ESSER and Student Achievement: Assessing the Impacts of the Largest One-Time Federal Investment in K12 Schools

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Grace Falken

June 2024

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Abstract

We estimate the effects of federal pandemic-relief funding (ESSER III) for K12 schools on district-level student achievement growth in 2023. We rely on student test achievement data from over 5,000 school districts across 30 states. Our novel identification strategy exploits variation in ESSER attributable to its allocation rules and their relationship to Title I. We find that each $1,000 increase in ESSER per pupil funds led to statistically significant increases in district math scores of 0.008 standard deviations and similar but statistically insignificant increases in ELA scores. Our heterogeneity analysis suggests impacts were not even across district pre-pandemic spending levels, student race, or urbanicity. Our estimates provide some insight into how much investment may be needed for a full academic recovery from the pandemic: to recover losses remaining after 2023, we estimate schools would need to spend $9,000 to $13,000 per pupil.

1. Introduction

During the COVID-19 pandemic, the federal government invested an unprecedented amount of funding in K12 schools through the Elementary and Secondary School Emergency Relief Fund (ESSER).[1] With nearly $200 billion in funding, ESSER represents the largest one-time investment in K12 schools in American history. Ninety percent of ESSER allocations went directly to local school districts to help with the safe reopening of in person learning and to support academic recovery. While the funding was largely unrestricted, 20 percent of the third wave of ESSER was designated to address "learning loss" (Office of Elementary & Secondary Education, 2021). Despite this substantial federal investment, there is currently no causal evidence about whether ESSER funds helped students catch up from the large declines in test achievement they experienced during the pandemic (National Center for Education Statistics, 2022a, 2022b).

Whether ESSER improved student achievement is a significant policy question. In the near term, evidence about the effectiveness of ESSER funding could inform state leaders' decisions about whether to provide additional funding for school systems after ESSER ends in September 2024.[2] More broadly, ESSER's impact speaks to long-standing debates about extent to which increased school funding leads to improved student achievement (Hanushek, 1989, 1994; Hedges et al., 1994). The most recent research on the question "does money matter?" finds increased spending improves outcomes such as test scores, educational attainment, wages during

[1] There were three waves of ESSER funding: ESSER I included $13.2 billion and was approved in March 2020 as part of the Education Stabilization Fund through the Coronavirus Aid Relief and Economic Security (CARES) Act; ESSER II included $54.3 billion and was approved in December 2020 as part of the Coronavirus Response and Relief Supplemental Appropriations Act (CRRSA); and ESSER III included $122 billion was approved in March 2021 as part of the American Rescue Plan (ARP) Act (Office of Elementary & Secondary Education, 2024).
[2] Lieberman (2023) provides some insight into a variety of district strategies for managing budget shortfalls including legislative movements to increase funds from state revenue sources, local bond levies, and the re-allocation of granted funds from charitable foundations.

adulthood, and reduces the likelihood of being arrested (Baron et al., 2024; Jackson et al., 2016, 2021; Kreisman & Steinberg, 2019; Lafortune et al., 2018; Papke, 2005). In a meta-analysis of the effects of increased school spending on student outcomes, Jackson and Mackevicius (2024) find that, on average, a $1,000 increase in per pupil spending over four years improves student test achievement by about .03 standard deviations on the test distribution. They also document that these effects are larger for higher poverty student populations.[3] Could ESSER funding have produced gains of similar magnitude?

In this paper, we answer that question by estimating plausibly causal impacts of ESSER funding on district-level achievement from 30 states. We instrument for ESSER per pupil funding using the share of children in a district's geographic area counted as formula-eligible for Title I funding. This is a main determination for Title I, though, as we describe below, the allocations also depend on other features of districts and states (Gordon & Reber, 2023). The identifying assumption of our design is that, after we control for district resources, poverty levels, and demographic characteristics in 2022-23, differences in funding attributable to the particular measurement of formula-eligible children are plausibly exogenous to the characteristics of the school district and the students served by those districts. For instance, the data on formula-eligible children (FEC) and formula-eligible percent (FEP) used to determine Title I are reported on a lag such that data from 2018 determined Title I funding for the 2020-21 school year.[4] This delay translates to variation in funding that is explained by changes in poverty

[3] They found little evidence of heterogeneity across capital and operating spending, for different spending levels, or by geography. See also Handel and Hanushek's (2023) meta-analysis, which reaches similar conclusions about the overall impact of increases in school spending.

[4] As we describe in more detail below, ESSER is allocated proportional to Title I funding, which has allocation rules that permit us to isolate exogenous changes in school district spending from unobserved factors that may influence student achievement. For clarity, and following the convention of Gordon and Reber (2023) we refer to the percent of children who are formula-eligible in district as the formula-eligible percent (FEP) and the number of formula-eligible children as the formula-eligible count (FEC).

over time and not reflective of current-year poverty conditions. We leverage variation in funding due to this reporting delay, differences between community demographics and those of students enrolled in public schools, and measurement error inherent in the FEC and FEP data to identify the impacts of ESSER on student achievement.

We find significant effects of ESSER funding on student achievement in math, though these effects are attenuated in models that include state fixed effects. This suggests that state-level factors significantly influenced academic recovery. The findings align with the idea that both the allocation of funds and their effectiveness vary across different contexts, potentially influenced by factors such as school accountability measures or union presence (Handel and Hanushek, 2024; McGee, 2023). Here, however, we find no evidence that the allocation of funds (across very broad spending subcategories), or other specific state-level factors, influenced the relationship between ESSER and student achievement. The pattern of findings for English Language Arts (ELA) tests are similar, though the ESSER effects are not significant in our preferred specification. In our preferred models, which do not include state fixed effects, the point estimates suggest that a $1,000 increase in ESSER funds per pupil resulted in a statistically significant increase of about 0.008 standard deviations in math and an insignificant increase in ELA achievement of the same magnitude. These estimates are broadly consistent with Jackson and Mackevicius (2024), whose pooled estimated average effect of a $1,000 increase in per-pupil spending over four years is about 0.032 standard deviations, or approximately 0.012 in the first year, assuming a linear increase over time.[5]

[5] Jackson and Mackevicius (2024) detail in their online Appendix C how their results are robust to their assumption of linear impacts across time when they adjust estimates to be on the same scale. As such, we think considering the impacts that would be implied by a linear time trend in the first of four years is appropriate.

We also find evidence of heterogeneity of ESSER effects. The estimated impacts of funding are significantly larger for lower-spending districts compared to their higher-spending counterparts. ESSER had a more substantial impact on districts serving predominantly non-Black and non-Hispanic populations. Our estimates are far larger in districts ranking in the lowest quartile for Black and Hispanic enrollment compared to those in the top quartile, where the impact was not statistically significant. Consistent with this, we also see larger effects in towns and rural areas, which serve larger shares of White students in our sample. There is little evidence that any of these results are related to changes in staffing ratios or the allocation of ESSER spending across capital or facilities, labor, supplies or materials, and contracts or purchased services.

Our work extends the prior research on funding that relies on plausibly exogenous sources of identifying variation in several ways. First, prior causal work has relied on identification based on variation in spending arising from school equity and adequacy lawsuits (e.g., Jackson et al., 2016) or variation within single states (e.g., Kreisman and Steinberg, 2019). We instead rely on exogenous variation linked to Title I funding, which is distinct both in centering federal revenue variation instead of state or local revenue and in allowing us to assess the impact of spending across and within states and across a much broader set of school districts.[6] Second, because ESSER funds are distributed proportionately to Title I, we focus on the neediest school districts (Fahle et al., 2023; Kuhfeld et al., 2022), a margin of analysis distinct from past research. Finally, to our knowledge, this is the only study that focuses on a

[6] Cascio et al. (2013) examine the effect of the introduction of Title I in Southern states on future spending and student dropout rates. Matsudaira et al. (2012) look at the effects of variation in Title I funds received at the school-level in a large, urban district using a regression discontinuity. Little other research appears to explore the impact of federal funding variation on student achievement deploying causal methods, but Goldhaber et al. (2024) exploit the same source of variation that we do here in assessing how ESSER funding affects school district staffing decisions.

funding increase that is time limited. In the wake of COVID-19, districts have strong incentive to spend a large amount of money on short order, obligating the last wave of ESSER funding by September 2024.[7]

2. The Allocation of ESSER Funding and Our Identification Strategy

Central to our study is the fact that the federal government allocated ESSER funding in proportion to Title I, with the intent of providing more support for higher-need schools and districts. For example, while the Detroit, MI public school district received about $25,800 per pupil across all waves of ESSER (about $16,600 of which was from ESSER III), Grosse Pointe, MI (a nearby suburb) only received about $860 per pupil ($560 from ESSER III). This allocation policy creates an obvious challenge for assessing ESSER's impact because the government allocated funds non-randomly. Our concern is that districts with higher allocations of ESSER may also have distinct underlying needs that relate to both funding and achievement. For example, higher-poverty districts tend to be in communities that had higher rates of COVID infections and deaths (Chen & Krieger, 2021; Finch & Hernández Finch, 2020; Karmakar et al., 2021), more negative impacts on mental health and well-being (Hall et al., 2022), higher unemployment (Tang et al., 2022), greater likelihood of child maltreatment (Wolf et al., 2024), and more remote schooling (Goldhaber et al., 2022). These and other factors likely caused greater learning loss during the pandemic and dampened academic recovery. If, in addition, these factors are correlated with the factors that influence ESSER allocations or spending, an ordinary least squares (OLS) estimate of the effects of ESSER spending will be biased.[8]

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[7] As we describe in more detail below, there is relatively little systematic evidence about how school districts used ESSER funding outside of broad expenditure categories. Moreover, official reports provide limited evidence on the resources that were purchased *because of* the additional ESSER funding given the fungibility of school spending (Gordon, 2004).

[8] For instance, there is evidence that more time spent in remote schooling is associated with the chronic absenteeism that is making academic recovery challenging (Dee, 2024; Goldhaber et al., 2023).

A naïve estimate of the impact of ESSER on achievement could be biased if states or communities invested in academic recovery in ways that are not accounted for by the controls in the model but are correlated with ESSER allocations.[9] For instance, district allocations are held harmless when districts experience declines in FEC that yield a drop in their Title I funding. Their allocation is held harmless at between 85 and 95 percent of their previous year Title I allocation (Gordon & Reber, 2023). A concern is that the likelihood of being held harmless could be correlated with achievement changes not well-captured by factors accounted for in statistical models. For instance, as a district becomes more affluent, low-income families could be priced out of the district, triggering hold harmless provisions, i.e., Title I allocations do not decline proportionately to the drop in FEP. If this increasing affluence is correlated with student achievement, then the OLS estimates would be upwardly biased.

We can address our concerns about bias by exploiting variation in ESSER attributable to its allocation rules and their relationship to Title I.[10] Title I combines four grants, each with distinct rules determining eligibility and weighting adjustments. The core determinants of each Title I grant, however, are the number of FEC in a district's geographic area and the percent of the local population that count represents (formula-eligible percent, FEP). Given geographic differences in poverty, there is considerable heterogeneity in how much ESSER funding districts receive. For example, the mean ESSER allocation per pupil in high-poverty states like

[9] As an example, if states or communities invested in mental health initiatives for students that targeted low-income areas and are funded outside of district budgets—one report estimates that as much as $1 billion in state-reserved funds were planned to target mental health and well-being supports (Council of Chief State School Officers, 2022, 2024)—the impacts of that programming would be correlated with ESSER and also impact achievement.

[10] It is worth noting that in some states these grant allocations are not 100% proportional due to the distribution of state-reserved ESSER funds to districts. In Washington state, for example, the state education agency granted each district that did not receive Title I funding in the pandemic years (and thus would not have otherwise received ESSER funds) with $50,000 of ESSER funds from the state withholding.

Mississippi, about $6,700, is almost four times greater than the mean in the low-poverty state of Connecticut, which received about $1,800 per pupil.

