Online Appendix

Campaign Connections

Samuel Bazzi and Claudio Labanca

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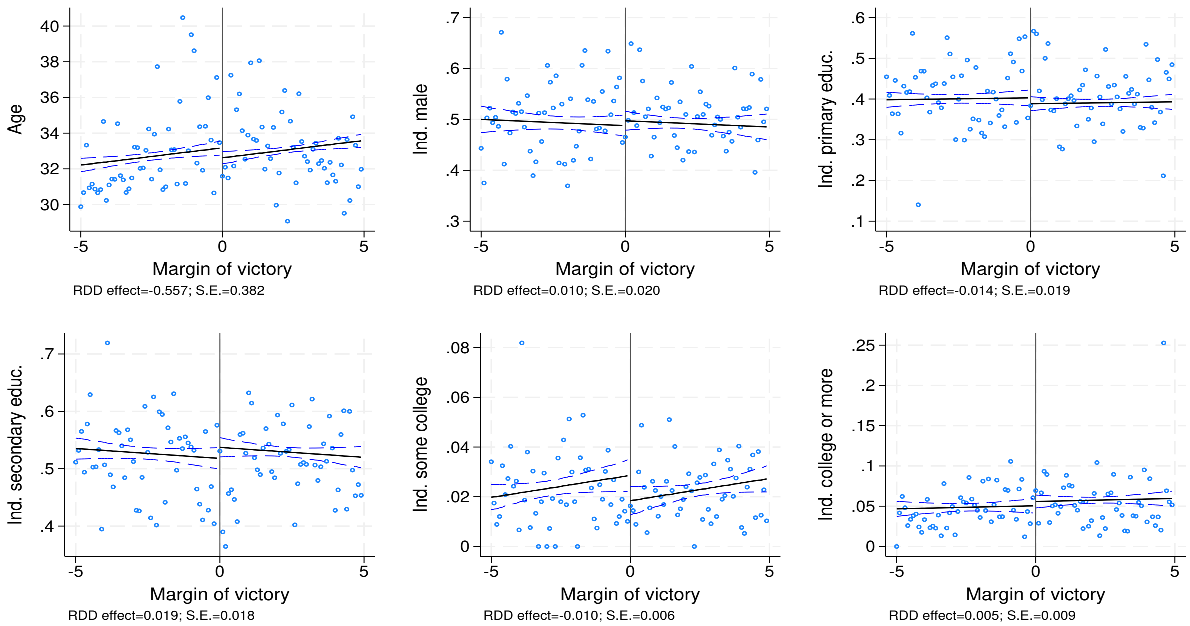
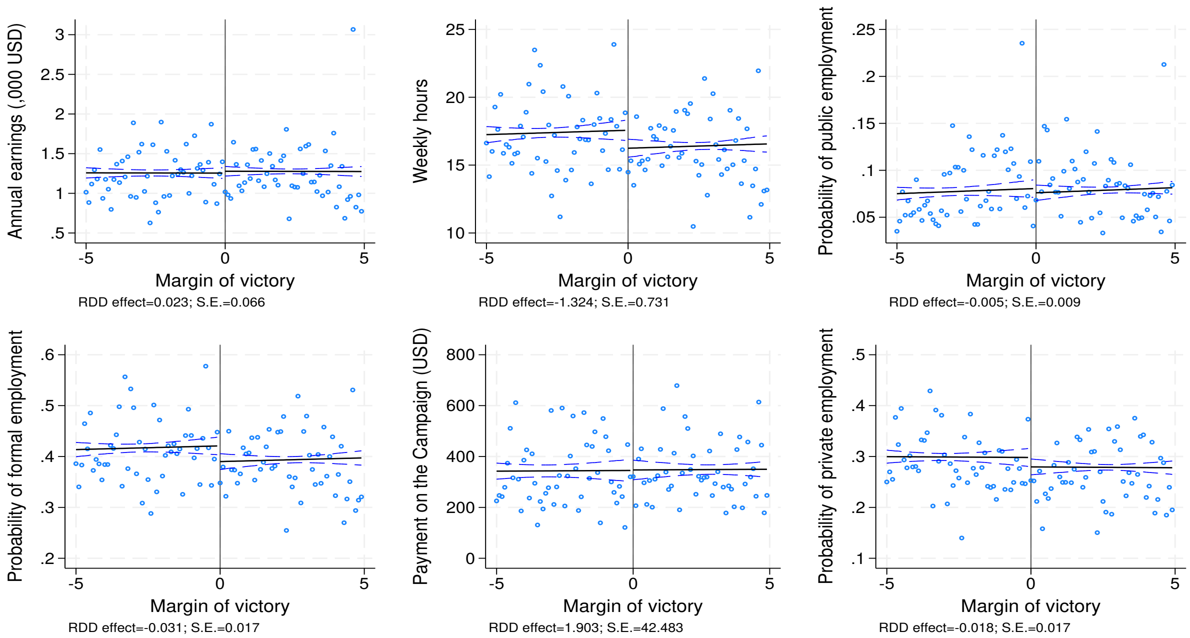
Figure A.1: RD balance tests on Campaign Workers' Characteristics (I)



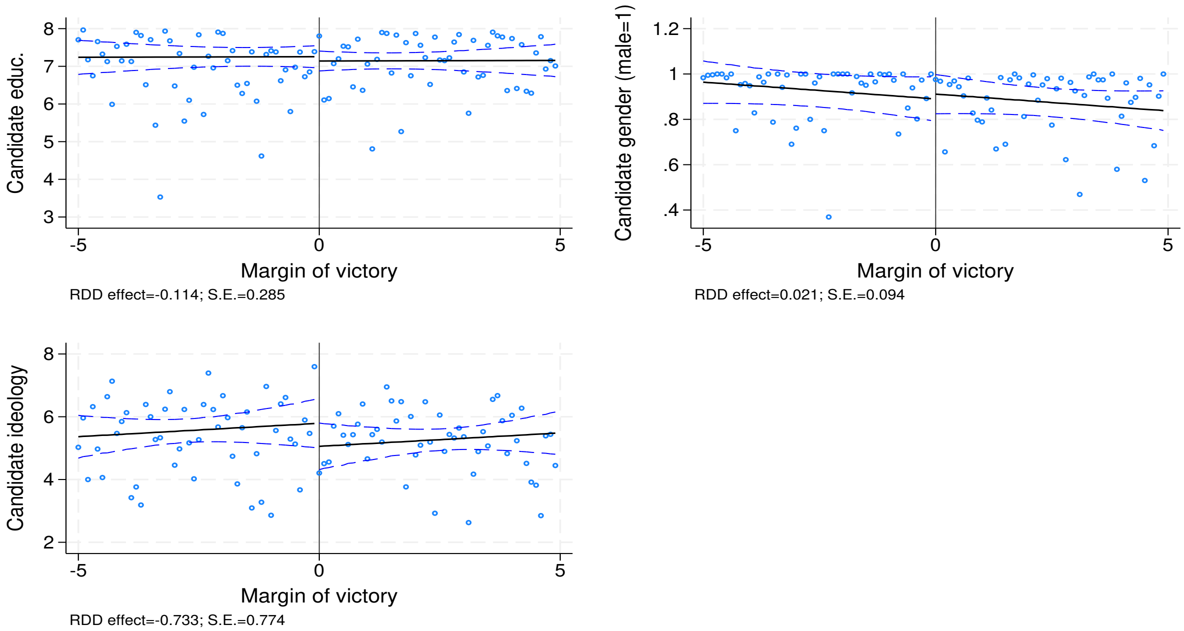
Notes: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: age, gender (1=male, 0=female), primary education dummy, secondary education dummy, college education dummy, post-graduate education dummy. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for a linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.2: RD balance tests on Campaign Workers' Characteristics (II)



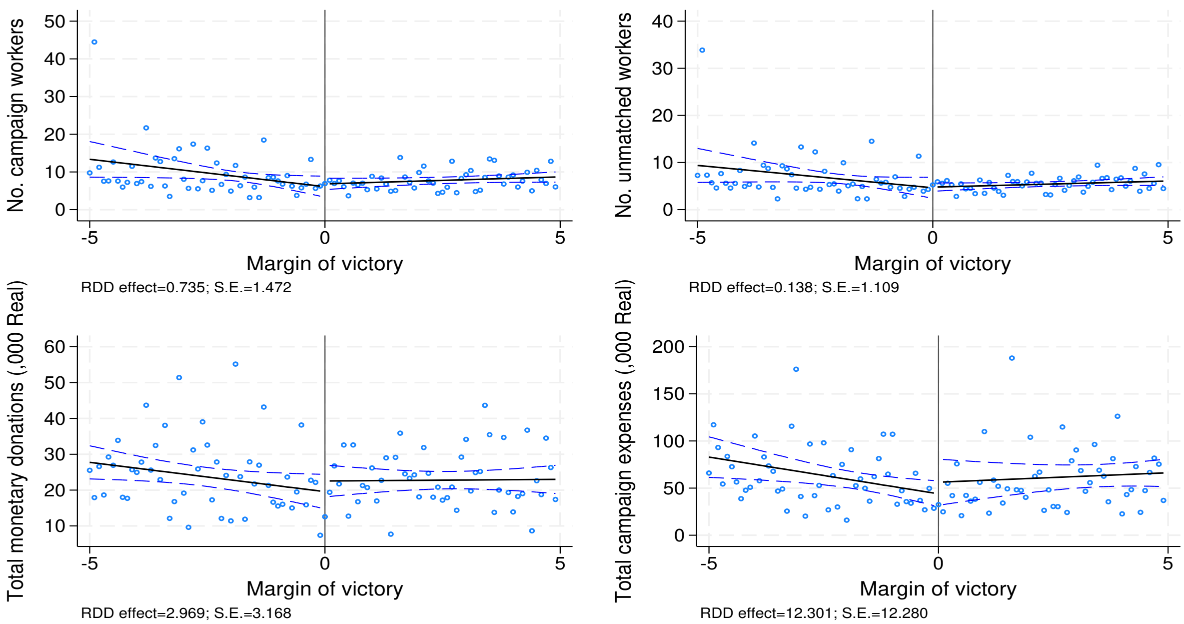
Notes: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: annual earnings, contractual weekly hours, a dummy variable for being employed in the public sector, a dummy variable for being employed formally, payment amount received (for contractual workers only). The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.3: RD balance tests on Candidate Characteristics



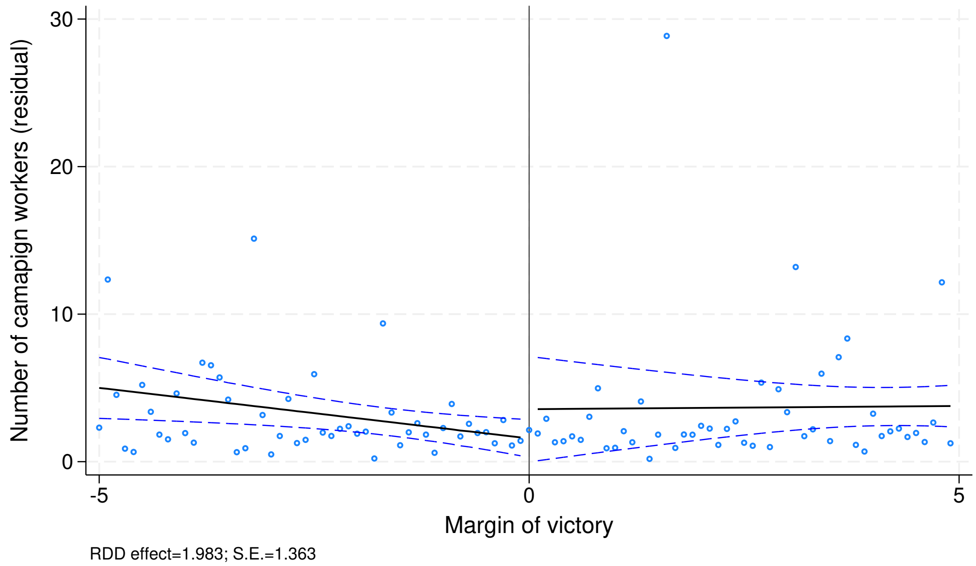
Notes: Moving from the top left to the bottom right figure we consider the following outcome variables measured in the election year: candidate education, candidate gender (male=1), candidate ideology. Candidate education in TSE data is classified as follows: (1) Illiterate; (2) Literate; (3) Incomplete Elementary School; (4) Complete Elementary School; (5) Incomplete High School; (6) Complete High School; (7) Incomplete Higher Education; (8) Complete Higher Education. The ideology index ranges from 1 to 10 with 1 being the extreme left and 10 the extreme right (see Power and Zucco, 2012, for details). The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.4: RD balance tests on Campaign Characteristics



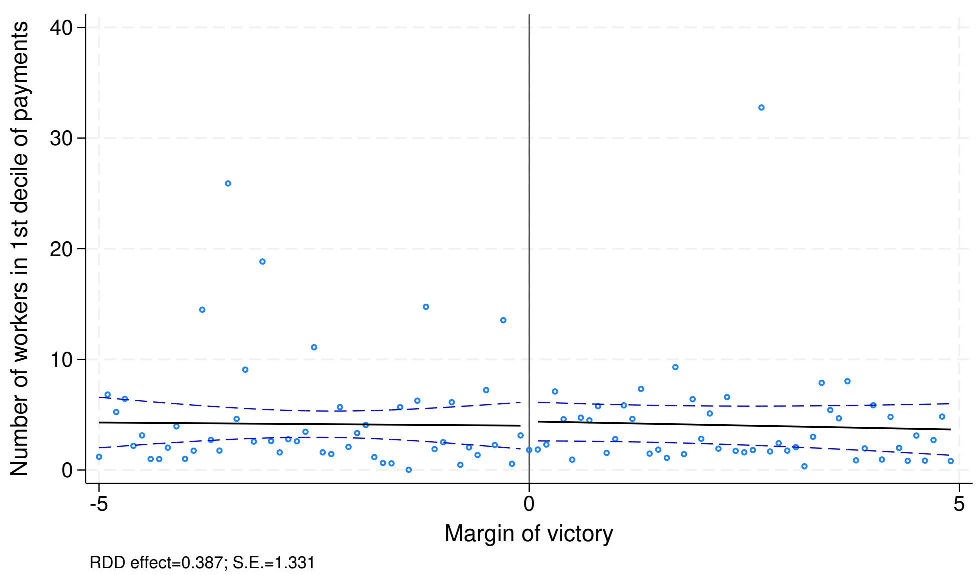
Notes: Moving from the top left to the bottom right figure we consider the following outcome variables which are measured at the campaign level: Number of workers employed by the campaign including those who cannot be matched to RAIS, number of contractual workers who are employed by the campaign and that cannot be matched to RAIS, total amount of monetary donations and campaign payments measured in thousands of Real. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.5: RD balance tests on Density of Workers



