Quantifying the Supply Response of Private Schools to Public Policies†

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School policies that cause a large demand shift between public and private schooling may cause some private schools to enter or exit the market. We study how the policy effects differ under a fixed versus changing market structure in the context of a public school funding reform in New York City. We find evidence of a reduction in private schools in response to the reform. Using a model of demand for and supply of private schooling, we estimate that 20 percent of the reform’s effect on school enrollments came from increased private school exit and reduced private school entry. (JEL H75, I21, I22, I28)

The set of schooling options in the United States has grown substantially over the last few decades (US Department of Education 2014), and many parents consider a range of options, from traditional public schools, to charter schools, to private schools, or even home schooling. For example, in the 2007 National Household Education Survey, 32 percent of parents said that they considered both public and private schools. This suggests that changes to the schooling market could cause demand shifts across these distinct education sectors.

Indeed, private schools are quite different from the typical public school. Private schools are usually independently run and tend to be smaller, with a mean per-grade enrollment of 31 students compared to 113 in public schools. Private schools also choose tuition rates, charging an average of $5,400 for elementary grades, and must attract enough students to cover costs. These forces lead to a more elastic supply of private schools; across several major cities, two-year entry and exit rates average up to 9 percent and 12 percent, respectively. Just as entry and exit can be a primary force behind aggregate outcomes in other industries, the churn of private schools

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may determine the quality of private education offered and cause demand shifts between the public and private sectors. Yet, perhaps due to data limitations, the education literature has paid little attention to the elastic supply of US private schools and its importance for school choice and achievement. In this paper we hope to contribute to a clearer picture of private school entry and exit.

Specifically, we examine the importance of private school entry and exit and its implications for the education market in the context of a large public school policy—the Fair Student Funding (FSF) reform in NYC. This reform provided some public schools with additional funding. We ask whether the supply of private schools was responsive to the public school reform, and if so, how the private sector response affected students’ enrollments and achievement.

We find that the reform affected students’ enrollment decisions, partially through a change in the supply of private schools. For each $1,000 projected increase in per student funding, a public elementary or middle school’s enrollment increased by 32 students. The supply of private schooling was indeed responsive to the public school reform. If a public school received a $1,000 projected funding increase per student, we find that local private school supply fell by 0.2 schools on average in the six years following the reform. This constitutes a 6 percent reduction in private school supply and was concentrated in low value-added schools. We develop and estimate a model that attributes 20 percent of the total public school enrollment effect to increased school exit and reduced school entry. Our findings demonstrate the importance of the private school sector in policy design. Endogenous private school exit, or “crowd out,” can alter students’ choice sets in ways that amplify enrollment shifts and drive changes in achievement.

We start in Section I by providing a conceptual framework that lays out the empirical plan. Section II describes NYC’s FSF reform, which sought to equalize per-student funding at public schools with similar student demographics. Starting in the 2007–2008 school year, the reform implemented a new funding formula that depended only on student characteristics. Overall, about half of the city’s K–12 public schools received funding increases, averaging $454 per student, while the other half saw no change.

This reform offers an attractive setting for analyzing the interaction between public and private schools. The formula change led to considerable variation in how much new funding public schools received. This variation allows us to look for differential effects on students and schools in different neighborhoods. NYC has an active and large private school sector; at the time of the reform, 21 percent of NYC K–8 students were enrolled in 884 private schools. Motivated by the reform’s suddenness and differential treatment across heterogeneous schools, we propose a difference-in-difference analysis of outcomes before and after the reform, using the variation in funding changes across public schools.

In Section III we describe the various datasets we put together. In Section IV we evaluate how the reform affected public school budgets, inputs, value-added, and enrollments. Because the reform was not fully implemented, we estimate that for every dollar in projected funding, schools received between $0.59 and $0.91. The

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2 We will refer to a school’s value-added as the causal effect of attending the school on student test scores.
public schools spent a large fraction of the additional funding on teacher salaries and benefits, and this is reflected in an estimated increase in school value-added of 0.04 math standard deviations for a $1,000 projected increase in funding per student. We then estimate that for a $1,000 projected increase in funding per student, a public school’s enrollment increased by 32 students (14 percent). The effect was concentrated among free and reduced price lunch and Black students.

Turning to the supply response, in Section V we find that the FSF reform caused a change in the supply of private schools. Here we take advantage of the market’s geography and exploit the fact that private schools were affected differentially by the policy depending on the amount of funding their public school neighbors received. We compare the number of private schools within a one-mile radius of public schools that received different funding changes and estimate that for each $1,000 per student projected funding increase, the number of nearby private schools fell by 0.2 (or 6 percent). We find that the decrease in supply is largest among low enrollment and low value-added private schools. At the end of Section V we address the concern that the distribution of public school funding increases may have been correlated with other time-varying factors that could have explained the private school supply change even in the absence of the reform.

As our conceptual framework highlights, our key observation is that some of the reform’s effect on enrollment was driven by changes in the private school sector. If the increased funding of public schools convinces enough private school students to switch to a public school, some incumbent private schools may have to close. These closures in turn cause other students, who would have stayed in the private sector, to switch to public schools. The private school supply response will likely be in the same direction as the initial displacement so that the response amplifies the enrollment effects of the school policy. The total effect of the policy therefore combines the direct enrollment changes from students making new choices from the same menu of schools and the indirect changes from students choosing a new school because their choice sets change. Whether the indirect effect empirically drives much of the total effect depends on the elasticity of the supply of private schools.

The difference-in-difference results imply that the private school supply is responsive to changes in the public schooling sector, but they do not quantify the extent to which the supply response explains the total enrollment increase at public elementary and middle schools with increased funding. We thus develop, in Section VI, a concise model that allows us to estimate counterfactual demand had the market structure not changed. The model captures student choices based on the student’s distance to the school, whether the school is the student’s zoned public school, the school’s change in funding from the reform, a private school preference, and the school’s time-invariant attractiveness. The model’s estimates, presented in Section VII, allow us to separate the direct and indirect effects, as we can estimate the direct effect by predicting student choices in the absence of school openings or closures. We find that the indirect effect explains 20 percent of the total enrollment change.

To assess the welfare impact of the supply response, we introduce a model of private school supply where incumbent schools decide whether to remain open or close based on their enrollments. We estimate that the average private school at risk of closing requires an enrollment of ten students per grade to stay open. We estimate
that the FSF policy increased two-year exit rates by 0.7 percentage points, which lowered the policy’s impact on student welfare by 5 percent.

We also use our model to estimate the value families place on a dollar of public school funding. Following Dynarski, Gruber, and Li (2009), we use Catholic school sibling discounts as an instrument for private school tuition and estimate that families value $1 of public school funding equivalently to $1.05–$1.62 of private school tuition.

This paper relates to several large literatures. The first strand examines interactions between public and private schools and has focused on whether school quality responds to competition (Hoxby 1994; McMillan 2005; Card, Dooley, and Payne 2010; Neilson 2021) and how students sort between the public and private sectors (Hoxby 2003; Epple, Figlio, and Romano 2004). The second strand evaluates school funding reforms and whether spending affects student outcomes. Work on school funding reforms and effects on private school enrollments includes Downes and Schoeman (1998), Hoxby (2001), and Estevan (2015).

There has been less work, however, assessing how the elasticity of private school supply affects evaluation of school choice or funding policies. While a limited literature has characterized private school entry and exit, only a few papers have examined empirically how entry or exit can affect a policy’s outcomes, primarily by comparing municipalities or states with different levels of policy exposure. Hsieh and Urquiola (2006) and Menezes-Filho, Moita, and Andrade (2014) find private school entry in response to Chile’s universal voucher program and Brazil’s Bolsa Familia program expansion, respectively, which led to increased socioeconomic stratification across the public and private school sectors. Similarly, Böhlmark and Lindahl (2015) find private school entry, albeit over a long time span, in response to a large-scale voucher program in Sweden. This entry led to improved local educational outcomes. In a school report card intervention in Pakistan, Andrabí, Das, and Khwaja (2017) find that experimentally treated villages see a decline in enrollment among low-performing private schools driven largely by school exit. In the United States, the evidence has been mixed. Gilraine, Macartney, and McMillan (2018) study a public school class size reduction policy in California and find that a reduction in the number of private schools and a decrease in private school enrollment led to positive achievement spillovers on the public school students. On the other hand, other papers find little evidence of private school entry in response to a small-scale voucher program (Rouse 1998) or crowd out of private schools by the rise of charter schools (Chakrabarti and Roy 2016). Our paper provides evidence on the importance of US private school supply responses and quantifies the impact on a variety of educational inputs and outcomes. We leverage highly local policy variation that allows us to control for community-wide trends that could threaten identification.

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3 For whether school resources matter for student outcomes, see Card and Krueger (1996); Hanushek (1996); Hoxby (2001); Cellini, Ferreira, and Rothstein (2010); Jackson, Johnson, and Persico (2016); and Lafortune, Rothstein, and Schanzenbach (2018).

4 Work on entry includes Downes and Greenstein (1996), Barrow (2006), and Ferreyra (2007) while work on exit includes Pandey, Sjoquist, and Walker (2009). Other work has looked at similar issues for two-year colleges (Cellini 2009).
I. Conceptual Framework and Empirical Strategy

A. Conceptual Framework

In this section, we establish a stylized conceptual framework to define the direct and indirect effects and motivate their importance for policy effects on school enrollments, student welfare, and achievement. We will present a full model, which we take to the data, in Section VI.

Student $i$ chooses between two schools: a public school ($j = 1$) and a private school ($j = 2$). Student $i$ gets utility $u_{i1} + \gamma x$ from attending the public school, where $x$ is additional public school funding, and utility $u_{i2}$ from attending the private school. There is a mass one of students. Define $\Delta u_i \equiv u_{i1} - u_{i2}$ as $i$’s difference in utilities between the public school, in the absence of extra funding, and the private school. Let $F_\Delta(\Delta u)$ be the smooth cumulative distribution function (CDF) of $\Delta u_i$ with derivative $f_\Delta$. Schools do not face capacity constraints nor engage in selective admissions. Students choose the school that gives them the higher utility among the schools that are open.

The private school has fixed characteristics, including price, and a payoff function, $\Pi(Q_2(\gamma x)) - FC_j$, from remaining open. Here, $\Pi(\cdot)$ is some function that is weakly increasing in the private school’s enrollment, $Q_2(\gamma x)$, with output expressed in monetary units. In particular, $\Pi(\cdot)$ could be a variable profit function for profit-maximizing schools with prices exceeding marginal costs; or $\Pi(Q_2) = \lambda Q_2$, $\lambda > 0$, for mission-based schools interested in educating the most students they can. The term $FC_j$ is the fixed operating cost to keeping $j$ open. It is private information for the school and is drawn from a distribution with smooth CDF $G$ and derivative $g$.

The private school closes if it would receive a negative payoff from remaining open. Thus, the probability the private school will close is $G(-\Pi(\gamma x))$, which depends on the public school’s funding through its effect on students’ choices. We consider schools that would remain open absent any change in public funding, so we impose that $G(-\Pi(0)) = 0$.

First, we consider how a change in funding affects school enrollments. Expected enrollment at school 1 is $E Q_1(x) = (1 - G(-\Pi(\gamma x)))(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))$, where the expectation is taken over the fixed cost distribution. For a small change in funding, the change in school 1’s expected enrollment is

$$
\begin{align*}
\frac{dE Q_1(x)}{dx} \bigg|_{x=0} &= \gamma f_\Delta(0) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \bigg|_{x=0} F_\Delta(0).
\end{align*}
$$

The first term, $\gamma f_\Delta(0)$, is the direct effect on the public school’s enrollment from the funding change. This term is weakly positive provided that funding does not make the public school less attractive ($\gamma \geq 0$). The second term, $\partial G(-\Pi(\gamma x)) / \partial x \bigg|_{x=0} F_\Delta(0)$,
captures the probability the private school will close due to the funding change and how the closure affects the public school’s enrollment. We label this term, which is also weakly positive, the indirect effect. This effect, driven by the endogenous change in market structure, thus amplifies the impact of a school’s funding on its enrollment.

