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INTERGOVERNMENTAL GRANTS TO SCHOOL DISTRICTS AND EDUCATIONAL OUTCOMES DURING THE COVID-19 PANDEMIC

Jeffrey Clemens
Philip G. Hoxie
Stan Veuger

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ABSTRACT

The federal government appropriated $189.5 billion in pandemic aid to school districts through the Elementary and Secondary School Emergency Relief (ESSER) fund. We evaluate their impact on schools with district poverty shares near 5%, where a threshold-driven increase in funds enables us to implement a difference-in-discontinuities design. Federal funds were passed on to residents through reductions in local revenue collections, including property taxes, and did not result in increased per-pupil expenditures by school districts. We find no evidence that additional funds mitigated the declines in test scores. We provide suggestive evidence that political engagement, especially from parents, increased in districts that qualified for additional funds. This combination of engagement and reduced taxation may explain the greater enrollment and faster reopening we observe in such districts.

Jeffrey Clemens
University of California, San Diego
Department of Economics
and NBER
jeffclemens@ucsd.edu

Philip G. Hoxie
University of California, San Diego
Department of Economics
phoxie@ucsd.edu

Stan Veuger
American Enterprise Institute
stan.veuger@aei.org

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 faced the challenge of balancing students' educational achievement and the potential for disease transmission associated with different learning modalities (in-person, virtual, or hybrid) (Gordon and Reber, 2020; Halloran et al., 2021, 2023; Oster et al., 2021). In Figure 1, we show changes in test scores for math and reading by school district poverty share. The patterns reflect widespread learning loss in districts across the poverty spectrum.

To address these and other challenges, Congress appropriated almost $1 trillion in emergency funding in the form of intergovernmental transfers to sub-national governments, with about $189.5 billion allocated for elementary and secondary education (Clemens and Veuger, 2021b; 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 received appropriations 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 conducts an empirical investigation into how effective these funds were in helping schools weather the pandemic, using traditional and pandemic-specific measures of school performance as well as budgetary outcome metrics.

We use a difference-in-discontinuities design that exploits a threshold in the Title 1-A funding formulas used to distribute ESSER funds to school districts. This discontinuity allows us to identify the impacts of pandemic-era federal funds on school district outcomes local to our policy discontinuity at the 5% policy threshold. ESSER funds were allocated to states using the pre-existing Title 1-A funding formulas. State education agencies (SEAs) then distributed the funds to school districts (LEAs) using these same formulas (Office of Elementary and Secondary Education, 2022, 2023). We use a statutory jump in the Title 1-A formulas to identify a $388 per-pupil increase in ESSER funds for school districts with more than 5% of children ages 5–17 in poverty (Congressional Research Service, 2017). This cutoff is determined using pre-COVID school district demographics, namely the 2018 vintage of the US Census Bureau's Small Area Income and Poverty Estimates (SAIPE). Therefore, the additional ESSER funds distributed to districts that are just on either side of the 5% poverty threshold are plausibly exogenous to pandemic-related outcomes. We then use the 5% poverty share discontinuity to evaluate whether

ESSER funds explain differences in traditional performance measures such as test scores, enrollment, and absenteeism; pandemic outcomes like reopening speed and area cases; staffing categories; and district budgets.

We find that additional ESSER funds did not increase either math or reading test scores in districts just above the 5% poverty qualification cutoff. While our point estimates are negative, they are insufficiently precise to rule out meaningful increases. That said, ESSER funds may have helped districts retain students and reduce absenteeism. We also find that ESSER funds may have helped schools reopen faster, as suggested by cellphone mobility data.[1] In our analysis of staffing, we show that additional ESSER funds may have led to increases in the number of administrators, but not in the number of teachers or overall non-teacher staff. Our set of estimates on expenditures is generally too imprecise to draw strong conclusions, and there is no evidence of detectable increases across broad expenditure categories as a result of qualifying for additional funds.

Our analysis of revenues sheds light on the primary reason why additional ESSER funds had no detectable impact on expenditure. We find evidence of statistically and economically large reductions in districts' revenue from local sources in response to receipt of additional ESSER funds. These results contradict the older literature on the "flypaper effect," which suggests that windfall revenues for subnational governments tend not to be offset by reductions in own-source revenue (Gramlich, 1977; Hines and Thaler, 1995; Fisher, 1982). They are complementary, however, to research from Gordon (2004), who finds that declines in local revenues offset increases in federal grants over a horizon of two years following census-driven updates to school districts' funding allocations. We emphasize that this passthrough of intergovernmental transfers to local taxpayers can explain the at most modest impacts we find on students' test scores.

The policy bundle of decreased local revenues, accelerated reopening, and stable expenditures can be rationalized by increased political engagement in districts that qualified for additional ESSER funds. We provide evidence that, in these districts, local meetings likely had

[1] Additionally, we find no evidence that reopening was correlated with additional spread of COVID-19, the primary justification for relatively slow rates of reopening. 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.

higher attendance and parents made up a larger share of voters than in districts that did not qualify for additional funds. As in the theory of collective budgets from Bradford and Oates (1971a, 1971b), citizens preferences were reflected in the passthrough of a one-time lump-sum grant.

Our findings 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 grants to state and local governments to support the provision of public services, economic activity, and pandemic mitigation efforts (Clemens, Hoxie, and Veuger, 2025; 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, reopening, student outcomes, and district budgets.

The second literature to which we contribute is the growing body of work analyzing specific issues surrounding education during the COVID-19 pandemic.[2] 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 returned to in-person learning later than White students. Halloran et al. (2021), Halloran et al. (2023), and Goldhaber et al. (2023) evaluate how learning modalities and other pandemic policies impacted test scores. Fahle et al. (2023) demonstrate 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) find that tutoring and other learning interventions had little impact on students' scores in four large districts.

There is also a growing literature on ESSER funds. Dewey et al. (2025) provide evidence suggesting that additional ESSER funds are correlated with academic recovery across school districts. However, ESSER funds themselves are derived from Title I funds, which are strongly correlated with district area poverty (Gordon and Reber, 2023). To address this, Dewey et al.

[2] This literature is also related to the traditional school quality literature (Betts, 1995; Card and Krueger, 1992; Hanushek, 1986; Jackson et al., 2016; Rivkin et al., 2005).

(2024) use random survey error in a district's SAIPE estimates in 2018, the survey used to apportion Title I funds and ESSER, as an instrument. When deploying this more plausibly causal research design, they find no impact of ESSER funds on test score recovery in either math or reading. Having implemented a plausibly causal design and estimated null effects, Dewey et al. (2024) nonetheless place greater emphasis on their OLS estimates, despite clear concerns around endogeneity and omitted-variable bias.[3] The non-SAIPE error instrumental-variables estimates in Dewey et al. (2024) use Title I funding shares as instruments for ESSER, which are highly correlated with district poverty (Gordon and Reber, 2023). As poverty is correlated with a wide variety of factors that influence educational outcomes, in the pandemic or otherwise, this identifying variation is strongly correlated with other baseline differences across school districts.

A related analysis by Goldhaber and Falken (2026) similarly uses a district's 2018 area poverty share as an instrument for ESSER funds in the cross section and reports meaningful effect sizes. This instrument again raises strong concerns regarding the exclusion restriction. Additionally, when Goldhaber and Falken (2026) include state fixed effects in their specifications, their point estimates declines by 60 to 90 percent in magnitude, reinforcing the aforementioned identification concerns and revealing significant sensitivity across specifications.

An important takeaway from these analyses is that the relatively strongly positive point estimates (i.e., of ESSER funds on achievement) in the existing literature are identified using variation that is strongly correlated with district poverty measures and are thus exposed to substantial endogeneity concerns. The more plausibly causal estimates presented in Dewey et al. (2024) and in our own analysis tend towards zero.

Although our difference-in-discontinuities results recover a local estimate, we contribute to this literature by providing credibly causal evidence, applicable to the relatively small and affluent districts near the 5% poverty share threshold, that incremental federal funds may have supported faster rates of reopening and improved rates of student retention but likely did not cause detectable improvements in test scores. Our difference-in-discontinuities design isolates changes that occur in otherwise similar districts just above and just below the Title 1-A qualification threshold for additional funds. By including district and time fixed effects, we net

[3] One important source of endogeneity arises from Dewey et al.'s (2024) use of spent ESSER funds instead of the total ESSER funds allocated to a district. For example, districts with higher value uses for the funds may be more likely to spend their allocation than other districts.

out baseline differences across these districts generated by the Title 1-A funds. Our estimates suggest that at least for relatively smaller and more affluent districts, this may have been because school boards elected to reduce property tax collections for their voters.

We also contribute to the large literature on the flypaper effect.[4] 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 may in fact be no flypaper effect at all in some cases. Lutz (2010) documents a roughly dollar-for-dollar decrease in local taxes in response to state education 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 passed on to local residents through reductions in local revenue collections, in particular through reductions in local property tax collections. If districts around the 5% poverty threshold tended to use ESSER funds to cut (or not increase) local revenue instead of increasing instructional expenditures, this may at least partially explain why meaningful increases in educational achievement are difficult to detect in our setting.

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 of 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. The Title 1 program, authorized by the Elementary and Secondary Education Act, is the largest federal program targeting local school

[4] The early theoretical foundations of this literature can be found in Bradford and Oates (1971a and 1971b).

districts, and it funnels most of its appropriations through the Part A funding formulas (Congressional Research Service, 2017; Skinner, 2016). These four formulas disperse funds across school districts and generally send more funds to districts that serve higher-need populations (Congressional Research Service, 2017; Skinner, 2016; Gordon and Reber, 2020; Gordon and Reber, 2023). Each formula allocates money to individual districts, and the sum of the funds constitutes a district's total amount of Title 1-A funding.[5] During the pandemic, Congress used the Title 1-A funding formulas to deliver emergency aid to school districts, leading to an increase in the share of funding to schools from federal sources. [6]

The four 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 allocates 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 assessed to be living in impoverished families, in foster care, or who benefit from Temporary Assistance for Needy Families (TANF) (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

[5] For a more complete overview of the funding formulas, see Skinner (2016), Congressional Research Service (2017), and Gordon and Reber (2023).
[6] We show trends of school district revenues by source in Panel A of Appendix Figure 1 and per pupil revenues for SY 2019 in Appendix Table 1.

boundaries.[7] These inputs are estimated with 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, $StateFactors_s$, considers a state's own expenditures on education three years prior, equity in funding across school districts, grant-specific state-level funding minima, and other grant-specific factors (Gordon and Reber, 2023). Finally, each formula has a hold-harmless provision, which takes into consideration the amount of funds appropriated in the previous year, $Funds_{it-1}^{g}$, to ensure that no district receives a large year-to-year decrease in funding. 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 FY2017, the qualification function requires a district to have at least 10 formula children and a 2% district poverty share (Congressional Research Service, 2017). If a district qualifies for the basic grant, the funding formula takes the number of formula children and scales them using state factors, with each formula child receiving equal weight within a given state. A district that would see its Basic Grant be reduced in a given year 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.

The Concentration Grant, which distributed $1.4B (8.5% of all Title 1-A funds) in FY2017, has two main differences from the Basic Grant (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 the 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 four years.

The other half of Title 1-A funds ($7.6B in FY2017) is 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 complex and rewards districts with either high counts of formula children or high poverty shares (Gordon and Reber, 2023). For the EFIG formula, the state factors include equity in funding across districts as well as state spending

[7] 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.

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 in our difference-in-discontinuities research design, as discussed below.

Each year, Title 1-A funds are determined through a multi-step process. First, Congress sets funding levels for each of the four formulas for the year. Then, the Department of Education calculates initial grants to school districts using the most recent SAIPE estimates for geographic school districts. These allocations are then sent to state education agencies (SEAs), which are allowed to reserve a small fraction of their state's Title 1-A funds for approved purposes. The state agencies allocate the remaining funds to their school districts (LEAs) following the same Title 1-A grant formulas, making slight modifications to the allocations to accommodate charter schools (Skinner, 2016).[8]

2.2 Funding During the Pandemic

In a typical recession, the budgets of state and local governments come under stress due to borrowing constraints and balanced budget requirements. These constraints make federal aid to sub-national governments an important policy tool during economic downturns (Clemens and Veuger, 2021a; Congressional Research Service, 2009). In pre-pandemic recessions, federal aid to K-12 public schools was included in spending packages such as the American Recovery and Reinvestment Act. However, the pandemic-era Elementary and Secondary School Emergency Relief Fund (ESSER) is unique in both the amount of funding Congress dedicated to school districts and in Congress' use of the existing Title 1-A formulas to determine funding levels (Gordon and Reber 2020).[9]

Between March 2020 and March 2021, Congress appropriated a total of about $190 billion to the ESSER program in the CARES Act, the Coronavirus Response and Relief Supplemental Appropriations Act (CRRSAA), and the American Rescue Plan (ARP). These funds were first given to state education agencies (SEAs) in proportion to their Title 1-A

[8] 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.

