RCTs meet Market Design: Treatment Effects and Spillovers in Matching Markets

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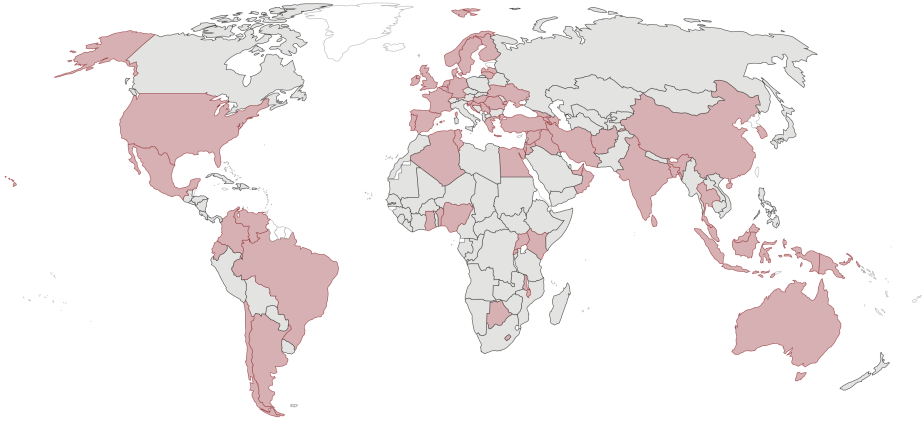
Motivation

- Many school districts around the world are implementing centralized assignment systems that rely on *matching mechanisms* to assign students to school/colleges: China (Chen and Kesten, 2017), Tunisia (Pop-Eleches and Urquiola, 2013), Brazil (Bo and Hakimov, 2016), among others, totaling 63 countries worldwide (Neilson, 2021).



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- **The problem:** (large) RCTs in matching markets violate the SUTVA assumption when the outcome of interest is an equilibrium object, e.g. admission, scores, etc.

This Paper...

- Studies how comparing average treatment vs control outcomes in a matching market yields a biased and inconsistent estimator of the ATET for equilibrium outcomes.
- Proposes a consistent estimator of the ATET and the residual spillover from observed average differences between the outcomes of treatment and control units.
- Applies this new estimator using administrative data from the Chilean *School Admission System* (in Spanish: *Sistema de Admisión Escolar*, SAE), and simulated experiments.

To Fix Ideas

Throughout the talk, I'll focus on a specific scenario:

- Setting → School Choice
- Experimental Units → Students
- Treatment → Information
- Matching Algorithm → Deferred Acceptance
- Outcome → Admission ✔
- Downstream Outcome → Test Scores

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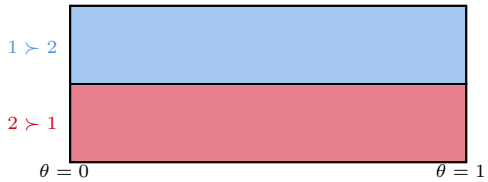

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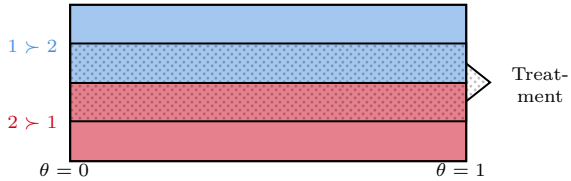

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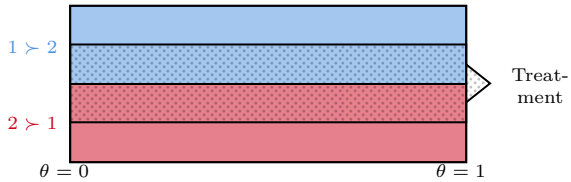

