Online Appendix

Quantifying the Supply Response of Private Schools to Public Policies

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ONLINE APPENDIX

A Appendix: Conceptual Framework

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

The private school has fixed characteristics, including price, and a payoff function, $\Pi(Q_2(\gamma x)) - FC_j$, from remaining open. $\Pi(.)$ is some function that is weakly increasing in the private school's enrollment, $Q_2(\gamma x)$, with output expressed in monetary units. In particular, $\Pi(.)$ could be a variable profit function for profit-maximizing schools with prices exceeding marginal costs. Or $\Pi(Q_2) = \lambda Q_2$, $\lambda > 0$, for mission-based schools interested in educating the most students they can. FC_j is the fixed operating cost to keeping j open. It is private information for the school and is drawn from a distribution with smooth CDF G and derivative g. The private school closes if it would receive a negative payoff from remaining open. Thus, the probability the private school will close is $G(-\Pi(\gamma x))$, which depends on the public school's funding through its effect on students' choices. We consider schools that would remain open absent any change in public funding, so we impose that $G(-\Pi(0)) = 0$.

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

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

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

$$F_\Delta(0)\frac{\partial G(-\Pi(\gamma x))}{\partial x}\Big|_{x=0} + \gamma f_\Delta(0) \tag{2}$$

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

$$\mathbb{E}\left[S(x)\right] = (1 - G(-\Pi(\gamma x)))S^{NC}(x) + G(-\Pi(\gamma x))S^{C}(x), \tag{3}$$

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

$$S^{NC}(x) = \mathbb{E}\left[u_2\right] + \int_{-\gamma x}^{\infty} (\Delta u + \gamma x) dF_{\Delta} \tag{4}$$

and

$$S^{C}(x) = \mathbb{E}\left[u_2\right] + \int_{-\infty}^{\infty} (\Delta u + \gamma x) dF_{\Delta} \tag{5}$$

with

$$S^{C}(x) - S^{NC}(x) = \int_{-\infty}^{-\gamma x} (\Delta u + \gamma x) dF_{\Delta} \tag{6}$$

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

$$S^{NC}(x) = \max_{\bar{\Delta}} \mathbb{E}\left[u_2\right] + \int_{\bar{\Delta}}^{\infty} (\Delta u + \gamma x) dF_{\Delta} \tag{7}$$

$$S^{C}(x) - S^{NC}(x) = \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_{\Delta} \tag{8}$$

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

(9)
$$\begin{aligned} \frac{d\mathbf{E}\left[\mathbf{S}(\mathbf{x})\right]}{dx} &= -\frac{\partial G(-\Pi(\gamma x))}{\partial x} S^{NC}(x) + (1 - P^{C}(\gamma x)) \frac{\partial S^{NC}(x)}{\partial x} + \frac{\partial G(-\Pi(\gamma x))}{\partial x} S^{C}(x) + G(-\Pi(\gamma x)) \frac{\partial S^{C}(x)}{\partial x} \\ &= \frac{\partial S^{NC}(x)}{\partial x} + G(-\Pi(\gamma x)) \left(\frac{\partial (S^{C}(x) - S^{NC}(x))}{\partial x} \right) + \frac{\partial G(-\Pi(\gamma x))}{\partial x} \left(S^{C}(x) - S^{NC}(x) \right) \end{aligned}$$

Take each term separately:

$$\begin{aligned} \frac{\partial S^{NC}(x)}{\partial x} &= \frac{\partial \max_{\bar{\Delta}} \int_{\bar{\Delta}}^{\infty} (\Delta u + \gamma x) dF_{\Delta}}{\partial x} \\ &= \gamma (1 - F_{\Delta}(-\gamma x)) \end{aligned} \tag{10}$$

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

$$\frac{\partial(S^C(x) - S^{NC}(x))}{\partial x} = \frac{\partial \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_\Delta}{\partial x} = \gamma F_\Delta(-\gamma x) \tag{11}$$

The third term:

$$\begin{aligned} \frac{\partial G(-\Pi(\gamma x))}{\partial x} \left(S^C(x) - S^{NC}(x)\right) &= \frac{\partial G(-\Pi(\gamma x))}{\partial x} \max_{\bar{\Delta}} \int_{-\infty}^{\bar{\Delta}} (\Delta u + \gamma x) dF_\Delta \\ &= \frac{\partial G(-\Pi(\gamma x))}{\partial x} \left(\mathbb{E}\left[\Delta u | \Delta u < -\gamma x\right] + \gamma x\right) F_\Delta(-\gamma x) \end{aligned} \tag{12}$$

Combining all the terms:

(13)

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

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

$$\left.\frac{d\mathbb{E}\left[S(x)\right]}{dx}\right|_{x=0} = \gamma(1 - F_\Delta(0)) + \left.\frac{\partial G(-\Pi(\gamma x))}{\partial x}\right|_{x=0} \mathbb{E}\left[\Delta u | \Delta u < 0\right] F_\Delta(0) \tag{14}$$

Suppose that students trade off a school's effect on their academic achievement (θ_{ij}) with the amount they pay for private education (p_j): $u_{i1} = \theta_{i1}$ and $u_{i2} = \theta_{i2} - \alpha p_2$. Let spending x have a constant effect βx on achievement. Then achievement, $A(x)$ is:

$$A^{NC}(x) = \mathbb{E}\left[\theta_2\right] + \int_{-\gamma x}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta \tag{15}$$

$$A^{C}(x) = \mathbb{E}\left[\theta_2\right] + \int_{-\infty}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta \tag{16}$$

(17)

$$\mathbb{E}\left[A(x)\right] = (1 - G(-\Pi(\gamma x)))A^{NC}(x) + G(-\Pi(\gamma x))A^{C}(x)$$
$$= \mathbb{E}\left[\theta_2\right] + \int_{-\gamma x}^{\infty} (\Delta u - \alpha p_2 + \beta x) dF_\Delta + G(-\Pi(\gamma x)) \int_{-\infty}^{-\gamma x} (\Delta u - \alpha p_2 + \beta x) dF_\Delta$$

(18)

$$\frac{d\mathbb{E}\left[A(x)\right]}{dx} = \gamma(1 - G(-\Pi(\gamma x)))(-\gamma x + \beta x - \alpha p_2) f_\Delta(-\gamma x) + \beta(1 - F_\Delta(-\gamma x)) + G(-\Pi(\gamma x))\beta F_\Delta(-\gamma x))$$
$$+ \frac{\partial G(-\Pi(\gamma x))}{\partial x} \int_{-\infty}^{-\gamma x} (\Delta u - \alpha p_2 + \beta x) dF_\Delta$$
$$= \gamma(1 - G(-\Pi(\gamma x)))(-\gamma x + \beta x - \alpha p_2) f_\Delta(-\gamma x) + \beta(1 - F_\Delta(-\gamma x) + G(-\Pi(\gamma x)) F_\Delta(-\gamma x)))$$
$$+ \frac{\partial G(-\Pi(\gamma x))}{\partial x} \mathbb{E}\left[\Delta u - \alpha p_2 + \beta x | \Delta u < -\gamma x\right] F_\Delta(-\gamma x)$$

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

(19)
$$-\gamma \alpha p_2 f_\Delta(0) + \beta(1 - F_\Delta(0)) + \left.\frac{\partial G(-\Pi(\gamma x))}{\partial x}\right|_{x=0} \mathbb{E}\left[\Delta u - \alpha p_2 | \Delta u < 0\right] F_\Delta(0)$$

B Appendix: FSF Reform Details

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

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

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

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

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

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

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

C Appendix: Data

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

C.A Private School Survey and NYSED Data

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

We thus supplement the PSS with data from the New York State Education Department (NYSED) that includes enrollments for private schools that register with the state and have a registration code. Because not all schools have registration codes, this data set does not capture all schools in the PSS and includes fewer school characteristics. But while the NYSED data is a smaller sample of more stable schools (those with registration codes), using it allows us to infer entry and exit with considerably more precision. For the difference-in-difference analysis of private school supply, we create our estimation sample by taking the PSS schools and keeping those with a single match in the NYSED data based on name and borough. But for the model of school choice, which relies on specifying the full set of schooling options, we include the rest of the PSS schools.

