Welfare Eligibility Manipulation: Evidence From Georgia

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Motivation

- Optimal targeting of social aid is a key issue in the design public policy
    - Developed countries: targeting uses **rich administrative data**
    - Developing countries: **proxy mean tests (PMTs)** are commonly used
- in both cases, optimal design of social aid policies will need to account for **manipulation**
- existing literature **documents the existence** of both demand- and supply-side manipulation
- demand-side:
    - tax evasion [Friedberg, 2000; Saez, 2010; Kleven et al., 2011; Kleven and Waseem, 2013]
    - access to health services [Miller et al., 2013]
    - corrupt local government [Foremny et al., 2017]
- supply-side:
    - teachers changing student grades in high stakes tests [Machin et al., 2020]
    - up-coding in health insurance [Geruso and Layton, 2020]
    - corrupt local government vote-buying [Camacho and Conover, 2011; Brollo et al., 2020]

Today

- We study a welfare programme in Georgia and ask 5 questions:
  1. Given the structure of the programme, how prevalent is **demand-side manipulation**?
  2. What do manipulators **look like**?
  3. How do manipulators **manipulate**?
  4. What are the (down-stream) **consequences** of household manipulation?
  5. How **costly** is manipulation to the government?
- We set up a model of manipulation a la Becker-Ehrlich [Becker, 1968; Ehrlich, 1973] [The model]
- We use a fuzzy difference-in-discontinuities (*FDD*) strategy to answer questions 2 and 4 [Grembi et al., 2016; Millán-Quijano, 2020].
- **Our main contribution:** Use rich data to describe precisely **how** demand-size welfare-manipulation happens and **its consequences**.
- Previous works:
  1. Based inference on counterfactual distributions [Saez, 2010; Chetty et al., 2011; Foremny et al., 2017; Persson, 2020; Zwiers, 2021].
  2. Use proxies to the *"real"* score [Miller et al., 2013; Best et al., 2020].

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The TSA Programme in Georgia

- The **T**argeted **S**ocial **A**ssistance is a nationwide programme that provides monthly unconditional cash transfers to low-income households in Georgia.
- From April 2015 the Programme uses a Proxy Means Test score *(PMT)* to allocate benefits.
  - The PMT uses information about income, assets, and demographic characteristics.
- Households receive monthly transfers according to their score and composition, based on a well-known schedule.

▸ Benefit Schedule

- Households may be reassessed due to:
  - Demographic or economic changes within HHs – **SSA-initiated Reassessment**.
  - If HHs believe PMT score does not adequately represent their level of need – **HH-initiated Reassessment**.

Large benefit discontinuities + possibility of HH-initiated reassessment ⇒ scope for **demand-side manipulation**.

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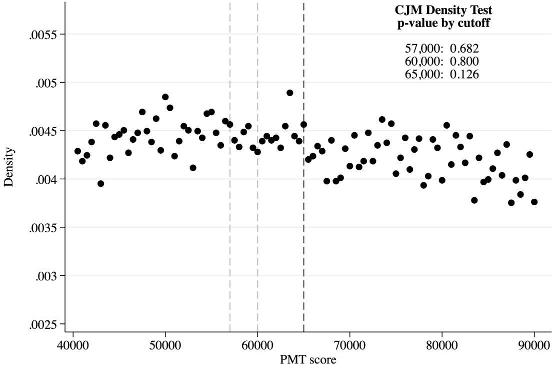
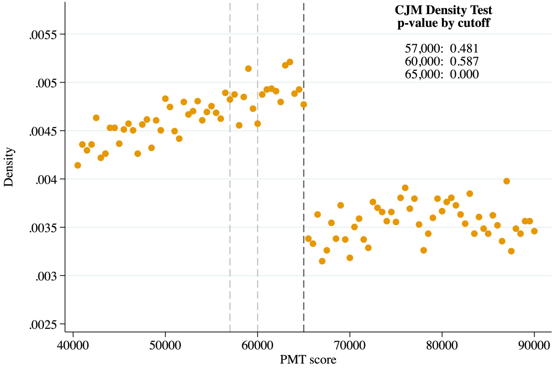
Evidence of Manipulation

PMT Score Distribution: Initial vs Final Scores



(a) First Interview

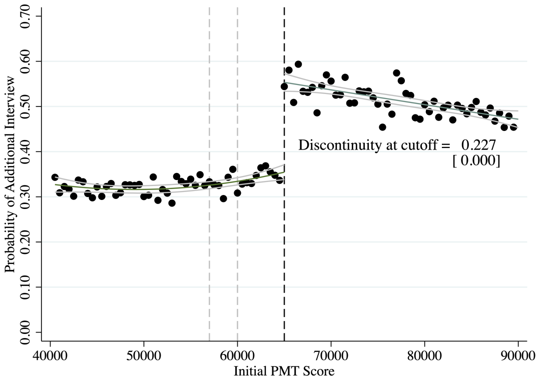


(b) Final Interview

Notes: Bin size of 500. Panel (a) shows the distribution of the PMT score for the first interview each household had. Panel (b) shows the distribution of the last PMT score each household received. The box in both figures contains CJM Density Test *p*-value from the Cattaneo et al. [2020] manipulation test using households with scores between the cutoff above and below each cutoff in the estimation, a polynomial of order 2, and data driven bandwidths, around each cutoff.

