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POLITICAL PARTIES DO MATTER IN U.S. CITIES ... FOR THEIR UNFUNDED PENSIONS

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ABSTRACT

Using data covering a wide range of municipal public-sector pension plans from 1962– 2014, I establish that unfunded pension benefits grow faster under Democratic-party mayors. The result is borne out in a generalized difference-in-differences (DiD) specification in levels and in growth rates as well as in a regression discontinuity design (RDD) focusing on narrow mayoral races. There is some evidence that the partisan effect is concentrated in police and fire-fighter plans. Being on a council-manager system matters very little to these patterns. While Tiebout sorting has been the proposed explanation for previous findings that parties do not matter for a range of fiscal outcomes in U.S. cities, Tiebout sorting may actually accentuate fiscal profligacy in the case of unfunded pensions.

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1 Introduction

In their influential study, Ferreira and Gyourko (2009) showed that the identity of the party in power does not matter for a range of fiscal outcomes in U.S. cities. Without contesting the results for the outcomes considered in said study, the present paper demonstrates that the party in power actually does matter for the biggest fiscal challenge that U.S. cities face in the coming decades, namely unfunded pension obligations.

Nearly all municipal government employees in the U.S. today are on Defined Benefit (DB) pension plans (Brown and Wilcox, 2009),[1] and these plans are underfunded by several trillion dollars.[2] Unlike federal social security, municipal and state benefit payments are legally binding commitments, and state laws make it almost impossible to renegotiate them (Burns, 2011; Trusts, 2013). Ultimately, it is therefore tomorrow's taxpayers that are on the hook. Where the funding gap is sufficiently large, it can easily culminate in municipal bankruptcy.[3] While fund management performance undeniably plays a role—see e.g. Novy-Marx and Rauh (2009)— the more critical problem is that increases in promised future benefits have for decades systematically outstripped increases in contributions needed to fund them (Greenhut 2009, p.40, Munnell, Aubry, and Quinby 2011, Brown and Dye 2015). At its core, it is clear that this is a political-economy problem: The benefits of increased pension benefits go to a narrowly defined group of public-sector employees, whose labor unions are well-organized and over-represented on public pensions' boards. By contrast, the costs are broadly dispersed to a constituency that is poorly organized, namely future tax payers.[4] In addition, unfunded pension liabilities are not properly labeled as debt, and are therefore largely budget-neutral, making them an attractive substitute to public-sector wage increases in the eyes of politicians (Johnson, 1997; Munnell et al., 2011; Mohan and Zhang, 2014). In summary, "consistent low-balling of pension costs over the past two decades has made it easy for elected officials

[1] This is in contrast to the Defined Contribution (DC) plans, such as 401(k) plans, that dominate the private sector. Washington State is the only of the 50 states where public employees' pensions are DC plans.

[2] It is difficult to give a precise number since there is no *universal* database that includes all plans. Novy-Marx and Rauh (2009) provide a precise estimate of \$3.23 trillion for the largest state and municipal plans; this set of the largest may amount to around ninety percent of the sum total in 2009. Pensions fundedness has eroded further since then.

[3] According to Anderson (2013), "between 2007 and 2013, residents of twenty-eight cities suffered drastic cuts in fire and police protection as their 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).

[4] The 'logic of collective action' suggests that government benefits will tend to go to narrow well-organized special interest groups but be financed by a broad base of taxpayers (Olson, 2009; Grossman and Helpman, 2001).

and union representatives to agree on very valuable benefits, for very much smaller current pay concessions."[5] These collective-action problems are well-understood by economists, if not by the public, and there is a number of theoretical political-economy papers that model unfunded pensions as a function of voter heterogeneity along the lines of this explanation (Glaeser and Ponzetto, 2014; Bouton, Conconi, Pino, and Zanardi, 2014; Bouton, Lizzeri, and Persico, 2016).[6]

This paper takes the collective-action problems that cause unfunded pensions as given, and asks the more narrow question of whether the party in power matters for the extent of these problems at the city-level.[7] Without looking at the data, the answer is far from clear. On the one hand, anecdotal evidence suggests that Democratic party politicians are closer to (and more dependent on the political support of) the public-sector unions who lobby for pension increases (Greenhut, 2009, p.137). On the other hand, there is no evidence that Democratic party mayors are more fiscally profligate than Republican ones on other issues (Ferreira and Gyourko, 2009), and it is not clear that the Democratic party is more fiscally profligate at any level of government.[8]

The primary source of data used in this paper is the U.S. Census' *Annual Survey of Public Pensions* (ASPP) for the years 1992–2015. The ASPP is then linked to a largely overlapping set of pension-plans is covered by the Census' *Historical Database on Public Employee-Retirement Systems* from 1962–1991. The main metric for pensions' fundedness are *Unfunded Actuarially Accrued Liabilities* (UAAL), i.e. the difference between current assets and projected future benefit payments. Plans' UAAL is only included in the ASPP's reports since 2012, i.e. there are two few years for a panel analysis of the UAAL.[9] In lieu of the stock of unfunded liabilities that is the UAAL, this paper's primary focus is therefore on the evolution of the two primary flows that govern the UAAL,

[5] Quote from a speech by Jeremy Gold, member of the American Academy of Actuaries and the Society of Actuaries Pension Financing Task Force, at MIT's Golub Center for Finance and Policy, in November 2015.

[6] Bouton et al. (2014) show that politicians may cater to a minority of voters (e.g. public employees) if that minority feels very intensely about an issue that the majority is oblivious to. Building on the model in Glaeser, Ponzetto, and Shapiro (2005), where politicians try to 'bring out their core', Glaeser and Ponzetto (2014) split the electorate into public-sector employees and other tax payers, where only the former understand the 'shrouded' benefits and costs of unfunded pension benefit increases.

[7] 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).

[8] It is perhaps worth emphasizing that not finding any partisan effect would not not imply that the core problem of unfunded pensions is not a political one; it would merely indicate that there are no differences across parties in the extent of the problem. Similarly, finding a partisan effect does not indicate that unfunded pensions are a problem that involves only one party.

[9] A plan's UAAL cannot be estimated from other available variables because it requires detailed knowledge of the plan's internal accounting and projected demographics.

namely *per capita benefit payments* (as they are being paid out to retirees) relative to *per capita contributions* (as they are being paid by active members).[10] Each municipal pension plan is uniquely mapped to the city that employs the plans' contributors and beneficiaries. Municipal plans can therefore be linked to city-level mayoral elections.[11] As well, pensions can be linked to information on whether a city is organized under the council-manager system (Vlaicu and Whalley, 2016; Ferreira and Gyourko, 2009).

A first observation is that over the half-decade from 1962 to 2015 there have been strong positive trends in per capita benefits that are unexplained by concurrent changes in contributions. In short, the growth in benefits has vastly outstripped the growth of contributions across the board.

The first set of results comes from a generalized difference-in-difference (DiD) estimation in levels, which includes plan fixed effects as well as varying region-specific trends. The core finding is that changes in the political party of the mayor have a sizeable effect on the pension plans in a city: Identifying within-city and over-time, spells with a Democratic-party mayor are associated with an increase in pension benefits of between 450$ and 750$ annually per person, between 4 and 5 percent of the average annual per capita pension. This effect is more pronounced relative to independent mayors than relative to Republican mayors, but is statistically significant for both comparisons. This finding is more pronounced for 'narrow-constituency' plans whose unions organize at the city level, namely police and fire-fighter plans. This remains true whether allowing for state-specific, county-specific and even city-specific time-trends. (In the last case, the regressions identify the differential growth of police and fire-fighter plans relative to other plans within the same city). The interaction between Democratic mayor with a dummy for the city being on the council-manager system is negative and sizeable (shaving off about half the partisan effect), but is not statistically significant.

