Political Parties Do Matter in U.S. Cities ... For Their Unfunded Pensions

ONLINE APPENDIX

Christian Dippel

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A Data Appendix

A1 Historical Background

The vast majority of teacher plans are organized state-wide despite the fact that teachers are municipal employees. By contrast, police, fire-fighters and other municipal employees tend to have pension plans that are organized at the municipal level. To a large degree, whether pension plans are organized at the municipal or state-level depends on the historical pattern of union organization. When public-sector unions expanded in the 1960s, they mostly organized themselves out of pre-existing trade associations (Freeman, 1986; Reder, 1988). Trade associations for police and fire-fighters had traditionally been organized at the city-level, and as a result police and fire-fighter unions are today mostly organized locally, and so are their pension plans.[1] By contrast, teachers unions had traditionally been organized at the state or even federal level. The two largest teachers unions, the NEA and AFT, emerged out of associations that even in the early 1960s had operated nation-wide (Greenhut, 2009, 212). As a result, while teachers unions collectively bargain for wages at the city-level, their pension plans are almost exclusively organized at the state-level.

A2 Elections-to-Plan Data

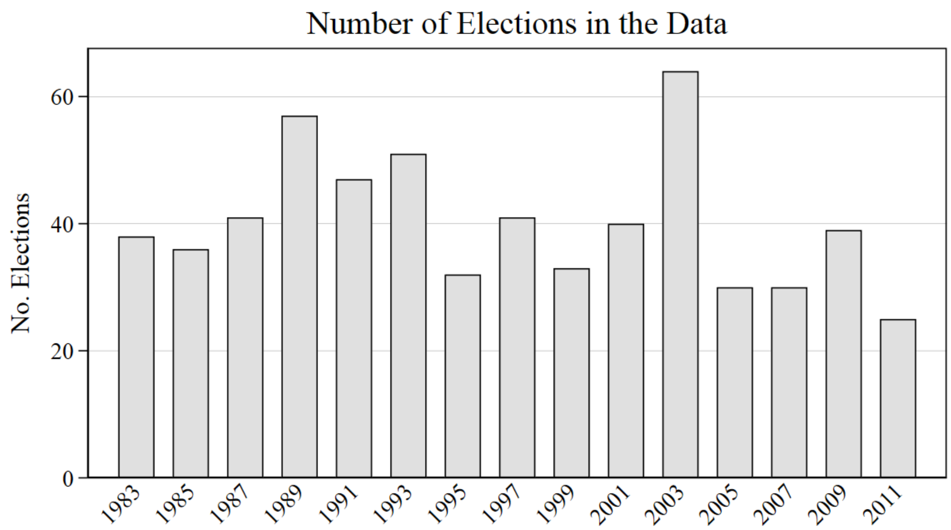
To be included in the analysis, (*i*) a city must be included in the sample of city elections in Ferreira and Gyourko (2009) or Vogl (2014), and (*ii*) and it must have a municipal pension plan covered in the ASPP data. Of the over 4,000 elections in the data, this is true for 1,200, covering 311 plans in 195 cities. Table A1 shows that the resulting linked data of elections are quite evenly spaced over the time-horizon covered by the ASPP data.

A3 Descriptives

Table A1 reports averages for changes in outcomes from one year before an election to four years after. The top row shows that over a five-year window around an election, per capita pension benefits go up on average by \$3,642 or \$2,582 in constant 2010 dollars. Per capita pension contributions go up on average by \$825 or \$661 in constant 2010 dollars. The eight main outcomes in

[1] While many police and fire-fighter unions belong to larger umbrella organizations (there is even an *International Association of Fire Fighters*), these are loose federations that play little role in collective bargaining.

Figure A1: Mayoral Elections linked to Municipal Pension Plans, Over Time



Notes: This bar chart reports, in two-year bins, on the number of mayoral elections in cities with municipal pensions covered by the *Annual Survey of Public Pensions* (ASPP).

