rural sites were asked to recruit 180 families; urban sites 360. In order for a family to be eligible, they had to (1) have income below the Federal Poverty guidelines, (2) include a pregnant woman or a child under the age of one, and (3) agree to participate in CCDP activities for five years. Further, each site was instructed to recruit a group of families that were representative (in terms of ethnicity and age of mother) of the low-income population that site served. Recruited families at each site were then randomly assigned to a treatment, a control, or a replacement group. The replacement group was used to replace families that dropped out of the program and were not included in the evaluation.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Effect sizes were calculated from posttest means. We report the average annual impact. **Results** = Treatment had a 0.002σ (0.018) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Parental Incentives and Early Childhood Achievement: A Field Experiment in Chicago Heights (Fryer et al., 2015). N families = 257. **Treatment Groups** = Two treatment conditions: condition one parents received cash rewards for participation in treatment programs; condition two received the same rewards but they were deposited into a trust fund which would be paid upon successful enrollment of their child in college. Control parents did not receive incentives.	**Treatment Defined** = Treatment parents had the opportunity to attend a "Parent Academy," in which they learned how to effectively involve themselves with their child's academic work. Parents also received homework assignments, which asked them to practice the skills learned in the sessions. All treatment programs offered monetary incentive, such that parents could receive up to $3,500 via successful completion of Parent Academy classes and assignments, as well as an additional $3,400 if their child performed well on interim evaluations as well as two large end-of-semester assessments. **Randomization** = Families were randomly assigned to a treatment group or control.	**Test Score** = Woodcock-Johnson III: Letter Word Identification, Applied Problems, Spelling, and Quantitative Concepts subtests; Peabody Picture Vocabulary Test. **Regression Specification** = OLS regressions controlling for children's pretest scores, race, gender, age, and mother's age. **Results** = The cash treatment had a 0.150σ (0.158) impact on math test scores and a 0.046σ (0.143) impact on reading test scores. The college treatment had a 0.224σ (0.166) impact on math test scores and a 0.119σ (0.169) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Poverty, Early Childhood Education, and Academic Competence: The Abecedarian Experiment (Ramey and Campbell, 1991). N students = 111, Ages = 0 - 8. **Treatment Groups** = Treatment children received a preschool intervention from infancy until they started school (approximately age 5) and an intervention during the first three years of primary school. Control students continued with the normal curriculum.	**Treatment Defined** = This study investigates the impact of the Abecedarian program on academic test scores. The sample consisted of healthy infants born to impoverished families in a small, southern town. Treatment children were enrolled in a preschool educational program that operated year round. As an infant, they were exposed to a curriculum that included cognitive and fine motor development, social and self-help skills, language, and gross motor skills. As the children grew older, they moved into a preschool program that placed special emphasis on language development and preliteracy skills. In addition, treatment students went through a 6-week summer transitional classroom experience the summer before kindergarten in order to best prepare them for the classroom experience. Treatment students continued to receive support for the first three years of school. This support came in the form of a home/school resource teacher (HST). The HST provided parents with activities designed for each child, served as a liaison between the school and parents, and helped families with non-school related problems that might affect the student's learning. **Randomization** = Eligible infants were matched based on High Risk scores (derived from factors such as maternal and paternal education levels, family income, and parents' marital status) and then one infant from each pair was randomly assigned to treatment and the other to control. Upon entry into kindergarten, children within each group were matched based on their 48-month IQ score and then the pairs were randomly split into the new treatment and control groups.	**Test Score** = The math and reading clusters from the Woodcock-Johnson Psycho-Educational Battery, Part 2: Tests of Academic Achievement; California Achievement Test: Math and Reading subtests. **Regression Specification** = Effect sizes were calculated from posttest means. We report the average annual impact across outcome measures. **Results** = Treatment had an annual impact of 0.082σ (0.110) impact on math test scores and 0.133σ (0.115) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Project Breakthrough: A Responsive Environment Field Experiment with Pre-School Children from Public Assistance Families (Cook County Department of Public Aid, 1969). N students = 184, Ages = 3 - 4. **Treatment Groups** = Treatment students received the Edison Responsive Environment (ERE) intervention. Control students continued with the normal curricula.	**Treatment Defined** = The ERE intervention gave students Talking Typewriters, which allowed students to select letters of the alphabet at will, and have the typewriter voice their selection. When the child demonstrated sufficient mastery of this machine, the typewriter would then ask children to select a specific letter. The typewriter was available everyday in class. **Randomization** = Students were paired by IQ and assigned randomly to either treatment or control. Note that the researchers had another parallel experiment where all students received intensive social work services and were also randomly assigned to the ERE intervention or a control group that only received the intensive social work services. However, these two groups of students were randomized at a different time than the two groups that received normal levels of social work services and therefore we cannot directly compare them. For this reason, we only focus on students that received normal levels of social work services.	**Test Score** = Peabody Picture-Vocabulary Test. **Regression Specification** = Effect sizes were calculated using the mean posttest scores. **Results** = Treatment had a 0.448σ (0.253) impact on reading test scores.
Promoting Academic and Social-Emotional School Readiness: The Head Start REDI Program (Bierman et al., 2008). N classrooms = 44, N children = 356, Age = 4 . **Treatment Groups** = The treatment group administered the Head Start REDI program, and the control group continued with the normal Head Start curriculum.	**Treatment Defined** = The intervention involved brief lessons, hands on extension activities and specific teaching strategies linked empirically with the promotion of both social-emotional competencies as well as language development and emergent literacy skills. Take-home materials were provided to parents to enhance skill development at home. The study included two cohorts of 4-year-old children that were recruited across two years. **Randomization** = Fourty-four Head Start classrooms were either randomized into an enriched intervention treatment condition or a usual practice control condition.	**Test Score** = Expressive One-Word Picture Vocabulary Test; Test of Language Development: Grammatical Understanding and Sentence Imitation subtests; Test of Preschool Early Literacy: Blending, Elision, and Print Knowledge subtests. **Regression Specification** = Two-level hierarchical linear model (child, classroom) controlling for gender, race, site, and cohort. **Results** = Treatment had a 0.16σ (0.10) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
Randomized Field Trial of an Early Literacy Curriculum and Institutional Support System (Cosgrove et al., 2006). N recruitment sites = 3, N classrooms = 38, N students = 466, Age = 4. **Treatment Groups** = Treatment group implemented the *Early Literacy and Learning Model* curriculum. Control group continued with normal curriculum. Sample drawn from low-performing elementary schools with at least one pre-K program.	**Treatment Defined** = The *Early Literacy and Learning Model* curriculum emphasized letter and sound recognition, as well as phonological and print awareness. Students engaged in both large- and small-group exercises, in which they typically experienced conversation, repetition, print exposure, and vocabulary exercises. Treatment teachers attended a five-day summer training session and a one-hour weekly coaching seminar. **Randomization** = Schools were stratified by recruitment site and randomly assigned to treatment.	**Test Score** = Test of Early Reading Ability. **Regression specification** = Two-level hierarchical linear model (child, classroom) controlling for pretest scores, age, gender, urbanicity, and whether the teacher had a bachelor's degree. **Results** = The *Early Literacy and Learning Model* curriculum had a 0.253σ (0.079) impact on reading test scores.
The Early Training Project for Disadvantaged Children: A Report After Five Years (Klaus and Gray, 1968). N students = 61, Ages = 4 - 5. **Treatment Groups** = Two treatment groups: one group of students attended a 10-week summer program plus weekly meetings whenever school was not in session for three years; the second group received the same intervention for two years. Control students received no such intervention. Sample drawn from "culturally deprived" African American families in segregated schools.	**Treatment Defined** = The intervention was designed to develop attitudes conducive to school success, including achievement motivation, persistence, delayed gratification, and interest in school-like activities. Treatment took place during a ten-week summer school program. Further, treatment students received weekly in-house visits to reinforce these lessons whenever school was not in session. **Randomization** = Students were randomly assigned to one of the three conditions.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Effect sizes were calculated using average growth between pretest and posttest scores. We report the average annual effect across the two treatment groups. **Results** = Treatment had a 0.250σ (0.139) impact on reading test scores.
The Effects of a Language and Literacy Intervention on Head Start Children and Teachers (Wasik et al., 2006). N preschools = 2, N teachers = 16, N students = 207. **Treatment Groups** = Treatment teachers received training in book reading and oral language strategies. Control teachers received no such training.	**Treatment Defined** = Treatment teachers were trained in the following classroom strategies: asking questions – designed to promote classroom discussion of the text, building vocabulary, and making connections – designed to introduce further applications of the target vocabulary. Each treatment teacher also received toys/props related to the text to incorporate into the lesson. **Randomization** = One preschool was randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test; Expressive One-Word Picture Vocabulary Test. **Regression Specification** = Effect sizes were calculated using the average growth between post and pretest scores. **Results** = Treatment had a 0.549σ (1.442) impact on reading test scores.

Appendix Table 1 (continued)

Study	Study Design	Results
The Effects of the Home Instruction Program for Preschool Youngsters (HIPPY) on Children's School Performance at the End of the Program and One Year Later (Baker et al., 1998). N families = 182, Age = 4. **Treatment Groups** = Treatment families implemented the HIPPY intervention. Control families received no such intervention.	**Treatment Defined** = Treatment mothers received a series of books to read to their children along with a set of guided workbooks. Books and workbook activities became successively harder as families progressed through the program. Treatment mothers implemented the HIPPY intervention daily. **Randomization** = Families were randomly assigned to treatment.	**Test Score** = Metropolitan Readiness Test: Math and Reading subtests. **Regression Specification** = Effect sizes were calculated using posttest scores adjusted for age, gender, family structure, and pretest scores, as well as the parent's race/ethnicity, education, and public-assistance status. We report the annual impact of the program. **Results** = Treatment had a 0.133σ (0.141) impact on math test scores and a 0.081σ (0.141) impact on reading test scores.
Using Television as a Teaching Tool: The Impacts of *Ready to Learn* Workshops on Parents, Educators, and the Children in their Care (Boller et al., 2004). N sites = 20, N parents/caretakers = 2,319. **Treatment Groups** = Treatment parents/caretakers attended a *Ready to Learn* workshop. Control parents/caretakers received no such intervention.	**Treatment defined** = The *Ready to Learn Television Service* supported the development of children's educational programming on the Public Broadcasting Service (PBS) and also provided public workshops to teach parents how to involve themselves with the educational content included in these programs. The goal of these programs was to extend those lessons introduced via the television into normal family life. **Randomization** = Families were randomly assigned to treatment.	**Test Score** = Woodcock and Muñoz-Sandoval test: Picture Vocabulary and Letter Word Identification subtests. **Regression specification** = OLS regression controlling for parental gender, race, education, attitudes toward television, English ability, whether the family lived in a rural area, and prior exposure to a *Ready to Learn* workshop, as well as the child's age and gender. **Results** = Treatment had a 0.023σ (0.062) impact on reading test scores.

Appendix Table 2 - Home Environment

Study	Study Design	Results
A Comparative Study of the Reading Achievement of Second Grade Pupils in Programs Characterized by a Contrasting Degree of Parent Participation (Ryan, 1964). N teachers = 10, N classrooms = 10, N students = 232, Grade = 2. **Treatment Groups** = Treatment classrooms incorporated specific parental involvement into their reading programs. The control group incorporated no such intervention. Sample drawn entirely from schools serving primarily middle-class families, as determined by the superintendent.	**Treatment Defined** = Treatment parents were asked to frequently read at home with their children. Upon completion of a book, students were asked to share the book with the rest of their class via a brief presentation, in which they named the title and author, and then read their favorite passage. Ten minutes of class time were set aside daily for these presentations. **Randomization** = Classrooms were randomly assigned to treatment. Eight students were dropped at random to balance the treatment and control groups on the basis of sample size, gender distribution, and pretest scores.	**Test Score** = Stanford Achievement Test: Paragraph Meaning and Word Meaning subtests. **Regression Specification** = Effect sizes were calculated using average growth between pre and posttest scores. We report the average effect size across outcome measures. **Results** = Treatment had a 0.212σ (0.634) impact on reading test scores.
Addressing Summer Reading Setback Among Economically Disadvantaged Elementary Students (Allington et al., 2010). N districts = 2, N schools = 17, N students = 1,713, N years = 3. **Treatment Groups** = Treatment students received books to read over the summer. Control students received no such intervention. Sample drawn from schools with at least 65 percent of the student body eligible for free or reduced price lunch.	**Treatment Defined** = Treatment students attended a book fair in the spring of each school year, where they selected 15 books, 12 of which they would receive to read over the summer. Available books fell into four categories: pop culture, popular book series, culturally relevant, and curriculum relevant. **Randomization** = A total of 1,082 students were randomly assigned to treatment.	**Test Score** = Florida Comprehensive Achievement Test. **Regression Specification** = Effect size was calculated using the average posttest scores. We report the average annual impact. **Results** = Treatment had a 0.046σ (0.033) impact on reading test scores.
An Investigation of the Effects of Daily, Thirty-Minute Home Practice Sessions Upon Reading Achievement With Second Year Elementary Pupils (Hirst, 1972). N schools = 2, N classrooms = 2, N students = 96, Grade = 2. **Treatment Groups** = Treatment students took part in at-home reading instruction with their parents. Control students received no such intervention.	**Treatment Defined** = Parents acted as tutors for 30-minute after school reading sessions at home, five days per week. Texts, lesson plans, and tutoring instructions were provided by the school. The intervention included approximately 80 sessions. **Randomization** = Students were stratified by gender and then randomly assigned to treatment.	**Test Score** = Gates-MacGinitie Reading Test: Vocabulary and Reading Comprehension subtests; Stanford Achievement Test: Word Study Skills subtest. **Regression Specification** = OLS regression controlling for a quadratic of pretest scores. We report the average effect size across outcome measures. **Results** = Treatment had a 0.113σ (0.120) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Assessing the Effectiveness of First Step to Success: Are Short-Term Results the First Step to Long-Term Behavioral Improvements? (Sumi et al., 2012). N recruitment sites = 5, N schools = 48, N teachers = 288, N students = 287, Grades = 1 - 3. **Treatment Groups** = Treatment students took part in the *First Step* intervention. Control students received no such intervention.	**Treatment Defined** = The *First Step* program has three core components: universal screening, classroom instruction, and parental education. Treatment parents and teachers received training from program coaches to learn how to teach students replacement behaviors and properly reward students when these behaviors were used appropriately. The intervention took place in-class and at home for approximately three months. **Randomization** = Schools were randomly assigned to treatment or control. Participating teachers in all schools then identified students that demonstrated an elevated risk for externalizing school behavior problems using Stages 1 and 2 of the Systematic Screening for Behavior Disorders. The three students from each classroom with the highest average scores across three behavioral indices were invited to participate in the study. Eighty-eight percent of invited students obtained consent from their parents and participated in the study.	**Test Score** = Woodcock-Johnson: Letter Word Identification subtest. **Regression Specification** = Two-level hierarchical linear model (student, classroom) controlling for pretest scores, age, grade, gender, race/ethnicity, free/reduced-lunch eligibility, special education status, self-reported teacher knowledge and skill, as well as scores on the Maladaptive Behavior Index. **Results** = Treatment had a -0.104σ (0.092) impact on reading test scores.
Collaboration Between Teachers and Parents in Assisting Children's Reading (Tizard et al.,1982). N schools = 6, N students = 1,867, Grades = K - 2, Location = London, UK, N years = 2. **Treatment Groups** = Two treatment conditions: students in condition one read aloud to their parents at home; students in condition two read aloud to their teachers in school. The control group did not participate.	**Treatment Defined** = Parents in condition one agreed to listen to their child as they read aloud and to complete a report card detailing what their child had read. Books were supplied to the student as needed – most children took home an average of two to four books per week. Condition two mirrored condition one, except teachers listened to children read aloud in small groups. **Randomization** = Schools were assigned at random to one of the two treatment conditions. Within each school, one classroom was selected at random to receive treatment, while the remainder served as a within-school control.	**Test Score** = National Foundation for Education Research Test A: Reading comprehension subtest. **Regression Specification** = Effect sizes were calculated using posttest means for each school. We report the average annual effect size across schools. **Results** = The home collaboration treatment had a 0.445σ (0.906) impact on reading test scores. The teacher help treatment had a -0.012σ (0.930) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Does Reading During the Summer Build Reading Skills? Evidence from a Randomized Experiment in 463 Classrooms (Guryan et al., 2014). N districts = 7, N schools = 59, N students = 5,319, Grades = 2 - 3, Location = NC. **Treatment Groups** = The treatment group was given reading comprehension lessons for the summer. The control group received no such intervention. Sample drawn entirely from North Carolina.	**Treatment Defined** = Treatment students were given six reading comprehension lessons in the spring that focused on reading activities that would foster engagement with books during the summer. Parents of treatment students were invited to an after-school family literacy event. Treatment students were mailed ten books, one per week, during the summer. Students were asked to mail a trifold, that included comprehension questions, after they read each book. Students in the control group received no books and participated in six mathematics lessons during the spring while treatment students participated in reading lessons. **Randomization** = Student-level randomization stratified by classroom. The teachers teaching the intervention-related treatment and control lessons were also randomly assigned to new classrooms for the teaching portion of the intervention.	**Test Score** = Iowa Test of Basic Skills: Reading Comprehension subtest. **Regression Specification** = OLS regressions controlling for pretest reading comprehension test score and classroom fixed effects. **Results** = Treatment had a 0.014σ (0.017) impact on reading test scores.
Effect of Early Literacy Intervention on Kindergarten Achievement (Phillips, 1990). N schools = 12, N classrooms = 18, N students = 325, Grade = K, Location = Canada. **Treatment Groups** = Three treatment conditions: students in condition one participated in the *Little Books* intervention at home; students in condition two participated in it in school only; students in condition three participated in the intervention both at home and in school. Control classrooms maintained their normal curricula.	**Treatment Defined** = Students participating in the *Little Books* intervention at home received a new book each week to read with their parents, who in turn received general guidelines of how to help their children read the book. Students participating in the intervention in school received a lesson plan to accompany each book. Those students participating in the intervention both at home and in school would read the same book in both locations, using the parental intervention to reinforce the lessons of the classroom intervention. Treatment took place in-class for 24 weeks. **Randomization** = Schools were grouped into blocks of four based on location (rural, rural collector, urban) and randomly assigned to treatment or control.	**Test Score** = Metropolitan Reading Readiness Test. **Regression Specification** = Effect sizes were calculated using the means of posttest scores. **Results** = The home treatment had a 0.000σ (0.816) impact on reading test scores. The school treatment had a -0.025σ (0.817) impact on reading test scores. The home and school treatment had a 0.337σ (0.822) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Effects of a Voluntary Summer Reading Intervention on Reading Achievement: Results From a Randomized Field Trial (Kim, 2006). N schools = 10, N students = 552, Grades = 3 - 5. **Treatment Groups** = Treatment students received access to eight free books over the summer. Control students were not granted such access.	**Treatment Defined** = Treatment students had access to eight free books over their summer vacation. Skill-appropriate texts were selected based on their semantic and syntactic difficulty. Book selection also took into account student reading preferences, which were assessed via a survey. **Randomization** = To construct the sample, schools were stratified by Title I eligibility and ranked by their percentage of black and Latino students. Researchers then selected the top four Title I schools and the top six non-Title I schools with the largest percentage of minority students. Students were then stratified by classroom and randomly assigned to treatment.	**Test Score** = Iowa Test of Basic Skills; Dynamic Indicators of Basic Early Literacy Skills: Oral Reading Fluency subtest. **Regression Specification** = OLS regression controlling for pretest scores and randomization block. We report the average effect size across outcome measures. **Results** = Treatment had a 0.012σ (0.040) impact on reading test scores.
Effects of Parent Involvement in Isolation or in Combination with Peer Tutoring on Student Self-Concept and Mathematics Achievement (Fantuzzo et al., 1995). N students = 72, Grades = 4 - 5, Location = Large urban city in northeastern United States. **Treatment Groups** = This study had two treatment groups. One group received a parental involvement intervention (PI) and the other group received both a parental involvement intervention and a reciprocal peer tutoring intervention in mathematics (RPT + PI). The control group received neither intervention.	**Treatment Defined** = Students in all three groups participated in two 45 minute math sessions per week for ten weeks. The control group worked on assignments individually during these sessions with teaching assistants available if necessary. The PI group also worked individually during sessions, but parents of this group would receive regular updates about the level of students' academic effort in the classroom and parent-initiated celebrations of students' achievement were planned. The RPT + PI group followed the same classroom routine and parent involvement intervention, but also received peer tutoring. Students were randomly paired with a tutor and rewards were given to each pairing when team goals were met. **Randomization** = Participants were stratified by pretest scores and then randomly assigned to one of the three groups.	**Test Score** = Stanford Diagnostic Mathematics Test III: Computation subtest. **Regression Specification** = Effect sizes were calculated using means adjusted for pretest scores. **Results** = The PI treatment had a 0.351σ (0.395) impact on math test scores. The RPT + PI treatment had a 0.744σ (0.406) impact on math test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Evaluation of the First 3 Years of the Fast Track Prevention Trial with Children at High Risk for Adolescent Conduct Problems (Bierman et al., 2002). N recruitment sites = 4, N schools = 54, N classrooms = 401, N students = 891, Grades = 1 - 3, N years = 3. **Treatment Groups** = Treatment group participated in the *Fast Track* intervention program. Control group did not implement such an intervention. Sample drawn from recruitment sites deemed high-risk due to crime and poverty statistics in the surrounding neighborhoods.	**Treatment Defined** = The *Fast Track* program attempts to address school and family risk factors relating to a child's behavior. The hypothesis guiding the program is that improvements in child competencies, parenting effectiveness, school context, and communications between the home and the school will increase gradually over time and lead to a reduction in antisocial behavior. The program content changes each year to keep pace with developmental needs of children and families. This study focuses on the impact of the first three years of the *Fast Track* program. **Randomization** = Schools were randomly assigned to treatment.	**Test Score** = Spache Diagnostic Reading Scale. **Regression Specification** = ANCOVA with gender, cohort, site, and baseline child and parent demographics as covariates. We report the average annual impact. **Results** = Treatment had a 0.02σ (0.03) impact on reading scores.
Experimental Evidence on the Effects of Home Computers on Academic Achievement among Schoolchildren (Fairlie and Robinson, 2013). N districts = 5, N schools = 15, N students = 1,123, Grades = 6 - 10, N years = 2. **Treatment Groups** = Treatment students received computers and control students did not.	**Treatment Defined** = Home computers are provided to treatment students with no strings attached. **Randomization** = Any student who reported not having a home computer at the beginning of the year was eligible for the study. Students were stratified by school and then randomly assigned to treatment or control.	**Test Score** = California Standardized Testing and Reporting program. **Regression Specification** = OLS regression controlling for sampling strata, school-year, and first quarter grades. **Results** = Treatment had a -0.06σ (0.05) impact on math test scores and a -0.05σ (0.05) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Fostering Development of Reading Skills Through Supplemental Instruction: Results for Hispanic and Non-Hispanic Students (Gunn et al., 2005). N schools = 13, N students = 299, Grades = K - 3, Location = OR. **Treatment Groups** = Students in the treatment group received supplemental reading instruction and a social behavior intervention; parents of treatment students received parenting training. The control group did not receive such instruction.	**Treatment Defined** = Reading instruction entailed 30 minutes of small-group or individual tutoring daily for two years in addition to normal class time. The instruction utilized the *Reading Mastery* and *Corrective Reading* curricula, both of which focus on developing fluent word-recognition. Social behavior interventions sought to reduce acting-out behaviors by teaching and reinforcing appropriate classroom behaviors. Parent instruction entailed group sessions, in which parents reviewed successful child interaction and communication strategies. **Randomization** = To be eligible for the study, students had to either perform below grade level on literacy assessments or exhibit aggressive social behaviors. Eligible students were grouped by community, grade, and ethnicity and then paired by reading ability as determined by the pretest. One student from each pairing was randomly assigned to treatment.	**Test Score** = Woodcock-Johnson Revised: Letter Word Identification, Word Attack, Passage Comprehension, and Reading Vocabulary subtests. **Regression Specification** = Effect sizes were calculated using the average posttest scores for each group. We report the annual impact across all subjects. **Results** = Treatment had a 0.170σ (0.133) impact on reading test scores.
Getting Parents Involved: A Field Experiment in Deprived Schools (Avvisati et al., 2014). N schools = 34, N classrooms = 183, N families = 970, Grade = 6, Location = Paris, France. **Treatment Groups** = Treatment parents attended meetings learning how to get involved with their child's education. Control parents were not invited to such meetings.	**Treatment Defined** = Treatment parents attended meetings to learn how to involve themselves with their children's education both at home and at school. Parents attended at least three initial meetings, the last of which took place after receipt of the end-of-term report card, and taught parents how to interpret and respond to their child's academic performance. After the third session, parents could attend additional meetings related to parenting strategies, use of the school-related internet, or sessions designed for non-French speakers. **Randomization** = Classrooms were stratified by school and randomly assigned to treatment.	**Test Score** = District standardized tests. **Regression Specification** = OLS regressions controlling for school fixed effects. **Results** = Treatment had a 0.020σ (0.060) impact on math test scores and a -0.035σ (0.064) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Head Start Children's Entry into Public School: A Report on the National Head Start/Public School Early Childhood Transition Demonstration Study (Ramey et al., 2000). N sites = 31, N schools = 413, N families = 7,515, Grades = K - 3, N years = 6. **Treatment Groups** = Treatment group received Transition Demonstration services. Control group received no such intervention. Sample drawn from families previously enrolled in the *Head Start* program.	**Treatment Defined** = The Transition Demonstration Project was designed as a comprehensive follow-up to the traditional *Head Start* program that focused on the environment in which children prepare for school. The program had three main goals: first, preparing schools to meet the needs of children at varying levels of development; second, preparing families to support the continued growth and academic development of their children; and third, preparing entire communities to invest in education for families and children. To meet these goals, local coordinators were granted the freedom to adopt measures they deemed appropriate within the context of their communities. The intervention was introduced after children had completed the *Head Start* program and were enrolled in Kindergarten. **Randomization** = Within each site, schools were placed into one of two blocks based on size and ethnic composition of their student body. One block from each site was randomly assigned to treatment.	**Test Score** = Woodcock-Johnson: Letter Word Identification, Passage Comprehension, Mathematics Computation, and Applied Problems subtests. **Regression Specification** = Effect sizes were calculated using the average posttest scores. **Results** = Treatment had a -0.015σ (0.131) impact on math scores and a -0.018σ (0.131) impact on reading scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Making Work Pay: Final Report on the Self-Sufficiency Project for Long-Term Welfare Recipients (Michalopoulos et al., 2002). N parents = 5729, Location = British Columbia and New Brunswick, Canada. **Treatment Groups** = Families assigned to treatment were given the opportunity to participate in a welfare program that increased their income. Control families were not offered enrollment into this program.	**Treatment Defined** = This study investigated the impact of the Self-Sufficiency Project (SSP) on child achievement. SSP offered a temporary earnings supplement for up to three years to individuals in British Columbia and New Brunswick, Canada. In order to participate, individuals had to be single parents who had been on income assistance for at least one year and left income assistance for full-time work. The supplement was given in addition to earnings from work. Participants continued to receive payouts as long as they stayed employed full-time (up to three-years). For full-time workers making minimum wage, the supplement would approximately double their income. **Randomization** = From the entire pool of eligible single-parents, the researchers randomly selected a sample to contact, interview, and invite to be part of the SSP study. Individuals from this sample that completed a survey and signed a consent form were then randomly assigned to treatment and control groups.	**Test score** = Peabody Picture Vocabulary Test-Revised. **Regression Specification** = Researchers report mean test scores for each experimental group. We report average annual impact. **Results** = The SSP had a 0.036σ (0.058) impact on reading test scores.
National Evaluation of Welfare-to-Work Strategies (Hamilton et al., 2001). N recruitment sites = 7, N families = 2,332, Ages = 3 - 5. **Treatment Groups** = Two treatment conditions: condition one implemented a Labor Force Attachment (LFA) intervention; condition two implemented a Human Capital Development intervention (HCD). The control group implemented no such intervention. Sample composed entirely of welfare recipients.	**Treatment Defined** = The LFA intervention emphasized rapid job placement, so that treatment subjects gained exposure to the job market and developed workplace habits and skills. The HCD intervention focused on developing education and basic skills prior to job placement, so that treatment subjects were more likely to excel at and keep their jobs. **Randomization** = In four recruitment sites, applicants were randomly assigned to one of the three conditions. In three of the recruitment sites, the program administrators picked their treatment of choice, and applicants were randomly assigned to either treatment or control.	**Test Score** = Woodcock-Johnson-Revised: Broad reading and math subtests. **Regression Specification** = Effect sizes were calculated using posttest means adjusted for baseline characteristics. We report annual impacts. **Results** = The LFA treatment had a 0.009σ (0.041) impact on math test scores and a 0.007σ (0.042) impact on reading test scores. The HCD treatment had a 0.007σ (0.042) impact on math test scores and a -0.001σ (0.043) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment (Sanbonmatsu et al., 2006). N children = 5,074, Ages = 6 - 20. Location = Boston, Baltimore, Chicago, Los Angeles, and New York. **Treatment Groups** = Treatment families received one of two types of housing vouchers, "experimental" or "Section 8". Control families did not receive a housing voucher.	**Treatment Defined** = Through a lottery for housing vouchers among families initially living in public housing, Moving To Opportunity (MTO) randomly assigned families into three groups. Families in an "experimental" group received housing vouchers eligible for use in low-poverty neighborhoods. Families in a "Section 8" group received traditional housing vouchers without neighborhood restrictions. Families in the control group did not receive a voucher, but were still eligible for public housing. **Randomization** = Random lottery.	**Test Score** = The Woodcock - Johnson Revised battery of tests. **Regression Specification** = OLS regression of test score on a treatment group assignment indicator and baseline covariates (child demographics, child health problems, child education, adult and household characteristics). **Results** = The effect of the experimental treatment was 0.006σ (0.014) on reading test scores and -0.002σ (0.013) on math test scores. The effect of the Section 8 group was 0.006σ (0.015) on reading test scores and -0.007σ (0.014) on math test scores.
Parent Tutoring as a Supplement to Compensatory Education for First Grade Children (Mehran and White, 1988). N students = 76, Grade = 1.**Treatment Groups** = Mothers of students assigned to the treatment group received tutor training. Mothers of students assigned to the control group received no training. Sample composed entirely of at-risk students as determined by their teacher.	**Treatment Defined** =Tutor training consisted of two 4-hour sessions in July, follow-up meetings twice a week during the summer, and follow-up meetings once a month during the school year that provided teaching methods for reading. Parents were then advised to tutor children for 30 minutes twice a week during the school year. This study lasted through April of the school year. **Randomization** = Researchers randomly assigned one of the two lowest scoring students to the treatment group and the other to the control group. Students with the third and fourth lowest scores were similarly assigned, and so on.	**Test Score** = Comprehensive Test of Basic Skills; Woodcock-Johnson Psycho-Educational Battery. **Regression Specification** = Effect sizes were calculated using the means of posttest scores adjusted for the covariance of pretest scores. We report the average effect size across outcome measures. **Results** = The treatment had a 0.164σ (0.281) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Parent Tutoring in Reading Using Literature and Curriculum Materials: Impact on Student Reading Achievement (Powell-Smith et al., 2000). N students = 36, Grade = 2, Location = rural/suburban school in the Pacific Northwest. **Treatment Groups** = Two treatment groups: the first received literature-based home tutoring from parents (LB) and the second received curriculum-based home tutoring from parents (CB). The control group received normal classroom instruction. Sample composed of low readers, as determined by their teachers.	**Treatment Defined** = All treatment parents participated in a single 1-1.5 hour training session. For both treatment groups, parents conducted four 20 minute tutoring sessions with students every week for 15 weeks. Parents in the LB treatment group received a list of books to read during the tutoring session. Parents in the CB treatment group received tutoring materials based on the reading text that students received instruction on in the classroom. Students in this group could either review the story read in class or select a new story. **Randomization** = Each parent/student pair was randomly assigned to one of the three groups.	**Test Score** = Test of Reading Fluency. **Regression Specification** = ANCOVA was used to analyze treatment effects. **Results** = The LB treatment had a -0.344σ (0.411) impact on reading test scores and the CB treatment had a -0.174σ (0.409) impact on reading test scores.
Supporting Families in a High-Risk Setting: Proximal Effects of the SAFEChildren Preventive Intervention (Tolan et al., 2004). N schools = 7, N families = 424, Grade = 1, Location = inner-city Chicago. **Treatment Groups** = Treatment families took part in the SAFEChildren intervention program for 22 weeks. Control group received no such intervention.	**Treatment Defined** = The SAFEChildren program is designed to develop a broad support network for children deemed at-risk of developing anti-social behaviors due to their neighborhood. Treatment parents met in groups weekly to work on parenting skills, family relationships, understanding and managing developmental and situational challenges, increasing support among parents, engaging the school, and managing neighborhood issues like violence. Treatment students underwent 30-minute reading tutoring sessions twice weekly. **Randomization** = In the spring, parents of kindergarten students enrolled in the seven participating schools were invited to participate in the study the following school year. Families that agreed to participate were stratified by their child's kindergarten classroom and 55 percent were randomly assigned to treatment.	**Test Score** = Woodcock-Johnson Diagnostic Reading Battery. **Regression Specification** = Two-level hierarchical linear model (year, child) controlling for family income, parental marriage status, gender, ethnicity, and school fixed effects. **Results** = Researchers found a 0.188σ (0.068) effect on reading scores.