[9] 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).

allocations. Then, the states were required to deliver no less than 90% of those funds to local education agencies (LEAs, or school districts) in proportion to each district's share of state Title 1-A funds from FY2020 (Skinner et al., 2022).[10] Both the funding levels and inputs for each of the FY2020 Title 1-A formula allocations were determined before the pandemic, largely by the 2018 SAIPE.[11]

The ESSER program had many goals, largely dictated by the unique strains the pandemic placed on school districts. These included the sudden adoption of remote learning and the subsequent adaptation of district facilities to accommodate returns to in-person learning (Skinner et al., 2022; Dewey et al., 2025; Alon et al., 2020; Alon et al., 2022; Halloran et al., 2021; Halloran et al., 2023; Oster et al., 2021).[12] ESSER funds were also supposed to help districts make up for lost classroom hours due to closures, bolster mental health resources, and mitigate layoffs, among other objectives (Skinner et al., 2022; Bryant et al., 2023).

School districts faced few constraints in how they used ESSER funding, so long as funds were deployed by legislatively specified deadlines. Initially, the $13B in ESSER I funding from the CARES Act, the $54B in ESSER II funding from the CRRSAA, and the remaining $123B in ARP ESSER funding needed to be spent by October 2021, October 2022, and October 2023 respectively. The 2023 deadline was extended by one year to 2024 (Skinner et al., 2022). A 2023 McKinsey survey of 487 school districts 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 ESSER funds on school (district) 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[13] and non-fiscal files for school years 2010 to 2023 (Institute of Education Sciences (IES), 2024b).[14] We attach to these data the annual

[10] ESSER I used Title 1-A funds from FY2019 and ESSER II and ARP ESSER used FY2020 allocations. We use the FY2020 formulas to create our research design since the vast majority of ESSER funds were delivered through ESSER II and ARP ESSER.

[11] In Appendix Table 2, we present a timeline of events relevant for the ESSER program and school districts during COVID-19.

[12] Closures also had an important role in shaping the pandemic's impact on labor markets (Alon et al., 2020; Alon et al., 2022).

[13] We deflate all fiscal variables by the Personal Consumption Expenditures Chain-Type Price Index, which we access through FRED (BEA, 2024).

[14] We access these data using the Urban Institute's (2024) Education Data Explorer where possible.

SAIPE estimates from the US Census Bureau (2023) because formula child count and poverty share estimates are key inputs into the Title 1-A funding formulas. Roughly 12,700 school districts have estimates from the SAIPE and data from the CCD. 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 may lack traditional geographic boundaries, which means we cannot include them in our analysis even though they received federal funds during COVID. We further restrict our panel to districts with non-zero enrollment, and with complete data for total federal revenues, enrollment, and SAIPE poverty estimates between 2017 and 2022.[15]

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 combine several iterations of the API to build a dataset of ESSER allocations that includes \$170B in funds.[16] 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 enrollment, staffing, expenditure, revenue, and poverty estimates, we are 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 scale 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.[17]

Next, we include additional variables to understand how districts performed during the pandemic. First, we add data on chronic absenteeism from Ed Data Express (2024), which the Biden administration identified as a key driver of low test performance during the pandemic

[15] In SY 2019-2020, all districts in California were attributed 0 teachers and staff employed in the CCD files. We have replaced these missing observations with their values from the previous school year. Staffing counts include teachers as a sub-category. For regressions that compare changes in staff and teacher counts, we drop districts that report teacher counts in excess of their total staff counts, as they are likely misreported.
[16] 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 each district's final allocation of ESSER Funds. We pulled the API on 12/1/2022, 10/19/2023, and 2/4/2024.
[17] Naturally, districts that do not enroll 3rd through 8th graders will not show up in the SEDA test score data.

(CEA, 2023). These data count students who are absent for at least 10% of school days. Second, we analyze a cellphone-based mobility measure from Parolin and Lee (2022), which measures district-level declines in cellphone visits around schools for calendar years (CY) 2020, 2021, and 2022 compared to their same month in 2019. We average these data by calendar year for each school district.[18] Finally, we include a measure of community average daily case rates around school districts from the COVID School Data Hub (CSDH). The CSDH apportions average daily county-level COVID cases per 100,000 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. While all districts in sample have CSDH case rate data, only 2,915 districts have complete mobility data.

Our main explanatory variable is ESSER funds per pupil, where the total amount of ESSER funds awarded across all three installments is summed and divided by a district's fall 2019 enrollment total (SY 2020).[19] 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 federal revenues per pupil before the pandemic and then add ESSER funds to create a composite federal funding per pupil measure in 2021.[20]

Given that many variables in our dataset have imperfect coverage, we include districts as data are available. This is why we describe our dataset as an unbalanced panel. Data availability also explains why observation counts differ across presented regressions even when these regressions analyze samples within the same bandwidth from the cutoff. We provide summary statistics for each year of data in our sample in Appendix Table 3 as well as a list of variables, along with descriptions, measurement frequencies, and sources in Appendix Table 4.

4. Methodology

4.1 The Difference-in-Discontinuities Design

[18] 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 through the COVID School Data Hub (CSDH, 2023).
[19] 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.
[20] This also requires using the special CCD exhibits to remove ESSER funds from the post-COVID federal revenues series so as not to double count funds.

ESSER funds are likely endogenous to school district outcomes. District poverty rates are closely related both to educational outcomes and to the allocation of ESSER funds (Hanushek et al., 2019). Figure 1 shows that while the pandemic reduced test scores in both reading and math across the board, the size of the decline was closely linked to district poverty shares. Moreover, the ESSER funding allocation process allowed states some opportunities to change the amount of funds received by districts. States were able to change ESSER allocations by withholding up to 10% of funds for their own use and by approving and making allocations to new charter schools (Skinner and Dortch, 2020; Office of Elementary and Secondary Education, 2020). School districts could also choose not to accept ESSER funds for unobserved reasons. The endogeneity of ESSER funds and omitted variables likely bias a simple OLS regression of outcomes on ESSER funds.

The potential sources of bias associated with a naïve regression of ESSER funds on school district performance measures motivate our use of a discontinuity in the Title 1-A formula as a source of identification within a difference-in-discontinuities research design. Districts that just qualified for additional Title 1-A and ESSER funds and districts that did not were likely to have continued on similar trends in the absence of the additional funds. We exploit this to estimate a local average treatment effect for the impact of ESSER funds on school performance and budget outcome variables for districts around the 5% poverty share threshold.

The difference-in-discontinuities design exploits the qualification functions for the EFIG and Targeted Grants. To qualify for funds from the EFIG and Targeted Grant formulas in FY2020, a district had to have an area poverty rate of at least 5% among children aged 5–17 in the 2018 SAIPE estimates. The ESSER funds were allocated in SY 2020 and SY 2021 to school districts based on the districts' shares of state Title 1-A funds from FY2020. As districts below the 5% cutoff only received ESSER funds in proportion to their Basic and Concentration Grant allocations, whereas districts above it received ESSER funds in proportion to their funds from all four formulas, a discontinuity in ESSER funds should form around the cutoff.

To implement the difference-in-discontinuities design, we first construct our running variable, the distance from the 5% poverty cutoff measured in formula children, which we term "centered formula children":

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

FC_{2018i} is the formula child count, i.e., 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. By multiplying the total number of children aged 5–17 by 0.05, we calculate the 5% poverty cutoff for each school district.

In Figure 2 we show the relationship between our "centered formula children" variable and both pre-pandemic federal funds and ESSER funds per pupil for districts within 100 formula children of the cutoff. In Panel A we see a discontinuity of about $135 per pupil in pre-COVID federal funds at the 5% poverty threshold, where the qualification cutoff is marked with a vertical line. This motivates using the difference-in-discontinuity approach as opposed to a simple regression discontinuity design.[21] In Panels B and C, we see a similarly clear increase in average ESSER funds per pupil right at the threshold, with all districts on display in Panel B and districts binned in Panel C.[22] Here the discontinuity amounts to around $400 per pupil. In Panel D, we show an event study using a series of total federal funds, including ESSER. We can see that the pre-pandemic difference in Title 1-A funds is stable over-time, and we see a strong increase of around $400 per pupil from ESSER funds during the pandemic.

As the 5% poverty threshold existed prior to the pandemic, we analyze the arrival of the ESSER funds using a difference-in-discontinuities design (Grembi et al., 2016). We estimate regressions of the following form, where *i* indexes school districts and *t* indexes time periods:

$$ESSER_{it} = \alpha + \sum_{t \neq Base}[\beta_t(D_i \cdot I\{Year = t\}_t) + \gamma_t(R_i \cdot I\{Year = t\}_t) + \theta_t(D_i \cdot R_i \cdot I\{Year = t\}_t)] + \delta_i + \tau_t + \varepsilon_{it} \tag{3a}$$

$$Y_{it} = \alpha + \sum_{t \neq Base}[\beta_t(D_i \cdot I\{Year = t\}_t) + \gamma_t(R_i \cdot I\{Year = t\}_t) + \theta_t(D_i \cdot R_i \cdot I\{Year = t\}_t)] + \delta_i + \tau_t + \varepsilon_{it}, \tag{3b}$$

Equation 3a is the first-stage regression equation used to identify the amount of ESSER funds per pupil associated with the 5% poverty threshold. Our parameters of interest are the post-

[21] Panels A, C, and D of Figure 2 weight bins by SY 2020 (fall 2019) enrollments.
[22] Following the visual inference recommendations from Korting et al. (2023), we present our discontinuity figures without trend lines.

COVID values of β_t, which capture the impact of crossing the 5% 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. We will also present a combined coefficient in our tables, which is the linear combination of all post-COVID values of β_t. The running variable, R_i, is the centered formula child count as described in Equation 2. We interact the qualification dummy and the running variable with a set of year dummies, $I\{Year = t\}_t$, 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.[23] 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. Finally, ε_i is an error term. We weight these regressions by district enrollment in fall 2019. Our robust standard errors are clustered at the district level since qualification for additional ESSER funds is determined district-by-district (Abadie et al., 2020; Abadie et al., 2023). 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, which is our preferred bandwidth.[24]

Equation 3b is the reduced-form equation. In this equation, we replace the left-hand side variable from Equation 3a, ESSER funds per pupil, with our outcome of interest, Y_{it}. We use equations 3a and 3b to estimate two objects of interest, the intent to treat (ITT) effect of qualifying for additional ESSER funds and the local average treatment effect (LATE) associated with additional ESSER funds. The post-COVID estimates of β_t in equation 3b show the relationship between qualifying for additional ESSER funds and changes in school district outcomes. These are our estimates of the ITT effect. Additionally, we can recover a LATE by scaling the post-COVID β_t estimates in equation 3b by the first-stage estimates from equation

[23] 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.

[24] 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. We provide a map of our 50 formula child sample in Appendix Figure 2.

3a. The LATE gives the impact of additional ESSER funds on district outcomes. However, the LATE requires the assumption that qualifying for additional ESSER funds only impacts district outcomes through receiving ESSER funds.

Where appropriate, we also use a simple cross-sectional regression discontinuity specification. We do this when investigating how ESSER funds jump across the 5% poverty threshold, when estimating COVID outcomes for which there are no pre-COVID data, or for graphing results as changes from their pre-COVID baselines. These regressions take the following form:

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

where *i* indexes school districts. D_i is a dummy for qualifying for additional ESSER funds by having a poverty share over the 5% threshold in the 2018 SAIPE, and R_i is our running variable of centered formula children from the 5% poverty share threshold. Here, we do not interact our right-hand side variables with a series of year dummies; rather, we run these regressions cross-sectionally year-by-year. Outcomes in this specification can be either observations for a specific year or changes from a pre-COVID baseline. Just as in Equations 3a and 3b, we can also include a second-degree polynomial in our running variable. We present robust standard errors for this specification and weight observations by enrollments in SY 2020. ε_i is an error term.

A limitation of our discontinuity-driven designs is that they produce estimates of treatment effects local to the 5% poverty qualification cutoff. In Appendix Table 5, we show how the typical school district in our preferred bandwidth sample, 50 formula children from the cutoff, compares to the universe of school districts from the CCD data. 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 than districts outside of our sample. Naturally, students in our difference-in-discontinuity sample attend schools in districts with an average poverty share of around 5%, 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. First, the discontinuity needs to be associated with a strong jump in ESSER funds per pupil, which we confirm in Figure 2 and Appendix Table 6. Panel A of Figure 2 shows a pre-existing jump in Title 1-A funds across the qualification threshold of about $135 per pupil. This jump then expands to around $400 per pupil, as shown in the un-binned scatterplot in Panel B and the binned scatterplot in Panel C of Figure 2. In Panel D of Figure 2, we present the event-study version of our first-stage results. This figure shows the coefficients on the qualification dummy for additional ESSER funds, interacted with dummies for each year, with combined federal and ESSER funds per pupil as the dependent variable. Here, we leave out SY 2020 as the base year. In the pre-COVID periods, the $135 per pupil gap in federal funding appears to be stable. However, in the post-COVID period, districts that qualified for additional ESSER funds saw a roughly $400 per pupil increase in their combined federal funding, consistent with our estimates from Appendix Table 6.[25] The jump in ESSER funds at the 5% poverty threshold represents a one-time 11% increase in discretionary instructional funding for the typical district in this sample.