Ferreira and Gyourko (2009) are measured in either log terms or percentage shares, and the same transformations are retained here for comparability.[2] City-level revenues, taxes and expenditures all increase by about 20 percent in nominal terms or around 14 percent in real terms over the same time window, while the share of city employees to city residents barely moves. There are no systematic five-year-changes in the four *shares* at the bottom.

B How Valuable are Pension Promises When they are Underfunded?

It is not totally obvious that public sector unions favor pension benefit expansions if they expect these to potentially go underfunded. To provide an answer, this section therefore discusses the various policy aims and trade-offs that politicians and union bargaining representatives face in making these decisions. An illuminating discussion on this question can be found in DiSalvo (2015,

[2]City-level controls as well as the fiscal outcomes in Ferreira and Gyourko (2009) are obtained from the Census Bureau's *Annual Survey of Governments*.

Table A1: Descriptives on Outcomes

	5-year Changes	Deflated
Pension Outcomes		
Δ_{+5} total benefits / #beneficiaries $_{t-1}$	4.149	3.099
	(5.302)	(4.649)
Δ_{+5} contributions / #active members $_{t-1}$	0.943	0.797
	(5.511)	(5.242)
City Fiscal Outcomes		
Δ_{+5} log per capita revenues $_{t-1}$	0.224	0.143
	(0.164)	(0.105)
Δ_{+5} log per capita taxes $_{t-1}$	0.225	0.144
	(0.165)	(0.104)
Δ_{+5} log per capita expenditures $_{t-1}$	0.237	0.151
	(0.187)	(0.119)
Δ_{+5} log # city employees per resident $_{t-1}$	0.003	
	(0.179)	
Δ_{+5} % spent on salaries $_{t-1}$	-0.008	
	(0.080)	
Δ_{+5} % spent on police departmnt $_{t-1}$	0.000	
	(0.015)	
Δ_{+5} % spent on fire departmnt $_{t-1}$	0.003	
	(0.026)	
Δ_{+5} % spent on parks and recreation $_{t-1}$	0.002	
	(0.026)	

Notes: This table reports averages for changes in outcomes from one year before an election to four years after. Standard deviations in parentheses. The second column additionally reports deflated values for variables defined in dollar-terms.

156-157). Empirically, Anzia and Moe (2019) provides compelling evidence that public sector unions do favor pension benefit expansions even if these can be expected to go underfunded.

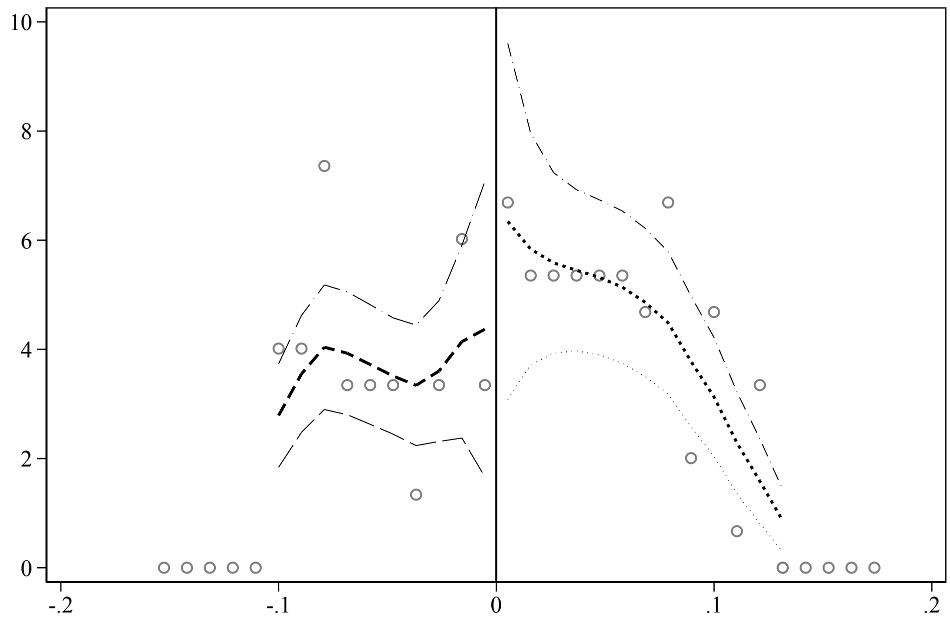
Pension benefit and contribution setting may be best characterized as a bargaining process between a politician and a public-sector union representative, in which the politician maximizes votes from core supporters (union-members) and other voters, while the union representative can earn rents from union members for generating higher benefits, and from the politician for mobilizing political support. The politician can promise pension benefits to secure the political support of their core supporters. In practice, the blind spots in the ARC discussed in Section 1.1 above, in combination with misleading budget neutrality of letting actual employer-contributions fall behind their actuarially required levels, make pension promises a ‘shrouded’ benefit from the politician’s point of view: they can bring out their core supporters while keeping a balanced budget in the eyes of other voters. Many of these features are incorporated in the theory in Glaeser and Ponzetto (2014). For a discussion of the general class of models on the interaction between special-interest-groups and vote-maximizing politicians, see Persson and Tabellini (2000, ch.7).

