The Distribution of School Spending Impacts*

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Abstract

We use estimates across all known "credibly causal" studies to examine the distributions of the causal effects of public K12 school spending on test scores and educational attainment in the United States. Under reasonable assumptions, for each of the 31 included studies, we compute the same parameter estimate. Method of moments estimates indicate that, *on average*, a $1000 increase in per-pupil public school spending (for four years) increases test scores by 0.044σ, high-school graduation by 2.1 percentage points, and college-going by 3.9 percentage points. The pooled averages are significant at the 0.0001 level. When benchmarked against other interventions, test score impacts are much smaller than those on educational attainment – suggesting that test-score impacts understate the value of school spending. The benefits to marginal capital spending increases take about five-to-six years to materialize, but after this, are similar to those of non-capital spending increases. The marginal spending impacts are much *less* pronounced for economically advantaged populations. Consistent with a cumulative effect, the educational attainment impacts are larger with more years of exposure to the spending increase. Average impacts are similar across a wide range of baseline spending levels – providing little evidence of diminishing marginal returns at current spending levels.

To speak to generalizability, we estimate the variability across studies attributable to effect heterogeneity (as opposed to sampling variability). This heterogeneity explains about 40 and 70 percent of the variation across studies for educational attainment and test scores, respectively, which allows us to provide a range of likely policy impacts. A policy that increases per-pupil spending for four years will improve test scores 92 percent of the time, and educational attainment even more often. We find suggestive evidence consistent with small *possible* publication bias, but demonstrate that any effects on our estimates are minimal.

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1 Introduction

For decades, social scientists have debated whether school spending affects student outcomes. This question is not just of academic importance, as public K–12 education is one of the largest single components of government spending (OECD, 2020). Also, Supreme Courts in several states in 2020 considered cases challenging the funding of public schools that hinge on, not just if (in some average sense), but the extent to which, in what contexts, and how reliably increases in school spending causally impact students.[1] As such, understanding if, how much, and in what contexts increased school spending improves student outcomes is of considerable societal importance.

School spending impacts likely differ across studies due to differences in context, policy implementation, and treated populations. As a result, single estimates, *however well-identified*, may not meaningfully reflect the impacts of future policies in *other* contexts (DellaVigna and Linos 2021; Tipton et al. 2020; Vivalt 2020; Bandiera et al. 2016; Dehejia et al. 2021). Without knowing how heterogeneous impacts tend to be across settings, there is no way to know how much the impacts of a particular study would generalize to a different setting (Tipton and Olsen 2018). Note that it is not the mere existence of heterogeneity that makes it difficult to make policy predictions from existing studies, rather, the difficulty stems from the lack of our understanding of that heterogeneity. Such an understanding can only be credibly obtained by examining impacts across several settings and contexts and among different populations.

Speaking to these issues, we perform a meta-analysis of all known "credibly causal" studies to quantify the averages and the spreads of the distributions of the causal effect of increased public K-12 school spending on test scores and educational attainment in the United States. This approach (a) provides pooled averages that are not driven by the particulars of any individual context or study, (b) provides *much* more precision than possible in any individual study to come to more precise conclusions, (c) facilitates more variability than available in individual studies to test new hypotheses, (d) allows one to measure and quantify the importance of treatment heterogeneity (i.e., the variability in impacts across studies not driven by sampling variability), which (e) facilitates the calculation of the a plausible range of policy impacts that one may expect to observe in new settings. In sum, this approach allows us to provide several new insights.

Hanushek (2003) reviewed 163 studies published before 1995 that related school resources to student achievement. He documented more than ten times as many positive and significant studies than would be expected by random chance if spending had no impact, and almost four times as many positive and significant estimates than negative and significant – strong evidence of a positive association between school spending and student achievement in these older studies.[2] In a meta-analysis of these data, Hedges et al. (1994) concluded that "*it shows systematic positive relations*

[1] States with cases in 2020 include Delaware, New York, Maryland, New Mexico, Illinois, and Tennessee.

[2] That is, Hanushek found that that 27 percent of these studies were positive and statistically significant and 7 percent are negative and significant. By not understanding the distribution of study impacts under the null hypothesis of no spending impacts, Hanushek (2003) did not interpret this as evidence of a positive school spending effect. It is worth noting that the older studies may have overstated statistical significance irrespective of sign. However, a general overstatement of statistical significance would not explain the over-representation of positive estimates.

between resource inputs and school outcomes." However, these older studies were observational (based on partial correlations) and therefore unlikely to reflect causal impacts that are informative for policy (Hedges et al. 1994; Jackson 2020). Understanding *causal* policy relationships requires an examination of studies that identify the *causal* impacts of school spending policies.

In the past decade there has been a notable increase in "credibly causal" papers using quasi-experimental variation (i.e., changes caused by specific identifiable policies) to identify the causal effect of school spending on student outcomes. However, (1) there are sizable differences in reported impacts across studies, (2) the reported effects are often noisy and not statistically significant, and (3) nontrivial differences across studies (how outcomes are reported, how the spending changes is measured, policy context, etc.) make it difficult to directly compare one study's findings to another. Moreover, due to heterogeneity across studies, it is unclear what impact (or range of impacts) policymakers can expect from increasing school spending by a specific amount. We seek to provide some clarity on these points by using formal meta-analytic techniques on all "credibly causal" estimates that relate school spending changes to student outcomes. This analysis not only addresses the perennial question of "does money matter?" but it also quantifies, based on the best evidence available, (a) how much, *on average*, student outcomes would improve from a policy that increases school spending by $1000 per pupil, (b) how the marginal effects differ for non-capital and capital spending, (c) how the marginal effects differ for children from low- and non-low-income families, (d) whether marginal school spending impacts vary by baseline spending levels (i.e., there are diminishing returns), (e) the extent to which estimates based on existing studies may generalize to other contexts, and (f) a range of policy impacts that can be expected in any given context.

Conducting a rigorous meta-analysis involves several steps. The first step is defining the study inclusion criteria *ex ante*. To focus on causal estimates, we require that the policy variation used in a study is a valid instrument (in the econometric sense) for school spending. We compile a list of all studies from the past 25 years that employ some kind of quasi-random or quasi-experimental policy variation in school spending and estimate impacts on student outcomes. Among these, we only include those studies that demonstrate that the variation used is plausibly exogenous (i.e., that the policy-induced changes in school spending are unrelated to other determinants of student outcomes or other policies) – this is analogous to the ignorability condition in an instrumental variables model.[3] We refer to this set of studies as "credibly causal." Because we are interested in the impacts of policies that change school spending, we focus on those "credibly causal" studies that demonstrate meaningful policy-induced variation in school spending. This second condition is analogous to the "instrument relevance" condition in an instrumental variables model.[4]

Meta-analysis is typically used for randomized experiments where there is a well-defined treatment and reporting is standard across studies. In contrast, school spending papers examine varia-

[3]This condition excludes all papers analyzed in well-known older literature reviews conducted in Hanushek (2003).

[4]Note that not all school spending *policies* lead to actual changes in school spending (due to states or districts shifting other monies around in response to policy changes). For example, local areas may decide to reduce their local property tax rates in response a state policy to provide additional money to schools districts. In such a setting, the additional state funds are used for tax savings rather being spent in the classroom. See Brunner et al. (2020) for an example of this kind of behaviour.

tion in spending based on a range of different policies and they report effects on several different outcomes in different ways. However, direct comparison across studies requires that we define the treatment and the outcomes in the same way. To this aim, we compute the same underlying empirical relationship from each study. Specifically, for each paper we capture the estimated policy impacts on K-12 per-pupil spending and the estimated impacts on outcomes. To make our estimates as comparable as possible, we (1) compute the average impacts for the full population (as opposed to particular sub-samples), (2) standardize all spending levels to 2018 CPI adjusted dollars, (3) convert all reported impacts into standardized effects, and (4), where possible, capture impacts four years after the spending change (that is, we keep student exposure to the spending increases consistent across studies). With these estimates, *for each paper*, we compute the estimated effect of a $1000 per-pupil increase in school spending (for four years) on standardized educational outcomes. That is, for each paper and outcome, we construct an instrumental variables (IV) estimate of the marginal policy-induced impact on standardized outcomes of exposure to a $1000 per-pupil spending increase (CPI adjusted to 2018 dollars) over four years. Once summarized using the same relationship, studies that are reported in starkly different ways are remarkably similar – suggesting much less heterogeneity than one might expect at first blush.

Another important innovation of our work is to propose a framework to compare the impacts of capital to non-capital spending. If school construction matters, a 40 million dollar construction project should affect student outcomes over the life of the building (about 50 years) and not just in the year the spending occurred. As such, a simple comparison of contemporaneous capital spending to contemporaneous outcomes is inappropriate and would drastically understate the marginal impacts of capital spending on outcomes. To account for this, we amortize large one-time capital payments over the useful life of the capital assets. We then relate the change in outcomes to the present discounted "flow" value to obtain the marginal impacts of capital spending. This approach leads to annual spending increases that can be compared to those of non-capital spending increases.

Speaking first to the "*does money matter?*" question, we show that 94 percent of all included studies find a positive overall effect of increased school spending (irrespective of significance). If positive and negative impacts were equally likely (as is the case if school spending did not matter), the likelihood of observing this many positive estimates or more is less that one in 4.3 *million*. Next, we quantify the magnitude of the impact of increased school spending on student outcomes using formal meta-analysis. Some are skeptical of meta-analysis outside of randomized experiments because individual studies may vary considerably due to effect heterogeneity – making a naively pooled estimate difficult to interpret. However, rather than avoiding direct comparison of studies because of heterogeneity, we seek to model and understand this heterogeneity to gain a deeper understanding of if, when, how, to what extent, and in what contexts, school spending affects student outcomes. To this aim, using the same relationship for each paper, we employ random effects meta-analysis that does not assume the existence of a single common effect, but rather explicitly estimates the extent of treatment effect heterogeneity across studies. This approach provides pooled average estimates that are robust to the inclusion of outlier and imprecise estimates,

and produces a plausible range of predicted policy impacts informed by the variability both within and across studies – see Meager (2019), Vivalt (2020), Bandiera et al. (2016), DellaVigna and Linos (2021), and Dehejia et al. (2021) for similar approaches.

Ninety-four percent of the studies with test score impacts have positive effects. The pooled meta-analytic estimate indicates that, *on average*, a \$1000 per-pupil increase in school spending (sustained over four years) increases test scores by 0.0438σ - about 1.9 percentile points. We can reject that the pooled average is zero at the 0.0001 significance level. However, almost 70 percent of the variability across studies reflects unobserved heterogeneity (due to different LATEs, policy context, etc.) so that one may observe estimates well outside this confidence interval in new settings. Based on this information, a policy *in a different context* that increased per-pupil school spending by \$1000 over a four-year period would have test score impacts between -0.0056σ and 0.093σ ninety percent of the time, and would lead to positive test score impacts 92 percent of the time.

Looking to educational attainment, our pooled meta-analytic estimate indicates that, *on average*, a \$1000 per-pupil increase in school spending increases educational attainment by 0.163σ (p-value<0.0001). This translates into a 2.1 percentage-point increase in high school graduation and a 3.9 percentage-point increase in the college-going rate.[5] In relative terms, this is a 2.5 percent increase in high school graduation and a 9.6 percent increase in college-going. All studies that examine impacts on educational attainment yield positive estimates. Heterogeneity is modest across the educational attainment studies (around 40%), so there is considerable external validity. Based on this information, a policy *in a different context* that increased per-pupil school spending by \$1000 over a four-year period would have high-school graduation impacts between 0.8 and 3.3 percentage points and college-going impacts between 1.6 and 6.3 percentage points ninety percent of the time. To better understand the education production function, we examine the cumulative impacts by comparing studies that report impacts of more versus fewer years of exposure to changes in spending. We find that educational attainment impacts increase with years of exposure to a spending increase.

To put our estimates into perspective, we benchmark our impacts against those of *other* well-known interventions with measurable effects on student test scores (such as class size reduction, or "No Excuses" charter school attendance). The \$1000 per-pupil spending impacts are on-par with these interventions. However, the benchmarked effects on educational attainment are much larger than those on test scores – suggesting that test scores may not measure *all* of the benefits of increased school resources (Card and Krueger 1992; Krueger 1998), and, more broadly, that test score impacts may only capture a portion of the overall benefits of educational inputs (Jackson 2018; Jackson et al. 2020). We also examine observable predictors of differences in outcomes. We find similar effects for studies that employ different estimation strategies – suggesting reasonable external validity irrespective of identification strategy. While the benefits to capital spending

[5] These calculations multiply the standardized impact (0.163σ) by the standard deviation of each outcome. The standard deviation of a binary variable is simply $p \times (1-p)$ where p is the proportion of positive outcomes. We use a standard deviation of high school graduation rate of 0.1275 (rate = 0.85) and standard deviation of college-going rate of 0.2419 (rate = 0.41) (Snyder et al. (2019).

increases take a few years to materialize, the average effects of increased capital and non-capital spending on test scores are similar. While we find that the impacts are quite stable along several observable dimensions, we *do* find that school spending impacts are smaller, on average, for more economically advantaged populations.

While our results accurately describe the literature, the distribution of impacts reported may not reflect the true distribution of impacts if there is publication bias. While there is no way to observe what is not observed, one *can* assess the extent to which potential publication bias impacts the estimates. To this aim, we implement several empirical approaches – removing imprecise studies (which are more susceptible to biases), employing the "trim and fill" method to impute potentially "missing studies" (Duval and Tweedie (2000)), and adjusting for a bias against non-significant effects (Andrews and Kasy 2019). In all cases, we find little evidence of significant impacts of possible publication bias. Additionally, we find no systematic differences between published and unpublished studies, or studies published in more or less selective outlets – further evidence of negligible publication bias.

Some have argued that while school spending may have mattered when baseline spending levels were low, the marginal impacts *may* be smaller at current spending levels (Jackson et al. (2016)). We test this formally with our data by examining whether the marginal impacts of school spending are larger in older studies (when national spending levels were lower) or in states with lower baseline spending levels (such as in the South). Precision-weighted models reveal that the marginal impacts are remarkably stable for a wide range of baseline per-pupil spending levels (for both test scores and educational attainment). This pattern suggest that policy impacts at current spending levels are likely to be similar to those from the past (after accounting for inflation).

This study moves beyond the question of whether money matters, and is the first to quantify the pooled average marginal impacts of a $1000 increase in per-pupil spending on student test scores and educational attainment across studies. It is also the first study to measure and quantify the range of true causal impacts supported by the existing literature. This allows us to measure the extent to which studies in this literature may estimate the same parameter, and then provide a plausible range of estimates one may observe in other contexts. We also show how one can compare the impacts of spending changes that affect students over different spans of time. Finally, we contribute to a small but growing literature (e.g., Hendren and Sprung-Keyser 2020) showing how (by carefully computing the same parameter across studies) one can combine a variety of estimates outside of randomized controlled trials to provide new and important policy insights.

The remainder of this paper is as follows: Section 2 discuses how we identify and select the studies to create our data set for analysis, Section 3 describes how we compute the same underlying parameter for each paper, Section 4 presents the formal meta-analytic methods, Section 5 presents the results including tests for publication biases, Section 6 presents evidence that there do not appear to be diminishing returns, and Section 7 concludes.

2 Data

We capture estimates from studies that employ quasi-experimental methods to examine the impacts of policy-induced changes (i.e., exogenous policy shocks) in K-12 per-pupil spending on student outcomes.[6]

2.1 Study Inclusion

Our inclusion criteria requires that the variation in spending is driven by policy and (following best econometric practice) requires that the variation used in a study is a valid instrument for school spending. That is, the policy examined must lead to meaningful changes in per-pupil school spending (the treatment), and the variation used must be exogenous to other determinants (i.e., non-school spending determinants) of student outcomes. Specifically, to be included, a study had to meet each of the following criteria:

1. The study relied on quasi-experimental or policy variation in school spending.[7] That is, the study used a quasi-experimental design (Regression Discontinuity, Event-Study, Instrumental Variables, or some combination of these) to isolate the impacts of *specific* school spending policy shocks (or features of a school spending policy) on student outcomes. Table A.2 indicates that there are no systematic differences by estimation strategy among included studies.

2. The study demonstrated that their analysis was based on policies (or policy-induced variation) that had a robust effect on school spending – enough to facilitate exploring the effect of school spending on student outcomes. That is, the study examined the effect of a particular policy that altered school spending or relied on an identifiable change in school spending caused by a specific policy. We included studies that showed a non-zero effect on spending at at least the 5 percent significance level.[8] We excluded studies of policies that did not demonstrate

[6] The two authors independently verified data captured from each study. We are currently in the process of contacting authors of all included studies to confirm our interpretation of their presented results, and we are updating accordingly.

[7] Some well-known studies are excluded based on this criterion. For example, Husted and Kenny (2000) does not rely on an identifiable change in school spending due to a policy. As they state "Our preferred resource equalization measure [...] equals the change in resource inequality since 1972 relative to the predicted change (that is, the unexplained change in inequality). A fall in this variable reflects either the adoption of state policies that have reduced districts' ability to determine how much to spend in their district or an otherwise unmeasured drop in spending inequality" (298).

[8] This corresponds to a first stage F-statistic of 3.85 for the policy instruments on per-pupil school spending. In a two-stage-least-squares (2SLS) framework, the typical threshold would be a first stage F-statistic of 10. We impose a weaker restriction. Still, some well-known studies are excluded based on this criterion. Specifically, van der Klaauw (2008) states that Title I "eligibility does not necessarily lead to a statistically significant increase in average per pupil expenditures" (750). Similarly, Matsudaira et al. (2012) do not find a robust association between the policy (Title I eligibility) and per-pupil spending and Hoxby (2001) reports that several policy parameters are not statistically significantly related to per-pupil spending. Some studies examine the effects of policies that influence school spending, but they do not report the effect of the policies on school spending in a way that allows us to construct a first-stage F-statistic. These include Downes et al. (1998), Figlio (1997), and more recently, Holden (2016).

effects on school spending (as they are, by definition, uninformative of the effects of school *spending* on outcomes).[9]

3. The study demonstrated that the variation in school spending examined was unrelated to other determinants of student outcomes, such as other policies or student demographics. That is, we include studies that provide evidence that (after including an appropriate set of controls) they make comparisons across entities with different levels of school spending but for which on average all else was equal.

To locate studies that meet this inclusion criteria, we searched for all papers on the topic published or made public since 1995. We do not look before 1995 because, based on an initial search, no studies that meet the inclusion criteria existed before 1995.[10] Empirical practices in this literature were not focused on causal estimation until the early 2000s (See Angrist and Pischke (2010) for a discussion of the "*credibility revolution*" in empirical economics). Indeed, the earliest "credibly causal" study we located was published in 2001, the majority of studies meeting this criteria were published after 2010, and the lion's share were written or published after 2015 (Figure 1). We compiled this list by starting with all studies cited in the Jackson (2020) literature review. We then supplemented this with Google Scholar searches on relevant terms (school spending and causal). We then consulted the cited papers and all papers that cite those on our list to identify other papers to possibly include. Finally, to avoid exclusion of unpublished papers or works in progress, we asked active researchers in the field to locate any additional papers beyond the list we compiled.[11] Using this approach, we identified 31 studies that met our conditions as of December 1, 2020. Where there are multiple versions of the same paper (e.g., a working paper and a published version) we use the most recent publicly-available version of each paper.[12]

2.2 Included Studies

Table 1 summarizes the 31 studies that satisfy the inclusion criteria.[13] We list the last names of the authors and the publication (or draft) year of each study (first column). We assign a unique Study ID to each of the 31 included studies (second column). Because we examine the impacts of school spending on different outcomes (test scores, educational attainment, longer-run outcomes), we include multiple entries for studies that present impacts on multiple outcomes.[14] While we examine the sign of the impacts for all studies meeting the inclusion criteria, we only

[9] We show in Tables A.8 and A.9 that our results are robust to excluding more studies based on more stringent "first-stage" significance level thresholds.

[10] Note that Hedges et al. (1994) find that "*most of the studies in Hanushek's data set are cross-sectional rather than longitudinal*" (p.12) – that is, relying on simple comparisons across locations or entities at a single point in time.

[11] This was done using a broad appeal on Twitter to a large network of economists and education-policy scholars.

[12] When studies were updated (which happened with unpublished work) we updated our database to reflect the most up-to-date version of the paper's analysis.

[13] Given the use of the same data and identification strategy, to avoid double counting we categorize Jackson et al. (2016) and Johnson and Jackson (2019) as representing a single study.

[14] Note: Baron (2020) is the only study that reports distinct effects of both non-capital and capital spending, and in this table we report the average of the test score estimates across the two spending types. For our analyses that distinguish across spending types, we include both estimates separately.