The possibility of reassessment $\Rightarrow$ PMT distribution is no longer smooth.

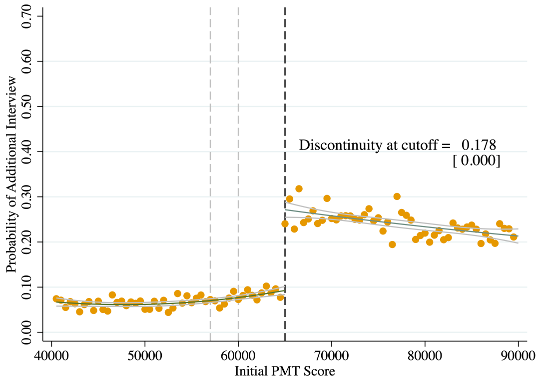
Reassessment Probabilities 1/2



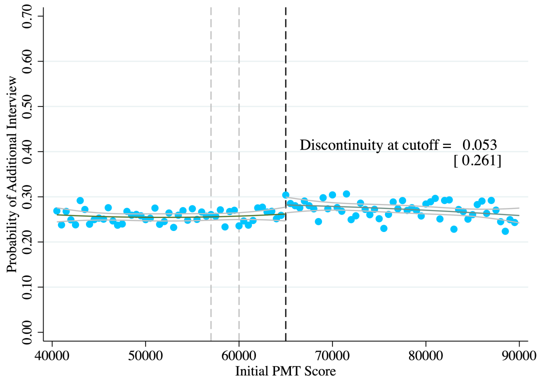
(a) All Reassessments

Notes: Each figure shows the probability of having an additional interview by the first PMT score each household obtained. We include in each figure the resulting RD estimate and p-value in brackets [ ], following Calonico et al. [2014].

Reassessment Probabilities 2/2



(a) Household-Initiated Reassessment



(b) Other Reason for Reassessment

Notes: Each figure shows the probability of having an additional interview by the first PMT score each household obtained. Panel a plots the probability that at least one additional interview was asked by the household. Panel b plot the probability that all PMT reassessments were initiated by the SSA. We include in each figure the resulting RD estimate and p-value in brackets [ ], following Calonico et al. [2014].

We call HH-initiated reassessments "manipulation attempts".

Empirical Approach

Data

- Core data: universe of all PMT interviews [April 2015 – June 2019]
  - Supplement this with the **reason for interview** and additional information regarding benefit removal and sanctions
  - NB: without **reason for interview**, we couldn't say anything about manipulation
- Link in 3 admin datasets:
  - Formal labour market data at 4 points in time [August 2018, February 2019, August 2019, and February 2020]
  - Education data for most recent 3 years [September 2017, September 2018, September 2019]
  - Ministry of Health data on vaccinations
- Conduct a household survey [Autumn 2019]:
  - Labour supply [formal, informal, unpaid]
  - Expenditure
  - Time between parents and children
  - a LOT of other stuff too

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Empirical Specification – *FDD*

Identification

- Y_i is an outcome variable.
- $z_{0,i}$ is the first PMT score a household received (running variable).
- We have two endogenous variables:
  - $R_i = 1$, if the household requested an additional interview (attempt to manipulate).
  - $B_{0,i}$ is the household initial benefit (according to the first PMT score).
- Two instruments:
  - $D_i = 1[z_{i,0} > 57{,}000 \ \& \ A_i = 0]$ or $D_i = 1[z_{i,0} > 65{,}000 \ \& \ A_i = 1]$, where $A_i = 1[60{,}000 < z_{0,i} \leq 70{,}000]$
  - $A_i \times D_i \implies$ **is the *FDD* indicator.**
- $g_1^{D,A}(z_{0,i})$ is a function of $z_{0,i}$ above and below each cutoff.
- X_i is a large set of HH characteristics, as well as a series of FEs.

$$R_i = \omega_1 D_i + \omega_2 A_i + \omega_3 A_i \times D_i + g_R^{D,A}(z_{0,i}) + X_i^{'} \omega + \mu_{R,i} \tag{1a}$$

$$B_{0,i} = \gamma_1 D_i + \gamma_2 A_i + \gamma_3 A_i \times D_i + g_B^{D,A}(z_{0,i}) + X_i^{'} \gamma + \mu_{B,i} \tag{1b}$$

$$Y_i = \theta_{\mathbf{R}} R_i + \theta_B B_{0,i} + \theta_3 A_i + g_Y^{D,A}(z_{0,i}) + X_i^{'} \theta + \mu_{Y,i} \tag{1c}$$

