Econ 219B
Psychology and Economics: Applications
(Lecture 7)

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Outline

1. Social Preferences Wave I: Altruism
2. Workplace Effort: Altruism
3. Shaping Social Preferences
4. Social Preferences Wave II: Warm Glow and Charitable Giving
5. Social Preferences Wave III: Inequity Aversion and Reciprocity
6. Workplace Effort: Inequity Aversion
7. Methodology: Field Experiments
8. Social Preferences Wave IV: Social Pressure and Signaling
9. Social Pressure I

Section 1

Social Preferences Wave I: Altruism

Pure Altruism

- First set of models (since 1970s): Pure altruism
  - Self with payoff x_s
    - Other with payoff x_o
    - Self assigns weight α to Other's utility:

$$U = u(x_s) + \alpha u(x_o)$$

  - First used to model within-family altruism (Becker, 1981; Becker and Barro, 1986)
  - Still very useful benchmark model

Section 2

Workplace Effort: Altruism

Bandiera-Barankay-Rasul (QJE, 2005)

- Impact of relative pay versus piece rate on productivity
- Standard model:
  - *Piece rate:* Worker i maximizes

$$\begin{aligned} & \max_{e_i} p e_i - c(e_i) \\ e_{iP}^* &= c'^{-1}(p) \end{aligned}$$

  - *Relative pay*: Worker i maximizes

$$\max_{e_i} p e_i - \gamma \sum_{j \neq i} \frac{e_j}{I-1} - c(e_i)$$

$$e_{iRP}^* = e_P^* = c'^{-1}(p)$$

Assume simple altruism

- Simple Altruism model:

$$U_i = u_i + \alpha \sum_{j \neq i} u_j$$

- *Piece rate*: Worker maximizes

$$\max_{e_i} pe_i - c(e_i) + \alpha \sum_{j \neq i} [pe_j - c(e_j)]$$

- Same solution as with $\alpha = 0$
  - *Relative pay*: Worker i maximizes

$$\max_{e_i} pe_i - \gamma \sum_{j \neq i} \frac{e_j}{I-1} - c(e_i) + \alpha \sum_{j \neq i} \left[pe_j - \gamma \sum_{q \neq j} \frac{e_q}{I-1} - c(e_j) \right]$$

  - Solution

$$c'(e^*_{iRP}) = p - \alpha\gamma(I-1) -> e^*_{iRP} < e^*_{iP}$$

Experiment Details

- Test for impact of social preferences in the workplace
  - Does productivity increase when switching to piece rate?
- Use personnel data from a fruit farm in the UK
- Measure productivity as a function of compensation scheme
- Timeline of quasi-field experiment:
  - First 8 weeks of the 2002 picking season $\rightarrow$ Fruit-pickers compensated on a relative performance scheme
    - Per-fruit piece rate is decreasing in the average productivity.
    - Workers that care about others have incentive to keep the productivity low
  - Next 8 weeks $\rightarrow$ Compensation switched to flat piece rate per fruit
  - Switch announced on the day change took place

Results: Productivity

- Dramatic 50 percent increase in productivity

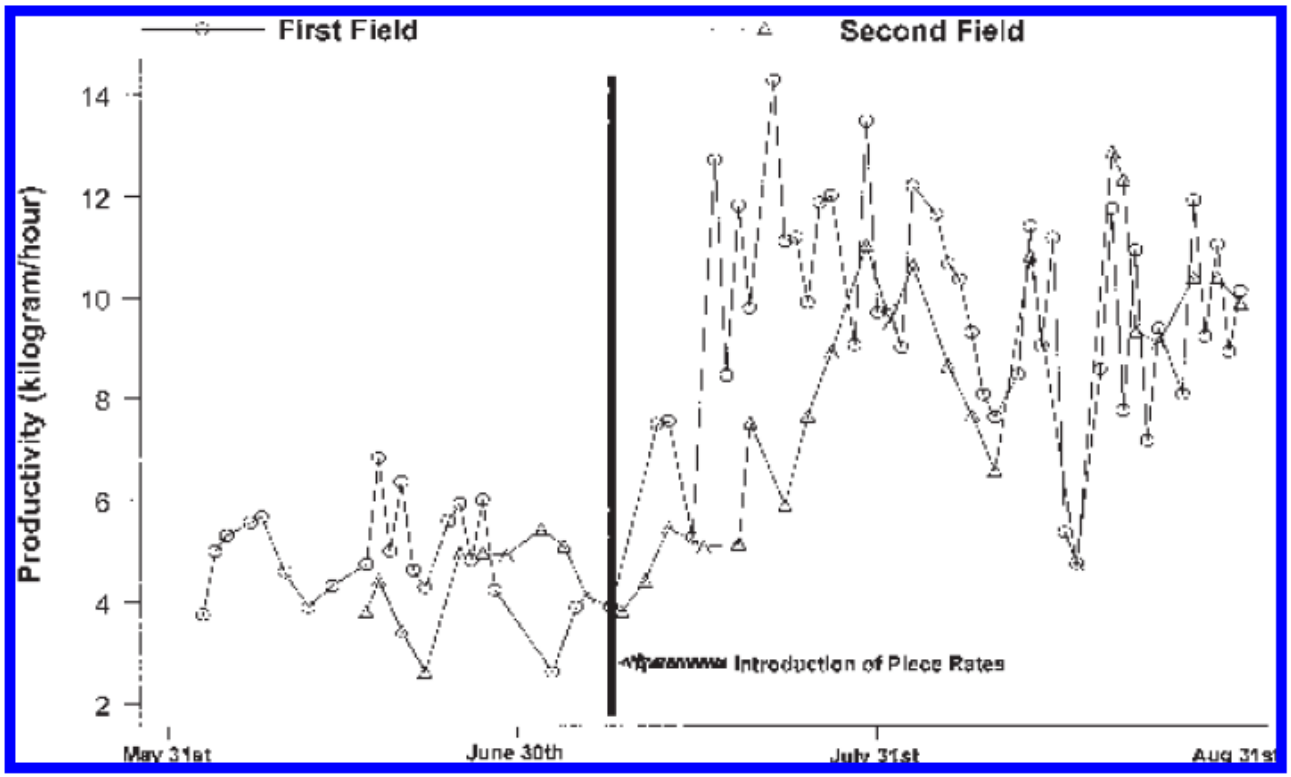


FIGURE I
Productivity (kilogram/hour) over the Season

Results: Other

- No other significant changes

	Relative incentives	Piece rates	Difference
Worker productivity (kg/hr)	5.01	7.98	
	(.243)	(.208)	2.97***
	[4.53, 5.49]	[7.57, 8.39]	
Kilos picked per day	Confidential		23.2***
Hours worked per day	Confidential		−.475
Number of workers in same field	41.1	38.1	−3.11
	(2.38)	(1.29)	
Daily pay	Confidential		1.80
Unit wage per kilogram picked	Confidential		−.105***

*** denotes significance at 1 percent. Sample sizes are the same as those used for the productivity regressions. Standard errors and confidence intervals take account of the observations being clustered by field-day. Productivity is measured in kilograms per hour. Daily pay refers to pay from picking only. Both daily pay and the unit wage per kilogram picked are measured in UK Pounds Sterling. Some information in the table cannot be shown due to confidentiality requirements.

- Is this due to response to change in piece rate?
  - No, piece rate went down → Incentives to work less (substitution effect)

Robustness Checks

- Results robust to controls
- Results are stronger the more friends are on the field

	(1a) Relative incentives	(1b) Relative incentives	(2a) Piece rates	(2b) Piece rates
Share of workers in the field who are friends	−1.68***	−5.52**	.072	1.17
	(.647)	(2.36)	(.493)	(1.60)
Share of workers in the field who are friends × number of workers in same field		1.60**		−.285
		(.684)		(.501)
Number of workers in same field		.182		.085
		(.117)		(.069)
Marginal effect of group size (at mean friends' share)		.236**		.076
		(.110)		(.065)
Worker fixed effects	Yes	Yes	Yes	Yes
Field fixed effects	Yes	Yes	Yes	Yes
Other controls	Yes	Yes	Yes	Yes
Adjusted R^2	.3470	.3620	.3065	.3081
Number of observations (worker-field-day)	2860	2860	4400	4400

Interpretations

- Two Interpretations:
  - Social Preferences:
    - Work less to help others
    - Work even less when friends benefit, since care more for them
  - Repeated Game
    - Enforce low-effort equilibrium
    - Equilibrium changes when switch to flat pay
- Test: Observe results for tall plant where cannot observe productivity of others (raspberries vs. strawberries)

Results: Comparison

- Compare Fruit Type 1 (Strawberries) to Fruit Type 2 (Raspberries)
  - No effect for Raspberries

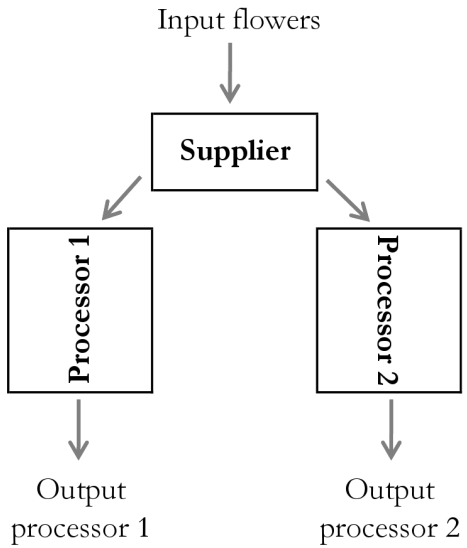
DEPENDENT VARIABLE = LOG OF WORKER'S PRODUCTIVITY
(KILOGRAM PICKED PER HOUR PER FIELD-DAY)
ROBUST STANDARD ERRORS REPORTED IN PARENTHESES, ALLOWING FOR CLUSTERING AT FIELD-DAY LEVEL

	(1) Fruit type 2	(2) Fruit type 1	(3) Fruit types 1 and 2 combined
Piece rate dummy (P_t)	−.063	.483***	
	(.129)	(.094)	
Piece rate × fruit type 2			−.100
			(.095)
Piece rate × fruit type 1			.490***
			(.092)

- → No Pure Social Preferences. However, can be reciprocity
- Important to control for repeated game effects → Field experiments

Hjort (2014 QJE)

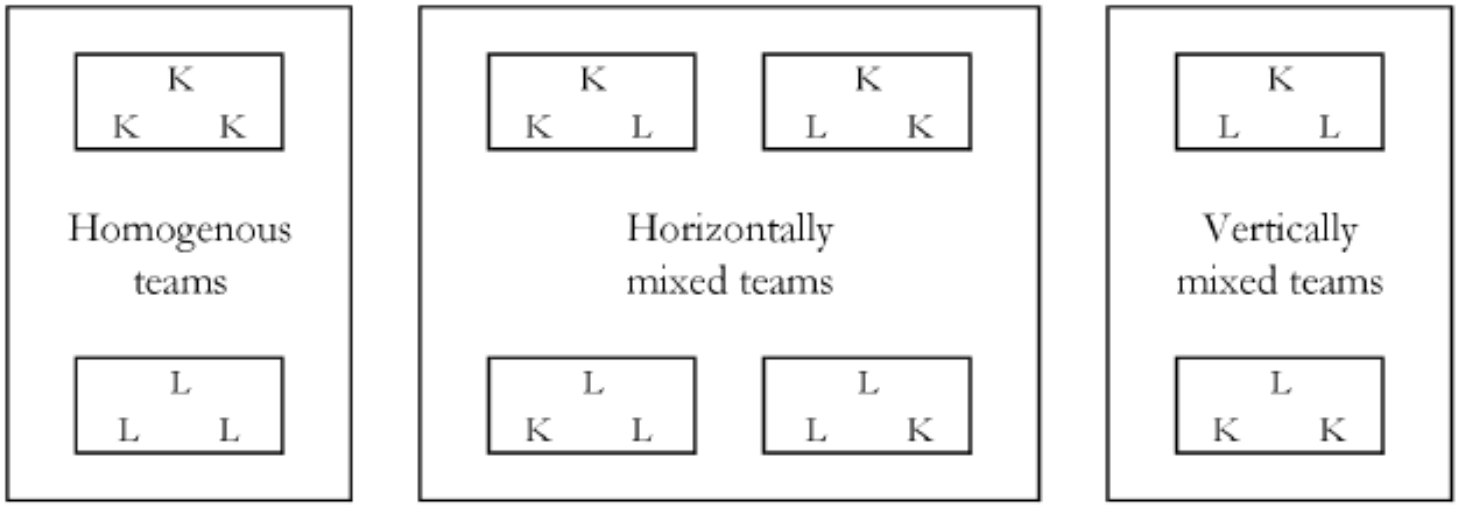
- Social preferences among co-workers as function of ethnicity
  - Kenya flower plant
  - Teams of 3: one supplier, two processors
  - Piece rate (at least initially) for two processors, and supplier gets pay for average productivity



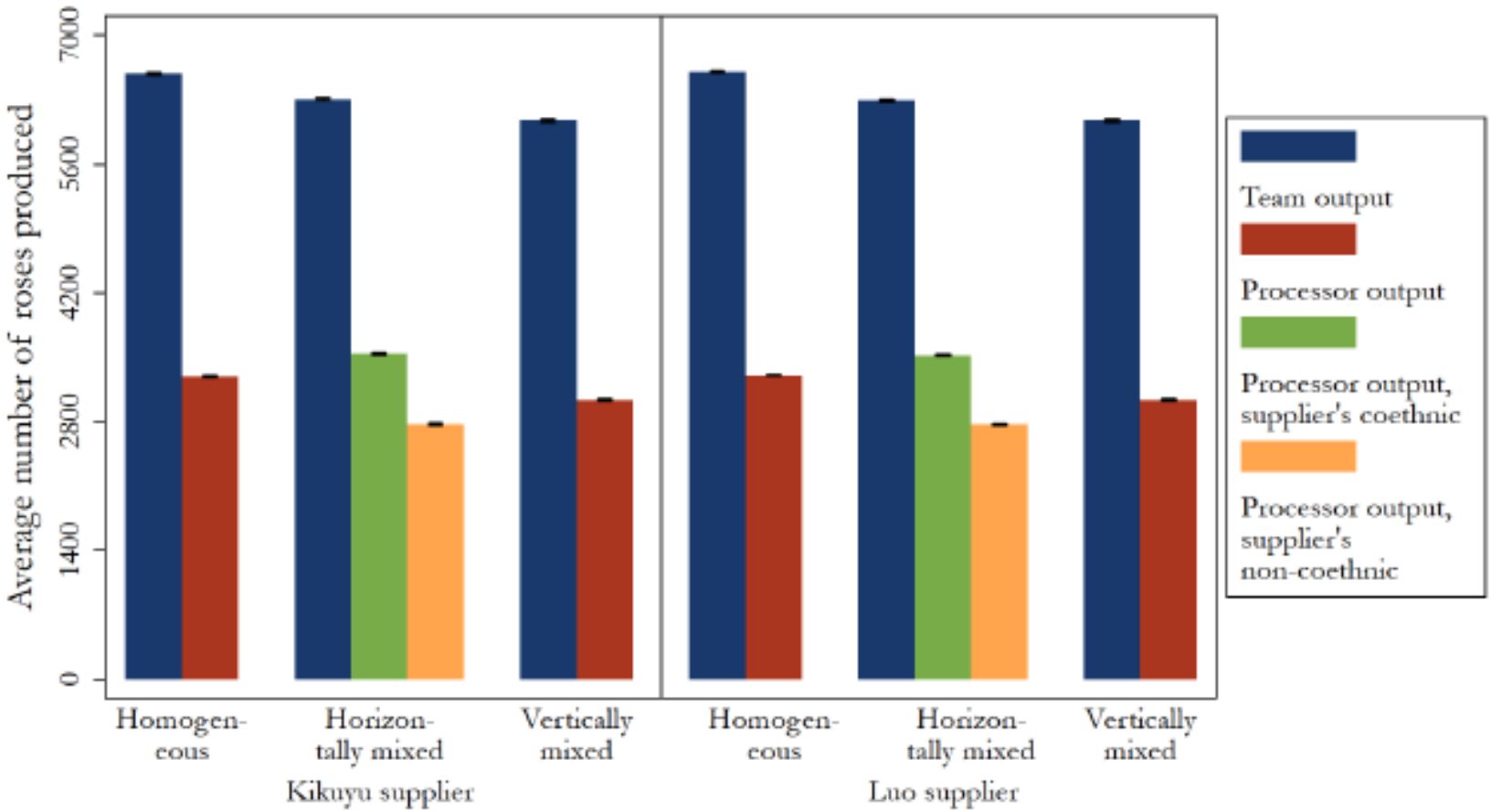
Manipulate Team Composition

- Different team ethnicity configurations of Luos and Kikuyu:
  - Vertically mixed teams → Work less hard to sort flowers
  - Horizontally mixed teams → Sort fewer flowers to non-coethnic
  - Findings strikingly aligned to predictions of model

Figure I.b
Team ethnicity configuration categories



Results

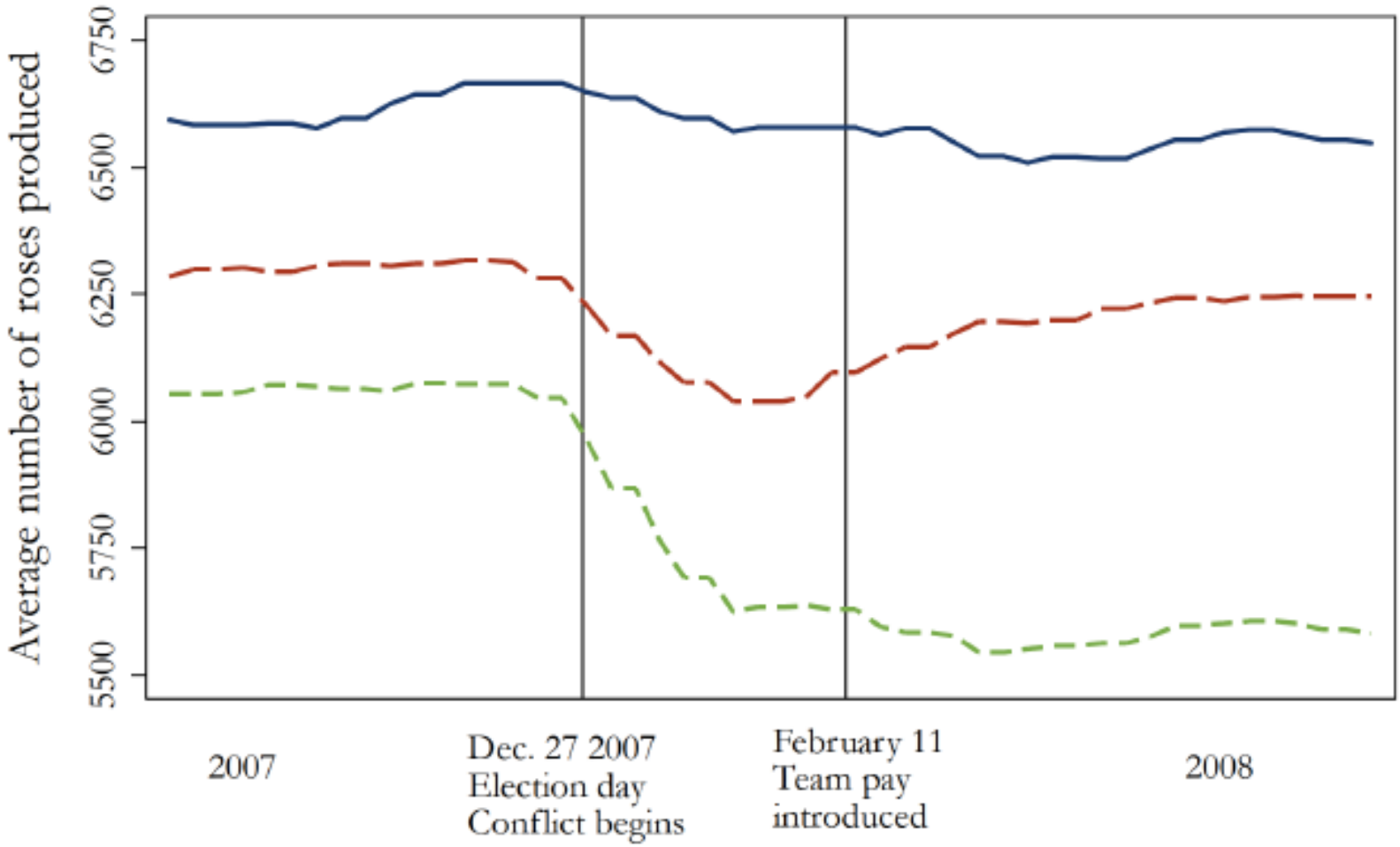

Average number of roses produced
0
1400
2800
4200
5600
7000
Homogen-eous
Horizon-tally mixed
Vertically mixed
Kikuyu supplier
Homogen-eous
Horizon-tally mixed
Vertically mixed
Luo supplier
Team output
Processor output
Processor output, supplier's coethnic
Processor output, supplier's non-coethnic

Explanations

- Two further pieces of evidence:
  1. Period of ethnic animosity and violence
  2. Switch to team pay for the processors
- Prediction of first change:
  - Exacerbate patterns
- Prediction of second change:
  - Reduce effect in horizontally-mixed teams
  - Not in vertically-mixed teams

