Education Markets I

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Economics of Education, Spring 2026

Motivation

Connection to Previous Lecture

- Last week: schools and teachers vary *substantially* in quality
- We can measure that variation using value-added models
- One natural followup question: what role can policy and markets play in improving the allocative efficiency of students to schools?
  - → Does *competition* raise quality?
  - → Do parents *value* school quality?
  - → Do they have the *information* to choose well?
- Today: the economics of school choice — theory and evidence

Friedman (1955): The Role of Government in Education

- Influential essay motivating school choice reforms
- Core proposal: government should finance education but not administer it
  - → Parents receive vouchers redeemable at any approved school, public or private
  - → Voucher amount set at the estimated per-pupil cost of public schooling
- Two distinct arguments for why this would improve outcomes:
  - → Supply-side: Competition forces schools to improve or lose students
  - → Demand-side: Choice lets families find their best match

Friedman (1955): Two Arguments for Vouchers

- Argument 1 — Competition improves quality:
  - → Public schools face no competitive pressure under neighborhood assignment
  - → Vouchers create a market: schools that fail to attract students lose funding
- Argument 2 — Choice improves match:
  - → Families differ in their preferences and children differ in their needs
  - → Neighborhood assignment forces a one-size-fits-all model
  - → Markets allow specialization and sorting to comparative advantage
- How far can eliminating attendance zone boundaries get us? Can government both *finance* and *administer* while allowing for the same mechanisms Friedman argues for?

Chubb & Moe (1990): The Institutional Case

- *Politics, Markets, and America's Schools* — book that sharply influenced the modern school choice reforms
- Argument comes more from institutional political science, not economics:
  - → Democratic governance of schools generates *bureaucratic capture*
  - → Interest groups (unions, administrators, politicians) dominate school policy
  - → The result: schools organized around adult interests, not student learning
- Markets are the solution: choice breaks the link between governance and geography
- Key empirical claim: school *organization* (autonomy, leadership, mission clarity) matters more than inputs — and democratic governance destroys effective organization (based on correlational evidence)

Rough Taxonomy of Choice Policies in the US

- School choice policies in the United States and abroad are wide-ranging
- Private schools: state policies that subsidize private schooling or other approved educational expenses through vouchers, ESAs, tax-credit scholarships, tax-credit ESAs, and tax credits/deductions
  - → Vouchers: Milwaukee, DC, Louisiana, Colombia, Chile, Sweden
  - → Statewide ESAs in Texas and Louisiana; universal voucher in Indiana
- Charter schools: Publicly funded, privately operated
  - → Authorized by local authorities but mostly independent, subject to state accountability
- Magnet schools: Specialized public schools with selective or lottery-based admissions, originating as a tool for voluntary integration in the 1980s
  - → Often themed: STEM, arts, language immersion
- Intra-district open enrollment: Families rank public schools within a district
  - → NYC, Boston, Denver

Some Facts

- U.S. voucher programs: ∼65 small-scale programs at time of Epple, Romano & Urquiola (2017) review of the literature
  - → Tax-revenue-funded: Milwaukee, Cleveland, DC, Louisiana
  - → Tax-credit-funded: Florida FTC (∼51,000 students)
  - → As of 2021, roughly 9% of total K-12 enrollment
- International voucher programs:
  - → Chile: 47% private enrollment by 2009
  - → Holland: 70% private enrollment
  - → Sweden: ∼10% private enrollment
- U.S. charter schools: As of 2021, roughly 6.6% of public school enrollment
- Magnet schools: As of 2021, roughly 6.5% of public school enrollment
- Charter and magnet school enrollment has grown rapidly; private school enrollment has been relatively flat for decades but will start growing in the coming years

Hoxby (2003)

- Hoxby (2003) draws emphasis on two distinct channels studied in the literature
- Channel 1: Allocation / Redistribution Effect (direct participant effect)
  - → Choice reallocates students across schools, changing the distribution of peers and resources
  - → This can be zero-sum — some students gain better peers at the expense of others
  - → Most of the early literature focused exclusively on this channel
- Channel 2: Productivity / Competitive Effect (market-level effect)
  - → Competition creates pressure that induces *all* schools to raise productivity
  - → Achievement per dollar spent — not just achievement levels
  - → If this channel dominates, choice is a "rising tide that lifts all boats"
- The key point: if productivity effects dominate, allocation gains/losses become less important

Hoxby (2003)

- The two-channel framework organizes the school choice literature:
  - → Participant effects (allocation): Does the student who *uses* choice benefit?
  - → Market effects (productivity): Do *all* schools improve under competition?
- Most RCTs and lottery-based studies estimate participant effects only
- Competitive effects are harder to identify — require market-level variation
- Crucially, both participant and market-level effects depend on underlying demand in the market
  - → If parents do not value school effectiveness or prioritize other factors, then school choice reforms may not work in practice
- What we will focus on next:
  - → What do we know about participant effects? (Vouchers, charters, magnets)
  - → Demand estimation — what do parents actually prioritize when choosing schools?
  - → Campos and Kearns (2024): Putting it all together in a case study in Los Angeles on market-level effects

Participant Effects

Randomized Admission Lotteries

- Some schools require students to apply for admission (e.g., charter schools, voucher schools, intra-district choice programs)
- Lotteries are usually used to break ties at oversubscribed schools
- The lottery variation provides useful variation to estimate causal effects
- The key to using lotteries is to understand the underlying assignment process i.e., students' *assignment risk*
- Some caveats to keep in mind:
  - → Lottery studies are restricted to *oversubscribed* schools that may be better due to popularity
  - → With heterogeneous effects, we have to ask: who selects into the lottery? Representative?
- Examples in the literature include
  - → Hoxby and Murarka 2009 and Abdulkadiroğlu et al. 2011 studying charter schools
  - → Abdulkadiroğlu et al. 2018 studying voucher schools
  - → Cullen et al. 2006 studying magnet programs
- Waiting for Superman

Randomized Admissions Lotteries

With a constant effects assumption, the lottery framework leverages random assignment Z_i to schools conditional on *risk set* R_i. This implies the instrument is unrelated to student ability, conditional on risk strata R_i :

$$E[Y_i|Z_i, R_i] = \alpha + \rho Z_i + \sum_{k=1}^{K} \delta_k R_{ik}$$

The instrument Z_i must predict enrollment (i.e., have a first stage with $\pi > 0$):

$$E[D_i|Z_i, R_i] = \mu + \pi Z_i + \sum_{k=1}^{K} \tau_k R_{ik}$$

Expressed differently, the two assumptions in a constant effects framework are:

1. Lottery offers are independent of student ability: $\varepsilon_i \perp\!\!\!\perp Z_i$
2. Lottery winners are more likely to enroll than losers: $E[D_i|Z = 1, R_i] > E[D_i|Z_i = 0, R_i]$

What do lotteries identify?

The conditional Wald estimand for applicants in risk set k is

$$\beta_{IV,k} \equiv \frac{E[Y_i|Z_i = 1, R_i = k] - E[Y_i|Z_i = 0, R_i = k]}{E[D_i|Z_i = 1, R_i = k] - E[D_i|Z_i = 0, R_i = k]}$$

and under the previous assumptions represents the causal effect of treatment for applicants in group k.

A two-stage least squares estimator with full set of risk set controls conveniently aggregates the set of $\beta_{IV,k}$ into a single-weighted average (Kolesar 2013):

$$\beta_{2SLS} = \sum_{k=1}^{K} \left[\frac{\omega_k \pi_k p_k (1 - p_k)}{\sum_l \omega_l \pi_l p_l (1 - p_l)} \right] \beta_{IV,k}$$

- $\omega_k = P(R_{ik} = 1)$: share of applicants in risk set k
- $\pi_k = E[D_i|Z_i = 1, R_{ik} = 1] - E[D_i|Z_i = 0, R_{ik} = 1]$: first-stage for applicants in risk set k
- $p_k(1 - p_k) = P(Z_i = 1|R_{ik} = 1)(1 - P(Z_i = 1|R_{ik} = 1))$: conditional offer variance

Heterogeneous Effects

- Constant effects assumption is perhaps too stringent but easily relaxed
  - → Angrist and Imbens 1994 and Kolesar 2013 imply

$$\beta_{2SLS} = \sum_{k=1}^{K} \left[\frac{\omega_k \pi_k p_k (1 - p_k)}{\sum_l \omega_l \pi_l p_l (1 - p_l)} \right] \beta_{LATE,k}$$

- In the LATE framework, we recover a weighted average of lottery-specific LATEs that are limited in informativeness due to families self-selecting into the lottery
- Some advantages of the LATE framework
  - → Characterization of compliers
  - → Characterization of complier fallback options
  - → Can recover complier potential outcome distributions

From LATEs to Complier Distributions

- The lottery IV estimand gives a local average treatment effect:

$$E[Y_i(1) - Y_i(0) \mid \mathcal{C}_i = 1], \qquad \mathcal{C}_i \equiv 1\{D_i(1) > D_i(0)\}$$

- Abadie (2002) shows that we can identify richer objects of interest
- With the same IV assumptions, we can identify treated and untreated *complier-specific* moments and distributions by replacing Y_i with a function $g(Y_i, X_i)$
- This allows us to move beyond average complier effects to distributional shifts

Abadie (2002): Key Result

Suppressing risk-set notation, under independence, exclusion, and monotonicity,

$$\frac{E[g(Y_i, X_i)D_i \mid Z_i = 1] - E[g(Y_i, X_i)D_i \mid Z_i = 0]}{E[D_i \mid Z_i = 1] - E[D_i \mid Z_i = 0]} = E[g(Y_i(1), X_i) \mid \mathcal{C}_i = 1]$$

$$\frac{E[g(Y_i, X_i)(1 - D_i) \mid Z_i = 0] - E[g(Y_i, X_i)(1 - D_i) \mid Z_i = 1]}{E[D_i \mid Z_i = 1] - E[D_i \mid Z_i = 0]} = E[g(Y_i(0), X_i) \mid \mathcal{C}_i = 1]$$

- Same Wald logic as before; only the object on the left-hand side changes
- The second expression just flips signs so the denominator stays positive

Choose $g(\cdot)$, Choose the Complier Object

- $g(Y_i, X_i) = Y_i$: treated and untreated complier means
- $g(Y_i, X_i) = 1\{Y_i \geq c\}$: probability a complier clears a policy-relevant threshold
- $g(Y_i, X_i) = 1\{Y_i \leq y\}$: complier CDF at point y
- $g(Y_i, X_i) = \frac{1}{h} K\left(\frac{Y_i - y}{h}\right)$: complier density at point y
- $g(Y_i, X_i) = C_{s(i)}$: average characteristics of schools attended by treated or untreated compliers

Let's keep all of these facts in mind, when assessing the following lottery-based studies

Vouchers, Charters, and Intra-District Choice

Vouchers

- Milwaukee Parental Choice Program: Rouse (1998)
- DC Opportunity Scholarship Program: Chingos (2018)
- Louisiana Scholarship Program: Abdulkadiroglu, Pathak and Walters (2018)

Charters

- Boston Charters: Abdulkadiroglu et al. (2011); Chabrier, Cohodes, and Oreopoulous (2016)
- Harlem Childrens Zone: Dobbie and Fryer (2011)

Intra-District Choice

- Magnets: Cullen et al. (2006); Bruhn et al. (2026)
- Open Enrollment: Deming et al. (2014)

Rouse (1998): The Milwaukee Parental Choice Program

- The first modern voucher program and study approximating lottery-based random assignment
- Milwaukee Parental Choice Program (MPCP), launched 1990
- Eligibility: families below 1.75× poverty line ($21K for a family of 3)
- Participating private schools were nonsectarian
- Compare lottery winners vs. lottery losers among oversubscribed schools
  - → Lotteries must be *imputed* from race × grade × year interactions
- An early lottery study but had many empirical problems: attrition, small sample, results sensitive

Rouse (1998): Findings

Lottery winners gain ∼1.5–1.6 NCE percentile points per year relative to losers; results driven by 1990 cohort

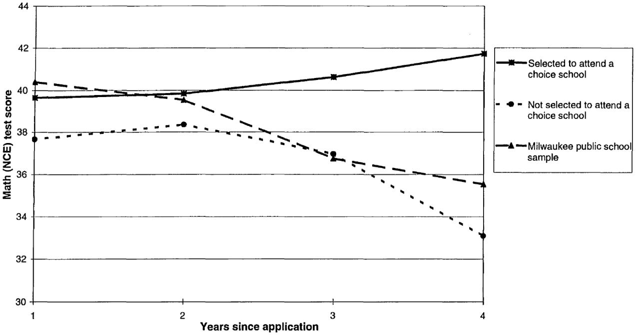


FIGURE I

Adjusted Math (NCE) Test Scores by Years Since Application to Choice Program, All Cohorts

Coefficient estimates from a regression of the math scores on dummy variables for years since application, years since application interacted with whether the student was selected to attend a choice school or whether the student was not selected to attend a choice school, whether the test score was imputed, and individual fixed-effects.

