

QUANTIFYING THE SUPPLY RESPONSE OF PRIVATE SCHOOLS TO PUBLIC POLICIES

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Abstract

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% of the reform's effect on school enrollments came from increased private school exit and reduced private school entry.

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1 Introduction

The set of schooling options in the United States has grown substantially over the few decades decade (U.S. 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% 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% and 12%, respectively.¹ Just as entry and exit can be a primary force behind aggregate outcomes in other industries, the churn of private schools 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 U.S. 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 New York City (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% reduction in private school supply and was concentrated in low value-added

¹Calculations use the 1999-2000 through 2009-10 editions of the NCES Private School Survey. The major cities are New York City, Chicago, Boston, and Philadelphia.

schools. We develop and estimate a model that attributes 20% 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 2 by providing a conceptual framework that lays out the empirical plan. Section 3 describes NYC’s FSF reform, which sought to equalize per-student funding at public schools with similar student demographics. Starting in the 2007-08 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% 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 4 we describe the various data sets we put together. In Section 5 we evaluate how the reform affected public school budgets, inputs, value-added, and enrollments.² 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 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%). The effect was concentrated among free and reduced price lunch and black students.

Turning to the supply response, in Section 6 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 1-mile radius of public schools that received different funding changes and

²We will refer to a school’s value-added as the causal effect of attending the school on student test scores.

estimate that for each \$1,000 per student projected funding increase, the number of nearby private schools fell by 0.2 (or 6%). We find that the decrease in supply is largest among low enrollment and low value-added private schools. At the end of Section 5 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 7, 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 8, 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% 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 11 students per grade to stay open. We estimate that the FSF policy increased two-year exit rates by 0.8 percentage points, which lowered the policy's impact on student welfare by 5%.

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 \$1.06 – \$1.63 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 (e.g., Hoxby 1994, McMillan 2005, Card, Dooley and Payne 2010, Neilson 2013) and how students sort between the public and private sectors (e.g., 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).³

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,⁴ 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 de Carvalho 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, Andrabi, Das and Khwaja (2014) find that experimentally treated villages see a decline in enrollment among low-performing private schools driven largely by school exit. In the U.S., 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

³For 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 (2015), and Lafortune, Rothstein and Schanzenbach (2018).

⁴Work 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 (e.g., Cellini 2009).

the rise of charter schools (Chakrabarti and Roy 2016). Our paper provides evidence on the importance of U.S. 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.

2 Conceptual Framework and Empirical Strategy

2.1 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 7.

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 1 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 CDF of Δu_i with derivative f_Δ . Schools do not face capacity constraints nor engage in selective admissions.⁵ 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. $\Pi(\cdot)$ 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. 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

⁵Throughout the paper we abstract away from school capacity constraints and admissions. We therefore will use enrollment changes to measure changes in demand. In Online Appendix G we discuss how binding this assumption is and how it might affect our results.

at school 1 is $\mathbf{E}Q_1(x) = (1 - G(-\Pi(\gamma x)))(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))$, 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:⁶

$$\underbrace{\frac{d\mathbf{E}Q_1(x)}{dx}\Big|_{x=0}}_{\text{Total Effect}} = \underbrace{\gamma f_\Delta(0)}_{\text{Direct Effect}} + \underbrace{\frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} F_\Delta(0)}_{\text{Indirect Effect}}$$

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, $\frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} F_\Delta(0)$, 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.⁷ 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 inequalities across school districts have often led to court challenges. Despite the controversy, it is unclear the degree to which families prefer higher funding. We find that enrollment is quite responsive to public funding ($\frac{d\mathbf{E}Q_1(x)}{dx}\Big|_{x=0} > 0$), 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 (γ), 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

⁶For the derivation of this result and others, see Online Appendix A.

⁷ $\frac{\partial G(-\Pi(\gamma x))}{\partial x} = -g(-\Pi(\gamma x))\frac{\partial \Pi}{\partial x}$ where $\frac{\partial \Pi}{\partial x} = \frac{\partial \Pi}{\partial Q_2} \frac{\partial Q_2}{\partial x}$, with $\frac{\partial \Pi}{\partial Q_2} \geq 0$ and $\frac{\partial Q_2}{\partial x} \leq 0$.

utility. Then for a small increase in public school funding:

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

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, $\frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} \mathbf{E}[\Delta u|\Delta u < 0] F_\Delta(0)$, 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 (θ_{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 βx on achievement. Then the effect of a small change in the public school's funding on expected total achievement, $\mathbf{E}[A(x)]$ is:

$$\frac{d\mathbf{E}[A(x)]}{dx}\Big|_{x=0} = \beta(1 - F_\Delta(0)) - \gamma\alpha p_2 f_\Delta(0) + \frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} \mathbf{E}[\Delta u - \alpha p_2|\Delta u < 0] F_\Delta(0)$$

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.

2.2 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, $\frac{d\mathbf{E}Q_1(x)}{dx}$. 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 $\frac{\partial G(-\Pi(\gamma x))}{\partial x}$ by comparing how the number of nearby private schools changes depending on how much funding local public schools receive. Our estimates show that $\frac{\partial G(-\Pi(\gamma x))}{\partial x} > 0$. We then use a parsimonious model to estimate the direct effect, $\gamma f_\Delta(0)$. 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 (γ/α). 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.⁸ 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.⁹

⁸In 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.

⁹We 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).

3 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. New York City (NYC), the largest school district in the U.S., stood to receive \$3.2 billion in new state funding.¹⁰ To determine how the additional money would be spent, NYC passed the Fair Student Funding (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.¹¹ 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." 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.

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

¹⁰The 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 2009-10 school year. In that year NYC received \$643 million of additional funding from the state.

¹¹The FSF allocation accounted for an average of 69% of the school's total allocation. The rest largely comes from city, state, or federal categorical and programmatic allocations.

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 2007-08 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.¹² 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 2007-08 school year, “winning” schools received 55% 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.¹³

¹²The 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.

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

3.1 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:

$$y_{kt} = \delta_k + \tau_t + \pi FSF_{kt} + \eta_{kt} \quad (1)$$

where y_{kt} is some outcome, δ_k are public school fixed effects, and τ_t are year fixed effects. FSF_{kt} is the projected increase in funding (in \$1,000s/student) from the reform in year t . For $t < 2008$, $FSF_{kt} \equiv 0$. Let $F\tilde{S}F_k$ be school k ’s projected full increase (in \$1,000s/student). For $t = 2008$, the planned incremental implementation means $FSF_{k,2008} = \min\{0.55F\tilde{S}F_k, \frac{400}{Enroll_{k,2008}}\}$. And for $t \geq 2009$, $FSF_{kt} = F\tilde{S}F_k$. 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 5.1 the data reasons we estimate the reduced form but also provide guidance for how the reduced form estimates would scale into 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 ($F\tilde{S}F_k$) 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.¹⁴

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:

$$y_{kt} = \delta_k + \tau_t + \sum_{T=t_{min}}^{t_{max}} \pi_T F\tilde{S}F_k 1\{t = T\} + \eta_{kt} \quad (2)$$

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,

¹⁴As described in Section 5.1, 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.

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 0 (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:

$$y_{kt} = \delta_k + \tau_t + \pi_1 FSF_{kt} + \pi_2 HypNegFSF_{kt} + \eta_{kt} \quad (3)$$

where $HypNegFSF_{kt}$ is the negative funding change (in \$1,000s/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 ($\pi_1 \neq 0$) and the left ($\pi_2 \neq 0$) 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 0 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.

4 Data and Descriptive Statistics

4.1 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. These data include the actual budgets and the 2007-08 hypothetical budget had the FSF reform not happened. The NYC DOE also creates annual School-Based Expenditure Reports that document how the schools spend their budgets each school year. We supplement these data with school characteristics from NY State Report Cards and the Common Core of Data. 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. These data allow us to track a student's school attended, residential Census block, and standardized test scores as long as the student attends a 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 diversity of students, with black students constituting 33% of enrollment and Hispanic students comprising 40%. 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% of the schools and all had \$0 projected funding changes. The average “winning” school had a projected funding increase of \$454/student, or about 6% of its operating budget. There is a large right tail as 6% of “winning” schools saw projected increases of over \$1,000/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).

4.2 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 snapshot). 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.

4.3 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’s (NCES) Private School Survey (PSS). This data set 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 Private School Survey. We use the data sets from the 2001-02 through 2011-12 school years.

The PSS has some measurement error, which likely overstates entry and exit. We thus

supplement the PSS with data from the New York State Education Department (NYSED) that includes enrollments for private schools that have a registration code with the state. Because not all schools have registration codes, this data set does not capture all schools in the PSS and includes fewer school characteristics. But while the NYSED 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. The test data are school-grade-year average test scores on the grade 4-8 math and 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 U.S. 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 New York City's educational landscape, as 21% of K-12 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 38% of the schools in our estimation sample offering Catholic instruction and 43% affiliated with another religion. Schools also tend to be relatively small, as 10% of schools enroll fewer than 10 students per grade and 24% enroll fewer than 20. Many of these schools serve minority populations. Over 45% 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.¹⁵ 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.

5 Policy’s Effect on Public Schools

5.1 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 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 2006-07 school year such that budget data from before this year is less comparable to later years.

We thus analyze the impact of the FSF reform on schools’ actual per-student budgets for the school years 2006-07 through 2013-14. As mentioned above, we keep only schools that educate elementary or middle schoolers. We also restrict all analysis to a balanced panel

¹⁵The actual numbers are likely between the PSS and NYSED numbers as the PSS overstates churn due to measurement error and the NYSED data understates 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.

of public schools that are open throughout the sample.¹⁶ Using our difference-in-difference specification (Equation 1), 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 3) to tease 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 (“Fair Student Funding”) 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 2007-08, 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 5.2 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).

5.2 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 cat-

¹⁶Of the schools affected by the reform, 52 (5%) closed before 2014. We find no statistical relationship between a school’s funding change and whether it closed.

egories, 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 4 more teachers and an increase in mean teacher salary of \$837. 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 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 class size experiment (Krueger 1999, Krueger 2003, Krueger and Whitmore 2001) and about one-fourth to one-half as large as the cumulative 10-year effects of state School Finance Reforms (Lafontaine et al. 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 et al. (2015), who also estimate benefit-cost ratios above 2. Like many of the most effective school finance reforms that Jackson et al. (2015) 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 understating 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.

5.3 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 13.8%). 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 5a, where the coefficient on the 2005-06 and 2006-07 school years is normalized to 0. 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 2002-03 and 2004-05.

The enrollment estimates might be noisy if the district responds to new overcrowding by changing students' zoned, or default, schools. Starting with the 2007-08 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 2007-08, and for prior years we set this equal to 0. We report the results in the last two columns of Table 4 and show the event study graph in Figure 5c. 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 6.

5.4 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 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 marginal 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 students who are black. This specification also allows us to assess the impact of FSF funding on the sorting of students by prior academic achievement. 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 similar patterns on achievement as we did in Panel B.

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

6 Policy's Effect on Private and Charter Schools

6.1 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

¹⁷While 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 non-switchers may have switched without the reform, and some switchers may have switched to other schools. We fully characterize counterfactual enrollments in Section 8.

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 Appendix H we provide suggestive evidence that private schools compete with their geographic neighbors.

Thus, for each public school subject to the FSF reform, we draw a 1-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.¹⁸ 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 0.20. Relative to the baseline average number of nearby private schools, this represents a 6% 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 0.463 (see Online Appendix Table A8 for the fixed effect estimates). Thus, with an average FSF increase of \$454/student, the FSF-induced crowd out accounts for 20% ($0.20 * .454/.463$) of the overall private school supply reduction.

6.2 Threats to Identification

Our identification assumption is that other factors that caused a private school to close or fail to open from 2007-08 to 2013-14 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 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

¹⁸Specifically, 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.

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 preexisting trends in the event study framework in Figure 6a. We see a statistically significant positive coefficient for the 2001-03 period but it is small in magnitude relative to the estimated effect, which we see grows 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 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 0, we find no relationship.

As a second test, we match public schools to private schools within a 1-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 Figures 6b and 6c that confirm the regression results. This indicates that differential neighborhood changes, such as vulnerability to

¹⁹The 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.

²⁰In 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.

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.

6.3 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 4.2, only 44 charter elementary and middle schools were open in 2005-06. 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 6d.

6.4 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 8th grade scores controlling for that cohort’s mean 4th 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.

6.5 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 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% 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% given the baseline share of 21%.²²

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.²³ 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%, 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 (1) public school enrollments or (2) 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.

²¹In 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.

²²A 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%) change in the private school share.

²³Note 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 1-mile radius.

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 city-wide 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.

7 Model and Estimation

7.1 Model

We offer a model that builds on our conceptual framework (Section 2) 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:

$$u_{ijgt} = \delta_{jg} - \gamma_g d_{ij} + \sigma_g \nu_{igt} + \tau_{gt} + \epsilon_{ijgt} \quad (4)$$

where δ_{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 τ_{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:

$$u_{ikgt} = \delta_{kg} - \gamma_g d_{ik} + \rho_g ZONED_{ikt} + \lambda_g FSF_{kt} + \epsilon_{ikgt} \quad (5)$$

where $ZONED_{ikt}$ is an indicator variable for whether public school k is i 's zoned public school, and FSF_{kt} is the projected funding increase per student the school received under the FSF reform (units of \$1,000s). The $ZONED_{ikt}$ variable accounts for the NYC public school choice system where many younger students are initially assigned to a default (zoned) school. The FSF_{kt} variable allows a school's attractiveness to change when it receives additional funding. ϵ 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: (1) some schools receive funding from the reform which may affect students' utilities from attending these schools; (2) the set of private schooling options changes as schools open and close; and (3) the private sector becomes more or less attractive. In particular, our within-sector measures of schools' attractiveness, δ , 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 are 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:

$$D_{jt}(stay; X, \beta) > F_{jt}. \quad (6)$$

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:

$$F_{jt} = \begin{cases} 0 & w.p. \ p_0 \\ F_j^{exp} & w.p. \ 1 - p_0^{relig} - p_{inf} \\ \infty & w.p. \ p_{inf} \end{cases} \quad (7)$$

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:

$$F_j^{exp} \sim \text{exponential}(\mu \text{NumGrades}_j). \quad (8)$$

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 μ , 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

7.2 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 matches student locations

²⁴To determine the sequence, demand is first calculated assuming all incumbents will stay.

²⁵Because 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.

²⁶We 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 (e.g., Hastings, Kane 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).

with school attended, we use 2010 Census population counts by age to construct student locations. For years other than 2010, we estimate population counts by linearly interpolating and extrapolating the change between the 2000 Census and 2010 Census. 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 2001-02, 2003-04, 2005-06, 2007-08, 2009-10, and 2011-12 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 2001-02.

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 (δ) 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 2001-02. 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 2007-08. 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: (1) the average distance from a student’s zoned school to her actual school among students opting for a public school; and (2) 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.²⁷ The extent to which a school’s enrollment

²⁷We still use variation from after the reform to identify these parameters in the model, but the data from

differs from the relative number of local school-aged children provides variation to identify δ . If school j has many school-aged children living nearby but a small enrollment, we would estimate a low δ_{jg} . The private enrollment share moments are then informative about how the attractiveness of private schools (τ_g) changes over time. Our moments derived from the student-level data provide variation to identify γ_g and ρ_g . The average distance from a student's zoned school to her actual public school helps identify the disutility from distance, γ_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 γ_g . Then, the percentage of public school students who attend their zoned school helps us pin down ρ_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 λ_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, λ_g . Then because we assumed the same λ_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 (τ) 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.6c), 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 τ 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 (σ) less precise, as we rely more on cross-sectional (borough) choice set variation rather than city-wide variation over time. Thus, we estimate versions of our model without τ and without σ and present the results in

before the reform are sufficient. The one exception is if school j was only open after the reform, estimating δ_{jg} requires data from after the reform.

Online Appendix Table A19. The estimates are fairly similar across the specifications.

We estimate the supply model parameters (μ , 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} . μ then 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 μ).

8 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 (Table 9). For kindergarteners, we estimate γ at 0.79, ρ at 4.12, λ at 0.19, and σ at 0.39. 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 kindergarteners, 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, λ , is positive for all but one grade, indicating that stu-

²⁸If 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.

²⁹We 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.

dents 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% ($0.19/1.74$) 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 29% ($0.34/1.17$) 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 0.681 (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 post-reform 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% 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 the direct effect makes up 80% (with a standard error of 1.8%) of the total effect (or the indirect effect makes up 20%). We further break out the predicted student

³⁰In 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.

³¹A 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.

flows by school type in Panel A of Table 10.³² 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 67% of schools will remain open regardless of demand, while 9% will close regardless of demand (Table 9). We estimate that the average school requires 11.5 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 not observed in the data, we can assess our model fit by comparing predicted versus actual closure rates for school’s 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 8% of schools’ decisions whether to exit depend on other schools’ decisions – i.e., for 92% of schools, either closing or staying open is a dominant strategy while for the other 8% 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 2 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 2011-12 ($\hat{\tau}_5$ ranges from -0.15 to -0.27) is related to the reform. In our baseline model we assume the reform only affects private school attractiveness through γ , and thus ignore the reform’s impact on the public sector as a whole through increased spending flexibility and changing teacher prices.³³ Using our supply estimates, we predict a two-year closure rates in the absence of the FSF increased spending of 7.44%, 6.57%, and 7.70% in 2008, 2010, and 2012, respectively. With the reform, we predict two-year closure rates of 7.93%, 7.62%, and 8.53% in 2008, 2010, and 2012, respectively (Table 10). We attribute the difference (an average of 0.8 percentage points) to the increased spending. We

³²Table 10 estimates flows that compare 2012 to 2006. In Online Appendix Table A21 we show the flows comparing 2006 to 2008 and 2010.

³³We explore how the predicted private school closure rate changes by varying the reform’s impact on private school attractiveness in Appendix Figure A.11.

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 2011-12 school year.³⁴

8.1 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 a tuition and a non-tuition component:

$$\hat{\delta}_{jg} = \alpha_{0g} + \alpha_1 \text{AvgTuition}_{jg} + \omega_{jg}. \quad (9)$$

The challenge in estimating α_1 is that tuition is likely endogenous. We therefore turn to the observation by Dynarski et al. (2009) that many Catholic private schools offer sibling discounts to families that send multiple children to the school. Dynarski et al. (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. The data collection is described in Online Appendix C.

We note that $\text{AvgTuition}_{jg} = 1\text{ChildPrice}_{jg} - \text{AvgDiscount}_{jg}$, where AvgDiscount_{jg} will be our instrument. To construct 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 0 siblings, 1 sibling, 2 siblings, or 3 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:

$$\begin{aligned} \text{AvgDiscount}_{jg} = & 1\text{ChildPrice}_{jg}(1 - \hat{\text{FracFam}}1_{jg}) - 2\text{ChildPrice}_{jg}\hat{\text{FracFam}}2_{jg} \\ & - 3\text{ChildPrice}_{jg}\hat{\text{FracFam}}3_{jg} - 4\text{ChildPrice}_{jg}\hat{\text{FracFam}}4_{jg} \end{aligned} \quad (10)$$

³⁴We 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.

where $NChildPrice_{jg}$ is the average tuition rate for a family with N children at the school and $FracFamN_{jg}$ is the estimated fraction of students attending school with $N-1$ siblings.³⁵

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 et al. (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 $AvgTuition_{jg} = 1ChildPrice_{jg} - AvgDiscount_{jg}$ on $AvgDiscount_{jg}$ 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 λ_g estimates from our demand model. Because the coefficient on private school tuition is slightly larger in magnitude than the average estimated λ_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 the families value \$1 of additional projected public school spending equivalently to \$1.06 – \$1.63 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)'s 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 73-113% 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% in the absence of private school exit, an increase of 5%.³⁸

³⁵We get very similar results when we estimate $FracFamN_{jg}$ using citywide demographics.

³⁶The 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.

³⁷We 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 5.1. We then divide the welfare sum by the range of actual spending. For details on the welfare and counterfactual calculations, see Online Appendix D.

³⁸In addition to the strong identifying assumption, we note two other limitations. First, the students

9 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 supply of private schooling explained 20% 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.³⁹ 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.⁴⁰ 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.

for whom a tuition increase at the local Catholic school affects their choice (and thus identify α_1) may not be the same students who switch in response to public school funding changes (and thus identify λ_g). 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 micro data on schooling choices of students from families of different sizes, we leave this for future work.

