The effects of investing in failing schools: Evidence from Los Angeles Unified School District*

Antonia Vazquez

October 11, 2025

Click here for the most recent version of this paper

Abstract

Can investing in failing schools help them improve? This paper studies this question using a natural experiment based on a 2017 lawsuit settlement that allocated substantial resources to the lowest-performing schools in the Los Angeles Unified School District (LAUSD). Using a difference-in-differences design, I compare 50 secondary schools that received $1,030 per student annually for three years to nearby public, noncharter schools that received no settlement funding. The intervention mandated hiring of additional staff members and allocating of funds for professional development, but allowed discretionary spending on initiatives for high-need students. I find that, in line with the intent of the settlement, schools hired more personnel, including instructional staff such as teachers and counselors and support personnel such as paraprofessionals and school service staff. In terms of performance outcomes, relative to the unfunded district schools, settlement schools experienced substantial reductions in suspension rates. These reductions were particularly notable given that the settlement triggered demographic sorting, with the treated schools losing students overall and shifting toward higher concentrations of economically disadvantaged students and lower Black enrollment. A simple bounding exercise that accounts for demographic sorting indicates that the settlement had meaningful effects on suspension rates, suggesting real improvements on noncognitive dimensions of schooling. Survey evidence suggests that a key mechanism was improvements to school climate as reported by staff and students.

*LBJ School of Public Affairs, University of Texas–Austin, USA; antonia.vazquez@utexas.edu. I am grateful to my dissertation committee members— Mary Evans, Raissa Fabregas, Andrew Waxman, and Manuela Angelucci— for their insightful guidance and support throughout this work. I especially thank Eric Chyn, Christopher Campos, and Scott Carrell for their substantial comments and feedback on this paper. I also thank Emileigh Harrison, Yumin Hong, Paula López Villalba, Richard Murphy, Francisco Pardo, Ljubica Ristovska, and seminar participants at the LBJ School Sack Lunch Seminar for helpful comments and suggestions.

1 Introduction

School spending in the United States represents a massive public investment, with K–12 public schools spending $918.5 billion annually nationwide.[1] Despite these enormous expenditures, low-income school districts are more than twice as likely to suffer funding gaps as higher-income districts,[2] and chronically low-performing schools are more than twice likely as other public ones to experience large enrollment declines (Goulas, 2024). An extended public debate has centered on how to address poor school performance, with closing low-performing schools being one option. Between 2000 and 2022, districts closed approximately 1,450 schools nationwide each year on average.[3] Supporters of closure argue that chronically underperforming schools cannot be fixed and that shuttering them allows scarce resources to be redirected toward more effective alternatives while giving families better options. School closures not only affect students but also have long-lasting adverse effects on the aggregate economy (Jang and Yum, 2024). In contrast, advocates for investment contend that low-performing schools fail because they have been systematically under-resourced, and that substantial, sustained funding can turn them around. While recent research demonstrates that increased school spending improves student outcomes on average (Jackson and Mackevicius, 2024), this evidence derives mainly from studies of general finance reforms or district-level policies rather than focused interventions in failing schools, making it unclear whether these positive effects extend to the most underperforming institutions.

This paper addresses this gap by examining how investment affects the lowest-performing schools. I study a natural experiment arising from a 2017 lawsuit settlement in the Los Angeles Unified School District (LAUSD) that delivered substantial resources to the district schools struggling the most. Fifty secondary schools that officials identified as the lowest-performing in the LAUSD received $1,030 per student annually for over three years, a figure representing approximately 13.5% of their existing budgets on average. This setting provides a rare opportunity to study whether large increases in investment in failing schools can improve them, directly informing a key policy debate. Several features of the setting are advantageous for this research: First, the investment was targeted at failing schools rather than general school populations. Second, the settlement generated substantial school-specific funding variation, as the recipient schools were selected on the basis of the settlement's terms rather than district educational policy decisions. Third,

[1] US Census Bureau, "Public Education Finances: 2023," available online.

[2] Halpin, John, James Morton, Rebecca Goldstein, and Erin Russ. "Closing America's Education Funding Gaps." The Century Foundation, July 28, 2020, available online.

[3] National Center for Education Statistics, "Fast Facts: School Closures," available available online.

the money was for critical needs and high-need students. In low-performing schools, this priority implies addressing particular challenges such as high suspension rates, a poor school climate, and low standardized test scores.

Using a difference-in-differences (DID) design, I compare outcomes between fifty high-need secondary schools that received the funding and nearby public, noncharter secondary schools that did not. My analysis relies on two key assumptions to provide credibly causal estimates. First, I assume that the outcomes of the treated and control schools would have followed similar trajectories in the absence of the intervention—the parallel trends assumption central to DID estimation. I provide evidence supporting this assumption by examining pretreatment trends in key outcomes and find no evidence of divergent trajectories between the treated and control schools before the intervention. Second, to verify that the schools actually spent the funding as intended, I examine whether the treated schools hired additional staff as stipulated by part of the settlement.

I examine multiple outcomes using administrative data from the California Department of Education and LAUSD, including student achievement in English and mathematics, suspension rates, staffing levels, enrollment, and demographics. Crucially, to explore potential mechanisms, I take advantage of rich school experience survey data that capture detailed perceptions of school climate from both students and staff. This comprehensive combination of administrative and survey data allows me to assess not only the direct effects of the intervention but also the mechanisms driving observed changes.

The settlement required treated schools to improve or provide more services for high-need students broadly defined, hire additional staff to improve school climate, and invest in teachers' professional development. My results show that schools successfully implemented these changes by increasing their employment of certified staff such as teachers by approximately 3 per 1,000 students and of support staff such as office staff by almost 2 per 1,000 students; these figures represent increases of approximately 5% over baseline levels. These resource increases translated into substantial improvements in the disciplinary climate, with reductions in suspension rates of 0.75 percentage points (pp), a substantial 44% decline from pretreatment levels. This effect size is more than double the magnitude of common school climate interventions such as restorative justice programs (Adukia et al., 2025). While the disciplinary improvements were substantial, academic outcomes showed more limited progress. The intervention improved English language arts (ELA) achievement by 2.8 pp (12.8% from baseline), and for mathematics, the point estimate is positive but nonsignificant. However, because of data limitations, I observe only two pretreatment periods

for academic outcomes, which cautions against a causal interpretation of these estimates.

A key empirical question—one to which the answers predicted by theory are ex ante ambiguous—is whether the settlement also affected enrollment and student composition. Better-resourced schools might attract students seeking improved opportunities, while litigation stigma could signal poor performance and deter enrollment. In practice, the treated schools lost 48 students on average (a 5.7% decline from baseline), experienced a 0.9 pp decrease in Black enrollment (a 6.2% decline), and saw free lunch eligibility increase by 3.6 pp (a 4.2% increase). These patterns suggest that families with better school choice options left the treated schools, such that economic disadvantage became more concentrated in them.

These shifts in student composition raise questions about the interpretation of my main findings, particularly the reduction in suspension rates. The event study results suggest the presence of pre-existing trends in enrollment, which I investigate using two complementary approaches. First, I conduct sensitivity analysis following Rambachan and Roth (2023), which shows that the enrollment effect loses statistical significance under modest violations of parallel trends ($\bar{M} = 0.5$). Second, I use the Synthetic Difference-in-Differences estimator, which better accounts for pre-treatment trends and yields an enrollment effect that is reduced by half and becomes statistically insignificant (p-value = 0.10). These results suggest the enrollment decline likely reflects pre-existing trends rather than a causal effect of the settlement. Nonetheless, to be conservative, I also consider the possibility that the compositional changes represent true treatment effects and conduct a bounding exercise following Horowitz and Manski (2000). This exercise assesses how sensitive my estimates are to different assumptions about the characteristics of students who left. I consider various scenarios regarding these students' suspension propensity and find that, across all reasonable bounding assumptions, the sign and magnitude of the results remain substantively unchanged, being statistically significant in 4 out of 5 scenarios. This suggests that the observed improvements in the disciplinary climate are not simply a mechanical result of the altered student composition but instead reflect genuine changes in school practices.

To understand whether the observed reduction in suspension rates reflects behavioral improvements, shifts in educator responses, or changes in administrative practices, I examine rich school experience survey data from students and staff. My findings suggests that students in the treated schools perceived meaningful improvements in overall school climate (a 43% improvement from baseline, a statistically significant effect at the 5% level) and better experiences with bullying (although this effect is not statistically significant). More strikingly, teachers and staff reported

substantial improvements across multiple dimensions of their work environment. They indicated that they had received professional development on bullying, that bullying incidents had become a less significant problem, and that behavioral issues were now addressed when they arose. Additionally, staff reported greater agreement that disciplinary practices at their schools were both fair and effective, alongside notable improvements in their perceptions of the overall school climate. These improvements were large in magnitude, ranging from 53% to 108% relative to baseline, and statistically significant at the 1% level. These patterns suggest that the intervention primarily operated through enhancing educators' capacity and approaches to discipline.

This research contributes to several strands of literature. First, it contributes to the school finance literature by addressing whether increased spending can improve the outcomes of the lowest-performing schools. While extensive research demonstrates that more school investment improves student outcomes, including test scores, graduation rates, college attendance, and adult earnings, with particularly strong effects for low-income students (Abott et al., 2020, Baron, 2022, Candelaria and Shores, 2019, Hyman, 2017, Jackson and Mackevicius, 2024, Jackson et al., 2016, Lafortune et al., 2018), these studies rarely examine failing schools directly. Rather, the research has focused primarily on broad expenditure increases from school finance reforms or close elections that differentially affected funding for diverse school populations. In contrast, my study directly examines a reform that impacted the lowest-quality schools. This distinction matters because low-performing schools may respond differently to additional resources than average schools due to the former's severer baseline challenges or different organizational capacity or intervention needs. Moreover, spending effectiveness varies considerably by resource allocation: Researchers have found larger effects from operational spending on personnel than from capital expenditures (Abott et al., 2020, Baron, 2022, Biasi, 2024); this suggests that the specific nature of the investment and the institutional context are crucial. My contribution is to provide direct evidence on whether an investment with targeted and discretionary components can improve outcomes in failing schools, examining both student achievement and the mechanisms through which resources impact outcomes in these challenging contexts. This analysis is essential because demonstrating that "money matters" in general may not suffice to conclude that investment can help the schools that need it most.