Separating the direct and indirect enrollment changes is essential in evaluating student preferences for public school funding. School funding is an important policy lever, and funding differences across school districts have often led to court challenges. Despite the controversy, the degree to which families prefer higher funding is unclear. We find that enrollment is quite responsive to public funding ($dE_{Q1}(x)/dx|_{x=0} > 0$), which seems to indicate that families place a high emphasis on public school funding. But to determine the true value families place on public funding ($\gamma$), we want to control for how students’ options have changed.

The decomposition of the total enrollment change into the direct and indirect changes also informs how we extrapolate to contexts with different private school configurations. The indirect enrollment change derives from the discrete closures of private schools. The larger the indirect effect, the more the policy’s outcome depends on the setting’s market structure. Consider a similar policy proposed in another school district. Even if the students care about school funding as much as NYC students do, we might expect a smaller enrollment response if the district’s private schools are not at risk of closing. The size of the indirect effect thus informs how much the policy’s effect on enrollment depends on the elasticity of private school supply.

The conceptual framework also highlights the different impacts the direct and indirect switchers have on student welfare. Let utilitarian student surplus $S(x)$ be students’ total utility. Then for a small increase in public school funding,

$$\frac{dE[S(x)]}{dx} \bigg|_{x=0} = \gamma (1 - F_\Delta(0)) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \bigg|_{x=0} E[\Delta u | \Delta u < 0] F_\Delta(0).$$

The first term, $\gamma (1 - F_\Delta(0))$, captures the increased utility for the inframarginal students who attend the public school even without a funding increase. The second term, $\frac{\partial G(-\Pi(\gamma x))}{\partial x} \bigg|_{x=0} E[\Delta u | \Delta u < 0] F_\Delta(0)$, captures the loss in utility from the indirect switchers. The expected loss in utility for these students could be large depending on the underlying preference heterogeneity. Finally, note that the direct switchers do not affect welfare. For a small change in funding to induce these students to switch schools, they must have been close to indifferent. Thus, by the envelope theorem, these students do not have a first-order effect on welfare.

The indirect effect can also have a disproportionate impact on student achievement. Suppose that students trade off a school’s effect on their academic achievement ($\theta_{ij}$) with the amount they pay for private education ($p_j$): $u_{i1} = \theta_{i1}$ and $u_{i2} = \theta_{i2} - \alpha p_2$. Let spending $x$ have a constant effect $\beta x$ on achievement. Then the
effect of a small change in the public school’s funding on expected total achievement, \( E[A(x)] \) is

\[
\frac{dE[A(x)]}{dx} \bigg|_{x=0} = \beta(1 - F(0)) - \gamma \alpha p_2 f(0)
\]

\[
+ \frac{\partial G(-\Pi(\gamma x))}{\partial x} \bigg|_{x=0} E[\Delta u - \alpha p_2 |\Delta u < 0]F(0).
\]

In this simple framework, the effect on achievement looks similar to the effect on welfare except now the direct switchers induce a first-order effect because while they are close to indifferent between the schools, they accept a drop in achievement for lower tuition. Indirect switchers incur an even larger achievement loss per student as these students are those who benefit the most from attending the private school.

**B. Empirical Map**

With the conceptual framework to motivate their importance, we devote much of this paper to measuring the direct and indirect effects. We start by examining how funding affects school inputs and outputs. In particular, we use a difference-in-difference framework to estimate the funding’s effect on school value-added and find that math value-added increased \((\beta > 0)\). With evidence that the funding changes altered the experience from attending certain public schools, we test whether the funding changes affected students’ enrollment decisions \((\gamma > 0)\) by estimating the total effect on enrollment, \( dE_{Q1}(x)/dx \). The regression compares how public schools’ enrollments change after the reform’s implementation and whether these changes are related to the size of the funding increase. Unlike the set-up in our conceptual framework, we do not observe a funding change at just one school \((j = 1)\) but rather across many public schools. We therefore measure how the outcomes vary with the size of the funding change. The direct effect then captures students’ sorting to new schools because funding changed at many schools, while keeping students’ choice sets fixed. The indirect effect instead describes the effect of students’ choice sets changing from private school entry and exit.

We demonstrate the potential importance of the indirect effect by showing that the number of private schools is responsive to public school funding. In terms of our conceptual framework, we will estimate \( \partial G(-\Pi(\gamma x))/\partial x \) by comparing how the number of nearby private schools changes depending on how much funding local public schools receive. Our estimates show that \( \partial G(-\Pi(\gamma x))/\partial x > 0 \). We then use a parsimonious model to estimate the direct effect, \( \gamma f(0) \). This allows us to recover the indirect effect as the difference between the estimated total effect and the estimated direct effect. We will estimate the valuation of public school funding relative to private school tuition \((\gamma/\alpha)\). The estimated model also provides the ingredients for welfare calculations, which we will view as suggestive given our lack of data on school capacities.
We focus on the funding’s effect on private school exit and entry. Private schools may make other supply decisions, such as lowering tuition, that could affect students’ choices. Among schools that remain open, the direction of this adjustment is theoretically ambiguous as these schools face increased competition from public schools due to the reform but possibly reduced private competition if neighboring private schools closed. For the schools that close, we can infer that there was no tuition rate that would have attracted enough students to keep the school open. Thus, in our counterfactual analysis, we will predict students’ choices had there been no supply response and schools did not adjust their characteristics.

II. Fair Student Funding Policy

In November 2006, the New York Court of Appeals upheld the Campaign for Fiscal Equity, Inc. v. New York ruling, which called for more equal per-student funding across New York public school districts. NYC, the largest school district in the United States, stood to receive $3.2 billion in new state funding. To determine how the additional money would be spent, NYC passed the FSF reform to fix funding inequities across public schools within NYC. Before the reform, schools that looked very similar in terms of their students’ demographics often received very different amounts of funding per student. The FSF reform changed the funding formula so that most of the school’s instructional budget would be determined by a simple formula that depended on enrollment, the percentage of students “below” and “well below” academic achievement standards, the percentage of English language learners, and the percentage of special education students. In addition to changing the size of a school’s budget, the reform removed most restrictions on how money had to be spent such that principals could exercise more control over spending.

The NYC Department of Education (DOE) cites two reasons that the funding inequities had come to exist before the FSF reform. The first is that, “budgets often carry forward subjective decisions made long ago. Sometimes these decisions were made for legitimate reasons now outdated, sometimes because of politics. Whatever the reason, schools receive different levels of funding for reasons unrelated to the needs of the school’s current students” (NYC DOE 2007). Past policies often included “hold harmless” clauses that meant that while some schools might receive additional benefits, no schools would be penalized by a new policy. As policies were layered upon previous policies, the “hold harmless” clauses meant that the previous policies would continue to affect funding levels for years.

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8 In online Appendix G we do not reject the null hypothesis that surviving schools’ test scores and revenues do not change in response to the reform.
9 We note, however, that the fact that there was no tuition rate that would have kept the school open could be used to bound from above the welfare loss from the school closure. We believe that any changes along the margin of whether a school is open are likely to have larger effects on student choices and outcomes than intensive margin changes of school characteristics. Additionally, in this project the data on other school characteristics are sparse, and we leave exploring supply decisions along other margins for other work (Dinerstein, Neilson, and Otero 2020).
10 The city was also required to provide an additional $2.2 billion. The state funding was to be phased in over four years but the financial crisis led to a freeze in funding for the 2009–2010 school year. In that year NYC received $643 million of additional funding from the state.
11 The FSF allocation accounted for an average of 69 percent of the school’s total allocation. The rest largely comes from city, state, or federal categorical and programmatic allocations.
The second reason relates to how the district accounted for teacher salaries. Prior to the reform, the district would tell each school, based on enrollments and its students’ demographics, how many teachers it could employ. This did not depend on the experience or salaries of the teachers, and the district would compensate a school for the salary differential from hiring more expensive teachers. Each school would then recruit and hire its own teachers. Thus, schools that hired more expensive (experienced) teachers received more money, and because the more experienced teachers tend to prefer schools in wealthier areas, the schools in poorer neighborhoods wound up with smaller budgets. The FSF reform changed this accounting so that a school’s budget would depend only on student characteristics and not increase if the school hired more expensive teachers.

The FSF reform affected school budgets starting in the 2007–2008 school year. The NYC DOE, using the school’s projected enrollment and student demographics, calculated each school’s instructional funding under the old and new (FSF) formulas. If the new formula led to more money than the old formula, then the school was expected eventually to receive the new amount. If the new formula led to less money than the old formula, the school was expected to still receive the old amount via a “hold harmless” clause. Therefore, there were no absolute “losing” schools, just “relative winners” and “relative losers.” The reform was implemented incrementally. In the 2007–2008 school year, “winning” schools received 55 percent of the projected funding increase, up to $400,000, with the expectation that they would get the full increase over the coming years. We provide more details about the reform and its implementation in online Appendix B.

In Figure 1 we graph the size of the projected funding change as a function of the difference in funding between the FSF and old formulas, holding fixed a school’s enrollment and demographics. The “hold harmless” clause truncates all funding changes from below at $0. In online Appendix Figure A.1, we show how two representative schools’ budgets were affected. Online Appendix Figure A.2 shows a sample calculation of how student enrollments and demographics determine the FSF amount.

The funding change interacted with a public school system that gives students increasing amounts of choice as they enter higher grades. Our empirical strategy will test how private schools are affected by the geographically closest public schools. The extent to which students attend schools very close to their homes will determine how concentrated the enrollment effect is and how likely we are to pick it up in our analysis. Because high school students tend to travel farther to school, high schools’ competitors are more dispersed geographically. We therefore will focus our analysis on funding changes at public schools enrolling elementary and middle school students.

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12 The reform changed the funding formula, not just the level, so that it would adjust to smaller or larger enrollments than predicted. Because some of these enrollment changes are endogenous, all empirical analysis will use the funding change with a fixed enrollment and student demographics.

13 The second reason we do not focus on high schools is that between 2002 and 2008 NYC opened more than 150 small high schools (Abdulkadiroglu, Hu, and Pathak 2013). This additional choice set variation is likely to overwhelm the effects from the FSF reform.
A. Empirical Strategy

The reform’s institutional details—its suddenness and varying treatment intensity across heterogeneous public schools—motivate our use of a difference-in-difference regression framework. Specifically, for public school $k$ in year $t$, we will estimate models of the form

\begin{equation}
    y_{kt} = \delta_k + \tau_t + \pi FSF_{kt} + \eta_{kt},
\end{equation}

where $y_{kt}$ is some outcome, $\delta_k$ is public school fixed effects, and $\tau_t$ is year fixed effects. The term $FSF_{kt}$ is the projected increase in funding (in $1,000s per student) from the reform in year $t$. For $t < 2008$, $FSF_{kt} \equiv 0$. Let $FSF_k$ be school $k$’s projected full increase (in $1,000s per student). For $t = 2008$, the planned incremental implementation means $FSF_{k,2008} = \min\{0.55FSF_k, 400/Enroll_{k,2008}\}$. And for $t \geq 2009$, $FSF_{kt} = FSF_k$. In all specifications, we cluster the standard errors by zip code.