³⁹ “Researchers Highlight Supply-Side Shortages for Voucher Programs” *Education Week*, April 4, 2014.

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

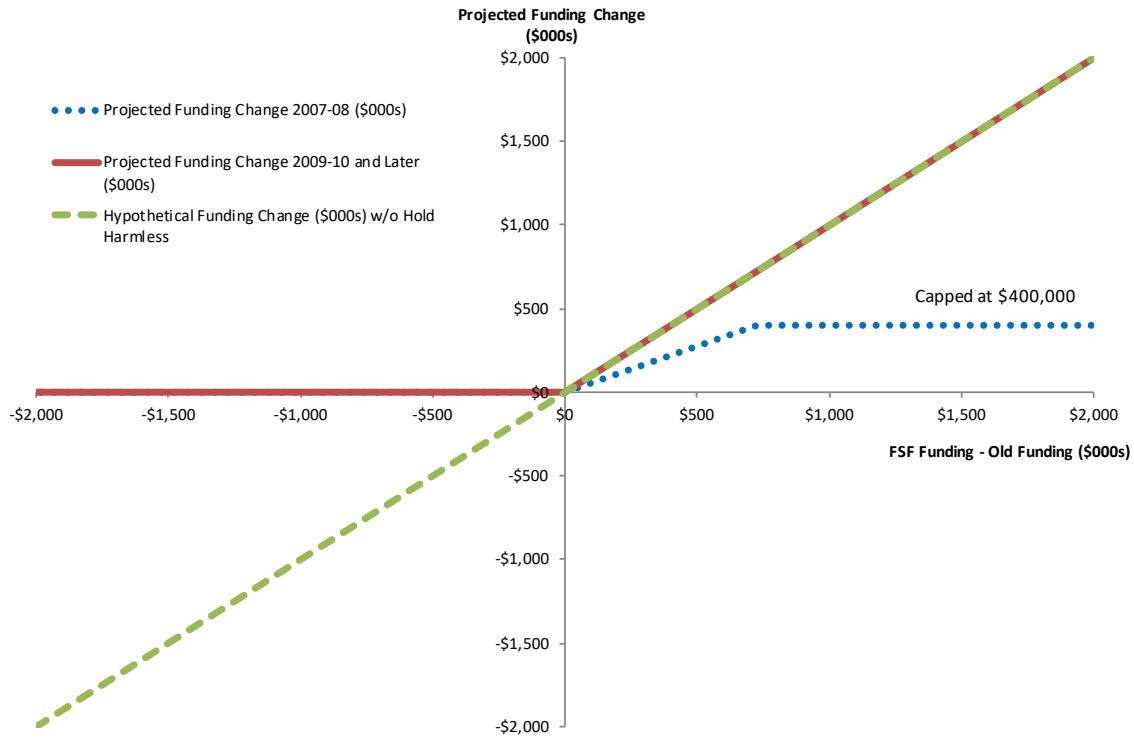
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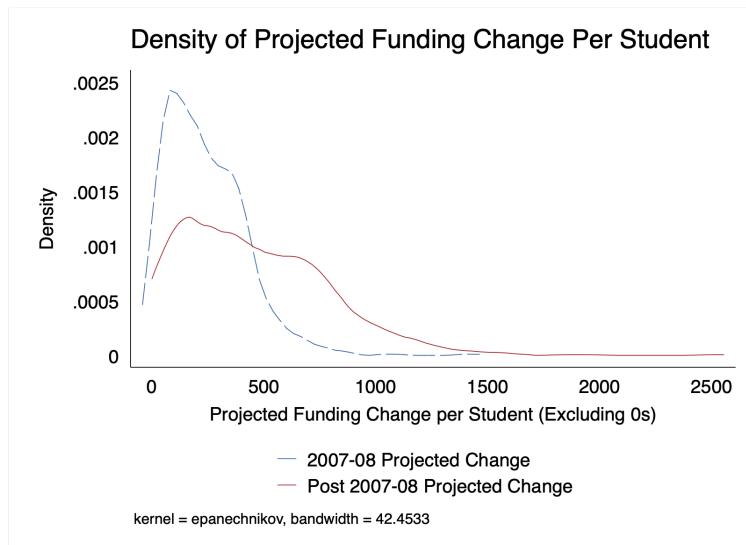
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Figure 1: Funding Change Formula



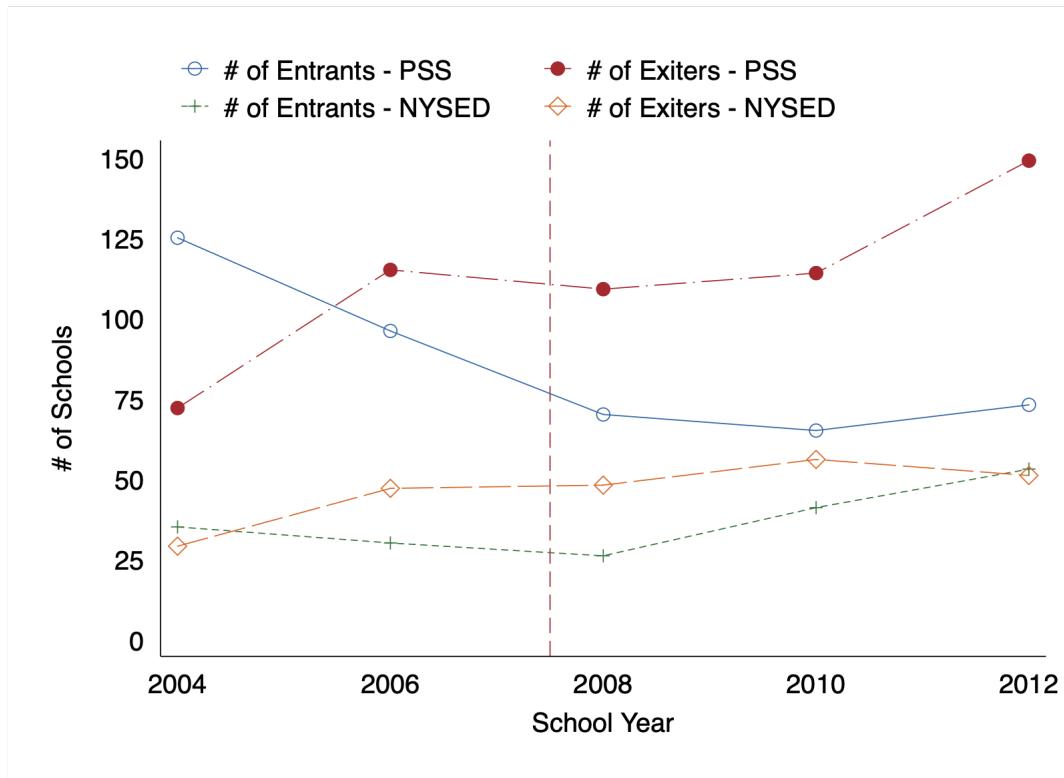
Note: Fixing a public school's enrollment and student demographics, the x-axis is the potential change in funding based on the FSF formula change. The y-axis is the projected change in funding and the solid line traces out the relationship. The dotted line shows the projected funding change in 2007-08, when the reform was to be phased in. The dashed line shows the hypothetical reform without a hold harmless clause. This hypothetical line will be the source of variation we use for robustness checks.

Figure 2: Density of Funding Change Per Student (Excluding 0s)



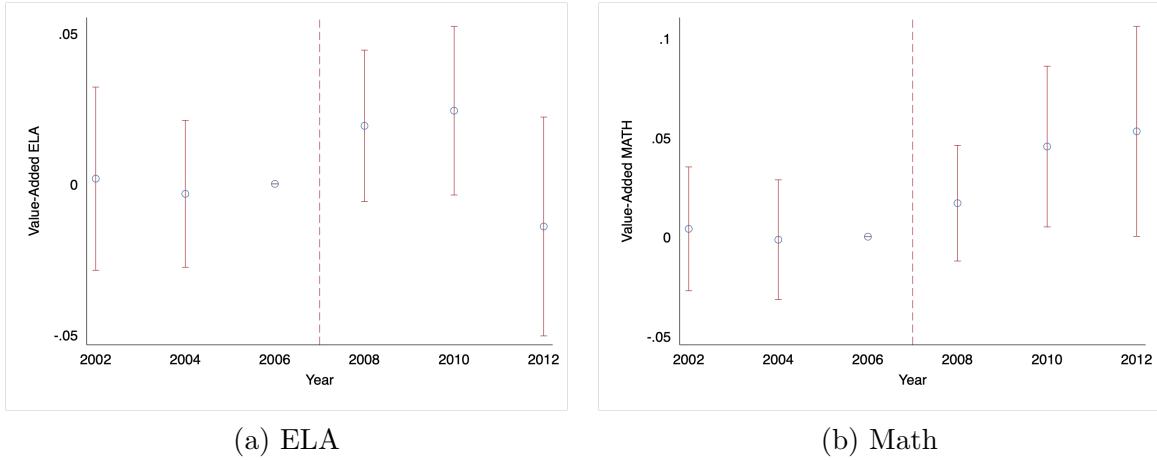
Note: The graph is an estimated kernel density of the funding change per student among public schools that received funding increases. 48.8% of schools had no funding change.

Figure 3: Number of Entrants and Exiters in NYC



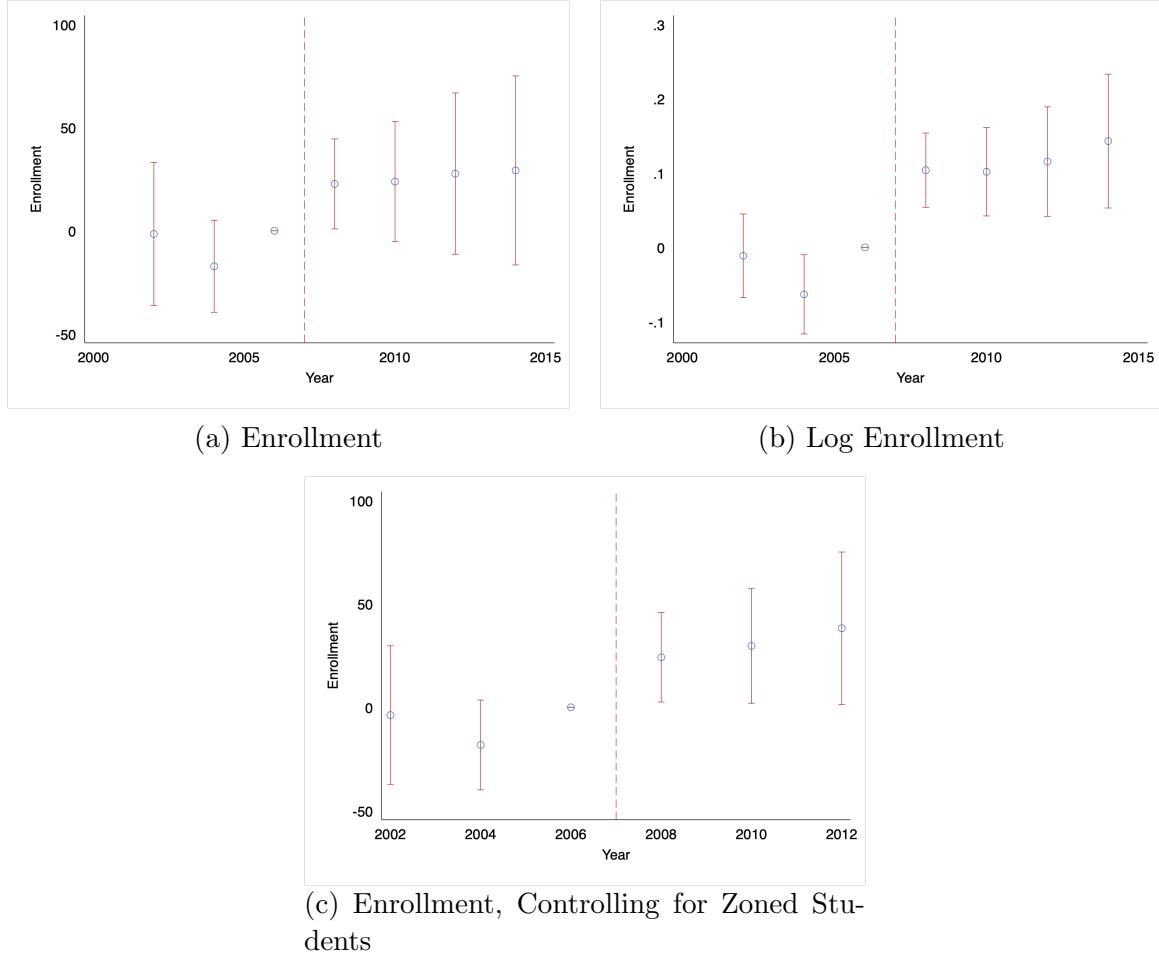
Note: “PSS” refers to schools in the Private School Survey, with entry and exit determined by when schools appear in the Private School Survey. The data come out every other year, so entry and exit refer to actions taken over two-year periods. “NYSED” refers to schools in the NYSED data, with entry and exit determined by when schools appear in the annual NYSED data. The red line marks the implementation of the FSF reform. Years on the x-axis refer to the spring of the school year (i.e., 2008 is the 2007-08 school year).

Figure 4: Value-Added Event Studies



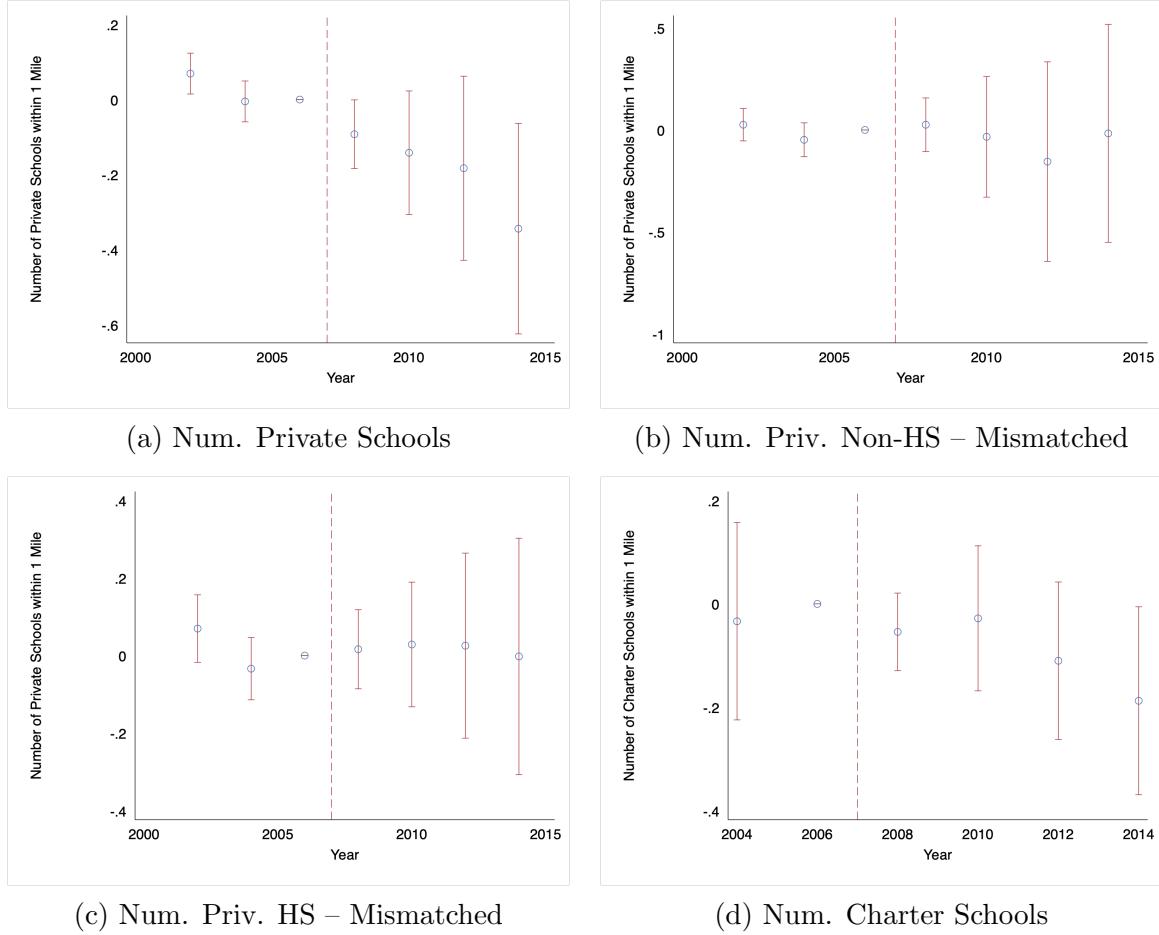
Note: Figure shows estimated coefficients (and 95% confidence intervals) on projected FSF funding (\$1,000s/student) for each two-year period from the difference-in-difference regression of value-added on projected FSF funding, school fixed effects, and year fixed effects. The 2005-06 – 2006-07 coefficient is normalized to 0.

Figure 5: Enrollment Event Studies



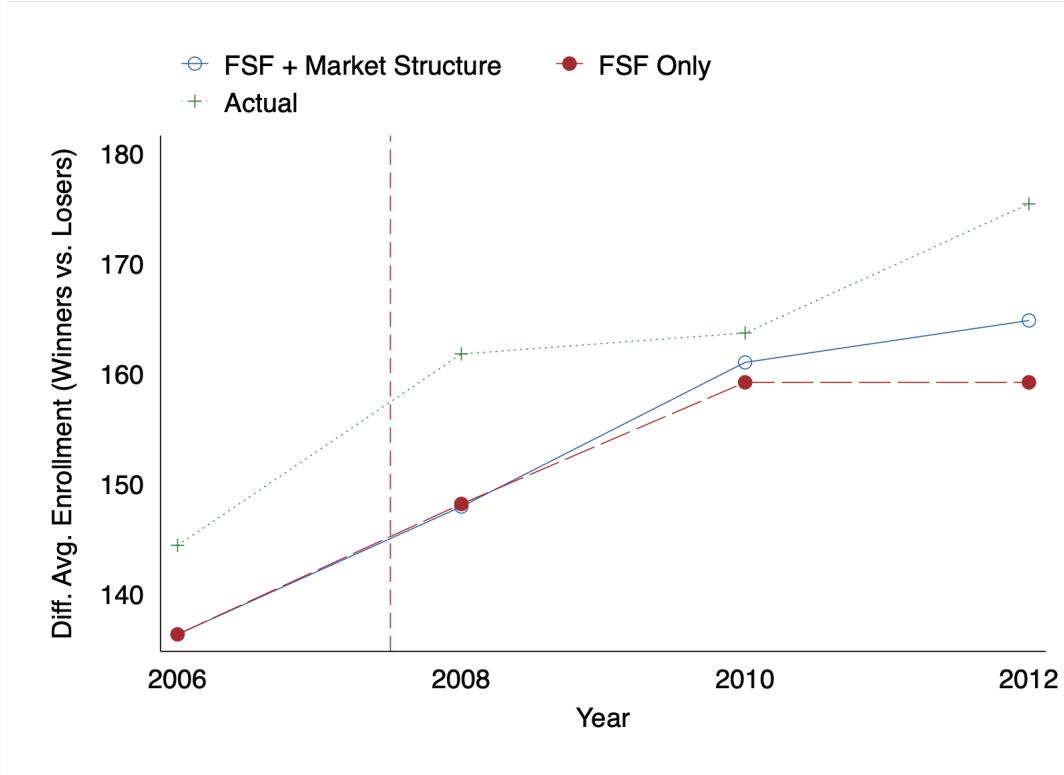
Note: Figure shows estimated coefficients (and 95% confidence intervals) on projected FSF funding (\$1,000s/student) for each two-year period from the difference-in-difference regression of enrollment (or log enrollment) on projected FSF funding, school fixed effects, and year fixed effects. The 2005-06 – 2006-07 coefficient is normalized to 0. The final panel includes the number of zoned students as a control.

