# Day 1: Identification by Design

Peter Hull

Design-Based Regression Inference
Fall 2024

# The Design of This Course

- This is a three-day intensive in design-based causal inference
  - Far from comprehensive: will focus on core concepts with regression/IV
  - Emphasis will be on practical lessons for applied work
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- 6-7 hours of lecture, two 30-minute coding demonstrations
  - Please ask questions in the Discord chat!
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- Feedback/follow-up: *peter_hull@brown.edu*

# Course Schedule

**Schedule**

| | | |
|---|---|---|
| Monday 9/9 | 6:00-7:50pm | Lecture 1: Selection-on-Observables |
| | 6:50-7:00pm | *Break* |
| | 7:00-7:50pm | Lecture 2: Design vs. Outcome Models |
| | 7:50-8:00pm | *Break* |
| | 8:00-8:50pm | Lecture 3: Design-Based IV |
| | 8:50-9:00pm | Application 1 Overview |
| Wednesday 9/11 | 6:00-6:30pm | Live-Coding Application 1 |
| | 6:30-6:40pm | *Break* |
| | 6:40-7:40pm | Lecture 4: Negative Weights |
| | 7:40-7:50pm | *Break* |
| | 7:50-8:50pm | Lecture 5: Clustering |
| | 8:50-9:00pm | Application 2 Overview |
| Friday 9/13 | 6:00-6:30pm | Live-Coding Application 2 |
| | 6:30-6:40pm | *Break* |
| | 6:40-7:40pm | Lecture 6: Recentering |
| | 7:40-7:50pm | *Break* |
| | 7:50-9:00pm | Lecture 7: Nonlinear Models |

# The Design *in* This Course

- Design-based methods use knowledge on the assignment process of as-if-randomly assigned shocks in order to estimate causal effects
  - Mimic analysis of "true" experiments, w/known randomization protocol
  - Contrasts with identification strategies that model untreated potential outcomes (e.g. parallel trends) without appealing to randomization

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- We'll get into all of this over the next few days, building up slowly...

# Outline

1. Preliminaries / Regression Recap

2. Selection on Observables

3. Design vs. Outcome Models

4. Design-Based IV

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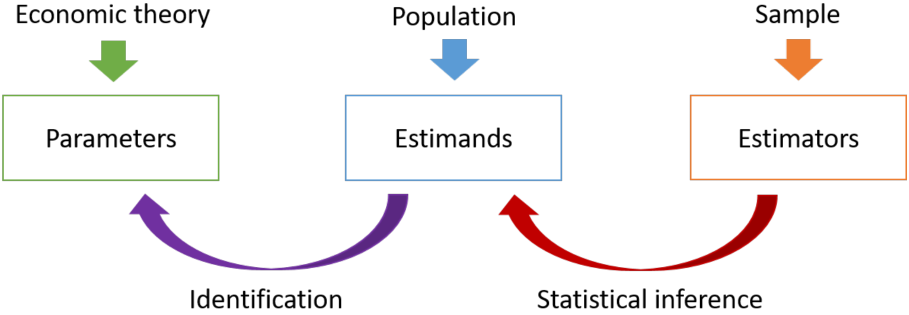
- **Estimators** are functions of observed data (i.e. the "sample")
  - E.g. a difference in sample means or ratio of OLS coefficients
  - Since data are random, so are estimators. Each has a distribution
  - We use knowledge of estimator distributions to learn about estimands (inference) and thus identified parameters

# The Lay of the Land




Separating out the different kinds of tasks in identification vs. inference can help make our lives easier!

