This course covers recent developments in empirical labor economics along with econometric methods commonly used in contemporary research on labor markets.

**Course Outline**

**Part I: Education and Human Capital**

*Returns to schooling*


Methods: Potential outcomes, instrumental variables (IV), regression discontinuity (RD)

*College quality*


Methods: Matching/selection on observables

**Part II: Self-selection**

*Roy and generalized Roy models*


Methods: Control functions, IV, marginal treatment effects

*Self-selection applications*


Methods: IV, discrete choice, dynamic models

**Part III: Discrimination**

*Group differences in the labor market*


Methods: Oaxaca-Blinder decompositions

*Experimental approaches*


Methods: Experiments, empirical Bayes, bounds

*Algorithmic bias*


Methods: Difference-in-differences

**Part IV: Minimum Wages**


Methods: Difference-in-differences (DiD), differential exposure designs

**Part V: Firm Wage Premia**


Methods: High dimensional fixed effects, Oaxaca decomp

**Part VI: Monopsony, Rent-Sharing, and Outside Options**


Methods: Discrete choice modeling, DiD, experiments, fixed effects
AEA Continuing Education 2021:
Labor Economics and Applied Econometrics

Lecture 1 - Education and Human Capital

Chris Walters

University of California, Berkeley and NBER
This course covers topics in modern labor economics.

We will also cover econometric tools that are commonly used in contemporary applied microeconomics.

**Instructors:**

- Pat Kline, pkline@econ.berkeley.edu
- Chris Walters, crwalters@econ.berkeley.edu

**Schedule:** January 6-7, 11:15AM-6:45PM (with breaks)

Syllabus, schedule, and slides available on course website.
Human Capital and Education

- First topic: human capital and education
- Motivated by human capital paradigm
  - Worker skills as a form of capital
  - Choose how much to invest in skills, balancing increased earnings in the future against opportunity cost of earnings foregone in the present
  - Key parameter: causal return to schooling
- The causal return to schooling answers a counterfactual question: how much more would a particular person earn if s/he spent more time in school?
- We will discuss such questions in the language of potential outcomes
Potential Outcomes

- Consider a person \( i \) deciding whether to attend college.

- The indicator \( D_i \in \{0, 1\} \) takes a value of 1 if \( i \) attends college, and 0 otherwise.

- \( Y_i(1) \) denotes \( i \)'s potential earnings if she attends college.

- \( Y_i(0) \) denotes \( i \)'s potential earnings if she does not attend college.

- Potential outcomes are defined by a hypothetical manipulation: what \textit{would happen} to a particular person in one condition or the other.

- The causal effect of college on person \( i \)'s earnings is defined as:

  \[
  \delta_i = Y_i(1) - Y_i(0).
  \]

- This simple model of causality is called the \textbf{Rubin causal model} (Holland 1986).
The Fundamental Problem of Causal Inference

- In the real world, a person either attends college, or she doesn’t
- This means only one potential outcome will ever be observed – the other is counterfactual
- The observed outcome, $Y_i$, equals $Y_i(0)$ if $D_i = 0$ and $Y_i(1)$ if $D_i = 1$. We can then write

$$Y_i = Y_i(0) + (Y_i(1) - Y_i(0))D_i$$

- Since we can never observe both $Y_i(0)$ and $Y_i(1)$, we can’t see $\delta_i$ for any individual. This is known as the fundamental problem of causal inference
- The econometric methods we will cover can be viewed as approaches to imputing missing potential outcomes
- We can never hope to recover $\delta_i$ for an individual person, but sometimes we can recover certain averages
Average Treatment Effects

- The **average treatment effect** for a population is defined as:

\[ ATE = E[Y_i(1) - Y_i(0)] \]

- “Treatment effects” language is adopted from medical trials

- \( Y_i(1) \) is i’s outcome if assigned the treatment (college)

- \( Y_i(0) \) is i’s outcome if assigned the control condition (no college)

- \( \delta_i = Y_i(1) - Y_i(0) \) is i’s **treatment effect**

- Other treatment effect parameters of interest include the **effect of treatment on the treated (TOT)**, and the **effect of treatment on the non-treated (TNT)**:

\[ TOT = E[Y_i(1) - Y_i(0)|D_i = 1] \]

\[ TNT = E[Y_i(1) - Y_i(0)|D_i = 0] \]
Consider a comparison of average observed earnings for individuals that attend college vs. those that don’t:

\[ E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = E[Y_i(1)|D_i = 1] - E[Y_i(0)|D_i = 0] \]

Add and subtract \( E[Y_i(0)|D_i = 1] \) on the right-hand side:

\[
E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = E[Y_i(1) - Y_i(0)|D_i = 1] \\
+ E[Y_i(0)|D_i = 1] - E[Y_i(0)|D_i = 0]
\]
Treatment Effects and Selection Bias

\[
E [Y_i|D_i = 1] - E [Y_i|D_i = 0] = E[Y_i(1) - Y_i(0)|D_i = 1]
\]

\[
+ E[Y_i(0)|D_i = 1] - E [Y_i(0)|D_i = 0]
\]

This expression decomposes the observed treatment/control difference into the TOT plus a selection bias term given by the difference in average \(Y_i(0)\)'s between treatment and control.

Selection bias arises if the observed outcome for the control group fails to match the missing counterfactual for the treatment group.
Note that we could’ve written

\[ E [Y_i|D_i = 1] - E [Y_i|D_i = 0] = \underbrace{E[Y_i(1) - Y_i(0)|D_i = 0]}_{TNT} \]

\[ + \underbrace{E[Y_i(1)|D_i = 1] - E [Y_i(1)|D_i = 0]}_{Selection \ Bias} \]

Here selection bias arises if the observed outcome for the treatment group fails to match the missing counterfactual for the control group.

Definition of selection bias depends on the question we’re asking – which counterfactual outcome are we trying to impute?
The RCT Ideal

- Suppose the treatment is assigned independently of potential outcomes:

\[(Y_i(1), Y_i(0)) \perp \!\!\!\!\perp D_i\]

- Then

\[
E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = E[Y_i(1)|D_i = 1] - E[Y_i(0)|D_i = 0] \\
= E[Y_i(1)] - E[Y_i(0)] \\
= ATE.
\]

- Assigning treatment randomly as in a randomized controlled trial (RCT) guarantees independence of potential outcomes from treatment

  - Randomization eliminates selection bias
  - Implies treatment/control difference = \(ATE = TOT = TNT\)

- Often the treatment of interest is not randomized. Other research designs aim to isolate comparisons that are as good as random
Human Capital Investments

- Return to the idea of human capital investment
- Start with a simple model of schooling investments, as in Card (1999)
- Individual $i$ chooses duration of schooling $S$ to maximize the present discounted value of earnings:

$$\int_{S}^{\infty} e^{-r_i t} Y_i(S) dt$$

- The potential earnings function $Y_i(S)$ now describes $i$'s potential earnings for every possible schooling level
- Attending $S$ years of school results in zero earnings until $S$, and then $Y_i(S)$ thereafter
- Interest rate $r_i$ determines how $i$ discounts future earnings
Optimal Schooling Choice

- Optimal schooling choice maximizes PDV:

\[ S_i^* = \arg \max_S \int_S^\infty e^{-r_i t} Y_i(S) dt \]

- First-order condition:

\[ \frac{Y_i'(S_i^*)}{Y_i(S_i^*)} = r_i \]

- Marginal benefit/marginal cost formula: at any \( S \), can invest current earnings \( Y_i(S) \) and earn return \( r_i \), or defer earnings to earn more later, with proportional return \( Y_i'(S)/Y_i(S) \)

- Optimal schooling equalizes returns on these two investments

- Individual \( i \)'s realized earnings are \( Y_i(S_i^*) \)
Ability Bias

- Empirical literature tries to estimate features of the potential earnings functions $Y_i(S)$

- Problem: As usual, we only see one earnings level for each person, corresponding to potential earnings at his/her chosen schooling level

- Why do people choose different levels of schooling? In the model differences must be driven either by variation in the discount rate, or in the potential earnings function

- “Ability bias:” Individuals that choose different schooling levels may have different potential earnings functions, leading observed returns to schooling to differ from causal returns

  - Label for selection bias in the returns to schooling context
Observed Returns to Schooling

- Consider an ordinary least squares (OLS) regression of observed earnings $Y_i$ on schooling $S_i$:

$$Y_i = a + bS_i + e_i$$

- The observed return to schooling is the OLS slope coefficient:

$$b = \frac{Cov(Y_i, S_i)}{Var(S_i)}$$

- Question: Should I be worried about whether $S_i$ is correlated with the error term $e_i$?
OLS Approximates the CEF

Answer: No. By definition, the OLS residual $e_i$ is orthogonal to the regressor $S_i$:

$$\text{Cov}(e_i, S_i) = \text{Cov}(Y_i - a - bS_i, S_i)$$

$$= \text{Cov}(Y_i, S_i) - b\text{Var}(S_i)$$

$$= \text{Cov}(Y_i, S_i) - (\text{Cov}(Y_i, S_i) / \text{Var}(S_i)) \text{Var}(S_i)$$

$$= 0.$$ 

OLS always gives a minimum mean squared error approximation to the conditional expectation function (CEF), $E[Y_i|S_i]$:

$$(a, b) = \arg \min_{(a_0, b_0)} E \left[ (E[Y_i|S_i] - a_0 - b_0 S_i)^2 \right].$$

OLS fits the CEF regardless of what model you have in mind. Better to ask: is the CEF economically interesting?
Our equation of interest is
\[ \log y_i = \alpha + \beta S_i + \gamma_1 \text{Exp}_i + \gamma_2 \text{Exp}_i^2 + \epsilon_i \]
A few ... characteristic of individuals of black race. This model typically explains around 30% of the variation in log earnings.

Comparisons of the fitted and actual data suggest that age-earnings profiles of different education groups are roughly parallel. As shown in Fig. 1a, after age 40 the age-earnings profiles of different education groups are roughly parallel.

Mean log wages for each education group (e.g., men with a junior college or Associate's degree) are graphed against the mean number of years of education for the group measured in the CPS. Mean log wages for each education group (e.g., men with a junior college or Associate's degree) are graphed against the mean number of years of education for the group measured in the CPS.

Despite economists' general satisfaction with the traditional measure of schooling, in the late 1980s the U.S. Census Bureau decided to shift toward a degree-based system of measuring post-high-school education (see Kominski and Siegel, 1992). Thus, individuals in the 1990 Census and recent Current Population Surveys were no longer asked how many years of college they had completed: rather they were asked to report their college degree and the new degree-based variable can be constructed from a cross-tabulation of responses to the two questions included in a supplement to the February 1990 CPS. Use of this concordance provides some rather surprising support for the linearity assumption of this concordance. Apart from men who report 11 years of schooling, or 12 years of education, or an Associate's degree in an academic program, denoted by "AA-Academic" in the graph, the fitted values obtained from models like (1/) that include a cubic term in potential experience and include a linear education term, a cubic in experience, and a dummy variable for individuals of black race. This change makes it more difficult to estimate the standard human capital earnings function. Nevertheless, a concordance between the older years-of-education categories and the new degree-based variables is exceptionally high return to the 16th year of schooling). Apart from this feature, Park (1994, 1996) finds that the linear functional form provides a surprisingly good fit to the data.

Fig. 2 shows wage and schooling data for a sample of men age 40-55 in the 1994-1996 Current Population Survey. Mean education by degree category estimated from February 1990 CPS.
Ability Bias

- Consider a constant effects potential earnings function:

\[ Y_i(S) = \alpha_i + \beta S \]

- The causal return \( \beta > 0 \) is the same for all people and schooling levels

- This model implies

\[ \frac{Y_i'(S_i^*)}{Y_i(S_i^*)} = r_i \implies S_i^* = \frac{1}{r_i} - \frac{\alpha_i}{\beta} \]

- Suppose the interest rate \( r_i \) is the same for everyone. Is the observed return to schooling too big or too small relative to the causal return?
Negative Ability Bias

▶ The observed return is too small

▶ When \( r_i = r \) for all \( i \), everyone earns the same amount:

\[
Y_i(S_i^*) = \alpha_i + \beta \left( \frac{1}{r} - \frac{\alpha_i}{\beta} \right) = \frac{\beta}{r}.
\]

▶ The observed return is therefore zero, which is less than the causal return \( \beta \)

▶ Intuition: The primary cost of schooling is the opportunity cost of earnings foregone. Higher-ability people face higher opportunity costs and so drop out earlier

▶ In this case “ability bias” is negative – the causal return exceeds the observed return
More generally, the observed return to schooling is

\[ b = \frac{\text{Cov}(Y_i(S_i^*), S_i^*)}{\text{Var}(S_i)} \]

\[ = \frac{\text{Cov} \left( \frac{\beta}{r_i}, \frac{1}{r_i} - \frac{\alpha_i}{\beta} \right)}{\text{Var} \left( \frac{1}{r} - \frac{\alpha_i}{\beta} \right)} \]

\[ = \beta \times \left( \frac{\sigma^2_{1/r} - \sigma_{\alpha,1/r}/\beta}{\sigma^2_{1/r} - 2\sigma_{\alpha,1/r}/\beta + \sigma^2_\alpha/\beta^2} \right). \]

Ability bias depends on variances and covariances of discount rates and ability across people.

Direction is unclear \textit{a priori}.

To get positive ability bias, need another force that overrides the basic opportunity cost story.

Chris Walters (UC Berkeley) Education and Human Capital
Estimating Causal Returns

- The observed return to schooling may be contaminated by ability bias of unclear sign and magnitude. How can we estimate the causal return?

- Maintain the simple constant-effects model for potential earnings:

  \[ Y_i(S) = \alpha_i + \beta S \]

- We can then write observed earnings as

  \[ Y_i = \bar{\alpha} + \beta S_i + \epsilon_i. \]

- Here \( \bar{\alpha} = E[\alpha_i] \) and \( \epsilon_i = \alpha_i - \bar{\alpha} \)

- Question: Should I be worried about whether \( S_i \) is correlated with the error term \( \epsilon_i \)?
$Y_i = \bar{\alpha} + \beta S_i + \epsilon_i.$

Answer: Yes. The coefficient $\beta$ is now defined as a parameter from a causal (potential outcomes) model, so there is no guarantee that $\text{Cov}(S_i, \epsilon_i) = 0$

Schooling is not randomly assigned, so it may not be independent of potential outcomes, summarized here by $\epsilon_i$

This means the OLS slope coefficient $b$ may not coincide with the causal effect $\beta$

Instrumental variables (IV) is a common research design that seeks to eliminate selection bias in nonexperimental data
Instrumental Variables

\[ Y_i = \bar{\alpha} + \beta S_i + \epsilon_i. \]

- Suppose we have a third variable, \( Z_i \), that satisfies two conditions:
  
  1. **First stage:** \( \text{Cov}(S_i, Z_i) \neq 0 \).
  2. **Exclusion restriction:** \( \text{Cov}(\epsilon_i, Z_i) = 0 \).

- First stage requires \( Z_i \) (the instrument) to be correlated with \( S_i \) (the endogenous variable).

- Exclusion requires the instrument to be uncorrelated with potential outcomes (here, \( \epsilon_i \)).
  - \( Z_i \) must be as good as randomly assigned.
  - \( Z_i \) cannot affect \( Y_i \) through channels other than \( S_i \).
The Population IV Coefficient

- Covariance between outcome and instrument:

\[ \text{Cov}(Y_i, Z_i) = \text{Cov}(\bar{\alpha} + \beta S_i + \epsilon_i, Z_i) \]

\[ = \beta \text{Cov}(S_i, Z_i) + \text{Cov}(\epsilon_i, Z_i) \]

- Exclusion implies the second term is zero, so

\[ \text{Cov}(Y_i, Z_i) = \beta \text{Cov}(S_i, Z_i) \]

- First stage implies \( \text{Cov}(S_i, Z_i) \neq 0 \), so we can divide by this covariance to solve for \( \beta \):

\[ \frac{\text{Cov}(Y_i, Z_i)}{\text{Cov}(S_i, Z_i)} = \beta. \]

- The ratio of covariances on the left is the population instrumental variables coefficient, \( \beta_{IV} \).
Divide the top and bottom of the IV coefficient by $Var(Z_i)$ to obtain:

$$\beta_{IV} = \frac{Cov(Y_i, Z_i)/Var(Z_i)}{Cov(S_i, Z_i)/Var(Z_i)}$$

The IV coefficient is a ratio of two regression coefficients:

- The reduced form regression of $Y_i$ on $Z_i$
- The first stage regression of $S_i$ on $Z_i$

Suppose $Z_i$ is binary. Then the IV coefficient becomes a Wald ratio of two differences in means:

$$\beta_{IV} = \frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[S_i|Z_i = 1] - E[S_i|Z_i = 0]}.$$

- Angrist and Krueger (QJE 1991): classic study reporting IV estimates of the return to education

- Instrumental variables strategy motivated by interaction between compulsory schooling and age-at-entry laws
  - Students can typically drop out of school on the day they turn 16
  - Birth date cutoff for starting age: Students usually start school in the fall of the calendar year in which they turn six

- Generates differences in mean educational attainment by date of birth
To construct their instrument, AK note that children usually begin school in the fall of the calendar year they turn six; some states have an explicit birth date cutoff to determine when a child starts school. In addition, compulsory schooling laws typically allow students to drop out on the day they reach a certain age (often 16). This leads to different compulsory schooling requirements for children born at different times. To see this, consider the following stylized comparison of two individuals, one born early in the year, and one born late:

<table>
<thead>
<tr>
<th>Birth date</th>
<th>School start date</th>
<th>Dropout date</th>
<th>Schooling at dropout date</th>
</tr>
</thead>
<tbody>
<tr>
<td>January 2, 1930</td>
<td>September 1, 1936</td>
<td>January 2, 1946</td>
<td>9.5 years</td>
</tr>
<tr>
<td>December 31, 1930</td>
<td>September 1, 1936</td>
<td>December 31, 1946</td>
<td>10.5 years</td>
</tr>
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</table>

These two children are born at different dates in the same calendar year, but begin school at the same time. They both complete their 9th year of schooling in Spring 1965. The child born January 2 is required to attend the first half of 10th grade, but is then legally allowed to drop out on January 2 of 1966. The child born December 31 is required to attend all of 10th grade, and half of 11th before dropping out. If both plan to drop out as soon as possible, the December child will end up with a full additional year of schooling. This makes instruments based on birth date appealing, since birth date seems likely to be unrelated to other determinants of earnings.

First stage
AK show that mean educational attainment across birth quarters follows a "racheting" pattern consistent with this idea. Individuals born in the first quarter of the year have less education than individuals born in later quarters. (They focus on quarter of birth – "QOB" – because this is the measure of birth date available in the census.) In the language of IV, this figure is a plot of the first stage. AK cite two more facts to argue that these cross-quarter differences are likely due to the compulsory schooling channel:
QOB Instruments

- AK’s instrument is date of birth

- Operationalize using quarter of birth (QOB), which is available in US Census data
  - $Z_i = 1 \{i \text{ was born in first quarter}\}$

- What do the first stage and exclusion restriction assumptions mean for a QOB instrument?
2.2.2 AK Results

Basic argument
To construct their instrument, AK note that children usually begin school in the fall of the calendar year they turn six; some states have an explicit birth date cutoff to determine when a child starts school. In addition, compulsory schooling laws typically allow students to drop out on the day they reach a certain age (often 16). This leads to different compulsory schooling requirements for children born at different times. To see this, consider the following stylized comparison of two individuals, one born early in the year, and one born late:

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AK show that mean educational attainment across birth quarters follows a ratcheting pattern consistent with this idea. Individuals born in the first quarter of the year have less education than individuals born in later quarters. (They focus on quarter of birth – “QOB” – because this is the measure of birth date available in the census.)

In the language of IV, this figure is a plot of the first stage. AK cite two more facts to argue that these cross-quarter differences are likely due to the compulsory schooling channel:

**FIGURE I**
Years of Education and Season of Birth
1980 Census
*Note.* Quarter of birth is listed below each observation.
The return to schooling

AK then look at the relationship between QOB and earnings. Figure 5 shows that log weekly earnings follow a similar ratcheting pattern to years of schooling:

Together with the first stage results in figure 1, this reduced form pattern implies that IV estimates using QOB as an instrument for schooling in a log earnings equation will be positive. The following table shows Wald estimates of the return to education for men:

The IV and OLS estimates are actually very similar. For the 1930-1939 cohort, the IV estimate is above the OLS, though they are not statistically distinguishable.

FIGURE V
Mean Log Weekly Wage, by Quarter of Birth
All Men Born 1930–1949; 1980 Census
The return to schooling

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### Table III


<table>
<thead>
<tr>
<th></th>
<th>(1) Born in 1st quarter of year</th>
<th>(2) Born in 2nd, 3rd, or 4th quarter of year</th>
<th>(3) Difference (std. error)</th>
</tr>
</thead>
<tbody>
<tr>
<td>In (wkly. wage)</td>
<td>5.1484</td>
<td>5.1574</td>
<td>−0.00898 (0.00301)</td>
</tr>
<tr>
<td>Education</td>
<td>11.3996</td>
<td>11.5252</td>
<td>−0.1256 (0.0155)</td>
</tr>
<tr>
<td>Wald est. of return to education</td>
<td>0.0715</td>
<td>0.0639</td>
<td>−0.0076 (0.0219)</td>
</tr>
<tr>
<td>OLS return to education</td>
<td>0.0801</td>
<td>0.0726</td>
<td>0.0075 (0.0004)</td>
</tr>
</tbody>
</table>


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</tr>
</thead>
<tbody>
<tr>
<td>In (wkly. wage)</td>
<td>5.8916</td>
<td>5.9027</td>
<td>−0.01110 (0.00274)</td>
</tr>
<tr>
<td>Education</td>
<td>12.6881</td>
<td>12.7969</td>
<td>−0.1088 (0.0132)</td>
</tr>
<tr>
<td>Wald est. of return to education</td>
<td>0.1020</td>
<td>0.0933</td>
<td>0.0087 (0.0239)</td>
</tr>
<tr>
<td>OLS return to education</td>
<td>0.0709</td>
<td>0.0622</td>
<td>0.0087 (0.0003)</td>
</tr>
</tbody>
</table>

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**Note:** The sample size is 247,199 in Panel A, and 327,509 in Panel B. Each sample consists of males born in the United States who had positive earnings in the year preceding the survey. The 1980 Census sample is drawn from the 5 percent sample, and the 1970 Census sample is from the State, County, and Neighborhoods 1 percent samples.

**Note b:** The OLS return to education was estimated from a bivariate regression of log weekly earnings on years of education.
QOB Interpretation

- IV estimates based on QOB suggest a return to schooling of 7-10% per year
- IV estimates are comparable to or bigger than corresponding OLS estimates
- Card (1999) finds a similar pattern for other IV strategies
- In our simple model, this suggests negative ability bias: people with lower earnings potential attend school for longer
- Other interpretations?
Heterogeneous Treatment Effects

- Our simple model assumed constant effects of schooling across people.
- Return to general potential outcomes model with binary treatment $D_i$ and potential outcomes $Y_i(1)$ and $Y_i(0)$.
- Suppose we have a binary instrument $Z_i$, and consider two new potential outcomes defined by a hypothetical manipulation of $Z_i$:
  - $D_i(1)$: $i$’s treatment status if $Z_i = 1$
  - $D_i(0)$: $i$’s treatment status if $Z_i = 0$
- Observed treatment is $D_i = D_i(0) + (D_i(1) - D_i(0))Z_i$.
IV Assumptions

1. **Independence/exclusion:** \((Y_i(1), Y_i(0), D_i(1), D_i(0)) \perp \perp Z_i\)

2. **First stage:** \(\Pr[D_i = 1|Z_i = 1] > \Pr[D_i = 1|Z_i = 0]\)

3. **Monotonicity:** \(D_i(1) \geq D_i(0) \quad \forall i\)

Relative to our constant effects IV setup, monotonicity is the novel assumption.

Monotonicity requires the instrument to affect everyone’s treatment status in the same direction.
Compliance Groups

Under monotonicity, we can partition the population into three groups defined by their behavioral responses to the instrument (Angrist, Imbens, and Rubin 1996):

1. **Always takers:** $D_i(1) = D_i(0) = 1$
2. **Never takers:** $D_i(1) = D_i(0) = 0$
3. **Compliers:** $D_i(1) = 1, D_i(0) = 0$

Compliers have $D_i(1) > D_i(0)$: their treatment status increases with the instrument

Monotonicity rules out **defiers** with $D_i(1) = 0, D_i(0) = 1$
Under these assumptions, IV identifies a **local average treatment effect** (LATE):

\[
\frac{E [Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)]
\]

This is the LATE theorem of Imbens and Angrist (1994)

LATE is the average treatment effect for compliers – individuals whose treatment status is determined by the instrument.
LATE Theorem: Proof

- Note that $Y_i = Y_i(D_i) = Y_i(D_i(Z_i))$, so by independence

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E[Y_i(D_i(1))|Z_i = 1] - E[Y_i(D_i(0))|Z_i = 0]$$

$$= E[Y_i(D_i(1)) - Y_i(D_i(0))].$$

- By monotonicity we either have $D_i(1) = D_i(0)$ or $D_i(1) > D_i(0)$, so

$$E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0] = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)] Pr[D_i(1) > D_i(0)]$$

- The same logic implies $E[D_i|Z_i = 1] - E[D_i|Z_i = 0] = Pr[D_i(1) > D_i(0)]$, so

$$\frac{E[Y_i|Z_i = 1] - E[Y_i|Z_i = 0]}{E[D_i|Z_i = 1] - E[D_i|Z_i = 0]} = E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0)].$$
Interpreting IV Estimates

- LATE interpretation suggests that QOB instrument identifies the causal effect of extra schooling for individuals on the margin of dropping out early around mid-century.

- Next, we will consider more recent evidence looking at other schooling margins.
Returns to College for Marginal Students: Zimmerman (2014)

- Observed return to college has increased dramatically in recent decades

- College wage premium rose from 50% to 97% between 1980 and 2008 (Acemoglu and Autor, 2011)

- May reflect fast growth of skill demand coupled with slow growth of skill supply (Goldin and Katz, 2008)

- At the same time, many students in the US start college but don’t finish

- 62% of students attending four-year colleges graduate within 6 years (NCES, 2020)

- Does college attendance improve earnings for academically marginal students?

- Zimmerman (JOLE 2014) leverages a regression discontinuity design to study returns for students on the margin of four-year college enrollment
Consider a setting with a binary treatment $D_i \in \{0, 1\}$, and potential outcomes $Y_i(1)$ and $Y_i(0)$.

Suppose the treatment is a deterministic and discontinuous function of an observed covariate $R_i$, such that

$$D_i = 1 \{R_i > c\}.$$

$R_i$ is called the **running variable** or **forcing variable**.

This is a **sharp RD** because the probability of treatment switches from zero to one at the threshold.

Zimmerman (2014): GPA cutoff for admission to state universities in Florida.
Regression Discontinuity Designs

- We get to observe $Y_i(1)$ when $R_i > c$ and $Y_i(0)$ when $R_i \leq c$

- Basic idea of the RD design: Compare observations just above and just below the threshold to infer treatment effect

- Intuitively, the treatment may be as good as randomly assigned for individuals in the neighborhood of $R_i = c$, so comparing treated and nontreated near $c$ reveals a treatment effect
Fig. 1. Assignment probabilities (SRD).

Fig. 2. Potential and observed outcome regression functions.

Source: Imbens and Lemieux (2008)
Key assumption: potential outcomes are smooth at the threshold

Formally:

\[
\lim_{r \to c^+} E [Y_i(d)|R_i = r] = \lim_{r \to c^-} E [Y_i(d)|R_i = r], \quad d \in \{0, 1\}
\]

Potential outcome CEFs must be continuous at the threshold

The population just below must not be discretely different from the population just above
RD Identification

If this assumption holds we have

$$\lim_{r \to c^+} E[Y_i | R_i = r] - \lim_{r \to c^-} E[Y_i | R_i = r]$$

$$= \lim_{r \to c^+} E[Y_i(1) | R_i = r] - \lim_{r \to c^-} E[Y_i(0) | R_i = r]$$

$$= E[Y_i(1) | R_i = c] - E[Y_i(0) | R_i = c]$$

$$= E[Y_i(1) - Y_i(0) | R_i = c]$$

When potential outcomes are smooth around the threshold, a comparison of individuals just above and just below yields the average treatment effect for those at the threshold.

Identification argument is nonparametric: we don’t need to assume anything about the distribution of potential outcomes other than continuity of CEFs.
RD Interpretation

- Core RD intuition: for those near the threshold, things could have gone either way
- Interpret RD as a local randomized trial among those sufficiently close to $R_i = c$
- Explains why RD evidence can be especially compelling relative to other research designs – close to RCT ideal
- “Local randomization” view motivates common RD diagnostics
  - Check balance of pre-determined characteristics for observations above and below the threshold
  - Look for anomalies in the distribution of the running variable around the threshold, which may indicate manipulation (McCrary, 2008)
Sometimes treatment is generated by a discontinuous assignment rule that isn’t deterministic

Suppose that

$$\lim_{r \to c^-} Pr[D_i = 1| R_i = r] < \lim_{r \to c^+} Pr[D_i = 1| R_i = r]$$

The probability of treatment jumps at $R_i = c$, but not necessarily from zero to one

This is a **fuzzy RD** scenario because treatment is only partly determined by the threshold

Zimmerman (2014): Students above GPA cutoff are eligible for admission, but not guaranteed
Fig. 3. Assignment probabilities (FRD).

Fig. 4. Potential and observed outcome regression (FRD).
Fuzzy RD Assumptions

- As before, assume the distributions of $Y_i(1)$ and $Y_i(0)$ are smooth around the threshold.

- Let $D_i(1)$ and $D_i(0)$ denote potential treatment statuses for individual $i$ if s/he were located above and below the threshold. Assume these are also smooth across the threshold, and

$$D_i(1) \geq D_i(0) \ \forall i$$

- Crossing the threshold weakly increases the likelihood of treatment for everyone.
Under these assumptions, we have

\[
\lim_{r \to c^+} E[Y_i|R_i = r] - \lim_{r \to c^-} E[Y_i|R_i = r] \\
\lim_{r \to c^+} E[D_i|R_i = r] - \lim_{r \to c^-} E[D_i|R_i = r]
\]

\[
= E[Y_i(1) - Y_i(0)|D_i(1) > D_i(0), R_i = c]
\]

The numerator on the left is the jump in outcomes at the threshold, as in a sharp RD.

The denominator is the change in the probability of treatment at the threshold.

The ratio of the jump in the outcome CEF to the jump in the treatment probability identifies an average treatment effect for individuals who switch treatment status at the threshold.

Sound familiar?
Fuzzy RD is IV

- Fuzzy RD is IV using a threshold indicator $Z_i = 1 \{R_i > c\}$ as an instrument for treatment in the neighborhood of the threshold.
- Think of fuzzy RD as a local randomized trial with non-compliance.
- Implies fuzzy RD estimates are local in two senses.
  - Local to the threshold, $R_i = c$ (also applies to sharp RD).
  - Only apply to compliers at the threshold (that’s the “local” in LATE).
RD Implementation

- Implementing RD requires estimating the left- and right-hand limits of average outcomes and treatment probabilities.

- Bias/variance tradeoff: using data away from the threshold increases sample size, but may introduce bias if potential outcomes are related to the running variable.

- In practice RD is usually implemented with local linear regression:
  - Regress outcome on the running variable among observations within a small bandwidth of the threshold, with weights that decline with distance to threshold.
  - RD estimate is difference in fitted regression functions above and below the threshold.

- Recent econometric literature proposes optimal bandwidths that balance bias and variance to minimize mean squared error, automated in \textit{rdrobust} Stata command (Imbens and Kalyanaraman, 2011; Calonico et al., 2014).
Zimmerman (2014) uses a GPA cutoff to estimate the returns to four-year college admission at public institutions in Florida. Students above the cutoff are eligible for admission to schools in the Florida State University System (SUS). In practice, the cutoff is relevant for admission to Florida International University (FIU), a large SUS campus in Miami. Population around the FIU admission cutoff has relatively low SAT scores (21st percentile nationwide) and low graduation rates. Estimates are therefore informative about returns to college for marginal students.
the case, the effect on overall SUS attendance (i.e., at attendance any campus) would be less than the effect on FIU attendance. Students affected by threshold-crossing attend state universities with relatively low intensity. Threshold-crossing is associated with an additional 0.644 full-time-equivalent SUS terms, or 1.41 terms per year of SUS attendance. This translates to delayed SUS graduation. As shown in FIG. 4—Admissions and FIU attendance. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.

FIG. 4.—Admissions and FIU attendance. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.
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FIG. 5.—SUS attendance and persistence. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.
figure 6 and panel B of table 4, threshold-crossing has no effect on the probability that students will have graduated from college by 4 or 5 years after high school. However, by 6 years after high school, a 5.7 percentage point gap in SUS graduation has opened up. Note that the $p$-value associated with this gap is 0.13. This corresponds to a 6-year graduation rate of 48%, statistically indistinguishable from the 49% 6-year rate for all FIU students reported in table A1.

Panel C of table 4 presents the effects of threshold-crossing on other academic outcomes. Threshold-crossing substantially reduces community college attendance. Threshold-crossers give up about 0.38 years of CC attendance for each additional year of SUS attendance, and 0.52 full-time-equivalent (FTE) terms of CC attendance for each FTE term of SUS attendance. The ratio of CC to SUS terms is larger in absolute value than the ratio of CC to SUS years because threshold-crossing students often attend SUS part time. Despite reduced CC attendance, there is no evidence that threshold-crossing reduces students' likelihood of receiving a 2-year degree or vocational certificate. Students above the threshold are no less likely to express the intent to attend an out-of-state or in-state private college than students just below the threshold.

C. Earnings Effects

Before turning to regression discontinuity estimates of earnings effects, it is informative to consider how earnings change over time for students above and below the admissions threshold. The left panel of figure 7 displays mean quarterly earnings by year since high school completion for FIG. 6.—SUS BA receipt by years elapsed since high school. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.
Fig. 8.—Quarterly earnings by distance from GPA cutoff. Lines are fitted values based on the main specification. Dots, shown every .05 grade points, are rolling averages of values within .05 grade points on either side that have the same value of the threshold-crossing dummy.
Table 5
Earnings Effects 8–14 Years after High School Completion

<table>
<thead>
<tr>
<th></th>
<th>Main</th>
<th>Controls</th>
<th>BW=.5</th>
<th>BW=.15</th>
<th>Local Linear</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Reduced-form estimates:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Above cutoff</td>
<td>372*</td>
<td>366**</td>
<td>409**</td>
<td>479**</td>
<td>410**</td>
</tr>
<tr>
<td></td>
<td>(141)</td>
<td>(130)</td>
<td>(154)</td>
<td>(198)</td>
<td>(147)</td>
</tr>
<tr>
<td><strong>Instrumental variables estimates:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>FIU admission</td>
<td>1,593*</td>
<td>1,575**</td>
<td>1,665**</td>
<td>1,700**</td>
<td>2,001*</td>
</tr>
<tr>
<td></td>
<td>(604)</td>
<td>(584)</td>
<td>(645)</td>
<td>(621)</td>
<td>(696)</td>
</tr>
<tr>
<td>Years of SUS attendance</td>
<td>815**</td>
<td>792**</td>
<td>833**</td>
<td>966***</td>
<td>977**</td>
</tr>
<tr>
<td></td>
<td>(276)</td>
<td>(262)</td>
<td>(271)</td>
<td>(305)</td>
<td>(306)</td>
</tr>
<tr>
<td>BA degree</td>
<td>6,547*</td>
<td>6,442*</td>
<td>7,366*</td>
<td>10,769</td>
<td>5,958**</td>
</tr>
<tr>
<td></td>
<td>(2,496)</td>
<td>(2,411)</td>
<td>(2,998)</td>
<td>(5,726)</td>
<td>(2,024)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>6,542</td>
<td>6,542</td>
<td>9,659</td>
<td>3,294</td>
<td>6,542</td>
</tr>
</tbody>
</table>

**NOTE.**—FIU = Florida International University; SUS = State University System; BA = bachelor’s degree. Standard errors are clustered within grade bins. The \( p \)-values are calculated using a clustered wild bootstrap-\( t \) procedure described in Sec. III and app. B. The dependent variable in each regression is average quarterly earnings in 2005 dollars. The “BW=.15” specification uses observations within .15 grade points above and below the cutoff and allows for a linear trend in distance from the cutoff. The “BW=.5” specification uses observations within the .5 grade points on either side of the cutoff and allows for a quartic polynomial in distance from the cutoff. The “Local Linear” specification is identical to the main specification, but it allows for linear slope terms in distance from the cutoff that differ above and below the threshold.

* Significant at the 10% level.
** Significant at the 5% level.
*** Significant at the 1% level.
Human Capital vs. Signaling

▶ Evidence so far suggests that education increases earnings

▶ Conventional human capital view is that schooling investments raise earnings by boosting productivity

▶ **Signaling models** (Spence, 1973) provide an alternative explanation for the return to schooling

▶ If employers cannot observe ability, schooling may serve as a costly signal that separates low- and high-ability types, rather than increasing productivity

▶ Implies schooling is pure social waste: burns resources to create inequality

▶ Distinguishing between human capital and signaling views is essential for education policy

▶ Signaling models provide an explanation for “sheepskin effects:” observed return to schooling is especially large for grade 12
Fig. 2. Relationship between mean log hourly wages and completed education, men aged 40–45 in 1994–1996 Current Population Survey. Mean education by degree category estimated from February 1990 CPS.
Clark and Martorell (JPE 2014) use an RD design to estimate the causal effect of high school graduation on earnings.

CM use the fact that students in Texas must pass exams before graduating high school.

Testing starts in 10th grade and students can try multiple times, but eventually face a “last chance” exam at the end of 12th grade.

Students who just barely fail vs. barely pass should have similar human capital, but differ in educational credentials.

RD therefore plausibly identifies the signaling value of a diploma.

There is some “slippage” even with last-chance exams – so the RD is fuzzy.
One possible explanation is that these estimates are biased upward by omitted variables. Omitted variable bias could also explain why other studies find large returns to diploma receipt conditional on

**TABLE 6**

**Associations between Diploma and Test Scores and Earnings**

**A. Mean Differences by Diploma Status**

<table>
<thead>
<tr>
<th></th>
<th>Last-Chance Sample (1)</th>
<th>Complete Grade 12, No College</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>All (2)</td>
</tr>
<tr>
<td>Earnings years 7–11</td>
<td>1,814.7</td>
<td>2,867.8</td>
</tr>
<tr>
<td></td>
<td>(138.1)</td>
<td>(79.3)</td>
</tr>
<tr>
<td>Observations</td>
<td>128,460</td>
<td>992,031</td>
</tr>
<tr>
<td>Mean earnings without diploma</td>
<td>12,400</td>
<td>12,673</td>
</tr>
<tr>
<td>Difference (%)</td>
<td>14.6</td>
<td>22.6</td>
</tr>
<tr>
<td>PDV earnings</td>
<td>8,054.5</td>
<td>8,731.0</td>
</tr>
<tr>
<td></td>
<td>(632.3)</td>
<td>(341.9)</td>
</tr>
<tr>
<td>Observations</td>
<td>37,571</td>
<td>340,028</td>
</tr>
<tr>
<td>Mean earnings without diploma</td>
<td>70,280</td>
<td>69,992</td>
</tr>
<tr>
<td>Difference (%)</td>
<td>11.5</td>
<td>12.5</td>
</tr>
</tbody>
</table>

Note.—Panel A shows mean differences in earnings by high school diploma status. T1, T2, and T3 refer to bottom, second-bottom, and third-bottom tertiles of the ability distribution as measured by initial exam scores. No restrictions are placed on the last-chance sample (i.e., we do not restrict to those with no college). Panel B shows estimates of a regression of PDV earnings through year 11 on a polynomial in the last-chance exam score (each column represents a separate regression) for students in the last-chance sample. All models are estimated with no additional covariates beyond those listed in the table (high school diploma in panel A and the test score polynomial terms in panel B) and an intercept.
student's score to be the minimum of these normalized scores. As such, students pass if and only if this normalized score is nonnegative. The dots are cell means, and the lines are fitted values from a regression of diploma receipt on a fourth-order polynomial in the score estimated separately on either side of the passing cutoff. The fraction of students with a diploma increases sharply as scores cross the passing threshold, from around 0.4 to 0.9. This implies that barely passing the last-chance exam substantially increases the probability of earning a diploma.

A. Main Estimates

We use fuzzy regression discontinuity methods (Angrist and Lavy 1999; Hahn et al. 2001) to exploit this discontinuity. In particular, we use passing status on the last-chance exam as an instrumental variable for diploma receipt in models that control for flexible functions of the exam scores (i.e., the variable on the horizontal axis in fig. 1).

\[ Y_i = b_0 + b_1 D_i + f(p_i) + \varepsilon_i; \]

FIG. 1.—Last-chance exam scores and diploma receipt. The graphs are based on the last-chance sample. See table 1 and the text. Dots are test score cell means. The scores on the \( x \)-axis are the minimum of the section scores (recentered to be zero at the passing cutoff) that are taken in the last-chance exam. Lines are fourth-order polynomials fitted separately on either side of the passing threshold.
tus even in the last-chance sample of students who remain in school until the end of grade 12. We return to this point in our discussion of the findings. Third, there is no indication of any jump in earnings at the passing cutoff.

The estimated discontinuities reported in table 3 are consistent with this last assertion. For each earnings outcome (i.e., for each year grouping), columns 1–4 report estimated discontinuities for first- through fourth-order polynomials, where the polynomials are fully interacted with an indicator for passing the last-chance exam. For each outcome, the estimated discontinuities are small in magnitude, small relative to the mean earnings of those who barely failed the exam (col. 1) and statistically indistinguishable from zero. Moreover, the estimates are robust to the choice of polynomial. Goodness-of-fit statistics suggest that the second-order polynomial is the preferred specification, and column 5 reports estimates from a model that uses this preferred polynomial and controls for baseline covariates. In column 6 we report estimates from a model in which the coefficients of the polynomial are restricted to be the same on either side of the passing cutoff. These estimates are more precise.

**FIG. 2.**—Earnings by last-chance exam scores. The graphs are based on the last-chance samples. See table 1 and the text. Dots are test score cell means. The scores on the x-axis are the minimum of the section scores (recentered to be zero at the passing cutoff) that are taken in the last-chance exam. Lines are fourth-order polynomials fitted separately on either side of the passing threshold.
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Returns to College Selectivity

- For many students the relevant choice margin is which college to attend rather than years of schooling or college vs. no college

- Very large differences in earnings between students attending different US colleges

- But there is also a lot of selection into college choice
FIGURE II: Children's Income Ranks by Age of Income Measurement

A. Mean Income Rank by Age and College Tier

<table>
<thead>
<tr>
<th>Age of Income Measurement</th>
<th>Ivy Plus</th>
<th>Other Elite</th>
<th>Other Four-Year</th>
<th>Two-Year</th>
<th>Cannot Link Children to Parents</th>
</tr>
</thead>
<tbody>
<tr>
<td>25</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>27</td>
<td></td>
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<td></td>
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<td>29</td>
<td></td>
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<td>31</td>
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<tr>
<td>35</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

B. Correlation of College Mean Income Rank across Ages

<table>
<thead>
<tr>
<th>Year-on-Year Correlation</th>
<th>0.80</th>
<th>0.85</th>
<th>0.90</th>
<th>0.95</th>
<th>1.00</th>
</tr>
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<tbody>
<tr>
<td>Age</td>
<td>25</td>
<td>27</td>
<td>29</td>
<td>31</td>
<td>33</td>
</tr>
</tbody>
</table>

Notes: Panel A plots the mean income rank by age for students who attended colleges in various tiers. Children's earnings are measured as the sum of individual wage earnings and self-employment income. We measure children's incomes at each age 25-36 and then assign percentile ranks based on their position in age-specific distribution of incomes for children born in the same birth cohort. "Ivy-Plus" includes the Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University. "Other Elite" is defined using all other colleges (excluding the Ivy-Plus group) classified as "Most Competitive" (Category 1) by Barron's Profiles of American Colleges (2009). "Other 4-Year" includes all other 4 year institutions excluding the "Ivy plus" and "Other Elite" groups, measured based on highest degree offered by the institution as recorded in IPEDS (2013). "2-Year" includes all two-year institutions. Panel B plots the (enrollment-weighted) correlation between the college-level mean rank at age 36 and the college-level mean rank at ages 25-36. The sample for both panels of this figure comprise the 1978 birth cohort, with individuals assigned to the college they were attending at age 22. Note that children cannot be linked to parents before the 1980 birth cohort.

Source: Chetty et al. (2020)
FIGURE I: Distributions of Parent Income by College

A. Parental Income Distribution at Selected Colleges

- Top 1%
- Percent of Students
- Parent Income Quintile
  - Harvard University
  - UC Berkeley
  - SUNY-Stony Brook
  - Glendale Community College

B. Parental Income Distribution at Ivy-Plus Colleges

- 3.7% of students from bottom 20%
- 14.2% of students from top 1%

C. Distribution of Bottom-Quintile Share Across Colleges

- p10 = 3.7%
- p50 = 9.3%
- p90 = 21.0%
- SD(Pct. of Parents in Q1) = 7.6%

Notes: This figure presents the distribution of parent incomes for children in the 1980-1982 birth cohorts. Panel A plots the percentage of students with parents in each income quintile at Harvard University, University of California at Berkeley, State University of New York at Stony Brook, and Glendale Community College, as well as the percentage of students with parents in the top income percentile for each school. Panel B plots the percentage of students with parents in each income percentile across all Ivy-Plus colleges, which include the eight Ivy-League colleges as well as the University of Chicago, Stanford University, MIT, and Duke University. Panel C plots the (enrollment-weighted) distribution of the fraction of children with parents in the lowest income quintile across all colleges. Parent income is defined as mean pre-tax Adjusted Gross Income in 2015 dollars during the period in which the child was ages 15-19. Parent income percentiles are constructed using the parents' rank in the national income distribution among parents with a child in the same birth cohort. Children are assigned to colleges using the college that they attended for the most years between ages 19 and 22, breaking ties by taking the college which a child first attends.
Hard to find good experiments and quasi-experiments that induce variation in attendance at more vs. less selective colleges

Dale and Krueger (2002, 2014) use a matching approach that compares outcomes for students who applied and were admitted to the same sets of colleges, but attended different schools

Based on a selection on observables assumption: college choice is independent of potential outcomes conditional on a set of observed covariates
Potential Outcomes Model

▶ Return to our causal model with binary treatment $D_i \in \{0, 1\}$ and potential outcomes $Y_i(1)$ and $Y_i(0)$

▶ Suppose treatment isn’t randomly assigned

▶ As we’ve seen, the observed difference between average outcomes for individuals with $D_i = 1$ and $D_i = 0$ may be contaminated by selection bias

▶ Suppose we also have data on a vector of observed covariates $X_i$

▶ Dale and Krueger: $D_i$ is attending a more selective college, and $X_i$ is the lists of colleges where a student applied and was admitted
Selection on Observables

- Selection-on-observables approaches are based on a **conditional independence assumption** (CIA):

\[(Y_i(1), Y_i(0)) \perp\!\!\!\!\perp D_i | X_i\]

- CIA is also called “unconfoundedness,” “ignorability,” “exogeneity”

- The idea is that while potential outcomes and treatment may not be independent in general, they are independent conditional on a set of observed covariates - treatment is as good as random conditional on \(X_i\)

- CIA necessarily holds in stratified RCTs, and may hold in non-experimental data with the right controls
Full Covariate Matching

- Under CIA an obvious approach is to simply compare treatment and control groups conditional on the covariates.

- Let $\Delta(x)$ denote the observed treatment/control difference for a particular value of the covariates:

$$\Delta(x) \equiv E[Y_i|D_i = 1, X_i = x] - E[Y_i|D_i = 0, X_i = x]$$

- CIA implies

$$\Delta(x) = E[Y_i(1)|D_i = 1, X_i = x] - E[Y_i(0)|D_i = 0, X_i = x]$$

$$= E[Y_i(1) - Y_i(0)|X_i = x]$$

$$\equiv ATE(x).$$

- Covariate-specific treatment/control contrasts capture conditional average treatment effects.

- By computing $\Delta(x)$ for every value of $x$ and then weighting appropriately, we can obtain any causal effect of interest. This is **full covariate matching**.
Under CIA, we can use full covariate matching to compute average treatment effects:

\[
ATE = \sum_x Pr [X_i = x] \Delta(x)
\]

\[
TOT = \sum_x Pr [X_i = x | D_i = 1] \Delta(x)
\]

\[
TNT = \sum_x Pr [X_i = x | D_i = 0] \Delta(x)
\]
OLS Regression as Matching

Consider an OLS regression of outcomes on a treatment indicator, controlling for indicators for every value of the covariates $X_i$:

$$Y_i = a + bD_i + \sum_x \pi_x 1\{X_i = x\} + e_i$$

This regression is **saturated** in the controls: there is a different coefficient for every value of $X_i$.

With saturated controls, the OLS coefficient is

$$b = \sum_x \left( \frac{\Pr[X_i=x] \Var(S_i|X_i=x)}{\sum_{x'} \Pr[X_i=x'] \Var(S_i|X_i=x')} \right) \Delta(x).$$

OLS with saturated controls is a version of full covariate matching.

- “Saturate-and-weight” theorem (Angrist and Pischke, 2009)
- Under CIA, generates a variance-weighted average treatment effect
CIA Methods

▶ In practice, full covariate matching may not be feasible (e.g. many-valued or continuous controls)

▶ There are a variety of approaches to controlling for $X_i$ in such cases:
  
  ▶ OLS with additive controls
  ▶ Nearest-neighbor or kernel matching
  ▶ Propensity score matching/reweighting

▶ These methods are not qualitatively different
  
  ▶ All are approaches to adjusting for covariates
  ▶ Coincide when the controls are flexible enough

▶ Key to the research design is the underlying CIA assumption, not the particular method used to control for $X_i$
Returns to College Selectivity: Dale and Krueger

- Dale and Krueger (QJE 2002, JHR 2014) take a matching/selection on observables approach to estimating the returns to college selectivity

- Research design: compare students who applied to, and were admitted by, the same colleges, but chose to attend different schools

- Intuition: Application choices capture a lot of students’ information about their own ability, while admission decisions capture a lot of colleges’ information about student ability

- Data: College and Beyond (C&B)
  - Survey of students enrolled at 34 colleges, more selective than the US average
  - 2014 paper matches C&B to administrative earnings data from the Social Security Administration (SSA)
### TABLE I
**ILLUSTRATION OF HOW MATCHED-APPLICANT GROUPS WERE CONSTRUCTED**

<table>
<thead>
<tr>
<th>Student</th>
<th>Matched-applicant group</th>
<th>Application 1</th>
<th>Application 2</th>
<th>Application 3</th>
<th>Application 4</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>School average SAT</td>
<td>School admissions decision</td>
<td>School average SAT</td>
<td>School admissions decision</td>
</tr>
<tr>
<td>Student A</td>
<td>1</td>
<td>1280</td>
<td>Reject</td>
<td>1226</td>
<td>Accept*</td>
</tr>
<tr>
<td>Student B</td>
<td>1</td>
<td>1280</td>
<td>Reject</td>
<td>1226</td>
<td>Accept</td>
</tr>
<tr>
<td>Student C</td>
<td>2</td>
<td>1360</td>
<td>Accept</td>
<td>1310</td>
<td>Reject</td>
</tr>
<tr>
<td>Student D</td>
<td>2</td>
<td>1355</td>
<td>Accept</td>
<td>1316</td>
<td>Reject</td>
</tr>
<tr>
<td>Student E</td>
<td>2</td>
<td>1370</td>
<td>Accept*</td>
<td>1316</td>
<td>Reject</td>
</tr>
<tr>
<td>Student F</td>
<td>Excluded</td>
<td>1180</td>
<td>Accept*</td>
<td>na</td>
<td>na</td>
</tr>
<tr>
<td>Student G</td>
<td>Excluded</td>
<td>1180</td>
<td>Accept*</td>
<td>na</td>
<td>na</td>
</tr>
<tr>
<td>Student H</td>
<td>3</td>
<td>1360</td>
<td>Accept</td>
<td>1308</td>
<td>Accept*</td>
</tr>
<tr>
<td>Student I</td>
<td>3</td>
<td>1370</td>
<td>Accept*</td>
<td>1311</td>
<td>Accept</td>
</tr>
<tr>
<td>Student J</td>
<td>3</td>
<td>1350</td>
<td>Accept</td>
<td>1316</td>
<td>Accept*</td>
</tr>
<tr>
<td>Student K</td>
<td>4</td>
<td>1245</td>
<td>Reject</td>
<td>1217</td>
<td>Reject</td>
</tr>
<tr>
<td>Student L</td>
<td>4</td>
<td>1235</td>
<td>Reject</td>
<td>1209</td>
<td>Reject</td>
</tr>
<tr>
<td>Student M</td>
<td>5</td>
<td>1140</td>
<td>Accept</td>
<td>1055</td>
<td>Accept*</td>
</tr>
<tr>
<td>Student N</td>
<td>5</td>
<td>1145</td>
<td>Accept*</td>
<td>1060</td>
<td>Accept</td>
</tr>
<tr>
<td>Student O</td>
<td>No match</td>
<td>1370</td>
<td>Reject</td>
<td>1038</td>
<td>Accept*</td>
</tr>
</tbody>
</table>

* Denotes school attended.
na = did not report submitting application.