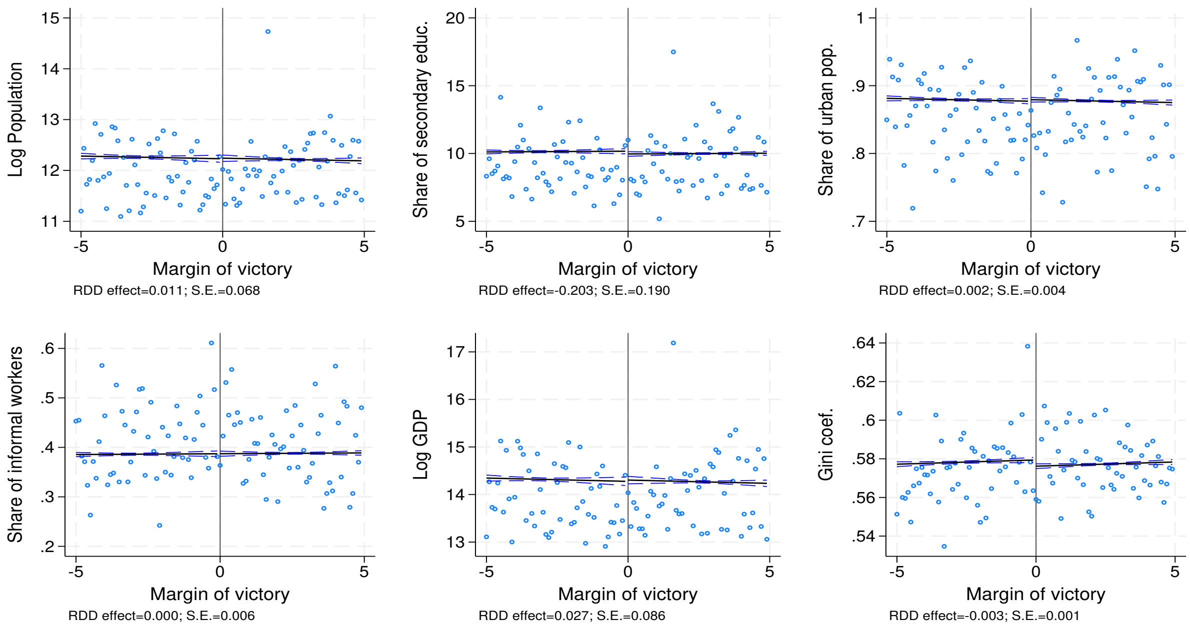
Notes: In this Figure the outcome variable is the residual number of workers in a campaign in the election year or prior years. Residuals are computed by regressing the number of workers per campaign-year on individual, year, and municipality-period fixed effects, then averaging within each campaign-period (year since the election). RDD effects are estimated by regressing the outcome on an indicator for being connected to a winning candidate, controlling for linear functions of the running variable on either side of the cutoff. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. We consider only workers matched to RAIS who are observable before the election. Standard errors are clustered at the municipality level. See also Appendix Table A.11 for more details on this test.

Figure A.6: RD balance tests on Number of Small Payments Workers



Notes: In this Figure we consider as outcome variable the number of contractual workers employed by the campaign who receive a payment in the first decile of the payment distribution across all campaigns of an election year. In this analysis, we consider the universe of contractual workers that are reported by TSE, including workers who cannot be matched to RAIS. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

Figure A.7: RD balance tests on Election Municipality Characteristics

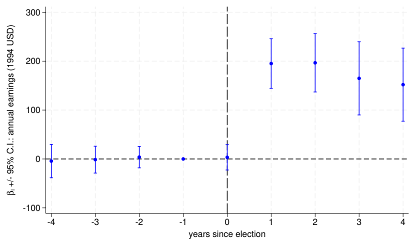


Notes: Moving from the top left to the bottom right figure we consider the following outcome variables which are measured at the municipality level: log population, share of population with secondary education, share of urban population, share of formal workers, log GDP per capita, Gini coefficient. We base these measures on 2000 Census data with the exception of informality rates which are based on Kovak (2013) 1991 Census data. The RDD effects are estimated from a regression of the outcome variable of interest on an indicator variable for being connected to a winning candidate. In this specification we control for linear functions of the running variable on each side of the cutoff, and for the interaction between municipality and election year fixed effects. We group observations in bins of 0.1% margin in length and we show 95% confidence intervals around the estimated effects. Standard errors are clustered at the municipality level.

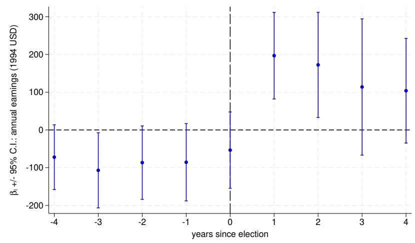
Figure A.8: Restricted Sample to Periods around Election

(a) Annual Earnings

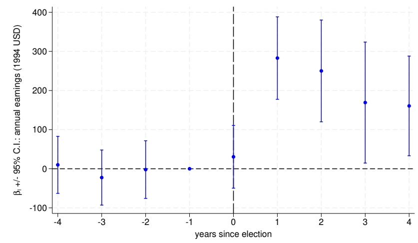
(i) DID



(ii) RD

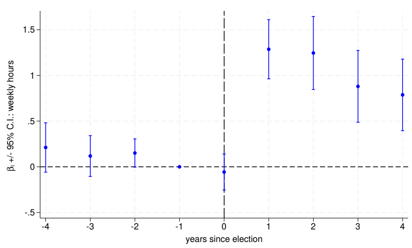


(iii) DID+RD

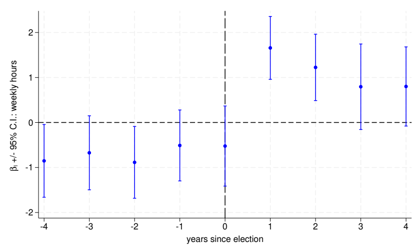


(b) Weekly Hours

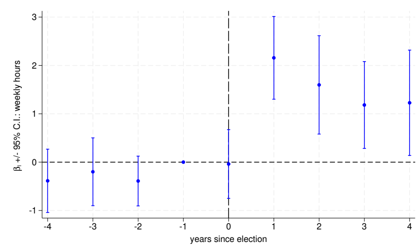
(i) DID



(ii) RD

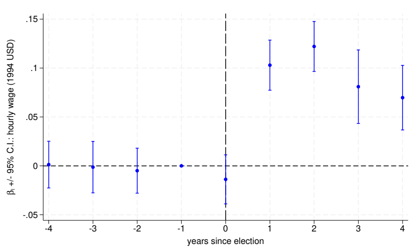


DID+RD

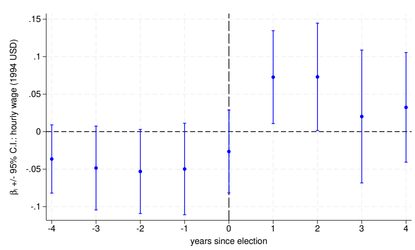


(c) Hourly Wage

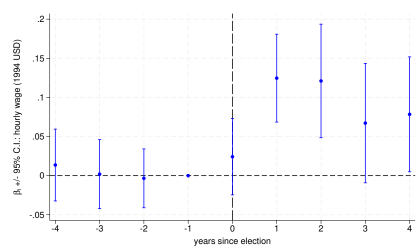
(i) DID



(ii) RD

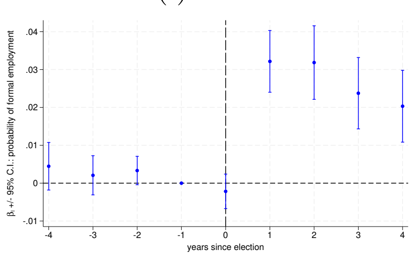


(ii) DID+RD

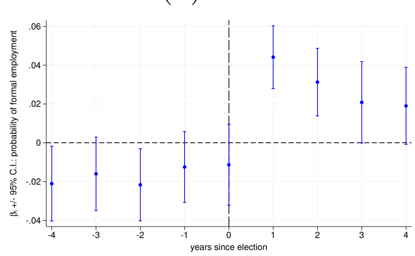


(d) Formal Employment

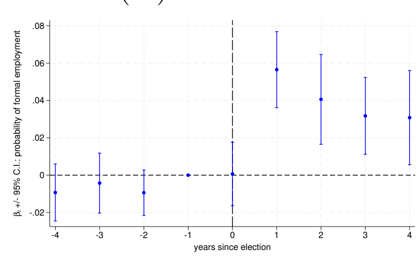
(i) DID



(ii) RD



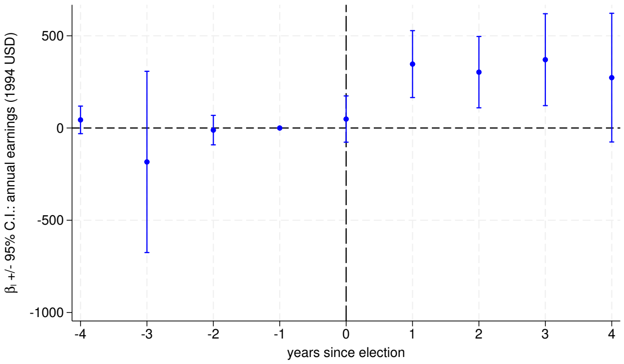
(iii) DID+RD



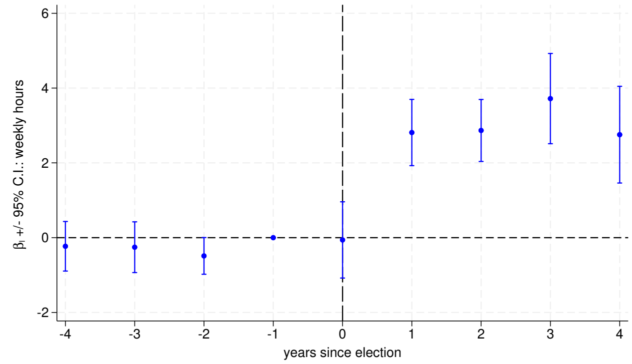
Notes: This figure re-estimates the specifications in Figure 2 including only 4 years prior and 4 years after the election year in the estimating equation. The specification is otherwise identical to that in Figure 2.

