GR 6307
Public Economics and Development

3. Anti-Poverty Programs: Reaching the Poor

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Outline

Motivating Facts

Theory

Evidence from Rich Countries

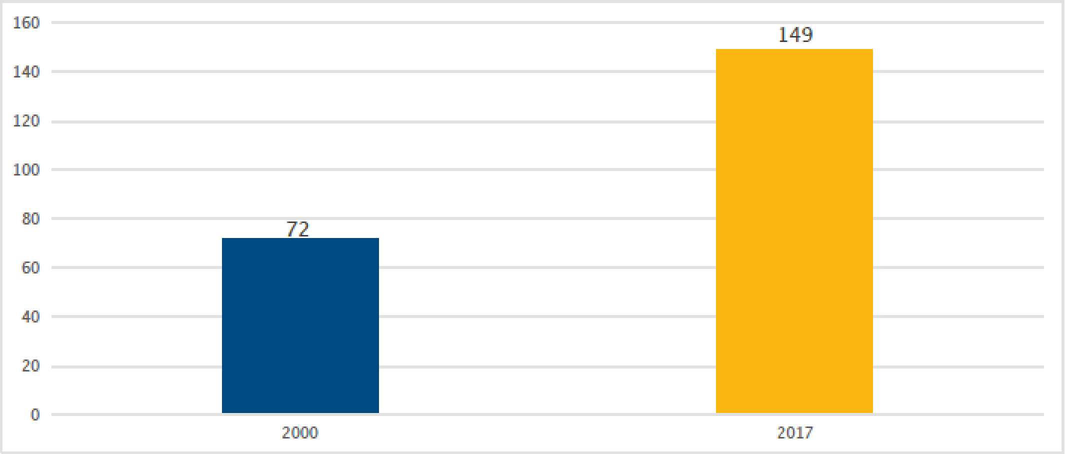
Targeting in Developing Countries: Who gets the Benefit?

Transfer Design: What is the Benefit?

Open Questions

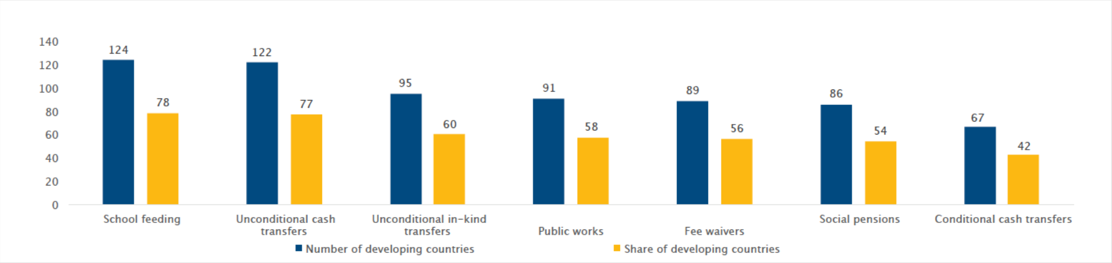
Trends in Social Programs over time and across countries

Figure 1. Number of developing countries with SSN programs



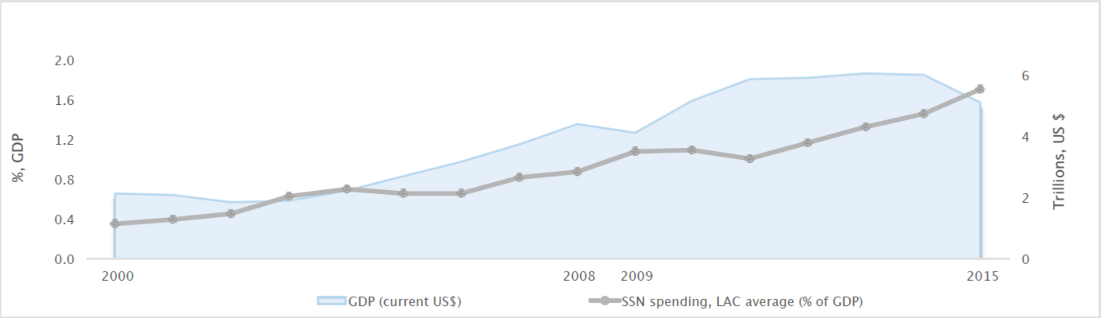
Trends in Social Programs over time and across countries

Figure 2. Number and share of developing countries with SSN instrument



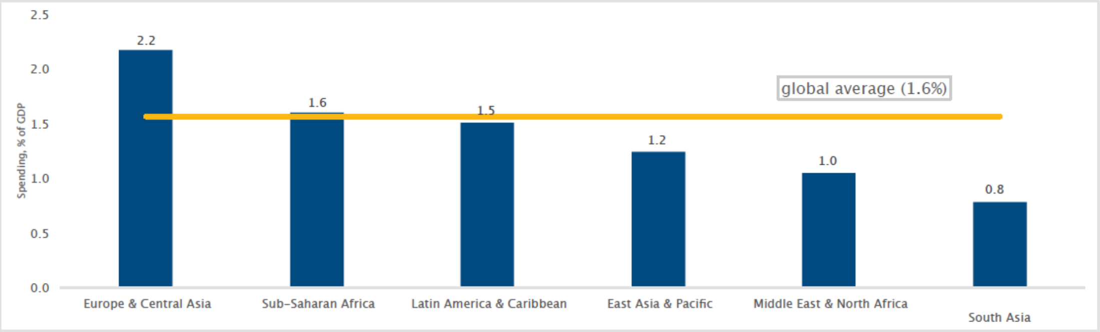
Trends in Social Programs over time and across countries

Figure 3. Spending on SSN programs in LAC over time, % of GDP



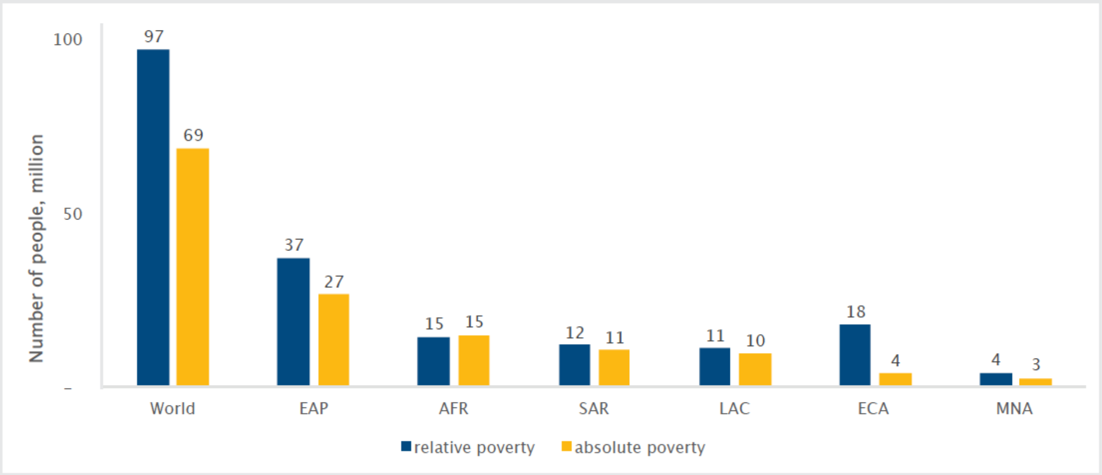
Trends in Social Programs over time and across countries

Figure 4. Spending on SSN programs across the regions, % of GDP



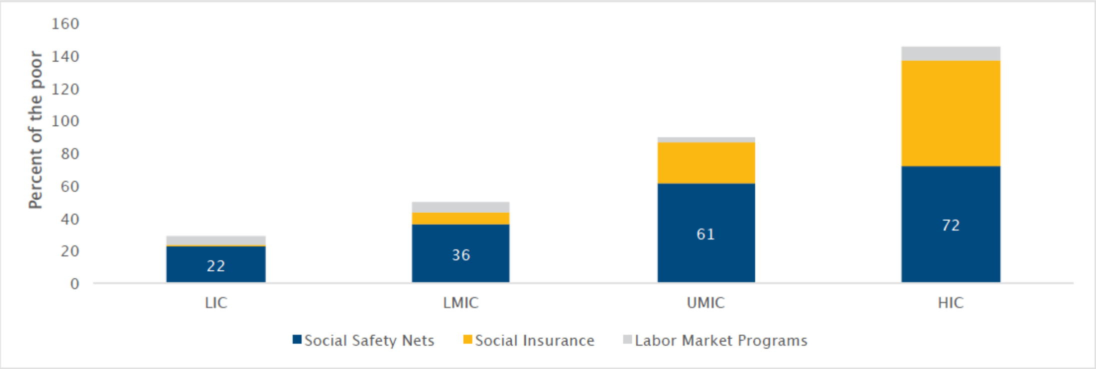
Trends in Social Programs over time and across countries

Figure 6. Estimated number of people escaping poverty because of SSN, millions



Trends in Social Programs over time and across countries

Figure 7. Coverage of the poor (bottom 20%) by SSN programs



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- Akerlof (AER 1978) *The Economics of "Tagging" as Applied to the Optimal Income Tax, Welfare Programs, and Manpower Planning*
- Nichols & Zeckhauser (AER 1982) *Targeting Transfers Through Restrictions on Recipients*
- Besley & Coate (AER 1992) *Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs*
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Saez (2002): Overview

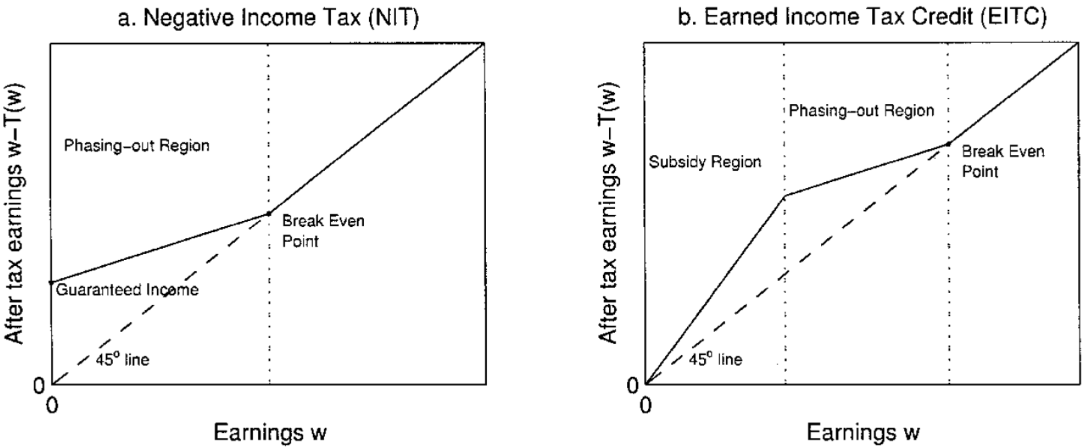
- Most rich countries provide *lots* of income support programs

- 2 Key margins along which people can respond:
  1. *Intensive* margin: Work, but earn less pre-tax/transfer income
  2. *Extensive* margin: Stop working

- This paper takes an optimal-tax approach to the design of income support programs.

- Key results
  1. Simple theory with both intensive- and extensive- margins
  2. Apply to 2 types of policies: Negative income taxes and Earned Income Tax Credit
  3. Conditions for EITC $\succ$ NIT: Extensive margin responses stronger than intensive-
  4. Calibration to US suggests EITC $\succ$ NIT

Saez (2002): NIT vs EITC



Saez (2002): Model Setup

- There are $I + 1$ occupations. Occupation 0 is unemployment.
- Salaries: $w_0 = 0$, $w_1 < \ldots < w_i < \ldots w_I$
- Government observes income and charges net taxes $T_i \to c_i = w_i - T_i$
- Total population normalized to one. $h_i =$proportion of individuals in occupation i. $\sum_{i=0}^{I} h_i = 1$
- Individuals choose which occupation to work in. In principle, depending on consumption in every occupation:

$$h_i = h_i \left(c_0, c_1, \ldots, c_I \right)$$

- The h_is embody all the behavioral responses

Saez (2002): Model Setup

- The government sets taxes to maximize welfare.
- Subject to resource constraint: Has to finance H of per-capita government spending

$$\sum_{i=0}^{I} h_i T_i = H$$

- Government attaches welfare weight g_i to people in each occupation
- NB not clear if $g_0 \lesseqgtr g_1$: e.g. "Lazy Poor" $\rightarrow g_0 < g_1$
- Weights are endogenous, depend on the tax schedule. Without income effects

$$\sum_{i=0}^{I} h_i g_i = 1$$

Saez (2002): Only Extensive Margin

- Suppose every individual is endowed with a skill $i \in \{0, 1, \ldots, I\}$.
- Only *extensive margin* choice: work as i or be unemployed.
- Without income effects, participation depends only on $c_i - c_0$
- Clearly $c_i \geq c_0 \forall i$. Define extensive elasticities

$$\eta_i = \frac{c_i - c_0}{h_i} \frac{\partial h_i}{\partial \left(c_i - c_0\right)}$$

Saez (2002): Only Extensive Margin

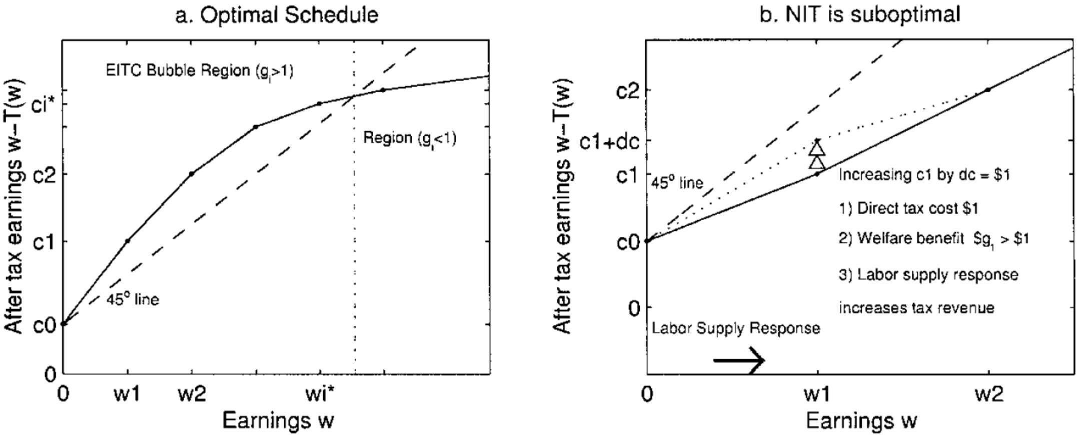
- *Proposition 1*: The optimal tax schedule satisfies

$$\frac{T_i - T_0}{c_i - c_0} = \frac{1}{\eta_i}\left(1 - g_i\right)$$

- Heuristic proof: Consider small increase dT_i in T_i. 2 effects
  - Mechanical Effect: $dM = \left(1 - g_i\right) h_i dT_i$
  - Behavioral Effect: $dB = \left(T_i - T_0\right) dh_i = -\left(T_i - T_0\right) h_i \eta_i \frac{dT_i}{(c_i - c_0)}$
  - Optimality: $dM + dB = 0$

Saez (2002): Extensive Margin: Implications for NIT vs EITC

- In the model with only extensive responses, NIT is always suboptimal, and EITC is usually optimal



Saez (2002): Only Intensive Margin

- Suppose now that people are only choosing how much to work.
- Really, people in occupation i are choosing whether to switch to $i-1$ or $i+1$.
- Then (again, w/out income effects) we can write the employment shares $h_i\left(c_{i+1}-c_i, c_i-c_{i-1}\right)$
- Now we can define the intensive elasticities

$$\zeta_i=\frac{c_i-c_{i-1}}{h_i} \frac{\partial h_i}{\partial\left(c_i-c_{i-1}\right)}$$

Saez (2002): Only Intensive Margin

- Proposition 2: The optimal tax schedule satisfies

$$\frac{T_i - T_{i-1}}{c_i - c_{i-1}} = \frac{1}{\zeta_i} \frac{(1 - g_i)\, h_i + (1 - g_{i+1})\, h_{i+1} + \ldots + (1 - g_I)\, h_I}{h_i}$$

- Heuristic proof: Consider small increase in tax for all jobs above i: $dT_i = dT_{i+1} = \ldots = dT_I = dT$. 2 Effects:
  - Mechanical Effect: People above i pay dT more. Valued at

$$dM = (1 - g_i)\, h_i + (1 - g_{i+1})\, h_{i+1} + \ldots + (1 - g_I)\, h_I$$

  - Behavioral effects: $dB = (T_i - T_{i-1})\, dh_i = -(T_i - T_{i-1})\, h_i \zeta_i \frac{dT}{c_i - c_{i-1}}$
  - Optimality: $dM + dB = 0$

Saez (2002): Intensive Margin: Implications for NIT vs EITC

- Look at optimality condition for $i = 1$:

$$\frac{T_1 - T_0}{c_1 - c_0} = \frac{1}{\zeta_1}\left[\frac{(g_0 - 1)\,h_0}{h_1}\right]$$

- Positive marginal tax (Negative Income Tax) iff $g_0 > 1$

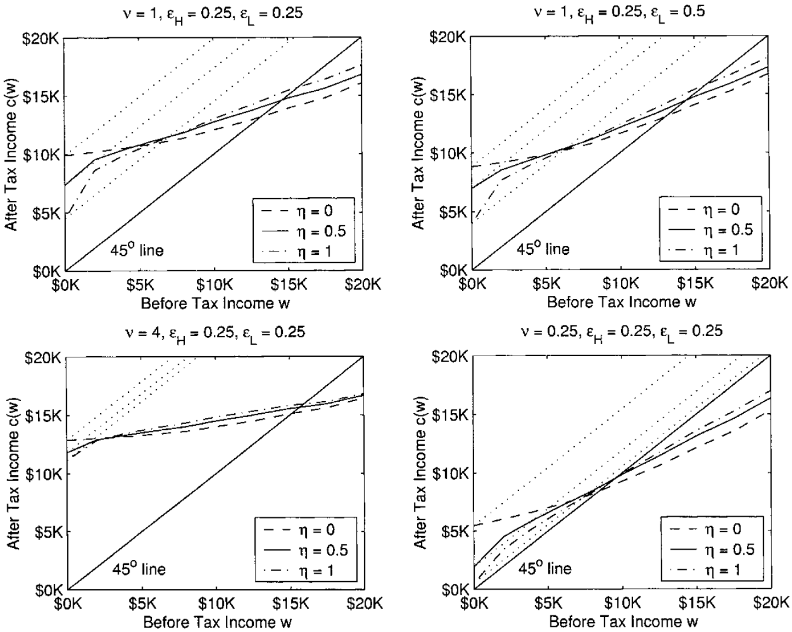
- More generally, $g_i > g_{i+1} \rightarrow$ increasing marginal tax rates, and no negative marginal tax rates (no EITC)

Saez (2002): Both Margins

- In the model with both margins, the optimal tax schedule satisfies

$$\frac{T_i - T_{i-1}}{c_i - c_{i-1}} = \frac{1}{\zeta_i h_i} \sum_{j=i}^{I} h_j \left[1 - g_j - \eta_j \frac{T_j - T_0}{c_j - c_0}\right]$$

- Ambiguous implications for optimal transfers: Depends on relative sizes of ηs and ζs.
- Calibration:
  - Extensive elasticity: $\eta \in \{0, 0.5, 1\}$ below \$20K, 0 above \$20K
  - Intensive elasticity ζ: Use traditional estimates of $\varepsilon = d \log y / d \log (1 - \tau)$. $\varepsilon_L \in \{0, 0.25, 0.5\}$ below \$20K, $\varepsilon_H \in \{0.25, 0.5\}$ above \$20K
  - No income effects.
  - Welfare: $g(c) = 1/(p \cdot c^v)$, p=marginal value of public funds, higher v = more redistributive preferences
  - $H = \$5K$
  - Income distribution calibrated using 1997 CPS and current tax schedule



ν = 1, ε_H = 0.25, ε_L = 0.25
ν = 1, ε_H = 0.25, ε_L = 0.5
ν = 4, ε_H = 0.25, ε_L = 0.25
ν = 0.25, ε_H = 0.25, ε_L = 0.25
After Tax Income c(w)
Before Tax Income w
$0K
$5K
$10K
$15K
$20K
45° line
η = 0
η = 0.5
η = 1

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Akerlof (1978): Overview

- Why not use the tax system to target transfers to the poor?
- Contrast two extreme cases:

1. Negative Income Tax: $T = -\alpha\bar{Y} + tY$. $\bar{Y}$ is per capita income, t is marginal tax rate.

$$\sum_i T = -\alpha n\bar{Y} + t\sum_i Y_i \Rightarrow t = \alpha + g \quad g \equiv \sum_i T_i / \sum_i Y_i$$

   Giving the poor a bigger transfer (α) requires a one-for-one increase in t

2. Perfect tag: Imagine we can identify a group of size βn that contains all the poor people and we give the amount α to everyone in this group. Now

$$t = \beta\alpha + g$$

   Giving more to the poor only costs $\beta < 1$

Akerlof (1978): Simple Optimal Tax Model

- 2 types of workers, skilled & unskilled. Each 50% of the population
- 2 types of jobs. Easy and difficult.
  - Skilled worker's output in difficult job is $q_D > q_E =$ skilled worker's output in easy job
  - Unskilled workers only work in the easy job. Produce output q_E
- Workers in difficult job pay tax t_D. Utility of skilled workers in difficult job is $u(q_D - t_D) - \delta$
- Utility of both types in easy job is $u(q_E + t_E)$
- Assume $u(q_D) - \delta > u(q_E)$ (otherwise everyone always takes easy job).

Akerlof (1978): Simple Optimal Tax Model

- Government chooses t_D, t_E to maximize expected utility

$$U = \tfrac{1}{2} \max \{u(q_D - t_D) - \delta, u(q_E + t_E)\} + \tfrac{1}{2} u(q_E + t_E)$$

- Budget balance requires

$$\begin{aligned} t_D = t_E \quad & \text{if} \quad u(q_D - t_D) - \delta \geq u(q_E + t_E) \\ t_E = 0 \quad & \text{if} \quad u(q_D - t_D) - \delta < u(q_E + t_E) \end{aligned}$$

- Optimum:

$$\begin{aligned} t_D^* &= t_E^* && \text{(budget balance)} \\ u(q_D - t_D^*) - \delta &= u(q_E + t_E^*) && \text{(binding IC)} \end{aligned}$$

Akerlof (1978): Tagging in Simple Model

- Introduce tagging: A proportion β of the poor have an observable tag.
- Difficult job taxed T_D. Untagged in easy job get T_E. Tagged get τ
- Government problem is now

$$\max_{T_D,T_E,\tau} U^{Tag} = \frac{1}{2}\max\left\{u\left(q_D - T_D\right) - \delta, u\left(q_E + T_E\right)\right\} + \frac{1}{2}\left(1-\beta\right)u\left(q_E + T_E\right) + \frac{1}{2}\beta u\left(q_E + \tau\right)$$

- Subject to

$$T_D = \left(1-\beta\right)T_E + \beta\tau \text{ if } u\left(q_D - T_D\right) - \delta \geq u\left(q_E + T_E\right)$$
$$\left(2-\beta\right)T_E + \beta\tau = 0 \text{ if } u\left(q_D - T_D\right) - \delta < u\left(q_E + T_E\right)$$

Akerlof (1978): Tagging Optimum

- Optimum features binding IC constraint

$$u\left(q_D - T_D^*\right) - \delta = u\left(q_E + T_E^*\right)$$

- More importantly, tagging allows more redistribution: $\tau^* > t_E^*$.
  - Rough proof by contradiction: Suppose that $\tau^* \leq T_E^*$.
  - Consider reducing T_E by ε and increasing τ by ε:

  1. Nobody is worse off (envelope theorem)
  2. This perturbation raises revenue
  3. This revenue can be used to redistribute

  $\Rightarrow$ τ^* or T_E^* is not optimally set

Akerlof (1978): Tags

- What makes a good tag?

1. Easily Observable. Low admin costs of using the tag
2. Correlated with need. Good at discriminating between needy and not (high β)
3. Immutable. People can't endogenously acquire the tag

- Examples?

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Nichols & Zeckhauser (1982): Overview

- What instruments should the government use to redistribute?
- Atkinson-Stiglitz (1976) $\rightarrow$ only income taxes/cash transfers.
- Akerlof (1978) $\rightarrow$ income taxes/cash transfers, but possibly dependent on *tags.*
- What about in-kind benefits? Commodity taxes/subsidies? Ordeals?
- This paper shows under what conditions to use these other instruments and how.

Nichols & Zeckhauser (1982): Income Tax Benchmark

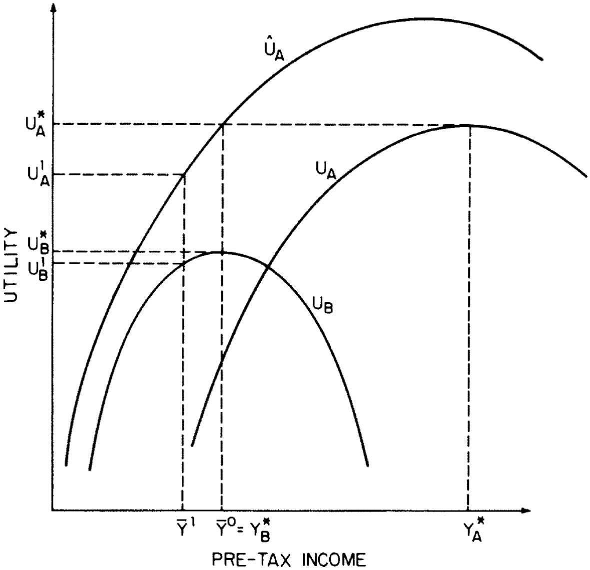
- Suppose there are two individuals. Beneficiary B and high-wage earner A. Want A to finance a transfer to B. What is the most efficient way to do it?

- Both individuals have the same utility $U(C, E)$, $C = Y + T$ consumption, E effort.
- Government can't see wages or hours, only income.
- Optimal policy takes the form: If $Y > \bar{Y}$ pay tax T; if $Y \leq \bar{Y}$ receive transfer T
- IC constraint that the high type doesn't masquerade is

$$U\left(Y_A^* - T, Y_A^*/W_A\right) > U\left(\bar{Y} + T, \bar{Y}/W_A\right)$$

- Result: $\bar{Y} < Y_B^*$. It's optimal to distort B's choice

Nichols & Zeckhauser (1982): Income Constraint



Nichols & Zeckhauser (1982): In-Kind Transfers

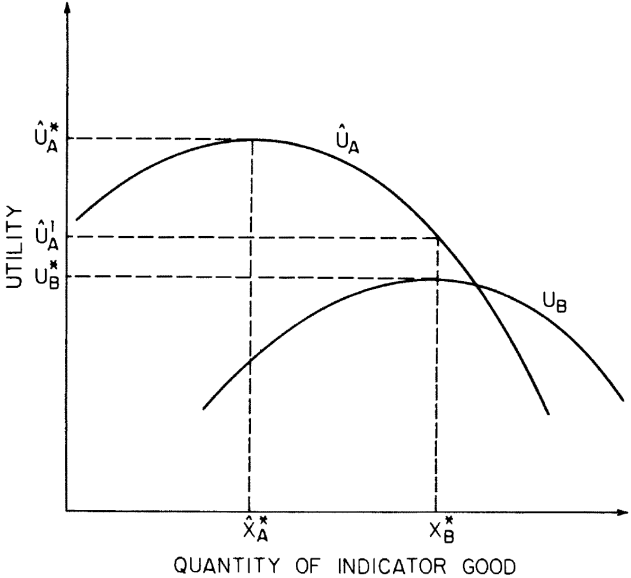
- Can it be optimal to give some of the transfer through an in-kind transfer instead of its cash equivalent?