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

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

To assess whether these judgment calls matter, we reproduce the supply regressions with different criteria for matching. We present the results in Appendix Table A10. The first set of columns follows the strict matching process, the last set follows the looser process, and the middle set of columns follows the strict matching process but includes preschools that also have kindergarteners. Within each set of columns, we vary whether we keep all matches, only matches that agree across the data sets on when the school opened and closed (up to a 1 year difference given that the PSS is released biennially), or only matches that agree on when the school opened and closed and whose difference in enrollments per grade across the data sets always falls within the 5th to 95th percentile. We find consistent results throughout the table, with somewhat smaller, but more precise, coefficients as we decrease the sample. We similarly show consistent event study results in Appendix Figure A.7. In fact, the slight evidence of a pre-trend in the main specification disappears once we drop schools with year or enrollment inconsistencies across the data sets.

C.B Private School Test Data

Our test score data on nonpublic schools come from the New York State Education Department. The test data are school-grade-year average test scores on the grade 4-8 math and ELA state tests. Only a handful of states even collect test data from private schools, so this paper uses some of the first test-based evidence of U.S. private school quality on a large fraction of the private school population in a geographic area. New York does not require that private schools take the test nor report the results. The schools that opt not to report the test results are a selected sample and are more likely to include high-tuition college prep schools.

The main data limitation is that we only have value-added estimates for 36% of the private school students. Based on observable characteristics from the PSS, schools with value-added estimates differ in several ways. We are more likely to have value-added estimates for Catholic schools, schools in the Bronx, schools with a higher percentage of Hispanic and

Asian students, schools with more students, and schools with fewer teachers. We are unlikely to have value-added estimates for non-religious schools, single-sex schools, and specialty schools. Schools in Manhattan and Brooklyn are also slightly underrepresented relative to the other boroughs.

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

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

C.D Private School Tuition and Expenditure

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

D Appendix: Structural Model Estimation

D.A Demand Model

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

$$\Delta n_g = n_{g,2010} - n_{g,2000} \tag{20}$$

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

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

We then construct distances from the student's implied residence to each school in her borough that educates students from her grade. We designate the student's zoned school as the closest public school that has zoned students. We combine these data with our enrollment data for public (Common Core of Data), charter (Common Core of Data), and private schools (Private School Survey) and our measures of FSF funding.

We estimate our demand model using data from the 2001-02, 2003-04, 2005-06, 2007-08, 2009-10, and 2011-12 school years to cover student enrollment decisions before and after the reform's implementation. We use data from every other year because that is the frequency of the PSS. We normalize $\delta_{kg} = 0$ for the public school in each borough with the largest average enrollment in grade g.

To estimate the demand parameters, we use an exactly-identified simulated method of moments procedure. We do this separately for each grade from kindergarten to eighth grade. We list the moments and parameters to be estimated in Table 8. We draw a single value per student location of ν_{igt} from a $N(0,1)$ distribution. We iterate back-and-forth, starting with a contraction mapping that holds fixed $\gamma, \sigma, \tau, \rho$, and λ and solves for the unique δ that makes the predicted school-grade enrollment shares match the associated moments. We then hold fixed δ and find the $\gamma, \sigma, \tau, \rho$, and λ that minimize a GMM objective function with the remaining moments and an identity weighting matrix.

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

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

D.B Supply Model

We estimate our supply model using private school closure decisions between 2002-2004, 2004-2006, 2006-2008, 2008-2010, and 2010-2012. For each year t, we fix the set of strategic private schools as those that were open in year $t-2$. We implement any changes to the public schools exogenously (e.g., increased projected funding) and then solve for an exit equilibrium. We compare the model's predicted exits to the actual exits. We infer exit from whether the school's last appearance in the PSS and check these with the NYSED data.

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

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

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

$$L = \sum_t \sum_j (C_{jt} \log(\hat{P}_{jt}) + (1 - C_{jt}) \log(1 - \hat{P}_{jt})) \tag{21}$$

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

D.C Counterfactuals

For the demand counterfactual, we predict 2005-06, 2007-08, and 2009-10, and 2011-12 school enrollments under two scenarios. In the first scenario, we use the estimated model except we fix the public school market structure (the schools that are open and students' zoned public schools) at the 2005-06 market structure. In the second scenario, we use the estimated model except we fix the public *and* private school market structures to the 2005-06 market structure. In both cases, we use the private-year shock from 2005-06 (τ_2). We estimate public school enrollments in both scenarios for each of the four years, split the public schools subject to the reform into FSF "winners" and "losers," and produce Figure 7. In 2006, we estimate that the difference in mean enrollment between "winners" and "losers" is 136.85. In 2012, we estimate that the difference in mean enrollment between "winners" and "losers" is 164.46 when the private market structure changes and 158.92 when it is held fixed.

We estimate the direct effect as the change in relative sizes of "winners" versus "losers" from the fixed market structure counterfactual divided by the change in relative sizes of "winners" versus "losers" from the counterfactual where the private school market structure is not fixed: (158.92-136.85)/(164.46-136.85) = 0.80. The indirect effect is the total effect minus the direct effect: 0.20. In Panel A of Table 10 we show the flows between predicted actual (with the reform) and predicted counterfactual (no reform) choices.

For the supply counterfactual, we predict the exits induced by the FSF reform. For each school j, we first calculate bounds on its F_j^{exp}, its non-zero and non-infinite fixed cost, based on its exit and non-exit decisions in sample. Consider a school that was open in 2004, 2006, and 2008 but closed in 2010 and suppose we estimate that the school's enrollments in those years were 100, 120, 100, and 90.[1] Then we would bound F_j^{exp} between 90 and 100 and draw $F_{j,s}^{exp}$ for each simulation iteration s from a truncated exponential distribution. For schools that never exit, the lower bound is 0, and for schools that always exit, the upper bound is infinite. For 4% of schools, the upper and lower bounds cross. In these cases, we impose that the upper bound is infinite.

With these bounds, we then estimate two sets of equilibria for each year. In the first, we set FSF projected funding equal to its actual value in each year. In the second, we set FSF projected funding equal to 0 for all schools.[2] For each scenario, we simulate 100 equilibria, where we draw separate fixed cost vectors, and then calculate a school's exit probability as the fraction of simulation iterations where we predict the school would exit. We attribute the increased exit probabilities in the second scenario – relative to the first scenario – to the reform.

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

D.D Welfare

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

[1]The estimated enrollments are from the estimated supply equilibrium in each year.