One objection to such arguments is that the budget neutrality of unfunded pensions should not matter because home-buyers capitalize future tax obligations into property values (Daly, 1969; Brinkman, Coen-Pirani and Sieg, 2018). However, empirical evidence that announcements of major adjustments in official funding levels of San Diego pensions reduced home prices (MacKay, 2014) suggests exactly that voters do not see the true under-funding of the pension plan if it is not officially announced to them. The reality of the ‘shroudedness’ of pension accounting means that most home buyers will not anticipate future taxes related to covering funding gaps.

If the ability to under-fund pensions is key to the ‘shroudedness’ of pension benefits, it also raises the question how union representatives and union members discount under-funded pension benefits relative to fully funded ones in practice. It is possible that there is no discount at all because under-funded benefits are still legally binding commitments. One caveat to that view is that even if all obligations end up being paid in full, many union members may belong to the tax base from which they are paid.[3] However, this should be equally true of funded benefits. If, as suggested by Inman (1982), retired pensioners are more likely to move out of the tax base, then

[3] They may also be homeowners, and unfunded pension obligations may be capitalized into house prices (Daly, 1969; Glaeser and Ponzetto, 2014; Brinkman, Coen-Pirani and Sieg, 2018).

Figure A2: McCrary Test for Bunching of the Running Variable



Notes: This figure shows the McCrary Test for manipulation of the running variable VSD_{jt}. The estimated discontinuity (the 'log difference in height') is 0.0245, with a standard error of 0.5739 (and a resulting t-stat of 0.04270, thus one cannot reject the hypothesis that the running variable has continuous support at the cutoff.

under-funding may actually be preferred.

There is also a separate question of how union members view biased actuarial assumptions (such as over-optimistic AARs), when these are likely to be eventually adjusted and lead to future increases in employee-paid actuarially required contributions. It seems probable that this scenario is not salient enough to impact the average union member's views of their benefits, although it is likely to be very salient to the union representatives on pension boards. The narrative evidence of union representatives pushing for and defending unrealistically high AARs supports this characterization (Greenhut, 2009; Anzia and Moe, 2016).

C Validity and Robustness Checks

C1 Testing the Validity of the Identification Assumptions of the RDD Approach

C1.1 Testing Bunching of the Running Variable

Ferreira and Gyourko (2009) (like most close election studies around that time) did not test for bunching of the running variable. However, Eggers et al. (2015) tested for bunching in a wide range of close elections including mayoral races in the U.S. (as well as historical and contemporary elections for the U.S. House, statewide gubernatorial, state legislative, and close elections in other countries), and conclude that the post-WW2 U.S. House appears to be the *only* setting where there is some evidence of heaping, i.e. that incumbents are more likely to win very close elections.[4] Figure A1 reports on a McCrary (2008) test in the data used here, confirming that there is no bunching of the running variable in the data.