- Usually, we think people must be at least as well off with the cash, so no.

- This is the case when demand depends only on its price and income (i.e. it's independent of leisure, ability, other observables). When it doesn't, it might act as a tag. N&Z call these *Indicator goods*

- Return to the simple income tax model. Out of total income $\bar{Y} + T$, B buys X_B^* of good X (at price 1). If A masquerades, she would only buy $\hat{X}_A^*$. $\Rightarrow X$ is an indicator good.

Nichols & Zeckhauser (1982): In-Kind Transfers



Nichols & Zeckhauser (1982): In-Kind Transfers

- Consider changing the transfer. Receive $\bar{X}$ in kind, remaining $T - \bar{X}$ in cash.
- If $\bar{X} < \hat{X}_A^*$, no effect
- If $\hat{X}_A^* < \bar{X} \leq X_B^*$ B suffers no loss, but makes mimicry more costly for A
- In general it is optimal to have $\bar{X} > X_B^*$ for deterrence.
- Similar argument can be applied to subsidies.

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Besley & Coate (1992): Overview

- Should people who receive transfers be required to work in exchange?

- Common policy, but is it efficient? 2 possible reasons

1. *Screening*. When we can't (easily) observe earnings (ability), work requirement $\rightarrow$ self-targeting

2. *Deterrence.* Encourage poverty-reducing investments

- B&C formalize this, provide sufficient conditions for workfare to dominate cash transfers.

Besley & Coate (1992): Model

- n individuals. Fraction γ of low types have ability $a_L < a_H =$ ability of high types
- $u(y, l) = y - h(l)$
- Poverty-alleviation program: $\{b_i, c_i\}_{i=L,H}$. Transfer b_i and requirement of c_i hours of work.
- PAP costs $n[\gamma b_L + (1-\gamma) b_H]$. Government objective is to minimize cost of guaranteeing everyone income z

Besley & Coate (1992): Model

- Individuals who accept $\{b_i, c_i\}$ can also provide private-sector labor:

$$l(b, c, a_i) = \begin{cases} \hat{l}(a_i) - c & \text{if } c \leq \hat{l}(a_i) \\ 0 & \text{otherwise} \end{cases}$$

  where $\hat{l}$ is optimal labor supply without PAP $\left(h'\left(\hat{l}\right) = a_i\right)$

- Yields private-sector earnings

$$y(c, a_i) = \begin{cases} a_i\left[\hat{l}(a_i) - c\right] & \text{if } c \leq \hat{l}(a_i) \\ 0 & \text{otherwise} \end{cases}$$

- and indirect utility

$$v(b, c, a_i) = b + y(c, a_i) - h(l(c, a_i) + c)$$

Besley & Coate (1992): Model

- Individuals will take the package intended for them iff

$$v(b_i, c_i, a_i) \geq v(0, 0, a_i)$$

- Assume that only the L types are poor without government intervention

$$y(0, a_H) > z > y(0, a_L)$$

Besley & Coate (1992): First-best benchmark

- Suppose policymakers observe abilities a_L, a_H.

- Now the government has to satisfy two constraints:

1. Participation constraints: $v(b_i, c_i, a_i) \geq v(0, 0, a_i)\,,\ i = L, H$

2. L types must escape poverty: $b_L + y(c_L, a_L) \geq z$

- PROPOSITION 1: *Cost-minimizing PSP is a welfare program. Low-ability individuals are offered a cash transfer that is just high enough to get them out of poverty, $z - y(0, a_L)$, and high-ability individuals are offered no benefits.*

Besley & Coate (1992): Screening

- Now suppose, govt only knows γ, the proportion of L types.
- Now the government also needs to respect incentive compatibility constraints:

$$v\left(b_L, c_L, a_L\right) \geq v\left(b_H, c_H, a_L\right) \quad v\left(b_H, c_H, a_H\right) \geq v\left(b_L, c_L, a_H\right)$$

- Note, without work requirements, these imply $b_L = b_H$
- The work requirement will allow the government to screen because H types have a higher opportunity cost of government work.
- But, the work requirement reduces the L types' income, so requires a bigger transfer to get them back to $y_L = z$.
- Tradeoff of increasing work requirement: lower transfers to H to respect IC, and higher transfers to L to achieve $y_L = z$

Besley & Coate (1992): Screening

- Define the *separating work requirement* c_L^s that gets the poor out of poverty and prevents masquerading

$$v\left(0,0,a_H\right)=v\left(z-y\left(c_L^s,a_L\right),c_L^s,a_H\right)$$

- PROPOSITION 2: *If both income-generating abilities and incomes are unobservable, one of the following two PAPs is cost-minimizing: (i) (welfare) impose no work requirements and offer both ability groups a transfer of* $z=y\left(0,a_L\right)$*; (ii) (workfare) offer self-categorized high-ability individuals no benefits and offer self-categorized low-ability individuals a transfer of* $z-y\left(c_L^s,a\right)$ *in exchange for a work requirement of* c_L^s*. A sufficient condition for the workfare solution to be cost-minimizing is that* $a_L<\left(1-\gamma\right)a_H$*.*

Besley & Coate (1992): Screening

- In the previous case we assumed the government can't observe earnings (so transfers can't depend on earnings).
- When the government *can* observe earnings, to mimic the low type, the high type must reduce her earnings to $y(c_L, a_L)$, requiring labor supply $y(c_L, a_L)/a_H$.
- We might even be able to implement the first-best benchmark if

$$v(0, 0, a_H) \geq z - h\left(\frac{y(0, a_L)}{a_H}\right)$$

(H prefers no benefit to earning $y(0, a_L)$ and consuming z)

- If this condition isn't met, we have to satisfy the IC constraint

$$v(b_H, c_H, a_H) \geq b_L + y(c_L, a_L) - h\left(\frac{y(c_L, a_L)}{a_H} + c_L\right)$$

Besley & Coate (1992): Screening

- Again, define the separating work requirement that satisfies

$$v(0,0,a_H) = z - h\left(\frac{y(\hat{c}_L^s, a_L)}{a_H} + \hat{c}_L^s\right)$$

- PROPOSITION 3: *If income-generating abilities are unobservable, individuals' incomes are observable and the benchmark PAP is not implementable, one of the following two programs is cost-minimizing: (i) (welfare) impose no work requirements and offer self-categorized high-ability individuals a transfer of* $z - h(y(0,a_L)/a_H) - v(0,0,a_H)$ *and offer self-categorized low-ability individuals a transfer of* $z - y(0,a_L)$*; (ii) (workfare) offer self-categorized high-ability individuals no benefits and offer self-categorized low-ability individuals a transfer of* $z - y(\hat{c}_L^s, a_L)$ *in exchange for a work requirement of* $\hat{c}_L^s$*. A sufficient condition for the workfare solution to be cost-minimizing is that* $\gamma a_L < (1-\gamma)\, h'\left(\frac{y(0,a_L)}{a_H}\right)\left(1 - \frac{a_L}{a_H}\right)$.

Besley & Coate (1992): Deterrence

- Assume again that the government observes ability. However ability depends on effort earlier in life.
- Probability of being high ability is $\pi(e)$ (strictly concave), costs e
- Individuals will choose e to maximize

$$\pi(e)\, v(b_H, c_H, a_H) + [1 - \pi(e)]\, v(b_L, c_L, a_L) - e$$

- e^* increasing in ex-post difference between high- and low-ability

$$e^*(\chi(\cdot)) \qquad \chi(b_L, c_L, b_H, c_H) \equiv v(b_H, c_H, a_H) - v(b_L, c_L, a_L)$$

Besley & Coate (1992): Deterrence

- Define the *maximal work requirement* c_L^m that satisfies

$$v\left(z - y\left(c_L^m, a_L\right), c_L^m, a_L\right) = v\left(0, 0, a_L\right)$$

- Note that $c_L^m > \hat{l}\left(a_L\right)$ and so the low-ability individuals do not work.
- PROPOSITION 4: *If income-generating abilities are observable but depend partly on choices made earlier in life, the cost-minimizing PAP either imposes no work requirements and offers low-ability individuals a transfer of* $z - y\left(0, a_L\right)$*, or imposes the maximal work requirement* c_L^m *on low-ability individuals and offers them a transfer of* z

Outline

Theory

- Saez (QJE 2002) *Optimal Income Transfer Programs: Intensive Versus Extensive Labor Supply Responses*
- Akerlof (AER 1978) *The Economics of "Tagging" as Applied to the Optimal Income Tax, Welfare Programs, and Manpower Planning*
- Nichols & Zeckhauser (AER 1982) *Targeting Transfers Through Restrictions on Recipients*
- Besley & Coate (AER 1992) *Workfare versus Welfare: Incentive Arguments for Work Requirements in Poverty-Alleviation Programs*
- Niehaus, Atanassova, Bertrand & Mullainathan (AEJ:Pol 2013) *Targeting with Agents*

Niehaus et al (2013): Overview

- How should targeting rules be designed when they must be implemented by corruptible agents?
- Consider a proxy means test (PMT) that must be implemented by an official who may be corrupt.
- Show that having more indicators (which makes identifying the poor easier statistically) can backfire if enforcement is weak, by making it easier to pretend that ineligible households are eligible and hence easier to receive bribes.
- Test the theory with data on Below Poverty Line (BPL) cards in Karnataka, India.

Niehaus et al (2013): Households

- The principal wants to allocate slots among a set of households.
- Household i has
  - Income $y_i \in \{\underline{y}, \overline{y}\}$
  - other characteristics $\mathbf{x}_i \in \mathbf{X}$
  - Values the slot at $v_i \sim G(v_i)$, exponential with rate $1/\eta$
- Joint distribution of attributes is $F(y_i, \mathbf{x}_i)$

Niehaus et al (2013): Official

- Principal would like to use y_i for targeting, but it's unobserved.
- Instead, use easier to observe characteristics $\mathbf{x}$
- A targeting rule is a subset $R \subseteq \mathbf{X}$: A household is eligible iff $\mathbf{x}_i \in R$
- The official (agent)
  - Implements R.
  - Observes $y_i, \mathbf{x}_i$, but not v_i
  - Cares about his income Y and the allocation.

$$U\left(Y, \{a_i\}\right) = Y + \underline{\alpha} \int_{y_i = \underline{y}} a_i di + \overline{\alpha} \int_{y_i = \overline{y}} a_i di$$

where $a_i \in \{0, 1\}$ indicates whether household i gets a slot; $(\underline{\alpha}, \overline{\alpha})$ summarize the official's distributive preferences

Niehaus et al (2013): Official

- If official violates R, he is caught with probability $\pi(a_i, \mathbf{x}_i, R)$
  - $\pi(a, \mathbf{x}, R) = 0$ if $a = \mathbf{1}\{\mathbf{x} \in R\}$
  - $\pi(a, \mathbf{x}, R) > 0$ if $a \neq \mathbf{1}\{\mathbf{x} \in R\}$
  - Punishment is a fine f

- Official allocates slots by establishing a menu of prices $p(y_i, \mathbf{x}_i) \geq 0$

$$\max_{\{p_i\}} \int [1 - G(p_i)]\,[p_i - c(y_i, \mathbf{x}_i)]\, dF(y_i, \mathbf{x}_i)$$

where the implicit marginal cost $c(y_i, \mathbf{x}_i)$ is

$$c(y_i, \mathbf{x}_i) = f\,[\pi(1, \mathbf{x}_i, R) - \pi(0, \mathbf{x}_i, R)] - \underline{\alpha}\mathbf{1}\left\{y_i = \underline{y}\right\} - \overline{\alpha}\mathbf{1}\left\{y_i = \overline{y}\right\}$$

Niehaus et al (2013): Official

- The official's problem looks just like a monopolist's problem.
- The solution will satisfy MR = MC, markups follow the rule $(p - c)/p = -1/\epsilon$

$$p^*(y_i, \mathbf{x}_i) = \max\{0, c(y_i, \mathbf{x}_i) + \eta\}$$

- The probability that household i gets a slot is then

$$\Pr(a_i = 1 | \mathbf{x}_i, y_i) = 1 - G(\max\{0, c(y_i, \mathbf{x}_i) + \eta\})$$

- Comparative statics:
  - prices increase in income iff $\underline{\alpha} > \overline{\alpha}$
  - prices decrease in eligibility (strictly if $f > 0$)
  - If f is sufficiently large, all eligible households get a slot at price 0. But might require arbitrarily harsh penalties, which seems unlikely.

Niehaus et al (2013): Principal

- Principal values the poor's surplus at $\underline{\omega}$. Similarly for the rich at $\overline{\omega} < 1/\eta < \underline{\omega}$
- Normalize cost of slots to 1.

$$V\left(\{p_i\}\right) = \int_{y_i=\underline{y}} \mathbf{1}\left\{v_i > p_i\right\}\left[\underline{\omega}\left(v_i - p_i\right) - 1\right] dG\left(v_i\right) dF\left(y_i, \mathbf{x}_i\right) + \int_{y_i=\overline{y}} \mathbf{1}\left\{v_i > p_i\right\}\left[\overline{\omega}\left(v_i - p_i\right) - 1\right] dG\left(v_i\right) dF\left(y_i, \mathbf{x}_i\right)$$

- Using the exponential distribution $G\left(v_i\right) = e^{-v_i/\eta}/\eta$

$$V\left(\{p_i\}\right) = \left(\underline{\omega}\eta - 1\right)\int_{y_i=\underline{y}} e^{-p_i/\eta} dF\left(y_i, \mathbf{x}_i\right) + \left(\overline{\omega}\eta - 1\right)\int_{y_i=\overline{y}} e^{-p_i/\eta} dF\left(y_i, \mathbf{x}_i\right)$$

Niehaus et al (2013): When agency doesn't matter

- Contrast with the case where the agent is completely honest, $p_i = 0$ if $\mathbf{x}_i \in R$, $p_i = \infty$ if $\mathbf{x}_i \notin R$. Now, principal solves

$$\max_{R \subseteq \mathbf{X}} (\underline{\omega}\eta - 1) \int_{y_i = \underline{y}} \mathbf{1}\{\mathbf{x}_i \in R\} \, dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1) \int_{y_i = \overline{y}} \mathbf{1}\{\mathbf{x}_i \in R\} \, dF(y_i, \mathbf{x}_i)$$

- PROPOSITION 1: *Let R^* be statistically optimal (solves above equation). Then*
  1. *As $f \to \infty$ the payoff from R^* approaches the constrained optimal payoff.*
  2. *As $\underline{\alpha} \to \infty$ while $\overline{\alpha} \to -\infty$ the payoff from R^* approaches the constrained optimal payoff.*
  3. *If $\underline{\alpha} = \overline{\alpha}$ and there exists $\tilde{\pi}$ such that $\pi(a_i, \mathbf{x}_i, R) = \tilde{\pi} \cdot \mathbf{1}\{a_i \neq \mathbf{1}\{\mathbf{x}_i \in R\}\}$ then rule R^* yields at least as high a payoff as any other nontrivial rule.*

Niehaus et al (2013): When agency doesn't matter

- In any of these cases agency issues are unimportant: Principal wants to use the statistically optimal rule.

- Third case is the interesting one: What does $\pi(a_i, \mathbf{x}_i, R) = \tilde{\pi} \cdot \mathbf{1}\{a_i \neq \mathbf{1}\{\mathbf{x}_i \in R\}\}$ mean?
  - Principal detects deviations from R with pr $\tilde{\pi}$, *regardless of what R is.* (e.g. random audits which fully verify $\mathbf{x}_i \in / \notin R$)
  - That means the choice of R doesn't affect how likely it is that the principal can detect deviations.
  - That means the choice of R doesn't affect the official's incentives to deviate from R. Every household is equally risky.
  - Then changing R to make one household eligible won't affect the probability that any other households get a slot.
  - Now question of what R should be is only question of whether R includes eligible households, the statistical question.

- Full verification of R seems like a stretch though. So what happens with more realistic information structures?

Niehaus et al (2013): Means testing

- Let's apply this framework to pure means testing $\mathbf{X} = \{\underline{y}, \overline{y}\}$
- Assume $\underline{\alpha} = \overline{\alpha} = 0$. Official only cares about profit
- w/pr π_e the principal observes the existence of a household
- w/pr $\pi_t \leq \pi_e$ the principal observe y_i
- Consider 2 policies:
  1. Universal eligibility: $R = \mathbf{X}$
  2. Means testing by official: $R = \underline{y}$
- Let's work through the math of which one is better

Niehaus et al (2013): Means testing

- Start with Universal Eligibility:
- The poor: $c\left(\underline{y}\right) = f\left[0 - \pi_e\right] \rightarrow p\left(\underline{y}\right) = \eta - f\pi_e$
- The rich: $c\left(\overline{y}\right) = f\left[0 - \pi_e\right] \rightarrow p\left(\overline{y}\right) = \eta - f\pi_e$
- Principal's value:

$$V\left(UE\right) = \left(\underline{\omega}\eta - 1\right)e^{\left(\frac{f\pi_e}{\eta} - 1\right)}F\left(\underline{y}\right) + \left(\overline{\omega}\eta - 1\right)e^{\left(\frac{f\pi_e}{\eta} - 1\right)}\left[1 - F\left(\underline{y}\right)\right]$$

Niehaus et al (2013): Means testing

- What about Means Testing?
- The poor: $c\left(\underline{y}\right) = f\left[0 - \pi_t\right] \rightarrow p\left(\underline{y}\right) = \eta - f\pi_t$
- The rich: $c\left(\overline{y}\right) = f\left[\pi_t - 0\right] \rightarrow p\left(\overline{y}\right) = f\pi_t + \eta$
- Principal's value:

$$V\left(MT\right) = \left(\underline{\omega}\eta - 1\right) e^{\left(\frac{f\pi_t}{\eta} - 1\right)} F\left(\underline{y}\right) + \left(\overline{\omega}\eta - 1\right) e^{\left(-\frac{f\pi_t}{\eta} - 1\right)} \left[1 - F\left(\underline{y}\right)\right]$$

Niehaus et al (2013): Means testing

- Comparing the two:

$$V(MT) - V(UE) = (1 - \overline{\omega}\eta) \underbrace{\left[e^{f\pi_e/\eta} - e^{-f\pi_t/\eta} \right]}_{\text{fewer rich recipients}} \frac{F(\underline{y})}{e^{\eta}}$$

$$- (\underline{\omega}\eta - 1) \underbrace{\left[e^{f\pi_e/\eta} - e^{f\pi_t/\eta} \right]}_{\text{fewer poor recipients if } \pi_e > \pi_t} \frac{1 - F(\underline{y})}{e^{\eta}}$$

$\Rightarrow$ With perfect enforcement or $\pi_t = \pi_e$, targeting is optimal.

$\Rightarrow$ If exclusion errors are sufficiently costly (high $\underline{\omega}$, $F(\underline{y})$ relative to $\overline{\omega}$, $1 - F(\overline{y})$) the constrained optimal policy is universal eligibility.

Niehaus et al (2013): Proxy Means Testing

- Now imagine that the principal wants to use land x_1 and jewelry x_2 as proxies for poverty.
- Should she use both? Or just land?

- The principal considers anyone whose total assets $x_i^1 + x_i^2 \leq y^*$ as poor. Optimal rule statistically is simply
$$R_{12} \equiv \left\{ \mathbf{x} : x^1 + x^2 \leq y^* \right\}$$
achieving perfect targeting when no agency concerns.
- Consider using just land:
$$R_1 \equiv \left\{ \mathbf{x} : x^1 \leq x^{1*} \right\}$$
- NB these are both scoring rules of the type $\sum h_n\left(x^n\right) < 0$
- Suppose the principal learns $x_j,\ j \in \{1, 2\}$ for household i with independent probability ϕ_j
- If the principal learns enough to determine the household is incorrectly classified, she fines the official f

Niehaus et al (2013): Proxy Means Testing

- LEMMA 1: *Fix any* $\phi_1 > 0$ *and let* x^{1*} *satisfy*

$$\mathbb{P}\left(x^1 + x^2 \leq y^* | x^1 = x^{1*}\right) \underline{\omega} \\ + \left[1 - \mathbb{P}\left(x^1 + x^2 \leq y^* | x^1 = x^{1*}\right)\right] \overline{\omega} = 1/\eta,$$

  or $x^{1*} = 0$ *if that equation has no solution. Then the rule* R_1 *defined by threshold* x^{1*} *is uniquely optimal within the class of rules that condition only on* x^1*.*

- Equate marginal benefits ($\mathbb{P}$ more poor people and $1 - \mathbb{P}$ more rich people get the transfer) with the marginal cost $1/\eta$.

- ..Even when there are agency issues.

Niehaus et al (2013): Proxy Means Testing

- Is there a rule that uses x^2 that's preferable to R_1?
- PROPOSITION 2: *Given a fixed rule R that conditions nontrivially on x^2, there exists $\phi_2^*(R) > 0$ such that if $\phi_2 < \phi_2^*(R)$, then rule R_1 yields a strictly higher payoff than R*
- Intuition:
  - Using x^2 improves targeting.
  - Using x^2 is harder to enforce for poor"ish" people: need to observe both x^1 and x^2 to be sure that someone is ineligible
  - Using x^2 is actually easier to enforce for very rich people: Observing *either* x^1 or x^2 sufficient to determine ineligibility.
  - Balance of effects depends on ϕ_2. As $\phi_2 \to 0$ R_1 becomes easier to enforce.

Niehaus et al (2013): Proxy Means Testing

- Is it always the case that having stronger enforcement increases welfare? No!
- PROPOSITION 3: *Let the probability of detecting a violation be constant ($\pi(a_i, \mathbf{x}_i, R) = \pi > 0$ whenever $a_i \neq \mathbf{1}\{\mathbf{x}_i \in R\}$), If R perfectly targets the poor, then $\partial V / \partial f \geq 0$. If R does not perfectly target the poor, so that there are some ineligible poor and some eligible rich, then there exist a scalar f^* and $\underline{\alpha}^*(f)$ and $\overline{\alpha}^*(f)$ such that if $f > f^*$, $\underline{\alpha} > \underline{\alpha}^*(f)$, and $\overline{\alpha} < \overline{\alpha}^*(f)$ then $\partial V / \partial f < 0$.*
- Intuition:
  - If you have a perfect rule, you want to force the official to use it
  - If you have an imperfect rule, then how much you want to force the official to use it depends on how much you disagree about who is deserving.
  - If sufficiently aligned: $\underline{\alpha}$ is large, and $\overline{\alpha}$ is small, then let the official use his discretion to violate your rule sometimes.

Outline

Motivating Facts

Theory

Evidence from Rich Countries

Targeting in Developing Countries: Who gets the Benefit?

Transfer Design: What is the Benefit?

Open Questions

Outline

Evidence from Rich Countries

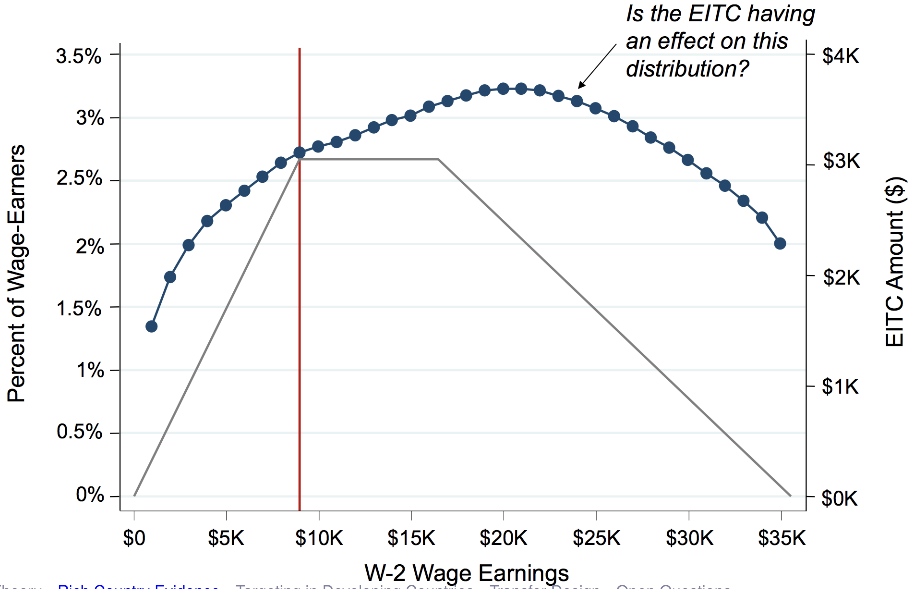
- Chetty, Friedman & Saez (AER 2013): *Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings*
- Deshpande & Li (2017) *Who is Screened Out? Application Costs and the Targeting of Disability Programs*
- Finkelstein & Notowidigdo (2019) *Take-up and Targeting: Experimental Evidence from SNAP*

Chetty et al (2013): Overview

- How do income support programs affect workers' earnings?
- Direct effect on the earnings of the beneficiaries
- Indirect effect on earnings of all workers in the same labor market
- Very hard to estimate these market-level impacts.
- Use variation across zip-codes in how many people are aware of EITC to estimate impact of EITC on the whole earnings distribution

Chetty et al (2013): EITC

Chetty et al (2013): EITC



Chetty et al (2013): A Simple Model

- Individuals make two choices:
  - labor supply l_i
  - tax evasion e_i
  - $z_i = wl_i$ is true earnings. Report $\hat{z}_i = wl_i - e_i$
- Taxes:
  - When $\hat{z}_i < K$ tax rate is $\tau_1 < 0$ (a work subsidy)
  - When $\hat{z}_i > K$ tax rate is $\tau_2 > 0$. $\boldsymbol{\tau} = (\tau_1, \tau_2)$
- Compliance behavior
  - Non-compliers always report $\hat{z}_i = K$ to maximize refund.
  - Compliers always report the truth $\hat{z}_i = z_i$
- Utility $U(C_i, l_i, \alpha_i) = C_i - h(l_i, \alpha_i)$
- Heterogeneity in α_i gives rise to earnings distribution $F(z)$

Chetty et al (2013): A Simple Model

- There are N cities of equal size $c = 1, \ldots, N$
- In city c, fraction λ_c of the workers are aware of taxes.
- Remaining $1 - \lambda_c$ optimize as if $\boldsymbol{\tau} = \mathbf{0}$
- Cities differ in skill distributions $G_c(\alpha_i)$ and the fraction of non-compliers θ_c
- Each city has an earnings distribution $F_c(z|\boldsymbol{\tau})$
- The goal is to estimate

$$F_c(z|\boldsymbol{\tau} \neq \mathbf{0}) - F_c(z|\boldsymbol{\tau} = \mathbf{0})$$

Chetty et al (2013): Empirical Strategy

- Basic strategy is to use cities where $\lambda_c = 0$ to estimate the counterfactual:

$$F_c\left(z|\boldsymbol{\tau} \neq \mathbf{0}, \lambda_c = 0\right) = F_c\left(z|\boldsymbol{\tau} = \mathbf{0}, \lambda_c = 0\right)$$

- How to measure λ_c? Use the degree of sharp bunching at K. Denote the fraction of individuals reporting $\hat{z}_i = K$ by ϕ_c. $\phi_c = \theta_c \lambda_c$ and so
- ASSUMPTION 1 (Tax Knowledge): *Individuals in neighborhoods with no sharp bunching at the kink have no knowledge of the policy's marginal incentives and perceive $\boldsymbol{\tau} = 0$: $\phi_c = 0 \Rightarrow \lambda_c = 0$.*
- Under assumption 1 cities with no bunching reveal the distribution when there are no taxes:

$$F_c\left(z|\boldsymbol{\tau} \neq \mathbf{0}, \phi_c = 0\right) = F_c\left(z|\boldsymbol{\tau} = \mathbf{0}, \phi_c = 0\right)$$

Chetty et al (2013): Empirical Strategy

- Can we use cities with no bunching as a counterfactual for cities with bunching?