# Example: The Simplest Experimental Story

- Throughout today, we'll consider the goal of estimating **parameter** $\beta$ in the constant-effects causal model

$$y_i = \beta x_i + \varepsilon_i$$

$(y_i, x_i)$ are the observed outcome/treatment; $\varepsilon_i$ is the unobserved "untreated" potential outcome (i.e. the value of $y_i$ if we set $x_i$ to 0)

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- We can estimate $\beta^{OLS}$ with the OLS **estimator**, $\hat{\beta}^{OLS} = \frac{\widehat{Cov}(y_i, x_i)}{\widehat{Var}(x_i)}$

# Inference vs. Identification

- Under mild conditions, the OLS estimator gets arbitrarily "close" to the regression estimand as the sample grows (i.e., $\hat{\beta}^{OLS} \xrightarrow{p} \beta^{OLS}$)
  - Moreover, the errors $\hat{\beta}^{OLS} - \beta^{OLS}$ approximately follow a known distribution (i.e. $\mathrm{N}(0, \hat{SE}^2)$, where $\hat{SE}$ is the robust standard errors)
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- That's all Stata can tell us when we *reg y x, r*. The rest is up to us
  - Outside of true experiments, we need to ponder whether $Cov(x_i, \varepsilon_i) = 0$ in order to say whether $\beta^{OLS}$ identifies $\beta$

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  - How can we assess / relax this strong condition?

# Regression Recap

- The **regression** of $y_i$ on $x_i = [x_{1i}, \ldots, x_{Ji}]'$ gives the best (MSE-minimizing) linear approximation to the CEF of $y_i \mid x_i$:

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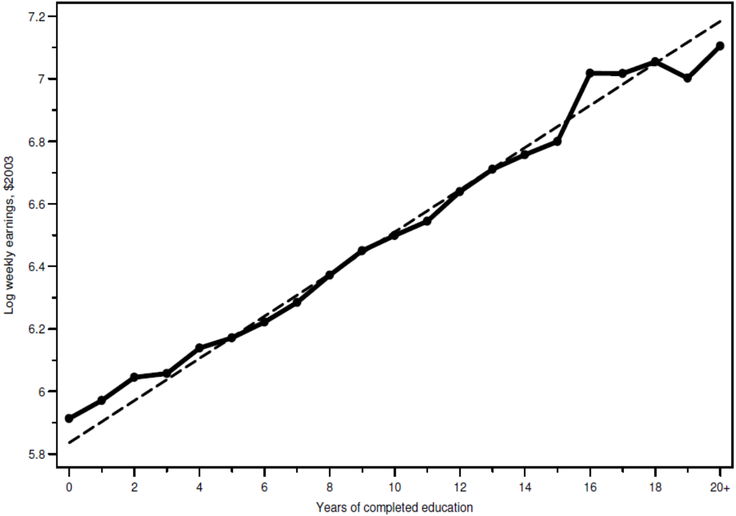
- By taking FOCs: $\beta^{OLS} = E[x_i x_i']^{-1} E[x_i y_i]$ (note: non-random)
  - OLS estimator: $\hat{\beta}^{OLS} = (\sum_i x_i x_i')^{-1} \sum_i x_i y_i$ (note: random)

# Regression Linearly Approximates the CEF




Notes: CEF and linear regression of average log weekly wages given schooling for white men aged 40-49 from the 1980 IPUMS 5% sample

# Regression Anatomy

- When $x_i = [x_{1i}, 1]'$, the two elements of $E[x_i x_i']^{-1} E[x_i y_i]$ are:
  - Slope $\beta_1^{OLS} = \frac{Cov(x_{1i}, y_i)}{Var(x_{1i})}$; intercept $\beta_2^{OLS} = E[y_i] - \beta_1 E[x_{1i}]$

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- The Frisch-Waugh-Lovell (FWL) theorem tells us that, more generally, the $k$-th non-constant slope coefficient is

$$\beta_k^{OLS} = \frac{Cov(\tilde{x}_{ki}, y_i)}{Var(\tilde{x}_{ki})}$$

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- Notice: $\tilde{x}_{ki} = x_{ki} - E[x_{ki} \mid x_{\neg k,i}]$ when $E[x_{ki} \mid x_{\neg k,i}]$ is linear...