The data shown on this table represent hypothetical students. Students F and G would be excluded from the matched-applicant subsample because they applied to only one school (the school they attended). Student O would be excluded because no other student applied to an equivalent set of institutions.
### TABLE V
LINEAR REGRESSIONS PREDICTING WHETHER STUDENT ATTENDED MOST SELECTIVE COLLEGE FOR C&B SAMPLE OF STUDENTS ADMITTED TO MORE THAN ONE SCHOOL

<table>
<thead>
<tr>
<th>Parameter estimates</th>
<th>Matched-applicant model*</th>
<th>Self-revelation model</th>
</tr>
</thead>
<tbody>
<tr>
<td>Predicted log (parental income)</td>
<td>-0.024</td>
<td>-0.037</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.030)</td>
</tr>
<tr>
<td>Own SAT score/100</td>
<td>0.020</td>
<td>0.021</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Female</td>
<td>0.034</td>
<td>0.033</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Black</td>
<td>0.056</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>-0.019</td>
<td>0.042</td>
</tr>
<tr>
<td></td>
<td>(0.064)</td>
<td>(0.074)</td>
</tr>
<tr>
<td>Asian</td>
<td>0.019</td>
<td>0.074</td>
</tr>
<tr>
<td></td>
<td>(0.026)</td>
<td>(0.050)</td>
</tr>
<tr>
<td>Other/missing race</td>
<td>-0.095</td>
<td>0.010</td>
</tr>
<tr>
<td></td>
<td>(0.093)</td>
<td>(0.081)</td>
</tr>
<tr>
<td>High school top 10 percent</td>
<td>-0.014</td>
<td>-0.020</td>
</tr>
<tr>
<td></td>
<td>(0.021)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>High school rank missing</td>
<td>-0.035</td>
<td>-0.040</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.058)</td>
</tr>
<tr>
<td>Athlete</td>
<td>0.056</td>
<td>0.059</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.045)</td>
</tr>
<tr>
<td>Average SAT score/100 of schools applied to</td>
<td>-0.122</td>
<td>0.149</td>
</tr>
<tr>
<td></td>
<td>(0.040)</td>
<td>(0.037)</td>
</tr>
<tr>
<td>One additional application</td>
<td>0.076</td>
<td>0.020</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Two additional applications</td>
<td>0.020</td>
<td>0.020</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>Three additional applications</td>
<td>0.020</td>
<td>0.020</td>
</tr>
<tr>
<td></td>
<td>(0.038)</td>
<td>(0.038)</td>
</tr>
<tr>
<td>N</td>
<td>5536</td>
<td>8257</td>
</tr>
</tbody>
</table>

Only students who were accepted by more than one school are included in the sample. Each equation also includes a constant term. Standard errors are in parentheses, and are robust to correlated errors among students who attended the same institution. Equations are estimated by WLS; weights are designed to make the sample representative of the population of students at the C&B institutions.

* Applicants are matched by the average SAT score (within 25 point intervals) of each school at which they were accepted and rejected. Model includes 1,079 dummy variables indicating each set of matched applicants.
### TABLE III
LOG EARNINGS REGRESSIONS USING COLLEGE AND BEYOND SURVEY, SAMPLE OF MALE AND FEMALE FULL-TIME WORKERS

<table>
<thead>
<tr>
<th>Model</th>
<th>Basic model: no selection controls</th>
<th>Matched-applicant model</th>
<th>Alternative matched-applicant models</th>
<th>Self-revelation model</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Full sample</td>
<td>Restricted sample</td>
<td>Similar school-SAT matches*</td>
<td>Exact school-SAT matches**</td>
</tr>
<tr>
<td>Variable</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>School-average SAT score/100</td>
<td>0.076</td>
<td>0.082</td>
<td>-0.016</td>
<td>-0.106</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.014)</td>
<td>(0.022)</td>
<td>(0.036)</td>
</tr>
<tr>
<td>Predicted log(parental income)</td>
<td>0.187</td>
<td>0.190</td>
<td>0.163</td>
<td>0.232</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.033)</td>
<td>(0.033)</td>
<td>(0.079)</td>
</tr>
<tr>
<td>Own SAT score/100</td>
<td>0.018</td>
<td>0.006</td>
<td>-0.011</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.014)</td>
</tr>
<tr>
<td>Female</td>
<td>-0.403</td>
<td>-0.410</td>
<td>-0.395</td>
<td>-0.476</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.018)</td>
<td>(0.024)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Black</td>
<td>-0.023</td>
<td>-0.026</td>
<td>-0.057</td>
<td>-0.028</td>
</tr>
<tr>
<td></td>
<td>(0.035)</td>
<td>(0.053)</td>
<td>(0.053)</td>
<td>(0.049)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>0.015</td>
<td>0.070</td>
<td>0.020</td>
<td>-0.248</td>
</tr>
<tr>
<td></td>
<td>(0.052)</td>
<td>(0.076)</td>
<td>(0.099)</td>
<td>(0.206)</td>
</tr>
<tr>
<td>Asian</td>
<td>0.173</td>
<td>0.246</td>
<td>0.241</td>
<td>0.368</td>
</tr>
<tr>
<td></td>
<td>(0.036)</td>
<td>(0.054)</td>
<td>(0.064)</td>
<td>(0.141)</td>
</tr>
<tr>
<td>Other/missing race</td>
<td>-0.188</td>
<td>-0.048</td>
<td>0.060</td>
<td>-0.072</td>
</tr>
<tr>
<td></td>
<td>(0.119)</td>
<td>(0.143)</td>
<td>(0.160)</td>
<td>(0.083)</td>
</tr>
<tr>
<td>High school top 10 percent</td>
<td>0.061</td>
<td>0.091</td>
<td>0.079</td>
<td>0.091</td>
</tr>
<tr>
<td></td>
<td>(0.018)</td>
<td>(0.022)</td>
<td>(0.026)</td>
<td>(0.032)</td>
</tr>
<tr>
<td>High school rank missing</td>
<td>0.001</td>
<td>0.040</td>
<td>0.016</td>
<td>0.029</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.026)</td>
<td>(0.038)</td>
<td>(0.066)</td>
</tr>
<tr>
<td>Athlete</td>
<td>0.102</td>
<td>0.088</td>
<td>0.104</td>
<td>0.169</td>
</tr>
<tr>
<td></td>
<td>(0.025)</td>
<td>(0.030)</td>
<td>(0.039)</td>
<td>(0.096)</td>
</tr>
<tr>
<td>Average SAT score/100 of schools applied to</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>One additional application</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Two additional applications</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Three additional applications</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Four additional applications</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Adjusted $R^2$</td>
<td>0.107</td>
<td>0.110</td>
<td>0.112</td>
<td>0.142</td>
</tr>
<tr>
<td>N</td>
<td>14,238</td>
<td>6,335</td>
<td>6,335</td>
<td>2,330</td>
</tr>
</tbody>
</table>
### Table 3
Comparing Parameter Estimates of the Effect of College Average SAT Score on Earnings Using C&B and SSA Data, 1976 Cohort

<table>
<thead>
<tr>
<th></th>
<th>C&amp;B sample&lt;sup&gt;a&lt;/sup&gt;</th>
<th></th>
<th>Merged C&amp;B and SSA sample&lt;sup&gt;b&lt;/sup&gt;</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Basic</td>
<td>Self–revelation</td>
<td>Basic</td>
<td>Self–revelation</td>
</tr>
<tr>
<td>Parameter estimate for school SAT/100</td>
<td>0.076 (.008)</td>
<td>-0.001 (.012)</td>
<td>0.068 (.007)</td>
<td>-0.007 (.012)</td>
</tr>
<tr>
<td></td>
<td>{.016} 14,238</td>
<td></td>
<td>0.058 (.009)</td>
<td>-0.015 (.015)</td>
</tr>
<tr>
<td>Sample restriction</td>
<td>Full-time workers</td>
<td>(according to C&amp;B survey)</td>
<td>Full-time workers</td>
<td>(according to C&amp;B survey)</td>
</tr>
<tr>
<td></td>
<td>Median earnings</td>
<td>greater than zero</td>
<td>Median earnings</td>
<td>greater than zero</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(SSA data)</td>
<td>(SSA data)</td>
<td>(SSA data)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>10,886 11,932</td>
<td>10,886 11,932</td>
<td>10,886 11,932</td>
</tr>
</tbody>
</table>
Table 8

**Effect of School Characteristics on 2007 Earnings (Black and Hispanic Students Only, 1989 Cohort)**

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>School SAT score/100</th>
<th>Log net tuition</th>
<th>Barron’s index</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Basic</td>
<td>Self-revelation</td>
<td>Basic</td>
</tr>
<tr>
<td>All black and Hispanic students</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parameter estimate for effect of quality measure on log 2007 earnings</td>
<td>0.067</td>
<td>0.076</td>
<td>0.173</td>
</tr>
<tr>
<td>Sample size</td>
<td>1,508</td>
<td></td>
<td></td>
</tr>
<tr>
<td>All black and Hispanic students, excluding historically black colleges and universities</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Parameter estimate for effect of quality measure on log 2007 earnings</td>
<td>0.122</td>
<td>0.120</td>
<td>0.187</td>
</tr>
<tr>
<td>Sample size</td>
<td>995</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Source: C&B Survey and Social Security Administration's Detailed Earnings Records.

Notes: Parameter estimates drawn from weighted least squares regression models. Weights were used to make the sample representative of the population of students at C&B schools. Both the basic and self-revelation models control for race, sex, predicted parental income, student SAT score, student high school grade point average, dummy variables indicating if high school grade point average or student SAT score was missing, and whether the student was a college athlete; the self-revelation model also controls for the average SAT score of the schools to which the student applied and dummies for the number of applications the student submitted. Two sets of standard errors are reported, one in parentheses and one in brackets. Standard errors in brackets are robust to correlated errors among students who attended the same institution. The Barron’s measure is coded as a continuous measure, ranging from 2 (Competitive colleges) to 5 (Most Competitive colleges) for our sample. Individuals are excluded if they earned less than $13,822 in 2007.
A recent paper by Mountjoy and Hickman (2020) updates the Dale/Krueger strategy using administrative data from Texas.

Rather than looking at overall return to selectivity, estimate a “value-added” model with a different effect for every college, conditioning on DK application/admission controls.

Relate college value-added to selectivity and other institution characteristics.

Consistent with DK, Mountjoy and Hickman find limited returns to selectivity.

Estimated college value-added is positively correlated with other inputs like instructional expenditures and faculty/student ratio.
Figure 3: Validating the Matched Applicant Approach: Ability Balance across College Treatments

- 10th Grade Standardized Math Score
- Balance Relative to UT-Austin (UT-A Mean = .68)

UT-Dallas
TAMU
UT-Tyler
Texas Tech
Houston
TAMU-Galveston
UT-Arlington
Texas State-San Marcos
North Texas
West Texas A&M
Sam Houston State
TAMU-Commerce
UT-Permian Basin
Stephen F. Austin State
TAMU-International
Texas Woman's
UT-El Paso
UT-Pan American
TAMU-Kingsville
Sul Ross State
Houston-Downtown
Prairie View A&M
Texas Southern

-2 -1.5 -1 -0.5 0

Raw Means Baseline Specification:
Admission Portfolio FEs Only

Notes: Each set of point estimates and robust 95% confidence intervals come from regressions of individual student standardized test scores on college treatment indicators, omitting UT-Austin as the reference treatment (signified by the vertical line at zero). The UT-Austin mean appears in parentheses below each plot. The Raw Means specification controls only for cohort fixed effects. The Baseline Specification controls solely for college admission portfolio fixed effects (and cohort fixed effects). See Appendix Table B.3 for the corresponding numerical estimates.
Figure 2: Baseline Value-Added Estimates and Comparison to Other Approaches

Raw Means Typical Controls Baseline Specification:
Admission Portfolio FEs Only

Notes:
Each set of point estimates and robust 95% confidence intervals come from regressions of individual student outcomes on college treatment indicators, omitting UT-Austin as the reference treatment (signified by the vertical line at zero). The UT-Austin outcome mean appears in parentheses below each plot. All specifications control for cohort fixed effects. The Raw Means specification controls for nothing else. The Typical Controls specification adds controls for demographics (gender, race, FRPL), high school academic preparation (10th grade test scores, advanced coursework, and top high school GPA decile indicator), and behavioral measures of non-cognitive skills (high-school attendance, disciplinary infractions, and an indicator for ever being at risk of dropping out). The Baseline Specification controls solely for college admission portfolio fixed effects (and cohort fixed effects). See Appendix Tables B.1 and B.2 for the corresponding numerical estimates.
Figure 5: Predicting Raw Mean Earnings vs. Causal Value-Added with Selectivity

Notes: The top panel plots raw mean earnings at each college, relative to UT-Austin, against the average SAT score of incoming students at each college. The bottom panel replaces the vertical axis with our main value-added estimates from Section 4. Correlations, regression slopes, and circle sizes are weighted by student enrollment.

Correlation = .93
Slope = 64
Figure 5: Predicting Raw Mean Earnings vs. Causal Value-Added with Selectivity

Notes: The top panel plots raw mean earnings at each college, relative to UT-Austin, against the average SAT score of incoming students at each college. The bottom panel replaces the vertical axis with our main value-added estimates from Section 4. Correlations, regression slopes, and circle sizes are weighted by student enrollment.

Correlation = -.039
Slope = -.51
Figure 7: Early Career Dynamics of the Return to College Selectivity

[-.05 0 .05 .1 .15]
Log Earnings
Effect of Attending a More Selective College
(100-Point Higher Mean SAT)
4 5 6 7 8 9 10
Years Since College Entry

Notes: Each point estimate and robust 95% confidence interval comes from a separate regression of individual log student earnings, measured at a given number of years since college entry, on the mean incoming SAT score of the student's college. The coefficients are scaled to correspond to a 100-point increase in mean SAT scores. All specifications control for cohort fixed effects. The Raw Specification controls for nothing else. The Main Specification controls for college admission portfolio fixed effects and our core set of covariate controls: gender, FRPL, race, 10th grade test scores, and high school attendance.

Students who attend higher "quality" colleges. A majority of papers in this vein—e.g., Brewer et al. (1999), Black and Smith (2006), Long (2008), and Dillon and Smith (2018)—have found consistent evidence for strong returns, while Dale and Krueger (2002, 2014) stand out as a notable exception. Our results highlight the importance of carefully defining college "quality," and provide some insight on why different strands of the literature have reached different conclusions. While we confirm Dale and Krueger (2002, 2014)'s main conclusion that college selectivity per se yields no meaningful economic returns (beyond a short-lived initial premium), this does not imply that college-level value-added differentials—a more flexible notion of quality—are absent. Rather, we document variation in causal value-added across colleges, and simply show that selectivity is orthogonal to it. More broadly, by letting each college have its own unique impact on student outcomes, our value-added approach lets the data determine the ordering of the quality space, rather than imposing an ex-ante ordering based on a single-dimensional college observable.

To conclude our results on selectivity, Figure 8 explores two additional outcomes: completing any BA, and completing a BA in a STEM field. The two left panels show that, similar to earnings, raw BA completion rates exhibit a very strong correlation with selectivity (top left), but this relationship weakens dramatically when replacing raw outcomes with causal value-added (bottom left). A 100-point increase

Black and Smith (2006) emphasize the need for multiple proxies to mitigate measurement error in univariate constructions of college quality. In the following subsection we show that our causal value-added estimates do covary with non-peer college inputs, while at the same time, they do not correspond perfectly to any one-dimensional observable college covariates.

Figure 7: Early Career Dynamics of the Return to College Selectivity
The top left panel plots earnings value-added against an index of non-peer inputs, constructed as the predicted factor from an enrollment-weighted one-factor model of instructional expenditures, full-time faculty share, tenure-track faculty share, and faculty-student ratio. The top right panel plots this non-peer inputs index against the average incoming SAT score at each college. The bottom left panel plots earnings value-added against the residuals of a college-level regression of the non-peer inputs index on average SAT scores. Likewise, the bottom right panel plots earnings value-added against the residuals of a college-level regression of average SAT scores on the non-peer inputs index. All correlations, regressions, and circles are weighted by student enrollment.

Variation. The bottom left plot shows that the positive relationship between value-added and non-peer inputs strengthens appreciably when controlling for selectivity: the partial correlation jumps to 0.67, and the slope nearly doubles to $1,438$ in extra predicted value-added for each standard deviation increase in non-peer inputs, residualized on selectivity. The bottom right panel shows the joint implication of this result and those from the previous subsection: selectivity, residualized on non-peer inputs, is actually a negative predictor of earnings value-added, perhaps reflecting a within-campus competition channel that becomes more apparent when comparing colleges with different peer composition but similar non-peer resources. These results further challenge popular notions of measuring college “quality” as positively-weighted indices of peer and non-peer inputs: although peer and non-peer inputs broadly move together, they each offer contrasting partial correlations with causal college value-added.
References


References


AEA Continuing Education 2021:
Labor Economics and Applied Econometrics

Lecture 2 - Self-Selection

Chris Walters

University of California, Berkeley and NBER
Self-selection

- Classic idea in labor economics: Self-selection
- Individuals choose between opportunities based on heterogeneous unobserved returns
- We’ve already encountered some versions of this in the context of the returns to schooling
- Applications: educational choice, occupational choice, labor force participation, immigration
- We’ll start with general discussion of self-selection models and related econometrics, then look at some applications
Roy Model

- Roy (1951) sought to understand the influence of occupational choice on the observed distribution of earnings.

- Consider individuals indexed by $i$ choosing a binary variable $D_i \in \{0, 1\}$ indicating occupation, e.g. hunting vs. fishing.

- $Y_{i1}$ and $Y_{i0}$ are $i$’s potential earnings associated with each choice.

- Realized earnings are $Y_i = Y_{i0} + (Y_{i1} - Y_{i0})D_i$.

- Pure Roy (1951) model: Individuals want to maximize $Y_i$, so choose the occupation with the best potential outcome:

$$D_i = 1 \{ Y_{i1} > Y_{i0} \}$$
Questions of interest:

- Will the best hunters hunt?
- Will the best fishermen/women fish?

Suppose potential earnings are given by

$$Y_{id} = p_d S_{id}, \ d \in \{0, 1\}$$

- $S_{id}$ is skill in occupation $d$, and $p_d$ is price of output
- A worker who is indifferent between the two occupations satisfies

$$\log S_{i1} = \log p_0 - \log p_1 + \log S_{i0}$$
Choose $D=1$

Choose $D=0$

$logS1 = logp1 - logp0 + logS0$
Roy Model: Special Cases

- Suppose there is no variation in potential earnings in sector 0, so $S_{i0} = \bar{S}_0$ ∀i.

- In this case the decision rule is

$$D_i = 1 \left\{ S_{i1} \geq \left( \frac{p_0}{p_1} \right) \bar{S}_0 \right\}$$

- Those with the most skill in sector 1 choose $D_i = 1$.

- Everyone with $D_i = 1$ earns more than anyone with $D_i = 0$. 
Choose $D=1$

Choose $D=0$

$logS1 = logp1 - logp0 + logS0$
Roy Model: Special Cases

- Suppose we have perfect correlation between \( \log S_{i0} \) and \( \log S_{i1} \):

\[
\log S_{i1} = \alpha_0 + \alpha_1 \log S_{i0}, \quad \alpha_1 > 0
\]

- This is a one-factor model

- Decision rule:

\[
D_i = 1 \left\{ \alpha_0 + \alpha_1 \log S_{i0} \geq \log p_0 - \log p_1 + \log S_{i0} \right\}
\]

\[
= 1 \left\{ (\alpha_1 - 1) \log S_{i0} \geq \log p_0 - \log p_1 - \alpha_0 \right\}
\]

- Higher skilled choose \( D_i = 1 \) iff \( \alpha_1 \geq 1 \)

- Note that \( \text{Var}(\log S_{i1}) = \alpha_1^2 \text{Var}(\log S_{i0}) \). Higher skilled choose the sector with higher variance of skill
\[ \alpha_1 = 2 \]

Choose D=1

Choose D=0

\[ \log S_1 = \log p_1 - \log p_0 + \log S_0 \]
Choose $D=1$

Choose $D=0$

$\alpha_1 = 0.3$

$logS_1 = logp_1 - logp_0 + logS_0$

---

$logS_1 = logp_1 - logp_0 + logS_0$
Generalized Roy Model

- Generalized Roy model (Eisenhauer et al., 2015): Preference for alternative $d$ depends on $Y_{id}$ as well as a heterogeneous cost $C_{id}$:

$$D_i = 1 \{ Y_{i1} - C_{i1} > Y_{i0} - C_{i0} \}$$

- Allows us to ask richer questions about selection on both levels and gains
  - Is average $Y_{i1}$ higher or lower for individuals that choose $D_i = 1$?
  - Is average $Y_{i0}$ higher or lower for individuals that choose $D_i = 1$?
  - Are average gains $Y_{i1} - Y_{i0}$ larger or smaller for individuals that choose $D_i = 1$?

- Close link between generalized Roy model and econometric models of treatment effect heterogeneity
Simple example of a selection model: Labor supply problem

$$\max_{c, h} c - v(h) \quad \text{s.t.} \quad c \leq wh + V$$

At interior solutions:

$$v'(h^*) = w$$

At corner solutions:

$$v'(0) > w$$

Reservation wage is $$w^* = v'(0)$$; work if $$w \geq w^*$$
Labor Supply Selection

- Suppose individuals' reservation wages are described by
  \[ w^*_i = X'_i \theta + \eta_i \]

- Offered wages are
  \[ w_i = X'_i \beta + \epsilon_i \]

- Assume \( E[\eta_i|X_i] = E[\epsilon_i|X_i] = 0 \), so \( X'_i \theta \) and \( X'_i \beta \) are population CEFs

- Individual \( i \) works (\( D_i = 1 \)) when
  \[ X'_i \beta + \epsilon_i \geq X'_i \theta + \eta_i \]
  \[ \iff X'_i (\beta - \theta) + (\epsilon_i - \eta_i) \geq 0 \]
  \[ \iff X'_i \psi \geq v_i \]
Labor Supply Selection

\[ D_i = 1 \{ X_i' \psi \geq v_i \} \]

- \( D_i^* = X_i' \psi - v_i \) is a latent index determining \( D_i \)

- We observe outcomes in the sample with \( D_i = 1 \). CEF in this sample is

\[
E \left[ w_i \mid X_i, D_i = 1 \right] = X_i' \beta + E \left[ \epsilon_i \mid X_i, v_i < X_i' \psi \right]
\]

- If \( \epsilon_i \) and \( v_i \) are independent, the last term is \( E \left[ \epsilon_i \mid X_i \right] = 0 \) and OLS recovers \( \beta \)

- This is equivalent to saying we have a random sample – selection into the sample is unrelated to outcomes

- If \( \epsilon_i \) and \( v_i \) aren’t independent, we’ll have \( E \left[ \epsilon_i \mid X_i, D_i = 1 \right] \neq 0 \), and OLS on observed sample is inconsistent
Selection with Normality

\[ E [w_i | X_i, D_i = 1] = X_i' \beta + E [\epsilon_i | X_i, v_i < X_i' \psi] \]

- Suppose that \( \epsilon_i \) and \( v_i \) are joint normal:
  \[
  (\epsilon_i, v_i) | X_i \sim N \left( (0, 0), \begin{bmatrix} \sigma^2_{\epsilon} & \rho \sigma_{\epsilon} \\ \rho \sigma_{\epsilon} & 1 \end{bmatrix} \right)
  \]

- Then we can work out the expected error conditional on \( D_i = 1 \)

- Under normality, conditional expectations are linear:
  \[ E [\epsilon_i | X_i, v_i] = \rho \sigma_{\epsilon} v_i. \]
The CEF of \( w_i \) in the observed sample is

\[
E \left[ w_i | X_i, D_i = 1 \right] = X_i' \beta + E \left[ \epsilon_i | X_i, v_i < X_i' \psi \right]
\]

\[
= X_i' \beta + \rho \sigma \epsilon E \left[ v_i | X_i, v_i < X_i' \psi \right]
\]

\[
= X_i' \beta + \rho \sigma \epsilon \cdot \lambda (X_i' \psi)
\]

Here \( \lambda(x) \) is the conditional expectation of a standard normal random variable truncated from above, also known as the inverse Mills ratio:

\[
\lambda(x) = -\frac{\phi(x)}{\Phi(x)}.
\]
Heckit

\[ E [w_i | X_i, D_i = 1] = X_i' \beta + \rho \sigma \varepsilon \cdot \lambda (X_i' \hat \psi) \]

- \( \hat \psi \) can be estimated via a first-step probit of \( D_i \) on \( X_i \)

- Then run a second-step regression in the \( D_i = 1 \) sample:

\[ w_i = X_i' \beta + \rho \sigma \varepsilon \cdot \lambda (X_i' \hat \psi) + u_i \]

- The Mills ratio is a control function or selection correction that accounts for selection into the observed sample

- This is Heckman’s (1974, 1976, 1979) two-step selection correction (“Heckit”)
Heckit Identification

- Suppose $X_i$ is just a constant. Then the second-step regression is

$$w_i = \beta + \rho \sigma_\varepsilon \cdot \lambda (\hat{\psi}) + u_i$$

$$= \delta + u_i$$

- The constant here is $\delta = (\beta + \rho \sigma_\varepsilon \lambda (\psi))$, so $\beta$ and $\rho \sigma_\varepsilon$ are not separately identified.

- More generally, if outcome and selection equations are saturated in $X_i$, main effects and Mills ratio term are not separately identified.

- This is unattractive – there is typically no reason to believe $E [w_i | X_i]$ is linear in $X_i$. 
Heckit Identification

- Solution: Suppose there are additional variables $Z_i$ in the selection equation, so

\[ D_i = 1 \{ X_i' \psi + Z_i' \pi > \nu_i \} \]

- Assume $E[\epsilon_i | X_i, Z_i] = 0$. Then second-step CEF is

\[ E[w_i | X_i, Z_i, D_i = 1] = X_i' \beta + \rho \sigma \epsilon \lambda (X_i' \psi + Z_i' \pi) \]

- If $\pi \neq 0$ this can be estimated even if $X_i$ is saturated since variation in $Z_i$ separately identifies the selection term.

- Identifying a Heckit without relying on functional form restrictions requires finding a $Z_i$ that shifts the probability of selection but is excludable from the outcome equation.

- Sound familiar?
The requirements for a good $Z_i$ in the Heckit model are the same as the requirements for a good instrument when we’re doing IV.

This is not a coincidence. Control function and IV are methods for solving the same problem.
To see the connection between control function and IV, consider a heterogeneous treatment effects model:

\[ Y_i(1) = \alpha_1 + \epsilon_{i1} \]
\[ Y_i(0) = \alpha_0 + \epsilon_{i0} \]

Here \( \alpha_d = E[Y_i(d)] \) so \( E[\epsilon_{id}] = 0 \)

If we had random samples of \( Y_i(1) \) and \( Y_i(0) \) we could run OLS (i.e., take means) and estimate \( ATE = \alpha_1 - \alpha_0 \)
Selection and Treatment Effects

\[ Y_i(1) = \alpha_1 + \epsilon_{i1} \]
\[ Y_i(0) = \alpha_0 + \epsilon_{i0} \]

- But we only observe \( Y_i(1) \) when \( D_i = 1 \), and we only observe \( Y_i(0) \) when \( D_i = 0 \)
- These are not random samples if treatment is not as good as randomly assigned
- We therefore have sample selection problems for both \( Y_i(1) \) and \( Y_i(0) \)
- Treatment effects estimation is a two-sided sample selection problem
- An instrument is needed to solve this problem
We have seen that IV and control function are two methods for solving the same problem.

How should we think about the relationship between parametric sample selection models and the nonparametric LATE model of Imbens and Angrist (1994)?

How should we think about the relationship between estimates produced by IV and control function?
IV and Selection Models

- To better understand the relationships between latent index models and the LATE model, consider a treatment effects model with a binary treatment and binary instrument:

\[ Y_{i}(1) = \alpha_{1} + \epsilon_{i1} \]
\[ Y_{i}(0) = \alpha_{0} + \epsilon_{i0} \]

- Suppose selection into the \( D_{i} = 1 \) sample follows the rule

\[ D_{i} = 1 \{ \psi_{0} + \psi_{1}Z_{i} > v_{i} \} \]

\[ (\epsilon_{i1}, \epsilon_{i0}, v_{i}) \perp \perp Z_{i} \]
\[ v_{i} \sim F(v) \]

- \( F(v) \) is some strictly increasing parametric distribution function (e.g. the normal CDF)
IV and Selection Models

\[ Y_i(1) = \alpha_1 + \epsilon_{i1} \]
\[ Y_i(0) = \alpha_0 + \epsilon_{i0} \]
\[ D_i = 1 \{ \psi_0 + \psi_1 Z_i > v_i \} \]
\[ (\epsilon_{i1}, \epsilon_{i0}, v_i) \perp \perp Z_i \]
\[ v_i \sim F(v) \]

- This selection model appears to be more restrictive than the LATE model, which involves no distributional assumptions
Vytlacil (2002) shows that this selection model is the LATE model, in the sense that

- The selection model satisfies the LATE assumptions
- The LATE assumptions imply that the selection model rationalizes the observed and counterfactual outcomes and treatments
LATE Model and Selection Model: Equivalence

- The first part of the proof is straightforward. Note that

\[ Y_i(0) = \alpha_0 + \epsilon_{i0}, \quad Y_i(1) = \alpha_1 + \epsilon_{i1}, \]

\[ D_i(0) = 1 \{ \psi_0 > v_i \}, \quad D_i(1) = 1 \{ \psi_0 + \psi_1 > v_i \} \]

- \( Y_i(d) \) and \( D_i(z) \) are functions of \((\epsilon_{i0}, \epsilon_{i1}, v_i)\) which are independent of \(Z_i\), so independence/exclusion are satisfied

- If \( \psi_1 > 0 \), then \( D_i(1) \geq D_i(0) \) and monotonicity is satisfied

- \( \Pr[D_i(1) > D_i(0)] = \Pr[\psi_0 + \psi_1 > v_i \geq \psi_0] > 0 \) since \( F(\cdot) \) is strictly increasing, so there is a first stage

- The selection model therefore satisfies the assumptions of the LATE framework
To show that the LATE model implies the selection model representation, first note that with a binary $Z_i$ the "parametric" assumption $v_i \sim F(v)$ is not really a restriction.

For any strictly increasing distribution function $G(\cdot)$ we can write

$$D_i = 1 \left\{ G^{-1}(F(\psi_0 + \psi_1 Z_i)) > G^{-1}(F(v_i)) \right\}$$

$$= 1 \left\{ \tilde{\psi}_0 + \tilde{\psi}_1 Z_i > \tilde{v}_i \right\},$$

where

$$\tilde{\psi}_0 = G^{-1}(F(\psi_0)), \quad \tilde{\gamma}_1 = G^{-1}(F(\psi_0 + \psi_1)) - G^{-1}(F(\psi_0))$$

$$\tilde{v}_i = G^{-1}(F(v_i))$$
LATE Model and Selection Model: Equivalence

\[ D_i = 1 \left\{ \tilde{\psi}_0 + \tilde{\psi}_1 Z_i > \tilde{v}_i \right\}, \]

- The new selection error \( \tilde{v}_i = G^{-1}(F(v_i)) \) has CDF \( G(\cdot) \)
- The same selection model can be represented with any distribution function
- It is therefore sufficient to show that the LATE model implies a selection model representation for SOME distribution function
LATE Model and Selection Model: Equivalence

Let $u_i \sim U(0, 1)$ be independent of $Z_i$, and define

$$U_i = \begin{cases} 
  u_i \times Pr[D_i(0) = 1], & D_i(0) = 1 \\
  Pr[D_i(0) = 1] + u_i \times Pr[D_i(1) > D_i(0)], & D_i(1) > D_i(0) \\
  Pr[D_i(1) = 1] + u_i \times Pr[D_i(1) = 0], & D_i(1) = 0 
\end{cases}$$

Then we can write

$$D_i = 1 \{ \psi_0 + \psi_1 Z_i > U_i \}$$

Here $\psi_0 = Pr[D_i(0) = 1]$, $\psi_1 = Pr[D_i(1) > D_i(0)]$, and $U_i \sim U(0, 1)$
Always takers  Compliers  Never takers  
Density of $U$  
\[
0 \quad 1 
\]

$\psi_0$  $\psi_0 + \psi_1$  

$\phi_0$  $\phi_0 + \phi_1$
LATE Model and Selection Model: Equivalence

- $U_i$ is uniform on $(0, \psi_0)$ for always takers, on $(\psi_0, \psi_0 + \psi_1)$ for compliers, and on $(\psi_0 + \psi_1, 1)$ for never takers.

- This model implies the same observed and counterfactual treatment choices and outcomes as the LATE model.

- We can equivalently represent the selection model with the distribution $F(\cdot)$ by applying $F^{-1}(\cdot)$ to both sides of the treatment selection equation.

- We have therefore shown that the LATE model and the selection model are equivalent: They are two ways of representing the same information.

- Vytlacil (2002) shows that this applies to the more general LATE model with multiple instruments.
IV and Control Function

- Selection model with uniform representation of selection error:

\[ Y_i(1) = \alpha_1 + \epsilon_{i1} \]
\[ Y_i(0) = \alpha_0 + \epsilon_{i0} \]
\[ D_i = 1 \{ \psi_0 + \psi_1 Z_i > U_i \} \]
\[ U_i \sim U(0, 1) \]
\[ (\epsilon_{i1}, \epsilon_{i0}, U_i) \perp \perp Z_i \]

- We’ve shown that this is the LATE model

- Does this mean that IV and control function estimates of treatment effects are also equivalent?
No. In fact, we cannot estimate this model by control function without further assumptions.

To form control functions we need to specify $E[\epsilon_{id}|U_i]$, which we haven’t done.

Control function yields estimates of $\alpha_1$ and $\alpha_0$, and therefore the $ATE$ $\alpha_1 - \alpha_0$.

The $ATE$ is not identified in the $LATE$ model – we can only get the $LATE$.

We have to assume more if we want to extrapolate from $LATE$ to $ATE$. 

Chris Walters (UC Berkeley)
To understand control function extrapolation, it's useful to start with what is nonparametrically identified in the LATE framework.

We know the average treatment effect for compliers is identified (LATE theorem).

It turns out that other features of complier potential outcomes are identified as well (Imbens and Rubin, 1997; Abadie, 2003).

Individuals with $D_i = Z_i = 1$ are a mix of always takers and compliers:

$$E[Y_i|D_i = Z_i = 1] = \left( \frac{\pi_{AT}}{\pi_{AT} + \pi_C} \right) E[Y_i(1)|AT] + \left( \frac{\pi_C}{\pi_{AT} + \pi_C} \right) E[Y_i(1)|C]$$
Complier Potential Outcomes

- **Always taker outcome** is observed directly as

\[ E[Y_i|D_i = 1, Z_i = 0] = E[Y_i(1)|AT] \]

- **Population shares** are also identified since

\[ \pi_{AT} = \Pr[D_i = 1|Z_i = 0] \]
\[ \pi_C = \Pr[D_i = 1|Z_i = 1] - \Pr[D_i = 1|Z_i = 0] \]

- We can then back out the average complier \( Y_i(1) \) as

\[ E[Y_i(1)|C] = \left( \frac{\pi_{AT}}{\pi_C} \right) E[Y_i|D_i = Z_i = 1] - \left( \frac{\pi_{AT}}{\pi_C} \right) E[Y_i(1)|AT] \]

- By the same reasoning, we can back out \( E[Y_i(0)|C] \) from the complier/never taker mix with \( D_i = Z_i = 0 \)
Control Function Extrapolation

- In the LATE framework we can identify:
  - $E [Y_i(1)|AT]$
  - $E [Y_i(0)|NT]$
  - $E [Y_i(1)|C]$
  - $E [Y_i(0)|C]$

- We can therefore identify means of $Y_i(1)$ and $Y_i(0)$ for two groups each

- In selection model notation, this yields two points on the curve $E [Y_i(d)|U_i]$ for each potential outcome
\[ E[Y_i] \]

\[ E[Y(1)|C] \]

\[ E[Y(0)|NT] \]

\[ E[Y(0)|C] \]

\[ E[Y(1)|AT] \]

\[ \frac{\psi_0}{2} \]

\[ \psi_0 + \frac{\psi_1}{2} \]

\[ \frac{1 + \psi_0 + \psi_1}{2} \]

\[ E[U_i] \]

\[ \text{LATE} \]
Extrapolation from LATE

Without further assumptions we cannot identify any other treatment effects.

But by specifying a functional form for $E [Y_i(d)|U_i]$, we can “connect the dots” and extrapolate to predict effects for always takers and never takers.

This allows us to predict the effects of policies that affect different subpopulations than the instrument at hand.

More generally, think of selection model as a device for extrapolating from available research design to predict impacts of other experiments.
\[ E[Y_i] \]

\[ E[Y(1)|C] \]
\[ E[Y(0)|NT] \]
\[ E[Y(0)|C] \]
\[ E[Y(1)|AT] \]

Assumption: Linear selection (Olsen, 1980)

\[ E[\epsilon_i | U_i] = \gamma_d U_i \]
Assumption: Linear selection (Olsen, 1980)

\[ E[\epsilon_{id} | U_i] = \gamma_d U_i \]
Assumption: Linear selection (Olsen, 1980)

\[ E[\epsilon_{id}|U_i] = \gamma_d U_i \]
Assumption: Linear selection (Olsen, 1980)

\[ E[\epsilon_{id}|U_i] = \gamma_d U_i \]
Assumption: Heckit model (Heckman, 1979)

\[ E[\epsilon_{id}|U_i] = \gamma_d \Phi^{-1}(U_i) \]
Letting $U_i \sim U(0, 1)$, choosing $E[Y_i(d)|U_i]$ implies a functional form for marginal treatment effects (MTE):

$$MTE(u) = E[Y_i(1) - Y_i(0)|U_i = u]$$

MTEs are average treatment effects for individuals at a particular percentile of the unobserved cost of taking treatment (Heckman et al., 1999, 2005, 2006; Carneiro et al., 2009, 2010)

$MTE(u)$ can be thought of as the LATE associated with a hypothetical instrument that shifts the probability of treatment from $u$ to $u + \Delta$ for small $\Delta$

With a continuous instrument, MTEs can be estimated as derivatives of average $Y_i$ with respect to the conditional probability of treatment (local IV; Heckman and Vytlacil, 1999)

With a discrete instrument, estimation involves extrapolation/interpolation from available LATEs (Brinch et al., 2017)
Marginal Treatment Effects

Many treatment effects of interest can be defined as weighted averages of MTEs – useful for thinking about external validity:

$$\int_0^1 \omega(u) MTE(u) du$$

Let $$\pi(z) = Pr [D_i = 1|Z_i = z]$$, and $$p = Pr [Z_i = 1]$$

Weights for notable treatment effects:

- **ATE**: $$\omega(u) = 1$$
- **TOT**: $$\omega(u) = \frac{p1 \{u < \pi(1)\} + (1 - p)1 \{u < \pi(0)\}}{\pi(1)p + \pi(0)(1 - p)}$$
- **TNT**: $$\omega(u) = \frac{p1 \{u \geq \pi(1)\} + (1 - p)1 \{u \geq \pi(0)\}}{(1 - \pi(1))p + (1 - \pi(0))(1 - p)}$$
- **LATE**: $$\omega(u) = \frac{1 \{\pi(0) \leq u < \pi(1)\}}{\pi(1) - \pi(0)}$$
MTE and Policy Counterfactuals

- Models for MTE can be used to predict the effects of policies that have not been implemented.

- Example: Suppose an experiment reduces the price of purchasing health insurance from $p_0$ to $p_1$, and the probability of purchase rises from $\pi_0$ to $\pi_1$.

- Individuals with $U_i = \pi_1$ are on the margin between purchasing and not purchasing – we might expect them to purchase in response to a further price cut.

- Heckit prediction of effect for marginal population:

$$\hat{MTE}(\pi_1) = \hat{\alpha}_1 - \hat{\alpha}_0 + (\hat{\gamma}_1 - \hat{\gamma}_0) \Phi^{-1}(\hat{\pi}_1)$$

- Can also use estimates of MTEs to predict $TOT$, $TNT$, $ATE$, or effects of other hypothetical policies.
Through the Looking Glass

- CF estimate of LATE:

\[ \hat{LATE} = \hat{\alpha}_1 - \hat{\alpha}_0 + \hat{E} [\epsilon_i \mid \psi_0 \leq U_i < \psi_0 + \psi_1] \]

- In the binary treatment/binary instrument case with two-sided non-compliance, the two-step estimate of LATE produced by any parametric selection model is algebraically equal to the IV estimate (Kline and Walters, 2019)

- The CF estimator exactly fits the IV estimates of mean potential outcomes regardless of functional form – it connects the dots in sample

- In binary/binary case IV and CF coincide when both are used to estimate LATE

- Equivalence serves as a natural benchmark for assessing overidentified selection models

- The assumption for \( E [\epsilon_{id} \mid U_i] \) only matters when it is used to predict treatment effects for other subpopulations
When to Extrapolate?

- When is it reasonable to extrapolate from LATE and predict the effects of new policies?
- It depends on the interpretation of $U_i$, and hence on the instrument.
- Equivalent to asking: when is the relationship between always taker/complier $Y_i(1)$'s likely to be a reliable guide to the relationship between complier/never taker $Y_i(1)$'s?
- If $Z_i$ is a price shift, $U_i$ may be viewed as (minus) willingness to pay and extrapolation may be sensible.
- What would extrapolation mean in other IV examples?
Selection into Preschool: Kline and Walters (2016)

- Selection model example: Kline and Walters (QJE 2016) investigate effect heterogeneity with respect to counterfactual treatment choices.

- Setting: Randomized evaluation of Head Start program
  - Public preschool for disadvantaged children
  - Largest preschool program in the US
  - Basic experimental impacts less impressive than earlier non-experimental analyses of HS
  - But alternative publicly subsidized preschools are now widely available for HS-eligible children. Are effects larger for kids who would otherwise stay home?
<table>
<thead>
<tr>
<th>Time period</th>
<th>Three-year-old cohort</th>
<th>Four-year-old cohort</th>
<th>Cohorts pooled</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1) Reduced form</td>
<td>(2) First stage</td>
<td>(3) IV</td>
</tr>
<tr>
<td>Year 1</td>
<td>0.194 (0.029)</td>
<td>0.699 (0.025)</td>
<td>0.278 (0.041)</td>
</tr>
<tr>
<td>N</td>
<td>1,970</td>
<td>1,601</td>
<td>3,571</td>
</tr>
</tbody>
</table>

Notes: This table reports experimental estimates of the effects of Head Start on test scores. The outcome is the average of standardized PPVT and WJIII scores, with each score standardized to have mean 0 and standard deviation 1 in the control group separately by applicant cohort and year. Columns (1), (4), and (7) report coefficients from regressions of test scores on an indicator for assignment to Head Start. Columns (2), (5), and (8) report coefficients from first-stage regressions of test scores on an indicator for assignment to Head Start. The reduced form IV estimates are obtained by instrumenting test scores with indicators for whether children were assigned to Head Start in years 2 and 3. The first-stage IV estimates are obtained by instrumenting test scores with indicators for whether children were assigned to Head Start in years 2 and 3. Standard errors are clustered by center of random assignment.
### TABLE III

**Preschool Choices by Year, Cohort, and Offer Status**

<table>
<thead>
<tr>
<th>Time period</th>
<th>Cohort</th>
<th>Offered</th>
<th></th>
<th>Not offered</th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
<td>(7)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Head Start</td>
<td>Other centers</td>
<td>No preschool</td>
<td>Head Start</td>
<td>Other centers</td>
<td>No preschool</td>
<td>C-complier share</td>
</tr>
<tr>
<td>Year 1</td>
<td>3-year-olds</td>
<td>0.851</td>
<td>0.058</td>
<td>0.092</td>
<td>0.147</td>
<td>0.256</td>
<td>0.597</td>
<td>0.282</td>
</tr>
<tr>
<td></td>
<td>4-year-olds</td>
<td>0.787</td>
<td>0.114</td>
<td>0.099</td>
<td>0.122</td>
<td>0.386</td>
<td>0.492</td>
<td>0.410</td>
</tr>
<tr>
<td></td>
<td>Pooled</td>
<td>0.822</td>
<td>0.083</td>
<td>0.095</td>
<td>0.136</td>
<td>0.315</td>
<td>0.550</td>
<td>0.338</td>
</tr>
</tbody>
</table>

Notes: This table reports shares of offered and nonoffered students attending Head Start, other center-based preschools, and no preschool, separately by year and age cohort. All statistics are weighted by the reciprocal of the probability of a child’s experimental assignment. Column (7) reports estimates of the share of compliers drawn from other preschools, given by minus the ratio of the offer’s effect on attendance at other preschools to its effect on Head Start attendance.
Kline and Walters (2016): Notation

- $Z_i \in \{0, 1\}$: Randomized experimental offer
- $D_i(z)$: Potential preschool choice.
  - $h$: Head Start
  - $c$: Other preschool center
  - $n$: No preschool

- Monotonicity restriction:
  \[ D_i(1) \neq D_i(0) \implies D_i(1) = h \]

- People only respond to a Head Start offer by enrolling in Head Start
Monotonicity implies that the population can be partitioned into five groups:

- **n-compliers**: $D_i(1) = h$, $D_i(0) = n$
- **c-compliers**: $D_i(1) = h$, $D_i(0) = c$
- **n-never takers**: $D_i(1) = D_i(0) = n$
- **c-never takers**: $D_i(1) = D_i(0) = c$
- **Always takers**: $D_i(1) = D_i(0) = h$
The Head Start experiment identifies a LATE:

\[
E [Y_i | Z_i = 1] - E [Y_i | Z_i = 0] \\
E [1 \{D_i = h\} | Z_i = 1] - E [1 \{D_i = h\} | Z_i = 0]
\]

\[
= E [Y_i(h) - Y_i(D_i(0)) | D_i(1) \neq D_i(0)]
\]

\[\equiv LATE_h\]

This is an effect relative to a mix of counterfactuals:

\[LATE_h = S_c LATE_{ch} + (1 - S_c) LATE_{nh}\]

\[LATE_{nh}\] and \[LATE_{ch}\] are effects for \(n\) and \(c\) compliers relative to specific counterfactuals

\(S_c\) is the share of \(c\)-compliers among all compliers
Kline and Walters (2016): Policy Relevant Parameters

- $LATE_h$ is the policy-relevant parameter for a marginal expansion of Head Start

- Policymaker does not control substitution from other programs

- Not feasible to target policies based on unobserved behavioral responses

- Effect heterogeneity is not always policy-relevant

- Need a clear motivation for decomposing into “subLATEs” $LATE_{ch}$ and $LATE_{nh}$

- Scientific interest in understanding small experimental impacts

- Relevant for policies that change the counterfactual or nature of selection
SubLATEs aren’t nonparametrically identified by the experiment

Estimate via 3-alternative selection model:

\[ U_i(h) = \psi_h(X_i, Z_i) + v_{ih} \]

\[ U_i(c) = \psi_c(X_i) + v_{ic} \]

\[ U_i(n) = 0 \]

\[
(v_{ih}, v_{ic})|X_i, Z_i \sim N \left(0, \begin{bmatrix} 1 & \rho(X_i) \\ \rho(X_i) & 1 \end{bmatrix} \right)
\]

\( X_i \) is a vector of covariates, including demographics and experimental sites
Kline and Walters (2016): Control Functions

- Restrictions on potential outcome CEFs:

\[ E[Y_i(d)|X_i, Z_i, \nu_{ih}, \nu_{ic}] = \mu_d(X_i) + \gamma_{dh}\nu_{ih} + \gamma_{dc}\nu_{ic} \]

- Averaging over individuals in a particular care alternative gives

\[ E[Y_i(d)|X_i, Z_i, D_i = d] = \mu_d(X_i) + \gamma_{dh}\lambda_h(X_i, Z_i, d) + \gamma_{dc}\lambda_c(X_i, Z_i, d) \]

- \( \lambda_d(X_i, Z_i, D_i) \) are bivariate versions of the Heckit Mills ratio

- Additive separability between observables and unobservables is key

- Estimates of \( \mu_d(X) \), \( \gamma_{dh} \), and \( \gamma_{dc} \) are used to construct model-based estimates of subLATEs
9. MODEL EVALUATION

In practice, researchers often estimate selection models that impose additive separability assumptions on exogenous covariates, combine multiple instruments, and employ additional smoothness restrictions that break the algebraic equivalence of structural LATE estimates with IV. The equivalence results developed above provide a useful conceptual benchmark for assessing the performance of structural models in such applications. An estimator derived from a properly specified model of treatment assignment and potential outcomes should come close to matching a nonparametric IV estimate of the same parameter. Significant divergence between these estimates would signal that the restrictions imposed by the structural model are violated.

Figure 3 shows an example of this approach to model assessment from Kline and Walters' (2016) reanalysis of the Head Start Impact Study (HSIS)—a randomized experiment with two-sided non-compliance (Puma, Bell, Cook, and Heid (2012)). On the vertical axis are nonparametric IV estimates of the LATE associated with participating in the Head Start program relative to a next best alternative for various subgroups in the HSIS defined by experimental sites and baseline child and parent characteristics. On the horizontal axis are two-step control function estimates of the same parameters derived from a heavily over-identified selection model involving multiple endogenous variables, baseline covariates, and excluded instruments. Had this model been saturated, all of the points would lie on the 45 degree line. In fact, a Wald test indicates these deviations from the 45 degree line cannot be distinguished from noise at conventional significance levels, suggesting that the approximating model is not too far from the truth.

Passing a specification test does not obviate the fundamental identification issues inherent in interpolation and extrapolation exercises. As philosophers of science have long argued, however, models that survive empirical scrutiny deserve greater consideration than those that do not (Popper(1959), Lakatos (1976)). Demonstrating that a tightly restricted model yields a good fit to IV estimates not only bolsters the credibility of the model but also provides a useful benchmark for assessing the performance of other models. The figure reproduced here from Figure A.III of Kline and Walters (2016) illustrates this approach. The horizontal axis displays the average predicted LATE in each group, and the vertical axis shows corresponding IV estimates. The dashed line is the 45-degree line. The chi-squared statistic and $p$-value come from a bootstrap Wald test of the hypothesis that the 45 degree line fits all points up to sampling error. See Appendix F of Kline and Walters (2016) for more details.
Estimates of the subLATE for \( n \)-compliers, \( LATE_{nh} \), are stable across specifications and indicate that the impact of moving from home care to Head Start is large—on the order of 0.37 standard deviations. By contrast, estimates of \( LATE_{ch} \), though more variable across specifications, never differ significantly from zero.

