Eliminating In-School Lead Exposure: Evidence from Baltimore's Bottled Water Intervention

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Abstract

Decades of public policy have reduced airborne lead exposure but overlook water-based exposure, particularly in schools. Even low levels of lead harm children's development, yet effects of school-age exposure remain unknown. This paper evaluates Baltimore City Public Schools' 2007 switch from tap to bottled drinking water over lead contamination concerns. Using stacked synthetic control methods, I find this policy increased testing proficiency by 4-6 percentage points, with larger effects for lower-achieving, Title I eligible, and less diverse schools. These findings highlight the educational consequences of in-school environmental hazards, underscoring the need to address aging school infrastructures.

1 Introduction

Lead exposure is harmful to children's development, even at very low levels (Aizer & Currie, 2019; Aizer, Currie, Simon, & Vivier, 2018; Zhang et al., 2013). Over the past 50 years, U.S. policies[1] banning lead in gasoline, paint, and plumbing materials have driven historic reductions in blood lead levels. Schools, where children spend much of their day and consume a large share of their water, largely fall outside of these regulations. This paper evaluates Baltimore City Public Schools' district-wide bottled water provision policy, which effectively eliminated school-based lead exposure, to determine if this is a cost-effective way to reduce lead exposure and improve academic performance.

Despite overall progress, millions of Americans – especially children – continue to face exposure to lead in school drinking water, where monitoring and remediation are inconsistent. In public water systems, lead concentrations above 15 parts per billion (ppb) require remedial action, yet no amount of lead is considered safe for children. These regulations do not require testing within schools, leaving exposures from internal plumbing and fixtures largely unaccounted for.

Reports from public water utilities substantially underrepresent in-school exposure. In California, hundreds of schools reported lead levels above 15 ppb while no water utilities reported lead above 15 ppb (Lobo, Laraway, & Gadgil, 2022). Testing within schools is also scarce: in 2020, only 43% of U.S. schools test their drinking water, and 37% of those identified elevated levels of lead (Office of Government Accountability, 2024). One study reported that 13-81% of schools within a state had at least one water sample exceeding 5 ppb of lead (Cradock et al., 2022). Public concern about lead in water intensified after the Flint Water Crisis in 2015 but school lead exposure long predates Flint. Between 2000 and 2001, 57.4% of Philadelphia schools tested above 20 ppb (Bryant, 2004) and similar patterns have been documented nationwide (Casey, 2025; Huang, 2024; Rumpler, 2022). Given that

[1] Residential Lead-Based Paint Hazard Reduction Act of 1992 (Paint), Clean Air Act (Gasoline), Clean Water Act & Safe Drinking Water Act (Water)

children consume much of their daily water at school, these exposures are widespread, under recognized, and often unremediated.

Irrespective of the nationwide scale of this problem, comprehensive policy responses remain rare. School districts have largely not implemented systematic solutions or received sufficient funding support for regular testing and remediation. Baltimore City Public Schools ("Baltimore") stands out as a notable exception. Faced with persistent contamination, prohibitive testing costs, and limited government or public resources to support costly large-scale infrastructure replacement, the district shut off all drinking water fountains and installed drinking water coolers in all district schools in November 2007 (Figure A.5).

The intervention effectively eliminated all waterborne in-school lead exposure for the 140 schools within the district, regardless of individual school conditions (Bowie, 2016; Bowie & Prudente, 2019). This policy followed the district's history of non-compliance and failure to implement alternative measures [2]. What began as a temporary measure has become a long-running bottled water program that currently serves more than 80,000 students daily and is being considered as a unique and replicable solution by other districts and states currently facing dangerously antiquated school infrastructure that puts their students at risk.

This policy provides a unique opportunity to study school-aged children's lead exposure and the immediate effects of eliminating in-school exposure on educational outcomes. In schools, there are limited outside options for drinking water, so compliance of students with the new drinking water is expected to be high. Further, schools are largely representative of the population of children because they are large public institutions with compulsory attendance. Furthermore, children spend approximately 6 hours of their day in school, and the EPA estimates that they consume roughly half of their daily water intake during this time (2000). By isolating exposure within schools, this policy allows me to expand our understanding of lead's negative impacts in a policy-relevant environment where children's

[2] Beginning in the 1990s, the district attempted regular testing, temporarily turning off contaminated fountains, or flushing all drinking water outlets daily. These efforts were largely unsuccessful and inconsistently implemented.

daily water intake is monitored, underscoring the importance of schools as a critical setting for studying children's environmental exposures.

In addition to its policy relevance, my research makes three contributions to the existing literature on pollution and adolescent lead exposure. First, Baltimore's transition to bottled water in schools affects children over the age of five, allowing me to examine the effects of lead throughout later childhood. In contrast, existing literature establishes that early-life exposure negatively impacts future testing proficiency, IQ, and behavior (Aizer & Currie, 2019; Aizer et al., 2018; Grosse, Matte, Schwartz, & Jackson, 2002; Grönqvist, Nilsson, & Robling, 2020). These studies rely on blood lead levels measured early in childhood to infer long-run effects. Because blood lead levels capture only a snapshot of exposure, they may misestimate children's current or cumulative burden of exposure, which can vary with genetic factors, health, age, route of exposure, and nutrition (Abelsohn & Sanborn, 2010; Lidsky & Schneider, 2003). A small but growing body of research has begun to examine exposure in school-aged children, showing that both acute shocks of high exposure and cumulative exposure harm academic achievement (Hollingsworth, Huang, Rudik, & Sanders, 2025; Trejo, Yeomans-Maldonado, & Jacob, 2024).

Second, this is the first study exposure *within* schools rather than relying on children's residential proximity to hazards as a proxy for exposure. Much of this literature identifies exposure by linking homes to nearby toxic sites and finds that children who live closer to these sites have higher blood lead levels and worse academic outcomes (Klemick, Mason, & Sullivan, 2020; Rau, Urzúa, & Reyes, 2015). Other work identifies spatial patterns in lead exposure using neighborhood characteristics and geographic disparities in observed blood lead levels, or investigates how proximity to vehicle traffic influences exposure and children's outcomes (Aizer & Currie, 2019; Hollingsworth et al., 2025; Trejo et al., 2024). By focusing on schools, my research highlights an unstudied and under-regulated source of exposure that may compound existing residential risks.

Finally, this paper contributes to the growing body of research on public health interventions focused on reducing children's lead exposure. Existing literature focuses on large-scale policies like the de-leading of gasoline (Aizer & Currie, 2019; Grönqvist et al., 2020; Hollingsworth et al., 2025) and the effectiveness of local and state-level lead hazard control grants for risk assessment and abatement and education-based programs[3] on reducing exposure (Billings & Schnepel, 2018; Sorensen, Fox, Jung, & Martin, 2019). My research complements these studies by examining a recent intervention in an educational setting, providing evidence on the role of schools in lead exposure and mitigation.

To evaluate the effects of Baltimore's bottled water policy, I employ stacked synthetic control methods (SSCM). Using this method, I match individual schools from Baltimore to a "synthetic" treated school composed of a weighted average of control schools that match the treated Baltimore schools' characteristics. This method generates a credible comparison group that mirrors the pre-policy trajectories of Baltimore, reducing concerns arising from only one treated cluster and the appropriateness of other Maryland districts as a control group.

I estimate the average treatment effect for Baltimore schools by estimating the gap between treated and synthetic schools before constructing placebo-variance confidence intervals, which I present as event-study plots. Further, I extend the analysis to estimate differences between subgroups of schools to explore heterogeneity in treatment effects across different school contexts.

