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Does School Spending Matter? The New Literature on an Old Question

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ABSTRACT

Social scientists have long sought to examine the causal impact of school spending on child outcomes. For a long time, the literature on this topic was largely descriptive so that it had been difficult to draw strong causal claims. However, there have been several recent studies in this space that employ larger data-sets and use quasi-experimental methods that allow for much more credible causal claims. Focusing on studies of students in the United States, this paper briefly discusses the older literature and highlights some of its limitations. It then describes a recent quasi-experimental literature on the impact of school spending on child outcomes, highlights some key papers, and presents a summary of the recent findings. Policy implications and areas for future research are discussed.

Does School Spending Matter? The New Literature on an Old Question

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I Introduction

There has been a long-standing debate regarding whether increasing the financial resources available to public schools can improve child outcomes in general, and low-income children's outcomes in particular. Shedding light on this issue, in 1966, James Coleman and coauthors conducted the first large-scale U.S. study to link student achievement outcomes to family background and school characteristics. The report employed data from a cross-section of students in 1965 and examined the cross-sectional relationship (i.e. at a given point in time) between school spending, family background and test scores. The report concluded that "*it is known that socioeconomic factors bear a strong relation to academic achievement. When these factors are statistically controlled, however, it appears that differences between schools account for only a small fraction of differences in pupil achievement*" (pp. 21-22). Since Coleman et al. (1966), many social scientists have estimated the relationship between school spending and student outcomes in order to better ascertain whether increased financial resources for public schools improve child outcomes.

II The Old Literature

Prior to 1995, all U.S.-based studies relating student outcomes to measures of per-pupil spending were observational (i.e. correlational) in nature. These studies either (a) estimated the relationship between school spending and student outcomes after accounting for family background, (b) estimated the relationship between changes in school spending over time within a particular geographical area (such as a state or district) and changes in student outcomes after accounting for family background, or (c) some combination of the two.

II.1 Spending and Outcomes Tend to Move Together

Summarizing these older studies, in an influential literature review, Hanushek (2003) examines the findings of 163 studies relating school resources to student achievement that were published prior to 1995. When assessing the results of several studies, it is helpful to consider what one would observe if school spending were indeed unrelated to student outcomes. If school spending and student outcomes were unrelated, then 2.5 percent of studies should be significant and positive and 2.5 percent should be significant and negative (with a two sided p-value of less than 0.05). Hanushek (2003) finds that 27 percent of these early studies were statistically significant and positive while 7 percent were significant and negative – there are more than 10 times as many positive and significant studies than would be expected by random chance alone if the true effect were zero.

It is important to point out that the 163 studies include both single-state studies and those that combine multiple states. This distinction is important. If school spending effects are heterogeneous, they may be large in some states and nonexistent in others. Accordingly, if school spending matters *on average*, but the effects are heterogeneous, one should see larger impacts in multi-state studies (which may reflect the average of positive effects and some null effects) than in individual state studies (some of which could be real null effects). This is exactly what Hanushek's data show. Among the 74 studies that used multiple states, 35 percent were statistically significant and positive while only 1 percent were statistically significant and negative. While Hanushek looks at this and concludes that there is little association between resources and outcomes, the statistical reasoning dictates otherwise. Indeed, Hedges et al. (1994) conduct a formal meta-analysis of much of the data studied in Hanushek (2003) and conclude that the older studies on school spending and student outcomes suggest a strong association between school spending and student outcomes. To put it bluntly, any claim that there is little evidence of a statistical link between school spending and student outcomes is demonstrably false.

II.2 The Old Literature Should Not be Taken as Causal

While the literature on school spending and student outcomes prior to 1995 indicate a real and economically meaningful association between school spending and student outcomes, these pat-

terns do not speak to the question of whether money matters. That is, the evidence produced before 1995 do not provide strong evidence for or against the notion that there is a causal relationship between increased school spending and improved student outcomes. This is for two key reasons.

The first reason is straightforward. The older studies compared students from different households across schools, so that the observed relationships are correlational. Simply comparing outcomes among families that attend schools with different levels of spending does not yield a causal relationship because there may be many other differences between these families and schools. While one can always include additional controls to help mitigate omitted variables bias, if there are any remaining unobserved student characteristics that predict achievement and are correlated with school spending (or family background, or any other covariate in the models) then the estimated relationship will be biased. As the common saying goes "*correlation does not imply causation*" and similarly "*lack of correlation does not imply lack of causation*." To complicate this approach further, adding additional controls can in some cases exacerbate any underlying biases and influence the result in unpredictable and counter-intuitive ways (see Elwert and Winship 2014, Lechner 2008). The second reason is subtler. Even if there are no omitted variables or confounding, because school spending is a function of family background (since families select into neighborhoods based on schools, and school spending is based, in part, on property values that are a function of wealth) it can be difficult to disentangle the two. If one does not appropriately model the relationships between school spending, family background, and student outcomes, a regression model using observational variation is unlikely to correctly attribute "blame" to the correct variable.[1]

One can overcome these limitations of the older literature by relying on exogenous (i.e., external) shocks to school spending that are both (a) unrelated to other determinants of student outcomes, and (b) not driven by the decisions of the individual families under study. With such independent variation in school spending, one can then credibly disentangle school spending from family background, and disentangle variation in school spending from other underlying differences. This is the approach taken in the new literature on school spending.