Results

Figure II
Output in homogeneous and mixed teams across time



Section 3

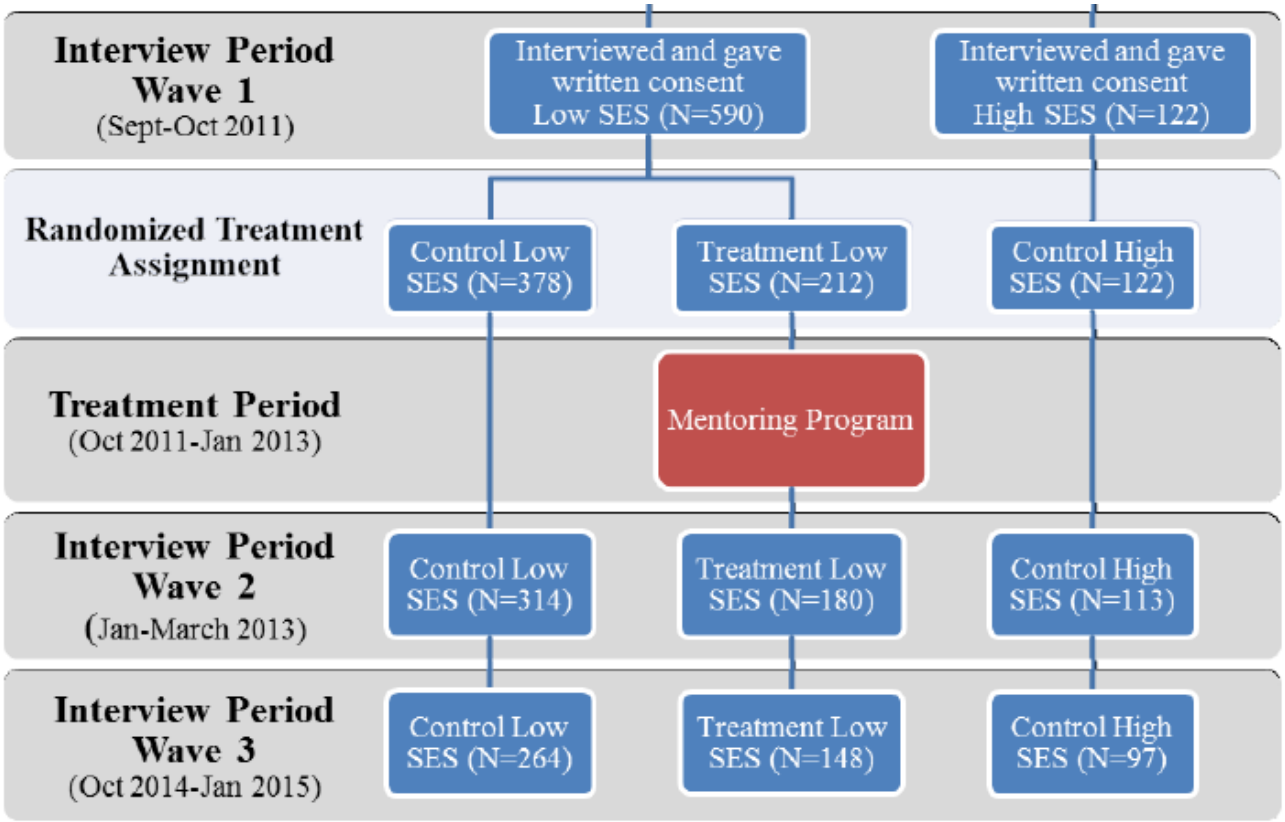
Shaping Social Preferences

Introduction

- In given economic setting, take preferences as given (Becker, '*De Gustibus non est disputandum*')
- But over medium-term, preferences can shift
- Focus on evolution of social preferences
- Example 1: **Hjort (2014 QJE)** – conflict affects social preferences between workers of different ethnicity

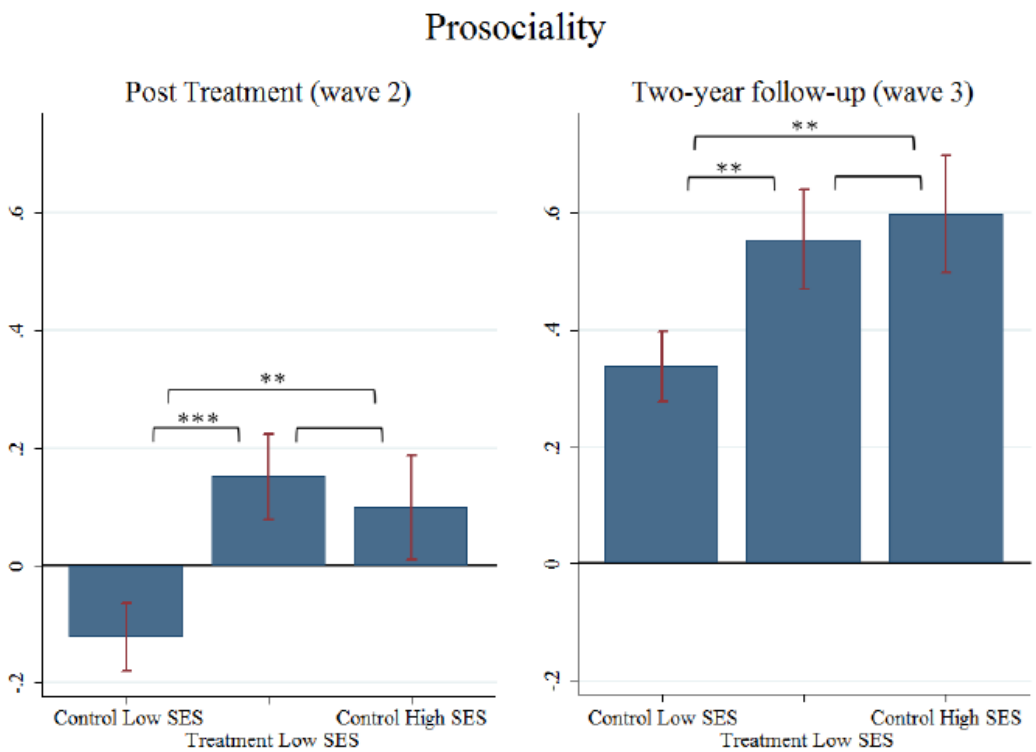
Example 2: Deckers, Falk, Kosse, Schildberg-Hörisch (2016)

- Program in Germany for low income children ages 7-9, assign
  - 1.5 year mentoring program
  - OR control



Measures

- Measure 1: Children 6 stars (traded with toys) between themselves and another (anonymous) child, local or in Africa
- Measure 2: Ask 'How much do you trust others'



Example 3: Cappelen et al. (2017)

- Example 3: Cappelen, List, Samek, and Tungodden (2017)
  - They set up a pre-school (!) in Chicago Heights, IL, high-poverty area
  - Randomized access to preschool
  - Examine, among other things, the impact on social preferences
- Slides courtesy of Anya

CHECC

Chicago Heights Early Childhood Center

- In 2010-2012, households with children ages 3-4 years were recruited and randomized into one of three groups:

Preschool

Free, 9-month full-day preschool for the child (no direct intervention for the parent)

Parent Academy

Free, 9-month incentivized parenting program for parents to learn how to teach to child at home. Parents incentivized (could earn up to $7,000)

Control ("Family Program")

Neither child or parent received interventions. Families invited to activities to reduce attrition.

CHECC Academic Outcomes

- Parent Academy has large impact on “non-cognitive” or executive functioning skills - working memory, attention, inhibitory control – which fade over time (Fryer, Levitt, List, 2015).

- Preschool has large impact on “cognitive” skills – reading, writing, math – which also fade by 2nd grade (Fryer, Levitt, List, Samek, work in progress).

- Both programs also show some differential impacts by sub-group, e.g., for PK the effects are concentrated among lowest performing kids.

Our Experiment

- Take advantage of the CHECC RCT to study the causal impact of Preschool/Parent Academy programs (relative to control group) on social preferences.
- Return to children from original CHECC program 2 years later (when they are 7-8 years old) and conduct social preference experiments.

- Prior agreement with School District 170 (9 schools) to follow up with children in-class. We reached all children attending SD170 at the time of the social preference experiment.
- Our children feed into several districts (no agreements with other districts), so we capture 38% of Preschool (N=84), 38% of Parent Academy (N=89) and 35% of Control (N=130).

Overview of Experiments

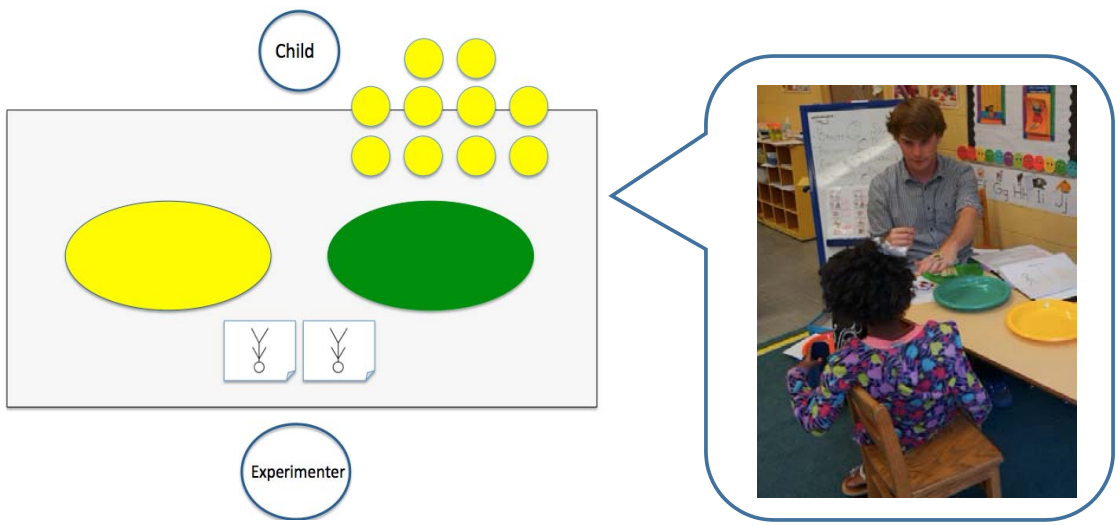
Experiment	Type	Description
Dictator	Stakeholder	Allocate coins between self and another child.
Efficiency	Spectator	Choose between an unfair and efficient allocation or a fair and inefficient allocation.
Luck	Spectator	Allocate stickers between a lucky child and an unlucky child.
Merit	Spectator	Allocate stickers between a child who did well and a child who did not do well.

Luck Game

[…] these kids are not the same as the two you decided for before […]

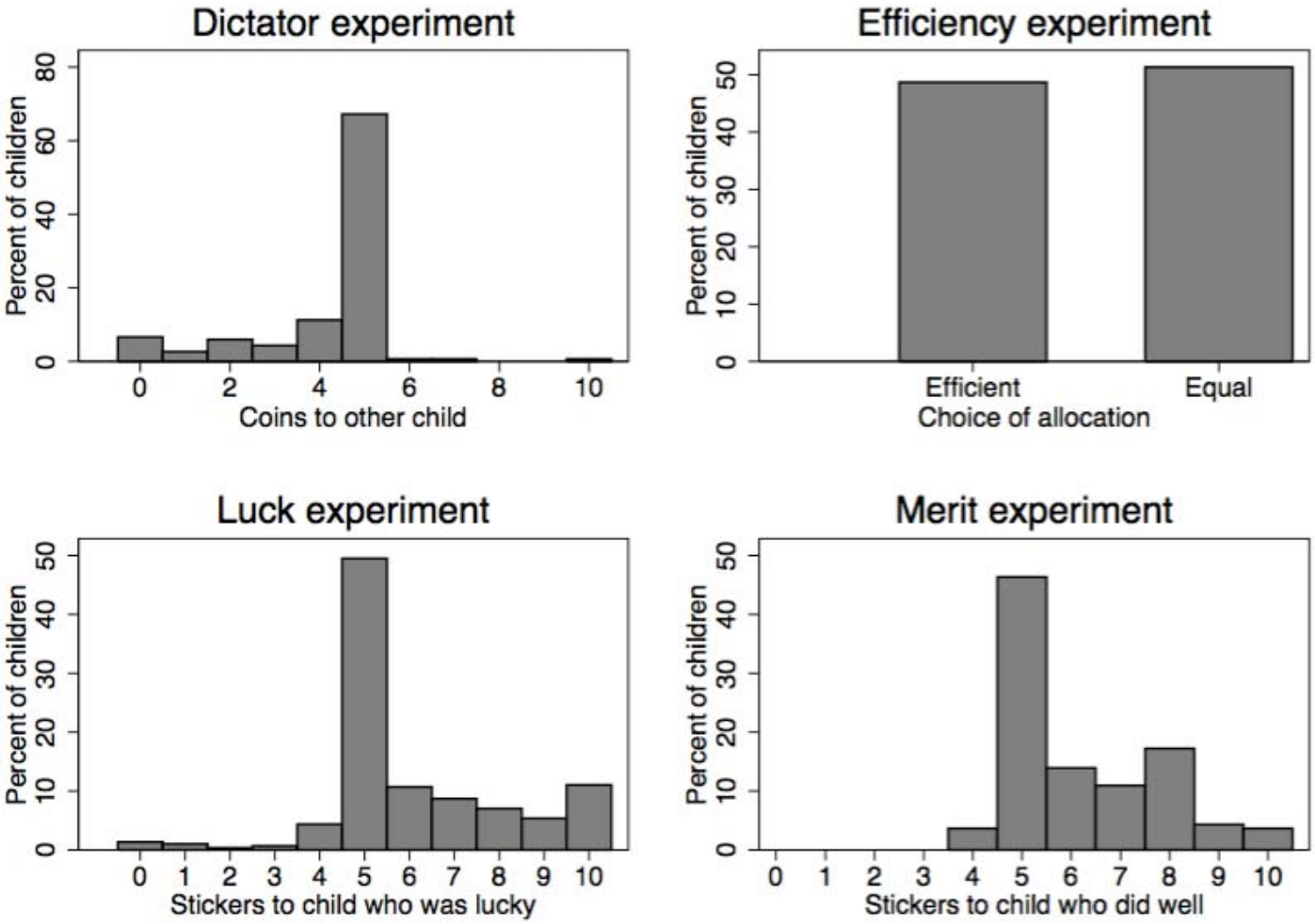
We are going to decide what each of the kids get by flipping a token. If [it] up GREEN, […] the kid with the GREEN plate gets all the stickers and the kid with the YELLOW plate is given no stickers [repeat for Yellow].

Even though this is what the kids got, ***now YOU can decide whether you want to change the number of stickers given to each of them. You can choose to split up the stickers any way you want.***



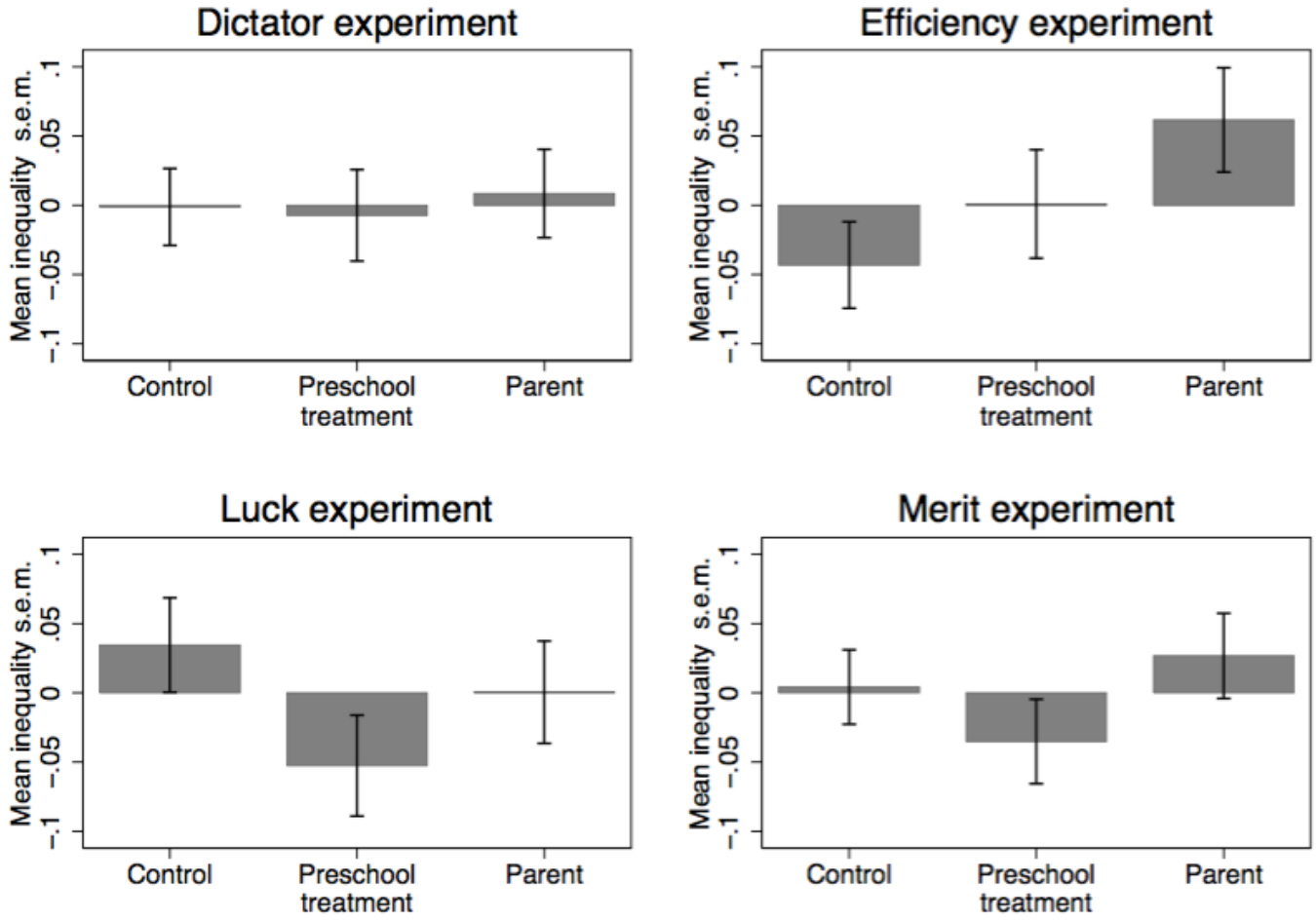
Summary of Results

- Percent of children implementing each decision



$$\text{Inequality} = \frac{|\text{Income Person } A - \text{Income Person } B|}{\text{Total Income}}$$

- Mean inequality implemented, relative to average, by treatment.