Our estimates of \( LATE_{nh} \) are somewhat smaller than the average treatment effects of Head Start relative to home care displayed in Table VII. This is a consequence of the reverse Roy pattern captured by the control function coefficients: families willing to switch from home care to Head Start in response to an offer have stronger than average tastes for Head Start, implying smaller than average gains. We can reject that predicted

\[
\begin{array}{cccc}
\text{Parameter} & \text{(1) IV} & \text{(2) Covariates} & \text{(3) Sites} & \text{(4) Full model} \\
LATE_h & 0.247 & 0.261 & 0.190 & 0.214 \\
& (0.031) & (0.032) & (0.076) & (0.042) \\
LATE_{nh} & 0.386 & 0.341 & 0.370 & 0.370 \\
& (0.143) & (0.219) & (0.088) & \\
LATE_{ch} & 0.023 & -0.122 & -0.093 & \\
& (0.251) & (0.469) & (0.154) & \\
\end{array}
\]
Kirkeboen, Leuven and Mogstad (QJE 2016) study the payoffs to field of study in Norway

Substantive questions:

What are the payoffs to different fields of study, e.g., social science vs. engineering?

Do individuals sort across fields according to comparative advantage?

Different angle on returns to institutions and selectivity we saw earlier
Kirkeboen et al. (2016): Context

- Context: Norwegian higher education
- Norway has a centralized admissions process
  - Apply to field/institution simultaneously (e.g. teaching at University of Oslo)
  - Rank up to 15 choices
  - Applications scored based on high school GPA, then ranked by application score
  - Then places are assigned in turn: Best applicant gets favorite choice, next best gets highest choice for which he qualifies, and so on
- Rank cutoffs generate instruments for every field
Note: This figure reports mean earnings by field for our sample of applicants and for all applicants. Earnings are measured eight years after application. The measures of earnings are regression adjusted for year of application.

Figure 2. Mean earnings by field: Sample and all applicants
Note: This figure shows the sample fraction that is offered or complete the preferred field by application score. We pool all admission cutoffs and normalize the data so that zero on the x-axis represents the admission cutoff to the preferred field. We plot unrestricted means in bins and include estimated local linear regression lines on each side of the cutoff.

Figure 3. Admission thresholds and preferred field offer and completion
Consider a potential outcomes model with $J + 1$ fields of study.

Potential outcomes: $Y_i(0), Y_i(1), \ldots, Y_i(J)$

Interested in estimating averages of treatment effects:

$$\Delta_i(j, k) = Y_i(j) - Y_i(k)$$

Instrument is assigned field: $Z_i \in \{0, 1, \ldots, J\}$

Potential treatment choices: $D_i(0), D_i(1), \ldots, D_i(J)$

Treatment and instrument indicators: $D_{ij}(z) = 1\{D_i(z) = j\}$, $Z_{ij} = 1\{Z_i = j\}$
Kirkeboen et al. (2016): Framework

▶ Assumptions:

\[(Y_i(0), ..., Y_i(J), D_i(0), ..., D_i(J)) \perp \perp Z_i\]

\[D_{ij}(j) \geq D_{ij}(k), \forall i, j, k\]

▶ The second assumption says that moving the instrument from \(k\) to \(j\) makes everyone more likely to choose \(j\) - extension of monotonicity

▶ Think of this as an “encouragement design” where \(Z_i\) is offered field and \(D_i\) is enrolled field
Tempting to estimate a 2SLS model with $J$ endogenous variables:

$$Y_i = \alpha + \sum_{j=1}^{J} \beta_j D_{ij} + \epsilon_i$$

$$D_{ij} = \lambda_j + \sum_{k=1}^{J} \pi_{jk} Z_{ik} + \eta_{ij}$$

It turns out that $\beta_j$ is not generally interpretable as an average treatment effect under our assumptions – LATE theorem doesn’t generalize to multiple endogenous variables.

Issue: Moving $Z_i$ from $k$ to $j$ makes everyone more likely to choose $j$ but may shift people across all other pairs of fields, creating $(J+1)J$ compliance groups.

Need restrictions on effect heterogeneity or substitution patterns to interpret $\beta_j$ as a LATE.
Convenient feature of centralized assignment: the fallback field is known for everyone

Conditional on fallback (below-threshold) field $k$, can identify LATE for compliers who switch from $k$ to $j$ at the threshold:

$$\beta_{jk} = E [Y_i(j) - Y_i(k)|D_i(j) = j, D_i(k) = k, j \text{ ranked above } k]$$

Similarly, conditional on fallback $j$, can identify LATE for compliers who switch from $j$ to $k$:

$$\beta_{kj} = E [Y_i(k) - Y_i(j)|D_i(k) = k, D_i(j) = j, k \text{ ranked above } j]$$

A constant-effects model implies $\beta_{jk} = -\beta_{kj}$. What about a Roy model?
<table>
<thead>
<tr>
<th>Completed field ($j$)</th>
<th>Humanities</th>
<th>Soc Science</th>
<th>Teaching</th>
<th>Health</th>
<th>Science</th>
<th>Engineering</th>
<th>Technology</th>
<th>Business</th>
<th>Law</th>
</tr>
</thead>
<tbody>
<tr>
<td>Humanities</td>
<td>21.4*</td>
<td>-4.7</td>
<td>-22.9*</td>
<td>5.0</td>
<td>-38.5**</td>
<td>6.9</td>
<td>-42.2**</td>
<td>-156.3</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(11.0)</td>
<td>(9.8)</td>
<td>(12.1)</td>
<td>(11.9)</td>
<td>(14.7)</td>
<td>(48.3)</td>
<td>(10.6)</td>
<td>(437.3)</td>
<td></td>
</tr>
<tr>
<td>Social Science</td>
<td>18.7**</td>
<td>9.8</td>
<td>-10.8</td>
<td>55.5**</td>
<td>-55.4**</td>
<td>-110.4</td>
<td>-28.4**</td>
<td>-76.1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(6.7)</td>
<td>(11.6)</td>
<td>(13.0)</td>
<td>(21.5)</td>
<td>(20.6)</td>
<td>(103.0)</td>
<td>(10.7)</td>
<td>(86.4)</td>
<td></td>
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<tr>
<td>Teaching</td>
<td>22.3**</td>
<td>31.4**</td>
<td>1.8</td>
<td>23.5**</td>
<td>-33.9**</td>
<td>-35.3</td>
<td>-21.1**</td>
<td>22.8</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(5.0)</td>
<td>(7.9)</td>
<td>(6.6)</td>
<td>(9.5)</td>
<td>(12.5)</td>
<td>(37.1)</td>
<td>(7.1)</td>
<td>(127.9)</td>
<td></td>
</tr>
<tr>
<td>Health</td>
<td>18.8**</td>
<td>30.7**</td>
<td>7.7**</td>
<td>28.9**</td>
<td>-27.9**</td>
<td>-43.4**</td>
<td>-17.4**</td>
<td>-55.2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(6.3)</td>
<td>(7.6)</td>
<td>(2.8)</td>
<td>(7.6)</td>
<td>(10.4)</td>
<td>(20.8)</td>
<td>(4.0)</td>
<td>(97.7)</td>
<td></td>
</tr>
<tr>
<td>Science</td>
<td>53.7**</td>
<td>69.6**</td>
<td>38.6**</td>
<td>29.6**</td>
<td></td>
<td></td>
<td>-2.2</td>
<td>16.8</td>
<td>-4.9</td>
</tr>
<tr>
<td></td>
<td>(18.4)</td>
<td>(22.4)</td>
<td>(14.2)</td>
<td>(11.5)</td>
<td></td>
<td></td>
<td>(18.1)</td>
<td>(10.5)</td>
<td>(148.3)</td>
</tr>
<tr>
<td>Engineering</td>
<td>59.8</td>
<td>-5.5</td>
<td>75.2**</td>
<td>0.2</td>
<td>52.4**</td>
<td>-46.0</td>
<td>-13.0</td>
<td>-57.7</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(50.6)</td>
<td>(58.2)</td>
<td>(37.5)</td>
<td>(16.4)</td>
<td>(21.0)</td>
<td>(43.9)</td>
<td>(23.7)</td>
<td>(166.6)</td>
<td></td>
</tr>
<tr>
<td>Technology</td>
<td>41.9**</td>
<td>58.7**</td>
<td>22.1*</td>
<td>32.5**</td>
<td>68.1**</td>
<td>-5.6</td>
<td>7.0</td>
<td>-53.1</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(10.8)</td>
<td>(10.1)</td>
<td>(12.4)</td>
<td>(10.1)</td>
<td>(9.6)</td>
<td>(12.0)</td>
<td>(9.5)</td>
<td>(147.5)</td>
<td></td>
</tr>
</tbody>
</table>
The first is that field of study may affect employment probabilities, which could bias the estimates with log earnings as dependent variable. However, we find fairly small impacts of field of study on employment. Furthermore, if marginal workers have lower potential earnings, any bias coming from employment effects should make it less likely to find evidence of comparative advantage.

Second, one might be worried that the conclusions drawn about selection patterns are driven by heterogeneity across subfields within our broader definition of fields (see Table II). To address this concern, we have reestimated the model given by equations (14) and (15) with treatment variables defined according to subfields instead of broader fields. These estimates, reported in Online Appendix Figure B.V, suggest that aggregation to broader fields is not driving the conclusion that compliers tend to prefer fields in which they have comparative advantage.31

**FIGURE XII**

**Testable Implication of Sorting Based on Comparative Advantage**

This figure graphs the distribution of the differences in relative payoffs to field \(j\) versus \(k\) between individuals whose preferred field is \(j\) and next-best alternative is \(k\), and those with the reverse ranking. To construct this graph, we use the complier-weighted distribution of estimates in Online Appendix Table B.VI.

---

31. The results are also robust to including the set of applicants who apply to only one broad field but have a preferred subfield with a cutoff.

- Selection models can be useful for thinking about external validity: who participates in the experiment?
- Walters (JPE 2018) studies this question in the context of charter schools
  - Publicly funded schools operating outside of traditional public school districts
  - When oversubscribed, admit applicants by random lottery
  - Studies based on lottery applicants show that urban charters boost achievement (Abdulkadiroglu et al., 2011; Dobbie and Fryer, 2011)
  - But lottery applicants are a small, highly-selected population. Would charter expansion produce gains for broader groups of students?
- Walters (2018) models selection into charter application with a dynamic generalized Roy model
Walters (2018): Setting

- Setting: charter schools in Boston
- Boston charters employ “No Excuses” practices
  - Extended instructional time, strict discipline, high expectations
  - Earlier lottery studies demonstrate large improvements in achievement for applicants (Abdulkadiroglu et al., 2011)
  - Continuing controversy over charter expansion
- Decentralized application process, separate for every school
  - Students must take steps outside of normal enrollment process
  - May apply to as many charters as desired, or none
Walters (2018): Selection Process

Three stage selection process:

1. Students decide whether to apply to charter schools, $A_i \in \{0, 1\}$
2. Charters randomize offers among applicants, $Z_i \in \{0, 1\}$
3. Applicants with $Z_i = 1$ decide whether to attend charter; students with $Z_i = 0$ remain in traditional public school

Once enrolled in a school, students earn test scores $Y_i$

Randomization at stage 2 makes $Z_i$ a good instrument for charter attendance in the population with $A_i = 1$
## TABLE 1
**Descriptive Statistics for Boston Middle School Students**

<table>
<thead>
<tr>
<th></th>
<th><strong>All Boston Students</strong></th>
<th><strong>Charter Applicants</strong></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Mean (1)</td>
<td>Standard Deviation (2)</td>
</tr>
<tr>
<td><strong>A. Charter School Applications and Attendance</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Applied to charter school</td>
<td>.175</td>
<td>.380</td>
</tr>
<tr>
<td>Applied to more than one charter</td>
<td>.046</td>
<td>.210</td>
</tr>
<tr>
<td>Received charter offer</td>
<td>.125</td>
<td>.331</td>
</tr>
<tr>
<td>Attended charter school</td>
<td>.112</td>
<td>.316</td>
</tr>
<tr>
<td><strong>B. Student Characteristics</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>.492</td>
<td>.500</td>
</tr>
<tr>
<td>Black</td>
<td>.460</td>
<td>.498</td>
</tr>
<tr>
<td>Hispanic</td>
<td>.398</td>
<td>.490</td>
</tr>
<tr>
<td>Subsidized lunch</td>
<td>.821</td>
<td>.383</td>
</tr>
<tr>
<td>Special education</td>
<td>.226</td>
<td>.418</td>
</tr>
<tr>
<td>Limited English proficiency</td>
<td>.212</td>
<td>.409</td>
</tr>
<tr>
<td>4th-grade math score</td>
<td>−.520</td>
<td>1.070</td>
</tr>
<tr>
<td>4th-grade reading score</td>
<td>−.636</td>
<td>1.137</td>
</tr>
<tr>
<td><strong>C. Nearby Schools</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Miles to closest charter school</td>
<td>2.105</td>
<td>1.168</td>
</tr>
<tr>
<td>Miles to closest district school</td>
<td>.512</td>
<td>.339</td>
</tr>
<tr>
<td>Value-added of closest district school</td>
<td>.032</td>
<td>.154</td>
</tr>
<tr>
<td>Observations</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
As can be seen in column 1, both instruments generate strong first-stage shifts in charter enrollment. A lottery offer increases the probability of charter attendance by 64 percentage points, while a 1-mile increase in differential distance decreases the probability of charter attendance by 2.6 percentage points. Columns 2 and 3 show that the two instruments produce roughly similar estimates of the effects of charter attendance, though the distance estimates are less precise. The distance instrument generates estimates of 0.45 and 0.38 in math and reading compared to lottery estimates of 0.55 and 0.49.

The argument in Section IV.A suggests that the interaction of lotteries and distance can be used to describe the nature of selection on unobservables. Figure 2 presents an empirical sketch of this idea by splitting the charter applicant sample into terciles of differential distance. Lottery estimates for these three groups show smaller test score gains for students who apply from farther away. The hypothesis that effects are equal across terciles is rejected at marginal significance levels in math ($p < 0.08$) though not in reading ($p < 0.33$). This pattern suggests that students who are willing to travel farther to attend charter schools experience smaller

<table>
<thead>
<tr>
<th>Instrument</th>
<th>First Stage (1)</th>
<th>Second Stage</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td></td>
</tr>
<tr>
<td>Lottery offer</td>
<td>.641 (.025)</td>
<td>.553 (.087)</td>
<td>.492 (.092)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>1,601</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Differential distance</td>
<td>-.026 (.002)</td>
<td>.453 (.212)</td>
<td>.380 (.217)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>9,156</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

**TABLE 3**

**Two-Stage Least Squares Estimates of Charter School Effects**

Note.—This table reports 2SLS estimates of the effects of charter school attendance on eighth-grade test scores. The endogenous variable is an indicator equal to one if a student attended a charter school at any time prior to the test. The first row instruments for charter attendance using a lottery offer indicator, and the second row instruments for charter attendance using distance to the closest charter school minus distance to the closest district school. Column 1 reports first-stage impacts of the instruments on charter school attendance, and cols. 2 and 3 report second-stage effects on math and reading scores. The lottery sample is restricted to charter school applicants, while the distance sample includes all Boston students. The lottery models control for lottery portfolio indicators. The distance models control for sex, race, subsidized lunch, special education, limited English proficiency, the value-added of the closest traditional public school, and fourth-grade math and reading scores.

- Student $i$’s utility from attending a charter school ($S_i = 1$):
  \[ U_{i1} = \mu(d_i) + v_i \]

- $d_i$ is distance to charter

- $v_i$: Unobserved taste for charter schools
  - Decompose into $v_i = \theta_i + \xi_i$, with $\xi_i$ learned after application

- Utility of attending traditional public school normalized to $U_{i0} = 0$

- Charter applicants pay utility cost $\gamma$

- Generalized Roy model: $v_i$ may be related to potential outcomes in charter and traditional schools, $Y_i(1)$ and $Y_i(0)$
Students choose applications $A_i$ at cost $\gamma A_i$

Charters offer $Z_i = 1$ with probability $\pi A_i$

Students choose school $S_i$

Students earn test scores $Y_i$

Students know $d_i, \theta_i$

Students learn $\xi_i$
Walters (2018): Enrollment Stage

- Solve the model by backward induction
- At the enrollment stage, students choose schools to maximize utility:

\[ S_i = \arg \max_{j \in C(Z_i)} U_{ij} \]

- Choice set depends on whether the student has received a charter offer:

\[ C(Z_i) = \{0\} \cup \{Z_i\} \]

- Before learning \( \xi_i \), expected utility is:

\[ w(Z_i | d_i, \theta_i) = E \left[ \max_{j \in C(Z_i)} U_{ij} | d_i, \theta_i \right] \]

\[ = \begin{cases} 0, & Z_i = 0 \\ E \left[ \max \{\mu(d_i) + \theta_i + \xi_i, 0\} | d_i, \theta_i \right], & Z_i = 1 \end{cases} \]
Expected utility $w(Z_i|d_i, \theta_i)$ is increasing in $Z_i$ because a charter offer provides an option value at the school enrollment stage.

Example of $Emax$, key concept in dynamic discrete choice:

- Students can reoptimize in response to new information, so $E[\max U_{ij}] \geq \max E[U_{ij}]$
- Students plan ahead, knowing they will later make an $Emax$ decision

If $\xi_i \sim logistic$, we have

$$w(Z_i|d_i, \theta_i) = \log (1 + Z_i \times [\mu(d_i) + \theta_i])$$

$$\Pr [S_i = 1|Z_i, d_i, \theta_i] = \frac{Z_i \times \exp(\mu(d_i) + \theta_i)}{1 + Z_i \times \exp(\mu(d_i) + \theta_i)}$$
Random assignment makes the lottery stage simple

The probability that student $i$ receives a lottery offer is

$$\Pr[Z_i = 1|A_i] = \pi A_i$$

Applicants receive offers with probability $\pi$, non-applicants do not receive offers.
Walters (2018): Application Stage

- Forward-looking students choose applications to maximize expected utility:

\[ A_i = 1 \{ \pi w(1, d_i, \theta_i) - \gamma > 0 \} \]

- With logistic \( \xi_i \), the application rule is:

\[ A_i = 1 \{ \theta_i > \exp(\gamma/\pi) - 1 - \mu(d_i) \} \]

- If \( \theta_i \sim N(0, \sigma^2_{\theta}) \), application stage becomes a probit:

\[ \Pr [A_i = 1|d_i] = \Phi \left( \frac{\mu(d_i) + 1 - \exp(\gamma/\pi)}{\sigma_{\theta}} \right) \]
Lottery compliers apply to charter schools, then accept offers if admitted

LATE for lottery compliers conditional on distance is:

$$LATE(d_i) = E [Y_i(1) - Y_i(0)|\theta_i > \exp(\gamma/\pi) - 1 - \mu(d_i), \mu(d_i) + \theta_i + \xi_i > 0]$$

If distance $d_i$ is independent of potential outcomes, we can use variation in LATEs by distance to understand the nature of selection (requires exclusion restriction)

Students who apply from far away are more selected than students who apply from close by

$LATE(d_i)$ traces out the relationship between unobserved preferences and treatment effects

Informative about external validity
In practice, Walters (2018) takes a parametric approach to estimation.

Likelihood of student $i$’s choices:

$$\mathcal{L}(A_i, Z_i, S_i|d_i) = \int \Phi \left( \frac{\mu(d_i) - \exp(\gamma/\pi) - 1}{\sigma_{\theta}} \right)^{A_i} \left[ 1 - \Phi \left( \frac{\mu(d_i) - \exp(\gamma/\pi) - 1}{\sigma_{\theta}} \right) \right]^{1-A_i}$$

$$\times \pi^{A_i Z_i} (1 - \pi)^{A_i (1-Z_i)}$$

$$\times \left[ \frac{\exp(\mu(d_i) + \theta_i)}{1 + \exp(\mu(d_i) + \theta_i)} \right]^{A_i Z_i S_i} \left[ \frac{1}{1 + \exp(\mu(d_i) + \theta_i)} \right]^{A_i Z_i (1-S_i)} \ dF(\theta_i)$$

Estimate parameters by simulated maximum likelihood, approximating integral by drawing from the distribution of $\theta_i$. 

Chris Walters (UC Berkeley) Self-Selection
Suppose potential outcomes are given by:

\[ E[Y_i(s)|d_i, \theta_i] = \alpha_d + \beta_d \theta_i, \ s \in \{0, 1\} \]

We can estimate these parameters via the OLS regression:

\[ Y_i = \alpha_0 + (\alpha_1 - \alpha_0)S_i + \beta_0 \theta^*(A_i, Z_i, S_i, d_i) + (\beta_1 - \beta_0)S_i \theta^*(A_i, Z_i, S_i, d_i) + e_i \]

Control function \( \theta^*(A_i, Z_i, S_i, d_i) \) is predicted value of unobserved taste \( \theta_i \) given \( i \)'s observed choices and instruments – generalization of Heckit Mills ratio

\( \alpha_1 - \alpha_0 \) is the ATE, while \( \beta_1 \) and \( \beta_0 \) govern selection
The homogeneous charter model includes 43 parameters and generates a log likelihood value of 212,917. Allowing charter heterogeneity adds seven parameters and increases the log likelihood by 854. A likelihood ratio test therefore rejects the model with homogeneous charter schools (p < .01).

Likewise, the single normal model is decisively rejected in a test against the two-mass mixture model (p < .01). I therefore focus on estimates from the mixture model with heterogeneous charter schools and report results for the other two models when these comparisons are useful. Appendix B provides further goodness-of-fit diagnostics for the mixture model.

Table 5 displays utility and application cost estimates from the mixture model. Column 1 shows estimates of the utility parameters $a_j$ and $b_j$, while...
The main effects in columns 2 and 4 of table 7 imply that charter attendance raises eighth-grade math and reading scores by 0.71 and 0.52, on average. Nonwhite students, poor students, and students with lower past achievement lag behind other students in traditional public schools and receive larger benefits from charter school attendance. In this sense, charter schools tend to reduce achievement gaps between racial and socioeconomic groups. This finding is consistent with previous lottery-based estimates showing larger charter impacts for poorer and lower-achieving students.

### TABLE 7

<table>
<thead>
<tr>
<th></th>
<th>Math Scores</th>
<th>Reading Scores</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Public School Outcome</td>
<td>Charter Effect</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Constant/main effect</td>
<td>-.390 (.015)</td>
<td>.705 (.092)</td>
</tr>
<tr>
<td>Female</td>
<td>-.024 (.015)</td>
<td>.060 (.046)</td>
</tr>
<tr>
<td>Black</td>
<td>-.193 (.025)</td>
<td>.250 (.073)</td>
</tr>
<tr>
<td>Hispanic</td>
<td>-.100 (.025)</td>
<td>.260 (.078)</td>
</tr>
<tr>
<td>Subsidized lunch</td>
<td>-.128 (.022)</td>
<td>.192 (.056)</td>
</tr>
<tr>
<td>Special education</td>
<td>-.370 (.020)</td>
<td>.097 (.065)</td>
</tr>
<tr>
<td>Limited English proficiency</td>
<td>.075 (.020)</td>
<td>-.091 (.069)</td>
</tr>
<tr>
<td>Value-added of closest district school</td>
<td>.136 (.049)</td>
<td>.003 (.138)</td>
</tr>
<tr>
<td>4th-grade math score</td>
<td>.476 (.011)</td>
<td>-.122 (.033)</td>
</tr>
<tr>
<td>4th-grade reading score</td>
<td>.066 (.011)</td>
<td>-.019 (.034)</td>
</tr>
<tr>
<td>Charter school preference, $\theta_i$</td>
<td>.058 (.016)</td>
<td>-.096 (.047)</td>
</tr>
<tr>
<td>Idiosyncratic preference, $\tau_{ij}$</td>
<td>...</td>
<td>-.017 (.052)</td>
</tr>
</tbody>
</table>

* $p$-values: no selection on unobservables .001 .051
FIG. 2. — Relationship between distance to charter schools and lottery estimates. This figure displays relationships between lottery-based IV estimates of charter school effects on eighth-grade test scores and distance that applicants travel to apply. Panel A shows results for math scores, and panel B displays results for reading. Estimates come from a 2SLS model that interacts charter school attendance with indicators for terciles of the differential distance between the closest charter school and the closest traditional public school. The instruments are interactions of a lottery offer indicator with differential distance terciles, and both stages control for lottery portfolio indicators and tercile main effects. Dashed lines indicate 95 percent confidence intervals.
Equation (10) also has implications for heterogeneity across charter schools. Specifically, average utilities should be larger for charters that generate larger test score gains. Figure 3 plots school-specific ATE estimates against school-specific mean utilities. In contrast to the prediction of equation (10), this relationship is downward sloping in both math and reading, implying that less popular charter schools tend to produce larger test score impacts. The hypothesis that these parameters lie on a line with weakly positive slope is rejected in both math and reading ($p < .01$).

### B. Selection and Charter School Effects

These test results imply that students do not sort into charter schools to maximize test scores. To further explore the pattern of selection into the charter sector, I next consider a summary measure of the relationship between achievement impacts and preferences for charter schools. Define the preference index $P_i = \beta_1 v_i(\cdot)$.

<table>
<thead>
<tr>
<th>Preference Coefficient</th>
<th>Math Scores</th>
<th>Test Score Gain Coefficient</th>
<th>Ratio</th>
<th>Reading Scores</th>
<th>Test Score Gain Coefficient</th>
<th>Ratio</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td></td>
</tr>
<tr>
<td>Female</td>
<td>-0.046</td>
<td>0.060</td>
<td>-1.313</td>
<td>-0.019</td>
<td>0.407</td>
<td></td>
</tr>
<tr>
<td>Black</td>
<td>-0.465</td>
<td>0.250</td>
<td>-0.538</td>
<td>0.199</td>
<td>-0.429</td>
<td></td>
</tr>
<tr>
<td>Hispanic</td>
<td>-0.376</td>
<td>0.260</td>
<td>-0.691</td>
<td>0.243</td>
<td>-0.646</td>
<td></td>
</tr>
<tr>
<td>Subsidized lunch</td>
<td>-0.298</td>
<td>0.192</td>
<td>-0.644</td>
<td>0.149</td>
<td>-0.499</td>
<td></td>
</tr>
<tr>
<td>Special education</td>
<td>-0.228</td>
<td>0.097</td>
<td>-0.426</td>
<td>0.134</td>
<td>-0.588</td>
<td></td>
</tr>
<tr>
<td>Limited English proficiency</td>
<td>-0.118</td>
<td>-0.091</td>
<td>0.773</td>
<td>-0.074</td>
<td>0.626</td>
<td></td>
</tr>
<tr>
<td>Value-added of closest district school</td>
<td>-1.156</td>
<td>0.003</td>
<td>-0.003</td>
<td>-0.041</td>
<td>0.036</td>
<td></td>
</tr>
<tr>
<td>4th-grade math score</td>
<td>0.138</td>
<td>-0.122</td>
<td>-0.883</td>
<td>-0.043</td>
<td>-0.315</td>
<td></td>
</tr>
<tr>
<td>4th-grade reading score</td>
<td>0.161</td>
<td>-0.019</td>
<td>-0.117</td>
<td>-0.078</td>
<td>-0.481</td>
<td></td>
</tr>
<tr>
<td>Charter school</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>preference, $\theta_i$</td>
<td>1.000</td>
<td>-0.096</td>
<td>-0.096</td>
<td>-0.039</td>
<td>-0.039</td>
<td></td>
</tr>
<tr>
<td>Idiosyncratic preference, $\tau_{ij}$</td>
<td>1.000</td>
<td>-0.017</td>
<td>-0.017</td>
<td>0.010</td>
<td>0.010</td>
<td></td>
</tr>
<tr>
<td>$p$-values: test score maximization</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>&lt;.001</td>
</tr>
</tbody>
</table>

Note. — This table reports tests of restrictions implied by test score maximization based on coefficients for observed characteristics and unobserved tastes. Estimates come from the two-mass mixture model in col. 3 of table 4. Column 1 reports the coefficient on each variable in the charter school utility function, and cols. 2 and 4 report the additional test score gain resulting from charter attendance for students with each characteristic. Columns 3 and 5 report ratios of impacts on test score gains to impacts on preferences. The $p$-values come from Wald tests of the hypothesis that all ratios in a column are equal and weakly positive. The tests are based on methods described by Kodde and Palm (1986) for jointly testing equality and inequality constraints.
To focus on marginal students drawn into the charter sector by expansion, panel D also plots a variant of the average marginal treatment effect (AMTE) parameter discussed by Heckman et al. (2016, 2018). For a student $i$ receiving at least one charter offer, let $j^* \in \text{arg max}_{j \in \mathcal{O}Z_i(t), j \neq 0} V_{ij}$ denote the preferred charter school among those offering seats. Define $D^*_t(t) = \mathbb{E} \left[ Y_{ij}^* - Y_i^0 | j^* \right.$ $\left. \leq t, \mathcal{O}Z_i(t) \neq 0 \right]$.

For small $t$, this parameter describes causal effects for students who are on the margin of deciding whether to remain in traditional public schools and would be induced to enter the charter sector by a small increase in the attractiveness of charter schools. Figure 6 reports $D^*_t(t)$ in each coun-

![Graph](https://example.com/graph.png)
References


References


References

Labor Market Discrimination

- This lecture covers labor market discrimination

- Large gaps in labor market outcomes across demographic groups, e.g. by race, sex, and age
  - Wages
  - Labor force participation
  - Unemployment rates
  - Occupations, job mobility, non-wage compensation

- Theories of discrimination offer explanations for why group membership *per se* might be important

- Evidence looks at effects of group membership on outcomes

- See Altonji and Blank (1999), Lang and Lehmann (2012), and Guryan and Charles (2013) for reviews
declining labor force participation of black men (Brown 1984; Chandra 2000; Juhn 2003). In addition, early improvements can also be credited to both the rise in the relative level of educational attainment (Smith and Welch 1989) and the relative quality of the schools attended by blacks (Card and Krueger 1993). Nevertheless, it is difficult to come up with plausible estimates of the effects of human capital that would fully explain the wage convergence in the 1960s and early 1970s. On the other hand, they make the absence of further convergence in the late 1970s and much of the 1980s even more surprising.

The very large gains made by black men after the mid-to-late 1980s cannot be accounted for by nonearners in the Current Population Survey (CPS) since there was little change during this period. While the proportion of black men age 22–64 who were in prison or jail (and thus not in the CPS sample) grew (Western 2006, table 1.1; Western and Pettit 2005), the increase in incarceration rates cannot explain the large convergence from a black–white earnings ratio of 0.62 in 1987 to 0.77 in 2000. Moreover, Neal (2006) shows that skill convergence between young black and white men stopped and may even have reversed itself among those born after 1960. Thus, overall skill convergence should have slowed after 1990, making it difficult to explain why earnings convergence reasserted itself.

3.2 Employment Differentials

Much less attention has been paid to racial employment and unemployment differentials than to wage differentials although the former are in many ways more dramatic. In 2008, the labor force participation rate of black men age 25–54 was 83.7 percent compared with 91.5 percent among white men. The unemployment rate was 9.1 percent.

![Figure 1. Ratio of Median Earnings: Black Men/White Men, 1967–2009](image)

*Figure 1. Ratio of Median Earnings: Black Men/White Men, 1967–2009*

*Source: Lang and Lehmann (2012)*
Figure 2.
Female-to-Male Earnings Ratio and Median Earnings of Full-Time, Year-Round Workers 15 Years and Older by Sex: 1960 to 2016

Note: The data for 2013 and beyond reflect the implementation of the redesigned income questions. The data points are placed at the midpoints of the respective years. Data on earnings of full-time, year-round workers are not readily available before 1960. For more information on recessions, see Appendix A. For information on confidentiality protection, sampling error, nonsampling error, and definitions, see <www2.census.gov/programs-surveys/cps/techdocs/cpsmar17.pdf>.

Percentage Gap Between Median Men's and Women's Wages, for All Full-Time Workers (2006 or Latest Year Available)

OECD average = 17.6%
Ratio of Median Earnings by Gender and Race (% of White Male)

- White Women, 16 years and over
- Black Men, 16 years and over
- Black Women, 16 years and over
Civilian Labor Force Participation Rate: 20 years and over, Black or African American Men

Civilian Labor Force Participation Rate: 20 years and over, White Men

Shaded areas indicate U.S. recessions

Source: U.S. Bureau of Labor Statistics

myf.red/g/jgQs
against blacks. Figure 3 documents the decline in prejudice as measured by national polls and surveys. The data show large declines since the 1950s and 1960s in whites’ expression of prejudiced views on school segregation, social interaction, and blacks in politics. While we cannot completely discount the possibility that whites are merely becoming more cautious in expressing what are now socially unacceptable views, there is behavioral evidence to support the change. In the late 1950s, over half of whites said they would not vote for a black president. The evidence of the 2008 election suggests that this proportion has declined significantly. In 1958, 94 percent of Americans disapproved of marriage between a white and a black. By 2007, this figure was 17 percent.


Figure 3. Trends in Prejudice Measures, 1956–2003.
Defining Discrimination

- What is discrimination? Arrow (1973):

  “The fact that different groups of workers, be they skilled or unskilled, black or white, male or female, receive different wages, invites the explanation that the different groups must differ according to some characteristic valued on the market. In standard economic theory, we think first of differences in productivity. The notion of discrimination involves the additional concept that personal characteristics of the worker unrelated to productivity are also valued in the market.”

- This is a starting point, but as Altonji and Blank (1999) point out:
  - Defining “productivity” is not straightforward
  - Human capital investments (or technological changes) that affect productivity may be altered by discrimination
Theories of Discrimination

Theories of discrimination generally fall into two broad categories:

- **Taste-based discrimination**: Employers have prejudices that favor one group over another (Becker, 1957)

- **Statistical discrimination**: Employers use group membership to make inferences about productivity (Aigner and Cain, 1977)

N.B.: Both are illegal with respect to treatment of protected groups – race, color, religion, sex (including gender identity and pregnancy), national origin, age, disability, genetic information, sexual orientation, or parental status

We will mostly focus on empirical evidence regarding the effects of protected characteristics rather than trying to distinguish between types of discrimination
Oaxaca-Blinder Decompositions

- Classic tool for measuring discrimination: the **Oaxaca-Blinder decomposition** (Oaxaca, 1973; Blinder, 1973)

- OB method decomposes a difference between groups into a component explained by observed characteristics, and a component explained by returns to characteristics

- Consider individuals in two groups, $G_i \in \{A, B\}$

- Group average outcomes are $\bar{Y}_A$ and $\bar{Y}_B$, where $\bar{Y}_g \equiv E[Y_i | G_i = g]$

- We hope to explain group differences with a vector of observed covariates $X_i$
Oaxaca-Blinder Decompositions

- Quantity to be explained:

\[ \Delta \equiv \bar{Y}_A - \bar{Y}_B \]

- Run a separate regression for each group:
  - Group A: \( Y_i = X_i \beta_A + \epsilon_i \)
  - Group B: \( Y_i = X_i \beta_B + \epsilon_i \)

- \( X_i \) includes a constant

- OLS coefficient vector for group \( g \):

\[
\beta_g = E [X_i X'_i | G_i = g]^{-1} E [X_i Y_i | G_i = g]
\]
Oaxaca-Blinder Decompositions

▶ By construction OLS fits each group’s average:

\[ \bar{Y}_g = \bar{X}_g \beta_g. \]

▶ Therefore we can write

\[ \Delta = \bar{X}_A' \beta_A - \bar{X}_B' \beta_B \]

\[ = (\bar{X}_A - \bar{X}_B)' \beta_A + \bar{X}_B' (\beta_A - \beta_B) \]

Explained by \( X \)'s  
Explained by \( \beta \)'s

▶ First term answers the question: How much more would A’s make than B’s if both groups were paid like A’s for observables?

▶ Second term answers the question: How much more would A’s make than B’s if both groups had the B’s observables?

▶ If \( X \) includes all characteristics relevant to productivity, second term may be attributable to discrimination
Oaxaca-Blinder Decompositions

\[ \Delta = (\bar{X}_A - \bar{X}_B)' \beta_A + \bar{X}_B (\beta_A - \beta_B) \]

\(\text{Explained by } X\text{'s}\)

\(\text{Explained by } \beta\text{'s}\)

- Can also write the alternative decomposition:

\[ \Delta = (\bar{X}_A - \bar{X}_B)' \beta_B + \bar{X}_A (\beta_A - \beta_B) \]

\(\text{Explained by } X\text{'s}\)

\(\text{Explained by } \beta\text{'s}\)

- New first term answers the question: How much more would A’s make than B’s if both groups were paid like B’s for observables?

- New second term answers the question: How much more would A’s make than B’s if both groups had the A’s observables?
Decomposition 1: What if $B'$'s had the same return to $X$ as $A'$'s?
Decomposition 2: What if A’s had the same return to X as B’s?
OB Decompositions and Causality

- There is a close connection between OB decompositions and our discussion of estimating treatment effects under CIA

- Let $D_i = 1\{G_i = A\}$ denote membership in group A, and re-interpret group as treatment status in a selection on observables scenario

- Let $Y_i(1)$ and $Y_i(0)$ denote $i$’s potential outcomes, and suppose CIA holds: $(Y_i(1), Y_i(0)) \perp \perp D_i | X_i$

- Consider a linear model for the conditional mean of each potential outcome:

$$E[Y_i(d)|X_i] = X_i' \beta_d, \ d \in \{0, 1\}$$

- CIA implies $\beta_d$ can be obtained by regressing $Y_i$ on $X_i$ in the sample with $D_i = d$
Once we have the $\beta_d$’s, we can use them to compute any treatment effect of interest

Oaxaca-Blinder versions of average treatment effect parameters:

\[
TOT_{OB} = E [Y_i|D_i = 1] - E [X_i|D_i = 1]' \beta_0
\]

\[
ATE_{OB} = E [X_i]' (\beta_1 - \beta_0)
\]

\[
TNT_{OB} = E [X_i|D_i = 0]' \beta_1 - E [Y_i|D_i = 0]
\]

Oaxaca uses a linear model to impute missing potential outcomes using each group’s regression function
Alternative Decompositions

- **Oaxaca decomposition of observed difference between treatment and control:**

\[
E [Y_i|D_i = 1] - E [Y_i|D_i = 0] = E [X_i|D_i = 1]' \beta_1 - E [X_i|D_i = 0]' \beta_0
\]

\[
= E [X_i|D_i = 1]' (\beta_1 - \beta_0) + (E [X_i|D_i = 1] - E [X_i|D_i = 0])' \beta_0
\]

- **Selection bias**

- **TOT**

- **TNT**

- **We could’ve instead written**

\[
E [X_i|D_i = 0]' (\beta_1 - \beta_0) + (E [X_i|D_i = 1] - E [X_i|D_i = 0])' \beta_1
\]

- **Selection bias**

- **TNT**

- **TOT**

- **The OB decomposition is not unique for the same reason the definition of selection bias is not unique: there are multiple counterfactuals we might like to impute**
Oaxaca as Reweighting

▶ Oaxaca-Blinder counterfactual for the treatment group:

\[ E[Y_i(0)|D_i = 1] = E[X_i|D_i = 1]'\beta_0 \]

▶ Kline (2011): Can rewrite the OB counterfactual as a weighted average of control outcomes:

\[ E [X_i|D_i = 1]'\beta_0 = E [\tilde{w}(X_i)Y_i|D_i = 0] \]

▶ Weights are

\[
\tilde{w}(X_i) = X'_iE [X_iX'_i|D_i = 0]^{-1} \times E \left[ X_i \frac{p(X_i)}{1-p(X_i)} \left( \frac{1-\pi}{\pi} \right) |D_i = 0 \right]
\]

▶ Here \( p(X_i) \equiv \Pr[D_i = 1|X_i] \) is the propensity score and \( \pi = \Pr [D_i = 1] \) is the unconditional probability of treatment

▶ Oaxaca is a version of propensity-score reweighting
Oaxaca as Reweighting

\[ \tilde{w}(X_i) = X'_i E [X_iX'_i | D_i = 0]^{-1} \times E \left[ X_i \frac{p(X_i)}{1 - p(X_i)} \left( \frac{1 - \pi}{\pi} \right) | D_i = 0 \right] \]

▶ Weights are fitted values from an OLS regression of the conditional odds of treatment on \( X_i \) in the control group

▶ Oaxaca-Blinder estimator is **doubly robust**: works if either \( E[Y_i(0)|X_i] \) or \( (p(X_i)/(1 - p(X_i))) \) is linear in \( X_i \)

▶ Note that if controls are saturated linearity is guaranteed (not a restriction)

▶ Think of Oaxaca as another method of adjusting for observables under CIA – one that is particularly easy to implement and interpret
Chetty et al. (2020) perform Oaxaca-style decompositions of racial differences in income into components explained and unexplained by parent income.


- Parent income averaged over five years
- Child income averaged over two years, at ages between 31 and 37

Race measured in 2010 census.

Also match to American Community Survey (ACS) data – provides data on hours, wages, education, occupation.
FIGURE III: Intergenerational Mobility by Race

A. All Children

Panel A replicates Figure IIa, including series for Hispanics, Asians, and American Indians. Panel B replicates Panel A for children whose mothers were born in the U.S. Panel B is based on the subsample of children whose mothers appear in the 2000 Census long form or the 2005-2015 American Community Survey because information on parental birthplace is available only for those individuals. See notes to Figure II for further details.
FIGURE V: Black-White Gaps in Individual Income, by Gender

A. Males

- Diff. at p=25: 9.7
- Diff. at p=75: 12.0

B. Females

- Diff. at p=25: -1.4
- Diff. at p=75: -1.0

Notes:
These figures replicate Figure IVb separately for male children (Panel A) and female children (Panel B). Individual income ranks are computed within a child’s cohort pooling across race and gender. See notes to Figure IV for further details.
A. Males

Total white/black gap for males: 18 percentage points

Mean Child Individual Income Rank vs Parent Household Income Rank

- White (Intercept: 41.36, Slope: 0.29)
- Black (Intercept: 31.80, Slope: 0.27)

Total gap

Diff. at p=25: 9.7

Diff. at p=75: 12.0
A. Males

Total white/black gap for males: 18 percentage points

Explained by parent income: 8 percentage points (44\% of gap)

Diff. at p=75: 12.0

Explained by slope and intercept: 10 percentage points (56\% of gap)

- White (Intercept: 41.36, Slope: 0.29)
- Black (Intercept: 31.80, Slope: 0.27)
FIGURE V: Black-White Gaps in Individual Income, by Gender

A. Males

Diff. at p=25: 9.7  
Diff. at p=75: 12.0

B. Females

Diff. at p=25: -1.4  
Diff. at p=75: -1.0

Notes:
These figures replicate Figure IVb separately for male children (Panel A) and female children (Panel B). Individual income ranks are computed within a child’s cohort pooling across race and gender. See notes to Figure IV for further details.
B. Females

Total white/black gap for females: 5 percentage points

- Total gap
- Diff. at p=75: -1.0
- Diff. at p=25: -1.4

- **White** (Intercept: 33.30, Slope: 0.25)
- **Black** (Intercept: 34.86, Slope: 0.25)
B. Females

Total white/black gap for females: 5 percentage points

Explained by parent income: 6 percentage points (120% of gap)

Diff. at p=75: -1.0

Explained by slope and intercept: -1 percentage point (-20% of gap)

Diff. at p=25: -1.4

- White (Intercept: 33.30, Slope: 0.25)
- Black (Intercept: 34.86, Slope: 0.25)
FIGURE VI: Black-White Gaps in Wage Rates, Hours, and Employment, by Gender

A. Wage Rank, Females

Diff. at p=25: 1.9
Diff. at p=75: 1.5

B. Wage Rank, Males

Diff. at p=25: 6.4
Diff. at p=75: 7.9

C. Hours Worked, Females

Diff. at p=25: -1.0
Diff. at p=75: -1.3

D. Hours Worked, Males

Diff. at p=25: 10.6
Diff. at p=75: 8.1

E. Employment Rates, Females

Diff. at p=25: -2.0
Diff. at p=75: -2.4

F. Employment Rates, Males

Diff. at p=25: 18.9
Diff. at p=75: 11.4

Notes:
This figure shows the relationship between children’s employment outcomes and their parents’ household income, by race and gender. All children’s outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
FIGURE VI: Black-White Gaps in Wage Rates, Hours, and Employment, by Gender

A. Wage Rank, Females

Diff. at p=25: 1.9

Diff. at p=75: 1.5

B. Wage Rank, Males

Diff. at p=25: 6.4

Diff. at p=75: 7.9

C. Hours Worked, Females

Diff. at p=25: -1.0

Diff. at p=75: -1.3

D. Hours Worked, Males

Diff. at p=25: 10.6

Diff. at p=75: 8.1

E. Employment Rates, Females

Diff. at p=25: -2.0

Diff. at p=75: -2.4

F. Employment Rates, Males

Diff. at p=25: 18.9

Diff. at p=75: 11.4

Notes:
This figure shows the relationship between children's employment outcomes and their parents' household income, by race and gender. All children's outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
FIGURE VI: Black-White Gaps in Wage Rates, Hours, and Employment, by Gender

A. Wage Rank, Females

Diff. at p=25: 1.9

Diff. at p=75: 1.5

B. Wage Rank, Males

Diff. at p=25: 6.4

Diff. at p=75: 7.9

C. Hours Worked, Females

Diff. at p=25: -1.0

Diff. at p=75: -1.3

D. Hours Worked, Males

Diff. at p=25: 10.6

Diff. at p=75: 8.1

E. Employment Rates, Females

Diff. at p=25: -2.0

Diff. at p=75: -2.4

F. Employment Rates, Males

Diff. at p=25: 18.9

Diff. at p=75: 11.4

Notes:
This figure shows the relationship between children's employment outcomes and their parents' household income, by race and gender. All children's outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
C. Hours Worked, Females

Weekly Hours Worked in ACS (Age >= 30)

- Diff. at p=25: -1.0
- Diff. at p=75: -1.3

Parent Household Income Rank

White
Black

Notes:
This figure shows the relationship between children's employment outcomes and their parents' household income, by race and gender. All children's outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
FIGURE VI: Black-White Gaps in Wage Rates, Hours, and Employment, by Gender

A. Wage Rank, Females

- Diff. at p=25: 1.9
- Diff. at p=75: 1.5

B. Wage Rank, Males

- Diff. at p=25: 6.4
- Diff. at p=75: 7.9

C. Hours Worked, Females

- Diff. at p=25: -1.0
- Diff. at p=75: -1.3

D. Hours Worked, Males

- Diff. at p=25: 10.6
- Diff. at p=75: 8.1

E. Employment Rates, Females

- Diff. at p=25: -2.0
- Diff. at p=75: -2.4

F. Employment Rates, Males

- Diff. at p=25: 18.9
- Diff. at p=75: 11.4

Notes:

This figure shows the relationship between children's employment outcomes and their parents' household income, by race and gender. All children's outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
FIGURE VI: Black-White Gaps in Wage Rates, Hours, and Employment, by Gender

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Diff. at p=25: 1.9
Diff. at p=75: 1.5

B. Wage Rank, Males
Diff. at p=25: 6.4
Diff. at p=75: 7.9

C. Hours Worked, Females
Diff. at p=25: -1.0
Diff. at p=75: -1.3

D. Hours Worked, Males
Diff. at p=25: 10.6
Diff. at p=75: 8.1

E. Employment Rates, Females
Diff. at p=25: -2.0
Diff. at p=75: -2.4

F. Employment Rates, Males
Diff. at p=25: 18.9
Diff. at p=75: 11.4

Notes:
This figure shows the relationship between children's employment outcomes and their parents' household income, by race and gender. All children's outcomes in this figure are obtained from the American Community Survey and all panels include only children observed in the 2005-15 ACS at age 30 or older. Panels A and B plot mean wage ranks vs. parental household income percentile, by race and gender. Panels C and D replicate A and B using mean weekly hours of work as the outcome, while Panels E and F use annual employment rates as the outcome. Wages are computed as self-reported annual earnings divided by total hours of work; they are missing for those who do not work. We convert wages to percentile ranks by ranking individuals relative to others in the same birth cohort who received the ACS survey in the same year. Hours of work are defined as total annual hours of work divided by 51 and are coded as zero for those who do not work. Employment is defined as having positive hours of work in the past 12 months. To protect confidentiality, bins in which there are fewer than 10 children who are employed or not employed are suppressed in Panels E and F. In each figure, the best-fit lines are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines at the 25th and 75th parent income percentiles.
FIGURE VII: Black-White Gaps in Educational Attainment and Incarceration, by Gender

A. High School Completion Rates, Females
- Diff. at p=25: 3.5
- Diff. at p=75: 1.5

B. High School Completion Rates, Males
- Diff. at p=25: 8.3
- Diff. at p=75: 4.2

C. College Attendance Rates, Females
- Diff. at p=25: 2.8
- Diff. at p=75: 3.6

D. College Attendance Rates, Males
- Diff. at p=25: 6.5
- Diff. at p=75: 7.7

E. Incarceration, Females

F. Incarceration, Males
- Diff. at p=25: -8.2
- Diff. at p=75: -3.2

Notes:
Panels A-D show the relationship between children's educational attainment and their parents' household income, by race and gender. Data on educational attainment is obtained from the American Community Survey. Panels A and B plot the fraction of children who complete high school by parental income percentile, by race and gender. Panels C and D replicate Panels A and B using college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. High school completion is defined as having a high school diploma or GED. College attendance is defined as having obtained "at least some college credit." Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
A. High School Completion Rates, Females

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- Diff. at p=75: 1.5

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- Diff. at p=25: 8.3
- Diff. at p=75: 4.2

C. College Attendance Rates, Females

- Diff. at p=25: 2.8
- Diff. at p=75: 3.6

D. College Attendance Rates, Males

- Diff. at p=25: 6.5
- Diff. at p=75: 7.7

E. Incarceration, Females

F. Incarceration, Males

- Diff. at p=25: -8.2
- Diff. at p=75: -3.2

Notes:
Panels A-D show the relationship between children's educational attainment and their parents' household income, by race and gender. Data on educational attainment is obtained from the American Community Survey. Panels A and B plot the fraction of children who complete high school by parental income percentile, by race and gender. Panels C and D replicate Panels A and B using college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. High school completion is defined as having a high school diploma or GED. College attendance is defined as having obtained "at least some college credit". Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
D. College Attendance Rates, Males

Panel D of Figure VII presents the relationship between children's college attendance rates and their parents' household income, by race and gender. Data on college attendance is obtained from the American Community Survey. Panels D replicate Panels A and B using data on college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. College attendance is defined as having obtained "at least some college credit." Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
FIGURE VII: Black-White Gaps in Educational Attainment and Incarceration, by Gender

A. High School Completion Rates, Females

Diff. at p=25: 3.5
Diff. at p=75: 1.5

C. College Attendance Rates, Females

Diff. at p=25: 2.8
Diff. at p=75: 3.6

D. College Attendance Rates, Males

Diff. at p=25: 6.5
Diff. at p=75: 7.7

E. Incarceration, Females

F. Incarceration, Males

Diff. at p=25: -8.2
Diff. at p=75: -3.2

Notes:
Panels A-D show the relationship between children's educational attainment and their parents' household income, by race and gender. Data on educational attainment is obtained from the American Community Survey. Panels A and B plot the fraction of children who complete high school by parental income percentile, by race and gender. Panels C and D replicate Panels A and B using college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. High school completion is defined as having a high school diploma or GED. College attendance is defined as having obtained "at least some college credit". Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
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B. High School Completion Rates, Males
- Diff. at p=25: 8.3
- Diff. at p=75: 4.2

C. College Attendance Rates, Females
- Diff. at p=25: 2.8
- Diff. at p=75: 3.6

D. College Attendance Rates, Males
- Diff. at p=25: 6.5
- Diff. at p=75: 7.7

E. Incarceration, Females

F. Incarceration, Males
- Diff. at p=25: -8.2
- Diff. at p=75: -3.2

Notes:
Panels A-D show the relationship between children's educational attainment and their parents' household income, by race and gender. Data on educational attainment is obtained from the American Community Survey. Panels A and B plot the fraction of children who complete high school by parental income percentile, by race and gender. Panels C and D replicate Panels A and B using college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. High school completion is defined as having a high school diploma or GED. College attendance is defined as having obtained “at least some college credit.” Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
E. Incarceration, Females

FIGURE VII: Black-White Gaps in Educational Attainment and Incarceration, by Gender

A. High School Completion Rates, Females

B. High School Completion Rates, Males

C. College Attendance Rates, Females

D. College Attendance Rates, Males

E. Incarceration, Females

F. Incarceration, Males

Notes:
Panels A-D show the relationship between children's educational attainment and their parents' household income, by race and gender. Data on educational attainment is obtained from the American Community Survey. Panels A and B plot the fraction of children who complete high school by parental income percentile, by race and gender. Panels C and D replicate Panels A and B using college attendance as the outcome. Panels A-B include only children observed in the 2005-15 ACS at age 19 or older, while Panels C-D include those observed at age 20 or older. High school completion is defined as having a high school diploma or GED. College attendance is defined as having obtained "at least some college credit". Panels E and F plot incarceration rates vs. parent income percentile, by race and gender. Incarceration is defined as being incarcerated on April 1, 2010 using data from the 2010 Census short form. The children in our sample are between the ages of 27-32 at that point. The best-fit lines in Panels A-D are estimated using OLS regressions on the binned series. We report white-black differences based on the best-fit lines (in Panels A-D) and based directly on the non-parametric estimates (in Panel F) at the 25th and 75th parent income percentiles. To protect confidentiality, bins in which there are fewer than 10 children who exhibit the outcome or who do not exhibit the outcome are suppressed.
Experimental and Non-Experimental Approaches

- Observational decompositions provide valuable descriptive evidence, but suffer from the usual issues with non-experimental research designs.

