Intergovernmental Grants to School Districts and Educational Outcomes During the COVID-19 Pandemic*

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Abstract

During the COVID-19 pandemic the US federal government appropriated $189.5 billion in emergency aid to school districts. These funds were supplied in part to help districts replace lost revenues and to mitigate the harmful effects of the pandemic on learning and other measures of district performance. We use a difference-in-discontinuities design to evaluate the causal impact of additional federal funds on several measures of school district performance through 2023. We find that additional funds resulted in higher enrollment retention and faster reopening, but likely did not mitigate declines in test scores. Additionally, we find that federal funds were passed on to residents through reductions in local revenue collections, including the property tax, such that incremental federal funds did not result in increased per-pupil expenditures by school districts.

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1. Introduction

The COVID-19 pandemic created significant challenges for the public sector in the United States in general, and for public schools in particular. School districts were faced with the challenge of weighing the trade-offs between the potential for disease transmission and the educational achievement of students during the pandemic (Gordon and Reber, 2020; Halloran et al., 2021, 2023; Oster et al., 2021) associated with different learning modalities (in-person, virtual, or hybrid). To help address these and other challenges faced by state and local governments, Congress appropriated almost $1 trillion in emergency funding in the form of intergovernmental transfers to sub-national governments, with about $189.5 billion allocated to funding elementary and secondary education (Clemens and Veuger, 2021; Office of Elementary and Secondary Education, 2023). The largest program for aiding public schools was the Elementary and Secondary School Emergency Relief Fund (ESSER), which was appropriated funds in each of the Coronavirus Aid, Relief, and Economic Security (CARES), Coronavirus Response and Relief Supplemental Appropriations (CRRSA), and American Rescue Plan (ARP) Acts. This paper will provide an empirical investigation into how effective these funds were in helping schools weather the pandemic, using a variety of traditional measures of school performance as well as pandemic-specific measures.

To evaluate the impacts of the ESSER program on pandemic school district outcomes, we use a difference-in-discontinuities design that exploits a threshold in the funding formulas for delivering funds to school districts. The ESSER funds were allocated to states using the pre-existing Title 1A funding formulas. State education agencies (SEAs) then distributed the funds to school districts (LEAs) using the same formulas (Office of Elementary and Secondary

Education, 2022, 2023). We use a statutory jump in the Title 1A formula at the 5 percent formula child percentage cutoff to identify a roughly 50% increase in ESSER funds for school districts just over the cutoff. We then use this discontinuity to evaluate whether these funds can explain differences in traditional performance measures such as test scores, enrollments, and absenteeism, pandemic outcomes like reopening speed and area cases, staffing categories, and district budgets.

We find that additional ESSER funds do not increase test scores for either math or reading in districts just above the 5% poverty qualification cutoff. If anything, our point estimates are negative. However, ESSER funds may have helped districts retain students and decrease absenteeism. We also find that ESSER funds may have helped schools to open faster, as suggested by cellphone mobility data, and that this reopening was not associated with additional spread of COVID-19, which was the primary motivation for relatively slow rates of reopening.[1] These funds may have led to small increases in the number of teachers later in our sample period and to increases in the number of administrators. Finally, we find evidence that districts did not substantially change their expenditures, but they did reduce local revenue sources in response to receiving additional ESSER funds. These results contradict the "flypaper effect," which suggests that windfall revenues for subnational governments are not offset by reductions in own-source revenue (Gramlich, 1977; Hines and Thaler, 1995; Fisher, 1982). They are complementary, however, to findings from Gordon (2004), however, who finds that declines in local revenues

[1] Data from UNESCO (2023) suggests that the US kept schools closed longer than in the EU. In October of 2020, the US is marked as having "partially open" schools whereas EU member countries' schools were "open" with the exception of Czechia, which was "closed." By the end of the tracker in February of 2022, the EU would be completely open, whereas the US would be marked "partially open," the same status in which it was categorized in April of 2020.

offset increases in federal grants over a horizon of two years following census-driven updates to school districts' funding allocations.

As mentioned above, the ESSER program funds were appropriated using the Title 1A funding formulas and pre-COVD school district data (Office of Elementary and Secondary Education, 2022, 2023). This formula has a discontinuity, where districts with more than 5% of their population ages 5 to 17 in poverty receive significantly more Title 1A funds, and in turn more ESSER funds (Congressional Research Service, 2017). We find that crossing the 5 percent threshold and qualifying for additional funds corresponds with a 50% increase in ESSER funds. This cutoff is determined using pre-COVID school district factors, mainly the 2018 vintage of the US Census Bureau's Small Area Income and Poverty Estimates (SAIPE), which makes the additional funds distributed to districts that are just on either side of the 5% poverty threshold plausibly exogenous to pandemic related outcomes.

We mainly contribute to three literatures. First, our analysis of the ESSER program adds to a body of work evaluating the broad set of COVID spending programs. These programs include the Economic Impact Payments to households (Chetty et al., 2020), the Paycheck Protection Program to support employment linkages (Autor et al., 2022a, 2022b; Hubbard and Strain, 2020), the Municipal Lending Facility (Haughwout et al., 2021), and fiscal transfers for state and local governments to support the provision of public services, economic activity, and pandemic mitigation efforts (Clemens, Hoxie, and Veuger, 2022; Clemens, Hoxie, Kearns, and Veuger, 2023; Clemens and Mahajan, 2025; Green and Loualiche, 2021). We add to this literature by analyzing the effects of the ESSER program on school enrollment, re-opening, student outcomes, and district budgets.

The second literature that we contribute to is the growing body of work on the specific issues surrounding schools during the COVID-19 pandemic. Early in the pandemic, Gordon and Reber (2020) outlined how Congress could design an aid package to schools using the experience of the Great Recession as a guide. As many schools switched to virtual learning, and subsequently returned to in-person instruction, Oster et al. (2021) showed that Black and Hispanic students experienced a slower return to in-person learning than White students. Halloran et al. (2021) and Halloran et al. (2023) evaluate how learning mode and other pandemic policies impacted test scores. Fahle et al. (2023) demonstrates that poorer districts with more non-White students saw larger declines in test scores, but that differences between groups of students within districts were small. Carbonari et al. (2024) investigate the impact of tutoring and other learning interventions on students scores in 4 large districts and find little evidence of impacts. Our analysis contributes to this literature by providing evidence that incremental federal funds may have supported faster rates of reopening and improved rates of student retention, though not in detectable improvements in test scores.

We also contribute to the large literature on the flypaper effect. Early work on the flypaper effect focused on the empirical finding that a fiscal transfer from an intergovernmental grant increased sub-national government spending more than an equivalent increase in personal income (Gramlich, 1977; Hines and Thaler, 1995; Fisher, 1982). However, later research suggests that the flypaper effect may dissipate as governments adjust to new levels of transfers (Gordon, 2004). There is even evidence that there may be no flypaper effect at all in some cases. Lutz (2010) documents roughly a dollar for dollar decrease in local taxes in response to grants in New Hampshire. More recently, Shoag, Tuttle, and Veuger (2019) suggest that the flypaper effect may be muted by a local government's ability to access and adjust other revenue streams.

We contribute to this literature by providing evidence that incremental federal funds were largely distributed to local residents through reductions in local revenue collections, in particular through reductions in local property tax collections.

This paper proceeds as follows. In Section 2 we discuss how the federal government typically supports local districts and how ESSER funds were distributed. In Section 3 we turn to the construction of our dataset and in Section 4 we describe the difference-in-discontinuities design we use to identify the causal impact of ESSER funds on district outcomes. We present our results in Section 5 and conclude in Section 6.

2. Federal Funding for Public Schools

2.1 Funding Before the Pandemic

When compared to state and local sources, the federal government is typically a relatively minor contributor to public K-12 school district budgets, as shown in panel A of Figure 1 and Table 1. During the pandemic, the Title 1-A funding formula was used to deliver emergency aid to school districts, leading to an increase in the share of funding to schools from federal sources. The Title 1 program, authorized by the Elementary and Secondary Education Act, is the largest federal program targeting local school districts, which funnels most of its appropriations through the formulas outlined in Part A (Congressional Research Service, 2017; Skinner, 2016). The Title 1-A formulas consist of four sub-formulas that try to disperse funds across a large number of school districts, while sending more funds to public school districts that serve higher-need populations, albeit imperfectly (Congressional Research Service, 2017; Skinner, 2016; Gordon

and Reber, 2020; Gordon and Reber, 2023). These sub-formulas each allocate money to school districts individually, and the sum of the funds constitutes a school district's total amount of Title 1-A funding.[2]

The four sub-formulas determine the Basic Grant, which allocates funds to all but about 70 school districts nationally, the Targeted Grant, the Education Finance Incentive Grant (EFIG), and the Concentration Grant, which is meant to allocate funds to the largest and poorest districts. Each formula takes a slightly modified version of the following form:

$$Funds_{it}^{g} = Q^{g}(FC_{it}, PovShare_{it}) \times$$
$$\max\{W^{g}(FC_{it}, PovShare_{it}, StateFactors_{s}), H^{g}(Funds_{it-1}^{g})\} \quad (1)$$

where *g* denotes one of the four formulas, *i* indexes school districts, *s* indexes states, and *t* indexes the fiscal year. Each formula has three main components, a qualification function $Q^{g}(\cdot)$, a weighting function $W^{g}(\cdot)$, and a hold harmless function $H^{g}(\cdot)$. In this paper we will focus on exploiting the qualification dummy for two grant formulas, the Targeted and EFIG formulas, which create small variations in Title 1-A funds that will map directly into large differences in ESSER funds in the COVID-19 relief packages.

The grant functions take four major inputs. The first two inputs are the formula child count, FC_{it}, and the poverty share, $PovShare_{it}$, for a given district *i* in fiscal year *t*. The formula child count is the number of children aged 5-17 living within a geographic school district that are assessed to be living in impoverished families, that are in foster care, or that benefit from TANF

[2] For a more complete overview of the funding formulas, see Skinner (2016), Congressional Research Service (2017), and Gordon and Reber (2023).

(Skinner, 2016). The poverty share, $PovShare_{it}$, is the formula child count for a given school district divided by the total number of children aged 5-17 living within that school district's boundaries.[3] These inputs are estimated by the Small Area Income and Poverty Estimates (SAIPE) for school districts from the United States Census Bureau (2023) using American Community Survey data supplemented by tax data. The third input, $StateFactors_s$, varies by grant, but they take into account a state's own expenditures on education three years prior, equity in funding across school districts, and grant specific state-level funding minima, and other factors (Gordon and Reber, 2023). Finally, each formula has its own hold-harmless provision, which takes into consideration the amount of funds appropriated last year, $Funds_{it-1}^{g}$, when allocating funds for the upcoming year. In most cases, hold-harmless provisions limit year-to year-funding reductions to 15% of last year's total, ensuring each district receives at least 85% of the nominal funds it received the previous year (Skinner, 2016).

For the Basic Grant, which made up 41.5% ($6.5B) of all Title 1-A funds in FY 2017, the qualification function requires a district to have more than 10 formula children and a 2% district poverty share (Congressional Research Service, 2017). If a district qualifies for the basic grant, the number of formula children is then scaled by state factors, with each formula child receiving equal weight. However, if a district loses money from an initial Title 1-A allocation, possibly due to a decline in its number of formula children, an increase in non-formula children living within its boundaries, or a decline in state average per pupil expenditures on education, it is guaranteed a fraction of its previous fiscal year's allocation through the hold harmless function. The district is entitled to no less than 85% of its previous fiscal year's basic grant allocation, so long as it still qualifies for the Basic Grant.

[3] Note that these measures are estimates geographically and do not align with actual enrollment in the public school district for which these measures are attached.

The Concentration Grant, which distributed $1.4B (8.5% of all Title 1-A funds) in FY 2017, works very similarly to the Basic Grant, with two main differences besides the funding levels (Congressional Research Service, 2017). First, the Concentration Grant requires districts to have either 6,500 formula children or a 15% poverty share to qualify for a grant. Second, a district that qualified for a Concentration Grant in a previous year is entitled to hold-harmless funds even if it no longer qualifies for a Concentration Grant (crossing the formula child or poverty share threshold) for up to 4 years.

Finally, half of all Title 1-A funds ($7.6B in FY 2017) are appropriated through the EFIG and Targeted Grant formulas (Congressional Research Service, 2017). To qualify for these grant formulas, a district must have at least 10 formula children and a poverty share above 5%. In these formulas, the weighting function for formula children is quite complex and rewards districts with either high counts of formula children or high district poverty shares (Gordon and Reber, 2023). For the EFIG formula, the state factors take into consideration equity in funding across districts as well as state spending on education compared to state incomes (Skinner, 2016). Crossing the 5% poverty share and qualifying for the EFIG and Targeted Grant formulas is the discontinuity that we use to identify the impacts of ESSER funds in our difference in discontinuities research design, as discussed below.

To distribute funds to school districts, the Department of Education uses the funding levels for each formula as determined by Congress and calculates initial grants to school districts using the most recent SAIPE estimates for geographic districts. These allocations are then sent to the state departments of education (SEA), which are allowed to reserve a small fraction of their state's Title 1-A funds for approved purposes, and then the state agencies allocate the remaining funds to their school districts (LEAs) following the grant formulas, making slight modifications

to the allocations to accommodate charter schools (Skinner, 2016).[4] The 5% poverty share threshold to qualify for EFIG and Targeted funds, as measured in 2018, is the threshold we use to identify changes in ESSER funds in this paper, as discussed below.

2.2 Funding During the Pandemic

In a typical recession, the budgets of state and local governments come under stress due to their limited ability to borrow and common balanced budget requirements. This makes federal aid to sub-national governments an important policy tool during economic downturns (Clemens and Veuger, 2021). In previous recessions, federal aid to K-12 public schools has been included in spending packages, such as the American Recovery and Reinvestment Act during the Great Recession, as state and local revenue sources tend to contract during recessions (Congressional Research Service, 2009). However, the three spending bills that appropriated funds to the pandemic-era Elementary and Secondary School Emergency Relief Fund (ESSER) are unique in both the amount of funding that they dedicated to school districts as well as in their use of the existing Title 1-A formulas to determine funding levels (Gordon and Reber 2020).[5]

The ESSER Program was appropriated a total of just over $190B in funds by the CARES Act, the Coronavirus Response and Relief Supplementary Appropriations Act (CRRSAA), and the American Rescue Plan (ARP) between March 2020 and March 2021. These funds were first given to state education agencies (SEAs) in proportion to their Title 1-A allocations. Then, the

[4] Explicitly calculating the state-level adjustments to accommodate public charter schools is not feasible with publicly available national datasets. To do this final step of calculations would require not only knowing individual charter school enrollments, but also the regular public-school districts in which those students live.
[5] As part of the American Recovery and Reinvestment Act, Congress delivered most of the $97 billion (nominal) aid to education (including higher education) through a simple formula based on state population of persons aged 5 to 24 and total population. Many of the remaining appropriations were delivered through existing programs, but only $10 billion (nominal) followed the Title 1-A formula (Congressional Research Service, 2009).

states needed to deliver no less than 90% of those funds to local education agencies (LEAs, or school districts) in proportion to their share of state Title 1-A funds from FY 2020 (Skinner et al., 2022). We present an abbreviated timeline of events relevant for the ESSER program and school districts during COVID-19 in Table 2.

The ESSER Program had a myriad of goals, which were dictated in large part by the unique focus that the pandemic placed on school districts. One of the primary features of the pandemic was school closures during spring semester of 2020 and into the 2021 school year. These closures likely had an important role in shaping the associated economic recession, and they placed adopting new learning modes that could be done from home, adapting facilities to accommodate a return to in-person learning, and other issues of safety for both teachers and students at the center of policy debates (Skinner et al. 2022; Alon et al., 2020; Alon et al., 2022; Halloran et al., 2021; Halloran et al., 2023; Oster et al., 2021). In addition to these goals directly related to mitigating the disruption of the virus itself, the ESSER program had a number of additional stated objectives around making up for lost class time during the closures, bolstering student, teacher, and staff mental health resources, and mitigating layoffs, among other objectives (Skinner et al, 2022; Bryant et al., 2023). School districts were left relatively unconstrained in how they used their funding, so long as they spent their funds by each program's deadline. The $13B in ESSER I funding from the CARES Act had to be spent by September 2021, the $54B in ESSER II funding from the CRRSAA had to be spent by September 2022, and the remaining $123B in ARP ESSER funding initially needed to be spent by September 2023, but was extended for one additional year to 2024 (Skinner et al., 2022). A survey of 487 school districts in 2023 by McKinsey and Company suggested that as much as $90B in ESSER funds was left unspent as of June 2023 (Bryant et al., 2023).