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

Next, define estimated representative utilities as:

$$\hat{v}_{ijgt} = \hat{\delta}_{jg} - \hat{\gamma}_g d_{ij} + \hat{\sigma}_g \nu_{igt} + \hat{\tau}_{gt} \tag{22}$$

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

$$\hat{v}_{ikgt,FSF} = \hat{\delta}_{kg} - \hat{\gamma}_g d_{ik} + \hat{\rho}_g ZONED_{ikt} + \hat{\lambda}_g FSF_{kt} \tag{23}$$

$$\hat{v}_{ikgt,noFSF} = \hat{\delta}_{kg} - \hat{\gamma}_g d_{ik} + \hat{\rho}_g ZONED_{ikt} \tag{24}$$

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

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

1. **No FSF Funding and No Supply Response**:

$$\hat{w}_{1t} = \frac{1}{N_t}\sum_{i=1}^{N_t}\frac{1}{S}\sum_{s=1}^{S}\ln\{\sum_{l\in\hat{C}_{s,t,noFSF}} exp(\hat{v}_{ilgt,noFSF})\} \tag{25}$$

2. **FSF Funding and No Supply Response**:

$$\hat{w}_{2t} = \frac{1}{N_t}\sum_{i=1}^{N_t}\frac{1}{S}\sum_{s=1}^{S}\ln\{\sum_{l\in\hat{C}_{s,t,noFSF}} exp(\hat{v}_{ilgt,FSF})\} \tag{26}$$

3. **FSF Funding and Supply Response**:

$$\hat{w}_{3t} = \frac{1}{N_t}\sum_{i=1}^{N_t}\frac{1}{S}\sum_{s=1}^{S}\ln\{\sum_{l\in\hat{C}_{s,t,FSF}} exp(\hat{v}_{ilgt,FSF})\} \tag{27}$$

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

To estimate the size of the unconditional transfer that would have produced the same change in welfare as the reform, we sum the estimated welfare change for the 3 years post-reform ($\Delta\hat{w} = \sum_{t\in\{2008,2010,2012\}}(\hat{w}_{3t} - \hat{w}_{1t})/|\hat{\alpha}_1|$) and sum the projected FSF funding

[3] We maintain that students' choice sets are the schools in the borough the students lives in. We avoid introducing this borough restriction into the notation, but we keep the same market definition as in the estimated model.

$(TotFSF = \sum_{t \in \{2008,2010,2012\}} \sum_k FSF_{kt})$. Because $TotFSF$ captures projected funding, we scale it by our range of first stage coefficients (.59, .91) from Section IV.A. Our bounds are then $(\Delta\hat{w}/(0.91TotFSF), \Delta\hat{w}/(0.59TotFSF))$. We make analogous calculations for the welfare change without exit, where we replace $\hat{w}_{3t}$ with $\hat{w}_{2t}$.

E Appendix: Public School Expenditure

E.A 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:

$$y_k = \phi_0 + \phi_1' X_{1k} + \phi_2' X_{2k} + \omega_k. \tag{28}$$

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

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

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

E.B 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:

$$Expend_{kt}^{c} = \delta_k^c + \tau_t^c + \pi^c FSFChange_{kt} + \eta_{kt}^c \tag{29}$$

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

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

E.C Changes in Teacher and Classroom Characteristics

We run similar regressions where instead of using category expenditure as our outcome we look at the effect of additional funding on teacher and classroom characteristics. In Table A5b we present the results. We find that a school that received a projected funding increase of \$1,000 per student increased its number of teachers by 4.3 after the reform. At the same time, we find these schools' teachers tend to be more experienced and that class size falls slightly. Using teacher-level data from the New York City Department of Education, we find that for each projected funding increase of $1,000 per student, a school's average annual teacher salary increased by $830.

F Appendix: Achievement Calculations

F.A 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):

$$\begin{aligned} y^s_{i,k,g,t} = {} & \beta_1 y^{math}_{i,g-1,t-1} + \beta_2 (y^{math}_{i,g-1,t-1})^2 + \beta_3 (y^{math}_{i,g-1,t-1})^3 + \beta_4 y^{ela}_{i,g-1,t-1} \\ & + \beta_5 (y^{ela}_{i,g-1,t-1})^2 + \beta_6 (y^{ela}_{i,g-1,t-1})^3 + X_i' \beta_7 + \theta^s_{k,g,t} + \epsilon^s_{i,k,g,t}. \end{aligned} \tag{30}$$

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. X_i variables are indicators for male, Black, Hispanic, English-language learners, special education students, and free or reduced price lunch students. We use the estimated school-grade-year fixed effects and take their mean (across grades) as our value-added measures.

F.B Estimating Private School Value-Added

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

$$\bar{y}^s_{j,8,t} = \alpha \bar{y}^s_{j,4,t-4} + \mu^s_t + \theta^s_j + \epsilon^s_{j,g,t} \tag{31}$$

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.[4] Our estimates thus average over multiple years.

G Appendix: Model Simplifications

G.A No Capacity Constraints

In our empirical analysis, we assume that no schools face capacity constraints. While this assumption likely does not hold for all schools, aggregate enrollments in NYC are declining during much of the sample period, so on average schools' capacity constraints are likely to be loosened. Collecting data on individual schools' capacity constraints, however, can

[4] Our 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.

be a challenge. We do not know of any data on private schools' capacity constraints. For public schools, we have some limited data on school capacities from NYC's 2005-06 and 2007-08 "Enrollment – Capacity – Utilization" reports. These reports use a building's room configurations and a formula for the number of students per room to calculate a building's capacity.

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

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

Private schools' exit decisions should not be sensitive to capacity constraints because constraints only bind when demand hits an upper bound while exit depends on demand hitting a lower bound. But the estimation of the direct and indirect effects could be sensitive to the presence of capacity constraints. If a school is capacity constrained, then we are likely to underestimate its δ_j in our demand model.[5] 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.[6]

[5]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 are loose descriptions of first-order effects.

[6]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%.

G.B Private Schools' Characteristics Held Fixed

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

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

G.C 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.[7] 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.

[7] 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.

H Appendix: Alternate Specifications

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

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

Our difference-in-difference framework should control for time-invariant heterogeneity, but in understanding the reform's effects, we might be interested in which private schools are most exposed to additional public school funding. An observation in this analysis is a private school. We estimate its reform exposure as the average projected funding increase ($1,000s per student) at the five closest public schools ("Mean FSF * Dist1to5"). We present the results in Appendix Table A9. We find higher exposure among Catholic schools, schools with high student-teacher ratios, lower tuition, and higher fractions of Hispanic students.