- Link between Oaxaca and treatment effect estimation under CIA is useful for thinking about potential issues.

- Oaxaca decompositions can overstate or understate the extent of discrimination.
  - Productivity-relevant characteristics that differ across groups may be excluded from $X_i$ (omitted variable bias).
  - Discrimination may affect the distribution of $X_i$ (bad control).

- Motivating by these issues, a parallel strand of literature uses experimental approaches to measure discrimination.
Audit and Correspondence Studies

► Common experimental approach to studying discrimination: audit and correspondence studies

► In-person audit studies: send pairs of auditors matched on personal characteristics but different on some dimension of interest (e.g. race) to apply for a real job

► Resume correspondence studies: send fictitious resumes to real jobs with randomly assigned names that signify protected characteristics

► Pioneered by Bertrand and Mullainathan (2004) to study racial discrimination

► Baert (2018) counts 90 correspondence studies on hiring discrimination since 2005

► See Bertrand and Duflo (2017) for a survey of audit and correspondence studies in economics
Bertrand and Mullainathan (2004)

- BM sent four fake resumes to (almost) every employment ad posted in the Boston Globe or Chicago Tribune between summer 2001 and spring 2002 in sales, administrative support, clerical and customer services

- Choose racially distinctive names based on empirical likelihood ratios among all children born in MA 1974-79

- Resume “bank” built from characteristics of real resumes posted on job search web sites

- Every job receives two resumes with white names and two resumes with black names

- Main outcome: Employer callback
## Table A1—First Names Used in Experiment

<table>
<thead>
<tr>
<th>White female Name</th>
<th>L(W)/L(B)</th>
<th>Perception White</th>
<th>African-American female Name</th>
<th>L(B)/L(W)</th>
<th>Perception Black</th>
</tr>
</thead>
<tbody>
<tr>
<td>Allison</td>
<td>∞</td>
<td>0.926</td>
<td>Aisha</td>
<td>209</td>
<td>0.97</td>
</tr>
<tr>
<td>Anne</td>
<td>∞</td>
<td>0.962</td>
<td>Ebony</td>
<td>∞</td>
<td>0.9</td>
</tr>
<tr>
<td>Carrie</td>
<td>∞</td>
<td>0.923</td>
<td>Keisha</td>
<td>116</td>
<td>0.93</td>
</tr>
<tr>
<td>Emily</td>
<td>∞</td>
<td>0.925</td>
<td>Kenya</td>
<td>∞</td>
<td>0.967</td>
</tr>
<tr>
<td>Jill</td>
<td>∞</td>
<td>0.889</td>
<td>Lakisha</td>
<td>∞</td>
<td>0.967</td>
</tr>
<tr>
<td>Laurie</td>
<td>∞</td>
<td>0.963</td>
<td>Latonya</td>
<td>∞</td>
<td>1</td>
</tr>
<tr>
<td>Kristen</td>
<td>∞</td>
<td>0.963</td>
<td>Latoya</td>
<td>∞</td>
<td>1</td>
</tr>
<tr>
<td>Meredith</td>
<td>∞</td>
<td>0.926</td>
<td>Tamika</td>
<td>284</td>
<td>1</td>
</tr>
<tr>
<td>Sarah</td>
<td>∞</td>
<td>0.852</td>
<td>Tanisha</td>
<td>∞</td>
<td>1</td>
</tr>
</tbody>
</table>

**Fraction of all births:**
- White female: 3.8 percent
- African-American female: 7.1 percent

Notes: This table tabulates the different first names used in the experiment and their identifiability. The first column reports the likelihood that a baby born with that name (in Massachusetts between 1974 and 1979) is White (or African-American) relative to the likelihood that it is African-American (White). The second column reports the probability that the name was picked as White (or African-American) in an independent field survey of people. The last row for each group of names shows the proportion of all births in that race group that these names account for.
### Table A1: First Names Used in Experiment

<table>
<thead>
<tr>
<th>White male Name</th>
<th>L(W)/L(B)</th>
<th>Perception White</th>
<th>African-American male Name</th>
<th>L(B)/L(W)</th>
<th>Perception Black</th>
</tr>
</thead>
<tbody>
<tr>
<td>Brad</td>
<td>∞</td>
<td>1</td>
<td>Darnell</td>
<td>∞</td>
<td>0.967</td>
</tr>
<tr>
<td>Brendan</td>
<td>∞</td>
<td>0.667</td>
<td>Hakim</td>
<td></td>
<td>0.933</td>
</tr>
<tr>
<td>Geoffrey</td>
<td>∞</td>
<td>0.731</td>
<td>Jamal</td>
<td>257</td>
<td>0.967</td>
</tr>
<tr>
<td>Greg</td>
<td>∞</td>
<td>1</td>
<td>Jermaine</td>
<td>90.5</td>
<td>1</td>
</tr>
<tr>
<td>Brett</td>
<td>∞</td>
<td>0.923</td>
<td>Kareem</td>
<td>∞</td>
<td>0.967</td>
</tr>
<tr>
<td>Jay</td>
<td>∞</td>
<td>0.926</td>
<td>Leroy</td>
<td>44.5</td>
<td>0.933</td>
</tr>
<tr>
<td>Matthew</td>
<td>∞</td>
<td>0.888</td>
<td>Rasheed</td>
<td>∞</td>
<td>0.931</td>
</tr>
<tr>
<td>Neil</td>
<td>∞</td>
<td>0.654</td>
<td>Tremayne</td>
<td>∞</td>
<td>0.897</td>
</tr>
<tr>
<td>Todd</td>
<td>∞</td>
<td>0.926</td>
<td>Tyrone</td>
<td>62.5</td>
<td>0.900</td>
</tr>
</tbody>
</table>

**Fraction of all births:**

- White: 1.7 percent
- African-American: 3.1 percent
### Table 1—Mean Callback Rates by Racial Soundingness of Names

<table>
<thead>
<tr>
<th>Sample:</th>
<th>Percent callback for White names</th>
<th>Percent callback for African-American names</th>
<th>Ratio</th>
<th>Percent difference (p-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>All sent resumes</td>
<td>9.65</td>
<td>6.45</td>
<td>1.50</td>
<td>3.20 (0.0000)</td>
</tr>
<tr>
<td></td>
<td>[2,435]</td>
<td>[2,435]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Chicago</td>
<td>8.06</td>
<td>5.40</td>
<td>1.49</td>
<td>2.66 (0.0057)</td>
</tr>
<tr>
<td></td>
<td>[1,352]</td>
<td>[1,352]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Boston</td>
<td>11.63</td>
<td>7.76</td>
<td>1.50</td>
<td>4.05 (0.0023)</td>
</tr>
<tr>
<td></td>
<td>[1,083]</td>
<td>[1,083]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Females</td>
<td>9.89</td>
<td>6.63</td>
<td>1.49</td>
<td>3.26 (0.0003)</td>
</tr>
<tr>
<td></td>
<td>[1,860]</td>
<td>[1,886]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Females in administrative jobs</td>
<td>10.46</td>
<td>6.55</td>
<td>1.60</td>
<td>3.91 (0.0003)</td>
</tr>
<tr>
<td></td>
<td>[1,358]</td>
<td>[1,359]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Females in sales jobs</td>
<td>8.37</td>
<td>6.83</td>
<td>1.22</td>
<td>1.54 (0.3523)</td>
</tr>
<tr>
<td></td>
<td>[502]</td>
<td>[527]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Males</td>
<td>8.87</td>
<td>5.83</td>
<td>1.52</td>
<td>3.04 (0.0513)</td>
</tr>
<tr>
<td></td>
<td>[575]</td>
<td>[549]</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: The table reports, for the entire sample and different subsamples of sent resumes, the callback rates for applicants with a White-sounding name (column 1) and an African-American-sounding name (column 2), as well as the ratio (column 3) and difference (column 4) of these callback rates. In brackets in each cell is the number of resumes sent in that cell. Column 4 also reports the p-value for a test of proportion testing the null hypothesis that the callback rates are equal across racial groups.
### Table 2—Distribution of Callbacks by Employment Ad

<table>
<thead>
<tr>
<th>Equal Treatment:</th>
<th>No Callback</th>
<th>1W + 1B</th>
<th>2W + 2B</th>
</tr>
</thead>
<tbody>
<tr>
<td>88.13 percent</td>
<td>83.37</td>
<td>3.48</td>
<td>1.28</td>
</tr>
<tr>
<td>[1,166]</td>
<td>[1,103]</td>
<td>[46]</td>
<td>[17]</td>
</tr>
<tr>
<td>Whites Favored (WF):</td>
<td>1W + 0B</td>
<td>2W + 0B</td>
<td>2W + 1B</td>
</tr>
<tr>
<td>8.39 percent</td>
<td>5.59</td>
<td>1.44</td>
<td>1.36</td>
</tr>
<tr>
<td>[111]</td>
<td>[74]</td>
<td>[19]</td>
<td>[18]</td>
</tr>
<tr>
<td>African-Americans Favored (BF):</td>
<td>1B + 0W</td>
<td>2B + 0W</td>
<td>2B + 1W</td>
</tr>
<tr>
<td>3.48 percent</td>
<td>2.49</td>
<td>0.45</td>
<td>0.53</td>
</tr>
<tr>
<td>[46]</td>
<td>[33]</td>
<td>[6]</td>
<td>[7]</td>
</tr>
</tbody>
</table>

Ho: WF = BF

$p = 0.0000$
<table>
<thead>
<tr>
<th>Name</th>
<th>Percent callback</th>
<th>Mother education</th>
<th>Name</th>
<th>Percent callback</th>
<th>Mother education</th>
</tr>
</thead>
<tbody>
<tr>
<td>Emily</td>
<td>7.9</td>
<td>96.6</td>
<td>Aisha</td>
<td>2.2</td>
<td>77.2</td>
</tr>
<tr>
<td>Anne</td>
<td>8.3</td>
<td>93.1</td>
<td>Keisha</td>
<td>3.8</td>
<td>68.8</td>
</tr>
<tr>
<td>Jill</td>
<td>8.4</td>
<td>92.3</td>
<td>Tamika</td>
<td>5.5</td>
<td>61.5</td>
</tr>
<tr>
<td>Allison</td>
<td>9.5</td>
<td>95.7</td>
<td>Lakisha</td>
<td>5.5</td>
<td>55.6</td>
</tr>
<tr>
<td>Laurie</td>
<td>9.7</td>
<td>93.4</td>
<td>Tanisha</td>
<td>5.8</td>
<td>64.0</td>
</tr>
<tr>
<td>Sarah</td>
<td>9.8</td>
<td>97.9</td>
<td>Latoya</td>
<td>8.4</td>
<td>55.5</td>
</tr>
<tr>
<td>Meredith</td>
<td>10.2</td>
<td>81.8</td>
<td>Kenya</td>
<td>8.7</td>
<td>70.2</td>
</tr>
<tr>
<td>Carrie</td>
<td>13.1</td>
<td>80.7</td>
<td>Latonya</td>
<td>9.1</td>
<td>31.3</td>
</tr>
<tr>
<td>Kristen</td>
<td>13.1</td>
<td>93.4</td>
<td>Ebony</td>
<td>9.6</td>
<td>65.6</td>
</tr>
<tr>
<td>Average</td>
<td></td>
<td>91.7</td>
<td>Average</td>
<td></td>
<td>61.0</td>
</tr>
<tr>
<td>Overall</td>
<td></td>
<td>83.9</td>
<td>Overall</td>
<td></td>
<td>70.2</td>
</tr>
<tr>
<td>Correlation</td>
<td>−0.318</td>
<td>(p = 0.404)</td>
<td>Correlation</td>
<td>−0.383</td>
<td>(p = 0.309)</td>
</tr>
</tbody>
</table>

Notes: This table reports, for each first name used in the experiment, callback rate and average mother education. Mother education for a given first name is defined as the percent of babies born with that name in Massachusetts between 1970 and 1986 whose mother had at least completed a high school degree (see text for details). Within each sex/race group, first names are ranked by increasing callback rate. "Average" reports, within each race-gender group, the average mother education for all the babies born with one of the names used in the experiment. "Overall" reports, within each race-gender group, average mother education for all babies born in Massachusetts between 1970 and 1986 in that race-gender group. "Correlation" reports the Spearman rank order correlation between callback rate and mother education within each race-gender group as well as the p-value for the test of independence.
### Table 8: Callback Rate and Mother's Education by First Name

<table>
<thead>
<tr>
<th>White female</th>
<th>African-American female</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Name</strong></td>
<td><strong>Percent callback</strong></td>
</tr>
<tr>
<td>Emily</td>
<td>7.9</td>
</tr>
<tr>
<td>Anne</td>
<td>8.3</td>
</tr>
<tr>
<td>Jill</td>
<td>8.4</td>
</tr>
<tr>
<td>Allison</td>
<td>9.5</td>
</tr>
<tr>
<td>Laurie</td>
<td>9.7</td>
</tr>
<tr>
<td>Sarah</td>
<td>9.8</td>
</tr>
<tr>
<td>Meredith</td>
<td>10.2</td>
</tr>
<tr>
<td>Carrie</td>
<td>13.1</td>
</tr>
<tr>
<td>Kristen</td>
<td>13.1</td>
</tr>
<tr>
<td>Average</td>
<td></td>
</tr>
<tr>
<td>Overall</td>
<td></td>
</tr>
<tr>
<td>Correlation</td>
<td>-0.318</td>
</tr>
</tbody>
</table>

Notes: This table reports, for each first name used in the experiment, callback rate and average mother education. Mother education for a given first name is defined as the percent of babies born with that name in Massachusetts between 1970 and 1986 whose mother had at least completed a high school degree (see text for details). Within each sex/race group, first names are ranked by increasing callback rate. "Average" reports, within each race-gender group, the average mother education for all the babies born with one of the names used in the experiment. "Overall" reports, within each race-gender group, average mother education for all babies born in Massachusetts between 1970 and 1986 in that race-gender group. "Correlation" reports the Spearman rank order correlation between callback rate and mother education within each race-gender group as well as the p-value for the test of independence.

For each first name in our experiment, we compute the fraction of babies with that name and, in that gender-race cell, whose mothers have at least completed a high school degree.

In Table 8, we display the average callback rate for each first name along with this proxy for social background. Within each race-gender group, the names are ranked by increasing callback rate. Interestingly, there is significant variation in callback rate across names.

We use a smaller data set of Massachusetts births for the years 1970 to 1986 (kindly provided to us by Steven Levitt). This longer time span (compared to that used to assess name frequencies) was imposed on us for confidentiality reasons. When fewer than 10 births with education data available are recorded in a particular education-name cell, the exact number of births in that cell is not reported and we impute five births. Our results are not sensitive to this imputation. One African-American female name (Latonya) and two male names (Rasheed and Hakim) were imputed in this way. One African-American male name (Tremayne) had too few births with available education data and was therefore dropped from this analysis. Our results are qualitatively similar when we use a larger data set of California births for the years 1989 to 2000 (kindly provided to us by Steven Levitt).
Critiques of Audit Studies

- Audit and correspondence studies have been criticized for a variety of reasons (e.g. by Heckman and Siegelman, 1993 and Heckman, 1998)
  - Demand effects in in-person audit experiments
  - Behavior of average vs. marginal firms
  - Distinctively black names may signify attributes other than race (Gaddis, 2017)
  - Economic significance of callback outcome
  - Effects may be due to statistical rather than taste-based discrimination (do we care?)
- As noted by Guryan and Charles (2013), despite issues with interpretation, results from correspondence studies appear to demonstrate firms illegally use protected characteristics in the hiring process
Variation in Discrimination: Kline and Walters (forthcoming)

- Correspondence studies typically focus on market-level averages of discrimination

- Distribution of discrimination across employers is important for both research and policy
  
  - Economic models imply equilibrium impact of discrimination depends on prejudice of marginal employer rather than the average (Becker, 1957)
  
  - Enforcement of anti-discrimination law requires identifying individual offenders (e.g., EEOC charges)

- Kline and Walters (forthcoming) revisit correspondence evidence to study variation across employers

- Basic idea: Correspondence studies sending multiple applications per job provide a window into employer heterogeneity
Starting point is a model for callbacks as independent Bernoulli trials

Potential outcomes of application \( i \in \{1, \ldots, N \} \) to job \( j \in \{1, \ldots, J \} \) as a function of race \( r \in \{b, w\} \):

\[
Y_{ij}(r) \overset{iid}{\sim} \text{Bernoulli}(p_{jr})
\]

Key restriction is iid assumption: repeated trials at the same job are draws from a stable callback process

A job is defined by its callback probabilities, \( p_{jb} \) and \( p_{jw} \)

Discriminators have \( p_{jb} \neq p_{jw} \)
Independent trials implies callback counts $C_{jw}$ and $C_{jb}$ are binomial:

$$f(C_{jw}, C_{jb}|p_{jw}, p_{jb}) = \binom{N_w}{C_{jw}} p_{jw}^{C_{jw}} (1 - p_{jw})^{N_w - C_{jw}} \times \binom{N_b}{C_{jb}} p_{jb}^{C_{jb}} (1 - p_{jb})^{N_b - C_{jb}}$$

Next, think about the joint distribution of $p_{jw}$ and $p_{jb}$ across jobs:

$$(p_{jw}, p_{jb}) \sim G(p_w, p_b)$$

This is a hierarchical model

- Binomial trials for each job
- Heterogeneous success probabilities across jobs
Kline and Walters (forthcoming): Importance of $G(\cdot)$

- Distribution function $G(\cdot)$ describes heterogeneity in callback levels and discrimination. Share of jobs that discriminate is:

$$\bar{\pi} = \int_{p_w \neq p_b} dG(p_w, p_b)$$

- $G(\cdot)$ can also help us interpret evidence for individual jobs

- By Bayes’ rule, share of non-discriminators among jobs with callback counts $(C_{jw}, C_{jb})$ is:

$$\Pr[p_{jw} \neq p_{jb} | C_{jw}, C_{jb}] = \frac{f(C_{jw}, C_{jb} | p_{jw} \neq p_{jb}) \Pr(p_{jw} \neq p_{jb})}{f(C_{jw}, C_{jb})}$$

$$= \int_{p_w \neq p_b} f(C_{jw}, C_{jb} | p_w, p_b) dG(p_w, p_b) \bar{\pi}$$

$$f(C_{jw}, C_{jb})$$

- If we knew $G(\cdot)$, we could calculate this probability

- **Empirical Bayes** approach: plug in an estimator $\hat{G}(\cdot)$ of the cross-job distribution to form posteriors for individual jobs
Kline and Walters (forthcoming): Identification of $G(\cdot)$

- It turns out that some features of $G(\cdot)$ are identified with only a few applications per job

- Share of jobs with callback counts $(c_w, c_b)$:

$$f(c_w, c_b) = \left( \begin{array}{c} N_w \\ c_w \end{array} \right) \left( \begin{array}{c} N_b \\ c_b \end{array} \right) E \left[ p_jw^{c_w} (1 - p_jw)^{N_w - c_w} p_jb^{c_b} (1 - p_jb)^{N_b - c_b} \right]$$

$$= \left( \begin{array}{c} N_w \\ c_w \end{array} \right) \left( \begin{array}{c} N_b \\ c_b \end{array} \right) \sum_{m=0}^{N_w-c_w} \sum_{n=0}^{N_b-c_b} (-1)^{m+n} \left( \begin{array}{c} N_w \\ m \end{array} \right) \left( \begin{array}{c} N_b \\ n \end{array} \right) E \left[ p_jw^{c_w+m} p_jb^{c_b+n} \right]$$

- Unconditional callback probabilities are functions of moments of $G(\cdot)$
- Can solve for all moments $E[p_jw^m p_jb^n]$ for $0 \leq m \leq N_w$ and $0 \leq n \leq N_b$
- With two or more apps per race at each job, can identify measures of heterogeneity, e.g. $\text{Var}(p_jb - p_jw)$
<table>
<thead>
<tr>
<th></th>
<th>$p_b$ (1)</th>
<th>$p_w$ (2)</th>
<th>$p_b - p_w$ (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Mean</strong></td>
<td>0.063</td>
<td>0.094</td>
<td>-0.031</td>
</tr>
<tr>
<td></td>
<td>(0.006)</td>
<td>(0.007)</td>
<td>(0.006)</td>
</tr>
<tr>
<td><strong>Standard deviation</strong></td>
<td>0.152</td>
<td>0.199</td>
<td>0.082</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.012)</td>
<td>(0.016)</td>
</tr>
<tr>
<td><strong>Correlation with $p_w$ or $p_f$</strong></td>
<td>0.927</td>
<td>1.00</td>
<td>-0.717</td>
</tr>
<tr>
<td></td>
<td>(0.051)</td>
<td>-</td>
<td>(0.119)</td>
</tr>
<tr>
<td></td>
<td>( p_b ) (1)</td>
<td>( p_w ) (2)</td>
<td>( p_b - p_w ) (3)</td>
</tr>
<tr>
<td>------------------------</td>
<td>---------------</td>
<td>---------------</td>
<td>---------------------</td>
</tr>
<tr>
<td><strong>Mean</strong></td>
<td>0.153</td>
<td>0.177</td>
<td>-0.023</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.007)</td>
<td>(0.005)</td>
</tr>
<tr>
<td><strong>Standard deviation</strong></td>
<td>0.290</td>
<td>0.308</td>
<td>0.102</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.007)</td>
<td>(0.012)</td>
</tr>
<tr>
<td><strong>Correlation with ( p_w ) or ( p_f )</strong></td>
<td>0.944</td>
<td>1.00</td>
<td>-0.336</td>
</tr>
<tr>
<td></td>
<td>(0.017)</td>
<td>-</td>
<td>(0.066)</td>
</tr>
<tr>
<td><strong>Skewness</strong></td>
<td>3.76</td>
<td>3.65</td>
<td>-4.45</td>
</tr>
<tr>
<td></td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.82)</td>
</tr>
</tbody>
</table>
Calculating discrimination probabilities requires more than a few moments of $G(\cdot)$

But we can use the moments we have to calculate bounds

Let $\mu(G)$ denote list of moments for distribution $G(\cdot)$, and $f$ the list of observed callback probabilities

Lower bound on the share of jobs that discriminate:

$$\bar{\pi} \geq \min_G \int_{p_w \neq p_b} dG(p_w, p_b) \text{ s.t. } f = B\mu(G)$$

Discretize $G \implies$ this is a tractable linear programming problem

We can use this approach to bound the overall share of discriminators, and the share of jobs with particular callback configurations that discriminate
Figure I: Lower bounds on posterior probabilities of discrimination, BM data
Kline and Walters (forthcoming): Decisions

- Results so far suggest it is possible to obtain informative posteriors for some individual jobs.
- Can we use correspondence evidence to make decisions about which employers to investigate?
- Let $\delta(C_{jw}, C_{jb}) \in \{0, 1\}$ indicate decision to investigate as a function of callbacks.
- Consider a simple loss function:

$$L_j(\delta(C_{jw}, C_{jb})) = \gamma_1 \delta(C_{jw}, C_{jb})1\{p_{jw} = p_{jb}\} + \gamma_2(1 - \delta(C_{jw}, C_{jb}))1\{p_{jw} \neq p_{jb}\}$$

- $\gamma_1$ and $\gamma_2$ reflect costs of type I and type II errors.
- Optimal decision rule minimizes risk (expected loss):

$$\delta^*(C_{jw}, C_{jb}) = \arg \min_{\delta(\cdot)} E \left[ L_j(\delta(C_{jw}, C_{jb})) \right]$$
Optimal decision is to investigate when posterior exceeds a cost-based threshold:

$$\delta^*(C_{jw}, C_{jb}) = 1 \left\{ \Pr[p_{wj} \neq p_{bj} | C_{jw}, C_{jb}] > \frac{\gamma_1}{\gamma_1 + \gamma_2} \right\}.$$ 

Unlike frequentist hypothesis testing, posterior threshold rule controls the false discovery rate, $FDR \equiv \Pr[p_{wj} = p_{bj} | \delta(C_{jw}, C_{jb}) = 1]$.

Close link to literature on multiple testing (Benjamini and Hochberg, 1995; Storey, 2002; Efron, 2012).

With knowledge of $G(\cdot)$, can trace out tradeoff between type I and II errors (detection/error tradeoff curve).
Kline and Walters (forthcoming): Parametric Model

- Study detection/error tradeoffs with a parametric model for $G(\cdot)$

- Mixed logit model for callback to application $i$ at job $j$:

$$\Pr(Y_{ij} = 1|\alpha_j, \beta_j, R_{ij}, X_{ij}) = \frac{\exp(\alpha_j - \beta_j 1\{R_{ij} = b\} + X'_{ij}\psi)}{1 + \exp(\alpha_j - \beta_j 1\{R_{ij} = b\} + X'_{ij}\psi)}.$$ 

- $R_{ij}$ indicates race, $X_{j\ell}$ includes other randomly-assigned characteristics (GPA, experience, etc.)

- Two-type mixing:

$$\alpha_j \sim N(\alpha_0, \sigma^2_\alpha),$$

$$\beta_j = \begin{cases} 
\beta_0, & \text{with prob. } \frac{\exp(\tau_0 + \tau_\alpha \alpha_j)}{1 + \exp(\tau_0 + \tau_\alpha \alpha_j)} \\
0, & \text{with prob. } \frac{1}{1 + \exp(\tau_0 + \tau_\alpha \alpha_j)}. 
\end{cases}$$

- Kline and Walters (forthcoming) also consider decisions with continuous loss and partial identification of $G(\cdot)$ (minimax analysis)
Table V: Mixed logit parameter estimates, NPRS data

<table>
<thead>
<tr>
<th>Types</th>
<th>Constant (1)</th>
<th>No selection (2)</th>
<th>Selection (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>𝛼₀</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Distribution of logit($p_w$):</td>
<td>-4.71</td>
<td>-4.93</td>
<td>-4.93</td>
</tr>
<tr>
<td></td>
<td>(0.22)</td>
<td>(0.24)</td>
<td>(0.28)</td>
</tr>
<tr>
<td></td>
<td>𝜎_𝛼</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>4.74</td>
<td>4.99</td>
<td>4.98</td>
</tr>
<tr>
<td></td>
<td>(0.22)</td>
<td>(0.25)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>Discrimination intensity:</td>
<td>β₀</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.456</td>
<td>4.05</td>
<td>4.05</td>
</tr>
<tr>
<td></td>
<td>(0.108)</td>
<td>(1.56)</td>
<td>(1.58)</td>
</tr>
<tr>
<td>Discrimination logit:</td>
<td>τ₀</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>-1.59</td>
<td>-1.56</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.42)</td>
<td>(1.10)</td>
</tr>
<tr>
<td></td>
<td>τ_α</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>-</td>
<td>-</td>
<td>-0.005</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.180)</td>
</tr>
<tr>
<td>Fraction with $p_w \neq p_b$ :</td>
<td>1.00</td>
<td>0.168</td>
<td>0.170</td>
</tr>
<tr>
<td>Log-likelihood</td>
<td>-2,792.1</td>
<td>-2,788.2</td>
<td>-2,788.2</td>
</tr>
<tr>
<td>Parameters</td>
<td>15</td>
<td>16</td>
<td>17</td>
</tr>
<tr>
<td>Sample size</td>
<td>2,305</td>
<td>2,305</td>
<td>2,305</td>
</tr>
</tbody>
</table>
Figure V: Detection/error tradeoffs, NPRS data

- Share of non-discriminators not investigated
- Share of discriminators investigated

Graph: Line plot showing the relationship between the share of non-discriminators not investigated and the share of discriminators investigated. The graph includes data points for 2 pairs.
Figure V: Detection/error tradeoffs, NPRS data
Figure V: Detection/error tradeoffs, NPRS data

- Share of non-discriminators not investigated
- Share of discriminators investigated

- 2 pairs
- 5 pairs
- 5 pairs (HQ black, LQ white)
Figure V: Detection/error tradeoffs, NPRS data

- Share of non-discriminators not investigated
- Share of discriminators investigated

- 2 pairs
- 5 pairs
- 5 pairs (HQ black, LQ white)
Disparate Impacts of Probation: Rose (forthcoming)

- Large racial disparities in criminal justice outcomes motivate studies of discrimination in the criminal justice system.
- Concerns that formally neutral policies may have disproportionate effects on protected groups.
  - Disparate treatment vs. disparate impact.
  - Algorithmic fairness and bias (Kleinberg et al., 2017; Yang and Dobbie, 2020).
- Rose (forthcoming QJE) studies the impacts of probation rules on racial gaps in incarceration.
Most convicted offenders in the US serve sentences on probation ("community supervision") rather than in prison.

While on probation, violations of technical rules (e.g., failure to pay fees or fines) may result in incarceration.

Probation revocations account for 25% of prison admissions, and are more common among black probationers.

Technical rules are meant to serve two purposes:
- Support reintegration/rehabilitation of offenders
- Identify those who are likely to offend again

Key question: Do probation violations accurately target high-risk offenders?
Rose (forthcoming): Probation reform

- Rose (forthcoming) studies a 2011 North Carolina reform that reduced punishments for technical probation violations.

- Prior to reform, revocations for nonpayment of fees/fines or failing drug/alcohol tests were common.

- Afterwards, revocations were reserved for new crimes or absconding (fleeing supervision).

- Using administrative data from NC, Rose (forthcoming) employs a difference-in-differences design to study the impacts of this reform.
  
  - Treatment group: Offenders on supervised probation in NC.
  
  - Control group: unsupervised probationers (less serious offenses/not monitored for violations).
Difference-in-Differences

▶ Consider individuals in two groups \( g \in \{A, B\} \) observed in two time periods \( t \in \{pre, post\} \)

▶ Treatment switches on for group A in the post period:
\[
D_{igt} = 1\{g = A, t = post\}
\]

▶ Let \( Y_{igt}(d) \) denote potential outcome for individual \( i \) in group \( g \) in period \( t \) with treatment status \( d \in \{0, 1\} \)

▶ We observe \( E[Y_{i,A,post}(1)] \), and \( E[Y_{igt}(0)] \) for \( (g, t) \in \{(A, 0), (B, 0), (B, 1)\} \)

▶ Objective: calculate the treatment effect for group A in the post period, \( E[Y_{i,A,post}(1) - Y_{i,A,post}(0)] \)

▶ Requires imputing the unobserved counterfactual for the treated group in the post period, \( E[Y_{i,A,post}(0)] \)
Difference-in-Differences

- The core of a diff-in-diff design is an additive model for non-treated potential outcomes:

\[ E[Y_{igt}(0)] = \alpha_g + \gamma_t \]

- \( \alpha_g \) is a time-invariant group effect
- \( \gamma_t \) is a group-invariant time effect
- Groups can be different, and time periods can be different. Key is no time-varying group-specific confounders
- Additive model implies parallel trends in non-treated potential outcomes across groups:

\[ E[Y_{i,A,post}(0)] - E[Y_{i,A,pre}(0)] = E[Y_{i,B,post}(0)] - E[Y_{i,B,pre}(0)] \]

\[ = \gamma_1 - \gamma_0. \]
Difference-in-Differences

- With an additive model, the observed change in outcomes for group B captures the counterfactual change in outcomes for group A, so:

\[ E[Y_{i,A,post}(0)] = E[Y_{i,A,pre}(0)] + (E[Y_{i,B,post}(0)] - E[Y_{i,B,pre}(0)]) \]

- The treatment effect for group A in the post period is then:

\[ E[Y_{i,A,post}(1) - Y_{i,A,post}(0)] = (E[Y_{i,A,post}(1)] - E[Y_{i,A,pre}(0)]) - (E[Y_{i,B,post}(0)] - E[Y_{i,B,pre}(0)]) \]

- Diff-in-diff looks at the change in outcomes for the treated group, subtracting off the change for the control group to eliminate time effects.

- Implement with linear regression:

\[ Y_{igt} = \alpha_g + \gamma_t + \beta D_{gt} + \epsilon_{igt} \]

- Coefficient \( \beta \) captures difference in changes over time for treatment vs. control.
Graph showing pre and post data for treatment and control groups.
A. Technical revocation

This figure plots effects of the 2011 JRA reform on technical revocation and arrests. Panels A and B include all supervised probationers starting their spells within four years of the reform. Each line represents a three-month cohort of probationers who start their spells where the line intersects the x-axis. The y-axis measures the share of this cohort experiencing the relevant outcome over the following year. That is, each line is the failure function for that cohort and outcome. Technical revocation is an indicator for having probation revoked with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before revocation for any rule violations. Events are therefore mutually exclusive. Panel C plots mean one-year technical revocation and arrest rates for supervised probationers minus the same measure for unsupervised probationers. The same cohort definitions are used. Effects are normalized relative to the cohort starting four quarters before the reform, indicated by the solid red line. This is the last cohort to spend the full first year of their probation spells under the pre-reform regime. The dotted red line indicates the first cohort whose first year of probation falls completely post-reform. Dashed lines indicate 95% confidence intervals formed from standard errors clustered at the individual level.
B. Arrests

![Graph showing the effects of the 2011 JRA reform on technical revocation and arrests.](chart)

- **A. Technical revocation**
  - Graph showing the probability of technical revocation over time.
  - The y-axis measures the share of the cohort experiencing the relevant outcome over the following year.

- **B. Arrests**
  - Graph showing the probability of arrest over time.
  - The y-axis measures the share of the cohort experiencing the relevant outcome over the following year.

**Notes.**
This figure plots effects of the 2011 JRA reform on technical revocation and arrests. Panels A and B include all supervised probationers starting their spells within four years of the reform. Each line represents a three-month cohort of probationers who start their spells where the line intersects the x-axis. The y-axis measures the share of this cohort experiencing the relevant outcome over the following year. That is, each line is the failure function for that cohort and outcome. Technical revocation is an indicator for having probation revoked with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before revocation for any rule violations. Events are therefore mutually exclusive. Panel C plots mean one-year technical revocation and arrest rates for supervised probationers minus the same measure for unsupervised probationers. The same cohort definitions are used. Effects are normalized relative to the cohort starting four quarters before the reform, indicated by the solid red line. This is the last cohort to spend the full first year of their probation spells under the pre-reform regime. The dotted red line indicates the first cohort whose first year of probation falls completely post-reform. Dashed lines indicate 95% confidence intervals formed from standard errors clustered at the individual level.
**Figure III**

**Effects of Reform by Race**

**A. Technical revocation**

**B. Arrests**

---

**Notes.** This figure plots effects of the 2011 JRA reform on technical revocation and arrests separately by race. It includes all supervised probationers starting their spells either 1-3 years before (pre) or 0-2 years after the reform (post). “B” refers to black probationers, while “W” refers to non-black. The y-axis measures the share of each group experiencing the relevant outcome over the first year of their probation spell. Technical revocation is an indicator for having probation revoked for rule violations with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before revocation for any rule violations. Shaded areas reflect 95% confidence intervals formed using standard errors clustered at the individual level.
### TABLE III
**Difference-in-Differences Estimates of Reform Impacts**

#### A. All offenders

<table>
<thead>
<tr>
<th></th>
<th>Technical revoke</th>
<th>Arrest</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
</tr>
<tr>
<td>Post-reform</td>
<td>-0.00172***</td>
<td>-0.00205***</td>
</tr>
<tr>
<td></td>
<td>(0.000273)</td>
<td>(0.000288)</td>
</tr>
<tr>
<td>Treated</td>
<td>0.143***</td>
<td>0.133***</td>
</tr>
<tr>
<td></td>
<td>(0.00103)</td>
<td>(0.00102)</td>
</tr>
<tr>
<td>Post-x-treat</td>
<td>-0.0532***</td>
<td>-0.0530***</td>
</tr>
<tr>
<td></td>
<td>(0.00135)</td>
<td>(0.00135)</td>
</tr>
</tbody>
</table>

| N              | 546006           | 546006       | 546006       | 546006       |

**Notes.** This table includes all treated and untreated probation spells beginning 1-3 years before the reform and 0-2 years afterwards. Post is indicator for starting probation after Dec. 1, 2011, the date JRA reforms took effect. Technical revocation is an indicator for having probation revoked with no intervening criminal arrest. Arrest is an indicator for a criminal arrest before revocation for any rule violations. Demographic controls include five-year age bins and indicators for race and gender. Criminal history controls include fixed effects for criminal history points and prior sentences to supervised probation or incarceration. Controls are included in columns 2 and 4. Standard errors in parentheses are clustered at the individual level. 

* p< 0.05, ** p< 0.01, *** p< 0.001.
### B. Non-black offenders

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Post-reform</strong></td>
<td>-0.000522</td>
<td>-0.000875**</td>
<td>-0.00693***</td>
<td>-0.00666***</td>
</tr>
<tr>
<td></td>
<td>(0.000317)</td>
<td>(0.000334)</td>
<td>(0.00199)</td>
<td>(0.00190)</td>
</tr>
<tr>
<td><strong>Treated</strong></td>
<td>0.122***</td>
<td>0.112***</td>
<td>0.0450***</td>
<td>-0.000334</td>
</tr>
<tr>
<td></td>
<td>(0.00130)</td>
<td>(0.00126)</td>
<td>(0.00209)</td>
<td>(0.00207)</td>
</tr>
<tr>
<td><strong>Post-x-treat</strong></td>
<td>-0.0356***</td>
<td>-0.0360***</td>
<td>0.0198***</td>
<td>0.0179***</td>
</tr>
<tr>
<td></td>
<td>(0.00173)</td>
<td>(0.00172)</td>
<td>(0.00304)</td>
<td>(0.00295)</td>
</tr>
<tr>
<td><strong>N</strong></td>
<td>328784</td>
<td>328784</td>
<td>328784</td>
<td>328784</td>
</tr>
</tbody>
</table>
### C. Black offenders

<table>
<thead>
<tr>
<th></th>
<th>Column 1</th>
<th>Column 2</th>
<th>Column 3</th>
<th>Column 4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Post-reform</td>
<td>-0.00387***</td>
<td>-0.00412***</td>
<td>-0.0118***</td>
<td>-0.0112***</td>
</tr>
<tr>
<td></td>
<td>(0.000509)</td>
<td>(0.000534)</td>
<td>(0.00295)</td>
<td>(0.00281)</td>
</tr>
<tr>
<td>Treated</td>
<td>0.167***</td>
<td>0.160***</td>
<td>-0.00496</td>
<td>-0.0464***</td>
</tr>
<tr>
<td></td>
<td>(0.00167)</td>
<td>(0.00167)</td>
<td>(0.00274)</td>
<td>(0.00268)</td>
</tr>
<tr>
<td>Post-x-treat</td>
<td>-0.0741***</td>
<td>-0.0736***</td>
<td>0.0228***</td>
<td>0.0233***</td>
</tr>
<tr>
<td></td>
<td>(0.00215)</td>
<td>(0.00214)</td>
<td>(0.00399)</td>
<td>(0.00383)</td>
</tr>
<tr>
<td>N</td>
<td>217222</td>
<td>217222</td>
<td>217222</td>
<td>217222</td>
</tr>
</tbody>
</table>
Rose (forthcoming): Interpretation

- Rose finds that eliminating technical violations led to large reductions in probation revocation and modest increases in re-offending.

- Effects on revocation are much larger for black probationers than white probationers, while effects on re-offending are comparable.

- Reform eliminated large racial gap in revocations with no impact on racial gap in re-offending.

- This suggests technical revocations target re-offenders more accurately for white probationers.
To formally analyze targeting accuracy, ignore time dimension and consider LATE setup:

- \( Z_i \): indicator equal to one if \( i \) is subject to technical rules
- \( R_i(1), R_i(0) \): \( i \)'s potential revocations as a function of \( Z_i \)
- \( Y_i(1), Y_i(0) \): \( i \)'s potential re-offending as a function of \( R_i \)

Probability of a technical revocation among those who would not otherwise be revoked:

\[
\Pr(R_i(1) = 1|R_i(0) = 0)
\]

This is the share of compliers (\( R_i(1) > R_i(0) \)) among the population of compliers and never takers (\( R_i(0) = 0 \))
By the law of total probability, we can write

\[ \Pr(R_i(1) = 1|R_i(0) = 0) = \mu_0\pi_0 + \mu_1\pi_1 \]

\(\mu_k = \Pr(Y_i(0) = k|R_i(0) = 0)\) describes the distribution of re-offending risk among compliers and never-takers.

\(\pi_0 = \Pr(R_i(1) = 1|R_i(0) = 0, Y_i(0) = 0): \) False positive rate (Type I error)

\(\pi_1 = \Pr(R_i(1) = 1|R_i(0) = 0, Y_i(0) = 1): \) True positive rate (one minus Type II error)

Note that under the LATE model assumptions, all of these terms are identified.
Rose (forthcoming): Targeting

- Oaxaca decomposition of racial difference in revocation rates:

\[
\Pr(R_i(1) = 1 \mid R_i(0) = 0, B_i = 1) - \Pr(R_i(1) = 1 \mid R_i(0) = 0, W_i = 1)
\]

\[
= \sum_{k=0}^{1} \mu_k, W (\pi_{k,B} - \pi_{k,W}) + \sum_{k=0}^{1} (\mu_{k,B} - \mu_{k,W}) \pi_{k,B}
\]

- First term is due to differences in targeting accuracy (true/false positive rates)

- Second term is due to differences in re-offending risk
## TABLE IV
### Decomposition of Racial Gaps in Revocations

<table>
<thead>
<tr>
<th>Overall rates</th>
<th>Decomposition</th>
<th>White</th>
<th>Black</th>
<th>Gap</th>
<th>Share of gap explained</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Probability of technical revoke:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$Pr(R(1) = 1)$</td>
<td></td>
<td>0.039</td>
<td>0.082</td>
<td>0.043</td>
<td>100.0%</td>
</tr>
<tr>
<td><strong>Distribution of risk:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$Pr(Y(0) = 1)$</td>
<td></td>
<td>0.313</td>
<td>0.376</td>
<td>0.063</td>
<td>9.8%</td>
</tr>
<tr>
<td>$Pr(Y(0) = 0)$</td>
<td></td>
<td>0.687</td>
<td>0.624</td>
<td>-0.063</td>
<td>-13.3%</td>
</tr>
<tr>
<td><strong>True / false positive rates:</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$Pr(R(1)=1</td>
<td>Y(0) = 1)$</td>
<td></td>
<td>0.070</td>
<td>0.068</td>
<td>-0.002</td>
</tr>
<tr>
<td>$Pr(R(1)=1</td>
<td>Y(0) = 0)$</td>
<td></td>
<td>0.025</td>
<td>0.091</td>
<td>0.066</td>
</tr>
</tbody>
</table>

Notes: This table decomposes the difference in technical revocation between black and white probationers into the contributions of differences in reoffending risk and differences in the likelihood of revocation conditional on arrest risk. The decomposition applies to the population with $R_i(1) = 0$ (⇠ 90% of the population). These are individuals who are not revoked for breaking rules even after the reform. Estimates are based on core differences-in-differences results without controls from Table III. The decomposition calculates the contribution of differences in risk using black targeting rates as baseline, and differences in targeting using white risk as baseline. The first row is -1 times the race-specific post-x-treat effect for technical violations. The second row is the sum of the constant, treat, and post-x-treat effects from differences-in-differences estimates for arrests. Both rows are re-scaled by 1 minus the sum of the constant, treat, and post-x-treat effects for technical violations, since this measures $Pr(R_i(1) = 0)$. The final two rows are calculated as described in the text. Appendix Section A4 provides complete details on how the decomposition is calculated.
Figure V
Efficiency and Equity of Technical Violation Rule Types

Notes. This figure plots estimates of the share of potential reoffenders over a three year period who break technical rules before they reoffend (x-axis) against the share of non-reoffenders who do not break technical rules. Estimates come from simulating the model estimated in Section VD using a different set of rules. Each point is labeled with a combination of “F” for fees / fines violations, “D” for drug / alcohol violations, “R” for reporting violations, and “O” for all other, reflecting the sets of rules enforced in the simulation. The points labeled “FDRO” therefore reflect the set of rules punishable with incarceration before the 2011 reform, and “R” reflect the set punishable afterwards. The dotted gray line starts at (1, 0) and has a slope of -1. This line reflects what would be achieved by randomly revoking a fraction of probationers at the start of their spells, which naturally would catch equal shares of reoffenders and non-reoffenders.
References


References


AEA Continuing Education 2021: Labor Economics and Applied Econometrics

Lecture #1: The Minimum Wage

Patrick Kline

UC Berkeley
Minimum wages: Background

Long presumption that minimum wages reduce employment (e.g., Stigler, 1946)

Evidence from time series / state panel regressions on aggregates typically find small but significant disemployment effects (Brown, 1999)

Some limitations of older literature

1. Exogeneity of min wage changes (who is the control group?)
2. Aggregates mask distributional impacts
3. Selective reporting bias

Important advances in methods and data in recent years have changed views on costs/benefits of min wage
Question A:

If the federal minimum wage is raised gradually to $15-per-hour by 2020, the employment rate for low-wage US workers will be substantially lower than it would be under the status quo.

Responses

Responses weighted by each expert's confidence
Card and Krueger (1995): Do all t-stats = 2?

**Figure 1. Relation of Estimated t Ratios to Sample Size**


**Figure 2. Relation of Estimated Employment Elasticity to Standard Error**

Figure 9. Wolfson and Belman (2015) Data

Notes: The left panel shows a binned density plot for the $z$-statistics $X/\Sigma$ in the Wolfson and Belman (2015) data. The solid gray lines mark $|X|/\Sigma = 1.96$, while the dash-dotted gray line marks $X/\Sigma = 0$. The right panel plots the estimate $X$ against its standard error $\Sigma$. The gray lines mark $|X|/\Sigma = 1.96$. 
Selection correct the t-stats

A selection model for reporting results

\[ \Pr(\text{report} \mid Z) \propto \begin{cases} 
\beta_{p,1} & \text{if } Z < -1.96 \\
\beta_{p,2} & \text{if } -1.96 \leq Z < 0 \\
\beta_{p,3} & \text{if } 0 < Z \leq 1.96 \\
1 & \text{if } Z < 1.96 
\end{cases} \]

Selection bias when \( \beta_p \)'s <1.

Latent population model of \( Z \) stats

\[ \Theta \sim \bar{\theta} + t(\nu) \cdot \tau \]

where \( \bar{\theta} \) is unselected mean, \( \tau \) is scale parameter and \( \nu \) is degrees of freedom for Student’s t-distribution (low \( \nu \) means fat tails)

Estimate by maximum likelihood treating studies as independent
Maximum likelihood estimates

<table>
<thead>
<tr>
<th>Table 3—Selection Estimates for Wolfson and Belman (2015)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\bar{\theta}$</td>
</tr>
<tr>
<td>-----------------</td>
</tr>
<tr>
<td>0.018</td>
</tr>
<tr>
<td>(0.009)</td>
</tr>
</tbody>
</table>

*Notes:* Meta-study estimates from minimum wage data, with standard errors clustered by study in parentheses. Publication probabilities $\beta_p$ measured relative to omitted category of estimates positive and significant at 5 percent level.

- Severe selection: $\sim 30\%$ chance of reporting an insignificant result!
- Mean employment-MW elasticity borderline significant (the irony!)
  - Incredibly fat tails ($\nu < 2 \Rightarrow$ variance doesn’t exist!)
  - No accounting for study quality
Card and Krueger (1994): a trip down memory lane

Evaluate effects of April 1992 increase in NJ min wage from $4.25 to $5.05

Surveyed 410 fast-food restaurants in NJ and PA before and after change

Two designs:
1. Diff in diff: compare NJ to PA
2. Exposure (gap) design: compare initially low wage to high wage establishments

Key findings:
- No (dis-)employment effect (possibly positive)
- Some evidence of cost pass-through to consumers
First stage looks good!
Two designs

- **Diff in Diff**
  \[ \Delta E_i = a + X_i'b + cNJ_i + \varepsilon_i \]
  where \( X_i \) is baseline store characteristics

- **Exposure design**
  \[ \Delta E_i = \tilde{a} + \tilde{X}_i'\tilde{b} + \tilde{c}GAP_i + \tilde{\varepsilon}_i \]
  where \( \tilde{X}_i \) may include \( NJ_i \) and

\[
GAP_i = NJ_i \cdot \max \left\{ \frac{5.05 - W_{1i}}{W_{1i}}, 0 \right\}
\]
### Table 4—Reduced-Form Models for Change in Employment

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>Model (i)</th>
<th>Model (ii)</th>
<th>Model (iii)</th>
<th>Model (iv)</th>
<th>Model (v)</th>
</tr>
</thead>
<tbody>
<tr>
<td>New Jersey dummy</td>
<td>2.33</td>
<td>2.30</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>(1.19)</td>
<td>(1.20)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Initial wage gap&lt;sup&gt;a&lt;/sup&gt;</td>
<td>—</td>
<td>—</td>
<td>15.65</td>
<td>14.92</td>
<td>11.91</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(6.08)</td>
<td>(6.21)</td>
<td>(7.39)</td>
</tr>
<tr>
<td>Controls for chain and ownership&lt;sup&gt;b&lt;/sup&gt;</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Controls for region&lt;sup&gt;c&lt;/sup&gt;</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Standard error of regression</td>
<td>8.79</td>
<td>8.78</td>
<td>8.76</td>
<td>8.76</td>
<td>8.75</td>
</tr>
<tr>
<td>Probability value for controls&lt;sup&gt;d&lt;/sup&gt;</td>
<td>—</td>
<td>0.34</td>
<td>—</td>
<td>0.44</td>
<td>0.40</td>
</tr>
</tbody>
</table>

**Notes:** Standard errors are given in parentheses. The sample consists of 357 stores with available data on employment and starting wages in waves 1 and 2. The dependent variable in all models is change in FTE employment. The mean and standard deviation of the dependent variable are −0.237 and 8.825, respectively. All models include an unrestricted constant (not reported).

<sup>a</sup>Proportional increase in starting wage necessary to raise starting wage to new minimum rate. For stores in Pennsylvania the wage gap is 0.

<sup>b</sup>Three dummy variables for chain type and whether or not the store is company-owned are included.

<sup>c</sup>Dummy variables for two regions of New Jersey and two regions of eastern Pennsylvania are included.

<sup>d</sup>Probability value of joint $F$ test for exclusion of all control variables.
Do consumers pay more?

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>(i)</th>
<th>(ii)</th>
<th>(iii)</th>
<th>(iv)</th>
<th>(v)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. New Jersey dummy</td>
<td>0.033</td>
<td>0.037</td>
<td>—</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.014)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. Initial wage gap(^a)</td>
<td>—</td>
<td>—</td>
<td>0.077</td>
<td>0.146</td>
<td>0.063</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.075)</td>
<td>(0.074)</td>
<td>(0.089)</td>
</tr>
<tr>
<td>3. Controls for chain and(^b)</td>
<td>no</td>
<td>yes</td>
<td>no</td>
<td>yes</td>
<td>yes</td>
</tr>
<tr>
<td>ownership</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. Controls for region(^c)</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>no</td>
<td>yes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>5. Standard error of regression</td>
<td>0.101</td>
<td>0.097</td>
<td>0.102</td>
<td>0.098</td>
<td>0.097</td>
</tr>
</tbody>
</table>

Notes: Standard errors are given in parentheses. Entries are estimated regression coefficients for models fit to the change in the log price of a full meal (entrée, medium soda, small fries). The sample contains 315 stores with valid data on prices, wages, and employment for waves 1 and 2. The mean and standard deviation of the dependent variable are 0.0173 and 0.1017, respectively.

