Quantifying the Supply Response of Private Schools to Public Policies

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Abstract

School policies that cause a large demand shift between public and private schooling may cause some private schools to enter or exit the market. We study how the policy effects differ under a fixed versus changing market structure in the context of a public school funding reform in New York City. We find evidence of increased private school exit and reduced entry in response to the reform. Using a model of demand for and supply of private schooling, we estimate that 32% of the reform's effect on school enrollments came from increased private school exit and reduced private school entry.

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1 Introduction

The set of schooling options in the United States has grown substantially over the 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 up to 9% and 12%, respectively.[1] Just as entry and exit can be a primary force behind aggregate outcomes in other industries, the churn of private schools may determine the quality of private education offered and cause demand shifts between the public and private sectors. Yet, perhaps due to data limitations, the education literature has paid little attention to the elastic supply of U.S. private schools and its importance for school choice and 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 entry and exit and its implications for the education market in the context of a large public school policy — the Fair Student Funding (FSF) reform in New York City (NYC). This reform provided some public schools with additional funding. We ask whether the supply of private schools was responsive to the public school reform, and if so, how the private sector response affected students' enrollments and aggregate achievement.

We find that the reform affected students' enrollment decisions, partially through a change in the supply of private schools. For each $1,000 increase in per student funding, a public elementary or middle school's enrollment increased by 36 students. The supply of private schooling was indeed responsive to the public school reform. If a public school received a $1,000 funding increase per student, we find that a local private school was 4.8 percentage points more likely to close in the six years following the reform. This constitutes 30% of the baseline closure rate. We develop and estimate a model that attributes 32% of the total

[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 enrollment effect to increased school exit and reduced school entry. 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 may have led them to lower-quality schools. This sorting potentially undid some 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 student characteristics. Overall, about half of the city's K–12 public schools received funding increases, averaging \$450 per student, while the other half saw no change.

This reform offers an attractive setting for analyzing the interaction between public and private schools. The formula change led to considerable variation in how much new funding public schools received. This variation allows us to look for differential effects on students and schools in different neighborhoods. NYC has an active and large private school sector; at the time of the reform, 20% of NYC students were enrolled in 946 private schools.

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. We estimate that relative to schools that did not receive increased funding, enrollments increased by 36 students (16%) for every \$1,000 increase in funding per student.

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 a \$1,000 per student funding increase was 4.8 percentage points (or 30%) more likely to close in the next six years. We also examine 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. At the end of Section 5 we address the concern that the distribution of public school funding increases may have been correlated with other time-varying factors that could have explained the private school supply change even in the absence of the reform.

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

The 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 total enrollment increase at public elementary and middle 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, a private school preference, 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 32% of the total enrollment change.

To assess the welfare impact of the supply response, we introduce a model of private school supply where incumbent schools decide whether to remain open or close based on their enrollments. We estimate that the average Catholic private school at risk of closing requires an enrollment of 21 students per elementary grade and 30 students per middle grade to stay open. We estimate that the FSF policy induced an exit rate of 1.7%, which lowered the policy's impact on student welfare by 14%.

We also use our model to estimate the value families place on a dollar of public school funding. Following Dynarski, Gruber and Li (2009), we use Catholic school sibling discounts as an instrument for private school tuition and estimate that families value $1 of public school funding equivalently to $0.72 of private school tuition.

We end by assessing 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 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.[2]

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.[3] Students who switched public schools tended to move to similar quality schools. Students who switched between private schools tended to move to slightly higher-quality private schools.[4] Private school students who switched to public schools may have experienced a large decrease in the value-added of the schools they attended, which potentially offset some of the achievement increase associated with the public school improvement. It highlights how a minority of the students switching schools can determine a large fraction of the aggregate achievement impact.

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.[5] The second strand evaluates school funding reforms and whether spending affects student outcomes.[6] 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,[7] only a few papers have examined empirically how entry or exit can affect a policy's outcomes. Hsieh and Urquiola (2006) find that Chile's universal

[2]The calculations require several assumptions and thus our analysis demonstrates suggestive, rather than definitive, achievement effects.

[3]Using a differences-in-differences framework, we estimate that a school with $1,000 in additional funding per student had an increase in mathematics value-added of 0.023 test standard deviations. The increase in ELA was a statistically insignificant 0.004 standard deviations.

[4]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.

[5]Influential empirical work includes Hoxby (1994), McMillan (2005), Card, Dooley and Payne (2010), and Neilson (2013). Work on whether private schools cause stratification includes Hoxby (2003), and Epple, Figlio and Romano (2004).

[6]Work on school funding reforms and effects on private schools includes Downes and Greenstein (1996), Downes and Schoeman (1998), and Hoxby (2001). For whether school resources matter for student outcomes, see Card and Krueger (1996), Hanushek (1996), Hoxby (2001), Cellini, Ferreira and Rothstein (2010), and Jackson, Johnson and Persico (2014).

[7]Work on entry includes Downes and Greenstein (1996), Barrow (2006), and Ferreyra (2007) while work on exit includes Pandey, Sjoquist and Walker (2009). Other work has looked at similar issues for two-year colleges (e.g., Cellini 2009).

voucher program led to considerable private 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. Andrabi, Das and Khwaja (2014) introduce school report cards to a random set of villages in Pakistan and find the decline in enrollment among low-performing private schools was driven by school exit. Our paper provides evidence on the importance of U.S. private school supply responses 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.[8] Each student $i \in \mathcal{I}$ chooses the school that gives her the highest utility.

A public school k's demand is thus:

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

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. Let F_j be the minimum number of tuition-paying students the school needs to stay open. School j

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

closes if $D_j(X^{FSF}, C_j) < F_j$. If j closes, then the remaining schools face less 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 $D_k(X^{FSF}, C_k(X^{FSF}))$.

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.[9]

Even if the indirect effect does not explain the majority of the total effect, it can have a disproportionately large impact on achievement. Families face a tradeoff in the amount they pay for education and the education's quality.[10] Students switching between two public

[9]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. Among schools that remain open, the direction of this adjustment is theoretically ambiguous as these schools face increased competition from public schools due to the reform but possibly reduced private competition if neighboring private schools closed. In Appendix E we do not reject the null hypothesis that surviving schools' test scores and revenues do not change in response to the reform. For the schools that close, we can infer that there was no tuition rate that would have attracted enough students to keep the school open. Thus, in our structural counterfactual, we will predict students' choices had there been no supply response and schools did not adjust their characteristics. We note, however, that the fact that there was no tuition rate that would have kept the school open could be used to bound from above the welfare loss from the school closure. We believe that any changes along the margin of whether a school is open are likely to have larger effects on student choices and outcomes than intensive margin changes of school characteristics. Therefore, we think focusing on the extensive margin is potentially of first-order importance. Additionally, in this project the data on other school characteristics are sparse, but we hope to explore supply decisions along other margins in future work.

[10]This might imply that private schools, which usually charge tuition, offer higher-quality academic

schools, neither of which charges tuition, or between two private schools, which may charge relatively similar tuition, 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.[11,12] In a similar manner, by amplifying the enrollment shift across sectors, the indirect effect drives changes in public expenditure on education.[13]

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

Separating the direct and indirect enrollment changes is also essential in evaluating student preferences for public school funding. School funding is an important policy lever, and funding inequalities across school districts have often led to court challenges. Despite the controversy, it is unclear 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 value families place on public funding, we 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

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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 small positive premium. Many papers have found a positive premium for test scores or graduation rates, especially for minority students attending urban 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)

[11] 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.

[12] 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.

[13] As Besley and Coate (1991) point out, this switching can have important redistributive consequences.

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 characterize the preference for public funding, 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.[14] 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.[15] 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 schools that received significant funding increases with exit rates for private schools located far from public schools that received money. Our estimates show that $\frac{\partial C_k}{\partial X_k^{FSF}} < 0$.

[14]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.

[15]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.

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 enrollment changes to measure changes in demand. In Appendix E we discuss how binding this assumption is and how it might affect our results.

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.[16] To determine how the additional money would be spent, NYC passed the Fair Student Funding (FSF) reform to fix funding inequities across public schools within NYC. Before the reform, schools that looked very similar in terms of their students' demographics often received very different amounts of funding per student. The FSF reform changed the funding formula so that most of the school's 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.[17]

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

[16]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.

[17]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 reform, the district would tell each school, based on enrollments and its students' demographics, how many teachers it could employ. This did not depend on the experience or salaries of the teachers, and the district would compensate a school for the salary differential from hiring more expensive teachers. Each school would then recruit and hire its own teachers. Thus, schools that hired more expensive (experienced) teachers received more money, and because the more experienced teachers tend to prefer schools in wealthier areas, the schools in poorer neighborhoods wound up with smaller budgets. The FSF reform changed this accounting so that a school's budget would depend only on student characteristics and not increase if the school hired more expensive teachers.

The FSF reform affected school budgets starting in the 2007-08 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.[18] 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."[19]

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, holding fixed a school's enrollment and demographics. 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) on the x-axis and its actual funding change on the y-axis. In Figure A.1, we show how two representative schools' budgets were affected.

The funding change interacted with a public school system that gives students increasing amounts of choice as they enter higher grades.[20] Our empirical strategy will test how private

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

[19]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.