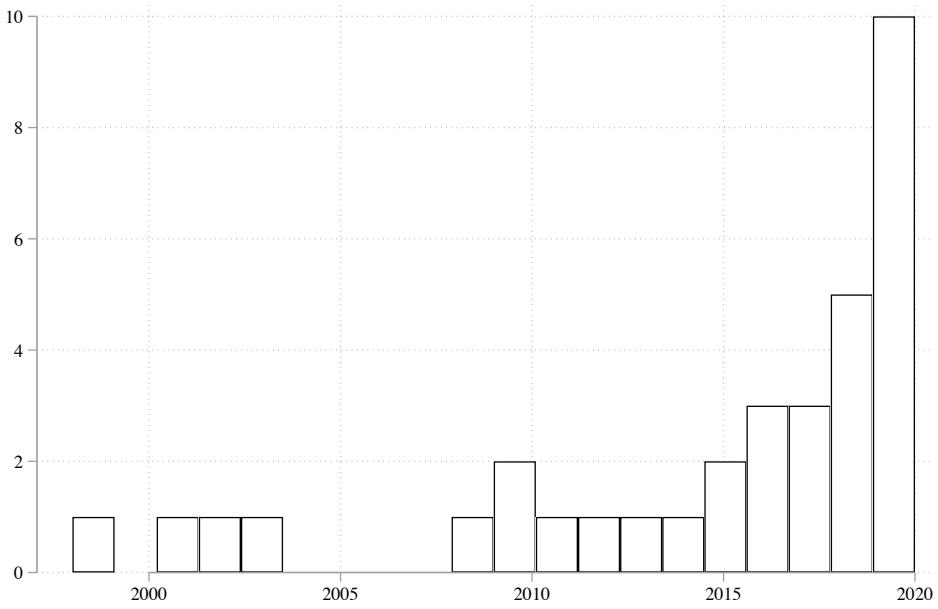


Figure 1: Count of Studies per Year

capture the estimated impacts on test scores and educational attainment for analyses that quantify the relationship between spending and outcomes; there are too few studies of *other* outcomes to provide credible pooled estimates. As such, we provide an Observation ID (third column) for test score and educational attainment outcomes only. Of 31 unique studies, 24 present estimates of test score impacts (either test scores or proficiency rates), 12 present estimates of impacts on educational attainment (dropout, high-school graduation, or post-secondary enrollment), and 3 examine longer-run impacts (wages or income mobility).[15]

Table 1 also reports the results of calculations for each paper (detailed in Section 3). For each study-outcome combination, we report the sign of the relationship between school spending and the average student outcome for the study's full sample. Positive values indicate a positive association between school spending and *improved* outcomes.[16] For test score effects, we report the sign of the impact on the *average test scores across all subjects and grade levels* reported in the study. We also report, for each Observation ID, the estimated marginal impact of a $1000 per-pupil spending increase (in 2018 dollars) sustained over four years on the standardized outcome (Effect) and its standard error (SE). The last column (Sig) reports whether the average marginal effect is statistically significant at the 5 percent level. Table A.1 presents details on the estimation strategy and spending type examined in each study.

[15] One study (Card and Payne (2002)) examines test score inequality, which is not directly comparable to other studies and therefore not included in the formal meta analysis but is included as a "credibly causal" study on the topic in the coin test analysis.

[16] For example, Lee and Polachek (2018) and Cascio et al. (2013) examine impacts on dropout rates. The reported effects are reverse-coded so that a positive coefficient indicates improved outcomes (in this case, reduced dropout).

Table 1: Summary of Studies

Study	Study ID	Obs ID	Outcome(s)	Sign	Effect	SE	Sig
Abott Kogan Lavertu Peskowitz (2020)	1	1	High school graduation	pos	0.0847	0.0876	
Abott Kogan Lavertu Peskowitz (2020)	1	2	Test scores	pos	0.1158	0.0667	
Baron (2020)	2	3	College enrollment	pos	0.2044	0.1788	
Baron (2020)	2	4	Test scores	pos	0.0308	0.0857	
Biasi (2019)	3	.	Income mobility	pos	.	.	
Brunner Hyman Ju (2020)	4	5	Test scores	pos	0.0531	0.0173	*
Candelaria Shores (2019)	5	6	High school graduation	pos	0.1435	0.0374	*
Card Payne (2002)	6	.	Test score gaps	pos	.	.	
Carlson Lavertu (2018)	7	7	Test scores	pos	0.0902	0.0475	
Cascio Gordon Reber (2013)	8	8	High school dropout	pos	1.1837	0.4388	*
Cellini Ferreira Rothstein (2010)	9	9	Test scores	pos	0.2120	0.0992	*
Clark (2003)	10	10	Test scores	pos	0.0148	0.0116	
Conlin Thompson (2017)	11	11	Test proficiency rates	pos	0.0323	0.0253	
Gigliotti Sorensen (2018)	12	12	Test scores	pos	0.0424	0.0098	*
Goncalves (2015)	13	13	Test proficiency rates	neg	-0.0048	0.0568	
Guryan (2001)	14	14	Test scores	pos	0.0281	0.0689	
Hong Zimmer (2016)	15	15	Test proficiency rates	pos	0.3265	0.1788	
Hyman (2017)	16	16	College enrollment	pos	0.1109	0.0518	*
Jackson Johnson Persico (2015), Jackson Johnson (2019)	17	17	High school graduation	pos	0.1897	0.0386	*
Jackson Johnson Persico (2015), Jackson Johnson (2019)	17	.	Years of education, Wages, Poverty	pos	.	.	
Jackson Wigger Xiong (2020)	18	18	College enrollment	pos	0.1504	0.0470	*
Jackson Wigger Xiong (2020)	18	19	Test scores	pos	0.0363	0.0104	*
Johnson (2015)	19	20	High school graduation	pos	0.3417	0.1790	
Johnson (2015)	19	.	Wages, Poverty	pos	.	.	
Kogan Lavertu Peskowitz (2017)	20	21	Test scores	pos	0.0190	0.0127	
Kreisman Steinberg (2019)	21	22	High school graduation	pos	0.1045	0.0547	
Kreisman Steinberg (2019)	21	23	Test scores	pos	0.0779	0.0237	*
Lafortune Rothstein Schanzenbach (2018)	22	24	Test scores	pos	0.0164	0.0133	
Lafortune Schonholzer (2019)	23	25	Test scores	pos	0.2330	0.1032	*
Lee Polachek (2018)	24	26	High school dropout	pos	0.4778	0.1053	*
Martorell Stange McFarlin (2016)	25	27	Test scores	pos	0.0304	0.0270	
Miller (2018)	26	28	High school graduation	pos	0.1367	0.0349	*
Miller (2018)	26	29	Test scores	pos	0.0515	0.0137	*
Neilson Zimmerman (2014)	27	30	Test scores	pos	0.0248	0.0187	
Papke (2008)	28	31	Test proficiency rates	pos	0.1652	0.0244	*
Rauscher (2020)	29	32	Test scores	pos	0.0286	0.0159	
Roy (2011)	30	33	Test scores	pos	0.3804	0.1563	*
Weinstein Stiefel Schwartz Chalico (2009)	31	34	High school graduation	pos	0.3791	0.4034	
Weinstein Stiefel Schwartz Chalico (2009)	31	35	Test scores	neg	-0.0541	0.0368	

3 Constructing The Same Parameter Estimate for All Papers

To assess a literature, one must compare studies to each other. However, unlike clinical trials, studies on school spending policy are rarely reported in ways that facilitate direct comparison. For example, Lafortune et al. (2018) report the impacts (after ten years) of school finance reforms on 4th and 8th grade test-score gaps between high- and low income districts. In contrast, Hong and Zimmer (2016) report the impacts of passing a bond referenda on test proficiency rates 1 through 13 years after bond passage in in 4th and 7th grade. While both studies report positive school spending impacts, the time frames are different, the time periods are different, the size of the spending increases are different, one study reports relative changes while the other reports absolute changes, and one study reports impacts on standardized test scores while the other looks at proficiency rates. It is unclear which study implies larger marginal school spending impacts – or even how similar the study impacts are. Because studies report effects in different ways and on different scales, or define school spending differently, we extract information from each paper that allows us to standardize estimates for comparability across papers.[17]

3.1 The Common Parameter Estimate

For each study, we compute the effect of a $1000 per-pupil spending increase (in 2018 dollars), sustained for four years, on standardized outcomes for the overall population affected by the policy. We compute separate estimates for test scores and educational attainment outcomes. We detail how we compute this empirical relationship (or parameter estimate) for each study. Because studies do not all report impacts in this form, computing this parameter requires several steps. We lay out these step and any additional required assumptions in the following subsections. We will show that none of these assumptions change our final conclusions in any appreciable way.

Step 1: Choice of outcomes

We report effects on student achievement (measured by test scores or proficiency rates) and educational attainment (measured by dropout rates, high school graduation, or college (postsecondary) enrollment). If multiple test-score outcomes are reported (e.g., proficiency rates and raw scores) we use the impacts on raw scores. This allows for standardized test score effects that are more comparable across studies, and avoids comparing impacts across thresholds of differing difficulty (i.e., where some areas have higher proficiency standards than others).[18] For the educational attainment outcomes, we capture impacts on high-school completion measures and college enrollment. For studies that report multiple of these measures, we take the highest level reported.[19]

[17] We detail the information we capture from each paper in Table A.5.

[18] In one case, Kogan et al. (2017), multiple raw score effects were reported. We took the estimates for the preferred outcome indicated by the authors.

[19] For example, if effects are reported for college enrollment and high school graduation, we take the college enrollment effects. Similarly, if effects are reported for high school graduation and high school dropout, we take the high-school graduation effects. This particular decision rule is further justified because: (a) dropout rates are

Step 2: Computing Population Average Treatment Effects

For much of our analysis, we seek one estimate per outcome per study. When studies report estimates for multiple specifications, we capture estimates from the authors' preferred specification. When there is a reported overall estimate across all populations (e.g. high-income and low-income), all subjects (e.g. Math and English), and all grade levels (e.g., 8th grade and 4th grade), we take the overall estimate as reported in the study. When studies report effects by subject, grade level, or population, we combine across estimates to generate an overall estimate and standard error for analysis.[20] When we combine test score effects across subjects for the same grade, we assume these stem from the same population and use the simple average as our overall effect.[21] [22] We combine test score effects across grade levels using a precision-weighted average.[23] When we combine test score or educational attainment effects across populations (i.e., high- and low-income), we use the population-weighted average (i.e., put greater weight on the larger population) as our overall study effect.[24] This ensures that our overall estimate is as representative as feasible of what the effect would be for the entire population, and facilitates comparison across studies.

Step 3: Standardize the Effect on the Outcome

Studies report effects on test scores with different scales, and may report impacts on different outcomes (e.g. district proficiency rates or high school completion). To facilitate comparison across studies, we convert each study's estimated effect into student-level standardized units if the effect is not already reported in these units. When effects are not reported in student-level standardized units, we divide the reported effect, Δy, by the student-level standard deviation of the outcome to

notoriously difficult to measure (Tyler and Lofstrom 2009) and therefore a less reliable measure of educational attainment, and (b) different entities often measure dropout rates is very different ways.

[20] Note that we estimate our main models across a range of assumed correlations in Section A.4. These have little effect on our main results.

[21] We follow Borenstein (2009) Chapter 24 to compute the standard error of the average effect, and assume a correlation of 0.5 when combining subjects for the same grade.

[22] In the single paper (Baron (2020)) that presents impacts for two separate types of spending (non-capital and capital) on one outcome (test scores), we use the simple average of the impacts of both spending types as our single overall effect for the coin test analysis; we include both (non-capital and capital) distinct estimates of effects on test score outcomes for our meta-analysis. To compute the standard error of the overall test score effect for Baron (2020) we assume a correlation of zero.

[23] Precision weighting is a way to aggregate multiple estimates into a single estimate with the greatest statistical precision. Instead of a simple average, this approach more heavily weights more precise estimates (i.e., placing more weight on the estimate that is the most reliable). We follow Borenstein (2009) Chapter 23 to compute the standard error of the precision-weighted average as the reciprocal of the sum of the weights (inverse variances). This calculation of the standard error assumes a correlation of zero between the estimates.

[24] We follow Borenstein (2009) Chapter 24 to compute the standard error of the average effect, and assume a correlation of zero when combining outcomes for different populations. We use the relative sample sizes reported in the study to weight. For example, in Lafortune et al. (2018) we combine the estimates for the top and bottom income quintiles (using the relative sample sizes) and assume a correlation of zero between these estimates. We make an exception in one case: Cascio et al. (2013) report dropout rate estimates for Black and White students. For this study we population-weight by an estimated share White = 0.9 and share Black = 0.1 rather than the 0.68/0.32 shares reported for the study sample.

capture the estimated effect on the outcome in student-level standard deviation units (i.e. σ_y).[25]

Step 4: Equalize the Years of Exposure

Because education is a cumulative process, one would expect larger effects for students exposed to school spending increases for a longer period of time. Indeed, we show evidence of this empirically in Section 5.4. To account for this, we standardize all our effects to reflect (where possible) the effect of being exposed to a spending increase for four years. Several studies report the dynamic effects of a school-spending policy (i.e., the effect over time). For test scores, when the dynamic effects are reported, we take the outcome measured four years after the policy change.[26] Some papers do not report dynamic effects, and only report a single change in outcome after a policy-induced change in spending. In such cases, we take the average reported effect.[27] Because high school lasts four years, many papers report the effect on educational attainment of four years of exposure, but not all do.[28] [29] We adjust the outcomes to reflect four years of exposure by dividing the overall effect by the number of years of exposure and then multiplying by four. We test the assumption that the educational effects increase linearly with years of exposure, and find that it holds empirically in Section 5.2. Formally, the standardized four-year effect is $\frac{\Delta y_{4-year}}{\sigma_y}$.

Step 5: Equalize the Size of the Spending Change

Each included study isolates the effect of the policy on spending (and that of the policy on outcomes) from other potential confounding factors and policies. We seek to determine the change in outcomes associated with a particular change in per-pupil spending. To ensure comparability of dollar amounts across time, we adjust reported dollars in each study into 2018 equivalent dollars using the Consumer Price Index (CPI).[30] Because we measure the impacts of exposure to four years of a spending change, we relate this four-year outcome effect to the change in spending during these same four years. For each study we collect the *average* change in per-pupil spending over the

[25] To perform this standardization, we need to gather information from each paper on the standard deviation of the outcome of interest. This standard deviation is generally reported in summary statistics. In some cases, the standard deviation may be reported at the school or district level. In such cases, **we convert the school- or district-level standard deviation into a student-level standard deviation** by dividing the school or district-level standardized estimate impacts by the square root of the school or district size. For binary outcomes such as proficiency rates, graduation rates, or college-going rates, we use the fact that the standard deviation of a binary variable is simply $p \times (1-p)$. In the three studies that report on graduation rates for relatively old samples, Jackson et al. (2016), Johnson (2015) and Weinstein et al. (2009), we adjust estimated effects using the standard deviation of graduation rates that prevailed at that time from national aggregate statistics (0.77), rather than using the standard deviation reported for the study sample. This choice make studies more comparable by using the same standardization across studies of the same outcome and time period.

[26] Note that some papers may refer to this as a year-three effect when the define the policy year as year zero, while other may refer to this at the year four effect if policy year is year 1.

[27] In many cases, the average exposure is less than four years so that (if at all) we may *understate* the magnitude of any school spending effects for these studies.

[28] Papers that report effect for years of exposure other than 4 are: Abott et al. (2020), Jackson et al. (2016)/Johnson and Jackson (2019), and Kreisman and Steinberg (2019).

[29] We capture the effect of referendum passage on college enrollment 8 years post-election in the case of Baron (2020) to ensure comparability with the other studies which report on the same outcome.

[30] We adjust based on the article's reported $ year, and the last year of data if no $ year reported.

four years preceding the observed outcome, $\Delta(\$1000_{4-year,\$2018})$.[31] When the effect of spending on outcomes is directly reported in a study, we record this estimate directly. See Section 3.2 for a detailed description of accounting for capital spending.

Step 6: The Standardized 4-Year $1000 Spending Effect

After standardizing the impact of the policy on both student outcome(s) and per-pupil spending, for each study we compute the change in the standardized outcome per $1000 policy-induced change in school spending (averaged over four years and in 2018 dollars). Formally, our standardized effect for outcome y is $\mu_y = [\frac{\Delta y_{4-year}}{\sigma_y}]/\Delta(\$1000_{4-year,\$2018})$. This parameter estimate is computed for each study j, denoted μ_{yj}, and is comparable across different studies.[32] μ_y can be interpreted an Instrumental Variables (IV) estimate of the marginal impacts of school spending on outcomes using the exogenous policy-induced variation in school spending as the instrument. This calculation implicitly imposes a linearity assumption such that a policy that increases spending by $500 per-pupil would have a smaller effect than a policy that increases per-pupil spending by $1000. We show that this assumption is supported by the data in Appendix A.5.

To illustrate the importance of computing the same parameter from each paper, consider the following two papers: Lafortune et al. (2018) report that the "*implied impact is between 0.12 and 0.24 standard deviations per $1,000 per pupil in annual spending*" while Clark (2003) reports that "*the increased spending [...] had no discernable effect on students' test scores*", reporting small positive statistically insignificant impacts. At first blush, these two studies suggest very different school spending impacts. However, when compared based on the same empirical relationship, the papers are similar. Specifically, precision aside, μ_{yj} for Clark (2003) is 0.0148σ. By comparison, the large positive impact in Lafortune et al. (2018) is based the change in the **test-score gap** between high- and low-income groups (a *relative* achievement effect) over ten years. Using their estimates of absolute overall test score impacts over 4 years, yields a μ_{yj} of 0.0164σ.[33] Remarkably, despite the two studies coming to **very** different conclusions and reporting their results is very different

[31] For a policy that leads to a permanent shift in spending, the *total* four-year change in spending would be 4 times the permanent shift and the *average* would simply be the permanent shift. However, because spending can vary across years following policy enactment, the duration of exposure and duration of the policy may not be the same. In these cases, we use the average increase in spending during the preceding four years. For example, a policy may have increased per-pupil spending by $100 in the first year, and increased linearly up to a $400 increase in the 4th year. In this case, we would use the *average* increase in spending during the four years, which is $250. If a study does not report spending change in the four years preceding the observed outcome, we capture the change in spending and the contemporaneous measured outcome. This decision likely *understates* the true spending effect because these models may not account for the benefit of spending in previous years.

[32] We also capture the associated standard error of the estimate. When studies report the effects on spending and then on outcome, our standardized effect μ is a ratio of two estimates: the estimated change in the outcome divided by the estimated change in spending. In these cases, where studies report the effect of a *policy* and not of a specific *dollar change*, we must account for this in computing the standard error. We follow Kendall et al. (1994) and use a Taylor expansion approximation for the variance of a ratio.If β and δ are both estimates, if $Cov(\beta, \delta) = 0$, the standard deviation of $\frac{\beta}{\delta}$ is approximately $\sqrt{\frac{\mu_\beta^2}{\mu_\delta^2}[\frac{\sigma_\beta^2}{\mu_\beta^2} + \frac{\sigma_\delta^2}{\sigma_\beta^2}]}$.

[33] In their study, using relative absolute versus achievement gains matters. Specifically, they report test-score *declines* for high-income areas which makes the relative gains larger but the absolute gains smaller.

ways, when compared based on a common parameter, they are, in fact, remarkably similar.

3.2 Making Capital Spending Comparable to Non-Capital

A key contribution of this work is to provide a framework, informed by theory, to allow for a direct comparison of the marginal impacts of capital spending to those of non-capital spending. Increases in non-capital spending go toward educational inputs that are used in the same year (such as teacher salaries or transportation fees). In contrast, because capital spending goes toward durable assets that are used for years after the initial financial outlay, it is inappropriate to relate outcomes in a given year to spending on capital *that same year*. To account for the difference in timing between when capital spending occurs and when the inputs purchased may affect outcomes, we use the annualized accounting value of the one-time increase in spending.

To assess the value of $1000 in capital spending comparable to the same in non-capital spending requires some reasonable assumptions. Specifically, it is clear that a one-time (i.e. non-permanent) $1000 increase in spending to hire an additional teacher for a single year may be reflected in outcomes in that year. In contrast, such spending on a building should be reflected in improved outcomes for the life of the asset. In a simplistic case, where the asset does not depreciate (i.e., there is no wear and tear and the asset is equally valuable over its life), one would distribute the total cost of the asset equally over the life of an asset. For example, if the life of a building is 50 years and the building costs $25,000,000, the one-time payment of $25,000,000 would be equally distributed across the 50-year life span and be equivalent to spending $25,000,000/50=$500,000 per year. Note that, with no depreciation, for a school of 600 students, this seemingly large one-time payment of $25M would be equivalent to $500,000/600= $833.33 per-pupil per year.

