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Hold Harmless for Whom? The Impact of COVID Era Policies on School Funding, Teachers, and Students

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Hold Harmless for Whom? The Impact of COVID Era Policies on School Funding, Teachers, and Students

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Abstract:

This study evaluates the fiscal and academic consequences of New York City's hold harmless policy during COVID-19, which aimed to stabilize school expenditures amid unexpected enrollment declines by restoring schools' funding up to initial levels. We examine how school racial composition predicts whether or when schools receive hold harmless "treatment" and assess the impact of hold harmless on financial resources, staffing, and student outcomes, exploring heterogeneity by timing of policy announcement. Although schools with higher White student shares were no more likely than those with higher Hispanic or Black shares to receive hold harmless funds, schools with higher Black shares that did receive them saw larger per-pupil allocations due to deeper enrollment losses. Overall, hold harmless schools experienced significant increases in per-pupil spending, and reduced pupil-teacher ratio and class size, while maintaining the size of the teaching workforce. We find hold harmless had no effect on attendance or chronic absenteeism in 2021 or 2022, but improved both in 2023, when it was announced earlier. Although funds often rolled over to later years, we find no corresponding gains in student outcomes. Overall, the policy effectively preserved school-level spending and staffing – as intended – with some improvements in student outcomes when announced early.

Keywords: Hold Harmless, School Spending, Teacher Retention, Class Size

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I. Introduction

In March 2021, the New York City Department of Education (NYCDOE) announced it would use a portion of the American Rescue Plan funding for a hold harmless policy to shield schools with lower-than-projected enrollment from mid-year funding adjustments, commonly referred to "clawbacks".[1] Framed as a temporary measure to "provide stability for principals and staff in a period of fiscal uncertainty" (NYCDOE 2021), holding schools harmless for enrollment shortfalls meant higher per-pupil funding in hold harmless "treatment" schools (those benefiting from the policy) than otherwise similar schools funded according to usual weighted student formula (WSF).[2] Similar hold harmless policies were implemented in academic years 2022 and 2023.

Critics raised two broad concerns. First, protecting funding for under-enrolled schools may preserve under-utilized classrooms or staff, diverting resources that could otherwise support higher-need and/or growing schools. If schools that lose enrollment disproportionately serve White students, resources may be inadvertently diverted away from Black or Hispanic students. A second related concern is that the hold harmless policy appeases teachers' unions and protects staffing levels but is an ineffective strategy to address pandemic learning loss (Bhole, Campanile, and McCarthy 2024). Indeed, the benefits of stabilization for personnel and planning may come at the expense of interventions that more effectively support students. The hold

[1] In a typical school year, NYCDOE will claw back school funding mid-year if their enrollment falls short of projections made before the start of the academic year.
[2] Allocating federal relief funds to hold schools harmless involved an opportunity cost, as these resources could instead have been directed toward other pandemic-response priorities, including academic recovery programs (Smith, 2022).

harmless policy reignited a broader debate: should limited relief funds be used to shore up the budgets of shrinking schools (and protect staffing) or targeted to the neediest students? Answers to these questions are especially timely as states adjust to changes in federal tax and spending policy that may affect education revenues (e.g., Lieberman 2026). Whether hold harmless simultaneously provided financial stability, protected teachers, and improved student outcomes —or only some (or none) of these— are empirical questions that we address in this paper.

Specifically, this paper addresses three questions. The first concerns the link between hold harmless and school racial and ethnic composition. Were disproportionately White (Asian, Black, Hispanic) schools more likely to be held harmless? Did they receive more hold harmless funding, as critics feared? The second question concerns consequences for resources. Did hold harmless protect school budgets (i.e. expenditure per pupil) and staffing (i.e. number of teachers, pupil-teacher ratio, and average class size) from cuts due to enrollment shortfalls? The third concerns student outcomes. Did hold harmless resources translate into improved student performance, specifically, higher attendance and lower chronic absenteeism?[3]

This paper leverages detailed school-level data from academic year (AY) 2016 to AY 2023 to examine the impacts of hold harmless "treatment" (HH) on resources, staffing, and student outcomes. We first explore whether school racial composition predicts the probability and size of HH funding. Then, we estimate the impact of HH using a difference-in-differences (DiD) design and exploit the variation in HH timing across schools to explore heterogeneity by HH cohort (defined as those receiving HH funding in AY 2021, 2022, or 2023) and persistence of impacts. To assess whether pooling by duration and timing of first treatment introduces bias, we estimate a flexible DiD model that captures treatment spell in addition to HH cohort and

[3] We focus on attendance outcomes because test scores were unavailable during the pandemic.

persistence.[4] We then examine whether impacts varied with the number of years of HH funding and explore sensitivity to alternative specifications, continuous measures of HH funding, and restricting the sample to the subset of schools that are "ever" hold harmless. Finally, we explore differences by school grade level (elementary, middle, high).

To preview the results, we find racial composition is a weak predictor of HH treatment. Disproportionately White schools are no more or less likely to receive any HH funding than disproportionately Hispanic or Black schools. That said, racial composition *does* predict the *amount* of HH funding. A 10 percentage point increase in Black share (with a concomitant decrease in share Hispanic, the reference group) is associated with roughly $32-$39 additional HH dollars per pupil in all years. Asian and White share are weaker and insignificant predictors of HH funding. We see no evidence that the hold harmless policy disproportionately benefited White students; instead, it slightly benefited schools with larger Black representation.

As for impacts on school resources, HH increased spending per pupil and reduced both pupil-teacher ratio and class size, primarily by preserving teacher staffing levels in the context of decreased enrollment (rather than adding teachers). While we find no evidence of impacts on attendance outcomes in 2021 or 2022, both outcomes improve in 2023, due, perhaps, to the early announcement of hold harmless which may allow more effective use of funds. While some HH funds are rolled over to subsequent years, we find no corresponding persistence in improved student outcomes. Moreover, schools treated multiple times experienced larger increases in per pupil expenditures and reductions in pupil teacher ratio, but these resource gains reflect the

[4] Recent work, such as Callaway and Sant'Anna (2021), shows that traditional two-way fixed effects (TWFE) estimators can produce biased estimates when treatment timing varies across units. Their approach addresses this bias in settings with absorbing treatment, where units remain treated once treatment begins. However, the design of hold harmless policy allows HH treatment to be discontinued, as schools may be under-enrolled in some years but not others-. Indeed, roughly half of HH schools in our sample experience at least one exit from HH status. As a result, DiD estimators that rely on an absorbing-treatment assumption are not well suited to our setting.

rollover of HH and did not translate into further improvements in student outcomes. Finally, while HH increased resources for elementary, middle, and high schools, student outcomes improved only in elementary. Results are robust to continuous measures of HH funding, sample restrictions to ever hold harmless schools, and other alternative model specifications.

This study contributes new evidence on the effects of a policy designed to hold schools harmless for fiscal shocks, e.g., enrollment volatility. While prior research has largely focused on district-level stabilization mechanisms, NYC's hold harmless policy offers a rare opportunity to evaluate both the intended and unintended impacts of fiscal aid provided directly to schools.

II. School Funding in NYC

Fair Student Funding and Other School Funding

NYCDOE allocates about two-thirds of its school budget to its (roughly) 1,800 schools using a weighted student funding (WSF) model, called Fair Student Funding (FSF), designed to promote equity, transparency, and school-level discretion in budgeting (NYC Comptroller 2023; Subramanian 2013). Unlike position-based or incremental funding models, FSF distributes resources based on the characteristics of each school's student body. Schools receive a base per-pupil allocation and a variable amount determined by a set of "weights" reflecting specific student attributes, such as economic disadvantage, disability status, English language learner status, prior academic achievement, and grade level (with middle and high school students weighted more heavily than elementary school students), among others.[5]

[5] Under NYC's Fair Student Funding (FSF) formula, each student receives a base weight (typically 1.0), with additional weights for certain characteristics (e.g., 0.65 for being economically disadvantaged) applied in an additive manner (not multiplicative). Thus, a student with multiple needs generates the sum of all applicable weights. FSF also includes a fixed base allocation (roughly $200–$350K per school), and provides extra funding for specialized high schools, career and technical education (CTE) schools, and others.

Before the start of each academic year (typically in January), NYCDOE projects each school's weighted enrollment and allocates preliminary budgets, according to the FSF formula.[6] In October, actual (weighted) enrollment is compared to these projections, and mid-year "register adjustments" reconcile differences – either clawbacks if the projection was too high or increases if it was too low (NYC Comptroller 2023). Midyear adjustments are intended to ensure funding reflects the FSF weights.