- ASSUMPTION 2A (Cross-Sectional Identification): *Individuals' skills do not vary across cities with different levels of knowledge about the tax credit:*

$$G\left(\alpha_i|\lambda_c\right) = G\left(\alpha_i\right)\ \forall\lambda_c$$

- Now we can compare cities with and without bunching:

$$\hat{\Delta F} = F\left(z|\boldsymbol{\tau}\right) - F\left(z|\boldsymbol{\tau}, \phi_c = 0\right)$$

Chetty et al (2013): Empirical Strategy

- Assumption 2A is strong (why?)
- We can relax it by studying earnings changes when people become eligible for EITC. Here, use birth of first child which makes people eligible for EITC.
- ASSUMPTION 2B (Panel Identification): *Changes in skills when an individual becomes eligible for the credit do not vary across cities with different levels of knowledge about the tax credit:*

$$G_t\left(\alpha_i|\lambda_c\right) - G_{t-1}\left(\alpha_i|\lambda_c\right) = G_t\left(\alpha_i\right) - G_{t-1}\left(\alpha_i\right)\ \forall\lambda_c$$

- Under this assumption we can construct a difference in differences estimator:

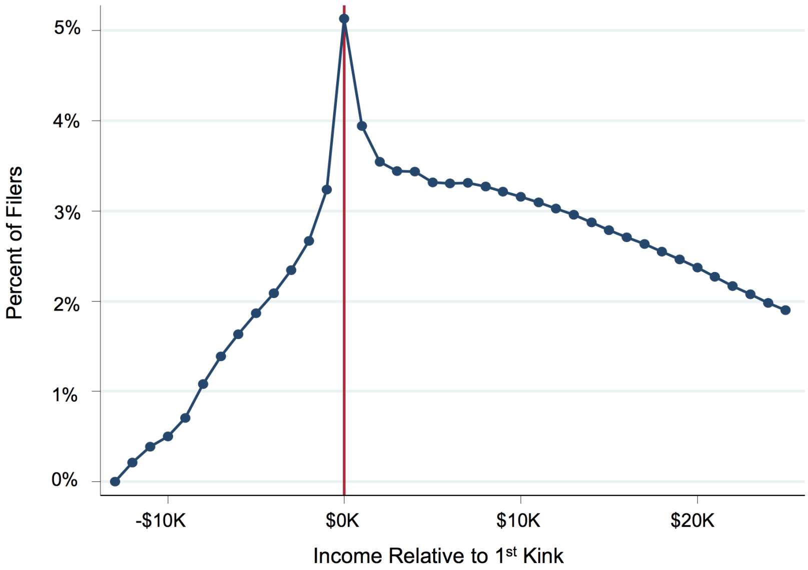
$$\begin{aligned}\hat{\Delta F}_{DD} = &\left[F_t\left(z|\boldsymbol{\tau}\right) - F_t\left(z|\boldsymbol{\tau},\phi_c = 0\right)\right] \\ &- \left[F_{t-1}\left(z|\boldsymbol{\tau}\right) - F_{t-1}\left(z|\boldsymbol{\tau},\phi_c = 0\right)\right]\end{aligned}$$

Chetty et al (2013): Data

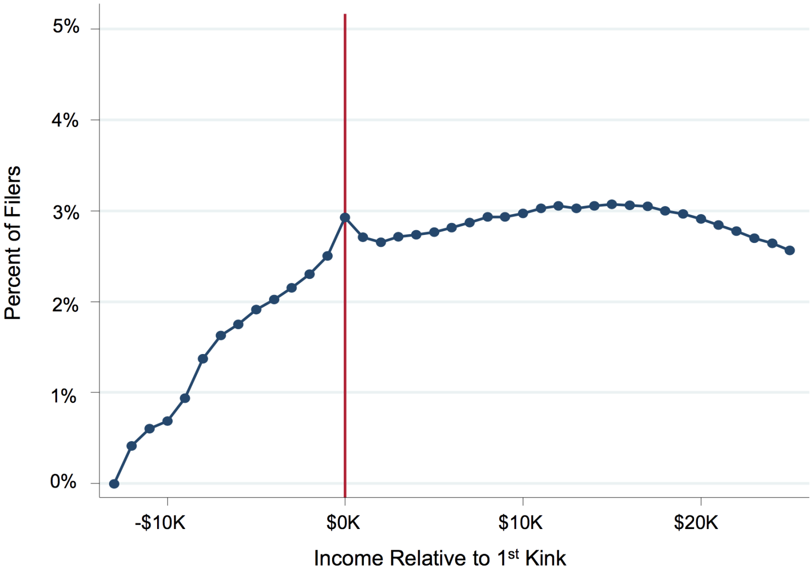
- Universe of US tax returns 1996-2009. Restrict to ppl with a dependent, income<$50K $\rightarrow$ 77.6 mn taxpayers, 1 bn obs.

Variable	Mean (1)	Std. Dev. (2)
Income Measures		
Total Earnings	$20,091	$10,784
Wage Earnings	$18,308	$12,537
Self-Employment Income	$1,770	$6,074
Non-Zero Self-Emp. Income	19.6%	39.7%
Tax Credits		
EITC Refund Amount	$2,543	$1,454
Claimed EITC	88.9%	31.4%
Professionally Prepared Return	69.6%	46.0%
Demographics		
Age	37	13
Number of Children	1.7	0.8
Married	30.3%	45.9%
Female (for single filers)	73.0%	44.4%
Number of Observations	219,742,011	

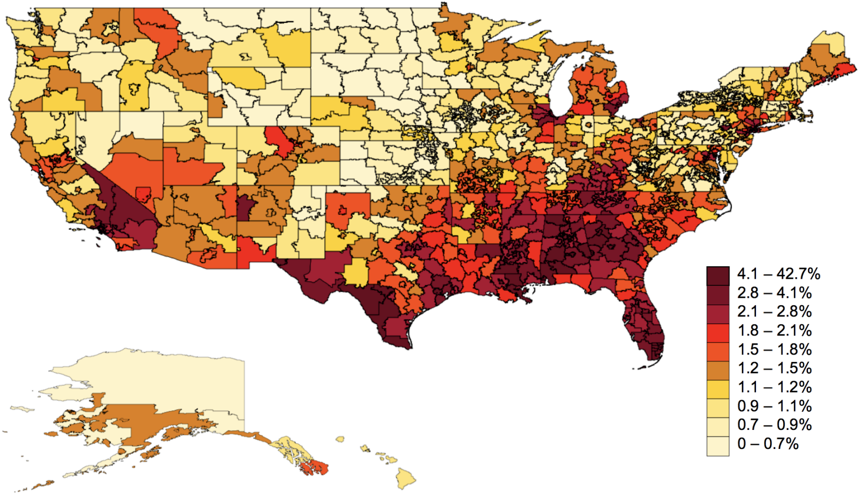
Earnings Distribution in Texas



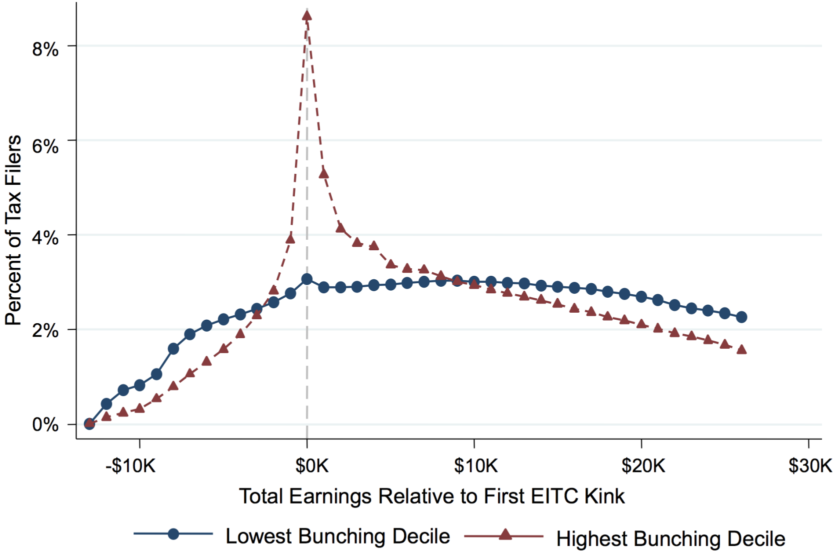
Earnings Distribution in Kansas

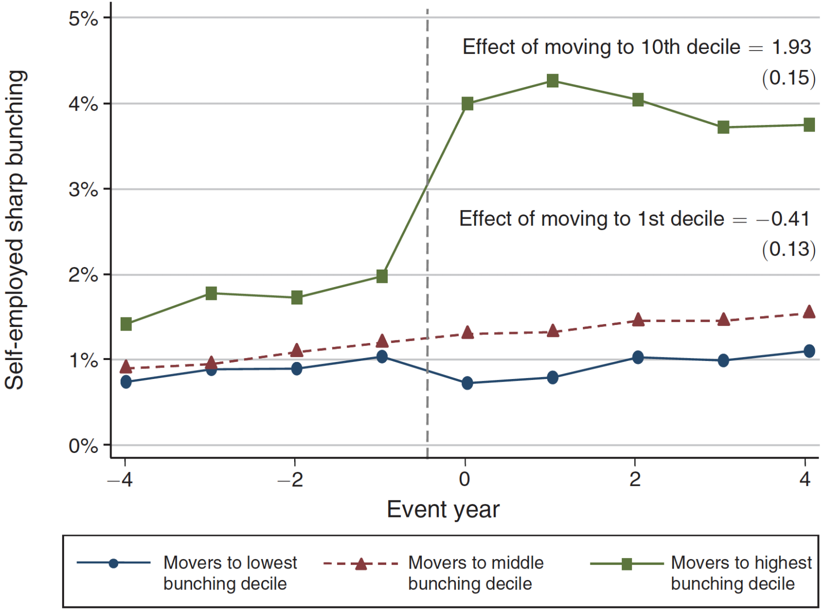


Fraction of Tax Filers Who Report SE Income that Maximizes EITC Refund in 2008

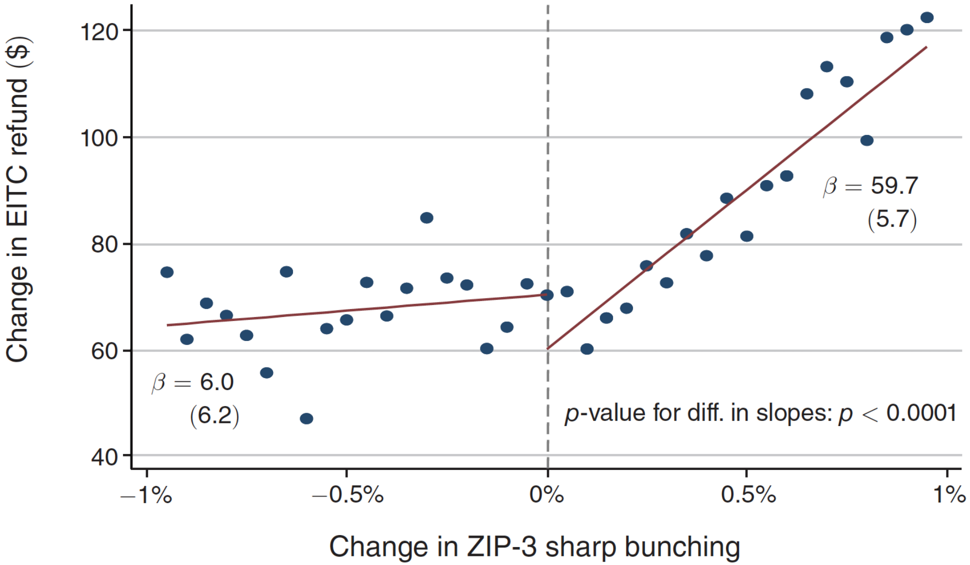


Earnings Distributions in Lowest and Highest Bunching Deciles

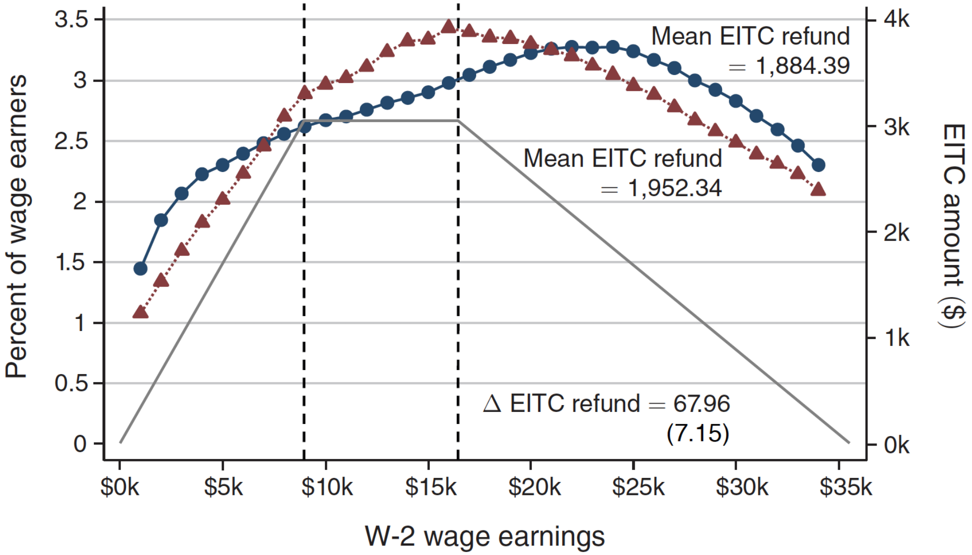



Self-employed sharp bunching
5%
4%
3%
2%
1%
0%
Effect of moving to 10th decile = 1.93
(0.15)
Effect of moving to 1st decile = −0.41
(0.13)
−4
−2
0
2
4
Event year
Movers to lowest bunching decile
Movers to middle bunching decile
Movers to highest bunching decile

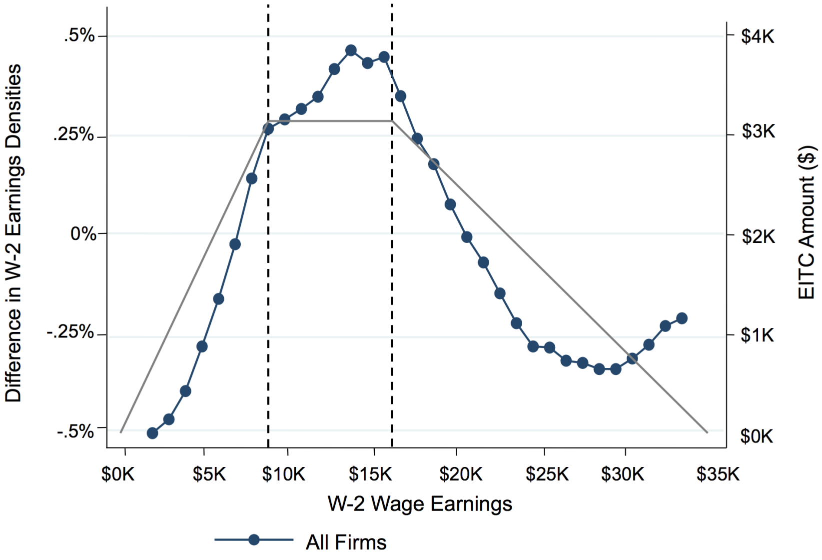
Panel B. EITC refund amount



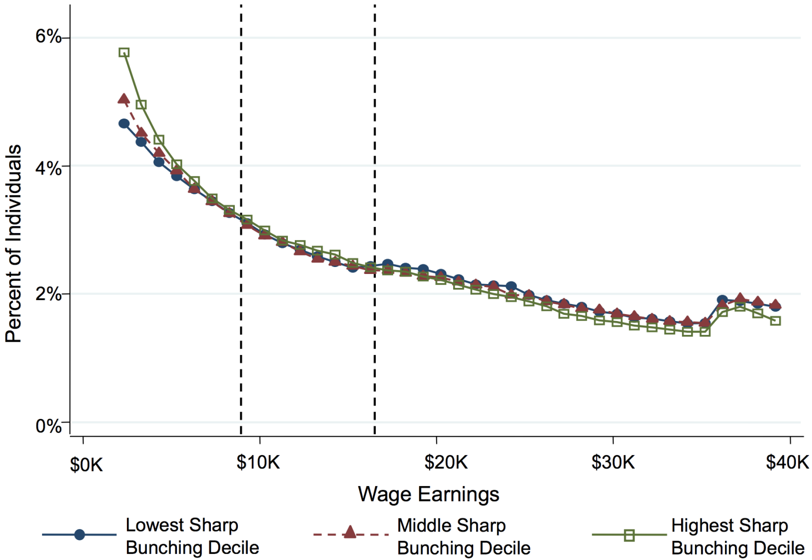
Panel A. Wage earners with one child



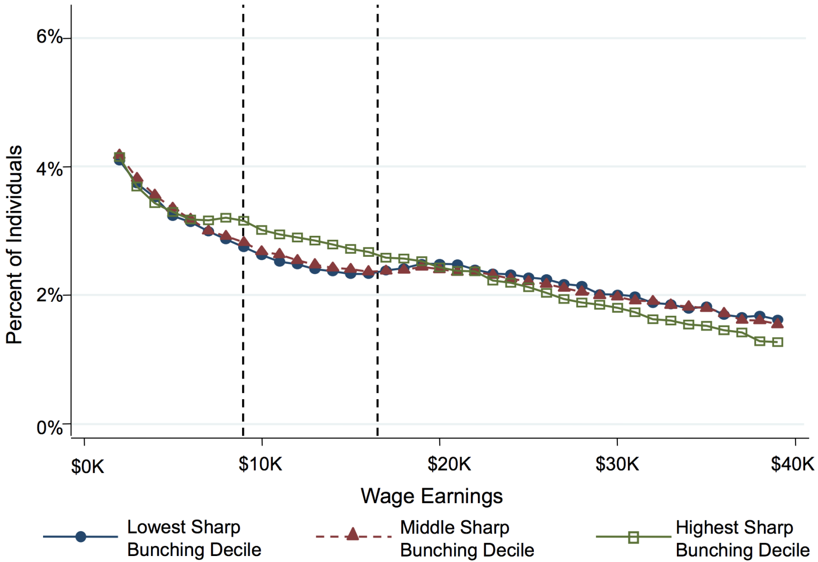
Difference in Wage Earnings Distributions Between Top and Bunching Decile Wage Earners with One Child



Earnings Distribution in the Year Before First Child Birth for Wage Earners



Earnings Distribution in the Year of First Child Birth for Wage Earners



Simulated EITC Credit Amount for Wage Earners Around First Child Birth

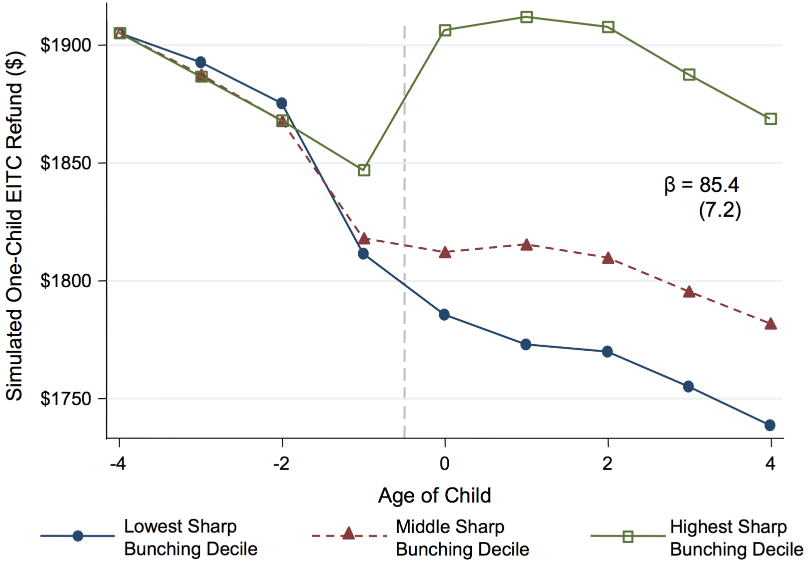


Table 3—Elasticity Estimates Based on Change in EITC Refunds around Birth of First Child

	Mean elasticity (1)	Phase-in elasticity (2)	Phase-out elasticity (3)	Extensive elasticity (4)
Panel A. Wage earnings				
Elasticity in US 2000–2005	0.21	0.31	0.14	0.19
	(0.012)	(0.018)	(0.015)	(0.019)
Elasticity in top decile ZIP-3s	0.55	0.84	0.29	0.60
	(0.020)	(0.031)	(0.020)	(0.034)
Panel B. Total earnings				
Elasticity in US 2000–2005	0.36	0.65	0.11	0.36
	(0.017)	(0.030)	(0.006)	(0.019)
Elasticity in top decile ZIP-3s	1.06	1.70	0.31	1.06
	(0.029)	(0.047)	(0.010)	(0.040)

Table 4—Impact of EITC on Wage Earnings Distribution of EITC-Eligible Households

	Percent of EITC-eligible households below threshold			
	50 percent of poverty line (1)	100 percent of poverty line (2)	150 percent of poverty line (3)	200 percent of poverty line (4)
No EITC counterfactual	13.15	31.31	53.81	77.06
EITC with no behavioral response	8.92	21.37	41.56	70.82
EITC with avg. behavioral response in United States	8.16	21.00	41.97	71.29
EITC with top decile behavioral response	6.73	20.24	42.56	72.08

Outline

Evidence from Rich Countries

- Chetty, Friedman & Saez (AER 2013): *Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings*
- Deshpande & Li (2017) *Who is Screened Out? Application Costs and the Targeting of Disability Programs*
- Finkelstein & Notowidigdo (2019) *Take-up and Targeting: Experimental Evidence from SNAP*

Deshpande & Li (2017): Overview

- When eligibility is hard to observe/verify, application costs can act as a screening device (ordeal).
- Study this in the context of disability insurance.
- Use closings of Social Security Administration offices to get variation in application costs.
- Find large impacts,
  - Fewer people apply
  - Targeting worsens
- Suggests social costs of closing SSA offices outweigh social benefits 5 to 1

Deshpande & Li (2017): A Simple Framework

- Consider an increase in application costs from η to $\eta' > \eta$.
- Assume adjudicators don't change their standards. Then targeting efficiency increases iff

$$\mathbb{P}\left(R|A,\eta'\right) > \mathbb{P}\left(R|A,\eta\right)$$

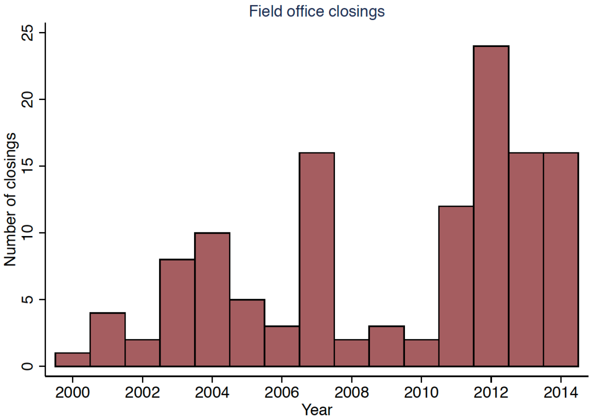
  where $\mathbb{P}\left(R|A,\eta\right)$ is the probability of receiving benefits conditional on applying.
- Rewrite: Targeting improves iff

$$1 < \frac{\mathbb{P}\left(R|A,\eta'\right)}{\mathbb{P}\left(R|A,\eta\right)} = \frac{\frac{\mathbb{P}(R|\eta')}{\mathbb{P}(A|\eta')}}{\frac{\mathbb{P}(R|\eta)}{\mathbb{P}(A|\eta)}} = \frac{\frac{\mathbb{P}(R|\eta')}{\mathbb{P}(R|\eta)}}{\frac{\mathbb{P}(A|\eta')}{\mathbb{P}(A|\eta)}} = \frac{\Delta_R + 1}{\Delta_A = 1}$$

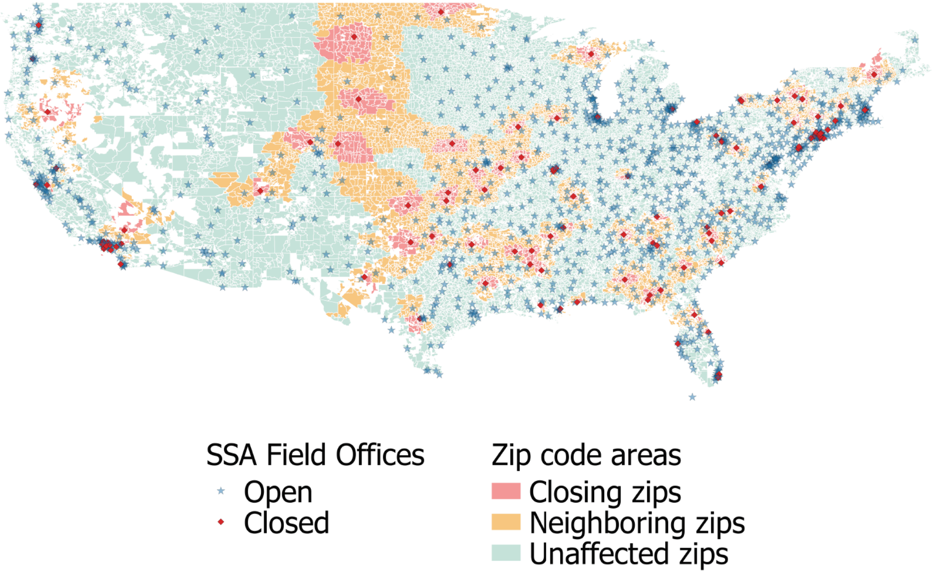
  where $\Delta_R = \frac{\mathbb{P}(R|\eta') - \mathbb{P}(R|\eta)}{\mathbb{P}(R|\eta)}$ and $\Delta_A = \frac{\mathbb{P}(A|\eta') - \mathbb{P}(A|\eta)}{\mathbb{P}(A|\eta)}$

Deshpande & Li (2017): Context

- To get Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI), you have to apply to the Social Security Administration.
- They review medical/work history, determine eligibility.
- 1,230 Social Security Field Offices provide in-person help.



Deshpande & Li (2017): SSA Office Closures



Deshpande & Li (2017): Data

- Data from the Social Security Administration
  - All field offices with addresses & closing dates
  - walk-in wait times
  - Staff numbers at each office
  - volume of calls to 800 numbers
  - application data
  - decision data
  - ZIP code of applicants
- Collapse by ZIP and assign each ZIP code a nearest, second nearest, and third nearest SSA office.

Deshpande & Li (2017): Empirical Approach

- Compare applications and receipts in the ZIP codes where the office closed to ZIP codes where the office hasn't closed yet, but will do.

