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ABSTRACT

This paper studies the biggest fiscal challenge currently facing many U.S. cities, namely public-sector pension obligations. Employing a regression discontinuity design (RDD), it tests whether the mayor's party impacts a city's public-sector pensions. Pension benefits are shown to grow faster under Democratic-party mayors, while contribution payments simultaneously fall behind. Previous research showed that parties do not matter in U.S. cities for a wide range of fiscal expenditure types, purportedly because voters impose fiscal discipline. This paper shows that parties can matter when expenditures benefit a narrow interest group and are difficult to observe for tax payers.

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1 Introduction

Unfunded public-sector pension obligations are the biggest fiscal challenge that many U.S. cities (and states) face in the coming decades: Many pension plans are severely underfunded (Novy-Marx and Rauh, 2009), and, unlike federal social security, municipal and state pension obligations are legally binding commitments (Burns, 2011; Trusts, 2013). Ultimately, it is therefore tomorrow's taxpayers that are on the hook when plans are under-funded.[1] Where the funding gap is sufficiently large, it can culminate in municipal bankruptcy.[2]

Pension plan under-funding is at its core a political economy problem: Pension benefits are very valuable to public-sector unions, who also happen to be very well organized. At the same time, the costs of pension benefits are easily obfuscated: if benefit promises are funded by their actuarially required contributions, these costs are perfectly observable in the budget. If, however, benefits go unfunded, their costs disappear from view because unpaid contributions are by definition not on the budget, despite the fact that the benefits continue to be legally binding commitments (Johnson, 1997; Munnell, Aubry, and Quinby, 2011; Mohan and Zhang, 2014). Furthermore, even fully paid actuarially required contributions will often fall significantly short of covering the true costs of pension benefits because the actuarial assumptions underlying official calculations (about, e.g. expected returns on plan assets, or expected retirement ages of workers) are too generous, arguably because these assumptions are rubber-stamped by pension boards in a highly politicized process (Greenhut, 2009; Novy-Marx and Rauh, 2011; Anzia and Moe, 2016).[3] This misleading budget neutrality and overly optimistic actuarial assumptions together create a 'fiscal illusion' that makes under-funded pension benefit increases an attractive substitute to wage increases in politicians' eyes. As one prominent insider summarizes, "consistent low-balling of pension costs over the past two decades has made it easy for elected officials and union representatives to agree on very valuable benefits, for very much smaller current pay concessions."[4]

[1] The paper refers to 'under-funded' plans, and 'unfunded' benefits or obligations.

[2] According to Anderson (2013), between 2007 and 2013, twenty-eight cities went into bankruptcy or receivership." These bankruptcies are not mono-causal, but a re-negotiation of pension obligations is usually the most important order of business once a city has gone into bankruptcy, as was the case in Detroit in 2014 (The Economist, 2014).

[3] Illustrating this point, Novy-Marx and Rauh (2009) estimate that the appropriately calculated aggregate funding gap for the largest state and municipal plans is above $3 trillion. compared to an official level closer to $1 trillion. The relationship between government pension actuaries and pension boards is plagued by similar principal-agent problems as those between rating agencies and investment banks that issued mortgage backed securities in the mid-2000s: conservative actuaries do not get the job (Malanga, 2016).

[4] Quote from a speech by the late Jeremy Gold, member of the American Academy of Actuaries and the Society of

For a primary instead of secondary source, a quote from an interview with the former mayor of Houston, Lee P. Brown, is revealing: During a tight re-election campaign that he narrowly won with 51.7 percent of votes, he was instrumental in a large increase in municipal employees' pension benefits. He later justified his decision to increase pension benefits by the fact that "it was budget neutral", and that he did not "have the funds to give municipal employees the raises they deserved" (Boylan, 2016).

The objective of this paper is not to estimate the "political economy portion" of the problem of pension plan under-funding, but to utilize narrow mayoral elections for a regression discontinuity design (RDD) that tests whether there is a partisan divide in this political economy problem.[5] This approach relates to a larger literature and a well-known finding that a broad range of expenditure types (such as total revenue and taxation, expenditure shares for major items like schools and sanitation, as well as municipal employment) consistently *do not* respond to the party of the mayor in U.S. cities (Ferreira and Gyourko, 2009). This fact is usually interpreted as evidence that voters "elect policies, not politicians," and are able to impose fiscal disciplines on politicians, in part through Tiebout-sorting, i.e. by voting with their feet.[6] However, the political economy literature suggests that fiscal indiscipline is more likely if expenditures have hidden costs that are not salient or easily observable to voters (Besley and Burgess, 2002; Adsera, Boix, and Payne, 2003; Strömberg, 2004; Ferraz and Finan, 2008).[7] This literature further suggests fiscal indiscipline is more likely if expenditures go to benefits for a narrow and well-organized constituency (i.e. unions) as opposed to being broadly redistributive (Olson 1965, Persson and Tabellini 2000, ch.7).

While the case for a political economy explanation of pension under-funding is compelling, it is not clear that one should expect a partisan divide along this dimension: The evidence does not suggest that Democratic party mayors are more fiscally profligate than Republican ones on other issues, nor that the Democratic party is more fiscally profligate at any level of government. However, the qualitative evidence does suggest that Democratic party politicians at the municipal level tend to be closer to (and more dependent on the political support of) public-sector unions

Actuaries Pension Financing Task Force, at MIT's Golub Center for Finance and Policy, in November 2015.

[5] Not finding a partisan effect would not imply the absence of a political economy problem, and finding a partisan effect does not indicate that the political economy problem involves only one party.

[6] This contrasts with findings from other countries where parties appear to be influential in shaping city level fiscal outcomes, perhaps because of less Tiebout-sorting (Pettersson-Lidbom, 2008; Fiva, Folke, and Sørensen, 2018).

[7] Glaeser and Ponzetto (2014) label such expenditures as 'shrouded benefits.'

(Greenhut, 2009, p.137). Political economy theories that emphasize the importance of 'bringing out the base' would thus predict a partisan tilt whereby Democratic party mayors are more likely to expand pension benefits (Glaeser, Ponzetto, and Shapiro, 2005; Glaeser and Ponzetto, 2014).[8]

This paper analyzes whether per capita benefits and per capita contributions in municipal pension plans depend on the party affiliation of a city's mayor.[9] Many unobserved factors that can also influence pensions may determine whether city residents elect a Democratic or Republican party mayor. To gain identification on the effect of the mayor's party, the paper therefore uses an RDD around close elections, using an extension of the mayoral election data produced by Ferreira and Gyourko (2009). By focusing on narrow election victories, the RDD controls for confounding factors that independently shape the outcome of interest. Standard tests for bunching of the running variable (i.e. Democratic Party vote share) do not come close to rejecting its smoothness around the winning cutoff, suggesting the RDD identifying assumptions hold in this data, as also suggested by previous research (Eggers, Fowler, Hainmueller, Hall, and Snyder Jr, 2015).

The core finding of the paper is that changes in the political party of the mayor have a sizeable effect on the per capita benefits of a city's pension plan. Having a Democratic Party mayor is associated with $2,000–$3,000 larger increases in annual per capita benefits per person (expressed in constant 2010 dollars) from one year before to four years after an election. This effect is roughly equal the average five-year increase in plans' per capita benefits in the data, and is roughly five percent of the average level of annual per capita benefits. The effect on per capita benefits is robust to a range of specifications, different polynomials and optimal RDD bandwidth selection. By contrast, the highly visible fiscal outcomes considered in Ferreira and Gyourko (2009) are all confirmed to indeed not respond to the party of the mayor.[10]

Varying the time horizon reveals that the effect can be discerned starting three years after the election, and is economically and statistically strongest five years later, before becoming less precise six years out. This is consistent with the fact that pension benefits can adjust only slowly,

[8] Many cities are "institutionally nonpartisan" in that they prohibit party labels from being printed on election ballots. However this de jure constraint appears to have little bearing on the actual de facto importance of parties in a given city. See (Ferreira and Gyourko, 2009, fn.7).

[9] With publicly available data, it is not possible to say whether benefits are unfunded in an actuarial accounting sense because this can only be determined with knowledge of the full distribution of current workers' expected retirement ages, and their full expected benefit payments relative to the contributions asked of them. Instead the paper studies whether benefit increases are larger than what can be statistically explained by concurrent contribution increases.

[10] This is worth confirming because the sample of cities, the time period and empirical specification are all somewhat different from the setup in Ferreira and Gyourko (2009).

through collective bargaining or by statute.

Further investigation into other outcomes provides evidence on the mechanisms that link concurrent benefit expansions to the subsequent erosion of plans' funding levels. The data shows that as benefits expand in a plan, (*i*) cities begin to lag behind on their required contribution payments, (*ii*) active contribution-paying members begin to retire, thus swelling the ranks of pension recipients inside a plan, and (*iii*) the plans' asset base begins to erode. These pieces of evidence create the link between benefit expansions and plans' funding levels.

As a final exercise, the data are split by plan type or by election type, i.e. comparing incumbent vs challenger wins. By plan type, the data reveal that the effect is concentrated in pension plans for police and fire-fighters, i.e. the most well-organized employee groups. By election type, the data reveal that the effect is concentrated in elections where the challenger wins, i.e. it is driven by Democratic Party mayors entering office rather than continuing in office.

