GR 6307
Public Economics and Development

4. Building State Capacity with Data & Technology

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Spring 2018

Outline

Prediction Problems in Government

Examples of Data & Technology in Development

Outline

Prediction Problems in Government

- Kleinberg, Ludwig, Mullainathan & Obermeyer (AER PnP 2015)*Prediction Policy Problems*
- Kleinberg, Lakkarju, Leskovec, Ludwig & Mullainathan (WP 2017)*Human Decisions and Machine Predictions*

Kleinberg et al 2015: Prediction

- In empirical policy research, we often focus on*causal inference*. Policy choices depend on understanding the counterfactual–what would happen without the policy.
- But, there are many policy problems where causal inference is not central, or even necessary!
- Consider 2 toy examples:
  1. There is a drought. Should we invest in a rain dance to increase the chance of rain?
  2. It is cloudy. Should I take an umbrella to work?
- In both cases data are going to be useful. But they require different estimators:
  1. Do rain dances cause rain?*Causal inference*
  2. Is the chance of rain high enough to merit an umbrella?*Prediction inference*
- This paper: Policy prediction problems are everywhere, and machine learning can help us solve them more effectively.

Kleinberg et al 2015: Prediction and Causation

- Consider this framework for thinking about this.
- Let Y be an outcome (rain) that depends on X_0 (a policy choice) and other Xs.
- The policymaker must pick X_0 (umbrella, rain dance) to maximize known payoff $\pi\left(X_0, Y\right)$.
- Decision depends on

$$\frac{d\pi\left(X_0, Y\right)}{dX_0} = \frac{\partial}{\partial X_0} \underbrace{\pi\left(Y\right)}_{\text{prediction}} + \frac{\partial \pi}{\partial Y} \underbrace{\frac{\partial Y}{\partial X_0}}_{\text{causation}}$$

1. Prediction: We know the payoff, but we need to evaluate it at Y, so we need to predict what Y will be
2. Causality: How much will Y change if I change X_0?

- 2 things to note
  1. Prediction is useful when $\partial\pi/\partial X$ depends on Y (benefit of umbrella depends on rain)
  2. Only $\hat{Y}$ enters the decision, so we just need a low error estimate of $\hat{Y}$, not an unbiased or causal one

Kleinberg et al 2015: Machine Learning

- Standard empirical techniques aren't great for prediction because they focus on **unbiasedness.**
- e.g. Suppose you have 2 variables to predict y and you get OLS estimates $\hat{\beta}_1 = 1 \pm 0.001$ and $\hat{\beta}_2 = 4 \pm 10$. What's the best prediction? $x_1 + 4x_2$? or perhaps the unbiased estimator x_1 since $\hat{\beta}_2$ is noisy?
- General setup:
  - Suppose you have a dataset D of n points $(y_i, x_i) \sim G$
  - Use this data to pick a function $\hat{f} \in \mathcal{F}$ to predict the y value of a new data point $(y, x) \sim G$
  - Goal is to minimize a loss function $\mathcal{L}\left(y, \hat{f}\right) = \left(y - \hat{f}\left(x\right)\right)^2$

Kleinberg et al 2015: Machine Learning

- OLS minimizes *in-sample* error by choosing among linear functions $\mathcal{F}_{lin}$

$$\hat{f}_{OLS} = \underset{f_\beta \in \mathcal{F}_{lin}}{\arg\min} \sum_{i=1}^{n} (y_i - f(x_i))^2$$

- This does great in-sample (sum over the is in *this* dataset). But could do *arbitrarily badly* on a new dataset (out of sample)!!

$$\begin{aligned} MSE(x) &\equiv \mathbb{E}_D\left[\left(\hat{f}(x) - y\right)^2\right] \\ &= \underbrace{\mathbb{E}_D\left[\left(\hat{f}(x) - \mathbb{E}_D[\hat{y}_0]\right)^2\right]}_{\text{Variance}} + \underbrace{\left(\mathbb{E}_D[\hat{y}_0] - y\right)^2}_{\text{Bias}^2} \end{aligned}$$

Kleinberg et al 2015: Machine Learning

- So what's the alternative? ML techniques minimize

$$\hat{f}_{ML} = \arg\min_{f \in \mathcal{F}} \sum_{i=1}^{n} \left(y_i - f\left(x_i\right)\right)^2 + \lambda R\left(f\right)$$

- $R\left(f\right)$ is a *regularizer* that penalizes choosing functions that create variance. Constructed so that the set of functions $\mathcal{F}_c = \{f | R\left(f\right) \leq c\}$ creates more variable predictions as c increases.
- For linear models, larger coefficients generate more variable predictions so a natural regularizer is $R\left(f_\beta\right) = \|\beta\|^d$ ($d = 1$=LASSO, $d = 2$=ridge)
- λ is the price at which we trade off variance and bias. OLS has infinite price for bias $1/\lambda = \infty$.

Kleinberg et al 2015: Machine Learning

- How should we pick λ? A key insight of machine learning is that you can ask the data to tell you:
- Split the data into f subsets (folds).
- For a set of potential λs estimate the algorithm on $f-1$ of the folds and then see which λ predicts best on the fth fold (the hold-out fold). This procedure is called (f-fold) *cross-validation*
- This is cool because they expand the set of predictors we can consider:
  - They allow for *wide* data (e.g. text data, internet activity data) where we have many more variables than data points.
  - Much more flexible functional forms
- **But NB, you will get a great $\hat{y}$, you don't get any guarantee that you have useful $\hat{\beta}$s**

Kleinberg et al 2015: Policy Examples

1. Who should get joint replacement surgery?
2. Which teacher will have the greatest value added?
3. How long will unemployment spells last?
4. How should health inspections be targeted?
5. Predicting at-risk youth
6. Which borrowers are credit-worthy?

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Kleinberg et al 2017: Introduction

- Machine learning is all about prediction. This paper looks at what, at first glance, is an ideal application.
- After an arrest, judges decide whether defendants await trial in jail or at home. Law says this bail decision depends only on the judges prediction of whether the defendant will reoffend or flee if released.
- Here use data on 758,027 defendants in NYC between 2008 and 2013 to predict probability of reoffending.
- *Can we use these predictions to understand and improve judges' decisions?*

1. Needs methods from*both* machine learning and microeconometrics.
2. *Omitted payoffs:* We can predict probability of reoffending, but what if there are other things judges care about?
3. *Selective labels:* We only see the crime outcomes of people who are released. What would those who were jailed have done if they had been released?

Kleinberg et al 2017: Data & Context

- Shortly after someone is arrested, there's a bail hearing. Judges can a) release; b) set a dollar bail; c) detain with no chance of bail.
- Judges asked to decide based on a prediction of whether the defendant would fail to appear in court or be re-arrested for a new crime.
- Data on all arrests in NYC between 11/2008 and 11/2013: 1,460,462 cases.
- 758,027 subject to a pre-trial release decision. Randomly sample 203,338 cases to keep in a"lock box": Not used for training the algorithm or writing drafts of the paper, will only be used for the final version.
- Working data is 554,689 cases.

Table 1: Summary Statistics

	Full Sample	Judge Releases	Judge Details	P-value
Sample Size	554,689	408,283	146,406	
Release Rate	.7361	1.0000	0.00	
Outcomes				
Failure to Appear (FTA)	.1521	.1521		
Arrest (NCA)	.2581	.2581		
Violent Crime (NVCA)	.0372	.0372		
Murder, Rape, Robbery (NMRR)	.0187	.0187		
Defendant Characteristics				
Age	31.98	31.32	33.84	<.0001
Male	.8315	.8086	.8955	<.0001
White	.1273	.1407	.0897	<.0001
African American	.4884	.4578	.5737	<.0001
Hispanic	.3327	.3383	.3172	<.0001
Arrest County				
Brooklyn	.2901	.2889	.2937	.0006
Bronx	.2221	.2172	.2356	<.0001
Manhattan	.2507	.2398	.2813	<.0001
Queens	.1927	.2067	.1535	<.0001
Staten Island	.0440	.0471	.0356	<.0001

Arrest Charge				
Violent Crime				
Violent Felony	.1478	.1193	.2272	<.0001
Murder, Rape, Robbery	.0581	.0391	.1110	<.0001
Aggravated Assault	.0853	.0867	.0812	<.0001
Simple Assault	.2144	.2434	.1335	<.0001
Property Crime				
Burglary	.0206	.0125	.0433	<.0001
Larceny	.0738	.0659	.0959	<.0001
MV Theft	.0067	.0060	.0087	<.0001
Arson	.0006	.0003	.0014	<.0001
Fraud	.0696	.0763	.0507	<.0001
Other Crime				
Weapons	.0515	.0502	.0552	<.0001
Sex Offenses	.0089	.0086	.0096	.0009
Prostitution	.0139	.0161	.0078	<.0001
DUI	.0475	.0615	.0084	<.0001
Other	.1375	.1433	.1216	<.0001
Gun Charge	.0335	.0213	.0674	<.0001
Drug Crime				
Drug Felony	.1411	.1175	.2067	<.0001
Drug Misdemeanor	.1142	.1156	.1105	<.0001