The most direct evidence on interventions to improve failing schools comes from the research on School Improvement Grants (SIG), under which chronically underperforming schools were required to implement turnaround models designed by the federal government. These programs bundled money with dramatic changes such as replacement of principals and at least 50% of the schools'

staff. While recent meta-analytic evidence finds these interventions generated moderate positive gains in math achievement (Schueler et al., 2022), they differed fundamentally from resource-based investment in that they were highly prescriptive and disruptive. In contrast, I study a resource-based approach that preserves school autonomy; the Los Angeles settlement provided substantial funding while allowing schools considerable discretion in addressing their challenges. This distinction is important because local actors may better understand their specific problems and have greater buy-in for solutions they help design. My analysis addresses a critical gap by testing whether this gentler, resource-focused approach can achieve improvements without the disruption of imposed turnaround models.

Last, this paper also contributes to the school accountability literature by examining whether investment-based approaches ("carrots") can improve low-performing schools, contrasting with the punishment-based mechanisms ("sticks") that have dominated accountability policy. While extensive research documents how accountability pressure generates positive responses but also problematic unintended consequences, including teaching to the test, strategic student exclusion, and gaming behaviors (Booher-Jennings, 2005, Cullen and Reback, 2006, Dee and Jacob, 2011, Figlio, 2006, Figlio and Rouse, 2006, Figlio and Getzler, 2006, Jacob, 2005, Jacob and Levitt, 2003, Neal and Schanzenbach, 2010, Reback, 2008, Reback et al., 2014), less is known about whether positive incentives can achieve improvements without these negative side effects. The Los Angeles settlement provided schools substantial resources to help them improve rather than threatening them with sanctions; from this we can glean insights into whether "carrots" can succeed where "sticks" have yielded mixed results.

In summary, this study contributes to the literature in several important ways that advance our understanding of how to improve failing schools. First, it provides causal evidence on the effects of investment specifically in failing schools, bridging a critical gap between general school finance research and the policy question of whether targeted funding can help the lowest-performing institutions. Second, it demonstrates that resource-based approaches granting schools some autonomy can improve the disciplinary climate even when academic gains are modest, which suggests that nontest outcomes may be important early indicators of school improvement. Finally, using rich survey data from both students and staff, it identifies that the improvements operated primarily through enhanced educator capacity rather than direct student behavior changes, providing insights into the mechanisms through which investment affects the school environment.

2 LAUSD Settlement: *Community Coalition v. LAUSD*

In July 2015, the Community Coalition of South Los Angeles and Reyna Frias filed a lawsuit against LAUSD, alleging the district had misspent approximately $2 billion in funds designated for high-need students. The lawsuit centered on California's Local Control Funding Formula (LCFF), enacted in 2013, which provides additional funding to districts serving large populations of low-income students, English language learners (ELLs), and foster youths. Under the LCFF, funding is tied to the number of duplicated students, that is, those who fall into one or more support categories (low-income, English learner, and/or foster youth), are counting twice for funding purposes.

The plaintiffs accused LAUSD of violating state funding requirements by improperly counting prior special education spending as expenditures for high-need students, thereby depriving these populations of resources intended for their educational support. LAUSD settled the lawsuit in July 2017 and finalized the agreement in September 2017 (see a detailed timeline in Figure 1). The district agreed to reallocate $171.6 million over three years (2017–18 through 2019–20) to "school innovation funds" dedicated specifically for services for high-need students.

School Selection. The settlement identified 50 low-performing secondary schools (20 middle schools and 30 high schools) as the highest-priority recipients based on objective criteria, including the percentage of unduplicated students, math scores, suspension rates, and the rates of foster youths and homeless students.

Funding Allocation. The 50 identified schools received funding allocated proportionally based on their "duplicated count" of high-need students, with a per-pupil rate of approximately $1,030 per duplicated student per year, a figure corresponding to a 13.5% increase in the schools' budgets on average.

Implementation Requirements. The settlement established two mandatory requirements while preserving considerable school autonomy. First, the schools had to implement tiered student support teams ("Achievement Through Support" Teams), with more comprehensive services for larger schools, including restorative justice advisors, counselors, and psychiatric social workers. Second, the schools had to allocate at least 10% of the remaining funds to foundational professional development in mathematics or ELA. Beyond these requirements, schools had discretion in spending within six broad categories focused on high-need student services (see Appendix A for detailed implementation requirements).

3 Data

My analysis draws on administrative data from the California Department of Education and LAUSD combined with information from the settlement agreement.

3.1 Analysis sample

The sample includes noncharter LAUSD schools that were operational in academic year 2016/17. I impose the following restrictions to improve comparability between the treatment and control schools: I exclude continuation and special education schools, retain only secondary schools in the same local districts as treated schools, and restrict the sample to schools operating throughout 2014/15 to 2019/20. I set this end date to avoid potential confounding from the COVID-19 pandemic and start date to ensure all the schools have pretreatment data, as two of the beneficiary schools opened in August 2014. These criteria yield a final sample of 170 schools (121 control, 49 treatment) observed over 6 academic years (2014/15 to 2019/20).[4]

3.2 Outcome data

I examine different outcomes to assess the implementation and effects of the settlement. First, I analyze expenditure outcomes to understand how the schools used the additional funds. Second, I examine student outcomes, focusing on disciplinary and academic measures. Third, I use student and staff answers to a school experience survey. All the data come from the California Department of Education and LAUSD.

3.2.1 Expenditure outcomes

For the expenditure outcomes, I examine two categories of staff data that capture different types of school employees.[5]

Certified employees. These are school staff required to hold valid teaching credentials or permits and include (1) teachers who provide direct instruction, (2) administrative personnel in positions requiring certification but not providing direct instruction, and (3) pupil services staff

[4]The settlement listed 50 schools, but I have 49 in my analysis sample because one school merged with another after the settlement.

[5]Complete school-level budget data for 2019 and 2020 were not publicly accessible. After submitting multiple unsuccessful requests for these data to the district, I opted to focus on staffing data as the most reliable available indicator of resource allocation changes.

such as counselors, librarians, and psychologists who hold specialized credentials. These data are available for the years up to academic year 2017/18.

Classified staff. These are employees in positions not requiring certification and include (1) paraprofessionals who provide instructional support under teacher supervision (teaching assistants, aides, translators), (2) office/clerical staff supporting administration and business services, and (3) other support staff including health services, maintenance, food service, security, and transportation personnel.

3.2.2 Student outcomes

Given that the settlement mandated significant investments in services for high-need students, including academic support and mental health services, and in school climate initiatives such as restorative justice programs, I focus on outcome measures that directly reflect these intervention areas.

Suspensions. I measure suspension rates as the unduplicated count of students suspended divided by cumulative enrollment at the school level.

Test scores. Ideally, I could analyze raw test scores, but the California Department of Education publicly provides performance data only in categorical levels as follows: the percentage standard exceeded, percentage standard met, percentage standard met and above, percentage standard nearly met, and percentage standard not met. I therefore focus on the percentage of students who met or nearly met standards as my measure of academic achievement. I use data from 2015/16 to 2018/19.

3.2.3 School experience survey outcomes

To measure experiences and perceptions of school climate, I use data from LAUSD's School Experience Survey (SES), an annual survey that captures comprehensive feedback from teachers, staff, students, and parents across all LAUSD schools. I examine the survey responses for the 2014/15–2019/20 academic years.

For students, I focus on measures of bullying experiences and overall perceptions of school climate. The survey has high response rates of 80% across the years in my analysis sample. The bullying indicators assess four key forms of peer victimization occurring on school property: physical aggression (being pushed, shoved, slapped, hit, or kicked), relational aggression (being subject to the spreading of mean rumors or lies), sexual harassment (experiencing sexual jokes, comments,

or gestures), and appearance-based teasing. The school climate measures capture students' perceptions across six dimensions of their school environment, including adult respect and help, feelings of safety and belonging, and overall satisfaction with their school experience. See Appendix B for details on survey questions.

The staff questionnaire captures responses from principals, other administrators (such as assistant principals), teachers, school administrative assistants, counselors, and other school staff, providing comprehensive insights into educators' and staff members' experiences and perceptions of the school climate and capturing their preparedness to address climate issues and assessments of the school environment. The survey has a high response rate of 82% across the years in my analysis sample. The survey measures several key dimensions of school climate, including professional development preparedness (specifically regarding antibullying training), engagement in addressing bullying incidents, and personal safety perceptions during school hours. Staff members also evaluate the school's disciplinary climate through their perceptions of adult respect toward students and the fairness and effectiveness of the school's discipline. Additionally, staff assess the prevalence and severity of various climate challenges within their schools, including harassment and bullying among students, physical fighting, general disruptive behavior, racial or ethnic conflicts, and student respect toward staff members. See Appendix B for details on survey questions.

To analyze these multidimensional survey outcomes systematically, I construct summary indices following the methodology outlined in Anderson (2008). This approach involves creating weighted averages of related standardized outcomes, with the weights calculated to maximize the information captured in each index. For example, to create the student bullying index, I combine responses across the four victimization categories (physical aggression, relational aggression, sexual harassment, and appearance-based teasing), taking the proportions of students reporting each type of incident as "not a problem" and creating a summary measure using the Anderson procedure. Similarly, I construct indices for school climate, staff preparedness for handling bullying, perceptions of disciplinary effectiveness, and overall staff assessments of school climate. This indexing approach provides clear, interpretable measures of the treatment effects across conceptually related outcomes. See Appendix B for a list of the questions I used to construct each index.

3.3 Summary statistics

Table 1 compares pretreatment characteristics between the beneficiary schools and similar LAUSD secondary schools in 2016/17. The former served more disadvantaged student populations and had

significantly worse baseline performance across multiple dimensions: higher suspension rates, lower achievement on state standardized tests in math and ELA, and poorer school climate as measured by the student and staff survey indices.