This paper's main contribution is to show that the voter-imposed fiscal discipline that makes parties irrelevant for the more visible budget items in U.S. cities does not hold for 'shrouded' types of expenditures that benefit narrow constituencies and whose costs are not easily observed or understood by all tax payers.[11] This finding is situated in a broader literature applying RD designs to local elections and local public finance (Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Vogl, 2014; Fiva et al., 2018).[12]

The paper's other contribution is to the literature on public-sector pension funding. Most observers agree that underfunded public-sector pensions are first and foremost a political economy problem (Mitchell and Smith, 1994; Greenhut, 2009; Burns, 2011; Trusts, 2013; Kelley, 2014; Gale and Krupkin, 2016; Anzia and Moe, 2016). However, rigorous studies on pension benefit funding have tended to focus on pensions' fund management and on accounting practices (Novy-Marx and Rauh, 2009, 2014a,b; Brown and Wilcox, 2009). In providing causally identified evidence on

[11] Instead of creating fiscal discipline, Tiebout-sorting may even accentuate the incentive to support underfunded pensions for the recipients of the benefits: Today's public-sector employees need to live reasonably close to their plan's tax base in order to work there, but are free to move away upon retirement. Consistent with this logic, Johnson (1997) finds evidence that public employees that accrue unfunded pension benefits are systematically more likely to retire early and leave their municipality so as to be shielded from later tax increases aimed at closing pensions' funding gap.

[12] A related literature uses local RD designs to study the effects of party turnover itself, with the primary outcome being patronage politics and public-office hiring (Akhtari, Moreira, and Trucco, 2017; Colonnelli, Prem, and Teseo, 2018). This literature is primarily focused on developing countries, since patronage politics has largely disappeared from U.S. local politics since the professionalization of municipal bureaucracies starting in the 1930s (Grindle, 2012), although a fascinating recent study by Ornaghi (2018) suggests it may have been prevalent well into the 1970s.

the political economy drivers of pension plans' under-funding, this paper contributes to bridging that gap.

2 Data

The Pension Plan Data is based on the U.S. Census' *Annual Survey of Public Pensions* (ASPP). The ASPP in its present form covers the years 1992–2015. A largely overlapping set of pension-plans is covered by the Census' *Historical Database on Public Employee-Retirement Systems*, which includes 1962, 1967, and 1972–1991. Fortunately, plans can be manually linked across the two data-sources. The paper refers to the linked data as the ASPP. The ASPP contains information on plans' assets, on per capita benefit payments (as they are being paid out to retirees) and per capita contributions (as they are being paid by active members). While a plan's funding level cannot be directly observed in the ASSP, changes in the flow of benefits relative to the flow of contributions are the two main ingredients that determine funding levels. Appendix A.1 gives a more rigorous analysis of plans' funding levels and how they relate to past benefit promises and contribution payments.

The ASPP covers municipal- as well as state-level plans. This paper considers only the municipal plans, where each plan is mapped to its corresponding city. This paper's unit of observation is a city's pension plan in a year.[13] (The plan-to-city mapping is many-to-one, i.e. each plan is uniquely mapped to a city but the opposite is not true.) This omits two types of public-sector plans: plans for state employees, and plans for municipal employees that are organized as state-wide plans. The second type is made up principally of teacher plans, the vast majority of whom are organized state-wide. By contrast, non-teacher municipal employees (i.e. police, fire-fighters and 'general administrators and workers') usually have pension plans that are managed at the municipal level. Appendix B provides a discussion of the historical roots of these patterns.

In the time-series, the city-election data used in this paper extends the Ferreira and Gyourko (2009) data to cover 2005-2014. In the cross-section, the data for Southern cities collected by Vogl (2014) are added. Only the subset of cities with municipal pension plans appears in the analysis undertaken here. To be included in the analysis, a city (*i*) must be included in the sample of cities in Ferreira and Gyourko (2009) or Vogl (2014), and (*ii*) and it must have a municipal pension

[13] Cities can run their employees' pensions under the umbrella of state-level pension plans. For example, Los Angeles teachers' pensions are managed by state-wide CalSTRS. Those plans cannot be statistically related to city politics.

plan covered in the ASPP data. Furthermore, I follow Ferreira and Gyourko (2009) in dropping elections in which the two main candidates were from the same party.

Of the over 4,000 elections in the data, this is true for 1,200, covering 311 plans in 195 cities. Figure A1 in Appendix B shows that the resulting linked data of elections are quite evenly spaced over the time-horizon from 1962 that is covered by the ASPP data. For reference, Appendix C lists all cities in the data and their occurrences in the data.

2.1 Defining the Key Variables

From the perspective of testing a political economy theory of politicians making commitments to an interest group, the ideal measure of an increase in pension benefits would be changes in the net present value of future benefits to member i relative to changes in that same member i's contributions. The ASPP pension data can only approximate this because observed *per capita benefit payments* are paid to current pensioners, while observed *per capita contribution payments* are paid by current active members.[14]

This inherent data limitation creates a bias against finding a concurrent effect of elections if the majority of the eventual benefactors of a promised benefit expansion are in fact still active employees.[15] This is addressed partly by the fact that the main outcome considered in what follows is the 5-year change in per-capita benefits, measured as the change from the year before an election to the end of the mayor's term four years after the election, i.e. $\Delta_{t+5}\text{Benefits}_{it}$. Motivated by the fact that pension benefits are adjusted infrequently (often as as part of a new round of collective bargaining), this time horizon is chosen as the longest time horizon that still falls under the term of the mayor whose election is the treatment variable.

2.2 Descriptive Statistics

Table 1 reports averages for changes in outcomes from one year before an election to four years after. Since the elections in the data span more than fifty years, the second column additionally reports deflated values (in constant 2010 dollars) for nominal variables. The top row shows that

[14] One implication of these features of the ASPP data is that it is unappealing to directly study the ratio of benefits to contributions because these flows pertain to different populations. Instead, the focus will therefore be on changes in per capita benefits, while controlling in most specifications for changes in per capita contributions.

[15] Indeed, some of the benefit expansions discussed in Appendix A will go undetected in this data.

Table 1: Descriptives on Outcomes

	5-year Changes	Deflated
Pension Outcomes		
Δ per capita pension benefits $_{t-1}$	3.642	2.582
	(5.146)	(4.312)
Δ per capita pension contributions $_{t-1}$	0.825	0.661
	(4.924)	(4.684)
City Fiscal Outcomes		
Δ log per capita revenues $_{t-1}$	0.297	0.148
	(0.208)	(0.098)
Δ log per capita taxes $_{t-1}$	0.267	0.139
	(0.187)	(0.098)
Δ log per capita expenditures $_{t-1}$	0.296	0.150
	(0.217)	(0.109)
Δ log # city employees per resident $_{t-1}$	0.008	
	(0.175)	
Δ % spent on salaries $_{t-1}$	-0.015	
	(0.079)	
Δ % spent on police departm $_{t-1}$	0.009	
	(0.068)	
Δ % spent on fire departm $_{t-1}$	0.014	
	(0.088)	
Δ % spent on parks and recreation $_{t-1}$	0.009	
	(0.071)	
Observations	1,195	1,195

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.

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 Ferreira and Gyourko (2009) are measured in either log terms or percentage shares, and the same transformations are retained here for comparability.[16] 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. Unsurprisingly, there are no systematic five-year-changes in the four *shares* at the bottom.

3 Framework and Results

3.1 Identification Framework

To identify the effect of the party in power, this paper relies on a regression discontinuity design around close elections. Among non-experimental identification strategies, the RDD has become increasingly popular because it entails perfect knowledge of the selection process (i.e. the discontinuity) and because it requires comparatively weak assumptions (Lee, , and Butler, 2004; Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Dal Bó, Dal Bó, and Snyder, 2009; Lee and Lemieux, 2010; Ferraz and Finan, 2011; Eggers et al., 2015; Fiva et al., 2018; Akhtari et al., 2017; Colonnelli et al., 2018; Ornaghi, 2018). The RD design entails a treatment (i.e. having a Democratic mayor, $\text{D}_{jt} = 1$) that is an exact function of (or varies sharply with) an underlying running variable (i.e. the *vote share Democratic candidate*, VSD_{jt}):

$$\Delta\text{Benefits}_{it} = \beta_D \text{D}_{jt} + f(\text{VSD}_{jt}) + \beta_X X_{it} + \epsilon_{it}. \tag{1}$$

The RDD always requires the researcher to choose a functional form $f(\text{VSD}_{jt})$ and to choose a bandwidth around the cutoff. For the choice of functional form , this paper follows current best practice in RDD by using only local linear or quadratic approximations at either side of the threshold. For the choice of bandwidth, there is always a tradeoff between precision and bias: Including observations further away from the discontinuity improves precision by including more data, but

[16] 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*.

also introduces bias because the identifying assumptions are more likely to hold close to the discontinuity. This paper follows best practice by using a data-driven choice of bandwidth that is determined by an explicit optimization criterion rather than the researcher's discretion (Cattaneo, Idrobo, and Titiunik, 2018). On average across specifications, this criterion selects the 384 observations closest to the cutoff.

3.2 Main Results

The identifying assumption of the RDD is that the electorate's preferences can be held constant in a narrow window around the same vote share, where the relevant vote share is obviously the one that narrowly elects one party or candidate over the closest rival. One common test for the validity of the RDD approach in elections is to verify that there is no bunching at the cutoff, e.g. no disproportionate amount of close wins relative to close losses (McCrary, 2008). Figure A2 in Appendix B.3 reports on the results of a McCrary (2008) test, confirming that there is no bunching of the running variable in the data, consistent with findings on U.S. mayoral races in previous research (Eggers et al., 2015). Another common test for the validity of the RDD approach is to verify that covariates are balanced across the cutoff. After trimming the sample to include only elections within a ten-percent window around the winning cutoff, Table 6 in Appendix B.3 shows that per capita plan benefits, and per capita plan contributions, as well as all the highly visible fiscal outcomes considered in Ferreira and Gyourko (2009, TableII) are all balanced across the cutoff in the year before each election in the data. As well, the election year is balanced across, i.e. within the fifty years of data used here, close Democratic party wins did not occur systematically earlier or later than close defeats.