Because the projected funding was not always fully implemented, this specification is the reduced form for a model where $FSF_{kt}$ instruments for actual funding per student. We discuss in Section IV A the reasons we estimate the reduced form but
also provide guidance for how the reduced form estimates would scale into instrumental variable (IV) estimates. To avoid confusion, we will refer to the year-specific projected funding change ($FSF_{kt}$) as the “projected funding” change, the post-2008 projected funding change ($FSF̃_{kt}$) as the “projected full funding” change, the actual change to the instructional budget as the “actual FSF budget” change, and the actual change to the full budget as the “actual total budget” change.\footnote{As described in Section IVA, the full budget includes categories separate from the FSF instructional spending. Because these other spending categories were not targeted by the reform, the projected changes in the FSF and full budgets are identical.}

Our identifying assumption will be that $η_{kt}$ is uncorrelated with the projected funding change, conditional on school and year fixed effects. To offer supporting evidence, we will assess pre-trends with event study coefficient graphs estimated from

\begin{equation}
  y_{kt} = δ_k + τ_t + \sum_{T=t_{min}}^{t_{max}} π_TFSF_k + η_{kt},
\end{equation}

where lack of pre-trends would imply $π_T$ is constant for $T < 2008$. The specification we implement will pool adjacent years for statistical power and because the private school data is biennial.

While the event study framework will highlight the time pattern of the estimated effects, it is limited in addressing confounders that are time varying. For instance, the beginning of the Great Recession, which coincided with the reform’s initial implementation, could confound our estimates but would not show up in pre-trends. Thus, we will leverage the reform’s “hold harmless” clause, which divided public schools into those that received more money under the new formula and those that hypothetically would have lost money but whose budgets were held constant. The function translating a school’s potential funding change (the difference in funding between the old and new FSF formulas) into the projected funding change thus had a kink at zero (Figure 1). This kink allows us to separate the effects of the potential funding change, which was a function of school characteristics and other unobservables, from the projected funding change. To the right of the kink, both the funding change and unobservable characteristics could have driven outcomes. But to the left of the kink, only the unobservable characteristics were relevant, as all these public schools received no funding change.

We will use the following specification:

\begin{equation}
  y_{kt} = δ_k + τ_t + π_1FSF_{kt} + π_2HypNegFSF_{kt} + η_{kt},
\end{equation}

where $HypNegFSF_{kt}$ is the negative funding change (in $1,000s per student) the school would have received in the absence of the “hold harmless” clause. If the unobservable characteristics were driving our results, then we would expect to see that the potential funding change affected outcomes both to the right ($π_1 ≠ 0$) and the left ($π_2 ≠ 0$) of the kink. It is unlikely that the unobservables would only matter on one side of the kink, particularly because the kink’s placement was driven by the reform’s aggregate budget. If instead the funding change itself caused changes in outcomes, then we would expect to see that the potential funding change only
mattered to the right of the kink ($\pi_1 \neq 0, \pi_2 = 0$), where the potential change was actually implemented.

The same logic allows us to separate the impact of the reform’s funding changes from the reform’s other changes that may have applied differentially across public schools. As an example, recall that prior to the reform, a teacher’s cost to an individual school was simply the district mean salary, while after the reform, the teacher’s individual salary came out of the school’s budget. Thus, schools with higher salary teachers would have seen a relative price increase from the reform. Consider again the left and right sides of the kink in Figure 1. On the right, schools received additional funding and faced new teacher prices. On the left, schools did not receive additional funding but still faced new teacher prices. As an example, the schools at the far left likely had the most expensive teachers before the reform and went from a budget shadow price of zero for hiring expensive teachers to a shadow price equal to the teacher’s salary. Thus, the effect of variation on the left side of the kink on outcomes reveals the role of the changing prices in driving outcomes. To be clear, even if we find no independent role of changing teacher prices in explaining our results, the estimates of the effects of increased funding are specific to an environment where higher teacher salaries must be paid for from the school’s fixed budget (and principals have considerable autonomy) and may not apply broadly to school districts with different institutions.

III. Data and Descriptive Statistics

A. Traditional Public Schools

To provide a complete picture of traditional public, charter, and private schooling in NYC and how they interact, we bring together data from several sources. For traditional public schools, we use budget data from the NYC DOE to calculate how the FSF reform affected schools’ budgets (NYC DOE 2004–2015, 2006–2017, 2007–2017). These data include the actual budgets and the 2007–2008 hypothetical budget had the FSF reform not happened. The NYC DOE also creates annual School-Based Expenditure Reports (NYC DOE 1999–2015) that document how the schools spend their budgets each school year. We supplement these data with school characteristics from NY State Report Cards (NYSED 2000, 2002–2014) and the Common Core of Data (NCES 2000–2013). These data include enrollments, grade average test scores, measures of student demographics, and measures of teacher experience.

We also make use of student-level data from the NYC DOE (2000-2013). These data allow us to track a student’s school attended, residential census block, and standardized test scores as long as the student attends an NYC public school. We also observe a student’s zoned school where the student has priority for a spot. Students may request to attend schools other than their zoned school provided the school is under capacity. The data do not include students who attend private schools. Despite this limitation, the data allow us to assess the extent to which students are switching schools within the NYC public school system and how the reform affects their achievement.

We present summary statistics for our traditional public school sample in the first column of Table 1. We have over 1,300 public schools in our sample, with a mean
enrollment per grade of 113. The public schools educate a diverse set of students, with Black students constituting 33 percent of enrollment and Hispanic students comprising 40 percent. In online Appendix Table A2, we provide summary statistics for schools in 2006 to characterize how the market looked just before the reform.

The key to our empirical strategy will be that the FSF reform affected NYC public schools differentially. In Figure 2 we graph estimated kernel densities of the size of the projected funding increase for the “winning” schools. The “losing” schools comprised 48.8 percent of the schools and all had $0 projected funding changes. The average “winning” school had a projected funding increase of $454 per student, or about 6 percent of its operating budget. There is a long right tail as 6 percent of “winning” schools saw projected increases of over $1,000 per student.

While the NYC DOE claimed that much of the funding increase went to schools because of past policies that have no relevance to today, the “winning” and “losing” schools still look different along some school characteristics. We investigate these differences in online Appendix Table A1 and find that schools with inexperienced teachers and more students who are limited English proficient and Hispanic were more likely to see funding increases. Despite these differences, neighborhoods at similar income levels often received very different funding changes (online Appendix Figure A.3, as described in online Appendix E).

### B. Charter Schools

Charter schools’ funding levels were not directly affected by the reform. Like private schools, though, they may have been affected in equilibrium. We thus collect data on charter schools from the Common Core of Data and provide summary statistics in the second column of Table 1 (and online Appendix Table A2 for a 2006

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**Table 1—Traditional Public, Charter, and Private School K–8 Summary Statistics**

<table>
<thead>
<tr>
<th>School characteristics</th>
<th>Traditional public schools</th>
<th>Charter schools</th>
<th>Private schools</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Common Core</td>
<td>PSS</td>
<td>NYSED</td>
</tr>
<tr>
<td>Number of schools</td>
<td>1,356</td>
<td>1,002</td>
<td>929</td>
</tr>
<tr>
<td>Catholic (%)</td>
<td>32</td>
<td>37</td>
<td>7</td>
</tr>
<tr>
<td>Other religious (%)</td>
<td>47</td>
<td>44</td>
<td>62</td>
</tr>
<tr>
<td>Nonreligious (%)</td>
<td>21</td>
<td>19</td>
<td>31</td>
</tr>
<tr>
<td>Enrollment per grade (mean)</td>
<td>112.64</td>
<td>30.27</td>
<td>28.68</td>
</tr>
<tr>
<td>With enrollment per grade &lt; 10 (%)</td>
<td>5</td>
<td>18</td>
<td>10</td>
</tr>
<tr>
<td>With enrollment per grade &lt; 20 (%)</td>
<td>6</td>
<td>41</td>
<td>24</td>
</tr>
<tr>
<td>Black (%)</td>
<td>33</td>
<td>17</td>
<td>18</td>
</tr>
<tr>
<td>Hispanic (%)</td>
<td>40</td>
<td>13</td>
<td>7</td>
</tr>
<tr>
<td>Percent schools with &gt; 50% minority</td>
<td>78</td>
<td>41</td>
<td>45</td>
</tr>
</tbody>
</table>

Notes: Traditional public and charter school data come from the Common Core of Data. Private school data come from the Private School Survey (PSS) and New York State Education Department (NYSED) data. For school characteristics, we use the characteristics from the year closest to 2005–2006. The fifth column is our estimation sample and only includes the PSS elementary and middle schools that we can match uniquely to NYSED data on private schools. The last two columns are schools in our estimation sample that enter after 2001–2002 or exit before 2013–2014. Minority students are Black or Hispanic.
The charter sector experienced fast growth during our sample, as just 44 schools were open in 2006 but over 170 schools were operating by the end of our sample. Thus, we expect any potential “crowd-out” to take the form of reduced entry. Charter schools in NYC educate mostly minority students and are fairly large, with an average of 67 students per grade.

C. Private Schools

We also collect data from several sources on private schools so that we can analyze how they make supply decisions in response to the reform. We build a census of schools from the National Center for Education Statistics’ (NCES) PSS. This dataset is published every other year and includes school characteristics such as enrollment, religious affiliation, number of teachers, and location. We infer private school entry and exit based on the first and last times the school appears in the PSS. We use the datasets from the 2001–2002 through 2011–2012 school years.

The PSS has some measurement error, which likely overstates entry and exit. We thus supplement the PSS with data from the NYSED includes enrollments for private schools that have a registration code with the state. Because not all schools have registration codes, this dataset does not capture all schools in the PSS and includes fewer school characteristics. But while the NYSED (2000–2014) data is a smaller sample of more stable schools (those with registration codes), using it allows us to infer entry and exit with considerably more precision. For the difference-in-difference analysis of private school supply, we create our estimation sample by taking the PSS schools and keeping those with a single match in the NYSED data based on name and borough. But for the model of school choice, which relies on specifying the full set of schooling options, we include the rest of the PSS schools. We further investigate the results of this matching exercise in online Appendix C and demonstrate the robustness of our results to stricter and looser matching criteria.
To measure private schools’ effects on student achievement, we use test score data on nonpublic schools from the NYSED (2000–2012). The test data are school-grade-year average test scores on the grades 4–8 math and English language arts (ELA) state tests. Only a handful of states even collect test data from private schools, so this paper uses some of the first test-based evidence of US private school quality on a large fraction of the private school population in a geographic area. We provide more details on data sources in online Appendix C.

Private schooling plays a large role in NYC’s educational landscape, as 21 percent of K–8 students attended private schools at the time of the reform. The private sector, therefore, is large enough such that a change in supply could be economically significant for the public sector. Private schools in NYC are a heterogeneous group, with 37 percent of the schools in our estimation sample offering Catholic instruction and 44 percent affiliated with another religion. Schools also tend to be relatively small, as 10 percent of schools enroll fewer than 10 students per grade and 24 percent enroll fewer than 20. Many of these schools serve minority populations. Over 45 percent of the NYC private schools have Black and Hispanic students make up over half of their enrollment. Table 1 provides summary statistics of the NYC private schools open during our sample period, and online Appendix Table A2 characterizes the schools open just before the reform.