[20]Public elementary students typically (65%) attend their local ("zoned") school while a minority of students opt for other schools. 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, and the selective public high schools rank the applicants. NYC runs a centralized matching system that assigns students to schools.

schools are affected by the geographically closest public schools. The extent to which students attend schools very close to their homes will determine how concentrated the enrollment effect is and how likely we are to pick it up in our analysis.

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 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 document how the schools spend their budgets each school year. We supplement these data with school characteristics from NY State Report Cards and the Common Core of Data. These data include enrollments, grade average test scores, measures of 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 2 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.[21] The average "winning" school expected to receive a funding increase of $454/student, or about 6% of its operating budget. There is a large right tail as 5% of "winning" schools saw increases of over $1,000/student.

While the NYC DOE claimed that much of the funding increase went to schools because of past policies that have no relevance to today, the "winning" and "losing" schools still look different along some school characteristics. We investigate these differences in Appendix B and find that schools with inexperienced teachers and more students who are limited English proficient and Hispanic were more likely to see funding increases. Despite these differences,

[21] Here we exclude specialized public schools and charter schools, which were not directly affected by the reform. In our estimated model of school choice, we include these as schooling options.

neighborhoods at similar income levels often received very different funding changes (Figure A.3, as described in Appendix B).

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 (PSS). This data set is published every other year and includes school characteristics such as enrollment, religious affiliation, number of teachers, and location. We infer private school entry and exit based on the first and last times the school appears in the Private School Survey. We use the data sets from the 1999-2000 through 2011-12 school years.

The PSS has some measurement error, which likely overstates entry and exit. We thus supplement the data with private school enrollment data from the New York State Education Department (NYSED). This data set does not capture all schools in the PSS and includes fewer school characteristics, but it allows us to infer entry and exit with considerably more precision. For the reduced form analysis of entry and exit, our estimation sample will consist of the schools in the NYSED data. But for the model of school choice, which relies on specifying the full set of schooling options, we include the rest of the PSS schools.

To measure private schools' quality of education, we use test score data on nonpublic schools from the NYSED. The test data are school-grade-year average test scores on the grade 4-8 math and ELA state tests. Only a 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, which we assess in Appendix D. We provide more details on data sources in Appendix A.

Private schooling plays a large role in New York City's educational landscape, as 20.0% of K-12 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 42% of the schools in our estimation sample offering Catholic instruction and 41% 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 garner widespread interest are not representative of private schooling in NYC. Table 1 provides summary statistics of the NYC private schools open during the 2005-06 school year.

Many private schools also face a high probability of having to close. In Figure 3 we plot the number of NYC entrants and exiters in the PSS and NYSED data every two years. We define entry as the first time a school appears in the data and exit as the last time a school appears. In most years, there are between 75 and 125 PSS entrants and exiters and between 20 and 50 NYSED entrants and exiters.[22] This amount of churn is quite large compared to the 700-900 schools that are active at a given time. The frequency of closure, even before the reform, potentially provides us with the 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

We start by estimating the reform's total effect on public school enrollments by comparing how enrollments changed at public schools that received money under the reform (relative "winners") with public schools that did not (relative "losers"). This differential change in enrollments across public schools combines students switching between two public schools, students 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.

We quantify this enrollment effect by running a differences-in-differences regression where we compare enrollments across public schools before and after the reform depending on their change in funding from the reform. For public school k in year t:

$$f(enrollment_{kt}) = \delta_k + \tau_t + \pi FSF_k After2007_t + \eta_{kt}. \qquad (3)$$

Our coefficient of interest is π, which measures how the policy's impact varied with the level of the funding change, as measured in \$1,000s of per student funding. We focus on elementary and middle schools because there are a number of other concurrent policies affecting high schools, such as the breaking up of large schools. Table 2 reports the results.

[22] The actual numbers are likely between the PSS and NYSED numbers as the PSS overstates churn due to measurement error and the NYSED data understates churn because it misses some of the smaller schools.

We find that a funding increase of $1,000 per student predicts an estimated relative enrollment increase of 15.6% (or 36.5 students).[23] We discuss the last column of Table 2 in Subsection 5.4.[24]

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 teachers and increased the average teacher experience and quality. 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. The FSF dollars were also disproportionately spent on instructional support services, which includes after-school activities. The spending on teachers combined hiring more teachers and employing more expensive (experienced) teachers.[25,26]

These uses of the funding translated into higher school math value-added. We discuss the reform's effect on achievement in Section 8. Changes in peer quality could be another mechanism causing students to switch schools or test scores to improve. Because we see schools' funding change at the same time as students switch schools, we are limited in our ability to identify peer effects, though we find some evidence consistent with changes to peer composition.[27] Peer effects may therefore have amplified the effects of other input changes.

[23]We find the effects are concentrated among students receiving free or reduced price lunch and African-American students (Table A2). Because we do not have panel data on private schools' enrollments by free or reduced price lunch status, we do not directly incorporate heterogeneous preferences by income type into our demand model later in the paper. However, attempts to classify students according to weaker proxies lead us to similar conclusions. This is mostly driven by the fact that the largest funding increases and the highest private school exit rates are in neighborhoods that are predominantly low-income and minority. Thus, these types of students are very influential in generating our estimates, even with our current demand specification. Furthermore, we will have an i.i.d. idiosyncratic preference for private schooling which soaks up some of the preference variation driven by income differences.

[24]We also take the main specification - enrollment regressed on the fixed effects and the funding change per student - and plot estimated π coefficients by school-year in Figure A.5, where we normalize each school's enrollment by its 2005-06 level. We see no evidence of pre-trends related to the reform's funding change, and the enrollment effect shows up quickly after the reform's implementation.

[25]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.

[26]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.

[27]Students moving to public schools that see funding increases are positively selected on baseline test

When we assess the reform's impact on aggregate achievement, we will likely overstate the benefits if peer effects are big drivers of the value-added improvements.

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 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 school could no longer operate without running losses.

To test whether private schools indeed closed in response to the FSF reform, we want to compare private school closure rates across private schools that are and are not close substitutes to public schools that received more money. Ideally we would observe students' first and second choices and measure the degree of substitutability between schools as the frequency with which they appear among a student's top two choices. Because we lack such detailed individual-level data, we measure a private school's level of substitutability with the public school as the distance between the schools. 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.[28]

Thus, for each private school j that was active in the 2005-06 school year, we define its competitor set as the 10 closest public schools k that serve the same grades,[29] provided the schools are fewer than 15 miles apart.[30] We measure the intensity of the treatment on a public school, FSF_k, as its funding change per student (in units of \$1,000s). The mean

scores relative to students moving to those schools before the reform. Students leaving these schools are also negatively selected relative to students leaving before the reform.

[28] We run additional specifications where we measure substitutability based on (1) which public schools local students attend, (2) driving time, and (3) estimates from our school choice model presented below. We also try additional functional forms of how competition declines with distance. Results are very similar and available upon request.

[29] We match on indicator variables for whether the public or private school serves elementary and high school students, respectively.

[30] 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 2.1 miles, respectively. We use great-circle distance in miles. 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, available upon request, are very similar when using driving times or varying the distance cutoff or number of matches.

value for “winning” schools is 0.45 ($450 per student). We run two specifications to measure the effect of the increased competition on the probability a private school closes. Our first 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:

$$Pr(Exit_j) = \Phi(\alpha_1 \sum_{k=1}^{5} FSF_k + \alpha_2 \sum_{k=6}^{10} FSF_k + \epsilon_j) \tag{4}$$

where j indexes the private school and k indexes the public school match from closest (1) to furthest (10). $Exit_j$ is an indicator variable for whether private school j closed between the 2007-08 and 2012-13 school years. Φ is the normal CDF, which generates a probit model, though all results are similar with a linear probability model. We also run specifications that include public school controls (X_k) and NYC public school subdistrict fixed effects (θ_d). As these controls are defined at the public school match, we sum them across all of j's matches.

Our identification assumption is that other factors that caused a private school to close from 2007-08 to 2012-13 were orthogonal to the funding increase at nearby public schools, conditional on the observed public school characteristics ($E(\epsilon_j|FSF, X, d) = 0$). 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. We discuss potential threats to our identification assumption at the end of this section.

We expect that the larger the funding increase, the more likely the 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 $\alpha_1 > \alpha_2$. As seen in Table 3, our estimates of α are positive. We also estimate that $\alpha_1 > \alpha_2$. If the closest five public schools get a total of $1,000 additional funding per student, the private school is 4.0 percentage points more likely to close when we evaluate the regressors at their means. For the further out public schools, the effect is on the order of 2 percentage points. The results are similar when we include measures of the public school's demographics.[31]

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

[31] All results are qualitatively similar if we express FSF_k as the total funding change at the school without dividing by the number of students.

reform to fade out linearly with distance:

$$Pr(Exit_j) = \Phi(\beta_1 \sum_{k=1}^{10} FSF_k + \beta_2 \sum_{k=1}^{10} Distance_{jk} FSF_k + \nu_j) \tag{5}$$

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 receives an increase of $1,000/student, the private school is 4.8 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 1.4 percentage points. These are large effects as the overall closure rate is 16%, which indicates that the indirect effect on student sorting from private school closures is likely to be important.