Table 2 reports the core results of the paper. As an anchor, the top-panel of Table 2 reports results for the highly visible fiscal outcomes in Ferreira and Gyourko (2009, TableII). None of these highly visible budget items respond to whether there is a Democratic Party or Republican Party mayor in power. Columns 1 and 2 include no controls other than a linear or quadratic function $f(\text{VSD}_{jt})$ of the running variable. As a point of comparison, columns 3–4 also report on the equivalent OLS estimations, again with no controls other than a linear or quadratic function $f(\text{VSD}_{jt})$. The OLS point estimates are somewhat different from the RD point estimates but the key observation is the consistent lack of any statistical relation between the mayor's party and any

Table 2: Effect of Democratic Mayor on Visible Fiscal Outcomes vs Pension Outcomes

	(1)	(2)	(3)	(4)
	RDD		OLS	
	quadratic	linear	quadratic	linear
City Fiscal Outcomes				
Δ_{+5} log per capita revenues $_{t-1}$	-0.037	-0.029	-0.008	-0.050
	[0.569]	[0.576]	[0.919]	[0.438]
Δ_{+5} log per capita taxes $_{t-1}$	-0.011	-0.027	0.044	-0.047
	[0.800]	[0.453]	[0.471]	[0.222]
Δ_{+5} log per capita expenditures $_{t-1}$	-0.036	-0.029	-0.032	-0.042
	[0.469]	[0.489]	[0.664]	[0.401]
Δ_{+5} log # city employees per resident $_{t-1}$	-0.065	-0.045	-0.076	-0.049
	[0.193]	[0.257]	[0.264]	[0.255]
Δ_{+5} % spent on salaries $_{t-1}$	0.014	0.010	0.028	0.009
	[0.426]	[0.503]	[0.192]	[0.528]
Δ_{+5} % spent on police departmnt $_{t-1}$	0.159	0.024	0.205	0.039
	[0.158]	[0.634]	[0.199]	[0.260]
Δ_{+5} % spent on fire departmnt $_{t-1}$	-0.000	-0.001	0.005	-0.004
	[0.969]	[0.853]	[0.472]	[0.458]
Δ_{+5} % spent on parks and recreation $_{t-1}$	0.001	0.000	0.003	-0.001
	[0.879]	[0.997]	[0.731]	[0.874]
Pension Outcomes				
Δ_{+5} total benefits / #beneficiaries $_{t-1}$	5.128***	3.322***	3.814***	3.517***
	[0.000]	[0.000]	[0.001]	[0.000]
Δ_{+5} contributions / #active members $_{t-1}$	1.210*	0.819	1.285***	0.521
	[0.056]	[0.323]	[0.002]	[0.418]

Notes: The top-panel of this table reports on the effect of having a Democratic Party mayor on the city-level fiscal outcomes considered in Ferreira and Gyourko (2009, TableII). *(b)* Columns 1–2 report on the RD results, which will be the empirical focus from here on, with no controls and only a a linear or quadratic approximation of $f(\text{VSD}_{jt})$ included. As a point of comparison, this table also reports results from an OLS estimation in columns 3–4. *(c)* In columns 1–2, the choice of bandwidth is automated based on MSE-minimization (Cattaneo et al., 2018, 4.2.4), separately for each estimation. In columns 3–4, the bandwidth was set so that the number of observations equals the average number of observations in columns 1–2 (N=384). *(c) p-values* are reported in brackets for standard errors clustered at the city-level.

of the variables under any of the specifications.

The bottom-panel of Table 2 reports on the core estimation of the paper, i.e. estimating the effect of a close Democratic Party victory on per capita pension benefits in a city's pension plans. Results for pensions are strikingly different than those for the more visible budget items in the top panel. Depending on the specification, a city's pension plan's average per capita benefit payments increase by \$3,500–\$5,000 more in five years after a narrow Democratic Party mayoral win. Contributions per active member increase by less (around \$500 to \$1,200) and the effect is much less precise. It is worth reiterating the point made in footnote 9: The researcher can only measure if an increase in payments is statistically large relative to increases in observed contributions, not if it is too large in an accounting-sense. In later specifications, contributions will be included as a control variable when estimating the effect of a Democratic Party win on plan benefits.

As discussed, the choice of considering benefits four years after the election reflects a tradeoff: On the one hand, a longer time period is preferred because changing benefits (through statutory changes or in the next round of collective bargaining) takes time, and because benefit increases to current workers may be more pronounced than to current retirees, and the former can only be detected in the ASPP data once workers become retirees. On the other hand, too long of a time period means going beyond the next election, which frequently means a different mayor.

Table 3 considers different time horizons. The pattern that emerges is that the effect on pension benefits is still weak two years after the election (i.e. Δ_{+3}), begins to show up three years after the election (i.e. Δ_{+4}) is strongest at the end of the mayor's electoral cycle (i.e. Δ_{+5}), and then loses precision in the year after, i.e. into the subsequent mayoral term. A similar pattern can be observed for contributions, where increases show up most precisely three and four years after an election.

The patterns in columns 1–2 hold true in columns 3–4 when control variables are added that may influence the evolution of pensions, namely 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. Column 5–6 additionally investigate the response of pensions benefits *conditional* on contributions changes. Results are broadly unaffected, suggesting that the main effect on benefits is on average not offset by contributions.

Since the elections and the pension plans in the data range from the 1960s to today, the robustness of the results to converting all nominal figures into constant 2010 dollar terms is also checked,

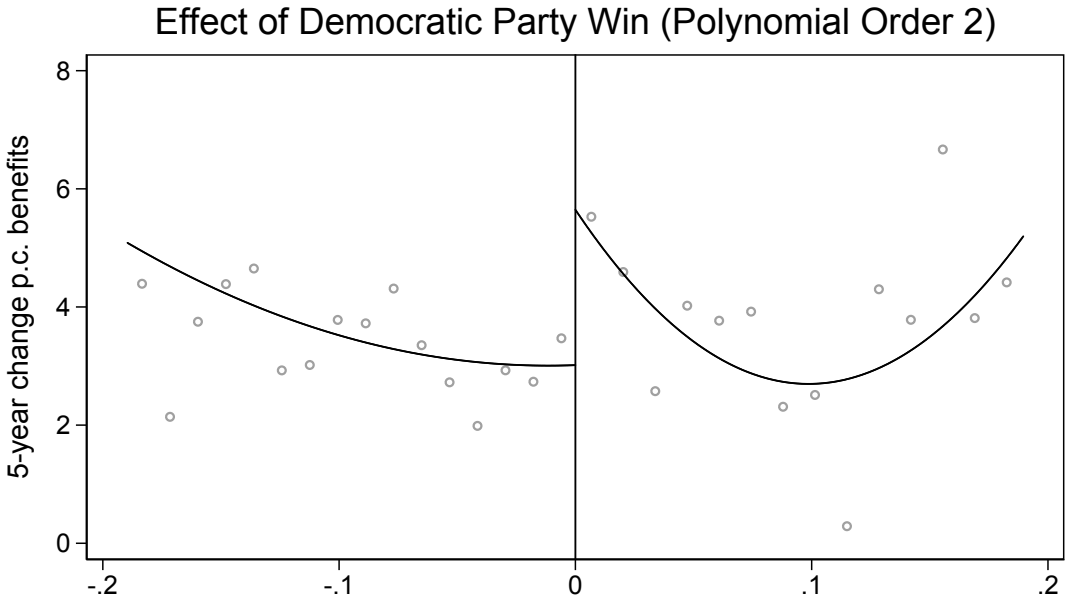
Table 3: Robustness and Time Path of Adjustments

	(1)	(2)	(3)	(4)	(5)	(6)
total benefits / # beneficiaries						
Δ_{+3}	2.379*	0.624	2.726*	0.849	2.734*	0.845
	[0.095]	[0.558]	[0.068]	[0.401]	[0.066]	[0.404]
Δ_{+4}	3.766***	1.899**	3.413***	2.108**	3.474***	2.030**
	[0.001]	[0.019]	[0.003]	[0.018]	[0.002]	[0.022]
Δ_{+5}	5.128***	3.322***	3.105***	2.924***	3.216***	2.903***
	[0.000]	[0.000]	[0.007]	[0.002]	[0.007]	[0.003]
Δ_{+6}	3.309**	2.683**	2.512*	2.393**	0.315	1.455
	[0.022]	[0.022]	[0.055]	[0.026]	[0.819]	[0.157]
total contributions / # active members						
Δ_{+3}	-0.224	-0.397	-0.460	-0.653		
	[0.883]	[0.637]	[0.779]	[0.572]		
Δ_{+4}	1.110	1.674***	1.440	1.812***		
	[0.332]	[0.003]	[0.182]	[0.007]		
Δ_{+5}	1.210*	0.819	0.733	0.920		
	[0.056]	[0.323]	[0.315]	[0.268]		
Δ_{+6}	0.130	0.559	0.073	0.888*		
	[0.925]	[0.124]	[0.956]	[0.071]		
total benefits / # beneficiaries (constant 2010 dollars)						
Δ_{+3}	1.638	0.259	2.088*	0.604	2.082*	0.598
	[0.171]	[0.768]	[0.093]	[0.465]	[0.094]	[0.469]
Δ_{+4}	3.126***	1.438**	2.738***	1.605**	2.720***	1.554**
	[0.001]	[0.040]	[0.004]	[0.025]	[0.004]	[0.028]
Δ_{+5}	4.194***	2.360***	2.160**	2.218***	2.388**	2.375***
	[0.000]	[0.002]	[0.017]	[0.004]	[0.011]	[0.002]
Δ_{+6}	2.700**	2.027**	1.886*	1.723**	-0.154	0.959
	[0.026]	[0.038]	[0.057]	[0.033]	[0.886]	[0.214]
total contributions / # active members (constant 2010 dollars)						
Δ_{+3}	-0.352	-0.516	-0.599	-0.790		
	[0.813]	[0.525]	[0.707]	[0.484]		
Δ_{+4}	0.654	1.396***	1.258	1.458***		
	[0.597]	[0.002]	[0.204]	[0.008]		
Δ_{+5}	1.103**	0.540	0.554	0.624		
	[0.020]	[0.453]	[0.362]	[0.392]		
Δ_{+6}	0.327	0.354	0.017	0.512		
	[0.756]	[0.221]	[0.988]	[0.156]		
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 on the RD results, which will be the empirical focus from here on, with no controls and 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. *(c) p-values* are reported in brackets for standard errors clustered at the city-level.

in the two bottom panels of Table 3. The qualitative patterns on the evolution of the changes in benefits and contributions hold true in constant-dollar terms. In magnitude, the results are slightly smaller in constant 2010 dollar terms.