Example 4: Rao (2014)

- Example 3: **Rao (2014):** Consider the impact of exposure to students of different social class on preferences
- Remarkable impacts over just 1-2 years of exposure
- Slides courtesy of Gautam

Elite Private Schools in Delhi

Elite private schools are:

- Expensive: Tuition \$500-\$2500/year (25-110% of median annual household income)
  - Public schools are free
- Selective: In my sample, accept $\approx 7\%$ of applicants
  - Strictly regulated admissions criteria
    - Neighborhood
    - Older siblings in same school
    - Parents alumni, parent interview

Policy Innovation

Policy change in Delhi in 2007:

- 20% admissions quota in private schools for poor students
  - Household income cutoff: $2000/year
- Schools which received subsidized land from *state* govt.
  - Over 90% of elite private schools
- No fees for poor children
- No tracking

Variation across classrooms

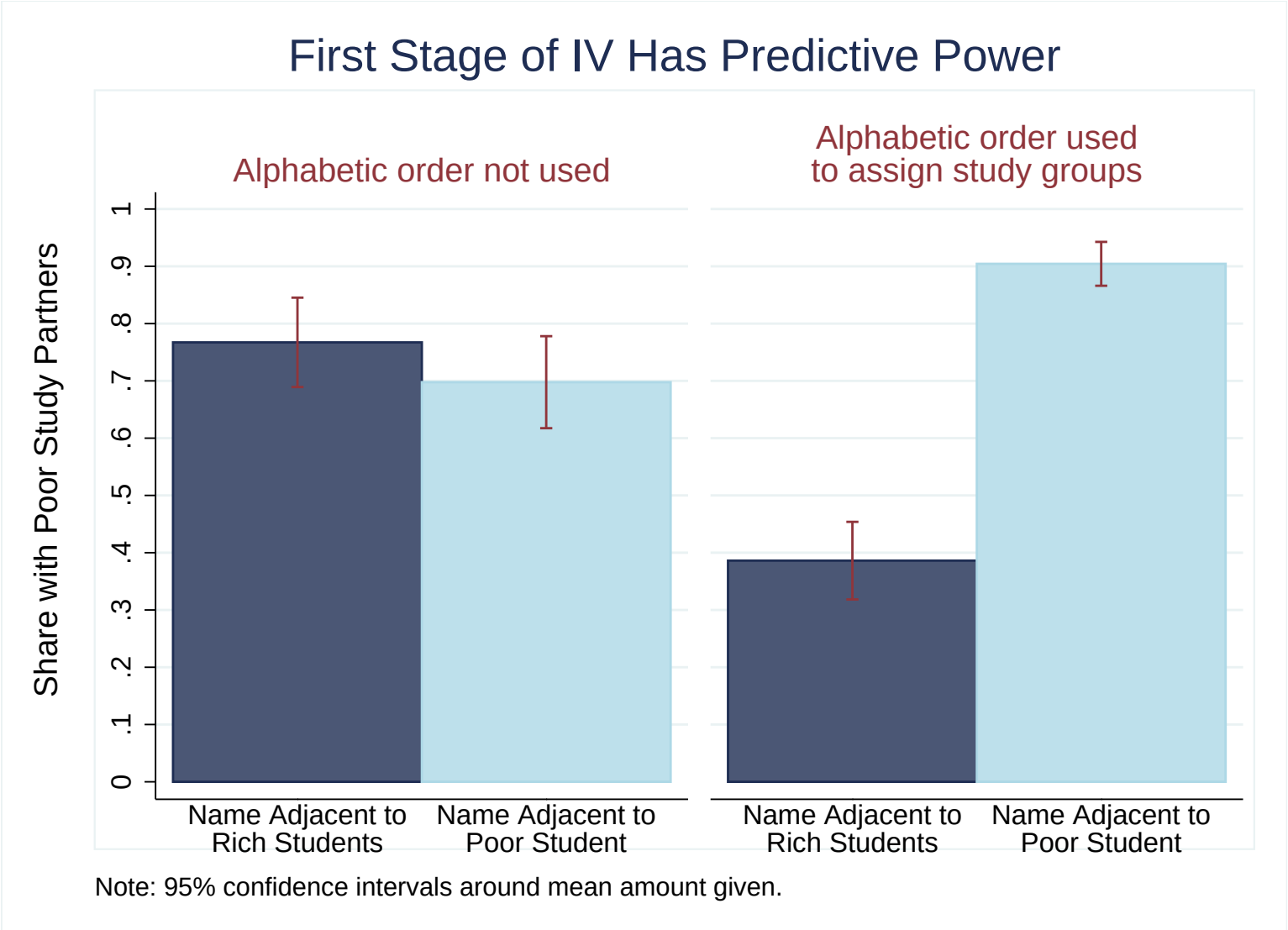
Sample for this paper:

- $k = 14$ schools
  - 9 Treatment Schools
  - 2 Delayed Treatment Schools
  - 3 Control Schools
- $n = 2017$ randomly selected students in 14 schools
  - in Grades 2-5
- Over-sample control, delayed treatment schools
  - Treatment schools in same neighborhoods

Variation within classroom (IV strategy)

- 1 hr a day working in small groups of 2-4 students
- Some schools ($k = 7$) use alphabetic order of *first* name to assign study groups.
  - Exogeneous variation in personal interactions
- Other schools ($k = 4$) frequently shuffle groups
  - Only "direct" effect of name

Alphabetic Order Predicts Study Partners

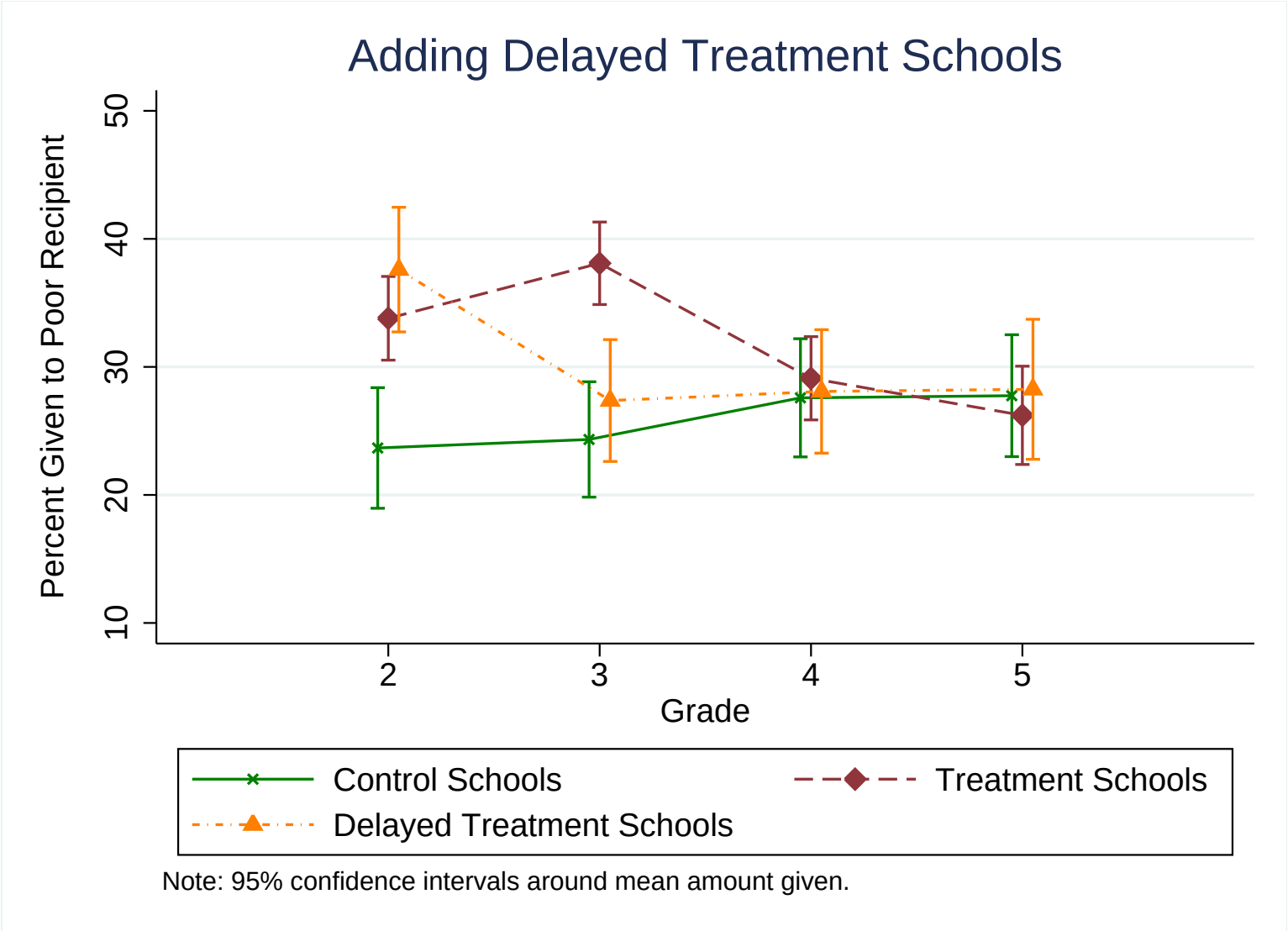


Note: 95% confidence intervals around mean amount given.

Dictator Games

- Students endowed with 10 Rupees, choose to share $x \in [0, 10]$
  - Can exchange money for candy later (Rs. 1 per piece)
- Vary the identity of the recipient
  - Game 1: **Poor** student in a school for poor children
  - Game 2: **Rich** student in a private (control) school
  - Order randomized
- Name and photographs of school shown to subjects.
  - Debriefing: Subjects understood recipient poor / rich

Dictator Game with Poor Recipient



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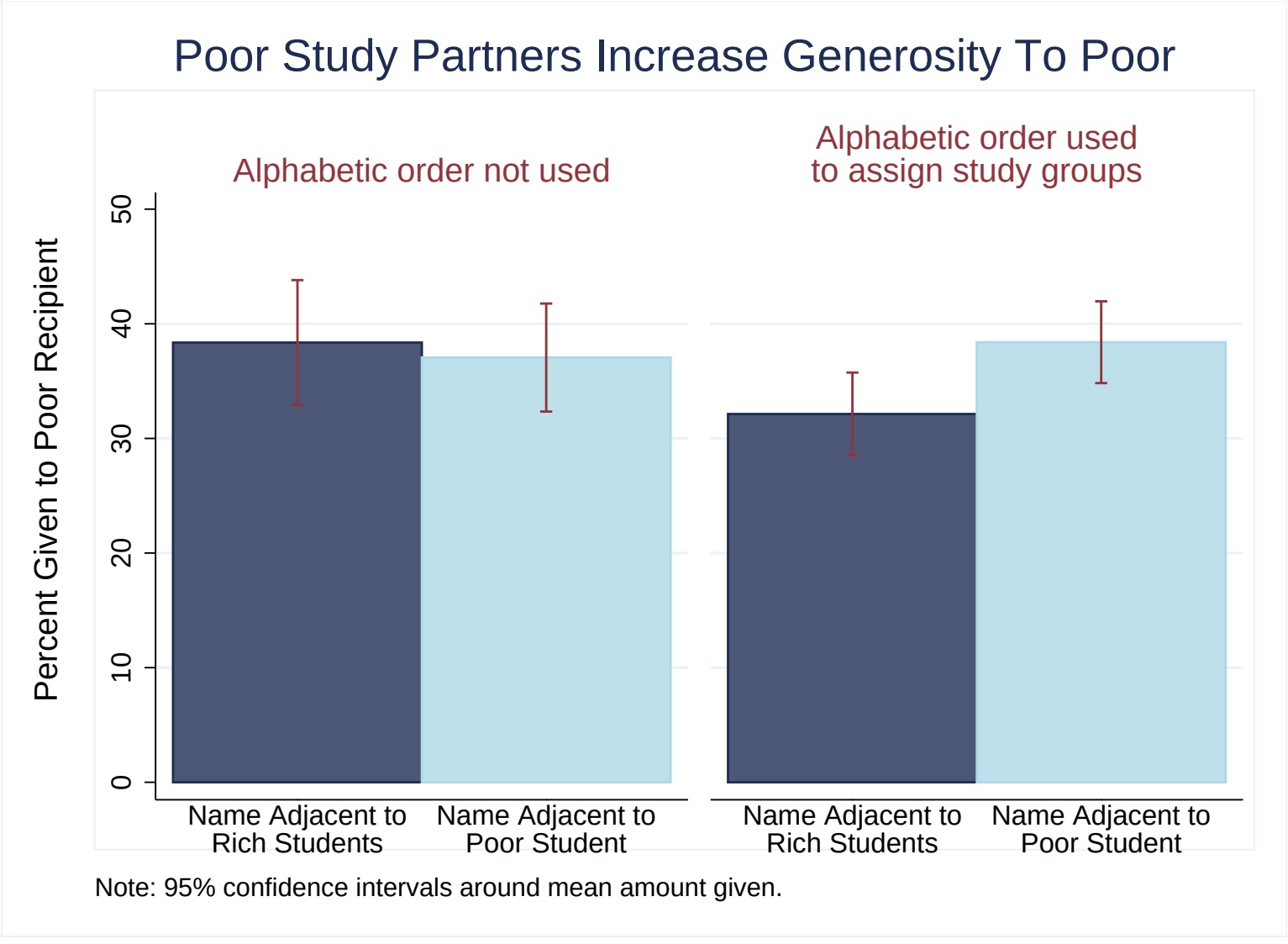
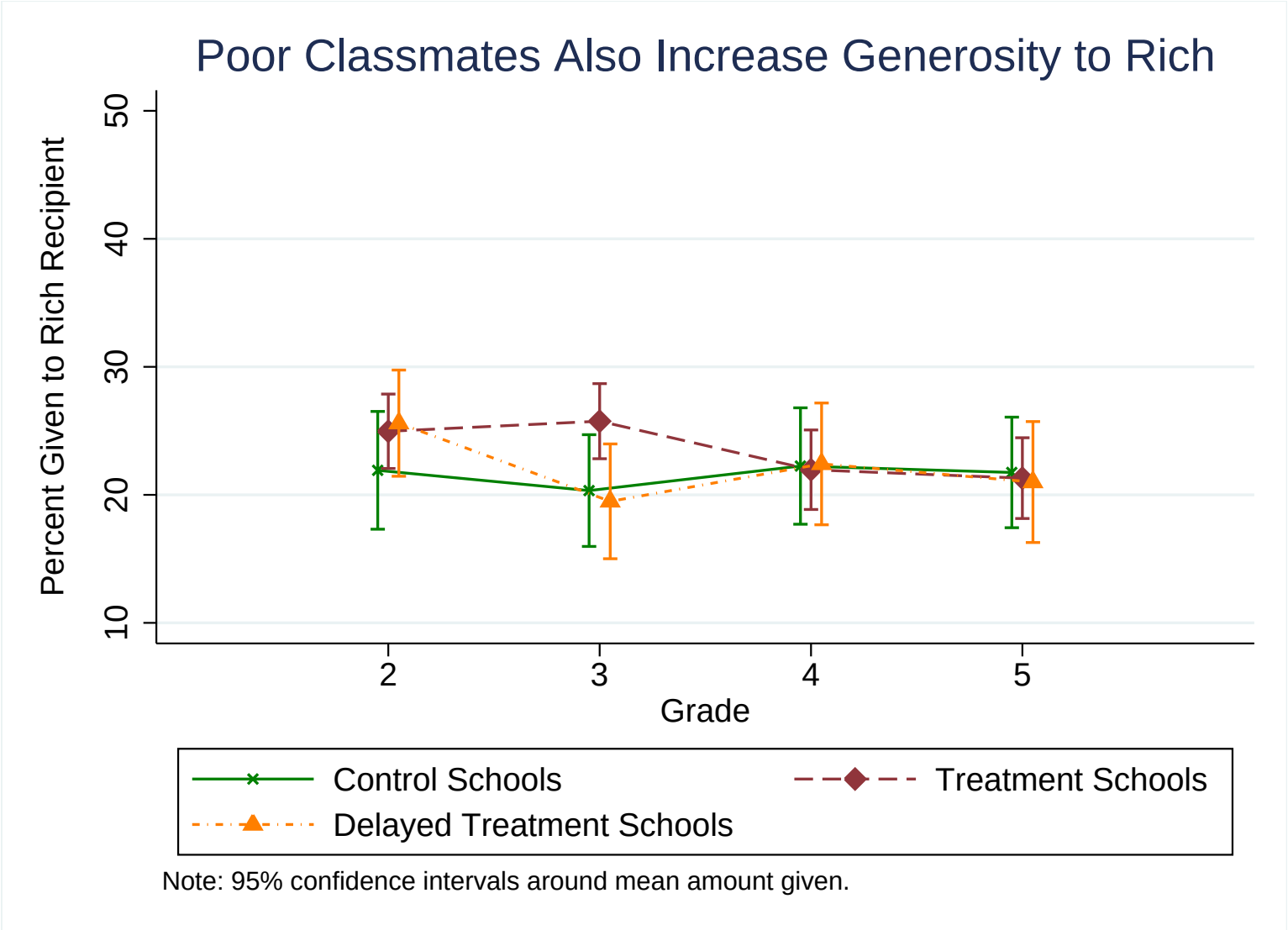
Dictator Game with Poor Recipient



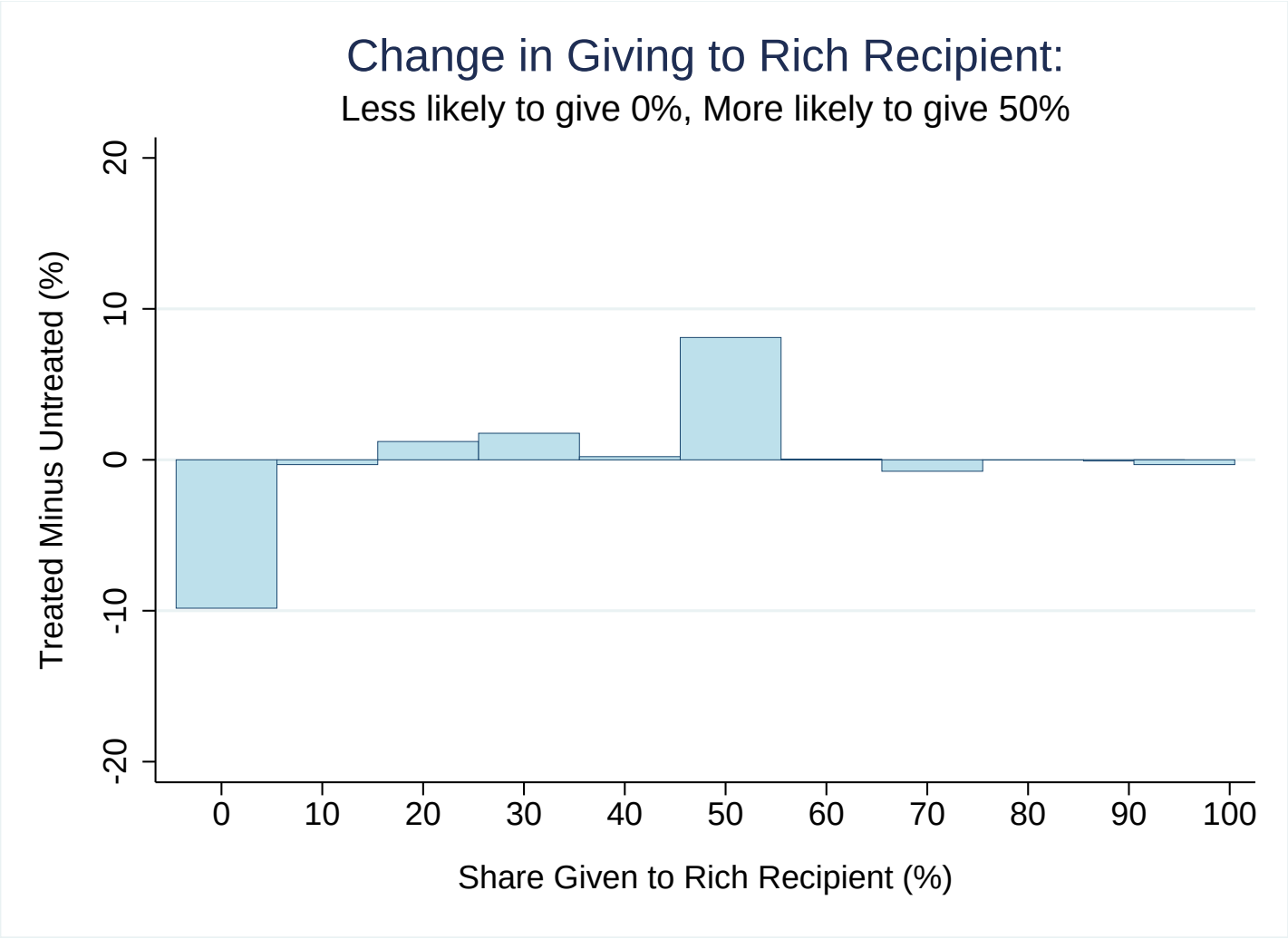
Dictator Game with Poor Recipient - Regressions

Dependent Variable: Share Given to Poor Recipient in Dictator Game (%)				
Specification:	(1) DiD	(2) DiD	(3) IV	(4) DiD+IV
Sample:	Full Sample	Younger Sibs	Treated Class	Full Sample
Treated Classroom	12.22***	12.95***		8.747**
	(1.901)	(2.274)		(3.510)
Has Poor Study Partner			7.53**	12.08***
			(3.147)	(4.313)
Controls	Yes	Yes	Yes	Yes
Fixed Effects	School, Grade	School, Grade	Classroom	School, Grade
p-value (CGM)	< 0.01	< 0.01	.	.
Control Mean	27.12	26.75	33.77	27.12
Control SD	27.22	26.53	28.13	27.22
N	2015	1141	677	2015

Dictator Game with Rich Recipient



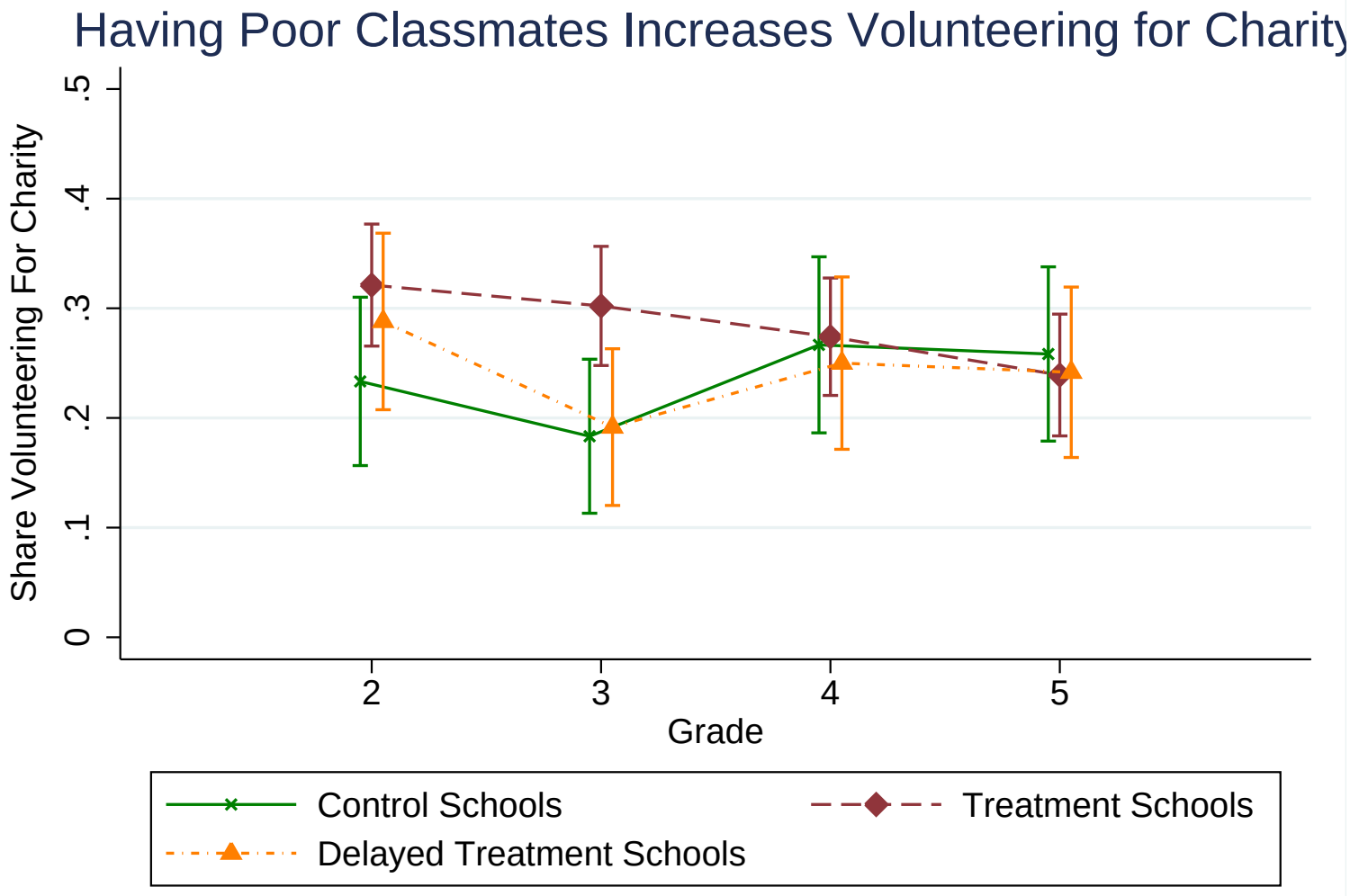
Changes in amounts given to rich recipients



Volunteering for charity

- Schools offer volunteer opportunity for charities
  - Spend two weekend afternoons in school to help fundraise for a children's NGO
- Participation is strictly voluntary
  - Only 28% of students participate
- Administrative data on attendance

Volunteering for charity



Note: 95% confidence intervals around mean amount given.