Rouse (1998): Findings

Lottery winners do not gain much in reading

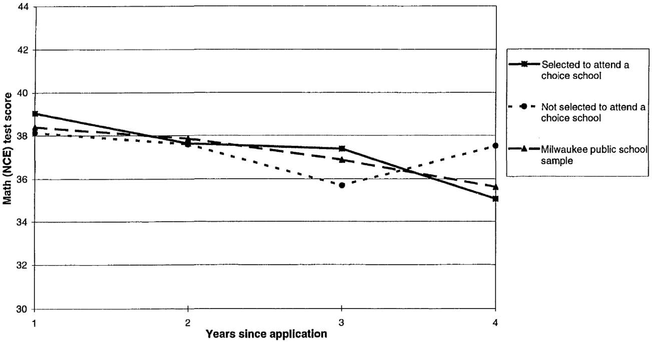


FIGURE II
Adjusted Reading (NCE) Test Scores by Years Since Application to Choice Program, All Cohorts

Coefficient estimates from a regression of the reading scores on dummy variables for years since application, years since application interacted with whether the student was selected to attend a choice school or whether the student was not selected to attend a choice school, and individual fixed-effects.

Chingos and Kisida (2022): The Effect of the DC School Voucher Program on College Enrollment

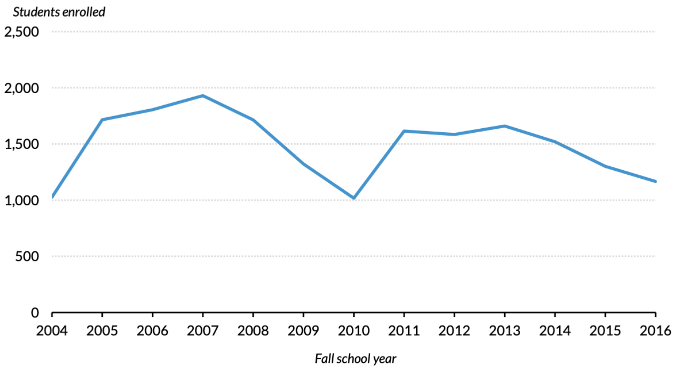
- DC OSP: A federally funded U.S. voucher experiment
- Earlier studies found positive impacts on high school graduation but relied on a selected sample of parent's self-reported outcomes
- Authors link college enrollment records with lottery records to experimentally assess college impacts
- Focus on first two cohorts: 2004 and 2005

Chingos and Kisida (2022): Findings

FIGURE 1

Enrollment in DC Opportunity Scholarship Program

2004–16



Chingos and Kisida (2022): Findings

TABLE 1

Descriptive Statistics

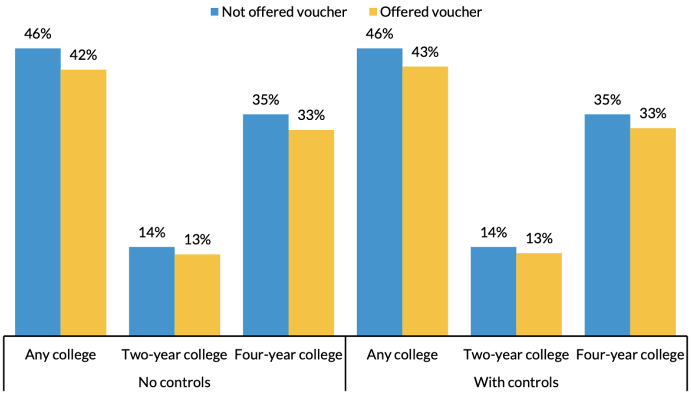
	Control	Treatment	Difference	*p*-value of difference
Race and ethnicity				
Black	88%	87%	-0.4%	0.82
Hispanic	8%	11%	2.7%	0.07
Not black or Hispanic	5%	3%	-2.2%	0.03
Female	51%	49%	-2.0%	0.43
Parents/guardians married	19%	19%	-0.3%	0.86
Parent/guardian owns home	14%	14%	0.7%	0.70
Age	12.3	12.4	0.1	0.54
Income missing	19%	18%	-0.1%	0.96
Family income	$18,640	$17,860	-$780	0.21
Charter at baseline	26%	26%	-0.3%	0.87
Observations (unweighted)	648	946		

Source: Author's calculations from OSP application data.

Chingos and Kisida (2022): Findings

FIGURE 3

Effect of OSP Voucher Offer on College Enrollment within Two Years of Expected High School Graduation



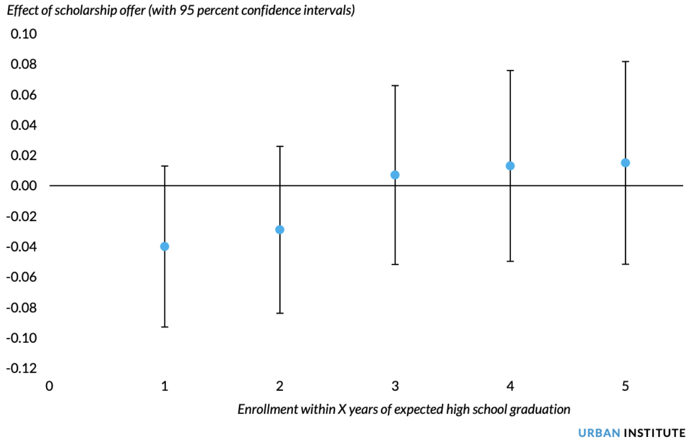
URBAN INSTITUTE

Source: Author's calculations from linked OSP and NSC data.

Chingos and Kisida (2022): Findings

FIGURE 4

Effect of OSP Voucher Offer on College Enrollment within 1–5 Years of High School Graduation



Source: Author's calculations from linked OSP and NSC data.

Abdulkadiroglu, Pathak, and Walters (2018): Can School Choice Reduce Achievement?

- Central argument for school choice: parents choose wisely
  - $\rightarrow$ Revealed preference logic — choosing $\Rightarrow$ better off
  - $\rightarrow$ Previous lottery studies for vouchers suggest weakly positive effects, if any at all

- Supply side largely ignored: which schools opt in to participate?
  - $\rightarrow$ Voucher programs require private schools to apply for eligibility
  - $\rightarrow$ If low-quality schools are more likely to enter $\Rightarrow$ adverse selection on the supply side

- Louisiana Scholarship Program (LSP): large-scale voucher program for low-income students at low-performing public schools
  - $\rightarrow$ Statewide expansion in 2012 $\Rightarrow$ first year with substantial oversubscription

Why Focus on Supply-Side Adverse Selection?

- The no-top-up rule: schools must accept the voucher as full tuition payment
- Average voucher amount: $5,311 vs. $8,605 public per-pupil expenditure
- This screens out high-quality, high-tuition private schools — only low-tuition schools participate
- LSP participating schools had:
  - → 15% lower tuition than non-participating private schools
  - → -12.4% enrollment decline in the decade before entering the program
  - → Compare: $+2.8\%$ enrollment growth at non-LSP private schools

APW (2018): Enrollment Trends at LSP vs. Non-LSP Schools

Schools entering the LSP were already in decline — falling enrollment, low tuition

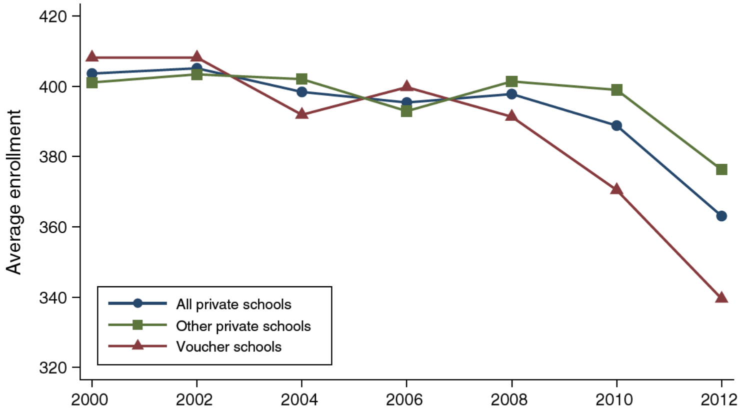


FIGURE 2. ENROLLMENT TRENDS IN LOUISIANA PRIVATE SCHOOLS

APW (2018): Findings

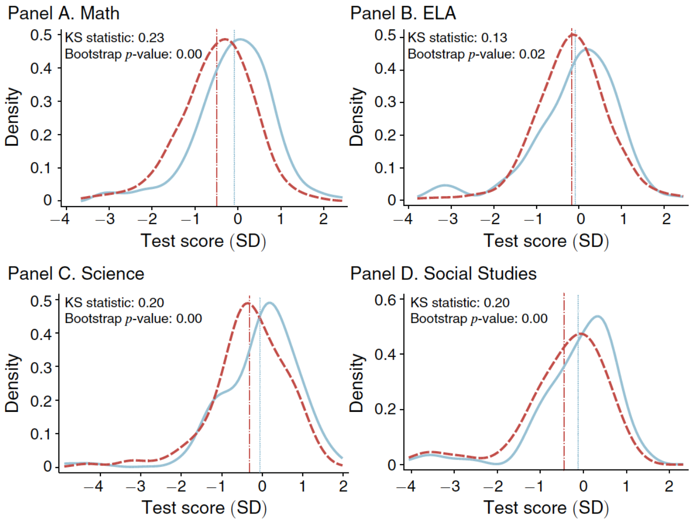
Sizable learning losses for lottery winners and compliers

TABLE 4—TWO-STAGE LEAST SQUARES ESTIMATES OF VOUCHER EFFECTS ON TEST SCORES

Subject	First stage (1)	Reduced form (2)	2SLS (3)	OLS (4)
Math	0.679	−0.281	−0.413	−0.386
	(0.029)	(0.061)	(0.091)	(0.066)
Observations		1,247		
ELA	0.679	−0.055	−0.081	−0.120
	(0.029)	(0.053)	(0.079)	(0.056)
Observations		1,248		
Science	0.689	−0.181	−0.263	−0.282
	(0.030)	(0.066)	(0.095)	(0.065)
Observations		1,221		
Social studies	0.690	−0.229	−0.331	−0.270
	(0.030)	(0.060)	(0.089)	(0.059)
Observations		1,220		