Figure 1: RD plot, Effect on Pension Benefits



Notes: This figure visualizes the core result in column 3 of the top-panel in Table 3.

An appealing feature of any RDD is the transparency afforded by the fact that it is so easily graphically illustrated. Figure 1 visually displays the core result for per capita pension benefit increases, using the specification in column 3 of the top-panel in Table 3. The benefits data are relatively noisy even when conditioning on controls, but the discontinuity is clearly visible at the cutoff.

3.3 From Benefits to Under-Funding

As previously discussed, employers (unlike employees) have the ability to not pay part of their required contributions, thus creating lower funding levels in the future. If it is indeed the artificial budget neutrality of under-funded pension benefits that makes them an attractive form of political pork, then we should see under-funding going up with benefit increases. This means cities should start falling behind on their contribution payments. A useful feature of the ASPP is that it reports both the employee-paid and employer-paid portions of contribution payments.

Because the employee-paid portion is taken out of payroll, employees always pay their portion in full. With the split between employee-paid and employer-paid actuarially required contributions fixed, a decline in the employer-paid relative to the employee-paid actual contributions is therefore a direct proxy for pension under-funding.

The top-left panel of Table 4 indeed shows that starting four years after a narrow Democrat win, i.e. coincident with the most pronounced benefit increases, cities begin to fall behind on their portion of the contribution payments. The results in the top-left panel of Table 4 are the most prima-facie evidence that the main estimate on benefit increases is directly linked to pensions under-funding.

Table 4: Mechanisms for Adjustment

	(1)	(2)		(3)	(4)
employer contributions / employee contributions			log # active members		
Δ_{+3}	0.059	-2.685	Δ_{+3}	0.027	0.015
	[0.957]	[0.167]		[0.587]	[0.688]
Δ_{+4}	1.724	-0.785	Δ_{+4}	-0.082*	-0.033
	[0.575]	[0.600]		[0.050]	[0.539]
Δ_{+5}	-4.389**	-3.452**	Δ_{+5}	-0.163***	-0.101*
	[0.041]	[0.043]		[0.005]	[0.050]
Δ_{+6}	-5.416**	-4.829**	Δ_{+6}	-0.052	0.044
	[0.015]	[0.018]		[0.405]	[0.465]
# beneficiaries / # contributors			log plan assets in $		
Δ_{+3}	-0.190	-0.214	Δ_{+3}	0.122	0.118
	[0.445]	[0.331]		[0.391]	[0.286]
Δ_{+4}	-0.180	-0.246	Δ_{+4}	-0.001	-0.010
	[0.531]	[0.323]		[0.991]	[0.911]
Δ_{+5}	0.206*	0.119*	Δ_{+5}	-0.059	-0.063
	[0.096]	[0.091]		[0.665]	[0.559]
Δ_{+6}	0.107	-0.033	Δ_{+6}	-0.125	-0.154
	[0.553]	[0.790]		[0.375]	[0.166]
Polynomial	quadratic	linear		quadratic	linear

Notes: *(a)* This table reports on four additional pension outcomes that shed light on the adjustment mechanisms: The top-left panel reports on the ratio of employer to employee contributions. The top-right panel reports the number of retires vs active contributors. The bottom-left reports on the log of active members (contributors). The bottom-right reports on the log of plan assets. *(b) p-values* are reported in brackets for standard errors clustered at the city-level.

On average benefit increases will also incentivize public sector employees to retire earlier.[17] Consistent with this, the top-right panel suggests that starting around four years after the election,

[17] This is discussed in detail in Appendix A.

there is a drop in active, i.e. contributing, members. More pertinently, the bottom-left shows that the ratio of benefit recipients to contributing members is falling at this time; i.e. fewer workers pay into the pension plan, and more retirees take benefits out of it. Consistent with these pieces of evidence, the bottom-right panel of Table 4 shows evidence that plans' asset base begins to be negatively affected around four years after the election. That effect is very imprecisely estimated, but this is not surprising given the considerable smoothing that occurs in plans' actuarial asset calculations.

3.4 Evidence on Politics

In looking for more evidence on the political economy mechanisms, one hypothesis that suggests itself is that the benefit increases associated with a Democratic Party mayor should be strongest for those plans representing the most well-organized groups of municipal employees. As discussed in Section 2, teachers' pensions are almost always organized in state-wide plans and thus not part of the data here. That leaves a comparison between plans for police and fire-fighters and those for 'general municipal employees'. The data is split roughly equally between these two kinds of plans. (Pension plans for police and fire-fighters are organized jointly in roughly half of the cases so that it is not practical to split the sample further between these two.) The top-two panels of Table 5 show that the effect is indeed concentrated in pension plans for police and fire-fighters relative to general city employee plans. The baseline effect of $3,322 for the linear specification in Table 3 decomposes into a larger effect of $5,516 for beneficiaries in police and fire-fighter plans, and a practically non-existent effect for general plans. This evidence is again highly suggestive of a political economy explanation for the problem of under-funded pensions.

Incumbent mayors are not constrained to expand pension benefits after the election, indeed they may prefer to do so before an election to mobilize political support more effectively. A second hypothesis that suggests itself is therefore that the benefit increases associated with a Democratic Party mayor should be stronger for challengers than for incumbent mayors. Indeed, the bottom-two panels of Table 5 suggest that the effect of a Democratic Party win on pension benefits is concentrated in elections where the narrow winner was a challenger rather than an incumbent.[18]

[18] The data happens to again be split roughly equally between these two kinds of elections. Being a challenger does not necessarily imply running against an incumbent; the incumbent may not have run.

Table 5: Plan Types and Election Types

	(1)	(2)	(3)	(4)
Plan Type: police and fire-fighters				
Δ_{+5} per capita pension benefits $_{t-1}$	7.290***	5.516***	5.973***	4.158***
	[0.000]	[0.001]	[0.001]	[0.003]
Δ_{+5} per capita pension contributions $_{t-1}$	1.704	0.539	1.135	0.026
	[0.155]	[0.710]	[0.234]	[0.984]
Plan Type: general municipal plans				
Δ_{+5} per capita pension benefits $_{t-1}$	1.065	0.329	0.706	-0.065
	[0.430]	[0.769]	[0.506]	[0.943]
Δ_{+5} per capita pension contributions $_{t-1}$	0.825**	0.847***	0.624*	0.699**
	[0.048]	[0.009]	[0.077]	[0.013]
Mayor Type: winner=challenger				
Δ_{+5} per capita pension benefits $_{t-1}$	6.136***	4.992***	4.893***	3.948***
	[0.000]	[0.000]	[0.000]	[0.000]
Δ_{+5} per capita pension contributions $_{t-1}$	0.364	1.616*	0.406	1.230*
	[0.733]	[0.083]	[0.616]	[0.077]
Mayor Type: winner=incumbent				
Δ_{+5} per capita pension benefits $_{t-1}$	1.218	0.868	0.345	0.122
	[0.575]	[0.626]	[0.844]	[0.934]
Δ_{+5} per capita pension contributions $_{t-1}$	-0.400	-1.206	-0.514	-1.261
	[0.668]	[0.394]	[0.560]	[0.363]
Polynomial	quadratic	linear	quadratic	linear
Inflation-Adjusted			✓	✓

Notes: *(a)* This table slices the baseline results into sub-samples: First by the beneficiary group, and then by the type of electoral win. Of the total of 1,195 plan-elections, 567 are for police and fire-fighter plans, and 628 for general plans. Further, 537 are for elections where the winner was the challenger and 658 are for plans where the winner was the incumbent. *(b)* Columns 1–2 report on data in nominal terms, with no added controls and only a linear or quadratic approximation of $f(\text{VSD}_{jt})$ included. Columns 3–4 report on data in constant 2010 dollars. *(c) p-values* are reported in brackets for standard errors clustered at the city-level.

4 Conclusion

This paper's focus is on the biggest fiscal challenge that U.S. cities face in the coming decades, namely public-sector pension obligations. Using a regression discontinuity design around close elections, the paper's core finding is that per capita pension benefits increase by about $2,000–3,000 per person (expressed in constant 2010 dollars) after a narrow Democratic Party mayoral win. At the same time as benefits go up, cities begin falling behind on their contributions. This is suggestive evidence for the political economy nature of the problem of underfunded public-sector pension plans: the shrouded nature of pension benefits and the misleading budget neutrality of letting them go underfunded make pensions a useful political tool to politicians seeking to get elected. Consistent with this narrative, the effects are concentrated in the plans of the most well-organized municipal employees, namely police and fire-fighters.

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Appendix A Background on Pension Under-Funding

This paper's empirical focus is on changes in benefits relative to changes in contributions, since these are what is observed in the ASPP Census data. Section Appendix A.1 explains how these *flows* of benefits and contributions determines the (future) *stock* of pensions' funding levels. Section Appendix A.2 discusses the value of under-funded pension benefits relative to fully funded ones in practice.

