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Quantifying the Supply Response of Private Schools to Public Policies

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Abstract

Public school policies that cause a large demand shift between public and private schooling may cause some private schools to enter or exit the market. This private school supply response further alters students' choices and likely amplifies the policy's effect. Thus, the policy effects under a fixed versus a changing market structure may be very different. To study this difference, we consider New York City's Fair Student Funding reform, which changed the budgets of the city's public schools starting in the 2007-08 school year. We find that relative to the schools that did not receive additional funding, elementary public schools that benefited from the reform saw an estimated increase in enrollment of 6.5%. We also find evidence of private school exit in response to the reform by comparing private schools located close to or far from public schools that received additional funding. A private school located next to a public school that received an average (6%) increase in its budget was an estimated 1.5 percentage points, on a base of 12%, more likely to close in the subsequent two years. We estimate a concise model of demand for and supply of private schooling and estimate that 30% of the total enrollment increase came from increased private school exit and reduced private school entry. Finally, we assess the reform's impact on aggregate achievement. We find that while the reform improved school quality at the public schools that received additional funding, the sorting of some students from private to public schools led them to lower-quality schools. This sorting undid much of the reform's positive achievement effect.

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1 Introduction

The set of schooling options in the United States has grown substantially over the last 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 median per-grade enrollment of 26 students compared to 103 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 10% and 15%, respectively.[1] Just as entry and exit can be a primary force behind aggregate outcomes in other industries,[2] the churn of private schools may determine the quality of private education offered and cause demand shifts between the public and private sector. 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 aggregate 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 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 aggregate achievement.

[1] Calculations use the 1999-2009 editions of the NCES Private School Survey. The major cities are New York City, Chicago, Boston, and Philadelphia. Public school entry and exit are less frequent. Across the same cities, one-year entry and exit rates average 2% and 1%, respectively. Calculations use the 2005-2010 editions of the Common Core of Data.

[2] Firm entry and exit have been associated in the macroeconomics literature with long-term productivity growth (e.g. Bartelsman and Doms 2000, Foster, Haltiwanger and Syverson 2008), the size and length of business cycle (e.g. Clementi, Khan, Palazzo and Thomas 2014), and the size of international trade flows (e.g. Melitz 2003).

We find that the reform affected students' enrollment decisions, partially through a change in the supply of private schools. For each student-equivalent increase in funding,[3] a public elementary school's enrollment increased by 0.12 students. The supply of private schooling was indeed responsive to the public school reform. If a public school received an average funding increase, we find that a local private school was 1.5 percentage points more likely to close in the two years following the reform. This constitutes 12% of the baseline closure rate. We develop and estimate a model that attributes 30% of the total public school enrollment effect to increased school exit and reduced school entry. This demand shift from the private to public sector increased the cost of the reform by 23%. Finally, we find that while the reform improved student achievement at the public schools that received additional funding, the sorting of some students from private to public schools led them to lower-quality schools. This sorting undid much of the reform's positive achievement effect. 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 aggregate achievement.

We start in Section 2 by providing a conceptual framework that lays out the empirical strategy. 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 whether students had poverty status, low prior test scores, English as a second language, and special needs. Schools that were underfunded according to the new funding formula would expect to receive the new funding amount. Schools that were overfunded were allowed to keep their previous funding levels. Overall, about half of the city's K–12 public schools received additional funding, averaging a 6% budget increase, 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, 20% of NYC students were enrolled in 771 private schools.

[3]We define a student-equivalent as the funding equal to the school's per-student expenditure.

In Section 4 we describe the various data sets we put together. In Section 5 we evaluate how the reform affected public school enrollments and private school entry and exit. Our strategy for estimating the policy's effect on enrollment boils down to a differences-in-differences analysis of public school enrollments before and after the reform, using the variation in funding changes across public schools.[4] We find that elementary schools that received additional funding had their enrollments increase by an estimated 6.5% relative to the schools that did not receive increased funding. For each student-equivalent increase in funding, defined as the funding equal to the school's per-student expenditure, we estimate an increase of 0.12 students for elementary schools.

More importantly for our general conclusions, 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 closure rates across private schools that were located at varying distances from public schools that received large funding increases. We find that a private school located next to a public school that received an average funding increase (per-student spending on 46 students, or 6% of the average school budget) was 1.5 percentage points (or 12%) more likely to close in these two years. The estimated effect is particularly large for non-religious private schools and private elementary schools. This is not surprising as non-religious private schools are likely the most substitutable with public schools because they do not differentiate themselves with religious instruction. Private elementary schools are most exposed to a concentrated enrollment shock because elementary students typically attend schools close to their residences. In contrast, a public high school that receives more funding might have its effect on private schools diffused.[5] We also look at private school entry and find that private schools were less likely to open within a one-mile radius of public schools that received large funding increases.

[4]To mitigate possible confounding variation, we use the suddenness of the reform. The reform's timing was driven by the resolution of a New York State court case that suddenly provided the NYC schools district with more state funding. We exploit this plausibly exogenous timing to identify the reform's effects on student enrollments and achievement. For potential time-varying confounders, which could pose a threat despite the reform's suddenness, we provide a series of robustness checks and placebo tests that confirm our findings.

[5]NYC is divided into 32 subdistricts. Most public elementary students (88%) attend a school in their subdistrict while a minority (36%) of high school students do.

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 closures even in the absence of the reform. As an example, perhaps certain neighborhoods were declining in some unobservable way that was correlated with the FSF formula-based increase in the neighborhood schools' budgets. These unobservable factors could have precipitated nearby private school closures. We assess these threats to identification by running a series of robustness checks and placebo tests. In particular, we take advantage of the reform's "hold harmless" clause, which held that no public school would lose funding even if the new FSF formula called for a funding decrease. The function translating a school's potential funding change (the difference in funding between the old and new FSF formulas) into the actual funding change thus had a kink at 0. This kink allows us to separate the effects of the potential funding change, which may have been correlated with unobserved neighborhood deterioration, from the actual funding change. We find that the potential funding changes were only associated with private school closures when the potential change was actually implemented. When the "hold harmless" clause determined that the actual funding change would be 0, we find no relationship.

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. Under nearly all plausible scenarios, the private school supply response will 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 reduced form 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 6.5% enrollment increase at public elementary schools with increased funding. We thus develop, in Section 6, 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, and the school's total quality. The model's estimates, presented in Section 7, 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 30% of the total enrollment change.

To push further and predict what would happen in the event of a larger change in school funding, we introduce a model of private school supply. Along with our student demand model, we use a supply model where incumbent schools decide whether to remain open or close based on their enrollments. We estimate that the average Catholic private school at risk of closing requires an enrollment of 21 students per elementary grade and 25 students per middle grade to stay open.[6] We then simulate a larger funding change and predict subsequent school closures. Our results show that private schools would continue to close with a larger funding increase. In the initial FSF policy, we estimate an exit rate of 1.6% due to the policy. If we were to double the funding change, we predict that 1.4% of the remaining schools would exit.

Even though the indirect effect does not explain the majority of the total effect, it has a disproportionately large impact on public schools' expenditure and achievement. The indirect effect amplifies student switching from private to public schools, which brings more students into the public system. The public school district must therefore cover the education costs of students who would have otherwise cost the district almost nothing as private school students. This switching has important redistributive consequences. The provision of free public schooling can redistribute from higher-wealth households that send their children to private school while still paying taxes to lower-wealth households that send their children to public school (see Besley and Coate (1991)). If higher-wealth families become more likely to send their children to public school, then the level of redistribution may fall.[7]

[6] We estimate larger required enrollments for non-Catholic religious schools.

[7] There are some students who switch from private to public schools due to the increased public school

By similar logic, the indirect effect may have outsized importance for aggregate achievement. Families face a tradeoff in the amount they pay for education and the education's quality.[8] Students switching between two public schools, neither of which charges tuition, or between two private schools, which may charge relatively similar tuitions, may not see a large change in school quality. But a private school student considering a move to a public school would see a large decrease in tuition. The student may then accept a relatively large drop in the quality of school she attends. The indirect effect, by causing students to switch to schools with different tuition rates, might thus lead to large effects on achievement.[9,10]

We therefore assess the reform's impact on aggregate achievement in New York City in Section 8. We use student-level assessment data on public school students and novel school-cohort assessment data on private school students. With these data we construct value-added measures for grade 4-8 mathematics and English language arts tests.[11]

The overall impact on achievement operates through several channels. Students who stayed at their public schools saw a slight increase in achievement from the FSF additional funding.[12] Students who switched public schools tended to move to similar public schools.

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funding and not because their private school closed (direct switchers). Because these students are marginal, they are possibly from less wealthy families than the private school students who switch only because their school closed (indirect switchers). Thus, not only does the indirect effect amplify private to public switching, it increases switching among the higher-wealth students and further diminishes monetary redistribution.

[8] This might imply that private schools, which usually charge tuition, offer higher-quality academic instruction than public schools. However, families may also pay private school tuition for other school characteristics like religious instruction rather than for higher-quality academic instruction. Whether there is a private school academic premium is of course an empirical question, and later in this paper we estimate a slight positive premium. Many papers have found a positive premium for test scores or graduation rates, especially for minority students attending Catholic schools (e.g., Evans and Schwab 1995, Neal 1997, Figlio and Stone 1999, Peterson, Howell, Wolf and Campbell 2003, Altonji, Elder and Taber 2005), while some have found no premium (e.g., Goldhaber 1996).

[9] If there is heterogeneity in how much students' achievement changes at a private school, then the direct switchers are likely the students with the smallest achievement losses from leaving the private school. These students, by revealed preference, would like to switch to the public school, so this might bound their achievement losses. The indirect switchers are likely to experience larger achievement losses.

[10] Students switching from private to public schools could also improve the quality of the public education if, for instance, these students induce positive peer effects.

[11] The calculations require a few assumptions and thus our analysis demonstrates suggestive, rather than definitive, achievement effects.

[12] Using a differences-in-differences framework, we estimate that a school with additional funding equal to 46 students (the average increase) had an increase in mathematics value-added of 0.010 test standard deviations. The increase in ELA was a statistically insignificant 0.003 standard deviations. Value-added in this context measures the school's impact on a student's test scores when controlling for student demographics including prior test scores.

Students who switched between private schools tended to move to similar private schools.[13] Countering the achievement increase, private school students who switched to public schools experienced a large decrease in the value-added of the schools they attended. This decrease offset much of the achievement increase associated with the aforementioned public school improvement. It highlights how a minority of the students switching schools can determine much of the aggregate achievement impact. Our findings reveal that the nature of a policymaker's objective matters critically for policy evaluation in this context, as redistributive and overall achievement effects can be opposing in direction to observed quality changes within affected public schools. We further expand on the importance of the indirect effect in the next section.

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 and how students sort between the public and private sectors.[14] The second strand evaluates school funding reforms and whether spending affects student outcomes.[15] There has been little 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,[16] only a few papers have examined how entry can affect a policy's outcomes. These papers have focused on Latin American policies. Hsieh and Urquiola (2006) find that Chile's universal voucher program led to considerable private

[13]Students who left closing private schools ended up at better private schools on average because the closing private schools had lower estimated value-added than the private schools that stayed open. But private schools entering the market generally have higher value-added, so the reduced private school entry meant that some students would have attended better (new) private schools in the absence of the reform.

[14]An exhaustive literature review is beyond the scope of this paper, but influential work includes Hoxby (1994), Dee (1998), Nechyba (1999), Hoxby (2003b), Greene and Kang (2004), McMillan (2005), Card, Dooley and Payne (2010), and Neilson (2013). Work on whether private schools cause stratification includes Clotfelter (1976), Epple and Romano (1998), Hoxby (2003a), and Epple, Figlio and Romano (2004).

[15]Work on school funding reforms and effects on private schools includes Sonstelie (1979), Downes and Greenstein (1996), Downes and Schoeman (1998), Hoxby (2001), Sonstelie, Brunner and Ardon (2000), and Nechyba (2003). For whether school resources matter for student outcomes, see Card and Krueger (1996), Hanushek (1996), Downes and Figlio (1997), Hoxby (2001), Angrist and Lavy (2002), Card and Payne (2002), Goolsbee and Guryan (2006), Cellini, Ferreira and Rothstein (2010), and Jackson, Johnson and Persico (2014).

[16]Work on entry includes Downes and Greenstein (1996), Barrow (2006), and Ferreyra (2007) while work on exit includes Pandey, Sjoquist and Walker (2009). Other work has looked at similar issues for public schools (e.g., Engberg, Gill, Zamarro and Zimmer 2012, Epple, Jha and Sieg 2013) and two-year colleges (e.g., Cellini 2009, Cellini 2010).

school entry but that public schools in communities with more private school entry did not improve student outcomes. Menezes-Filho, Moita and de Carvalho Andrade (2014) examine the Bolsa Familia program expansion in Brazil, which increased school attendance among the poorest children. They argue that this led to private entry, which perpetuated socioeconomic inequality, as the best public school students sought private options to avoid the worst students. Both papers test how the policy effects varied by municipality. Our paper provides evidence on the importance of U.S. private school supply responses, especially exit, and quantifies the impact on aggregate achievement. We also leverage 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 motivate and define the direct and indirect effects. We will present a full model, which we take to the data, in Section 6.

Student i chooses among a set of $\mathcal{J}$ private schooling options and a set of $\mathcal{K}$ public schooling options. For each school l she gets utility $u_{il}(X_l^{FSF})$ where X_l^{FSF} is the school's funding level, set exogenously by the FSF reform.[17] Each student $i \in \mathcal{I}$ chooses the school that gives her the highest utility.

A public school k's demand is thus:

$$D_k(X^{FSF}|\mathcal{J},\mathcal{K}) = \sum_{i \in \mathcal{I}} \mathbb{1}\{u_{ik}(X_k^{FSF}) > u_{il}(X_l^{FSF})\ \forall\ l \neq k, l \in \mathcal{J} \cup \mathcal{K}\}. \tag{1}$$

D_k depends on the vector of exogenously set funding levels, X^{FSF}, as well as which other schools are open ($\mathcal{J} \cup \mathcal{K} \setminus k$). Suppose that we can summarize the competitive impact of k's set of competitors with a one-dimensional index C_k. Then we can write public school k's demand as $D_k(X^{FSF}, C_k)$.

Private school $j \in \mathcal{J}$ must attract enough students to cover its operating costs, F_j. School j closes if $D_j(X^{FSF}, C_j) < F_j$. If j closes, then the remaining schools face less

[17] Private school j, which does not receive FSF funding, has $X_j^{FSF} \equiv 0$.

competition for students, so $C_k(X^{FSF})$ is an equilibrium object. It depends on X^{FSF}, the vector of schools' exogenous funding levels, because these help determine schools' demand. Thus, public school k's demand can be rewritten as $D_k(X^{FSF}, C_k(X^{FSF}))$.[18]

Suppose one public school, k, receives an FSF funding increase. Then the total effect of the change in X_k^{FSF} on D_k is:

$$\underbrace{\frac{dD_k}{dX_k^{FSF}}}_{\text{Total Effect}} = \underbrace{\frac{\partial D_k}{\partial X_k^{FSF}}}_{\text{Direct Effect}} + \underbrace{\frac{\partial D_k}{\partial C_k}\frac{\partial C_k}{\partial X_k^{FSF}}}_{\text{Indirect Effect}} \tag{2}$$

The first term, $\frac{\partial D_k}{\partial X_k^{FSF}}$, is the direct effect on k's demand from the funding change. This term should be weakly positive provided that X_k^{FSF} is valued positively. The second term, $\frac{\partial D_k}{\partial C_k}\frac{\partial C_k}{\partial X_k^{FSF}}$, captures the change in competition from private school entry and exit due to the reform and how this change affects k's demand. We label this term the indirect effect. The derivative of demand with respect to competition should be negative, as more competition lowers demand. The derivative of competition with respect to X_k^{FSF} should also be negative, as the increasing attractiveness of k will make it harder for some private schools to stay open. Private school closures then decrease the competition that public school k faces. The indirect effect captures the change in demand for a public school related to the exit and entry of private school competitors.[19]

The decomposition of the total enrollment change into the direct and indirect changes also informs how we extrapolate to larger funding changes or 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 proposed funding increase twice the size of the FSF reform. If the 6.5% enrollment response to the FSF reform were dominated by the indirect effect, then the predicted enrollment increase from the larger reform would depend mostly

[18]In the conceptual framework, we assume that public schools do not open or close in response to schools' funding levels. Thus, X^{FSF} only determines C_k through entry and exit in the more elastic private sector.

[19]We focus on changes in the level of competition due to private school exit and entry. Private schools may make other supply decisions that could affect the degree of competition in the market. For instance, a private school could adjust its tuition rate. The direction of this adjustment is theoretically ambiguous as schools that remain open face increased competition from public schools due to the reform but possibly reduced private competition if neighboring private schools closed. We consider other supply decisions beyond the scope of this paper and hope to explore them in future work.

on how competitive the remaining private schools are with the public schools and how close these schools are to closing. Similarly, even if students in another school district care about school funding as much as NYC students do, we might not expect as large an 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.

Separating the direct and indirect enrollment changes is also essential in evaluating student preferences for public school funding. Public 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 whether higher funding leads to higher school quality. We find that enrollment is quite responsive to public funding, which seems to indicate that families place a high emphasis on public school funding. But to determine the true impact of public funding on preferences, we only want to consider students making new choices from the same options.