Appendix Table 2 (continued)

Study	Study Design	Results
The Effects of a Negative Income Tax on School Performance: Results of an Experiment (Maynard & Murnane, 1979). N students = 851, Grades = 4 - 10, Location = Gary, IN. **Treatment Groups** = Families assigned to the treatment group received a negative income tax and the control group families continued with the typical income tax system. All participants were impoverished and black.	**Treatment Defined** = The study investigated the impact of a negative income tax program on child achievement. The major features of this program were that participants were guaranteed a minimum annual income and there was a benefit reduction rate (the amount by which the negative income tax payment was reduced for each dollar of income a family earned). It was therefore expected that a negative income tax for poor families would decrease parents' employment and increase total family income. **Randomization** = Families were randomly assigned to treatment and control.	**Test score** = Iowa Test of Basic Skills: Reading subtest. **Regression Specification** = Researchers used a multiple linear regression model controlling for pretest scores, child baseline characteristics, family baseline characteristics, and school characteristics. **Results** = Treatment had a 0.045σ (0.061) impact on reading test scores.
The Effects of a Voluntary Summer Reading Intervention on Reading Activities and Reading Achievement (Kim, 2007). N students = 331, Grades = 1 - 5. **Treatment Groups** = Treatment students received books to read over the summer. Control students received no books.	**Treatment Defined** = Treatment students received books matched to their personal preferences and reading level. Additionally, upon completion of a book, children were instructed to send a postcard to their teachers answering questions about the text. Children were instructed to read ten books over the summer. **Randomization** = Students were stratified by grade and classroom and then randomly assigned to treatment.	**Test Score** = Stanford Achievement Test. **Regression Specification** = Effect size was calculated using average growth between pre and posttest scores. **Results** = Treatment had a 0.037σ (0.120) impact on reading test scores.
The Effects of Training Parents in Teaching Phonemic Awareness on the Phonemic Awareness and Early Reading of Struggling Readers (Warren, 2009). N parents = 10, N students = 10, Grades = K - 1. **Treatment Groups** = Treatment parents received instruction on how to educate their child in phonemic awareness. Control parents received instruction in reading aloud to their children.	**Treatment Defined** = Parents received training once a week for 30 minutes over 10 weeks. Parents taught their children for 30 minutes daily over ten weeks. **Randomization** = Eligible parents came from federally-subsidized housing and had children who scored in the bottom 20 percent of the Dynamic Indicators of Basic Early Learning Skills (DIBELS) Letter-Naming Fluency subtest and below ten initial sounds in the DIBELS Initial Sounds Fluency subtest. 30 potentially eligible parents were identified; ten enrolled. Half of these parents were randomly assigned to the treatment group.	**Test Score** = Dynamic Indicators of Basic Early Learning Skills: Phoneme Segmentation Fluency and Nonsense Word Fluency subtests. **Regression Specification** = Effect sizes were calculated using the average difference in pre and posttest scores. The effect sizes for the two subtests were averaged together. **Results** = Treatment had a 0.233σ (0.639) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
The Impact of a Literature-Based Program on Literacy Achievement, Use of Literature, and Attitudes of Children from Minority Backgrounds (Morrow, 1992). N schools = 2, N classrooms = 9, N students = 166, Grade = 2. **Treatment Groups** = Two treatment groups: treatment group one received literature-based instruction in school and participated in a reading-at-home program; treatment group two received just the school-based instruction. The control group continued with their regular curricula.	**Treatment Defined** = Treatment entailed establishment of the following elements: classroom literacy centers – quiet spaces stocked with roughly five to eight books per child; three teacher-guided literature activities per week, including discussion of past texts and composition of original written work; and an independent reading and writing period three to five times weekly. Further, children in the reading-at-home group read at home at least twice weekly with their parents. **Randomization** = Eligible classrooms were randomly assigned to one of the three conditions. To be eligible, classrooms had to meet the following criteria: literature was not an integral part of the reading curriculum, teachers had no previous training from the district in literature-based instruction, and none had well-designed literacy centers.	**Test Score** = California Test of Basic Skills: Language and Reading subtests. **Regression Specification** = The effect size was calculated for each subtest using the average growth between post and pre test scores. The resulting effect sizes were averaged across subtests. **Results** = Treatment one had a 0.251σ (0.820) impact on reading test scores and treatment two had a 0.046σ (0.817) impact on reading test scores.
The Impact of Parental Training in Methods to Aid Beginning Reading on Reading Achievement and Reading Attitudes of First-Grade Students (Peeples, 1996). N students = 50, Grade = 1, Location = Madison County, MS. **Treatment Groups** = Treatment group parents received Home Enrichment Learning Program (HELP) or tutor training, and control group parents received no training.	**Treatment Defined** = HELP, the parent tutor training, consisted of methods to assist first grade students with beginning reading. The training consisted of a one-hour home visit where researchers discussed the importance of reading, factors associated with reading, and methods to aid reading at home. **Randomization** = Students in this study were chosen from a population of 800 students entering the first grade for the first time and who were participating in a beginning reading program in Madison County School District. Fifty students were then chosen randomly and independently from a population using Cohen's (1965) formula. They were assigned to the control or treatment groups using a computer-generated list of numbers.	**Test Score** = Gates-MacGinitie Reading Test. **Regression Specification** = The effect size was calculated using average posttest scores. **Results** = Treatment had a 0.949σ (0.298) impact on reading test scores.

Appendix Table 2 (continued)

Study	Study Design	Results
Towards Reduced Poverty Across Generations: Early Findings from New York City's Conditional Cash Transfer Program (Riccio et al., 2010). N families = 4750, N students = 11,311, Grades = 4, 7, and 9, Location = New York City. **Treatment Groups** = Treatment parents received incentives, while control parents did not. Sample drawn from districts in New York City with families at or below 130 percent of federal poverty level.	**Treatment Defined** = Parents in the treatment group were offered a set of 22 incentives ranging from 20 to 600 dollars based on education-focused conditions (e.g. children's school attendance, test scores, attendance at parent-teacher conferences), health-focused conditions (e.g. maintaining health insurance. going to doctor, dentist), and workforce-focused conditions (e.g.working or being in job training). **Randomization** = Random lottery.	**Test score** = New York state tests. **Regression Specification** = OLS controlling for characteristics of families. Standard errors adjusted to account for multiple observations per family. We report the average annual impact. **Results** = Treatment had a -0.005σ (0.022) impact on math test scores and a 0.005σ (0.023) impact on reading test scores.