C1.2 Covariate Balance Across the Close-Election Cutoff

If the identifying assumptions of the RD design hold, covariates should be balanced across the cutoff. Table A2 reports on the balancedness of city and election covariates, after trimming the sample to include only elections within a ten-percent window around the winning cutoff. Table A2 includes the election year, and per capita benefits and contributions in the year before the election. The table also reports on one-year lags of the eight main fiscal outcomes in Ferreira and Gyourko (2009, TableII). Since these are all defined in per capita terms, the table also separately reports on the underlying totals.

[4] Vogl (2014) finds some evidence that in cities in the U.S. South black mayoral candidates are more likely to see close wins than close losses. However, as Eggers et al. (2015) note, his evidence is based on only 38 close mayoral races in the South between a white and black candidate.

Table A2: Covariate Balance

	Democrat Vote Share 40-50%	Democrat Vote Share 50-60%	Difference
election-year	1996	1996	-0.17
	(7.920)	(8.161)	[0.874]
Δ_{+5} total benefits / #beneficiaries $_{t-1}$	15.432	15.345	-0.088
	(10.065)	(8.128)	[0.942]
Δ_{+5} contributions / #active members $_{t-1}$	2.664	2.368	-0.296
	(4.188)	(1.225)	[0.439]
log per capita revenues $_{t-1}$	0.533	0.407	-0.125
	(0.686)	(0.625)	[0.203]
log per capita taxes $_{t-1}$	-0.565	-0.669	-0.103
	(0.773)	(0.700)	[0.349]
log per capita expenditures $_{t-1}$	0.496	0.390	-0.106
	(0.698)	(0.681)	[0.308]
log # city employees per resident $_{t-1}$	-3.957	-4.096	-0.139*
	(0.634)	(0.541)	[0.092]
% spent on salaries $_{t-1}$	0.372	0.384	0.012
	(0.096)	(0.100)	[0.403]
% spent on police departmnt $_{t-1}$	0.061	0.072	0.011**
	(0.030)	(0.035)	[0.030]
% spent on fire departmnt $_{t-1}$	0.095	0.118	0.023***
	(0.043)	(0.055)	[0.002]
% spent on parks and recreation $_{t-1}$	0.045	0.053	0.008
	(0.035)	(0.054)	[0.276]
log total population $_{t-1}$	12.354	12.247	-0.107
	(1.658)	(1.327)	[0.592]
log total revenues $_{t-1}$	12.866	12.760	-0.105
	(1.863)	(1.517)	[0.670]
log total expenditures $_{t-1}$	12.833	12.727	-0.106
	(1.852)	(1.524)	[0.667]
Observations	99	134	233

Notes: Column 1 reports on average characteristics of city- (or pension plan-)years were the Democratic Party candidate narrowly lost. Column 2 reports on average characteristics of city- (or pension plan-)years were the Democratic Party candidate narrowly won. Standard deviations in parentheses. Column 3 reports on the difference between the two, with the p-value reported in brackets.