The second assumption is that districts close to and on both sides of the 5% poverty threshold would have remained on parallel trends in the absence of the ESSER program. If this assumption holds, we would expect the covariates of districts just on either side of the qualification threshold to be on parallel trends before COVID, even if the pre-existing discontinuity in federal funding generates differences in levels.

To test this assumption, we look at covariate balance in trends. For this analysis, we place covariate trends on the left-hand side of Equation 4. Specifically, we show estimates using the change 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

[25] We show a range of specifications for our first stage in Appendix Table 6. Under our preferred specification for the first stage, which uses a 50-formula child bandwidth and a first-degree polynomial, crossing the 5% poverty threshold results in an estimated additional $388 per pupil in ESSER funding, as shown in Column 3. This estimate has an F-statistic of 34. Across the six specifications shown in Appendix Table 6, none has an F-statistic below 17 or an estimate of the additional ESSER funds of under $350 per pupil.

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 Panel A of Appendix Figure 3 and in Appendix Table 7. Broadly, these results support our difference-in-discontinuities approach. Only a single trend showed a statistically significant difference across the threshold: the share of white students. However, the scatter plots reveal that this trend is driven by outlier districts away from the qualification threshold.[26]

Next, we describe how *levels* of covariates change across the cutoff in Panel B of Appendix Figure 3 and in Appendix Table 8.[27] We find evidence of small differences that are likely directly associated with the Title 1-A program, which aims to increase teacher counts. Indeed, this is why we adopt a difference-in-discontinuities design, which differences out these baseline gaps in levels, rather than a static regression discontinuity design.

5. Results

This section presents estimates of the effects of additional ESSER funds on school district performance and decisions during the pandemic. To establish that the qualification discontinuity causes an outcome difference, we rely on three complementary pieces of evidence. First, the regression tables must show a statistically significant coefficient on the qualification dummy. Second, event study figures need to exhibit flat pre-trends whenever pre-COVID data are

[26] Scatterplots of trend changes around the cutoff are displayed in Appendix Figure 3 Panel A. Each cell of Appendix Table 7 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 separate rows. In our preferred specification, a 50-formula child bandwidth and a first-degree polynomial, we see that only the share of White students seems to exhibit a slight differential 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 Appendix Table 7. These estimates show qualitatively similar results to those using our preferred polynomial choice in Panel A of Appendix Table 7.

[27] In Panel B of Appendix Figure 3 and in Appendix Table 8, we show estimates of Equation 4 that replace ESSER funds per pupil with potential covariates in levels on the left-hand side. Across districts immediately surrounding the cutoff, both the enrollment level and the White student share are smooth in levels. We observe statistically notable, but economically 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 1-A 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. We note that enrollment and the share of students who are White exhibit economically non-trivial estimated differences around the cutoff, though the estimates are noisy. Visual inspection of Panel B of Appendix Figure 3 shows that 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. Our full set of results is robust to dropping this outlying district, Leander Independent School District in Texas.

available. Third, binned-scatter plots centered on the qualification threshold should reveal a discrete jump in the outcome of interest. Because we examine a wide range of outcomes, we do not present all three diagnostics in the body of the text, though we generally provide graphical evidence in the appendix.

5.1 District Performance During the Pandemic

As we saw in Figure 1, test scores declined during the pandemic. Multiple sets of researchers have associated these decreased test scores with virtual learning during SY 2020 and SY 2021(Halloran et al., 2021; Oster et al. 2021; Goldhaber et al., 2023). Enrollment also declined sharply. Nationally, about 1.25 million students were "missing" from public schools in SY 2021 compared to SY 2020 (measured in the fall semester). By SY 2023, enrollment had only recovered to a 750,000 student decrease compared to SY 2020. Students in grades K-8 saw larger trend declines in enrollment than high school students.[28] Against this backdrop we investigate if the funds delivered through ESSER allowed schools to alleviate the decrease in test scores and to retain additional students.

5.1.1 Traditional Measures of School Performance

In Table 1, we report estimates for our main district performance outcomes. For each outcome variable, we show estimates for our preferred specification. These regressions use a first degree polynomial and a 50 formula child bandwidth around the qualification threshold.[29] Columns 1 through 6 show how test scores for math and reading evolved in response to the additional \$388 per student in ESSER funds. Regardless of bandwidth choice, we see no evidence that ESSER funds had an effect on test scores during SY 2022 and SY 2023.[30] Using our math scores coefficient for 2023 in Column 2, we can rule out effects larger than a 0.018 (2019) standard deviation increase in scores from ESSER funds. Math test scores declined about

[28] In Panel A of Appendix Figure 4, we show national trends in enrollment in levels. In Panel B of Appendix Figure 4, we present a more detailed look at enrollment changes across grade-level groupings, indexing enrollment changes for grades K-5, grades 6-8, and grades 9-12 to their pre-COVID values.
[29] We present accompanying tables of a full set of first- and second-degree polynomial estimates at alternative bandwidths in Appendix Tables 9 and 10.
[30] No testing was conducted in 2021. The baseline for these regressions is the last available testing year in the Spring of 2019 (SY 2019).

0.1 standard deviations from SY 2019, which means we can rule out that the $388 per student in ESSER funds addressed more than 18% of the decline in scores. For reading scores, the precision of our estimates only allows us to rule out effects larger than 0.038 standard deviations as a result of incremental ESSER funds using the estimates in Column 5. The maximum effect size for reading that we can rule out is a much larger share of the 0.05 standard deviations of learning loss from SY 2023 compared to SY 2019. We present event study versions of these estimates in Panel A and B of Appendix Figure 5.[31] Overall, our point estimates center around zero impacts of ESSER funds on test scores, but our standard errors do not allow us to rule out meaningful effect sizes that would be consistent with the broader literature on school quality inputs (Jackson and Mackevicius, 2021).

We present our results for enrollment in Columns 7 through 9 of Table 1.[32] Our preferred specification in Column 8 shows that an additional $388 per pupil in ESSER funds 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. Aggregating the coefficients for SY 2021, SY 2022, and SY 2023 shows a statistically significant total retention of about 283 student-years over the three school years. These estimates suggest that 2.3% of pre-COVID enrollment was retained by the $388 per student in ESSER funds, or about 1.9 student-years per $10,000 of ESSER funds. Panel C of Appendix Figure 5 shows these estimates as an event study.[33] 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. Enrollment declines were about 5% for the median district in 2022 compared to 2019 and about 2% nationally (Burtis and Goulas, 2023).

We show the impact of additional ESSER funds on enrollment by grade in Table 2. These regressions use our preferred 50 formula child bandwidth and first-degree polynomial specification. First, in Column 1, we show the same estimate as that in Column 8 of Table 1 for

[31] A large driver of the imprecision in these estimates is the decline in sample available, as many districts do not have SEDA estimates, or are high schools that do not partake in the underlying testing.
[32] Since enrollment is measured in the fall semester, we use SY 2020 as the base year for these regressions.
[33] Appendix Figure 6 shows changes in enrollments from SY 2020 (fall 2019) for SY 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 1.

total enrollment changes. 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 1–5, grades 6–8, and finally grades 9–12, with the latter three categories typically corresponding to elementary, middle, and high school students. We can see from Columns 3, 4, and 5 that most of the 283 student-year increase came from movement in lower-grade levels, especially grades 1 through 8. We find no evidence that qualifying for additional ESSER funds increased enrollments for high school students.

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 more, in the final three columns of Table 1. Across specifications, we find that qualifying for additional ESSER funds had a limited effect on absenteeism in SY 2020 and 2021, but may have reduced absenteeism in SY 2023, although these estimates are individually imprecise. However, the combined coefficient shows that ESSER funds likely decreased the number of absentee students 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.[34]

In order to probe the possibility of negative selection in the population of students taking standardized tests, we investigate how the population of test takers changed in response to ESSER funds in Appendix Table 11.[35] Overall, it seems unlikely that negative selection can explain the negative point estimates for the impact of ESSER funds on test scores.[36]

[34] 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).

[35] We use test score data from Halloran et al. (2023), which includes counts of the test taking population for each district. We first show how the total number of test takers changed in 2021, 2022, and 2023 in Column 1. We find suggestive evidence that the count of test takers increased in 2021, but these estimates are imprecise. In Columns 2 and 3, we show how counts of socioeconomically advantaged and not socioeconomically advantaged test takers changed in response to qualifying for additional ESSER funds. In Column 3, we can see a statistically significant increase of about 230 socioeconomically disadvantaged test takers in 2022. However, Column 4 shows that this increase is a very modest share of the overall testing population.

[36] A second channel of interest may be that non-disadvantaged students left public schools for private schools. Using data on private school enrollments in 2020 and 2022 from the private school universe survey (Institute of Education Sciences, 2024b), we find suggestive evidence that private school enrollment within 5 miles of a school district that

5.1.2 Pandemic-Specific Measures of Performance

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

Table 3 displays our estimates of the impact of ESSER funds on mobility. We show results separately for each year of data and 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. 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. In 2021, we see this gap increase to almost 9%. Finally, the mobility gaps persist 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.[37]

Many observers and stakeholders were concerned that opening schools would increase COVID-19 cases in the neighboring populations. In Appendix Table 13, 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.[38] 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 residents per day.[39]

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qualified for additional ESSER funds decreased slightly compared to 2020, and that private school enrollments 5 to 10 miles from a district that qualified for additional ESSER funds increased slightly. These results are shown in Appendix Table 12. Although these results are statistically imprecise, the confidence intervals and point estimates include estimates that would be complementary to our overall enrollment finding in terms of magnitudes.

[37] We show scatterplots with mobility changes against the running variable in enrollment weighted bins in Appendix Figure 7. Here we can clearly see the discontinuity emerge and grow in each subsequent year, which corroborates our estimates in Table 3.

[38] 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.

[39] Using alternative case rates from the New York Times (2023), we find broadly similar results to those shown in Appendix Table 13. 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. 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

5.2 Measuring School District Choices

We now turn to estimates of how additional ESSER funds impacted school district choices pertaining to 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 staffing and budget outcomes, we return to presenting full event study estimates from Equation 3b since several years of pre-COVID data are available in our dataset.

5.2.1 District Staffing

We show the estimated impact of an additional $388 per pupil of ESSER funds on district full-time-equivalent staffing levels per student in Table 4. Throughout the table we use our preferred specification: 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 estimates for changes in total staff per pupil. We then break total staff into two mutually exclusive groups, teachers and non-teachers, and show estimates for teachers per pupil in Column 2 and non-teachers per pupil in Column 3.[40] 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 aides, and district administrators. Our estimates of the impact of qualifying for additional ESSER funds on total staff, teachers, and non-teachers do not show meaningful changes 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 each year, which is around 11% of the pre-COVID baseline of 147 staff per 1,000 students. Similarly, we do not see a large change in the numbers of counselors per pupil or instructional aides in Columns 4 and 5. Increasing support staff was identified by California as a "top priority" for use of ARP ESSER allocations in 2021, and these findings show that we cannot detect increases in these staffing categories (California State Board of Education, 2021). Column 6 reports, however, that ESSER funds may have increased the number of district administrators.

additional ESSER funds on outcomes at our qualification cutoff, but we cannot untangle the causal relationship between the outcome variables themselves.

[40] 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.

5.2.2 District Finances

Our final set of outcomes are district level expenditures and revenues. In Panel A of Table 5 we investigate the causal relationship between additional ESSER funds and school district revenue sources.[41] 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 \$544 per pupil decrease in revenues across the three years that is not significant at conventional confidence levels. In order to understand which revenues declined, we show separate estimates for federal revenues, state revenues, and local revenues per pupil in Columns 2 through 4, respectively.[42] Column 2 suggests that in SY 2021, districts that qualified for an additional \$388 per pupil in ESSER funds 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 three years, which is likely a partial accounting of ESSER funds. In Column 3 we show that districts that qualified for additional ESSER dollars did not see a meaningful amount of 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.

The last major category of revenues is from local sources, which typically make up the majority of school revenues. From Column 4, we 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. We also show the decline in local revenues as an event study in Figure 3, which includes a flat pre-COVID trend in local revenues per pupil across the qualification threshold. We can further contextualize this local revenue decline by looking at changes in property tax collections. In Column 5, we can see that about 80% of the revenue decline came from decreases in revenues from property taxes, with property taxes decreasing by \$718 per pupil across the three years.

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

[42] In the Common Core of Data, federal revenues 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.

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 could be the result of rate cuts, or relative decreases compared to districts that did not qualify for additional funds and chose to raise additional local revenues. First, in Figure 4, using data from the National Council of State Legislatures (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 Barbers Hill Independent School District in Texas, Peaster Independent School District in Texas, and Lake Geneva School District in Wisconsin.[43] In fact, Wisconsin schools broadly seem to have used their ESSER funds explicitly as a temporary budget stopgap to curb property tax increases (Wisconsin Policy Forum, 2021). To take one clear example, Lake Geneva School District called for a referendum to increase taxes after their ESSER funds ran out in 2024, touting to voters that they used these funds for routine budget needs (Lake Geneva Schools, 2025). These anecdotes provide suggestive evidence that districts may have lowered their property tax rates. Federal relief dollars effectively crowded out local revenue in school budgets.