APW (2018): Findings

Notable leftward shift in treated complier distribution



APW (2018): Findings

Characteristics of Fallback Schools

TABLE 10—CHARACTERISTICS OF TREATMENT AND FALLBACK SCHOOLS FOR VOUCHER APPLICANTS

	All applicants		Voucher compliers	
	Offered (1)	Not offered (2)	Offered (3)	Not offered (4)
Voucher school	0.730	0.051	1.000	0.000
Charter school	0.044	0.140	0.000	0.141
Other public school	0.216	0.772	0.000	0.819
Unknown school type	0.010	0.037	0.000	0.040
Fraction Basic or above: math	0.540	0.590	0.436	0.611
ELA	0.561	0.586	0.497	0.565

APW (2018): Findings

Quality Differences Seem to Explain the Negative Gains

TABLE 11—VOUCHER EFFECTS BY MEASURES OF SCHOOL QUALITY

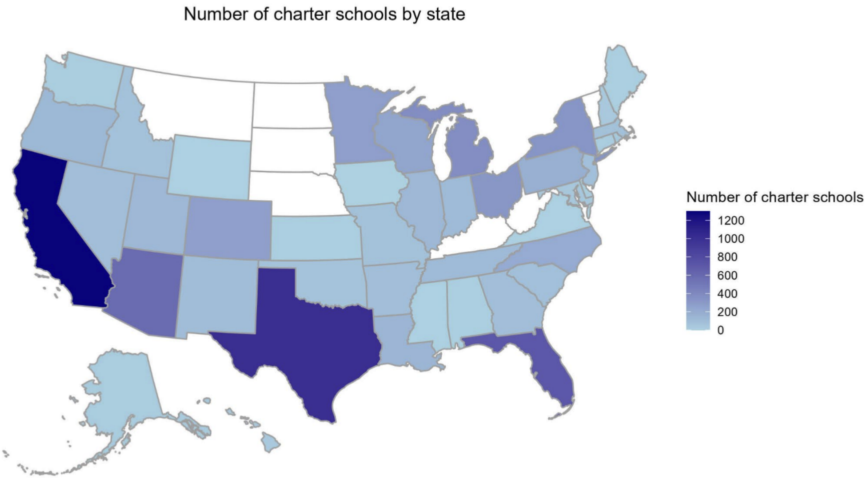
	By change in log enrollment		By tuition ($1,000s)		By performance sanction	
Subject	Main effect (1)	Interaction (2)	Main effect (3)	Interaction (4)	Sanctioned (5)	Not sanctioned (6)
Math	−0.352	−0.092	−0.355	0.263	−0.384	−0.452
	(0.098)	(0.223)	(0.091)	(0.121)	(0.118)	(0.139)
Observations	938		1,050		672	575
p-value	0.679		0.030		0.709	
ELA	−0.039	−0.015	−0.037	0.167	−0.129	−0.023
	(0.091)	(0.332)	(0.087)	(0.106)	(0.113)	(0.111)
Observations	939		1,051		673	575
p-value	0.963		0.114		0.501	
Science	−0.214	−0.397	−0.196	0.118	−0.277	−0.248
	(0.111)	(0.276)	(0.100)	(0.113)	(0.149)	(0.113)
Observations	918		1,031		653	568
p-value	0.150		0.299		0.876	
Social studies	−0.273	0.186	−0.265	0.170	−0.322	−0.341
	(0.104)	(0.313)	(0.090)	(0.121)	(0.125)	(0.129)
Observations	917		1,030		653	567
p-value	0.552		0.158		0.919	

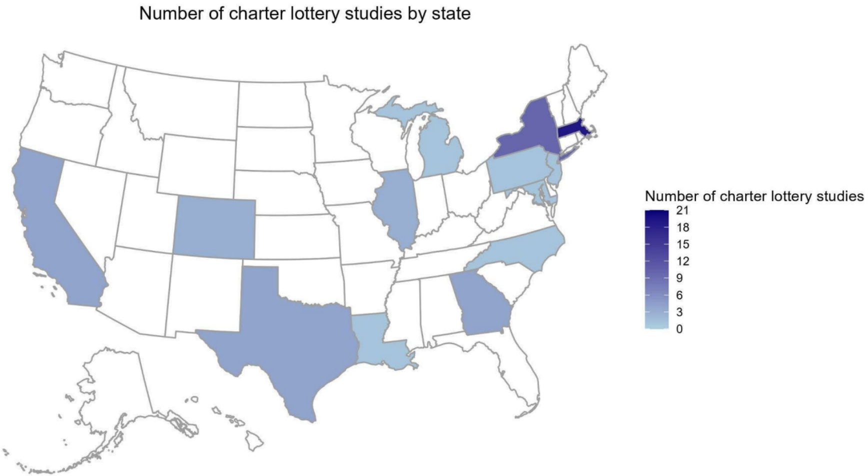
Takeaway on Voucher Lotteries

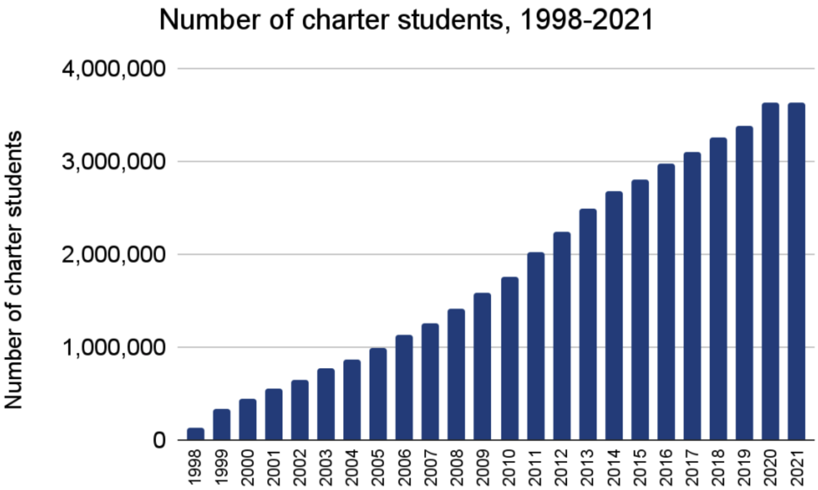
- The sign of voucher (any any school) effects depends critically on the quality of both the focal and fallback school
- Negative in Louisiana — program design attracted the worst private schools
- Mixed in U.S. experiments (Milwaukee, DC)
- Evidence suggests that program design is a first-order consideration, not just offering choice alone
- Epple et al. (2017): “Vouchers have been neither the rousing success imagined by proponents nor the abject failure predicted by opponents”

Charter Schools

- Publicly funded schools governed by private organizations or groups
- Established in the US in the 1990s to promote innovation, competition, and choice in education
- Held accountable through performance contracts or charters
- Supporters argue they provide more options and improve achievement; opponents argue they drain resources and contribute to segregation
- Performance varies widely; research shows mixed results
- Over 3 million (7%) students enrolled in charter schools as of 2020


Number of charter schools by state
Number of charter schools
1200
1000
800
600
400
200
0


Number of charter lottery studies by state
Number of charter lottery studies
21
18
15
12
9
6
3
0


Number of charter students, 1998-2021
Number of charter students
4,000,000
3,000,000
2,000,000
1,000,000
0
1998
1999
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
2011
2012
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2014
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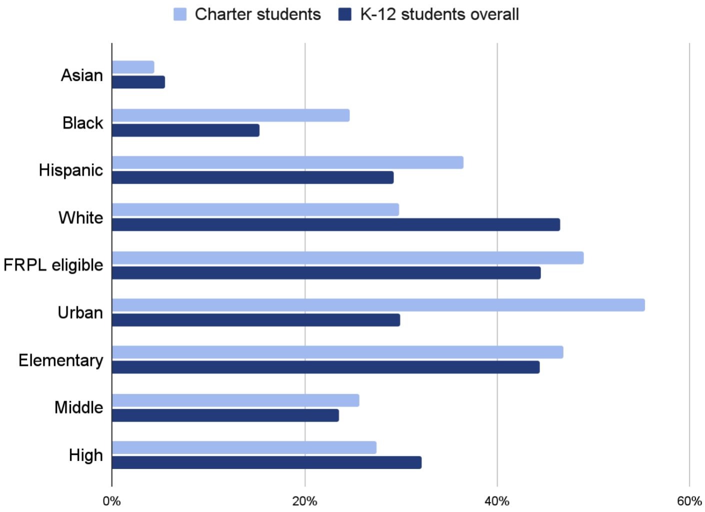
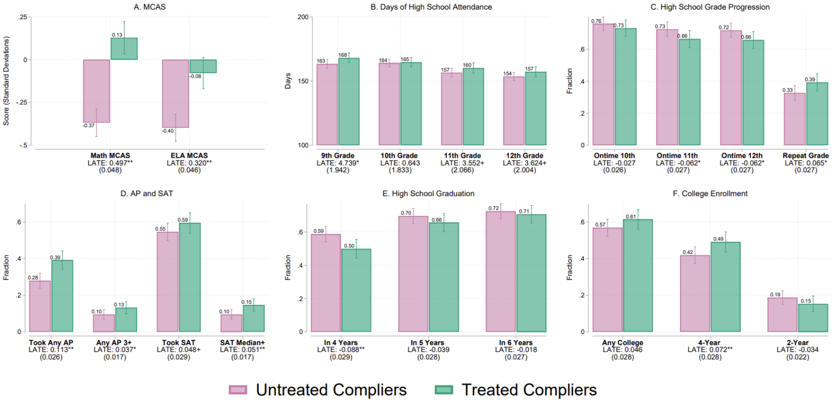
2016
2017
2018
2019
2020
2021


Charter students
K-12 students overall
Asian
Black
Hispanic
White
FRPL eligible
Urban
Elementary
Middle
High
0%
20%
40%
60%

Charter School Evidence from Boston

Figure 1: The Impact of Charter School Attendance on School Outcomes



Notes: This figure shows impact estimates of Boston charter school attendance on school outcomes. For details, see Online Appendix Tables A.1, A.2 and A.3. Sample size varies by outcome (see Tables), for unconditional outcomes, N = 9,562, + p<0.10 * p<0.05 ** p<0.01 *** p<0.001.