Because closure rates at small private schools may be particularly sensitive to changes in competition, in the final two columns of Table 3 we show results where we estimate separate effects based on whether the private school's pre-reform enrollment was less than 250 (the median in sample). We find that the effects on closure are specific mostly to small private schools. In Appendix C we investigate further how exit rates differ by school type and find the effect concentrated in elementary/middle schools and Catholic schools.

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:

$$Entry_k = \zeta_0 + \zeta_1 FSF_k + \xi_k \tag{6}$$

where $Entry_k$ is an indicator for whether public school k had a private school entrant within 1 mile between 2007-08 and 2012-13. We run a similar regression using the number of entrants within 1 mile. We present results in Table 4. We find that for each funding increase of \$1,000/student, a public school was 10.7pp less likely to have an private school entrant within 1 mile. The overall entry rate was 8.5%. 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. 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 incorrectly attribute the private school closures to the reform. 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. We find that reform variation does not predict pre-reform private school closures (Table A6).

The other main threat to identification would be if events unrelated to the FSF reform but occurring at the same time might have caused the school closures. The most obvious candidate would be the financial crisis. As wealth or job stability fell, families might have removed their children from private schools even without the FSF reform. If the recession differentially affected families living near the public schools that benefited from the FSF reform, then our regression results could be a product of factors unrelated to the FSF reform.

We run two additional placebo tests to assess whether the recession, or other events concurrent with the reform's timing, threatens our results. We first run 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 school 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 5). When the "hold harmless" clause determined that the actual funding change would be 0, we find no relationship. We run similar tests for our other results, as displayed at the end of Table 2, Table 4, and Table 8.

As a second test, we match private schools active in 2005-06 to the public schools and their FSF reform treatments, but we match private elementary schools to nearby 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 5 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. A private school's exit probability only reacted to funding changes at public schools of the same level. This indicates that differential neighborhood changes, such as vulnerability to the recession, are unlikely to be driving our results.

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

5.5 Discussion

Based on regression results, the FSF reform led to an enrollment increase at schools that received additional funding relative to schools that did not, and private schools located next to public schools that received funding were more likely to close. But these results do not allow us to quantify the impact of private entry and exit on (1) public school enrollments or (2) student welfare.

The total effect on enrollment combines the direct effect where students switch to the public school even if no school opens or closes and the indirect effect from private schools opening and closing. To separate these effects, we need to determine the counterfactual demand for a closing school had it stayed open. Ideally we would find two private schools affected similarly by the reform and otherwise identical except that only one school closed. The education market, however, is complicated as schools' enrollments depend on a set of differentiated competitors. The exercise thus proves nearly impossible as it requires each school's competitors to be identical. To account for the complexity of how schools' enrollments vary with their set of competitors, we therefore turn to a model of school demand.[32]

Second, to this point we have detailed variation in outcomes within NYC. But to assess the city-wide impact of the school funding and the associated crowd out of private schools on student welfare, we need a model of school demand and supply.

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 its effect on welfare.

In the model, students choose a school based on the school's total quality (net of price), the distance from the student's residence to the school, whether the school is the student's zoned public school, the funding change from the reform, and an idiosyncratic preference for private education. Schools compete against each other by trying to attract students and close if demand is below a threshold necessary to cover fixed operating costs.

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

$$u_{ijgt} = \delta_{jg} - \gamma_g d_{ij} + \sigma_g \nu_{igt} + \epsilon_{ijgt} \tag{7}$$

where δ_{jg} is the school-grade's total quality, d_{ij} is the distance from i's residence to j, and $\nu_{igt} \sim N(0,1)$ is an idiosyncratic preference for private schools. Student i's utility from

[32] 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.

attending public school k for grade g in year t is:

$$u_{ikgt} = \delta_{kg} - \gamma_g d_{ik} + \rho_g ZONED_{ikt} + \lambda_g FSF_{kt} + \epsilon_{ikgt} \tag{8}$$

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 per student the school received under the FSF reform (units of \$1,000s). The $ZONED_{ikt}$ variable accounts for the NYC public school choice system where many younger students are initially assigned to a default (zoned) school. The FSF_{kt} variable allows a school's total quality to change when it receives additional funding. ϵ is an i.i.d. Type I Extreme Value error. This gives rise to a logit demand system where schools' expected enrollment shares will depend on the model parameters as well as the schools that are open in that school year.

Over different school years, two elements in the model change: (1) some schools receive funding from the reform which may affect students' utilities from certain schools and (2) the set of private schooling options changes as schools open and close. 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, other than the FSF funding. 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.

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:

$$D_{jt}(stay; X, \beta) > F_{jt}. \tag{9}$$

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,[33] 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 to stay open:

$$F_{jt} = \begin{cases} 0 & w.p.\ \ p^{relig} \\ F_{jt}^{exp} & w.p.\ \ 1 - p^{relig} \end{cases} \tag{10}$$

We parameterize F_{jt}^{exp} as an exponential random variable with its mean depending on the

[33] 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.

number of elementary and middle school grades the school serves:

$$F_{jt}^{exp} \sim exponential(\mu_{elem}^{relig} NumGradeElem_{jt} + \mu_{mid}^{relig} NumGradeMid_{jt}). \tag{11}$$

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.[34] We choose this sequence because schools with the highest demand have the most number of families who need to know whether the school will remain open. These schools therefore face the most aggregate pressure to make an early decision.[35]

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 E.[36]

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. We place each student at the geographic centroid of the Census block where she lives. We then construct distances from the student's implied residence to each school in her borough that educates students from her grade. We designate the student's zoned school as the closest public school that has zoned students. We combine this data with our enrollment data for

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

[35] Because our counterfactual depends mostly on estimated local closure rates, and not as much on which specific private school closes, the choice of the sequential order does not appear to drive our results.

[36] We also abstract away from a school's attractiveness changing depending on which students choose to attend the school, as could be the case with peer effects or preferences to attend a school with students of similar demographics (e.g., Hastings, Kane and Staiger 2010, Epple, Jha and Sieg 2013). Instead, we keep the school's attractiveness, net of the funding change, fixed.

public and private schools and our measures of FSF funding.[37]

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 reform's implementation. We estimate our supply model using school closure decisions between after 2005-06. These decisions are most closely related to the FSF reform.

To estimate the demand parameters, we use an exactly-identified simulated method of moments procedure. The first set of moments comes 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. As an additional moment, we use the covariance of the private enrollment share and the private share of schools across borough-years. The second set of moments are constructed from the NYC student-level data. We calculate two additional grade-specific moments: (1) the average distance from a student's zoned school to her actual school among students opting for a public school; and (2) the percentage of public school students who attend their zoned school.

We can identify the parameters on time-invariant characteristics using the student sorting patterns prior to the reform.[38] 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_{jg}. 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. For the size of the idiosyncratic preference for private schools, the covariance of private enrollment and school share is informative. If a borough's

[37] In addition to private schools and traditional public schools, we include specialized public schools and charter schools as options for children. These schools are neither zoned schools nor have their funding change from the FSF reform.

[38] 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.

private enrollment share is relatively constant over time even as the percentage of its schools that are private falls, we infer that some students have strong private school preferences such that they are likely to attend a private school even if there are fewer schools than usual.

We then exploit how enrollments responded to the reform to identify λ_g. Once the reform occurred, we observe how many students switched from one public school to another public school that received a larger funding increase. These public school switchers did not have either of their most preferred options eliminated, so their sorting pins down the effect of the FSF funding on preferences, λ_g.[39] 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 estimate the supply model parameters (μ^{relig}_{elem}, μ^{relig}_{mid}, and 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 after 2005-06.[40] 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.[41]

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 $\hat{\mu}^{relig}_{elem} < \hat{\mu}^{relig}_{mid}$.

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

[40] If a school appears in the NYSED data, we determine exit based on that data. If not, we infer exit from the school's last appearance in the PSS.

[41] We solve the model sequentially via backward induction, starting with the schools with lowest predicted enrollment in the case where no schools exit. For a given fixed cost draw, either always exiting or always staying is a strictly dominated strategy for some schools, which allows us to iterate on the elimination of strictly dominated strategies and simplifies the estimation.

7 Results and Counterfactuals

We estimate the demand model separately for each grade from kindergarten to eighth grade. We find large effects on utility of distance and whether the public school is the zoned school (Table 6). For kindergarteners, we estimate γ at 0.83, ρ at 4.04, λ at 0.16, and σ at 0.28. The distance and zoned school coefficients decline in magnitude as students become older, which is consistent with older students traveling farther to school. These two sets of coefficients are large relative to the estimates of school total quality. For kindergarteners, an increase of one mile in distance is the equivalent of about half a standard deviation in the estimated school-grade fixed effects. Similarly, changing a kindergarten student's zoned school equals about 2.5 standard deviations.