For example, consider a school district with one public and one private school where the public school receives additional funding exogenously. Suppose the private school starts with 20 students but once the public school receives the funding, 5 students leave the private school for the public school (direct switchers). These students have the same two school options before and after, but due to the funding change they switch from the private to the public school. By these students' revealed preference, the public school's attractiveness increases by enough to cause 5 switches. Now suppose the private school needs at least 20 students to remain open, so once the 5 students leave the school must close. This forces the remaining 15 students to attend the public school (indirect switchers). These students, however, do not have the same school options before and after the funding change. Indeed, if their private school were to remain open, these students would stay. While the overall public enrollment increase is 20 students, the public school's attractiveness does not increase by enough to cause all 20 to switch voluntarily. To evaluate the effect of the funding on preferences, we only want to count the 5 direct switchers. Furthermore, the 15 indirect switchers are actually worse off because their preferred school, even after the funding change, has closed. The size of the indirect effect thus has important welfare implications, as it measures the number of students whose welfare decreases.

2.2 Empirical Strategy

We devote much of this paper to measuring the direct and indirect effects.

We start by using a differences-in-differences framework to estimate the total effect, $\frac{dD_k}{dX_k^{FSF}}$. The regression compares how schools' enrollments change after the reform's implementation and whether these changes are related to the size of the funding increase.[20] We note that unless the number of students in the school district changes, one school's enrollment increase must be offset by enrollment decreases at other schools.[21] We are therefore measuring the demand shift among public schools from a change in funding at certain schools.

We then 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 equation, we will measure $\frac{\partial C_k}{\partial X_k^{FSF}}$ by comparing private school exit rates for private schools located near public school that received significant funding with exit rates for private schools located far from public schools that received money. Our estimates show that $\frac{\partial C_k}{\partial X_k^{FSF}} < 0$. We then use a parsimonious model to estimate the direct effect, $\frac{\partial D_k}{\partial X_k^{FSF}}$. This allows us to recover the indirect effect as the difference between the estimated total effect and the estimated direct equilibrium effect. After estimating the size of the indirect effect, we assess its importance for aggregate achievement.

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

[20]Unlike the set up in our conceptual framework, we do not observe a funding change at just one school (k) 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.

[21]From 2005-06 to 2009-10 the aggregate enrollment in NYC declined from 1.18 million students to 1.14 million students. The average school experienced a decline in enrollment.

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.[22] 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.[23] 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 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 students who are English language learners, and the percentage of special education students. In addition to changing the size of a school's budget, the reform removed some 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

[22]The city was also required to provide an additional \$2.2 billion. The state funding was to be phased in over four years but the financial crisis led to a freeze in funding for the 2009-10 school year. In that year NYC received \$643 million of additional funding from the state.

[23]The *Campaign for Fiscal Equity, Inc. v. New York* decision did not mandate funding equalization within school districts but the spirit of the decision may have pushed NYC toward funding equalization.

salaries of the teachers, and the district would compensate a school for the salary differential from hiring more expensive teachers. Each school would then recruit and hire its own teachers. Thus, schools that hired more expensive (experienced) teachers received more money, and because the more experienced teachers tend to prefer schools in wealthier areas, the schools in poorer neighborhoods wound up with smaller budgets. The FSF reform changed this accounting so that a school's budget would be fixed and not increase if the school hired more expensive teachers.[24]

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 funding under the old and new (FSF) formulas.[25] 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." In the 2007-08 school year, the FSF reform was partially implemented. "Winning" schools received 55% of the expected funding increase, up to $400,000, with the expectation that they would get the full increase over the coming years.[26]

In Figure 1 we graph the size of the actual funding increase as a function of the difference in funding between the FSF and old formulas. The "hold harmless" clause truncates all funding changes from below at $0. The truncation forms a kink in the relationship between a school's potential funding change (the difference in funding between the old and new FSF formulas) and its actual funding change. We will later use this kink to separate the effects of the potential funding change, which was a function of school characteristics, from the actual funding change.

As an example to demonstrate the reform more concretely, see Figure 2a, which shows

[24]Several other cities, including Houston and Cincinnati, made a similar change from staff-based resource allocation to student-weighted allocation (Miles and Roza 2006).

[25]The reform changed the funding formula, not just the level, so that it would adjust to smaller or larger enrollments than predicted.

[26]California passed a similar school funding reform, the Local Control Funding Formula, which began in the 2013-14 school year. The reform shares many similarities with the FSF reform. The reform changed the school funding formula to a student-weighted formula with higher weights for low-income students. The plan is to phase in the reform over 8 years, and it includes a "hold harmless" clause. The key difference is that the reform changed school district, rather than individual school, funding.

how the reform affected P.S. 189 Lincoln Terrace. Under the old approach, the school would have received $5,354,931, and under the FSF approach, it would receive $6,227,823. This school is thus a relative "winner" and should receive an additional $872,892. In the first year, however, the funding increase is capped at $400,000. Appendix Figure A.1 shows how the FSF amount was determined. Figure 2b shows a "relative loser" under the reform. J.H.S. 045 William J. Gaynor would have received $2,833,949 under the old approach, compared to just $1,980,306 under the new approach. But because the reform does not take away money from schools, the school gets to keep the full $2,833,949. In Section 4 we will provide statistics on the funding increases across all schools and compare the "relative winners" and "relative losers."

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 direct enrollment effect is and how likely we are to pick it up in our analysis. Public elementary students typically (65%) attend their local ("zoned") school. A minority of students opt for other schools, usually when a sibling attends another school or if a school has unused capacity.[27] Even though 35% of elementary students do not attend their zoned school, 88% attend a school in their subdistrict. Middle school students are afforded more options in most subdistricts, with 58% of students attending a school other than their "zoned" middle school and 19% attending a school outside of their subdistrict. By high school, students have choice across many schools, and 74% attend schools outside their subdistricts. Students apply to high schools by ranking schools according to their preferences, and the selective public high schools rank the applicants. NYC then runs a centralized matching system that assigns students to schools.

[27] Three subdistricts do not assign students zoned schools and encourage students to choose among several options.

4 Data and Descriptive Statistics

4.1 Public Schools

To provide a complete picture of public and private schooling in NYC and how they interact, we bring together data from several sources. For public schools, we use 2007-08 budget data from the NYC DOE to calculate how much the FSF reform affected schools' budgets. These data include the actual 2007-08 budget and the hypothetical budget had the FSF reform not happened. The NYC DOE also creates annual School-Based Expenditure Reports that tell us how the schools spend their budgets each school year. We supplement these data with school characteristics from NY State Report Cards. These data include enrollments, grade average test scores, measures of the student's demographics, and measures of teacher experience.

We also make use of student-level data from the NYC DOE. These data allow us to track students' school attended, zoned school, and standardized test scores as long as the student attends a NYC public school. 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.

The key to our empirical strategy will be that the FSF reform affected NYC public schools differentially. In Figure 3 we graph estimated kernel densities of the size of the funding increase for the "winning" schools. The "losing" schools comprised 48.8% of the schools and all received $0 funding changes. In Figure 3a, we graph the size of the funding increase, both the first-year amount and the eventual amount, as a percentage of the school's total operating budget. In the 2007-08 school year, an average "winning" school received a funding increase equal to 3% of its budget. There is a large right tail as 5% of "winning" schools saw increases of over 7% of their budgets. The density for the eventual funding increase is nearly the 2007-08 density stretched to the right by a factor of 2. The average school expected to receive an eventual funding increase of 6% of its budget.[28] In Figure 3b we show the funding increases expressed as the amount of money received divided by the school's per

[28] The size of this funding increase is of the same order of magnitude as the increase in school district funding due to the NYSTAR program, as studied in Rockoff (2010), where school districts on average saw an increase in revenue of 6.75% between the 1998-99 and 2001-02 school years.

student expenditure, a measure of the funding change in units of "student-equivalents."[29] The average "winning" school received funding equivalent to 21 students in 2007-08 and 46 students upon full implementation of the reform, but 10% of "winning" schools would eventually receive funding equivalent to at least 100 students. The FSF reform therefore created relative "winners" and "losers" but also created variation among the "winners" that we will use in our analysis.

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. To highlight some of these differences, 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:

$$y_k = \phi_0 + \phi_1' X_{1k} + \phi_2' X_{2k} + \omega_k. \tag{3}$$

Table 1 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 total funding increase divided by the per student expenditure. Schools with more students who received free or reduced 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 funding increase shows a weaker relationship between school characteristics and how much funding schools received. The main predictors of the funding increase are the percentage of a school's students that are black and the percentage that have passed each grade on time.[30] Both measures are negatively associated with the size of

[29]The measure does not literally represent the number of additional students the school can afford because if more students were to enroll, the school's funding would increase.

[30]The NY stability measure captures the percentage of students who are in the grade normally associated with a certain age. In other words, this measures the percentage of students who have not been held back a grade.

the funding increase. Because the "winning" and "losing" schools differ statistically along a few characteristics, we will use the timing of the reform to separate the reform's effects from changes related to the schools' 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 4. 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 will 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.

4.2 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. This data set is published every other year and includes school characteristics such as enrollment, religious affiliation, number of teachers, and location. The data set has some measurement error, though this has decreased over time. We therefore spot checked the data set with Internet searches. We determine 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 1999-2000 through 2011-12 school years.

To measure private schools' quality of education, we use test score data on nonpublic schools 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 few 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, but about 75% of the schools claim to. 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. We assess this possible selection in Appendix C. We provide more details on all data sources in Appendix A.

Private schooling plays a large role in New York City's educational landscape, as 20.1% of K-12 students attend private schools. This figure compares favorably to other large cities, as 13% of both Boston and Chicago students attend private schools. 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 39% of the schools offering Catholic instruction and 46% affiliated with another religion. Schools also tend to be relatively small, as 12% of schools enroll fewer than 10 students per grade and 20% enroll fewer than 20. Many of these schools serve minority populations. Almost 40% of the NYC private schools have a majority of students who are black or Hispanic. Thus, the elite Manhattan prep schools that appear prominently in popular culture are not representative of private schooling in NYC. Table 2 provides summary statistics of the NYC private schools open during the 2007-08 school year, when the FSF reform began.

To further demonstrate that NYC private schools are not mainly serving students from high-income backgrounds, we draw spatial maps of the Brooklyn Census tracts in Figure 5. The left map shades each census tract according to its 2000 Census median income for households with children. The right map shades based on the 2000 Census percentage of adults who graduated from high school. In both maps, the darker shades correspond 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 borough 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.

Also unlike the elite prep schools, many private schools face a high probability of having to close. Because we are measuring the supply response of private schools along the extensive margin and how this interacts with public school funding, we require that schools actually open or close with some frequency. In Figure 6 we plot the number of NYC entrants and exiters in the Private School Survey 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.[31] In most years, there are between 75 and 125 entrants and exiters, which is quite large compared to the roughly 770 schools that are active at a given time.[32]

Many of these exits come from schools that recently entered, and indeed age is a main predictor of exit. But when we fix the 1999-2000 cohort of private schools, almost 40% of them have exited within 10 years, and the exits happen relatively smoothly across the decade. There is certainly considerable heterogeneity as some schools face a very low probability of having to close. But the presence of a large mass of schools that are at risk of closing indicates that lower private school demand from increased public school funding could shift some schools below their break even point and cause them to close. The frequency of closure, even before the reform, provides us with the necessary statistical power to test whether private schools near FSF "winners" are more likely to close.

5 Policy's Effect on Public and Private Schools

5.1 Enrollment Changes in Public Schools

For the FSF reform to have a large enough impact to cause some private schools to close, student enrollment decisions must be responsive to the funding increase. To establish that demand appears responsive, we compare how enrollments changed at public schools that received money under the reform (relative "winners") and public schools that did not (relative "losers"). This differential change in enrollments across public schools is the policy's total effect on enrollment, which combines students switching between two public schools, students

[31] Occasionally a school will drop out of the data but then reappear several years later. We do not count this as exit. We discuss this data choice, which does not qualitatively affect our results, in Appendix A.

[32] Some areas of the city have consistent net exit over time, but entry and exit in most years in the same zip code are positively correlated. There are some long-term trends that can explain the entry and exit, but much of it seems to be churn.

switching from a still-open private school to a public school, and students switching from a newly-closed private school to a public school. Later we will break down the policy's total effect into the direct and indirect effects.

To establish that demand is responsive to the effects of the reform, we plot the average enrollment per school for "winners" and "losers" from 2005-06 to 2011-12 in Figure 7. The "losers" see a clear drop in their average enrollment per school following the reform's implementation in the 2007-08 school year. At the bottom of the figure we plot the difference between the two curves, which highlights the change starting in 2007-08. Cohort sizes are declining in NYC over these years, so the difference between the "winners" and "losers" is mostly driven by enrollment drops for the "losers."

We quantify this enrollment effect by running a differences-in-differences regression where we compare enrollments across FSF relative "winning" and "losing" public schools before and after the reform. For public school k in year t:

$$f(enrollment_{kt}) = \delta_k + \tau_t + \pi FSF_k After2007_t + \eta_{kt}. \tag{4}$$

Our coefficient of interest is π, which measures how the policy's impact varied with the level of the funding change. Table 3 reports the results. When we use a dependent variable of log enrollment and compare "winners" to "losers," our estimate of π is 0.123 (0.009), indicating that the "winners" saw an enrollment jump of 12% after the reform relative to the "losers." Much of this enrollment difference may be driven by students switching between two public schools, especially at the high school level when a change in student preferences can more easily lead to different enrollment outcomes. We therefore run the same regression but restrict our sample to elementary schools. Our estimate of π is indeed smaller, at 0.065 (0.017). In the third column of the table, we show results where we use a left-hand-side of enrollment and estimate an effect for elementary schools of 19.1 (11.1) students. We also measure how the enrollment effect differs with the size of the funding increase. We define "FSF" as the funding change divided by the per student expenditure (in units of 100s). This measure expresses the funding change in units of "student-equivalents." We find that a funding increase of 100 student-equivalents predicts an estimated enrollment increase of 6.5% (or 11.8 students). These enrollment shifts are consistent with demand shifting quickly across schools and make it plausible that some private schools might lose enough students to consider closing.

In Appendix B we explore the mechanisms that led to the large demand shifts by examining how the "winners" used their additional funds. Our evidence indicates that students likely shifted toward the "winners" because they hired more experienced and better teachers. Using the School-Based Expenditure Reports to compare expenditures across different categories for "winners" and "losers," we find that schools used \$0.59 of each marginal dollar on teacher salaries and benefits. This represented a shift toward spending money on teachers as just \$0.36 of the average dollar was spent on teachers. Despite the increase in spending on teachers, schools that received additional funding saw a decrease in their number of teachers but an increase in their teachers' experience.[33,34] We also find evidence that class size in grade 10 core classes went down slightly.[35]

These uses of the funding translated into higher school value-added. We discuss the reform's effect on achievement in Section 8. But to preview our results, we find that a school that received a funding increase of 100 student equivalents had an increase of value-added of 0.02 test standard deviations (σ) in mathematics. We find a smaller (0.006σ) effect for ELA, which is not statistically significant. The increase in math value-added is the equivalent of replacing 92 bottom quartile math teachers with top quartile math teachers (an average of 0.2 replacements per school).[36]

[33]Salaries are determined centrally, so schools could not necessarily attract teachers by offering them higher salaries than other schools could offer. However, the reform likely increased teacher experience at "winning" schools due to the change from staff-based resource allocation to student-weighted allocation. "Relative losing" schools, which in the past could attract the most expensive and experienced teachers, now could no longer afford all of them, so many of them ended up at the "winners." See Appendix B for more details.

[34]Boyd, Lankford, Loeb, Rockoff and Wyckoff (2008) find that the high-poverty schools had started narrowing the gap in teacher qualifications and experience between 2000 and 2005.

[35]There is some debate as to whether class size plays an important role in the education production function. Some studies on U.S. education find that class size may matter (e.g., Chetty, Friedman, Hilger, Saez, Schanzenbach and Yagan 2011) while others do not (e.g., Hoxby 2000).

[36]This calculation uses the estimate from Kane, Rockoff and Staiger (2008) that top quartile elementary math teachers in NYC produce 0.33σ higher value-added than bottom quartile teachers. When we add up the FSF funding increases, we calculate that one teacher is replaced for every 209 student-equivalents in funding.

5.2 Private School Exit

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 enough fixed operating costs and was already close to the break even point, then the loss of a handful of students could have made it so the private 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. Without detailed individual-level data on which schools public and private students attend, we measure a private school's level of substitutability with the public school as the distance between the schools. Previous work has established that a student's distance to a school is an important determinant in her school preferences (e.g., Walters 2014). Schools close to each other are thus likely to compete over the same students while schools far from each other are less substitutable.

For each private school j that was active in the 2007-08 school year, we match it to the 10 closest public schools k that serve the same grades,[37] provided the schools are fewer than 15 miles apart.[38] Over 80% of private schools are matched to 10 public schools. The median and mean distances between the private school and a matched public school are 1.4 and 2.2 miles, respectively.[39]

We measure the intensity of the treatment on a public school by dividing the size of the eventual funding increase by the school's per student expenditure (units of student-equivalents). This variable thus represents the number of additional students the public school can "afford" according to its pre-reform level of spending per student. By expressing

[37] We match on indicator variables for whether the public or private school serves elementary and high school students, respectively. Results are similar if we also require schools to match on whether they educate middle school students.