III The New Literature on School Spending

The "credibility revolution" (Angrist and Pischke, 2010) in empirical economics started to take root in the mid-1990s. The vast majority of the studies examined in Hanushek (2003) were written before this "revolution" and would not be deemed credible by existing standards of evidence. This

[1]The intuition is as follows. If one does not properly specify the relationship between school resources and family background (say one uses a contemporaneous measure of school spending as opposed to a cumulative measure of school spending, or alternatively one includes spending in levels as opposed to logs), because school spending is often endogenous to family income in observational data, the specification errors in school spending are likely to be correlated with family income. In this scenario, the model will attribute the benefits of school spending to family income. I have verified this with simulated data.

is not to say that all studies on school spending written before 1995 are "wrong" or that the authors of these studies made mistakes. The point is that the older studies are not based on the most up-to-date or credible research designs, and should therefore not be taken as causal. Accordingly, to determine whether there is a causal relationship between school spending and student outcomes one should *only* examine studies that can be deemed credibly causal by current standards.

In the past five years, a new literature relating school spending to student outcomes has emerged. This new literature is heavily influenced by the credibility revolution and the studies rely on empirical models that employ exogenous independent variation in school spending in order to disentangle the influence of school spending from that of family background and other influences. What distinguishes the new research from the old research is that is it design based. That is, the new studies lay out a clear comparison group to which some treated group will be compared. These studies do not simply use any variation in school spending, but rather rely on changes in school spending that are known and understood. The ability to identify exactly why one family is exposed to more school spending than another is critical to assessing the extent to which the results in each study can be interpreted causally. In each of these newer studies, the source of variation in school spending is transparent, and that source can credibly be argued to be unrelated to family background and other attributes. While none of these studies is perfect, each study is clear about the possible sources of any bias, and each study conducts considerable sensitivity analysis on the main results. Here I will summarize the results from this more credibly causal set of studies and draw some conclusions.

III.1 School Finance Reforms (National Studies)

Ideally, one would want to run an experiment in which money was randomly dropped on some school districts but not others. To ensure that one makes comparison among similar populations, one could then compare the outcomes of cohorts that were in school during and after the money drop to the outcomes of cohorts from the same school district before the money drop. Note that the fact that the money is dropped from above ensures that it is not driven by the decisions of the individual school districts or parents. The fact that the timing and location of the money drop is random ensures that the places that received the money drop were not also areas in which families were becoming richer or poorer, etc. While such a money drop does not exist in reality, School Finance Reforms (SFRs) provide a context that approximates this idealized experiment.

A key source of variation used in many studies is school finance reforms. In most states, before the 1970s, local property taxes accounted for most resources spent on K12 schooling (Howell and Miller, 1997). Because the local property tax base is typically higher in areas with higher home values, and there are high levels of residential segregation by socioeconomic status, heavy reliance on local financing contributed to affluent districts' ability to spend more per student. In response to large within-state differences in per-pupil spending across wealthy/high-income and

poor districts, state supreme courts overturned school finance systems in 28 states between 1971 and 2010. Because of these court decisions, many states implemented SFRs that led to important changes in public education funding. Most of these court-ordered SFRs changed the parameters of spending formulas to reduce inequality in public-school spending and weaken the relationship between per-pupil school spending and the wealth and income level of the district.

Jackson et al. (2015) examine SFRs that occurred between 1971 and 1990. They compare the changes in spending in previously low-spending and high-spending districts in years before and after a court-mandated SFR. They classify districts as low- or high-spending based on whether their average per-pupil spending levels were in the bottom or top 25 percent of districts in their state as of 1972, before any reforms were implemented. Figure 1 shows that in states that passed SFRs, low-spending districts experienced greater increases in per-pupil spending than similar districts in non-reform states, while high-spending districts experienced decreases – reducing spending gaps between previously low- and high-spending districts in reform states.

Having established that court-mandated SFRs affected school spending differently in different kinds of districts, Jackson et al. (2016) use more detailed information about the specific reforms enacted in each state to "predict" how much of an increase each district would receive based on the behaviors of similar districts in other states passing similar kinds of reforms. The basic idea behind this approach is as follows: if certain kinds of reforms have systematic and predictable effects on certain kinds of school districts, then one can predict district-level changes in school spending after a court-ordered SFR based *only* on factors that are unrelated to potentially confounding changes in unobserved determinants of school spending and student outcomes (e.g., local commitment to education or the state of the local economy). With this clean, predicted variation in spending, one can then test whether in those districts that are predicted (based on pre-reform characteristics) to experience larger reform-induced spending increases, cohorts exposed to the reform have better outcomes than unexposed cohorts. The increased financial resources that suddenly become available to some cohorts but not others within some districts but not others due to the statewide passage of a SFR approximates the money drop analogy laid out above. By relating outcomes with only the quasi-random reform-induced variation in school spending (rather than all variation in spending), one removes the confounding effect of unobserved factors that might influence both school spending and student outcomes (such as other local policies or changes in family background, etc.).

Figure 2 shows that exposed cohorts in reform districts predicted to experience larger per-pupil school spending increases during their school age years did experience larger spending increases, while exposed cohorts in reform districts predicted to experience smaller spending increases saw little change in school spending. Importantly, the figure also shows that predicted increases in per-pupil spending induced by SFRs are also correlated with increased years of educational attainment among exposed cohorts (relative to the unexposed cohorts). Using this variation in an instrumental

variables framework, they find that that a 10% increase in per pupil spending each year for all 12 years of public school leads to 0.31 more completed years of education, about 7% higher wages, and a 3.2 percentage-point reduction in the annual incidence of adult poverty. They also find that the effects are more pronounced for children from low-income families.