The coefficient on FSF funding, λ, is positive for all grades, indicating that students shift their enrollments toward FSF "winners" after the reform.[42] The coefficient on the FSF funding increase indicates that an increase in funding of $1,000 per kindergarten student is equivalent to about 10% of a standard deviation in the estimated school-grade fixed effects. 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.[43]

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 post-reform had no private schools opened or closed. We then compare this model-predicted counterfactual demand to the model-predicted actual demand, where the funding reform is implemented and private schools opened and closed.[44] We estimate that 68% of the reform's enrollment increase at "winners" came from students valuing FSF "winners" higher after the reform. In other words, we estimate that the direct effect makes up 68% of the total effect (or the indirect effect makes up 32%). The school closures, and reduced entry, appear to amplify the direct sorting significantly. The magnitude of the indirect effect highlights how

[42] The estimated coefficient is 0 for sixth graders. During this time many public schools were changing from K-6 to K-5, thus leading to considerable reorganization of sixth grade, which we believe swamps our ability to identify γ.

[43] When we regress a school-grade's actual enrollment in year t on our model's predicted enrollment for year t and a set of school-grade fixed effects, we estimate a coefficient of 0.836 (0.008). This predictive power is notable because our estimation moments are not designed to capture these year-to-year fluctuations. Our model's reliance on market structure changes to predict enrollment changes thus appears reasonable.

[44] A few public schools also closed during this period. In our model predictions of counterfactual and actual demand, we keep these public schools in students' choice sets.

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 1% of Catholic schools, 60% of non-Catholic religious schools, and 55% of non-religious schools will remain open regardless of demand (Table 6). 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 30 students per middle grade to stay open. For non-Catholic religious schools, we get a slightly higher middle estimate (47), which may reflect that many Catholic parishes considerably subsidize their schools.[45]

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 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, we lack sufficient variation in grade structure across schools to identify the elementary and middle grade parameters separately, so we estimate a common parameter of 9 students per grade.

Using our supply estimates, we predict that on average 1.7% of the private schools active in 2005-06 exited because of the reform. We furthermore estimate that these exits lowered the policy's impact on student welfare by 14%.

7.1 Estimating the Value to Public School Funding

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

[45] 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.

into a tuition and a non-tuition component:

$$\hat{\delta_{jg}} = \alpha_{0g} + \alpha_1 AvgTuition_{jg} + \omega_{jg}. \tag{12}$$

The challenge in estimating α_1 is that tuition is likely endogenous. We therefore turn to the observation by Dynarski et al. (2009) that many Catholic private schools offer sibling discounts to families that send multiple children to the school. Dynarski et al. (2009) argue that, unlike the single-child price, these discounts are set randomly and can thus be used as an instrument for tuition. We thus collected middle school tuition and discount data for the Catholic private schools that are still open. The data collection is described in Appendix A.

We note that $AvgTuition_{jg} = OneChildPrice_{jg} - AvgDiscount_{jg}$, where $AvgDiscount_{jg}$ will be our instrument. To construct $AvgDiscount_{jg}$, we match each K-8 school to the closest Census block group and use 2010 data on family size to estimate the fraction of students who are in school with 0 siblings, 1 sibling, 2 siblings, or 3 siblings. We then use the price schedule for families of different sizes, interacted with the estimated distribution of local family size, to construct the average discount:

$$\begin{aligned} AvgDiscount_{jg} = OneChildPrice_{jg} - TwoChildPrice_{jg} FracF\hat{am}Two_{jg} \\ -ThreeChildPrice_{jg} FracF\hat{am}Three_{jg} - FourChildPrice_{jg} FracF\hat{am}Four_{jg} \end{aligned} \tag{13}$$

where $NChildPrice_{jg}$ is the average tuition rate for a family with N children at the school and $FracF\hat{am}N_{jg}$ is the estimated fraction of students attending school with $N-1$ siblings.[46]

Our identifying assumption is that the average discount at a school, which depends on the price schedule and local demographics, is orthogonal to other determinants of the attractiveness of the school. This is more restrictive than the Dynarski et al. (2009) condition that the discount is orthogonal to determinants of a school's attractiveness that differentially appeal to larger families. We acknowledge that this assumption is strong but are comforted by the first stage results, shown in Table 7. Our first stage regresses $AvgTuition_{jg} = OneChildPrice_{jg} - AvgDiscount_{jg}$ on $AvgDiscount_{jg}$ and grade fixed effects and yields a precisely-estimated coefficient of -1. This indicates that the average discount is uncorrelated with the one child price. To the extent we worry about school unobservables that correlate with the one child price, these do not seem to correlate with the discount.

Using our IV estimate of the value of a dollar of tuition in utils, we can compare it to our λ_g estimates for our demand model. Because the coefficient on private school tuition is

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

slightly large in magnitude than the average estimated λ_g, we estimate that families value \$1 of additional public school spending equivalently to \$0.72 of private school tuition. We can also consider what size lump sum transfer, which is not conditional on attending a certain type of school, would generate the same increase in student welfare as the FSF reform. We estimate that a transfer 42% of the cost of the FSF reform would have generated an equivalent increase in student welfare. To highlight the role of the private school supply response, the welfare-equivalent transfer would have been 48% in the absence of private school exit.[47]

8 Aggregate Achievement

We have analyzed how the FSF reform and its associated private school supply response affected students' choices and schools' enrollments. Now we turn to another outcome — aggregate achievement — that is important for policymakers and potentially 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 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."[48] Due to data constraints, we will treat all of a school's students as receiving the same level of quality.[49]

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

[47]In addition to the strong identifying assumption, we note two other limitations. First, the students for whom a tuition increase at the local Catholic school affects their choice (and thus identify α_1) may not be the same students who switch in response to public school funding changes (and thus identify λ_g). Our model assumes homogeneous coefficients, but to the extent this is an inaccurate representation of preferences, our coefficient estimates are possibly driven by different sets of students. Second, the fact that tuition at certain schools varies with family size is inconsistent with our ϵ errors in our demand specification being i.i.d. Because we lack micro data on schooling choices of students from families of different sizes, we leave this for future work.

[48]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.

[49]Even in the absence of peer effects, there could be heterogeneity in students' achievement 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 instruction.

other states' testing programs, a large number of NY private schools also take the tests and report their results.[50] This allows us to compare achievement across the two sectors. A limitation of our data on private school test scores is that we only observe the mean scores for each school-grade-year. In Appendix D we discuss the adjustments we make to account for students entering or leaving a cohort. These adjustments require some strong assumptions and thus our results should be taken as merely suggestive effects on achievement.

To measure school quality, we estimate public schools' value-added with standard methods for student-level data. 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.[51] See Appendix D for details. We calculate a private school annual premium of 0.03σ in math and 0.05σ in ELA.[52]

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-grade and school fixed effects as well as our policy measure:

$$\hat{VA}_{kgt} = \lambda_{gt}^{VA} + \kappa_{k}^{VA} + \pi^{VA} FSF_k After2007_t + \mu_{kgt}^{VA}. \tag{14}$$

We run separate regressions for math and ELA and present the results in Table 8. While we do not find any statistically significant relationship between FSF funding and ELA value-added, we estimate that a school that received \$1,000 per student had an increase of math 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

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

[51] 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).

[52] While these estimated premiums are local to the context, they are broadly in line with estimates from NYC's 1997-98 – 1999-2000 voucher trial. 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, neither statistically significant. Our estimates come from data starting in 2000-2001. Because exit is negatively selected, we see improvements in aggregate private value-added over time, which could explain our slightly higher estimate in ELA. The Mayer et al. (2002) estimates are sensitive to choices about coding ethnicity and dealing with missing baseline scores, as noted by Krueger and Zhu (2004). More recently, Chingos and Peterson (2015) find that the voucher trial led to a large increase in college enrollment for African-American students.

qualitatively similar results when we estimate value-added with only students who stayed in the same school post-2007 or only students who switched schools post-2007. In the last two columns we repeat our specification test that uses the "hold harmless" clause and find our results do not seem to be driven by omitted factors.

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 funding experienced no change in value-added due to the reform.[53]

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.[54]

8.2 The Sorting Effect

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 essentially 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 A7 for the regression results. The reform also decreased private school entry, and we find that entrants are slightly negatively selected on average.[55] Therefore, students switching away from private schools

[53]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).

[54]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.

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

that closed, or failed to open, experienced an increase in school quality on average.

Finally, some students switched from private to public schools. Whether these students ended up at higher quality schools on average depends largely on the private school premium. We calculate this part of the sorting effect two ways. We first calculate the sorting effect using the ("smaller") private school premium we estimate. In this case, the sorting effect is roughly 0. We then 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, and find a negative effect.

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 9.[56] 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 $16 million from ELA improvements and $67 million from math improvements.

Depending on the size of the private school premium, the sorting effect possibly 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 could have been up to 18% 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 eighth graders. The total effect on these students' earnings ranged from $11-15 million in ELA and $55-67 million in math, depending on the size of the private school premium. How close the policy's benefits came to its costs therefore depended on the size of the sorting effect. This highlights the importance of considering demand shifts from the private sector, even for a policy targeting the public sector.

[56] 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%.