Charter School Evidence from Boston

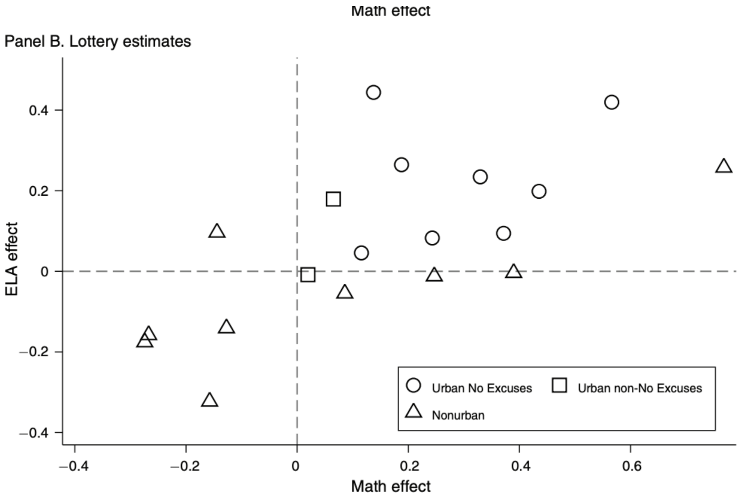


FIGURE 2. SCHOOL-SPECIFIC TREATMENT EFFECTS

Notes: This figure plots school-specific math effects against school-specific ELA effects. The sample used to con-

Dobbie & Fryer (2011): Harlem Children's Zone

- HCZ Promise Academy lottery estimates — a single school network
- Results (2SLS, per year enrolled):
  - → Middle school math: $+0.229\sigma$/year → cumulative 3-year gain of $\sim 0.69\sigma$
  - → Elementary math: $+0.19\sigma$/year → cumulative effect of $\sim 1.15\sigma$
- Large enough to close the Black-white math gap by 9th grade
- The critical policy finding: schools, not community programs, are the primary driver
  - → Students living *outside* the HCZ (no access to community programs) benefit just as much
  - → Sibling spillovers (community but not school): $+0.051\sigma$ in math, statistically insignificant
- "Community programs appear neither necessary nor sufficient," if achievement is the focus

Fryer (2014): Practices Matter

- RCT in Houston: injects No Excuses charter practices into traditional public schools
- The *practices* are what drive the gains, not the charter governance structure
- The lesson from Boston charters
  - → Extended learning time
  - → High-dosage tutoring
  - → Data-driven instruction
  - → Selective teacher evaluation
- Perhaps you do not need a charter to produce charter-like gains, you need the right practices

Takeaway on Charter Lotteries

- Charter effects are highly heterogeneous — not all charters are created equal
- Urban No Excuses charters (Boston, HCZ) produce very large gains (0.2–0.4σ/year in math); non-urban charters often show zero or negative effects
- The gains appear driven by specific *practices* (tutoring, extended time, data-driven instruction), not the charter governance structure itself (Fryer 2014)
- Suggests that the policy-relevant question is not "charter/choice vs. traditional" but whether schools adopt effective instructional models
- Chabrier, Cohodes, and Oreopoulos (2016): lottery-based estimates averaged across charters are positive but modest — the large effects are concentrated among a subset of schools

Intradistrict Systems of Choice

- Both charter and voucher options mostly rely on parents selecting outside of incumbent school districts
- Alternative forms of choice exist within school districts
    1. Magnet programs
    2. Selective schools
- Magnet and other schools with selective criteria still mostly rely on families self-selecting
- In recent years, school districts have created centralized systems of choice covering the entire district (more on this next week)

Cullen, Jacob & Levitt (2006): Chicago School Choice Lotteries

- 194 lotteries at 19 oversubscribed Chicago high schools ($\sim$ 19,000 applications from $\sim$ 14,500 8th graders)
- Lottery winners attend "better" schools on observables:
  - $\rightarrow$ $+5.8$ pp in share of peers above national norms
  - $\rightarrow$ -5.6 pp in free-lunch peers
  - $\rightarrow$ $+4.3$ pp in peer graduation rates
- Yet, zero effects on all traditional academic outcomes
  - $\rightarrow$ 4 of 6 test score measures are *negative*
  - $\rightarrow$ 4-year graduation rate slightly *lower* (-4.4 pp)

Cullen, Jacob, and Levitt (2006): Why Are Effects Null?

- CJL Explanation: Parents choose for non-academic reasons
  - → Most popular schools have high peer achievement levels but not high value-added

- Alternative Explanation: In messy, intra-district decentralized markets, there is an active after-market after offers are made
  - → Families opt-in and apply to magnets
  - → Many undersubscribed magnets fill seats after the formal lottery process
  - → Comparing lottery winners to lottery losers may result in magnet-to-magnet comparisons if lottery losers enroll in other undersubscribed magnets

Bruhn, Campos, Chyn & Shrivatsa (2026): Magnets with Close Substitutes

- Setting: LAUSD magnet schools — 635 lotteries across 53 oversubscribed magnets, 2004–2017
- Conventional lottery IV replicates the CJL pattern: math -0.043σ, ELA -0.060σ, non-cognitive -0.072σ
- But the comparison is prone to substitution bias: lottery losers don't go to neighborhood schools — they enroll in other *undersubscribed* magnets
- Accounting for substitution bias substantially alters the conclusions
- We also find that magnet schools largely pay for themselves (MVPF=8.8)

Bruhn, Campos, Chyn & Shrivatsa (2026): Findings

Substitution Bias is Empirically Relevant

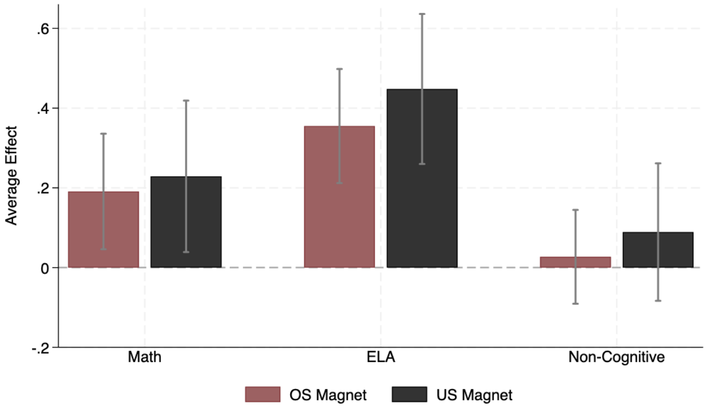
Table 5: Enrollment Outcomes: Data vs. Model

	Offered	Not Offered
Panel A: Data		
Share enrolled in oversubscribed magnet	0.85	0.44
Share enrolled in undersubscribed magnet	0.08	0.36
Share enrolled in neighborhood school	0.06	0.20
Panel B: Model Implied		
Share enrolled in oversubscribed magnet	0.87	0.37
Share enrolled in undersubscribed magnet	0.08	0.40
Share enrolled in neighborhood school	0.05	0.23

Bruhn, Campos, Chyn & Shrivatsa (2026): Findings

Magnet schools are effective relative to neighborhood schools

Figure 3: Average Magnet Effects



Bruhn, Campos, Chyn & Shrivatsa (2026): Findings

Demand is not perfectly aligned with quality

Table 8: Demand for Magnet Programs

	(1)	(2)	(3)	(4)
Peer Quality	0.267		0.261	0.042
	(0.078)		(0.079)	(0.105)
Pr(Exceptional)		-0.423	-0.331	-0.004
		(0.197)	(0.184)	(0.225)
Share Black + Latino				-0.121
				(0.241)
Share Poverty				-0.681
				(0.219)
Constant	-0.694	-0.536	-0.678	0.025
	(0.059)	(0.036)	(0.062)	(0.198)
R^2	0.194	0.026	0.210	0.353

Notes: Each column regresses the estimated school mean utility ($\hat{\delta}_j$) on school attributes. The dependent variable is the posterior mean of δ_j. Peer quality is the average of mean standardized math and ELA test scores of enrolled students. Pr(Exceptional) is the posterior probability that the school's causal effects on math, ELA, and socio-emotional outcomes all fall in the top decile of the school-specific ATE distribution. Share Black + Latino and Share Poverty are time-averaged school demographic shares from enrollment data in our sample period. We report heteroskedasticity-robust standard errors in parentheses.

Takeaway on Intradistrict Choice Lotteries

- Conventional lottery estimates in intradistrict systems often show zero or small negative test score effects (CJL 2006; Hastings, Kane & Staiger 2009)
- Two complementary explanations:
  - → Parents may choose on non-academic dimensions (safety, peers, proximity, extracurriculars) rather than value-added
  - → Substitution bias: lottery losers enroll in other choice options, not neighborhood schools — so the lottery comparison understates effects relative to the true counterfactual (BCCS 2026)
- Once substitution bias is addressed, magnets show meaningful gains relative to neighborhood schools and largely pay for themselves
- Gains from choice are concentrated among students escaping low-quality fallback schools (Deming et al. 2014)
- So far, we have focused on lotteries and participant effects. What about the other mechanism through which choice improves outcomes, competition?

Brief Overview on Competitive Effects Evidence

- Theory suggests that choice injects competition and schools should raise quality, if parents value effective schools (Friedman 1962; Hoxby 2003)
- Evidence from Tiebout competition (Hoxby 2000): higher scores, lower spending; Rothstein (2007) challenges findings
- Charter/voucher effects on incumbent public schools are *positive but small* (Figlio & Hart 2014; Gilraine, Petronijevic & Singleton 2023)
- Competition works best when schools *cannot select students* and families can *observe quality*; when either condition fails, competition devolves into sorting rather than improvement (Hsieh & Urquiola 2006)
- Overall: evidence points to modest, but positive competitive effects. Easier to poke holes at research designs

The evidence on the effectiveness of choice initiatives is mixed. Why?

1. Supply-side: in some settings, the choice programs simply don't offer better options
   - → Louisiana vouchers attracted lower quality schools (adverse selection via program design)
   - → Non-No-Excuses charters perform no better than traditional public schools
   - → Chile's voucher system enabled cream-skimming rather than quality improvement
2. Demand-side: even when good schools are available, families may not choose them
   - → Cullen et al.: parents choose schools with high achievement *levels* but not high value-added
   - → BCCJ (2026): parents seem willing to enroll in lower quality schools if the school is associated with fewer disadvantaged students

- Both stories require understanding demand: What do families want? Do they value effectiveness? Do they have information?

Demand Estimation

The Random Utility Model

- A family i chooses among J schools to maximize utility:

$$U_{ij} = \underbrace{V_{ij}}_{\text{representative utility}} + \underbrace{\varepsilon_{ij}}_{\text{unobserved}}$$

  where $V_{ij} = V(x_{ij}, s_i)$ depends on school attributes x_{ij} (quality, distance, peers) and family characteristics s_i

- ε_{ij} captures everything the econometrician does not observe: idiosyncratic tastes, private information, school-family match quality
- The family chooses the school with the highest utility:

$$\text{Choose } j \iff U_{ij} > U_{ik} \quad \forall\, k \neq j$$

- The implied choice probabilities are:

$$P_{ij} = \Pr\Big(\varepsilon_{ik} - \varepsilon_{ij} < V_{ij} - V_{ik} \quad \forall\, k \neq j\Big) = \int \mathbf{1}\Big(V_{ij} + \varepsilon_{ij} > V_{ik} + \varepsilon_{ik} \quad \forall\, k \neq j\Big)\, f(\varepsilon_i)\, d\varepsilon_i$$

- Key point: Different models arise from different assumptions about $f(\varepsilon)$

Train (2009), Ch. 2

Two Identification Principles

- Only differences in utility matter
  - → Adding a constant c to all utilities does not change behavior: $U_{ij} + c > U_{ik} + c \iff U_{ij} > U_{ik}$
  - → With J alternatives, we can include at most $J - 1$ alternative-specific constants (school fixed effects), normalizing one to zero
- The scale of utility is arbitrary
  - → Multiplying all utilities by $\lambda > 0$ does not change choices
  - → Estimated coefficients are relative to the error variance
  - → But *ratios of coefficients* are scale-free and directly interpretable
- Practical implication for demand models: Sociodemographic variables (income, race) do not vary across schools for a given family. They therefore enter utility *interacted* with school attributes. For example,

$$V_{ij} = \beta_1 \,\text{Quality}_j + \beta_2 \cdot \text{Distance}_{ij} + \beta_3 \cdot \frac{\text{Distance}_{ij}}{\text{Income}_i}.$$

  This would capture the idea that higher income families are less sensitive to distance

What Do Choice Probabilities Look Like?