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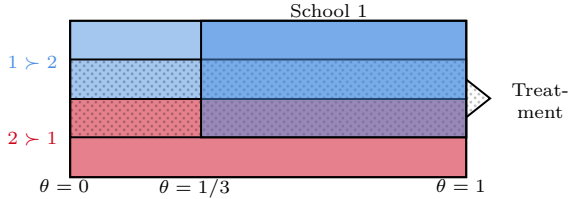

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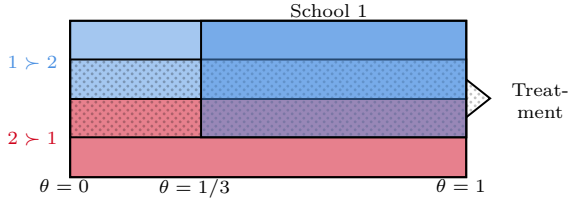

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- **No!** The actual treatment effect on the treated group is $66.\bar{6}\%$-50%=$16.\bar{6}\%$.

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- Assume φ satisfies **equal treatment of equals** (ETE): for any $i, j \in I$ if $\succ_i = \succ_j$, then the assignment probabilities induced by φ are the same for i and j. This will induce a stratified randomized controlled trial (Abdulkadiroğlu et al., 2017).

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- Outcome Y_i could depend on treatment status of other units $j \neq i$. Of course, this will depend on the properties of φ, PP profiles, size of $|T|$ relative to $|I|$, and capacities q_s.

Naïve Estimator

Compute sample mean of treatment group minus that of control group (like in toy model):

$$\widehat{ATET} := \sum_{i \in T} Y_i/|T| - \sum_{i \in I \setminus T} Y_i/|I \setminus T|.$$

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Proof.

$$\begin{aligned}\widehat{ATET} \to & \mathbb{E}[Y_i | i \in T, \succ_T^1, \succ_{I\setminus T}^0] - \mathbb{E}[Y_i | i \notin T, \succ_T^1, \succ_{I\setminus T}^0] \pm \mathbb{E}[Y_i | i \in T, \succ_I^0] \\ = & \tau + \underbrace{\mathbb{E}[Y_i | i \in T, \succ_I^0] - \mathbb{E}[Y_i | i \notin T, \succ_T^1, \succ_{I\setminus T}^0]}_{Spillover}.\end{aligned}$$

Note: If φ satisfies ETE, $\mathbb{E}[Y_i | i \in T, \succ_I^0] = \mathbb{E}[Y_i | i \notin T, \succ_I^0]$, and if T induces $H-$monotonic preferences, $\mathbb{P}[\mu(i) \in H | i \in T, \succ_T^1, \succ_{I\setminus T}^0] \geq \mathbb{P}[\mu(i) \in H | i \in T, \succ_I^0]$. This implies that Spillover ≥ 0, concluding that $\widehat{ATET}$ (weakly) overestimates τ. □

Intuition

- Treatment group affects assignment probabilities of control group: treatment shifts the preference profile $\succ_T$ and thus also the resulting random allocation of $I \setminus T$.
- Observed average outcome of the control group, $\sum_{i \in I \setminus T} Y_i / |I \setminus T|$, an inaccurate estimator of $\mathbb{E}[Y_i | i \in T, \succ_I^0]$.
- Solution: consistently estimate $\mathbb{E}[Y_i | i \in T, \succ_I^0]$.
- Note: if the treatment group is *small*, i.e. the relative mass of T converges to 0 ($|T|/|I| \to 0$ as $|I| \to \infty$), then the naïve estimator $\widehat{ATET}$ is consistent.