Our identification strategy hinges on Title I determinations and their timing. To help clarify our approach, we present a timeline of the relevant Title I determinations, associated data for FEC and FEP, and impacted ESSER allocations in Figure 1. As Figure 1 shows, ESSER I allocations in 2020 were determined by Title I allocations for the 2019-20 school year, which themselves were anchored to FEC and FEP data from 2017. Similarly, ESSER II and III allocations in 2020 and 2021, respectively, were determined by Title I for 2020-21, which was anchored to FEC and FEP data from 2018. Because of the staggered timing, districts had spent most of ESSER I and more than half of ESSER II by the start of the 2022-23 school year, our analysis focuses on ESSER III. Importantly, the Title I allocation formula and process can lead to meaningful differences in funding per FEC in several ways (Gordon & Reber, 2023). First, state minimums (a guaranteed funding floor) inflate the Title I dollars allocated per FEC in less-populated states relative to more densely populated states. Second, dollars allocated per FEC are adjusted based on state per-pupil expenditure (SPPE), so states that spend less money per student have, all else equal, lower allocations per FEC. Third, Education Finance Incentive Grants (EFIG)—one of the four grants under Title I—adjust state allocations according to SPPE relative to state per capita income and also how equally funds are distributed across districts, so allocations per FEC differ across grants. Finally, Title I allocations also vary per FEC within states due to (1) differences in the weighting of FEC for allocations above certain FEP thresholds and (2) adjustments to allocations to maintain hold-harmless provisions.[11]

[11] Gordon and Reber (2023) delve into these factors in greater detail, but generally, allocations are determined through an iterative process whereby districts are guaranteed to maintain some level of their prior-year funding if their initial allocation declines (due, for example, to falling below the qualification threshold of a grant). Because these districts then receive a "boost" from their initial allocation to remain at a certain level, and the overall budget

As we describe in more detail below, the key mechanism we rely on to estimate the effects of ESSER on student test achievement is the fact that ESSER allocations are determined by *prior* measures of district poverty (based on the FEC and FEP data year) and less correlated with district poverty in the years when the Title I/ESSER funds were disbursed. Our central assumption is that *conditional on district characteristics*—such as demographics and the share of free/reduced price lunch (FRPL) eligible students in the 2022-23 school year—the extra funding districts receive because of the above allocation rules are uncorrelated with unobserved district-level factors that influence student achievement.[12]

We provide a visual illustration of this source of cross-sectional identifying variation in Figure 2, in which we plot ESSER III allocations per pupil against the district FEP in Panel A and the relationship between district FRPL eligible students and FEP in Panel B. Panel A illustrates that while there is a positive correlation between FEP and ESSER allocations (0.64 for the school districts in the figure and 0.86 in our sample, described below), there is also variation in allocations for districts with the same FEP. This occurs because of differences across states, hold-harmless adjustments, and kinks in how FEC are weighted to determine allocations. Panel B, which plots 2018 FEP (which determined Title I in 2020-21) against the percentage of FRPL eligible students in each district in 2022-23, shows a much noisier connection between FRPL and FEP. The time lag in FEP leads to a mismatch with current-year poverty levels, but this noise also comes from differences in the demographics of children in a district area relative to children enrolled in the district. Additionally, mismatches between these two measures of poverty come

constraint of the program, districts unaffected by hold harmless clauses must make up the budget gap by receiving funds slightly lower than their initial allocation.

[12] We use data on FRPL eligibility from the Common Core of Data which is noted as a flawed source in part because if districts participate in the Community Eligibility Program (providing free lunch to all students regardless of eligibility) then participation counts overstate eligibility in that district (National Center for Education Statistics, 2020). Because FRPL is thus top-constrained in some districts we additionally re-estimate our models dropping those censored districts—a robustness check we describe in greater detail below.

from the fact that while FRPL numbers are, with the exception of Community Eligibility Programming, a reflection of the *observed* share of enrolled students in the district, FEP and FEC are *estimates* calculated based on several data sources and thus inherently have some amount of error (US Census Bureau, 2023).

3. Data and Empirical Specifications

3.1 Data Sources & Measures

Our analyses rely on four main sources of publicly available data: (1) district characteristics such as demographics, staffing, finance, and enrollment from the Common Core of Data (CCD); (2) district-level FEC and FEP in 2018 used to determine Title I allocations for the 2020-21 school year and for constructing our instrument;[13] (3) ESSER allocations from the ESSER Expenditure Dashboard, maintained by the Edunomics Lab at Georgetown University, with some exceptions we describe below; and (4) data from the Stanford Education Data Archive 2023 (SEDA) to measure student achievement.[14] The SEDA data include student test data from 30 states, spanning 5,689 school districts for math and 5,331 school districts for ELA. Note that the SEDA data are missing several large, populous states including Texas and New York, so the findings we describe may not generalize. We provide more details about SEDA data and the other datasets below and list the included states by subject in Appendix Table A.1.

For general information about districts, we use files from the CCD. Specifically, we use CCD data to observe district type (e.g., charter, traditional district, etc.), urbanicity, enrollment, and student demographics, all for the 2022-23 school year, and finance variables, such as total

[13] These data are available upon request from the Department of Education Office of Formula Grants/School Support and Accountability.
[14] We additionally use data on instruction modality at the district-by-week level from the Return to Learn Tracker maintained by the American Enterprise Institute (2021). The sample for this dataset is limited to districts with three or more schools.

revenue per pupil for the 2019-20 school year. Because the CCD maintains records for all districts in the country, these data allow us to describe how our analytic sample of districts compares to all other districts in the U.S. and consider heterogeneity within districts we observe. Most important to our analysis are the CCD data on FRPL qualification rates in a district. These data are top-censored at 100 percent of students in high-poverty areas qualifying for the Community Eligibility Program (National Center for Education Statistics, 2020). Due to this constraint, we assess the validity of our results using two alternative measures of poverty: 1) estimates maintained by SEDA of the share of students qualifying for FRPL for the 2021-22 school year that are corrected for top-censoring due to participation in the Community Eligibility Program;[15] and 2) the National Center for Education Statistics' (NCES) school neighborhood poverty estimates for the 2020-21 school year (Geverdt & Nixon, 2018), which takes the average income-to-poverty ratio across schools within each district.

We connect the CCD data on the universe of public school districts in the U.S. to data that the Department of Education (ED) leverages to determine Title I funding. Specifically, the Title I formula used to determine federal allocations to districts is based predominantly on the FEC and FEP in that district.[16] The Census' Small Area Income and Poverty Estimates (SAIPE) data present counts of children between the ages 5 and 17 who are in poverty in a district's geographic area which are estimated using a combination of IRS tax return data from the prior year and 5-year American Community Survey (ACS) data (US Census Bureau, 2023). Because SAIPE data are published years after they are collected, and Title I funding is determined prior to

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[15] SEDA describes their process for constructing these covariates and correcting for Community Eligibility censoring in detail in their documentation for SEDA 4.1 (Fahle et al., 2021).

[16] Formula-eligible children include the following categories: children between the ages 5 and 17 who are in poverty in a district's geographic area, children qualifying for Temporary Assistance for Needy Families (TANF), neglected and delinquent children, and foster children.

the start of each school year, Title I funding uses the most recent (but still lagged) data available to determine allocations. For the 2020-21 school year, in which ESSER II and ESSER III were determined, ED used 2018 SAIPE data to determine the number of FEC in the poverty category. For this study, we use FEP (predominantly consisting of 2018 measures of children in poverty) to instrument for ESSER allocations per pupil enrolled in the district.

To observe district-level ESSER allocations across all three funding waves nationwide, we predominantly use data maintained by the Edunomics Lab at Georgetown University (Edunomics Lab, n.d.). We matched states whose ESSER data did not include district IDs (either state or LEAID) to our CCD data using district names. In a few cases where reported ESSER funds were disaggregated across multiple schools or entities within a district, we aggregated allocations to the district-level. Three states did not have complete data on ESSER allocations available from the Edunomics Lab dashboard: Oklahoma, Rhode Island, and Utah. For Oklahoma and Utah, we instead use publicly available data from the National Education Association; for Rhode Island we use ESSER allocation data reported by the Rhode Island Department of Education.

We additionally use data from the Edunomics Lab dashboard to describe district-level ESSER spending across four main categories: capital or facilities, labor (salary/benefits), supplies and materials, and contracts or purchased services.[17] These categories are the most consistently observable across states that report disaggregated spending (Edunomics Lab, 2023). We use data on spending across these categories for the subsample of districts for which we observe it in our analyses of potential mechanisms.

[17] This categorization involves some aggregation across categories that varies by state. We followed any aggregation patterns noted in the Edunomics documentation (Edunomics Lab, 2023) and tried to apply those same judgements consistently across other states if not noted. Our final crosswalks of reported file categories to the umbrella bins we use in this analysis are available upon request.

While the Edunomics Lab also collected and aggregated data on overall district ESSER spending, we privilege ESSER allocation data for our main variable of interest for two main reasons. First, allocation data circumvent the potential issue of reverse causality and fungibility inherent in spending data. For example, districts could plan programming or expenses and then charge that programming to ESSER even though it would have existed in the absence of additional funding. Second, it has been an ongoing question whether districts will be able to spend the monumental investment of ESSER in the allotted time (Lieberman, 2024).

For the 2018-19, 2021-22, and 2022-23 school years, SEDA has published district-by-subgroup-level achievement data as well as district covariates (Reardon et al., 2024). Due to limitations and sparsity of test score data in the pandemic, we treat the gap between achievement in 2018-19 and 2021-22 as a measure of learning loss experienced during the pandemic and changes from 2021-22 and 2022-23 as a measure of post-pandemic recovery. This follows the convention established by the authors of this dataset (Fahle et al., 2024). These data depend on two main sources: for 2018-19, achievement data is from the EDFacts database; for 2021-22 and 2022-23, the SEDA team collected state-reported data via webscraping and outreach to state education offices. The SEDA data are limited in several ways that impact the states and districts we can include in our analytic sample. The 2018-19 and 2021-22 dataset excludes 10 states whose data either consisted of too few categories of achievement or is not reported at the necessary level of aggregation. Data from 2022-23 exclude an additional 10 states, mostly due to changes in state tests or proficiency cutoffs. Districts throughout the country may also be left out of the SEDA data due to missing data (i.e., only one of the two considered school years reported), a change in their Local Education Agency ID (LEAID), if more than 40 percent of their students took alternative assessments, if the district does not have geographic boundaries (e.g., charters, specialized districts), or if their data are suppressed due to low levels of enrollment.

Our primary results center on ESSER III allocations due to the timing of these grant disbursals and our period of interest. ESSER III was signed in March 2021. Prior to that, districts had already received and were using $68B from ESSER I and II. With the addition of ESSER III, federal relief funding totaled $190B. By the start of the 2022-23 school year nearly all of ESSER I and over 56 percent of ESSER II had been spent,[18] though the actual amount might be higher given reporting delays in ESSER spending. This means most of the impact of ESSER II was likely concentrated in prior school years. Nevertheless, as we describe below, we present estimates that allow for the possibility that earlier waves of ESSER also impact student achievement in the 2022-23 school year.

Because our analysis centers on the impact on 2022-23 school year achievement, impacts of any ESSER spending prior to testing in 2021-22 are implicitly included in 2021-22 achievement, which we include as a cubic in all models.[19] We think it is unlikely, however, that much impact of ESSER III shaped scores in 2021-22, since ESSER spending in most districts did not appear to pick up until the end of 2022 (FutureEd, 2023; Gartner, 2023).[20] Again, we test our results for robustness to this design choice below.