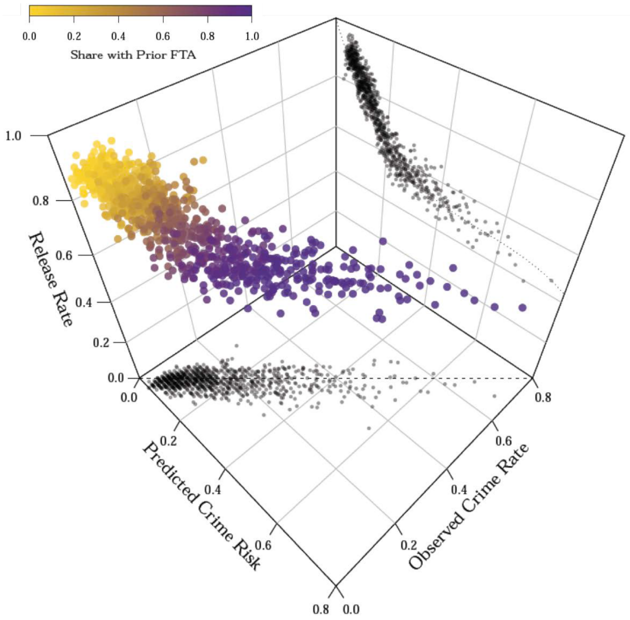
	Full Sample	Judge Releases	Judge Detains	P-value
Defendant Priors				
FTAs	2.093	1.305	4.288	$<.0001$
Felony Arrests	3.177	2.119	6.127	$<.0001$
Felony Convictions	.6157	.3879	1.251	$<.0001$
Misdemeanor Arrests	5.119	3.349	10.06	$<.0001$
Misdemeanor Convictions	3.122	1.562	7.473	$<.0001$
Violent Felony Arrests	1.017	.7084	1.879	$<.0001$
Violent Felony Convictions	.1521	.1007	.2955	$<.0001$
Drug Arrests	3.205	2.144	6.163	$<.0001$
Felony Drug Convictions	.2741	.1778	.5429	$<.0001$
Misdemeanor Drug Convictions	1.049	.5408	2.465	$<.0001$
Gun Arrests	.2194	.1678	.3632	$<.0001$
Gun Convictions	.0462	.0362	.0741	$<.0001$

Kleinberg et al 2017: Machine Learning

- Form predictions $\hat{y} = m(x)$ to minimize loss function $L(y, \hat{y})$
- Consider functions $m(x)$ that generate predicted probabilities in $[0, 1]$
- Use Bernoulli loss

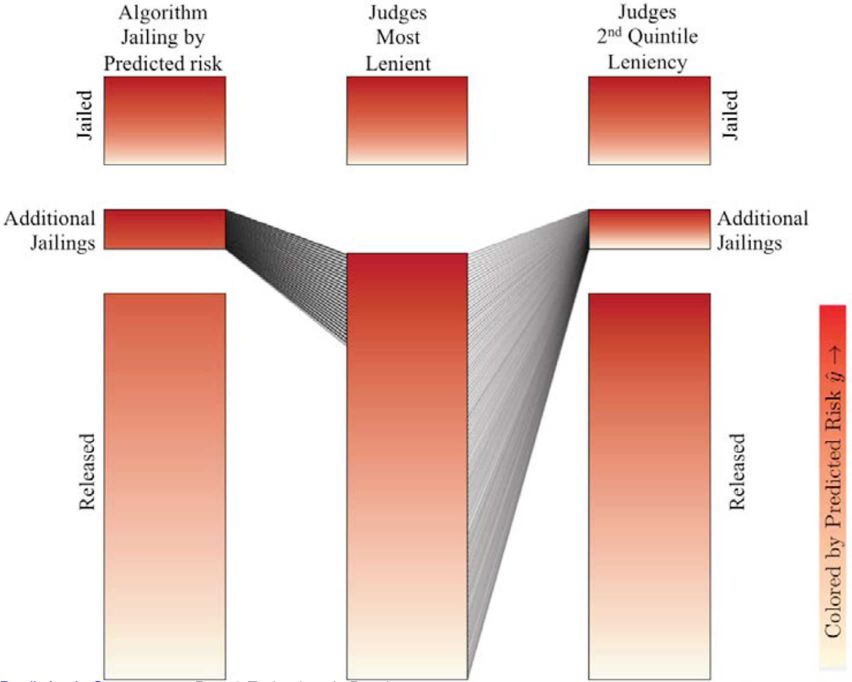
$$L(y_i, m(x_i)) = -[y_i \log(m(x_i)) + (1 - y_i)\log(1 - m(x_i))]$$

- For this paper, use gradient boosted decision trees to form $m(x_i)$. Average multiple trees built sequentially where each iteration up-weights observations that fit poorly in the sequence of trees up to that point.


0.0
0.2
0.4
0.6
0.8
1.0
Share with Prior FTA
1.0
0.8
0.6
0.4
0.2
0.0
Release Rate
0.0
0.2
0.4
0.6
0.8
Predicted Crime Risk
0.0
0.2
0.4
0.6
0.8
Observed Crime Rate

Kleinberg et al 2017: Misranking?

- The riskiest 1% of defendants have a predicted risk of 62.6%, but 48.5% of them are released, and reoffend 56.3% of the time!
- Suggests Judges are misranking the defendants. But could also be that the judges use an even higher threshold risk for detention.
- Look across judges of different leniencies. If it's the case that more lenient judges just have a higher threshold risk, then the predicted risk scores should be good at predicting which people will be jailed by a less lenient judge relative to a lenient judge.
- Identified by quasi-random assignment of cases to judges: It depends on who happens to be on duty in that borough$\times$ court house$\times$ day


Algorithm
Jailing by
Predicted risk
Judges
Most
Lenient
Judges
2nd Quintile
Leniency
Jailed
Jailed
Additional
Jailings
Additional
Jailings
Released
Released
Colored by Predicted Risk $\hat{y} \rightarrow$

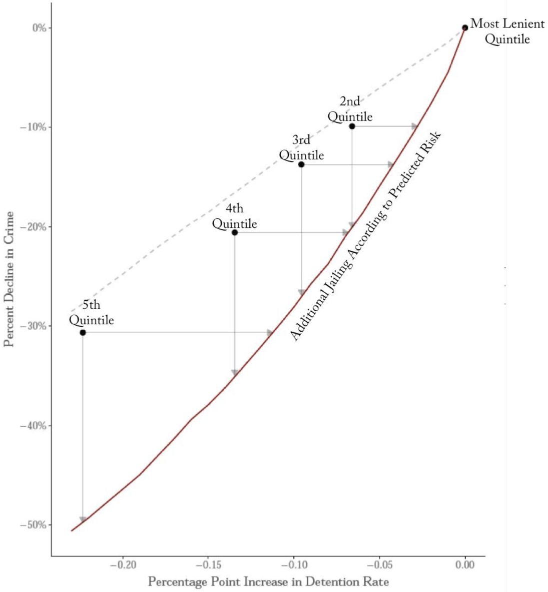


Figure 5: Comparing Impact of Detaining in order of Predicted Risk To What Judges Achieve

Kleinberg et al 2017: Alternative Policies

- So can we use the algorithm to improve release decisions?
- Consider 2 types of policies

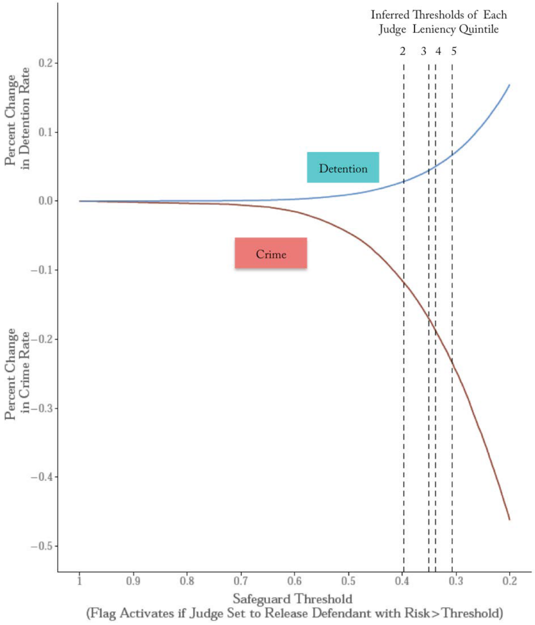
1. Contraction: A warning whenever the judge is about to release a high-risk defendant. Like a driver assist system that warns when the car does something potentially dangerous.
2. Reranking: Risk tool danks defendants and makes recommendations for all decisions. Like an auto-pilot that the judge could overrule. Improves both high- and low-risk decisions.

- Now we need a counterfactual: 2 issues arise

1. Compliance: Will the judge pay attention to the warnings / comply with the risk score?
2. Reranking involves releasing some of the jailed defendants. How much crime*would they have* committed? A missing labels problem.

