What You See and What You Get: Direct and Indirect Political Dividends of Public Policies

— Online Appendix[1] —

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[1]Parts of this online appendix may overlap or partly reproduce background information from other manuscripts currently under review or in print that use the same datasets.

Appendix A Summary Statistics

Table A1 shows information about the population and sample for each survey. Tables A3 and A2, that follow, present summary statistics for the main variables in each survey. The complete question wording is reported in Appendix B.

As A1 shows, each survey included two separate lotteries taken a few weeks apart. Many individuals who participated in one also participated in the other. Moreover, the number of units being assigned in each lottery varied, and so do the number of participants. Hence, probability of selection varied across lotteries too. In order to account for these irregularities we include sampling weights in our analyses of the recent lotteries survey to account for endogeneity induced by our sampling process. Treatment assignment takes place at the lottery level, which means that, conditional on applying for lottery j, winning a lottery j is randomly determined. However, in our recent-lotteries survey (which was the first of the two surveys that we carried out), instead of sampling at the lottery-level, we selected respondents based on ever winning (not ever winning) any lottery of the two lotteries from which we were sampling rather sampling on winning (not winning) a given lottery j. This could lead to endogeneity because ever winning (not winning) depends on a series of unobserved factors (date in which subjects signed up, bureaucractic selection of individuals included in lotteries roll, etc.) that could affect being included in a lottery roll. Since we sample all winners, all winners get a weight of 1 and all non-winners get a weight of 1/probability of sampling a non-winner.

Table A1: Lotteries Included in the Surveys

	Early Lotteries Survey 2019-12 – 2020-07		Recent Lotteries Survey 2017-05 – 2018-01	
Edital	03/2011	06/2011	17.2016	20.2016
Lottery Date	2011-11-06	2011-08-13	2016-10-19	2016-11-05
Non-Winners	295,235	318,789	580,983	484,151
Winners	2,983	6,505	2,299	612
Delivery Dates	2011-12 – 2012-11		2017-12 – 2018-12	
Pre-Sample	22,157		8,032	
Contacted	3,772		1,283	
Interviewed	2,119		795	

Notes: Early lotteries were for the Park Imperial, Park Royal, Destri, Toledo, Rio Bonito, Estoril, Sevilha, Taroni, and Cascais housing projects, while the late lotteries included units in the Vila Carioca, Safira, Porto Fino, and Ametista projects. "Contacted" is defined as whether the selected person was found by our field team.

Table A2: Summary Statistics Recent (2016) Lotteries Survey

	N	Min	Mean	Median	Std. Dev.	Max	Missing
Win. Lottery‡	1267	0.00	0.23	0.00	0.42	1.00	0
Compliance‡	1267	0.00	0.21	0.00	0.41	1.00	0
Age	1267	22.28	47.07	46.13	13.25	85.55	0
Sex (male)	1267	0.00	0.30	0.00	0.46	1.00	0
Race (white)	1267	0.00	0.25	0.00	0.44	1.00	0
Religion (any)	1267	0.00	0.85	1.00	0.36	1.00	0
Children (N)	1267	0.00	0.90	1.00	1.24	11.00	0
Schooling (years)	1267	0.00	9.01	11.00	4.14	15.00	0
Registry (in CadUnico)†	1267	0.00	0.16	0.00	0.37	1.00	0
Yrs. Formal Emplymt†	1267	0.00	2.61	3.00	2.20	5.00	0
Av. Earnings Formal Emplymt†	1267	0.00	4.35	6.28	3.23	9.13	0
PT Evaluation Index	1267	-1.50	0.00	-0.05	1.00	3.54	0
Incumbent at Treatment Evaluation Index	1211	-1.62	0.04	-0.07	1.03	2.78	56
Attribution to Lula/Dilma/PT	753	0.00	0.79	1.00	0.40	1.00	514
Attribution to Paes/Pezão/Temer/MDB	753	0.00	0.07	0.00	0.26	1.00	514
Lula Evaluation	1197	-2.00	0.19	0.00	1.24	2.00	70
Dilma Evaluation	1195	-2.00	-0.30	0.00	1.20	2.00	72
PT Partisanship	1267	0.00	0.05	0.00	0.21	1.00	0
Paes Evaluation	1192	-2.00	-0.35	0.00	1.17	2.00	75
Pezão Evaluation	1195	-2.00	-1.35	-2.00	0.87	2.00	72
Temer Evaluation	1166	-2.00	-1.19	-1.00	0.89	2.00	101
Vote for Pedro Paulo	725	0.00	0.03	0.00	0.18	1.00	542

Notes: For items derived directly from a single survey question we report the raw values of the survey answers. In the analyses reported in the paper, we standardized all main outcomes there were already not standardized by construction in order to facilitate presentation and interpretation. In all cases, standardization was performed such that the mean and variance in the control group correspond to zero and one. †Age was measured using data from private vendors, employment and earnings from employment was measured using RAIS data, and Registry was measured using CadUnico data.‡See Appendix G for a discussion of compliance measures and winning the lottery was obtained by the researchers by digitizing lottery rolls and results from public sources.

Table A3: Summary Statistics (2011) Lotteries Survey

	N	Min.	Mean	Med.	Std Dev.	Max	Missing
Win. Lottery‡	3923	0.00	0.23	0.00	0.42	1.00	0
Compliance‡	3923	0.00	0.12	0.00	0.32	1.00	0
Age†	3923	27.59	49.46	47.64	11.33	87.85	0
Sex (male)	3923	0.00	0.38	0.00	0.49	1.00	0
Race (white)	3923	0.00	0.24	0.00	0.43	1.00	0
Registry (in CadUnico)†	3923	0.00	0.16	0.00	0.37	1.00	0
Yrs. Formal Emplymt†	3923	0.00	4.43	5.00	3.17	8.00	0
Av. Earnings Formal Emplymt†	3923	0.00	4.68	5.92	2.67	8.67	0
PT Eval. Index	3923	-1.20	0.00	-0.16	1.00	3.33	0
Lula Eval.	3782	-2.00	0.12	1.00	1.41	2.00	141
Dilma Eval.	3770	-2.00	-0.70	-1.00	1.24	2.00	153
Partisanship PT	3923	0.00	0.06	0.00	0.24	1.00	0
Vote PT 2018	2886	0.00	0.22	0.00	0.42	1.00	1037
Paes Eval.	3700	-2.00	0.02	0.00	1.29	2.00	223
Vote for Inc. 2016	2088	0.00	0.02	0.00	0.14	1.00	1836
Vote for Paes 2020	3190	0.00	0.48	0.00	0.50	1.00	734
Temer Eval.	3674	-2.00	-1.13	-1.00	0.99	2.00	250
Bolso. Eval.	3716	-2.00	-0.08	0.00	1.40	2.00	207
Inc. at Treat. Eval. Index	3833	-1.69	0.01	-0.14	1.01	2.41	90
Curr. Inc. Eval Index	3840	-1.82	0.00	0.02	1.00	2.75	83
Crivella Eval.	3717	-2.00	-0.80	-1.00	1.24	2.00	206
Witzel Eval.	3629	-2.00	-0.31	0.00	1.23	2.00	294
Mobilization Index	3906	-3.26	-0.00	0.15	0.99	2.38	17
Turnout 2016	3856	0.00	0.83	1.00	0.38	1.00	67
Turnout 2018	3870	0.00	0.87	1.00	0.34	1.00	53
Exp. Turnout 2020	3411	0.00	0.93	1.00	0.25	1.00	512
Support Cand.	3881	0.00	0.14	0.00	0.34	1.00	42
Talk to Cand.	3883	0.00	0.09	0.00	0.29	1.00	40
Attrib. to Lula/Dilma/PT	3903	0.00	0.42	0.00	0.49	1.00	20
Attrib. to Paes/Pezão/Temer/MDB	3903	0.00	0.01	0.00	0.12	1.00	20

Notes: For items derived directly from a single survey question we report the raw values of the survey answers. In the analyses reported in the paper, we standardized all main outcomes there were already not standardized by construction in order to facilitate presentation and interpretation. In all cases, standardization was performed such that the mean and variance in the control group correspond to zero and one. †Age was measured using data from private vendors, employment and earnings from employment was measured using RAIS data, and Registry was measured using CadUnico data.‡See Appendix G for a discussion of compliance measures and winning the lottery was obtained by the researchers by digitizing lottery rolls and results from public sources.

Appendix B   Survey Question Wording

The outcomes in the survey analysis were based on the following survey questions. If there are differences in the question wording between surveys, we note the survey in square brackets. English translations by the authors in brackets.

Politicians' Evaluations

- Lula evaluation: E como avalia o trabalho que o ex-presidente Lula realizou durante seus mandatos?[And how do you evaluate the work that former president Lula carried out during his presidency?]
- Temer evaluation: E como avalia o trabalho que o atual presidente Michel Temer está realizando [recent lotteries survey]? [And how do you evaluate the work that president Temer is carrying out?] E como avalia o trabalho que o ex-presidente Michel Temer realizou durante seu mandato [early lotteries survey]? [And how do you evaluate the work that former president Temer carried out during his presidency?]
- Dilma evaluation: Como você avalia o trabalho que a ex-presidente Dilma Rousseff realizou durante os seus mandatos? [And how do you evaluate the work that former president Dilma Rousseff carried out during his presidency?]
- Paes evaluation: E como avalia o trabalho que o ex-prefeito do Rio de Janeiro, Eduardo Paes, realizou durante seus mandatos?[And how do you evaluate the work that former Mayor Eduardo Paes carried out as a mayor?]
- Pezão evaluation: E como avalia o trabalho que o governador do Rio de Janeiro, Pezão, está realizando durante seu mandato [recent lotteries survey]? [And how do you evaluate the work that governor Pezão is carrying out?] E como avalia o trabalho que ex-governador do Rio de Janeiro, Luiz Fernando Pezão realizou durante seu mandato [early lotteries survey]? [And how do you evaluate the work that former governor Pezão carryed out as a governor?]
- Bolsonaro evaluation: Como você avalia o trabalho que o presidente Jair Bolsonaro está realizando? [And how do you evaluate the work that president Jair Bolsonaro is carrying out?]
- Crivella evaluation: Pesando agora na prefeitura da cidade do Rio, como avalia o trabalho que o Prefeito Marcelo Crivella está realizando? [Thinking about Rio de Janeiro, how do you evaluate the work that mayor Crivella is carrying out as a mayor?]
- Witzel evaluation: E como avalia o trabalho que o governador do Rio de Janeiro, Wilson Witzel, está realizando durante seu mandato? [And how do you evaluate the work that governor Witzel is carrying out?]

Party ID (Workers' Party ID)

- Você simpatiza com algum partido político? Sim, Não [Do you like any party? Yes/No]
- Com qual partido você simpatiza? [Which party do you like?] [Spontaneous: coded from a list of parties]

Vote choice

- Vote for Haddad in 2018: Em quem você votou para presidente no segundo turno? Coded from the list of candidates plus null/blank. [Who did you vote for president in the runoff?]
- Vote for Paes in 2020: Neste ano haverá nova eleição para prefeito. Imagine que os candidatos fossem o atual prefeito Marcelo Crivella e o ex-prefeito Eduardo Paes, em quem você votaria? [This year there will be new elections for mayor. Imagine that the candidates are the current mayor Marcelo Crivella and former mayor Eduardo Paes. Who would you vote for?]
- Vote Pedro Paulo in 2016: Em quem você votou para prefeito do Rio no primeiro turno? [Who did you vote for mayor of Rio in the first round?]