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  - Finally, by conditional random assignment, $E[x_i \mid w, \varepsilon] - E[x_i \mid w] = 0$
  - Thus, $Cov(\tilde{x}_i, \varepsilon_i) = 0$ and we have identification: $\beta^{OLS} = \beta$

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- Best to use group-dummy $w_i$, such that linear $E[x_i \mid w_i]$ is trivial
  - Otherwise, good to check sensitivity to more flexible control specs (e.g. add interactions or higher-order polynomials)

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- D&K estimate effects of attending a more selective college (e.g., a private school) on adult earnings
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- *Ex post* empirical validation:
  - Conditional on the selection controls, $x_i$ appears uncorrelated with other baseline observables (demographics, etc)

# Dale and Krueger Estimates (from MHE)

| | No Selection Controls | | | Selection Controls | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Private School | 0.135 | 0.095 | 0.086 | 0.007 | 0.003 | 0.013 |
| | (0.055) | (0.052) | (0.034) | (0.038) | (0.039) | (0.025) |
| Own SAT score/100 | | 0.048 | 0.016 | | 0.033 | 0.001 |
| | | (0.009) | (0.007) | | (0.007) | (0.007) |
| Predicted log(Parental Income) | | | 0.219 | | | 0.190 |
| | | | (0.022) | | | (0.023) |
| Female | | | -0.403 | | | -0.395 |
| | | | (0.018) | | | (0.021) |
| Black | | | 0.005 | | | -0.040 |
| | | | (0.041) | | | (0.042) |
| Hispanic | | | 0.062 | | | 0.032 |
| | | | (0.072) | | | (0.070) |
| Asian | | | 0.170 | | | 0.145 |
| | | | (0.074) | | | (0.068) |
| Other/Missing Race | | | -0.074 | | | -0.079 |
| | | | (0.157) | | | (0.156) |
| High School Top 10 Percent | | | 0.095 | | | 0.082 |
| | | | (0.027) | | | (0.028) |
| High School Rank Missing | | | 0.019 | | | 0.015 |
| | | | (0.033) | | | (0.037) |
| Athlete | | | 0.123 | | | 0.115 |
| | | | (0.025) | | | (0.027) |
| Selection Controls | N | N | N | Y | Y | Y |

Notes: Columns (1)-(3) include no selection controls. Columns (4)-(6) include a dummy for each group formed by matching students according to schools at which they were accepted or rejected. Each model is estimated using only observations with Barron's matches for which different students attended both private and public schools. The sample size is 5,583. Standard errors are shown in parentheses.

# Aside: The Link to Propensity Scores

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  - In our constant-effects model, $y_i = \beta x_i + \varepsilon_i$, things don't really get more complicated with multivalued/continuous $x_i$

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- If we know/estimate the propensity score in a first step, we could alternatively use the *recentered* $\tilde{x}_i = x_i - Pr(x_i = 1 \mid w_i)$ directly
  - We'll come back to this idea...

# Outline

# Why are Multi-Way FE Different?

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- We need a different justification for this sort of regression...

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  $\implies$ regression identifies it
  - Logic clearly extends to more than two FEs, time-varying controls, unit-specific trends, or any other model for $E[\varepsilon \mid w]$

# Example: Finkelstein (2007)

- Boiling multi-way FE regression specs down to simpler "diff-in-diff' comparisons can make the content of the outcome model clearer
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    - Post 1965, $x_{it} = 1$ for all markets; previously far from random

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  - Event study version: $y_{it} = \alpha_i + \tau_t + \sum_s \beta_s (1 - x_{i,Pre}) \mathbf{1}[t = s] + \nu_{it}$; expect flat pre/post trends if the model is right...