Figure 6: Supply of Schools Event Studies



Note: Figure shows estimated coefficients (and 95% confidence intervals) on projected FSF funding (\$1,000s/student) for each two-year period from the difference-in-difference regression of number of private (or charter) schools within 1 mile of a public school on projected FSF funding, school fixed effects, and year fixed effects. The 2005-06 – 2006-07 coefficient is normalized to 0.

Figure 7: Direct and Indirect Effect Counterfactuals



Note: 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 are estimates from the data.

Table 1: Traditional Public, Charter, and Private School Summary Statistics

School Characteristics	Traditional Public Schools		Charter Schools		Private Schools		
	Common Core	Common Core	PSS	NYSED	Estimation Sample (PSS and NYSED Match)	Entrants (since 2002)	Exiters (by 2014)
Number of Schools	1,354	173	1,002	929	823	115	203
% Catholic			32%	33%	38%	7%	56%
% Other Religious			47%	42%	43%	62%	32%
% Non-Religious			21%	25%	19%	31%	11%
Enrollment per Grade (Mean)	112.69	67.00	30.27	28.68	31.16	19.58	20.79
% with Enrollment per Grade < 10	5%	1%	18%	18%	10%	30%	10%
% with Enrollment per Grade < 20	6%	1%	41%	45%	24%	58%	25%
% Black	33%	65%	17%		18%	18%	42%
% Hispanic	40%	27%	13%		15%	7%	28%
% of Schools with > 50% Minority	78%	98%	41%		45%	43%	72%

Traditional public and charter school data come from the Common Core of Data. Private school data comes 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 2005-06. 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 2001-02 or exit before 2013-14. Minority students are black or Hispanic.

Table 2: Public School Budget Regressions

	Actual Total Budget per Student	Actual Total Budget per Student	Actual FSF Budget per Student
Projected Total Budget (\$) per Student	0.594*** (0.156)	0.561*** (0.160)	
Hold Harmless (\$) per Student		0.054 (0.099)	
Projected FSF (\$) per Student			0.905*** (0.094)
(Spring) Years	2007-2014	2007-2014	2008-2014
Fixed Effects	Year, School	Year, School	Year, School
N	7,841	7,841	5,856
R-Squared	0.080	0.080	0.717

* < 10%, ** < 5%, *** < 1%. Data span the 2006-07 through 2013-14 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 2007-08 onward. Standard errors are clustered at the zip code level.

Table 3: Regressions of Estimated Value-Added on FSF Change

	Estimated Value- Added (ELA)	Estimated Value- Added (Math)						
FSF	0.011 (0.011)	0.039** (0.017)	0.005 (0.014)	0.034* (0.019)	0.025 (0.018)	0.074*** (0.024)	0.013 (0.012)	0.043** (0.018)
Hyp Neg FSF						-0.003 (0.006)	-0.006 (0.008)	
Sample	All Students	All Students	Stayers	Stayers	Switchers	Switchers	All Students	All Students
Fixed Effects	Year, School	Year, School						
N	7,549	7,549	7,504	7,504	7,445	7,445	7,549	7,549
R-Squared	0.066	0.025	0.065	0.028	0.045	0.022	0.066	0.025

* < 10%, ** < 5%, *** < 1%. Data span 2000-01 through 2011-12 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, and free or reduced lunch. School-subject-year VA is the mean VA 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 (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 comes from the NYC DOE.

Table 4: Difference-in-Difference Regressions of Enrollment

	Enroll	In(Enroll)	Enroll	Enroll	Enroll
FSF	31.941 (21.157)	0.138*** (0.036)	24.067 (21.503)	36.984* (20.280)	29.144 (20.693)
Hyp. Neg. FS			10.953 (7.967)		10.905 (7.688)
Change in Zoned Students				0.248*** (0.036)	0.248*** (0.036)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School
N	12,347	12,347	12,347	12,347	12,347
R-Squared	0.115	0.085	0.115	0.134	0.135

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. "Projected 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. The "Change in Zoned Students" indicates the difference in number of students for whom the school is a zoned school relative to a 2007-08 baseline. This variable is only available for 2007-08 and onward and is set to 0 for prior years. Standard errors are clustered by zip code. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets and enrollments are drawn from the Common Core of Data.

Table 5: Heterogeneity in Enrollment Response

Panel A: Enrollments									
	FRL Enroll	ELL Enroll	Black Enroll	Hispanic Enroll	White Enroll				
FSF	15.885 (16.807)	-10.015 (15.281)	24.941*** (7.158)	2.004 (10.982)	-5.445 (3.347)				
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School				
N	7,786	7,786	7,786	7,786	7,786				
R-Squared	0.088	0.743	0.365	0.006	0.011				
Panel B: Mean Characteristics All Students									
	FRL Percent	ELL Percent	Black Percent	Hispanic Percent	White Percent	ELA Score t-1 Mean	Math Score t-1 Mean	ELA Score t Mean	Math Score t Mean
FSF	0.003 (0.013)	0.004 (0.004)	0.019*** (0.007)	0.004 (0.005)	-0.002 (0.003)	-0.067*** (0.017)	-0.017 (0.022)	-0.033* (0.017)	-0.014 (0.023)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	7,786	7,786	7,786	7,786	7,786	7,608	7,612	7,657	7,657
R-Squared	0.105	0.976	0.442	0.050	0.002	0.014	0.016	0.031	0.038
Panel C: Mean Characteristics Switchers									
	FRL Percent	ELL Percent	Black Percent	Hispanic Percent	White Percent	ELA Score t-1 Mean	Math Score t-1 Mean	ELA Score t Mean	Math Score t Mean
FSF	0.011 (0.010)	0.002 (0.006)	0.032*** (0.008)	-0.003 (0.006)	-0.005 (0.003)	-0.110*** (0.027)	-0.089*** (0.031)	-0.015 (0.027)	-0.005 (0.028)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	6,812	6,812	6,812	6,812	6,812	6,618	6,622	6,689	6,690
R-Squared	0.171	0.965	0.470	0.014	0.003	0.006	0.002	0.005	0.008

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. "FSF" is the projected per-student funding change (in \$1,000s). "FRL" refers to students who received free or reduced price lunch. "ELL" are students who are English language learners. ELA and Math scores have been standardized to have mean 0 and standard deviation 1 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.

Table 6: Number of Private Schools Regressions

	Num Private Schools w/i 1 Mile	Num Private Schools w/i 1 Mile	Num Private HS w/i 1 Mile	Num Private Non-HS w/i 1 Mile	Num Charter Schools w/i 1 Mile	Num Charter Schools w/i 1 Mile
FSF	-0.201** (0.099)	-0.231** (0.099)			-0.092 (0.355)	-0.072 (0.371)
Hyp. Neg. FSF		0.046 (0.048)				-0.025 (0.109)
Mismatched FSF			0.012 (0.105)	-0.033 (0.201)		
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,043	11,043	5,079	3,052	3,050	3,050
R-Squared	0.074	0.075	0.050	0.067	0.471	0.471

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 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 (HS or non-HS) from the private schools counted on the left-hand-side. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey 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.

Table 7: Heterogeneity in Private School Supply Response

	Num Private Schools w/i 1 Mile												
				Above Median Percent Minority		Below Median Percent Minority		Above Median Enrollment		Below Median ELA VA		Above Median Math VA	
	Catholic	Non-Catholic Religious	Non-Religious										
FSF	-0.086 (0.054)	-0.090 (0.055)	-0.024 (0.045)	-0.084 (0.078)	-0.113** (0.051)	-0.061 (0.064)	-0.141* (0.078)	0.046 (0.035)	-0.055 (0.036)	0.034 (0.025)	-0.039 (0.042)		
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043	11,043
R-Squared	0.195	0.016	0.043	0.132	0.029	0.058	0.035	0.059	0.071	0.009	0.131		

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. An observation is a public school-school year. "FSF" is the projected per-student funding change (in \$1,000s). Private school percent minority and enrollment are taken from the 2005-06 school year. ELA VA and Math VA are estimated fixed effects from regressions of school-year-8th grade average test scores on school fixed effects and four-year lagged school-year-4th grade average test scores. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey 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.

Table 8: Model Parameters and Moments

<u>Demand Model</u>			
Parameters (per Grade)	#	Moments (per Grade)	#
δ school-grade utility	J_g+K_g-5	school-grade enrollment shares	J_g+K_g-5
γ utility from distance (miles)	1	mean distance (miles) from zoned school to public school attended	1
ρ (dis)utility from zoned school	1	fraction attending zoned school	1
λ utility from projected FSF (\$1000s/student)	1	change in annual share for FSF winners	1
σ sd of private school preference	1	cov(private enroll, private schools)	1
τ year-specific utility from private schools	5	yearly private enrollment share	5
<u>Supply Model</u>			
Parameters	#		
ρ_0 probability cost =0	1		
ρ_{inf} probability cost =infinity	1		
μ mean cost per grade (students)	1		

Demand parameters are estimated with method of simulated moments. Supply parameters are estimated with maximum simulated likelihood.

Table 9: Model Estimates

	Grade								
	K	1	2	3	4	5	6	7	8
Demand Moments									
mean distance (miles) from zoned school to public school attended	0.56	0.59	0.63	0.69	0.75	0.85	1.25	1.30	1.42
fraction attending zoned school	0.75	0.74	0.73	0.72	0.70	0.67	0.51	0.54	0.53
change in annual share for FSF winners	0.11%	-0.40%	-0.28%	0.32%	1.45%	2.25%	6.51%	4.78%	6.80%
cov(private enroll, private schools)	0.005	0.003	0.003	0.003	0.003	0.003	0.003	0.003	0.003
private enroll share 2002	0.266	0.219	0.210	0.209	0.210	0.211	0.220	0.219	0.215
private enroll share 2004	0.243	0.199	0.195	0.191	0.194	0.192	0.200	0.198	0.196
private enroll share 2006	0.246	0.205	0.204	0.203	0.203	0.200	0.208	0.206	0.200
private enroll share 2008	0.229	0.197	0.196	0.204	0.203	0.200	0.211	0.204	0.208
private enroll share 2010	0.228	0.194	0.194	0.198	0.196	0.197	0.206	0.208	0.204
private enroll share 2012	0.199	0.165	0.164	0.165	0.166	0.163	0.171	0.171	0.167
Demand Parameters									
γ disutility from distance (miles)	0.78 (0.01)	0.77 (0.01)	0.73 (0.01)	0.67 (0.00)	0.65 (0.00)	0.62 (0.00)	0.67 (0.00)	0.57 (0.00)	0.51 (0.01)
ρ utility from zoned school	4.12 (0.00)	4.11 (0.01)	4.10 (0.00)	4.06 (0.00)	4.01 (0.00)	3.82 (0.00)	2.70 (0.00)	2.85 (0.00)	2.84 (0.00)
λ utility from FSF (\$1000s/student)	0.19 (0.06)	0.13 (0.07)	0.09 (0.08)	0.25 (0.06)	0.34 (0.05)	0.26 (0.06)	0.00 (0.04)	0.10 (0.06)	0.08 (0.03)
σ sd of private school preference	0.39 (0.25)	0.65 (0.05)	0.60 (0.46)	0.90 (0.01)	1.78 (0.13)	0.00 (0.66)	0.76 (0.02)	1.02 (0.18)	0.91 (0.01)
τ_1 utility from private schools 2004	0.00 (0.06)	0.00 (0.09)	0.02 (0.05)	0.00 (0.07)	0.01 (0.07)	0.00 (0.04)	-0.05 (0.05)	-0.07 (0.06)	-0.06 (0.01)
τ_2 utility from private schools 2006	0.00 (0.06)	0.00 (0.09)	-0.01 (0.04)	0.01 (0.07)	-0.01 (0.07)	0.00 (0.05)	0.00 (0.05)	0.00 (0.07)	0.00 (0.01)
τ_3 utility from private schools 2008	-0.06 (0.06)	-0.03 (0.10)	-0.03 (0.06)	0.09 (0.08)	0.09 (0.08)	0.01 (0.05)	0.03 (0.05)	0.03 (0.07)	0.07 (0.01)
τ_4 utility from private schools 2010	-0.10 (0.07)	-0.09 (0.10)	-0.04 (0.08)	0.07 (0.09)	0.12 (0.09)	0.01 (0.04)	0.00 (0.06)	0.00 (0.08)	0.00 (0.01)
τ_5 utility from private schools 2012	-0.27 (0.07)	-0.24 (0.10)	-0.23 (0.05)	-0.12 (0.09)	-0.19 (0.08)	-0.15 (0.05)	-0.21 (0.06)	-0.15 (0.09)	-0.18 (0.02)
δ standard deviation of estimates	1.74	1.09	1.11	1.08	1.17	1.00	1.37	1.27	1.28
Supply Parameters									
p_0 probability cost = 0	0.67 (0.07)								
p_{inf} probability cost = infinity	0.09 (0.02)								
μ mean cost per grade (students)	11.45 (2.59)								

Demand parameters are estimated with method of simulated moments. The supply parameters are estimated with maximum simulated likelihood, using an exponential distribution. The mean of the distribution depends on the number of elementary and middle grades. Supply model standard errors were estimated using 100 clustered bootstrap replications with a cluster defined by a borough-year.

Table 10: Counterfactual Results

Panel A: Demand Counterfactual						
Initial School Category	Final School Category					School Count
	Winners	Losers	Other Public	Private Non-Closers	Private Entrants/Exiters	
	Winners	0	0	0	0	
	Losers	3193	0	0	0	
	Other Public	286	0	0	0	
	Private Non-Closers	3262	0	0	0	
	Private Entrants/Exiters Direct	1323	0	0	0	
Private Entrants/Exiters Indirect	13984	11907	647	11137		297

Panel B: Supply Counterfactual						
	Year			% Difference	Projected FSF Spending (millions)	School Count
	2008	2010	2012			
Predicted Two-Year Exit Rate, FSF	7.93%	7.62%	8.53%			
Predicted Two-Year Exit Rate, no FSF	7.44%	6.57%	7.70%			
Difference	0.50%	1.06%	0.84%			
Change in Student Welfare (millions)	\$74	\$142	\$141			
Change in Student Welfare (millions), No Exit	\$77	\$151	\$148			
% Difference	4.03%	5.86%	5.06%			
Projected FSF Spending (millions)	\$104	\$219	\$215			

In Panel A, "Initial School Category" refers to predicted enrollments in 2006. "Final School Category" refers to predicted enrollments from a counterfactual that implements the FSF funding change and changes the set of private schools open based on which schools entered and exited from 2006-2012. "Winners" are public schools that received additional money from FSF, "Losers" are public schools that were part of the FSF reform but were held harmless, "Other Public" schools includes specialty and charter schools, "Private Non-Closers" are private schools open in 2006 and 2012, and "Private Entrants/Exiters" are schools that entered or exited from 2006-2012. "Direct" refers to the predicted enrollment changes from a counterfactual where the set of schools stays at the 2006 market structure while "Indirect" refers to the predicted enrollment changes from the change in market structure. In Panel B, the predicted exit rate without FSF comes from schools receiving fixed cost draws higher than their predicted enrollments in the given year, where the predicted enrollments include the change in the private sector's attractiveness (τ). The exit rate with FSF implements the FSF reform via additional public school funding.

Table 11: Tuition IV Estimates

	First Stage		IV
	Average Tuition	Delta	
Average Discount (\$1000s)	-1.035*** (0.052)		
Average Tuition (\$1000s)		-0.168** (0.075)	
N	324	324	

* < 10%, ** < 5%, *** < 1%. An observation is a school-grade. The regression includes Catholic schools and grades 6-8. "Delta" is the estimated school-grade fixed effect from the demand model. Average tuition is the first-child price minus the average discount, where the average discount is a weighted-average of the second-child, third-child, and fourth-child discounts.

FOR ONLINE PUBLICATION

A Appendix: Conceptual Framework

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 1 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 CDF of Δu_i with derivative f_Δ . 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. $\Pi(\cdot)$ 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. 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 $\mathbf{E}Q_1(x) = (1 - G(-\Pi(\gamma x)))(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))$, 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:

$$\begin{aligned}\frac{d\mathbf{E}Q_1(x)}{dx} &= -\frac{\partial G(-\Pi(\gamma x))}{\partial x}(1 - F_\Delta(-\gamma x)) + (1 - G(-\Pi(\gamma x)))\gamma f_\Delta(-\gamma x) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \\ &= F_\Delta(-\gamma x)\frac{\partial G(-\Pi(\gamma x))}{\partial x} + (1 - G(-\Pi(\gamma x)))\gamma f_\Delta(-\gamma x)\end{aligned}$$

When evaluating at $x = 0$ and $G(-\Pi(0)) = 0$:

$$F_\Delta(0)\frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} + \gamma f_\Delta(0)$$

Let utilitarian student surplus $S(x)$ be students' total utility and let $\mathbf{E}[\mathbf{S}(\mathbf{x})]$ be the expected surplus where the expectation is taken over the probability the private school closes:

$$\mathbf{E}[S(x)] = (1 - G(-\Pi(\gamma x)))S^{NC}(x) + G(-\Pi(\gamma x))S^C(x),$$

where $S^{NC}(x)$ is surplus when the private school does not close and $S^C(x)$ is surplus when

the private school closes.

$$S^{NC}(x) = \mathbf{E}[u_2] + \int_{-\gamma x}^{\infty} (\Delta u + \gamma x) dF_{\Delta}$$

and

$$S^C(x) = \mathbf{E}[u_2] + \int_{-\infty}^{\infty} (\Delta u + \gamma x) dF_{\Delta}$$

with

$$S^C(x) - S^{NC}(x) = \int_{-\infty}^{-\gamma x} (\Delta u + \gamma x) dF_{\Delta}$$

We can recast the expression for $S^{NC}(x)$ in terms of a representative agent's maximization problem:

$$\begin{aligned} S^{NC}(x) &= \max_{\bar{\Delta}} \mathbf{E}[u_2] + \int_{\bar{\Delta}}^{\infty} (\Delta u + \gamma x) dF_{\Delta} \\ S^C(x) - S^{NC}(x) &= \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_{\Delta} \end{aligned}$$

Now consider a small exogenous change in funding from 0 to x . Taking the total derivative, the change in surplus is:

$$\begin{aligned} \frac{d\mathbf{E}[\mathbf{S}(\mathbf{x})]}{dx} &= -\frac{\partial G(-\Pi(\gamma x))}{\partial x} S^{NC}(x) + (1 - P^C(\gamma x)) \frac{\partial S^{NC}(x)}{\partial x} + \frac{\partial G(-\Pi(\gamma x))}{\partial x} S^C(x) + G(-\Pi(\gamma x)) \frac{\partial S^C(x)}{\partial x} \\ &= \frac{\partial S^{NC}(x)}{\partial x} + G(-\Pi(\gamma x)) \left(\frac{\partial (S^C(x) - S^{NC}(x))}{\partial x} \right) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} (S^C(x) - S^{NC}(x)) \end{aligned}$$

Take each term separately:

$$\begin{aligned} \frac{\partial S^{NC}(x)}{\partial x} &= \frac{\partial \max_{\bar{\Delta}} \int_{\bar{\Delta}}^{\infty} (\Delta u + \gamma x) dF_{\Delta}}{\partial x} \\ &= \gamma(1 - F_{\Delta}(-\gamma x)) \end{aligned}$$

where we use the Envelope Theorem and that $\bar{\Delta} = -\gamma x$ solves the representative agent's problem. The second term (excluding $G(-\Pi(\gamma x))$):

$$\frac{\partial (S^C(x) - S^{NC}(x))}{\partial x} = \frac{\partial \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_{\Delta}}{\partial x} = \gamma F_{\Delta}(-\gamma x)$$

The third term:

$$\begin{aligned} \frac{\partial G(-\Pi(\gamma x))}{\partial x} (S^C(x) - S^{NC}(x)) &= \frac{\partial G(-\Pi(\gamma x))}{\partial x} \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_{\Delta} \\ &= \frac{\partial G(-\Pi(\gamma x))}{\partial x} (\mathbf{E}[\Delta u | \Delta u < -\gamma x] + \gamma x) F_{\Delta}(-\gamma x) \end{aligned}$$

Combining all the terms:

$$\begin{aligned}
\frac{d\mathbf{E}[\mathbf{S}(\mathbf{x})]}{dx} &= \frac{\partial S^{NC}(x)}{\partial x} + \gamma G(-\Pi(\gamma x)) \left(\frac{\partial(S^C(x) - S^{NC}(x))}{\partial x} \right) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} (S^C(x) - S^{NC}(x)) \\
&= \gamma(1 - F_\Delta(-\gamma x)) + \gamma G(-\Pi(\gamma x))F_\Delta(-\gamma x) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} (\mathbf{E}[\Delta u | \Delta u < -\gamma x] + \gamma x) F_\Delta(-\gamma x) \\
&= (1 - F_\Delta(-\gamma x))\gamma + F_\Delta(-\gamma x) \left(\gamma G(-\Pi(\gamma x)) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} (\mathbf{E}[\Delta u | \Delta u < -\gamma x] + \gamma^2 x) \right)
\end{aligned}$$

When evaluating at $x = 0$ and $G(-\Pi(0)) = 0$:

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

Suppose that students trade off a school's effect on their academic achievement (θ_{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 βx on achievement. Then achievement, $A(x)$ is:

$$\begin{aligned}
A^{NC}(x) &= \mathbf{E}[\theta_2] + \int_{-\gamma x}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta \\
A^C(x) &= \mathbf{E}[\theta_2] + \int_{-\infty}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta \\
\mathbf{E}[A(x)] &= (1 - G(-\Pi(\gamma x)))A^{NC}(x) + G(-\Pi(\gamma x))A^C(x) \\
&= \mathbf{E}[\theta_2] + \int_{-\gamma x}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta + G(-\Pi(\gamma x)) \int_{-\infty}^{-\gamma x} (\Delta u - \alpha p_2 + \beta x) dF_\Delta
\end{aligned}$$

$$\begin{aligned}
\frac{d\mathbf{E}[A(x)]}{dx} &= \gamma(1 - G(-\Pi(\gamma x)))(-\gamma x + \beta x - \alpha p_2) f_\Delta(-\gamma x) + \beta(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))\beta F_\Delta(-\gamma x) \\
&\quad + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \int_{-\infty}^{-\gamma x} (\Delta u - \alpha p_2 + \beta x) dF_\Delta \\
&= \gamma(1 - G(-\Pi(\gamma x)))(-\gamma x + \beta x - \alpha p_2) f_\Delta(-\gamma x) + \beta(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))F_\Delta(-\gamma x) \\
&\quad + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \mathbf{E}[\Delta u - \alpha p_2 + \beta x | \Delta u < -\gamma x] F_\Delta(-\gamma x)
\end{aligned}$$

When evaluating at $x = 0$ and $G(-\Pi(0)) = 0$:

$$-\gamma \alpha p_2 f_\Delta(0) + \beta(1 - F_\Delta(0)) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \Big|_{x=0} \mathbf{E}[\Delta u - \alpha p_2 | \Delta u < 0] F_\Delta(0)$$

B Appendix: FSF Reform Details

Announced in May 2007, the FSF reform applied to most NYC public schools. In addition to charter schools, the reform does not apply to schools in Districts 75 (certain students with disabilities) and 79 (alternative schools and programs for students who experienced an education interruption).