4 Empirical Framework

4.1 Estimation strategy

My empirical approach uses a DID design to estimate the causal effect of the funding intervention on school outcomes. The baseline specification is:

$$Y_{it} = \alpha_i + \lambda_t + \delta \cdot (\text{Lawsuit}_i \times \text{Post}_t) + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome variable, e.g., the suspension rate, for school i in year t, α_i denotes school fixed effects, λ_t denotes year fixed effects, Lawsuit_i is an indicator equal to 1 for schools that benefited from the lawsuit, Post_t is an indicator equal to 1 for years 2018 and after (postsettlement), $\text{Lawsuit}_i \times \text{Post}_t$ is the interaction term capturing the treatment effect, δ is the DID estimator, and ε_{it} is the error term. For statistical inference, I cluster the standard errors at the school level. This clustering approach ensures robustness to arbitrary forms of correlation in the error terms within schools while maintaining the assumption that the errors are independent across schools. School-level clustering is appropriate given that the "treatment assignment" is at the school level and schools represent the primary unit of analysis.

The school fixed effects (α_i) control for time-invariant unobserved differences between treated and control schools. The parameter of primary interest is δ, which measures the average treatment effect of the settlement funding on school outcomes in the postsettlement period.

4.2 Identification strategy

The validity of this DID approach relies on the parallel trends assumption. In this case, this means that in the absence of the lawsuit, the outcomes of the schools that benefited from it would have followed trajectories similar to those of the schools that did not. To provide evidence that this assumption holds, I use an event study specification that estimates:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k=-3}^{2} \delta_k \cdot (\text{Lawsuit}_i \times \mathbf{1}_{t=2018+k}) + \varepsilon_{it} \tag{2}$$

where Y_{it} is the outcome variable, e.g., the suspension rate, for school i in year t, α_i denotes school fixed effects, λ_t denotes year fixed effects, Lawsuit_i is an indicator equal to 1 for schools that benefited from the lawsuit, $\mathbf{1}_{t=2018+k}$ represents indicators for years relative to the 2017/18 settlement (where $k = -1$ is academic year 2016/17 and omitted as the reference), δ_k is the treatment effect coefficients for each of the years relative to settlement, and ε_{it} is the error term, clustered at the school level.

The event study specification allows me to provide support for the the parallel trends assumption by examining whether the treated and control schools exhibited differential pretreatment trends. Evidence of parallel pretreatment trends would support the counterfactual assumption that the treated schools' outcomes would have evolved similarly to control schools' in the absence of the intervention. The pretreatment coefficients (δ_{-3} and δ_{-2}) test whether the treated and control schools' outcomes followed parallel trends before the intervention. These coefficients should be statistically insignificant.

As an additional robustness check, I use the synthetic difference-in-differences (SDID) estimator developed by Arkhangelsky et al. (2021). SDID combines strengths from the synthetic control and traditional DID methods. Similarly to synthetic control, SDID reweights the control units and matches the pretreatment trends, thereby weakening reliance on the strict parallel trends assumption. Similarly to DID, SDID remains invariant to additive unit-level shifts and permits valid large-panel inference.

The SDID estimator assigns greater weight to control units whose pretreatment trajectories more closely resemble those of the treated units and emphasizes time periods that are similar to the treatment periods. This weighting scheme enhances robustness by ensuring that comparisons draw primarily on the most relevant controls and time periods, rather than relying equally on all the available data. By focusing on similar units and periods, SDID reduces the potential for bias from poor counterfactual matches, making it particularly well suited for settings where the treatment and control groups may differ in their baseline characteristics.

5 Results

5.1 Effects on school spending

Following the settlement, the beneficiary schools received substantial funding. While the settlement terms mandated expenditure in certain categories such as hiring of staff or professional development

training, schools retained considerable discretion over the allocation of the remaining funds as long as they used them on services for high-need students.

Table 2 presents the DID estimates examining how the settlement funding affected school spending decisions. The results provide strong evidence that the settlement schools used their additional funding to expand staffing capacity, confirming that the financial intervention tangibly improved school resources. Relative to control schools, the treated schools increased their certified staff by approximately 3 per 1,000 students, an effect that is statistically significant at the 1% level and represents a 4.9% increase relative to the baseline level of 63 certified staff per 1,000 students in the treated schools. This substantial increase encompassed teachers providing direct instruction, administrative personnel in certified positions, and specialized pupil services staff such as counselors and psychologists, all critical components of school capacity who require specialized credentials. Additionally, the treated schools expanded their classified staff by almost 2 per 1,000 students, a significant effect at the 10% level corresponding to a 4.8% increase from the baseline of 37 classified staff per 1,000 students. This category includes paraprofessionals, clerical staff, and other support personnel who do not require certification but provide essential operational and instructional assistance. The event study results presented in Panels (a) and (b) in Figure 2 confirm that the pretreatment trends of treated and control schools followed similar paths for both staffing outcomes, supporting the parallel trends assumption underlying the DID design. In summary, the schools that received settlement funds meaningfully expanded their human capital resources rather than using the money for other purposes.

5.2 Effects on student outcomes and enrollment

Table 3 presents the estimated effects of the settlement funding on key measures of school climate and academic achievement, and Figures 3 and 4 present the results from the event study analyses. Beginning with school climate, the treated schools experienced a significant reduction in suspension rates of 0.75 pp, which represents a 44% decrease relative to their baseline suspension rate of 1.7%. This reduction is more than twice the magnitude of the effect that Adukia et al. (2025) find for a restorative justice intervention in Chicago schools.

Turning to academic achievement, treated schools show improvements in both subjects tested. In English Language Arts (ELA), schools achieved a statistically significant increase of 2.8 percentage points in the percentage of students meeting the state standard, significant at the 1% level and corresponding to a 12.8% improvement from the baseline of 21.9%. In mathematics, treated

schools increased the percentage of students who nearly met or met standards by 1.5 percentage points, though this effect is not statistically significant. One caveat on interpreting these results causally is that because of data limitations I observe only two pre-periods (See Figure 4).

To address potential concerns about the validity of the parallel trends assumption and improve the comparability of the two groups of schools, I use an alternative estimation strategy. Specifically, I use an SDID estimator (Arkhangelsky et al., 2021). Table 4 presents results largely consistent with the main findings, providing evidence that the observed treatment effects are unlikely to be driven by violations of the parallel trends assumption or by observable differences between the treated and control schools.

It is possible that the settlement and the money schools received affected enrollment and student body composition, although what direction we should expect for this effect is theoretically ambiguous. The settlement may have generated two opposing forces: On one hand, the additional resources and potentially improved school quality could have attracted new students to the treated schools, while on the other hand, the fact that a school was identified as needing remedial funding through litigation could have signaled poor performance and deterred enrollment.[6] Moreover, it is unclear which types of students may decide to move, if any.

If the treated schools were perceived as improving because of the additional resources, they may have attracted students of varying academic abilities, such as high-achieving students seeking better opportunities, average students drawn to enhanced programs, or struggling students hoping for additional support. Each scenario would imply a different effect: The arrival of more high-achieving students would make outcomes such as test scores and suspension rates appear better than the true treatment effect, while an influx of struggling students could make these same outcomes appear worse. Conversely, if the litigation stigma caused the treated schools to be perceived as low-quality, high-achieving students might have exited to other schools, leaving behind a more disadvantaged student body.

I thus examine whether the settlement affected student enrollment patterns and demographic composition across the treated and control schools. Table 5 presents the estimated effects on enrollment and student demographics, while Figures 5, 6, and 7 display the event study results. The results reveal changes in the size and composition of the treated schools' student bodies. On average, the treated schools experienced an enrollment decline of 48 students (approximately

[6]The settlement received substantial news coverage. See, for example, Article 1, Article 2, Article 3, Article 4, and Article 5.

5.7% from their baseline levels), suggesting that the intervention led to net outflows rather than enrollment increases. In terms of student body composition, the treated schools experienced a 0.9 pp decrease in Black student enrollment and a 3.6 pp increase in their enrollments of students eligible for free lunch, indicating a concentration of economic disadvantage. Additionally, the share of students belonging to other racial categories increased by 0.5 pp, a small change given that each of these groups represents less than 2% of enrollment. The enrollment shares of Hispanic, White, and Asian students remained statistically unchanged. These patterns suggest that families with greater school choice options, likely those with more resources and flexibility, left the treated schools after the settlement, while more economically disadvantaged families remained or were drawn to these schools.

The results presented thus far raise two potential concerns about the validity of my estimates for enrollment. First, the event study in Figure 5 suggests the possibility of preexisting trends in enrollment, which would indicate violations of the parallel trends assumption. Second, even if the enrollment changes reflect the true treatment effects, the shifts in student composition could bias my main findings if the families who left the treated schools included those with systematically different suspension risks.

I address the concern about the parallel trends assumption using two complementary approaches. First, I conduct formal sensitivity analysis following Rambachan and Roth (2023), which provides methods to bound the treatment effects under a relaxed version of the parallel trends assumption. Figure 8 shows that the original confidence interval (shown in red) excludes zero, but the robust confidence intervals become progressively wider as we allow for larger violations of parallel trends (measured by $\bar{M}$). At $\bar{M} = 0.5$, the robust 95% confidence interval already includes zero, indicating that even modest deviations from parallel trends (equivalent to half the size of the maximum pretreatment trend violation) are sufficient to overturn the statistical significance of the enrollment effect. This suggests the enrollment decline may not be robust to realistic violations of the parallel trends assumption. Second, I employ the SDID estimator. Table 6 shows that the SDID estimate for enrollment is reduced by half and becomes statistically insignificant (p-value = 0.10), also suggesting that the enrollment decline likely reflects preexisting trends rather than a causal effect of the settlement.

While the evidence suggests that the enrollment changes likely reflect preexisting trends rather than treatment effects, I nonetheless conduct a conservative bounding exercise that treats the observed compositional changes as real effects. This approach addresses the concern that shifts

in student composition could bias my main estimates. If the families who left included students with higher (or lower) suspension risk, the observed decline in disciplinary incidents could be overestimated (or underestimated), reflecting changes in student body composition rather than changes in school practices alone. To assess this potential bias, I implement a bounding exercise following Horowitz and Manski (2000) that examines how sensitive my estimates are to different assumptions about the characteristics of the students who left.

The bounding approach is informative because I cannot observe the counterfactual suspension outcomes for the students who exited the treated schools, which creates a missing data problem that could bias my treatment effect estimates positively or negatively. To establish bounds around the true treatment effect, I consider five scenarios that span a range of plausible assumptions about the propensity of departing students to be suspended.