A final third of school funding is distributed through a plethora of distinct programs – as many as eighty in some years – typically earmarked for specific purposes although some offer additional principal discretion. These include state and federal programs, such as Title I, programs of other NYC agencies (e.g., the City Council), and, to a lesser extent, philanthropic or donative sources (Rothbart, Schwegman, and Shybalkina 2022). Clawback mechanisms are rare in these programs.[7]

COVID Era Hold Harmless and its Implementation

During COVID, enrollment in NYC traditional public schools, as in other large urban districts, declined sharply: 4.7% in AY 2021 and 3.8% in AY 2022 (NYC IBO 2022), resulting in the largest two-year decline in over a decade (Konrad 2024). The declines hit some schools harder than others. Some saw steep declines overall and some saw shifts in student attributes, with consequences for FSF weighted enrollments. In response, the NYCDOE adopted a hold harmless policy to protect schools whose actual weighted enrollment fell below their projected weighted enrollment (NYC Comptroller 2023). We refer to this shortfall as an *unfavorable enrollment variance,* and for simplicity, we describe schools with such shortfalls as "under-

[6] Projections are typically made in January, prior to the start of the academic year. Although the projection methodology is not publicly disclosed, projected enrollment is highly correlated with enrollment in the prior year.
[7] While most non-FSF allocations are made before the start of the academic year, smaller additional funding may be issued throughout the year to address specific needs or conditions.

enrolled" schools.[8] In each year, HH status for each school was based on the enrollment variance. Thus, a school could qualify for HH in one year but not another, depending on enrollment relative to projection.

NYCDOE introduced the COVID hold harmless policy in March 2021, announcing a 25% protection to schools facing budget cuts due to enrollment losses, despite earlier warnings that under-enrolled schools should expect normal clawbacks.[9] This was expanded to 100% later that month, using federal stimulus funds to restore $130 million in anticipated clawbacks for 877 schools (NYC Comptroller 2023; Martin 2021). While hold harmless funding averaged $430 per pupil in 2021, some schools received as much as $2,812 per pupil. Additional resources for enrollment exceeding projections (positive enrollment variance) were provided to schools in accordance with the FSF formula.[10]

At the time, the hold harmless policy was framed as a one-time measure, with a requirement to forfeit unspent funds at year's end. While welcome, the March announcement allowed for limited use of the funds, as key deadlines for hiring and procurement had passed. Under pressure from principals and the teachers' union, NYCDOE allowed schools to roll over up to a third of hold harmless funds into the following academic year (AY 2022), providing greater flexibility to maintain staff (Zimmerman 2021a, 2021b).

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[8] Enrollment variances reflect deviations in the number of students *and* the composition of students (i.e. changes in their weights). Since we do not observe actual weighted enrollment, we can only approximate the hold harmless assignment rule by comparing the actual number of students to the projected number of students. In doing so, we find that approximately 86% of under-enrolled schools benefit from hold harmless, suggesting unfavorable enrollment variances were largely driven by smaller than expected numbers, rather than types of students.

[9] Specifically, the hold harmless policy restored FSF allocations to schools whose actual weighted enrollments fell below projections, bringing schools up to the total FSF amounts they would have received had their registers remained at pre-pandemic projected levels (NYC Comptroller 2023; Martin 2021). Funds were primarily provided by the Coronavirus Aid, Relief, and Economic Security (CARES) Act, the Coronavirus Response and Relief Supplemental Appropriations (CRRSA) Act, and the American Rescue Plan Act (ARPA).

[10] Amid pressure from union leaders and elected officials, the city was urged to fully protect school budgets during the pandemic. Union representatives warned that budget clawbacks would lead to cuts in essential staff, including teachers, while the Chancellor cautioned that full protection could be costly and disproportionately benefit wealthier schools (Zimmerman and Zimmer 2021).

The timing of the announcement of the 2022 hold harmless policy mirrored 2021, but the amount was larger.[11] Announced in February (2022), $324 million in anticipated clawbacks were restored for 1,211 schools (NYC Comptroller 2023), averaging $588 and as much as $5,287 per pupil.

The following year was different. In AY 2023, the hold harmless announcement came in November, allowing schools to use the funds for services in the late fall and winter and extending the planning horizon for spring spending – facilitating effective resource use. At the same time, only 775 schools were provided a total of $120 million in hold harmless funding, less than either 2021 or 2022.[12]

To be clear, hold harmless funds were always earmarked to support teacher costs (Oates 2021). Thus, the policy might be viewed as a teacher retention measure to prevent layoffs, stabilize staffing, and maintain readiness for potential post-COVID enrollment rebound (Bhole, Campanile, and McCarthy 2024).[13] In this case, the success of the policy might be judged by its impact on teacher retention in the short run and sufficiency of teaching staff in the years that follow. Impacts on students might be viewed as secondary (or even unintended) consequences.

Implications for Empirical Work

The impact of HH treatment on school resources and student outcomes is likely to differ

[11] Planning for AY 2022 began with great optimism about a return to "normal", as vaccine rollouts expanded and COVID rates declined. In May 2021, Mayor de Blasio and Chancellor Porter announced that all instruction would return to in-person only, ending hybrid and split schedules (Zimmer and Zimmerman 2021). Yet, enrollment continued to fall and declines were harder to predict than anticipated.

[12] In addition to the hold harmless policy, NYCDOE offered funding in AY 2023 for schools that were held harmless in AY 2022. This additional funding was announced in June 2022, offering a 50% hold harmless against downward revisions in enrollment projections; 93% of HH schools in AY 2022 received this allocation in AY 2023 because projections were "aggressively low," predicting 3.9% further declines in enrollments from prior year (AY 2022) actual enrollments (NYC Comptroller 2023). Empirically, this is captured in our model as a portion of the persistence in HH treatment effects (persistence of HH in 2022 on 2023 spending, etc.).

[13] In fact, schools were directly encouraged by NYCDOE to "re-fund" teachers funded with FSF allocations, and instead assign the salaries to the hold harmless allocation to "reduce overscheduling in FSF after the mid-year adjustment" (Oates 2021), freeing up discretionary FSF dollars for other uses.

across academic years for three key reasons. First, the policy was announced over halfway through the academic year in the first two years limiting the use of the funds, but early in year three, allowing principals to incorporate the additional resources into critical planning decisions and to use the resources in the fall in addition to the spring.[14] In contrast, announcements in 2021 and 2022 occurred after the hiring window had closed and most discretionary funds were allocated, limiting the strategic use of the new resources and making it infeasible to use the additional resources during the fall and winter. Instead, schools had to make one-off purchases or roll over unspent funds. This suggests larger improvements in staffing and student outcomes in the year when hold harmless was announced early (AY 2023) than in years with later announcement dates (AY 2021 and AY 2022). While this hypothesis is difficult to test directly, we examine alternative explanations to assess the plausibility that policy timing drove differences in HH effectiveness.

Alternatively, broader contextual factors may have enabled or constrained how effectively schools used HH funds, regardless of announcement timing or allocation size. For example, remote learning during 2021 posed obstacles to learning, including limited technology access, disruption, and reduced in-person engagement (Blanden, Doepke, and Stuhler 2023; Santibanez and Guarino 2021). AY 2021 also coincided with the peak of the COVID-19 crisis, which may have contributed to particularly poor outcomes that year—even in the presence of

[14] WSF money is discretionary, and principals have influence over operational policies, including busing (Cordes and Schwartz 2019) and meal programs (Corcoran, Elbel, and Schwartz 2016; Schwartz and Rothbart 2020), as well as teacher hiring and retention decisions. This autonomy, in principle, allows school leaders to align resources more closely with their understanding of students' needs (Ladd 2008; Malen, Egan, and Croninger 2017; Candelaria, Crutchfield and McGill 2024). Such optimization requires advanced planning, one reason why FSF relies on projected enrollments from the previous January to provide schools with their initial FSF allocations each July. Still, school decisions are constrained by union salary schedules, pupil-teacher ratio mandates, and required student services, among others (Levin et al 2019).

supplemental funding. AY 2022 and 2023, on the contrary, had fewer acute disruptions to educational practice.

Further, differences in enrollment trends, student attributes and administrative structures may mean the impact of HH differs across grade level (elementary, middle, high school), and the impact may be larger (smaller) for schools "treated" repeatedly.

III. Literature Review

This paper contributes to five strands of literature: first, research examining the impact of hold harmless policies on school resources and student outcomes; second, research examining equity in the distribution of resources; third, cutback budgeting literature, which documents potential equity consequences of education spending cuts made during periods of fiscal stress; fourth, research on the consequences of teacher layoffs and retention; and finally, recent work examining the impact of school funding during the COVID-19 pandemic.

Despite limited evidence on efficacy, hold harmless provisions are commonly used in federal, state and local funding programs to guarantee a minimum level of funding for recipient jurisdictions (e.g., school districts or municipalities) – often based on prior funding – to shield them from abrupt funding cuts during economic downturns, crises that reduce tax revenues or shifts in student enrollment (Baker and Corcoran 2012; Gigliotti and Sorensen 2018; Rothbart 2022). Hold harmless policies in education are often criticized for funding "ghost students" who are no longer enrolled and diverting limited resources from growing or high-need schools (Cunningham 2014; Garth Smith and Barnard, 2024). Hold harmless provisions may undermine the equalizing aims of school finance formulas by channeling funds through off-formula mechanisms, diluting redistributive efforts and preserving resources for schools in more advantaged areas (Baker and Corcoran, 2012). Bhole, Campanile, and McCarthy (2024) and others argue that hold harmless is politically motivated to appease special interest groups, such as teachers' unions, by prioritizing teacher job protection over student needs.