[38] We choose 15 miles as the cutoff because New York State provides students with transportation to non-urban private schools within 15 miles. The regulation does not apply to NYC. Results are qualitatively similar when we use smaller cutoffs.

[39] We use great-circle distance in miles. Results are very similar when using driving times.

the treatment in units of student-equivalents, we think that this is the most intuitive measure of the impact on private schools.[40] We label this variable "FSF_k" and express it in units of 100 student-equivalents. The mean value for "winning" schools is 0.46.

Our first regression specification divides the matched public schools into bins depending on which public schools are closest to the given private school and tests how the impact of the FSF reform on a private school's probability of closing varies by bin. We run the following regression:

$$Exit_j = \alpha_1 Dist1to2_{jk} FSF_k + \alpha_2 Dist3to5_{jk} FSF_k + \alpha_3 Dist6to10_{jk} FSF_k + \psi' X_k + \theta_d + \epsilon_{jk} \quad (5)$$

where j indexes the private school, k indexes the public school, and d indexes the 32 school subdistricts within NYC. $Exit_j$ is an indicator variable for whether private school j closed between the 2007-08 and 2009-10 school years. $Dist1to2_{jk}$ is an indicator variable for whether public school k is either the closest or second closest public school to private school j. Similarly, $Dist3to5_{jk}$ is an indicator for the third to fifth closest schools and $Dist6to10_{jk}$ is an indicator for the sixth to tenth closest schools. Finally, we include fixed effects for the public school's subdistrict. We also weight the observations by the inverse of the number of matches for private school j and we cluster standard errors at the private school.[41]

Our identification assumption is that other factors that caused a private school to close from 2007-08 to 2009-10 were orthogonal to the funding increase at nearby public schools, conditional on the observed public school characteristics ($E(\epsilon_{jk}|FSF, X_k, d) = 0$). 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. There are other stories that could invalidate our identification assumption, and we discuss those in the next subsection.

We expect that the larger the funding increase in terms of students, the more likely the

[40]This is merely a scaling choice. Results, available upon request, are similar if we use the size of the eventual funding increase.

[41]We also try two-way clustering on private and public schools. These results actually have smaller standard errors on the coefficients of interest, possibly because local private school competition could produce negative correlations in closures. Results are available upon request.

competing private schools are to lose students and close, so the α coefficients are likely to be positive. But private schools are likely most substitutable with the closest public schools, so we expect $\alpha_1 > \alpha_2 > \alpha_3$.

As seen in Table 4, our estimates of α are positive. We also estimate that $\alpha_1 > \alpha_2$ and $\alpha_1 > \alpha_3$. If the closest or second closest public school gets enough additional funding equal to 100 students, the private school is 4.3 percentage points more likely to close. For the further out public schools, the effect is on the order of 1 percentage point. The results are relatively stable when we include measures of the public school's demographics and teacher experience.

Because the effect of distance between schools is likely more continuous than the discrete jumps we have used above, we run a second regression where we allow the effect of the FSF reform to fade out linearly with distance:

$$Exit_j = \beta_1 FSF_k + \beta_2 Distance_{jk} FSF_k + \gamma' X_k + \mu_d + \nu_{jk} \tag{6}$$

As before, we weight the observations by the inverse of the number of matches and we cluster standard errors at the private school. In this regression, we expect $\beta_1 > 0$, but because the effect should weaken as schools are further apart geographically, we expect $\beta_2 < 0$.

Our results confirm these predictions. If a public school next door to the private school gets additional funding equal to 100 students, the private school is 3.4 percentage points more likely to close. The effect decreases with distance such that for every mile separating the public and private school, the effect weakens by 0.6 percentage points. The results are mostly invariant to the inclusion of public school controls.

Both regressions predict that if a very close public school gets enough additional funding equal to 100 students, the private school is about 3-4 percentage points more likely to close. Because the average public school funding increase translates to 46 students, a very close public school that gets an average funding increase leads to a 1.5 percentage point increase in the exit rate of private schools. This effect is 12% of the overall closure rate (12.5%). The size of this effect indicates that the indirect effect on student sorting from private school closures is likely to be important.

5.3 Private School Entry

In addition to causing some private schools to exit, the public school funding increases may have deterred private school entry. Identifying potential entrants and their exact locations, especially in a city with little available real estate, is difficult. We therefore cannot run our preferred regressions, which examine how a private school's action depends on the funding changes at its several close public competitors. Instead we run regressions with the public school as our unit of observation and look for differential entry patterns within a one-mile radius of each public school.

Specifically, we run the following regression:

$$Entry_k = \zeta_0 + \zeta_1 FSF_k + \xi_k \tag{7}$$

where $Entry_k$ is an indicator for whether public school k had a private school entrant within 1 mile between 2007-08 and 2009-10. We run a similar regression using the number of entrants within 1 mile. We present results in Table 5. We find that for each funding increase of 100 student-equivalents, a public school was 3.4pp less likely to have an private school entrant within 1 mile. The overall entry rate was 13.6%. We thus find evidence that the increased public school competition affected the private school supply by deterring entry.

5.4 Threats to Identification

As mentioned earlier, the public schools that benefited the most from the FSF reform were not randomly chosen. If these public schools were located in areas that had difficulty supporting a private school, the private schools might have closed even in the absence of the reform.[42] We address two types of threats to identification.

The first threat is that certain neighborhoods might have had different preexisting trends. 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

[42] We will describe the threats to identification and our various checks in relation to our primary result: private schools exit in response to the reform. When possible, we execute the same checks for the other main reduced form results in the paper. The results of the checks are consistent with our causal interpretations. Results are available upon request.

attribute the private school closures to the reform when in fact the unobservable trend was the true cause. We check for differential preexisting trends by comparing pre-reform outcomes across schools that would be differentially affected by the reform once the reform was actually implemented.

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 additional placebo tests to discount this possibility.

We examine the first threat to identification – different preexisting trends – by running a series of placebo tests in which we match the private schools from a school year prior to 2007-08 to the public schools and their FSF reform treatments. We thus test whether the FSF reform predicted that the closest private schools closed before the reform was even enacted. These regressions capture the extent to which the "treated" private schools were more likely to close due to slow-changing conditions rather than sudden events like the FSF reform. We run the test on private school closures from 1999-2000 to 2001-02, 2001-02 to 2003-04, 2003-04 to 2005-06, and 2005-06 to 2007-08. As seen in Table 6, the FSF reform only predicts closures from 2007-08 to 2009-10, which indicates that our baseline regressions are not just picking up non-FSF factors that are slow-changing. We thus rule out preexisting trends as a threat to our causal interpretations.

We then 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 a placebo test that makes use of the "hold harmless" clause in the FSF reform. The FSF reform 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 via the "hold harmless" clause. The function translating a school's potential funding change (the difference in funding between the old and new FSF formulas) into the actual funding change thus had a kink at 0.

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 actual funding change. To the right of the kink, both the reform 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 were equally affected by the reform. If the unobservable characteristics were driving our results, then we would expect to see that the potential funding change affected private schools closure rates both to the left and the right 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 reform itself caused the private school closures, then we would expect to see that the potential funding change only mattered to the right of the kink, where the potential change was actually implemented.

We therefore run a placebo test where instead of using the reform's actual funding changes, we use the potential funding changes and split the effects by whether the change was implemented (right of the kink) or not (left of the kink). We find that the potential funding changes were only associated with private school closures when the potential change was actually implemented (see the first column of Table 7). When the "hold harmless" clause determined that the actual funding change would be 0, we find no relationship.[43]

As a second test, we match private schools active in 2007-08 to the public schools and their FSF reform treatments, but we match private elementary schools to public high schools and vice versa. If the effect were recession-specific, then the effect should show up regardless of whether the local public school that received money was an elementary or high school. The results in the second column of Table 7 show that indeed the treatment to the local public high school did not predict private elementary school exits and the treatment to the local public elementary school did not predict private high school exits.[44] A private school's exit probability only reacted to funding changes at public schools of the same level. This

[43]If we run similar tests with "placebo kinks," we find that no matter where we place the "placebo kink," the relationship between private school closures and the potential funding change is stronger to the right of the kink, where the funding changes are positive and largest.

[44]As seen in Figure 6, the number of exiters in NYC increased after the reform's implementation. While this increase could be explained by factors like the financial crisis, we do not see such a clear trend in other large cities. Figures are available upon request.

indicates that differential neighborhood changes, such as vulnerability to the recession, are unlikely to be driving our results.

Another main trend in the NYC education market at this time was the expansion of the charter school sector. In 2007-08 the charter sector was still relatively new and there were only 60 active schools (Hoxby, Murarka and Kang 2009). But private schools may have been particularly susceptible to competition from charter schools, and if charter schools opened (or expanded) near public school "winners" then our estimated effect could be due to charter expansion and not the FSF reform. We run a series of regressions where we add measures of charter school penetration to our baseline regression. These measures include private school j's distance to the closest charter school and the number of charter schools within 1, 3, 5, or 20 miles of the private school. As seen in Table 8, the inclusion of these measures hardly changes our estimated coefficients of interest. We run similar regressions where our charter school controls account for *new* charter schools and find the same results (available upon request). Our estimates thus do not seem related to a charter school story.

5.5 Heterogeneous Effects

Private schools clearly are quite heterogeneous in ways that could affect how responsive they would be to changes in the public schooling sector. We divide our sample of private schools into different groups and look for heterogeneous effects. While we lack the statistical power to reject equality across groups in most cases, the results suggest interesting differences. We first check how the effects differ for private high schools versus private schools that end before grade 9 (usually K-8). We might expect that high schools would be more responsive to the public school funding increase because students have more control over which school they attend via the centralized school assignment. Also, high schools often offer more diverse forms of instruction relative to elementary schools. Therefore, the same funding increase might be spent in a more dynamic way that could attract more students. On the other hand, because high school students often travel farther for school because they can navigate public transportation better and the public high school choice system allows it, a private school may be competing against many schools from across the city. The effect of a funding increase at a local public high school may not have as large an impact. This second story is consistent with our results in the first two columns of Table 9, which show that the effect of the funding increase on private school exit appears smaller for the high schools.

The other basic way that private schools differ from each other is that schools often offer religious instruction in addition to the typical academic instruction, and the importance of the religious component helps determine how substitutable a private school is with a public school. When we compare regression results across Catholic, religious non-Catholic, and non-religious private schools, we see that the effect appears strongest for non-religious schools (last three columns of Table 9). This result is intuitive as non-religious schools are likely most substitutable with public schools. For a religious non-Catholic school, if a public school located next door gets funding equivalent to 100 students, the probability of closing increases by 2.5 percentage points. Surprisingly, the supply of Catholic private schools appears only slightly responsive to the FSF reform. Catholic schools have traditionally been closer substitutes for public schools than are non-Catholic schools. One possible explanation for the result is that the Catholic schools are often run by the archdiocese while the other schools are more likely to be independently run. The archdiocese can offer schools implicit insurance by sharing resources across the city. This archdiocesan control may make individual schools less responsive to their immediate environment.

5.6 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 (6.5% effect for elementary schools), and private schools located next to public schools that received funding were more likely to close (1.5 percentage points more likely to close if the public school received an average funding increase). This likelihood of closure declined as the distance between the schools grew. But these results do not allow us to (1) quantify the impact of private entry and exit on public school enrollments or (2) predict whether the school closure rate would scale up with a larger policy change.

The total effect on enrollment (6.5%) combines the direct effect where students switch to the public school even if no school opens or closes and the indirect effect from private schools opening and closing. To separate these effects, we need to determine the counterfactual demand for a closing school had it stayed open. Ideally we would find two private schools affected similarly by the reform and otherwise identical except that only one school closed. The education market, however, is complicated as schools' enrollments depend on a set of differentiated competitors. The exercise thus proves nearly impossible as it requires that each school's competitors were 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.[45]

Second, the FSF reform is just one of many reforms across U.S. school districts that led to an increase in public school funding, and many public school advocates argue that funding needs to increase further. We therefore want to estimate whether this indirect effect, driven by private school exit, would scale up with a larger public school funding increase. It is possible that the private school closure rate would indeed scale up because even the surviving schools may have lost demand to the public schools. On the other hand, these survivors may have benefitted from the closures of their neighbors and may be better protected from negative demand shocks. Additionally, private school heterogeneity may mean that after the FSF-related closures, the survivors are schools that are less substitutable with public schools or simply much farther from the break even point. To predict these out-of-sample effects, we need a model of school demand and supply.

6 Model and Estimation

6.1 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 make out-of-sample predictions.

In the model, students choose a school based on the school's total quality (net of price)

[45] We could alternatively estimate the direct effect by looking for two identical public schools that received equivalent funding increases except only one public school had a nearby private school close. But again, this proves impossible as we would need the other nearby public and private schools to be identical.

and the distance from the student's residence to the school. 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 school year t is:

$$u_{ijgt} = \delta_{jg} - \gamma_g d_{ij} + \epsilon_{ijgt} \tag{8}$$

where δ_{jg} is the school-grade's total quality and d_{ij} is the distance from i's residence to j. Student i's utility from attending public school k for grade g in school year t is:

$$u_{ikgt} = \delta_{kg} - \gamma_g d_{ik} + \rho_g ZONED_{ikt} + \lambda_g FSF_{kt} + \epsilon_{ikgt} \tag{9}$$

where $ZONED_{ikt}$ is an indicator variable for whether public school k is i's zoned public school, and FSF_{kt} is the amount of additional funding the school received under the FSF reform, divided by the per-student expenditure (units of 100). 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 total quality to change when it receives additional funding. ϵ is an iid 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.

On the supply side, an incumbent private school j makes a single decision: 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:

$$D_{jt}(stay; X, \beta) > F_{jt}. \tag{10}$$

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,[46] we express F_{jt} such that there is probability p that the school will not close regardless of demand and probability $1-p$ the school must attract enough students

[46] These schools may be able to borrow resources from the future, such as future donations, to stay open. We consider the dynamic nature of this problem interesting but beyond the scope of this paper.

to stay open:

$$F_{jt} = \begin{cases} 0 & w.p. \;\; p^{relig} \\ F_{jt}^{exp} & w.p. \;\; 1 - p^{relig} \end{cases}$$

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

$$F_{jt}^{exp} \sim exponential(\mu_{elem}^{relig} NumGradeElem_{jt} + \mu_{mid}^{relig} NumGradeMid_{jt}).$$

Our parameters to be estimated are p^{relig}, the probability the school will stay open regardless of demand, and μ_{elem}^{relig} and μ_{mid}^{relig}, the average number of students the schools needs to attract per elementary and middle grade, respectively. Cost structures may vary by the school's religious association ($relig$), so we estimate separate parameters for Catholic schools, non-Catholic religious schools, and non-religious schools. Schools make the stay or close choice sequentially, from the school with the highest demand to the school with the lowest demand.[47] We choose this sequential decision structure 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, even before other schools have acted.

The demand model uses the $\lambda_g FSF_{kt}$ term to account for the increased enrollment shares of the FSF "winners." But the model leverages the importance of distance in school choice so that the effect of an FSF funding increase at another school depends on spatial competition. The demand estimates then relate to the supply side by correlating these school-specific competition shocks induced by FSF with the observed exit decisions.

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 Appendix D.

[47]To determine the sequence, demand is first calculated assuming all incumbents will stay.

6.2 Estimation

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 with school attended, we use 2010 Census population counts to construct student locations. The data indicate which Census block a child lived in, and we place the student at the geographic centroid of the block. 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.[48] We combine this data with our enrollment data for public and private schools and our measures of FSF funding.

We estimate our demand model using data from the 2001-02, 2003-04, 2005-06, 2007-08, and 2009-10 school years to cover student enrollment decisions before and after the implementation of the FSF reform. Our demand model is held constant across the years except that the students' choice sets account for entry and exit in each year and the FSF funding enters starting in 2007-08. In particular, our measures of schools' total quality, δ, are fixed across years. This means that our model attributes enrollment changes over time to changes in competition from entry and exit rather than changing school characteristics. This assumption that schools' non-FSF total qualities are fixed over time is necessary for identification of the indirect effect, as we must predict a closing school's quality had it remained open. We estimate our supply model using school closure decisions between 2005-06 and 2009-10. These decisions are most closely related to the FSF reform.

To estimate the demand parameters, we use an exactly-identified method of moments procedure. The first set of moments come from aggregate enrollment data. For each school-grade, we calculate its average observed enrollment share across all five estimation school years. Then because the model holds schools' total quality (δ) fixed across years, our predicted enrollment shares will not necessarily match enrollment shares in a given year.To exploit how the FSF reform affected enrollment shares over time, 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" after the reform was implemented. The second set of moments are constructed from the NYC

[48]Future versions will use the NYC DOE's web tool.

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 time-invariant parameters using the student sorting patterns prior to the reform.[49] The extent to which a school's enrollment differs from the relative number of local school-aged children helps identify δ. If school j has many school-aged children living nearby but a small enrollment, we would estimate a low δ_j. Our moments derived from the student-level data help identify γ_g and ρ_g. The average distance from a student's zoned school to her actual public school identifies 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 identifies γ_g. Then, the percentage of public school students who attend their zoned school helps us pin down ρ_g.

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 funding increase. These public school switchers did not have any of their most preferred options eliminated, so their sorting pins down the effect of the FSF funding on preferences, λ_g.[50] 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.