Kleinberg et al 2017: Contraction

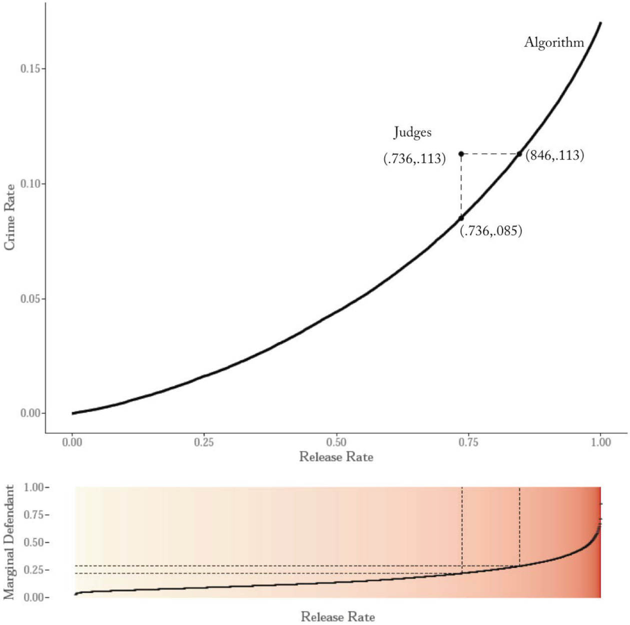
- What threshold risk score should trigger the warning?
- Low threshold averts more crime, but triggers more warnings, and increases jailings of people who would not reoffend.
- Implicitly, the judges have some threshold too that we can compare to.



Inferred Thresholds of Each Judge Leniency Quintile
2
3
4
5
Detention
Crime
Percent Change in Detention Rate
Percent Change in Crime Rate
0.2
0.1
0.0
−0.1
−0.2
−0.3
−0.4
−0.5
1
0.9
0.8
0.7
0.6
0.5
0.4
0.3
0.2
Safeguard Threshold
(Flag Activates if Judge Set to Release Defendant with Risk>Threshold)

Kleinberg et al 2017: Reranking

- Reranking creates a selective labels problem: We only see crime rates of those who are jailed.
- Approach here: Impute based on observables. Then do 2 bounding exercises
  1. Decompose algorithm's gains into
     1.1 jail a high risk defendant and release an *average* risk defendant
     1.2 release a low risk defendant and jail an *average* risk defendant. Selective labels problem only comes in here.
  2. Assume imputing underestimates by α and use a prediced risk of $\min\{1, \alpha\hat{y}\}$ and do robustness to α.


Algorithm
Judges
(.736,.113)
(846,.113)
(.736,.085)
Crime Rate
0.15
0.10
0.05
0.00
0.00
0.25
0.50
0.75
1.00
Release Rate
Marginal Defendant
1.00
0.75
0.50
0.25
0.00
Release Rate

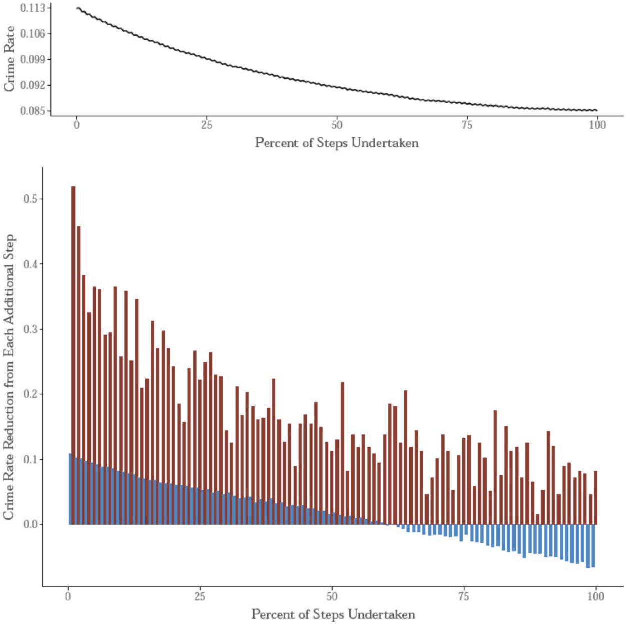

0.113
0.106
0.099
0.092
0.085
Crime Rate
0
25
50
75
100
Percent of Steps Undertaken
0.5
0.4
0.3
0.2
0.1
0.0
Crime Rate Reduction from Each Additional Step
0
25
50
75
100
Percent of Steps Undertaken

Table 5: Policy Simulation Under Different Assumptions

	Assume $y = min(1, \alpha\hat{y})$ for Additional Releases Beyond Most Lenient Judge Quintile's Release Rate						
	Value of α						
	1	1.25	1.5	2	3	$\cdots$	∞
Algorithm's Crime Rate	.0854	.0863	.0872	.0890	.0926		.1049
at Judge's Jail Rate	(.0008)	(.0008)	(.0008)	(.0009)	(.0009)		(.0009)
Percentage Reduction	-24.68%	-24.06%	-23.01%	-21.23%	-18.35%		-14.39%
Algorithm's Jail Rate	.1531	.1590	.1642	.1733	.1920		.2343
at Judge's Release Rate	(.0011)	(.0011)	(.0011)	(.0011)	(.0012)		(.0013)
Percentage Reduction	-41.85%	-40.13%	-38.37%	-34.87%	-29.36%		-18.51%

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Abelson et al 2014

- Work with GiveDirectly to see how data can be used to improve targeting.
- Use satellite data and roof material (thatch vs metal) to predict poverty.
  - Metal roof better: Mosquitos live in thatch, leak and collapse regularly.
  - Metal roof is expensive:$\$564$.
  - Good proxy for poverty
- Aggregate up to the village level to rank villages for GiveDirectly operations.

Abelson et al 2014

(a) (b)

Figure 1: Homes in central east Africa with (a) metal and (b) thatched roofs.

Abelson et al 2014

Figure 2: Example of metal roof in center of satellite image.

Figure 3: Example of thatched roof in center of satellite image.

Abelson et al 2014

- Data has issues:
  - Satellite data is from google maps: Sometimes there are clouds, some pictures from the wet season, others from the dry season.
- Training data from GiveDirectly which does census in villages it works in and has roof type in it.
- Set up a crowdsourcing application (using Flask in Python)

Abelson et al 2014

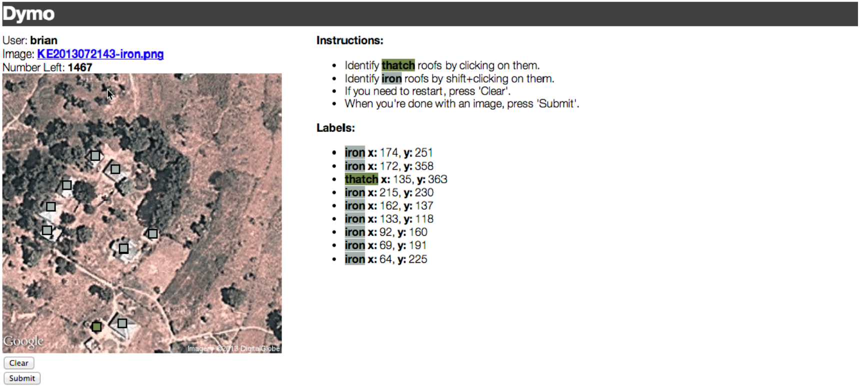

Figure 6: Screen shot of application deployed for crowdsourced labeling of roofs in satellite images.

Abelson et al 2014

- Train algorithms for two problems:
  1. How many roofs in each 400 x 400 pixel satellite image?
  2. What fraction of the roods are metal?
- Use random forests for this problem.

Abelson et al 2014

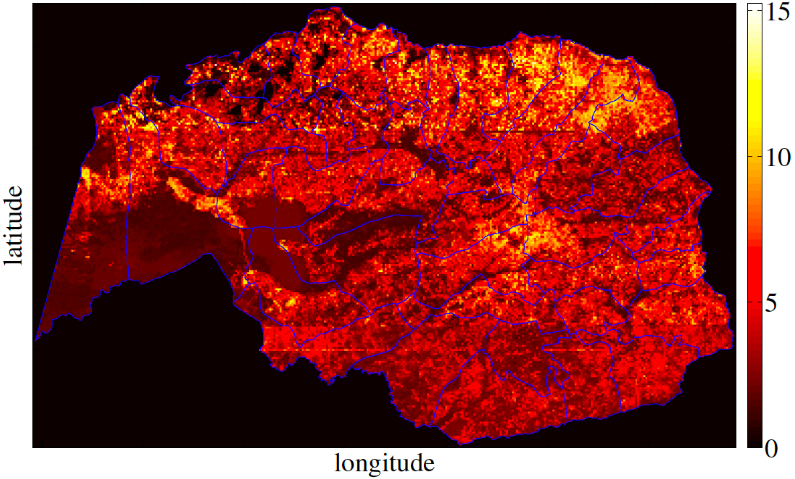

Figure 10: Heat map of number of total estimated roofs per 400×400 pixel image in the region of interest.