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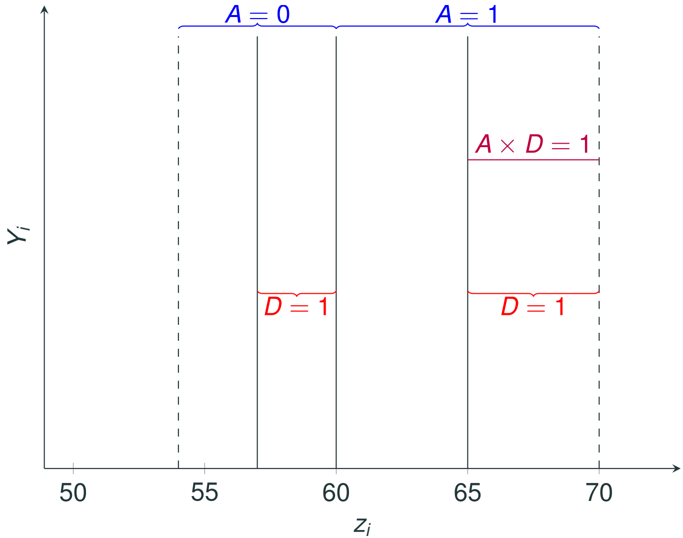
Our focus - sample selection for our *FDD* estimations

Identification



- We use households with $z_i \in [54{,}000,\ 70{,}000]$
- Additional filters:
  - Excluded HH whose first interview was after 2018.
  - Excluded HHs with only SA-initiated interviews.
- The idea behind *FDD*:
  - We use changes around 57,000 to account for the effects driven by B_0.
  - The remaining differences around 65,000 represent the effect of manipulation attempts.

Identification

In order to identify θ_R we require the following assumptions to hold:

1. our running variable is continuous through the key cutoff of interest [No Bunching]
2. that expected potential outcomes are smooth through the cutoff
   A. the continuity of household characteristics through the cutoff [X Balance]
   B. the smooth evolution of key policy parameters through the cutoff [Policy Parameters Balance]
3. The additional *FDD* assumptions are:
   A. R_i is only discontinuous when $A_i = 1$ [R_i]
   B. the effect of B_0 is homogeneous over the cutoffs [dY/dB_0]
4. We also show that:
   A. the first stage for R is homogeneous over different populations [FS - R]
   B. the first stage on B_0 takes into account differences in the size of the change in benefits [FS - B_0]

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Core Results

Summary of the results

We still have 3 questions to answer:

2. Who are the manipulators? [Compliers]
    - They are marginally poorer and are more likely to be rural
3. How do they manipulate? [ΔX]
    - We observe reductions in movable agricultural assets, reported salaries, and house size.
4. What are the effects of manipulation attempts?
    - Female labor market participation increases [LM - Ind, LM - HH]
        - The effect is stronger if the manipulation attempt is unsuccessful. [By success]

          $\Rightarrow$ successful manipulation **crowds out** LM participation in relative sense
    - Expenditure in children increases. [Expenditure]

Additional question:

5. What is impact on kids? Relates to “parental time (↓) vs money (↑)” literature [Caucutt et al., 2020; Agostinelli and Sorrenti, 2021; Nicoletti et al., 2023; Mullins, 2022, ...].
    - We do not find effects on early childhood [0 to 5], or on older children and youths [Education].

Wrapping up

Wrapping up

We formulated 5 questions:

1. Given the structure of the programme, do we see **demand-side manipulation**?
   - We document substantial demand-side manipulation
   - Our manipulation estimates align with estimations using bunching techniques [Bunching]
2. What do manipulators **look like**?
   - Complier households are poorer and rural
3. How do manipulators **manipulate**?
   - Households take advantage of the possibility for reassessments.
   - Households change movable rural assets and declared income.
4. What are the (down-stream) **consequences** of household manipulation?
   - increased engagement in formal LM for women, though only when manipulation is not successful
   - increase in HH spending almost exclusively on children
   - HH has more money, mom works more – we dont find significant impact on kids
5. How **costly** is manipulation to the government?
   - Manipulation has a considerable financial cost to the Government [Cost]

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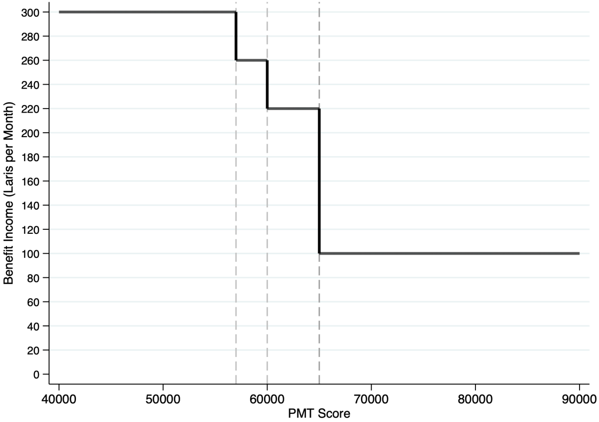
Supporting slides

Georgia

- **Georgia:**
  - 1 USD = 2.95 GEL (in December 2019)
  - Median income = USD 370
  - Population = 3.7 million
  - Poverty = 17.1%
- **TSA:**
  - Annual budget around 270 Million Lari per year (9% of the social security budget).
  - Reaches 12% of the population.
  - The transfer represents on average 31% of the total income of its beneficiaries.