Unlike an observational study, this is a design-based study in which the source of the variation in school spending is well-defined and understood. Because the school spending changes used in this study are driven by state-level legislative action, they do not reflect individual family's decisions. Also, because the changes induced by the SFRs were outside of the control of local policy makers (since it was a statewide policy change), they are unrelated to other polices that may have been implemented by local authorities. While this empirical approach lends itself to credible causal inference, this is but a single study. To better understand if school spending matters, it is important to look at a range of different studies, each of which is similarly credible, but that use different data, different methods, and different samples.

III.2 Recent School Finance Reforms (National Studies)

Jackson et al. (2016) examine school spending impacts using variation due to court-ordered SFRs that occurred between 1972 and 1990. Because current school spending levels are more than twice as high than during the 1970s, and school spending may exhibit diminishing marginal returns, it is reasonable to wonder if one would observe positive school spending impacts at current school spending levels. Speaking to this question, there are a number of studies that analyze the impacts of recent school finance reforms that occurred between 1990 and present day. For example, Lafortune et al. (2018) study the impact of post-1990 school finance reforms on absolute and relative spending and achievement in low-income school districts. Using an event-study research design that exploits the apparent randomness of reform timing, they show that reforms lead to sharp, immediate, and sustained increases in school spending in low-income school districts. Using test score data from the National Assessment of Educational Progress (NAEP), they find that SFRs increase student achievement in these low-income districts. They conclude that a one-time $1,000 increase in per-pupil annual spending sustained for 10 years increased test scores by between 0.12 and 0.24 standard deviations.

Also, in a recent working paper Brunner et al. (2018) examine recent SFRs and explore impacts by union strength. Specifically, they examine whether teacher unions affected the fraction of reform-induced state aid that passed through to local spending and the allocation of these funds. They find that districts with strong teacher unions increased spending nearly dollar-for-dollar with state aid, and spent the funds primarily on teacher compensation. In contrast, districts with weak unions used aid primarily for property tax relief, and spent remaining funds on hiring new teachers. Importantly, they document that the greater expenditure increases in strong union districts led to

larger increases in student achievement. In another paper Candelaria and Shores (2017) provide evidence on the effect of court-ordered SFRs that took place between 1989 and 2010 on per-pupil revenues and high school graduation rates. They use similar event-study models to the other studies. They find that seven years after reform, the highest poverty districts in a reform state experienced an 11.5 to 12.1 percent increase in per-pupil spending and a 6.8 to 11.5 percentage-point increase in graduation rates. Consistent with diminishing marginal returns to school spending, these impacts are smaller than those documented in Jackson et al. (2016). However, these estimated impacts are large, economically important, and statistically significant. They indicate that increased school spending may have economically important effects even at current school spending levels.

In one of the few papers to examine the long-run impacts of recent school finance reforms, Biasi (2015) shows that equalizing school expenditure between high- and low-income districts increased income mobility for low-income students, with small negative effects on high-income pupils. These results are in line with Jackson et al. (2016) who show that increasing school spending improves long-run outcomes of disadvantaged students.

III.3 Changes in Underlying Revenue Sources (National Studies)

Given that SFRs are the result of some legislative action, it is helpful to have studies that rely on variation that is not a direct consequence of legislative action (which could potentially be endogenous to other policies). To address this critique, researchers have approximated the ideal experiment by taking the school funding formulas as given, and then examining what happens when the underlying variables of the formulas change. One such paper is Jackson et al. (2018). In this paper, the authors rely on the fact that during the Great Recession state tax receipts (state income taxes and sales taxes) fell very suddenly relative to that for local taxes or federal taxes. As a result, states in which funding formulas relied much more heavily on state taxes to fund education were also those that experienced the largest drops in their per-pupil revenues. Importantly, these changes were beyond the control of parents and policy makers. Also, because the reliance of states on state revenues was unrelated to recession intensity (that is, states that were more reliant on state taxes to fund public schools were no more or less likely to experience large economic downturns during the recession) these changes were unrelated to changes in underlying family characteristics.

To show the patterns visually, in the left panel of Figure 3, they plotted the trajectory over time in both per-pupil spending and standardized test scores before and after the Great Recession for states that were more heavily reliant on state revenues for public education (relative to those that were less reliant). Those states that were most heavily reliant on state taxes were those for which the declines in per-pupil spending were most severe after the recession. The figure also shows that these states had larger declines in standardized scores than other states. A similar figure is presented in the right panel of Figure 3 which shows that affected cohorts in these states also experienced lower

high-school completion rates. Using this recession-induced variation in an instrumental variables framework, they find that exposure to ten percent lower per-pupil spending over the previous four years leads to about 5 percent of a standard deviation lower test scores and 1.4 percentage-points lower graduation rates. The test score effects are similar in magnitude to those in studies based on spending increases.

Another similar paper is Miller (2017). This paper takes the funding formulas as given, and then asks what happens when state property values rise and fall. Because state funding formulas treat districts differently (by income levels, housing values, etc.), the same state-level change in house prices translates into different changes in per-pupil spending across individual districts within a state. Also, because different states use different funding formulas, the same increase or decrease in home values translates into different changes in the level of per-pupil spending across states. He estimates the effect of education spending on district-level student outcomes in 24 states by leveraging changes in revenue driven by property value variation. To do this, he feeds state-level changes in property values into the fixed school finance formulas that determine how state aid and local revenue respond to those changes to create a simulated instrument for school spending. Importantly, because the changes driven by the formulas are beyond districts and parents' control, the simulated school spending changes are highly predictive of real changes in revenue and spending but are unrelated to other district policies and underlying changes within districts. Using these simulated changes as instruments, a 10 percent increase in spending increases graduation rates by 3 to 5 percentage points and student test scores by 0.07 to 0.09 standard deviations. These estimated impacts are similar to those found in Jackson et al. (2018).