Appendix Table 3 - Schools

Study	Study Design	Results
A Multistate District-Level Cluster Randomized Trial of the Impact of Data-Driven Reform on Reading and Mathematics Achievement (Carlson et al., 2011). N states = 7, N districts = 59, N schools = 538, N students = 31,110, Grades = 3 - 8. **Treatment Groups** = The treatment group received data driven instruction lessons and the control group did not receive any such lessons.	**Treatment Defined** = The Johns Hopkins Center for Data-Driven Reform in Education (CDDRE) worked with treatment districts to implement quarterly student benchmark assessments and provide district and school leaders with extensive training on interpreting and using the data to guide reform. Control districts did not receive any training or consultants. Each district received one year of treatment, but treatment was implemented in waves. **Randomization** = The CDDRE contacted state departments of education in seven states - AL, AZ, IN, MS, OH, PA, and TN - and asked them to nominate districts with large numbers of low-performing schools. District officials were contacted and those that agreed were included in the randomization procedure. District officials then identified schools within their district that they would want to include in treatment. Generally, low performing schools were chosen. After this recruitment process, the randomization process occurred at the district level. The randomization was stratified by state and recruitment wave.	**Test Score** = Various state-administered tests standardized at the state level. **Regression Specification** = Two-level hierarchical linear model (school, district) controlling for pretest score at the school level and school level demographics as well as district level demographics. **Results** = Treatment had a 0.059σ (0.029) impact on math test scores. Treatment had a 0.033σ (0.020) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
A Randomized Experiment of a Cognitive Strategies Approach to Text-Based Analytical Writing for Mainstreamed Latino English Language Learners in Grades 6-12 (Kim et al., 2011). N schools = 15, N teachers = 103, N students ≈ 3,000, Grades = 6 - 11, Location = Santa Ana Unified School District. **Treatment Groups** = Treatment teachers were selected to participate in Pathway Project professional development. Control teachers were not.	**Treatment Defined** = The Pathway Project teaches teachers how to integrate cognitive strategy instruction and process writing to develop students' text-based analytical writing abilities. Teachers assigned to a Pathway Project classroom attended a mix of full-day and after-school sessions for intensive training and support from Pathway Project developers over the course of a school year (46 total hours of training). Each participating teacher was paid a $1,000 stipend to complete all research activities. Teachers in the control group were given classroom resources and received the Pathway professional development in the third year of the study. **Randomization** = Classrooms were assigned to grade-school blocks. Within these blocks, classrooms were randomly assigned to either the Pathway intervention or the control group.	**Test score** = California Standards Test. **Regression Specification** = Three-level hierarchical linear model (student, classroom, school randomization block) controlling for pretest scores. **Results** = Treatment had a 0.046σ (0.035) impact on reading test scores.
A Study of Cooperative Learning in Mathematics, Writing, and Reading in the Intermediate Grades: A Focus Upon Achievement, Attitudes, and Self-Esteem by Gender, Race, and Ability Group (Glassman, 1989). N schools = 2, N classrooms = 24, N students = 441, Grades = 3 - 5, Location = Bay Shore, NY. **Treatment Groups** = Treatment classrooms incorporated cooperative learning strategies into their curricula. Control classrooms continued with their normal curricula.	**Treatment Defined** = Cooperative learning is designed to change student attitudes toward academic success by focusing on group achievement. In treatment classrooms, students were organized into teams of similar ability and completed group assignments in reading, writing, and mathematics following an initial presentation by the teacher. These treatment classes supplanted normal reading, writing, and math courses for the school year. Treatment teachers underwent an 11-week training period prior to implementation of the experiment. **Randomization** = Classes were stratified by grade and matched based on pretest performance. One class from each pairing was assigned at random to treatment.	**Test Score** = Iowa Test of Basic Skills: Mathematics and Reading subtests. **Regression Specification** = Posttest scores adjusted for pretest scores were used to calculate effect sizes. **Results** = Treatment had a 0.011σ (0.408) impact on math test scores and a 0.040σ (0.408) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots (Abdulkadiroglu et al., 2009). N students ≈ 11,000, Grades = K - 12, Location = Boston, MA. **Treatment Groups** = Students in the charter treatment won admission lotteries to oversubscribed charter schools. Students in the pilot treatment won admission lotteries to oversubscribed pilot schools. Control students were losers of the respective lotteries.	**Treatment Defined** = This study utilizes random admission lotteries to investigate the impacts of charter and pilot schools on students' achievement. A charter school is a public school that operates fairly autonomously within guidelines laid down by its state. Charter schools are generally free to manage day-to-day operations, hire teachers and let them go, choose salary schedules, and make curricular decisions. Pilot schools have some of the independence of charter schools – they determine their own budgets, staffing, curricula, and scheduling. However, pilots remain part of the Boston school district and their teachers are Boston Teachers Union members covered by most contract provisions related to pay and seniority. Pilot schools are subject to external review, but the review process to date appears to be less extensive and structured than the external state charter reviews. **Randomization** = Student admission lotteries.	**Test Score** = Massachusetts Comprehensive Assessment System: Mathematics and Reading subtests. **Regression Specification** = OLS regressions. Charter school regressions include dummies for (combination of schools applied to)*(year of application). Pilot school regressions include dummies for (first choice)*(year of application)*(walk zone status). **Results** = Winning a lottery to a charter school had a 0.337σ (0.071) impact on math test scores and a 0.201σ (0.068) impact on reading test scores. Winning a lottery to a pilot school had a -0.026σ (0.069) impact on math test scores and a 0.052σ (0.067) impact on reading test scores.
Alternative Routes to Teaching: The Impacts of Teach for America (TFA) on Student Achievement and Other Outcomes (Glazerman et al., 2006). N schools = 17, N classrooms = 100, N students = 1,800, Grades = 1 - 5, Regions = Baltimore, Chicago, Compton, Houston, New Orleans, and the Mississippi Delta **Treatment Groups** = Treatment students received teaching from a TFA teacher. Control students received teaching from a non-TFA teacher.	**Treatment Defined** = This study is intended to estimate the impact of having a TFA teacher. The control group consisted of those teachers who were not a member of the TFA corps at the time of the study or at any time in the past. **Randomization** = Within each region, schools were randomly selected. Students were stratified by grades within school and randomly assigned to classrooms either taught by a TFA or non-TFA teacher.	**Test Score** = Iowa Test of Basic Skills: Mathematics and Reading subtests. **Regression Specification** = The ITT estimates are estimated using a nested model with the student-level model nested in the block-level model. The model controls for student-level characteristics and block fixed-effects. **Results** = Treatment had a 0.15σ (0.04) impact on math test scores and 0.03σ (0.04) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
An Evaluation of a Pilot Program in Reading for Culturally Disadvantaged First Grade Students (Bowers, 1972). N schools = 4, N students = 200, Grade = 1. **Treatment Groups** = Treatment classrooms implemented the *Distar* reading program. Control classrooms continued with their normal curricula. Sample drawn from students eligible for Title 1 assistance, and who scored below the 25th percentile on the Metropolitan Reading Readiness Test.	**Treatment Defined** = The *Distar* program is a professional development program designed to change how teachers help struggling, disadvantaged youth. The program emphasizes a systematic approach to decoding instruction, in which children learn the standard rules of reading. Treatment teachers attended a one-week instructional workshop prior to the start of the school year. **Randomization** = Researchers randomly selected a sample of 50 eligible students from each school. Within each of these samples, students were randomly assigned to treatment. Teachers were randomly assigned to instruct treatment classrooms.	**Test Score** = Gates-MacGinitie Reading Test: Vocabulary and Comprehension subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the posttest means adjusted for pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.257σ (0.181) impact on reading test scores.
An Evaluation of Reading Recovery (Center et al., 1995). N schools = 10, N students = 70, Grade = K - 1, Location = New South Wales, Australia. **Treatment Groups** = Treatment students participated in Reading Recovery (RR). Control students continued with business-as-usual.	**Treatment Defined** = RR is an early intervention for low performing students. It consists of extensive professional development for the teachers and one-on-one 30-minute daily lessons to accelerate the literacy learning of these children. **Randomization** = Teachers in the 10 participating schools identified 20 students whom they considered to be at the greatest risk of failure. These students were tested using the Clay Diagnostic Survey. The 12 lowest scoring students from each school were randomly assigned to three groups: the treatment group, the control group, or a holding group. The holding group was excluded from the analysis and solely existed to delay the entry of control students into the RR program (when treatment students completed/dropped out of RR, they were replaced by a holding student).	**Test Score** = The Burt Word Reading Test and the Neale Analysis of Reading Ability. **Regression Specification** = For each outcome measure, effect sizes were calculated using the growth between pretest and posttest scores. We report the average effect across all outcome measures. **Results** = The impact of receiving RR was 1.582σ (0.321) on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
An Evaluation of Teachers Trained Through Different Routes to Certification: Final Report (Constantine et al., 2009). N districts = 20, N schools = 63, N teachers = 174, N students = 2,610, Grades = K - 5, Location = CA, IL, WI, LA, GA, NJ, and TX. **Treatment Groups** = Treatment students were taught by alternatively certified (AC) teachers. Control students were taught by traditional certified (TC) teachers.	**Treatment Defined** = TC programs place teachers in classrooms only after they have completed teaching certification requirements while AC programs place teachers in schools before they have completed their requirements. **Randomization** = To be eligible, teachers had to (1) be relative novices (three or fewer years of teaching experience prior to 2004–2005, five or fewer years prior to 2005–2006); (2) teach in regular classrooms (for example, not in special education classrooms); and (3) deliver both reading and math instruction to all their own students. In the study schools, every grade that contained at least one eligible AC teacher and one eligible TC teacher was included. Students in these study grades were randomly assigned to be in the class of an AC or a TC teacher.	**Test score** = The California Achievement Test, 5th Edition. **Regression Specification** = OLS regression that controls for student characteristics (pretest scores in all subjects, race, gender, and free/reduced price lunch status), years of teaching experience, and school fixed-effects. **Results** = AC teachers had an impact of -0.05σ (0.032) on math test scores and -0.01σ (0.050) on reading test scores.
An Evaluation of the Teacher Advancement Program (TAP) in Chicago: Year One Impact Report (Glazerman et al., 2009). N schools = 16, N students = 3,501, Grades = K - 8, Location = Chicago, IL. **Treatment Groups** = Treatment schools implemented the Teacher Advancement Program (TAP). Control schools continued with business-as-usual.	**Treatment Defined** = TAP attempts to increase school and teacher quality through incentives. Teachers receive performance bonuses based on value added to student achievement and classroom observations. Principals receive bonuses based on school-wide value added and quality of program implementation. Other school staff receive incentives based on school-wide value added. TAP also includes weekly meetings of teachers and a teacher mentor component. **Randomization** = Schools were grouped by readiness to participate and then randomly assigned to treatment or control (schools with higher readiness had a higher probability of selection). Analyses are weighted to account for this. Within each group, constrained minimization was utilized to ensure that schools were balanced across school size, predominant race, and geographic location.	**Test Score** = Illinois Standards Achievement Test: Mathematics and Reading subtests. **Regression Specification** = Effect sizes were calculated using posttest means adjusted for family poverty, special needs, language, race/ethnicity, grade level, and over normal age for a grade. **Results** = Treatment had a -0.04σ (0.06) impact on math test scores and -0.04σ (0.05) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
An Investigation of the Effects of a Comprehensive Reading Intervention on the Beginning Reading Skills of First Graders at Risk for Emotional and Behavioral Disorders (Mooney, 2003). N schools = 7, N students = 47, Grade = 1. **Treatment Groups** = Treatment students received the *Sound Partners* reading intervention program. The control group received no such intervention. Sample drawn from students at risk of developing emotional and behavioral disorders as determined by both their teachers and a psychological screening test.	**Treatment Defined** = All students followed the district's core curriculum. Treatment students received the *Sound Partners* intervention, which entails approximately 30 minutes of reading tutoring five times weekly throughout the school year in addition to the normal curriculum. The intervention targets phonological awareness, letter-sound relationships, word identification, text reading, and writing. **Randomization** = Students were randomly assigned to treatment.	**Test Score** = Woodcock Reading Mastery Test-Revised: Basic Reading Skills and Reading Comprehension subtests; Dynamic Indicators of Basic Early Literacy Skills: Phoneme Segmentation Fluency, Nonsense Word Fluency, and Oral Reading Fluency subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.278σ (0.299) impact on reading test scores.
Are High-Quality Schools Enough to Increase Achievement Among the Poor? Evidence from the Harlem Children's Zone (Dobbie and Fryer, 2011). N students ≈ 850, Grades = K - 8, Location = New York City. **Treatment Groups** = The treatment group consists of students that won the lottery to attend the Promise Academy charter schools in Harlem Children's Zone. The control group consists of students that applied and did not win the lottery.	**Treatment Defined** = The Promise Academy charter schools are "No Excuses" charter schools. They have an extended school day and year, additional classes and tutoring for struggling students, high-quality teachers, and provide free medical, dental, and mental-health services. The schools also provide student incentives for achievement, nutritious cafeterias meals, and support for parents in the form of food baskets, meals, and bus fare. **Randomization** = Student admission lotteries.	**Test Score** = State math and reading tests. **Regression Specification** = OLS regressions that control for gender, race, free lunch status, grade fixed effects, and year fixed effects. The middle school regressions additionally control for previous test scores in the same subject, special education status in previous grades, and whether the student spoke English as a second language in previous grades. **Results** = Winning the lottery had a 0.121σ (0.049) impact on math test scores and a 0.036σ (0.042) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Assessment Data - Informed Guidance to Individualize Kindergarten Reading Instruction: Findings from a Cluster-Randomized Control Field Trial (Al Otaiba et al., 2011). N schools = 14, N teachers = 44, N students = 556, Grade = K. **Treatment Groups** = Treatment teachers received Individualized Student Instruction for Kindergarten (ISI-K) and Assessment to Instruction (A2i) training. Control received general professional development that the treatment teachers also received.	**Treatment Defined** = The baseline of professional development that both groups received included a researcher-delivered summer day-long workshop on Response to Intervention approaches and individualized instruction. ISI-K training was meant to help teachers differentiate classroom reading instruction. The ISI-K intervention supports teachers' ability to use assessment data to inform instructional amounts, types, and groupings. Teachers can use A2i software to analyze students' language and reading scores and determine recommended amounts of instruction. **Randomization** = Fourteen schools were matched on several demographic criteria as well as reading test scores. One school from each matched-pair was then randomly assigned to treatment.	**Test Score** = AIMSWeb Letter Sound Fluency; Woodcock-Johnson III: Picture Vocabulary, Letter Word Identification, and Word Attack subtests; Dynamic Indicators of Basic Early Literacy Skills: Nonsense Word Fluency and Phoneme Segmenting Fluency subtests. **Regression Specification** = A hierarchical multivariate linear model. We report the average impact across all outcomes. **Results** = Treatment had an impact of 0.18σ (0.10) on reading test scores.
Can a Mixed-Method Literacy Intervention Improve the Reading Achievement of Low-Performing Elementary School Students in an After-School Program? Results From a Randomized Controlled Trial of READ 180 Enterprise (Kim et al., 2011). N students = 296, Grades = 4 - 6, Location = Southeastern MA. **Treatment Groups** = Treatment students received READ 180 instruction during an after-school program. Control students were assigned to a regular district after-school program.	**Treatment Defined** = READ 180 uses a combination of teacher-directed instruction, computer-based reading lessons, and independent reading. The program allows for differentiated instruction in each of the components of reading. **Randomization** = The sample of eligible students included children who scored below proficiency on the Massachusetts Comprehensive Assessment System. Students in this eligible sample who returned consent forms were stratified by school and grade and then randomly assigned into either the treatment or the control group.	**Test Score** = Stanford Achievement Test 10: Reading Comprehension, Vocabulary, and Spelling subtests; Dynamic Indicators of Basic Early Literacy Skills: Oral Reading Fluency subtest. **Regression Specification** = OLS regression controlling for student characteristics including pretest fluency score and school-grade randomization block fixed-effects. **Results** = Treatment had an impact of 0.20σ (0.09) on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Can Interdistrict Choice Boost Student Achievement? The Case of Connecticut's Interdistrict Magnet School Program (Bifulco et al., 2009). N students = 494, Grades = 6 - 8 Location = near Hartford, CT. **Treatment Groups** = The treatment group consisted of students who won an admission lottery and were assigned to magnet schools. The control group consisted of students who lost the same lottery.	**Treatment Defined** = This study investigates the impact of attending two inter-district magnet schools. The goal was to promote racial and economic integration by allowing students from different school districts to integrate. One school served grades 6-8, and the other served grades 6-12. **Randomization** = Lottery-based admissions to charter schools. Admission lotteries held in each of five districts for each of the two schools.	**Test Score** = Connecticut Mastery Test - 8th grade reading test. **Regression Specification** = OLS regression controlling for pretest scores (fall of fourth grade, fall of sixth grade) and individual covariates (age, gender, ethnicity, free-lunch eligibility, and special education status). **Results** = The impact of winning the lottery is 0.037σ (0.046) on math test scores and 0.081σ (0.054) on reading test scores.
Career Academies: Impacts on Students' Engagement and Performance in High School (Kemple and Snipes, 2000). N schools = 9, N students = 1,764, Grade = 8 - 9, N years = 4. **Treatment Groups** = Treatment students received the *Career Academy* intervention. Control students received no such intervention.	**Treatment Defined** = Treatment students remain with the same group of teachers throughout high school to develop stronger, supportive educational relationships. Their curriculum includes both academic lessons and vocational material, while the school builds relationships with local employers to provide students with career and work-based learning opportunities. **Randomization** = Students were assigned at random to treatment.	**Test Score** = National Educational Longitudinal Survey of 1988: Math and Reading Comprehension batteries. **Regression Specification** = Effect sizes were calculated using regression-adjusted posttest means, controlling for background characteristics. We report the annual impact of the program. **Results** = Treatment had a 0.004σ (0.019) impact on math test scores and a -0.012σ (0.019) impact on reading test scores.
Charter Schools in New York City: Who Enrolls and How They Affect Their Students' Achievement (Hoxby and Murarka, 2009). N schools = 42, N students = 32,551, Grade = 3 - 8, Location = New York City, NY. **Treatment Groups** = Treatment group is comprised of lottery winners to charter schools while the control group is comprised of lottery losers.	**Treatment Defined** = A charter school is a public school that operates fairly autonomously within guidelines laid down by its state. Charter schools are generally free to manage day-to-day operations, hire teachers and let them go, choose salary schedules, and make curricular decisions. This study looked at charter schools throughout New York City. **Randomization** = Student admission lotteries.	**Test Score** = New York State Examinations. **Regression Specification** = OLS regression of student achievement on charter dummy, students' pretreatment covariates, lottery, school and grade fixed effects. We report annual impacts. **Results** = Treatment had a 0.092σ (0.016) impact on math test scores and a 0.039σ (0.016) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Classroom Assessment for Student Learning: The Impact on Elementary School Mathematics in the Central Region (Randel et al., 2011). N districts = 32, N schools = 67, N teachers = 409, N students ≈ 4,700, Grade = 4, Location = CO. **Treatment Groups** = Treatment teachers participated in the Classroom Assessment for Student Learning (CASL) professional development program. Control teachers did not.	**Treatment Defined** = CASL is a self-executing professional development program in which teachers learn from a CASL textbook and use CASL assessments to better understand their students' progress. Intervention schools were given all available CASL material as if they had purchased it and researchers were not involved in the implementation of CASL in any way. Intervention schools also created teams of three to six teachers for support and discussion of the CASL material. Control schools were given $1,000 in order to eliminate the alternative hypothesis that any impact was the result of the schools receiving more resources. The study consisted of a training year and an implementation year. We only report results from the implementation year. **Randomization** = Researchers invited public schools in Colorado that were of sufficient size to have at least one 4th grade and one 5th grade teacher. For their search, researchers focused on the 55 districts that had greater than six total schools and elementary principals that had signed up to be on the Mid-continent Research for Education and Learning's mailing list. In order to randomly assign the schools that volunteered to participate, districts were first assigned to nine blocks. The first six blocks were the six districts that had multiple elementary schools that agreed to participate. The final three blocks consisted of districts that only had one elementary school that agreed to participate. These blocks were grouped based on locale, location in Colorado, and date the schools elected to participate. A random number generator was then used to randomly assign half of the schools within each block to treatment and the other half to control. When there was an odd number of schools in a block, the additional school was assigned to control.	**Test Score** = Math scores from the Colorado state test. **Regression Specification** = Regressions controlling for school-level pretest scores, student-level pretest scores, randomization block fixed effects, and a student's grade. We report the average annual impact. **Results** = Treatment had a 0.096σ (0.073) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Closing the Achievement Gap: A Structured Approach to Group Counseling (Campbell and Brigman, 2005). N schools = 20, N students = 480, Grades = 5 - 6. **Treatment Groups** = The treatment group took part in group counseling utilizing the *Student Success Skills* model. The control group did not take part in such counseling. Sample drawn entirely from students scoring between the 25th and 60th percentile on the math and reading subtests of the Florida Comprehensive Achievement Test.	**Treatment Defined** = Treatment entailed group counseling for 45 minutes, once a week, for eight weeks, followed by four follow-up sessions over the next four months. The goal of the *Student Success Skills* model was to develop academic, social, and self-management skills. **Randomization** = Students were stratified by school and grade and assigned randomly to treatment.	**Test Score** = Florida Comprehensive Achievement Test: Mathematics and Reading subtests. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = Treatment had a 0.490σ (0.116) impact on math test scores and a 0.238σ (0.114) impact on reading test scores.
Combining Cooperative Learning and Individualized Instruction: Effects on Student Mathematics Achievement, Attitudes, and Behaviors (Slavin et al., 1984). N schools = 6, N classrooms = 18, N students = 504, Grades = 3 - 5. **Treatment Groups** = Treatment schools were assigned to one of two treatment conditions: condition one implemented Team-Assisted Individualization (TAI) strategies into their curriculum; condition two utilized the same curriculum as the TAI group, but did not implement a team environment. The control group maintained their normal curricula.	**Treatment Defined** = All treatment students worked on an individualized curriculum via a series of instruction sheets, worksheets, and final assessments. Students in the TAI group were assigned to four or five member teams, each with a mix of high and low mathematics achievers as determined by the pretest. Teams were reassigned after four weeks. Students asked their teammates for help if necessary. At the end of each week, a team score was calculated by summing the final assessment scores from each team member. Students in the individual condition received the same instructions, worksheets, and final assessments as the TAI group, but did not work in a team-setting. Treatment replaced the normal mathematics curriculum. **Randomization** = Schools were assigned at random to one of the three conditions.	**Test Score** = Comprehensive Test of Basic Skills. **Regression Specification** = Effect sizes were calculated using average growth between pretest and posttest scores. **Results** = The TAI treatment had a 0.109σ (1.001) impact on test scores. The TAI without the team environment had a 0.102σ (1.001) impact on test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Comer's School Development Program in Prince George's County, Maryland: A Theory-Based Evaluation (Cook et al., 1999). N schools = 23, N students ≈ 12,000, Grades = 7 - 8, Location = Prince George's County, MD. **Treatment Groups** = Treatment schools implemented the School Development Program and control schools did not.	**Treatment Defined** = The School Development Program has three program structures: the School Planning and Management Team, the Social Support Team, and the Parent Team. These three structures are supposed to work together according to three process principles: 1) adults within the school should cooperate with each other, always putting student needs above their own; 2) the school should operate with a problem-solving rather than a fault-finding orientation; and 3) decisions should be reached by consensus rather than vote. Comer believes that if the program structures operate under these processes, then the processes will spread within the school, staff will focus on attaining widely shared goals, trust will be shared, and staff will understand and meet children's needs. **Randomization** = Twenty one schools were paired up with regards to racial composition and previous years' test scores. Schools within each pair were then randomly assigned to treatment or control using a coin toss. One school was not paired and was assigned randomly to treatment or control. Note that two pilot schools were included in the treatment sample because "no obvious differences were found when they were or were not included in the analyses".	**Test Score** = Maryland State Readiness Test: Math score. **Regression Specification** = School-level outcome means were adjusted using MANOVA, for pretest scores, unreliability in these scores, average school-level socioeconomic status, enrollment size, and elementary school California Achievement Test scores. We report annual impacts. **Results** = Treatment had a 0.008σ (0.033) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Comparing Instructional Models for the Literacy Education of High-Risk First Graders (Pinell et al., 1994). N districts = 10, N schools = 40, N students = 403, Grade = 1. **Treatment Groups** = Four treatment conditions: condition one received the *Reading Recovery* (RR) intervention; condition two received the *Reading Skills* (RS) intervention; condition three received a Direct Instruction (DI) intervention; and condition four received a reading and writing (R&W) intervention. Control students continued with the normal curricula. Sample composed of those students with the lowest test scores within each school.	**Treatment Defined** = Students in the RR condition received daily 30-minute lessons, in which they read familiar texts, independently read instructional-level texts, analyzed and discussed these texts, composed sentences, and reconstructed cut-up sentences. Students in the RS condition took part in daily 30-minute exercises designed to develop independent reading strategies. Students in the DI condition received individualized tutoring in fundamental reading skills. Students in the R&W condition received small-group instruction designed to develop a systematic approach to reading. **Randomization** = Researchers selected four schools from each district, one of which employed the RR program prior to the study. This school was automatically assigned to the RR condition. The remaining schools from each district were assigned at random to the other conditions. Each school then offered a pool of ten students with the worst test scores, and four of these were assigned randomly to the treatment specific to their school. The remaining six students in each pool were considered control.	**Test Score** = The Woodcock Reading Mastery Tests and the Gates-MacGintie Reading Test. **Regression Specification** = Hierarchical linear model (student, school) controlling for pretest scores. **Results** = The RR treatment had a 0.484σ (0.218) impact on reading test scores. The RS treatment had a 0.154σ (0.203) impact on reading test scores. The DI treatment had a 0.190σ (0.221) impact on reading test scores. The R&W treatment had a 0.222σ (0.250) impact on reading test scores.
Direct Instruction in Fourth and Fifth Grade Classrooms (Sloan, 1993). N schools = 7, N teachers = 10, N students = 173, Grades = 4 - 5, Location = IN. **Treatment Groups** = Treatment teachers received Direct Instruction training. Control teachers continued business-as-usual.	**Treatment Defined** = The professional development focused on instructing teachers how to use instructional and questioning strategies associated with Direct Instruction. **Randomization** = The researcher first contacted fifteen elementary schools across a district in Indiana. Fourteen principals agreed to let the researcher reach out to the fourth and fifth grade teachers in their schools. Ten teachers contacted agreed to participate in the study. Five of these teachers were randomly assigned to treatment and the other five to control.	**Test Score** = Comprehensive Test of Basic Skills. **Regression Specification** = Effect sizes were calculated using average growth between pretest and posttest scores. **Results** = Treatment had a 0.090σ (0.633) impact on math test scores and a 0.090σ (0.633) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Early College, Early Success: Early College High School Initiative (ECHSI) Impact Study (Berger et al., 2013). N students = 2,458, Grades = 9 - 12. **Treatment Groups** = The treatment group consists of students who were offered admission to an Early College from a lottery and a comparison group that included students who participated in the lottery but were not offered admission.	**Treatment Defined** = Early Colleges partner with colleges and universities to offer all students an opportunity to earn an associates degree or up to two years of college credits toward a bachelors degree during high school at no or low cost to the students. The underlying assumption is that engaging underrepresented students in a rigorous high school curriculum tied to the incentive of earning college credit will motivate them and increase their access to additional postsecondary education and credentials after high school. **Randomization** = Random admission lotteries.	**Test score** = Standardized state assessment scores in reading and mathematics. **Regression Specification** = Two-level hierarchical linear model (student, school) controlling for gender, race, low income, and standardized achievement scores in prior reading and mathematics. **Results** = Winning the lottery of an Early College had a 0.14σ (0.04) impact on reading scores and a 0.05σ (0.04) impact on math scores.
Effect of Technology-Enhanced Continuous Progress Monitoring on Math Achievement (Ysseldyke and Bolt, 2007). N districts = 7, N schools = 8, N classrooms = 80, N students = 1,880. **Treatment Groups** = Treatment classrooms incorporated the *Accelerated Math* technology-enhanced progress monitoring system (*AM*) for a full year. Control classrooms maintained normal curricula. Sample drawn from schools who previously expressed interest in the *AM* system, but had not yet implemented it.	**Treatment Defined** = The *AM* progress monitoring system tracks student performance via regular mathematics exercises and assessments. The software generates practice exercises tailored to the individual and provides both students and teachers with immediate feedback. Teachers are thus able to adjust their classroom instruction based on individual performance. The treatment was in addition to normal class time. **Randomization** = Teachers were stratified by school and grade then assigned at random to treatment. In schools where teachers taught multiple classes, classrooms were stratified by school and grade then assigned randomly to treatment.	**Test Score** = The STAR Math assessment; Terra Nova tests: Math subtests. **Regression Specification** = OLS regression controlling for pretest scores and school fixed effects. The average effect across both subtests is reported. **Results** = Treatment had an impact of 0.215σ (0.114) on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Effectiveness of Paraeducator-Supplemented Individual Instruction: Beyond Basic Decoding Skills (Vadasy et al., 2007). N schools = 9, N teachers = 26, N students = 46, Grades = 2 - 3. **Treatment Groups** = Treatment students received extracurricular reading tutoring administered by paraeducators between October and March. The control group received no such tutoring during this time. Sample drawn from students who scored between the 10th and 37th percentile on the word identification subtest from the Woodcock Reading Mastery Test-Revised.	**Treatment Defined** = Treatment students received 30 minutes of extracurricular tutoring per day, four days per week, for 15 weeks. These tutoring sessions included 15 minutes of phonics instruction and 15 minutes of oral passage reading. Target skills included letter-sound correspondences, decoding, sight word reading, spelling, and phonics generalizations. Paraeducators received three hours of training prior to the study, plus an additional 60 to 90 minutes of on-site training. **Randomization** = Students were stratified by school and assigned randomly to treatment.	**Test Score** = Dynamic Indicators of Basic Early Literacy: Oral Reading Fluency subtest; Woodcock Reading Mastery Test-Revised: Word Attack and Word Identification subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means adjusted for pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.502σ (0.311) impact on reading test scores.
Effects of a Volunteer Tutoring Model on the Early Literacy Development of Struggling First Grade Students (Pullen et al., 2004). N students = 49, Grade = 1, Location = FL. **Treatment Groups** = Treatment group received volunteer tutoring from January to April and the control group received normal classroom instruction.	**Treatment Defined** = Tutors implemented a tutoring model that included repeated reading of familiar texts, explicit coaching in decoding and word strategy, and reading new books for forty 15-minute sessions throughout the term. Each tutor was provided with materials such as a guided lesson plan, checklist, and leveled books to use during sessions. Tutors were university students who were recruited and hired for this study. Tutors typically were education majors with limited tutoring experience. Tutors received four hours of training and demonstrated mastery of the tutoring model prior to interacting with students. **Randomization** = Students were tested using the measure of invented spelling. Those who scored at or below the 30th percentile on the invented spelling assessment were eligible to participate in the study. Eligible students were randomly assigned to a treatment or control group.	**Test Score** = Woodcock Diagnostic Reading Battery: Letter Word Identification and Word Attack subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means. We report the average effect across all outcome measures. **Results** = Treatment had a 0.626σ (0.300) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Effects of Academic Tutoring on the Social Status of Low-Achieving, Socially Rejected Children (Coie and Krehbiel, 1984). N schools = 7, N students = 40, Grade = 4, Location = Durham, NC. **Treatment Groups** = 3 treatment groups: treatment group one received academic skill training; treatment group two received social skill training; treatment group three received both. The control group received no such training.	**Treatment Defined** = Academic training entailed a meeting with individual tutors for 45 minutes twice weekly from October to April. The social skill training entailed pairing a child with a more popular, same-sex peer and coaching the child on positive behaviors before and after the interaction; training took place in class once a week for six weeks. All trainers were undergraduates who were coached by the authors prior to treatment. **Randomization** = All students from the sample schools completed both a sociometric evaluation and the California Achievement Tests. From these examinations, researchers identified 40 students who scored below the 36th percentile on their reading test scores and who were ranked as unpopular by their peers. Researchers assigned these students randomly to one of the four groups.	**Test Score** = The math and reading portions of the California Achievement Tests. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means adjusted for pretest scores. We report the average effect across all outcome measures. **Results** = The academic training treatment had a 0.773σ (0.464) impact on math test sores and a 0.472σ (0.453) impact on reading test scores. The social skills training treatment had a 0.326σ (0.452) impact on math test sores and 0.397σ (0.452) impact on reading test scores. The combined training treatment had a 0.505σ (0.454) impact on math test sores and a 0.616σ (0.458) impact on reading test scores.
Effects of Intensive Reading Remediation for Second and Third Graders and a 1-Year Follow-Up (Blachman et al., 2004). N districts = 4, N schools = 11, N students = 89, Grades = 2 - 3. **Treatment Groups** = Treatment group utilized an intensive reading intervention in place of their traditional remedial reading instruction. The control group continued with normal remedial reading instruction. Sample drawn from students with demonstrated difficulty reading as determined by the pretest.	**Treatment Defined** = Treatment children received 50 minutes of individual reading tutoring, five days per week, between September and June. These sessions replaced remedial reading instruction that would otherwise have been implemented by the school. **Randomization** = Students were stratified by school, grade, and gender and then randomly assigned to treatment.	**Test Score** = Woodcock-Johnson Mastery Tests-Revised: Word Identification and Word Mastery subtests; Woodcock-Johnson Psycho-Educational Battery-Revised: Calculation and Applied Problems subtests ; the Gray Oral Reading Tests Third Edition. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means adjusted for pretest scores. We report the average effects for math and reading outcomes. **Results** = Treatment had a -0.275σ (0.243) impact on math test scores and a 0.728σ (0.249) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Effects of Peer-Assisted Learning Strategies With and Without Training in Elaborated Help Giving (Fuchs et al., 1999). N classrooms = 24, Grades = 2 - 4. **Treatment Groups** = Two treatment groups: one received training in elaborated help-giving prior to implementing peer-assisted learning strategies (PALS); the other implemented PALS without such training. The control group continued with its regular curriculum. All classrooms included at least some children with chronic reading difficulties and problematic social behaviors.	**Treatment Defined** = The treatment groups incorporated three 35-minute PALS sessions per week into their normal reading time. During these sessions, a high-ability student was paired with a low-ability student (ability determined by the teacher), and the pair would undertake three activities: partner reading, paragraph shrinking/summarization, and prediction relay. Students offered each other feedback and were awarded points for completing reading activities and giving feedback. The team with the highest points was recognized by the class at the end of each week. Those students selected for training in help-giving learned strategies on how to identify reading difficulties and potential solutions. **Randomization** = Classrooms were stratified by grade and half were randomly assigned to implement the PALS program; among those selected for treatment, half were again stratified by grade and randomly assigned for advanced instruction in help-giving strategies.	**Test Score** = Stanford Diagnostic Reading Test: Reading Comprehension subtest. **Regression Specification** = Effect sizes were calculated for each grade strata and each treatment. We report the average effect across grades for each treatment. **Results** = The PALS treatment had a 0.749σ (0.517) on reading test scores and the PALS with help-giving training treatment had a 0.355σ (0.504) impact on reading test scores.
Effects of Reading Decodable Texts in Supplemental First-Grade Tutoring (Jenkins et al., 2004). N schools = 11, N students = 121, Grade = 1. **Treatment Groups** = Two treatment groups: one attended tutoring that included more decodable texts, the other attended tutoring with less decodable texts. The control group maintained their normal curricula. Sample drawn from students who scored at or below the 25th percentile on the Wide Range Achievement Test.	**Treatment Defined** = All treatment students attended 30-minute tutoring sessions four days per week, for 25 weeks. Each session included the following components: practicing letter-sound relations, reading decodable words, spelling, reading nondecodable words, and text reading. As time went on, text reading progressively occupied more of each session. The sessions for the group with more-decodable texts included tutoring on storybooks with a higher concentration of words that could be deconstructed from previous phonetic instruction. **Randomization** = Students were randomly assigned to the three groups.	**Test Score** = Woodcock Reading Mastery Tests-Revised: Word Attack, Word Identification and Passage Comprehension subtests; Wide Range Achievement Test-Revised: Reading subtest; Test of Word Reading Efficiency: Sight Word and Phonetic Vocabulary subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means. We report the average effect across all outcome measures. **Results** = The more decodable treatment had a 0.646σ (0.282) impact on reading test scores. The less decodable treatment had a 0.673σ (0.279) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Effects of Whole Class, Ability Grouped, and Individualized Instruction on Mathematics Achievement (Slavin and Karweit, 1985). N classrooms = 22, N students = 480, Grades = 3 - 5, Location = Hagerstown, MD. **Treatment Groups** = Three treatment groups: the first received the *Missouri Mathematics Program* (*MMP*); the second received the *Ability Grouped Active Teaching* (*AGAT*) curriculum; the third received the *Team Assisted Individualization* (*TAI*) curriculum. The control group maintained their normal curricula.	**Treatment Defined** = All teachers in the treatment group received three hours of training and additional implementation assistance for one of the following curricula: the *MMP* was a whole-class, group-paced curriculum focused on active and effective teaching in the form of frequent questions and feedback; the *AGAT* curriculum divided the class into small groups based on skill – teachers differentiated material and pace between the groups, and also incorporated frequent question and answer; the *TAI* curriculum had children complete independent work while receiving help from peers of similar skill level, while teachers provided guidance and assistance as necessary. **Randomization** = Teachers were assigned randomly to one of the four groups.	**Test Score** = Comprehensive Tests of Basic Skills: Mathematics Computations and Concepts/Applications subtests. **Regression Specification** = Effect sizes were calculated using posttest means for each outcome. We report the average effect size across the two subtests. **Results** = The MMP treatment had a 0.180σ (0.587) impact on math test scores. The AGAT treatment had a 0.751σ (0.655) impact on math test scores. The TAI treatment had a 0.361σ (0.641) impact on math test scores.
Enhancing First-Grade Children's Mathematical Development with Peer-Assisted Learning Strategies (Fuchs et al., 2002). N teachers = 20, N students = 327, Grade = 1. **Treatment Groups** = Treatment classrooms incorporated peer-assisted learning strategies (PALS) into their curricula. The control group continued with normal curricula.	**Treatment Defined** = All teachers followed the district's core curriculum. Treatment teachers incorporated PALS exercises in class for 30-minutes, three times weekly, for 16 weeks. During these exercises, students worked cooperatively on math games officiated by the teacher. Students were paired by mathematics ability for three-week cycles. The stronger student acted first as tutor to the lower-performing student, and these roles switched halfway through the cycle. Teachers reassigned the pairings at the end of each cycle. **Randomization** = Teachers were stratified by school and assigned at random to treatment.	**Test Score** = The Stanford Achievement Test. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = Treatment had an impact of 0.250σ (0.449) on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Enhancing Kindergarteners' Mathematical Development: Effects of Peer-Assisted Learning Strategies (Fuchs et al., 2001). N schools = 5, N teachers = 20, N students = 228, Grade = K. **Treatment Groups** = Treatment classrooms incorporated peer-assisted learning strategies (PALS) into their curricula. The control group maintained normal curricula.	**Treatment Defined** = All teachers followed the district's core curriculum. Treatment teachers incorporated PALS exercises in class for 20-minutes, twice weekly, for 15 weeks. During these exercises, students worked cooperatively on math games officiated by the teacher. Students were paired by mathematics ability for two-week cycles. The stronger student acted first as tutor to the lower-performing student. These roles switched halfway through the cycle. Teachers reassigned the pairings at the end of each cycle. **Randomization** = Teachers were stratified by school and assigned at random to treatment.	**Test Score** = Stanford Early School Achievement Test: Mathematics subtest. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = Treatment had a 0.161σ (0.451) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Enhancing the Efficacy of Teacher Incentives Through Loss Aversion (Fryer et al., 2015). N schools = 9, N students ≈ 2,150, Grades = K - 8, Location = Chicago Heights, IL. **Treatment groups** = Teachers were assigned to control or one of four incentivized treatment groups - "Loss", "Gain", "Team Loss" and "Team Gain".	**Treatment Defined** = "Loss" teachers are paid a lump sum in advance and asked to give their money back if their students don't improve sufficiently; "Gain" teachers receive financial incentives in the form of bonuses at the end of the year linked to student achievement; "Team Loss" teachers are equivalent to "Loss" teachers but their payout is also based on the improvement of students taught by a teammate teacher in the same school; and "Team Gain" teachers are equivalent to "Gain" teachers but their payout is also based on the improvement of students taught by a teammate teacher in the same school. The experiment was run two separate years, randomizing teachers each year. Note that the "Team Gain" treatment arm does not exist in the second year of the experiment. We report results for the pooled "Loss" and pooled "Gain" treatments. Note that although math, reading, and science test scores were incentivized (the latter only for fourth and seventh grade science teachers), the main analysis of the paper focuses on math achievement due to most students having multiple reading teachers and the science sample being so small. **Randomization** = Participating teachers with one homeroom class for the entire day were randomly assigned to one of the five groups; for teachers with classes throughout the day, each class was randomly assigned to one of the five groups. Note that teachers in the "team treatments" were paired with a teacher in the same school and treatment group who taught similar grades, subjects, and students.	**Test Score** = Illinois State Achievement Test; Iowa Test of Basic Skills. **Regression Specification** = OLS regressions with individual level controls (gender, race, free lunch eligibility, limited english proficiency status, special education status and baseline ThinkLink test scores), school fixed effects, and grade fixed effects. **Results** = The "Loss" treatment had a 0.197 σ (0.071) impact on math test scores. The "Gain" treatment had a 0.097 σ (0.076) inmpact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Evaluation of Experience Corps: Student Reading Outcomes (Morrow-Howell et al., 2009). N students = 881, Grades = 1 - 3. Location = Boston, MA; New York City, NY; and Port Arthur, TX. **Treatment Groups** = Treatment students participated in the Experienced Corps (EC) program and control students did not.	**Treatment Defined** = The EC program recruits volunteers aged 55+ to mentor and tutor children who are at risk of academic failure. Volunteers receive training focused on literacy and relationship building. Volunteers work with students one-on-one for about 15 hours per week. **Randomization** = At the beginning of the school year, all students in need of reading assistance were referred to the Experience Corps program. All referred students were then randomly assigned to the treatment or control group.	**Test Score** = Woodcock-Johnson: Word Attack and Passage Comprehension subtests; Peabody Picture Vocabulary Test. **Regression Specification** = For each outcome measure, effect sizes were calculated using posttest means adjusted for pretest scores, gender, site, grade, race, classroom behavior, individualized education program, and limited english proficiency. The average effect size is reported. **Results** = Treatment had a 0.075σ (0.067) impact on reading test scores.
Evaluation of Quality Teaching for English Learners (QTEL) Professional Development: Final Report (Bos et al., 2012). N districts = 8, N schools = 52, N teachers = 303, N students = 8,720, Grades = 6 - 8. **Treatment Groups** = Teachers in the treatment group had access to QTEL professional development; the control teachers did not have access.	**Treatment Defined** = QTEL is a professional development program that prepares teachers to instill comfort and ease with the English language, rather than focusing on isolated and discrete language skills. The program was originally designed for teachers that taught English as a second language, but is available to all teachers. Participating teachers receive the following instruction: small conferences with their peers over the summer; one-on-one coaching sessions with QTEL staff four to six times throughout the year; and monthly lesson-design meetings with QTEL staff. We only report results for the cohort that was exposed to three years of the experiment. **Randomization** = Schools were stratified by district and assigned at random to the treatment group.	**Test Score** = The California Standards Test for English Language Arts. **Regression Specification** = Effect sizes were calculated using regression-adjusted posttest means. **Results** = Treatment had a 0.01σ (0.03) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Evaluation of the DC Opportunity Scholarship Program: Final Report (Wolf et al., 2010). N students = 2,300. **Treatment groups** = Treatment students were offered a private school voucher. Control students applied to the same program but were not offered a voucher.	**Treatment Defined** = The District of Columbia Opportunity Scholarship Program (OSP) is the first federally funded private school voucher program in the United States. Students who applied and were selected by the program were given the option to move from a public school to a participating private school of their choice. **Randomization** = Students that applied to the program were randomly selected to receive scholarship offers. Note that the program only conducted a random lottery within a grade band (K-5, 6-8, or 9-12) if that grade band was over-subscribed.	**Test Score** = Stanford Achievement Test 9: Mathematics and Reading subtests. **Regression Specification** = OLS regressions controlling for student pretest scores, if a student attended a school labeled as needing improvement between 2003 and 2005, student's age at time of application, student's entering grade, gender, race, special needs, mother's education, mother's employment, household income, number of children in household, and the number of months the student's family has lived at its current address. We report the average annual impact. **Results** = Winning the lottery had a 0.004σ (0.026) impact on math test scores and a 0.026σ (0.028) impact on reading test scores.
Evaluation of the Early Start to Emancipation Preparation Tutoring Program in Los Angeles County, CA (Courtney et al., 2008). N students = 402. **Treatment Groups** = Treatment group received tutoring services from the Early Start to Emancipation Preparation (ESTEP) program. Control group continued with usual instruction. Note the sample consists of students aged 14 and 15 that are one to three years behind grade-level reading or math skill. All students are in foster care.	**Treatment Defined** = Treatment students received an average of 18 hours of tutoring in reading or math, up to a maximum of 65 total hours over the two years of the evaluation. The program was also designed to inform students about educational resources available to them, as well as create a mentoring relationship between the tutor and student. Tutors were local community-college students. **Randomization** = Students were referred to the study by their emancipation preparation advisor. Students who elected to participate were then assigned randomly into the treatment group.	**Test Score** = Woodcock-Johnson III: Letter Word Identification, Calculation, and Passage Comprehension subtests. **Regression Specification** = OLS regression controlling for student's baseline scores, gender, race, ethnicity, physical health, mental health, substance abuse, level of social support, whether the student was placed in a group home, whether the student previously ran away from foster care, and the type of foster care. **Results** = Treatment had a 0.048σ (0.048) impact on math test scores and a 0.016σ (0.045) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Evaluation of the Effectiveness of the Alabama Math, Science, and Technology Initiative (AMSTI) (Newman et al., 2012). N schools = 82, N teachers ≈ 780, N students ≈ 20,000, Grades = 4 - 8, Location = AL. **Treatment Groups** = Treatment schools implemented AMSTI. Control schools continued as usual.	**Treatment Defined** = AMSTI involves comprehensive professional development delivered through a 10-day summer institute and follow-up training during the school-year; access to program materials, manipulatives, and technology needed to deliver hands-on, inquiry based instruction, and in-school support by AMSTI lead teachers and site specialists who offer mentoring and coaching for instruction. **Randomization** = From the eligible schools that applied to the program, researchers made an effort to select a sample that was representative of the population of schools in the regions involved. Pairs of similar schools were selected from the pool of applicants based on similarity in mathematics achievement, the percentage of minority students, and the percentage of students from low-income households. Within each pair, schools were randomly assigned either to the AMSTI condition or to the control condition.	**Test Score** = Stanford Achievement Test. **Regression Specification** = Two-level hierarchical linear model (student, school) controlling for pretest score, grade level, racial/ethnic minority status, eligibility for free or reduced-price lunch, proficiency in English, gender, and matched pairs fixed effects. Note that we only report the results of the first year of the experiment because the researchers did not report reading impacts in the second year. **Results** = AMSTI had a 0.05σ (0.02) impact on math test scores and a 0.06σ (0.02) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Evaluation of the i3 Scale-Up of Reading Recovery: Year One Report (May et al., 2013). N schools = 147, N students = 866, Grade = 1. **Treatment groups** = Treatment students participated in the Reading Recovery program. Control students did not.	**Treatment Defined** = Reading Recovery is a short-term intervention designed to help low performing first grade students catch up to their peers. Teachers involved in the Reading Recovery program are specifically trained on how to work with struggling students and to implement the program's instructional approach. During a school year, these teachers spend approximately half of their work day working with the same eight low-performing students. **Randomization** = 628 schools were enrolled in the i3 scale-up of Reading Recovery. These schools were randomly assigned to 3 blocks. One of these blocks was randomly chosen to participate in a RCT of Reading Recovery during the 2011-2012 school year. Within each of these schools, a subsample of low-performing students was identified using the Observation Survey of Early Literacy. In each school, the eight students with the lowest scores were matched according to pretest scores and English Language Learner status. One student in each pair was randomly assigned to treatment and the other to control.	**Test Score** = The Iowa Test of Basic Skills: Composite Reading score. **Regression Specification** = A three-level hierarchical linear model (student, matched-pair, school) controlling for pretest scores. **Results** = Treatment had a 0.47σ (0.05) impact on reading scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Experimental Estimates of Education Production Functions (Krueger, 1999). N students = 11,600, Grade = K - 3. **Treatment Groups** = Two treatment conditions: condition one classrooms reduced their class size; condition two classrooms continued with normal class size but included a teacher's assistant. Control classrooms continued with normal class size.	**Treatment Defined** = Condition one classrooms reduced their class size to 13 - 17 students from a normal level of 22 - 25 students. Condition two classrooms included a teacher's assistant to help manage the larger class size. **Randomization** = Students were stratified by school and assigned at random to one of the three conditions. Each school had at least one classroom for each treatment condition.	**Test Score** = Stanford Achievement Test: Mathematics and Reading subtests. **Regression Specification** = OLS regression controlling for race, gender, free lunch status, teacher's race, teacher's experience, teacher's education, fraction of classmates in class previous year, average fraction of classmates together previous year, fraction of classmates on free lunch, fraction of classmates who attended kindergarten, current grade, first grade in sample, and school fixed-effects. We only report results for the small classroom treatment, because results for the other treatment were not reported by math and reading. **Results** = Initial assignment to small classes had a 0.107σ (0.033) impact on math test scores and a 0.133σ (0.033) impact on reading test scores.
Explaining Charter School Effectiveness (Angrist et al., 2011). N schools = 22, N students = 9,141, Grades = 4 - 8 and 10, Location = MA. **Treatment Groups** = Treatment group comprised of lottery winners to charter schools while the control group comprised of lottery losers.	**Treatment Defined** = A charter school is a public school that operates fairly autonomously within guidelines laid down by its state. Charter schools are generally free to manage day-to-day operations, hire teachers and let them go, choose salary schedules, and make curricular decisions. This study looks at charter schools throughout the state of Massachusetts. **Randomization** = Student admission lotteries.	**Test Score** = Massachusetts Comprehensive Assessment System. **Regression Specification** = ITT regression of student achievement on baseline demographic characteristics and a dummy variable set representing every combination of charter school lotteries, year and grade effects. **Results** = Treatment had a 0.201σ (0.042) impact on math test scores and a 0.075σ (0.035) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Final Reading Outcomes of the National Randomized Field Trial of Success for All (Borman et al., 2007). N schools = 35, N students = 2,108, Grades = K - 2, Location = 11 states (largely concentrated in urban Midwest locations, such as Chicago and Indianapolis, and in the rural small town South), N years = 3. **Treatment Groups** = Treatment schools adopted the Success for All model. Control schools did not adopt this model. Sample drawn from high poverty schools and is a majority African American.	**Treatment Defined** = The intervention is purchased as a comprehensive package, which includes materials, training, ongoing professional development, and a well-specified "blueprint" for delivering and sustaining the model. Schools that elect to adopt Success for All implement a program that organizes resources to attempt to ensure that every child will reach the 3rd grade on time with adequate basic skills and will continue to build on those skills throughout the later elementary grades. **Randomization** = Cluster randomized trial, with schools randomized into the treatment or control conditions. Note that control schools actually implemented Success for All, but only in grades 3-5. Treatment schools implemented Success for All in grades K-2. Comparisons were then made between the treated K-2 students and the untreated K-2 students. Researchers claim that observations for treatment fidelity did not reveal any significant contamination due to this research design.	**Test Score** = Woodcock-Johnson: Word Attack, Word Identification, and Passage Comprehension subtests. **Regression Specification** = Hierarchical linear model (student, school) controlling for average school pretest scores. We report the average annual effect across subtests. **Results** = Treatment had a 0.090σ (0.060) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Financial Incentives and Student Achievement: Evidence from Randomized Trials (Fryer, 2011). N students ≈ 27,000, Grades = 2 (Dallas), 4 and 7 (NYC), and 9 (Chicago). **Treatment Groups** = In each district, students in the treatment group received monetary incentives for performance in school according to a simple incentive scheme. Control students were not incentivized.	**Treatment Defined** = In Dallas, students were paid to read books. In New York, students were rewarded according to interim assessments. In Chicago, students were paid for classroom grades. **Randomization** = School-level randomization where the method employed is called re-randomization – minimize the maximum z-score from an equation regressing pre-assigned treatments on race, previous year test score, free lunch status and English Language Learner eligibility.	**Test Score** = State assessments used by each city. **Regression Specification** = OLS regression controlling for reading and math achievement scores from the previous two years, race, gender, free/reduced lunch eligibility, English language learner status, the percent of black students in the school, the percent of Hispanic students in the school, and the percent of free/reduced lunch students in the school. For Dallas, regressions also include a control for whether the student took the English or Spanish version of the ITBS/Logramos test in the previous year. For Dallas and New York City, regressions also include an indicator for being in special education. For New York City, regressions also include controls for the number of recorded behavioral incidents a student had in the previous year, as well as the number of recorded behavioral incidents the school had in the previous year. **Results** = Paying students to read books had a 0.079σ (0.086) impact on math test scores and 0.012σ (0.069) impact on reading test scores. Paying students for performance on standardized tests had a 0.008σ (0.041) impact on math test scores and a -0.008σ (0.025) impact on reading test scores. Rewarding ninth graders for their grades had a -0.010σ (0.023) impact on math test scores and a -0.006σ (0.028) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Full-day versus Half-day Kindergarten: An Experimental Study (Holmes and McConnell, 1990). N schools = 20, N students = 637. **Treatment Groups** = Treatment students attended full-day kindergarten and control students attended half-day kindergarten.	**Treatment Defined** = Treatment schools had a full-day kindergarten schedule whereas control schools operated on a half-day kindergarten schedule. **Randomization** = Ten of the elementary schools in the school system were randomly chosen to be in the treatment group. The randomization was stratified by whether or not the school was a Chapter I (low socio-economic status) school.	**Test Score** = California Achievement Tests. **Regression Specification** = Posttest means for the treatment and control groups were compared using Students t-tests. **Results** = The treatment had an impact of -0.290σ (0.088) on math test scores and an impact of 0.112σ (0.083) on reading test scores.
Homework in Arithmetic (Koch, 1965). N teachers = 3, N classrooms = 3, N students = 85, Grade = 6. **Treatment Groups** = Two treatment groups: the first received a longer daily arithmetic assignment, while the second received a shorter daily arithmetic assignment. The control group received no arithmetic homework.	**Treatment Defined** = The first treatment group received a daily homework assignment in arithmetic that took approximately 30 minutes to complete. The second treatment group received a daily homework assignment in arithmetic that took approximately 15 minutes. All classes used the same arithmetic textbook. **Randomization** = Classes were randomly assigned to one of the three conditions.	**Test Score** = Iowa Test of Basic Skills: Arithmetic Concepts and Arithmetic Problem Solving subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = The shorter assignment intervention had a 0.158σ (1.417) impact on math test scores. The longer assignment intervention had a 0.261σ (1.444) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Impact of eMINTS Professional Development on Student Achievement (Brandt et al., 2013). N schools = 60, N teachers = 191, N students = 3610, Grades = 7 - 8, Location = MO, N years = 2 - 3. **Treatment Groups** = The program consisted of two treatment groups: Teachers in the first treatment group received a two-year professional development program, eMINTS Comprehensive. Teachers in the second treatment group received the same professional development program plus Intel Teach Program, which adds a third year to the original program length. Control teachers did not receive eMINTS or the Intel Teach Program. Sample drawn from high poverty rural schools.	**Treatment Defined** = The eMINTS program is based on inquiry based learning, high quality lesson design, establishing a community of learners, and technology integration. It provides teachers with approximately 240 hours of professional development spanning two years and support that includes monthly classroom visits. The eMINTS and Intel Teach Program combines additional professional development and Intels' suite of Web-based teaching tools to build on what teachers learned in the first two years of the program. **Randomization** = Participating schools were randomly assigned to one of the three groups. Schools had to meet requirements under Title I or Missouri's historical requirements for Title II.D.	**Test Score** = Missouri Assessment Program. **Regression Specification** = Two-level hierarchical linear model (student, school) controlling for block fixed-effects, pretest scores, student gender, race, free/reduced-price lunch status, Limited English Proficient status, Individualized Education Program status, teacher gender, years of teaching, and if the teacher had a graduate degree. Note that we report the average impact across both treatment groups due to the researchers not reporting impacts separately until the third year. For similar reasons, we only report results from the first year of implementation. **Results** = The eMINTS treatment had a 0.067σ (0.044) impact on math test scores and a 0.007σ (0.047) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Impacts of Comprehensive Teacher Induction: Results from the Second Year of a Randomized Controlled Study (Isenberg et al., 2009). N districts = 17, N students ≈ 3,000, Grades = K - 6. **Treatment Groups** = Treatment schools implemented a comprehensive induction program for one or two years. Control schools continued as usual.	**Treatment Defined** = New teachers at treatment schools were provided induction services by either the Educational Testing Service or the New Teacher Center (districts were able to select which service they wanted to receive). Teachers exposed to the intervention were assigned to a full-time mentor for support and training, offered monthly professional development sessions, opportunities to observe veteran teachers, and a colloquium at the end of the school year. **Randomization** = Researchers invited districts that met certain criteria (at least 570 teachers in elementary schools, at least 50 percent of students eligible for free/reduced-price lunch, in the continental U.S., and no prior exposure to comprehensive induction) to participate in the study. In smaller participating districts, all elementary schools with eligible teachers (K-6 teacher, not in departmentalized middle schools, new to the profession, and not already receiving support) were included in the study. Larger districts could elect to only provide the researchers with a subset of elementary schools. Participating schools within each district were randomly assigned to a treatment or control group using constrained minimization. Within participating schools, all eligible teachers were included in the study. Based on the school's willingness to participate, treatment schools were then placed into groups that either received one year or two years of intervention.	**Test Score** = The state math and reading assessments that each district administered. **Regression Specification** = Researchers used a two-level linear hierarchical model (student, school) controlling for student-level pretest scores, student gender, student race/ethnicity, special education status, English-language learner status, free/reduced-price lunch status, overage for grade, teacher age, teacher age squared, teacher gender, race/ethnicity, indicator showing if a teacher's race/ethnicity matches that of a majority of students, teacher route into teaching, teacher highest degree, teacher holds a degree in an education-related field, first-year teacher, teacher hired after the school year began, teacher attended a competitive college, teacher held a non-teacher job for five or more years, grade fixed-effects, and district fixed-effects. We report average annual impacts. **Results** = Treatment had a 0.000σ (0.044) impact on reading test scores and a -0.010σ (0.044) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Improving Students' Reading Comprehension Skills: Effects of Comprehension Instruction and Reciprocal Teaching (Spörer et al., 2009). N schools = 2, N students = 210, Grades = 3 - 6, Location = Germany. **Treatment Groups** = Three treatment conditions: condition one utilized traditional reciprocal teaching (RT) strategies; condition two utilized instructor guided reading strategies (IG); and condition three utilized the reciprocal teaching in pairs (RTP) strategies. Control students continued with the normal curriculum.	**Treatment Defined** = Students in condition one focused on developing the following four reading strategies: summarizing, questioning, clarifying, and predicting. Students in condition two focused on the same reading strategies, but instruction was carried out in small groups of 4 - 6 students led by an instructor. Students in condition three were first taught the four reading strategies, and then practiced them in pairs. All conditions received two, 45-minute lessons per week. **Randomization** = First, one school was randomly assigned to the traditional instruction condition as control group, whereas the other school was assigned to the intervention. Second, students from the treatment school were randomly assigned to treatment groups.	**Test Score** = Diagnostischer Test Deutsch, assessed 12 weeks after the completion of treatment. **Regression Specification** = ANCOVA analysis controlling for pretest scores. **Results** = The RT treatment had a 0.681σ (0.203) impact on reading test scores. The RTP treatment had a 0.282σ (0.179) impact on reading test scores. The IG treatment had a 0.159σ (0.198) impact on reading test scores.
Information and Employee Evaluation: Evidence from a Randomized Intervention in Public Schools (Rockoff et al., 2012). N principals = 223, N students = 1,434, Location = New York City. **Treatment Groups** = Treatment principals received reports detailing the teacher's value added of all teachers in their school and training on how to interpret this data. Control principals did not have access to these reports and went about business as usual.	**Treatment Defined** = The intervention consisted of giving New York City principals reports detailing the value added of their teachers relative to similar teachers in NYC and training principals on how to use this information. The program was offered to principals in NYC schools containing any grade in 4-8. Principals had to sign-up and complete a survey in order to participate. Over 1,000 principals were eligible to participate, but only 305 signed up. Out of the 305 that signed up, only 223 completed the necessary survey. **Randomization** = Principals in the study were assigned to blocks by grade configuration of their schools (elementary, middle, and K-8 schools). Principals within each block were then randomly assigned to treatment via a random number.	**Test Score** = State test in mathematics and reading. **Regression Specification** = Researchers estimated the impact of the intervention by regressing student-level achievement gains on an indicator for if the student was in a treatment school, allowing for random effects at the teacher and school level. The main specification reported does not include any covariates, but the researchers showed that adding in student-level or teacher-level covariates does not significantly alter the results. **Results** = Treatment had a 0.028σ (0.017) impact on math test scores and a 0.008σ (0.013) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Information and Student Achievement: Evidence from a Cellular Phone Experiment (Fryer, 2013). N students = 1,907, Grades = 6 - 7, Location = Oklahoma City Public Schools. **Treatment Groups** = Three groups of treatment students were provided with free cellular phones and daily information about the link between human capital and future outcomes via text messages. Treatment one students received a monthly allocation of credits on their phone and received daily informational messages. Treatment two students received daily informational messages and were required to read books and take quizzes to receive additional credits on their phone. Treatment three students were required to read books and take quizzes to receive additional credits on their phone. Control students did not receive a phone, informational messages, or non-financial incentives.	**Treatment Defined** = Treatment one - students received a cell phone (pre-loaded with 300 minutes) with daily informational text messages and a fixed allocation of 200 credits on a monthly schedule. Treatment two - students received a cell phone (pre-loaded with 300 minutes), received daily informational text messages, and were required to read books and complete quizzes to confirm their additional understanding of those books in order to receive additional credits on a bi-weekly basis. Treatment three - students received a cell phone (pre-loaded with 300 minutes) and were required to read books and complete quizzes about those books in order to receive additional credits on a biweekly schedule. **Randomization** = Sixth and seventh grade students from the 22 eligible schools in Oklahoma City Public Schools (all schools with sixth and seventh grade students that were not designated alternative education academies) were eligible to participate in the program. Of those 4,810 students, 1,907 returned consent forms and were randomized into one of the four groups.	**Test Score** = Oklahoma Core Curriculum Criterion Referenced Tests. **Regression Specification** = An ITT regression controlling for student-level demographics and school fixed effects. **Results** = Information had a -0.027σ (0.039) impact on math test scores and a 0.040σ (0.041) impact on reading test scores. Non-financial incentives had a -0.023σ (0.047) impact on math test scores and 0.023σ (0.050) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Injecting Charter School Best Practices Into Traditional Public Schools: Evidence From Field Experiments (Fryer, 2014). N schools = 20, N students = 39,464, Grades = K - 5, Location = Houston, TX. **Treatment Groups** = Treatment schools implemented a five-pronged intervention; control schools received no such intervention. Sample composed entirely of low-performing schools.	**Treatment Defined** = Treatment schools implemented the following five practices: increased instructional time; replacing principals and teachers who failed to adequately increase student achievement; implementing daily high-dosage mathematics tutoring for fourth graders; use of data-driven curricula; and fostering a culture of high expectations. **Randomization** = Schools were ranked by aggregate reading and math scores on state achievement tests for grades three through five, as well as by Stanford 10 scores for Kindergarten through second grade. The bottom two schools were automatically assigned to treatment. The remaining 18 schools were placed into pairs based on aggregate scores and one school from each pairing was assigned randomly to treatment.	**Test Score** = Statewide math and reading assessments developed by the Texas Education Agency. **Regression Specification** = OLS regression controlling for student gender, race, free/reduced price lunch status, English language proficiency, special education accommodations, and enrollment in a gifted or talented program, as well as the school-wide composition of student gender, race/ethnicity, free/reduced price lunch, English language proficiency, special education status, and students in gifted/talented program. **Results** = Treatment had a 0.066σ (0.035) impact on math and a 0.034σ (0.023) impact on reading.
KIPP Middle Schools: Impacts on Achievement and Other Outcomes (Tuttle et al., 2013). N schools = 10, N students $\approx$ 1,000, Grades = 5 - 8. **Treatment Groups** = The treatment group consists of students that won a lottery to attend one of the Knowledge Is Power Program (KIPP) charter schools. The control group consists of students that applied and did not win the lotteries.	**Treatment Defined** = KIPP is a national network of public charter schools targeting low-income families. The goal of KIPP is to prepare students for college and set them up to succeed in life. Note only 10 of 53 KIPP middle schools could be included in the experimental sample due to schools not being over-subscribed. **Randomization** = Student admission lotteries.	**Test Score** = State math and reading tests. **Regression Specification** = OLS regression controlling for student's age, gender, race/ethnicity, free lunch status, individualized education program status, pretreatment test scores, whether the student's primary home language is English, whether the household has only one adult, family income, mother's education, school fixed-effects, grade fixed-effects, and lottery year fixed-effects. We report average annual impacts. **Results** = WInning the lottery had a 0.11σ (0.04) impact on math test scores and a 0.05σ (0.05) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Literacy Learning of At-Risk First-Grade Students in the Reading Recovery Early Intervention (Schwartz, 2005). N teachers = 37, N students = 148, Grade = 1, Location = 14 states. **Treatment Groups** = Treatment students participated in Reading Recovery (RR). Control students did not.	**Treatment Defined** = RR is an early intervention for low performing students. It consists of extensive professional development for the teachers and one-on-one 30 minute daily lessons to accelerate the literacy learning of their students. Note that students initially assigned to the control group participated in RR after the completion of the experiment. **Randomization** = Two of lowest scoring students from each classroom were randomized into treatment or control.	**Test Score** = Slosson Oral Reading Test-Revised. **Regression Specification** = The effect size was calculated using posttest means. **Results** = RR had a 0.934σ (0.245) impact on reading test scores.
Longer-Term Impacts of Mentoring, Educational Services, and Learning Incentives: Evidence from a Randomized Trial in the United States (Rodriguez-Planas, 2012). N recruitment sites = 7, N schools = 11, N students = 1,069, Grades = 9 - 12, N years = 5. **Treatment Groups** = Treatment students received the Quantum Opportunity Program (QOP). Control students had access only to those opportunity programs available locally. Sample drawn from students whose eighth-grade GPA fell below the 67th percentile.	**Treatment Defined** = Treatment students took part in an after school program designed to develop social and employment readiness, as well as boost academic performance. Students received $1.25 for every hour they devoted to educational activities, as well as a significant reward if they enrolled in postsecondary education. Those students who graduated on-time received assistance in postsecondary placement. Treatment lasted a total of 750 hours per year. **Randomization** = Students were stratified by school and assigned at random to treatment.	**Test Score** = Achievement tests developed by the National Education Longitudinal Study. **Regression Specification** = OLS regression controlling for gender, age, eighth-grade GPA, race/ethnicity, and school. We report average annual impacts. **Results** = Treatment had a 0.012σ (0.014) impact on math test scores and a 0.013σ (0.016) impact on reading test scores.
Longitudinal Effects of Classwide Peer Tutoring (Greenwood et al., 1989). N schools = 6, N students = 416, Grades = 1 - 4, N years = 4. **Treatment Groups** = Treatment group implemented Classwide Peer Tutoring (CWPT). Control group maintained normal curricula. Sample drawn from schools serving communities of low socioeconomic status.	**Treatment Defined** = At the start of each week, treatment students were assigned into tutor-tutee pairs, and these pairings were assigned to one of two teams. Tutees earned points for their team by completing tasks set by their tutors. Teachers determined the content to be tutored each week. **Randomization** = Four schools were assigned randomly to treatment; the remaining two schools were assigned to the control group.	**Test Score** = The Basic Battery of the Metropolitan Achievement Test. **Regression Specification** = Posttest scores adjusted for pretest scores were used to calculate effect sizes. **Results** = Treatment had a 0.106σ (0.206) impact on math test scores and a 0.162σ (0.209) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Mastery Learning and Student Teams: A Factorial Experiment in Urban General Mathematics (Slavin and Karweit, 1984). N schools =16, N classrooms = 44, N students = 1,092, Grade = 9, Location = Philadelphia, PA. **Treatment Groups** = Treatment classrooms implemented one of three curricula: a mastery curriculum, a team-based curriculum, or both. The control classrooms utilized a focused-instruction curriculum.	**Treatment Defined** = All classrooms used a standard course of instruction composed of 26 units. Students in the mastery condition were tested at the end of each unit to determine if they had achieved at least 80 percent mastery. Those who did not achieve mastery received remedial instruction, while those who did received enrichment instruction in the same unit. Students in the team-based condition were organized into four-member teams and quizzed each week. Team members' improvement in scores were summed to generate a team score, and the highest-scoring team was recognized each week. In the focused-instruction condition, students worked individually and did not receive remedial instruction. **Randomization** = Teachers were stratified by school and assigned randomly to treatment.	**Test Score** = Comprehensive Test of Basic Skills: mathematics computations and the concepts and applications subtests. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = Implementing both mastery and team conditions had a 0.244σ (0.451) impact on test scores, while implementing teams alone had a 0.183σ (0.438) impact, and implementing mastery alone had a 0.015σ (0.403) impact on test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
National Board Certification and Teacher Effectiveness: Evidence from a Random Assignment Experiment (Cantrell et al., 2008). N teachers = 198, N students ≈ 3800, Grades = 2 - 5, Location = Los Angeles Unified School District. **Treatment Groups** = Treatment classrooms were taught by teachers that had applied for certification at any point in time and control classrooms were taught by teachers that never applied.	**Treatment Defined** = Treatment students were taught in classrooms where teachers had applied to the National Board for Professional Teaching Standards (NBPTS) for certification. Note that in order to apply for the National Board certification teachers have to have a minimum of three years teaching experience. The researchers therefore restricted the control group to classrooms with teachers that had at least three years of experience. **Randomization** = Invitations were sent out to all elementary schools in the Los Angeles Unified School District and school participation was voluntary. Teachers in participating schools were matched to NBPTS records and grade 2-5 teachers that had ever applied for NBPTS certification were selected. The research team matched each of these teachers with another teacher in the same school, grade, and calendar track to serve as comparison. Principals were then asked to identify two classes that they would be willing to assign to either of these paired teachers. The researchers randomly assigned each pair of teachers to the classes that the principals chose for them. After classroom randomization, no further contact was made with the schools from the research team.	**Test Score** = California Standards Test: math and language subtests. **Regression Specification** = Student outcome regressions controlled for school-by-year-by-grade fixed effects, baseline math and reading scores interacted with grade, race/ethnicity, ever retained, Title I, eligible for free lunch, homeless, migrant, gifted and talented, special education, English language development, and the means of these variables among all students in the class. **Results** = Teachers who applied to certification had an impact of -0.015σ (0.071) on math test scores and an impact of -0.019σ (0.060) on reading test scores relative to teachers who never applied.