Given the SEDA data restrictions, we observe student achievement for 5,632 districts in math and 5,278 districts in ELA across all three school years, representing 54-56 percent of

[18] We thank Maggie Cicco from Edunomics for providing this figure.
[19] Despite the surge in funding, research suggests districts were still ramping up academic recovery programs early in the 2021-22 school year as they confronted a host of implementation challenges (Carbonari et al., 2022).
[20] One potential concern with controlling for lagged achievement is that if we believe ESSER III may be affecting 2022 test scores we may be overcontrolling for the impacts of ESSER III (Elwert & Winship, 2014), thus interrupting the causal path between our control for ESSER III and our outcome of interest. That said, if we believe increased funds may have increased achievement, this control may bias our estimates down because we are attributing some of the positive gains from having funds awarded to our lagged achievement controls.

nationwide student enrollment. In Table 1 we present descriptive statistics for districts that are in our analytic sample and those that are not in our sample due to data availability. We weight all averages by student enrollment, so we can interpret these characteristics as those of districts the typical student in our sample attends. Students in our sample, on average, attend districts that received slightly higher ESSER allocations per pupil and served fewer enrolled students than districts excluded from our analysis. Students in our sample also attend school with more students identifying as Asian, Black, Multiracial, or White and less likely to identify as American Native, Hawaiian/Pacific Islander, or Hispanic than those outside of our sample. Students in our ELA sample are also attending districts with lower shares of FEP, are more likely to be in a suburb, and are less likely to be in a rural area than districts excluded from this analysis.

Appendix Table A.2 describes the districts in the sample by quartile of ESSER III per pupil allocations. Districts receiving greater ESSER allocations per pupil were both higher-poverty districts and experienced larger learning loss between the 2018-19 and 2021-22 school years than their relatively lower-poverty peers. For instance, districts in the top quartile of per pupil ESSER III allocations (receiving an average of over $4,000 per pupil) had nearly 67 percent of students eligible for FRPL and had learning losses of over .18 standard deviations in math and .11 standard deviations in ELA. By contrast, districts in the lowest quartile of ESSER III allocations (receiving about $500 per pupil) had less than 25 percent of students eligible for FRPL and smaller learning losses, about .11 standard deviations in math and .07 standard deviations in ELA. Districts across the distribution of ESSER allocations also differ in terms of urbanicity and the type of students served. High allocation districts, for instance, are more likely to be in cities and serving higher shares of Black and Hispanic students.

3.2 Empirical Approach

As a point of comparison, we estimate the naïve relationship between ESSER funding and achievement using an OLS model:

$$Y_{ij}^{2023} = \beta_0 + \beta_1 ESSER_i + \beta_2 Y_{ij}^{2022} + \beta_3 X_i + \gamma_j + \varepsilon_{ij} \qquad (1)$$

Where we predict average achievement in spring 2023 separately for math and ELA (Y_{ij}^{2023}) in district *i* in state *j* as a function of the district's total ESSER per pupil allocation ($ESSER_i$), a cubic of prior-year achievement in that same subject (Y_{ij}^{2022}), a suite of district characteristics (X_i), and state fixed effects in some models as indicated (γ_j). Namely, our additional controls from the 2022-23 school year include district proportions of FRPL, district proportions of student race, indicators for district urbanicity, and district total revenue per pupil in 2019-20.

Applying the instrumental variable strategy introduced earlier, our primary empirical model takes the form of a two-stage least squares regression. Specifically, our first stage takes the following form:

$$ESSER_{ij} = \beta_0 + \beta_1 FEP_i^{2021} + \beta_2 Y_{ij}^{2022} + \beta_3 X_i + \gamma_j + \varepsilon_{ij} \qquad (2)$$

In words, we predict ESSER per-pupil allocations in district *i* in state *j* as a function of the formula-eligible percentage of children (FEP_i^{2021}) in that district used to determine Title I funding in the 2020-21 school year,[21] and all of the covariates we include in model (1) to predict achievement.[22] Namely, these include a cubic of lagged achievement from 2021-22, the same

[21] We additionally estimated these models using both FEP and FEC as instruments for ESSER. This did not meaningfully change our results, however the model failed a test for overidentification (Basmann, 1960; Sargan, 1958) and therefore we only present results using FEP alone. It is also worth emphasizing here that while we control for district characteristics such as demographic representation, these factors are not included in the ESSER formula and thus do not determine allocation amounts.

[22] We weight the regressions based on district enrollment; the findings are nearly identical if instead we weight by the number of test takers in each district.

vector of district characteristics as above (X_i), and state fixed effects for some models (γ_j).[23] We estimate this model separately for the districts for which we observe math and ELA outcomes.

We present the results from this first stage regression in Table 2. The estimated findings suggest that a 10 percentage point increase in FEP in a district increases the average ESSER III allocation per pupil in by roughly \$1,317 in math and \$1,345 in ELA.[24] One final detail worth noting is that the R^2 of these models is not 1 because adjustments to Title I (and ESSER) allocations are made for a variety of reasons including how current-year allocations compare to past allocations. Our estimates fail to capture variation in allocations attributable to hold harmless provisions, which sustain some amount of district funding despite declines in the formula-determined allocation amount.

To formally assess our theoretical argument that ESSER funds are endogenous to student achievement, we additionally conduct a Durbin-Wu-Hausman test (Durbin, 1954; Hausman, 1978; Wu, 1973), finding that our first-stage residuals are a significant predictor of test scores. It is also worth noting that FEP appears to be a strong instrument, returning an *F*-statistic of 344 and 348 for the math and ELA samples, respectively.

Having assessed the integrity of the instrument, we estimate our two-stage least squares (2SLS) model as follows:

$$Y_{ij}^{2023} = \beta_0 + \beta_1 \widehat{ESSER_i} + \beta_2 Y_{ij}^{2022} + \beta_3 X_i + \gamma_j + \varepsilon_{ij} \qquad (3)$$

[23] Our results throughout are robust to additionally controlling for lagged, pre-pandemic achievement from spring for 2019.

[24] When we only draw comparisons within states, this same change in FEP increases ESSER III allocations per pupil by \$1,334 in math and \$1,354 in ELA.

Where all variables included are as defined earlier. Note that for all models we use 2023 year-scale achievement in math and reading from SEDA as our outcomes (Y_{ij}^{2023}), controlling for lagged achievement in the same subject in 2021-22.

As we note above, the identifying assumption of this model is that, *once we control for 2022-23 district-level characteristics*, FEP in a district should only affect test score achievement through its determination of ESSER allocations. FRPL eligibility and formula eligibility are governed by slightly different definitions. Specifically, students qualify for FRPL if their household income is lower than 1.85 times the federal poverty line (again, in schools implementing the Community Eligibility Program all students are counted as FRPL-eligible). Title I formula eligibility, by contrast, is determined by the Census Bureau's estimates of youth in a district's geographic boundary between the ages 5 to 17 whose household income is below the poverty line in addition to children in foster care, those qualifying for Temporary Assistance for Needy Families (TANF), and delinquent children. Figure 3 demonstrates how some states like Mississippi are greatly impacted by Community Eligibility Programming and how the relationship between these measures differs across contexts.

This model leverages three main sources of identifying variation: first, the time lag in reporting means that estimates of formula-eligible children are from 2018; second, not all children included in estimations of FEC and FEP are enrolled in public schools; third, measurement error in the estimates that comprise FEC and FEP. Of these three, we can only observe the first source of variation using data available. Specifically, we estimated an alternative specification where we use the change in FEP between 2018 and 2022 as an instrument, controlling for FEP in 2022 instead of FRPL. The F-statistic on the differential FEP instrument was only 16.8, implying a weak instrument (Lee et al., 2022; Young, 2022). As a result, our estimates were meaningfully less precise and smaller in magnitude, suggesting that it is the combination of these sources rather than this variation alone which identifies our relationship of interest. We additionally test a specification where we include the interaction between FEP and state fixed effects in the first stage to allow for variation in the allocations per pupil to vary more flexibly across states, but do not find meaningfully different results.[25]

As a bounding exercise for our estimates, we also estimate these models inputting a range of values for ESSER allocations. While our preferred estimates use ESSER III allocations, the collinearity between the distribution of each wave of ESSER and Title I allocations implies that we cannot use this estimation strategy to isolate distinct effects across these grants. As such, estimating this model using the smallest dollar value across these grants (ESSER I) and the combination of all grants (total ESSER) simply changes the scale of our estimated effect.

4. Results

In Table 3 we present OLS and 2SLS estimates of the effect of increases in school spending on spring 2023 state assessments in math (Panel A) and ELA (Panel B). We show results with and without state fixed effects. The OLS estimates leverage all sources of variation in ESSER III allocations whereas the 2SLS rely on variation that is driven by the three sources of identifying variation we outline above. While we prefer to control for 2022 levels of achievement, our results are robust to controlling for both 2019 and 2022 achievement.[26]

[25] Using this alternative instrument, we find the impact of ESSER in math is 0.007 (significant at the five percent level) and in ELA is 0.013 (significant at the five percent level. While this ELA estimate is larger in magnitude than our main results, our standard errors for this estimate are similar across both subjects and the estimates are not statistically significantly different from what we show in Table 3 column 4.

[26] We also estimated this model controlling for cubics of 2022 achievement for both subjects and find slightly larger (0.010 in our 2SLS model without state fixed effects, significant at the 5 percent level) estimates for ELA than in our main results. Our preferred point estimate for math is marginally significant in this specification and unchanged in magnitude (0.008)

We begin by presenting the naïve OLS estimates, which are statistically significant in both math and ELA. Without controlling for district demographics and urbanicity (column 1), our point estimates suggest that increases of $1,000 per pupil in ESSER allocations increase student math and ELA achievement by 0.010 and 0.011 standard deviations (SDs) respectively. These estimates attenuate once we include district covariates (column 2) and even more so in specifications that include state fixed effects (column 3); the point estimates are no longer statistically significant in the state fixed effects specification for math and only marginally significant for ELA.

Column 4 of Table 3 provide estimates from our preferred 2SLS specification. We prefer this model because it is based on variation between and within states and addresses our concerns that the OLS models may be biased by relying on plausibly exogenous variation in funding. Relative to the OLS estimates in column 1, the 2SLS point estimate for math, 0.008 SDs, is slightly smaller in magnitude.[27] In ELA (Panel B), we also observe slightly smaller ESSER estimates when using the 2SLS approach; note, however, that the imprecision of the estimates suggests we cannot rule out ELA achievement gains that are a similar magnitude as those reported in the Jackson and Mackevicius (2024) meta-analysis.

In column 5 we report the findings in the 2SLS specification that includes state fixed effects in both the first and second stages, which leads to attenuation of the point estimates. In the case of math achievement, the estimate is about a third as large and in ELA it is less than a tenth as large. For the results in column 4 and 5 we additionally test for whether our estimated

[27] We also estimate OLS models that include controls for our instrument, FEP as well as the average state-per-pupil revenue, excluding revenue from federal sources, between 2016-17 and 2018-19, which is a more minor input to the Title I funding formulas (Gordon & Reber, 2023). In theory, when we account for both FEP and SPPE, the remaining variation captured by the ESSER allocation control on its own should be primarily from kinks in the formula and hold harmless adjustments. Our estimates of the association of ESSER and achievement closely resemble those presented in column 2 of Table 3 (0.005 in math with district covariates and 0.009 in ELA; the estimate for ELA is not statistically significant).

impacts of ESSER on achievement are statistically significantly different from those estimated in our analogous OLS models (columns 2 and 3, respectively); we find no evidence that these coefficients are statistically distinguishable from one another (Clogg et al., 1995). It is challenging to know what to make of the difference in findings between models with and without state fixed effects. Specifically, their inclusion removes some of the variation in ESSER spending and achievement that diminishes the precision of our models as is apparent in Table 3.