A second set of results comes from redefining the outcome to be the growth in per capita benefits from a year before to a year after an election, again with plan fixed effects and varying region-specific trends. This variation is more closely tied to elections, and less driven by long-run

[10] The paper uses the terms 'unfunded' or 'excess' benefits as a shorthand for observed changes in per capita benefit payments after conditioning out observed changes in per capita contributions. Actuarially unfunded benefit payments could only be measured with full knowledge of a plans' future beneficiaries' age structure, and the actual promises made to them relative to contributions asked of them, which is knowledge unavailable to the researcher.

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

trends in plan characteristics and political preferences. In the growth-specification, a Democratic win is again associated with a differential increase in plan benefits relative to costs. The magnitude of the effect is smaller when focusing only on changes around elections: a Democratic mayor is associated with an increase in annual per capita pension benefits of around \$150 per person. In the growth-specification, there is no differential effect by plan type, indicating that the presence of this interaction in levels-specification is explained by a longer-running co-evolution of a city's pension plan benefits and the party in power. The interaction with the council-manager system is again negative and insignificant, but in this specification also negligible in size.

To gain better identification on the effect of the mayor's party, the paper uses a regression discontinuity design (RDD) around close elections. By focusing on narrow election victories, the RDD controls for confounding factors that independently shape the outcome of interest (Lee, Moretti, and Butler, 2004; Ferreira and Gyourko, 2009; Eggers, Fowler, Hainmueller, Hall, and Snyder Jr, 2015). Relating the growth in a pension plan's unfunded per capita benefit payments to the mayoral election winner, the RDD shows that a Democratic mayor on average increased excess annual per capita benefits between \$250 and \$450, i.e. within the range suggested by the preceding two sets of results. There is again no differential effect by plan type, re-affirming that this differential effect in the DiD is either explained by confounding trends or at least not driven by changes around elections.

In summary, the paper finds robust and consistent evidence for a partisan effect on the current crisis in municipal pensions' fundedness.[12] Because the RRD identifies a local treatment effect around close elections, it cannot distinguish whether this partisan effect is explained by differences in ideology or 'pork barrel politics'.

Regardless of its explanation, the partisan effect is seemingly at odds with the result in Ferreira and Gyourko (2009) that the identity of the party in power has no explanatory power for any of a wide range of fiscal outcomes in U.S. cities. However, this puzzle dissolves on closer inspection. The main explanation for the apparent lack of partisan effects in Ferreira and Gyourko (2009) is Tiebout-sorting, e.g. city residents voting with their feet. However, while the average voter pays attention to a city's spending on public goods like education, and to a city's overall budgetary

[12] This does not imply that the broader political-economy problems to do with public-sector pensions are a phenomenon that only involves the Democratic party, merely that the problem is more pronounced with Democratic mayors.

discipline, the same voter is likely to be un-attentive to the problem of future liabilities arising from unfunded public pensions. In fact, Tiebout-sorting is likely to 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.

In the following, section 2 provides background information on how the actuarial accounting of public pensions works, and on the political processes by which their fundedness can change. section 3 describes the data. Section 4 presents the results. Section 5 concludes.

2 Background

2.1 Actuarial Accounting

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

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

There are a number of drivers of a plan's UAAL as described by equation (1).

Fund Management: Assets are determined by the cumulative return that past contributions have earned. As such, fund management plays some role.

Contributions: A key driver of the gap represented by equation (1) is whether benefits have been defined too generously relative to contributions, or whether insufficient contributions have been paid into the pension system. In Defined Contribution (DC) plans, this is not an issue because a pensioner simply receives the actual return on what they paid into the pension. In a DC system, equation (1) equals zero by construction. However, almost all municipal pensions in the U.S.

are *Defined Benefit* (DB) plans. Demographics do not matter for equation (1) if contributions are defined appropriately for the level of benefits, and are paid in full. However, the UAAL increases every year in which this is not the case.

Discounting: The *Actuarially Assumed Return* (AAR) is the assumed future return on assets earned by the plans. At a higher projected return, future benefit obligations are discounted more steeply. Changing the AAR can significantly affect a plan's UAAL. One contentious question is whether the AAR (which is typically between 7.5 and 8 percent) accurately reflects a plan's *expected* rate of return. In recent years, returns have often been considerably below this number. A second contentious question 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).

2.2 Changing Pension Benefits and Contributions in Practice

When pension benefits are expanded, either through collective bargaining or legislation, actuarial accountants calculate what increases in contributions are required to cover the "'normal cost' of these benefit increases. Benefit increases take a number of forms. The simplest is an across-the-board increase of benefits, e.g. 25% higher benefits for all recipients. A more common way is to "enhance the benefits formula." Many plans are on formulas such as "2 at 50," which means a worker can retire starting as early as age 50, and draw a pension that equals 2% of their last annual salary for every year they worked. So a policeman who has been in service 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. 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.

It is unlikely that required contributions adjust to cover the benefit increases inherent in such formula changes. This depends on whether and how actuarial calculations incorporate changes in expected retirement ages of active plan members, and there are likely to be "blind spots" in actuarial calculations in this respect (Mitchell and Smith, 1994, 282).[13]

[13] Another frequently used way of increasing benefits is through increases in cost-of-living-adjustments (Gale and

For most plans, their AAR of between 7–8% percent is out of step with their actual returns (Wall Street Journal, 2016). Some states have in the past allowed plans to neutralize the transmission from benefit expansion to required contribution by letting them simultaneously increase their AAR (Mitchell and Smith 1994, footnote1, Kelley 2014, p24). By contrast, during the recent years of lower returns, it has been politically difficult to bring the AAL in line with actual earned returns. This is because a lower transmits directly into required pension contributions from 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 points, so that "it is in no one's interest to make more realistic assumptions about returns." Anzia and Moe (2016) provides 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.

2.3 The Spatial Scope of Unions and Pension Plans

Whether pension plans are organized at the municipal or state-level depends to a degree on the scope at which unions operate. Police and fire-fighter associations have traditionally almost always been organized at the city-level, and as a result police and fire-fighter unions are mostly locally organized today, and so are their pension plans.[14] By contrast, teachers unions have 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. As a result, very few teachers' pensions plans are included in the data used in this study.

Krupkin, 2016). There is also a number of ways of boosting benefits —e.g. 'DROP accounts' and 'OPEBs'— that would not show up in the average per capita benefits and contributions that I study in this paper. DROP accounts allow workers to retire and draw a pension while simultaneously re-entering employment with same public employer and drawing a salary. Other-than-pension post-employment benefits (OPEBs) are a grab-bag of benefits such as healthcare that are not included in the core benefit payments (Greenhut, 2009, 64,96).

[14] 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.

3 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 linked across the two data-sources, thanks to a rich set of identifiers in both. From here, the paper will refer to the linked dataset as the ASPP. The ASPP contains rich information on asset values and fund performance going back in time, but without the plans' internally projected NPV of future benefit payments, information on the asset side alone is not enough to calculate the UAAL as described by equation (1). Unfortunately, plans' UAAL is only included in the ASPP's reports beginning in 2012.[15] In lieu of the stock of unfunded liabilities that is the UAAL, this paper's primary focus is therefore on the evolution of the two primary flows that govern the UAAL, namely *per capita benefit payments* (as they are being paid out to retirees) relative to *per capita contributions* (as they are being paid by active members). Each plan in this data is then mapped to its corresponding city. (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.)

The City-Election Data used in this paper is an extension of the data collected by Ferreira and Gyourko (2009).[16] The data on city-management is reported every five years in the *International City Managers Association* (ICMA) *Municipal Year Book*. I use ICMA's 1992 *Municipal Year Book*,[17] updated by Vlaicu and Whalley (2016) for changes in city-management that occurred after 1992.