Full TSA Benefit Schedule

Institutional Context
Welfare Cost



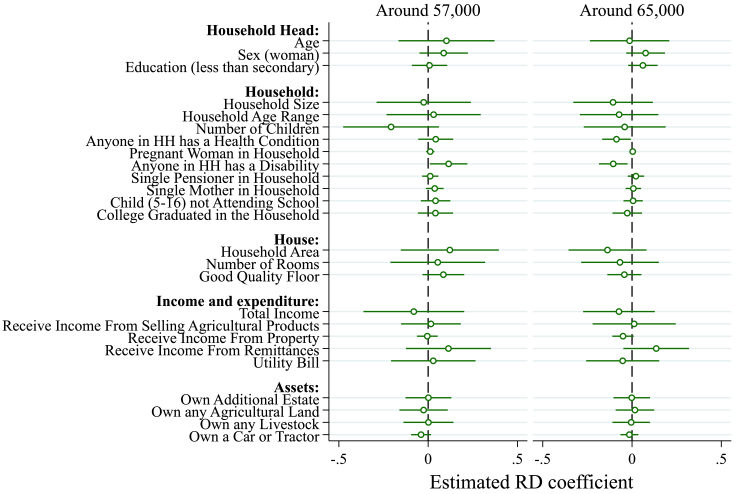
PMT score	Benefit per household member	Benefit per child
0 to 30,000	60	50
30,001 to 57,000	50	50
57,001 to 60,000	40	50
60,001 to 65,000	30	50
65,001 to 100,000	0	50
100,000 or more	0	0

Notes: Benefit Income-PMT score schedule for the sample median household structure of two adults, two children.

Identification: HH Characteristics are Balanced Across Threshold

Identification

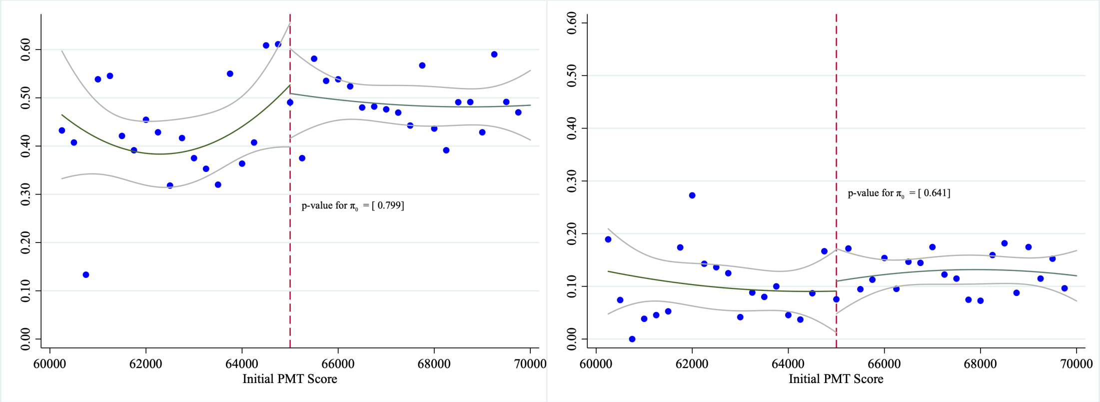
Figure 4: Covariate Balance



Notes: We present a series of estimates for λ from: $X_i = \lambda D_i + g_1^D(z_{0,i}) + \upsilon_i$

▸ Identification

Figure 5: Policy parameters balance

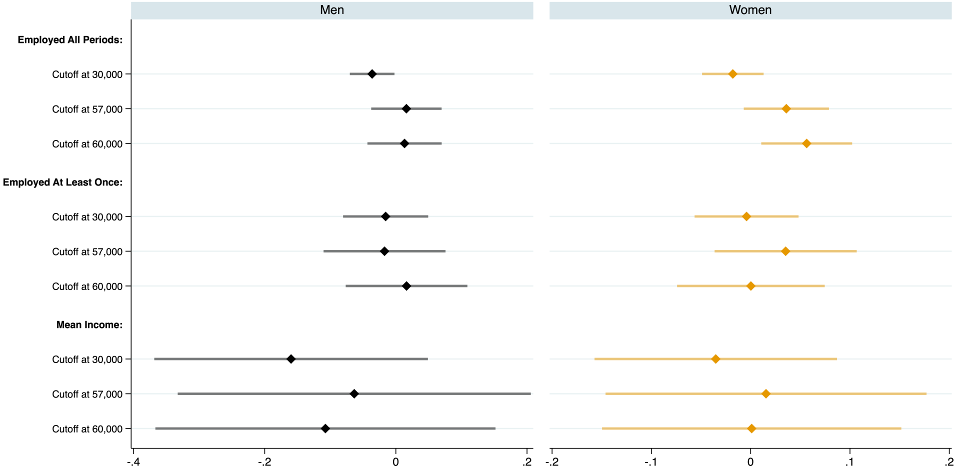


(a) p (b) q

Notes: In each graph we present the respective p-value for the parameter π_0 from a regression of the form $y_i = \pi_0 D_i + g_1^D(z_{0,i}) + \epsilon_i$, where $D_i = \mathbb{1}[z_{0,i} > 65,000]$ and $g_1^D(z_{0,i})$ is a polynomial of order 2 in $z_{0,i}$ above and below the cutoff.