\(^a\)Proportional increase in starting wage necessary to raise the wage to the new minimum-wage rate. For stores in Pennsylvania the wage gap is 0.

\(^b\)Three dummy variables for chain type and whether or not the store is company-owned are included.

\(^c\)Dummy variables for two regions of New Jersey and two regions of eastern Pennsylvania are included.
Table 8—Estimated Effect of Minimum Wages on Numbers of McDonald’s Restaurants, 1986–1991

<table>
<thead>
<tr>
<th>Independent variable</th>
<th>Dependents variable: proportional increase in number of stores</th>
<th>Dependents variable: (number of newly opened stores) ÷ (number in 1986)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(i)</td>
<td>(ii)</td>
</tr>
<tr>
<td><strong>Minimum-Wage Variable:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Fraction of retail workers in affected wage range 1986&lt;sup&gt;a&lt;/sup&gt;</td>
<td>0.33</td>
<td>—</td>
</tr>
<tr>
<td>2. (State minimum wage in 1991)&lt;sup&gt;b&lt;/sup&gt; ÷ (average retail wage in 1986)&lt;sup&gt;b&lt;/sup&gt;</td>
<td>—</td>
<td>0.38</td>
</tr>
<tr>
<td></td>
<td>(0.20)</td>
<td>(0.19)</td>
</tr>
<tr>
<td><strong>Other Control Variables:</strong></td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. Proportional growth in population, 1986–1991</td>
<td>—</td>
<td>—</td>
</tr>
<tr>
<td>5. Standard error of regression</td>
<td>0.083</td>
<td>0.083</td>
</tr>
</tbody>
</table>

Notes: Standard errors are shown in parentheses. The sample contains 51 state-level observations (including the District of Columbia) on the number of McDonald’s restaurants open in 1986 and 1991. The dependent variable in columns (i)–(iv) is the proportional increase in the number of restaurants open. The mean and standard deviation are 0.246 and 0.085, respectively. The dependent variable in columns (v)–(viii) is the ratio of the number of new stores opened between 1986 and 1991 to the number open in 1986. The mean and standard deviation are 0.293 and 0.091, respectively. All regressions are weighted by the state population in 1986.

<sup>a</sup>Fraction of all workers in retail trade in the state in 1986 earning an hourly wage between $3.35 per hour and the “effective” state minimum wage in 1990 (i.e., the maximum of the federal minimum wage in 1990 ($3.80) and the state minimum wage as of April 1, 1990).

<sup>b</sup>Maximum of state and federal minimum wage as of April 1, 1990, divided by the average hourly wage of workers in retail trade in the state in 1986.
The power of zero

A carefully thought out and transparent attempt to evaluate a minimum wage change with microdata

- Results a bit under-powered to detect clear positive but precise enough to reject big negative

- Inferential issue: no clustering
  - Debatable if we want to cluster (Abadie et al., 2020)
    - Are we conducting inference on the effect in this state or some hypothetical new state drawn from a super-population?
    - Does every DiD paper need to be a meta-analysis?
  - Either way dependence less of an issue for GAP design
Outrage ensues

Businessweek: “A Minimum Wage Study with Minimum Credibility”

*Political correctness seems to have crept into the inner sanctum of the AEA, discrediting its scholarly journal and debasing its top prize. Unless the association cleans up its act, it can kiss its credibility goodbye*

James Buchanan in the WSJ

*Just as no physicist would claim that ‘water runs uphill,’ no self-respecting economist would claim that increases in the minimum wage increase employment. Such a claim, if seriously advanced, becomes equivalent to a denial that there is even minimal scientific content in economics, and that, in consequence, economists can do nothing but write as advocates for ideological interests.*

Merton Miller in the WSJ

*Raising the minimum wage by law above its market determined equilibrium, they argue, actually costs nobody anything. (Or at worst, costs nobody very much because it’s only a small, marginal increment, after all.) Is all this too good to be true? Damn right. But it sure plays well in the opinion polls. I tremble for my profession.*
Aftermath

What to make of these results?

▶ Card-Krueger argue that positive employment effects reflect monopsony power

▶ Brown (1999) argues that monopsony would imply output expands so prices should fall. Concludes that:

“Based on the available evidence, the monopsony model will not replace the competitive diagram in the souls of labor economists.”

Unresolved: do GAP design and diff in diff identify the same parameter?

▶ Diff in diff measures market-wide response

▶ GAP measures effect of raising wage on a single firm holding market constant
One funeral at a time?

A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it. – Max Planck (1948)
Study the effect of 1996 fed min wage hike on a large multi-establishment retailer

Leverage high frequency data to assess validity of GAP design

Contrast overall employment effect with relative employment effect (teenage vs adult labor)

Main finding: insignificant aggregate disemployment effect but small *increase* in relative employment of teenage workers
Two Gaps

Gap of employee $i$ at store $j$ is:

$$Gap_{ij} = \max \{0, (MW_j - w_{ij})/w_{ij}\}$$

Store $j$’s average gap is:

$$Gap_j = \frac{1}{N_j} \sum_i Gap_{ij}$$

Store $j$’s relative gap is:

$$\frac{Gap_j^{teen} - Gap_j^{adult}}{1 + Gap_j^{adult}}$$

Assess validity of design via monthly cross sectional regression of outcomes on each gap + controls. Plot coefficients on gap.
Modest effect of avg gap on wage, nothing on employment

---

**A. Average Wage**

- Feb96, Aug96, Feb97, Aug97, Feb98

**B. Effect of Avg. Wage Gap on Ln(Avg. Wage)**

- Feb96, Aug96, Feb97, Aug97, Feb98

**C. Full-Time Equivalent Employment**

- Feb96, Aug96, Feb97, Aug97, Feb98

**D. Effect of Avg. Wage Gap on FTE Employment**

- Feb96, Aug96, Feb97, Aug97, Feb98

---

**Fig. 1.**—Vertical lines indicate dates of federal minimum wage increases (October 1, 1996, and September 1, 1997). Panels A and C plot group means for high- (low-) impact stores, defined as those with average wage gaps above (below) the sample median. Panels B and D plot coefficient estimates from monthly regressions of log average wage ($B$) or full-time equivalent employment ($D$) on the store average wage gap (see eq. 2). Regression models include all fixed store-level controls as in table 4.
Substantial increase in relative wages and employment of teenagers
Quality upgrading?

Fig. 4.—Effect of relative wage gap on fraction of teenagers from high-income ZIP codes. Vertical lines indicate dates of federal minimum wage increases (October 1, 1996, and September 1, 1997). Figure plots coefficient estimates from monthly regressions of the fraction of teenage employees who live in high-income ZIP codes on the store relative wage gap (see eq. [2]). Regression model includes all fixed store-level controls as in table 4, col. 4. Coefficients are rescaled to measure differences associated with a .01 difference in the store relative wage gap.
Little evidence of an effect on productivity

Table 10
Reduced-Form Effects of Wage Gaps on Sales Growth and Shrinkage

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sales Growth</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Store adult wage gap</td>
<td>.566*</td>
<td>.639</td>
<td>.911**</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.296 )</td>
<td>(.390 )</td>
<td>(.349)</td>
<td></td>
</tr>
<tr>
<td>Store teenage wage gap</td>
<td>.194</td>
<td>-.072</td>
<td>-.312</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(.194 )</td>
<td>(.257 )</td>
<td>(.226)</td>
<td></td>
</tr>
<tr>
<td>Sample includes CA, DE, MA,</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>OR, and VT</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of stores</td>
<td>&gt;600</td>
<td>&gt;600</td>
<td>&gt;600</td>
<td>&gt;700</td>
</tr>
<tr>
<td></td>
<td>.33</td>
<td>.32</td>
<td>.33</td>
<td>.31</td>
</tr>
</tbody>
</table>

|                                | (1)   | (2)   | (3)   | (4)   |
| Change in Shrinkage            |       |       |       |       |
| Store adult wage gap           | -.009 | .005  | .004  |       |
|                                | (.016) | (.019) | (.017) |       |
| Store teenage wage gap         | -.012 | -.014 | -.014 |       |
|                                | (.012) | (.015) | (.012) |       |
| Sample includes CA, DE, MA,    | No    | No    | No    | Yes   |
| OR, and VT                     |       |       |       |       |
| Number of stores               | >600   | >600   | >600   | >700  |
|                                | .11    | .11    | .11    | .11   |

Note.—Sales growth is the change in the log of total sales between the first 6 months (February–July 1996) and the last 6 months (February–July 1998) of the sample period. Change in shrinkage is the change in the yearly shrinkage rate between the fiscal year ending in February 1996 and the fiscal year ending in February 1998. All regressions include store-level control variables as in table 4, col. 5, plus a control for the change in full-time equivalent employment. Robust standard errors in parentheses.

Note: shrinkage = inventory loss due to shoplifting / theft, etc
Summary

Gap / exposure design seems unconfounded

No discernable effect on overall employment

Relative employment of teens increased slightly
  ▶ Many possible explanations: compositional changes, changes in application behavior, monopsony
  ▶ Hard to distinguish between them

Limitations:
  ▶ Average gap was small
  ▶ Difficult to adjust for seasonal in retail employment
  ▶ Employment effects might grow over longer horizons.
US min wage variation tends to be small and short run in nature

Hungary experienced a large (60%) and persistent (∼8 years) increase in min wage in 2001

Use firm level exposure design to infer MW effects

Findings:
1. Small disemployment effects
2. Substantial cost pass-through to consumers
Huge, permanent, change..
Firm level exposure design

Estimating equations:

\[
\frac{y_{it} - y_{i2000}}{y_{i2000}} = \alpha_t + \beta_t FA_{it} + \gamma_t X_{it} + \varepsilon_{it}
\]

- \( y_{it} \) gives firm \( i \)'s outcome (employment, wages) in year \( t \)
- \( FA_{it} \) ("fraction affected") gives the fraction of firm \( i \)'s employees in 2000 whose wage was below year \( t \) minimum
- Weight by log firm revenue in 2000 (logs address extreme skew in revenue)
Firm exposure in 2002 raises wages but lowers employment.
Wage-employment elasticities are small, trivial dynamics

<table>
<thead>
<tr>
<th>Table 2—Employment and Wage Effects</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td><strong>Panel A. Change in firm-level employment</strong></td>
</tr>
<tr>
<td>Fraction affected</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>Employment elasticity with respect to MW (directly affected)</td>
</tr>
<tr>
<td><strong>Panel B. Change in firm-level average wage</strong></td>
</tr>
<tr>
<td>Fraction affected</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>Employment elasticity with respect to wage</td>
</tr>
<tr>
<td><strong>Panel C. Change in firm-level average cost of labor</strong></td>
</tr>
<tr>
<td>Fraction affected</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>Employment elasticity with respect to cost of labor</td>
</tr>
<tr>
<td>Controls</td>
</tr>
</tbody>
</table>

Notes: This table shows the firm-level relationship between the fraction of workers exposed to the minimum wage and the change in employment (panel A), the change in average wage (panel B), and the change in average cost of labor (panel C). The cost of labor includes wages, social security contributions, and non-wage labor expenses. The estimation approach is a two-stage least squares estimation (2SLS), subject to the following restrictions: (1) Employment elasticities are not defined for 2SLS estimation (2) Employment elasticities are not defined for 2SLS estimation (3) Employment elasticities are not defined for 2SLS estimation (4) Employment elasticities are not defined for 2SLS estimation (5) Employment elasticities are not defined for 2SLS estimation (6) Employment elasticities are not defined for 2SLS estimation
Figure A7. Employment Elasticity in the Literature and in this Paper
Bigger effects in tradeable sectors
Effect on prices

**Figure 4. Effect on Price Index and Revenue in the Manufacturing Sector**
Poor only slightly more likely to consume MW-intensive goods

Figure 5. The relationship between household income and the minimum wage content of consumption
### Table 5—Incidence of the Minimum Wage

<table>
<thead>
<tr>
<th>Description</th>
<th>Changes between 2000 and 2002</th>
<th>Changes between 2000 and 2004</th>
</tr>
</thead>
<tbody>
<tr>
<td>Change in total labor cost relative to revenue in 2000</td>
<td>0.038</td>
<td>0.021</td>
</tr>
<tr>
<td>Change in revenue relative to revenue in 2000 ($\Delta Revenue$)</td>
<td>0.066</td>
<td>0.036</td>
</tr>
<tr>
<td>Change in materials relative to revenue in 2000 ($\Delta Material$)</td>
<td>0.033</td>
<td>0.014</td>
</tr>
<tr>
<td>Change in miscitems relative to revenue in 2000 ($\Delta MiscItems$)</td>
<td>0.005</td>
<td>0.005</td>
</tr>
<tr>
<td>Incidence on consumers ($\Delta Revenue - \Delta Material - \Delta MiscItems$)</td>
<td>0.028</td>
<td>0.017</td>
</tr>
<tr>
<td>Change in profits relative to revenue in 2000 ($\Delta Profit$)</td>
<td>-0.011</td>
<td>-0.008</td>
</tr>
<tr>
<td>Change in depreciation relative to revenue in 2000 ($\Delta Depr$)</td>
<td>0.001</td>
<td>0.003</td>
</tr>
<tr>
<td>Incidence on firm owners ($-\Delta Profit - \Delta Depr$)</td>
<td>0.010</td>
<td>0.005</td>
</tr>
<tr>
<td>Fraction paid by consumers (percent)</td>
<td>74</td>
<td>77</td>
</tr>
<tr>
<td>Fraction paid by firm owners (percent)</td>
<td>26</td>
<td>23</td>
</tr>
</tbody>
</table>
A rationalizing framework

- Monopsonistic competition: each firm produces a different product variety $\omega$
- Three factors of production: labor, capital, materials. Derived labor demand is $l(\omega)$
- Model yields firm-level demand elasticities

\[
\frac{\partial \ln l(\omega)}{\partial \ln MW} = -s_L \eta - \frac{SK \sigma KL}{\text{scale effect}} - \frac{SM \sigma ML}{\text{ML substitution}}
\]

\[
\frac{\partial \ln p(\omega) q(\omega)}{\partial \ln MW} = \frac{s_L}{\text{price effect}} - \frac{s_L \eta}{\text{scale effect}}
\]

\[
\frac{\partial \ln k(\omega)}{\partial \ln MW} = s_L (-\eta + \sigma KL), \quad \frac{\partial \ln m(\omega)}{\partial \ln MW} = s_L (-\eta + \sigma ML)
\]

Note: $\eta$ is determined in equilibrium and depends on fraction of firms affected by min wage. It is smallest when all firms are affected by min wage.
Estimation

Calibrate shares \((s_L, s_K, s_M)\) from microdata leaving 3 unknown structural elasticities:

\[ \eta, \sigma_{KL}, \sigma_{ML} \]

- Proxy elasticity wrt MW with treatment effects
  - Recall that MW change is large, so implicitly assuming iso-elastic demand
  - 4 equations and 3 unknowns \(\implies\) over-determined system

- Fit via classical minimum distance (see Wooldridge, 2010)
  - Equivalent to stacking moments and treating as an SUR
SUR representation

Dataset of four moments, 3 regressors \((s_L, s_K, s_M)\), no intercept:

\[
\frac{\partial \ln l(\omega)}{\partial \ln MW} = -\eta \cdot s_L - \sigma_{KL} \cdot s_K - \sigma_{ML} \cdot s_M + \varepsilon_L
\]

\[
\frac{\partial \ln p(\omega)q(\omega)}{\partial \ln MW} = (1 - \eta) \cdot s_L - 0 \cdot s_K - 0 \cdot s_M + \varepsilon_R
\]

\[
\frac{\partial \ln k(\omega)}{\partial \ln MW} = (\sigma_{KL} - \eta) \cdot s_L - 0 \cdot s_K - 0 \cdot s_M + \varepsilon_K
\]

\[
\frac{\partial \ln m(\omega)}{\partial \ln MW} = (\sigma_{ML} - \eta) \cdot s_L - 0 \cdot s_K - 0 \cdot s_M + \varepsilon_M
\]

where \(\text{Cov}(\varepsilon_L, \varepsilon_R, \varepsilon_K, \varepsilon_M) = \Sigma\) is estimated from microdata

- Here SUR = multivariate weighted least squares
- Coefficient restrictions exploited in conjunction w/ \(\Sigma\) to improve precision
Materials key to getting neoclassical model to work.

<table>
<thead>
<tr>
<th>Panel A. Estimated parameters</th>
<th>All firms (1)</th>
<th>Manufacturing (2)</th>
<th>Tradable (3)</th>
<th>Non-tradable (4)</th>
<th>Export (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Output demand, $\eta$</td>
<td>0.11</td>
<td>0.98</td>
<td>1.34</td>
<td>-0.37</td>
<td>3.64</td>
</tr>
<tr>
<td></td>
<td>(0.22)</td>
<td>(0.46)</td>
<td>(0.41)</td>
<td>(0.50)</td>
<td>(0.98)</td>
</tr>
<tr>
<td>Capital-labor substitution, $\sigma_{KL}$</td>
<td>3.35</td>
<td>2.60</td>
<td>2.34</td>
<td>3.94</td>
<td>4.63</td>
</tr>
<tr>
<td></td>
<td>(0.62)</td>
<td>(1.01)</td>
<td>(0.83)</td>
<td>(1.59)</td>
<td>(2.45)</td>
</tr>
<tr>
<td>Material-labor substitution, $\sigma_{ML}$</td>
<td>0.03</td>
<td>0</td>
<td>0.01</td>
<td>0</td>
<td>0</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.10)</td>
<td>(0.13)</td>
<td>(0.09)</td>
<td>(0.26)</td>
</tr>
</tbody>
</table>

| Panel B. Empirical moments | Employment elasticity | -0.23 | -0.31 | -0.49 | -0.08 | -0.84 |
|                           | Revenue elasticity   | 0.08  | -0.05 | -0.17 | 0.11  | -0.65 |
|                           | Materials elasticity | 0.05  | -0.17 | -0.26 | 0.04  | -0.73 |
|                           | Capital elasticity   | 0.62  | 0.37  | 0.28  | 0.70  | 0.50  |
|                           | Price elasticity     | 0.25  |       |       |       |       |

| Panel C. Moments predicted by the estimated parameters | Employment elasticity | -0.24 | -0.33 | -0.51 | -0.12 | -0.95 |
|                                                        | Revenue elasticity   | 0.16  | 0.003 | -0.09 | 0.12  | -0.49 |
|                                                        | Materials elasticity | -0.01 | -0.18 | -0.33 | 0     | -0.67 |
|                                                        | Capital elasticity   | 0.58  | 0.29  | 0.23  | 0.22  | 0.1   |
|                                                        | Price elasticity     | 0.18  | 0.23  | 0.25  | 0.12  | 0.18  |
|                                                        | Share of labor, $s_L$| 0.18  | 0.23  | 0.25  | 0.12  | 0.18  |
|                                                        | Share of capital, $s_K$ | 0.08  | 0.07  | 0.08  | 0.07  | 0.08  |
|                                                        | Share of materials, $s_M$ | 0.74  | 0.70  | 0.67  | 0.81  | 0.74  |
|                                                        | No. of moments used  | 4     | 4     | 4     | 4     | 4     |
|                                                        | No. of estimated parameters | 3     | 3     | 3     | 3     | 3     |
|                                                        | SSE                 | 5.64  | 0.76  | 1.00  | 2.20  | 2.02  |

Notes: We estimate the parameters of the model presented in Section V using a minimum-distance estimator. In each column we use the empirical moments based on our benchmark estimates with controls. The estimated parameters with standard errors can be found in panel A. Panels B and C report the empirical and the predicted moments, respectively. SSE reports the weighted sum of squared errors.
Summary

Cost effects of min wage largely passed through to consumers!
(cost-push inflation / redist thru prod market)
Materials share key to rationalizing small losses
Summary

Cost effects of min wage largely passed through to consumers! (cost-push inflation / redist thru prod market) Materials share key to rationalizing small losses

Policy implications

- Price increases in tradeable sectors are a win for small country like Hungary
- Price increases to domestic consumers more problematic
Summary

Cost effects of min wage largely passed through to consumers! (cost-push inflation / redist thru prod market)
Materials share key to rationalizing small losses

Policy implications

▶ Price increases in tradeable sectors are a win for small country like Hungary
▶ Price increases to domestic consumers more problematic

Generalizability:

▶ What about the U.S.? What are the effects of smaller less persistent MW changes?
▶ How to distinguish effects of firm-specific from aggregate MW changes?
Examine 138 state minimum wage changes in the U.S.

Assess impact on state-wide frequency distribution of wages via DiD

- Publicly available data! CPS benchmarked to QCEW

Methodological insight: use distributional impacts to infer employment losses

- How does this work?

- Recall: impact on distribution $\neq$ distribution of impacts!
The basic idea

Key assumptions: exclusion restriction (no effect above $\bar{W}$) + sign restrictions (emp gains in $[MW, \bar{W}]$, losses in $(0, MW)$)
Estimating job loss

*Distributional* event study specification:

\[
\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt}
\]

- \(E_{sjt}\) is employment in $0.25 wage bin \(j\) of state \(s\) at time \(t\)
- \(N_{st}\) is population in state \(s\) at time \(t\)
- \(\alpha_{\tau k}\) effect of min wage hike \(\tau\) periods ago on bins \([k, \ k+1]\) above state min wage
- \(\mu_{sj}\) state by wage bin FE
- \(\rho_{jt}\) bin by year FE
- \(\Omega_{sjt}\) controls for “small” min wage changes
- Cluster on state (i.e., meta-analysis std errors)
Target parameters

*Distributional* event study specification:

\[
\frac{E_{sjt}}{N_{st}} = \sum_{\tau=-3}^{4} \sum_{k=-4}^{17} \alpha_{\tau k} I_{sjt}^{\tau k} + \mu_{sj} + \rho_{jt} + \Omega_{sjt} + u_{sjt}
\]

- Scaled decrease in employment below new minimum

\[
\Delta a_{\tau} = \frac{\sum_{k=-3}^{-1} (\alpha_{\tau k} - \alpha_{-1k})}{EPOP_{-1}}
\]

- Scaled increase above new minimum (setting $\bar{W} - MW = 4$) due to “bunching”:

\[
\Delta b_{\tau} = \frac{\sum_{k=0}^{4} (\alpha_{\tau k} - \alpha_{-1k})}{EPOP_{-1}}
\]

- Net (scaled) employment change at horizon $\tau$ is $\Delta a_{\tau} + \Delta b_{\tau}$
No net employment losses 5 years out..

\[ \Delta a = 0.021 (0.003) \]
\[ \Delta b = -0.018 (0.004) \]
\[ \% \Delta \text{ affected employment} = 0.028 (0.029) \]
\[ \% \Delta \text{ affected wage} = 0.068 (0.010) \]

**Figure II**
Impact of Minimum Wages on the Wage Distribution
Not much in the way of dynamics

**FIGURE III**

Impact of Minimum Wages on the Missing and Excess Jobs over Time
No net employment effect on new entrants or incumbents

**Figure IV**

Impact of Minimum Wages on the Wage Distribution by Pretreatment Employment Status: New Entrants and Incumbents
Substantial wage spillovers above minimum
Except among new entrants and tradeable sectors..

**TABLE IV**

<table>
<thead>
<tr>
<th></th>
<th>%Δ Affected wage</th>
<th>Spillover share of wage increase</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>%Δ w</td>
<td>%Δ w(_{No\ spillover})</td>
</tr>
<tr>
<td>Overall</td>
<td>0.068***</td>
<td>0.041***</td>
</tr>
<tr>
<td></td>
<td>(0.010)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Less than high school</td>
<td>0.077***</td>
<td>0.048***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.009)</td>
</tr>
<tr>
<td>Teen</td>
<td>0.081***</td>
<td>0.053***</td>
</tr>
<tr>
<td></td>
<td>(0.015)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>High school or less</td>
<td>0.073***</td>
<td>0.043***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>Women</td>
<td>0.070***</td>
<td>0.045***</td>
</tr>
<tr>
<td></td>
<td>(0.011)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Black or Hispanic</td>
<td>0.045***</td>
<td>0.037***</td>
</tr>
<tr>
<td></td>
<td>(0.012)</td>
<td>(0.010)</td>
</tr>
<tr>
<td>Tradeable</td>
<td>0.058</td>
<td>0.065**</td>
</tr>
<tr>
<td></td>
<td>(0.073)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Nontradeable</td>
<td>0.056***</td>
<td>0.043***</td>
</tr>
<tr>
<td></td>
<td>(0.014)</td>
<td>(0.006)</td>
</tr>
<tr>
<td>Incumbent</td>
<td>0.095***</td>
<td>0.055***</td>
</tr>
<tr>
<td></td>
<td>(0.020)</td>
<td>(0.011)</td>
</tr>
<tr>
<td>New entrant</td>
<td>0.019</td>
<td>0.023***</td>
</tr>
<tr>
<td></td>
<td>(0.013)</td>
<td>(0.006)</td>
</tr>
</tbody>
</table>

*Notes* The table reports the effects of a minimum wage increase on wages based on the event study analysis (see equation (1)) exploiting 138 state-level minimum wage changes between 1979 and 2016. The table reports the percentage change in affected wages with (column (1)) and without (column (2)) taking spillovers into account for all workers, workers without a high school degree, teens, individuals with high school or less schooling, women, black or Hispanic workers, in tradeable industries, in nontradeable industries; those who were employed one year before the minimum wage increase (incumbents); and those who did not have a job one year before (new entrants). The first column is the estimated change in the affected wages calculated according to equation (2), and the second column assumes no spillovers (see equation (3)). In the last column, the spillover share of the wage effect is calculated by subtracting 1 from the ratio of the estimates in the second to the first column. Robust standard errors in parentheses are clustered by state; significance levels are *0.10, **0.05, ***0.01.*
Reconciling with conventional panel estimates

The behavior of the mean is sensitive to the response of very high wages

**Figure VI**


Employment elas. = -0.089 (0.025)
**Summary**

*Market-wide* fluctuations in state min wages seem to generate tiny employment losses or even small employment gains.

No appreciable dynamics.

Prior time series analyses of aggregates likely confounded by sensitivity of mean to top quantiles.
Methodological lesson: power of going beyond the mean

Key insight of paper was that MW fluctuations should not strongly affect top quantiles of the wage distribution ⇒ sufficient to evaluate MW impact on jobs with $W < \bar{W}$

- Used distributional regressions to find threshold $\bar{W}$ above which treatment effects are trivial
- See Fortin, Lemieux, and Lloyd (2018) for other approaches to distributional regression

Implicitly a restriction on joint distribution of potential outcomes: workers pushed from wages levels below to just above new MW

- Reforms only affecting attractiveness of some options and not others often yield similar identification of adjustment margins
Little support in 2015 for view that MW increases productivity

Question B:

Increasing the federal minimum wage gradually to $15-per-hour by 2020 would substantially increase aggregate output in the US economy.

Responses

Responses weighted by each expert's confidence

Source: IGM Economic Experts Panel
www.igmchicago.org/igm-economic-experts-panel
Min wage lit has focused on market level tradeoff between employment and wage inequality.

But in principle there can also be a beneficial *reallocation* effect of the minimum wage: workers move from less to more productive firms.

Can potentially raise total productivity in the economy if market imperfections “protected” unproductive firms in the first place (Hsieh and Klenow, 2009) or if capital intensity was inefficiently low to begin with (Acemoglu, 2001).

Germany instituted first national minimum wage in January 2015.

- NOT indexed to local cost of living so disproportionately hit less productive East German firms.
- Study reallocation effect of policy.
Distributional DiD (initially low vs high wage workers)
Wages of affected individuals go up, no effect on employment probs

Table 2: Effect of the Minimum Wage on Wages and Employment: Individual Approach

<table>
<thead>
<tr>
<th></th>
<th>(1) Changes relative to 2011 vs 2013</th>
<th>(2) [4.5, 8.5]</th>
<th>(3) [8.5, 12.5]</th>
<th>(4) [12.5, 20.5]</th>
<th>(5) Difference-in-difference (1) minus (3) (2) minus (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wage bin in t-2</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel (a): Hourly Wages</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2014 vs 2016</td>
<td>0.067 (0.0006)</td>
<td>0.023 (0.0003)</td>
<td>0.006 (0.0001)</td>
<td>0.061 (0.0006)</td>
<td>0.016 (0.0003)</td>
</tr>
<tr>
<td>2012 vs 2014 (Placebo)</td>
<td>0.017 (0.0005)</td>
<td>0.009 (0.0003)</td>
<td>0.006 (0.0001)</td>
<td>0.010 (0.0006)</td>
<td>0.003 (0.0003)</td>
</tr>
<tr>
<td>Baseline Change (2011 vs 2013)</td>
<td>0.199 (0.0009)</td>
<td>0.118 (0.0005)</td>
<td>0.080 (0.0002)</td>
<td>0.036 (0.0005)</td>
<td></td>
</tr>
<tr>
<td>Panel (b): Daily Wages</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2014 vs 2016</td>
<td>0.118 (0.0010)</td>
<td>0.047 (0.0005)</td>
<td>0.012 (0.0002)</td>
<td>0.107 (0.0010)</td>
<td>0.036 (0.0005)</td>
</tr>
<tr>
<td>2012 vs 2014 (Placebo)</td>
<td>0.022 (0.0009)</td>
<td>0.012 (0.0005)</td>
<td>0.006 (0.0002)</td>
<td>0.015 (0.0009)</td>
<td>0.006 (0.0005)</td>
</tr>
<tr>
<td>Baseline Change (2011 vs 2013)</td>
<td>0.220 (0.0004)</td>
<td>0.064 (0.0002)</td>
<td>-0.002 (0.0001)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel (c): Employment (1 if employed)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2014 vs 2016</td>
<td>0.009 (0.0004)</td>
<td>0.003 (0.0002)</td>
<td>0.002 (0.0001)</td>
<td>0.007 (0.0004)</td>
<td>0.001 (0.0003)</td>
</tr>
<tr>
<td>2012 vs 2014 (Placebo)</td>
<td>0.003 (0.0004)</td>
<td>0.000 (0.0002)</td>
<td>0.001 (0.0001)</td>
<td>0.002 (0.0004)</td>
<td>-0.001 (0.0003)</td>
</tr>
<tr>
<td>Baseline Change (2011 vs 2013)</td>
<td>-0.242 (-0.184)</td>
<td>-0.184 (-0.141)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Panel (d): Employment, full-time equivalents</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>2014 vs 2016</td>
<td>0.034 (0.0004)</td>
<td>0.018 (0.0002)</td>
<td>0.006 (0.0001)</td>
<td>0.029 (0.0004)</td>
<td>0.013 (0.0003)</td>
</tr>
<tr>
<td>2012 vs 2014 (Placebo)</td>
<td>0.010 (0.0003)</td>
<td>0.006 (0.0002)</td>
<td>0.002 (0.0001)</td>
<td>0.009 (0.0004)</td>
<td>0.004 (0.0003)</td>
</tr>
<tr>
<td>Baseline Change (2011 vs 2013)</td>
<td>-0.180 (-0.193)</td>
<td>-0.193 (-0.179)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: In panel (a), we report the excess hourly wage growth in the 2014 vs 2016 post-policy period and
Initially low wage workers move to better firms.
Higher firm wage FEVs and lower churn rates

Figure 5: Reallocation Effects of the Minimum Wage: Individual Approach

(a) Firm's Wage Premium

(b) Firm's AKM Fixed Effect

(c) Firm Size

(d) Firm's Churning Rate
Figure 5: Exposure to the Minimum Wage across Regions

Notes: The figure shows the exposure to the minimum wage across 401 regions (districts). Regional exposure to the minimum wage is measured using the gap
Big effects on *market* wages, nothing on employment.
Reallocation from smaller to bigger firms

Figure 8: Evidence for Reallocation: Regional Approach

(a) Number of Firms
(b) Number of Micro Firms (1-2 Employees)
(c) Average Firm-Size
(d) Averge Firm FE
(e) Firm Exit
Bigger effects among workers initially in non-tradeables

Figure 9: Heterogeneity of Reallocation Responses

Notes: This figure shows the effect of the minimum wage on the reallocation of low-wage workers to firms that pay a higher average daily wages. Row (1) shows the benchmark estimate when all workers are included in the sample (as in panel (b) in Table 3). In rows (2) and (3), the sample is split into men and women, respectively. Rows (4) and Row (5) estimate the reallocation effect separately for workers who were employed in the tradable and in the non-tradable sector at baseline. We classify sectors into tradable and non-tradable using method 1 in Mian and Sufi (2014).
Summary

- Labor markets are frictional and, when left to their own, can generate misallocation.
- The minimum wage seems to kill less productive firms in less competitive industries.
- But no effect on aggregate employment because workers are reallocated to more productive businesses.
- Possible that total output rose (allocative efficiency).


AEA Continuing Education 2021: Labor Economics and Applied Econometrics

Lecture #2: Firm Wage Premia

Patrick Kline

UC Berkeley
Slichter (1950): a 1940 wage survey from Boston

<table>
<thead>
<tr>
<th></th>
<th>Average Hourly Earnings in All Plants (cents)</th>
<th>Average Hourly Earnings in Lowest Plant (cents)</th>
<th>Average Hourly Earnings in Highest Plant (cents)</th>
<th>Spread between High and Low Plants (cents)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Common labor</td>
<td>57.9</td>
<td>44.8</td>
<td>74.1</td>
<td>29.3</td>
</tr>
<tr>
<td>Janitor</td>
<td>55.3</td>
<td>41.0</td>
<td>70.5</td>
<td>29.5</td>
</tr>
<tr>
<td>Watchman</td>
<td>59.6</td>
<td>45.2</td>
<td>74.0</td>
<td>28.8</td>
</tr>
<tr>
<td>Producing and processing laborers</td>
<td>64.2</td>
<td>44.8</td>
<td>100.7</td>
<td>55.9</td>
</tr>
<tr>
<td>Producing and processing operators</td>
<td>72.0</td>
<td>57.8</td>
<td>88.6</td>
<td>30.8</td>
</tr>
<tr>
<td>Receiving and shipping clerks</td>
<td>68.0</td>
<td>50.0</td>
<td>89.6</td>
<td>39.6</td>
</tr>
<tr>
<td>Machinists</td>
<td>87.5</td>
<td>70.0</td>
<td>105.0</td>
<td>35.0</td>
</tr>
<tr>
<td>Steamfitter</td>
<td>86.4</td>
<td>70.0</td>
<td>105.0</td>
<td>35.0</td>
</tr>
<tr>
<td>Electrician</td>
<td>88.0</td>
<td>67.9</td>
<td>105.0</td>
<td>37.1</td>
</tr>
<tr>
<td>Carpenter</td>
<td>81.5</td>
<td>65.0</td>
<td>99.5</td>
<td>34.5</td>
</tr>
<tr>
<td>Sheet metal workers</td>
<td>85.4</td>
<td>77.8</td>
<td>90.5</td>
<td>12.7</td>
</tr>
<tr>
<td>Millwright</td>
<td>86.1</td>
<td>82.5</td>
<td>95.5</td>
<td>13.0</td>
</tr>
<tr>
<td>Maintenance helper</td>
<td>67.1</td>
<td>50.7</td>
<td>82.0</td>
<td>31.3</td>
</tr>
<tr>
<td>Female producing laborers</td>
<td>45.1</td>
<td>33.8</td>
<td>63.4</td>
<td>29.6</td>
</tr>
<tr>
<td>Female producing operators</td>
<td>47.9</td>
<td>37.7</td>
<td>58.3</td>
<td>20.6</td>
</tr>
<tr>
<td>Firemen</td>
<td>78.4</td>
<td>63.0</td>
<td>90.8</td>
<td>27.8</td>
</tr>
</tbody>
</table>

Slichter: “neither wage rates nor hourly earnings represent the price of labor”
Slichter studies “structure of wages” using industry-level data from 1939 Economic Census (firm data was not available).

Discovers 7 regularities about wages of unskilled men:

1. Positive correlation with wages of skilled co-workers
2. Negative correlation with % female
3. Positive correlation with industry value-added / worker-hour
4. Positive correlation with sales / worker-hour
5. Negative correlation with payroll / sales
6. Positive correlation with sales margin (i.e. value added / sales)
7. Stable over time (high correlation of industry wage rank)

Interpretation: “the results of this study give strong support to the proposition that managerial policy is important in determining inter-industry wage differences.”
Was Slichter right that some industries pay higher wages?
Use panel data to study what happens when workers switch industries
Compare to cross-sectional estimates of wage premia to infer degree of unobserved sorting
Bias correcting the variance of fixed effect estimates

All models amount to:

\[ y_i = D_i' \beta + X_i \delta + \varepsilon_i \]

where \( D_i \) is a vector of industry dummies and \( X_i \) is a vector of controls that may or may not include individual fixed effects

- If each industry fixed effect \( \hat{\beta}_j \sim N ( \beta_j, \sigma_j^2 ) \), then

\[ \mathbb{E} \left[ \hat{\beta}_j^2 \right] = \beta_j^2 + \sigma_j^2. \]

- Suppose we have consistent standard error estimates \( \{ \hat{\sigma}_j \}_{j=1}^J \)

- Then a consistent bias corrected standard deviation of industry wage premia is

\[ \sqrt{ \frac{1}{J-1} \sum_{j=1}^J (\hat{\beta}_j - \bar{\beta})^2 - \frac{1}{J} \sum_{j=1}^J \hat{\sigma}_j^2 } \]

Note: ignoring variability in \( \bar{\beta} = \frac{1}{J} \sum_{j=1}^J \beta_j \) which is of smaller order.
### Table I

**Estimated Wage Differentials for One-Digit Industries—May CPS**

(Standard Errors in Parentheses)

<table>
<thead>
<tr>
<th>Industry</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1974</td>
<td>1979</td>
<td>1984</td>
<td>1984 Total Compensation</td>
</tr>
<tr>
<td>Construction</td>
<td>.195</td>
<td>.126</td>
<td>.108</td>
<td>.091</td>
</tr>
<tr>
<td></td>
<td>(.021)</td>
<td>(.031)</td>
<td>(.034)</td>
<td>(.035)</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>.055</td>
<td>.044</td>
<td>.091</td>
<td>.131</td>
</tr>
<tr>
<td></td>
<td>(.020)</td>
<td>(.029)</td>
<td>(.032)</td>
<td>(.032)</td>
</tr>
<tr>
<td>Transportation &amp; Public Utilities</td>
<td>.111</td>
<td>.081</td>
<td>.145</td>
<td>.203</td>
</tr>
<tr>
<td></td>
<td>(.021)</td>
<td>(.031)</td>
<td>(.034)</td>
<td>(.034)</td>
</tr>
<tr>
<td>Wholesale &amp; Retail Trade</td>
<td>-.128</td>
<td>-.082</td>
<td>-.111</td>
<td>-.136</td>
</tr>
<tr>
<td></td>
<td>(.020)</td>
<td>(.030)</td>
<td>(.033)</td>
<td>(.033)</td>
</tr>
<tr>
<td>Finance, Insurance and Real Estate</td>
<td>.047</td>
<td>-.010</td>
<td>.055</td>
<td>.069</td>
</tr>
<tr>
<td></td>
<td>(.022)</td>
<td>(.035)</td>
<td>(.034)</td>
<td>(.034)</td>
</tr>
<tr>
<td>Services</td>
<td>-.070</td>
<td>-.055</td>
<td>-.078</td>
<td>-.111</td>
</tr>
<tr>
<td></td>
<td>(.021)</td>
<td>(.030)</td>
<td>(.032)</td>
<td>(.032)</td>
</tr>
<tr>
<td>Mining</td>
<td>.179</td>
<td>.229</td>
<td>.222</td>
<td>.231</td>
</tr>
<tr>
<td></td>
<td>(.035)</td>
<td>(.058)</td>
<td>(.075)</td>
<td>(.075)</td>
</tr>
<tr>
<td>Weighted Adjusted Standard Deviation of Differentials(^b)</td>
<td>.097**</td>
<td>.069**</td>
<td>.094**</td>
<td>.126**</td>
</tr>
<tr>
<td>Sample Size</td>
<td>29,945</td>
<td>8,978</td>
<td>11,512</td>
<td>11,512</td>
</tr>
</tbody>
</table>

---

\(^a\) Other explanatory variables are education and its square, 6 age dummies, 8 occupation dummies, 3 region dummies, sex dummy, race dummy, central city dummy, union member dummy, ever married dummy, veteran status, marriage \(\times\) sex interaction, education \(\times\) sex interaction, education squared \(\times\) sex interaction, 6 age \(\times\) sex interactions, and a constant. Each column was estimated from a separate cross-sectional regression.

\(^b\) Weights are employment shares for each year.

**\(^{**} F\) test that industry wage differentials jointly equal 0 rejects at the .000001 level.**
Worker FE estimates $\approx$ Cross-Sectional Estimates!

### TABLE IV
THE EFFECTS OF UNMEASURED LABOR QUALITY$^a$

<table>
<thead>
<tr>
<th>Industry</th>
<th>(1) Fixed Effects Unadjusted for Measurement Error</th>
<th>(2) Fixed Effects Adjusted for Measurement Error I$^b$</th>
<th>(3) Fixed Effects Adjusted for Measurement Error II$^c$</th>
<th>(4) Levels</th>
</tr>
</thead>
<tbody>
<tr>
<td>Construction</td>
<td>.063 (.033)</td>
<td>.098 (.060)</td>
<td>.174 (.060)</td>
<td>.174 (.024)</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>.028 (.031)</td>
<td>.055 (.058)</td>
<td>.107 (.058)</td>
<td>.064 (.022)</td>
</tr>
<tr>
<td>Transportation and Public Utilities</td>
<td>.019 (.035)</td>
<td>.060 (.059)</td>
<td>.049 (.059)</td>
<td>.114 (.024)</td>
</tr>
<tr>
<td>Wholesale and Retail Trade</td>
<td>- .042 (-.031)</td>
<td>- .068 (-.056)</td>
<td>- .125 (-.056)</td>
<td>- .133 (-.023)</td>
</tr>
<tr>
<td>Finance, Insurance and Real Estate</td>
<td>.027 (.036)</td>
<td>.017 (.061)</td>
<td>.018 (.061)</td>
<td>.035 (.025)</td>
</tr>
<tr>
<td>Services</td>
<td>- .040 (-.032)</td>
<td>- .088 (-.056)</td>
<td>- .128 (-.057)</td>
<td>- .079 (-.023)</td>
</tr>
<tr>
<td>Mining</td>
<td>.067 (.004)</td>
<td>.122 (.057)</td>
<td>.142 (.058)</td>
<td>.156 (.040)</td>
</tr>
</tbody>
</table>

$^a$ Data set is three matched May CPS’s pooled together: 1974–1975, 1977–1978, and 1979–1980. Sample size is 18,122. Levels are 1974, 1977, and 1979 data pooled. Results of the 1975, 1978, and 1980 sample are qualitatively the same. Controls for fixed effects regressions are change in education and its square, change in occupation, 3 region dummies, change in union membership, experience squared, change in marital status, year dummies, and a constant. Controls for level regressions are the same as Table I plus year dummies.

$^b$ Adjustment I assumes 3.4 per cent error rate and that misclassifications are proportional to industry size. See Appendix for description.

$^c$ Adjustment II assumes average error rate is 3.4 per cent and misclassifications are allocated according to employer-employee mismatches. See Appendix for description.
No evidence of compensating differentials

TABLE VI
ANALYSIS OF INDUSTRY WAGE DIFFERENTIALS WITH AND WITHOUT CONTROLS FOR WORKING CONDITIONS—QES 1977

<table>
<thead>
<tr>
<th>Industry</th>
<th>Coefficient (SE)</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Construction</td>
<td>.113</td>
<td>.100</td>
</tr>
<tr>
<td></td>
<td>(.098)</td>
<td>(.100)</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>.050</td>
<td>.046</td>
</tr>
<tr>
<td></td>
<td>(.086)</td>
<td>(.087)</td>
</tr>
<tr>
<td>Transportation</td>
<td>.113</td>
<td>.124</td>
</tr>
<tr>
<td></td>
<td>(.095)</td>
<td>(.096)</td>
</tr>
<tr>
<td>Wholesale &amp; Retail Trade</td>
<td>-.056</td>
<td>-.061</td>
</tr>
<tr>
<td></td>
<td>(.090)</td>
<td>(.091)</td>
</tr>
<tr>
<td>Finance, Insurance and Real</td>
<td>.071</td>
<td>.053</td>
</tr>
<tr>
<td>Estate</td>
<td>(.104)</td>
<td>(.105)</td>
</tr>
<tr>
<td>Services</td>
<td>-.107</td>
<td>-.104</td>
</tr>
<tr>
<td></td>
<td>(.090)</td>
<td>(.091)</td>
</tr>
<tr>
<td>Mining</td>
<td>.233</td>
<td>.308</td>
</tr>
<tr>
<td></td>
<td>(.205)</td>
<td>(.220)</td>
</tr>
<tr>
<td>10 Working Condition Variables (^b)</td>
<td>no</td>
<td>yes</td>
</tr>
<tr>
<td>Weighted Adjusted Standard</td>
<td>.113*</td>
<td>.118*</td>
</tr>
<tr>
<td>Deviation of 2-Digit Industry</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Premiums</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(R^2)</td>
<td>.496</td>
<td>.519</td>
</tr>
</tbody>
</table>

\(^a\) Other explanatory variables are education and its square, derived experience and its square, sex, race, 3 region dummies, tenure with employer and its square, union status, and 8 occupation dummies. Sample size is 1,033.

\(^b\) Working condition variables are weekly hours, variables indicating dangerous or unhealthy conditions on the job and whether the danger/threat is serious, commuting time, second and third shift dummies, two dummies indicating extent of choice of overtime, and two dummies indicating whether the physical working conditions are pleasant.

\(*\) F test that industry wage differentials jointly equal 0 is rejected at .00005 level.
People don't quit high wage jobs

<table>
<thead>
<tr>
<th>Independent Variables</th>
<th>(1) Tenure</th>
<th>(2) Quit b</th>
</tr>
</thead>
<tbody>
<tr>
<td>Industry wage premium</td>
<td>2.198</td>
<td>-0.073</td>
</tr>
<tr>
<td></td>
<td>(.676)</td>
<td>(.135)</td>
</tr>
<tr>
<td>Union (1 = yes)</td>
<td>3.179</td>
<td>-0.164</td>
</tr>
<tr>
<td></td>
<td>(.157)</td>
<td>(.037)</td>
</tr>
<tr>
<td>Other variables</td>
<td>Age dummies (6), Age * Sex (6), Education, Education Squared * Sex, Region Dummies (3), Race Dummy, Sex Dummy, Central City Dummy, Firm Size Dummies (4), Plant Size Dummies (4), Marriage Dummy, Marriage * Sex, Veteran Status Dummy</td>
<td>Education, Education Squared, Region Dummies (3), Race Dummy, Sex Dummy, SMSA Dummy, (Age—Education—5) and its square</td>
</tr>
</tbody>
</table>

Sample Size: 8,978, 633

R²: .40, .20

---

a Mean (SD) of Tenure is 5.70 (7.61); Mean (SD) of Quit is .26 (.44).

b Quit equation was estimated with a linear probability model.
Even first differenced estimates of industry wage premia biased if there is sorting based on match effects

Basic idea:

▶ Good workers work in more productive industries but would be paid the same amount everywhere if known to be good
▶ When a worker is revealed to be “good” she moves to the good industry and experiences a wage change
▶ But the causal mechanism is the revelation that she is good, not the industry

Test by looking at exogenous separations associated with plant closings (as measured in CPS Displaced Workers Survey)
Mild evidence of endogenous mobility

Industry premia estimates from switchers very close to cross-sectional:
- Plant closing sample (pictured above): Slope = 0.79, $R^2 = 0.72$
- Layoffs sample (not pictured): Slope = 0.91, $R^2 = 0.81$

Suggests variance of industry wage effects somewhat overstated due to endogenous mobility
- Perhaps also treatment effect heterogeneity?
Abowd, Kramarz, Margolis (1999)

Industry is just a linear combination of firms. Are there big pay differences within the same industry?

- Use Employer-Employee data to study firm switchers
- Fixed effects specification:

\[ y_{it} = \alpha_i + \psi J(i,t) + X'_{it}\beta + \epsilon_{it} \]

- Computational problem: millions of fixed effects. Can’t invert \(X'X\)!
  - Approximate solution method
- Key findings: 90% of industry wage premia attributable to person effects
  - Explanation: industry switching estimates biased by nonrandom sorting of workers to firms within industry (Really??)
Approx FEs very weakly correlated with exact FEs in French data ⇒ original AKM results invalid!

Exact results find \( \frac{\text{Var}(\psi_{J(i,t)})}{\text{Var}(y_{it})} \approx 55\% \) in France and 45\% in Washington state

Note: \( \text{Cov} \left( \psi_{J(i,t)}, \alpha_i \right) < 0 \) – negative assortative matching!

Table 6: Summary of Pooled Human Capital Wage Components

<table>
<thead>
<tr>
<th>Component</th>
<th>Standard Deviation</th>
<th>( \ln w )</th>
<th>( x_\beta )</th>
<th>( \theta )</th>
<th>( \alpha )</th>
<th>( \nu_\eta )</th>
<th>( \psi )</th>
<th>( \varepsilon )</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Real Annualized Wage Rate (( \ln w ))</td>
<td>0.881</td>
<td>1.000</td>
<td>0.224</td>
<td>0.468</td>
<td>0.451</td>
<td>0.212</td>
<td>0.484</td>
<td>0.402</td>
</tr>
<tr>
<td>Time-Varying Personal Characteristics (( x_\beta ))</td>
<td>0.691</td>
<td>0.224</td>
<td>1.000</td>
<td>-0.553</td>
<td>-0.575</td>
<td>-0.099</td>
<td>0.095</td>
<td>0.000</td>
</tr>
<tr>
<td>Person Effect (( \theta ))</td>
<td>0.835</td>
<td>0.468</td>
<td>-0.553</td>
<td>1.000</td>
<td>0.961</td>
<td>0.275</td>
<td>0.080</td>
<td>0.000</td>
</tr>
<tr>
<td>Unobserved Part of Person Effect (( \alpha ))</td>
<td>0.802</td>
<td>0.451</td>
<td>-0.575</td>
<td>0.961</td>
<td>1.000</td>
<td>0.000</td>
<td>0.045</td>
<td>0.000</td>
</tr>
<tr>
<td>Non-time-varying Personal Characteristics (( \nu_\eta ))</td>
<td>0.229</td>
<td>0.212</td>
<td>-0.099</td>
<td>0.275</td>
<td>0.000</td>
<td>1.000</td>
<td>0.101</td>
<td>0.000</td>
</tr>
<tr>
<td>Firm Effect (( \psi ))</td>
<td>0.362</td>
<td>0.484</td>
<td>0.095</td>
<td>0.080</td>
<td>0.045</td>
<td>0.101</td>
<td>1.000</td>
<td>0.000</td>
</tr>
<tr>
<td>Residual (( \varepsilon ))</td>
<td>0.354</td>
<td>0.402</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>0.000</td>
<td>1.000</td>
</tr>
</tbody>
</table>

Notes: Based on 287,241,891 annual observations from 1986-2000 for 68,329,212 persons and 3,662,974 firms in California, Florida, Illinois, Maryland, Minnesota, North Carolina, and Texas. No single state contributed observations for all years. See Table 1.
Sources: Author’s calculations using the LEHD Program data base.

- Use a 100% extract from LEHD of 7 states instead of small subsamples
- Correlation becomes positive! (limited-mobility bias)
- \( \frac{\text{Var}(\psi_{J(t)})}{\text{Var}(y_{it})} \approx 20\% \) (less inflation due to sampling error)
Critiques of AKM

Not theoretically motivated

- Why same firm effect for different types of workers?
- Why wages monotone in productivity?

Negative assortativeness implausible

Endogenous mobility:

- Selection on match
- Selection on firm shocks

Person and firm effects inconsistent in short panels (Abowd, Creecy, Kramarz, 2002; Andrews, 2008)

- Variances biased upwards (same issue as Krueger-Summers)
- Correlation between FE, PE biased downwards
Study changes in German wage structure

- Earlier work by Dustmann, Ludsteck, and Schoenberg (2009) documented an increase in German wage dispersion
- Interpreted within traditional SDI framework – supply / demand / institutions
- Typical view: S+D influence price of skill, I is barrier to price adjustment
- Need SDI-(F) for firms/frictions?