The linked city-election to plan-year data consists of just over 8,000 plan-year observations, covering 524 plans in 278 cities. Table 1 reports descriptive statistics on the main variables in this data, separately by decade. For reference, Online Appendix A lists all cities in the data and their number of observations by decade.

[15] The *Center for Retirement Research at Boston College* (CRC) has collected the largest plans' UAALs from their annual going back to 2001 and made the data publicly available as the *Public Plans Data*. These data primarily cover state-level pension plans.

[16] I thank Fernando Ferreira for sharing the data used in Ferreira and Gyourko (2009). I thank Tom Vogl for sharing an extension of the same that included more Southern cities (Vogl, 2014). I then further extended the data to cover 2005–2014 elections.

[17] Downloadable from `https://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/4421`.

Table 1: Desciptives

Decade	**Observations**	p.c. benefits (in Thsd $)	p.c. contributions (in Thsd $)	% council-manager system	% police & firefighter plan	% teacher plan	% Democratic Mayor	% Republican Mayor
1972--1985	1,188	5.7	1.0	20.3	35.6	6.5	73.4	20.5
1986--1995	2,196	12.1	2.1	33.6	57.7	2.8	58.3	30.2
1996--2005	1,862	20.9	3.2	38.5	59.0	2.6	54.7	32.5
2006--2015	1,679	33.2	6.2	34.0	52.7	1.3	60.6	26.7

Notes: There are just over 8,000 plan-year observations in the ASPP data that can be mapped to city-election data. Because the regressions include a lagged dependent variable, this table reports on the 6,906 plan-year observations for which we observe the previous year's benefits. This effectively omits 1962, 1967, and 1972 from the data.

4 Results

A striking feature of the evolution of public-sector pensions' per capita benefits and contributions is that growth in the former has across the board far outstripped growth in the latter. This can be seen in Figure 1, which plots the evolution of the annual averages of per capita benefits and contributions in the ACPP data. This figure needs to be taken with a grain of salt. It may, for example, reflect the good investment return of pension funds. Nonetheless, it is hard to imagine that it does not in part reflect the reasons for the general under-fundedness of public-sector pension plans today.

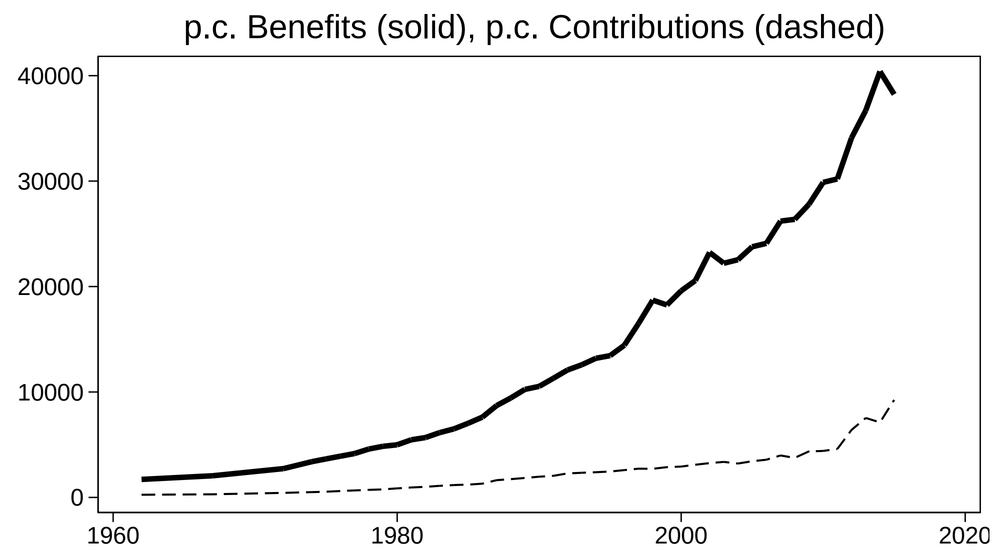
4.1 Generalized Difference-in-Difference Estimation

In this section, the evolution of per capita benefits relative to per capita contributions is investigated through the lens of a generalized difference-in-difference (DiD) model in levels. The core question of the paper is whether the party in power matters for the evolution of pension plans' fundedness. There are just over 8,000 plan-year observations in which we observe pension information as well as information about the last election. I estimate

$$\text{benefits}_{it} = \beta_D \mathrm{D}_{jt} + \beta_X X_{it} + \delta_i + \Omega_{tj} + \epsilon_{it}, \tag{2}$$

where subscript i denotes a plan, j denotes a city, and 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. The coefficient of interest,

Figure 1: per Capita Benefits and Contributions Over Time



Notes: Based on an unbalanced panel of yearly averages of 1,714 plans from the U.S. Census' *Annual Survey of Public Pensions* (ASPP).

β_D, estimates the extent to which a Democratic mayor has a differential effect on the evolution of excess benefits.

In specifications that include the mayor's party it is necessary to always include the lagged dependent variable benefits$_{it-1}$: Because per-capita benefits never decrease in nominal terms, any effect of a change in party gets 'locked in' in nominal terms, so that a Democratic-party mayor losing to a non-Democratic-party one does not reduce per capita plan benefits. Because the linear functional form of regressions does not capture this downward rigidity, it is essential to allow benefits$_{it-1}$ to "absorb" any increases in benefits that may have resulted from a Democratic mayor taking office in the previous period. Including the lagged dependent variable reduces the effective number of observations to 6,906 because it effectively omits 1962, 1967, and 1972 from the regressions.

Table 2 reports on estimations of equation (2). All regressions include plan fixed effects δ_i, and a range of linear time-trends Ω_{tj}. Plan controls X_{it} include per capita contributions and the lagged dependent benefits$_{it-1}$. Columns 1–4 investigate the effect of within-plan changes in a city's party in power. Column 1 investigates this conditioning only on a common linear time trend. This is the core result of the table, and it suggests that a Democratic mayor on average increased excess

Table 2: The Effect of Mayor Party

Outcome	p.c. benefits (in thsd $)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem-Mayor$_{jt}$	0.469	0.739	0.779	0.722	0.791	1.385	1.480	1.216
	[0.076]	[0.028]	[0.039]	[0.038]	[0.107]	[0.063]	[0.078]	[0.161]
Rep-Mayor$_{jt}$					0.403	0.804	0.878	0.612
					[0.404]	[0.241]	[0.259]	[0.448]
Lagged Dependent Variable	0.720	0.583	0.543	0.491	0.720	0.582	0.543	0.490
	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]	[0.000]
p.c. contributions (in thsd $)$_{it}$	0.018	0.013	0.016	0.025	0.018	0.013	0.016	0.025
	[0.268]	[0.435]	[0.369]	[0.255]	[0.270]	[0.435]	[0.363]	[0.253]
pval (test: Democrat=Republican)					0.1519	0.0624	0.0826	0.0518
year x fixed effects for:	common	state	county	city	common	state	county	city
plan fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.742	0.752	0.757	0.763	0.742	0.752	0.757	0.763

Notes: This table reports on 524 plans from the U.S. Census' ASPP which could be linked to available city-level election data. 325 plans are police and fire-fighters plans, 12 are teachers plans, and 187 are 'general plans'. With a lagged dependent variable included, and annual data being reported since 1972, the observations used cover 42 years from 1973–2015. All specifications include plan fixed effects. The number of observations is' in all columns. *p-values* for standard errors clustered at the plan level are reported in square brackets.

annual per capita benefits by about $469, which is four percent of the mean per capita benefit of 13,000$ in the data. Column 2–4 then add more fine-grained trends: column 2 conditions on state-specific time-trends; column 3 conditions on county-specific time-trends; column 4 is the most demanding specification which conditions on city-specific time-trends. With any of these added linear trends, having a Democratic mayor is always associated with a roughly 750$ increase in annual per capita benefits.