We then estimate the supply model (the fixed cost parameters μ_{elem}^{relig} and μ_{mid}^{relig} and the probability the school has zero fixed cost, p^{relig}) using maximum simulated likelihood and the demand estimates. We restrict the schools to private schools that were active in the 2005-06 school year and compare the model's predicted exits to the actual exits between 2005-06 and 2009-10. 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. We solve the model sequentially via backward induction, starting with

[49] We still use variation from after the reform to identify these parameters in the model, but the data from before the reform are sufficient. The one exception is if school j was only open after the reform, estimating δ_{jg} requires data from after the reform.

[50] Sorting from private schools that remained open to public schools that received additional funding also helps with identification.

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, which allows us to iterate on the elimination of strictly dominated strategies and simplifies the estimation.

The closure rates of schools with very low enrollments per grade help us pin down p^{relig}. If the closure rate for these schools is very low, then p^{relig} will be high, as a large percentage of schools must have zero fixed costs in our framework. The μ parameters then govern 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). Finally, we use the variation in schools' grade structures to separately identify μ^{relig}_{elem} from μ^{relig}_{mid}. For example, if closure rates are lower for K-5 schools relative to K-8 schools with equivalent demand per grade, then we would find $\hat{\mu}^{relig}_{elem} < \hat{\mu}^{relig}_{mid}$.

7 Results and Counterfactuals

We estimate the demand model separately for each grade from kindergarten to eighth grade. Kindergarten (and many sixth-grade) students may be particularly responsive to the FSF reform because they arguably do not have any switching costs. Younger students, however, might care less about school funding if the elasticity of school quality with respect to funding is smaller for lower grades where instruction methods (and enrichment programs) are less diverse. Older students likely derive less disutility from distance as they are better able to navigate public transportation.

We find large effects on utility of distance and whether the public school is the zoned school (Table 10). For kindergarteners, we estimate γ at 0.90, ρ at 3.88, and λ at 0.11. The distance and zoned school coefficients decline in magnitude as students become older, which is consistent with the change in the demand moments across grades (top panel of Table 10). Older students tend to travel farther to school and are less likely to attend their zoned schools. These two sets of coefficients are large relative to the estimates of school total quality. For kindergarteners, an increase of one mile in distance is the equivalent of moving from the median public school based on quality to the 9th percentile public school. Similarly, changing a kindergarten student's zoned school is about half the difference between the best and worst public schools.

The coefficient on FSF funding, λ, is positive for all grades, indicating that students shift their enrollments toward FSF "winners" after the reform. The coefficients are larger for elementary grades (averaging 0.11 across grades K-5) than for middle school grades (averaging 0.06 across grades 6-8). This pattern seems surprising given that the enrollment shares for FSF "winners" increase much more after the reform for the middle school grades than for the elementary school grades. In this case, the raw moments hide the fact that several public middle school "losers" closed after 2007, which caused the "winners" share to rise, perhaps independently of the reform. The model, which accounts for expected enrollment increases from other public schools closing, suggests that conditional on choice sets elementary schoolers respond more strongly to the FSF funding changes. The coefficient on the FSF funding increase is also very large. The average FSF "winning" public school gets a utility change equal to 14% of the total quality at the average public school. The large coefficient implies that the direct effect from the reform is important.

Our demand model attributes 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 on our model's predicted enrollment for year t and a set of school fixed effects, we estimate a coefficient of 0.51 (0.03). This predictive power is notable because our estimation moments are not designed to capture these year-to-year fluctuations.[51] Our model's reliance on market structure changes to predict enrollment changes thus appears reasonable.

To determine the percentage of the total change in enrollment at FSF "winners" that is due to the direct enrollment effect, we calculate each school's counterfactual demand in 2009-10 had no private schools opened or closed following the FSF reform. 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. We estimate that

[51] Our only demand moment that picks up time variation is the enrollment share of FSF "winners" after the reform. If we restrict our regression to the years prior to the reform, when we have no demand moments that capture year-to-year fluctuations and rely exclusively on variation from schools opening or closing, we get very similar fit.

70% of the reform's enrollment increase came from students valuing FSF "winners" higher after the reform. In other words, we estimate that the direct effect makes up 70% of the total effect (or the indirect effect makes up 30%).[52]

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 24% of Catholic schools, 61% of non-Catholic religious schools, and 77% of non-religious schools will remain open regardless of demand (Table 10). These differences reflect differences in exit rates for small schools across these religious categories. We estimate that the average Catholic school requires 21 students per elementary grade and 25 students per middle grade to stay open. For non-Catholic religious schools, we get slightly higher estimates (28 and 35), which may reflect that many Catholic parishes considerably subsidize their schools.[53]

The larger estimate for middle school grades is consistent with the change in instruction after grade 5, as most schools transition from a single teacher per grade to teachers who specialize in certain subjects. This specialization, while it can be combined across middle school grades, usually requires hiring more teachers. Thus, the larger estimated number of middle school students necessary to overcome fixed costs is consistent with the increased specialization, though we note that the estimates are quite imprecise. For non-religious schools, on the other hand, we estimate a much smaller number of middle school students

[52]To relate these model-predicted enrollments to the FSF reform, we run two differences-in-differences public school enrollment regressions analogous to Equation 4. First, we regress the model-predicted counterfactual enrollments on the change in FSF funding (in student-equivalents), year fixed effects, and school fixed effects. Second, we run the same regression using the model-predicted actual enrollments on the left-hand-side and compare the coefficient on FSF funding across the two regressions. The ratio of the coefficient in the counterfactual enrollment regression to the coefficient in the actual enrollment regression captures the direct effect's fraction of the total effect.

[53]The fact that Catholic schools have lower estimated parameters indicates that the relationship between exit rate and enrollment per grade is strongest for the Catholic schools. This result is consistent with Catholic schools operating within an archdiocese which may make more centralized decisions. This would induce a common fixed cost across the schools that would lead to the smallest schools closing. However, we cannot rule out other explanations that would induce a tight fixed cost distribution. For instance, the Catholic schools might make independent choices but have similar cost structures.

(5) per grade to cover costs. Our estimates for non-religious schools suffer from a small number of such schools and less variation in grade structure across schools. Our ability to separately identify μ_{elem} from μ_{mid} is thus limited.[54]

Using our supply estimates, we make an out of sample prediction of the enrollment change at FSF "winners" if the FSF reform were twice as large. We simulate this counterfactual by doubling the FSF funding change at schools that received money. Were a larger reform proposed, it would likely spread the funding across the relative "losing" schools as well. But for this counterfactual we seek to focus on the private schools that remained open even after the FSF reform. If these survivors are likely to close with a larger funding change, then private school exit is likely not unique to the specific FSF reform.

Our results indicate that indeed school closures are likely to continue with a larger funding change. Because the distribution across schools of the number of students necessary for keeping the school open has a high variance, we are unable to make strong predictions about a given school closing. Instead we simulate across many draws from the distribution and estimate the exit rate across all schools. We predict that on average 1.6% of the private schools active in 2007-08 exited because of the reform. When we double the reform, we predict that on average 1.4% of the remaining schools would exit. Thus, we find that private school exit likely affects policy evaluation across a large range of funding changes.

8 Aggregate Achievement and Expenditure

We have analyzed how the FSF reform and its associated private school supply response affected students' choices and schools' enrollments. Now we turn to other outcomes — aggregate achievement and public expenditure — that are important for policymakers and particularly affected by students switching between the private and public sectors.

The reform affected aggregate achievement through two channels. First, the reform gave additional funding to certain schools, which could have changed their quality. We call this the "quality effect." Second, students' enrollments shifted toward the schools that

[54]If we constrain the non-religious estimates so they are the same across elementary and middle school grades, we estimate costs equivalent to 41 students per grade. For the costs of the other types of schools, the elementary grade estimates are a bit smaller and the middle grade estimates larger. Our model fit and counterfactual results are largely unchanged.

received funding and away from other public schools and private schools. Even if no school's quality changed, if schools' enrollments changed then we might find an effect on aggregate achievement. We label this effect the "sorting effect."[55] Due to data constraints, we will treat all of a school's students as receiving the same level of quality.[56]

We measure schools' quality using test scores from the NY State mathematics and ELA tests for grades 3-8. These tests are administered to all public school students. Unlike most other states' testing programs, a large number of NY private schools also take the tests and report their results.[57] This allows us to compare achievement across the two sectors. A limitation of our data on private school test scores is that we do not observe individual students' scores. Instead we observe the mean scores for each school-grade-year. Below we discuss the adjustments we make because we only observe the mean scores. These adjustments require some strong assumptions and thus our results should be taken as suggestive, not definitive, effects on achievement.

To measure school quality, we estimate schools' value-added with standard methods. For public schools, we run student-level regressions with school fixed effects where we condition on a student's test scores in the previous grade. 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):

$$\begin{aligned} y^s_{i,k,g,t} = {} & \beta_1 y^{math}_{i,g-1,t-1} + \beta_2 (y^{math}_{i,g-1,t-1})^2 + \beta_3 (y^{math}_{i,g-1,t-1})^3 + \beta_4 y^{ela}_{i,g-1,t-1} \\ & + \beta_5 (y^{ela}_{i,g-1,t-1})^2 + \beta_6 (y^{ela}_{i,g-1,t-1})^3 + X_i' \beta_7 + \theta^s_{k,g,t} + \epsilon^s_{i,k,g,t}. \end{aligned} \tag{11}$$

A student's test score, $y^s_{i,k,g,t}$, is standardized so that scores across a subject-grade-year for public school students have mean 0 and standard deviation 1. We use the estimated school-grade-year fixed effects as our value-added measures. Students of course are not randomly assigned to schools. The validity of our estimates requires that, conditional on prior test

[55]The quality and sorting effects may not be independent if a school's quality depends on the types of students it enrolls. We abstract away from such interactions like peer effects because we cannot identify them separately from our quality and sorting effects. But we consider changes in peer effects from private school entry and exit an important avenue for future study.

[56]Even in the absence of peer effects, there could be heterogeneity in students' achievements at different schools. Without student-level private school data, we cannot measure such heterogeneity. Our estimates would change considerably if, say, the students leaving private school j were the students who derived the least benefit from that school's quality.

[57]The private schools usually only take tests in grades 4 and 8.

scores and student demographics, students with higher expected test scores do not select into certain schools. We estimate additional value-added regressions where we use only students who stayed in the same school post-2007 or only students who switched schools post-2007. As detailed below, we find similar qualitative results using these samples of students.

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.[58] We recover the estimated school fixed effect (θ_j^s) from the following regression for private school j:

$$\bar{y}_{j,8,t}^s = \alpha \bar{y}_{j,4,t-4}^s + \mu_t^s + \theta_j^s + \epsilon_{j,g,t}^s \tag{12}$$

where $\bar{y}_{j,g,t}^s$ is the average test score at private school j for grade g in year t.[59] We find that a cohort's average grade 4 test scores are only partial predictors of grade 8 scores, as $\hat{\alpha}$ is 0.21 (0.05) for math and 0.11 (0.05) for ELA.[60]

Because students may switch schools, cohort-level value-added may attribute some of a school's quality to the changing composition of students. Without student-level data we have no perfect way of fixing this problem. Instead, we make adjustments to private schools' value-added based on the changing composition of students in the public school system. For instance, if the students who leave the public school system are positively selected on test scores, then we assume some of these students went to private schools and we adjust the private schools' value-added downward. We provide more detail in Appendix C.

Our other data limitation is that we only have value-added estimates for 36% of the private school students.[61] We assume that the schools in our data are drawn from the same distribution of value-added as the missing schools. We discuss this strong assumption in Appendix C and offer evidence in support.

With these assumptions and adjustments, we calculate a private school annual premium of 0.03σ in math and 0.05σ in ELA. While these estimated premiums are local to the context,

[58] This methodology is similar to the "synthetic gains" methods used in the literature before student-level data were more widely available (e.g., Ehrenberg and Brewer 1995).

[59] As with the public school students' scores, we standardize the private school mean scores by the public school mean and standard deviation.

[60] When we run student-level regressions on public school students' test scores, we estimate very similar levels of predictiveness for four-year lagged test scores.

[61] While a higher fraction of private schools take the tests in any given year, our value-added measures require that the school cover grades 4 and 8 and report test scores over at least a four-year period.

they are broadly in line with estimates from NYC's 1997-98 – 1999-2000 voucher trial.[62]

8.1 The Quality Effect

We estimate the reform's effect on public school value-added using the same differences-in-differences framework we used to study enrollment effects. We regress estimated value-added on year, school, and grade fixed effects as well as our policy measure:

$$\hat{VA}_{kgt} = \lambda_t^{VA} + \kappa_k^{VA} + \omega_g^{VA} + \pi^{VA} FSF_k After2007_t + \mu_{kgt}^{VA}. \tag{13}$$

We run separate regressions for math and ELA and present the results in Table 11. While we do not find any statistically significant relationship between FSF funding and ELA value-added, the FSF funding led to higher math value-added. A school that received funding equivalent to 100 students had an increase of value-added of 0.02 test standard deviations. We also address concerns that the results may be driven by students switching schools. In the third through sixth columns we find qualitatively similar results when we calculate value-added with only students who stayed in the same school post-2007 or only students who switched schools post-2007.

As a specification test, we use the kink in the relationship between the potential FSF funding change and the actual FSF funding change and find that the relationship between FSF and value-added is only positive when the potential change was actually implemented. The change in value-added does not seem to be driven by omitted factors related to the potential funding change.

Our estimates of the effect of funding on value-added are relative measures. By construction any increase in value-added for some schools must be offset by a decrease for other schools. In assessing the reform's effect on aggregate achievement, we must translate these estimates to an absolute change. We assume that schools that did not receive additional

[62]NYC ran a randomized voucher trial that awarded \$1,400 vouchers to 1,300 students. Using the Iowa Test of Basic Skills, Mayer, Peterson, Myers, Tuttle and Howell (2002) find African-American students who attended private school enjoyed an annual test score premium of 0.10σ in reading and 0.17σ in math. Other students, however, did not see test score increases, as the premium across all students was roughly 0.01σ in reading and 0.04σ in math, both not statistically significant. Our estimates come from data starting in 2000-2001. Because entry is positively selected and exit is negatively selected, we see improvements in aggregate private value-added over time, which could explain our slightly higher estimate in ELA.

funding experienced no change in value-added due to the reform.[63]

This result is important because much of the literature has not found a causal relationship between school funding and school quality. The positive relationship between FSF funding and math value-added suggests that school funding can affect a school's quality and helps explain why we find such a large enrollment response to the reform. The possible effects on private school quality, from the increased competition from public schools, are also interesting and have been highlighted in other work (Neilson 2013). Unfortunately because our private school measures require long differences over four years, we are unable to measure such changes well. In our aggregate achievement calculation, we therefore assume that private schools do not change their quality.

8.2 The Sorting Effect

We have found that many students who stay at the same public school experience an increase in average achievement. But aggregate achievement effects also depend on students sorting between schools. We consider several types of student switches in response to the reform. First, students who switch public schools tended to shift toward schools that received FSF funding. These schools, though they increased their value-added after the reform, started with slightly lower value-added before the reform. The net effect on achievement from these switchers is zero.

Second, some students switched between two private schools. These students tended to switch to private schools with higher value-added. Much of this increase was driven by the school exits. When we regress an indicator for whether a school exited after the reform on measures of value-added, we find a negative association. See Table 12 for the regression results. Thus, school exit increased the average achievement for students who remained in the private sector.

The reform also decreased private school entry, and we find that entrants have high

[63]This assumption could overstate the increase in aggregate achievement if school quality is a scarce resource within the school district. For instance, if the "winners" improved by taking the best teachers from the other public schools, then the reform caused these "relative losing" schools to fall in quality and we are overstating the aggregate quality increase. Using teacher-level data, we find some evidence that quality rose at "winners" at the expense of "relative losers" (Appendix B).

value-added on average.[64] Because the FSF reform led to less sorting to new private schools, student achievement fell. We assume that the effects from increased exit and reduced entry cancel each other out.[65]

Finally, some students switched from private to public schools. Because we estimated a large average private school premium, many of these students experienced a large decrease in their school quality. Even though these students made up a minority of all switches, they potentially drove much of the sorting effect on achievement.

8.3 Net Effect on Achievement and Earnings

We convert the aggregate achievement effects to changes in present value of lifetime earnings and summarize the results in Table 13.[66] The quality effect led to an increase in the present value of lifetime earnings due to quality improvements at the public schools that received additional funding. The present value of lifetime earnings increased by up to \$34 million from ELA improvements and \$86 million from math improvements.

The sorting effect was dominated by students switching from private to public schools. This led to a decrease in earnings. We first calculate the sorting effect using the ("smaller") private school premium we estimate. We also note that most of the enrollment decrease in the private sector empirically came from African-American students, who may have a larger premium from attending private school. We thus calculate the sorting effect a second time, using the "larger" premium Mayer et al. (2002) estimate for African-American students.

The sorting effect dampens the total increase in aggregate achievement and, thus, lifetime earnings. We estimate that the reform had a positive effect on aggregate lifetime earnings, but the effect would have been up to 4 times larger for ELA and 1.5 times larger for math had there been no substitution from the private to public schools. For a simple cost-benefit analysis, the reform spent \$233 million annually on larger school budgets. If we assume the funding was spent equally across grades, \$89 million was spent annually on fourth through

[64] We cannot observe quality for potential entrants that do not enter. Our calculations thus describe entry prior to the FSF reform.