Field experiment on team selection

- Subjects are students from two elite private schools
  - One treatment school, one control school
  - We invite *athletic* poor students from a public school
- Students must choose teammates to run relay race
  - Tradeoff ability vs. social similarity
- $n = 342$

Team Selection Experiment Design

Stage 1: Randomization

- Randomized to sessions with varying stakes
  - Rs. 50, Rs. 200 or Rs. 500 per student for winning team
    - Rs. 500 ($10) approx. one month's pocket money
  - Variation in "price" of discrimination
- Brief mixing to judge socioeconomic status

Team Selection Experiment Design

Stage 2: Ability revelation and team selection

- Observe a 2-person race
  - Usually one poor and one rich student
    - Neither is from your school
  - Uniforms make school identifiable
- Pick which of the two runners you want as your partner
- Discrimination Picking the slower runner

Team Selection Experiment Design

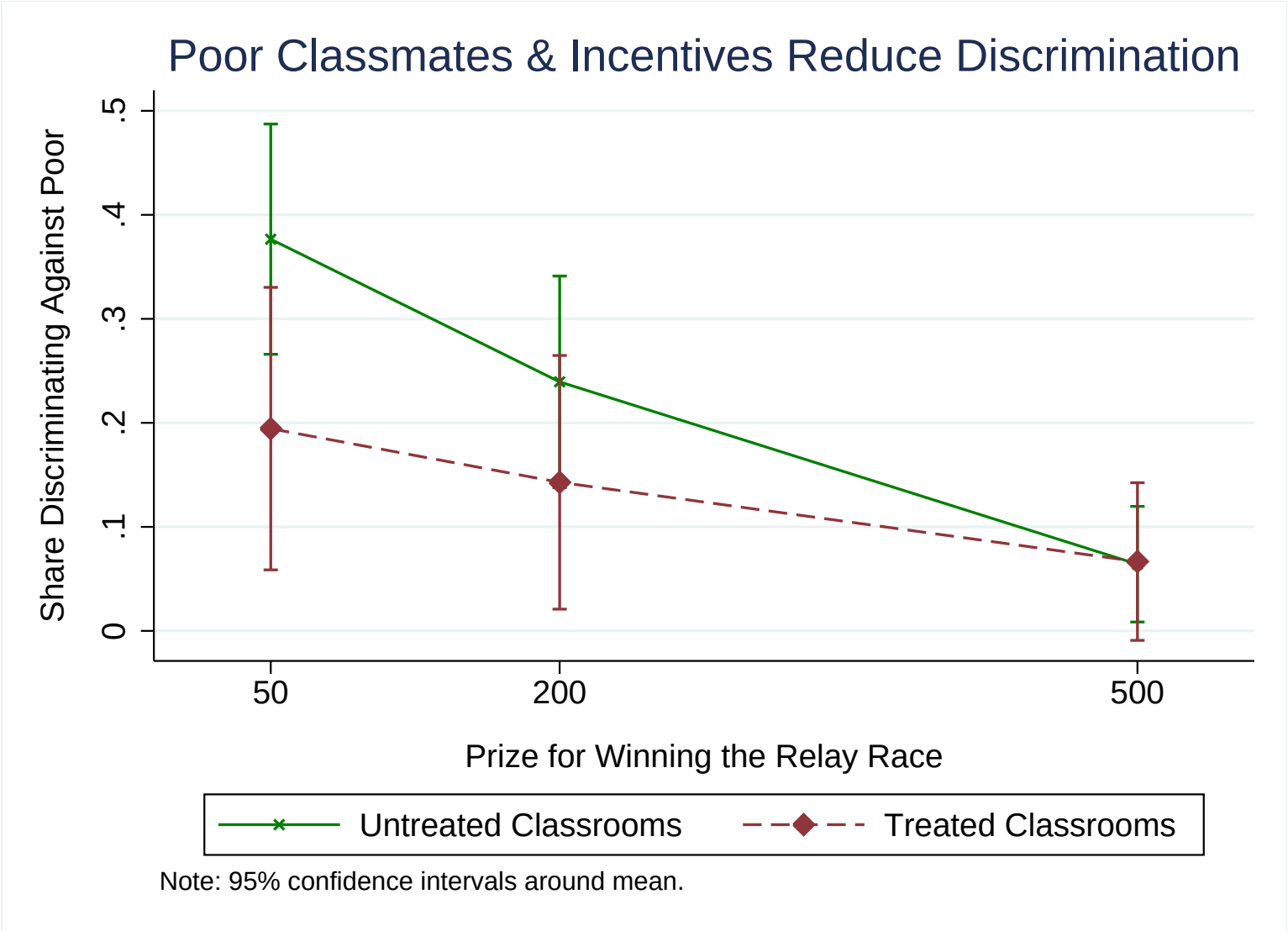
Stage 3: Choice implementation and relay race

- Students randomly picked to have their choices implemented
  - Plausible deniability provided
- Relay races held and prizes distributed as promised

Stage 4: Social interaction

- Must spend 2 hours playing with teammates
  - board games, sports, playground
- Was pre-announced

A quasi-demand curve for discrimination

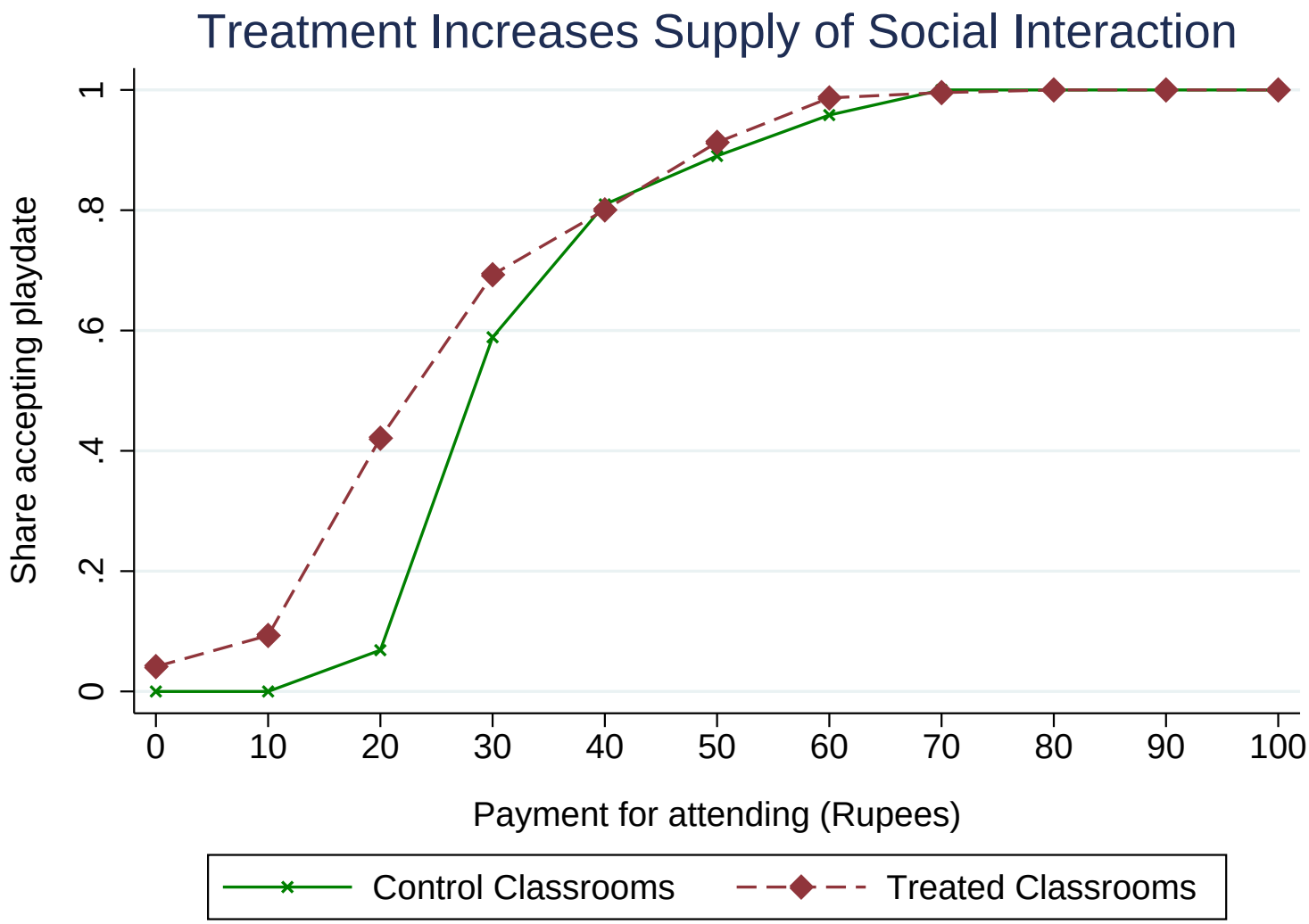


Willingness to Play Experiment

Invite students to a "play date" at poor school

- Opportunity to make new friends in neighborhood
- Elicit incentivized Willingness To Accept to attend
  - Using simple BDM mechanism
  - Students require payments to attend

Increase in supply of social interactions



◂ Play Date Tables

What part of the treatment is crucial?

- Personal interactions explain a lot of the overall effect
  - 70% of the change in "willingness to play"
  - 38% of the increase in giving to the poor
- Likely an underestimate of importance of interaction

Mechanisms

What's the mechanism? My speculation:

1. Interacting with poor children changes fairness notions
   - Makes students care more about equality of payoffs
   - Changes in preferences vs. norms / social image
2. Familiarity breeds fondness $\rightarrow$ discrimination $\downarrow$, socializing $\uparrow$
   - Change in prefs due to "mere-exposure"
   - Changes in beliefs
     - No effects on beliefs about niceness, intelligence, hard work.

Policy Relevance

- India-wide roll-out of this policy beginning in 2013-14
  - 400 million children under age 15
  - 30% of Indian students already attend private schools
  - Could have large-scale effects on social behaviors
    - Note unrepresentative sample

Section 4

Social Preferences Wave II: Warm Glow and Charitable Giving

Charitable Giving

- **Andreoni (2004, 2015).** Excellent survey of the theory and evidence
- Stylized facts:
  - US Giving large: 1.5 to 2.1 percent GDP
  - Most giving by individuals (Table 1)

Table 1 Sources of private philanthropy, 2011

Source of gifts	*Billions of dollars*	*Percent of total*
Individuals	217.8	73
Foundations	41.7	14
Bequests	24.4	8
Corporations	14.6	5
Total for all sources	298.5	100

Source: Giving USA, annual report 2012.

Giving over time

- Giving fairly constant over time (Figure 1)

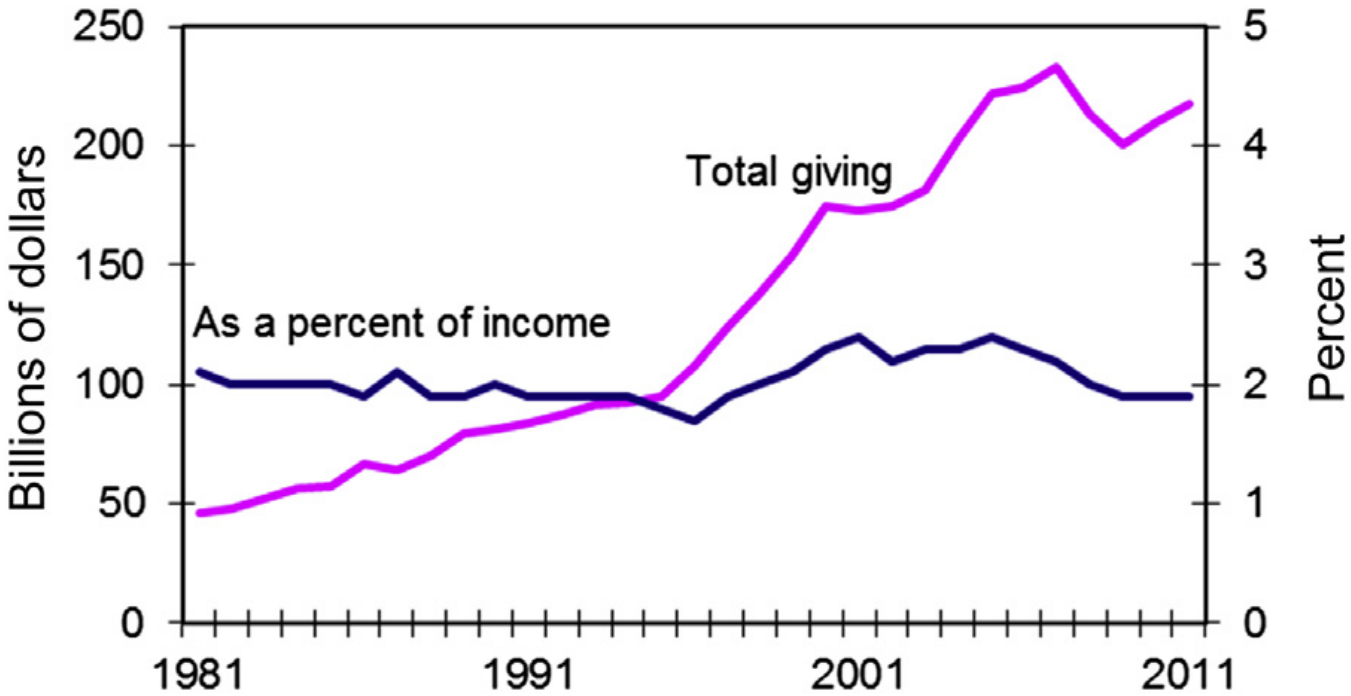


Figure 1 Giving by individuals, 1981–2011. Dollars are inflation-adjusted to 2011 values. Source: Giving USA, annual report 2012.

Pure Altruism

- Charitable giving important phenomenon – How do we understand it?
- **Model 1.** Pure altruism: Model utility of Self giving g_s

$$u\left(w_s - g_s\right) + \alpha f\left(G_{-s} + g_s\right)$$

where G_{-s} is giving by others, $f\left(\right)$ is production function of charity

- F.o.c.

$$-u'\left(w - g_s^*\right) + \alpha f'\left(G_{-s} + g_s^*\right) = 0$$

- More giving if more altruistic

$$\frac{\partial g_s^*}{\partial \alpha} = -\frac{f'\left(G_{-s} + g_s^*\right)}{u''\left(w - g_s\right) + \alpha f''\left(G_{-s} + g_s^*\right)} > 0$$

Pure Altruism

- How would giving change if giving by others (or by government) increases?

$$\frac{\partial g_s^*}{\partial G_{-s}} = -\frac{\alpha f''\left(G_{-s} + g_s^*\right)}{u''\left(w - g_s\right) + \alpha f''\left(G_{-s} + g_s^*\right)} \approx -1$$

- Prediction of strong crowd out of giving
  - If government spends on income of needy group, corresponding almost one-on-one decrease in giving
  - Evidence of crowding out: Limited crowd-out
- Problem (ii): Model predicts giving to one highest-value charity—Instead we observe dispersion across charities
- Problem (iii): In-person or phone requests for giving raise much more than impersonal requests (mail)

Warm Glow

- **Andreoni (1994)**: Warm-Glow or Impure altruism.
  - Utility
$$u(w_s - g_s) + av(g_s)$$
  - Agent gets warm glow utility $v(g_s)$ directly from giving
  - Utility $v(g_s)$ sharply concave
  - F.o.c.
$$-u'(w - g_s^*) + av'(g_s^*) = 0$$

- Predicts:
  - No crowd-out
$$\frac{\partial g_s^*}{\partial G_{-s}} = -\frac{0}{u''(w - g_s) + av''(g_s^*)} = 0$$
  - Small giving to several charities if $v(g)$ is charity specific
  - Glow can be higher for in-person requests (raises a)

Warm Glow

- Warm-Glow models a la Andreoni (1994) used to understand charitable giving and social preferences
- BUT: harder to extrapolate across contexts and quantity
  - How to understand warm glow function v?
  - How do we extrapolate warm glow parameter a?
- Progress (see later) using social signalling models

Section 5

Social Preferences Wave III: Inequity Aversion and Reciprocity

Charness-Rabin (QJE, 2002)

- Simplified model of preferences of s (self) when interacting with o (other):

$$\begin{aligned}(1-\rho)x_s + \rho x_o \text{ if } x_s &> x_o \\ (1-\sigma)x_s + \sigma x_o \text{ if } x_s &< x_o.\end{aligned}$$

- Captures:
  - selfishness ($\rho = \sigma = 0$)
  - baseline altruism (if $\rho = \sigma > 0$)
  - full altruism ($\rho = \sigma = 1/2$)
  - differentially so if ahead or behind ($\rho > \sigma$)
  - inequity aversion (**Fehr-Schmidt QJE, 1999,** $\rho > 0 > \sigma$)

Dictator Game: Forsythe et al. (1994)

- Dictator Game. Have \$10 and have to decide how to share
- **Forsythe et al. (GEB, 1994)**: sixty percent of subjects transfers a positive amount.
- Transfer \$5 if

$$\begin{aligned} \rho 5 + (1-\rho)5 &= 5 \geq \rho 0 + (1-\rho)10 -> \rho \geq 1/2 \text{ and} \\ \sigma 5 + (1-\sigma)5 &\geq \sigma 10 + (1-\sigma)0 -> \sigma \leq 1/2 \end{aligned}$$

- Transfer \$5 if

$$\begin{aligned} \rho &\geq .5 -> \text{Prefer giving \$5 to giving \$0} \\ .5 &\geq \sigma -> \text{Prefer giving \$5 to giving \$10} \end{aligned}$$

- Dictator game behavior consistent with inequity aversion
- Number of other experiments also consistent (including gift exchange)

Challenges

- Taking this to field data? Hard
- Issue 1:
  - Person s with disposable income M_s meets needy person o with income $M_o < M_s$
  - Person s decides on donation D
  - Assume parameters $\rho \geq .5 \geq \sigma$
  - This implies $\pi_s^* = \pi_s^* \rightarrow M_o - D^* = M_s + D^* \rightarrow D^* = (M_s - M_o)/2$
  - Wealthy person transfers half of wealth difference!
  - Clearly counterfactual

Challenges

- Issue 2:
  - Lab: n subjects, with n small
  - Field: Millions of needy people. Public good problem
- Issue 3:
  - Lab: Forced interaction.
  - Field: Sorting – can get around, or look for, occasions to give

Challenges

- In addition to payoff-based social preferences, intentions likely to matter
  - ρ and σ higher when s treated nicely by o
  - Model intentions of o
  - Positive reciprocity: Respond to being treated nicely
  - Negative reciprocity: Respond to being treated unfairly
  - More evidence of the latter in lab experiments

Section 6

Workplace Effort: Inequity Aversion

Social Comparisons in the Workplace

- Workers compare to co-workers
  - Get some utility from being paid more than others
  - Get high disutility from being paid less then others (inequity aversion)
  - $\rightarrow$ Wage compression
- Is there evidence of this?