3. Data

To assess the impacts of COVID funds to school districts on their performance during and after the pandemic, we build an unbalanced panel dataset from the more than 17,400 public school districts in the Common Core of Data (CCD) district fiscal and non-fiscal files from 2010 to 2022 (NCES, 2023). We also use the school enrollment and free- and reduced-price lunch files to build district measures of active schools and measures of the distribution of students on free and reduced-price lunch, and all students, across schools. We attach these data to the annual SAIPE estimates from the US Census Bureau (2023) in order to associate the formula child count and poverty share estimates to each school district, which are key inputs into the Title 1A funding formulas. This limits our dataset to the roughly 12,700 school district that have estimates from the SAIPE. The vast majority of these districts are categorized as traditional geographic school districts. Many charter schools do not receive poverty estimates from the SAIPE as they do not have traditional geographic boundaries in many cases, which means we cannot include them in our analysis even though they received federal funds during COVID. To be included in the unbalanced panel, a district needed to have complete data for total federal revenues, enrollments, and SAIPE poverty estimates between 2017 and 2022 as well as have a non-zero number of enrolled student.[6]

We supplement the fiscal, employment, enrollment, and district poverty data from the CCD and SAIPE with data on ESSER allocations to school districts from the Department of Education's Education Stabilization Fund API (US Department of Education, 2024). We

[6] In SY 2019-2020, all districts in California reported 0 teachers and staff employed. We have replaced these missing observations with their values from the previous school year.

combine several iterations of the API to build a dataset of ESSER allocations that includes $170B in funds.[7] These ESSER amounts are reported to the Department of Education by the SEAs on behalf of their districts as part of federal grant reporting guidelines.

In addition to enrollments, staffing, expenditure, revenue, and poverty estimates, we are also interested in how school districts performed in terms of student achievement during the pandemic. We use harmonized test score data from the Stanford Education Data Archive (SEDA), which uses the National Assessment of Educational Progress (NAEP) to make spring state-level testing of 3rd to 8th graders comparable across states and years. These data index state test scores to the 2019 NAEP distribution, putting each district's score in terms of 2019 standard deviations and demeaning each score by the 2019 average (Reardon, et al., 2024). We are able to match just over 7,000 districts with complete SEDA testing data for reading and math.

In addition to test scores and spending categories, we include several other variables to understand how districts performed during the pandemic. First, we include data on chronic absenteeism from Ed Data Express (2023), which the Biden Administration identified as a key driver of low-test performance during COVID (CEA, 2023). These data count students who are absent for at least 10% of school days. Second, we attach a cell phone based mobility measure from Parolin and Lee (2022), which measures the decline in cellphone visits around schools by district throughout 2020, 2021, and 2022 compared to their same month in 2019 as a baseline. We average these measures by year for each school district available.[8] Finally, we include a measure of community average daily case rates around school districts from the COVID School

[7] We combine multiple data vintages to build a DUNS ID to NCES ID crosswalk that allows us to capture districts that come in and out of the API vintages across pulls, using the most recent value available for that districts final allocation of ESSER Funds. We pulled the API on 12/1/2022, 10/19/2023, and 2/4/2024.

[8] We also corroborate these data with shares of the school year by learning mode (in-person, hybrid, or virtual) as reported during the 2020-2021 school year from the COVID School Data Hub (CSDH, 2023).

Data Hub (CSDH). To create this measure, the CSDH apportions average daily county-level COVID cases per 100k residents to zip codes by population and then attaches these measures to school districts (CSDH, 2023). We average each district's daily case rate across reporting weeks within SY 2021, the only year for which we have data. It is important to note that for these COVID outcomes we have imperfect coverage in our sample. 2,915 Districts have complete mobility data, while all districts in sample have CSDH case rate data. Throughout the paper we include districts as data are available for estimates with these data and with SEDA test scores.

Our main explanatory variable throughout this paper is ESSER funds per pupil, where the total amount of ESSER funds awarded across all three installments are summed and divided by a district's fall 2019 enrollment total (SY 2020).[9] Throughout this paper, outcomes measured on a per pupil basis use the SY 2020 enrollment estimates, taken in the fall of 2019. For event study style presentations, we will use total district revenues per pupil before the pandemic and then add ESSER funds to other revenues to create a composite funding per pupil measure in 2021. However, as mentioned above, ESSER funds per pupil are directly linked to district poverty rates, which are associated with many measures of poor educational outcomes (Hanusheck et al., 2019). The pandemic is not exceptional in this respect, as 2019 to 2022 test score declines in both reading and math are closely linked to district poverty shares, as shown in Figure 2. Therefore, we will use the formula child count and count of children aged 5-17 from the 2018 vintage SAIPE and a discontinuity in the Title-1A formula to study the impacts of ESSER funds on school district performance during the pandemic.

[9] We drop the top 0.01% of ESSER funds per pupil because the amounts are implausible on a per pupil basis. Additionally, some districts reported negative ESSER amounts, which we corrected by removing the errant negative sign.

The difference-in-discontinuities design provides a local average treatment effect for districts around the 5% poverty share threshold for the impact of ESSER funds on school performance and budget outcome variables. We will present estimates from 2017 to 2023, as data are available, but first a more detailed discussion of our estimation strategy is in order.

4. Methodology

The difference in discontinuity design exploits the qualification functions for the EFI and Targeted Grants. Both grant formulas require a district to have a 5% share of formula children out of all 5- to 17-year-olds in the 2018 SAIPE vintage to qualify for funds from those grants in FY 2020. The ESSER funds were allocated to school districts using the districts' respective share of state Title 1-A funds from FY 2020 in SY 2020 and SY 2021. A discontinuity in ESSER funds should form around the 5% poverty share cutoff, as districts on the lower end of the cutoff will only receive ESSER funds in proportion to their Basic and Concentration Grant allocations, whereas districts that cross the 5% threshold receive ESSER funds in proportion to their funds from potentially all four formulas.

To set up the difference-in-discontinuities around the 5% poverty share threshold, we first construct our running variable for each school district, distance from the cutoff in formula children. The running variable of centered formula children takes the following form:

$$R_i = FC_{2018i} - (ChildPop_{2018i} \times 0.05), \quad (2)$$

where i indexes school districts. FC_{2018i} is the formula child count (children aged 5-17 estimated to be in poverty in the 2018 SAIPE) within school district i, and $ChildPop_{2018i}$ is the total number of children aged 5-17 within district i in the 2018 SAIPE. We show the relationship between the running variable, centered formula children, and ESSER funds per pupil for districts within 100 formula children of the cutoff in Figure 3. In Panel A of Figure 3, we can see how pre-COVID federal funds display a discontinuity of about $135 per pupil, which motivates using the difference-in-discontinuity approach. In Panel B, we see the un-binned scatter which shows a clear increase in average ESSER funds per pupil right at the 5% poverty threshold, where the qualification cutoff is marked with a vertical line. We present the same relationship in a binned scatter plot of our running variable, centered formula child counts around the qualification cutoff, and ESSER funds per pupil in Panel C, weighted by district enrollment in the fall of 2019.[10] In this figure, we can clearly see a discontinuity of around $500 per pupil.[11]

As the 5% poverty threshold has existed in federal programs since 2001, we exploit the timing of the ESSER Funds to estimate a difference-in-discontinuities design similar to Grembi et al. (2016). We estimate regressions of the following form:

$$ESSER_{it} = \alpha + \sum_{y \neq Base}[\beta_y(D_i \cdot I\{Year = y\}_t) + \gamma_y(R_i \cdot I\{Year = y\}_t) + \theta_y(D_i \cdot R_i \cdot I\{Year = y\}_t)] + \delta_i + +\tau_t + \varepsilon_{it} \quad (3a)$$

$$Y_{it} = \alpha + \sum_{y \neq Base}[\beta_y(D_i \cdot I\{Year = y\}_t) + \gamma_y(R_i \cdot I\{Year = y\}_t) + \theta_y(D_i \cdot R_i \cdot I\{Year = y\}_t)] + \delta_i + +\tau_t + \varepsilon_{it}, \quad (3b)$$

[10] Following the visual inference recommendations from Korting et al. (2023), we present our discontinuity figures without trend lines.

[11] We exclude districts that record revenues per pupil in excess of $200,000 and enrollments above 17,864 in SY 2020.

where *i* indexes school districts and *t* indexes time periods.

Equation 3a is the first stage regression equation used to identify the amount of ESSER funds per pupil associated with the 5% poverty threshold in equation 3a. Our parameters of interest are the post-COVID values of β_y, which captures the impact of the poverty threshold on ESSER funds. We capture the impact of crossing the 5% poverty threshold and qualifying for additional funds with the dummy variable D_i, which takes a value of 1 if a district has crossed the poverty share threshold and 0 if it has not crossed the threshold. We will refer to D_i as the qualification dummy in our tables. The running variable, R_i, is the centered formula child count as described in Equation 2. We interact the running variable with a set of year dummies, $I\{Year = y\}_t$, which take a value of 1 in each included year and 0 otherwise, with the last available pre-COVID year of data left out as the base year. Given the availability and measurement periods of our datasets, the base year varies depending on the outcome variable.[12] These equations use a first-degree polynomial of the running variable, but we will present specifications using a second-degree polynomial as well. We include district and time fixed effects, shown as δ_i and τ_t, respectively. We weight all regressions by SY 2020 (fall 2019) enrollments and cluster standard errors by district. Finally, ε_i is an error term.

We are interested in how our outcomes of interest, Y_{it}, change at the discontinuity after COVID, which is captured by the post-COVID values of β_y in equation 3b. We can scale the parameters of interest from Equation 3b using the first-stage estimates in Equation 3a to understand the impact of ESSER funds on our outcomes. We weight these regressions by district enrollment in fall 2019 and use robust standard errors since treatment is assigned at the district

[12] For example, absenteeism is measured throughout the 2019-2020 school year, so any observation that overlaps with COVID is counted as in the "post" period. This explains why some outcomes have different numbers of pre-trend periods than other outcomes.

level (Abadie et al., 2023). Finally, we use bandwidths ranging from 45 centered formula children to 55 centered formula children from the poverty threshold. The sample of districts within 50 formula children of the threshold,[13] which is our preferred bandwidth, is mapped in Figure 4.[14] Additionally, we provide summary statistics for each year of data in our sample in Table 3 as well as a list of variables, along with descriptions, measurement frequencies, and sources in Appendix Table A.1.

Where appropriate, we also run a simple regression discontinuity specification. We do this when investigating COVID outcomes for which there are no pre-trends or for graphing results as changes from their pre-COVID baselines. These regressions take the following form:

$$Y_i = \alpha + \beta D_i + \gamma R_i + \theta(R_i \cdot D_i) + \varepsilon_i; \quad (4)$$

where *i* indexes school districts. Here, we do not interact our right-hand side variables with a series of year dummies; rather, we run these regressions cross-sectionally for each year at a time. Outcomes in this specification can be either observations for a specific year, or changes from a pre-COVID baseline, which should conform with our estimates from equation 3b for the appropriate year. Just as in Equations 3a and 3b, we can also use a second-degree polynomial for our running variable. We present robust standard errors for this specification and weight observations by enrollments in SY 2020. ε_i is again an error term.

The main limitation with the difference-in-discontinuities and regression discontinuity designs is that they produce estimates of treatment effects local to the 5% poverty qualification

[13] 50 students is about the capacity of one school bus, which can fit 48 passengers in 12 rows, 4 students across.
[14] Our sample does not capture the effects of ESSER funds in the South as many districts are either too large or too poor (or both) to be close enough to the 5% poverty cutoff.

cutoff for additional ESSER funds. We investigate how the typical school district in our preferred bandwidth sample, 50 formula children from the cutoff, compares to the entire universe of school districts from the CCD data in Table 4. We can see that the typical student in a school included in our difference-in-discontinuities sample attends a district that is smaller in terms of both students and number of schools, as well as significantly higher in socio-economic status. Naturally, students in our difference in discontinuity sample attend schools in districts with an average poverty share of 5.4%, whereas the typical student nationally attends a district with a poverty share just under 20%.

4.2 Diagnostics and First-Stage Results

The difference-in-discontinuities design relies on several assumptions typically associated with the difference-in-differences and regression discontinuity designs.

First, the discontinuity needs to be associated with a strong jump in ESSER funds per pupil, which we demonstrate in Table 5 using Equation 4. Our baseline 50-formula child bandwidth, first-degree polynomial estimator in Column 3 suggests that the poverty threshold is associated with an increase of $388 ($452) per pupil. When we use the second-degree polynomial, that estimate increases to $452 per pupil. Both of these estimates have F-statistics on the 5% poverty share dummy well above 10. We saw this discontinuity on visual display in Figure 3.

The roughly $400 per pupil in ESSER funds captured in the first-stage estimates of Table 5 are best contextualized by the district revenues and expenditures presented in Table 1. In the sample of districts used in the difference-in-discontinuities design, a typical district spends

$3,410 per pupil on instruction after accounting for instructor salaries. Therefore, the jump in ESSER funds at the 5% poverty threshold represents a very large one-time 11% increase in discretionary instructional funding for the typical district in this sample. Second, we need districts just on either side of the threshold to be similar in terms of their covariate trends before COVID, although the pre-existing discontinuity should generate some differences in levels. We now turn to an analysis of covariate balance in trends, which is the balance test with direct bearing on the validity of our estimation framework. For this analysis, we estimate Equation 4 using trends in covariates on the left-hand side of the regressions. Specifically, we show estimates using changes in absenteeism from SY 2017 to SY 2019, percentage change in enrollment from fall 2016 to 2019, percent change in total revenues per pupil from SY 2017 to SY 2019, percent change in federal revenues per pupil from SY 2017 to SY 2019, change in the share of enrollment that is white from fall 2016 to 2019, change in the share of enrollment that is Black from fall 2016 to 2019, changes in reading scores from spring 2017 to 2019 measured in 2019 standard deviations, changes in math scores from spring 2017 to 2019 measured in 2019 standard deviations, changes in teachers per pupil from fall 2016 to 2019, and changes in staff per pupil from fall 2016 to 2019, where per pupil outcomes use fall 2019 enrollments. The results are reported in Figure 5 Panel A and in Table 6. Each cell of the table shows the coefficient on the qualification dummy for crossing the 5% poverty threshold and qualifying for extra funds for a different bandwidth and outcome variable. We present results using the 45, 50, and 55 student bandwidths around the qualification cutoff. In our preferred specification, which uses a 50 formula child bandwidth and a first-degree polynomial, we can see that only the share of White students seems to exhibit a slight trend when comparing changes in districts just above the qualification cutoff and just below. We show estimates using a second-degree polynomial in

Panel B of Table 6. These estimates show qualitatively similar results to those using our preferred polynomial choice in Panel A. Scatters of trend changes around the cutoff can be inspected visually in Figure 5 Panel A. These results support our difference in discontinuities approach. Next, we turn to describing how levels of covariates change across the cutoff, which will be useful for better understanding the districts in our sample.

In Table 7 and Panel B of Figure 5, we show estimates of Equation 4 that replace ESSER funds per pupil with potential covariates in levels on the left-hand side. We show estimates for balance in levels of 10 covariates, mirroring the trends regressions in Table 6: absenteeism counts in SY 2019, enrollment in fall 2019, total revenues per pupil in SY 2020 (not including ESSER), federal revenues per pupil in SY 2020 (not including ESSER), share of enrollment that is White in fall 2019, share of enrollment that is Black in fall 2019, reading scores in spring 2019 measured in standard deviations, math scores in spring 2019 measured in standard deviations, teachers per pupil in fall 2019, and staff per pupil in fall 2019.