Mobilization

- Support a Candidate: Nas eleições de 2018, você declarou apoio a algum candidato? Por exemplo, colocou cartaz ou faixa na sua casa, adesivo na roupa, circulou mensagens de apoio no whatsapp, no facebook, foi em eventos de campanha? [In the 2018 elections, did you declare support to any candidate. For instance, did you have banners at home, did you wear stickers, circulated support messages on WhatsApp, Facebook, did you attend campaign events?]
- Talk to Candidate: Nas eleições de 2018, você conversou com algum candidato (a qualquer cargo: deputado estadual ou federal, governador, senador, presidente)? [In the 2018 elections, did you talk to any candidate running for office: state or federal deputy, governor, senator, or president?]
- Clientelism: Você sabe se as pessoas, de forma geral, recebem alguma ajuda de políticos e líderes comunitários em troca de voto e apoio político?[Do you know if people, in general, receive any help from politicians or community leaders in exchange for votes and political support?]
- Turnout in 2016: Você votou nas últimas eleições municipais de 2016, quando Crivella foi eleito? [Did you vote in the 2016 elections, when Crivella was elected?]
- Turnout in 2018: Você votou nas últimas eleições para presidente, em 2018?[Did you vote in the last presidential elections, in 2018?]
- Expected Turnout in 2020 [same question as Vote for Paes in 2020, but coding whether or not respondents say they are likely to vote]

Attribution

- Quem você considera o maior responsável pelo Minha Casa Minha Vida? [spontaneous; coded if mentioned politicians, parties, institutions] [Who is responsible for Minha Casa Minha Vida?]

Appendix C Balance Tests

We present balance tests for the joint null hypothesis (by regressing the treatment assignment indicator on the pre-treatment covariates) as well as the test for each covariate. For the balance tests regarding the recent lotteries, we also include the same survey weights we included in the main analysis. Also, for pooled analyses (including both lotteries in each survey), we either include lottery fixed effects or follow Lin (2013).

Table C1: Wald Test for Joint Null Hypothesis Test for Balance: Pooled Recent Lotteries

Res.Df	Df	F	Pr(>F)
1,256			
1,264	-8	1.54	0.1380

Note: Regression of treatment assignment on pre-treatment covariates (null model includes age). All standard errors are clustered at the individual level. Permutation p-value = 0.15

Table C2: F-Test for Joint Null Hypothesis Test for Balance: Pooled Early Lotteries

F	Df	p-value
0.49	6.00	0.82

Note: Regression of treatment assignment on pre-treatment covariates. All standard errors are clustered at the individual level. Permutation p-value = 0.799

Table C3: Pooled Balance Tests Recent Lotteries Survey: Covariates

	Estimate	Std. Error	t value	Pr(>|t|)	N
Sex (male)	0.05	0.04	1.19	0.24	1267
Race (white)	-0.01	0.04	-0.33	0.74	1267
Religion (any)	0.04	0.03	1.24	0.21	1267
Children (N)	-0.06	0.10	-0.62	0.53	1267
Schooling (years)	0.61	0.33	1.88	0.06	1267
Registry (CadUnico)	-0.04	0.03	-1.32	0.19	1267
Yrs in Formal Employment	0.39	0.17	2.26	0.02	1267
(Logged) Av. Formal Wages	0.50	0.23	2.12	0.03	1267
Not conditioning on age					
Sex (male)	0.04	0.04	0.95	0.34	1267
Race (white)	-0.01	0.04	-0.29	0.77	1267
Religion (any)	0.04	0.03	1.27	0.20	1267
Children (N)	-0.10	0.10	-0.98	0.33	1267
Schooling (years)	0.34	0.36	0.94	0.35	1267
Registry (CadUnico)	-0.04	0.03	-1.38	0.17	1267
Yrs in Formal Employment	0.27	0.19	1.41	0.16	1267
(Logged) Av. Formal Wages	0.29	0.27	1.07	0.28	1267

Notes: All standard errors are clustered at the individual level.

Table C4: Pooled Balance Tests Early Lotteries Survey: Covariates

	Estimate	Std. Error	t value	Pr(>|t|)	N
Lin Model					
Age	0.42	0.41	1.02	0.31	3923
Sex (male)	-0.02	0.02	-0.95	0.34	3923
Race (white)	0.00	0.01	0.15	0.88	3923
Registry (Cadunico)	-0.00	0.01	-0.34	0.73	3923
Years in Formal Employment	0.06	0.11	0.51	0.61	3923
Avg. Formal Wages	0.02	0.09	0.18	0.85	3923
Fixed Effects Model					
Age	0.33	0.40	0.85	0.40	3923
Sex (male)	-0.01	0.02	-0.43	0.66	3923
Race (white)	0.00	0.01	0.12	0.90	3923
Registry (Cadunico)	-0.01	0.01	-0.70	0.48	3923
Years in Formal Employment	0.10	0.11	0.96	0.34	3923
Avg. Formal Wages	0.07	0.09	0.72	0.47	3923

Notes: All standard errors are clustered at the individual level.

Table C5: Wald Test for Joint Null Hypothesis Test for Balance: Individual Lotteries - Recent Lotteries Survey

Lottery 17/2016				Lottery 20/2016			
Res.Df	Df	F	Pr(>F)	Res.Df	Df	F	Pr(>F)
786				461			
794	-8	1.52	0.1464	469	-8	0.49	0.8642

Notes: Regression of treatment assignment on pre-treatment covariates (null model includes age). All standard errors are clustered at the individual level. Permutation p-value 0.17 (Lottery 17/2016) and permutation p-value 0.89 (Lottery 20/2016)

Table C6: Wald Test for Joint Null Hypothesis Test for Balance: Individual Lotteries - Early Lotteries Survey

Lottery 03/2011				Lottery 06/2011			
Res.Df	Df	F	Pr(>F)	Res.Df	Df	F	Pr(>F)
1846				2063			
1852	-6	0.76	0.5979	2069	-6	0.74	0.6172

Notes: Regression of treatment assignment on pre-treatment covariates. All standard errors are clustered at the individual level. Permutation p-value 0.633 (Lottery 03/2011) and Permutation p-value 0.604 (Lottery 06/2011)

Table C7: Lottery 17/2016 Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(>|t|)	N
Sex (male)	0.07	0.04	1.77	0.08	796
Race (white)	-0.03	0.03	-0.94	0.35	796
Religion (any)	0.04	0.03	1.58	0.11	796
Children (N)	-0.02	0.10	-0.19	0.85	796
Schooling (years)	0.66	0.28	2.36	0.02	796
Registry (CadUnico)	-0.06	0.03	-2.33	0.02	796
Yrs in Formal Employment	0.24	0.17	1.46	0.14	796
(Logged) Av. Formal Wages	0.34	0.24	1.45	0.15	796
No conditioning on age					
Sex (male)	0.05	0.04	1.41	0.16	796
Race (white)	-0.03	0.03	-0.93	0.35	796
Religion (any)	0.05	0.03	1.73	0.08	796
Children (N)	-0.06	0.09	-0.63	0.53	796
Schooling (years)	0.25	0.33	0.78	0.44	796
Registry (CadUnico)	-0.06	0.03	-2.27	0.02	796
Yrs in Formal Employment	0.07	0.17	0.39	0.70	796
(Logged) Av. Formal Wages	0.06	0.26	0.24	0.81	796

Notes: All standard errors are clustered at the individual level.

Table C8: Lottery 20/2016 Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(>|t|)	N
Sex (male)	0.02	0.07	0.34	0.73	471
Race (white)	0.01	0.07	0.16	0.87	471
Religion (any)	0.03	0.05	0.60	0.55	471
Children (N)	-0.11	0.16	-0.68	0.50	471
Schooling (years)	0.55	0.57	0.96	0.34	471
Registry (CadUnico)	-0.01	0.05	-0.25	0.81	471
Yrs in Formal Employment	0.58	0.30	1.95	0.05	471
(Logged) Av. Formal Wages	0.69	0.38	1.81	0.07	471
No conditioning on age					
Sex (male)	0.02	0.07	0.29	0.77	471
Race (white)	0.01	0.07	0.20	0.84	471
Religion (any)	0.03	0.05	0.54	0.59	471
Children (N)	-0.14	0.16	-0.87	0.38	471
Schooling (years)	0.44	0.63	0.70	0.48	471
Registry (CadUnico)	-0.02	0.05	-0.38	0.70	471
Yrs in Formal Employment	0.51	0.33	1.55	0.12	471
(Logged) Av. Formal Wages	0.56	0.45	1.24	0.21	471

Notes: All standard errors are clustered at the individual level.

Table C9: Early Lotteries Balance Tests: Covariates

	Estimate	Std. Error	t value	Pr(>|t|)	N
		Lottery 03/2011			
Age	0.79	0.75	1.05	0.30	1853
Sex (male)	-0.05	0.03	-1.80	0.07	1853
Race (white)	0.00	0.03	0.15	0.88	1853
Registry (Cadunico)	0.01	0.02	0.60	0.55	1853
Years in Formal Employment	-0.16	0.21	-0.75	0.45	1853
Avg. Formal Wages	-0.20	0.18	-1.11	0.27	1853
		Lottery 06/2011			
Age	0.09	0.54	0.16	0.87	2070
Sex (male)	0.02	0.02	0.82	0.42	2070
Race (white)	0.00	0.02	0.02	0.99	2070
Registry (Cadunico)	-0.02	0.02	-1.29	0.20	2070
Years in Formal Employment	0.25	0.15	1.63	0.10	2070
Avg. Formal Wages	0.21	0.13	1.66	0.10	2070

Notes: All standard errors are clustered at the individual level.

Appendix D Attrition

We compare attrition patterns by regressing indicators for response and response conditional on "picking up" (the phone call) indicator on treatment assignment, respondent's sex (estimated based on subject's name using genderBR), pre-treatment earnings from a formal job, having a formal job, and interactions between treatment and these covariates. We then estimate a F-test of the hypothesis that all interaction coefficients are zero. In all models, we cluster standard error at the respondent level and we include sampling weights for the analysis of the 2016 survey. We find no systematic difference between in attrition patterns between treatment and control groups in both surveys.

In Tables D3 and D4 we show comparisons of attrition rates across treatment arms. In Columns (1) and (2) of both Tables, we regress a response indicator on treatment assignment, using Lin's (2013) model and fixed effects for each lottery, respectively. In columns (3) and (4) of both Tables, we compare attrition rates across treatment and control by regression a response indicator conditional of picking up the phone on treatment assignment. In all models, we cluster standard errors at the respondent level and we include sampling weights for the analysis of the 2016 survey. Overall, we do not observe different response rates across treatment and control groups even though there is some evidence of in models (1) and (2) of Table D3, at the 10% level of statistical significance. Unfortunately, we do not have age information available for all subjects we attempted (only for those interviewed). This means that we cannot examine attrition conditional on age, as we did for our main analyses in the paper, since randomization was (marginally) conditional on age in the lotteries conducted in 2016.