Each school-year the NYC school district projects enrollments by student type at each NYC public school. Based on this projected enrollment, it sets school budgets according to the FSF formula. The formula involves a foundation amount (which started at \$200,000) and funding that depends on the number of students and their needs. If the school's actual enrollment deviates significantly from the projected enrollment, the school will receive a mid-year budgetary adjustment.

The reform's implementation was planned to be incremental in two ways. First, for schools that stood to gain money ("winners"), their 2007-08 gains were capped at the minimum of \$400,000 and 55% of the total gain. Second, for schools that stood to lose money ("losers"), they experienced no funding change as they were held harmless for the potential loss.

While the hold harmless clause was supposed to expire, it is still a key determinant of funding allocations (see Table 2) and one reason education analysts argue the FSF reform was never fully implemented. This may be due to the district's Legacy Teacher Supplement, which gives schools funding to cover salary increases for teachers who worked at the school in 2007 as they move up the experience scale. We thus include the hold harmless clause in the projected funding increase and expect even "losing" schools to have the ability to retain many of their high-salary teachers.

Over time the district updated the funding weights by the increase in the district's average teacher salary. But due to a state budget shortfall, NYC never received the full expected amount and has not fully implemented the FSF funding levels. Over time the school district has made up for missing state money, first using stimulus funds from the American Recovery and Reinvestment Act of 2009 (ARRA) and then using city funds in 2011-12 to make up for the loss in ARRA funding. When faced with funding shortages, the district lowered funding levels at many schools, not just "winners." Our analysis uses the within-city funding variation for a given year and thus nets out these common funding shocks.

The FSF funding was provided to principals with few constraints on how it could be spent, unlike the rest of the budget, largely made up of categorical and programmatic allocations. In exchange for this autonomy, principals were evaluated based on student achievement.

In addition to the analysis presented in the paper, we show how the ordering of schools' funding changed due to the reform. In Appendix Figure A.8, we plot a school's per student funding in 2007 against its per student funding in 2014. We see some reordering of schools though also some persistence.

C Appendix: Data

As described in the text, we bring together many data sources for our empirical analysis. In this section, we describe some of the private school data sources, as well as data construction choices, in further detail.

C.1 Private School Survey and NYSED Data

We form a census of schools using the NCES's Private School Survey (PSS). While the PSS claims to cover all private schools, some schools are missing from the data in certain years while still showing up in adjacent waves. For instance, a school may appear in the 2003-04 and 2007-08 waves of the PSS but not in the 2005-06 wave. For the 2005-06 wave, 880 private schools appear in the data while an additional 66 schools do not appear but are open both in a year prior to 2007-08 and a year after 2007-08. These schools tend to be smaller than the average school and are more likely to be non-Catholic religious schools.

We thus supplement the PSS with data from the New York State Education Department (NYSED) that includes enrollments for private schools that register with the state and have a registration code. Because not all schools have registration codes, this data set does not capture all schools in the PSS and includes fewer school characteristics. But while the NYSED 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.

The matching between the PSS and NYSED data is subject to some judgment calls, which we explore in Appendix Table A3. Our baseline (“strict”) matching only keeps PSS schools that uniquely match to NYSED schools. In cases where an NYSED school may match to multiple PSS schools, we do not form any match. We find our PSS match rate is 81%. If instead we employ looser criteria and make judgment calls in the cases where an NYSED school could match to multiple PSS schools, we produce a match rate of 85%. For the stricter match, we are slightly more likely to match schools with larger enrollments and more teachers. Catholic schools match at high rates while the non-matches are concentrated in non-Catholic religious schools. For the NYSED schools, we achieve a strict match rate of 80% and a loose match rate of 91%. We are more likely to match schools with larger enrollments, Catholic schools, and schools that are open throughout the sample. Jewish schools in Brooklyn, in particular, are less likely to match.

In Panel C we take the matches and compare the data sets on their common variable: enrollment. We find that our matches tend to produce very similar enrollments per grade across the two data sources, which gives us confidence that the matching procedure works well.

To assess whether these judgment calls matter, we reproduce the supply regressions with different criteria for matching. We present the results in Appendix Table A10. The first

set of columns follows the strict matching process, the last set follows the looser process, and the middle set of columns follows the strict matching process but includes preschools that also have kindergarteners. Within each set of columns, we vary whether we keep all matches, only matches that agree across the data sets on when the school opened and closed (up to a 1 year difference given that the PSS is released biennially), or only matches that agree on when the school opened and closed and whose difference in enrollments per grade across the data sets always falls within the 5th to 95th percentile. We find consistent results throughout the table, with somewhat smaller, but more precise, coefficients as we decrease the sample. We similarly show consistent event study results in Appendix Figure A.7. In fact, the slight evidence of a pre-trend in the main specification disappears once we drop schools with year or enrollment inconsistencies across the data sets.

C.2 Private School Test Data

Our test score data on nonpublic schools come from the New York State Education Department. The test data are school-grade-year average test scores on the grade 4-8 math and 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 U.S. private school quality on a large fraction of the private school population in a geographic area. New York does not require that private schools take the test nor report the results. The schools that opt not to report the test results are a selected sample and are more likely to include high-tuition college prep schools.

The main data limitation is that we only have value-added estimates for 36% of the private school students. Based on observable characteristics from the PSS, schools with value-added estimates differ in several ways. We are more likely to have value-added estimates for Catholic schools, schools in the Bronx, schools with a higher percentage of Hispanic and Asian students, schools with more students, and schools with fewer teachers. We are unlikely to have value-added estimates for non-religious schools, single-sex schools, and specialty schools. Schools in Manhattan and Brooklyn are also slightly underrepresented relative to the other boroughs.

C.3 Matching Private School Survey and Private School Test Data

We match schools from the private school test data to the PSS using the schools' names and counties. We match name first based on common key words and then visually inspect each match to verify accuracy. For schools in either data set that do not match, we conduct manual name searches in the other data set. This matching method is imperfect as some schools share names or do not have standardized names. In 2007-08, we match 57% of the PSS schools to test data. The matched schools cover 51% of the private school enrollment. For 5% of the schools, we identify a possible match that we cannot claim is a sure match. We exclude these matches from our empirical analysis.

C.4 Private School Tuition and Expenditure

For schools that were still active in the 2012-13 school year, we have combined internet research and phone calls to collect current-year tuition data.

D Appendix: Structural Model Estimation

D.1 Demand Model

We simulate students using 2010 Census population counts by age to construct student locations. We place each student at the geographic centroid of the Census block where she lives. We assign students to grades by age, with 5-year-olds attending kindergarten, 6-year-olds attending first grade, etc. Because the Census data only covers 2010, we need to account for changes in population by geography (see Ferreyra and Kosenok (2018) for similar adjustments, at finer demographic groups). We use the 2000 Census counts of people ages 5-17 by block group and use the IPUMS crosswalk for block groups between the 2000 Census and 2010 Census (Ruggles, Flood, Goeken, Grover, Meyer, Paces and Sobek 2020). Let b index block, $g = g(b)$ index block group, and t index year. For a given age a , we observe $n_{ab,2010}$, the population count in block b in the 2010 Census. In the 2000 Census, we observe $n_{g,2000}$, the population count of ages 5-17 in block group g in the 2000 Census. For each block group g , we then calculate the change in youth population between 2000 and 2010:

$$\Delta n_g = n_{g,2010} - n_{g,2000} \quad (11)$$

where $n_{g,2010} = \sum_{a=5}^{17} \sum_{b:g(b)=g} n_{ab,2010}$.

We then linearly interpolate the population count, by block group, to year t in between 2000 and 2010: $n_{g,t} = n_{g,2000} + \frac{t-2000}{10} \Delta n_g$. For 2012, we extrapolate linearly: $n_{g,2012} = \max\{n_{g,2000} + \frac{12}{10} \Delta n_g, 0\}$. Within each block group, we keep the distribution of students by block and age constant at the 2010 Census distribution.

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 these data with our enrollment data for public (Common Core of Data), charter (Common Core of Data), and private schools (Private School Survey) and our measures of FSF funding.

We estimate our demand model using data from the 2001-02, 2003-04, 2005-06, 2007-08, 2009-10, and 2011-12 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 .

To estimate the demand parameters, we use an exactly-identified simulated method of moments procedure. We do this separately for each grade from kindergarten to eighth grade. We list the moments and parameters to be estimated in Table 8. We draw a single value per student location of ν_{igt} from a $N(0, 1)$ distribution. We iterate back-and-forth, starting

with a contraction mapping that holds fixed $\gamma, \sigma, \tau, \rho$, and λ and solves for the unique δ that makes the predicted school-grade enrollment shares match the associated moments. We then hold fixed δ and find the $\gamma, \sigma, \tau, \rho$, and λ that minimize a GMM objective function with the remaining moments and an identity weighting matrix.

We use a simplex algorithm (fminsearch) in Matlab to find the estimates that minimize the GMM objective function. We estimate standard errors with the formula for the GMM asymptotic variance-covariance matrix, where we fix the ν_{igt} draws.

Once we have the estimates of δ , we regress them on average tuition, instrumenting for average tuition with average discount as described in the text. This yields a coefficient on average tuition. We then average our λ_g estimate across the 9 grades and divide by the estimated coefficient on average tuition. This yields our reduced form estimate of \$0.96. We then scale this by dividing by the range of first stage coefficients (.59, .91) from Section 5.1.

D.2 Supply Model

We estimate our supply model using private school closure decisions between 2002-2004, 2004-2006, 2006-2008, 2008-2010, and 2010-2012. 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. We infer exit from whether the school's last appearance in the PSS and check these with the NYSED data.

We estimate the supply model parameters (μ , p_0 , and p_{inf}) using maximum simulated likelihood and the demand estimates. For each of 50 simulation iterations, we draw two independent uniform random variables between 0 and 1. Let F_j^s be school j 's fixed cost for simulation iteration s . If the first draw is below p_0 then we set the $F_j^s = 0$. If the first draw is greater than $1 - p_{inf}$, then we set F_j^s to a number larger than the number of students in NYC. If the first draw is in the interval $(p_0, 1 - p_{inf})$, then we invert the exponential CDF with parameter $\mu NumGrades_j$, evaluate it at the second draw, and set F_j^s equal to this value.

For each simulation iteration and set of candidate estimates, we calculate each private school's predicted enrollment under two scenarios: (1) all public schools and other private schools stay open and (2) all public schools stay open but all other private schools close. These are lower and upper predicted enrollment bounds. Predicted enrollment comes from our estimated demand model where we predict enrollment in the year t (using year t 's values for FSF funding and the private-year effect). We then compare these to each school's F_j^s and for those schools whose F_j^s fall outside their predicted enrollment bounds, we classify them as stayers or exiters accordingly. For remaining private schools whose exit decision has yet to be determined, we estimate new bounds where we fix the exit decisions of the previously classified private schools and construct bounds based on whether all remaining competitors exit or not. We iterate on this process until we have remaining schools who do not have a dominated strategy based on the already classified exit decisions. For these remaining schools, we solve the model sequentially via backward induction, starting with the schools with lowest predicted enrollment in the case where no schools exit. This process leads us to

the unique equilibrium for each simulation iteration.

For each private school j in year t , we estimate its probability of closing (\hat{P}_{jt}) as the fraction of the simulation iterations we predict it closes. Let C_{jt} be an indicator equal to 1 if school j actually closed in the data in between $t - 2$ and t . We then maximize the following simulated likelihood:

$$L = \sum_t \sum_j (C_{jt} \log(\hat{P}_{jt}) + (1 - C_{jt}) \log(1 - \hat{P}_{jt}))$$

We use a simplex algorithm (fminsearch) in Matlab and try different starting values. To estimate standard errors, we use a clustered bootstrap, where a cluster is a borough-year, to preserve the spatial structure of the data. We sample clusters randomly with replacement. Using the asymptotic distribution of the demand estimates, we draw a set of demand estimates. We then follow the same procedure above for estimating the supply model for 50 simulation draws. We repeat this process for 100 bootstrap iterations and use the standard deviation of the estimates as our standard errors.

D.3 Counterfactuals

For the demand counterfactual, we predict 2005-06, 2007-08, and 2009-10, and 2011-12 school enrollments under two scenarios. In the first scenario, we use the estimated model except we fix the public school market structure (the schools that are open and students' zoned public schools) at the 2005-06 market structure. In the second scenario, we use the estimated model except we fix the public *and* private school market structures to the 2005-06 market structure. In both cases, we use the private-year shock from 2005-06 (τ_2). We estimate public school enrollments in both scenarios for each of the four years, split the public schools subject to the reform into FSF “winners” and “losers,” and produce Figure 7. In 2006, we estimate that the difference in mean enrollment between “winners” and “losers” is 136.28. In 2012, we estimate that the difference in mean enrollment between “winners” and “losers” is 164.76 when the private market structure changes and 159.14 when it is held fixed.

We estimate the direct effect as the change in relative sizes of “winners” versus “losers” from the fixed market structure counterfactual divided by the change in relative sizes of “winners” versus “losers” from the counterfactual where the private school market structure is not fixed: $(159.14 - 136.28) / (164.76 - 136.28) = 0.80$. The indirect effect is the total effect minus the direct effect: 0.20. In panel (a) of Table 10 we show the flows between predicted actual (with the reform) and predicted counterfactual (no reform) choices.

For the supply counterfactual, we predict the exits induced by the FSF reform. For each school j , we first calculate bounds on its F_j^{exp} , its non-zero and non-infinite fixed cost, based on its exit and non-exit decisions in sample. Consider a school that was open in 2004, 2006, and 2008 but closed in 2010 and suppose we estimate that the school’s enrollments in those years were 100, 120, 100, and 90.⁴¹ Then we would bound F_j^{exp} between 90 and 100 and draw $F_{j,s}^{exp}$ for each simulation iteration s from a truncated exponential distribution. For schools

⁴¹The estimated enrollments are from the estimated supply equilibrium in each year.

that never exit, the lower bound is 0, and for schools that always exit, the upper bound is infinite. For 4% of schools, the upper and lower bounds cross. In these cases, we impose that the upper bound is infinite.

With these bounds, we then estimate two sets of equilibria for each year. In the first, we set FSF projected funding equal to its actual value in each year. In the second, we set FSF projected funding equal to 0 for all schools.⁴² For each scenario, we simulate 100 equilibria, where we draw separate fixed cost vectors, and then calculate a school's exit probability as the fraction of simulation iterations where we predict the school would exit. We attribute the increased exit probabilities in the second scenario – relative to the first scenario – to the reform.

The supply counterfactual assumes the FSF reform had no impact on the attractiveness of the private school sector relative to the public school “losers.” If, however, the reform also affected the attractiveness of the “losers,” say through increased spending flexibility, then we would be misstating the reform’s impact on private school closures. In Figure A.11 we explore the reform’s impact on the private school closure rate as a function of the fraction of the 2012 private school effect. Our base results assume the full effect (1 on the x-axis), but if part of the decrease in private school attractiveness is because of the FSF reform, we might scale back the 2012 effect in non-FSF counterfactuals. We see that scaling back the effect toward 0 decreases the private school closure rate by a factor of 2.

D.4 Welfare

Finally, we describe how we estimate student welfare in the various counterfactuals. For simulation iteration s (with the number of simulation iterations, S , equal to 100), let $\hat{C}_{s,t,FSF}$ be the set of schools that our supply model estimates would be open in year t when FSF projected funding matches the reform and let $\hat{C}_{s,t,noFSF}$ be the set of schools that our supply model estimates would be open in year t when FSF projected funding is 0 for all schools. Next, define estimated representative utilities as:

$$\hat{v}_{ijgt} = \hat{\delta}_{jg} - \hat{\gamma}_g d_{ij} + \hat{\sigma}_g \nu_{igt} + \hat{\tau}_{gt} \quad (12)$$

for a private school j , and for public school k :

$$\hat{v}_{ikgt,FSF} = \hat{\delta}_{kg} - \hat{\gamma}_g d_{ik} + \hat{\rho}_g ZONE D_{ikt} + \hat{\lambda}_g FSF_{kt} \quad (13)$$

$$\hat{v}_{ikgt,noFSF} = \hat{\delta}_{kg} - \hat{\gamma}_g d_{ik} + \hat{\rho}_g ZONE D_{ikt} \quad (14)$$

Let $\hat{v}_{ilgt,FSF}$ and $\hat{v}_{ilgt,noFSF}$ be the estimated representative utilities for school l (which could be public or private), where for private schools, $\hat{v}_{ilgt,FSF} = \hat{v}_{ilgt,noFSF}$.

Let N_t be the number of students in year t . We estimate average student welfare (in utils) in year t for each of 3 cases.⁴³

⁴²In further counterfactuals, we let FSF projected funding vary in between and beyond these points.