III.4 Overview of Multi-State Studies

The studies discussed thus far all rely on data from multiple states. As such, these studies do not answer the question of whether school spending matters in all states, or in all contexts, but rather provide estimates of whether school spending improves student outcomes on average. To gain a sense of all such recent US studies, I compiled a list of all studies on the impact of school spending on student outcomes that use some sort of quasi-experimental design (i.e. any non-observational study). I compiled this list by Google search, citation searches, and through consultation with active researchers in the field. To examine whether school spending matters on average, I first examine the estimated impacts for multi-state studies.

In the top half of Table 1, I summarize all of the national-level studies or multi-state studies. Of the 13 multi-state studies, 12 (92 percent) find a positive and statistically significant relationship between school spending and student outcomes. If each study were independent (which is perhaps an implausible scenario), one would not even expect a single study to be statistically significant and positive by random chance. If one were to be conservative, one could treat each source of

variation independently. There are school finance studies, tax limit studies, recession-based studies, house-price based studies, and studies that use the roll-out of Title-I. The single study that was not significant was a school finance reform study among which there are several positive estimated impacts. As such, using all five sources of variation, the evidence points to a causal positive impact of increased school spending on outcomes on average.

The fact that the early (and less credibly causal) multi-state studies indicate a positive relationship between school spending and student outcomes suggest that the positive association on average is real. The fact that this is also true using only the more recent credible design-based studies that rely on different samples, sources of variation and time periods, is compelling evidence that there is a positive causal relationship and that money does matter on average.

IV Money Does Not Always Matter: Within State Studies

Even though individual state-level studies and district-level studies cannot answer the question of whether money matters on average, they *can* answer the question of whether money matters in particular contexts and not others. Most of the state-level studies not only examine a particular location, but are also based on a particular kind of spending (as opposed to overall budget increases as in the national studies). Given that spending effects will likely depend on how it is spent, one might expect studies based on different geographic locations and based on different kinds of spending to be less consistent with each other even if spending matters on average. To gain a handle on the heterogeneity by spending type and location, it is helpful to consider the state-level or city-level studies by the type of spending examined.

IV.1 Unrestricted Spending

While many single-state studies examine specific forms of spending, not all of them do. Three studies exploit different sources of variation to examine the impact of increased school spending in Michigan. Papke (2008) uses a difference-in-difference (before vs after) analysis of outcomes in Michigan after the passage of a 1994 school finance reform. She finds that increases in spending have nontrivial, statistically significant effects on math test pass rates. Interestingly, the effects are larger for districts with initially poor performance. Roy (2011) examines this same reform somewhat more extensively and comes to largely the same conclusions. Hyman (2017) uses a different approach and exploits the fact that the school funding formulas set into motion a predictable time path of additional allowances that was equalizing over time – thus providing plausibly exogenous variation in spending. The basic idea is that, for reasons outside the control of districts and parents, initially low-spending districts received large initial allowances from the state that fell over time as school spending levels became more equal. Using the time path of the size of the State allowance to individual districts, he finds that students exposed to 10 percent more spending were 3 percent-

age points (7 percent) more likely to enroll in college and 2.3 percentage points (11 percent) more likely to earn a post-secondary degree. In contrast to some other studies finding that school spending has larger impacts for disadvantaged children, these effects were concentrated among districts that were lower poverty, and higher achieving at baseline.

Using data from New York, Gigliotti and Sorensen (2018) leverage variation in per-pupil expenditures from a specific provision of the state aid formula in New York State that allows districts to maintain prior levels of total state aid even as their student enrollment declines. Because of this provision, districts with declining enrollments tended to have systematically higher per-pupil expenditures over time. To isolate the influence of declining enrollment itself, they control directly for demographic changes associated with these enrollment losses. Using this variation, they find that $1000 in additional per-pupil spending leads to achievement gains of approximately 0.047 standard deviations in math and 0.042 standard deviations in English. Relying on discontinuities inherent in the funding formulas in Massachusetts, Guryan (2001) finds that increased school spending improves test scores. Similarly, using a regression-discontinuity design Lee and Polachek (2018) find that increased school spending led to increased high-school graduation rates. The only single-state study of unrestricted funds not to find positive and statistically significant effects is Clark. she examines the Kentucky Education Reform Act and finds that the increased spending induced by KERA did not improve test scores. Finally, Kogan et al. (2017) use a regression-discontinuity design to examine the impact of passing a referendum to increase school spending in Ohio. They found that referendum failure (as opposed to passage) led to lower instructional spending and lower student achievement growth.

Overall, of the nine single-state studies that examine the impacts of unrestricted spending, eight (88 percent) find a positive and statistically significant relationship between school spending and student achievement. This suggests that budget increases (that are unrestricted in the use of funds) will tend to improve student outcomes in most contexts.

IV.2 Textbook Spending

Some of the credible studies have been based on quirks in the school finance formulas that generate discontinuities in policies that can be exploited. One of the clearest examples of these is Holden (2016). That paper exploits a lawsuit in California that provided a one-time payment of $96.90 per student for textbooks if schools fell below a predetermined threshold of academic performance. This threshold allowed for variation in textbook spending that was unrelated to family background and not under the control of policy makers. Specifically, among schools with very similar achievement levels around the thresholds, whether a school had performance just above the threshold or just below is essentially random. Accordingly, any systematic difference in outcomes among those just above and below the cutoff can reasonably be attributed to the effect of the addi-

tional textbook spending. Exploiting this variation, she finds that textbook funding has significant positive effects on school-level achievement in elementary schools. In her preferred estimates, a one-time increase in funding of $96.90 per student improves school-average test scores by about 0.07 student-level standard deviations. This estimated impact is larger than most in the literature. However, this could potentially be rationalized by a model in which spending on textbooks was inefficiently low. One implication of this is that the result of this study are unlikely to generalize to all other forms of spending. However, this study is a valuable contribution, and a useful data point that helps us understand the relationship between school spending and student achievement.