We also provide two pieces of evidence that our results are likely not driven by changes in state funding. In Column 3 of Table 5 Panel A, we can see that state revenues do not jump with the qualification dummy during the pandemic, as our results are not significant at traditional levels and relatively small in magnitude. In Column 6, we also show that state formula funds do not show changes that jump at the 5% poverty threshold either after COVID. These formulas are typically used to equalize funding across districts within a state.

In Panel B of Table 5, we present estimates for the impact of qualifying for additional ESSER funds on district expenditures per pupil. We focus on total expenditures, elementary and

[43] Barbers Hill ISD lowered its tax rates from 1.26% to 1.15% in 2020 (Barbers Hill ISD, 2025). Peaster ISD has steadily lowered its total tax rate since FY2021, which is set the August before the Fiscal Year, from 1.372% down to 1.1664% in 2024 (Peaster ISD, 2024). Lake Geneva decreased its tax rate from 4.29 mill in FY2019 to 3.76 mill in FY2021 (Wisconsin Department of Public Instruction, 2019 and 2021).

secondary education expenditures, capital outlays, instructional spending (including salaries), salaries of all employees, and benefits of all employees on a per pupil basis in Columns 1 through 6, respectively. Across expenditure categories we find no evidence of changes in spending that are detectable at a conventional significance level.[44] The flypaper effect would suggest that revenues from ESSER should be associated with corresponding expenditure increases, which we do not detect at traditional significance levels across expenditure categories in Table 5 (Gramlich, 1977; Hines and Thaler, 1995; Fisher, 1982). Instead, the districts appear to have reduced tax revenues, passing the additional funds on to voters as in Lutz (2010).

Our estimates suggest that a gap between declining revenues and constant expenditures may have emerged as a result of additional ESSER funds. This would suggest that districts would have needed to raise debt or draw on assets to accommodate the imbalance. We provide suggestive evidence in support of the idea that districts may have issued new debt or relied on assets to accommodate local revenue reductions in Appendix Table 14, but the data quality is too low to draw strong conclusions.[45] 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 proposed and passed larger numbers of bonds than in 2020. The 2,134 bonds proposed by districts in 2022 almost matches the previous high in 2018 of 2,163 bonds proposed.

Finally, in Appendix Table 15 we explore whether or not compensation per employee meaningfully changed with additional ESSER funds.[46] We find that total compensation per staff

[44] 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 Appendix Figure 1.

[45] In Appendix 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. The debt data in the CCD files do not include critical details about the maturity and interest rates associated with school bonds. In Column 3, we show how the face value (par value) of total debt changed for districts that qualified for additional ESSER dollars. Across the three years, we find that districts that qualified for additional ESSER funds 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. To give additional context for these estimates, we examine changes in the interest payments made by districts in Column 4. The interest payment change should equal the change in the face value of debt, multiplied by the coupon rate. Using the estimates in Columns 3 and 4, the interest payment and total debt changes imply a coupon rate of about 11% on average for new debts during the pandemic. These rates would be in the right tail of the distribution of rates for school district bonds issued between 1997 and 2017 found by Biasi et al. (Forthcoming). Although a rate of 11% is not impossible, it likely reflects the lack of reliability of the CCD data.

[46] Appendix Table 15 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.

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.[47] These results suggest a change in the labor share of district revenues during the pandemic, although the combined effects are imprecisely estimated.

5.3 Potential Mechanisms for District Policy Responses

Our core results suggest that qualifying for additional ESSER funds allowed school districts to retain students, return to hybrid or in-person learning faster, and reduced local revenues relative to similarly situated districts that did not receive additional funds. At first glance, these results may be puzzling as enrollment retention and returning to in-person learning could be at odds with a revenue cut. Moreover, interest groups such as administrators and teachers might capture some of the funds as memoranda of understanding between unions and districts were likely updated in response to the pandemic (Peeples, 2020).

We provide evidence to support the idea that the policy mix that we find as a result of ESSER funds likely reflects increased political engagement, which may have been enabled by additional ESSER funds. We map the 2016 through 2024 responses of the Cooperative Election Study (CES) to school districts in our difference-in-discontinuities 50 formula child bandwidth sample (Schaffner et al., 2025).[48] For each question, we calculate the response rates across districts above and below the qualification cutoff, weighted by district enrollments. We use questions that capture whether or not a respondent voted or intends to vote in the upcoming election, attended a local school board or city council meetings, or contacted a public official in the past year. We then calculate the parental share among respondents that participated in each of these engagement activities. These data allow us to see if political engagement changed

[47] A similar pattern is reflected in Columns 2 and 3 for teachers and non-teachers. However, the increase in compensation during the pandemic seems 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.

[48] The CES is a large representative (around 60,000 respondents) survey cast around early October of each election year. To our knowledge, the CES is the only long-standing survey that has sufficient sample and asks engagement questions about voting, local government meeting attendance, and contacting local officials with demographics available to identify parents with children under age 18. For details, see the documentation in Schaffner et al. (2025).

differentially during the pandemic in districts that qualified for additional ESSER funds compared to districts that did not qualify for additional funds.

Figure 5 presents percent changes in electoral engagement from 2018 for districts in our difference-in-discontinuities sample.[49] In the top row, we show changes for all respondents in districts that qualified for additional ESSER funds in blue and districts that did not qualify for additional funds in red. Overall voting doesn't seem to have changed differentially across these sets of districts. However, changes in attendance at local government meetings appears to be higher in districts that received additional funds in 2020, 2022, and 2024, and a more modest difference in changes in rates of contacting local officials emerges for districts above the qualification threshold later in the sample period. The second row of Figure 5 shows the changes from 2018 in the parental share of respondents that participated in each political activity. We find the strongest evidence that changes in the parental share of voters were larger in 2020 for districts that received additional ESSER funds, and these changes may have persisted in 2022 and 2024. We find similar patterns for changes in the parental share of meeting attendees and public official contacts, but these results show significant overlap between the bootstrapped confidence intervals of both sets of districts. Overall, these data provide suggestive evidence that federal grant spending and related policy priorities reflected the preferences of local residents, which is consistent with several theoretical and empirical results in the literature on intergovernmental grants (Bradford and Oates, 1971a; Bradford and Oates, 1971b; Gordon, 2004; Lutz, 2010).

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 that the assistance provided by ESSER to local school districts may have helped them to retain students and re-open their schools faster. However, we see no evidence that ESSER funds helped to mitigate learning loss, at least in the short run. A meta-analysis by Jackson and Mackevicius (2021) suggests that an additional $1,000 per year of K-12

[49] These changes are weighted by enrollment. We provide 95% bootstrapped confidence intervals for both series.

school spending raises scores by 0.012 standard deviations, and we cannot rule out estimates of that magnitude.

In addition to our findings for school performance, we provide evidence on school district finances. While we find no statistically significant evidence that districts increased expenditures in SY 2021 or SY 2022, we do find evidence that districts that qualified for additional ESSER funds had statistically significant reductions in local revenues that were in excess of their ESSER funds. This pass through of federal funds may partially explain why ESSER had minimal impacts on learning loss for districts around the 5% poverty threshold. Moreover, increased political engagement, especially from parents, may have driven school boards to direct these funds towards property tax reductions and increased re-opening rates.

Our findings suggest that ESSER funds were successful in achieving some but not all of their policy goals, with positive effects on school reopenings and the retention of student enrollments. However, our findings apply specifically to districts in the neighborhood of the 5% poverty threshold for qualifying for additional ESSER funds. These districts have lower poverty rates than a typical U.S. school district and are smaller than the national average. Our results suggest that smaller and richer school districts used additional ESSER funds largely to reduce property tax burdens and open schools, in contrast to the predictions of the flypaper effect.

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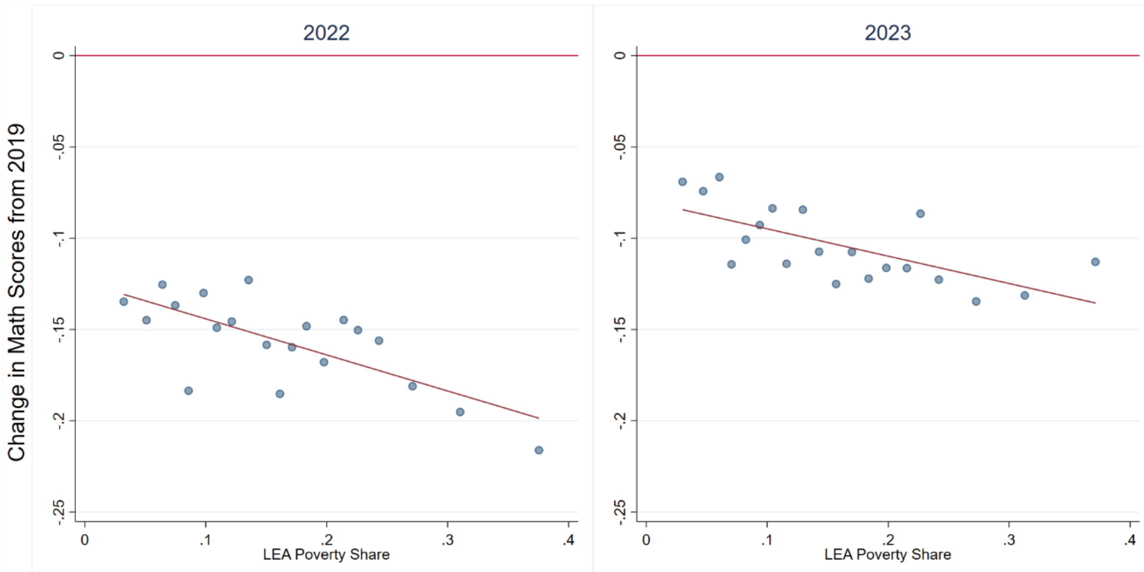
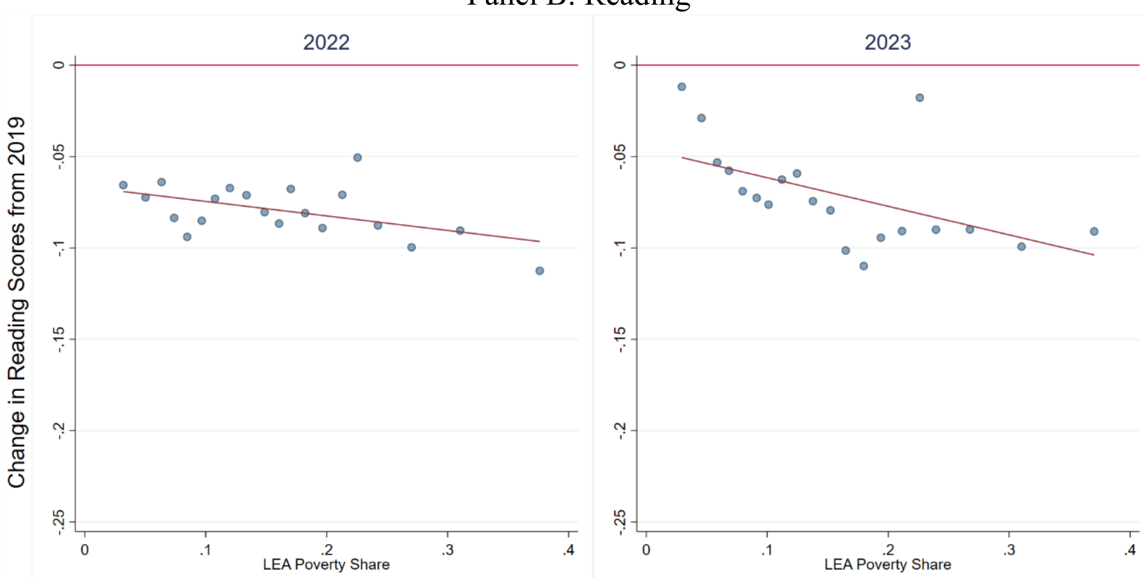
Figures

Figure 1: Pandemic Learning Loss Compared to 2019 by District Poverty Share in 2022 and 2023 for Math and Reading Scores of 3rd Through 8th Graders

Panel A: Math



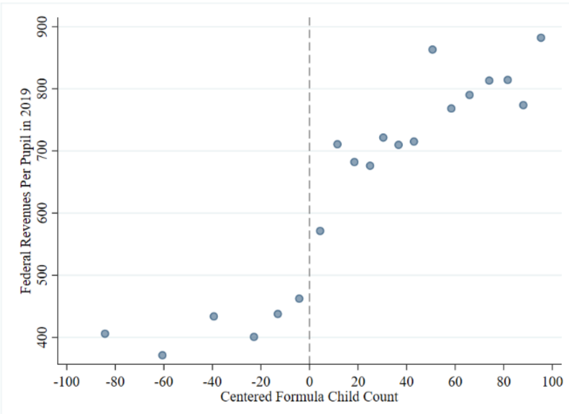
Panel B: Reading



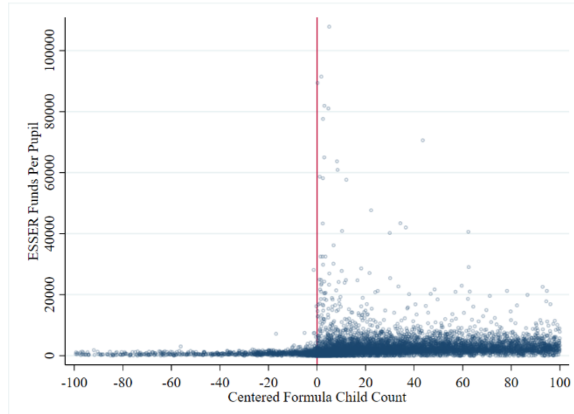
Note: This figure shows bins weighted by enrollments from SY 2020. The horizontal axis shows school district poverty shares from the 2018 SAIPE data. The vertical axis shows changes in test scores from the spring of 2019, measured in 2019 standard deviations using data from SEDA (Reardon, et al., 2024). 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. All plots include fitted regression lines.

