# Rules versus Home Rule

# Local Government Responses to Negative Revenue Shocks*

Daniel Shoag[†], Cody Tuttle[‡], and Stan Veuger[§]

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**Abstract**

Local governments rely heavily on sales tax revenue. We use national bankruptcies of big-box retail chains to study sudden plausibly exogenous decreases in this type of revenue. Treated localities respond by reducing spending on law enforcement and administrative services. We further study how cities with different degrees of autonomy vary in their response. Cities in home rule states, who have greater autonomy, react more swiftly by raising other types of revenue. A regression discontinuity analysis of cities in Illinois, where home rule status is triggered by crossing a population threshold, shows that this effect of local autonomy is causal: home rule leads to smaller revenue drops and stronger bond ratings.

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**JEL:** E6, H1, H7, K2, R5

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[†]Case Western Reserve University and Harvard Kennedy School

[‡]University of Maryland, College Park

[§]American Enterprise Institute for Public Policy Research, IE School of Global and Public Affairs, and Tilburg University

Local governments play an essential role in the provision of local public goods and deliver a wide range of government services. They are largely responsible for police protection and K-12 education, and they perform administrative functions such as providing building permits, issuing marriage licenses, and facilitating vehicle transfers. A failure to play these roles effectively can have dramatic consequences: a recent high-profile example is the water crisis in Flint, Michigan, where between 6,000 and 12,000 children were exposed to drinking water with high lead levels. This crisis was triggered by Flint's persistent financial dire straits, highlighting the challenges faced by local governments when dealing with negative revenue shocks. Despite the salience and importance of the ways in which local officials respond to such shocks, economists have paid scant attention to them. This paper changes that, by studying both how cities respond to sudden negative shocks to revenue, and how the broader institutional framework shapes that response.

These questions have taken on particular importance due to the increasing prevalence of e-commerce, which grew its share of all retail sales from 0.9 to 6.4 percent between 2000 and 2014 and continues to grow rapidly (Hortaçsu and Sylverson, 2015). This rise erodes the tax base of large numbers of cities in the U.S., over half of which rely on local sales tax revenue (National League of Cities, 2014). Bruce et al. (2015) estimate that the loss in sales tax revenue due to the rise of e-commerce amounted to close to 4 percent of total sales tax revenue by 2012, and will continue to increase rapidly. The importance of this development to local government finances may be reduced somewhat by the recent Supreme Court decision in *Wayfair versus South Dakota*, but local sales taxes are a volatile source of revenue at business cycle frequencies as well. Even property taxes, often thought to be a stable source of revenue for local governments, are susceptible to sizable shocks, as evidenced by the recent housing boom and bust. In addition, the restrictions introduced by the Tax Cuts and Jobs Act on the deductability of state and local taxes will place downward pressure on revenue raised directly from individual residents. In order to effectively design local government policy in this context, it is crucial to understand how governments respond to negative revenue shocks, especially those that are likely to be permanent.

In this paper, we use national big-box chain bankruptcies as natural experiments that allow us to analyze this response at the city level. We show that these bankruptcies provide a plausibly exogenous and discrete shock to local revenue, and we use that shock to study how expenditures respond. As individual big-box retailers typically account for roughly $20,000,000 in sales per year, a city losing one of these stores suffers a non-trivial hit to sales tax revenue from the chain store in question alone. In addition, Shoag and Veuger (2018) show that after a big-box store shuts down, many other nearby businesses end up closing up shop as well, exacerbating the consequences for local government finances. We compare cities that were home to the now defunct stores to cities where competitor retailers continued to operate to identify the causal impact of negative shocks to revenue on city budgets and behavior. We find that local governments that are hit by a big-box bankruptcy see their sales tax revenue decline by almost a fifth. In response, they decrease spending both on police protection and on administrative services.

We then proceed to investigate how the city's response depends on its level of autonomy and discretion, exploiting the fact that the degree of control that cities have over local policy varies both at the state level and within states. We study to what extent the size of the revenue drop varies with whether the city has "home rule" authority, an authority granted by state governments that allows some cities to implement certain policy changes without prior state approval. We show that cities that are more constrained experience a sharper drop in revenue and slower rebounds in revenue than cities with more discretionary authority. These more constrained cities do not even benefit in the long run from the restrictions on profligacy imposed by the state. Exploiting a feature of Illinois state law that automatically assigns home rule status to towns that surpass a population level of 25,000, we use a regression discontinuity design to show that these effects are, at least in part, causal: cities just above the cut-off enjoy better bond ratings and less revenue volatility than their counterparts just below the cutoff.