I find that Baltimore's bottled water intervention increased the percentage of students scoring proficient or better on math standardized tests by 5.66 percentage points and reading standardized tests by 3.94 percentage points. This is equivalent to an additional 30,139 proficient test scores and shrinks 1/5 - 1/4 of the achievement gap between Baltimore and the rest of Maryland. These results are robust to alternative matching criteria. Improvements are strongest for the lowest-achieving schools, schools with lower-than-average non-White

[3]Education programs include providing information to families on how to reduce household lead exposure, and referrals to remediation services or public assistance programs following elevated blood lead level testing results.

populations, and Title I eligible schools. Effects are also larger in larger schools.

Overall, bottled water, in lieu of access to potentially lead-contaminated piped water fountains, improves students' standardized test scores. This supports previous literature showing the adverse effects of lead on children's educational outcomes, while expanding this knowledge to include how school-based exposure has an impact. Further, this research provides evidence that school-based interventions can improve students' outcomes and warrants further research into their efficacy in settings beyond Baltimore City.

2 Background

Lead is a toxic heavy metal with well-documented effects on human health, particularly for children. Although naturally occurring, elevated lead levels in air, soil, and water are primarily the result of human activity. In the United States, lead was extensively used in gasoline, plumbing, paint, batteries, and ammunition until the 1970s, when Environmental Protection Agency (EPA) policies prohibited its continued use. These policies successfully reduced children's blood lead levels by approximately 70% (Pirkle et al., 1994). However, lead persists in the environment and the country's aging infrastructure, posing extended risks of exposure.

Children and pregnant women are the most vulnerable, as they absorb larger quantities of inhaled or ingested lead into their bloodstreams, developing brains, and nervous systems. Even at low exposure levels, lead is linked to behavioral problems, developmental delays, learning difficulties, and lower IQ in children and other neurological, cardiovascular, endocrine, and reproductive harm in adults (Jusko et al., 2008; Skerfving, Löfmark, Lundh, Mikoczy, & Strömberg, 2015; US EPA, 2023). Overall, lead exposure remains a major health concern for children in the United States (Swaringen et al., 2022).

Due to the widespread historical use of lead, children in the United States face exposure from multiple sources. Recent estimates suggest that approximately one in four households has hazardous lead concentrations in their soil. The CDC reports that approximately 29 million houses in the US contain aging lead paint and lead dust, with approximately 2.6 million housing young children (CDC, 2025; Filippelli et al., 2024). Further, despite the phase-out of leaded gasoline, the US is still a leading producer of airborne lead, primarily through aviation gasoline.

2.1 Waterborne Lead Exposure

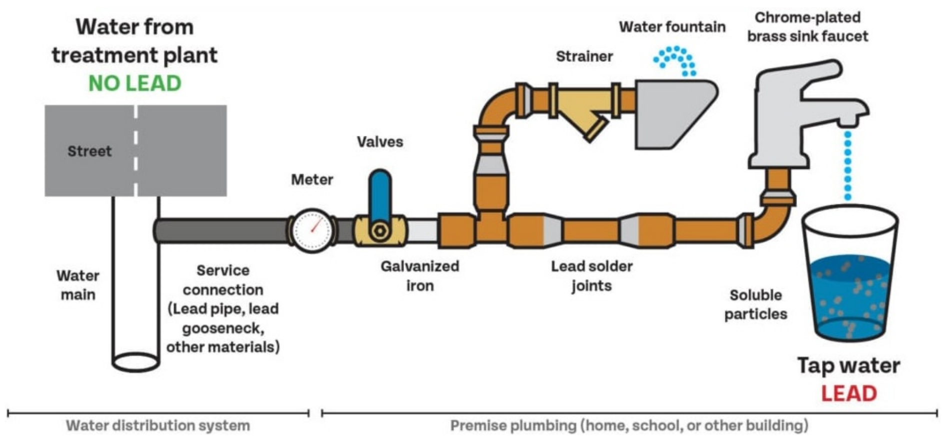
While the effects of soil and air exposure are well documented, the prevalence and effects of lead contamination in drinking water receive less attention. The water crisis in Flint, Michigan, in 2014 highlighted the potential for disaster when residents were exposed to lead well above the EPA Lead and Copper Rule (LCR) action level of 15 ug/L (Pieper et al., 2018). Recent reports indicate that across the country, while most public water utilities comply with the LCR, 186 million people between 2018 and 2020 were served by water systems with lead levels at or above 1 ug/L (Brown, Raymond, Homa, Kennedy, & Sinks, 2011; Fedinich, 2021). This is likely an underestimate of true exposure, as violations of the Lead and Copper Rule are underreported (Office of Water (EPA), 2008). With no identified safe threshold for children, the EPA's most recent efforts in 2024 aim to lower the action limit and mandate lead service line replacement.

The replacement of service lines is a slow process, and for many communities, water-based exposure stems from the 9.2 million remaining lead service lines connected to public water utilities (US EPA, 2023). These lines disproportionally impact low-income, Black families (Baehler et al., 2022). Service line lead exposure has only recently begun to be addressed through the 2024 Lead & Copper Rule Improvements, and infrastructure-based exposure, such as in schools, hospitals, and homes, is an afterthought. Even if this exposure is relatively low, one study showed an increase from baseline testing levels (0.5 ug/L) to the EPA limit of 15 ug/L caused an estimated increase of 1.6 ug/dL in students' blood lead levels (Lanphear, Burgoon, Rust, Eberly, & Galke, 1998).

2.2 Baltimore City School Lead Exposure

In schools, lead exposure primarily originates from aging infrastructure, including pipes, joints, and fixtures made of leaded materials (Figure 1). Over time and exacerbated by factors like water pH and treatment chemicals, corrosion builds up inside pipes and fixtures, breaks off, and dissolves into students' drinking water (Brown et al., 2011). When this occurs, the deposited lead is an invisible, tasteless contaminant that can only be identified through testing.

Figure 1: Potential Sources of Lead Contamination in Tap Water of Homes, Schools, and Other Buildings



Source: Environment America, *Get the Lead Out: Safe Drinking Water at School*, available at https://environmentamerica.org/resources/get-the-lead-out-safe-drinking-water-at-school/

Not only could lead testing fail to accurately estimate the exposure to due the the unpredictable nature of lead leaching into water., but it is also incredibly costly. For example, New York spent over $22.7 million on lead testing in one school year Ripstein (n.d.). In Baltimore, while remediation costs are yet to be fully quantified, the cost of testing and remediation was deemed to be more expensive than the $450,000 - $600,000 (2007 – 2025) annual spending on bottled water (Bowie, 2016; Rogers, 2025). Alternatives such as water filters and fountain replacements have been estimated at around $3.3 million for all city schools, with similar annual maintenance costs to water bottle provision (Bowie & Prudente, 2019).

While this program receives praise for its effectiveness, it faces criticism for high costs, environmental waste, and inconsistent water availability (Barclay, 2010). Bottled water provides a temporary solution, hindered by operational hurdles of stocking, cleaning, and maintenance of water coolers, suggesting that bottled water may not be an effective long-term solution (Kenney et al., 2020). However, as of 2016, approximately 80,000 Baltimore students continue to rely on bottled water in schools (Dennis, 2016) while the true impact of this program on students' health and academic outcomes remains unknown. Therefore, this policy provides a unique opportunity to study the effects of lead remediation in school environments on educational outcomes.

3 Data

3.1 Sources

The school-level outcomes from this paper are sourced from the Maryland Department of Education's "Maryland School Report Card" (MSRC). Aggregate testing proficiency for each school, by subject, is posted annually, reflecting performance on the Maryland School Assessment (MSA). The MSA tests 3rd – 8th-grade students in math and reading comprehension. Testing began in 2003 and continued until 2016. However, changes to the administration of the 2010 exams but not other districts makes this round and future rounds of testing non-comparable to the initial tests; therefore, I retain only scores from 2003 to 2009. For the purpose of this paper, I focus on elementary schools (grades 3-5).