⁴³We maintain that students’ choice sets are the schools in the borough the students lives in. We avoid

1. No FSF Funding and No Supply Response:

$$\hat{w}_{1t} = \frac{1}{N_t} \sum_{i=1}^{N_t} \frac{1}{S} \sum_{s=1}^S \ln \left\{ \sum_{l \in \hat{C}_{s,t,noFSF}} \exp(\hat{v}_{ilgt,noFSF}) \right\} \quad (15)$$

2. FSF Funding and No Supply Response:

$$\hat{w}_{2t} = \frac{1}{N_t} \sum_{i=1}^{N_t} \frac{1}{S} \sum_{s=1}^S \ln \left\{ \sum_{l \in \hat{C}_{s,t,noFSF}} \exp(\hat{v}_{ilgt,FSF}) \right\} \quad (16)$$

3. FSF Funding and Supply Response:

$$\hat{w}_{3t} = \frac{1}{N_t} \sum_{i=1}^{N_t} \frac{1}{S} \sum_{s=1}^S \ln \left\{ \sum_{l \in \hat{C}_{s,t,FSF}} \exp(\hat{v}_{ilgt,FSF}) \right\} \quad (17)$$

We then estimate the reform's impact on welfare (in utils) as $\hat{w}_{3t} - \hat{w}_{1t}$ and the reform's hypothetical impact on welfare were there no exit response as $\hat{w}_{2t} - \hat{w}_{1t}$. We then divide these impacts by $|\hat{\alpha}_1|$ from Section 8.1 to convert to monetary units. We report the estimates in panel (b) of Table 10.

To estimate the size of the unconditional transfer that would have produced the same change in welfare as the reform, we sum the estimated welfare change for the 3 years post-reform ($\Delta\hat{w} = \sum_{t \in \{2008, 2010, 2012\}} (\hat{w}_{3t} - \hat{w}_{1t}) / |\hat{\alpha}_1|$) and sum the projected FSF funding ($TotFSF = \sum_{t \in \{2008, 2010, 2012\}} \sum_k FSF_{kt}$). Because $TotFSF$ captures projected funding, we scale it by our range of first stage coefficients (.59, .91) from Section 5.1. Our bounds are then $(\Delta\hat{w}/(0.91TotFSF), \Delta\hat{w}/(0.59TotFSF))$. We make analogous calculations for the welfare change without exit, where we replace \hat{w}_{3t} with \hat{w}_{2t} .

E Appendix: Public School Expenditure

E.1 Funding Changes and School Characteristics

To test how the funding changes correlated with school characteristics, we regress a measure of the policy's impact on school k (y_k) on the demographics of the school's students (X_{1k}) and measures of teacher experience and turnover at the school (X_{2k}). All right-hand-side variables are set to their 2006-07 levels, and we include all schools that educate students in grades K-12:

$$y_k = \phi_0 + \phi'_1 X_{1k} + \phi'_2 X_{2k} + \omega_k. \quad (18)$$

introducing this borough restriction into the notation, but we keep the same market definition as in the estimated model.

Table A1 shows the results for two measures of y_k : an indicator variable for whether the school received additional money from the FSF reform and, conditional on receiving money, the projected funding increase per student. Schools with more students who received free or reduced price lunch and schools with more students with limited English proficiency were more likely to receive additional funding under the reform. We also expect that schools with more inexperienced teachers would receive additional funding because the reform sought to correct funding imbalances that penalized schools with less expensive teachers. We indeed see this pattern, as a school with 10pp more teachers with under three years of experience was 9.7pp more likely to receive funding. The regression that predicts the size of the projected funding increase shows that the projected increase is strongly predicted by the number of students with limited English proficiency, the number of Hispanic students, and measures of teacher certification, experience, and turnover. Because the “winning” and “losing” schools differ statistically along a few characteristics, we use the timing of the reform to separate the reform’s effects from cross-school constant differences.

Despite these differences, the school characteristics do not perfectly predict a school’s funding change from the reform. In particular, most NYC neighborhoods have some relative “winners” and some relative “losers.” We plot this spatial variation in Figure A.3. For each of the two panels, plotting Brooklyn and the Bronx respectively, we divide the borough according to U.S. Census tracts and shade the tract by the 2000 Census median income for households with children. The darker tracts are areas with higher median household income. We then overlay a series of public school locations where the circles are the schools that received money and the triangles are the schools that did not. The size of the circle is proportional to the funding increase. For both boroughs we see that schools that receive money tend to be located in poorer areas, but we still have considerable spatial variation as the “winners” and “losers” are not located in completely different types of neighborhoods. We use this spatial variation in relation to private school locations to see if private schools located near “winners” are more likely to close after the reform.

For comparison, we present private school locations in a similar format. We draw spatial maps of the Brooklyn and the Bronx Census tracts in Figure A.4. The maps shade each census tract according to its 2000 Census median income for households with children, with the darker shades corresponding to higher socioeconomic status. We add circles and triangles to the maps to indicate the locations of private schools with the circles representing schools that closed following the reform and triangles representing schools that did not. The private schools are dispersed throughout the boroughs and locate both in relatively high-income and relatively low-income areas. Some of these schools serve students who may not be able to afford a large tuition increase and who may be on the margin of attending a public or private school.

E.2 Expenditure of Funds

We explore the mechanisms that led to the large demand shifts by examining how the “winners” used their additional funds. We use the School-Based Expenditure Reports to compare expenditures across different categories for “winners” and “losers.” For each ex-

penditure category c , we regress a school's expenditure on the school's budget change due to the FSF reform and a set of school and year fixed effects:

$$Expend_{kt}^c = \delta_k^c + \tau_t^c + \pi^c FSFChange_{kt} + \eta_{kt}^c \quad (19)$$

The π^c coefficient captures what fraction of each additional dollar from the FSF reform is spent in category c , relative to expenditure in schools that did not receive additional money. We divide expenditure into seven categories: Teachers, Other Classroom Instruction, Instructional Support Services, Administrators, Other Direct Services, Field Support, and System-Wide Costs. Of these categories, we expect that spending on Teachers would have the largest impact on a school's quality, followed by spending on Other Classroom Instruction and Instructional Support Services. Spending on Field Support and System-Wide Costs are likely less related to a school's quality.

We present the results in Table A5a and find that for each additional dollar a school received from FSF \$0.56 went to teacher salaries and benefits. Not only is a large fraction of the additional funding spent on teachers, but the budget increase is disproportionately spent on teachers relative to teachers' share of expenditure before the FSF reform (0.36). Schools also spend \$0.17 and \$0.10 of each additional dollar on Other Classroom Instruction and Administrators, respectively. If we sum across all of the categories (odd columns), we estimate that the total change in expenditure is \$0.76 per dollar of increased funding.

E.3 Changes in Teacher and Classroom Characteristics

We run similar regressions where instead of using category expenditure as our outcome we look at the effect of additional funding on teacher and classroom characteristics. In Table A5b we present the results. We find that a school that received a projected funding increase of \$1,000 per student increased its number of teachers by 4.1 after the reform. At the same time, we find these schools' teachers tend to be more experienced and that class size falls slightly. Using teacher-level data from the New York City Department of Education, we find that for each projected funding increase of \$1,000 per student, a school's average annual teacher salary increased by \$837.

F Appendix: Achievement Calculations

F.1 Estimating Public School Value-Added

We use standard methods to estimate a public school's value-added. For student i at public school k in grade g and year t , we estimate a separate regression for each subject s (math or ELA):

$$\begin{aligned} y_{i,k,g,t}^s = & \beta_1 y_{i,g-1,t-1}^{math} + \beta_2 (y_{i,g-1,t-1}^{math})^2 + \beta_3 (y_{i,g-1,t-1}^{math})^3 + \beta_4 y_{i,g-1,t-1}^{ela} \\ & + \beta_5 (y_{i,g-1,t-1}^{ela})^2 + \beta_6 (y_{i,g-1,t-1}^{ela})^3 + X_i' \beta_7 + \theta_{k,g,t}^s + \epsilon_{i,k,g,t}^s. \end{aligned} \quad (20)$$

A student's test score, $y_{i,k,g,t}^s$, is standardized so that scores across a subject-grade-year for public school students have mean 0 and standard deviation 1. X_i variables are indicators for male, black, Hispanic, English-language learners, special education students, and free or reduced price lunch students. We use the estimated school-grade-year fixed effects and take their mean (across grades) as our value-added measures.

F.2 Estimating Private School Value-Added

We construct a private school's value-added by comparing a cohort's mean score on the grade 8 tests to its mean score on the grade 4 tests four years earlier. We recover the estimated school fixed effect (θ_j^s) from the following regression for private school j :

$$\bar{y}_{j,8,t}^s = \alpha \bar{y}_{j,4,t-4}^s + \mu_t^s + \theta_j^s + \epsilon_{j,g,t}^s \quad (21)$$

where $\bar{y}_{j,g,t}^s$ is the (standardized) average test score at private school j for grade g in year t . We then divide the estimated school fixed effect by 4 to convert from a four-year value-added measure to an annual measure. Note that our value-added measure does not vary with time. While a school's quality may fluctuate over time and even respond to market changes, the sparseness of our data limits our ability to analyze how quality changes over time.⁴⁴ Our estimates thus average over multiple years.

G Appendix: Model Simplifications

G.1 No Capacity Constraints

In our empirical analysis, we assume that no schools face capacity constraints. While this assumption likely does not hold for all schools, aggregate enrollments in NYC are declining during much of the sample period, so on average schools' capacity constraints are likely to be loosened. Collecting data on individual schools' capacity constraints, however, can be a challenge. We do not know of any data on private schools' capacity constraints. For public schools, we have some limited data on school capacities from NYC's 2005-06 and 2007-08 "Enrollment – Capacity – Utilization" reports. These reports use a building's room configurations and a formula for the number of students per room to calculate a building's capacity.

We first discuss how public school capacity constraints might affect our results. If the public school "winners" were capacity constrained prior to the FSF reform, then we would likely underestimate the demand shift toward these schools because the observed enrollment change would be less than the unobserved shift in latent demand. In this case, the total and direct enrollment effects might be underestimated. If the public school "relative losers"

⁴⁴Our data begin in the 2000-01 so our earliest private school value-added estimates come from 2004-05. We are missing private school average test score data for 2006-07 and 2007-08. We thus only have value-added estimates from the 2004-05, 2005-06, and 2008-09 fourth grade cohorts.

were more likely to be capacity constrained prior to the FSF reform, then we would likely overestimate the demand shift toward the “winners.”

To assess whether these possible biases are likely, we use our limited public school capacity data. We find that 35% of “winners” had pre-reform enrollments exceeding their capacities while 19% of “relative losers” had pre-reform enrollments exceeding their capacities. The average utilization rate was 87% of capacity. Some schools exceeded their nominal capacities; therefore, the capacities were not necessarily binding. The average over-capacity school exceeded its capacity by 19%, and some schools that looked capacity constrained according to the data still saw their enrollments increase over time.

Private schools’ exit decisions should not be sensitive to capacity constraints because constraints only bind when demand hits an upper bound while exit depends on demand hitting a lower bound. But the estimation of the direct and indirect effects could be sensitive to the presence of capacity constraints. If a school is capacity constrained, then we are likely to underestimate its δ_j in our demand model.⁴⁵ If we underestimate δ_j for a school that closed, then we would attribute more of the total enrollment effect to the direct effect than we should. Thus, we would underestimate the indirect effect. If we underestimate δ_j for a school that remained open, then we might over or under predict the direct effect. We would over predict the direct effect if school j remained capacity constrained even after the reform. We would under predict the direct effect if school j was capacity constrained before the reform, which led to an underestimate of δ_j , but no longer capacity constrained after the reform. In this case, we would predict too few students switching to school j .

Whether capacity constraints are binding for private schools is difficult to determine without data. But even the elite prep schools, which we might expect to be the most capacity constrained, often do not have wait lists.⁴⁶

G.2 Private Schools’ Characteristics Held Fixed

This paper focuses on private schools’ supply responses along the extensive margin of whether to open or close. Schools could make other supply decisions and we consider these beyond the scope of this paper. In our demand model we assume that private schools’ characteristics remain constant over time (other than sector-wide changes). If schools actually adjust their total quality then our demand estimates could be inconsistent. Note that we might over- or underestimate the indirect effect because it is theoretically ambiguous as to whether schools would optimally increase or decrease total quality. To sign this bias, we would need a fully-specified supply model that includes schools’ cost of providing quality.

Assessing whether schools adjusted their characteristics in response to the reform is difficult because we lack complete panel data. We therefore use our partial panel data on

⁴⁵The estimate depends not just on the own school’s capacity constraint but also those of neighboring schools and the general competitive structure of the local schooling market. These statements are loose descriptions of first-order effects.

⁴⁶Among the NY elite prep schools that appear in the 2007 edition of Peterson’s Guide to Private Secondary Schools, 36% do not report turning any prospective students away and 48% have admissions rates above 80%.

achievement and school assets and income, collected from IRS Form 990. We present the results in Appendix Table A17. The estimated regressions show no clear patterns between increases in FSF funding at local public schools and changes in private school characteristics, conditional on the schools remaining open. That said, the standard errors are very large and thus we acknowledge that our data are too coarse to rule out even large changes in private schools' characteristics.

G.3 Students' Choice Sets Include All Schools in the Borough

In our demand model, a student's choice set includes all public and private schools in her borough. We do not let students choose from schools in another borough. This constraint is violated in the data only rarely. Among public school elementary (middle) students, just 1.8% (3.0%) attend public schools in another borough for the 2007-08 school year. By comparison, high school students, who we do not include in our estimation, are more likely (16.8%) to attend public schools in other boroughs.

The more relevant consideration may be whether our model gives students too many options. It is unlikely families consider every public and private school in their borough.⁴⁷ While the large estimates for the disutility to distance and the utility to attending the zoned school should make far away options have small probabilities of being chosen, the logit functional form could inflate probabilities for unlikely events. To the first order, the logit functional form should then predict more sorting across the borough than is realistic. For instance, when a private school closes, we might over predict the number of students who would then switch to a far-away school. This over prediction, though, should mostly add noise to our model results, which compare outcomes at schools ("winners" vs. "losers") with different changes in their local competitors. The extent to which the functional form smooths out local differences would lead us to underestimate such results.

H Appendix: Alternate Specifications

We provide a variety of alternate specifications in the appendix tables. We provide more details on their construction here.

While we see limited evidence of pre-trends in the enrollment regressions, we might generally be worried that schools' projected funding changes might depend on short-term enrollment trends if the old funding formulas are slow to update to changes in student characteristics. In Appendix Table A6 we thus estimate enrollment regressions leaving out the last year before the reform's implementation and the first year after. We find the results largely unchanged and if anything, more statistically significant.

Our difference-in-difference framework should control for time-invariant heterogeneity, but in understanding the reform's effects, we might be interested in which private schools

⁴⁷We do see students traveling outside of their subdistrict to attend public school. Among public school elementary (middle) students, 11.9% (19.2%) attend public schools in another subdistrict for the 2007-08 school year. We allow for such attendance choices in the model.

are most exposed to additional public school funding. An observation in this analysis is a private school. We estimate its reform exposure as the average projected funding increase (\$1,000s per student) at the five closest public schools (“Mean FSF * Dist1to5”). We present the results in Appendix Table A9. We find higher exposure among Catholic schools, schools with high student-teacher ratios, lower tuition, and higher fractions of Hispanic students.

Instead of using a public school as the unit of analysis, we include a specification where we pool to the zip code, even if this washes out some of the FSF variation. We run two difference-in-difference specifications where we differ in how we define the FSF treatment. The first, “Total FSF,” calculates the total projected FSF funding change (in \$1,000s) at the zip code level and divides by total public enrollment in the zip code. The second, “Mean FSF,” takes the FSF variable as defined in the text for each school and takes an unweighted average within the zip code. Results, in Appendix Table A11, are similar across both alternatives and qualitatively consistent with the rest of the paper.

As mentioned in Section 5.1, the projected funding change was partially implemented. While we continue to present the reduced form results throughout the paper, in Appendix Table A12 we present IV results where we instrument for actual budget per student with projected funding per student. Note that we can only use years 2006-07 and later and thus lose considerable precision.

The supply regressions use the count of nearby private schools as the dependent variable. In Appendix Table A13 we present results from Poisson regressions.

In Appendix Table A15 we regress nearby private or charter enrollment on the FSF variation. We find noisy estimates that are qualitatively consistent with our other results. We use the effect on private school enrollment when comparing our results to Hoxby (2001).

To establish that students choose private schools at least partly based on geography, we regress each private school’s enrollment on the enrollment of all other private schools in the same zip code for that year and present results in Appendix Table A18. We include zip code fixed effects, so we characterize how deviations in a zip code’s private enrollment relate to a school’s own enrollment. We think of these regressions in the spirit of peer effects regressions where test scores are regressed on leave-out classroom mean test scores. If schools within a zip code operated in completely independent environments, we would expect a 0 coefficient in this regression. If schools have correlated shocks (e.g., local income shock) we would expect a positive coefficient. And if schools compete locally, we would expect spatial spillovers and a negative coefficient. This last case seems to dominate, which we take as indirect evidence that schools compete locally because students tend to choose from schools in a narrow geographic area.

In Table A14, we investigate private school exit by treating the private school as the unit of observation. In these probit regressions, an observation is a private school that was open in 2006-07 and the outcome is whether the school exited by 2013-14 (unless indicated otherwise in the “Years” row). For each private school, we match it to up to the 5 closest public schools (unless indicated otherwise in the “Max Number of Matches” row) within a 3-mile radius. We then calculate “Mean FSF” or “Mean Hyp. Neg. FSF” as the average projected funding change (post 2007-08) in thousands of dollars per student and the average amount held

harmless in thousands of dollars per student. For the versions interacted with “Distance,” the averages are weighted by the distances between the public and private schools.

In some specifications we include public school controls. These are unweighted means across the public school matches for the following variables: indicators for each subdistrict, percentages of students who are black, Hispanic, have limited English proficiency, and have been held back a grade.

Columns 1 and 2 present results from the mean projected funding change at the closest 5 public schools. Column 3 splits out the results by whether the public school is the closest, second closest, third closest, etc. Column 4 includes up to 10 public school matches. Columns 5 and 6 include distance-weighted measures of the funding change. Column 7 matches private schools to public schools of the opposite level (high schools to elementary schools and vice versa). Column 8 assesses whether FSF funding changes predict closures from *before* the reform.

A Appendix Figures

Figure A.1: Example School Budgets in 2007-08

Figure A.1a: School that Gets Additional Funding

School: P.S. 189 Lincoln Terrace

I. Old Approach	\$5,354,931
II. Fair Student Funding (FSF) Approach	\$6,227,823
Difference	\$872,892
III. Actual Budget	
Amount Under Old Approach	\$5,354,931
New FSF Allocation (55% of Difference up to \$400,000)	+ \$400,000
FSF Subtotal	= \$5,754,931
Other Funding	+ \$2,740,999
FY08 Budget	= \$8,495,930

Figure A.1b: School that Does Not Get Additional Funding

School: J.H.S. 045 William J. Gaynor

I. Old Approach	\$2,833,949
II. Fair Student Funding (FSF) Approach	\$1,980,306
Amount held harmless for:	\$853,643
III. Actual Budget	
FSF Formula Allocation	\$1,980,306
Hold Harmless Allocation	+ \$853,643
FSF Subtotal	= \$2,833,949
Other Funding	+ \$1,421,191
FY08 Budget	= \$4,255,140

Figure A.2: Breakdown of an Example School's FSF Funding Sources

School: P.S. 189 Lincoln Terrace

I. Foundation		\$200,000
II. Enrollment Funding	# Students	
K-5 Students	730	\$2,765,240
6-8 Students	374	\$1,530,034
III. Needs		
Poverty	942	\$856,278
Achievement Below Standards	0	\$0
Achievement Well Below Standards	0	\$0
ELL K-5	175	\$265,125
ELL 6-8	103	\$195,082
Special Education Services	91	\$416,064
IV. Total FSF Formula		\$6,227,823

Figure A.3: Locations of Public Schools

Figure A.3a: Public Schools in Brooklyn by HH Income

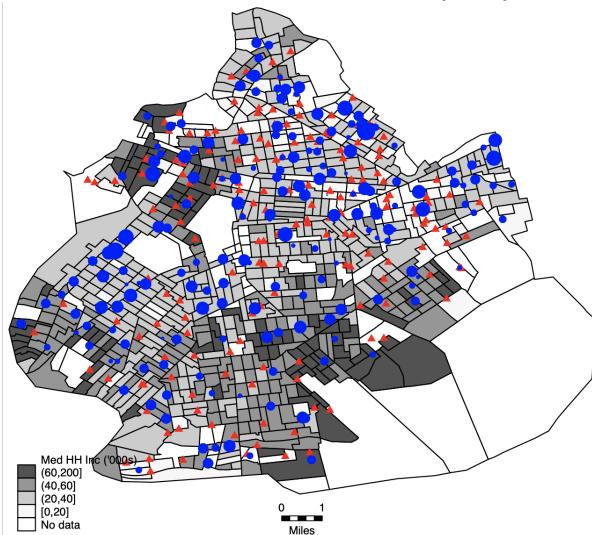
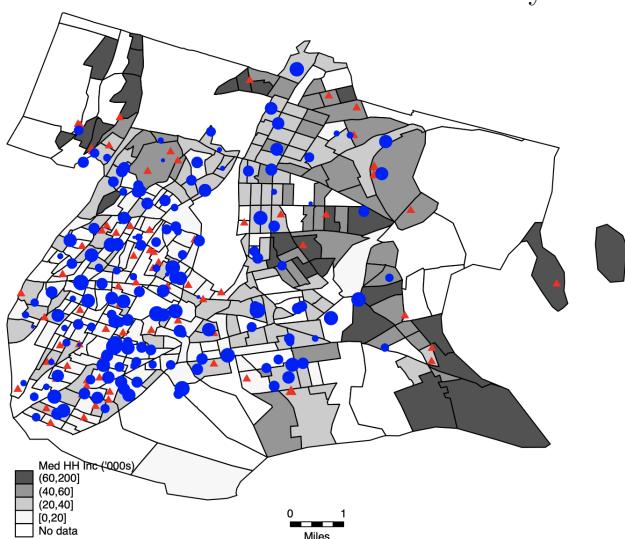


Figure A.3b: Public Schools in the Bronx by HH Income



Note: U.S. Census tracts are shaded according to 2000 Census median income for households with children. The circles are the public schools that received money and the triangles are the public schools that did not. The size of the circle is proportional to the funding increase.