Panel A of Table 7 shows the first-degree polynomial estimates of Equation 4 for the 45, 50, and 55 formula child bandwidths around the cutoff. Similar estimates deploying a second-degree polynomial can be found in Panel B. We note first that enrollment and the share White exhibit economically non-trivial discontinuities around the cutoff, though the estimates are noisy. A visual inspection of Figure 5 Panel B ameliorates potential concerns with these differences in levels, however, as these estimates are driven in large part by a district located three bins to the left of the cutoff, which is an outlier with respect to both enrollment and the share of students who are White.[15] In the districts immediately surrounding the cutoff, these levels of White student shares appear smooth in levels. We observe statistically stronger, but economically

[15] Our full set of results is robust to dropping this outlying district, Leander Independent School District in Texas.

modest, discontinuities with respect to baseline levels of federal revenues and teachers per pupil. These discontinuities in baseline levels are in line with what one should expect as a consequence of the Title IA formulas. The discontinuity in the baseline level of teachers per pupil is 0.005, implying 1 additional teacher per every 200 students. In 2019, the typical district in our sample had about 14 teachers for every 200 students. These differences may be directly associated with the Title 1A program, which aims to increase teacher counts. Indeed, this is why we adopt a difference-in-discontinuities design rather than a static regression discontinuity design.

5. Results

5.1 District Performance During the Pandemic

During the COVID-19 pandemic, districts were forced to balance responding to the disease and maintaining their regular educational mission. Naturally, the disruption of the pandemic response and the uncertainty about how best to mitigate the harms of COVID while continuing to teach students led to a variety of different district policy choices. In the aggregate, test scores declined during the pandemic, as we saw in Figure 2. These decreased test scores have increasingly been associated with virtual learning during SY 2020 and SY 2021 (Halloran et al., 2021; Oster et al. 2021). Moreover, during the pandemic enrollments declined sharply and they have only partially recovered since 2021. Panel A of Figure 6 shows that about 1.25 million students were "missing" from public schools in SY 2021 compared to SY 2020 (measured in the fall semester), returning enrollment to 2010 levels. By SY 2023, enrollment has only recovered to a 750,000 student decrease from SY 2020.

Against this backdrop we investigate if the funds to public schools delivered through ESSER allowed schools to stem the decrease in test scores and to retain additional students.

5.1.1 Traditional Measures of School Performance

We present our main district performance outcomes in Table 8. For each outcome variable, we present first-degree polynomial estimates for three bandwidths from the cutoff, 45, 50, and 55 formula children.[16] Columns 1 through 6 show how test scores for math and reading evolved in response to $388 per student in ESSER funds. Using our preferred specification of a 50-formula child bandwidth, ESSER funds seem to have not impacted test scores during SY 2022 and SY 2023.[17] Using our coefficient for 2023 for math scores in Column 2, we can rule out effects larger than a 0.018 (2019) standard deviations increase in scores from ESSER funds, compared to an about 0.1 standard deviation decline in scores on average in SY 2023 compared to SY 2019. Similarly, for reading scores we can rule out effects larger than 0.038 standard deviations in scores from ESSER funds. These effect sizes for reading are a much larger share of the learning loss from SY 2023 compared to SY 2019, which was about 0.05 standard deviations. We present these estimates as event studies in Panel A and B of Figure 7.[18]

We present our results for enrollments in Columns 7 through 9 of the same table. Since enrollment is measured in the fall semester, we use SY 2020 as the base year for enrollments. Our preferred specification in Column 8 shows that an additional $388 per pupil in ESSER funds

[16] We present an accompanying table of second-degree polynomial estimates in Table A.2.
[17] No testing was conducted in 2021. The baseline for these regressions is the last available testing year in the Spring of 2019 (SY 2019).
[18] A large driver of the imprecision in these estimates is the decline in sample available, as many districts do not have SEDA estimates available.

helped retain about 125 students on average in SY 2021. The point estimate remains positive but attenuates in magnitude in SY 2022 and SY 2023, consistent with enrollment rebounds exhibiting a moderate degree of "catch up" in districts to the left of the discontinuity over time. We report linear coefficients that add the SY 2021, SY 2022, and SY 2023 impacts, showing a total retention of about 283 student-years over the three school years, which is statistically significant at the 5% level. This represents an increase of about a 2.3% enrollment retention compared to the pre-COVID mean for our sample.

We also present these changes in enrollments from the pre-COVID baselinein a binned scatter plot to visually inspect the how changes behave around the 5% poverty cutoff for additional ESSER funds. Figure 8 shows changes in enrollments from fall 2019 for 2021, 2022, and 2023 against our centered running variable of formula children, weighting our bins by pre-COVID enrollments. We see a clear discontinuity in enrollment changes around the cutoff in 2021 and 2022, and an attenuated change in 2023, consistent with the estimates reported in Table 8. These results suggest that ESSER may have helped schools retain students, and that in the absence of ESSER funds enrollment declines might have been even higher than those observed, which were about 5% for the median district in 2022 compared to 2019 and about 2% nationally (Burtis and Goulas, 2023).

As our enrollment results suggest ESSER helped retain students, at least initially, we present a more detailed look at the impact of additional funds on enrollments across grade-level groupings. First, in Panel B of Figure 6, we show indexed trends in national enrollments by grade level. Here we again index enrollment for grades K-5, grades 6-8, and grades 9-12 to their pre-COVID values. We can see that nationally younger students saw larger trend breaks in enrollments than high school students during COVID, at least in percentage terms.

We show detailed impacts of additional ESSER funds on enrollments by grade in Table 9. First, in Column 1, we show the same regression as that in Column 8 of Table 8. Then, we present estimates for districts that had any enrollment at a given grade-level group in 2019 in Columns 2 through 6. We present estimates for pre-kindergarten, kindergarten, grades 1 through 5, grades 6 through 8, and finally grades 9 through 12, with the later three categories typically mapping on to elementary, middle, and high school students. Here, we can see from Columns 3, 4, and 5 that most of the 283 student-year increase over 3 years came from movement in lower-grade levels, especially grades 1 through 8.

Even if students are enrolled, they may attend class at lower rates as a result of the impacts of the pandemic. We display our measure of the impact of ESSER funds on absenteeism, which is defined as a student missing 10% of school days or morein the final columns of Table 8. Although the estimates for each individual year seem to show relatively imprecise estimates for absenteeism, the cumulative coefficient shows that the number of absentee students was likely decreased by ESSER funds by about 220 absentee students-years over the three years. The average school in our sample had about 337 absentee students annually before COVID-19. These results suggest that declines in test scores associated with ESSER may be the result of retaining lower-performing students, as schools with less absenteeism may have seen their test scores suffer from negative selection into their test-taking student population.[19]

In the next section, we evaluate how ESSER funds impacted pandemic-specific measures of school district performance.

[19] This is evidence that suggests increased absenteeism would help, rather than hurt test scores. These results are in conflict with the conclusions of the Biden CEA, which claimed that absenteeism was a driver of low test scores (CEA, 2023).

5.1.2 Pandemic Measures of Performance

We next consider two outcomes of unique importance to the pandemic, the speed at which school districts opened and their potential contribution to area cases of COVID-19. School districts offering more days of in person learning had higher mobility levels as measured by cellphone activity, relative to their 2019 baseline, than districts offering more hybrid or remote learning (Parolin and Lee, 2022). Given that these data come already differenced and without pre-trends, we present them in cross sections by year following Equation 4.

We display our results for estimates of the impact of ESSER funds on mobility changes in Panel A of Table 10. We display each year of data separately for 2020, 2021, and 2022 and show results for both first- and second-degree polynomials. We find that additional ESSER funds may have helped districts bring students back to the classroom at a higher rate than schools that received fewer funds. In 2020, districts receiving additional ESSER funds had about a 5% increase in mobility around their schools compared to mobility changes in below-cutoff districts, although these results attenuate and are statistically insignificant at the 95% confidence level with a second-degree polynomial specification. In 2021, we see this gap increase to almost 9%, and both first- and second-degree polynomial specifications are statistically significant at the 5% level. Finally, the mobility gaps persists even into 2022, as districts that received more funding appear to have had almost 11% more cellphone mobility compared to their 2019 baseline measures than districts below the 5% poverty threshold. Our results for 2022 are robust to using a second-degree polynomial. For further confirmation, we show the scatter plots associated with mobility changes against the running variable in enrollment weighted bins in Figure 9. Here, we

can clearly see the discontinuity emerge and grow in each subsequent year, which corroborates our estimates in Table 10.

Many observers and stakeholders were concerned that opening schools would increase COVID-19 cases in the neighboring populations. In Table 11, we investigate whether or not additional ESSER funding caused excess community spread of COVID-19, as measured by average daily case rates in SY 2021 in the surrounding area. We present estimates of Equation 4 for three bandwidths and both polynomial specifications. We find that ESSER funds had no detectable impact on case rates, and the zeros appear to be relatively precise. In our preferred specification, we can rule out effects larger than an increase of half a case per 100,000 residents per day. This upper bound can be compared to a control mean for districts below the qualification threshold of about 24 cases per 100,000 resident per day.[20] It is important to note that these results only suggest that daily case rates and increased mobility around schools do not appear to be correlated. That said our research design cannot determine that school openings caused or did not cause an uptick in COVID cases. Our difference-in-discontinuities design can identify the causal effect of additional ESSER funds on outcomes at our qualification cutoff. We cannot untangle the causal relationship between outcome variables, we can only identify that they may be correlated.

In the next section we present results that will speak to mechanisms that school districts may underpin our findings thus far.

5.2 Measuring School District Choices

[20] Using alternative case rates from the New York Times (2023), we find broadly similar results to those shown in Table 11.

We now turn to estimates of how additional ESSER funds impacted school district choices surrounding staffing and district budgets. These results may suggest mechanisms through which districts were able to retain students and return to in-person learning. For both sets of outcomes we return to presenting estimates of Equation 3a.

5.2.1 District Staffing and Composition

We show the estimated impacts of an additional $388 per pupil of ESSER funds on district full-time-equivalent staffing levels per student in Table 12. We present results for our preferred specification, which uses a first-degree polynomial and a bandwidth of 50 formula children from the qualification cutoff at the 5% poverty share threshold. In Column 1, we show changes in total staff per pupil, and then we break this total into teachers and non-teachers in Columns 2 and 3. In the remaining three columns we include categories of staff that may be of particular interest during the pandemic and with respect to federal grants: counselors, instructional aids, and district administrators. In order for Columns 2 and 3 to add to Column 1, we only include districts in which the number of teachers reported is smaller than the number of total staff reported, which should include teachers as a sub-category. We do not see meaningful changes in total staff or either of its sub-categories in Column 1 through 3. For total staff changes per pupil, we can rule out combined increases over the three school years greater than 16 staff members per 1,000 students, which is around 11% of the pre-COVID baseline of 147 teachers per 1,000 students. We do not see a meaningful change in the numbers of counselors per pupil or instructional aids in Columns 4 and 5. These two categories might have been associated with helping students manage the additional burdens of learning during the pandemic. Finally, in Column 6 we show

that ESSER funds seem to have increased the number of district administrators. Additional ESSER funds increased the count of district administrators by 1 administrator per 1,000 students compared to districts below the qualification threshold in SY 2023. This compares to a pre-COVID baseline of 2 administrators per 1,000 students in our sample.

5.2.2 District Finances

Finally, we turn to our last set of outcomes, district level expenditures and revenues. In Panel A of Table 13 we investigate the causal relationship between additional ESSER funds per pupil and school district revenue sources, also on a per pupil basis.[21] In Column 1, we show changes in total district revenues per pupil in response to an additional $388 per pupil in ESSER funds. These results show a negative point estimate that is not significant at conventional confidence levels. In Columns 2 through 4, we show results for federal, state, and local revenues.[22] Column 2 suggests that in SY 2021, districts on the right side of the cutoff saw increases in their federal revenues compared to schools below the cutoff. The combined coefficient suggests the above threshold districts received an additional $261 per pupil over 3 years. These funds are likely a partial accounting of ESSER funds. In Column 3 we show that districts above the cutoff did not see any additional funding from the state, with the combined coefficient suggesting an effect size of $102 per pupil over 3 years, which is less than 2% of the pre-COVID mean on an annual basis. We turn to local revenues, which typically make up the majority of school revenues, in

[21] Recall that all per pupil variables use enrollments from 2019 (SY 2020) to scale outcomes, so changes are driven only by the numerator.

[22] In the Common Core of Data, federal revenues do include COVID funds, however districts likely have not reported all of their funds in these data. There is also a special COVID exhibit for ESSER funds, but these data do not match the DOE transparency API, which is our source for ESSER funds.

Column 4. Here, we can see that an additional $388 per pupil in ESSER funds is associated with a significant *decrease* in local revenues in SY 2021 and SY 2022. The combined estimates suggest that over the three years, crossing the qualification cutoff for additional ESSER funds is associated with a $907 per pupil decrease in local revenues, or about 8% of the pre-COVID average. In Column 5 we can see that about 80% of this revenue decline came from decreases in revenues from property taxes. We also show the decline in local revenues as an event study in Figure 10.

Home prices rose rapidly during the pandemic (S&P Dow Jones Indices LLC, 2025). As tax assessments increase with home value appreciation, one would expect voters to ask for relief. We show several pieces of evidence to suggest that these revenue declines are the results of rate cuts. First, in Figure 11, using data from the National Council of State Legislators (2024) we show that property taxes were an active margin of adjustment during the pandemic. We present the fraction of states that implemented any increase or any decrease (not mutually exclusive) between 2016 and 2019 and between 2020 and 2023. For both periods, over 40% of states made some change to decrease property taxes. Additionally, we located the budgets and board meeting minutes for several districts in our sample that qualified for additional ESSER funds and found that they did in fact lower property tax rates during the pandemic. These districts are Babers Hill Independent School District in Texas[23], Peaster Independent School District in Texas[24], and Lake Geneva School District in Wisconsin.[25] These anecdotes provide suggestive evidence that

[23] Barbers Hill ISD lowered its tax rates from 1.26% to 1.15% in 2020 (Barbers Hill ISD, 2025)
[24] Pester ISD has steadily lowered its total tax rate since FY 2021, which is set the August before the Fiscal Year, from 1.372% down to 1.1664% in 2024 (Peaster ISD, 2024).
[25] Lake Geneva decreased its tax rate from 4.29 mill in FY 2019 to 3.76 mill in FY 2021 (Wisconsin Department of Public Instruction, 2019 and 2021).

districts may have lowered their property taxes in response to receiving revenues from the federal government. We turn to expenditures next.

In Panel B of Table 13, we present estimates for the impact of qualifying for additional ESSER funds on district expenditures. We present estimates for total expenditures, elementary and secondary education expenditures, capital outlays, instructional spending (including salaries), salaries of all employees, and benefits of all employees on a per pupil base in Columns 1 through 6, respectively. Across expenditure categories we find no evidence of changes in spending detectable at a conventional significance level. Interestingly, we also see almost no changes in the share of school expenditures among major revenue categories when we look at the full sample of districts, including instruction, capital outlays, and student support services, in Panel B of Table 13. These results are significant in part because the flypaper effect would suggest that revenues from ESSER should be associated with corresponding expenditure increases (Gramlich, 1977; Hines and Thaler, 1995; Fischer, 1982). Instead, the districts appear to have reduced tax revenues.

In Table 14, we explore how districts may have funded their revenue reductions after COVID. We present estimates of Equation 3b using the debt and asset variables available to us in the Common Core Data files.[26] We show estimates using total revenues, total expenditures, total debt outstanding, interest payments on debt, total assets, sinking fund assets, bond fund assets, and other assets on a per pupil basis. The fiscal gap can be seen in the differences between revenues in Column 1 and expenditures in Column 2. Between 2020 and 2022, a combined -$1,289 per pupil spending gap emerged ($430 per pupil per year). This gap is small, only about

[26] The debt data in the CCD files do not include critical details about the maturity and interest rates associated with school bonds. A new paper by Biasi et al. (Forthcoming) collects these data for 29 states but stops before the pandemic.

