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... FOR THEIR UNFUNDED PENSIONS

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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, using a regression discontinuity design (RDD) focusing on narrow mayoral races. Previous evidence shows that parties do not matter for a range of fiscal outcomes in U.S. cities, and suggests this is because Tiebout sorting imposes fiscal discipline. This paper shows that parties do matter for types of fiscal spending where benefits accrue to narrow constituencies and where costs are difficult to observe and understand for tax payers.

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1 Introduction

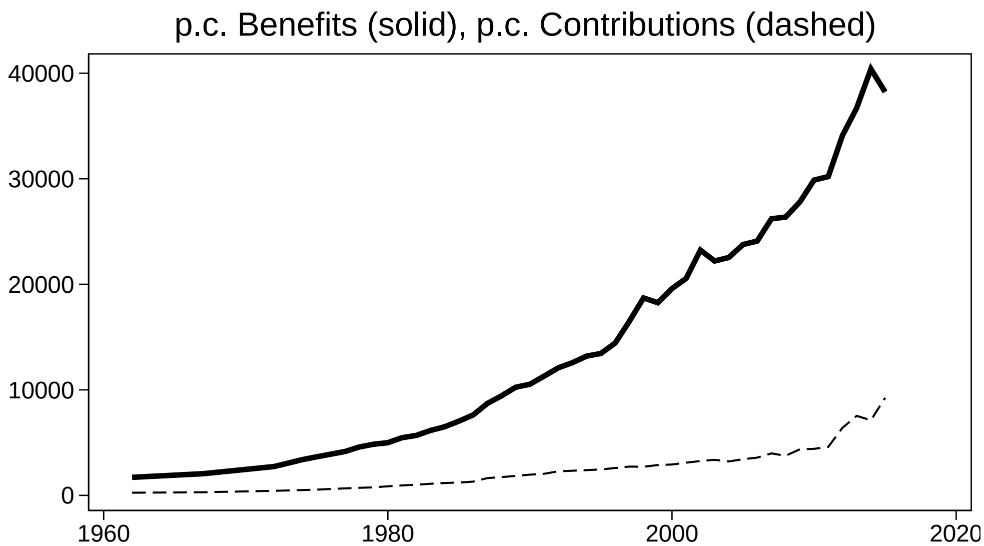
In their influential study, Ferreira and Gyourko (2009) show that the identity of the party in power does not appear to matter for any fiscal outcomes in U.S. cities. This stands in contrast to findings from other contexts where parties appear to be influential in shaping municipal/city level fiscal outcomes (Pettersson-Lidbom, 2008; Fiva, Folke, and Sørensen, 2018). The existing evidence suggests that in U.S. cities Tiebout-sorting, e.g. city residents' voting with their feet, disciplines politicians to keep budgets balanced. However, there is overwhelming empirical evidence in the political economy literature that fiscal discipline depends to a large extent on how salient or visible government action is to voters (Besley and Burgess, 2002; Adsera, Boix, and Payne, 2003; Ferraz and Finan, 2008). The logic of collective action further suggests that fiscal discipline is less likely when a narrowly defined constituency benefits from pork-barrel spending and the costs are broadly dispersed amongst taxpayers (Olson, 1965; Grossman and Helpman, 2001). The theory in Glaeser and Ponzetto (2014) combines these two insights in a model of public-sector pension funding, where the electorate consists of public-sector employees and other tax payers, and only the former understand the 'shrouded' benefits and costs of unfunded pension benefit increases.

Importantly, the types of fiscal spending considered by Ferreira and Gyourko (2009) (total revenue and taxation, expenditure shares, city employment and some crime statistics) are all highly visible budget items, and tend to benefit city residents broadly. This paper's contribution is to show that parties *do* matter in U.S. cities for types of fiscal spending that benefit narrow constituencies and whose benefits and costs are not easily observed or understood by all tax payers.

This paper's focus is on the biggest fiscal challenge that U.S. cities face in the coming decades, namely public-sector pension obligations, which are underfunded by several trillion dollars.[1] Pension under-funding is the result of benefit increases that are not accompanied by sufficient contribution increases. This is illustrated in Figure 1, which plots the evolution of the average of per capita benefits and contributions over the last decades. In theory, benefit increases lead actuarial accountants to re-calculate the contributions into the pension that are required to finance the higher benefits. In practice, there are a number of reasons benefit increases may not result in ap-

[1] 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.

Figure 1: per Capita Benefits and Contributions Over Time



Notes: Based on a panel of yearly averages of over 1,000 state and municipal plans from the U.S. Census' *Annual Survey of Public Pensions* (ASPP).

propriate contribution increases: Benefit formula changes that incentivize earlier retirement may not be reflected in actuarial calculations if the expected retirement age is not updated (Mitchell and Smith, 1994, 282). The transmission from benefit increases to required contributions can be neutralized if actuarially assumed rates of return on pension funds' assets are simultaneously increased (Mitchell and Smith 1994, footnote1, Kelley 2014, p24, Novy-Marx and Rauh 2011). As well, actuarial required contributions may adjust but employers may simply not pay them.[2] Unlike federal social security, municipal and state pension benefits are legally binding commitments (Burns, 2011; Trusts, 2013). Ultimately, it is therefore tomorrow's taxpayers that are on the hook when these are unfunded. Where the funding gap is sufficiently large, it can culminate in municipal bankruptcy.[3]

Pension under-funding is at its core a political economy problem. While it is clear that voters often fail to account for the Ricardian equivalence of public debt more generally, this failure is

[2] Required contributions are paid in part by employers and in part by employees. The employee portion is taken out of paychecks and cannot be shirked, but employers can shirk on their contributions (Brown and Dye, 2015).

[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).

more severe in the case of unfunded extensions of pension obligations because these are falsely treated as if they were budget neutral (Johnson, 1997; Munnell, Aubry, and Quinby, 2011; Mohan and Zhang, 2014).[4;5] This budget neutrality can make pension benefit increases an attractive substitute to wage increases in politicians' eyes, even if the municipal employer still needs to cover these benefit increases through taxes in the long run. This is illustrated by an interview with the former mayor of Houston, Lee P. Brown. During a tight re-election campaign that he narrowly won with 51.7 percent of votes, he was instrumental in a large increase in municipal employees' pension benefits. He later justified his decision to increase pension benefits by the fact that it was budget neutral, and that he did not "have the funds to give municipal employees the raises they deserved" (Boylan, 2016). One insider summarizes this problem as follows: "consistent low-balling of pension costs over the past two decades has made it easy for elected officials and union representatives to agree on very valuable benefits, for very much smaller current pay concessions."[6]

It is not clear that there should be a partisan tilt in this political economy problem. 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.[7] However, anecdotal evidence does suggest that Democratic party politicians at the municipal level tend to be closer to (and more dependent on the political support of) public-sector employees (Greenhut, 2009, p.137), and political-economy theories such as Glaeser, Ponzetto, and Shapiro (2005) and Glaeser and Ponzetto (2014) that emphasize the importance of 'bringing out the core', would thus give rise to the prediction of a partisan tilt.[8]

[4] This is no less true at the federal level, where conservative estimates by rating agencies put the net present value of unfunded social security and medicare obligations at at least three times the amount of federal debt held by the public (Scope Ratings, 2017). Yet these obligations receive far less coverage than the theater surrounding the annual raising of the debt ceiling (Ferguson, 2013).