To further explore the differences in our results with and without state fixed effects, we test the robustness of the ESSER estimate to the inclusion of a variety of state-level controls in Appendix Table A.3. Specifically, we include controls that may be indicative of state-level policies and/or the potential that other contextual factors may have influenced academic recovery: the district share of students that were remote or hybrid in the 2021-22 school year; a measure of youth access to computers and internet according to 2018-22 ACS estimates; state-level COVID cases and deaths from March 2020 through August 2022 ; the share of votes for Biden in the 2020 election; the state unemployment rate for the 2019-20 through 2021-22 school years; the total revenue going toward K12 education in 2019-20 at the state level; and right to work states as of 2024.[28] If these factors are correlated both with student outcomes and ESSER funding, our ESSER effect estimates would be biased. For instance, the amount of time that students spent in schools in remote or hybrid status during earlier years of the pandemic is

[28] The data on time of the 2020-21 school year spent in-person, in remote learning, or in a hybrid format are from the Return to Learn Tracker maintained by the American Enterprise Institute (2021). The data we use to measure student computing and internet access is from the National Center for Educational Statistics EDGE database records for 2018-2022 American Community Survey Education Tabulation for the children population (National Center for Education Statistics, 2022c). We use data on state-level COVID cases and deaths from the Centers for Disease Control and Prevention from March 2020; we aggregate cases and deaths within each state and school year, defined as starting in September (Centers for Disease Control and Prevention, 2024). Data on democratic vote share are from the CNN presidential election results for 2020 (CNN, 2020). Data on the unemployment rate are collected from Data Commons, which formats longitudinal state-by-month data from the Bureau of Labor Statistics (Data Commons, 2024). K12 finance data is from the CCD. We indicate Right to Work states according to the National Right to Work Legal Defense Foundation (2024).

associated with chronic absenteeism, which has been seen as an impediment to remediating learning loss (Dee, 2024; Malkus, 2024). School modality was also found to vary across district poverty levels (Goldhaber et al., 2023), which of course influences ESSER funding. Hence, failure to account for school modality might be expected to bias ESSER funding estimates downward. There is also evidence that how schools spend new funding is influenced by union power (Brunner et al., 2020), which may shape accountability standards (Strunk & Grissom, 2010).

Here we find that our results are largely robust to the inclusion of these state-level controls. Scanning across the columns in Table A.3, there is relatively little change in the estimated ESSER effect when any of the controls are entered independently, though the ESSER coefficient in math is attenuated (.005) and not statistically significant when all the controls (in column 8). For ELA, there is little change in the ESSER coefficient when we include controls individually, and it is marginally significant when all controls are in the same model.

5. Robustness, Bounding, and Heterogeneity

Because our identification strategy hinges on the inclusion of controls for current-year district characteristics, we assess the robustness of our results to the inclusion of alternative measures of district poverty in Table 4.[29] Our primary estimates (column 1, repeated from column 4 of Table 3) use the share of students in a district in 2022-23 qualifying for FRPL lunch according to the CCD to capture contemporaneous district poverty. If instead we use the SEDA

[29] We have also assessed the robustness of our results to controlling for multiple years of lagged scores, controlling for lagged scores as linear terms instead of cubics, and a variety of alternative specifications which are available upon request. Our results are almost identical when we weight by the number of students tested (i.e., how many tests contribute to the average scores) instead of total district enrollment. When we our results by the precision of the outcome estimate (i.e., the inverse of the standard error of the achievement outcome) we get slightly larger magnitude of estimated impacts which are more precise. Generally, we find that while our models struggle with precision, our point estimates tend to fall in a somewhat consistent—if not statistically indistinguishable—range from one another.

control for the share of students in a district qualifying for free/reduced-price lunch in 2021-22 (correlation with CCD FRPL of 0.77), we find that our results are slightly attenuated for both subjects and no longer statistically significant in math. Second, we use the average neighborhood income-to-poverty ratio amongst schools in each district from NCES (correlation with CCD FRPL of -0.53).[30] With this we find similar results for math and slightly attenuated, statistically insignificant, results for ELA.

There are several reasons to question which waves of ESSER spending might impact 2022-23 school year achievement. In practice, it is difficult to pin down precisely when ESSER allocations are spent (Silberstein & Roza, 2023), in part because reporting tends to be delayed and varies across states. As we noted above, estimates suggest that about half of ESSER II was unspent in September 2022, suggesting that ESSER II could have impacted achievement in the 2022-23 school year. On the other hand, ESSER III allocations were not fully spent at the end of 2023 (districts had until the end of September 2024 to obligate the funds).[31] Thus, to bound the estimates, we present models instrumenting for each distinct wave of ESSER and the sum of all waves in Table 5. These results serve to illustrate how we cannot distinguish differential impacts across ESSER waves due to their collinearity with each other. ESSER III funds represent about two thirds of all ESSER funding. If we are skeptical that our results may be misattributing impacts to too few dollars (i.e., the true impact we capture is the combined ESSER waves and Title I, or roughly $206 billion), then the estimates for our preferred model (column 1) would be 1.68 times too large. It follows that our results are smaller in magnitude, though still significant

[30] Note that the NCES income to poverty measure decreases in the case of greater poverty so we expect a negative correlation with FRPL measures.
[31] This problem could potentially be solved if we instead used data on ESSER spending across each wave, thus only scaling impacts by the amount of funds that we know have been spent. That said, differences in spending rates themselves might be endogenous to our outcome of interest and these data introduce challenges with consistency in reporting relative to when funds are spent.

in math, when we re-scale using the sum of all ESSER waves. We find that a $1,000 per pupil increase in total ESSER allocations yields a significant 0.005 increase in math achievement and an insignificant 0.005 increase in ELA—about 60 percent of our preferred estimates, as expected. If, however, our logic that controlling for 2021-22 achievement captures potential impacts of ESSER I and II on achievement, the only remaining source of undercounting funds comes from Title I, which would suggest our results are only 1.13 times too large and should be approximately 0.007 for each subject.

Next, we consider whether our estimates mask meaningful differences in the impacts of ESSER funds on achievement across districts and the students they serve. In Table 6, we report a series of heterogeneity analyses by district spending, student characteristics, and urbanicity. There is clear evidence from columns 1 (lowest quartile) and 2 (highest quartile) that students in low-spending districts benefit more from ESSER than those in higher spending districts. In both math and ELA, the ESSER coefficient is largest for districts in the bottom quartile of the per pupil spending distribution and decreases monotonically for districts spending more per pupil. And while the ELA estimates are not statistically significant for the full sample, they are significant for districts in the bottom two quartiles of spending.[32]

The ESSER point estimates are also larger for districts serving *lower* percentages of FRPL (columns 3 and 4) students, but the differences are only statistically significant for ELA and the estimates across quartiles are not significantly different from one another.[33] These findings contrast with evidence derived from the subset of districts in the SEDA data that report

[32] We test for significant differences across quartiles by including indicators for each quartile in our main model with interactions by quartile with ESSER. For math, the impact of ESSER on achievement in the top quartile is significantly different from the bottom quartile at the 1 percent level. For ELA, estimates for the 2nd and 3rd quartiles are significantly different from the lowest quartile at the 5 percent and 1 percent level, respectively; the highest quartile estimate is only statistically significantly different from the lowest at the 10 percent level.
[33] We additionally ran this model using the alternative measures of poverty included and Table 4 and they similarly did not illustrate trends in effects across the distribution of district poverty.

achievement results separately for economically disadvantaged and not economically disadvantaged students. When we examine results in this subset of districts (see Appendix Table A.4), we find that economically disadvantaged students benefit *more* from ESSER spending. The point estimates here suggest that a $1,000 increase in ESSER per pupil raised the test scores of low-income students by 0.013 standard deviations in math—meaningfully larger than our average effects. We find null impacts on ELA for low-income students, although the point estimate suggests a magnitude over 4 times greater than our estimated ELA impact for non-low-income students. It is not clear what drives the divergence in findings for this subset of districts reporting results separately by economic status from the results reported by quartile of FRPL students (in Table 6). One possible explanation could be that districts intentionally targeted resources to lower income students within districts, a strategy that has been observed in practice (Carbonari et al., 2022).

In columns 5 and 6 of Table 6, we report the estimates for districts in the top and bottom quartiles of Black and Hispanic students. Here we see clear evidence of much larger ESSER effects in districts serving *low* proportions of Black and Hispanic students and higher proportions of White students (columns 5 and 6). However, these estimates across quartiles are not statistically significantly different from one another. These findings are broadly consistent for districts serving the lowest quartile of Black students and districts serving the lowest quartile of Hispanic students, which we report in Appendix Table A.5.[34] When we examine SEDA districts that report achievement separately by race, the effect estimates are consistent with the

[34] By quartiles of Black students, both the lowest and 2nd-lowest quartiles have large, positive estimated effects while the top two quartiles' estimates are dampened in magnitude and no longer statistically significant. For quartiles by share of Hispanic students, we find significant, positive impacts only for ELA for the bottom three quartiles with meaningfully dampened magnitudes for the highest quartile. See Appendix Table A.4 for estimates with these categories separately.

heterogeneity by district quartile of Black and Hispanic students.[35] We present these results for districts reporting outcomes by subgroups in Appendix Table A.4.

Finally, we disaggregate our average estimate across district according to their urbanicity in columns 7 through 10. For math, the greatest impact appears to be for districts in towns (0.012 SD), followed by cities (0.008 SD), with insignificant and smaller estimates for districts in rural (column 7) and suburban (column 10) areas. ELA follows a slightly different pattern with positive, significant results first in rural areas (0.018) and towns (0.013), null positive results in suburbs (0.005), and null negative results in cities (-0.003).[36] On balance, these results suggest that towns experienced positive impacts across both subjects that were larger than our preferred estimates and dually significant. Impacts in cities were concentrated in math, while impacts in rural areas were concentrated in ELA. Suburban districts generally experienced smaller, insignificant impacts of ESSER on achievement in both subjects.[37]

The above heterogeneity results may seem contrary to widely held perceptions about the types of students enrolled in different districts. At first glance, for instance, it may be surprising that the magnitude of the effects of ESSER allocations are larger in low-spending districts and also in Whiter (i.e., lowest quartile percent Black and Hispanic) districts. But, of districts in the bottom quartile of pre-pandemic spending in our sample, the average enrollment of White

[35] Note, however, that only a small subsample of districts in the SEDA data report achievement for Black (about 500) and Hispanic (about 1000) students separately.

[36] Testing for significant differences in these estimates, we find that, relative to rural, our estimates for towns are significantly different at the 5 percent level. For ELA, our estimate in suburbs is significantly different from that in rural areas at the 5 percent level.

[37] We additionally tested whether these results were masking heterogeneity within urbanicity groups by district size but found no meaningful differences across subgroups. While not displayed, we also assessed how our results vary across district enrollment on its own, finding that as enrollment increases, we find larger impacts on student achievement in each subject up until we get to the 4th quartile of enrollment, for which our estimates are the smallest in magnitude and no longer statistically significant. One issue that may have stood in the way of large districts using ESSER funds effectively is the difficulty of coordinating COVID academic recovery interventions at a large scale (Carbonari et al., 2022).

students in 56 percent. By comparison, enrollment in the top quartile by pre-pandemic spending is 39 percent White, on average.[38] It also appears that the low-spending, relatively Whiter districts in which we observe the greatest magnitude of ESSER impacts are likely concentrated in towns and rural areas, also aligning with our urbanicity results. We find districts in rural areas have the lowest average pre-pandemic spending, at roughly $13,500 per pupil, and served about 72 percent White students. Districts in towns had the second lowest average spending at closer to $14,000 per pupil and served about 63 percent White students on average.[39] It is important to note that the patterns we observe across district student subgroups do not necessarily imply that ESSER's impacts vary *because* of student demographics. Rather, the results could reflect other district characteristics that happen to correlate with the student populations the districts serve.

6. Mechanisms

In this section we consider some mechanisms that may help explain ESSER's impact, Specifically, we explore whether changed student-to-teacher ratios (STRs) or differences in how districts spent ESSER funding influence the estimated effects of ESSER allocations. Both seem like plausible mediators. As Aldeman (2024) shows, staffing ratios across states have declined since the pandemic. ESSER may have impacted achievement by allowing districts to reduce class sizes or hire support staff. More generally, as Brooks and Springer (2024) show, districts made different decisions about how they planned to spend ESSER. These decisions (or spending strategies) may have been more or less effective. Of course, ESSER spending decisions are endogenous. But if these decisions impacted academic achievement, we nevertheless would

[38] These means are weighted by district enrollment and thus reflect the average demographic composition of students in this subgroup of districts. The share of White students increases monotonically with district pre-pandemic spending, implying that the heterogeneity results by race and pre-pandemic spending are not conflicting but rather capture the same variation in impacts.
[39] Average pre-pandemic spending per pupil was just over $16,000 for districts in cities and suburbs. Suburban districts serve a higher share of White students (48 percent) than those in cities (28 percent White), but both are meaningfully lower than districts in rural areas or towns.

expect to find attenuation of the ESSER coefficient estimates (that is, some of the variation in ESSER that predicts spending would instead be explained by variation in spending patterns).