In a more realistic scenario with depreciation, during the first year of a building's life, it is more valuable than in its 50th year, due to wear and tear and obsolescence. In our example, the first year's value would be greater than $500,000 and the last year's value less than $500,000. To account for this, we follow convention in accounting and apply the depreciated value of capital spending projects over the life of the asset. We assume annual depreciation of 7%, representing the asset losing 7% of its value each year. We depreciated expenses that went primarily to new building construction or sizable renovations over 50 years.[34] We depreciate expenditures of less durable assets (such as equipment or upgrading electrical wiring for technology) over 15 years.[35] For studies that report the proportion of capital spending that went to new building construction, we depreciate the capital amount proportionally between 50 and 15 years.[36] In Section A.4 we

[34] In 2013-14, the average age of school buildings in since original construction was 44 years (NCES 2016). Studies report on building age, including: Lafortune and Schonholzer (2019) (44.5 years), Martorell et al. (2016) (36 years), and Neilson and Zimmerman (2014) (well over 50 years).

[35] For example, Martorell et al. (2016) report that most of the spending went to renovations, and Cellini et al. (2010) provide an example of specific capital projects funded by a bond referenda, including to "improve student safety conditions, upgrade electrical wiring for technology, install fire doors, replace outdated plumbing/sewer systems, repair leaky rundown roofs/bathrooms, decaying walls, drainage systems, repair, construct, acquire, equip classrooms, libraries, science labs, sites and facilities..." (220).

[36] The coding of capital papers is described in Appendix Table A.6.

show that our main conclusions are robust to using lower and upper bounds of years depreciated.

For each study of capital spending, we compute the change in student outcomes for each $1000 in average flow value of the capital spending in the years preceding the measured effect.[37] We illustrate this depreciation in Figure 2, which shows the 15-year depreciation of a $7,800 per-pupil ($4.7 million per school) expenditure (as in Martorell et al. (2016)) and the 50-year depreciation of a $70,000 per-pupil ($42 million per school) expenditure (as in Neilson and Zimmerman (2014)). This transforms the extraordinarily large one-time expenditure over the projected life of the asset, which falls in value over time (e.g., an old building will need repairs as it ages). After computing the flow value of the capital outlay for each year after initial payment, we can relate observed student outcomes associated with the average depreciated value of the asset in those years.

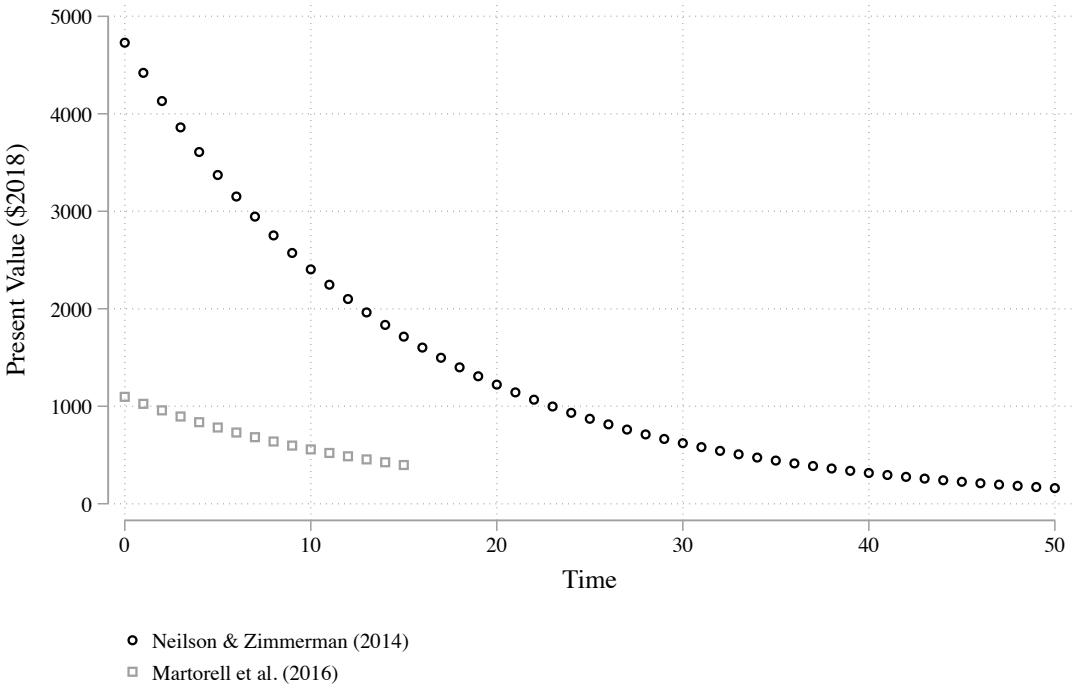


Figure 2: Exemplar Capital Expenditure Depreciation

Accounting for construction time

Because the typical capital project does not lead to contemporaneous changes in classroom experiences, it is reasonable to expect any possible improvement to take several years to materialize after the capital outlay. Indeed, large capital projects that involve entirely new construction or major upgrades to a new wing of a building can take multiple years to complete. Moreover, capital projects

[37] Depreciating the asset puts more value on the early years when test scores are measured and less on the years for which outcomes are not measured (many studies do not evaluate what the effect is more than 6 years after the funds are used). Because our parameter includes the spending change in the denominator, this reduces the reported school spending effect relative to not depreciating the asset. Accordingly, our approach may be considered conservative.

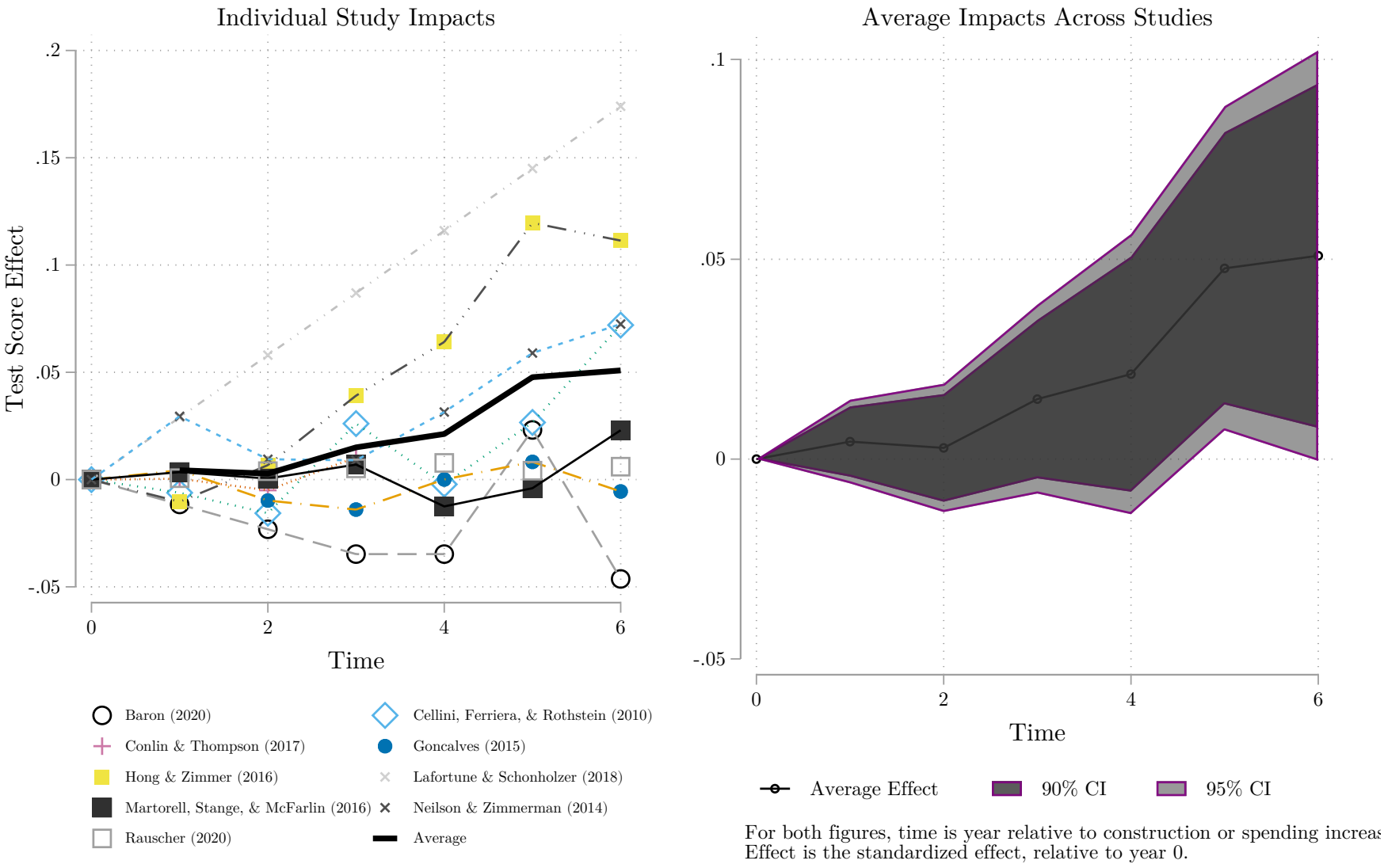
often entail some temporary disruption to everyday operations during the renovation/construction period, which may be deleterious to student outcomes. For these reasons, we assign the first two years of a capital spending project to a "construction/adjustment period" and measure outcomes six years after the increase in capital spending.[38]

To assess whether this temporal decision is reasonable, Figure 3 presents the dynamic effects of the 9 studies estimating changes in capital spending on student test score outcomes. The left panel plots for each study the *raw* effects, not the marginal per-$1000 effects, relative to a baseline year zero (t = 0) in which there should be no effect of the policy. Each of these studies report effects over time (relative to the year of the construction or the policy change).[39] Consistent with an initial disruption, in several cases there is an immediate dip in outcomes. And consistent with long-run benefits to capital spending, this initial dip is followed by a gradual increase in outcomes in most studies. By about 4 or 5 years after a capital spending increase, one observes improved outcomes in most cases. To more formally assess the evolution of outcomes over time, we present the average dynamic effect in the right panel of Figure 3.[40] We plot the average (across the nine studies) effects 1 through 6 years after the capital project or construction along with the 90 and 95 percent confidence intervals. Figure 3 shows the same per-study pattern of no change (or possibly a slight dip) in the first two years and then improving outcomes after about 5 or 6 years. Indeed, one rejects that the effect of capital spending is zero at the 5-percent level by year five. This pattern validates our assigning the first two years of these studies to a "construction/disruption" period and use of the six-year effect for capital spending increases as most comparable to non-capital spending four-year effects. Overall, the pattern indicates that (a) capital spending *does* improve outcomes on average, and (b) the benefits take between 4 and 6 years to materialize. We present more formal statistical tests in Section 5 that quantify the extent to which capital spending may affect outcomes.

[38] For eight of nine papers, the six-year estimate of the effect of capital spending changes on students is provided. When the six-year effect is not reported, we use the latest year reported. Conlin and Thompson (2017) reports only 3 years after spending shock, so we capture their three-year effect as our estimated effect. As a conceptual matter, if capital spending does not improve student outcomes over both the four-year and the six-year effects, the impacts of spending would be zero. This distinction matters only if one rejects the null hypothesis of zero spending impacts.

[39] For Lafortune and Schonholzer (2019), Neilson and Zimmerman (2014), Goncalves (2015) year one (t = 1) represents first year of occupancy at a new or renovated school. For all other studies, year one (t = 1) represents the first year after a capital bond was passed. In the case of Conlin and Thompson (2017) year one is the first year of program eligibility.

[40] Figures A.2 and A.3 aggregate effects over time by precision and random effects weightings.



For both figures, time is year relative to construction or spending increas
Effect is the standardized effect, relative to year 0.

Figure 3: Capital Spending Effects Over Time

4 Meta-Analytic Methods

We seek to quantitatively describe the distribution of school spending impacts on test scores and educational attainment. While the simple average and standard deviation of the study impacts (μ_{yj}) provides information about the distribution impacts, this approach can be very misleading in two important ways that a formal meta-analysis can address.

First, while the simple average across studies is an unbiased estimate of the center of the distribution of impacts, an inverse-variance weighted average is the minimum variance unbiased estimate (Hedges (1983), Hartung et al. (2008)). Intuitively, when forming an average, one would place less weight on less reliable estimates (i.e., those which have larger standard errors due to underpowered methods or small samples) and more weight on those that are more precisely estimated.[41] As such, the inverse-variance weighted average (or precision-weighted average) will be a more reliable measure (i.e., less sensitive to imprecise outliers) of the center of the distribution of impacts.

Second, because of sampling variability, the spread of the raw estimates may drastically overstate the spread of the distribution of *true* impacts. To inform policy, one must know how much of the spread across studies can be attributed to sampling variability (i.e., the chance variability across studies that is due to the choice of sample) versus real contextual differences (due to different treated populations, different policy types, and different estimation strategies) across studies. Understanding the role of these contextual differences is critical to being able to predict what one might expect to observe in a new context, and a failure to account for cross-study heterogeneity could lead to overconfidence in the ability to extrapolate to other settings.

To address both these limitations, we perform random effects meta-analysis to generate overall pooled estimates of the average effect of spending on student outcomes and the estimate the extent of heterogeneity across studies. We detail this approach below.

4.1 The Formal Model of the Distribution of Study Impacts

Where μ_{yj} is the observed effect of a \$1000 spending increase over four years on outcome y = {test scores, educational attainment} in study j, each study-outcome can be represented as in (1).

$$\mu_{yj} = \theta + \delta_j + \epsilon_j \tag{1}$$

In (1), θ is the pooled average effect across all studies (not necessarily the effect estimated by any individual study). There are two reasons that a study estimate would deviate from this average. The first is sampling variability (or within-study error) represented by δ_j. The second is treatment effect heterogeneity (or the between-study error) represented by ϵ_j. Where $\sigma^2_{\mu,yj}$ is the within-study variance for study j, and τ^2 is the variance of the study-specific deviations from the pooled mean, the study impacts are distributed around a grand mean θ with variance $\sigma^2_{\mu,yj} + \tau^2$.

[41] This logic assumes that the precision of an estimate is unrelated to the the study estimate. In section 5.5 we show that this is reasonable in our setting.

Where $\sigma^2_{\mu,yj}$ is treated as known and approximated by the squared standard error, $se^2_{\mu,yj}$, one can estimate τ^2 empirically by method of moments. Specifically, the estimated heterogeneity parameter $\hat{\tau}^2$ is identified based on the difference between the observed variability across studies and that which would be expected due to sampling variably alone.[42] Intuitively, if the confidence intervals for the individual studies tend to overlap it would suggest that τ is small, while non-overlapping intervals would suggest heterogeneity. Accounting for both sources of variability, the optimal inverse-variance weighted average across all J studies is $\hat{\theta}_{pw} = \frac{\sum \mu_{yj} w_{yj}}{\sum w_{yj}}$, where each study receives weight w_{yj} as in (2).

$$w_{yj} = \frac{1}{(\sigma^2_{\mu,yj} + \tau^2)} \tag{2}$$

To form the empirical analog of (2) and therefore $\hat{\theta}_{pw}$, one can use the square of the standard error ($se^2_{\mu,yj}$) as an estimate of $\sigma^2_{\mu,yj}$, and then estimate τ^2 by method of moments. The variables μ_{yj} and $se^2_{\mu,yj}$ come from the individual studies, while the parameters τ^2, $\hat{\theta}_{pw}$, and the standard error of the weighted average ($se_{\hat{\theta}_{pw}}$) can be estimated. We estimate this random effects model using weighted least squares, with inverse-variance weights. We estimate standard errors using robust variance estimation (RVE), a meta-analytic analog to the heteroskedasticity and cluster-robust standard errors (Hedges et al. 2010). Following best practice, we use small-sample corrections, including degrees-of-freedom adjustments, that result in confidence intervals with good coverage, even with fewer than ten studies (Tipton 2015).[43] Another helpful parameter from this estimation is the relative amount of between-study heterogeneity. This is referred to as I^2, and is the ratio of the variance of the between-study heterogeneity and the overall variance (reported in regression tables as % Cross-Study Var.).[44]

4.2 Confidence Intervals and Prediction Intervals

To answer "does money matter?," one can test the hypothesis that the average pooled effect is zero. For this, one would use the standard error of the estimate to form a t-test.[45] Similarly, one can use the standard error of the mean to compute a confidence interval for the pooled average.

$$CI = \hat{\theta}_{pw} \pm t^* \times se_{\hat{\theta}_{pw}} \tag{3}$$

[42] Formally, where $\tau = 0$, the precision-weighted grand mean is M$=\frac{\sum \mu_{yj}(1/se^2_{yj})}{\sum(1/se^2_{yj})}$. The sum of the standardized square deviations from M across studies is $Q = \sum_1^J (\frac{\mu_{yj} - M}{se_{yj}})^2$. Importantly, Q follows a χ^2 distribution with an expected value of degrees of freedom (df), which is the number of studies j minus 1. As such, $Q - df$ measures the extent to which the observed dispersion is greater than can be explained by sampling variability alone. This forms the basis for an estimate of τ^2. That is, the method of moments estimate of τ^2 is $(Q - df)/C$, where C is a factor based on the study weights used to compute Q. Dividing by C reverses this process so that the τ^2 units are the same as those used in the studies (see Borenstein et al. (2017) for a full derivation).

[43] We implement these estimators using the 'robumeta' package in Stata (Hedberg et al. 2017).

[44] Where $\tilde{\sigma}^2_\mu$ is a precision-weighted average of the individual within-study variances, $I^2 = \hat{\tau}^2/(\tilde{\sigma}^2_\mu + \hat{\tau}^2)$.

[45] All tests in this paper use the t-distribution with the appropriate degrees of freedom adjustment.

This confidence interval pertains to the pooled average across all studies and *does not* account for treatment heterogeneity (i.e., that different studies provide estimates of different true causal impacts). As such, it does not provide a sense of what to expect in *future* studies. For this, one would form a prediction interval that includes both sources of error. The prediction interval is given by equation (4).

$$PI = \hat{\theta}_{pw} \pm t^* \times \sqrt{se^2_{\hat{\theta}_{pw}} + \hat{\tau}^2} \tag{4}$$

The prediction interval is necessarily wider than the confidence interval because it also accounts for heterogeneity across studies. It represents the range of values that one can expect to observe in a randomly sampled new study. This is an important policy parameter. That is, while one can be near certain that, *on average*, policies that increase school spending improve student outcomes, policymakers may wish to know how likely they are to see a positive effect of a future policy in their particular context. While the confidence interval speaks to the former, the prediction interval speaks to the latter. We will discuss both as we interpret the results.

5 Results

5.1 Does School Spending Matter? The Coin Test

Before quantifying the extent to which increased spending affects outcomes, we perform a simple count-based test of whether the causal evidence supports the notion that increased school spending improves student outcomes. It is well-known that the standard vote-count approach (i.e., counting the share of *statistically significant* effects above some pre-specified threshold) is inconsistent (Hedges and Olkin (1980)). We present an alternative counting approach that does not suffer these consistency problems problems. Our test is based on simple counts of positive and negative estimates, irrespective of statistical significance. The value of this test is that it is intuitive, and can be used when little is known about a study other than the sign of the study impacts.

Using the measures of school spending impacts outlined above, we classify each study as revealing a positive or negative effect of school spending on outcomes. To be conservative (i.e., stacking the deck *against* finding a positive association), studies that examine impacts on multiple outcomes are classified as negative if the average impacts for any single outcome is negative (even if the impacts on all other outcomes is positive).[46] This test is based on the notion that if studies are independent and there were no association between school spending and student outcomes, half the studies would be positive while half would be negative. As such, the probability of observing X positive estimates out of N studies follows a binomial distribution with probability $p = 0.5$. Given the similarity to a series of fair coin tosses, we will coin the term the "*coin*" test.