IV.3 Capital and Construction Spending

Looking at another particular type of spending, Martorell et al. (2016) use a regression discontinuity design to examine the impacts of having additional capital spending in Texas. They study the achievement effects of nearly 1,400 capital campaigns initiated and financed by local school districts, comparing districts where school capital bonds were either narrowly approved or defeated by district voters. Overall, they find little evidence that school capital campaigns improve student achievement. Similar null impacts are found for capital spending (using a very similar design) in California (Cellini et al., 2010), and Ohio (Goncalves, 2015). However, Conlin and Thompson (2017) find positive impacts of capital spending toward new school construction in Ohio, and Hong and Zimmer (2016) find positive impacts of capital spending (again using an RD design) in Michigan. Looking at an even smaller unit of analysis, Neilson and Zimmerman (2014) use and event study framework to study a new school construction policy in New Haven CT and find positive impacts on student achievement. Using an event-study design, Lafortune and Schonholzer (2018) find similar benefits of new school construction in Los Angeles, CA.

As a whole, studies on the impact of capital spending in different states is mixed. Of the 7 studies identified, four are positive and three are null impacts. The fact that none were negative suggests that the average impact of capital spending is positive but that there may be considerable heterogeneity in that impact (and it may be zero in many cases).[2]

IV.4 Title I Spending

Many of the school spending papers that have relied on within-state variation examine Title I spending. Coincidentally, these studies all use data from New York City, all use the same regression discontinuity design, and all come to the same conclusion. Title I provides financial assistance to schools and districts with high numbers or high percentages of children from low-income families. Often, there is some cutoff percentage of low-income children above which additional funds are provided and below which they are not. Researchers have used this discontinuous jump in school

[2]It is worth noting that even in those studies that find no effect in the short run, some of them point to potential long run benefits (i.e. Martorell et al. (2016), and (Cellini et al., 2010)).

funding through the cutoff to identify the causal impact of additional school funding on student outcomes. Importantly, Title I funds are not unrestricted because schools "*must focus Title I services on children who are failing, or most at risk of failing, to meet state academic standards.*" Also, by definition, schools receiving Title I funds are among the lowest income schools in the state.

In an influential paper, van der Klaauw (2008) examines the impact of Title I funding on school finances and student performance in New York City public schools. Using a regression-discontinuity approach he finds that Title I eligibility did not improve student outcomes in high-poverty schools. However, he also finds that gaining Title I eligibility does not lead to a statistically significant increase in average per-pupil expenditures. As such, while van der Klaauw (2008) is a good test of the Title I program, it is not a good test of whether money matters. Weinstein (2009) also examine the Title I program in New York City and find that while Title I changes the mix of spending for high schools, it has very little impact on spending in elementary and middle schools. Not surprisingly, they find that Title I spending does not improve the achievement of students and may even reduce school-wide average test scores in elementary and middle schools. Matsudaira et al. (2012) find the same result using a similar approach in NYC. They find that Title I eligibility raises total revenues represents by between 3 and 4% relative to not being eligible. However, they find no statistically significant increase in average test scores.

While the results from NYC might lead one to conclude that Title I spending does not improve child outcomes, note that not all Title I papers find null impacts. Cascio et al. (2013) examine the roll-out of Title I using an event-study framework in multiple southern states. They document considerable supplanting of Title I funds such that receiving Title I money does not necessarily lead to increases in overall spending. However, they do find that white cohorts exposed to higher levels of Title I spending while in school had decreased dropout rates. Similarly, using nationally representative data, Johnson (2015) finds that county level Title I spending is associated with increased educational attainment. One potential explanation for this difference is that Title I was unsuccessful in NYC, but generally improved outcomes elsewhere. Also, it is possible that Title I improved longer-run outcomes that were not well detected by test scores (see Jackson (2018), and Beuermann and Jackson (2018) for discussions of this). Yet another plausible explanation is that Title I funds are often offset by reductions in taxes such that overall spending levels are often largely unchanged Gordon (2004). In such cases, a lack of a Title I effect does not speak to the broader impacts of school spending *per se*. Whatever the reasons, it is clear that increased Title I spending may not always improve student outcomes in all settings.

IV.5 Overview of Single-State Studies

While the national studies almost uniformly point to a positive causal relationship between school spending and student achievement on average, it is helpful to examine all the single-state

studies to gain a sense of whether this is true in all states or cities. There were 20 studies that examine relationships within an individual state (or city within a state). Of these, 13 (65%) found positive and significant relationships between spending and outcomes and none found negative and significant impacts. If in fact there were no relationship, assuming that each study was independent, one would expect one study to be positive and significant. Given that there are more than twelve times that, this pattern is consistent with positive overall impacts, in an average sense. However, the fact that in about one-third of the cases there is no significant relationship suggests that that a strong positive relationship may not exist in *all* settings or for all spending types.

To better understand why some studies find positive impacts while others do not, an examination of the few studies that are not positive is instructive. Three out of the seven papers that are not significant involve Title I spending, and three out of the seven involve capital spending. Given that 6 out of 7 of the studies that find no significant impact (86%) involve particular spending types may suggest that while overall budget increases may improve outcomes, increased funding tied to particular uses may not. In particular, the evidence is consistent with capital spending and Title I spending being less predictably effective than spending in general. Consistent with this interpretation, almost all of the national studies examine general increases in spending.