Abelson et al 2014

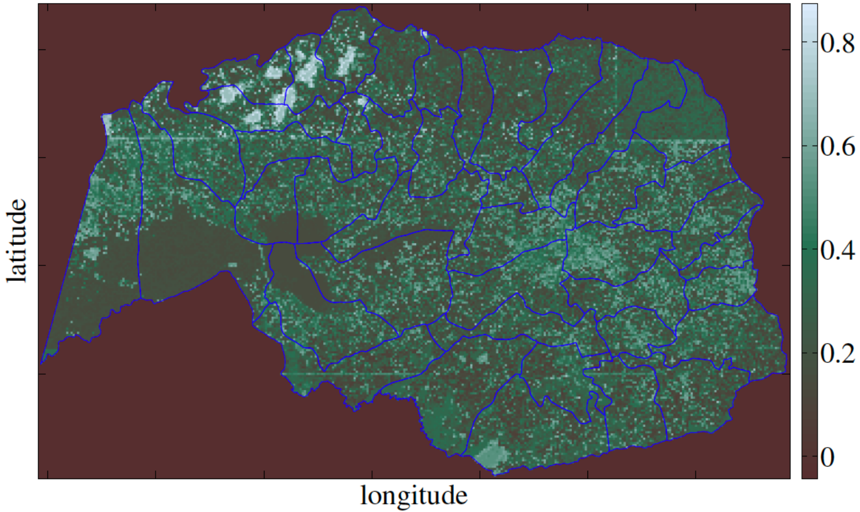

Figure 11: Heat map of proportion of roofs that are metal in the region of interest.

Abelson et al 2014

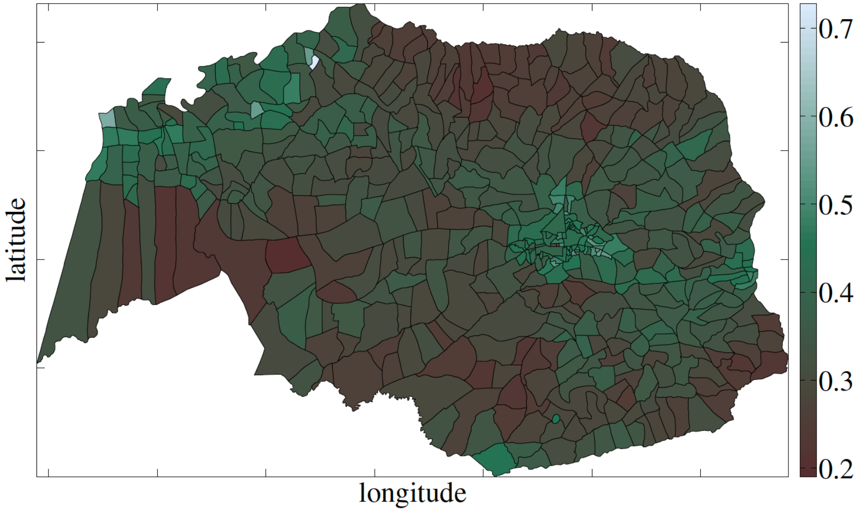

Figure 12: Estimated proportion of metal roofs in villages in the region of interest.

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Blumenstock et al 2015

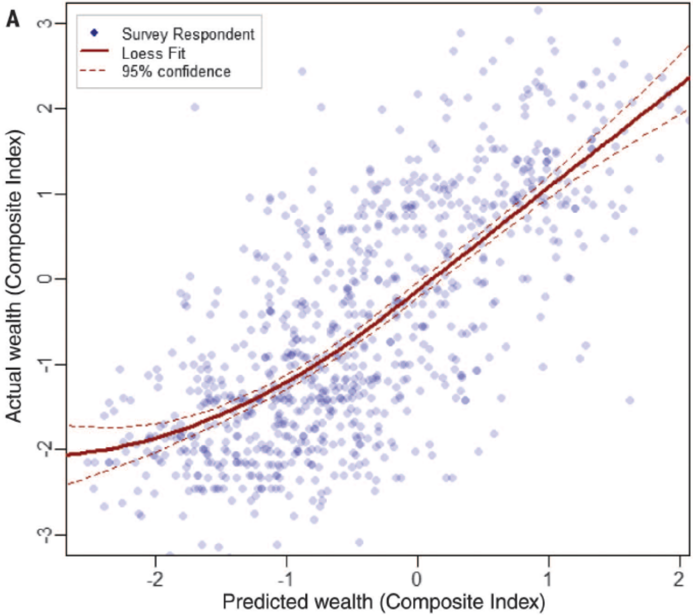
- In many countries, survey data is either completely missing, or unreliable (national stats shown to be off by as much as 50%)
- This paper will try to use metadata from mobile phone users to predict the user's socioeconomic characteristics.
  - Very fine-grained prediction
  - Usedul for applications that require individual-level information (targeting, policy interventions etc)
- Use data on billions of interactions over Rwanda's largest mobile phone network and a follow-up phone survey of 856 individual subscribers.

Blumenstock et al 2015

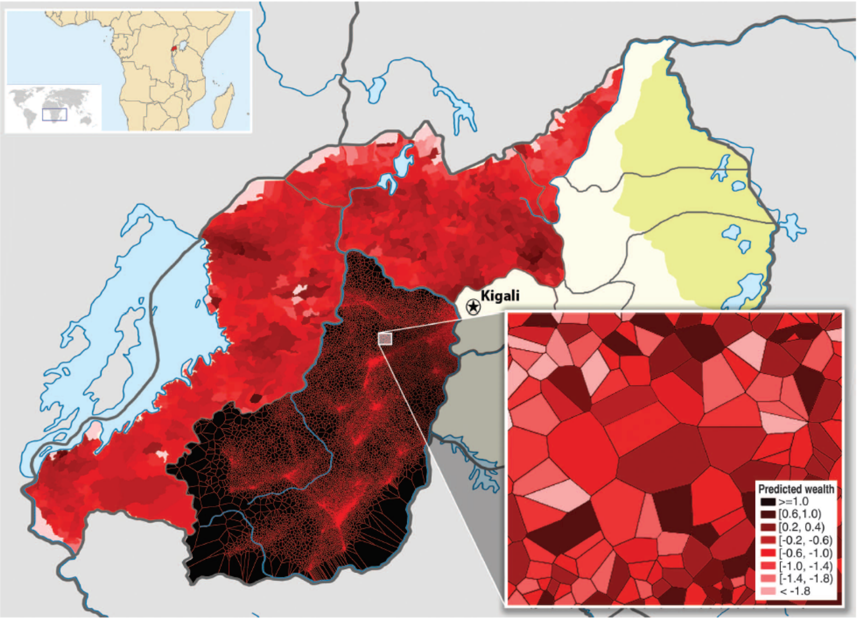
Table 1. Summary statistics for primary data sets. Phone survey data were collected by the authors in Kigali, in collaboration with the Kigali Institute of Science and Technology. Call detail records were collected by the primary mobile phone operator in Rwanda at the time of the phone survey. Demographic and Health Survey (DHS) data were collected by the Rwandan National Institute of Statistics. N/A, not applicable.

Summary statistic	Phone survey	Call detail records	DHS (2007)	DHS (2010)
Number of unique individuals	856	1.5 million	7377	12,792
Data collection period	July 2009	May 2008–May 2009	Dec. 2007–Apr. 2008	Sept. 2010–Mar. 2011
Number of questions in survey	75	N/A	1615	3396
Primary geographic units	30 districts	30 districts	30 districts	30 districts
Secondary geographic units	300 cell towers	300 cell towers	247 clusters	492 clusters

Blumenstock et al 2015



Blumenstock et al 2015



Blumenstock et al 2015

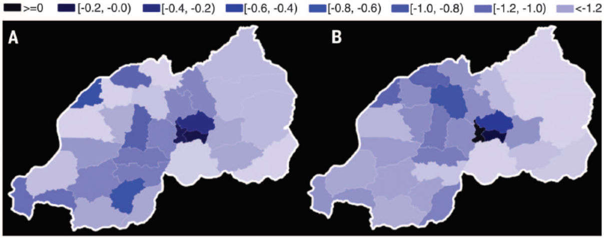
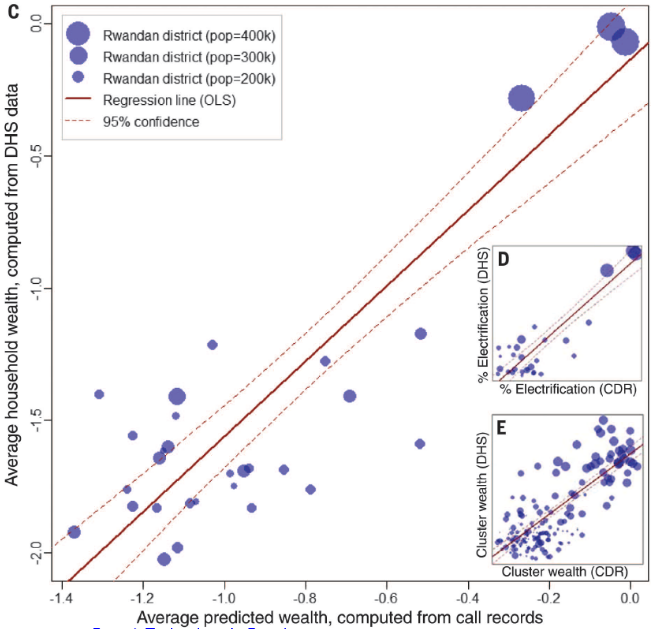


Fig. 3. Comparison of wealth predictions to government survey data. (**A**) Predicted composite wealth index (district average), computed from 2009 call data and aggregated by administrative district. (**B**) Actual composite wealth index (district average), as computed from a 2010 government DHS of 12,792 households. (**C**) Comparison of actual and predicted district wealth, for each of the 30 districts, with dots sized by population. (**D**) Comparison of actual and predicted rates of electrification, for each of the 30 districts. (**E**) Comparison of actual and predicted cluster wealth, for each of the 492 DHS clusters. CDR, call detail records.