[5] There is a long-established literature making the opposite claim, namely that Ricardian equivalence will hold at the local level because unfunded pensions can only be financed by future taxes, and this expectation is immediately capitalized into house prices (Daly, 1969). There is some empirical support for this claim (MacKay, 2014; Epple and Schipper, 1981). However, this evidence pertains to information about the state of a pension's under-funding, whereas decisions about benefit increases are usually made in closed session and hidden from public view and their link to pension under-funding is shrouded to most voters (Greenhut, 2009, 43).

[6] 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.

[7] It is perhaps worth emphasizing that not finding any partisan effect would not imply the absence of the political economy problem laid out here. Similarly, finding a partisan effect does not indicate that the political economy problem involves only one party.

[8] Many cities are "institutionally nonpartisan" in that they prohibit party labels from being printed on election ballots. However this de jure constraint appears to have little bearing on the actual de facto importance of parties in a

The primary source of data used in this paper is the U.S. Census' *Annual Survey of Public Pensions* (ASPP), which, after linking to its historical versions, covers the years 1962–2016. This paper's primary focus is on the evolution of a plan's *per capita benefit payments* relative to *per capita contributions*.[9] This paper's unit of observation is a pension plan in a year; the mapping from municipal pension plans to cities is many-to-one, because some cities have different plans for their major employee groups. Municipal plans can therefore be linked to city-level mayoral elections.[10]

This paper analyzes the evolution of per capita benefits relative to per per capita contributions in municipal pension plans as a function of the party of the city mayor, using an extension of the mayoral election data produced by Ferreira and Gyourko (2009). Many unobserved factors that can also influence pensions may determine whether city residents elect a Democratic or Republican party mayor. 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. Standard tests for bunching of the running variable do not come close to rejecting the smoothness of the Democratic Party vote share around the winning cutoff, which suggests the RDD identifying assumptions hold in the data (McCrary, 2008).

I first confirm that none of the fiscal outcomes in Ferreira and Gyourko (2009) not respond to the party of the mayor in the slightly different data and using the slightly different measures I use. This means the finding of no partisan effects on any of the highly visible major budget items in a city remains the benchmark for my analysis.

The core finding of the paper is that changes in the political party of the mayor have a sizeable effect on the per capita benefits of a city's pension plan. Having a Democratic Party mayor is associated with increases in annual per capita benefits of between 1,500$ and 3,000$ annually per person. The effect on per capita benefits is robust to a range of specifications, different polynomials and optimal RDD bandwidth selection.

Interestingly, visual RD plots suggest neither a strong relationship between the Democratic

given city. See (Ferreira and Gyourko, 2009, fn.7).

[9] 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.

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

Party vote share and pension benefits, nor an across-the-board shift in pension benefits when the Democratic party wins. Instead the results are driven by a spike in pension benefit increases right around the close election cutoff. This suggests that, rather than reflecting preferences by Democratic Party mayors broadly, commitments to pension benefit increases may primarily be political pork used to win close elections.

Pension benefits are typically adjusted as part of collective bargaining. Since collective bargaining arrangements are only adjusted infrequently, the core analysis studies changes in benefits four years out from the election, i.e. at the end of the narrowly won mayoral term. The effect indeed only shows up three years after the election, is economically and statistically strongest five years later, and then becomes less precise as at six years out. Since the data span elections from the 1960s to today, I also check the robustness of the patterns to deflating all nominal figures into constant dollar terms.

The data are then sliced in a number of ways to provide further evidence on the political-pork mechanism. First, the effect is concentrated in pension plans for police and fire-fighters relative to general city employee plans. This suggests that pork barrel politics is aimed at the most well-organized employee groups. Second, the effect is concentrated in cities without a council-manager system, i.e. those cities where the mayor actually has the most discretionary powers (Vlaicu and Whalley, 2016). This, when the narrow Democratic Party winner was the challenger than when they were the incumbent. This suggests that incumbents need to rely less on pork to win close elections.

This paper contributes to a literature on municipal/local fiscal politics. Its findings suggest that the Tiebout-sorting-induced fiscal discipline documented by Ferreira and Gyourko (2009) fails for types of fiscal spending that benefit narrow constituencies and whose costs are not easily observed or understood by all tax payers.[11] Municipal pension benefits do respond to the party of the mayor and their response is stronger when the pension represents better organized constituencies and when the mayor has more discretionary power.

This paper also contributes to a literature on the fundedness of public-sector pension plans.

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

Most observers agree that underfunded public-sector pensions are first and foremost a political economy problem (Mitchell and Smith, 1994; Greenhut, 2009; Burns, 2011; Trusts, 2013; Kelley, 2014; Gale and Krupkin, 2016; Anzia and Moe, 2016). However, rigorous studies on plans' fundedness have tended to focus on fund management performance or on accounting practices (Novy-Marx and Rauh, 2009, 2014a,b; Brown and Wilcox, 2009). To the best of my knowledge, this is the first paper that offers causally identified evidence on the political economy drivers of pension plans' under-funding.

This paper also speaks to a broader literature applying RDD designs to local elections (Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Vogl, 2014; Eggers, Fowler, Hainmueller, Hall, and Snyder Jr, 2015; Fiva et al., 2018). While studies of local elections outcomes in Europe and the U.S. have tended to focus on identifying the effect of the party in power, related research in developing countries has tended to focus on the effects of party turnover itself, with the primary outcome being patronage politics and public-office hiring (Akhtari, Moreira, and Trucco, 2017; Colonnelli, Prem, and Teseo, 2018). I test for, but find no difference between close Democratic party wins by challengers vs incumbents. This is consistent with a general view that patronage politics have disappeared from U.S. local politics since the professionalization of municipal bureaucracies starting in the 1930s (Grindle, 2012; Vlaicu and Whalley, 2016).[12]

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 on Pension Under-Funding

This paper's empirical focus is on changes in benefits relative to changes in contributions, since these are what can be readily observed in the ASPP Census data. This section explains how benefits and contributions relate to measures of under-funding, and discusses other determinants of pension fundedness.