Figure A.4: Locations of Private Schools

Figure A.4a: Private Schools in Brooklyn by HH Income

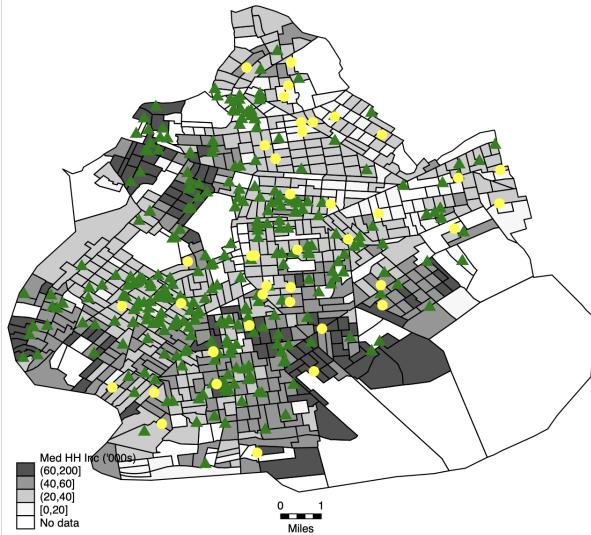
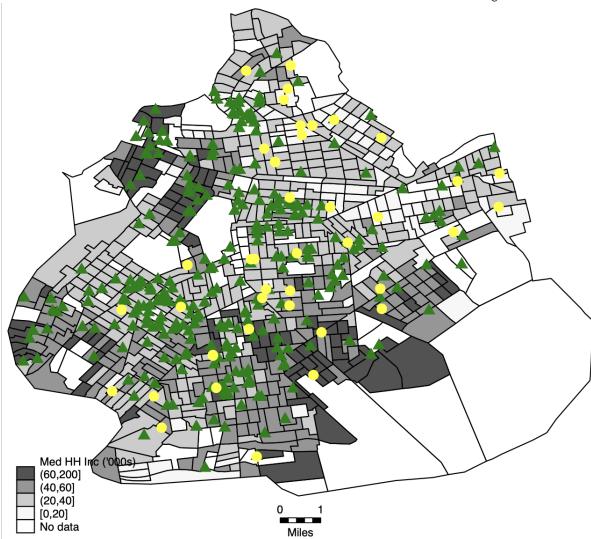
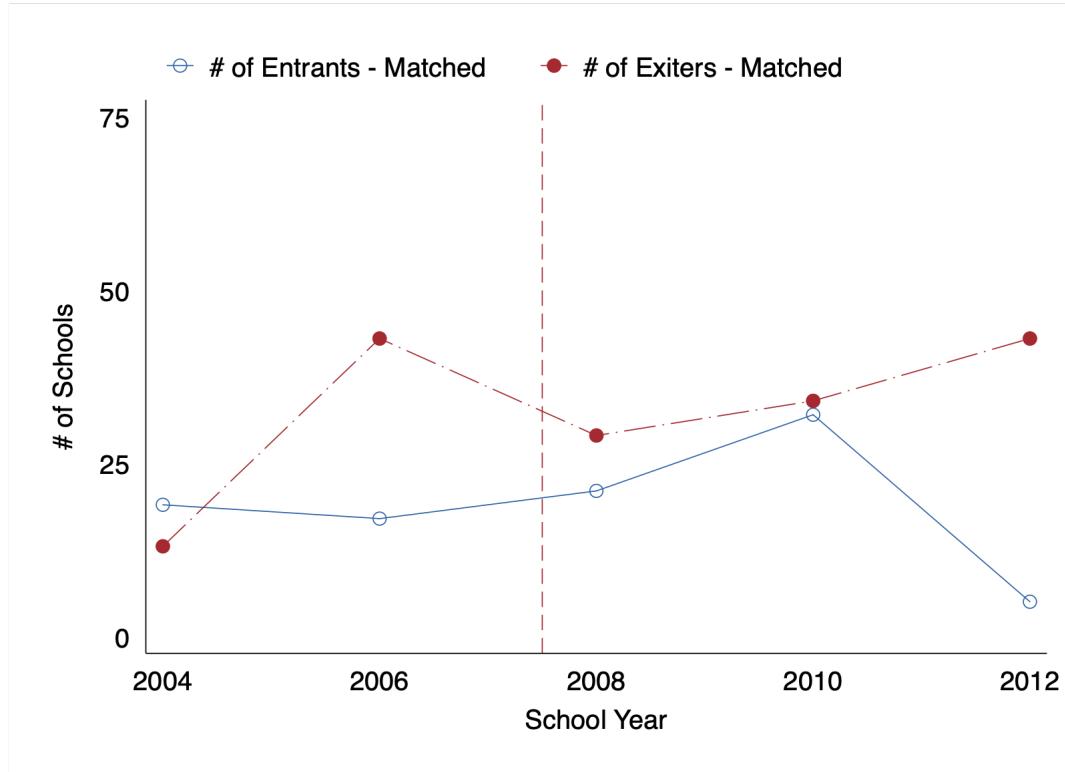


Figure A.4b: Private Schools in Bronx by HH Income



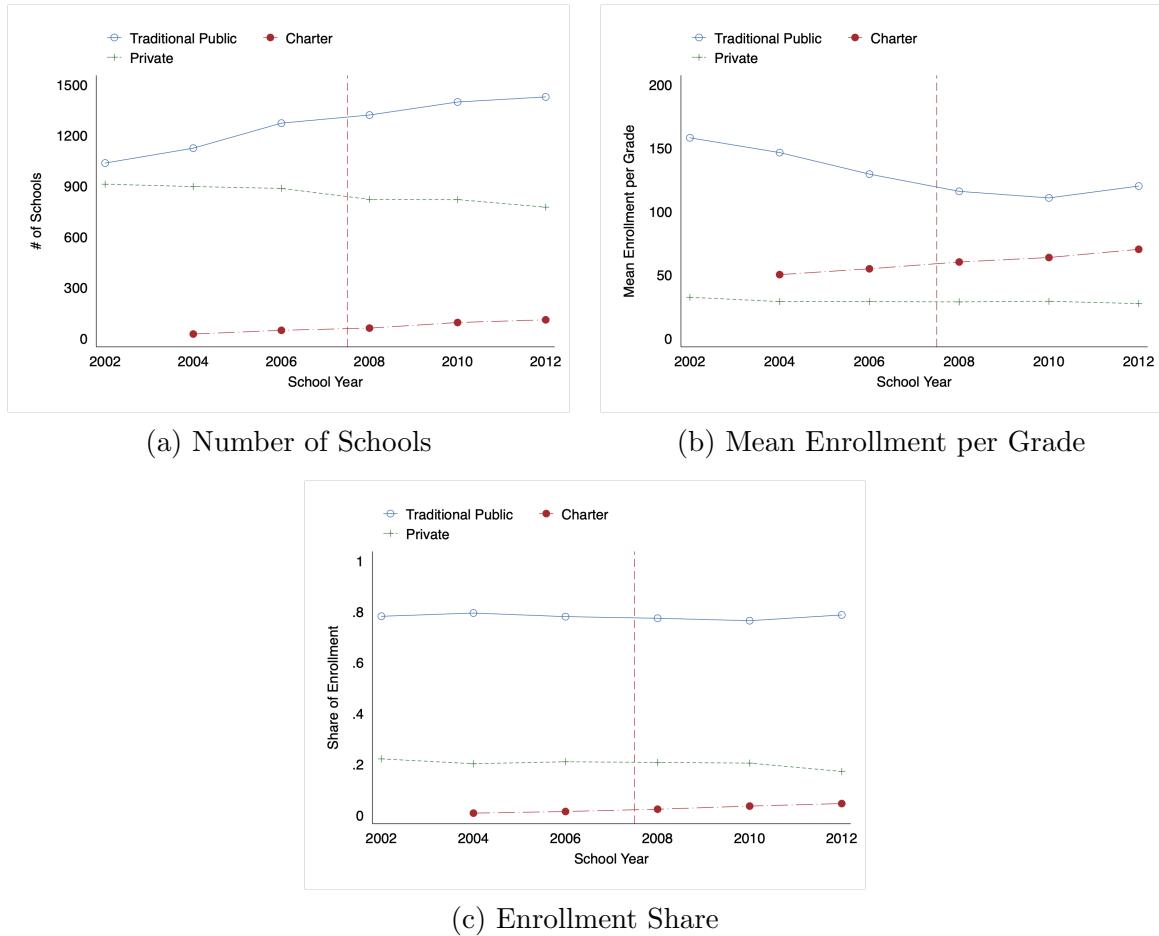
Note: U.S. Census tracts are shaded according to 2000 Census median income for households with children. The green triangles represent private schools that were open in 2006-07 that did not close in the next six years and the yellow circles are schools that did close in the next six years.

Figure A.5: Number of Entrants and Exiters in NYC – Matched Sample



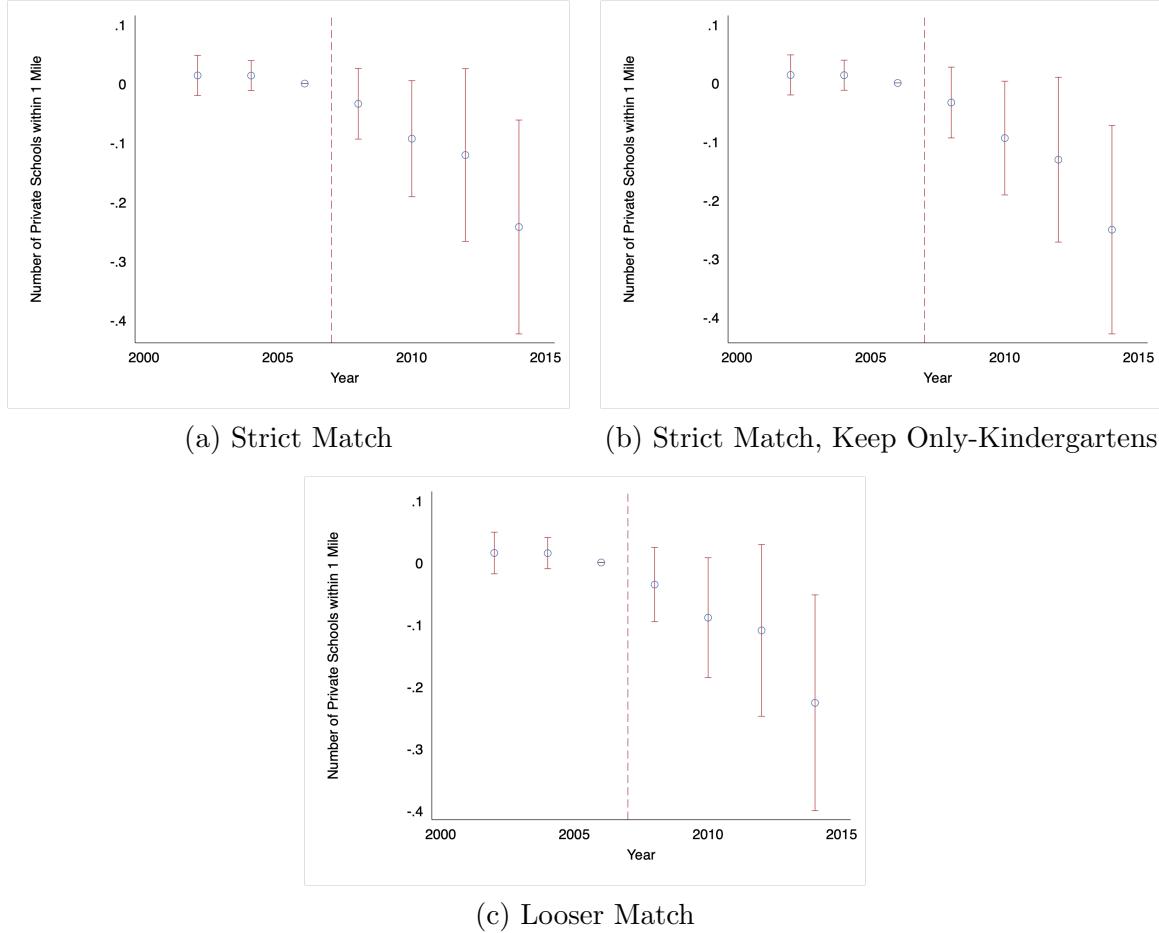
Note: The sample is the set of private schools in our estimation sample. These schools are in the Private School Survey (PSS) and match uniquely to the NYSED data. The PSS data come out every other year, so entry and exit refer to actions taken over two-year periods. The red line marks the implementation of the FSF reform. Years on the x-axis refer to the spring of the school year (i.e., 2008 is the 2007-08 school year).

Figure A.6: Number of Schools, Mean Enrollment, and Market Share by Sector



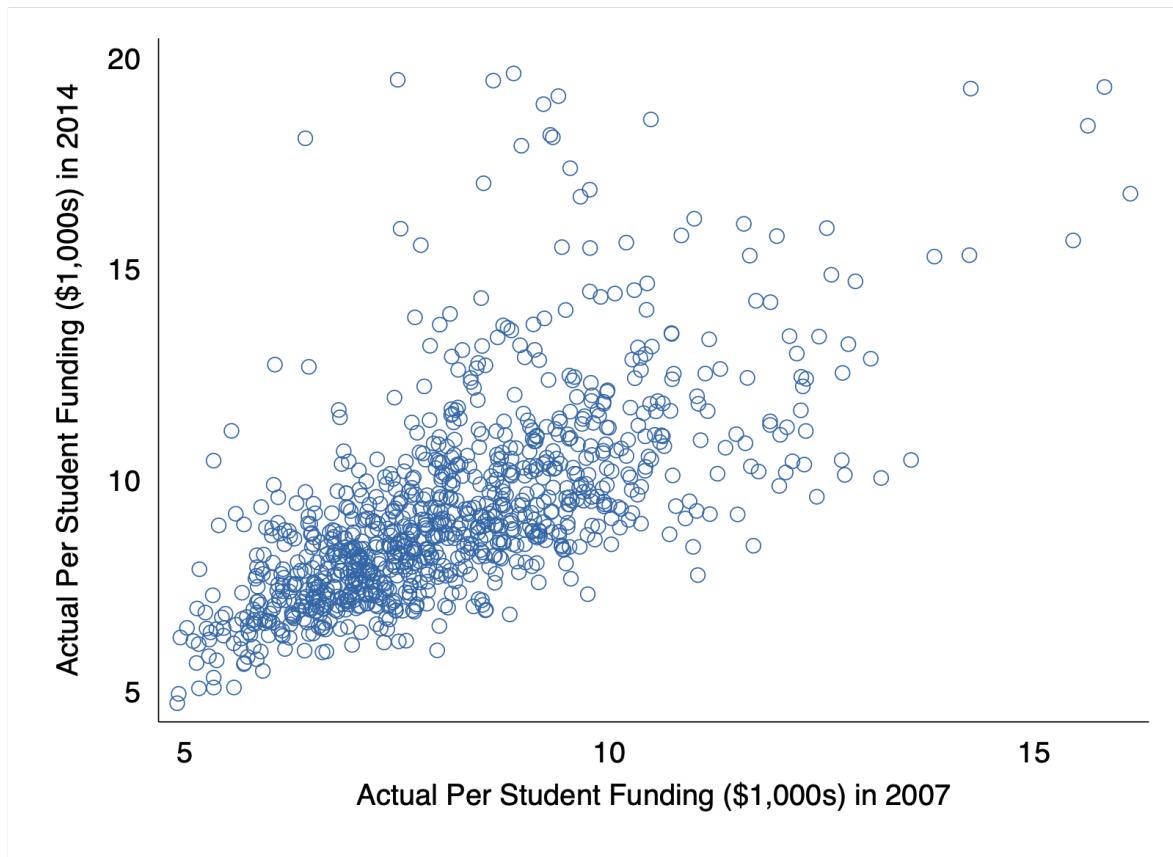
Note: Figure shows the number of schools, mean enrollment per grade, and enrollment market share by sector (traditional public, charter, private) over time.

Figure A.7: Supply of Schools Event Studies – Robustness



Note: Figure shows estimated coefficients (and 95% confidence intervals) on projected FSF funding (\$1,000s/student) for each two-year period from the difference-in-difference regression of number of private (or charter) schools within 1 mile of a public school on projected FSF funding, school fixed effects, and year fixed effects. The 2005-06 – 2006-07 coefficient is normalized to 0. The regressions drop private schools for which the Private School Survey (PSS) and NYSED data disagree on the first or last year the school was open within the sample (up to 1 year given that the PSS is every other year) and schools for which the enrollment per grade in the PSS minus the enrollment per grade in the NYSED data ever falls below the 5th percentile (-4.25) or above the 95th percentile (3.23). If a school's difference in enrollment per grade across the data sets falls outside this range in any year, the school is dropped from the sample for all years. The "Strict Match" sample includes schools in the PSS that match uniquely to the NYSED data. The "Strict Match, Keep Only-Kindergartens" expands this sample to include schools that end at Kindergarten. "Looser Match" includes schools in the PSS that match to the NYSED data, even if the match is not unique (i.e., multiple PSS schools match to the same NYSED school, as sometimes happens when schools have different campuses).