Test scores are reported by MLDS in three categories: basic (does not meet expectations), proficient (at expectations), and advanced (exceeds expectations). I create a panel of math and reading proficiency by computing the percentage of students that score proficient or better by grade level, and aggregating to the school level, weighted by the number of test takers in each grade. I will refer to this measure as "proficient" throughout. On average, schools in Baltimore score lower on standardized tests than schools in other districts, with only 62% of students proficient in math 65% of students proficient in reading, on average.

Comparatively, the proportion of proficient students in other districts is 78% and 79% , respectively (Table 1).

Table 1: School Characteristics in AY2006–07, by Treatment Status

	(1) Baltimore	(2)	(3) Other Districts	(4)	(5) Synthetic (Math)	(6)	(7) Synthetic (Reading)	(8)
	Mean	SD	Mean	SD	Mean	SD	Mean	SD
Enrollment	428.88	180.97	469.98	144.62	383.89	138.37	418.19	121.55
Pupil/Teacher	14.36	2.03	14.25	2.05	12.51	2.72	12.69	2.12
FRPL	0.81	0.13	0.35	0.23	0.59	0.19	0.58	0.21
% Non-White	0.89	0.19	0.49	0.33	0.78	0.33	0.76	0.34
Age of School	47.73	18.67	43.09	17.79	43.19	18.27	42.72	17.15
Math Proficiency	0.62	0.20	0.78	0.15	0.61	0.20	0.63	0.20
Reading Proficiency	0.65	0.17	0.79	0.14	0.63	0.18	0.64	0.19
N Schools	742		742		742		742	
N Baltimore Schools	74		0		0		0	
N Other Schools	0		668		668		668	

Note: This table provides a comparison of pre-treatment characteristics between Baltimore and other districts and synthetic weighted Baltimore. Column (5) provides characteristics of synthetic Baltimore generated via weights from the math outcome matching procedure. Column (7) provides characteristics of synthetic Baltimore generated via weights from the reading outcome matching procedure. Percentages are expressed as decimals (e.g., 0.81 = 81%).

I link test scores from the MSRC to Common Core of Data (CCD) public school characteristic information. The CCD is a nationwide panel that has surveyed school characteristics since 1986. From this panel, I collect the type of school, enrollment, count of full-time equivalent teachers (used to generate pupil-to-teacher ratio), percentage of non-White students, the percentage of students receiving free or reduced-price lunch (FRPL). On average, Baltimore schools are larger, with higher pupil-to-teacher ratios and are more racially diverse with a higher percentage of FRPL-eligible students (Table 1).

As mentioned in 2, schools built after 1986 were not permitted to use leaded plumbing materials. To leverage this, I collect the year of construction or rehabilitation of each school facility via a Public Information Act request to the Maryland Department of Education. Older schools are expected to produce larger quantities of lead and on average. On average, BCPS are 4.65 years older than non-BCPS schools, with the majority of schools in both samples being built prior to 1986.

Overall, differences between BCPS and other districts imply that the average school outside of Baltimore does not provide a good counterfactual for what may have happened in Baltimore if they did not adopt the bottled water policy. To ensure a proper counterfactual, I use SSCM to appropriately match treated and control schools, which is discussed in more detail in Section 4. Characteristics for the counterfactual synthetic math and synthetic reading matches are provided in Columns (5) – (8). Across all characteristics and proficiency outcomes, aside from enrollment and pupil-teacher ratio, the synthetic control matching procedure generates a more similar counterfactual[4] (Table 1).

3.2 Sample Restrictions

To prepare the sample for analysis, I start by limiting the panel to 2003 – 2009, due to the incomparability of 2010-2013 test years due to changes made to the administration procedure of the test. To ensure that the remaining non-treated schools fall within the convex hull of characteristics for BCPS and thus would create a sufficient donor pool for matching, I restrict the pool of potential donor schools to non Baltimore schools with AY2007 enrollment within two standard deviations of the treated schools' AY2007 enrollment. This ensures computationally unique weighting matrices for each treated school and reduces concerns of interpolation bias due to violations of the convex hull assumptions.

4 Empirical Strategy

I examine whether eliminating waterborne exposure to lead in schools via water bottle provision improves students' academic outcomes by exploiting the exogenous timing of program implementation in Baltimore. Treatment occurred at the school district level, with all schools in the Baltimore district restricting access to drinking fountains and providing bottled water

[4]In the primary matching specification, schools are matched only on their outcome measures, and variability in other characteristics is expected to remain.

in November 2007[5].

To estimate the effects of this program on students' math and reading proficiency with aggregate school-level data, I apply SSCM outlined by Wiltshire (2025), which expands the flexibility of the traditional synthetic control method (Abadie et al. 2010). The ability of SSCM to accommodate multiple treated units offers advantages over traditional methods in this application. Most notably, conducting this analysis at the district level limits the pool of potential donor schools to only the 24 other districts in Maryland.

Instead, using SSCM, I conduct school-to-school matching employing a data-driven approach to construct a suitable synthetic counterfactual school composed of a weighted combination of unexposed "donor" schools that best match the pre-period characteristics of each Baltimore school. In the main specification schools are matched on each year of pre-period proficiency outcomes. Alternative matching procedures including pre-period school characteristics are outlined in the Appendix.

This matching process creates a weighted combination of donor schools that serve as the synthetic counterfactual for each treated school Y_{st}^{Synth}. More formally, for each treated school, I estimate

$$Y_{st}^{Synth} = \sum_{i}^{I} w_{sj} Y_{jt} \quad \text{for } t \geq T_{2007} \tag{1}$$

where Y_{jt} is the observed outcome for donor school j at time t; w_{sj} are non-negative weights assigned to each donor school j for specific treated school s such that $\sum_i^I w_{sj} = 1$. w_{sj} are assigned based on the aforementioned matching criteria and remain consistent across all time periods but differ for each treated school s. Y_{st}^{No} is then calculated separately for each time period $t \geq T_{2007}$ as the synthetic counterfactual school outcome for school s in year t.

[5] According to Baltimore's board meeting minutes from January of 2007, initial provision was not provided to all schools. Some schools had access to bottled water prior to this period. Records regarding which schools previously utilized bottled water or which schools implemented bottled water for the first time do not exist within Baltimore's record system and thus were not able to be obtained through a Public Information Access Request. The direction of potential measurement error bias is expected to be towards zero.

For each school and time period, I then estimate the "gap" between the observed outcomes for the treated schools Y_{st}^{Treat}, and the weighted observed outcomes for synthetic counterfactual schools Y_{st}^{No}:

$$\tau_{st} = Y_{st}^{Treat} - Y_{st}^{No} \tag{2}$$

where τ_{st} is the estimated gap between treated and synthetic counterfactual outcomes for school s in year t. This procedure is repeated for every treated school s in every year t and aggregated based on weights δ_s to estimate the average treatment effect $\tau_t$in each year t using

$$\tau_t = \sum_{s=1}^{S} \delta_s \left(Y_{st}^{Treat} - Y_{st}^{Synth} \right) \quad \text{for } t \geq T_{2007} \tag{3}$$

where τ_t is the estimated average treatment effect (ATE) calculated for each period. τ_t is estimated as the weighted of the difference between the observed (Y_{st}^{Treat}) and synthetic counterfactual (Y_{st}^{No}) outcomes for each school s. In the primary specification, these weights are equal across schools. Alternative weighting specifications are presented in Section **??**. Lastly, I calculate placebo-variance-based 95% confidence intervals assuming homoskedasticity across units and the asymptotic normality of the estimand (Wiltshire 2025). These estimates are presented in Figure 4.