Blumenstock et al 2015



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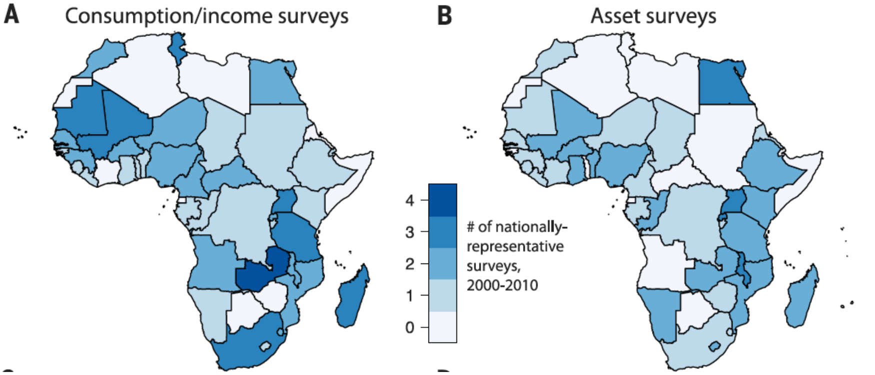
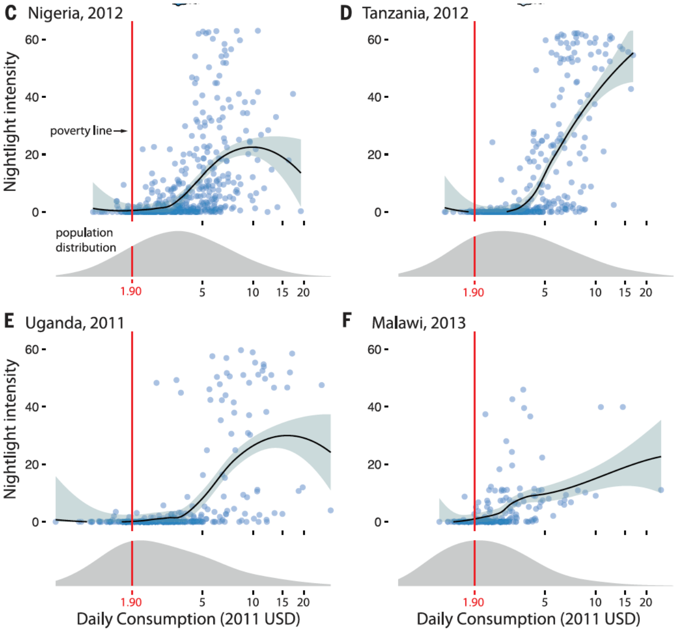
Jean et al 2016

- Focus on 5 African countries: Nigeria, Tanzania, Uganda, Malawi & Rwanda.
- One popular approach in data-scarce environments has been to use nightlights in satellite images.
- Combine satellite data with:
  - World Bank Living Standards Measurement Study (LSMS) surveys: measures expenditure
  - Demographic and Health Surveys (DHS): measures welath.
- Proceed in 3 steps
  1. Train a Convolutional Neural Network (CNN) on ImageNet to identify low-level features of images common to many image classification tasks
  2. Fine-tune the CNN by getting it to predict nightlights using daytime satellite images. Reduce the dimensionality of the daytime pictures to the features that tend to predict nightlights: a coarse measure of wellbeing
  3. Use ridge regression of survey data on cluster-level image features to predict expenditure and wealth.

Jean et al 2016




C Nigeria, 2012
D Tanzania, 2012
E Uganda, 2011
F Malawi, 2013
Nightlight intensity
60
40
20
0
poverty line
population distribution
1.90
5
10
15
20
Daily Consumption (2011 USD)

Jean et al 2016

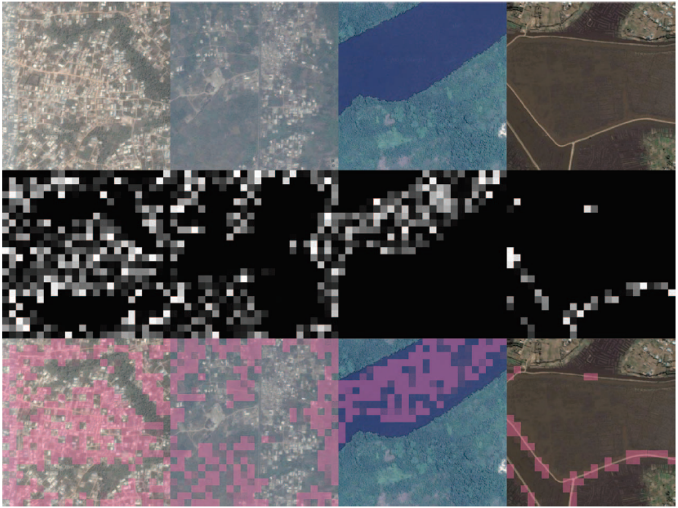
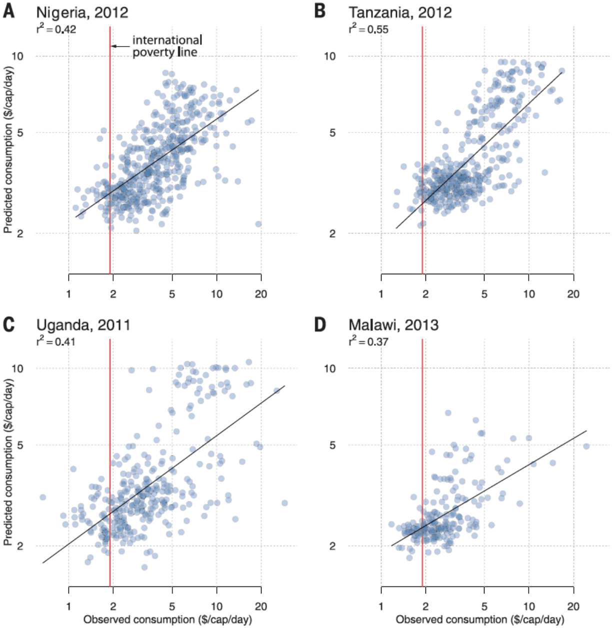


Fig. 2. Visualization of features. By column: Four different convolutional filters (which identify, from left to right, features corresponding to urban areas, nonurban areas, water, and roads) in the convolutional neural network model used for extracting features. Each filter "highlights" the parts of the image that activate it, shown in pink. By row: Original daytime satellite images from Google Static Maps, filter activation maps, and overlay of activation maps onto original images

Jean et al 2016



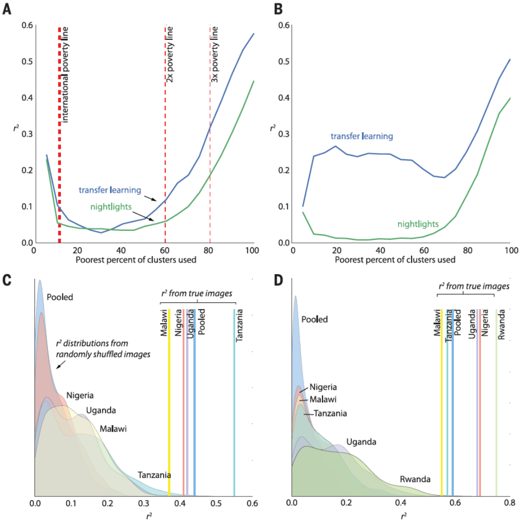


Fig. 4. Evaluation of model performance. (**A**) Performance of transfer learning model relative to nightlights for estimating consumption, using pooled observations across the four LSMS countries. Trials were run separately for increasing percentages of the available clusters (e.g., *x*-axis value of 40 indicates that all clusters below 40th percentile in consumption were included). Vertical red lines indicate various multiples of the international poverty line. Image features reduced to 100 dimensions using principal component analysis. (**B**) Same as (A), but for assets. (**C**) Comparison of r^2 of models trained on correctly assigned images in each country (vertical lines) to the distribution of r^2 values obtained from trials in which the model was trained on randomly shuffled images (1000 trials per country). (**D**) Same as (C), but for assets. Cross-validated r^2 values are reported in all panels.

Outline

Examples of Data & Technology in Development

- Abelson, Varshney & Sun (2014): *Targeting Direct Cash Transfers to the Extremely Poor*
- Blumenstock, Cadamuro & On (Science 2015) *Predicting Poverty and Wealth from Mobile Phone Metadata*
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- Muralidharan, Niehaus & Sukhtankar (AER 2016) *Building State Capacity: Evidence from Biometric Smartcards in India*
- Muralidharan, Niehaus & Sukhtankar (2018) *General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India*

Muralidharan et al 2016: Introduction

- Implementing antipoverty programs in developing countries is hard.
- Identifying beneficiaries and delivering payments to them is a key challenge.
- Lots of optimism around the use of biometric technology to supplant PIN numbers or paper-based IDs.
- But:
    1. requires all steps of implementation to go right,
    2. If it threatens rents, could be subverted by incumbent rent-seekers.
    3. Could exclude genuine beneficiaries due to technical problems
    4. Could just displace corruption to somewhere else and weaken bureaucrats' incentives to implement the program at all.
- Study the randomized rollout of a Smartcard payment system in NREGS workfare program in India

Muralidharan et al 2016: Context

- NREGS is the world's largest guaranteed employment scheme.
  - All households guaranteed 100 days' paid work a year.
  - Participants get jobcards to record employment & payment.
- Logistics, leakage and access have been issues in implementation.
  - slow and unreliable payment
  - overreporting of work and underpayment of workers are problems.
- Social Security Pensions (SSP) program targets Below Poverty Line families and old, widowed, or disabled people.