Table D1: Wald Test for Attrition Patterns: Survey Recent Lotteries

Interviewed Indicator				Interviewed Conditional on Picking up			
Res.Df	Df	Chisq	Pr(>Chisq)	Res.Df	Df	Chisq	Pr(>Chisq)
11,716				1,533			
11,719	-3	2.53	0.4701	1,536	-3	0.68	0.8770

Notes: All standard errors are clustered at the individual level.

In an effort to assess whether nonresponse could be leading to biased estimates, we compare estimates for administrative outcomes obtained using our survey sample against a "benchmark" estimates for the same outcomes using the whole population for which these data are observed and that does not suffer from nonresponse (attrition). Specifically, we estimate the effect of winning a lottery in 2011 on earnings in our survey sample and in the whole population. Table D5 shows that, in both the survey sample and in the whole population there is a null effect of winning a lottery on these labor outcomes. Furthermore, estimates are statistically indistinguishable from each other (p-value = 0.66, earnings outcomes; p-value

Table D2: Wald Test for Attrition Patterns Survey Early Lotteries

Interviewed Indicator				Interviewed Conditional on Picking up			
Res.Df	Df	Chisq	Pr(>Chisq)	Res.Df	Df	Chisq	Pr(>Chisq)
29,624				6,432			
29,627	-3	2.39	0.4950	6,435	-3	0.18	0.9808

Notes: All standard errors are clustered at the individual level.

= 0.40, formal job outcome). Unfortunately we do not have any post-treatment outcome using administrative data for the survey with 2016 lottery participants.

Table D5: Comparison of Survey Estimates to "Benchmark" Administrative Data Estimates

	Administrative Average (logged) Earnings	Administrative Formal Job	Survey Average (logged) Earnings	Survey Formal Job
Winning a lottery	-0.03	-0.01	0.03	0.02
	(0.04)	(0.01)	(0.12)	(0.03)
R^2	0.00	0.00	0.00	0.00
Adj. R^2	0.00	0.00	-0.00	-0.00
Num. obs.	623,410	623,410	3,923	3,923
RMSE	3.53	0.95	3.47	0.93
N Clusters	350,689	350,689	2,119	2,119

Notes: $^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table D3: Differential Attrition Rates: Survey with Recent Lotteries

	Resp. Lin Model	Resp. Fixed Effects Model	Resp. | Picking up Lin Model	Resp. | Picking up Fixed Effects Model
(Intercept)	0.10^{***}		0.66^{***}	
	(0.00)		(0.02)	
Winning Lottery	-0.02	-0.02	-0.12	-0.17
	(0.01)	(0.01)	(0.08)	(0.13)
Lottery ID	-0.00		-0.02^{**}	
	(0.00)		(0.01)	
Winning Lottery $\times$	-0.02		0.17	
Lottery ID	(0.02)		(0.16)	
R^2	0.00	0.00	0.00	0.00
Adj. R^2	-0.00	-0.00	-0.00	-0.00
Num. obs.	12,594	12,594	1,772	1,772
RMSE	3.06	3.06	4.81	4.81
N Clusters	7,764	7,764	1,096	1,096

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table D4: Differential Attrition Rates: Survey with Early Lotteries

	Resp. Lin Model	Resp. Fixed-Effects Model	Resp. | Picking up Lin Model	Resp. | Picking up Fixed-Effects Model
(Intercept)	0.12^{***}		0.56^{***}	
	(0.00)		(0.01)	
Winning Lottery	-0.00	-0.00	0.00	0.01
	(0.00)	(0.00)	(0.01)	(0.01)
Lottery Id	0.00		-0.01	
	(0.00)		(0.01)	
Winning Lottery × Lottery ID	0.01		0.05	
	(0.01)		(0.03)	
R^2	0.00	0.00	0.00	0.00
Adj. R^2	-0.00	-0.00	0.00	-0.00
Num. obs.	31,732	31,732	6,947	6,947
RMSE	0.33	0.33	0.50	0.50
N Clusters	17,117	17,117	3,771	3,771

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Appendix E Main Results Without Controls & Additional Specifications

In Tables E1 and E2, we show the estimates that are reported graphically in Figure 1. In Table E3, we show results that are reported graphically in Figure 2.

In this section, in Figure E1, we present the same estimates for ITT effects reported in the main body of the paper (Figure 1), but estimated without the inclusion of pre-treatment controls. Note that for the recent-lotteries, we do retain the variable age in the specification without controls, as age was the tie-breaker for the lottery selection, and thus an integral part of the selection mechanism.

We estimate the "political magnitude" of MCMV on Paes's votes in the following way. Paes's evaluation is 0.23 points higher among lottery winners in an evaluation scale ranging from -2 to 2. Considering that lottery nonwinners, on average, rate Paes's administration at -0.39, winning MCMV's lottery would not tip them past the center of the scale (i.e.,"regular"), whereas historically the association between voting for someone and evaluating them as excellent/good is about 0.8. We then conduct a simulation with generous assumptions. We assume all untreated individuals would vote for the mayor with probability defined by a standard logistic function of their evaluation of Paes, which can range from -2 to 2. An individual who evaluated the mayor at the midpoint of the scale would vote for him with probability 0.5, and individuals at the extreme with probabilities 0.12 and 0.88, respectively. We also assume every individual assigned to treatment turns out to vote (which is about 80% in Brazil) **and** receives a boost of 0.23 in their approval of Paes (which is the unstandardized effect of the effect we reported in Figure 1 calculated by multiplying the **largest** estimated effect size of 0.19 by the standard deviation of Paes's evaluation in the original scale for those assigned to control). Under these assumptions, Paes would get, on average, 130 more votes out of the 2,911 lottery winners. If we make the logistic steeper such that probability at the extremes shift to 0.02 and 0.98, this number increases to 176. We note, however, that Paes was term-limited in 2016 and supported Pedro Paulo in 2016 elections, so the real world pro-incumbent effect was likely even smaller. Finally, given that, descriptively, very few respondents attribute MCMV to Paes and/or PMDB, we do not think our set of findings suggests a strong association between becoming and attachment to local level politicians.

Finally, in Figure E2, we show results for the early lotteries using inverse probability weights to account for unequal probabilities of assignment to treatment.

Table E1: Recent Lotteries (Wave 1)

	ITT via FE (and controls)					ITT via Lin (and controls)					N
	Est.	SE	P-val	P-val†	Std. Est.	Est.	SE	P-val	P-val†	Std. Est.	
PT Eval. Index	0.011	0.076	0.889	0.889	0.011	0.054	0.084	0.519	0.519	0.054	1267
Incumbent at Treat. Eval. Index	0.198	0.084	0.018	0.037	0.193	0.228	0.100	0.023	0.046	0.222	1211
Attribution to Lula/Dilma/PT	0.196	0.092	0.033	0.066	0.203	0.190	0.100	0.059	0.119	0.196	753
Attribution to Paes/Pezao/ Temer/MDB	0.104	0.103	0.313	0.313	0.101	0.087	0.127	0.497	0.497	0.084	753

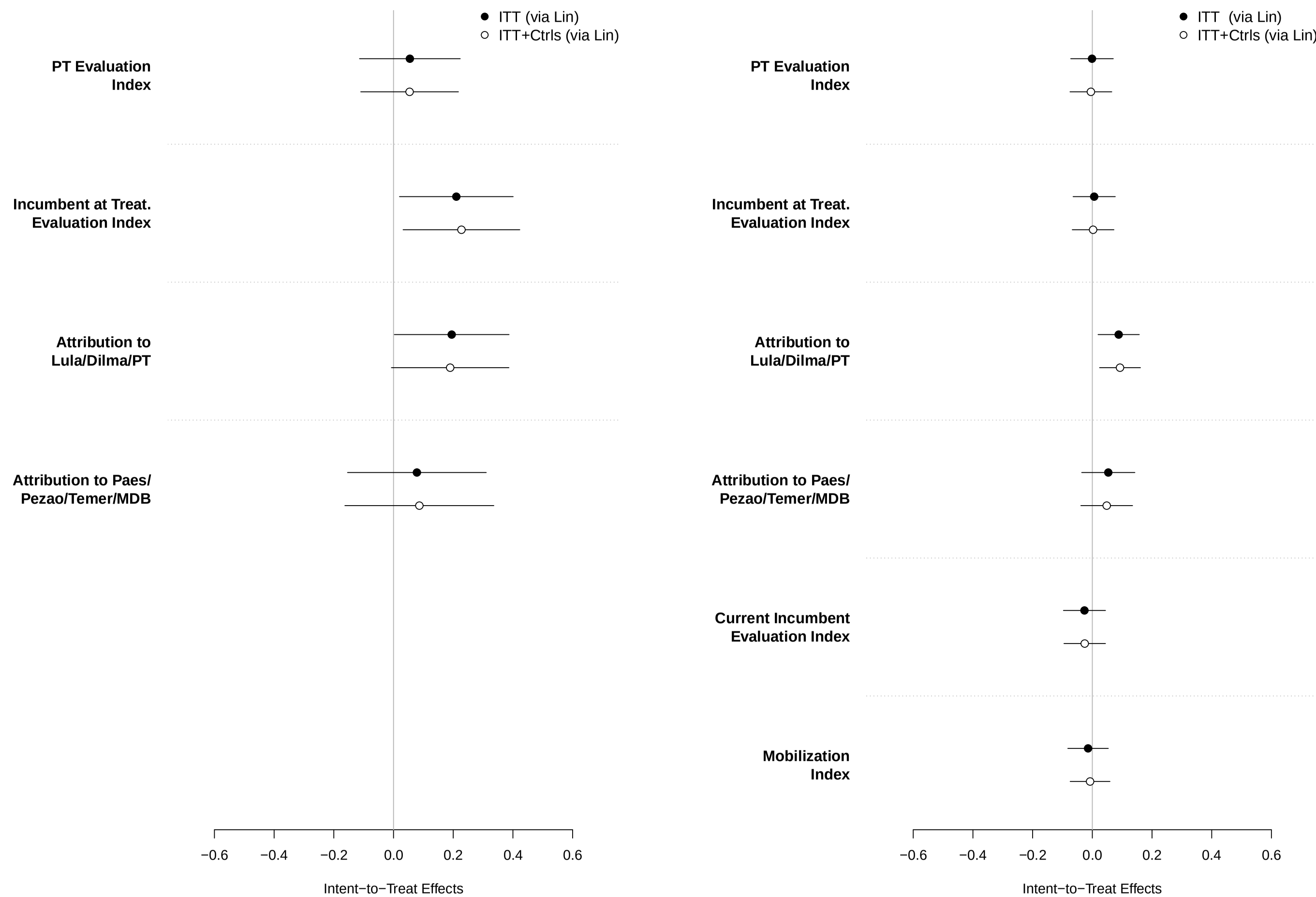
Table E2: Early Lotteries (Wave 2)

	ITT via FE (and controls)					ITT via Lin (and controls)					N
	Est.	SE	P-val	P-val†	Std. Est.	Est.	SE	P-val	P-val†	Std. Est.	
PT Eval. Index	0.003	0.034	0.935	0.950	0.003	-0.005	0.036	0.893	0.942	-0.005	3923
Incumbent at Treat. Eval. Index	0.023	0.034	0.509	0.950	0.023	0.003	0.036	0.942	0.942	0.003	3833
Current Incumbent Eval Index	0.002	0.034	0.950	0.950	0.002	-0.025	0.035	0.473	0.942	-0.025	3840
Mobilization Index	-0.015	0.033	0.660	0.950	-0.015	-0.008	0.034	0.824	0.942	-0.008	3906
Attribution to Lula/Dilma/PT	0.085	0.034	0.013	0.025	0.084	0.093	0.035	0.008	0.016	0.092	3903
Attribution to Paes/Pezao/Temer/MDB	0.030	0.041	0.458	0.458	0.029	0.048	0.044	0.276	0.276	0.047	3903