2% of baseline revenues, and imprecise. However, we cannot rule out more substantial gaps of roughly -$4,626 per student across the three years, or -$1,542 per student per year.

In the remainder of Table 14, we investigate how school districts changed their debt and asset levels to fund these revenue reductions. In Column 3, we show how the face value (par value) of total debt changed for districts that qualified for ESSER.[27] We find imprecise estimates that may be large enough to cover the fiscal gap. Across the three years, we find that districts that qualified for ESSER raised an additional $1,809 per student across the three years in debt. However, these estimates are noisy, likely in part due to data quality issues. We give additional context for these estimates by examining changes in the interest payments made by districts in Column 4. The interest payment change should be the change in the face value of debt, multiplied by the coupon rate. Using the combined estimates, these interest payments imply a coupon rate of about 11% on average paid on these new debts during the pandemic. These rates are in the right tail of those found for 1997 to 2017 by Biasi et al. (Forthcoming).[28] Although a rate of 11% is not impossible, it likely reflects the lack of reliability of the CCD data. Even so, there is some additional support for the possibility that school districts raised debt during the pandemic. Lieberman (2023) reports that school districts, through bond elections, in 2021 and 2022 have proposed and passed larger numbers of bonds than in 2020, and the count of bonds proposed in 2022, 2,134, almost matches the previous high in 2018 of 2,163 bonds proposed.

In addition to new debt, the district could also draw down existing cash and other liquid asset funds to cover their post-COVID fiscal gap. In Column 5 of Table 14, we show estimates for changes in a district's total assets. Total assets are made up of sinking funds, unspent bond

[27] The CCD files classify debt less than 1 year in maturity as short-term and everything else as long-term. Total debt here adds together long- and short-term debt outstanding at the end of the fiscal year for each district.
[28] Most bonds in the Biasi et al. (Forthcoming) dataset have annual interest rates between 2% and 6%, with the maximum rate at just under 10%.

funds, and other assets, which include general funds and other investments made by the district. In Column 5, we find imprecise but negative estimates for district assets. Across the three years, districts on average drew down their asset funds by -$620 per student per year, although these results are statistically insignificant. We can see the three main components of total assets in Column 6 through 8, which all show similarly imprecise estimates. However, these results do suggest that the majority of asset reductions came from unspent bond funds. Although these results are imprecise, they do suggest that districts may have supported their revenue reductions during COVID by drawing down their existing asset funds.

Finally, in Table 15 we will explore whether or not compensation per employee meaningfully changed with additional ESSER funds. This table reports estimates of Equation 3b, but the left-hand side variable is total compensation per employee. We use benefits and salaries as our measure of compensation, and we present these estimates for our three main staff categories: total staff, teachers, and non-teachers. In Column 1, we find that total compensation per staff member (teachers and non-teachers) increased in 2020 by $2,098 per employee, or a 2.5% increase over pre-COVID baseline compensation. Compensation may have remained elevated in 2021, but this result is imprecise. Then, in 2022, compensation returned to baseline. This pattern is similarly reflected in Columns 2 and 3 for teachers and non-teachers, respectively. However, the increase in compensation during the pandemic seem to be more pronounced for non-teachers, with a 10.6% increase in 2021 compared to the pre-COVID baseline that is statistically different than zero. Overall, the combined $5,894 in additional compensation per employee across the 3 years roughly translates into a $289 per student per year increase in expenditures using the pre-COVID student to staff ratio. These results suggest a change in the

labor share of district revenues during the pandemic, although these results should not be overemphasized as the combined effects are imprecise.

6. Conclusion

Emergency funding for K-12 public schools was an important component of recession spending packages during both the Great Recession and COVID, involving $50 billion and $190 billion in additional expenditures, respectively (Shores and Steinberg, 2022). In this paper we have provided causal evidence for a subset of school districts that the assistance provided by ESSER to local school districts may have helped retain students and open schools faster. However, we see no evidence that ESSER funds helped mitigate learning loss, at least in the short run. Meta analysis by Jackson and Mackevicius (2021) suggests that an additional $1,000 per year of K-12 school spending raises scores by 0.012 standard deviations, and we emphasize that while we obtain null estimates we cannot rule out estimates of that magnitude. Additionally, the insignificant and negative point estimates we obtain may in part reflect composition effects, since we find that additional federal funds led districts to retain more students.

In addition to our findings for school performance, we provide evidence on school district finances. Districts did not increase expenditures in SY 2021 or SY 2022, but they did cut local tax revenues when receiving additional ESSER funds. We also find evidence of staffing increases in SY 2023 for teachers, guidance counselors, and administrators in response to additional ESSER funds.

Overall, these findings suggest that ESSER funds, when delivered to lower poverty and smaller districts, were somewhat successful in achieving at least some of their policy goals and helping districts weather the pandemic.

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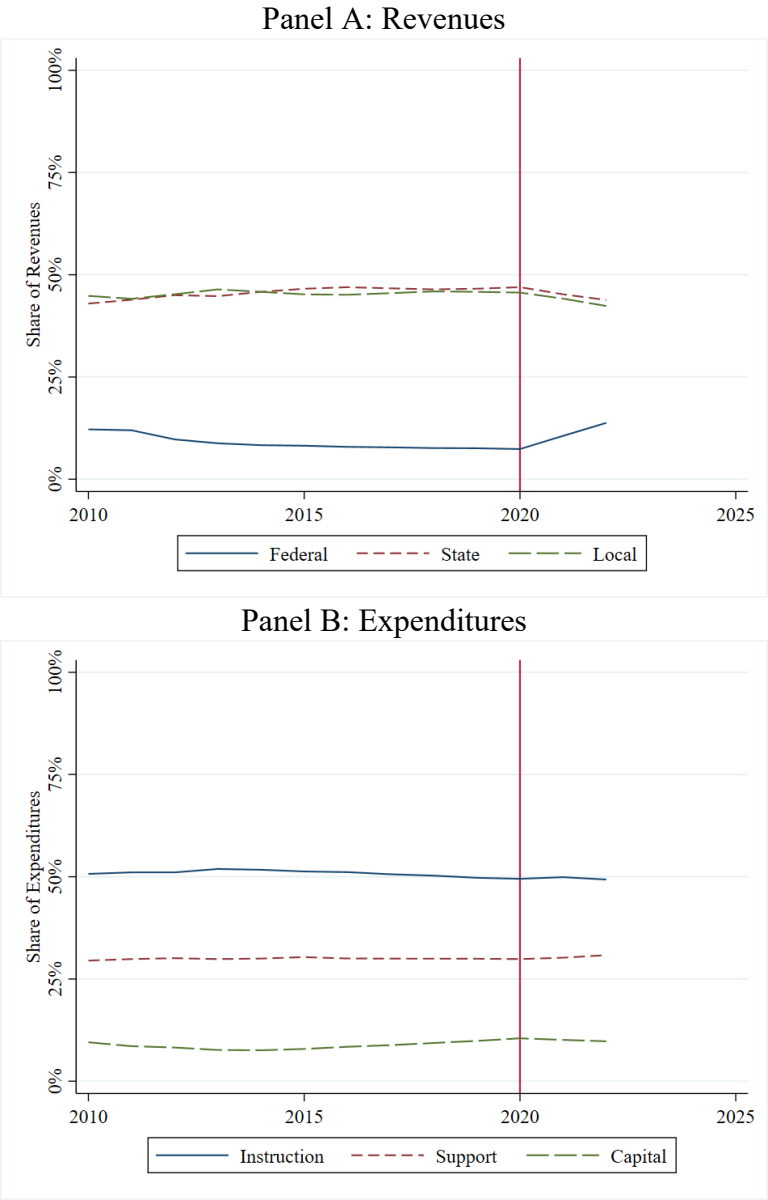
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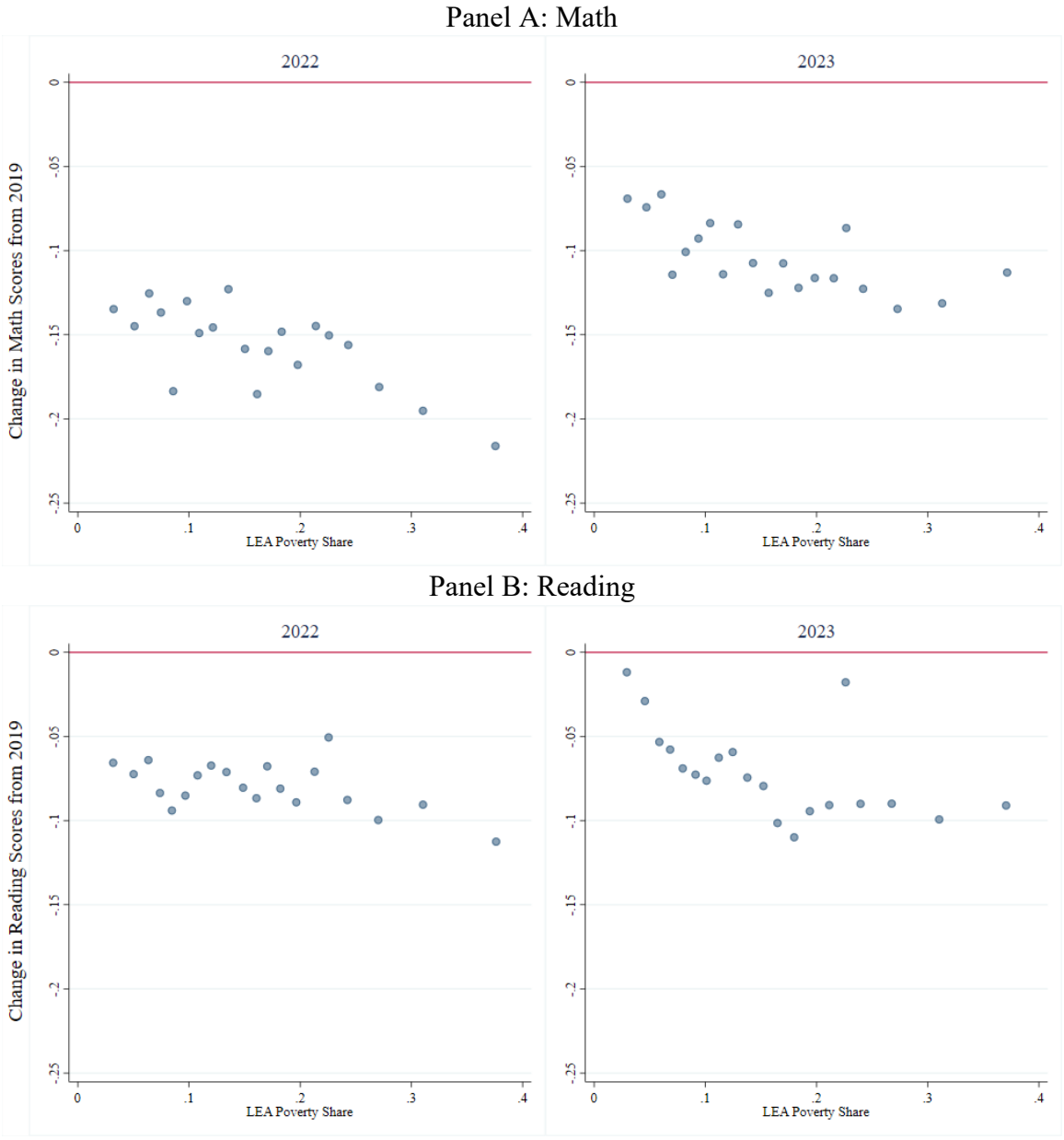
Figures

Figure 1: National Shares of School District Revenues and Expenditures



Note: This figure shows national shares of revenues by source, federal, state, and local in panel A over time. In Panel B, the figure shows shares of total expenditures for instruction, support services, and capital outlays over time. Data for this figure comes from NCES (2023).

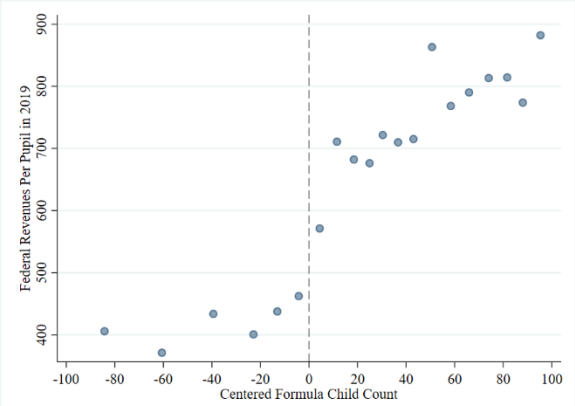
Figure 2: Pandemic Learning Loss Compared to 2019 by District Poverty Share in 2022 and 2023 for Math and Reading Scores of 3rd Through 8th Graders



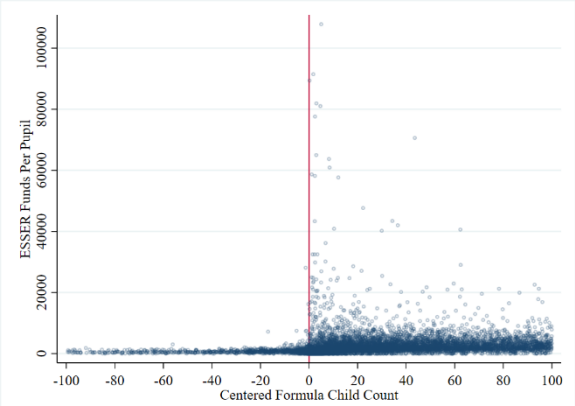
Note: This figure shows bins weighted by enrollments from SY 2020. The horizontal axis shows school district poverty shares from the 2018 SAIPE data. The vertical axis shows changes in test scores from the spring of 2019, measured in 2019 standard deviations using data from SEDA (Reardon, et al., 2024). Panel A shows changes from 2019 to 2022 and from 2019 to 2023 for math scores. Panel B shows changes from 2019 to 2022 and from 2019 to 2023 for reading scores.