9 Conclusion

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

Our empirical analysis indicates that private schooling supply was responsive to a public school funding reform. We estimate that a private school located next to a public school that received a funding increase of $1,000 per student was 4.8 percentage points more likely to close in the next six years. Using our model estimates, we find that this change in supply of private schooling explained 32% 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 some 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 has considered approving the third largest voucher program in the nation, but there is concern that there are too few existing private schools to accommodate the potential demand shift toward private schooling.[57] 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.[58]

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.[59] Even traditional public school exit has become more common, as several large cities with declining populations have started closing public schools. Students' menu of schooling options are likely to continue changing with the increased churn of schools.

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

[58] This is consistent with observed private school entry in response to the Milwaukee voucher program (Carnoy, Adamson, Chudgar, Luschei and Witte 2007).

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

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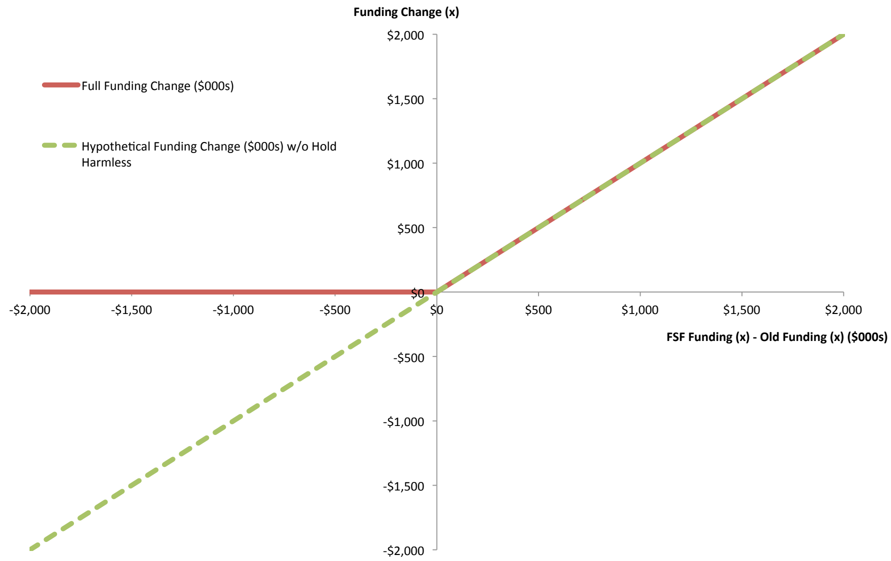
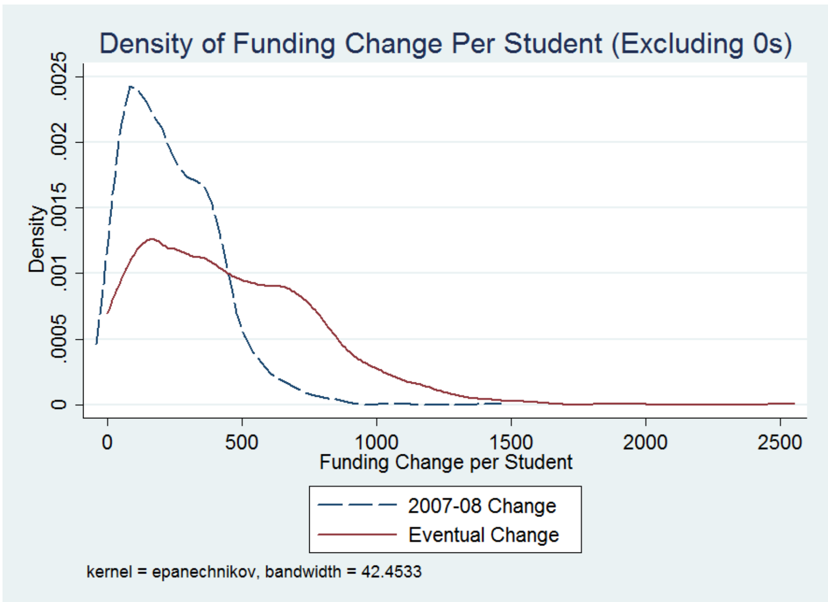
Figure 1: Funding Change Formula



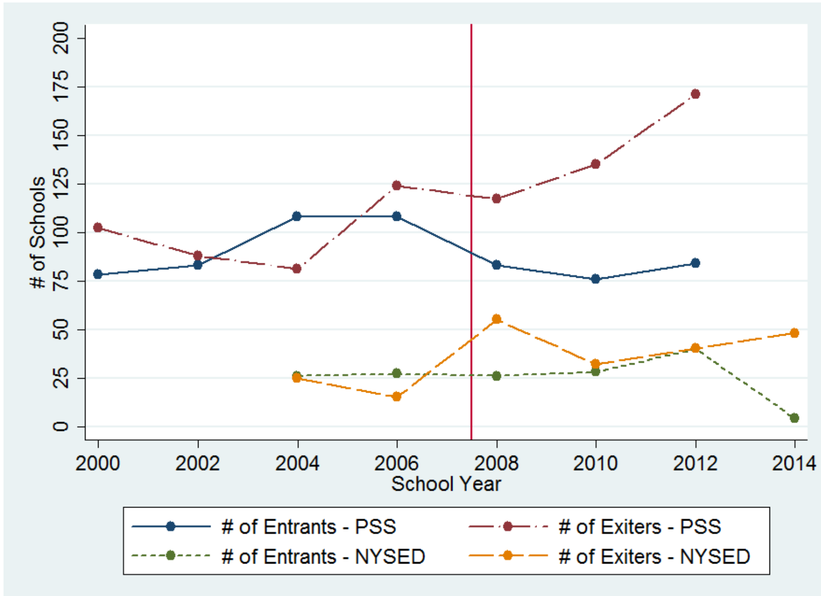
Note: Fixing a public school's enrollment and student demographics (x), the x-axis is the potential change in funding based on the FSF formula change. The y-axis is the actual change in funding and the solid line traces out the relationship.

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



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

Figure 3: Number of Entrants and Exiters in NYC



Note: PSS 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. NYSED entry and exit are determined by when schools appear in the annual NYSED data. The red line marks the implementation of the FSF reform.

Table 1: Private School Summary Statistics

School Characteristics	PSS Schools Active in 2005-06	Estimation Sample
Number of Private Schools	946	681
% Catholic	34%	42%
% Other Religious	41%	41%
% Non-Religious	25%	17%
% Elementary	66%	64%
% Secondary	15%	17%
% Combined	19%	18%
% Black	19%	18%
% Hispanic	17%	18%
% of Schools with > 50% Minority	43%	39%

The first column includes schools in the PSS that first appeared in the data in 2005-06 or earlier and last appeared in the data in 2005-06 or later. If the school did not appear in 2005-06 but appeared in years before and after, we use the school characteristics from the most recent year prior to 2005-06. The second column is our estimation sample and only includes the PSS schools active in 2005-06 that we can match to NYSED data on private schools. Elementary schools end before grade 9 and secondary schools start in grade 9. Minority students are black or hispanic.

Table 2: Differences-in-Differences Regressions of Enrollment

	Elementary and Middle Schools ln(Enroll)	Elementary and Middle Schools Enroll	Elementary and Middle Schools Enroll
FSF * After 2007	0.156***	36.475**	81.715
	(0.033)	(18.095)	(64.677)
Hyp. Neg. FSF * After 2007			-13.107
			(57.441)
Fixed Effects	Year, School	Year, School	Year, School
N	12,925	12,925	7,315
R-Squared	0.845	0.893	0.884

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 school years. The After2007 dummy variable is 1 starting in the 2007-08 school year. "FSF" is the per-student funding change (in $1,000s). Hyp. Neg. FSF is the FSF change in the absence of the Hold Harmless clause. For the last column, we narrow our sample to actual (or hypothetical) funding changes of less than $500/student in magnitude. Elementary and middle schools are schools that have students in grades K-8. Standard errors are clustered by school. Regressors are constructed using NYC DOE data on 2007-08 school budgets and enrollments are drawn from the Common Core of Data.

Table 3: Baseline Private School Exit Regressions

	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)
FSF * Dist1to5	0.037**	0.040*				
	(0.019)	(0.021)				
FSF * Dist6to10	0.022	0.021				
	(0.019)	(0.022)				
FSF			0.052***	0.048**		
			(0.013)	(0.019)		
FSF * Distance			-0.019**	-0.014		
			(0.008)	(0.009)		
FSF * (Enroll<250)					0.068***	0.063***
					(0.018)	(0.022)
FSF * (Enroll>250)					0.036**	0.025
					(0.016)	(0.021)
FSF * Distance * (Enroll<250)					-0.018	-0.013
					(0.012)	(0.012)
FSF * Distance * (Enroll>250)					-0.018*	-0.014
					(0.010)	(0.010)
Overall Exit Rate	0.16	0.16	0.16	0.16	0.16	0.16
Public School Controls		Yes		Yes		Yes
Fixed Effects		Subdistrict		Subdistrict		Subdistrict
N	681	681	681	681	681	681

* < 10%, ** < 5%, *** < 1%. The table reports marginal effects evaluated at the mean from a probit model. An observation is a private school that was open in 2006-2007 according to the NYSED, while Exit is 1 if the school exited the NYSED data in the next 6 years. FSF measures the public school's FSF per student funding change (in 000s). Dist1to5 indicates the 5 closest public schools to the private school. Dist6to10 indicates the 6th through 10th closest public schools to the private school. Distance between schools is measured in miles. (Enroll<250) is an indicator for whether the school's 2005-2006 enrollment was less than 250 students, the median in sample.