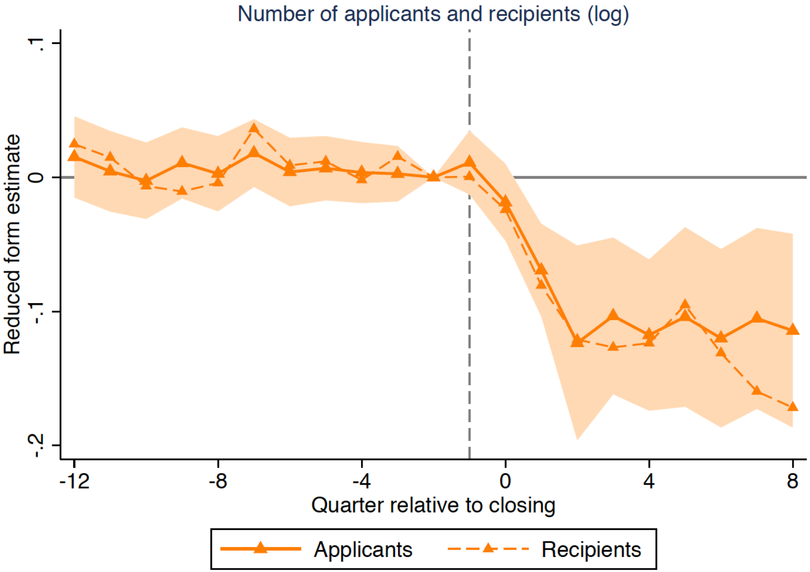
- For each closure, take the ZIPs that experience the closure as treated, and ZIPS that experience a closure more than 2 years in the future are control

- Event Study estimates:

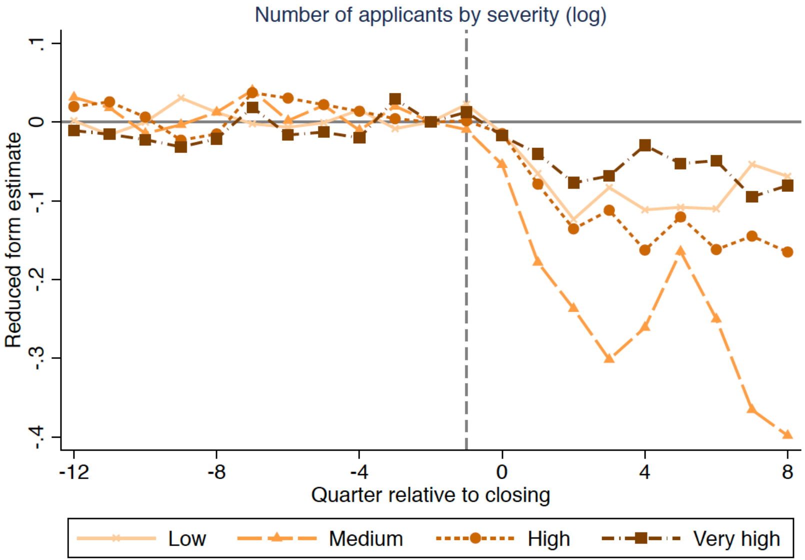
$$Y_{isct} = \alpha_i + \gamma_{st} + \sum_{\tau} D_{ct}^{\tau} + \sum \delta_{\tau} \left(Treated_{ic} \times D_{ct}^{\tau}\right) + \epsilon_{isct}$$

  where Y_{isct} is outcome for ZIP i in state s for closure c in quarter t; α_i are ZIP FEs; γ_{st} are state×quarter FEs; $Treated_{ic}$ indicates treatment; D_{ct}^{τ} are indicators for observations τ quarters before/after the closure.

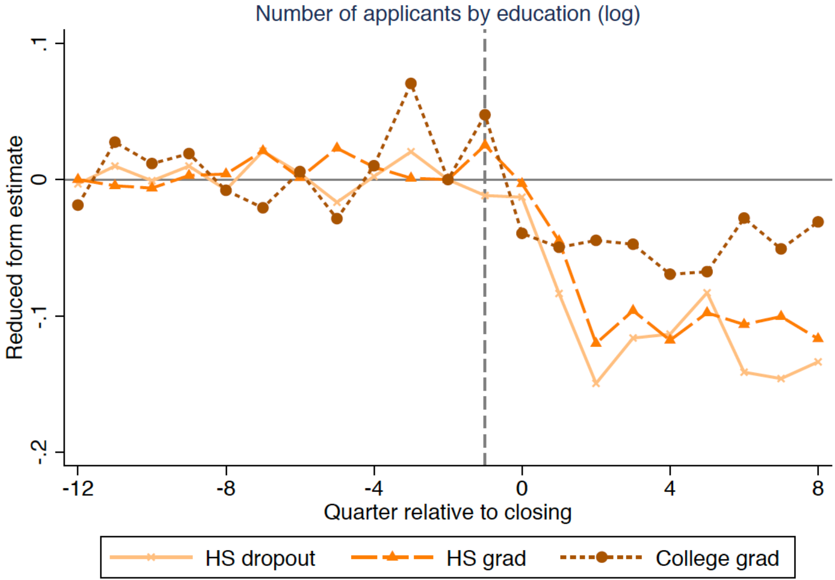
Deshpande & Li (2017): Results



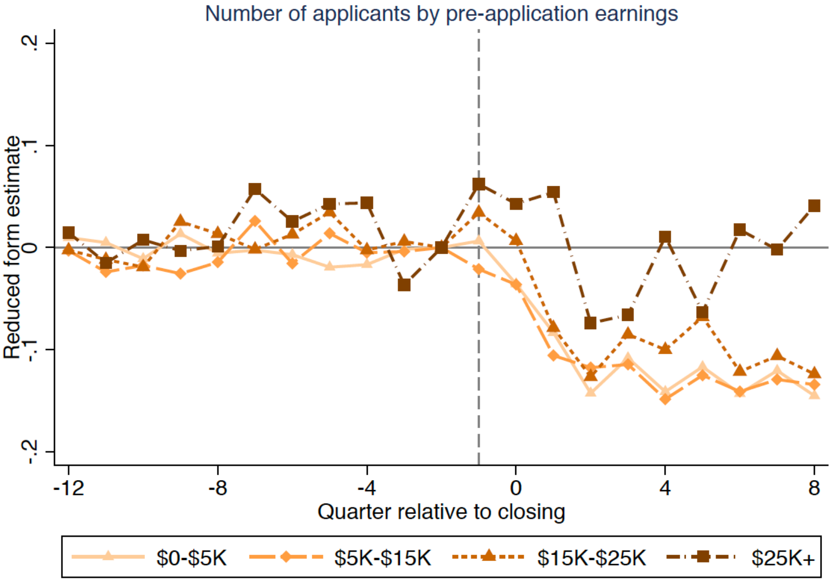
Deshpande & Li (2017): Subgroups



Deshpande & Li (2017): Subgroups



Deshpande & Li (2017): Subgroups



Deshpande & Li (2017): Spillovers

Table 4: Estimates of the Effect of Closings on Types of Application Costs

	Closing ZIP			Neighboring ZIP		
	Pt. Est.	Std. Err.	Mean	Pt. Est.	Std. Err.	Mean
Applications (log)	-0.110***	(0.0300)	39.7	-0.0539***	(0.0176)	42.5
Recipients (log)	-0.133***	(0.0312)	21.7	-0.0904***	(0.0181)	22.6
Congestion measures						
FO processing time	3.032***	(1.094)	28.8	2.804***	(0.731)	28.4
Walk-in wait times	4.352***	(1.412)	13.6	3.472***	(1.126)	16.3
Travel cost measures						
Driving time	9.974***	(1.636)	23.5			
Driving distance	11.98***	(1.338)	24.3			
Transit time	35.76***	(6.426)	89.4			

Deshpande & Li (2017): Mechanisms

- How do office closures affect applications?
  1. Congestion at neighboring offices
  2. Travel time

$$Y_{isct} = \alpha_i + \gamma_{st} + \beta Congestion_{ict} + \kappa Distance_{ict} + \delta NewOffice_{ict}$$

- To overcome endogeneity (why?) use closures and difference with next-closest office as instruments:

$$\begin{aligned} Congestion_{isct} &= \alpha_i + \gamma_{st} + \beta_1 \left(Treated_{ic} \times Post_t \times WDiff_{ic}\right) \\ &\quad + \beta_2 \left(TreatedNbr_{ic} \times Post_t\right) + \nu_{isct} \\ Distance_{isct} &= \alpha_i + \gamma_{st} + \kappa_1 \left(Treated_{ic} \times Post_t \times DDiff_{ic}\right) + \xi_{isct} \end{aligned}$$

where $WDiff_{isc}$ is difference in walk-in times with now-closest office; $TreatedNbr_{ic}$ indicates closing neighbor; $DDiff_{ic}$ is difference in driving distance with the now-closest office

Deshpande & Li (2017): Mechanisms

Table 5: IV Estimates of the Effect of Different Application Costs on Disability Applications

	First Stage Driving dist	Wait time	Red. Form Log(app)	OLS Log(app)	IV Log(app)	Inc after closing	OLS Δ in log(app)	IV Δ in log(app)
TrtXPostXDDiff	0.996***		-0.00320					
	(0.00442)		(0.00250)					
TrtXPostXWDiff		0.452***	-0.000454					
		(0.0981)	(0.000588)					
NbrXPost		4.564***	-0.0781***					
		(1.168)	(0.0107)					
New Office (TrtXPost)			-0.148***	-0.0830***	-0.0229	1	-0.083	-0.023
			(0.0348)	(0.0246)	(0.0221)			
Driving distance (km)				-0.000483	-0.000253	12	-0.006	-0.003
				(0.000431)	(0.000408)			
Wait time (min)				-0.00273***	-0.0158***	4.3	-0.012	-0.068
				(0.000647)	(0.00327)			
N	101,008	80,779	98,557	77,786	76,280			

Deshpande & Li (2017): Welfare

- Let the benefits of approving a disability application be

$$b_r(n) \equiv b_1 - c_1 - c_2(n)$$

where b_1 is net social benefit of providing benefits, c_1 is cost of reviewing application and c_2 is the cost to the applicant

- Let the costs of rejecting an application be

$$b_n(n) \equiv c_1 + c_2(n)$$

- And let F be the cost of running an office. Then social benefit of n offices is

$$\begin{aligned} W(n) &= b_r(n)\, r(n) - b_n(n)\left[a(n) - r(n)\right] - Fn \\ &= b_1 r(n) - \left[c_1 + c_2(n)\right] a(n) - Fn \end{aligned}$$

Deshpande & Li (2017): Welfare

- And so change in welfare from closing one office is

$$\begin{aligned} W(n-1) - W(n) = {} & \underbrace{b_1 \left[r(n-1) - r(n) \right]}_{\text{benefits to fewer recipients}} \\ & - \underbrace{\left[c_2(n-1) - c_2(n) \right] a(n-1)}_{\text{higher costs for applicants}} \\ & - \underbrace{c_1 \left[a(n-1) - a(n) \right]}_{\text{review fewer applications}} \\ & - \underbrace{c_2(n) \left[a(n-1) - a(n) \right]}_{\text{fewer applicants}} + F \end{aligned}$$

Deshpande & Li (2017): Welfare

Table 6: Costs and Benefits of Field Office Closings

Costs of closing (thousands)	
Lower receipt in areas surrounding closed office	\$3,100
Lower receipt in areas surrounding neighboring office	\$13,000
Higher applicant time and earnings decay	\$3,200
Total	\$19,300
Benefits of closing (thousands)	
Administrative savings from processing fewer applications	\$2,600
Administrative savings from closing field office	\$500
Application cost savings from discouraged applicants	\$1,000
Total	\$4,100

Outline

Evidence from Rich Countries

- Chetty, Friedman & Saez (AER 2013): *Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings*
- Deshpande & Li (2017) *Who is Screened Out? Application Costs and the Targeting of Disability Programs*
- Finkelstein & Notowidigdo (2019) *Take-up and Targeting: Experimental Evidence from SNAP*

Finkelstein & Notowidigdo (2019): Overview

- Incomplete take-up of safety net programs is pervasive.
- 2 typical explanations:
  1. Lack of information about eligibility
  2. Transaction costs of enrollment (including stigma)
- How does incomplete takeup affect social welfare?
  - “Ordeals” may serve as useful screens (Nichols & Zeckhauser 1982, Besley & Coate 1992)
  - Behavioral work suggesting ordeals have higher psychic costs for the poor $\Rightarrow$ Worsening targeting effectiveness.
- Ultimately empirical question

Finkelstein & Notowidigdo (2019): Overview

- This paper:
- framework for welfare effects of ordeal changes: How many people enroll? Which types of people?
- Apply framework to RCT of interventions aimed at elderly non-participants in SNAP
- 30K subjects
- 3 treatment arms

1. Information about eligibility
2. Information plus assistance with application
3. control

Finkelstein & Notowidigdo (2019): Framework

- 2 types of individuals: $j \in \{L, H\}$ with unobserved wages θ_j, $\theta_H > \theta_L$. Unit mass of each type.
- Individuals choose hours h_j and earn pre-tax income $\theta_j h_j$, incurring taxes $\tau(\theta_j h_j)$, leading to net of tax earning $y_j \equiv \theta_j h_j - \tau(\theta_j h_j)$.
- The program grants benefits B if income is below r^*.
- Individuals misperceive the benefit by ϵ_j so their *perceived* benefit of applying is $(1 + \epsilon_j) B$.
  - $\epsilon_j < 0$: underestimate benefits of applying
  - $\epsilon_j = 0$ for $j \in \{L, H\}$ is the neoclassical benchmark.

Finkelstein & Notowidigdo (2019): Framework

- Individuals have common utility
  - $u(x_j) - v(h_j)$ if they do not apply
  - $u(x_j) - v(h_j) - \left(\bar{\Lambda}\kappa_j + c\right)$ if they apply
- Disutility from applying:
  - c: individual-specific utility cost of applying. has distribution $f_j(c)$
  - $\bar{\Lambda}$: parameter affecting utility cost
  - κ_j: how this utility cost varies across types. e.g. ordeals can impose greater utility cost on H types $(\kappa_H > \kappa_L)$ or low types $(\kappa_H < \kappa_L)$

Finkelstein & Notowidigdo (2019): Framework

- Individuals make application and labor supply choices.
- Type js work h_j^A hours if they apply, h_j^{-A} if they don't.
- Low types: assume $y_j^A, y_j^{-A} < r^*$
- High types: assume $y_j^{-A} > r^*$ and to get the benefit, if apply $h_H^A = r^*/\theta_H$
- NB $h_j^A \leq h_j^{-A}$ for both types
- Individuals with $c < c_j^*$ will apply. Private welfare is therefore

$$\begin{aligned}
V_j &= \mathbb{P}\left(\text{apply}\right)\mathbb{E}\left[U\left(\right)|\text{apply}\right] + \mathbb{P}\left(\cancel{\text{a}}\text{pply}\right)\mathbb{E}\left[U\left(\right)|\ \cancel{\text{a}}\text{pply}\right] \\
&= \int_0^{c_j^*}\left(u\left(y_j^A + B\right) - v\left(h_j^A\right) - \bar{\Lambda}\kappa_j - c\right)dF_j(c) \\
&\quad + \int_{c_j^*}^{\infty}\left(u\left(y_j^{-A}\right) - v\left(h_j^{-A}\right)\right)dF_j\left(c\right)
\end{aligned}$$

Finkelstein & Notowidigdo (2019): Framework

- Individuals' decisions create a fiscal externality: Their hours decisions change tax revenue and this has a social cost that they don't internalize. For people who apply, the fiscal externality is $G_j^A = \tau\left(h_j^A \theta_j\right)$ while for those who don't it's $G_j^{-A} = \tau\left(h_j^{-A}\theta_j\right)$
- We can write total social welfare as

$$W = \underbrace{V_L + V_H}_{\text{Private Welfare}} - \underbrace{B(A_L + A_H)}_{\text{Program Cost}}$$
$$+ \underbrace{A_L G_L^A + (1 - A_L)\, G_L^{-A} + A_H G_H^A + (1 - A_H)\, G_H^{-A}}_{\text{Fiscal Externality}}$$

  where $A_j = F_j\left(c^*\right)$, the expected number of applicants of type j.
- Planner chooses tax system $\tau\left(\right)$ and transfer program (including ordeal $\bar{\Lambda}$) to maximize W.

Finkelstein & Notowidigdo (2019): Framework

- Characterize the marginal effects on welfare of the two treatments.
- Information treatment: Increase perceived benefits by $d\epsilon$
- Information plus assistance: increase benefits $d\epsilon$ and reduce cost of applying, $-d\bar{\Lambda}$
- Define:

$$\mu_j \equiv u\left(y_j^A + B\right) - u\left(y_j^A + (1+\epsilon_j)\,B\right) \quad \text{and} \quad \xi_j \equiv u'\left(y_j^A + B\right)$$

Finkelstein & Notowidigdo (2019): Framework

- We can write the effect of the information treatment on welfare as

$$
\begin{aligned}
\frac{dW}{dT}^{\text{Information only}} &= \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT}}_{\text{Change in Private Welfare}} \\
&- \underbrace{B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT}\right)}_{\text{Change in Mechanical Program Costs}} \\
&+ \underbrace{\left(G_L^A - G_L^{-A}\right)\frac{dA_L}{dT} + \left(G_H^A - G_H^{-A}\right)\frac{dA_H}{dT}}_{\text{Fiscal Externality}}
\end{aligned}
$$

Finkelstein & Notowidigdo (2019): Framework

- We can write the effect of the information plus assistance treatment as

$$
\begin{aligned}
\frac{dW}{dT}^{\text{Information + Assistance}} &= \underbrace{\mu_L \frac{dA_L}{dT} + \mu_H \frac{dA_H}{dT} + \kappa_L A_L + \kappa_H A_H}_{\text{Change in Private Welfare}} \\
&- \underbrace{B\left(\frac{dA_L}{dT} + \frac{dA_H}{dT}\right)}_{\text{Change in Mechanical Program Costs}} \\
&+ \underbrace{\left(G_L^A - G_L^{-A}\right)\frac{dA_L}{dT} + \left(G_H^A - G_H^{-A}\right)\frac{dA_H}{dT}}_{\text{Fiscal Externality}}
\end{aligned}
$$

Finkelstein & Notowidigdo (2019): Framework

- Define targeting as $e = E_L/(E_L + E_H)$, the share of enrollees who are L types.
- Then a treatment T increases targeting if $de/dT > 0$
- *Holding constant the change in applications due to an intervention, the change in social welfare in response to an improvement in targeting* $(de/dT > 0)$ *from an Information Only (or Information Plus Assistance) treatment is given by the following expression:*

$$\frac{\partial}{\partial (de/dT)} \left(\frac{dW}{dT} \right) \bigg|_{\frac{dA}{dT}} = \left[(\mu_L - \mu_H) + \left(G_L^A - G_L^{-A} \right) - \left(G_H^A - G_H^{-A} \right) \right] \times (E_H + E_L)$$

- Note this is increasing in $\mu_L - \mu_H$. Sufficient condition for better targeting to increase private welfare is $\epsilon_L \leq \epsilon_H \leq 0$ with at least 1 strict inequality, or $\epsilon_H = \epsilon_L < 0$

Finkelstein & Notowidigdo (2019): Empirical Setting

- Work with households aged 60 or over in Pennsylvania in 2016
- To enroll in SNAP, individuals must complete an application, provide documents verifying circumstances, and do an interview (in person or by phone). Provide info on income, various household expenses (medical expenses, rent, utilities etc).
- Once enrolled, eligible for 36 months of benefits. Benefits are a decreasing function of net income.
- SNAP benefits average 15% of annual income for eligible households.

Finkelstein & Notowidigdo (2019): Interventions

- Partnered with Benefits Data Trust (BDT), a national non-profit working to help people access benefits.
- State of Pennsylvania provided BDT admin data on individuals aged >60 and on Medicaid but not SNAP. Likely to be eligible for SNAP.
- Randomized the 30K individuals into equally-sized groups.
- Information Treatment got outreach materials informing them of likely SNAP eligibility and benefits they might receive, and info on how to call DHS to apply.
- Assistance component: Info Treatment contained BDT number. If individuals call BDT, they help with the application. Advise on documents, can populate and submit application on their behalf etc.
- BDT submitted $\sim 70\%$ of applications in this treatment arm, spent on average 47 mins on phone with applicants, 30 with those who don't end up applying.

Finkelstein & Notowidigdo (2019)

Table 1: Description of Study Population

	Original Outreach List	List, After Exclusions	After Exclusions Receving SNAP	After Exclusions Not Receiving SNAP	Study Population
	(1)	(2)	(3)	(4)	(5)
Observations (N)	229,584	143,923	84,038	59,885	31,888
Panel A - Demographics					
Age (as of October 31, 2015)	72.91	70.45	69.77	71.42	68.83
Share Age above Median = 65	0.72	0.66	0.66	0.66	0.50
Share Age 80+	0.27	0.18	0.15	0.23	0.16
Male	0.35	0.36	0.36	0.36	0.38
Share White[a]	0.71	0.79	0.79	0.79	0.75
Share Black[a]	0.17	0.10	0.11	0.07	0.08
Share Primary Language not English	0.04	0.03	0.03	0.03	0.04
Share Living in Philadelphia	0.18	0.00	0.00	0.00	0.00
Share Living in Pittsburgh	0.05	0.07	0.07	0.06	0.06
Share Last Medicaid Spell Starting before 2011	0.45	0.47	0.55	0.36	0.33
Share Enrolled in Medicaid for 2015 Full Year	0.83	0.84	0.89	0.77	0.73
Panel B - (Annual) Health Care Measures, 2015					
Total Health Care Spending ($)[b]	18,347	7,683	6,036	9,995	11,838
Number of Hospital Days	5.41	1.51	1.24	1.88	2.16
Number of ER Visits	0.41	0.41	0.41	0.40	0.50
Number of Doctor Visits	6.25	5.87	5.97	5.74	7.11
Number of SNF Days	66.23	1.57	0.85	2.58	2.67
Number of Chronic Conditions	6.50	4.93	5.08	4.70	5.45

Finkelstein & Notowidigdo (2019)

Table 2: Behavioral Responses to "Information Only" and "Information Plus Assistance"

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
SNAP Enrollees	0.058	0.105	0.176	
		[0.000]	[0.000]	[0.000]
SNAP Applicants	0.077	0.147	0.238	
		[0.000]	[0.000]	[0.000]
SNAP Rejections among Applicants	0.233	0.266	0.255	
		[0.119]	[0.202]	[0.557]
Callers	0.000	0.267	0.301	
		[0.000]	[0.000]	[0.000]
Adjusted Callers	0.000	0.289	0.301	
		[0.000]	[0.000]	[0.156]

Finkelstein & Notowidigdo (2019)

SNAP Applicants among Non-Callers	0.077	0.086	0.081	
		[0.063]	[0.324]	[0.363]
SNAP Applicants among Callers	0.000	0.313	0.602	
		[0.000]	[0.000]	[0.000]
SNAP Enrollees among Non-Callers	0.058	0.061	0.059	
		[0.442]	[0.713]	[0.688]
SNAP Enrollees among Callers	0.000	0.226	0.450	
		[0.000]	[0.000]	[0.000]
Observations (N)	10,630	5,314	10,629	

Notes: Columns 1 through 3 shows means by intervention arm with the p-value (relative to the control arm) in [square brackets]. Column 1 shows the control. Column 2 shows the Information Only arm (for the two equally-sized pooled sub-treatments). Column 3 shows the Information Plus Assistance arms (weighted so that the two pooled sub-treatments received equal weight). Column 4 reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. All outcomes are binary rates measured during the nine months from the initial mail date. All p-values are based on heteroskedasticity-robust standard errors. Callers are measured for the relevant call number and are therefore mechanically zero for the control; see text for a description of the adjusted caller rate.

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Table 4: Enrollee Monthly Benefits and Predicted Benefits

	Control	Information Only	Information Plus Assistance	P Value of Difference (Column 2 vs 3)
	(1)	(2)	(3)	(4)
Benefit Amount	145.94	115.38	101.32	
		[0.000]	[0.000]	[0.013]
Share $16 Benefit	0.192	0.312	0.367	
		[0.000]	[0.000]	[0.021]
Share $194 Benefit	0.206	0.164	0.147	
		[0.076]	[0.003]	[0.352]
Share $357 Benefit	0.060	0.052	0.040	
		[0.587]	[0.077]	[0.259]
Share Missing Benefit	0.073	0.043	0.028	
		[0.025]	[0.000]	[0.139]

Finkelstein & Notowidigdo (2019)

Predicted Benefit for Enrollees w/ Actual Benefit	140.20	112.49	102.93	
		[0.000]	[0.000]	[0.086]
Predicted Benefit for All Enrollees	138.65	114.01	104.03	
		[0.000]	[0.000]	[0.068]
Share of Enrollees in Household Size of 1	0.657	0.714	0.760	
		[0.038]	[0.000]	[0.036]
Benefit Amount for Enrollees in Household Size of 1	116.97	93.35	85.82	
		[0.000]	[0.000]	[0.134]
Observations (N)	613	559	1,861	

Notes: Sample is individuals who enrolled in the 9 months after their initial mailing. Columns 1 through 3 shows means by intervention arm with the p-value (relative to the control arm) in [square brackets] for SNAP enrollees. Column 1 shows the control. Column 2 shows the Information Only arm (with the two equally-sized sub-treatments pooled). Column 3 shows the Information Plus Assistance arms (weighted so that the two pooled sub-treatments received equal weight). Column 4 reports the p-value of the difference between the Information Plus Assistance and Information Only treatment arms. See text for a description of the predicted benefits. All p-values are based on heteroskedasticity-robust standard errors. N reports the sample size of enrollees.