Figure A.9: DID+RD: Alternative TWFE Estimator

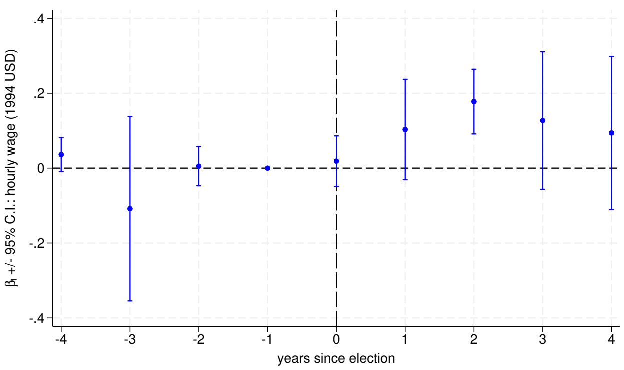
(a) Annual Earnings



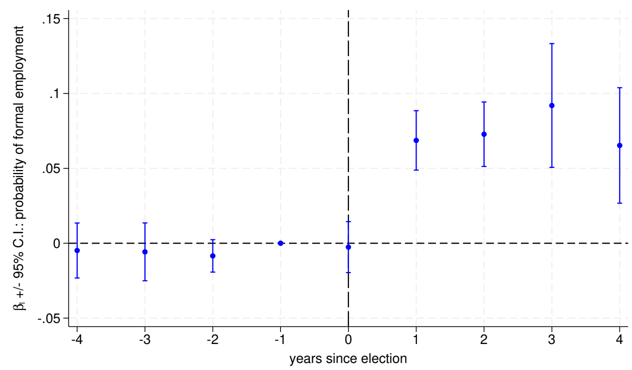
(b) Weekly Hours



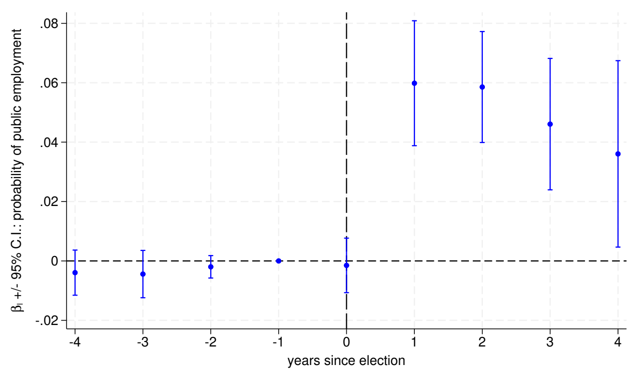
(c) Hourly Wage



(d) Formal Employment

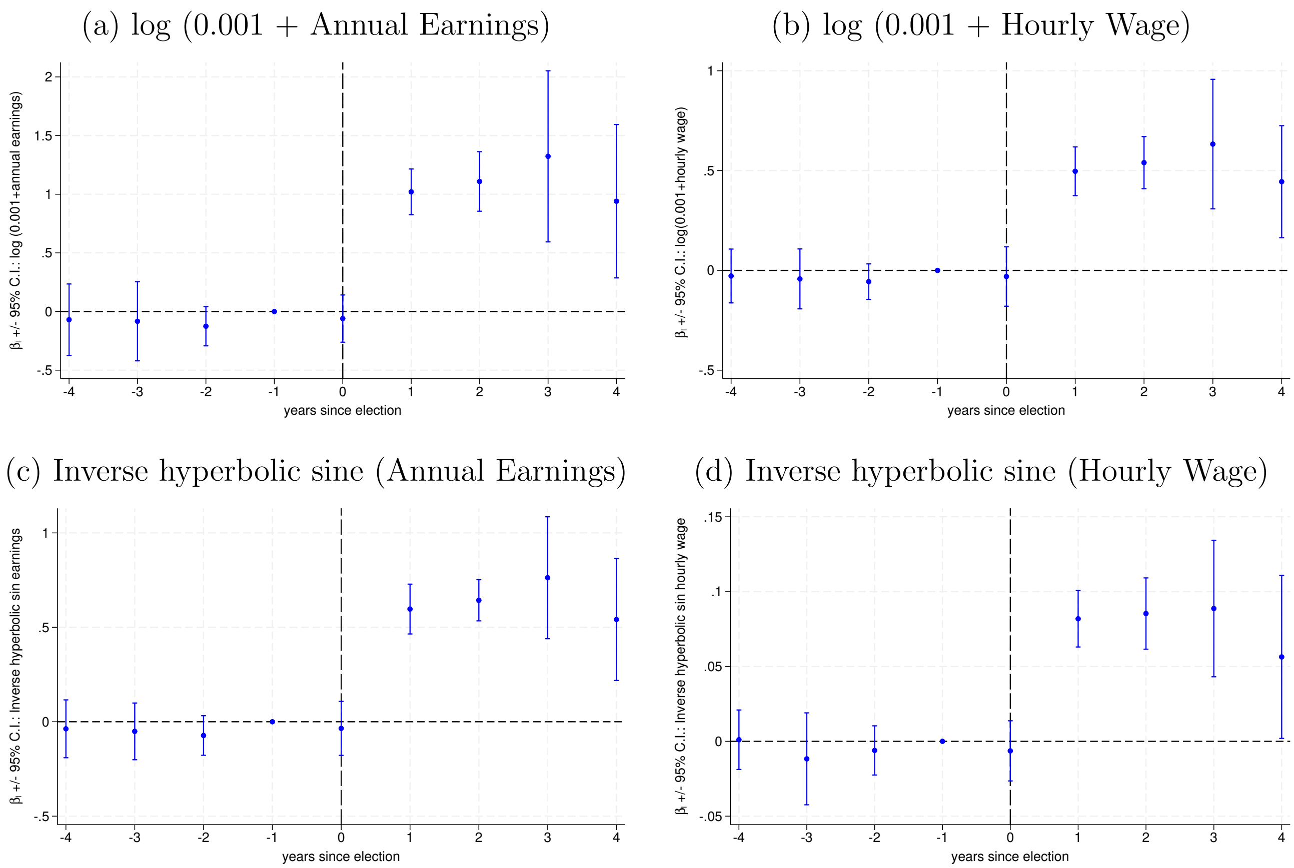


(e) Public Employment



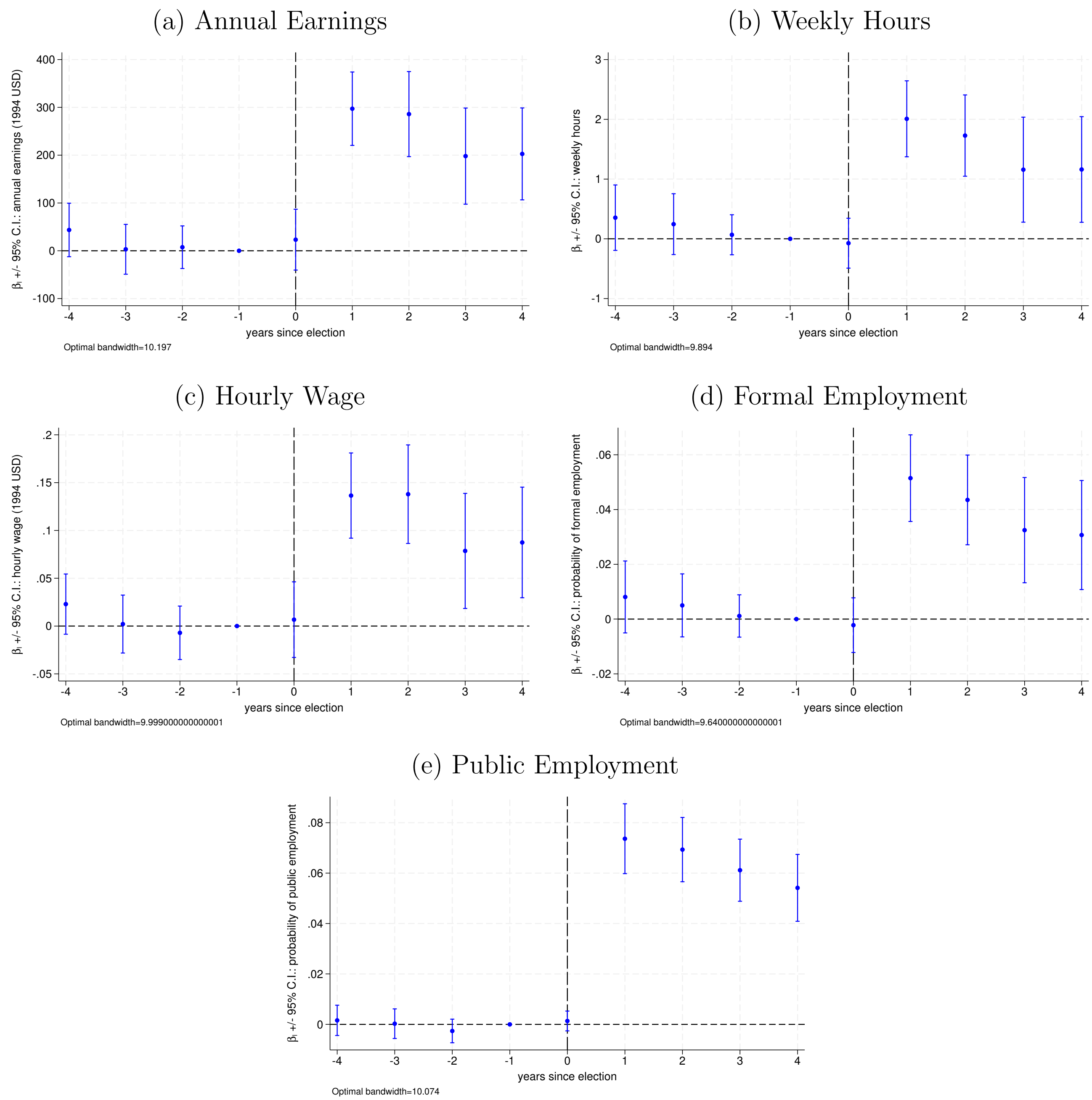
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the estimator based on De Chaisemartin and d'Haultfoeuille (2020).

Figure A.10: DID+RD: Alternative TWFE Estimator and Functional Form



Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the estimator based on De Chaisemartin and d'Haultfoeuille (2020). In panel (a) we use the log of 0.001 plus annual earnings as the dependent variable; in panel (b) we use log of 0.001 plus hourly wages as the dependent variable; in panel (c) we use the inverse hyperbolic sin of annual earnings as the dependent variables; in panel (d) we use the inverse hyperbolic sin of hourly wages as the dependent variables. The specification is otherwise identical to that in Figure A.9.

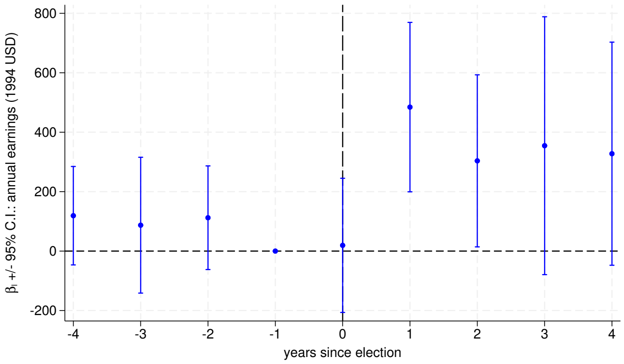
Figure A.11: DID+RD: Close Election within the Optimal Bandwidth



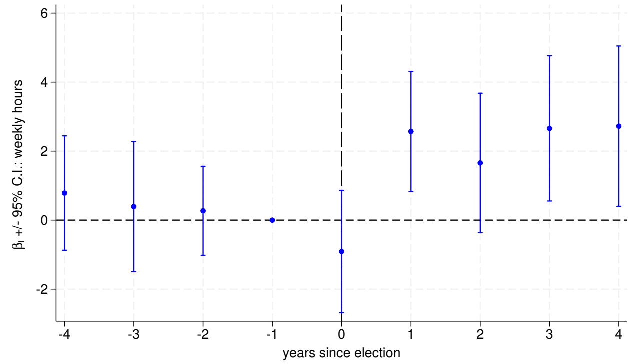
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using the optimal bandwidth based on Calonico et al. (2015). The specification is otherwise identical to that in Figure 2.