Identification: The effect of B_0 is homogeneous

Figure 7: The Effect of Benefit Income on Labor Market Outcomes is Homogeneous Across Cutoffs

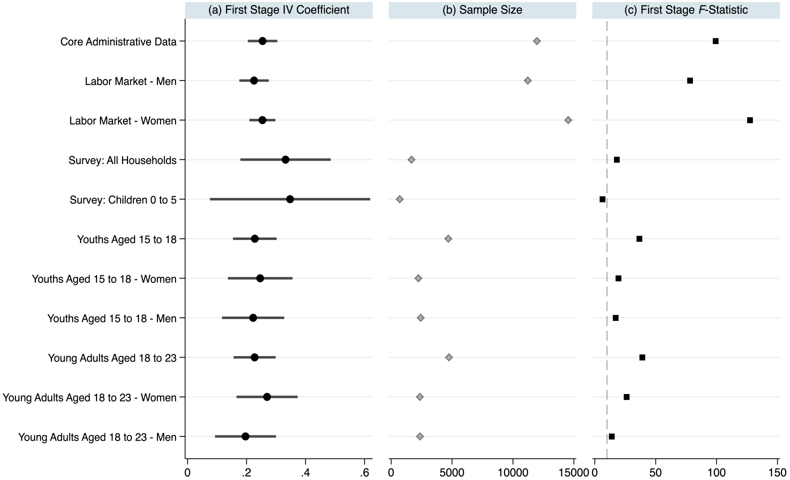


Notes: We plot the coefficient of the RD-IV coefficient of the effect of B_0 on different outcomes for each cutoff.

First Stage Estimates, Sample Size and Instrument Relevance

Identification

Figure 8: First stage coefficients, Sample size, and F-Tests for the effect of $A \times D$ on R

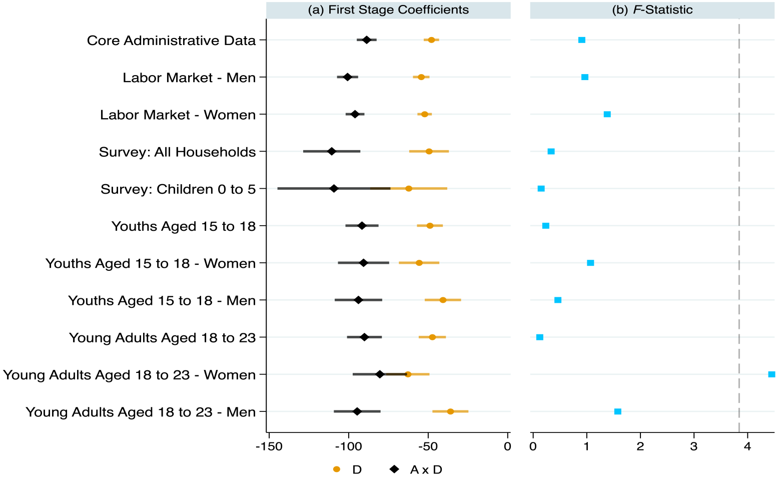


Notes: We plot the first stage coefficient of $A \times D$ for the endogenous variable R for different data sets we use in the paper. Dashed line at 10

First Stage Estimates for B_0

Identification

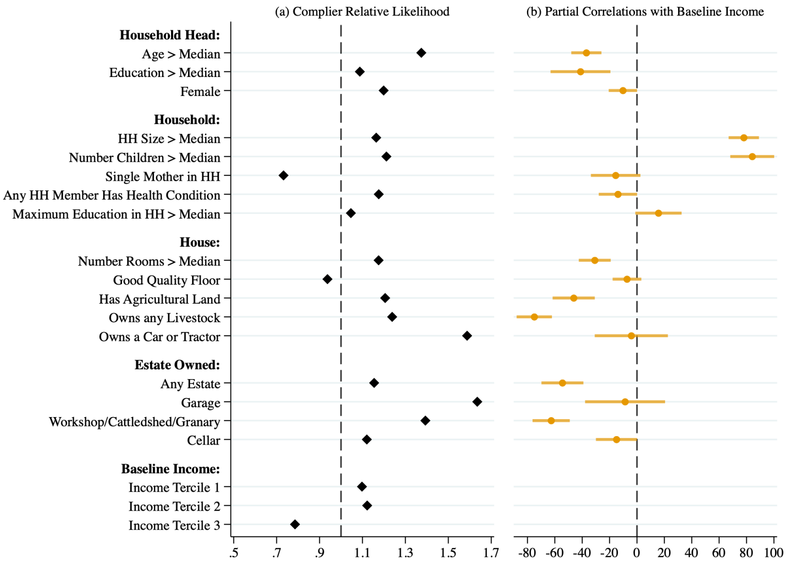
Figure 9: First stage coefficients and F-Tests for the difference of the effect of D and $A \times D$ on B_0



Notes: In Panel A we plot the first stage coefficients of D and $A \times D$ for the endogenous variable B_0 for different data sets we use in the paper. Panel B represents the F test for the null hypothesis $H_0 : \gamma_1 + \gamma_3 = 3\gamma_1$ following equation 1. Dashed line at 1.96^2

Who are the *manipulators* (IV Compliers)?