# Finkelstein Event Study

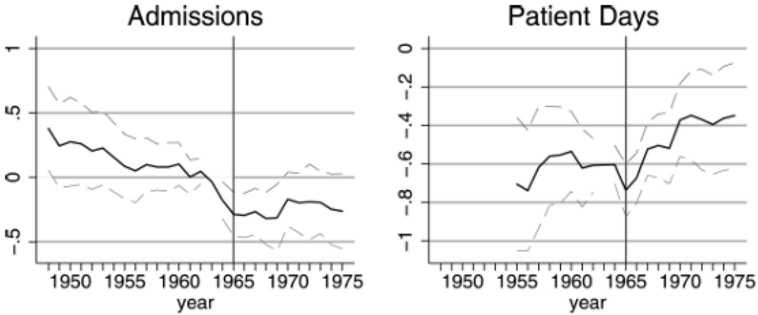


Figure II graphs the pattern of the $\lambda_t$ coefficients from estimating (1) for the log of the dependent variable given above each graph. The scale of the graph is normalized so that in the reference year (1963) it is the average difference in the dependent variable between the south and west (where Medicare had a larger impact) relative to the north and northeast (where Medicare had a smaller impact). The dashed lines show the 95 percent confidence interval on each coefficient relative to the reference year (1963). Time varying state-level controls ($X_{st}$) in all analyses consist of eight indicator variables for the number of years before (or since) the implementation of Medicaid in state $s$ (see text for more details).

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- Both strategies have *ex post* validations (balance tests / pre-trend checks), but the *ex ante* case for design is arguably easier to make
  - What $\varepsilon_{it}$ model is best? E.g. does parallel trends hold in levels or logs?

# Outline

1. Regression/IV Recap✓
2. Selection on Observables✓
3. Design vs. Outcome Models✓
4. Design-Based IV

# The Simplest IV Story

- Again start w/constant fx model $y_i = \beta x_i + \varepsilon_i$, now $Cov(x_i, \varepsilon_i) \neq 0$
  - E.g. $x_i$ is enrollment in this class and $y_i$ is later wages/happiness
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- Imagine the course was "oversubscribed"; I chose students by lottery
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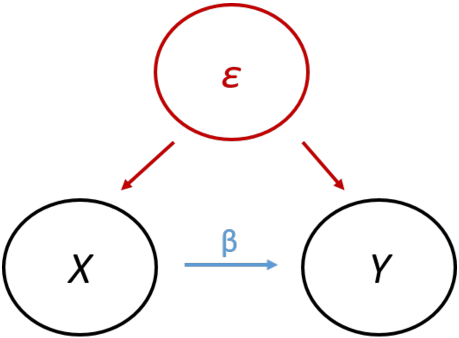
  - Randomness + no direct effects of $z_i$ on $y_i$ implies $Cov(z_i, \varepsilon_i) = 0$
- Plugging in the model for $\varepsilon_i = y_i - \beta x_i$, we have IV identification:

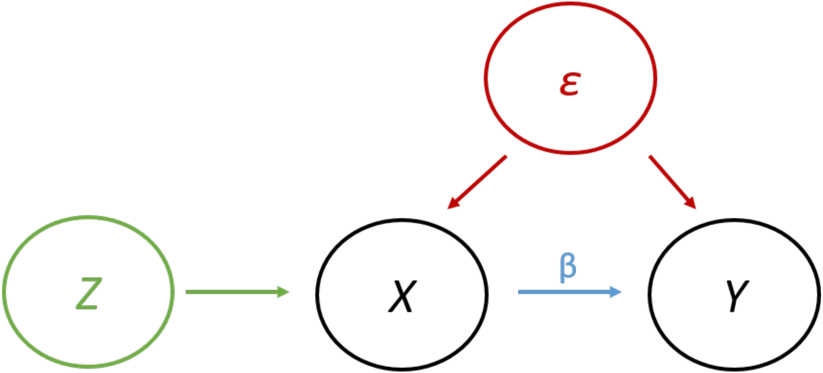
$$Cov(z_i, y_i - \beta x_i) = 0 \implies \frac{Cov(z_i, y_i)}{Cov(z_i, x_i)} = \beta$$

so long as $Cov(z_i, x_i) \neq 0$ ("relevance")