“Rolling”-AKM over 6-7 year intervals

- Each decomposition gives us a “snapshot” of labor market
- Did sorting change? Did importance of firms change?
- Check for endogenous mobility
Interval timing coincides with waves of liberalization of German labor market:

- Hartz reforms: 2003-2005
Growth in wage inequality primarily between establishments

**Figure IV**

Raw and Residual Standard Deviations from Alternative Wage Models
Wage dynamics of job changes

**Figure V**
Mean Wages of Job Changers Classified by Quartile of Mean Wage of Coworkers at Origin and Destination Establishment (A) 1985–1991, (B) 2002–2009
<table>
<thead>
<tr>
<th></th>
<th>Interval 1</th>
<th>Interval 2</th>
<th>Interval 3</th>
<th>Interval 4</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Dimensions / Summary Stats:</td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td></td>
</tr>
<tr>
<td>Number person effects</td>
<td>16,295,106</td>
<td>17,223,290</td>
<td>16,384,815</td>
<td>15,834,602</td>
<td></td>
</tr>
<tr>
<td>Number establishment effects</td>
<td>1,221,098</td>
<td>1,357,824</td>
<td>1,476,705</td>
<td>1,504,095</td>
<td></td>
</tr>
<tr>
<td>Sample size (person-year obs)</td>
<td>84,185,730</td>
<td>88,662,398</td>
<td>83,699,582</td>
<td>90,615,841</td>
<td></td>
</tr>
<tr>
<td>Std. Dev. Log Wages</td>
<td>0.370</td>
<td>0.384</td>
<td>0.432</td>
<td>0.499</td>
<td></td>
</tr>
<tr>
<td>Summary of Parameter Estimates:</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Std. dev. of person effects</td>
<td>0.289</td>
<td>0.304</td>
<td>0.327</td>
<td>0.357</td>
<td></td>
</tr>
<tr>
<td>Std. dev. of establ. effects</td>
<td>0.159</td>
<td>0.172</td>
<td>0.194</td>
<td>0.230</td>
<td></td>
</tr>
<tr>
<td>Std. dev. of Xb</td>
<td>0.121</td>
<td>0.088</td>
<td>0.093</td>
<td>0.084</td>
<td></td>
</tr>
<tr>
<td>Correlation of person/establ. effects (across person-year obs.)</td>
<td>0.034</td>
<td>0.097</td>
<td>0.169</td>
<td>0.249</td>
<td></td>
</tr>
<tr>
<td>RMSE of AKM residual</td>
<td>0.119</td>
<td>0.121</td>
<td>0.130</td>
<td>0.135</td>
<td></td>
</tr>
<tr>
<td>(degrees of freedom)</td>
<td>66,669,487</td>
<td>70,081,245</td>
<td>65,838,023</td>
<td>73,277,100</td>
<td></td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.896</td>
<td>0.901</td>
<td>0.909</td>
<td>0.927</td>
<td></td>
</tr>
<tr>
<td>Comparison Match Model</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>RMSE of Match model</td>
<td>0.103</td>
<td>0.105</td>
<td>0.108</td>
<td>0.112</td>
<td></td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.922</td>
<td>0.925</td>
<td>0.937</td>
<td>0.949</td>
<td></td>
</tr>
<tr>
<td>Std. Dev. of Match Effect*</td>
<td>0.060</td>
<td>0.060</td>
<td>0.072</td>
<td>0.075</td>
<td></td>
</tr>
</tbody>
</table>
**Table 2: Estimation Results for AKM Model, Fit by Interval**

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Dimensions / Summary Stats:</td>
<td></td>
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</tr>
<tr>
<td>Sample size (person-year obs)</td>
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</tr>
<tr>
<td>Std. Dev. Log Wages</td>
<td><strong>0.370</strong></td>
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<table>
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<tr>
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</thead>
<tbody>
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<th>RMSE of AKM residual (degrees of freedom)</th>
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<td>RMSE of Match model</td>
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Summary of Parameter Estimates:

| Std. dev. of person effects          | 0.289                  | 0.304                  | 0.327                  | 0.357                  |
| Std. dev. of establ. effects         | 0.159                  | 0.172                  | 0.194                  | 0.230                  |
| Std. dev. of Xb                      | 0.121                  | 0.088                  | 0.093                  | 0.084                  |
| Correlation of person/establ. effects| 0.034                  | 0.097                  | 0.169                  | 0.249                  |
| (across person-year obs.)            |                       |                       |                       |                       |
| RMSE of AKM residual                 | 0.119                  | 0.121                  | 0.130                  | 0.135                  |
| (degrees of freedom)                 | 66,669,487             | 70,081,245             | 65,838,023             | 73,277,100             |
| Adjusted R-squared                   | 0.896                  | 0.901                  | 0.909                  | 0.927                  |

Comparison Match Model

| RMSE of Match model                  | 0.103                  | 0.105                  | 0.108                  | 0.112                  |
| Adjusted R-squared                   | 0.922                  | 0.925                  | 0.937                  | 0.949                  |
| Std. Dev. of Match Effect*           | 0.060                  | 0.060                  | 0.072                  | 0.075                  |
Table 2: Estimation Results for AKM Model, Fit by Interval

<table>
<thead>
<tr>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Number person effects</td>
<td>16,295,106</td>
<td>17,223,290</td>
<td>16,384,815</td>
<td>15,834,602</td>
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<tr>
<td>Number establishment effects</td>
<td>1,221,098</td>
<td>1,357,824</td>
<td>1,476,705</td>
<td>1,504,095</td>
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<tr>
<td>Sample size (person-year obs)</td>
<td>84,185,730</td>
<td>88,662,398</td>
<td>83,699,582</td>
<td>90,615,841</td>
</tr>
<tr>
<td>Std. Dev. Log Wages</td>
<td><strong>0.370</strong></td>
<td><strong>0.384</strong></td>
<td><strong>0.432</strong></td>
<td><strong>0.499</strong></td>
</tr>
</tbody>
</table>

Summary of Parameter Estimates:

- Std. dev. of person effects: 0.289, 0.304, 0.327, 0.357
- Std. dev. of establ. effects: 0.159, 0.172, 0.194, 0.230
- Std. dev. of Xb: 0.121, 0.088, 0.093, 0.084
- Correlation of person/establ. effects (across person-year obs.): 0.034, 0.097, 0.169, 0.249

- RMSE of AKM residual (degrees of freedom): 0.119, 0.121, 0.130, 0.135
  - 66,669,487, 70,081,245, 65,838,023, 73,277,100
- Adjusted R-squared: 0.896, 0.901, 0.909, 0.927

Comparison Match Model

- RMSE of Match model: 0.103, 0.105, 0.108, 0.112
- Adjusted R-squared: 0.922, 0.925, 0.937, 0.949
- Std. Dev. of Match Effect*: 0.060, 0.060, 0.072, 0.075
Table 2: Estimation Results for AKM Model, Fit by Interval

<table>
<thead>
<tr>
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<th>Interval 2</th>
<th>Interval 3</th>
<th>Interval 4</th>
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<tr>
<td>Dimensions / Summary Stats:</td>
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<td>0.499</td>
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</table>

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<td>0.169</td>
<td>0.249</td>
</tr>
<tr>
<td>(across person-year obs.)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>RMSE of AKM residual</td>
<td>0.119</td>
<td>0.121</td>
<td>0.130</td>
<td>0.135</td>
</tr>
<tr>
<td>(degrees of freedom)</td>
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<td>65,838,023</td>
<td>73,277,100</td>
</tr>
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<td>0.927</td>
</tr>
</tbody>
</table>

Comparison Match Model

<table>
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<tr>
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<th>Interval 1</th>
<th>Interval 2</th>
<th>Interval 3</th>
<th>Interval 4</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>RMSE of Match model</td>
<td>0.103</td>
<td>0.105</td>
<td>0.108</td>
<td>0.112</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.922</td>
<td>0.925</td>
<td>0.937</td>
<td>0.949</td>
</tr>
<tr>
<td>Std. Dev. of Match Effect*</td>
<td>0.060</td>
<td>0.060</td>
<td>0.072</td>
<td>0.075</td>
</tr>
</tbody>
</table>
Growing firm component

Decomposition of Variance of Log Wages

- Var. Residual
- Cov. Xb with Person & Establ. Effects
- Cov. Person & Establ. Effects
- Var. Xb
- Var. Establishment Effects
- Var. Person Effects

- Total variance rises 82%
- Variance of estab. effects rises 108%
- Variance of person effects rises 52%
- 2×Covariance Rises 1200%
Diagnostics

Figure VI
Mean Residuals by Person/Establishment Deciles, 2002–2009
High firm effect jobs last longer

Figure 17b: Survivor Functions for Jobs Initiated in 1989
By Quartile of Estimated Establishment Effect

Notes: figure shows fraction of jobs held by full time male workers in IEB that were initiated in 1989 and are still present after number of years indicated by x-axis. Establishments are divided into quartiles based on their estimated establishment effects from an AKM model fit to data from 1985 to 1991. Quartile 1 refers to the lowest quartile of estimated establishment effects.
Change in returns to education largely due to change in estab effect!

Does this reflect changes in the sorting of workers to firms?

<table>
<thead>
<tr>
<th>Highest education qualification</th>
<th>(1) Change in mean log wage relative to apprentices</th>
<th>(2) Change in mean person effect</th>
<th>(3) Change in mean establishment effect</th>
<th>(4) Remainder</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Missing/none</td>
<td>-14.6</td>
<td>1.8</td>
<td>-12.2</td>
<td>-4.2</td>
</tr>
<tr>
<td>2. Lower secondary school or less (no vocational training)</td>
<td>-10.5</td>
<td>-0.1</td>
<td>-6.3</td>
<td>-4.1</td>
</tr>
<tr>
<td>4. Abitur with or without vocational training*</td>
<td>10.1</td>
<td>0.0</td>
<td>2.6</td>
<td>7.5</td>
</tr>
<tr>
<td>5. University or more</td>
<td>5.7</td>
<td>1.5</td>
<td>3.9</td>
<td>0.3</td>
</tr>
</tbody>
</table>

**Table V**


*Abitur* refers to Allgemeine Hochschulreife, a certificate of completion of advanced level high school.
Cross-Check: Mundlak (1978) Decomposition of Return to Education

Sorting index is coefficient from regression of mean schooling at firm \( \bar{S}_{J(i,t)} \) on individual schooling \( S_i \)

Mundlak comes from running \( w_{it} = \alpha + \beta S_i + \delta \bar{S}_{J(i,t)} + \varepsilon_{it} \)
Contribution of “pure” person component to variance of (unadjusted) industry wage differences ≈ 35-40%

Rise in between-group inequality explained by mix of dispersion in person and firm effects

But biggest contributor is increased correlation (i.e., sorting)

TABLE VI
CONTRIBUTION OF PERSON AND ESTABLISHMENT EFFECTS TO WAGE VARIATION ACROSS OCCUPATIONS AND INDUSTRIES

<table>
<thead>
<tr>
<th></th>
<th>Change in variance (Int. 1 to Int. 4)*</th>
<th>Share</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Panel A: Between occupations (342 three-digit occupations)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Std. dev. of mean log wages</td>
<td>0.233</td>
<td>0.243</td>
</tr>
<tr>
<td>Std. dev. of mean person effects</td>
<td>0.186</td>
<td>0.203</td>
</tr>
<tr>
<td>Std. dev. of mean establ. effects</td>
<td>0.101</td>
<td>0.104</td>
</tr>
<tr>
<td>Correlation of mean person effects and establ. effects</td>
<td>0.110</td>
<td>0.171</td>
</tr>
<tr>
<td>Panel B: Between industries (96 two-digit industries)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Std. dev. of mean log wages</td>
<td>0.173</td>
<td>0.184</td>
</tr>
<tr>
<td>Std. dev. of mean person effects</td>
<td>0.103</td>
<td>0.114</td>
</tr>
<tr>
<td>Std. dev. of mean establ. effects</td>
<td>0.104</td>
<td>0.110</td>
</tr>
<tr>
<td>Correlation of mean person effects and establ. effects</td>
<td>0.242</td>
<td>0.301</td>
</tr>
</tbody>
</table>

Notes: Decompositions based on estimated AKM models summarised in Table III. Occupation is based on main job in each year; establishments are assigned one industry per interval, using consistently-coded two-digit industry.
*Entry in column (5) represents change in variance or covariance component. Entry in column (6) is the share of the total change in variance explained. Shares do not add to 100% because Xb component and its covariances are omitted.
Changes driven by breakdown in bargaining?

Note: newer firms more variable regardless of time period!
Low paying firms not covered by collective bargaining

**Figure X**
Takeaway

AKM as a tool for studying changes in wage structure
- Decompose traditional wage gaps (education, industry, occupation) into person and firm components
- Maybe endogenous mobility not so bad?

Result: big changes in German labor market
- Firms growing more important both directly (wage effects) and indirectly (sorting)
- Timing lines up with institutional changes
- Major cohort effects in firm inequality
Gender wage gap: women paid less than men

- Traditional explanation: women less skilled
- Alternative hypothesis: nice girls don’t ask (Babcock and Laschever, 2009)

Examine outside of the lab by looking at E-E wage data from Portugal merged with firm Value Added measures from BvD

- Q1: do women get the same firm effs as men?
- Q2: do shocks to firm VA get shared equally with male and female employees?
Portugal gender gap similar to US

Figure 2 - Gender pay gap (%)

But Portuguese women have more schooling than men!

Note also that women work at larger but less productive firms. Are they trapped in bad jobs?

<table>
<thead>
<tr>
<th></th>
<th>Full sample</th>
<th>Workers at dual-connected firms</th>
<th>Full sample with VA data</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Males</td>
<td>Females</td>
<td>Males</td>
</tr>
<tr>
<td>Education (yrs)</td>
<td>8.0</td>
<td>8.8</td>
<td>8.6</td>
</tr>
<tr>
<td>Log Real Hrly Wage</td>
<td>1.59</td>
<td>1.41</td>
<td>1.71</td>
</tr>
<tr>
<td>(standard dev.)</td>
<td>(0.55)</td>
<td>(0.50)</td>
<td>(0.58)</td>
</tr>
<tr>
<td>Monthly Hours</td>
<td>162.6</td>
<td>158.0</td>
<td>162.8</td>
</tr>
<tr>
<td>(standard dev.)</td>
<td>(24.7)</td>
<td>(30.1)</td>
<td>(24.0)</td>
</tr>
<tr>
<td>Firm Size (#wkrs)</td>
<td>730</td>
<td>858</td>
<td>1,091</td>
</tr>
<tr>
<td>Fraction Female at Firm</td>
<td>0.24</td>
<td>0.70</td>
<td>0.30</td>
</tr>
<tr>
<td>Log VA/ Worker</td>
<td>3.08</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Number of person-year obs.</td>
<td>9.07M</td>
<td>7.23M</td>
<td>6.01M</td>
</tr>
<tr>
<td>Number of persons</td>
<td>2.12M</td>
<td>1.75M</td>
<td>1.45M</td>
</tr>
<tr>
<td>Number of firms</td>
<td>350K</td>
<td>336K</td>
<td>85K</td>
</tr>
</tbody>
</table>

Notes: Overall sample in columns 1-2 includes paid workers age 19-65 with potential experience ≥1. Sample excludes individuals with inconsistent employment histories. Wages are measured in real (2009=100) Euros per hour. Value added is measured in thousands of real Euros per year. All statistics are calculated across person-year observations. See text for definitions of connected and dual connected sets.
Women switch firms about as often as men

Appendix Table B1: Distributions of Number of Jobs Held in Sample Period, by Gender, and Mean Log Wage by Number of Jobs Held

<table>
<thead>
<tr>
<th># Jobs</th>
<th>Males (Person-year weighted)</th>
<th>Females (Person-year weighted)</th>
<th>Males (Person-weighted)</th>
<th>Females (Person-weighted)</th>
<th>Males</th>
<th>Females</th>
<th>Male-Female Gap</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>67.81</td>
<td>70.37</td>
<td>72.50</td>
<td>74.29</td>
<td>1.56</td>
<td>1.38</td>
<td>0.17</td>
</tr>
<tr>
<td>2</td>
<td>20.93</td>
<td>20.42</td>
<td>18.71</td>
<td>18.51</td>
<td>1.45</td>
<td>1.31</td>
<td>0.15</td>
</tr>
<tr>
<td>3</td>
<td>7.91</td>
<td>6.84</td>
<td>6.39</td>
<td>5.53</td>
<td>1.43</td>
<td>1.29</td>
<td>0.14</td>
</tr>
<tr>
<td>4</td>
<td>2.52</td>
<td>1.87</td>
<td>1.85</td>
<td>1.35</td>
<td>1.41</td>
<td>1.28</td>
<td>0.13</td>
</tr>
<tr>
<td>5</td>
<td>0.68</td>
<td>0.41</td>
<td>0.46</td>
<td>0.27</td>
<td>1.39</td>
<td>1.27</td>
<td>0.12</td>
</tr>
<tr>
<td>6</td>
<td>0.13</td>
<td>0.08</td>
<td>0.08</td>
<td>0.05</td>
<td>1.39</td>
<td>1.26</td>
<td>0.13</td>
</tr>
<tr>
<td>7</td>
<td>0.02</td>
<td>0.01</td>
<td>0.01</td>
<td>0.01</td>
<td>1.37</td>
<td>1.22</td>
<td>0.14</td>
</tr>
<tr>
<td>8</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>0.00</td>
<td>1.39</td>
<td>1.48</td>
<td>-0.09</td>
</tr>
</tbody>
</table>

# Obs. | 9,070,492 | 7,226,310 | 2,119,687 | 1,747,492 | 2,119,687 | 1,747,492 | -- |

Notes: tabulations based on analysis sample of male and female employees in QP data set -- see columns 1 and 2 of Table I. There are 15 males and 7 females with 8 jobs in the sample, accounting for 120 person-year observations for men and 56 person-year observations for women.
Appendix Table B2: Wages of Job Changes for Movers with 2+ Years of Data Before/After Job Change

<table>
<thead>
<tr>
<th>Origin/destination quartile</th>
<th>Number Changes (1)</th>
<th>Pct. Of Changes (2)</th>
<th>2 years before (3)</th>
<th>1 year before (4)</th>
<th>1 year after (5)</th>
<th>2 years after (6)</th>
<th>3 Year Change (%) Raw (7)</th>
<th>Adjusted* (Std Err) (9)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Males</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 to 1</td>
<td>13,787</td>
<td>43.2</td>
<td>1.14</td>
<td>1.14</td>
<td>1.16</td>
<td>1.20</td>
<td>5.6</td>
<td>0.5 (0.5)</td>
</tr>
<tr>
<td>1 to 2</td>
<td>9,139</td>
<td>28.7</td>
<td>1.19</td>
<td>1.18</td>
<td>1.35</td>
<td>1.37</td>
<td>17.6</td>
<td>11.6 (0.6)</td>
</tr>
<tr>
<td>1 to 3</td>
<td>6,283</td>
<td>19.7</td>
<td>1.20</td>
<td>1.19</td>
<td>1.48</td>
<td>1.51</td>
<td>30.6</td>
<td>23.9 (0.7)</td>
</tr>
<tr>
<td>1 to 4</td>
<td>2,682</td>
<td>8.4</td>
<td>1.28</td>
<td>1.27</td>
<td>1.71</td>
<td>1.75</td>
<td>47.3</td>
<td>39.0 (1.2)</td>
</tr>
<tr>
<td>2 to 1</td>
<td>7,293</td>
<td>21.2</td>
<td>1.34</td>
<td>1.35</td>
<td>1.22</td>
<td>1.27</td>
<td>-6.5</td>
<td>-12.0 (0.6)</td>
</tr>
<tr>
<td>2 to 2</td>
<td>12,326</td>
<td>35.8</td>
<td>1.37</td>
<td>1.38</td>
<td>1.40</td>
<td>1.42</td>
<td>5.0</td>
<td>-0.8 (0.6)</td>
</tr>
<tr>
<td>2 to 3</td>
<td>10,356</td>
<td>30.0</td>
<td>1.41</td>
<td>1.42</td>
<td>1.54</td>
<td>1.57</td>
<td>15.9</td>
<td>9.3 (0.5)</td>
</tr>
<tr>
<td>2 to 4</td>
<td>4,496</td>
<td>13.0</td>
<td>1.49</td>
<td>1.49</td>
<td>1.81</td>
<td>1.84</td>
<td>35.3</td>
<td>27.0 (0.9)</td>
</tr>
<tr>
<td>3 to 1</td>
<td>4,356</td>
<td>11.9</td>
<td>1.49</td>
<td>1.52</td>
<td>1.24</td>
<td>1.30</td>
<td>-19.4</td>
<td>-25.6 (0.7)</td>
</tr>
<tr>
<td>3 to 2</td>
<td>8,835</td>
<td>24.2</td>
<td>1.54</td>
<td>1.55</td>
<td>1.45</td>
<td>1.48</td>
<td>-5.8</td>
<td>-12.2 (0.6)</td>
</tr>
<tr>
<td>3 to 3</td>
<td>15,107</td>
<td>41.3</td>
<td>1.61</td>
<td>1.63</td>
<td>1.65</td>
<td>1.67</td>
<td>6.4</td>
<td>-0.3 (0.5)</td>
</tr>
<tr>
<td>3 to 4</td>
<td>8,246</td>
<td>22.6</td>
<td>1.73</td>
<td>1.75</td>
<td>1.94</td>
<td>1.97</td>
<td>24.7</td>
<td>16.0 (0.7)</td>
</tr>
<tr>
<td>4 to 1</td>
<td>1,634</td>
<td>5.4</td>
<td>1.79</td>
<td>1.83</td>
<td>1.39</td>
<td>1.43</td>
<td>-36.2</td>
<td>-43.3 (1.6)</td>
</tr>
<tr>
<td>4 to 2</td>
<td>3,245</td>
<td>10.7</td>
<td>1.82</td>
<td>1.86</td>
<td>1.58</td>
<td>1.61</td>
<td>-20.9</td>
<td>-28.1 (1.2)</td>
</tr>
<tr>
<td>4 to 3</td>
<td>6,589</td>
<td>21.7</td>
<td>1.93</td>
<td>1.97</td>
<td>1.85</td>
<td>1.88</td>
<td>-9.2</td>
<td>-13.1 (0.9)</td>
</tr>
<tr>
<td>4 to 4</td>
<td>18,830</td>
<td>62.1</td>
<td>2.29</td>
<td>2.32</td>
<td>2.41</td>
<td>2.45</td>
<td>15.9</td>
<td>6.1 (0.9)</td>
</tr>
<tr>
<td>Females</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1 to 1</td>
<td>24,130</td>
<td>60.9</td>
<td>1.05</td>
<td>1.04</td>
<td>1.05</td>
<td>1.08</td>
<td>2.9</td>
<td>-0.6 (0.4)</td>
</tr>
<tr>
<td>1 to 2</td>
<td>9,094</td>
<td>23.0</td>
<td>1.10</td>
<td>1.10</td>
<td>1.21</td>
<td>1.23</td>
<td>13.2</td>
<td>8.4 (0.5)</td>
</tr>
<tr>
<td>1 to 3</td>
<td>4,490</td>
<td>11.3</td>
<td>1.13</td>
<td>1.14</td>
<td>1.35</td>
<td>1.37</td>
<td>23.6</td>
<td>17.6 (0.6)</td>
</tr>
<tr>
<td>1 to 4</td>
<td>1,888</td>
<td>4.8</td>
<td>1.25</td>
<td>1.26</td>
<td>1.59</td>
<td>1.62</td>
<td>37.0</td>
<td>29.6 (1.2)</td>
</tr>
<tr>
<td>2 to 1</td>
<td>6,705</td>
<td>29.8</td>
<td>1.20</td>
<td>1.22</td>
<td>1.12</td>
<td>1.16</td>
<td>-4.5</td>
<td>-9.1 (0.5)</td>
</tr>
<tr>
<td>2 to 2</td>
<td>7,711</td>
<td>34.3</td>
<td>1.26</td>
<td>1.28</td>
<td>1.28</td>
<td>1.31</td>
<td>4.2</td>
<td>-1.2 (0.5)</td>
</tr>
<tr>
<td>2 to 3</td>
<td>5,495</td>
<td>24.5</td>
<td>1.33</td>
<td>1.35</td>
<td>1.44</td>
<td>1.46</td>
<td>12.6</td>
<td>6.4 (0.8)</td>
</tr>
<tr>
<td>2 to 4</td>
<td>2,562</td>
<td>11.4</td>
<td>1.44</td>
<td>1.45</td>
<td>1.69</td>
<td>1.73</td>
<td>29.0</td>
<td>20.7 (0.9)</td>
</tr>
<tr>
<td>3 to 1</td>
<td>3,283</td>
<td>16.7</td>
<td>1.38</td>
<td>1.40</td>
<td>1.15</td>
<td>1.20</td>
<td>-17.4</td>
<td>-23.0 (1.3)</td>
</tr>
<tr>
<td>3 to 2</td>
<td>4,762</td>
<td>24.2</td>
<td>1.42</td>
<td>1.45</td>
<td>1.34</td>
<td>1.37</td>
<td>-4.5</td>
<td>-10.9 (1.1)</td>
</tr>
<tr>
<td>3 to 3</td>
<td>7,245</td>
<td>36.8</td>
<td>1.51</td>
<td>1.53</td>
<td>1.54</td>
<td>1.56</td>
<td>5.3</td>
<td>-1.2 (0.7)</td>
</tr>
<tr>
<td>3 to 4</td>
<td>4,381</td>
<td>22.3</td>
<td>1.64</td>
<td>1.66</td>
<td>1.81</td>
<td>1.86</td>
<td>22.0</td>
<td>13.4 (0.9)</td>
</tr>
<tr>
<td>4 to 1</td>
<td>1,014</td>
<td>6.2</td>
<td>1.60</td>
<td>1.64</td>
<td>1.32</td>
<td>1.36</td>
<td>-24.6</td>
<td>-31.3 (2.8)</td>
</tr>
<tr>
<td>4 to 2</td>
<td>1,516</td>
<td>9.2</td>
<td>1.72</td>
<td>1.76</td>
<td>1.54</td>
<td>1.58</td>
<td>-13.7</td>
<td>-21.2 (1.3)</td>
</tr>
<tr>
<td>4 to 3</td>
<td>2,844</td>
<td>17.3</td>
<td>1.82</td>
<td>1.86</td>
<td>1.76</td>
<td>1.81</td>
<td>-1.3</td>
<td>-9.3 (0.9)</td>
</tr>
<tr>
<td>4 to 4</td>
<td>11,064</td>
<td>67.3</td>
<td>2.34</td>
<td>2.38</td>
<td>2.27</td>
<td>2.31</td>
<td>16.1</td>
<td>7.0 (0.8)</td>
</tr>
</tbody>
</table>

Notes: entries are mean log real wages for job changers to/from mixed-gender firms with at least 2 years of wages at the old job and the new job. Origin/destination quartiles are based on mean wages of coworkers in year before (origin) or year after (destination) job move.
Figure 2a: Mean Wages of Male Job Changers By O/D Co-worker Group
A closer look

Wage Changes of Movers vs. Changes of Co-workers, by Origin Group

Origin Group (based on mean co-worker wage at origin firm):

\[ E \left[ \Delta w \mid \Delta w_{\text{coworker}} \right] \approx 0.4 \Delta w_{\text{coworker}} \]
Figure 2b: Mean Wages of Female Job Changers by O/D Coworker Group

Mean Log Wage of Movers
- 4 to 4
- 4 to 3
- 4 to 2
- 4 to 1
- 1 to 4
- 1 to 3
- 1 to 2
- 1 to 1

Time (0 = first year on new job)
Women’s wages less sensitive to firm rank than men’s

Figure III: Comparison of Adjusted Wage Changes of Male and Female Job Movers by Quartile of Coworker Wages at Origin and Destination Firms

Notes: points represent regression adjusted mean log wage changes of male and female job movers in different origin/destination quartiles of mean coworker wages. For example “4 to 1” point shows mean wage changes for men and women who move from 4th quartile of coworker wages to 1st quartile. Fitted line is estimated by OLS to 16 points in the Figure.

dashed line = 45 degree line
solid line = fitted regression line
slope = 0.77 (0.02)
$R^2 = 0.99$
Gender-specific AKMs

Table 3: Summary of Estimated Models for Male and Female Workers

<table>
<thead>
<tr>
<th>Summary of Parameter Estimates: AKM Model</th>
<th>Males</th>
<th>Females</th>
<th>German Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>Std. dev. of pers. effects (person-yr obs.)</td>
<td>0.420</td>
<td>0.400</td>
<td>0.357</td>
</tr>
<tr>
<td>Std. dev. of firm effects (person-yr obs.)</td>
<td>0.247</td>
<td>0.213</td>
<td>0.230</td>
</tr>
<tr>
<td>Std. dev. of Xb (across person-yr obs.)</td>
<td>0.069</td>
<td>0.059</td>
<td>0.084</td>
</tr>
<tr>
<td>Correlation of person/firm effects</td>
<td>0.167</td>
<td>0.152</td>
<td>0.249</td>
</tr>
<tr>
<td>Adjusted R-squared</td>
<td>0.934</td>
<td>0.940</td>
<td>0.927</td>
</tr>
<tr>
<td>Correlation male / female firm effects</td>
<td></td>
<td></td>
<td>0.590</td>
</tr>
</tbody>
</table>

**Comparison job-match effects model:**

<table>
<thead>
<tr>
<th></th>
<th>Males</th>
<th>Females</th>
<th>German Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>Adjusted R-squared</td>
<td>0.946</td>
<td>0.951</td>
<td>0.949</td>
</tr>
<tr>
<td>Std. deviation match effect in AKM model</td>
<td>0.062</td>
<td>0.054</td>
<td>0.075</td>
</tr>
</tbody>
</table>

**Share of variance of log wages due to:**

<table>
<thead>
<tr>
<th></th>
<th>Males</th>
<th>Females</th>
<th>German Men</th>
</tr>
</thead>
<tbody>
<tr>
<td>person effects</td>
<td>57.6</td>
<td>61.0</td>
<td>51.2</td>
</tr>
<tr>
<td>firm effects</td>
<td>19.9</td>
<td>17.2</td>
<td>21.2</td>
</tr>
<tr>
<td>covariance of person/firm effects</td>
<td>11.4</td>
<td>9.9</td>
<td>16.4</td>
</tr>
<tr>
<td>Xb and associated covariances</td>
<td>6.2</td>
<td>7.5</td>
<td>5.2</td>
</tr>
<tr>
<td>residual</td>
<td>4.9</td>
<td>4.4</td>
<td>5.9</td>
</tr>
</tbody>
</table>
No evidence of compensating diff for hours

Appendix Table B7: Relationship Between Estimated Firm Effects and Mean Hours of Workers of Same Gender

<table>
<thead>
<tr>
<th>Models for Males</th>
<th>Models for Females</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>No Industry Controls</td>
</tr>
<tr>
<td></td>
<td>OLS</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
</tr>
</tbody>
</table>

A. Using Regular Contractual Hours

| Log Mean Hours of Workers at Firm (Same Gender) | -0.22 | -0.13 | -0.11 | 0.01  | -0.06 | -0.24 | 0.02  | -0.07 |
|                                              | (0.04) | (0.05) | (0.03) | (0.05) | (0.03) | (0.05) | (0.02) | (0.04) |
| First Stage Coeff.                           | --    | 0.52   | --    | 0.43   | --    | 0.68   | --    | 0.63   |
|                                              | (0.00) | (0.01) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |

B. Using Total Hours

| Log Mean Hours of Workers at Firm (Same Gender) | -0.16 | -0.12 | -0.06 | 0.02  | -0.05 | -0.13 | 0.03  | 0.03   |
|                                               | (0.03) | (0.05) | (0.03) | (0.05) | (0.03) | (0.05) | (0.02) | (0.04) |
| First Stage Coeff.                            | --    | 0.54   | --    | 0.45   | --    | 0.65   | --    | 0.60   |
|                                               | (0.00) | (0.01) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |

Notes: Dependent variable in columns 1-4 is estimated firm-specific wage premium for male employees at a firm. Dependent variable in columns 5-8 is estimated firm-specific wage premium for female employees. Entries represent coefficients of log mean hours of the gender group at the firm. Hours measure in Panel A is regular contractual hours. Hours measure in Panel B is total hours. Models in columns 3-4 and 7-8 include dummies for 20 major industries. All specifications include a constant. Models in even-numbered columns are estimated by IV, using the log mean hours of workers at the same firm in the other gender group as an instrument. Estimated first stage coefficients are reported in second row of the table. All models are fit to micro data for workers in the dual-connected set (n=11,025,257), with standard errors (in parentheses) clustered by firm (n=84,720 firms).
 Normalize gender specific FEs=0 below kink to compare levels (below kink is “competitive frontier”)

 Female FEs have lower VA elasticity. Ratio = 1.37/1.56≈ 0.9
Grouping estimate of relative rent sharing = 0.89

Estimated Firm Effects for Female and Male Workers:
Firm Groups Based on Mean Log VA/L

Note: 45 degree line shown
Estimated slope = 0.89

Note: implicitly using $VA/L$ as instrument for male FEs here
Oaxaca review

\[
E[\psi^M_{J(i,t)}|G(i) = M] - E[\psi^F_{J(i,t)}|G(i) = F] = \underbrace{E[\psi^M_{J(i,t)} - \psi^F_{J(i,t)}|G(i) = F]}_{\text{Bargaining}} \\
+ \underbrace{E[\psi^M_{J(i,t)}|G(i) = M] - E[\psi^M_{J(i,t)}|G(i) = F]}_{\text{Sorting}}
\]

Or Equivalently:

\[
E[\psi^M_{J(i,t)}|G(i) = M] - E[\psi^F_{J(i,t)}|G(i) = F] = \underbrace{E[\psi^M_{J(i,t)} - \psi^F_{J(i,t)}|G(i) = M]}_{\text{Bargaining}} \\
+ \underbrace{E[\psi^F_{J(i,t)}|G(i) = M] - E[\psi^F_{J(i,t)}|G(i) = F]}_{\text{Sorting}}
\]

Give women male firm effects
Assign men to same firms as women
Give men female firm effects
Assign women to same firms as men
Table 4a. Contribution of Firm-based Wage Components to Male-Female Wage Gap

<table>
<thead>
<tr>
<th>Gender Group:</th>
<th>Difference:</th>
<th>Males−Females (percent of overall gap)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Males (1)</td>
<td>Females (2)</td>
</tr>
<tr>
<td>1. Mean log wage of group</td>
<td>1.715</td>
<td>1.481</td>
</tr>
</tbody>
</table>

Means of Estimated Firm Effects:

2. Firm Effect for Males | 0.148 | 0.114 | 0.035 (14.9) |
3. Firm Effect for Females | 0.145 | 0.099 | 0.047 (19.9) |

4. Within-group Difference in Mean Effects for Males and Females | 0.003 | 0.015 | (1.2) (6.3) |

5. Mean Male Firm Effect for Men minus Mean Female Firm Effect for Women (Total contribution of Firm-based Wage Components) | 0.049 (21.2) |

6. Sample sizes | 6,012,521 | 5,012,736 |
## Contribution of Firm-Level Pay Components to Gender Wage Gap

<table>
<thead>
<tr>
<th>Gender Wage Gap</th>
<th>Total Contribution of Firm Components</th>
<th>Decompositions</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Sorting</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Using M Effects</td>
</tr>
<tr>
<td>All</td>
<td>-0.234</td>
<td>0.049</td>
</tr>
<tr>
<td></td>
<td>(21.2)</td>
<td>(14.9)</td>
</tr>
<tr>
<td>By Age Group:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Up to age 30</td>
<td>-0.099</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(28.2)</td>
<td>(18.9)</td>
</tr>
<tr>
<td>Ages 31-40</td>
<td>-0.228</td>
<td>0.045</td>
</tr>
<tr>
<td></td>
<td>(19.7)</td>
<td>(12.6)</td>
</tr>
<tr>
<td>Over Age 40</td>
<td>-0.336</td>
<td>0.069</td>
</tr>
<tr>
<td></td>
<td>(20.6)</td>
<td>(15.0)</td>
</tr>
<tr>
<td>By Education Group:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>&lt; High School</td>
<td>-0.286</td>
<td>0.059</td>
</tr>
<tr>
<td></td>
<td>(20.8)</td>
<td>(15.6)</td>
</tr>
<tr>
<td>High School</td>
<td>-0.262</td>
<td>0.061</td>
</tr>
<tr>
<td></td>
<td>(23.3)</td>
<td>(19.6)</td>
</tr>
<tr>
<td>University</td>
<td>-0.291</td>
<td>0.047</td>
</tr>
<tr>
<td></td>
<td>(16.1)</td>
<td>(8.7)</td>
</tr>
</tbody>
</table>

Notes: see text. Counterfactuals based on estimated two-way fixed effects models described in Table 3.
Sorting effect sets in gradually over 20s
Quick recap

- Male/Female firm effects highly correlated
- But women seem to only get 90% of the firm effect of men
- 5 log point gap in firm effs between genders
- Oaxaca decomp finds most of firm eff contribution occurs due to women being at different firms than men
- But large unexplained component for higher skilled women

Next: validate with rent sharing estimates for job-stayers
Table VI: Effects of Changes in Measured Surplus per Worker on the Change in Wages of Stayers

<table>
<thead>
<tr>
<th>Surplus Measure and Sample:</th>
<th>Number of Firms (1)</th>
<th>Estimated Rent Sharing Coefficients: Male Stayers (2)</th>
<th>Female Stayers (3)</th>
<th>Ratio: Column (3) / Column (2) (4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Excess Log Value Added per Worker (Winsorized at +/- 0.50). Sample = Stayers at Firms with Value Added Data, 2006-9</td>
<td>33,104</td>
<td>0.049</td>
<td>0.045</td>
<td>0.911</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.007)</td>
<td>(0.008)</td>
<td>(0.086)</td>
</tr>
<tr>
<td>2. Excess Log Value Added per Worker (Not Winsorized). Sample = Stayers at Firms with Value Added Data, 2006-9</td>
<td>33,104</td>
<td>0.035</td>
<td>0.031</td>
<td>0.894</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.006)</td>
<td>(0.091)</td>
</tr>
<tr>
<td>3. Excess Log Sales per Worker (Winsorized at +/- 0.50). Sample = Stayers at Firms with Sales Data, 2005-8</td>
<td>44,266</td>
<td>0.021</td>
<td>0.018</td>
<td>0.876</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.005)</td>
<td>(0.182)</td>
</tr>
</tbody>
</table>

Notes: Dependent variables are average change in wages of male or female workers at a firm (regression-adjusted for quadratic in age). Table entries are coefficients of the measured change in surplus per worker, as defined in row heading. Ratios in column 4 are estimated by instrumental variables, treating average change in wages of female stayers as the dependent variable, average change in wages of male stayers as the endogenous explanatory variable, and the change in surplus measure as the instrument. Standard errors, clustered by firm, in parentheses.
Summary

Results consistent with women being less aggressive negotiators (explains $\approx 20\%$ of gender wage gap)

- Wage ladder is “taller” for men than women – women only get 90% of male return to moving up a rung on the ladder
- And women seem to have more trouble climbing the ladder than men – their moves aren’t as directed up the ladder
- Even among women who stay at the same firm – a shock yields a larger effect on male than female wages.

Do other classic wage gaps (age, race) have firm component?

- IQ (Fredriksson, Hensvik, Skans, 2015)
- Elite education (Huneeus et al., 2015)
- Race (Gerard, Lagos, Severnini, and Card, 2018)
Workers don’t like inequality

Solution: break certain occupations off into a new firm

Weil (2014): the “fissured” workplace

Wage discrimination is rarely seen in large firms despite the benefits it could confer. As long as workers are under one roof, the problems presented by horizontal and vertical equity remain. But what if the large employer could wage discriminate by changing the boundary of the firm?
Goldschmidt and Schmeider (2017)

Study “on site” outsourcing in Germany using administrative records from IAB

Focus on Food Cleaning, Security, and Logistics (FCSL) as occupations most likely to be outsourced

Identify outsourcing events as when a large group of workers leave a “mother” establishment to start a new “daughter” establishment

- Flow of 10+ employees
- Daughter must be a FCSL firm offering business services
FCSL jobs gradually being outsourced

FIGURE I
Share of Firms with any Food/Cleaning/Security/Logistics workers, by Industry
Temp agencies and FCSL firms on the rise

(A) Worker in all Occupations

(B) Workers in Food / Cleaning / Security / Logistics Occupations

FIGURE II
AKM FEs for FCSL and non-FCSL highly correlated

Figure A-8: Comparing Estimated Wage Premia (AKM Effects) based on FCSL and Non-FCSL workers

Notes: The figure shows a binned scatter plot of AKM effects estimated using food, cleaning, security and logistics (FCSL) workers and non-FCSL workers. Both sets of AKM effects are normalized to have a mean of zero in the overall establishment distribution. Each dot corresponds to 1/20th of the observations. Sample is restricted to all German establishments with at least 50 employees.
Being outsourced lowers wages

**Figure IV**

Employment Outcomes of Outsourced and Nonoutsourced Workers before and after On-site Outsourcing
Wage losses on order of 10%
Wage losses entirely explained by drop in AKM FE

Panel A shows the average estimated establishment (AKM) effect of the establishments where the workers in the outsourced and control groups are working before \( t = -1 \) and after \( t = 0 \) the outsourcing event. The AKM effect is estimated from a wage regression including a full set of worker and establishment fixed effects using the universe of wage records for full-time male workers in Germany. Panel B shows regression estimates of the effects of being outsourced on log wages before and after the outsourcing event separately for workers who are outsourced into the first quartile of AKM FE and those who are outsourced into the fourth quartile of AKM FE.
Big firms and high wage firms outsource

### TABLE III

**THE EFFECT OF PROXIES FOR WAGE PREMIA ON THE PROBABILITY OF OUTSOURCING**

<table>
<thead>
<tr>
<th></th>
<th>All establishments</th>
<th></th>
<th></th>
<th></th>
<th>Estab. Panel</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
<td>(5)</td>
<td>(6)</td>
</tr>
<tr>
<td>Log estab size</td>
<td>0.0084***</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00016)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Log avg estab wage</td>
<td>0.00044</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.00032)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>AKM effect</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>0.0046***</td>
<td></td>
<td></td>
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</tr>
<tr>
<td></td>
<td>(0.00057)</td>
<td></td>
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</tr>
<tr>
<td>Wage premium to FSCL</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>workers over BSF firms</td>
<td>0.0015***</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(0.00026)</td>
<td></td>
<td></td>
<td></td>
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<td></td>
</tr>
<tr>
<td>Collective agreement</td>
<td></td>
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<td></td>
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</tr>
<tr>
<td></td>
<td>0.0091***</td>
<td></td>
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<td></td>
</tr>
<tr>
<td></td>
<td>(0.0013)</td>
<td></td>
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</tr>
<tr>
<td>Pay wages above standard</td>
<td></td>
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</tr>
<tr>
<td></td>
<td>0.0029**</td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0014)</td>
<td></td>
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<tr>
<td>Observations</td>
<td>2,086,507</td>
<td>2,086,505</td>
<td>1,892,408</td>
<td>1,769,077</td>
<td>68,577</td>
<td>68,595</td>
</tr>
<tr>
<td>Mean of dep var</td>
<td>0.012</td>
<td>0.012</td>
<td>0.011</td>
<td>0.014</td>
<td>0.02</td>
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<tr>
<td>Mean of indep var</td>
<td>4.788</td>
<td>4.285</td>
<td>0.003</td>
<td>1.162</td>
<td>0.81</td>
<td>0.34</td>
</tr>
</tbody>
</table>

*Notes.* Standard errors, in parentheses, are clustered at the establishment level. All regressions exclude East Germany before 1997 and establishments with fewer than 50 workers. Columns (5)–(6) include only establishments included in the IAB Establishment Panel Survey. All regressions control for state dummies, year dummies, and three-digit industry fixed effects. Dependent variable = 1 if the establishment was involved in either a general outsourcing event or an on-site outsourcing event in the following year, and 0 otherwise. “Collective agreement” = 1 if the establishment responded that they were bound by a collective agreement. “Pay wages above standard” = 1 if the establishment responded that they pay salaries and wages above the collectively agreed scale. “Wage premium to FSCL workers over BSF firms” is the ratio of the average wage paid to food, security, cleaning, and logistics workers at the establishment to the average wage paid to food, security, cleaning, and logistics workers employed by business services firms (BSF) or temp agencies in the same county and year. *p < .1, **p < .05, ***p < .01.*
Outsourcing a mediating factor for firm cohort effects?
Outsourcing explains 7-9% of growth in log wage variance

### TABLE IV
THE EVOLUTION OF THE WEST GERMAN WAGE STRUCTURE FROM 1985 TO 2008 AND THE ROLE OF OUTSOURCING

<table>
<thead>
<tr>
<th></th>
<th>Wage structure</th>
<th>Wage structure</th>
<th>Change from 1985 to 2008</th>
<th>Percent of change explained</th>
</tr>
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<tbody>
<tr>
<td>Panel A: Observed</td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Total variance of log daily wages</td>
<td>0.132</td>
<td>0.205</td>
<td>0.073</td>
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</tr>
<tr>
<td>Variance of estab effects</td>
<td>0.0289</td>
<td>0.0547</td>
<td>0.0258</td>
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</tr>
<tr>
<td>$2 \times \text{cov(person, estab effect)}$</td>
<td>-0.0050</td>
<td>0.0426</td>
<td>0.0475</td>
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<tr>
<td>85-15 log wage gap</td>
<td>0.655</td>
<td>0.934</td>
<td>0.279</td>
<td></td>
</tr>
<tr>
<td>85-50 log wage gap</td>
<td>0.385</td>
<td>0.512</td>
<td>0.127</td>
<td></td>
</tr>
<tr>
<td>50-15 log wage gap</td>
<td>0.270</td>
<td>0.422</td>
<td>0.152</td>
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</tr>
<tr>
<td>Panel B: Counterfactual I: DFL reweighting of CSL workers</td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>Total variance of log daily wages</td>
<td>0.132</td>
<td>0.198</td>
<td>0.067</td>
<td>8.9</td>
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<tr>
<td>Variance of estab effects</td>
<td>0.0289</td>
<td>0.0525</td>
<td>0.0236</td>
<td>8.4</td>
</tr>
<tr>
<td>$2 \times \text{cov(person, estab effect)}$</td>
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<td>0.0381</td>
<td>0.0431</td>
<td>9.4</td>
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<tr>
<td>85-15 log wage gap</td>
<td>0.655</td>
<td>0.916</td>
<td>0.260</td>
<td>6.7</td>
</tr>
<tr>
<td>85-50 log wage gap</td>
<td>0.385</td>
<td>0.503</td>
<td>0.118</td>
<td>7.1</td>
</tr>
<tr>
<td>50-15 log wage gap</td>
<td>0.270</td>
<td>0.412</td>
<td>0.142</td>
<td>6.4</td>
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<tr>
<td>Panel C: Counterfactual II: adjusting daily wage and AKM effect of additional outsourced workers</td>
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<tr>
<td>Total variance of log daily wages</td>
<td>0.132</td>
<td>0.200</td>
<td>0.068</td>
<td>7.1</td>
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<tr>
<td>$2 \times \text{cov(person, estab effect)}$</td>
<td>-0.0050</td>
<td>0.0408</td>
<td>0.0457</td>
<td>3.8</td>
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<tr>
<td>85-15 log wage gap</td>
<td>0.655</td>
<td>0.925</td>
<td>0.270</td>
<td>3.3</td>
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<tr>
<td>85-50 log wage gap</td>
<td>0.385</td>
<td>0.510</td>
<td>0.125</td>
<td>1.6</td>
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<tr>
<td>50-15 log wage gap</td>
<td>0.270</td>
<td>0.415</td>
<td>0.144</td>
<td>4.7</td>
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<tr>
<td>Percent working in CLS occupations</td>
<td>0.127</td>
<td>0.138</td>
<td>0.011</td>
<td></td>
</tr>
<tr>
<td>Percent outsourced</td>
<td>0.039</td>
<td>0.099</td>
<td>0.060</td>
<td></td>
</tr>
</tbody>
</table>

Notes. Sample are all full-time male workers in West Germany, excluding workers in food occupations or food industries. Panel A shows the observed wage structure in 1985 and 2008 as well as the estimated components due to the variance of establishment effects and the covariance of establishment with person effects. 85-15 log wage gap refers to the difference between the 85th and 15th percentiles of log daily wages. Panel B shows the counterfactual where workers in cleaning, security, and logistics (CLS) occupations in 2008 are reweighted to keep them at the same percentiles of the AKM distribution as in 1985 using DFL reweighting (see text). Panel C shows the counterfactual where a random fraction of workers in CSL business service firms and temp agencies are “insourced” in 2008 by adding 10 log points to their log wage and establishment effect. The fraction to be insourced is picked so that the fraction of outsourced workers remains at the 1985 level.
Thoughts

Boundaries of the firm are changing
▶ Easier to pay workers less by segregating them in new establishment
▶ Wage losses of “fissuring” largely explained by AKM FE
▶ Validation of causal interpretation

Related literature echoing Gibbons-Katz (1992) uses AKM FE to explain wage effects of mass layoffs
▶ In Germany AKM FEs explain nearly all of wage loss (Schmieder, Von Wachter, Heining, 2018)
▶ In Washington state, FEs explain $\sim 17\%$ of wage loss (Lachowska, Mas, Woodbury, 2020).
▶ Important differences in structure of job losses between countries? (Bertheau et al, 2021)
Econometrics of AKM

\[ Y_{it} = \alpha_i + \psi_{j(i,t)} + X'_{it} \xi + \varepsilon_{it} \]

where \( j(i, t) \in \{1, \ldots, J\} \) gives identity of current employer.

Matrix representation:

\[ Y = D\alpha + F\psi + X\xi + \varepsilon \]

- Isomorphic to standard panel model but with \( J \) treatments.
- Treat \( Z = (D, F, X) \) as fixed (i.e. all expectations conditional on \( Z \))

Identification:

- Exogeneity: \( \mathbb{E} [\varepsilon] = 0 \) (plausible?)
- Rank condition: need at least one restriction on the \( \{\psi_j\}_{j=1}^J \) within each “connected set” of firms
Variance decomposition

Target parameter: size weighted variance of firm effects

\[ \theta_\psi = \sum_{j=1}^{J} s_j \left( \psi_j - \bar{\psi} \right)^2, \]

where \( s_j \) is firm \( j \)’s employment share and \( \bar{\psi} = \sum_{j=1}^{J} s_j \psi_j \).

Customary to use OLS estimates \( \hat{\psi} \) to compute “plug-in” estimates of variance components, e.g.:

\[ \hat{\theta}_\psi = \sum_{j=1}^{J} s_j \left( \hat{\psi}_j - \hat{\psi} \right)^2 \]

\[ = \sum_{j=1}^{J} s_j \left( \hat{\psi}_j \right)^2 - \left( \hat{\psi} \right)^2 \]
Bias in the square

OLS is unbiased

$$\mathbb{E} \left[ \hat{\psi}_j \right] = \psi_j$$

But the square of an unbiased estimator is upward biased

$$\mathbb{E} \left[ (\hat{\psi}_j)^2 \right] = \mathbb{E} \left[ (\hat{\psi}_j - \psi_j + \psi_j)^2 \right]$$

$$= \mathbb{E} \left[ (\hat{\psi}_j - \psi_j)^2 \right] + 2\mathbb{E} \left[ \hat{\psi}_j - \psi_j \right] \psi_j + \psi_j^2$$

$$= \psi_j^2 + \mathbb{V} \left[ \hat{\psi}_j \right]$$

bias
Bias of plugin

By same argument plug-in estimator is biased

\[
\mathbb{E} \left[ \hat{\theta}_\psi \right] = \sum_{j=1}^{J} s_j \mathbb{E} \left[ (\hat{\psi}_j)^2 \right] - \mathbb{E} \left[ (\hat{\psi})^2 \right] \\
= \sum_{j=1}^{J} s_j \left\{ \psi_j^2 + \mathbb{V} \left[ \hat{\psi}_j \right] \right\} - (\bar{\psi})^2 - \mathbb{V} \left[ \hat{\psi} \right] \\
= \theta_\psi + \sum_{j=1}^{J} s_j \mathbb{V} \left[ \hat{\psi}_j \right] - \mathbb{V} \left[ \hat{\psi} \right] \\
\underbrace{\mathbb{V} \left[ \hat{\psi} \right]}_{\text{bias}} \text{ term typically negligible when } J \text{ is large.}
Correcting the bias

Bias is weighted average of squared standard errors on firm effects:

\[ \mathbb{E} \left[ \hat{\theta}_\psi - \theta_\psi \right] \approx \sum_{j=1}^{J} s_j \mathbb{V} \left[ \hat{\psi}_j \right] \]
Correcting the bias

Bias is weighted average of squared standard errors on firm effects:

$$E \left[ \hat{\theta}_j - \theta_j \right] \approx \sum_{j=1}^{J} s_j \sqrt{V[\hat{\psi}_j]}$$

Can't we just do Krueger-Summers style correction based on conventional het-consistent ("robust") standard errors $\hat{V}_{HC}[\hat{\psi}_j]$?