▸ Characterising Compliers ▸ Results

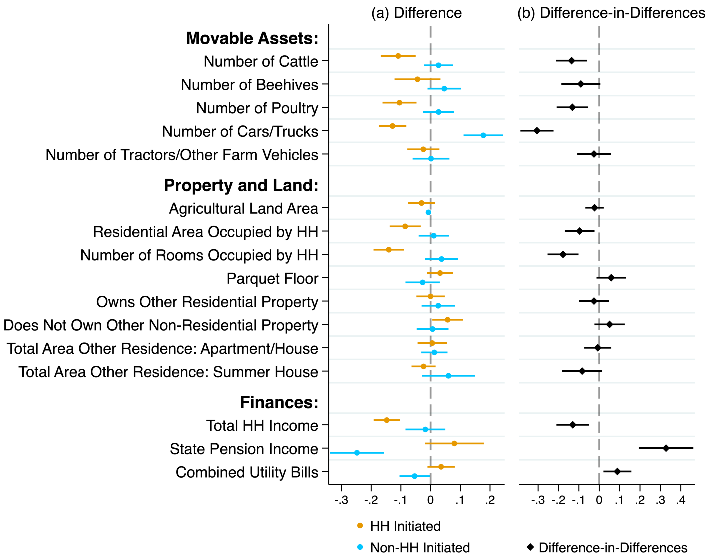


Manipulators are:

- marginally poorer.
- more likely to have movable (agricultural) assets.

How do *manipulators manipulate*?

Results



Manipulators reduce:

- movable agricultural assets
- living space
- salaries

Formal Labour Market – Admin Data I

Results

Table 1: Welfare Eligibility Manipulation Leads to Increased Formal Labor Market Engagement for Women

	(1)	(2)	(3)	(4)	(5)	(6)
	Men			**Women**		
	Employed At Least Once	Employed All Periods	Mean Income	Employed At Least Once	Employed All Periods	Mean Income
OLS						
Repeat Interview	–0.027**	0.009	–24.637***	–0.011	0.004	–2.920
	(0.012)	(0.007)	(7.264)	(0.010)	(0.006)	(3.348)
CW-OLS						
Repeat Interview	–0.010	0.012	–14.898*	–0.009	0.007	–1.344
	(0.013)	(0.008)	(7.774)	(0.010)	(0.006)	(3.578)
2SLS						
Repeat Interview	–0.112	0.202	–127.498	0.312*	0.257**	64.521
	(0.311)	(0.186)	(186.933)	(0.181)	(0.110)	(75.602)
SW F-Statistic: R.I.	20.435	20.435	20.435	44.620	44.620	44.620
$\overline{Y}_0$	0.256	0.072	105.716	0.179	0.052	43.963
Observations	11,220	11,220	11,220	14,544	14,544	14,544

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Initial benefit is measured in 100s of Laris.

Manipulation attempts lead to an increase in female labor market participation.

Table 2: Household Level Analysis of Labor Market Engagement Reflects What we Find at the Individual Level

	(1)	(2)	(3)	(4)	(5)
	At Least One Adult Employed at Least Once	All Adults Employed at Least Once	At Least One Adult Employed All Periods	All Adults Employed All Periods	Mean Labor Income of Household
OLS					
Repeat Interview	–0.069***	0.009	–0.009	0.007	–55.305***
	(0.012)	(0.007)	(0.009)	(0.004)	(8.805)
CW-OLS					
Repeat Interview	–0.064***	0.014*	–0.004	0.012***	–51.457***
	(0.014)	(0.008)	(0.010)	(0.005)	(9.660)
2SLS					
Repeat Interview	0.131	0.032	0.439**	0.023	26.746
	(0.269)	(0.139)	(0.202)	(0.057)	(194.333)
SW F-Stat: Repeat Interview	30.216	30.216	30.216	30.216	30.216
$\overline{Y}_0$	0.371	0.074	0.130	0.021	156.997
Observations	11,695	11,695	11,695	11,695	11,695

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Initial benefit is measured in 100s of Laris.

Total income does not significantly increase at the household level.

Formal Labour Market – Admin Data III

Results Success II

Table 3: Successful Welfare Manipulation Attempts Crowd out Female Labor Force Participation

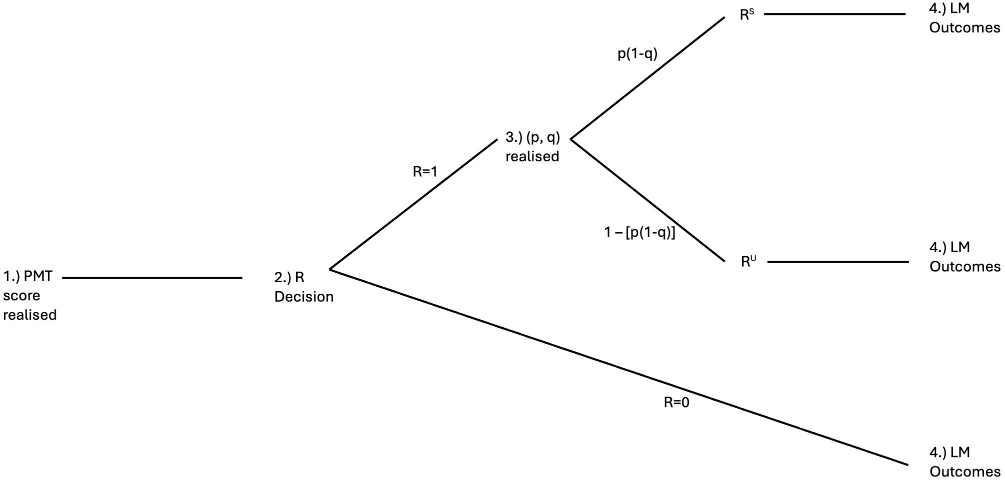
	(1)	(2)	(3)	(4)	(5)	(6)
	Men			**Women**		
	Employed At Least Once	Employed All Periods	Mean Income	Employed At Least Once	Employed All Periods	Mean Income
(a) Unsuccessful Manipulation Attempts						
2SLS						
Repeat Interview	–0.046	0.416	–38.626	0.468*	0.375**	119.573
	(0.547)	(0.342)	(328.086)	(0.255)	(0.156)	(106.478)
SW *F*-Statistic: R.I.	9.281	9.281	9.281	33.717	33.717	33.717
$\overline{Y}_0$	0.261	0.071	110.120	0.183	0.055	47.250
Observations	10,434	10,434	10,434	13,485	13,485	13,485
(b) Successful Manipulation Attempts						
2SLS						
Repeat Interview	–0.200	0.343	–134.008	0.316	0.296	8.906
	(0.441)	(0.264)	(264.137)	(0.358)	(0.210)	(147.866)
SW *F*-Statistic: R.I.	27.158	27.158	27.158	27.701	27.701	27.701
$\overline{Y}_0$	0.261	0.071	110.120	0.183	0.055	47.250
Observations	10,130	10,130	10,130	13,244	13,244	13,244