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

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

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

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

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

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

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

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

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A Appendix Figures

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

Figure A.1a: School that Gets Additional Funding

School: P.S. 189 Lincoln Terrace

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

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

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

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

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

School: P.S. 189 Lincoln Terrace

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

Figure A.3: Locations of Public Schools

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

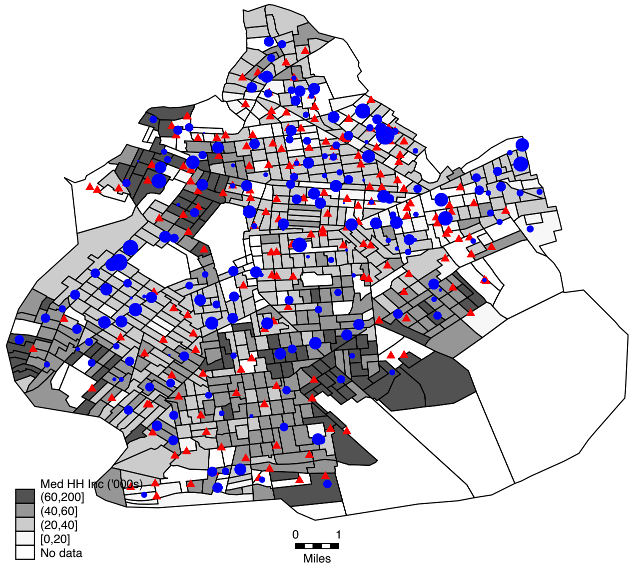
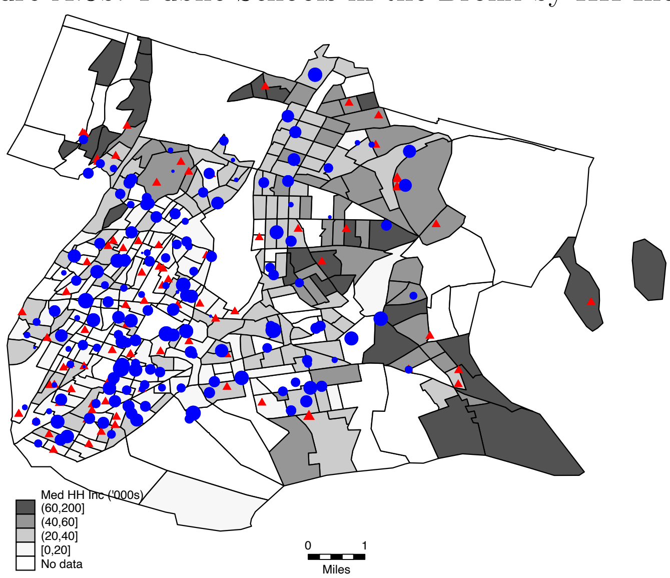


Figure A.3b: Public Schools in the Bronx by HH Income



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

Figure A.4: Locations of Private Schools

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

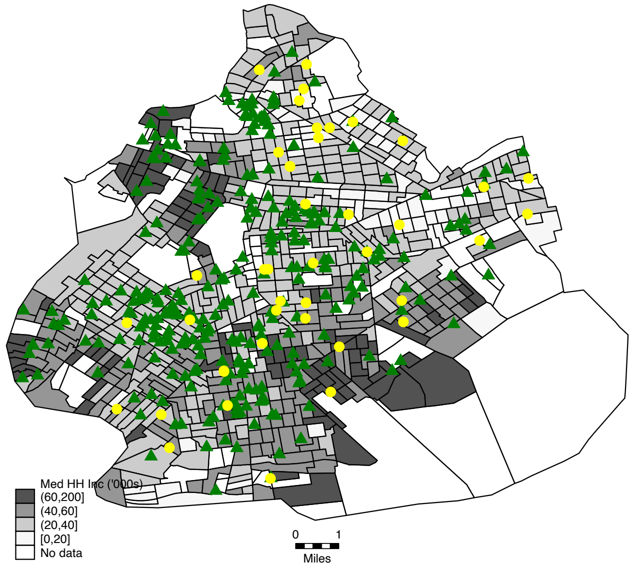
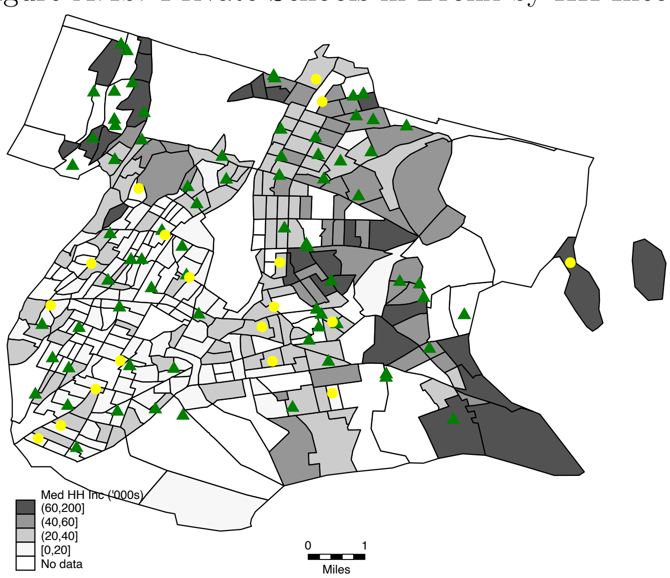
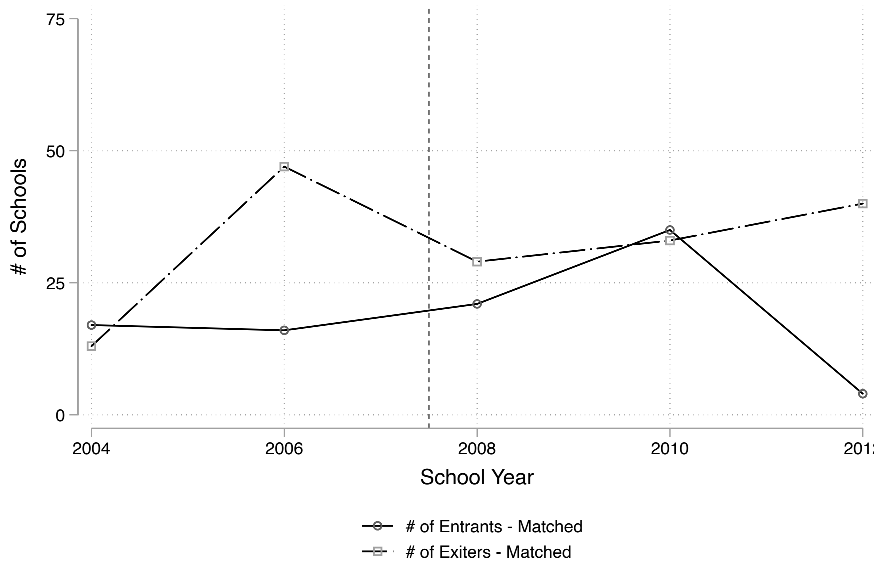


Figure A.4b: Private Schools in Bronx by HH Income



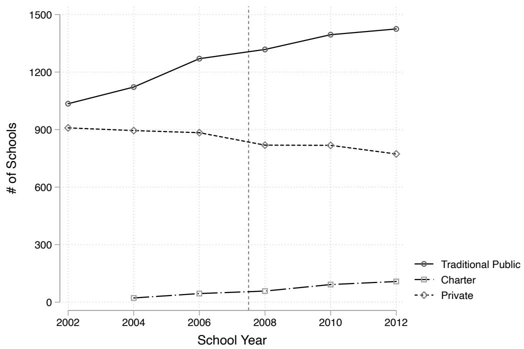
Note: U.S. Census tracts are shaded according to 2000 Census median income for households with children. The green triangles represent private schools that were open in 2006-07 that did not close in the next six years and the yellow circles are schools that did close in the next six years.

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

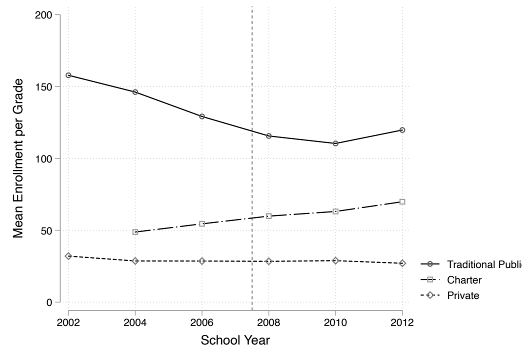


Note: The sample is the set of private schools in our estimation sample. These schools are in the Private School Survey (PSS) and match uniquely to the NYSED data. The PSS data come out every other year, so entry and exit refer to actions taken over two-year periods. The red line marks the implementation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