Figure 3: Discontinuities in Federal Revenues and ESSER Funds at the 5% Poverty Qualification Threshold

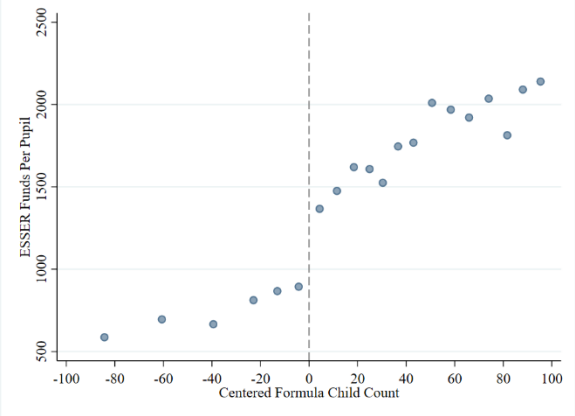
Panel A: Binned Federal Revenues in 2019 Per Pupil



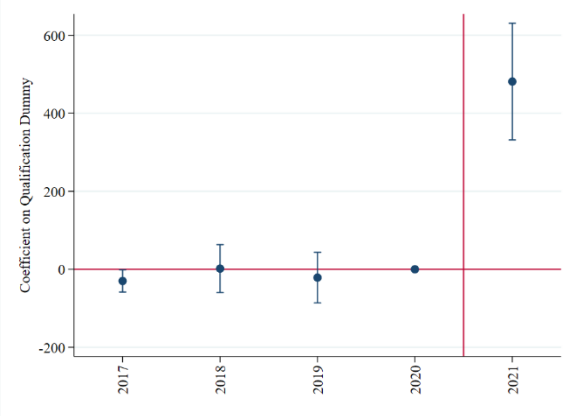
Panel B: Un-Binned ESSER Funds Per Pupil



Panel C: Binned ESSER Funds Per Pupil

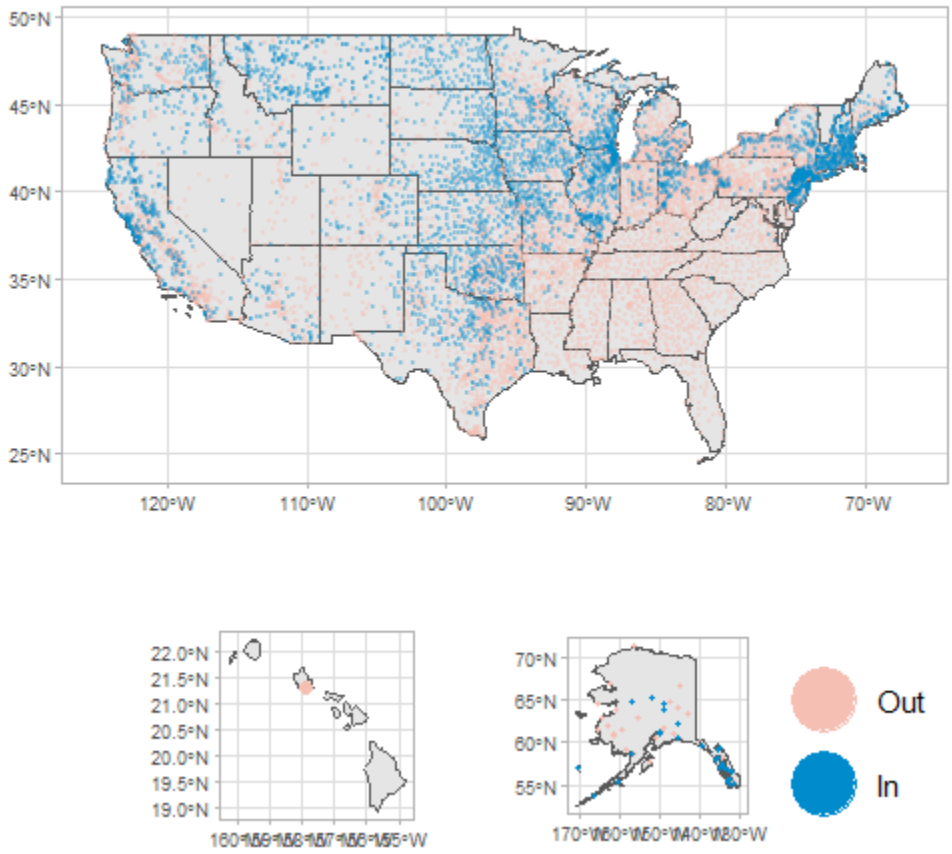


Panel D: Event-Study of Federal Revenues and ESSER Funds Per Pupil



Note: This figure shows first stage scatter plots matching to Equations 4 and 3a in the text. Scatter plots in Panels A and C use bins weighted by SY 2020 enrollment. Panel D shows coefficients from estimates of Equation 3a, described in the text. We show the qualification threshold associated with the 5% poverty share with a vertical line.

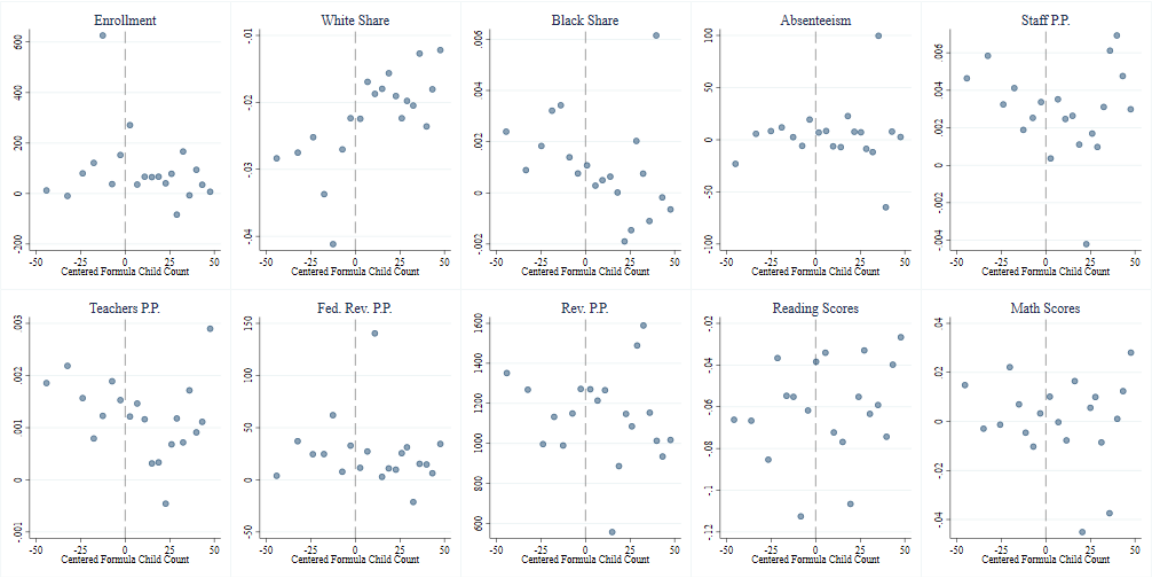
Figure 4: Map of Districts Included in the 50 Formula Child Bandwidth Difference-in-Discontinuities Sample



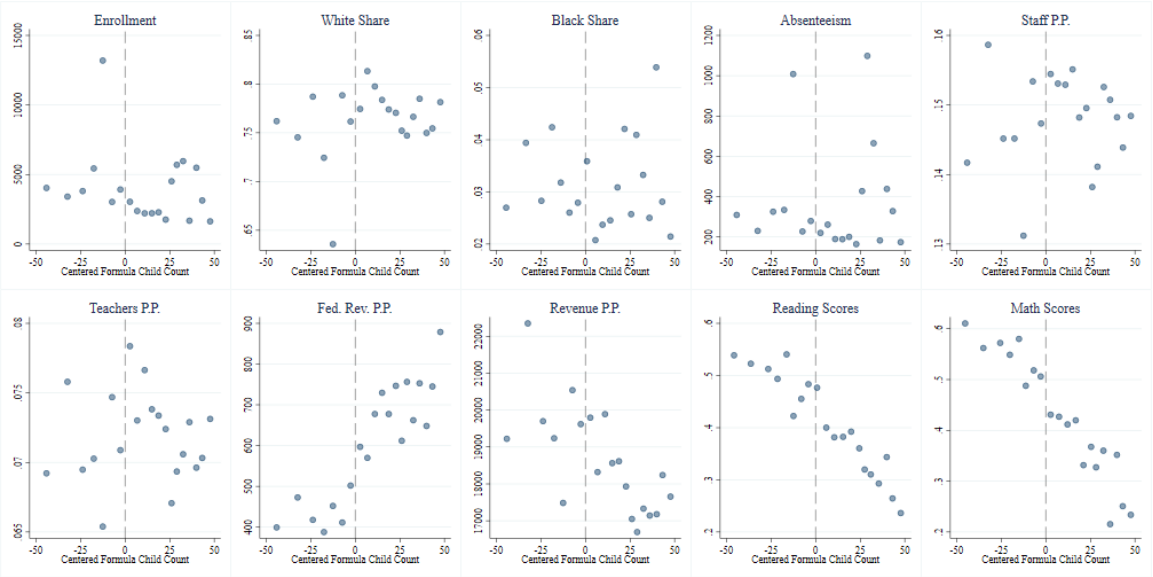
Note: This figure shows districts in the 50 formula child bandwidth sample in blue and all other districts in red.

Figure 5: Covariate Smoothness at the 5% Poverty Qualification Cutoff Before COVID in Levels and Trends

Panel A: Trends



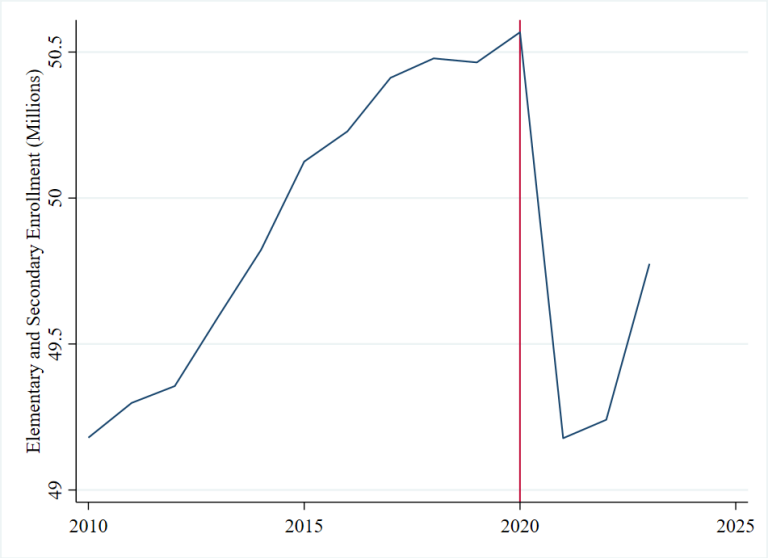
Panel B: Levels



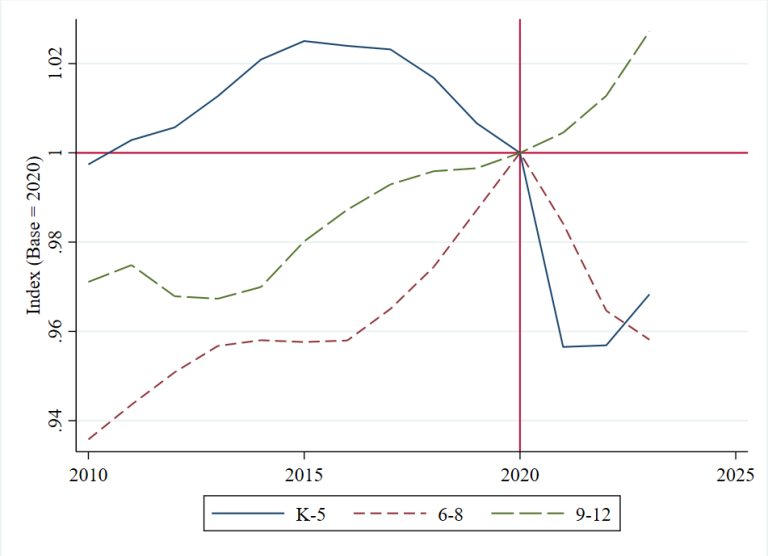
Note: This figure shows balance checks for covariate differences across the qualification cutoff as pre-COVID trends in Panel A and in levels in Panel B akin to equation 4 in the text. Scatters are weighted by SY 2020 enrollment. We show the qualification threshold associated with the 5% poverty share with a vertical dashed line.

Figure 6: National Enrollment Trends in Elementary and Secondary Schools

Panel A: Total Enrollments



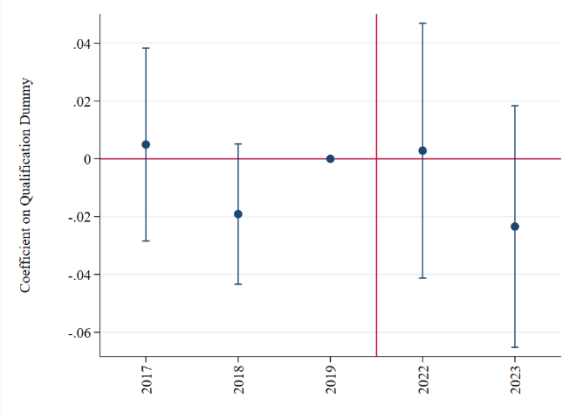
Panel B: Indexed Enrollment Trends by Grade Group



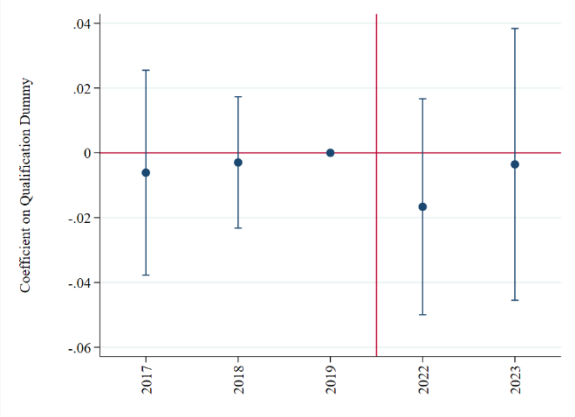
Note: Panel A of this figure shows national enrollment trends for all public schools. Panel A shows indexed enrollment trends for public schools for grades kindergarten through 5th (blue line), 6th through 8th (red line), and 9th through 12th (green line). This figure uses data from the Common Core of Data (NCES, 2023).

Figure 7: Event Studies for the Impact of Qualifying for Additional ESSER Funds Per Pupil on School District Performance from 2017 to 2023