V Conclusions

Social scientists have long sought to examine the causal impact of school spending on child outcomes. The literature on this topic can be put into two clear categories: an older literature that relies on observation variation to correlate school spending and student outcomes, and a newer literature that relies on quasi-experimental methods to uncover relationships that are plausibly causal.

The older literature provides strong support for there being a positive economically important association between increased school spending and improved student outcomes. That is, despite claims to the contrary, the application of reasonable statistical reasoning to the patterns across studies would lead one to conclude that there is a strong statistical link between spending and outcomes. However, because this older literature is entirely observational in nature, these studies do not speak to the causal question of whether increased school spending improves student outcomes. To do this, one must examine the more recent quasi-experimental literature.

The recent quasi-experimental literature that relates school spending to student outcomes overwhelmingly support a causal relationship between increased school spending and student outcomes. All but one of the several multi-state studies find a strong link between spending and outcomes – indicating that money matters on average. Importantly, this is true across studies that use different data-sets, examine different time periods, rely on different sources of variation, and employ different statistical techniques. While one can poke holes in each individual study, the robustness of the patterns across a variety of settings is compelling evidence of a real positive causal relationship

between increased school spending and student outcomes on average. However, an examination of single-state studies suggests that, *on average*, money matters, but that this is not always so in all settings or in all contexts. In particular, studies of Title I spending and capital spending in individual states uncover null impacts in some cases.

Exactly what contexts increased school spending are most likely to improve student outcomes remains an open question. However, recent evidence suggests that school spending is more effective in areas with well-established Head Start programs (Johnson and Jackson, 2017). Also, Brunner et al. (2018) provide some evidence that the marginal impact of school spending may vary by union strength. More generally, one might expect school spending increases to improve outcomes in settings with stronger incentives to promote student outcomes. However, this is an active area of research. By and large, the question of whether money matters is essentially settled. Researchers should now focus on understanding what kinds of spending increases matter the most, and also in what contexts school spending increases are most likely to improve student outcomes.

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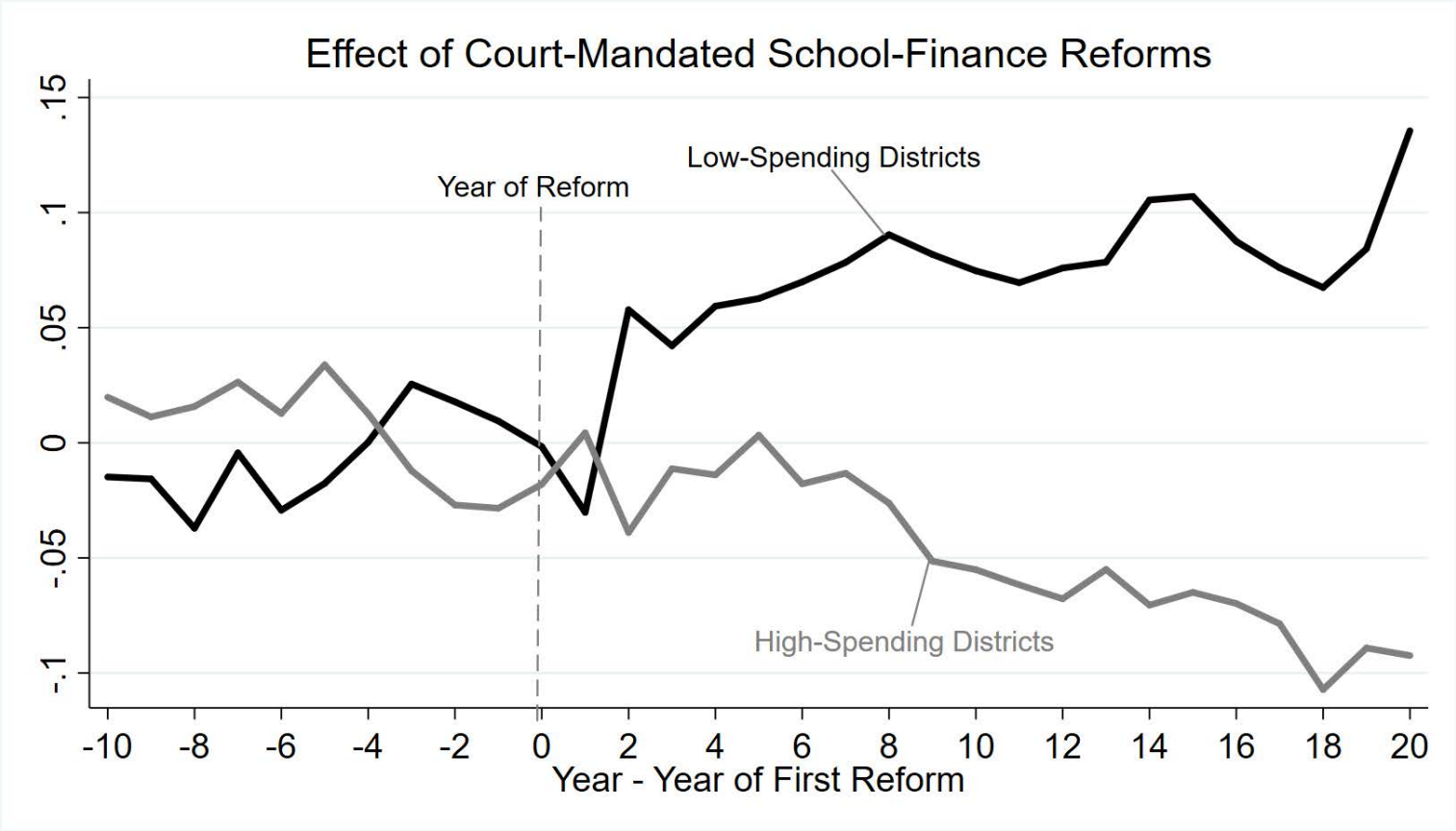
Table 1. List of Studies