The basic metrics of a public pension's funding gap is the difference between its assets and the actuarial calculations of its future benefit obligations *already committed* to its pensioners and

[12] A fascinating recent study by Ornaghi (2018) suggests it may have been prevalent well into the 1970s.

active members. This gap is referred to as a plan'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

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

In Defined Contribution (DC) plans, expression (1) equals zero by construction. However, almost all municipal pensions in the U.S. are *Defined Benefit* (DB) plans, and in DB plans, there are a number of drivers of a plan's UAAL as described by expression (1). Clearly, one such driver is fund management since $\text{Assets}_{i\tau}$, i.e. the plan's assets at time τ are determined by the returns that its past contributions have earned.[13] Another key driver is discounting: The *Actuarially Assumed Return* (AAR) is the assumed future return on assets earned by the plans, and at a higher projected return, future benefit obligations are discounted more steeply. The AAR is often legislated at the state-level, although individual plans can also set their own AAR. 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, Novy-Marx and Rauh 2011). 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.[14] In recent years, returns have often been considerably below this number, but it has been politically difficult to bring the AAL in line with actual earned returns (Wall Street Journal, 2016). 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

[13] Accounting rules can also play a role in determining a pension's assets: At different points in time and under different state-regulated accounting regimes, assets were listed as *Book Value Assets* (BVA), i.e. essentially their purchase price, *Market Value Assets* (MVA), i.e. adjusting in real time to stock and bond market movements, or *Actuarially Valued Assets* (AVA), which can thought of as a smoothed average of the MVA.

[14] A second and related 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).

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.

The key driver of the UAAL in the end are benefit increases, especially when these are not offset by increase in required or actual contributions. Benefit increases take a number of forms. The simplest is an across-the-board increase of benefits, e.g. 10% 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.

In principle, 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. However, there are a number of 'blind spots' in such actuarial calculations. For example, actuarial calculations often do not adequately incorporate changes in expected retirement ages of active plan members (Mitchell and Smith, 1994, 282).

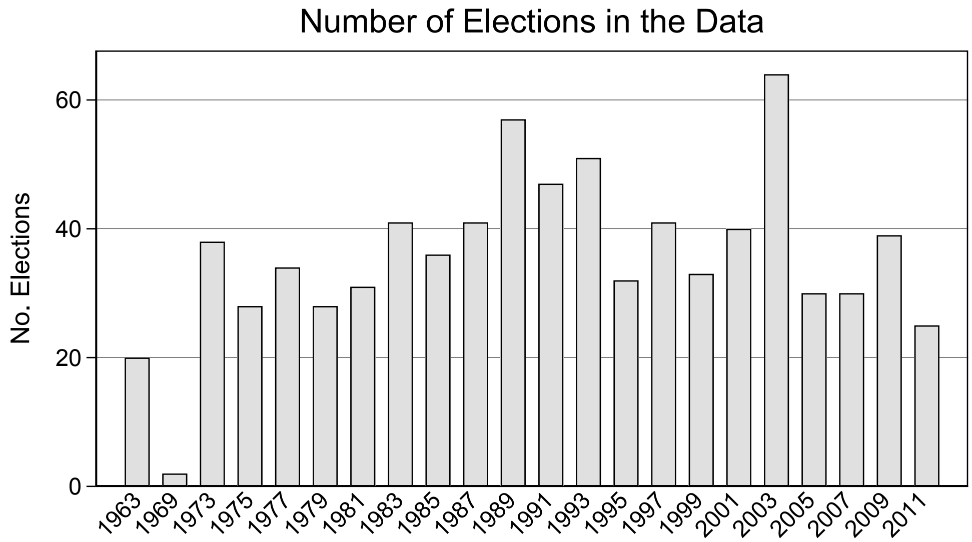
Last but not least, municipalities and states can chose to simply not pay their portion of actuarially required contributions. While the employee portion of the required contributions is taken out of paychecks and cannot be shirked, there is no real legal recourse against employers shirking on their contributions in the short and medium run, although they still owe them in the long run. Evidence suggests that a significant chunk of pension under-funding has to do with employers not paying their contributions (Brown and Dye, 2015; Munnell, Aubry, Cafarelli, et al., 2015).

This paper will be investigating changes in per capita benefits relative to changes in required employee contributions. Section 4.2 will provide further discussion of these.

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

Figure 2: Mayoral Elections linked to Municipal Pension Plans, Over Time



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

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 expression (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).

The ASPP covers municipal- as well as state-level plans. Naturally, this paper considers only the municipal plans, where each plan is 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.) This omits principally two types of plans: One is plans for state employees, which are naturally state-plans. A second is plans for municipal employees whose pensions are organized in state-wide plans. Principally, this is true for teachers, for whom the vast majority of pensions plans are

[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.

state-wide. By contrast, police, fire-fighters and other municipal employees tend to have pension plans that are organized at the municipal level. To a large degree, these patterns are explained by the historical patterns of union organization. Whether pension plans are organized at the municipal or state-level depends to a degree on the scope at which unions operate. When public-sector unions expanded in the 1960s, they mostly organized themselves out of pre-existing trade associations (Freeman, 1986; Reder, 1988). Trade associations for police and fire-fighters had traditionally 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.[16] 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.

In the time-series, the city-election data used in this paper extends the Ferreira and Gyourko (2009) data to cover 2005-2014. In the cross-section, the data for Southern cities collected by Vogl (2014) are added.

Only the subset of cities with municipal pension plans appears in the analysis undertaken here. To be included in the analysis, (*i*) a city must have any municipal pension plan, (*ii*) it must be included in the sample of cities in Ferreira and Gyourko (2009) or Vogl (2014), (*iii*) and its plan must be covered in the ASPP data.

Of the over 4,000 elections in the data, this is true for 1,200, covering 311 plans in 195 cities. For reference, Online Appendix A lists all cities in the data and their number of observations by decade. These elections are quite evenly spaced over the time-horizon covered by the ASPP data, as shown in Figure 3.

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

[16] 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.

4 Framework and Results

4.1 Identification Framework

To identify the effect of the party in power, this paper relies on a regression discontinuity design around close elections. Among non-experimental identification strategies, the RDD has 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, Moretti, and Butler, 2004; Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009; Dal Bó, Dal Bó, and Snyder, 2009; Ferraz and Finan, 2011; Eggers et al., 2015; Fiva et al., 2018; Akhtari et al., 2017; Colonnelli et al., 2018; Ornaghi, 2018).

The RD design entails 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}):

$$\Delta\text{Benefits}_{it} = \beta_D \text{D}_{jt} + f(\text{VSD}_{jt}) + \beta_X X_{it} + \epsilon_{it}. \tag{2}$$

Including a flexible function $f(\text{VSD}_{jt})$ of the running variable itself in equation (2) 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 $f(\text{VSD}_{jt})$.[17] However, Gelman and Imbens (2018) in particular have identified a number of problems arising from the use of higher-order polynomials. Namely, while they 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. 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

[17] 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."

in the following equation

$$\Delta\text{Benefits}_{it} = \beta_1 \text{VSD}_{jt} + \beta_D \text{D}_{jt} + \beta_2 \text{D}_{jt} \times \text{VSD}_{jt} + \beta_X X_{it} + \epsilon_{it}. \tag{3}$$

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.