Across all 31 studies, 29 report positive impacts of school spending on student outcomes. If there were no relationship between spending and outcomes, the likelihood of observing 29 or more

[46] Using this conservative definition, we classify Weinstein et al. (2009) as negative even though they find positive impacts on educational attainment.

positive effects out of 31 is 1 in 4,320,893. This is the same likelihood of flipping a fair coin (i.e., a coin that has a 50/50 chance of head or tails) 31 times and and getting 29 (or more) heads. While one could quibble with this test on the grounds that each study is not a purely independent draw (since some studies examine overlapping policy changes), this is compelling evidence of a positive test score effect on student outcomes – that is, evidence that, on average, policies that increase school spending improve student outcomes.[47]

We present the same analysis by outcome in Table 2. For each specific outcome, the number of available credible studies is more limited, which leads to a lower level of confidence about the relationship. Despite this, for all outcomes, most papers find positive impacts of school spending. Of 24 studies that look at test scores, 22 find that increased school spending increases educational attainment. If there were no effect, observing this high a number of positive studies (or more) would occur with probability 1 in 55,738 – extremely unlikely. Of the 13 studies that estimate effects of school spending on educational attainment (dropout, high school completion, or college going), all 13 find that increased school spending leads to increased educational attainment. If there were no effect, this high number of positive studies would occur with probability 1 in 8,192 – compelling evidence that specific policies that increase school spending improve students' educational attainment. The final outcome studied is adult earnings. All three independent studies that link changes in school spending to adult earnings find positive impacts. With only 3 studies, there is the possibility that this occurred by chance. Even so, if there were no effect, this high a number of positive studies would occur by chance with probability 1 in 8. In sum, the pattern of results is statistically incompatible with the notion that "money does not matter" and provides overwhelming evidence that policies that increase school spending improve student outcomes *on average*. We next explore the extent to which school spending improves outcomes.

Table 2: Coin Test by Outcome Examined

Outcome	Papers	Positive	Positive & Significant	% Positive	1 in X Chance
All Studies	31	29	14	0.94	4320893
Test Score	24	22	9	0.92	55738
Educational Attainment	13	13	7	1	8192
Wages (income mobility)	3	3	2	1	8

5.2 How Much Does School Spending Matter?

To assess the extent to which school spending matters, we examine the distribution of the school spending effects for each outcome type across all studies for which they can be computed. We first present a forest plot of all the estimates and describe the distribution of raw estimates with simple averages and medians. We then provide the more rigorous analysis of the center and spread of

[47] In Appendix Section A.4, we present our main meta-analytic models using the conservative approach of clustering estimates from studies based on the same policies as if they came from the *same* study. This adjustment increases the precision of our estimates, tightening the prediction interval of the likelihood of positive effects from 92 to 96 percent.

the distributions of causal impacts, along with a discussion of the importance of treatment effect heterogeneity, based on random-effects meta-analysis.

We present a forest plot for the test score and educational attainment impacts separately in Figure 4. For each included study that examines impact on test scores or educational attainment, we plot the estimate of a $1000 increase in per-pupil school spending (2018 CPI adjusted) sustained over four years. We also plot the 95% confidence interval associated with each study estimate. Studies are presented in descending order by the estimated impact. To show the meta-analytic results visually, the 90% confidence interval for the pooled average is in pink, and the 90% prediction interval for a new study in a different context is in blue.

Test Score Impacts

The forest plot in the top panel of Figure 4 indicates that the most precisely estimates studies are those in the middle of the distribution, and the most imprecise estimates tend to be at the extremes of the distribution of effects. This pattern suggests that the most reliable estimates are near the median of the distribution. The 25th percentile of school spending effects on test scores is 0.0219 and the 75th percentile is 0.1405. This range of estimates underscores (a) that school spending effects are largely positive, and (b) it is important to look at the literature as a whole to gauge magnitudes of impacts. The simple average of the estimated test score effects is 0.0826σ, while the median is 0.0394σ. The fact that the median is notably smaller than the mean suggests that some studies with large effects are inflating the average. Given that these largest estimates are also the least precise suggests that a precision-weighted average may be more appropriate than a simple average.

Test Score Meta-regression Results

We present meta-regression results in Table 3. We report the pooled average impacts for all test score studies in column (1), for non-capital spending on test scores in column (2), capital spending on test scores in column (3), and the effects of non-capital spending on educational attainment in column (4). For each model, we report the pooled effect in addition to the standard error of the pooled effect. Importantly, we also report τ, an estimate of the between study variability – which is critical to helping estimate what one could expect in *other* settings.

Looking at our preferred test score estimate (column 1), the pooled effect across all studies implies that a $1000 increase in per-pupil spending (in 2018 dollars and sustained over four years) would increase average test scores by roughly 4.4 percent of a standard deviation. As one might have expected based on the forest plot, this is very slightly larger than the median across all studies and smaller than the simple average. The 95 percent confidence interval for this pooled average lies well above zero and is between 0.026σ and 0.062σ. Note the narrower 90 percent confidence interval for this pooled average is depicted in pink in Figure 4.

While the model indicates that the pooled average of the impacts is greater than zero (at the one-tenth of one percent significance level), this does not mean that one should expect positive

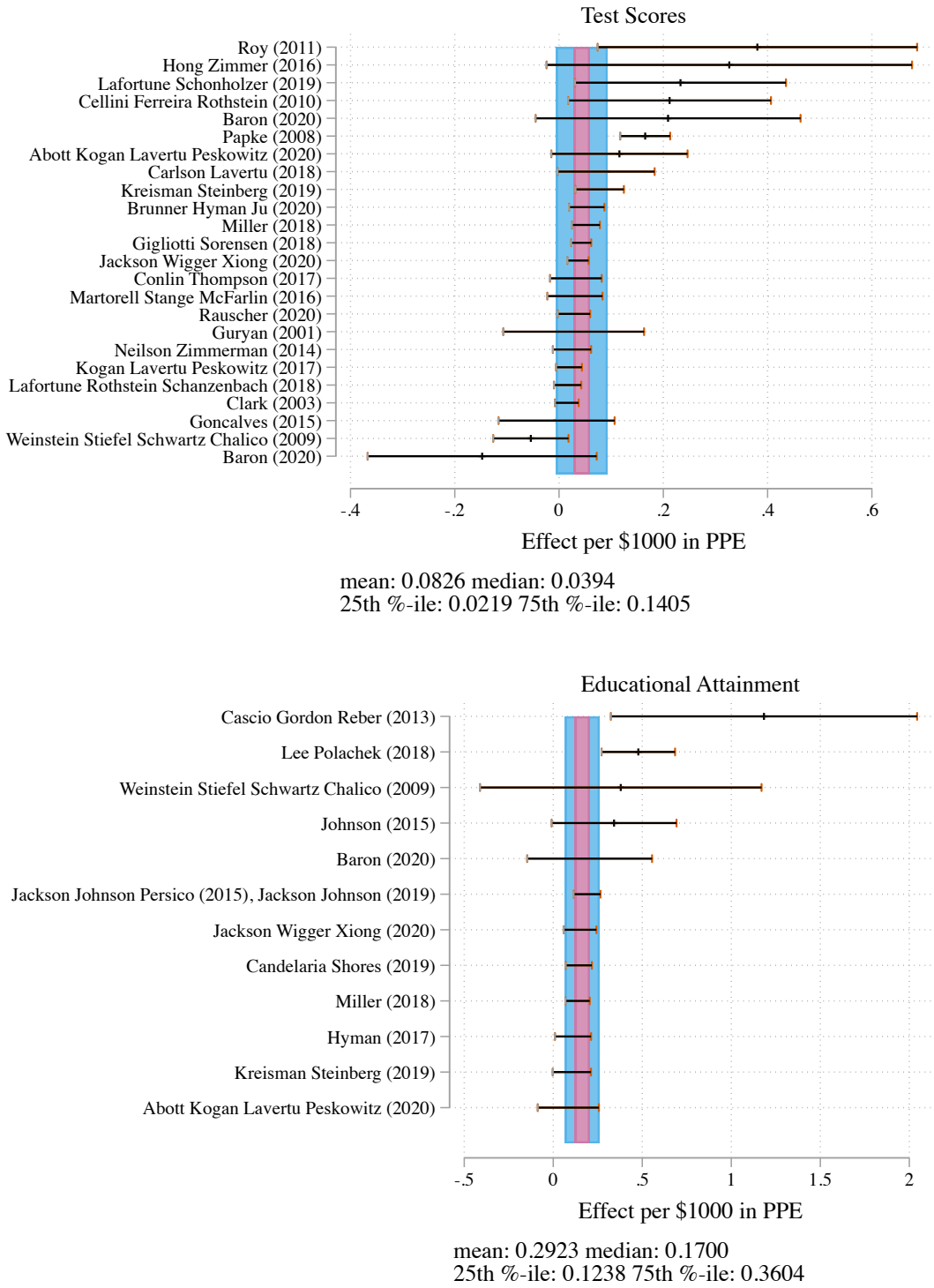


Figure 4: Overall Estimates

spending impacts more than 99 percent of the time. The models estimates that 68.1 percent of the variability in impacts reflects heterogeneity across studies (i.e., not all contexts will have the

Table 3: Meta-Analysis Estimates

	(1) Overall Test Scores	(2) Non-Capital Test Score	(3) Capital Test Score	(4) Overall Test Scores	(5) Overall Educational Attainment
Average Effect	0.0438***	0.0476***	0.0341***	0.0474***	0.163***
	(0.00925)	(0.0125)	(0.00822)	(0.0124)	(0.0250)
Capital				-0.0123	
				(0.0156)	
N	24	15	9	24	12
τ	0.0285	0.0302	0.0255	0.0295	0.0534
% Cross-Study Var.	0.681	0.762	0.428	0.696	0.391
90% PI	[-0.006,0.093]	[-0.006,0.102]	[-0.010,0.078]		[0.066,0.261]
Prob. Pos	0.928	0.927	0.899	0.922	0.997

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

same treatment effect), which suggests there is uncertainty about what one would observe in other settings. More specifically, the standard deviation of heterogeneity across studies (i.e., τ) is 0.0285. This implies that any two studies may have true causal impacts that differ by about 0.0285σ simply due to treatment heterogeneity. Intuitively, the model estimates this level of heterogeneity because relatively precise studies like Papke (2008) and Rauscher (2020) both have positive effects but *do not* have overlapping margins of error. The model takes this as evidence that these studies likely come from contexts with different effects (both of which are positive), and infers a positive average effect with nontrivial heterogeneity across contexts. An implication of this estimate is that even though the pooled effect is 0.044, in other contexts one would expect estimates between -0.0056σ and 0.0932σ about 90 percent of the time. The 90 percent prediction interval for what one would expect in a new study is depicted in blue. This prediction interval contains the point estimates of 15 of the 24 studies, and (with the exception of Papke 2008) those that lie outside this range are very imprecise. Another policy-relevant summary of the predicted impacts is that a policy that increases school spending by $1000 over a four-year period would increase test scores 93 percent of the time - more than 9 times out of 10.

Capital Versus Non-Capital Spending Impacts on Test Scores

In a recent review, Jackson (2020) points out that while the impacts of operational spending are consistently positive, the impacts for capital spending are less clear. Additionally, Baron (2020) finds positive test score impacts for operational spending increases but no such pattern for capital spending.[48] However, it is possible that many capital studies may not *individually* have the statistical power to detect reasonable effects. As such, it may be useful to formally examine

[48] It is noteworthy that, as shown in Figure 3, Baron's results stands in contrast to most other studies of the effects of capital spending on outcomes.

whether marginal capital and non-capital spending impacts differ across several studies.

Looking at the pooled estimates for non-capital and capital spending (columns 2 and 3), the average effect is somewhat larger for non-capital spending (0.0476σ) than for capital (0.0341σ). However, the average impacts for both spending types are individually significant at the 1 percent level, and one cannot reject that they have the same effects on average. A formal test of for the difference in effects (column 4) involves estimating a meta-regression with a capital indicator variable – representing the difference between the average for capital spending and others. The meta-regression is a weighted least squares regression where each study j is weighted by the inverse of its estimated precision $1/(se^2_{\mu,yj} + \hat{\tau}^2)$. The test that the capital indicator is zero yields a p-value greater than 0.5.[49] That is, even though capital projects tend to yield small and sometimes negative effects in the first few years, within 6 years of the capital expenditure the overall impacts are positive, and the marginal impact of the present flow-value of the capital expense is similar to that of the marginal effect of contemporaneous non-capital spending. This suggests that both capital and non-capital spending increases matter for student outcomes, and that after a few years the economic value of spending is on the same order of magnitude across the two types. This similarity also suggests that our modelling decisions to generate comparable marginal impacts of per-pupil capital to non-capital spending were reasonable.

To put these capital estimates in perspective, we consider two typical kinds of projects. A new elementary school construction would typically cost about \$27.5M and house 624 students (Abramson (2015)). This is a one-time expense of about \$44,000 per pupil. Assuming a 50 year life of the asset, and distributing the value of this capital spending over the life of the asset (while accounting for depreciation at 7% per year), this would be associated with an average per-pupil flow value in the first four years of about \$2693. Using the estimates from column 3, one would expect test scores to increase by about $2.69\times0.0341=0.092\sigma$ six years after the capital outlay. Given the depreciation of the building, *an extrapolation beyond the variation in the data suggests that* the marginal effect might fall to about half this amount after 15 years. By way of comparison, a modest set of upgrades (i.e., a \$1,000,000 renovation project) may cost 1000000/600=\$1667 per pupil. Assuming a 15 year life of the asset, this would be associated with an average per-pupil flow value in the first four years of about \$150. This would increase test scores by about $0.15\times0.0341 = 0.0051\sigma$ six years after the capital outlay. This is smaller than what most individual studies can detect. This calculation highlights that, given the economic life of capital assets, even though the expected annual marginal benefits are relatively small (often smaller than most individual studies have power to detect), the lifetime benefits are likely similar to those for non-capital spending. These facts reinforce the importance of (a) calculating the flow value of large one-time capital outlays, and (b) the increased statistical precision afforded by formal meta-analysis that facilitates more conclusive statements than those possible in any individual study.

[49] See Appendix Figures A.4 and A.5 for separate forest plots by spending types.

Educational Attainment Impacts

The forest plot of all the estimated impacts on educational attainment outcomes are the bottom panel of Figure 4. The 25th percentile of school spending effects on educational attainment is 0.1238σ and the 75th percentile is 0.3604σ. This range of positive estimates underscores the importance of looking at the literature as a whole to gauge magnitudes. As with test scores, the more precisely estimated studies lie close to the median of the distribution and some of the larger estimated impacts are imprecise. The simple average of the educational attainment effects is 0.2923σ, while the median is 0.17σ. As with test scores, the nontrivial difference between the mean and the median reflects the fact that the mean is more heavily influenced by the large, imprecise positive estimates. This suggests that the precision-weighted average would be more appropriate than a simple average and would likely be similar to the median.

Educational Attainment Meta-regression Results

The pooled meta-analytic average school spending effect for educational attainment (column 5 of Table 3) is 0.163σ. This is similar to the median across all studies. To aid interpretation, we convert the pooled impacts to high school completion and college going rates. For high-school graduation (with a standard deviation of 0.1275 in 2018) the estimates suggests that, on average, increasing school spending by \$1000 (sustained for 4 years) would increase high-school graduation rates by 0.1275×0.163=2.1 percentage points. For college-going (with a standard deviation of about 0.2419 in 2018) this suggests that on average, increasing school spending by \$1000 (sustained for 4 years) would increase high-school graduation rates by 0.2419×0.163=3.94 percentage points.

Estimates for educational attainment suggest modest levels of contextual heterogeneity. That is, roughly 39 percent of the variability in causal impacts across studies can be explained by heterogeneity, indicating a high level of external validity. This results occurs because the pooled effect tends to fall within the margin of error for most studies, so the model concludes that the pooled effect is largely representative of what one would be likely observe in other settings. A policy that increased school spending by \$1000 per pupil sustained for four years *in some other context* would lead to impacts between 0.0658σ and 0.2605σ about 90 percent of the time. This implies high school completion impacts between $0.0658\times.1275$= 0.8 percentage points and $0.2605\times.1275$= 3.3 percentage points about 90 percent of the time and, college completion impacts between 0.0658×0.2419=1.6 percentage points and 0.2605×0.2419=6.3 percentage points 90 percent of the time. Put differently, a policy that increased school spending by \$1000 over a four year period would be expected to increase educational attainment over 99 percent of the time.

Benchmarking the Impacts on Test Scores and Educational Attainment

To put theses estimates into perspective, it is helpful to compare the magnitude of the school spending impacts to those of other interventions. We show this for three separate interventions.

Class Size: Using Project STAR, Chetty et al. (2011) find that reducing class size by roughly

seven students increases test scores by 0.12σ (4.76 percentile points) and college-going (by age 20) by 1.8 percentage points. Also, Dynarski et al. (2013) find that reducing class size by seven students increases college-going (by age 30) by 2.7 percentage points. Using this as a benchmark, our test score impacts of 0.0438σ are equivalent to reducing class size by $7\times0.0438/0.12=2.55$ students, while our college-going impacts of 3.94 percentage points are equivalent to reducing class size by between $7\times3.94/1.8=15.32$ students and $7\times3.94/2.7=10.21$ students.

Teacher Quality: Chetty et al. (2014) find that increasing teacher quality by one standard deviation increases test scores by 0.12σ and college going by 0.82 percentage points. Using this as a benchmark, our test score impacts of 0.0438σ would be equivalent to increasing teacher quality by $0.0438/0.12=0.365$ standard deviations, while our college-going impacts of 3.94 percentage points would be equivalent to increasing teacher quality by $3.94/0.82=4.8$ standard deviations.

High-achieving charter schools: High-achieving charter schools increase test scores by over 0.3σ (Angrist et al. (2016)) and increase college going by as much as 10 percentage points (Booker et al. (2011); Davis and Heller (2019)). As such, our $1000 school spending impacts on test scores are equivalent to about 15 percent of the impacts of attending a high-achievement charter school, while our college-going impacts are equivalent to almost 40 percent. One may worry that these comparisons are skewed by the large test score impacts in Angrist et al. (2016) or by pulling estimates from different sets of schools. To assuage this concern, we take estimates from Dobbie and Fryer (2020) who find that "No Excuses" charter schools in Texas increase test scores by 0.093σ and college going by 2.5 percentage points. Our $1000 school spending impacts on test scores are equivalent to about half of the impacts of attending a Texas "No Excuses" Charter, while our college-going impacts are about 150 percent of the impacts.

For all three benchmarking interventions, our school spending effects are economically meaningful. However, a consistent pattern is that irrespective of the benchmark, the spending impacts on educational attainment are at least three times as large as those on test scores. Importantly, these differences in magnitude between test score and educational attainment impacts are not driven by a comparison across studies, because *this same pattern holds within those studies that examine impacts on both outcomes*. Among the 6 studies that report on both test scores and educational attainment, 5 indicate larger educational impacts than on test scores (Jackson et al. (2021), Baron (2020), Miller (2018), Weinstein et al. (2009), and Kreisman and Steinberg (2019)), while only one does not (Abott et al. 2020). This suggest that the school spending impacts as measured by test scores may not capture the full benefits of school spending policy (Card and Krueger 1992; Jackson et al. 2016). It is also consistent with the view that educational output is only partially measured by test scores, and that a focus on test score impacts may lead one to understate the benefits of school quality on student outcomes (Beuermann et al. 2020; Jackson 2018; Jackson et al. 2020).

5.3 Assessing Heterogeneous Effects by Income Level

An important policy question is the extent to which school spending impacts vary for students from more or less economically advantaged backgrounds. While some studies document larger policy

impacts for low-income students (or schools and districts that enroll large shares of low-income students), because many policies may lead to larger spending increases for low-income students (such as many school finance reforms) the policy effect may reflect a combination of differences in the spending changes experienced across income groups and differences in the marginal response to spending changes across income groups.

We seek to disentangle these two channels by exploiting the fact that some studies provide separate estimates of policy impacts by income status, and some policies are targeted to schools that enroll large shares of low-income students (such as Title I). Because some studies report impacts by the income status of the student, while others report impacts by the income status of the school or district, low-income estimates are not perfectly comparable across studies. As such, while the students informing the low-income estimates will disproportionately be from low-income families, the share of low income students may vary across studies. This introduces a kind of measurement error that may bias us away from detecting significant impacts. Another source of error stems from the fact that the definition of low-income status differs across studies – some define low income as being in the bottom quintile of the income distribution, while others define low income based on free-lunch eligibility.[50] Changing income distributions further complicates comparisons. Caveats aside, the question is sufficiently important that the hypothesis is worth testing, albeit imperfectly.

To avoid confounding differences in spending changes with differences in responsiveness to spending changes by income, we compute marginal spending impacts for low-income and non-low-income groups separately for those studies which report effects by income status. We first perform a simple coin test analysis for the 14 study-outcome combinations that provide impacts by income status (see Table A.15).[51] Of these 14 studies, 11 have larger marginal impacts for the low-income groups. The likelihood of observing this many or more studies with this pattern by random chance (under a null hypothesis of no difference) is just under 3 percent, or 1 in 35 – suggesting that marginal impacts are larger for low-income groups than non-low-income groups.