- The choice probability integral
$$P_{ij} = \int \mathbf{1}\Big(V_{ij} + \varepsilon_{ij} > V_{ik} + \varepsilon_{ik} \;\; \forall\, k \neq j\Big)\, f(\varepsilon_i)\, d\varepsilon_i$$
is a $(J-1)$-dimensional integral — generally has no closed form

- Different assumptions on $f(\varepsilon)$ give us different models:

Model	Assumption on $f(\varepsilon)$	Closed form?
Logit	iid Type I Extreme Value	Yes
Nested logit	GEV (correlated within nests)	Yes
Probit	$\varepsilon \sim \mathcal{N}(0, \Omega)$	No
Mixed logit	β_i random, ε iid EV	No

The Logit Model

- Distributional assumption: Each ε_{ij} is iid Type I Extreme Value:

$$f(\varepsilon_{ij}) = e^{-\varepsilon_{ij}} \, e^{-e^{-\varepsilon_{ij}}}$$

- This yields the logit choice probability (McFadden 1974):

$$P_{ij} = \frac{e^{V_{ij}}}{\sum_{k=1}^{J} e^{V_{ik}}}$$

- Properties:
  - → Closed-form expression — no simulation required
  - → Log-likelihood is globally concave — unique maximum
  - → Sigmoid relationship between V_{ij} and P_{ij}

Interpreting Coefficients

- Suppose representative utility takes the form:

$$V_{ij} = \beta_{\text{qual}} \cdot \text{Quality}_j + \beta_{\text{dist}} \cdot \text{Distance}_{ij} + \beta_{\text{peers}} \cdot \text{Peers}_j + \beta_3 \cdot \text{Quality}_j \cdot \text{Income}_i$$

- Individual coefficients are not directly interpretable (scale is arbitrary)
- But ratios of coefficients are scale-free — the key object:

$$\text{Willingness to Travel (WTT)} = -\frac{\beta_{\text{qual}}}{\beta_{\text{dist}}}$$

“Families are willing to travel X additional miles for a one-unit improvement in school quality”

- The logit model can capture *systematic* heterogeneity by interacting demographics with school attributes ($\beta_3 \cdot \text{Income}_i \times \text{Quality}_j$). But *cannot* capture random (unobserved) taste variation

The IIA Problem

- For any two schools j and k:

$$\frac{P_{ij}}{P_{ik}} = \frac{e^{V_{ij}}}{e^{V_{ik}}} = e^{V_{ij}-V_{ik}}$$

- This ratio depends *only* on j and k, not on any other school in the choice set
- This is the Independence of Irrelevant Alternatives (IIA)
- Implication: the cross-elasticity of P_{ij} w.r.t. an attribute of school k is the *same* for all $j \neq k$
- This forces proportional substitution: if a school is removed, its market share is redistributed proportionally to all remaining schools

IIA Issues: Magnet School Closure Example

- Suppose a representative student has initial choice probabilities:

$$P(\text{General}) = 0.50, \quad P(\text{Arts}) = 0.20, \quad P(\text{Magnet STEM B}) = 0.10, \quad P(\text{Magnet STEM A}) = 0.20.$$

- In multinomial logit,

$$\frac{P_j}{P_k} = \exp(V_j - V_k),$$

so removing STEM A leaves the odds between any two remaining schools unchanged.

- Therefore the remaining probabilities are just renormalized:

$$P_j^{new} = \frac{P_j}{1 - P(\text{STEM A})}.$$

- After STEM A closes:

$$P(\text{General})^{new} = \frac{0.50}{0.80} = 0.625, \qquad P(\text{Arts})^{new} = \frac{0.20}{0.80} = 0.25, \qquad P(\text{STEM B})^{new} = \frac{0.10}{0.80} = 0.125.$$

- So the closure of a STEM school does not make STEM B relatively more attractive than Arts. Any extra within-STEM substitution is ruled out unless that similarity is fully captured in observed utility

Nested Logit

- Group schools into nests of similar alternatives. Allow correlation in ε within nests but not across nests
- In the context of demand for schools:
  - → Nest A: Traditional public schools
  - → Nest B: Charter schools
  - → Nest C: Magnets
- The nesting parameter $\lambda_k \in (0, 1]$ controls within-nest correlation:
  - → $\lambda_k = 1$: collapses to standard logit (no within-nest correlation)
  - → $\lambda_k \to 0$: alternatives within the nest become perfect substitutes
- Within each nest, substitution is proportionate (IIA holds locally). Across nests, substitution is not proportionate — closing a STEM magnet sends more students to STEM magnets
- Limitations: Must pre-specify the nesting structure. Still cannot capture random taste variation

Probit

- Assume $\varepsilon_i \sim \mathcal{N}(0, \Omega)$ with a full covariance matrix
- Choice probability:
$$P_{ij} = \int \mathbf{1}\Big(V_{ij} + \varepsilon_{ij} > V_{ik} + \varepsilon_{ik} \ \ \forall\, k \neq j\Big)\, \phi(\varepsilon_i; \Omega)\, d\varepsilon_i$$
A $(J-1)$-dimensional integral with *no closed form* — must be simulated (GHK simulator is common)
- Benefits of the probit:
  - → Full flexibility in substitution patterns (through Ω)
  - → Handles random taste variation and temporally correlated errors
- Probit limitation: Computationally expensive as J gets large
  - → Identification of the unrestricted covariance matrix requires a lot of data
  - → Empirical applications in the literature limited to settings with relatively small choice sets

Mixed Logit

- Mixed logit is a workhorse model
  - → Random taste variation
  - → Flexible substitution patterns

- The random coefficients setup: Family i's utility from school j:

$$U_{ij} = \beta_i' x_{ij} + \varepsilon_{ij}$$

where $\beta_i \sim f(\beta \mid \theta)$ and ε_{ij} remains iid extreme value

- The parameter θ characterizes the population distribution of preferences
  - → e.g., mean b and covariance W if $\beta \sim \mathcal{N}(b, W)$

- Conditional on β_i, the choice probability is standard logit:

$$L_{ij}(\beta_i) = \frac{e^{\beta_i' x_{ij}}}{\sum_k e^{\beta_i' x_{ik}}}$$

The Mixed Logit Probability

- The unconditional mixed logit probability integrates over the population:

$$P_{ij} = \int \left(\frac{e^{\beta' x_{ij}}}{\sum_k e^{\beta' x_{ik}}} \right) f(\beta \mid \theta)\, d\beta$$

- A *mixture* of logit probabilities, weighted by $f(\beta \mid \theta)$
- Special cases:
  - → Standard logit: $f(\beta)$ degenerate at $\beta = b$ (no heterogeneity)
  - → Mixture models: $f(\beta)$ discrete with M mass points — M family "types"
- Approximation result (McFadden & Train 2000): Mixed logit can approximate *any* random utility model to arbitrary accuracy
- No closed form — probability must be *simulated*:

$$\check{P}_{ij} = \frac{1}{R} \sum_{r=1}^{R} L_{ij}(\beta^r), \qquad \beta^r \sim f(\beta \mid \theta)$$

A Concrete School Choice Parameterization

- Write representative utility with explicit random coefficients:

$$V_{ij} = \underbrace{\bar{\alpha} \cdot \text{Quality}_j + \sigma_\alpha \, \nu_i^\alpha \cdot \text{Quality}_j}_{\text{random coefficient on quality}} + \underbrace{\bar{\gamma} \cdot \text{Distance}_{ij} + \sigma_\gamma \, \nu_i^\gamma \cdot \text{Distance}_{ij}}_{\text{random coefficient on distance}}$$

  where $\nu_i^\alpha, \nu_i^\gamma \sim N(0,1)$

- Parameters to estimate:
  - → $\bar{\alpha}$, σ_α: mean and SD of preferences for school quality
  - → $\bar{\gamma}$, σ_γ: mean and SD of distance sensitivity

- $\sigma_\alpha > 0$ means families *differ* in how much they value quality; some care a lot, others are indifferent

- The *distribution* of WTT across the population:

$$\text{WTT}_i = -\frac{\bar{\alpha} + \sigma_\alpha \, \nu_i^\alpha}{\bar{\gamma} + \sigma_\gamma \, \nu_i^\gamma}$$

  Not a single number but a distribution

Why Mixed Logit Relaxes IIA

- In standard logit, the odds between two schools do not depend on any other option:

$$\frac{P_{ij}}{P_{ik}} = \exp\Big(\beta'(x_{ij} - x_{ik})\Big).$$

  This is the IIA property.

- In mixed logit, this remains true *conditional on a family's tastes* β:

$$\frac{P_{ij}(\beta)}{P_{ik}(\beta)} = \exp\Big(\beta'(x_{ij} - x_{ik})\Big).$$

- But observed choice probabilities average over many different types of families with different tastes:

$$P_{ij} = \int \frac{e^{\beta' x_{ij}}}{\sum_\ell e^{\beta' x_{i\ell}}} f(\beta \mid \theta)\, d\beta.$$

  After integrating over β, the odds between j and k no longer generally simplify.

- Intuition: families with strong STEM preferences place more weight on STEM-like schools, so when one STEM school disappears, substitution comes disproportionately from those families.

- Implication: substitution patterns are no longer proportional; cross-elasticities can differ across pairs of schools.

Rank-Ordered Logit: A Ranking as Sequential Choice

- In many school choice settings, family i submits a rank-order list over K schools
- Suppose family i submits the ordered list

$$j_1 \succ j_2 \succ \cdots \succ j_{T_i},$$

  where $T_i \leq K$ may be smaller than the total number of schools.

- Rank-ordered logit (or *exploded logit*) treats this as a sequence of choices from shrinking choice sets:

$$P_i(j_1, \ldots, j_{T_i}) = \prod_{t=1}^{T_i} \frac{\exp(V_{ij_t})}{\sum_{\ell \in C_{it}} \exp(V_{i\ell})}, \qquad V_{ij} = x'_{ij}\beta,$$

  where C_{i1} is the full menu and $C_{i,t+1} = C_{it} \setminus \{j_t\}$

- Intuition:
  - → rank 1 is the most preferred school from the full menu
  - → rank 2 is the most preferred among the schools not already ranked above it and so on..

- This uses more information than observing only the first choice, but it remains a logit model (IIA at each stage). Can relax and estimate a mixed rank-ordered logit

Estimation

- For each family i, we observe a choice object y_i:
  - → a single chosen school, or a submitted rank-order list
- A demand model with parameters θ implies the probability of that observed object $P_i(y_i \mid \theta)$, and we can estimate via maximum likelihood:

$$\mathcal{L}(\theta) = \prod_i P_i(y_i \mid \theta), \qquad \ell(\theta) = \sum_i \log P_i(y_i \mid \theta)$$

- The workhorse models:
  - → First-choice logit: $P_i(y_i = j \mid \theta) = \frac{e^{V_{ij}}}{\sum_k e^{V_{ik}}}$
  - → Rank-ordered logit: $P_i(y_i = j_1 \succ \cdots \succ j_{T_i} \mid \theta) = \prod_{t=1}^{T_i} \frac{e^{V_{ij_t}}}{\sum_{\ell \in C_{it}} e^{V_{i\ell}}}$
  - → Mixed logit: integrate the choice or ranking probability over $f(\beta \mid \theta)$
- Computational diferences:
  - → Logit / nested logit: $P_i(y_i \mid \theta)$ has closed form $\Rightarrow$ exact MLE
  - → Probit / mixed logit: no closed form $\Rightarrow$ simulated MLE

$$\check{P}_i(y_i \mid \theta) = \frac{1}{R} \sum^{R} P_i(y_i \mid \beta^r), \qquad \beta^r \sim f(\beta \mid \theta)$$

What Do We Learn from Estimated Demand?