Figure 2: Discontinuities in Federal Revenues and ESSER Funds at the 5% Poverty Qualification Threshold

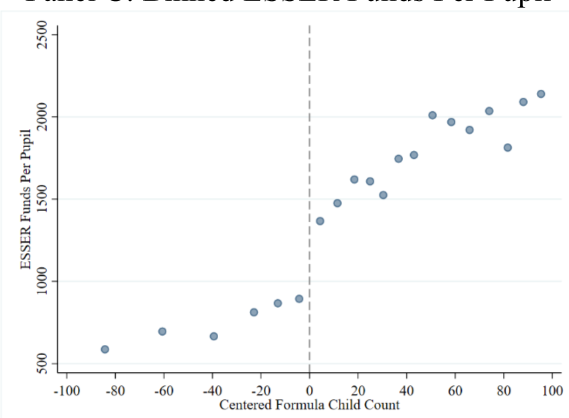
Panel A: Binned Federal Revenues in 2019 Per Pupil



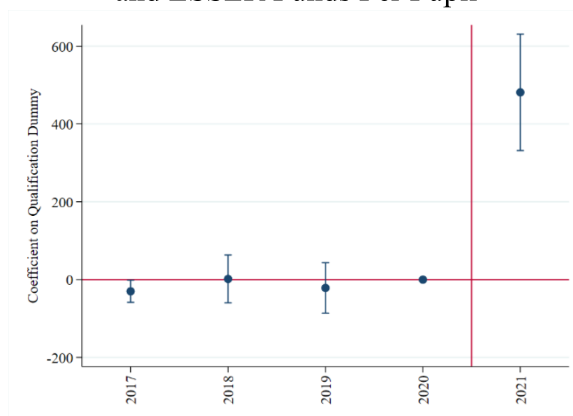
Panel B: Un-Binned ESSER Funds Per Pupil



Panel C: Binned ESSER Funds Per Pupil

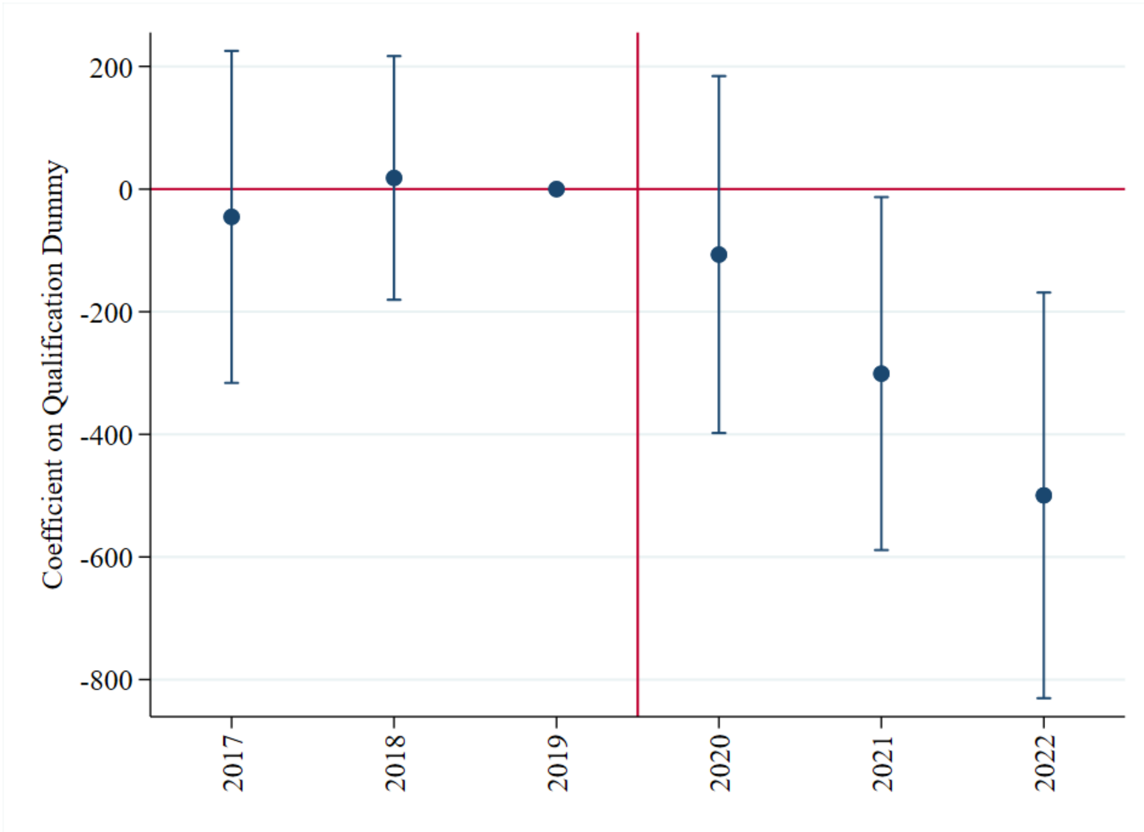


Panel D: Event-Study of Federal Revenues and ESSER Funds Per Pupil



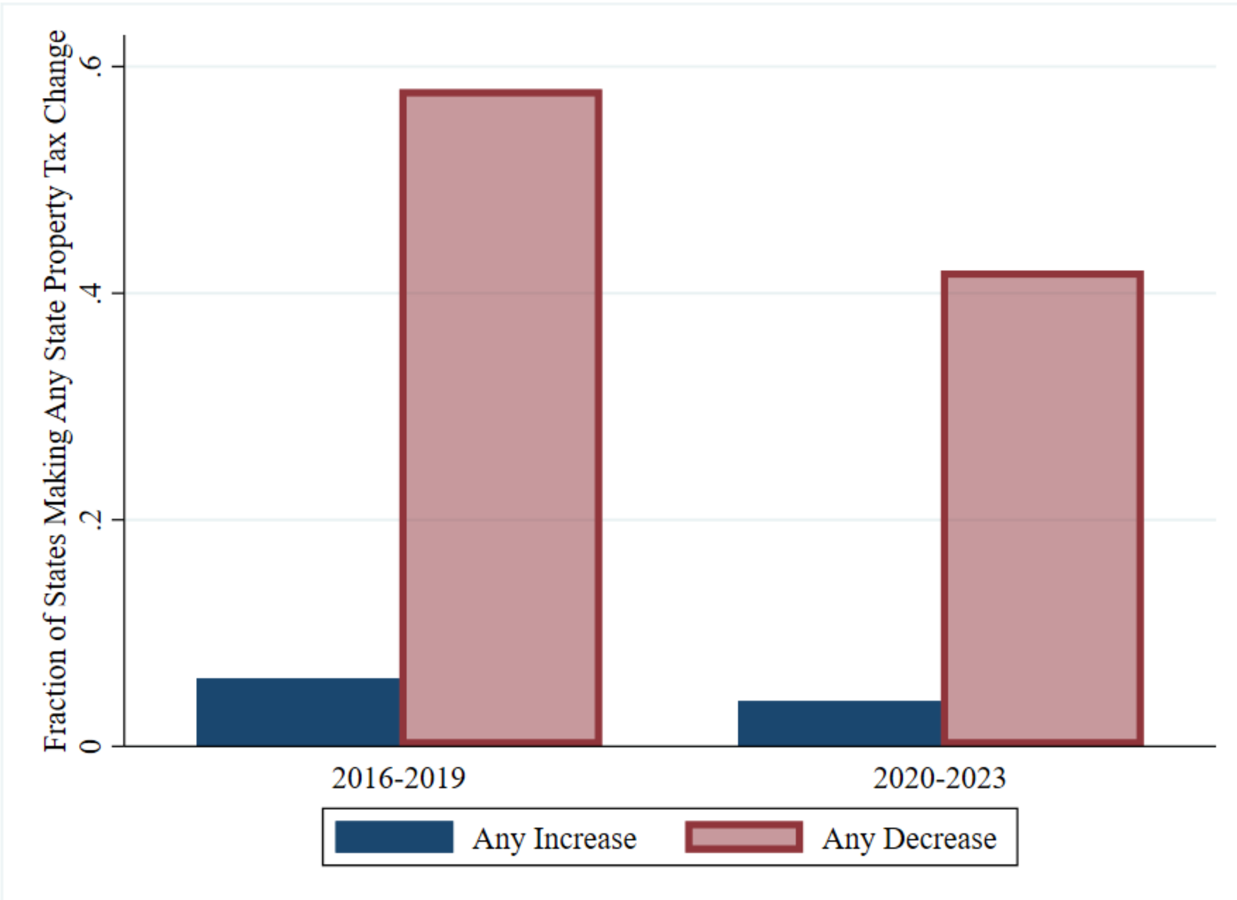
Note: This figure shows first stage scatter plots matching to Equations 4 and 3a in the text. Scatter plots in Panels A and C use bins weighted by SY 2020 enrollment. Panel D shows coefficients from estimates of Equation 3a, described in the text. We show the qualification threshold associated with the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

Figure 3: Event Studies for the Impact of Qualifying for Additional ESSER Funds Per Pupil on School District Revenues from Local Sources from SY 2017 to 2022



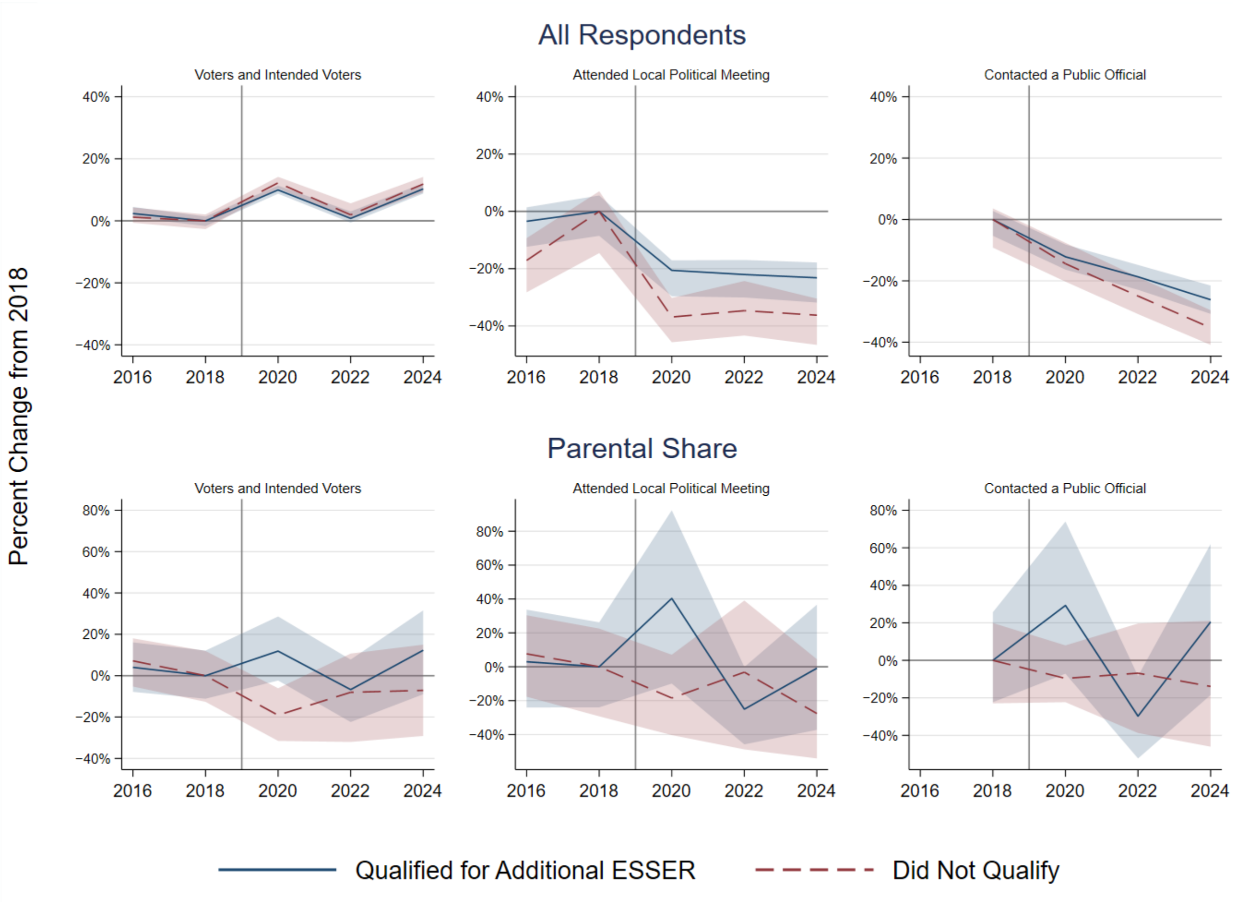
Note: This figure shows estimates of coefficient on the qualification dummy interactions from Equation 3b as described in the text. These regressions correspond to Table 5 Column 4.