Notes: $\dagger Indicatesp-valuewithB-Hcorrection.$$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$

Table E3: Heterogenous Effects of Wait Since Enrollment (with controls, fixed effects, Figure 2)

	Short Wait			Long Waot				
	Estimate	Std. Error	Pr(>|t|)	Estimate	Std. Error	Pr(>|t|)	n	p_diff
PT Eval. Index	0.152	0.105	0.149	-0.217	0.117	0.063	1267	0.007
Treat. Inc. Eval. Index	0.272	0.116	0.019	0.101	0.121	0.408	1211	0.238
Attrib. Lula/Dilma/PT	0.252	0.113	0.027	0.163	0.153	0.287	753	0.376
Attrib. Paes/Pezao/Temer/MDB	0.164	0.146	0.260	0.039	0.147	0.789	753	0.826
Lula Eval.	0.128	0.103	0.212	-0.238	0.116	0.040	1197	0.011
Dilma Eval.	0.116	0.108	0.282	-0.200	0.117	0.089	1195	0.020
Paes Eval.	0.188	0.109	0.084	0.191	0.127	0.134	1192	0.917

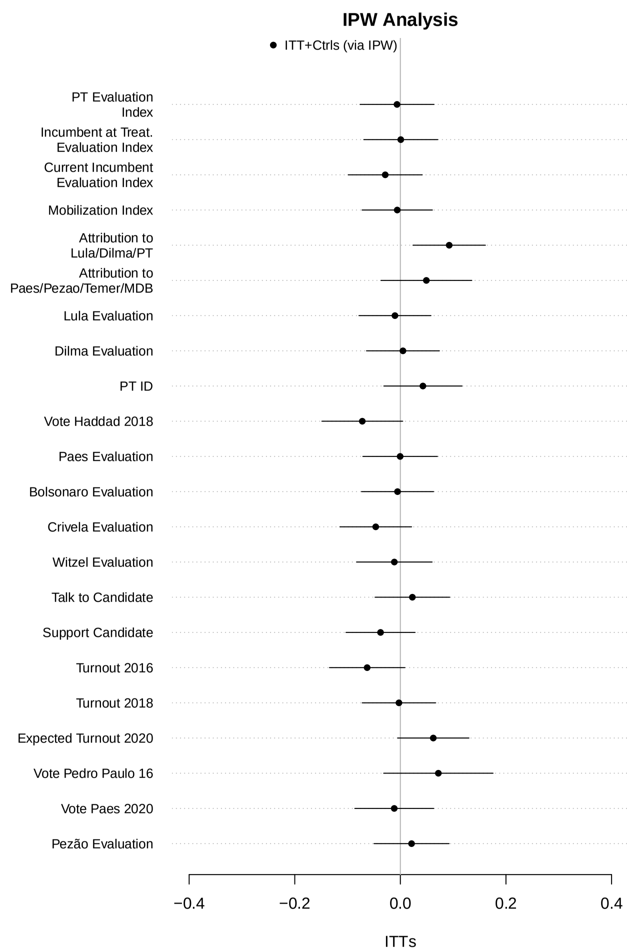


(a) Recent Lotteries (Wave 1)

(b) Early Lotteries (Wave 2)

Figure E1: MCMV Effects on Incumbent Evaluation and Mobilization (with and without controls)

Notes: Figure reports intent-to-treat effects on outcomes listed in Table 1.



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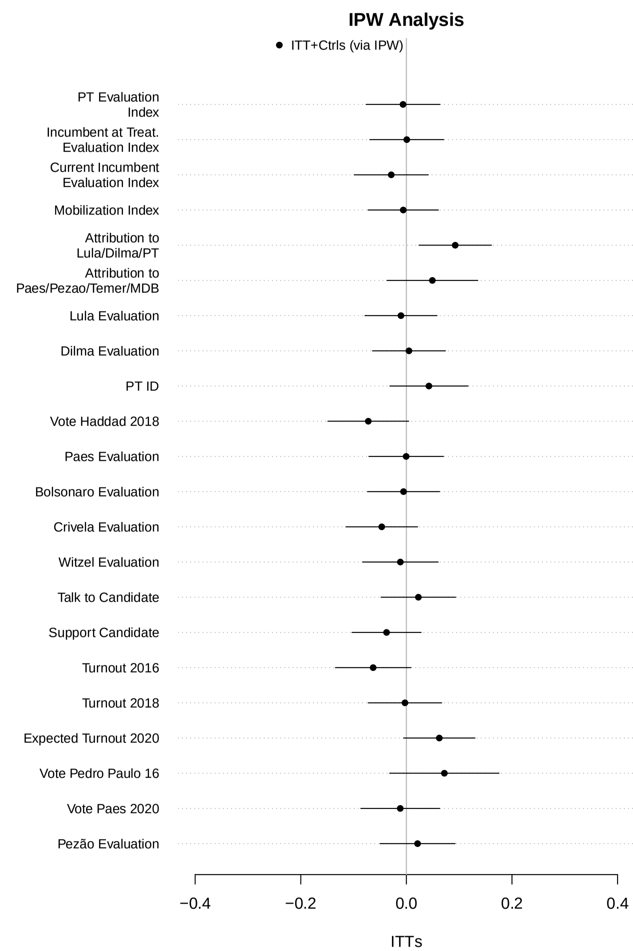
(a) Early Lotteries (Controls, IPW)



(b) Early Lotteries (No controls, IPW)

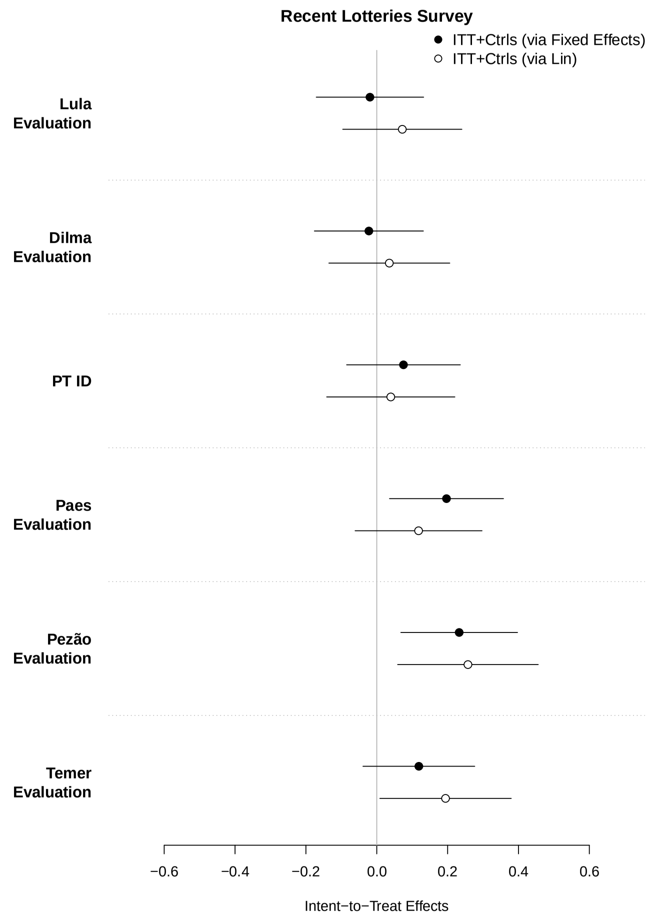
Figure E2: MCMV Effects on Incumbent Evaluation and Mobilization (with and without controls, IPW, early lotteries)

?? Notes: Figure reports intent-to-treat effects on outcomes for early lotteries survey.

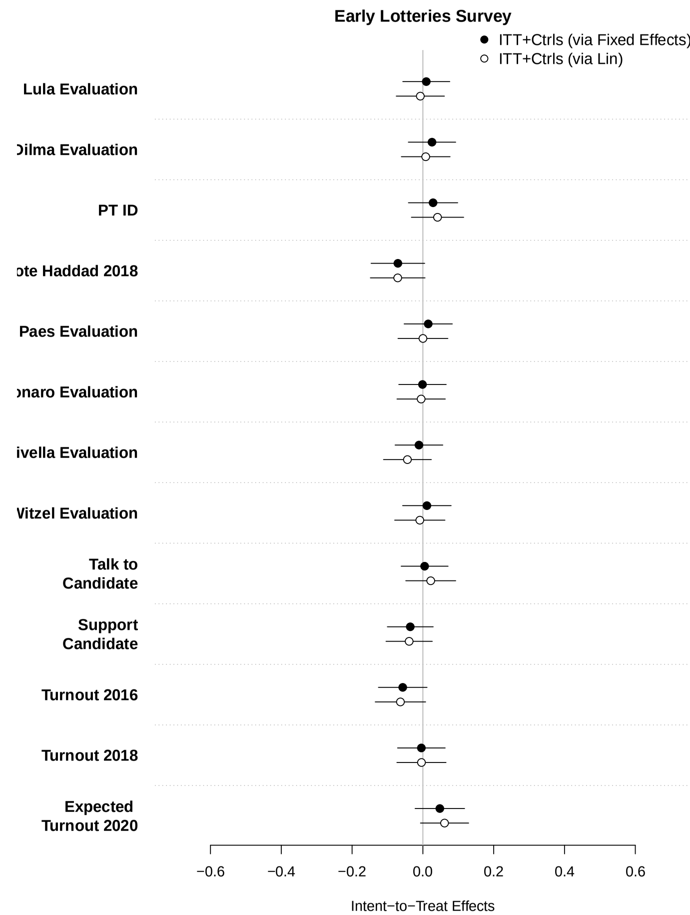
Appendix F Results for Individual Survey Items & Additional Analyses

For simplicity and parsimony, Figure 1, in the main body of the paper, only reports results for the summary indices that were produced by combining individual survey items, as described in Table 1. We report, in Figure F1 results for each of these items separately. The substantive message is clear. We find no effects for any of the individual items analyzed. Results are all but identical if we do not include pre-treatment controls, as reported in Figure F2.

We also examine the predictors of wait times in Tables F1 and F2.