Card-Mas-Moretti-Saez (AER 2012)

- Study of job satisfaction for UC employees
  - Examine the impact of salary comparisons
- UC is ideal setting:
  - Salaries are public
  - But not as easy to access
  - Sacramento Bee posted them online

Design

- Email survey to staff at various University of California Campuses
- Field experiment on content of survey
- Mention to some, but not others, the website of the Sacramento Bee: "*Are you aware of the web site created by the Sacramento Bee newspaper that lists salaries for all State of California employees? (The website is located at www.sacbee.com/statepay, or can be found by entering the following keywords in a search engine: Sacramento Bee salary database).*"
- Counting on human curiosity for first stage...
- Follow-up survey to measure job satisfaction and interest in moving to other job
- Impact on stated job satisfaction and reported intention to look for new job

Results

Table 4: Effect of Information Treatment on Measures of Job Satisfaction

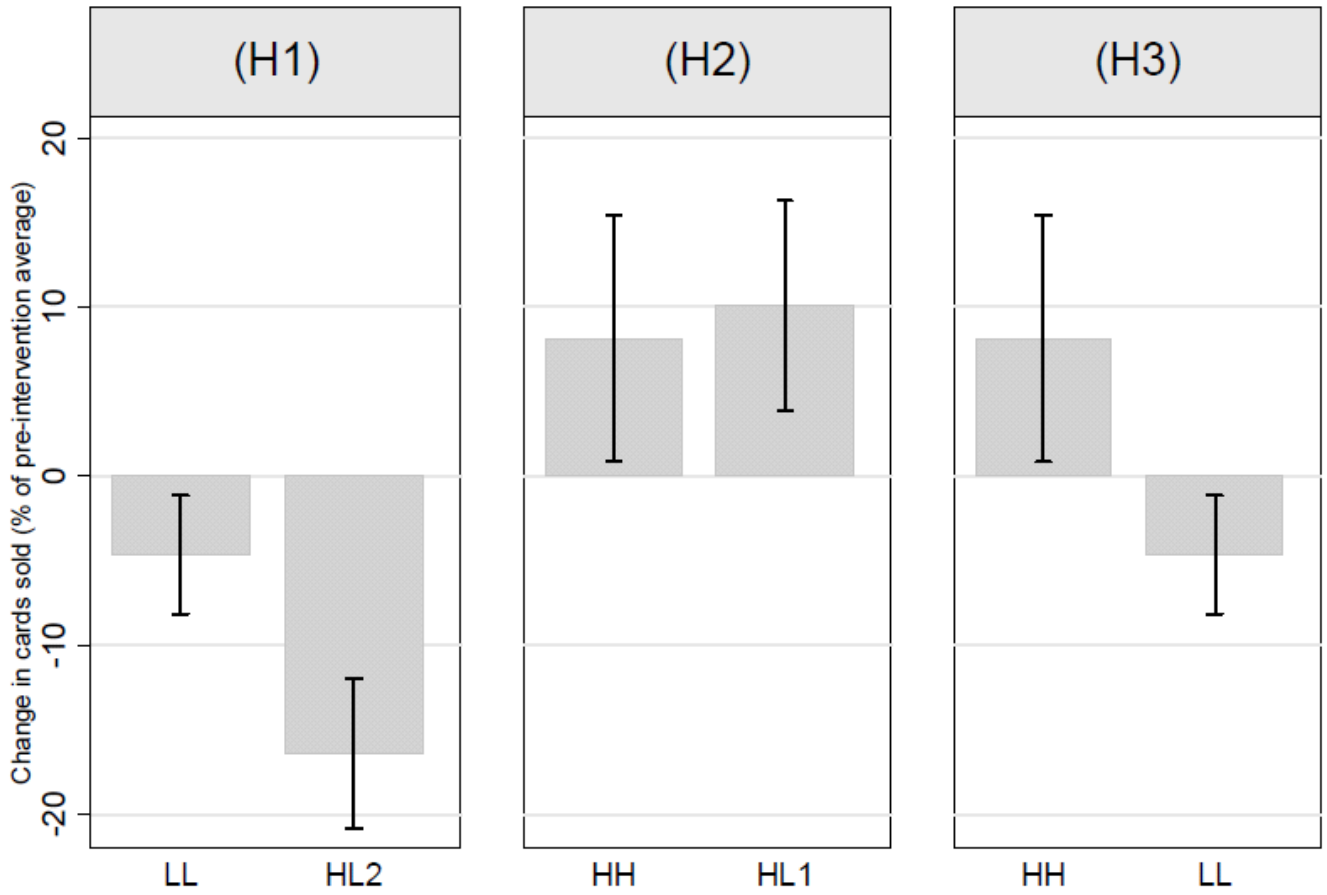
	Satisfaction Index (10 point scale)			Reports Very likely to Look for New Job (Yes = 1)			Dissatisfied and Likely Looking for a New Job (Yes = 1)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Treated individual	-2.0 (2.2)	--	--	1.0 (1.2)	--	--	2.0 (1.1)	--	--
I. Treated individual with earnings ≤ median pay in unit	--	-6.3 (2.9)	--	--	4.3 (1.8)	--	--	5.2 (1.8)	--
II. Treated individual with earnings > median pay in unit	--	2.0 (2.6)	2.2 (2.6)	--	-2.0 (1.6)	-2.0 (1.6)	--	-0.9 (1.3)	-0.9 (1.3)
II-I	--	8.3 (3.5)	--	--	-6.3 (2.4)	--	--	-6.1 (2.1)	--
Treated × earnings in first quartile in pay unit	--	--	-15.0 (4.0)	--	--	8.0 (2.6)	--	--	8.1 (2.4)
Treated × earnings in second quartile in pay unit	--	--	1.9 (3.9)	--	--	0.8 (2.5)	--	--	2.5 (2.3)
P-value for exclusion of treatment effects	0.36	0.05	0.00	0.85	0.03	0.01	0.08	0.01	0.00
Mean of the dependent variable in the control group [standard deviation]		274.2 [66.1]			21.9 [41.4]			12.9 [33.5]	

Notes: All models are estimated by OLS. All coefficients and means are multiplied by one hundred. Standard errors, clustered by campus/department, are in parentheses (818 clusters for all models). "Earnings" refers to total UC payments in 2007. Pay unit refers to the respondent's department or administrative unit. Median pay is computed seperately for faculty and staff. The satisfaction index is the average of responses for the questions: "How satisfied are you with your wage/salary on this job?", "How satisfied are you with your job?", and "Do you agree or disagree that your wage is set fairly in relation to others in your department/unit?". Responses to each of these questions are on a 1-4 scale and are ordered so that higher values indicate greater satisfaction. The variable "Dissatisfied and Likely Looking for a New Job" is 1 if the respondent is below the median value of the satisfaction index and reports being "very likely" to make an effort to find a new job. See text and Appendix Table A3 for further details on the construction of the dependent variables. In addition to the explanatory variables presented in the table, all models include controls for campus × (staff/faculty), a cubic in earnings, and main effects. The sample size is 6,411.

Cohn, Fehr, Herrmann, Schneider (JEEA 2014)

- Workers hired in pairs to sell cards
- On second work day, pay randomly made different
- 25% pay cut for both workers, or only one worker
- Effect on effort?

Effect on Effort



(error bars represent standard error of the mean; spare workers excluded)

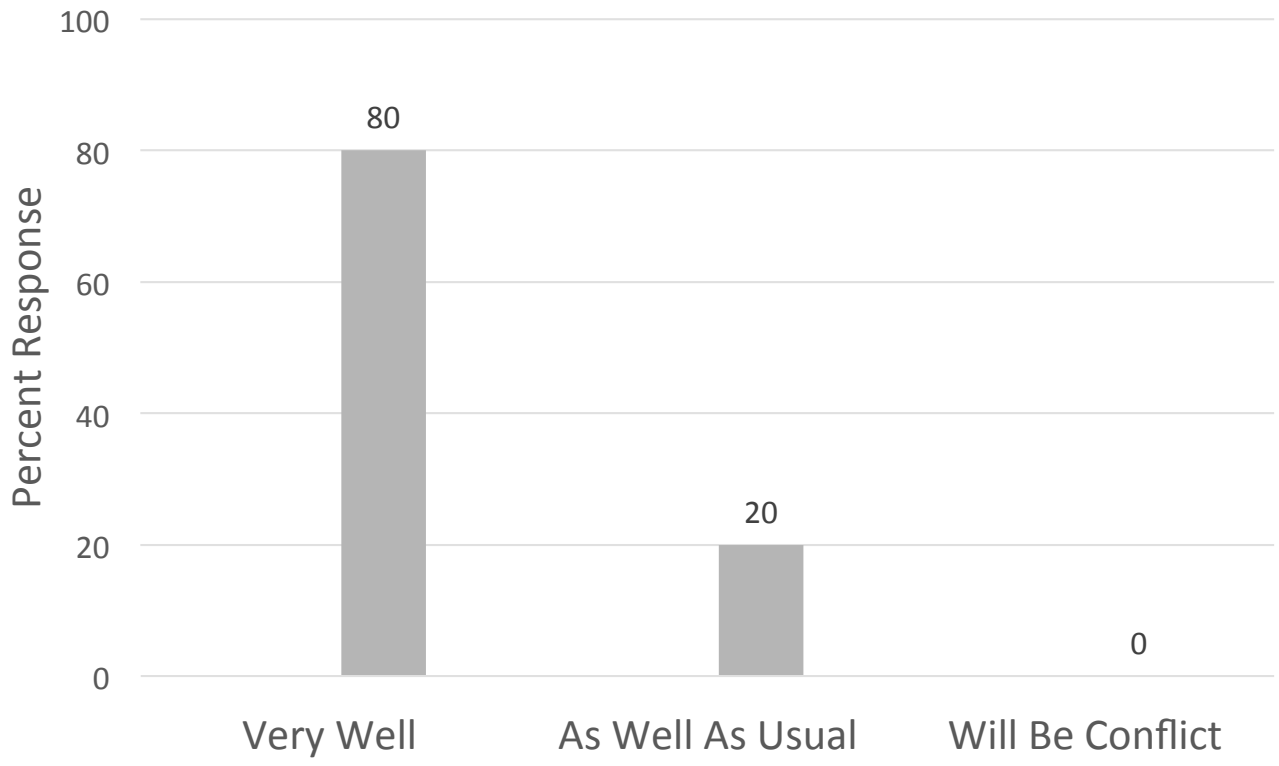
- Notice: Return to gift exchange next lecture

Breza, Kaur, Shamdasani (forthc.)

- Experiment randomizing pay comparisons, as well as reasons
- Slides courtesy of Supreet

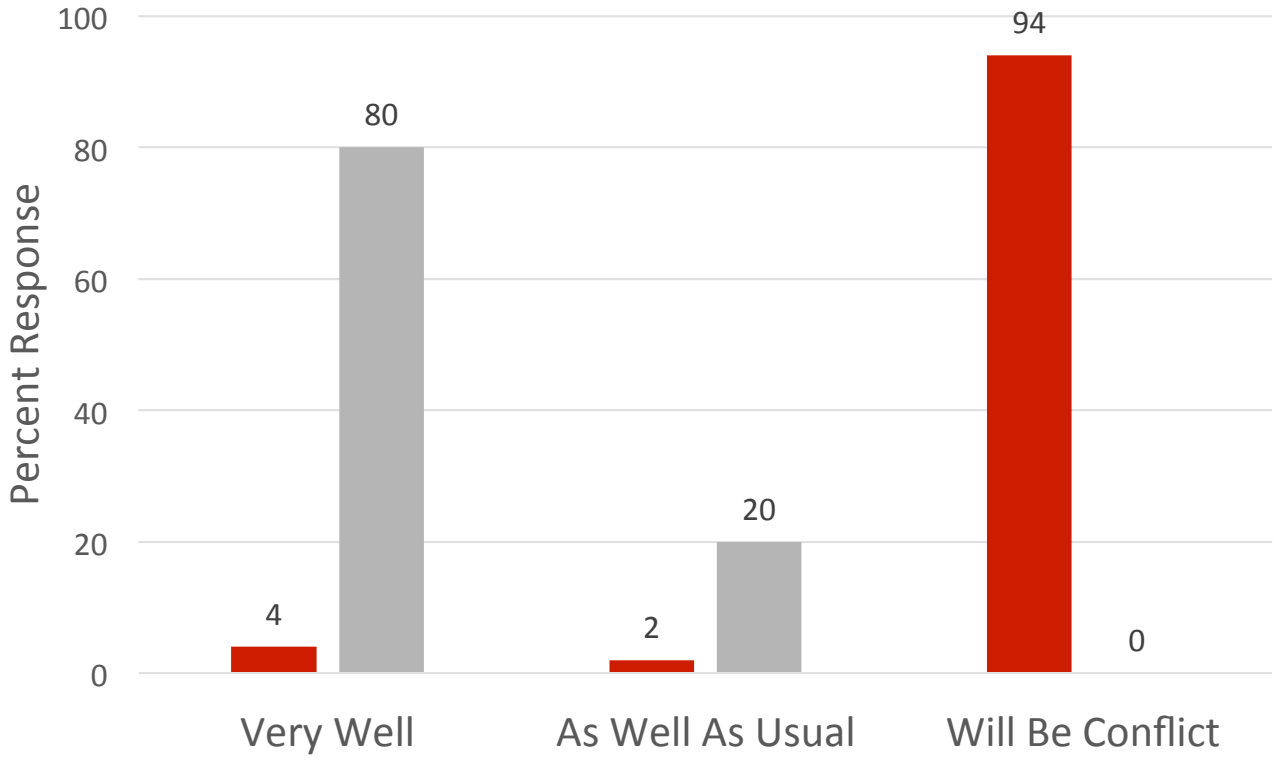
Motivation: Relative Pay Concerns

3 people from a village get hired to work on a construction site together. The prevailing wage is Rs. 250. The contractor pays them Rs. 250/day. How well will they work together?



Motivation: Relative Pay Concerns

3 people from a village get hired to work on a construction site together. The prevailing wage is Rs. 250. The contractor pays them different wages, based on their strength: Rs. 250/day, Rs. 270/day, and Rs. 290/day. How well will they work together?



Preview

Field experiment with manufacturing workers over 1 month period

- Workers paid fixed wage for attendance
- Compare workers who earn same absolute wage, but differ in peers' wages

Effects of Pay Disparity:

- Being paid less than your peers:
  - Output decrease of 0.33 standard deviations (22%)
  - Attendance decrease of 12% (leaving 9% of earnings on table)
- Attendance decreases also for higher-paid and median earners
- Decrease in team cohesion / cooperation (endline teamwork tasks)
- Negative morale effects disappear when pay difference is clearly justified
  - Productivity differences are easy to observe
  - Differences in productivity swamp differences in pay

Distinguish from:

- Career concerns / learning
- Peer effects

Framework (adapted from DellaVigna et al 2015)

Worker i receives wage offer w_i from firm, and chooses:
(i) whether to work
(ii) level of effort (incomplete contracting).

Outside option (from not working): $R_{it} \equiv R_i + \varepsilon_{it}$

Payoff from working:

$$V\left(w_i, \mathbf{w_{-i}}\right) = w_i - c\left(e_i\right) + M\left(w_i, \mathbf{w_{-i}}\right) e_i$$

where:

e_i = effort level chosen, where $e_i \geq 0$
$c(\cdot)$ = convex effort cost
$\mathbf{w_{-i}}$ = wages of co-workers (peers)
$M\left(w_i, \mathbf{w_{-i}}\right)$ = morale effect

Framework

Conceptualize relative pay concerns as reference dependence

$$V\left(w_i, \mathbf{w_{-i}}\right) = w_i - c\left(e_i\right) + M\left(w_i, \mathbf{w_{-i}}\right) e_i$$

$$M\left(w_i, \mathbf{w_{-i}}\right) = -\alpha f\left(w_i - w_R \mid w_i < w_R\right) + \beta f\left(w_i - w_R \mid w_i > w_R\right) + g\left(w_i\right)$$

where:

w_R determined by peer wages & productivity (i.e., $w_R \equiv r\left(\mathbf{w_{-i}}, \theta\right)$)
α reflects utility effect of being paid less than w_R
β reflects utility effect of being paid more than w_R

Note: α and β are reduced form – reflect own preferences & social dynamics

Predictions

- Changes in $1_{w_i<w_R}$ and $1_{w_i>w_R}$ will affect both labor supply & effort
- Direction of effects reveal signs of α and β
  - Under most formulations/theories: $\alpha < 0$
  - Sign of β ambiguous (loss aversion, inequity aversion, social undermining)

Context

- Low-skill manufacturing
  - Rope, brooms, incense sticks, candle wicks, plates, floor mats, paper bags…
  - Factory sites in Orissa, India
  - Partner with local contractors (set training and quality standards)
  - Output sold in local wholesale market

- Workers employed full-time over one month
  - Seasonal contract jobs (common during agri lean seasons)
  - Primary source of earnings

- Flat daily wage for attendance
  - Typical pay structure in area

- Sample
  - 378 workers
  - Adult males, ages 18-65
  - All have experience with flat daily wages; 45% have worked under piece rates

Experiment Design

Construct design to accomplish 3 goals:

1. **Clear reference group for each worker**

2. Variation in co-worker pay, holding fixed own pay

3. Variation in perceived justification for pay differences

1. Reference Group = Product Team

- Teams of 3 workers each
- All team members produce *same* product
- Each team within factory produces *different* product
  - E.g. Team 1 makes brooms, Team 2 makes incense sticks, …
- Factory structure
  - 10 teams in each factory
  - 10 products: brooms, incense sticks, rope, wicks, plates, etc.
- Note: Individual production
  - Hire staff to measure worker output after each day

Experiment Design

Construct design to accomplish 3 goals:

1. Clear reference group for each worker

2. **Variation in co-worker pay, holding fixed own pay**

3. Variation in perceived justification for pay differences

Wage Treatments

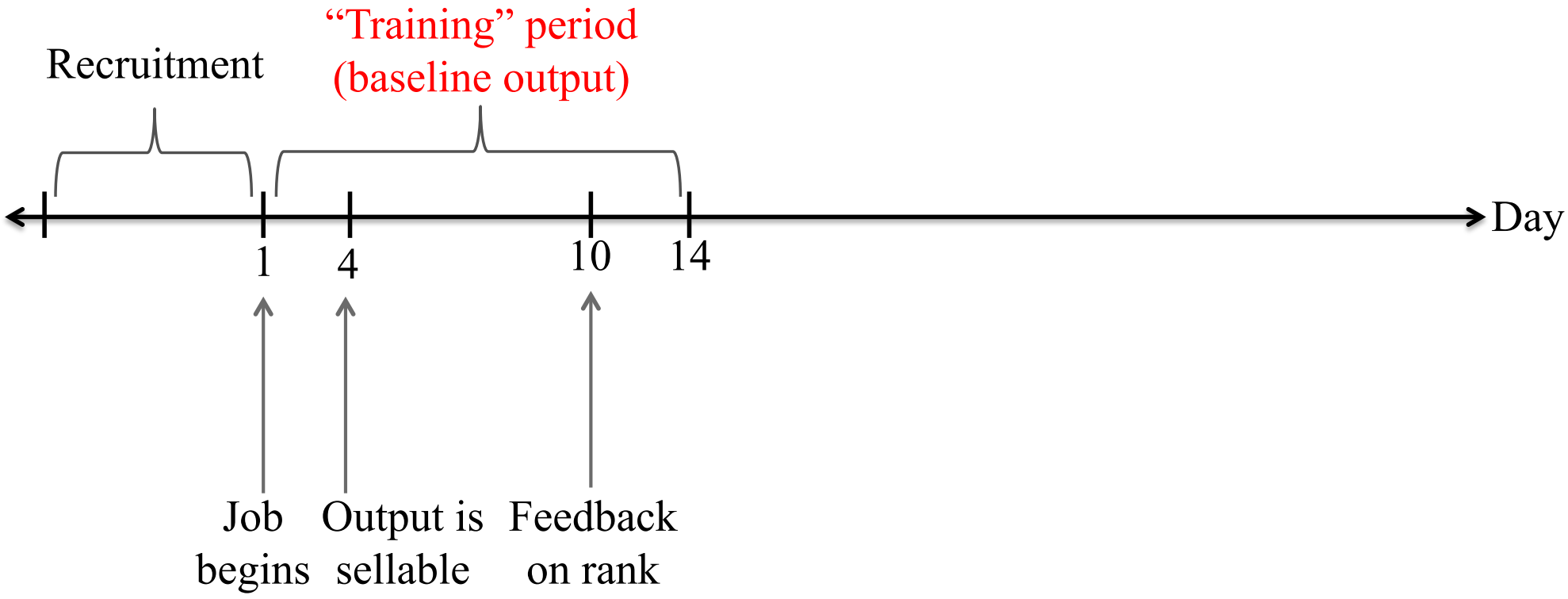
Design: Wage Treatments

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}

- Expect $w_i < w_R$

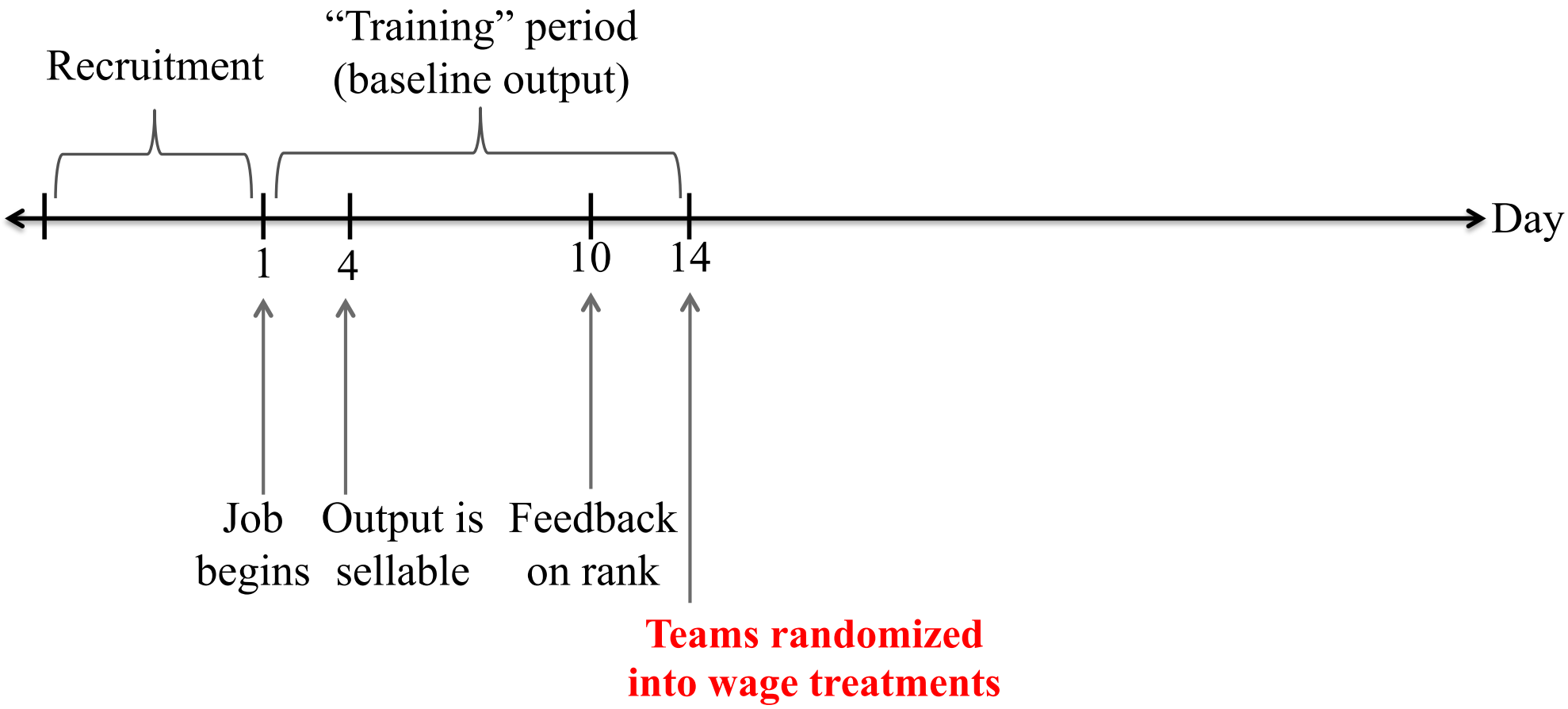
- Predictions
  - H_0: $\alpha = 0$: same output
  - H_1: $\alpha < 0$: output lower under Heterogeneous pay