Many private schools also face a high probability of having to close. In Figure 3 we plot the number of NYC entrants and exiters in the PSS and NYSED data every two years. We define entry as the first time a school appears in the data and exit as the last time a school appears. In most years, there are between 75 and 125 PSS entrants and exiters and between 20 and 50 NYSED entrants and exiters. This amount of churn is quite large compared to the almost 700 schools that are active at a given time. The frequency of closure, even before the reform, provides us with the statistical power to test whether private schools near FSF “winners” are more likely to close. In the last two columns of Table 1 we characterize the entrants and exiters from our matched estimation sample. Both types of schools are smaller than average. Catholic schools are underrepresented among entrants and overrepresented among exiters. Exiters are also more likely to educate minority students. In online Appendix Table A4 and online Appendix Figure A.6 we show how sectoral market shares, number of schools, and average school size have changed over time.

IV. Policy’s Effect on Public Schools

A. Effect of Reform on Public School Budgets

We start by estimating the reform’s effect on actual school spending per student, which can be thought of as the first stage of an IV specification. The projected funding increase may differ from the actual funding increase for three reasons. First, the funding formula has a fixed foundational component and thus is nonlinear in the number of students. As students’ school choices react to funding changes, the

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15 The actual numbers are likely between the PSS and NYSED numbers as the PSS overstates churn due to measurement error and the NYSED data understate churn because it misses some of the smaller schools. For the entry and exit patterns in our matched sample, see online Appendix Figure A.5.
funding per student may decrease mechanically. Second, the state budget shortfall meant the reform was not fully implemented. And third, the reform affects only part of a school’s budget, and the other components may be changing as well.

This last reason hints at the difficulty we have in characterizing exactly how the reform affected actual funding per student. Over time, NYC has changed which expenditures it classifies as incurred by a specific school and which expenditures are district wide. Thus, nominal school budgets may increase as more expenditures become classified as school specific, but the actual spending that affects students may be unchanged. In particular, NYC made several accounting alterations starting in the 2006–2007 school year such that budget data from before this year are less comparable to later years.

We thus analyze the impact of the FSF reform on schools’ actual per-student budgets for the school years 2006–2007 through 2013–2014. As mentioned above, we keep only schools that educate elementary or middle schoolers. We also restrict all analysis to a balanced panel of public schools that are open throughout the sample. Using our difference-in-difference specification (equation (4)), we find that for each dollar in projected total funding, schools actually received $0.59 (Table 2). Interestingly, when we add the hypothetical negative funding change the school was held harmless for, we find that it had a fairly precise zero effect on actual budgets. This confirms that we can use the hold harmless specification (equation (6)) to tease

16 Of the schools affected by the reform, 52 (5 percent) closed before 2014. We find no statistical relationship between a school’s funding change and whether it closed.
out effects of the funding change separately from confounders and other aspects of the reform.

Because the categorization of school versus district-wide spending changes over time, we might worry that the budget estimates misstate the actual impact of the funding on students. We therefore include a third specification where we put the actual FSF budget as the dependent variable. This analysis isolates the effect of the reform on the portion of the budget ("FSF") that is targeted for instruction and that is more flexible, as the other funding categories are typically tied to specific uses. While this outcome might more closely represent the actual funding students benefit from, the downside is that the categorization is only stable starting in 2007–2008, the first year of the reform’s implementation. Therefore, identification in this regression comes from schools whose first-year incremental funding increase was lower than its full projected increase. We estimate that for each dollar in projected funding, the portion of schools’ budgets targeted for instruction increased by $0.91 (column 3 of Table 2).

Finally, instead of examining budgetary data, we can analyze how money was spent. In online Appendix E, we use the School-Based Expenditure Reports and find that for each dollar in projected funding, schools spent an additional $0.76. In Section IV.B we decompose the spending changes by category.

Given that we have a range of estimates and that they largely rely on variation only from later years, we will present the reduced form regressions in the following subsections. But to get a sense of how the reduced form estimates might translate into IV estimates, we would scale them by a factor between 1.1 (1/0.91) and 1.7 (1/0.59).

Table 2—Public School Budget Regressions

<table>
<thead>
<tr>
<th></th>
<th>Actual total budget per student</th>
<th>Actual total budget per student</th>
<th>Actual FSF budget per student</th>
</tr>
</thead>
<tbody>
<tr>
<td>Projected total budget ($) per student</td>
<td>0.594 (0.156)</td>
<td>0.561 (0.160)</td>
<td></td>
</tr>
<tr>
<td>Hold harmless ($) per student</td>
<td>0.054 (0.099)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Projected FSF ($) per student (Spring) years</td>
<td>0.905 (0.094)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>7,841</td>
<td>7,841</td>
<td>5,856</td>
</tr>
<tr>
<td>$^2$ R</td>
<td>0.080</td>
<td>0.080</td>
<td>0.717</td>
</tr>
</tbody>
</table>

Notes: Data span the 2006–2007 through 2013–2014 school years and include schools that are open throughout the entire period. The left-hand-side variables are the actual total or FSF budget per student at each school. “FSF” refers to the portion of the budget that is affected directly by the reform. “Projected total budget” is the projected FSF funding plus other category funding. FSF funding is intended to cover expenses related to instruction. The right-hand-side variables are the total or FSF budget per student as determined by the initial FSF formula (i.e., the projected amounts). The “hold harmless” amount was the amount a school would have lost under the FSF formula but for which it was held harmless. FSF variables are only available from 2007–2008 onward. Standard errors are clustered at the zip code level.
B. Changes in School Characteristics

Before assessing how the reform affected enrollments, we examine whether the increased funding changed schools’ characteristics. We start by assessing where the money was spent. Using the School-Based Expenditure Reports to compare expenditures across different categories, we find with our difference-in-difference framework that schools used $0.56 of each marginal dollar on teacher salaries and benefits (online Appendix Table A5, panel A). This represented a shift toward spending money on teachers as just $0.36 of the average dollar was spent on teachers. The FSF dollars were also spent on administrator salaries and other instructional spending, which includes other classroom staff, textbooks, librarians, and classroom supplies, though in proportions similar to pre-FSF spending.

The spending on teachers combined hiring more teachers and employing more expensive (experienced) teachers. In panel B of online Appendix Table A5, we show estimates of the reform’s impact on school characteristics. For each $1,000 in projected spending per student, schools had four more teachers and an increase in mean teacher salary of $830. These teachers had more total experience in the district, and we find suggestive, though not statistically significant, evidence that class sizes fell slightly.

The economics of education literature attributes a large fraction of the variance in student outcomes to teacher effects (see Jackson, Rockoff, and Staiger 2014 for an overview). We therefore might expect that the fact that the FSF spending changed the mix of teachers across schools would lead to changes in a school’s value-added. We first use standard techniques to estimate a school’s value-added, separately for ELA and math (see online Appendix F for details). We then put the school’s estimated value-added in our difference-in-difference framework and report the results in Table 3. We find only small and statistically insignificant effects of FSF projected

<table>
<thead>
<tr>
<th>Sample</th>
<th>Fixed effects</th>
<th>Observations</th>
<th>R²</th>
</tr>
</thead>
<tbody>
<tr>
<td>Hyp neg FSF</td>
<td>Year, school</td>
<td>7,549</td>
<td>0.066</td>
</tr>
<tr>
<td></td>
<td>All students</td>
<td>7,549</td>
<td>0.025</td>
</tr>
<tr>
<td></td>
<td>Stayers</td>
<td>7,504</td>
<td>0.065</td>
</tr>
<tr>
<td></td>
<td>Stayers</td>
<td>7,504</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>Switchers</td>
<td>7,445</td>
<td>0.045</td>
</tr>
<tr>
<td></td>
<td>Switchers</td>
<td>7,445</td>
<td>0.022</td>
</tr>
<tr>
<td></td>
<td>All students</td>
<td>7,549</td>
<td>0.066</td>
</tr>
<tr>
<td></td>
<td>All students</td>
<td>7,549</td>
<td>0.025</td>
</tr>
</tbody>
</table>

Notes: Data span 2000–2001 through 2011–2012 school years. Each column is a separate regression of a school’s estimated value-added (units of standard deviations on a test) in a subject on the school’s projected change in per-student funding ($1,000s) due to the FSF reform. The estimated value-added is the estimated school-subject-grade-year fixed effect in a regression of the student’s test score on cubic functions of her ELA and math test scores in the previous grade, separate dummy variables for gender, Black, Hispanic, English-language learner, special education student, and student eligible for free or reduced price lunch. School-subject-year value-added is the mean value-added across grades. Test scores come from the NY State ELA and math tests in grades 4 and 8 and the NYC tests in grades 3, 5, 6, and 7. “Hyp neg FSF” is the hypothetical negative funding change per student ($1,000s) had the reform not had a “hold harmless” clause. The main regressions include all students. The middle columns estimate a school’s value-added only for students who stayed in the same school post-2007 (columns 3 and 4) or students who switched schools post-2007 (columns 4 and 5). Each regression includes year and school fixed effects. Standard errors are clustered by zip code. Test data come from the NYC DOE.
funding on ELA value-added. But for math, we estimate that $1,000 in projected per-student funding leads to a 0.039 standard deviation increase in value-added. This effect holds for students who did not switch schools post-2007 (0.034 standard deviations) and is strongest for students who switched schools post-2007 (0.074 standard deviations), though we cannot statistically reject equal effects. In the last two columns we repeat our specification test that uses the “hold harmless” clause and find our results do not seem to be driven by omitted factors. We report event study graphs in Figure 4. We find little evidence of pre-trends and find that the effect on math value-added starts right after the reform and seems to increase over time.

If we scale the effect on math value-added by the range of first stage estimates, we get effects on the order of 0.043–0.066 standard deviations. These annual estimates are large and in line with the Tennessee Project STAR (Student/Teacher Achievement Ratio) class size experiment (Krueger 1999, 2003; Krueger and Whitmore 2001) and about one-fourth to one-half as large as the cumulative ten-year effects of state school finance reforms (Lafortune, Rothstein, and Schanzenbach 2018). They also suggest potentially large effects on students’ future incomes. Using the Chetty, Friedman, and Rockoff (2014) estimate that a change in test scores of 0.134 standard deviations increases discounted lifetime earnings by $7,000, we estimate that a $1,000 increase in per-student funding has a financial return of $2,239–$3,453 based on math scores (or $631–$974 based on ELA scores). The math results imply that the increased school funding easily pays for itself in future student earnings. Such high returns to school funding are consistent with Jackson, Johnson, and Persico (2016), who also estimate benefit-cost ratios above two. Like many of the most effective school finance reforms that Jackson, Johnson, and Persico (2016) analyze, the FSF reform targets students from low-income families and has most of the additional funding go toward instructional spending.

Our estimates of the effect of funding on value-added are relative measures and may understate or overstate aggregate effects. The main candidate for underestimating aggregate effects is that the increased spending autonomy, enjoyed by nearly all public schools regardless of funding change, improved achievement. Indeed, NYC
test scores rose considerably relative to the rest of the state, though such a comparison is at best suggestive. The main candidate for overstating aggregate effects is that school quality is determined, at least in part, by a scarce resource such as high value-added teachers. Turning back to online Appendix Table A5, panel B, we see that the treatment effect on teacher experience in the district exceeded the treatment effect on teacher experience at a given school, implying that some of the teacher reallocation may have come from switching schools within district.

C. Enrollment Changes in Public Schools

The increases in school inputs and learning outputs may have shifted students’ preferred schools. We now estimate the reform’s total effect on public school enrollments by comparing how enrollments changed at public schools that received money under the reform (relative “winners”) with public schools that did not (relative “losers”). This differential change in enrollments across public schools combines students choosing one public school over another, students choosing a public school instead of a still-open private school, and students choosing a public school instead of a newly closed (or never-opened) private school. Later we will break down the policy’s total effect into the direct and indirect effects.