Appendix Table 3 (continued)

Study	Study Design	Results
Paying to Learn: The Effect of Financial Incentives on Elementary School Test Scores (Bettinger, 2012). N students = 873, Grades= 3 - 6, Location = Coshocton, OH. **Treatment Groups** = Treatment students were incentivized by cash to pass tests. Control students were not incentivized.	**Treatment Defined** = Students received $15 for each test on which they scored proficient or better. They received more for advanced or accelerated designation. Payment was given in the form of "Coshocton children's bucks", gift certificates redeemable at any store in Coshocton. **Randomization** = The unit of randomization was the grade level at each of four eligible elementary schools. Each year, eight of sixteen eligible grade-school combinations were selected via lottery to receive financial incentives. First, the district randomly selected one grade per school. After these four drawings, Conschocton conducted a fifth drawing in which they chose four additional grade-school combinations from amongst the remaining possibilities.	**Test Score** = Terra Nova or Ohio Achievement standardized math and reading test. **Regression Specification** = OLS regression controlling for grade, school, time fixed-effects, age, gender, race, free or reduced-price lunch status, pretest scores, and an indicator for outcome test taken. Results are pooled over three years. Standard errors are clustered at the grade-school level. **Results** = Treatment had a 0.1328σ (0.0485) impact on math test scores and a 0.0103σ (0.0454) impact on reading test scores.
Prevention and Remediation of Severe Reading Disabilities: Keeping the End in Mind (Torgesen et al., 1997). N schools = 13, N students = 180, Grades = K - 2, N years = 3. **Treatment Groups** = Three treatment conditions: condition one entailed phonological awareness training plus synthetic phonics instruction (PASP), condition two entailed implict embedded phonics instruction (EP), and condition three entailed a regular classroom support group (RCS). The control group maintained normal curricula. Sample drawn entirely from children with low phonological language ability.	**Treatment Defined** = All treatment children received 80 minutes of supplemental individual instruction per week for 30 months. Children in the PASP condition received explicit instruction in how to sound-out words. Students in the EP group learned words through phonics games, contextual definitions, sentence construction, and reading exercises. The RCS group received tutoring in the skills and activities currently being taught in their curriculum. **Randomization** = Students were randomly assigned to one of the four conditions.	**Test Score** = Woodcock Johnson Mastery Tests-Revised: Word Attack, Word Identification, and Passage Comprehension subtests. **Regression Specification** = Average posttest scores were used to calculate effect sizes. Average annual effect across outcome measures is reported. **Results** = The PASP treatment had a 0.286σ (0.103) impact on reading test scores. The EP treatment had a 0.112σ (0.098) impact on reading test scores. The RCS treatment had a 0.094σ (0.097) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program (Rouse, 1998). N students = 2,258, Grades = K - 8, Location = Milwaukee, WI, N years = 4. **Treatment Groups** = Treatment groups received a voucher to attend a private school. Control students did not receive a voucher. Sample composed entirely of students whose family income fell at least 1.75 times below the poverty line.	**Treatment Defined** = Treatment students received vouchers to at least partially offset the cost of attendance at a nonsectarian private school of their choice. **Randomization** = Students were stratified by the grade and school for which they were applying, then randomly assigned to treatment.	**Test Score** = The reading and math subtests of the Iowa Test of Basic Skills. **Regression Specification** = OLS regression controlling for school and grade applying, as well as gender and family income. Note we only report the results for one year after randomization due to the magnitude of attrition in later years. **Results** = Treatment had a 0.105σ (0.082) impact on math test scores and a 0.049σ (0.075) impact on reading test scores.
Putting Books in the Classroom Seems Necessary But Not Sufficient (McGill-Franzen et al., 1999). N schools = 6, N teachers = 18, N students = 456, Grade = K, Location = Large urban eastern school district. **Treatment Groups** = Teachers in the first treatment group received training and books for their classroom. Teachers in the second treatment group did not receive training, but received books. Control teachers did not receive training or books.	**Treatment Defined** = The professional development focused on techniques for encouraging children to pick up books and read them. The training covered topics such as physical design of the classroom, effective book displays, importance of reading aloud to children, and small-group lessons using teacher made-materials. **Randomization** = Eighteen kindergarten teachers, three each from six schools, were randomly assigned into three groups – (a) training and books, (b) no training and books, (c) no training and no books.	**Test Score** = Peabody Picture Vocabulary Test. **Regression Specification** = Average gains from pre to posttest scores were used to calculate effect sizes. **Results** = The training and books treatment had a 0.118σ (0.578) impact on reading test scores. The books treatment had a -0.465σ (0.585) on reading test scores.
Repeated Reading Intervention: Outcomes and Interactions with Readers' Skills and Classroom Instruction (Vadasy and Sanders, 2008). N schools = 13, N students = 162, Grades = 2 - 3. **Treatment Groups** = Treatment students received the *Quick Reads* tutoring program. Control group received no tutoring. Sample drawn from students with demonstrated difficulty reading, as determined by pretest scores.	**Treatment Defined** = The *Quick Reads* program builds reading fluency via repeated reading strategies, in which tutors first introduced a passage, and then students read it approximately three to four times. Tutoring sessions were conducted in pairs four days per week for 15 weeks. **Randomization** = Students were stratified by grade and school and placed at random into pairs. Pairings were then assigned at random to treatment.	**Test Score** = Woodcock Reading Mastery Tests-Revised: Word Reading Accuracy subtest; Test of Word Reading Efficiency: Sight Word subtest; Gary Oral Reading Test 4: Rate and Comprehension subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.300σ (0.158) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
School Choice as a Latent Variable: Estimating the "Complier Average Causal Effect" of Vouchers in Charlotte (Cowen, 2008). N students = 1,143, Grades = 2 - 8. Location = Charlotte, NC. **Treatment Groups** = Treatment students were offered a voucher to offset the cost of tuition at a private school. Control students applied but did not receive a voucher. Only low-income applicants were considered for the voucher program.	**Treatment Defined** = The vouchers offered grants of up to $1,700 annually to at least partially offset the cost of tuition at a private school. Treatment students could attend the private school of their choice. **Randomization** = Random lottery.	**Test Score** = The Iowa Test of Basic Skills. **Regression Specification** = OLS regressions controlling for family income, mother's education, mother's race, whether both parents lived at home, and student's gender. **Results** = Being offered a voucher had a 0.237σ (0.131) impact on math test scores and a 0.292σ (0.134) impact on reading scores.
School Choice in Dayton, Ohio after Two Years: An Evaluation of the Parents Advancing Choice in Education Scholarship Program (West et al., 2001). N students = 515, Grades = 2 - 9, Location = Dayton, OH. **Treatment Groups** = Students from the treatment group received scholarships to help with the cost of private schools. Control group comprises of students who lost lottery for scholarships. Analysis sample consists of only those students who were in public school at the time of randomization. Vouchers were also offered to students already in private school.	**Treatment Defined** = Parents Advancing Choice in Education offered low income parents scholarships to help defray the costs of sending their children to private schools in Dayton, Ohio. **Randomization** = Random lottery.	**Test Score** = Iowa Test of Basic Skills. **Regression Specification** = OLS regression controlling for pretest math and reading scores. We report annual impacts. **Results** = Treatment had a 0.054σ (0.112) impact on math test scores and a 0.078σ (0.112) impact on reading test scores.
School Choice in New York City After Three Years: An Evaluation of the School Choice Scholarships Program (Mayer et al., 2002). N families = 1,960, Grades = 1 - 5, Location = New York City. **Treatment Groups** = Treatment families were offered a scholarship funded by the School Choice Scholarships Foundation (SCSF). Control families were not offered a scholarship. To be eligible for the scholarship, students had to be eligible for free or reduced-price lunch.	**Treatment Defined** = Treatment families received $1,400 annually from the SCSF for at least three years. The scholarship was designed to at least partially offset the cost of private-school attendance. Families could select the private school of their choice. **Randomization** = Eligible applicants were stratified by whether their school's test scores were above or below the city-wide median. Eighty-five percent of the treatment group was selected at random from those applicants whose schools were below the median. The remaining fifteen percent was selected at random from those applicants whose schools were above the median.	**Test Score** = The reading and mathematics subtests of the Iowa Test of Basic Skills. **Regression Specification** = OLS regression controlling for prestest scores and whether the student came from a public school whose test scores were below the median. We report the average annual impact over three years. **Results** = Treatment had a 0.020σ (0.033) impact on math test scores and a 0.003σ (0.033) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Summer School Effects in a Randomized Field Trial (Zvoch and Stevens, 2012). N students = 93, Grades = K - 1. **Treatment Groups** = Treatment students were invited to participate in a summer literacy program. Control students were not.	**Treatment Defined** = The literacy program was a five-week program that lasted for 3.5 hours a day, four days a week. In the program, students received classroom instruction on fundamental literacy topics, were assigned homework, completed in-class work packets, and practiced literacy skills in small groups with students of a similar skill level. **Randomization** = In the years preceding the intervention, all students below certain cutoff scores on the Nonsense Word Fluency test or Test of Oral Reading Fluency were invited to participate in summer school. In 2010, all students that fell below the cutoff scores were invited and not included in the analysis. The district then established upper bounds so that approximately 50 kindergartners and 50 first graders fell in the range between the cutoff scores and the upper bound scores. Students that fell in this range of scores were considered the experimental sample and randomized into treatment or control.	**Test Score** = Dynamic Indicators of Basic Early Literacy: Nonsense Word Fluency subtest for kindergarten students and the Test of Oral Reading Fluency for the first grade students. **Regression Specification** = OLS regressions. The ITT results reported by the researchers do not contain any covariates as controls. However, they do note that in models that contained student characteristic variables, the treatment effects remained qualitatively similar. **Results** = Treatment had a 0.691σ (0.280) impact on reading test scores.
Teacher Behavior and Pupil Performance: Reconsideration of the Mediation of Pygmalion Effects (Alpert, 1975). N schools = 13, N teachers = 17, N classrooms = 17, N students = 352, Grade = 2, Location = New York City. **Treatment Groups** = Treatment teachers were asked to increase the frequency of certain behaviors. Control teachers received no such intervention. Sample drawn exclusively from Catholic schools.	**Treatment Defined** = Over a period of 11 weeks, treatment teachers were asked to increase target behaviors. These target behaviors included more reading group time, maximizing "best" reading time – those periods when the teacher reported feeling most motivated to teach, covering more materials in their reading group, including fewer pupils in their reading group, and utilizing more good verbal behaviors (praising students' reading, support, reinforcement, praising students' behavior, encouraging questions, etc.) – and positive reinforcement of student success. **Randomization** = Schools were randomly assigned to treatment.	**Test Score** = The vocabulary and reading comprehension subtests of the Gates-MacGintie Reading Test. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.072σ (0.557) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Teacher Incentives and Student Achievement: Evidence from New York City Public Schools (Fryer, 2013). N schools = 396, N students = 185,612, Grades = K - 12, Location = New York City. **Treatment Groups** = Treatment schools received financial incentives. Control schools did not.	**Treatment Defined** = Treatment involved giving schools financial incentives based on whether they met the annual performance target set by the Department of Education. Schools were free to distribute money among teachers at their own discretion. **Randomization** = Schools were randomly assigned based on average proficiency ratings, poverty rates and student demographic characteristics. Final experimental sample consisted of 233 treatment schools and 163 control schools.	**Test Score** = State math and reading test scores. **Regression Specification** = Regressions include test scores from previous years, demographic characteristics and school level controls. We report the annual impact. **Results** = Treatment had a -0.030σ (0.019) impact on math test scores and a -0.018σ (0.021) impact on reading test scores.
Teacher Pay for Performance: Experimental Evidence from the Project on Incentives in Teaching (Springer et al., 2010). N teachers = 296, N students = 23,784, Grades = 5 - 8. **Treatment Groups** = Treatment teachers participated in the Project on Incentives in Teaching (POINT) and control teachers did not.	**Treatment Defined** = POINT was open to middle school mathematics teachers. POINT allowed for treatment teachers to receive an incentive for sufficiently high value added. All treatment and control teachers received a stipend of $750. **Randomization** = Schools were stratified into ten groups based on student scores in prior years. Randomization was done within strata. Clusters of teachers were then assigned to treatment or control status. Clusters were defined based on course groups. Assignments to treatment and control were permanent for the duration of the project.	**Test Score** = Tennessee Comprehensive Assessment Program. **Regression Specification** = Linear models controlling for pretest scores, race/ethnicity, gender, English Language Learner status, special education status, free/reduced-price lunch status, number of days of suspension and unexcused absences, teacher's value-added from the year before the experiment, and the average pretest score of students in a teacher's class. The models also include block fixed effects and cluster random effects. We report the average annual impact across three years. **Results** = Treatment had an impact of 0.017σ (0.023) on math test scores and an impact of 0.003σ (0.012) on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Teacher Study Group: Impact of the Professional Development Model on Reading Instruction and Student Outcomes in First Grade Classrooms (Gersten et al., 2010). N recruitment sites = 3, N schools = 19, N teachers = 84, N students = 575, Grade = 1. **Treatment Groups** = Treatment teachers took part in a teacher study group (TSG) intervention. Control teachers utilized the district's normal professional development program. Sample drawn from schools already utilizing the *Reading First* program.	**Treatment Defined** = Treatment entailed a professional development program designed to integrate research-based comprehension and vocabulary instruction into the classroom. Teachers in treatment schools read and discussed instructional methods in small groups and designed curricula to incorporate these strategies. Treatment occurred over sixteen 75-minute sessions held twice a month from October to June. **Randomization** = Schools were assigned at random to treatment. Seven students from each classroom were selected at random for the student sample.	**Test Score** = Woodcock Diagnostic Reading Battery: Oral Vocabulary, Reading Vocabulary, Passage Comprehension, Letter Word Identification, and Word Attack subtests; Dynamic Indicators of Basic Early Literacy Skills: Oral Reading Fluency subtests. **Regression Specification** = Two-level hierarchical linear model (student, teacher) controlling for pretest scores, as well as teacher and school fixed effects. We report the average effect across all outcome measures. **Results** = Treatment had a 0.225σ (0.205) impact on reading test scores.
Team Pay for Performance: Experimental Evidence From the Round Rock Pilot Project on Team Incentives (Springer et al., 2012). N schools = 9, N teacher teams = 159, N students = 17,383, Grades = 6 - 8, Location = Round Rock Independent School District in Texas. **Treatment Groups** = Treatment teachers received monetary awards based on their students' performance and control teachers did not.	**Treatment Defined** = Performance awards were distributed to teams of teachers based on their collective contribution to student test score gains in the four core subjects. Team performance was based on a value-added measure of student performance or standardized test scores. Teams were predefined by the district and were organized such that each team had at least one teacher for each core subject. Each team typically oversaw the learning experience of 100-140 students. **Randomization** = In each year of the 2-year study, teams were randomized to either the awards intervention or control condition using block-randomization design. Blocks were defined by grades within school.	**Test Score** = Texas Assessment of Knowledge and Skills; Stanford Achievement Test. **Regression Specification** = Two-level hierarchical linear model (student, team) controlling for pretreatment test scores and demographics. We report the average impact across years and outcomes. **Results** = Treatment had an impact of 0.000σ (0.020) on math test scores and an impact of -0.002σ (0.017) on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The (Surprising) Efficacy of Academic and Behavioral Intervention with Disadvantaged Youth: Results from a Randomized Experiment in Chicago (Cook et al., 2014). N students = 106, Grades = 9 - 10, Location = South Side of Chicago. **Treatment Groups** = Students in treatment one participated in "Becoming a Man" (BAM). Students in treatment two participated in BAM and received tutoring. Control students continued business as usual. The sample consisted entirely of male students.	**Treatment Defined** = BAM included in-school programming that exposes youth to pro-social adults, and provides them with social-cognitive skill training that follows the principles of cognitive behavioral therapy. Students in treatment two also received high-dosage, small group tutoring. **Randomization** = From the student population, 106 high-risk males were chosen to participate based on an "academic risk index." These students were stratified by grade and then randomly assigned to the three groups.	**Test Score** = EXPLORE and PLAN tests, developed by ACT Inc. **Regression Specification** = An OLS regression controlling for age, grade, prior math and reading test scores, Individualized Education Plan status, previous year GPA, absences, suspensions and disciplinary incidents, and free lunch eligibility. **Results** = Treatment one had a 0.611σ (0.227) impact on math test scores and a -0.071σ (0.319) impact on reading test scores. Treatment two had a 0.425σ (0.226) impact on math test scores and a -0.043σ (0.262) impact on reading test scores.
The Effect of School Choice on Participants: Evidence from Randomized Lotteries (Cullen et al., 2006). N districts = 1, N schools = 19, N students = 14434, Grade = 8, Location = Chicago, N years = 2. **Treatment Groups** = Treatment students received an offer of admission to a school of their choice. Control students did not receive admission. Sample composed of oversubscribed high schools that determined admission via random lottery.	**Treatment Defined** = This study uses randomized lotteries that determine high school admission in Chicago Public Schools to investigate the impact of school choice. The authors exploit the fact that Chicago's public school students can apply to gain access to public schools outside of their neighborhood school (this is known as an open enrollment system). **Randomization** = Oversubscribed schools stratify applicants by gender and race, and offer admission via a random lottery.	**Test Score** = Reading subtest of the Test of Academic Proficiency. **Regression Specification** = OLS regression controlling for race, pretest scores, age, free-lunch eligibility, special education, bilingual education, living with biological parent, attending assigned eighth grade school, census tract characteristics (fraction Black, fraction Hispanic, poverty rate, fraction high school graduates, fraction homeowners, fraction not in labor force, crime index, fraction of high school students attending private schools), and lottery fixed-effects. **Results** = Winning a lottery to any school had a -0.038σ (0.015) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Effective Instruction of Comprehension: Results and Description of the Kamehameha Early Education Program (Tharp and Roland, 1982). N schools = 2, N classrooms = 8, Grade = 1, Location = HI. **Treatment Groups** = Treatment classrooms implemented the Kamehameha Early Education Program (KEEP) intervention. Control classrooms received no such intervention. Sample drawn entirely from semi-rural public schools.	**Treatment Defined** = The KEEP intervention is a small-group program designed to boost reading comprehension among at-risk students. The program utilizes face-to-face student-teacher interaction to teach comprehension instruction, as well as sight vocabulary and analytic phonics. The intervention took the place of normal class time. **Randomization** = Students were randomly assigned to treatment.	**Test Score** = The Gates-MacGintie Reading Test and the Metropolitan Achievement Test. **Regression Specification** = Effect sizes were calculated using the average posttest scores. We report the average effect across all tests. **Results** = Treatment had a 0.300σ (0.141) impact on reading test scores.
The Effectiveness of Extended Day Programs: Evidence from a Randomized Field Experiment in the Netherlands (Meyer and Klaveren, 2013). N schools = 7, N students = 188, Grades = 5 - 7, Location = Netherlands. **Treatment Groups** = Treatment students received an offer to participate in an extended school day program. The control group received no such offer.	**Treatment Defined** = The extended day program consisted of an additional two hours of language instruction, two hours of math instruction, and one hour of excursions per week. The intervention was conducted at one of the participating schools, and classes were composed of 10 students. **Randomization** = Students were randomly assigned to treatment or control.	**Test Score** = Standardized tests typically used in Dutch elementary schools. **Regression Specification** = OLS regressions controlling for math pretest scores, gender, minority status, parents' education, family structure, and class size. **Results** = Assignment to treatment had a 0.087σ (0.067) impact on math test scores and a 0.005σ (0.081) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Effectiveness of Secondary Math Teachers from Teach for America and the Teaching Fellows Programs (Clark et al., 2013). N states = 11, N districts = 15, N schools = 82, N teachers = 287, N students = 12,699, Grades = 6 - 12. **Treatment Groups** = Two treatment groups: Students in treatment one were taught by teachers from the Teach for America (TFA) program. Students in treatment two were taught by teachers from the The New Teacher Project Teaching Fellows program. Control students were taught by teachers who did not enter teaching through either of these programs.	**Treatment Defined** = TFA and Teaching Fellows programs take a distinctive approach to addressing the need for high-quality teachers of hard-to-staff subjects in high-poverty schools. TFA and the Teaching Fellows programs have highly selective admissions criteria designed to admit only applicants who have demonstrated a high level of achievement in academics or other endeavors and who possess characteristics that the programs view as being associated with effective teaching. **Randomization** = In each participating school, authors identified "classroom matches"—two or more classes covering the same middle or high school math course at the same level, with at least one class taught by a teacher from the program being studied (TFA or Teaching Fellows) and at least one class taught by another teacher, referred to as a comparison teacher, who did not enter teaching through a highly selective alternative route. Students were randomly assigned to these classes.	**Test Score** = For middle school students, authors obtained scores on state-required assessments. For high school students, authors administered end-of-course math assessments developed by the Northwest Evaluation Association. **Regression Specification** = OLS regressions that controlled for students' pretest scores, baseline characteristics, and classroom match indicators. **Results** = Assignment to TFA teachers had a 0.07σ (0.02) impact on math test scores. Assignment to Teaching Fellows had a 0.00σ (0.02) impact on math test scores.
The Effectiveness of Team-Accelerated Instruction on High Achievers in Mathematics (Karper and Melnick, 1993). N classrooms = 12, N students = 247, Grades = 3 - 5, Location = Hershey, PA. **Treatment Groups** = Treatment classrooms implemented the Team-Accelerated Instruction (TAI) math program. Control classrooms continued with their normal curricula.	**Treatment Defined** = The TAI program creates a competitive classroom environment, in which teams of students earn points by completing common goals. Students progress through the program's subject matter as rapidly as they are able. **Randomization** = Four classrooms per grade were included in the study, and two classrooms within each grade were assigned to the treatment condition. Students were stratified by grade and randomly assigned to treatment or control classrooms.	**Test Score** = Iowa Test of Basic Skills: math concepts and math computation subtests. **Regression Specification** = Effect sizes were calculated using the average growth between posttest and pretest scores. **Results** = Treatment had a -0.037σ (0.227) impact on math scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Effects of A One-Year Staff Development Program on the Achievement Test Scores of Fourth-Grade Students (Cole, 1992). N schools = 1, N teachers = 12, N students = 268, Grade = 4, Location = MS. **Treatment Groups** = Treatment teachers received the Mississippi Teacher Assessment Instrument staff development program. Control teachers continued business-as-usual.	**Treatment Defined** = Treatment teachers underwent a comprehensive staff development training program using Mississippi Teacher Assessment Instrument modules as training materials. The 14 Mississippi Teacher Assessment Instrument teacher (pedagogical) behavior competencies include topics such as planning instruction to achieve selected objectives, organizing instruction to take into account individual differences among learners, and obtaining and using information about the needs and progress of individual learners. **Randomization** = Teachers were randomly assigned to the treatment or control group.	**Test Score** = Math and reading scores from the Stanford Achievement Test. **Regression Specification** = Effect sizes were calculated using average posttest score adjusted for pretest scores. **Results** = Treatment had a 0.508σ (0.586) impact on math test scores and a 0.566σ (0.589) impact on reading test scores.
The Effects of 'Brain Gym' as a General Education Intervention: Improving Academic Performance and Behaviors (Nussbaum, 2010). N students = 364, Grades = 2 - 6, Location = East TX. **Treatment Groups** = Treatment students were assigned to classrooms that used the 'Brain Gym' curriculum. Control students were assigned to classrooms that continued with their normal curricula.	**Treatment Defined** = Brain Gym is a movement based program designed to promote whole-brain learning. It is derived from the fundamental premise that learning occurs as humans receive sensory stimuli and initiate movement. **Randomization** = Students were randomly assigned to classrooms and then classrooms were randomly assigned to treatment and control groups.	**Test Score** = The math and reading subtests of the Texas Assessment of Knowledge and Skills. **Regression Specification** = Effect sizes were calculated from the growth between pre and posttest means. **Results** = Brain Gym had a 0.130σ (0.105) impact on math test scores and a 0.188σ (0.106) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Effects of Peer-Assisted Literacy Strategies for First-Grade Readers With and Without Additional Mini-Skills Lessons (Mathes and Babyak, 2001). N schools = 5, N teachers = 30, N students = 130, Grade = 1. **Treatment Groups** = Two treatment groups: one incorporated Peer-Assisted Learning Strategies (PALS) into their curricula, the other incorporated PALS and small-group mini-lessons. Control group maintained their normal curricula.	**Treatment Defined** = During PALS sessions, higher-ability students were paired with lower-ability students as determined by the teacher. These pairings would earn points by completing assigned tasks. These 35-minute sessions occurred three times per week for 14 weeks. Students in the mini-lesson group were placed into groups of three and received 15 to 20 minutes of fluency instruction from their teachers three times weekly for six weeks. All interventions took the place of normal classtime instruction. **Randomization** = Schools were stratified by demographic similarity. Researchers determined the number of teachers to recruit from each type of school in order to create a representative stratified sample. Volunteer teachers were randomly assigned to the three groups.	**Test Score** = The Woodcock Reading Mastery Tests-Revised. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = The PALS treatment had a 0.779σ (0.465) impact on reading test scores. The PALS and mini-lessons treatment had a 0.854σ (0.499) impact on reading test scores.
The Effects of Structured One-on-One Tutoring in Sight Word Recognition of First-Grade Students At-Risk for Reading Failure (Mayfield, 2000). N students = 60, Grade = 1, Location = LA. **Treatment Groups** = Treatment students received 15 minutes per day of one-on-one tutoring and control students were read to aloud in small groups for 15 minutes per day.	**Treatment Defined** = Tutoring was done as a part of the Edmark Reading Program, where America Reads volunteers were trained for two hours either individually or in small groups by the researcher. **Randomization** = Principals and teachers selected a sample of students from the bottom 20 to 30% of first grade readers. Students in the selected sample were then randomly assigned to treatment or control groups.	**Test Score** = Woodcock Reading Mastery Tests-Revised: Word Identification and Passage Comprehension subtests. **Regression Specification** = Effect sizes were calculated using average posttest scores. **Results** = Treatment had a 0.346σ (0.184) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Effects of Theoretically Different Instruction and Student Characteristics on the Skills of Struggling Readers (Mathes et al., 2005). N schools = 6, N students = 298, Grade = 1. **Treatment Groups** = Treatment one students participated in the Proactive Reading program. Treatment two students participated in the Responsive Reading program. Control students did not participate in any such program.	**Treatment Defined** = All interventions took place outside of normal class for 40 minutes per day, five days per week, in groups of three. Proactive Reading systematically built reading skills to develop fluency. As the intervention progressed, words gradually became more complicated and texts became more difficult. Responsive Reading had teachers tailor daily lessons to student needs. Teachers offered explicit instruction in reading and gradually let students become more independent as the intervention progressed. All schools in the study used an enhanced curriculum that built upon the district's normal curriculum by offering assessment measures to help teachers identify if and how students were struggling with reading. **Randomization** = The sample was drawn from students at risk of developing persistent reading difficulty, as determined by the pretest. Eligible students were stratified by school and then assigned at random to one of the three groups.	**Test Score** = The Woodcock-Johnson reading battery. **Regression Specification** = For each outcome measure, effect sizes were calculated using the posttest means. We report the average effect across all outcome measures. **Results** = The *Proactive Reading* treatment had a -0.067σ (0.157) impact on math test scores and a 0.283σ (0.158) impact on reading test scores. The *Responsive Reading* treatment had a -0.133σ (0.156) impact on math test scores and a 0.250σ (0.156) impact on reading test scores.
The Efficacy of an Early Literacy Tutoring Program Implemented by College Students (Allor and McCathren, 2004). N students = 137, Grade = 1. **Treatment Groups** = Treatment group received tutoring and control group received no tutoring.	**Treatment Defined** = College students administering the tutoring to treatment group students received three one-hour group training sessions and additional assistance on site. Each student was tutored on average 2 to 3 times per week for 15-20 minutes per session. **Randomization** = At-risk students were identified by low test scores and teacher recommendations. Eligible students were then randomly selected for either the treatment or control group.	**Test Score** = Woodcock Johnson-Revised: Word Identification, Word Attack, and Passage Comprehension subtests; Test of Word Reading Efficiency: Real Word and Nonword subtest; Dynamic Indicators of Basic Early Literacy Skills: Phoneme-Segmentation Fluency and Nonsense-Word Fluency subtests. **Regression Specification** = Effect sizes were calculated using average posttest scores. We report the average effects size across cohorts and outcome measures. **Results** = Treatment had a 0.422σ (0.222) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Evaluation of Charter School Impacts: Final Report (Gleason et al., 2010). N schools = 36, N students = 2,330. **Treatment Groups** = The treatment group were students that won lotteries to attend charter schools. The control group were students that lost those same lotteries.	**Treatment Defined** = This study investigated the impact of charter schools on students' achievement. The researchers focused on charter schools with 4th-7th grade entry grades and that were at least two years old. Thirty-six charter schools (from multiple states) were eligible and willing to participate with 2005-2006 or 2006-2007 entry cohorts. **Randomization** = Student admission lotteries. In order to be considered in the experimental sample, students had to apply to one of the 36 charter schools during an experimental lottery and give consent to participate in the study.	**Test Score** = State math and reading tests. **Regression Specification** = OLS regressions controlling for pretest reading and math achievement, disciplinary measures, student demographics, family characteristics, school enrollment, and application history. We report the average annual impact of winning a lottery to one of these 36 charter schools. **Results** = Admission to a charter school had a -0.03σ (0.03) impact on math test scores and a 0.04σ (0.03) impact on reading test scores.
The Evaluation of Enhanced Academic Instruction in After-School Programs: Final Report (Black et al., 2009). N schools = 27, N students = 1,218, Grades = 2 - 5. **Treatment Groups** = Treatment group incorporated enhanced academic instruction into their after-school programs. Control group maintained their regular after-school programs. All schools had preexisting after-school programs.	**Treatment Defined** = The enhanced program entailed 45 minutes of structured instruction at the start of the two- to three-hour after-school program; this formal instruction took the place of passive academic support like homework help or tutoring. The goal of this enhanced instruction was the development of new skills, as opposed to the completion of assignments. All after-school programs were offered four days per week. Schools had their choice of implementing either the reading or math programs based on student needs, but were limited to one. **Randomization** = To be eligible for the study, students had to perform at most two years below grade-level in reading or math. Eligible students who applied to the program were then stratified by after-school center and by grade and then assigned at random to treatment. All participants were either already enrolled in after-school programs or referred to such programs based on their poor performance.	**Test Score** = Stanford Achievement Test: Mathematics and Reading subtests. **Regression Specification** = The standardized difference of means was calculated for each outcome measure. The means were adjusted for pretest scores, gender, race/ethnicity, free or reduced-price lunch status, age, whether the student is from a single-parent household, whether the student was over-age for their grade, and mother's education level. We report the average effect across outcome measures and cohorts. **Results** = Treatment had a 0.09σ (0.04) impact on math test scores and a -0.04σ (0.05) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Impact of Elementary Mathematics Coaches on Student Achievement (Campbell and Malkus, 2011). N districts = 5, N schools = 36, N classrooms = 1,169, N students = 24,759, Grades = 3 - 5, Location = VA, N years = 3. **Treatment Groups** = Treatment schools received a mathematics coach to work with their teachers. Control schools received no such intervention.	**Treatment defined** = Coaches attended five mathematics courses in numbers and operations, geometry and measurement, algebra and functions, and probability and statistics before entering their designated school. Coaches also attended a leadership training course one year after entering their school. Coaches worked with teachers to design both curricula and assessments as well as facilitate classtime instruction. **Randomization** = Each district sorted its schools into groups of three based on their demographic composition and history of performance on mathematics assessments. Two schools from each group of three were selected randomly for treatment.	**Test Score** = The Standards of Learning assessment (the standardized state assessment of Virginia). **Regression Specification** = Three-level hierarchical linear model (student, class, school) controlling for age, gender, english-proficiency status, special education status, free/reduced-lunch status, minority status, whether the teacher had a masters degree, teacher's experience at the school, Title I eligibility, school size, and indicators for past academic performance. We report the average effect across all cohorts and grades. **Results** = Treatment had a 0.049σ (0.090) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Impact of Indiana's System of Interim Assessments on Mathematics and Reading Achievement (Konstantopoulos et al., 2013). N schools = 57, N students ≈ 20,000, Grades = K - 8, Location = IN. **Treatment Groups** = Treatment schools participated in Indiana's Diagnostic Assessment Tools. Control schools continued as usual.	**Treatment Defined** = In 2008, the Indiana Department of Education introduced the Diagnostic Assessment Tools. This program consisted of two commercial products, mCLASS (grades K-2) and Acuity (grades 3-8). With mCLASS, teachers are provided with detailed diagnostic measures of their K-2 students in literacy and numeracy. Acuity provides teachers with multiple-choice online assessments in reading and mathematics for Grades 3-8. The assessments are approximately 30 minutes long, typically completed in groups in class, and aligned to the state standards. When a school adopts these products, their teachers are provided with training on how to effectively use them. **Randomization** = The pool of eligible schools were placed into 4 blocks based on locales. From these blocks, 70 schools were randomly drawn. Eleven of these schools were dropped due to previous use of products from the vendors in the study or a school closure. The remaining 59 schools were randomized into an intervention group or a control group in an unbalanced manner (35 treatment and 24 control).	**Test Score** = For grades 3-8, the mathematics and reading ISTEP^{+} (Indiana's state test). In grades K-2, the math and reading portions of Terra Nova. **Regression Specification** = A two-level hierarchical linear model (student, school) controlling for gender, age, race, socioeconomic status, special education status, limited English proficiency status, and school-level percentages of females, minorities, lower socioeconomic status, and limited English proficiency students. **Results** = Treatment had an impact of 0.127σ (0.069) on math test scores and 0.078σ (0.050) impact on reading test scores, respectively.