Appendix A.1 Actuarial Accounting

The basic metric of a public pension's funding gap is the difference between its assets and its discounted future benefit obligations committed to its pensioners and active members. This gap is referred to as a plan's *Unfunded Actuarially Accrued Liabilities* (UAAL). The actuarial accounting that goes into calculating a plan's UAAL is complicated, but it can be broadly summarized (at time τ) by the following expression

$$\text{UAAL}_{i\tau} = \text{Assets}_{i\tau} - \sum_{t>\tau}^{\infty} \frac{\text{Benefits}_{it}}{(1+AAR_i)^{t-\tau}}. \tag{2}$$

The AAR is the *Actuarially Assumed Return* on a plan's assets, and at a higher AAR future benefit obligations are discounted more steeply. A plan's current assets consist of past contributions paid into the plan (by employers and employees) and the return that was earned on those. In Defined Contribution (DC) plans, benefits payments are directly tied to asset returns and expression (2) equals zero by construction. However, almost all municipal pensions in the U.S. are *Defined Benefit* (DB) plans, where future benefits in (2) are legally binding obligations, irrespective of a plan's funding.

A funding gap opens up in a plan for essentially two reasons that we may describe as political-economy problems.[19] The first reason is straightforward: the ARC are divided into employer- and employee-paid portions, and while the employee portion is taken out of paychecks and cannot be shirked, the employing government can in fact simply not pay their contributions (Brown and

[19] In addition, funding gaps can also open up because of variation in fund management since $\text{Assets}_{i\tau}$, i.e. the plan's assets at time τ are determined by the returns on past contributions. However, there is so much smoothing in actuarial return calculations of pension funds that short run variation in fund management performance is not a strong force in the data.

Dye, 2015; Brinkman, Coen-Pirani, and Sieg, 2018). This may be attractive to an employer because it relieves budgetary pressure in the short run, although the obligation is still owed in the long run. Evidence suggests that a significant chunk of pension under-funding is caused by employers not paying their contributions (Brown and Dye, 2015; Munnell, Aubry, Cafarelli, et al., 2015).

The second reason is less straightforward: when the actuarial assumptions used to calculate the ARC are unrealistic (e.g. an AAR that is too high), then this will not have an impact on plans' official funding levels since these are calculated based on the same actuarial assumptions. However, when these assumptions are adjusted or made more realistic, such changes can lead to fairly sudden jumps in funding calculations. To better understand this second reason for underfunding, the following provides a discussions of the forms that benefit expansions can take, and of the actuarial assumptions that link benefit expansions to the calculation of actuarially required contributions (ARC).

Benefit increases can occur through collective bargaining, but are also frequently determined by statute or by the executive.[20] Benefit increases take a number of forms. The simplest form is a straightforward increase of benefits, e.g. 10% higher benefits for all recipients. A more common (and more 'shrouded') form is "formula enhancements". Many plans are on formulas such as "2 at 50," which means a worker can retire starting at age 50, and draw a pension that equals 2% of their last annual salary for every year of service. So a policeman who has worked since age 20 could retire at age 50 and receive 60% of their last year's salary as a pension, or retire at age 65 and draw 90% of their last annual salary. Formula enhancements take the form of moving a "2 at 50" formula to a "3 at 50" formula, or a "3 at 55" formula to a "3 at 50" formula (Greenhut, 2009).

Actuarially required contributions (ARC) are calculated by independent actuarial accountants, and are by definition 'actuarially adequate' under the given actuarial assumptions. These actuarial assumptions are, however, determined by pension boards and are heavily politicized choices (Greenhut 2009, 43, Anzia and Moe 2016, 9).[21] One important set of actuarial assumptions pertains to modeling retirement choices: Many benefit enhancements will in practice lead to earlier retirement, which reduces a member's years of contributions (and thus lowers the expected

[20] At the municipal level, this means the city council or the mayor. At the state level, this means the state legislature or the governor (Anzia and Moe, 2016, p.9).

[21] There are exceptions. For example, in some states, the ARC are legislated to be a fixed share of payroll (Anzia and Moe, 2016, p.17).

asset base) and increases that member's years of drawing benefits. Actuarial models often do not adequately account for changes in expected retirement ages, thus creating blind spots in the ARC calculations (Mitchell and Smith, 1994, 282). A second important actuarial assumption is the AAR: pension boards have in the past neutralized the transmission from benefit expansion to the ARC by simultaneously increasing their AAR (Mitchell and Smith 1994, footnote1, Kelley 2014, 24, Novy-Marx and Rauh 2011). The AAR in most plans is between 7 and 8 percent, and it is almost always higher than actual returns have been over the last decade (Wall Street Journal, 2016).[22] Efforts to lower plans' AAR have been the most acrimonious battleground in the pension field in recent years, typically fought out between union representatives on and treasury representatives on a plan's board. Lowering the AAR is consequential because it immediately opens up a gap in expression (2), which then immediately results in higher ARC for both employers and employees (Gillers, 2016). The Economist (2017) reports that the *National Association of State Retirement Administrators* estimates that cutting the AAR by 0.25 percentage points increases the required contribution rate of plans' active members (as a proportion of payroll) by two to three percentage points, so that "it is in no one's interest to make more realistic assumptions about returns." Anzia and Moe (2016) provide an illustrative account of the bruising political battles surrounding efforts to reduce the state pensions' AAR in Rhode Islands in 2011 and California in 2015.

Appendix A.2 The Value of Under-funded Pension Promises

This section provides an answer to the question why both public sector unions and politicians may favor unfunded pension benefit expansions.

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 Appendix A.1

[22] A related issue is whether the practice of discounting future obligations at the expected rate of return on assets is appropriate. Logically, it is inconsistent to discount a stream of effectively 'risk-free obligations' at the rate of return of a risky portfolio of assets (Novy-Marx and Rauh, 2009, 2011, 2014a,b; Brown and Wilcox, 2009). Yet, state laws sanction public-sector plans to do precisely this (while simultaneously prohibiting private-sector 401(k) plans from doing the same).

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 et al., 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.[23] 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 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).

[23] They may also be homeowners, and unfunded pension obligations may be capitalized into house prices (Daly, 1969; Glaeser and Ponzetto, 2014; Brinkman et al., 2018).

Appendix B Data Appendix

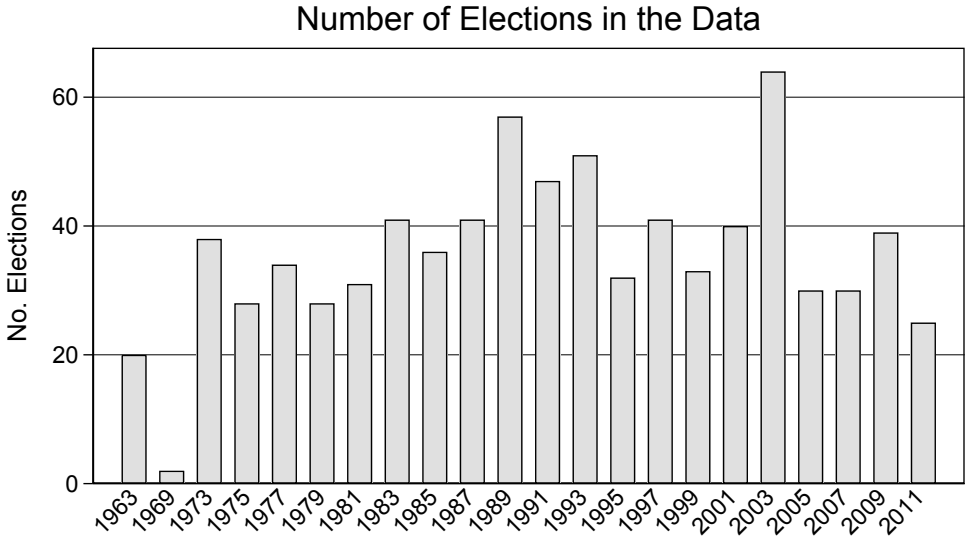
Appendix B.1 Historical Background

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.[24] 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.

Appendix B.2 Elections-to-Plan Data

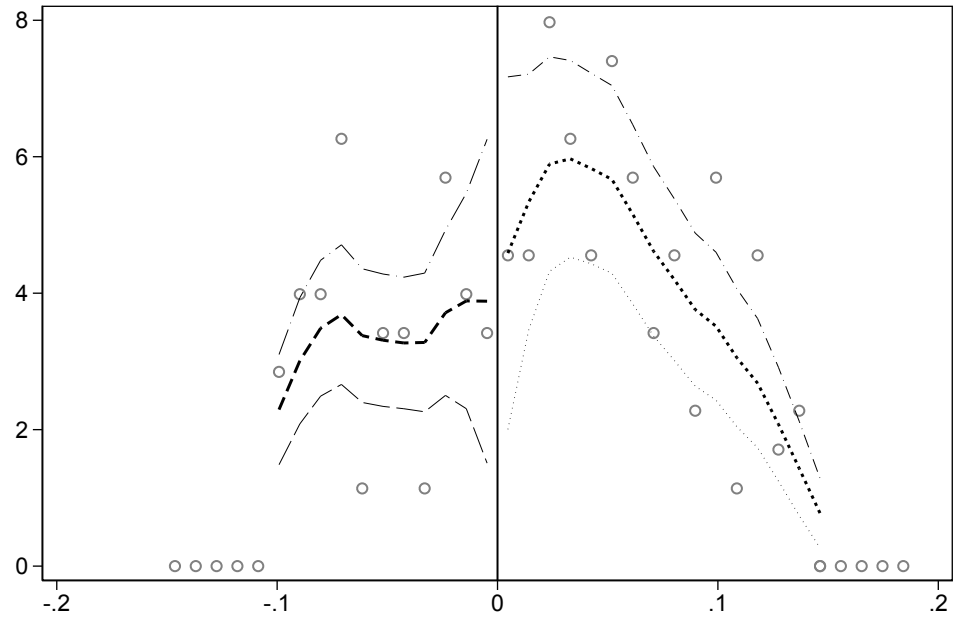
To be included in the analysis, (*i*) a city must be included in the sample of city elections in Ferreira and Gyourko (2009) or Vogl (2014), and (*ii*) and it must have a municipal pension plan covered in the ASPP data. Of the over 4,000 elections in the data, this is true for 1,200, covering 311 plans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

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).