It is well-known that in specifications with fixed effects and lagged dependent variables, there is the potential for Nickell bias to affect the coefficients of interest (Nickell, 1981). However, this bias converges to zero as the total number of time periods increases, and the number of years in the data here is much greater than the environments that have typically been associated with Nickel bias. Concretely, the formula for Nickel-bias is given by $plim_{N\to\infty} \simeq \frac{-(1+\gamma)}{T-1}$, where γ is the relationship between the dependent variable y in period t and the dependent variable in period $t-1$. Across specifications, $\hat{\gamma}$ is around 0.6 on average, and with 42 years (1973–2015, omitting 1962, 1967, and 1972), the implies a bias of $\frac{-1.6}{42}$. This equals -0.038, which is never more than 5%

of the estimated coefficient of interest in columns 1–4 of Table 2.

Columns 5–8 of Table 2 add a separate dummy for having a Republican dummy, so that the comparison group becomes the relatively small number of independent mayors in the data. Relative to that more narrow comparison group, the effect of a Democratic mayor becomes larger in magnitude but also less precisely estimated. Having a Republican major is not statistically different from having an independent major. Importantly, the Hausman test reported at the table bottom shows that having a Democratic mayor is also significantly different from having a Republican major across columns 6–8.

The remainder of section 4.1 investigates whether the effect of a Democratic mayor depends on the type of pension plan or on the management form of the city. On the one hand, many observers suggest that the political-economy channels discussed before are particularly pernicious for police and fire-fighters plans; on the other hand, these constituencies tend to be more associated with the Republican party (Greenhut, 2009). In other words, one may expect the partisan effect of Democratic mayors to vary for police and fire-fighter plans, but it is not clear in which direction. One may also expect the effect to be less pronounced when the city is run under the council-manager system, since this system is viewed as being less subject to the political-economy channels discussed above (Vlaicu and Whalley, 2016).

As a starting point, the specification

$$\text{benefits}_{it} = \Omega_{tj} \times \Theta_i + \Omega_{tj} \times \Theta_{jt} + \beta_X X_{it} + \delta_i + \epsilon_{it} \tag{3}$$

inspects the differential evolution of per capita benefits by plan-characteristics Θ_i and by city-management-form Θ_{jt}. Subscript i denotes a plan, subscript j denotes a city, δ_i, and X_{it} are the same as in equation (2).[18]

This data is not

Police and fire-fighter plans are grouped into a single category because they operate shared pension plans in about 20 percent of cases.[19] This category comprises over 50 percent of the plans

[18] The inclusion of the lagged dependent variable implies the omission of all data-points in 1962, 1967 and 1972, which reduces the number of observations. The qualitative patterns in the estimations results of equation (3) are unaffected by this.

[19] The union-affiliation of a plans can be very easily coded from its name. Plans that are specifically for police and fire-fighters almost always say so in the name of the plan. In the rare instance where they do not use those terms, they use other similar terms like 'law enforcement'.

Table 3: Evolution of Pension Benefits by Plan Type and City-Management-Form

Outcome	*p.c. benefits (in thsd $)*				*p.c. benefits ($)*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
D(narrow-constituency)$_{i}$ x t	0.145	0.103	0.152	0.196	143.71	101.21	151.93	205.08
	[0.000]	[0.006]	[0.000]	[0.000]	[0.000]	[0.011]	[0.001]	[0.000]
D(council-manager)$_{jt}$ x t					-0.026	-0.015	-0.589	-0.691
					[0.945]	[0.959]	[0.014]	[0.023]
D(narrow-constituency)$_{i}$ x t x					0.001	0.001	0.001	0.001
D(council-manager)$_{jt}$					[0.339]	[0.219]	[0.068]	[0.091]
year x fixed effects for:	common	state	county	city	common	state	county	city
plan fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.746	0.749	0.763	0.768	0.758	0.761	0.773	0.779

Notes: This table reports on from the U.S. Census' ASPP. All specifications include plan fixed effects, the lagged dependent variable benefits$_{it-1}$, and time trends, and additionally control for changing contributions$_{it}$. *p-values* for standard errors clustered at the plan level are reported in square brackets.

in the data. Their prevalence reflects the tendency of police and fire-fighter unions to organize at the city-level. I also create a teachers-plan category. However, only 3 percent of plans are teachers plans in the municipal data. This reflects the tendency of teachers unions to organize at the state-level; see the background discussion in section 2.3. The other 47 percent of plans constitute the residual 'general civil service' category.

Table 3 reports the results of estimating equation (3). Columns 1–4 investigate plan-specific time trends, i.e. the interaction $\Omega_t \times \Theta_i$. All four columns report on whether there is a differential increase in unfunded benefits for police and fire-fighter plans, relative to a common (only spatially varying) time trend. Indeed, per capita benefits appear to grow more rapidly in police and fire-fighter plans. The coefficient estimate in column 1 implies that benefits in police and fire-fighter plans grow by about $145 more per year than those of other plans. This is three-quarters of the (unreported) common year trend of $200 in column 1. In unreported specifications, I checked for a a separate trend for teachers plans and found none, which may however be due to the sparsity of teacher plans in the municipal data. Columns 2, 3, and 4 allow the time-trend to be, respectively, state-specific, county-specific, or city-specific. Thus, column 4 identifies the differential growth of police and fire-fighter plans relative to other plans within the same city. The estimated magnitude of the differential time trend of police and fire-fighter plans varies with these differential time-

Table 4: The Effect of Mayor Party by Plan-Type

Outcome	*p.c. benefits (in thsd $)*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem-Mayor$_{jt}$ x D(narrow-constituency)$_i$	0.833	1.177	1.346	1.130				
	[0.067]	[0.041]	[0.038]	[0.058]				
Dem-Mayor$_{jt}$ x D(broad-based plan)$_i$	0.060	0.240	0.140	0.276	0.060	0.237	0.136	0.273
	[0.782]	[0.310]	[0.550]	[0.282]	[0.784]	[0.316]	[0.562]	[0.288]
Dem-Mayor$_{jt}$ x D(narrow-constituency[Police&Fire])$_i$					0.904	1.239	1.427	1.202
					[0.061]	[0.042]	[0.039]	[0.059]
Dem-Mayor$_{jt}$ x D(narrow-constituency[Teachers])$_i$					-0.246	0.165	0.130	0.120
					[0.241]	[0.636]	[0.751]	[0.802]
year x fixed effects for:	common	state	county	city	common	state	county	city
plan fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.742	0.752	0.757	0.763	0.742	0.752	0.757	0.763

Notes: This table reports on estimating equation (4). Data comes from 524 plans from the U.S. Census' ASPP which could be linked to available city-level election data. 325 plans are police and fire-fighters plans, 12 are teachers plans, and 187are 'general plans'. With a lagged dependent variable included, and annual data being reported since 1972, the observations used cover 42 years from 1973–2015. All specifications include plan fixed effects. The number of observations is 6,906 in all columns. *p-values* for standard errors clustered at the plan level are reported in square brackets.

trends, but the basic pattern of a significant differential growth-term repeats itself across columns.

Columns 5–8 additionally investigate the effect of the city-management-form, i.e. the interaction $\Omega_t \times \Theta_{jt}$. As perhaps expected, there is a negative coefficient on the council-manager form and it is significant in column 7–8. It is, however, very small: While narrow-constituency plans grow at a differential rate of 152$ a year in column 7, the differential rate of council-manager-form cities' plans is minus 60 cents a year, less than one half of a percent of that. (In columns 5–8 the outcome is scaled in dollars instead of in thousands of dollars to make coefficient-magnitudes readable.) In summary, Table 3 does not suggest the council-manager-system as an important mediating factor in this analysis, but does suggest that the type of plan matters.