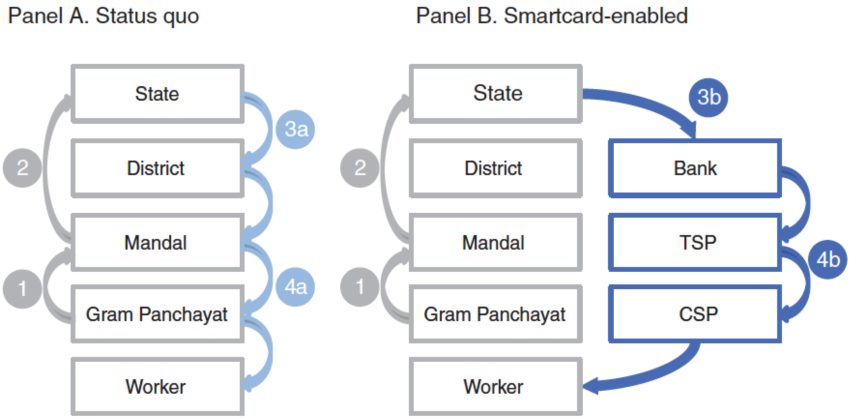
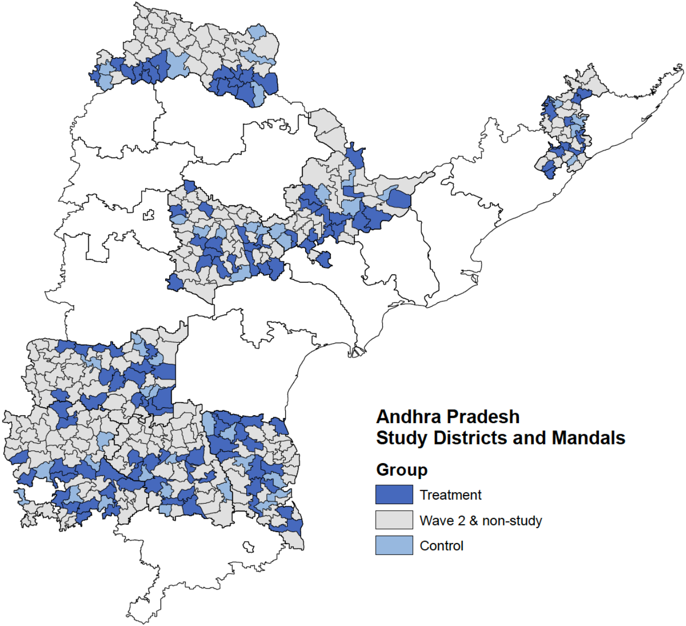


FIGURE 1. COMPARISON OF TREATMENT AND CONTROL PAYMENT SYSTEMS

- Work directly with government of Andhra Pradesh.
- Randomize the order in which the Smartcard system rolled out across mandals (sub-districts)
- Randomized 296 mandals into
  - treatment (112)
  - control (45)
  - "buffer" (139)
- Buffer to maximize time between treatment and control and to permit gathering endline data in treatment mandals before rollout to control districts.
- Randomization stratified by district and the first principal component of mandal-level socio-economic characteristics.



Andhra Pradesh
Study Districts and Mandals
Group
Treatment
Wave 2 & non-study
Control

Muralidharan et al 2016: Data

1. Household surveys representative of universe of NREGS jobcard holders (Octobber 2010 baseline and 2012 endline)
   1.1 sample of jobcard holders who had recently participated in NREGS
   1.2 sampled a panel of 880 villages, avg 6 households sampled per village.
   1.3 Audits of worksites to count workers.
2. Administrative data
   2.1 land under cultivation from District Statistical Handbooks
   2.2 Cost data and prices from National Sample Survey
   2.3 mandal-level headcounts of livestock.
   2.4 Geocoded locations of each census village in the 2001 census.

Muralidharan et al 2016: First Stage

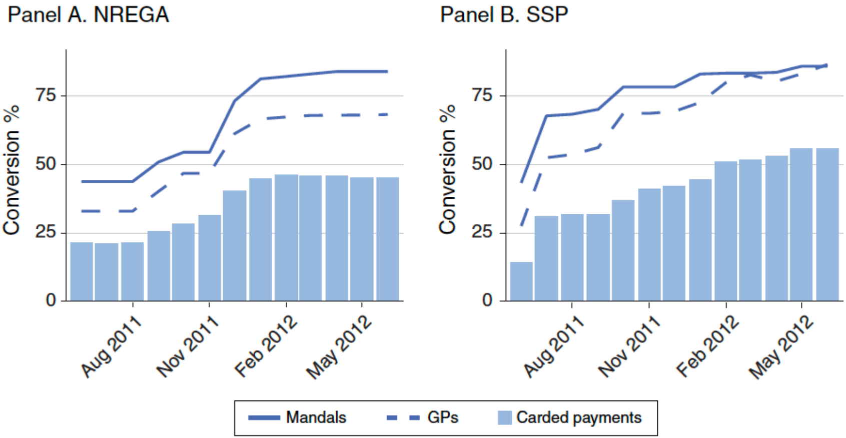


FIGURE 3. ROLLOUT OF SMARTCARD INTEGRATION WITH WELFARE PROGRAMS

TABLE 1—OFFICIAL AND SELF-REPORTED USE OF SMARTCARDS

	Official data		Survey data	
	Carded GP (1)	Mean fraction carded payments (2)	Payments generally carded (village mean) (3)	Most recent payment carded (village mean) (4)
Panel A. NREGS				
Treatment	0.67	0.45	0.38	0.38
	(0.045)	(0.041)	(0.043)	(0.042)
District fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.45	0.48	0.36	0.36
Control mean	0.0046	0.0017	0.039	0.013
Observations	880	880	818	818
Level	GP	GP	GP	GP
Panel B. SSP				
Treatment	0.79	0.59	0.45	0.45
	(0.042)	(0.038)	(0.052)	(0.049)
District fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.57	0.57	0.38	0.38
Control mean	0	0	0.069	0.044
Observations	880	880	878	878
Level	GP	GP	GP	GP

Muralidharan et al 2016: Estimation

- Start out by just estimating the difference between treated and control mandals:

$$Y_{imd} = \alpha + \beta Treated_{md} + \delta District_d + \lambda PC_{md} + \epsilon_{imd}$$

  where Y_{imd} is outcome of household i in mandal m in district d and PC_{md} is the principal component of socioeconomic variables used for stratification.
- In the survey data, to increase precision, also include the baseline panchayat-mean level of the outcome:

$$Y_{ipmd} = \alpha + \beta Treated_{md} + \gamma \bar{Y}^0_{pmd} + \delta District_d + \lambda PC_{md} + \epsilon_{ipmd}$$

- Many possible channels, $\Rightarrow \beta$ is the composite effect of a mix of several factors. But this is the policy-relevant GE estimate of the total effect due to the effective presence of NREGS
- If spillovers across mandals, then β misestimates the Total Treatment Effect (TTE): The difference in average outcomes when all units are treated and when no units are treated.

Muralidharan et al 2016: Results

TABLE 2—ACCESS TO PAYMENTS

	Time to collect (min)				Avg. payment lag (days)		Abs. payment lag deviation (days)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	−22	−22	−6.1	−3.5	−5.8	−10	−2.5	−4.7
	(9.2)	(8.7)	(5.2)	(5.4)	(3.5)	(3.5)	(0.99)	(1.6)
BL GP mean		0.079		0.23		0.013		0.042
		(0.041)		(0.07)		(0.08)		(0.053)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	No	No	No	Yes	Yes	Yes	Yes
Adjusted R^2	0.06	0.08	0.07	0.11	0.17	0.33	0.08	0.17
Control mean	112	112	77	77	34	34	12	12
Observations	10,191	10,120	3,789	3,574	14,213	7,201	14,213	7,201
Level	Indiv.	Indiv.	Indiv.	Indiv.	Indiv.-week	Indiv.-week	Indiv.-week	Indiv.-week
Survey	NREGS	NREGS	SSP	SSP	NREGS	NREGS	NREGS	NREGS

TABLE 3—OFFICIAL AND SURVEY REPORTS OF PROGRAM BENEFITS

	Official		Survey		Leakage	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. NREGS						
Treatment	11	9.6	35	35	−24	−25
	(12)	(12)	(16)	(16)	(13)	(13)
BL GP mean		0.13		0.11		0.096
		(0.027)		(0.037)		(0.038)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.03	0.05	0.05	0.06	0.04	0.04
Control mean	127	127	146	146	−20	−20
Observations	5,143	5,107	5,143	5,107	5,143	5,107
Panel B. SSP						
Treatment	4.3	5.1	12	12	−7.5	−7
	(5.3)	(5.4)	(5.9)	(6.1)	(3.9)	(3.9)
BL GP mean		0.16		0.0074		−0.022
		(0.092)		(0.022)		(0.026)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.00	0.01	0.01	0.01	0.01	0.01
Control mean	251	251	236	236	15	15
Observations	3,330	3,135	3,330	3,135	3,330	3,135

TABLE 4—ILLUSTRATING CHANNELS OF LEAKAGE REDUCTION

	Ghost households (percent)		Other overreporting (percent)		Bribe to collect (percent)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. NREGS						
Treatment	−0.0095	−0.0091	−0.082	−0.084	−0.0035	−0.0036
	(0.02)	(0.021)	(0.033)	(0.036)	(0.0085)	(0.0085)
BL GP mean		−0.017		0.016		0.000041
		(0.067)		(0.044)		(0.000041)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.02	0.02	0.05	0.04	0.01	0.01
Control mean	0.11	0.11	0.26	0.26	0.021	0.021
Observations	5,278	5,242	3,953	3,672	10,375	10,304
Level	Hhd	Hhd	Hhd	Hhd	Indiv.	Indiv.