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%.

*Successful manipulation **crowds out** LM participation in relative sense*

Thinking More About Success of Manipulations

Results
Success I



Expenditure – Survey Data

Results

Table 5: Welfare Eligibility Manipulation Attempts Lead to Significant Increases in Child-Related Expenditure

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
						Children			
	Total	Food	Food Outside of House	Alcohol Tobacco	Adult Clothing	Total	Clothing	Education	Childcare
OLS									
Repeat Interview	–40.523	–11.749	–0.647**	–3.983	–3.041***	–2.133	0.440	–1.848*	–0.725
	(33.057)	(9.162)	(0.293)	(2.689)	(0.811)	(2.423)	(1.878)	(1.068)	(0.494)
CW-OLS									
Repeat Interview	–40.229	–12.754	–0.716**	–3.906	–3.091***	–2.785	0.267	–2.267*	–0.784
	(35.897)	(9.584)	(0.301)	(2.776)	(0.839)	(2.538)	(1.925)	(1.176)	(0.539)
2SLS									
Repeat Interview	96.333	–49.255	0.516	–51.534	0.064	87.949*	55.881*	25.879	6.188
	(330.187)	(139.620)	(4.691)	(50.765)	(12.353)	(47.617)	(31.343)	(17.387)	(9.904)
SW *F*-Statistic: R.I.	8.771	8.771	8.771	8.771	8.771	8.771	8.771	8.771	8.771
$\overline{Y}_0$	422.331	142.361	0.667	17.681	5.916	30.909	23.806	6.187	0.915
Observations	1,670	1,670	1,670	1,670	1,670	1,670	1,670	1,670	1,670

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. Each column summarizes the results for the respective outcome variable following the system of equations **??** and **??** using information from the household survey. All estimations control for the first PMT first score above and below the cutoff, first monthly household benefit awarded, , and region-by-quarter and interview time fixed effects. The CW-OLS calculation follows Bhuller et al. [2020].

Manipulation attempts lead to higher expenditure on children.

Early Childhood Investments

Results

Table 6: There is no Strong Evidence of a Reduced Form Effect for Early Childhood Investments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Administrative Data		**Survey Data**				
	Full Vaccines	Full exc. DTaP/ IPV/ Hib/ HepB	Any Health Check-ups	Number of Health Check-ups	Screen-time	Time Spent Together – Total	Time Spent Together – Reading
OLS							
Repeat Interview	–0.014	–0.029	–0.047	–0.170	–5.533	–7.714	1.023
	(0.020)	(0.022)	(0.046)	(0.450)	(6.069)	(5.906)	(2.088)
CW-OLS							
Repeat Interview	–0.019	–0.029	–0.051	–0.415	–4.305	–10.343*	0.190
	(0.023)	(0.025)	(0.047)	(0.461)	(6.146)	(5.868)	(2.060)
2SLS							
Repeat Interview	0.015	–0.190	0.567	3.445	–20.803	7.604	–4.377
	(0.327)	(0.395)	(0.557)	(5.628)	(83.031)	(74.238)	(28.642)
SW F-Stat: Repeat Interview	12.390	12.390	3.554	3.554	3.554	3.554	3.554
$\overline{Y}_0$	0.197	0.281	0.805	5.252	64.299	74.119	15.678
Observations	3,148	3,148	701	701	701	701	701

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%.

We find no effects on younger children.