In Table 7 we estimate models akin to our preferred specification (Table 3 column 4) assessing each of these potential mechanisms. In column 1 we include a control for the change in student-teacher ratio (STR) between 2021-22 and 2022-23, constructed from CCD data. When we consider changes in staffing ratios, the ESSER coefficients remain largely unchanged, although the coefficient on change in STR is statistically significant for math. This result suggests that while changes in staffing are predictive of achievement, they do not appear to be attributable to ESSER. We additionally assessed whether our instrument was predictive of the change in STR control and found a statistically insignificant relationship, underscoring that this does not appear to be a mechanism for our effect of interest.

We also aggregated ESSER III spending data from Edunomics Lab so that we could include controls for the percent of ESSER III allocated funds *spent* on capital and facilities, salaries and other personnel costs, supplies and materials, and contracts or purchased services; we omit the remaining share of funds in a catch-all "other" category (results in column 2). These data are sparsely available, so we replace missing values with the sample mean and include a set of indicators for missingness in each category in our specification. Generally, we find that our results for the impact of ESSER on achievement are statistically indistinguishable from our preferred results, suggesting that patterns of spending (or at least those that we can observe) do not explain-away the impact of ESSER allocations.

7. Discussion and Conclusions

In this paper, we investigate the academic impact of ESSER, the largest one-time federal investment in K12 schools. Consistent with the literature on funding impacts (e.g., Jackson & Mackevicius, 2024), we find that additional ESSER funding leads to student achievement gains.

We interpret our findings as causal because they are based on plausibly exogenous sources of funding variation and are robust to a variety of specification checks.

Our work contributes to the literature on school funding by using a novel identification strategy that exploits allocation rules associated with federal pandemic relief funding. Specifically, we use variation in ESSER allocations driven by historical differences in the share of children in a district's geographic area counted as formula-eligible for Title I to estimate ESSER's impact on achievement. This approach allows us to examine the impact of spending across and within states for a larger sample of school districts than is possible in prior studies that use court-mandated policy shifts to identify spending variation.

Using variation in district achievement within and across states, we estimate that, on average, each $1,000 increase in ESSER per pupil spending led to statistically significant increases in district math test scores of 0.008 standard deviations and a statistically insignificant increase but equivalent increase in ELA scores. These estimates are broadly comparable to the meta-analytic estimates of causal research provided by Jackson and Mackevicius (2024). At the same time, our results may understate ESSER's effects because some ESSER funds were used for physical plant improvements (e.g., HVAC), whose effects could extend beyond the study's timeframe (Biasi et al., 2024). Although districts have clearly not fully recovered, our results suggest that ESSER has contributed to addressing learning losses.

We put the contribution of ESSER funding into context in Figure 4, which shows the trend in learning loss from 2019 to 2022 and the extent to which there is recovery in the 2022-23 school year.[40] Between 2019 and 2022, student achievement in our sample dropped by about .152 SDs in math and .093 SDs in ELA and recovered by .046 SDs in math and .018 SDs in ELA

[40] Note, we re-scale these figures to center 2019 achievement to be zero in each subject.

in the 2022-23 school year. Not all the recovery in the 2022-23 SY was because of ESSER funding, however. We can see this by scaling our preferred estimates (0.008 SDs for math and ELA) by the average ESSER III allocation per pupil ($2,187 for our math sample, $2,173 for our ELA sample) and then dividing this product by the average growth observed for each sample between spring 2022 and spring 2023 (0.046 for math, 0.018 for ELA). This calculation suggests that about 35 percent of the 2022-23 math recovery and 87 percent of the *much smaller ELA recovery* can be attributed to ESSER III funding.

The above estimates of ESSER's contribution to recovery are based on the ESSER effect estimates that leverage within and cross-state variation. Estimates from models with state fixed effects (column 5 of Table 3) imply far smaller (and not statistically significant) ESSER impacts: 14 percent of math recovery and 6 percent of ELA recovery. States were precluded from mandating how districts spent ESSER funds (U.S. Department of Education, 2022). But they may have still influenced spending decisions, timelines, and recovery through their approval of district spending plans (Lieberman, 2022). States also could have influenced achievement through investments made using the 10 percent allocation of ESSER III funds set aside for states (Council of Chief State School Officers, 2024) or by incentivizing certain types of recovery programs.[41] Our heterogeneity analyses show that the average effects of ESSER also mask some differences in impacts across student demographic groups, district rurality, and pre-pandemic per pupil spending. Other research shows that low-income students and students of color experienced the greatest losses during the pandemic (Fahle et al., 2023). But in the districts in the

[41] For example, Texas House Bill 4545 "requires Texas school districts to implement a at a minimum supplemental instruction, an accelerated learning committee, and modified teacher assignments" (Texas Education Agency, 2021). Other states have instead used their state-withheld ESSER funds to support their policy priorities, signaling to districts how to use the recovery money. For example, Tennessee spent over $200 million on summer school and tutoring, Georgia put $5 million towards engagement and attendance in rural areas, and Connecticut used $36 million to fund after school and summer programming (Council of Chief State School Officers, 2024).

sample, ESSER's benefits were concentrated among White students. We also find differences in ESSER impacts across urbanicity and pre-pandemic revenue per pupil. We find that ESSER accelerated learning recovery the most in towns, benefiting achievement in math and ELA. In cities, the biggest gains were in math; rural areas gains were concentrated in ELA. Finally, districts that prior to the pandemic spent less per pupil benefitted most in terms of the impact of ESSER funds on achievement, suggesting that the marginal dollar went farther in lower-spending districts. While we focus on the marginal dollar impact of ESSER, if we broke out estimates according to state- or district-specific ESSER allocation amounts, those higher-poverty entities would have larger effects due to the greater total funding received.

There are several limitations to our study to keep in mind. First, as noted earlier, our results may not be nationally representative since they are based on data from only 30 states. Second, our analysis of academic recovery compares different cohorts of students, not the same group of students over time. As a result, we may miss nuances related to any varied impacts of COVID on different age groups or the movement of COVID-impacted students in and out of testing grades. Finally, we focus only on test scores, overlooking important non-test outcomes that have long-term implications for postsecondary success (Backes et al., 2022; Jackson, 2018) and that, like test scores, remain below pre-pandemic averages (Malkus, 2024).

Pinning down *how* ESSER funding impacted student learning is difficult because data on ESSER spending also has several limitations. Official reports on the use of ESSER funds, for example, are often vague and school budgets are fungible (Goldhaber et al., 2024). Because the federal government delivered the funds with few strings attached, there are no oversight mechanisms to track their usage and gauge their effectiveness (Roza, 2022). As noted earlier, we know from ESSER spending plans that districts intended to leverage the funds in different ways

(Bryant et al., 2022; Dusseault & Pillow, 2021; Malkus, 2021). The aforementioned analysis from Brooks and Springer (2024), for example, finds that higher poverty districts were more likely to propose using ESSER funds for long term capital investments than lower poverty districts. But because data on *actual* spending are not current and ESSER spending is fungible (Goldhaber et al., 2024), rigorously examining *how* ESSER spending impacted student achievement is challenging. When additional school spending data become available, this will remain an important area for future research.

Although ESSER spending had positive effects, the broader trajectory of academic recovery suggests many students have yet to catch up to pre-pandemic levels of achievement (Fahle et al., 2024).[42] With this, our estimates can provide some insight into how much future investment may be needed for full recovery. As shown in Figure 4, the loss from pre-pandemic levels at the end of 2023 school year remained .11 SDs in math and .07 SDs in ELA, since some recovery had already occurred by the end of 2021-22 school year (Fahle et al., 2024). To recover from these remaining losses, our estimates suggest schools would need between $9,000 and $13,000 additional funds per pupil, assuming the return on those funds is similar to what we estimated for ESSER III.[43] Scaling this by the roughly 50 million public school students yields a range of $450 billion to $650 billion. These estimates do not preclude the existence of other resources—including unspent ESSER allocations or the potential that student recovery continues to exceed what is explained by ESSER resources alone. Still, the magnitude of our estimated cost of recovery is striking. To put it in perspective, public schools spent an average of about $14,800

[42] Additionally, while benchmarking academic recovery to pre-pandemic levels provides a useful metric, we should recognize that the levels of achievement before COVID-19 were inadequate for many underserved students.
[43] This is calculated by taking the 0.13 loss in math and 0.09 loss in ELA and dividing by the ESSER coefficients in Table 3, column 4 of Panel A and B in Table 3 (0.008 for both subjects). Estimates to recover the full learning loss identified in 2021-22 (i.e., excluding recovery from 2021-22 to 2022-23) are $16,000 per pupil for math and $11,000 for ELA, a range of $550 billion to $800 billion.

per pupil in the 2019-20 school year (National Center for Education Statistics, 2024), so our high-end estimate nearly doubles current spending.

But while this extrapolated cost of recovery may appear high, estimates of leaving pandemic-related losses unaddressed suggest that large additional expenditures may be worthwhile. Doty et al. (2022) argue that the decline in test scores from the pandemic could translate into $900 billion in future foregone wages. Hanushek and Strauss (2024) predict an even higher cost of unremedied learning loss: $31 trillion, or about 35 times Doty et al.'s (2022) estimate. The difference between the two estimates stems from the fact that Doty and others consider lost wages alone, while Hanushek and Strauss consider lost wages as well as broader effects on the nation's productivity and growth. Regardless of which assumptions are privileged, both estimates far exceed the full-recovery cost estimate.

In short, our analysis suggests that ESSER funding helped address some of the academic decline students experienced during the pandemic. It also suggests that full recovery could require significant additional resources. But, as others have noted, the amount of funding schools have is not all that matters; *how* schools use funding—in both purpose and efficiency—also matters (Handel & Hanushek, 2023; McGee, 2023). With that in mind, if COVID relief funding continues, the federal government could consider providing clearer guidance to districts about spending strategies or require regular outcomes reporting (or some other form of accountability). Meanwhile, researchers and policymakers need to know much more about how different spending strategies influence student outcomes. Indeed, four years after start of the pandemic, the optimal spending patterns and resource allocation models for COVID-19 recovery remain far from clear.