1. Trade-offs / willingness to travel: If value-added is measured in SDs and distance in miles,

$$\text{WTT}_i = -\frac{\beta_{i,\text{VA}}}{\beta_{i,\text{dist}}}.$$

2. Substitution patterns: Which schools are close substitutes? If a school closes, where do students go? Estimated demand delivers cross-elasticities and counterfactual reallocation across schools

3. Welfare: Demand models imply expected utility from the available choice set. So if we open new schools or expand choice, the change in consumer surplus (Small and Rosen 1981):

$$\Delta EU_i = \log\left(\sum_{j \in \text{New Choice Set}} e^{V_{ij}}\right) - \log\left(\sum_{j \in \text{Old Choice Set}} e^{V_{ij}}\right).$$

   If utility includes a monetary cost (distance cost) coefficient $\alpha_i < 0$, this can be converted into money-metric (distance-metric) welfare :

$$EV_i = \frac{\Delta EU_i}{-\alpha_i}.$$

4. Policy counterfactuals: What happens if we open a school at location ℓ, raise quality q, provide value-added information, or redraw boundaries? Estimated demand predicts effects on enrollment, sorting, and welfare

Do Parents Value Effectiveness?

The Central Demand-Side Question

- Choice-based reforms “work” if families choose schools that raise achievement
- If parents value proximity, peers, or demographics over value-added, then expanding choice won’t necessarily raise outcomes
- The lottery evidence suggests that families may not always prioritize school effectiveness
- Three approaches we will cover today:
  - → Revealed preference from rank-order lists (APSW 2020)
  - → Structural decomposition of information vs. preferences (Ainsworth et al. 2023)
  - → Experimental variation in information and the role of social interactions (Campos 2025)

Abdulkadiroğlu, Pathak, Schellenberg & Walters: Do Parents Value School Effectiveness?

- Setting: NYC centralized high school choice
  - → ~90,000 applicants per year across 400+ programs
  - → Deferred Acceptance mechanism ⇒ truthful ranking is weakly dominant
  - → Rank-order lists directly reveal preferences
- Demand model: Rank-ordered logit with school-by-covariate-cell mean utilities δ_{cj}
  - → 360 covariate cells: borough × sex × race × lunch × income × ability tercile
  - → Allows preferences to vary across observable family types
- On the achievement side, develop a new econometric approach for estimating school effectiveness. For our focus on demand, there is a key decomposition of average school outcomes for a given school j:

$$\overline{Y}_{jc} = \underbrace{Q_j}_{\text{peer quality}} + \underbrace{ATE_j}_{\text{school effectiveness}} + \underbrace{M_{jc}}_{\text{student-school match}}$$

- Peer quality explains 47% of variance in school average outcomes; school ATE explains 28%
- Key exercise: Associate δ_{jc} with elements of $\overline{Y}_{jc}$:

$$\hat{\delta}_{cj} = \kappa_c + \rho_1 Q^*_j + \rho_2 ATE^*_j + \rho_3 M^*_{cj} + \xi_{cj}$$

Abdulkadiroğlu, Pathak, Schellenberg & Walters: Findings

Little weight assigned to school effectiveness after we account fo peer quality

TABLE 8—PREFERENCES FOR PEER QUALITY AND REGENTS MATH EFFECTS

	Value-added models				Control function models			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. No controls for school characteristics								
Peer quality	0.416		0.438	0.406	0.407		0.439	0.437
	(0.061)		(0.063)	(0.067)	(0.057)		(0.059)	(0.059)
ATE		0.244	−0.033	−0.022		0.219	−0.051	−0.047
		(0.047)	(0.046)	(0.047)		(0.046)	(0.043)	(0.043)
Match effect				−0.072				−0.172
				(0.047)				(0.054)
Observations	21,684							
Panel B. With controls for school characteristics								
Peer quality	0.310		0.314	0.286	0.299		0.303	0.308
	(0.060)		(0.059)	(0.060)	(0.056)		(0.056)	(0.056)
ATE		0.157	−0.005	0.005		0.144	−0.008	−0.003
		(0.042)	(0.039)	(0.040)		(0.040)	(0.035)	(0.035)
Match effect				−0.068				−0.142
				(0.039)				(0.044)
Observations	20,200							

Abdulkadiroğlu, Pathak, Schellenberg & Walters: Findings

Why do we think parents place little weight on school effectiveness after accounting for peer quality?

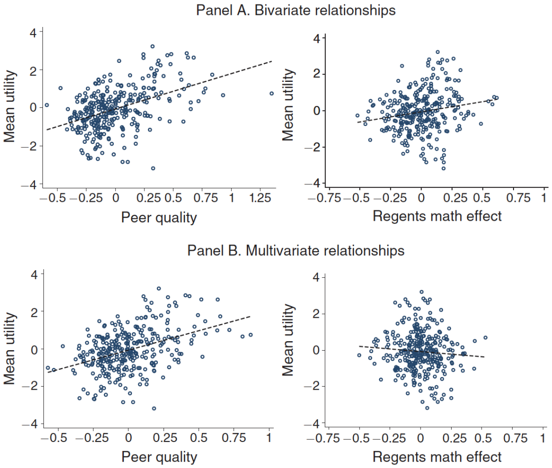


Figure 3. Relationships among Preferences, Peer Quality, and Regents Math Effects

Ainsworth et al. (2023): Why Do Households Leave VA on the Table?

- Ainsworth et al. (2023) assess the information channel with a field experiment
- Setting: Romanian high school choice
  - → Serial dictatorship mechanism $\Rightarrow$ incentive compatible (truthful ranking is dominant)
  - → $\sim$144,000 students/year, 15 cohorts, $\sim$3,800 tracks
- The descriptive facts:
  - → Students attend tracks at the 67th percentile of VA in their feasible set
  - → Could gain a lot in terms of VA by switching to the best feasible option
  - → By contrast, only 0.32 SD of selectivity (peer quality) is left on the table
- Families are much closer to the selectivity frontier than the VA frontier — they optimize more on peer composition than on school effectiveness
- Key question: Is this because families *lack information* about VA, or because they *prefer other attributes*?

Ainsworth et al. (2023): Findings

Families leaving a lot of VA on the table, less so for peer quality

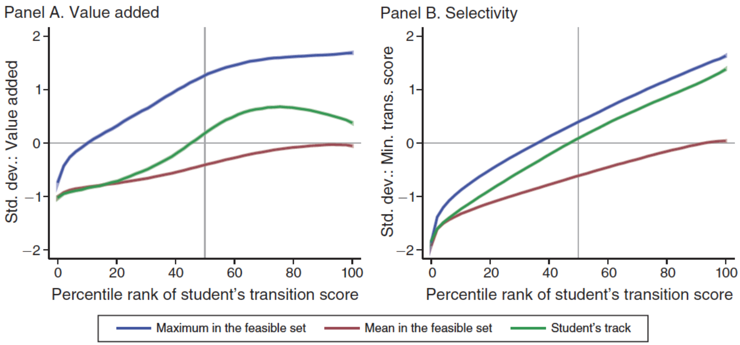


Figure 2. Choice Patterns by Transition Score

Ainsworth et al. (2023): Findings

Information improved low-achieving students enrolled school VA

TABLE 7—AVERAGE TREATMENT EFFECTS ON THE VALUE ADDED OF STUDENTS' TRACKS

	All students	Low achieving	High achieving
Treated	0.048	0.121	−0.002
	(0.025)	(0.049)	(0.023)
Effect in percentage points	0.58	1.45	−0.02
Predicted pass rate	62.9	29.2	83.2
Clusters	78	78	77
Students	2,692	1,012	1,680

Ainsworth et al. (2023): Findings

Beliefs sharpened, but only for families with low-achieving students

TABLE 11—EFFECTS ON THE ACCURACY OF HOUSEHOLDS' VALUE ADDED SCORES

	x^{th} Most preferred track in the baseline						
	All tracks	Most preferred	Second most preferred	$\geq$ Third most preferred	$\geq$ Fourth most preferred	$\geq$ Fifth most preferred	$\geq$ Sixth most preferred
Treated	−0.055	0.032	−0.033	−0.101	−0.124	−0.156	−0.181
	(0.034)	(0.041)	(0.053)	(0.045)	(0.053)	(0.060)	(0.063)
Mean abs. difference Baseline	1.02	0.93	1.07	1.06	1.11	1.16	1.18
Mean abs. difference Follow-up	1.00	0.86	1.03	1.06	1.12	1.14	1.15
Clusters	76	76	75	76	76	76	76
Students	1,525	1,263	962	1,352	1,134	967	868
Student-tracks	4,970	1,263	962	2,745	2,100	1,727	1,487

Ainsworth et al. (2023): Findings

Correcting beliefs still does not lead to a "maxing" out of value-added

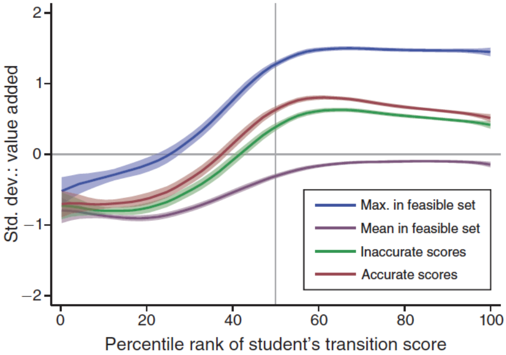


FIGURE 3. THE VALUE ADDED OF STUDENTS' TRACKS UNDER ACCURATE BELIEFS

Campos (2025): Social Interactions, Information, and Preferences for Schools

- Natural next question after APSW and Ainsworth et al: how do parents respond to information about *both* peer and school quality? How does changing the information environment affect long-run outcomes? How important are parental interactions in overcoming relatively weak demand for school quality?

- Setting: LAUSD Zones of Choice
  - → Neighborhood-based public high school choice markets; applications submitted in 8th grade
  - → $\sim$20,000 students across 104 feeder-school $\times$ cohort cells in 2019 and 2021

- Field experiment:
  - → Baseline survey elicits beliefs about peer quality (*Incoming Achievement*) and school effectiveness (*Achievement Growth*)
  - → Cross-randomize information about IA, AG, or both using treatment letters and pedagogical videos
  - → Randomize feeder-school saturation (high / low / pure control) to identify spillovers and social interactions

- What this paper adds:
  - → Separates peer quality from school effectiveness in the information itself
  - → Measures beliefs directly *after* investing time and effort in educating parents about school and peer quality
  - → Tests whether social interactions amplify information effects
  - → Links induced choice shifts to later achievement, socio-emotional outcomes, and college enrollment