[65] We motivate this assumption in Appendix C.

[66] We estimate the reform's effect on test scores in standard deviation units. We then sum these effects across all fourth through eighth graders. We convert this test score change to changes in 28-year-old wages using estimates from Chetty, Friedman and Rockoff (2014). We then assume that students enjoy these wage increases from ages 22 to 65, that annual wage growth is 2%, and that the discount rate is 5%.

eighth graders. The total effect on these students' earnings ranged from $8-21 million in ELA and $55-80 million in math, depending on the size of the private school premium. Whether the policy's benefits outweighed its costs therefore depended on the size of the sorting effect.[67] This highlights the importance of considering demand shifts from the private sector, even for a policy targeting the public sector.

8.4 Effect on Expenditure

We estimate that the private school enrollment share fell by 1.2pp due to the reform.[68] We now calculate how these new public school students affected public education spending. Student funding depends on students' characteristics, such as their previous test scores and whether they are English language learners. For a conservative estimate, we assume that these switchers had no special "needs." Elementary students cost $3,788 each, middle school students cost $4,091 each, and high school students cost $3,902 each.[69] We calculate that the private to public switchers cost the school district $53.5 million, which is 23% of the total increased funding from the FSF reform. The increased public spending on education can have consequences for other governmental programs as well as the level of monetary redistribution through public education.

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.

[67] Even if the policy's benefits outweighed its actual costs, other cost-equivalent policies might have produced larger benefits.

[68] The calculation requires some assumptions. See Appendix C for details.

[69] These estimates do not include all spending. In particular, they exclude spending on capital and teacher pensions. We assume no changes in spending in these areas.

Our empirical analysis indicates that private schooling supply was responsive to a public school funding reform. We estimate that a private school located next to a public school that received an average funding increase was 1.5 percentage points more likely to close in the next two years. Using our model estimates, we find that this change in supply of private schooling explained 30% of the enrollment increase that the public school "winners" enjoyed. These private school exits caused some private school students to attend lower-quality schools, which potentially undid much of the reform's positive impact on achievement.

Our results have important policy implications as they show that the private sector is likely to adjust to schooling policies. For example, Tennessee is on the verge of 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.[70] 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.[71] Even traditional public school exit has become more common. Several large cities with declining populations have closed a group of schools at once, but some cities like NYC have started to close public schools on a case-by-case basis. Students' menu of schooling options are likely to continue changing with the increased churn of schools.

Finally, our study of a public school funding reform finds evidence that families value the funding in their school choices and that schools use the additional funding to increase the quality of their instruction. The sensitivity of preferences and quality to funding is important for a broad range of policies that affect a school's resources, even if the policies do not explicitly target them. Thus, policymakers need to account carefully for how they might affect schools' budgets.

[70] "Researchers Highlight Supply-Side Shortages for Voucher Programs" *Education Week*, April 4, 2014.

[71] 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).

We have abstracted away from several key components of school demand and supply that warrant further study. In particular, we have not focused on peer effects in school choice. Several papers have demonstrated that families choose schools based on the demographics of the other students at the school (e.g., Hastings, Kane and Staiger 2010, Epple et al. 2013). To the extent private school entry or exit changes other schools' demographics, these peer effects could lead to large changes in the types of students attending each school. We consider this an important avenue for further research.

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10 Figures and Tables

Figure 1: Funding Change Formula

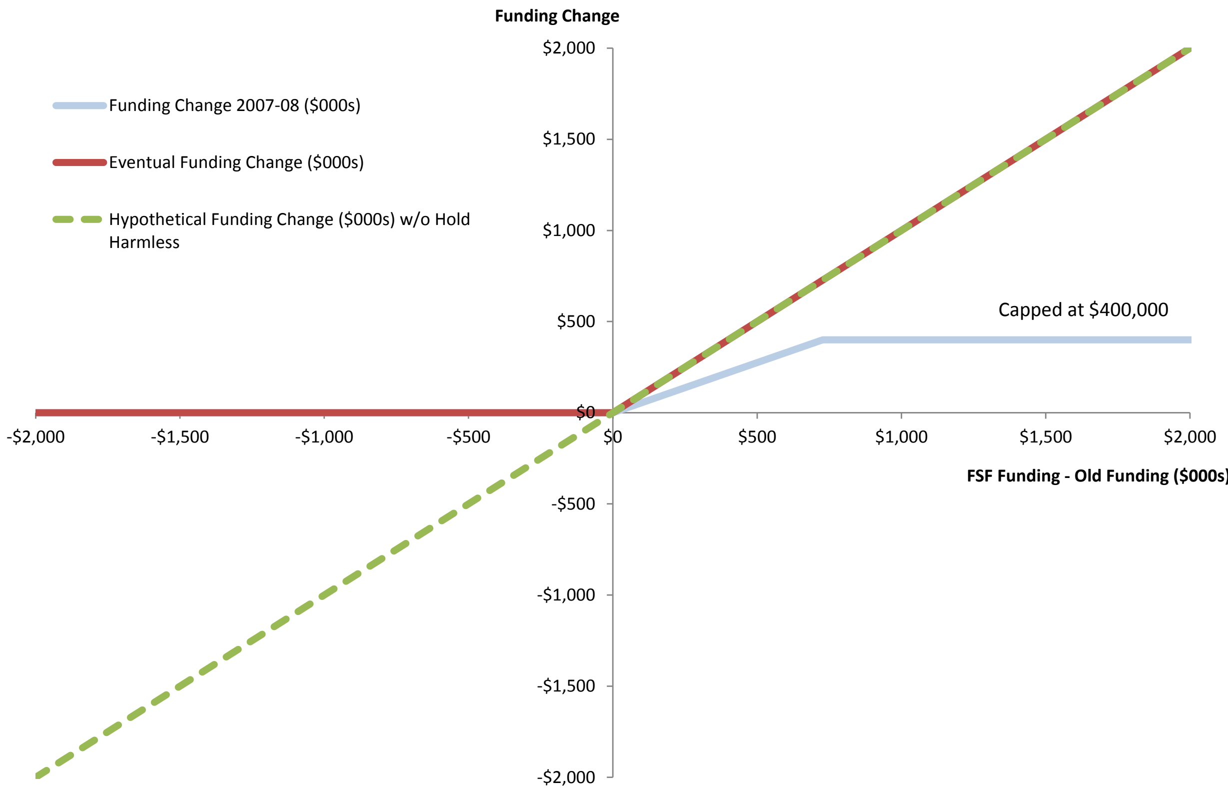


Figure 2: Example Schools

Figure 2a: 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 2b: 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 3: Density of Funding Change (Excluding 0s)

Figure 3a: Density of Funding Change as % of Budget (Excluding 0s)

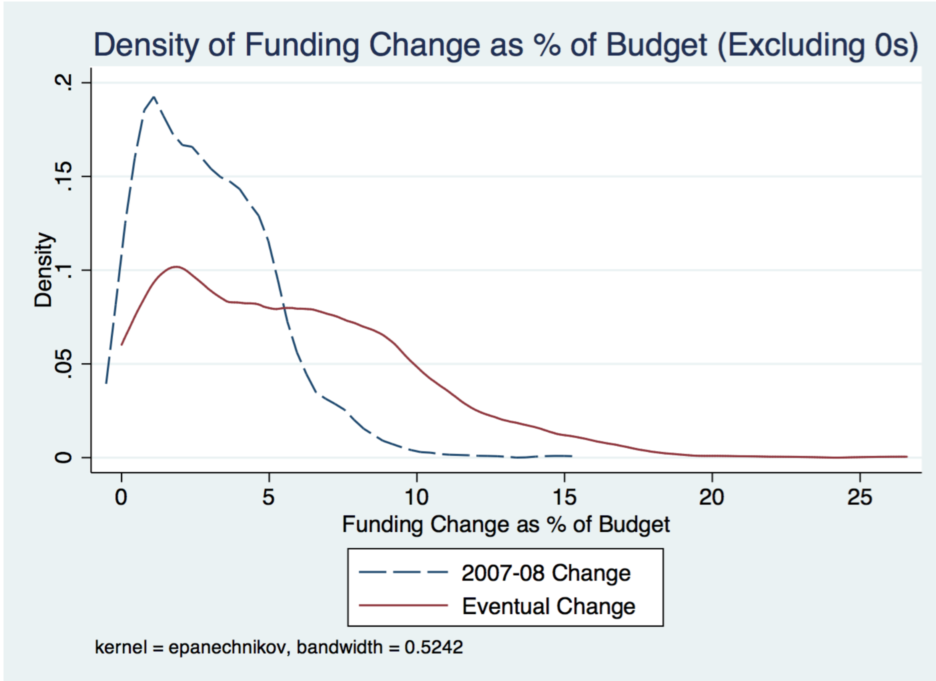
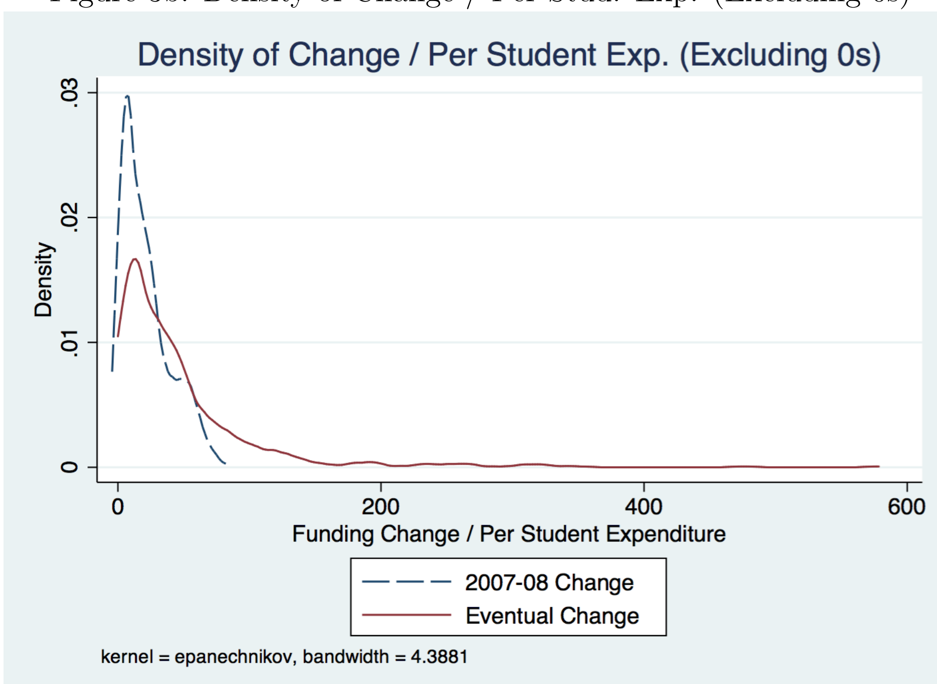


Figure 3b: Density of Change / Per Stud. Exp. (Excluding 0s)