We quantify this enrollment effect by running a difference-in-difference regression where we compare enrollments across public schools before and after the reform depending on their change in funding from the reform. Table 4 reports the results. We find that a projected funding increase of $1,000 per student predicts an estimated relative enrollment increase of 32 students (or 13.7 percent). To assess whether this estimate can be interpreted causally, we examine the hold harmless specification. We find that the funding schools would have lost in the absence of the “hold harmless” clause predicts a smaller enrollment increase, though estimates are a bit noisy. We can further examine pre-trends in Figure 5 panel A, where the coefficient on the 2005–2006/2006–2007 school years is normalized to zero. We find that the increase in enrollment comes very quickly after the reform.
is first implemented and that its size is large relative to the slight downward trend we observe between 2002–2003 and 2004–2005.

The enrollment estimates might be noisy if the district responds to new overcrowding by changing students’ zoned, or default, schools. Starting with the 2007–2008 school year, we know each student’s zoned school. We thus construct a new regressor as the change in a school’s number of zoned students relative to 2007–2008, and for prior years we set this equal to zero. We report the results in the last two columns of Table 4 and show the event study graph in Figure 5 panel C. We note that the coefficients are largely unchanged though the FSF coefficient in the main specification now becomes marginally statistically significant as controlling for the number of zoned students reduces the unexplained variance. The event study figure also reveals an increasing effect of the FSF funding over time. Increasing treatment effects are consistent with gradual crowd-out of private schools, which we will explore in Section V.

**D. Heterogeneity in the Enrollment Response**

To understand better the enrollment response, we examine heterogeneity across different student types. We start by classifying students according to free and reduced

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Figure 5. Enrollment Event Studies

Notes: Figure shows estimated coefficients (and 95 percent confidence intervals) on projected FSF funding ($1,000s per student) for each two-year period from the difference-in-difference regression of enrollment (or log enrollment) on projected FSF funding, school fixed effects, and year fixed effects. The 2005–2006/2006–2007 coefficient is normalized to zero. The final panel includes the number of zoned students as a control.
price lunch status, English-language learner status, and ethnicity. We then run our
difference-in-difference specification where the dependent variable is number of
students of a specific classification and present the results in panel A of Table 5.
We find that the enrollment response is concentrated among free and reduced price
lunch students and Black students.

Next, we assess the degree to which the students whose school choices are mar-
ginal with respect to the reform differ from students at those same schools who are
inframarginal. To do this, we place average characteristics as our dependent variable
in the difference-in-difference regression (following Gruber, Levine, and Staiger 1999)
and present the results in panel B. We find that the FSF funding leads to slight
increases in the percent of Black students. This specification also allows us to assess
the impact of FSF funding on the sorting of students by prior academic achieve-
ment. We find that based on prior-year test scores, the FSF funding actually leads
to decreases in the prior-year ELA achievement of a school’s students and a small
but statistically insignificant decrease in math. In panel C, we conduct a similar
analysis but restrict our sample to students who were not in the same school in the
previous year. The FSF coefficient thus measures whether students switching into
FSF schools change in composition once the reform is implemented. We find a clear
pattern of switchers having lower prior achievement scores.

The last result about switchers being negatively selected on past achievement is
interesting in light of the literature on school finance reform (Nechyba 1999, Epple

### Table 5—Heterogeneity in Enrollment Response

<table>
<thead>
<tr>
<th></th>
<th>FRL</th>
<th>ELL</th>
<th>Black</th>
<th>Hispanic</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>enroll</td>
<td>enroll</td>
<td>enroll</td>
<td>enroll</td>
</tr>
<tr>
<td><strong>Panel A. Enrollments</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FSF</td>
<td>15.885</td>
<td>-10.015</td>
<td>24.941</td>
<td>2.004</td>
</tr>
<tr>
<td>(16.807)</td>
<td>(15.281)</td>
<td>(7.158)</td>
<td>(10.982)</td>
<td></td>
</tr>
<tr>
<td>Fixed effects</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
</tr>
<tr>
<td>Observations</td>
<td>7,786</td>
<td>7,786</td>
<td>7,786</td>
<td>7,786</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.088</td>
<td>0.743</td>
<td>0.365</td>
<td>0.006</td>
</tr>
<tr>
<td></td>
<td>FRL</td>
<td>ELL</td>
<td>Black</td>
<td>Hispanic</td>
</tr>
<tr>
<td></td>
<td>percent</td>
<td>percent</td>
<td>percent</td>
<td>percent</td>
</tr>
<tr>
<td><strong>Panel B. Mean characteristics all students</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FSF</td>
<td>0.003</td>
<td>0.004</td>
<td>0.019</td>
<td>0.004</td>
</tr>
<tr>
<td>(0.013)</td>
<td>(0.004)</td>
<td>(0.007)</td>
<td>(0.005)</td>
<td>(0.017)</td>
</tr>
<tr>
<td>Fixed effects</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
</tr>
<tr>
<td>Observations</td>
<td>7,786</td>
<td>7,786</td>
<td>7,786</td>
<td>7,786</td>
</tr>
<tr>
<td>$R^2$</td>
<td>0.105</td>
<td>0.976</td>
<td>0.442</td>
<td>0.050</td>
</tr>
<tr>
<td></td>
<td>FRL</td>
<td>ELL</td>
<td>Black</td>
<td>Hispanic</td>
</tr>
<tr>
<td></td>
<td>percent</td>
<td>percent</td>
<td>percent</td>
<td>percent</td>
</tr>
<tr>
<td><strong>Panel C. Mean characteristics switchers</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FSF</td>
<td>0.011</td>
<td>0.002</td>
<td>0.032</td>
<td>-0.003</td>
</tr>
<tr>
<td>(0.010)</td>
<td>(0.006)</td>
<td>(0.008)</td>
<td>(0.006)</td>
<td>(0.027)</td>
</tr>
<tr>
<td>Fixed effects</td>
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<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
</tr>
<tr>
<td>Observations</td>
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</tr>
<tr>
<td>$R^2$</td>
<td>0.171</td>
<td>0.965</td>
<td>0.470</td>
<td>0.014</td>
</tr>
</tbody>
</table>
| Notes: Data span the 2001–2002 through 2013–2014 school years. “FSF” is the projected per-student funding
change ($1,000s). “FRL” refers to students who received free or reduced price lunch. “ELL” refers to students who
are English-language learners. ELA and math scores have been standardized to have mean zero and standard devi-
ation one for each grade-year among public school students. Switchers are students who were not in the school in
the previous year. Standard errors are clustered by zip code.
and Ferreyra 2008) that finds changes in peer quality as a potential amplifier of the effects of school funding on achievement. Assuming peer effects are increasing in past peer achievement, we find little evidence that the effect of the FSF funding is amplified through peer selection. We offer two important caveats to this analysis. First, we do not have prior test scores for students switching from private schools, and these students could be highly selected on achievement. We thus add two columns to the end of Table 5 showing the effects of the funding on current year test scores, which includes students previously attending private schools. We find more mixed results here, though these test scores combine selection and treatment effects of the funding. Second, students could be selected on dimensions not reflected in test scores.

The switcher analysis also allows us to speculate about students’ counterfactual enrollments in the absence of the reform. In online Appendix Table A7, we run our difference-in-difference specification where the outcome is the count of net switchers into a public school from a specific source. While most increased switching is between traditional public schools, 30 percent of the increased switching comes from private schools or out of the district. This substantial enrollment shift from outside of the public system motivates our analysis of the private and charter sectors.17

V. Policy’s Effect on Private and Charter Schools

A. Private School Supply

The FSF reform appeared to increase the attractiveness of certain public schools. The private schools that were the closest substitutes to the “winning” public schools were likely to lose some students to the public schools on the margin unless the private schools lowered their tuition rates or increased the quality of their instruction. The loss of some students could simply translate to slightly lower enrollments. If a private school, however, had large fixed operating costs and was already close to the break-even point without much room to change tuition, then the loss of a handful of students could have made it so the school could no longer operate without running losses.

To test whether private schools indeed closed in response to the FSF reform, we want to compare private school closure rates across private schools that are and are not close substitutes to public schools that received more money. Ideally we would observe students’ first and second choices and measure the degree of substitutability between schools as the frequency with which they appear among a student’s top two choices. Because we lack such detailed individual-level data, we measure a private school’s level of substitutability with the public school as the distance between the schools. In online Appendix H we provide suggestive evidence that private schools compete with their geographic neighbors.

17 While the analysis of switchers is informative about enrollment flows, switching, or lack thereof, does not necessarily describe students’ counterfactual enrollments in the absence of the reform. Indeed, some nonswitchers may have switched without the reform, and some switchers may have switched to other schools. We fully characterize counterfactual enrollments in Section VII.
Thus, for each public school subject to the FSF reform, we draw a one-mile radius around the school’s location and count the number of private schools in each school year that educate students at the same level of schooling.\footnote{Specifically, we classify schools as educating elementary and/or high school students and match accordingly. We do not require schools to match on whether they educate middle school students because so many private schools are K–8 and thus would not serve as matches for public elementary schools.} The average public school has 3.4 private schools within this geographic area pre-reform. We use this private school count as a dependent variable in a difference-in-difference specification and present the results in Table 6. We find that an increase in projected funding of $1,000 per student reduces the number of nearby private schools by 0.21. Relative to the baseline average number of nearby private schools, this represents a 6 percent reduction. We also benchmark this estimate to the overall decline in private school supply over this period. Relative to the omitted 2014 fixed effect, the 2007 estimated fixed effect is 0.451 (see online Appendix Table A8 for the fixed effect estimates). Thus, with an average FSF increase of $454 per student, the FSF-induced crowd-out accounts for 21 percent \((0.21 \times 0.454/0.451)\) of the overall private school supply reduction.

### B. Threats to Identification

Our identification assumption is that other factors that caused a private school to close or fail to open from 2007–2008 to 2013–2014 were orthogonal to the funding increase at nearby public schools, conditional on public school and year fixed effects. Because the public school “winners” were not a random group, the private schools located near them were likely not a random group. But unless those

<table>
<thead>
<tr>
<th></th>
<th>Number private schools within 1 mile</th>
<th>Number private high schools within 1 mile</th>
<th>Number private non-high schools within 1 mile</th>
<th>Number charter schools within 1 mile</th>
<th>Number charter schools within 1 mile</th>
</tr>
</thead>
<tbody>
<tr>
<td>FSF</td>
<td>-0.208</td>
<td>-0.235</td>
<td>-0.092</td>
<td>-0.072</td>
<td>-0.072</td>
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<tr>
<td></td>
<td>(0.096)</td>
<td>(0.097)</td>
<td>(0.355)</td>
<td>(0.371)</td>
<td>(0.371)</td>
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<tr>
<td>Hyp neg FSF</td>
<td>0.041</td>
<td></td>
<td></td>
<td>-0.025</td>
<td>(0.109)</td>
</tr>
<tr>
<td></td>
<td>(0.046)</td>
<td></td>
<td></td>
<td>(0.097)</td>
<td>(0.179)</td>
</tr>
<tr>
<td>Mismatched FSF</td>
<td></td>
<td>0.011</td>
<td></td>
<td>-0.072</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.179)</td>
<td></td>
<td>(0.179)</td>
<td></td>
</tr>
<tr>
<td>Fixed effects</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
<td>Year, school</td>
</tr>
<tr>
<td>Observations</td>
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<td>11,456</td>
<td>5,442</td>
<td>3,159</td>
<td>3,050</td>
</tr>
<tr>
<td>R²</td>
<td>0.073</td>
<td>0.073</td>
<td>0.048</td>
<td>0.069</td>
<td>0.471</td>
</tr>
</tbody>
</table>

Notes: Data span the 2001–2002 through 2013–2014 school years. An observation is a public school-school year. “FSF” is the projected per-student funding change (in $1,000s). “Hyp neg FSF” is the FSF change in the absence of the “hold harmless” clause. For the “mismatched FSF” regressions, an observation is a public school-school year where the public school is of the opposite level (high school or non-high school) from the private schools counted on the left-hand side. Budget-based regressors are constructed using NYC DOE data on 2007–2008 school budgets. Private school counts are determined by a school’s presence in the PSS and NY State private school registration data. Charter school counts are determined by presence in the Common Core of Data. Standard errors are clustered at the zip code level.
schools were more or less likely to close in this period in the absence of the FSF reform, our identification assumption would hold. We address two types of threats to identification.