In Scenario 1, I assume that the departures concentrated among well-behaved students, that is, students who would not have been suspended under any circumstances. This would suggest that my estimated treatment effects understate the true policy impact, as the remaining student body would be more challenging to discipline. In Scenario 2, I assume that the departing students had the same suspension rate as the treated schools' baseline (1.7%), meaning approximately 0.81 of the 48 students who departed would have been suspended. In Scenario 3, I assume that the departing students had suspension rates matching their demographic group's baseline, with that of Hispanic students being 1.10% and that of Black students 4.67%; this results in approximately 1.11 expected suspensions among the 48 departing students. In Scenario 4, I assume that the departing students had elevated suspension rates reflecting twice the treated schools' baseline (3.4%), meaning approximately 1.63 of the 48 students who departed would have been suspended. In Scenario 5, the most conservative, I assume that the departures were among high-suspension-risk students, with suspension rates twice their demographic baseline (Hispanic students at 2.19% ($2 \times 1.10\%$) and Black students at 9.34% ($2 \times 4.67\%$)), resulting in approximately 2.22 expected suspensions among the 48 departing students. This final scenario represents the worst case and would suggest that my estimated treatment effects overstate the true policy impact.

For each scenario, I impute counterfactual suspension outcomes for the students who would have remained under the control condition, reconstructing what the treated schools' suspension rates would have looked like with their original student composition. I then recompute the treatment effects using these adjusted denominators and suspension counts. This exercise provides bounds on the true treatment effect under different assumptions about selection into departure.

The results have the same sign and similar magnitude across all scenarios (see Table 7). The original estimate shows a 0.75 pp reduction in suspension rates (significant at the 5% level). Under Scenario 1, where well-behaved students departed, the treatment effect increases to -0.82 pp (significant at the 1% level), suggesting even stronger policy impacts than initially estimated. Under Scenario 2, where the departing students had the same suspension rate as the treated schools' baseline, the treatment effect remains substantial at -0.71 pp (significant at the 5% level). Under Scenario 3, where the departing students had suspension rates matching their demographic baseline, the estimated reduction is -0.67 pp (significant at the 5% level). Under Scenario 4, where the departing students had twice the treated schools' baseline suspension rate, the treatment effect is -0.60 pp (significant at the 10% level). Even under Scenario 5, the most conservative, where the departed students were at the highest risk of suspension, with rates twice as high as their demographic baseline, the estimated reduction remains -0.49 pp, although it is not statistically significant.

Across all reasonable bounding assumptions, the sign of my main results remains unchanged, and the magnitude remains similar. Even under the most conservative scenario, where the departing students would have had suspension rates twice their demographic baseline (Scenario 5), the estimated reduction in suspension rates represents meaningful improvements in the disciplinary climate (29% relative to the treated schools' baseline of 1.7%). This robustness suggests that the observed improvements are not a mechanical result of the altered student composition but reflect genuine changes in school practices.

6 Why did suspension rates improve?

The reduction in suspension rates following the funding intervention raises the question of what mechanisms drove this change. While the settlement aimed to improve schools' environment, the observed decrease in suspensions could reflect effects operating through several pathways: genuine improvements in student behavior, shifts in how educators began to respond to disciplinary incidents, or changes in administrative practices around suspension decisions. Understanding these mechanisms is crucial for interpreting the policy's effectiveness and informing future interventions.

To disentangle these competing explanations, I leverage comprehensive survey data from both students and school staff collected through LAUSD's SES. This annual survey captures feedback from teachers, staff, students, and parents across all LAUSD schools and provides detailed measures

of school climate, bullying experiences, and disciplinary practices. The student survey assesses experiences with four key forms of peer victimization on school property (physical aggression, relational aggression, sexual harassment, and appearance-based teasing), alongside six dimensions of school climate including perceptions of adult respect, safety, belonging, and overall satisfaction.

The staff component of the survey provides equally comprehensive insights, capturing responses from principals, administrators, teachers, counselors, and support staff. The staff questionnaire measures preparedness to address climate issues through professional development indicators, engagement in addressing bullying incidents, personal safety perceptions, and assessments of disciplinary fairness and effectiveness. Staff also evaluate the prevalence of various climate challenges including harassment, bullying, physical fighting, disruptive behavior, racial conflicts, and respectfulness of students toward staff.[7]

To analyze these outcomes, I build summary indices following the methodology outlined in Anderson (2008). I create all the indices so that higher positive values consistently indicate better outcomes: improved school climate, reduced bullying, enhanced safety, and more effective disciplinary practices. This uniform scaling allows straightforward interpretation of the treatment effects across all the survey measures, with positive coefficients representing improvements in the school environment and student experiences.

Tables 8 and 9 present the estimated treatment effects on student and staff survey outcomes, respectively, and Figures 9 and 10 show the event study results. Students reported meaningful improvements in overall school climate (coefficient of 0.147, significant at the 5% level, representing a 43% increase from baseline). Despite this positive shift in perceptions of the school environment, reported bullying experiences show no statistically significant change, with a positive point estimate (coefficient of 0.12, not statistically different from zero) suggesting that direct peer-to-peer victimization may have remained stable or potentially increased slightly.

The results for staff point toward changes in educator capacity and approach. Teachers and staff reported significant improvements across all measured dimensions. Staff indicated that bullying incidents had become a less significant problem, decreasing 53% from baseline (0.26 standard deviations (SD)). More importantly, they reported substantial improvements in their ability to handle bullying incidents and received enhanced professional development on these issues, showing a 108% improvement from baseline (0.38 SD). Staff perceptions of disciplinary fairness and effectiveness improved by 88% from baseline (0.31 SD), while overall assessments of school climate

[7]See Appendix B for details on all the questions I included in each index.

increased by 61% from baseline (0.35 SD). All these changes are statistically significant at the 1% level.

These patterns suggest the intervention operated primarily through enhancements to educators' capacity and approaches to discipline, rather than direct changes in student behavior. The lack of change in student-reported bullying experiences, combined with students' improved perceptions of overall school climate, suggests that while peer interactions remained relatively constant, the broader school environment became more supportive. Meanwhile, the substantial improvements in staff preparedness, confidence in handling disciplinary issues, and perceptions of disciplinary effectiveness indicate that the intervention successfully enhanced educators' tools and approaches for managing student behavior. This interpretation aligns with the settlement's emphasis on restorative justice training and suggests that the reduced suspension rates reflect more thoughtful, effective disciplinary practices rather than simply more lenient enforcement or fundamentally different student behavior.

7 Conclusion

This paper provides new evidence on a fundamental question in education policy: Can investing in failing schools help them improve? Using a natural experiment from a 2017 settlement in LAUSD, I examine the effects of a substantial increase in the resources delivered to the district's 50 lowest-performing secondary schools. The settlement provided $1,030 per student annually for three years, a figure representing a 13.5% budget increase on average.

The findings reveal a nuanced picture of how investment affects failing schools. On the positive side, the schools successfully implemented the required changes and achieved meaningful improvements in their disciplinary climate. They increased their employment of certified staff by approximately 3 per 1,000 students and support staff by almost 2 per 1,000 students, which represent 5% increases over baseline levels. More importantly, suspension rates fell by 0.75 pp, a remarkable 44% reduction from pretreatment levels. Survey evidence suggests this improvement reflects genuine changes in educator practices and school climate rather than administrative adjustments, with teachers reporting enhanced capacity to address behavioral issues and more effective disciplinary practices. However, the treated schools also experienced demographic sorting, losing 48 students on average, such that their concentration of economically disadvantaged students rose. Nonetheless, a simple bounding exercise that accounts for this demographic sorting indicates that the settlement

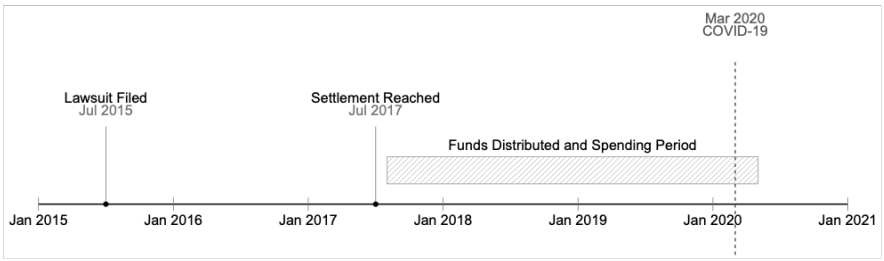
still had meaningful effects on school climate.

These findings extend the robust evidence that money matters for schools to the specific context of failing institutions. I thereby address a critical gap between general school spending research and policy debates about how to improve the lowest-performing schools. While previous studies demonstrate positive effects of increased spending across diverse school populations, my research shows that investment can meaningfully improve practices even in the most challenging educational environments.

The results highlight how important it is to examine outcomes beyond test scores in evaluations of school interventions. Despite the limited changes in academic achievement, the substantial improvements in disciplinary climate represent meaningful progress in noncognitive dimensions of schooling, which matter for students' long-term development. This finding underscores that school quality encompasses multiple dimensions and that interventions may succeed along some margins while showing limited effects on others.