Table A3: Re-Estimate Table 2 with Bandwidths Manually Set

	(1)	(2)	(3)	(4)	(5)	(6)
Bandwidth in [-0.25, 0.25]						
Δ_{+4}	3.772	2.732	3.493	2.607	1.506	1.547
	[1.073; 6.471]	[0.677; 4.787]	[0.961; 6.025]	[0.722; 4.492]	[-0.700; 3.712]	[-0.122; 3.216]
Δ_{+5}	3.89	2.445	3.536	2.371	3.44	2.299
	[1.693; 6.086]	[0.741; 4.148]	[1.315; 5.757]	[0.681; 4.060]	[1.090; 5.789]	[0.559; 4.038]
Δ_{+6}	2.765	2.145	2.216	2.171	3.173	2.51
	[0.0211; 5.510]	[0.105; 4.186]	[-0.208; 4.639]	[0.326; 4.016]	[0.983; 5.363]	[0.841; 4.180]
Bandwidth in [-0.20, 0.20]						
Δ_{+4}	3.801	2.937	3.384	2.76	1.317	1.559
	[1.001; 6.601]	[0.780; 5.094]	[0.855; 5.913]	[0.786; 4.734]	[-0.893; 3.528]	[-0.196; 3.313]
Δ_{+5}	3.844	2.821	3.46	2.695	3.309	2.662
	[1.549; 6.138]	[1.063; 4.579]	[1.162; 5.758]	[0.940; 4.451]	[0.884; 5.734]	[0.829; 4.494]
Δ_{+6}	2.82	2.23	2.126	2.137	3.106	2.641
	[-0.0653; 5.706]	[0.0758; 4.384]	[-0.308; 4.561]	[0.189; 4.085]	[0.901; 5.312]	[0.893; 4.390]
Bandwidth in [-0.15, 0.15]						
Δ_{+4}	3.65	3.466	3.205	3.273	0.692	1.592
	[0.544; 6.757]	[1.078; 5.853]	[0.372; 6.038]	[1.059; 5.486]	[-1.737; 3.120]	[-0.434; 3.619]
Δ_{+5}	3.119	3.6	2.601	3.371	2.539	3.09
	[0.651; 5.588]	[1.644; 5.555]	[0.248; 4.953]	[1.405; 5.338]	[-0.0671; 5.144]	[0.978; 5.202]
Δ_{+6}	1.62	3.051	0.637	2.723	2.111	3.146
	[-1.722; 4.962]	[0.622; 5.481]	[-1.991; 3.265]	[0.522; 4.925]	[-0.259; 4.481]	[1.175; 5.118]
Bandwidth in [-0.10, 0.10]						
Δ_{+4}	3.832	3.443	3.081	3.182	-0.105	1.269
	[0.252; 7.411]	[0.796; 6.090]	[-0.264; 6.425]	[0.799; 5.566]	[-2.986; 2.775]	[-0.817; 3.355]
Δ_{+5}	2.261	3.489	1.037	3.218	1.256	3.102
	[-0.811; 5.334]	[1.434; 5.543]	[-1.730; 3.805]	[1.159; 5.278]	[-1.635; 4.147]	[0.812; 5.393]
Δ_{+6}	2.479	2.309	-0.492	2.018	1.165	3.104
	[-1.193; 6.151]	[-0.415; 5.033]	[-3.419; 2.435]	[-0.287; 4.323]	[-1.711; 4.040]	[0.919; 5.289]
Polynomial	quadratic	linear	quadratic	linear	quadratic	linear
Controls			✓	✓	+ contrib.	+ contrib.

Notes: *(a)* This table re-estimates Table 2 when the bandwidth is set manually. *(b)* Columns 1–2 report the RD results with only a linear or quadratic function $f(\text{VSD}_{jt})$ included. Columns 3–4 add as control variables the year of the election, the log of city population, the log of total revenue, the log of total city employees, and per capita benefits, all measured in the year before the election. Columns 5–6 add the five-year change in per capita contributions as a control when benefits are the outcome. *(c)* 95th percentile CI reported in square brackets, with standard errors clustered at the state-year level. Unlike the other RDD estimations, this table reports on an OLS estimation around the cutoff.

Table A4: Re-Estimate Table 2 without the South

	(1)	(2)	(3)	(4)	(5)	(6)
total benefits / # beneficiaries						
Δ_{+3}	3.261	2.216	3.032	1.836	3.137	1.091
	[0.0855; 6.436]	[-0.127; 4.559]	[-0.267; 6.330]	[-0.353; 4.025]	[-0.0414; 6.315]	[-1.010; 3.193]
	{-0.333; 6.855}	{-0.545; 4.977}	{-0.754; 6.817}	{-0.787; 4.458}	{-0.470; 6.743}	{-1.419; 3.602}
	N=424	N=401	N=390	N=412	N=335	N=354
Δ_{+4}	3.773	2.258	3.969	2.572	3.033	2.492
	[1.434; 6.112]	[0.396; 4.120]	[1.707; 6.230]	[0.863; 4.282]	[0.646; 5.420]	[0.653; 4.331]
	{1.216; 6.330}	{0.0917; 4.425}	{1.489; 6.448}	{0.589; 4.555}	{0.360; 5.706}	{0.333; 4.651}
	N=403	N=429	N=388	N=439	N=316	N=323
Δ_{+5}	5.650	3.887	3.801	2.972	3.847	2.820
	[2.749; 8.550]	[1.792; 5.982]	[1.356; 6.245]	[1.115; 4.830]	[1.308; 6.386]	[0.846; 4.793]
	{2.448; 8.851}	{1.405; 6.369}	{1.018; 6.583}	{0.762; 5.183}	{0.981; 6.713}	{0.453; 5.187}
	N=408	N=372	N=431	N=394	N=415	N=383
Δ_{+6}	3.960	4.251	4.026	4.106	5.174	4.946
	[0.591; 7.329]	[1.551; 6.951]	[0.927; 7.125]	[1.569; 6.643]	[2.425; 7.924]	[2.610; 7.283]
	{0.156; 7.764}	{1.037; 7.465}	{0.565; 7.487}	{1.060; 7.152}	{2.079; 8.269}	{2.087; 7.806}
	N=379	N=332	N=382	N=307	N=336	N=248
Polynomial	quadratic	linear	quadratic	linear	quadratic	linear
Controls			✓	✓	+ contrib.	+ contrib.