Next, we turn to investigating how these factors interact with the main coefficient of interest. In equation

$$\text{benefits}_{it} = \beta_D \mathrm{D}_{jt} + \beta_D^i \mathrm{D}_{jt} \times \Theta_i + \beta_X X_{it} + \delta_i + \Omega_{tj} + \epsilon_{it}, \tag{4}$$

β_D^i estimates the differential effect of the mayor's party on the evolution of excess benefits, depending on plan i's characteristics Θ_i. All regressions continue to include per capita contributions

Table 5: The Effect of Mayor Party by City-Management Form

Outcome	*p.c. benefits (in thsd $)*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem-Mayor$_{jt}$	0.429	0.953	1.034	0.989				
	[0.116]	[0.017]	[0.021]	[0.022]				
Dem-Mayor$_{jt}$ x D(council-manager)$_{jt}$	0.161	-0.542	-0.687	-0.701	-0.127	-0.656	-0.439	-0.566
	[0.783]	[0.448]	[0.344]	[0.342]	[0.733]	[0.150]	[0.370]	[0.276]
Dem-Mayor$_{jt}$ x D(narrow-constituency)$_{i}$					0.751	1.429	1.798	1.510
					[0.096]	[0.039]	[0.025]	[0.046]
Dem-Mayor$_{jt}$ x D(broad)$_{i}$					0.096	0.471	0.284	0.479
					[0.750]	[0.157]	[0.365]	[0.156]
Dem-Mayor$_{jt}$ x D(narrow-constituency)$_{i}$ x D(council-manager)$_{jt}$					0.520	0.129	-0.614	-0.323
					[0.650]	[0.922]	[0.637]	[0.810]
year x fixed effects for:	common	state	county	city	common	state	county	city
plan fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.738	0.748	0.753	0.759	0.738	0.748	0.753	0.759

Notes: This table reports on estimating equation (5), and is otherwise structured the same and uses the same data as Table 4. and using Data comes from 524 plans from the U.S. Census' ASPP which could be linked to available city-level election data. 325 plans are police and fire-fighters plans, 12 are teachers plans, and 187are 'general plans'. With a lagged dependent variable included, and annual data being reported since 1972, the observations used cover 42 years from 1973–2015. All specifications include plan fixed effects. The number of observations is 6,906 in all columns. *p-values* for standard errors clustered at the plan level are reported in square brackets.

and the lagged dependent benefits$_{it-1}$, as well as plan fixed effect δ_i and time-trends Ω_{tj}, but for parsimony these are no longer reported. Table 4 reports the results. Columns 1–4 show that the effect of a Democratic mayor is concentrated in 'narrow-constituency' plans. Columns 5–8 break this up and show this is specifically the plans representing police and fire-fighters unions. Columns 2–4 and 6–8 again introduce region-specific linear trends that become increasingly fine-grained. In columns 4 and 8, the regressions identify the differential effect of Democratic mayors on police and fire-fighter plans relative to other plans within the same city. Equation

$$\text{benefits}_{it} = \beta_D \text{D}_{jt} + \beta_D^i \text{D}_{jt} \times \Theta_i + \beta_D^j \text{D}_{jt} \times \Theta_{jt} + \beta_X X_{it} + \delta_i + \Omega_{tj} + \epsilon_{it} \tag{5}$$

further expands on equation (4) to additionally interact the effect of the mayor's party with the city-management form, where β_D^j estimates whether the effect of a Democratic major is smaller in cities under the council-manager system.

Table 5 reports the results. The interaction β_D^j is negative across columns 1–4, and it is sizeable in magnitude relative to the main coefficient β_D. However, it is never statistically significant. Decomposing the effect of Democratic mayor by plan type (in the same way as columns 1–4 of Table 4) leaves this basic pattern in place: There is some indication that the council-manager system may mitigate the partisan effect but this interaction never reaches levels of statistical significance that would allow drawing strong conclusions.

4.2 Growth-Specification

Like section 4.1, this section also investigates the effect of the mayor's party in a difference-in-differences setting, again with plan fixed effects and varying region-specific trends. The difference is that the level of per capita benefits and per capita contributions is replaced with the two-year change in these figures around an election, i.e. from a year before to a year after an election. The effect is again optionally interacted with plan-type Θ_i or city-management-form Θ_{jt}, so that this section's estimations are all subsumed in equation (5), with benefits$_{it}$ and contributions$_{it}$ being replaced by Δbenefits$_{it}$ and Δcontributions$_{it}$. By focusing on differences in growth-rates around election outcomes, an election becomes an observation, reducing the effective sample from 6,906 (in Table 2, Table 4, and Table 5) to 3,489 in Table 6. In doing so, section 4.2 serves as a transition to the RDD analysis, because the RDD will use the sub-sample of 3,489 elections that were closely contested (as precisely defined in section 4.2).

Columns 1–4 report on specifications where the effect of a Democratic mayor on Δbenefits$_{it}$ is estimated conditional on different region-specific time-trends. Column 1 includes plan fixed effects and Δcontributions$_{it}$ as the only controls. Column 2 adds a common linear trend, column 3 a state-specific trend, and column 4 a city-specific linear trend. The magnitude of the effect of a Democratic mayor on the change in benefits varies between \$159 (in column 1) and \$118 (in column 4). Column 5 adds a separate indicator for a Republican win, leaving independent mayors as the omitted category. Democratic mayors significantly increase the growth in per capita pension relative to independent majors as well as Republican ones, as the Hausman test-statistic at the table-bottom shows. Column 6 adds the interaction with plan-characteristics, i.e. $\beta_D^i \mathrm{D}_{jt} \times \Theta_i$. While Table 4 reported a highly significant interaction between Democratic mayor and the indicator for a police or fire-fighter plan, this association completely disappears in the growth-specification. This suggests the interaction in Table 4 is either driven by confounding long-run

Table 6: The Effect of Mayor Party on Changes in Benefits

Outcome	Δ p.c. benefits (in thsd $)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dem-Mayor$_{jt}$	0.159	0.125	0.139	0.118	0.289	0.132	0.163	0.183
	[0.009]	[0.047]	[0.022]	[0.047]	[0.049]	[0.097]	[0.057]	[0.106]
Rep-Mayor$_{jt}$					0.160			
					[0.294]			
Dem-Mayor$_{jt}$ x D(narrow-constituency)$_{i}$						0.050		-0.043
						[0.673]		[0.799]
Dem-Mayor$_{jt}$ x							-0.013	-0.177
D(council-manager)$_{jt}$							[0.901]	[0.185]
Dem-Mayor$_{jt}$ x D(narrow-constituency)$_{i}$ x								0.281
D(council-manager)$_{jt}$								[0.179]
Δ p.c. contributions (in thsd $)$_{it}$	0.363	0.354	0.344	0.324	0.363	0.363	0.359	0.359
	[0.070]	[0.072]	[0.076]	[0.048]	[0.070]	[0.070]	[0.070]	[0.070]
pval (test: Democrat=Republican)					0.0258			
year x fixed effects for:		common	state	city				
plan fixed effects	Y	Y	Y	Y	Y	Y	Y	Y
R-squared	0.380	0.387	0.402	0.535	0.380	0.380	0.375	0.375

Notes: This table reports on estimating equation (5), and is otherwise structured the same and uses the same data as Table 4. and using Data comes from 524 plans from the U.S. Census' ASPP which could be linked to available city-level election data. 325 plans are police and fire-fighters plans, 12 are teachers plans, and 187are 'general plans'. With a lagged dependent variable included, and annual data being reported since 1972, the observations used cover 42 years from 1973–2015. All specifications include plan fixed effects. The number of observations is 6,906 in all columns. *p-values* for standard errors clustered at the plan level are reported in square brackets.

trends between a city's pension plan benefits and the party in power, or it is at a minimum not driven by changes in benefits that occur right around elections. Column 7–8 add interactions with city management-form, i.e. $\mathrm{D}_{jt} \times \Theta_{jt}$. As in section 4.1, these are not significant. In summary, the main finding of a partisan effect comes out as consistently in the growth-specification as it did in the levels-specification. Columns 6–8 do not, however, suggest that there is any heterogeneity in this partisan effect when it is investigated around elections. The same will turn out to be true in the RDD analysis in section 4.3.