48.8% of schools had no funding change

Figure 4: Locations of Public Schools

Figure 4a: Public Schools in Brooklyn by HH Income

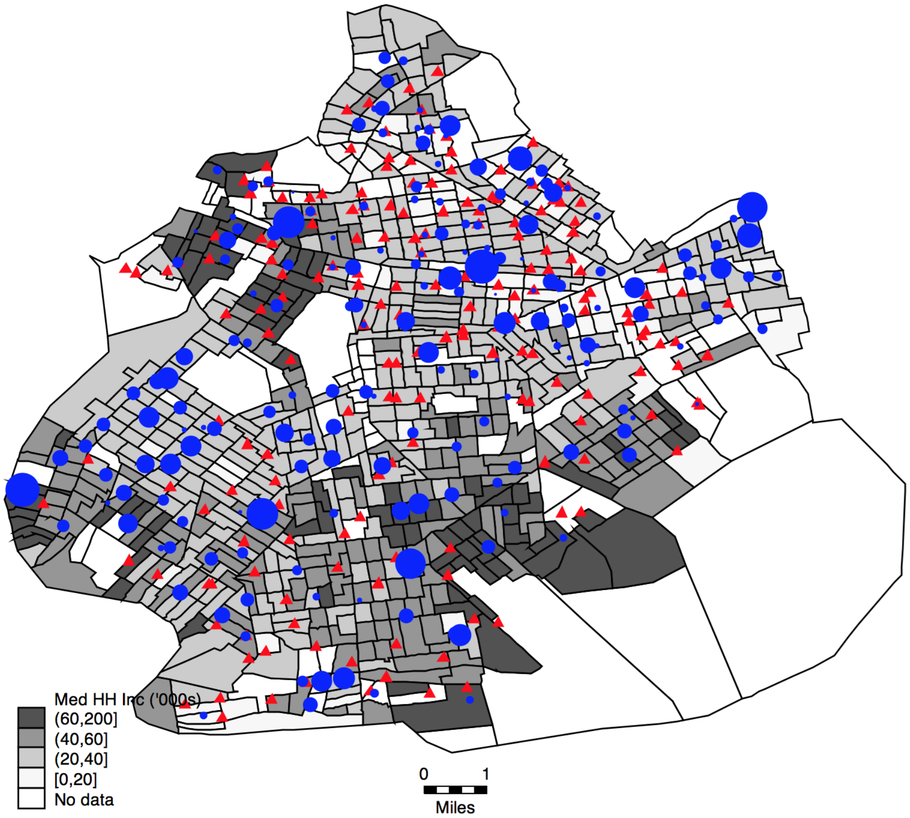


Figure 4b: Public Schools in the Bronx by HH Income

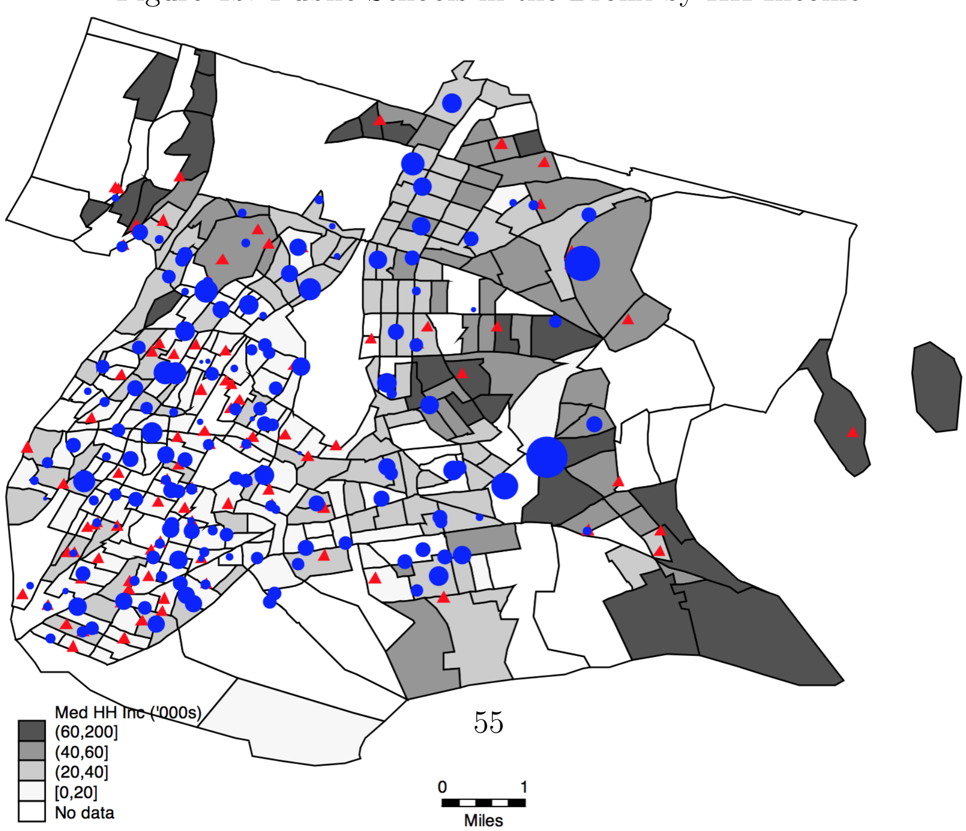


Figure 5: Locations of Private Schools

Figure 5a: Private Schools in Brooklyn by HH Income

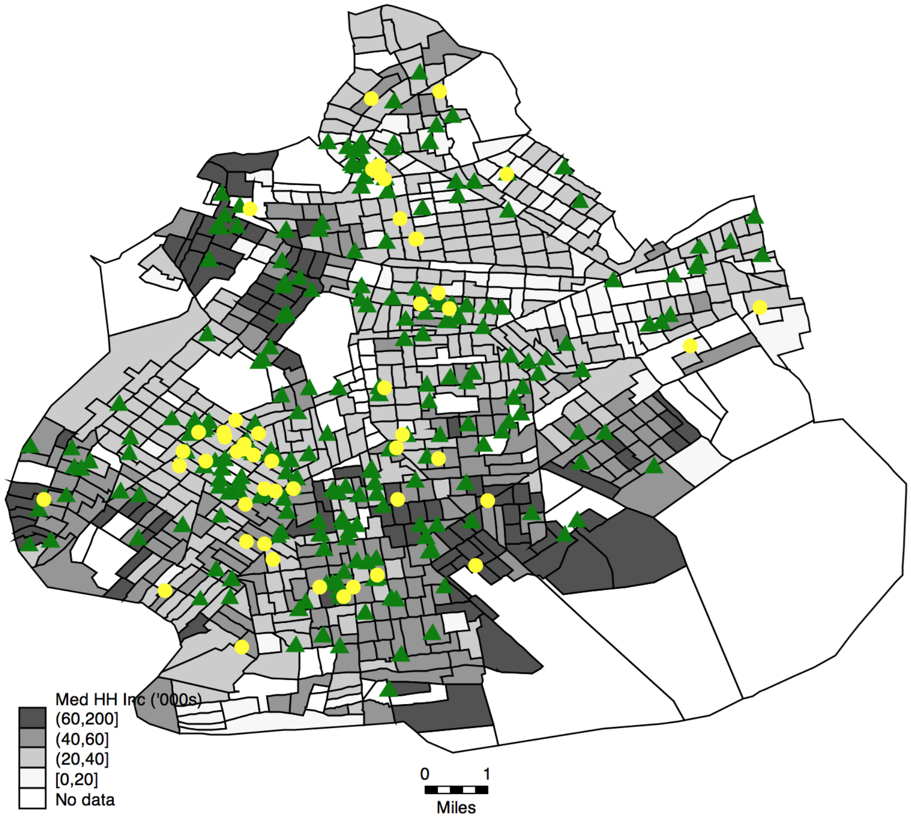


Figure 5b: Private Schools in Brooklyn by Adult Education

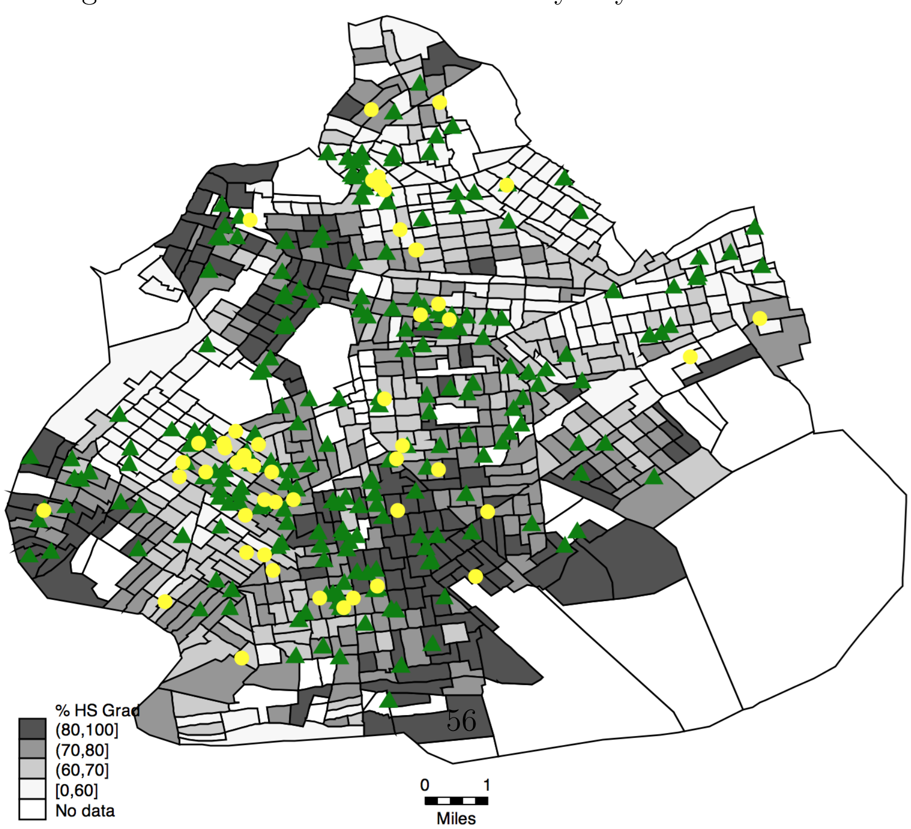
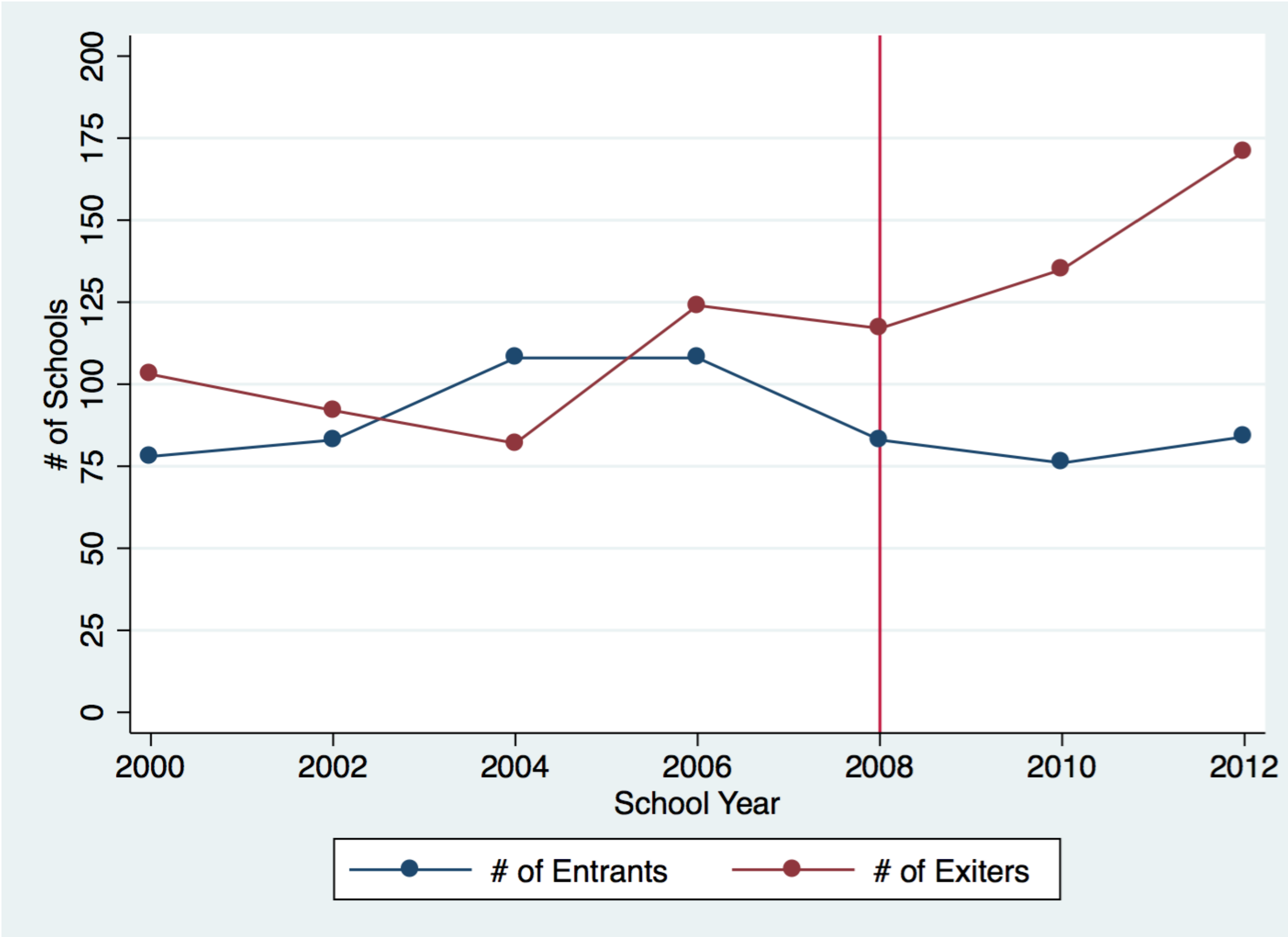


Figure 6: Number of Entrants and Exiters in NYC



Note: Entry and exit are 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. The red line marks the implementation of the FSF reform.

Figure 7: Enrollment for FSF "Winners" and "Losers"

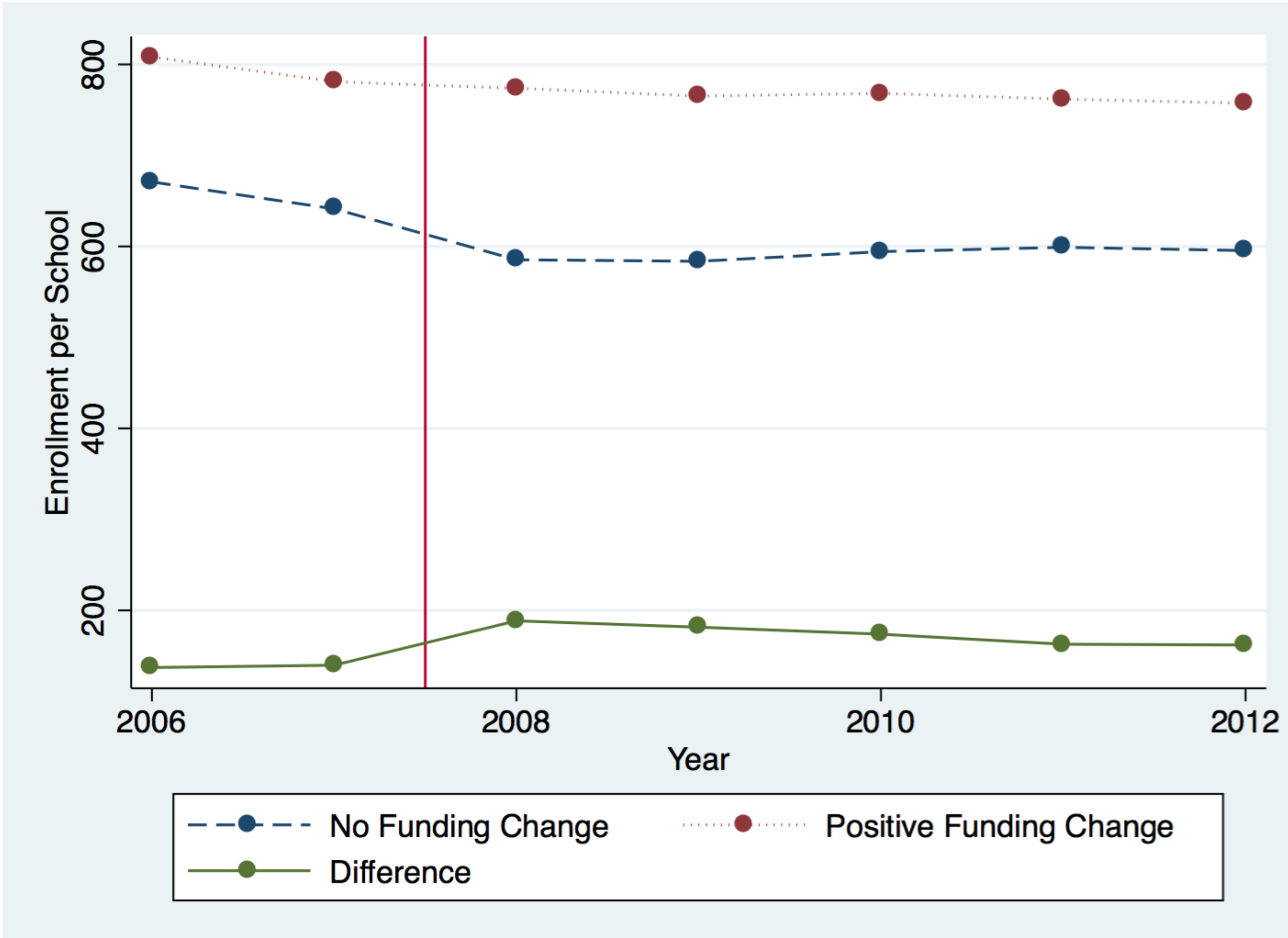


Table 1: Regressions of Funding Change on Public School Demographics and Teacher Characteristics

		Regressions	
	Mean	1(Funding Change > 0)	Total Funding Change / Exp. Per Student
% Free + Reduced Lunch	0.73	0.162**	-0.125
		(0.064)	(0.113)
% Stability	0.90	-0.103	-1.658***
		(0.221)	(0.340)
% Limited English Proficiency	0.14	0.613***	-0.046
		(0.140)	(0.209)
% Black	0.35	-0.104	-0.472***
		(0.096)	(0.175)
% Hispanic	0.40	0.164	0.165
		(0.115)	(0.186)
% Teacher No Valid Certificate	0.06	0.672	-0.014
		(0.409)	(0.634)
% Teacher without Certification	0.11	-0.068	0.454
		(0.242)	(0.382)
% Teachers < 3 Years Experience	0.19	0.971***	-0.269
		(0.119)	(0.190)
% Teacher Turnover (within 5 Years)	0.21	-0.185	0.079
		(0.132)	(0.279)
% Turnover (All)	0.18	0.035	-0.159
		(0.214)	(0.403)
Constant		0.177	2.153***
		(0.223)	(0.356)
N		1,222	615
R-Squared		0.180	0.166

* < 10%, ** < 5%, *** < 1%. The last two columns are regressions of funding change measures on a public school's demographic and teacher characteristics in 2006-07. The last column measures the funding change expressed in student-equivalents, conditional on receiving money. The % Stability is a NY State measure that captures the percentage of students who are in the grade normally associated with a certain age.

Table 2: Private School Summary Statistics

School Characteristics	Schools Active in 2007-08
Number of Private Schools	771
% Catholic	39%
% Other Religious	46%
% Non-Religious	15%
% Elementary	64%
% Secondary	17%
% Combined	18%
Enrollment per Grade (Mean)	42
% with Enrollment per Grade < 10	12%
% with Enrollment per Grade < 20	20%
% Black	17%
% Hispanic	16%
% of Schools with > 50% Minority	39%

Most data come from the 2007-08 NCES Private School Survey. The exceptions come when a school appears in earlier and later versions of the NCES Private School Survey but not in the 2007-08 version. We consider the school as still open during 2007-08 and use its characteristics from the closest observation before 2007-08. Elementary schools end before grade 9 and secondary schools start in grade 9. Minority students are black or hispanic.

Table 3: Differences-in-Differences Regressions of Enrollment

	All Schools ln(Enroll)	Elementary Schools ln(Enroll)	Elementary Schools Enroll	Elementary Schools ln(Enroll)	Elementary Schools Enroll
Winner * After 2007	0.123***	0.065***	19.114*		
	(0.009)	(0.017)	(11.046)		
FSF * After 2007				0.065***	11.784
				(0.025)	(19.053)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School
N	13,335	7,982	7,982	7,936	7,936
R-Squared	0.877	0.890	0.901	0.887	0.900

* < 10%, ** < 5%, *** < 1%. Data span 2001-02 through 2011-12 school years. "Winner" schools are those that received funding from the FSF reform. The After2007 dummy variable is 1 starting in the 2007-08 school year. "FSF" is the funding change divided by the per student expenditure, in units of 100 student-equivalents. Elementary schools are schools that have students in grades K-5. Standard errors are clustered by school.

Table 4: Baseline Private School Exit Regressions

	1(Exit)	1(Exit)	1(Exit)	1(Exit)
FSF * Dist1to2	0.043**	0.039**		
	(0.018)	(0.017)		
FSF * Dist3to5	0.005	0.002		
	(0.014)	(0.013)		
FSF * Dist6to10	0.013	0.009		
	(0.012)	(0.012)		
FSF			0.034**	0.032**
			(0.015)	(0.015)
FSF * Distance			-0.006	-0.006*
			(0.004)	(0.004)
Overall Exit Rate	0.12	0.12	0.12	0.12
Public School Controls		Yes		Yes
Fixed Effects	Subdistrict	Subdistrict	Subdistrict	Subdistrict
N	7,144	7,144	7,144	7,144
R-Squared	0.034	0.035	0.034	0.035

* < 10%, ** < 5%, *** < 1%. An observation is a private school matched to a public school. FSF measures the public school's FSF funding change divided by the per student expenditure (in 00s). Dist1to2 is a dummy for whether the public school is the first or second closest public school to the private school. Distance between public and private schools is measured in miles. Standard errors are clustered at the private school and observations are weighted by the inverse of the number of public school matches.

Table 5: Private School Entry Regressions

	1(Any Entrant within 1 Mile)	Number of Entrants within 1 Mile
FSF	-0.034*	-0.052*
	(0.018)	(0.027)
Constant	0.136***	0.173***
	(0.030)	(0.045)
N	1,091	1,091
R-Squared	0.002	0.003

* < 10%, ** < 5%, *** < 1%. An observation is a public school. FSF measures the public school's FSF funding change divided by the per student expenditure (in 00s). The dependent variable measure the number of entrants between 2007-08 and 2009-10 within 1 mile of a public school. Standard errors are clustered by subdistrict.

Table 6: Placebo Tests - Mismatched Timing

	1999-2000 to 2001-2 1(Exit)	2001-2 to 2003-4 1(Exit)	2003-4 to 2005-6 1(Exit)	2005-6 to 2007-8 1(Exit)	2007-8 to 2009-10 1(Exit)
FSF	0.009	-0.005	0.014	0.011	0.032**
	(0.010)	(0.013)	(0.013)	(0.010)	(0.015)
FSF * Distance	-0.000	0.001	0.002	0.000	-0.006*
	(0.003)	(0.005)	(0.005)	(0.003)	(0.004)
Constant	0.022	0.103*	0.042	0.111*	0.258***
	(0.067)	(0.054)	(0.062)	(0.058)	(0.099)
Public School Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict	Subdistrict	Subdistrict	Subdistrict
R-Squared	0.032	0.042	0.071	0.038	0.035

* < 10%, ** < 5%, *** < 1%. Each column matches the private schools active at the beginning of the indicated time period to public schools and their 2007-08 FSF funding changes. The exit dummy is 1 if the private school exited in the indicated period. The last column is our baseline regression. Standard errors are clustered at the private school and observations are weighted by the inverse of the number of public school matches.