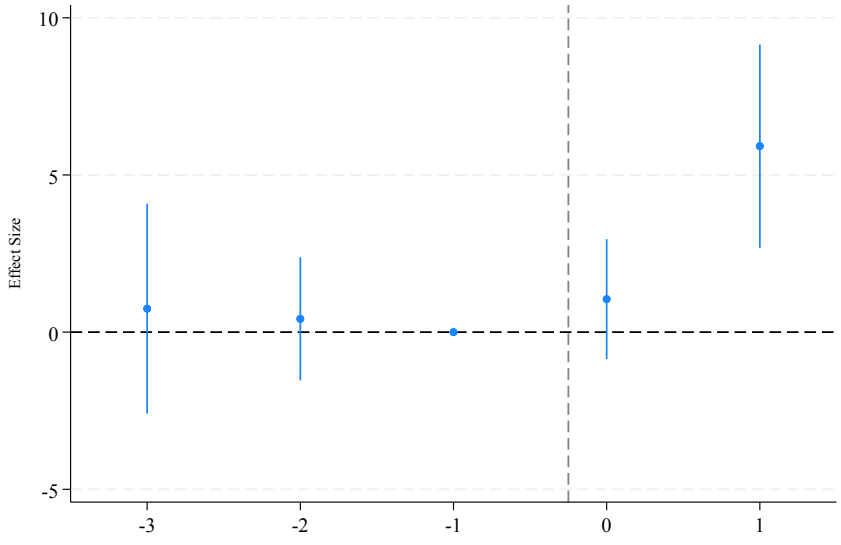
8 Figures and Tables

Figure 1: *Community Coalition v. LAUSD Timeline*

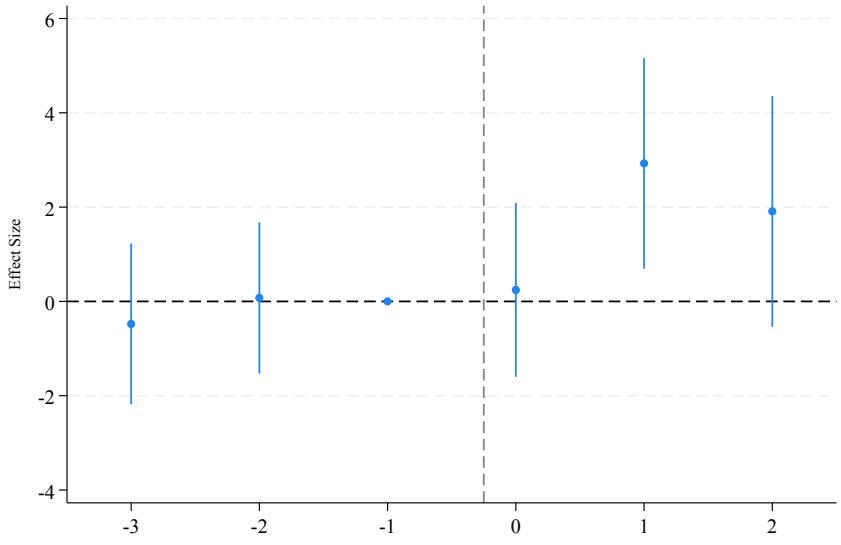


Notes: This figure illustrates the timeline of the LAUSD–ACLU settlement and its implementation. In July 2015, the American Civil Liberties Union (ACLU) and other advocacy organizations filed a lawsuit against the Los Angeles Unified School District (LAUSD) alleging improper use of hundreds of millions of dollars allocated under the Local Control Funding Formula (LCFF). The lawsuit reached settlement in July 2017, with LAUSD agreeing to allocate $150 million specifically for services for high-need students. The settlement funds were distributed over three academic years during the spending period of 2017/18–2019/20. Following the conclusion of the mandated spending period in 2020, the district retained discretionary authority over any remaining unspent funds. The figure shows the key phases of litigation, settlement negotiation, fund distribution, and postsettlement period that form the basis for the empirical analysis in this study.

Figure 2: Effects on spending outcomes



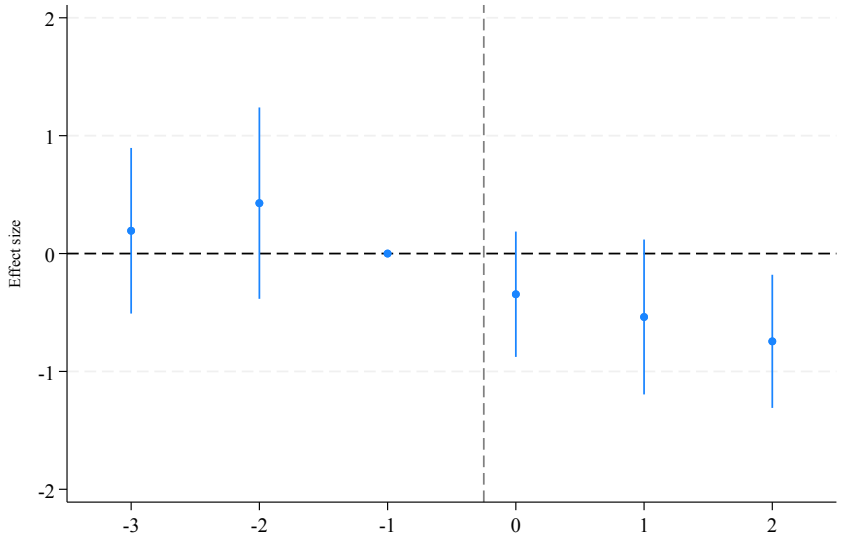
(a) Certified staff



(b) Classified staff

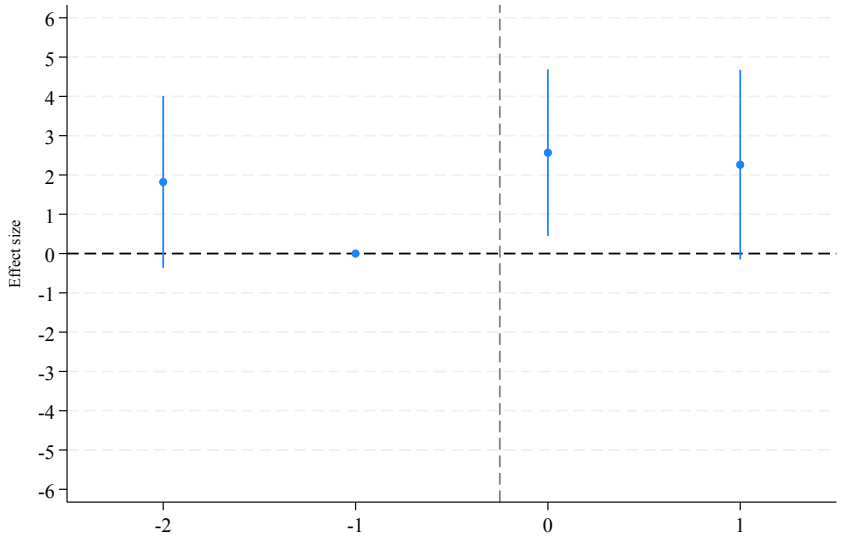
Notes: This figure reports event study estimates of the settlement's effects on school staffing: Panel (a) for certified staff, which includes teachers, administrators, and pupil services personnel requiring credentials, and Panel (b) for classified staff, which includes paraprofessionals, clerical, and other support personnel. All regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 3: Effect on suspension rate

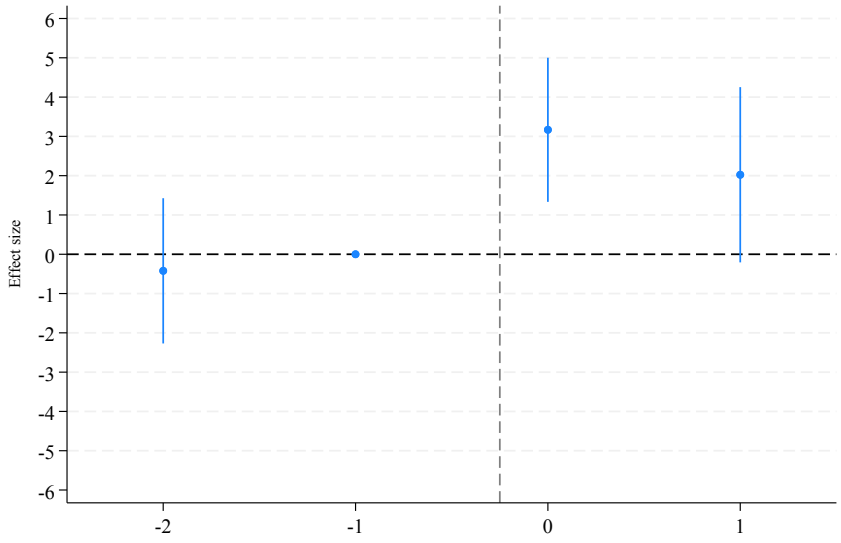


Notes: This figure reports event study estimates of the settlement's effects on the student suspension rate. The suspension rate is measured as the percentage of students suspended at least once during the academic year. The regression includes school and year fixed effects and clusters standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 4: Effects on students' performance outcomes