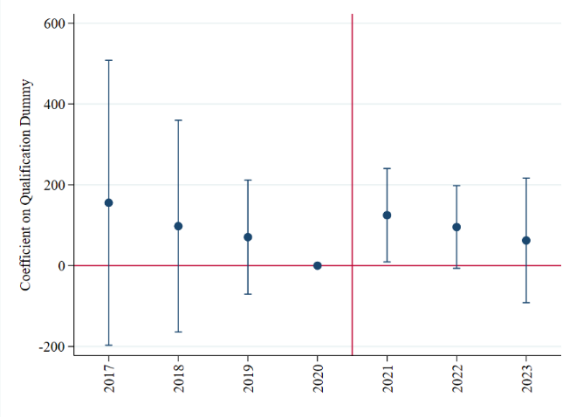
Panel A: Math



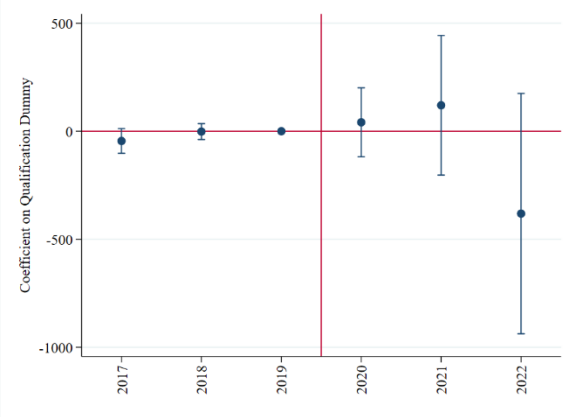
Panel B: Reading



Panel C: Enrollment

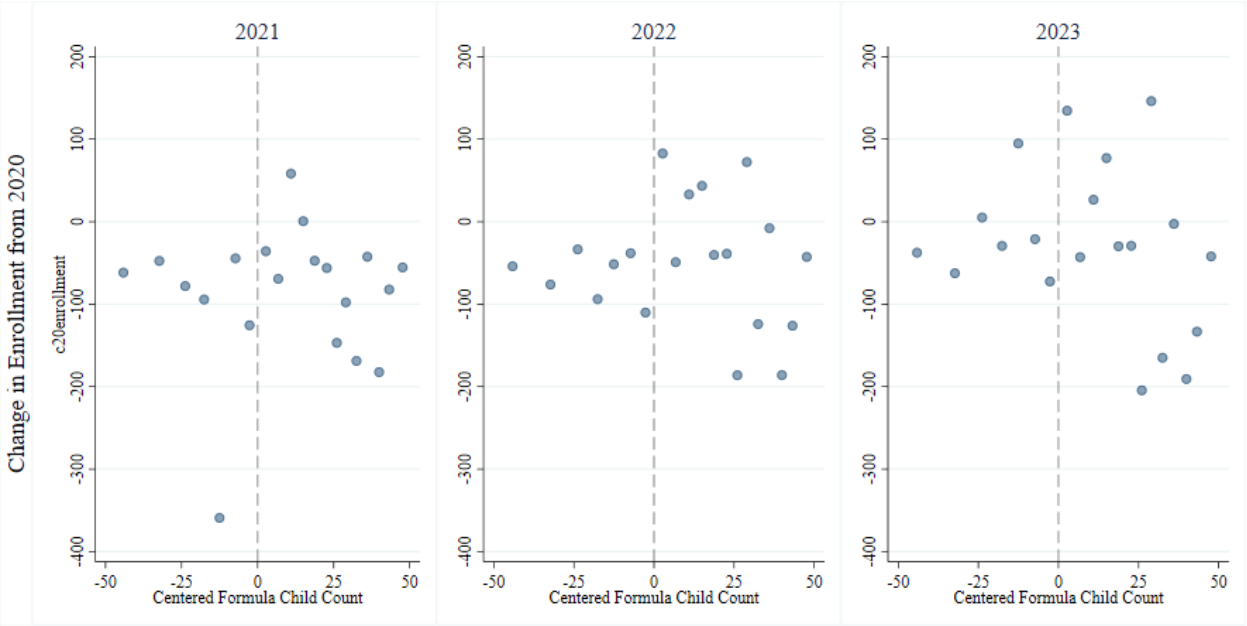


Panel D: Absenteeism



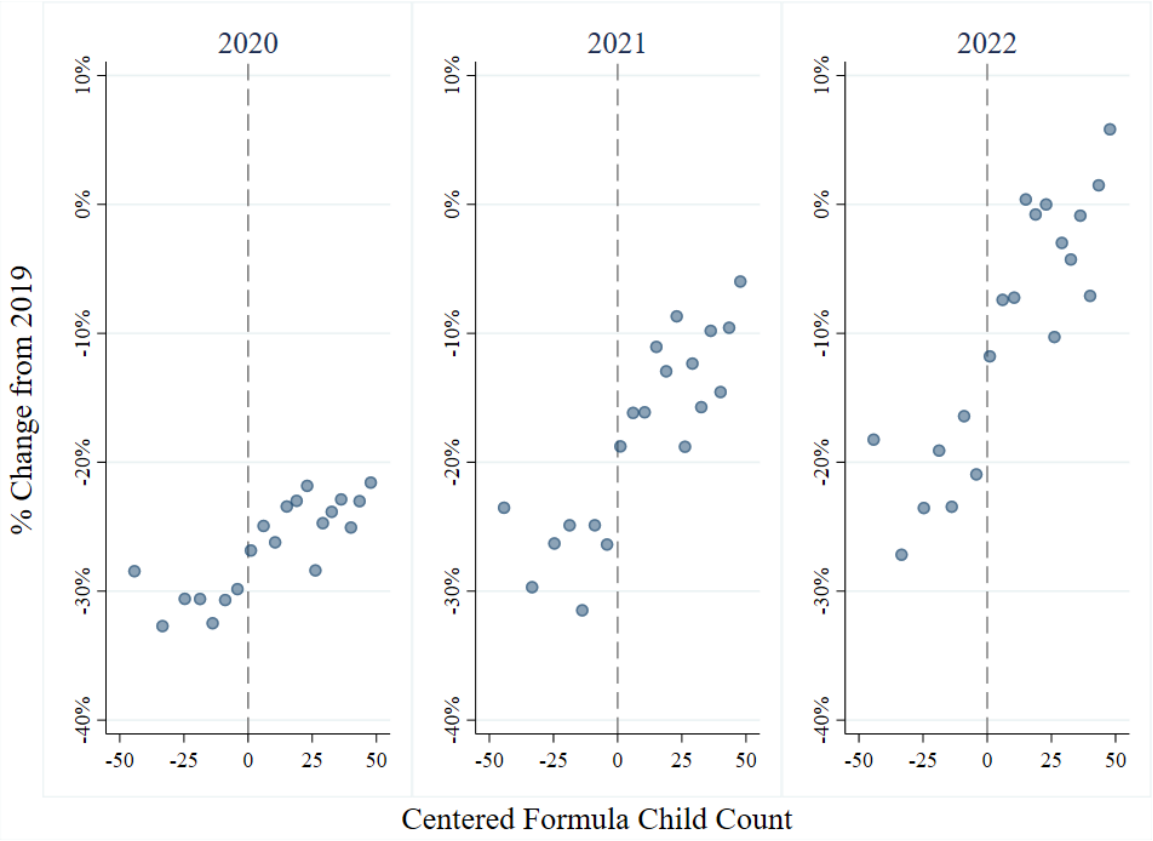
Note: These figures show coefficients from the qualification dummy interactions for each year as described in Equation 3b. These regressions correspond to Columns 2, 5, 8, and 11 of Table 8.

Figure 8: Binned Scatter of Changes in Enrollment from SY 2020 Across the 5% Poverty Qualification Cutoff in 2021, 2022, and 2023



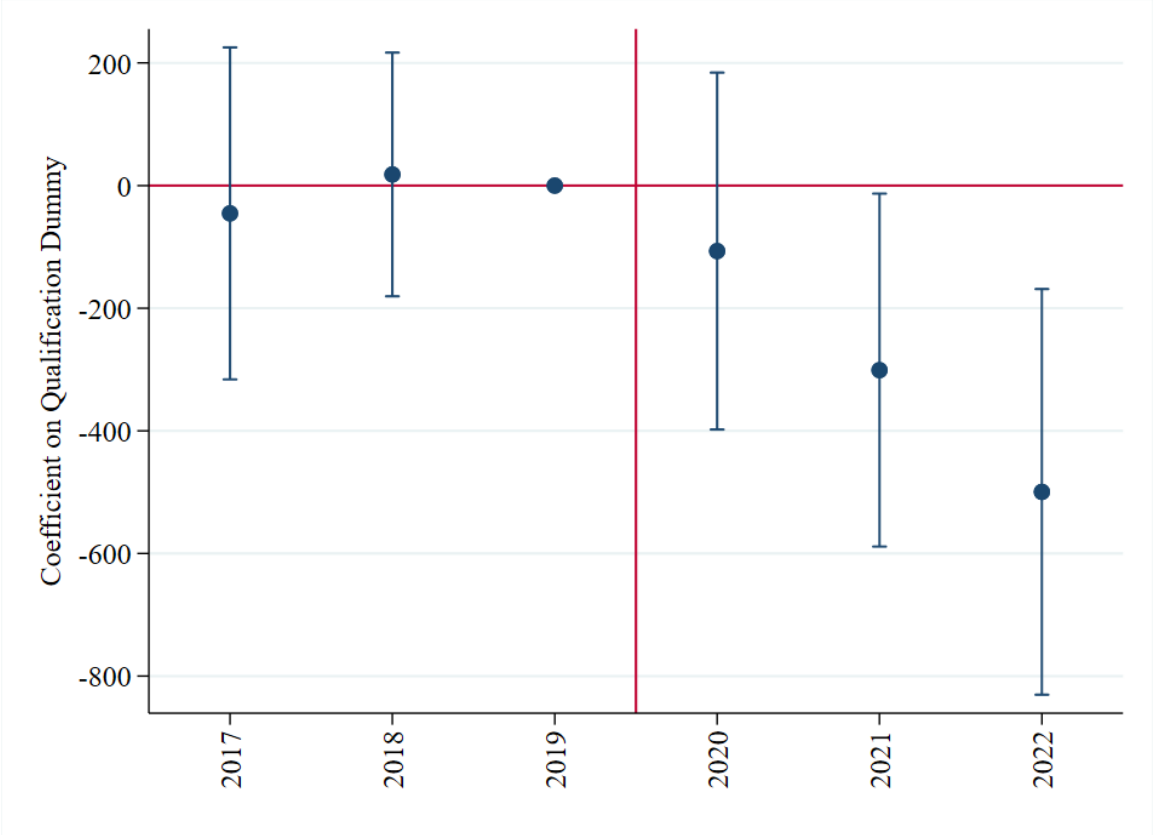
Note: This figure shows weighted binned scatters for changes in enrollment from SY 2020 on the vertical axis and the centered formula child count running variable on the horizontal axis. We show the qualification threshold associated with the 5% poverty share with a vertical dashed line.

Figure 9: Binned Scatter of Year Over Year Changes in Cell Phone Mobility from 2019 Across the 5% Poverty Qualification Cutoff in 2020, 2021, and 2022



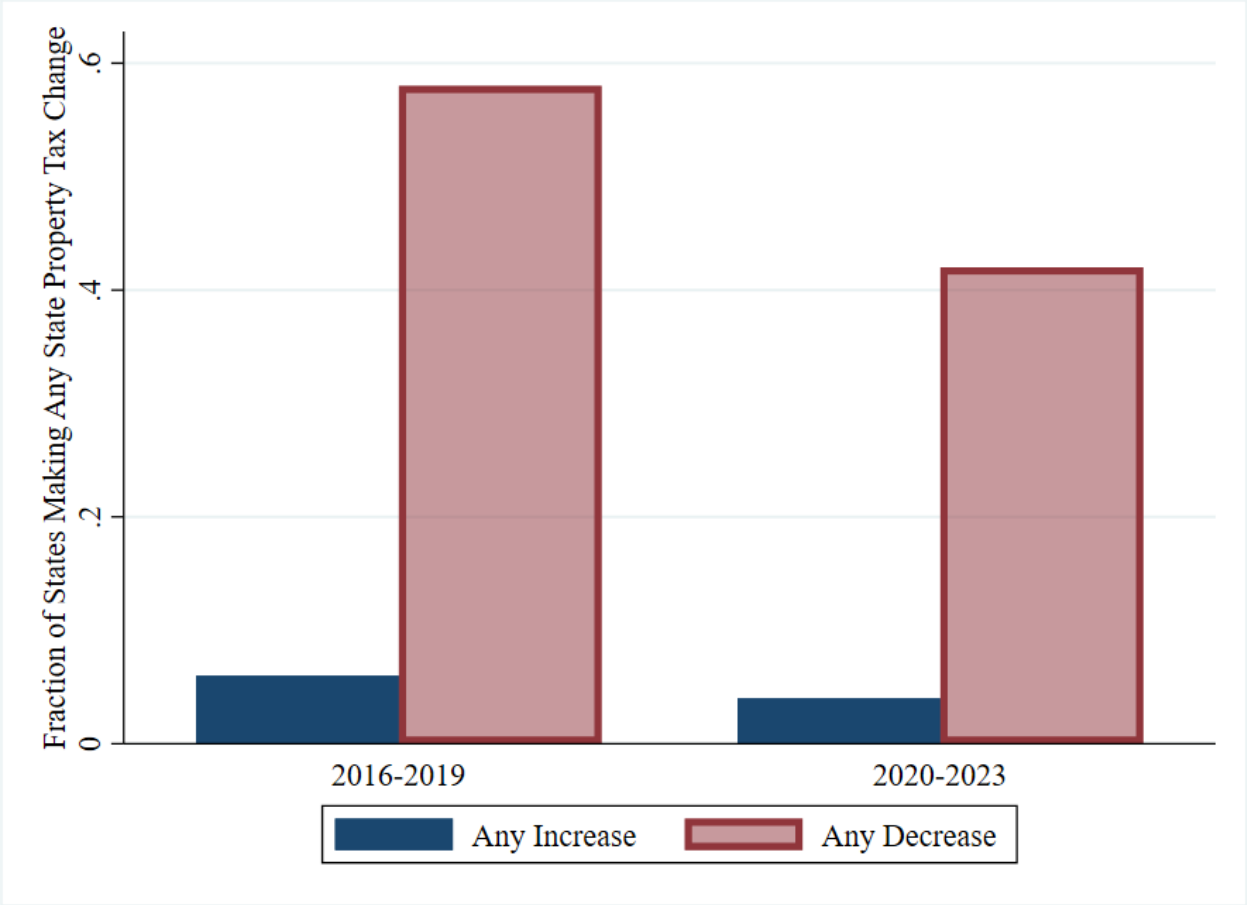
Note: This figure shows weighted binned scatters for changes in year over year cell phone mobility around schools on the vertical axis and the centered formula child count running variable on the horizontal axis. We show the qualification threshold associated with the 5% poverty share with a vertical dashed line.

Figure 10: Event Studies for the Impact of Qualifying for Additional ESSER Funds Per Pupil on School District Revenues from Local Sources from 2017 to 2022



Note: This figure shows estimates of coefficient on the qualification dummy interactions from Equation 3b as described in the text. These regressions correspond to Table 13 Column 4.

Figure 11: States That Made Any Increase or Decrease to their Property Taxes Before or After the Start of the COVID-19 Pandemic



Note: This figure shows the share of states that made any change that increased their property tax levies (in blue) or decreased their property tax levies (in red) for 2016-2019 and then 2020-2023. These data come from the National Council of State Legislators (2024)/

Tables

Table 1: Distract Financial Snapshot Around the 5% Poverty Share Qualification Cutoff in SY 2019

	Below (1)	Above (2)	Total (3)
Total Revenues	19511.7	17816.8	18407.9
	(7085.5)	(7560.9)	(7441.0)
Federal Revenues	*455.3*	*680.6*	*602.0*
	(527.3)	*(1034.8)*	*(897.6)*
State Revenues	*5764.8*	*6773.2*	*6421.5*
	(2500.8)	*(3795.3)*	*(3433.6)*
Local Revenues	*13291.6*	*10363.0*	*11384.5*
	(7169.3)	*(7328.3)*	*(7404.4)*
Total Expenditures	19714.9	17827.6	18485.9
	(7092.3)	(7590.7)	(7473.5)
Instruction Current Exp.	*10039.7*	*8839.7*	*9258.2*
	(4039.1)	*(3429.9)*	*(3697.4)*
Salaries for Instruction	*6397.2*	*5553.9*	*5848.0*
	(2592.9)	*(2187.1)*	*(2370.3)*
Enrollment	5235.0	3261.9	3950.1
	(7262.3)	(4956.6)	(5937.7)
N	693	3,983	4,676

Note: This table shows means and standard deviations for select variables for the sample of districts in the 50 formula child bandwidth sample. All revenue variables are on a per pupil basis using SY 2020 enrollments. Column 1 shows districts below the 5% poverty threshold, Column 2 shows districts above the threshold, and Column 3 shows all districts in the 50 formula child bandwidth sample. We weight calculations by SY 2020 enrollments.

Table 2: Consolidated Timeline Relevant for the Delivery of ESSER Funds

Event	Calendar Date
Every Student Succeeds Act Signed into Law	2015
Every Student Succeeds Act Fully Implemented	2017
SAIPE Data Collection for FY2020 Title 1A Funds	2018
Title 1A Appropriation Levels Set by Congress for FY2020	2019
First US COVID Case Confirmed by CDC	January 2020
First COVID School Closure in Washington State	February 2020
Families First Coronavirus Response Act Signed into Law	March 2020
CARES Act Signed into Law, Appropriating $30.75B for ESSER I	March 2020
CRRSA Signed into Law, Appropriating $54.3B for ESSER II	December 2020
ARP Signed into Law, Appropriating $122B for ESSER III	March 2021
First US COVID-19 Vaccine Delivered to the Public	December 2021
Spending Deadline for ESSER I	October 2022
Spending Deadline for ESSER II	October 2023
Spending Deadline for ARP ESSER (ESSER III)	October 2024

Note: This table shows a timeline of selected events relevant for COVID-19 and the delivery of ESSER funds.