Figure 4: 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 Legislatures (2024).

Figure 5: Percent Changes in Electoral Engagement by Activity by ESSER Qualification Status from 2016 to 2024



Note: This figure shows percent changes from 2018 levels for several measures of electoral engagement for respondents in school districts within the difference-in-discontinuities sample. We use responses from the Cooperative Election Study (Schaffner et al., 2025) to map political participation into school districts using respondents zip codes and crosswalk files from (Missouri Census Data Center, 2024). The top row shows changes in participation in the following activities for all respondents: voting, attending a local government meeting, and contacting a public official. These changes are calculated as percentage changes from the 2018 baseline values for districts above and below the 5% poverty share threshold for districts within our 50 formula child bandwidth. It is important to not that district composition changes with the sample across survey years. We bootstrap 95% confidence bands by resampling 500 times by zip code and qualification status. In the second row, we show the parental share of respondents for each activity. This includes, the parental share of respondents who voted, the parental share of respondents who attended a local meeting, and the parental share of respondents who contacted a public official.

Tables

Table 1: Estimates of the Impact of Qualifying for Additional ESSER Funds on Measures of School District Performance

	Math	Reading	Enrollment	Absenteeism
Qualification · I{2023}	-0.0234	-0.00356	62.44	
	(0.0213)	(0.0214)	(78.61)	
Qualification · I{2022}	0.00283	-0.0166	95.55	-381.1
	(0.0225)	(0.0170)	(52.21)	(283.7)
Qualification · I{2021}			124.8*	120.2
			(58.96)	(164.7)
Qualification · I{2020}				41.43
				(81.40)
First-Degree Polynomial	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y
Observations	6,547	5,962	32,732	25,910
R2	0.915	0.908	0.997	0.685
Band	50	50	50	50
Combined Coefficient	-0.0206	-0.0202	282.8	-219.5
	(0.0413)	(0.0361)	(130.7)	(103.0)
Pre-COVID Mean	0.429	0.438	3,902.8	337.2

Note: This table shows estimates of Equation 3b as described in the text for math scores, reading scores, enrollment counts, and absenteeism counts. Math scores and reading scores are measured in standard deviations of the 2019 test score distribution. Enrollment and absenteeism are presented in student-years, where enrollment is measured in the fall semester and absenteeism measures whether or not a student misses more than 10% of school days within a school year. We present estimates for a bandwidth for 50 formula children from the 5% poverty threshold using a first-degree polynomial specification. We present a linear combination 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 2: Estimates of the Impact of Qualifying for Additional ESSER Funds on Enrollments by Grade Level Groups

	Total (1)	PK (2)	K (3)	1-5 (4)	6-8 (5)	9-12 (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	32,732	22,010	30,800	31,192	30,870	22,939
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 combination 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 3: Estimates of the Impact of Qualifying for Additional ESSER Funds on Year-Over-Year Cellphone Mobility Changes from CY 2019 Baseline

	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	2,990	2,990	2,975	2,975	2,915	2,915
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 2022. 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 4: Estimates of the Impact of Qualifying for Additional ESSER Funds on School District Staffing Per 1000 Pupils

	Tot. Staff (1)	Teachers (2)	Non-Teachers (3)	Counselors (4)	Inst. Aids (5)	Dist. Admin (6)
Qualification · I{2023}	0.160	-0.316	0.477	-0.011	1.231	0.722***
	(4.487)	(0.984)	(4.331)	(0.132)	(1.028)	(0.179)
Qualification · I{2022}	3.089	0.968	2.121	0.101	0.666	0.050
	(1.793)	(0.595)	(1.597)	(0.089)	(0.600)	(0.079)
Qualification · I{2021}	0.616	0.203	0.414	-0.095	0.166	0.0043
	(1.574)	(0.571)	(1.243)	(0.075)	(0.584)	(0.062)
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	30,247	30,247	30,247	29,383	29,154	28,973
R2	0.768	0.778	0.771	0.748	0.849	0.812
Band	50	50	50	50	50	50
Combined Coefficient	3.866	0.854	3.012	-0.005	2.063	0.777
	(6.205)	(1.691)	(5.743)	(0.248)	(1.813)	(0.255)
Pre-COVID Mean	146.9	71.34	75.52	2.233	19.37	2.172

Note: This table shows estimates of Equation 3b as described in the text for total staff per 1000 pupils, teachers per 1000 pupils, non-teaching staff per 1000 pupils, guidance counselors per 1000 pupils, instructional aides per 1000 pupils, and district administrators per 1000 pupils. 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 combination 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 5: 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	28,056	28,056	28,056	28,056	26,021	28,056
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	28,056	28,056	28,056	28,056	28,056	28,056
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

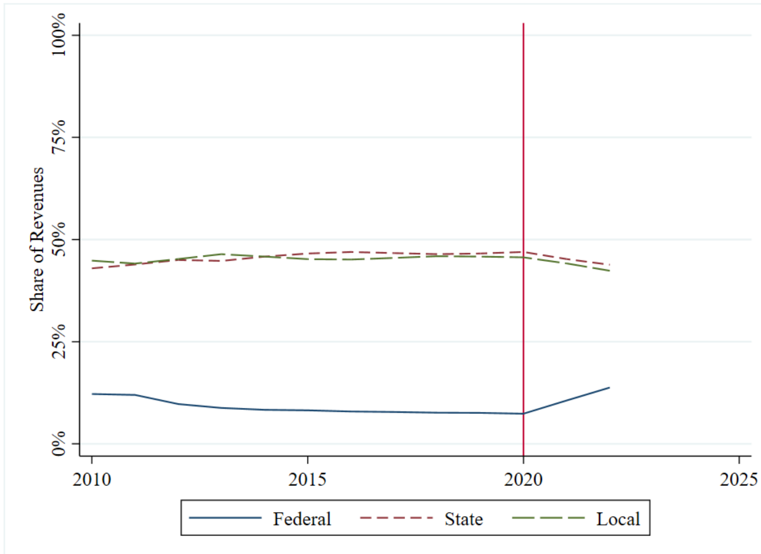
Note: This table shows estimates of Equation 3b as described in the text for revenues in Panel A and expenditures in Panel B. In Panel A we show estimates for total revenues per pupil, federal revenues per pupil, state revenues per pupil, local (own source) revenues per pupil, property tax revenues per 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 combination 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.

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

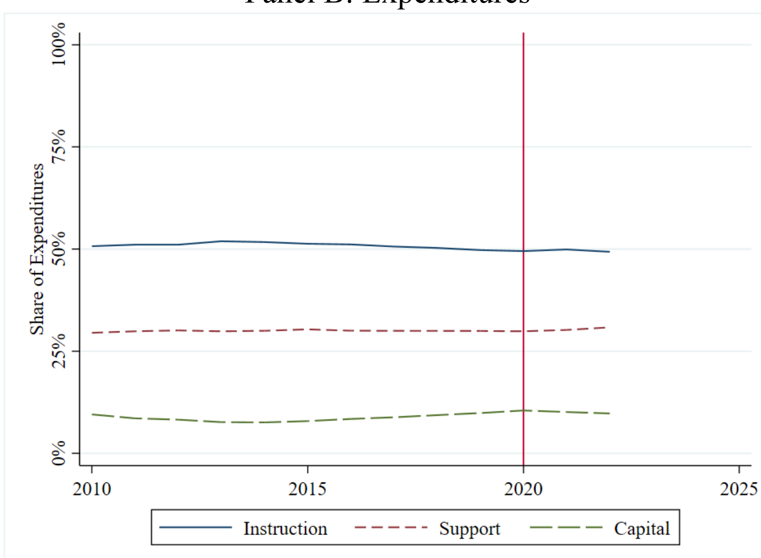
Appendix Figures

Appendix Figure 1: National Shares of School District Revenues and Expenditures

Panel A: Revenues

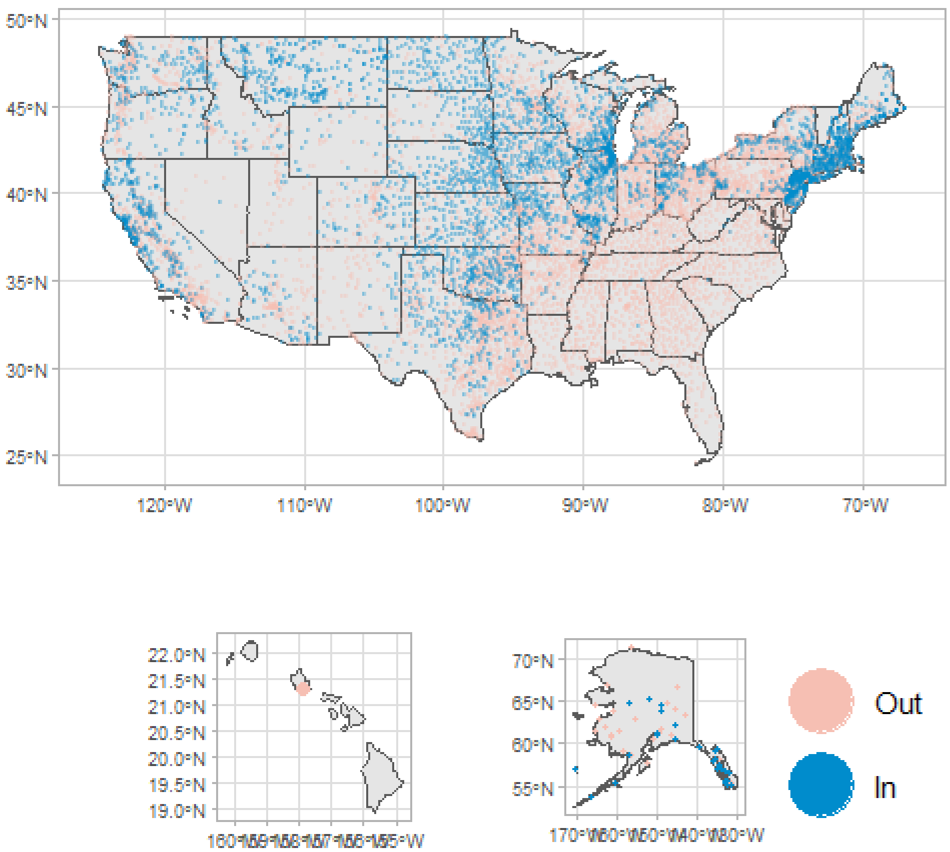


Panel B: Expenditures



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 IES (2024).