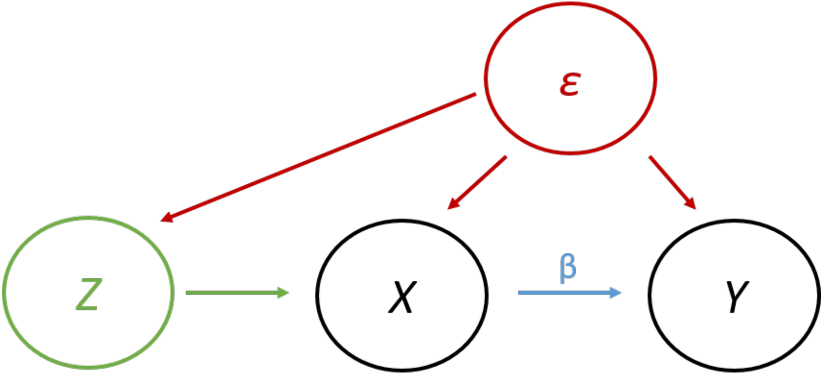
# Regression “Endogeneity”



# Instrument "Exogeneity" / "Validity"

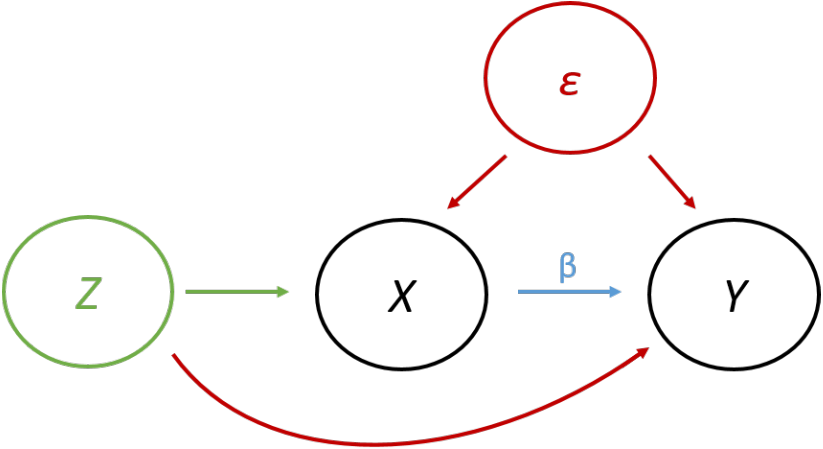


# Threats to Validity: Instrument Assignment




We will later formalize this as a failure of instrument "independence"

# Threats to Validity: Direct Effects




We will later formalize this as a failure of instrument "exclusion"

# Adding Controls and Instruments

- Basic IV is $\frac{Cov(z_i,y_i)}{Cov(z_i,x_i)} = \frac{Cov(z_i,y_i)/Var(z_i)}{Cov(z_i,x_i)/Var(z_i)} = \rho/\pi$ from the regressions:

$$y_i = \kappa + \rho z_i + \nu_i \quad \text{, the "reduced form"}$$

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- RF&FS are the nuclei of IV; the design-based approach starts w/them

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- This makes both reduced form and first stage regressions causal
    - As before, best to both tell a clear *ex ante* story about where the $z_i \mid w_i$ variation comes from and validate as-if-random assignment *ex post*
- New twist: have to also argue exclusion in order to interpret RF/FS
    - Can both argue *ex ante* and sometimes test *ex post*: e.g. by looking at effects of $z_i$ on other plausible treatment channels

# Example: Abdulkadiroglu et al. (2016)

- AAHP are interested in the effect of “takeover” charter schools: ones which convert a low-performing traditional public school (TPS)
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  - Specifically, match each takeover school to a TPS using baseline test score performance and control for match cell fixed effects
- Exclusion: takeovers only affect later test scores via charter enrollment
  - Check whether there are takeover effects in the transition (pre-charter) year 0; develop a strategy to use these effects to relax exclusion