Table 4: Private School Entry Regressions

	1(Any Entrant within 1 Mile)	Number of Entrants within 1 Mile	1(Any Entrant within 1 Mile)	Number of Entrants within 1 Mile
FSF	-0.107**	-0.254***	-0.441**	-1.312***
	(0.052)	(0.092)	(0.162)	(0.374)
Hyp. Neg. FSF			0.118	0.637**
			(0.172)	(0.258)
Constant	0.383***	0.641***	0.446***	0.822***
	(0.042)	(0.110)	(0.056)	(0.143)
N	1,219	1,219	679	679
R-Squared	0.005	0.006	0.016	0.032

* < 10%, ** < 5%, *** < 1%. An observation is a public school. FSF measures the public school's FSF funding change per student (in 000s). Hyp. Neg. FSF is the FSF change in the absence of the Hold Harmless clause. For the last two columns, we narrow our sample to actual (or hypothetical) funding changes of less than $500/student in magnitude. The dependent variables are functions of the number of entrants after 2006-07, according to the NYSED, within 1 mile of a public school. Standard errors are clustered by subdistrict.

Table 5: Placebo Tests - Variation across Relative Losers and Level Mismatch

	Using Hold Harmless 1(Exit)	Mismatched Levels 1(Exit)
FSF	0.042**	-0.001
	(0.020)	(0.024)
FSF * Distance	-0.014	0.002
	(0.009)	(0.010)
Hyp. Negative FSF	0.004	
	(0.010)	
Hyp. Negative FSF * Distance	0.002	
	(0.004)	
Overall Exit Rate	0.16	0.19
Public School Controls	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict
N	681	558

* < 10%, ** < 5%, *** < 1%. The table reports marginal effects evaluated at the mean from a probit model. The first regression includes the actually implemented funding change as well as the hypothetical. The second regression matches private elementary schools to public high schools and vice versa. An observation is a private school that was open in 2006-2007, according to the NYSED. FSF measures the public school's FSF per student funding change (in 000s). Hyp. Negative FSF is the FSF change in the absence of the Hold Harmless clause. Distance is measured in miles.

Table 6: Model Estimates

		Grade								
Demand Parameters		K	1	2	3	4	5	6	7	8
γ	disutility from distance (miles)	-0.83	-0.84	-0.77	-0.70	-0.67	-0.65	-0.71	-0.58	-0.52
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
ρ	utility from zoned school	4.04	4.14	3.98	3.98	3.90	3.73	2.50	2.77	2.80
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
λ	utility from FSF ($1000s/student)	0.16	0.07	0.10	0.10	0.17	0.09	0.00	0.06	0.16
		(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
σ	sd of private school preference	0.28	0.84	0.00	0.01	0.95	0.47	3.19	2.62	2.18
		(0.01)	(0.02)	(0.32)	(0.01)	(0.04)	(0.01)	(0.06)	(0.01)	(0.02)
δ	standard deviation	1.64	1.74	1.16	1.07	1.16	1.07	1.73	1.43	1.40
Supply Parameters		Cath.	Other Relig.	Non-Relig.						
p	probability cost = 0	0.01	0.60	0.55						
		(0.19)	(0.08)	(0.09)						
μ_{elem}	mean cost per elem. grade (stud.)	20.98	17.61							
		(15.40)	(24.00)	9.00						
μ_{mid}	mean cost per middle grade (stud.)	29.71	46.62	(20.00)						
		(17.50)	(24.00)							

Demand parameters are estimated with method of simulated moments. The supply parameters are estimated with maximum simulated likelihood, using an exponential distribution. The mean of the distribution depends on the number of elementary and middle grades. 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 change in the enrollment share for public schools that received additional FSF funding from before to after the reform. The fourth demand moment is the covariance between a borough-year's private enrollment share and the share of schools that are private schools. The standard deviation of private school preference is from a normal distribution. Supply model standard errors were estimated using 100 block bootstrap replications with block size one square mile.

Table 7: Tuition IV Estimates

	First Stage	IV
	Average Tuition	Delta
Average Discount ($1000s)	1.037***	
	(0.052)	
Average Tuition ($1000s)		-0.140**
		(0.068)
N	321	321
R-Squared	0.551	0.132

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

Table 8: 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	0.004	0.023*	0.006	0.019	-0.012	0.023	0.019	0.056
	(0.008)	(0.013)	(0.008)	(0.013)	(0.015)	(0.018)	(0.026)	(0.039)
Hyp Neg FSF * After 2007							-0.005	-0.062
							(0.025)	(0.038)
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	28,078	28,073	27,774	27,770	21,553	21,516	15,880	15,877
R-Squared	0.261	0.252	0.238	0.217	0.176	0.230	0.221	0.236

* < 10%, ** < 5%, *** < 1%. Data span 2000-01 through 2011-12 school years. Each column is a separate regression of a school's estimated value-added (units of standard deviations on a test) in a subject on the school's change in per student funding (000s) due to the FSF reform. The estimated value-added is the estimated school-subject-grade-year fixed effect in a regression of the student's test score on cubic functions of her ELA and math test scores in the previous grade, separate dummy variables for gender, black, Hispanic, English-language learner, special education, and free or reduced lunch. Test scores come from the NY State ELA and Math tests in grades 4 and 8 and the NYC tests in grades 3, 5, 6, and 7. "Hyp Neg FSF" is the hypothetical negative funding change per student (000s) had the reform not had a hold harmless clause. The main regressions include all students. The middle columns estimate a school's value-added only for students who stayed in the same school post-2007 (columns 3 and 4) or students who switched schools post-2007 (columns 4 and 5). The last two columns restrict the sample to schools whose positive or negative FSF change was less than $500/student. Each regression includes year-grade, and school fixed effects. Standard errors are clustered by school. Test data comes from the NYC DOE.

Table 9: 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	$16	$67
Sorting Effect (Smaller Premium)	-$1	$0
Sorting Effect (Larger Premium)	-$5	-$12
Total Effect (Smaller Premium)	$15	$67
Total Effect (Larger Premium)	$11	$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.

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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 a census of schools using the NCES's Private School Survey (PSS).

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 2003-04 and 2007-08 waves of the PSS but not in the 2005-06 wave. For the 2005-06 wave, 880 private schools appear in the data while an additional 66 schools do not appear but are open both in a year prior to 2007-08 and a year after 2007-08. These schools tend to be smaller than the average school and are more likely to be non-Catholic religious schools.

We treat these schools as neither entering nor exiting in 2005-06 and include these schools in our summary statistics. In the summary statistics, we include these schools and use their characteristics (e.g., enrollment per grade) from the most recent pre-2005 PSS wave that includes the school.

We also 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.[60] Catholic school closures tend to receive media attention and thus verification of their status was typically easy. Based on our internet searches, we find that up to 28% of the inferred exiters did not actually exit.

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.

[60] 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.

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

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 2012-13 school year, we have combined internet research and phone calls to collect current-year tuition data.

B Appendix: Public School Expenditure

B.1 Funding Changes and School Characteristics

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

$$y_k = \phi_0 + \phi_1' X_{1k} + \phi_2' X_{2k} + \omega_k. \tag{15}$$

Table A1 shows the results for two measures of y_k: an indicator variable for whether the school received additional money from the FSF reform and, conditional on receiving money, the total funding increase per student. 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 that the funding increase is strongly predicted by the number of students with limited English proficient, the number of Hispanic students, and measures of teacher certification, experience, and turnover. Because the "winning" and "losing" schools differ statistically along a few characteristics, we use the timing of the reform to separate the reform's effects from 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 A.3. For each of the two panels, plotting Brooklyn and the Bronx respectively, we divide the borough according to U.S. Census tracts and shade the tract by the 2000 Census median income for households with children. The darker tracts are areas with higher median household income. We then overlay a series of public school locations where the circles are the schools that received money and the triangles are the schools that did not. The size of the circle is proportional to the funding increase. For both boroughs we see that schools that receive money tend to be located in poorer areas, but we still have considerable spatial variation as the "winners" and "losers" are not located in completely different types of neighborhoods. We use this spatial variation in relation to private school locations to see if private schools located near "winners" are more likely to close after the reform.

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

B.2 Expenditure of Funds

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

$$Expend_{kt}^{c} = \delta_k^c + \tau_t^c + \pi^c FSFChange_k * After2007_t + \eta_{kt}^c \quad (16)$$

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 A3a 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.3 Changes in Teacher and Classroom Characteristics

We run similar regressions where instead of using category expenditure as our outcome we look at the effect of additional funding on teacher and classroom characteristics. In Table A3b 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.[61] 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

[61] 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.

funding increase, a school's average annual teacher salary increased by $261.