Figure A.12: DID+RD: Close Election within a 1% Margin

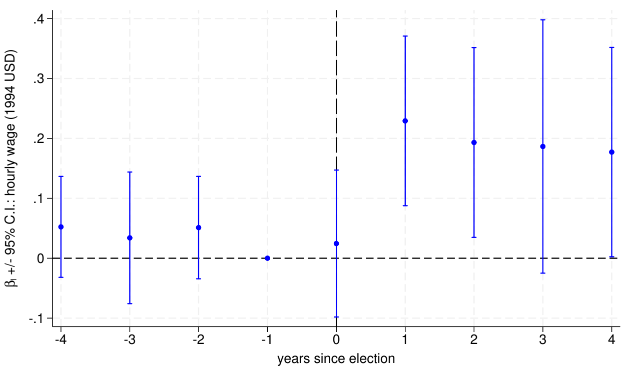
(a) Annual Earnings



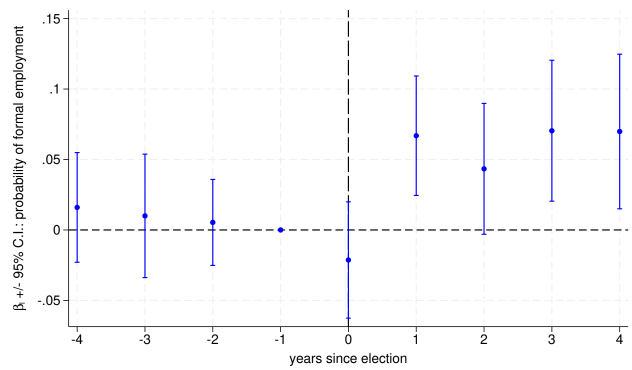
(b) Weekly Hours



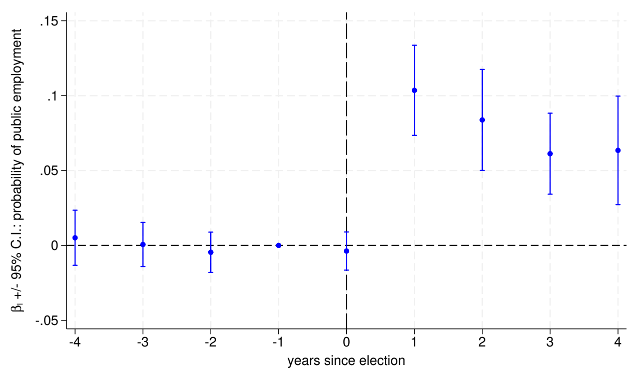
(c) Hourly Wage



(d) Formal Employment

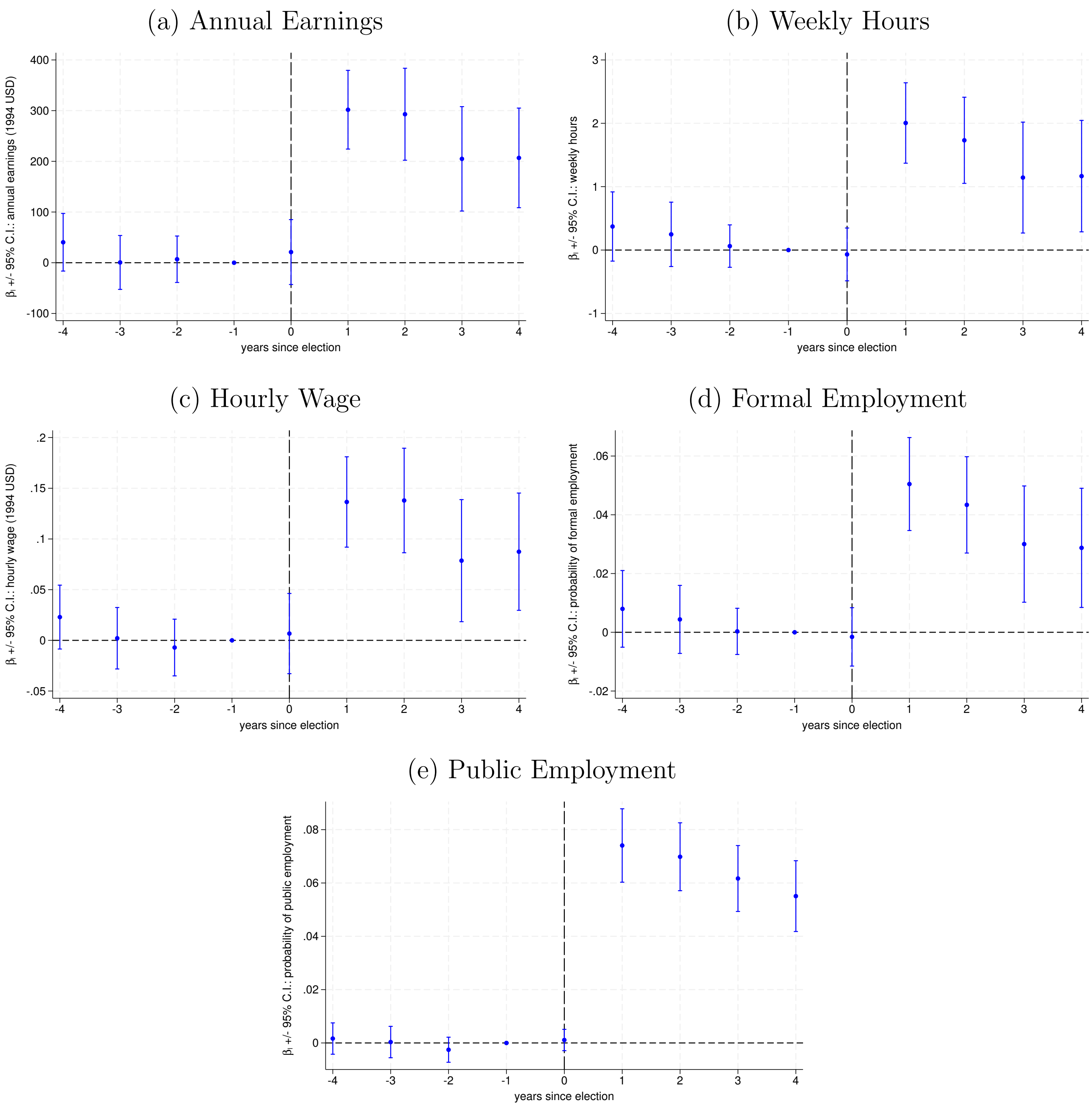


(e) Public Employment



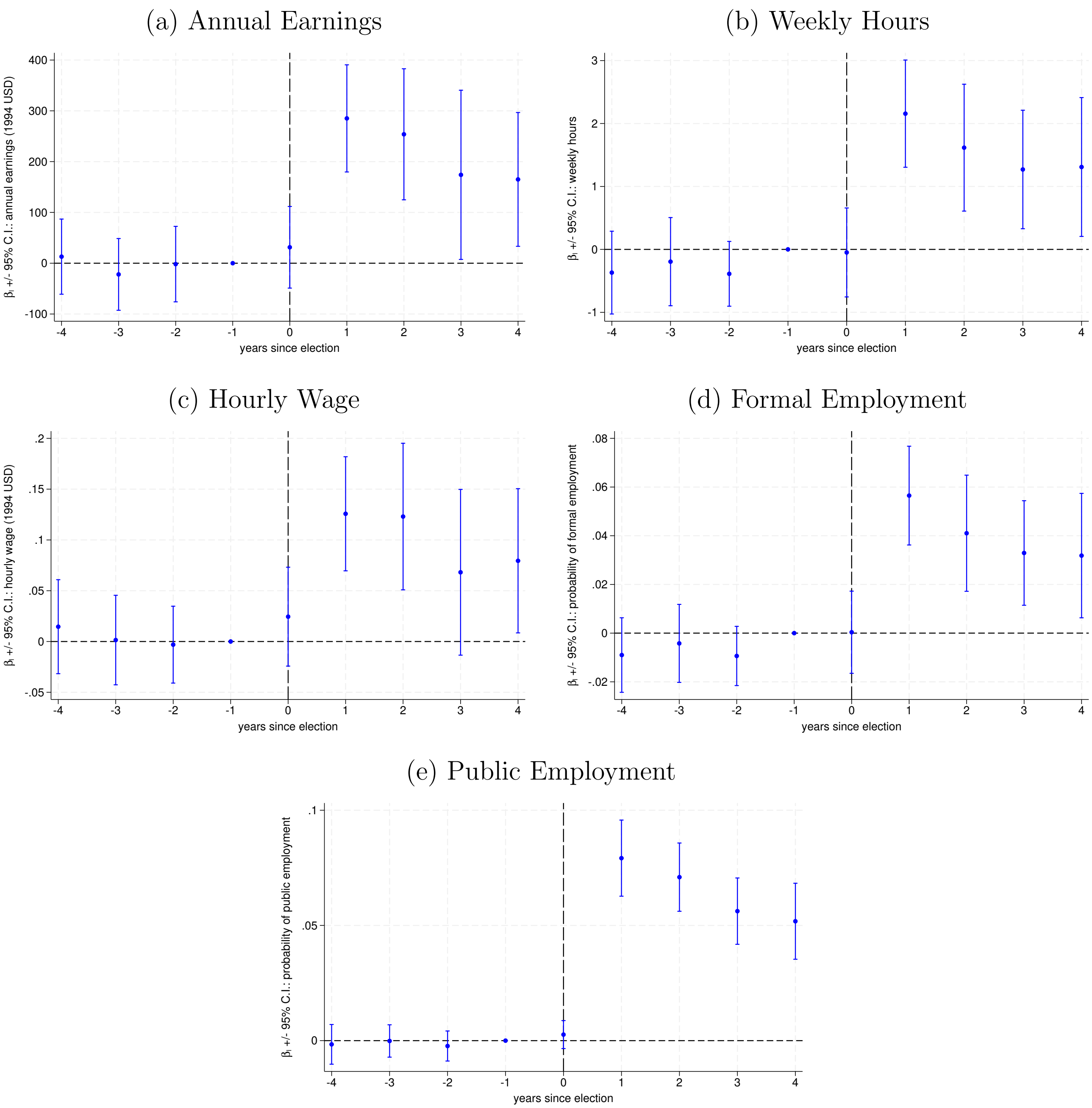
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using a 1% victory margin bandwidth instead of 5%. The specification is otherwise identical to that in Figure 2.

Figure A.13: DID+RD: Close Election within a 10% Margin



Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using a 10% victory margin bandwidth instead of 5%. The specification is otherwise identical to that in Figure 2.

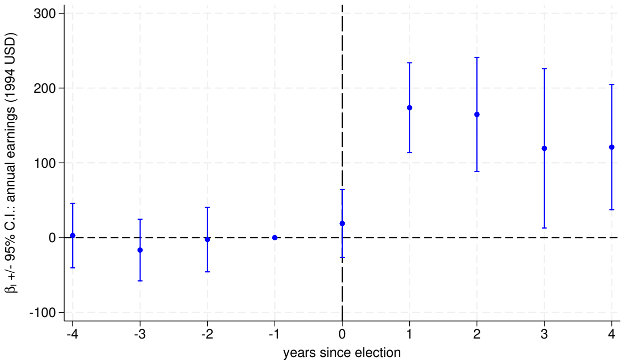
Figure A.14: DID+RD: Two-way Clustering on Municipality and Worker



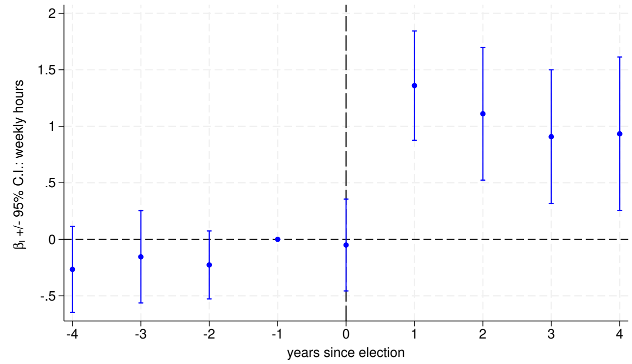
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 using two-way clustering by election municipality and worker. The specification is otherwise identical to that in Figure 2.

Figure A.15: DID+RD: Sample that Includes Unmatched CPF Numbers

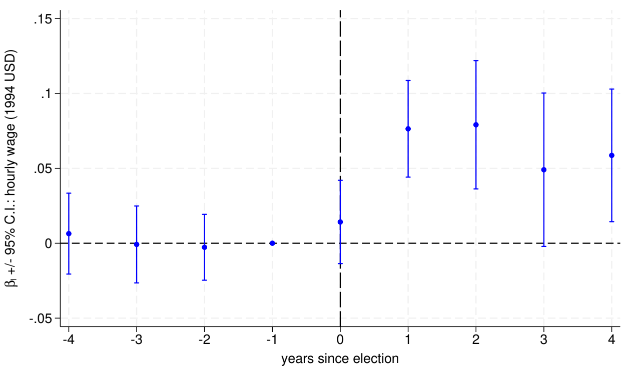
(a) Annual Earnings



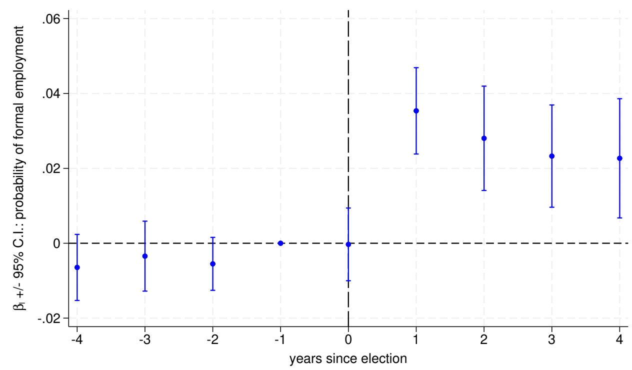
(b) Weekly Hours



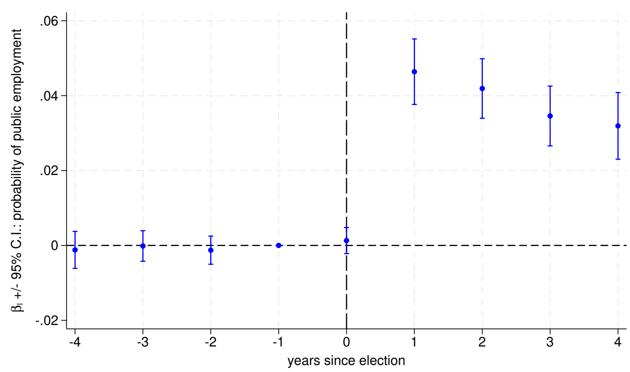
(c) Hourly Wage



(d) Formal Employment



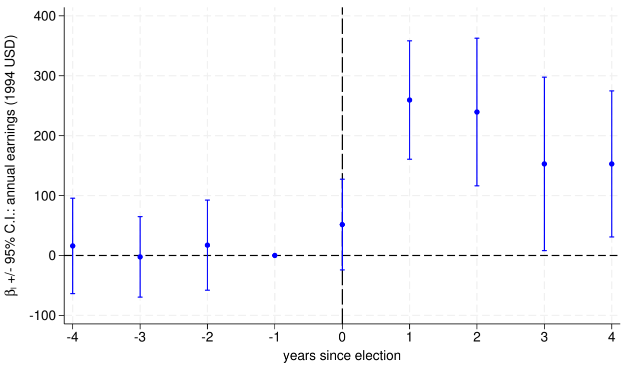
(e) Public Employment



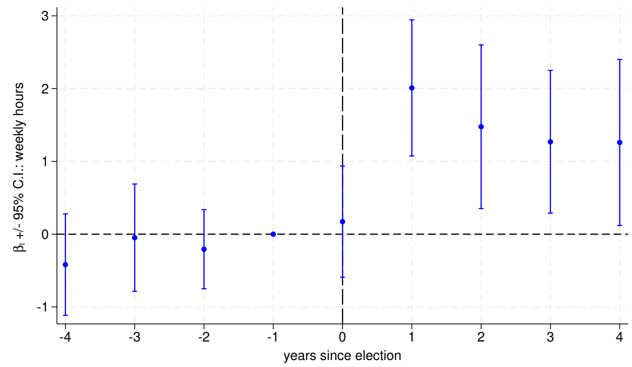
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 including all workers who were never formally employed over the study period (i.e., their CPF numbers were never observed in RAIS). The specification is otherwise identical to that in Figure 2.

Figure A.16: DID+RD: Sample that Excludes Donors

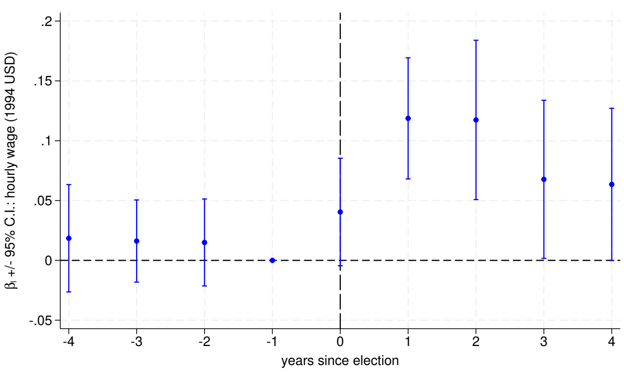
(a) Annual Earnings



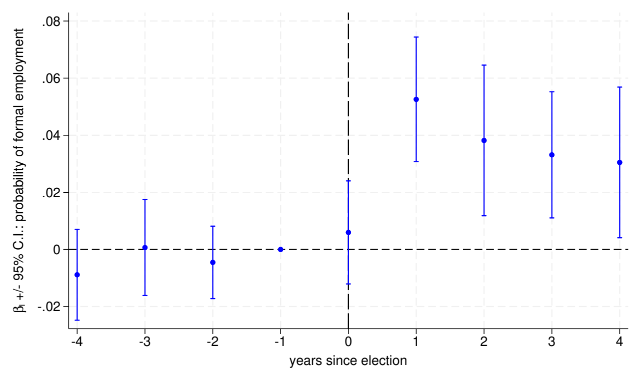
(b) Weekly Hours



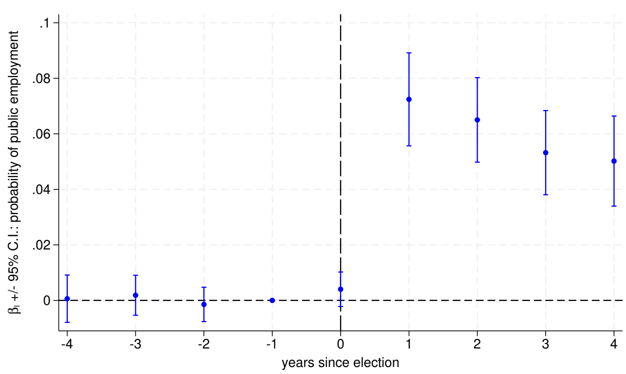
(c) Hourly Wage



(d) Formal Employment

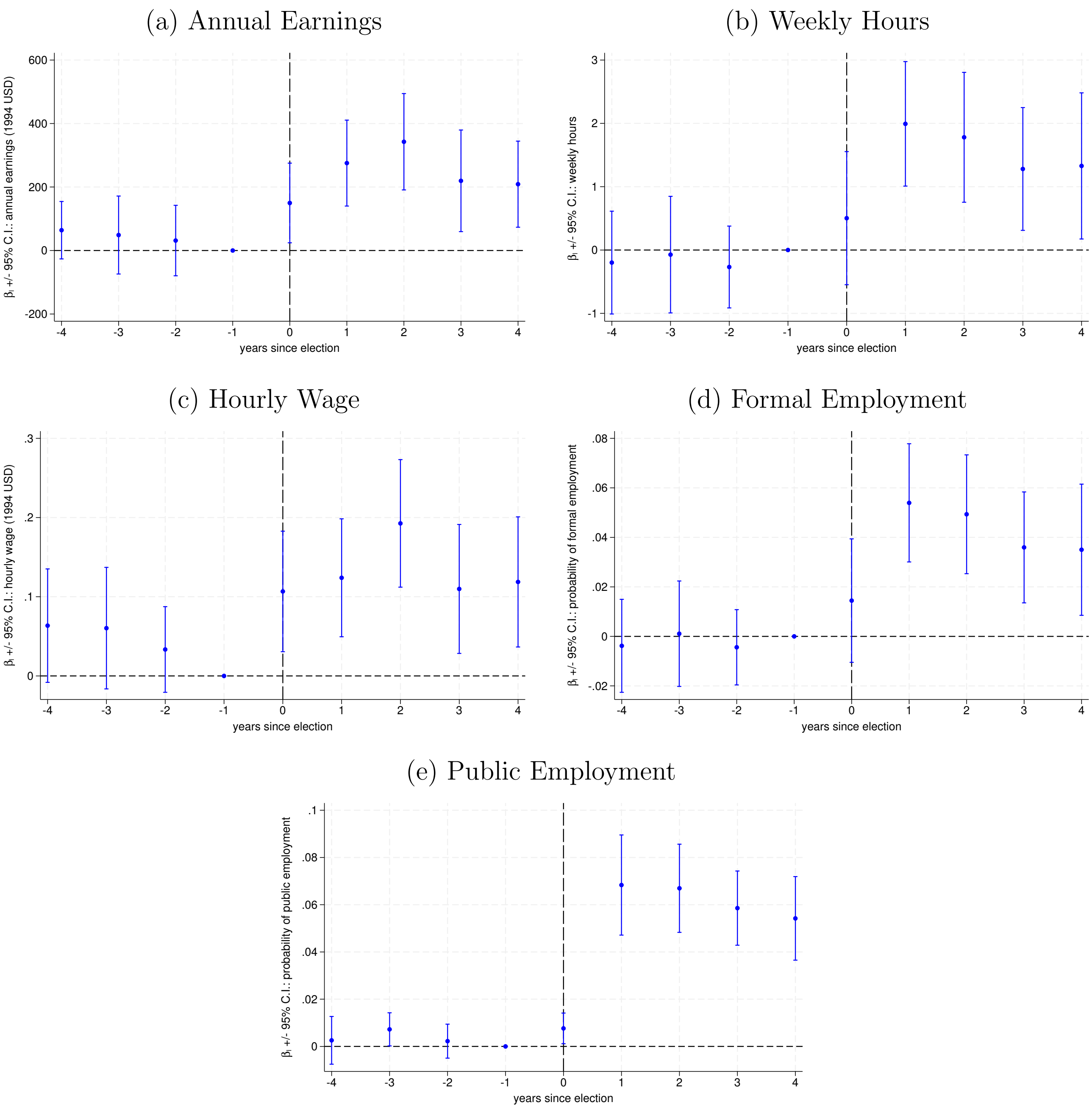


(e) Public Employment



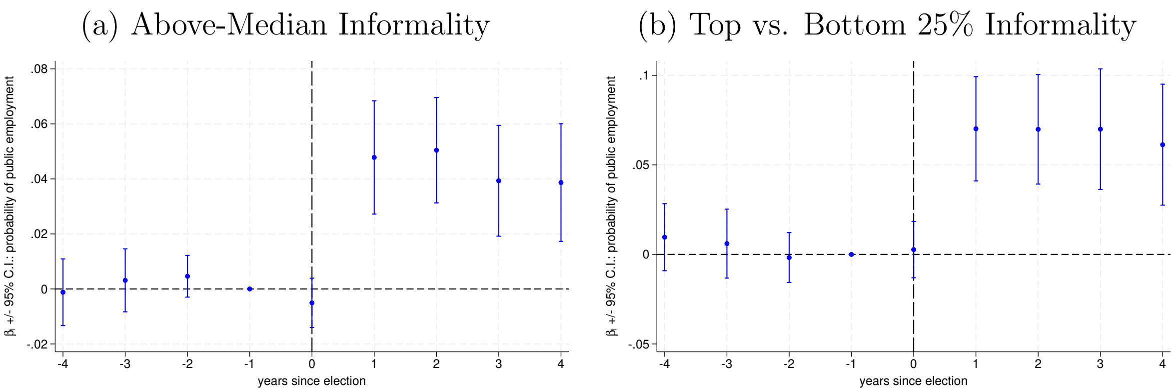
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 excluding all workers who donated to a political campaign in any of the years in our sample period. The specification is otherwise identical to that in Figure 2.