Table 3: Select Difference-in-Discontinuities Sample Summary Statistics Across Sample Years

	2017	2018	2019	2020	2021	2022	2023	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Math Scores	0.428	0.429	0.428			0.306	0.356	0.390
	(0.267)	(0.268)	(0.272)			(0.278)	(0.297)	(0.280)
Reading Scores	0.470	0.439	0.407			0.338	0.399	0.411
	(0.255)	(0.245)	(0.245)			(0.264)	(0.287)	(0.262)
Enrollment	3858.3	3897.0	3910.2	3950.1	3863.3	3898.3	3920.6	3899.7
	(5675.9)	(5758.8)	(5824.5)	(5937.7)	(5783.1)	(5896.6)	(5944.1)	(5831.8)
Total Schools	6.209	6.227	6.221	6.277	6.278	6.379	6.412	6.286
	(6.592)	(6.679)	(6.767)	(6.935)	(6.912)	(7.118)	(7.364)	(6.914)
Total Revenues	17611.2	17937.2	18466.1	18628.8	19110.8	19312.5		18511.1
	(6788.4)	(6908.6)	(7463.1)	(8234.6)	(7122.4)	(15158.6)		(9127.8)
Federal Revenues	577.1	586.4	604.3	604.0	1049.6	1435.7		809.5
	(694.4)	(849.6)	(918.7)	(727.4)	(988.9)	(2003.4)		(1169.4)
Fed. Rev. w/ ESSER	577.5	585.8	602.0	598.3	2206.4	1046.9		936.1
	(709.4)	(848.3)	(897.6)	(723.5)	(1797.0)	(856.5)		(1198.4)
Teachers PP	0.0706	0.0711	0.0714	0.0718	0.0739	0.0744	0.0751	0.0726
	(0.0190)	(0.0191)	(0.0192)	(0.0191)	(0.0196)	(0.0199)	(0.0201)	(0.0195)
Enr. Share White	0.785	0.777	0.770	0.763	0.756	0.746		0.766
	(0.166)	(0.169)	(0.173)	(0.176)	(0.177)	(0.180)		(0.174)
Mobility Change				-0.266	-0.179	-0.0973		-0.181
from 2019				(0.155)	(0.225)	(0.277)		(0.235)
Formula Children	233.1	218.5	212.5	197.2	191.8	238.5		215.3
	(302.3)	(288.1)	(299.0)	(267.0)	(279.7)	(350.1)		(299.3)
Pop Ages 5 to 17	4074.2	4071.7	4061.4	4056.8	4063.8	4350.1		4113.0
	(5924.4)	(5969.7)	(6002.7)	(6051.0)	(6122.4)	(6548.8)		(6107.1)

Note: This table shows summary statistics for the 50 formula child bandwidth sample for each sample year, as data are available. Means are presented with standard deviations in parentheses. We weight calculations by SY 2020 enrollments.

Table 4: Comparison of All US School Districts to the Difference-in-Discontinuities Sample With a 50 Formula Children Bandwidth

	All Districts (1)	Diff-in-Disc Sample (2)	Out of Diff-in-Disc Sample (3)
Math Scores	0.0132	0.429	-0.0143
	(0.363)	(0.269)	(0.351)
Reading Scores	0.0428	0.438	0.0167
	(0.331)	(0.250)	(0.318)
Enrollment	43456.8	3888.5	47082.9
	(86565.3)	(5753.0)	(89558.5)
Total Schools	62.17	6.219	67.29
	(128.6)	(6.679)	(133.1)
Total Revenues	14544.4	18004.8	14227.3
	(4914.2)	(7067.8)	(4537.4)
Federal Revenues	1070.4	589.3	1114.5
	(726.1)	(826.2)	(699.9)
Fed. Rev. with ESSER	1085.8	588.4	1131.4
	(753.9)	(822.3)	(730.5)
Teachers PP	0.0618	0.0710	0.0610
	(0.0142)	(0.0191)	(0.0133)
Enr. Share White	0.493	0.778	0.467
	(0.288)	(0.169)	(0.282)
Formula Children	9502.9	221.4	10353.5
	(23317.3)	(296.6)	(24184.8)
Pop Ages 5 to 17	49306.7	4069.1	53452.3
	(100347.1)	(5965.3)	(103848.4)

Note: This table shows summary statistics for select variables measured pre-COVID as averages between SY 2017 and SY 2019. Column 1 shows all public school districts, Column 2 shows districts within the 50 formula child bandwidth sample, and Column 3 shows districts not in the 50 formula child bandwidth sample. Means are presented with standard deviations in parentheses. We weight calculations by SY 2020 enrollments.

Table 5: First Stage Regression Discontinuity Estimates of ESSER Funding at the 5% Poverty Qualification Cutoff

	45		50		55	
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification Dummy	390.0***	467.1***	388.2***	452.4***	358.6***	474.7***
	(71.57)	(112.7)	(66.27)	(109.0)	(62.99)	(105.8)
First-Degree Polynomial	Y		Y		Y	
Second-Degree Polynomial		Y		Y		Y
Observations	4413	4413	4696	4696	4986	4986
R2	0.0605	0.0606	0.0669	0.0670	0.0770	0.0775
F-Statistic	29.69	17.17	34.31	17.24	32.41	20.13
Below Mean	816.9	816.9	808.4	808.4	798.6	798.6

Note: This table shows estimates of Equation 4 as described in the text. F-statistics for the qualification dummy and below-cutoff means are presented at the bottom of the table. We show robust standard errors in parentheses. We weight estimates by SY 2020 enrollments.

* p<0.05;** p<0.01; *** p<0.001

Table 6: Test for Pre-Treatment Differences at the 5% Poverty Qualification Cutoff Before COVID in Trends

	Panel A: First-Degree Polynomial									
	Math	Reading	Absent-eeism	Tot. Rev	Fed. Rev	Teachers PP	Staff PP	Enroll-ment	Share White	Share Black
Band	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
45	0.00491	0.0122	31.65	267.5	6.494	-0.0000453	-0.00165	-146.1	0.0105*	-0.00230
	(0.0180)	(0.0165)	(23.64)	(253.8)	(38.06)	(0.000667)	(0.00186)	(171.1)	(0.00435)	(0.00121)
50	-0.00462	0.00702	18.51	213.4	0.0943	-0.000841	-0.00134	-137.8	0.00941*	-0.00181
	(0.0170)	(0.0161)	(21.49)	(226.2)	(35.79)	(0.000642)	(0.00166)	(167.1)	(0.00413)	(0.00102)
55	-0.00134	0.00894	15.73	241.6	-2.493	-0.00103	-0.000866	-132.2	0.00896*	-0.00153
	(0.0164)	(0.0154)	(20.16)	(208.4)	(33.90)	(0.000624)	(0.00156)	(162.9)	(0.00401)	(0.000956)
	Panel B: Second-Degree Polynomial									
	Math	Reading	Absent-eeism	Tot. Rev	Fed. Rev	Teachers PP	Staff PP	Enroll-ment	Share White	Share Black
Band	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
45	0.0166	0.0455*	44.19	161.7	1.247	0.0000974	0.000876	63.41	0.00549	0.000450
	(0.0251)	(0.0221)	(43.34)	(395.8)	(47.57)	(0.000911)	(0.00247)	(177.4)	(0.00457)	(0.00142)
50	0.0241	0.0426*	53.06	253.9	10.49	0.000976	-0.0000628	-3.560	0.00815	-0.000877
	(0.0237)	(0.0207)	(40.54)	(344.3)	(47.22)	(0.000910)	(0.00238)	(173.4)	(0.00458)	(0.00120)
55	0.0128	0.0311	49.24	191.3	12.31	0.000691	-0.000994	-39.97	0.00885	-0.00134
	(0.0232)	(0.0198)	(38.81)	(325.2)	(46.95)	(0.000878)	(0.00237)	(170.4)	(0.00462)	(0.00127)

Note: Note: This table shows estimates for the coefficient of the qualification dummy in Equation 4 as described in the text for covariate pre-COVID trends. These coefficients are shown for the 45, 50, and 55 formula child bandwidth samples as well as first-degree polynomial estimates in Panel A and second-degree polynomial estimates in Panel B. We show robust standard errors in parentheses. We weight estimates by SY 2020 enrollments.

* p<0.05;** p<0.01; *** p<0.001

Table 7: Test for Covariate Smoothness in Levels at the 5% Poverty Qualification Cutoff Before COVID in Levels

Band	Math (1)	Reading (2)	Absent-eeism (3)	Tot. Rev (4)	Fed. Rev (5)	Teachers PP (6)	Staff PP (7)	Enroll-ment (8)	Share White (9)	Share Black (10)
Panel A: First-Degree Polynomial										
45	-0.0480	-0.0151	-284.0	787.4	154.8***	0.00714***	0.0115*	-4170.7*	0.0592*	-0.00519
	(0.0392)	(0.0389)	(151.9)	(879.4)	(46.64)	(0.00212)	(0.00525)	(2007.7)	(0.0258)	(0.00571)
50	-0.0430	-0.0177	-207.1	160.1	134.8**	0.00544**	0.00941	-3532.0	0.0535*	-0.00273
	(0.0367)	(0.0364)	(153.5)	(838.9)	(42.80)	(0.00204)	(0.00501)	(1996.3)	(0.0245)	(0.00528)
55	-0.0305	-0.00686	-169.1	349.0	126.4**	0.00550**	0.00816	-3223.5	0.0502*	-0.00299
	(0.0354)	(0.0350)	(155.5)	(808.8)	(40.03)	(0.00196)	(0.00485)	(1975.2)	(0.0238)	(0.00517)
Panel B: Second-Degree Polynomial										
45	-0.0437	-0.0205	-213.0	1440.9	76.63	0.00857**	0.0137	-1709.0	0.0259	0.000519
	(0.0621)	(0.0636)	(153.1)	(1229.5)	(80.51)	(0.00316)	(0.00787)	(1224.2)	(0.0306)	(0.00821)
50	-0.0515	-0.0171	-328.0*	1999.5	123.3	0.0102***	0.0157*	-3154.9*	0.0417	-0.00387
	(0.0578)	(0.0592)	(150.1)	(1158.3)	(76.36)	(0.00296)	(0.00740)	(1328.7)	(0.0285)	(0.00754)
55	-0.0653	-0.0311	-338.9*	1344.2	134.8	0.00901**	0.0160*	-3540.0*	0.0490	-0.00289
	(0.0554)	(0.0563)	(142.1)	(1127.4)	(72.23)	(0.00284)	(0.00707)	(1421.2)	(0.0282)	(0.00728)

Note: Note: This table shows estimates for the coefficient of the qualification dummy in Equation 4 as described in the text for covariates in levels. These coefficients are shown for the 45, 50, and 55 formula child bandwidth samples as well as first-degree polynomial estimates in Panel A and second-degree polynomial estimates in Panel B. We show robust standard errors in parentheses. We weight estimates by SY 2020 enrollments.

* p<0.05;** p<0.01; *** p<0.001

Table 8: Estimates of the Impact of Qualifying for Additional ESSER Funds on Measures of School District Performance

	Math			Reading			Enrollment			Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Qualification · I{2023}	-0.0389	-0.0234	-0.0233	-0.0227	-0.00356	-0.00459	89.22	62.44	28.90			
	(0.0221)	(0.0213)	(0.0205)	(0.0224)	(0.0214)	(0.0202)	(80.85)	(78.61)	(77.12)			
Qualification · I{2022}	-0.00305	0.00283	0.00499	-0.0322	-0.0166	-0.0131	119.5*	95.55	70.16	-457.9	-381.1	-369.4
	(0.0234)	(0.0225)	(0.0217)	(0.0176)	(0.0170)	(0.0162)	(54.92)	(52.21)	(50.67)	(283.7)	(283.7)	(281.3)
Qualification · I{2021}							150.5*	124.8*	99.58	183.3	120.2	103.3
							(60.46)	(58.96)	(58.33)	(163.7)	(164.7)	(165.9)
Qualification · I{2020}										51.87	41.43	42.33
										(78.29)	(81.40)	(84.41)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5991	6547	7036	5427	5962	6459	30751	32732	34762	24311	25910	27521
R2	0.914	0.915	0.916	0.907	0.908	0.912	0.997	0.997	0.997	0.685	0.685	0.684
Band	45	50	55	45	50	55	45	50	55	45	50	55
Combined Coefficient	-0.0419	-0.0206	-0.0183	-0.0550	-0.0202	-0.0177	359.1	282.8	198.6	-222.7	-219.5	-223.8
	(0.0428)	(0.0413)	(0.0397)	(0.0375)	(0.0361)	(0.0341)	(138.2)	(130.7)	(127.8)	(91.27)	(103.0)	(116.0)
Pre-COVID Mean	0.434	0.429	0.420	0.445	0.438	0.430	4008.3	3902.8	3810.3	344.6	337.2	330.3

Note: This table shows estimates of Equation 3b as described in the text for math scores, reading scores, enrollment counts, and absenteeism counts. We present estimates for three bandwidths, 45, 50, and 55 formula children from the 5% poverty threshold using a first-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Table 9: Estimates of the Impact of Qualifying for Additional ESSER Funds on Enrollments by Grade Level Groups

	Total	PK	K	1-5	6-8	9-12
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification · I{2023}	62.44	-36.86	15.88	29.98	39.23*	9.246
	(78.61)	(32.82)	(12.35)	(27.94)	(19.15)	(45.74)
Qualification · I{2022}	95.55	-32.10	22.39*	59.77*	33.96*	9.501
	(52.21)	(34.37)	(10.46)	(24.12)	(15.32)	(31.78)
Qualification · I{2021}	124.8*	4.081	34.14	62.48	25.37*	8.281
	(58.96)	(6.203)	(21.80)	(39.80)	(10.40)	(16.67)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
Observations	32732	22010	30800	31192	30870	22939
R2	0.997	0.930	0.994	0.997	0.997	0.997
Band	50	50	50	50	50	50
Combined Coefficient	282.81	-64.87	72.41	152.22	98.56	27.03
	(130.7)	(66.9)	(43.3)	(63.67)	(42.00)	(92.95)
Pre-COVID Mean	3,902.8	98.73	277.6	1,450.0	943.1	1,490.7

Note: This table shows estimates of Equation 3b as described in the text for total enrollments, pre-kindergarten, kindergarten, grades 1st through 5th, grades 6th through 8th, and grades 9th through 12th as counts. We present estimates for 50 formula children from the 5% poverty threshold. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Table 10: Estimates of the Impact of Qualifying for Additional ESSER Funds on Year Over Year Cellphone Mobility Changes from 2019 Bassline

	Dep. Var: Mobility Change from 2019 Baseline					
	2020		2021		2022	
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification Dummy	0.0530***	0.0354	0.0897***	0.0624*	0.108***	0.0761*
	(0.0146)	(0.0215)	(0.0212)	(0.0294)	(0.0262)	(0.0384)
First-Degree Polynomial	Y		Y		Y	
Second-Degree Polynomial		Y		Y		Y
Observations	2990	2990	2975	2975	2915	2915
R2	0.0416	0.0430	0.0868	0.0885	0.100	0.101
Band	50	50	50	50	50	50
Control Mean	-0.307	-0.307	-0.263	-0.263	-0.209	-0.209

Note: This table shows estimates of Equation 4 as described in the text for year over year cellphone mobility changes compared to 2019. We show first- and second-degree polynomial estimates using a bandwidth of 50 formula children for 2020, 2021, and 2023. Below-cutoff means of the dependent variable are presented at the bottom of the table. We show robust standard errors in parentheses. We weight estimates by SY 2020 enrollments.

* p<0.05;** p<0.01; *** p<0.001

Table 11: Estimates of the Impact of Qualifying for Additional ESSER Funds on Area Average Daily COVID Cases per 100,000 Residents in SY 2021

	Dep. Var: Area Daily Cases per 100k in 2021					
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification Dummy	-0.896	0.274	-1.584	0.772	-1.539	0.190
	(0.888)	(1.317)	(0.834)	(1.260)	(0.804)	(1.229)
First-Degree Polynomial	Y		Y		Y	
Second-Degree Polynomial		Y		Y		Y
Observations	4412	4412	4695	4695	4983	4983
R2	0.0224	0.0238	0.0169	0.0215	0.0180	0.0206
Band	45	45	50	50	55	55
Control Mean	24.40	24.40	24.25	24.25	24.28	24.28

Note: This table shows estimates of Equation 4 as described in the text for area average daily case rates of COVID-19. We show first- and second-degree polynomial estimates using bandwidths of 45, 50, and 55 formula children for SY 2021. Below-cutoff means of the dependent variable are presented at the bottom of the table. We show robust standard errors in parentheses. We weight estimates by SY 2020 enrollments.