The papers proceeds as follows. In section 1, we review the prior research on local government responses to negative shocks and how they are influenced by the level of autonomy enjoyed by local governments, and preview how we contribute to this existing body of research. In section 2, we assess the size and persistence of the revenue shocks produced by big-box store bankruptcies, while the focus of section 3 is how cities respond to these shocks. How those responses vary with home rule status is discussed in section 4, while we establish the causal link from home rule to differences in outcomes in section 5. We conclude in section 6.

## 1 Negative Shocks to Local Government Finances

Inspired by Tiebout's (1956) seminal article, much research on local public finance has focused on the provision of different bundles of local public goods. These local public goods needs to be paid for, and the typical starting point for analyses of their financing is that of Bradford and Oates (1971), that government revenue and private income are fungible. If the local government is hit by an unexpected shock to its revenue, in our case a negative one, voters will reoptimize. Assuming that there is no other change in the desirability of the various public and private goods, the logical response - and under certain political-economy assumptions, the predicted response - is to reoptimize and to raise new revenue to keep spending from falling as much as it would if it went down by the full amount of the negative revenue shock.

The amount of new revenue raised will be limited by two key factors. First, to the extent that the negative revenue shock is non-negligible as a share of total income, public plus private, desired spending levels will now be lower than before. For a given amount of yearly revenue lost, this reduction will of course be larger if the shock is permanent. Assuming a marginal propensity to consume public goods of 5 cents per dollar of income, a permanent negative revenue shock of 1 dollar should permanently reduce desired revenue raised by 5 cents. Second, there may be institutional and political constraints on how fast adjustments are made and whether they can be made at all. For example, Poterba (1994) studies state responses to economic downturns and how those responses are influenced by state-level budget rules and politics. He finds that immediately after an unexpected budget deficit, states decrease spending. In subsequent years, states close the deficit with tax increases. This finding is echoed by Cromwell and Ihlanfeldt (2015), who find that Florida municipalities reacted to the loss of property tax and intergovernmental transfer revenue during the Great Recession by increasing property tax rates, and by reducing capital expenditures as well as non-essential public services. Follette and Lutz (2011) similarly provide evidence for pro-cyclical local government responses to downturns. Similar logic applies on the spending side and for positive revenue shocks.

This phenomenon has received signficant attention, and is in this context often referred to as the “flypaper effect," the idea that shocks have more of an effect where they hit (Gramlich, 1977; Fisher, 1982; Hines and Thaler, 1995). The evidence for this phenomenon is decidedly mixed. For example, Gordon (2004) examines plausibly exogenous changes in Title I funding for school districts that occur shortly after the release of the Decennial Census. She finds evidence in support of the flypaper effect in the first year after the change: an increase in Title I funding leads to an increase in instructional spending. Three years out, however, localities adjust to the change in in-flows by decreasing revenue from other sources. This decrease in other revenue coupled with the increase in Title I funding yields a zero net change in instructional spending in the long-run. Knight (2002) shows that what looks like a flypaper effect in the context of the federal highway aid program is actually the result of grants being endogenous to spending priorities, while Lutz (2002) documents tax reductions that increase almost one for one with school grant receipts in New Hampshire.

On the other hand, Baicker (2004) finds that counties respond to sudden spending increases triggered by a capital crime conviction by contemporaneously raising taxes and cutting expenditures, specifically and in flypaper-type fashion, on public safety. Boylan and Ho (2017) find that the negative shock to state government finances induced by the Great Recession led to long-term cuts to education and health spending that were not undone during the recovery. These cuts did not simply eliminate wasteful spending but led to worse educational outcomes (Jackson et al., 2018) and are hard to fit into the Bradford-Oates framework. On the flipside, an example of a permanent positive revenue shock is studied by David and Ferreira (2017), who observe that rising housing prices between 1990 and 2009 caused a 20% increase in real per-pupil public-school spending. Singhal (2008) rationalizes flypaper-type responses like these with a model of special-interest politics and confirms the existence of the phenomenon in the context of tobacco control policies. Leduc and Wilson (2017) provide additional evidence of such dynamics by showing that state-level highway spending increases in response to federal grants are greater in states with more political contributions from the public-works sector.

More directly related to the type of revenue shock we study, shocks to sales tax revenue that are likely to be permanent or at least different from business cycle fluctuations, is Agrawal's (2015) investigation of how local governments respond to the growing shift to e-commerce. Agrawal argues that because of enforcement problems and legal complications, the Internet serves as a sales tax haven. Using variation in Internet penetration, he finds that municipalities and states, chasing after disappearing revenue in a race to the bottom, reduced sales tax rates dramatically in response to the shift to online retail. These dynamics contributed to the rapid decline in sales tax revenue observed by Bruce et al. (2015). Diversification of revenue sources and use of rainy-day funds are examples of ways to deal with such unexpected negative shocks to a drop in a certain type of revenue. We provide a comprehensive, nationwide set of estimates of the size and composition of cities' responses to such events. Initially, an unexpected sales tax loss translates to a fall in own-source revenue and spending cuts. Eventually, cities adjust by increasing revenue from property taxes, financial transactions, and charges or fees. This diversification response is strongly supported by the normative framework in Seegert (2016).