The benefit of using this methodology, as opposed to traditional SCM, is greater flexibility and precision available by using school-level characteristics and outcomes instead of aggregated district-level characteristics. Compared to conventional difference-in-differences, this model ensures a reasonable counterfactual group that is more likely to represent the true, unobservable counterfactual for BCPS. Further, utilizing SSCM to generate control unit weights ensures that these weights are blind to treatment outcomes and only selected based on pre-treatment characteristics, eliminating concerns about extrapolation bias.

Finally, the assumptions required for these outcomes to have causal interpretation are as follows: First, the treated units' characteristics should lie within the convex hull of donor pool characteristics to produce a match, which I discussed above. Next, the outcomes of the untreated units are not affected by the intervention implemented on the treated units, which is expected to hold because non-BCPS students do not have access to the bottled water provided inside of BCPS schools, which also implies that there is unlikely to be SUTVA violations due to spillovers between treated and donor pool units. This intervention should also not affect the outcome before the intervention starts – which is plausible because this intervention is directly impacting students' lead exposure, which would otherwise not be reduced without the presence of alternative drinking water sources.

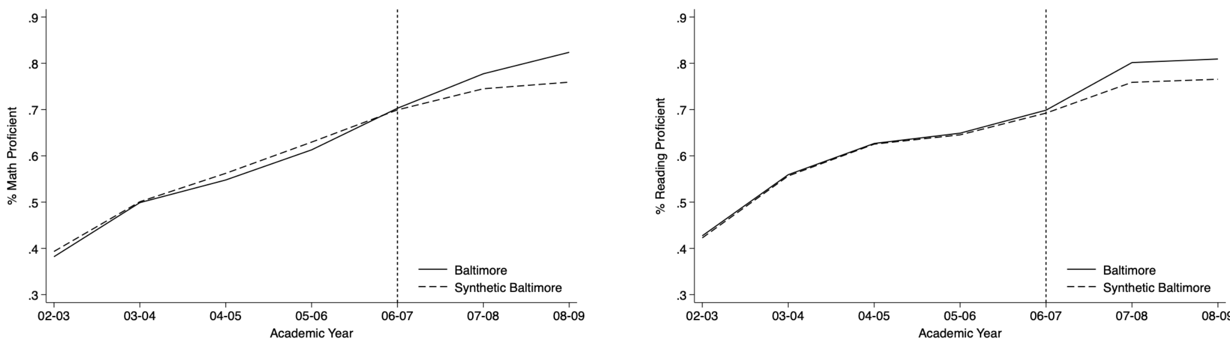
5 Results

In this section, I present evidence of the impact of Baltimore's bottled water intervention on students' achievement. First, I document pre- and post-treatment trends in math and reading proficiency between treated Baltimore schools and their synthetic counterparts. Then, I quantify these differences in an event study plot and weighted difference in difference estimations. I follow this analysis with robustness checks and subgroup analysis to explore heterogeneity of treatment effects. Overall, my results conclude that in-school exposure negatively impacts student success, and eliminating in-school exposure improves elementary school children's testing proficiency in math and reading.

To start, Figure 2 plots the raw change in math proficiency (Panel A) and reading proficiency (Panel B) from AY2003 – AY2009 for average Baltimore schools and average synthetic Baltimore schools as estimated by Equation (1). There is a close match in the testing proficiency of Baltimore and synthetic Baltimore prior to treatment. After this point, Baltimore schools exhibit an increase in testing proficiency, while synthetic Baltimore schools do not exhibit the same trend.

To test if this change is statistically significant, I estimate the gap between each indi-

Figure 2: Trend in Testing Proficiency, Baltimore vs Synthetic Baltimore, AY2003–AY2009



Panel A: Math

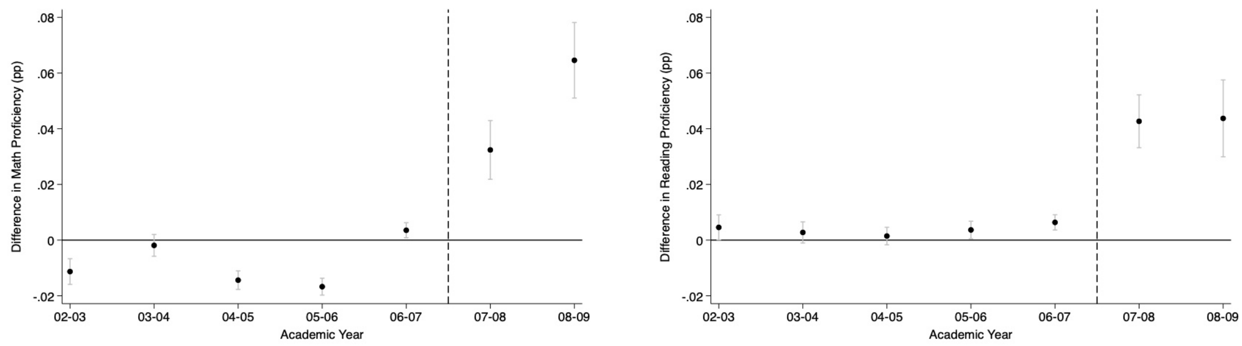
Panel B: Reading

Note: This figure presents the percent of students who scored proficient or better on math (Panel A) and reading (Panel B) standardized testing in Baltimore, compared to a synthetic version of Baltimore. Results are presented from AY2003 to AY2009. The vertical line represents the academic year prior to treatment.

vidual Baltimore school and its weighted synthetic Baltimore school counterpart following Equation (2) before aggregating in Equation (3). Then, I generate placebo-variance-based 95% confidence intervals using a random draw of 1,000 iterations of placebo-estimates from non-Baltimore schools. The results are presented as an event study in Figure 4. These results of these figures are interpreted as the percentage point difference in proficiency between Baltimore and synthetic Baltimore. For math proficiency, the average of this effect in the post-period is approximately 5 percentage points, with proficiency improving over time. Comparatively, reading proficiency demonstrates an initial increase of approximately 4 percentage points and remains stable in the post-period.

As another measure of proficiency, I also provide estimates for the percent of students scoring in the advanced category separately. These results are similar in magnitude to the overall change in proficiency results: 4.5 percentage point increase in the percent of student scoring advanced in math exams, and a 4.3 percentage point increase in the percent of students scoring advanced on reading exams. Based on an average pre-period difference of 21.1 % in math proficiency between Baltimore and other districts, this policy closes approximately 1/5 of the achievement gap for math scores. For reading, with a pre-period average of 17.5 %, 1/4 of the gap was closed. Trejo et al. (2024) similarly find immediate effects of changes in elementary school students' lead exposure, finding that increases in lead exposure led to an immediate decrease in standardized math scores.