4.2 Defining Variables

The main outcome considered in what follows is the 5-year change in per-capita benefits, $\Delta_{t+4}\text{Benefits}_{it}$ measured as the change from the year before an election to the end of the mayor's term four years after the election. This is the longest time horizon that still falls under the term of the mayor whose election is the treatment. The reason for this is that pension benefits are adjusted as part of union bargaining and union bargaining only takes place every couple of years.

It is worth emphasizing that the ASPP allows the researcher to measure only concurrent per capita benefit payments paid to pensioners, and not changes in future benefit claims of currently active members. On the flipside, changes in per capita contributions are measured as concurrent per capita contributions of active members. In evaluating changes in per capita benefits relative to per capita contributions, one is therefore by construction comparing receipts to pensioners with payments from active members.

This could introduce a bias against finding any results if unions would collectively bargain for future benefit increases reserved for currently active members. In practice, however, benefit expansions negotiated by unions do not seem to ever be reserved for active members. (Perhaps in part because retired union members pay the same dues.

A second consideration is that some of the benefit expansions discussed in section 2 do not lead to bigger pensions but to *earlier* retirement at equal pension pay. For example, while enhancing a pension formula from "2 at 50" to "3 at 50" will primarily increase pension benefits, enhancing a pension formula from "3 at 55" to "3 at 50" will leave pension benefits unchanged but incentivize earlier retirement. To the extent that the latter enhancement will have a bigger short-run impact (on those in the age window 50 to 55), this is another reason to focus on the longest time horizon that still falls under the term of the narrowly elected mayor.

Because benefit increases that are accompanied by appropriate increases in contributions do not affect the fundedness of pensions, changes in employee contributions are in principle an important control variable in what follows. As discussed in Section 2, employees always pay their full actuarially required contributions. In practice, they turn out not to respond much at the close election cutoff and their inclusion as a control variable turns out not to be important to the main results.

Table 1 reports averages for changes in outcomes from one year before an election to four years after. Since the elections in the data span more than fifty years, the second column additionally reports deflated values for variables that are defined in dollar-terms. The top row shows that over a five-year window around an election, per capita pension benefits go up on average by 3,642$ or 2,582 constant 2010 dollars. Per capita pension contributions go up on average by 825$ or 661 constant 2010 dollars. The eight main outcomes in Ferreira and Gyourko (2009) are measured in either log terms or percentage shares, and the same transformations are retained here for comparability. City-level revenues, taxes and expenditures all increase by about 20 percent in nominal terms or around 14 percent in real terms over the same time window, while the share of city employees to city residents barely moves. Unsurprisingly there is no systematic movement in the four outcomes at the bottom. These are expenditure shares for specific purposes and there is no reason for these to move when averaged over all elections.

4.3 Results

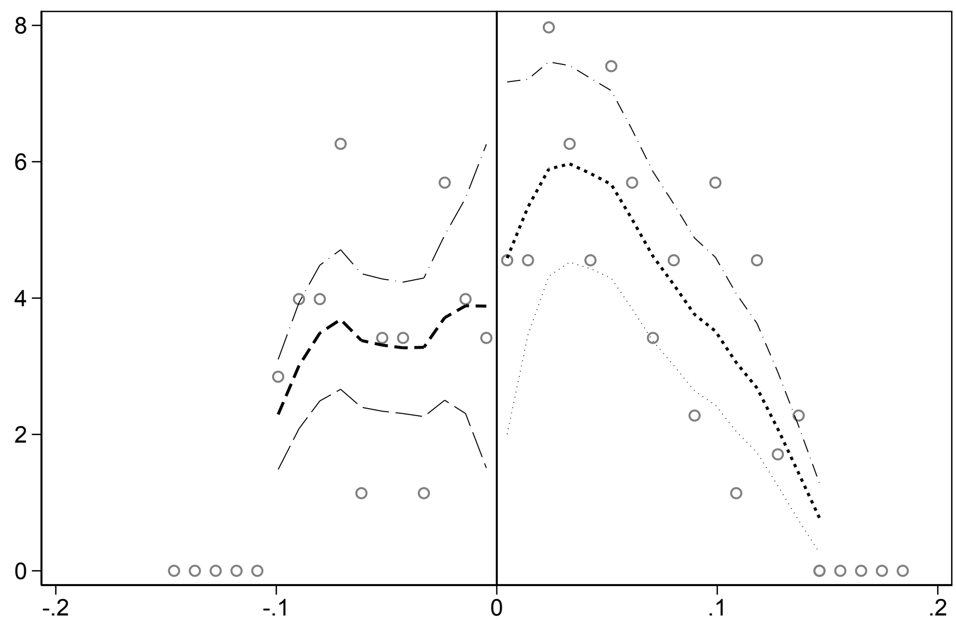
The identifying assumption of the RDD is that the electorate's preferences can be held constant in a narrow window around the same vote share, where the relevant vote share is obviously the one that narrowly elects one party or candidate over the closest rival. The logic of applying the RD design to close election hinges the outcome of a close election being quasi-random. A common test for the validity of the RDD approach in elections is to verify that there is no bunching at the cut-

Table 1: Descriptives on Outcomes

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

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

Figure 3: McCrary Test for Bunching of the Running Variable



Notes: This figure shows the McCrary Test for manipulation of the running variable VSD_{jt}. The estimated discontinuity (the 'log difference in height') is 0.0245, with a standard error of 0.5739, and a resulting t-statistics of 0.0427 not rejecting the hypothesis that the running variable has continuous support at the cutoff.

off, e.g. no disproportionate amount of close wins relative to close losses (McCrary, 2008). Early studies in this literature, e.g. Ferreira and Gyourko (2009), commonly did not test for this. 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.[18] Figure 3 shows that the McCrary (2008) test confirms there is no bunching of the running variable in the data.

If the identifying assumptions of the RD design hold, covariates should be to be balanced across the cutoff. Table 2 reports on the balancedness of city and election covariates, after trimming the sample to include only elections within a ten-percent window around the winning cutoff. Table 2 includes the election year, and per capita benefits and contributions in the year before the

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

Table 2: Covariate Balance

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

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

Table 3: Effect of Democratic Mayor on Fiscal Outcomes in Ferreira and Gyourko (2009)

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

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

election. The table also reports on one-year lags of the eight main fiscal outcomes in Ferreira and Gyourko (2009, TableII). Since these are all defined in per capita terms, the table also separately reports on the underlying totals. Of the fourteen covariates, only the percentage of revenues spent on salaries displays a marginally significant difference.