4.3 Regression Discontinuity Design

The results in section 4.1 provide compelling suggestive evidence for a partisan effect of Democratic-party mayors on the evolution of public-sector pension plans. However, the DiD setup cannot ultimately rule out the possibility that unobserved trends confound the results. It may, for example, be that a city-electorate's attitudes becomes simultaneously more positive towards Democratic mayors and towards more generous public-sector pensions (and perhaps specifically towards better pension benefits for police and fire-fighters).

To get better causal identification on the effect of the party in power, I therefore now turn to a regression discontinuity design (RDD) identification strategy. Among non-experimental identification strategies, the RDD has gained increasing credence and popularity, in part because it entails perfect knowledge of the selection process (i.e. the discontinuity) and because it requires comparatively weak assumptions (Lee and Lemieux, 2010). The most prominent application of the RD design to political economy applications has been the use of close election outcomes (Lee et al., 2004; Ferreira and Gyourko, 2009; Dal Bó, Dal Bó, and Snyder, 2009; Ferraz and Finan, 2011). The identifying assumption in this case 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.[20]

The 'sharp RDD' that will be applied here consists of the regression of an outcome (i.e. $\Delta\text{Benefits}_{it}$) on a treatment (i.e. having a Democratic mayor, $\text{D}_{jt} = 1$) that is a sharp or exact function of an underlying running variable (i.e. the vote share for the Democratic candidate VSD_{jt}):

$$\Delta\text{Benefits}_{it} = \beta_D \text{D}_{jt} + f(\text{VSD}_{jt}) + \beta_X X_{it} + \epsilon_{it}. \tag{6}$$

Including a flexible function $f(\text{VSD}_{jt})$ of the running variable itself in equation (6) captures any underling differences in the electorate's preferences and other unobservable that may correlate with who wins the election.

The RDD always requires the researcher to (*i*) make a choice of functional form $f(\text{VSD}_{jt})$ and to (*ii*) choose a bandwidth of how to discount data that is further away from the discontinuity. In choice (*i*), traditional approaches starting with Hahn, Todd, and Van der Klaauw (2001) have favored using flexible higher-polynomial approximations to $f(\text{VSD}_{jt})$.[21] However, Gelman and

[20] The logic of applying the RD design to close election hinges the outcome of a close election being quasi-random. Some studies have called this assumption into question for the U.S. House of Representatives in recent decades. However, Eggers et al. (2015) investigate the validity of this assumption in a wide rang close elections including historical and contemporary elections for the U.S. House, statewide gubernatorial, state legislative, and mayoral races in the U.S., as well as 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.

[21] As Lee and Lemieux (2010) state: "from an applied perspective, a simple way of relaxing the linearity assumption is to include polynomial functions of running variable in the regression model."

Imbens (2018) in particular have identified a number of problems arising from the use of higher-order polynomials. As a result, best practice in RDD now favors using only local linear or at most local quadratic approximations at either side of the threshold, as in the following equation

$$\Delta\text{Benefits}_{it} = \beta_1 \text{VSD}_{jt} + \beta_D \text{D}_{jt} + \beta_2 \text{D}_{jt} \times \text{VSD}_{jt} + \beta_X X_{it} + \epsilon_{it}. \tag{7}$$

In choice (*ii*), there is always a tradeoff between precision and bias: Including observations further away from the discontinuity improves precision by including more data but also introduces bias, since the identifying assumptions are more likely to hold close to the discontinuity. Traditional approaches have provided little guidance on the choice of bandwidth, but best practice now favors 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). Section 4.3 will follow these best-practice recommendations.

Table 7 reports on the full sample available for the RDD analysis mapped to pension-plans data. Each observation has pension-benefits and pension-contributions data in at least one of the two years before the election, and in at least one of the two years after an election. The outcome of interest $\Delta\text{Benefits}_{it}$ is defined as a plan i's per capita benefits averaged for the two years after the election minus its average for the two years before the election. There is some ambiguity in what sample to select for comparison. The full sample includes 569 Democratic-party wins and 316 Republican wins where the runner-up's party is unknown. The full sample also include 596 elections where a Democratic-party candidate beat another Democratic-party candidate.[22] Further, for comparability with other electoral settings where there are no Independents, e.g. the U.S. House, one may wish to focus narrowly on only elections where Democratic-party candidates face Republicans. I therefore define and report on eight possible slicings of the data:

1. Baseline
2. Omit 316 elections with { Rep vs Unknown }, since these may not involve a Democratic-party candidate

[22] There are 199 elections where a Republican beat a Republican, but none of them have data on both $\Delta\text{Benefits}_{it}$ and $\Delta\text{contributions}_{it}$. Since most municipal elections are nominally non-partisan, this is not unusual. In particular, elections where two candidates of the same party face off are not primaries.

Table 7: The RDD Sample

	runner-up:	Unkown	Dem	Ind	Rep	TOTAL
winner:	Dem	569	596	188	858	2,211
winner:	Ind	0	51	0	0	51
winner:	Rep	316	497	0	0	813

Notes: This table reports on the full sample of mayoral elections mapped to pension-plans data. See in-text discussion of the possible slicings of this data for the RD analysis.