Appendix Table 3 (continued)

Study	Study Design	Results
The Impact of Two Professional Development Interventions on Early Reading Instruction and Achievement (Garet et al., 2008). N states = 4, N districts = 6, N schools = 90, N teachers = 270, N students $\approx 5,000$, Grade = 2. **Treatment Groups** = Two treatment groups: Teachers in treatment one participated in a reading content-focused professional development program. Teachers in treatment two participated in the same professional development and additionally received in-school coaching. Control teachers did not receive any professional development or coaching.	**Treatment Defined** = Teachers in both treatment groups participated in a teacher institute series that began in the summer and continued through the school year. Throughout the course of the year, treatment teachers attended eight seminar days that each consisted of 6 hours of instruction for a total of 48 hours of professional development. On top of this, teachers in treatment two also received approximately 60 hours of in-school coaching. **Randomization** = Schools were randomly assigned to treatment one, treatment two, or control such that there were equal numbers of schools assigned to each group in a given district. In five of the districts, schools were grouped into blocks of similar characteristics (percentage of minority students or geographic region, depending on the district) and then one-third of each block was randomly assigned to each treatment group. The remaining district was just randomly split into thirds.	**Test Score** = The reading state test scores used in a given district. **Regression Specification** = Regressions controlling for school level pretest scores, student-level gender, age, race/ethnicity, and a separate poverty measure provided by each district. **Results** = The professional development treatment had a 0.08σ (0.08) impact on reading test scores. The professional development plus coaching treatment had a 0.03σ (0.09) impact on reading test scores.
The Influence of Massive Rewards on Reading Achievement in Potential Urban School Dropouts (Clark and Walberg, 1968). N classrooms = 9, N students = 110, Ages = 10 - 13. **Treatment Groups** = The treatment group increased the amount of verbal praise rendered to each student. The control group maintained its normal level of verbal praise.	**Treatment Defined** = Students received rewards during remedial reading sessions in the form of verbal praise; students reported the number of times they received such praise daily. Teachers in the treatment group were asked to at least double the amount of verbal praise rendered to each student. **Randomization** = To be eligible for the study, students had to score one to four years behind grade level on nationally-standardized achievement tests – ranking them as potential dropouts. These students were assigned randomly to one of nine after-school remedial reading programs. Five of these classrooms were assigned at random to treatment.	**Test Score** = Science Research Associates Reading Test, Intermediate Form. **Regression Specification** = Effect sizes were calculated using posttest means of the outcome measure. **Results** = Treatment had a 0.588σ (0.685) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
The Potential of Urban Boarding Schools for the Poor: Evidence from SEED (Curto and Fryer, 2014). N students = 221. **Treatment Groups** = Treatment students received admission to a SEED school; control students applied but did not receive admission to a SEED school. All students included in the sample were black.	**Treatment Defined** = SEED schools are five-day-a-week urban boarding schools that have an extended school day, provide extensive after-school tutoring, utilize data-driven curricula, and maintain a culture of high expectations. The middle schools focus on developing basic math and reading skills, while high schools utilize a college-preparatory curriculum that requires students to take the SAT or ACT, as well as apply to at least five colleges. This study utilizes the fact that when a SEED school is oversubscribed, it determines admission via a random lottery. **Randomization** = Admission to an oversubscribed SEED school is determined by random lottery stratified by gender.	**Test Score** = The DC CAS test. **Regression Specification** = OLS regression controlling for student pretest scores, gender, free lunch eligibility, special education status, and English language learner status. **Results** = Winning the lottery had a 0.218σ (0.082) impact on math test scores and a 0.201σ (0.086) impact on reading test scores.
The Prevention, Identification, and Cognitive Determinants of Math Difficulty (Fuchs et al., 2005). N schools = 10, N classrooms = 41, N students = 127, Grade = 1. **Treatment Groups** = Treatment group received math tutoring in addition to normal class time. The control group continued with their normal curricula. Sample drawn from students deemed at-risk of developing mathematics difficulty.	**Treatment Defined** = Students selected for treatment received extra math tutoring immediately following their normal mathematics instruction. Tutoring occurred in small groups three times per week for 16 weeks in addition to regular class time. The purpose of this tutoring was to curb mathematics difficulty before it developed. **Randomization** = Students were stratified by classroom and assigned at random for treatment.	**Test Score** = Woodcock-Johnson III: Applied Problems and Computation subtests. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. The average effect across both subtests is reported. **Results** = Treatment had a 0.300σ (0.179) impact on math test scores.
The Reading Connection: A Leadership Initiative Designed to Change the Delivery of Educational Services to At-Risk Children (Compton, 1992). N students = 483, Grade = 1, Location = Kalamazoo, MI. **Treatment Groups** = Treatment group implemented the *Reading Connection* program. Control group maintained traditional remedial reading services. Sample drawn from students already enrolled in small-group remedial reading instruction.	**Treatment Defined** = The *Reading Connection* program is an early intervention designed to curb reading difficulty before it leads to persistent failure in school. The intervention entailed individual tutoring for 30 minutes per day, five days per week, for 12 - 16 weeks. The goal of this tutoring was to both build reading fluency and develop self-monitoring skills so that students can assess and resolve their own difficulties in the future. **Randomization** = Students were assigned at random to treatment.	**Test Score** = The Iowa Test of Basic Skills. **Regression Specification** = Effect sizes were calculated using posttest means. **Results** = Treatment had a 0.216σ (0.092) impact on reading test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
Transfer Incentives for High-Performing Teachers: Final Results from a Multisite Randomized Experiment (Glazerman et al., 2013). N states = 7, N districts = 10, N students ≈ 7,000. **Treatment Groups** = Treatment students were taught by high-quality teachers who filled vacancies through a transfer incentive program. Teaching vacancies in control schools were filled as usual, without incentives.	**Treatment Defined** = Talent Transfer Initiative (TTI) allows for the highest-performing teachers in each district to receive a pay raise to move into schools serving the most disadvantaged students. Teachers ranking in roughly the top 20 percent within their subject and grade were offered $20,000 if they transferred and remained in a set of designated schools. **Randomization** = Researchers first identified low-achieving schools that had a vacancy within a teaching team. Whenever possible, schools were matched within district based on the grade and subject of the vacancy and student demographics. Within these matched blocks, schools were randomly assigned to either treatment or control status.	**Test Score** = Math and reading state tests. **Regression Specification** = Regression model controlling for pretest scores, race/ethnicity, gender, English Language Learner status, special education status, free/reduced-price lunch status, over-age-for-grade status, an indicator of whether the student belonged to a study team that had at least one retention-stipend teacher, grade dummies, and block dummies. **Results** = Treatment had a 0.120σ (0.070) impact on math test scores and a 0.058σ (0.050) impact on reading test scores.
Using Knowledge of Children's Mathematics Thinking in Classroom Teaching: An Experimental Study (Carpenter et al., 1989). N schools = 24, N teachers = 40, N students ≈ 480, Grade = 1, Location = Madison, WI. **Treatment Groups** = Treatment group attended a 4-week workshop; control group attended two 2-hour workshops.	**Treatment Defined** = The treatment workshop helped teachers learn how children develop addition/subtraction skills, focusing on cognitive processes applied to different word problems. Teachers then developed strategies to build math skills utilizing these processes. The workshop met five hours a day, four days a week for the first four weeks of the teachers' summer vacation. Control workshops focused on nonroutine problem solving. **Randomization** = Stratified by school, teachers were randomly assigned to treatment or control. Six male and six female students were randomly selected from each class for the analysis. In two instances, there were less than 12 total students in a class, in which case the entire class was used.	**Test Score** = Iowa Test of Basic Skills-Level 7: Computation and Mathematics Problems subtests. **Regression Specification** = Effect sizes were calculated for the two subtests using means adjusted for pretest scores. We report the average effect size on these two tests. **Results** = Treatment had a 0.396σ (0.319) impact on math test scores.

Appendix Table 3 (continued)

Study	Study Design	Results
When Less May Be More: A 2 Year Longitudinal Evaluation of a Volunteer Tutoring Program Requiring Minimal Training (Baker et al., 2000). N students = 84, Grades = 1 - 2. **Treatment Groups** = Students in the treatment group received one-on-one tutoring and the control group received normal classroom instruction.	**Treatment Defined** = Tutors received a 1-2 hour training session either at the beginning of the school year or anytime throughout. Treatment group students attend tutoring sessions for 30 minutes twice a week during the school year. Students were tested at the beginning of first grade, the end of first grade, and the end of second grade. **Randomization** = Eligible students were referred by teachers. They were students with poor reading skills and little academic experiences with adults at home or otherwise. Eligible students were randomly assigned to treatment and control groups through a process called Rapid Letter Naming.	**Test Score** = Woodcock Reading Mastery Tests-Revised: Word Identification, Word Comprehension and Passage Comprehension subtests. **Regression Specification** = Effect sizes were calculated using posttest means adjusted for pretest scores. We report the average annual impact across all outcome measures. **Results** = Treatment had a 0.184σ (0.156) impact on reading test scores.
When Schools Stay Open Late: The National Evaluation of the 21st Century Community Learning Centers Program (James-Burdumy et al., 2005). N districts = 12, N centers = 26, N students = 2,308. **Treatment Groups** = Treatment students were able to enroll in 21st Century after-school centers. Control students were unable to enroll in these centers for two years after randomization.	**Treatment Defined** = 21st Century Community Learning Centers ran an after-school program for treatment students. Centers offered homework sessions, academic activities, and enrichment activities. **Randomization** = Random assignment was conducted at the center level. Applicants to each of the centers were randomized into treatment and control. For seven sites, random assignment took place at the beginning of the 2000-2001 school year and for the other five sites, random assignment took place at the beginning of the 2001-2002 school year.	**Test Score** = Stanford Achievement Test. **Regression Specification** = Regression model controlling for student characteristics. **Results** = Treatment had a -0.021σ (0.046) impact on math test scores and a 0.008σ (0.046) impact on reading test scores.

Appendix Table 4 - Curriculum

Study	Study Design	Results
A Mixed-Method Multi-Level Randomized Evaluation of the Implementation and Impact of an Audio-Assisted Reading Program for Struggling Readers (Lesnick, 2006). N districts = 2, N schools = 9, N classrooms = 59, N students = 233, Grades = 3 and 5. **Treatment Groups** = Treatment classrooms implemented the *New Heights* reading intervention. Control classrooms continued with their normal curricula. Sample drawn from students who were at least nine months below grade level in reading, as determined by the pretest.	**Treatment Defined** = The *New Heights* program builds reading fluency via repeated reading strategies with audio assistance. Students select their own book from the interventions' reading list, the teacher gives a brief introduction to introduce new vocabulary or skills, and then the student reads through the text once. Students then elect to re-read the book with or without the audio assistance, complete a worksheet connected to the text, or conference with the teacher. The intervention lasted approximately 20 to 30 minutes per day, five days per week for eighteen weeks. **Randomization** = Students were sorted into blocks by school, grade, classroom, and gender. Half the students in each block (up to a maximum of six) were assigned randomly to treatment. The remainder from each block were assigned to the control group.	**Test Score** = Dynamic Indicator of Basic Early Literacy Skills; Test of Word Reading Efficiency. **Regression Specification** = MANCOVA analysis controlling for pretest scores, gender, race/ethnicity, free/reduced lunch status, and English-language-learner status. Effect sizes were averaged across outcome measures. **Results** = Treatment had a -0.028σ (0.108) impact on reading test scores.
A Multisite Cluster Randomized Field Trial of Open Court Reading (Borman et al., 2008). N schools = 6, N classrooms = 57, N students = 1,099, Grades = 1 - 5. **Treatment Groups** = Treatment classrooms implemented the *Open Court Reading* (*OCR*) curriculum. Control classrooms continued with their normal curricula.	**Treatment Defined** = The *OCR* curriculum provides textbooks, workbooks, decodable texts, and anthologies to develop reading fluency. The core components of the curriculum include: preparing to read – which builds phonemic awareness, sound and letter familiarity, phonics, fluency, and word knowledge; reading and responding – which builds textual-thinking skills, vocabulary, reading proficiency, as well as comprehension, inquiry, and investigation strategies; and language arts – which develops writing skills, spelling, grammar usage and mechanics, vocabulary, penmanship, and listening. **Randomization** = Classrooms were placed into blocks by school and grade. Within each block, classrooms were randomly assigned to treatment.	**Test Score** = TerraNova Comprehensive Test of Basic Skills V: Reading Comprehension and Vocabulary subtests. **Regression Specification** = Effect sizes were calculated using average postest scores. **Results** = Treatment had a 0.187σ (0.288) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
A Multisite Cluster Randomized Trial of the Effects of CompassLearning Odyssey Math on the Math Achievement of Selected Grade 4 Students in the Mid-Atlantic Region: Final Report (Wijekumar et al., 2009). N schools = 32, N teachers = 122, N students = 2,854, Grade = 4, Locations = DE, NJ, and PA. **Treatment Groups** = Treatment teachers received the CompassLearning Odyssey Math program. Control teachers continued with business as usual.	**Treatment Defined** = Odyssey Math is a computer-based math curriculum developed by CompassLearning Inc. to improve math learning for K–12 students. The software consists of a web-accessed series of learning activities, assessments, and math tools. CompassLearning professional development trainers presented the learning activities, math tools, and assessments as available options to intervention teachers during the summer professional development session. Five days of Odyssey Math professional development were purchased for each treatment teacher, consisting of two large group presentations and three in-class coaching sessions. **Randomization** = A volunteer sample of teachers and their classrooms were randomly assigned to treatment and control conditions within schools.	**Test Score** = TerraNova Basic Battery: Math subtest. **Regression Specification** = Multilevel hierarchical linear model (student, teacher, school) controlling for pretest scores. **Results** = Treatment had a 0.02σ (0.03) impact on math test scores.
A Randomized Experimental Evaluation of the Impact of Accelerated Reader/Reading Renaissance Implementation on Reading Achievement in Grades 3 to 6 (Nunnery et al., 2006). N teachers = 44, N students = 1,023, Grades = 3 - 6. **Treatment Groups** = Treatment teachers incorporated both the *Accelerated Reader* program and the *Reading Renaissance* program. Control teachers maintained their normal curricula.	**Treatment Defined** = The *Accelerated Reader* program is a software-based curriculum, in which students select a book of their choice and complete reading comprehension quizzes. The program identifies weaknesses in reading comprehension and suggests texts to redress these difficulties. The *Reading Renaissance* program is a professional development program, which suggests teachers incorporate 30 to 60 minutes of reading time in-class. It also trains teachers in the appropriate use of the *Accelerated Reader* software. **Randomization** = Teachers were randomly assigned to pairs within grade level, and then one teacher from each pairing was assigned randomly to treatment.	**Test Score** = The STAR Reading test. **Regression Specification** = Effect size was calculated using the average growth between pre and posttest scores. We report the average effect across all grades. **Results** = Treatment had a 0.182σ (0.302) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
A Randomized Field Trial of the Fast ForWord Language Computer-Based Training Program (Borman et al., 2009). N schools = 8, N students = 415, Grades = 2 and 7, Location = Baltimore, MD. **Treatment Groups** = Treatment students received supplemental Fast ForWord program instruction. Control students did not.	**Treatment Defined** = Fast ForWord Language is an adaptive computer program for language instruction. It is designed to build oral language comprehension skills and other critical skills necessary to become a better reader. **Randomization** = Students were deemed eligible for the intervention if they scored below the 50th percentile on the Total Reading outcome for the district administered Comprehensive Test of Basic Skills, Fifth Edition. Eligible students were stratified by school and grade level and assigned randomly to treatment or control.	**Test Score** = Comprehensive Test of Basic Skills V: Language and Reading Comprehension subtests. **Regression Specification** = OLS regression controlling for pretest scores, teacher rating of students' abilities, demographic controls, and school fixed effects. **Results** = Treatment had a 0.088σ (0.168) impact on reading test scores.
A Study on the Effects of Houghton Mifflin Harcourt's Journeys Program: Year 1 Final Report (Resendez and Azin, 2012). N schools = 6, N teachers = 44, N students = 1,046, Grades = K - 2. **Treatment Groups** = Treatment classrooms implemented the Journeys program. Control classrooms continued with their normal curricula.	**Treatment Defined** = The Journeys program is a comprehensive reading and language arts curriculum. The program includes reading, writing, and grammar exercises segmented into thematic units. Weekly lessons focused on one unit for five weeks, creating continuity among lesson content. **Randomization** = Teachers were stratified by school and randomly assigned to treatment.	**Test Score** = Iowa Test of Basic Skills. **Regression Specification** = Three-level hierarchical linear model (changes over time, student, teacher). **Results** = Treatment had a 0.175σ (0.049) impact on reading test scores.
Action Research: Implementing *Connecting Math Concepts* (Snider and Crawford, 1996). N classrooms = 2, N students = 46, Grade = 4. **Treatment Groups** = Treatment classrooms implemented the *Connecting Math Concepts* (CMC) curriculum. Control classrooms continued with their normal curricula.	**Treatment Defined** = The CMC curriculum develops multiple math skills within each lesson, stressing the relationship between skills. Students thus learn to develop complex mathematical procedures with simple math skills. **Randomization** = Students were assigned at random to the treatment or control classrooms.	**Test Score** = The National Achievement Test. **Regression Specification** = Effect size was calculated using the average growth between posttest and pretest scores. **Results** = Treatment had a 0.258σ (0.296) impact on math test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
An Efficacy Study on Scott Foresman's *Reading Street* Program: Year One Report (Wilkerson et al., 2006). N districts = 3, N schools = 5, N teachers = 48, N students = 944, Grades = 1 - 3. **Treatment Groups** = Teachers in the treatment group included the *Reading Street* Program in their curriculum. The control group did not alter their curriculum. No teachers had previously used *Reading Street* materials.	**Treatment Defined** = The *Reading Street* Program provides reading materials and a curriculum designed to develop critical reading skills: phonemic awareness, phonics, vocabulary, comprehension, and fluency. Teachers administer the program for at least 90 minutes in-class. The program also recommends that students needing additional help receive up to 30 minutes of small-group tutoring, and those students who continue to struggle after such intervention receive individualized attention outside of normal class time. **Randomization** = Teachers were stratified by school and grade and then assigned randomly to treatment or control.	**Test Score** = Gates-MacGintie Reading Test, Fourth Edition; Dynamic Indicators of Basic Early Literacy: Oral Reading Fluency subtest. **Regression Specification** = A two-level hierarchical linear model (student, school) controlling for gender, ethnicity, special education status, grade, and school. **Results** = Treatment had a -0.095σ (0.103) impact on reading test scores.
An Evaluation of the Effects of Paired Learning in a Mathematics Computer-Assisted-Instruction Program (Turner, 1985). N teachers = 4, N classrooms = 12, N students = 275, Grades = 3 - 4, Location = Goodyear, AZ. **Treatment Groups** = Classrooms in the first treatment group had students take part in computer-assisted-instruction individually. Classrooms in the second treatment group had students take part in computer-assisted-instruction in pairs. Control classrooms continued with their normal curricula.	**Treatment Defined** = The computer-assisted instruction software presents the material, evaluates the student response, and progressively adjusts the material based on these responses. Instruction lasted for fifteen minutes, three days per week in lieu of normal class time. Students working in pairs helped each other find the correct answer. Pairings were rotated so that, by the end of the experiment, all students in a class had worked together at some point. Students in the individual treatment worked alone and directed questions toward the teacher. **Randomization** = Classrooms were stratified by teacher and assigned at random to one of the three conditions.	**Test Score** = Comprehensive Test of Basic Skills: Mathematics subtests. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = The individual treatment had a 0.278σ (0.711) impact on math scores. The paired treatment had a 0.395σ (0.714) impact on math scores.

Appendix Table 4 (continued)

Study	Study Design	Results
An Experimental Study of the Effects of the Accelerated Reader Program and a Teacher Directed Program on Reading Comprehension and Vocabulary of Fourth and Fifth Grade Students (Knox, 1996). N schools = 1, N students = 77, Grades = 4 - 5. **Treatment Groups** = All students received the same reading list. The treatment group reviewed their books with both the researcher and the Accelerated Reader computer program. The control group reviewed their books with their teachers. No student had previously used the Accelerated Reader program.	**Treatment Defined** = The Accelerated Reader program is designed to turn reading into a game. Upon completion of a book, a computer program tests students on reading comprehension and awards points based on these tests. The program also offers feedback based on these test results. Students select their own books. Rewards were awarded when a student reached a specific point-threshold. **Randomization** = Students were stratified by grade and then paired across classrooms according to their pretest score. One student from each pair was assigned randomly to the treatment group.	**Test Score** = Stanford Achievement Test: Vocabulary and Reading Comprehension subtests. **Regression Specification** = Effect sizes were calculated for each outcome measure using means adjusted for pretest scores. The researchers report the results by grade and subtests. We report the weighted average of the effects for subtests and grades. **Results** = Treatment had a -0.115σ (0.324) impact on reading test scores.
Comparative Effectiveness of Scott Foresman Science: A Report of a Randomized Experiment in Five School Districts (Miller and Jaciw, 2007). N districts = 5, N teachers = 92, N students = 2,638, Grades = 3 - 5. **Treatment Groups** = Treatment teachers used the Scott Foresman Science curriculum. Control teachers continued with their normal curricula.	**Treatment Defined** = Scott Foresman Science is a year-long science curriculum. The curriculum provides materials for both students and teachers aimed at developing independent investigative skills. In addition to science instruction, the curriculum features Leveled Reader. These are student readers designed to provide the teacher with an easy way to differentiate instruction and provide reading support at, below, and above grade level. Treatment teachers were provided a one-half day workshop with the materials for the curriculum. **Randomization** = Volunteer teachers within each district were assigned to treatment or control by coin toss.	**Test Score** = Northwest Evaluation Association Test: Reading achievement subtest. **Regression Specification** = The effect size was calculated using posttest means adjusted for pretest scores. **Results** = The impact of treatment on reading tests was 0.05σ (0.04).

Appendix Table 4 (continued)