Table 3 reports results when the RD design is estimated for the main fiscal outcomes in Ferreira and Gyourko (2009, TableII). Columns 1 and 2 include no controls other than a linear or quadratic of the running variable. None of these highly visible budget items respond to whether there is a Democratic Party or Republican Party mayor in power. As a point of comparison, columns

Table 4: Baseline Results: Effect of Democratic Mayor on Pensions Benefits

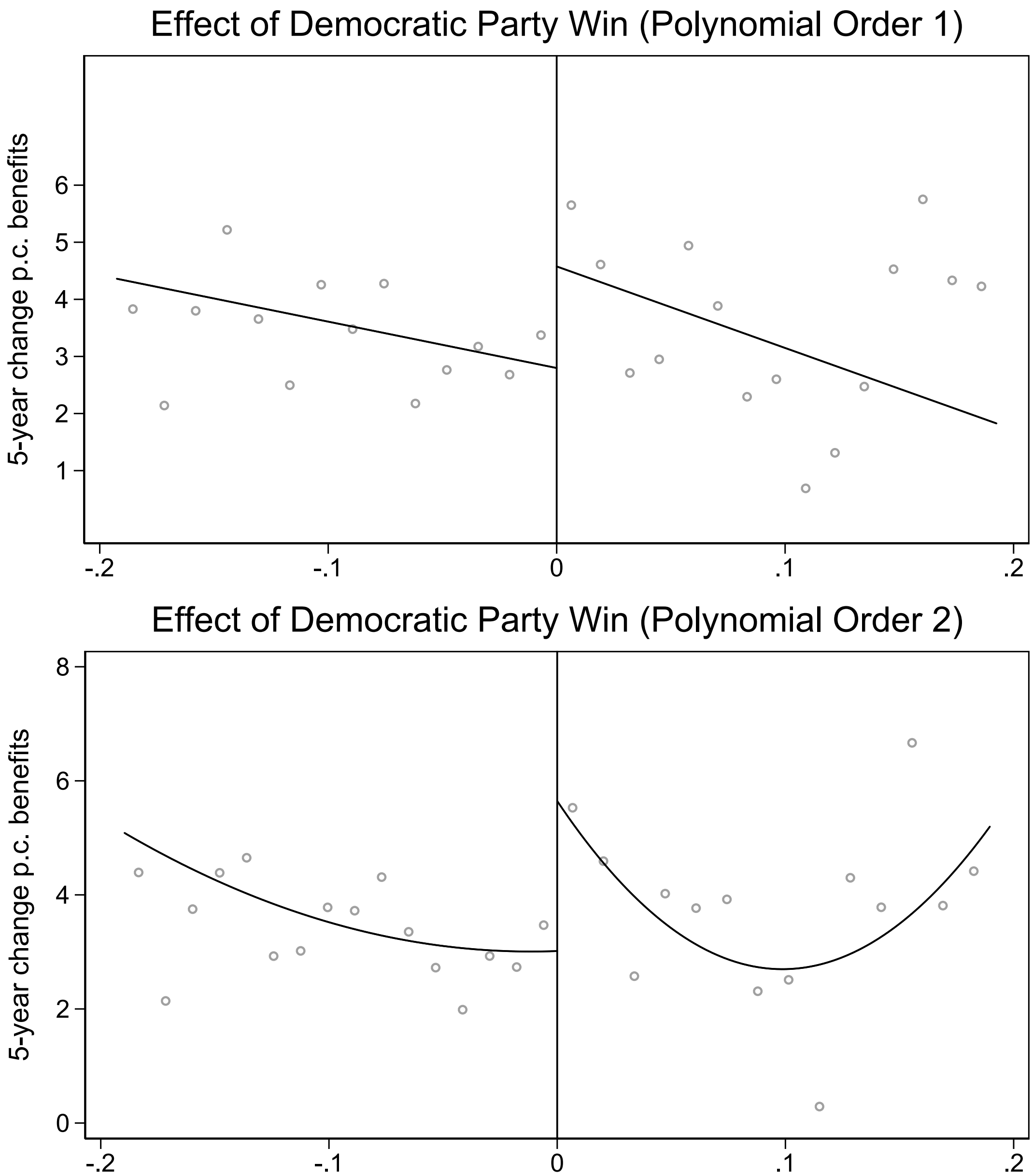
	(1)	(2)	(3)	(4)
	RDD		OLS	
	linear	quadratic	linear	quadratic
Pension Outcomes				
Δ_{+5} per capita pension benefits $_{t-1}$	1.667**	2.994***	2.125***	2.429***
	[0.017]	[0.000]	[0.008]	[0.009]
Δ_{+5} per capita pension benefits $_{t-1}$	0.100	-0.524	-0.168	-0.574
	[0.930]	[0.762]	[0.901]	[0.814]

Notes: *(a)* This table reports on the effect of having a Democratic Party mayor on municipal pension outcomes. *(b)* Columns 1–2 report on the RD results, which will be the empirical focus from here on, with no controls and only a linear or quadratic approximation of $f(\text{VSD}_{jt})$ included. As a point of comparison, this table also reports results from an OLS estimation in columns 3–4. *(c)* In columns 1–2, the choice of bandwidth is automated based on MSE-minimization (Cattaneo et al., 2018, 4.2.4), separately for each estimation. In columns 3–4, the bandwidth was set so that the number of observations equals the average number of observations in columns 1–2 (N=371). *(d) p-values* are reported in brackets for standard errors clustered at the city-level.

3–4 also report on the equivalent OLS estimations, again with no controls other than a linear of quadratic of the running variable. The OLS point estimates are somewhat different from the RD point estimates but the key observation is the consistent lack of any statistical relation between the mayor's party and any of the variables under any of the specifications. This sets an important benchmark because it confirms that the core results in Ferreira and Gyourko (2009) hold up in the slightly different sample and transformation of the data studied here.

Table 4 reports on the core estimation of the paper, i.e. expression (3) estimated with per capita pension benefits as the outcome. Table 4 follows the same structure as Table 3. Columns 1 and 2 include no controls other than a linear or quadratic of the running variable. As a robustness check, columns 3–4 report on the equivalent OLS estimation. The core result is that a city's pension plan's benefits increase by between 1,667$ and 2,994$ in the five-years after a narrow Democratic Party mayoral win. As discussed in Section 4.2, per capita benefit expansions are less valuable to recipients when accompanied by per capita contribution increases. It is therefore important to investigate how contributions respond, and if they do respond, to adequately control for them in order to obtain a measure of unfunded benefit increases. However, the results in Table 4 show that contributions, perhaps surprisingly, do not significantly go up with close mayoral elections.