Figure 3: Estimated Difference in Testing Proficiency, AY2003–AY2009

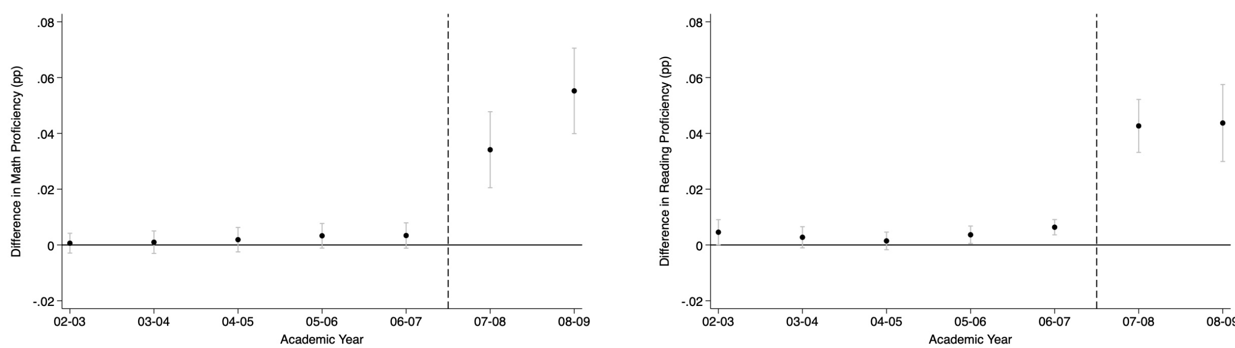


Panel A: Math

Panel B: Reading

Note: This figure displays the differences in math proficiency (Panel A) and reading proficiency (Panel B) between treated and synthetic schools. The vertical dashed line represents the treatment. Standard errors are estimated using random inference placebo draws. Synthetic control matching was performed using all pre-period years of the outcome measure.

Figure 4: Estimated Difference in Testing Proficiency, % of Students Advanced, AY2003–AY2009



Panel A: Math

Panel B: Reading

Note: This figure displays the differences in math proficiency (Panel A) and reading proficiency (Panel B) between treated and synthetic schools. The vertical dashed line represents the treatment. Standard errors are estimated using random inference placebo draws. Synthetic control matching was performed using all pre-period years of the outcome measure.

For more straightforward comparability across specifications, I use the weights generated by the SSCM estimator (w_{sj}) to estimate the average treatment effect using the following weighted two-way fixed-effects (TWFE) specification:

$$Y_{st} = \beta_1 \text{Proficient}_s \times \text{Post}_t + \delta_s + \delta_t + \epsilon_{st} \quad (4)$$

These results are presented in Table 2 and suggest an average increase of 5.66 (3.94) percentage points in math (reading) proficiency following the implementation. Due to the unique measurement of my proficiency measures, it is difficult to compare the magnitude of my effects to other existing research. However, Zhang et al (2013) similarly find that the odds of scoring below proficient increase with exposure. Regarding the difference in effect size between reading and math proficiency, the education literature has shown math proficiency to be more sensitive to policy mechanisms than reading. However, existing lead studies find that reduced childhood lead exposure improves later reading proficiency more than math proficiency, which is consistent with my results (Aizer et al., 2018; Sorensen et al., 2019).

Table 2: Difference-in-Differences Estimates, Math and Reading Proficiency

	(1) Math	(2) Reading
Treated × Post	0.0566***	0.0394*
	(0.0154)	(0.0197)
N	5194	5194
N Treated	74	74
N Control	668	668

Note: This table shows the results from the weighted TWFE estimation (Equation (4)) using the weights generated by the synthetic control matching procedure. Standard errors are shown in parentheses and are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Robustness

While synthetic control methods reduce concerns of extrapolation bias by generating weighting matrices, one concern is that results may be sensitive to matching method selection, which could change the pool of donor schools. In the main specification, I match only on each pre-period year of the testing outcome variable in each specification. In Table 3, I present these results in column (1), which is equivalent to the results presented in Table 2. In column (2), I retain these criteria while also including pre-period average of pupil-teacher ratio, percent of students receiving free and reduced price lunch (FRPL), percent of non-White students, and the age of the school in 2007. In column (3), I match on all pre-period years of the outcome, as well as include all pre-period years of pupil-teacher ratio, percent of FRPL students, percent non-White and school age. Results for both math (Panel A) and reading (Panel B) are robust to alternative specifications of the matching criteria.

Table 3: Difference-in-Differences Estimates, Robustness to Alternative Matching Criteria

	(1)	(2)	(3)
Panel A: Math			
Treated × Post	0.057***	0.057***	0.057***
	(0.015)	(0.015)	(0.015)
Panel B: Reading			
Treated × Post	0.039*	0.039*	0.039*
	(0.020)	(0.020)	(0.020)
N	5194	5194	5194
N Treated	74	74	74
N Control	668	668	668

Note: This table shows the results from the weighted difference-in-differences estimation using alternative matching criteria. Column (1) represents matching only on the outcome (main specification). Column (2) represents matching on all pre-period years of the outcome and the pre-period average of pupil-teacher ratio, % receiving free or reduced-price lunch (FRPL), % non-White, and the age of the school in 2007. Column (3) matches on all pre-period years of the outcome and all pre-period years of pupil-teacher ratio, % FRPL, % non-White, and age of the school in 2007. Standard errors are shown in parentheses and are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

7 Heterogeneity

To better understand which students are most impacted by the elimination of in-school lead exposure, Table 4 explores heterogeneous treatment effects by re-estimating Equation

(4) by achievement, school age, Title I eligibility, and racial diversity subgroups, utilizing the same weights as the main specification. Column (1) provides the estimates from Table 2. Columns (2) and (3) estimate equation (3) separately for the upper 50th percentile of Baltimore testing proficiency and lower 50th percentile, respectively. For both math and reading specification, improvements in testing proficiency are larger for the lowest achieving students with increases of 9.0 percentage points in math and 4.5 percentage points in reading.

As discussed in Section 2, the Safe Drinking Water Act prohibits the use of lead plumbing infrastructure beginning in 1986. When limiting the sample of schools to those built before 1986 (which includes all but one Baltimore school), math proficiency has an increase of 6.4 percentage points and reading proficiency by 4.5 percentage points (Column 4). These results are consistent with the full sample results, confirming improvements in testing outcomes stem from changes in students' water-borne lead exposure.

To better understand how different income groups may be affected by this policy, I create an indicator for if a school is eligible for Title I funds. This threshold, in most cases, is satisfied when 40% or more students receive free or reduced-price lunches. Columns (5) and (6) provide estimates for Title I eligible and non-eligible schools. Similar to the age of the school, the majority of Baltimore schools are Title I eligible, with improvements of 4.5 and 3.1 percentage points, respectively. Other work also finds larger effects for lower socioeconomic status elementary students (Trejo et al., 2024) and students exposed during childhood (Hollingsworth et al., 2025). Further, low-income groups are exposed to larger quantities of lead in their home environments, and this traditionally leads to higher blood lead levels.

Next, I compare the upper 50th percentile of non-White schools to the lower 50th percentile, with cutoffs based on Baltimore characteristics. Here, the effects are larger in schools with lower non-White enrollment. This could be compounding the low-income results, as lead service lines disproportionately affect Black families (Brown et al., 2011). One implication of this is that eliminating in-school exposure reduces Black students' overall lead exposure by a smaller proportion. Another explanation from medical literature suggests that, because of a higher predisposition to iron deficiency, which alters how the body metabolizes and absorbs lead, Black students may be more sensitive to lead exposure, thus smaller quantities of lead may be producing negative cognitive effects (Ngueta 2014).