To quantify these differences, we use meta-regression. For those studies that allow for different marginal school spending impacts for low-income and non low-income populations, we compute separate estimates of μ_{yj}. To connote this, we add the subscript inc such that $\mu_{yj,inc}$ is the effect of an increase in per pupil spending of \$1000 (sustained over four years) for study j on outcome y for population $inc \in \{high, low\}$. We then estimate a random effects meta-regression, of the form below, where each study-outcome is weighted by the inverse of its precision. That is, the weight for study j and outcome y for income level inc is $1/(se^2_{\mu,yj,inc} + \hat{\tau}^2_{yj})$ estimated by method of moments.

$$\mu_{yj,inc} = \theta + (LowIncome_{j,inc} \times \beta_1) + (NonLowIncome_{j,inc} \times \beta_2) + \delta_{j,inc} + \epsilon_{j,inc} \tag{5}$$

The variable $LowIncome_{j,inc}$ is equal to 1 for observations pertaining to a low-income popula-

[50] We detail how studies define low income in Table A.7.

[51] There are 2 additional studies that that examine impacts on achievement or attainment gaps by income (Biasi (2019) and Card and Payne (2002)). These studies do show that school spending policies reduced gaps in student outcomes by income status, but they do not allow one to disentangle spending differences from response differences.

tion, which we define in two ways (specified below), $NonLowIncome_{j,inc}$ is equal to 1 for observations pertaining to a higher-income population. β_1 and β_2 indicate the *difference* between the average effect for the average student and those from low-income populations and non-low-income populations, respectively. We report results in Table 4, which shows consistently lower estimated effects for economically advantaged populations compared to the average overall population, and a consistent pattern of larger impacts for the less economically advantaged populations than for economically advantaged populations.[52]

We include two different categorizations of low-income. Our first categorization includes only those studies with distinct estimates for low-income populations (columns 1 and 3), and our second also includes studies that present overall estimates for Title I, a program explicitly aimed at providing funding to schools with low-income students (columns 2 and 4). In column 1, the average effect of spending on test score outcomes is 0.044. The coefficient on low-income is 0.00636 – a small and not statistically significant point estimate – while the coefficient on non-low-income is -0.0262, which is sizeable (and significant at the 10% level). The formal test of whether the marginal impacts are the same for the low- and non-low-income groups and no different from zero yields a p-value of 0.0497 – indicating that we can reject the null (at the 5% level) that the coefficients on the income-level indicators are equal to zero.

Our results in column 2, which expands the definition of low-income to also include overall Title I estimates, show a similar pattern. The estimated impacts of increased spending on test scores is not discernibly different for low-income and the overall population, and the estimated impact for the non-low-income population remains significantly lower (at the 5% level) than for the overall population. Indeed, the estimated effect for non-low-income populations is more than 60% lower for this population than for the overall population.[53] The joint test of whether the marginal impacts are the same for the low- and non-low-income groups and no different from zero (using this expanded definition of low-income) yields a p-value of 0.0792 – indicating that we can reject the null (at the 10% level) that the coefficients on the income-level indicators are equal to zero.

Our results for differential impacts by income status for educational attainment are similar, though less precise. Using our more restrictive definition of low-income (Table 4 column 3), we find no discernible difference between effects of spending for overall and low-income populations, and more than 60% lower impacts (-0.138) for non-low-income populations, though the difference is not statistically significant. With an expanded definition of low-income to also include overall Title I studies (column 4), the results are similar. Using the estimates from column 4, increasing school spending by $1000 (sustained over 4 years) would increase high-school graduation rates of the low-income population by (0.206-0.0573)×0.1275 = 1.9 percentage points, and college-going rates by about (0.206-0.0573)×0.2419 = 3.6 percentage points. For non-low-income populations,

[52] We present our main models with a conservative approach to account for possible correlations across studies which identify changes form the same policies by clustering estimates of the same policy as if they stemmed from the same study. Our results become more pronounced and precise (Table A.14).

[53] As a robustness check, in Appendix Table A.16 we estimate models that exclude three studies for which the estimated impacts on spending were not clearly reported separately by income – potentially biasing our estimates. These studies are: Baron (2020), Brunner et al. (2020)), and Goncalves (2015). The results are very similar.

that increase in spending would be expected to increase high school graduation rates by just (0.171-0.122)×.1275 = 0.6 percentage points, and college-going rates by about (0.171-0.129)×.2419 = 1 percentage point. These are sizable differences in effects.

Taken as a whole, these results show consistently lower marginal effects for economically advantaged populations compared to the average overall population – patterns consistent with the marginal school spending impacts varying by socioeconomic status. Importantly, our results suggest that larger policy impacts of school spending is not just due to lower income populations receiving larger increases in spending (which does often happen), but also likely reflects more responsiveness to the same increases in spending by less economically advantaged student populations compared to more economically advantaged populations.

Table 4: Meta-Regressions w/ LI

	(1) Test Scores	(2) Test Scores	(3) Educational Attainment	(4) Educational Attainment
Average Effect	0.0440***	0.0489***	0.223***	0.206***
	(0.0124)	(0.0122)	(0.0780)	(0.0745)
Low-Income	0.00636		-0.0998	
	(0.0182)		(0.102)	
Low-Income (w/ Title I)		-0.0103		-0.0573
		(0.0201)		(0.102)
Non-Low-Income	-0.0262*	-0.0311**	-0.138	-0.122
	(0.0146)	(0.0145)	(0.0890)	(0.0858)
N	31	31	19	19
τ	0.0282	0.0280	0.0774	0.0807
% Cross-Study Var.	0.669	0.666	0.415	0.435
Low-Income = Non-LI = 0 (p-val)	0.0497	0.0792	0.299	0.328

Standard errors in parentheses

All Low-Income Estimates are comparisons with Non-Low-Income except in the case of Goncalves (2015).

Low-Income w/ Title I is an indicator that additionally captures all Title I studies, even those which do not present distinct by-income effects.

* $p < .1$, ** $p < .05$, *** $p < .01$

5.4 Do Longer-Run Impacts Increase with Exposure?

Given that learning is a cumulative process, one would expect that the benefits to increased spending would increase with exposure. Indeed, to make studies comparable to each other, we assumed that the impacts are linear in the years of exposure and adjusted all estimates to reflect four-year impacts. Because some studies show the effects of four year of exposure to a spending increase while others present effects of 9 years and 12 years, we can test if that assumption is reasonable.

First, we plot the estimates (not adjusted for exposure) on educational attainment in Figure 5. We represent more precise studies with larger circles. There are several relatively precise estimate

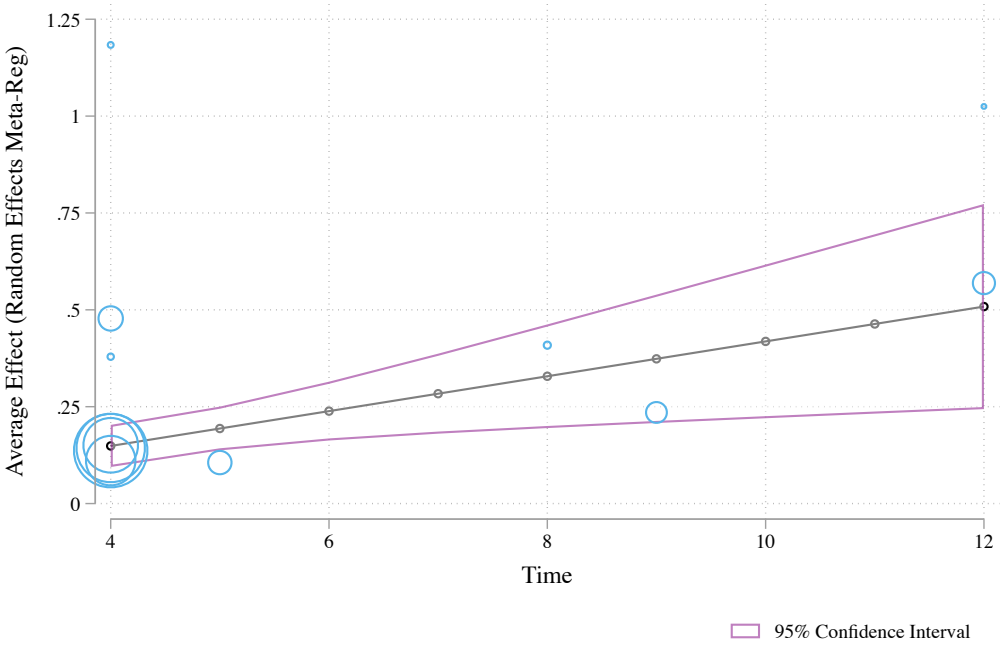


Figure 5: Educational Attainment by Years of Exposure

pertaining to four years of exposure centered around 0.15. There are two observations pertaining to 8 and 9 years of exposure that are both above the center of the four-year impacts, and two studies (one very imprecise large estimate) that relates 12 years of exposure to increased spending with even larger overall impacts. The pattern indicates larger overall impacts for estimates that relate to more years of exposure (per $1000 per-pupil spending increase).

To formally test this notion, we run a meta-regression on the years-unadjusted effects (denoted $\ddot{y}_j$), and include the years of exposure underlying each estimate as a covariate. If impacts are increasing with years of exposure, as suggested visually, then studies that report the impacts of more years of exposure should report larger educational attainment impacts. In addition, we can directly test if the average four-year effect (the shortest exposure reported) is similar to the four times the average impact of an additional year of exposure. This would be a direct statistical test of the notion that the educational attainment impacts increase linearly with years of exposure. In a regression this is achieved by estimating equation (6) by random effect meta-regression.

$$\ddot{y}_j = \alpha + \beta \times (Exposure_j - 4) + \epsilon \tag{6}$$

In this model $(Exposure_j - 4)$ is the years of exposure to the spending change minus 4 so that α is the average estimated 4-year impact (identified off those studies with four years of exposure). The parameter β is the increase in the impact associated with each additional year of exposure. The formal test for whether there in greater educational attainment with more years of exposure to increased spending is whether $\beta = 0$. This test yields a p-value of 0.03 – suggesting that the

effects increase with years of exposure. As described above, a formal test for linearity is whether $\alpha - (4 \times \beta) = 0$. This test yields a p-value of 0.7 – suggesting that the impacts may increase linearly with years of exposure. In sum, the data indicate that the educational attainment impacts increase with years of exposure and that the increase in educational attainment is approximately linear in years of exposure. This is both (a) a substantively important result to inform policy, and (b) validates our modelling assumptions.

5.5 Publication Biases

Our analysis may be biased if certain kinds of studies – especially those which find no effect of a given policy or intervention – are systematically not published. There are two kinds of publication biases that one may worry about in our context. First, journals may be less likely to publish studies that are not statistically significant. If so, assuming that there is an overall positive effect, those studies with larger positive impacts (and therefore larger t-statistics) will be more likely to be published – such that the average among published studies may overstate effects. Second, if researchers and journals are more likely to publish results consistent with "preferred" results, precisely estimated impacts of all signs will be published (because they are credible), but imprecise studies (where the results are are more ambiguous) of the non-preferred sign are disproportionately not published. This would lead to a meta-analytic average biased toward the preferred result. We conduct several tests to assess the extent to which these are a concern in our setting. We detail these tests in Appendix Section A.9, and summarize them here:

1. Using meta-regression, we compare the average estimates of published and unpublished studies and find no difference in impacts.

2. We compare the average impacts of studies published in the most elite journal (where selection biases may be most severe (Brodeur et al. (2016)) to other journals, and find no evidence of differences by journal prestige or publication status.

3. To assess whether there is a bias toward statistically significant impacts among the included studies, we show that there is no excess density (i.e., overrepresentation) of studies right at the significance threshold (i.e., a t-statistic of 1.96). A histogram of all studies shows slightly *less* density above the significance threshold and regression evidence uncovers no indication of a discontinuous shift in density through that threshold.

4. To explicitly adjust for any publication bias against insignificant impacts, we implement the selection adjustment suggested in Andrews and Kasy (2019). They suggest estimating the publication probabilities (based on the t-statistics) for studies, and using these to produce bias-corrected estimators and confidence sets.[54] Their approach yields similar estimates to our preferred model. See Figure 6.[55]

[54] This approach re-weights the distribution of studies to account for differences is publication probability.

[55] We also follow Mathur and VanderWeele (2020) and adjust our estimates assuming extreme levels of selection to

5. To examine whether there is evidence of bias against imprecise studies with a negative sign, we test for asymmetry in a plot of study impacts against their precision. The plot does indicate some asymmetry among very imprecise studies – suggestive of possible publication bias. We account for this in two ways, both shown in Figure 6. First, we follow Stanley et al. (2010) and focus only on precise studies were publication biases are unlikely to exist. Using only the most precise half of studies (where there is no evidence of asymmetry), the estimates are similar to our preferred estimates. Second, we implement the "trim and fill" method (Duval and Tweedie (2000)) by imputing "missing" studies to create symmetry in the distribution of estimates at all precision levels, which also yields similar estimates to our main models.

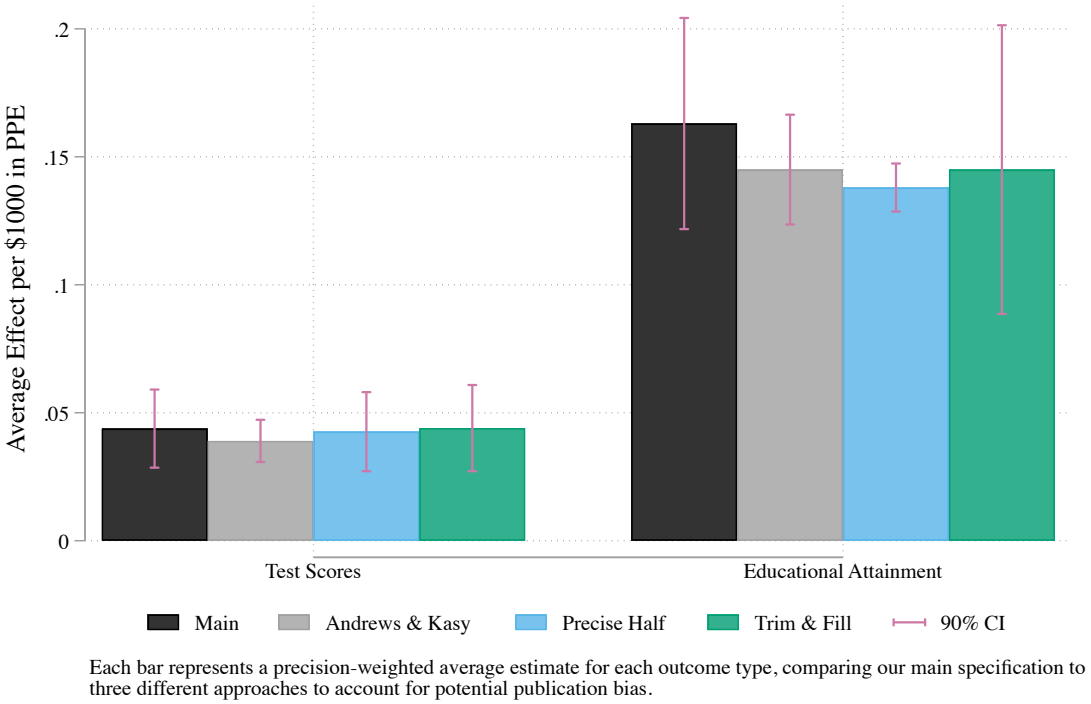


Figure 6: Three Approaches to Publication Bias

In sum, across several empirical tests and adjustment for publication bias, we find little evidence that our estimates are appreciably impacted by publication bias. Indeed, in all models that adjust for possible publication bias, the point estimates lie within the confidence interval for our main estimate. Given the consistent pattern of results (i.e., over 90 percent of study impacts are positive), the fact that publication bias is unlikely to explain our positive overall association is not entirely surprising. The robustness of our effect is is also driven by the fact that we employ precision-weighed estimates that down-weight those studies most susceptible to bias.

report "worst case" scenario lower-bound estimates. Under selection of this form, the test score impacts fall by only 43 percent, the educational attainment impacts fall by only 23 percent, and both remain positive and significant at the 5 percent level.

6 Examining Evidence of Diminishing Returns

Under optimizing behaviour, schools would spend the first thousand dollars on the inputs that produce the most output, and then the next thousand dollars on the second most productive input and so on. If so, school spending would exhibit diminishing marginal returns. Informed by this notion, some scholars hypothesize that the level of school spending in United States is sufficiently high that the marginal impact of spending is approaching zero. To shed light on this, we examine if the marginal impacts of school spending depend on the baseline spending level in the study context. Per-pupil school spending levels have more than doubled in the past thirty years (Hill and Zhou (2006)), and at any given point in time some states spend much more per pupil than others. In principle, studies based on recent policies in high spending states such as New York (e.g., Gigliotti and Sorensen (2018) and Lee and Polachek (2018)) would have smaller marginal impacts on average than studies of old policies (such as the roll-out of Title I in 1965 examined in Cascio et al. (2013)) or in lower-spending states such as Texas (e.g., Martorell et al. 2016). To assess this, in Figure 7 we plot the marginal spending impact against the baseline spending for all papers. Each circle represents a single study-outcome, and larger circles connote more precise estimates. We also include the precision-weighted linear relationship along with the 95% confidence interval.

The scatter-plot of marginal test score impacts (left) shows little evidence that marginal impacts are smaller at higher baseline spending levels. While there are some large positive marginal impacts at lower spending levels (e.g., Hong and Zimmer (2016) and Roy (2011)), these studies are all very imprecise relative to those with smaller estimated impacts at similar baseline spending levels (e.g., Clark (2003) or Brunner et al. (2020)). A precision-weighted linear regression of the scatter-plot yields a slightly *positive* slope with a p-value above 0.1. The scatter-plot for educational attainment (right panel) follows a similar pattern. There is evidence of larger estimates at very low levels of spending, but these estimates are also imprecise. A precision-weighted linear regression of the scatter-plot yields a slightly positive slope with a p-value above 0.1. For both outcomes, the marginal impacts are remarkably similar across a wide range of per-pupil spending levels. After accounting for the precision of the estimates, there is no evidence of diminishing returns between \$8,000 and \$20,000 per-pupil. Given a national average of \$14,439 per-pupil (NCES 2020), these patterns suggests that educational spending in the United States is not yet "*on the flat*."

Note that because education is a very labor intensive field, as wages rise in many sectors, wages for educators will also rise with minimal ability to reduce workers (Baumol and De Ferranti (2012)). This could explain rising education costs that would not represent movement along the productivity schedule (i.e. going from the most to the least productive input) – potentially explaining the constant marginal impacts, on average, across a wide range of spending levels. Another explanation is that, because public educators do not have a profit motive, school spending is not allocated to the **most** productive inputs on the margin, but rather based on rules of thumb or heuristics so that additional monies go toward bundles of inputs that are generally similarly productive.