The first threat is that certain neighborhoods might have had different counterfactual trends in the absence of the reform. For instance, if certain neighborhoods were declining in some unobservable way that was correlated with the FSF reform’s funding change for that neighborhood’s schools, we might incorrectly attribute the private school closures to the reform. We check for differential pre-existing trends in the event study framework in Figure 6 panel A. We see a statistically significant positive coefficient for the 2001–2003 period but it is small in magnitude relative to the estimated effect, which we see increases in magnitude over time.\(^{19,20}\)

The other main threat to identification would be if events unrelated to the FSF reform but occurring at the same time might have caused the school closures. The

\(^{19}\)The standard errors increase considerably for later years. The last coefficient includes just one school year while others include two. Furthermore, the supply of schools is decreasing more rapidly toward the end of the sample.

\(^{20}\)In online Appendix Figure A.7 we provide event study graphs from stricter criteria for matching between the PSS and NYSED data and find the pre-trend goes away.
most obvious candidate would be the financial crisis. As wealth or job stability fell, families might have removed their children from private schools even without the FSF reform. If the recession differentially affected families living near the public schools that benefited from the FSF reform, then our regression results could be a product of factors unrelated to the FSF reform.

We run two additional placebo tests to assess whether the recession, or other events concurrent with the reform’s timing, threatens our results. We first run the “hold harmless” test and report results in column 2 of Table 6. We find that the potential funding changes were only associated with decreased private school supply for the schools that actually received funding increases. When the “hold harmless” clause determined that the actual funding change would be zero, we find no relationship.

As a second test, we match public schools to private schools within a one-mile radius, but we match private elementary schools to nearby public high schools and vice versa. If the effect were recession specific, then the effect would likely show up regardless of whether the local public school that received money was an elementary or high school. The results in the third and fourth columns of Table 6 show that indeed the treatment to the local public high school did not predict private elementary school supply decreases and the treatment to the local public elementary school did not predict private high school supply decreases. A public school’s projected funding change only affected supply of private schools at the same level. We show the corresponding event study graphs in Figure 6 panels B and C that confirm the regression results. This indicates that differential neighborhood changes, such as vulnerability to the recession, are unlikely to be driving our results.

Thus, a threatening confounder would have to be correlated with the reform’s timing, the spatial variation of how the funding was distributed, and the level of schooling within the neighborhood that received the funding. We consider such confounders unlikely.

C. Charter School Supply

We conduct a similar analysis for charter schools and report the results at the end of Table 6. As noted in Section IIIB, only 44 charter elementary and middle schools were open in 2005–2006. Thus, our estimates of the funding’s effect on charter school supply are very noisy. That said, the point estimate indicates a 0.09 reduction in the number of nearby charter schools for each projected $1,000 in funding per student. We present the “hold harmless” specification in the last column and show the event study graph in Figure 6 panel D.

D. Heterogeneity in the Supply Response

In Table 7, we investigate whether the supply response is driven by certain types of private schools. We find that the supply reduction is stronger in religious schools and similar in schools above and below median in their percentage of minority students. We have a bit more economic intuition for the other sources of heterogeneity we test. Because operating a school likely involves some fixed costs, schools with smaller enrollments may be at higher risk of closing due to a negative demand
shock. We find evidence consistent with this, as schools with below median enrollment are most affected by the public school funding increase.

Finally, we classify private schools based on their ELA and math value-added. As we lack student-level test score data for private schools, we compare schools’ mean eighth grade scores controlling for that cohort’s mean fourth grade scores four years prior. This admittedly makes our value-added estimates rather speculative; we describe how we construct them in online Appendix F. We find a reduced supply of schools below median in ELA or math value-added. This finding, combined with the increased value-added from public school FSF funding, suggests that the reform may have had a large positive impact on aggregate achievement. A complete analysis, however, would require estimating the difference in value-added between the sectors.

E. Discussion

Based on regression results, the FSF reform led to an enrollment increase at schools that received additional funding relative to schools that did not, and nearby private school supply decreased.\(^{21}\) The estimated enrollment effects appear large, even with the evidence that schools spent the money primarily on teachers and

\(^{21}\) In online Appendix H, we provide a series of alternate specifications and robustness checks. In online Appendix Table A16, we show how the standard errors vary with the form of clustering.
increased math value-added, and warrant further discussion. Most of the literature’s estimates of the effect of public school funding on enrollments come from variation across districts. Given the smaller geographic distances, especially in such a dense city as NYC, we might expect enrollments to be more elastic with respect to within-district funding variation.

We benchmark our estimates to those from Hoxby (2001), who estimates the effect of school finance equalization on district per-pupil spending (Table II, column 2) and private enrollment share (Table IV, column 3). Taking the inverse ratio of these coefficients, she finds that the private school share increases by 0.17 (0.361/2.1) for a 1-log point reduction in per-pupil spending. In our context, $1,000 per student translates to roughly a 12 percent increase in an average school’s budget. If we scale Hoxby’s estimate by −0.12, we get a decrease in the private school share of 2.0 percentage points, or 10 percent given the baseline share of 21 percent.

In online Appendix Table A15 we estimate the effect of FSF funding on local private enrollment using the difference-in-difference specification. Our estimates are somewhat noisy but imply an 80-student decrease in nearby private enrollment for every $1,000 in projected spending per student. If we scale this by our estimates from Table 2 and divide by the average nearby private enrollment of 869, we estimate that $1,000 in per student funding leads to a decrease in private enrollment of 10–16 percent, in line with the back of the envelope calculation using Hoxby’s estimates.

These results, however, do not allow us to quantify the impact of private entry and exit on (i) public school enrollments or (ii) student welfare. The total effect on enrollment combines the direct effect where students switch to the public school even if no school opens or closes and the indirect effect from private schools opening and closing. To separate these effects, we need to determine the counterfactual demand for a closing school had it stayed open. Ideally we would find two private schools affected similarly by the reform and otherwise identical except that only one school closed. The education market, however, is complicated as schools’ enrollments depend on a set of differentiated competitors. The exercise thus proves nearly impossible as it requires each school’s competitors to be identical. To account for the complexity of how schools’ enrollments vary with their set of competitors, we therefore turn to a model of school demand.

Second, to this point we have detailed variation in outcomes within NYC. But to assess the aggregate citywide impact of the school funding and the associated crowd-out of private schools on student welfare, we need a model of school demand and supply. A supply model also allows us to characterize schools’ cost structures and the degree to which private schools’ supply decisions change depending on their competitors’ actions.

---

22 A typical change in per-pupil spending in Hoxby’s context from a “stringent” foundation aid school finance equalization scheme is smaller and generates a 1 percentage point (or 9 percent) change in the private school share.

23 Note that the 80-student decrease is larger than the public school’s estimated increased enrollment, likely because some of the private school students displaced by a school closure end up at schools outside the one-mile radius.
VI. Model and Estimation

A. Model

We offer a model that builds on our conceptual framework (Section I) by capturing student choices and school closure decisions. We do not intend to model every feature of the schooling market and we will later discuss how some of our simplifications might affect our results. Rather, we show how a simple estimated model can provide insight into the size of the indirect effect and its effect on welfare.

In the model, students choose a school based on the school’s time-invariant attractiveness, the distance from the student’s residence to the school, whether the school is the student’s zoned public school, the funding change from the reform, and an idiosyncratic preference for private education. Schools compete against each other by trying to attract students and close if demand is below a threshold necessary to cover fixed operating costs.

Specifically, student $i$’s utility from attending private school $j$ for grade $g$ in year $t$ is

$$u_{ijgt} = \delta_{jg} - \gamma_g d_{ij} + \sigma_g \nu_{igt} + \tau_{gt} + \epsilon_{ijgt},$$

where $\delta_{jg}$ is the school-grade’s attractiveness, $d_{ij}$ is the distance from $i$’s residence to $j$, $\nu_{igt} \sim N(0,1)$ is an idiosyncratic preference for private schools, and $\tau_{gt}$ is a year-specific utility from private schools. Student $i$’s utility from attending public school $k$ for grade $g$ in year $t$ is

$$u_{ikgt} = \delta_{kg} - \gamma_g d_{ik} + \rho_g \text{ZONE}D_{ikt} + \lambda_g \text{FSF}_{kt} + \epsilon_{ikgt},$$

where $\text{ZONE}D_{ikt}$ is an indicator variable for whether public school $k$ is $i$’s zoned public school, and $\text{FSF}_{kt}$ is the projected funding increase per student the school received under the FSF reform (units of $1,000$s). The $\text{ZONE}D_{ikt}$ variable accounts for the NYC public school choice system where many younger students are initially assigned to a default (zoned) school. The $\text{FSF}_{kt}$ variable allows a school’s attractiveness to change when it receives additional funding. The term $\epsilon$ is an i.i.d. type I extreme value error. This gives rise to a logit demand system where schools’ expected enrollment shares will depend on the model parameters as well as the schools that are open in that school year.

Over different school years, three elements in the model change systematically:

(i) some schools receive funding from the reform which may affect students’ utilities from attending these schools; (ii) the set of private schooling options changes as schools open and close; and (iii) the private sector becomes more or less attractive. In particular, our within-sector measures of schools’ attractiveness, $\delta$, are fixed across years. This means that our model attributes within-sector enrollment changes over time to changes in competition from entry and exit rather than changing school characteristics, other than the FSF projected funding. This assumption that schools’ non-FSF attractiveness is fixed over time is necessary for identification of the indirect effect, as we must predict a closing school’s attractiveness had it remained open.
On the supply side, an incumbent private school \( j \) makes a single decision each period: whether to stay in the market. Private school \( j \) stays in the market in school year \( t \) if and only if its demand exceeds its costs:

\[
D_{jt}(\text{stay}; X, \beta) > F_{jt}.
\]

Here, \( F_{jt} \) is the number of students necessary to cover fixed operating costs (including the opportunity cost of selling off assets) and is public information. Because many very small schools do not actually close and some very large schools close, we express \( F_{jt} \) such that there is probability \( p_0 \) that the school will not close regardless of demand, probability \( p_{inf} \) that the school will close regardless of demand, and probability \( 1 - p_0 - p_{inf} \) the school must attract enough students to stay open:

\[
F_{jt} = \begin{cases} 
0, & \text{w.p. } p_0; \\
F_{j}^{exp}, & \text{w.p. } 1 - p_0 - p_{inf}; \\
\infty, & \text{w.p. } p_{inf}.
\end{cases}
\]

We parameterize \( F_{j}^{exp} \) as an exponential random variable with its mean depending on the number of total elementary and middle school grades the school serves:

\[
F_{j}^{exp} \sim \text{exponential}(\mu \text{NumGrades}_j).
\]

Our parameters to be estimated are \( p_0 \), the probability the school will stay open regardless of demand; \( p_{inf} \), the probability the school will close regardless of demand; and \( \mu \), the average number of students the schools needs to attract per grade. Schools make the stay-or-close choice sequentially, from the school with the highest demand to the school with the lowest demand.\(^{24}\) We choose this sequence because schools with the highest demand have the most number of families who need to know whether the school will remain open. These schools therefore face the most aggregate pressure to make an early decision.\(^{25}\)

We have made several simplifications in the model. First, schools also enter the market, as observed in the data, but entry will only affect students’ choice sets and is treated as orthogonal to the incumbents’ exit decisions. Second, schools’ only decision is to stay or exit. In particular, schools do not change their academic quality, tuition, or expenditure per student. Third, schools do not face capacity constraints. We discuss some of the model’s simplifying assumptions in online Appendix G.\(^{26}\)

### B. Estimation and Identification

We bring together data on student locations and school enrollments over time to estimate the model. Because we lack complete student-level data that match student

\(^{24}\) To determine the sequence, demand is first calculated assuming all incumbents will stay.