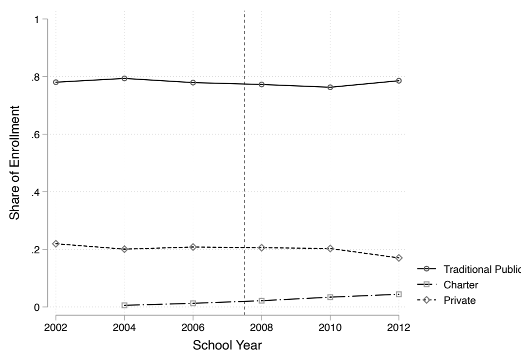
Figure A.6: Number of Schools, Mean Enrollment, and Market Share by Sector



(a) Number of Schools



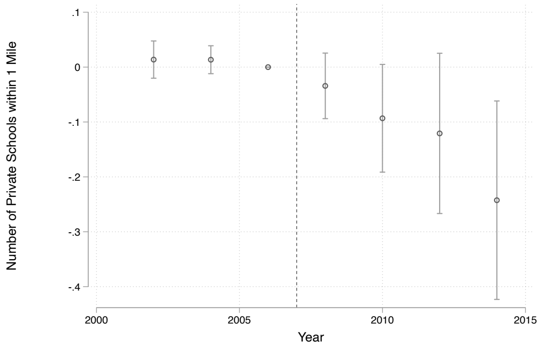
(b) Mean Enrollment per Grade



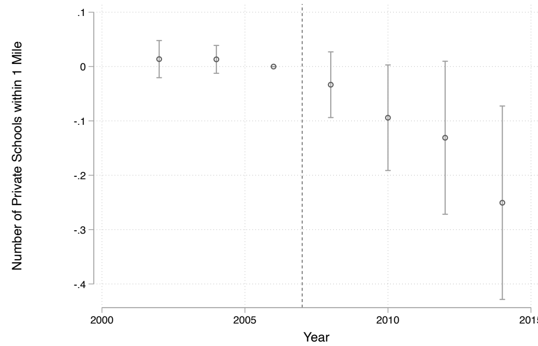
(c) Enrollment Share

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

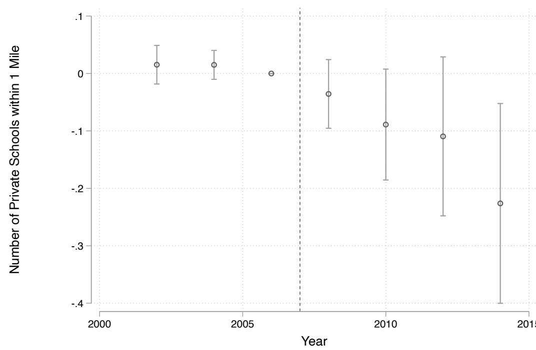
Figure A.7: Supply of Schools Event Studies – Robustness



(a) Strict Match



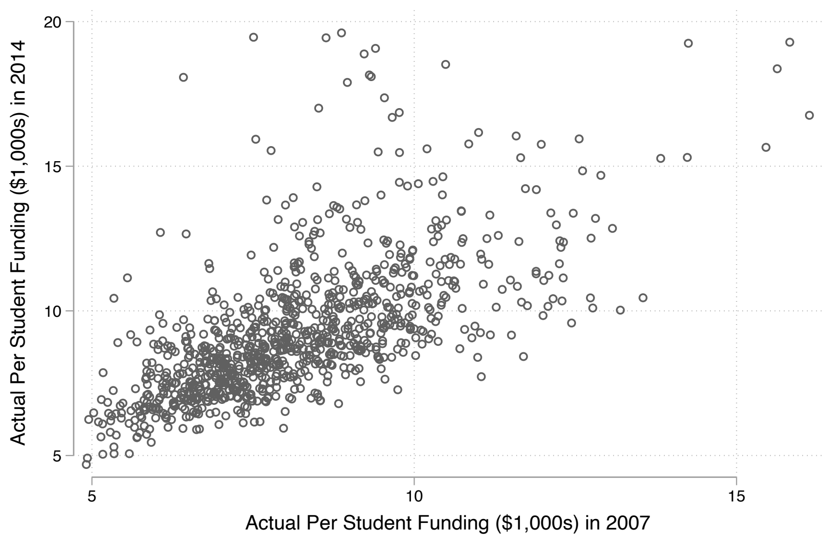
(b) Strict Match, Keep Only-Kindergartens



(c) Looser Match

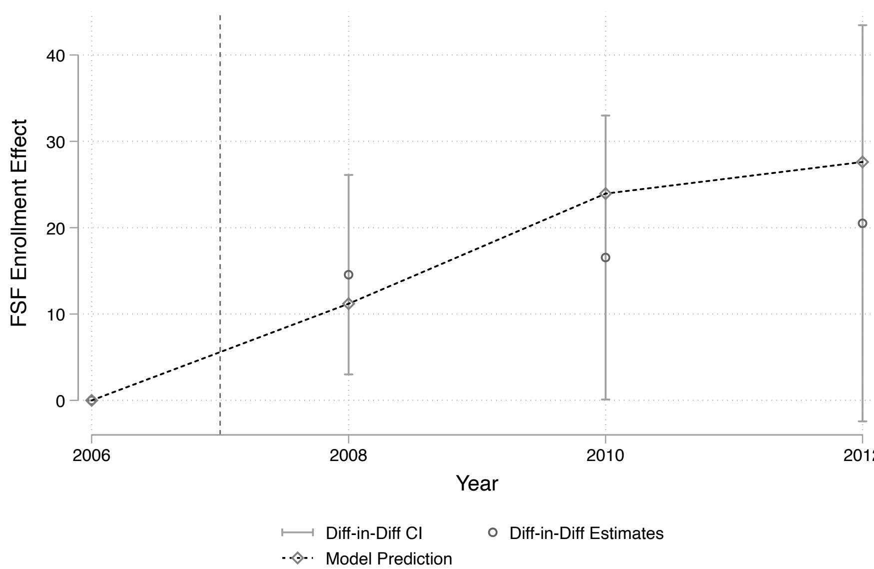
Note: Figure shows estimated coefficients (and 95% confidence intervals) on projected FSF funding ($1,000s/student) for each two-year period from the difference-in-difference regression of number of private (or charter) schools within 1 mile of a public school on projected FSF funding, school fixed effects,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

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



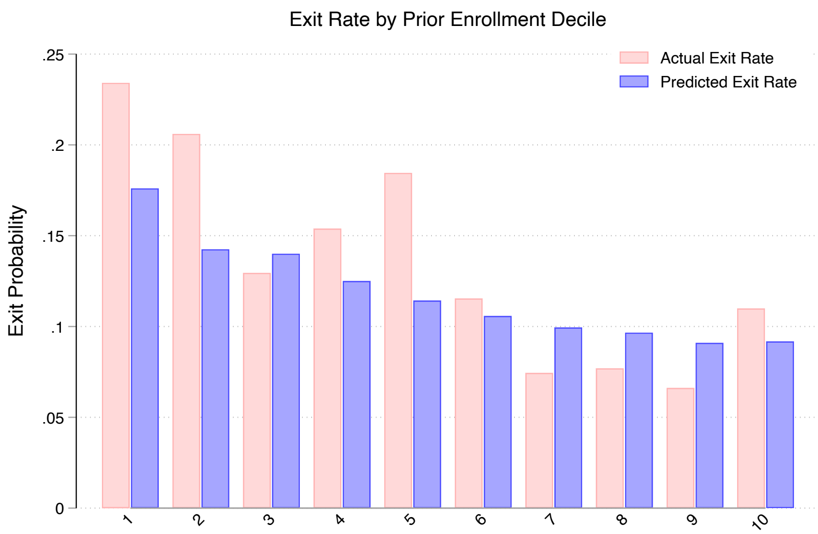
Note: Figure plots a school's per student funding in 2007 against its per student funding in 2014. While some schools reorder across years, there is a high level of persistence.