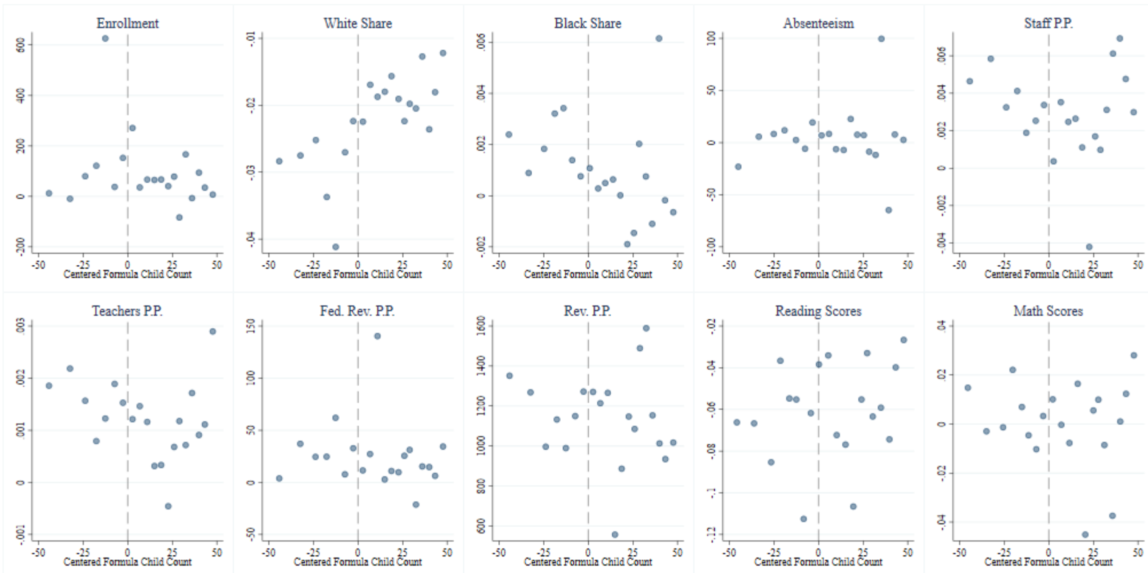
Appendix Figure 2: Map of Districts Included in the 50 Formula Child Bandwidth Difference-in-Discontinuities Sample



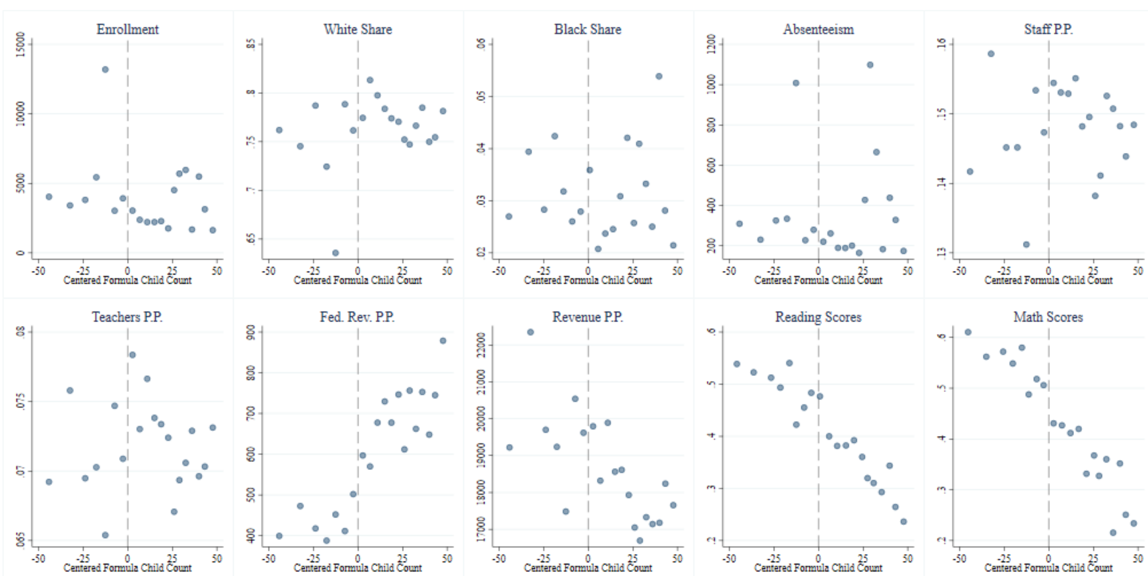
Note: This figure shows districts in the 50 formula child bandwidth sample in blue and all other districts in red.

Appendix Figure 3: Covariate Smoothness at the 5% Poverty Qualification Cutoff Before COVID in Trends and Levels

Panel A: Trends



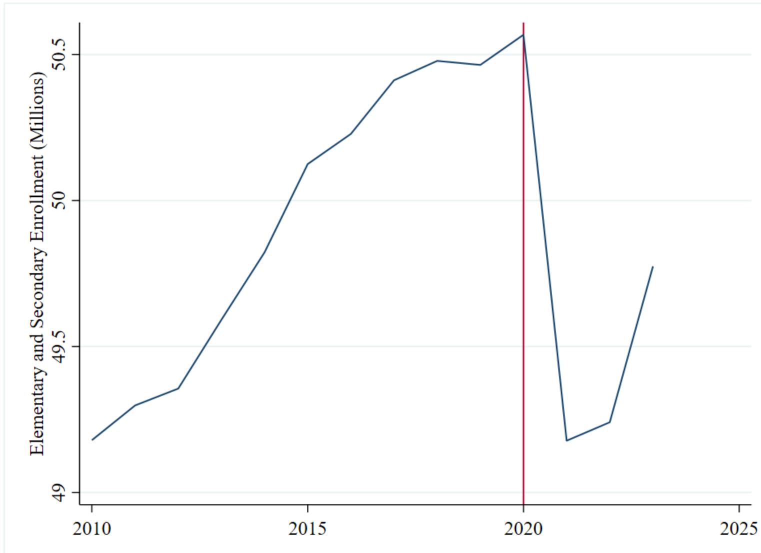
Panel B: Levels



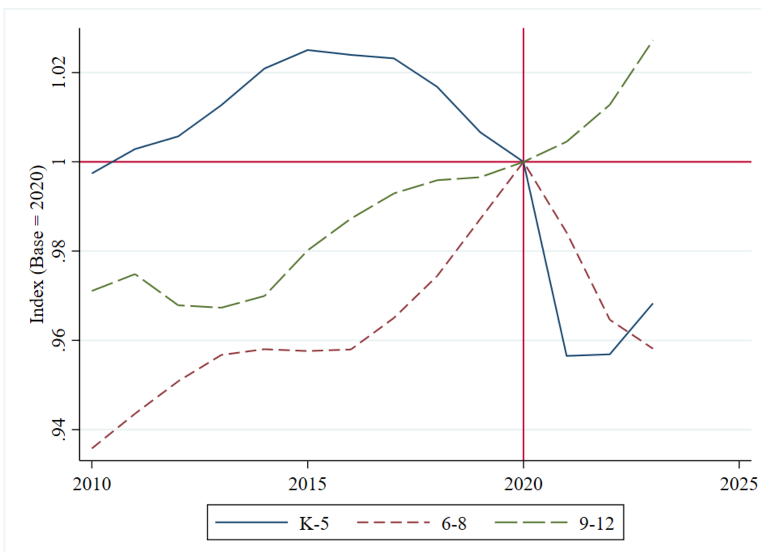
Note: This figure shows balance checks for covariate differences across the qualification cutoff as pre-COVID trends in Panel A and in levels in Panel B akin to equation 4 in the text. Scatters are weighted by SY 2020 enrollment. We show the qualification threshold associated with the 5% poverty share with a vertical dashed line. We show trends from SY 2017 to SY 2020 for enrollment, White share, Black share, staff per pupil, and teachers per pupil and trends from SY 2017 to SY 2019 for absenteeism, federal revenues per pupil, total revenues per pupil, reading scores, and math scores.

Appendix Figure 4: National Enrollment Trends in Elementary and Secondary Schools

Panel A: Total Enrollments



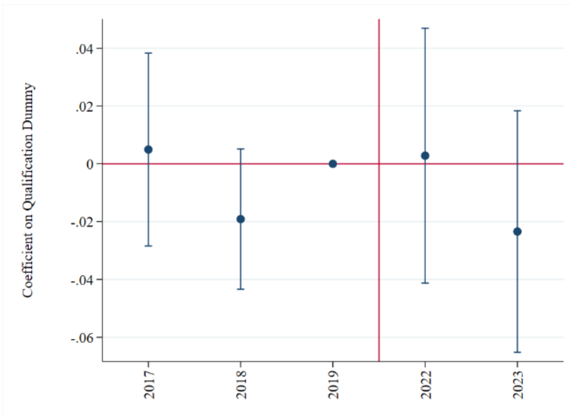
Panel B: Indexed Enrollment Trends by Grade Group



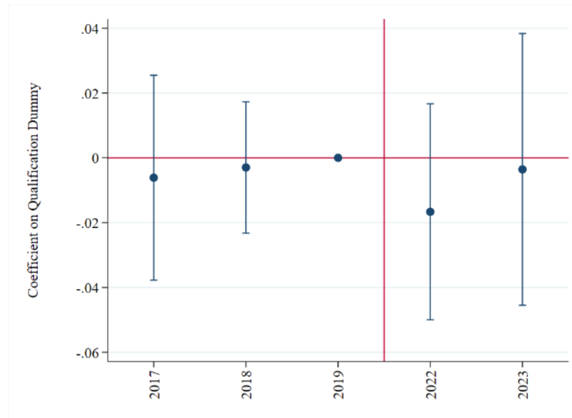
Note: Panel A of this figure shows national enrollment trends for all public schools. Panel B shows indexed enrollment trends for public schools for grades kindergarten through 5th (blue line), 6th through 8th (red line), and 9th through 12th (green line). This figure uses data from the Common Core of Data (IES, 2024b).

Appendix Figure 5: Event Studies for the Impact of Qualifying for Additional ESSER Funds Per Pupil on School District Performance from SY 2017 to 2023