3. Omit 596 elections with { Dem vs Dem }

4. Omit 51+188 elections involving an Independent

5. Omit 316 and 596

6. Omit 316 and 51+188

7. Omit 51+188 and 596

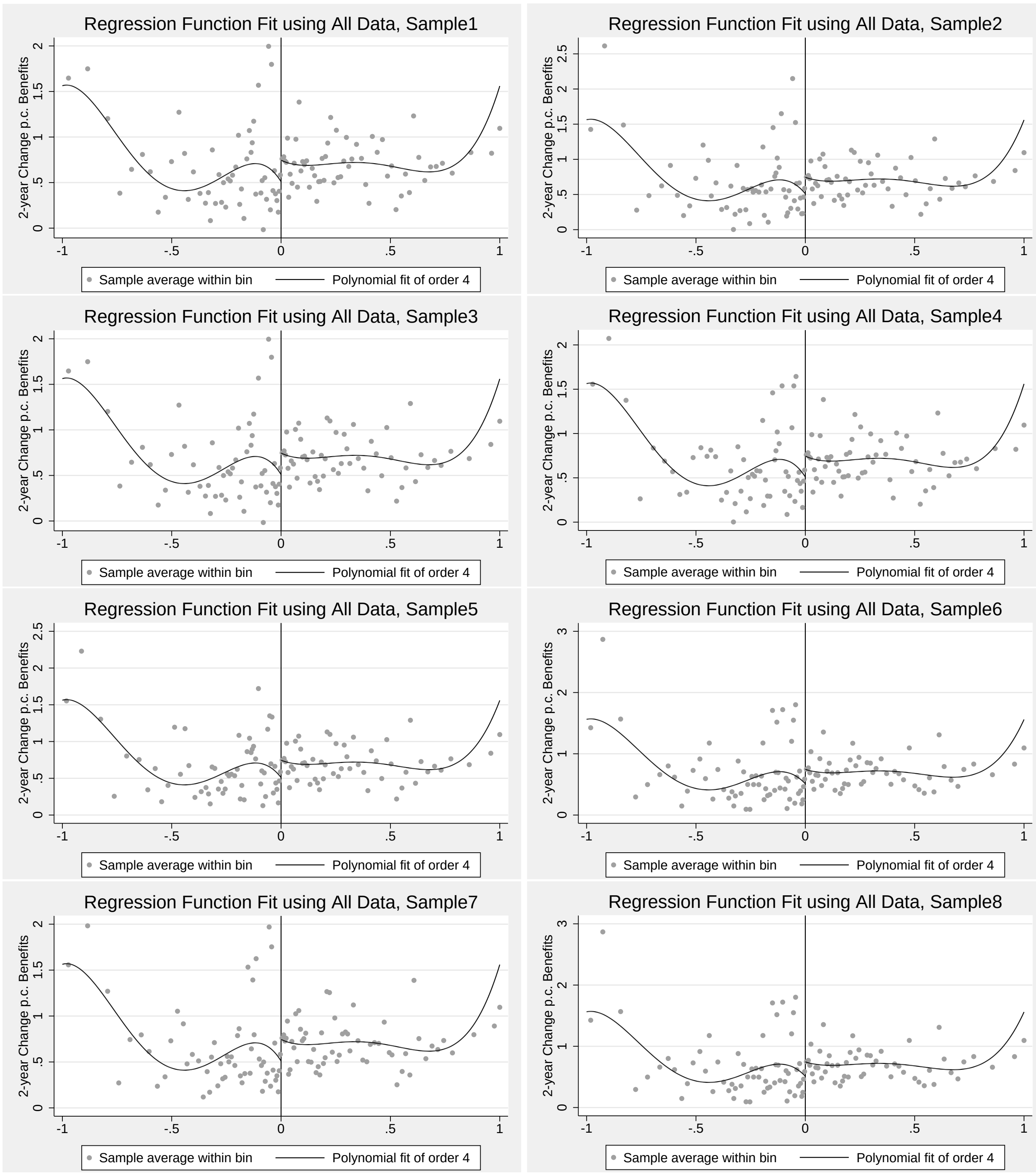
8. Omit 316 and 596 and 51+188

The author's favored slicing is the third one: this omits Democratic vs Democratic races, but retains races against Independents and assumes that a Republican winner's unknown runner-up was from the Democratic party. However, it will turn out that these different slicings do not vary much in the results they generate.

An appealing feature of any RDD is the transparency afforded by the fact that it can be illustrated graphically. For this purpose, the RD-plots in Figure 2 present *global* fits of the relationship between $\Delta\text{Benefits}_{it}$ and the Democratic-party vote-share, on either side of the winning discontinuity, as well as local sample means, represented by dots.[23] The purpose of Figure 2 is to describe the data and provide *suggestive* evidence for the existence of a statistical difference at the cutoff. This evidence is clearly there. However, while the higher-order polynomials used in Figure 2 provide a good description of the data overall, they typically give a poor approximation locally, i.e. around the 50% win-margin where the RD analysis estimates the effect of a Democratic-party mayor (Gelman and Imbens, 2018). Best practice for the RD analysis in equation (7) is therefore to

[23]The global polynomial is calculated using the original observations, not the binned observations.

Figure 2: RD plots for Eight Data-Slicings



Notes: This figure plots RD plots for the eight data-slicings outlined above. All plots are generated using the STATA-command `rdplot Y RV , binselect(qsmv) kernel(triangular)`. By default, each each uses quantile-spacing over all the data in a given slicing. The running variable on the horizontal axis is always the Democratic-party vote share.

use local linear or local quadratic approximations at either side of the threshold, coupled with a data-driven choice of bandwidth. I implement this using STATA's `rdrobust`-routine, which automatically chooses the bandwidth used for estimation to minimize a Mean Squared Error (MSE) criterion (Cattaneo et al., 2018, 4.2.4). This gives a different MSE-optimal bandwidth on each side of the cutoff (and thus different numbers of observations) for each of the eight data-slicing, and depending on whether a local-linear or local-quadratic approximation is chosen in estimating equation (7).

The eight columns in Table 8 correspond to the eight possible data-slicings discussed in the text. The author's favored slicings are the third and seventh ones, which omits races between two Democratic-party candidates, but assumes that a Republican winner's unknown runner-up was from the Democratic party. In the third slicing in column 3, races against Independents are retained. In column 7, they are dropped. Results are indistinguishable, suggesting that the Democratic-mayor-effect is at play in comparison to both Republicans and Independents. In Panel B, equation (7) is estimated with a local-quadratic instead of local-linear approximation. The estimated RD effects are marginally bigger and marginally less precisely estimated. For a visual complement to Table 8, Figure 3 shows the RD plot for the actual data used in the local-quadratic estimation of equation (7).

Overall, the magnitude of the effect in all RDD estimations always falls in between the estimates from the DiD estimation in levels (\$469 in column 1 of Table 2) and the growth-specification (\$159 in column 1 of Table 6). As in the prior analysis, the change in per capita contributions is always included as a control because we are interested in changes in benefits that are not explained by changes in contributions. In addition, because the outcome of interest $\Delta\text{Benefits}_{it}$ is defined in nominal terms, there may be some concern about the fact that I pool elections from the 1972–2015. Panels C and D therefore replicate Panels A and B with a year control added to the analysis. The RD estimate is largely unaffected by this. In particular, the estimates in columns 3 and 7 that were used as a comparison to the difference-in-difference results are practically unchanged from Panel B to D.

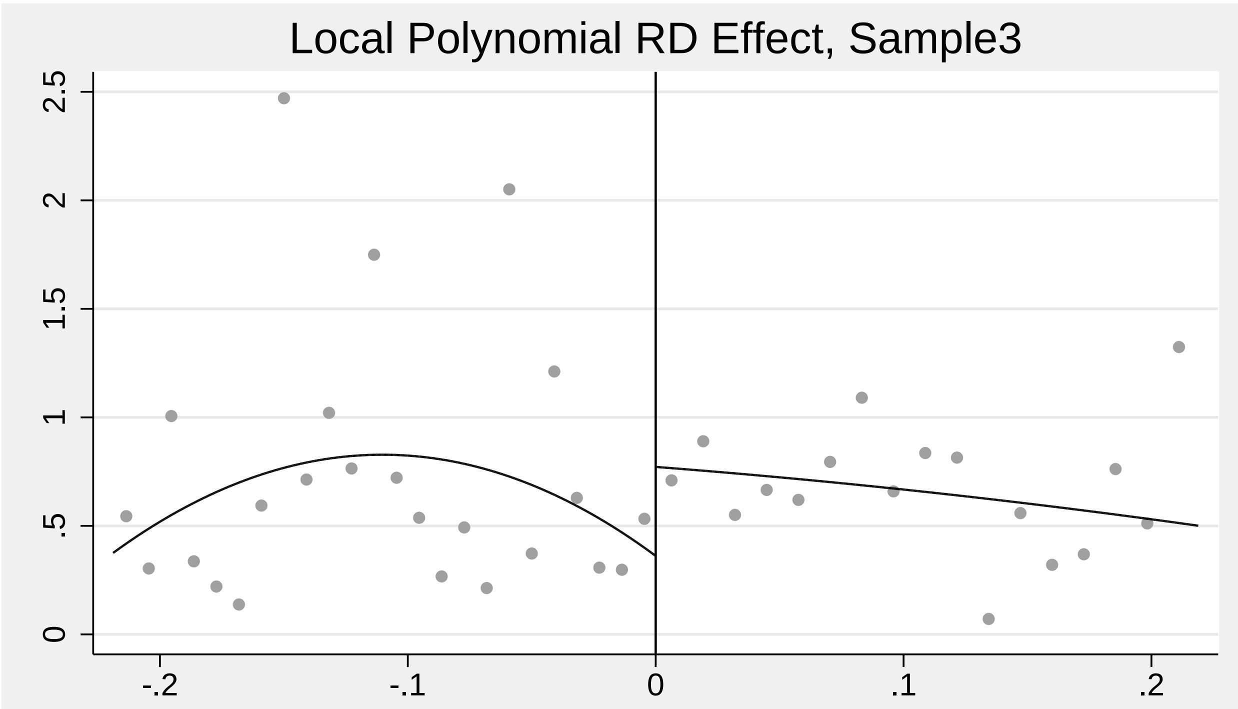
It is worth noting that the use of covariates in RD analysis needs to be approached with care. While added covariates can improve precision, they can also introduce serious biases into the analysis (Calonico, Cattaneo, Farrell, and Titiunik, 2018). A sufficient balance condition for the RD