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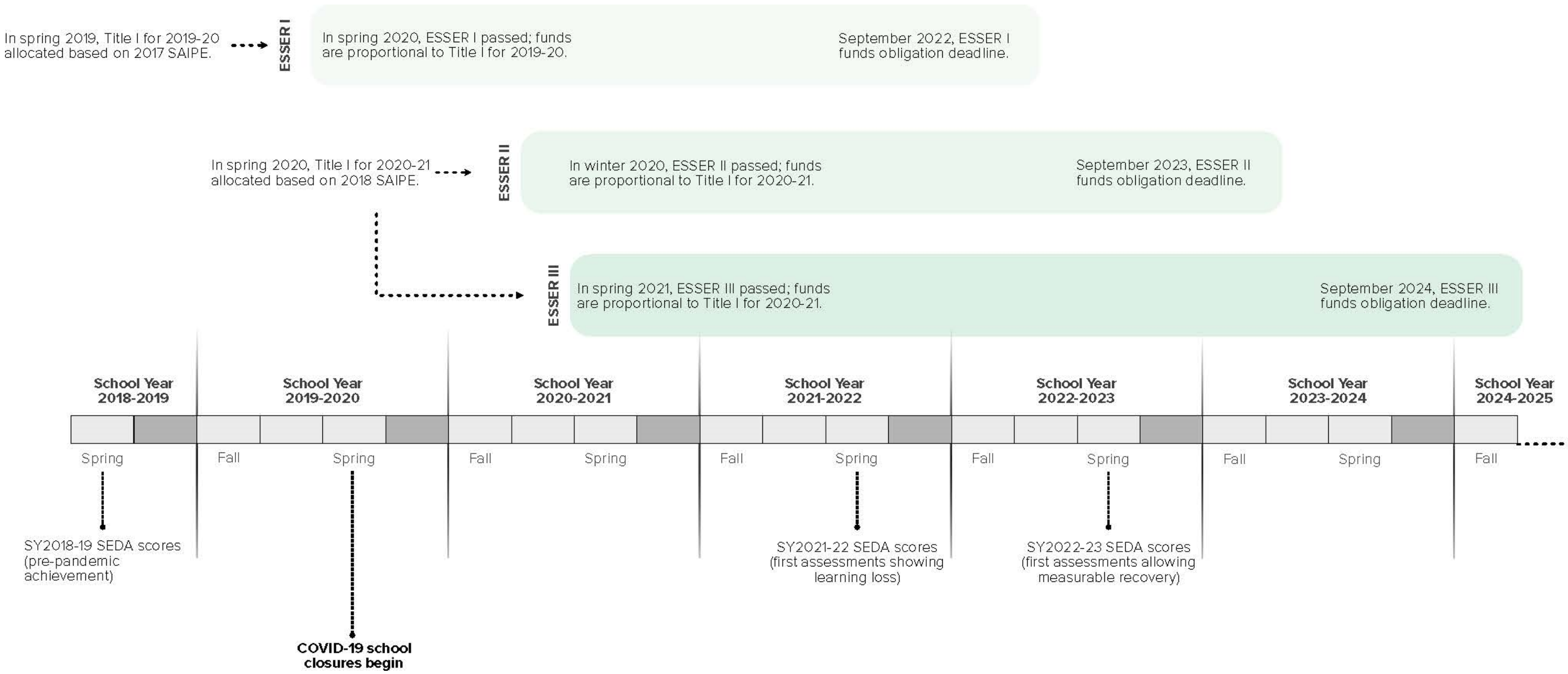
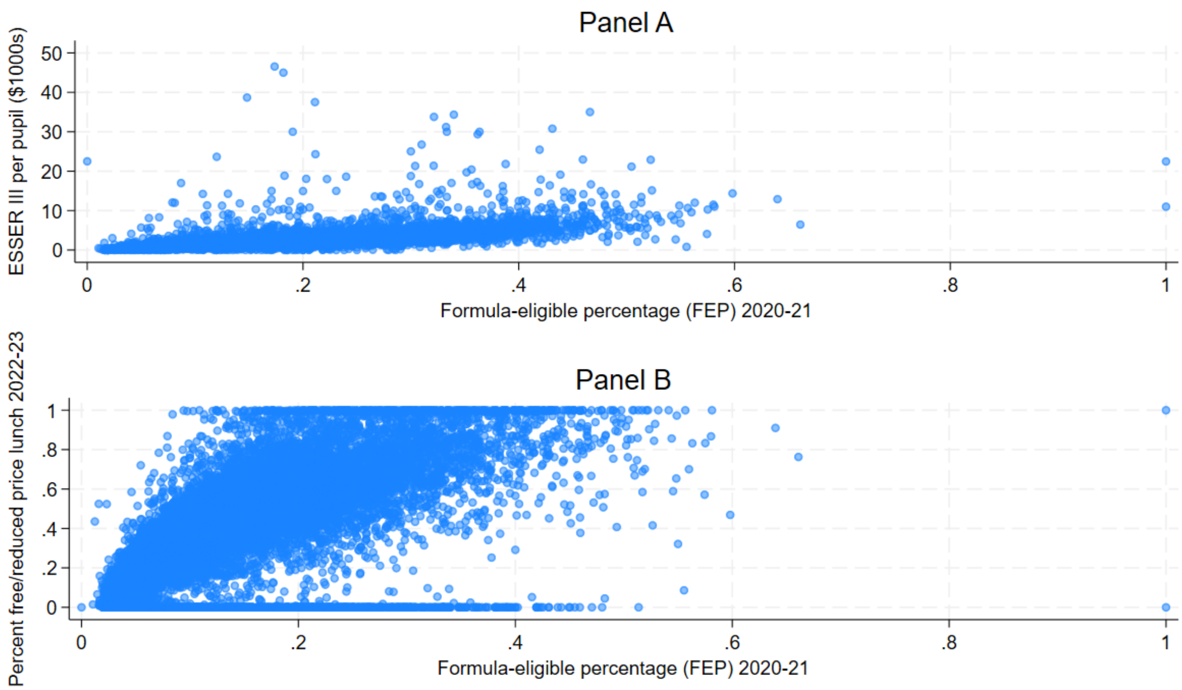
Figures and Tables

Figure 1. Timeline of outcome measures and ESSER events



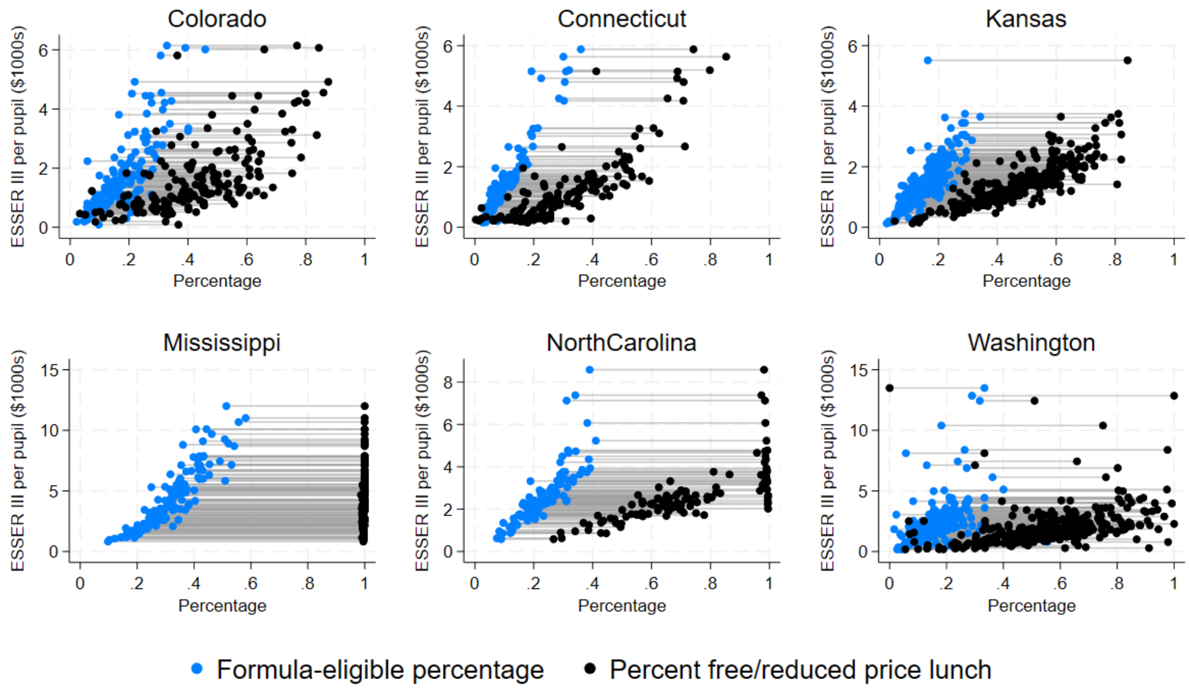
Note. ESSER I was passed into law in March of 2020; ESSER II was passed in December 2020; ESSER III was passed in March of 2021. Fund dispersals would have been slightly delayed from these dates of passage as they were administered. Title I is largely determined by the number of formula-eligible children in a district, the primary source for which is the SAIPE. ESSER=Elementary and Secondary School Emergency Relief Fund; SAIPE=Small Area Income and Poverty Estimates; SEDA=Stanford Education Data Archive.

Figure 2. Illustration of differences in the relationships between ESSER allocations, formula-eligible percents, and free/reduced price lunch



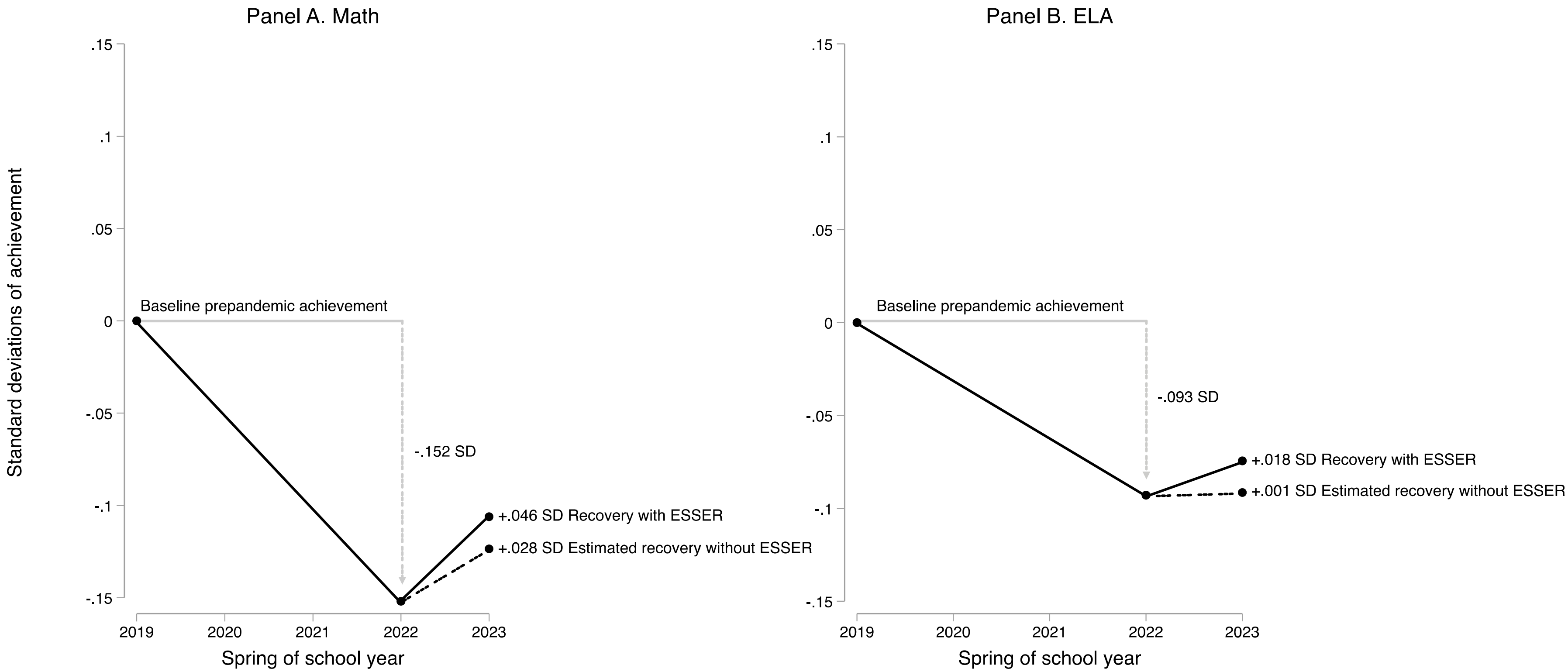
Note. Panel A presents a district-level plot of the relationship between formula-eligible percentages (FEP) for fiscal year 2020 (2020-21 school year) and ESSER III allocations per pupil in $1,000s. Panel B presents FEP and percent free or reduced-price lunch in 2022-23.

Figure 3. Differences in formula-eligible percentage and free/reduced-price lunch across states



Note. Each dot represents the district value for the indicated measure, with districts connected by the grey lines. Free/reduced price lunch values are for the 2022-23 school year. Formula-eligible percentage uses Small Area Income and Poverty Estimates (SAIPE) data from 2018 and determined Title I allocations for the 2020-21 school year (determined in the 2020 fiscal year).