	Ghost payments (Rs)		Other overreporting (Rs)		Underpayment (Rs)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel B. SSP						
Treatment	−2.9	−2.4	−2.7	−3.1	−2.3	−2.4
	(2.7)	(2.7)	(2.9)	(3)	(1.9)	(2)
BL GP mean		0.19		0.024		−0.02
		(0.16)		(0.01)		(0.045)
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.01	0.01	0.01	0.01	0.01	0.01
Control mean	11	11	1.7	1.7	2.5	2.5
Observations	3,330	3,135	3,165	2,986	3,165	2,986

TABLE 5—ACCESS TO PROGRAMS

	Proportion of Hhds doing NREGS work		Was any Hhd member unable to get NREGS work in:		Is NREGS work available when anyone wants it?		Did you have to pay anything to get this NREGS work?		Did you have to pay anything to start receiving this pension?	
	Study period (1)	Study period (2)	May (3)	January (4)	All months (5)	All months (6)	NREGS (7)	NREGS (8)	SSP (9)	SSP (10)
Treatment	0.072	0.071	−0.023	−0.027	0.027	0.024	−0.0003	−0.00054	−0.046	−0.055
	(0.033)	(0.033)	(0.027)	(0.033)	(0.015)	(0.015)	(0.0015)	(0.0015)	(0.031)	(0.039)
BL GP mean		0.14				−0.023		−0.0064		0.025
		(0.038)				(0.027)		(0.0031)		(0.046)
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.05	0.06	0.10	0.11	0.02	0.02	0.00	0.00	0.05	0.05
Control mean	0.42	0.42	0.2	0.42	0.035	0.035	0.0022	0.0022	0.075	0.075
Observations	4,943	4,909	4,748	4,496	4,755	4,715	7,185	6,861	581	352
Level	Hhd	Hhd	Hhd	Hhd	Hhd	Hhd	Indiv.	Indiv.	Indiv.	Indiv.

Outline

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- Muralidharan, Niehaus & Sukhtankar (2018) *General Equilibrium Effects of (Improving) Public Employment Programs: Experimental Evidence from India*

Muralidharan et al 2018: Introduction

- Public employment programs are a common anti-poverty policy in developing countries.
- India's National Rural Employment Guarantee Scheme (NREGS) is the world's largest such program: 600 million eligible participants and 0.5% of GDP.
- Given the scale of the program, you might expect such a program to have important GE effects.
- But these are very hard to estimate:

1. No experimental variation
2. NREGs implemented very differently in different places, so not clear what you'd be comparing:"construct validity"
3. Spillovers across districts make identification very challenging.

- This paper tries to overcome these challenges with an experiment in Andhra Pradesh

Muralidharan et al 2018: Context

- NREGS program in India is enormous.
- But several well-known issues with program implementation:
  - Rationing of jobs
  - Corruption through over-invoicing for work not done, or paying workers less than their due.
  - Payment slow and unreliable.
- Government of AP introduced smartcards to improve program implementation
  - Payments delivered by banks instead of government-run post offices
  - identification through biometric smartcard instead of paper documents and ink stamps

Muralidharan et al 2018: Experiment

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- Randomize the order in which the Smartcard system rolled out across mandals (sub-districts)
- Randomized 296 mandals into
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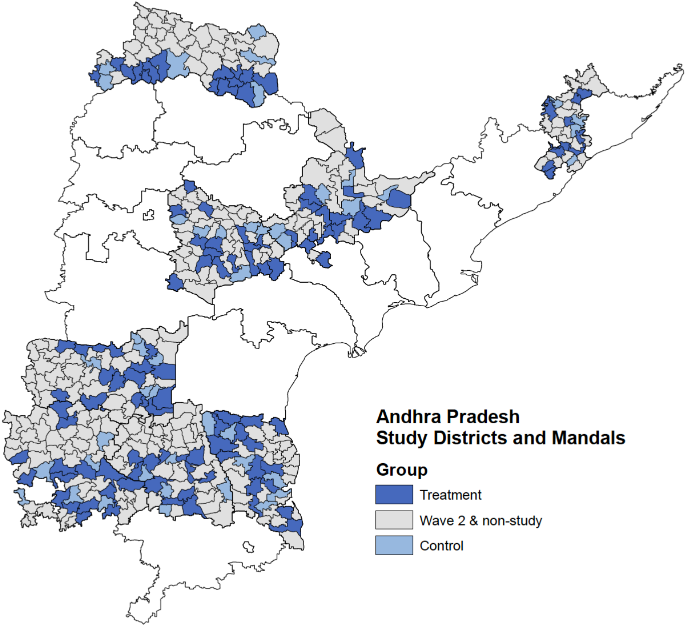
- Randomization stratified by district and the first principal component of mandal-level socio-economic characteristics.


Andhra Pradesh
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Group
Treatment
Wave 2 & non-study
Control

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1. Socio-Economic and Caste Census (SECC). independent nation-wide census to rank households by socio-economic status to tdetermine whteher“Below Poverty Line”.
2. Household surveys representative of universe of NREGS jobcard holders (Octobber 2010 baseline and 2012 endline)
   2.1 sample of jobcard holders who had recently participated in NREGS
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   3.4 Geocoded locations of each census village in the 2001 census.

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  where Y_{imd} is outcome of household i in mandal m in district d and PC_{md} is the principal component of socioeconomic variables used for stratification.
- In the survey data, to increase precision, also include the baseline panchayat-mean level of the outcome:

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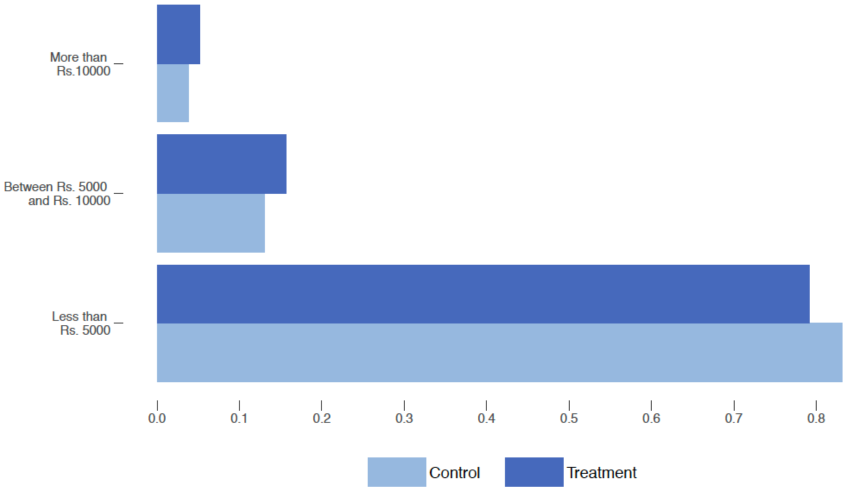
- Many possible channels, $\Rightarrow$ β is the composite effect of a mix of several factors. But this is the policy-relevant GE estimate of the total effect due to the effective presence of NREGS
- If spillovers across mandals, then β misestimates the Total Treatment Effect (TTE): The difference in average outcomes when all units are treated and when no units are treated.

Figure A.1: Effects on income/month: SECC



Muralidharan et al 2018: Income

(b) Survey data (Rs. per year)

	Total income		NREGA	Agricultural labor	Other labor	Farm	Business	Miscellaneous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment	9511	8761	914	3276	3270	2166	-642	528
	(3723)	(3722)	(588)	(1467)	(1305)	(2302)	(1325)	(2103)
BL GP Mean		.025						
		(.071)						
Adj. R-squared	0.04	0.04	0.01	0.06	0.06	0.02	0.01	0.01
Control Mean	69122	69122	4743	14798	9322	20361	6202	13695
N	4908	4874	4907	4908	4908	4908	4908	4908

Muralidharan et al 2018: Wages

Table 2: Wages (June)

	Wage realization (Rs.)		Reservation wage (Rs.)	
	(1)	(2)	(3)	(4)
Treatment	6.6	7.8	4.9	5.5
	(3.6)	(3.6)	(2.9)	(2.8)
BL GP Mean		.15		.12
		(.053)		(.043)
Adj. R-squared	.07	.07	.05	.05
Control Mean	128	128	97	97
N	7304	7090	12905	12791

This table shows treatment effects on wage outcomes from the private labor market in June using survey data. "Wage realization (Rs.)" in columns 1-2 is the average daily wage (Rs. = Rupees) an individual received while working for someone else in June 2012. "Reservation wage (Rs.)" in columns 3-4 is the daily wage at which he or she would have been willing to work for someone else in June 2012 (and is available for nearly all respondents and not just those who reported working for a wage). The outcome is elicited through a question in which the surveyor asked the respondent whether he or she would be willing to work for Rs. 20 and increased this amount in increments of Rs. 5 until the respondent answered affirmatively.