Study	Pos and Sig	Neg and Sig	Not Sig	Outcomes	Variation	State	Type of Spending
Multi-State Studies							
Jackson Johnson and Persico (2015)	Y			Education, Wages	CO-SFR	ALL	Any
Johnson and Jackson (2018)	Y			Education, Wages, other	CO-SFR	ALL	Any
Lafortune, Rothstien, Shanzenbach (2018)	Y			Test Scores	SFR	ALL	Any
Candelaria and Shores (2018)	Y			Graduation Rates	CO-SFR	ALL	Any
Brunner, Hyman, and Ju (2018)	Y			Test Scores	SFR	ALL	Any
Biasi (2018)	Y			Income mobility	SFR	ALL	Any
Card and Payne (2002)	Y			SAT score inequality	CO-SFR	ALL	Any
Hoxby (2001)			Y	Dropout rates	SFR	ALL	Any
Downes and Figlio (1997)	Y			Test Scores	DiD-Tax Limit	ALL	Any
Johnson (2015)	Y			Graduation Rates	DiD	ALL	Title I
Jackson, Wigger and Xiong (2018)	Y			Test Scores	IV-Recession	ALL	Any
Miller (2018)	Y			Test Scores	IV-House Values	ALL	Any
Cascio, Gordon, and Reber (2013)	Y			Dropout	Event Study	SOUTH	Title I
Single-State Studies							
Hyman (2017)	Y			College-going	Rules-Based IV	MI	Any
Gigliotti (2018)	Y			Test Scores	Rules-Based IV	NY	Any
Papke (2008)	Y			Test Scores (pass rates)	IV-SFR	MI	Any
Roy (2011)	Y			Test Scores	Policy (SFR)	MI	Any
Guryan (2001)	Y			Test Scores	Rules-Based IV	MA	Any
Clark (2003)			Y	Test Scores	Rules-Based IV	KY	Any
Lee and Polachek (2018)	Y			Graduation Rates	RD-Referenda	NY	Any
Holden (2016)	Y			Test scores	RD	CA	Textbooks
Husted and Kenny (2000)	Y			SAT Scores	DiD	CA	Any
Cellini, Ferreira, and Rothstein (2010)			Y	Test Scores	RD-Bonds	CA	Capital
Lafortune and Schonholzer (2018)	Y			Test Scores	Event-Study	CA	Capital
Martorell, Stang, McFarlin (2016)			Y	Test Scores	RD-Bonds	TX	Capital
Conlin and Thompson (2017)	Y			Test Scores	IV	OH	Construction
Goncalves (2015)			Y	Test Scores	Event-Study	OH	Construction
Hong and Zimmer (2016)	Y			Test Scores	RD-Bonds	MI	Capital
Kogan, Lavertu, and Peskowitz (2017)	Y			Test Scores	RD-Referenda	OH	Any
Zimmerman and Neilson (2014)	Y			Test Scores	Event-study	New Haven, CT	Construction
Van Der Klaue (2008)			Y	Test Scores	RD Title I	NY City	Title-I
Matsudaira, Hosek, Walsh (2012)			Y	Test Scores	RD-Title I	NYC*	Title-I
Weinstein, M. G., Stiefel, et al (2009)			Y	Test Scores	RD-Title I	NYC*	Title-I

Notes: DiD = Difference in Difference, IV = Instrumental Variables, RD = Regression Discontinuity, SFR = School Finance Reform