Finkelstein & Notowidigdo (2019)

Table 5: Demographic and Health Characteristics: Applicants and Enrollees

	Applicants				Enrollees			
	Means			P Value	Means			P Value
	Control	Info Only	Info Plus Assistance	Info Plus Assistance vs Info Only	Control	Info Only	Info Plus Assistance	Info Plus Assistance vs Info Only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
<u>Panel A - Predicted Benefits</u>								
Predicted Benefits	148.26	125.65	115.36		138.65	114.01	104.03	
		[0.000]	[0.000]	[0.037]		[0.000]	[0.000]	[0.068]
<u>Panel B - (Annual) Health Care Measures, 2015</u>								
Total Health Care Spending ($) [a]	9,424	8,605	8,334		10,238	9,532	8,603	
		[0.517]	[0.300]	[0.781]		[0.661]	[0.208]	[0.459]
Total Number of Visits and Days	13.33	11.67	9.92		14.79	10.90	9.92	
		[0.331]	[0.018]	[0.166]		[0.058]	[0.008]	[0.467]
Weighted Total Number of Visits and Days	4,661	3,273	2,818		5,407	3,288	2,779	
		[0.128]	[0.022]	[0.442]		[0.064]	[0.011]	[0.461]
Number of Chronic Conditions	6.21	5.55	5.27		6.54	5.43	5.37	
		[0.094]	[0.006]	[0.383]		[0.019]	[0.005]	[0.875]

Finkelstein & Notowidigdo (2019)

Panel C - Demographics								
Share Age above Median = 65	0.41	0.46	0.46		0.39	0.43	0.46	
		[0.072]	[0.014]	[0.764]		[0.282]	[0.006]	[0.159]
Share Age 80+	0.06	0.11	0.14		0.07	0.12	0.14	
		[0.001]	[0.000]	[0.042]		[0.005]	[0.000]	[0.085]
Male	0.41	0.40	0.38		0.39	0.42	0.38	
		[0.983]	[0.232]	[0.250]		[0.446]	[0.444]	[0.104]
Share White [b]	0.67	0.73	0.74		0.71	0.78	0.78	
		[0.005]	[0.000]	[0.554]		[0.004]	[0.001]	[0.958]
Share Black [b]	0.10	0.08	0.11		0.11	0.07	0.10	
		[0.103]	[0.577]	[0.011]		[0.011]	[0.833]	[0.004]
Share Primary Language not English	0.08	0.06	0.04		0.06	0.05	0.03	
		[0.141]	[0.000]	[0.012]		[0.242]	[0.002]	[0.067]
Share Living in Pittsburgh	0.05	0.06	0.07		0.05	0.06	0.07	
		[0.385]	[0.066]	[0.459]		[0.374]	[0.028]	[0.310]
Share Last Medicaid Spell Starting before 2011	0.25	0.30	0.29		0.26	0.33	0.31	
		[0.022]	[0.017]	[0.704]		[0.009]	[0.026]	[0.348]
Observations (N)	817	781	2,519		613	559	1,861	

Finkelstein & Notowidigdo (2019)

Table 6: Demographic and Health Characteristics: Callers and non-Callers

	Callers (1)	Non-callers (2)	P Value of Difference (3)
Panel A - Predicted Benefits			
Predicted Benefits	106.99	114.68	[0.000]
Predicted Enrollment	0.05	0.05	[0.752]
Panel B - (Annual) Health Care Measures, 2015			
Total Health Care Spending ($) [a]	7,316	13,656	[0.000]
Total Number of Visits and Days	9.52	13.50	[0.000]
Weighted Total Number of Visits and Days	2,853	5,064	[0.000]
Number of Chronic Conditions	5.16	5.48	[0.024]

Finkelstein & Notowidigdo (2019)

Panel C - Demographics

Share Age above Median = 65	0.49	0.51	[0.014]
Share Age 80+	0.16	0.17	[0.190]
Male	0.38	0.38	[0.977]
Share White [b]	0.77	0.74	[0.000]
Share Black [b]	0.09	0.07	[0.006]
Share Primary Language not English	0.03	0.05	[0.000]
Share Living in Pittsburgh	0.06	0.06	[0.658]
Share Last Medicaid Spell Starting before 2011	0.32	0.34	[0.044]
Observations (N)	4,597	11,346	

Finkelstein & Notowidigdo (2019): Interpreting Results

- How can we take these results back to the normative framework?

1. Add exogenous probability π_j that application is accepted
2. Allow for different benefit levels $\overline{B}$ for low types and B_{min} for high types with $\overline{B} > B_{min}$

- Expressions for welfare effects are as before but mechanical effect becomes

$$\pi_H B_{min} \frac{dA_H}{dT} + \pi_L \overline{B} \frac{dA_L}{dT}$$

- $B_{min} = \$16/$month, $\overline{B} = \$178/$month and $\pi_L = \pi_H = 0.75$ yields $\pi_L B_L = \$4,086$ for L types and $\$432$ for H types.
- Baseline: Fiscal externality is public cost of processing applications: $G_L^A = G_H^A = -g = -\$267$ and $G_L^{-A} = G_H^{-A} = 0$
- First-order approximation to expected utility and time cost of applying of \$75 yields $\epsilon_L = -0.98$ and $\epsilon_H = -0.83$

Finkelstein & Notowidigdo (2019): Interpreting Results

- To make statements about social welfare impact of interventions need
  - $G_j^A - G_j^{-A}$. Use $-g$
  - μ_J. First-order approximation to utility yields approximations $\mu_H \approx \xi_H \pi_H \epsilon_H B_{min}$ and $\mu_L \approx \xi_L \pi_L \epsilon_L \overline{B}$.
- Write welfare effect as ratio of marginal benefits (private welfare increase) to marginal social costs (mech and fiscal externality) and use these approximations to get

$$MVPF^{\text{Info}} = \frac{-\epsilon_L \pi_L \overline{B} \frac{dA_L}{dT} - \epsilon_H \pi_H B_{min} \frac{dA_H}{dT}}{\left(\pi_L \overline{B} + g\right) \frac{dA_L}{dT} + \left(\pi_H B_{min} + g\right) \frac{dA_H}{dT}} \approx 0.89$$

$$MVPF^{\text{I + A}} = \frac{-\epsilon_L \pi_L \overline{B} \frac{dA_L}{dT} - \epsilon_H \pi_H B_{min} \frac{dA_H}{dT} - \left(A_H - A_L + \frac{dA_H}{dT} + \frac{dA_L}{dT}\right) \frac{dc}{dT}}{\left(\pi_L \overline{B} + g\right) \frac{dA_L}{dT} + \left(\pi_H B_{min} + g\right) \frac{dA_H}{dT}}$$

$$\approx 0.93$$

Outline

Motivating Facts

Theory

Evidence from Rich Countries

Targeting in Developing Countries: Who gets the Benefit?

Transfer Design: What is the Benefit?

Open Questions

Outline

Targeting in Developing Countries: Who gets the Benefit?

- Alatas, Banerjee, Hanna, Olken & Tobias (AER 2012) *Targeting the Poor: Evidence from a Field Experiment in Indonesia*
- Alatas, Banerjee, Hanna, Olken, Purnamasari & Wai-Poi (JPE 2016) *Self-Targeting: Evidence from a Field Experiment in Indonesia*
- Banerjee Hanna Olken & Sumarto (WP 2018) *The (lack of) Distortionary Effects of Proxy-Means Tests: Results from a Nationwide Experiment in Indonesia*
- Haushofer, Niehaus, Paramo, Miguel & Walker (2022) *Targeting Impact versus Deprivation*

Alatas et al (2012): Overview

- How effective are proxy means tests (PMT) in practice in a developing country?
- In particular, do communities know more than the proxies and can that be leveraged to improve targeting?
- What is the tradeoff?
  - Communities have better information.
    - Assets capture permanent income. Wouldn't observe severe negative shocks.
    - Poor PMT seen as illegitimate by community.
  - Communities may disagree with government
    - May perceive poverty differently
    - May favor friends/relatives of elites
- This paper does an experiment in Indonesia to explore this

Alatas et al (2012): Experimental Design

- Indonesia has a large targeted cash transfer: Bantuan Langsung Tunai (BLT)
  - Provides \$10/month to 19 million households
  - Seen as badly targeted: WB estimates 45% of funds go to nonpoor households, 47% of poor excluded.
- For the experiment sample
  - 640 villages in 3 provinces (North Sumatra, South Sulawesi, Central Java).
  - Within each, sample a neighborhood.
  - 30% urban, 70% rural
- In each village,
  - Government and an Indonesian NGO Mitra Samya implemented an unconditional cash transfer.
  - Each beneficiary household to receive RP. 30,000 ($\sim$\$3)
  - Overall number of beneficiaries set in advance

Alatas et al (2012): Experimental Design

T1 *PMT*. Government used 49 indicators (home attributes, assets, hh composition, education, occupation etc) and a regression to create poverty scores. Enumerators collect this data and generate poverty scores. Lowest scores got the transfer.

T2 *Community Targeting.* Facilitator holds community meeting. List all households in the village and go through them one by one to rank them by poverty. After ranking finalized, quota of beneficiaries revealed.

T3 *Hybrid.* Do the community ranking. Then "verify" 1.5 $\times$ quota using PMT scores.

- Community subtreatments:
  - Elite capture: Either whole community or only elite invited to the meeting
  - Randomize the order in which households are ranked to test for effort effects (fatigue).
  - Preferences: Some meetings in the day (more women) others in the evening (more men), others place high emphasis on poverty.

Alatas et al (2012): Randomization

- To ensure balance, create 51 strata by geography.
- Randomize 640 subvillages into the 3 main treatments. Equal proportions in each stratum (up to integer constraint)
- Randomize the community and hybrid villages into subtreatments, stratifying by stratum $\times$ treatment
- Timing
  - Nov/Dec 2008. Carry out baseline survey
  - Dec 2008/ Jan 2009: Implement treatments, create beneficiary lists
  - Feb 2009: Distribute funds, interview village heads
  - March 2009: Endline survey

Alatas et al (2012): Randomization

Table 1—Randomization Design

Community/hybrid subtreatments			Main treatments		
			Community	Hybrid	PMT
Elite	10 poorest first	Day	24	23	
		Night	26	32	
	No 10 poorest first	Day	29	20	
		Night	29	34	
Whole community	10 poorest first	Day	29	28	
		Night	29	23	
	No 10 poorest first	Day	28	33	
		Night	20	24	
		Total	214	217	209

Alatas et al (2012): BLT targeting

TABLE 2—SUMMARY STATISTICS

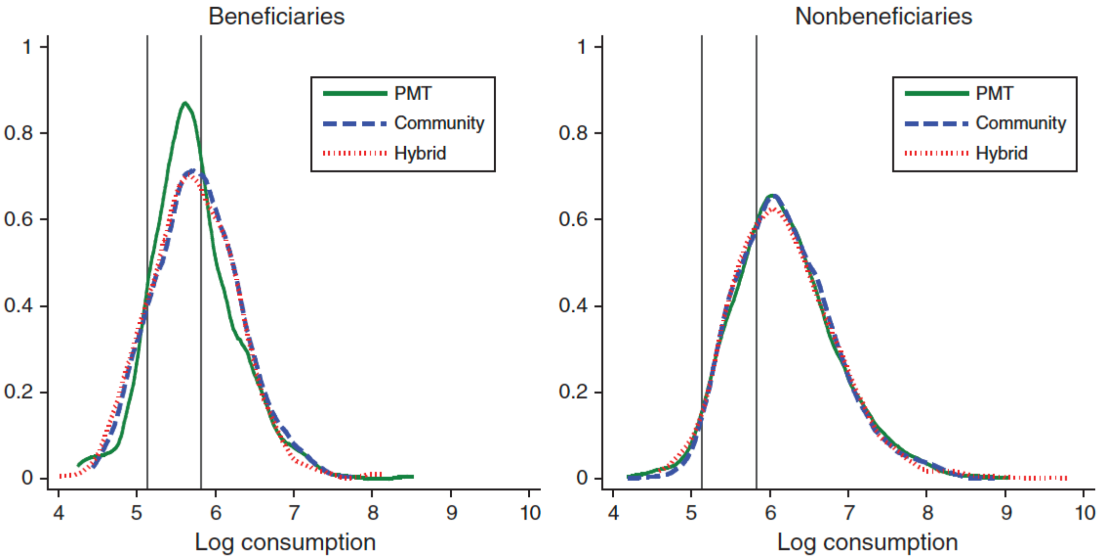
Variable	Obs	Mean	SD
Panel A. Consumption from baseline survey			
Per capita consumption (Rp. 1,000s)	5,753	557.501	602.33
Panel B. Mistargeting variables			
On beneficiary list	5,756	0.30	0.46
Error rate based on consumption	5,753	0.32	0.47
Inclusion error (nonpoor = rich + middle)	3,725	0.20	0.40
Exclusion error (poor = near + very poor)	2,028	0.53	0.50
Error rate based on consumption – rich	1,843	0.14	0.35
Error rate based on consumption – middle income	1,882	0.27	0.44
Error rate based on consumption – near poor	1,074	0.59	0.49
Error rate based on consumption – very poor	954	0.46	0.50
Panel C. Rank correlations between treatment results and …			
Per capita consumption	640	0.41	0.34
Community (excluding subvillage head)	640	0.64	0.33
Subvillage head	640	0.58	0.41
Self-assessment	637	0.40	0.34

Alatas et al (2012): Targeting Results

TABLE 3—RESULTS OF DIFFERENT TARGETING METHODS ON ERROR RATE BASED ON CONSUMPTION

		By income status		By detailed income status				
Sample:	Full population (1)	Inclusion error (2)	Exclusion error (3)	Rich (4)	Middle income (5)	Near poor (6)	Very poor (7)	Per capita consumption of beneficiaries (8)
Community treatment	0.031*	0.046**	0.022	0.028	0.067**	0.49	−0.013	9.933
	(0.017)	(0.018)	(0.028)	(0.021)	(0.027)	(0.038)	(0.039)	(18.742)
Hybrid treatment	0.029*	0.037**	0.009	0.020	0.052**	0.031	−0.008	−1.155
	(0.016)	(0.017)	(0.027)	(0.020)	(0.025)	(0.037)	(0.037)	(19.302)
Observations	5,753	3,725	2,028	1,843	1,882	1,074	954	1,719
Mean in PMT treatment	0.30	0.18	0.52	0.13	0.23	0.55	0.48	366

Alatas et al (2012): Targeting Results



Alatas et al (2012): Elite Capture

- Is the reason that community targeting doesn't dominate PMT elite capture?
- Include elite subtreatment.
- Also estimate whether households connected to the elite are more likely to receive the transfer.

$$\begin{aligned} ERROR_{ivk} = \alpha &+ \beta_1 COMMUNITY_{ivk} + \beta_2 HYBRID_{ivk} \\ &+ \beta_3 ELITE_{ivk} + \beta_4 CONN_{ivk} \\ &+ \beta_5 \left(COMMUNITY_{ivk} \times CONN_{ivk} \right) \\ &+ \beta_6 \left(HYBRID_{ivk} \times CONN_{ivk} \right) \\ &+ \beta_7 \left(ELITE_{ivk} \times CONN_{ivk} \right) + \gamma_k + \varepsilon_{ivk} \end{aligned}$$

TABLE 7—ELITE TREATMENTS

	Attendance (survey data)	Full sample error rate	Full sample error rate		On beneficiary list	
	(1)	(2)	(3)	(4)	(5)	(6)
Community treatment	0.367***	0.029	0.033	0.048*	0.042*	0.054*
	(0.038)	(0.018)	(0.023)	(0.025)	(0.025)	(0.028)
Hybrid treatment	0.370***	0.027	0.024	0.008	0.025	0.012
	(0.037)	(0.018)	(0.022)	(0.024)	(0.022)	(0.023)
Elite subtreatment	−0.301***	0.004	0.016	−0.013	−0.015	−0.039
	(0.034)	(0.016)	(0.020)	(0.029)	(0.021)	(0.032)
Elite × hybrid				0.062		0.051
				(0.041)		(0.043)
Elite connectedness			−0.025	−0.025	−0.063***	−0.063***
			(0.021)	(0.021)	(0.021)	(0.021)
Elite connectedness × community treatment			−0.015	−0.013	−0.067**	−0.078**
			(0.035)	(0.038)	(0.033)	(0.036)
Elite connectedness × hybrid treatment			0.010	0.010	−0.013	−0.001
			(0.033)	(0.035)	(0.033)	(0.035)
Elite connectedness × elite treatment			−0.029	−0.034	0.041	0.064
			(0.031)	(0.047)	(0.030)	(0.042)
Elite connectedness × elite treatment × hybrid				0.003		−0.047
				(0.063)		(0.060)
Observations	287	5,753	5,753	5,753	5,756	5,756
Mean in PMT treatment	0.11	0.30	0.30	0.30	0.28	0.28

Alatas et al (2012): Alternative Welfare Metrics

$RANKCORR_{vkR} = \alpha + \beta_1 COMMUNITY_{vk} + \beta_2 HYBRID_{vk} + \gamma_k + \varepsilon_{vkR}$,
$RANKCORR_{vkR}$ is rank correlation between targeting rank list and well-being measure R

TABLE 9—ASSESSING TARGETING TREATMENTS USING ALTERNATIVE WELFARE METRICS

	Consumption (r_g) (1)	Community survey ranks (r_c) (2)	Subvillage head survey ranks(r_e) (3)	Self-assessment (r_s) (4)
Community treatment	−0.065**	0.246***	0.248***	0.102***
	(0.033)	(0.029)	(0.038)	(0.033)
Hybrid treatment	−0.067**	0.143***	0.128***	0.075**
	(0.033)	(0.029)	(0.038)	(0.033)
Observations	640	640	640	637
Mean in PMT treatment	0.451	0.506	0.456	0.343

Table 12—What is the Community Maximizing?

	Rank according to welfare metric			Targeting rank list in		
	Community survey ranks (r_c) (1)	Subvillage head survey ranks(r_e) (2)	Self-assessment (r_s) (3)	PMT villages (4)	Community villages (5)	Hybrid villages (6)
Log per capita consumption	0.176***	0.145***	0.087***	0.132***	0.197***	0.162***
	(0.008)	(0.008)	(0.004)	(0.013)	(0.014)	(0.014)
Panel A. Household demographics						
Log HH size	0.164***	0.134***	0.073***	–0.028	0.154***	0.078***
	(0.011)	(0.010)	(0.006)	(0.019)	(0.019)	(0.021)
Share kids	–0.125***	–0.094***	–0.037***	–0.296***	–0.068*	–0.141***
	(0.021)	(0.021)	(0.012)	(0.035)	(0.041)	(0.039)

Panel B. Ability to smooth shocks						
Elite connected	0.092***	0.044***	0.025***	0.062***	0.051***	0.043***
	(0.008)	(0.009)	(0.005)	(0.016)	(0.015)	(0.015)
Total connectedness	−0.039***	−0.021**	−0.015***	−0.016	−0.019	−0.054***
	(0.010)	(0.009)	(0.005)	(0.017)	(0.017)	(0.019)
Number of family members outside subvillage	0.012***	0.010***	0.006***	0.020***	0.001	0.001
	(0.004)	(0.003)	(0.002)	(0.006)	(0.006)	(0.006)
Participation through work to community projects	0.002	0.021**	0.005	0.000	0.010	0.003
	(0.011)	(0.010)	(0.006)	(0.018)	(0.019)	(0.019)
Participation through money to community projects	0.061***	0.041***	0.024***	0.056***	0.058***	0.034*
	(0.009)	(0.009)	(0.005)	(0.016)	(0.016)	(0.018)
Participation in religious groups	0.027***	0.033***	0.014**	0.033**	0.012	0.029
	(0.010)	(0.010)	(0.006)	(0.016)	(0.017)	(0.017)
Total savings	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Share of savings in a bank	0.096***	0.069***	0.052***	0.121***	0.103***	0.075***
	(0.011)	(0.010)	(0.006)	(0.018)	(0.021)	(0.020)
Debt as share of consumption	0.005***	0.004***	0.002***	0.002	0.007***	0.008***
	(0.001)	(0.001)	(0.000)	(0.002)	(0.001)	(0.001)
Panel C. Discrimination against minorities?						
Ethnic minority	−0.024*	−0.019	−0.003	0.012	−0.051**	−0.011
	(0.014)	(0.014)	(0.008)	(0.026)	(0.025)	(0.024)
Religious minority	0.012	−0.007	−0.014*	−0.018	0.025	0.012
	(0.018)	(0.017)	(0.008)	(0.030)	(0.032)	(0.033)

Panel D. Correcting for earnings ability

HH head with primary education or less	−0.028***	−0.025***	−0.037***	−0.108***	−0.011	−0.066***
	(0.009)	(0.009)	(0.005)	(0.017)	(0.018)	(0.017)
Widow	−0.104***	−0.083***	−0.012	0.009	−0.108***	−0.026
	(0.014)	(0.014)	(0.008)	(0.027)	(0.024)	(0.028)
Disability	−0.045***	−0.037***	−0.026***	−0.079***	0.009	0.012
	(0.016)	(0.014)	(0.008)	(0.027)	(0.026)	(0.027)
Death	−0.041*	−0.031	−0.010	−0.111***	−0.013	−0.059
	(0.025)	(0.025)	(0.015)	(0.042)	(0.048)	(0.043)
Sick	−0.038***	−0.041***	−0.028***	0.007	−0.018	−0.044**
	(0.011)	(0.011)	(0.006)	(0.018)	(0.019)	(0.019)
Recent shock to income	−0.001	−0.005	−0.013**	−0.019	0.009	−0.012
	(0.009)	(0.009)	(0.005)	(0.016)	(0.016)	(0.017)
Tobacco and alcohol consumption	−0.0002***	−0.0002***	−0.0001***	−0.0002***	−0.0002***	−0.0001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Observations	5,337	4,680	5,724	1,814	1,876	1,889

Outline

Targeting in Developing Countries: Who gets the Benefit?

- Alatas, Banerjee, Hanna, Olken & Tobias (AER 2012) *Targeting the Poor: Evidence from a Field Experiment in Indonesia*
- Alatas, Banerjee, Hanna, Olken, Purnamasari & Wai-Poi (JPE 2016) *Self-Targeting: Evidence from a Field Experiment in Indonesia*
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- Haushofer, Niehaus, Paramo, Miguel & Walker (2022) *Targeting Impact versus Deprivation*

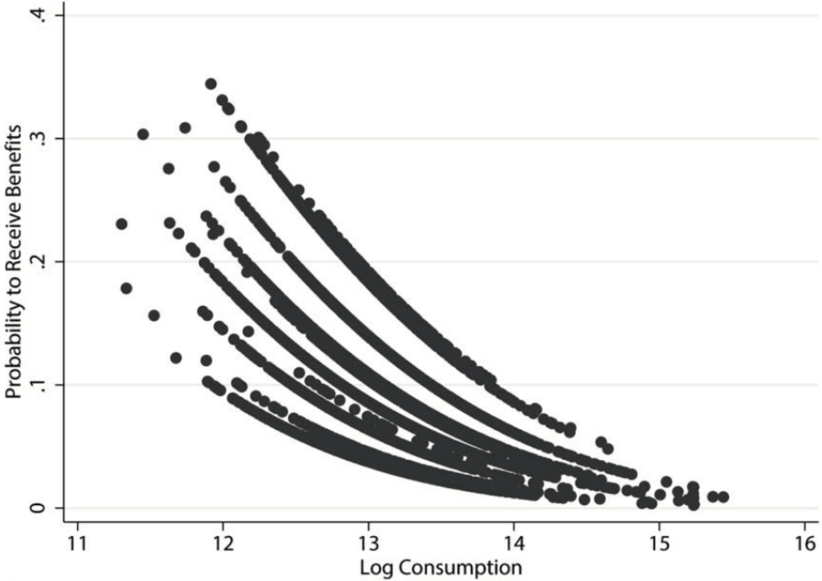
Alatas et al (2016): Overview

- Do application costs (ordeals a la Nichols & Zeckhauser) improve targeting in a developing country context?
- If so, how big should application costs be?
- Implement an experiment in Indonesia to learn about this.
- Use experimental results together with a model to learn about selection margins and effects of alternative policies.

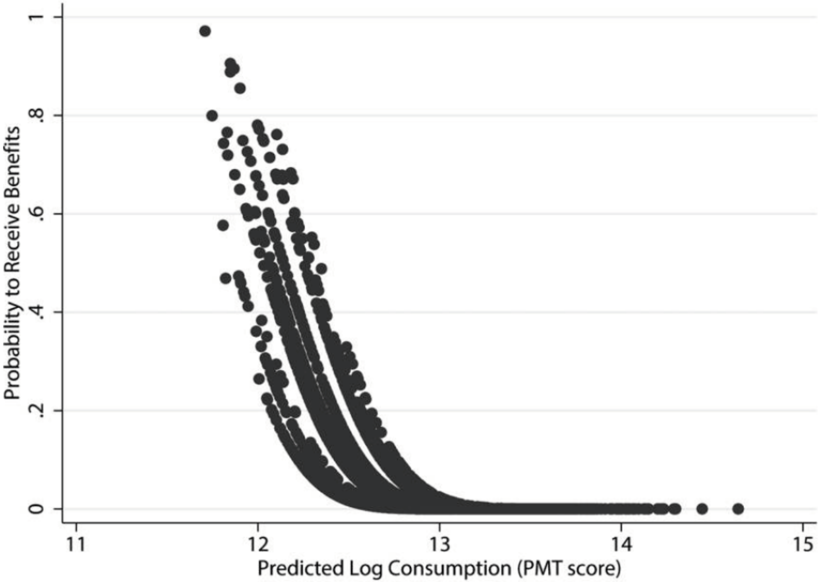
Alatas et al (2016): Setting

- Study a large transfer program in Indonesia: Program Keluarga Harapan (PKH)
- Targeted at households with consumption/capita < 80% of poverty line, and with pregnant woman/children 0-5/children with <9 years of schooling.
- Recipients get \$67–\$250/year (3.5–13% of avg consumption)
- 2.4 million households enrolled in 2013.
- Targeting is hard: per-capita consumption is hard to observe.
- The government does a PMT.
  - Every 3 years, stats bureau does a nationwide survey.
  - Potentially eligible households + local leaders' recommendations surveyed. Asked 30 questions.
  - Supplemented with location-level data
  - Generate district-level formula for predicting consumption.

Alatas et al (2016): Targeting



Alatas et al (2016): Targeting



Alatas et al (2016): Experiment Design

- PKH expanded to new areas in 2011
- Work in six districts (2 each in Lampung, South Sumatra, and Central Java)
- randomly select 400 villages stratifying to have 30% urban, 70% rural and by geography.

T1 *Automatic Screening*: Government enumerators make list of potentially poor households. Administer survey. Computer generated scores below cutoff for that district $\rightarrow$ receive transfer

T2 *Self-Targeting:* The test was the same, but households had to go to a central registration station to apply. Meetings held in each village to publicize the program, and emphasize that survey would be verified. On predetermined day, households could visit office from 8AM-5PM, get a number and line up for their interview.

- Within Self-targeting treatment also cary distance between village and office.

Alatas et al (2016): Experimental Design

TABLE 1
EXPERIMENTAL DESIGN

	Number of Villages (Households)
Automatic screening	200 (1,998)
Self-targeting:	
Close subtreatment	100 (1,000)
Far subtreatment	100 (1,000)
Total	200 (2,000)

NOTES.—This table provides the number of villages in each treatment cell. The number of households in each cell is shown in parentheses.

Alatas et al (2016): Timing

Time Line of the Experiment

	Self-Targeting Villages	Automatic Screening Villages
December 2010 to March 2011	Baseline survey	
January to April 2011	Application process publicized. Registration days: Households that showed up to apply received the PMT interview at the registration site. Verification process: A subset of households received home visits and received another PMT interview.	Prescreen list: Households suggested by village leaders or BPS enumerators were added to the prescreen list. PMT interviews: BPS enumerators conducted home visits and PMT interviews with all prescreened households.
Early August 2011	Midline survey	
Late August 2011	Beneficiary lists were announced to the villages. First round of PKH benefits distributed.	
January to March 2012	Endline survey	

Alatas et al (2016): Model

- Households live for two periods. risk neutral, care about consumption.
- Per-period income of y. Only y^o is observable to the government, so $y = y^o + y^u$.
- Applying for the transfer costs $c\left(l, y\right)$ where l is the distance to the office.
- If apply, receive the transfer with probability $\mu\left(y^o\right)$, $\mu' \leq 0$.
- Even observable income measured with error by PMT so $\mu\left(y^o\right) = \mathbb{P}\left(y^o + \pi < y^*\right)$, π is iid noise.
- 2 types of households:
  - sophisticated: Understand $\mu\left(y^o\right)$
  - unsophisticated: Know $\lambda\left(y\right)$=probability someone with income y receives transfer
- Transfer recipients receive income b in second period. δ is discount factor
- Households also get a utility shock $\varepsilon \sim F\left(\varepsilon\right)$ that encourages/discourages them from applying.