An appealing feature of any RDD is the transparency afforded by the fact that it is so easily



Notes: This two-panel figure reports on the baseline estimation of equation (3), with no controls added and the bandwidth chosen through MSE-minimization for each specification separately. The top panel shows the RD plot where $f(\text{VSD}_{jt})$ is linear, on the bottom panel it is a quadratic function. The two panels are the visual representations of the estimation results reported in columns 1 and 2 of Table 4. Both panels use the same underlying data but different optimal bandwidth are chosen under the two polynomials (Cattaneo et al., 2018, 4.2.4), and the visual data bins therefore aggregate over somewhat different data points in the two panels.

graphically illustrated. Figure 4 visually displays the baseline estimation results for equation (3), with either linear or quadratic function of the running variable and no other controls added. Thus, the two panels are the visual representations of the estimation results reported in columns 1 and 2 of Table 4. The discontinuity is clearly visible in both panels. Interestingly, there is little evidence for an overall relationship between the electorates' vote share for the Democratic party and the evolution of pension benefits. The discontinuity appears to be drive by a jump up in benefits only in the range of narrow Democratic party victories. If one takes out the narrow range around the close-election cutoff, then the relationship appears flat. This suggests that, rather than reflecting overall Democratic-party preference, pension pension benefit increases may be political pork used to bring out the base in close elections, as in Glaeser et al. (2005); Glaeser and Ponzetto (2014). If this is the case, then the RD design identifies the causal effect of a *close* Democratic victory. Benefit increases are more valuable to their recipients when they are not fully offset by increases in required contributions. Changes in employee contributions are therefore in principle an important control variable in studying effects on benefits. In practice, employee contributions turn out not to respond at the close election cutoff, as Figure **??** shows.

As discussed in Section 4.2, changes in benefits are studied four years out from the election, i.e. at the end of the narrowly won mayoral term, because collective bargaining arrangements are only adjusted infrequently. Table 5 investigates what the results look like for different time horizons. In columns 3–4, Table 5 also adds the following control variables from Table 2: the year of the election, the log of city population, the log of total revenue, the log of total city employees, and per capita benefits, all measured in the year before the election. Since the data span elections from the 1960s to today, I also check the robustness of the patterns to deflating all nominal figures into constant dollar terms in the bottom half of Table 5.

The pattern that emerges is that the effect on pension benefits indeed only begins to show up three years after the election (i.e. Δ_{+4}), and only when expressed in constant dollar terms. The effect is strongest five years after the election (i.e. Δ_{+6}), but then loses precision in the year after, i.e. mid-way through the subsequent mayoral term.

If the core results are driven by political pork that is used to bring out the base, then three hypotheses suggest themselves: First, the effect should be stronger where this base is better organized. Second, the effect should be stronger when the mayor has more discretionary powers

Table 5: Robustness: Time-horizon and Deflating

	(1) linear	(2) quadratic	(3) linear	(4) quadratic
Δ_{+3} per capita pension benefits $_{t-1}$	0.077	0.171	0.436	0.510
	[0.930]	[0.880]	[0.428]	[0.421]
Δ_{+4} per capita pension benefits $_{t-1}$	-0.185	1.021	0.642	1.471
	[0.846]	[0.438]	[0.451]	[0.178]
Δ_{+5} per capita pension benefits $_{t-1}$	1.667**	2.994***	1.993***	2.106***
	[0.017]	[0.000]	[0.002]	[0.008]
Δ_{+6} per capita pension benefits $_{t-1}$	3.006***	3.833***	2.122**	2.727**
	[0.008]	[0.004]	[0.043]	[0.016]
Δ_{+7} per capita pension benefits $_{t-1}$	1.246	1.859	1.264	1.352
	[0.271]	[0.157]	[0.235]	[0.259]
Deflated				
Δ_{+3} per capita pension benefits $_{t-1}$	-0.300	0.022	0.381	0.443
	[0.695]	[0.984]	[0.333]	[0.338]
Δ_{+4} per capita pension benefits $_{t-1}$	0.195	1.184	0.766	1.592**
	[0.797]	[0.242]	[0.165]	[0.033]
Δ_{+5} per capita pension benefits $_{t-1}$	0.867	2.254***	1.236***	1.360**
	[0.124]	[0.002]	[0.007]	[0.012]
Δ_{+6} per capita pension benefits $_{t-1}$	2.418***	3.361***	1.686***	2.053***
	[0.005]	[0.001]	[0.007]	[0.003]
Δ_{+7} per capita pension benefits $_{t-1}$	1.111	1.949*	1.031	1.039
	[0.208]	[0.064]	[0.102]	[0.152]
Polynomial	linear	quadratic	linear	quadratic
Controls			✓	✓

Notes: *(a)* The baseline results study pension benefits four years after the election (i.e. Δ_{+5} denotes five years after the baseline year before the election) This table investigates different time horizons, from three to six years after the election. The bottom panel transforms the outcomes into constant 2010 dollars. slices the baseline results into sub-samples in three different ways: by plan type, by city type, and by mayor type. *(b)* Columns 1–2 report on the RD results, which will be the empirical focus from here on, with no controls and only a linear or quadratic approximation of $f(\text{VSD}_{jt})$ included. Columns 3–4 add as control variables the year of the election, the log of city population, the log of total revenue, the log of total city employees, and per capita benefits, all measured in the year before the election. *(c)* *p-values* are reported in brackets for standard errors clustered at the city-level.

to make campaign commitments Third, the effect may be differ between incumbent mayors and challengers. It is not clear in which direction this effect will go. On the one hand, incumbents may have a broader range of political tools and may have less need to use pension benefit increases to brig out the base. On the other hand, incumbents may be better able to credibly commit to push through pension benefit increases after their are re-elected since they are more familiar with collective bargaining processes and with the parties involved.

Fortunately, the data allow me to test all of these hypotheses. First, pension plans can be divided into police and fire-fighter plans and other municipal plans.[19] This makes intuitive sense because police and fire-fighters are clearly the most well-organized employee group at the city level. Second, cities can be divided into those run by a powerful mayor and those organized under the council-manager system.[20] Third, elections can be divided into those won by incumbents and those won by challengers. Fortuitously, all three of these slicings results in relative similar-sized sub-samples, with the most uneven breakdown that into mayoral cities (N=768) and council-manager cities (N= 390).

Table 6 reports the results for these sub-slicings. The table shows that the effect is indeed concentrated in pension plans for police and fire-fighters relative to general city employee plans, suggesting that pork barrel politics is concentrated where the beneficiaries are a more narrowly defined and better organized interest group. The table further shows that the effect is concentrated in cities without a council-manager system, i.e. those cities where the mayor actually has the most discretionary powers (Vlaicu and Whalley, 2016). lastly, the table shows that the effect is more pronounced when the narrow Democratic Party winner was the challenger than when they were the incumbent. This suggests that incumbents need to rely less on pork to win close elections. Figures 5–7 represent each of these three breakdowns visually. Each figure reports on the specifications with a quadratic polynomial, for nominal and constant-dollar values, i.e. columns 2 and 4 of Table 6.