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.

Appendix B.3 Testing the Validity of the Identification Assumptions of the RDD Approach

Appendix B.3.1 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.[25] 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.

Appendix B.3.2 Covariate Balance Across the Close-Election Cutoff

If the identifying assumptions of the RD design hold, covariates should be to be balanced across the cutoff. Table 6 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 6 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. Of the fourteen covariates, only the percentage of revenues spent on salaries displays a marginally significant difference.

[24] 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.

[25] 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 6: Covariate Balance

	Democrat Vote Share 40-50%	Democrat Vote Share 50-60%	Difference
election-year	1992	1990	-2.156
	(10.600)	(11.805)	[0.111]
per capita pension benefits $_{t-1}$	13.527	12.591	-0.936
	(10.293)	(8.596)	[0.399]
per capita pension contributions $_{t-1}$	2.342	1.955	-0.387
	(3.924)	(1.298)	[0.224]
log per capita revenues $_{t-1}$	0.256	0.135	-0.121
	(0.882)	(0.810)	[0.271]
log per capita taxes $_{t-1}$	-0.851	-0.921	-0.070
	(0.938)	(0.802)	[0.537]
log per capita expenditures $_{t-1}$	0.241	0.113	-0.128
	(0.859)	(0.825)	[0.247]
log # city employees per resident $_{t-1}$	-3.963	-3.960	0.003
	(0.615)	(0.560)	[0.968]
% spent on salaries $_{t-1}$	0.369	0.395	0.027*
	(0.115)	(0.096)	[0.052]
% spent on police departm $_{t-1}$	0.056	0.057	0.001
	(0.039)	(0.081)	[0.897]
% spent on fire departm $_{t-1}$	0.088	0.097	0.009
	(0.055)	(0.113)	[0.469]
% spent on parks and recreation $_{t-1}$	0.045	0.038	-0.007
	(0.045)	(0.098)	[0.498]
log total population $_{t-1}$	12.344	12.419	0.075
	(1.563)	(1.389)	[0.672]
log total revenues $_{t-1}$	12.592	12.706	0.114
	(1.831)	(1.595)	[0.608]
log total expenditures $_{t-1}$	12.576	12.684	0.108
	(1.815)	(1.596)	[0.626]
Observations	117	178	295

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.

Appendix C Cities in the Pension-Plan to Election Matched Data-Set

Alabama (Birmingham) *No Obs 1973-85: 22; No Obs 1986-95: 28; No Obs 1996-2005: 24; No Obs 2006-15: 19.* •

Alabama (Dothan) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 8; No Obs 2006-15: 0.* •

Alabama (Montgomery) *No Obs 1973-85: 8; No Obs 1986-95: 9; No Obs 1996-2005: 2; No Obs 2006-15: 4.* •

Alabama (Phenix City) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 0; No Obs 2006-15: 0.* •

Alabama (Tuscaloosa) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 9; No Obs 2006-15: 3.* •

Alaska (Anchorage municipality) *No Obs 1973-85: 3; No Obs 1986-95: 8; No Obs 1996-2005: 1; No Obs 2006-15: 4.* •

Arizona (Phoenix) *No Obs 1973-85: 7; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 6.* •

Arizona (Tucson) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 8; No Obs 2006-15: 10.* •

Arkansas (Pine Bluff) *No Obs 1973-85: 0; No Obs 1986-95: 19; No Obs 1996-2005: 20; No Obs 2006-15: 6.* •

Arkansas (Rogers) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 5; No Obs 2006-15: 0.* •

California (Fresno) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 14; No Obs 2006-15: 7.* •

California (Long Beach) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 2.* •

California (Los Angeles) *No Obs 1973-85: 33; No Obs 1986-95: 30; No Obs 1996-2005: 24; No Obs 2006-15: 30.* •

California (Oakland) *No Obs 1973-85: 13; No Obs 1986-95: 9; No Obs 1996-2005: 5; No Obs 2006-15: 0.* •

California (Pasadena) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 5; No Obs 2006-15: 0.* •

California (Sacramento) *No Obs 1973-85: 9; No Obs 1986-95: 9; No Obs 1996-2005: 9; No Obs 2006-15: 8.* •

California (San Diego) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 5; No Obs 2006-15: 8.* •

California (San Francisco) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 10.* •

California (San Jose) *No Obs 1973-85: 22; No Obs 1986-95: 20; No Obs 1996-2005: 20; No Obs 2006-15: 15.* •

Colorado (Denver) *No Obs 1973-85: 34; No Obs 1986-95: 20; No Obs 1996-2005: 11; No Obs 2006-15: 11.* •

Colorado (Fort Collins) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 0; No Obs 2006-15: 0.* •

Colorado (Littleton) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 0; No Obs 2006-15: 0.* •

Colorado (Longmont) *No Obs 1973-85: 3; No Obs 1986-95: 14; No Obs 1996-2005: 10; No Obs 2006-15: 8.* •

Connecticut (Bristol) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 16.* •

Connecticut (Cromwell) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 5.* •

Connecticut (Darien) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 9.* •

Connecticut (East Hartford) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 7.* •

Connecticut (Fairfield) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 2.* •

Connecticut (Farmington) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 3; No Obs 2006-15: 4.* •

Connecticut (Granby) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 5.* •

Connecticut (Greenwich) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 9.* •

Connecticut (Hamden) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 4.* •

Connecticut (Hartford) *No Obs 1973-85: 7; No Obs 1986-95: 10; No Obs 1996-2005: 5; No Obs 2006-15: 8.* •

Connecticut (Middletown) *No Obs 1973-85: 3; No Obs 1986-95: 6; No Obs 1996-2005: 0; No Obs 2006-15: 3.* •

Connecticut (Milford) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 5; No Obs 2006-15: 7.* •

Connecticut (New Britain) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 12; No Obs 2006-15: 16.* •

Connecticut (New Haven) *No Obs 1973-85: 21; No Obs 1986-95: 17; No Obs 1996-2005: 13; No Obs 2006-15: 12.* •

Connecticut (Norwalk) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 18; No Obs 2006-15: 23.* •

Connecticut (Norwich) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 3; No Obs 2006-15: 8.* •

Connecticut (Stamford) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 7; No Obs 2006-15: 15.* •

Connecticut (Suffield) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 3.* •

Connecticut (Torrington) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 18; No Obs 2006-15: 9.* •

Connecticut (Wallingford) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 0.* •

Connecticut (Waterbury) *No Obs 1973-85: 9; No Obs 1986-95: 8; No Obs 1996-2005: 6; No Obs 2006-15: 10.* •

Connecticut (Westbrook) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 0.* •

Connecticut (Westport) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 3; No Obs 2006-15: 15.* •

Delaware (Wilmington) *No Obs 1973-85: 6; No Obs 1986-95: 14; No Obs 1996-2005: 9; No Obs 2006-15: 11.* •

Florida (Apopka) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 2.* •

Florida (Bradenton) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 6; No Obs 2006-15: 1.* •

Florida (Cape Coral) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 10.* •

Florida (Davie) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 7.* •

Florida (Dunedin) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 0.* •

Florida (Fort Lauderdale) *No Obs 1973-85: 0; No Obs 1986-95: 10; No Obs 1996-2005: 14; No Obs 2006-15: 19.* •

Florida (Fort Pierce) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 11.* •

Florida (Hialeah) *No Obs 1973-85: 4; No Obs 1986-95: 9; No Obs 1996-2005: 2; No Obs 2006-15: 8.* •

Florida (Hollywood) *No Obs 1973-85: 0; No Obs 1986-95: 27; No Obs 1996-2005: 24; No Obs 2006-15: 24.* •

Florida (Kissimmee) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 0.* •

Florida (Lake Worth) *No Obs 1973-85: 1; No Obs 1986-95: 26; No Obs 1996-2005: 14; No Obs 2006-15: 3.* •

Florida (Melbourne) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 7.* •

Florida (Miami Beach) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 4.* •

Florida (Miami) *No Obs 1973-85: 19; No Obs 1986-95: 17; No Obs 1996-2005: 18; No Obs 2006-15: 20.* •

Florida (Ocala) *No Obs 1973-85: 5; No Obs 1986-95: 4; No Obs 1996-2005: 3; No Obs 2006-15: 17.* •

Florida (Orlando) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 18.* ●

Florida (Ormond Beach) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 6.* ●

Florida (Pinellas Park) *No Obs 1973-85: 1; No Obs 1986-95: 6; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Florida (Plantation) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 8; No Obs 2006-15: 8.* ●

Florida (St. Petersburg) *No Obs 1973-85: 11; No Obs 1986-95: 21; No Obs 1996-2005: 20; No Obs 2006-15: 18.* ●

Florida (Tallahassee) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 7; No Obs 2006-15: 15.* ●

Florida (Tampa) *No Obs 1973-85: 22; No Obs 1986-95: 18; No Obs 1996-2005: 9; No Obs 2006-15: 10.* ●

Florida (West Palm Beach) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 16; No Obs 2006-15: 3.* ●

Georgia (Albany) *No Obs 1973-85: 6; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Georgia (Atlanta) *No Obs 1973-85: 33; No Obs 1986-95: 30; No Obs 1996-2005: 17; No Obs 2006-15: 30.* ●

Georgia (Savannah) *No Obs 1973-85: 0; No Obs 1986-95: 5; No Obs 1996-2005: 6; No Obs 2006-15: 5.* ●

Illinois (Addison) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 5.* ●

Illinois (Alton) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 6.* ●

Illinois (Arlington Heights) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 18; No Obs 2006-15: 3.* ●

Illinois (Aurora) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 6; No Obs 2006-15: 16.* ●

Illinois (Berwyn) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 6.* ●

Illinois (Bloomington) *No Obs 1973-85: 0; No Obs 1986-95: 14; No Obs 1996-2005: 15; No Obs 2006-15: 0.* ●