Videos Help Educate Parents

Watch Video

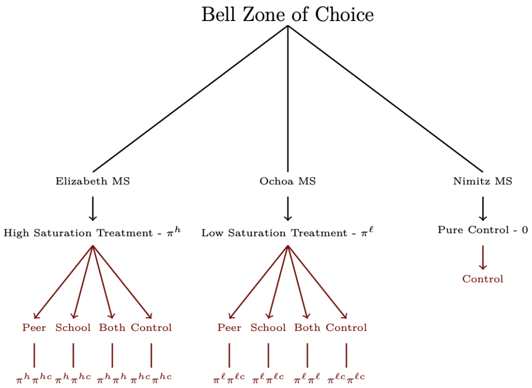
English

Spanish

Campos (2025):Findings

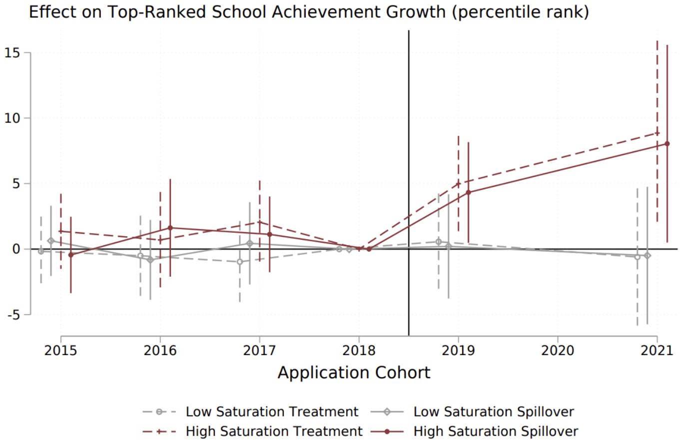
Experimental Design

Figure 3: Assignment to Treatment



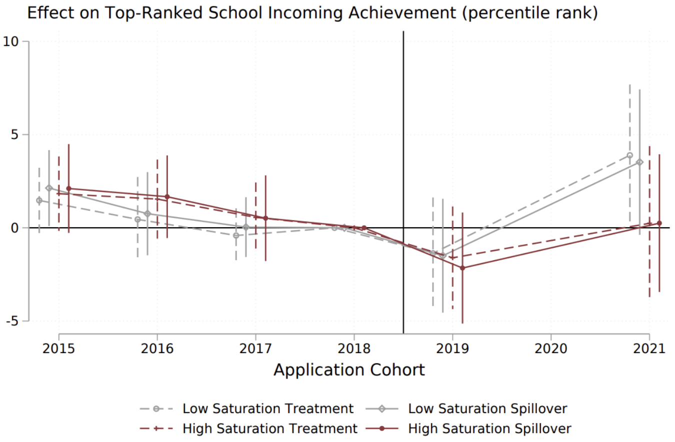
Campos (2025):Findings

Increased Demand for Higher Value-Added Schools Only in High Saturation Schools; Spillovers sizable



Campos (2025):Findings

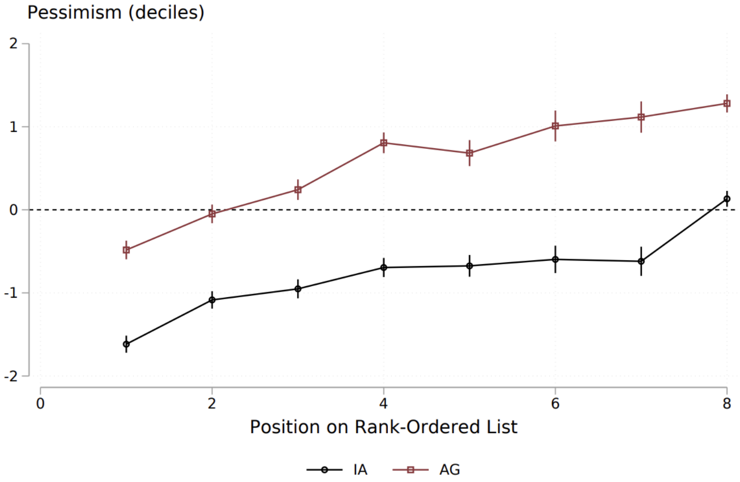
Demand for peer quality unchanged



(b) Impacts on Most-Preferred Incoming Achievement

Campos (2025):Findings

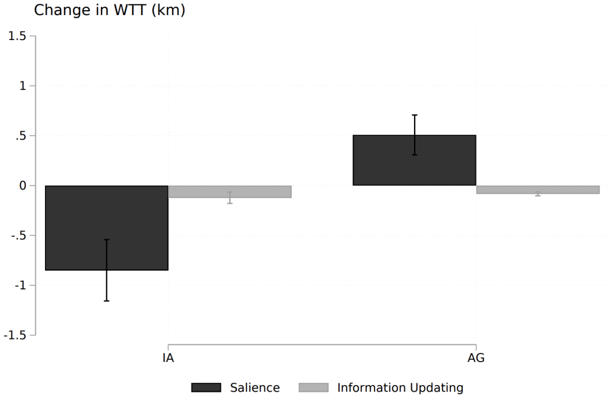
Families understate value-added across most of their ROL; overestimate peer quality across most of their ROL



Campos (2025):Findings

Decomposition suggests changes in choices are due to salience

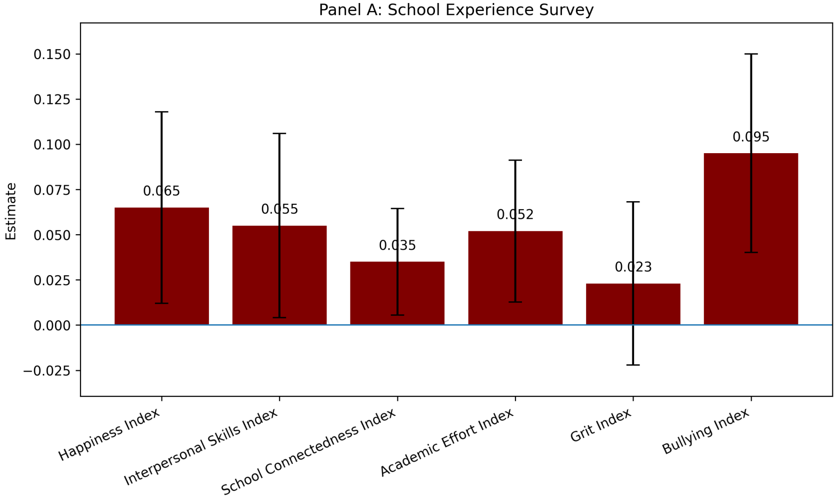
Figure 8: Decomposition of Utility Weight Impacts



(a) Treatment Effects

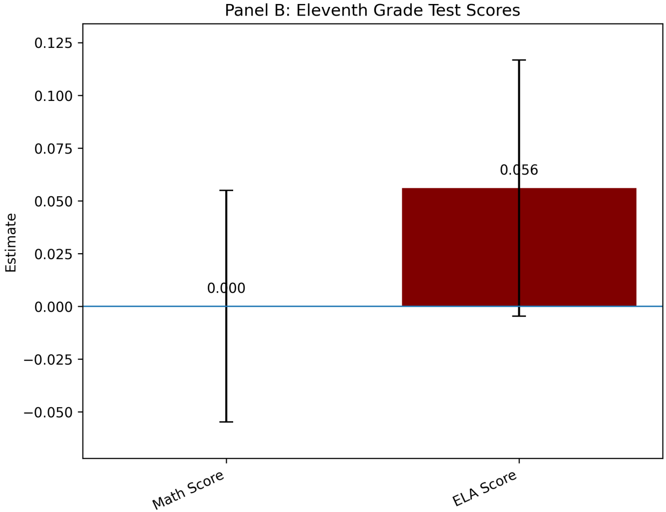
Campos (2025):Findings

A host of non-cognitive outcomes improve many years later



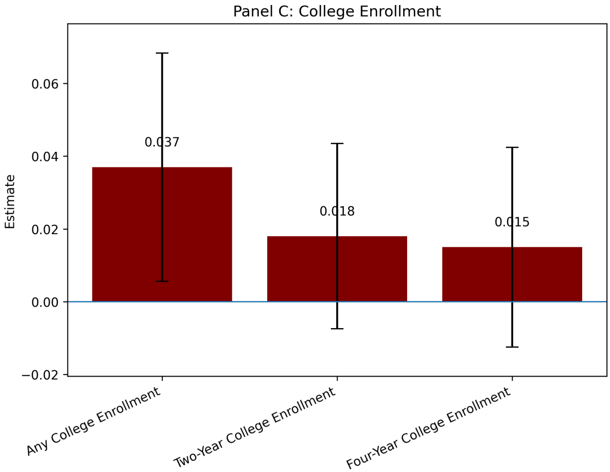
Campos (2025):Findings

Achievement improves in the high-reach schools/clusters



Campos (2025):Findings

College enrollment impacts also sizable



Taking stock: The Demand-Side Constraint

- Parents value many things besides effectiveness:
  - → Proximity, peer composition, demographics, curricular focus
- They often lack accurate beliefs about which schools add the most value
- Information interventions help but close only a fraction of the gap:
  - → Ainsworth et al. (2023): correcting beliefs closes 17–25% of VA gap
  - → Campos (2025): VA information seems preferred to peer quality, and VA information can elevate numerous important outcomes
- Still preferences, not just information frictions, seem to be an important constraint
- What can happen when demand is aligned in a way that is consistent with parents rewarding effective schools? Under what conditions can that happen?

Campos and Kearns (2024)

Campos & Kearns (2024): Institutional Background

- LAUSD's Zones of Choice (ZOC) program merges multiple school catchment areas into a single "zone" — students can attend *any* school in the zone
- Each zone contains $\sim$5 campuses — a manageable choice set (far smaller than NYC's 400+ schools)
- Parents submit zone-specific rank-ordered preference lists; assignments determined by a centralized immediate-acceptance (Boston) mechanism with lottery tiebreaking
- ZOC neighborhoods are overwhelmingly low-income and Hispanic:
  - → 88% Hispanic, 85% poor, only 3% of parents with a college degree

Timeline and Key Institutional Features

- 2007: Belmont Zone pilot in central LA
- July 2012: Formal expansion to 16 zones with centralized administration
- Fall 2013: First cohort under the expanded program enters high school
- Key features:
  - → Personalized, administrator-led information sessions at feeder middle schools
  - → Schools observe families' reported rank-ordered preferences (Boston mechanism)
  - → Most schools operate *below capacity* due to district-wide enrollment decline — losing students means losing teachers and resources

Conceptual Framework: Zones of Choice Setup

1. Before ZOC
   - → Decisions to live in neighborhoods $j = 1, \cdots, J$ are made in a pre-period
   - → Students living in neighborhood j are assigned school j (one school per neighborhood)
   - → School quality α_j determined by principal effort: $\alpha_j = f(e_j)$
   - → Prinicipals independently determine effort resulting in pre-ZOC quality levels

$$\alpha_0 = (f(e_{10}), \cdots, f(e_{J0}))$$

2. Introducing ZOC
   - → A ZOC is created for schools $j \in \mathcal{J}$
   - → Students assigned to any $j \in \mathcal{J}$ as their neighborhood school can choose from the entire menu
   - → Principals strategically determine school quality (effort)

Conceptual Framework: School Choice

- Utility of student i attending school $j \in \mathcal{J}$ is

$$\begin{aligned} U_{ij} &= V_{ij} + \varepsilon_{ij} \\ &= \delta(\alpha_j, \nu_j) - \lambda d_{ij} + \varepsilon_{ij} \end{aligned}$$

- δ_j: school j's popularity
  1. α_j : school quality
  2. ν_j: other school characteristics
- d_{ij} - distance from student i's residence to school j
- λ - distance cost
- $\varepsilon_{ij} \sim EVT1 | \delta_j, d_{ij}$

Conceptual Framework: School Quality

- Principals exert effort to increase α_j

$$\alpha_j = f(e_j)$$

with $f' > 0$ and $f'' < 0$

- Principal j utility increasing in enrollment shares and decreasing in effort

$$u_j = \theta S_j(Q(\alpha_j, \alpha_{-j}^{BR})) - e_j$$

→ $Q(\alpha_j, \alpha_{-j}^{BR}) = (\alpha_1^{BR}, \cdots, \alpha_j, \cdots, \alpha_N^{BR})$ is a vector of school quality

→ θ: relative preference for market shares

Competition Index: Option Value Gain (OVG)

A student with neighborhood school $j(i)$ whose choice set expands to $\mathcal{J}$ has an option value gain

$$OVG_i = \frac{1}{\lambda}\left(E[\max_{k \in \mathcal{J}} U_{ik}] - E[U_{ij(i)}]\right),$$

and with iid EVT1 errors,

$$OVG_i = \frac{1}{\lambda}\left(\ln\left(\sum_{k \in \mathcal{J}} e^{V_{ij}}\right) - V_{ij(i)}\right).$$