Alatas et al (2016): Application decision

- Sophisticated households who apply have expected utility

$$y - c\left(l, y\right) + \mu\left(y^{o}\right)\delta\left(y + b\right) + \left[1 - \mu\left(y^{o}\right)\right]\delta y + \varepsilon$$

- Unsophisticated households who apply have EU

$$y - c\left(l, y\right) + \lambda\left(y\right)\delta\left(y + b\right) + \left[1 - \lambda\left(y\right)\right]\delta y + \varepsilon$$

- If households don't apply, they have EU $y + \delta y$
- Expected gain from applying for the sophisticated hhs is

$$-c\left(l, y\right) + \mu\left(y^{o}\right)\delta b + \varepsilon \equiv g\left(y^{o}, y, l\right) + \varepsilon$$

- For unsophisticated, it's

$$-c\left(l, y\right) + \lambda\left(y\right)\delta b + \varepsilon \equiv h\left(y, l\right) + \varepsilon$$

Alatas et al (2016): Application decision

- Application probabilities:

$$A_s(y^o, y, l) = \mathbb{P}(g(y^o, y, l) > \varepsilon) = 1 - F(-g(y^o, y, l))$$
$$A_u(y, l) = \mathbb{P}(h(y, l) > \varepsilon) = 1 - F(-h(y, l))$$

- Unsophisticated households have consistent beliefs:

$$\lambda(y) = \lambda_{ind}(y) = \frac{\begin{pmatrix} \alpha \int\int \mu(y^o) A_s(y^o, y, l) \vartheta(y^o, l|y) \, dldy \\ +(1-\alpha)\int\int \mu(y^o) A_u(y, l) \vartheta(y^o, l|y) \, dldy \end{pmatrix}}{\begin{pmatrix} \alpha \int\int A_s(y^o, y, l) \vartheta(y^o, l|y) \, dldy \\ +(1-\alpha)\int\int A_u(y, l) \vartheta(y^o, l|y) \, dldy \end{pmatrix}}$$

where α is proportion of sophisticated hhs

Alatas et al (2016): Application cost

- Simple benchmark:
  - all hhs unsophisticated.
  - Time cost of applying is τl. Wages are $w = \phi y$ so monetary cost is $\tau l \phi y$.
  - No shocks $\varepsilon = 0$

$$\text{apply iff } h(y) = -\tau l \phi y + \delta \lambda (y) b \geq 0$$

- lhs is decreasing in $y \rightarrow$ threshold y^* below which people apply (Nichols & Zeckhauser 1982).
- Now add back in shocks:

$$\text{apply iff } \tau l \phi y - \delta \lambda (y) b \leq \varepsilon$$

- Consider y_1 and $y_2 > y_1$: They show up at relative rates

$$\frac{1 - F(\tau l \phi y_1 - \delta \lambda (y_1) b)}{1 - F(\tau l \phi y_2 - \delta \lambda (y_2) b)}$$

  the higher this is, the better targeted the transfer

Alatas et al (2016): Application cost

- Now consider increasing the ordeal l: Differentiating, targeting improves iff

$$\frac{f\left(\tau l\phi y_2 - \delta\lambda\left(y_2\right)b\right)}{1-F\left(\tau l\phi y_2 - \delta\lambda\left(y_2\right)b\right)}\tau\phi y_2 - \frac{f\left(\tau l\phi y_1 - \delta\lambda\left(y_1\right)b\right)}{1-F\left(\tau l\phi y_1 - \delta\lambda\left(y_1\right)b\right)}\tau\phi y_1 > 0$$

- More generally, a sufficient condition for targeting efficiency to be improving as l increases is that the hazard rate is increasing in y

$$\frac{\partial}{\partial y}\frac{f\left(\tau l\phi y - \delta\lambda\left(y\right)b\right)}{1-F\left(\tau l\phi y - \delta\lambda\left(y\right)b\right)} > 0$$

- This rules out thick tailed income distributions.
- More generally, shows that single crossing $(c_{ly} > 0)$ isn't sufficient to make ordeal optimal

Alatas et al (2016): Nonlinear application cost

- Application costs may affect poor and rich households differently.
- To capture this, put a kink in the application cost: There are two ways to get to the office, by bus or walking.
- Walking: calorie cost γl; time cost $\tau l w$: slow but cheap
- Bus: fixed cost ν; time cost λw: fast but costly: $\lambda < \tau$

$$D = \begin{cases} \text{bus} & \text{if } v + \lambda l \phi y < \gamma l + \tau l \phi y \\ \text{walk} & \text{if } \nu + \lambda l \phi y \geq \gamma l + \tau l \phi y \end{cases}$$

- Apply iff

$$-\min\left\{\gamma l + \tau l \phi y, \nu + \lambda l \phi y\right\} + \delta \lambda \left(y\right) b \geq \varepsilon$$

- lhs decreasing in y so richer hhs apply less.
- Effect of change in l also stronger for people who walk, who are on average poorer.

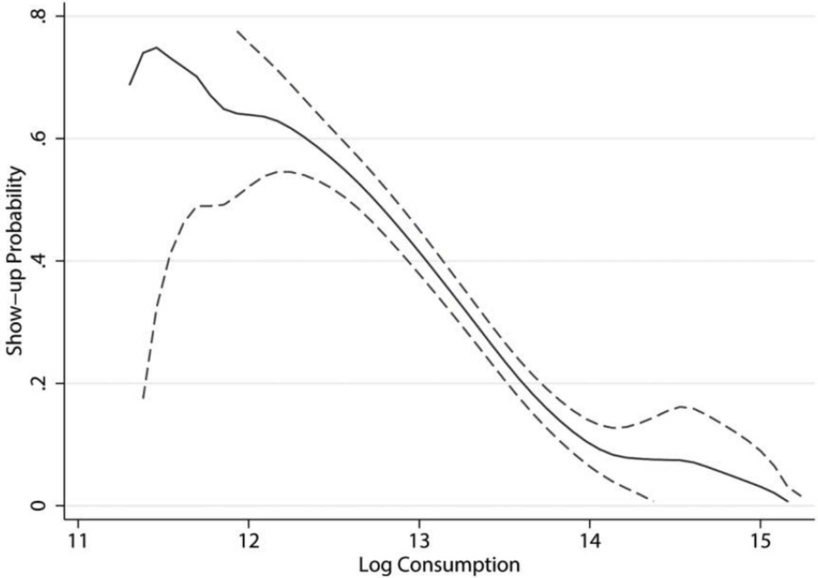
Alatas et al (2016): Sophistication

- All this so far only for unsophisticated households. Perceive pr of transfer as $\lambda\left(y\right)$
- Sophisticated households understand the probability is $\mu\left(y^{o}\right)$
- How does unobserved income y^{u} affect behavior?
  - For sophisticated hhs, only effect is through $c\left(l, y^{o}+y^{u}\right)$
  - For unsophisticated, both through $\lambda\left(y^{o}+y^{u}\right)$ and $c\left(l, y^{o}+y^{u}\right)$

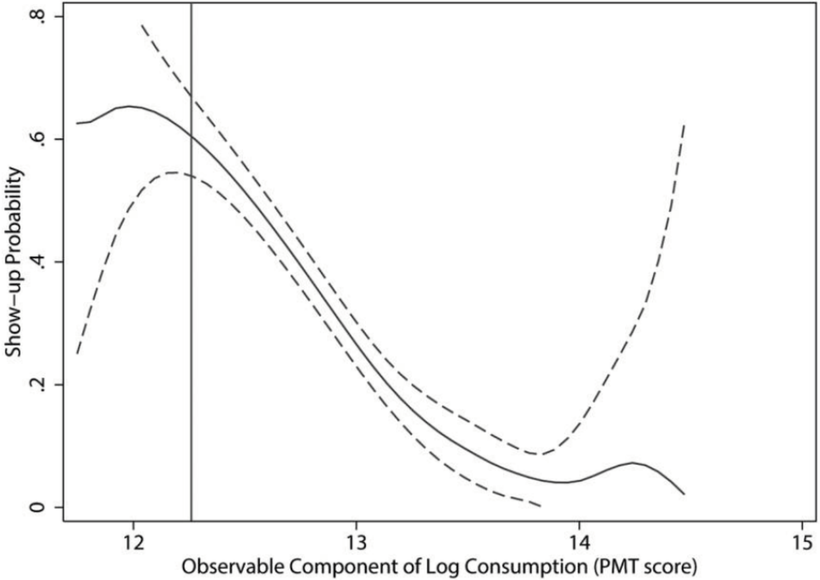
  $\Rightarrow$ expect more selection on unobservables for unsophisticated HHs
- Selection on unobservables is good though when income is poorly observed by PMT:
  - Sophisticated rich households who know they can pass PMT apply
  - unsophisticated rich households unsure they can pass PMT so don't apply

  $\Rightarrow$ benefit of unsophisticated households? Secret PMT formula?

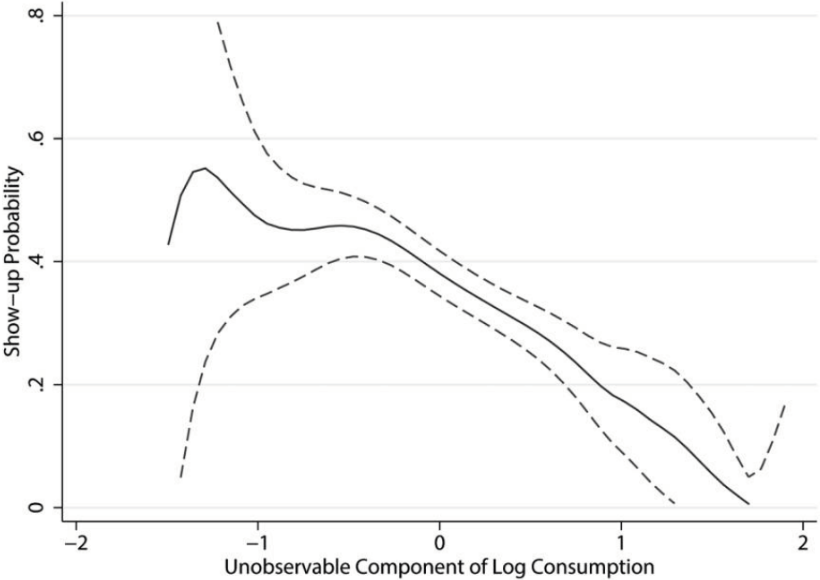
Alatas et al (2016): Self-Selection



Alatas et al (2016): Selection on Observables



Alatas et al (2016): Selection on Unobservables



Alatas et al (2016): Self-Selection

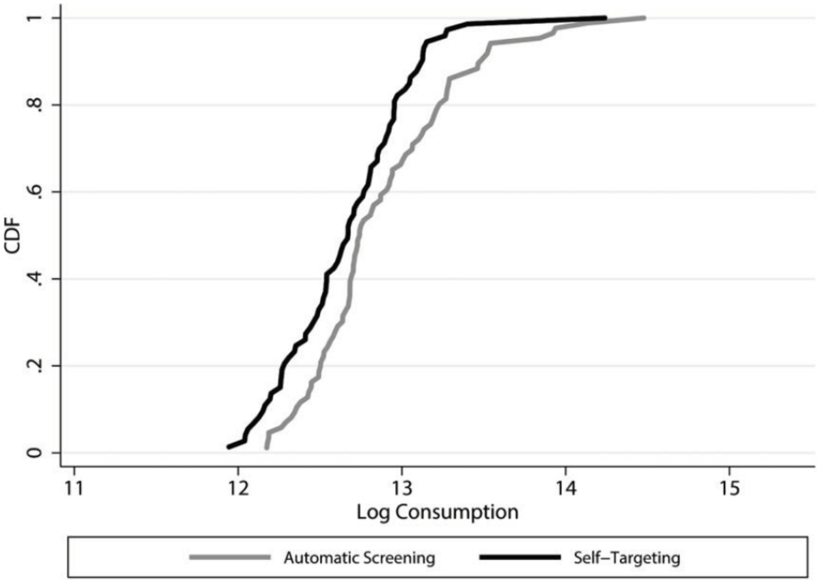
- Logit: $\mathbb{P}\left(\text{show-up}_i = 1\right) = \exp\left(\alpha + \gamma y_i^o + \psi y_i^u\right)/1 + \exp\left(\alpha + \gamma y_i^o + \psi y_i^u\right)$

TABLE 4

PROBABILITY OF SHOWING UP AS A FUNCTION OF THE OBSERVED AND UNOBSERVED COMPONENTS OF BASELINE LOG PER CAPITA CONSUMPTION

	SHOWED UP		
	All (1)	Very Poor (2)	Not Very Poor (3)
Observable consumption (y_i^o)	−2.217***	−.325	−2.310***
	(.201)	(1.785)	(.208)
Unobservable consumption (y_i^u)	−.907***	−.775	−.908***
	(.136)	(.581)	(.138)
Stratum fixed effects	No	No	No
Observations	2,000	114	1,886
Mean of dependent variable	.377	.658	.360

Alatas et al (2016): Self-Targeting vs Automatic



Alatas et al (2016): Self-Targeting vs Automatic

	Log Consumption (Beneficiaries; Baseline; OLS) (1)	Log Consumption (Beneficiaries; Baseline + Midline; OLS) (2)	Receives Benefits (Logit) (3)	Error (Logit) (4)	Exclusion Error (Logit) (5)	Inclusion Error (Logit) (6)
	A. No Stratum Fixed Effects					
Self-targeting	−.208***	−.193***	12.142**	−.190	−.506	−.311
	(.076)	(.060)	(4.894)	(.126)	(.402)	(.210)
Log consumption			−1.016***			
			(.280)			
Log consumption × self-targeting			−.964**			
			(.383)			
Observations	159	904	3,996	3,998	249	3,749
Mean of dependent variable	12.78	13.61	.0398	.0870	.880	.0344
	B. With Stratum Fixed Effects					
Self-targeting	−.114	−.175***	15.180***	−.209	−.649	−.331*
	(.077)	(.058)	(5.295)	(.140)	(.441)	(.192)
Log consumption			−1.042***			
			(.283)			
Log consumption × self-targeting			−1.202***			
			(.416)			
Observations	159	904	3,489	3,938	113	3,130
Mean of dependent variable	12.78	13.61	.0456	.0884	.761	.0412

Alatas et al (2016): Changing the Ordeal

- Compare far (average 1.83 km) to close (0.27 km) subtreatments

$$\mathbb{P}\left(\text{show-up}_i = 1\right) = \frac{\exp(\alpha + \beta \text{Close}_v + \gamma y_{vi} + \eta \text{Close}_v \times y_{vi})}{1 + \exp(\alpha + \beta \text{Close}_v + \gamma y_{vi} + \eta \text{Close}_v \times y_{vi})}$$

	No Stratum Fixed Effects			With Stratum Fixed Effects		
	(1)	(2)	(3)	(4)	(5)	(6)
Close subtreatment	.205	1.345	.185	.275	.485	.179
	(.146)	(2.841)	(.237)	(.168)	(2.920)	(.314)
Log consumption		−1.434***			−1.446***	
		(.143)			(.144)	
Close subtreatment × log consumption		−.093			−.023	
		(.217)			(.218)	

Alatas et al (2016): Model and Mechanisms

- Use GMM to estimate model parameters.
- Assumptions:
  - $\varepsilon \sim logistic$ with mean v_ε and sd σ_ε
  - Unsophisticated beliefs: probit $\lambda(y) = \Phi(\gamma + \pi y)$
  - Transfer's NPV. Use government credit scheme's interest rate 22%

$\Rightarrow$ 5 parameters to fit: $v_\varepsilon, \sigma_\varepsilon, \alpha, \lambda(y), \gamma, \pi$

- Specify $c(y_i, l_i) = \text{wage}_i \times (\text{traveltime}_i + \text{waitingtime}) + \text{travelmoney}_i$

Alatas et al (2016): Model and Mechanisms

- Moments:
- Show-up rates in 5 quintiles of consumption and far/close treatments $\Rightarrow$ 10 moments
- Show-up rates in {top/bottom tercile y^o} $\times$ {top/bottom tercile y^u} $\Rightarrow$ 4 moments
- Show-up rates in top/bottom quartiles of distance $\Rightarrow$ 2 moments

$$\begin{aligned}
\mathrm{E}\left[\Phi\left(\gamma+\pi y_i\right)-\text{benefit}_i \middle| \text{show-up}_i=1\right]&=0\\
\mathrm{E}\left[\left[\Phi\left(\gamma+\pi y_i\right)-\text{benefit}_i\right]\left(y_i-\overline{y}\right) \middle| \text{show-up}_i=1\right]&=0\\
\mathrm{E}\left[\lambda_{ind}\left(y_i\right)-\text{benefit}_i \middle| \text{show-up}_i=1\right]&=0\\
\mathrm{E}\left[\left[\lambda_{ind}\left(y_i\right)-\text{benefit}_i\right]\left(y_i-\overline{y}\right) \middle| \text{show-up}_i=1\right]&=0
\end{aligned}$$

Alatas et al (2016): Model Parameters

ESTIMATED PARAMETER VALUES FOR THE MODEL

v_ε	σ_ε	α	γ	π
−79,681	59,715	.50	8.04	−.72
(6,798)	(11,734)	(.07)	(.63)	(.05)

NOTE.—This table reports the estimated mean v_ε and standard deviation σ_ε of the utility shock (ε), the fraction of sophisticated households (α), and the constant γ and log consumption coefficient π in the λ function. The parameters are estimated using two-step feasible GMM. For each step, we choose 100 random initial conditions and minimize the objective function using a trust-region-reflective algorithm. Bootstrapped standard errors, calculated using 100 bootstrap iterations, are in parentheses.

Alatas et al (2016): Mechanisms

	Show-Up Rate (Experimental) (1)	Predicted Show-Up Probability (Model): Baseline Model (2)	$\sigma_\varepsilon = \hat{\sigma}_\varepsilon/2$ (3)	$\sigma_\varepsilon = 0$ (4)	Assuming Same Travel Technology (5)	Constant $\mu(\cdot)$ and $\lambda(\cdot)$ (6)
A. Logistic Regressions						
Close	1.509	−1.365	−1.825	−1.791	−1.367	−1.742
	(2.972)	(3.098)	(3.472)	(3.765)	(2.967)	(2.18)
Log consumption	−1.423***	−1.630***	−2.181***	−2.456***	−1.631***	−.103
	(.148)	(.163)	(.193)	(.204)	(.166)	(.118)
Close × log consumption	−.105	.105	.141	.138	.106	.136
	(.227)	(.238)	(.268)	(.29)	(.228)	(.166)
Observations	1,971	5,913,000	5,913,000	5,913,000	5,913,000	5,913,000
p-value		.522	.483	.509	.513	.391
B. Show-Up Rates						
Above poverty line, far	34.09	34.55	30.04	28.12	34.54	45.89
Above poverty line, close	38.99	37.37	33.11	31.17	37.37	47.15
Below poverty line, far	53.23	71.94	72.94	73.83	71.92	46.53
Below poverty line, close	59.32	65.52	65.81	66.25	65.52	43.84
C. Show-Up Rate Ratios						
Poor to rich ratio, far	1.561	2.082	2.428	2.626	2.082	1.014
	(.213)	(.203)	(.244)	(.262)	(.199)	(.14)
Poor to rich ratio, close	1.522	1.753	1.987	2.126	1.753	.93
	(.169)	(.183)	(.214)	(.221)	(.19)	(.141)
Difference of ratios	.040	.329	.441	.5	.329	.084
	(.268)	(.271)	(.322)	(.34)	(.281)	(.197)
p-value		.448	.338	.288	.456	.893

Alatas et al (2016): Alternative Policies

	Show-Up Rate (Experimental)			Predicted Show-Up Probabilities (Model)					
	Automatic Screening (Scaled) (1)	Automatic Screening (in Sample) (2)	Self-Targeting (3)	Baseline Model (4)	Far Distance + 3 km (5)	Far Distance + 6 km (6)	Far Wait Time × 3 (7)	Far Wait Time × 6 (8)	Perfect Targeting (9)
	A. Program Statistics								
Mean show-up rate (%)	34.62	34.62	37.84	37.93	37.67	37.53	36.79	35.39	5.83
Mean benefit receipt (%)	4.38	4.38	3.64	4.11	4.10	4.09	4.06	4.00	5.83
Mean eligible benefit receipt (%)	.63	.63	.73	.86	.86	.86	.86	.86	5.83
Mean ineligible benefit receipt (%)	3.75	3.75	2.91	3.24	3.23	3.23	3.20	3.14	.00
	B. Average Household Costs for Households That Show Up (Rupees)								
Average cost to households	1,021	1,021	13,674	13,831	15,947	17,218	24,187	37,460	7,621
Average cost to beneficiary households	938	938	12,464	12,797	14,774	16,130	21,987	34,968	7,621
Average cost to nonbeneficiary households	1,033	1,033	13,803	13,957	16,091	17,351	24,459	37,777	. . .
	C. Government Costs and Benefits Paid (Rupees)								
Administrative costs, per household	4,768	31,054	6,764	6,781	6,734	6,710	6,576	6,326	1,042
Expected benefits, per household	332,028	332,028	306,108	353,230	352,305	351,742	349,750	344,826	472,990
	D. Poverty Gap								
Poverty gap under fixed budget (%)	2.736	2.741	2.720	2.724	2.724	2.724	2.724	2.725	2.610
Reduction in poverty gap relative to perfect targeting (%)	29.91	27.42	38.58	36.38	36.71	36.37	36.47	36.26	100.00

Outline

Banerjee et al (2018): Overview

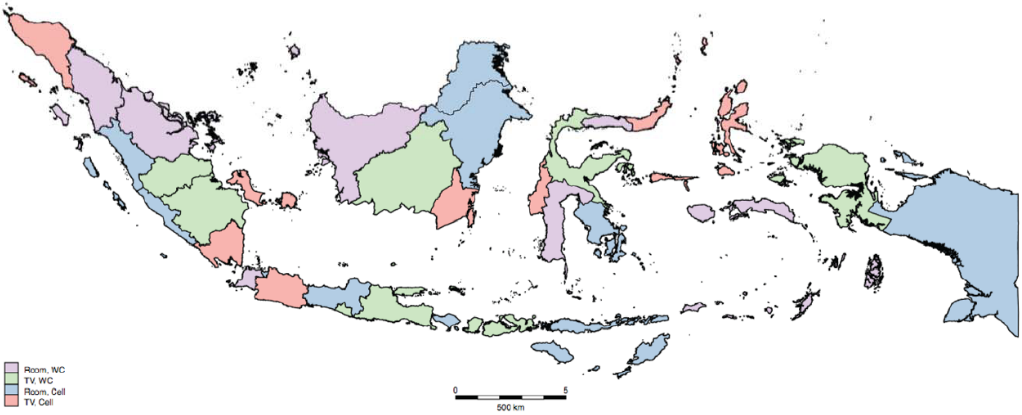
- Proxy-means tests are a very common way to target transfers.
- We might expect that these tests induce behavioral responses, distorting consumption baskets.
- This paper:
  - add questions to Indonesia's poverty census.
  - Survey households 6 months later to ask about consumption $\Rightarrow$ reported response
  - Look at administrative data on sales to assess $\Rightarrow$ actual response

Banerjee et al (2018): Setting

- Indonesia conducts nationwide censuses of the poor approximately every 3 years.
- Proxy-means testing determines households' eligibility for transfer programs from cash transfers to health insurance.
- Most recent census: June-August 2015, covered 25 mn households, 92 mn individuals.
- Experiment: Add 2 questions to the survey:
    1. either: Q on flat-screen TV ownership or # rooms in the house
    2. either: Q on # active SIM cards or whether have "swan neck" toilet
- Households probably believe that these additional questions will be used to determine benefit eligibility
- The questions were *not* actually used in the PMT formula.
- Randomized across the 34 provinces (stratifying by 5 regions).

Banerjee et al (2018): Randomization

Figure 1: Map of Randomization



Notes: This map shows the treatment assignment of each of Indonesia's 34 provinces.

Banerjee et al (2018): Data

- Use 3 main datasets

1. household survey data (SUSENAS) for March 2016 and March 2017 (300k households each). Worked with Statistics agency to add relevant questions.

2. Monthly TV sales of flat-screen TVs from January 2013–December 2016. Data aggregated to randomization arm$\times$month.

3. Yearly active SIM cards by province for 2015–2017 from Ministry of Information and Communications.

Banerjee et al (2018): Effect on Self-Reported Assets

- Estimate

$$Asset_{hdp} = \beta_0 + \beta_1 TVTreat_p + \beta_2 CellTreat_p + \mathbf{X}'_{dp}\boldsymbol{\gamma} + \alpha_r + \varepsilon_{hdp}$$

- $Asset_{hdp}$ is self-reported asset for HH H in district d in province p.
- Randomization ensures balance in expectation. To improve precision
  1. Include strata fixed effects α_r
  2. Code 1,388 asset variables from 2007-2015 SUSENAS surveys and average by district $\times$ urbanization. Use double-LASSO to select controls (Belloni, Chernozhukov & Hansen, 2014)

Banerjee et al (2018): Effect on Self-Reported Assets

Table 1: Treatment Effect on Self-Reported Asset Acquisition

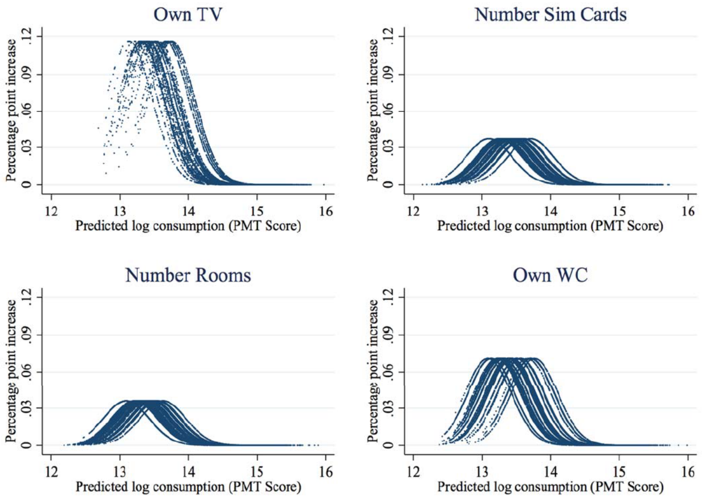
	(1) Own TV	(2) Nb. Sim Cards	(3) Nb. Rooms	(4) Own WC
	Panel A: 2016 Outcome Data			
TV Treatment	-0.0171	-0.00319		
	(0.00447)	(0.0318)		
Cell Treatment	-0.00265	0.0125		
	(0.00463)	(0.0277)		
Room Treatment			-0.140	-0.000209
			(0.179)	(0.00468)
WC Treatment			0.128	0.00587
			(0.160)	(0.00468)
Observations	291,414	291,414	291,414	291,414
Controls	Lasso	Lasso	Lasso	Lasso
Strata FE	YES	YES	YES	YES
Dep. Variable Mean	0.110	2.183	6.150	0.672
FWER adjusted p-value	0.005	0.735	0.735	0.584

Banerjee et al (2018): Effect on Self-Reported Assets

	Own TV	Nb. People with Phones	Nb. Rooms	Own WC
	Panel B: 2017 Outcome Data			
TV Treatment	-0.00463	-0.0169		
	(0.00524)	(0.0330)		
Cell Treatment	0.00505	-0.0217		
	(0.00473)	(0.0319)		
Room Treatment			-0.196	-0.00118
			(0.167)	(0.00602)
WC Treatment			0.0466	0.00433
			(0.152)	(0.00600)
Observations	297,276	297,276	297,276	297,276
Controls	Lasso	Lasso	Lasso	Lasso
Strata FE	YES	YES	YES	YES
Dep. Variable Mean	0.116	1.957	6.229	0.696
FWER adjusted p-value	0.823	0.823	0.722	0.823

Banerjee et al (2018): Effect on Self-Reported Assets

- Why only lie for TV? Redo PMT adding in these 4 attributes.