Figure A.17: DID+RD: Excluding 2012 Elections



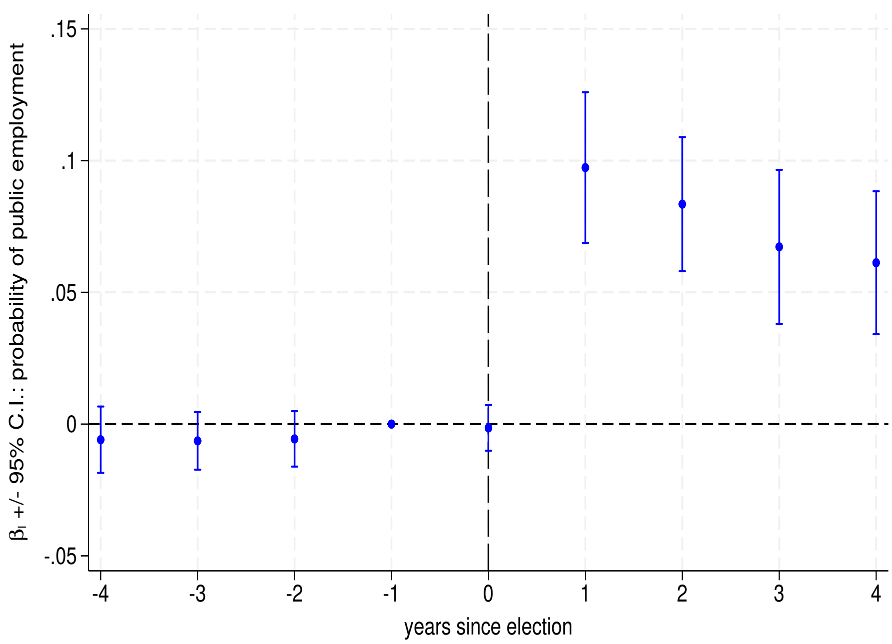
Notes: This figure re-estimates the column (iii) DID+RD specification in Figure 2 excluding workers in 2012 elections.

Figure A.18: A Pathway to Public Sector Employment amid Informality, with Additional Controls for Informality Confounds



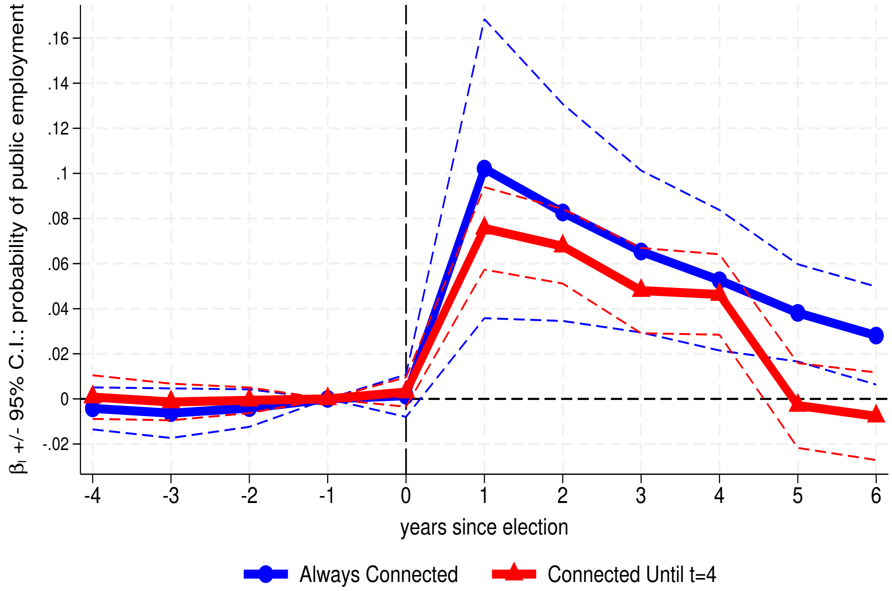
Notes: This figure re-estimates the graphs in Figure 4 including additional interactions of year FE, the win indicator, and five predetermined municipality characteristics: log population, share of the population with secondary education, share of the population in an urban area, log GDP, and the Gini coefficient.

Figure A.19: Figure 3 Re-Estimated for Challenger Connections Only



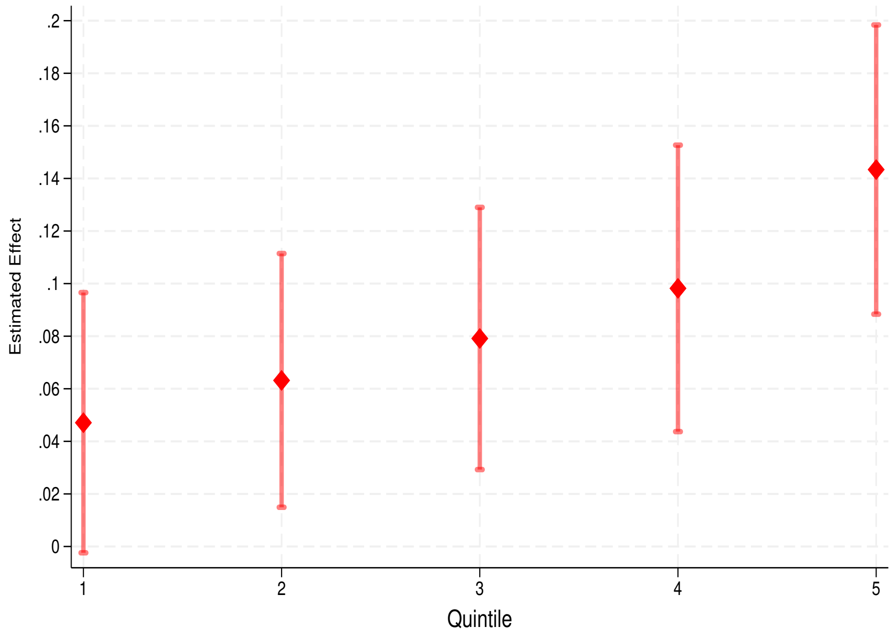
Notes: This figure re-estimate the specification in Figure 3 restricted to workers connected to candidates running in elections in which there are no incumbents running.

Figure A.20: Heterogenous Public Sector Employment Effects across Workers Connected to One-Term versus Two-Term Mayors



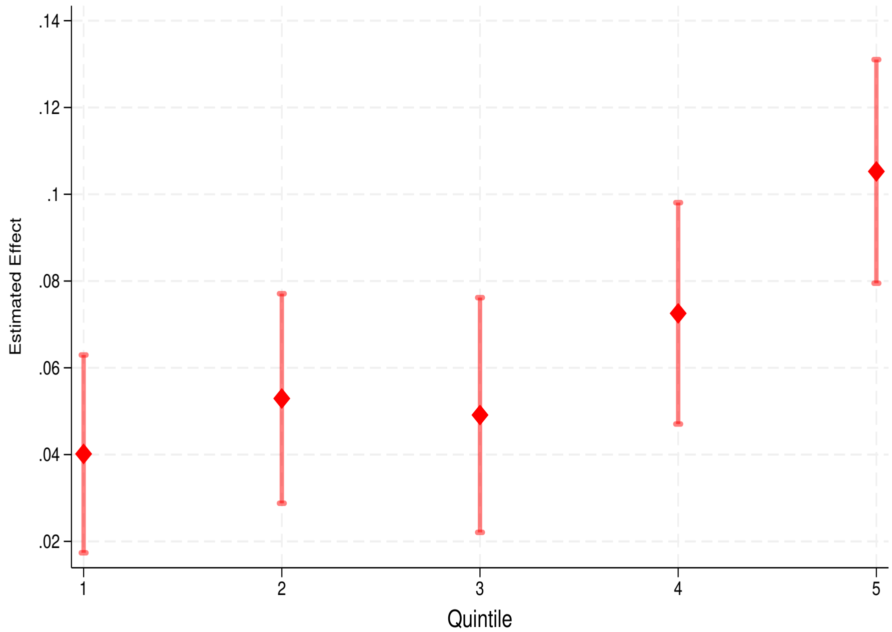
Notes: This figure re-estimates the specification in Figure 3 allowing for differential responses by workers connected to candidates who win a second election (*always connected*) versus those connected to candidates who lose the second election (*connected until* $t = 4$).

Figure A.21: Heterogeneous Effects by Amount Paid on the Campaign, with Additional Controls



Notes: This figure re-estimates the specification in Figure 7 including a set of interactions between a dummy for being connected to a winning candidate and the following controls: average total earnings in the four years before the election, the share of years the supporter was employed in the public sector in the fours years prior to the election, an indicator for high school completion, an indicator for primary school completion, age and gender. The specification is otherwise identical to Figure 7.

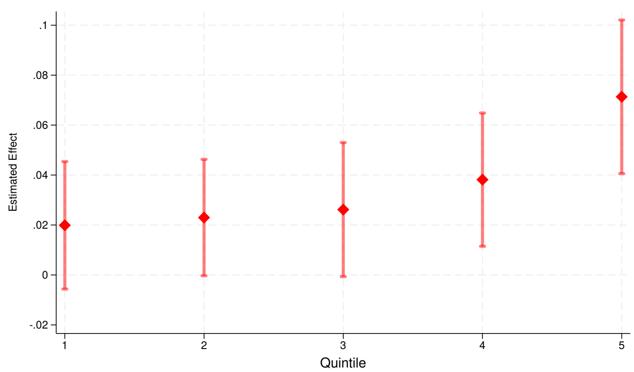
Figure A.22: Heterogeneous Effects by Amount Paid on the Campaign, Quintiles within Municipality



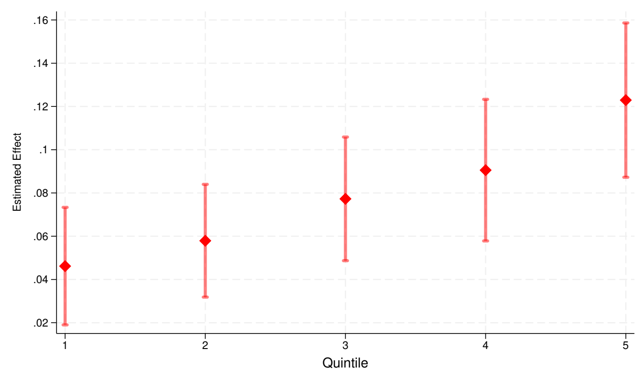
Notes: This figure re-estimates the specification in Figure 7 considering quintiles of the distribution of payments within a municipality and election year, rather than within an election year. The specification is otherwise identical to Figure 7.

Figure A.23: Heterogenous effects By Amount Paid on the Campaign: Employed vs Non-employed

(a) Employed in the election year



(b) Not employed in the election year



Notes: This figure re-estimates the specification in Figure 7, separately for contractual workers with formal employment in the election year (Panel a) and those without formal employment in the election year (Panel b). The specification remains otherwise identical to Figure 7.

A.2 Additional Tables

Table A.1: Descriptive Statistics on Elections

	(1) Elections with dedicated staff	(2) Elections with contractors	(3) Elections with both labor types	(4) Population of elections
Population (1,000 ppl)	328.042	51.404	467.271	31.257
Share adults with secondary educ.	9.399	4.109	10.614	3.312
Share urban pop.	0.858	0.667	0.906	0.588
GDP per capita (1,000 Real)	3481.482	407.661	5247.826	215.357
Gini coefficient	0.560	0.562	0.562	0.554
Turnout	0.858	0.860	0.843	0.873
Candidate Education	7.197	6.642	7.307	6.335
Share Male Candidates	0.904	0.877	0.906	0.900
Mean candidate Ideology (1-extreme left, 10 extreme right)	4.567	5.216	4.508	5.187
Winner ideology (1-extreme left, 10 extreme right)	4.864	5.366	4.944	5.421
Margin of victory	0.171	0.166	0.192	0.158
Share of incumbent candidate	0.124	0.183	0.173	0.144
# Candidates	3.357	2.929	3.558	2.679
Observations	659	4548	172	22240

Notes: Statistics on electoral district are based on 2000 Census data. Candidate education in TSE data is classified as follows: (1) Illiterate; (2) Literate; (3) Incomplete Elementary School; (4) Complete Elementary School; (5) Incomplete High School; (6) Complete High School; (7) Incomplete Higher Education; (8) Complete Higher Education. The ideology index ranges from 1 to 10 with 1 being the extreme left and 10 the extreme right (see Power and Zucco, 2012 for details). In this table first and second rounds of elections are considered as different elections.