Timeline for Each Round



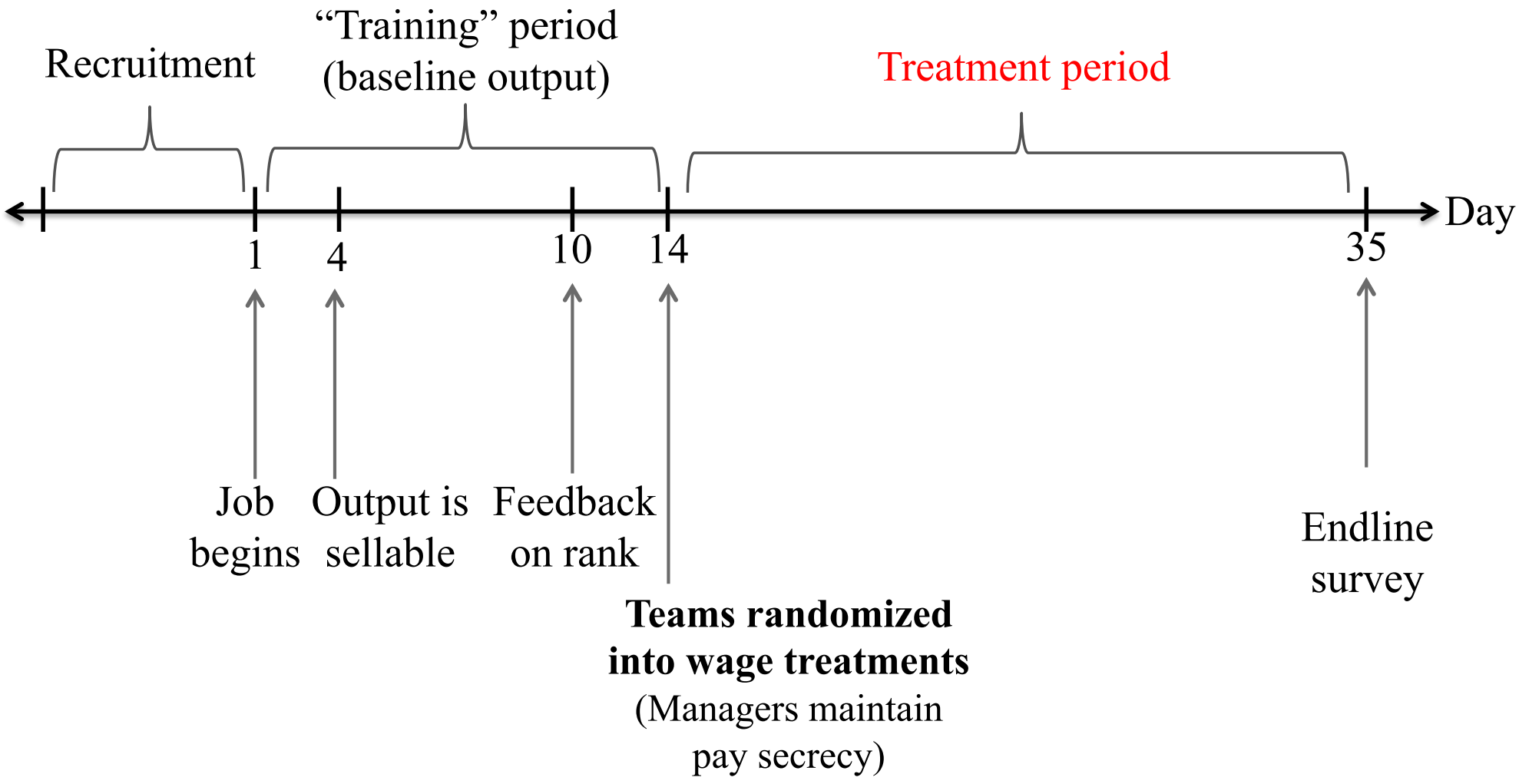
- Training: all workers receive same training wage
  - Set to prevailing wage in area (outside option)
- Day 1: workers are told post-training wage may depend on baseline productivity

Timeline for Each Round



- All workers get a pay <u>increase</u> of 5-15% (wages above outside option)
- Each worker privately told his individual wage
  - Managers maintain pay secrecy

Timeline for Each Round



Experiment Design

Construct design to accomplish 3 goals:

1. Clear reference group for each worker

2. Variation in co-worker pay, holding fixed own pay

3. **Variation in perceived justification for pay differences**
   - **2 tests**

Justifications I: Productivity differences

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}

- Productivity is continuous
- Discrete fixed differences in wages

→ Variation in { ΔWage / ΔProductivity } among co-workers

Justifications II: Observability

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}

- 10 production tasks
- Differ in observability of co-worker output
  - Quantify task observability at baseline
- Stratify treatment assignment by task (across rounds)

Outline

- (Brief) Framework
- Experiment Design
- **Estimation & Results**
  - Wage treatments
  - Perceived justifications
  - Team cohesion (endline teamwork tasks)
  - Endline survey responses – beliefs & perceptions
  - Gift exchange (if time)
- Alternate Explanations?

Did workers learn co-worker wages?

Use endline survey to verify knowledge of co-worker wages

	Compressed Teams	Heterogeneous Teams	P-value of Difference
Panel A: Own Team			
Indicator for knows correct wage of both coworkers	0.909	0.742	0.003***
	(0.288)	(0.440)	
Indicator for knows correct wage of at least one coworker	0.958	0.871	0.031**
	(0.200)	(0.337)	
Indicator for has no opinion of wages of either coworker	0.034	0.075	0.206
	(0.181)	(0.265)	
Indicator for has no opinion of wages of both coworkers	0.079	0.129	0.203
	(0.271)	(0.337)	
Panel B: Other Teams			
Indicator for knows correct wage of all team members	0.086	0.019	0.000***
	(0.280)	(0.138)	
Indicator for has no opinion of wages of any team member	0.883	0.857	0.237
	(0.321)	(0.351)	
Indicator for has no opinion of wages of all team members	0.866	0.826	0.070*
	(0.341)	(0.380)	

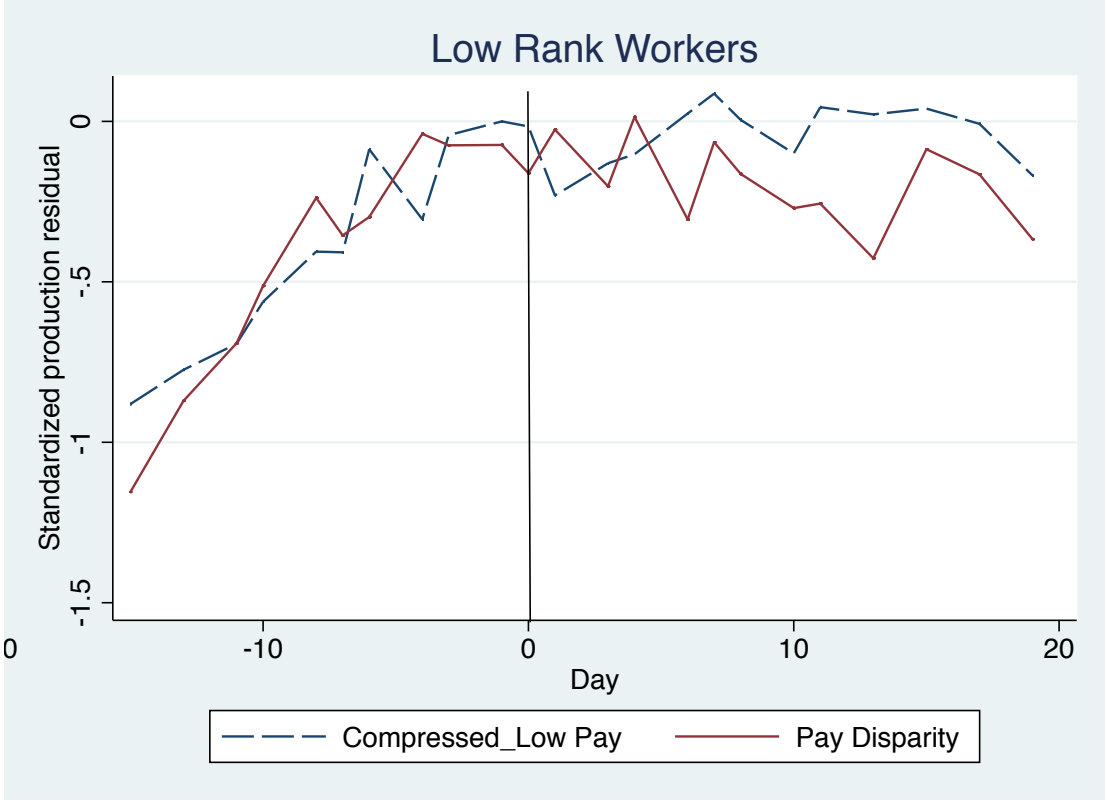
Measurement

- Production = 0 when workers are absent

- Pooling across tasks
  - 10 production tasks
  - Standardize output within each task (using mean and standard deviation in baseline period)
  - Enables pooling across tasks using a consistent unit (standard deviations)

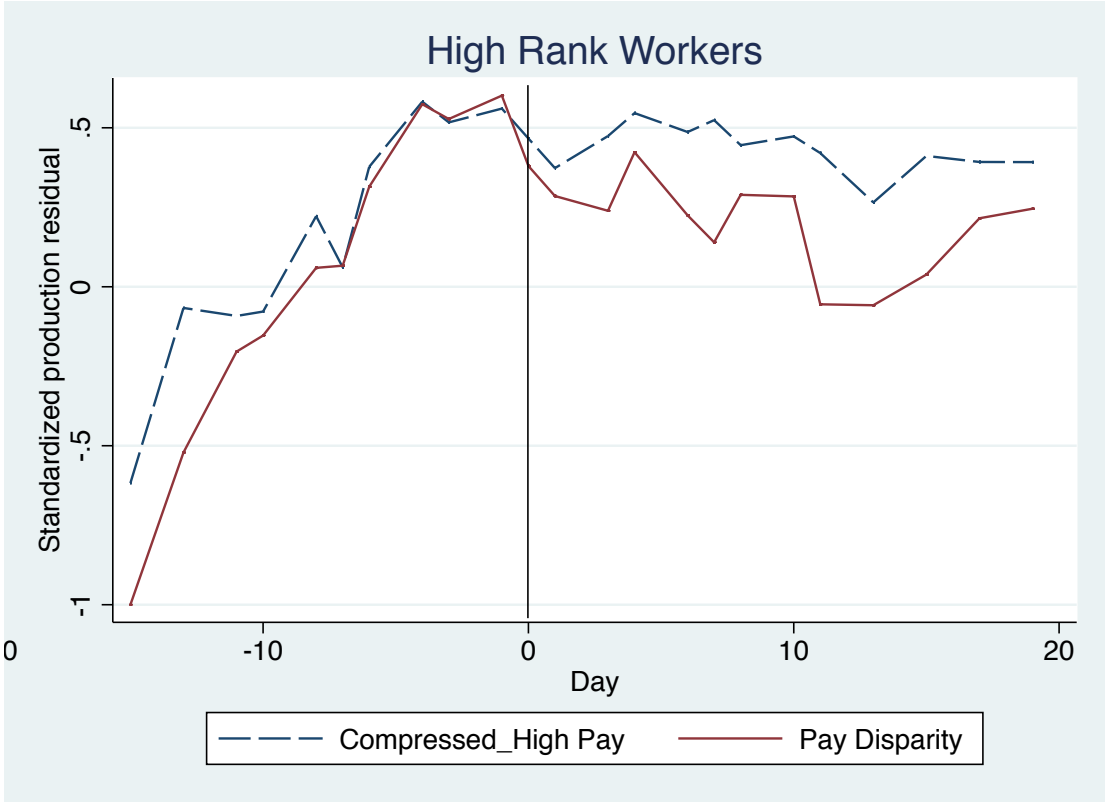
Effects of Relative Pay Differences

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}



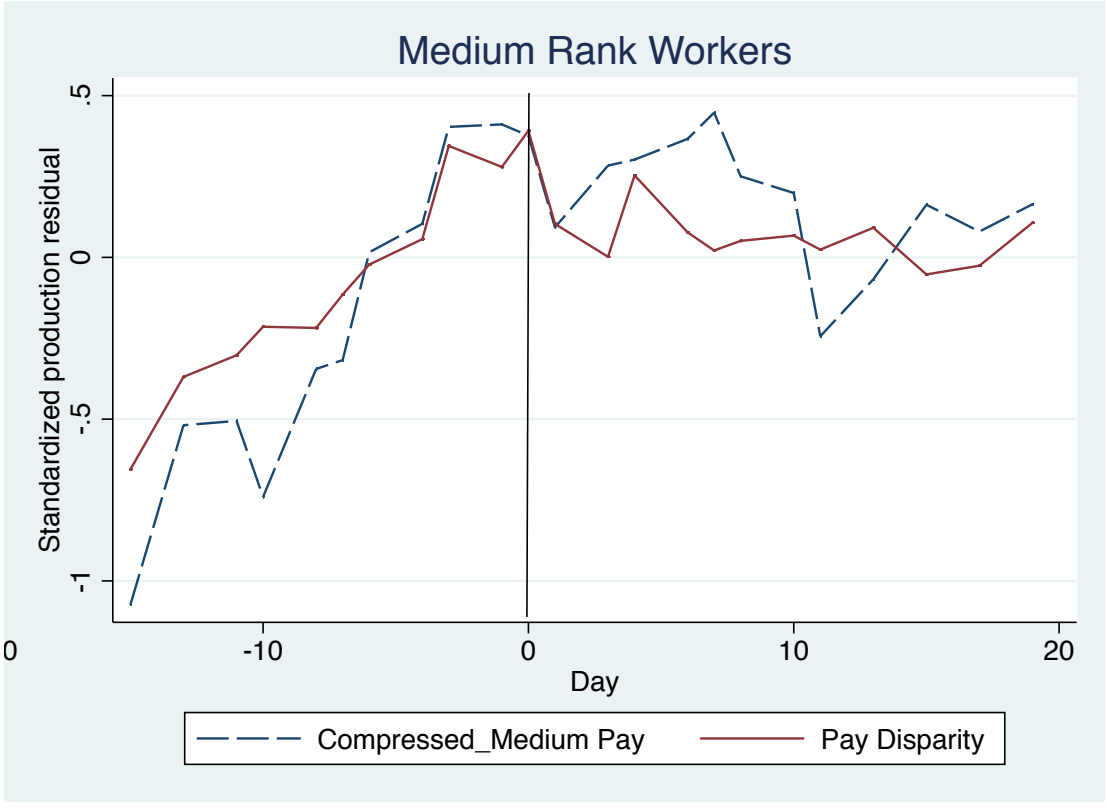
Effects of Relative Pay Differences

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}



Effects of Relative Pay Differences

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}



Empirical Specification

Worker Rank	Heterogeneous	Compressed_L	Compressed_M	Compressed_H
Low productivity	w_{Low}	w_{Low}	w_{Medium}	w_{High}
Medium productivity	w_{Medium}	w_{Low}	w_{Medium}	w_{High}
High productivity	w_{High}	w_{Low}	w_{Medium}	w_{High}

Include other ("irrelevant") cells to help estimate fixed effects:

$$
\begin{aligned}
y_{it} = {} & \alpha_1 Post_t \times Het_i \times RankL_i + \alpha_2 Post_t \times Het_i \times RankM_i + \alpha_3 Post_t \times Het_i \times RankH_i \\
& \alpha_4 Post_t \times RankL_i + \alpha_5 Post_t \times RankM_i + \alpha_6 Post_t \times RankH_i \\
& + \mathbf{\bar{Irrel}'_{it}}\theta + \mathbf{\bar{Neigh}'_{it}}\gamma + \lambda_i + \tau_t + \varepsilon_{it}
\end{aligned}
$$

- Irrelevant=1 for non-relevant comparison cells.
  - Add full interactions with rank*post - no change in value of main coefficients.
  - Results robust to excluding irrelevant observations
- Neighbor=1 if team had another team in same production room
  - Add full interactions with rank*post - coefficients identified off teams with no neighbors.
  - Results robust to excluding neighbor controls

Effects of Relative Pay Differences

	Output (std dev.) (1)	Output (std dev.) (2)	Attendance (3)	Attendance (4)
Post x Pay disparity x Low wage	-0.385	-0.332	-0.113	-0.120
	(0.134)***	(0.128)***	(0.055)**	(0.053)**
Post x Pay disparity x Med wage	-0.262	-0.226	-0.126	-0.129
	(0.201)	(0.187)	(0.056)**	(0.060)**
Post x Pay disparity x High wage	-0.288	-0.172	-0.106	-0.104
	(0.199)	(0.181)	(0.076)**	(0.052)**
Individual fixed effects?	No	Yes	No	Yes
Post-treatment Compressed Mean	-0.099	-0.099	0.939	0.939
N	8375	8375	8375	8375

- Low relative pay:
  - 22% reduction in output
  - 12.7% reduction in attendance
  - Leave 9% of earnings on the table (endline data on overall earnings)
- Attendance declines for all workers

Effects of Relative Pay Differences

	Output (std dev.) (1)	Output (std dev.) (2)	Attendance (3)	Attendance (4)	Output | Attendance (5)
Post x Pay disparity x Low wage	-0.385	-0.332	-0.113	-0.120	-0.204
	(0.134)***	(0.128)***	(0.055)**	(0.053)**	(0.114)*
Post x Pay disparity x Med wage	-0.262	-0.226	-0.126	-0.129	-0.061
	(0.201)	(0.187)	(0.056)**	(0.060)**	(0.114)
Post x Pay disparity x High wage	-0.288	-0.172	-0.106	-0.104	-0.009
	(0.199)	(0.181)	(0.076)**	(0.052)**	(0.152)
Individual fixed effects?	No	Yes	No	Yes	No
Post-treatment Compressed Mean	-0.099	-0.099	0.939	0.939	0.015
N	8375	8375	8375	8375	7678

- Can attendance account for the full output decline among lower paid workers?
- Naïve back of envelope:
  - Attendance accounts for 60% of L-rank effect
  - Attendance fully accounts for effects on M and H rank workers

Perceived Justifications I: Productivity Differences

- Difference in pre-period output between yourself and your higher-paid peer (for L and M rank)

- Indicator for being above mean difference
  - Corresponds to 0.375 standard deviations

Perceived Justifications I: Productivity Differences

	Definition of Perceived Justification Indicator	
	Large baseline productivity difference between co-workers	
Dependent variable	Output	Attendance
	(1)	(2)
Panel A — Pooled Treatment Effects		
Post x Pay disparity	-0.358***	-0.167***
	(0.133)	(0.039)
Post x Pay disparity x Perceived justification	0.292*	0.159**
	(0.173)	(0.061)
Post x Perceived justification	0.0483	-0.0500
	(0.107)	(0.032)
Panel B — Treatment Effects Separately by Rank		
Post x Pay disparity x Low wage	-0.448***	-0.168***
	(0.147)	(0.061)
Post x Pay disparity x Low wage x Perceived justification	0.467**	0.181**
	(0.231)	(0.087)
Post x Pay disparity x Med wage	-0.270	-0.170**
	(0.224)	(0.075)
Post x Pay disparity x Med wage x Perceived justification	0.127	0.150
	(0.267)	(0.094)
Number of observations (worker-days)	8375	8375