- No, because HC standard errors break down (are inconsistent) when # of regressors grow in proportion to sample size.
- Same problem for bootstrap (Bickel and Freedman, 1983)
- To handle high dimensionality: swap usual het-consistent estimators $\hat{V}_{HC}[\hat{\psi}_j]$ for het-unbiased estimators $\hat{V}_{HU}[\hat{\psi}_j]$. Noise averages out across estimates.
Bias correction: homoscedastic case

Andrews et al (2008): bias correct assuming $\mathbb{V} [\varepsilon] = I\sigma^2$

$$\mathbb{V} \left[ \hat{\psi} \right] = \left( \tilde{F}' \tilde{F} \right)^{-1} \sigma^2$$

where $\tilde{F}$ is residualized version of $F$ (against $D$ and $X$).

▶ Estimate $\mathbb{V} \left[ \hat{\psi} \right]$ using DoF adjusted regression MSE

$$\hat{\sigma}^2 = \frac{SSR}{n - \text{dim}(Z)}$$

▶ But homoscedasticity is a strong assumption
  ▶ Can’t be correct if outcome is bounded
  ▶ And in the case of log wages there is ample evidence that error variance differs by gender / experience (e.g., Lemieux, 2006)
Bias correction: heteroscedasticity

Index each person-year observation by $\ell = \ell (i, t)$

- Suppose errors $\{\varepsilon_\ell\}$ are mutually independent
- But potentially heteroscedastic with variances $\sigma^2_\ell = \mathbb{V}[\varepsilon_\ell]$

Yields familiar “sandwich” variance expression (White, 1980)

$$
\mathbb{V} [\hat{\psi}] = \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \tilde{F}' \Omega \tilde{F} \right) \left( \tilde{F}' \tilde{F} \right)^{-1}
$$

where $\Omega = \text{diag} \left( \sigma^2_1, \ldots, \sigma^2_n \right)$.

Estimation challenge: How to get the error variances $\{\sigma^2_\ell\}_{\ell=1}^n$?
Write AKM as high-dimensional regression:

\[ Y_\ell = Z'_\ell \beta + \varepsilon_\ell, \quad \text{for } \ell = 1, \ldots, n. \]

- Let \( \hat{\beta}_{-\ell} \) denote the OLS estimator of \( \beta \) obtained after leaving out obs \( \ell \). (Requires leave-out connectedness)
- “Cross-fit” estimator of \( \sigma^2_\ell \) is unbiased:

\[
\hat{\sigma}^2_\ell = Y_\ell \underbrace{\left( Y_\ell - Z'_\ell \hat{\beta}_{-\ell} \right)}_{\text{leave-out prediction error}}
\]
Cross-fitting

“Cross-fit” estimator of $\sigma^2_\ell$ is unbiased:

$$\hat{\sigma}^2_\ell = Y_\ell \left(Y_\ell - Z'_\ell \hat{\beta}_{-\ell}\right)$$

leave-out prediction error

$$= (\varepsilon_\ell + Z'_\ell \beta) \left(\varepsilon_\ell + Z'_\ell \left(\beta - \hat{\beta}_{-\ell}\right)\right)$$

Intuition: leave-out breaks corr between $\hat{\beta}$ and $\varepsilon_\ell$

$$E \left[\varepsilon_\ell \left(\beta - \hat{\beta}_{-\ell}\right)\right] = E \left[\varepsilon_\ell \left(\sum_{l \neq \ell} Z_l Z'_l\right)^{-1} \sum_{l \neq \ell} Z_l \varepsilon_l\right]$$

$$= \left(\sum_{l \neq \ell} Z_l Z'_l\right)^{-1} \sum_{l \neq \ell} Z_l E[\varepsilon_\ell \varepsilon_l]$$

$$= 0$$
Bias correction

Proxy $\Omega$ with $\hat{\Omega} = diag \{ \hat{\sigma}_\ell^2 \}_{\ell=1}^n$ to get unbiased variance estimates

$$\hat{\nabla}_{HU} \left[ \hat{\psi} \right] = \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \tilde{F}' \hat{\Omega} \tilde{F} \right) \left( \tilde{F}' \tilde{F} \right)^{-1}$$

Bias corrected estimator of $\theta_\psi$ is:

$$\hat{\theta}_{\psi,HU} = \hat{\theta}_\psi - \sum_{j=1}^{J} s_j \hat{\nabla}_{HU} \left[ \hat{\psi}_j \right] + \hat{\nabla}_{HU} \left[ \hat{\psi} \right]$$

- plugin
- average squared stderr
- stderr of mean
Generalization

What about other variances and covariances?

▶ KSS consider more general (co-)variance components

\[ \theta = \beta' A \beta \]

where \( A \) is user specified matrix.

▶ General bias correction formula:

\[ \hat{\theta}_{HU} = \hat{\theta} - \sum_{\ell=1}^{n} B_{\ell\ell} \hat{\sigma}_{\ell}^2 \]

where \( B_{\ell\ell} = Z'_{\ell} (\sum_{l=1}^{n} Z_l Z'_l)^{-1} A (\sum_{l=1}^{n} Z_l Z'_l)^{-1} Z_{\ell} \) gives influence of \( \varepsilon_{\ell}^2 \) on \( \hat{\theta} \). Mathematical intuition:

\[ \hat{\theta} = \theta + \sum_{\ell=1}^{n} B_{\ell\ell} \varepsilon_{\ell}^2 + o_p(1) \]
A useful trick:

\[ \hat{\sigma}_\ell^2 = Y_\ell \left( Y_\ell - Z'_\ell \hat{\beta}_\ell \right) \]
\[ = Y_\ell \frac{\left( Y_\ell - Z'_\ell \hat{\beta} \right)}{1 - P_{\ell\ell}} \]

where \( \{P_{\ell\ell}\} \) are the diagonal elements of \( P = Z (Z'Z)^{-1} Z' \).

▶ Note: only need to compute \( \hat{\beta} \) once!

▶ In large problems can stochastically approximate \( \{B_{\ell\ell}, P_{\ell\ell}\} \)
  (CHK size application in <1hr)

▶ Code / executables available at GitHub repository
Application to Italian data

Administrative records from Italian province of Veneto

Compare plug-in (AKM), homoscedasticity-only (HO) estimator of Andrews (2008), and KSS

Base sample: two wage observations per worker

► With a single wage change per worker we can ignore serial correlation / clustering when computing firm effect variances
► Allows us to focus on importance of heteroscedasticity, but throws away some of the data
► Analyzing 6 year panel via leave-worker-out yields similar results

Split by age: older workers move less ⇒ more bias
Bias correction to variance of firm effs
Homoscedastic correction about half way between naive plug-in and KSS

Variance Decomposition

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Younger Workers</th>
<th>Older Workers</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Variance of Firm Effects</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.0358</td>
<td>0.0368</td>
<td>0.0415</td>
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<tr>
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<td>0.0295</td>
<td>0.0270</td>
<td>0.0350</td>
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<tr>
<td>Leave Out (KSS)</td>
<td>0.0240</td>
<td>0.0218</td>
<td>0.0204</td>
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<tr>
<td><strong>Variance of Person Effects</strong></td>
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<td>0.0647</td>
<td>0.2046</td>
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<td>0.1910</td>
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<tr>
<td><strong>Covariance of Firm, Person Effects</strong></td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Plug in (PI)</td>
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<td>−0.0058</td>
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<td>0.8184</td>
<td>0.9524</td>
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<td>Leave Out (KSS)</td>
<td>0.8976</td>
<td>0.8091</td>
<td>0.9489</td>
</tr>
</tbody>
</table>
Large bias in correlation coefficient
Flips sign in age-specific samples!

### Variance Decomposition

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<td>0.0147</td>
<td>0.0075</td>
<td>0.0171</td>
</tr>
<tr>
<td><strong>Correlation of Firm, Person Effects</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.0565</td>
<td>-0.1040</td>
<td>-0.0334</td>
</tr>
<tr>
<td>Homoscedasticity Only (HO)</td>
<td>0.1649</td>
<td>0.0726</td>
<td>0.0475</td>
</tr>
<tr>
<td>Leave Out (KSS)</td>
<td>0.2830</td>
<td>0.2092</td>
<td>0.2744</td>
</tr>
<tr>
<td><strong>Coefficient of Determination (R^2)</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.9546</td>
<td>0.9183</td>
<td>0.9774</td>
</tr>
<tr>
<td>Homoscedasticity Only (HO)</td>
<td>0.9029</td>
<td>0.8184</td>
<td>0.9524</td>
</tr>
<tr>
<td>Leave Out (KSS)</td>
<td>0.8976</td>
<td>0.8091</td>
<td>0.9489</td>
</tr>
</tbody>
</table>
Small decrease in total explanatory power of model

Note: HO estimate is familiar “adjusted” $R^2$, which seems to exhibit negligible bias.

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Younger Workers</th>
<th>Older Workers</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Variance of Firm Effects</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.0358</td>
<td>0.0368</td>
<td>0.0415</td>
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<tr>
<td>Homoscedasticity Only (HO)</td>
<td>0.0295</td>
<td>0.0270</td>
<td>0.0350</td>
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<tr>
<td>Leave Out (KSS)</td>
<td>0.0240</td>
<td>0.0218</td>
<td>0.0204</td>
</tr>
<tr>
<td><strong>Variance of Person Effects</strong></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.1321</td>
<td>0.0843</td>
<td>0.2180</td>
</tr>
<tr>
<td>Homoscedasticity Only (HO)</td>
<td>0.1173</td>
<td>0.0647</td>
<td>0.2046</td>
</tr>
<tr>
<td>Leave Out (KSS)</td>
<td>0.1119</td>
<td>0.0596</td>
<td>0.1910</td>
</tr>
<tr>
<td><strong>Covariance of Firm, Person Effects</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Plug in (PI)</td>
<td>0.0039</td>
<td>−0.0058</td>
<td>−0.0032</td>
</tr>
<tr>
<td>Homoscedasticity Only (HO)</td>
<td>0.0097</td>
<td>0.0030</td>
<td>0.0040</td>
</tr>
<tr>
<td>Leave Out (KSS)</td>
<td>0.0147</td>
<td>0.0075</td>
<td>0.0171</td>
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<td>0.9489</td>
</tr>
</tbody>
</table>
Estimates from 6 year panel nearly identical after accounting for serial correlation

Leaving match out yields same answer as leaving whole worker out
⇒ sufficient to “cluster” std err estimates $\hat{\psi}_j$ by match

**TABLE A.I**

<table>
<thead>
<tr>
<th>Variance of Firm Effects</th>
<th>Pooled</th>
<th>Younger Workers</th>
<th>Older Workers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Plug-in</td>
<td>0.0304</td>
<td>0.0303</td>
<td>0.0376</td>
</tr>
<tr>
<td>Leave Person-Year Out</td>
<td>0.0296</td>
<td>0.0302</td>
<td>0.0314</td>
</tr>
<tr>
<td><strong>Leave Match Out</strong></td>
<td>0.0243</td>
<td>0.0221</td>
<td>0.0265</td>
</tr>
<tr>
<td>Leave Worker Out</td>
<td>0.0241</td>
<td>0.0227</td>
<td>0.0270</td>
</tr>
</tbody>
</table>
Projecting fixed effects onto observables

- Common to project fixed effect estimates $\hat{\psi}$ onto covariates
- Problem: $\hat{\psi}$ are correlated with one another
- Dependence hinges on design because

$$\hat{\psi} = \psi + \left( \tilde{F}' \tilde{F} \right)^{-1} \tilde{F}' \varepsilon$$

(correlated noise)

- Solution: use HU variance estimator

$$\hat{\mathbf{V}}_{HU} \left[ \hat{\psi} \right] = \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \tilde{F}' \hat{\Omega} \tilde{F} \right) \left( \tilde{F}' \tilde{F} \right)^{-1}$$

$$= \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \sum_{\ell=1}^{n} \tilde{f}_\ell \tilde{f}_\ell' \hat{\sigma}_\ell^2 \right) \left( \tilde{F}' \tilde{F} \right)^{-1}$$
Connection to HC2

HC2 estimator (Mackinnon and White, 1985) is:

$$\hat{V}_{HC2} \left[ \hat{\psi} \right] = \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \sum_{\ell=1}^{n} \tilde{f}_\ell \tilde{f}'_\ell \frac{\left( Y_\ell - Z'_\ell \hat{\beta} \right)^2}{1 - P_{\ell\ell}} \right) \left( \tilde{F}' \tilde{F} \right)^{-1}$$

- HC2 is unbiased under *homo*-scedasticity but otherwise inconsistent when $\text{dim} \left( \tilde{F} \right) \propto n$.

HU estimator is:

$$\hat{V}_{HU} \left[ \hat{\psi} \right] = \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \sum_{\ell=1}^{n} \tilde{f}_\ell \tilde{f}'_\ell \frac{Y_\ell \left( Y_\ell - Z'_\ell \hat{\beta} \right)}{1 - P_{\ell\ell}} \right) \left( \tilde{F}' \tilde{F} \right)^{-1}$$

- Unbiased under arbitrary heteroscedasticity.
Standard errors on projection

Projection of $\psi$ onto $W$ is linear combination:

$$(W'W)^{-1} W' \psi = \nu' \psi$$

- Estimator of variance of projection coefficients is

$$\hat{\nu}_{HU} [\nu' \hat{\psi}] = \nu' \left( \tilde{F}' \tilde{F} \right)^{-1} \left( \tilde{F}' \Omega \tilde{F} \right) \left( \tilde{F}' \tilde{F} \right)^{-1} \nu$$

- Suppose $\nu$ is $J \times 1$ (i.e., single projection coefficient of interest)
- Provided $\nu'$ doesn’t place “too much” weight on any particular coefficient KSS show that:

$$\frac{\nu' \left( \hat{\psi} - \psi \right)}{\sqrt{\hat{\nu}_{HU} [\nu' \hat{\psi}]}} \rightarrow N (0, 1)$$

lincom_KSS: high-dim version of Stata “lincom” command
Naive “robust” std err order of magnitude too small!

<table>
<thead>
<tr>
<th>PROJECTING FIRM EFFECTS ONTO COVARIATES&lt;sup&gt;a&lt;/sup&gt;</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
</tr>
<tr>
<td>Older Worker</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Log Firm Size</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Older Worker × Log Firm Size</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Predicted Gap in Firm Effects (Older vs. Younger Workers)</td>
</tr>
</tbody>
</table>

<sup>a</sup>This table reports the coefficients from projections of firm effects onto worker and firm characteristics in the pooled leave-one-out sample. A constant is included in each model. Standard errors based on equation (7) reported in parentheses. Naive Eicker–White (HC1) standard errors shown in square brackets. “Predicted Gap in Firm Effects” reports the predicted difference in firm effects between older and younger workers according to either Column (1) or Column (2) evaluated at the median firm size of 12 workers.

Naive std error on old dummy off by a factor of 24 in Col 2! Leave out std error reveals that older workers no more likely to work at high paying firms after adjusting for firm size.
Testing high dimensional hypotheses about fixed effects

Do the firm effects for younger workers equal those faced by older workers?

\[ H_0 : \psi^O_j = \psi^Y_j \quad \text{for } j = 1, \ldots, J \]

- \( J = 8,578 \Rightarrow \) cannot rely on standard \( \chi^2 (8578) \) approximation to F-test
- Bootstrap also fails

KSS: test by estimating the variance component

\[ \theta_{H_0} = \frac{1}{8578} \left( \psi^O - \psi^Y \right)' \left( \tilde{F}' \tilde{F} \right) \left( \psi^O - \psi^Y \right) \]

Intuition:
- If \( H_0 \) is true, we must have \( \theta_{H_0} = 0 \)
- \( \tilde{F}' \tilde{F} \) gives optimal (i.e. inverse variance) weighting of differences \( \hat{\psi}^O - \hat{\psi}^Y \) under homoscedasticity
Testing high dimensional hypotheses about fixed effects

Do the firm effects for younger workers equal those faced by older workers?

\[ H_0 : \psi_j^O = \psi_j^Y \quad \text{for } j = 1, \ldots, J \]

KSS: test by estimating the variance component

\[
\theta_{H_0} = \frac{1}{8578} \left( \psi^O - \psi^Y \right)' \left( \tilde{F}' \tilde{F} \right) \left( \psi^O - \psi^Y \right)
\]

Under \( H_0 \): \( \hat{\theta}_{H_0} \) converges to \( N \left( 0, \mathbb{V} \left[ \hat{\theta}_{H_0} \right] \right) \).

- Estimation of \( \mathbb{V} \left[ \hat{\theta}_{H_0} \right] \) explained in paper.

- Test statistic is simple t-stat \( \frac{\hat{\theta}_{H_0}}{\sqrt{\mathbb{V}_{HU} \left[ \hat{\theta}_{H_0} \right]}} \)
Firm effects highly correlated across age groups

But can decisively reject that they are exactly the same

PI Slope: .501; PI Correlation: .54.
KSS Slope: .987; KSS Correlation: .89.
Test Statistic for equal firm effects: 3.95.
Do different racial groups share equally in firm effects?

Fit AKM model to Brazilian data 2002-2014

- Bias correct via KSS
- Apply high dimensional Oaxaca decomp ala CCK (2016)
- Usual sorting component additionally decomposed based upon regional racial / education shares
Estab effs ~14-16% of variance for each group

<table>
<thead>
<tr>
<th></th>
<th>White male</th>
<th>Non-white male</th>
<th>White female</th>
<th>Non-white female</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
<td>(3)</td>
<td>(4)</td>
</tr>
<tr>
<td>Standard deviation of log wages</td>
<td>0.670</td>
<td>0.582</td>
<td>0.685</td>
<td>0.554</td>
</tr>
<tr>
<td>Mean log wages</td>
<td>1.989</td>
<td>1.768</td>
<td>1.782</td>
<td>1.557</td>
</tr>
<tr>
<td>A. AKM decomposition</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Std. dev. of person effects (across person-yr obs.)</td>
<td>0.484</td>
<td>0.415</td>
<td>0.527</td>
<td>0.437</td>
</tr>
<tr>
<td>Std. dev. of estab effects (across person-yr obs.)</td>
<td>0.304</td>
<td>0.279</td>
<td>0.304</td>
<td>0.266</td>
</tr>
<tr>
<td>Std. dev. of covariates (across person-yr obs.)</td>
<td>0.275</td>
<td>0.181</td>
<td>0.181</td>
<td>0.185</td>
</tr>
<tr>
<td>Correlation of person/estab. effects</td>
<td>0.275</td>
<td>0.167</td>
<td>0.264</td>
<td>0.102</td>
</tr>
<tr>
<td>Adjusted R-squared of model</td>
<td>0.501</td>
<td>0.676</td>
<td>0.918</td>
<td>0.897</td>
</tr>
<tr>
<td>Percentage of variance of log wages due to:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>person effect</td>
<td>52.1%</td>
<td>50.9%</td>
<td>59.1%</td>
<td>62.1%</td>
</tr>
<tr>
<td>establishment effect</td>
<td>20.6%</td>
<td>23.3%</td>
<td>19.7%</td>
<td>23.1%</td>
</tr>
<tr>
<td>covariance of person and estab. effects</td>
<td>18.0%</td>
<td>11.4%</td>
<td>18.0%</td>
<td>7.7%</td>
</tr>
<tr>
<td>estab. effects+covariance person and estab. effects</td>
<td>38.6%</td>
<td>34.5%</td>
<td>37.7%</td>
<td>30.9%</td>
</tr>
<tr>
<td>Number of establishments</td>
<td>1,284,740</td>
<td>717,098</td>
<td>1,162,373</td>
<td>508,088</td>
</tr>
<tr>
<td>Number of movers</td>
<td>4,052,299</td>
<td>1,771,840</td>
<td>2,645,495</td>
<td>930,306</td>
</tr>
<tr>
<td>Number of person-year observations</td>
<td>39,661,514</td>
<td>16,605,062</td>
<td>27,014,349</td>
<td>8,900,093</td>
</tr>
<tr>
<td>B. AKM decomposition</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Std. dev. of estab effects (across person-yr obs.)</td>
<td>0.287</td>
<td>0.253</td>
<td>0.293</td>
<td>0.236</td>
</tr>
<tr>
<td>Correlation of person/estab. effects</td>
<td>0.354</td>
<td>0.261</td>
<td>0.375</td>
<td>0.260</td>
</tr>
<tr>
<td>Percentage of variance of log wages due to:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>establishment effect</td>
<td>16.3%</td>
<td>19.3%</td>
<td>17.2%</td>
<td>17.2%</td>
</tr>
<tr>
<td>covariance of person and estab. effects</td>
<td>21.3%</td>
<td>15.7%</td>
<td>23.0%</td>
<td>16.2%</td>
</tr>
<tr>
<td>estab. effects+covariance person and estab. effects</td>
<td>39.6%</td>
<td>35.0%</td>
<td>40.2%</td>
<td>33.4%</td>
</tr>
<tr>
<td>C. KSS decomposition</td>
<td></td>
<td></td>
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</tr>
<tr>
<td>Std. dev. of estab effects (across person-yr obs.)</td>
<td>0.271</td>
<td>0.233</td>
<td>0.273</td>
<td>0.213</td>
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<tr>
<td>Correlation of person/estab. effects</td>
<td>0.468</td>
<td>0.597</td>
<td>0.480</td>
<td>0.369</td>
</tr>
<tr>
<td>Percentage of variance of log wages due to:</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>establishment effect</td>
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<td>16.4%</td>
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<td>14.0%</td>
</tr>
<tr>
<td>covariance of person and estab. effects</td>
<td>22.8%</td>
<td>17.6%</td>
<td>24.8%</td>
<td>18.4%</td>
</tr>
<tr>
<td>estab. effects+covariance person and estab. effects</td>
<td>39.1%</td>
<td>34.0%</td>
<td>39.6%</td>
<td>32.5%</td>
</tr>
<tr>
<td>Number of establishments</td>
<td>749,977</td>
<td>325,034</td>
<td>600,499</td>
<td>173,697</td>
</tr>
<tr>
<td>Number of movers</td>
<td>3,551,977</td>
<td>1,423,252</td>
<td>2,328,539</td>
<td>645,146</td>
</tr>
<tr>
<td>Number of person-year observations</td>
<td>22,305,141</td>
<td>8,761,529</td>
<td>13,972,235</td>
<td>3,708,699</td>
</tr>
</tbody>
</table>

Bias corrections to variance shares small w/ 12 years of data
But correction to worker-estab correlation is substantial..
Race gap in estab effs most important for coll educated

Bars give gap between whites and non-whites. Percentages are portion of overall gap attributable to firm components.
How stable are firm effects?

Answer using admin data from Washington state (2002-2014)

- Administrative hours records allow computation of hourly wage
- Secular increase in inequality + Great Recession make for an interesting test environment

Fit AKM to rolling two year windows of administrative ("TV-AKM")

- Bias correct variance components ala KSS
- Compute autocorrelation of firm effects across windows
Secular increase in log wage variance
Mostly explained by increase in variance of person effects
Firm effects highly persistent ($\rho \approx .98$)

Figure 7: Autocorrelation of Firm Effects for Wages
Little bias from imposing constant effects

Figure 9: Does Allowing for Time-Varying Firm Heterogeneity Actually Matter?

(a) TV-AKM vs. AKM

Plug-in slope: .98
KSS adjusted slope: .99
Constant effects also predict separations equally well.

Regression slope AKM: -0.13
Regression slope for TV-AKM: -0.11
KSS adjusted slope AKM: -0.13
KSS adjusted slope for TV-AKM: -0.1
Summary

- Statistical firm wage effects temporally stable and correlate strongly with worker retention and productivity
- But not all workers share equally in firm effects
- And “fissuring” the firm via outsourcing leads to wage losses largely explained by firm effects
- Next lecture: What do firm effects tell us about how labor markets actually function?
References


Monopsony Overview

- Joan Robinson (1933) proposed theory of monopsony in *The Economics of Imperfect Competition*.
- Card, Cardoso, Heining, Kline (2018) contrast perspective of IO literature with that of labor literature:

> Although economists seem to agree that part of the variation in the prices of cars and breakfast cereal is due to factors other than marginal cost, the notion that wages differ substantially among equally skilled workers remains highly controversial.
Borrow from IO literature on differentiated product markets
  ▶ Basic idea: firms imperfect substitutes in eyes of workers
  ▶ Endows firm with power to set wages

Study conditions under which stable firm effects arise along with their interpretation

Link to older empirical literature on rent sharing
Setup

- Two observable worker types $S \in \{L, H\}$ w/ corresponding market supplies ($\mathcal{L}, \mathcal{H}$)
- $J$ firms, differentiated vertically by amenities $a_{Sj}$
- Worker-firm pairings yield match effects $\{\epsilon_{iSj}\}$ that are private information to workers

Indirect utility of working at firm $j$ for skill type $S$:

$$u_{iSj} = \beta_S \ln (w_{Sj} - b_S) + a_{Sj} + \epsilon_{iSj}$$

Here $b_S$ is a type-specific reservation wage / outside option

- Analogous to Stone-Geary min consumption level
- Will not work for less, no matter amenity level.
Labor supply to firm

Assuming $\epsilon_{iSj} \sim EVI$ we have LS curves

$$\ln L_j (w_{Lj}) = \ln (L \lambda_L) + \beta_L \ln (w_{Lj} - b_L) + a_{Lj}$$
$$\ln H_j (w_{Hj}) = \ln (H \lambda_H) + \beta_H \ln (w_{Hj} - b_H) + a_{Hj}$$

where $\lambda_S \equiv \sum_{k=1}^{J} \exp (\beta_S \ln (w_{Sk} - b_S) + a_{Sk})$
Labor supply to firm

Assuming $iS_j \sim EVI$ we have LS curves

$$
\ln L_j(w_{Lj}) = \ln (L\lambda_L) + \beta_L \ln (w_{Lj} - b_L) + a_{Lj}
$$

$$
\ln H_j(w_{Hj}) = \ln (H\lambda_H) + \beta_H \ln (w_{Hj} - b_H) + a_{Hj}
$$

where $\lambda_S \equiv \sum_{k=1}^{J} \exp (\beta_S \ln (w_{Sk} - b_S) + a_{Sk})$

Supposing $J$ is large can approximate $\lambda_S$ as constant
Labor supply to firm

Assuming $\epsilon_{iSj} \sim EVI$ we have LS curves

$$
\ln L_j (w_{Lj}) = \ln (L \lambda_L) + \beta_L \ln (w_{Lj} - b_L) + a_{Lj}
$$
$$
\ln H_j (w_{Hj}) = \ln (H \lambda_H) + \beta_H \ln (w_{Hj} - b_H) + a_{Hj}
$$

where $\lambda_S \equiv \sum_{k=1}^{J} \exp (\beta_S \ln (w_{Sk} - b_S) + a_{Sk})$

Supposing $J$ is large can approximate $\lambda_S$ as constant

Approximation yields variable LS elasticity:

$$
e_{Sj} = \beta_S \cdot \frac{w_{Sj}}{w_{Sj} - b_S}
$$

- Decreasing in $w_{Sj}$ (infinite at $w_{Sj} = b_S$)
- Approach competitive model as $\beta_S \rightarrow \infty$
Firm’s problem

Firm $j$’s output given by

$$Y_j = T_j f (L_j, H_j)$$

where $T_j$ is TFPQ and $f (., .)$ is CRTS fn
Firm’s problem

Firm \( j \)'s output given by

\[ Y_j = T_j f (L_j, H_j) \]

where \( T_j \) is TFPQ and \( f (., .) \) is CRTS fn

Choose wages to minimize costs subject to output \( \geq Y \)

\[
\min_{w_{Lj}, w_{Hj}} w_{Lj} L_j (w_{Lj}) + w_{Hj} H_j (w_{Hj})
\]

\[
\text{s.t. } T_j f (L_j (w_{Lj}), H_j (w_{Hj})) \geq Y
\]

- Firm wage discriminates on \( S \) but not \( \epsilon_{iSj} \) (2nd degree)
- Large market approximation: ignores effect of wage choice on behavior of other firms
Wage rule

FOC yields monopsony “markdown” rule (Robinson, 1933):

\[ w_{Sj} = \frac{e_{Sj}}{1 + e_{Sj}} \frac{T_j f_S \mu_j}{\text{MRPL}} \]

\[ \text{exploitation} \]

- \( \mu_j \) is Lagrange multiplier on output constraint
- Firm will choose output to equate MC \( (\mu_j) \) with MR
- Wage sets MFC \( \left( \frac{1 + e_{Sj}}{e_{Sj}} \right) w_{Sj} \) equal to MRPL
Wage rule

FOC yields monopsony “markdown” rule (Robinson, 1933):

\[ w_{Sj} = \frac{e_{Sj}}{1 + e_{Sj}} \left( T_j f_{Sj} \mu_j \right) \]

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\[ \text{MRPL} \]

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- Firm will choose output to equate MC \((\mu_j)\) with MR
- Wage sets MFC \(\left( \frac{1+e_{Sj}}{e_{Sj}} \right) w_{Sj}\) equal to MRPL

Using \( e_{Sj} = \frac{\beta_s w_{Sj}}{w_{Sj} - b_s} \) we get

\[ w_{Sj} = \frac{1}{1 + \beta_s} b_s + \frac{\beta_s}{1 + \beta_s} \text{MRPL}_j \]

- Wage is a weighted average of outside option and MRPL
- \( \frac{\beta_s}{1+\beta_s} \) analogous to Nash bargaining weight
A Baseline Case

Linear production (efficiency units)

\[ Y_j = T_j [(1 - \theta) L_j + \theta H_j] \equiv T_j N_j \]

With fixed product price \( P_j^0 \), value added per eff unit of labor is:

\[ v_j \equiv P_j^0 Y_j / N_j = P_j^0 T_j \]

Letting \( s_H = H_j L_j + H_j \), empirical studies typically use:

\[ \tilde{v}_j = P_j^0 Y_j / (L_j + H_j) = v_j [(1 - \theta) + 2 \theta s_H] \]
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Wages are linear in \( v_j \):

\[ w_{Lj} = \frac{1}{1 + \beta_L} b_L + \frac{\beta_L}{1 + \beta_L} (1 - \theta) v_j \]
\[ w_{Hj} = \frac{1}{1 + \beta_H} b_H + \frac{\beta_H}{1 + \beta_H} \theta v_j \]
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\[
\begin{align*}
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w_{Hj} &= \frac{1}{1 + \beta_H} b_H + \frac{\beta_H}{1 + \beta_H} \theta v_j
\end{align*}
\]

Letting \( s_H = \frac{H_j}{L_j + H_j} \), empirical studies typically use

\[ \tilde{v}_j = P_j^0 Y_j / (L_j + H_j) = v_j [(1 - \theta) + 2\theta s_H] \]
Elasticities

Suppose reservation wages determined by pay in “competitive fringe” sector that pays $b$ per eff unit, so that

$$b_L = (1 - \theta) \, b, \quad b_H = \theta \, b$$

Log wages become

$$\ln w_{Lj} = \ln \frac{(1 - \theta) \, b}{1 + \beta_L} + \ln (1 + \beta_L R_j)$$

$$\ln w_{Hj} = \ln \frac{\theta \, b}{1 + \beta_H} + \ln (1 + \beta_H R_j)$$

where $R_j = v_j / b$ gives ratio of $j$’s labor prod relative to competitive fringe
Elasticities

Suppose reservation wages determined by pay in “competitive fringe” sector that pays $b$ per eff unit, so that

$$b_L = (1 - \theta) b, \quad b_H = \theta b$$

Log wages become

$$\ln w_{Lj} = \ln \left( \frac{1 - \theta}{1 + \beta_L} \right) b + \ln (1 + \beta_L R_j)$$

$$\ln w_{Hj} = \ln \left( \frac{\theta b}{1 + \beta_H} \right) + \ln (1 + \beta_H R_j)$$

where $R_j = v_j / b$ gives ratio of $j$’s labor prod relative to competitive fringe

Potentially type-specific “rent sharing” elasticity

$$\xi_j \equiv \frac{d \ln w_{Sj}}{d \ln v_j} = \frac{\beta_S R_j}{1 + \beta_S R_j}$$
Three generations of rent-sharing elasticities

**Group 1: Industry-level profit measure**
Christofides-Oswald (QJE 1992), Canadian manufacturing 0.140 (0.035)
Blanchflower-Oswald-Sanfey (QJE 1996), US manufacturing 0.060 (0.024)

**Group 2: Firm-level profit measure, mean firm wage**
Abowd-Lemieux (QJE 1993), Canadian manufacturing 0.220 (0.081)
Van Reenen (QJE 1996), UK manufacturing 0.290 (0.089)
Barth-Bryson-Davis-Freeman (JOLE 2016), US 0.160 (0.002)

**Group 3: Firm-level profit measure, individual-specific wage**
Guiso-Pistaferri-Schivardi (JPE 2005), Italy 0.069 (0.025)
Card-Devicienti-Maida (ReStud 2014), Italy 0.073 (0.031)
Card-Cardoso-Kline (QJE 2014), Portugal, between firm 0.156 (0.006)
Card-Cardoso-Kline (QJE 2014), Portugal, stayers 0.049 (0.007)
Bagger-Christensen-Mortensen (mimeo), Danish manufacturing 0.090 (0.020)
A calibration

\[ \xi_j \equiv \frac{d \ln w_{Sj}}{d \ln v_j} = \frac{\beta_S R_j}{1 + \beta_S R_j} \]

Modern estimates give \( \xi_j \approx 0.1 \Rightarrow \beta_S R_j \approx 0.1 \)

Suppose \( e_S \approx 4 \) (20% markdown), then

- \( R_j \approx 1.3 \) (30% more productive than competitive fringe)
- \( \beta_S \approx 0.08 \) (workers get 8 cents of every dollar of MRP)
A link to AKM

When $\beta_L = \beta_H$, we have the AKM representation

$$\ln w_{Lj} = \ln \left( \frac{1 - \theta}{1 + \beta} \right) + \ln \left( 1 + \beta R_j \right)$$

$$\ln w_{Hj} = \ln \left( \frac{\theta b}{1 + \beta} \right) + \ln \left( 1 + \beta R_j \right)$$

For small $\beta R_j$, firm effects nearly linear in productivity $\psi_j \approx \beta R_j$

Limitations
- Firm profits derived entirely from labor market
- Amenities have no effect on $\psi_j$
A link to AKM

When $\beta_L = \beta_H$, we have the AKM representation

$$\ln w_{Lj} = \ln \left( \frac{1 - \theta}{1 + \beta} \right) + \ln (1 + \beta R_j)$$

$$\alpha_L \underbrace{+} \quad \psi_j$$

$$\ln w_{Hj} = \ln \left( \frac{\theta b}{1 + \beta} \right) + \ln (1 + \beta R_j)$$

$$\alpha_H \underbrace{+} \quad \psi_j$$

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Limitations

- Firm profits derived entirely from labor market
- Amenities have no effect on $\psi_j$
Downward sloping product demand

Suppose $P_j = P_j^0 Y_j^{-1/\varepsilon}$ where $\varepsilon > 1$ gives elasticity of demand

- Now avg labor productivity is decreasing in scale

$$v_j = \frac{P_j Y_j}{N_j} = T_j P_j = T_j P_j^0 Y_j^{-1/\varepsilon}$$

- Monopoly rents: mark $P_j$ up over $\mu_j$ by a factor $\frac{\varepsilon}{\varepsilon-1}$
Downward sloping product demand

Suppose $P_j = P_j^0 Y_j^{-1/\varepsilon}$ where $\varepsilon > 1$ gives elasticity of demand

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- Monopoly rents: mark $P_j$ up over $\mu_j$ by a factor $\frac{\varepsilon}{\varepsilon-1}$

Setting $\mu_j = (1 - \frac{1}{\varepsilon}) P_j$ we get

$$w_{Lj} = \frac{b (1 - \theta)}{1 + \beta_L} \left[ 1 + \beta_L \left( \frac{\varepsilon - 1}{\varepsilon} \right) v_j / b \right]$$

$$w_{Hj} = \frac{b \theta}{1 + \beta_H} \left[ 1 + \beta_H \left( \frac{\varepsilon - 1}{\varepsilon} \right) v_j / b \right]$$

- $(\frac{\varepsilon-1}{\varepsilon})$ converts avg to marginal labor productivity

- Amenities affect wages indirectly through $v_j$
Suppose $\beta_H = \beta_L = \beta$ and take logs to get

$$
\ln w_{Lj} = \ln \left( \frac{b (1 - \theta)}{1 + \beta} \right) + \ln \left[ 1 + \beta R'_j \right] \\
\quad + \ln \left( \frac{1}{\alpha_L} \right) \\
\ln w_{Hj} = \ln \left( \frac{b \theta}{1 + \beta} \right) + \ln \left[ 1 + \beta R'_j \right] \\
\quad + \ln \left( \frac{1}{\alpha_H} \right)
$$

- $R'_j \equiv \left( \frac{\varepsilon - 1}{\varepsilon} \right) v_j / b$ is ratio of marginal labor productivity to productivity in competitive fringe.
- Firm effects explainable by labor productivity b/c amenities only shift intercept (rather than slope) of LS curve.
Rent sharing

Wage elasticity wrt value added is:

\[
\xi_{sj} = \frac{d \ln w_{sj}}{d \ln v_j} = \frac{\beta_s R'_j}{1 + \beta_s R'_j}
\]
Rent sharing

Wage elasticity wrt value added is:

$$\xi_{Sj} = \frac{d \ln w_{Sj}}{d \ln v_j} = \frac{\beta_s R'_j}{1 + \beta_s R'_j}$$

Letting $$m_j \equiv \frac{d \ln N_j}{d \ln v_j}$$, we expect somewhat smaller wage responses to TFPQ shocks than to TFPR

$$\frac{d \ln w_{Sj}}{d \ln P^0_j} = \frac{\varepsilon}{\varepsilon + m_j} \xi_{Sj}$$

$$\frac{d \ln w_{Sj}}{d \ln T_j} = \frac{\varepsilon - 1}{\varepsilon + m_j} \xi_{Sj}$$
A simplified example

- Suppose a single labor type $L$ of measure 1
- Set $b = 0$ so that LS exhibits constant elasticity $\beta$
A simplified example

- Suppose a single labor type $L$ of measure 1
- Set $b = 0$ so that LS exhibits constant elasticity $\beta$
- Production is $Y_j = T_j L_j = T_j \exp(\beta \ln w_j + a_j)$

The corresponding wage rule is:

$$w_j = \frac{\beta}{1 + \beta} MRP = \frac{\beta}{1 + \beta} \frac{\varepsilon - 1}{\varepsilon} T_j P_j^0 Y_j^{-1/\varepsilon}$$
A simplified example

- Suppose a single labor type $L$ of measure $1$
- Set $b = 0$ so that LS exhibits constant elasticity $\beta$
- Production is $Y_j = T_j L_j = T_j \exp (\beta \ln w_j + a_j)$

Corresponding wage rule is:

$$w_j = \frac{\beta}{1 + \beta} MRP = \frac{\beta}{1 + \beta} \frac{\varepsilon - 1}{\varepsilon} T_j P_j^0 Y_j^{-1/\varepsilon}$$

Solve out for reduced form

$$\ln w_j = \ln \left[ \frac{\beta}{1 + \beta} \frac{\varepsilon - 1}{\varepsilon} \right] + \ln P_j^0 T_j - \frac{1}{\varepsilon} \ln Y_j$$

$$= \ln \left[ \frac{\beta}{1 + \beta} \frac{\varepsilon - 1}{\varepsilon} \right] + \ln P_j^0 T_j - \frac{1}{\varepsilon} [\ln T_j + \beta \ln w_j + a_j]$$

$$= \text{constant} + \frac{\varepsilon}{\varepsilon + \beta} \ln P_j^0 + \frac{\varepsilon - 1}{\varepsilon + \beta} \ln T_j - \frac{1}{\varepsilon + \beta} a_j$$

prod demand TFPQ amenities
Interpretation: supply and demand at the firm level

Fig. 9.—Effect of total factor productivity shock (single skill group). MFC = marginal factor cost. A color version of this figure is available online.

Note: $S$ refers to $L$ on previous slide
Summary

Simple “differentiated workplaces” foundation for monopsony easily adapted to many empirical settings

- Forges a link between AKM effects and pass through of productivity shocks to wages
- Microfoundation for firm level supply-demand analysis/study of rents

Extensions

- Imperfect substitution/task assignment at firm level (Haanwinckel, 2018; Lindner et al., 2019)
- Interactions with min wage/other institutions (Haanwinckel, 2018; Berger, Herkenhoff, Mongey, 2019)
Summary

Simple “differentiated workplaces” foundation for monopsony easily adapted to many empirical settings

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Extensions

- Imperfect substitution / task assignment at firm level (Haanwinckel, 2018; Lindner et al, 2019)
- Interactions with min wage / other institutions (Haanwinckel, 2018; Berger, Herkenhoff, Mongey, 2019)
Azar, Berry, Marinescu (2019)

Fit differentiated workplace model of LS to online job postings from CareerBuilder.com

- Follow closely standard approaches in empirical IO (e.g., Berry, 1994, BLP, 1996)

Advantages of studying CB

- Posted wages
- Observed application behavior (instead of just realized matches)
- Low search costs on platform

Challenges:

- How to convert application elasticities to LS elasticities?
- Finding exogeneous variation in wages
Nested logit model

Break job vacancies into markets $m$ defined by occupation by geography cells (SOC-6 × CZ)

Indirect utility of worker $i$ applying to job vacancy $j \in J_{mt}$ in market $m$ in week $t$ is:

$$u_{ijmt} = \delta_j + \gamma_m z_{ijm} + \theta_m \tilde{z}_{im} + \nu_{imt} (\lambda_m) + \lambda_m \epsilon_{ijmt}$$

- $\delta_j$ - “mean utility” of job $j$ (treat as fixed effect)
- $z_{ijm}$ - log distance of $i$ to job $j$
- $\tilde{z}_{im}$ - indicator for $i$ in same CZ as $j$
- $\nu_{imt} (\lambda_m)$ - market random effect with scale parameter $\lambda_m$
- $\epsilon_{ijmt}$ - idiosyncratic match $\sim$ EV1
- Outside option: don’t apply ($j = 0$)
Mean utility

Mean utilities obey:

\[ \delta_j = \beta x_j - \alpha \ln w_j + \xi_j \]

- \( x_j \) - job characteristics
- \( w_j \) - posted wage
- \( \xi_j \) - unobserved job “quality”

\( \text{Cov} (\ln w_j, \xi_j) > 0 \Rightarrow \text{omitted variable bias} \)

Ideal instruments for \( \ln w_j \):

- productivity shock
- change in market structure (e.g., merger / outsourcing event)
Nested logit estimation via two-step

Bottom level: choosing jobs within a market

Probability of applying to job $j$ conditional on choosing at least 1 job in market $m$

$$s_{ijmt} = \frac{\exp \left( \frac{\delta_j + \gamma_m z_{ijm}}{\lambda_m} \right)}{\sum_{k \in J_{mt}} \exp \left( \frac{\delta_k + \gamma_m z_{ikm}}{\lambda_m} \right)}$$

Can be estimated via conventional alternative specific logit using within market data

Yields scaled mean utilities $\delta_j/\lambda_m$
Recovering the scale parameter

Top level: which (if any) market to enter

Probability of applying to market $m$ is

$$s_{imt} = \frac{\exp (\theta_m \tilde{z}_{im} + \lambda_m I_{imt})}{1 + \exp (\theta_m \tilde{z}_{im} + \lambda_m I_{imt})}.$$

Estimate via another logit. Recover scale parameters $\lambda_m$. Use to form estimates $\hat{\delta}_j$ of $\delta_j$. 
Final step: IV

Explore two sets of instruments for $\ln w_j$ in final equation

$$\hat{\delta}_j = \beta x_j - \alpha \ln w_j + \xi_j + \text{noise}$$

“BLP instruments”: $\#$ of vacancies in market / size of other firms in market

“Hausman instruments”: wages paid by same firm in other markets

▶ Problem: what if firm wage in other markets reflects unobserved amenities?

▶ Solution: use predicted wage in other markets (based on CZ-SOC fixed effects + job title fixed effects)

▶ Intuition: firms that face stiffer competition in other markets it also pays higher wages in this market
Instrumenting flips the sign of wage

But parameter estimates somewhat sensitive to instrument set

<table>
<thead>
<tr>
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<th>OLS</th>
<th>IV: Number of Vacancies</th>
<th>IV: BLP Instruments</th>
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<td>33.69</td>
<td>13.35</td>
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</table>

Note: Vacancy level elasticity > firm level > market level
Hausman instrument somewhat yield lower elasticities

<table>
<thead>
<tr>
<th></th>
<th>IV: Average Wage of Same Firm in Other Markets</th>
<th>IV: Average Wage of Same Firm in Other Markets (Excluding Same CZ and Same SOC)</th>
<th>IV: Average Predicted Wage of Same Firm in Other Markets</th>
<th>IV: Average Predicted Wage of Same Firm in Other Markets (Excluding Same CZ and Same SOC)</th>
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<tr>
<td>Job Title FE</td>
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<td>Median Market-Level Elasticity</td>
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<td>1.035</td>
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<td>1.058</td>
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<tr>
<td>Kleibergen-Paap F-stat</td>
<td>508.3</td>
<td>146.3</td>
<td>186.7</td>
<td>52.92</td>
</tr>
</tbody>
</table>

*p < 0.1, **p < 0.05, ***p < 0.01
Lots of heterogeneity across occupations

Nurses and truckers are OCCs that have long been suspected of being monopsonistic (Rose, 1987; Staiger, Spetz, Phibbs, 2010)
Application elasticities to LS elasticities

Definitions:

- $R(w)$ is the flow of new recruits as a function of wage
- $s(w)$ is the separation rate

Steady state: $s(w)N(w) = R(w) \Rightarrow N(w) = \frac{R(w)}{s(w)}$
Application elasticities to LS elasticities

Definitions:

- \( R(\omega) \) is the flow of new recruits as a function of wage
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Steady state: \( s(\omega) N(\omega) = R(\omega) \implies N(\omega) = \frac{R(\omega)}{s(\omega)} \)

SS elasticity of LS: \( \epsilon = \frac{d \ln N}{d \ln \omega} = \epsilon_R - \epsilon_s \).
Application elasticities to LS elasticities

Definitions:
- \( R(w) \) is the flow of new recruits as a function of wage
- \( s(w) \) is the separation rate

Steady state: \( s(w) N(w) = R(w) \Rightarrow N(w) = R(w)/s(w) \)

SS elasticity of LS: \( \epsilon = \frac{d \ln N}{d \ln w} = \epsilon_R - \epsilon_s. \)

Two (strong) assumptions:
1. app elasticity \( \approx \epsilon_R \)
2. \( \epsilon_R \approx \epsilon_s \)

\((1) + (2) \Rightarrow \epsilon \approx 2 \times \text{app elasticity}\)
Summary

Standard IO tools can be applied to study labor market competition

- Results somewhat sensitive to instrument set
- Doubling the app elasticity is a crude way to assess the full LS elasticity

Extensions:

- How best to define labor markets? (Manning and Petrongolo 2017; Nimczik, 2017; Caldwell and Daniele, 2018)
- Direct evidence (e.g., mergers / firm entry) on effects of changes in market structure (Arnold, 2019; Manelici and Vasquez, 2019)
How much market power do employers have on online labor markets?

Study relationship between reward and availability for Mturk tasks to estimate labor supply curve

- use double machine learning (DML) procedure of Chernozhukov et al (2018) to infer causality
- validate with experiments

Main result: labor supply elasticity to “requester” is very low
A toy model

Requester posts batch of $N$ jobs with private value $p$ that need to be completed in time interval $[0, T]$.

- A fraction $\lambda$ of users see the request
- Distribution of reservation wages is $F(w)$

Requester chooses a wage to maximize

$$\Pi(w) = \int_0^T e^{-rt} N(w, t) (p - w) F(w) \lambda dt$$

where $N(w, t)$ is the stock of unfilled jobs, which evolves according to

$$\dot{N}(w, t) = -\lambda F(w) N(w, t).$$
Duration elasticity

\[ \dot{N}(w, t) = -\lambda F(w) N(w, t). \]

With constant fill rate \( \lambda F(w) \), expected duration to fill \( N \) jobs (ignoring censoring at \( T \)) is

\[ \bar{d} = \frac{N}{\lambda F(w)} \]

Imposing \( F(w) \propto w^\eta \), we have

\[ \ln \bar{d} = \ln N - \ln \lambda F(w) \]
\[ \propto \ln N - \ln \lambda - \eta \ln w \]
Quasi-static interpretation

For short $T$, effective LS curve $L(w)$ is

$$L(w) \propto F(w)$$

Elasticity of labor supply equals duration elasticity

$$\frac{d \ln L}{d \ln w} = \frac{d \ln F}{d \ln w} = \frac{d \ln \bar{d}}{d \ln w} = \eta.$$
Econometric framework

MTurk data consist of a series of scraped human input task batches (HITs). Relationship of interest is:

\[
\ln (duration_h) = -\eta \ln (reward_h) + \nu_h + \epsilon_h
\]

- \(duration_h\) is the time it took for the HIT to disappear from Mturk
- \(reward_h\) is the payment for completing the HIT
- \(\nu_h\) confounders

\(\eta\) is duration elasticity

- Frictionless competitive model \(\eta = \infty\)
- Is this a reasonable benchmark?
Panel data estimator

\[ \ln (\text{duration}_h) = -\eta \ln (\text{reward}_h) + \nu_h + \epsilon_h \]

Fixed effects for confounders

\[ \nu_h = \rho_{r(h)} + \tau_{t(h)} + \delta_{d(h)} + \delta_{N(h)} \]

Estimate \( \eta \) by OLS
DML estimator

Partially linear model:

\[
\ln (duration_h) = -\eta \ln (reward_h) + g_0 (Z_h) + \epsilon_h \\
\ln (reward_h) = m_0 (Z_h) + \mu_h 
\]

where \(Z_h\) is high dimensional vector of HIT features.

1. Estimate first stage function \(m_0 (Z_h)\) and reduced form

\[
l_0 (Z_h) = \mathbb{E} [\ln (duration_h) | Z_h] = g_0 (Z_h) + m_0 (Z_h) 
\]

via random forest procedure utilizing classification trees (Breiman, 2001).