Mid- and Late-Period Childhood Skill Investments

Results

Table 7: Household Manipulation Attempts Lead to no Changes in High School Attendance or in Unversity Attendance

	(1)	(2)	(3)	(4)	(5)	(6)
	High School			University		
	All	Males	Females	All	Males	Females
OLS						
Repeat Interview	–0.007	–0.017	0.002	–0.033*	–0.039	–0.031
	(0.014)	(0.022)	(0.019)	(0.020)	(0.028)	(0.032)
CW-OLS						
Repeat Interview	–0.003	–0.013	0.018	–0.033	–0.033	–0.030
	(0.015)	(0.024)	(0.020)	(0.022)	(0.030)	(0.035)
2SLS						
Repeat Interview	0.248	0.299	0.312	–0.045	–0.724	0.318
	(0.361)	(0.581)	(0.427)	(0.342)	(0.688)	(0.393)
SW F-Stat: Repeat Interview	15.630	7.229	9.046	23.614	6.055	20.969
$\overline{Y}_0$	0.810	0.776	0.846	0.287	0.221	0.354
Observations	6,764	3,502	3,251	4,749	2,366	2,357

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%.

We find no effects on older children.

Characterising Compliers

- We characterize compliers by calculating the ratio of the first stage coefficient on the instrument for each binary (or binarized) characteristic to the overall first stage coefficient.
- By Bayes' rule this ratio of first stage estimates yields the complier relative likelihood of a given characteristic:

$$\text{FS Ratio} = \frac{P[R_{1i} > R_{0i} \mid x_{1i} = 1]}{P[R_{1i} > R_{0i}]} = \frac{P[x_{1i} = 1 \mid R_{1i} > R_{0i}]}{P[x_{1i} = 1]} = \text{Complier Rel. Likelihood}$$

Complier reweighted OLS – step-by-step

- predict the treatment variable with all key exogenous variables, and split the sample into 5 quintiles based on the predicted treatment index.
- For each quintile, we run a separate first stage equation, in order to estimate the proportion of compliers in each quintile.
  - The mechanics are the same as when we characterise compliers
- then re-weight our core sample so that the proportion of compliers in each quintile matches the proportion of the estimation sample for the quintile.
  - quintiles with few compliers will receive lower weights, and quintiles with a higher proportion of compliers will received higher weights.

Bunching style estimations

Summary

Table 8: Existing Bunching Estimates are Consistent With Our FDD Approach

	(1)
[1] **First Generation Bunching Approach**	0.133
à la Chetty et al. [2011]; Foremny et al. [2017]	[0.050]
[2] **Second Generation Bunching Approach**	0.114
à la Zwiers [2021]	[0.031]
[3*a*] **FDD Approach**: Welfare Manipulation Attempt	0.254
	(0.026)
[3*b*] **FDD Approach**: Successful Welfare Manipulation Attempt	0.117
	(0.018)

If we estimate the size of the bunching (manipulation) using literature's methods, we find estimates consistent with our estimations of manipulation attempts.

The cost of welfare manipulation

TSA Schedule | Summary

Table 9: The Direct Costs Associated With Manipulation Are Substantial

(a) The Cost of Manipulation	
Additional Benefit Payments	
[1] Additional Households at Higher Benefit Level	403
[2] Additional Monthly Benefit Payments (Lari)	106,150
Additional Interview Costs	
[3] Additional Visits to Households	38
[4] Additional Visit Costs	3,116
[5] Total Additional Costs (Lari) Due to Manipulation	109,266
(b) Baseline Costs as a Benchmark	
[6] Number of Households at Baseline	4,854
[7] Total Baseline Benefit Costs (Lari)	441,800
[8] **Cost of Manipulation as a Percentage of Baseline Costs**	**24.73%**

Without manipulation the Government could have increased benefits to households above 65,000 by 25%, reducing the incentives to manipulate.

Welfare Eligibility Manipulation

An individual will choose to engage in welfare eligibility manipulation if the expected value of manipulation (V_R) exceeds that of accepting their initial benefit level (V_A):

$$E(V_R) > E(V_A). \tag{2}$$

When requesting a repeat interview, the individual may receive a higher benefit level B^+ with exogenously determined probability p, or may receive the same benefit level as their initial allocation B^0. The cost of requesting a repeat interview is C. Then:

$$E(V_R) = p(1-q)U(B^+) + (1-p)(1-q)U(B^0) + qU(B^-) - C. \tag{3}$$

$$E(V_A) = U(B^0). \tag{4}$$

Rearranging yields, we know that a given household will request a reassessment if:

$$p(1-q)\Delta U^+ - q\Delta U^- > C. \tag{5}$$

Identification in FDD

We start from Equation 1c:

$$Y_i = \theta_R R_i + \theta_B B_{0,i} + \theta_3 A_i + g_Y^{D,A}(z_{0,i}) + X_i^{'}\theta + \mu_{Y,i}$$

We take expectations of Y with respect to our instruments $(D, A \times D)$, we can identify θ_B by subtracting $Y^{10} - Y^{00}$:

$$\theta_B = \frac{Y^{10} - Y^{00}}{B_0^{10} - B_0^{00}}$$

Now, using variation around $65,000$, by subtracting $Y^{11} - Y^{01}$:

$$Y^{11} - Y^{01} = \theta_R(R^{11} - R^{01}) + \theta_B(B^{11} - B^{01})$$

Under the assumption that the effect of 1 additional Lari is the same around $57,000$ and $65,000$, after reorganizing, we get:

$$\theta_R = \frac{(Y^{11} - Y^{01}) - (Y^{10} - Y^{00}) \times \left(\frac{B_0^{11} - B_0^{01}}{B_0^{10} - B_0^{00}}\right)}{R^{11} - R^{01}}$$