Figure 4. Observed changes in achievement over time and estimated impact of ESSER III Funding



Note. Points represent means of district-level achievement weighted by student enrollment; we normalize 2019 achievement to zero for ease of comparison. Gaps in achievement between spring 2019 and spring 2022 capture pandemic learning loss; growth between 2022 and 2023 illustrate post-pandemic recovery. Achievement is standardized by subject and grade such that one unit is one standard deviation. The dashed line in each panel represents our estimated level of achievement without the effect of ESSER III funds we observe. We calculate these values by subtracting the product of our estimates in column 4 of Table 3 by the average ESSER III allocation for each subject sample from the weighted average of 2023 student achievement.

Table 1. Characteristics of districts for the average student by sample inclusion

	All districts	By availability of math outcomes		By availability of English language arts outcomes	
		Math sample	Not in sample	ELA sample	Not in sample
ESSER III allocation per pupil ($1000s)	2.1	2.2***	2.0	2.2***	2.0
Title I formula-eligible percent 2020-21	16.6	16.6	16.5	16.3***	16.9
District enrollment (1000s)	39.2	35.8***	43.7	36.8***	42.1
% Free/reduced price lunch 2022-23	49.7	49.1***	50.4	48.8***	50.8
% American Native 2022-23	0.9	0.6***	1.3	0.6***	1.3
% Asian 2022-23	5.5	5.9***	4.9	6.1***	4.7
% Black 2022-23	14.2	14.8***	13.4	14.4	14.0
% Hawaiian/Pacific Islander 2022-23	0.4	0.3***	0.4	0.3***	0.4
% Hispanic 2022-23	27.8	24.0***	32.7	24.5***	31.7
% Multiracial 2022-23	4.9	5.3***	4.4	5.3***	4.4
% White 2022-23	45.3	47.4***	42.7	46.9***	43.5
% Districts in rural area	16.2	15.9	16.5	15.0***	17.5
% Districts in cities	30.3	30.3	30.1	30.1	30.4
% Districts in suburbs	42.4	42.6	42.1	44.1***	40.4
% Districts in towns	11.2	11.1	11.3	10.8	11.6
District revenue per pupil 2019-20 ($1000s)	15.5	15.4	15.6	15.6**	15.3
N	13,234	5,632	7,602	5,278	7,956

Note. Means are weighted by district enrollment. Stars indicate statistically significant differences from the subject's sample of districts that are excluded from our analysis due to missing outcome data, *p<0.10, **p<0.05, ***p<0.01.

Table 2. First stage regressions, predicting district ESSER III allocations ($1000s)

	Math sample			English language arts sample		
	(1)	(2)	(3)	(4)	(5)	(6)
Title I formula-eligible percent 2020-21		13.177***	13.335***		13.454***	13.535***
		(0.590)	(0.750)		(0.661)	(0.804)
% Free/reduced price lunch 2022-23	1.227**	0.458**	0.588**	1.286**	0.452**	0.551*
	(0.494)	(0.186)	(0.283)	(0.567)	(0.210)	(0.301)
% American Native 2022-23	1.319	0.310	-0.019	1.428	0.527	0.124
	(1.227)	(0.954)	(0.926)	(1.414)	(1.040)	(0.986)
% Asian 2022-23	0.161	0.411	0.217	0.124	0.350	0.121
	(0.512)	(0.297)	(0.488)	(0.498)	(0.284)	(0.474)
% Black 2022-23	0.735	-0.290	0.365	1.225*	-0.124	0.586
	(0.533)	(0.469)	(0.425)	(0.627)	(0.508)	(0.439)
% Hispanic 2022-23	-1.343**	-1.079**	-1.221*	-1.231***	-1.113**	-1.272*
	(0.508)	(0.494)	(0.710)	(0.440)	(0.439)	(0.712)
% Hawaiian/Pacific Islander 2022-23	-11.235***	-0.075	1.135	-9.031*	3.478	1.757
	(3.788)	(3.253)	(2.183)	(5.109)	(4.229)	(3.565)
% Multiracial 2022-23	-6.448***	-3.963***	-4.796***	-7.868***	-5.132***	-5.472***
	(1.723)	(1.266)	(1.210)	(1.772)	(1.163)	(1.172)
% Districts in towns (ref. rural)	0.097	0.011	-0.045	0.158*	0.053	-0.023
	(0.073)	(0.052)	(0.046)	(0.084)	(0.052)	(0.036)
% Districts in suburbs (ref. rural)	-0.246	0.292*	0.248*	-0.127	0.329**	0.285**
	(0.173)	(0.151)	(0.130)	(0.189)	(0.158)	(0.134)
% Districts in cities (ref. rural)	0.603***	0.764***	0.611***	0.735***	0.808***	0.627***
	(0.182)	(0.163)	(0.133)	(0.203)	(0.166)	(0.134)
District revenue per pupil	0.074***	0.079***	0.089***	0.078***	0.079***	0.092***
2019-20 ($1000s)	(0.014)	(0.011)	(0.017)	(0.013)	(0.011)	(0.017)
2022 achievement	-1.703***	-0.189	-0.018	-1.896***	-0.138	0.093
(same subject)	(0.419)	(0.398)	(0.431)	(0.492)	(0.425)	(0.442)
2022 achievement^2	1.157***	0.426***	0.371**	1.526***	0.710*	0.620
(same subject)	(0.205)	(0.153)	(0.149)	(0.395)	(0.359)	(0.364)
2022 achievement^3	-0.836	-0.969	-0.986	-1.271	-1.555	-1.683
(same subject)	(0.659)	(0.668)	(0.669)	(0.997)	(1.001)	(1.006)
State fixed effects			X			X
N	5,632	5,632	5,632	5,278	5,278	5,278
Instrument *F*-statistic		498.405	316.008		413.892	283.572
R^2	0.683	0.816	0.838	0.687	0.824	0.845

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. F-statistics for an individual test on our instrument (FEP), listed at the bottom of the table. We report first stages separately for districts for which we observe math and English language arts scores because some states only report data usable by the Stanford Education Data Archive (SEDA) for a single subject. FEP for 2020-21 used data from the Census from 2018. All covariates of district characteristics are from the 2022-23 school year.

Table 3. Predicted impacts of ESSER III allocations per pupil on 2022-23 school year achievement

	Ordinary least squares			Two-stage least squares	
	(1)	(2)	(3)	(4)	(5)
	Panel A. Math 2023 achievement				
ESSER III allocations	0.010***	0.006**	0.004	0.008**	0.003
per pupil ($1000s)	(0.002)	(0.002)	(0.002)	(0.004)	(0.004)
n	5,632	5,632	5,632	5,632	5,632
	Panel B. English language arts 2023 achievement				
ESSER III allocations	0.011**	0.008**	0.002*	0.008	0.0005
per pupil ($1000s)	(0.004)	(0.003)	(0.001)	(0.005)	(0.003)
n	5,278	5,278	5,278	5,278	5,278
District covariates		X	X	X	X
State fixed effects			X		X

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. District covariates (included in models 2, 3, 4, and 5) include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year.

Table 4. Two-stage least squares estimates using alternative poverty measures as controls

	Preferred estimates	Alternative poverty measures	
	(1)	(2)	(3)
Panel A. Math 2023 achievement			
ESSER III allocation per pupil ($1000s)	0.008**	0.006	0.008**
	(0.004)	(0.004)	(0.003)
n	5,632	5,632	5,632
Panel B. English language arts 2023 achievement			
ESSER III allocation per pupil ($1000s)	0.008	0.006	0.004
	(0.005)	(0.006)	(0.006)
n	5,278	5,278	5,278
SEDA free/reduced price lunch (2021-22)		X	
NCES neighborhood poverty estimates (2020-21)			X

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Model 1 controls for the portion of students in each district qualifying for free/reduced-price lunch in 2022-23 from the Common Core of Data. Model 2 instead replaces this control with the district average income-to-poverty ratio from the NCES School Neighborhood Poverty Estimates. Model 3 instead controls for free/reduced price lunch in a district in 2021-22 from SEDA. ESSER = Elementary and Secondary School Emergency Relief Fund; NCES = National Center for Education Statistics; SEDA= Stanford Education Data Archive.

Table 5. Two-stage least squares estimates instrumenting for each ESSER wave

	ESSER III	ESSER I	ESSER II	ESSER II + III	Total ESSER
	(1)	(2)	(3)	(4)	(5)
Panel A. Math 2023 achievement					
ESSER wave allocation per	0.008*	0.080**	0.018**	0.005**	0.005**
pupil ($1000s)	(0.004)	(0.036)	(0.008)	(0.002)	(0.002)
n	5,632	5,641	5,653	5,630	5,656
Panel B. English language arts 2023 achievement					
ESSER wave allocation per	0.008	0.077	0.017	0.005	0.005
pupil ($1000s)	(0.005)	(0.052)	(0.011)	(0.004)	(0.003)
n	5,278	5,285	5,295	5,276	5,299

Note. Standard errors in parentheses are clustered by state $^*p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Model 1 presents our preferred estimates instrumenting for ESSER III. Models 2, 3, 4, and 5 instrument for ESSER I, ESSER II, the sum of ESSER II and III, and the sum of all three waves of ESSER allocations, respectively. ESSER = Elementary and Secondary School Emergency Relief Fund.

Table 6. Heterogeneity of two-stage least squares estimates across student subgroups and district characteristics

	Subgroups of districts by characteristic quartiles						Subgroups of districts by urbanicity			
	Lowest quartile per pupil revenue 2019-20	Highest quartile per pupil revenue 2019-20	Lowest quartile % FRPL	Highest quartile % FRPL	Lowest quartile % Black or Hispanic	Highest quartile % Black or Hispanic	Rural districts	City districts	Town districts	Suburban districts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Math 2023 achievement									
ESSER III allocations	0.014***	0.001	0.005	0.002	0.014**	0.006	0.004	0.008*	0.012***	0.004
per pupil ($1000s)	(0.005)	(0.003)	(0.005)	(0.004)	(0.005)	(0.004)	(0.006)	(0.004)	(0.004)	(0.006)
n	1,415	1,409	1,401	1,415	1,410	1,409	2,030	446	1,180	1,976
	Panel B. English language arts 2023 achievement									
ESSER III allocations	0.022***	-0.011	0.011	0.007	0.020**	0.001	0.018**	-0.003	0.013**	0.005
per pupil ($1000s)	(0.003)	(0.010)	(0.006)	(0.005)	(0.007)	(0.006)	(0.006)	(0.007)	(0.004)	(0.008)
n	1,320	1,324	1,315	1,324	1,323	1,319	1,755	415	1,112	1,996

Note. Standard errors in parentheses are clustered by state, **p*<0.10, ***p*<0.05, ****p*<0.01. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity (except for in models 7 through 10 where this is excluded); district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Models 1 and 2 present results for math (panel A) and ELA (panel B) for districts in the first (lowest) and fourth (highest) quartiles according to total district revenue per pupil in 2019-20. The average per pupil revenue for districts in Models 1 and 2 are approximately $11,000 and $23,000, respectively. Models 3 through 6 apply this same logic to quartiles by FRPL qualification (models 3 and 4) and for the combined share of enrolled students who are Black or Hispanic (models 5 and 6) and for. Average shares qualifying for FRPL are 10% and 82% for the samples reported in models 3 and 4. Average share of students who are Black or Hispanic in models 5 and 6 is approximately 4% and 66%, respecitvely. Models 7 through 10 are estimated on subgroups of districts according to their CCD urbanicity. CCD = Common Core of Data; ESSER = Elementary and Secondary School Emergency Relief Fund; SEDA= Stanford Education Data Archive; FRPL=free/reduced-price lunch.