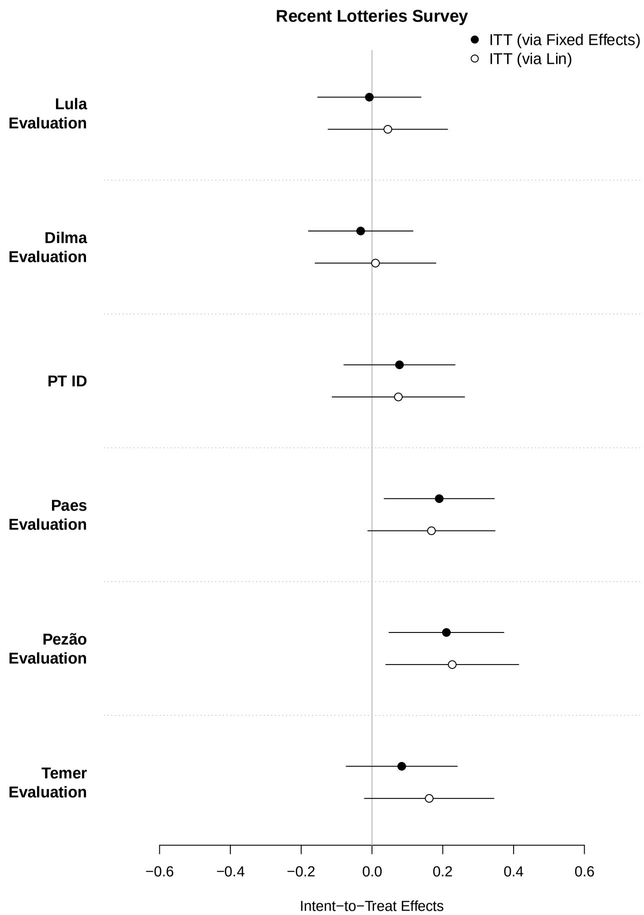
(a) Recent Lotteries (Controls)



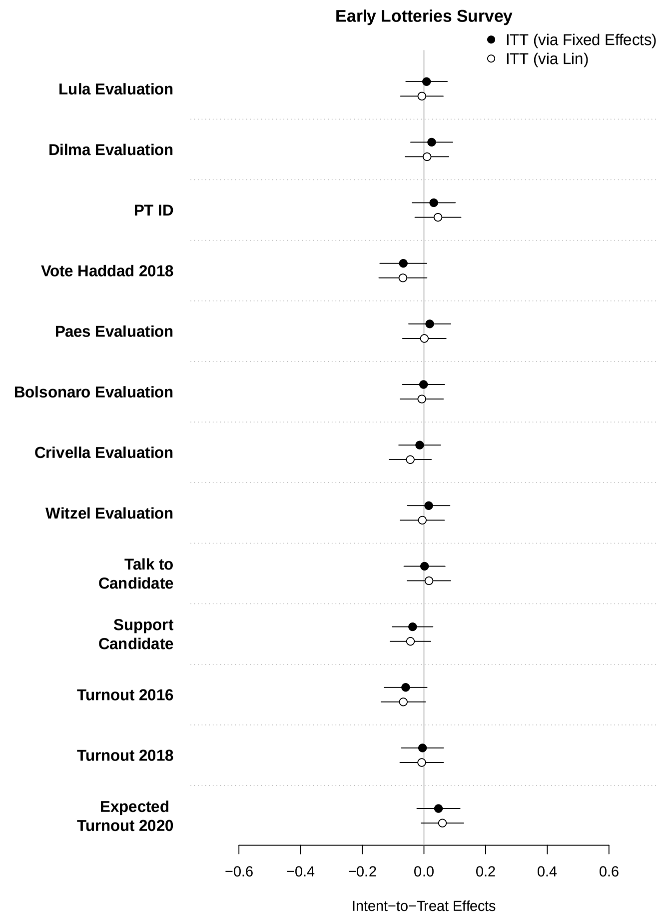
(b) Early Lotteries (Controls)

Figure F1: MCMV Effects on Incumbent Evaluation and Mobilization (individual outcomes, controls

Notes: Figures report intent-to-treat effects on individual outcomes.



(a) Recent Lotteries (No Controls)



(b) Early Lotteries (No Controls)

Figure F2: MCMV Effects on Incumbent Evaluation and Mobilization (individual outcomes, no controls)

Notes: Figures report intent-to-treat effects on individual outcomes.

Table F1: Predictors of Wait Time

	DV: Wait Time
Sex	0.07
	$[-0.24; 0.38]$
Race	0.07
	$[-0.24; 0.37]$
Religion	0.07
	$[-0.31; 0.44]$
Children	0.10^{*}
	$[0.00; 0.19]$
Years of	0.02
Schooling	$[-0.02; 0.06]$
CadUnico	-0.26
Registry	$[-0.65; 0.12]$
Formal	0.05
job	$[-0.13; 0.22]$
Average income	0.04
formal job (logged)	$[-0.08; 0.16]$
Age	0.03^{*}
	$[0.02; 0.04]$
R^2	0.06
Adj. R^2	0.05
Num. obs.	1267
RMSE	15.62
N Clusters	795

* 0 outside the confidence interval.

Table F2: Predictors of Wait Time and Interaction with Treatment Assignment

	DV: Wait Time
Treatment Indicator	-0.66
	$[-2.32; 1.00]$
Sex	0.07
	$[-0.24; 0.38]$
Race	0.07
	$[-0.24; 0.38]$
Religion	0.06
	$[-0.31; 0.44]$
Children	0.10^{*}
	$[0.00; 0.19]$
Years of Schooling	0.02
	$[-0.02; 0.06]$
CadUnico Registry	-0.26
	$[-0.65; 0.13]$
Formal job	0.05
	$[-0.13; 0.22]$
Average income formal job (log)	0.04
	$[-0.09; 0.16]$
Age	0.03^{*}
	$[0.02; 0.04]$
Sex×Treat	0.06
	$[-0.43; 0.54]$
Race×Treat	-0.10
	$[-0.64; 0.45]$
Religion×Treat	0.40
	$[-0.27; 1.07]$
Children×Treat	0.05
	$[-0.13; 0.24]$
Years of Schooling×Treat	0.00
	$[-0.06; 0.06]$
CadUnico Registry×Treat	0.09
	$[-0.59; 0.77]$
Formal job×Treat	-0.01
	$[-0.29; 0.28]$
Average income formal job (log)×Treat	-0.02
	$[-0.22; 0.19]$
Age×Treat	0.01
	$[-0.01; 0.03]$
Adj. R^2	0.04
Num. obs.	1267
Num. individuals	795

* 0 outside the confidence interval.

Appendix G Complier Average Causal Effects

In the main body of the paper we concentrate on ITT effects because non-compliance is a feature of the MCMV program. But not all lottery winners, however, decided to become program recipients or even learned about being lottery winners. All lotteries are public; however, only lottery winners are notified directly, and they are often not found by city officials. In the recent lotteries survey, we define treatment as "knowing about winning," which we measured that by coding whether the telegrams sent to lottery winners were received by them or a family member[2] as well as by asking, in our survey, whether the subject learned about winning an MCMV lottery. When we combine both measures, we find that 199 out of 286 individual-lottery winners learned about winning the lottery, and 69 out of 981 individual-lottery non-winners believed they were informed that they won the lottery.[3] In our second survey, we define treatment as becoming a program participant based on whether the lottery participant or their spouse signed a MCMV contract with the CEF;[4] 221 out of 912 individual-lottery winners became MCMV recipients and 249 out of 3,012 individual-lottery non-winners signed MCMV agreements.[5] Noncompliance is a feature of the program, and we are interested in learning about effects of the program on political behavior – and the fact that individuals do not learn about winning, choose not to take up or are deemed ineligible and cannot become recipients is of interest. Therefore, we focus our main analyses on the intent-to-treatment parameter and only report the complier average causal effects in this Appendix.

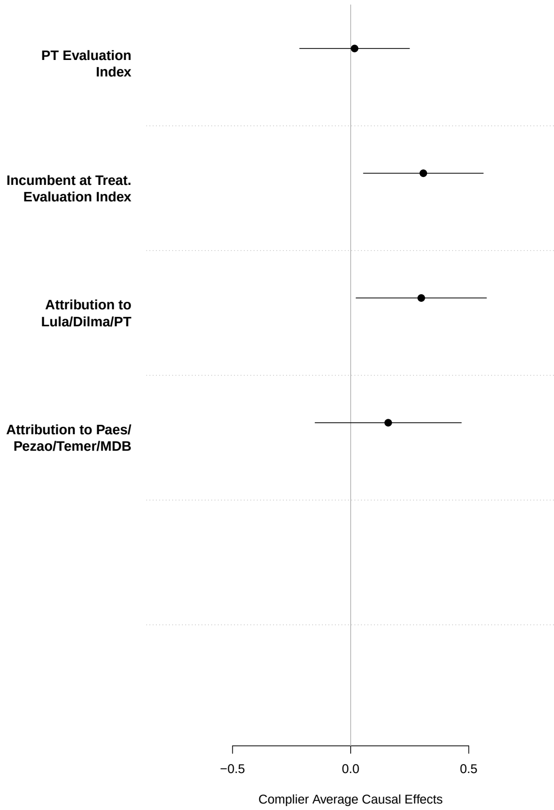
We estimate complier average causal effects (CACE) through a two-stage least squares procedure, employing the same pre-treatment controls and lotteries fixed effects. Results are presented in Figure G1.

[2]City hall shared these telegrams with us and a research assistant hand-coded them.

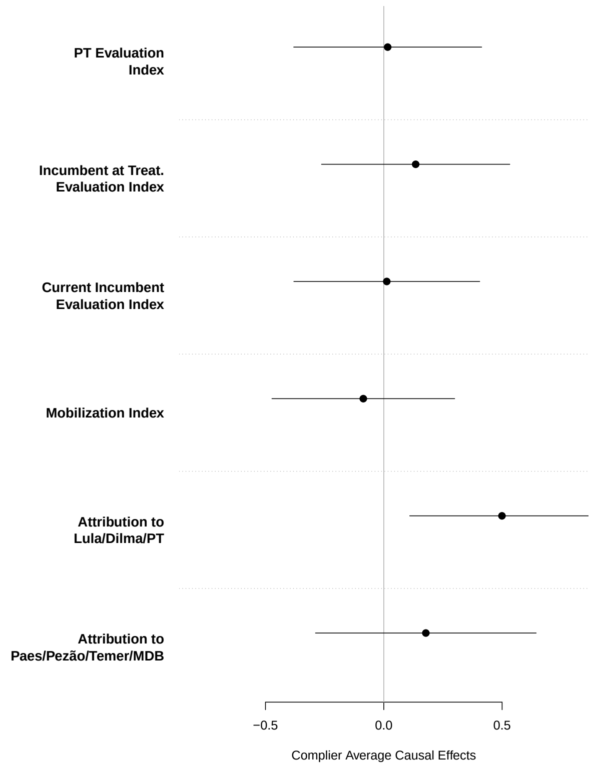
[3]Some non-winners sometimes participate in other social programs or are genuinely confused about government notifications, which leads them to declare being notified as lottery winners. More importantly, our survey data are measured at the individual level. Therefore, compliance (D_i) is measured at the individual-level i and not individual-lottery ij level.

[4]We obtained on all signers of MCMV agreements through an Access to Information request and we used the national social registry (Cadunico) to determine spousal status with lottery winners.

[5]Our agreement data are not linked to the lottery identification so individuals might have become MCMV beneficiaries through winning other lotteries held before or – more likely – after the ones we used in our survey. Also, some individuals may have become beneficiaries because of forced relocation or elderly/special needs lotteries – lottery applicants, under some infrequent circumstances may become beneficiaries through other pathways which could cause noncompliance. Like in our first survey, compliance (D_i) is measured at the individual i level.



(a) Recent Lotteries (Wave 1)



(b) Early Lotteries (Wave 2)

Figure G1: MCMV Effects on Incumbent Evaluation and Mobilization

Notes: Figure reports complier-average-causal effects on outcomes listed in Table 1, and for which we presented ITT estimates in Figure 1.