Gigliotti and Sorensen (2018) is the only study we are aware of that examines the impact of district-level hold-harmless policies specifically. They find that New York State's hold harmless provisions protected shrinking school districts, increased school resources and improved test scores, with benefits broadly distributed across the state.[15]

Still, proponents of hold harmless emphasize the importance of preserving teacher employment during periods of fiscal instability.[16] This was particularly relevant during COVID-19, because of widespread concern about teacher layoffs and voluntary separations (Amin 2020; McNichol and Leachman 2020; Burtis and Goulas, 2023; Cordes et al 2023). Teacher retention may have downstream benefits for children by reducing uncertainty, maintaining teacher morale, or reducing class size (Smith 2022).

A broader literature explores the impact of WSF formulas, generally finding improvements in equity (Blagg, Lafortune, and Monarrez 2022; Candelaria, Crutchfield, and McGill 2024; Jarmolowski, Alderman, and Roza 2022), although the impact is often tempered by off-formula allocations (Darden and Cavendish 2012; Atchison and Levin 2023). [17] Many

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[15] For evidence that increasing per-pupil funding through school finance reforms, government grants, or school district budget referenda improves student outcomes see Lafortune, Rothstein, and Schanzenbach 2018; Jackson, Wigger, and Xiong 2021; Handel and Hanushek 2023; Baron, Hyman and Vasquez 2024. Overall, this quasi-experimental research suggests that increasing district spending reduces class sizes and teacher turnover, increases teacher salaries and qualifications, and improves student outcomes such as test scores, attendance, and suspensions. School finance reform research also suggests it improves resource equity, mostly focusing on differences in impacts by district socioeconomic composition (Card and Payne 2002; Corcoran and Evans 2015; Jackson, Johnson, and Persico 2016; Lafortune, Rothstein, and Schanzenbach 2018), but also racial disparities (Rothbart 2020). Related work focuses on non-instructional spending (e.g., social workers, counselors, and health services), finding improved attendance and school climate and reduced suspensions (Sorensen and Goldsmith 2017; Reback 2010; Carrell and Hoekstra 2014).

[16] The American Recovery and Reinvestment Act (ARRA) of 2009, for example, provided funding to offset revenue losses during the Great Recession and mitigate teacher layoffs (Rebell 2011; Chakrabarti and Setren 2015).

[17] WSF is also intended to increase principal autonomy and budget discretion which improves student outcomes, especially those with higher percentage of White, Hispanic, free or reduced-price lunch-eligible (FRPL), bilingual

districts continue to fund schools regressively (Blagg, Lafortune, and Monarrez 2022), due in part to their higher average teacher salaries (Condron and Roscigno 2003; Iatarola and Stiefel 2003; Rubenstein and Miller 2005; Roza, Davis, and Guin 2007; Rubenstein et al 2007; Lee and Fuller 2022),[18] and, perhaps, limited political leverage of low-income schools (Condron and Roscigno 2003) or specific strategic features, such as higher funding for magnet or selective schools (Atchison and Levin 2023). These mechanisms can produce a positive correlation between test scores and school funding.

A wide range of studies finds that cutbacks – the other side of the hold harmless coin – reduce equity (Knight 2017; Knight et al 2022).[19] School districts may disproportionately cut non-instructional expenditures, protecting classroom teachers, for example (Berne and Stiefel 1993; Pendola 2022; Chakrabarti and Sutherland 2012) and states may distribute cuts by running their need-based aid formula "in reverse," disproportionately affecting high-need districts (Reschovsky and Schwartz 1992; Zhao and Coyne 2015; Rothbart 2022).

While a variety of studies document negative consequences of teacher layoffs such as increased class size (Rivkin, Hanushek, and Kain 2005; Evans, Schwab, and Wagner 2019), diminished teacher morale and school climate (Guin 2004; Kraft 2015; Goldhaber, Lavery, and Theobald 2016), and increased turnover and uncertainty (Goldhaber, Lavery, and Theobald 2016; Strunk et al 2018; Kraft and Bleiberg 2022), it is unclear whether protecting teachers from layoffs during fiscal crises – and distributing cuts elsewhere – mitigates these harms or improves student outcomes. Averting layoffs may preserve educational quality, particularly in high-

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and special education students (Levin et al 2013; Bluestein and Goldsmith 2021; Merkle 2022; Candelaria, Crutchfield, and McGill 2024; Jackson 2023).

[18] More-senior teachers tend to sort into more affluent schools with lower minority representation, even though schools serving more disadvantaged students often have lower pupil-teacher ratios (Rubenstein, Schwartz, and Stiefel 2006).

[19] For example, in New York State during the Great Recession (Rebell 2011).

poverty schools most vulnerable to cuts because they tend to be more reliant on more junior teachers who are more likely to be laid off (Knight and Strunk 2016; Chakrabarti and Sutherland 2015).[20]

Finally, a small set of studies focus on the COVID era, documenting a positive correlation between school spending and student outcomes (Dewey et al 2024; Relyea et al 2023), To our knowledge, causal evidence is not yet available.

IV. Data, Measures, and Sample

Data Sources and Measures

We use matched, longitudinal school-level data from the New York State Education Department (NYSED) and NYCDOE for AY 2016-2023. NYSED School Report Card (SRC) provides annual measures of total expenditures ($TotExp_{st}$), number of teachers ($Teachers_{st}$), and enrollment ($Enroll_{st}$) where s and t index schools and academic years, respectively. NYCDOE provides school-level data on average class size ($ClassSize_{st}$), projected enrollment ($ProjEnr_{st}$), a hold harmless treatment indicator (HH_{st}) and hold harmless funding ($HHpp_{st}$ in thousands), and student-level demographic and outcome data.[21] We also construct a time-invariant indicator, $EverHH_s$, which equals one if school s is hold harmless in one or more years (zero if s is never treated) as well as indicators for lagged treatment (HH_{st-1} and HH_{st-2}).

Key school resource measures are expenditure per pupil (EPP_{st}) as $TotExp_{st}$ / $Enroll_{st}$,[22] and pupil-teacher ratio (PTR_{st}) as $Enroll_{st}$ / $Teachers_{st}$. Average class size ($ClassSize_{st}$) is an

[20] Evidence from the Great Recession suggests that high-poverty and majority-minority schools are more likely to experience layoffs (Knight and Strunk, 2016; Chakrabarti and Sutherland, 2015), partly due to their reliance on state aid and seniority-based policies (Goldhaber et al., 2015; Kraft and Bleiberg, 2022).

[21] We also observe initial funding each year (except 2018), including FSF and non-FSF allocations.

[22] All financial measures are inflation-adjusted to 2019 dollars using CPI for All Urban Consumers. Data on $TotExp_{st}$ starting in 2019 come from the SRC. Before 2019, $TotExp_{st}$ was reported in the NYC School Based Expenditure Reports (SBER); however, the two measures rely on different definitions of school-level spending (e.g., inconsistent

enrollment-weighted average across grades for elementary and middle schools. For high schools, it is measured in English classes to avoid cross-subject weighting issues.[23]

We aggregate student-level data to construct school level outcome and sociodemographic variables including student attendance rate ($Attend_{st}$), the average percentage of school days present at school, and chronic absenteeism ($ChronicAbs_{st}$), the percentage of students with attendance less than 90%. Sociodemographics include percentage of students who are Hispanic ($Hispanic_{st}$), Asian ($Asian_{st}$), Black ($Black_{st}$), White ($White_{st}$), or other race ($Other_{st}$), the percentage eligible for free or reduced-price lunch ($FRPL_{st}$, i.e., "poor"), female ($Female_{st}$), English Language Learners (ELL_{st}), and students with disabilities (SWD_{st}). We measure school size as lagged enrollment ($Enroll_{st-1}$) and define enrollment variance (EV_{st}) as the ratio of actual to projected enrollment ($Enroll_{st}$ /$ProjEnr_{st}$) such that $EV_{st} < 1$ if enrollment fell below projections (under-enrolled) and $EV_{st} > 1$ if enrollment exceeded projections.[24]

Sample and Descriptive Statistics

Our analytic sample consists of 1,330 continuously operating traditional public schools with stable grade spans between AY 2016 and 2023.[25] Table 1 shows mean characteristics in 2019, the last full pre-COVID school year. NYC schools are diverse; roughly 40% of the students are Hispanic, with the remainder roughly evenly split among Asian, Black and White

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rules for allocating district expenditures such as transportation and professional development). For consistency, we report spending for the period after 2018, though our findings are robust to using a (partially) harmonized measure we developed to improve comparability between the two $TotExp_{st}$ measures. Results using this alternative measure are available upon request.

[23] To be sure, PTR_{st} and $ClassSize_{st}$ are correlated, but there are reasons to believe they may measure teacher resources distinctly. Not all teachers are assigned to classrooms, and it seems plausible that when enrollment is down schools may reduce the number of classes and reassign teachers to other tasks, especially with the protections of a hold harmless policy. The correlation between PTR_{st} and $ClassSize_{st}$ in our sample was 0.60 in AY 2019 (the last pre-pandemic year) and fell as low as 0.41 in AY 2021 before rebounding to pre-pandemic levels.

[24] As noted above, EV_{st} can only approximate HH assignment rule, because we do not observe actual weighted enrollments. During HH years, a large majority of schools with EV_{st} below one are HH schools.