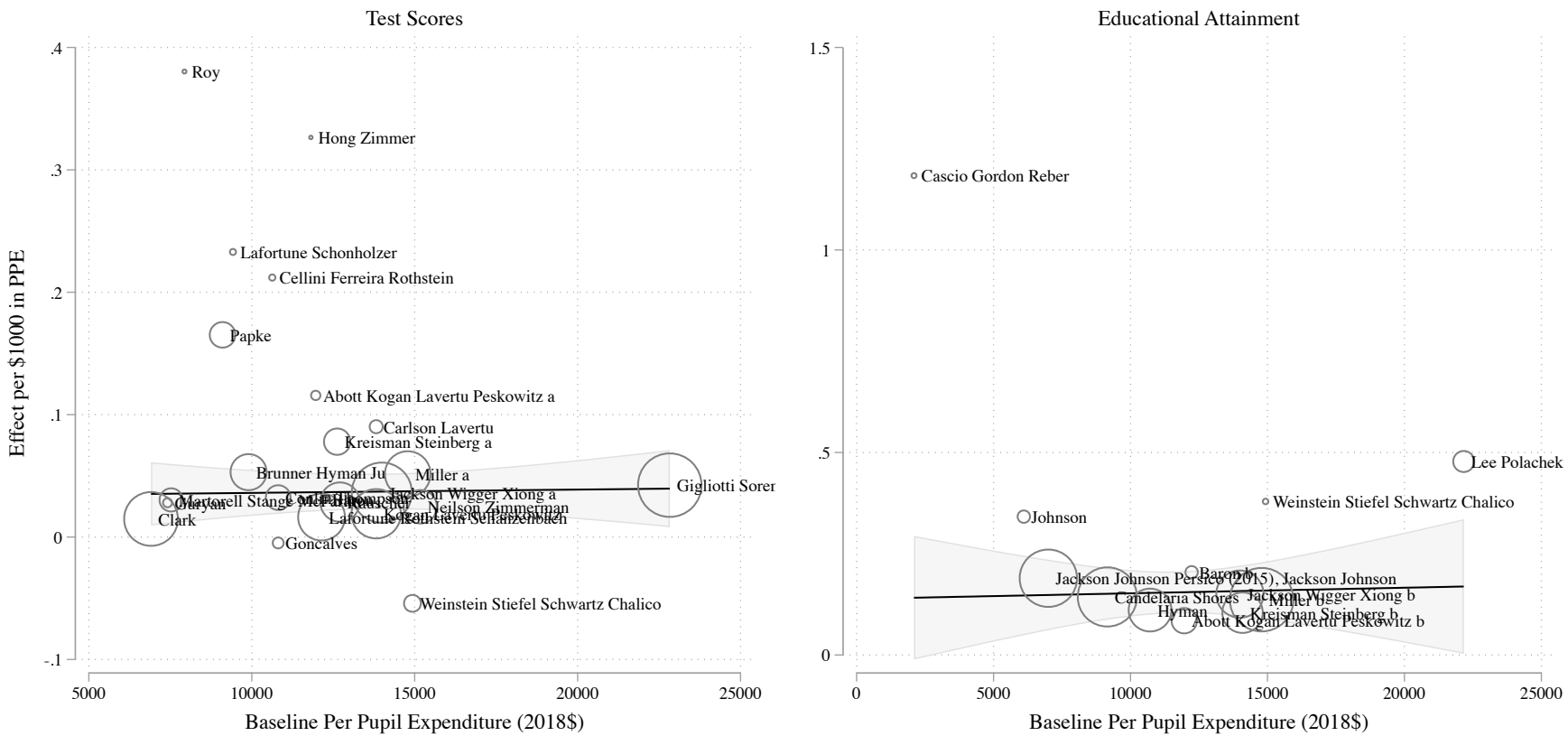


Figure 7: Marginal Impacts by Baseline Spending Level

7 Discussion and Conclusions

We collected and classified all known credible causal studies of the impact of public school spending on student outcomes in the United States. Of these 31 studies, 29 found positive impacts of policies that increased school spending on student outcomes. That is, the most credible evidence to date is extraordinarily inconsistent with the notion that money does not matter. Importantly, to shed light on magnitudes, we estimate the centers and spreads of the distributions of causal school spending impacts on test scores and educational attainment. Based on precision-weighted random-effects meta-analysis, *on average*, a \$1000 increase in school spending (sustained over four years) increases test scores by 0.044σ, increases high-school graduation by 2.1 percentage points, and college-going by about 3.9 percentage points. In relative terms, this is a 2.5 percent increase in high school graduation and a 9.6 percent increase in college-going. We find little indication that these pooled effects are skewed by publication biases.

We find that school spending impacts on educational attainment are larger than on test scores – when benchmarked against the impacts of other interventions – suggesting that using test scores to estimate school spending impacts, while informative, may understate the long-term benefits of school spending. Another key result of this analysis is that marginal school spending effects are very similar across a wide range of baseline spending levels – suggesting little evidence of diminishing returns to school spending at current levels. We present an approach that allows for an economically meaningful direct comparison of the causal effects of large one-time capital spending increases to those of annual (mainly operational) spending increases. We find that capital spending increases take about 5-6 years to materialize into improved outcomes, at which point (using our approach) the marginal effects are similar to other forms of school spending. We find little evidence of larger impacts for low-income populations as compared to the overall average population, though we *do* find that the effects for more economically advantaged populations are lower for both test scores and educational attainment.

Importantly, accounting for underlying variability due to context and differences in policy implementation indicates that not all policies will have similar impacts in the future. We find evidence of considerable treatment heterogeneity (i.e., variability unexplained by sampling variability) for test score impacts, and modest heterogeneity for educational attainment impacts. Using our estimates of the underlying heterogeneity, we "*predict*" that a policy that increases per-pupil spending \$1000 for at least four years will lead to positive test-score impacts over 92 percent of the time, and positive educational attainment impacts more than 99 percent of the time. While we document relatively consistent estimates across a variety of observable dimensions *on average*, further research uncovering *why* impacts are larger in some contexts than others (such as Brunner et al. (2020) and Johnson and Jackson (2019)) may be fruitful.

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A Appendix

A.1 Estimation Strategy

Table A.1: Summary of Estimation Strategy

Study	Est. Strategy	Spending Type
Abott Kogan Lavertu Peskowitz (2020)	Regression Discontinuity	operational
Baron (2020)	Regression Discontinuity	capital
Baron (2020)	Regression Discontinuity	operational
Biasi (2019)	Event Study	Any
Brunner Hyman Ju (2020)	Event Study DiD	Any
Candelaria Shores (2019)	Event-Study DiD	Any
Card Payne (2002)	Difference in Difference	Any
Carlson Lavertu (2018)	Regression Discontinuity	School Improvement Grant
Cascio Gordon Reber (2013)	Event Study	Title I
Cellini Ferreira Rothstein (2010)	Regression Discontinuity	capital
Clark (2003)	Event-Study DiD	Any
Conlin Thompson (2017)	Event Study	capital
Gigliotti Sorensen (2018)	Instrumental Variables	Any
Goncalves (2015)	Event Study	capital
Guryan (2001)	Instrumental Variables	Any
Hong Zimmer (2016)	Regression Discontinuity	capital
Hyman (2017)	Instrumental Variables	Any
Jackson Johnson Persico (2015), Jackson Johnson (2019)	Event-Study DiD	Any
Jackson Wigger Xiong (2020)	Instrumental Variables	Any
Johnson (2015)	Event-Study DiD	Title I
Kogan Lavertu Peskowitz (2017)	Regression Discontinuity	Any
Kreisman Steinberg (2019)	Instrumental Variables	Any
Lafortune Rothstein Schanzenbach (2018)	Event-Study DiD	Any
Lafortune Schonholzer (2019)	Event-Study DiD	capital
Lee Polachek (2018)	Regression Discontinuity	Any
Martorell Stange McFarlin (2016)	Regression Discontinuity	capital
Miller (2018)	Instrumental Variables	Any
Neilson Zimmerman (2014)	Event-Study DiD	capital
Papke (2008)	Instrumental Variables	Any
Rauscher (2020)	Regression Discontinuity	capital
Roy (2011)	Instrumental Variables	SFR
Weinstein Stiefel Schwartz Chalico (2009)	Regression Discontinuity	Title I

We assign a single, primary estimation strategy for each paper. Regression Discontinuity studies are those whose identification is dependent on a cutoff point for some running variable. Event Study studies are those whose identification strategy is driven by a policy or rollout over time. Instrumental Variables studies are those whose identification is driven by a change that occured conditional on a policy, but not RD.

Table A.2: Meta-Regressions w/ Estimation Strategy

	(1) Overall Test Scores	(2) Non-Capital Test Score	(3) Capital Test Score	(4) Overall Educational Attainment
RD	0.00122	-0.000704	0.00362	0.114
	(0.0167)	(0.0293)	(0.0232)	(0.179)
IV	0.0390*	0.0423*		-0.0415
	(0.0209)	(0.0230)		(0.0510)
Average Effect	0.0294***	0.0271**	0.0347**	0.173***
	(0.00796)	(0.0119)	(0.0144)	(0.0500)
N	24	15	9	12
τ	0.0282	0.0308	0.0345	0.0538
% Cross-Study Var.	0.677	0.769	0.579	0.395
RD = IV = 0 (p-val)	0.168	0.169	0.876	0.480

Standard errors in parentheses

Event Study (strategy) omitted

* $p < .1$, ** $p < .05$, *** $p < .01$

A.2 Main Models by Strength of First Stage

Table A.3: Meta-Analysis, F-stat > 10

	(1) Overall Test Scores	(2) Non-Capital Test Score	(3) Capital Test Score	(4) Overall Ed. Attainment
Average Effect	0.0460***	0.0488***	0.0396**	0.187***
	(0.00988)	(0.0124)	(0.0155)	(0.0456)
N	19	11	8	7
τ	0.0282	0.0284	0.0365	0.0873
% Cross-Study Var.	0.713	0.715	0.517	0.603

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.4: Meta-Analysis, F-stat > 20

	(1) Overall Test Scores	(2) Non-Capital Test Score	(3) Capital Test Score	(4) Overall Ed. Attainment
Average Effect	0.0553***	0.0656***	0.0355***	0.137***
	(0.0135)	(0.0190)	(0.0127)	(0.00795)
N	13	7	6	4
τ	0.0311	0.0321	0.0328	0
% Cross-Study Var.	0.737	0.750	0.518	0

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

A.3 Estimates Captured per Paper

Table A.5: Summary of per-study steps

study	outcome	effect per $1000	$ Δ: source	outcome Δ: source
Abott Kogan Lavertu Peskowitz (2020)	High school graduation	0.0850	$417 (2012$): Table 8 Expend. P.P. Operations, $\leq 5yrs$, Bandwidth $+/-10$	0.0174: Table 8 Grad. Rate, $\leq 5yrs$, Bandwidth $+/-10$, standardized (Table 2 Grad. Rate (4yr), Passed)
Abott Kogan Lavertu Peskowitz (2020)	Test scores	0.1160	$417 (2012$): Table 8 Expend. P.P. Operations, $\leq 5yrs$, Bandwidth $+/-10$	0.066: Table 8 Math/ELA (SDs), $\leq 5yrs$, Bandwidth $+/-10$
Baron (2020)	College enrollment	0.2040	$424.86 (2018$): Table 4 Op. Expenditures PP, averaged across 1-4yrs Relative to the Election (341.2, 362.52, 541.88, 453.85)	0.08: Table 5 Panel (a) Postsecondary Enrollment 8yrs (0.08) multiplied by baseline (0.2, Table 2), standardized (Table 2, Postsecondary Enrollment Share)
Baron (2020)	Test scores	-0.1470	$4600 (2010$): "the average per-pupil bond campaign approved in Wisconsin is only approximately $4,600 per pupil" (22-23), depreciated over 15 years and averaged over first 6 years	-0.046: Figure 9 panel (c) Average WKCE Score, cubic Year 6 (relative to election)

Baron (2020)	Test scores	0.2090	$424.86 (2018$): Table 4 Op. Expenditures PP, averaged across 1-4yrs Relative to the Election (341.2, 362.52, 541.88, 453.85)	4.42: Table 5 Panel (a) Average WKCE Scale Score 4yrs (4.42) divided by student-level SDs (43.2, table note)
Brunner Hyman Ju (2020)	Test scores	0.0530	$498 (2015$): Table 2 Current Expenditures, State Aid, Expanded controls Yes	0.007: Table 7 All Districts Years postreform, multiply by 4 (years)
Candelaria Shores (2019)	High school graduation	0.1430	$795.02 (2010$): .1xbaseline (Table 2 Weighted Mean Total revenues)	0.197: Table 5, Full log(Rev/Pupil), standardized (Table 2, Graduation rates)
Carlson Lavertu (2018)	Test scores	0.0900	$2048.79 (2014$): Table 8 Dynamic RD model SIG eligibilty, average Year 1-4 (351.54, 1980.886, 2572.86, 3289.86)	0.221, 0.171: Table 5 Dynamic model SIG eligibility Year 4 of SIG, average Reading and Math
Cascio Gordon Reber (2013)	High school dropout	1.1840	$100 (2009$):"each additional $100 increase in annual current expenditure per pupil..." (pg. 152)	-3.46, 0.66: Table 7 Δ White and Black high school dropout (reverse sign), population weighted (0.9/0.1) and translated to SD units based on baseline (pg 147, population-weighted)

Cellini Ferreira Rothstein (2010)	Test scores	0.2120	$6300 (2010$): "the average bond proposal in close elections is about $6,300 per pupil" pg. 249, depreciated over 15 years and averaged over first 6	0.103, 0.160: Table VII, Academic achievement 6 yrs later Reading and Math, standardized ("the year-six point estimates correspond to effects of roughly 0.067 student-level standard deviations for reading and 0.077 for mathematics" (p. 252)
Clark (2003)	Test scores	0.0150	$1094.28 (2001$): Table 3 Current expenditures per pupil Post-reform (1=yes)	0.023: Table 6 Composite, Kentucky x post model (3)
Conlin Thompson (2017)	Test proficiency rates	0.0320	$4000 (2013$): "Capital expenditure and capital stock variables in Panels A and B are listed in $1000s" (Table 3 note) x4 (years), depreciated 15 years averaged over first 3	0.081, 0.07: Table 3 Capital Exp PP_t model (2) Percent Proficient in Math and Reading, relative to time t-3, standardized (Table 1 % Proficient in Math and Reading)
Gigliotti Sorensen (2018)	Test scores	0.0420	$1000 (2016$): "models... measure the effect of a $1000 spending increase" (pg. 175)	0.0468, 0.042: Table 4 PPE Math and Reading

Goncalves (2015)	Test proficiency rates	-0.0050	$23740.4 (2010$): Table 1 Construction Cost Per Pupil Total, depreciated over 36.875 (weighted between 15 and 50 based on "60-65% of projects are new facilities" (6), averaged across first 6 years	1.266, -1.442: Table 4 6+ yr. Completion Exposure Math and Reading, standardized (baseline Avg. Proficiency Table 4)
Guryan (2001)	Test scores	0.0280	$1000 (1991$): "median estimate…implies that a one standard deviation increase in per-pupil spending ($1,000)…" (pg. 21)	0.039, 0.032, -0.034, -0.026: Table V and Table VI Math and Reading, subject-combined and standardized (assumed student-level SD of 100), then precision-weighted across grades
Hong Zimmer (2016)	Test proficiency rates	0.3270	$8123 (2000$): Table 1 Avg. bond amount per pupil, depreciated over 26.9 years (weighted between 15 and 50 based on Table 4 Passed a measure New building) averaged over 6 years	2.13, 1.44: Table 5 4th7th proficiency Relative year 6, standardized based on Table 3 proficiency baseline

Hyman (2017)	College enrollment	0.1110	$1000 (2012$): "interpretation…is that $1,000 of additional spending during each of grades four through seven…" (pg. 269)	0.03: Table 4 model (4) Enroll in postsecondary schooling, standardized (baseline Table 1 All districts and cohorts Enrolls in postsecondary school)
Jackson Johnson Persico (2015), Jackson Johnson (2019)	High school graduation	0.1900	$480 (2000$): Table I All Per pupil spending (avg., ages 5-17) ($4,800) x0.1	0.07053: Table III Prob(High School Graduate) model (7), standardized based on avg. national baseline graduation rate of 0.77
Jackson Wigger Xiong (2020)	College enrollment	0.1500	$1000 (2015$): "preferred model, a $1000 reduction in per-pupil spending…" (pg. 14)	0.0124: Table 3 model (8) Per-Pupil Spending (thousands), standardized based on Table 1 College Enrollment Rate baseline
Jackson Wigger Xiong (2020)	Test scores	0.0360	$1000 (2015$): "preferred model, a $1000 reduction in per-pupil spending…" (pg. 14)	0.0385: Table 3 model (4) Per-Pupil Spending (thousands)

Johnson (2015)	High school graduation	0.3420	$85 (2000$): "results indicate that a $100 increase in per-pupil Title I funding..." times 0.85 passed through in real dollars seen by students (Figure 9)	0.0225: Table 2 first column County Title I per-pupil spending (00s), average ages five to seventeen, standardized based on avg. national baseline graduation rate of 0.77
Kogan Lavertu Peskowitz (2017)	Test scores	0.0190	-$303.096 (2010$): Table 3 Total average Election year-3 years after (-0.0187, -0.0435, -0.0385, -0.000332), times 12000 ("District spending per pupil is just under $12,000 annually" (pg. 384))	-0.14: Table 7 3 years after, to student-level SD units based on footnote 34
Kreisman Steinberg (2019)	High school graduation	0.1050	$1000 (2011$): specification, abstract	0.021: Table 8 Graduation, standardized based on Table 1 Graduation rate baseline
Kreisman Steinberg (2019)	Test scores	0.0780	$1000 (2011$): specification, abstract	0.097, 0.077: Table 5 Reading and Math
Lafortune Rothstein Schanzenbach (2018)	Test scores	0.0160	$907 (2013$): Table 4 Mean Total exenditures	0.004: Table 8 Post event x years elapsed times 4 (years)

Lafortune Schonholzer (2019)	Test scores	0.2330	$15000 (2013$): "projects we study…$15,000 per pupil" (footote 6), depreciated over 50 years average across first 6 years	0.031xyear - 0.016, 0.021xyear - 0.004: Table 3 2SLS New School + Newschool Trend, Math and English Language Arts, 6 years
Lee Polachek (2018)	High school dropout	0.4780	$169.40 (2018$): Table 2 (percent change) times baseline spend by authors' calculations ($16939.79)	-0.1837: Table 3 9th-12th Grade Cubic, standardized based on baseline dropout rate Table 1 Mean Dropout Rate 9-12th Grade
Martorell Stange McFarlin (2016)	Test scores	0.0300	$7800 (2010$): "average per-pupil size of capital campaigns in Texas, the state we study in this paper, is about $7800" (pg. 14), depreciated over 15 years averaged over first 6 years	0.016, 0.03: Table 5 Standardized Test Scores 6 years after bond passage Reading and Math
Miller (2018)	High school graduation	0.1370	$1371.9 (2013$): specification, 0.1 times baseline spend $13,719.24 (pg. 30)	0.384: Table 4 10th Grade Cohort 1-4 years, standardized based on Table 1 Graduation Rate 4-year lag

Miller (2018)	Test scores	0.0520	$1371.9 (2013$): specification, 0.1 times baseline spend $13,719.24 (pg. 30)	0.775, 0.879, 0.929, 0.477: Table 5 4th Grade Math and Reading and 8th Grade Math and Reading, subject-combined then precision-weighted across grades
Neilson Zimmerman (2014)	Test scores	0.0250	$70000 (2005$): "about $70,000 in the New Haven SCP" (pg. 25), depreciated over 50 years averaged over first 6 years	0.153, 0.031: Table 6 > 5 Reading and Math, FE
Papke (2008)	Test proficiency rates	0.1650	$684.75 (2004$): 0.1 times baseline spend $6847.5 (Table 3 Average Expenditure per Pupil 1992-2004)	36.77: Table 7 Fixed Effects-Instrumental Variables log(average eral per pupil expend), standardized based on baseline Table 5 average 50th percentile first three years

Rauscher (2020)	Test scores	0.0290	$2840 (2014$): "narrowly passing a bond measure increases capital spending by $2,840 per student" (pg. 120-1), depreciated over 15 years averaged across first 6 years	47.77, 12.36: Table 4 models (3) and(6) 6 Years after election Low-SES achievement and High-SES achievement, to student-level standard deviation units extrapolating from "These estimates amount to 0.40 to 0.57 standard deviations..." (pg. 119), distributed across estimated students per school (NCES data)
Roy (2011)	Test scores	0.3800	$1000 (2010$): specification, "reading estimates... for every $1,000" (pg. 159)	0.057, 0.061: Table 8 Instrumental variables regressions Lagged spending 1998-2001 Reading and Math, standardized based on baseline SE (Footnote 35)
Weinstein Stiefel Schwartz Chalico (2009)	High school graduation	0.3790	$391.7 (2003$): Table 6 Direct Expenditure Title I model (2)	3.59: Table 8 Graduation Rate Title I model (2), standardized based on avg. national baseline graduation rate of 0.77

Weinstein Stiefel Schwartz Chalico (2009)	Test scores	-0.0540	$284.3 (2003$): Table 5: Direct Expenditure Title I model (2)	-0.011, -.031: Table 7 Title I Math and Reading

This describes the steps per *overall* study-outcome (and by spending type, relevant for Baron (2020)).