Study	Study Design	Results
Computer Assisted Instruction as an Enhancer of Remediation (Hotard and Cortez, 1983). N students = 190, Grades = 3 - 6. **Treatment Groups** = Treatment students incorporated 10 minutes daily of computer-assisted-instruction (CAI) into their curricula. Control classrooms continued with their normal curricula.	**Treatment Defined** = For six months, treatment students received 10 minutes of CAI instruction for mathematics daily in addition to their normal math lab instruction. Each lesson had students solve a variety of problems based on the material currently being taught in class. The software automatically adjusted the difficulty of its problems based on student performance. **Randomization** = Students were matched by their pretest scores and then one student from each pairing was assigned randomly to treatment.	**Test Score** = Comprehensive Test of Basic Skills. **Regression Specification** = Effect size was calculated using the average growth between pre and posttest scores. **Results** = Treatment had a 0.193σ (0.145) impact on math test scores.
Computer-Assisted Instruction to Prevent Early Reading Difficulties in Students at Risk for Dyslexia: Outcomes from Two Instructional Approaches (Torgesen et al., 2009). N students = 112, Grade = 1. **Treatment Groups** = Two treatment conditions: condition one implemented the *Read, Write, and Type* program; condition two implemented the *Lindamood Phoneme Sequencing Program for Reading, Spelling, and Speech*. Control students received no such intervention.	**Treatment Defined** = The interventions were implemented over four, 50-minute sessions per week from October through May. The first half of each lesson was devoted to direct reading instruction from teachers, and the remaining time was devoted to practicing these skills on the computer. Students in condition one received lessons in alphabetic reading skills, while students in condition two received explicit instruction in phonemic awareness. **Randomization** = Students were stratified by school and then randomly assigned to one of the three groups.	**Test Score** = Comprehensive Test of Phonological Processing; Woodcock Reading Mastery Test- Revised: Word Identification, Word Attack, and Passage Comprehension subtests; Test of Word Reading Efficiency: Word Efficiency and Phonemic Decoding subtests. **Regression Specification** = Effect sizes were calculated using posttest means. We report the average impact across all outcome measures. **Results** = The *Read, Write, and Type* program had a 0.459σ (0.238) impact on reading test scores. The *Lindamood Phoneme Sequencing Program for Reading, Spelling, and Speech* program had a 0.702σ (0.240) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Costs, Effects, and Utility of Microcomputer Assisted Instruction (Fletcher et al., 1990). N schools = 1, N classrooms = 4, N students = 60, Grades = 3 and 5, Location = Saskatchewan, Canada. **Treatment Groups** = Treatment classrooms utilized microcomputer assisted mathematics instruction. Control classroom continued with their normal curricula.	**Treatment Defined** = Third-grade treatment students used the *Milliken Math Sequences* software to practice mathematics skills introduced in class for an average of 12 minutes per day, five days per week. Fifth-grade treatment students utilized the software for mathematics practice, drilling up to 15 minutes, four days per week. All computerized mathematics practice took the place of normal class time. **Randomization** = One classroom from each grade was designated the treatment classroom, while the other served as control. Students were assigned at random to the treatment or control classrooms in their grade.	**Test Score** = Canadian Test of Basic Skills. **Regression Specification** = Effect sizes were calculated using posttest scores adjusted for prettest scores. We report the average effect across grades. **Results** = Treatment had a 0.421σ (0.322) impact on math test scores.
Does Rainbow Repeated Reading Add Value to an Intensive Intervention Program for Low-progress Readers? An Experimental Evaluation (Wheldall, 2000). N sites = 2, N students = 40, Grades = 2 - 7, Location = Australia. **Treatment Groups** = All students were participating in an intensive literacy intervention. Treatment students received a supplemental literacy program, the *Rainbow Reading Program*. Control students continued with their normal curricula.	**Treatment Defined** = All students included in the study were classified as low-progress readers and were attending a literacy program focused on repeated reading. The treatment students supplemented this program with the *Rainbow Reading Program* – a repeated reading program where students read along with an audio tape. **Randomization** = Within each site, students were ranked by baseline reading accuracy and were paired with a student of comparable reading ability. One student of each pair was randomly assigned to treatment and the other was assigned to control.	**Test Score** = The Burt Word Reading Test. **Regression Specification** = Effect sizes were calculated using the average growth between pre and posttest scores. **Results** = Treatment had a 0.018σ (0.105) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). N districts = 27, N schools = 105, N teachers = 347, N students = 9,392, Grades = 1, 4, and 6. **Treatment Groups** = The eight treatment groups all used different software products. Teachers in the control group continued using their normal curricula.	**Treatment Defined** = Treatment classrooms were given the following software products: First grade reading: Destination Reading, Waterford Early Reading Program, Headsprout and Plato Focus; Fourth grade reading: LeapTrack and Academy of Reading; Sixth grade math: Larson Pre-Algebra and Achieve Now. **Randomization** = Districts volunteered and identified schools. Then teachers volunteered and were assigned to treatment and control.	**Test Score** = Each district's nationally normed tests. If a district did not administer a standardized test, they used Stanford Achievement Test. Only administered tests to one randomly selected treatment and control classroom per school. **Regression Specification** = Two-level hierarchical model (student, classroom) controlling for pretest scores, age, gender, teachers' years of experience, education level, and school fixed effects. **Results** = The impacts for each software product are as follows: Destination Reading = 0.091σ (0.082); Headsprout = 0.014σ (0.052); Plato Focus = 0.024σ (0.066); Waterford Early Reading Program = 0.020σ (0.068); Academy of Reading = -0.008σ (0.050); LeapTrack = 0.094σ (0.036); Larson Pre-Algebra = 0.113σ (0.076); Achieve Now = -0.028σ (0.069).
Effectiveness of Selected Supplemental Reading Comprehension Interventions: Impacts on a First Cohort of Fifth-Grade Students (James-Burdumy et al., 2009). N districts = 10, N schools = 89, N teachers = 268, N students = 6,350, Grade = 5. **Treatment Groups** = The four treatment groups all used different software products. Control schools continued with their normal curricula.	**Treatment Defined** = The four reading comprehension curricula were Project CRISS, ReadAbout, Read for Real, and Reading for Knowledge. Districts involved in the study were required to have at least 12 Title I schools and schools must not already have been using any of the four curricula. **Randomization** = Schools were randomly assigned to one of the four treatment groups or control group within each district. When possible, schools in a district were blocked into groups with similar pretest scores and then randomization occurred within each block. When blocks were not possible, a Chromy selection procedure was implemented.	**Test Score** = Group Reading Assessment and Diagnostic Evaluation. **Regression Specification** = OLS regression model that controls for student pretest scores, English language learner status, race/ethnicity, teacher race, school urbanicity, and district fixed effects. **Results** = Project CRISS had a -0.04σ (0.04) impact on reading test scores. ReadAbout had a -0.07σ (0.05) impact on reading test scores. Read for Real had a -0.06σ (0.04) impact on reading test scores. Reading for Knowledge had a -0.11σ (0.04) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Effects of Health-Related Physical Education on Academic Achievement: Project SPARK (Sallis et al., 1999). N schools = 7, N students = 759, Grades = 4 - 5, Location = Southern CA. **Treatment Groups** = In the first treatment, specialists conduct physical education programs. In the second treatment, a specialist trains classroom teachers to conduct physical education classes. The control group continues with its current physical education program. All participating schools did not employ physical education specialists prior to the study.	**Treatment Defined** = Physical education classes were administered to fourth graders three days a week throughout the school year; classes were designed to promote independent physical activity via weekly fitness goals and family involvement. The classes continued through fifth grade. **Randomization** = Schools were stratified by the percentage of ethnic minority students and then randomly assigned into one of the three groups.	**Test Score** = Metropolitan Achievement Tests: Language and Reading subtests. **Regression Specification** = Effect sizes were calculated using the average growth between pre and posttest scores. We report the average effect across subtests. We only report results from the first cohort because there appears to be an error in the results reported for the second cohort in the published article. **Results** = The specialist treatment had a -0.006σ (0.456) impact on math scores and a 0.101σ (0.458) impact on reading scores. The trained teacher treatment had a 0.000σ (0.456) impact on math scores and a 0.103σ (0.458) impact on reading scores.
Effects of Targeted Intervention on Early Literacy Skills of At-Risk Students (Wang and Algozzine, 2008). N schools = 6, N students = 139, Grade = 1. **Treatment Groups** = Treatment students replaced their remedial reading instruction with a targeted literacy intervention. Control students continued with their normal remedial instruction. Sample drawn from students at risk of persistent reading failure, as determined by the pretest.	**Treatment Defined** = The authors developed their own intervention targeted at the following skills: phonemic awareness, letter-sound correspondence, reading phonetically, fluency building, and sight-word practice. Instruction lasted approximately 10 to 15 minutes daily in-class. **Randomization** = Randomly selected two schools to serve as control schools.	**Test Score** = Woodcock Reading Mastery Test-Revised: Word Identification, Word Attack, and Passage Comprehension subtests; Dynamic Indicators of Basic Early Literacy Skills: Phoneme Segmentation Fluency and Nonsense Word Fluency subtests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between posttest and pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 0.303σ (0.873) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Efficacy of Collaborative Strategic Reading with Middle School Students (Vaughn et al., 2011). N recruitment sites = 2, N schools = 6, N teachers = 17, N classrooms = 61, N students = 866, Grades = 7 - 8. **Treatment Groups** = Treatment classrooms implemented the *Collaborative Strategic Reading* (*CSR*) intervention. Control classrooms did not implement such an intervention.	**Treatment defined** = The *CSR* intervention teaches students to reflect upon their comprehension of the text. The intervention follows a set strategy: first, students reviewed the topic of the text prior to reading; second, students read the text, noting where they had difficulty; third, students assessed how they were able to overcome those difficulties; fourth, students worked in small groups to discuss the text and any unresolved difficulties. The intervention took place in class for 50 minutes per day, twice weekly, over 18 weeks. **Randomization** = Students were randomly assigned to classes, and classes were stratified by teacher and randomly assigned to treatment.	**Test Score** = The Gates-MacGintie Reading Test. **Regression Specification** = Effect size was calculated using the average growth between pre and posttest scores. **Results** = Treatment had a 0.073σ (0.072) effect on reading test scores.
Empirical Evaluation of *Read Naturally* Effects (Christ and Davie, 2009). N districts = 4, N schools = 6, N students = 109, Grade = 3. **Treatment Groups** = The treatment group used *Read Naturally* software for 30 minutes daily in-class. The control group engaged in non-reading activities during the same time. No school had previously used the *Read Naturally* program.	**Treatment Defined** = The *Read Naturally* program utilizes computer software to build reading fluency via the following three strategies: repeated reading, reading with a model, and progress monitoring with feedback. The program also builds vocabulary via reading-for-meaning strategies. **Randomization** = To be eligible for the study, students had to fall below the 40th percentile on both a standardized test of oral fluency (either the Dynamic Indicators of Basic Early Literacy Skills or the AIMSweb Test of Early Literacy) and a measure of reading comprehension (Measures of Academic Progress) administered at the end of second grade. Eligible students who agreed to participate were stratified by classroom and assigned at random to the treatment group.	**Test Score** = Dynamic Indicators of Basic Early Literacy Skills; Test of Word Reading Efficiency; Gray Oral Reading Tests IV: Reading Fluency and Accuracy measures; Woodcock Reading Mastery Test-Revised: Word Identification subtest . **Regression Specification** = Effect sizes were calculated for each outcome measure using posttest means adjusted for pretest covariates. We report the average effect across all outcome measures. **Results** = Treatment had a 0.248σ (0.197) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Evaluation Research on the Effectiveness of *Fluency Formula*: Final Report (Sivin-Kachala and Bialo, 2005). N districts = 2, N classrooms = 12, N students = 128, Grade = 2. **Treatment Groups** = The treatment group implemented the *Fluency Formula* reading curriculum. The control group maintained their normal classroom curricula. No classrooms had access to *Fluency Formula* materials beforehand.	**Treatment Defined** = *Fluency Formula* builds oral fluency by focusing on the following units: partner reading, choral reading, expressive reading, reading theater, repeated reading, and expert reading. In-class instruction lasts for at least 15 minutes per day, five days per week; 15-minute take-home assignments are also given once per week. Students requiring additional instruction receive small-group tutoring in-class for at least 15 minutes. **Randomization** = Numerous teachers from each district volunteered for the study; researchers matched pairs of classrooms with similar reading ability, ethnic composition, and teacher characteristics. One classroom from each pair was randomly assigned to treatment.	**Test Score** = Woodcock-Johnson III: Passage Comprehension subtest. **Regression Specification** = The effect size was calculated using the growth between pre and posttest means. **Results** = Treatment had a -0.271σ (0.178) impact on reading test scores.
Fostering the Development of Vocabulary Knowledge and Reading Comprehension Through Contextually-Based Multiple Meaning Vocabulary Instruction (Nelson and Stage, 2007). N classrooms = 16, N students = 283, Grades = 3 and 5. **Treatment Groups** = Treatment classrooms altered their curricula to include contextual vocabulary instruction. Control classrooms continued with their normal curricula.	**Treatment Defined** = Treatment classrooms incorporated into their curricula contextually-based multiple meaning vocabulary instruction, which taught students to derive the meanings of words from a given context. The treatment was designed to boost vocabulary and reading comprehension. Instruction lasted approximately 20 - 30 minutes per day during each school day. **Randomization** = Classrooms were stratified by grade and then randomly assigned for treatment.	**Test Score** = Gates-MacGintie Reading Tests IV: Vocabulary and Reading Comprehension subtests. **Regression Specification** = Effect sizes were calculated for each outcome measure using the growth between pre and posttest means. We report the average effect across subtests. **Results** = The treatment had a 0.205σ (0.502) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Impact of Thinking Reader Software Program on Grade 6 Reading Vocabulary, Comprehension, Strategies, and Motivation (Drummond et al., 2011). N schools = 32, N teachers = 92, N students = 2,140, Grade = 6, Locations = CT, MA, and RI. **Treatment Groups** = Treatment teachers received three Thinking Reader digital novels to read with their students and participated in professional development to learn how to use the software. Control teachers used the schools' regular curricula.	**Treatment Defined** = Thinking Reader is software in which students read novels on computers and respond to prompts from animated coaches. Treatment teachers received two, 6-hour training sessions in the fall and three individual coaching sessions throughout the school year (≈8 hours). **Randomization** = Within each school, sixth grade reading/ELA teachers were randomly assigned to treatment or control.	**Test Score** = Gates-MacGintie Reading Tests: Vocabulary and Reading Comprehension subtests. **Regression Specification** = Multilevel hierarchical linear model (student, teacher, school) controlling for students' pretest scores, English language learner status, special education status, teachers' education and years of experience, school poverty, and school size. **Results** = Treatment had a -0.005σ (0.053) impact on reading test scores.
Improving Reading Comprehension and Social Studies Knowledge in Middle School (Vaughn et al., 2013). N schools = 2, N teachers = 5, N classes = 27, N students = 419, Grade = 8. **Treatment Groups** = Treatment classrooms adopted a Promoting Acceleration of Comprehension and Content Through Text (PACT) model. Control classrooms continued with their normal curricula.	**Treatment Defined** = PACT is a program designed to improve text comprehension and content learning. The model has 5 key components: 1) A comprehension canopy that contains a motivational springboard and an overarching issue or question, 2) essential words or key vocabulary related to the unit, 3) knowledge acquisition (appropriate text-based instruction and reading) 4) team-based learning comprehension check, and 5) team-based learning knowledge application. Students in an intervention class received instruction during their regularly scheduled social studies classes. Teachers implemented PACT instruction for 30 full class periods (six to eight weeks). **Randomization** = In participating schools, students were first randomly assigned to classes. Classes were then randomly assigned to treatment or control.	**Test Score** = Gates-MacGinitie: Reading Comprehension subtest. **Regression Specification** = A multilevel, multiple-group structural equation model was used to create latent estimates for student outcomes. The effect size was calculated using the constructed latent estimates for each group and their corresponding variances. **Results** = Treatment had a 0.195σ (0.077) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Improving Reading Fluency and Comprehension in Elementary Students Using Read Naturally (Arvans, 2009). N schools = 1, N students = 82, Grades = 2 - 4. **Treatment Groups** = Treatment students utilized the *Read Naturally* software. Control students received no such intervention. Sample drawn from students in need of additional reading help as determined by the pretest.	**Treatment Defined** = Treatment students used the *Read Naturally* software for 30 to 45 minutes daily, five days per week, for eight weeks. The software offered children their choice of 12 texts. The software offered pronunciation and vocabulary help as needed. Students could move on to the next story only if they independently read the story out loud with no more than three errors. **Randomization** = Students were paired by race, grade, gender, and pretest score and then one student from each pairing was randomly assigned to treatment.	**Test Score** = Dynamic Indicators of Basic Early Literacy Skills: Oral Reading Fluency subtest; Expressive Vocabulary Tests; Peabody Picture Vocabulary Test; and the cognitive and achievement batteries of the Woodcock-Johnson III. **Regression Specification** = Effect sizes were calculated using the average growth between pre and posttest scores. We report the average effect across all outcomes. **Results** = Treatment had a 0.096σ (0.221) impact on reading test scores.
Individualizing a Web-Based Structure Strategy Intervention for Fifth Graders' Comprehension of Nonfiction (Meyer et al., 2011). N schools = 2, N students = 131, Grade = 5, Location = PA. **Treatment Groups** = All students used the Intelligent Tutor Structure Strategy (ITSS) computer software. Treatment students received individual lessons from the program. Control students utilized the program's normal curriculum.	**Treatment Defined** = The ITSS software builds reading comprehension by reading passages of increasing difficulty with students before asking them to read by themselves. The program aims to build familiarity with the following text structures: comparison, problem-and-solution, cause-and-effect, sequence, and description. Students in the treatment condition received remediation or enrichment lessons depending on their demonstrated proficiency. Remediation lessons had students read texts of similar or easier complexity, while enrichment lessons had students read the most complex text suited to their ability. The intervention occurred in-class over three, 30-minute blocks per week. **Randomization** = Students were stratified by pretest scores and elementary school and then randomly assigned to treatment.	**Test Score** = The Gray Silent Reading Test. **Regression Specification** = The effect size was calculated using the average growth between pre and posttest scores. **Results** = Treatment had a 0.266σ (0.265) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Large-Scale Randomized Controlled Trial with 4th Graders Using Intelligent Tutoring of the Structure Strategy to Improve Nonfiction Reading Comprehension (Wijekumar et al., 2012). N teachers = 130, N students = 2,643, Grades = 4. **Treatment Groups** = Treatment teachers had access to professional development and a web-based intelligent tutoring system (ITSS). Control teachers continued with normal curricula.	**Treatment Defined** = Treatment group received the structure strategy though ITSS. Structure strategy is an approach to improving reading comprehension that focuses on common patterns used by writers to organize texts and organize main ideas. ITSS was designed to deliver instruction within existing ELA curriculum for one class period a week and provide one on one tutoring. **Randomization** = Classrooms were randomly assigned to treatment and control within each school. If a school did not have enough classrooms, schools with similar characteristics were grouped and then randomized within that group. All schools volunteered to participate.	**Test Score** = The Gray Silent Reading Test. **Regression Specification** = Multilevel hierarchical linear model (student, classroom, school) controlling for pretest scores, gender, and school locale. **Results** = Treatment had a 0.10σ (0.06) impact on reading test scores.
National Assessment of Title I Interim Report: Volume II: Closing the Reading Gap: First Year Findings from a Randomized Trial of Four Reading Interventions for Striving Readers (Torgesen et al., 2006). N districts = 27, N schools = 50, N students = 772, Grades = 3 and 5. **Treatment Groups** = Four treatment groups: group one implemented the *Corrective Reading* program, group two implemented the *Failure Free Reading* program, group three implemented the *Spell Read P.A.T.* program, and group four implemented the *Wilson Reading* program. The control group continued with their normal curricula. Sample drawn from students at-risk of developing reading difficulty as determined by pretest scores.	**Treatment defined** = All interventions took place for 50 minutes, five days per week, in groups of three students. The intervention replaced normal reading instruction. The *Spell Read* program builds phonemic awareness via specific sound tasks as well as reading and writing activities. The *Corrective Reading* program uses scripted lessons and rapid exercises to build word identification and fluency. The *Wilson Reading* program uses direct, multi-sensory, structured reading to build understanding of the structure of language. The *Failure Free* program builds vocabulary through a combination of computer-based lessons, workbook exercises, and teacher-led instruction. **Randomization** = Schools were sorted into blocks based on the percentage of students eligible for free/reduced lunch. Schools were then stratified by block and, within each block, were randomly assigned to one of the four treatment conditions. Within each school, students were randomly assigned to treatment or control.	**Test Score** = Woodcock Reading Mastery Test-Revised: Word Attack, Word Identification, and Passage Comprehension subtests; Test of Word Reading Efficiency: Phonemic Decoding and Sight Word subtests; The Group Reading and Diagnostic Evaluation. **Regression Specification** = Two-level hierarchical linear model (student, school) controlling for grade, pretest scores, and block. **Results** = The *Corrective Reading* program had a 0.148σ (0.148) impact on reading test scores. The *Failure Free Reading* program had a 0.048σ (0.137) impact on reading test scores. The *Spell Read P.A.T.* program had a 0.179σ (0.137) impact on reading test scores. The *Wilson Reading* program had a 0.176σ (0.147) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Reading and Language Outcomes of a Multiyear Randomized Evaluation of Transitional Bilingual Education (Slavin et al., 2011). N districts = 6, N schools = 6, N students = 482, Grade = K. **Treatment Groups** = Treatment students participated in a transitional bilingual education program (TBE). Control students utilized a Structured English Immersion (SEI) program. Sample composed entirely of students who were Spanish dominant as determined by the pretest.	**Treatment Defined** = Students in the TBE condition received reading instruction in Spanish for their Kindergarten year. The intervention focused on developing knowledge of the letter sounds, phonics, vocabulary, and concepts of the Spanish language. Most treatment students transitioned to English-language reading classes in second grade. Control utilized the same curriculum as treatment, however all classes were taught in English. All students received regular ESL instruction. **Randomization** = Students were stratified by school and randomly assigned to treatment.	**Test Score** = Peabody Picture Vocabulary Test; Woodcock-Johnson: Word Identification, Word Attack, and Passage Comprehension subtests. **Regression Specification** = Effect sizes were calculated using posttest means adjusted for pretest scores. We report the average annual impact across all subtests. **Results** = Treatment had a -0.046σ (0.070) impact on reading test scores.
Segmentation / Spelling Instruction as Part of a First-Grade Reading Program: Effects on Several Measures of Reading (Uhry and Shepherd, 1993). N classrooms = 2, N students = 22, Grade = 1, Location = New York City, NY. **Treatment Groups** = The treatment classroom incorporated phonetic segmentation and spelling techniques into their curriculum. The control classroom retained a reading-based curriculum. Sample drawn from predominately white, middle class, and college educated families.	**Treatment Defined** = All classrooms in the study used the same vocabulary lists. Treatment students were trained to spell phonetically via a series of segmentation, dictation, and computer exercises. The intervention took place over two 20-minute sessions per week from October to May in place of normal reading instruction. **Randomization** = Students were stratified by gender and reading ability as determined by the teacher, then randomly assigned to treatment.	**Test Score** = Woodcock Reading Mastery Tests: Word Attack and Word Identification subtests; the Gray Oral Reading Tests. **Regression Specification** = For each outcome measure, effect sizes were calculated using the average growth between postest and pretest scores. We report the average effect across all outcome measures. **Results** = Treatment had a 1.413σ (0.594) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Spatial Temporal Mathematics at Scale: An Innovative and Fully Developed Paradigm to Boost Math Achievement Among All Learners (Rutherford et al., 2010). N schools = 34, Grades = 2 - 5, Location = CA. **Treatment Groups** = Treatment group implemented the *Spatial Temporal Math* (*ST Math*) curriculum. The control group continued their regular curricula. No schools had previous experience with the *ST Math* curriculum.	**Treatment defined** = The *ST Math* curriculum utilizes software to present mathematical concepts through a series of pictures and games. The goal of this curriculum is to develop spatial reasoning skills and problem solving techniques. The program was administered for 45 minutes in class twice weekly. **Randomization** = To be eligible for the study, schools had to be in the lowest three deciles of the Academic Performance Index - a weighted composite of student standardized test scores. Eligible schools who applied to participate were assigned at random to one of two groups: one implemented treatment for second and third graders, the other implemented treatment for fourth and fifth graders.	**Test Score** = California Standards Test: Math subtest. **Regression Specification** = OLS regression controlling for grade, percent of English Language Learners in each school, percent of students on free/reduced lunch, and the mean test scores of the same grade from the previous year. **Results** = Treatment had a 0.290σ (0.140) impact on math test scores.
Teaching Children to Become Fluent and Automatic Readers (Kuhn et al., 2006). N schools = 8, N classrooms = 24, N students = 396, Grade = 2. **Treatment Groups** = Two treatment groups: one implemented a repeated-reading curriculum; the other implemented a wide-reading curriculum. The control group continued its normal curricula.	**Treatment Defined** = In schools implementing a repeated-reading treatment, teachers introduced and discussed a text in class at the start of the week. Students then read the same text approximately four to seven times throughout the week between class time and homework. In schools implementing a wide-reading curriculum, teachers introduced and discussed approximately three texts per week, with students re-reading the texts approximately twice between class time and homework. **Randomization** = Schools were randomly assigned to one of the three groups.	**Test Score** = Test of Word Reading Efficiency: Significant Word Efficiency subtest; the Gray Oral Reading Test; Wechsler Individual Achievement Test: Reading Comprehension subtest. **Regression Specification** = Effect sizes were calculated for each outcome measure using posttest means. We report the effect across all outcome measures. **Results** =The repeated-reading curriculum treatment had a 0.145σ (0.501) impact on reading test scores. The wide-reading curriculum treatment had a 0.163σ (0.501) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Technology's Edge: The Educational Benefits of Computer-Aided Instruction (Barrow et al., 2009). N districts = 3, N schools = 17, N students = 3,451. **Treatment Groups** = Treatment classrooms used computer-aided instruction. Control classrooms continued with traditional curricula.	**Treatment Defined** = The study uses a group of computer programs known as I Can Learn. The system is comprised of a software computer package that is designed to deliver instruction through technology on a one-on-one basis to every student. The curricula is designed to meet the National Council of Teachers of Mathematics standards as well as each individual district's course objectives for pre-algebra and/or algebra. In addition, the software package also includes a classroom management tool for educators, and the company provides on-site support for administrators and teachers. **Randomization** = All pre-algebra and algebra classes in participating schools were grouped into 60 randomization pools. These pools were defined within a school and typically represent a class period. Within each pool, classes were randomly assigned to treatment and control groups.	**Test score** = State math tests. **Regression Specification** = OLS regressions controlling for pretest scores, randomization pools, and demographic characteristics. **Results** = Treatment had a 0.137σ (0.111) impact on math test scores.
The Effect of Computer Assisted Instruction in Improving Mathematics Performance of Low Achieving Ninth Grade Students (Bailey, 1991). N schools = 1, N teachers = 4, N classes = 4, N students = 46, Grade = 9, Location = Urban high school in Hampton, VA. **Treatment Groups** = Treatment students were taught the standard curriculum augmented with computer-assisted instruction in their math class. Control students were taught the standard curriculum.	**Treatment Defined** = Computer-assisted instruction is a method of using computers as a tool to present individualized instructional material. Students in the treatment group used the same textbook as control students. Treatment students also used software that assisted them in learning mathematics skills of concepts, computations, and problem solving. **Randomization** = The sample of students was identified from students scoring below the 30th percentile on the Iowa Test of Basic Skills mathematics subtest in the previous year and receiving a D or F in their 8th grade mathematics course. Consent forms were sent to the parents of eligible students. Students who returned the consent form were randomly assigned to treatment or control.	**Test Score** = Test of Achievement and Proficiency: Math subtest. **Regression Specification** = Effect size was calculated using the average growth between posttest and pretest scores. **Results** = Treatment had a 0.728σ (0.304) impact on math test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
The Effect of Second-Language Instruction on the Reading Proficiency and General School Achievement of Primary-Grade Children (Potts, 1967). N classrooms = 4, N students = 80, Grade = 1 and 2, Location = NY. **Treatment Groups** = Treatment students received instruction in French, control students did not.	**Treatment Defined** = The treatment group received French instruction by the audio-lingual method for 15 minutes daily over the course of the school year. The control group was given dance instruction during this time period. **Randomization** = Students (stratified by gender) and teachers were randomly assigned to the classrooms. Each classroom was then randomly divided in half. Half of the class was assigned to treatment and the other half was assigned to control.	**Test Score** = California Achievement Test. **Regression Specification** = Effect size was calculated using the posttest means adjusted for language mental age and nonlanguage mental age as found on the California Test of Mental Maturity. **Results** = Treatment had a 0.04σ (0.22) impact on reading test scores.
The Effectiveness of a Program to Accelerate Vocabulary Development in Kindergarten (VOCAB) (Goodson et al., 2010). N districts = 35, N schools = 65, N teachers = 130, N students = 1,319, Grade = K, Location = Mississippi Delta region. **Treatment Groups** = Treatment students participated in K-PAVE, a kindergarten curriculum designed to enhance students' vocabulary knowledge. Control students continued with their normal curricula.	**Treatment Defined** = K-PAVE is built around three components that support the acquisition of vocabulary in young students: (1) instruction on a large set of thematically related target words; (2) interactive book reading to build vocabulary and comprehension skills; and (3) adult-child conversations to build vocabulary and oral language skills. The K-PAVE program was designed as a 24 week supplement to the core language arts program used in each school. **Randomization** = Participating schools were placed in three blocks based on previous participation in reading initiatives. Within the blocks, schools were matched on school performance classification, percentage of free or reduced-price meal students, percentage of African American students, locale, and region. After being matched, schools were randomly assigned to treatment or control. Furthermore, the researchers randomly selected two consenting kindergarten teachers from treatment schools to collect data from.	**Test Score** = The Expressive Vocabulary Test-2. **Regression Specification** = Researchers used a three-level linear hierarchical model (student, teacher, school) controlling for student and school baseline demographics. **Results** = Treatment had a 0.141σ (0.052) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
The Effectiveness of Computer Assisted Instruction of Chapter I Students in Secondary Schools (Davidson, 1985). N schools = 1, N students = 67, Grades = 9 - 12. **Treatment Groups** = Treatment classes implemented a computer-assisted learning intervention. Control classrooms received no such intervention. Sample drawn from Chapter I students – those who failed to achieve 80 percent of grade-level objectives on local or state standardized tests.	**Treatment Defined** = Treatment students completed mathematics practice problems on a computer for at least 20 minutes per day for 13 weeks. This intervention used time that would otherwise have been allocated to in-class mathematics exercises. **Randomization** = Students were sorted into five classes by school administrators and then classes were randomly assigned to treatment.	**Test Score** = Metropolitan Achievement Test Battery: Mathematics Instructional subtest. **Regression Specification** = Effect size was calculated using posttest means adjusted for pretest scores. **Results** = Treatment had a 0.121σ (1.119) impact on math test scores.
The Effects of Computer Assisted Instruction as a Supplement to Classroom Instruction in Reading Comprehension and Arithmetic (Easterling, 1982). N schools = 3, N students = 72, Grade = 5. **Treatment Groups** = Two treatment conditions: condition one students utilized the *SRA Computer Drill and Instruction: Mathematics* program; condition two utilized the *MicroSystem80* reading program. The control group continued with their normal curricula.	**Treatment Defined** =The *SRA* software offers practice with immediate feedback in basic arithmetic skills. The *MicroSystem80* software introduces students to the basic rules of logic and drills students on critical reasoning skills. Treatment students worked for a total of four hours on the computer in addition to normal class time. **Randomization** = Six boys and six girls were randomly selected from each school. These were matched to another student in the same school on the basis of gender and pretest scores. In two randomly selected schools, pairings were stratified by gender and assigned at random to one of the two treatments. Pairings from the third school served as the control group.	**Test Score** = California Achievement Test. **Regression Specification** = Effect sizes were calculated using average growth between pre and posttest scores. **Results** = The math instruction treatment had a 0.318σ (0.301) impact on math test scores and a -0.099σ (0.299) impact on reading test scores. The reading instruction treatment had a 0.179σ (0.299) impact on math test scores and a -0.010σ (0.299) impact on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
The Enhanced Reading Opportunities (ERO) Study Final Report: The Impact of Supplemental Literacy Courses for Struggling Ninth-grade Readers (Somers et al., 2010). N districts = 10, N schools = 34, N students = 5,595, Grade = 9. **Treatment Groups** = Treatment one students participated in the Reading Apprenticeship Academic Literacy (RAAL) intervention. Treatment two students participated in the Xtreme Reading intervention. Control students remained in regularly scheduled elective classes.	**Treatment Defined** = The goal of both of the reading interventions is to help struggling adolescent readers develop the strategies and routines used by proficient readers. To do so, each program supports instruction in the following areas: (1) student motivation and engagement; (2) reading fluency; (3) vocabulary; (4) comprehension; (5) phonics and phonemic awareness; and (6) writing. **Randomization** = Within each district, high schools were randomly assigned to use one of the two supplemental literacy programs. Eligible students within each of the participating high schools were randomly assigned either to enroll in the ERO class or to take one of their school's regularly offered elective classes.	**Test Score** = State test scores. **Regression Specification** = OLS regressions controlling for students' baseline test scores, whether students were overage at the start of ninth grade, and randomization block fixed-effects. **Results** = The ERO program had a 0.07σ (0.035) impact on math test scores and a 0.11σ (0.037) impact on reading test scores.
The Impact of Challenging Geometry and Measurement Units on Achievement of Grade 2 Students (Gavin et al., 2013). N schools = 11, N teachers = 24, N students = 380, Grade = 2, Location = CT, KY, SC, and TX. **Treatment Groups** = Treatment teachers implemented Project M^2 in their classrooms. Control teachers did not.	**Treatment Defined** = Project M^2 gave treatment students challenging geometry and measurement units. The purpose is to help primary students learn more complex geometry and measurement concepts in depth. Teachers in treatment group attended a 4-day summer institute and received an additional day of professional development prior to the implementation of each unit. This was a three year intervention. **Randomization** = Participants were recruited from both lower and higher socioeconomic districts. Teachers were stratified by school and randomly assigned to either treatment or control.	**Test Score** = Iowa Test of Basic Skills: Mathematics Concepts subtest. **Regression Specification** = Two-level hierarchical linear model (student, class) controlling for pretest scores. **Results** = Treatment had an impact of 0.071σ (0.112) on math test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
The Impact of Collaborative Strategic Reading on the Reading Comprehension of Grade 5 Students in Linguistically Diverse Schools (Hitchcock et al., 2011). N districts = 5, N schools = 26, N students = 1,355, Grade = 5, Location = OK and TX. **Treatment Groups** = Treatment students participated in Collaborative Strategic Reading (CSR). Control students continued with their normal curricula.	**Treatment Defined** = CSR is a set of instructional strategies designed to improve the reading comprehension of students with diverse abilities. Teachers implement CSR at the classroom level using scaffolded instruction to guide students in the independent use of four comprehension strategies; students apply the strategies to informational text while working in small cooperative learning groups. The goals are to improve reading comprehension and conceptual learning so that academic performance also improves. Treatment teachers received a two day training session on CSR. **Randomization** = Classrooms in participating schools were randomly assigned to CSR treatment or control.	**Test Score** = The Group Reading Assessment and Diagnostic Evaluation. **Regression Specification** = Two-level hierarchical linear model (student, classroom) controlling for student-level pretest scores, English language learner status, teachers' Spanish fluency, and school fixed-effects. **Results** = Treatment had an impact of 0.05σ (0.03) on reading test scores.
The Missouri Mathematics Effectiveness Project: An Experimental Study in Fourth-Grade Classrooms (Good and Grouws, 1979). N schools = 27, N teachers = 40, Grade = 4, Location = Tulsa Public School system. **Treatment Groups** = Treatment teachers used a new mathematics curriculum. Control teachers continued with their normal curricula.	**Treatment Defined** = Treatment teachers used a mathematics curriculum that the researchers found successful in a previous correlational study. The curriculum emphasized student practice and teacher presentations through a daily teaching routine the teachers were expected to follow. Treatment teachers attended two 90-minute training sessions and received a curriculum manual that they were instructed to read before the start of the experiment. Control teachers received similar training and materials after the completion of the experiment. **Randomization** = Schools were matched by student socioeconomic status and then one school from each pair was randomly assigned to treatment and the other to control.	**Test Score** = Science Research Associates Mathematics test. **Regression Specification** = Effect size was calculated using average growth between pre and posttest scores. **Results** = Treatment had a 0.648σ (0.395) impact on math test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
The Relationship Between Supplemental Computer Assisted Mathematics Instruction and Student Achievement (Manuel, 1987). N schools = 3, N students = 190, Grades = 3 - 6, Location = Omaha, NE. **Treatment Groups** = Classrooms in treatment one incorporated Computer Curriculum Corporation (CCC) software into their curricula. Classrooms in treatment two incorporated Apple software into their curricula. The control classrooms continued with their normal curricula.	**Treatment Defined** = Both treatment programs focused on teaching addition, subtraction, multiplication, division, and problem solving. The lessons were differentiated among students by pretest and Cognitive Skills Index test scores. The CCC software progressively adjusted the difficulty of its assessments based on student performance, and it included limited graphics and no gaming techniques. The Apple software had teachers set the difficulty of assessments, and it incorporated extensive graphics and some gaming techniques. Treatment is in addition to normal class time. **Randomization** = Students were stratified by grade, gender, and ability (as determined by the Cognitive Skills Index) and then assigned randomly to treatment or control.	**Test Score** = California Test of Basic Skills: Mathematics subtest. **Regression Specification** = Average growth in test scores was used to calculate effect sizes. **Results** = The CCC software treatment had a 0.066σ (0.161) impact on math test scores. The Apple software treatment had a -0.118σ (0.230) impact on math test scores.
Two-Year Impacts of a Universal School-Based Social-Emotional and Literacy Intervention: An Experiment in Translational Developmental Research (Jones et al., 2011). N schools = 18, N students = 1,184, Grades = K - 3, Location = New York City, NY. **Treatment Groups** = Treatment group implemented the 4R's ("Reading, Writing, Respect, and Resolution") Program. The control group continued the regular curriculum.	**Treatment Defined** = The 4R's program combines academic instruction in language arts with emotional development. The goal of the program is to curb aggression via anger-management, listening, assertiveness, cooperation, negotiation, mediation, community-building, countering bias, and celebration of differences. **Randomization** = 24 schools agreed to participate and were matched in pairs based on similar demographics. The nine best matched pairs of schools were selected for the study and the other three pairs were reserved as backups. One school from each pairing was assigned at random to treatment.	**Test Score** = New York State standardized assessments of math and reading. **Regression Specification** = Three-level hierarchical linear model (time, student, school) controlling for socioeconomic status, community risk factors, student behavioral risk, gender, race, teacher experience, class size, a survey measure of how burnt out a teacher is, and school fixed-effects. We report annual effects. **Results** = Treatment had an impact of -0.051σ (0.169) on math test scores and -0.012σ (0.184) on reading test scores.

Appendix Table 4 (continued)

Study	Study Design	Results
Using Enrichment Reading Practices to Increase Reading Fluency, Comprehension, and Attitudes (Reis et al., 2008). N schools = 2, N students = 558, Grades = 3 - 5. **Treatment Groups** = Treatment teachers incorporated the *Schoolwide Enrichment Reading* (*SEM-R*) program into their curricula. Control teachers continued with their normal curricula.	**Treatment Defined** = *SEM-R* is an enrichment reading program where curriculum is customized based on students' learning styles, needs, and interests. Treatment students participated in one hour of the school's normal reading program and one hour of the *SEM-R* intervention. Control students received two hours of the school's normal reading program. **Randomization** = Students and teachers were randomly assigned to treatment or control.	**Test Score** = Iowa Test of Basic Skills: Reading Comprehension subtest. **Regression Specification** = Two-level hierarchical linear model (student, classroom) controlling for oral fluency and school fixed-effects. **Results** = Treatment had an impact of 0.28σ (0.25) on reading test scores.

The Production of Human Capital in Developed Countries: Evidence from 196 Randomized Field Experiments Web Appendices

Contents

1 Appendix A: Data Appendix

We conducted a relatively exhaustive search of all randomized field experiments in education. This appendix describes our search procedure, the selection process, how we categorized the included studies, and the information gathered from each study.

1.1 Search Procedure

This section details our systematic approach to find all field experiments in education.

What Works Clearinghouse

We began by searching all "quick reviews" and "single study reviews" in the What Works Clearinghouse (WWC). WWC was created by the U.S. Department of Education's Institute of Education Sciences in 2002. Its goal is to provide reviews of education studies, policies, and interventions in order for researchers to determine "what works" in education. Currently, WWC has over 10,500 reviews available in an online searchable database. Eligible studies are reviewed by a team of WWC's certified staff against WWC standards and assigned a rating. The highest rating of the Clearinghouse is reserved for studies that meet standards without reservations. This implies that groups compared in the study were determined through a random process, there was low overall attrition from the sample, the differential attrition across groups was low, and there were no confounding factors (that is, no factor is present that all treatment students in one group are exposed to and no students in the comparison group are exposed to. If a confounding factor is present, it would be impossible to distinguish between the effect of the intervention and the effect of the factor). Out search of WWC produced 115 randomized field experiments that met standards without reservations.

Literature Views

We then expanded our search by looking through recent education literature reviews. Specifically, we referenced Almond and Currie (2011), Fryer (2010), Heckman and Kautz (2013), Nye, Turner, and Schwartz (2006), Yeager and Walton (2011), Yoon et al. (2007), Obara (2010), Kretlow and Bartholomew (2010), Carneiro and Heckman (2003), and Heckman (1999).

Databases

Next, we conducted relatively broad searches of known databases that include education papers. Specifically, we queried ERIC, JSTOR, and EconLit. In each database, we searched for all phases generated by concatenating one element from the set of strings [“early childhood”, “education”, “housing”, “neighborhood”, “parent”, “school”, “student”, “teacher”] with one element from [“experiment”, “random assignment”, “randomization”]. For ERIC and EconLit, we collected all hits that searching for these 24 unique phrases returned. For some phrases, JSTOR’s search algorithm returned thousands of results. Due to resource and time constraints, we decided to only collect the top 200 (as determined by “relevance”) results for each phrase in JSTOR. The thousands of hits we found through the database searches are available upon request.

Narrowing the Sample

The methods described above returned over 10,000 citations to check. To conduct this laborious task, we had a team of five research assistants skim every article and select papers that explicitly mentioned a random process determining the experimental sample. Further, if a research assistant determined during their quick read that the paper was obviously not education-related or the experimental sample was post-high school, the study was screened out at this point.

Other Papers

It is important to note that we didn’t restrict ourselves just to the studies produced by the systematic search described above. When reviewing the studies produced by the search procedure above, if we noticed the original study cited a study that would pass the screening criteria, we would include the cited study in our sample for further review. Also, we used our own knowledge of field experiments and advice from our colleagues to catch potential field experiments that our above search missed. Most papers caught in these manners were unpublished working papers.

Using all the above approaches, we found 859 potential studies.

1.2 Inclusion Restrictions

This section details how we narrowed our set of studies from the 859 potential studies to the final analysis sample. Table 1 from the main text summarizes how many papers were excluded for the various exclusion reasons discussed below.

Verifiably Random Process

We only included experiments that had treatment and control groups determined by a verifiably random process. Therefore, quasi-random experiments that were determined by natural processes or studies that compared subject by post hoc matching were excluded from our analysis. Further, if an experiment did not have a control group that continued business-as-usual (i.e. the control group did not receive some sort of dosage that compromised the comparison), the experiment was excluded. Studies dropped for these reasons are labeled as "Design Issues" in Table 1.

Intent-to-Treat Analyses

We only included studies that used the initial randomization assignments to estimate the impact of an intervention. We rejected studies that attempted to use econometric/statistical techniques to correct for mobility after randomization. Studies dropped for this reason are labeled as "Design Issues" in Table 1.

Pre-College Outcomes

We only included experiments with posttreatment outcomes that were collected from children aged 0 to 18. Studies dropped for this reason are labeled as "College Sample/Outcomes" in Table 1.

Highly Developed Country

We only included experiments that took place in highly developed countries. We consider countries as highly developed if they received a classification of "Very High Human Development" in United Nations Development Programme (2010). A country is classified as "Very High Human Development" if they score in the top quartile on an index of human development that includes life expectancy, mean years of schooling, expected years of schooling, and gross national income per capita. Studies dropped for this reason are labeled as "Countries w/o Very High HDI" in Table 1.

Standardized Math or Reading Outcome

We only included experiments that reported norm-referenced reading or mathematics test scores as an outcome measure at posttreatment (e.g. scores from the Peabody Picture Vocabulary Test, the Woodcock-Johnson Test of Achievement, the Iowa Test of Basic Skills, the Stanford Achievement Test, or state tests). Note that studies that report standardized scores as an outcome for later follow-ups but not immediately

following the conclusion of the experiment are excluded from our analysis. Studies dropped for this reason are labeled as "No Standardized Reading or Math" in Table 1.

One Paper Per Experiment

We only included one paper for every experimental randomization. For some experiments, such as the Perry Preschool Project and the Milwaukee voucher program, there are multiple publications detailing the impacts of the experiment at various follow-ups, investigating the initial impacts using different analytical techniques, or just using the data to investigate new theories or statistical methods. In an attempt to not give too much weight to any one experiment, we only included one impact estimate for each experiment. If there were multiple intent-to-treat estimates for an impact, we included the study that first reported the results from the intervention. In addition, our search procedure would sometimes return multiple versions of the same paper. In this case, we would only included the most recent or published version of the paper. Studies dropped for these reasons are labeled as "Repeat Paper" in Table 1.

Other

Some papers did not provide us with enough information to enable us to determine if they should be included or to calculate the effect sizes. These papers were excluded and labeled as "Insufficient Info" in Table 1.

We were unable to locate the text for a small number of the titles that our initial search returned. If research assistants were unable to find a paper through online resources and readily available library resources, we submitted all of the information we obtained through our search to Harvard's Interlibrary Loan system.[1] If Harvard Library staff were unable to locate the title through this process, the paper was excluded and labeled as "Paper Not Located" in Table 1.

Some experiments passed all of the criteria described above, however, we excluded the paper due to the experimental sample being so specific that it did not seem comparable. For example, some studies restricted their samples to special education students with ADHD, autistic students, or delivered speech therapy to students with speech impairments. Studies dropped for this reason are labeled as "Sample Issues" in Table 1.

[1] Harvard Library has cooperative partnerships with other universities and institutions from around the world to locate copies of books or papers requested through this service.

1.3 Categorization

For ease of exposition, we divide the sample of studies into categories and sub-categories. Below we give a brief definition of the three main categories and a number of sub-categories that we reference in the paper. As noted in the main text, the assignment of studies to categories is a bit arbitrary – one could easily argue that some studies fit under multiple categories. Therefore, we provide a table at the end of this section that displays our categorization of all of the studies.

1.3.1 Main Categories

Early Childhood

Any experiment with outcomes measured before children enter kindergarten is categorized as early childhood – independent of the nature of the treatment. Therefore, this category includes experiments that investigate the impacts of preschool attendance, home-based initiatives, and different preschool models on early achievement.

Home

Home environment experiments focus on parenting, income constraints, neighborhood environment, and a student's access to educational resources in their household. Note if an experiment takes place at school and focuses on these inputs, then it is still considered a home-based experiment. For example, parenting classes that take place in a school auditorium are considered a home intervention.

School

School-based experiments target K-12 curricula, teachers, management practices, students, principals, and other school resources. Any experiment where the dosage is applied on students through a school setting – such as offering families vouchers to attend private schools or after-school programs – We categorize as a school-based intervention. Note if an experiment takes place at home and focuses on these inputs, then it is still considered a school-based experiment. For example, if tutors from the school tutor students in their living rooms, this is considered a school-based experiment.