[25] Focusing on these schools removes others that are under some form of school reorganization - e.g., phasing in, phasing out, phasing up, phasing down, etc.

students. Two percent identify as multiracial or another race ("other").[26] Roughly three-quarters are poor, 19% SWD, and 15% ELL. On average, pupil-teacher ratio was about 14, class size roughly 26, per-pupil expenditures above $16,000 and school size roughly 1,000 students.

[Table 1 about here.]

As shown in Table 1, in 2021, almost sixty percent of all schools were HH, increasing to almost 80 percent in 2022 and dropping to less than half by 2023. Almost all schools (95%) benefited from the hold harmless provision at some point in our study period. Schools "never" HH differ from the "ever" schools markedly. Never HH schools spent $1,700 more per pupil, had 0.8 fewer pupils per teacher, 0.9 fewer students per class, enrolled 281 fewer students, and had roughly 12 fewer teachers than ever HH schools. Never HH schools were disproportionately Black, Hispanic and SWD, compared to ever HH schools.

Prior to the pandemic, NYCDOE projections were quite accurate (mean EV is 1) with little evidence of systematic over or underestimation for both HH and non-HH schools. Further, while there is little difference between HH schools in 2021 and 2022, the 2023 cohort of HH had a lower percentage of Asian and White students and greater percentage of Black, Hispanic, and FRPL students.

V. Empirical Strategy

We begin by exploring the link between pre-pandemic (2019) school racial composition and hold harmless status in each year of the policy:

$$(1)\ HH_{st} = \alpha + \delta_A Asian_s + \delta_B Black_s + \delta_W White_s + \delta_O Other_s + X'_s\theta + \varepsilon_s$$

Specifically, we link HH_{st} to the percentage of the students Asian, Black, White, and Other, respectively, in 2019 (the year prior to the pandemic), along with X'_s – a vector of 2019

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[26] All descriptive statistics are pupil weighted. Other race/ethnicity suppressed here and in later tables.

demographic variables including $Female_s$, $FRPL_s$, SWD_s, ELL_s and $Enrollment_s$– and ε (an error term with the usual properties). Percentage Hispanic is the omitted group. We estimate the model separately for each year of the study period. In this model, δ coefficients capture the increase (or decrease) in the probability school *s* is HH in year *t* with a one percentage point increase in the percentage of the students in the corresponding racial group in 2019. Taken together, coefficients different from zero – whether positive or negative - suggest disparate racial impact of the hold harmless provision and allow us to explore differences in these disparities across the three cohorts – 2021, 2022, 2023. We also estimate this model substituting "ever HH" in for HH_{st}.

Then, to examine potential disparate racial impact in the *amount* of HH funding among HH schools, we substitute $HHpp_{st}$ as the dependent variable and estimate for each year using the sample of HH schools in that year. In these models, δ captures the change in per-pupil HH funding associated with a one percentage point change in the share of students in each race group and coefficients different from zero again suggest disparate racial impact of the level of hold harmless funding. We again estimate the model separately for each year of our study period.

Together, these regressions shed light on the extent to which race shapes the benefits of HH funding. These models shed light on whether concerns about disproportionate financial benefits to schools with larger White percentages bear out empirically.

Estimating Impacts of HH Funding

The centerpiece of our empirical work is a regression model aimed at estimating the impact of HH treatment on school resources, staffing, and student outcome, leveraging within-school variation in HH over time.

We begin with a "naïve" DiD model comparing outcomes before and after the hold harmless policy, for HH schools versus non-HH schools. The key identifying assumption is that, absent the hold harmless policy, outcomes in HH and non-HH schools would have evolved similarly over time.[27]

To be specific, we specify our naïve model as:

$$(2)\ Y_{st} = \alpha + \beta_1\ HH_{st} +\ Z'_{st}\theta + \mu_t +\ \gamma_s + \varepsilon_{st}$$

Where Y_{st} is the outcome of interest (e.g., expenditure per pupil, staffing, or academic outcomes) for school *s* in year *t*; HH_{st} is as defined above; Z'_{st} includes time-varying school characteristics including $Asian_{st}$, $Black_{st}$, $White_{st}$, $Other_{st}$, *Female, FRPL, SWD, ELL* and $Enroll_{st-1}$; year fixed effects, μ_t, capture common citywide factors such as COVID; school fixed effects, γ_s, control for time-invariant school characteristics, such as building size and location. The coefficient of interest, β_1, captures the average effect of HH on school and student outcomes. Standard errors are clustered at the school level.

Heterogeneity in the Treatment Effect by HH Cohort and Persistence of Effects

We follow with a more flexible specification that accounts for staggered treatment adoption and the persistence of treatment effects, relaxing the assumption of homogeneous treatment effects across years in two ways. First, we use a parsimonious staggered-entry two-way fixed effects (TWFE) DiD specification that allows for heterogeneity in effects by HH cohort (year of HH funding). Second, we add terms to capture the persistence of impacts one and two years following HH funding, using the following specification:

$$(3)\ Y_{st} = \alpha + CohortYear'(\beta_1 HH_{st} + \beta_2\ HH_{st-1} + \beta_3\ HH_{st-2}) +\ X'_{st}\theta +\ \mu_t + \gamma_s + \varepsilon_{ist}$$

Where $CohortYear'_{st}$ is a vector of indicators for each HH cohort year (i.e., treatment years AY 2021-2023), which we interact with current (HH_{st}) and lagged treatment status indicators

[27]As shown in Figures 1 and 2, unconditional trends for school resources, staffing and attendance outcomes, which suggest pre-pandemic trajectories are parallel. Although not a formal test of the parallel trends assumption, these bolster our confidence that a causal interpretation is warranted.

(HH_{st-1} and HH_{st-2}); other variables defined above. Coefficients β_1 capture contemporaneous treatment effects of hold harmless, yielding different effect estimates for each HH cohort (2021, 2022 and 2023). Coefficients β_2 capture the impact of 2021 (2022) HH on outcomes a year later, in 2022 (2023) and β_3 captures the impact of 2021 HH on outcomes two years later, in 2023.

Robustness Checks and Alternative Specifications

We explore the robustness of our results to an alternative estimator which relaxes the assumption of homogeneous impacts by duration and timing of first HH treatment, that is, the number of years a school is HH (e.g., different effects for schools treated three times than schools treated once or twice) and which year treatment begins and ends (AY 2021-2022, AY 2021-2023, etc.). We use Wooldridge's flexible DiD estimator (2023), which allows for treatment entry and exit, enabling us to explore heterogeneity by treatment duration, measured using the timing of first and last HH treatment. To do so, we create separate treatment indicators that classify schools into seven mutually exclusive groups by first and last year of hold harmless funding. Each group is, then, characterized by a specific treatment spell.[28] We re-estimate model (4) substituting this larger set of treatment indicators for the more parsimonious set, allowing the coefficients to differ across academic years. In this way, we will gain insight into heterogeneity in the treatment effects – that is, whether (and how) the effects of HH differ across the three policy years, persist (or fade) over time, and if such patterns vary by the duration of treatment and timing of first and last treatment (treatment spell).

We then conduct several additional robustness checks and supplementary analyses. First, we use post-estimation tests to investigate the possibility that the impact of HH changes with the number of years a school is protected by HH – driven, perhaps, by learning by principals or other

[28] Descriptive statistics for each treatment spell are provided in Appendix Table A1.

school personnel in how to best use these resources.[29] Second, we replace our binary treatment indicators with HH funding per pupil. Third, we limit our sample to the set of schools that are ever hold harmless, removing the unusual set of schools that were never hold harmless. Finally, we stratify by grade level to explore potential heterogeneity in the impact of hold harmless at the elementary, middle and high school levels, which vary in average enrollment loss during COVID (elementary schools had larger declines) as well as attendance patterns.

VI. Results

Racial Disparities in HH Treatment

To begin, contrary to the fears of critics, we see no evidence that hold harmless disproportionately benefited schools that were predominantly White. In fact, Table 2 shows evidence that racial composition is not a statistically significant predictor of ever receiving HH funding (column 1), in AY 2021 (column 2), or in AY 2022 (column 3), with only moderate associations emerging in 2023 that – if anything – suggest the probability of HH funding decreases with school percentage of White and Asian students, as compared to Hispanic and Black (column 4). In AY 2023, a 10 percentage point increase in percent Asian is associated with 3 percentage point lower probability of HH treatment (and a similar 10-point increase in White share with a 7 point lower probability), compared to schools with more Hispanic students. This is roughly 6.8% (15.9% for White) of the share of schools held harmless in 2023, which is 44%.

[Table 2 about here.]

Meanwhile, among HH schools, those with greater percentages of Black students receive larger per-pupil HH funding in all three treatment years (columns 5–7 of Table 2), despite the

[29] We assess whether 2023 treatment effects increase with times hold harmless, comparing (1) third-time treatment (T21_23*2023) to first- and second-time treatment, and (2) second-time treatment against first-time treatment.

concerns of critics that the policy could disproportionately benefit whiter, wealthier schools. A 10 percentage point increase in percent Black is associated with a $34 HH funding increase in 2021 (9.8% of the mean HH allocation that year), $32 in 2022 (6.5%), and $38 in 2023 (9.6%) as compared to Hispanics. In contrast, in some years, schools with higher percentages of Asian or White students receive less HH funding compared to Hispanics. For instance, in 2021, a 10 percentage point increase in percent Asian is associated with $32 lower HH funding per pupil (9.1% of the mean). In 2023, a 10 percentage point increase in percent White is associated with $43 lower HH funding per pupil (10.9% of the mean).