Table A.2: Additional Descriptive Statistics on Relevant Variables

	Mean	SD	N
Percentiles of informality rate, campaign payments and private earnings			
75th percentile informality rate	0.748	–	3,926
25th percentile informality rate	0.405	–	3,926
Median informality rate	0.596	–	3,926
20th percentile of campaign payments (1994 USD)	55.695	8.434	2,844,062
40th percentile of campaign payments (1994 USD)	101.040	18.336	2,844,062
60th percentile of campaign payments (1994 USD)	159.122	28.834	2,844,062
80th percentile of campaign payments (1994 USD)	293.890	50.482	2,844,062
1st tercile of private earnings 4 years before the election	1984.499	516.755	415,853
1st tercile of private earnings 2 years before the election	2537.823	680.933	411,746
2nd tercile of private earnings 4 years before the election	2668.451	459.852	516,204
2nd tercile of private earnings 2 years before the election	3354.560	826.745	511,400
Share of public sector campaign workers in			
Professional or Managerial Occupations	0.207	–	–
Technical or Supervisory Occupations	0.502	–	–
Other White Collar Occupations	0.103	–	–
Blue Collar Occupations	0.188	–	–
Occupation requiring middle school	0.167	–	–
Occupation requiring high school	0.312	–	–
Occupation requiring college degree	0.121	–	–
Share of public sector workers in			
Professional or Managerial Occupations	0.342	–	–
Technical or Supervisory Occupations	0.415	–	–
Other White Collar Occupations	0.090	–	–
Blue Collar Occupations	0.153	–	–
Occupation requiring middle school	0.108	–	–
Occupation requiring high school	0.227	–	–
Occupation requiring college degree	0.214	–	–

Notes: This table presents additional descriptive statistics for variables included in the analysis. Informality rates are based on data from the 1991 Census. Percentiles of campaign payments are calculated separately for each election year and then adjusted to 1994 USD to ensure comparability across years. The table reports the average and standard deviation across election years. Terciles of private earnings are derived from residuals obtained from a regression of earnings (in 1994 USD) on period-by-municipality fixed effects. For this table, the residuals were re-centered by adding the constant term estimated in these regressions. The table reports the average and standard deviation of the within-campaign terciles of the re-centered residuals. The share of workers in occupations requiring middle school, high school, or college education refers to those employed in occupations requiring exactly these levels. The overall scale of required education in public sector jobs ranges from 1 to 10, which explains why the shares presented in the table do not sum to one.

Table A.3: DID+RD Specification that Includes Unmatched CPF Numbers

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	153.728***	167.734***	153.366***	156.138***	155.346***
	(29.787)	(29.985)	(29.781)	(35.412)	(36.658)
Mean D.V. post-election, runner-up	830.123	830.123	830.123	830.123	830.123
Observations	1,004,823	1,004,823	1,004,823	1,004,823	1,004,823
	Panel (b): Weekly Hours				
Post-election × Mayor	1.271***	1.481***	1.280***	1.213***	1.194***
	(0.209)	(0.203)	(0.210)	(0.225)	(0.226)
Mean D.V. post-election, runner-up	11.450	11.450	11.450	11.450	11.450
Observations	1,004,823	1,004,823	1,004,823	1,004,823	1,004,823
	Panel (c): Hourly Wage				
Post-election × Mayor	0.067***	0.073***	0.066***	0.074***	0.071***
	(0.015)	(0.015)	(0.015)	(0.018)	(0.020)
Mean D.V. post-election, runner-up	0.403	0.403	0.403	0.403	0.403
Observations	1,004,823	1,004,823	1,004,823	1,004,823	1,004,823
	Panel (d): Formal Employment				
Post-election × Mayor	0.032***	0.037***	0.032***	0.031***	0.030***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Mean D.V. post-election, runner-up	0.273	0.273	0.273	0.273	0.273
Observations	1,004,823	1,004,823	1,004,823	1,004,823	1,004,823
	Panel (e): Public Employment				
Post-election × Mayor	0.041***	0.041***	0.041***	0.042***	0.041***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Mean D.V. post-election, runner-up	0.046	0.046	0.046	0.046	0.046
Observations	1,004,823	1,004,823	1,004,823	1,004,823	1,004,823
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table re-estimates Table 2 including all workers who were never formally employed over the study period (i.e. their CPF numbers were never observed in RAIS). See the notes below Table 2 for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: DID+RD Specification that Excludes 2012 Elections

	(1)	(2)	(3)	(4)	(5)
	Panel (a): Earnings				
Post-election × Mayor	204.889***	223.559***	200.717***	220.244***	222.692***
	(65.815)	(65.361)	(66.553)	(83.164)	(85.770)
Mean D.V. post-election runner-up	1429.687	1429.687	1429.687	1429.687	1429.687
Observations	355,880	355,880	355,880	355,880	355,880
	Panel (b): Weekly Hours				
Post-election × Mayor	1.647***	1.929***	1.668***	1.512***	1.530***
	(0.404)	(0.391)	(0.403)	(0.454)	(0.447)
Mean D.V. post-election runner-up	20.616	20.616	20.616	20.616	20.616
Observations	355,880	355,880	355,880	355,880	355,880
	Panel (c): Hourly Wage				
Post-election × Mayor	0.084**	0.092***	0.080**	0.107**	0.105**
	(0.033)	(0.032)	(0.033)	(0.042)	(0.045)
Mean D.V. post-election runner-up	0.697	0.697	0.697	0.697	0.697
Observations	355,880	355,880	355,880	355,880	355,880
	Panel (d): Formal Employment				
Post-election × Mayor	0.043***	0.050***	0.044***	0.041***	0.041***
	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
Mean D.V. post-election runner-up	0.491	0.491	0.491	0.491	0.491
Observations	355,880	355,880	355,880	355,880	355,880
	Panel (e): Public Employment				
Post-election × Mayor	0.058***	0.058***	0.057***	0.058***	0.059***
	(0.009)	(0.009)	(0.009)	(0.008)	(0.008)
Mean D.V. post-election runner-up	0.081	0.081	0.081	0.081	0.081
Observations	355,880	355,880	355,880	355,880	355,880
Individual FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
Margin×period FE	yes	yes	yes	yes	yes
Municipality×period FE	yes	no	yes	yes	yes
Municipality×election year×period FE	no	yes	no	no	no
Margin×period FE×Mayor	no	no	yes	no	no
Quadratic Margin×period FE×Mayor	no	no	no	yes	no
Cubic Margin×period FE×Mayor	no	no	no	no	yes

Notes: This table re-estimates Table 2 excluding workers in 2012 elections. See the notes below Table 2 for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.5: Breakdown of the Effects on Public Sector Employment by Occupation

	(1) Professional or Managerial	(2) Technical or Supervisory	(3) Other White Collars	(4) Blue Collars
Post-election × Mayor	0.014***	0.036***	0.007***	0.012***
	(0.003)	(0.004)	(0.002)	(0.003)
Mean D.V. post-election, runner-up	0.023	0.040	0.009	0.020
Observations	559,230	559,230	559,230	559,230

Notes: This table present a breakdown of the effects on public sector employment by occupation. We group workers in the public sector in four categories of occupations as described in Appendix B.3. Thus we consider the following outcome variables: probability of working in the public sector in professional or managerial occupations (column 1), probability of working in the public sector in technical or supervisory occupations (column 2), probability of working in the public sector in other white collar occupations (column 3), probability of working in the public sector in blue collar occupations (column 4). For each outcome variable, the table shows the coefficient estimated from the interaction between a dummy for being connected to a winning candidate and a post-election dummy that takes value 1 in periods (years relative to the election) 1 to 4. This specification otherwise follows the baseline from column 1 of Table 2 and includes the following controls: the interaction between post-election dummy and a dummy for being connected to the winning candidate (which is labelled as "Mayor" in the table), individual fixed effects, year fixed effects, the interactions between period (year relative to the election) and municipality fixed effects, and the interactions between margin of victory and period fixed effects. We impute a zero probability of employment for workers who are not observed in RAIS in a given year; we restrict to workers who are linked to elected or runner-up mayoral candidates in close election (i.e. 5% victory margin bandwidth); we estimate effects in the 8-years window around the election. Standard errors that are clustered at the municipality level. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.6: Individual-Level Selection in High- versus Low-Informality Municipalities

	(1) High Informality Private earnings 4 years before the election	(2) Low Informality Private earnings 4 years before the election	(3) High Informality Private earnings 2 years before the election	(4) Low Informality Private earnings 2 years before the election
	Panel A: High-Low Informality Split – Above- vs. Below-Median			
Mayor × Tercile 3	0.009	0.026***	0.007	0.019***
	(0.008)	(0.005)	(0.008)	(0.005)
Mayor × Tercile 2	0.017**	0.018***	0.010	0.010**
	(0.008)	(0.004)	(0.009)	(0.005)
Mayor × Tercile 1	-0.006	0.006	-0.005	-0.002
	(0.011)	(0.005)	(0.011)	(0.004)
Mayor	0.074***	0.041***	0.076***	0.045***
	(0.017)	(0.015)	(0.017)	(0.014)
Mean y pre-treatment	0.078	0.078	0.078	0.078
Mean D.V. post-treatment runner-up	0.085	0.085	0.085	0.085
Observations	120,003	353,230	120,003	353,230
	Panel B: High-Low Informality Split – Top-25%- vs. Bottom-25%			
Mayor × Tercile 3	0.029*	0.028***	0.013	0.023***
	(0.016)	(0.005)	(0.018)	(0.005)
Mayor × Tercile 2	0.014	0.026***	0.005	0.016***
	(0.018)	(0.004)	(0.022)	(0.005)
Mayor × Tercile 1	-0.020	0.011**	-0.044**	0.003
	(0.021)	(0.005)	(0.022)	(0.005)
Mayor	0.103***	0.045***	0.110***	0.050***
	(0.032)	(0.014)	(0.032)	(0.013)
Mean y pre-treatment	0.078	0.078	0.078	0.078
Mean D.V. post-treatment runner-up	0.085	0.085	0.085	0.085
Observations	35,038	307,036	35,038	307,036

Notes: This table re-estimates Table 6 for municipalities with high- versus low-informality rates following the definitions in panels (a) and (b) of Figure 4. The specification is otherwise identical to Table 6; see the notes therein for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.7: Positive Selection in Public Sector Jobs by Occupation

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election
	Panel (a): Professional or Managerial Occupations	
Mayor × Tercile 3	0.002	0.001
	(0.002)	(0.002)
Mayor × Tercile 2	0.000	0.001
	(0.002)	(0.002)
Mayor × Tercile 1	-0.003*	-0.003*
	(0.002)	(0.002)
Mayor	0.012***	0.012***
	(0.004)	(0.004)
Mean D.V. post-election, runner-up	0.020	0.020
Observations	485,337	485,337
	Panel (b): Technical or Supervisory Occupations	
Mayor × Tercile 3	0.012***	0.007**
	(0.003)	(0.003)
Mayor × Tercile 2	0.008**	0.002
	(0.003)	(0.003)
Mayor × Tercile 1	-0.003	-0.004
	(0.003)	(0.003)
Mayor	0.027***	0.028***
	(0.009)	(0.008)
Mean D.V. post-election, runner-up	0.037	0.037
Observations	485,337	485,337
	Panel (c): Other White Collar Occupations	
Mayor × Tercile 3	0.003**	0.005***
	(0.001)	(0.001)
Mayor × Tercile 2	0.004***	0.003**
	(0.001)	(0.001)
Mayor × Tercile 1	0.003**	0.001
	(0.002)	(0.001)
Mayor	0.008***	0.008***
	(0.002)	(0.002)
Mean D.V. post-election, runner-up	0.008	0.008
Observations	485,337	485,337
	Panel (d): Blue Collar Occupations	
Mayor × Tercile 3	0.003	0.000
	(0.002)	(0.002)
Mayor × Tercile 2	0.003*	0.003
	(0.002)	(0.002)
Mayor × Tercile 1	0.002	-0.000
	(0.002)	(0.002)
Mayor	0.009*	0.010**
	(0.005)	(0.005)
Mean D.V. post-election, runner-up	0.018	0.018
Observations	485,337	485,337

Notes: This table re-estimates Table 6 distinguishing between workers in four occupation groups (see also Section B.3 for details on the occupation classification). We consider workers in professional or managerial occupations in panel a; technical or supervisory occupations in panel b; other white collar occupations in panel c; blue collar occupations in panel d. The specification is otherwise identical to Table 6; see the notes therein for details. Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

Table A.8: Incumbents vs. Challenger Connections: Heterogeneity by Age, Education and Productivity

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual Has [. . .]					
	Age≤30	30<age<50	Age≥50	primary educ.	second. educ.	tertiary educ.
	Panel (a): Incumbents					
Post-election × Mayor	-0.001	0.029**	0.004	0.019	0.022*	0.007
	(0.010)	(0.011)	(0.004)	(0.011)	(0.012)	(0.005)
Mean D.V. post-election, runner-up	0.037	0.044	0.007	0.039	0.043	0.013
Observations	73,927	73,927	73,927	73,927	73,927	73,927
	Panel (b): Challengers					
Post-election × Mayor	0.038***	0.039***	0.006***	0.043***	0.038***	0.009***
	(0.004)	(0.004)	(0.002)	(0.005)	(0.004)	(0.002)
Mean D.V. post-election, runner-up	0.039	0.053	0.009	0.041	0.048	0.021
Observations	256,803	256,803	256,803	256,803	256,803	256,803
	Individual Has Pre-Election Private Sector Earnings in [. . .]					
	Tercile 1	Tercile 2	Tercile 3			
	Panel (c): Incumbents					
Post-election × Mayor	0.006*	0.004	-0.000			
	(0.004)	(0.003)	(0.005)			
Mean D.V. post-election, runner-up	0.005	0.005	0.008			
Observations	73,927	73,927	73,927			
	Panel (d): Challengers					
Post-election × Mayor	0.006***	0.010***	0.013***			
	(0.001)	(0.002)	(0.002)			
Mean D.V. post-election, runner-up	0.004	0.006	0.006			
Observations	256,803	256,803	256,803			

Notes: This table presents DID+RD estimates obtained by estimating our baseline specification of equation (1) using the probability of public employment for different types of workers as the dependent variable. We estimate separate regressions for workers who are employed by incumbents in Panels (a) and (c) and by challengers in Panels (b) and (d). The outcomes capture the probability of public employment for workers of different age (columns 1-3 in panels a and b), education (columns 4-6 in panels a and b) and levels of private sector earnings (column 1-3 in panels c and d). We group workers into age and education groups based on the maximum age and education level displayed in the year before the election. We group workers in terciles of private sector earnings based on average earning figures in the 4 years before the election. All specifications include the following controls: individual fixed effects, year fixed effects, municipality times period (years relative to the election) fixed effects and the interaction between margin of victory and period fixed effects. In all specifications, we impute zero employment for workers who are not observed in RAIS in a given year; we restrict to workers who are linked to elected or runner-up mayoral candidates; we estimate effects in the 8-years window around the election. Standard errors are clustered at the municipality level. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

A.3 Potential Mechanisms: Full Discussion

This section explores two potential mechanisms to explain the role of campaigns in shaping positive selection of campaign workers in public employment. First, we examine whether campaigns may act as screening devices to overcome imperfect information on workers' ability (Stiglitz, 1975): working on the campaign may allow politicians to assess a worker's ability and to subsequently select those workers who are best suited to take a public sector position. Second, we explore a relational contracting mechanism (e.g., Levin, 2003; MacLeod and Malcomson, 1998): working on the campaign may give rise to an implicit contract by which high-ability workers provide their time to the campaign, possibly foregoing higher pay outside the campaign during the election period, in exchange for a secure and well-paid public sector job after the election. We find stronger suggestive evidence for relational contracting.