Table 4: Subgroup Heterogeneity Analysis

	(1) Average	(2) High Achieve	(3) Low Achieve	(4) Older than 1986	(5) Newer than 1986	(6) Title I Elig	(7) Title I Not Elig	(8) High Non-White	(9) Low Non-White
Panel A: Math									
Treated × Post	0.057***	0.024	0.090***	0.064***	0.015	0.045***	-0.013	0.046***	0.056***
	(0.015)	(0.031)	(0.012)	(0.020)	(0.028)	(0.010)	(0.016)	(0.004)	(0.018)
N	5194	4263	931	4550	644	2380	2814	602	4592
N Treated	74	37	37	73	1	72	2	37	37
N Control	668	572	96	577	91	268	400	49	619
Panel B: Reading									
Treated × Post	0.039*	0.035	0.045***	0.045**	0.016*	0.031*	-0.024	0.013***	0.040**
	(0.020)	(0.024)	(0.013)	(0.019)	(0.008)	(0.016)	(0.014)	(0.000)	(0.019)
N	5194	4326	868	4550	644	2380	2814	602	4592
N Treated	74	37	37	73	1	72	2	37	37
N Control	668	581	87	577	91	268	400	49	619

Note: This table presents heterogeneity in the weighted difference-in-differences estimates from Equation (4) "High" and "Low" subgroups are defined using the 50th percentile among treated schools. Standard errors, shown in parentheses, are clustered at the school level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

It may also be valuable to understand the effects while taking into account the school's enrollment. I do this by adjusting the weighting procedure in Equation (3). Prior estimates aggregated the treatment effect based on equal weights for all schools ($\delta_s = 1$). In Table 5, I present these estimates weighted instead by the enrollment of the school in the year prior to treatment ($\delta_s = \text{Enrollment}_{2007}$). The results of this specification are marginally larger, with 6.5 percentage point increase in math proficiency and a 4.6 percentage point increase in reading proficiency. These results indicate that effects are larger for students at larger institutions.

Table 5: Synthetic Control Difference-in-Differences with Enrollment Weights

	(1) Math	(2) Reading
Treated × Post	0.065***	0.046**
	(0.016)	(0.022)
N	5194	5194
N Treated	74	74
N Control	668	668

Note: This table shows the results from the TWFE equation of Equation (), including weights generated from Equation (3), where δ_s is equal to a school's enrollment. Larger schools therefore receive greater weight than smaller schools. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

8 Cost Effectiveness

An alternative method of quantifying the improvements in proficiency is relative to the cost of the program. For the academic year 2008, Baltimore spent $675,000 on bottled water. Relative to baseline enrollment, it costs around $2.15 per student. Based on a 5.66 percentage point increase in math proficiency on a baseline enrollment of 313,947 students, this policy increased the number of students scoring proficient on math exams by 17,769. Similarly, an increase of 3.95 percentage points for reading proficiency translates to an additional 12,370 students scoring proficient on reading standardized tests. Overall testing proficiency therefore increased by a combined 30,139. This translates to a cost of $22 per additional proficient test score. These are expected to be an overestimate of costs because the initial cost of installing the water coolers and water bottles is higher than the ongoing maintenance cost and provision of bottled water. For perspective, in academic year 2009, Baltimore's per-pupil expenditure was $14,454.28.

Comparatively,

9 Conclusion

I find significant positive effects of the Baltimore water bottle intervention on the academic outcomes of children living in the district. Using a SSCM methodology, which alleviates concerns about a single treated district and produces a more accurate control group, the percent of elementary school students scoring proficient or better on math exams increases by 5.77 percentage points following distribution of bottled water coolers. The percentage of these students scoring proficient or better on reading exams increases by 3.97 percentage points.

These results are robust to alternative specifications of matching criteria and more conservative than traditional TWFE estimates (Appendix). They imply that testing proficiency improves immediately post-policy, and may continue to improve over time. This result is novel, as aside from Trejo et al. (2024), no existing studies explore the immediate impact of lead exposure on school-aged students' outcomes.

My results further indicate that these effects may shrink achievement gaps between the highest and lowest achieving schools but may not be sufficient to counteract the disparities in outside exposure between White and non-White students to racial achievement gaps.

These results overall provide new evidence that lead exposure negatively impacts school-aged children. This is particularly relevant as - out of - states currently have no school-based requirements for testing or remediation, and federal guidance for testing is limited to October 2024 regulations. This paper provides evidence that providing bottled water to school students reduces their exposure to lead and improves their academic, and therefore development, outcomes and may be a consideration in districts across the United States facing the decision of how to remediate lead within their schools.

A Appendix

A.1 Supplementary Figures

Figure 5: Water Bottle Cooler in Baltimore City School with Students and Teacher

Source: (Bowie, 2019), available at https://www.baltimoresun.com/2016/04/09/water-from-a-fountain-not-in-baltimore-city-schools/

A.2 Two-Way Fixed Effects Results for Main Specification

Table 6: Traditional Difference-in-Differences Estimation Results for Main Specification

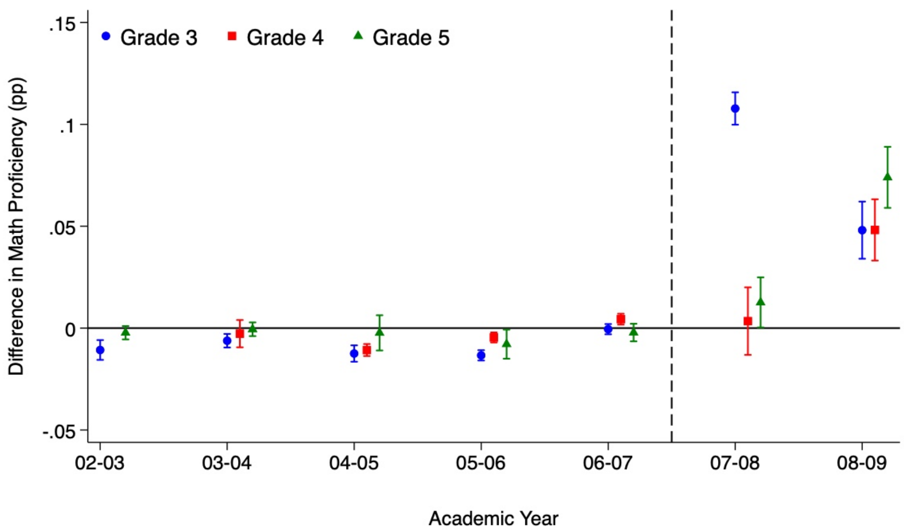
	(1) Math	(2) Reading
Treated × Post	0.146***	0.101***
	(0.0121)	(0.0114)
N	5194	5194
N Treated	74	74
N Control	668	668

Note: This table shows the results from the TWFE regression of Equation (). Estimates are not weighted using synthetic controls. Standard errors are shown in parentheses and are clustered at the district level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

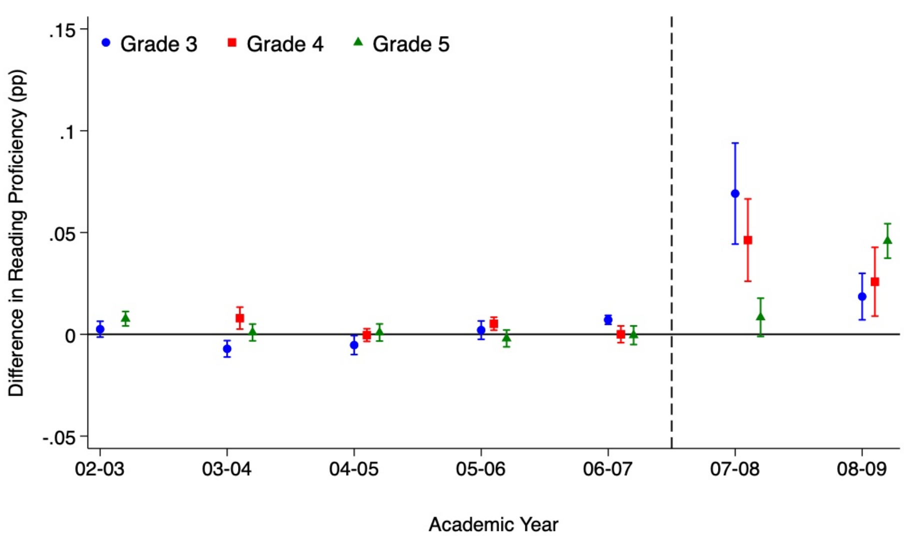
A.3 Grade Level Analysis

To further explore heterogeneity, in an attempt to capture dynamic effects of reduced lead exposure over a child's lifetime, I separate the analysis by grade level. The estimates for each grade are produced separately, with their own unique weights.