Banerjee et al (2018): Effect on Asset Acquisition

- To estimate impact on TV sales estimate

$$LogSales_{mg} = \beta_0 + \beta_1 TVTreat \times Post_{mg} + \beta_2 CellTreat \times Post_{mg} + \beta_3 Post_{mg} + \alpha_g \times m + \varepsilon_{gm}$$

  where $LogSales_{mg}$ is sales in month m in randomization group g (TV-cell, TV-toilet, room-cell, room-toilet)

- Need to think about the time-series structure of the data

1. Use Newey-West (1987) standard errors with 3 lags
2. panel-corrected model with AR(1) errors.

Banerjee et al (2018): Effect on Asset Acquisition

Table 2: Treatment Effect on Actual Television Sales

	(1) Log Sales	(2) Log Sales	(3) Log Sales
TV Treatment x Post	0.0540	0.0540	0.0517
	(0.0563)	(0.0806)	(0.0475)
Cell Treatment x Post	0.190	0.190	0.0771
	(0.0563)	(0.0806)	(0.0505)
Observations	192	192	192
Model/Standard Errors	Robust	Newey	Panel-Corrected AR(1)
Dep. Variable Mean	10.77	10.77	10.77
Treat coeff = -0.97 F-Statistic	331	161.3	463.3
Treat coeff = -0.97 P-Value	0	0	0

Banerjee et al (2018): Effect on Asset Acquisition

- To estimate impact on SIM card ownership estimate

$$LogSubscribers_p = \beta_0 + \beta_1 CellTreat_p + \beta_2 TVTreat_p + \varepsilon_p$$

in province-level data separately in each year.

Banerjee et al (2018): Effect on Asset Acquisition

Table 3: Treatment Effect on SIM Card Ownership

	(1) Log Subscribers	(2) Log Subscribers	(3) Log Subscribers
	Panel A: 2015 Data		
Cell Treatment	-0.225	-0.102	-0.106
	(0.406)	(0.146)	(0.153)
TV Treatment	-0.258	-0.192	-0.189
	(0.418)	(0.156)	(0.148)
Observations	34	34	34
Log population control	N	Y	Y
Strata FE	N	N	Y
Dep. Variable Mean	14.95	14.95	14.95
	Panel B: 2016 Data		
Cell Treatment	-0.251	-0.129	-0.135
	(0.401)	(0.148)	(0.159)
TV Treatment	-0.249	-0.184	-0.175
	(0.414)	(0.159)	(0.152)
Observations	34	34	34
Log population control	N	Y	Y
Strata FE	N	N	Y
Dep. Variable Mean	15.17	15.17	15.17

Banerjee et al (2018): Effect on Asset Acquisition

	Panel C: 2017 Data		
Cell Treatment	-0.173	-0.0478	-0.0529
	(0.403)	(0.129)	(0.109)
TV Treatment	-0.0445	0.0227	0.0121
	(0.408)	(0.127)	(0.103)
Observations	34	34	34
Log population control	N	Y	Y
Strata FE	N	N	Y
Dep. Variable Mean	15.55	15.55	15.55

Notes: This table provides estimates of the treatment effects of the different targeting questions in the PBDT on actual active SIM card subscribers. We have yearly, province level data from 2015 to 2017. All regressions are estimated using OLS, with robust standard errors.

Outline

Targeting in Developing Countries: Who gets the Benefit?

- Alatas, Banerjee, Hanna, Olken & Tobias (AER 2012) *Targeting the Poor: Evidence from a Field Experiment in Indonesia*
- Alatas, Banerjee, Hanna, Olken, Purnamasari & Wai-Poi (JPE 2016) *Self-Targeting: Evidence from a Field Experiment in Indonesia*
- Banerjee Hanna Olken & Sumarto (WP 2018) *The (lack of) Distortionary Effects of Proxy-Means Tests: Results from a Nationwide Experiment in Indonesia*
- Haushofer, Niehaus, Paramo, Miguel & Walker (2022) *Targeting Impact versus Deprivation*

Haushofer Et Al (2022): Motivation

- Targeting is important in anti-poverty programs. Big literature now on how to identify "deprived" (low wealth/consumption/living standards etc) households (Alatas et al 2012; Blumenstock et al 2015; Brown et al 2018; hanna & Olken 2018,...)
- But there's more to it than just looking for currently deprived households. In general we also want to consider how much each household would benefit from assistance.
- Accumulating evidence of heterogeneous treatment effects, even of cash assistance (de Mel et al 2008; Hussam et al 2022; Haushofer & Shapiro 2016)
- This paper combines these insights. Build on ML methods to identify heterogeneity of treatment effects (Wager & Athey 2018; Chernozukhov et al 2018; Athey et al 2019) and conceptual framework to think about targeting on deprivation or on impact.

Haushofer Et Al (2022): Model

- Need to allocate a treatment to a subset of households h to maximize social welfare:

$$\sum_h \sum_{t=0}^{\bar{t}} W\left(Y_{h,t}\left(T_h\right)\right)$$

where $Y_{h,t}$ is outcome (consumption, wealth etc) in period t and $T_h \in \{0,1\}$ is treatment assignment

- Welfare function W satisfies $W' > 0$ and $W'' \leq 0$
- Using potential outcomes notation $Y^1_{h,t}$ and $Y^0_{h,t}$ rewrite as

$$\sum_h \sum_{t=0}^{\bar{t}} W\left(Y^0_{h,t} + T_h \Delta_{h,t}\right)$$

where $Y^0_{h,t}$ is the untreated PO and $\Delta_{h,t} \equiv Y^1_{h,t} - Y^0_{h,t}$

Haushofer Et Al (2022): Model

- Government can't observe each household's $Y_{h,t}^{0}$ or $\Delta_{h,t}$.
- Instead, observe
  - baseline covariates $X_h \in \mathbf{X}$
  - Realized outcomes from an experimental sub-sample: $Y_{h,t}^{T_h}$
- Select a rule $r : \mathbf{X} \to \{0,1\}$ to assign treatment
- With the experimental data, the planner can form predictions

$$\hat{Y}_t^0 (X_h) \text{ of } \mathbb{E}\left[Y_{h,t}^0 | X_h, t\right]$$
$$\hat{\Delta}_t (X_h) \text{ of } \mathbb{E}\left[Y_{h,t}^1 - Y_{h,t}^0 | X_h, t\right]$$

Haushofer Et Al (2022): Model

- With the predictions, the planner can target.
- e.g. target deprivation:

$$r^D(X_h) = \mathbf{1}\left(\hat{Y}_0^0(X_h) \leq q_\phi^{\hat{Y}}\right)$$

- or; e.g. target impact:

$$r^I(X_h) = \mathbf{1}\left(\hat{\Delta}(X_h) \geq q_{1-\phi}^{\hat{\Delta}}\right)$$

- or target (social) welfare gain:

$$d\hat{W}(X_h) \equiv \sum_{t=0}^{\bar{t}} \left[W\left(\hat{Y}_t^0(X_h) + \hat{\Delta}_t(X_h)\right) - W\left(\hat{Y}_t^0(X_h)\right)\right]$$

$$r^*(X_h) = \mathbf{1}\left(d\hat{W}_h \geq q_{1-\phi}^{d\hat{W}}\right)$$

Haushofer Et Al (2022): Intervention

- Use the intervention studied in Egger et al. (2022). Give Directly (GD) cash transfers in western Kenya.
- 653 villages in 84 sublocations. Transfers between 2014 and 2017.
- GD defines households with thatched roofs as eligible $\rightarrow$ 35-40% of hhs eligible. Eligible hhs receive \$1,000: 75% of mean annual expenditures
- 2-level randomization:
  1. randomly assign 33 sub-locations to high saturation, 35 to low-saturation
  2. in High saturation sub-locations, 2/3 villages treated. In low, 1/3.

1. Baseline and endline surveys $\rightarrow$ 4,749 eligible households with both baseline & endline data on PMT inputs (hh size, children's ages, asset ownership, employment status etc)

Haushofer Et Al (2022): Empirical Approach

- Split data into 4 parts: Hi/Lo deprivation; and Hi/Lo impact:

Algorithm 1: Select most-deprived and most-impacted groups

Split data into set $\mathcal{K}$ of folds;
foreach $K \in \mathcal{K}$ **do**
  Training data $K' \leftarrow \mathcal{K} \setminus K$ *other* folds ;
  $\{\hat{y}^{0,K} : \{\mathbf{X}, T\} \rightarrow \mathbb{R}\} \leftarrow$ predictor of $y^0_{h,t}$ learned from training data K';
  $\hat{y}^{0,K}_h \leftarrow \frac{1}{\bar{t}} \sum_{t=0}^{\bar{t}} \hat{y}^{0,K}(X_h, t)$, i.e. integrate over time;
  Classify observations in bottom 50% of $\{\hat{y}^{0,K}_h\}$, $h \in K$, as most deprived (D);
  $\{\hat{\Delta}^K : \{\mathbf{X}, T\} \rightarrow \mathbb{R}\} \leftarrow$ predictor of $\Delta_{h,t}$ learned from training data K';
  $\{\hat{\Delta}^K_h \leftarrow \frac{1}{\bar{t}} \sum_{t=0}^{\bar{t}} \hat{\Delta}^K(X_h, t)$, i.e. integrate over time;
  Classify observations in top 50% of $\{\hat{\Delta}^K_h\}$, $h \in K$, as most impacted (I);
end

- Use $\mathcal{K} = 5$ and repeat 150 times and report averages

Haushofer Et Al (2022): Empirical Approach

- Learn deprivation $\mathbb{E}\left[Y_{h,t}^{0}|X_h,t\right]$ using random forests
- Learn Conditional Average Treatment Effect (CATE) function $\mathbb{E}\left[Y_{h,t}^{1}-Y_{h,t}^{0}|X_h,t\right]$ through causal forests using generalized random forests (GRF) by Athey et al (2019)
- With households classified into groups $S=D,I$ form the **predicted** averages:

$$\bar{\hat{y}}^{0}\left(S\right)=\frac{1}{|S|}\sum_{h\in S}\hat{y}^{0}\left(X_h\right)\qquad\bar{\hat{\Delta}}\left(S\right)=\frac{1}{|S|}\sum_{h\in S}\hat{\Delta}\left(X_h\right)$$

- This creates a problem: We classified households using predictions, so we will tend to suffer from a *winner's curse:* Large outliers drive classification. e.g. large values of $Y_h^0-\hat{Y}_h^0$ will tend to be classified as deprived, but then we'll overestimate deprivation of deprived group.

Haushofer Et Al (2022): Empirical Approach

- Use **actual** averages to estimate outcomes (and validate classification):

$$\bar{y}^0(S) = \frac{1}{|S|}\sum_{h\in S} y_h^0 \qquad \bar{\Delta}(S) = \frac{2}{|S|}\sum_{h\in S}\left(Y_h^1 T_h - Y_h^0(1-T_h)\right)$$

- For inference do three things:
  1. Bootstrap confidence intervals
  2. Follow Chernozukhov et al (2018): CIs implied by median standard errors of OLS averages across replications
  3. Randomization inference CI under sharp null of no heterogeneity

Haushofer Et Al (2022): Results

Table 1: Predicted per capita untreated outcomes (y_h^0) by group

Statistic	(1) All	(2) Most deprived (D)	(3) Most impacted (I)	(4) Difference (D)-(I)
Panel A: Consumption				
Predicted	750	542	923	-381
Actual	729	503	911	-408
				(-567,-338)
				[-472,-344]
Panel B: Assets				
Predicted	232	85	343	-258
Actual	213	53	336	-283
				(-419,-247)
				[-314,-253]
Panel C: Income				
Predicted	304	186	321	-135
Actual	297	170	323	-153
				(-235,-68)
				[-188,-119]

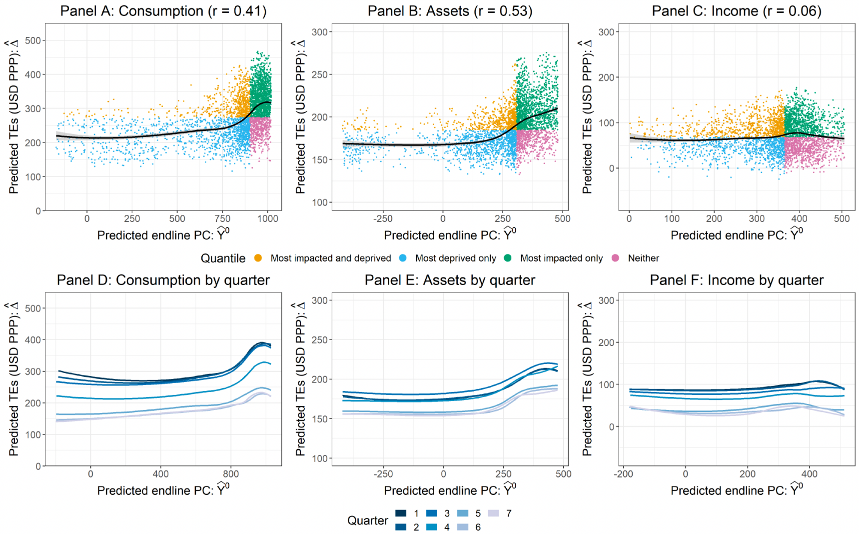
Haushofer Et Al (2022): Results

Table 2: Predicted Average Treatment Effects (Δ_h) by group

Statistic	(1) All	(2) Most deprived (D)	(3) Most impacted (I)	(4) Difference (D)-(I)	(5) RI p-value $I - I^C > 0$
Panel A: Consumption					
Predicted	277	247	326	-79	
Actual	310	247	405	-159	0.00
				(-268,31)	
				[-244,-73]	
Panel B: Assets					
Predicted	189	178	207	-29	
Actual	182	154	188	-34	0.53
				(-68,91)	
				[-82,14]	
Panel C: Income					
Predicted	69	66	94	-28	
Actual	85	79	94	-15	0.38
				(-37,187)	
				[-77,47]	

Haushofer Et Al (2022): Results

Figure 1: Predicted treatment effects (Δ_h) plotted against the predicted untreated per capita values (y_h^0)



Haushofer Et Al (2022): Optimal Policy

- $W(\hat{y}) = \left(1 - e^{-\alpha \hat{y}}\right)/\alpha$ for $\alpha > 0$ or $W(\hat{y}) = \hat{y}$ for $\alpha = 0$

Table 4: Overlap of socially optimal households to target with most deprived and most impacted

	(1)	(2)	(3)	(4)	(5)
CARA: α	CE	Most deprived	Most impacted	Choice	α_c
Panel A: Consumption					
0.0000	\$50	0.26	1.00	I	
0.0005	\$49	0.27	0.95	I	
0.0010	\$49	0.29	0.92	I	
0.0075	\$41	0.35	0.81	I	$\leftarrow$ 0.012
0.0150	\$33	0.36	0.79	D	

Haushofer Et Al (2022): In the Paper...

- The paper has a bunch of other goodies in it:
- Other ML methods
- Other outcomes
- Spillovers

Outline

Motivating Facts

Theory

Evidence from Rich Countries

Targeting in Developing Countries: Who gets the Benefit?

Transfer Design: What is the Benefit?

Open Questions

Outline

Transfer Design: What is the Benefit?

- Bryan, Chowdhury, Mobarak, Morten & Smits (2021) *Encouragement and Distortionary Effects of Conditional Cash Transfers*
- Balboni, Bandiera, Burgess, Ghatak & Heil (2022) *Why Do People Stay Poor?*
- Cunha, De Giorgi & Jayachandran (2017) *The Price Effects of Cash Versus In-Kind Transfers*

Bryan Et Al (2021): Motivation

- Conditional Cash Transfers (CCTs) started in the late 90s in LatAm. By 2014 64 non-OECD countries had a CCT
- Payment is conditional on completing some behavior. Is this "better" than an Unconditional Cash Transfer (UCT)?
- If the behavior has a social benefit larger than its cost and would not be undertaken in the absence of the condition, CCT can be better than UCT
- But CCT also requires monitoring compliance with the condition. $\Rightarrow$ a tradeoff.
- Economic theory also suggests CCT can be distortionary if households undertake behavior with higher social cost than benefit in order to satisfy conditions
- This paper presents theory and an experiment on migration to study these issues.

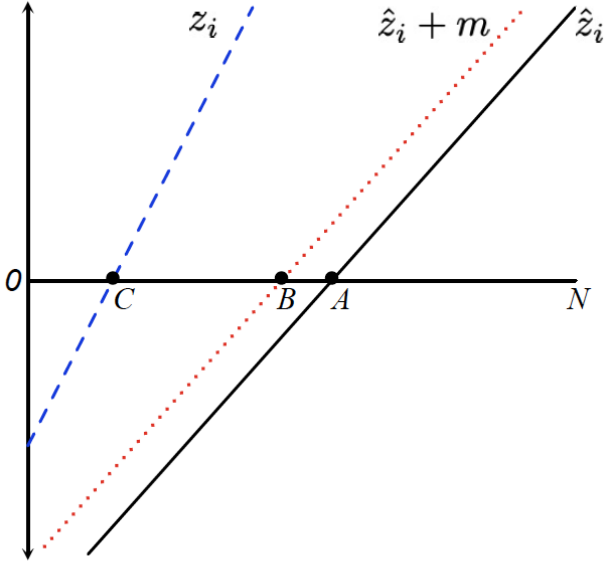
Bryan Et Al (2021): Theory

- Simple model to illustrate concepts. Framed as a migration decision but more widely applicable.
- Households in a community $i \in N$ are characterized by a pair $z_i, \hat{z}_i$
  - z_i is the *social* benefit of household i migrating, measured in \$
  - $\hat{z}_i$ is the household's *perceived* benefit of migrating
- **Under-investment:** $z_i \geq 0$ but $\hat{z}_i < 0$: Household "should" migrate, but does not
- **Over-investment:** $z_i < 0$ but $\hat{z}_i \geq 0$: Household "shouldn't" migrate, but it does
- Model can capture behavioral biases, (network) externalities etc.

Bryan Et Al (2021): Theory

- Assume away income effects and we can consider a CCT that pays households m iff they migrate.
- CCT has an **encouragement effect** if there are households for whom $\hat{z}_i < 0$ and $z_i \geq 0$ but $\hat{z}_i + m \geq 0$
- CCT has a **distortionary effect** if there are households for whom $\hat{z}_i < 0$ and $z_i < 0$ but $\hat{z}_i + m \geq 0$
- *Claim:* In a community of N households, order them such that $\hat{z}_1 < \hat{z}_2 < \ldots < \hat{z}_N$. If the household ordering also satisfies $z_1 < z_2 < \ldots < z_N$ then the social welfare gain from a CCT will be either i) monotonically increasing in m; ii) monotonically decreasing in m; or iii) inverted-U shape in m.

Bryan Et Al (2021): Theory



Bryan Et Al (2021): Experimental Design

- Labor demand/wages are low during pre-harvest season in many places
- Rural Eastern Indonesia, West Timor one such area. Some households send seasonal migrants to cities.
- Sampled 5 villages in Timor Tengga Utara (TTU) Regency in West Timor.
- 855 households interviewed, and 775 eligible: (i) ≥ 1 hh member aged 21+; (ii) land ≤ 200 Are (2 hectares).
- Households randomized into either UCT of IDR 150K ($\sim$\$10) with no conditions; or CCT. CCT had to migrate, received half of transfer in origin village and half in the destination. Randomized into 3 groups: (1) Low (IDR 150K); (2) High (IDR 300K); (3) Low with surprise (unexpected extra 225K in destination)
- Need (3) to try and disentangle selection effects they are interested in from direct effect of larger transfer.

Dependent variable:	Accepting cash transfer offer				Check-in at a destination (CCT subsample)				Migration season income (Rp. 10k)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
CCT high	-0.366***	-0.355***	-0.366***	-0.355***	0.114*	0.137**	0.107*	0.137**	12.233	5.176
	(0.055)	(0.065)	(0.055)	(0.066)	(0.047)	(0.042)	(0.047)	(0.047)	(9.904)	(11.064)
CCT low	-0.414***	-0.403***							45.282**	31.946*
	(0.050)	(0.061)							(11.793)	(14.347)
CCT low+	-0.430***	-0.449***			0.013	0.000			47.476**	40.926**
	(0.055)	(0.069)			(0.026)	(0.027)			(11.313)	(12.505)
CCT low/low+			-0.422***	-0.427***						
			(0.046)	(0.059)						
F-test, p-values:										
Low = low+	0.776	0.428								
High = low/low+			0.258	0.053						
High = low+									0.002	0.002
E(Y | UCT)	0.928	0.928	0.928	0.928					134.056	134.056
E(Y | CCT low)					0.191	0.191				
E(Y | CCT-low/low+)							0.207	0.207		
Controls		✓		✓		✓		✓		✓
Observations	775	708	775	708	526	474	526	474	708	708
R^2	0.210	0.249	0.210	0.248	0.110	0.164	0.110	0.164	0.060	0.114

Dep. var.:	Work (any)		Salaried work		Sector: Trade/retail		Sector: Manufacturing		Sector ranked by average earnings[a]		Food security index	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
CCT high	0.067**	0.071**	0.103*	0.080*	0.090**	0.086*	-0.017	0.011	-0.222	0.052	-0.095	-0.090
	(0.024)	(0.021)	(0.039)	(0.033)	(0.021)	(0.034)	(0.022)	(0.033)	(0.297)	(0.310)	(0.111)	(0.102)
CCT low	0.028	0.026	0.088**	0.057	0.059	0.053	0.068**	0.091**	0.018	0.328	-0.021	-0.041
	(0.031)	(0.035)	(0.031)	(0.030)	(0.064)	(0.050)	(0.023)	(0.025)	(0.333)	(0.306)	(0.125)	(0.144)
CCT low+	0.047*	0.049	0.068	0.045	0.014	0.011	0.077	0.088**	0.549**	0.632**	-0.045	0.009
	(0.021)	(0.025)	(0.046)	(0.042)	(0.046)	(0.036)	(0.042)	(0.031)	(0.152)	(0.191)	(0.078)	(0.080)
F-test (p-values):												
Low = low+	0.626	0.633	0.282	0.397	0.653	0.584	0.811	0.934	0.173	0.341	0.833	0.713
High = low/low+	0.427	0.342	0.520	0.534	0.056	0.119	0.177	0.192	0.028	0.019	0.942	0.515
High = low+	0.504	0.488	0.491	0.361	0.078	0.059	0.138	0.193	0.021	0.069	0.744	0.516
Controls		✓		✓		✓		✓		✓		✓
E[Y]	0.898	0.898	0.216	0.216	0.106	0.106	0.053	0.053	2.767	2.767	-0.000	-0.000
(st. dev.)	(0.302)	(0.302)	(0.412)	(0.412)	(0.308)	(0.308)	(0.224)	(0.224)	(1.810)	(1.810)	(1.001)	(1.001)
N	708	708	708	708	227	227	227	227	227	227	686	686
R^2	0.050	0.080	0.048	0.134	0.116	0.137	0.144	0.233	0.149	0.230	0.048	0.104

Outline

Transfer Design: What is the Benefit?

- Bryan, Chowdhury, Mobarak, Morten & Smits (2021) *Encouragement and Distortionary Effects of Conditional Cash Transfers*
- Balboni, Bandiera, Burgess, Ghatak & Heil (2022) *Why Do People Stay Poor?*
- Cunha, De Giorgi & Jayachandran (2017) *The Price Effects of Cash Versus In-Kind Transfers*

Balboni et al. (2022): Motivation

- Questions in development economics don't get much bigger than "Why do people stay poor?"
- Most poor individuals are employed but have low earnings. Why?

1. The poor have the same opportunities as everyone else, so it must be that they have traits that are unsuitable for other occupations.
2. The poor do not have the same opportunities. They take low-earning jobs because they are born poor. A **Poverty Trap**

- Distinguishing between these two hypotheses is *hard*. Both have the same predictions for equilibrium allocation of people across occupations.
- To tell them apart need to observe what happens when you cross the threshold level of affluence to escape poverty trap.
  - Under view 1 you return to where you started
  - Under view 2 you will escape poverty forever

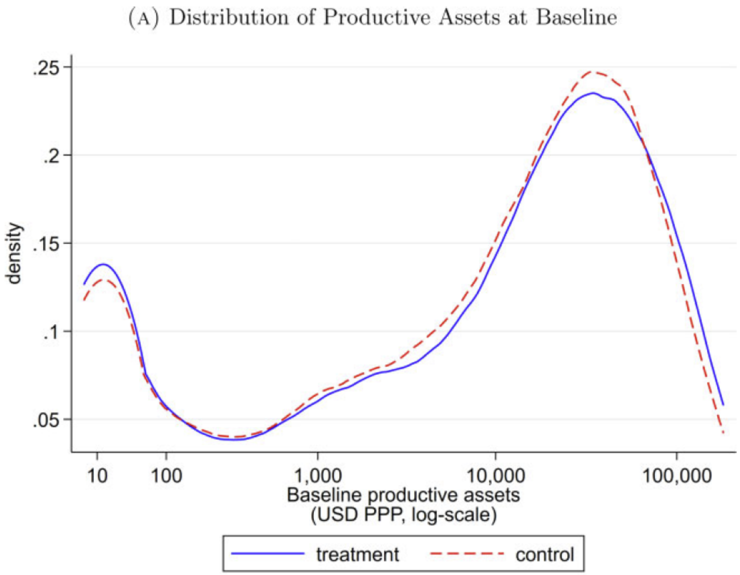
Balboni et al. (2022): Setting

- Test for poverty traps using BRAC's Targeting the Ultra-Poor (TUP) program in Bangladesh.
- Bandiera et al. (2017) study impacts using data on 23K hhs in 1,309 villages in 13 poorest districts. Of these, 6K hhs are considered "extremely poor".
- Program offers one-off transfer of productive assets (a cow in 91% of cases) and training to use it to women from ultra-poor hhs.
- Baseline survey in 2007, then follow-ups in 2009, 2011, 2014 and 2018 (11 years!)