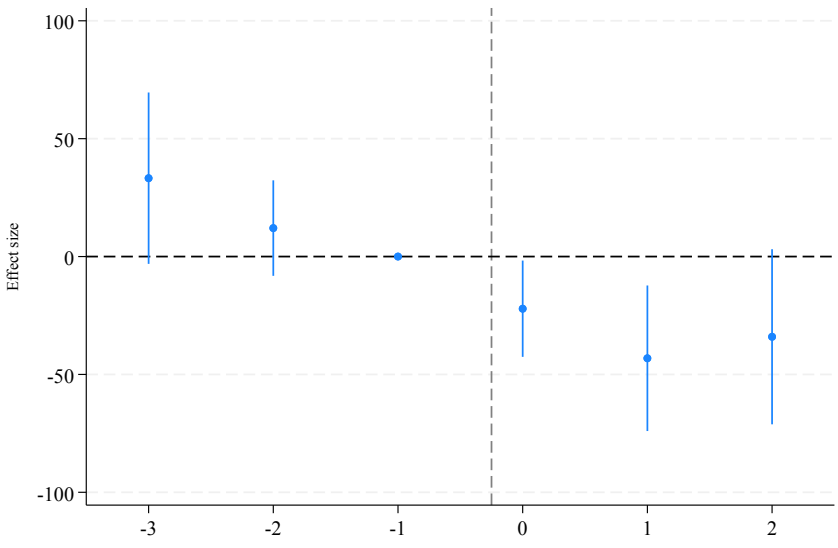
(a) Math



(b) ELA

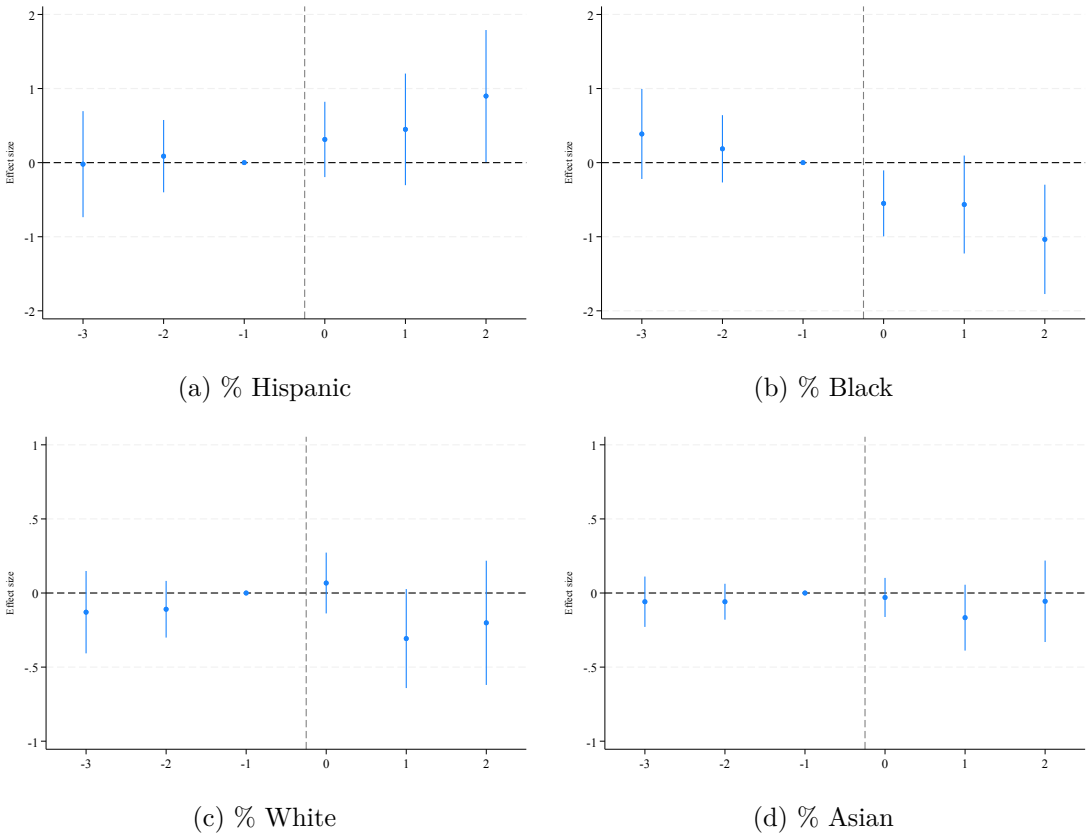
Notes: This figure reports event study estimates of the settlement's effects on students' performance outcomes. The math and English language arts (ELA) outcomes are the percentage of students who met or nearly met the state standards for the math standardized test (Panel (a)) and the percentage of students who met the state standards for the ELA standardized test (Panel (b)), respectively. The regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 5: Effect on enrollment



Notes: This figure reports event study estimates of the settlement's effects on school enrollment. The regression includes school and year fixed effects and clusters standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 6: Effects on racial composition



(a) % Hispanic

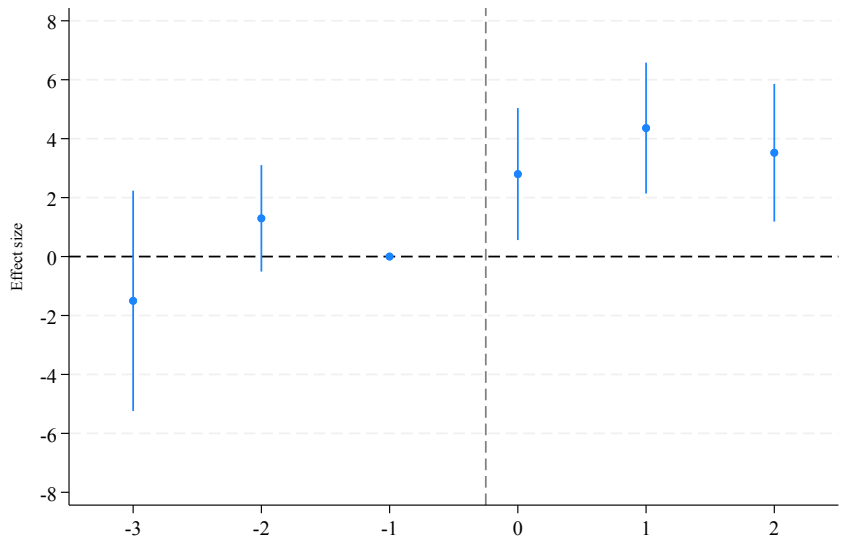
(b) % Black

(c) % White

(d) % Asian

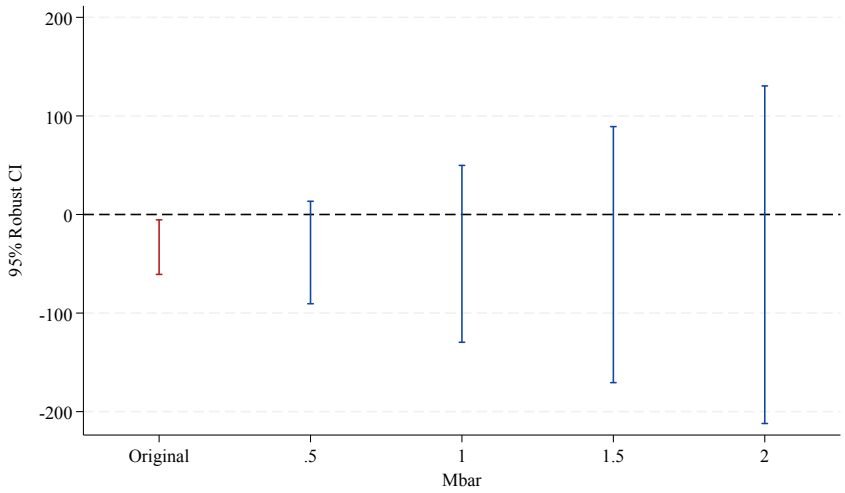
Notes: This figure reports event study estimates of the settlement's effects on schools' racial composition. The regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 7: Effects on percentage of students eligible for free lunch



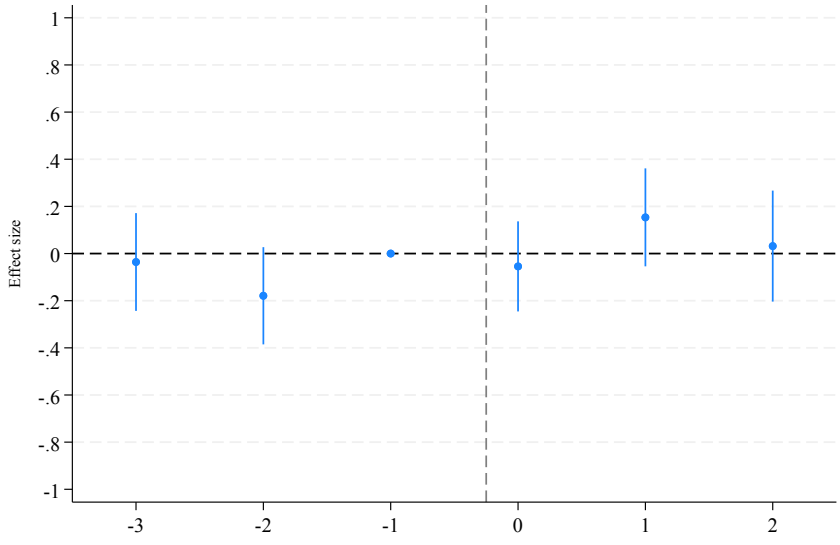
Notes: This figure reports event study estimates of the settlement's effects on the percentage of students eligible for free lunch. The regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period.

Figure 8: Sensitivity analysis of enrollment effects

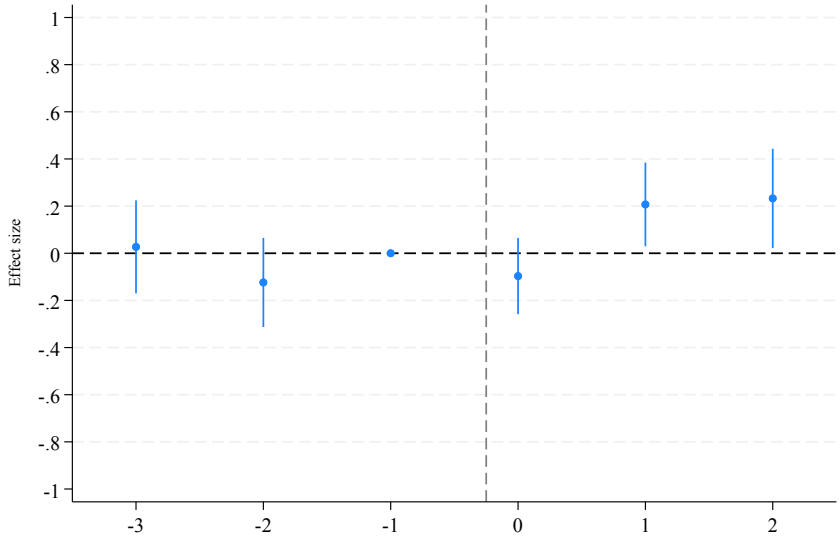


Notes: This figure shows sensitivity analysis results following Rambachan and Roth (2023). The red confidence interval represents the original estimate. Blue confidence intervals show robust bounds under increasingly relaxed parallel trends assumptions, where Mbar represents the maximum allowed violation of parallel trends relative to pretreatment trends.