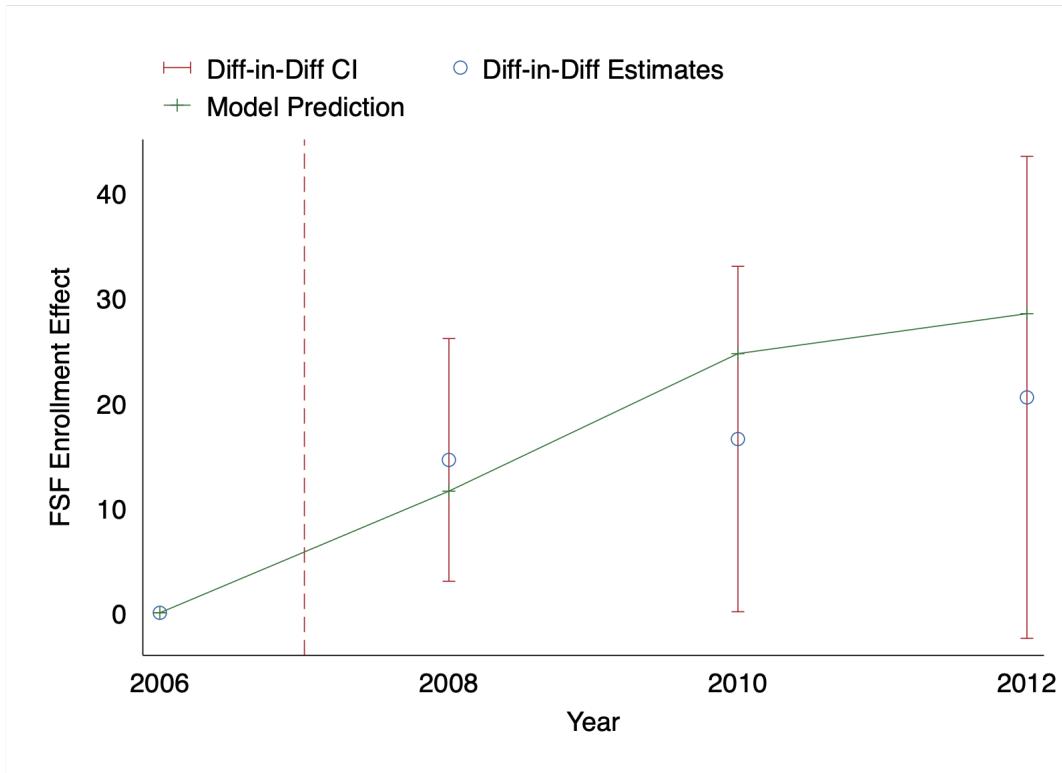
Figure A.8: Public Schools' Per Student Budgets Before and After FSF Implementation



Note: Figure plots a school's per student funding in 2007 against its per student funding in 2014. While some schools reorder across years, there is a high level of persistence.

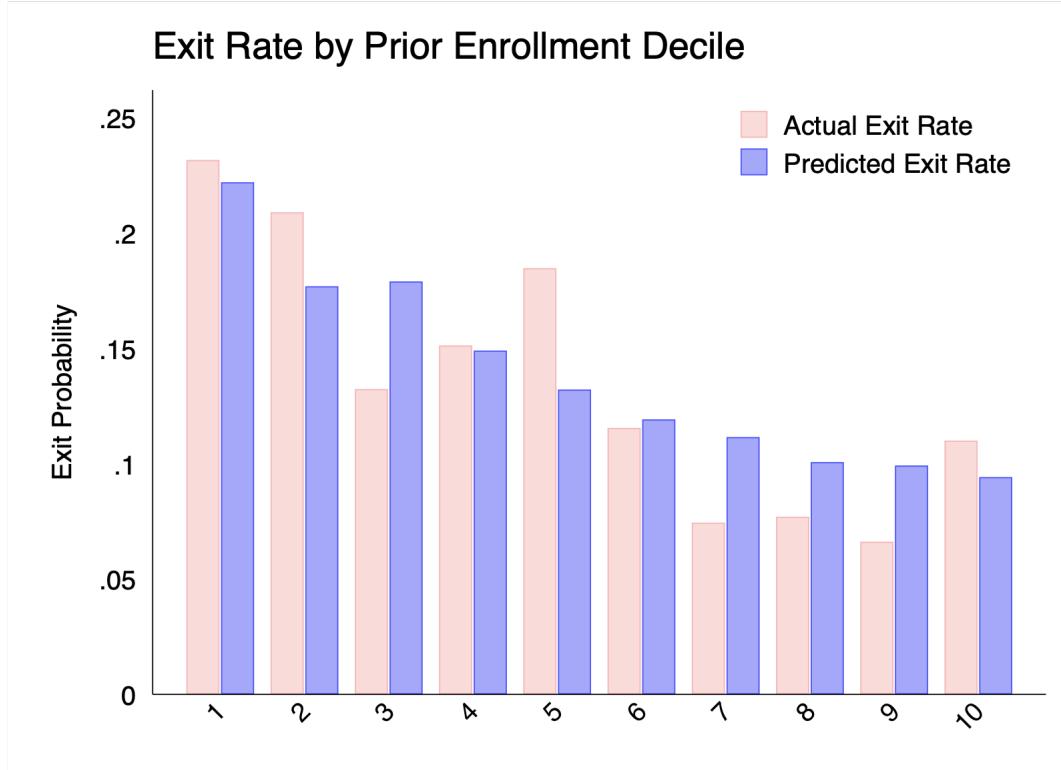
In Appendix Figure A.8, we plot a school's per student funding in 2007 against its per student funding in 2014. We see some reordering of schools though also some persistence.

Figure A.9: Demand Model Estimates Compared to Difference-in-Difference Estimates of FSF Enrollment Effects



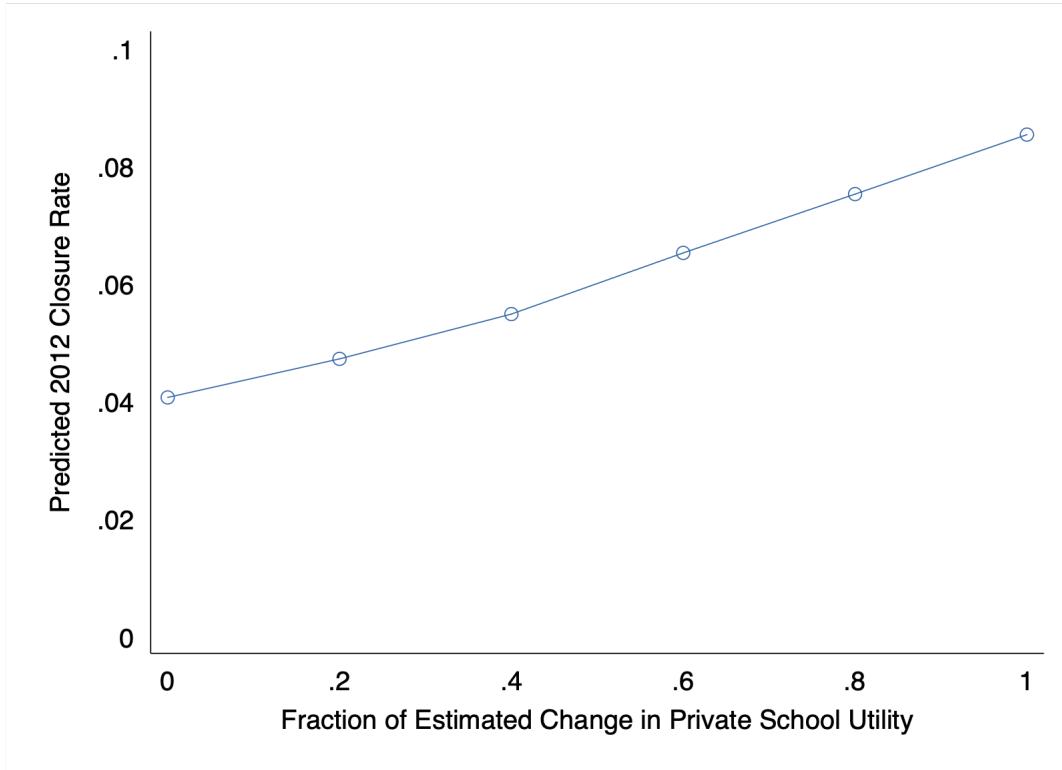
Note: Figure depicts the model prediction for the impact of the FSF reform on the enrollment of “winners” relative to “losers.” With the model, we predict enrollments with and without the reform, where the reform affects both student utility from attending schools with increased projected funding and which private schools are open. The dots show the point estimates from a difference-in-difference specification where the outcome is enrollment and the treatment is whether the school is an FSF “winner” (after 2006).

Figure A.10: Actual and Estimated Closure Rates by Prior Enrollment Decile



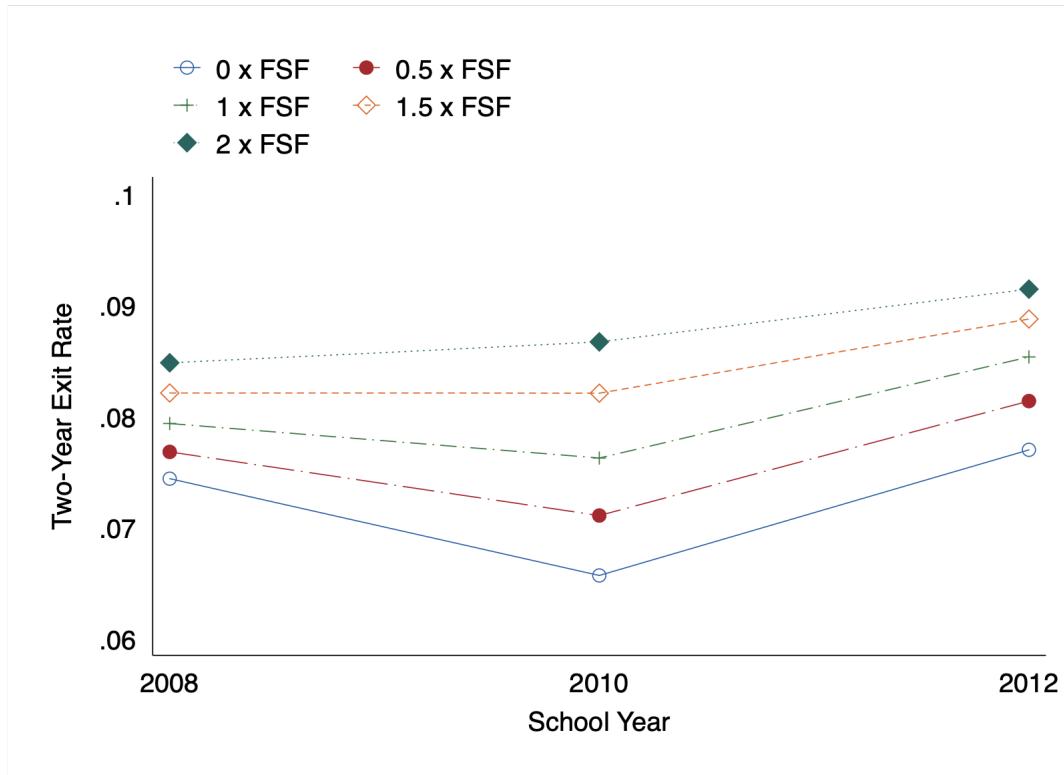
Note: Figure compares the model-predicted and actual two-year closure rates for private schools depending on the decile of their enrollment two years prior. Decile 1 includes the schools in the bottom 10% of enrollment two years prior.

Figure A.11: Private School Closure Rate and FSF Impact on Sectoral Utility



Note: Figure depicts the model predicted private school rate for 2012 as a function of the sectoral utility. The x-axis is expressed as a fraction of the estimated τ_{5g} .

Figure A.12: Estimated Closure Rates as a Function of the Size of the FSF Reform



Note: Figure depicts the model predicted private school two-year closure rates for different funding increases. The increases are multiples of the FSF reform's projected funding increase, with "1 x FSF" indicating the FSF reform and "2 x FSF" indicating a reform where projected increases are twice as large.

B Appendix Tables

Table A1: Regressions of Funding Change on Public School Demographics and Teacher Characteristics

	Mean	Regressions	
		1(Projected Funding Change > 0)	Projected Funding Change per Student
% Free + Reduced Lunch	0.73	0.199*** (0.062)	30.804 (60.940)
% Stability	0.90	-0.154 (0.221)	-182.724 (188.537)
% Limited English Proficiency	0.14	0.637*** (0.131)	222.085** (105.693)
% Black	0.35	-0.100 (0.066)	63.325 (67.864)
% Hispanic	0.40	-0.036 (0.078)	211.149*** (70.813)
% Teacher No Valid Certificate	0.06	0.598 (0.407)	-207.612 (343.724)
% Teacher without Certification	0.11	-0.153 (0.243)	139.284 (210.738)
% Teachers < 3 Years Experience	0.19	0.965*** (0.118)	440.646*** (101.545)
% Teacher Turnover (within 5 Years)	0.21	-0.191 (0.132)	-345.619** (151.209)
% Turnover (All)	0.18	-0.031 (0.215)	604.183*** (217.449)
Constant		0.300 (0.217)	288.770 (189.184)
N		1,222	615
R-Squared		0.129	0.141

* < 10%, ** < 5%, *** < 1%. The last two columns are regressions of projected funding change measures on a public school's demographic and teacher characteristics in 2006-07. The left-hand-side of the first regression is an indicator for whether the public school was projected to receive money. The left-hand-side of the second regression is the projected funding increase per student and is limited to schools with projected increases. The % Stability is a NY State measure that captures the percentage of students who are in the grade normally associated with a certain age. The dependent variables come from NYC Department of Education data on school budgets in 2007-08. The right-hand-side variables are drawn from NYSED School Report Cards.

Table A2: Traditional Public, Charter, and Private School Summary Statistics – 2006 School Year

School Characteristics	Traditional Public Schools		Charter Schools		Private Schools		
	Common Core	Common Core	PSS	NYSED	Estimation Sample (PSS and NYSED Match)	Entrants (2005- 2006)	Exiters (2007- 2008)
Number of Schools	1,050	44	688	693	584	15	26
% Catholic			39%	39%	43%	20%	50%
% Other Religious			44%	40%	40%	47%	31%
% Non-R eligious			17%	21%	17%	33%	19%
Enrollment per Grade (Mean)	127.68	54.48	31.40	32.10	32.43	26.73	14.76
% with Enrollment per Grade < 10	0%	0%	13%	12%	9%	13%	15%
% with Enrollment per Grade < 20	1%	0%	34%	35%	30%	53%	81%
% Black	32%	68%	18%		19%	19%	40%
% Hispanic	40%	26%	15%		17%	11%	23%
% of Schools with >50% Minority	75%	98%	42%		43%	53%	73%

Traditional public and charter school data come from the Common Core of Data. Private school data comes from the Private School Survey (PSS) and New York State Education Department (NYSED) data. 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 sixth column includes schools in our estimation sample that enter in 2005 or 2006. The seventh column includes schools in our estimation sample that exit in 2007 or 2008. These entrants and exiters were all active in 2006. Minority students are black or Hispanic.

Table A3: Matching between the PSS and NYSED

<u>Panel A: Private School Survey</u>		
	Matches	Non-Matches
Number of Schools (Strict)	816	186
Number of Schools (Loose)	847	155
Enrollment (Mean)	297	267
Number of Teachers (Mean)	25	23
Catholic	43%	15%
Other Religious	40%	63%
Non-Religious	18%	22%

<u>Panel B: New York State Education Dept Data</u>		
	Matches	Non-Matches
Number of Schools (Strict)	741	188
Number of Schools (Loose)	846	83
Enrollment (Mean)	340	262
Catholic	39%	14%
Jewish	20%	37%
Other Religious	17%	11%
Non-Religious	24%	39%
Entrant (First Year After 2001)	8%	25%
Exiter (Last Year Before 2014)	17%	35%
The Bronx	13%	11%
Brooklyn	37%	51%
Manhattan	21%	13%
Queens	23%	18%
Staten Island	7%	7%

<u>Panel C: Data Differences</u>				
	Mean	Median	5th Percentile	95th Percentile
Difference in Enrollment per Grade	-0.21	0.00	-4.25	3.23

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2011-12 school years. Characteristics of matched and non-matched schools are based on the strict match. Difference in enrollment per grade is calculated as the PSS enrollment per grade minus the NYSED data enrollment per grade. The number of PSS schools differs slightly between the strict and loose match because the loose match includes preschools that have a kindergarten grade. The number of matched schools differs slightly between the PSS and NYSED because we implement our sample selection criteria (e.g., elementary and middle schools; schools that were active between 2002 and 2012) separately by data set to be able to compare matches versus non-matches for a given dataset. In the reduced form analysis, we use the enrollment variables from the PSS and the entry and exit variables from NYSED to determine sample eligibility.

Table A4: Number of Schools, Mean Enrollment, and Enrollment Share by Sector

	2002	2004	2006	2008	2010	2012
<u>Number of Schools</u>						
Traditional Public	1,034	1,122	1,270	1,318	1,395	1,425
Charter	0	23	45	58	91	107
Private	909	895	884	819	818	773
<u>Mean Enrollment per Grade</u>						
Traditional Public	158	146	129	116	110	120
Charter	0	50	54	60	63	70
Private	32	29	29	28	29	27
<u>Share of Enrollment</u>						
Traditional Public	0.78	0.79	0.78	0.77	0.76	0.79
Charter	0.00	0.01	0.01	0.02	0.03	0.04
Private	0.22	0.20	0.21	0.21	0.20	0.17

The table shows how number of schools, mean enrollment per grade, and enrollment share change over time, by sector. The sample is all schools in the Common Core (traditional public and charter) and the Private School Survey (private). We include even schools that we cannot match to the NYSED private school data to avoid understating private shares.

Table A5: Expenditure and School Characteristics Regressions

Table A.5a: Regressions of Expenditure Categories on FSF Change

	Other Teachers	Other Classroom Instructio n Teachers	Other Classroom Instructio n n	Instructional Support Services	Instructional Support Services	Administrators	Administrators	Other Direct Services	Other Direct Services	Field Support	Field Support	System- Wide Costs	System- Wide Costs	
FSF	0.557*** (0.098)	0.548*** (0.098)	0.170*** (0.045)	0.128*** (0.046)	-0.189** (0.087)	-0.076 (0.094)	0.095** (0.044)	0.153*** (0.048)	0.119 (0.093)	0.138 (0.097)	0.007 (0.008)	0.014* (0.008)	-0.002 (0.002)	-0.002 (0.002)
Hyp Neg FSF		0.014 (0.064)		0.063** (0.029)		-0.169** (0.069)		-0.088*** (0.028)		-0.028 (0.046)		-0.009* (0.005)		-0.001 (0.001)
Category's Fraction of Expenditure in 2006-07	0.357	0.357	0.104	0.104	0.120	0.120	0.083	0.083	0.165	0.165	0.019	0.019	0.171	0.171
Year, School Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
N	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759	8,759
R-Squared	0.447	0.447	0.189	0.191	0.252	0.254	0.217	0.219	0.192	0.192	0.130	0.130	0.994	0.994

* < 10%, ** < 5%, *** < 1%. Data span 2004-05 through 2011-12 school years. Each column is a separate regression of an expenditure category on the projected budget change due to the FSF reform. Each regression includes year and school fixed effects. Standard errors are clustered by zip code. "Teachers" refers to salary and benefits paid to teachers. "Other Classroom Instruction" includes spending on other classroom staff, textbooks, librarians, and classroom supplies. "Instructional Support Services" includes services like counseling, drug prevention programs, and after school activities. "Administrators" include salary and benefits for principals, assistant principals, supervisors, secretaries, and school aides. "Other Direct Services" includes spending on ancillary services (food, transportation, safety, computers), building services, and regional support. "Field Support" includes spending on sabbaticals, leaves, termination pay, and salary additions. "System-Wide Costs" includes support for central administration, debt service and retiree benefits, and funds for non-public schools. Data come from NYC DOE line-item expenditures.

Table A.5b: Regressions of School Characteristics on FSF Change

	Number of Teachers	Number of Non-Teachers	% Teachers < 3 Years Experience	% Teachers with MA Degree	Mean Class Size Grades 1-6	Mean Class Size Grade 8	Mean Years in District	Mean Years at School	Mean Teacher Salary
FSF	4.073*** (0.985)	1.327*** (0.330)	-0.086*** (0.017)	0.009 (0.009)	-0.464 (0.399)	-0.697 (0.931)	0.444** (0.193)	0.199 (0.129)	837,417** (381,163)
Dep Var Mean in 2006-07	106.0	17.0	0.12	0.29	21.6	21.2	8.0	5.0	62647.0
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	5,812	5,812	5,812	5,812	4,759	2,126	6,805	6,805	6,805
R-Squared	0.132	0.767	0.454	0.452	0.057	0.023	0.503	0.598	0.823

* < 10%, ** < 5%, *** < 1%. Data span 2004-05 through 2011-12 school years. Each column is a separate regression of a school characteristic on the school's budget change due to the FSF reform. "FSF" is the projected funding change per student (\$1,000s). Each regression includes year and school fixed effects. Standard errors are clustered by zip code. School characteristics come from NYSED Report Cards.

Table A6: Enrollment Regressions – Leave out 2006-07 and 2007-08

	Enroll	In(Enroll)
FSF	36.934 (23.589)	0.161*** (0.041)
Fixed Effects	Year, School	Year, School
N	10,382	10,382
R-Squared	0.115	0.091

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years, leaving out 2006-07 and 2007-08. "FSF" is the projected per-student funding change (in \$1,000s). Standard errors are clustered by zip code.