\(^{25}\) Because our counterfactual depends mostly on estimated local closure rates, and not as much on which specific private school closes, the choice of the sequential order does not appear to drive our results.

\(^{26}\) We also abstract away from a school’s attractiveness changing depending on which students choose to attend the school, as could be the case with peer effects or preferences to attend a school with students of similar demographics (Hastings, Kaine, and Staiger 2010; Epple, Jha, and Sieg 2018). Instead, we keep the school’s attractiveness, net of the funding change, fixed. For an analysis of charter school entry and exit with endogenous peer characteristics, see Ferreyra and Kosenok (2018).
locations with school attended, we use 2010 census population counts by age to construct student locations (US Census Bureau 2010). For years other than 2010, we estimate population counts by linearly interpolating and extrapolating the change between the 2000 census and 2010 census (US Census Bureau 2010). We place each student at the geographic centroid of the census block where she lives. We then construct distances from the student’s implied residence to each school in her borough that educates students from her grade. We designate the student’s zoned school as the closest public school that has zoned students. We combine this data with our enrollment data for public, charter, and private schools and our measures of FSF projected funding. We provide further details on the simulation process and demand and supply model estimation in online Appendix D.

We estimate our demand model using data from the 2001–2002, 2003–2004, 2005–2006, 2007–2008, 2009–2010, and 2011–2012 school years to cover student enrollment decisions before and after the reform’s implementation. We use data from every other year because that is the frequency of the PSS. We normalize $\delta_{kg} = 0$ for the public school in each borough with the largest average enrollment in grade $g$. We estimate our supply model using school closure decisions for each model year after 2001–2002.

To estimate the demand parameters, we use an exactly identified simulated method of moments procedure. We list the moments and parameters to be estimated in Table 8. The first set of moments comes from aggregate enrollment data. For each school-grade, we calculate its average observed enrollment share across all six estimation school years. Then because the model holds schools’ attractiveness ($\delta$) fixed across years, our predicted enrollment shares will not necessarily match enrollment shares in a given year. We first add a set of private enrollment shares for each year after 2001–2002. Then, to exploit how the FSF reform affected enrollment shares over time within the public school sector, we add a moment for each grade’s enrollment share for FSF “winners” after the FSF reform was implemented. This moment captures how enrollments systematically shifted toward FSF “winners” starting in 2007–2008. As an additional moment, we use the covariance of the private enrollment share and the private share of schools across borough-years. The second set of moments are constructed from the NYC student-level data. We calculate two additional grade-specific moments: (i) the average distance from a student’s zoned school to her actual school among students opting for a public school; and (ii) the percentage of public school students who attend their zoned school.

We can identify the parameters on time-invariant characteristics using the within-sector student sorting patterns prior to the reform. The extent to which a school’s enrollment differs from the relative number of local school-age children provides variation to identify $\delta$. If school $j$ has many school-age children living nearby but a small enrollment, we would estimate a low $\delta_{jg}$. The private enrollment share moments are then informative about how the attractiveness of private schools ($\tau_g$) changes over time. Our moments derived from the student-level data provide variation to identify $\gamma_g$ and $\rho_g$. The average distance from a student’s zoned school to their actual school among students opting for a public school; and (ii) the percentage of public school students who attend their zoned school.

We still use variation from after the reform to identify these parameters in the model, but the data from before the reform are sufficient. The one exception is if school $j$ was only open after the reform, estimating $\delta_{jg}$ requires data from after the reform.
her actual public school helps identify the disutility from distance, $\gamma_g$. Specifically, we leverage public school students who do not attend their zoned school. The extent to which these students attend nearby alternatives rather than far-away alternatives is informative about $\gamma_g$. Then, the percentage of public school students who attend their zoned school helps us pin down $\rho_g$. For the size of the idiosyncratic preference for private schools, the covariance of private enrollment and school share is informative. If a borough’s private enrollment share is relatively constant over time even as the percentage of its schools that are private falls, we infer that some students have strong private school preferences such that they are likely to attend a private school even if there are fewer schools than usual.

We then exploit how enrollments responded to the reform to identify $\lambda_g$. Once the reform occurred, we observe how many students switched from one public school to another public school that received a larger projected funding increase. These public school switchers did not have either of their most preferred options eliminated, so their sorting pins down the effect of the FSF funding on preferences, $\lambda_g$. Then because we assumed the same $\lambda_g$ for all students, we can apply our estimate to private school students and assess how many would have switched schools even if their private school had not closed. This estimates the direct effect.

Including the private school-year shocks ($\tau$) allows us to absorb trends unrelated to the reform. The private enrollment share drops dramatically at the end of our sample (online Appendix Figure A.6 panel C), and while this could partially reflect the reform’s uniform impact on the public sector—e.g., increased spending flexibility even for public schools whose funding is unchanged—we worry that other factors like income loss from the Great Recession are more likely. We thus include $\tau$ to exploit variation in the size of the funding change across public schools rather than across sectors. This may, however, make our estimates of the standard deviation of the idiosyncratic private school preference ($\sigma$) less precise, as we rely more on

### Table 8—Model Parameters and Moments

<table>
<thead>
<tr>
<th>Panel A. Demand model</th>
<th>Parameters (per grade)</th>
<th>#</th>
<th>Moments (per grade)</th>
<th>#</th>
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</thead>
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<tr>
<td>$\delta$ School-grade utility</td>
<td>$J_g + K_g - 5$</td>
<td>1</td>
<td>School-grade enrollment shares</td>
<td>$J_g + K_g - 5$</td>
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<tr>
<td>$\gamma$ Utility from zoned school</td>
<td>1</td>
<td>Mean distance (miles) from zoned school to public school attended</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>$\rho$ Utility from projected FSF</td>
<td>1</td>
<td>Fraction attending zoned school</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>$\lambda$ Utility from zoned school</td>
<td>1</td>
<td>Change in annual share for FSF winners</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>$\sigma$ SD of private school preference</td>
<td>1</td>
<td>Cov(private enroll, private schools)</td>
<td>1</td>
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</tr>
<tr>
<td>$\tau$ Year-specific utility from private schools</td>
<td>5</td>
<td>Yearly private enrollment share</td>
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</tbody>
</table>

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<tr>
<th>Panel B. Supply model</th>
<th>Parameters</th>
<th>#</th>
</tr>
</thead>
<tbody>
<tr>
<td>$p_0$ Probability cost = 0</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>$p_{inf}$ Probability cost = infinity</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>$\mu$ Mean cost per grade (students)</td>
<td>1</td>
<td></td>
</tr>
</tbody>
</table>

Notes: Demand parameters are estimated with method of simulated moments. Supply parameters are estimated with maximum simulated likelihood.
cross-sectional (borough) choice set variation rather than citywide variation over time. Thus, we estimate versions of our model without $\tau$ and without $\sigma$ and present the results in online Appendix Table A19. The estimates are fairly similar across the specifications.

We estimate the supply model parameters ($\mu$, $p_0$, and $p_{inf}$) using maximum simulated likelihood and the demand estimates. For each year $t$, we fix the set of strategic private schools as those that were open in year $t - 2$. We implement any changes to the public schools exogenously (e.g., increased projected funding) and then solve for an exit equilibrium. We compare the model’s predicted exits to the actual exits.\(^{28}\) For each model iteration we simulate fixed cost draws from the exponential distribution and compare the school’s draw to its predicted enrollment based on the demand model’s estimates and the other set of schools potentially remaining open.\(^{29}\)

The closure rates of schools with very low enrollments per grade help us pin down $p_0$. If the closure rate for these schools is very low, then $p_0$ will be high, as a large percentage of schools must have zero fixed costs in our framework. The closure rates of schools with very high predicted enrollments per grade similarly help us identify $p_{inf}$. Then, $\mu$ governs how quickly the closure rate drops off for schools with larger demand. If the closure rate is fairly flat as a school’s demand increases, then fixed costs must be quite heterogeneous and we will estimate a flatter exponential distribution (larger values of $\mu$).

\section*{VII. Results and Counterfactuals}

We estimate the demand model separately for each grade from kindergarten to eighth grade. We find large effects on utility of distance and whether the public school is the zoned school (see Table 9 for even grades and online Appendix Table A19 for odd grades). For kindergartners, we estimate $\gamma$ at 0.78, $\rho$ at 4.13, $\lambda$ at 0.19, and $\sigma$ at 0.43. The distance and zoned school coefficients decline in magnitude as students become older, which is consistent with older students traveling farther to school. These two sets of coefficients are large relative to the estimates of school attractiveness. For kindergartners, an increase of one mile in distance is equivalent to almost half a standard deviation in the estimated school-grade fixed effects. Similarly, changing a kindergarten student’s zoned school equals almost 2.5 standard deviations.

The coefficient on FSF funding, $\lambda$, is positive for all grades, indicating that students shift their enrollments toward FSF “winners” after the reform. The coefficient on the FSF funding increase indicates that an increase in projected funding of $1,000 per kindergarten student is equivalent to about 11 percent ($0.19/1.74$) of a standard deviation in the estimated school-grade fixed effects. This ratio hits its maximum in grade 4 where a projected funding increase of $1,000 per student is equivalent to

\(^{28}\) If a school appears in the NYSED data, we determine exit based on that data. If not, we infer exit from the school’s last appearance in the PSS.

\(^{29}\) We solve the model sequentially via backward induction, starting with the schools with lowest predicted enrollment in the case where no schools exit. For a given fixed cost draw, either always exiting or always staying is a strictly dominated strategy for some schools (i.e., school $j$’s exit decision does not depend on others’), which allows us to iterate on the elimination of strictly dominated strategies and simplifies the estimation.
33 percent (0.39/1.17) of a standard deviation. The large coefficient implies that the direct effect from the reform is quantitatively important.