* p<0.05;** p<0.01; *** p<0.001

Table 12: Estimates of the Impact of Qualifying for Additional ESSER Funds on School District Staffing Per Pupil

	Tot. Staff	Teachers	Non-Teachers	Counselors	Inst. Aids	Dist. Admin
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification · I{2023}	0.0002	-0.0003	0.0005	-0.00001	0.0012	0.0007***
	(0.0045)	(0.0010)	(0.0043)	(0.0001)	(0.0010)	(0.0002)
Qualification · I{2022}	0.0031	0.0010	0.0021	0.0001	0.0007	0.0001
	(0.0018)	(0.0006)	(0.0016)	(0.0001)	(0.0006)	(0.0001)
Qualification · I{2021}	0.0006	0.0002	0.0004	-0.0001	0.0002	0.000004
	(0.0016)	(0.0006)	(0.0012)	(0.0001)	(0.0006)	(0.0001)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
Observations	30247	30247	30247	29383	29154	28973
R2	0.768	0.778	0.771	0.748	0.849	0.812
Band	50	50	50	50	50	50
Combined Coefficient	0.0039	0.0009	0.0030	-0.00001	0.0021	0.0008
	(0.0062)	(0.0017)	(0.0057)	(0.0002)	(0.0018)	(0.0003)
Pre-COVID Mean	0.147	0.0713	0.0755	0.00223	0.0194	0.00217

Note: This table shows estimates of Equation 3b as described in the text for teachers per pupil, staff per pupil, staff at schools per pupil, guidance counselors per pupil, instructional aids per pupil, and district administrators per pupil. Per pupil measures use SY 2020 enrollments. We present estimates for 50 formula children from the 5% poverty threshold and a first-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Table 13: Estimates of the Impact of Qualifying for Additional ESSER Funds on School District Revenues and Expenditures Per Pupil

	Panel A: Revenues					
		Sub-Totals			Exhibits	
	Total	Fed.	State	Local	Prop. Tax.	St. Fmla.
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification · I{2022}	-357.9	89.44	52.17	-499.5**	-390.8**	54.30
	(214.3)	(50.88)	(122.3)	(168.8)	(125.0)	(65.62)
Qualification · I{2021}	-182.1	148.8**	-29.94	-300.9*	-382.8***	117.6
	(245.5)	(45.80)	(207.5)	(146.8)	(115.9)	(63.44)
Qualification · I{2020}	-3.504	23.16	80.12	-106.8	55.14	15.39
	(173.5)	(33.03)	(82.42)	(148.4)	(85.58)	(44.38)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
Observations	28056	28056	28056	28056	26021	28056
R2	0.898	0.862	0.820	0.940	0.955	0.913
Band	50	50	50	50	50	50
Combined Coefficient	-543.5	261.3	102.36	-907.2	-718.4	187.3
	(486.1)	(108.0)	(308.6)	(373.9)	(243.5)	(138.6)
Pre-COVID Mean	18,015.1	589.0	6,282.6	11,143.4	8,726.1	3,908.6
	Panel B: Expenditures					
	Total	ELSEC.	Capital	Instruct.	Salaries	Benefits
	(1)	(2)	(3)	(4)	(5)	(6)
Qualification · I{2022}	192.3	85.67	7.600	87.54	102.3	-48.80
	(555.6)	(138.0)	(505.8)	(87.46)	(72.04)	(61.17)
Qualification · I{2021}	454.0	64.00	266.4	61.75	83.97	-63.07
	(463.4)	(208.7)	(406.1)	(130.9)	(53.54)	(191.9)
Qualification · I{2020}	99.22	32.61	-92.44	41.99	-3.243	70.90
	(369.3)	(95.81)	(310.0)	(56.82)	(51.84)	(40.04)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y
Observations	28056	28056	28056	28056	28056	28056
R2	0.829	0.956	0.346	0.956	0.973	0.929
Band	50	50	50	50	50	50
Combined Coefficient	745.5	182.3	181.6	191.3	183.0	-41.0
	(1,217.2)	(317.7)	(1,112.5)	(196.1)	(147.1)	(214.2)
Pre-COVID Mean	17,992.8	14,887.4	1,752.8	9,125.8	8,438.2	3,859.4

Note: This table shows estimates of Equation 3b as described in the text for revenues in Panel A and expenditures in Panel B. In Panel A we show estimates for total revenues per pupil, federal revenues per pupil, state revenues per pupil, local (own source) revenues per pupil, property tax revenues per pupil and state formula revenues per pupil. In Panel B, we show total expenditures per pupil, elementary and secondary education expenditures per pupil, capital outlays per pupil, expenditures on instruction (including compensation) per pupil, salaries for all employees per pupil,

and benefits for all employees per pupil. Per pupil measures use SY 2020 enrollments. We present estimates for 50 formula children from the 5% poverty threshold and a first-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Table 14: Estimates of the Impacts of Qualifying for Additional ESSER Funds on School District Debts and Assets

						Asset Types		
	Tot. Rev.	Tot. Exp.	Tot. Debt	Debt. Interest	Tot. Assets	Sinking Fund	Bond Fund	Other Assets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Qual. · I{2022}	-357.9	192.3	969.0	78.12	-717.6	-227.0	-271.2	-219.4
	(214.3)	(555.6)	(911.9)	(98.55)	(778.7)	(148.6)	(676.9)	(313.0)
Qual. · I{2021}	-182.1	454.0	129.8	23.53	-816.1	-156.8	-718.2	58.79
	(245.5)	(463.4)	(764.3)	(40.62)	(716.1)	(166.9)	(638.8)	(279.3)
Qual. · I{2020}	-3.504	99.22	710.6	102.9	-327.7	-182.7	-186.4	41.40
	(173.5)	(369.3)	(628.4)	(80.95)	(597.9)	(155.3)	(584.8)	(262.3)
First-Degree Polynomial	Y	Y	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y
Observations	28,056	28,056	28,056	28,056	28,056	28,056	28,056	28,056
R2	0.898	0.829	0.846	0.749	0.714	0.652	0.443	0.866
Band	50	50	50	50	50	50	50	50
Combined Coefficient	-543.51	745.45	1,809.39	204.54	-1,861.41	-566.45	-1,175.74	-119.21
	(486.14)	(1,217.20)	-2,064.37	(185.12)	(1,835.18)	(419.29)	(1,688.49)	(601.33)
Pre-COVID Mean	18,015.10	17,992.80	11,488.10	457.60	7497.60	561.00	1740.90	5195.70

Note: This table shows estimates of Equation 3b as described in the text for total revenues per pupil, total expenditure per pupil, total outstanding debt per pupil, interest payments on debt per pupil, total assets per pupil, sinking fund assets per pupil, bond fund assets per pupil, and other assets per pupil. Total debt is the face value (par value) of outstanding debts of all maturities at the end of the fiscal year. Per pupil measures use SY 2020 enrollments. We present estimates for 50 formula children from the 5% poverty threshold and a first-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Table 15: Estimates of the Impacts of Qualifying for Additional ESSER Funds on Total Compensation Per Employee by Type

	Tot. Staff	Teachers	Non-Teachers
	(1)	(2)	(3)
Qualification · I{2022}	-193.70	-173.80	-1,363.10
	(1,232)	(1,260)	(3,990)
Qualification · I{2021}	3,989.40	3,178.80	7,979.20*
	(3,003)	(3,876)	(4,006)
Qualification · I{2020}	2,097.90*	1,635.90	6,065.30
	(948)	(917)	(3,280)
First-Degree Polynomial	Y	Y	Y
LEA FEs	Y	Y	Y
Year FEs	Y	Y	Y
Observations	27,978	27,854	27,749
R2	0.876	0.873	0.715
Band	50	50	50
Combined Coefficient	5,894	4,641	12,682
	(3,971)	(4,836)	(9,364)
Pre-COVID Mean	84,813	100,243	75,133

Note: This table shows estimates of Equation 3b as described in the text for total compensation per employee for three employee categories. Total compensation includes both benefits and salaries. Column 1 presents estimates for all staff, then Column 2 presents estimates for teachers, and finally Column 3 presents estimates for non-teachers. All columns use both compensation and staff counts that update in each year. These staff counts are the same full time equivalent counts used above on a per pupil bases. We present estimates for 50 formula children from the 5% poverty threshold and a first-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001

Appendix Tables

Table A1: Variable Descriptions

Variable	Description	Measurement period	Source
ESSER Funds Per Pupil	Real funds from ESSER I, II, and III per Fall 2019 enrollee. Top 0.01% of observations have been trimmed	2020-2021	DOE Transparency API
ESSER Funds Per Pupil Series (for event study first stage)	ESSER Funds, which are added as a lump sum in SY 2021, appended to the federal revenue series with reported ESSER funds from the special exhibits removed.	End of year.	CCD, DOE API
Qualification Dummy	Takes a value of 1 if a district in SY 2019 has a poverty share above 5%, which is calculated using data from the SAIPE estimates of kids in poverty and all kids in the district area (not necessarily enrolled). Kids in poverty are formula kids.	2018	SAIPE
Centered Formula Child Count	The number of formula children in a district (from SAIPE) subtracted from that district's normalized 5% poverty threshold. The threshold is the number of formula kids that would constitute a 5% share of their total district school age pop. These are measured in 2017.	Fall 2017	SAIPE
Enrollment (and detailed enrollment)	Counts of children enrolled (not the same as school aged pop)	Fall of each school year	CCD
Absenteeism	Counts of students determined to be chronically absent (DOE definition)	End of school year	DOE
Teachers PP	Full time equivalent teacher count per enrollee (both current)	Fall of each school year	CCD
Total Staff PP	Full time equivalent staff count per enrollee (both current)	Fall of each school year	CCD
Spending PP Vars	Real dollars per enrollee (both current values)	End of school year	CCD
Covid Cases in Schools (Total, Staff)	Cumulative count of cases in school (total and amongst staff)	Throughout the school year	Covid School Data Hub (Oster)

Covid Cases from Area (Zip)	Area Covid Case Rate per 100k, attached by district HQ zip code	Throughout the school year	Covid School Data Hub (Oster)
Movement Changes	Cell phone movement change compared to same period in 2019. Expressed as -0.05 is -5%	Throughout the school year	Parolin and Lee (2021)
Fixed Spending PP Vars	Real dollars per enrollee Fall 2019	End of school year	CCD
White Share	White fraction of students.	Beginning of school year	CCD
Black Share	Black fraction of students.	Beginning of school year	CCD
Federal Revenues PP	All federal revenues, including emergency funds and regular transfers such as Title 1, for each FY. These are divided by the number of pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Revenues PP	All revenues for each FY. These are divided by the number of pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Reading Scores	Average NAEP score for Reading for the entire district for 3-8th grades, scaled in 2019 standard deviations.	Spring of every other school year: 2017, 2019, 2022, 2023 (next would be 2025).	SEDA
Math Scores	Average NAEP score for Math for the entire district for 3-8th grades, scaled in 2019 standard deviations.	Spring of every other school year: 2017, 2019, 2022, 2023 (next would be 2025).	SEDA
State Revenues PP	All state revenues, including formula assistance and other state funds, divided by pupils in SY 2020 (Fall 2019). These do not include funds passed through the state, such as Title 1.	End of the school year	CCD
State Formula Funds PP	State revenues from funding formulas (e.g. local control funding in CA) divided by pupil in SY 2020 (Fall 2019).	End of the school year	CCD
Local Revenues	All local revenues, including from parcel, sales, property, and other taxes and revenue sources, divided by pupils in SY 2020 (Fall 2019)	End of the school year	CCD

Property Tax Revenues	Local revenues from property taxes divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Total Expenditures	Total current expenditures divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Current Expenditures on Elementary and Secondary Education	Total expenditures on K-12 education divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Capital Outlays	Total capital outlays for all types of capital, including buildings, divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Instruction Expenditures	Expenditures on classroom instruction, including salaries and supplies, divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Salaries	Expenditures on salaries for all employees divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Benefits	Expenditures on Benefits for all employees divided by pupils in SY 2020 (Fall 2019).	End of the school year	CCD
Total Schools	Total count of schools in the district with at least one enrolled student.	Beginning of school year	CCD
High Free and Reduced Priced Lunch Schools	Count of schools in the district with at least one enrolled student and that where above the district median for share of students receiving free and reduced-price lunch.	Beginning of school year	CCD
HHI for Free and Reduced Priced Lunch Students	The Herfindahl index for the distribution of students receiving free and reduced priced lunch across district schools with at least one enrolled student. A value of 1 means all FRPL students attend one school.	Beginning of school year	CCD
HHI for All Students	The Herfindahl index for the distribution of all students across district schools with at least one enrolled student. A value of 1	Beginning of school year	CCD

	means all students attend one school.		
Any Tax Powers	Districts that have any power to raise or lower taxes of any type as of 2018.	2018	Verstegen (2018)
Property Tax Powers	Districts that have the power to raise or lower property taxes in any way as of 2018.	2018	Verstegen (2018)
Property Tax Increases or Decreases	State property tax changes. For increases, states that have any action that increases property taxes gets coded as a 1. For Decreases, states that have any action that decreases property taxes gets coded as a 1. These are not mutually exclusive.	Throughout the school year	NCSL
School Staff PP	Count of full-time equivalent staff that work at the schools in district divided by pupils in SY 2020.	Fall of each school year	CCD
Guidance Counselors PP	Count of full-time equivalent guidance counselors divided by pupils in SY 2020.	Fall of each school year	CCD
Instructional Aides PP	Count of full time equivalent instructional aids divided by pupils in SY 2020.	Fall of each school year	CCD
District Admin per pupil	Count of full-time equivalent district administrative staff divided by pupils in SY 2020.	Fall of each school year	CCD

Table A2: Robustness of Estimates of the Impact of Qualifying for Additional ESSER Funds on Measures of School District Performance to Second-Degree Polynomials

	Math			Reading			Enrollment			Absenteeism		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Qualification · I{2023}	-0.0424	-0.0591	-0.0532	-0.0541	-0.0691*	-0.0545	101.1	129.0	152.7			
	(0.0324)	(0.0312)	(0.0298)	(0.0338)	(0.0317)	(0.0301)	(104.2)	(97.61)	(92.56)			
Qualification · I{2022}	-0.0247	-0.0259	-0.0249	-0.0547*	-0.0667**	-0.0617**	87.55	125.4	147.3*	-165.1	-336.7*	-349.1*
	(0.0293)	(0.0282)	(0.0275)	(0.0255)	(0.0244)	(0.0232)	(84.38)	(77.13)	(72.16)	(100.3)	(139.1)	(165.6)
Qualification · I{2021}							58.65	114.7*	144.1**	79.76	186.9	186.4
							(51.84)	(53.10)	(54.98)	(145.0)	(147.6)	(143.7)
Qualification · I{2020}										96.92	102.1	84.96
										(96.07)	(89.62)	(80.06)
Second-Degree Polynomial	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	5991	6547	7036	5427	5962	6459	30751	32732	34762	24311	25910	27521
R2	0.914	0.915	0.916	0.908	0.908	0.912	0.997	0.997	0.997	0.691	0.689	0.689
Band	45	50	55	45	50	55	45	50	55	45	50	55
Combined Coefficient	-0.0672	-0.0850	-0.0781	-0.1088	-0.1358	-0.1162	247.34	369.08	444.11	11.54	-47.67	-77.77
	(0.0590)	(0.0565)	(0.0546)	(0.0563)	(0.0531)	(0.0505)	(220.89)	(203.95)	(191.51)	(226.16)	(202.79)	(172.91)
Pre-COVID Mean	0.434	0.429	0.420	0.445	0.438	0.430	4008.3	3902.8	3810.3	344.6	337.2	330.3

Note: This table shows estimates of Equation 3b as described in the text for math scores, reading scores, enrollment counts, and absenteeism counts. We present estimates for three bandwidths, 45, 50, and 55 formula children from the 5% poverty threshold using a second-degree polynomial specification. We present a linear combinations of all post COVID coefficients as well as the SY 2017 to SY 2019 pre-COVID mean for all districts of the dependent variable at the bottom of the table. Regressions are weighted by SY 2020 enrollments and standard errors clustered by district are shown in parentheses.

* p<0.05;** p<0.01; *** p<0.001