Estimator

Algorithm 1: Consistent estimator of $\mathbb{E}[Y_i | i \in T, \succ_I^0]$

Data: $\succ_{I \setminus T}, |T|, H$ (and some large B)
Function: $\varphi : (\succ, seed) \mapsto \mu$ satisfying ETE
Init: $\widehat{\succ}_T^0 \in \mathcal{P}^{|T|}, Y^0 \in \{0,1\}^{|T| \times B}$
Result: $\bar{Y}^0$ ▷ (a consistent estimator of $\mathbb{E}[Y_i | i \in T, \succ_I^0]$)
for $b \in \{1, \ldots, B\}$ **do**
  $\widehat{\succ}_T^0 \leftarrow \texttt{SampleWithReplacement}(\succ_{I \setminus T}, |T|)$;
  $\widehat{\succ}_I^0 \leftarrow \texttt{Append}(\widehat{\succ}_T^0, \succ_{I \setminus T})$;
  $\hat{\mu} \leftarrow \varphi(\widehat{\succ}_I^0, seed_b)$;
  $Y^0[:, b] \leftarrow \mathbf{1}[\hat{\mu}[: |T|] \in H]$;
  ▷ (save simulated treatment outcomes under resampled preferences)
$\bar{Y}^0 \leftarrow \sum_{b=1}^{B} \sum_{i=1}^{|T|} Y^0[i, b] / (B|T|)$ ▷ (simulated sample mean)

In English: Simulate the match replacing the preferences reported by T with random preferences reported by T^c. Do this many times and take the average outcome for T.

Consistency

Lemma

If φ satisfies ETE, then Algorithm 1 yields a simulated sample mean $\bar{Y}^0$ that is a consistent estimator of $\mathbb{E}[Y_i | i \in T, \succ_I^0]$.

Proof.

Under random assignment to T, $\succ_{I \setminus T}$ and $\succ_T^0$ both (strongly) converge to $\succ^0$ as $|I| \to \infty$ and $|T|/|I|$ is held constant. Sampling with replacement from $\succ_{I \setminus T}$, implies the distribution of $\widehat{\succ}_I^0$ (strongly) converges to $\succ^0$. As φ satisfies ETE, given a converging sequence $\widehat{\succ}_I^0$, we have $\varphi(\widehat{\succ}_I^0) \to \varphi(\succ^0)$ and $\left| \mathbb{E}[Y_i | i \in T, \widehat{\succ}_I^0] - \mathbb{E}[Y_i | i \in T, \succ_I^0] \right| \xrightarrow{p} 0$. □

Consistency

Lemma

If φ satisfies ETE, then Algorithm 1 yields a simulated sample mean $\bar{Y}^0$ that is a consistent estimator of $\mathbb{E}[Y_i | i \in T, \succ_I^0]$.

Lemma

Under mild invertibility conditions, this estimator is also asymptotically normal.

Proof.

Appendix A.3 in Agarwal and Somaini (2018). □

Consistency

Lemma

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Lemma

Under mild invertibility conditions, this estimator is also asymptotically normal.

Theorem

Under the previous conditions, $\hat{\tau} := \sum_{i \in T} Y_i / |T| - \bar{Y}^0$ is a consistent and asymptotically normal estimator of τ.

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Extension: Counterfactual Scale-Up/Down Treatments

Recall that the definition of a treatment depends on the fraction of treated population. We can modify the previous estimator to simulate different fractions of treated individuals, and the estimator is still consistent and asymptotically normal. Thus, we can estimate the ATET under scaled-up/down versions of the treatment.

Corollary

Let $\theta \in (0,1)$ be the relative mass of the treatment group: $\theta := |T|/|I|$ and define the ATET as a function of this mass as $\tau(\theta)$. Then, $\hat{\tau}(\theta) = \bar{Y}^1(\theta) - \bar{Y}^0(\theta)$ is a consistent and asymptotically normal estimator of $\tau(\theta)$.

Extension: Difference-in-Differences

In absence of an RCT, and provided that the parallel trends assumption holds, we can use a modified version of the Difference-in-Differences estimator to estimate the ATET:

Corollary

Let time periods be indexed by $t \in \{0, 1\}$, where Y_{it} is the outcome of individual i in period t and $t = 1$ denotes the post-intervention period, and suppose the parallel trends assumption holds:

$$\mathbb{E}[Y_{i0}|i \in T, \succ_I^0] - \mathbb{E}[Y_{i0}|i \in T^c, \succ_I^0] = \mathbb{E}[Y_{i1}|i \in T, \succ_I^0] - \mathbb{E}[Y_{i1}|i \in T^c, \succ_I^0]$$

Then,

$$\hat{\tau}_{DID} = \left(\sum_{i \in T} Y_{i1}/|T| - \bar{Y}_1^0 \right) - \left(\sum_{i \in T} Y_{i0}/|T| - \sum_{i \in T^c} Y_{i0}/|T^c| \right)$$

is a consistent and asymptotically normal estimator of τ.