Figure 9: Effects on students' school experience indices



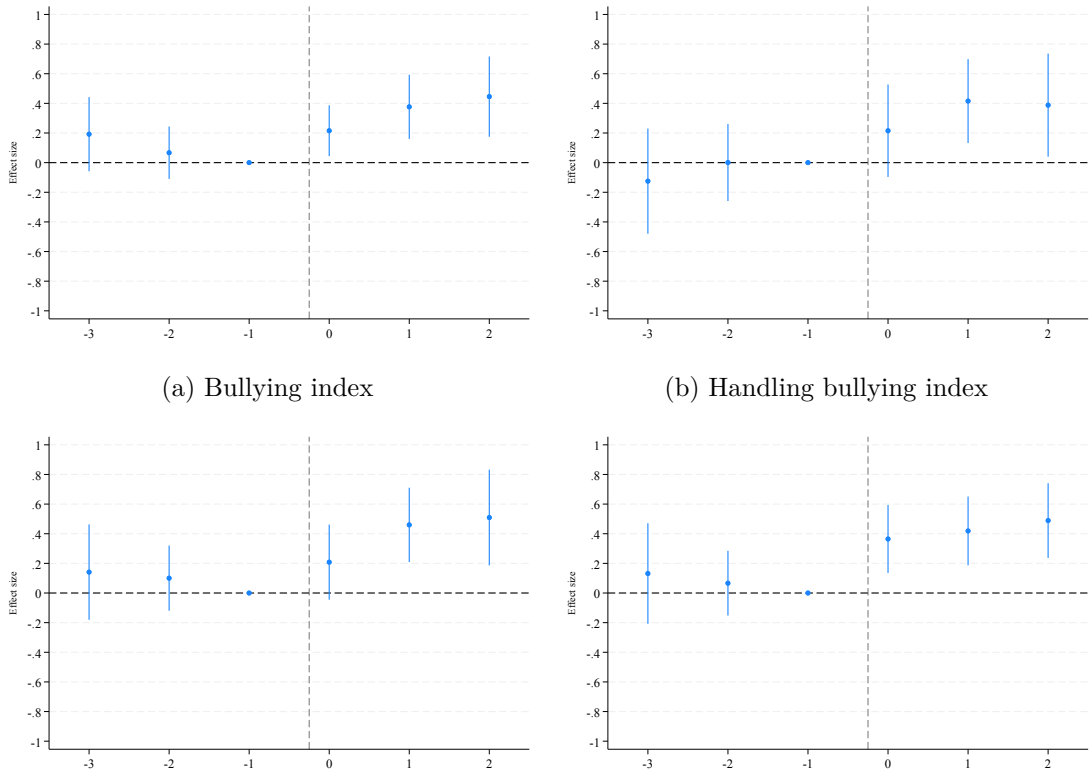
(a) Bullying index



(b) School climate index

Notes: This figure reports event study estimates of the settlement's effects on survey outcomes about students' school experience. The regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period. I construct the survey indices following Anderson (2008) and normalize them to have mean zero and standard deviation one. The bullying index in Panel (a) combines four survey questions about students' experiences with physical aggression (being pushed, shoved, slapped, hit, or kicked), relational aggression (being the subject of mean rumors or lies), sexual harassment (experiencing sexual jokes, comments, or gestures), and appearance-based teasing. The school climate index in Panel (b) includes six survey questions on adult respect and help, feelings of safety and belonging at the school, and overall satisfaction with their school experience.

Figure 10: Effects on staff's school experience indices



(a) Bullying index

(b) Handling bullying index

(c) Discipline index

(d) School climate index

Notes: This figure reports event study estimates of the settlement's effects on survey outcomes on staff's school experience. The regressions include school and year fixed effects and cluster standard errors at the school level. The x-axis shows years relative to the settlement implementation (year 0). The vertical dashed line indicates the start of the settlement period. I construct the survey indices following Anderson (2008) and normalize them to have mean zero and standard deviation one. On all indices, a higher value means a better outcome. The bullying index in Panel (a) includes three questions on how much of a problem staff thinks bullying, physical fighting, and discrimination is among students. The handling bullying index in Panel (B) contains two survey questions related to receiving professional development on preventing bullying and whether they address bullying. The discipline index in Panel (c) includes two questions about the fairness and effectiveness of discipline at school. The school climate index in Panel (d) has four questions on safety, respect, and disruptive behavior. Staff includes teachers, administrators, counselors, and other school personnel.

Table 1: Summary statistics

	Lawsuit beneficiary					Comparison group				
	Mean	SD	Min	Max	N	Mean	SD	Min	Max	N
Demographics										
N of students enrolled	833.59	407.55	288	1961	49	1187.20	682.70	189	2953	121
% Hispanic	82.09	16.16	28	99	49	80.42	19.17	20	99	121
% Black	14.42	16.39	0	69	49	7.51	12.19	0	71	121
% White	1.21	0.89	0	5	49	4.98	7.32	0	35	121
% Asian	0.89	2.18	0	14	49	3.20	5.16	0	26	121
% less than 2% races	1.39	1.39	0	5	49	3.90	6.06	0	45	121
% free lunch	87.41	5.42	74	99	49	83.61	7.85	53	97	121
% english learner	26.35	6.93	9	42	49	15.12	8.49	0	41	121
% Homeless	4.21	1.88	1	10	49	3.03	1.98	0	12	121
% Foster	1.29	1.02	0	4	49	0.56	0.40	0	2	121
Employees										
Certified staff per 1,000 st	63.56	11.90	48	99	49	54.81	6.47	44	77	121
Classified staff per 1,000 st	37.80	14.54	16	83	49	27.58	9.65	6	61	121
Main outcomes										
Suspension rate	1.70	2.11	0	12	49	0.64	1.11	0	9	121
% near or met math standard	28.30	8.89	9	48	49	44.64	8.41	25	77	121
% met ELA standard	21.90	8.42	5	41	49	32.48	7.61	18	50	121
% not meet ELA standard	44.37	16.40	8	78	49	25.65	13.03	0	55	121
% not meet math standard	69.56	9.94	45	91	49	46.88	12.40	2	71	121
Survey indexes outcomes: students										
Bullying experiences	-0.51	1.11	-2.71	1.42	44	-0.42	1.04	-2.85	2.00	116
School climate	-0.73	0.68	-2.01	1.02	44	-0.21	0.94	-1.88	3.95	116
Survey indexes outcomes: staff										
Handling bullying	-0.35	1.06	-1.96	3.09	45	0.43	1.02	-1.86	3.34	107
Bullying among students	-0.49	1.12	-1.97	2.02	45	0.36	0.98	-1.42	2.91	107
Discipline	-0.35	0.82	-1.65	2.70	45	0.53	1.11	-1.41	3.79	107
School climate	-0.57	0.83	-1.91	1.47	45	0.47	1.03	-1.40	3.70	107
N Schools			49					121		

Notes: Summary statistics correspond to academic year 2016/17. The comparison group includes all public, noncharter secondary schools in Los Angeles Unified School District outside of the North West district. I construct survey indices following Anderson (2008) and normalize them to have mean zero and standard deviation one. On all indices, a higher value means a better outcome. For students, the bullying index combines four survey questions about students' experiences with physical aggression (being pushed, shoved, slapped, hit, or kicked), relational aggression (being the subject of mean rumors or lies), sexual harassment (experiencing sexual jokes, comments, or gestures), and appearance-based teasing. The school climate index includes six survey questions on adult respect and help, feelings of safety and belonging at the school, and overall satisfaction with their school experience. For staff, the handling bullying index contains two survey questions related to receiving professional development on preventing bullying and whether they address bullying. The bullying index includes three questions on how much of a problem staff thinks bullying, physical fighting, and discrimination are among students. The discipline index includes two questions about the fairness and effectiveness of discipline at school. The school climate index has four questions on safety, respect, and disruptive behavior.

Table 2: Effects on school spending outcomes

	(1)	N
Certified staff per 1,000 st	3.093**	850
	(1.199)	
Classified staff per 1,000 st	1.828*	1020
	(0.942)	

Notes: Certified staff includes teachers, administrators, and pupil services personnel requiring credentials. Certified staff data are available for years through academic year 2018/19. Classified staff includes paraprofessionals, clerical, and other support personnel not requiring certification. All regressions include school and year fixed effects. Standard errors are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effects on students' outcomes

	(1)	N
Suspension rate	-0.750**	1020
	(0.313)	
% Math nearly met or met	1.512	679
	(1.033)	
% ELA met	2.804***	679
	(0.867)	

Notes: All regressions include school and year fixed effects and cluster standard errors at the school level. The suspension rate is measured as the percentage of students suspended at least once during the academic year. The math and ELA outcomes are the percentage of students who met or nearly met the state standards for the math standardized test and the percentage of students who met the state standards for the English language arts standardized test. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Robustness: Effects on students' outcomes

	Synthetic DiD (1)
Suspension rate (%)	-0.575*
	(0.321)
% Nearly met/met math	2.184**
	(1.019)
% Met ELA	2.759***
	(0.820)

Notes: Estimates from the synthetic difference-in-differences estimation. Suspension rate is measured as the percentage of students suspended at least once during the academic year. The math and ELA outcomes are the percentage of students who met or nearly met the state standards for the math standardized test, and the percentage of students who met the state standards for the English Language Arts standardized test. Bootstrapped standard errors in parentheses (1,000 replications). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effects on enrollment and student body composition

	(1)	N
N of students enrolled	-48.203**	1020
	(19.001)	
% Hispanics	0.531	1020
	(0.344)	
% Blacks	-0.908***	1020
	(0.285)	
% Whites	-0.067	1020
	(0.145)	
% Asian	-0.045	1020
	(0.116)	
% Other less 2% races	0.489***	1020
	(0.117)	
% Free lunch	3.629***	1020
	(0.827)	

Notes: All regressions include school and year fixed effects. Standard errors are clustered at the school level. Demographic variables are measured as percentages of total enrollment. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Robustness: Effects on enrollment and student body composition

	Synthetic DiD
	(1)
N of students enrolled	-23.051
	(13.994)
% Hispanic	0.551
	(0.332)
% Black	-0.767***
	(0.274)
% White	-0.129
	(0.154)
% Asian	-0.109
	(0.093)
% Free lunch	3.143***
	(0.806)

Notes: Estimates from synthetic difference-in-differences estimation. Demographic variables are measured as percentages of total enrollment. Bootstrapped standard errors in parentheses (1,000 replications). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Treatment effects under different assumptions about characteristics of departing students

Variable	Original	Scenario 1	Scenario 2	Scenario 3	Scenario 4	Scenario 5
Treatment Effect	-0.750**	-0.815***	-0.708**	-0.669**	-0.600*	-0.485
	(0.313)	(0.308)	(0.307)	(0.307)	(0.307)	(0.307)
Observations	1020	1020	1020	1020	1020	1020

Notes: This table presents treatment effect estimates from difference-in-differences regressions under different assumptions about the characteristics of the students who departed the treated schools. The dependent variable is the suspension rate. Original shows the baseline estimates. Scenario 1 assumes that the departing students were well behaved (zero suspension probability). Scenario 2 assumes that the departing students had the same suspension rate as the treated schools' baseline (1.7%). Scenario 3 assumes that the departing students had suspension rates matching their demographic group's baseline (1.10% for Hispanic, 4.67% for Black students). Scenario 4 assumes that the departing students had suspension rates twice the treated schools' baseline (3.4%). Scenario 5 assumes that the departing students were high risk, with suspension rates twice the baseline for their demographic group (2.19% for Hispanic, 9.34% for Black students). All regressions include school and year fixed effects. Standard errors clustered at the school level are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects on students' school experience indices

	(1)	N
Bullying index	0.115	960
	(0.077)	
School climate index	0.147**	960
	(0.071)	