[19] It would be attractive to study police and fire-fighter plans separately but this is not practical because roughly half of these plans are actually shared between the two groups. Teacher plans would be worthy of separate investigation but, as discussed in Section 3, almost all teacher plans are state-wide plans.

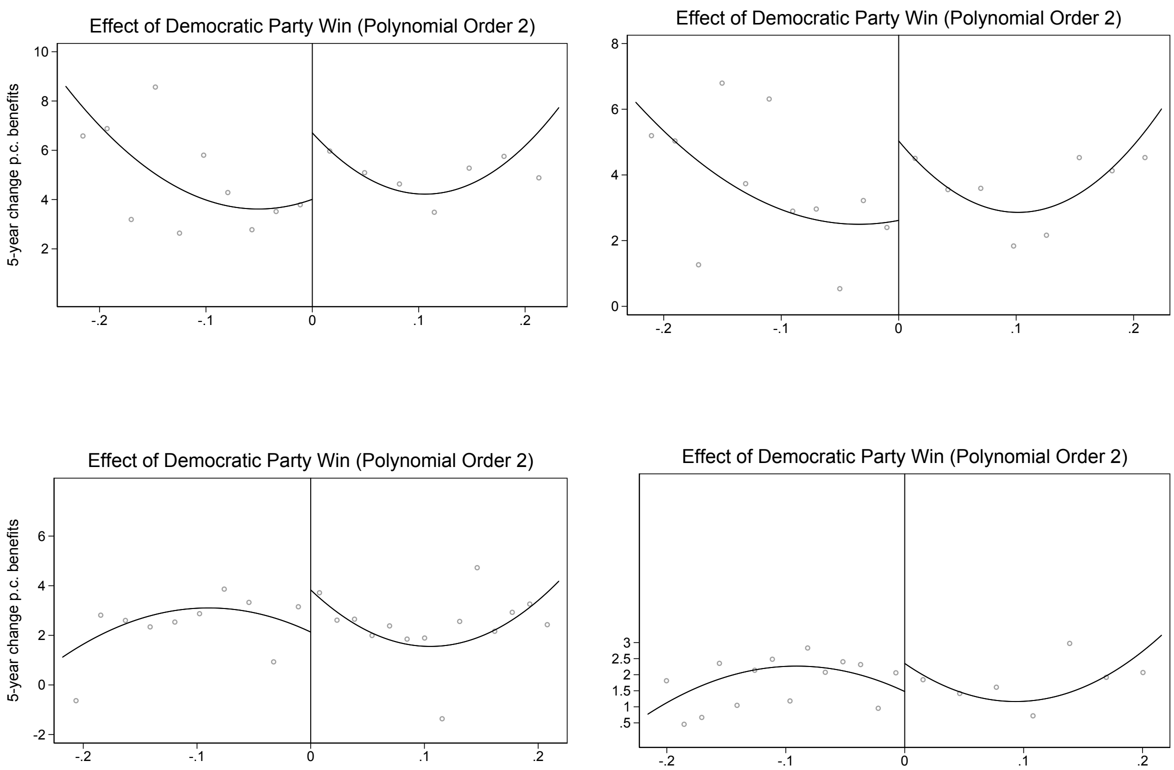
[20] The data on city-management is reported in the *International City Managers Association* (ICMA) *Municipal Year Book*, downloadable from `https://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/4421`. Note that cities under the council-manager system still have elected mayors, who are just less powerful.

Table 6: Suggestive Evidence on Mechanisms

	(1) linear	(2) quadratic	(3) linear	(4) quadratic
baseline (N=1,195)	1.667**	2.994***	0.867	2.254***
	[0.017]	[0.000]	[0.124]	[0.002]
By Plan Type				
police and fire-fighters (N=567)	2.077*	2.771*	1.709*	2.663**
	[0.054]	[0.052]	[0.059]	[0.024]
other municipal plans (N= 628)	-0.370	1.670	-0.426	0.870
	[0.622]	[0.106]	[0.466]	[0.251]
By City Type				
mayor system (N=768)	2.437***	2.356**	0.912	1.870**
	[0.004]	[0.022]	[0.156]	[0.048]
council manager system (N= 427)	1.235	1.536	0.849	1.048
	[0.353]	[0.356]	[0.400]	[0.383]
By Mayor Type				
incumbent (N=658)	1.033	2.614	0.851	1.419
	[0.350]	[0.108]	[0.351]	[0.243]
challenger (N= 537)	2.342***	3.036***	1.720**	2.541***
	[0.006]	[0.003]	[0.031]	[0.009]
Polynomial	linear	quadratic	linear	quadratic
Deflated:			✓	✓

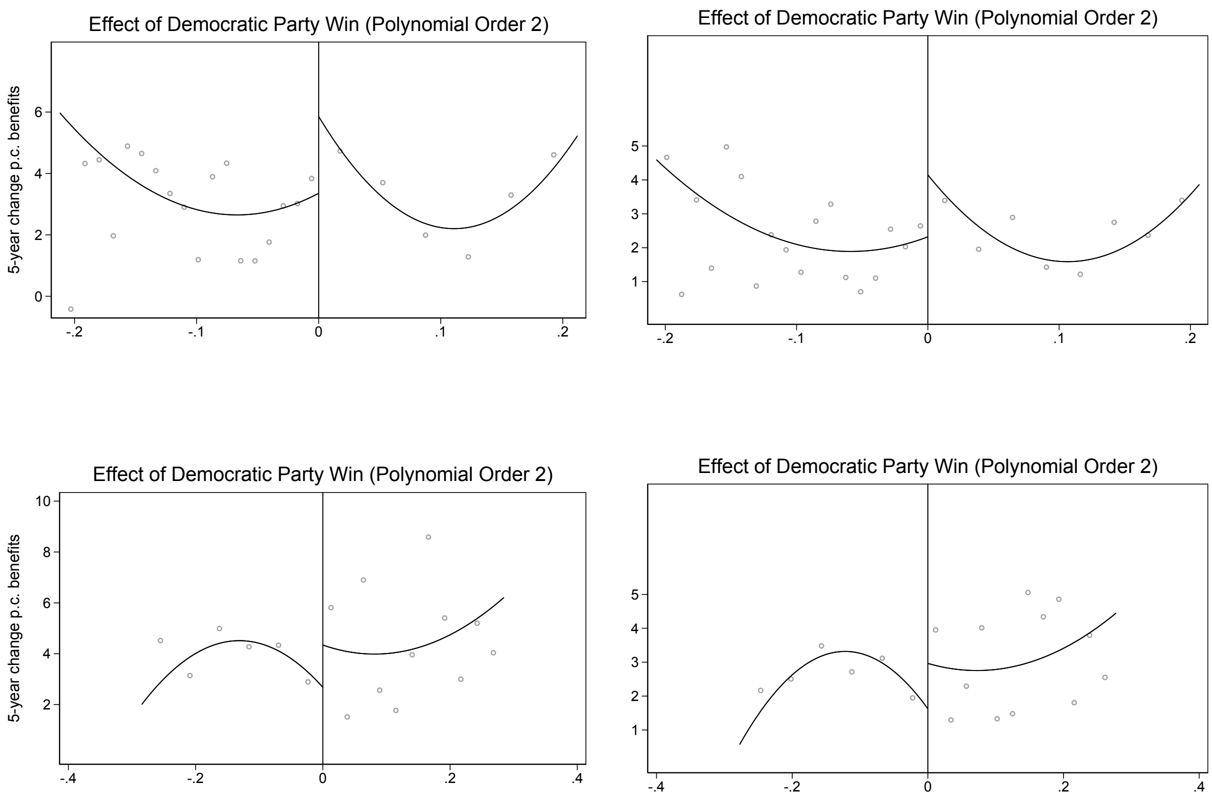
Notes: *(a)* This table slices the baseline results into sub-samples in three different ways: by plan type, by city type, and by mayor type. *(b)* Columns 1–2 report on data in nominal terms, with no added controls and only a linear or quadratic approximation of $f(\text{VSD}_{jt})$ included. Columns 1–2 report on data in constant 2010 dollars. *(c) p-values* are reported in brackets for standard errors clustered at the city-level.