Extension: Instrumental Variables

Suppose we are interested in computing treatment effects on some other downstream outcome S_i (e.g. test scores). Then, even though we cannot simulate the test scores from the match, we can still use the previous estimator as a first stage to identify the treatment effect of school-admissions on the downstream outcome.
[Note: even if we have spillovers of Y_i on Y_j, we cannot allow for spillovers of Y_i on S_j, i.e. if I get into a good school and increase my test scores, this cannot impact the test scores of someone else through a channel different from admission into H.]

Corollary

Let S_i be a downstream outcome that depends on Y_i, and define the average treatment effect of Y_i on S_i as $\delta := \mathbb{E}[S_i|Y_i = 1] - \mathbb{E}[S_i|Y_i = 0]$. Then,

$$\hat{\delta} := \frac{\sum_{i \in T} S_i/|T| - \sum_{i \in T^c} S_i/|T^c|}{\hat{\tau}} \to \delta.$$

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Data

1. Chile's School Admission System (SAE):
   - 2016 applicants (seeking admission in 2017): Magallanes region.
   - Applying to entry-level high school year (*primero medio*, a.k.a. 9th grade).
   - 1040 applicants, applying to 24 schools.
   - Data on individually submitted preferences (ROLs), random tie-breaking numbers, school capacities and resulting DA allocation.
2. Quality of Education Agency (Agencia de Calidad de la Educación):
   - 2018 data for nationwide standardized test (SIMCE)
   - SIMCE test scores for each individual student in 10th grade
3. Ministry of Education (MINEDUC):
   - Schooling Records: grades and attendance for 10 years of schooling (1st to 10th grade)
   - Mapping from students to schools

Summary Statistics

Table: Summary Statistics

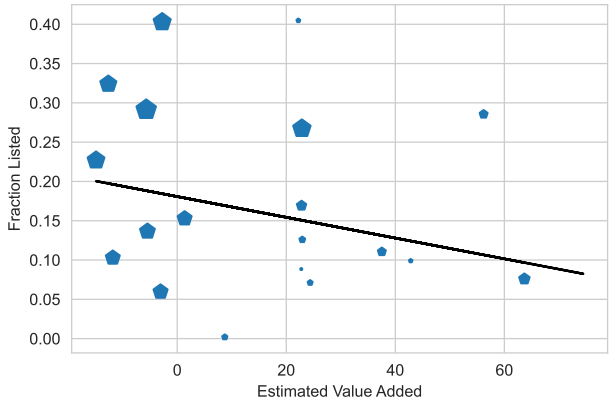
Variable	Mean	St. Dev.	Min.	P25	Median	P75	Max.
Panel A: Schools (N=24)							
Vacancies	53.67	55.78	0	5	31	97	173
Copayment Indicator	0.7917	0.4149	0	1	1	1	1
Coed. Indicator	0.4583	0.5090	0	0	0	1	1
Panel B: Students (N=1,040)							
Female Indicator	0.4769	0.4997	0	0	0	1	1
Low-SES Indicator	0.5221	0.4998	0	0	1	1	1
(Younger) Sibling Indicator	0.0837	0.2770	0	0	0	0	1
Panel C: Applications (N=3,846)*							
Priority Indicator	0.0941	0.2920	0	0	0	0	1
List Length	3.70	1.58	2	3	3	5	15
Panel D: Allocation (N=966)							
1st pref. Indicator	0.6605	0.4738	0	0	1	1	1
2nd pref. Indicator	0.1594	0.3663	0	0	0	0	1
3rd pref. Indicator	0.1046	0.3061	0	0	0	0	1

(*List length is collapsed)

Value Added Estimation

For now: fixed effects with rich controls (10 years of GPA and attendance records, previous SIMCE scores, and demographics). Later: Angrist et al. (2021) paper on VA estimation.