Figure 6: Heterogeneity by Grade Level



Panel A: Math



Panel B: Reading

Note: Results from allsynth estimates of Equation XX. Point estimates represent $\tau_{post} - \tau_{pre}$ using random inference for standard errors.

The point estimates are as follows. These estimates are from calculating the tau pre - tau post differences using random inference for the standard errors.

Table 7: Heterogeneous Effects by Grade (Grades 3–5)

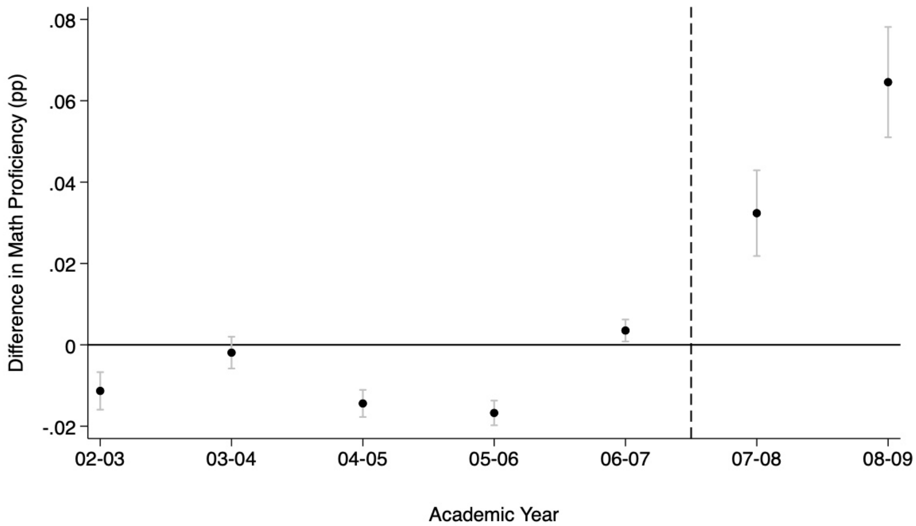
	Grade 3	Grade 4	Grade 5
Panel A: Math			
$\tau_{Post} - \tau_{Pre}$	0.044***	0.034***	0.027***
	(0.010)	(0.009)	(0.003)
Panel B: Reading			
$\tau_{Post} - \tau_{Pre}$	0.084***	0.029***	0.046***
	(0.006)	(0.007)	(0.005)

Standard errors in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

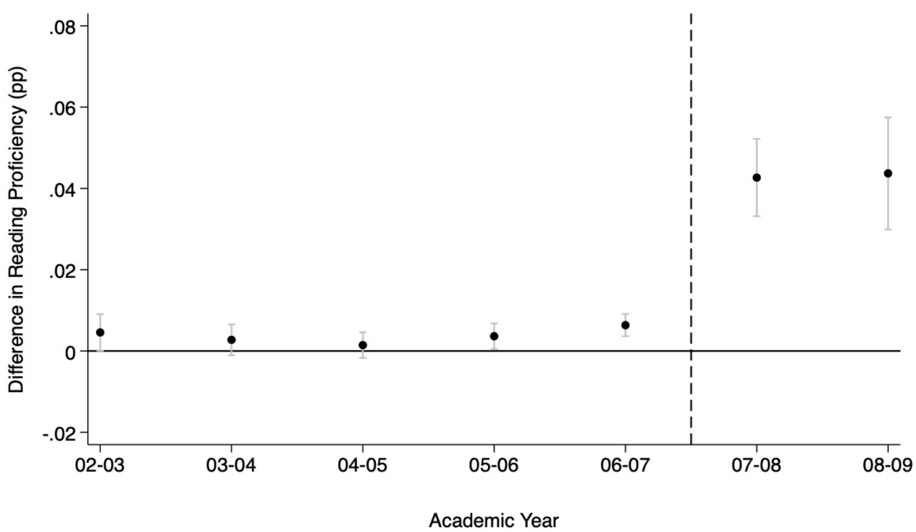
A.4 Robustness to Alternative Specifications

A.4.1 Limiting Potential Donor Pool to Schools within 1 SD of Treated Schools Enrollment

Figure 7: Estimates Limiting Donor Pool to those within 1 SD Enrollment



Panel A: Math

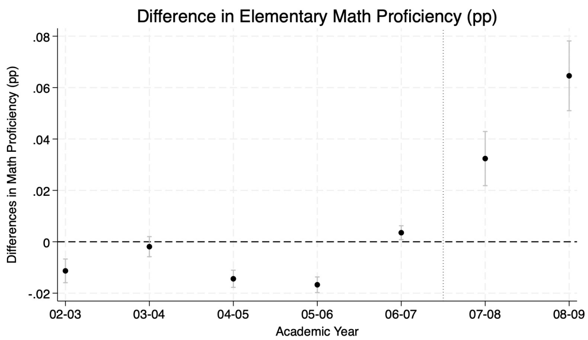


Panel B: Reading

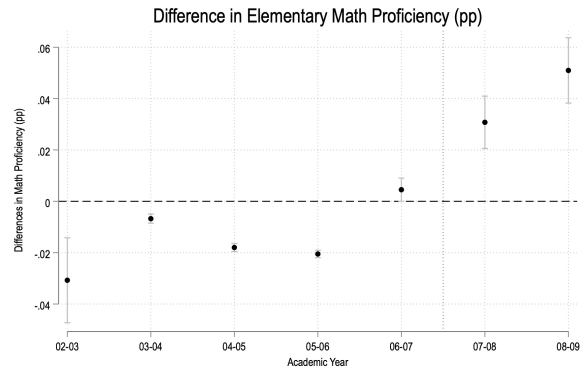
Note: Results from allsynth estimates of Equation XX. Point estimates represent $\tau_{post} - \tau_{pre}$ using random inference for standard errors.

A.4.2 With and Without Covariates

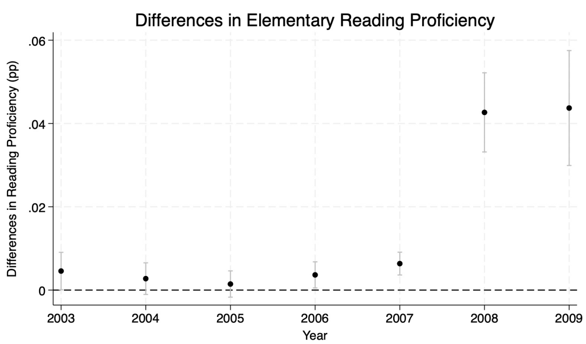
Figure 8: Math and Reading Results with Alternative Matching Criteria



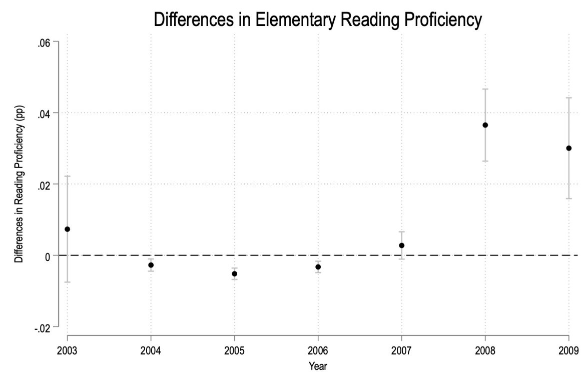
Panel A: Math (Saturated Match)



Panel B: Math (Average Pre-Period Match)



Panel C: Reading (Saturated Match)