1.3.2 Sub-Categories

Home – Parental Involvement

These experiments investigate the impact of increasing parents' involvement on their children's academic achievement. Treatments that teach parents how to be effective tutors at home, incentivize parents for various behaviors, or give parents information on effective parenting practices are included in this sub-category.

Home – Educational Resources

These experiments investigate the impact of giving children or families household resources that have potential educational returns. Treatments that provide the household of students with books, computers, or internet are included in this sub-category.

Home – Poverty Reduction

These experiments investigate the impact of increasing the income of a student's family. Treatments that increase income through tax reform, direct payments, or by increasing parents' employment (welfare-to-work programs) are included in this sub-category.

Home – Neighborhood Quality

These experiments investigate the impact of neighborhood quality on students' outcomes. Treatments that move students or families from high-poverty to low-poverty neighborhoods or increase the quality of neighborhoods by implementing community programs are included in this sub-category.

School – Student Incentives

These experiments investigate the impact of incentivizing students' educational inputs and/or outputs on students' outcomes. These incentives are financial or non-financial. Treatments that pay students or award them prizes for number of books read, grades on report cards, or scores on standardized test scores are included in this sub-category.

School – High-Dosage Tutoring

These experiments investigate the impact of high-dosage tutoring. We define high-dosage as being tutored in groups of 6 or fewer for more than three days per week or being tutored at a rate that would equate to 50 hours or more over a 36-week period.[2] Note that if the tutor is a child's parent, the experiment is classified as "Home – Parental Involvement".

School – Low-Dosage Tutoring

[2]The definition used in Dobbie and Fryer (2013) is "being tutored in groups of 6 or fewer for more than three days per week." We add to the Dobbie and Fryer (2013) definition because not all studies in our sample report days and group size.

These experiments investigate the impact of low-dosage tutoring. All tutoring programs that do not meet the thresholds described above to be considered high-dosage are labeled low-dosage. Note that if the tutor is a child's parent, the experiment is classified as "Home – Parental Involvement".

School – Teacher Certification

These experiments investigate the impact of teachers obtaining certification through alternative routes or obtaining additional certifications that are not necessary to teach. Teachers obtaining certification through programs such as Teach For America or New York City Teaching Fellows or teachers receiving National Board Certification are included in this sub-category.

School – Teacher Incentives

These experiments investigate the impact of incentivizing teachers to improve student outcomes, move to new schools, or change teaching practices. These incentives can be financial or non-financial and can be awarded to individual teachers or a group of teachers. Treatments that pay teachers for their students' performance on standardized tests, offer teachers bonuses for transferring to low-achieving schools, or give schools financial awards based on predetermined benchmarks (which is then distributed to the teachers) are included in this sub-category.

School – General Professional Development

These experiments investigate the impact of general professional development (PD) programs. Treatments that provide teachers a summer institute or monthly seminars discussing issues such as classroom management or beneficial classroom practices, provide teachers with experienced coaches/mentors, or implement induction programs for new teachers are included in this sub-category. Note that we classify long-term packaged programs in a separate sub-category, "School – Managed Professional Development", discussed below.

School – Managed Professional Development

These experiments investigate the impact of managed PD – packaged programs that have precise training and curriculum materials that schools and districts can implement over an extended period of time in an effort to increase teacher effectiveness. Examples of managed PD include Success for All, Reading Recovery, the Alabama Math, Science, and Technology Initiative, and eMINTS.

School – Data-Driven Instruction

These experiments investigate the impact of using data to guide classroom instruction or school-wide managing practices. Treatments that provide principals with objective progress reports comparing their

teachers' performance to the performance of other teachers throughout the district or implement continuous progress monitoring in classrooms are included in this sub-category.

School – Extended Time

These experiments investigate the impact of exposing students to high quantities of schooling. Treatments that increase the length of school days, enroll students in after-school academic programs, or increase the number of days in a school year are included in this sub-category.

School – Vouchers

These experiments investigate the impact of giving families vouchers that offset some or all of the cost of private-school attendance.

School – School Choice

These experiments investigate the impact of allowing students and families to choose which public schools they attend. Typically, students apply to their choice public school (in the same district or a district close to where they reside) and if a school becomes over-subscribed, admission is determined through a random lottery. Chicago, Illinois and Hartford, Connecticut are examples of cities with school choice programs.

School – Charters

These experiments investigate the impact of charter schools on students. A charter school is a school that receives public funding but operates independently of the established public school system in which it is located. Typically, evaluations are conducted using the random admission lotteries of over-subscribed charter schools.

School – No Excuse Charters

These experiments investigate the impact of charter schools that adopt the "No Excuses" approach on students. These schools emphasize frequent testing, dramatically increased instructional time, parental pledges of involvement, aggressive human capital strategies, a "broken windows" theory of discipline, and a relentless focus on math and reading achievement. As shown in Dobbie and Fryer (2013) and Angrist, Pathak, and Walters (2013), charter schools that adhere to "No Excuses" practices are more effective at increasing students' test scores than other charter schools. Note that this sub-category is a subset of the sub-category "School – Charters".

School – Teaching Strategy

These experiments investigate the impact of changing classroom teaching practices. Treatments that implement individualized instruction, ability-grouped instruction, reciprocal teaching, or smaller class sizes are included in this category.

Note that treatments that implement packaged curricula (e.g. Accelerated Reader, Scott Foresman's Reading Street, Rainbow Reading, and Fluency Formula) or simple curriculum changes are not included in our analysis as they don't align with traditional economic choice variables in a concise way and because of the potential effects of publication bias on these types of studies. Appendix Table 4 describes all such studies we found using our search procedure outlined above.

School – Curriculum

These experiments investigate the impact of changing K-12 curricula. They mostly focus on packaged programs (e.g. Accelerated Reader, Scott Foresman's Reading Street, Rainbow Reading, and Fluency Formula), software products, or simple curriculum changes (e.g. new textbooks, new vocabulary words, and repeated reading). These studies are included in Appendix Table 4 but not described in the text nor included in the meta-analysis, as they don't align with traditional economic choice variables in a concise way and because of the potential effects of publication bias on these types of studies.

Other

Experiments that did not fit into any of the above sub-categories were categorized as "other".

1.3.3 Assigned Categories

See the table below for the main categories and sub-categories assigned to each experiment found through our search process. Note that if an experiment has multiple treatment arms, it is possible for the treatment arms to have different categorizations. Also, if a treatment has characteristics of more than one main category, the experiment is excluded from our meta-analysis in an attempt to avoid interactions of the categories.

Online Appendix Table 1: Categorization

Study	Main Category	Sub-Category
(1)	(2)	(3)
A Comparative Study of the Reading Achievement of Second Grade Pupils in Programs Characterized by a Contrasting Degree of Parent Participation (Ryan, 1964).	Home	Parental Involvement
A Mixed-Method Multi-Level Randomized Evaluation of the Implementation and Impact of an Audio-Assisted Reading Program for Struggling Readers (Lesnick, 2006).	School	Curriculum
A Multisite Cluster Randomized Field Trial of Open Court Reading (Borman et al., 2008).	School	Curriculum
A Multisite Cluster Randomized Trial of the Effects of CompassLearning Odyssey Math on the Math Achievement of Selected Grade 4 Students in the Mid-Atlantic Region (Wijekumar et al. 2009).	School	Curriculum
A Multistate District-Level Cluster Randomized Trial of the Impact of Data-Driven Reform on Reading and Mathematics Achievement (Carlson et al., 2011).	School	Data-Driven
A Randomized Experiment of a Cognitive Strategies Approach to Text-Based Analytical Writing for Mainstreamed Latino English Language Learners in Grades 6-12 (Kim et al., 2011).	School	Other
A Randomized Experimental Evaluation of the Impact of Accelerated Reader/Reading Renaissance Implementation on Reading Achievement in Grades 3 to 6 (Nunnery et al., 2006).	School	Curriculum
A Randomized Field Trial of the Fast ForWorld Language Computer-Based Training Program (Borman et al., 2009)	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
A Study of Cooperative Learning in Mathematics, Writing, and Reading in the Intermediate Grades: A Focus Upon Achievement, Attitudes, and Self-Esteem by Gender, Race, and Ability Group (Glassman, 1989).	School	Teaching Strategy
A Study on the Effects of Houghton Mifflin Harcourt's Journeys Program: Year 1 Final Report (Resendez and Azin, 2012)	School	Curriculum
Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots (Abdulkadiroglu et al., 2009). – Charters Treatment	School	Charters, No Excuse Charters
Accountability and Flexibility in Public Schools: Evidence from Boston's Charters and Pilots (Abdulkadiroglu et al., 2009). – Pilots Treatment	School	Charters
Action Research: Implementing Connecting Math Concepts (Snider and Crawford, 1996).	School	Curriculum
Addressing Summer Reading Setback Among Economically Disadvantaged Elementary Students (Allington et al, 2010).	Home	Educational Resources
Alternative Routes to Teaching The Impacts of Teach for America (TFA) on Student Achievement and Other Outcomes (Glazerman et al., 2006).	School	Teacher Certification
An Efficacy Study on Scott Foresman's Reading Street Program: Year One Report (Wilkerson et al., 2006).	School	Curriculum
An Evaluation of a Pilot Program in Reading for Culturally Disadvantaged First Grade Students (Bowers, 1972).	School	General PD
An Evaluation of Curriculum, Setting, and Mentoring on the Peformance of Children Enrolled in Pre-Kindergarten (Assel et al., 2006). – DDM Treatment	Early	Early

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
An Evaluation of Curriculum, Setting, and Mentoring on the Peformance of Children Enrolled in Pre-Kindergarten (Assel et al., 2006). – DDN Treatment	Early	Early
An Evaluation of Curriculum, Setting, and Mentoring on the Peformance of Children Enrolled in Pre-Kindergarten (Assel et al., 2006). – LBM Treatment	Early	Early
An Evaluation of Curriculum, Setting, and Mentoring on the Peformance of Children Enrolled in Pre-Kindergarten (Assel et al., 2006). – LBN Treatment	Early	Early
An Evaluation of Reading Recovery (Center et al., 1995).	School	Managed PD
An Evaluation of Teachers Trained Through Different Routes to Certification: Final Report (Constantine et al., 2009).	School	Teacher Certification
An Evaluation of the Effects of Paired Learning in a Mathematics Computer-Assisted-Instruction Program (Turner, 1985). – Individual Treatment	School	Curriculum
An Evaluation of the Effects of Paired Learning in a Mathematics Computer-Assisted-Instruction Program (Turner, 1985). – Paired Treatment	School	Curriculum
An Evaluation of the Teacher Advancement Program (TAP) in Chicago: Year One Impact Report (Glazerman et al., 2009).	School	Teacher Incentives
An Experimental Study of the Effects of the Accelerated Reader Program and a Teacher Directed Program on Reading Comprehension and Vocabulary of Fourth and Fifth Grade Students (Knox, 1996).	School	Curriculum
An Investigation of the Effects of a Comprehensive Reading Intervention on the Beginning Reading Skills of First Graders at Risk for Emotional and Behavioral Disorders (Mooney, 2003).	School	High-Dosage Tutoring

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
An Investigation of the Effects of Daily, Thirty-Minute Home Practice Sessions Upon Reading Achievement With Second Year Elementary Pupils (Hirst, 1972).	Home	Parental Involvement
Are High-Quality Schools Enough to Increase Achievement Among the Poor? Evidence from the Harlem Children's Zone (Dobbie and Fryer, 2011).	School	Charters, No Excuse Charters
Assessing the Effectiveness of First Step to Success: Are Short-Term Results the First Step to Long-Term Behavioral Improvements? (Sumi et al., 2012).	Home, School	Parental Involvement, General PD
Assessment Data - Informed Guidance to Individualize Kindergarten Reading Instruction: Findings from a Cluster-Randomized Control Field Trial (Al Otaiba et al., 2011).	School	Data-Driven
Beyond the Pages of a Book: Interactive Reading and Language Development in Preschool Classrooms (Wasik and Bond, 2001).	Early	Early
Can a Mixed-Method Literacy Intervention Improve the Reading Achievement of Low-Performing Elementary School Students in an After-School Program? Results From a Randomized Controlled Trial of READ 180 Enterprise (Kim et al. 2011).	School	High-Dosage Tutoring
Can Interdistrict Choice Boost Student Achievement? The Case of Connecticut's Interdistrict Magnet School Program (Bifulco et al., 2009).	School	School Choice
Career Academies: Impacts on Students' Engagement and Performance in High School (Kemple and Snipes, 2000).	School	Other
Charter Schools in New York City: Who Enrolls and How it Affects their Students' Achievements (Hoxby, 2009).	School	Charters

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Children At-Risk for Poor School Readiness: The Effect of an Early Intervention Home Visiting Program on Children and Parents (Necoechea, 2007).	Early	Early
Classroom Assessment for Student Learning: The Impact on Elementary School Mathematics in the Central Region (Randel et al., 2011).	School	General PD
Closing the Achievement Gap: A Structured Approach to Group Counseling (Campbell and Brigman, 2005).	School	Other
Collaboration Between Teachers and Parents in Assisting Children's Reading (Tizard et al., 1982). – Home Treatment	Home	Parental Involvement, Educational Resources
Collaboration Between Teachers and Parents in Assisting Children's Reading (Tizard et al., 1982). – School Treatment	School	Low-Dosage Tutoring
Combining Cooperative Learning and Individualized Instruction: Effects on Student Mathematics Achievement, Attitudes, and Behaviors (Slavin et al., 1984). – Curriculum Treatment	School	Curriculum
Combining Cooperative Learning and Individualized Instruction: Effects on Student Mathematics Achievement, Attitudes, and Behaviors (Slavin et al., 1984). – TAI Treatment	School	Teaching Strategy
Comer's School Development Program in Prince George's County, Maryland: A Theory-Based Evaluation (Cook et al., 1999).	School	General PD
Comparative Effectiveness of Scott Foresman Science: A Report of a Randomized Experiment in Five School Districts (Miller and Jaciw, 2007).	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Comparing Instructional Models for the Literacy Education of High-Risk First Graders (Pinell et al., 1994). – DI Treatment	School	High-Dosage Tutoring
Comparing Instructional Models for the Literacy Education of High-Risk First Graders (Pinell et al., 1994). – RR Treatment	School	Managed PD
Comparing Instructional Models for the Literacy Education of High-Risk First Graders (Pinell et al., 1994). – RS Treatment	School	Managed PD
Comparing Instructional Models for the Literacy Education of High-Risk First Graders (Pinell et al., 1994). – RW Treatment	School	Managed PD
Computer Assisted Instruction as an Enhancer of Remediation (Hotard and Cortez, 1983).	School	Curriculum
Computer-Assisted Instruction to Prevent Early Reading Difficulties in Students at Risk for Dyslexia: Outcomes from Two Instructional Approaches (Torgesen et al., 2009). – LIPS Treatment	School	Curriculum
Computer-Assisted Instruction to Prevent Early Reading Difficulties in Students at Risk for Dyslexia: Outcomes from Two Instructional Approaches (Torgesen et al., 2009). – RWT Treatment	School	Curriculum
Costs, Effects, and Utility of Microcomputer Assisted Instruction (Fletcher et al., 1990).	School	Curriculum
CSRP's Impact on Low-Income Preschoolers' Preacademic Skills: Self-Regulation as a Mediating Mechanism (Raver et al., 2011).	Early	Early
Direct Instruction in Fourth and Fifth Grade Classrooms (Sloan, 1993).	School	General PD

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Does Rainbow Repeated Reading Add Value to an Intensive Intervention Program for Low-Progress Readers? An Experimental Evaluation (Wheldall, 2000).	School	Curriculum
Does Reading During the Summer Build Reading Skills? Evidence from a Randomized Experiment in 463 Classrooms (Guryan et al., 2014).	Home	Educational Resources
Early College, Early Success: Early College High School Initiative (ECHSI) impact study (Berger et al., 2013).	School	Student Incentives
Early Intervention in Low-Birth-Weight Premature Infants: Results Through Age 5 Years From the Infant Health and Development Program (Brooks-Gunn et al., 1994).	Early	Early
Educational Effects of the Tools of the Mind Curriculum: A Randomized Trial (Barnett et al., 2008).	Early	Early
Effect of Early Literacy Intervention on Kindergarten Achievement (Phillips, 1990). – Home Treatment	Home	Parental Involvement, Educational Resources
Effect of Early Literacy Intervention on Kindergarten Achievement (Phillips, 1990). – Home+School Treatment	Home, School	Parental Involvement, Educational Resources, Curriculum
Effect of Early Literacy Intervention on Kindergarten Achievement (Phillips, 1990). – School Treatment	School	Curriculum
Effect of Technology-Enhanced Continuous Progress Monitoring on Math Achievement (Ysseldyke and Bolt, 2007).	School	Data-Driven

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Effective Early Literacy Skill Development for Young Spanish-Speaking English Language Learners: An Experimental Study of Two Methods (Farver et al., 2009). – English Treatment	Early	Early
Effective Early Literacy Skill Development for Young Spanish-Speaking English Language Learners: An Experimental Study of Two Methods (Farver et al., 2009). – Transitional Treatment	Early	Early
Effectiveness of Paraeducator-Supplemented Individual Instruction: Beyond Basic Decoding Skills (Vadasy et al., 2007).	School	High-Dosage Tutoring
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – AN Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – AoR Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – DR Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – Headsprout Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – LT Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – Larson Treatment	School	Curriculum
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – PF Treatment	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Effectiveness of Reading and Mathematics Software Products: Findings From Two Student Cohorts (Campuzano et al., 2009). – WE Treatment	School	Curriculum
Effectiveness of Selected Supplemental Reading Comprehension Interventions: Impacts on a First Cohort of Fifth-Grade Students (James-Burdumy et al., 2009). – Project CRISS Treatment	School	Curriculum
Effectiveness of Selected Supplemental Reading Comprehension Interventions: Impacts on a First Cohort of Fifth-Grade Students (James-Burdumy et al., 2009). – Read for Real Treatment	School	Curriculum
Effectiveness of Selected Supplemental Reading Comprehension Interventions: Impacts on a First Cohort of Fifth-Grade Students (James-Burdumy et al., 2009). – ReadAbout Treatment	School	Curriculum
Effectiveness of Selected Supplemental Reading Comprehension Interventions: Impacts on a First Cohort of Fifth-Grade Students (James-Burdumy et al., 2009). – Reading for Knowledge	School	Curriculum
Effects of a Voluntary Summer Reading Intervention on Reading Achievement: Results From a Randomized Field Trial (Kim, 2006).	Home	Educational Resources
Effects of a Volunteer Tutoring Model on the Early Literacy Development of Struggling First Grade Students (Pullen et al., 2004).	School	High-Dosage Tutoring
Effects of Academic Tutoring on the Social Status of Low-Achieving, Socially Rejected Children (Coie and Krehbie, 1984). – Mentoring Treatment	School	Other
Effects of Academic Tutoring on the Social Status of Low-Achieving, Socially Rejected Children (Coie and Krehbie, 1984). – Tutoring Treatment	School	High-Dosage Tutoring

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Effects of Academic Tutoring on the Social Status of Low-Achieving, Socially Rejected Children (Coie and Krehbie, 1984). – Tutoring+Mentoring Treatment	School	High-Dosage Tutoring
Effects of an Early Literacy Professional Development Intervention on Head Start Teachers and Children (Powell et al., 2010).	Early	Early
Effects of Health-Related Physical Education on Academic Achievement: Project SPARK (Sallis et al., 1999). – Specialist Treatment	School	Curriculum
Effects of Health-Related Physical Education on Academic Achievement: Project SPARK (Sallis et al., 1999). – Trained Treatment	School	Curriculum
Effects of Intensive Reading Remediation for Second and Third Graders and a 1-Year Follow-Up (Blachman et al., 2004).	School	High-Dosage Tutoring
Effects of Parent Involvement in Isolation or in Combination with Peer Tutoring on Self-Concept and Math (Fantuzzo et al., 1995). – PI Treatment	Home	Parental Involvement
Effects of Parent Involvement in Isolation or in Combination with Peer Tutoring on Self-Concept and Math (Fantuzzo et al., 1995). – PI+RPT Treatment	Home, School	Parental Involvement, Teaching Strategy
Effects of Peer-Assisted Learning Strategies With and Without Training in Elaborated Help Giving (Fuchs et al., 1999). – PALS Treatment	School	High-Dosage Tutoring
Effects of Peer-Assisted Learning Strategies With and Without Training in Elaborated Help Giving (Fuchs et al., 1999). – PALS-HG Treatment	School	High-Dosage Tutoring

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – BB Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – CC Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – CCorn Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – CCwL Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – DD Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – DLM Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – ELLM Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – LB Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – LE Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – LFC Treatment	Early	Early

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – PA Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – PC Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – Pre-K Math Treatment	Early	Early
Effects of Preschool Curriculum Programs on School Readiness (Preschool Curriculum Evaluation Research Consortium, 2008). – RSL Treatment	Early	Early
Effects of Reading Decodable Texts in Supplemental First-Grade Tutoring (Jenkins et al., 2004). – Less Treatment	School	High-Dosage Tutoring
Effects of Reading Decodable Texts in Supplemental First-Grade Tutoring (Jenkins et al., 2004). – More Treatment	School	High-Dosage Tutoring
Effects of Targeted Intervention on Early Literacy Skills of At-Risk Students (Wang and Algozzine, 2008).	School	Curriculum
Effects of Whole Class, Ability Grouped, and Individualized Instruction on Mathematics Achievement (Slavin and Karweit, 1985). – AGAT Treatment	School	Teaching Strategy
Effects of Whole Class, Ability Grouped, and Individualized Instruction on Mathematics Achievement (Slavin and Karweit, 1985). – MMP Treatment	School	Curriculum
Effects of Whole Class, Ability Grouped, and Individualized Instruction on Mathematics Achievement (Slavin and Karweit, 1985). – TAI Treatment	School	Teaching Strategy

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Efficacy of a Direct Instruction Approach to Promote Early Learning (Salaway, 2008).	Early	Early
Efficacy of Collaborative Strategic Reading with Middle School Students (Vaughn et al., 2011).	School	Curriculum
Empirical Evaluation of Read Naturally Effects (Christ and Davie, 2009).	School	Curriculum
Enhancing First-Grade Children's Mathematical Development with Peer-Assisted Learning Strategies (Fuchs et al., 2002).	School	High-Dosage Tutoring
Enhancing Kindergarteners' Mathematical Development: Effects of Peer-Assisted Learning Strategies (Fuchs et al., 2001).	School	Low-Dosage Tutoring
Enhancing the Efficacy of Teacher Incentives Through Loss Aversion (Fryer et al., 2012). – Gain Treatment	School	Teacher Incentives
Enhancing the Efficacy of Teacher Incentives Through Loss Aversion (Fryer et al., 2012). – Loss Treatment	School	Teacher Incentives
Evaluation of Child Care Subsidies: Findings from Project Upgrade in Miami (Layzer et al., 2007). – BELL Treatment	Early	Early
Evaluation of Child Care Subsidies: Findings from Project Upgrade in Miami (Layzer et al., 2007). – Breakthrough treatment	Early	Early
Evaluation of Child Care Subsidies: Findings from Project Upgrade in Miami (Layzer et al., 2007). – Ready Set Leap Treatment	Early	Early
Evaluation of Curricular Approaches to Enhance Preschool Early Literacy Skills (Fischel et al., 2007). – LB Treatment	Early	Early

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Evaluation of Curricular Approaches to Enhance Preschool Early Literacy Skills (Fischel et al., 2007). – WF Treatment	Early	Early
Evaluation of Experience Corps: Student Reading Outcomes (Morrow-Howell et al., 2009).	School	High-Dosage Tutoring
Evaluation of Quality Teaching for English Learners (QTEL) Professional Development: Final Report (Bos et al., 2012).	School	General PD
Evaluation of the DC Opportunity Scholarship Program: Final Report (Wolf et al., 2010).	School	Vouchers
Evaluation of the Early Start to Emancipation Preparation Tutoring Program in Los Angeles County, CA (Courtney et al., 2008).	School	Low-Dosage Tutoring
Evaluation of the Effectiveness of the Alabama Math, Science, and Technology Initiative (AMSTI) (Newman et al., 2012).	School	Managed PD
Evaluation of the First 3 Years of the Fast Track Prevention Trial with Children at High Risk for Adolescent Conduct Problems (Bierman et al., 2002).	Home, School	Parental Involvement, Curriculum
Evaluation of the i3 Scale-Up of Reading Recovery: Year One Report (May et al., 2013).	School	Managed PD
Evaluation Research on the Effectiveness of Fluency Formula: Final Report (Sivin-Kachala and Bialo, 2005).	School	Curriculum
Experimental Estimates of Education Production Functions (Krueger, 1999).	School	Teaching Strategy

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Experimental Evidence on the Effects of Home Computers on Academic Achievement among Schoolchildren (Fairlie and Robinson, 2013).	Home	Educational Resources
Explaining Charter School Effectiveness (Angrist et al., 2011).	School	Charters
Final Reading Outcomes of the National Randomized Field Trial of Success for All (Borman et al., 2007).	School	Managed PD
Financial Incentives and Student Achievement: Evidence from Randomized Trials (Fryer, 2011). – Chicago Treatment	School	Student Incentives
Financial Incentives and Student Achievement: Evidence from Randomized Trials (Fryer, 2011). – Dallas Treatment	School	Student Incentives
Financial Incentives and Student Achievement: Evidence from Randomized Trials (Fryer, 2011). – NYC Treatment	School	Student Incentives
Fostering Development of Reading Skills Through Supplemental Instruction: Results for Hispanic and Non-Hispanic Students (Gunn et al., 2005).	Home, School	Parental Involvement, High-Dosage Tutoring
Fostering the Development of Vocabulary Knowledge and Reading Comprehension Through Contextually-Based Multiple Meaning Vocabulary Instruction (Nelson and Stage, 2007).	School	Curriculum
Full-Day versus Half-Day Kindergarten: An Experimental Study (Holmes and McConnell 1990).	School	Extended Time
Getting Parents Involved: A Field Experiment in Deprived Schools (Avvisati et al., 2014).	Home	Parental Involvement

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Head Start Children's Entry into Public School: A Report on the National Head Start/Public School Early Childhood Transition Demonstration Study (Ramey et al., 2000).	Home, School	Parental Involvement
Head Start Impact Study: Final Report (Puma et al., 2010).	Early	Early
Homework in Arithmetic (Koch, 1965). – full treat	School	Teaching Strategy
Homework in Arithmetic (Koch, 1965). – half treat	School	Teaching Strategy
Impact of eMINTS Professional Development on Student Achievement (Brandt et al., 2013).	School	Managed PD
Impact of Thinking Reade Software Program on Grade 6 Reading Vocabulary, Comprehension, Strategies, and Motivation (Drummond et al., 2011).	School	Curriculum
Impacts of Comprehensive Teacher Induction: Results from the Second Year of a Randomized Controlled Study (Isenberg et al., 2009).	School	General PD
Improving Reading Comprehension and Social Studies Knowledge in Middle School (Vaughn et al., 2013).	School	Curriculum
Improving Reading Fluency and Comprehension in Elementary Students Using Read Naturally (Arvans, 2009).	School	Curriculum
Improving Students' Reading Comprehension Skills: Effects of Comprehension Instruction and Reciprocal Teaching (Spörer et al., 2009). – IG Treatment	School	Teaching Strategy

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Improving Students' Reading Comprehension Skills: Effects of Comprehension Instruction and Reciprocal Teaching (Spörer et al., 2009). – RT Treatment	School	Curriculum
Improving Students' Reading Comprehension Skills: Effects of Comprehension Instruction and Reciprocal Teaching (Spörer et al., 2009). – RTP Treatment	School	Teaching Strategy
Individualizing a Web-Based Structure Strategy Intervention for Fifth Graders' Comprehension of Nonfiction (Meyer et al., 2011).	School	Curriculum
Information and Employee Evaluation: Evidence from a Randomized Intervention in Public Schools (Rockoff et al., 2012).	School	Data-Driven
Information and Student Achievement: Evidence from a Cellular Phone Experiment (Fryer, 2013). – Incentive Treatment	School	Student Incentives
Information and Student Achievement: Evidence from a Cellular Phone Experiment (Fryer, 2013). – Information Treatment	School	Other
Injecting Charter School Best Practices into Traditional Public Schools: Evidence from Field Experiments (Fryer, 2014).	School	Charters, No Excuse Charters
KIPP Middle Schools: Impacts on Achievement and Other Outcomes (Tuttle et al., 2013).	School	Charters, No Excuse Charters
Large-Scale Randomized Controlled Trial with 4th Graders Using Intelligent Tutoring of the Structure Strategy to Improve Nonfiction Reading Comprehension (Wijekumar et al., 2012).	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Literacy Learning of At-Risk First-Grade Students in the Reading Recovery Early Intervention (Schwartz, 2005).	School	Managed PD
Longer-Term Impacts of Mentoring, Educational Services, and Learning Incentives: Evidence from a Randomized Trial in the United States (Rodriguez-Planas, 2012).	School	Low-Dosage Tutoring, Student Incentives
Longitudinal Effects of Classwide Peer Tutoring (Greenwood et al., 1989).	School	High-Dosage Tutoring
Longitudinal Results of the Ypsilanti Perry Preschool Project: Final Report (Weikart et al., 1970).	Early	Early
Making Work Pay: Final Report on the Self-Sufficiency Project for Long-Term Welfare Recipients (Michalopoulos et al., 2002).	Home	Poverty Reduction
Mastery Learning and Student Teams: A Factorial Experiment in Urban General Mathematics (Slavin and Karweit, 1984). – Both Treatment	School	Teaching Strategy
Mastery Learning and Student Teams: A Factorial Experiment in Urban General Mathematics (Slavin and Karweit, 1984). – Mastery Treatment	School	Teaching Strategy
Mastery Learning and Student Teams: A Factorial Experiment in Urban General Mathematics (Slavin and Karweit, 1984). – Teams Treatment	School	Teaching Strategy
National Assessment of Title I Interim Report: Volume II: Closing the Reading Gap: First Year Findings from a Randomized Trial of Four Reading Interventions for Striving Readers (Torgesen et al., 2006). – CR Treatment	School	Curriculum
National Assessment of Title I Interim Report: Volume II: Closing the Reading Gap: First Year Findings from a Randomized Trial of Four Reading Interventions for Striving Readers (Torgesen et al., 2006). – FFR Treatment	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
National Assessment of Title I Interim Report: Volume II: Closing the Reading Gap: First Year Findings from a Randomized Trial of Four Reading Interventions for Striving Readers (Torgesen et al., 2006). – SR Treatment	School	Curriculum
National Assessment of Title I Interim Report: Volume II: Closing the Reading Gap: First Year Findings from a Randomized Trial of Four Reading Interventions for Striving Readers (Torgesen et al., 2006). – WR Treatment	School	Curriculum
National Board Certification and Teacher Effectiveness: Evidence from a Random Assignment Experiment (Cantrell et al., 2008).	School	Teacher Certification
National Evaluation of Welfare-to-Work Strategies (Hamilton et al., 2001). – HCD Treatment	Home	Poverty Reduction
National Evaluation of Welfare-to-Work Strategies (Hamilton et al., 2001). – LFA Treatment	Home	Poverty Reduction
National Impact Evalutation of the Comprehensive Child Development Program: Final Report (St. Pierre et al., 1997).	Early	Early
Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment (Sanbonmatsu et al., 2006). – Experimental Treatment	Home	Neighborhood Quality
Neighborhoods and Academic Achievement: Results from the Moving to Opportunity Experiment (Sanbonmatsu et al., 2006). – Section 8 Treatment	Home	Neighborhood Quality
Parent Tutoring as a Supplement to Compensatory Education for First Grade Children (Mehran and White, 1988).	Home	Parental Involvement

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Parent Tutoring in Reading Using Literature and Curriculum Materials: Impact on Student Reading Achievement (Powell-Smith et al., 2000). – CB Treatment	Home	Parental Involvement
Parent Tutoring in Reading Using Literature and Curriculum Materials: Impact on Student Reading Achievement (Powell-Smith et al., 2000). – LB Treatment	Home	Parental Involvement
Parental Incentives and Early Childhood Achievement: A Field Experiment in Chicago Heights (Fryer et al., 2015). – Cash treatment	Early	Early
Parental Incentives and Early Childhood Achievement: A Field Experiment in Chicago Heights (Fryer et al., 2015). – College treatment	Early	Early
Paying to Learn: The Effect of Financial Incentives on Elementary School Test Scores (Bettinger, 2012).	School	Student Incentives
Poverty, Early Childhood Education, and Academic Competence: The Abecedarian Experiment (Ramey and Campbell, 1991).	Early	Early
Prevention and Remediation of Severe Reading Disabilities: Keeping the End in Mind (Torgesen et al., 1997). – EP Treatment	School	High-Dosage Tutoring
Prevention and Remediation of Severe Reading Disabilities: Keeping the End in Mind (Torgesen et al., 1997). – PASP Treatment	School	High-Dosage Tutoring
Prevention and Remediation of Severe Reading Disabilities: Keeping the End in Mind (Torgesen et al., 1997). – RCS Treatment	School	High-Dosage Tutoring
Private School Vouchers and Student Achievement: An Evaluation of the Milwaukee Parental Choice Program (Rouse, 1998).	School	Vouchers

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Project Breakthrough: A Responsive Environment Field Experiment with Pre-School Children from Public Assistance Families (Cook County Department of Public Aid, 1969).	Early	Early
Promoting Academic and Social-Emotional School Readiness: The Head Start REDI Program. (Bierman et al., 2008).	Early	Early
Putting Books in the Classroom Seems Necessary But Not Sufficient (McGill-Franzen et al., 1999). – Books Treatment	School	Other
Putting Books in the Classroom Seems Necessary But Not Sufficient (McGill-Franzen et al., 1999). – Books+Training Treatment	School	General PD
Randomized Field Trial of an Early Literacy Curriculum and Instituional Support System (Cosgrove et al., 2006).	Early	Early
Reading and Language Outcomes of a Multiyear Randomized Evaluation of Transitional Bilingual Education (Slavin et al., 2011).	School	Curriculum
Repeated Reading Intervention: Outcomes and Interactions with Readers' Skills and Classroom Instruction (Vadasy and Sanders, 2008).	School	High-Dosage Tutoring
School Choice as a Latent Variable: Estimating the "Complier Average Causal Effect" of Vouchers in Charlotte (Cowen, 2008).	School	Vouchers
School Choice in Dayton, Ohio after Two Years: An Evaluation of the Parents Advancing Choice in Education Scholarship Program (West et al., 2001).	School	Vouchers
School Choice in New York City After Three Years: An Evaluation of the School Choice Scholarships Program (Mayer et al., 2002).	School	Vouchers