Figure: VA is negatively correlated with listing probability



Note: pentagon sizes are proportional to available slots.

Simulated Experiment

Simulated treatment changes the preferences of T to follow VA-profile: rank schools in decreasing VA order (best strategy for someone optimizing expected future test scores)

Outcome of interest: admission in a top-5 VA school.

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Simulated treatment changes the preferences of T to follow VA-profile: rank schools in decreasing VA order (best strategy for someone optimizing expected future test scores)

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Table: Naïve estimator vs. Consistent estimator

	(1) Naïve	(2) Consistent
Treatment Effect	0.0635^{***}	0.0462^{*}
	(0.0174)	(0.0180)
Control Mean	0.0558^{***}	0.0730^{***}
	(0.0123)	(0.0127)
N	1040	1040

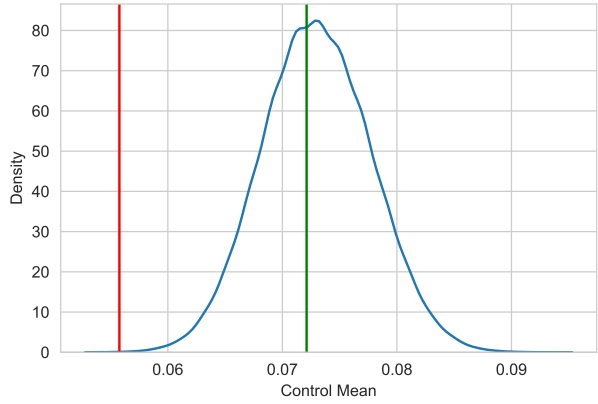
Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Control Mean

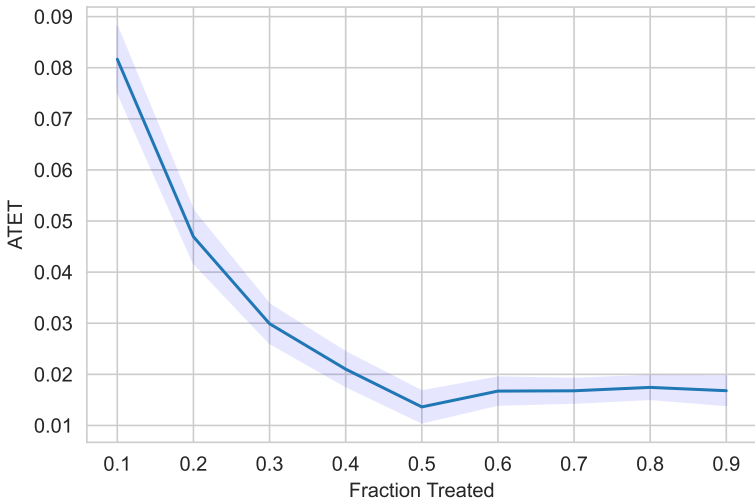
What's happening here? The control mean is being correctly simulated:

Figure: Consistent Estimation of the Control Mean



Note: Red line denotes naïve control mean estimate and green line denotes true mean in absence of treatment.

Scaling the Experiment



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Concluding remarks

- Conventional estimators that compare equilibrium outcomes of treatment vs. control units in matching markets are biased and inconsistent.
- This may be corrected with a simple resampling procedure, which yields a consistent estimator of the ATET. Other flavors: stratified resampling, pscore-weighted resampling, among others.
- Vast applicability (beyond school choice), and potential extensions for future work: e.g. imperfect compliance, distributional treatment effects.

Thank you!

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