School Spending and Teacher Retention

As shown in Table 3, HH increased school spending and supported teacher retention in the face of enrollment declines (perhaps as intended) and produced sizable reductions in pupil-teacher ratios and class sizes (perhaps unintentionally). Table 3 columns 1 and 5 show estimates of the effects of HH on school expenditures. The "naïve" estimate in column 1 indicates HH increased spending per pupil by an average of $607, about a 5% boost compared to the mean for never HH schools (shown in Table 1). The results in column 5 suggest some heterogeneity in those effects by HH cohort, ranging from $521 in AY 2021 to $684 in AY 2022. Our preferred specification that allows for persistence of treatment effects (column 9), yields larger estimates that are more similar across treatment years: HH treatment increases expenditures per pupil by roughly $779 in AY 2021, $774 in AY 2022, and $693 in AY 2023. We also find a persistence of the treatment effect (though effects decline with time) that lasts for at least two years, increasing spending per pupil by $656 and $491 in AY 2022 and 2023 for schools held harmless in 2021 and increasing spending per pupil by $539 in AY 2023 for schools held harmless in 2022. For schools treated repeatedly, effects are additive (e.g., schools that are HH in 2021 and

2022 are expected to spend $1,430 (which equals $774 + $656) – more per pupil in AY 2022).

[Table 3 about here.]

Table 3 also provides some evidence that HH improved teacher retention, in turn decreasing both pupil-teacher ratio and class sizes. By design, HH targeted schools with unexpected large declines in enrollments, but the number of teachers in HH schools either did not change (as suggested in the naïve model shown in column 2) or declined modestly in only one treatment year, AY 2021 (a decline of roughly two teachers off a mean of 70, with no evidence of decreases in AYs 2022 and 2023; columns 6 and 10). As a result, HH reduced pupil-teacher ratios by 0.48 on average, roughly 3.4% of the mean (column 3), and between 0.40 (AY 2021) to 0.66 (AY 2022) students per teacher once accounting for the persistence of impacts. As shown in column 11, effects on pupil-teacher ratios were roughly similar in all three HH years and improvements in pupil-teacher ratio persisted one year post-HH funding (though effect sizes decline the year after treatment and fade entirely by the second year post-HH). Finally, as shown in column 12, increases in school resources translated into reduced class sizes; average class size fell by roughly 2.4 students in 2021 (a 9.3% reduction relative to the mean) and by one student in both 2022 and 2023 (a 4.0% reduction). Effects on class sizes also persist one year, though they decline the first year post-HH and fade entirely by the second post-HH year.

Student Outcomes

Despite the large increases in spending and teacher resources, the results in Table 4 suggest that HH funding only improves student outcomes in 2023 (AY 23 in columns 3-6), and not on average (columns 1 and 2) or in the first two policy years (AY 21 and 22 in columns 3-6). Improvements in attendance outcomes in 2023 are notable: in preferred models (columns 5 and 6), we find a 0.6 percentage point increase in the attendance rate (mean is 91.0 percent, meaning a mean absenteeism rate of 9.0 percent) and a 2.0 percentage point decrease in the chronic absenteeism rate (mean of 27.2 percent). Both models suggest HH funding reduced absenteeism by about 7 percent (0.6/9.0 for the absence rate and 2.0/27.2 for chronic absenteeism rate, respectively). Given HH only improved attendance outcomes in 2023, it is perhaps not surprising that we do not find evidence of persistent effects (only observed for the 2021 and 2022 cohorts).

[Table 4 about here.]

Robustness Checks and Alternative Specifications

Using Wooldridge's flexible DiD estimator (2023), we find evidence that our parsimonious model that accounts for HH cohort and persistence of effects provides unbiased estimates, even though it imposes some homogeneity constraints on the treatment effects across treatment spells.[30] As shown in appendix Table A2, there is no evidence that treatment effects vary by treatment spell (duration and first year of HH funding) once accounting for HH cohort and persistence of effects (as we already do for results in Tables 3 and 4). For example, the estimated effect of HH funding for the third time (in AY 2023) on expenditures per pupil is $1,694 (as shown in appendix Table A2 column 1), which is statistically indistinguishable from the sum of coefficients in Table 3 column 9 for AY 2023 treatment plus the persistent effects from HH treatment in 2021 and 2022 ($693 + $549 + $491 = $1,733). This suggests coefficients from our preferred specification are, in fact, additive and unbiased. We repeat similar Wald tests across all analogous coefficients, comparing results in appendix Table A2 (the fully flexible estimator) to those in Tables 3 and 4 (our preferred estimator), and in all cases we fail to reject the null hypothesis of equivalence between model estimates.

[30] In addition to serving as a robustness check, appendix Table A2 provides useful insights into treatment dynamics. For example, impacts on per-pupil spending are additive over time, with the largest gains occurring in schools' final years of treatment (following a "staircase" pattern consistent with the persistent effects shown in Table 3).

Using the results in appendix Table A2, we can also test whether the benefits of HH increase with repeated exposure, comparing schools treated once, twice, or three times. Schools treated three consecutive years experienced the largest increases in EPP_{st}, followed by twice-treated schools, with once-treated schools experiencing the smallest gains. However, this gradient reflects persistent effects of fund rollover documented in Table 3 (as noted above, differences in effect sizes by cumulative exposure are roughly equivalent to the sum of the relevant coefficients reported in Table 3). We find no evidence of increased benefits of repeated/cumulative exposure beyond persistent treatment effects already documented in Tables 3 and 4.[31]

Appendix Table A3 shows estimates of the impacts HH dollars, using per-pupil HH funding in thousands of dollars ($HHpp_{st}$) and again attending to differences by treatment year. Results are consistent with those from models that use binary measures of treatment. In all three years, greater HH funding is associated with increases in school resources and magnitudes are similar across years, while improvements in attendance and chronic absenteeism are concentrated in 2023 alone.

For appendix Table A4, we restrict the sample to schools that are ever HH (i.e., excluding schools that were never HH). Our results remain virtually unchanged from the full sample, demonstrating the findings are not reliant on comparisons to never HH schools.

As shown in appendix Tables A5-A7, elementary, middle and high schools all experienced resource gains of roughly similar magnitudes as the main results, though middle

[31] In addition, we estimate models that exclude time-varying racial composition controls. Results are virtually unchanged from those in Tables 3 and 4, suggesting our findings are not artifacts of student sorting based on schools' previous HH funding or other forms of endogeneity due to contemporaneous measurement of school racial composition. Results available upon request of the authors.

schools saw slightly higher increases in school resources and staffing, while high schools experienced slightly smaller changes in expenditures per pupil and average class size.

Impacts on attendance and absenteeism varied more by school level. HH improved elementary schools' attendance rates and chronic absenteeism in 2023, but not in middle schools or high schools. We find, in fact, some evidence that HH, if anything, harmed high schools' attendance outcomes in 2022. Taken together, this suggests the benefits of HH for attendance outcomes were concentrated in elementary schools in the year in which policy announcement came earlier in the school year, though we could only speculate on why HH had larger effects on attendance in those grades.

VII. Discussion and Conclusions

The debate over whether "money matters" in education has shifted to how, and under what conditions, funding affects student outcomes (Hanushek 2016; Handel and Hanushek 2023). Yet, little evidence exists on whether hold harmless policies exacerbate racial disparities in school resources, can stabilize teacher retention, and/or mitigate the harmful effects of crises on academic outcomes. We explore the effects of NYCDOE's hold harmless policy during COVID to shed light on these questions, finding that the biggest beneficiaries were teachers, that there was no evidence that schools with a more White students benefited disproportionately, and that the evidence on attendance impacts was mixed (which only occur when the policy is announced early in the school year).

Prior work on hold harmless policies and cutback budgeting has raised concerns about potential racial disparities in funding (Reschovsky and Schwartz 1992; Cunningham 2014; Rothbart 2022; Garth Smith and Barnard 2024). Despite critics' concerns, NYC schools with larger percentage of White students were no more likely to receive HH funding. Instead, increases in Black student percentage were associated with larger HH allocations. While differences were somewhat modest, this suggests these concerns largely did not materialize.

The average effects of HH treatment on school resources, however, are large. Perhaps as intended, HH increased expenditures per pupil, which largely kept teachers in their schools (HH school head counts decreased modestly in 2021 and not at all in other years), resulting in reductions in pupil-teacher ratio and HH schools offering smaller class sizes. The effects on resources seem pedagogically meaningful; the size of impacts on expenditures per pupil, for example, are of comparable size to the mean effects of school finance reforms on revenues per pupil (see, for example, Lafortune, Rothstein, and Schanzenbach 2018), while impacts on class sizes are nearly large enough to bring the vast majority of schools in compliance with New York State's new maximum class size mandates passed in 2022 (NYCDOE, 2025). If we were to project out, however, it seems likely that if enrollments were to stabilize (and hold harmless discontinued), that pupil-teacher ratios and class sizes would likely rise again in previously treated HH schools, because they would eventually face traditional clawback mechanisms.