Table 8: RD Effect: Democratic Mayor on Pensions Benefits

Outcome	Δ *p.c. benefits (in thsd $)*							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: local linear								
RD-coefficient (Dem-Mayor)	0.344	0.236	0.367	0.400	0.268	0.280	0.387	0.293
	[0.006]	[0.011]	[0.004]	[0.005]	[0.010]	[0.008]	[0.006]	[0.009]
effective N left of cutoff	277	223	280	262	233	212	267	229
effective N right of cutoff	1102	1066	652	1102	652	1106	805	826
Panel B: local quadratic								
RD-coefficient (Dem-Mayor)	0.415	0.302	0.477	0.443	0.375	0.299	0.488	0.360
	[0.010]	[0.015]	[0.005]	[0.012]	[0.007]	[0.022]	[0.007]	[0.013]
effective N left of cutoff	424	364	424	401	369	307	413	319
effective N right of cutoff	1049	1037	695	1106	726	1105	708	722
Panel C: A + year-ctrl								
RD-coefficient (Dem-Mayor)	0.364	0.256	0.307	0.416	0.210	0.302	0.404	0.293
	[0.004]	[0.009]	[0.015]	[0.003]	[0.046]	[0.004]	[0.004]	[0.010]
effective N left of cutoff	309	255	318	272	278	233	280	244
effective N right of cutoff	986	989	824	1073	830	1085	719	756
Panel D: B + year-ctrl								
RD-coefficient (Dem-Mayor)	0.397	0.271	0.448	0.502	0.341	0.322	0.531	0.368
	[0.015]	[0.036]	[0.009]	[0.004]	[0.019]	[0.012]	[0.003]	[0.010]
effective N left of cutoff	444	363	444	413	358	289	426	304
effective N right of cutoff	968	964	643	1195	652	1195	749	752
Panel E: A for narrow-plans								
RD-coefficient (Dem-Mayor)	0.345	0.311	0.344	0.367	0.273	0.349	0.332	0.279
	[0.024]	[0.046]	[0.034]	[0.037]	[0.106]	[0.058]	[0.078]	[0.153]
effective N left of cutoff	138	113	146	146	113	117	138	105
effective N right of cutoff	330	336	257	257	243	303	217	217
Panel F: B for narrow-plans								
RD-coefficient (Dem-Mayor)	0.426	0.425	0.453	0.373	0.428	0.340	0.362	0.328
	[0.021]	[0.026]	[0.018]	[0.074]	[0.036]	[0.122]	[0.107]	[0.162]
effective N left of cutoff	221	181	230	206	188	191	217	178
effective N right of cutoff	481	482	409	406	370	430	312	310

Notes: The table reports the results of estimating equation (7). Columns 1–8 correspond to the eight possible data-sub-slicings discussed in the text. The preferred slicings are columns 3 and 7, which omit Democratic vs Democratic races. Panels A, C, and E report on a local-linear approximation; Panels B, D, and F report on a local-quadratic approximation. *p-values* in brackets for RD-bias-corrected standard errors (Cattaneo et al., 2018, 4.3.2).

Figure 3: Estimating Equation (3) with Plan-Type-Specific Fixed Effects



Notes: The figure shows the RD plot for the actual data used in the local-quadratic estimation of equation (7), for the preferred third slicing of the data, i.e. column 3 of Panel B in Table 8.

Table 9: Controls Balanced Across Discontinuity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:				**Δ per capita contributions**				
RD-coefficient (Dem-Mayor)	-0.065	0.050	-0.059	-0.090	0.049	0.033	-0.083	0.028
	[0.551]	[0.020]	[0.589]	[0.485]	[0.019]	[0.154]	[0.518]	[0.350]
Outcome:				**year**				
RD-coefficient (Dem-Mayor)	-0.458	-0.090	-0.543	-1.061	-0.213	-1.121	-0.581	-0.654
	[0.672]	[0.944]	[0.633]	[0.336]	[0.871]	[0.374]	[0.607]	[0.601]

Notes: The table verifies that the two control variables added to the analysis are balanced across the discontinuity of the running variable: reported are the results of performing the RD analysis of equation (7) for the two controls added to the analysis, i.e. the change in per capita contributions that is always included, and a linear control for year that is added in Panels C and D. The number of observations is the same as in Table 8. *p-values* in brackets for RD-bias-corrected standard errors (Cattaneo et al., 2018, 4.3.2).

estimate to remain consistent is that the covariates do not display any jumps at the discontinuity (Cattaneo et al., 2018, 4.4.1). Table 9 shows that this is the case for both per capita contributions and the year control in all data-slicings except the second one in the case of per capita contributions. Importantly, in the preferred slicings in columns 3 and 7, the covariates appear well-balanced across the discontinuity.

Lastly, in Panels E and F, I replicate the analysis for only the 'narrow-based' plans associated with police and fire-fighters. As in the growth-specification in section 4.2 there is again no suggestion that the effect of a Democratic mayor is more pronounced for narrow-based plans. The point estimates in Panels E and F are almost unchanged relative to Panels A and B. This is in stark contrast to the results in Table 2, where the interaction between Democratic mayor and narrow-constituency plans came through very strongly. Another possibility is that the DiD in levels may be confounded by a city's electorate becoming simultaneously more likely to elect a Democratic mayor and to support higher pension benefits specifically for police and fire-fighters. Another possibility is that the differential effect is real, i.e. police and fire-fighter plans' excess benefits grow more under Democratic mayors, but that it is not driven by the changes around elections that identify the RDD.

5 Conclusion

It is well-established that the identity of the party in power appears not to matter for a wide range of fiscal outcomes in U.S. cities (Ferreira and Gyourko, 2009). Without contesting the results for the outcomes hitherto considered by others, this paper demonstrates that the party in power in fact does matter for the biggest fiscal challenge that U.S. cities face in the coming decades, namely unfunded pension obligations.

This is shown through three empirical setups: in a generalized difference-in-difference (DiD) analysis in levels that studies several hundred municipal pension plans from 1972–2015, in a similar analysis that studies the growth in pension benefits around elections, and in a regression discontinuity design (RDD) that compares only close mayoral races. The difference-in-difference analysis has the advantage that all the available data can be included and a wide array of different fixed effects can be used to control for different regional time-trends in the evolution of pension benefits. The regression discontinuity analysis uses a much more narrow set of data but has a stronger claim to identification overall. The growths-specification sits in between.

All three setups show a significant partisan effect of Democratic-party mayors on pension benefits. In the DiD setup, a Democratic-party mayor is estimated to increase average per capita annual benefits by 469$ a year more than can be explained by changes in plan-contributions. In the growth-setup, which focuses more narrowly on variation around elections, this effect is estimated to be around 159$. In the RDD setup, a Democratic-party mayor is estimated to increase average annual per capita benefits between \$250 and \$450, i.e. within the range suggested by the preceding two sets of results.

In the difference-in-difference analysis, this effect appears to be heavily concentrated in 'narrow-constituency' plans represented by police and fire-fighters' unions. In the growth-specification and the RDD analysis, however, this differential effect is not present. One possibility is that the differential effect exists but is not driven by variation around elections. Another possibility is that the difference-in-difference analysis may be confounded by a city's electorate becoming simultaneously more likely to elect a Democrat-party mayor and to support higher pension benefits specifically for police and fire-fighters.

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Online Appendix

to

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

Online Appendix A 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.* •