Table 7: Placebo Tests - Variation across Relative Losers and Level Mismatch

	Sample: All Schools 1(Exit)	Sample: Level Mismatch 1(Exit)
FSF	0.037**	-0.006
	(0.017)	(0.010)
FSF * Distance	-0.007*	-0.003
	(0.004)	(0.002)
Hypothetical FSF (Losers)	-0.023	
	(0.025)	
Hyp. FSF (Losers) * Distance	-0.005	
	(0.005)	
Overall Exit Rate	0.12	0.13
Public School Controls	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict
N	7,144	6,455
R-Squared	0.036	0.039

* < 10%, ** < 5%, *** < 1%. The first column regression includes measures of the reform's actual funding change for schools that received money and measures of the reform's hypothetical funding change for schools that could have lost money but did not via the "hold harmless" clause. We define FSF (or Hypothetical FSF) as the (hypothetical) funding change (divided by the per student expenditure) at the public school. An observation is a private school matched to a public school. An observation in the second column is a private elementary-public high school match or a private high-public elementary school match. Standard errors are clustered at the private school and observations are weighted by the inverse of the number of public school matches.

Table 8: Baseline Regressions plus Charter School Controls

	1(Exit)	1(Exit)	1(Exit)	1(Exit)
FSF	0.033*	0.035**	0.031*	0.031*
	(0.017)	(0.017)	(0.017)	(0.017)
FSF * Distance	-0.008**	-0.008**	-0.008**	-0.008**
	(0.004)	(0.004)	(0.004)	(0.004)
Distance to Closest Charter		0.002		
		(0.003)		
Num Charters <= 1 Mile			-0.031	
			(0.026)	
Num Charters <= 3 Miles			0.008	
			(0.009)	
Num Charters <= 5 Miles			-0.000	
			(0.006)	
Num Charters <= 20 Miles				-0.006
				(0.004)
Constant	0.372**	0.328*	0.421**	0.268*
	(0.181)	(0.174)	(0.170)	(0.153)
Public School Controls	Yes	Yes	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict	Subdistrict	Subdistrict
N	7,024	7,024	7,024	7,024
R-Squared	0.033	0.033	0.035	0.034

* < 10%, ** < 5%, *** < 1%. The first column is our baseline regression. Additional columns add charter school controls to our baseline. Distance to Closest Charter is the distance between the private school and the closest charter school. Num Charters <=X Miles is the number of charter schools within X miles of the private school. Standard errors are clustered at the private school and observations are weighted by the inverse of the number of public school matches.

Table 9: Heterogeneous Effects

	Schooling Level		Type of Religious Instruction		
	Non-High School	High School	Catholic	Other Religious	Non-Religious
	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)
FSF	0.052	0.011	0.023	0.025	0.068
	(0.046)	(0.017)	(0.018)	(0.026)	(0.045)
FSF * Distance	-0.019	-0.007*	-0.012**	-0.003	-0.006
	(0.031)	(0.004)	(0.006)	(0.006)	(0.005)
Overall Exit Rate	0.12	0.13	0.09	0.15	0.14
Public School Controls	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict	Subdistrict	Subdistrict	Subdistrict
N	5,040	2,104	3,052	3,058	1,034
R-Squared	0.050	0.060	0.079	0.056	0.160

* < 10%, ** < 5%, *** < 1%. Private high schools serve grades 9-12 while non-high schools do not serve any of those grades. Standard errors are clustered at the private school and observations are weighted by the inverse of the number of public school matches.

Table 10: Model Estimates

Demand Moments		Grade K	1	2	3	4	5	6	7	8
	mean distance (miles) from zoned school to public school attended	0.53	0.54	0.63	0.69	0.76	0.85	1.28	1.31	1.42
	fraction attending zoned school	0.75	0.75	0.72	0.71	0.69	0.66	0.49	0.54	0.53
	mean annual share for FSF winners after 2007	0.43	0.45	0.45	0.45	0.45	0.44	0.46	0.45	0.45
	mean annual share for FSF winners before 2007	0.42	0.44	0.44	0.44	0.43	0.41	0.37	0.41	0.40
Demand Parameters										
γ	disutility from distance (miles)	0.90	0.85	0.80	0.73	0.69	0.67	0.74	0.61	0.55
		(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
ρ	utility from zoned school	3.88	4.02	3.87	3.89	3.82	3.64	2.28	2.53	2.58
		(0.01)	(0.01)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)
λ	utility from FSF (100s of "students")	0.11	0.09	0.15	0.03	0.15	0.12	0.06	0.04	0.09
		(0.06)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)	(0.03)
δ	standard deviation	1.80	1.74	1.25	1.22	1.24	1.18	1.50	1.39	1.44
Supply Parameters		Cath.	Other Relig.	Non-Relig.						
p	probability cost = 0	0.24	0.61	0.77						
		(0.19)	(0.08)	(0.09)						
μ_{elem}	mean cost per elem. grade (stud.)	21.2	28.3	27.8						
		(15.4)	(24.0)	(23.6)						
μ_{mid}	mean cost per middle grade (stud.)	24.5	35.9	5.4						
		(17.5)	(24.0)	(20.0)						

Demand parameters are estimated with method of 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. The first demand moment is the mean distance (miles) from a student's zoned public school to the school she actually attended, conditional on it being a public school. The second demand moment is the fraction of students attending public school who attend their zoned school. The third demand moment is the mean annual enrollment share for public schools that received additional FSF funding. The fourth row is not an actual demand moment but is shown for comparison with the third demand moment. Supply model standard errors were estimated using 50 block bootstrap replications with block size one square mile.

Table 11: Regressions of Estimated Value-Added on FSF Change

	Estimated Value-Added (ELA)	Estimated Value-Added (Math)	Estimated Value-Added (ELA)	Estimated Value-Added (Math)	Estimated Value-Added (ELA)	Estimated Value-Added (Math)	Estimated Value-Added (ELA)	Estimated Value-Added (Math)
FSF * After 2007	0.006	0.021**	0.007	0.018**	0.010	0.037**		
	(0.007)	(0.009)	(0.007)	(0.009)	(0.012)	(0.015)		
Pos FSF * After 2007							0.012*	0.032***
							(0.007)	(0.010)
Hyp Neg FSF * After 2007							-0.024**	-0.045**
							(0.011)	(0.017)
Sample	All Students	All Students	Stayers	Stayers	Switchers	Switchers	All Students	All Students
Fixed Effects	Year, Grade, School	Year, Grade, School	Year, Grade, School	Year, Grade, School	Year, Grade, School	Year, Grade, School	Year, Grade, School	Year, Grade, School
N	30,803	30,797	30,444	30,441	24,273	24,234	30,803	30,797
R-Squared	0.247	0.255	0.233	0.228	0.174	0.219	0.248	0.255

* < 10%, ** < 5%, *** < 1%. Data span 2000-01 through 2012-13 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 budget change 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. 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. "FSF" is the funding change divided by the per student expenditure, in units of 100 student-equivalents. "Pos FSF" is the "FSF" variable conditional on it being positive. "Hyp Neg FSF" is the hypothetical negative funding change divided by the per student expenditure (units of 100 student-equivalents) 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, grade, and school fixed effects. Standard errors are clustered by school.

Table 12: Relationship between Private School Value-Added and Exit

	1(Exit)	1(Exit)	1(Exit)
Value-Added ELA + Math	-0.980**		
	(0.466)		
Value-Added ELA		-1.343	
		(0.820)	
Value-Added Math			-1.853**
			(0.865)
Constant	-1.579***	-1.514***	-1.577***
	(0.148)	(0.137)	(0.145)
Marginal Effect	-0.117**	-0.177*	-0.214**
	(0.053)	(0.106)	(0.095)
N	208	220	214

* < 10%, ** < 5%, *** < 1%. Estimates are from probit models with marginal effects, evaluated at the mean, at the bottom. The value-added measures are estimated school fixed effects from a regression of the school's average 8th grade scores in year t on its average 4th grade scores four years prior. The tests are the NY State ELA and Math tests. Exit is measured between 2007-08 and 2009-10.

Table 13: Reform's Effect on Aggregate Achievement

	Change in Present Value of Lifetime Earnings ($millions) - ELA	Change in Present Value of Lifetime Earnings ($millions) - Math
Quality Effect	$34	$86
Sorting Effect (Smaller Premium)	-$13	-$6
Sorting Effect (Larger Premium)	-$25	-$31
Total Effect (Smaller Premium)	$21	$80
Total Effect (Larger Premium)	$8	$55

Numbers correspond to the reform's effect on aggregate present value of lifetime earnings (millions of dollars) for fourth through eighth graders as calculated from the reform's effect on aggregate achievement. The calculations take the change in achievement and convert it to wage differences using estimates from Chetty et al (2014). We then sum these wage differences from ages 22-65 while discounting at a 5% annual rate and assuming 2% annual wage growth. The quality effect measures how much aggregate achievement would change if school quality changed but no students switched schools. The sorting effect measures how much aggregate achievement would change if students switched schools but school quality was fixed. The total effect combines the quality and sorting effects. The smaller premium uses our estimates for the private school premium. The larger premium uses estimates from Mayer et al (2002) for African-Americans in the NYC voucher experiment.

A 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.

A.1 Private School Survey

We form our census of schools using the NCES's Private School Survey (PSS). We remove specialty schools from our entry and exit regressions, though because they are still market participants we include them in our structural estimation. Specifically, we designate schools as specialty schools if (a) a school is located in a private home used primarily as a family residence; (b) a school does not educate any students beyond kindergarten; (c) a school specializes in special education; (d) a school specializes in vocational or technical training; (e) a school is an alternative school; or (f) a school is an early childhood program or day care center. Including these schools dampens our exit estimates slightly.

We infer school entry from the first time a school appears in the data and exit from the last time a school appears. 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 2005-06 and 2009-10 waves of the PSS but not in the 2007-08 wave. For the 2007-08 wave, 656 private schools appear in the data while an additional 115 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 treat these schools as neither entering nor exiting in 2007-08 and include these schools in our summary statistics and school entry and exit regressions. In the summary statistics, we include these schools and use their characteristics (e.g., enrollment per grade) from the most recent pre-2007 PSS wave that includes the school. If we were to remove these schools from our regression analysis, we would find stronger effects of the reform on private school exit.[72]

We also spot check our inferred school exit with internet searches. Verifying whether (and when) a school closed can be difficult as some schools lack an online presence.[73] Catholic school closures tend to receive media attention and thus verification of their status was typically easy. Based on our internet searches, we categorize schools as exiting based on our best guess. We find that up to 28% of the inferred exiters may not have exited. Because

[72]If schools are randomly missing from some waves of the PSS, then we might overestimate exit rates, particularly in later years because we have fewer future PSS waves in which the school may reappear. While this overestimated exit rate should dampen our results if it functions as classical measurement error, our placebo test where we compare the FSF reform's association with private school exit in earlier years could be subject to less measurement error. If we run our baseline regressions, and the placebo tests, measuring exit as disappearing from the data in the subsequent PSS wave, we find very similar results. Our baseline results are only minimally affected and the placebo estimates are still close to 0 and lack statistical significance.

[73]We also called many of the schools with the phone numbers found in the PSS. The phone calls to our inferred exiters led to very limited response.

this process requires numerous judgment calls, we do not adjust our data and instead rely on the inferred entry and exit based on when a school appears in the PSS. If, however, we reclassify exit according to our internet-collected data, our results are quite similar except with more precision. Results are available upon request.

A.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 few 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, but about 75% of the schools claim to. 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. In our calculation of the effects on aggregate achievement, we assume that the 36% are representative of the whole. We assess the validity of this assumption in Appendix C. 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 we drop from our regression analysis, as described in the previous subsection. Schools in Manhattan and Brooklyn are also slightly underrepresented relative to the other boroughs.

A.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.

A.4 Private School Tuition and Expenditure

Our main analysis does not use data on private school tuition and expenditure. But for some additional analysis, we collect such data. No single source contains this information, so we pull from several sources. We start with the 1999-2000, 2003-04, and 2007-08 waves of the NCES's School and Staffing Survey. Each survey includes detailed tuition and expenditure data but only samples 10-15% of schools. We also use data from IRS Form 990, which many nonprofits fill out. Religious schools, however, are exempt from filing requirements and thus the data only cover about 20% of enrollment. For independent schools, we use data from the National Association of Independent Schools, which agreed to share its data with us. Finally, for schools that were still active in the 2009-10 school year, we have combined internet research and phone calls to collect current-year tuition data.

B Appendix: Public School Expenditure

B.1 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 expenditure 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:

$$Expend^c_{kt} = \delta^c_k + \tau^c_t + \pi^c FSFChange_k * After2007_t + \eta^c_{kt} \qquad (14)$$

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 A1a and find that for each additional dollar a school received from FSF \$0.59 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 \$0.10 and \$0.21 of each additional dollar on Other Classroom Instruction and Instructional Support Services, respectively. The schools, however, are substituting away from spending on Field Support and System-Wide Costs, which fall with each additional dollar by \$0.07 and \$0.18, respectively. It thus appears that schools spent their additional funding in ways that most directly affect their school quality.

B.2 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 A1b we present the results, with the right-hand-side FSF_k variable measuring the funding change divided by the per student expenditure (in units of 100s). We find that a school that received funding equivalent to 100 students decreased its number of teachers by 3.54 after the reform. At the same time, we find these schools' teachers tend to be more experienced and that class size in elementary schools does not dramatically change while class size in grade 10 core classes goes down slightly.[74] Schools receiving money thus spend more on teachers not by increasing the number of teachers (relative to schools that did not receive money) but by hiring more experienced and expensive teachers. Indeed, using teacher-level data from the New York City Department of Education, we find that for each 100 student equivalents of funding increase, a school's average annual teacher salary increased by $261.

B.3 Teacher Movement

The teacher-level data also allow us to follow individual teachers over time and verify that teachers responded to the reform by switching schools. The data span the 2006-07 to 2012-13 school years, so our pre-reform data is limited. We are still able to uncover several patterns in the data.

First, teachers who started at a "relative loser" in our data were more likely to switch to a "winner" than teachers who started at a "winner" were to switch to a "relative loser." Of the teachers who started at "relative losers," 6.9% switched to "winners," while just 5.0% of teachers who started at "winners" switched to "relative losers." The teachers switching from "winners" to "relative losers" also averaged more years of experience in the NYC school district than teachers making the opposite switch (4.9 vs. 4.0 years). There thus seemed to be a shift in teacher experience from "relative losers" to "winners." There was also more teacher churn at "winners" than "relative losers." More teachers both entered and exited the school, often from outside of the school district.

Second, we test whether teacher movements might have affected school quality. We found that the reform increased the "winners" value-added compared to the "relative losers." Therefore, we use the teacher data to check whether teacher movements could have explained this finding and, in particular, whether the "winners' " value-added increase came at the expense of the "relative losers." We use our public school grade-year value-added estimates and examine how a school's value-added changed over time with the arrival (or departure) of teachers. Unfortunately our teacher data do not indicate which grade the teacher taught, so we average the school-grade-year value-added estimates across grades and regress year-to-year changes on measures of the number of entering and exiting teachers in all grades.

We present the results in Table A2. For both types of schools, especially the "relative losers," the addition of new teachers from outside the district was associated with value-added decreases. On the other hand, new teachers coming from other NYC public schools were associated with value-added increases. Losing teachers to outside the district was not associated with a significant change in value-added. For "relative losers," losing teachers to other NYC schools was often associated with decreases in value-added. "Winners," however, often saw their value-added increase when losing teachers to other NYC schools. These

[74] While the decrease in core subject class size seems inconsistent with a decrease in number of teachers and increase in students, we do not observe the number of teachers in core subjects.

results indicate that teacher moves do seem related to changes in value-added and could explain some of the increase in value-added at "winning" schools. We find some evidence that this increase could come at the expense of "relative losing" schools, as teachers leaving "relative losers" for "winners" were associated with a decrease in value-added. Thus, our assumption that the reform only led to public school quality gains might overestimate the actual change.

C Appendix: Achievement Calculations

C.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):

$$\begin{aligned} y^s_{i,k,g,t} = {} & \beta_1 y^{math}_{i,g-1,t-1} + \beta_2 (y^{math}_{i,g-1,t-1})^2 + \beta_3 (y^{math}_{i,g-1,t-1})^3 + \beta_4 y^{ela}_{i,g-1,t-1} \\ & + \beta_5 (y^{ela}_{i,g-1,t-1})^2 + \beta_6 (y^{ela}_{i,g-1,t-1})^3 + X_i' \beta_7 + \theta^s_{k,g,t} + \epsilon^s_{i,k,g,t}. \end{aligned} \tag{15}$$

A student's test score, $y^s_{i,k,g,t}$, is standardized so that scores across a subject-grade-year for public school students have mean 0 and standard deviation 1. We use the estimated school-grade-year fixed effects as our value-added measures.