2. Form resids:

\[
\hat{\xi}_h = \ln (duration_h) - \hat{l}_0 (Z_h) \\
\hat{\mu}_h = \ln (reward_h) - \hat{m}_0 (Z_h) 
\]
DML estimator

Frisch-Waugh style estimator of $\eta$ based on residuals:

$$
\hat{\eta} = \left( \sum_h \hat{\mu}_h^2 \right)^{-1} \sum_h \hat{\xi}_h \hat{\mu}_h
$$

- Problem: model selection errors in $\hat{\xi}_h$ and $\hat{\mu}_h$ could be correlated, amplifying regularization bias
- Solution: split sample to obtain independent $\hat{\xi}_h^{(1)}$ and $\hat{\mu}_h^{(2)}$

Chernozhukov et al (2018): high-level conditions under which sample splitting ensures $\hat{\eta} \overset{P}{\to} \eta$

- Tricky to verify these conditions
- Depends on (unknown) “sparsity” of DGP
### Table 1—Duration Elasticities from Observational MTurk Data

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
</tr>
</thead>
<tbody>
<tr>
<td>log reward</td>
<td>0.186</td>
<td>-0.0600</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.0947)</td>
<td>(0.0585)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>log reward-ML res.</td>
<td></td>
<td>-0.0958</td>
<td>-0.0787</td>
<td>-0.198</td>
<td>-0.181</td>
<td>-0.0299</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.00558)</td>
<td>(0.00651)</td>
<td>(0.0281)</td>
<td>(0.0161)</td>
<td>(0.00402)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>644,873</td>
<td>629,756</td>
<td>644,873</td>
<td>629,756</td>
<td>93,775</td>
<td>292,746</td>
<td>258,352</td>
</tr>
<tr>
<td>Clusters</td>
<td>41,167</td>
<td>26,050</td>
<td>41,167</td>
<td>26,050</td>
<td>6,962</td>
<td>18,340</td>
<td>24,923</td>
</tr>
<tr>
<td>Type</td>
<td>OLS</td>
<td>FE</td>
<td>ML</td>
<td>ML–FE</td>
<td>ML</td>
<td>ML</td>
<td>ML</td>
</tr>
</tbody>
</table>

Notes: This table presents $\eta$ estimates using data scraped from MTurk. Units are HIT batches. Column 1 presents the unadjusted coefficient from a bivariate regression of log duration on log reward. Column 2 estimates the specification in equation (2). Column 3 presents estimates from an OLS regression of the residualized log duration on the residualized log reward, as in equation (5) averaged across the two sample splits. Column 4 adds the fixed effects in column 2 as further controls to column 3. Columns 5–7 present the double ML estimate from different scraped subsamples. Standard errors are clustered at the requester level.
Experiments

Retention experiments
▶ Hire workers for a translation tasks at a common wage
▶ Then ask if they want to do the task again at an experimentally manipulated wage
▶ Get retention probability elasticity

Recruitment experiments
▶ Offer to hire workers to perform a new task at manipulated wage
▶ Get recruitment probability elasticity
Retention elasticities centered around 0.1

<table>
<thead>
<tr>
<th>Panel A. Horton et al. (2011) probability of accepting offer</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reward</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>SE</td>
</tr>
<tr>
<td>Reward</td>
</tr>
<tr>
<td>(0.0219)</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Panel B. Dube et al. (2017) probability of accepting offer</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reward</td>
</tr>
<tr>
<td></td>
</tr>
<tr>
<td>Observations</td>
</tr>
<tr>
<td>SE</td>
</tr>
<tr>
<td>Reward</td>
</tr>
<tr>
<td>(0.0171)</td>
</tr>
</tbody>
</table>

Notes: Coefficients from equation (6) from “retention” experiments, and calculated elasticities, assessed at the specification sample mean. Units are individual workers. Robust standard errors in parentheses.


Recruitment elasticities <0.1

**Table 3—Recruitment Elasticities from Three Experiments**

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Reward</td>
<td>0.00186</td>
<td>0.0451</td>
<td>0.0287</td>
<td>0.00744</td>
</tr>
<tr>
<td></td>
<td>(0.00188)</td>
<td>(0.0587)</td>
<td>(0.0104)</td>
<td>(0.00385)</td>
</tr>
<tr>
<td>Observations</td>
<td>600</td>
<td>1,800</td>
<td>338</td>
<td>2,738</td>
</tr>
<tr>
<td>$\eta$</td>
<td>0.0497</td>
<td>0.0724</td>
<td>0.115</td>
<td>0.0610</td>
</tr>
<tr>
<td>SE</td>
<td>0.0503</td>
<td>0.0944</td>
<td>0.0417</td>
<td>0.0290</td>
</tr>
<tr>
<td>Average reward</td>
<td>83.33</td>
<td>4</td>
<td>10.04</td>
<td>22.13</td>
</tr>
<tr>
<td>Experiment</td>
<td>Spot diff.</td>
<td>Classify reviews</td>
<td>Brainstorming</td>
<td>Pooled</td>
</tr>
</tbody>
</table>

*Notes:* Coefficients from equation (6) estimated from “recruitment” experiments, and calculated elasticities, assessed at the experimental sample mean. Units are individual workers. The pooled specification includes experiment fixed effects, and is weighted by the inverse of the standard deviation of rewards within each experiment. Robust standard errors in parentheses.
Summary

Even in a thick labor market, various measures of labor supply to the firm appear inelastic in the short run.

Requesters that are in a hurry should (and probably do) pay higher wages that are still below their private valuations.

How different would the reward distribution be if requesters were required to be price takers?

- Would a minimum wage reduce efficiency here?
- What if there were a separate minimum wage for "urgent" projects?
- How would the rewards distribution change if employers bid on workers?
Employment prospects of nurses closely tied to local hospitals

Are RN wages suppressed below MPL?

Test for strategic dependence in wage setting (oligopsony)
- Nurse Pay Act of 1990: VA hospitals switch from national wage scale to matching local competitors
- Initial degree of under / over-payment provides an IV for VA wage
- See if non-VA hospitals respond or are price takers
VAs that underpaid experience large boost

Fig. 1.—Difference between the market wage and the VA wage in 1990 and its association with the change in the VA wage from 1990 to 1992. Each point represents data for a single VA hospital in our sample, with the simple regression line for these data also displayed.
Table 3

<table>
<thead>
<tr>
<th>Independent Variables</th>
<th>VA Only (1)</th>
<th>Non-VA Only (2)</th>
<th>Non-VA Only (3)</th>
<th>Non-VA Only (4)</th>
<th>Non-VA Only (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wage gap at nearest VA in 1990 (log market wage – log (VA wage))</td>
<td>.830 (.055)</td>
<td>.090 (.034)</td>
<td>.161 (.061)</td>
<td>.345 (.067)</td>
<td>.344 (.065)</td>
</tr>
<tr>
<td>Wage gap at nearest VA in 1990 × dummy if &gt; 15 miles to VA</td>
<td>-.109 (.075)</td>
<td>-.154 (.072)</td>
<td>-.146 (.071)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wage gap at nearest VA in 1990 × dummy if &gt; 30 miles to VA</td>
<td>-.033 (.064)</td>
<td>-.112 (.091)</td>
<td>-.120 (.091)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dummy if &gt; 15 miles to VA</td>
<td></td>
<td>-.008 (.006)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Dummy if &gt; 30 miles to VA</td>
<td></td>
<td>.000 (.008)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>MSA dummies?</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>$R^2$</td>
<td>.559</td>
<td>.011</td>
<td>.017</td>
<td>.281</td>
<td>.282</td>
</tr>
<tr>
<td>No. of observations</td>
<td>155</td>
<td>1,179</td>
<td>1,179</td>
<td>1,179</td>
<td>1,179</td>
</tr>
</tbody>
</table>

VA wage gap strongly predicts non-VA wage growth
Table 5
Two-Stage Least Squares Estimates of RN Labor Supply Elasticities

<table>
<thead>
<tr>
<th>Independent Variables</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Change in the log wage gap between hospital and its two nearest competitors</td>
<td>.076</td>
<td>.080</td>
<td>.016</td>
<td>.185</td>
<td>.185</td>
<td>.127</td>
</tr>
<tr>
<td></td>
<td>(.137)</td>
<td>(.133)</td>
<td>(.177)</td>
<td>(.138)</td>
<td>(.135)</td>
<td>(.185)</td>
</tr>
<tr>
<td>Dummy if VA hospital</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(.014)</td>
<td></td>
<td></td>
<td>(.014)</td>
</tr>
<tr>
<td>MSA dummies?</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>“FAR” instruments included?</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>“GAP” instruments used?</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>p-value for test of the over-identifying restrictions</td>
<td>.71</td>
<td>.45</td>
<td>.31</td>
<td>.20</td>
<td>.20</td>
<td>.12</td>
</tr>
<tr>
<td>No. of observations</td>
<td>1,334</td>
<td>1,334</td>
<td>1,334</td>
<td>1,334</td>
<td>1,334</td>
<td>1,334</td>
</tr>
</tbody>
</table>

Implied LS to non-VA hospitals very low
Summary

Strong evidence of strategic dependence in wage setting

Implied LS elasticity to hospitals $\sim 0.1$

- But are wages really set according to exploitation index?
- How to distinguish from “collusion”? 

Ongoing work

- Spillovers from company-specific min wages (Derenoncourt, Noelke, Weil, 2020)
- Spillovers from actual min wage (Haanwinckel, 2018)
- Links between concentration and wage setting (Berger, Herkenhoff, Mongey, 2019; Arnold, 2019)
Kline, Petkova, Williams, Zidar (2019)

Study effect of winning a patent on firm productivity and wages using treasury tax files

Patents are designed to provide firms w/ temporary monopoly rights: are monopoly rents shared w/ workers?
  ▶ Patent grants a truly firm-specific shock
  ▶ Competitive benchmark: wages shouldn’t adjust

1st time patenting firms are small (median firm size = 17)
  ▶ Unlikely to have much market power over new hires
  ▶ But potentially have power over incumbents

Main findings:
  ▶ Patents raise productivity
  ▶ And wages of incumbent workers
  ▶ But not entry wages
Obtaining a US patent (crash course)

Discover a novel, non-obvious, useful idea

Submit application to USPTO central office ("filing date")
  ▶ Central office routes application to the supervisory patent examiner (SPE) of the appropriate art unit
  ▶ SPE assigns application to a patent examiner

Examiner issues an initial decision ("initial decision date")
  ▶ Allowance (roughly 10% of initial decisions) or "rejection"
  ▶ "Rejection" is a revise & resubmit
  ▶ Applicant and examiner may engage in many rounds of revision
Research design

Two valuable patent applications submitted by two separate firms to the USPTO in the same year
Research design

Two valuable patent applications submitted by two separate firms to the USPTO in the same year

They are routed to the same art unit
Research design

Two valuable patent applications submitted by two separate firms to the USPTO in the same year.

They are routed to the same art unit.

One is initially allowed and the other is not.
Research design

Two valuable patent applications submitted by two separate firms to the USPTO in the same year

They are routed to the same art unit

One is initially allowed and the other is not

Assume parallel trends for initially allowed/rejected patents (DiD)

- Validate w/ event studies + balance tests + low-value patents
Problem: Many patents worthless

Solution: predict ex-ante value using app characteristics

Kogan, Papanikolaou, Seru, and Stoffman (2017; KPSS)

Estimate excess stock return responses to patent grant announcements

Empirical bayes posterior valuations $\xi_j$ for each patent $j$

Use $\xi_j$ to identify valuable patents in a broader sample

Fit RE Poisson QML explaining $\xi_j$ in terms of firm and application characteristics that are fixed at the time of application

Extrapolate to non-public firms and to rejected applications

Very strong explanatory power ($R^2 = 0.69$)
Problem: Many patents worthless

Solution: predict ex-ante value using app characteristics

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- Estimate excess stock return responses to patent grant announcements
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Problem: Many patents worthless

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▶ Estimate excess stock return responses to patent grant announcements
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▶ Extrapolate to non-public firms and to rejected applications
▶ Very strong explanatory power ($R^2 = .69$)
## Poisson model

<table>
<thead>
<tr>
<th>Term</th>
<th>KPSS value ($\xi$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$1$ (patent family size = 1)</td>
<td>0.28 (0.06)</td>
</tr>
<tr>
<td>log(patent family size)</td>
<td>0.23 (0.04)</td>
</tr>
<tr>
<td>$1$ (number of claims = 1)</td>
<td>0.68 (0.19)</td>
</tr>
<tr>
<td>log(number of claims)</td>
<td>0.30 (0.03)</td>
</tr>
<tr>
<td>$1$ (revenue = 0)</td>
<td>1.42 (0.14)</td>
</tr>
<tr>
<td>log(revenue)</td>
<td>0.14 (0.02)</td>
</tr>
<tr>
<td>$1$ (employees = 0)</td>
<td>0.45 (0.07)</td>
</tr>
<tr>
<td>log(employees)</td>
<td>-0.01 (0.02)</td>
</tr>
<tr>
<td>application year</td>
<td>-0.03 (0.05)</td>
</tr>
<tr>
<td>(application year)$^2$</td>
<td>-0.01 (0.01)</td>
</tr>
<tr>
<td>decision year</td>
<td>0.30 (0.06)</td>
</tr>
<tr>
<td>(decision year)$^2$</td>
<td>-0.03 (0.01)</td>
</tr>
<tr>
<td>constant</td>
<td>-1.40 (0.21)</td>
</tr>
<tr>
<td>log($\sigma$)</td>
<td>0.24 (0.05)</td>
</tr>
</tbody>
</table>

| N     | 596 |
| # groups | 260 |

*Notes:* Random effects are by art unit. Standard errors are in parentheses.
Predicted vs. actual patent value

Notes: The fitted ξ values on the x-axis are obtained from a Poisson model of ξ on the DWPI count of unique countries where the application was filed, the number of claims in the application, the application year, the initial decision year, the revenue of the firm in the year of application, the number of employees in the application year, and art unit random effects.
Event study: Surplus (EBITD + W2) per worker

Notes: Two-way standard errors are clustered by (1) art unit, and (2) application year by decision year. Regressions include art unit by application year by calendar year fixed effects and firm fixed effects. Values along the x-axis for the Q5 series are offset from their integer value to improve readability. Surplus is EBITD (earnings before interest, tax, and depreciation) + W2 wage bill. Q5 is quintile 5 of predicted patent value. < Q5 are the remaining four quintiles. 95% confidence intervals shown. Dotted red line is pooled DID impact for a top quintile patent application receiving an initial allowance post-decision.
Event study: Wage bill per worker

Notes: Two-way standard errors are clustered by (1) art unit, and (2) application year by decision year. Regressions include art unit by application year by calendar year fixed effects and firm fixed effects. Values along the x-axis for the Q5 series are offset from their integer value to improve readability. Q5 is quintile 5 of predicted patent value. < Q5 are the remaining four quintiles. 95% confidence intervals shown. Dotted red line is pooled DID impact for a top quintile patent application receiving an initial allowance post-decision.
Within firm inequality

- Gender:
  - Male earnings
  - Female earnings

- Inventors:
  - Inventor earnings
  - Non-inventor earnings

- Non-inventors:
  - Male earnings
  - Female earnings

- Officers:
  - Officer earnings
  - Non-officer earnings

- Quartiles:
  - Q1 earnings
  - Q2 earnings
  - Q3 earnings
  - Q4 earnings

- Coefficient (1K 2014 USD per worker)
- Percent Impact
No impact on earnings of new hires.

Impacts concentrated among firm stayers.
Within-firm heterogeneity: Firm Stayers

-5 0 5 10 15 20 25

Gender:
- Male earnings
- Female earnings

Inventors:
- Inventor earnings
- Non-inventor earnings

Quartiles:
- Q1 earnings
- Q2 earnings
- Q3 earnings
- Q4 earnings

Coefficient (1K 2014 USD per worker)

Percent Impact
### TABLE VIII
**Pass-Through Estimates**

<table>
<thead>
<tr>
<th></th>
<th>Wage bill per worker</th>
<th>Avg male earnings</th>
<th>Avg noninventor earnings</th>
<th>Avg stayer earnings</th>
<th>Avg stayer earnings minus earnings in app yr</th>
<th>Avg noninventor stayer earnings</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>OLS (1) IV (2)</td>
<td>OLS (3) IV (4)</td>
<td>OLS (5) IV (6)</td>
<td>OLS (7) IV (8)</td>
<td>OLS (9) IV (10)</td>
<td>OLS (11) IV (12)</td>
</tr>
<tr>
<td><strong>Panel A: Surplus/worker</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Impact</td>
<td>0.16 (0.01)</td>
<td>0.18 (0.01)</td>
<td>0.13 (0.00)</td>
<td>0.20 (0.01)</td>
<td>0.19 (0.01)</td>
<td>0.19 (0.01)</td>
</tr>
<tr>
<td></td>
<td>(0.29 (0.12))</td>
<td>(0.53 (0.18))</td>
<td>(0.19 (0.11))</td>
<td>(0.61 (0.30))</td>
<td>(0.51 (0.27))</td>
<td>(0.16 (0.22))</td>
</tr>
<tr>
<td>Elasticity</td>
<td>0.19</td>
<td>0.35</td>
<td>0.19</td>
<td>0.25</td>
<td>0.19</td>
<td>0.18</td>
</tr>
<tr>
<td></td>
<td>(0.35)</td>
<td>(0.54)</td>
<td>(0.47)</td>
<td>(0.56)</td>
<td>(0.47)</td>
<td>(0.50)</td>
</tr>
<tr>
<td>Observations</td>
<td>103,437</td>
<td>95,004</td>
<td>100,901</td>
<td>99,558</td>
<td>99,558</td>
<td>94,909</td>
</tr>
<tr>
<td>First-stage $F$</td>
<td>12.12</td>
<td>10.60</td>
<td>9.34</td>
<td>13.38</td>
<td>13.38</td>
<td>8.93</td>
</tr>
<tr>
<td>Exogeneity</td>
<td>0.29</td>
<td>0.08</td>
<td>0.14</td>
<td>0.43</td>
<td>(0.21,1.36)</td>
<td>(0.11,1.14)</td>
</tr>
<tr>
<td>Anderson-Rubin 90% CI</td>
<td>(0.10,0.57)</td>
<td>(0.27,0.98)</td>
<td>(–0.01,0.43)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>Panel B: Value added/worker</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Impact</td>
<td>0.07</td>
<td>0.08</td>
<td>0.06</td>
<td>0.08</td>
<td>0.06</td>
<td>0.06</td>
</tr>
<tr>
<td></td>
<td>(0.00)</td>
<td>(0.19)</td>
<td>(0.15)</td>
<td>(0.08)</td>
<td>(0.49)</td>
<td>(0.41)</td>
</tr>
<tr>
<td>Elasticity</td>
<td>0.15</td>
<td>0.47</td>
<td>0.14</td>
<td>0.33</td>
<td>0.13</td>
<td>0.10</td>
</tr>
<tr>
<td></td>
<td>(0.67)</td>
<td>(0.78)</td>
<td>(0.65)</td>
<td>(0.72)</td>
<td>(0.65)</td>
<td>(0.72)</td>
</tr>
<tr>
<td>Observations</td>
<td>103,437</td>
<td>95,004</td>
<td>100,901</td>
<td>99,558</td>
<td>99,558</td>
<td>94,909</td>
</tr>
<tr>
<td>Exogeneity</td>
<td>0.17</td>
<td>0.08</td>
<td>0.35</td>
<td>0.08</td>
<td>0.11</td>
<td>0.03</td>
</tr>
<tr>
<td>Anderson-Rubin 90% CI</td>
<td>(0.07,0.61)</td>
<td>(0.15,1.03)</td>
<td>(–0.01,0.38)</td>
<td>(0.15,1.39)</td>
<td>(0.08,1.15)</td>
<td>(0.18,1.17)</td>
</tr>
</tbody>
</table>
Retention response concentrated among “top half”
Separation-wage elasticity of $\sim 1.5$

<table>
<thead>
<tr>
<th>Retention of Application Cohort</th>
<th>All (1)</th>
<th>Above median (2)</th>
<th>Men (3)</th>
<th>Women (4)</th>
<th>Noninventors (5)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Retention elasticity</td>
<td>1.22</td>
<td>1.41</td>
<td>0.80</td>
<td>1.17</td>
<td>1.31</td>
</tr>
<tr>
<td></td>
<td>(0.58)</td>
<td>(0.65)</td>
<td>(0.35)</td>
<td>(0.80)</td>
<td>(0.68)</td>
</tr>
<tr>
<td>Separation elasticity</td>
<td>-1.62</td>
<td>-2.76</td>
<td>-1.14</td>
<td>-1.73</td>
<td>-1.66</td>
</tr>
<tr>
<td>Observations</td>
<td>99,558</td>
<td>81,728</td>
<td>88,100</td>
<td>71,591</td>
<td>94,909</td>
</tr>
<tr>
<td>First-stage $F$</td>
<td>7.81</td>
<td>5.80</td>
<td>31.13</td>
<td>3.61</td>
<td>6.74</td>
</tr>
<tr>
<td>Exogeneity</td>
<td>0.034</td>
<td>0.029</td>
<td>0.041</td>
<td>0.060</td>
<td>0.047</td>
</tr>
<tr>
<td>Anderson-Rubin 90% CI</td>
<td>(0.459, 3.080)</td>
<td>(0.597, 4.091)</td>
<td>(0.283, 1.524)</td>
<td>(0.233, 8.687)</td>
<td>(0.422, 3.655)</td>
</tr>
</tbody>
</table>

*Notes.* This table reports IV estimates of the effect of increases in selected earnings measures on the retention of employees. The excluded instrument is the interaction of the top quintile of ex ante value $\xi$ category with a postdecision indicator and an indicator for the application being initially allowed. Controls include the main effect of value category interacted with a postdecision indicator and interaction of lower quintile value category with a postdecision indicator interacted with an indicator for initially allowed, firm fixed effects, and art unit by application year by calendar year fixed effects. Standard errors (reported in parentheses) are two-way clustered by (i) art unit and (ii) application year by decision year. "Separation Elasticity" is computed from the retention elasticity via a Taylor approximation. Specifically, the separation elasticity estimate is $-\frac{\hat{R}}{1-R}\hat{\epsilon}$, where $\hat{\epsilon}$ is the IV estimate of the elasticity of retentions with respect to the wage and $\hat{R}$ is the mean retention rate among firms with high ex ante value patents. "Exogeneity" reports a $p$-value for the test of the null hypothesis that IV and OLS estimators have the same probability limit. "Above median" refers to members of the application cohort who earned above that firm's median in the application year. Stayers are defined as those who were employed by the same firm in the year of application. Earnings are measured in thousands of 2014 dollars.
Rent sharing redux

Firm shocks matter for worker wages, even when firms are small

But pass through is unequal across groups

- Men get more than women
- Incumbents more than new hires
- Inventors more than non-inventors

No one model to rule them all

- CCHK model would be misleading here – wage responses not proportional to hiring responses
- Important to separate retention and recruitment margins, especially when training / hiring costs substantial
- Pay for performance?

Comparable to economy-wide studies? (e.g., Garin and Silverio, 2017; Lamadon, Mogstad, Seltzer, 2019)
Vanilla DMP model says wages \((w)\) set via Nash bargaining to divide match surplus:

\[
\underbrace{W + J}_{\text{value of match}} - \underbrace{(U + V)}_{\text{outside options}}
\]

- Implies wages sensitive to value of unemployment \(U\)
- Under continuous renegotiation should apply to both incumbent workers and new hires

Test if wages sensitive to increase in UI generosity
- Diff-in-diff using Austrian reforms to UI benefit
- Key finding: no effect on incumbent wage growth
Little effect on wage growth of incumbents

Figure 4: Nonparametric Benefit Changes and Wage Effects

Note: The figure plots reform-induced replacement rate changes and wage effects for all four reform. Observations are binned by their base year (year before the reform was enacted) earnings percentile on the x-axis. The dashed orange line indicates the wage growth that the reform would induce in the calibrated bargaining model with a wage-benefit sensitivity of 0.48. The red circles indicate the wage effects that the reform induced at the one- and two-year horizon. Section 4.2 provides more information.
Mixed evidence on wage response of new hires

Table 4: Wage Effects by Individual Labor Market Status Transition Types

<table>
<thead>
<tr>
<th>Panel A: Effects by Transition Type</th>
<th>Full Sample</th>
<th>Job Stayers</th>
<th>Recalled Workers</th>
<th>Job Movers</th>
</tr>
</thead>
<tbody>
<tr>
<td>Time Horizon</td>
<td>1-Year</td>
<td>2-Year</td>
<td>1-Year</td>
<td>2-Year</td>
</tr>
<tr>
<td>Est. Wage Effect</td>
<td>-0.014</td>
<td>-0.022</td>
<td>-0.026</td>
<td>-0.027</td>
</tr>
<tr>
<td></td>
<td>(0.016)</td>
<td>(0.030)</td>
<td>(0.013)</td>
<td>(0.021)</td>
</tr>
<tr>
<td>Base-Year Transition Rate</td>
<td></td>
<td></td>
<td>0.828</td>
<td>0.705</td>
</tr>
<tr>
<td>Mincr + Ind.-Occ. FEs</td>
<td>X</td>
<td>X</td>
<td>X</td>
<td>X</td>
</tr>
</tbody>
</table>

| Panel B: Employment-Unemployment-Employment Movers |
|---------------------------------------------------|-------------------------------------------------|
| 1-Year Earnings Effects                           | 2-Year Earnings Effects                         |
| (1) (2) (3) (4)                                   | (5) (6) (7) (8)                                 |
| Est. Wage Effect                                  | -0.372  -0.215 -0.337 -0.249                    | -0.126  -0.104 -0.064 -0.115                    |
|                                                   | (0.146) (0.140) (0.161) (0.228)                | (0.147) (0.154) (0.194) (0.237)                |
| Base-Year Transition Rate                         | 0.022  0.022 0.022 0.022                        | 0.035  0.035 0.035 0.035                        |
| Transition-Specific Controls                      | X      X      X      X                          | X      X      X      X                          |
| Mincr + Ind.-Occ. FEs                             | X      X      X      X                          | X      X      X      X                          |
| Firm FEs                                          | X      X      X      X                          | X      X      X      X                          |

- Wrong signed effect on stayers and EUE movers
- But can’t rule out positive effects on recalled workers / all job movers.
Large class of “sequential auction” models predict wages depend not just on current but also prior firm (Postel-Vinay and Robin, 2002; Bagger, Lentz, Postel-Vinay, Robin, 2016)

More general principle: outside options *at the time of hire* should affect the wage

Do firms price discriminate based on where the workers are hired from?

Examine using Italian wage records

- Records include the reason for each job separation (e.g., fired, laid off, resignation)
- Measure hiring wage as average earnings in 1st year on the job
Preliminaries: coding job transitions

Job histories of workers $i \in \{1, ..., n\}$ across job matches $m \in \{1, ..., M_i\}$.

- $Q_{im} = 1$ iff worker $i$ quits match $m$ ("EE transition")
- Destination firm is $j(i, m) \in \{1, ..., J\}$

Origin firm/state is

$$h(i, m) = \begin{cases} j(i, m - 1), & \text{if } Q_{i,m-1} = 1 \text{ and } m > 1, \\ U, & \text{if } Q_{i,m-1} = 0 \text{ and } m > 1, \\ N, & \text{if } m = 1, \end{cases}$$

- $U$ is "hired from non-employment"
- $N$ is "new labor force entrant."
Dual Wage Ladder (DWL) specification

The log *hiring* wage for worker $i$ in match $m$ is:

$$y_{im} = \alpha_i + \psi_{j(i,m)} + \lambda_{h(i,m)} + X'_{im} \delta + \varepsilon_{im}.$$ 

- Similar to AKM model for *mean* wage in a match + “origin effect” for firm/state from which worker was hired
- O/D effs capture “where you’re from” vs “where you’re at”
Dual Wage Ladder (DWL) specification

The log hiring wage for worker $i$ in match $m$ is:

$$y_{im} = \alpha_i + \psi_{j(i,m)} + \lambda_{h(i,m)} + \mathbf{X}'_{im}\delta + \varepsilon_{im}.$$ 

- Similar to AKM model for mean wage in a match + “origin effect” for firm/state from which worker was hired
- O/D effs capture “where you’re from” vs “where you’re at”

Treat $\{\alpha_i\}_{i=1}^N, \{\psi_j, \lambda_j\}_{j=1}^J$ as unrestricted fixed effects

- Note: each firm is a separate 2D type!
- SA models traditionally restrict $\psi_j = \psi(p_j), \lambda_j = \lambda(p_j)$ [PVR, 2002a,b; Cahuc et al, 2006; Bagger et al, 2016; Bagger and Lentz, 2019]
Let $\varepsilon_i = (\varepsilon_{i1}, \ldots, \varepsilon_{iM_i})'$ and $\mathcal{W}_i = \{ j(i, m), h(i, m), X_{im}, \alpha_i \}_{m=1}^{M_i}$.

We assume

$$\mathbb{E} [\varepsilon_i | \mathcal{W}_i] = 0.$$  

- Rules out selection on time-varying component present at time of hiring.
- Does not prohibit selection on $(\psi, \lambda)$
- Implied by standard SA models, which typically assume efficient mobility along stable job-ladder in $p$
Dynamics: three examples

Career Path #1: two EUE transitions \((Q_{i1} = 0, Q_{i2} = 0)\)

\[
E[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)}
\]
Dynamics: three examples

Career Path #1: two EUE transitions \((Q_{i1} = 0, Q_{i2} = 0)\)

\[
\mathbb{E}[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)}
\]

Career path #2: two EE transitions \((Q_{i1} = 1, Q_{i2} = 1)\)

\[
\mathbb{E}[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)} + \lambda_{j(i,2)} - \lambda_{j(i,1)}
\]

Observations:

▶ Path #1 yields destination based wage growth ala AKM
▶ Path #2 vs #3: wage penalty of \(\lambda_{j(i,1)} - \lambda_{j(i,1)}\) for displacement
Dynamics: three examples

Career Path #1: two EUE transitions ($Q_{i1} = 0, Q_{i2} = 0$)

$$\mathbb{E}[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)}$$

Career path #2: two EE transitions ($Q_{i1} = 1, Q_{i2} = 1$)

$$\mathbb{E}[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)} + \lambda_{j(i,2)} - \lambda_{j(i,1)}$$

Career path #3: EUE followed by EE ($Q_{i1} = 0, Q_{i2} = 1$)

$$\mathbb{E}[y_{i3} - y_{i2} \mid \mathcal{W}_i] = \psi_{j(i,3)} - \psi_{j(i,2)} + \lambda_{j(i,2)} - \lambda_U$$

Observations:

- Path #1 yields destination based wage growth ala AKM
- Path #2 vs #3: wage penalty of $\lambda_{j(i,1)} - \lambda_U$ for displacement
The PVR model

PVR show that the poaching wage $\phi$ must satisfy:

$$U(\phi(\epsilon, p, q)) = U(\epsilon q) - \kappa \int_{q}^{p} \bar{F}(x) U'(\epsilon x) \, dx$$

where $\bar{F}(x) = 1 - F(x)$ and $\kappa = \frac{\lambda_1}{\rho + \delta + \mu}$ is fn of offer arrival, discount rate, etc.
The PVR model

PVR show that the poaching wage \( \phi \) must satisfy:

\[
U(\phi(\epsilon, p, q)) = U(\epsilon q) - \kappa \int_q^p \bar{F}(x) U'(\epsilon x) \, dx
\]

where \( \bar{F}(x) = 1 - F(x) \) and \( \kappa = \frac{\lambda_1}{\rho + \delta + \mu} \) is fn of offer arrival, discount rate, etc.

If \( U(x) = \ln x \) then poaching wage can be written:

\[
\ln \phi(\epsilon, p, q) = \underbrace{\ln \epsilon}_{\text{worker type}} + \underbrace{\ln q}_{\text{poached firm type}} - \underbrace{\kappa \int_q^p \frac{\bar{F}(x)}{x} \, dx}_{\text{option val of type upgrade}}
\]

- Poaching wage is decreasing in the productivity gap between poaching and poached firms (compensating diff)
DWL representation

By Fund Thm of Calculus, option value can be written

\[ \kappa \int_q^p \frac{\bar{F}(x)}{x} dx = I(q) - I(p), \]

where

\[ I(z) \equiv \kappa \int_z^{\infty} \frac{\bar{F}(x)}{x} dx \text{ is upgrade from } z \text{ to } p_{max} \]
DWL representation

By Fund Thm of Calculus, option value can be written

\[ \kappa \int_{q}^{p} \frac{\bar{F}(x)}{x} dx = I(q) - I(p), \text{ where} \]

\[ I(z) \equiv \kappa \int_{z}^{\infty} \frac{\bar{F}(x)}{x} dx \text{ is upgrade from } z \text{ to } p_{\max} \]

Implies poaching wages obey log-linear reduced form:

\[ \ln \phi(\varepsilon, p, q) = \ln \varepsilon + I(p) + \ln q - I(q) \]

\[ = \alpha(\varepsilon) = \psi(p) = \lambda(q) \]

\[ \triangleright \psi'(p) < 0 \text{ (comp diff for expected wage growth)} \]
\[ \triangleright \lambda'(q) > 0 \text{ (tougher to poach from more productive firm)} \]
\[ \triangleright \text{Exogenous mobility: worker goes to more productive firm} \]
Properties of O/D effs

\[ \ln \phi (\varepsilon, p, q) = \ln \varepsilon + I(p) + \ln q - I(q) \]

1. Productivity identified from sum of firm’s O+D effs:

\[ \psi (p) + \lambda (p) = \ln p \]

2. O/D effs are negatively correlated across firms:

\[ \mathbb{C} (\psi (p), \lambda (p)) < 0 \]

3. Excess variance of O vs D effs:

\[ \nabla [\lambda (p)] > \nabla [\psi (p)] \]
Bagger et al (2014) extension

BF-PVR allow workers to extract a share $\beta \in [0, 1]$ of rent.
BF-PVR allow workers to extract a share $\beta \in [0, 1]$ of rent.

Optimal poaching wage becomes:

\[
\ln \phi (\epsilon, p, q, \mathcal{X}, \mathcal{E} | \beta) = \alpha(\epsilon) + g(\mathcal{X}) + \mathcal{E} \\
+ \beta \ln p + I(p | \beta) + (1 - \beta) \ln q - I(q | \beta),
\]

\[
= \psi(p) + (1 - \beta) \ln q - \lambda(q),
\]

where $\mathcal{X}$ is labor market experience, $\mathcal{E}$ is a transitory shock to worker productivity, and $I(z | \beta) = (1 - \beta)^2 \kappa \int_z^\infty \frac{\bar{F}(x)/x}{1 + \kappa \beta \bar{F}(x)} dx$ is decreasing in $z$ and $\beta$. 
Bagger et al (2014) extension

BF-PVR allow workers to extract a share $\beta \in [0, 1]$ of rent.

Optimal poaching wage becomes:

$$\ln \phi (\epsilon, p, q, \mathcal{X}, \mathcal{E} \mid \beta) = \alpha(\epsilon) + g(\mathcal{X}) + \mathcal{E} + \beta \ln p + l(p \mid \beta) + (1 - \beta) \ln q - l(q \mid \beta),$$

where $\mathcal{X}$ is labor market experience, $\mathcal{E}$ is a transitory shock to worker productivity, and $l(z \mid \beta) = (1 - \beta)^2 \kappa \int_{z}^{\infty} \frac{\bar{F}(x)/x}{1 + \kappa \beta \bar{F}(x)} dx$ is decreasing in $z$ and $\beta$.

Observe that:

- As $\beta \to 0$, BF-PVR $\to$ PVR
- As $\beta \to 1$, BF-PVR $\to$ AKM! (no origin effs)
O/D effs in BF-PVR

\[
\ln \phi (\epsilon, p, q, X, \mathcal{E} \mid \beta) = \alpha(\epsilon) + g(X) + \mathcal{E} + \beta \ln p + I(p \mid \beta) + (1 - \beta) \ln q - I(q \mid \beta) = \psi(p) + \lambda(q)
\]

- Productivity identified by \( \psi(p) + \lambda(p) = \ln p \)
- But large \( \beta \) can overcome comp. diff:
  \[ \beta > 1/2 \Rightarrow \psi'(p) > 0 \Rightarrow C(\psi(p), \lambda(p)) > 0 \]
- Shape restrictions
  1. Origin effs **concave** in \( \ln p \): \( \frac{d^2}{d(\ln p)^2} \lambda(p) < 0 \)
  2. Dest effs **convex** in \( \ln p \): \( \frac{d^2}{d(\ln p)^2} \psi(p) > 0 \)
Bounds on worker bargaining power

Consider *firm*-level variance components (firm-size weighted):

$$V_J[\psi], \ V_J[\lambda], \ C_J[\psi, \lambda],$$
Bounds on worker bargaining power

Consider *firm*-level variance components (firm-size weighted):

\[ \nabla J[\psi], \quad \nabla J[\lambda], \quad \mathcal{C}_J[\psi, \lambda], \]

- Excess variance of destination effects places lower bound on bargaining strength:

\[
\beta \geq \frac{1}{2} + \frac{\nabla J[\psi] - \nabla J[\lambda]}{2 \nabla J[\psi + \lambda]}.
\]

**Intuition:** as \( \beta \) grows, we approach AKM specification
Bounds on worker bargaining power

Consider *firm*-level variance components (firm-size weighted):

\[ \mathbb{V}_J[\psi], \quad \mathbb{V}_J[\lambda], \quad \mathbb{C}_J[\psi, \lambda], \]

- Excess variance of destination effects places lower bound on bargaining strength:

\[ \beta \geq \frac{1}{2} + \frac{\mathbb{V}_J[\psi] - \mathbb{V}_J[\lambda]}{2 \mathbb{V}_J[\psi + \lambda]}. \]

**Intuition:** as \( \beta \) grows, we approach AKM specification

- \( \beta > 1/2 \Rightarrow \) inequality restriction on O/D eff correlation:

\[ \rho_J(\psi, \lambda) \geq \sqrt{\frac{\mathbb{V}_J[\psi]}{\mathbb{V}_J[\psi + \lambda]}} \left( 1 - \frac{3}{10} \sqrt{\frac{\mathbb{V}_J[\lambda]}{\mathbb{V}_J[\psi + \lambda]}} \right) \]

**Intuition:** \( \beta > 1/2 \Rightarrow \) O/D effs both increasing in \( p \)
Median resignation yields job next month

Median time between jobs for other separations 5 months
Diagnostic #1: Is there a wage penalty for displacement?

Two workers $i$ and $\ell$ transition between the same firms $j$ and $k$

- Worker $i$ has EE ("voluntary") transition
  \[
  \mathbb{E}[y_{i2} - y_{i1} | W_i] = \psi_k - \psi_j + \lambda_j - \lambda_N
  \]

- Worker $\ell$ has EUE ("involuntary") transition
  \[
  \mathbb{E}[y_{\ell2} - y_{\ell1} | W_\ell] = \psi_k - \psi_j + \lambda_U - \lambda_N
  \]

Penalty for involuntary separation is

\[
\lambda_j - \lambda_U = \mathbb{E}[y_{i2} - y_{i1} | W_i] - \mathbb{E}[y_{\ell2} - y_{\ell1} | W_\ell]
\]

Rather than exact match on first two employers, group workers by coworker wage quartile at jobs #1 & #2 (16 groups)
Roughly constant penalty

Note: Each dot represents the adjusted log hiring wage change from job#1 to job#2 for different combinations of origin/destination quartiles of mean-coworkers wages. These dots are computed for two groups of workers. The first group (x-axis) corresponds to workers that voluntarily quit their first job. The second group (y-axis) corresponds to workers that were involuntarily separated from their first job.
Diagnostic #2: Does it matter who lays you off?

Recall that DWL model predicts making 2 involuntary transitions \((Q_{i1} = 0, Q_{i2} = 0)\) yields AKM style model of wage changes:

\[
E[y_{i3} - y_{i2} \mid W_i] = \psi_{j(i,3)} - \psi_{j(i,2)}
\]

- Identity \(j(i,1)\) of first employer is excludable!
- Test by comparing workers whose first employer was in top / bottom tercile of coworker wages
1st job irrelevant for workers displaced twice

Constant: -.007; Slope: .999

Wage change among Workers initially hired by High-Wage Employer

Wage change among Workers initially hired by Low-Wage Employer

Constant: -.007; Slope: .999
Roughly 4% penalty for hiring from non-employment
(Note: we have normalized $\lambda_N = 0$)

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Std Dev of log hiring wages</td>
<td>0.5286</td>
<td>0.4706</td>
<td>0.5623</td>
</tr>
<tr>
<td>Mean origin effect</td>
<td>0.0556</td>
<td>0.0536</td>
<td>0.0687</td>
</tr>
<tr>
<td>among involuntarily separated</td>
<td>0.0561</td>
<td>0.0543</td>
<td>0.0690</td>
</tr>
<tr>
<td>Origin effect when hired from unemployment ($\lambda_u$)</td>
<td>0.0163</td>
<td>0.0136</td>
<td>0.0220</td>
</tr>
</tbody>
</table>

**Bias-Corrected Variance Components**

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Std Dev of worker effects</td>
<td>0.2823</td>
<td>0.2479</td>
<td>0.2798</td>
</tr>
<tr>
<td>Std Dev of destination firm effects</td>
<td>0.2580</td>
<td>0.2434</td>
<td>0.2828</td>
</tr>
<tr>
<td>Std Dev of origin effects</td>
<td>0.0439</td>
<td>0.0454</td>
<td>0.0431</td>
</tr>
<tr>
<td>Std Dev of origin effects (among poached workers)</td>
<td>0.0761</td>
<td>0.0782</td>
<td>0.0798</td>
</tr>
<tr>
<td>Correlation of worker, destination firm effects</td>
<td>0.3157</td>
<td>0.2351</td>
<td>0.3441</td>
</tr>
<tr>
<td>Correlation of worker, origin effects</td>
<td>0.1200</td>
<td>0.1629</td>
<td>0.0757</td>
</tr>
<tr>
<td>Correlation of destination firm, origin effects</td>
<td>0.0316</td>
<td>0.0308</td>
<td>0.0000</td>
</tr>
</tbody>
</table>

**Percent of Total Variance Explained by**

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Men</th>
<th>Women</th>
</tr>
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<tbody>
<tr>
<td>Worker effects</td>
<td>28.52%</td>
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</tr>
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<td>Destination firm effects</td>
<td>23.81%</td>
<td>26.74%</td>
<td>25.29%</td>
</tr>
<tr>
<td>Origin effects</td>
<td>0.69%</td>
<td>0.93%</td>
<td>0.59%</td>
</tr>
<tr>
<td>Covariance of worker, destination</td>
<td>16.46%</td>
<td>12.81%</td>
<td>17.23%</td>
</tr>
<tr>
<td>Covariance of worker, origin</td>
<td>1.06%</td>
<td>1.66%</td>
<td>0.58%</td>
</tr>
<tr>
<td>Covariance of destination, origin</td>
<td>0.26%</td>
<td>0.31%</td>
<td>0.00%</td>
</tr>
<tr>
<td>$X\delta$ and associated covariances</td>
<td>1.66%</td>
<td>3.51%</td>
<td>0.09%</td>
</tr>
<tr>
<td>Residual</td>
<td>27.55%</td>
<td>26.30%</td>
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</tr>
</tbody>
</table>
# It Ain’t Where You’re From

## Table 4: Variance Decomposition across Person-Job Observations --- DWL Model

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td>Std Dev of log hiring wages</td>
<td>0.5286</td>
<td>0.4706</td>
<td>0.5623</td>
</tr>
<tr>
<td>Mean origin effect among involuntarily separated</td>
<td>0.0556</td>
<td>0.0536</td>
<td>0.0687</td>
</tr>
<tr>
<td>Mean origin effect among voluntarily separated</td>
<td>0.0561</td>
<td>0.0543</td>
<td>0.0690</td>
</tr>
<tr>
<td>Origin effect when hired from unemployment ($\lambda_u$)</td>
<td>0.0163</td>
<td>0.0136</td>
<td>0.0220</td>
</tr>
</tbody>
</table>

### Bias-Corrected Variance Components

<table>
<thead>
<tr>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>Std Dev of worker effects</td>
<td>0.2823</td>
<td>0.2479</td>
<td>0.2798</td>
</tr>
<tr>
<td>Std Dev of destination firm effects</td>
<td>0.2580</td>
<td>0.2434</td>
<td>0.2828</td>
</tr>
<tr>
<td>Std Dev of origin effects</td>
<td>0.0439</td>
<td>0.0454</td>
<td>0.0431</td>
</tr>
<tr>
<td>Std Dev of origin effects (among poached workers)</td>
<td>0.0761</td>
<td>0.0782</td>
<td>0.0798</td>
</tr>
<tr>
<td>Correlation of worker, destination firm effects</td>
<td>0.3157</td>
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<td>0.3441</td>
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*Note:* This table reports the variance decomposition based upon the DWL model across person-job observations. We also report the (firm-size weighted) corresponding average of the origin effects for individuals that were involuntarily separated as well as the estimated origin effect when hired from unemployment. All origin effects are represented as $\lambda_u$. The total variance explained by worker effects is 23.81%. The variance explained by destination firm effects is 23.81%. The variance explained by origin effects is 0.69%.
Dest effs $\approx 14 \times$ as variable as orig effs across firms

Table 5: Variance Decomposition across Firms

<table>
<thead>
<tr>
<th></th>
<th>Pooled</th>
<th>Men</th>
<th>Women</th>
</tr>
</thead>
<tbody>
<tr>
<td># of firms with identified destination and origin effect</td>
<td>297,865</td>
<td>201,080</td>
<td>99,508</td>
</tr>
</tbody>
</table>

**Bias-Corrected Variance Components**

<table>
<thead>
<tr>
<th></th>
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</tr>
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<tbody>
<tr>
<td>Std of Destination Effects</td>
<td>0.2590</td>
<td>0.2449</td>
<td>0.2724</td>
</tr>
<tr>
<td>Std of Origin Effects</td>
<td>0.0707</td>
<td>0.0721</td>
<td>0.0510</td>
</tr>
<tr>
<td>Correlation of destination, origin</td>
<td>0.2511</td>
<td>0.2491</td>
<td>0.3168</td>
</tr>
<tr>
<td>Std of Destination + Origin Effects</td>
<td>0.2851</td>
<td>0.2720</td>
<td>0.2926</td>
</tr>
</tbody>
</table>

Lower Bound on Bargaining Power | 0.8819   | 0.8703   | 0.9182   |

Lower Bound on Correlation of Destination, Origin Effects | 0.8409   | 0.8288   | 0.8824   |

*Note:* Here we report the variance decomposition across firms where each firm has an identified origin and destination firm effect. Variance components are weighted by average firm-size over 2005-2015 as recorded by official INPS records collected in the dataset *Anagrafica*, see text for details. Variance components corrected using the leave-out bias correction of Kline, Saggio and Sølvsten (2020). The lower bounds on the bargaining power and correlation of destination and origin firm effects are based upon equation (5)-(6), see text for details.
Implied std dev of log productivity = .28
Compare to std log VA/L ≈ 0.8

Table 5: Variance Decomposition across Firms

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Lower Bound on Bargaining Power

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<td>0.8819</td>
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Note: Here we report the variance decomposition across firms where each firm has an identified origin and destination firm effect. Variance components are weighted by average firm-size over 2005-2015 as recorded by official INPS records collected in the dataset Anagrafica, see text for details. Variance components corrected using the leave-out bias correction of Kline, Saggio and Sølvsten (2020). The lower bounds on the bargaining power and correlation of destination and origin firm effects are based upon equation (5)-(6), see text for details.
Need $\beta > .88$ to explain excess orig eff var
Which would require O/D corr $> .84$, but empirical corr is only .25..

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<th>Women</th>
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<tr>
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<td>99,508</td>
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**Bias-Corrected Variance Components**

- Std of Destination Effects: 0.2590, 0.2449, 0.2724
- Std of Origin Effects: 0.0707, 0.0721, 0.0510
- Correlation of destination, origin: **0.2511**, 0.2491, 0.3168
- Std of Destination + Origin Effects: 0.2851, 0.2720, 0.2926

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**Note:** Here we report the variance decomposition across firms where each firm has an identified origin and destination firm effect. Variance components are weighted by average firm-size over 2005-2015 as recorded by official INPS records collected in the dataset Anagrafica, see text for details. Variance components corrected using the leave-out bias correction of Kline, Saggio and Sølvsten (2020). The lower bounds on the bargaining power and correlation of destination and origin firm effects are based upon equation (5)-(6), see text for details.
Heterogeneity: law firms have important origin effs

Figure 4: Variability of Origin and Destination Effects by Sector

Note: This figure reports leave-out corrected standard deviations of destination and origin firm effects for selected sectors of the Italian economy (2-Digit 2007 Ateco codes). All variance components are firm-size weighted. The dashed line is the 45 degree line.
But even among law firms O/D correlation too low

<table>
<thead>
<tr>
<th>Sector</th>
<th>SD of Destination Effects</th>
<th>SD of Origin Effects</th>
<th>Correlation of Origin, Destination Effects</th>
<th>Lower Bound on Bargaining Power</th>
<th>Lower Bound on Correlation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Retail</td>
<td>0.1585</td>
<td>0.0597</td>
<td>0.2260</td>
<td>0.8269</td>
<td>0.7868</td>
</tr>
<tr>
<td>Construction</td>
<td>0.1959</td>
<td>0.0639</td>
<td>-0.0693</td>
<td>0.9211</td>
<td>0.8786</td>
</tr>
<tr>
<td>Restaurants / Hotels</td>
<td>0.3206</td>
<td>0.0706</td>
<td>0.0675</td>
<td>0.9413</td>
<td>0.9018</td>
</tr>
<tr>
<td>Hairdressing / Care Centers</td>
<td>0.2284</td>
<td>0.0641</td>
<td>0.1399</td>
<td>0.8979</td>
<td>0.8567</td>
</tr>
<tr>
<td>Law Firms</td>
<td>0.1468</td>
<td>0.1359</td>
<td>0.0399</td>
<td>0.5369</td>
<td>0.5758</td>
</tr>
<tr>
<td>Manufacturing</td>
<td>0.1585</td>
<td>0.0536</td>
<td>0.2737</td>
<td>0.8409</td>
<td>0.7992</td>
</tr>
<tr>
<td>Transportation</td>
<td>0.3028</td>
<td>0.0859</td>
<td>-0.0632</td>
<td>0.9401</td>
<td>0.8969</td>
</tr>
<tr>
<td>Cleaning / Security</td>
<td>0.2777</td>
<td>0.0842</td>
<td>0.0874</td>
<td>0.8966</td>
<td>0.8551</td>
</tr>
<tr>
<td>Temp Agencies</td>
<td>0.0639</td>
<td>0.0206</td>
<td>0.1651</td>
<td>0.8702</td>
<td>0.8291</td>
</tr>
<tr>
<td>Management / Consulting / Tech</td>
<td>0.1847</td>
<td>0.0870</td>
<td>0.4190</td>
<td>0.7406</td>
<td>0.6991</td>
</tr>
</tbody>
</table>

Note: This table reports leave-out corrected standard deviations of destination and origin firm effects within selected sectors of the Italian economy (2-Digit 2007 Ateco codes). All variance components are firm-size weighted. The lower bounds on the bargaining power and correlation of destination and origin firm effects are based upon equation (5)-(6), see text for details.
O/D effs both increasing in VA

Figure 5: Origin and Destination Effects by Value Added
(a) Value Added per Worker
But violate shape restrictions
Also: BF-PVR requires $\beta > \max_p \frac{d\psi (p')}{d \ln p} \approx 0.92$

Note: each dot is mean within a VA bin (same as previous fig)
Female dest effs less sensitive to VA

Same slope as found in Portugal [Card, Cardoso, Kline, 2015]
Same for orig effs but female suffer greater penalty for EUE
Where you’re from irrelevant for gender gap
Initially explained by where you’re at. Evolution due to other factors.

Figure 8: Gender Wage Gap and the DWL Model
(a) Entered Labor Market in 2005
Summary

Where you’re hired from doesn’t seem to matter quantitatively for most workers

Two notable exceptions:

- There is an important penalty for being hired from non-employment
- Highly skilled hierarchical professions (e.g. law) seem to exhibit origin effects
Why aren’t hiring origins more important?

They likely are important for elite workers (NBA players, C-suite executives, star lawyers) who are expected to negotiate and typically have objective performance metrics that can be used to justify their pay.

But most jobs commit to posted wages, likely for a mix of information and horizontal equity reasons:

- Hall and Krueger (2012): bargaining only common among high skilled jobs in US
- Caldwell & Harmon (2019): in Denmark only 31% of manual and 51% of professional jobs engage in negotiation
- Postel-Vinay and Robin (2004): less productive firms commit not to match to avoid costly moral hazard \[\Rightarrow\] dual labor markets
- Card, Moretti, Mas, and Saez (2012): horizontal inequity in pay generates potentially costly morale problems
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11. Card, D., Mas, A., Moretti, E., & Saez, E. (2012). Inequality at work:
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labor market inequality: Evidence and some theory. Journal of Labor
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treatment and structural parameters. The Econometrics Journal, 21(1),
C1-C68.
employer minimum wages.
where you’re from, it’s where you’re at: hiring origins, firm heterogeneity,