Figure 5: Effect by Plan Type



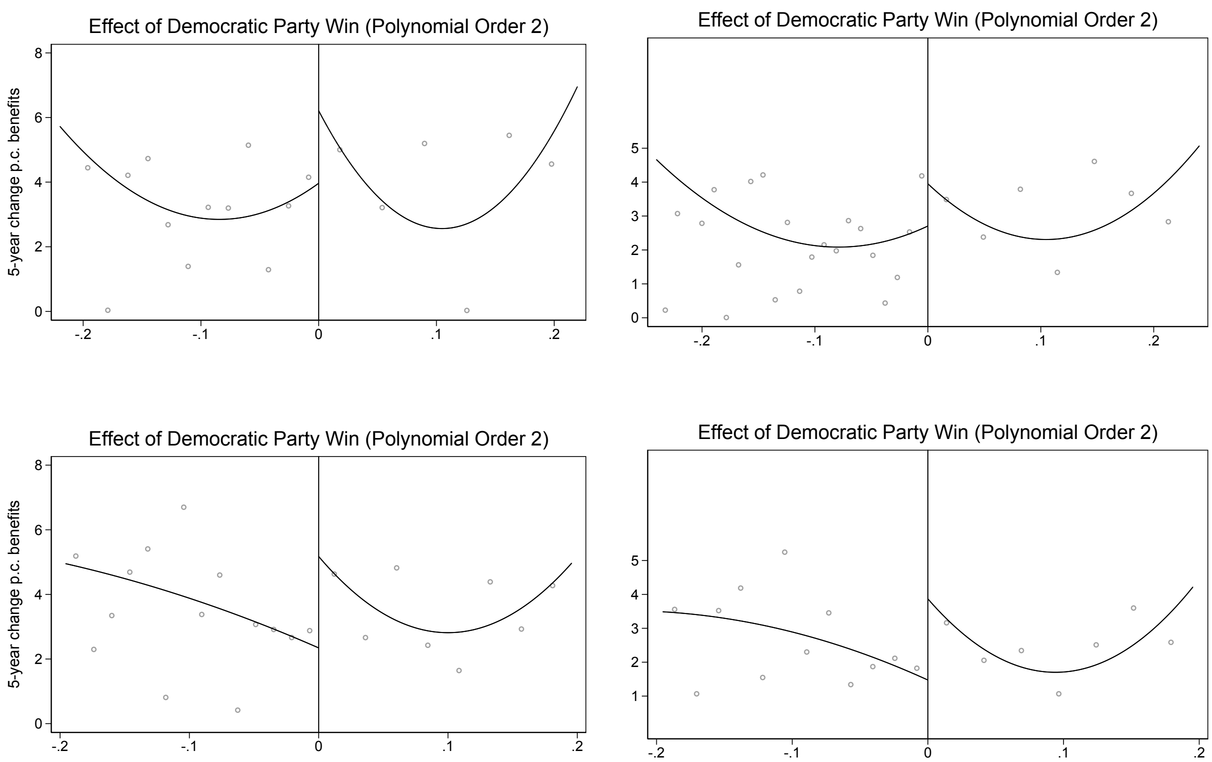
Notes: This figure displays the RD plot when the estimation of equation (3) is performed separately by two broad types of pension plans, with police and fire-fighter plans being the best-organized public-sector employees. The top panel reports on pension plans for police and fire-fighters (N=567). The bottom panel reports on other municipal pension plans (N= 628). In both panels, the right figure uses deflated data.

Figure 6: Effect by City Type



Notes: This figure displays the RD plot when the estimation of equation (3) is performed separately by two broad types of cities. The top panel reports on close mayoral elections in cities without a council-manager system (N=768). The bottom reports on close mayoral elections in cities on the council manager system (N= 390). In both panels, the right figure uses deflated data.

Figure 7: Effect by Mayor Type



Notes: This figure displays the RD plot when the estimation of equation (3) is performed separately by whether the winner of the close election was the incumbent or not. The top panel reports on close mayoral elections where the winner was the incumbent (N=658). The bottom panel reports on mayoral elections where the winner was the challenger (N= 537). In both panels, the right figure uses deflated data.

5 Conclusion

In their influential study, Ferreira and Gyourko (2009) show that the identity of the party in power does not appear to matter for any fiscal outcomes in U.S. cities. This stands in contrast to findings from other contexts where parties appear to be influential in shaping municipal/city level fiscal outcomes (Pettersson-Lidbom, 2008; Fiva et al., 2018). The existing evidence suggests that in U.S. cities Tiebout-sorting, e.g. city residents' voting with their feet, disciplines politicians to keep budgets balanced.

However, the types of fiscal spending considered by Ferreira and Gyourko (2009) are all highly visible budget items, and tend to benefit city residents broadly. This paper shows that parties *do* matter in U.S. cities for types of fiscal spending that benefit narrow constituencies and whose benefits and costs are not easily observed or understood by all tax payers.

This paper's focus is on the biggest fiscal challenge that U.S. cities face in the coming decades, namely public-sector pension obligations. Using a regression discontinuity design around close elections, the paper's core finding is that per capita pension benefits increase by about 1,500$ and 3,000$ annually per person after a narrow Democratic Party mayoral win.

Rather than reflecting overall Democratic Party preferences, the results appear to be primarily the result of political pork used to win close elections. This is suggested by the fact that pensions only respond right around the close election cutoff, as well as by the fact that pensions increase primarily for the most well-organized employee groups.

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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.* •