These papers, including ours, fall under the umbrella of "fiscal federalism," a literature pioneered by Musgrave (1959) and Oates (1972) that discusses public finance and the role of federal, state, and local governments. Questions about the appropriate level of decentralization are central to the literature on fiscal federalism. In general, the fiscal federalism literature argues that decentralized provision of public goods increases economic welfare by satisfying heterogeneous preferences across jurisdictions, albeit at the cost of a potential race to the bottom fueled by tax competition. If we envision the fiscal and tax policies of a municipality as a public good, this same logic applies. Decentralized decision-making about these policies benefits from greater efficiency in satisfying the varied circumstances unique to each municipality (Oates, 1999). We directly test this hypothesis by studying the effect of the greater autonomy provided by "home rule" on the fiscal health of cities, a concrete example of the type of institutional feature that affects the speed with and extent to which local governments can respond to revenue shocks.

In the U.S. context, home rule is a term that refers to a greater level of autonomy local governments receive from their state. Debates about whether local governments should have such greater autonomy usually touch on efficiency and effectiveness of different forms of governance. Home rule supporters argue that greater autonomy allows local citizens to address problems specific to their communities according to their preferences and with expedience (Tiebout, 1956). With home rule, local governments do not have to wait for approval from the state legislature or state officials to carry out policies. Stronger home rule also reduces the chance of state legislatures "impos[ing] unfunded mandates" on local governments (Richardson et al., 2003). At the same time, state governments can use the time they save to focus on state-wide affairs (Vanlandingham, 1968). On the other hand, supporters of tighter state control over local governments argue that states can address local issues more effectively because they possess more technical expertise and can produce greater uniformity of governance and regulation (Richardson et al., 2003; Fajgelbaum et al., 2015).[1] This combination of considerations leads us to predict that localities with home rule will recover faster from a negative revenue shock, while the impact of home rule on long-term fiscal health is more ambiguous.

Although there is a long tradition of economists using theory to weigh the pros and cons of home rule in taxation (e.g. Secrist, 1914), there is limited empirical research on how cities fiscal policy is shaped by such features of their institutional and legal environment. Most work in this area has come in the form of case studies at the city or state level. Carroll and Johnson (2010), for example, find that towns in Connecticut and Maine, which have home rule, draw revenue from more diverse sources than towns in Minnesota and Vermont, which do not. We build on this type of research here, but expand it to the national level and exploit plausibly exogenous local variation in shocks to local government revenue, from nationwide bankruptcies of big-box chains, to identify causal responses. We find that, as predicted, cities with home rule recover more rapidly from negative shocks to revenue by drawing from a broader range of revenue sources. The flypaper effect is thus muted in such towns, as we expected.

The existing empirical evidence regarding concerns about the long-term fiscal health of cities with home rule is limited and mixed. Banovetz (2002) finds that 30 years of home rule in Illinois coincided with significant increases in tax rates as well as the adoption of new types of taxes, and that in some 5% of municipalities with home rule, voters, the courts, or the state legislature chose to retract that authority. That said, he argues that non-home rule municipalities, while not directly comparable, also witnessed tax hikes, and interprets the uncommon occurence of repeal as support for home rule status. Similarly, Latzko (2008) notes that while Pennsylvania counties with home rule increased their spending more than non-home rules counties, he finds no evidence of higher tax rates in home rule counties. We contribute to this literature by bringing evidence to the table on the causal impact of

[1] We discuss home rule as a legal and institutional construct in more detail in section 4.

home rule. We do this by exploiting a discontinuity in Illinois law that makes it so that cities with a population over 25,000 are automatically given home rule. In this analysis, we find that home rule cities have better bond ratings and a greater degree of financial stability, which suggests that the benefits of flexibility outweigh the discipline imposed by rules issued by higher levels of government.

## 2 Size and Persistence of the Revenue Shocks

In 2008, two major electronics retailers (Circuit City and CompUSA) and one major department store (Mervyn's) filed for bankruptcy and promptly liquidated the overwhelming majority of their existing stores. This was not due, however, to some industry-wide shock. Best Buy, JC Penney, and Kohl's - a competing electronics big-box retailer and two major department store chains, respectively - continued to operate healthily. The chains that went bankrupt and the ones that continued to operate faced similar local business environments, and there is little to suggest that location choices (as opposed to prior corporate decisions) drove their fate, as discussed at length in Shoag and Veuger (2018). They show, among other things, that pre-trends in employment and business activity look similar for neighborhoods around eventually defunct and non-defunct stores; that bankruptcies had a large impact even if they control for zip-year effects, use variation in bankruptcy timing only, or allow for year-specific slopes for zip code level traits such as median house price; and that the neighborhoods look similar when we compare characteristics ranging from racial composition to access to public transit. All of this serves to sustain the idea that the negative revenue shocks induced by these big-box bankruptcies are orthogonal to local economic trends, that they are not the result of weak demand or slow population growth in the host cities, and that they are plausibly exogenous shocks to the localities' economies. Note that all of this is true even when we allow, among other things, for arbitrary trends within zip codes. This makes it exceedingly difficult to construct counterfactuals that can explain the patterns we observe in local business activity.