Muralidharan et al 2018: Employment

Table 3: Employment (June)

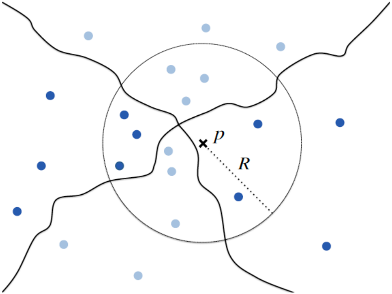
	Days unpaid/idle		Days worked on NREGS		Days worked private sector	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment	-1.2	-1.2	.59	.5	.44	.53
	(.59)	(.59)	(.39)	(.37)	(.57)	(.56)
BL GP Mean		.16		.2		.22
		(.052)		(.04)		(.068)
Adj. R-squared	0.06	0.07	0.03	0.04	0.01	0.02
Control Mean	17	17	3.6	3.6	7.9	7.9
N	14163	14078	18330	18194	14514	14429

Muralidharan et al 2018: Spillovers

- Want to test for spillovers between neighboring mandals, and to estimate a total treatment effect that accounts for them.
- In principle, outomes in each panchayat could be arbitrary functions of the treatment status of *all* other GPs. Need to make some sort of functional form assumptions to make progress.
- Assume that spillovers are a function of the fraction of GPs within a radius R of panchayat p that are treated: N_p^R
- Despite random assignment, N_p^R isn't random, its correlated with location: It's larger for GPs in the middle of the Mandal than near the edges.
- Construct $\tilde{N}_p^R$: fraction of GPs within a radius R of panchayat p that are treated *and* in a different mandal.
  - use $\tilde{N}_p^R$ to test for existence of spillovers and as an instrument

$$Y_{ipmd} = \alpha + \beta \tilde{N}_p^R + \delta District_d + \lambda PC_{md} + \epsilon_{imd}$$

Figure 3: Constructing measures of exposure to spatial spillovers



This figure illustrates the construction of measures of spatial exposure to treatment for a given panchayat p (denoted by the black X symbol) and radius R in a treatment mandal. Dark (light) blue dots represent treatment (control) panchayats; black lines represent mandal borders. As in the text, N_p^R is the fraction of GPs within a radius R of panchayat p which were assigned to treatment. $\tilde{N}_p^R$ is the fraction of GPs within a radius R of panchayat p and within a different mandal (excluding GPs in the same mandal from both the numerator and denominator) which were assigned to treatment. The entire sample of census GP in mandals that were used in randomization are included. In the figure above, these measures are $N_p^R = \frac{5}{11}$ and $\tilde{N}_p^R = \frac{1}{3}$.

(a) Wage (June)

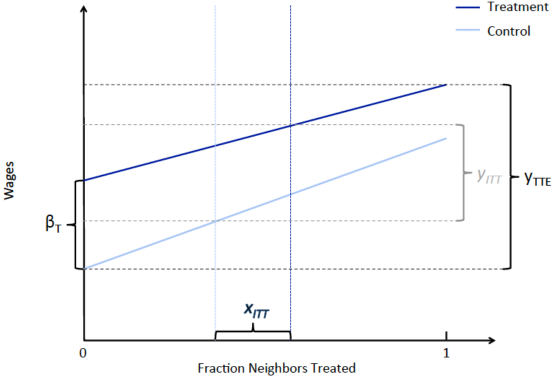
	Wage realization (Rs.)					Reservation wage (Rs.)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	7.8	17	22	21	24	5.5	7.2	8.3	5.7	-.8
	(6.6)	(6.9)	(8.6)	(8.6)	(11)	(4.9)	(5.3)	(6.2)	(7.1)	(7)
Treatment	12	11	14	14	15	3	2.6	3.8	4.1	5.5
	(4.4)	(5.9)	(7.5)	(10)	(12)	(3.1)	(4)	(5.1)	(6.8)	(8.4)
Pooled	9.9	12	13	13	16	3.3	3.4	3.6	3.1	2.9
	(3.6)	(4.8)	(6.2)	(7.8)	(9.5)	(2.9)	(3.5)	(4.4)	(5.6)	(6.4)
F-test for equality	.4	.48	.59	.32	.34	.42	1.2	.76	.067	.86
p-value	.53	.49	.44	.57	.56	.52	.27	.38	.80	.35
N	6560	7049	7192	7245	7269	11614	12498	12732	12818	12852
% of pooled sample	90	97	99	100	100	90	97	99	100	100

(b) Employment (June)

	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Control	1.2	1.3	1.8	2.9	4.3	-.76	-1.5	-1.7	-3.4	-5.4
	(1.1)	(1.2)	(1.6)	(2)	(2.3)	(1.2)	(1.4)	(1.8)	(2)	(2.2)
Treatment	.42	1.3	1.8	2.5	3.3	-1.1	-2.3	-3.1	-3.5	-4.3
	(.8)	(1)	(1.3)	(1.6)	(1.9)	(.81)	(1.1)	(1.3)	(1.5)	(1.8)
Pooled	.81	1.4	1.8	2.5	3.3	-1.1	-2	-2.5	-3.3	-4.2
	(.71)	(.85)	(1.1)	(1.3)	(1.5)	(.72)	(.91)	(1.1)	(1.3)	(1.5)
F-test for equality	.78	.0015	.00014	.071	.32	.15	.58	.95	.003	.33
p-value	.38	.97	.99	.79	.57	.69	.44	.33	.96	.56
N	13008	13995	14300	14397	14441	12722	13689	13977	14064	14095
% of pooled sample	90	97	99	100	100	90	97	99	100	100

Muralidharan et al 2018: Total Treatment Effects

Figure 4: Conceptual illustration of Total Treatment Effect (TTE) after adjusting for spillovers



Muralidharan et al 2018: Estimation

- To estimate the total treatment effect, estimate

$$Y_{ipmd} = \alpha + \beta_T T_m + \beta_N N_p^R + \beta_{TN} T_m \cdot N_p^R + \delta District_d + \lambda PC_{md} + \epsilon_{ipmd}$$

- Then the total treatment effect of a universal rollout is

$$\bar{\beta} = \beta_T + \beta_N + \beta_{TN}$$

- Instrument for N_p^R using $\tilde{N}_p^R$

Table 8: Test of equality between unadjusted and total treatment effect estimates

(a) Wage (June)

	Wage realization (Rupees)					Reservation wage (Rupees)				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	18	23	24	22	26	8.5	9.8	9.9	8.7	7.8
	(5.6)	(6.6)	(7.8)	(9.1)	(11)	(4)	(4.6)	(5.4)	(6.5)	(7.4)
Unadjusted treatment effect	7.6	7.1	7	6.6	6.5	4.3	5	5.1	4.9	4.9
	(3.5)	(3.6)	(3.6)	(3.6)	(3.6)	(3)	(2.9)	(2.9)	(2.9)	(2.9)
Difference	11	15	17	16	19	4.2	4.8	4.7	3.7	3
	(6.6)	(7.5)	(8.6)	(9.8)	(11)	(5)	(5.4)	(6.1)	(7.1)	(7.9)
Chi-square statistic	2.6	4.3	3.8	2.6	2.9	.68	.78	.61	.27	.14
Control Mean	127	128	128	128	128	97	97	97	97	97
N	6560	7049	7192	7245	7269	11614	12498	12732	12818	12852

(b) Employment (June)

	Days worked private sector					Days unpaid/idle				
	(1) R = 10	(2) R = 15	(3) R = 20	(4) R = 25	(5) R = 30	(6) R = 10	(7) R = 15	(8) R = 20	(9) R = 25	(10) R = 30
Total treatment effect	1.6	2.3	2.8	3.5	4.4	-2.6	-3.7	-4.3	-5.1	-6.3
	(1.1)	(1.3)	(1.5)	(1.7)	(1.9)	(1)	(1.3)	(1.5)	(1.6)	(1.8)
Unadjusted treatment effect	.47	.45	.44	.45	.45	-1.4	-1.3	-1.2	-1.3	-1.3
	(.59)	(.57)	(.57)	(.57)	(.57)	(.63)	(.59)	(.59)	(.59)	(.59)
Difference	1.1	1.9	2.4	3.1	3.9	-1.2	-2.5	-3	-3.9	-5
	(1.2)	(1.4)	(1.6)	(1.8)	(2)	(1.2)	(1.4)	(1.6)	(1.7)	(1.9)
Chi-square statistic	.84	1.7	2.1	2.9	3.9	1.1	3.2	3.5	5.1	7.2
Control Mean	7.8	7.9	7.9	7.9	7.9	17	17	17	17	17
N	13008	13995	14300	14397	14441	12677	13640	13928	14015	14046

Papers

- Banerjee et al ID cards
- Muralidharan et al