Table A.6: Summary of capital depreciation decision

Study	Depreciate over (years)	Life of project description
Baron (2020)	15	"bond funds are frequently used to repair, maintain, and modernize existing structures, rather than to build new schools" (23) // note that this doesn't exclude new building, but with cost of $4600 per pupil much more in-line with smaller-scale updates
Cellini Ferreira Rothstein (2010)	15	"Anecdotally, bonds are frequently used to build new permanent classrooms that replace temporary buildings (e.g., Sebastian (2006)), although repair, maintenance, and modernization are common uses as well' (220) // Table 1 average amount per pupil is of smaller magnitude than full-building construction
Conlin Thompson (2017)	15	this paper doesn't specify, and they translate effects into per-$1000 but the OH program was for both new construction and renovations
Goncalves (2015)	36.875	"I corresponded with an OSFC employee who reported that about 60-65
Hong Zimmer (2016)	26.9	for the three years of data they have more detailed spending, percent new building is about 34
Lafortune Schonholzer (2019)	50	"We restrict attention only to large new school construction project" // "Nearly $11 billion was spent over this period, about 86

Martorell Stange McFarlin (2016)	15	"typical capital campaigns deliver only modest facility improvements for the average student" (14) // "evidence is stronger for the claim that capital campaigns increase exposure to renovated schools" (20)
Neilson Zimmerman (2014)	50	"Of 42 school buildings, 12 had been rebuild completely by 2010, and 18 had been significantly renovated. . . school renovations were generally substantial, incurring costs similar to those of new construction" (20)
Rauscher (2020)	15	looks at CA bonds, which "can be used only for construction, rehabilitation, equipping school facilities, or acquisition/lease of real property for school facilities" (113)

Table A.7: Studies with LI and non-LI estimates

Study	Outcome	non-LI $	LI $	non-LI effect	LI effect	LI definition
Abott Kogan Lavertu Peskowitz (2020)	Test scores	279.99	609.19	0.2572	0.0460	"compare spending and educational outcomes between districts that are above or below our sample median in terms of poverty rates among 5–17-year-olds (according to the American Community Survey)" (9)
Abott Kogan Lavertu Peskowitz (2020)	High school graduation	279.99	609.19	0.1396	0.0295	"compare spending and educational outcomes between districts that are above or below our sample median in terms of poverty rates among 5–17-year-olds (according to the American Community Survey)" (9)

Baron (2020)	College enrollment	489.26	489.26	0.1916	1.5968	"I classify a school district as having an initially-high share of economically disadvantaged students if its share falls above the median of the Wisconsin 2000-01 school district distribution (the earliest year this variable is made publicly available)." (18)
Baron (2020)	Test scores	489.26	489.26	0.0237	0.3785	"I classify a school district as having an initially-high share of economically disadvantaged students if its share falls above the median of the Wisconsin 2000-01 school district distribution (the earliest year this variable is made publicly available)." (18)

Brunner Hyman Ju (2020)	Test scores	527.60	527.60	0.0303	0.0682	"We separate the effects of SFRs by within-state 1980 income terciles because reforms were designed to differentially impact state aid for low- and high-income districts, with the goal of equalizing school funding" (478)
Candelaria Shores (2019)	High school graduation	915.52	915.52	0.0528	0.3685	"state-specific poverty quartiles, defined using free lunch eligibility status" (39)
Goncalves (2015)	Test proficiency rates	1160.92	1160.92	-0.0048	0.0077	Poorest 25% (Table 3)
Hyman (2017)	College enrollment	1093.70	1093.70	0.1590	0.0111	"districts with below-median 1995 district-level fraction receiving free lunch" (276)
Jackson Johnson Persico (2015), Jackson Johnson (2019)	High school graduation	710.59	686.24	0.0654	0.2709	"...a child is defined as low income if parental family income falls below two times the poverty line for any year during childhood" (165)

Johnson (2015)	High school graduation	123.95	123.95	0.1321	0.8094	
Kreisman Steinberg (2019)	Test scores	1116.33	1116.33	0.0264	0.0618	tercile of poverty (economically disadvantaged) (Table 6)
Kreisman Steinberg (2019)	High school graduation	1116.33	1116.33	-0.0199	0.2140	tercile of poverty (economically disadvantaged) (Table 6)
Lafortune Rothstein Schanzenbach (2018)	Test scores	672.62	1484.28	-0.0059	0.0189	"bottom or top quintile, respectively, of the state district-level income distribution" (Table 5)
Rauscher (2020)	Test scores	223.21	223.21	0.0162	0.0626	"The CDE defines low-SES students as those who are eligible for free or reduced-price lunch *or* whose parents both have less than a high school diploma…I refer to the distinction as SES throughout the article" (114)

Sign indicates the sign of the overall effect for the study's full sample. Reported effect and standard error are based on the four-year effects (described in Section 3).

This represents all studies included in our meta-analyses which report separate effects for LI and non-LI populations. The studies not included in our analyses, but relevant for identifying whether effects of spending are generally larger for LI populations include: Biasi (2019) on income mobility, Card & Payne (2002) on test score gaps, JJP (2015) on wages and poverty, Johnson (2015) on wages and poverty. These papers all find either a decrease in outcome gaps between LI and non-LI groups, or specifically more pronounced effects for LI individuals exposed to increased spending. This assumes the *same* dollar change for LI and non-LI districts in Hyman (2017). Without additional information about within- and across-district demographic heterogeneity, we are unable to capture (potentially) different spending changes for LI and non-LI students despite evidence in the paper which suggests money was distributed disproportionately to non-LI schools within districts.

A.4 Sensitivity and Robustness Analyses

Correlation bounds

Our preferred analysis assumes 0.5 correlation between dependent effects (math/reading) and 0 correlation between independent effects (across grades or populations). We re-run our main specifications with updated assumed correlations between effects within studies to generate one overall effect per study. We re-run our main specifications with assumed correlations for dependent effects from 0.25 to 0.75 and for independent effects from 0 to 0.5.

Table A.8: Meta-Analysis (w/in pop. low (0.25) // across pop. low (0))

	(1)	(2)	(3)	(4)	(5)
	Overall Test Scores	Non-Capital Test Score	Capital Test Score	Overall Test Scores	Overall Educational Attainment
Average Effect	0.0441***	0.0476***	0.0355***	0.0475***	0.163***
	(0.00929)	(0.0125)	(0.00934)	(0.0125)	(0.0250)
Capital				-0.0113	
				(0.0161)	
N	24	15	9	24	12
τ	0.0288	0.0303	0.0272	0.0299	0.0534
% Cross-Study Var.	0.702	0.775	0.489	0.716	0.391

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.9: Meta-Analysis (w/in pop. low (0.25) // across pop. high (0.5))

	(1)	(2)	(3)	(4)	(5)
	Overall Test Scores	Non-Capital Test Score	Capital Test Score	Overall Test Scores	Overall Educational Attainment
Average Effect	0.0441***	0.0479***	0.0347***	0.0477***	0.162***
	(0.00946)	(0.0128)	(0.00906)	(0.0127)	(0.0244)
Capital				-0.0120	
				(0.0163)	
N	24	15	9	24	12
τ	0.0292	0.0308	0.0264	0.0302	0.0513
% Cross-Study Var.	0.697	0.775	0.447	0.711	0.372

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.10: Meta-Analysis (w/in pop. high (0.75) // across pop. low (0))

	(1)	(2)	(3)	(4)	(5)
	Overall Test Scores	Non-Capital Test Score	Capital Test Score	Overall Test Scores	Overall Educational Attainment
Average Effect	0.0436***	0.0476***	0.0332***	0.0474***	0.163***
	(0.00926)	(0.0125)	(0.00739)	(0.0124)	(0.0250)
Capital				-0.0130	
				(0.0153)	
N	24	15	9	24	12
τ	0.0282	0.0301	0.0238	0.0293	0.0534
% Cross-Study Var.	0.664	0.751	0.374	0.681	0.391

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.11: Meta-Analysis (w/in pop. high (0.75) // across pop. high (0.5))

	(1)	(2)	(3)	(4)	(5)
	Overall Test Scores	Non-Capital Test Score	Capital Test Score	Overall Test Scores	Overall Educational Attainment
Average Effect	0.0436***	0.0477***	0.0323***	0.0475***	0.162***
	(0.00943)	(0.0127)	(0.00688)	(0.0126)	(0.0244)
Capital				-0.0138	
				(0.0154)	
N	24	15	9	24	12
τ	0.0285	0.0305	0.0218	0.0295	0.0513
% Cross-Study Var.	0.658	0.751	0.305	0.674	0.372

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Depreciation

Our preferred analysis assumed buildings are depreciated 50 years and non-buildings are depreciated 15 years. We re-run our main specifications with lower and upper bounds on years across which capital investments are depreciated. At a lower bound, we depreciate buildings at 30 and non-buildings at 10 years. At an upper bound, we depreciate buildings at 50 and non-buildings at 30 years.

Table A.12: Depreciation Sensitivity Meta-Analysis

	(1) Baseline	(2) Low Bound (years dep.)	(3) High Bound (years dep.)
Average Effect	0.0341***	0.0280***	0.0421***
	(0.00822)	(0.00664)	(0.0112)
N	9	9	9
τ	0.0255	0.0211	0.0312
% Cross-Study Var.	0.428	0.431	0.442

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Clustering like policy studies

We present our main meta-analyses, using an extreme conservative approach to assigning dependence between estimates of the same policies (across different studies) by clustering those estimates as if they stemmed from the *same* study.[56] We assign dependence for studies of an Ohio capital subsidy program (Conlin and Thompson (2017), Goncalves (2015)), Michigan Proposal A (Hyman (2017), Papke (2008), Roy (2011), School Finance Reforms (Lafortune et al. (2018), Brunner et al. (2020)), and Title I (Cascio et al. (2013), Johnson (2015)). While the pooled averages are slightly attenuated, the prediction intervals are narrower leading to *stronger* evidence of positive impacts (Table A.13). Similarly, our main findings by income-level are more pronounced (Table A.14).

[56] We use the "study()" option in the "robumeta" Stata command.

Table A.13: Meta-Analysis Estimates, Cluster Same Policies

	(1) Overall Test Scores	(2) Non-Capital Test Score	(3) Capital Test Score	(4) Overall Test Scores	(5) Overall Educational Attainment
Average Effect	0.0365***	0.0373***	0.0370***	0.0372***	0.157***
	(0.00576)	(0.00735)	(0.0119)	(0.00751)	(0.0213)
Capital				-0.00158	
				(0.0113)	
N	24	15	9	24	12
τ	0.0195	0.0195	0.0310	0.0208	0.0417
% Cross-Study Var.	0.500	0.571	0.526	0.531	0.281
90% PI	[0.003,0.070]	[0.003,0.072]	[-0.018,0.092]		[0.080,0.234]
Prob. Pos	0.964	0.964	0.868	0.942	1.000

Standard errors in parentheses

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.14: Meta-Regressions w/ LI, Cluster Same Policies

	(1) Test Scores	(2) Test Scores	(3) Educational Attainment	(4) Educational Attainment
Average Effect	0.0338***	0.0380***	0.196***	0.193***
	(0.00673)	(0.00626)	(0.0623)	(0.0623)
Low-Income	0.0242***		-0.0540	
	(0.00904)		(0.0873)	
Low-Income (w/ Title I)		0.00190		-0.0430
		(0.0209)		(0.0861)
Non-Low-Income	-0.0130	-0.0171**	-0.127*	-0.123*
	(0.00820)	(0.00788)	(0.0740)	(0.0739)
N	31	31	19	19
τ	0.0191	0.0203	0.0583	0.0591
% Cross-Study Var.	0.482	0.512	0.286	0.292
Low-Income = Non-LI (p-val)	1.83e-09	0.358	0.383	0.328

Standard errors in parentheses

All Low-Income Estimates are comparisons with Non-Low-Income except in the case of Goncalves (2015).

Low-Income w/ Title I is an indicator that additionally captures all Title I studies, even those which do not present distinct by-income effects.

* $p < .1$, ** $p < .05$, *** $p < .01$

A.5 Linearity in Spending

One of our implicit modelling assumptions is that the effect of a policy on outcomes would be related linearly to the size of the spending increases caused by the policy. To test this directly, in Figure A.1 we plot the raw, standardized policy effect on student outcomes against the change in per pupil expenditures ($2018) caused by the the same policy. Each study is represented by a circle, and larger circles indicate more precise outcome estimates. We also report the resulting coefficients from precision-weighted OLS regressions. Our linearity assumption is well-supported by the data, and adding a quadratic-in-spend term does not improve fit. That is, the precision-weighed regression line relating the policy induced outcome change to the policy-induced spending change fits the data very well for both test scores and educational attainment. Also, for both outcomes, one fails to reject the quadratic model (the p-value on the quadratic terms yield p-values larger than 0.1). It is worth noting that the regression of the policy-induced outcome change on the policy-induced spending change is an instrumental variables (IV) estimate of the impact of spending on outcomes using the policy-induced changes as instruments.

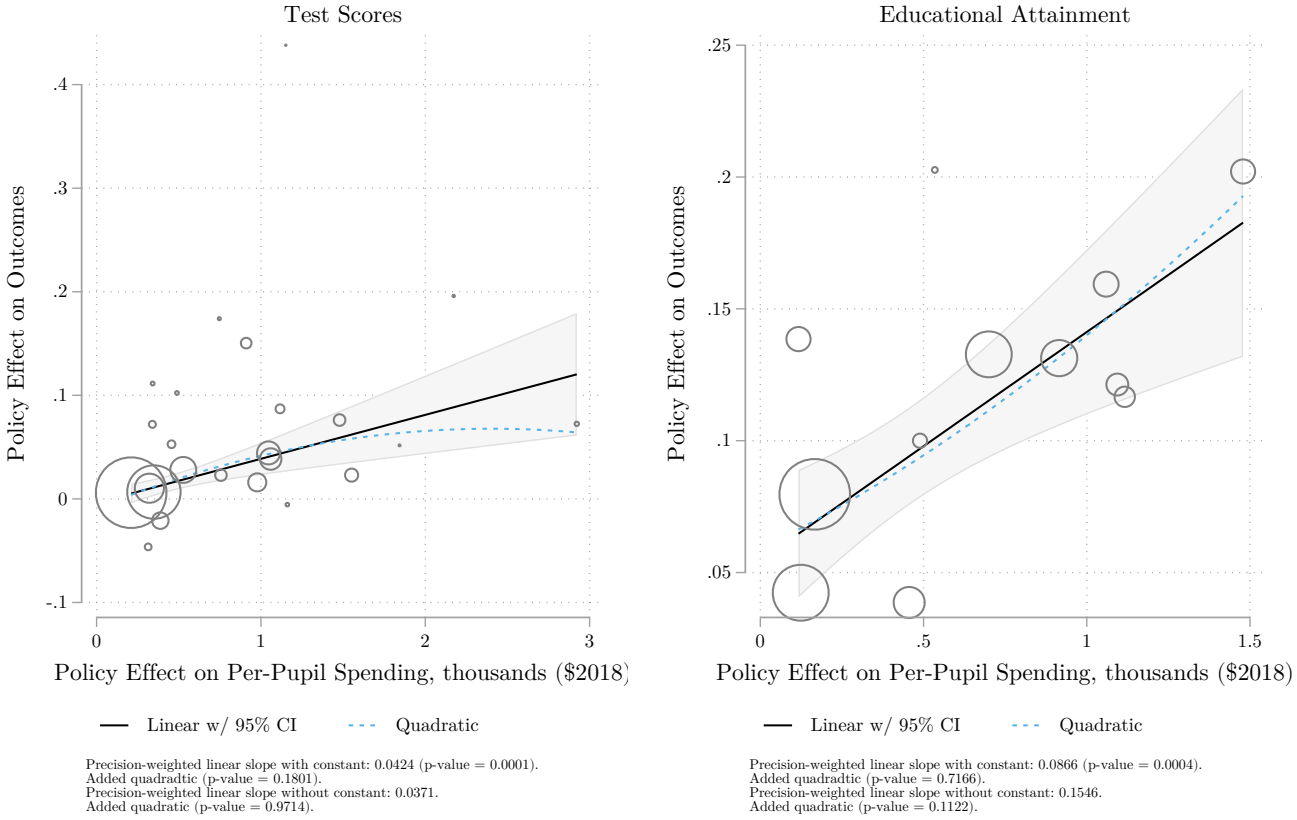


Figure A.1: Linearity in Spending

A.6 Capital Over Time

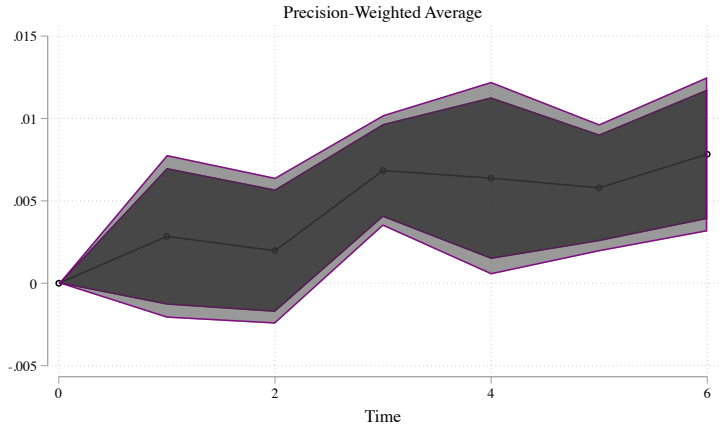


Figure A.2: Capital Spending Effect Estimates Over Time (Precision-Weighted)

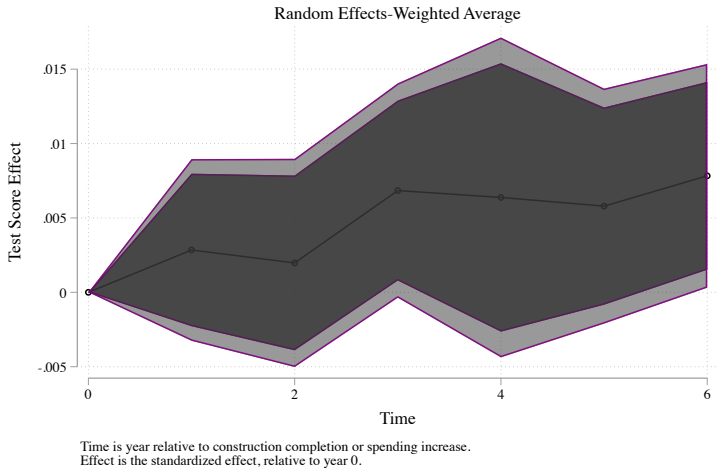


Figure A.3: Capital Spending Effect Estimates Over Time (RE Precision-Weighted)

A.7 Forest Plots by Spending Type

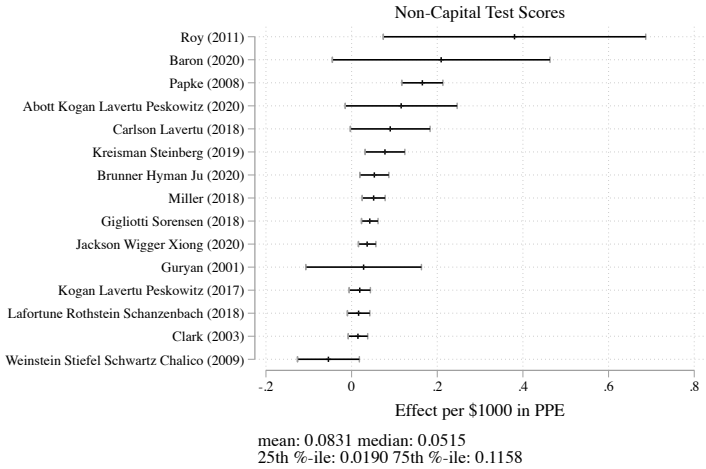


Figure A.4: Non-Capital Test Score

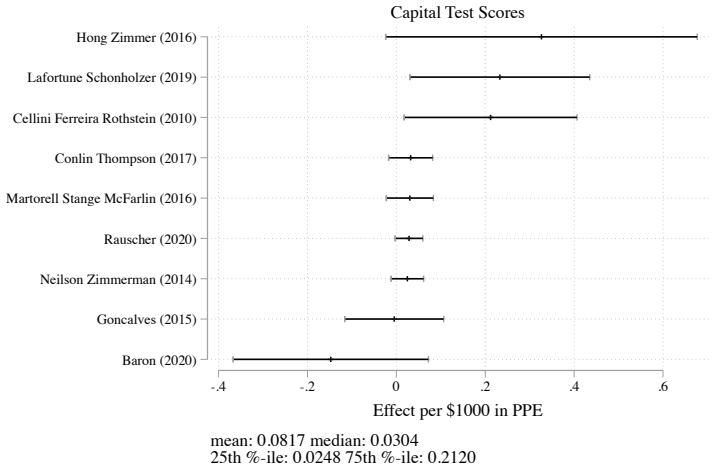


Figure A.5: Capital Test Score

A.8 Additional Tests by Income Level

We present by-outcome vote count for comparisons between low-income and non-low-income estimates in Table A.15.

Table A.15: Coin Test for Studies w/ LI and non-LI Estimates

Outcome	Papers	LI> non-LI	% LI> non-LI	1 in X Chance
All Studies	14	11	0.79	35
Test Score	7	6	0.86	16
Educational Attainment	7	5	0.71	4

LI > non-LI represents the count (or percent) of studies whose effect per $1000 for non-LI populations is larger than the effect for LI populations.