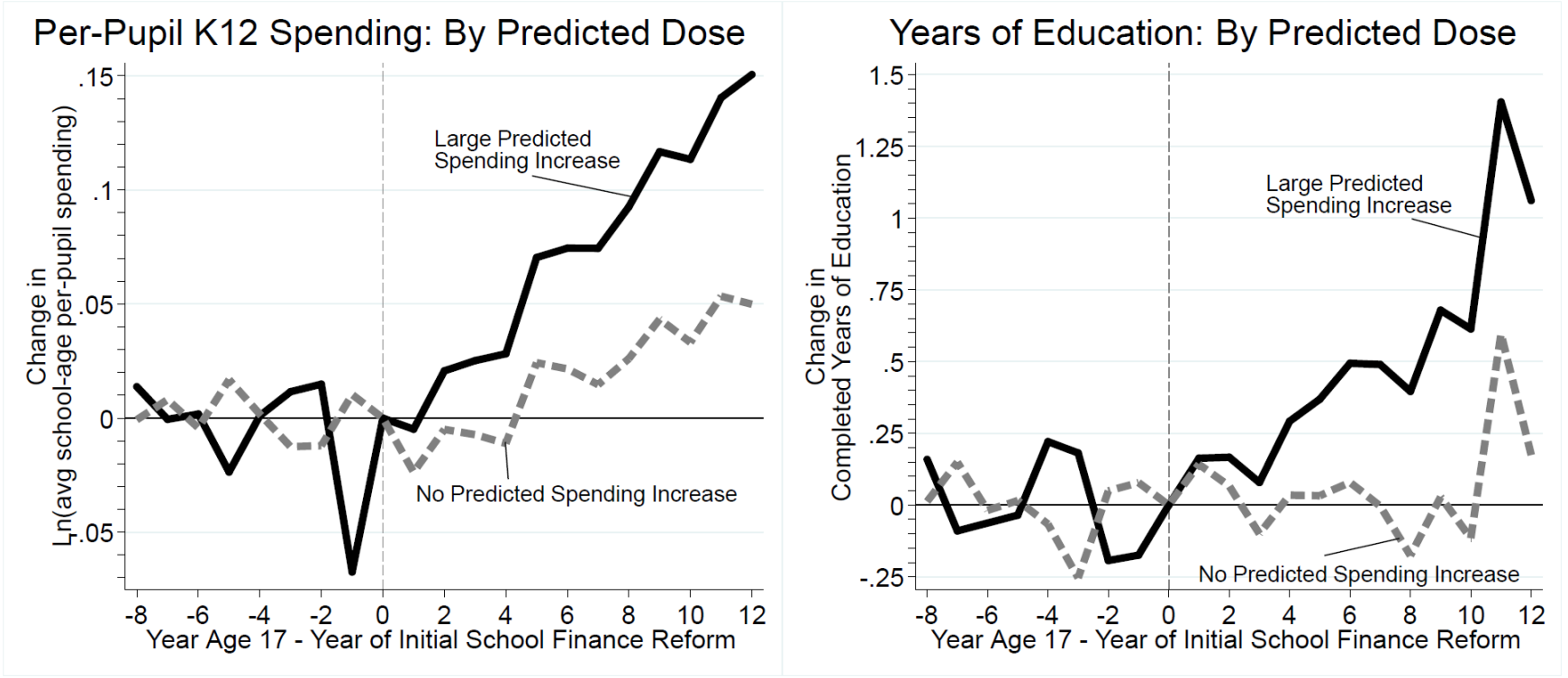
*These studies are not reported as being based in New York City. However, this location can be inferred.

Figure 1. School Finance Reform Event Study by Pre-Reform Spending



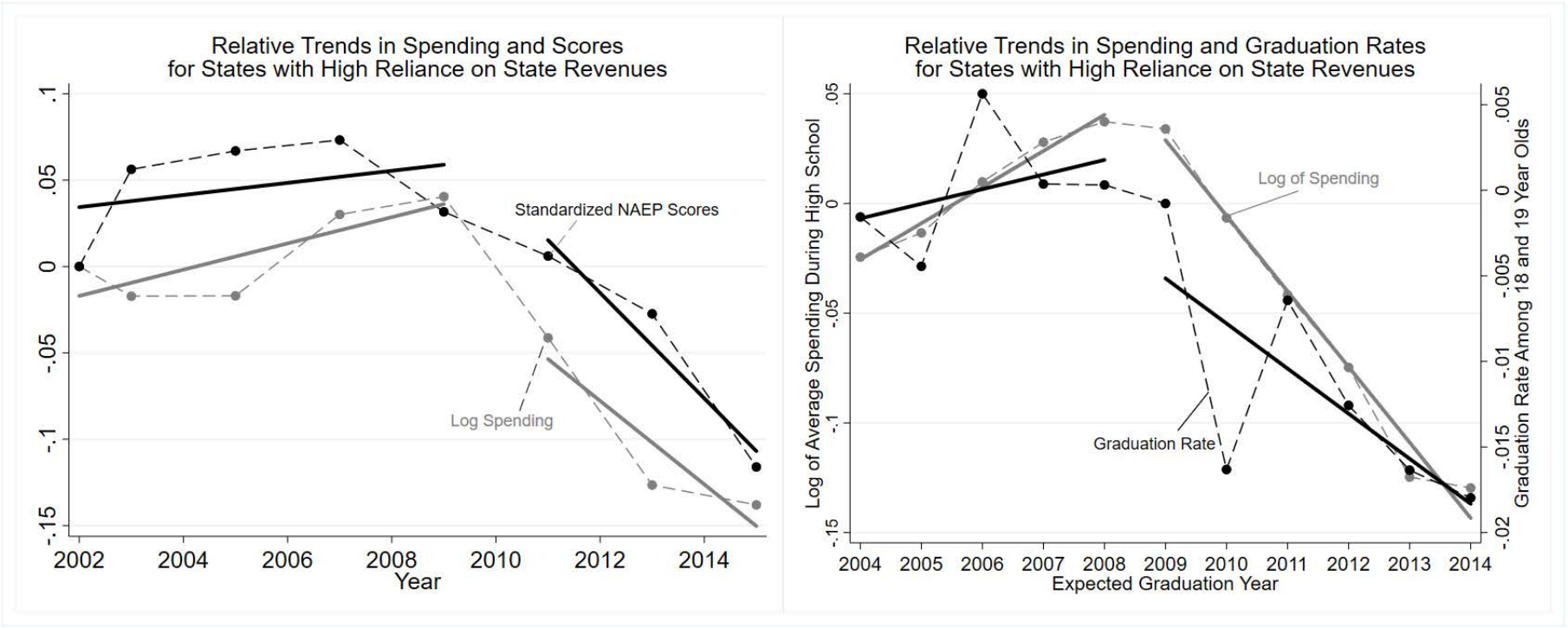
Notes: This figure is taken from Jackson et al. (2015) with permission from the authors. See the source paper for further details.

Figure 2. School Finance Reform Event Study by Predicted Dose



Notes: This figure is taken from Jackson et al. (2016) with permission from the authors. See the source paper for further details.

Figure 3. Event Study by Reliance on State Revenues



Notes: This figure is taken from Jackson et al. (2018) with permission from the authors. See the source paper for further details.