In this way, it seems like the hold harmless policy "worked" as intended. Teacher retention was a clear goal of hold harmless, as schools were instructed to spend HH funds on teacher costs. This also aligned with the priorities of teachers' unions – and perhaps principals – who may value job security and stability in the labor force over alternative emergency uses (e.g. tutoring or credit-recovery programs), especially amid economic uncertainty.

Despite large preservation of resources in HH schools in the first two policy years, we find no evidence of spillovers for student attendance in 2021 or 2022. Hold harmless treatment in 2023, however, improved attendance outcomes, perhaps due to improved planning or earlier use of the additional funding. That year, NYCDOE announced HH funding early in the academic year, enabling more strategic planning, budgeting, and earlier intervention. Larger effects in 2023 do not appear to be driven by repeated treatment or changes in student composition. Gains in attendance outcomes were concentrated in elementary schools, while we find some evidence that high schools (which received less hold harmless funding in general) experienced diminished student outcomes.

Differences in the effects of HH treatment across treatment years also may reflect external factors to the school, such as family circumstances, transportation availability, illness, or student disengagement, which may have limited the policy's observable effects. A priori, one may have thought that hold harmless may have played a stabilizing role by preventing further deterioration. We do not find much evidence of this. This explanation is also undermined by the finding that the null effects on attendance outcomes were similar in 2021 (when COVID-related disruptions were worse) to those in 2022. Still, HH may have been most effective as schools transitioned out of the acute pandemic period. External labor markets had loosened and schools had more "normal" operations, and the ability to retain teachers was more consequential. Moreover, had NYCDOE not funded a hold harmless policy, under-enrolled schools might have experienced unusually high student disengagement and reductions in attendance during COVID, especially once they made unusually large cuts to their teacher labor force, and we cannot rule out the possibility that HH funding mitigated these unusually large deviations in outcomes.

We draw several policy implications from these findings. First, policymakers can take actions to ensure that crises do not worsen racial disparities in school resources. NYCDOE could have routed aid through the FSF formula (essentially increasing the "base allocation" for all students), but instead chose to partially roll back the formula. Funding "ghost students" with hold harmless policies could undermine WSFs' equity gains and amplify COVID-era inequalities, but in practice we find little evidence that this policy produced major racial disparities.

Second, while retaining teachers has value, it may not be the most effective way to support students during crises. We provide evidence that HH funding was used to retain teachers in the face of shrinking enrollment. While appealing, it is unclear whether this use of resources improved student attendance outcomes as much as other interventions might have. That said, the cutbacks that might have been implemented in the absence of hold harmless may well have been particularly disadvantageous in the context of COVID, such that the null effects we see may be better (or worse) than what might emerge under other circumstances. While it makes sense to consider the views of teachers and teachers' unions when setting policies related to emergency resources, their preferences may not always coincide with those of parents, students, and principals.

Third, the timing of policy announcements and implementation of hold harmless policy may matter. Contradictory mid-year announcements about the hold harmless policy likely confounded planning. Emergency funds allocated before budget deadlines may have larger effects on student outcomes because this allows schools to prepare for core expenses like staffing and to start using resources in the fall semester. Predictable timelines – and clarity about future funding – appear to be crucial for effective planning during crises, which seems relevant for other policies and is worthy of future study.

Fourth, schools that received hold harmless for three consecutive years – typically those with the steepest enrollment declines – experienced the largest funding increases. Without an exit strategy, these schools risk becoming long-term "fiscal wards," receiving disproportionate funding while serving fewer students. At the same time, schools risk facing abrupt reductions in funding in the absence of hold harmless and when the district returns to formula-based funding. Pairing hold harmless policies with right-sizing or consolidation plans may help prevent temporary aid from becoming a permanent subsidy and from undermining the equity goals that formula-based funding is intended to address.

In sum, the hold harmless experience in NYC offers broader lessons for how districts can respond to crises with flexibility, foresight, and attention to fairness, while remaining vigilant about whether such measures transition from temporary relief to structural distortion. Such lessons are especially pertinent as schools, districts, and states, adjust to major changes in federal tax and spending policy that are likely to affect education revenues (Lieberman 2026).

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Table 1. School Variable Means, by HH Status, 2019

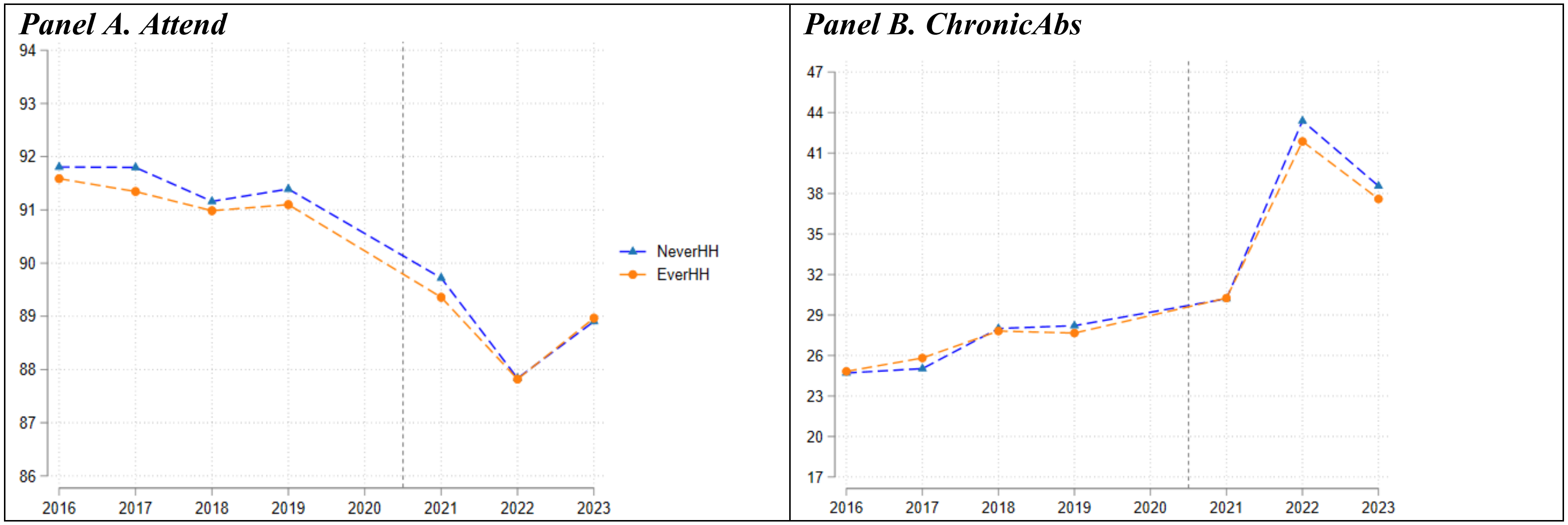
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			2021		2022		2023	
	Ever HH	Never HH	HH=1	HH=0	HH=1	HH=0	HH=1	HH=0
HH_21	59.9	0	100	0	59.1	49.0	50.5	62.6
HH_22	82.6	0	81.5	74.5	100	0	75.2	81.4
HH_23	49.5	0	41.7	54.1	45.1	54.2	100	0
Resources:								
EPP	16092.7	17777.4	16213.2	16099.9	15957.0	17054.3	16231.9	16113.1
Teachers	70.6	57.9	68.7	71.9	70.9	66.4	70.8	69.4
PTR	14.2	13.4	14.3	14.1	14.3	13.8	14.0	14.4
ClassSize	26.3	25.4	26.2	26.2	26.4	25.7	26.1	26.4
School Chars.:								
Enroll	1071.5	779.5	1051.3	1069.3	1070.4	1009.9	1062.1	1056.3
$Enroll_{t-1}$	1081.6	801.1	1060.4	1081.9	1081.6	1017.5	1077.5	1063.0
EV	1.0	1.0	1.0	1.0	1.0	1.0	1.0	1.0
Attend	91.9	91.6	92.3	91.4	92.0	91.6	90.9	92.7
ChronicAbs	24.2	25.8	23.4	25.4	23.9	25.6	27.1	22.0
Percentage:								
Asian	19.3	12.6	20.0	17.6	19.5	16.5	15.8	21.5
Black	21.3	22.8	19.1	24.5	21.2	22.0	26.5	17.3
Hispanic	40.4	44.6	40.2	41.1	39.8	44.2	43.5	38.3
White	16.5	17.9	18.1	14.5	16.9	15.1	11.7	20.4
Female	49.0	49.2	48.7	49.3	49.2	47.9	49.7	48.4
SWD	18.9	20.7	18.7	19.3	18.9	19.0	19.5	18.5
ELL	15.0	15.6	16.3	13.1	14.5	16.9	14.2	15.6
FRPL	73.3	73.5	72.2	74.8	72.7	75.8	76.6	70.7
Grade Level:								
Elementary	48.9	63.0	58.5	37.2	49.3	50.7	36.3	60.1
Middle	33.5	17.3	34.2	30.9	35.0	23.8	33.1	32.7
High	33.1	34.0	20.5	50.6	32.6	35.4	45.9	23.1
Schools	1,264	66	757	573	1,044	286	626	704

Notes: Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served), full-time special education, alternative, and charter schools. Ever HH schools are treated in AY 2021, 2022, and/or 2023; Never HH schools are never held harmless. All figures weighted by student enrollment. Means for "Other" race suppressed because of small size.