C.2 Estimating Private School Value-Added

As mentioned in the text, 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^s_j) from the following regression for private school j:

$$\bar{y}^s_{j,8,t} = \alpha \bar{y}^s_{j,4,t-4} + \mu^s_t + \theta^s_j + \epsilon^s_{j,g,t} \tag{16}$$

where $\bar{y}^s_{j,g,t}$ 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.[75] Our estimates thus average over multiple years.

Our cohort-level value-added estimates may attribute some of a school's quality to the changing composition of students. As we lack student-level data, we cannot follow students in and out of schools. We instead infer compositional changes in the whole sector by looking at students who enter or leave the public school sector. Using these inferred sector-wide

[75] Our public school 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 2007-08 fourth grade cohorts.

changes, we estimate a private school premium and then adjust each school's value-added estimate equally so that the enrollment-weighted private school premium matches our sector-wide estimate. This adjustment is made across the whole sector and thus all heterogeneity in private school value-added across schools comes from changes in cohorts' test scores and not differential changes in student body composition across schools. In the next subsection we provide more detail on how we estimate the private school premium.

Our other data limitation is that we only have value-added estimates for 36% of the private school students. We assume that the schools in our data are drawn from the same distribution of value-added as the missing schools. While typically we would worry that schools with higher value-added select into taking the tests and reporting the results, some of the best-known schools do not take the tests. We suspect that these schools can rely on their brand to attract students rather than needing to appeal to testing data.

To assess the validity of this assumption, we compare average test scores at private schools for which we have value-added estimates versus private schools for which we do not have value-added estimates.[76] We regress a school's standardized average test score on grade-year indicator variables and an indicator for whether the school has value-added estimates. Our estimates on the value-added indicator are 0.0097 (0.0453) for ELA and -0.1569 (0.0456) for math. We cannot reject the null hypothesis that the average ELA test score is the same for both types of private schools. For math, we find evidence that the schools with value-added estimates are negatively selected on average test scores. While we obviously cannot test whether the types of private schools differ on value-added measures, the schools for which we have value-added estimates do not appear to be positively selected on average test scores.

C.3 Estimating the Private School Premium

We estimate the private school premium in several steps. First, we standardize the private school cohort average test scores by the public school students' mean and standard deviation on the same test. Second, we add up the "total standardized test scores" in the private school data for each test. For example, if a private school has 100 students and its standardized mean scores are 0.01 standard deviation (σ), then the school's "total standardized score" is 1σ. We scale up the sector's total standardized score by dividing by the fraction of NYC private school students who appear in the testing data.[77] At this point we have an estimate for the "total standardized test score" in the private sector for each test.

Third, we make adjustments for possible compositional changes in the students in the private sector. We infer such changes from students entering or leaving the public sector. We examine test scores for students who just entered the NYC public school system between grades 5 and 8 and find that these students have substantially lower scores than the average. We also examine prior-year test scores for students who left the NYC public school system

[76] Some schools appear in the testing data but not frequently enough to construct cohort value-added estimates.

[77] Here we use the assumption that the students not in our testing data are drawn from the same distribution as the schools that are in the data.

after grades 4 through 7 and find that these students have higher test scores than average.[78] We assume that some of these students entering the public schools came from private schools and that some of these students leaving the public schools went to private schools.[79] Based on the relative flows in and out of the public and private school sectors,[80] we estimate that 22% of the public school student churn comes from students switching to or from private schools and 78% of the churn comes from students moving in or out of the district.[81] We assume that students switching between sectors are drawn from the same distribution as students entering or leaving the district.

To calculate the private school premium, we compare the private sector's "total standardized test scores" for grade 4 in year t with its "total standardized test scores" for grade 8 in year $t+4$. We then adjust the grade 8 scores by 22% of the change in the public sector's "total standardized test scores" from students entering or leaving the system between grades 4 and 8 in years t through $t+4$. If the private sector has more "total standardized test scores" in grade 8 relative to that cohort's grade 4 figure (after adjusting for the compositional change) then we would calculate a positive private school premium. Finally, we divide the adjusted net change in "total standardized test scores" by the number of eighth graders in the private sector. These calculations yield an average private school premium of 0.05σ in ELA and 0.03σ in math.

We also note that most of the enrollment decrease in the private sector comes from African-American students, who may have a larger premium from attending private school. We thus calculate the sorting effect a second time, using the "larger" premium Mayer et al. (2002) estimate for African-American students. These estimates correspond to a 0.10σ premium in ELA and 0.17σ premium in math.

C.4 Estimating Counterfactual School Quality

Our main results compared changes in an outcome (public school enrollments, the supply of private schools, school quality) across areas of the city differentially affected by the reform. But to determine the reform's impact on aggregate achievement, we need to take a stand on how these outcomes would have evolved city-wide in the absence of the reform. Thus, to estimate counterfactual school quality and enrollments we add assumptions about city-wide changes.

To estimate counterfactual public school quality in the absence of the reform, we use our differences-in-differences estimates from Table 11. We assume that schools that did not receive additional funding experienced no change in value-added due to the reform. Thus,

[78] Over time, students who leave the NYC public school system go from being positively selected to being slightly negatively selected.

[79] Most of the students entering or leaving NYC public schools left the district altogether.

[80] We have private school enrollment data every other year. For the years in which we do not have data, we use the previous year's data. Our results are robust to a range of assumptions regarding private enrollments in years when we do not observe them.

[81] The flows for public school students differ between ELA and math test-takers. We count students entering or exiting the public schools using the math test data because the numbers appear more stable.

to generate counterfactual school quality we subtract the differences-in-differences coefficient multiplied by the funding change in 100 student-equivalents. As an example, if a school's value-added was 0 and it received a funding increase equivalent to 100 student equivalents, its counterfactual value-added would be -0.021 in math.

We are unable to estimate private school quality changes in response to the reform because our cohort value-added methods require at least four years to pass. We therefore assume that each school's private school quality was unaffected by the reform. If private schools improved their quality, possibly due to increased competition from the public sector, then we would underestimate the reform's effect on quality.

C.5 Estimating Counterfactual School Enrollments

Finally, we estimate counterfactual school enrollments. Within the private sector, we assume that the reform did not cause student sorting toward better or worse schools. As discussed in the text, private schools that closed had lower estimated value-added than those that remained open.[82] Entrants, on the other hand, generally have higher value-added.[83] Increased exit thus raised the private school premium while reduced entry lowered it. We find preliminary evidence that the reform may have deterred more entry than the exit it caused by estimating Equation 7 but replacing the number of entrants with the number of exiters. We estimate that a public school receiving 100 student equivalents of funding had 0.0176 (0.0608) more exiters after the reform. This estimated coefficient is smaller in magnitude than our entry estimate of -0.0524 (0.0266). If we take these estimates literally, and assume exiters and entrants had comparable enrollments per school, the reform-induced change in private school value-added would be roughly zero in math and negative in ELA. But because these regressions are merely suggestive,[84] we simply assume no quality change in the private sector.[85]

Across sectors, we need to estimate how the reform affected the private sector's enrollment share. Our main results found a decrease in private school supply in areas of the city that received large increases in public school funding. To translate these estimates to a city-wide effect, we note that our supply model attributed a 1.6% private school closure rate to the reform. We do not have an explicit model of entry, so we turn to the results discussed in the

[82]Private schools that exited after the reform had average value-added of -0.079σ in ELA and -0.089σ in math, both statistically significant with p=0.05.

[83]Schools that entered the market since the 1997-1998 school year and have not exited have average value-added of 0.061σ in ELA and 0.027σ in math, both statistically significant with p=0.01.

[84]In particular, the distribution of private schools prior to the reform is not random, which affects the weighting in this exit regression. If a public school has 1 of its 8 private school neighbors exit, we are treating this equivalent to another public school for which 1 of its 2 private school neighbors exits. Our preferred exit specification does not have such weighting issues.

[85]Even in the absence of entry or exit, students could sort to better or worse private schools. We see students on average sorting to better schools, but this observation is not specific to the years surrounding the reform. In principle we could use a model to predict students sorting among private schools. But because our current model does not account for the unobserved deterred entry, we cannot use our estimates for this purpose.

previous paragraph where we estimated that the effect on entry was roughly three times as large as the effect on exit.[86] After we account for the fact that exiters and entrants tended to be smaller schools than the schools that remain open, we estimate that the private sector's enrollment share decreased by 1.2 percentage points in response to the reform.[87]

For public schools, we turn to our differences-in-differences estimates from Table 3. We undo the reform's enrollment effects by subtracting off the differences-in-differences coefficient multiplied by the funding change in 100 student-equivalents. Because some of the "winners" enrollment increases came at the expense of the "relative losers," this procedure will underestimate the public enrollment share. We thus scale up the public enrollments proportionally so that the sector's share only decreased by 1.2 percentage points.

D Appendix: Model Simplifications

D.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 in this 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" 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. Even though some schools exceeded their nominal capacities, the capacities were not necessarily binding. The average over-capacity

[86] Again, we note that these regressions are not our preferred specifications and should be taken as suggestive.

[87] The private sector's pre-reform share was 20.1%. The sector's 2009-2010 enrollment share was 17.1%, so we attribute not quite half of the decrease in the private enrollment share to the reform. The calculation of the enrollment share depends on how we treat private schools that drop from the data for a given year but are in the data in adjacent years. If we remove these schools from the calculation, the pre-reform share was 18.3% and the 2009-2010 share was 16.6%.

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.[88] 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.[89]

D.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, however, we assume that private schools' characteristics remain constant over time. 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 achievement and school assets and income, collected from IRS Form 990. For each characteristic – average ELA test scores, average math test scores, total assets per student, and total income per student – we measure its change between 2006 and 2010 for each school, conditional on the school being open both years.[90] We then use these changes as our dependent variables in regressions following our private school regressions, Equation 5 and Equation 6.

[88]The 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 should be seen as loose descriptions of first-order effects.

[89]Among 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%.

[90]Our achievement data do not include 2006, so we use 2005 data.

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. We find some evidence that private schools' average ELA test scores may have increased and their income per student may have decreased, but the results only hold with Equation 6. When we use Equation 5, the ELA result flips sign and the income per student result loses statistical precision.

We acknowledge though that our data are too coarse to rule out any changes in private schools' characteristics.

D.3 Limited Student Heterogeneity

In our demand model the only source of student heterogeneity, other than the type I extreme value error, is the student's residential location. If, however, some students have preferences for certain types of schools then we may not properly capture substitution patterns, especially when a school closes. As an example, if certain students have a strong preference for private schools, then we might overestimate the indirect effect. Our model predicts that these students in closing schools would substitute to other schools according to the average substitution patterns. We would thus over predict the number of these students moving to public schools.

While random-coefficient logit models can be identified with aggregate enrollment data, in the presence of changing choice sets, our exactly identified model is unable to accommodate additional parameters.[91] Instead, we assess the potential heterogeneity in students' preferences for private schools by imposing a normal random preference for private schools with mean 0 and standard deviation 1. Our results do not change dramatically, primarily because the estimated disutility to distance and utility to attending the zoned school, which are pinned down in the model by public school moments, are so large.

It is possible that student heterogeneity in preferences for private schools is even larger, but we consider this unlikely to affect our main results. First, the form of heterogeneity could affect the reform's impact on the private enrollment share. However, the observed decrease in the private sector's citywide enrollment share implies there must be sizable substitution across sectors. Some of this substitution could be driven by city-wide shocks unrelated to the reform. But we find that private sector enrollments fell the most near public schools that received the largest funding increases.[92] Thus, for student heterogeneity in preferences for attending a private school to change our results significantly, it would have to predict that some students stay in the private sector but do not attend a local private school, even though there are still a range of nearby private options still open. This would require a very low disutility to distance for students with a preference for private schools. We have not

[91] Of course we can add estimation moments and plan to do so in future versions.

[92] To test this, we estimate Equation 4 except we replace the dependent variable with the private sector enrollment (of the same schooling grades) within a one-mile radius of the public school. We estimate that for each increase of 100 student equivalents in a public school's funding, the private enrollment within a one-mile radius fell by 119 students after the reform. The average private enrollment within a one-mile radius of public schools prior to the reform was 852.

modeled heterogeneity in the disutility to distance and leave this for future work.

Second, the amount of heterogeneity could determine the size of the indirect effect relative to the direct effect, conditional on the change in the private enrollment share. We do not expect the relative size of the indirect size to be particularly sensitive to the amount of heterogeneity, given how we estimate the model. By intuition, we might expect that more heterogeneity in preferences for private school would cause the students who need to find a new school because their prior school closed to prefer other private schools overwhelmingly. This might diminish the indirect effect. On the other hand, our model imposes the same λ coefficient for all students. Our estimate for λ is largely pinned down by sorting patterns within the public sector. Thus, conditional on an estimated λ, larger student heterogeneity in preferences for private school might mean fewer private school students are on the margin of switching to a public school, which decreases the number of direct private to public switchers.[93] These two forces have opposite effects on the relative size of the indirect effect. A model where a student's private school preference interacts with λ could lead to different results.

While measuring the level of unobserved heterogeneity is impossible, we offer several reasons it may not be very large among the students most affected by the reform. The decline in private school enrollment was concentrated among African-American students, who based on observables (which we do not include in the model) look similar to many public school students and may not have an extremely large private school preference. Furthermore, when private schools closed prior to the reform, we see enrollment increases at nearby public schools, which implies that some students leaving private schools opt for public schools.

D.4 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.[94] 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.

[93] This statement depends on how many students at private school j have a public school as their next preferred option. Depending on the schooling equilibrium, increased heterogeneity in preferences for private schools could leave fewer private school students who have a public school as their next preferred option.

[94] We 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.

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.

A Appendix Figures

Figure A.1: 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

B Appendix Tables

Table A1: Expenditure and School Characteristics Regressions

Table A1a: Regressions of Expenditure Categories on FSF Change

	Teachers	Other Classroom Instruction	Instructional Support Services	Administrators	Other Direct Services	Field Support	System-Wide Costs
FSF Change * After 2007	0.588**	0.095	0.213***	0.145**	0.264***	-0.070***	-0.183**
	(0.249)	(0.065)	(0.063)	(0.065)	(0.073)	(0.018)	(0.076)
Category's Fraction of Expenditure in 2006-07	0.357	0.104	0.120	0.083	0.165	0.019	0.171
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	10,588	10,588	10,588	10,588	10,588	10,588	10,588
R-Squared	0.953	0.869	0.930	0.942	0.916	0.868	0.932

* < 10%, ** < 5%, *** < 1%. Data span 2004-05 through 2011-12 school years. Each column is a separate regression of an expenditure category on the budget change due to the FSF reform. Each regression includes year and school fixed effects. Standard errors are clustered by school. "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 counsling, 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.

Table A1b: Regressions of School Characteristics on FSF Change

	Number of Teachers	Number of Non-Teachers	% Teachers < 3 Years Experience	% Teachers with MA Degree	% Teacher Turnover	Average Class Size Grades 1-6	Average Class Size Grade 10
FSF * After 2007	-3.542**	-1.029***	-1.748***	0.481	0.052	-0.199	-0.517**
	(1.621)	(0.326)	(0.603)	(0.423)	(0.544)	(0.406)	(0.238)
Dep Var Mean in 2006-07	106.0	17.0	11.7	28.7	14.6	21.6	21.2
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	7,869	7,869	7,869	7,869	7,835	4,912	1,857
R-Squared	0.949	0.843	0.693	0.878	0.535	0.675	0.657

* < 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 funding change divided by the per student expenditure, in units of 100 student-equivalents. Each regression includes year and school fixed effects. Standard errors are clustered by school. Average Class Size for Grade 10 is calculated over only core classes.

Table A2: Regressions of Changes in School Value-Added on Measures of Teacher Moves

	Change in Average Estimated Value-Added (ELA)	Change in Average Estimated Value-Added (Math)	Change in Average Estimated Value-Added (ELA)	Change in Average Estimated Value-Added (Math)
Num. Entrants (from out of District)	-0.004**	-0.004***	-0.001	-0.005***
	(0.002)	(0.002)	(0.001)	(0.001)
Num. Entrants (from Winners)	0.009**	0.006	0.005*	0.005*
	(0.004)	(0.004)	(0.003)	(0.003)
Num. Entrants (from Losers)	0.010***	0.008**	0.007*	0.006*
	(0.004)	(0.004)	(0.004)	(0.003)
Num. Exiters (to out of District)	0.002	0.001	-0.001	-0.002
	(0.002)	(0.002)	(0.001)	(0.001)
Num. Exiters (to Winners)	0.002	-0.004	-0.001	0.001
	(0.004)	(0.004)	(0.003)	(0.003)
Num. Exiters (to Losers)	-0.008*	-0.005	0.002	0.003
	(0.004)	(0.004)	(0.004)	(0.004)
Constant	-0.022***	-0.005	-0.015**	0.008
	(0.007)	(0.007)	(0.007)	(0.007)
Sample	"Losers"	"Losers"	"Winners"	"Winners"
N	3,019	3,019	3,000	2,999
R-Squared	0.005	0.005	0.003	0.011

* < 10%, ** < 5%, *** < 1%. Data span 2006-07 through 2012-13 school years. Each column is a separate regression with the dependent variable the year-to-year change in a school's estimated value-added (units of standard deviations on a test) in a subject, averaged across grades 4 through 8. 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. 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. The right-hand-side variables measure the number of teachers entering or exiting the school during year t. Entering and exiting teachers are split out depending on whether they transition between the school and outside the district, a "winner," or a "loser." The first two columns include "losing" schools and the next two columns include "winning" schools.