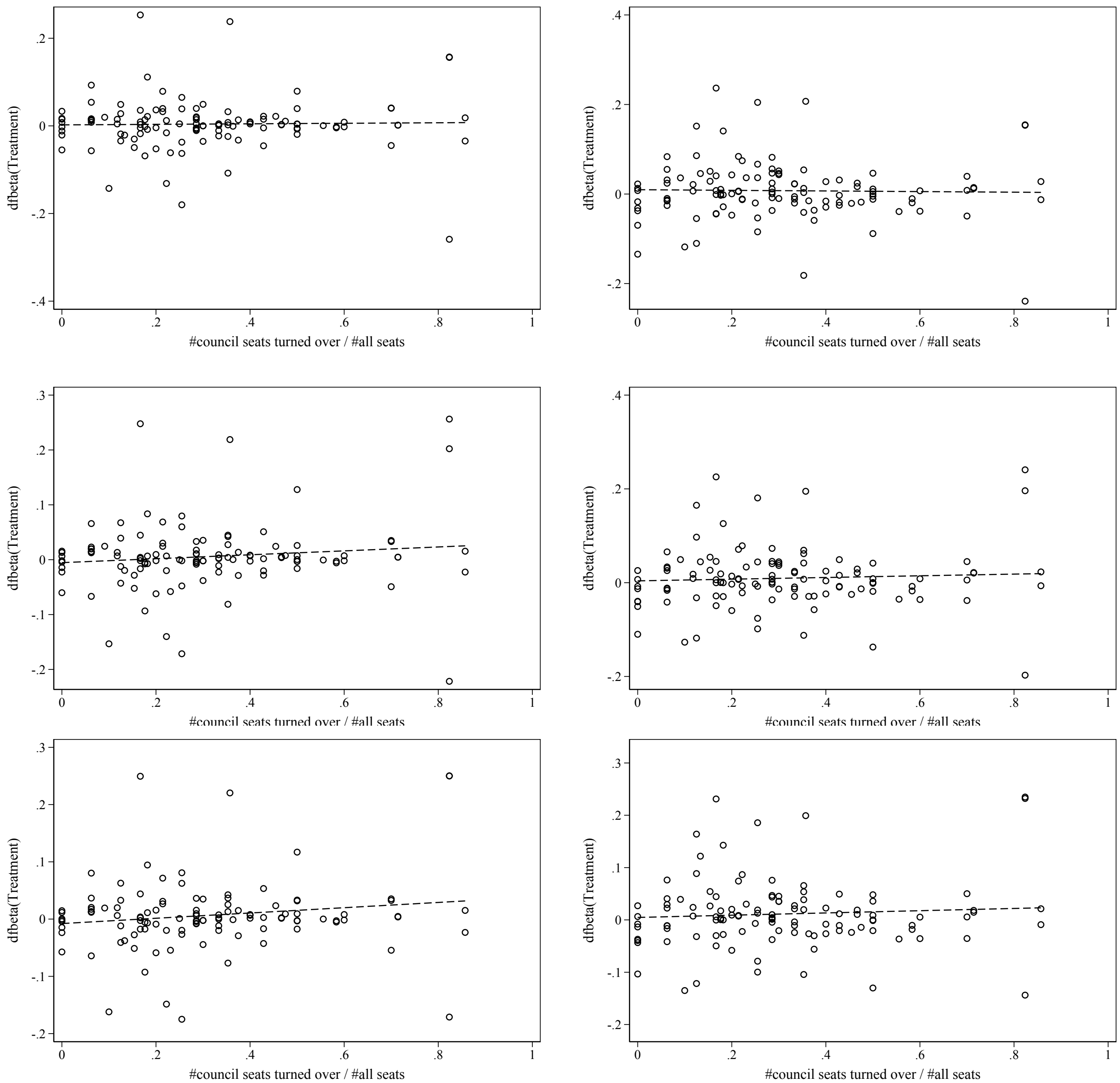
Notes: *(a)* The baseline results study pension benefits four years after the election (i.e. Δ_{+5} denotes five years after the baseline year before the election) This table investigates different time horizons, from three to six years after the election. The bottom-two panels transforms the outcomes (and nominal controls) into constant 2010 dollars. *(b)* Columns 1–2 report the RD results with only a linear or quadratic function $f(\text{VSD}_{jt})$ included. Columns 3–4 add as control variables the year of the election, the log of city population, the log of total revenue, the log of total city employees, and per capita benefits, all measured in the year before the election. Columns 5–6 add the five-year change in per capita contributions as a control when benefits are the outcome. *(c)* For point estimation, the choice of bandwidth is automated for each estimation separately, based on MSE-minimization (Cattaneo, Idrobo and Titiunik, 2019, 4.2.4). A triangular kernel function is used. 95th percentile CI reported in square brackets, with standard errors clustered at the state-year level. As a check on inference, Cattaneo, Idrobo and Titiunik (2019, 4.3.2) recommend alternatively choosing the bandwidth to minimize the coverage error (CER) of the confidence intervals. The corresponding CI are reported in braces.