# Abdulkadiroglu et al. Results

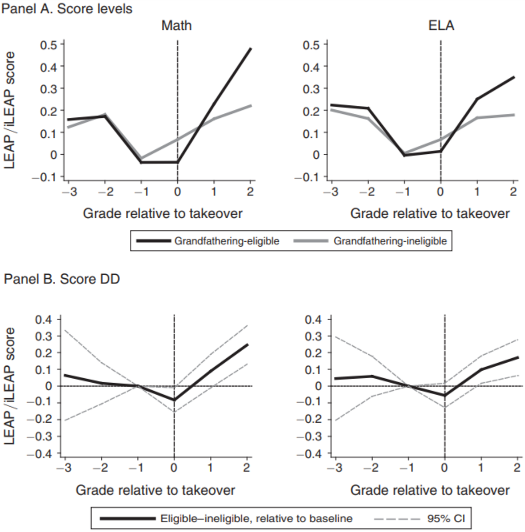


FIGURE 2. TEST SCORES IN THE RSD GRANDFATHERING SAMPLE

*Notes:* Panel A plots average LEAP/iLEAP math and ELA scores of students in the RSD legacy middle school matched sample. Panel B plots achievement growth relative to the baseline (−1) grade. Estimates in both panels control for matching cell fixed effects. Scores are standardized to those of students at direct-run schools in New Orleans RSD, by grade and year. Grade 0 is the last grade of legacy school enrollment.

# Abdulkadiroglu et al. Results (Cont.)

| | | Comparison group mean (1) | OLS (2) | 2SLS First stage (3) | 2SLS Enrollment effect (4) |
|---|---|---|---|---|---|
| *Panel A. All grades* | | | | | |
| (Fifth through eighth) | Math (N = 5,625) | −0.089 | 0.123<br>(0.020) | 1.073<br>(0.052) | 0.212<br>(0.038) |
| | ELA (N = 5,621) | −0.092 | 0.082<br>(0.018) | 1.075<br>(0.052) | 0.143<br>(0.039) |
| *Panel B. By grade* | | | | | |
| Fifth and sixth grades | Math (N = 2,579) | −0.091 | 0.099<br>(0.035) | 0.738<br>(0.041) | 0.165<br>(0.068) |
| | ELA (N = 2,579) | −0.116 | 0.023<br>(0.033) | 0.745<br>(0.042) | 0.101<br>(0.070) |
| Seventh and eighth grades | Math (N = 3,046) | −0.086 | 0.133<br>(0.020) | 1.355<br>(0.070) | 0.231<br>(0.037) |
| | ELA (N = 3,042) | −0.071 | 0.104<br>(0.019) | 1.352<br>(0.070) | 0.171<br>(0.036) |

# Abdulkadiroglu et al.: Comparison to Lottery IV

| | | | | 2SLS | | |
|---|---|---|---|---|---|---|
| | | | | First stage | | |
| | | Comparison group mean (1) | OLS (2) | Immediate offer (3) | Waitlist offer (4) | Enrollment effect (5) |
| *Panel A. All grades* | | | | | | |
| (Sixth through eighth) | Math (N = 2,202) | 0.059 | 0.301 | 0.760 | 0.562 | 0.270 |
| | | | (0.022) | (0.063) | (0.067) | (0.056) |
| | ELA (N = 2,205) | 0.103 | 0.148 | 0.759 | 0.562 | 0.118 |
| | | | (0.020) | (0.063) | (0.067) | (0.051) |
| *Panel B. By potential exposure* | | | | | | |
| First exposure year (sixth and seventh grades) | Math (N = 881) | 0.056 | 0.347 | 0.519 | 0.397 | 0.365 |
| | | | (0.044) | (0.034) | (0.038) | (0.086) |
| | ELA (N = 882) | 0.058 | 0.239 | 0.521 | 0.394 | 0.220 |
| | | | (0.044) | (0.034) | (0.038) | (0.088) |
| Second and third exposure year (seventh and eighth grades) | Math (N = 1,321) | 0.061 | 0.294 | 0.921 | 0.665 | 0.242 |
| | | | (0.021) | (0.088) | (0.091) | (0.054) |
| | ELA (N = 1,323) | 0.129 | 0.131 | 0.918 | 0.668 | 0.083 |
| | | | (0.020) | (0.088) | (0.091) | (0.047) |

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- Before then, you have the chance to play with a real-world application