Figure 1. School Resources by Hold Harmless Status

Notes: Sample includes NYC public schools that operate continuously from 2016-2022, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are excluded. EPP reported in 2019 dollars using Urban CPI. Dashed line in 2020 indicates beginning of COVID period.

Figure 2. Student Outcomes by Hold Harmless Status



Notes: Sample includes NYC public schools that operate continuously from 2016-2022, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are excluded. Dashed line in 2020 indicates beginning of COVID period.

Table 2. Regression Results: Hold Harmless Treatment and Baseline Racial Composition, 2019

	(1) EverHH	(2) HH_21	(3) HH_22	(4) HH_23	(5) HHpp_21	(6) HHpp_22	(7) HHpp_23
Asian	0.001	0.001	0.002**	-0.003***	-3.146***	0.589	-0.410
	(0.000)	(0.001)	(0.001)	(0.001)	(0.646)	(0.795)	(1.042)
Black	-0.000	-0.000	0.000	0.000	3.446***	3.240***	3.811***
	(0.000)	(0.001)	(0.001)	(0.001)	(0.684)	(0.798)	(0.995)
White	-0.000	0.003*	-0.000	-0.007***	-0.520	-0.472	-4.317***
	(0.001)	(0.002)	(0.001)	(0.002)	(1.051)	(1.190)	(1.377)
Mean DV	0.957	0.581	0.810	0.443	347.306	496.124	396.408
School Controls	X	X	X	X	X	X	X
N	1330	1330	1330	1330	757	1044	626
R^2	0.007	0.052	0.016	0.088	0.191	0.071	0.180

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Columns 5, 6, and 7 include schools that are treated in the indicated HH year (2021, 2022, and 2023, respectively). Reference group is Hispanic; coefficients for "Other" race suppressed. All columns are weighted by student enrollment.

Table 3. Regression Results: Hold Harmless and School Resources, Difference-in-Differences Model, 2016-2023

	EPP			Teachers			PTR			Class Size		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
HH_t	607.48***			-0.49			-0.48***			-1.45***		
	(49.36)			(0.69)			(0.06)			(0.25)		
HH_t												
2021		520.88***	778.84***		-1.97**	-2.47***		-0.35***	-0.40***		-2.28***	-2.39***
		(48.89)	(62.11)		(0.81)	(0.92)		(0.09)	(0.10)		(0.61)	(0.61)
2022		684.02***	773.53***		-0.12	0.02		-0.61***	-0.66***		-0.92***	-0.95***
		(88.65)	(105.49)		(0.94)	(1.14)		(0.09)	(0.11)		(0.16)	(0.16)
2023		651.70***	693.24***		0.86	0.63		-0.54***	-0.57***		-0.89***	-0.89***
		(103.07)	(103.88)		(1.17)	(1.13)		(0.10)	(0.10)		(0.23)	(0.23)
HH_{t-1}												
2022			655.63***			-1.84*			-0.24***			-0.89***
			(104.06)			(1.00)			(0.09)			(0.15)
2023			549.06***			0.15			-0.46***			-0.67**
			(132.40)			(1.45)			(0.17)			(0.27)
HH_{t-2}												
2023			490.71***			-2.09			-0.16			0.08
			(123.19)			(1.28)			(0.11)			(0.22)
MeanDV	16,594.2	16,594.2	16,594.2	69.6	69.6	69.6	14.0	14.0	14.0	25.5	25.5	25.5
Controls	X	X	X	X	X	X	X	X	X	X	X	X
Schl FE	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X
N	6649	6649	6649	10639	10639	10639	10639	10639	10639	10533	10533	10533
Schools	1330	1330	1330	1330	1330	1330	1330	1330	1330	1321	1321	1321

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively. The period of study is 2016-2023. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference groups are 2019 and Hispanic. All columns are weighted by student enrollment.

Table 4. Regression Results: Hold Harmless and Student Outcomes, Difference-in-Differences Model, 2016-2023

	Attend			ChronicAbs		
	(1)	(2)	(3)	(4)	(5)	(6)
HH_t	-0.06			-0.02		
	(0.25)			(0.79)		
HH_t						
2021		-0.31	-0.36		0.63	0.79
		(0.27)	(0.28)		(0.75)	(0.78)
2022		-0.24	-0.24		0.67	0.70
		(0.15)	(0.16)		(0.51)	(0.56)
2023		0.67***	0.63***		-2.15***	-2.00***
		(0.15)	(0.15)		(0.51)	(0.53)
HH_{t-1}						
2022			-0.03			0.01
			(0.16)			(0.57)
2023			-0.01			0.23
			(0.18)			(0.60)
HH_{t-2}						
2023			-0.31**			1.11**
			(0.16)			(0.56)
MeanDV	91.0	91.0	91.0	27.2	27.2	27.2
Controls	X	X	X	X	X	X
Schl FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	9309	9309	9309	9309	9309	9309
Schools	1330	1330	1330	1330	1330	1330

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. The period of study is 2016-2023. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference groups are 2019 and Hispanic. School controls include lagged enrollment, and percentage of race, SWD, ELL, and female students. All columns are weighted by student enrollment.

Appendix

Table A1. Distribution of Schools by HH Treatment Spell

Treatment Spell	Schools	%	Once treated, always treated
T21_21	65	4.9	No
T21_22	376	28.3	No
T21_23	241	18.1	Yes
T22_22	197	14.8	No
T22_23	230	17.3	Yes
T23_23	80	6.0	Yes
T21_0_23	75	5.6	No
Never treated	66	5.0	NA
Total	1,330		

Notes: Subscripts following "T" denote the first treatment year; subscripts following the underscore denote the last year of treatment (e.g., T21_22 = treated in 2021–2022). 'Once treated, always treated' refers to schools that once they received HH during the 2021–2023 period they received it for the years after. The cases where this is not true would be mismeasured in standard DiD models that assume treatment is permanent.

Table A2. Regression Results: Hold Harmless and School Resources, Heterogeneity by Duration and Timing of First Treatment

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
Once (2021):						
t21_21 x 2021	916.79***	-7.96**	-0.15	-1.85	-0.74	1.33
	(149.58)	(3.70)	(0.35)	(1.25)	(0.58)	(1.59)
t21_21 x 2022	505.23**	-0.94	-0.10	-0.82***	-0.47	0.94
	(253.94)	(2.45)	(0.21)	(0.25)	(0.33)	(1.12)
t21_21 x 2023	462.32	-0.55	-0.34	0.40	-0.27	0.20
	(344.36)	(3.47)	(0.45)	(0.64)	(0.39)	(1.24)
Once (2022):						
t22_22 x 2022	868.02***	-1.31	-0.52***	-1.22***	-0.37	0.88
	(167.06)	(1.36)	(0.16)	(0.26)	(0.27)	(0.93)
t22_22 x 2023	587.17*	-1.01	-0.44	-0.35	-0.14	-0.06
	(349.77)	(2.27)	(0.40)	(0.70)	(0.35)	(1.13)
Once (2023):						
t23_23 x 2023	827.48**	-0.64	-0.52	-0.17	0.25	-0.79
	(349.37)	(2.88)	(0.48)	(0.72)	(0.50)	(1.50)
Twice (2021-22):						
t21_22 x 2021	788.93***	-3.00***	-0.31***	-3.74***	-0.08	0.45
	(71.81)	(0.85)	(0.10)	(0.57)	(0.34)	(0.90)
t21_22 x 2022	1430.00***	-2.65**	-0.72***	-2.12***	-0.22	0.63
	(167.27)	(1.27)	(0.15)	(0.21)	(0.26)	(0.88)
t21_22 x 2023	1043.73***	-2.55	-0.56	-0.36	-0.44	1.76
	(343.38)	(2.26)	(0.39)	(0.62)	(0.34)	(1.07)
Twice (2021&23):						
t21_0_23 x 2021	770.62***	-0.80	-0.51**	-0.85	-0.57	1.34
	(186.17)	(1.91)	(0.23)	(1.94)	(0.61)	(1.62)
t21_0_23 x 2022	296.14	-2.25	0.29	-0.24	-0.12	-0.08
	(252.66)	(1.45)	(0.21)	(0.34)	(0.38)	(1.33)
t21_0_23 x 2023	981.92**	-3.81*	-0.48	-0.07	0.02	-0.56
	(415.18)	(2.19)	(0.43)	(0.74)	(0.42)	(1.44)

Twice (2022-23):						
t22_23 x 2022	347.13**	1.94	-0.38**	-0.49	-0.39	0.96
	(174.08)	(1.92)	(0.17)	(0.31)	(0.30)	(1.01)
t22_23 x 2023	1088.43***	1.54	-1.03**	-1.15*	0.54	-1.66
	(345.20)	(2.78)	(0.40)	(0.68)	(0.38)	(1.17)
Thrice (2021-23):						
t21_23 x 2021	727.98***	-0.47	-0.60***	-0.65	-0.66	1.05
	(88.57)	(1.27)	(0.12)	(1.12)	(0.41)	(1.13)
t21_23 x 2022	1115.36***	0.42	-0.88***	-1.27***	-0.55*	1.16
	(186.89)	(1.69)	(0.17)	(0.26)	(0.30)	(0.92)
t21_23 x 2023	1694.12***	-0.98	-1.27***	-0.92	0.18	-0.93
	(363.62)	(2.43)	(0.40)	(0.64)	(0.40)	(1.29)
Mean DV	16,594.2	69.6	14.0	25.5	91.0	27.2
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	6649	10639	10639	10533	9309	9309
Schools	1330	1330	1330	1321	1330	1330

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. The period of study is 2016-2023. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference groups are 2019 and Hispanic. All columns are weighted by student enrollment. Group indicators follow the format *tYY_ZZ*, where *YY* is the first year a school received hold harmless (HH) treatment, and *ZZ* is the last. For example, *t21_21* denotes schools treated only in 2021, *t21_23* those treated in all three COVID years (2021–2023), and *t21_0_23* schools treated in 2021 and 2023 but not in 2022.