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



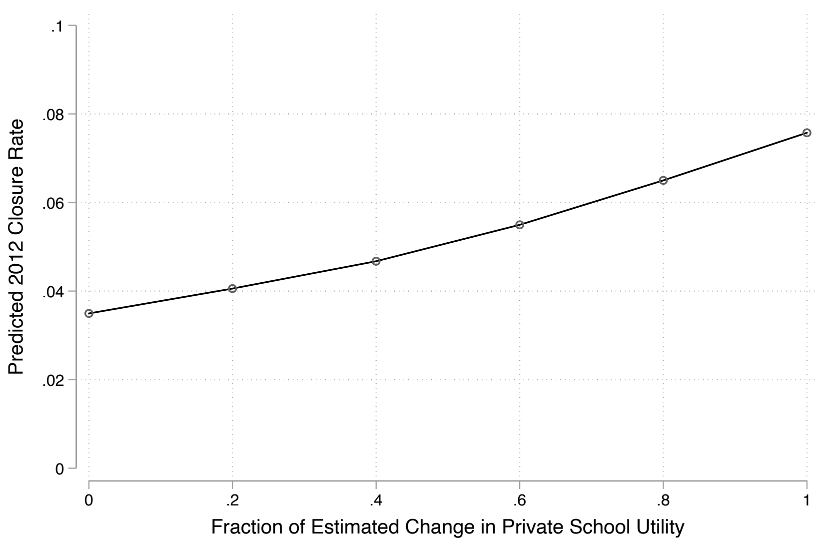
Note: Figure depicts the model prediction for the impact of the FSF reform on the enrollment of "winners" relative to "losers." With the model, we predict enrollments with and without the reform, where the reform affects both student utility from attending schools with increased projected funding and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

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



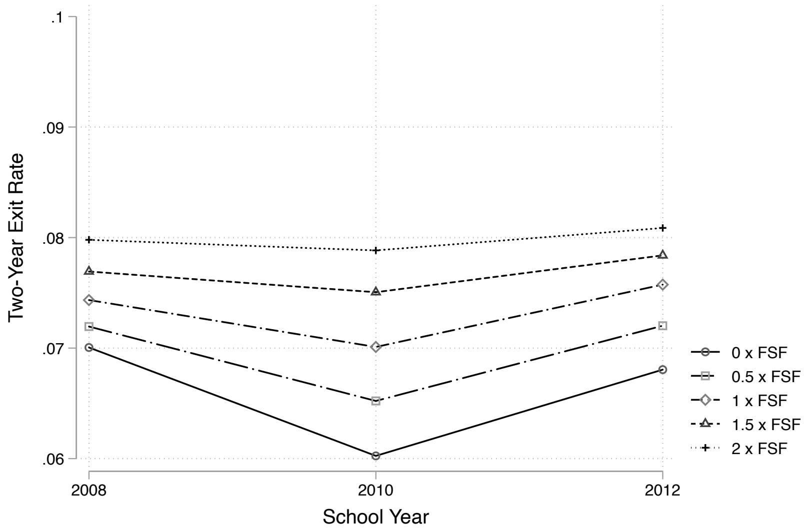
Note: Figure compares the model-predicted and actual two-year closure rates for private schools depending on the decile of their enrollment two years prior. Decile 1 includes the schools in the bottom 10% of enrollment two years prior.

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



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

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



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

B Appendix Tables

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

		Regressions	
	Mean	1(Projected Funding Change > 0)	Projected Funding Change per Student
% 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

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

Table A2: Traditional Public, Charter, and Private School K-8 Summary Statistics – 2006 School Year

	Traditional Public Schools	Charter Schools	Private Schools				
School Characteristics	Common Core	Common Core	PSS	NYSED	Estimation Sample (PSS and NYSED Match)	Entrants (2005-2006)	Exiters (2007-2008)
Number of Schools	1,050	44	688	693	584	15	28
% Catholic			39%	39%	43%	29%	62%
% Other Religious			44%	40%	40%	57%	33%
% Non-Religious			17%	21%	17%	14%	5%
Enrollment per Grade (Mean)	127.68	54.48	31.40	32.10	32.43	21.69	14.04
% with Enrollment per Grade < 10	0%	0%	13%	12%	9%	14%	19%
% with Enrollment per Grade < 20	1%	0%	34%	35%	30%	57%	86%
% Black	32%	68%	18%		19%	26%	33%
% Hispanic	40%	26%	15%		17%	28%	25%
% of Schools with > 50% Minority	75%	98%	42%		43%	71%	67%

Traditional public and charter school data come from the Common Core of Data. Private school data comes from the Private School Survey (PSS) and New York State Education Department (NYSED) data. The fifth column is our estimation sample and only includes the PSS elementary and middle schools that we can match uniquely to NYSED data on private schools. The sixth column includes schools in our estimation sample that enter in 2005 or 2006. The seventh column includes schools in our estimation sample that exit in 2007 or 2008. These entrants and exiters were all active in 2006. Minority students are Black or Hispanic.

Table A3: Matching between the PSS and NYSED

Panel A: Private School Survey	Matches	Non-Matches		
Number of Schools (Strict)	816	186		
Number of Schools (Loose)	847	155		
Enrollment (Mean)	297	267		
Number of Teachers (Mean)	25	23		
Catholic	43%	15%		
Other Religious	40%	63%		
Non-Religious	18%	22%		
Panel B: New York State Education Dept Data				
	Matches	Non-Matches		
Number of Schools (Strict)	741	188		
Number of Schools (Loose)	846	83		
Enrollment (Mean)	340	262		
Catholic	39%	14%		
Jewish	20%	37%		
Other Religious	17%	11%		
Non-Religious	24%	39%		
Entrant (First Year After 2001)	8%	25%		
Exiter (Last Year Before 2014)	17%	35%		
The Bronx	13%	11%		
Brooklyn	37%	51%		
Manhattan	21%	13%		
Queens	23%	18%		
Staten Island	7%	7%		
Panel C: Data Differences				
	Mean	Median	5th Percentile	95th Percentile
Difference in Enrollment per Grade	-0.21	0.00	-4.25	3.23

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

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

	2002	2004	2006	2008	2010	2012
Number of Schools						
Traditional Public	1,035	1,122	1,270	1,318	1,395	1,425
Charter	0	22	45	58	92	108
Private	909	895	884	819	818	773
Mean Enrollment per Grade						
Traditional Public	158	146	129	116	110	120
Charter	0	49	54	60	63	70
Private	32	29	29	28	29	27
Share of Enrollment						
Traditional Public	0.78	0.79	0.78	0.77	0.76	0.79
Charter	0.00	0.01	0.01	0.02	0.03	0.04
Private	0.22	0.20	0.21	0.21	0.20	0.17

The table shows how number of schools, mean enrollment per grade, and enrollment share change over time, by sector. The sample is all schools in the Common Core (traditional public and charter) and the Private School Survey (private) with students in grades K-8. We include even schools that we cannot match to the NYSED private school data to avoid understating private shares.