Illinois (Calumet City) *No Obs 1973-85: 0; No Obs 1986-95: 14; No Obs 1996-2005: 6; No Obs 2006-15: 6.* ●

Illinois (Carol Stream) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 0.* ●

Illinois (Champaign) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 7; No Obs 2006-15: 14.* ●

Illinois (Chicago Heights) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 6; No Obs 2006-15: 4.* ●

Illinois (Chicago) *No Obs 1973-85: 53; No Obs 1986-95: 50; No Obs 1996-2005: 38; No Obs 2006-15: 24.* ●

Illinois (Cicero) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Illinois (DeKalb) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 4; No Obs 2006-15: 0.* ●

Illinois (Decatur) *No Obs 1973-85: 0; No Obs 1986-95: 14; No Obs 1996-2005: 20; No Obs 2006-15: 12.* ●

Illinois (Des Plaines) *No Obs 1973-85: 0; No Obs 1986-95: 12; No Obs 1996-2005: 16; No Obs 2006-15: 5.* ●

Illinois (Dolton) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 2; No Obs 2006-15: 4.* ●

Illinois (Downers Grove) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 8.* ●

Illinois (East St. Louis) *No Obs 1973-85: 0; No Obs 1986-95: 14; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Illinois (Elgin) *No Obs 1973-85: 0; No Obs 1986-95: 10; No Obs 1996-2005: 14; No Obs 2006-15: 8.* ●

Illinois (Elk Grove Village) *No Obs 1973-85: 0; No Obs 1986-95: 9; No Obs 1996-2005: 16; No Obs 2006-15: 4.* ●

Illinois (Elmhurst) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 0; No Obs 2006-15: 4.* ●

Illinois (Elmwood Park) *No Obs 1973-85: 0; No Obs 1986-95: 20; No Obs 1996-2005: 7; No Obs 2006-15: 0.* ●

Illinois (Evanston) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 8; No Obs 2006-15: 10.* ●

Illinois (Freeport) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 5; No Obs 2006-15: 3.* ●

Illinois (Glen Ellyn) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 10; No Obs 2006-15: 3.* ●

Illinois (Glenview) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 5.* ●

Illinois (Granite City) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 8.* ●

Illinois (Hanover Park) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 8; No Obs 2006-15: 0.* ●

Illinois (Harvey) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 4; No Obs 2006-15: 4.* ●

Illinois (Hoffman Estates) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 4.* ●

Illinois (Lansing) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Illinois (Lombard) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 1; No Obs 2006-15: 9.* ●

Illinois (Maywood) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 4; No Obs 2006-15: 0.* ●

Illinois (Moline) *No Obs 1973-85: 0; No Obs 1986-95: 9; No Obs 1996-2005: 8; No Obs 2006-15: 8.* ●

Illinois (Mount Prospect) *No Obs 1973-85: 0; No Obs 1986-95: 13; No Obs 1996-2005: 18; No Obs 2006-15: 7.* ●

Illinois (Naperville) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 13; No Obs 2006-15: 13.* ●

Illinois (Niles) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 7.* ●

Illinois (Northbrook) *No Obs 1973-85: 0; No Obs 1986-95: 5; No Obs 1996-2005: 1; No Obs 2006-15: 10.* ●

Illinois (Oak Forest) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Illinois (Oak Lawn) *No Obs 1973-85: 0; No Obs 1986-95: 12; No Obs 1996-2005: 16; No Obs 2006-15: 7.* ●

Illinois (Oak Park) *No Obs 1973-85: 0; No Obs 1986-95: 2; No Obs 1996-2005: 5; No Obs 2006-15: 6.* ●

Illinois (Orland Park) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 3.* ●

Illinois (Palatine) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 7; No Obs 2006-15: 10.* ●

Illinois (Park Ridge) *No Obs 1973-85: 0; No Obs 1986-95: 2; No Obs 1996-2005: 14; No Obs 2006-15: 10.* ●

Illinois (Pekin) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 6; No Obs 2006-15: 0.* ●

Illinois (Peoria) *No Obs 1973-85: 3; No Obs 1986-95: 18; No Obs 1996-2005: 11; No Obs 2006-15: 2.* ●

Illinois (Quincy) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 6.* ●

Illinois (Rock Island) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 10; No Obs 2006-15: 4.* ●

Illinois (Rockford) *No Obs 1973-85: 6; No Obs 1986-95: 20; No Obs 1996-2005: 20; No Obs 2006-15: 6.* ●

Illinois (Schaumburg) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 3; No Obs 2006-15: 10.* ●

Illinois (Skokie) *No Obs 1973-85: 0; No Obs 1986-95: 2; No Obs 1996-2005: 16; No Obs 2006-15: 18.* ●

Illinois (Springfield) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 14; No Obs 2006-15: 16.* ●

Illinois (Streamwood) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 8; No Obs 2006-15: 0.* ●

Illinois (Tinley Park) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 10; No Obs 2006-15: 3.* ●

Illinois (Urbana) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 14; No Obs 2006-15: 9.* ●

Illinois (Wheaton) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 4; No Obs 2006-15: 0.* ●

Illinois (Wheeling) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 8; No Obs 2006-15: 6.* ●

Illinois (Wilmette) *No Obs 1973-85: 0; No Obs 1986-95: 10; No Obs 1996-2005: 4; No Obs 2006-15: 4.* ●

Indiana (Anderson) *No Obs 1973-85: 0; No Obs 1986-95: 13; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Indiana (Columbus) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Indiana (East Chicago) *No Obs 1973-85: 0; No Obs 1986-95: 2; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Indiana (Evansville) *No Obs 1973-85: 3; No Obs 1986-95: 11; No Obs 1996-2005: 5; No Obs 2006-15: 0.* ●

Indiana (Fort Wayne) *No Obs 1973-85: 6; No Obs 1986-95: 15; No Obs 1996-2005: 1; No Obs 2006-15: 0.* ●

Indiana (Frankfort) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 8.* ●

Indiana (Gary) *No Obs 1973-85: 3; No Obs 1986-95: 10; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Indiana (Greensburg) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 0.* ●

Indiana (Hammond) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 6; No Obs 2006-15: 0.* ●

Indiana (Huntington) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 2.* ●

Indiana (Indianapolis) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 1.* ●

Indiana (Kokomo) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Indiana (Lake Station) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 1.* ●

Indiana (Marion) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 1; No Obs 2006-15: 0.* ●

Indiana (Muncie) *No Obs 1973-85: 0; No Obs 1986-95: 5; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Indiana (New Castle) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 4; No Obs 2006-15: 2.* ●

Indiana (South Bend) *No Obs 1973-85: 8; No Obs 1986-95: 20; No Obs 1996-2005: 14; No Obs 2006-15: 0.* ●

Iowa (Council Bluffs) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Iowa (Davenport) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Iowa (Dubuque) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Iowa (Mason City) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Kansas (Wichita) *No Obs 1973-85: 0; No Obs 1986-95: 14; No Obs 1996-2005: 15; No Obs 2006-15: 10.* ●

Kentucky (Bowling Green) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Kentucky (Covington) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Kentucky (Henderson) *No Obs 1973-85: 1; No Obs 1986-95: 2; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Kentucky (Paducah) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Louisiana (Baton Rouge) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 5; No Obs 2006-15: 3.* ●

Louisiana (Lake Charles) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 0; No Obs 2006-15: 0.* •

Louisiana (New Orleans) *No Obs 1973-85: 37; No Obs 1986-95: 30; No Obs 1996-2005: 15; No Obs 2006-15: 15.* •

Louisiana (Shreveport) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 1; No Obs 2006-15: 4.* •

Maryland (Baltimore) *No Obs 1973-85: 22; No Obs 1986-95: 20; No Obs 1996-2005: 15; No Obs 2006-15: 22.* •

Massachusetts (Arlington) *No Obs 1973-85: 4; No Obs 1986-95: 6; No Obs 1996-2005: 4; No Obs 2006-15: 8.* •

Massachusetts (Attleboro) *No Obs 1973-85: 2; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 3.* •

Massachusetts (Boston) *No Obs 1973-85: 16; No Obs 1986-95: 14; No Obs 1996-2005: 10; No Obs 2006-15: 10.* •

Massachusetts (Chicopee) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 8; No Obs 2006-15: 5.* •

Massachusetts (Everett) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 3; No Obs 2006-15: 0.* •

Massachusetts (Fall River) *No Obs 1973-85: 11; No Obs 1986-95: 7; No Obs 1996-2005: 4; No Obs 2006-15: 3.* •

Massachusetts (Gloucester) *No Obs 1973-85: 0; No Obs 1986-95: 1; No Obs 1996-2005: 8; No Obs 2006-15: 6.* •

Massachusetts (Holyoke) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 10.* •

Massachusetts (Lawrence) *No Obs 1973-85: 1; No Obs 1986-95: 10; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

Massachusetts (Leominster) *No Obs 1973-85: 3; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 10.* •

Massachusetts (Marlborough) *No Obs 1973-85: 5; No Obs 1986-95: 10; No Obs 1996-2005: 4; No Obs 2006-15: 8.* •

Massachusetts (Melrose) *No Obs 1973-85: 0; No Obs 1986-95: 5; No Obs 1996-2005: 8; No Obs 2006-15: 0.* •

Massachusetts (New Bedford) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 4; No Obs 2006-15: 6.* •

Massachusetts (Northampton) *No Obs 1973-85: 3; No Obs 1986-95: 10; No Obs 1996-2005: 8; No Obs 2006-15: 5.* •

Massachusetts (Peabody) *No Obs 1973-85: 11; No Obs 1986-95: 9; No Obs 1996-2005: 3; No Obs 2006-15: 5.* •

Massachusetts (Quincy) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 5; No Obs 2006-15: 0.* •

Massachusetts (Salem) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 5; No Obs 2006-15: 3.* •