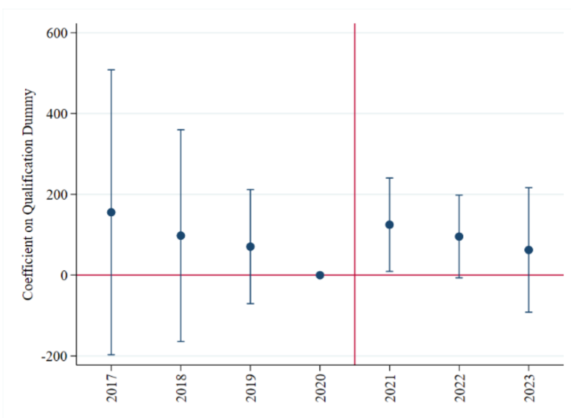
Panel A: Math



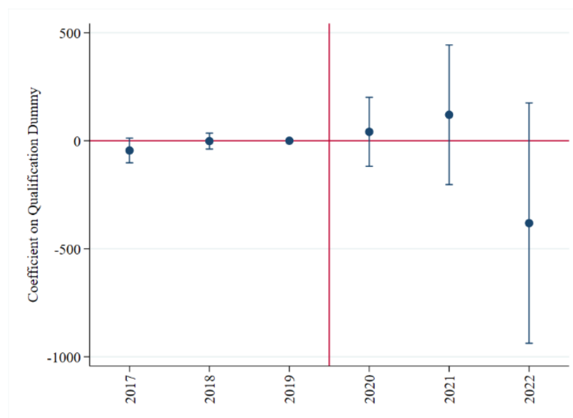
Panel B: Reading



Panel C: Enrollment

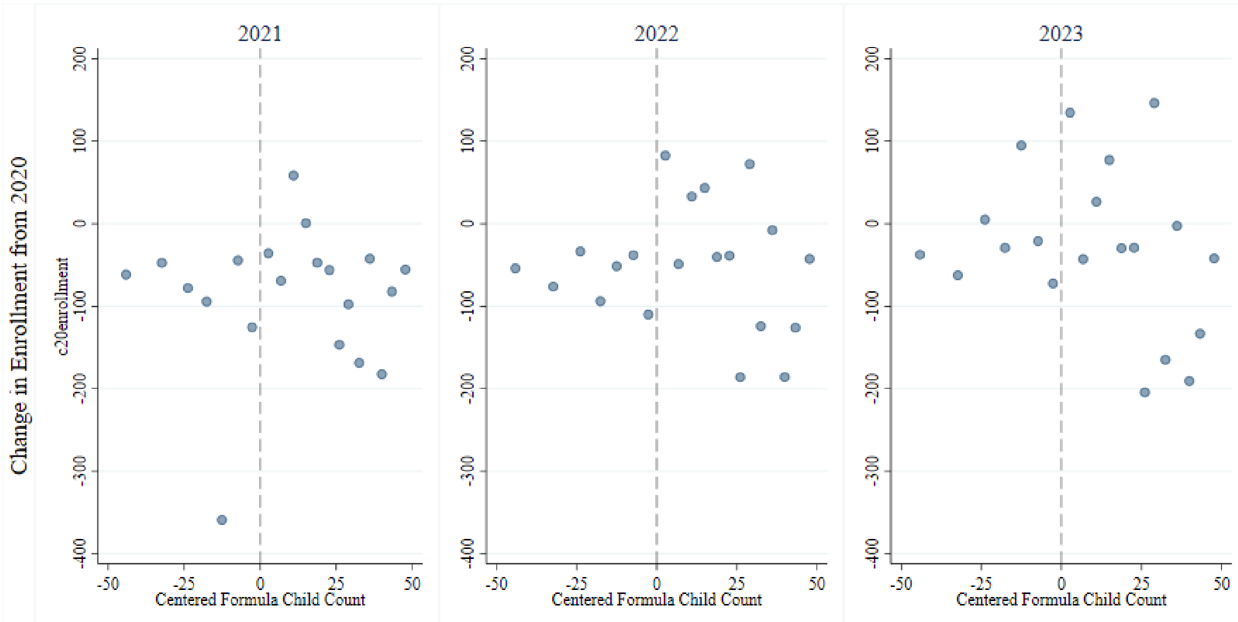


Panel D: Absenteeism



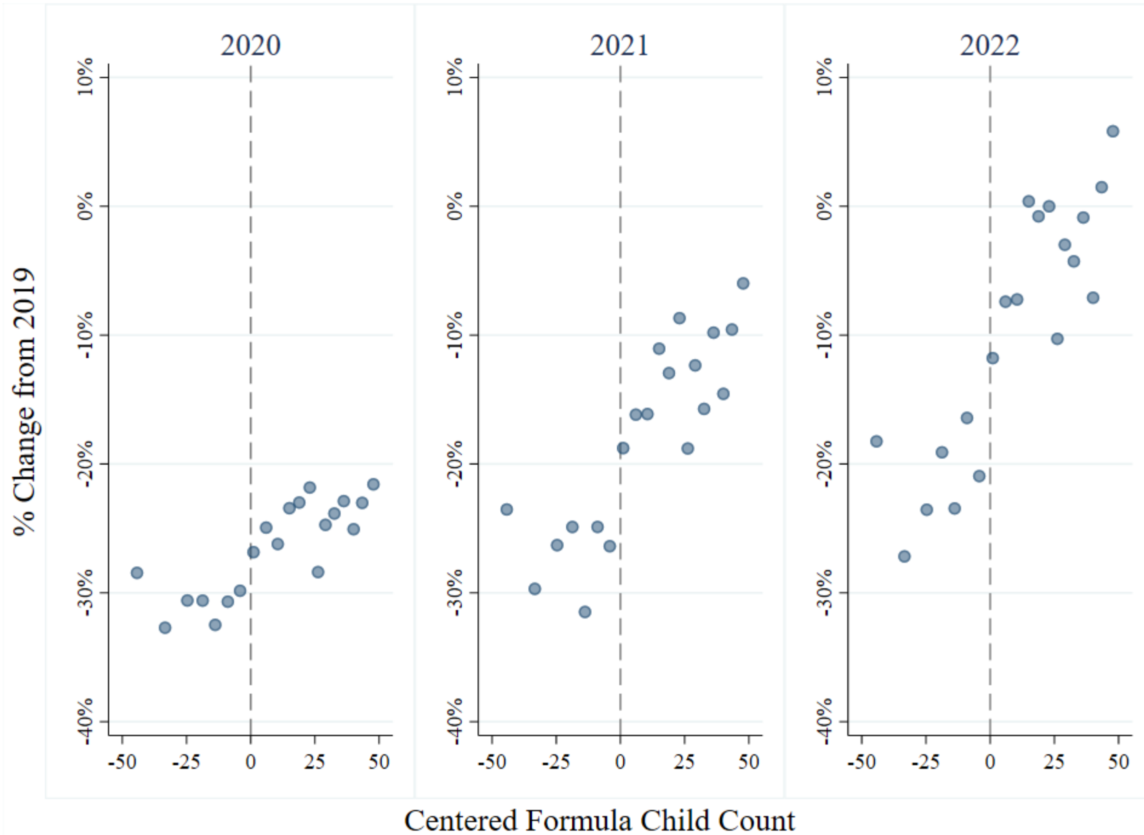
Note: These figures show coefficients from the qualification dummy interactions for each year as described in Equation 3b. These regressions correspond to Columns 2, 5, 8, and 11 of Table 1.

Appendix Figure 6: Binned Scatter of Changes in Enrollment from SY 2020 Across the 5% Poverty Qualification Cutoff in SY 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.

Appendix Figure 7: Binned Scatter of Year Over Year Changes in Cell Phone Mobility from 2019 Across the 5% Poverty Qualification Cutoff in CY 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.

Appendix Tables

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

	Below (1)	Above (2)	Total (3)
Total Revenues	19,511.7	17,816.8	18,407.9
	(7,085.5)	(7,560.9)	(7,441.0)
Federal Revenues	*455.3*	*680.6*	*602.0*
	(527.3)	*(1034.8)*	*(897.6)*
State Revenues	*5,764.8*	*6,773.2*	*6,421.5*
	(2,500.8)	*(3,795.3)*	*(3,433.6)*
Local Revenues	*13,291.6*	*10,363.0*	*11,384.5*
	(7,169.3)	*(7,328.3)*	*(7,404.4)*
Total Expenditures	19,714.9	17,827.6	18,485.9
	(7,092.3)	(7,590.7)	(7,473.5)
Instruction Current Expenditures	*10,039.7*	*8,839.7*	*9,258.2*
	(4,039.1)	*(3,429.9)*	*(3,697.4)*
Salaries for Instruction	*6,397.2*	*5,553.9*	*5,848.0*
	(2,592.9)	*(2,187.1)*	*(2,370.3)*
Enrollment	5,235.0	3,261.9	3,950.1
	(7,262.3)	(4,956.6)	(5,937.7)
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 and expenditure variables are on a per pupil basis using SY 2020 enrollments. Instruction current expenditures includes salaries for instruction. 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.

Appendix 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 1-A Funds	2018
Title 1-A 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.

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

	2017 (1)	2018 (2)	2019 (3)	2020 (4)	2021 (5)	2022 (6)	2023 (7)	Total (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	3,897.0	3,910.2	3,950.1	3,863.3	3,898.3	3,920.6	3,899.7
	(5675.9)	(5,758.8)	(5,824.5)	(5937.7)	(5,783.1)	(5,896.6)	(5,944.1)	(5,831.8)
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	17,611.2	17,937.2	18,466.1	18,628.8	19,110.8	19,312.5		18511.1
	(6788.4)	(6,908.6)	(7,463.1)	(8,234.6)	(7,122.4)	(15,158.6)		(9127.8)
Federal Revenues	577.1	586.4	604.3	604.0	1,049.6	1,435.7		809.5
	(694.4)	(849.6)	(918.7)	(727.4)	(988.9)	(2,003.4)		(1169.4)
Fed. Rev. w/ ESSER	577.5	585.8	602.0	598.3	2,206.4	1,046.9		936.1
	(709.4)	(848.3)	(897.6)	(723.5)	(1,797.0)	(856.5)		(1198.4)
Teachers per Pupil	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	4,074.2	4,071.7	4,061.4	4,056.8	4,063.8	4,350.1		4,113.0
	(5,924.4)	(5,969.7)	(6,002.7)	(6,051.0)	(6,122.4)	(6,548.8)		(6,107.1)

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. Math and reading scores are presented in standard deviations from the 2019 testing distribution. Enrollment, total schools, formula children, and population ages 5 to 17 are presented in counts. Total revenues, federal revenues, and federal revenues with ESSER funds are presented in 2023 USD per enrollee in SY 2020. Teachers per pupil (PP) are full time equivalent teacher counts per enrollee in SY 2020. Enrollment share white is presented as a share, which ranges from 0 to 1, and mobility change from 2019 is presented as a share, where -0.2 reflects a 20% decline in mobility from 2019.

Appendix Table 4: 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 school 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 2018 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.	2018	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 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.	Throughout the calendar year	Parolin and Lee (2021)

Fixed Spending PP Vars	Real dollars per enrollee in SY 2020	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 and demeaned.	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 and demeaned.	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 Current 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
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 aides 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

Appendix Table 5: 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	43,456.8	3,888.5	47,082.9
	(86,565.3)	(5,753.0)	(89,558.5)
Total Schools	62.17	6.219	67.29
	(128.6)	(6.679)	(133.1)
Total Revenues	14,544.4	18,004.8	14,227.3
	(4,914.2)	(7,067.8)	(4,537.4)
Federal Revenues	1,070.4	589.3	1,114.5
	(726.1)	(826.2)	(699.9)
Fed. Rev. with ESSER	1,085.8	588.4	1,131.4
	(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	9,502.9	221.4	10,353.5
	(23,317.3)	(296.6)	(24,184.8)
Pop Ages 5 to 17	49,306.7	4,069.1	53,452.3
	(100,347.1)	(5,965.3)	(103,848.4)

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. Math and reading scores are presented in standard deviations from the 2019 testing distribution. Enrollment, total schools, formula children, and population ages 5 to 17 are presented in counts. Total revenues, federal revenues, and federal revenues with ESSER funds are presented in 2023 USD per enrollee in SY 2020. Teachers per pupil (PP) are full time equivalent teacher counts per enrollee in SY 2020. Enrollment share white is presented as a share, which ranges from 0 to 1.

Appendix Table 6: 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	4,413	4,413	4,696	4,696	4,986	4,986
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

Appendix Table 7: 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. We show trends from SY 2017 to SY 2020 for enrollment, White share, Black share, staff per pupil, and teachers per pupil and trends from SY 2017 to SY 2019 for absenteeism, federal revenues per pupil, total revenues per pupil, reading scores, and math scores.

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

Appendix Table 8: Test for Covariate Smoothness in Levels at the 5% Poverty Qualification Cutoff Before COVID in Levels

					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	-0.0480	-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					
	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	-0.0437	-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

Appendix Table 9: 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	5,991	6,547	7,036	5,427	5,962	6,459	3,0751	3,2732	3,4762	2,4311	2,5910	2,7521
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	4,008.3	3,902.8	3,810.3	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. Math scores and reading scores are measured in standard deviations of the 2019 test score distribution. Enrollment and absenteeism are presented in student-years, where enrollment is measured in the fall semester and absenteeism measures whether or not a student misses more than 10% of school days within a school year. 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 combination 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 Table 10: 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	5,991	6,547	7,036	5,427	5,962	6,459	30,751	32,732	34,762	24,311	25,910	27,521
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 combination 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 Table 11: Estimates of the Impact of Qualifying for Additional ESSER Funds on Counts and Shares of Test Takers by Socioeconomic Group

	Counts			Shares
		Soc.	Not	Not
	All	Adv.	S.A.	S.A.
	(1)	(2)	(3)	(4)
Qualification · I{2023}	-51.11	118.8	-169.9	-0.006
	(430.4)	(284.6)	(188.6)	(0.016)
Qualification · I{2022}	70.45	-159.5	229.9*	0.000
	(177.6)	(220.3)	(99.54)	(0.015)
Qualification · I{2021}	1292.9	832.4	460.5	0.040
	(1103.7)	(836.4)	(356.0)	(0.034)
First Degree Polynomial	Y	Y	Y	Y
LEA FEs	Y	Y	Y	Y
Year Fes	Y	Y	Y	Y
Observations	10,152	10,152	10,152	10,152
R2	0.978	0.959	0.863	0.726
Band	50	50	50	50
Combined Coefficient	1,312	792	521	0.03
	(748)	(663)	(378)	(0.06)
Pre-COVID Mean	4,053.0	3,159.8	893.2	0.279

Note: This table shows estimates of Equation 3b as described in the text for counts of total test takers, counts of socioeconomically advantaged test takers, counts of not-socioeconomically advantaged test takers, and the share of non-socioeconomically advantaged test takers out of all test takers. We present estimates for 50 formula children from the 5% poverty threshold. We present a linear combination 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 Table 12: Estimates of the Impact of Qualifying for Additional ESSER Funds on Percent Changes in Private School District Enrollment Around Public Schools Between SY 2022 and SY 2020

	Percent Change in Enrollment from SY 2020			
	5 Mile Radius		5-10 Mile Radius	
	All (1)	In Grade (2)	All (3)	In Grade (4)
Qualification Dummy	-0.119	-0.256	0.054	0.109
	(0.138)	(0.266)	(0.076)	(0.109)
First Degree Polynomial	Y	Y	Y	Y
Observations	1,597	1,543	2,159	2,129
R2	0.008	0.004	0.000	0.001
Band	50	50	50	50
Pre-COVID Enrollment	1,669.2	1,371.8	4,668.3	3,875.5

Note: This table shows estimates of Equation 4 as described in the text for percent changes in private school enrollment between SY 2022 and SY 2020 (measured in October of the fall semester). These data come from the Private School Universe Survey (Institute of Education Sciences, 2024b). Private school enrollment is calculated based on proximity to the public school district of interest. We sum all private school enrollments and private school enrollments in overlapping grades to the reference public school in a 5 mile radius and then a 5 mile to 10 mile radius donut in SY 2020 and SY 2022. Then, we construct the percentage change across the two school years. Column 1 shows estimates for percent changes in all private school students within a 5 mile radius of the reference public school. Column 2 shows estimates for percent changes in private school enrollments in grades that overlap with the reference public school district within 5 miles of the reference public school district. Column 3 shows estimates for percent changes in all private school students within a 5 mile to 10 mile radius donut from the reference public school. Column 4 shows estimates for percent changes in private school enrollments in grades that overlap with the reference public school district within a 5 mile to 10 mile radius donut from the reference public school district. We present estimates for 50 formula children from the 5% poverty threshold. We present a linear combination 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 Table 13: 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	4,412	4,412	4,695	4,695	4,983	4,983
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

Appendix 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	-543.51	745.45	1,809.39	204.54	-1,861.41	-566.45	-1,175.74	-119.21
Coefficient	(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	7,497.60	561.00	1,740.90	5,195.70

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. Total assets are made up of sinking funds, unspent bond funds, and other assets, which include general funds and other investments made by the district. 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 combination 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 Table 15: Estimates of the Impacts of Qualifying for Additional ESSER Funds on Total Compensation Per Employee by Type

	Tot. Staff (1)	Teachers (2)	Non-Teachers (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 basis. We present estimates for 50 formula children from the 5% poverty threshold and a first-degree polynomial specification. We present a linear combination 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.

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