Figure A3 provides evidence that the core results are not likely to be driven by turnover in city councils. I was able to locate records of city council elections for 146 city-years— roughly one-third of the city-election observations that play any role in Table 2— and coded these up. On average, three-quarters of council seats in these data were up for re-election at the same time as the mayor. These consisted of cities were all seats are up for re-election at the same time as the mayor, as cities were half of the council seats are up. On average, 29% of seats of council seats turned over at the same time as the mayor, through a combination of candidates losing or not re-running.

Because the share of city-election observations with available council data is only one-third, I do not include city-council data as a control. Instead, I calculate observations' influence on the core results (their *bfbeta*) after each of the six estimations in Table A3 (using the bandwidth $[-0.20, 0.20]$, and Δ_{+5}) because this row is most comparable to Table 2). I then simply correlate the *bfbeta* with the turnover of council seats to check whether more influential observations tended to also see more council turnover during the mayoral election.

The evidence for this is exceedingly weak, as can be seen in Figure A3. There is basically no relationship between a city-year observation's influence on the core results and its city council turnover during the same election. The correlation coefficients across the six sub-panels (moving left-to-right, and then top-to-bottom, equivalent to the six columns in Table A3) are $\{0.0209, -0.0221, 0.119, 0.0572, 0.1518, 0.0678\}$.

Figure A3: Correlating Observation-Influence with Turnover in City Council



Notes: I calculate observations' *bfbeta* after each of the six estimations (columns) in Table A3 (using the bandwidth $[-0.20, 0.20]$, and the time-horizon Δ_{+5}). This figure's six panels plot these six sets of *bfbetas* against the turnover of council seats to check whether more influential observations tended to also see more council turnover during the mayoral election: Table A3 columns 1–2 in the top panel, 3–4 in the middle panel, 5–6 in the bottom. The correlation coefficients across the six sub-panels are $\{0.0209, -0.0221, 0.119, 0.0572, 0.1518, 0.0678\}$.

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