Our demand model attributes within-sector changes in a school’s enrollment over time primarily to changes in the market structure from entry or exit. While enrollments might fluctuate for other reasons, we find that our model does well in
predicting enrollment changes. When we regress a school’s actual enrollment in year $t$, conditional on being open, on our model’s predicted enrollment for year $t$ and a set of school and private-year fixed effects, we estimate a coefficient of 0.680 (0.012) (results in online Appendix Table A20). This predictive power is notable because our estimation moments do not target these year-to-year fluctuations. Our model’s reliance on market structure changes to predict enrollment changes thus appears reasonable. We further assess fit by comparing our model predictions of the reform’s total enrollment effect to the data. In Figure 7 we plot the difference in mean enrollment between FSF “winners” and FSF “relative losers” over time. The solid line reflects our model prediction while the dotted line is the data. Our model slightly undershoots the extent to which the “winners” start out larger in 2006, but the model-predicted change once the reform is implemented tracks the data closely.30

To determine the percentage of the total change in enrollment at FSF “winners” relative to “losers” that is due to the direct enrollment effect, we calculate each school’s counterfactual demand postreform had no private schools opened or closed. We then compare this model-predicted counterfactual demand to the model-predicted actual demand, where the funding reform is implemented and private schools opened and closed.31 We present the results in Figure 7. We estimate that 80 percent of the reform’s enrollment increase at “winners” relative to “losers” came from students valuing FSF “winners” higher after the reform. In other words, we estimate that

$$\text{FSF + market structure} - \text{FSF only}$$

$\text{Actual}$

**Figure 7. Direct and Indirect Effect Counterfactuals**

*Notes:* Figure depicts counterfactual predictions from the estimated demand model. The y-axis is the difference in average enrollment between FSF “winners” and FSF “relative losers.” The solid line implements the funding change and the market structure response. The dashed line implements the funding change but keeps the market structure as it was in 2006. The dotted line is estimates from the data.

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30 In online Appendix Figure A.9 we show how the model prediction compares to the difference-in-difference results with a public school’s enrollment as the outcome and the treatment summarized by a dummy variable for being an FSF “winner” after 2006. The model estimates and difference-in-difference estimates line up well.

31 A few public schools also closed during this period. In our model predictions of counterfactual and actual demand, we keep these public schools in students' choice sets.
the direct effect makes up 80 percent (with a standard error of 3.1 percent) of the total effect (or the indirect effect makes up 20 percent). We further break out the predicted student flows by school type in panel A of Table 10. The school closures, and reduced entry, appear to amplify the direct sorting significantly. The magnitude of the indirect effect highlights how important the more elastic segment—the private sector—is to calculating the effects of policies on the public sector. An analysis that did not account for changes in the market structure would have predicted a significantly smaller enrollment jump from the reform.

On the supply side, we estimate that 74 percent of schools will remain open regardless of demand, while 9 percent will close regardless of demand (Table 9). We estimate that the average school requires 9.8 students per grade to stay open. All estimates are fairly precise. The estimated model predicts that closures depend on a school’s enrollment were it to stay open. While counterfactual enrollment is

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32 Table 10 estimates flows that compare 2012 to 2006. In online Appendix Table A21 we show the flows comparing 2006 to 2008 and 2010.
not observed in the data, we can assess our model fit by comparing predicted versus actual closure rates for schools based on their enrollments two years prior. We plot these distributions in online Appendix Figure A.10 and find that the model fits the data well.

We can also characterize the degree to which schools’ supply decisions are interrelated. We estimate that 7 percent of schools’ decisions whether to exit depend on other schools’ decisions—i.e., for 93 percent of schools, either closing or staying open is a dominant strategy while for the other 7 percent of schools, the decision depends on what competing schools do. Within these interrelated decisions, many are determined through iteration on dominated strategies. For an average of just two schools, the specified order of action determines which schools close.

We use our supply estimates to estimate the impact of the reform on two-year private school closure rates. As private schools likely compete with multiple public schools, we cannot easily assign them to “treatment” and “control” groups as we do for public schools. Thus, we need to take a stand on whether the change in the private sector’s large drop in attractiveness in 2011–2012 ($\hat{\tau}_5$ ranges from $-0.33$ to $-0.17$) is related to the reform. In our baseline model we assume the reform only affects private school attractiveness through $\gamma$, and thus ignore the reform’s impact on the public sector as a whole through increased spending flexibility and changing teacher prices.\footnote{We explore how the predicted private school closure rate changes by varying the reform’s impact on private school attractiveness in online Appendix Figure A.11.} Using our supply estimates, we predict two-year closure rates in the absence of the FSF increased spending of 7.01 percent, 6.02 percent, and 6.81 percent in 2008, 2010, and 2012, respectively. With the reform, we predict two-year closure rates of 7.43 percent, 7.01 percent, and 7.57 percent in 2008, 2010, and 2012, respectively (Table 10). We attribute the difference (an average of 0.7 percentage points) to the increased spending. We note that this is likely a considerable underestimate of the total supply response because it does not include effects on entry and is estimated using closures through only the 2011–2012 school year.\footnote{We further predict closure rates for different levels of funding increases. We classify hypothetical policies as multiples of the actual FSF policy and plot the predicted closure rates in online Appendix Figure A.12. We estimate exit rates that are close to linear in the size of the funding increase.}

A. Estimating the Value to Public School Funding

We also use our model to estimate the value families place on a dollar of public school funding, a potentially important policy parameter. Many analyses of public education markets are unable to place a monetary value on school characteristics because all public schools are free. In our setting, where we have both public and private options, we could in principle use families’ valuations of private school tuition rates to value a dollar of public school spending. Specifically, we can decompose our estimated private school-grade fixed effects into tuition and nontuition components:

\begin{equation}
\hat{\delta}_{jg} = \alpha_{0g} + \alpha_1 \text{AvgTuition}_{jg} + \omega_{jg}.
\end{equation}

\footnote{We explore how the predicted private school closure rate changes by varying the reform’s impact on private school attractiveness in online Appendix Figure A.11.}

\footnote{We further predict closure rates for different levels of funding increases. We classify hypothetical policies as multiples of the actual FSF policy and plot the predicted closure rates in online Appendix Figure A.12. We estimate exit rates that are close to linear in the size of the funding increase.}
The challenge in estimating $\alpha_1$ is that tuition is likely endogenous. We therefore turn to the observation by Dynarski, Gruber, and Li (2009) that many Catholic private schools offer sibling discounts to families that send multiple children to the school. Dynarski, Gruber, and Li (2009) argue that, unlike the single-child price, these discounts are set randomly and can be used as an instrument for tuition. We thus collected middle school tuition and discount data for the Catholic private schools that are still open (Dinerstein and Smith 2013-2014). The data collection is described in online Appendix C.

We note that $\text{AvgTuition}_{jg} = 1\text{ChildPrice}_{jg} - \text{AvgDiscount}_{jg}$, where $\text{AvgDiscount}_{jg}$ will be our instrument. To construct $\text{AvgDiscount}_{jg}$, we match each K–8 school to the closest census block group and use 2010 data on family size to estimate the fraction of students who are in school with zero siblings, one sibling, two siblings, or three siblings. We then use the price schedule for families of different sizes, interacted with the estimated distribution of local family size, to construct the average discount:

\[
\text{AvgDiscount}_{jg} = 1\text{ChildPrice}_{jg} \left( 1 - \text{FracFam}\hat{1}_{jg} \right) - 2\text{ChildPrice}_{jg}\text{FracFam}\hat{2}_{jg} - 3\text{ChildPrice}_{jg}\text{FracFam}\hat{3}_{jg} - 4\text{ChildPrice}_{jg}\text{FracFam}\hat{4}_{jg},
\]

where $N\text{ChildPrice}_{jg}$ is the average tuition rate for a family with $N$ children at the school and $\text{FracFam}\hat{N}_{jg}$ is the estimated fraction of students attending school with $N - 1$ siblings.\footnote{We get very similar results when we estimate $\text{FracFam}\hat{N}_{jg}$ using citywide demographics.}

Our identifying assumption is that the average discount at a school, which depends on the price schedule and local demographics, is orthogonal to other determinants of the attractiveness of the school. This is more restrictive than the Dynarski, Gruber, and Li (2009) condition that the discount is orthogonal to determinants of a school’s attractiveness that differentially appeal to larger families. We acknowledge that this assumption is strong but are comforted by the first stage results, shown in Table 11. Our first stage regresses $\text{AvgTuition}_{jg} = 1\text{ChildPrice}_{jg} - \text{AvgDiscount}_{jg}$ on $\text{AvgDiscount}_{jg}$ and grade fixed effects and yields a precisely estimated coefficient of $-1$. This indicates that the average discount is uncorrelated with the one child price. To the extent we worry about school unobservables that correlate with the one child price, these do not seem to correlate with the discount.

Using our IV estimate of the value of a dollar of tuition in utils, we can compare it to our $\lambda_g$ estimates from our demand model. Because the coefficient on private school tuition is slightly larger in magnitude than the average estimated $\lambda_g$, we estimate that families value $1$ of additional projected public school spending equivalently to $0.96$ of private school tuition. But if we scale this estimate due to the partial implementation of the FSF reform, we estimate that families value $1$ of additional public school spending equivalently to $1.05–1.62$ of private school tuition. This range indicates that families would value increased public spending
more than a reduction in private school tuition, a result reminiscent of Deming and Walters’ (2017) findings on the effectiveness of spending versus tuition reduction in postsecondary education.36

We can also consider what size lump sum transfer, which is not conditional on attending a certain type of school, would generate the same increase in student welfare as the FSF reform. We estimate that a transfer of 74–112 percent of the cost of the FSF reform would have generated an equivalent increase in student welfare, where the range comes from the partial implementation.37 Highlighting the role of the private school supply response, the welfare-equivalent transfer would have been 77–118 percent in the absence of private school exit, an increase of 5 percent.38

VIII. Conclusion

The FSF reform provided additional funding to certain public schools. More generally, it took an existing equilibrium and changed the characteristics of certain schools. Based on simple economic theory, even agents not targeted by the reform may react to the market’s changes. We thus need to consider the interactions between private schools and changes to the public sector. In particular, action along the extensive margin of whether to stay open can lead to a very different equilibrium.

Our empirical analysis indicates that private schooling supply was responsive to a public school funding reform. We estimate that the supply of private schools within a mile of a public school that received a projected funding increase of $1,000 per student fell by 0.2 schools. Using our model estimates, we find that this change in

36 The valuation of public school spending is specific to the context and rules that governed how principals could use the funding. For instance, the valuation might be lower in a context with less spending flexibility.
37 We first sum the welfare change in panel B of Table 10 across years. We then sum the projected spending change across years and multiply by the range of first stage coefficients from Section IVA. We then divide the welfare sum by the range of actual spending. For details on the welfare and counterfactual calculations, see online Appendix D.
38 In addition to the strong identifying assumption, we note two other limitations. First, the students for whom a tuition increase at the local Catholic school affects their choice (and thus identify α1) may not be the same students who switch in response to public school funding changes (and thus identify λg). Our model assumes homogeneous coefficients, but to the extent this is an inaccurate representation of preferences, our coefficient estimates are possibly driven by different sets of students. Second, the fact that tuition at certain schools varies with family size is inconsistent with our ε errors in our demand specification being i.i.d. Because we lack microdata on schooling choices of students from families of different sizes, we leave this for future work.
supply of private schooling explained 20 percent of the enrollment increase that the public school “winners” enjoyed.

Our results have important policy implications as they show that the private sector is likely to adjust to schooling policies. For example, Tennessee has considered approving the third largest voucher program in the nation, but there is concern that there are too few existing private schools to accommodate the potential demand shift toward private schooling. While we have focused on how policy can decrease the supply of private schools, our estimates of considerable supply side elasticity suggest that the private sector may be responsive enough to fill the shortage.

School entry and exit are likely to continue shaping education markets in the next decade. The growth of the charter school sector has increased the number of independently run schools whose viability depends on the number of students they can attract. As the sector has matured, the charter school exit rate has increased. Even traditional public school exit has become more common, as several large cities with declining populations have started closing public schools. Students’ menu of schooling options are likely to continue changing with the increased churn of schools.

REFERENCES


40 Schools up for charter renewal closed at a 12.9 percent rate in 2012 compared to 6.2 percent in 2011. The closure rate from schools not up for renewal increased from 1.5 percent in 2011 to 2.5 percent in 2012 (National Association of Charter School Authorizers 2012).


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