Higher paid peer close in productivity
→ pay disparity lowers performance

Perceived Justifications I: Productivity Differences

Dependent variable	Definition of Perceived Justification Indicator: Large baseline productivity difference between co-workers — Output (1)	Definition of Perceived Justification Indicator: Large baseline productivity difference between co-workers — Attendance (2)
Panel A — Pooled Treatment Effects		
Post x Pay disparity	-0.358***	-0.167***
	(0.133)	(0.039)
Post x Pay disparity x Perceived justification	0.292*	0.159**
	(0.173)	(0.061)
Post x Perceived justification	0.0483	-0.0500
	(0.107)	(0.032)
Panel B — Treatment Effects Separately by Rank		
Post x Pay disparity x Low wage	-0.448***	-0.168***
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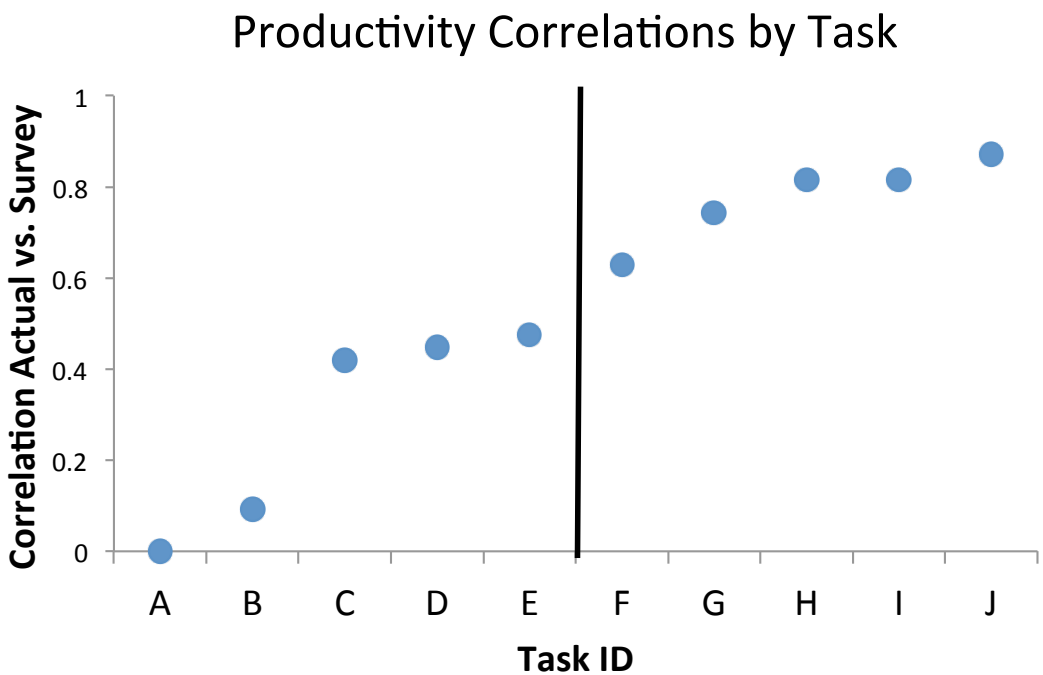
Post x Pay disparity x Low wage x Perceived justification	0.467**	0.181**
	(0.231)	(0.087)
Post x Pay disparity x Med wage	-0.270	-0.170**
	(0.224)	(0.075)
Post x Pay disparity x Med wage x Perceived justification	0.127	0.150
	(0.267)	(0.094)
Number of observations (worker-days)	8375	8375

Higher paid peer much > productive
→ no effect of pay disparity

- Results also hold for continuous difference measure
- Results robust to controls for own baseline productivity

Perceived Justifications: Observability

- 10 production tasks in each worksite
- Ex-ante quantify observability of co-worker output at baseline (using pilots)
  - Can worker accurately state own productivity relative to peers?
  - All teammates paid the same wage (no signal)
- Cut-off: 0.5 = mean (also median)



Perceived Justifications II: Observability

Dependent variable	Definition of Perceived Justification Indicator: Large baseline productivity difference between co-workers — Output (1)	Large baseline productivity difference between co-workers — Attendance (2)	Co-worker output is highly observable — Output (3)	Co-worker output is highly observable — Attendance (4)
Panel A — Pooled Treatment Effects				
Post x Pay disparity	-0.358***	-0.167***	-0.384***	-0.153***
	(0.133)	(0.039)	(0.131)	(0.031)
Post x Pay disparity x Perceived justification	0.292*	0.159**	0.395**	0.0996**
	(0.173)	(0.061)	(0.161)	(0.046)
Post x Perceived justification	0.0483	-0.0500	-0.0518	0.0332
	(0.107)	(0.032)	(0.103)	(0.028)
Panel B — Treatment Effects Separately by Rank				
Post x Pay disparity x Low wage	-0.448***	-0.168***	-0.513***	-0.158**
	(0.147)	(0.061)	(0.160)	(0.071)
Post x Pay disparity x Low wage x Perceived justification	0.467**	0.181**	0.512**	0.121
	(0.231)	(0.087)	(0.220)	(0.077)
Post x Pay disparity x Med wage	-0.270	-0.170**	-0.248	-0.157**
	(0.224)	(0.075)	(0.227)	(0.068)
Post x Pay disparity x Med wage x Perceived justification	0.127	0.150	0.0890	0.0876
	(0.267)	(0.094)	(0.293)	(0.118)
Post x Pay disparity x High wage			-0.386	-0.139*
			(0.242)	(0.071)
Post x Pay disparity x High wage x Perceived justification			0.582**	0.0884
			(0.269)	(0.078)
Number of observations (worker-days)	8375	8375	8375	8375

Co-worker output is observable → no effect of pay disparity

Tests for Team Cohesion

- Cooperative games on last day (fun farewell)
  - Performance determined by your own effort and cooperation with others
- Paid piece rates for performance
- No benefit to the firm
  - Decrease in Heterogenous team performance is not about punishing the firm (rules out reciprocity)
- Note: conducted in later rounds only

Games 1: Tower building

- Workers play in their assigned product teams
- Each team is given raw materials
  - e.g. cardboard, pens, rubberbands, playing cards…
- Task: build as high a tower as possible
- Performance: piece rate for tower height

Games 1: Tower building

Dependent variable: Tower height

	(1)	(2)	(3)	(4)
Pay disparity	-9.376***	-18.89**	-20.49***	-28.12***
	(3.487)	(8.068)	(5.532)	(7.729)
Pay disparity x Observable task		17.81*		
		(9.472)		
Pay disparity x Large productivity difference			17.11**	
			(6.815)	
Pay disparity x Justified (average)				32.80***
				(9.307)
Dependent variable mean	53.97	53.97	53.97	53.97
R-squared	0.291	0.397	0.397	0.429
N	80	80	80	80

Notes: All regressions include round fixed effects. Standard errors clustered by team.

- Teams with pay disparity perform 17% worse on average
  - Effects concentrated in cases where pay disparity is not clearly justified
- Lower effort even when no detriment to the firm
- Is this general disgruntlement, or specifically within-team dynamics?

Endline Survey – Social Cohesion

- Cohesion breakdown: interactions outside workplace?
- Hypothetical - Would you and unit-mate do each of the following outside work:
  - Seek advice, visit socially, borrow money

Dependent variable	Number of interactions (1)	Number of interactions (2)	At least one interaction with both co-workers (3)	At least one interaction with both co-workers (4)
Pay disparity	-0.236*	-0.349**	-0.127*	-0.173**
	(0.124)	(0.169)	(0.070)	(0.085)
Pay disparity x Perceived justification		0.208		0.0845
		(0.245)		(0.137)
Compressed mean	0.779	0.779	0.265	0.265

- Potential relevance of wage compression in poor countries
  - Social cohesion – essential for function of informal markets (credit, insurance,…)
  - Breakdown of cohesion in labor market could affect a variety of transactions

Endline Survey – Happiness

Panel A: Believes wage was set fairly in relation to teammates

	Full team (1)	High rank (2)	Medium rank (3)	Low rank (4)	Low + Med ranks (5)
Pay disparity	-0.0826	0.363***	-0.168	-0.498***	-0.291**
	(0.0917)	(0.120)	(0.166)	(0.170)	(0.114)
Pay disparity x Justified (avg)	0.171	-0.108	0.207	0.532	0.317
	(0.146)	(0.190)	(0.339)	(0.321)	(0.192)
Constant	0.729***	0.492**	0.738**	1.033***	0.865***
	(0.137)	(0.233)	(0.334)	(0.250)	(0.165)
Observations	358	121	119	118	237
R-squared	0.087	0.244	0.200	0.237	0.168

Panel B: Above median happiness (World Values Survey)

	Full team (1)	High rank (2)	Medium rank (3)	Low rank (4)	Low + Med ranks (5)
Pay disparity	-0.259***	-0.448***	-0.265	-0.102	-0.201**
	(0.0946)	(0.163)	(0.161)	(0.145)	(0.0999)
Pay disparity x Justified (avg)	0.336**	0.349	0.632*	0.267	0.436**
	(0.144)	(0.225)	(0.337)	(0.239)	(0.196)
Constant	0.965***	1.098***	0.712***	1.048***	0.914***
	(0.0992)	(0.158)	(0.224)	(0.144)	(0.115)
Observations	358	121	119	118	237
R-squared	0.186	0.315	0.307	0.277	0.192

Section 7

Methodology: Field Experiments

Introduction

- Field Experiments combine advantages of field studies and natural experiments:
  - Field setting (External Validity)
  - Randomization (Internal Validity)
- Common in Development, Public, Psychology and Economics, Labor
- Uncommon in IO (except for Demand estimation), Corporate Finance, Asset Pricing, Macro
- Difficulties: large sample (costly) and getting approval for implementation

Definition 1

- Definition 1. Card, DellaVigna, and Malmendier (*JEP* 2011) '*Randomized allocation to treatment and control groups for study purposes in a field setting*'
  - Excludes studies with no randomization (Bandiera et al., 2005 and on)
  - Includes social experiments run by the government
  - Includes experiments run by firms (Ausubel, 1999)
  - Excludes incidental randomization (i.e., lottery winnings, or Vietnam draft number)

Definition 2

- Definition 2. Harrison and List (*JEL* 2004): Broader definition, does not emphasize randomized allocation
  - But then how to separate from natural experiments?
  - Emphasis on laboratory versus field: 4 groups
    1. *(Conventional) Laboratory Experiment*
    2. *Artefactual Laboratory Experiment.* This is laboratory experiment in the field (i.e., on non-students)
    3. *Framed Field Experiment.* Experiment in the field with natural setting, but people aware of experimental treatments
    4. *Natural Field Experiment.* Experiment in the field, subjects unaware of manipulations

1. Some Advice for Field Experiments

What to do if planning a field experiment?

Advice 1. Read how-to manuals and previous field experiments: **Duflo-Glennerster-Kremer (NBER, 2006)**

- Great discussion of practical issues: Compliance, Sample Size,...
- Discussion of statistical issue, such as power tests
- Targeted toward development

2. Choose Experiment Type

Advice 2. Choose what type of Experiment

- *Large-Scale Experiment.* Example: Bandiera et al. (2005)
  - More common in Development
  - Convince large company or organization (World Bank, Government)
  - Need substantial funding
  - Examples among students:
    - Damon Jones: field experiment on tax preparers
    - However (also Damon): H&R Block experiment fell through after 1-year plans
    - Mariana Carrera: Safeway experimen on drug generics

2. Choose Experiment Type

Advice 2. Choose what type of Experiment

- *Small-Scale Experiment.* Example: Falk (2008)
  - More common in Psychology and Economics
  - Need to convince non-profit or small company
  - Limited funds needed – often company will pay
  - Example among students:
    - Dan Acland: projection bias and gym attendance
    - Vinci Chow: commitment devices for on-line computer game play
    - Pete Fishman: small video store randomized advertising

3. Necessary Components

Advice 3. Need two components:

1. Interesting economic setting:
   - Charity, Gym, Village in Kenya
   - Does Video Games matter? Yes, increasingly so
2. Economic model to test
   - Examples: Self-control, reciprocity, incentives
   - Avoid pure data-finding experiments
   - Insurance. If you can, pick a case where 'either' result is interesting
   - Best scenario: Do a field experiment tied to a model to infer parameters

4. Key Issues

Advice 4. Keep in mind three key issues

1. *Power calculations.* Will your sample size be enough?
   - Crucial to do ex ante to avoid wasting time and money
   - Simple case:
     - Assume outcome binary variable, dep.variable is share p doing 1 (Ex: giving to charity, taking up comm. device)
     - Standard error will be $\sqrt{p(1-p)/n}$
     - Example: $p = .5$, s.e. is .05 with $n = 100$, .025 with $n = 400$

4. Key Issues

Advice 4. Keep in mind three key issues

2. *Pilots.* So many things can go wrong – try to do small pilot
   - Use to spot problems in implementation
   - Do not overinfer results from pilot (sample too small)
3. *Human Subjects* approval
   - At Berkeley, takes about 2 months
   - More about this later

5. Before Going to Field...

Advice 5. Do a lot of work before going to the field!

- Power studies – YES
- But also: *Model*
  - To the extent possible, write down model
  - Do Monte Carlo of data
  - Estimate model on Monte Carlo data
  - Which parameters are identified?
  - Use that to refine design
  - Gift exchange design (DLMR above): one year before going to the field
- Also, Registration of design on AEA Registry

6. Other Practical Issues

Advice 6. Other practical issues:

- Keep in mind *implementation* of randomization
  - Example: Cross Designs hard to implement correctly
  - Example: **Green-Gerber (APSR, 2001)** on voter turnout:
    - cross-randomize phone calls, mailings, in-person visits
    - Hard to implement $\rightarrow$ Lead to loss of randomization
  - OK if just computerized implementation (ex: loan offers)
- Monitor what happens in the field *continuously*
- Build in *data redundancy* to catch errors or implementation problems
  - 'Did you see a flyer on the door?' in DellaVigna-List-Malmendier (2009)

7. Finding Funding

Advice 7. Start looking soon for funding. Some options:

- Russel Sage Small Grant Program: $7,500 (two to three months wait, once-in-career) (http://www.russellsage.org/research/behavioral-economics)
- NSF dissertation improvement grant website (http://www.nsf.gov/funding/ pgm_summ.jsp?pims_id=13453)
- Look at CVs of assistant professors in your field or job market students (Jonas' advice)
- Ask your advisor $\rightarrow$ May know of some funding sources

Section 8

Social Preferences Wave IV: Social Pressure and Signaling

Introduction

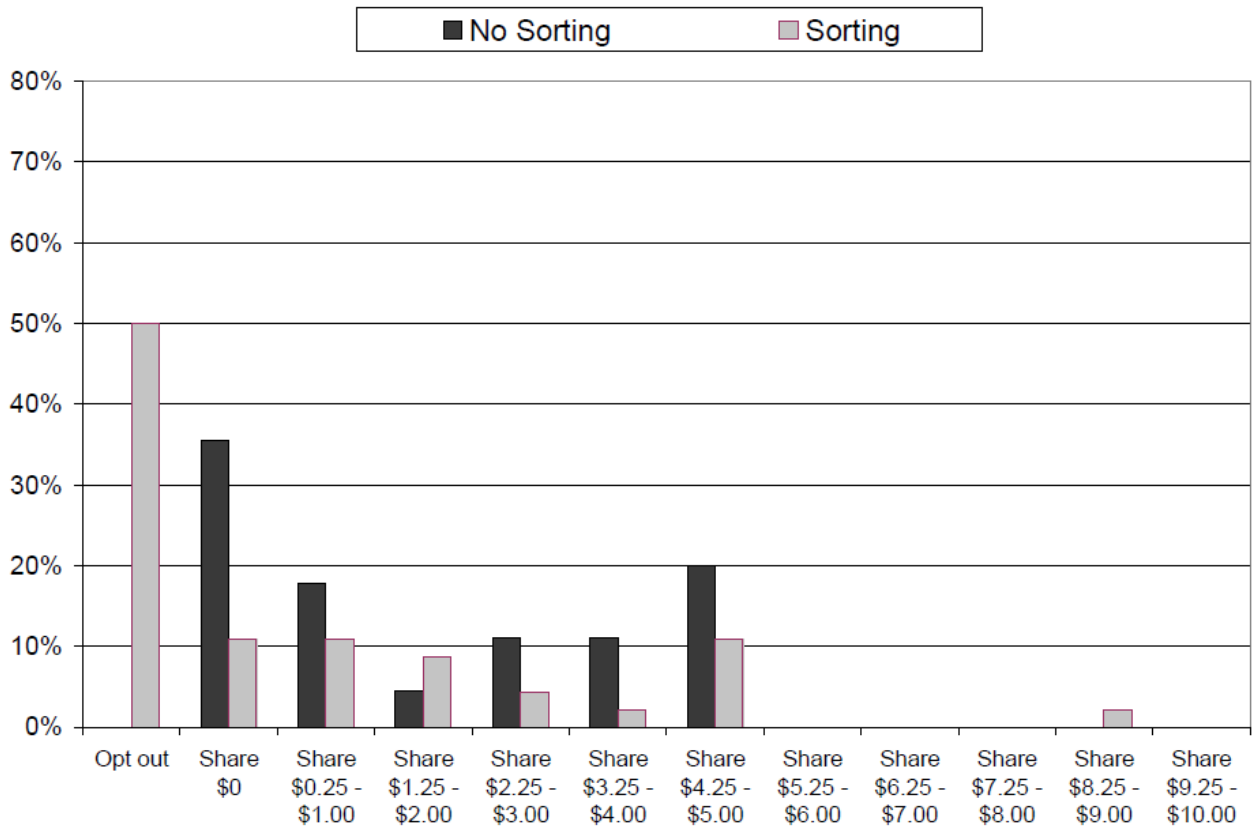
- Last 15 years: Evidence to suggest that altruism/warm glow/inequity aversion/reciprocity only part of story

- Dictator games with sorting (**Dana, Cain, and Dawes, 2007**; **Lazear, Malmendier, and Weber, AEJ Applied 2012**):
  - Subject can play dictator game ($10 to share)
  - OR can sort out and have privately $10
- Predictions of models of altruism/warm glow/inequity aversion/reciprocity:
  - Individuals who offer 0 still would offer 0 or sort out
  - Individuals who give to other would stay in and give

Results?

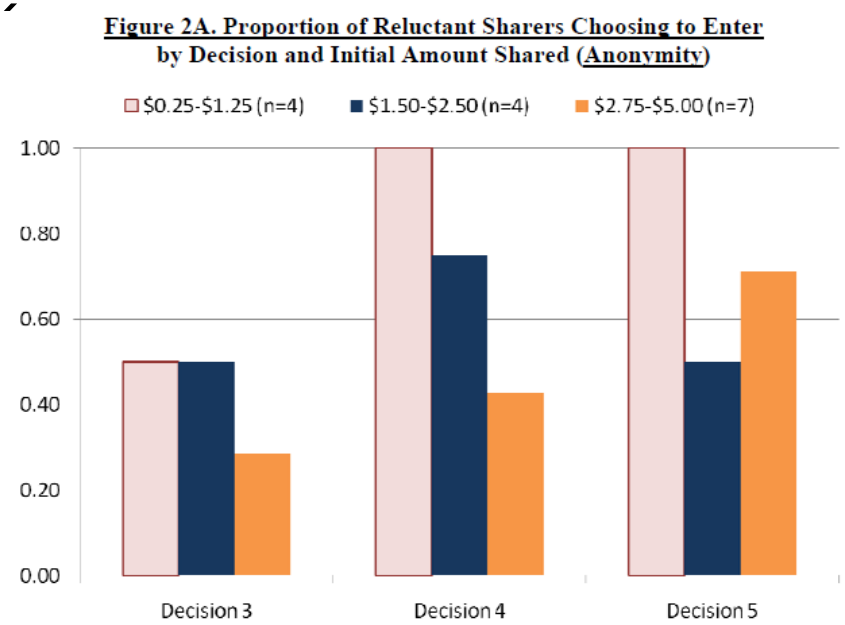
- From Lazear, Malmendier, and Weber (2012)

Figure 1A. Distributions of Amounts Shared
(Experiment 1, Berkeley)



Results?