Table A5: Expenditure and School Characteristics Regressions

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

	Teachers	Other Classroom Instruction	Instructional Support Services	Administrators	Other Direct Services	Field Support	System-Wide Costs
FSF	0.557	0.170	-0.189	0.095	0.119	0.007	-0.002
	(0.098)	(0.045)	(0.087)	(0.044)	(0.093)	(0.008)	(0.002)
Category's Fraction of Expenditure in 2006-07	0.357	0.104	0.120	0.083	0.165	0.019	0.171
Year, School Fixed Effects	Y	Y	Y	Y	Y	Y	Y
N	8,759	8,759	8,759	8,759	8,759	8,759	8,759
R-Squared	0.447	0.189	0.252	0.217	0.192	0.130	0.994

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

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

	Number of Teachers	Number of Non-Teachers	% Teachers < 3 Years Experience	Mean Class Size Grades 1-6	Mean Class Size Grade 8	Annual Salary	Tenure at the School	Tenure in the District
FSF	4.343	0.482	-0.083	-0.120	-0.915	830.214	0.198	0.440
	(1.035)	(0.916)	(0.015)	(0.479)	(0.877)	(402.348)	(0.115)	(0.181)
Dep Var Mean in 2006-07	106.0	17.0	0.12	21.6	21.2	62,647.0	5.0	8.0
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	5,812	5,812	5,812	4,759	2,123	6,812	6,812	6,812
R-Squared	0.133	0.628	0.458	0.055	0.027	0.823	0.598	0.504

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

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

	Enroll	ln(Enroll)
FSF	36.943	0.160
	(23.594)	(0.041)
Fixed Effects	Year, School	Year, School
N	10,379	10,379
R-Squared	0.115	0.090

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

Table A7: Effect of Funding on Net Switchers by Sector

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

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

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

	Number Private Schools within 1 Mile	Number Private Schools within 1 Mile	Number Private High Schools within 1 Mile	Number Private Non-High Schools within 1 Mile	Number Charter Schools within 1 Mile	Number Charter Schools within 1 Mile
FSF	-0.208	-0.235			-0.092	-0.072
	(0.096)	(0.097)			(0.355)	(0.371)
Hyp Neg FSF		0.041				-0.025
		(0.046)				(0.109)
Mismatched FSF			0.011	-0.072		
			(0.093)	(0.179)		
(Year==2001)	0.306	0.287	-0.094	0.351		
	(0.090)	(0.091)	(0.069)	(0.204)		
(Year==2002)	0.326	0.307	-0.072	0.364		
	(0.092)	(0.093)	(0.066)	(0.206)		
(Year==2003)	0.391	0.372	-0.029	0.430		
	(0.094)	(0.095)	(0.066)	(0.204)		
(Year==2004)	0.460	0.441	0.035	0.532	0.221	0.220
	(0.095)	(0.096)	(0.055)	(0.187)	(0.179)	(0.179)
(Year==2005)	0.476	0.457	0.061	0.537	0.461	0.461
	(0.094)	(0.095)	(0.062)	(0.186)	(0.216)	(0.216)
(Year==2006)	0.504	0.485	0.106	0.576	0.912	0.912
	(0.088)	(0.090)	(0.059)	(0.183)	(0.298)	(0.298)
(Year==2007)	0.451	0.432	0.101	0.523	1.053	1.053
	(0.082)	(0.084)	(0.051)	(0.173)	(0.315)	(0.314)
(Year==2008)	0.508	0.505	0.133	0.549	1.107	1.095
	(0.078)	(0.078)	(0.052)	(0.162)	(0.307)	(0.303)
(Year==2009)	0.469	0.469	0.137	0.503	1.570	1.556
	(0.067)	(0.067)	(0.045)	(0.129)	(0.412)	(0.406)
(Year==2010)	0.428	0.428	0.159	0.428	1.881	1.867
	(0.060)	(0.060)	(0.037)	(0.126)	(0.391)	(0.384)
(Year==2011)	0.428	0.428	0.202	0.424	2.208	2.194
	(0.055)	(0.055)	(0.035)	(0.083)	(0.393)	(0.384)
(Year==2012)	0.248	0.248	0.131	0.262	2.319	2.306
	(0.040)	(0.040)	(0.023)	(0.057)	(0.420)	(0.410)
(Year==2013)	0.185	0.185	0.100	0.217	2.619	2.606
	(0.040)	(0.040)	(0.020)	(0.052)	(0.410)	(0.399)
(Year==2014)					2.845	2.831
					(0.440)	(0.429)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,456	11,456	5,442	3,159	3,050	3,050
R-Squared	0.073	0.073	0.048	0.069	0.471	0.471

Data span the 2001-02 through 2013-14 school years. An observation is a public school-school year. "FSF" is the projected per-student funding change (in $1,000s). "Hyp Neg FSF" is the FSF change in the absence of the Hold Harmless clause. For the "Mismatched FSF" regressions, an observation is a public school-school year where the public school is of the opposite level (HS or non-HS) from the private schools counted on the left-hand-side. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey and NY State private school registration data. Charter school counts are determined by presence in the Common Core of Data. Standard errors are clustered at the zip code.

Table A9: Relationship of Private School Characteristics with FSF Exposure

	1(Religious)	1(Catholic)	Student-Teacher Ratio	Enrollment	Fraction Black	Fraction Hispanic	Tuition	Income	Assets	Mean ELA Grade 4 Scores	Mean Math Grade 4 Scores	Mean ELA Grade 8 Scores	Mean Math Grade 8 Scores
Mean FSF * Dist1to5	0.295	0.599	8.234	-38.797	0.079	0.390	-15.444	-66,420.030	-120,770.352	-0.221	-0.034	-0.253	-0.134
	(0.180)	(0.194)	(2.597)	(109.610)	(0.090)	(0.075)	(8.190)	(34,984.491)	(69,175.190)	(0.198)	(0.178)	(0.211)	(0.179)
Constant	0.790	0.354	15.014	360.664	0.213	0.116	11.382	21,019.621	37,196.219	0.323	-0.062	0.411	0.202
	(0.065)	(0.061)	(0.907)	(28.463)	(0.028)	(0.018)	(3.024)	(8,436.808)	(17,010.126)	(0.048)	(0.038)	(0.052)	(0.055)
Distance Radius (miles)	3	3	3	3	3	3	3	3	3	3	3	3	3
Max Number of Matches	5	5	5	10	5	5	5	5	10	5	5	5	5
Public School Controls	No	No	No	No	No	No	No	No	No	No	No	No	No
N	577	577	577	577	577	577	267	76	76	209	207	176	171

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

Table A10: Supply Regressions – Robustness to Matching

	Strict Match			Strict Match, Keep Only-Kindergartens			Looser Match		
	All	Drop Schools with Year Mismatch	Drop Schools with Year or Enroll Mismatch	All	Drop Schools with Year Mismatch	Drop Schools with Year or Enroll Mismatch	All	Drop Schools with Year Mismatch	Drop Schools with Year or Enroll Mismatch
FSF	-0.208	-0.135	-0.121	-0.190	-0.129	-0.126	-0.168	-0.094	-0.115
	(0.096)	(0.054)	(0.057)	(0.102)	(0.053)	(0.057)	(0.106)	(0.057)	(0.056)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,456	10,165	9,346	11,842	10,135	9,236	11,810	10,244	9,417
R-Squared	0.073	0.221	0.201	0.076	0.221	0.195	0.089	0.235	0.198

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

Table A11: Market-Level Regressions

	Market: Zip Code					
	Priv #	Public Enroll	Private Enroll	Priv #	Public Enroll	Private Enroll
Total FSF	-0.136	140.211	-17.613			
	(0.249)	(185.280)	(111.414)			
Mean FSF				-0.114	71.332	-15.004
				(0.243)	(192.096)	(107.106)
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	1,813	1,813	1,813	1,813	1,813	1,813
R-Squared	0.078	0.063	0.083	0.078	0.063	0.083