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Segmentation / Spelling Instruction as Part of a First-Grade Reading Program: Effects on Several Measures of Reading (Uhry and Shepherd, 1993).	School	Curriculum
Spatial Temporal Mathematics at Scale: An Innovative and Fully Developed Paradigm to Boost Math Achievement Among All Learners (Rutherford et al., 2010).	School	Curriculum
Summer School Effects in a Randomized Field Trial (Zvoch and Stevens, 2012).	School	Extended Time
Supporting Families in a High-Risk Setting: Proximal Effects of the SAFEChildren Preventive Intervention (Tolan et al., 2004).	Home, School	Parental Involvement, Low-Dosage Tutoring
Teacher Behavior and Pupil Performance: Reconsideration of the Mediation of Pygmalion Effects (Alpert, 1975).	School	Teaching Strategy
Teacher Incentives and Student Achievement: Evidence from New York City Public Schools (Fryer, 2013).	School	Teacher Incentives
Teacher Pay for Performance: Experimental Evidence from the Project on Incentives in Teaching (Springer et al., 2010).	School	Teacher Incentives
Teacher Study Group: Impact of the Professional Development Model on Reading Instruction and Student Outcomes in First Grade Classrooms (Gersten et al., 2010).	School	General PD
Teaching Children to Become Fluent and Automatic Readers (Kuhn et al., 2006). – Repeated-Reading Treatment	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
Teaching Children to Become Fluent and Automatic Readers (Kuhn et al., 2006). – Wide-Reading Treatment	School	Curriculum
Team Pay for Performance: Experimental Evidence From the Round Rock Pilot Project on Team Incentives (Springer et al., 2012).	School	Teacher Incentives
Technology's Edge: The Educational Benefits of Computer-Aided Instruction (Barrow et al., 2009).	School	Curriculum
The (Surprising) Efficacy of Academic and Behavorial Intervention with Disadvantaged Youth Results from a Randomized Experiment in Chicago (Cook et al., 2014). – BAM Treatment	School	Other
The (Surprising) Efficacy of Academic and Behavorial Intervention with Disadvantaged Youth Results from a Randomized Experiment in Chicago (Cook et al., 2014). – BAM+Tutoring Treatment	School	High-Dosage Tutoring
The Early Training Project for Disadvantaged Children: A Report After Five Years (Klaus and Gray, 1968).	Early, School	Early
The Effect of Computer Assisted Instruction in Improving Mathematics Performance of Low Achieving Ninth Grade Students (Bailey, 1991).	School	Curriculum
The Effect of School Choice on Participants: Evidence from Randomized Lotteries (Cullen et al., 2006).	School	School Choice
The Effect of Second-Language Instruction on the Reading Proficiency and General School Achievement of Primary-Grade Children. (Potts, 1967).	School	Curriculum
The Effective Instruction of Comprehension: Results and Description of the Kamehameha Early Education Program (Tharp and Roland, 1982).	School	Teaching Strategy

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
The Effectiveness of a Program to Accelerate Vocabulary Development in Kindergarten (VOCAB) (Goodson et al., 2010).	School	Curriculum
The Effectiveness of Computer Assisted Instruction of Chapter I Students in Secondary Schools (Davidson, 1985).	School	Curriculum
The Effectiveness of Extended Day Programs: Evidence from a Randomized Field Experiment in the Netherlands (Meyer and Klaveren, 2013).	School	Extended Time
The Effectiveness of Secondary Math Teachers from Teach for America and the Teaching Fellows Programs (Clark et al., 2013). – TFA Treatment	School	Teacher Certification
The Effectiveness of Secondary Math Teachers from Teach for America and the Teaching Fellows Programs (Clark et al., 2013). – Teaching Fellows Treatment	School	Teacher Certification
The Effectiveness of Team-Accelerated Instruction on High Achievers in Mathematics (Karper and Melnick, 1993).	School	Teaching Strategy
The Effects of a Language and Literacy Intervention on Head Start Children and Teachers (Wasik et al., 2006).	Early	Early
The Effects of a Negative Income Tax on School Performance: Results of an Experiment (Maynard and Murname, 1979).	Home	Poverty Reduction
The Effects of A One-Year Staff Development Program on the Achievement Test Scores of Fourth Grade Students (Cole, 1992).	School	General PD
The Effects of a Voluntary Summer Reading Intervention on Reading Activities and Reading Achievement (Kim, 2007).	Home	Educational Resources

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
The Effects of Brain Gym as a General Education Intervention: Improving Academic Performance and Behaviors (Nussbaum, 2010).	School	Other
The Effects of Computer Assisted Instruction as a Supplement to Classroom Instruction in Reading Comprehension and Arithmetic (Easterling, 1982). – Mathematics Treatment	School	Curriculum
The Effects of Computer Assisted Instruction as a Supplement to Classroom Instruction in Reading Comprehension and Arithmetic (Easterling, 1982). – Reading Treatment	School	Curriculum
The Effects of Peer-Assisted Literacy Strategies for First-Grade Readers With and Without Additional Mini-Skills Lessons (Mathes and Babyak, 2001). – PALS Treatment	School	High-Dosage Tutoring
The Effects of Peer-Assisted Literacy Strategies for First-Grade Readers With and Without Additional Mini-Skills Lessons (Mathes and Babyak, 2001). – PALS+ML Treatment	School	High-Dosage Tutoring
The Effects of Structured One-on-One Tutoring in Sight Word Recognition of First-Grade Students At-Risk for Reading Failure (Mayfield, 2000).	School	High-Dosage Tutoring
The Effects of the Home Instruction Program for Preschool Youngsters (HIPPY) on Children's School Performance at the End of the Program and One Year Later (Baker et al., 1998).	Early	Early
The Effects of Theoretically Different Instruction and Student Characteristics on the Skills of Struggling Readers (Mathes et al., 2005). – Proactive Treatment	School	High-Dosage Tutoring

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
The Effects of Theoretically Different Instruction and Student Characteristics on the Skills of Struggling Readers (Mathes et al., 2005). – Responsive Treatment	School	High-Dosage Tutoring
The Effects of Training Parents in Teaching Phonemic Awareness on the Phonemic Awareness and Early Reading of Struggling Readers (Warren, 2009).	Home	Parental Involvement
The Efficacy of an Early Literacy Tutoring Program Implemented by College Students (Allor and McCathren, 2004).	School	High-Dosage Tutoring
The Enhanced Reading Opportunities (ERO) Study Final Report: The Impact of Supplemental Literacy Courses for Struggling Ninth-grade Readers (Somers et al., 2010).	School	Curriculum
The Evaluation of Charter School Impacts: Final Report (Gleason et al., 2010).	School	Charters
The Evaluation of Enhanced Academic Instruction in After-School Programs: Final Report (Black et al., 2009).	School	Extended Time
The Impact of a Literature-Based Program on Literacy Achievement, Use of Literature, and Attitudes of Children from Minority Backgrounds (Morrow, 1992). – Home+School Treatment	Home, School	Parental Involvement
The Impact of a Literature-Based Program on Literacy Achievement, Use of Literature, and Attitudes of Children from Minority Backgrounds (Morrow, 1992). – School Treatment	School	Curriculum
The Impact of Challenging Geometry and Measurement Units on Achievement of Grade 2 Students (Gavin et al., 2013).	School	Curriculum

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
The Impact of Collaborative Strategic Reading on the Reading Comprehension of Grade 5 Students in Linguistically Diverse Schools (Hitchcock et al., 2011).	School	Curriculum
The Impact of Elementary Mathematics Coaches on Student Achievement (Campbell and Malkus, 2011).	School	General PD
The Impact of Indiana's System of Interim Assessments on Mathematics and Reading Achievement (Konstantopoulos et al., 2013).	School	Data-Driven
The Impact of Parental Training in Methods to Aid Beginning Reading on Reading Achievement and Reading Attitudes of First-Grade Students. (Peeples, 1996).	Home	Parental Involvement
The Impact of Two Professional Development Interventions on Early Reading Instruction and Achievement (Garet et al. 2008). – Institute Series Treatment	School	General PD
The Impact of Two Professional Development Interventions on Early Reading Instruction and Achievement (Garet et al. 2008). – Institute Series+Coaching Treatment	School	General PD
The Influence of Massive Rewards on Reading Achievement in Potential Urban School Dropouts (Clark and Walberg, 1968).	School	Student Incentives
The Missouri Mathematics Effectiveness Project: An Experimental Study in Fourth-Grade Classrooms (Good and Grouws, 1979).	School	Curriculum
The Potential of Ubran Boarding Schools for the Poor: Evidence from SEED (Curto and Fryer, 2014).	School	Charters, No Excuse Charters

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
The Prevention, Identification, and Cognitive Determinants of Math Difficulty (Fuchs et al., 2005).	School	High-Dosage Tutoring
The Reading Connection: A Leadership Initiative Designed to Change the Delivery of Educational Services to At-Risk Children (Compton, 1992).	School	High-Dosage Tutoring
The Relationship Between Supplemental Computer Assisted Mathematics Instruction and Student Achievement (Manuel, 1987). – Apple Treatment	School	Curriculum
The Relationship Between Supplemental Computer Assisted Mathematics Instruction and Student Achievement (Manuel, 1987). – CCC Treatment	School	Curriculum
Towards Reduced Poverty Across Generations: Early Findings from New York City's Conditional Cash Transfer Program (Riccio et al., 2010).	Home	Parental Involvement
Transfer Incentives for High-Performing Teachers: Final Results from a Multisite Randomized Experiment (Glazerman et al., 2013).	School	Teacher Incentives
Two-Year Impacts of a Universal School-Based Social-Emotional and Literacy Intervention (Jones, et al., 2011).	School	Curriculum
Using Enrichment Reading Practices to Increase Reading Fluency, Comprehension, and Attitudes (Reis et al., 2008).	School	Curriculum
Using Knowledge of Children's Mathematics Thinking in Classroom Teaching: An Experimental Study (Carpenter et al., 1989).	School	General PD
Using Television as a Teaching Tool: The Impacts of Ready to Learn Workshops on Parents, Educators, and the Children in their Care (Boller et al., 2004).	Early	Early

Online Appendix Table 1 (continued)

Study	Main Category	Sub-Category
(1)	(2)	(3)
When Less May Be More: A 2 Year Longitudinal Evaluation of a Volunteer Tutoring Program Requiring Minimal Training (Baker et al., 2000).	School	Low-Dosage Tutoring
When Schools Stay Open Late: The National Evaluation of the 21st Century Community Learning Centers Program (James-Burdumy et al., 2005).	School	Extended Time

Notes: This table presents the main categories and sub-categories assigned to each treatment from papers we found that passed our inclusion criteria. These categories are described in the text and Online Appendix A. Note that if a treatment fit into multiple main categories, the treatment was not included in our meta-analysis. Further, curriculum studies were not included in our meta-analysis.

1.4 Data Collected

For every randomized field experiment found using the search procedure described above, we calculated the impact (in standard deviations) of the intervention on standardized math and reading outcomes and collected data on key demographic and implementation features of the experiment. This section details all of the information we collected for each experiment.

1.4.1 Effect Sizes

We calculated estimates of the pooled effect sizes on reading and/or math test scores in standard deviations for each experiment that passed our inclusion restrictions. Studies reported results in a variety of ways and we had to manipulate these results in order to have comparable impacts across all experiments. Below are some of the common calculations we performed.

Scale Scores

If impacts were presented as scale score points on a test, we would divide the coefficient by the standard deviation given in the summary statistics. If no standard deviation was given in the paper, we would instead use the standard deviation from a national or norming sample.

Multiple Measures

When a study reported math or reading impacts for multiple standardized measures, we would average the impacts across all standardized measures for each subject.

Subsamples

When a study reported impacts by subsamples (e.g. by grade, by race, by cohort, etc.) and did not report pooled estimates, we would report the weighted average of the impacts across the given subsamples.

Hedge's g

When a study only reported means and standard deviations, we used this information to calculate a statistic known as Hedge's g and its corresponding standard error (see Hedges 1981 and Lipsey and Wilson 2000). In cases where studies reported impacts but did not provide enough information to estimate impacts and standard errors in standard deviation units (a common example of this was a paper reporting the impact but failing to provide any standard errors or p-values), we would instead calculate Hedge's g with reported means and standard deviations.

Standard Errors

Unfortunately, without having access to the micro-data, it was not possible to calculate the appropriate standard errors for every effect size. In an attempt to not overstate the significance of an effect size, we were overly conservative when calculating standard errors that were not already reported in a study. For example, when calculating Hedge's g, we used the number of units randomized to calculate the standard errors. Although Slavin et al. (1984) had a sample of 504 students, randomization was done at the school level (N = 6) and hence the standard errors reported in our tables were large.

In cases where p-values were given for impacts instead of standard errors, we would assume the p-value was calculated using a normal distribution and back out an estimate of the standard error.[3]

Annual Impacts

For comparability across all studies, we only used annual impacts in our meta-analysis. When a study lasted for multiple years and only reported cumulative impacts, we would divide the study by the length of the intervention to estimate annual impacts. For standard errors in this case, we divided the cumulative standard error by the square root of the length of the intervention.[4]

Other

We documented all assumptions and calculations we made for each study and these files can be obtained upon request. Unique cases that did not utilize some combination of the methods above were rare and were dealt with on a case by case basis. Note that if there was not enough information presented in a paper for us to make credible assumptions, the study was excluded.

1.4.2 Demographic Variables

For each demographic variable described below, if the paper did not provide enough information for us to determine the quantity of interest, we recorded the value as missing. Any assumptions made to calculate these variables were recorded in a text field included in the final column of the dataset.

Age

The age range of students in the experiment.

Grade

[3] Usually not enough information was given for us to estimate the degrees of freedom of a t-distribution.

[4] This follows from $\text{Var}\left(\sum_{i=1}^{N} X_i\right) = \sum_{i}^{N} \text{Var}(X_i) + \sum_{i \neq j} \text{Cov}\left(X_i, X_j\right)$ where X_i represents an impact in year i. Assuming equal variances and non-negative covariances, it follows that estimates of annual standard errors have an upper bound of $\text{SE}\left(\sum_{i=1}^{N} X_i\right) / \sqrt{N}$ (when $\sum_{i \neq j} \text{Cov}\left(X_i, X_j\right) = 0$). We chose this calculation for our estimates as it is the most conservative under these assumptions.

The grade range of students in the experiment.

Note that since studies typically only report grade or age, not both, we usually only have data on one or the other. In our analysis, for studies that are missing age data, we impute the age assuming the typical age-grade mapping of the American education system. We assign to every grade the age students typically turn in the middle of that grade (6 in kindergarten, 7 in first grade, and so forth). If preschool/early childhood experiments do not report an age, I assign them an average age of 4.5 years. Our age results are similar when using beginning of year age for the imputations.

English-Language Learner

An indicator that is 1 if a majority of the students in the experimental sample are English-Language Learners and 0 otherwise.

Disadvantaged

An indicator that is 1 if a majority of the students in the experimental sample are disadvantaged and 0 otherwise. There is unfortunately no industry standard for what constitutes "disadvantaged" and papers wildly differed in their definitions and amount of information presented to the readers. Therefore, for this data collection, we considered a sample disadvantaged if authors emphasized that a majority of students in their sample came from an environment that would lead readers to believe they are substantially below average with respect to poverty (e.g. points out they are "low-socioeconomic status", "at-risk", "low-income", majority have "free or reduced-price lunch status", etc.) or presented summary statistics that enable the reader to easily reach this conclusion.

Black

An indicator that is 1 if a majority of the students in the experimental sample are black and 0 otherwise.

Hispanic

An indicator that is 1 if a majority of the students in the experimental sample are Hispanic and 0 otherwise.

Low-Ability

An indicator that is 1 if an experiment targets students of low-ability and 0 otherwise. There is unfortunately no industry standard for what constitutes "low-ability" and papers wildly differed in their definitions and amount of information presented to the readers. Therefore, for this data collection, We considered a

sample low-ability if authors emphasized that a majority of students in their sample were substantially below average on typical achievement measures (e.g. points out a majority are "behind grade-level", only included the two worst students from every classroom, "Normal Curve Equivalent scores are below 30", etc.) or presented summary statistics that enable the reader to easily reach this conclusion.

1.4.3 Implementation Details

For each implementation variable described below, if the paper did not provide enough information for us to determine the quantity of interest, we recorded the value as missing. Any assumptions made to calculate these variables were recorded in a text field included in the final column of the dataset.

First Year of Experiment

The year the experiment was first implemented in the field.

Year of Publication

The year the paper was published. For unpublished work (such as working papers and dissertations), we report the year of the most recent version found.

Length of Treatment

The amount of time the average cohort was exposed to the experiment. During the data collection process, we record this in the same unit the author uses (days, weeks, months, semesters, years, etc.) and differentiate between academic years and calendar years. In the cleaned data, I convert all lengths to calendar years using typical assumptions (e.g. a calendar year is equivalent to 12 months, 52 weeks, or 365 days; an academic year is equivalent to 9 months; a semester is half of an academic year; a summer recess is 3 months; etc.). See the code for all conversions used.

Location

The location of the experiment. Authors varied in the amount of detail provided to readers. Although some would present the exact name of the schools, school-districts, cities, or states that their experiment was implemented in, some were more vague and only gave characterizations such as "a small rural school", if anything at all. Using the information the authors present, I created 7 location indicators (1 if an experiment took place in the given location and 0 otherwise):

- U.S.A. Northeast Region – An experiment took place in Maine, Massachusetts, Rhode Island, Connecticut, New Hampshire, Vermont, New York, Pennsylvania, New Jersey, Delaware, or Maryland.

- U.S.A. Southeast Region – An experiment took place in West Virginia, Virginia, Kentucky, Tennessee, North Carolina, South Carolina, Georgia, Alabama, Mississippi, Arkansas, Louisiana, or Florida.
- U.S.A. Southwest Region – An experiment took place in Texas, Oklahoma, New Mexico, or Arizona.
- U.S.A. Midwest Region – An experiment took place in Ohio, Indiana, Michigan, Illinois, Missouri, Wisconsin, Minnesota, Iowa, Kansas, Nebraska, South Dakota, or North Dakota.
- U.S.A. West Region – An experiment took place in Colorado, Wyoming, Montana, Idaho, Washington, Oregon, Utah, Nevada, California, Alaska, or Hawaii.
- U.S.A. National – An experiment was a national evaluation that took place across many regions.
- Foreign – An experiment took place in a country other than the U.S.A.

Note that if an experiment was not designed to be a national evaluation but spanned multiple regions, the experiment would have two or more regional indicators with a value of 1.

Number of Standardized Math Constructs

The number of standardized math outcome measures authors collect at posttest.

Number of Standardized Reading Constructs

The number of standardized reading outcome measures authors collect at posttest.

Randomization Unit

The unit researchers used for their randomization (e.g. student, school, family, classroom, teacher, etc.).

Number of Randomization Units

The number of students/schools/families/classrooms/etc. that were randomly assigned to treatment or control at the beginning of the experiment.

Number of Units Randomized into Treatment

The number of randomization units that were randomly assigned to treatment at the beginning of the experiment.

Number of Units Randomized into Control

The number of randomization units that were randomly assigned to control at the beginning of the experiment.

Number of Students in Sample at Randomization

The number of students who were randomly allocated to treatment or control at the beginning of the experiment. Note that in many cases, students were assigned to treatment or control because they were part of a larger unit that was randomized (e.g. schools, classrooms, families, etc.).

Number of Students in Sample at Post-Test

The number of students present at the end of the experiment and who have non-missing standardized math or reading outcomes.

Subject Focus of Experiment

If the focus of the experiment is math achievement , reading achievement, both math and reading, or not subject related.

Type of Publication

Whether the experiment was written up in a peer-reviewed journal, a dissertation, an unpublished working paper, a government-funded publication, a firm-funded publication, or "other".

Tutoring Hours

For experiments that had a tutoring element, the number of hours of tutoring a student would receive in 36 weeks if the rate of tutoring continued at the same pace.

Individual Tutoring

For experiments that had a tutoring element, an indicator that was 1 if tutoring was one-on-one and 0 otherwise.

2 Appendix B: Life-Cycle Model

This appendix describes the life-cycle model used in Section 4 to investigate the long term impacts of the best education experiments found through our literature search. The model draws heavily from the Social Genome Model (SGM) described in Winship and Owen (2013). However, due to the lack of source code available – even upon request – and the limited description in the available guide, our procedure for creating the dataset and running the simulation may slightly diverge from the methods in Winship and Owen (2013). We describe our procedure below so that any deviations are apparent. Further, our source code and data are available online so researchers can easily adapt the model to their own needs.

2.1 Model

The model is identical to the model described in Winship and Owen (2013) and is reiterated here.

The simple theoretical model assumes that cognitive and non-cognitive skill formation varies across an individual's life and is dependent on the stock of skills in previous stages of life. Specifically, Winship and Owen (2013) define six different life-stages: circumstances at birth (CAB), early childhood (EC), middle childhood (MC), adolescence (AD), transition to adulthood (TTA), and adulthood (AH). The model then assumes that every outcome in a given stage depends on all revealed outcomes from the stages preceding it. Formally, given a vector of circumstances at birth, CAB_i, for individual i, each outcome in the vector of early childhood outcomes, EC, is modeled as

$$EC\ Outcome_i = \beta_0^{ec} + \beta_{cab}^{ec} CAB_i + \varepsilon_i^{ec}.$$

Similarly, each of the MC outcomes is given by

$$MC\ Outcome_i = \beta_0^{mc} + \beta_{cab}^{mc} CAB_i + \beta_{ec}^{mc} EC_i + \varepsilon_i^{mc}.$$

For the adolescent life-stage we have

$$AD\ Outcome_i = \beta_0^{ad} + \beta_{cab}^{ad} CAB_i + \beta_{ec}^{ad} EC_i + \beta_{mc}^{ad} MC_i + \varepsilon_i^{ad}.$$

Outcomes when transitioning to adulthood would be

$$TTA\ Outcome_i = \beta_0^{tta} + \beta_{cab}^{tta} CAB_i + \beta_{ec}^{tta} EC_i + \beta_{mc}^{tta} MC_i + \beta_{ad}^{tta} AD_i + \varepsilon_i^{tta}.$$

And finally, adult outcomes are modeled as

$$AH\ Outcome_i = \beta_0^{ah} + \beta_{cab}^{ah} CAB_i + \beta_{ec}^{ah} EC_i + \beta_{mc}^{ah} MC_i + \beta_{ad}^{ah} AD_i + \beta_{tta}^{ah} TTA_i + \varepsilon_i^{ah}.$$

Where β_ψ^λ are the partial correlations of realized outcomes from the ψ life-stage ("0" represents an intercept) with the given LHS outcome in the λ life-stage.

2.2 Simulation

2.2.1 Data

Unfortunately, as discussed by Winship and Owen (2013), there is not yet a dataset with rich enough information that follows an individual from birth through adult outcomes. Therefore, in order to conduct simulations using the above model, we combine two well known public datasets: the National Longitudinal Survey of Youth 1979 (NLSY79) and the NLSY79 Child and Young Adult survey (CNLSY). The NLSY79 follows a nationally representative sample of 12,686 men and women who were between the ages of 14 and 22 when they were first interviewed in 1979. The sample was interviewed annually through 1994 and then biennially thereafter. The CNLSY follows all children born to the female respondents in the NLSY79.[5] These children were first interviewed in 1986 and then biennially thereafter.

Combining these two datasets together, we have a rich set of outcomes for each life-stage discussed above. From the CNLSY, we observe CAB, EC, MC and AD outcomes. From the NLSY79, we observe TTA and AH outcomes. Importantly, a subset of the CAB and AD outcomes exist in both datasets that allow us to link the datasets together in the simulation described below. Table 3 details the specific variables that were used for each life-stage and what datasets they were available in. The variables include a mix of cognitive skills (e.g. standardized test scores), non-cognitive skills (e.g. self esteem and hyperactivity indices), and important life outcomes (e.g. teen birth, drug use, and graduation).[6] Tagsets that can be used to download the raw data from the Bureau of Labor Statistics and the code used to create these variables

[5] As of the most recent survey with data available (2012), there were 11,512 CNLSY respondents ranging in age from 1 to 42.

[6] Note that in order to protect respondents' identities, the NLSY79 top-coded all income variables and the numerical cutoff for the top-code varied over the years. We first convert all income values to 2010 dollars and then re-top-code all income variables to the minimum (in 2010 dollars) top-code that the NLSY79 ever used.

from the raw data are available on this paper's companion website.[7]

We restrict the NLSY79 sample to only include the 6,111 respondents in a cross-sectional sample designed to represent the non-institutionalized civilian segment of people living in America at the time of the first interview. This drops respondents that were in a supplemental minority and poor sample (5,295 respondents) or in a supplemental military sample (1,280 respondents). Further, we limit our NLSY79 sample to respondents with valid race information.[8] Similarly, we restrict the CNLSY sample to only include children of the NLSY79 sample we defined above and who had valid race information themselves.[9] All analyses described below use unweighted data.

2.2.2 Imputation

For all outcomes, we use a simple procedure to impute values for respondents with missing values.[10] Within a dataset, we sort outcomes within each life-stage in increasing order of percent of missing responses. Starting with the youngest life-stage available in a given dataset, we then use the outcomes with no missing responses[11] as explanatory variables in a linear model[12] to predict the missing values of the outcome in that life-stage with the least amount of missing responses. We then include this newly imputed variable in the set of explanatory variables and use this set to predict the next outcome in that life-stage. We continue this procedure until all missing values in the youngest life-stage are imputed. We then include all of these variables as explanatory variables to predict the missing values of the variable in the second youngest life-stage with the least amount of missing responses. We add this imputed variable to the set of explanatory variables and continue this procedure until all values in a dataset have been imputed. After this procedure, we round all binary and categorical variables to the nearest integer.[13][14] This procedure is done separately for both of the CNLSY and NLSY79 datasets.

See Winship and Owen (2013) for a discussion of the imputation methods. Online Appendix Table 2

[7] Raw data from the Bureau of Labor Statistics can be found here https://www.nlsinfo.org/investigator/.

[8] This drops an additional 13 respondents from the sample.

[9] 5,791 children in the CNLSY had mothers from our analysis sample. Of these, 3 were dropped because they were missing race information.

[10] Missing in this case means the respondent did not have a valid response for any ages around the given life-stage. See Table 3 and the code generating the variables for the specific age ranges for each variable.

[11] In the CNLSY, race, gender, and mother's age at first birth have no missing responses. In the NLSY79, race and gender have no missing responses.

[12] Unlike Winship and Owen (2013), all outcomes are predicted using OLS. For example, this means that binary outcomes are predicted using linear probability models instead of logit or probit models

[13] The binary and categorical variables are gender, race, marital status of parents, low birth weight, high school grad status, criminal conviction, teen parent, lives independently from parents, marijuana use, other drug use, early sex, suspension, fighting, hitting, damaging property, religious service attendance, school clubs, and college completion.

[14] The continuous variables are maternal educational attainment (we impute grade), maternal age at birth, maternal age at first birth, family income, mother's AFQT score, cognitive stimulation score, emotional support score, PPVT score, math achievement, reading achievement, antisocial behavior, hyperactivity, GPA, self-esteem index, and gender role attitudes.

presents summary statistics for each outcome pre- and post-imputation.

Online Appendix Table 2: Imputation Statistics

	Before Imputation			After Imputation		
	Mean	Std. Dev.	N	Mean	Std. Dev.	N
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: CNLSY						
Male	0.517	0.500	5,788	0.517	0.500	5,788
Black	0.140	0.347	5,788	0.140	0.347	5,788
Hispanic	0.108	0.310	5,788	0.108	0.310	5,788
Other Race	0.042	0.201	5,788	0.042	0.201	5,788
Mom Age at Birth	26.257	6.030	5,787	26.259	6.032	5,788
Mom Age at First Birth	23.020	5.482	5,788	23.020	5.482	5,788
Mom Married at Birth	0.810	0.392	5,526	0.806	0.395	5,788
Family Income (Birth)	2.978	2.270	5,139	2.927	2.328	5,788
Low Birth Weight	0.078	0.268	5,180	0.077	0.266	5,788
Mom's AFQT Score	45.829	28.847	5,520	45.664	28.682	5,788
Cognitive Stimulation Score	0.000	0.999	4,593	-0.066	1.002	5,788
Emotional Support Score	0.000	0.999	4,551	-0.062	1.017	5,788
PPVT Score	0.000	1.000	2,836	-0.045	1.019	5,788
Math Achievement ($\approx$ Age 5)	-0.068	0.996	4,563	-0.091	0.997	5,788
Reading Achievement ($\approx$ Age 5)	-0.070	1.003	4,553	-0.105	0.981	5,788
Antisocial Behavior ($\approx$ Age 5)	0.009	1.002	4,930	0.019	0.989	5,788
Hyperactivity ($\approx$ Age 5)	0.021	1.004	4,944	0.046	1.012	5,788
Math Achievement ($\approx$ Age 10)	-0.007	1.005	4,430	-0.008	0.991	5,788
Reading Achievement ($\approx$ Age 10)	-0.005	0.999	4,433	-0.005	0.986	5,788
Antisocial Behavior ($\approx$ Age 10)	-0.002	1.003	4,665	0.002	0.979	5,788
Hyperactivity ($\approx$ Age 10)	-0.005	1.001	4,726	-0.009	0.987	5,788
High School Grad Status (Age 19)	0.886	0.318	3,730	0.888	0.315	5,788
GPA	2.964	0.784	4,318	2.985	0.792	5,788
Criminal Conviction	0.166	0.372	3,727	0.157	0.364	5,788
Teen Parent	0.180	0.385	3,126	0.177	0.381	5,788
Lives Independently (Age 19)	0.230	0.421	3,550	0.215	0.411	5,788
Math Achievement ($\approx$ Age 14)	0.007	0.996	4,140	0.015	0.969	5,788
Reading Achievement ($\approx$ Age 14)	0.009	0.993	4,143	0.008	0.964	5,788
Family Income ($\approx$ Age 14)	62718.443	43660.289	4,764	63243.321	44494.305	5,788
Marijuana Use	0.341	0.474	4,063	0.336	0.473	5,788
Other Drug Use	0.069	0.253	3,774	0.065	0.246	5,788
Early Sex	0.219	0.413	3,489	0.187	0.390	5,788
Suspension	0.129	0.335	5,086	0.134	0.340	5,788
Fighting	0.080	0.272	3,743	0.072	0.259	5,788
Hitting	0.203	0.403	3,745	0.188	0.391	5,788
Damaging Property	0.095	0.293	1,362	0.082	0.275	5,788
Self-Esteem Index	-0.001	1.004	3,661	-0.004	1.005	5,788
Religious Service Attendance	3.041	1.688	3,749	3.036	1.410	5,788
Gender Role Attitudes	2.063	0.496	3,382	2.056	0.488	5,788
School Clubs	0.707	0.455	2,369	0.668	0.471	5,788
Panel B: NLSY79						

Male	0.491	0.500	6,098	0.491	0.500	6,098
Black	0.118	0.323	6,098	0.118	0.323	6,098
Hispanic	0.078	0.268	6,098	0.078	0.268	6,098
Other Race	0.053	0.224	6,098	0.053	0.224	6,098
Mom Age at Birth	25.816	6.376	5,286	25.799	6.398	6,098
Mom Age at First Birth	21.642	4.613	4,377	21.714	4.550	6,098
High School Grad Status (Age 19)	0.764	0.425	5,942	0.761	0.426	6,098
GPA	2.630	0.881	4,064	2.454	0.929	6,098
Criminal Conviction	0.099	0.299	3,022	0.108	0.310	6,098
Teen Parent	0.144	0.351	6,096	0.143	0.351	6,098
Lives Independently (Age 19)	0.423	0.494	5,217	0.421	0.494	6,098
Math Achievement ($\approx$ Age 19)	0.000	0.999	5,754	-0.004	0.994	6,098
Reading Achievement ($\approx$ Age 19)	0.000	0.999	5,754	0.000	0.997	6,098
Family Income ($\approx$ Age 19)	59755.565	36149.078	5,008	59485.324	36070.492	6,098
Marijuana Use	0.504	0.500	4,382	0.503	0.500	6,098
Other Drug Use	0.225	0.418	4,375	0.225	0.418	6,098
Early Sex	0.105	0.306	5,703	0.104	0.305	6,098
Suspension	0.210	0.407	5,860	0.210	0.407	6,098
Fighting	0.230	0.421	4,407	0.237	0.425	6,098
Hitting	0.356	0.479	4,411	0.359	0.480	6,098
Damaging Property	0.175	0.380	4,380	0.176	0.381	6,098
Self-Esteem Index	0.000	0.999	3,918	0.023	1.012	6,098
Religious Service Attendance	3.020	1.674	5,986	3.020	1.660	6,098
Gender Role Attitudes	1.879	0.553	6,006	1.878	0.554	6,098
School Clubs	0.652	0.476	5,713	0.646	0.478	6,098
Family Income ($\approx$ Age 29)	54453.909	35224.495	5,654	54580.951	35744.767	6,098
College Completion (Age 29)	0.230	0.421	5,886	0.233	0.423	6,098
Lives Independently ($\approx$ Age 29)	0.885	0.319	5,851	0.884	0.321	6,098
Family Income ($\approx$ Age 40)	69373.287	44033.432	4,968	66322.348	42926.644	6,098

Notes: This table reports the means, standard deviations, and number of observations before and after imputation for each outcome used in the life-cycle simulation.

2.2.3 Running the Simulation

ESTIMATING COEFFICIENTS

Using the two imputed datasets and the equations above, we are able to estimate the coefficients for each outcome in a life-stage. However, an issue arises in linking the life-stages across these two data sources. Due to the age of respondents at first interview in the NLSY79, the data from earlier life stages is not as rich as in the CNLSY. Therefore, the NLSY79 does not contain all of the CAB, EC, MC, and AD variables that the CNLSY has. In order to overcome this, we define a set of linking variables, LINK, that contains all outcomes that are available in both the NLSY79 and the CNLSY.[15] We can then estimate the following two equations in the NLSY79 dataset to obtain coefficients for each TTA and AH outcome:

$$TTA\ Outcome_i = \beta_0^{tta} + \beta_{link}^{tta} LINK_i + \varepsilon_i^{tta}$$
$$AH\ Outcome_i = \beta_0^{ah} + \beta_{link}^{ah} LINK_i + \beta_{tta}^{ah} TTA_i + \varepsilon_i^{ah}.$$

GENERATING THE BASELINE

Once we have estimated the model with the modification necessary to link the two datasets together, we can then use the coefficients to generate a synthetic baseline by predicting the values of all variables included in our simulation for the CNLSY respondents. Starting with the set of CAB variables in the CNLSY dataset, we predict the set of EC variables using the coefficients from our estimations. Using the newly predicted variables, we then predict the set of MC variables and afterwards predict the set of AD variables. Using the CAB and predicted EC, MC, and AD variables, we then use the coefficients from the linking equations to predict the TTA and then the AH variables.

For the continuous variables in the EC, MC, and AD, we add in the residuals from the regression that estimated the partial correlations for the given variable. This leaves the predicted continuous variables in these life-stages identical to their values in the imputed datasets. As described in Winship and Owen (2013), this attempts to capture realized unobservables. For all binary and categorical variables, we again round them to the nearest integer.

PROPAGATING THE EFFECT

Given the impact of an intervention at some life-stage, we can again use the estimated coefficients from all life-stages to propagate the effects of the intervention through to adult outcomes. For example, if we have an intervention that increases the reading scores of all students at age ten by 0.5σ, we can simulate the

[15] See Table 3 for a list of these variables.

long term impacts of this intervention by increasing age ten reading scores of all students in the sample by 0.5σ and then predicting post intervention values of the AD variables using the partial correlations we found in the imputed CNLSY dataset. We then use the set of predicted LINK variables and partial correlations found in the NLSY79 to predict the post intervention TTA variables./footnoteNote that the LINK variables that occurred before age ten will be unchanged as they occurred before the intervention. Using these predicted TTA values and the same set of predicted LINK variables, we again use partial correlations from the NLSY79 to predict post intervention values of family income at age 40.

We can use similar procedures to simulate the long-term effect of multiple impacts across different life-stages or impacts that target certain subsamples. Further, we can simulate the long-term effects of interventions that target any measures that are included in our model.

Similar to when we generated the synthetic baseline, for all variables included in the CNLSY, we again add in the residuals from the regressions that estimated the partial correlations.

Calculating the Impact

After running the procedure described above for a given intervention impact (or multiple impacts at once), we then have a dataset of baseline values and post intervention values of all variables in our model. Comparing a post-intervention estimation of an outcome to the baseline estimation would then provide us with an estimated impact of the intervention on the given outcome. For example, we could compare post intervention family income at age 40 to baseline family income at age 40 to estimate the impact on age 40 family income. Further, we could also investigate the impact an intervention would have on any outcome included in our data (e.g. high school graduation rates, adolescence test scores, drug use, teenage pregnancies, family income at age 29).