Table A3. Regression Results: Impacts of Hold Harmless Allocations per Pupil on School Resources and Student Outcomes

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
$HHpp_t$						
2021	2.07***	-0.00**	-0.00***	-0.00***	-0.00	-0.00
	(0.12)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
2022	2.02***	-0.00***	-0.00***	-0.00***	-0.00	0.00
	(0.09)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
2023	1.75***	-0.00***	-0.00***	-0.00***	0.00***	-0.00***
	(0.12)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$HHpp_{t-1}$						
2022	1.50***	-0.00***	-0.00***	-0.00***	0.00*	-0.00**
	(0.17)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
2023	1.59***	-0.00***	-0.00***	-0.00	-0.00	0.00*
	(0.13)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
$HHpp_{t-2}$						
2023	1.02***	-0.00***	-0.00	0.00	0.00	-0.00
	(0.21)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mean DV	16,594.2	69.6	14.0	25.5	91.0	27.2
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	6644	7973	7973	7897	6644	6644
Schools	1329	1329	1329	1319	1329	1329
R^2	0.95	0.99	0.86	0.65	0.83	0.85

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference group is Hispanic. All columns are weighted by student enrollment.

Table A4. Regression Results: Impacts of Hold Harmless on School Resources and Student Outcomes, Ever HH Schools

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
HH_t						
2021	813.60***	-2.71***	-0.42***	-2.65***	-0.37	0.78
	(63.71)	(0.96)	(0.10)	(0.64)	(0.29)	(0.82)
2022	861.44***	-0.16	-0.74***	-1.05***	-0.30	1.04
	(111.17)	(1.31)	(0.12)	(0.18)	(0.19)	(0.64)
2023	691.41***	0.66	-0.57***	-0.95***	0.65***	-2.01***
	(104.32)	(1.20)	(0.10)	(0.24)	(0.16)	(0.56)
HH_{t-1}						
2022	705.94***	-1.96*	-0.28***	-0.99***	-0.06	0.16
	(106.97)	(1.05)	(0.09)	(0.16)	(0.17)	(0.60)
2023	572.84***	0.19	-0.45**	-0.82***	0.01	0.31
	(124.34)	(1.74)	(0.18)	(0.28)	(0.21)	(0.71)
HH_{t-2}						
2023	512.22***	-2.09	-0.16	-0.05	-0.29*	1.13*
	(127.18)	(1.31)	(0.11)	(0.24)	(0.17)	(0.61)
Mean DV	(63.71)	(0.96)	(0.10)	(0.64)	(0.29)	(0.82)
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	6319	10111	10111	10010	8847	8847
Schools	1264	1264	1264	1255	1264	1264
R^2	0.94	0.98	0.82	0.66	0.84	0.85

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference year is 2019 and the reference racial group is Hispanic. All columns are weighted by student enrollment. Excludes 5% of the schools in the sample that are not treated by HH .

Table A5. Regression Results: HH Cohort, School Resources and Student Outcomes, Elementary Schools

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
HH_t						
2021	394.65***	0.10	-0.43***	-0.86***	0.32	-0.87
	(98.67)	(0.51)	(0.11)	(0.30)	(0.25)	(0.79)
2022	907.69***	-0.24	-0.68***	-0.94***	0.02	-0.18
	(137.80)	(0.63)	(0.12)	(0.17)	(0.15)	(0.69)
2023	767.56***	-0.92	-0.52***	-0.88***	0.37***	-1.47***
	(162.83)	(0.58)	(0.13)	(0.15)	(0.11)	(0.50)
HH_{t-1}						
2022	122.29	-0.26	-0.14	-0.34**	0.29*	-0.89
	(147.32)	(0.62)	(0.12)	(0.17)	(0.15)	(0.70)
2023	579.28**	-1.29	-0.25	-0.31*	-0.04	0.37
	(228.10)	(0.89)	(0.22)	(0.18)	(0.13)	(0.62)
HH_{t-2}						
2023	0.75	-0.44	-0.12	0.02	0.11	-0.03
	(188.80)	(0.72)	(0.17)	(0.17)	(0.13)	(0.64)
Mean DV	18,862.9	50.1	13.2	23.6	92.1	26.6
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	2961	4135	4135	4123	3542	3542
Schools	595	595	595	594	595	595
R^2	0.95	0.97	0.86	0.82	0.86	0.91

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference year is 2019 and the reference racial group is Hispanic. All columns are weighted by student enrollment.

Table A6. Regression Results: HH Cohort, School Resources and Student Outcomes, Middle Schools

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
HH_t						
2021	894.85***	-1.05	-0.60**	-2.07**	-0.00	-0.25
	(135.69)	(1.61)	(0.24)	(0.97)	(0.67)	(2.10)
2022	1248.72***	0.10	-1.13***	-1.54***	-0.12	-0.23
	(222.53)	(0.90)	(0.16)	(0.44)	(0.30)	(1.31)
2023	1125.07***	-1.41	-0.64***	-1.31***	-0.15	1.03
	(247.83)	(0.96)	(0.19)	(0.31)	(0.20)	(0.80)
HH_{t-1}						
2022	445.60**	-0.03	-0.54***	-0.97***	-0.24	0.94
	(215.91)	(1.10)	(0.17)	(0.29)	(0.25)	(0.97)
2023	953.43***	-0.62	-0.62***	-0.79	-0.21	0.85
	(330.18)	(0.95)	(0.23)	(0.50)	(0.28)	(1.10)
HH_{t-2}						
2023	998.84***	-0.43	-0.76***	-0.94***	-0.22	0.69
	(262.39)	(1.27)	(0.21)	(0.32)	(0.21)	(0.90)
Mean DV	16,326.6	66.4	13.5	26.6	92.4	24.1
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	1196	1676	1676	1675	1436	1436
Schools	240	240	240	240	240	240
R^2	0.92	0.98	0.89	0.69	0.75	0.80

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate p<0.10, p<0.05, and p<0.01, respectively. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference year is 2019 and the reference racial group is Hispanic. All columns are weighted by student enrollment.

Table A7. Regression Results: HH Cohort, School Resources and Student Outcomes, High Schools

	(1) EPP	(2) Teachers	(3) PTR	(4) ClassSize	(5) Attend	(6) ChronicAbs
HH_t						
2021	807.76***	-2.84	-0.40	1.84	-1.53*	2.93
	(136.31)	(2.72)	(0.26)	(1.74)	(0.82)	(1.92)
2022	669.90***	0.59	-0.56**	-1.15***	-0.95**	3.21***
	(182.88)	(2.52)	(0.23)	(0.37)	(0.45)	(1.22)
2023	622.64***	-0.86	-0.39*	-0.28	0.49	-1.18
	(143.14)	(2.36)	(0.20)	(0.71)	(0.39)	(1.15)
HH_{t-1}						
2022	829.54***	1.08	-0.65***	-0.49	0.49	-2.12
	(228.74)	(2.07)	(0.18)	(0.33)	(0.52)	(1.56)
2023	716.10***	-0.45	-0.47	-1.85***	-0.04	0.30
	(187.15)	(3.07)	(0.35)	(0.69)	(0.55)	(1.50)
HH_{t-2}						
2023	403.78*	1.98	-0.45*	0.68	0.40	-1.04
	(214.40)	(2.85)	(0.24)	(0.68)	(0.50)	(1.62)
Mean DV	14,019.6	106.5	15.1	27.5	87.8	32.5
Controls	X	X	X	X	X	X
School FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
N	1520	2128	2128	2054	1824	1824
Schools	304	304	304	296	304	304
R^2	0.92	0.99	0.80	0.58	0.81	0.82

Notes: Standard errors are in parentheses and clustered at the school level. Statistical significance is indicated by: *, **, and ***, which indicate $p<0.10$, $p<0.05$, and $p<0.01$, respectively. The period of study is 2016-2023. Sample includes NYC public schools that operate continuously from 2016-2023, excluding those phasing in or out (under reorganization of grades served). Full-time special education, alternative, and charter schools are not included. Reference groups are 2019 and Hispanic. All columns are weighted by student enrollment.