Table 7. Two-stage least squares results including covariates to test mechanisms

	(1)	(2)
Panel A. Math 2023 achievement		
ESSER III allocation per pupil ($1000s)	0.008**	0.008**
	(0.004)	(0.004)
n	5,568	5,632
Panel B. English language arts 2023 achievement		
ESSER III allocation per pupil ($1000s)	0.007	0.008*
	(0.005)	(0.005)
n	5,208	5,278
Change in student-teacher ratio	X	
Share of ESSER spending across categories		X

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. For each model we add the indicated set of covariates to assess their mediation of our estimated impact of ESSER on achievement. In model 1, we add a control for the change in student-teacher ratio from 2021-22 to 2022-23. In model 2, we add controls for the share of ESSER funding spent (as a percent of total allocations) on capital/facilities, labor costs, supplies/materials, and contracts/purchased services, with an omitted other capturing the remainder of spending.

Appendix: Supplemental results tables

Table A.1 States included in Stanford Education Data Archive achievement records

	Math sample	ELA sample
Alabama	x	
Arkansas	x	
California	x	x
Connecticut	x	x
Georgia	x	x
Illinois	x	x
Indiana	x	x
Kansas	x	x
Kentucky	x	x
Louisiana	x	x
Massachusetts	x	x
Michigan	x	x
Mississippi	x	x
Nevada	x	x
New Hampshire	x	x
New Jersey	x	x
North Carolina	x	x
Ohio	x	x
Oklahoma	x	x
Oregon	x	x
Pennsylvania	x	x
Rhode Island	x	x
South Dakota	x	x
Tennessee	x	x
Utah	x	x
Virginia	x	x
Washington	x	x
West Virginia	x	
Wisconsin	x	x
Wyoming		x

Note. State inclusion in sample by subject demarcated with x's. Only states included in one of the two samples listed.

Table A.2 Characteristics of districts for the average student by ESSER III allocation per pupil

	All districts by subject sample		Districts grouped by ESSER III per pupil allocations							
			Math sample				ELA sample			
	Math sample	ELA sample	1st quartile	2nd quartile	3rd quartile	4th quartile	1st quartile	2nd quartile	3rd quartile	4th quartile
ESSER III Allocation Per Pupil ($1,000s)	2.19	2.17	0.52	1.22	2.08	4.34	0.50	1.15	1.98	4.22
Title I formula-eligible percent (FEP) 2020-21	16.62	16.29	6.35	11.71	18.03	27.33	6.09	11.13	17.14	26.54
District enrollment (1000s)	35.80	36.76	14.74	30.63	31.93	59.88	14.15	26.63	23.23	70.26
% Free/reduced price lunch 2022-23	49.14	48.75	23.32	43.01	57.77	67.12	22.60	40.27	54.83	68.89
% American Native 2022-23	0.58	0.56	0.43	0.62	0.64	0.63	0.42	0.60	0.62	0.60
% Asian 2022-23	5.89	6.12	10.56	6.03	4.40	3.31	10.88	6.14	4.99	3.50
% Black 2022-23	14.82	14.42	5.34	10.14	13.27	27.22	4.99	9.25	12.77	26.01
% Hawaiian/Pacific Islander 2022-23	0.32	0.31	0.24	0.27	0.56	0.21	0.24	0.28	0.44	0.28
% Hispanic 2022-23	24.02	24.53	14.04	21.73	28.24	30.17	13.58	20.89	28.25	32.12
% Multiracial 2022-23	5.28	5.35	5.84	5.69	5.47	4.38	5.85	5.87	5.29	4.69
% White 2022-23	47.36	46.92	63.36	55.46	47.35	28.50	63.84	56.91	47.60	27.38
% Districts in rural area	15.91	15.00	11.95	16.85	22.43	12.88	12.36	15.48	21.98	11.33
% Districts in cities	30.34	30.11	11.60	20.45	31.58	51.70	10.37	20.66	25.76	54.10
% Districts in suburbs	42.63	44.11	71.11	50.22	31.32	23.70	72.42	51.44	37.51	23.57
% Districts in towns	11.11	10.78	5.34	12.49	14.67	11.72	4.86	12.42	14.75	11.01
District total revenue per pupil 2019-20 ($1,000s)	15.42	15.62	15.88	14.73	14.35	16.48	16.07	14.97	14.64	16.48
Change in math achievement 2019 to 2022	-0.15		-0.12	-0.14	-0.16	-0.18				
Change in math achievement 2022 to 2023	0.05		0.04	0.04	0.04	0.06				
Change in ELA achievement 2019 to 2022		-0.09					-0.07	-0.09	-0.10	-0.11
Change in ELA achievement 2022 to 2023		0.02					0.02	0.01	0.02	0.02
N	5,632	5,278	1,406	1,410	1,408	1,408	1,318	1,321	1,320	1,319

Note. Means are weighted by district enrollment.

Table A.3 Robustness of results to additional controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Math 2023 achievement								
ESSER III allocation per pupil ($1000s)	0.009**	0.008**	0.008**	0.007**	0.007*	0.008**	0.008**	0.005
	(0.003)	(0.003)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
n	5,632	5,632	5,632	5,632	5,632	5,632	5,632	5,632
Panel B. English language arts 2023 achievement								
ESSER III allocation per pupil ($1000s)	0.008	0.008*	0.008*	0.007	0.005	0.009*	0.007	0.006*
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)
n	5,278	5,278	5,278	5,278	5,278	5,278	5,278	5,278
District share of instruction remote and hybrid	X							X
State youth access to computer and internet		X						X
State COVID cases and deaths			X					X
State democratic vote share 2020				X				X
State unemployment rate					X			X
State total K12 education revenue 2019-20						X		X
Indicators for Right to Work states							X	X

Note. Standard errors in parentheses are clustered by state, **p*<0.10, ***p*<0.05, ****p*<0.01. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Model 1 includes separate controls for the share of instruction in 2020-21 conducted remotely and in hybrid format, omitting the share of the year in-person; missing values are controlled for with separate indicators and replaced with the sample mean. Model 2 instead controls for the ACS 2018-22 share of youth with access to a computer at home and, separately, the share with access to internet at home. Model 3 controls for the state-level COVID cases and deaths in each school year starting in March 2020 through 2021-22. Model 4 controls for the democratic candidate vote share at the state level in the 2020 presidential election. Model 4 controls for the state unemployment rate, averaged across each school year from 2019-20 through 2021-22. Model 6 controls for the total revenue from all sources going towards K12 education in 2019-20 at the state level. Model 7 flags right to work states. ESSER = Elementary and Secondary School Emergency Relief Fund; NCES = National Center for Education Statistics; SEDA= Stanford Education Data Archive.

Table A.4 Two-stage least squares estimates for districts reporting achievement outcomes by student subgroups

Outcome group	Districts reporting Black student achievement		Districts reporting Hispanic student achievement		Districts reporting White student achievement		Districts reporting average achievement for student subgroups by economic disadvantage		
	Black students	All students	Hispanic students	All students	White students	All students	Economically disadvantaged	Not economically disadvantaged	All students
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Math 2023 achievement									
ESSER III allocations	0.0001	0.002	0.003	0.005	0.008***	0.011***	0.013***	0.008	0.012***
per pupil ($1000s)	(0.005)	(0.005)	(0.004)	(0.004)	(0.003)	(0.003)	(0.005)	(0.006)	(0.004)
n	583	583	1,184	1,184	3,320	3,320	2,663	2,663	2,663
Panel B. English language arts 2023 achievement									
ESSER III allocations	-0.022	-0.012	-0.011	-0.010	0.003	0.003	0.004	0.001	0.004
per pupil ($1000s)	(0.013)	(0.011)	(0.007)	(0.010)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
n	518	518	1,075	1,075	3,174	3,174	2,751	2,751	2,751

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. Because subgroup average outcomes are not reported consistently, for each type of subgroup reported we first estimate our model predicted achievement in the indicated subject for the indicated subgroup and then estimate our model for average achievement across all students amongst that same sample of reporting districts. This second estimation allows for comparison to our main results in Table 3. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of for the indicated subgroup of achievement in the same subject from the prior school year. CCD = Common Core of Data; ESSER = Elementary and Secondary School Emergency Relief Fund.

Table A.5 Heterogeneity of two-stage least squares estimates across district demographics

	Subgroups of districts by quartiles of district's enrolled student demographics							
	Lowest quartile % Black	2nd quartile % Black	3rd quartile % Black	Highest quartile % Black	Lowest quartile % Hispanic	2nd quartile % Hispanic	3rd quartile % Hispanic	Highest quartile % Hispanic
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Math 2023 achievement								
ESSER III allocations per pupil ($1000s)	0.013***	0.016***	0.005	0.005	0.006	0.007	0.006	0.008*
	(0.003)	(0.004)	(0.006)	(0.004)	(0.005)	(0.007)	(0.007)	(0.004)
n	1,407	1,403	1,411	1,411	1,413	1,398	1,409	1,412
Panel B. English language arts 2023 achievement								
ESSER III allocations per pupil ($1000s)	0.012***	0.026***	0.011	0.003	0.010*	0.016***	0.016**	0.002
	(0.004)	(0.005)	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)	(0.007)
n	1,321	1,313	1,324	1,320	1,324	1,310	1,321	1,323

Note. Standard errors in parentheses are clustered by state, *p<0.10, **p<0.05, ***p<0.01. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity; district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Models 1 through 4 are estimated on subgroups of districts sorted by quartile of the percent of enrolled students who are Black (Q1=0.4%; Q4=32%). Models 5 through 8 do the same for the share of students who are Hispanic (Q1=2%, Q4=45%). CCD = Common Core of Data; ESSER = Elementary and Secondary School Emergency Relief Fund; SEDA= Stanford Education Data Archive; FRPL=free/reduced-price lunch.

Table A.6 Heterogeneity of two-stage least squares estimates across student subgroups and district characteristics with state fixed effects

	Subgroups of districts by quartiles of characteristics						Subgroups of districts by urbanicity			
	Lowest quartile revenue per pupil 2019-20	Highest quartile revenue per pupil 2019-20	Lowest quartile % FRPL	Highest quartile % FRPL	Lowest quartile % Black or Hispanic	Highest quartile % Black or Hispanic	Rural districts	City districts	Town districts	Suburban districts
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Math 2023 achievement										
ESSER III allocations per pupil ($1000s)	0.001	-0.002	-0.003	0.004	0.006	0.004	0.005*	0.003	0.005*	-0.003
	(0.006)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.004)	(0.003)	(0.007)
n	1,415	1,409	1,401	1,415	1,410	1,409	2,030	446	1,180	1,976
Panel B. English language arts 2023 achievement										
ESSER III allocations per pupil ($1000s)	0.008***	-0.003	-0.0001	0.004*	0.003	0.004*	0.007**	0.003	0.004	-0.002
	(0.002)	(0.003)	(0.003)	(0.002)	(0.005)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)
n	1,320	1,324	1,315	1,324	1,323	1,319	1,755	415	1,112	1,996

Note. Standard errors in parentheses are clustered by state, $^{*}p<0.10$, $^{**}p<0.05$, $^{***}p<0.01$. All models are weighted by district enrollment in 2022-23. District covariates in all models include the following: district demographics for the 2022-23 school year including the portion of students qualifying for free/reduced-price lunch, and the portion who are American native, Asian, Black, Hispanic, Hawaiian or Pacific Islander, and multiracial; indicators for district urbanicity (except for in models 7 through 10 where this is excluded); district total revenue per pupil in 2019-20; and a cubic of achievement in the same subject as the indicated outcome from the prior school year. Models 1 and 2 present results for math (panel A) and ELA (panel B) for districts in the first (lowest) and fourth (highest) quartiles according to total district revenue per pupil in 2019-20. The average per pupil revenue for districts in Models 1 and 2 are approximately $11,000 and $23,000, respectively. Models 3 through 6 apply this same logic to quartiles by combined share of enrolled students who are Black or Hispanic (models 3 and 4) and for FRPL qualification. Average share of students who are Black or Hispanic in models 3 and 4 is approximately 4% and 66%, respectively. Average shares qualifying for FRPL are 10% and 82% for the samples reported in models 5 and 6. Models 7 through 10 are estimated on subgroups of districts according to their CCD urbanicity. CCD = Common Core of Data; ESSER = Elementary and Secondary School Emergency Relief Fund; SEDA= Stanford Education Data Archive; FRPL=free/reduced-price lunch.