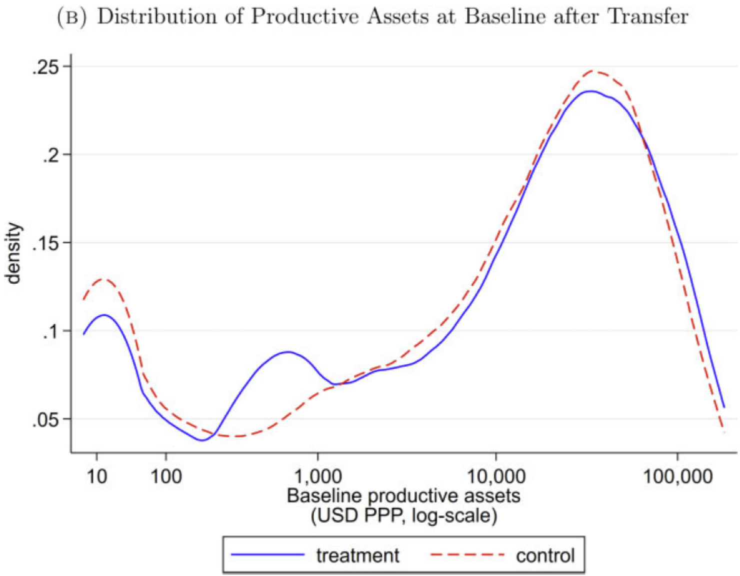
THE ECONOMIC LIVES OF WOMEN IN BANGLADESHI VILLAGES AT BASELINE

	Ultra-poor (1)	Near poor (2)	Middle class (3)	Upper class (4)
In labor force	0.74	0.67	0.69	0.71
	(0.44)	(0.47)	(0.46)	(0.46)
Total hours worked per year	990.91	767.62	555.83	496.83
	(893.68)	(811.72)	(596.80)	(493.42)
Total days worked per year	252.06	265.07	303.55	325.62
	(136.74)	(141.27)	(122.21)	(102.25)
Hourly income (BDT)	5.61	5.63	9.83	21.67
	(21.22)	(10.93)	(38.09)	(69.95)
Years of formal education	0.56	1.26	1.99	3.72
	(1.63)	(2.43)	(2.99)	(3.74)
Literate	0.07	0.17	0.27	0.51
	(0.26)	(0.37)	(0.44)	(0.50)
Body mass index (BMI)	18.38	18.96	19.49	20.60
	(2.40)	(2.56)	(2.82)	(3.40)
Household savings (1,000 BDT)	0.15	0.40	1.62	8.61
	(0.83)	(1.24)	(10.62)	(29.29)
Productive assets (1,000 BDT)	5.03	12.87	145.36	801.77
	(30.43)	(71.59)	(310.50)	(945.29)
Productive assets + loans (1,000 BDT)	5.64	14.77	150.22	812.83
	(30.92)	(72.47)	(312.51)	(947.65)
Observations	6,732	7,340	6,742	2,215

Balboni Et Al (2022): Baseline Wealth Distribution



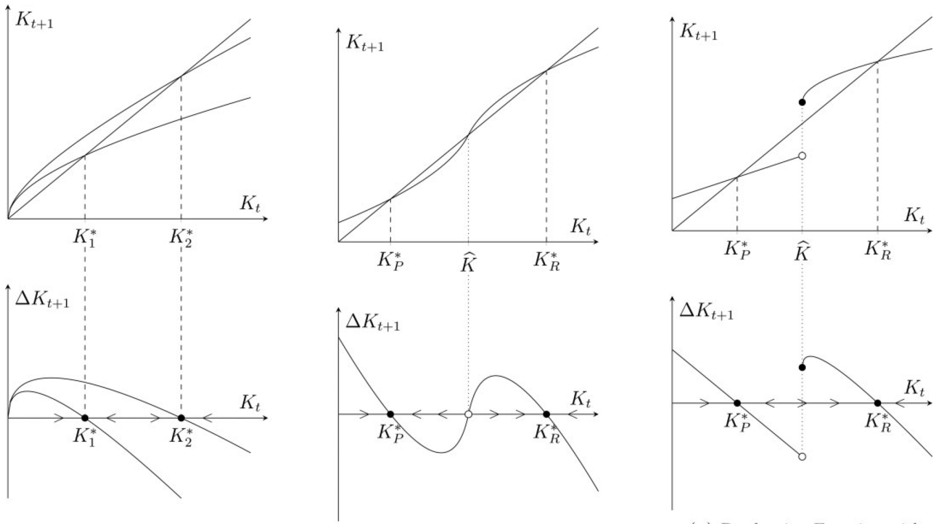
Balboni Et Al (2022): Baseline Wealth Distribution After Transfers



Balboni Et Al (2022): Model

- Simple framework to model the dynamics of capital accumulation. Define a transition equation
$$K_{i,t+1} = \Phi_i\left(K_{i,t}\right)$$
- e.g. $Y_i = A_{iv} f\left(K_i\right)$ where A_{iv} captures productivity traits of individual and village. Then
$$\Phi_i\left(K_{i,t}\right) = s_i A_{i,v} f\left(K_{i,t}\right) + (1-\delta) K_{i,t}$$
- Define steady state(s) as fixed point(s) of $\Phi_i(\cdot)$: $K_i^* = \Phi_i\left(K_i^*\right)$

Balboni Et Al (2022): Model



(A) Globally Concave Production Function

(B) S-shaped Production Function

(C) Production Function with Indivisibilities

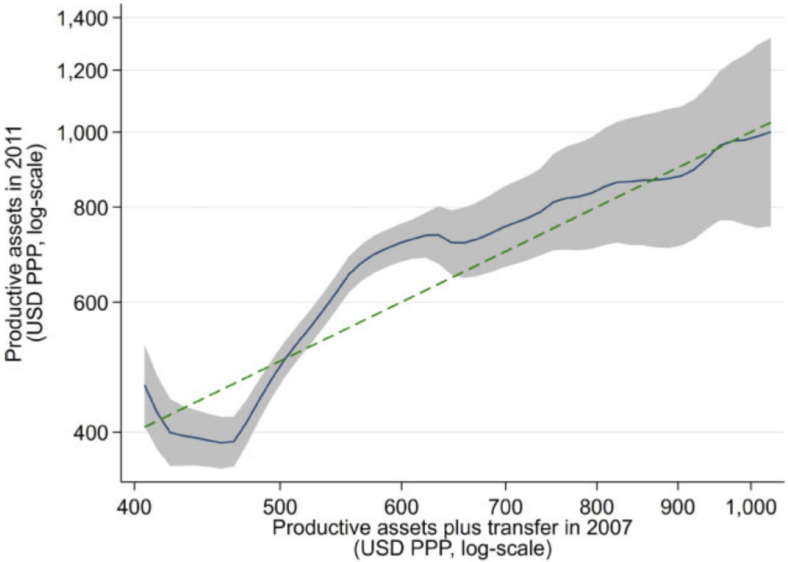
Balboni Et Al (2022): Are There Poverty Traps? Method

- Proceed in 2 steps

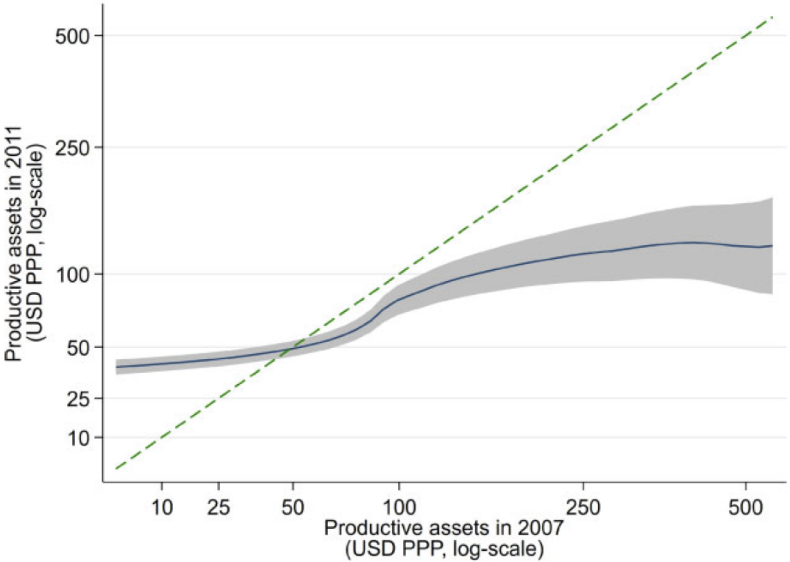
1. Is there a globally concave transition equation? Use Komarova & Hidalgo (2019) nonparametric shape test
2. If not, try and find the threshold $\hat{K}$ distinguishing the two steady states.

- In practice, do this by estimating transition equation non-parametrically and then finding its intersection with the 45-degree line numerically.

Balboni Et Al (2022): Transition Equation in Treatment Villages



Balboni Et Al (2022): Transition Equation in Control Villages



Balboni Et Al (2022): Dynamics of Wealth

- Numerical approximation suggests threshold $\hat{k} = 2.34\ (0.284)$ which is 9,379 BDT (US$508)
- Now estimate effect on dynamics of wealth accumulation.
- Define $\Delta k_i = k_{i,3} - k_{i,1}$=wealth accumulation in 4 years excluding the transfer.

$$\Delta k_i = \alpha + \beta_0 \mathbb{I}\left[k_{i,1} > \hat{k}\right] + \beta_1 \left(k_i - \hat{k}\right) + \beta_2 \mathbb{I}\left[k_{i,1} > \hat{k}\right] \times \left(k_{i,1} - \hat{k}\right) + \varepsilon_i$$

Balboni Et Al (2022): Wealth Dynamics in the short run

	Dependent variable: log change of productive assets 2007–2011				
	Panel A		Panel B		
	Treatment (1)	Treatment (2)	Control (3)	Control (4)	Both (5)
Above $\hat{k}$	0.297***	0.475***	– 0.020	– 0.097	– 0.020
	(0.043)	(0.070)	(0.052)	(0.598)	(0.057)
Baseline assets		– 2.199***		– 0.463*	
		(0.698)		(0.266)	
Above $\hat{k} \times$ baseline assets		1.969***		– 0.097	
		(0.729)		(0.269)	
Treatment					– 0.483***
					(0.059)
Above $\hat{k} \times$ treatment					0.318***
					(0.070)
Constant	– 0.138***	– 0.282***	0.345***	– 0.680	0.345***
	(0.033)	(0.057)	(0.046)	(0.592)	(0.050)
N	3,292	3,292	2,450	2,450	5,742

Balboni Et Al (2022): Wealth Dynamics in the longer run

DIFFERENCE-IN-DIFFERENCES ESTIMATES OF LONG-RUN DYNAMICS

	Productive assets (1)	Cows (2)	Land (3)	Cons. (4)	Net earnings (5)	Net earnings self-empl. (6)	Total hours (7)	Hours self-empl. (8)
Year 2 × above $\hat{k}$	260	514	1,018	$-2{,}298^{**}$	$-1{,}878^{***}$	-803^{***}	-211^{***}	-110^{***}
	(897)	(365)	(770)	(976)	(329)	(253)	(39)	(15)
Year 4 × above $\hat{k}$	$3{,}374^{**}$	$3{,}346^{***}$	1,178	−847	−443	−242	84^{**}	99^{***}
	(1,658)	(452)	(1,531)	(1,078)	(346)	(265)	(41)	(18)
Year 7 × above $\hat{k}$	2,302	$2{,}522^{***}$	821	1,561	$2{,}151^{***}$	$1{,}817^{***}$	21	−15
	(2,570)	(408)	(2,470)	(1,120)	(426)	(353)	(42)	(19)
Year 11 × above $\hat{k}$	$10{,}686^{**}$	$2{,}239^{***}$	$9{,}758^{**}$	$3{,}534^{***}$	$1{,}462^{**}$	864^{**}	87^{**}	74^{***}
	(5,003)	(374)	(4,878)	(1,267)	(703)	(424)	(41)	(17)
N	15,713	15,713	15,713	14,988	15,713	15,713	15,713	15,713
p-value year 2 vs. 4	.092	$< .01$	.924	.232	$< .01$	.097	$< .01$	$< .01$
p-value year 2 vs. 7	.448	$< .01$	.939	$< .01$	$< .01$	$< .01$	$< .01$	$< .01$
p-value year 2 vs. 11	.040	$< .01$	.076	$< .01$	$< .01$	$< .01$	$< .01$	$< .01$

Balboni Et Al (2022): Quantifying Misallocation

- If there are poverty traps, then how much efficiency is lost due to poverty? i.e. what if there were no capital constraints? Turn to a model
- Individuals allocate time endowment R between self-employment in livestock rearing l and wage labor h. They can also hire external labor h'
- Production of livestock is $q = AF\left(\bar{k}, l + h'\right) = f\left(\bar{k}\right) g\left(l + h'\right)$. Assume capital stock $\bar{k}$ is given so people only choose labor.
- Face constraints: demand on labor market $h \leq \bar{H}$, and supply on labor market $h' \leq \bar{N}$
- Make functional form assumptions so that individuals solve

$$\max_{l \geq 0, h \geq 0, h' \geq 0} Af\left(\bar{k}\right) g\left(l + h'\right) + wh - w'h' - \frac{1}{2}\left(\sqrt{\psi_l} l + \sqrt{\psi_h} h\right)^2$$

Balboni Et Al (2022): Quantifying Misallocation

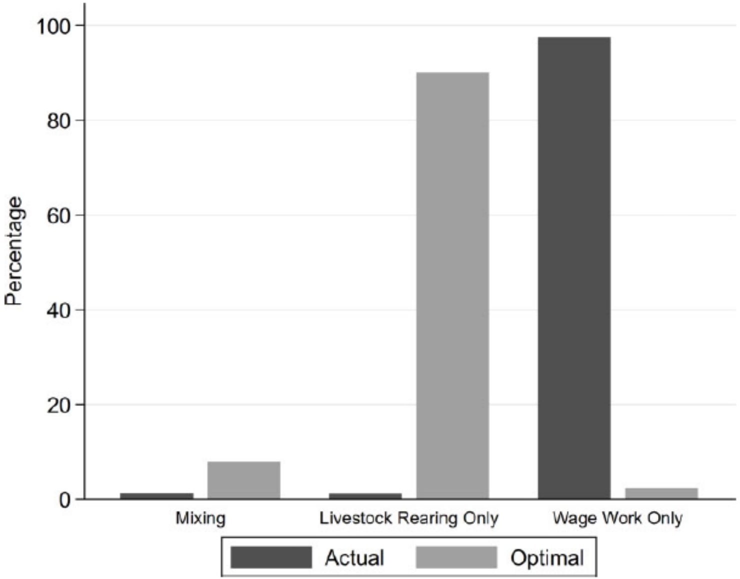
- This has solution with FOCs

$$
\begin{aligned}
Af\left(\bar{k}\right)g'\left(l+h'\right) &= \psi_l l + \sqrt{\psi_l \psi_h} h \\
w &= \sqrt{\psi_l \psi_h} l + \psi_h h \\
Af\left(\bar{k}\right)g'\left(l+h'\right) &= w'
\end{aligned}
$$

- The model is calibrated to recover each individual's A, ψ_l, ψ_h by using the baseline and year-2 data on hours devoted to each occupation and assuming a production function

$$
f\left(\bar{k}_i\right)g\left(l_i+h_i'\right) = \left(ak_i^2+bk_i\right)\left(l_i+h_i'\right)^{\beta}
$$

Balboni Et Al (2022): Quantifying Misallocation



Outline

Transfer Design: What is the Benefit?

- Bryan, Chowdhury, Mobarak, Morten & Smits (2021) *Encouragement and Distortionary Effects of Conditional Cash Transfers*
- Balboni, Bandiera, Burgess, Ghatak & Heil (2022) *Why Do People Stay Poor?*
- Cunha, De Giorgi & Jayachandran (2017) *The Price Effects of Cash Versus In-Kind Transfers*

Cunha et al (2017): Overview

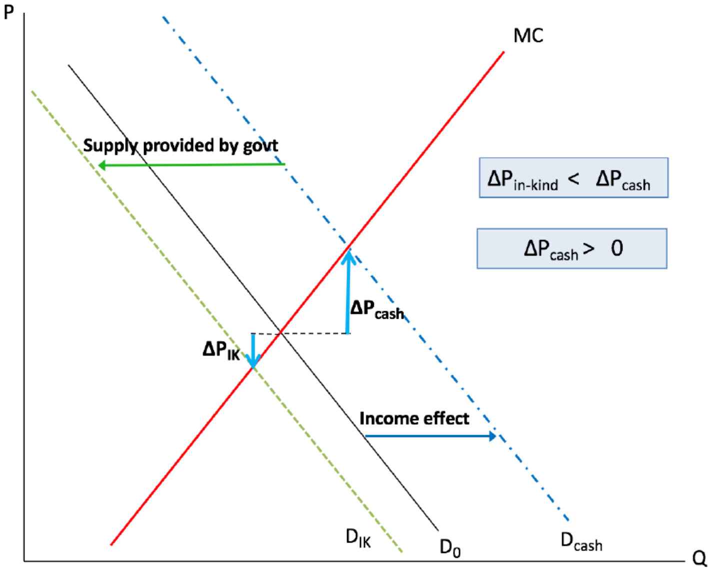
- Should transfers be cash or in-kind?
- In-kind transfers could do better at targeting.
- Cash transfers have lower admin costs, and give recipients freedom to choose.
- Also: transfers may affect prices.
  - cash transfers increase demand for normal goods $\rightarrow$ prices increase
  - in-kind transfers increase demand for normal goods, but also increase supply $\rightarrow$ lower prices under in-kind transfer
- Test this in an experiment in Mexico.

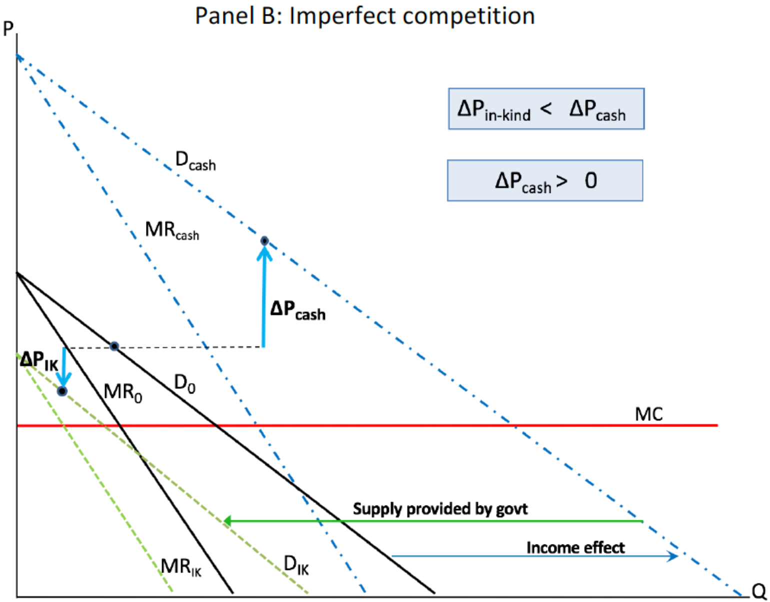
Cunha et al (2017): Conceptual Framework

- Small open economy $\rightarrow$ no price effects. Prices pinned down by world prices
- Think of partially-closed village economies. Prices depend on local conditions.
- Local suppliers are shopkeepers, supplying packaged food bought from outside the village.
- Case 1: Perfect competition
  - Cash transfer of X_{Cash} $\rightarrow$ demand shifts out, prices go up: $\partial p / \partial X_{Cash} > 0$
  - In-kind transfer with value X_{InKind} $\rightarrow$ same demand shift. Influx of supply $\rightarrow$ residual demand facing local suppliers shifts left.

$$\frac{\partial p}{\partial X_{InKind}} - \frac{\partial p}{\partial X_{Cash}} < 0$$

Panel A: Perfect competition




Panel B: Imperfect competition
P
Q
ΔP_in-kind < ΔP_cash
ΔP_cash > 0
D_cash
MR_cash
ΔP_cash
ΔP_IK
MR_0
D_0
MC
Supply provided by govt
Income effect
MR_IK
D_IK

Cunha et al (2017): Conceptual Framework

- Case 3: Cournot-Nash.
- N firms with constant MC c and linear demand $p = d - Q$.
- Equilibrium prices: $p = (d + Nc) / (N + 1)$
- Transfer changes d. Δd larger for cash transfer. Then $\Delta p/p = \Delta d / (d + Nc)$
- In a more general Cournot model, can show dependence on degree of competition

$$\frac{\partial^2 p}{\partial N \partial X_{Cash}} < 0$$

$$\frac{\partial}{\partial N}\left(\frac{\partial p}{\partial X_{InKind}} - \frac{\partial p}{\partial X_{Cash}}\right) > 0$$

Cunha et al (2017): Context

- Study Programa de Apoyo Alimentario (PAL) in Mexico
- Operates in 5,000 very poor, rural villages in Mexico.
- Villages eligible if population under 2.5K, and classified as highly marginalized.
- Typically poorer than Progresa/Oportunidades villages.
- PAL provides monthly in-kind allotment:
  - 7 basics (corn flour, rice, beans, pasta, biscuits, fortified milk powder, vegetable oil)
  - 2-4 supplements (canned tuna/sardines, lentils, corn starch, chocolate powder, cereal)
- Allotment inframarginal for most households (consumption > allotment)

Cunha et al (2017): Experimental Design

- During roll-out of the program, 208 villages randomly selected
- Each village randomly assigned to in-kind, cash, or control.
- In-kind transfer of MX$150 is $\sim 18\%$ of households' food expenditures.
- Cash transfer is $\sim 8\%$ of recipients' income, 7% increase in village income.
- Value of the in-kind transfer: Market value is MX$ 206
  - 116 pesos-worth consumed (infra-marginal)
  - 35 pesos of extra consumption of transferred goods (extra-marginal, value at 2/3 discount)
  - 55 pesos not consumed, presumably resold (extra-marginal, assume transaction costs of selling 2/3 of value)
  - $\rightarrow$ value to recipients is 146 pesos

Cunha et al (2017): PAL transfer

Item	Type	Amount per box (kg)	Value per box (pre-program, in pesos)	Calories, as % of total box	Village change in supply (ΔSupply)
	(1)	(2)	(3)	(4)	(5)
Corn flour	basic	3	15.7	20%	1.00
Rice	basic	2	12.7	12%	0.61
Beans	basic	2	21.0	13%	0.29
Fortified powdered milk	basic	1.92	76.2	17%	8.62
Packaged pasta soup	basic	1.2	16.2	8%	0.93
Vegetable oil	basic	1 (lt)	10.4	16%	0.25
Biscuits	basic	1	18.7	8%	0.81
Lentils	supplementary	1	10.3	2%	3.73
Canned tuna/sardines	supplementary	0.6	14.8	2%	1.55
Breakfast cereal	supplementary	0.2	9.3	1%	0.90

Cunha et al (2017): Data and Empirical Strategy

- Want to estimate regressions like

$$p_{gsv} = \alpha + \beta_1 InKind_v + \beta_2 Cash_v + \phi p_{gv,t-1} + \sigma I_{gv} + \varepsilon_{gsv}$$

  where p_{gsv} is price of good g at store s in village v, I_{gv} indicates imputed price

- Data from surveys of stores and households. Baseline data from 2003Q4 & 2004Q1. Follow-up survey in 2005Q4.

- Survey data on prices of 66 food items from stores, markets.

- Price data missing for 19% of village-goods. Impute from household survey (expenditure/quantity).

- End up with 360 stores in 194 villages and 12,940 good-village-store observations

Cunha et al (2017): Price Effects

	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only	All PAL goods	Basic PAL goods only
Outcome =	price	price	price	price	Δprice	Δprice
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	-0.037*	-0.033	-0.036*	-0.033	-0.062**	-0.025
	(0.020)	(0.020)	(0.020)	(0.020)	(0.029)	(0.024)
Cash	0.002	0.014	0.003	0.012	0.000	0.039
	(0.023)	(0.027)	(0.023)	(0.026)	(0.031)	(0.029)
Lagged normalized unit value	0.027	0.127***				
	(0.021)	(0.042)				
Observations	2,335	1,617	2,335	1,617	2,335	1,617
***Effect size:* In-kind - Cash**	**-0.039****	**-0.047****	**-0.038****	**-0.045****	**-0.063****	**-0.064****
***H_0*: In-kind = Cash (p-value)**	**0.02**	**0.04**	**0.03**	**0.04**	**0.02**	**0.02**

Cunha et al (2017): Persistence

	All PAL goods		Basic PAL goods only	
Outcome =	price	price	price	price
	(1)	(2)	(3)	(4)
In-kind	-0.031	-0.056**	-0.038	-0.056
	(0.022)	(0.026)	(0.031)	(0.035)
In-kind x Above median length of treatment	-0.021	-0.011	-0.022	-0.018
	(0.034)	(0.035)	(0.040)	(0.043)
Above median length of treatment	0.004	-0.002	0.018	0.013
	(0.028)	(0.029)	(0.033)	(0.035)
In-kind x Development index		-0.047**		-0.037
		(0.022)		(0.023)
Development index		0.036**		0.039**
		(0.015)		(0.016)
Observations	1,818	1,665	1,258	1,150

Cunha et al (2017): Heterogeneity

	Below-median development	Above-median development	All villages	Villages with market power	Villages without market power	Below-median price correlation	Above-median price correlation
Outcome =	price	price	price	price	price	price	price
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
In-kind	-0.036	-0.033	-0.033	-0.048*	-0.005	-0.060**	-0.019
	(0.031)	(0.027)	(0.021)	(0.025)	(0.021)	(0.028)	(0.028)
Cash	0.015	-0.007	-0.007	0.007	-0.005	0.002	-0.014
	(0.032)	(0.037)	(0.032)	(0.029)	(0.026)	(0.032)	(0.032)
Development index below-median x In-kind			-0.006				
			(0.018)				
Development index below-median x Cash			0.018				
			(0.031)				
Market power village x In-kind							
Market power village x Cash							
Price correlation below-median x In-kind							
Price correlation below-median x Cash							
Observations	1,094	1,210	2,304	1,733	602	1,115	1,220
***Effect size:* In-kind - Cash**	**-0.051****	**-0.027**	**-0.026**	**-0.055*****	**0.000**	**-0.063*****	**-0.006**
H_0: In-kind = Cash (p-value)	**0.02**	**0.37**	**0.37**	**0.01**	**1.00**	**0.01**	**0.81**

Cunha et al (2017): Effects on Producers

Outcome =	Farm profits	Farm costs	ln(Expenditure per capita)	ln(Expenditure per capita)	Asset index	Asset index
	(1)	(2)	(3)	(4)	(5)	(6)
In-kind	143.87	134.01	0.115**		0.084	
	(89.839)	(119.511)	(0.046)		(0.075)	
Cash	186.16*	345.32**	0.064		-0.040	
	(106.082)	(140.378)	(0.052)		(0.106)	
Producer x In-Kind			0.001	-0.018	0.077	0.055
			(0.060)	(0.046)	(0.115)	(0.088)
Producer x Cash			0.087	0.015	0.266*	0.229**
			(0.068)	(0.051)	(0.142)	(0.109)
Producer			-0.161***	-0.003	-0.308***	-0.007
			(0.050)	(0.036)	(0.092)	(0.071)
Control for pre-period outcome?	yes	yes	yes	yes	yes	yes
Village FE				yes		yes
Observations	4,924	5,038	5,534	5,534	5,571	5,571
***Effect size:* In-kind - Cash**	**-42.29**	**-211.31***	**0.050**		**0.124**	
***H_0*: In-kind = Cash (p-value)**	**0.67**	**0.08**	**0.25**		**0.20**	
***Effect size:* Producer x In-Kind - Producer x Cash**			**-0.086**	**-0.033**	**-0.189**	**-0.174***
***H_0*: Producer x In-Kind = Producer x Cash (p-value)**			**0.13**	**0.47**	**0.13**	**0.07**

Outline

Open Questions

- How do transfers interact with labor market formality? Should transfers be made conditional on having (previously held) formal work?
- How can governments reduce the administrative burden of enforcing conditionality/eligibility? What role does technology play in this?
- What types of transfer programs face the fewest political economy challenges? In what circumstances will governments adopt what types of policies?
- Who should administer transfer programs? What level of government? How much to rely on self-reporting?
- How much do eligibility requirements reduce incentives to graduate out of eligibility? To stay in rural areas?