Appendix H Additional Public Opinion Data on MCMV

None of the general population public opinion surveys distinguish between MCMV's tiers in their question wording, but we believe that survey respondents have Tier 1 in their minds when answering questions because of MCMV Tier 1's distinctive-looking housing projects, government ads, and politicians credit claiming activity, both of which are focused on Tier 1 (Bueno 2022).

Figure H1 reports the full breakdown across income categories of support for the main social policies in the Ibope survey we analyzed, which is also reported more synthetically in Table H1.[6]

In the general population, the MCMV is better evaluated among beneficiaries than non bene-

[6] It is worth noting that the survey has plausible estimates of *Bolsa Família* and MCMV beneficiaries (23% and 1.5%, respectively).

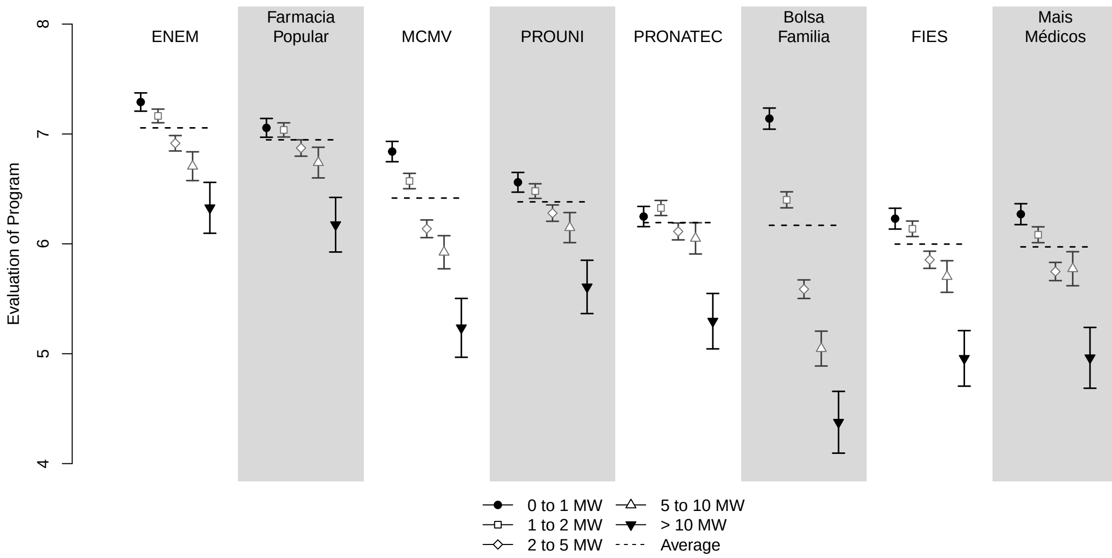


Figure H1: Evaluation of Programs and Initiatives by Family Income (2015)

Notes: Respondents were asked to rate programs or initiatives on a 0–10 scale. Family income is measured in multiples of monthly minimum wages, which at the time was R$ 788 (then about US$ 200). See text for summary descriptions of the programs.

ficiaries (evaluation is 1.21 higher than the baseline evaluation of 6.39 ($p << 0.01$). However, this includes all MCMV tiers in all types of municipalities. If we restrict our analysis to individuals whose reported family income is below 2 minimum wages (potentially eligible MCMV beneficiaries), the positive effect is smaller (0.97, $p < 0.01$) and even smaller if we restrict the analysis to cities that – as Rio de Janeiro – are their state capitals (0.89, $p = 0.06$). Further restricting the analysis to the metropolitan area of Rio de Janeiro (-0.70, $p = 0.64$) or to the city itself (-0.70, $p = 0.64$) yields negative point estimates that are not statistically significant. These results are compatible with the findings of our survey and suggest that, perhaps, the MCMV was less effective in capitals in general and in Rio in particular.

Table H1 presents more complete data from the the IBOPE survey, which was discussed in the main body of the paper. When asked about which government programs or initiatives they remembered (up to three), 12.8% cited *Bolsa Família*, 7.9% cited MCMV, and the next program was remembered by only 2.4% (ENEM, the unified college admissions exam). About 70% of respondents could not cite a single program. Both were substantially better known than *Farmácia Popular*, a program that distributes free medicines for selected chronic illnesses and is known by 28% of respondents.

Data from a survey close to the 2014 election (BEPS 2014) also shows that MCMV is less polarizing than other social policies across income levels (Figure H1).

Table H1: Knowledge and Evaluation of Social Programs and Initiatives (2015)

Program	Knowledge (%)	Evaluation (0–10)		
		Average	Std. Dev.	Poorest-Richest
Bolsa Família	59.1	6.2	3.4	-2.8
MCMV	49.8	6.4	3.2	-1.6
Farmácia Popular	27.8	6.9	2.9	-0.9
ENEM	27.4	7.1	2.8	-1.0
PRONATEC	24.8	6.2	2.9	-1.0
Mais Médicos	22.9	6.0	3.3	-1.3
FIES	19.1	6.0	3.0	-1.3
PROUNI	18.9	6.4	2.8	-1.0
None	16.6			

Notes: Knowledge was assessed by asking respondents whether they knew about 27 selected government programs or initiatives. Respondents were also asked to rate 8 of those programs or initiatives on a 0–10 scale. The last column reports the difference in average evaluations between those earning up to 2 and those earning more than 10 minimum wages. ENEM is a national exam for high-school students used as entrance exam for many universities; PRONATEC provides technical education; FIES is a private college financing program; PROUNI is a stipend for low-income students in public universities. See text for the other programs.

Appendix I Delivery of MCMV units over time

Figure I1 describes the pace of delivery of MCMV units over time. As mentioned in the main body of the paper, the MCMV program began delivering units in late 2009. Just before elections many municipalities began making their first deliveries, but the total number of units delivered was still quite small. The program gathered steam around the 2012 municipal elections and as a result, the second presidential electoral cycle in our sample is the one in which more municipalities made their first deliveries and in which most units were delivered. By the third presidential cycle most municipalities in the country had already delivered some MCMV unit and the number of units delivered began to slow.

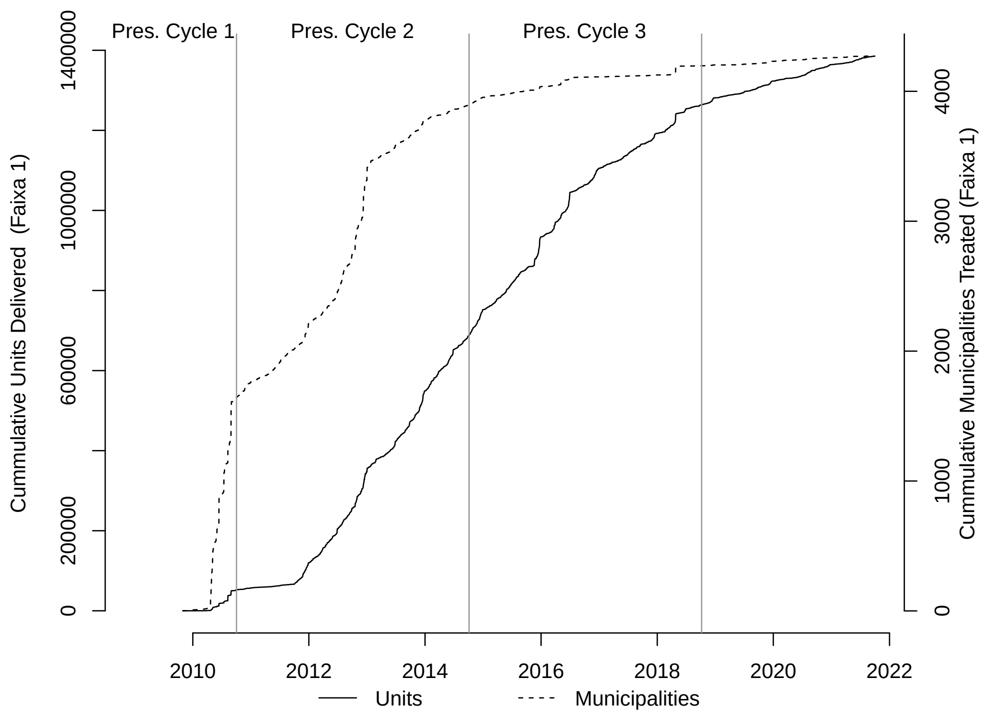
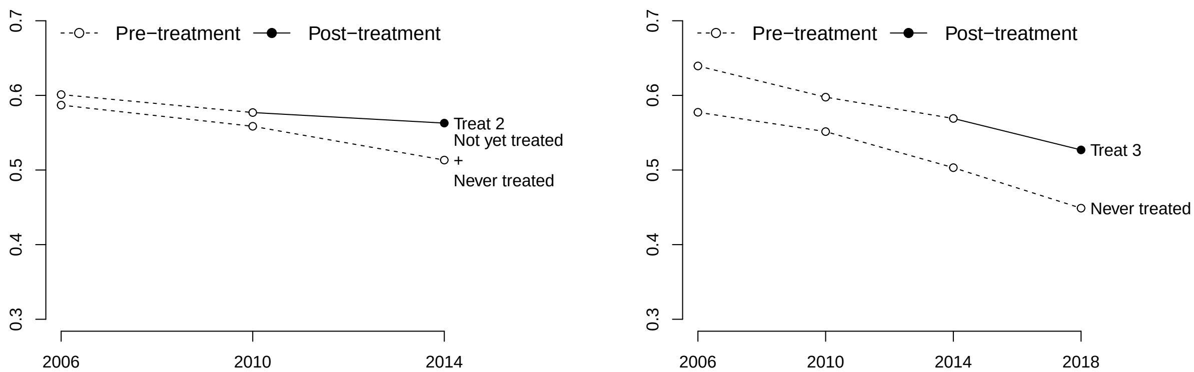


Figure I1: Delivery of MCMV Units Over Time

Notes: Data are from the Brazilian government obtained with freedom of information requests by the authors.

Appendix J Assessment of pre-treatment trends

Figure J1 shows pre- and post- treatment values for PT vote shares in the treatment and control municipalities in the second and third presidential cycles we analyze in the paper. These cycles are the ones in which we find MCMV effects on vote share and also the ones for which we have at least one pre-treatment period in which the PT was already the incumbent.



(a) Cycle 2 (b) Cycle 3

Figure J1: PT Presidential Performance by MCMV Treatment Status (2006–2020) Dashed lines and open dots indicate untreated periods whereas solid lines and solid dots indicate treated periods. See text for details.

Our data are composed of our groups of municipalities. `Treat 1` (N=1726), `Treat 2` (N=2192), and `Treat 3` (N=249) are municipalities treated for the first time in each of the three cycles we analyze. The remaining are `Never Treated` municipalities (N=1367). In our analysis, we use as controls for each period both this group of never treated municipalities as well as those that were not yet treated by the end of the respective cycle. For example, in the analysis of the second cycle, we compare `Treat 2` with a control group composed of `Treat 3`, which were "not yet treated,“ and those in the `Never Treated` group.

In the left-hand panel, we see that the pre-treatment slope for the municipalities in the `Treat 2` were very similar and that they clearly diverge in the subsequent treatment period. Similarly, in the right-hand panel, we see that the PT's electoral performance worsened slightly more than in the treatment group between 2006 and 2010, then slightly less between 2010 and 2014 and, again, less when treatment was dispensed.