- More than half of positive givers sort out instead!
- Need to increase dictator game payout to \$12 (Decision 5) to lure givers back



- Further evidence: Dictator games with moral wriggle room (Dana, Weber, and Kuang, 2007)
  - Avoid (free) information to justify not sharing

Social Pressure

- DellaVigna, List, and Malmendier (QJE 2012)
  - Pay a disutility cost $S > 0$ if do not give when asked
  - No disutility cost if can avoid to meet the solicitor or recipient
  - Give mostly *because asked*
- Can explain
  - Sort out in dictator game with sorting
  - Wanting to ignore information
  - Give small amount to charities, no crowd out of giving
  - Also: Give more in higher social pressure environments
- Key prediction specific to Social Pressure model:
  - *Altruism/Glow*: Agent seeks giving occasions to get warm glow
  - *Social Pressure*: Agents avoids giving occasions to avoid social pressure
- Drawback of model
  - Social Pressure cost is reduced form

Signaling

- Ex. Benabou and Tirole (2003)
  - Individuals have an altruism weight α
  - Individuals 'forget' their altruism α
  - They infer it from their own behavior in a signaling game:
  - Behave generously to convince one self (and others)
- More in this next lecture

Section 9

Social Pressure

Introduction: Milgram Experiment

- Early experiments: *Milgram experiment* post-WWII
- Motivation: Do Germans yield to pressure more than others?
  - Subjects: Adult males in US
  - Recruitment: experiment on punishment and memory
  - Teacher asks questions, administers shock for each wrong answer
  - Initial shock: 15V
  - Increase amount up to 450V (not deadly, but very painful)
  - Learner visible through glass (or audible)
  - Learner visibly suffers and complains

Results

1. 62% subjects reach 450V
2. Subjects regret what they did ex post
3. When people asked to predict behavior, almost no one predicts escalation to 450V

- It's not the Germans. *Most* people yield to social pressure
- Furthermore, naivete' — Do not anticipate giving in to social pressure
- Social Pressure likely to be important in organization and public events

Asch (1951)

- Second classical psychology experiment: **Asch (1951)**
  - Subjects are shown two large white cards with lines drawn on them
    - First card has three lines of substantially differing length on them
    - Second card has only one line.
  - Subjects are asked which of the lines in the first card is closest in length to the line in the second card
- Control treatment: subjects perform the task in isolation $\rightarrow$ 98 percent accuracy
- High social-pressure treatment: subjects choose after 4 to 8 subjects (confederates) unanimously choose the wrong answer $\rightarrow$ Over a third of subjects give wrong answer

Interpretations

- Social Pressure Interpretation:
  - Avoid disagreeing with unanimous judgment of the other participants
  - Result disappears if confederates are not unanimous
- Alternative interpretation: Social learning about the rules of the experiment
- Limitation: subjects not paid for accuracy

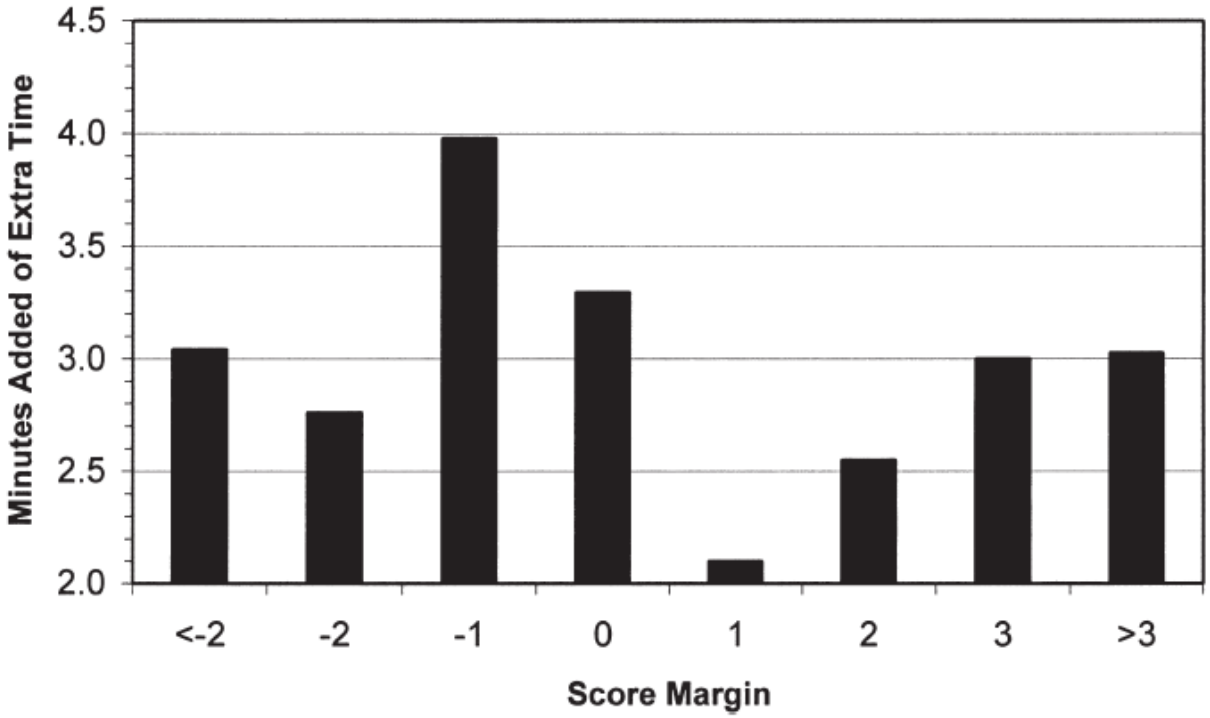
A more recent example

- An example of social pressure in a public event
- **Garicano, Palacios-Huerta, and Prendergast (REStat, 2006)**
  - Soccer games in Spanish league
  - Injury time at end of each game (0 to 5 min.)
  - Make up for interruptions of game
  - Injury time: last chance to change results for teams
- Social Pressure Hypothesis: Do referees provide more injury time when it benefits more the home team?
  - Yielding to social pressure of public
  - No social learning plausible
  - Note: referees professionals, are paid to be independent

Results I

- Figure 1 – Clear pattern, very large effects

FIGURE 1.—INJURY TIME AWARDED BY SCORE MARGIN



Number of minutes awarded by referees as a function of the margin in favor of the home team at the end of the match. Score margin = (goals scored by home team) − (goals scored by visiting team). Note: 3.3% of the matches ended with score differences smaller than −2; 5.2%, with score differences greater than 3.

Results II

- Table 5. Response to incentives → After 1994, 3 points for winning (1 for drawing, 0 for losing).

TABLE 5.—MARGINAL EFFECT OF INCENTIVES ON INJURY TIME

Statistic	[1]	[2]
Constant	3.50**	3.11**
	(0.14)	(0.32)
Score Difference	**−1.53****	**−1.56****
	(0.18)	**(0.18)**
Year Effect	0.81**	0.7**
	(0.18)	(0.21)
Year × Score Difference	**−0.58***	**−0.52***
	(0.23)	**(0.23)**
Yellow Cards		0.07**
		(0.02)

Results III

- Table 6. Response to social pressure: size of audience

TABLE 6.—EFFECT OF THE SIZE AND COMPOSITION OF THE CROWD ON REFEREE BI

Statistic	[1]	[2]
Constant	3.23**	2.94**
	(0.18)	(0.20)
Score Difference	**−0.93****	**−0.96****
	(0.20)	**(0.21)**
Year Effect	0.36**	0.33**
	(0.11)	(0.11)
Attendance	0.00	0.00
	(0.00)	(0.00)
Attendance × Score Difference	**−0.02****	**−0.02****
	(0.00)	**(0.00)**
Yellow Cards		0.07**
		(0.02)
Budget Home		

Social Pressure in the Workplace

- **Mas-Moretti (AER 2009).** Evidence of response to social pressure in the workplace
  - Workplace setting $\rightarrow$ Large retail chain
  - Very accurate measure of productivity, scanning rate
  - Examine what happens to productivity (speed of scanning) in response to entry of faster/slower coworkers
  - Schedule determined 2 weeks in advance
  - Social Pressure: Are others observing the employer?
- Slides courtesy of Enrico

Data

- We observe all the transactions that take place for 2 years in 6 stores. For each transaction, we observe the number of items scanned, and the length of the transaction in seconds.
- We define individual productivity as the number of items scanned per second.
- We know who is working at any moment in time, where, and whom they are facing
- Unlike much of the previous literature, our measure of productivity is precise, worker-specific and varies with high-frequency.

Institutional features

- Workers in our sample perform the same task use the same technology, and are subject to the same incentives
- Workers are unionized
- Compensation is a fixed hourly payment
- Firm gives substantial scheduling flexibility to the workers

What is the relationship between individual effort and co-worker permanent productivity?

- First we measure the *permanent* component of productivity of each worker

$$y_{itcs} = \theta_i + \Sigma_{j \neq i} \pi_j W_{jtcs} + \psi X_{itcs} + \gamma_{dhs} + \lambda_{cs} + e_{itcs.}$$

  For each worker *i*, 10 minute period and store, we average the permanent productivity of all the co-workers (excluding *i*) who are active in that period: $\Delta\overline{\theta}_{-ist}$

- Second, we regress ten minutes *changes* in individual productivity on *changes* in average permanent productivity of co-workers

Finding 1: There is a positive association between changes in co-worker permanent productivity and changes in individual effort

	(1)	(2)
Δ Co-worker permanent Productivity	0.176 (0.023)	0.159 (0.023)
Controls	No	Yes

$$\Delta y_{itcs} = \beta \Delta \bar{\theta}_{-ist} + \gamma_{tds} + \psi \Delta X_{tcs} + e_{itcs}$$

i = individual
t = 10 minute time interval
c = calendar date
s = store

Finding 2: The magnitude of the spillover effect varies dramatically depending on the skill level

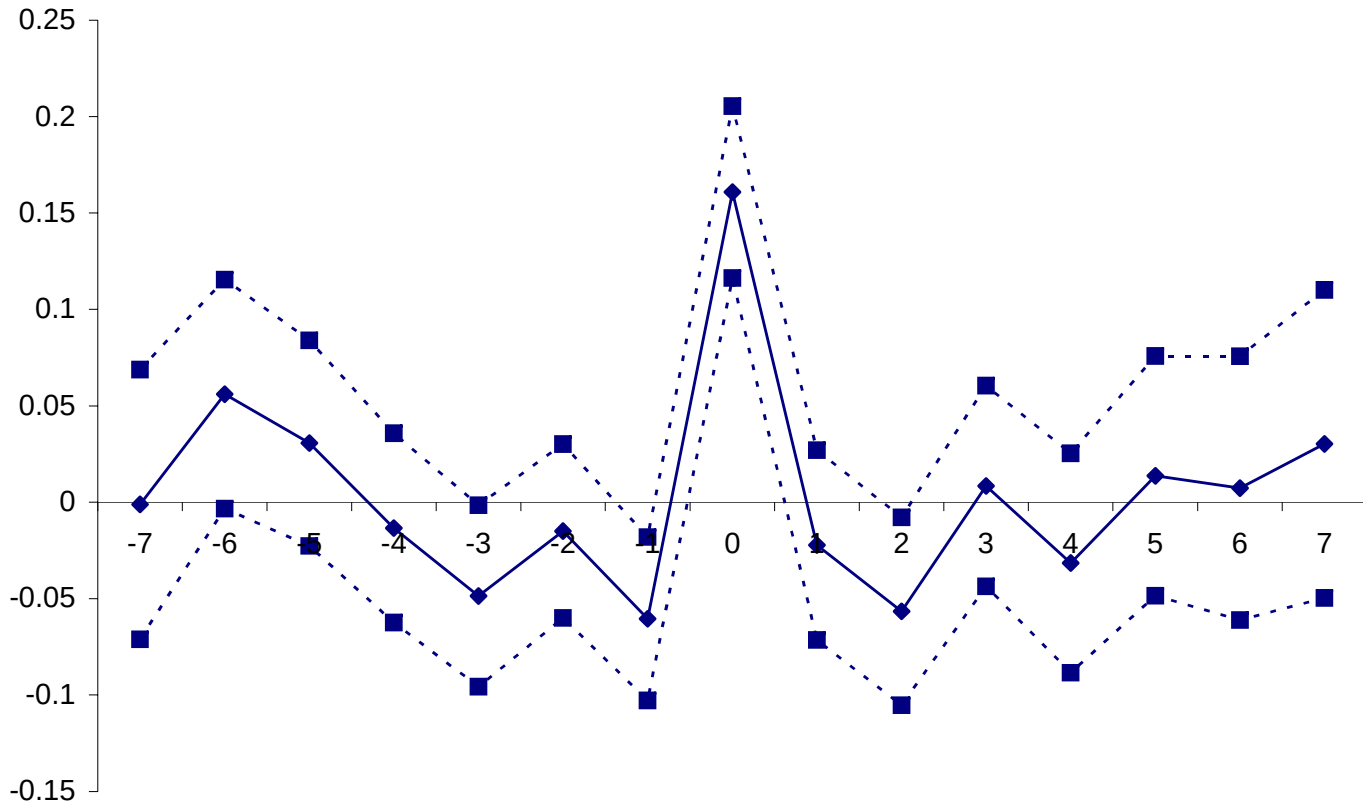
	(2)	(3)
Δ Co-worker permanent productivity	0.159 (0.023)	0.261 (0.033)
Δ Co-worker permanent prod. × Above average worker		-0.214 (0.046)
Observations	1,734,140	1,734,140
Controls	Yes	Yes

$$\Delta y_{itcs} = \beta \Delta \bar{\theta}_{-ist} + \gamma_{tds} + \psi \Delta X_{tcs} + e_{itcs}$$

What Determines Variation in Co-Workers Quality?

- Shifts are pre-determined
- Management has no role in selecting specific workers for shifts
- We measure co-workers productivity using permanent productivity (not current)
- Our models are in first differences: We use variation within a day and within a worker

The lags and leads for the effect of changes of average co-worker productivity on reference worker productivity



$$\Delta y_{itcs} = \beta_{-7}\Delta\bar{\theta}_{-i(t-7)cs} + \beta_{-6}\Delta\bar{\theta}_{-i(t-6)cs} + \beta_{-5}\Delta\bar{\theta}_{-i(t-5)cs} + \beta_{-4}\Delta\bar{\theta}_{-i(t-4)cs} + \beta_{-3}\Delta\bar{\theta}_{-i(t-3)cs} + \beta_{-2}\Delta\bar{\theta}_{-i(t-2)cs}$$
$$+ \beta_{-1}\Delta\bar{\theta}_{-i(t-1)cs} + \beta_{0}\Delta\bar{\theta}_{-i(t)cs} + \beta_{1}\Delta\bar{\theta}_{-i(t+1)cs} + \beta_{2}\Delta\bar{\theta}_{-i(t+2)cs} + \beta_{3}\Delta\bar{\theta}_{-i(t+3)cs} + \beta_{4}\Delta\bar{\theta}_{-i(t+4)cs} + \beta_{5}\Delta\bar{\theta}_{-i(t+5)cs}$$
$$+ \beta_{6}\Delta\bar{\theta}_{-i(t+6)cs} + \beta_{7}\Delta\bar{\theta}_{-i(t+7)cs} + \zeta\mathbf{M} + e_{itcs},$$

What explains spillovers?

- There are at least two possible explanations (Kendal and Lazear, 1992)
  - Guilt / Contagious enthusiasm
  - Social pressure ("I care what my co-workers think about me")
- We use the spatial distribution of register to help distinguish between mechanisms

- Guilt / Contagious enthusiasm implies that the spillover generate by the entry of a new worker should be larger for those workers who can observe the entering worker

- Social pressure implies that the spillover generate by the entry of a new worker should be larger for those workers who who are observed by the new worker

Finding 3

- Moreover, the addition of a worker behind an incumbent worker, regardless of her productivity, results in increased productivity of the incumbent worker.

- The addition of a worker in front, on the other hand, *decreases* productivity of the incumbent worker.

- This finding suggests that there is still scope for free-riding, but only when the free-riding is difficult to observe by other workers.

Table 5: Models by spatial orientation and proximity

	(1)	(3)
Δ Co-worker permanent productivity behind	0.233	
	(0.019)	
Δ Co-worker permanent productivity in front	0.007	
	(0.018)	
Δ Co-worker permanent productivity behind & closer		0.162
		(0.016)
Δ Co-worker permanent productivity in front & closer		0.016
		(0.015)
Δ Co-worker permanent productivity behind & farther		0.100
		(0.018)
Δ Co-worker permanent productivity in front & farther		0.003
		(0.018)

Voter Turnout

- Final Example: Effect of Social Pressure on Voting
  - Large literature of field experiments to impact voter turnout
  - Typical design: Day before (local) election reach treatment household and encourage them to vote
  - Some classical examples

Paper	Treatment (1)	Election type or question (2)	Variable t (3)	Year (4)	Place (5)	Sample size (6)	Control group t_T (7)	Treatment group t_C (8)	Exposure rate $e_T - e_C$ (9)	Persuasion rate (10)
Field Experiments										
Gerber and Green [2000]	Door-to-door canvassing	Federal elect.	Turnout	1998	New Haven	$N = 14{,}473$	0.422	0.463	0.270	0.263
	Canvassing + mail + calls	Federal elect.	Turnout	1998	New Haven	$N = 14{,}850$	0.422	0.448	0.270	0.167
Green, Gerber, and Nickerson [2003]	Door-to-door canvassing	Local elect.	Turnout	2001	6 cities	$N = 18{,}933$	0.286	0.310	0.293	0.118
Green and Gerber [2001]	Phone calls by youth vote	General elect.	Turnout	2000	4 cities	$N = 4{,}377$	0.660	0.711	0.737	0.205
	Phone calls 18–30-year-olds	General elect.	Turnout	2000	2 cities	$N = 4{,}377$	0.405	0.416	0.414	0.045

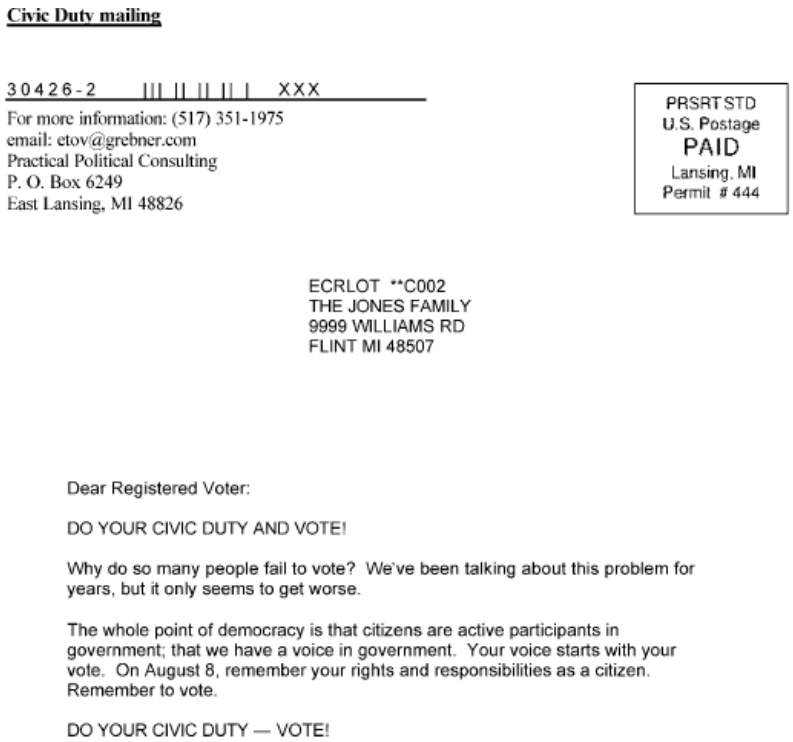
Example

- In these experiments, typically mailings are the cheapest, but also the least effective get-out-the-vote treatment

- **Gerber, Green, and Larimer (APSR, 2008):** Add social pressure to these treatments
- Setting:
  - August 2006, Michigan
  - Primary election for statewide offices
  - Voter turnout 17.7% registered voters
- Experimental sample: 180,000 households on Voter File
- Mailing sent 11 days prior to election

Experimental Design

- Control households get no mail (N=100,000)
- *Civic Duty Treatment.* 'DO YOUR CIVIC DUTY—VOTE!"'



Civic Duty mailing

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Dear Registered Voter:

DO YOUR CIVIC DUTY AND VOTE!

Why do so many people fail to vote? We've been talking about this problem for years, but it only seems to get worse.

The whole point of democracy is that citizens are active participants in government; that we have a voice in government. Your voice starts with your vote. On August 8, remember your rights and responsibilities as a citizen. Remember to vote.

DO YOUR CIVIC DUTY — VOTE!

Experimental Design

- *Hawthorne Treatment.* Information that voters' turnout records are being studied

Dear Registered Voter:

YOU ARE BEING STUDIED!

Why do so many people fail to vote? We've been talking about this problem for years, but it only seems to get worse.

This year, we're trying to figure out why people do or do not vote. We'll be studying voter turnout in the August 8 primary election.

Our analysis will be based on public records, so you will not be contacted again or disturbed in any way. Anything we learn about your voting or not voting will remain confidential and will not be disclosed to anyone else.

DO YOUR CIVIC DUTY — VOTE!

Experimental Design

- *Self-Information Treatment.* Give information on own voting record

Dear Registered Voter:

WHO VOTES IS PUBLIC INFORMATION!

Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse.

This year, we're taking a different approach. We are reminding people that who votes is a matter of public record.

The chart shows your name from the list of registered voters, showing past votes, as well as an empty box which we will fill in to show whether you vote in the August 8 primary election. We intend to mail you an updated chart when we have that information.

We will leave the box blank if you do not vote.

DO YOUR CIVIC DUTY—VOTE!

OAK ST	Aug 04	Nov 04	Aug 06
9999 ROBERT WAYNE		Voted	_____
9999 LAURA WAYNE	Voted	Voted	_____

Experimental Design

- *Other-Information Treatment.* Know if neighbors voted!

Dear Registered Voter:

WHAT IF YOUR NEIGHBORS KNEW WHETHER YOU VOTED?

Why do so many people fail to vote? We've been talking about the problem for years, but it only seems to get worse. This year, we're taking a new approach. We're sending this mailing to you and your neighbors to publicize who does and does not vote.

The chart shows the names of some of your neighbors, showing which have voted in the past. After the August 8 election, we intend to mail an updated chart. You and your neighbors will all know who voted and who did not.

DO YOUR CIVIC DUTY — VOTE!

MAPLE DR	Aug 04	Nov 04	Aug 06
9995 JOSEPH JAMES SMITH	Voted	Voted	_____
9995 JENNIFER KAY SMITH		Voted	_____
9997 RICHARD B JACKSON		Voted	_____

Results

- Substantial impacts especially when neighbors get to see
- All the results are highly statistically significant
- Results huge given that 1/3 of recipients probably never opened the mailer
- Impact: Obama campaign considered using this, but decided too risky

TABLE 2. Effects of Four Mail Treatments on Voter Turnout in the August 2006 Primary Election

	Experimental Group				
	Control	Civic Duty	Hawthorne	Self	Neighbors
Percentage Voting	29.7%	31.5%	32.2%	34.5%	37.8%
N of Individuals	191,243	38,218	38,204	38,218	38,201

Section 10

Next Lecture

Next Lecture

- Social Preferences
  - Social Pressure II
  - Social Norms
  - Reciprocity and Gift Exchange
- Non-Standard Beliefs
- Overconfidence