Table A7: Effect of Funding on Net Switchers by Sector

	Public Net Switchers	Charter Net Switchers	Private/Out of District Net Switchers
FSF	18.993 (12.429)	0.094 (0.506)	8.371*** (2.470)
Fixed Effects	Year, School	Year, School	Year, School
N	11,098	11,098	11,098
R-Squared	0.067	0.130	0.431

* < 10%, ** < 5%, *** < 1%. "FSF" is the projected per-student funding change (in \$1,000s). Net public switchers are the number of students who attended a different public school the prior year (excluding kindergarteners) minus the number of students who left for a different public school. Net charter switchers are the numbers of students who attended a charter school the previous year minus the number of students who left for a charter school. Private/out of district switchers are the number of students who attended a private or out of district school the previous year minus the number of students who left the public school for a private or out of district school. Standard errors are clustered by zip code.

Table A8: Number of Private Schools Regressions – Reporting Fixed Effects

	Num Private Schools w/i 1 Mile	Num Private Schools w/i 1 Mile	Num Private HS w/i 1 Mile	Num Private Non-HS w/i 1 Mile	Num Charter Schools w/i 1 Mile	Num Charter Schools w/i 1 Mile
FSF	-0.201** (0.099)	-0.231** (0.099)			-0.092 (0.355)	-0.072 (0.371)
Hyp. Neg. FSF		0.046 (0.048)				-0.025 (0.109)
Mismatched FSF			0.012 (0.105)	-0.033 (0.201)		
(Year==2001)	0.314*** (0.094)	0.293*** (0.094)	-0.098 (0.089)	0.367* (0.194)		
(Year==2002)	0.334*** (0.096)	0.313*** (0.097)	-0.074 (0.085)	0.381* (0.197)		
(Year==2003)	0.402*** (0.098)	0.381*** (0.099)	-0.027 (0.084)	0.442** (0.197)		
(Year==2004)	0.471*** (0.099)	0.450*** (0.100)	0.042 (0.066)	0.543*** (0.179)	0.221 (0.179)	0.220 (0.179)
(Year==2005)	0.488*** (0.098)	0.467*** (0.099)	0.066 (0.077)	0.543*** (0.179)	0.461** (0.216)	0.461** (0.216)
(Year==2006)	0.517*** (0.092)	0.496*** (0.093)	0.110 (0.076)	0.584*** (0.172)	0.912*** (0.298)	0.912*** (0.298)
(Year==2007)	0.463*** (0.086)	0.442*** (0.087)	0.107* (0.060)	0.529*** (0.152)	1.053*** (0.315)	1.053*** (0.314)
(Year==2008)	0.524*** (0.081)	0.521*** (0.081)	0.141** (0.061)	0.550*** (0.140)	1.107*** (0.307)	1.095*** (0.303)
(Year==2009)	0.489*** (0.068)	0.489*** (0.068)	0.145*** (0.049)	0.498*** (0.109)	1.570*** (0.412)	1.556*** (0.406)
(Year==2010)	0.445*** (0.062)	0.445*** (0.062)	0.166*** (0.043)	0.415*** (0.113)	1.881*** (0.391)	1.867*** (0.384)
(Year==2011)	0.443*** (0.056)	0.443*** (0.056)	0.204*** (0.043)	0.442*** (0.072)	2.208*** (0.393)	2.194*** (0.384)
(Year==2012)	0.255*** (0.041)	0.255*** (0.042)	0.129*** (0.027)	0.273*** (0.066)	2.319*** (0.420)	2.306*** (0.410)
(Year==2013)	0.191*** (0.041)	0.191*** (0.041)	0.107*** (0.026)	0.226*** (0.066)	2.619*** (0.410)	2.606*** (0.399)
(Year==2014)					2.845*** (0.440)	2.831*** (0.429)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,043	11,043	5,079	3,052	3,050	3,050
R-Squared	0.074	0.075	0.050	0.067	0.471	0.471

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 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 (HS or non-HS) from the private schools counted on the left-hand-side. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey 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.

Table A9: Relationship of Private School Characteristics with FSF Exposure

	1(Religious)	1(Catholic)	Student-Teacher Ratio	Enrollment	Fraction Black	Fraction Hispanic	Tuition	Income	Assets	Mean ELA Grade 4 Scores	Mean Math Grade 4 Scores	Mean ELA Grade 8 Scores	Mean Math Grade 8 Scores
Mean FSF * Dist1to5	0.370*	0.595***	9.354***	-91.578	0.017	0.464***	-17.281*	-49,427,041*	-80,524,368	-0.301	-0.090	-0.348	-0.148
	(0.200)	(0.195)	(2.990)	(120.557)	(0.107)	(0.088)	(9.039)	(28,419,304)	(52,080,264)	(0.200)	(0.187)	(0.221)	(0.225)
Constant	0.778***	0.379***	14.979***	368.850***	0.225***	0.106***	11.788***	21,412,568*	35,082,199**	0.347***	-0.048	0.450***	0.202***
	(0.068)	(0.051)	(0.916)	(31.103)	(0.036)	(0.018)	(3.302)	(8,713,747)	(16,092,550)	(0.049)	(0.042)	(0.061)	(0.070)
Distance Radius (miles)	3	3	3	3	3	3	3	3	3	3	3	3	3
Max Number of Matches	5	5	5	10	5	5	5	5	10	5	5	5	5
Public School Controls	No	No	No	No	No	No	No	No	No	No	No	No	No
N	530	530	530	530	530	530	246	66	66	193	193	161	157

* < 10%, ** < 5%, *** < 1%. An observation is a private school that was open in 2006-2007 according to the NYSED, and dependent variables are school characteristics measured in the most recent year available before 2007. "FSF" measures the public school's projected FSF per student funding change (in 000s) and mean FSF is the unweighted mean of this value across the relevant public schools. Dist1to5 indicates the 5 closest public schools to the private school. Standard errors are clustered at the subdistrict.

Table A10: Supply Regressions – Robustness to Matching

	Strict Match			Strict Match, Keep Only-Kindergartens				Looser Match			
	Drop Schools with Year Mismatch		Drop Schools with Year or Enroll Mismatch	Drop Schools with Year Mismatch		Drop Schools with Year or Enroll Mismatch	Drop Schools with Year Mismatch		Drop Schools with Year or Enroll Mismatch	Drop Schools with Year Mismatch	
	All	All	All	All	All	All	All	All	All	All	All
FSF	-0.201** (0.099)	-0.135** (0.054)	-0.121** (0.057)	-0.195* (0.105)	-0.129** (0.053)	-0.126** (0.057)	-0.170 (0.108)	-0.170 (0.108)	-0.094 (0.057)	-0.115** (0.056)	
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,043	10,165	9,346	11,163	10,135	9,236	11,222	10,244	9,417		
R-Squared	0.074	0.221	0.201	0.075	0.221	0.195	0.087	0.235	0.198		

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. An observation is a public school-school year. "FSF" is the projected per-student funding change (in \$1,000s). The three sets of columns differ by the strictness of matching. The first set is the baseline sample matched with stricter criteria. The second set of columns keeps kindergarten-only schools that were dropped from the baseline sample. The third set of columns keeps a sample based on looser matching. Within each set of columns, the second and third columns drops schools for which the Private School Survey and NYSED data disagree on the first or last year the school was open within the sample (up to 1 year given that the PSS is every other year). The third set of columns also drops all schools for which the PSS and NYSED disagree on enrollments per grade in any given year with the NYSED data with more than 4.25 more students per grade or the PSS with more than 3.23 more students per grade (the 5th and 95th percentiles). Standard errors are clustered at the zip code.

Table A11: Market-Level Regressions

	Market: Zip Code						
	Priv #		Pub Enroll		Priv Enroll		
	Total FSF	-0.136 (0.249)	140.211 (185.280)	-24.862 (106.986)	Priv #	Pub Enroll	Priv Enroll
Mean FSF					-0.114 (0.243)	71.332 (192.096)	-22.102 (102.894)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	1,813	1,813	1,813	1,813	1,813	1,813	1,813
R-Squared	0.078	0.063	0.092	0.078	0.063	0.063	0.092

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. "FSF" is the projected per-student funding change (in \$1,000s). Total FSF calculates the total projected funding change (in \$1,000s) in the market and divides by the total number of public school students. Mean FSF is the unweighted mean of the projected funding change per student (\$1,000/student) across schools in the market. Standard errors are clustered by zip code.

Table A12: IV Regressions

	Per Student Budget (\$1000s)	Enroll	Num Private Schools w/i 1 Mile
FSF Projected Policy Change (\$1000s/student)	0.593*** (0.156)		
Budget (\$1000s/student)		16.156 (28.723)	-0.231 (0.183)
Fixed Effects	Year, School	Year, School	Year, School
N	7,827	7,827	6,247

* < 10%, ** < 5%, *** < 1%. Data span the 2006-07 through 2013-14 school years. "FSF Projected Policy Change" is the projected per-student funding change (in \$1,000s). Standard errors are clustered by zip code.

Table A13: Poisson Regressions

	Num Private Schools w/i 1 Mile	Num Private Schools w/i 1 Mile	Num Private HS w/i 1 Mile	Num Private Non- HS w/i 1 Mile	Num Charter Schools w/i 1 Mile	Num Charter Schools w/i 1 Mile
FSF	-0.046 (0.030)	-0.052* (0.031)			-0.068 (0.097)	-0.065 (0.104)
Hyp. Neg. FSF		0.009 (0.012)				-0.004 (0.030)
Mismatched FSF			0.000 (0.039)	-0.028 (0.068)		
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,042	11,042	5,079	3,052	3,024	3,024

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. An observation is a public school-school year. Estimates are coefficients from Poisson models. "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 (HS or non-HS) from the private schools counted on the left-hand-side. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey 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.

Table A14: Private School Exit Regressions

	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)
Mean FSF * Dist1to5	0.242** (0.109)	0.275* (0.145)		0.523* (0.279)				
FSF * Dist1		0.992*** (0.362)						
FSF * Dist2		-0.286 (0.349)						
FSF * Dist3		0.148 (0.305)						
FSF * Dist4		0.302 (0.323)						
FSF * Dist5		-0.002 (0.329)						
Mean FSF * Dist6to10			0.283 (0.272)					
Mean FSF				0.783*** (0.265)	0.763*** (0.263)	0.092 (0.228)	0.208 (0.161)	
Mean FSF * Distance (miles)				-0.640** (0.269)	-0.661** (0.276)	-0.015 (0.097)	-0.150 (0.177)	
Mean Hyp. Neg. FSF					-0.003 (0.102)			
Mean Hyp. Neg. FSF * Distance (miles)					0.030 (0.054)			
Years	2006-14	2006-14	2006-14	2006-14	2006-14	2006-14	2006-14	2002-05
Mismatched Levels						x		
Distance Radius (miles)	3	3	3	3	3	3	3	3
Max Number of Matches	5	5	5	10	5	5	5	5
Public School Controls		Yes		Yes	Yes	Yes	Yes	Yes
N	487	487	487	487	487	487	372	268

* < 10%, ** < 5%, *** < 1%. The table reports marginal effects evaluated at the mean from a probit model. An observation is a private school that was open in 2006-2007 according to the NYSED, while Exit is 1 if the school exited the NYSED data by 2014 (unless indicated otherwise in the "Years" row). "FSF" measures the public school's FSF per student projected funding change (in 000s) and mean FSF is the unweighted mean of this value across the relevant public schools. Dist1to5 indicates the 5 closest public schools to the private school. Dist6to10 indicates the 6th through 10th closest public schools to the private school. Distance between schools is measured in miles. Standard errors are clustered at the zip code. Demographics are means across the public school matches. Public school controls are a set of indicators for each subdistrict, and the percentages of students who are black, Hispanic, have limited English proficiency, and have been held back a grade. "Mismatched Levels" refers to private-public matches of opposite levels (elementary and high school).

Table A15: Private and Charter Enrollment Regressions

	Private	Charter
	Enrollment w/i 1 Mile	Enrollment w/i 1 Mile
FSF	-80.106 (49.831)	-27.288 (118.442)
Fixed Effects	Year, School	Year, School
N	4,891	3,050
R-Squared	0.092	0.607

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years.
"FSF" is the per-student funding change (in \$1,000s). Standard errors are clustered by zip code.

Table A16: Number of Private School Regressions – Varying Assumptions for Standard Errors

	Standard Error	Number of Clusters
Cluster by Zip Code	(0.099)	159
Cluster by Subdistrict	(0.080)	32
Cluster by School	(0.094)	816
Wild Bootstrap, Cluster by Zip Code	(0.099)	159
Wild Bootstrap, Cluster by Subdistrict	(0.077)	32
Wild Bootstrap, Cluster by School	(0.093)	816
Robust	(0.094)	

Table shows different types of standard errors for the regression in Table 6, Column 1. Clustered wild bootstrap estimates come from 1,000 bootstrap iterations using the Rademacher distribution. "Robust" refers to Huber-White standard errors.

Table A17: Private School Characteristics Regressions

	Enrollment	ELA Grade 4 Mean (std)	Math Grade 4 Mean (std)	ELA Grade 8 Mean (std)	Math Grade 8 Mean (std)	Income (\$000s)	Assets (\$000s)
Nearby Mean FSF	-36.255 (37.395)	-0.075 (0.115)	-0.095 (0.110)	0.009 (0.137)	0.010 (0.160)	-9,399.741 (6,188.051)	-8,417.470 (24,247.628)
Mean in 2007	296	0.26	-0.04	0.37	0.19	3,192	4,635
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	973	1,960	1,974	1,631	1,642	89	89
R-Squared	0.066	0.033	0.083	0.020	0.211	0.326	0.204

* < 10%, ** < 5%, *** < 1%. Data span 2002-03 through 2011-12 school years. Each column is a separate regression of a private school characteristic or outcome on the average FSF (\$1,000s/student) at the 5 closest public schools. Each regression includes year and private school fixed effects. Standard errors are clustered by zip code.

Table A18: Regressions of Private School Enrollment on Zip Enrollment

	Own Enrollment	Log(Own Enrollment)
Other Enrollment	-0.064*** (0.016)	
Log(Other Enrollment)		-0.755*** (0.055)
Fixed Effects	Zip Code, Year	Zip Code, Year
N	5,163	4,412
R-Squared	0.255	0.279

* < 10%, ** < 5%, *** < 1%. An observation is a private school - year. Data covers 2000-2001 and later. Other enrollment is the total enrollment among all other private schools in the same zip code.

Table A19: Estimates of Variations of Demand Model

Demand Parameters	Grade								
	K	1	2	3	4	5	6	7	8
<u>Baseline</u>									
γ	0.78	0.77	0.73	0.67	0.65	0.62	0.67	0.57	0.51
ρ	4.12	4.11	4.10	4.06	4.01	3.82	2.70	2.85	2.84
λ	0.19	0.13	0.09	0.25	0.34	0.26	0.00	0.10	0.08
σ	0.39	0.65	0.60	0.90	1.78	0.00	0.76	1.02	0.91
τ_1	0.00	0.00	0.02	0.00	0.01	0.00	-0.05	-0.07	-0.06
τ_2	0.00	0.00	-0.01	0.01	-0.01	0.00	0.00	0.00	0.00
τ_3	-0.06	-0.03	-0.03	0.09	0.09	0.01	0.03	0.03	0.07
τ_4	-0.10	-0.09	-0.04	0.07	0.12	0.01	0.00	0.00	0.00
τ_5	-0.27	-0.24	-0.23	-0.12	-0.19	-0.15	-0.21	-0.15	-0.18
<u>Omitting 2012</u>									
γ	0.83	0.83	0.77	0.70	0.71	0.67	0.70	0.58	0.52
ρ	4.04	4.13	3.98	3.98	3.96	3.78	2.47	2.76	2.82
λ	0.07	0.08	0.15	0.24	0.42	0.29	0.09	0.07	0.31
σ	0.00	0.00	0.00	0.00	4.73	3.50	1.12	1.78	3.40
τ_1	0.00	0.00	-0.02	-0.05	-0.14	-0.13	-0.02	-0.11	-0.20
τ_2	0.00	0.01	0.02	0.05	0.00	-0.01	0.00	0.00	-0.11
τ_3	-0.05	0.00	0.01	0.07	0.00	-0.01	0.03	-0.04	-0.04
τ_4	-0.09	-0.04	0.01	0.09	-0.01	-0.01	0.00	0.00	0.01
<u>No Idiosyncratic Private School Preference</u>									
γ	0.83	0.83	0.77	0.70	0.67	0.65	0.69	0.57	0.51
ρ	4.06	4.16	4.00	4.00	3.90	3.73	2.52	2.82	2.87
λ	0.12	0.10	0.09	0.21	0.22	0.15	0.00	0.01	0.14
τ_1	0.00	-0.02	-0.02	-0.03	-0.06	-0.06	-0.05	-0.06	-0.07
τ_2	0.00	0.01	0.02	0.05	0.01	-0.01	0.00	0.00	-0.02
τ_3	-0.05	0.00	0.00	0.10	0.06	0.02	0.04	0.03	0.06
τ_4	-0.09	-0.05	-0.02	0.08	-0.01	0.01	0.00	0.00	0.00
τ_5	-0.24	-0.19	-0.17	-0.07	-0.13	-0.13	-0.17	-0.15	-0.14
<u>No Private School Year Shocks</u>									
γ	0.83	0.83	0.77	0.70	0.67	0.65	0.69	0.57	0.51
ρ	4.07	4.16	4.00	4.00	3.90	3.73	2.52	2.82	2.86
λ	0.40	0.27	0.21	0.16	0.24	0.16	0.06	0.08	0.14
σ	0.09	0.00	0.00	0.01	0.54	0.00	0.00	0.55	0.00

Demand parameters are estimated with method of simulated moments. The baseline model includes idiosyncratic private school preferences, private school year shocks, and 6 years of data through 2012. The variations make one change to the baseline model but keep the same moments and estimate with an identity weighting matrix.

Table A20: Fit of Demand Model

	Enrollment (Data)
Enrollment (Model Prediction)	0.681*** (0.012)
Fixed Effects	School, Private*Year
N	10,759
Number of Schools	2,517
Within R-Squared	0.418

* < 10%, ** < 5%, *** < 1%. Estimates are from a fixed effects regression using data and model predictions from the 2001-02, 2003-04, 2005-06, 2007-08, 2009-10, and 2011-12 school years.

Table A21: Demand Counterfactuals – 2008 and 2010

Table A.21a: Demand Counterfactual – 2008

Panel A: 2008 Demand Counterfactual

Initial School Category	Final School Category					School Count
	Winners	Losers	Other Public	Private Non-Closers	Private Entrants/Exiters	
Winners		0	0	0	0	495
Losers	1660		0	0	0	519
Other Public	149	0		0	0	64
Private Non-Closers	2229	0	0		0	697
Private Entrants/Exiters Direct	161	0	0	0		
Private Entrants/Exiters Indirect	635	821	41	1661		105

Table A.21b: Demand Counterfactual – 2010

Panel B: 2010 Demand Counterfactual

Initial School Category	Final School Category					School Count
	Winners	Losers	Other Public	Private Non-Closers	Private Entrants/Exiters	
Winners		0	0	0	0	495
Losers	3193		0	0	0	519
Other Public	286	0		0	0	64
Private Non-Closers	3922	0	0		0	612
Private Entrants/Exiters Direct	663	0	0	0		
Private Entrants/Exiters Indirect	4396	3710	210	4314		190

In "Initial School Category" refers to predicted enrollments in 2006. "Final School Category" refers to predicted enrollments from a counterfactual that implements the FSF funding change and changes the set of private schools open based on which schools entered and exited between 2006 and the relevant year (2008 for Panel A, 2010 for Panel B). "Winners" are public schools that received additional money from FSF, "Losers" are public schools that were part of the FSF reform but were held harmless, "Other Public" schools includes specialty and charter schools, "Private Non-Closers" are private schools open in 2006 and the relevant year, and "Private Entrants/Exiters" are schools that entered or exited from 2006 to the relevant year. "Direct" refers to the predicted enrollment changes from a counterfactual where the set of schools stays at the 2006 market structure while "Indirect" refers to the predicted enrollment changes from the change in market structure.