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

Table A12: IV Regressions

	Per Student Budget ($1000s)	Enroll	Number Private Schools within 1 Mile
FSF Projected Policy Change ($1000s/student)	0.593		
	(0.156)		
Budget ($1000s/student)		16.156	-0.271
		(28.723)	(0.193)
Fixed Effects	Year, School	Year, School	Year, School
N	7,827	7,827	6,503

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

Table A13: Poisson Regressions

	Number Private Schools within 1 Mile	Number Private Schools within 1 Mile	Number Private High Schools within 1 Mile	Number Private Non-High Schools within 1 Mile	Number Charter Schools within 1 Mile	Number Charter Schools within 1 Mile
FSF	-0.045	-0.050			-0.068	-0.065
	(0.027)	(0.028)			(0.097)	(0.104)
Hyp Neg FSF		0.007				-0.004
		(0.011)				(0.030)
Mismatched FSF			-0.001	-0.030		
			(0.032)	(0.052)		
Fixed Effects	Year, School	Year, School	Year, School	Year, School	Year, School	Year, School
N	11,455	11,455	5,442	3,159	3,024	3,024

Data span the 2001-02 through 2013-14 school years. An observation is a public school-school year. Estimates are coefficients from Poisson models. "FSF" is the projected per-student funding change (in $1,000s). "Hyp Neg FSF" is the FSF change in the absence of the Hold Harmless clause. For the "Mismatched FSF" regressions, an obesrvation is a public school-school year where the public school is of the opposite level (HS or non-HS) from the private schools counted on the left-hand-side. Budget-based regressors are constructed using NYC DOE data on 2007-08 school budgets. Private school counts are determined by a school's presence in the Private School Survey and NY State private school registration data. Charter school counts are determined by presence in the Common Core of Data. Standard errors are clustered at the zip code.

Table A14: Private School Exit Regressions

	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)	1(Exit)
Mean FSF * Dist1to5	0.251	0.286		0.569				
	(0.110)	(0.146)		(0.277)				
FSF * Dist1			0.982					
			(0.360)					
FSF * Dist2			-0.298					
			(0.351)					
FSF * Dist3			0.162					
			(0.306)					
FSF * Dist4			0.024					
			(0.308)					
FSF * Dist5			0.335					
			(0.317)					
Mean FSF * Dist6to10				0.207				
				(0.273)				
Mean FSF					0.779	0.748	0.047	0.203
					(0.265)	(0.262)	(0.239)	(0.160)
Mean FSF * Distance (miles)					-0.626	-0.656	-0.027	-0.149
					(0.268)	(0.275)	(0.103)	(0.178)
Mean Hyp. Neg. FSF						0.026		
						(0.102)		
Mean Hyp. Neg. FSF * Distance (miles)						0.018		
						(0.052)		
Years	2006-14	2006-14	2006-14	2006-14	2006-14	2006-14	2006-14	2002-05
Mismatched Levels							x	
Distance Radius (miles)	3	3	3	3	3	3	3	3
Max Number of Matches	5	5	5	10	5	5	5	5
Public School Controls		Yes		Yes	Yes	Yes	Yes	Yes
N	487	487	487	487	487	487	368	268

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

Table A15: Private and Charter Enrollment Regressions

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

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

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

	Standard Error	Number of Clusters
Cluster by Zip Code	(0.096)	161
Cluster by Subdistrict	(0.081)	32
Cluster by School	(0.092)	838
Wild Bootstrap, Cluster by Zip Code	(0.095)	161
Wild Bootstrap, Cluster by Subdistrict	(0.078)	32
Wild Bootstrap, Cluster by School	(0.091)	838
Robust	(0.092)	

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

Table A17: Private School Characteristics Regressions

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

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

Table A18: Regressions of Private School Enrollment on Zip Enrollment

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

An observation is a private school - year. Data covers 2000-2001 and later. Other enrollment is the total enrollment among all other private schools in the same zip code.

Table A19: Demand Estimates – Odd Grades and Variations of Demand Model

Demand Parameters	Grade 1	Grade 3	Grade 5	Grade 7
Baseline				
γ	0.768	0.676	0.624	0.564
	(0.008)	(0.008)	(0.002)	(0.006)
ρ	4.118	4.070	3.846	2.839
	(0.001)	(0.002)	(0.001)	(0.001)
λ	0.158	0.244	0.072	0.041
	(0.077)	(0.075)	(0.059)	(0.052)
σ	0.852	1.207	1.765	0.670
	(0.075)	(0.033)	(0.093)	(0.376)
τ_1	-0.002	-0.001	-0.003	-0.063
	(0.052)	(0.050)	(0.076)	(0.066)
τ_2	-0.001	0.000	-0.006	-0.006
	(0.055)	(0.052)	(0.077)	(0.073)
τ_3	-0.053	0.060	-0.019	0.022
	(0.061)	(0.060)	(0.082)	(0.077)
τ_4	-0.078	0.026	-0.099	0.011
	(0.068)	(0.069)	(0.086)	(0.086)
τ_5	-0.264	-0.196	-0.329	-0.165
	(0.066)	(0.069)	(0.079)	(0.099)
Omitting 2012				
γ	0.762	0.666	0.629	0.567
ρ	4.099	4.033	3.875	2.770
λ	0.123	0.300	0.433	0.103
σ	0.890	0.083	3.682	1.016
τ_1	0.002	0.000	0.020	-0.087
τ_2	-0.001	-0.006	-0.055	0.000
τ_3	-0.052	0.074	0.065	0.053
τ_4	-0.079	0.070	-0.015	0.001
No Idiosyncratic Private School Preference				
γ	0.763	0.670	0.618	0.563
ρ	4.105	4.048	3.822	2.836
λ	0.136	0.251	-0.049	0.030
τ_1	0.008	0.003	0.013	-0.059
τ_2	-0.005	0.006	-0.010	-0.001
τ_3	-0.030	0.076	-0.064	0.027
τ_4	-0.076	0.052	-0.106	0.012
τ_5	-0.234	-0.113	-0.272	-0.149
No Private School Year Shocks				
γ	0.763	0.670	0.618	0.564
ρ	4.115	4.049	3.823	2.835
λ	0.380	0.249	0.231	0.075
σ	0.396	0.002	0.372	0.479

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

Table A20: Fit of Demand Model

	Enrollment (Data)
Enrollment (Model Prediction)	0.680
	(0.012)
Fixed Effects	School, Private*Year
N	10,761
Number of Schools	2,519
Within R-Squared	0.418

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

Table A21: Demand Counterfactuals – 2008 and 2010

Table A.21a: Demand Counterfactual – 2008

Panel A: 2008 Demand Counterfactual

Initial School Category	Final School Category: Winners	Losers	Other Public	Private Non-Closers	Private Entrants/Exiters	School Count
Winners	-	0	0	0	0	495
Losers	1608	-	0	0	0	519
Other Public	143	0	-	0	0	64
Private Non-Closers	2141	0	0	-	0	697
Private Entrants/Exiters Direct	154	0	0	0	-	105
Private Entrants/Exiters Indirect	817	989	50	2497	-	

Table A.21b: Demand Counterfactual – 2010

Panel B: 2010 Demand Counterfactual

Initial School Category	Final School Category: Winners	Losers	Other Public	Private Non-Closers	Private Entrants/Exiters	School Count
Winners	-	0	0	0	0	495
Losers	3092	-	0	0	0	519
Other Public	273	0	-	0	0	64
Private Non-Closers	3776	0	0	-	0	612
Private Entrants/Exiters Direct	638	0	0	0	-	190
Private Entrants/Exiters Indirect	4634	3918	225	5752	-	

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