- OVG measures expected welfare gain of additional schooling options in willingness to travel units
- School market shares can be expressed in terms of OVG
- OVG summarizes competitive pressure that schools face

Model Implications and Empirical Map

1. **Changes in enrollment patterns**

   Provided differentiation in terms of α_j or ν_j, a non-negligible share of students will enroll in non-neighborhood schools

Model Implications and Empirical Map

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Model Implications and Empirical Map

1. **Changes in enrollment patterns**

2. **Competitive effects**

   For each $j \in \mathcal{J}$, change in school quality is

$$\Delta\alpha_j = \alpha_j^{BR}(\alpha_{-j}, \alpha) - \alpha_{j0} \geq 0$$

Model Implications and Empirical Map

1. **Changes in enrollment patterns**

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Empirical analog:
Matched difference-in-differences design

Model Implications and Empirical Map

1. **Changes in enrollment patterns**
2. **Competitive effects**
3. **Competitive effects are increasing in OVG**

   α_j^{BR} is increasing in OVG for each $j \in \mathcal{J}$

   Empirical analog:
   OVG treatment effect heterogeneity

Model Implications and Empirical Map

1. **Changes in enrollment patterns**
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   α_j^{BR} is increasing in OVG for each $j \in \mathcal{J}$

   Empirical analog:
   OVG treatment effect heterogeneity

Research Design: Difference-in-Differences

- Compare ZOC students to comparable non-ZOC LAUSD students before and after the 2012 expansion:

$$Y_i = \mu_{j(i)} + \mu_{t(i)} + \sum_k \beta_k \cdot \text{ZOC}_{j(i)} \cdot \mathbf{1}\{t(i) - 2013 = k\} + X_i'\psi + u_i$$

- $\mu_{j(i)}$: school fixed effects $\mu_{t(i)}$: year fixed effects
- Event study coefficients β_k trace out dynamic treatment effects
- Matched sample: each ZOC school matched to a non-ZOC school in the same poverty-share and Hispanic-share deciles

Demand for non-neighborhood schools

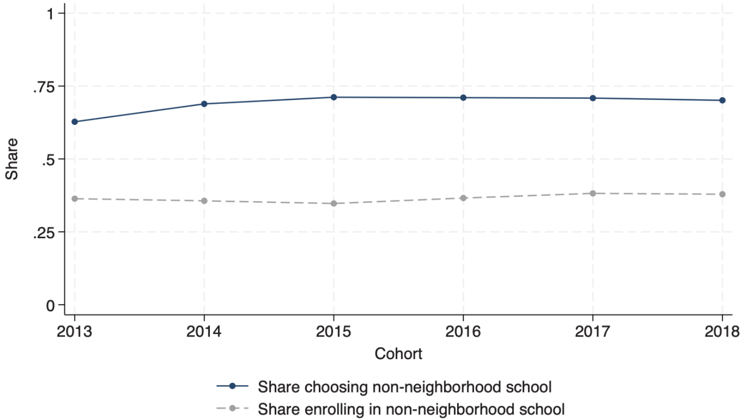
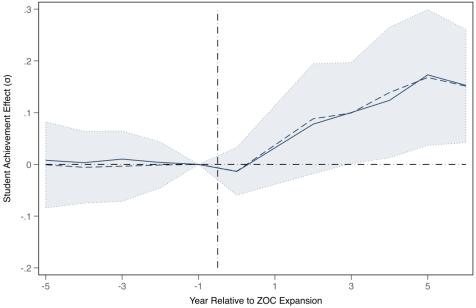


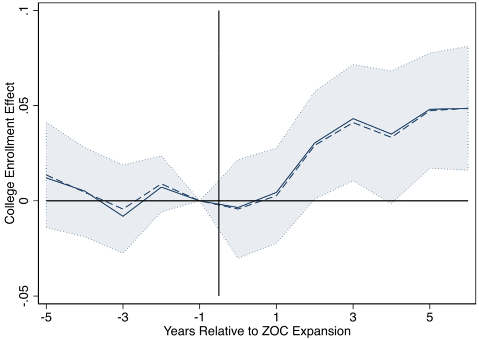
FIGURE II

Demand and Enrollment for Non-Neighborhood Schools

Market-Level Effects: Sizable Achievement and College Enrollment Gains



(A) Achievement



(B) Four-Year College Enrollment

School Quality Improvements Concentrated in the Bottom of the Distribution

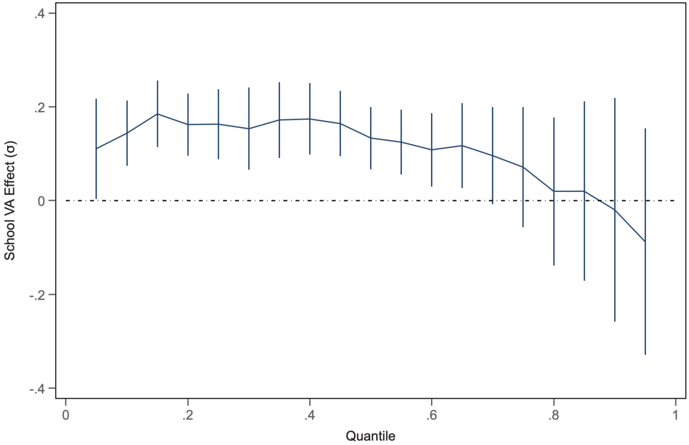


FIGURE IV

Quantile Treatment Effects on School Effectiveness

Heterogeneity: Gains Concentrated at the Bottom

- Quantile treatment effects: gains concentrated in the bottom half of the school effectiveness distribution
- The *worst schools improved the most* — consistent with competitive pressure raising the floor
- This is the “rising tide” channel from Hoxby (2003): competition-induced productivity gains, not just reallocation
- Evidence pointing towards competition playing a role but suggestive at this point
- Studying demand can yield more insights

Parents value school effectiveness

TABLE II

PREFERENCES FOR SCHOOL ATTRIBUTES

	(1)	(2)	(3)	(4)
Panel A: Baseline rank-ordered logit estimates				
School quality	0.140**			0.123*
	(0.0540) [.018]			(0.0622) [.056]
Peer quality		0.137		0.0939
		(0.145) [.331]		(0.158) [.569]
Match Quality			−0.0124	−0.0132
			(0.0778) [.906]	(0.0689) [.529]
R-squared	0.487	0.480	0.475	0.468
Panel B: Rank-ordered logit + quadratic distance				
School quality	0.179***			0.164**
	(0.0532) [.003]			(0.0621) [.019]
Peer quality		0.125		0.0755
		(0.153) [.451]		(0.163) [.642]
Match quality			−0.0386	−0.0345
			(0.0815) [.658]	(0.0711) [.648]
R-squared	0.494	0.480	0.476	0.474
Panel C: Rank-ordered logit + quadratic distance + school controls				
School quality	0.127***			0.101**
	(0.0407) [.005]			(0.0394) [.015]
Peer quality		−0.00618		−0.00598
		(0.112) [.961]		(0.113) [.961]
Match quality			−0.179***	−0.132**
			(0.0622) [.019]	(0.0559) [.051]
R-squared	0.616	0.608	0.628	0.607
Observations	876	876	876	876
Zone × cell × year FE	X	X	X	X

The Option Value Gain (OVG): Measuring Competition

- Pre-ZOC: each school is a neighborhood monopoly. ZOC introduces a strategic effort game among principals
- OVG measures the expected welfare gain from expanding a student's choice set:

$$\text{OVG}_i = \frac{1}{\lambda}\left[\ln\left(\sum_k e^{V_{ik}}\right)_{\text{expanded}} - \ln\left(\sum_k e^{V_{ik}}\right)_{\text{original}}\right]$$

- Serves two roles: (a) welfare measure for families, (b) competition index for schools
- High-OVG students have attractive outside options $\Rightarrow$ their neighborhood school faces more pressure
- Model predicts: (1) all ZOC schools improve, (2) schools facing higher OVG improve more

Heterogeneity in competitive pressure at baseline explain a sizable portion of the effects

TABLE III

Option Value Gain and Treatment Effect Heterogeneity

	Reading (1)	Reading (2)	Reading (3)	Reading (4)	Reading (5)	Reading (6)	Reading (7)
Panel A: School-level OVG							
Post-ZOC	0.085**	0.080*	0.043	0.063	0.083**	0.076	
	(0.041)	(0.043)	(0.054)	(0.053)	(0.041)	(0.060)	
Post-ZOC × School OVG	0.002***	0.002***	0.002***	0.002**	0.002***	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Panel B: Individual-level OVG							
Post-ZOC	0.096***	0.091**	0.053	0.074	0.093***	0.087	
	(0.035)	(0.037)	(0.049)	(0.047)	(0.035)	(0.056)	
Post-ZOC × OVG	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Panel C: Individual-level aggregated OVG							
Post-ZOC	0.084**	0.078**	0.045	0.069	0.081**	0.081	
	(0.036)	(0.038)	(0.051)	(0.048)	(0.036)	(0.057)	
Post-ZOC × $OVG_{3,4}$	0.153***	0.153***	0.149***	0.146***	0.153***	0.090***	0.088***
	(0.028)	(0.028)	(0.027)	(0.027)	(0.028)	(0.024)	(0.024)
Gender		X				X	X
Race/ethnicity			X			X	X
SES				X		X	X
Lagged test scores					X	X	X
Zone-year FE							X
Observations	221,954	221,954	221,954	221,954	221,954	221,954	221,954

Why? Demand Side

- Families choose quality: 1 SD effectiveness $\Rightarrow$ 0.14 SD increase in popularity
- School quality is the strongest predictor of preferences, even after controlling for peer quality, teacher attributes, and course offerings
- Stark contrast with APSW (2020) in NYC:
  - $\rightarrow$ NYC: conditional on peers, VA weight $= -0.05$ (effectively zero)
  - $\rightarrow$ ZOC: quality dominates demand
- Hypothesis: Demographic homogeneity eliminates scope for peer-composition sorting
  - $\rightarrow$ When everyone is 88% Hispanic and 85% poor, peer race/income is nearly constant across schools
  - $\rightarrow$ Families are pushed toward quality-correlated attributes
  - $\rightarrow$ Do policymakers like/want segregated schools?

Some Fragile Details

1. Heterogeneous populations $\Rightarrow$ families sort on peer composition $\Rightarrow$ schools compete by *screening*, not improving
2. Large choice sets $\Rightarrow$ information frictions and choice overload
3. Poor information provision $\Rightarrow$ families cannot identify quality differences
4. Capacity-constrained schools $\Rightarrow$ no credible enrollment threat
5. Different cost structures $\Rightarrow$ competition could reduce productivity when educating disadvantaged students is costly

Summary of ZOC

- Neighborhood-based school choice expansion reduced within-district achievement and four-year college enrollment gaps
- Market-level effects are the most notable and underscore the malleability of factors contributing to neighborhood quality
- Various pieces of evidence supporting a competitive effects among public schools in a large US school district
- Results pose questions for future research
  - → What changed within ZOC schools? Additional qualitative and quantitative evidence necessary
  - → Can this policy be replicated in other settings?

Summing up the lecture

- Choice policies vary enormously in effectiveness and their implementation
- Different research designs get at different aspects of policy reforms
  - → Lotteries ⇒ participant effects
  - → Quasi-experimental market-level designs ⇒ competition
- Charter lotteries have been useful in allowing us to identify *practices* that seem to work and are transportable
- A general takeaway: Demand disciplines supply and affects both participant and competition effects
  - → It is more common to find that parents struggle identifying effective schools, not a good sign for recent efforts expanding vouchers/ESAs with low accountability provisions
  - → Choice policies can be successful but the details can be fragile