We present models which restrict low-income estimates to only include those studies for which estimated impacts on spending are clearly reported separately by income in columns 2 and 4 of Table A.16.

Table A.16: Meta-Regressions w/ LI

	(1) Test Scores	(2) Test Scores	(3) Test Scores	(4) Test Scores	(5) Educational Attainment	(6) Educational Attainment
Average Effect	0.0423***	0.0426***	0.0474***	0.0478***	0.223***	0.206***
	(0.0126)	(0.0129)	(0.0124)	(0.0126)	(0.0780)	(0.0745)
Low-Income	0.0112	0.00596			-0.0998	
	(0.0181)	(0.0202)			(0.102)	
Low-Income (w/ Title I)			-0.00499	-0.0134		-0.0573
			(0.0197)	(0.0219)		(0.102)
Non-Low-Income	-0.0246*	-0.0272	-0.0296**	-0.0324**	-0.138	-0.122
	(0.0149)	(0.0165)	(0.0147)	(0.0163)	(0.0890)	(0.0858)
N	31	25	31	25	19	19
τ	0.0282	0.0297	0.0281	0.0293	0.0774	0.0807
% Cross-Study Var.	0.669	0.716	0.667	0.710	0.415	0.435
Low-Income = Non-LI (p-val)	0.0183	0.0774	0.151	0.360	0.622	0.429

Standard errors in parentheses

All Low-Income Estimates are comparisons with Non-Low-Income except in the case of Goncalves (2015).

Low-Income w/ Title I is an indicator that additionally captures all Title I studies, even those which do not present distinct by-income effects.

* $p < .1$, ** $p < .05$, *** $p < .01$

A.9 Details of Publication Bias Tests

Table A.17 presents our preferred estimates (columns 1 and 5) along with estimates using several approaches to potential publication bias.

Table A.17: Meta-Regressions w/ Publication Bias Adjustments

	Test Scores				Educational Attainment			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Avg. Effect	0.0438***	0.039***	0.0426***	0.044***	0.163***	0.145***	0.138***	0.145***
	(0.00925)	(0.006)	(0.00936)	(0.0102)	(0.0250)	(0.0213)	(0.00568)	(0.03418)
N	24	24	12	26	12	12	6	15

Standard errors in parentheses

Test Score: (1) Robumeta (2) Andrews & Kasy (3) SE < .026 (4) Meta Trim&Fill

Educational Attainment: (5) Robumeta (6) Andrews & Kasy (7) SE < .08 (8) Meta Trim&Fill

* $p < .1$, ** $p < .05$, *** $p < .01$

1. Studies that find null results may be less likely to be published than studies that find significant effects (Franco et al. (2014), Christensen and Miguel (2018)). If one is able to observe studies that are not published, a simple test for publication bias compares estimates from studies that are published to those that are not published. In line with this, we compare average estimates of published and unpublished studies and find no difference in impacts.[57] In Table A.18, the coefficients on the indicator for "Unpublished" show no evidence that there is any difference in average effects reported in published versus unpublished papers for both test scores and educational attainment outcomes.

2. Related to the first test, if there are biases against publication of certain kinds of studies, one might expect these biases to be most pronounced at the most selective journals (Brodeur et al. (2016)). Informed by this notion, we compare the average impacts of studies published in the most elite journals to studies published in other journals, and similarly find no differences across journal prestige (in columns 2 and 4 of Table A.18, the formal tests of equality across publication type and publication status yield p-vals of 0.699 and 0.675 for test scores and educational attainment, respectively). That is, we do not find evidence that publication status or type have any bearing on the estimates reported in studies of effects of school spending.

3. Publication bias is thought to be most prevalent among imprecise studies (Andrews and Kasy (2019)), and when there are biases against publication of insignificant studies, one might observe an over-representation of studies right at the significance threshold (in social sciences this would be the 5 percent level pertaining to a t-statistic of 1.96) and an under-representation of studies right below the significance threshold (Brodeur et al. (2020)). To

[57] Of course, we cannot observe the unobservable – or those papers which are fully not shared in any form, published or not.

test for this in our data, we test for a discontinuity in the cumulative density of t-statistics at 1.96. We show that there is no over-representation of studies right at the significance threshold (t-statistic = 1.96) in Figure A.7. In Table A.19, we show that there is no significant jump in density, by outcome type or combining across both test score and education attainment outcomes, at the significance threshold (t-stat > 1.96).

4. Even though we find limited evidence of selection of significant impacts, we implement a model that accounts for any such selection (should it exist). To this aim, we show results for the Andrews and Kasy (2019) selection adjustment using their web application in Figures A.8 and A.9. They propose estimating the publication probabilities (based on the t-statistics) for studies, and using these probabilities to produce bias-corrected estimators and confidence sets. More specifically, using the relative publication probabilities, this approach re-weights the distribution of studies to account for differences in publication probability (up-weighting studies that are least likely to be observed). For both test scores and educational attainment, their model fails to reject the null of no selection at the 1.96 t-statistic threshold. Reassuringly, their adjustment approach yields similar estimates to our preferred model (columns 2 and 5 of Table A.17).

5. We test whether there is bias against imprecise, negative estimates. In a stylized world, with no publication bias, a scatter plot of study impacts against the precision of each study should be roughly symmetric around the grand mean (Borenstein (2009)). However, with publication bias, the scatter plot around the grand mean will be asymmetric – suggesting that there are some "missing" studies. In this stylized world with publication bias, while all or most precise studies will be published, there may be an over-representation of published imprecise estimates in the "desired" direction and no (or few) published imprecise estimates in the "undesirable" direction. We account for this kind of publication bias in two ways: First, we impute "missing" (imprecise, negative) studies and re-estimate our models. Second, we separately drop the least precise estimates (the least-precise half) and re-estimate our models. Neither appreciably impacts our estimates.

   We visualize the Duval and Tweedie (2000) "trim and fill" approach in Figure A.6, where black circles indicate the individual study impacts. The distribution of effects are largely symmetrical around the mean for very precise studies (at the top of the figures), but the distribution may be asymmetric for studies with standard errors greater than about 0.13 and 0.15 for test scores and education attainment, respectively (the bottom of the plots). That is, while there is little visual evidence of publication bias among precisely estimated studies, there is some suggestive evidence that imprecise positive studies with large impacts may be more likely to be published (or written) than imprecise studies with negative or small impacts. To be clear, because (a) our inclusion criteria requires that the policy has meaningful impacts on school spending and (b) one would expect there to be some effect heterogeneity across states and policies, some asymmetry is likely even absent publication bias. Even so, to be

conservative one can assume that any asymmetry is due to publication bias, and assess the impacts of this asymmetry on the estimated pooled average. We follow this approach.

In the left panel of Figure A.6, to create symmetry, the "trim and fill" approach imputes two "missing" studies of test score outcomes (green triangles) – both of which are negative and very imprecise. These imputed studies are outside of the more precise range employed for our first test of bias – validating that approach. The re-estimated pooled effect that includes these two additional imputed studies is 0.044 (Table A.17 column 4) – very similar to our original estimate including all observed estimates. Following this same approach for educational attainment, "trim and fill" imputes three additional negative and relatively imprecise estimates. The re-estimated pooled effect that includes the three additional imputed studies is 0.145 (Table A.17 column 8) – also similar to our original estimate including all observed estimates. The fact that estimates do not change very much with the imputed data also reflects the fact that the evidence of asymmetry is only among very imprecise estimates, which receive lower weight in our precision-weighted pooled average. This suggests that the impacts of any *potential* publication bias on our estimates are small (at most creating a bias of 5 percent).

When we estimate our main model on all studies using a drastic approach of dropping the majority of the data (Stanley et al. (2010)), specifically those test score studies with an estimated standard error of 0.026 or less (Table A.17 column 3) and educational attainment studies with estimated standard errors of 0.08 or less (Table A.17 column 7), our results are similar to our main models. We indicate these precision levels in the higher horizontal lines in the funnel plot in Figure A.6. Above this cut-off, estimates are very tightly clustered around the pooled average.[58] In this most precise sample (where there is no evidence of asymmetry), the coefficient estimate for test scores is 0.0426 (Table A.17 column 3). This is very similar to our preferred estimate – indicating minimal bias. Following this same approach for educational attainment, when we restrict our sample to studies with standard errors below 0.08, the Egger's tests indicates no asymmetry, and the regression estimate is 0.138 (Table A.17 column 7).[59]

In sum, across multiple approaches to testing and accounting for potential publication bias, our main results hold, suggesting that if this bias exists it is minimal – our conclusions do not change.

[58] The p-values on both the intercept and slope associated with the Egger's test for this sample are both above 0.1.

[59] The Eggers test is the simply the p-value associated with the y-intercept being different from zero in a regression on the study effects against its precision. When the funnel is asymmetric, this p-value will be small.

Table A.18: Meta-Regressions w/ Publication Type

	(1) Test Score	(2) Test Score	(3) Educational Attainment	(4) Educational Attainment
Unpublished	-0.0160	-0.00468	-0.0438	-0.0142
	(0.0215)	(0.0213)	(0.0408)	(0.0294)
Top Field Journal		-0.00182		
		(0.0152)		
Field Journal		0.0319		0.102
		(0.0293)		(0.154)
Average Effect	0.0487***	0.0373***	0.180***	0.151***
	(0.0114)	(0.0102)	(0.0395)	(0.0262)
N	24	24	12	12
τ	0.0302	0.0334	0.0682	0.0861
% Cross-Study Var.	0.706	0.746	0.512	0.625
Top Field = Field = Unpublished = 0 (p-val)		0.699		0.675
Unpublished = 0 (p-val)	0.456	0.826	0.284	0.628

Standard errors in parentheses

Reference category High Impact ommitted.

High Impact: American Economic Journal, Quarterly Journal of Economics, Review of Economics and Statistics, Sociology of Education.

Top Field: Journal of Econometrics, Journal of Public Economics.

Field: Economics of Education Review, Education Economics, Education Finance and Policy, Educational Evaluation and Policy Analysis, Public Finance Review, Russell Sage Foundation Journal of the Social Sciences, Journal of Public Administration Research and Theory, Journal of Urban Economics

* $p < .1$, ** $p < .05$, *** $p < .01$

Table A.19: Regressions to test for jump at 5% significance, Outcome: Cumulative T-stat density

	(1) Test Scores $1 < tstat < 3$	(2) Educational Attainment $1 < tstat < 3$	(3) All Outcomes $1 < tstat < 3$	(4) All Outcomes $1.5 < tstat < 2.5$
Sig, 5%-level (ind)	-0.0195	0.0885	0.0405	-0.0336
	(0.0241)	(0.0747)	(0.0402)	(0.0251)
N	14	5	19	12

Standard errors in parentheses

All models include controls for the t-stat and the square of the t-stat.

In colums 3 and 4 pooled models (with both outcome types) we include an indicator for the outcome and interact t-stat and t-stat squared with the outcome.

* $p < .1$, ** $p < .05$, *** $p < .01$

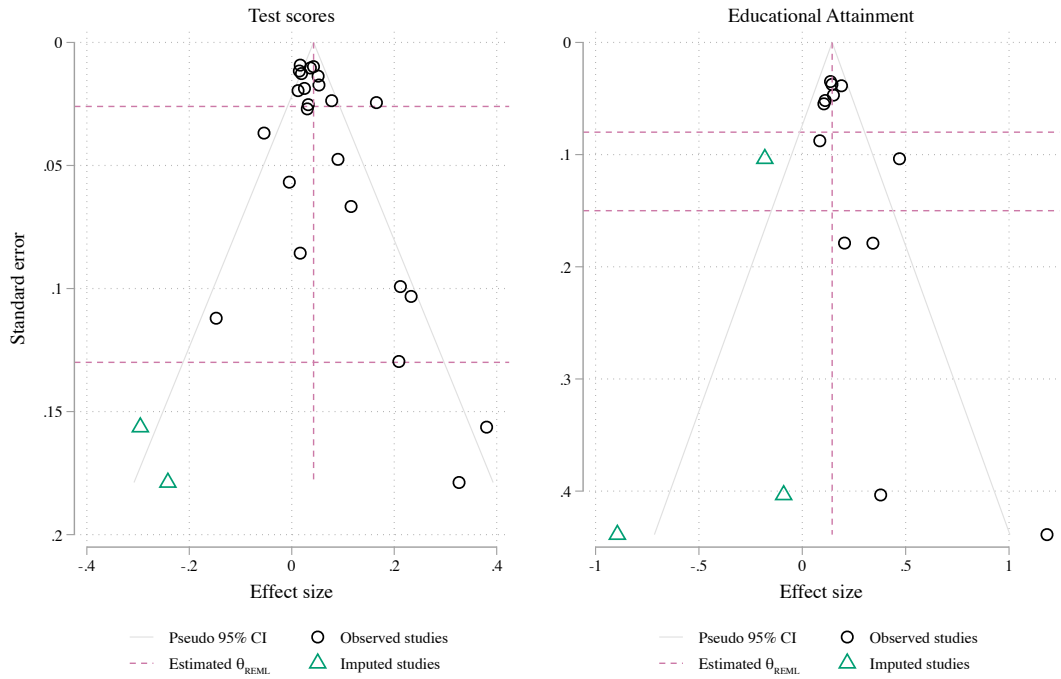


Figure A.6: Funnel Plots

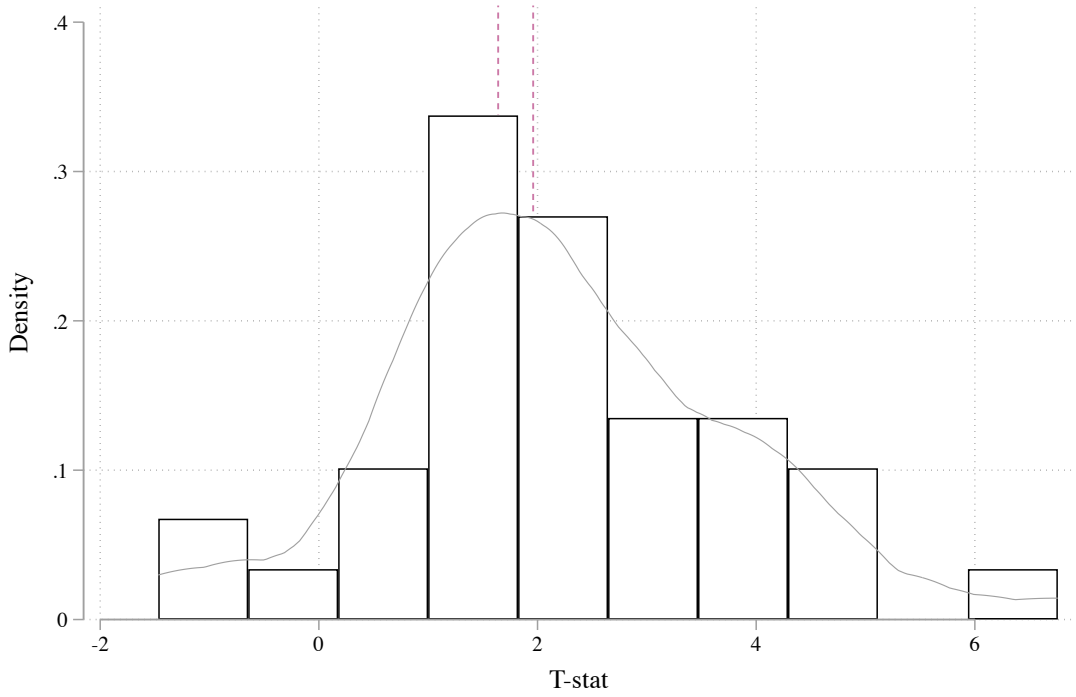


Figure A.7: Histogram of *all* effects

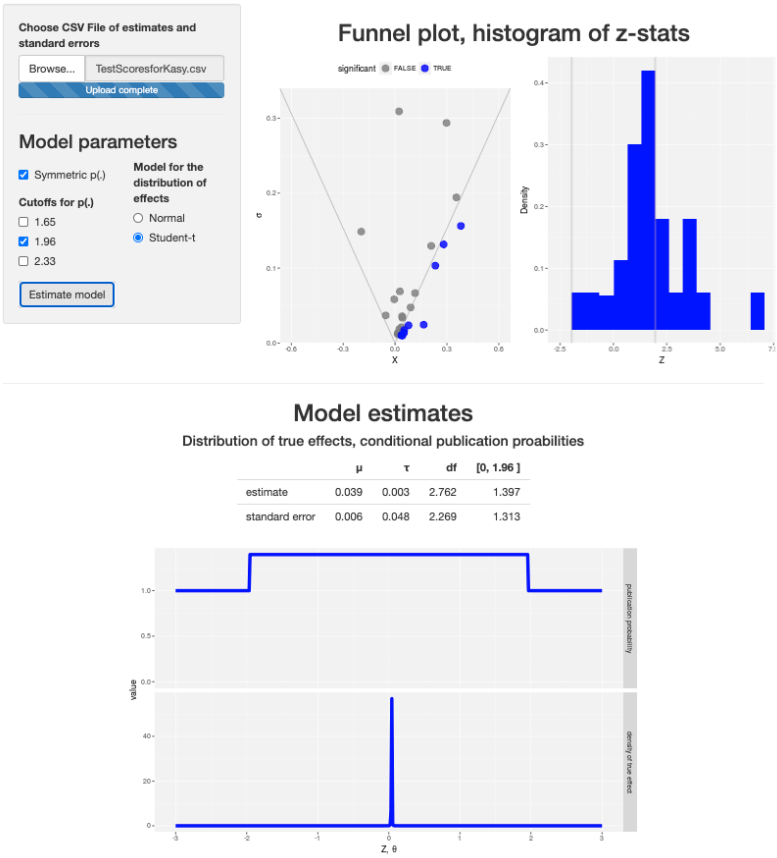
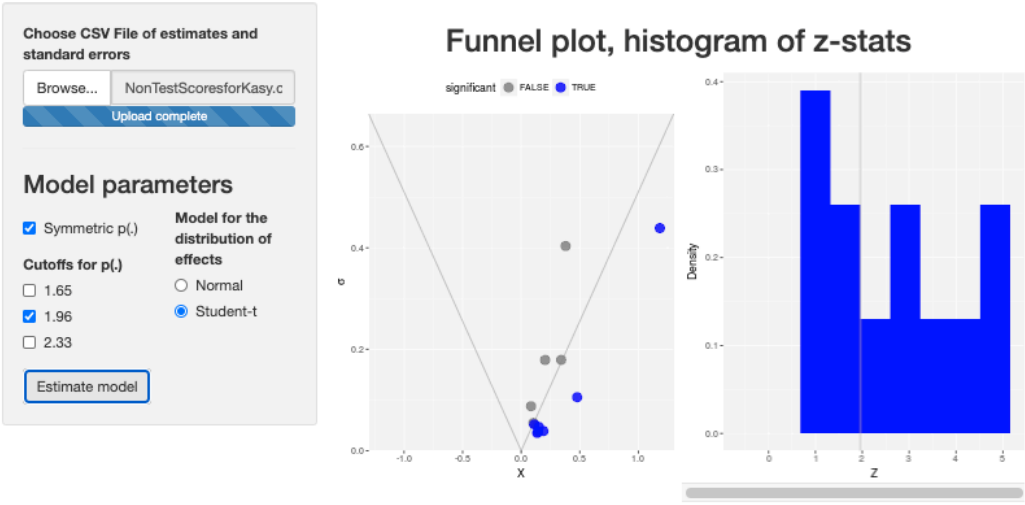


Figure A.8: Test Scores



Model estimates

Distribution of true effects, conditional publication proabilities

	μ	τ	df	[0, 1.96]
estimate	0.145	0.000	4.229	0.741
standard error	0.013	0.000	2.506	0.699

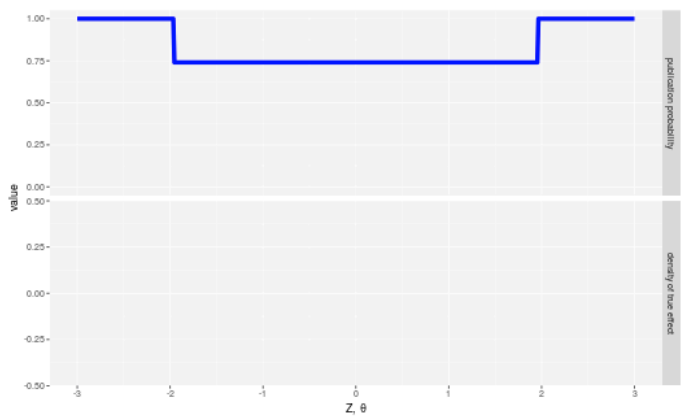


Figure A.9: Non-Test Scores