Notes: All regressions include school and year fixed effects. Standard errors are clustered at the school level. I construct the survey indices following Anderson (2008) and normalize them to have mean zero and standard deviation one. The bullying index in Panel (a) combines four survey questions about students' experiences with physical aggression (being pushed, shoved, slapped, hit, or kicked), relational aggression (being the subject of mean rumors or lies), sexual harassment (experiencing sexual jokes, comments, or gestures), and appearance-based teasing. The school climate index in Panel (b) includes six survey questions on adult respect and help, feelings of safety and belonging at the school, and overall satisfaction with their school experience. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Effects on staff's school experience indices

	(1)	N
Bullying index	0.260***	912
	(0.082)	
Handling bullyng index	0.381***	911
	(0.113)	
Discipline index	0.312***	912
	(0.106)	
School climate index	0.358***	912
	(0.095)	

Notes: All regressions include school and year fixed effects. Standard errors are clustered at the school level. I construct the survey indices following Anderson (2008) and normalize them to have mean zero and standard deviation one. On all indices, a higher value means a better outcome. The bullying index in Panel (a) includes three questions on how much of a problem staff thinks bullying, physical fighting, and discrimination are among students. The handling bullying index in Panel (b) contains two survey questions related to receiving professional development on preventing bullying and whether they address bullying. The discipline index in Panel (c) includes two questions about the fairness and effectiveness of discipline at school. The school climate index in Panel (d) has four questions on safety, respect, and disruptive behavior. Staff includes teachers, administrators, counselors, and other school personnel. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

References

Abott, Carolyn, Vladimir Kogan, Stéphane Lavertu, and Zachary Peskowitz, "School District Operational Spending and Student Outcomes: Evidence from Tax Elections in Seven States," *Journal of Public Economics*, March 2020, *183*, 104142.

Adukia, Anjali, Benjamin Feigenberg, and Fatemeh Momeni, "From Retributive to Restorative: An Alternative Approach to Justice in Schools," Working Paper 31675, National Bureau of Economic Research, Cambridge, MA February 2025. Revised February 2025.

Anderson, Michael L., "Multiple Inference and Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects," *Journal of the American Statistical Association*, 2008, *103* (484), 1481–1495.

Arkhangelsky, Dmitry, Susan Athey, David A. Hirshberg, Guido W. Imbens, and Stefan Wager, "Synthetic Difference-in-Differences," *American Economic Review*, 2021, *111* (12), 4088–4118.

Baron, E. Jason, "School Spending and Student Outcomes: Evidence from Revenue Limit Elections in Wisconsin," *American Economic Journal: Economic Policy*, February 2022, *14* (1), 1–39.

Biasi, Barbara, "What Works and For Whom? Effectiveness and Efficiency of School Capital Investments Across The U.S.," 2024.

Booher-Jennings, Jennifer, "Below the Bubble: 'Educational Triage' and the Texas Accountability System," *American Educational Research Journal*, 2005, *42* (2), 231–268.

Candelaria, Christopher A. and Kenneth A. Shores, "Court-Ordered Finance Reforms in the Adequacy Era: Heterogeneous Causal Effects and Sensitivity," *Education Finance and Policy*, January 2019, *14* (1), 31–60.

Cullen, Julie Berry and Randall Reback, "Tinkering Toward Accolades: School Gaming under a Performance Accountability System," in Timothy J. Gronberg and Dennis W. Jansen, eds., *Advances in Applied Microeconomics*, Vol. 14, Bingley: Emerald Group Publishing Limited, 2006, pp. 1–34.

Dee, Thomas S. and Brian Jacob, "The Impact of No Child Left Behind on Student Achievement," *Journal of Policy Analysis and Management*, 2011, *30* (3), 418–446.

Figlio, David N., "Testing, Crime, and Punishment," *Journal of Public Economics*, 2006, *90*, 837–851.

_ **and Cecilia Elena Rouse**, "Do Accountability and Voucher Threats Improve Low-Performing Schools?," *Journal of Public Economics*, 2006, *90* (1–2), 239–255.

_ **and Lawrence S. Getzler**, "Accountability, Ability, and Disability: Gaming the System?," in Timothy J. Gronberg and Dennis W. Jansen, eds., *Advances in Applied Microeconomics*, Vol. 14, Bingley: Emerald Group Publishing Limited, 2006, pp. 35–49.

Goulas, Sofoklis, "Underachieving and Underenrolled: Chronically Low-Performing Schools in the Post-Pandemic Era," Technical Report, Thomas B. Fordham Institute September 2024.

Horowitz, Joel L. and Charles F. Manski, "Nonparametric Analysis of Randomized Experiments with Missing Covariate and Outcome Data," *Journal of the American Statistical Association*, 2000, *95* (449), 77–84.

Hyman, Joshua, "Does Money Matter in the Long Run? Effects of School Spending on Educational Attainment," *American Economic Journal: Economic Policy*, November 2017, *9* (4), 256–280.

Jackson, C. Kirabo and Claire L. Mackevicius, "What Impacts Can We Expect from School Spending Policy? Evidence from Evaluations in the United States," *American Economic Journal: Applied Economics*, January 2024, *16* (1), 412–446.

_ , Rucker C. Johnson, and Claudia Persico, "The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms *," *The Quarterly Journal of Economics*, February 2016, *131* (1), 157–218.

Jacob, Brian A., "Accountability, Incentives and Behavior: The Impact of High-Stakes Testing in the Chicago Public Schools," *Journal of Public Economics*, 2005, *89* (5–6), 761–796.

Jacob, Brian and Steven D. Levitt, "Rotten Apples: An Investigation of the Prevalence and Predictors of Teacher Cheating," *Quarterly Journal of Economics*, 2003, *118* (3), 843–877.

Jang, Youngsoo and Minchul Yum, "Aggregate and Intergenerational Implications of School Closures: A Quantitative Assessment," *American Economic Journal: Macroeconomics*, 2024, *16* (3), 90–130.

Lafortune, Julien, Jesse Rothstein, and Diane Whitmore Schanzenbach, "School Finance Reform and the Distribution of Student Achievement," *American Economic Journal: Applied Economics*, April 2018, *10* (2), 1–26.

Neal, Derek and Diane Whitmore Schanzenbach, "Left Behind by Design: Proficiency Counts and Test-Based Accountability," *The Review of Economics and Statistics*, May 2010, *92* (2), 263–283.

Rambachan, Ashesh and Jonathan Roth, "A More Credible Approach to Parallel Trends," *Review of Economic Studies*, 2023, *90* (5), 2555–2591.

Reback, Randall, "Teaching to the Rating: School Accountability and the Distribution of Student Achievement," *Journal of Public Economics*, 2008, *92* (5–6), 1394–1415.

_ , Jonah Rockoff, and Heather L. Schwartz, "Under Pressure: Job Security, Resource Allocation, and Productivity in Schools under No Child Left Behind," *American Economic Journal: Economic Policy*, 2014, *6* (3), 207–241.

Schueler, Beth E., Catherine Armstrong Asher, Katherine E. Larned, Sarah Mehrotra, and Cynthia Pollard, "Improving Low-Performing Schools: A Meta-Analysis of Impact Evaluation Studies," *American Educational Research Journal*, October 2022, *59* (5), 975–1010.

Online Appendix for:
The effects of investing in failing schools: evidence from Los Angeles Unified School District

Antonia Vazquez

A Settlement Implementation Requirements

Authorized Expenditure Categories. Schools could spend additional funds on six types of services for high-need students: (1) significant increased investments in high-need students, including academic support and mental health and social-emotional support; (2) increasing A-G and Advanced Placement course access for high school students; (3) Linked Learning (an approach that incorporates rigorous academics, career and technical education, work-based learning, and student supports); (4) school climate initiatives, including restorative justice; (5) high school graduation and student recovery for drop-out prevention; and (6) parent and community engagement.

Tiered Support Structure. School tiers were determined by the total funding amount each school received, calculated using the school's duplicated student count and roughly corresponding to enrollment size. These tiered school climate bundles, known as Achievement through Support Teams (ATS), provide wellness, restorative, child welfare and attendance, dropout prevention, intervention, and recovery, and trauma-informed supports to schools.

Tier 1 schools needed to hire one Restorative Justice Teacher Advisor. Tier 2-3 schools needed either a Pupil Services and Attendance Counselor or a Psychiatric Social Worker plus a Restorative Justice advisor (with schools choosing the Psychiatric Social Worker also receiving Resilience Classroom Curriculum). Tier 4 schools required all three positions (PSA Counselor, Psychiatric Social Worker, and Restorative Justice advisor) plus the mandatory Resilience Classroom Curriculum. The Resilience Classroom Curriculum includes resiliency screenings, trauma-informed teacher professional development, and after-school family resiliency education.

B Survey Items Used in Analysis

This appendix lists the survey questions used to construct the outcome indices analyzed in this study.

B.1 Student Survey Questions

Bullying Experiences Index

How many times have other students from your school... (0 Times, 1 Time, 2 or 3 Times, 4 or More Times)

- Pushed, shoved, slapped, hit, or kicked you when they weren't just kidding around?
- Had mean rumors or lies been spread about you?
- Had sexual jokes, comments, or gestures made to you?
- Been teased about what your body looks like?

School Climate Index

How strongly do you agree or disagree with the following statement about your school?

- I am happy to be at this school
- I feel like I am part of this school
- Adults at this school treat all students with respect
- I feel safe in this school
- If I told a teacher or other adult at this school that another student was bullying me, they would try to help me
- This school is a supportive and inviting place for students to learn

B.2 Staff Survey Questions

Bullying Perceptions Index

How much of a problem at this school is...

- Harassment or bullying among students?
- Physical fighting between students?
- Racial/ethnic conflict among students?

Professional Development Index

Please indicate how much you agree or disagree with the following statement about this school

- I have received professional development on preventing bullying
- I address bullying that occurs in my school

Disciplinary Effectiveness Index

Please indicate how much you agree or disagree with the following statement about this school

- This school handles discipline problems fairly
- This school effectively handles student discipline and behavioral problems

School Climate Index

Please indicate how much you agree or disagree with the following statement about this school

- I feel safe on school grounds during the day
- Adults at this school treat all students with respect

How much of a problem at this school is...

- Disruptive student behavior
- Lack of respect for staff by students