We report analytical placebo tests of these pre-treatment trends in Table J1. These are models analogous to the ones we reported in the main body of the paper except that we added a pre-treatment period to each model. As a preliminary point, the treatment effects we reported in Table 2 correspond to the differences in the last two coefficients for each cycle in this table, and the hypotheses tests in our two-period DiD apply to this difference.

An examination of the pre-treatment interaction coefficients show that, in fact, treatment and control groups were not on clear divergent paths prior to the treatment dates. In the first column, `2010 Election`$\times$`MCMV` is on the borderline of statistical significance (p=0.098). While this is a somewhat ambiguous result, the magnitude of the coefficient is also much smaller than what we observe the following post-treatment period. In the second column, there are two pre-treatment coefficients. `2014 Election`$\times$`MCMV` is small and not significant. `2010 Election`$\times$`MCMV`, in turn, is statistically significant, but the direction of the coefficient is in the opposite direction of both the 2014 pre-treatment coefficient and the 2018 treatment effect.

Table J1: Placebo Test (Presidential Elections)

	2014 Election	2018 Election
2006 Election	0.587^{***}	0.577^{***}
	(0.004)	(0.005)
2010 Election	-0.028^{***}	-0.026^{***}
	(0.006)	(0.006)
2014 Election	-0.074^{***}	-0.074^{***}
	(0.006)	(0.007)
2018 Election		-0.129^{***}
		(0.007)
MCMV	0.014^{**}	0.062^{***}
	(0.006)	(0.011)
2010 Election$\times$MCMV	0.004	-0.016
	(0.007)	(0.015)
2014 Election$\times$MCMV	0.035^{***}	0.004
	(0.008)	(0.017)
2018 Election$\times$MCMV		0.016
		(0.019)
N	11416	4845
Municipalities	3808	1616

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Appendix K Extended Analysis of Electoral Results

In this section, we report more detailed results from our DiD analysis as well as several robustness checks. In Section 4.2, we report the political magnitude of MCMV's introduction on votes for the Workers' Party in presidential elections. The electoral returns are estimated by computing the number of votes received by the Workers' Party in municipalities with MCMV that can be attributed to MCMV based on our estimates in Table 2(a). For the each cycle we added the number of votes received by the Workers' Party in presidential elections in localities with MCMV and multiplied it by 0.031 for the 2010–2014 cycle and by 0.017 for the 2014-2018 cycle. That yields just over one million votes in the second cycle and about 530 thousand votes in the third cycle.

Appendix K.1 Complete DiD estimates

In the main body of the paper we only reported the causal coefficient of interest in Table 2. In this section we report the full set of coefficients from our traditional DiD specification, defined in Equation 2. Tables K1 and K2 report complete estimates for presidential and municipal elections. The coefficient on the post-treatment dummy is the trend in the control group between periods and the coefficient on the treatment group dummy is the pre-treatment differences between the groups. The coefficient on the interaction is the difference between variation over time in the treatment group and the control group, and is the coefficient of interest.

Table K1: Difference-in-Differences Estimates for Presidential Elections

	2006–2010	2010–2014	2014–2018	Combined
(Intercept)	0.595^{***}	0.559^{***}	0.503^{***}	
	(0.003)	(0.004)	(0.005)	
Post (dummy)	-0.026^{***}	-0.045^{***}	-0.054^{***}	0.021^{***}
	(0.001)	(0.002)	(0.002)	(0.002)
MCMV (dummy)	0.075^{***}	0.018^{***}	0.066^{***}	0.047^{***}
	(0.005)	(0.005)	(0.012)	(0.003)
POST×MCMV	0.005^{**}	0.031^{***}	0.012^{**}	0.018^{***}
	(0.002)	(0.003)	(0.006)	(0.002)
N	11132	7613	3232	21977
Municipalities	5567	3808	1616	5570

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Robust standard errors clustered by municipality. Combined estimation includes electoral period fixed-effects.

Table K2: Difference-in-Differences Estimates for Municipal Elections

	2008–2012	2012–2016	Combined
(Intercept)	0.378^{***}	0.434^{***}	
	(0.005)	(0.005)	
POST	0.057^{***}	-0.027^{***}	0.007^{***}
	(0.006)	(0.007)	(0.002)
MCMVTRUE	0.006	0.001	0.008^{**}
	(0.007)	(0.008)	(0.004)
Post×MCMV	0.018^{**}	-0.003	0.006
	(0.009)	(0.011)	(0.005)
N	11140	5640	16780
Municipalities	5570	2820	5570

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Notes: Robust standard errors clustered by municipality. Combined estimation includes electoral period fixed-effects.

Appendix K.2 Alternative specifications of MCMV treatment

In the main body of the paper we presented results from a DiD anlaysis in which we operationalized MCMV as a binary variable that took on the value of one if the municipality delivered any number of homes in the electoral period. In this section we examine alterantive specifications of the independent variable.

In Table K3 we report the same analysis employing two different continuous operationalization of MCMV presence in the municipality, namely the number of homes delivered by 100 inhabitants and the number of homes delivered relative to the housing deficit, as estimated by the *Ministério das Cidades* for 2009.

The results are very similar to what we reported in the main body of the paper. For the presidential elections, we see statistically significant results for both operationalziations of intensity of MCMV in the first two cycles and in the combined analysis. Both operationalizations also tell a similar substantive story. In the combined analysis, for instance, the incumbent candidate received just under half a percentage point more votes in a treated municipality with the median intensity of MCMV, and this effect increases to just over 2 p.p. in municipalities at the 95$^{\text{th}}$ percentile of MCMV intensity.

For municipal elections, as in the main body of the paper, we do not see statistically significant effects in the second cycle, but the pooled estimates for each operationalization of

Table K3: Estimates with Continuous Operationalization of MCMV

(a) Presidential Elections

	MCMV by 100 people				MCMV to housing deficit			
	2006–2010	2010–2014	2014–2018	Combined	2006–2010	2010–2014	2014–2018	Combined
Post ×MCMV	0.011***	0.012***	0.007	0.012***	0.040***	0.014***	0.014	0.018***
	(0.004)	(0.002)	(0.006)	(0.002)	(0.007)	(0.003)	(0.016)	(0.003)
N 11128	7606	3232	21966	11130	7606	3222	21958	
Municipalities	5564	3803	1616	5569	5565	3803	1611	5565

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

(b) Mayoral Elections

	MCMV by 100 people			MCMV to housing deficit		
	2008–2012	2012–2016	Combined	2008–2012	2012–2016	Combined
Post×MCMV	0.028***	0.002	0.015***	0.053***	0.004	0.030***
	(0.009)	(0.012)	(0.005)	(0.016)	(0.028)	(0.008)
N	11128	5640	14338	11130	5630	14335
Municipalities	5564	2820	5455	5565	2815	5450

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

MCMV presence imply a boost to the incumbent of 0.3 and 0.4 p.p. in median intensity municipalities and 2.2 and 2.4 p.p. in high intensity ones.

Appendix K.3 Conditioning on pre-treatment covariates

Table K4 reports results for the same binary operationalization shown in Table 2, in the main body of the paper, but estimated on data that were pre-processed by matching treated and untreated municipalities on income per capita (and on its log), on population, the housing deficit (and the square of the housing deficit), an indicator for whether the municipality was part of a metropolitan region. In the main body of the paper we had already reported the matched results for the combined analysis of the three cycles. Here we also report the cycle-by-cycle results. We see stronger results for both the first and third cycles and slightly weaker results for the second cycle when compared with our main estimates.

Table K4: Estimates on Matched Datasets

(a) Presidential Elections

	2006–2010	2010–2014	2014–2018	Combined
Post×MCMV	0.010***	0.024***	0.016**	0.018***
	(0.003)	(0.003)	(0.008)	(0.002)
N	10566	13152	1494	25212
Municipalities	3685	3573	616	5491

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

(b) Mayoral Elections

	2008–2012	2012–2016	Combined
Post×MCMV	0.023**	−0.008	0.006
	(0.012)	(0.014)	(0.007)
N	10996	4816	13474
Municipalities	4278	1945	5025

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

We also implemented the doubly-robust DiD estimator proposed by Sant'Anna and Zhao (2020), as an alternative strategy to condition on the same pre-treatment covariates that we matched on, above. Not surprisingly, as shown in Table K5, results tell the same substantive story as those obtained through matching.

Table K5: Doubly-robust DiD Estimates Conditioning on Covariates

(a) Presidential Elections

	2006–2010	2010–2014	2014–2018
Post×MCMV	0.008^{**}	0.030^{***}	0.009^{**}
	(0.002)	(0.002)	(0.007)
N	11120	7606	3222

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

(b) Mayoral Elections

	2008–2012	2012–2016
Post×MCMV	0.018^{**}	-0.001
	(0.009)	(0.011)
N	11128	5628

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Appendix K.4 Keeping only eventually treated controls

A number of municipalities implemented MCMV in the first cycle. In our main analysis, and in our matching analysis for that matter, we compared these municipalities with the ones that did not implement MCMV in that cycle. Some of these "control" municipalities, however, implemented MCMV in the subsequent cycle while others never did and a few did in the third cycle. Presumably, the subset of control municipalities that implemented MCMV in the second cycle are "more similar" to the ones that implemented in the first cycle than are the ones that never implemented MCMV (or did so in the third cycle). Hence, we also re-estimated the DID analysis of the first cycle keeping as controls only these "more similar" municipalities that implemented MCMV in the second cycle.

Our results, reported in Figure K6, indicate an effect of 0.014 (SE=0.005, p<0.01), which is just smaller than the estimate reported for this period in the main body of the paper, but still substantial.

Table K6: Keeping only most similar controls (2006–2010)

	2006–2010
(Intercept)	0.598^{***}
	(0.010)
Post	-0.029^{***}
	(0.005)
MCMV	-0.021^{**}
	(0.010)
Post×MCMV	0.014^{***}
	(0.005)
N	4882
Municipalities	2441

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Appendix K.5 Local elections in which incumbents could run

We can identify whether an individual was the incumbent and whether she was term limited in each election with relative precision by matching candidates across elections based on their tax identification number and/or electoral registration numbers. This should capture all cases except those in which sitting mayors leave office during the term. Identifying an incumbent-supported candidate is somewhat harder, and we employed a combination of several different approaches. If the mayor did not personally run, we first assumed that any candidate from the mayor's party at the time of election was supported by her. In order to identify the mayors *current* party (at the time of the election), we combined party membership in the previous election updated, if necessary, by information available in the public dataset of party members in order to capture post-electoral party-switches. Finally, in cases in which neither procedure yielded a candidate, we considered as the incumbent-supported candidate the individuals whose who electoral coalition included the current party of the incumbent.

These strategies, combined, allowed us to identify an incumbent in about 90% of municipalities in our cycles of interest (last column in Table K7). The first columns in Table K7 indicate that about 3/4 of incumbents were not term limited in any election (i.e. could run), and about 2/3 of those eligible actually do so.

Table K7: Data on Mayoral Incumbent Candidates

	Could Run	Mayor Ran	Incumb. Cand Identified
2008–2012	68.79	44.98	92.64
2012–2016	75.79	50.78	89.95
2016–2020	77.54	52.21	83.39

While we can conjecture that in elections in which the incumbent is personally running there should offer greater opportunities of direct attribution and therefore greater effects of MCMV, the decision to run is highly endogenous to factors that affect the probability of electoral victory. Non-term limited incumbents might choose not to run in anticipation of electoral defeat, but many mayors often leave office mid-term to run for "higher offices", such as state or federal legislatures, for which election happens in the off-cycle.