Massachusetts (Taunton) *No Obs 1973-85: 10; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 1.* •

Massachusetts (Waltham) *No Obs 1973-85: 9; No Obs 1986-95: 6; No Obs 1996-2005: 7; No Obs 2006-15: 1.* •

Massachusetts (Westfield) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 4; No Obs 2006-15: 0.* •

Massachusetts (Woburn) *No Obs 1973-85: 6; No Obs 1986-95: 10; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

Michigan (Ann Arbor) *No Obs 1973-85: 6; No Obs 1986-95: 3; No Obs 1996-2005: 10; No Obs 2006-15: 8.* •

Michigan (Dearborn Heights) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 5; No Obs 2006-15: 0.* •

Michigan (Detroit) *No Obs 1973-85: 23; No Obs 1986-95: 18; No Obs 1996-2005: 14; No Obs 2006-15: 20.* •

Michigan (Farmington Hills) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 8; No Obs 2006-15: 1.* •

Michigan (Flint) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 6; No Obs 2006-15: 5.* •

Michigan (Kalamazoo) *No Obs 1973-85: 7; No Obs 1986-95: 7; No Obs 1996-2005: 7; No Obs 2006-15: 10.* •

Michigan (Lincoln Park) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 12; No Obs 2006-15: 4.* •

Michigan (Madison Heights) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 7.* ●

Michigan (Oak Park) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 10; No Obs 2006-15: 9.* ●

Michigan (Roseville) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 6; No Obs 2006-15: 3.* ●

Michigan (Royal Oak) *No Obs 1973-85: 5; No Obs 1986-95: 3; No Obs 1996-2005: 2; No Obs 2006-15: 8.* ●

Michigan (Southfield) *No Obs 1973-85: 3; No Obs 1986-95: 20; No Obs 1996-2005: 12; No Obs 2006-15: 0.* ●

Michigan (Sterling Heights) *No Obs 1973-85: 0; No Obs 1986-95: 2; No Obs 1996-2005: 14; No Obs 2006-15: 0.* ●

Michigan (Taylor) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 2; No Obs 2006-15: 0.* ●

Michigan (Troy) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 9; No Obs 2006-15: 2.* ●

Michigan (Warren) *No Obs 1973-85: 13; No Obs 1986-95: 14; No Obs 1996-2005: 6; No Obs 2006-15: 6.* ●

Michigan (Wyoming) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 3.* ●

Minnesota (Bloomington) *No Obs 1973-85: 0; No Obs 1986-95: 7; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Minnesota (Minneapolis) *No Obs 1973-85: 42; No Obs 1986-95: 35; No Obs 1996-2005: 20; No Obs 2006-15: 10.* ●

Minnesota (Rochester) *No Obs 1973-85: 0; No Obs 1986-95: 7; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Mississippi (Jackson) *No Obs 1973-85: 4; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 0.* ●

Missouri (Columbia) *No Obs 1973-85: 0; No Obs 1986-95: 10; No Obs 1996-2005: 4; No Obs 2006-15: 7.* ●

Missouri (Kansas City) *No Obs 1973-85: 36; No Obs 1986-95: 40; No Obs 1996-2005: 40; No Obs 2006-15: 16.* ●

Missouri (Kansas) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 7; No Obs 2006-15: 1.* ●

Missouri (Springfield) *No Obs 1973-85: 0; No Obs 1986-95: 7; No Obs 1996-2005: 8; No Obs 2006-15: 10.* ●

Missouri (St. Joseph) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 0.* ●

Missouri (St. Louis) *No Obs 1973-85: 44; No Obs 1986-95: 31; No Obs 1996-2005: 24; No Obs 2006-15: 11.* ●

Nebraska (Lincoln) *No Obs 1973-85: 4; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 10.* ●

Nebraska (Omaha) *No Obs 1973-85: 33; No Obs 1986-95: 24; No Obs 1996-2005: 24; No Obs 2006-15: 22.* ●

New Hampshire (Manchester) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 10.* ●

New Jersey (Jersey City) *No Obs 1973-85: 9; No Obs 1986-95: 10; No Obs 1996-2005: 6; No Obs 2006-15: 8.* ●

New Jersey (Newark) *No Obs 1973-85: 9; No Obs 1986-95: 10; No Obs 1996-2005: 1; No Obs 2006-15: 0.* ●

New York (New York) *No Obs 1973-85: 52; No Obs 1986-95: 39; No Obs 1996-2005: 31; No Obs 2006-15: 46.* ●

North Carolina (Charlotte) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 8.* ●

North Carolina (Winston-Salem) *No Obs 1973-85: 9; No Obs 1986-95: 10; No Obs 1996-2005: 6; No Obs 2006-15: 10.* ●

North Dakota (Bismarck) *No Obs 1973-85: 3; No Obs 1986-95: 24; No Obs 1996-2005: 30; No Obs 2006-15: 0.* ●

North Dakota (Fargo) *No Obs 1973-85: 0; No Obs 1986-95: 23; No Obs 1996-2005: 14; No Obs 2006-15: 14.* ●

North Dakota (Minot) *No Obs 1973-85: 0; No Obs 1986-95: 17; No Obs 1996-2005: 16; No Obs 2006-15: 6.* ●

Oklahoma (Lawton) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 6; No Obs 2006-15: 2.* ●

Oklahoma (Oklahoma City) *No Obs 1973-85: 25; No Obs 1986-95: 12; No Obs 1996-2005: 18; No Obs 2006-15: 15.* •

Oklahoma (Tulsa) *No Obs 1973-85: 22; No Obs 1986-95: 6; No Obs 1996-2005: 6; No Obs 2006-15: 6.* •

Oregon (Portland) *No Obs 1973-85: 2; No Obs 1986-95: 6; No Obs 1996-2005: 0; No Obs 2006-15: 1.* •

Pennsylvania (Allentown) *No Obs 1973-85: 8; No Obs 1986-95: 20; No Obs 1996-2005: 4; No Obs 2006-15: 6.* •

Pennsylvania (Erie) *No Obs 1973-85: 0; No Obs 1986-95: 21; No Obs 1996-2005: 13; No Obs 2006-15: 6.* •

Pennsylvania (Lancaster) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 16; No Obs 2006-15: 10.* •

Pennsylvania (Philadelphia) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 7; No Obs 2006-15: 22.* •

Pennsylvania (Pittsburgh) *No Obs 1973-85: 32; No Obs 1986-95: 23; No Obs 1996-2005: 11; No Obs 2006-15: 11.* •

Pennsylvania (Scranton) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 3; No Obs 2006-15: 19.* •

Pennsylvania (State College) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 1; No Obs 2006-15: 9.* •

Pennsylvania (Wilkes-Barre) *No Obs 1973-85: 0; No Obs 1986-95: 3; No Obs 1996-2005: 6; No Obs 2006-15: 16.* •

Pennsylvania (Williamsport) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 24.* •

Rhode Island (Cranston) *No Obs 1973-85: 0; No Obs 1986-95: 16; No Obs 1996-2005: 7; No Obs 2006-15: 7.* •

Rhode Island (Newport) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

South Carolina (Greenville) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 10; No Obs 2006-15: 1.* •

Tennessee (Chattanooga) *No Obs 1973-85: 9; No Obs 1986-95: 11; No Obs 1996-2005: 16; No Obs 2006-15: 5.* •

Tennessee (Knoxville) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 10.* •

Tennessee (Memphis) *No Obs 1973-85: 22; No Obs 1986-95: 18; No Obs 1996-2005: 20; No Obs 2006-15: 20.* •

Texas (Abilene) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 6; No Obs 2006-15: 0.* •

Texas (Amarillo) *No Obs 1973-85: 3; No Obs 1986-95: 10; No Obs 1996-2005: 10; No Obs 2006-15: 5.* •

Texas (Austin) *No Obs 1973-85: 14; No Obs 1986-95: 20; No Obs 1996-2005: 20; No Obs 2006-15: 22.* •

Texas (Beaumont) *No Obs 1973-85: 3; No Obs 1986-95: 10; No Obs 1996-2005: 6; No Obs 2006-15: 2.* •

Texas (Corpus Christi) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 3.* •

Texas (Dallas) *No Obs 1973-85: 24; No Obs 1986-95: 20; No Obs 1996-2005: 12; No Obs 2006-15: 26.* •

Texas (Fort Worth) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 8; No Obs 2006-15: 4.* •

Texas (Houston) *No Obs 1973-85: 15; No Obs 1986-95: 26; No Obs 1996-2005: 24; No Obs 2006-15: 21.* •

Texas (Longview) *No Obs 1973-85: 0; No Obs 1986-95: 6; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

Texas (McAllen) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 0.* •

Texas (Midland) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 10; No Obs 2006-15: 1.* •

Texas (Odessa) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 0.* •

Texas (San Antonio) *No Obs 1973-85: 11; No Obs 1986-95: 10; No Obs 1996-2005: 6; No Obs 2006-15: 6.* •

Texas (Temple) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 8; No Obs 2006-15: 0.* •

Texas (Tyler) *No Obs 1973-85: 0; No Obs 1986-95: 4; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

Virginia (Newport News) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 0; No Obs 2006-15: 2.* •

Virginia (Richmond) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 2; No Obs 2006-15: 10.* •

Washington (Seattle) *No Obs 1973-85: 0; No Obs 1986-95: 0; No Obs 1996-2005: 3; No Obs 2006-15: 10.* •

Washington (Tacoma) *No Obs 1973-85: 0; No Obs 1986-95: 7; No Obs 1996-2005: 10; No Obs 2006-15: 8.* •

West Virginia (Charleston) *No Obs 1973-85: 0; No Obs 1986-95: 8; No Obs 1996-2005: 2; No Obs 2006-15: 0.* •

Wisconsin (Milwaukee) *No Obs 1973-85: 6; No Obs 1986-95: 7; No Obs 1996-2005: 0; No Obs 2006-15: 4.* •