B.4 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 A4. 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 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: Heterogeneous Effects on Private School Exit

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 column of Table A5, which shows 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 Catholic schools (the last column of Table A5). Particularly in large urban areas, many of the students attending Catholic schools are non-Catholic minorities who may not have a strong preference for religious education.[62]

D Appendix: Achievement Calculations

D.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):

$$\begin{aligned} y^s_{i,k,g,t} = &\beta_1 y^{math}_{i,g-1,t-1} + \beta_2 (y^{math}_{i,g-1,t-1})^2 + \beta_3 (y^{math}_{i,g-1,t-1})^3 + \beta_4 y^{ela}_{i,g-1,t-1} \\ &+\beta_5 (y^{ela}_{i,g-1,t-1})^2 + \beta_6 (y^{ela}_{i,g-1,t-1})^3 + X_i' \beta_7 + \theta^s_{k,g,t} + \epsilon^s_{i,k,g,t}. \end{aligned} \tag{17}$$

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

[62]When we add in schools that are in the PSS but not NYSED data, we find strong effects for non-religious schools as well.

school-grade-year fixed effects as our value-added measures.

D.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_j^s) from the following regression for private school j:

$$\bar{y}^s_{j,8,t} = \alpha \bar{y}^s_{j,4,t-4} + \mu^s_t + \theta^s_j + \epsilon^s_{j,g,t} \tag{18}$$

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.[63] 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 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.[64] 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

[63] 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 2008-09 fourth grade cohorts.

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

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.

D.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.[65] 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 after grades 4 through 7 and find that these students have higher test scores than average.[66] 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.[67] Based on the relative flows in and out of the public and private school sectors,[68] 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.[69] 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 composi-

[65] 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.

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

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

[68] 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.

[69] 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.

tional 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.

The private schools that closed (or failed to enter) are negatively selected. We estimate that relative to the schools that remain open, exiters are 0.083σ worse in math and 0.056σ worse in ELA and entrants are 0.076σ worse in math and 0.016σ worse in ELA. Thus, using our estimated private school premium, exiters and entrants offer the same quality instruction as the average public school. Using the larger estimates from Mayer et al. (2002), and assuming that all of the students in the voucher lottery used attended schools that remained open, we estimate that exiters and entrants are better than the average public school.

D.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 8. We assume that schools that did not receive additional funding experienced no change in value-added due to the reform. Thus, to generate counterfactual school quality we subtract the differences-in-differences coefficient multiplied by the funding change per student. As an example, if a school's value-added was 0 and it received a funding increase of \$1,000/student, its counterfactual value-added would be -0.023 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.

D.5 Estimating Counterfactual School Enrollments

Finally, we estimate counterfactual school enrollments. 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 1.7% private school closure rate to the reform. We do not have an explicit model of entry, but we find preliminary evidence that the reform may have deterred as much entry as the amount of exit it caused by estimating Equation 6 but replacing the number of entrants with the number of exiters. We estimate that a public school receiving a funding increase of $1,000 per student had 0.26 (0.11) more exiters after the reform. This estimated coefficient is similar in magnitude to our entry estimate of -0.25 (0.09).[70] Furthermore, we estimate that the reform's effect on surviving private school enrollments, which combines students arriving at these schools from closing neighbors and students leaving to attend the better funded public schools, is roughly 0. Thus, for our calculations we need to consider simply students leaving the private sector and students switching between public schools.

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 0.7 percentage points in response to the reform.[71]

For public schools, we turn to our differences-in-differences estimates from Table 2. We undo the reform's enrollment effects by subtracting off the differences-in-differences coefficient multiplied by the funding change per student. 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 0.7 percentage points.

E Appendix: Model Simplifications

E.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

[70] We note that these regressions are not our preferred specifications and should be taken as suggestive. 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.

[71] The private sector's pre-reform share was 19.8%. The sector's 2011-2012 enrollment share was 17.4%, so we attribute about 30% of the decrease in the private enrollment share to the reform.

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 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.[72] 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.[73]

E.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.

[72] 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.

[73] 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%.

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.[74] We then use these changes as our dependent variables in regressions following our private school regressions, Equation 4 and Equation 5.

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 5. When we use Equation 4, 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.

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

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

The more relevant consideration may be whether our model gives students too many options. It is unlikely families consider every public and private school in their borough.[75] While the large estimates for the disutility to distance and the utility to attending the zoned school should make far away options have small probabilities of being chosen, the logit functional form could inflate probabilities for unlikely events. To the first order, the logit functional form should then predict more sorting across the borough than is realistic. For instance, when a private school closes, we might over predict the number of students who would then switch to a far-away school. This over prediction, though, should mostly add noise to our model results, which compare outcomes at schools ("winners" vs. "losers") with different changes in their local competitors. The extent to which the functional form smooths out local differences would lead us to underestimate such results.

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

[75] 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.

A Appendix Figures

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

Figure A.1a: School that Gets Additional Funding

School: P.S. 189 Lincoln Terrace

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

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

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

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

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

School: P.S. 189 Lincoln Terrace

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

Figure A.3: Locations of Public Schools

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

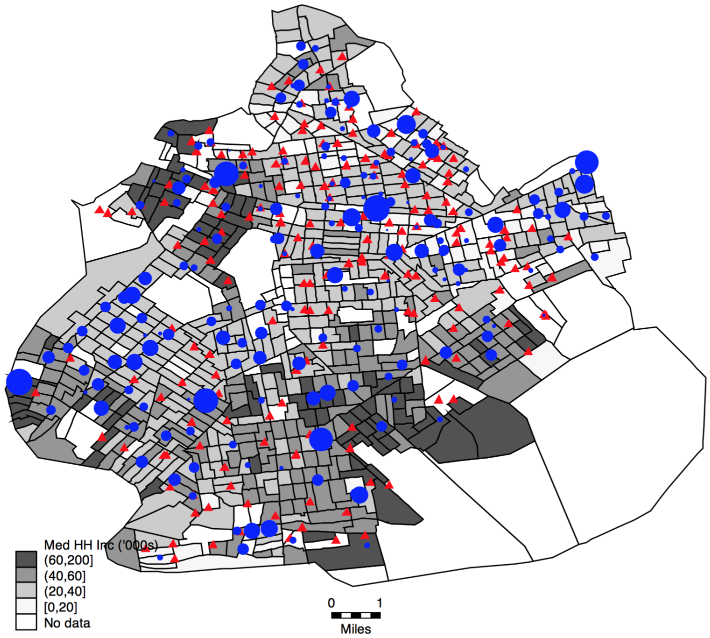
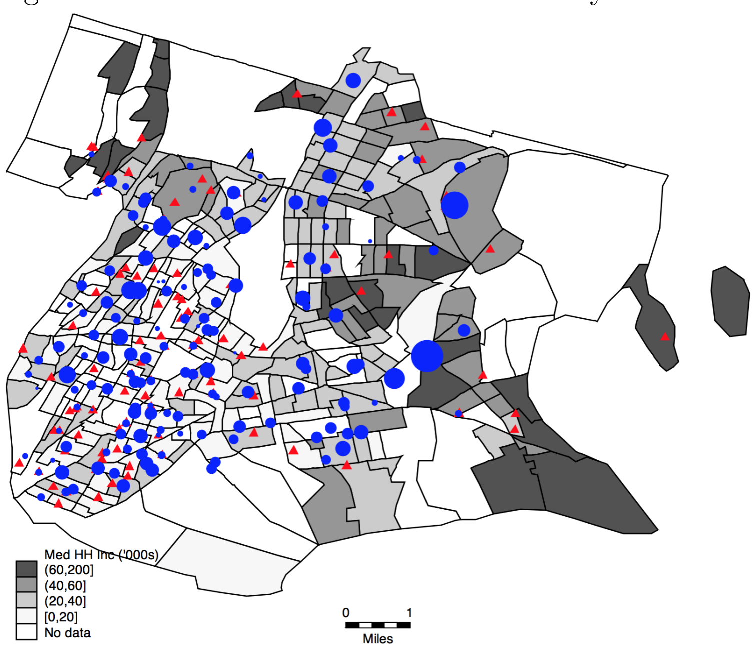