With this caveat in mind, we report, in Table K8, estimates in the subsample of municipalities in which the mayor was not term limited and the incumbent could, in principle, run for office. As in the main body of the paper, we find positive effects on the first cycle, but not on the second. Effects in the first cycle are weaker and confidence intervals are wider because the sample is substantially smaller. That said, the estimate of 0.014 in the sample of municipalities with non-term limited mayors falls clearly within the 0.95 confidence interval of the original estimate (c.i. $= [0.001, 0.035]$).

Table K8: Local elections, non term-limited mayors

	2008–2012	2012–2016	Pooled
(Intercept)	0.611^{***}	0.389^{***}	
	(0.005)	(0.005)	
POST (dummy)	-0.189^{***}	0.027^{***}	0.028^{***}
	(0.007)	(0.008)	(0.004)
MCMV (dummy)	0.014^{*}	0.005	0.010^{**}
	(0.007)	(0.008)	(0.005)
Post×MCMV	0.014	−0.007	0.004
	(0.010)	(0.012)	(0.007)
N	3444	4312	7263
Municipalities	1722	2156	2935

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Appendix L MCMV Allocation

We analyze the effects of alignment with the presidency and with the minister's parties on funds for MCMV using a close-race regression-discontinuity design. Municipalities where the president's (or the minister's) party won the local election by a narrow vote margin are considered aligned and places where the president's party lost by a narrow vote margin are considered unaligned. Municipalities should, in expectation, differ only in the presence or absence of a mayor who belongs to the president's (or minister's) party. Table L4 shows no large or systematic partisan bias exists in the allocation of resources to fund MCMV. We also examine whether the central government provides aligned mayors with money earlier in their term to give them a head start. Table L3 shows that partisanship is not a predictor for the average number of years it takes a municipality to receive funding for MCMV. In sum, we fail to find evidence suggestive of uneven distribution of funds across localities.

In addition to balance tests, we also examine the manipulation of the running variable around the thresholds. The evidence is not consistent with manipulation of the running variable (using local polynomial density estimation).[7]

Table L4 reports RDD using races in two-candidate races. We remove 7 agreements that were signed after May 2016, post President Rousseff's removal from office – the results are very similar if these agreements are included. Units are total transfers in each municipality, during the mayors' term, pooled across election cycles. The Progressive Party, which held the ministry of urban affairs between 2005 and 2015, and the Workers' Party held the presidency between 2003 and 2016.

[7]For pooled elections years (alignment with the president's party), we find a p-value < 0.1, but examining the election years separately and for pooled elections years (alignment with the minister's party), there is no evidence consistent with manipulation.

Table L1: Balance Test (President's Party Alignment)

Covariate	Est. Conv.	Est. Robust.	Robust Std. Error	p-value
Income per capita	-22.79	-26.88	14.95	0.07
Doctors per thousand pop.	-0.07	-0.09	0.08	0.27
Education (IDH)	-0.01	-0.02	0.02	0.25
Income (IDH)	-0.01	-0.01	0.01	0.41
Longevity (IDH)	-0.01	-0.02	0.02	0.23
Illiteracy rate	0.07	0.31	2.32	0.89
Infant mortality	3.68	4.64	3.41	0.17
Population	-2414.16	2209.29	7323.20	0.76
Poverty rate	4.51	5.53	3.71	0.14
Vote for Lula	0.01	0.01	0.03	0.78
Vote for FHC	-0.02	-0.02	0.03	0.50
Vote for PT (fed. dep.)	0.02	0.02	0.02	0.30
Votes for PSDB (fed. dep.)	0.04	0.05	0.04	0.16
Vote for PT (governor)	-0.03	-0.03	0.02	0.18
Vote for PSDB (governor)	-0.00	-0.00	0.04	0.94
Votes for PT (state dep.)	0.01	0.00	0.02	0.84
Votes for PSBD (state dep.)	0.03	0.04	0.04	0.24
Turnout	-0.04	-0.04	0.02	0.04

Table L2: Balance Test (Minister's Party Alignment)

Covariate	Est. Conv.	Est. Robust	Robust Std. Error	p-value
Income per capita	15.95	20.01	17.77	0.26
Doctors per thousand pop.	0.12	0.14	0.08	0.08
Education (IDH)	0.01	0.01	0.02	0.58
Income (IDH)	0.02	0.02	0.02	0.26
Longevity (IDH)	0.00	0.01	0.02	0.60
Illiteracy rate	-1.52	-1.90	3.00	0.53
Infant mortality	-1.39	-2.05	3.40	0.55
Population	-5718.98	-7575.74	8486.56	0.37
Poverty rate	-5.79	-7.18	4.51	0.11
Vote for Lula	0.01	0.01	0.03	0.67
Vote for FHC	0.01	0.01	0.03	0.67
Vote for PT (fed. dep.)	-0.02	-0.02	0.02	0.25
Votes for PSDB (fed. dep.)	0.00	-0.00	0.04	0.93
Vote for PT (governor)	0.02	0.02	0.03	0.48
Vote for PSDB (governor)	-0.02	-0.03	0.05	0.52
Votes for PT (state dep.)	0.01	0.01	0.01	0.70
Votes for PSBD (state dep.)	0.03	0.04	0.03	0.29
Turnout	0.02	0.02	0.03	0.36

Table L3: Association between Year of Funding and Mayor's Party

Mayor's Party	Estimate	Std. Error	Statistic	p-value
PT	-0.03	0.08	-0.40	0.69
PP	0.04	0.09	0.39	0.70
PSDB	-0.00	0.07	-0.06	0.95

Notes: Table L3 shows a regression of number of years it takes a municipality to receive MCMV funding on mayor's partisanship (from the first year of the mayor's term). Dependent variable is the average number of years it took a municipality to receive funding (from the first year of the mayor's term) of agreements. Units are municipality-agreements. Municipality and term fixed effects included. Standard errors clustered at the municipal level.

Table L4: RDD President Party and Minister Estimates, *Minha Casa Minha Vida* Contracts (logged reais per capita), 2009–2016

	President's Party	Minister's Party
	(logged reais per capita), 2009–2016	
Conventional	0.181	-0.158
	(0.370)	(0.372)
Bias-Corrected	0.222	-0.247
	(0.370)	(0.372)
Robust	0.222	-0.247
	(0.434)	(0.439)
Kernel	Triangular	Triangular
Bandwidth	mserd	mserd

+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix M Pre-Analysis Plan Considerations

We registered hypotheses related to this project, which includes this manuscript and other manuscripts currently under review or in preparation. We focus our discussion regarding our PAP for the sections that directly relate to this manuscript.

Outcomes and Adjusting for multiple comparisons We deviated from our pre-analysis plan in determining which variables we used as main outcome for the hypotheses analyzed here. In all of the instances in which we deviated from the PAP in determining the main outcome, we used an overall composite index containing all of the variables related to that hypothesis rather than choosing a single outcome related to that hypothesis. We took this step precisely as a strategy to reduce the number of comparisons we were making. Furthermore, we also report all of the single outcomes that composes our indices in the Appendix and all of the single components of the index were included in our PAP as either the main or secondary outcome for a given hypothesis. Our procedure for creating these indices was pre-registered. The table below describes our deviations:

Table M1: Comparing PAP and Manuscript's Main Outcome

Hypothesis	Main Outcome	PAP Main Outcome
PT Evaluation	PT Eval. Index Index	Dilma Eval./PT ID.
Mobilization	Mobilization Index	Talked recently to candidate
Inc. Evaluation	Inc. at Treat. Eval. Index	Dilma Eval.
	Curr. Inc. Eval. Index at treatment.	Crivella Eval.

In our amendment to our pre-analysis plan, we stated that we would employ corrections to account for multiple comparisons within the same general hypothesis, according to the following rule: for hypotheses for which there is a single outcome, there would be no correction, and, for other hypotheses, we specified that the primary outcome is not subject corrections. We did not include corrections in this manuscript because they would inflate our p-values and we are showing null results.

Attrition We followed our pre-registered protocol for examining and evaluating attrition.

Control Variables We pre-registered age, sex, race, formal employment and wages as our control variables for the 2011 lotteries study. In any case, our adjusted and unadjusted results are quite similar.

Hypotheses

Based on our PAP, we expectations about 2011 lotteries survey were:

Beneficiaries' preferences, voting behaviors, and job evaluations will be more favorable to those who were incumbents at the moment in which they became beneficiaries than nonbeneficiaries (gratitude)

On average, however, beneficiaries' partisan preferences will not differ from nonbeneficiaries (no partisan retrospection)

We also expect a more positive evaluation of current incumbents, even though they are not directly related to the benefits.

Beneficiaries' preferences, voting behaviors, and job evaluations will be more favorable to incumbents at the time of the survey than nonbeneficiaries (blind-retrospection)

Beneficiaries will be more engaged in politics than nonbeneficiaries

Overall, we expected greater direct electoral returns than what we found.

Non-beneficiaries: We did not registered a pre-analysis plan for the analyses regarding non-beneficiaries (DiD).

Appendix N Ethics Discussion

This research project was submitted for review at Human Subjects Research Review Boards at *Yale University* (exempt, protocol #2000020455), Emory University (approved, protocol #IRB00101802), and FGV Ethics Committee (approved, #04/2017). Our survey with early lottery winners was partly conducted during the Covid-19 pandemic. All of our survey interviews in 2019 and 2020 were conducted via phone and enumerators were working from home.

We collected subjects' names and contact information from public sources and via private vendors, respectively. This data collection was in accordance with national laws regarding privacy of personal information.

We obtained voluntary and informed consent via phone. Our enumerators read our consent form to subjects who then agreed or declined to answer our questionnaire. In addition to reading our consent form, we also offered subjects the option to receive the consent form via WhatsApp (a messaging application widely used in Brazil). That way, subjects would have our consent form in writing too. The consent form explicitly stated this questionnaire was part of a research project conducted by universities and subjects were also provided with a local research institution review board's contact information to facilitate access (via phone, email, or in person). We also trained enumerators to answer questions related to the nature of the research project to make it as clear as possible to subjects that we were not involved with any political group or governmental agency. Our survey did not engage in deception and we do not anticipate having intervened in the political process because the intervention analyzed here was conducted by the government (without collaboration from the authors).

For our qualitative MCMV interviews, we obtained oral consent in person and individuals were compensated for their time and reimbursed for transportation costs.

Appendix O Data Sources & Transparency

1. Survey data was collected via telephone surveys conducted with company E-Field.
2. Data on *Minha Casa, Minha Vida* enrollments through requests using the Law of Access to Information.
3. Qualitative intreviews conducted in mid-2018 with support from company E-Field
4. Data on formal jobs and income from formal labor obtained from RAIS (access via institution EBAPE-FGV)
5. Subjects' gender using R package `genderBR`.
6. Cadunico (national registry data) data obtained via request to the Ministry of Social Development, which was subsequently merged into what is now the Ministry of Women, Family and Human Rights
7. Brazilian Electoral Panel Studies (2014): https://publications.iadb.org/en/publication/12807/brazilian-electoral-panel-studies-beps
8. IBOPE (2015, Study 04683): https://www.cesop.unicamp.br/eng/banco_de_dados/page:408?ext=html