In order to investigate the productivity screening mechanism, Appendix Table A.9 re-estimates Table 6 adding flexible controls for earnings received while on the campaign. To the extent that the campaign allows politicians to uncover workers' ability, we would expect differences in earnings on the campaign to capture relevant differences in productivity across workers. Furthermore, if the positive selection of workers in the public sector is driven by productivity differentials that are uncovered during the campaign, we would expect that such selection patterns weaken after conditioning on earnings in the campaign period. Appendix Table A.9 below shows that this is not the case: positive selection (proxied by pre-election private sector earnings) remains strong even when comparing workers with similar campaign-period earnings.

The analysis in the previous paragraph relies on the assumption that pay during the campaign serves as a proxy for revealed productivity. However, it may also or instead reflect the worker's outside option at the time of the campaign. To investigate this hypothesis, in Appendix Table A.10 we re-estimate Appendix Table A.9 controlling for proxies of outside options. Specifically, in Panel A, we flexibly control for the ratio between pay on the campaign and pay in the best-paying job, distinguishing between using all pre-election years (columns 1 and 2) versus using only the two years prior to the election to define the best-paying job (columns 3 and 4). In Panel B, we separately and flexibly control for campaign pay and earnings from the best-paying job, again distinguishing, as in Table 6, between earnings in all years prior (columns 1 and 2) and the two years prior to the election (columns 3 and 4). The pattern of selection appears even stronger than in Table A.9, further corroborating the conclusion that productivity screening is unlikely to play a major role in our setting.[1]

Campaigns may also facilitate relational contracts with benefits to workers, candidates, and newly elected mayors. In particular, capital-constrained candidates may use the promise of future high-pay jobs in the public sector as a way to grow the size and quality of their campaign workforce. The upside for newly elected mayors is that they would have a workforce from which they can draw capable workers to staff their new bureaucracy and, ideally, deliver higher-quality governance that might increase their odds of re-election. This mechanism helps explain the differential selection of higher-ability workers, as proxied by prior earnings in the private sector (Table 4 and Table 6). It also helps explain the stronger effects for challengers than for incumbents insomuch as challengers have longer horizons over which to capitalize on the relational contract.

Several additional results shed further light on a relational contracting mechanism. First,

[1] Note that Table A.9 uses a different sample, as it does not necessarily require information on prior formal earnings. When we re-estimate the specification from Table A.9 using the sample from Table A.10, we obtain very similar results. We also reach the same conclusions as in Table A.10 when conducting a comparable analysis using educational qualifications—rather than private-sector earnings—as the basis for selection.

connected workers into the public sector earn nearly USD 300 more annually than unconnected workers (Appendix Figure A.24). Second, we find evidence consistent with connected workers earning a wage premium relative to unconnected workers, where we benchmark that premium against other public sector employees working in the same municipality, occupation, and year. Figure A.25 shows that this wage ratio—of own to comparator public employees—is roughly 15 p.p. higher for connected workers than for those who worked for campaigns that narrowly lost.[2] This is a sizable differential given that unconnected workers earn, on average, 65% as much as the comparator public employees in the post-election period. Moreover, this premium is relatively larger for connected workers who worked for capital-poor candidates, measured by total monetary donations to the campaign. Compared to connected workers from the richest-50% of campaigns, those from the bottom-50% earn 8.3 p.p. higher relative wages (panel a, Figure A.27).[3] Finally, we find, descriptively, that richer campaigns pay their staff relatively more while working on the campaign (panel b, Figure A.27).

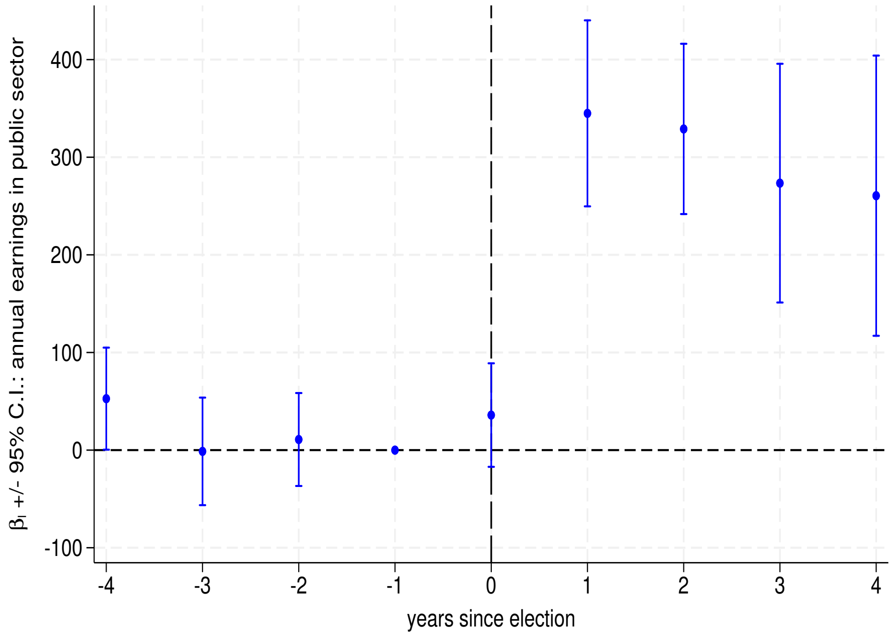
Capital-constrained candidates paying lower wages during the campaign and offering higher wage premiums after the election is consistent with an implicit relational contract (e.g., MacLeod and Malcomson, 1998). Under this view, resource-constrained politicians motivate and retain workers who earn lower wages during the campaign by promising and delivering future wage premiums and secure public sector jobs. While we cannot rule out alternative mechanisms given the data at hand,[4] the results and discussion in this section offer a novel perspective toward a broader understanding of the role of campaigns in shaping public employment.

[2] While workers who enter the public sector after the election may be a selected sample, the premium remains statistically and economically significant when we expand the analysis to all workers who find employment in the formal sector after the election (see Appendix Figure A.26.)

[3] Recall from Appendix Figure A.4 that total donations and campaign expenditures are continuous across the victory margin cutoff.

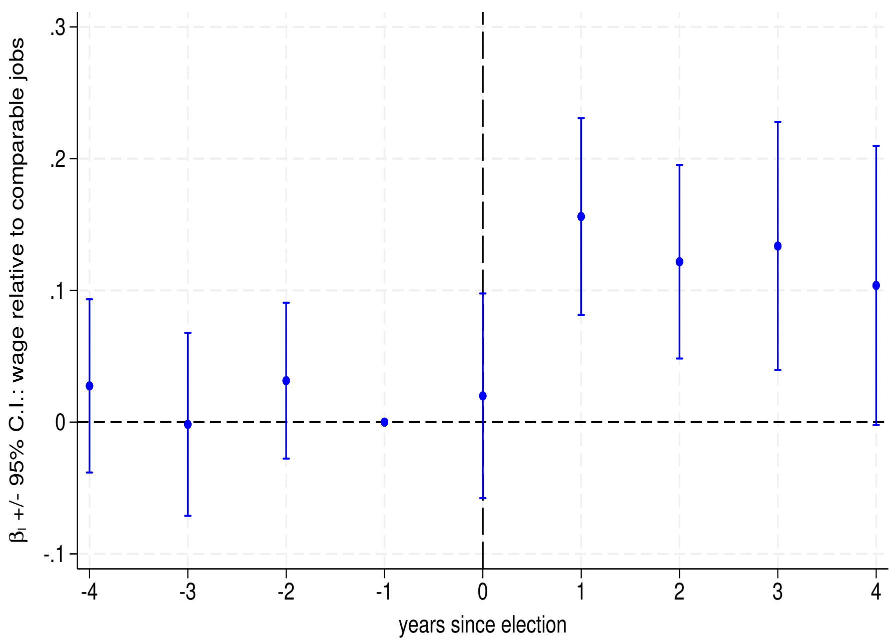
[4] In particular, the post-election wage premium for campaign workers may be consistent an efficiency wage mechanism for retaining productive municipal employees newly hired by challengers facing re-election prospects in four years (see Katz, 1986, for a review of the efficiency wage literature). Alternatively, it may reflect a reciprocity mechanism by which the premium is given as a gift to incentivize greater effort among the new civil servants (e.g., DellaVigna et al., 2022; Gneezy and List, 2006), which in turn helps the new mayor to build a stronger reputation of commitment to work. However, the fact that certain (capital-constrained) campaigns rely more heavily than others on wage premiums suggests that these theories are unlikely to fully explain our findings.

Figure A.24: Earnings in the Public Sector



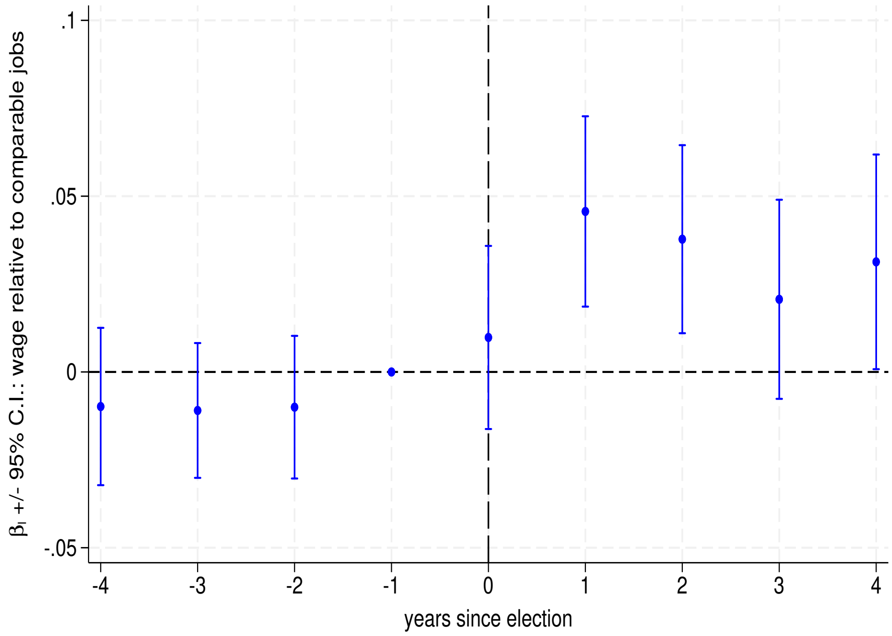
Notes: This figure re-estimate the specification in Figure 2 Panel a (iii) using earnings from the public sector (in 1994 USD) as the dependent variable. We report 95% confidence intervals based on standard errors that are clustered at the municipality level.

Figure A.25: Campaign Workers' Pay Relative to Similar Jobs in the Public Sector



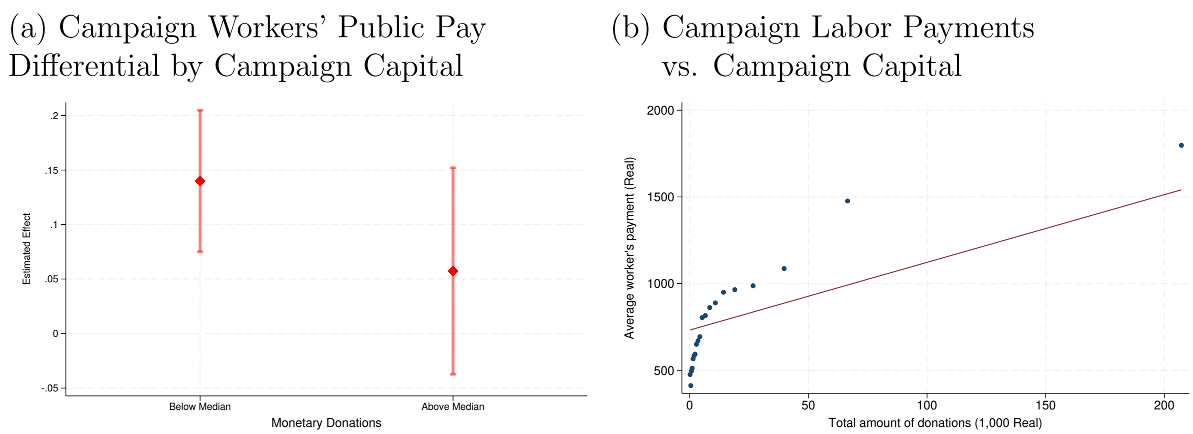
Notes: This figure re-estimates the specification in Figure 2 using as outcome variable the ratio between hourly wage and the average hourly wage among workers in the same municipality, occupation (3 digit CBO), year and sector (public vs others). We focus only on workers who are employed for at least one year in the public sector between time 1 and 4. The specification is otherwise identical to Figure 2.

Figure A.26: Campaign Workers' Pay in all Sectors Relative to Similar Jobs



Notes: This figure re-estimates the specification in Figure A.25 to include also workers who were not employed in the public sector for at least one year between time 1 and 4. The specification is otherwise identical to Figure A.25.

Figure A.27: Campaign Capital and Heterogeneous Returns to Connections



Notes: Panel (a) re-estimates the specification in Figure A.25 allowing the coefficients to vary with total donations to the campaign being above versus below the median. We report 95% confidence intervals based on standard errors that are clustered at the municipality level. Panel (b) plots the relationship between average pay received by contractual workers during the campaign and total amount of donations received by the campaign. Observations are grouped in 20 equal-sized bins based on the x-variable. For each bin we plot the average value of the observations within the bin. The linear fit line (in red) is estimated starting from the underlying data.

Table A.9: Positive Selection into the Public Sector Controlling for Earnings on the Campaign

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election
Mayor × Tercile 3	0.018***	0.013***
	(0.004)	(0.004)
Mayor × Tercile 2	0.016***	0.009**
	(0.004)	(0.004)
Mayor × Tercile 1	0.003	-0.003
	(0.004)	(0.004)
Mayor	0.048***	0.050***
	(0.012)	(0.012)
Campaign earnings × mayor × period fixed effects	yes	yes
Mean D.V. post-election runner-up	0.085	0.085
Observations	485,330	485,330

Notes: This table re-estimates Table 6 including controls for earnings received on the campaign interacted with a dummy for working in a winning campaign (mayor) and period (years relative to the election) fixed effects. The specification is otherwise identical to Table 6; see the notes therein for details. Significance levels: $*$: 10% $**$: 5% $***$: 1%.