Figure A.3b: Public Schools in the Bronx by HH Income



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

Figure A.4: Locations of Private Schools

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

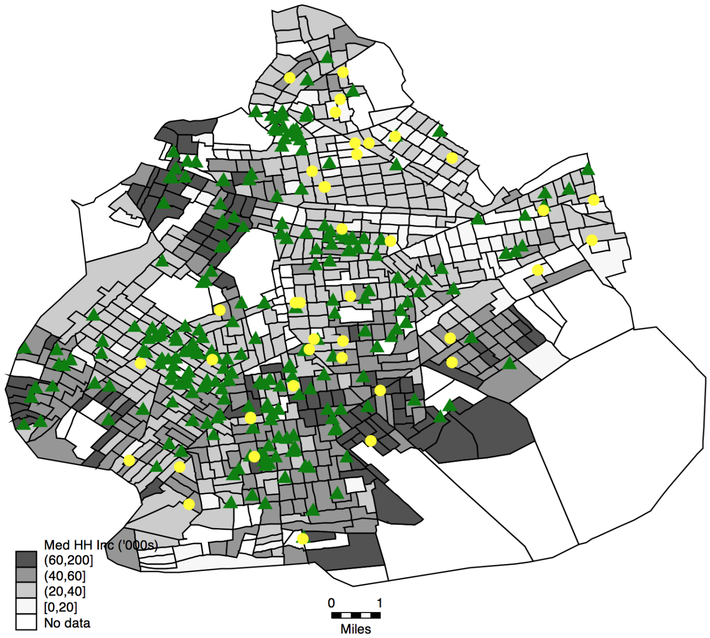
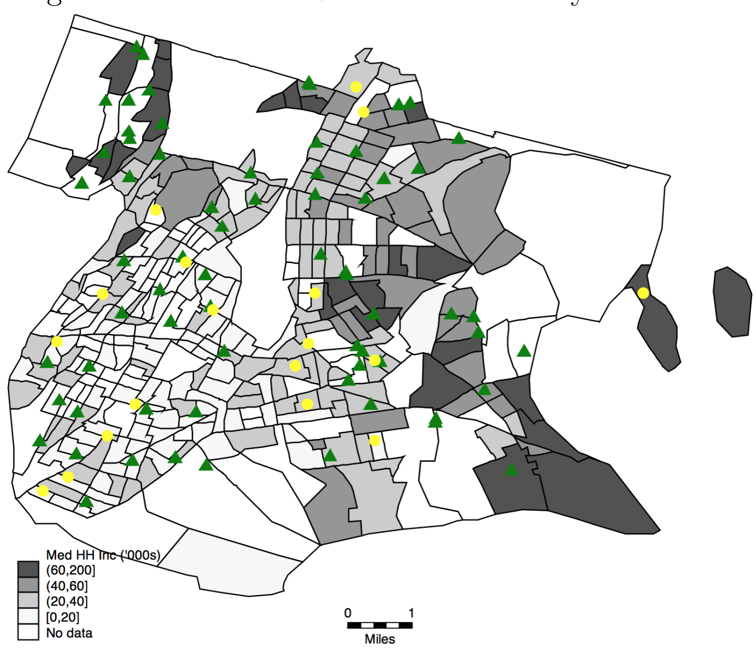
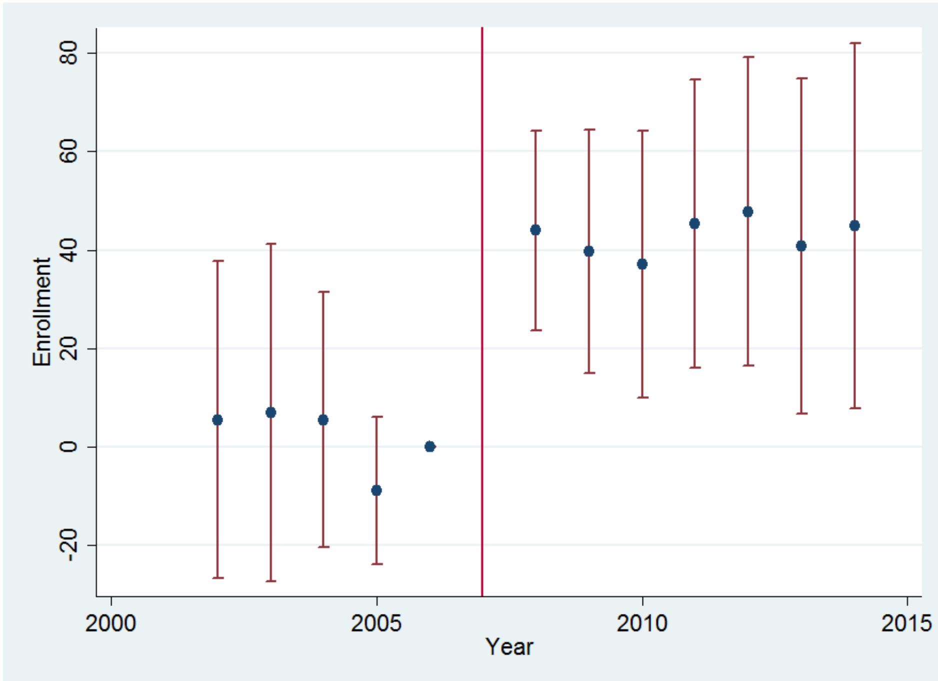


Figure A.4b: Private Schools in Bronx by HH Income



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

Figure A.5: Coefficients of Enrollment Regressed on FSF Funding Change



Note: The figure shows estimated coefficients (and 90% confidence intervals) on FSF funding ($1,000s/student) for each year from the differences-in-differences regression of enrollments with school fixed effects and year fixed effects. The enrollments in the 2005-2006 school year are normalized to 0.

B Appendix Tables

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

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

* < 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 left-hand-side of the first regression is an indicator for whether the public school received money. The left-hand-side of the second regression is the funding increase per student and is limited to schools that received increases. The % Stability is a NY State measure that captures the percentage of students who are in the grade normally associated with a certain age. The dependent variables come from NYC Department of Education data on school budgets in 2007-08. The right-hand-side variables are drawn from NYSED School Report Cards.

Table A2: Differences-in-Differences Regressions of Enrollment - by Race

	FRL Enroll	Asian Enroll	Black Enroll	Hispanic Enroll	White Enroll
FSF * After 2007	59.477***	5.791	32.218***	-1.703	-0.769
	(12.983)	(5.896)	(7.528)	(12.228)	(4.004)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School
N	10,014	12,925	12,925	12,925	12,925
R-Squared	0.821	0.966	0.933	0.929	0.967

* < 10%, ** < 5%, *** < 1%. Data span the 2001-02 through 2013-14 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 per-student funding change (in $1,000s). Elementary and middle schools are schools that have students in grades K-8. Standard errors are clustered by school. Regressors are constructed using NYC DOE data on 2007-08 school budgets and enrollments are drawn from the Common Core of Data.

Table A3: Expenditure and School Characteristics Regressions

Table A.3a: 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 counseling, drug prevention programs, and after school activities. "Administrators" include salary and benefits for principals, assistant principals, supervisors, secretaries, and school aides. "Other Direct Services" includes spending on ancillary services (food, transportation, safety, computers), building services, and regional support. "Field Support" includes spending on sabbaticals, leaves, termination pay, and salary additions. "System-Wide Costs" includes support for central administration, debt service and retiree benefits, and funds for non-public schools. Data come from NYC DOE line-item expenditures.

Table A.3b: 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	4.021***	1.031***	-0.069***	0.013	-0.000	-0.182	-1.192
	(1.337)	(0.300)	(0.015)	(0.009)	(0.011)	(0.384)	(1.108)
Dep Var Mean in 2006-07	106.0	17.0	0.12	0.29	0.15	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	4,910	1,855
R-Squared	0.946	0.835	0.699	0.876	0.534	0.678	0.647

* < 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 per student (000s). 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. School characteristics come from NYSED Report Cards.

Table A4: 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 come from NYC DOE and 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.

Table A5: Heterogeneous Effects

	Schooling Level	Religious Instruction
	1(Exit)	1(Exit)
FSF * Not High	0.075***	
	(0.025)	
FSF * High	0.012	
	(0.037)	
FSF * Distance * Not High	-0.038**	
	(0.019)	
FSF * Distance * High	-0.005	
	(0.010)	
FSF * Catholic		0.054***
		(0.020)
FSF * Other Religious		0.032
		(0.026)
FSF * Non Religious		-0.028
		(0.055)
FSF * Distance * Catholic		-0.014
		(0.010)
FSF * Distance * Other Religious		-0.013
		(0.014)
FSF * Distance * Non Religious		-0.007
		(0.033)
Overall Exit Rate	0.16	0.16
Public School Controls	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict
N	681	681

* < 10%, ** < 5%, *** < 1%. The table reports marginal effects evaluated at the mean from a probit model. An observation is a private school that was open in 2006-2007, according to the NYSED. FSF measures the public school's FSF per student funding change (in 000s). Distance between public and private schools is measured in miles.

Table A6: Placebo Tests - Mismatched Timing

	2003-4 to 2007-8	2007-08 to 2011-12
	1(Exit)	1(Exit)
FSF * (Enroll<250)	0.006	0.034**
	(0.007)	(0.015)
FSF * (Enroll>250)	-0.008	-0.011
	(0.009)	(0.015)
FSF * Distance * (Enroll<250)	-0.002	-0.008
	(0.004)	(0.008)
FSF * Distance * (Enroll>250)	0.001	-0.000
	(0.005)	(0.006)
Overall Exit Rate	0.11	0.14
Public School Controls	Yes	Yes
Fixed Effects	Subdistrict	Subdistrict
N	549	684

* < 10%, ** < 5%, *** < 1%. The table reports marginal effects evaluated at the mean from a probit model. 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. An observation is a private school that was open at the beginning of the indicated time period, according to the NYSED. FSF measures the public school's FSF per student funding change (in 000s). Distance between public and private schools is measured in miles. (Enroll<250) is an indicator for whether the school's enrollment in the first year of the window was less than 250 students.

Table A7: Relationship between Private School Value-Added and Exit

	1(Exit)	1(Exit)	1(Exit)
Value-Added ELA + Math	-0.347***		
	(0.100)		
Value-Added ELA		-0.564***	
		(0.190)	
Value-Added Math			-0.630***
			(0.174)
N	198	205	203

* < 10%, ** < 5%, *** < 1%. Estimates are marginal effects, evaluated at the mean, from probit models. 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 and data comes from the NYSED.