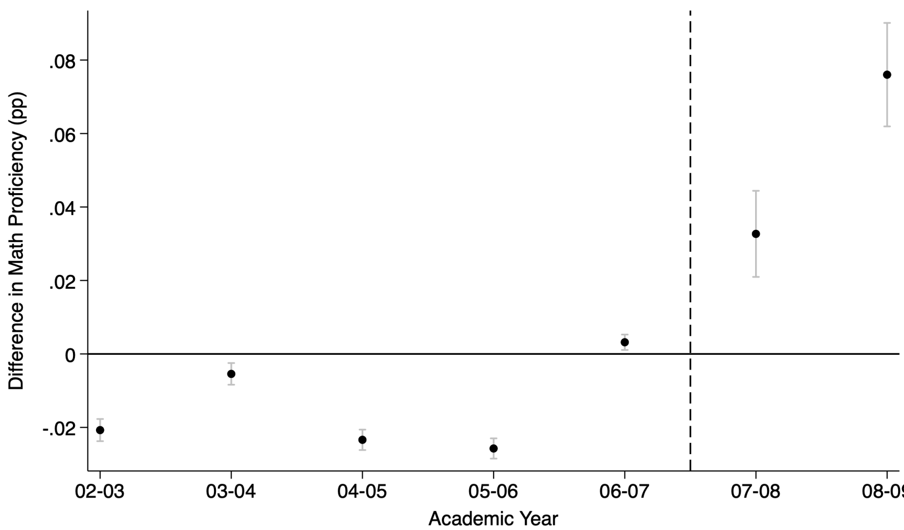
Panel D: Reading (Average Pre-Period Match)

Note: Synthetic control difference for math (Panels A and B) and reading (Panels C and D). Panels A and C include estimates generated from matches on every pre-period level of outcomes, every pre-period level of pupil–teacher ratio, % receiving free or reduced-price lunch (FRPL), % non-White, and the age of the school in 2007. Panels B and D include estimates generated from matches on every pre-period level of outcomes and on *average* pre-period levels of pupil–teacher ratio, % FRPL, % non-White, and the age of the school in 2007.

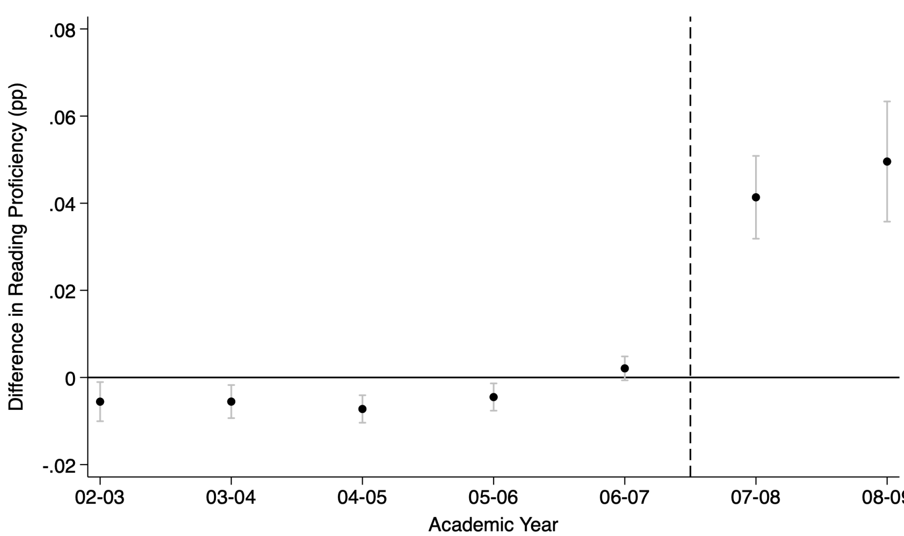
A.4.3 Only Using Treated Schools with Unique Weighting Matrices

Treated schools may be too different from control schools, violating the assumption that treated schools fall into the convex hull of donor schools. This concern can be mediated by estimate the effects using only schools that have a unique weighting matrix.

Figure 9: Math and Reading Estimates from Schools with Unique Weighting Matrix



Panel A: Math



Panel B: Reading

Note: Results from allsynth estimates of Equation XX. Point estimates represent $\tau_{post} - \tau_{pre}$ using random inference for standard errors.

A.5 Propensity Score Matching

While synthetic control matching provides the most appropriate estimating strategy in the event of a single treated cluster, as is the case here with only Baltimore receiving treatment, propensity score matching may provide a valuable comparison. The results of this specification are similar in magnitude to the synthetic control difference in difference, but are not statistically significant.

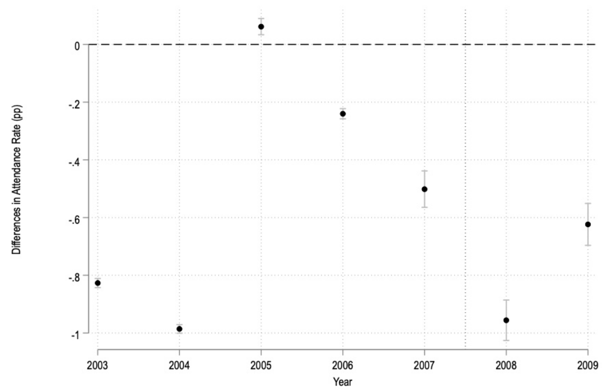
Table 8: TWFE Estimates Using Propensity Score Matching Weights

	(1) Radius	(2) Kernel
Treated × Post	0.038	0.038
	(0.024)	(0.033)
Observations	6552	6517
Weighted	X	X

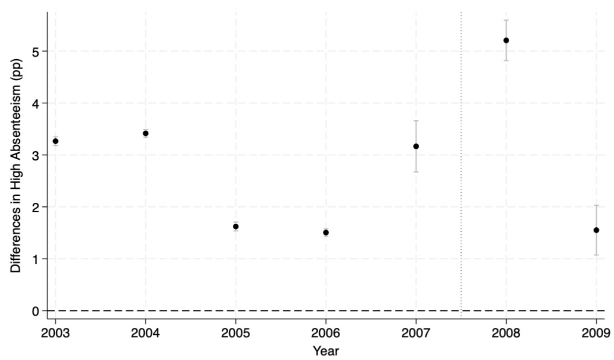
Notes: Each column represents TWFE estimates without additional controls, using propensity score matching weights generated on 2006 levels of enrollment, pupil-teacher ratio, percent FRPL, percent non-White, and school age. Radius column uses a caliper of 0.2. Standard errors are clustered at the school level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.6 Attendance

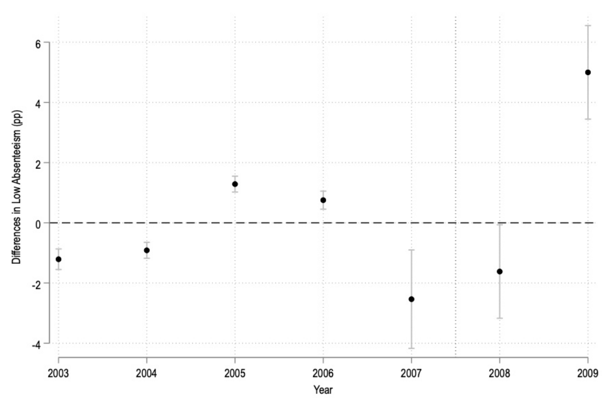
Figure 10: SSCM Results for Attendance Measures



Panel A: Attendance Rate



Panel B: High Absenteeism



Panel C: Low Absenteeism

Note: The above specifications provide SSCM results for (1) attendance rate, (2) the percent of students missing more than 20% of days, and (3) the percent of students missing fewer than 5% of days. All data are truncated at the 95% and 5%.

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