Table A.10: Public Sector Selection Controlling for Outside Options

	(1) Private earnings 4 years before the election	(2) Private earnings 2 years before the election	(3) Private earnings 4 years before the election	(4) Private earnings 2 years before the election
Panel A: Flexible Controls for Pay on the Campaign Relative to Best-paying Job				
Mayor × Tercile 3	0.037***	0.026***	0.062***	0.048***
	(0.005)	(0.005)	(0.007)	(0.006)
Mayor × Tercile 2	0.030***	0.018***	0.056***	0.040***
	(0.005)	(0.005)	(0.006)	(0.006)
Mayor × Tercile 1	0.014***	0.003	0.042***	0.026***
	(0.005)	(0.005)	(0.007)	(0.007)
Mayor	0.034***	0.042***	0.013	0.025**
	(0.013)	(0.013)	(0.013)	(0.013)
Best-paying job in any pre-election year	Yes	Yes	No	No
Best-paying job within two years before election	No	No	Yes	Yes
Mean y pre-treatment	0.078	0.078	0.078	0.078
Mean D.V. post-treatment runner-up	0.085	0.085	0.085	0.085
Observations	344,020	344,020	262,073	262,073
Panel B: Flexible Controls for Pay on the Campaign and Highest Pre-election Pay				
Mayor × Tercile 3	0.035***	0.024***	0.062***	0.048***
	(0.005)	(0.005)	(0.007)	(0.006)
Mayor × Tercile 2	0.029***	0.017***	0.054***	0.038***
	(0.005)	(0.005)	(0.006)	(0.006)
Mayor × Tercile 1	0.015***	0.004	0.040***	0.024***
	(0.005)	(0.005)	(0.006)	(0.006)
Mayor	0.029**	0.037***	0.006	0.018
	(0.013)	(0.013)	(0.014)	(0.013)
Best-paying job in any pre-election year	Yes	Yes	No	No
Best-paying job within two years before election	No	No	Yes	Yes
Mean y pre-treatment	0.078	0.078	0.078	0.078
Mean D.V. post-treatment runner-up	0.085	0.085	0.085	0.085
Observations	345,335	345,335	265,124	265,124

Notes: This table re-estimates Table A.9, controlling for income from the best-paying job prior to the election. In Panel A, we flexibly control for the ratio of campaign pay to pay in the best-paying job, distinguishing between using all pre-election years (columns 1 and 2) and using only the two years prior to the election (columns 3 and 4) to define the best-paying job. In Panel B, we separately—and flexibly—control for campaign pay and earnings from the best-paying job, again distinguishing between earnings in all pre-election years (columns 1 and 2) and in the two years prior to the election (columns 3 and 4). Significance levels: * p<0.10, ** p<0.05, *** p<0.01.

A.4 Further Probing of the Manipulation Test

In our setting, the implementation of standard RD manipulation tests is challenging for multiple reasons. First, differently from the standard RD specification, our core regression conditions on individual, year and municipality times period fixed effects. Second, we cluster the standard errors at the municipality level. Unfortunately, standard routines to implement manipulation tests cannot easily accommodate fixed effects or clustering (McCrary, 2008; Cattaneo et al., 2020).

In order to test whether the density of workers is smooth around the cutoff we proceed as follows. First, for each campaign worker and year, we determine the number of workers employed in the campaign to which she/he participates. We imputed zeros in years in which a campaign employs no worker. Then, we run a regression of the number of workers employed in each campaign and year on individual, year and municipality times period fixed effects. Thus we use the estimated residuals from this regression as the dependent variable in a (RD) regression having on the right-hand side an indicator variable for being connected to a winning candidate and flexible controls for the margin of victory (i.e., the running variable). We cluster standard errors at the municipality level. The regression is estimated using a sample with one observation per campaign-period (year since the election), based on average residuals within each campaign-period, and includes only the election year and preceding years. We implement the RD design using two alternative specifications. First, we restrict the analysis to elections within a 5% margin of victory, and we control for linear splines of the running variable on both sides of the cutoff. The results of this specification are presented in Appendix Figure A.5. Second, we base the estimation on the Calonico et al. (2017) `rdrobust` procedure that selects the bandwidth using a data-driven procedure, and that includes local polynomial functions of the running variable as controls. The results of this specification are presented in Table A.11 below. Across specifications, we do not find a significant discontinuity in the number of campaign workers at the cutoff. This suggests that manipulation is unlikely to be a concern in our setting.

Table A.11: Manipulation Test

Dep. Var.: Residual number of campaign workers	
RD effect	0.9901
	(1.662)
Cluster-robust z-score for H_0:RD effect=0	0.647
Cluster-robust p-value for H_0:RD effect=0	0.518
Bandwidth	9.306
Observations	434,925

Note: The table shows the RD effects obtained from a regression of the residual number of campaign workers on an indicator variable for being connected to a winning candidate. We estimate this regression based on the estimator proposed by Calonico et al. (2017) controlling for local linear functions of the running variable and using local quadratic regressions for the bias correction. The bandwidth is determined based on Calonico et al. (2017). In determining campaign size, we consider only contractual workers who can be matched to RAIS and are therefore observable in the years prior to the election if they held formal employment. Standard errors in parenthesis are clustered at the municipality level.

B Data Construction

B.1 RAIS linked employer-employee data

Brazilian law requires every establishment in the country to submit detailed annual reports with individual information on its employees to the Ministry of Labor (*Ministério de Trabalho*, MTE). The collection of the reports is called *Relação Anual de Informações Sociais*, or RAIS. By design, RAIS covers all formally employed workers in any sector and tracks workers nationwide over time as they transition between formal jobs. MTE estimates today that 97 percent of all formally employed workers in Brazil are covered in RAIS, and that coverage exceeded 90 percent throughout the 1990s.

RAIS contains job-spell-level information on workers' characteristics, such as earnings, contractual weekly hours and education, as well as establishment' characteristics, such as legal status and industry. As concerns earnings, RAIS reports, among other measures, average monthly wages in multiples of the minimum wage. In order to obtain annual earnings we multiply the average monthly wage by the December U.S. dollar equivalent of the minimum wage times 12. We deflate this earning measure to August 1994 using the U.S. consumer price index (from Global Financial Data). We select August 1994 for deflation because in that month U.S. dollars and Brazilian reals were exchanged at parity. Hourly wages are obtained as the ratio of annual earnings divided by the product of contractual weekly hours multiplied by 52.14 (the number of weeks in a year). For workers with multiple employment spells during a calendar year, we keep the worker's last recorded job spell and, if there are multiple spells spanning into the final month of the year, the highest-paid job spell (randomly dropping ties). Workers with no formal employment in a year are not in RAIS. In order to obtain a balanced panel, we assign zero earnings, contractual hours and employment to missing years for workers who appear in RAIS at least once in our sample period. As in Colonnelli et al. (2020), we classify a worker as employed in the public sector if she/he is employed by a plant reporting a legal status (*Natureza Jurídica*) of "Public Administration" (*Administração Pública*).

We select dedicated staff of political organizations based on the legal status and industry of their employers. Specifically, we select workers in establishments associated with a CNAE (1.0 or 2.0) industry classification code of 91928 (*Atividades de organizacoes politicas*), or legal status classification codes 3123, 3255, 3263, 3271, 3280 (*Partido Político*) or 4090 (*Candidato a Cargo Político Eletivo*). Since political parties and campaigns enter the legal status classification only in 2002, the selection of dedicated staff in years prior to 2002 is based on *CNAE* only. However, 62% of the political campaigns and 73% of the political parties in our final sample are classified as political organizations in CNAE between 2002 and 2014, suggesting that CNAE likely captures most of the workers in political organizations prior to 2002. We exclude from the analysis dedicated staff who also work for a campaign on a contractual basis (0.18% of the observations), and we start the analysis in 1994 when the CNAE industry classification is introduced in RAIS allowing us to track workers in political organizations.

In Table 1 we assign workers to education groups based on RAIS categories as follows: RAIS categories 1 to 5 are classified as Primary Education; RAIS categories 6 and 7 as Secondary Education, RAIS category 8 is College Education; and RAIS category 9 is Post-Graduate Education.

B.2 Matching Dedicated Staff to Election Data

In order to match dedicated staff of political organization to election data we use the Tax Register of Brazilian Firms from the Brazilian Federal Revenue Service Agency (*Receita Federal do Brasil*). In any given year, this register provides information on starting, ending date (if any), ownership and type of activity for the universe of businesses created up to that year. Important for this study, the register provides a match of each employer tax-identifier (CNPJ) to the business name (*Nome Empresarial*) which, in the case of a political organization, often contains the name of the political party or candidate associated to a CNPJ.[1] We match business names from the tax register to RAIS based on CNPJ. Thus we use the resulting dataset to match labor market outcomes of dedicated staff to election data in TSE, following a procedure which differs for workers in political campaigns versus political parties. In what follows we describe this procedure.

Procedure to match campaign workers to election data. The matching of dedicated staff of political campaigns from RAIS to political candidates in TSE data consists of the following steps:

1. We merge the two datasets using candidate name and year, where year in RAIS is the year(s) in which a political worker is employed by the campaign in RAIS, and year in TSE data refers to the year of an election. Candidate name in RAIS is obtained from the business name (*Nome Empresarial*) that is reported in the tax register of Brazilian firms.

2. For the observations that are not matched in the previous step, we replace the year from RAIS with the year as indicated in the business name (*Nome Empresarial*). This is available only for a limited number of observations. Then we merge with TSE data based on candidate name and year.

3. For the observations that are not matched in the previous steps, we use candidate name and political position, where political position is extracted, when available, using the business name contained in the tax register of Brazilian firms.

4. For the remaining observations we merge based on candidate name only.

All candidate names that could be matched through this procedure appear only once in a mayoral election and election year. More than 90% of observations that find a match are merged in step 1.

Procedure to match political party workers to election data. The matching of dedicated staff of political parties in RAIS to political candidates in TSE data consists of the following steps:

1. We merge the two datasets based on year, party name, municipality and federal level. The variable year in RAIS refers to the year in which a worker is employed in the political party in RAIS, while year in TSE data refers to the year of an election. The variable federal level indicates whether a party branch in RAIS is federal or local (based on information available from the tax register in 2014).

2. For observations that are not merged in step 1 we merge based on year, party name, and municipality.

[1] We use the 2016 version of this register, which includes firms registered until that year.

3. For observations that are not merged in the previous steps we augment the year in RAIS by i and merge based on year ($+i$ in RAIS), party name, municipality and federal level

4. For observations that are not merged in the previous steps, we augment the year in RAIS by i and merge based on year ($+i$ in RAIS), party name and municipality
We repeated the prior steps (together) for each i =1,2,3,4 until we find a match.

5. For observations that are not merged in the previous, we repeat steps 1 to 4 using coalition name rather than party name. This is done because parties may not have their own candidate in an election but they support the candidate of a coalition.

Unfortunately, not all business names in the tax register contain useful information for the matching. This results in the loss of some organizations and workers among those selected in RAIS. Appendix Table B.1 compares characteristics of workers in our final sample (columns 1 and 2), to the population of formal workers in political organizations (column 3 and 4) and to the overall population of formal workers (column 5 and 6). Relative to the overall population of workers, workers in our final sample are more likely to be females, younger, educated, and, despite working slightly more hours on average, they earn less than the average worker (see column 7). A comparison of columns 3 and 5 indicates, however, that most of these facts hold if we compare the population of workers in political organization to the overall population of workers. This suggests that our sample, while allowing us to link workers to electoral outcomes, does not substantially distort the composition of the population of formal workers in political organizations.[2]

Table B.1: Descriptive statistics on workers in political organizations

	Workers linked to a candidate (1994-2014)		Population of Workers in political organizations (1994-2014)		Population of workers (1994-2014)		Difference (1 vs 5)	Difference (3 vs 5)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Mean	Std. Dev	Mean	Std. Dev.	Mean	Std. Dev.	T-stat	T-stat
Annual earnings (USD)	4,939.991	6,511.708	3,546.051	8,422.452	7,503.431	11,960.371	-29.13	-110.00
Contractual hours	41.677	6.844	41.719	5.927	41.417	5.898	2.81	11.91
Age	32.361	10.826	36.271	11.728	34.356	11.050	-7.80	38.23
Male	0.373	0.484	0.440	0.496	0.608	0.488	-35.90	-79.22
Primary Education	0.249	0.432	0.282	0.450	0.421	0.494	-29.44	-72.26
Secondary Education	0.565	0.496	0.493	0.500	0.424	0.494	21.04	32.32
College Education	0.069	0.254	0.071	0.257	0.037	0.188	9.38	31.16
Post-Graduate Education	0.114	0.318	0.151	0.358	0.117	0.322	-0.81	21.93
Observations	5,476		54,819		861,121,840			

Note: The table shows descriptive statistics for workers in our final sample (columns 1 and 2), the population of formal workers in political organizations (column 3 and 4) and the overall population of formal workers (column 5 and 6). In column 7 and 8 we show t-statistics of the difference between average values in column 1 and 5 and column 3 and 5, respectively. These t-statistics are obtained as the ratio of the difference between mean values and the square root of the sum of the standard deviations divided by the number of observations: T-stat i vs j= $\frac{Mean(X_i)-Mean(X_j)}{\sqrt{\frac{Std.Dev.(X_i)^2}{N_i}+\frac{Std.Dev.(X_j)^2}{N_j}}}$.

Finally, we note that the matching procedure links workers in a political party to elections that take place up to 4 years after the employment in the political organization. This is done with the idea that a political connection may affect a worker's career even after being employed in the organization. The majority of workers in our sample (55%), however, are employed in the organization at the time of the election, and more than 75% of all workers are linked to an election that is held no later than one year after the worker left the organization.

[2]One noticeable exception to this is workers' age, which is higher in the overall population of political workers than the population average.

B.3 Education and occupation categories in Rais

We group education information from nine Rais education categories into four categories as shown in Table B.2.

Table B.2: Education Categories

	Rais category	Education Level
1.	9.	Post-Graduate Education
2.	8.	College Education
3.	6.-7.	Secondary Education
4.	1.-5.	Primary Education

Occupation indicators derive from the 3-digit CBO classification codes in our nationwide Rais data and are reclassified to conform to ISCO-88.[3] We map Rais occupations into ISCO-88 occupations and regroup them into four categories as shown in Table B.3.

Table B.3: Occupation Categories

	ISCO-88 occupation category	Occupation Level
1.	Legislators, senior officials, and managers	Professional or Managerial
	Professionals	Professional or Managerial
2.	Technicians and associate professionals	Technical or Supervisory
3.	Clerks	Other White Collar
	Service workers and sales workers	Other White Collar
4.	Skilled agricultural and fishery workers	Blue Collar
	Craft and related workers	Blue Collar
	Plant and machine operators and assemblers	Blue Collar
5.	Elementary occupations	Blue Collar

[3]See the online documentation at *econ.ucsd.edu/muendler/brazil*.