In addition, here, in Table 1, we test whether cities with stores from these two types of chains were on parallel trends in terms of different characteristics of localities' public finances. We use ESRI Business Analyst data supplied by Harvard University's Center for Geographical Analysis to calculate the number of Best Buy, Circuit City, CompUSA, JC Penney, Kohl's, or Mervyn's stores exist in each municipality in the U.S. in 2006. ESRI uses business data from InfoUSA, which compiles employment, sales, and location information on businesses in the United States, to construct its Business Analyst data. InfoUSA collects lists of establishments from phone directories, business filings, utility connections, press releases, web directories, annual reports, and other sources. It then surveys these establishments by phone (between 12 and 18 million establishments per year).

The financial characteristics tested include changes between 2005 and 2007 in debt outstanding; in debt retired; in house prices; in property tax, charges & fees, and miscellaneous revenue; in own-source revenue; in state intergovernmental revenue; in expenditures; and in sales tax revenue. To calculate these changes we use data from the U.S. Census of State and Local Government Finance for 2004 through 2012. The U.S. Census of State and Local Government Finance is conducted in full every five years (years ending in '2' and '7'). In other years, data is collected from a sample of local governments, and a new sample is chosen every five years (years ending in '4' and '9'). In all years, the Census collects data from in-sample local governments on revenues (taxes, charges, interest, etc.), expenditures (education, health, public safety, infrastructure, etc.), debt, and financial assets. In our analysis, we include cities that are in the Census of State and Local Government Finances for at least one year pre-2008 and at least one year post-2008, and present in the data for at least five years.[2] In addition, we remove a municipality if the change between the minimum and maximum values of sales or total revenue, or of total expenditures, is greater than 500% to ensure that our results are not driven

[2] Table A.1 shows that these sampling restrictions do not affect our results.

by outliers in terms of growth or by cities that fundamentally changed their tax system. We also drop one city with a population that appears to be miscoded, outlier cities with more than 50 of the big-box stores in our study (Houston, San Antonio, New York, and Los Angeles), cities with zero of such big-box stores (as there can be no bankruptcies there), cities with more sales tax revenue than total revenue, and cities that had zero sales tax revenue during 2004-2007 (as there can be no negative sales tax revenue shock there). In addition, we use data on local house prices from Zillow. We use the median price of houses sold in each month and in each zip code, convert those prices to the municipality level, and then take the mean of the monthly data to get an annual measure. This leaves us with a sample of between 322 and 450 cities, depending on data availability for each variable.[3]

As Table 1 shows, the bankruptcy variables (i.e. $BankruptDummy_i$ and $BankruptCount_i$) are not associated with different changes in financial characteristics before the bankruptcies occurred, which further supports the claim that the two types of chains were located in cities that were on parallel paths in the years before 2008. Given all this, national-level bankruptcies allow us to identify the effects of negative revenue shocks on local government finances by deploying a difference-in-difference design.

Specifically, we compare cities that were home to a bankrupt chain to cities that were home to a comparable surviving chain, and we do so both before and after the bankruptcies in 2008. Specifically, we run regressions of the following kind:

$$ln(Revenue^h_{it}) = \alpha + \beta(BankruptDummy * Post)^c_{it} + \theta(OperatingDummy * Post)^c_{it} + \delta_i + \gamma_t + \varepsilon_{it} \quad (1)$$

[3]Table A. 2 provides summary statistics for cities that were hit with bankruptcies and cities that were not, both before 2008 and for the full period, as well as for cities with and without home rule.

where $Revenue_{it}^{h}$ is revenue in category $h$, where the category is either total sales tax and gross receipt revenue or total own-source revenue in municipality $i$ in year $t$. $BankruptDummy_{i}^{c}$ equals 1 when municipality $i$ contains one or more of the treatment chains of type $c$. The store type is electronics, department store, or pooled (i.e. both). $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. The interaction term is our variable of interest. We control for whether or not municipality $i$ contains any operating stores in category $c$ after the bankruptcy year using the interaction term formed by $OperatingDummy_{i}^{c}$ and $Post_t$[4]. Finally, $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. One of the columns reports results for regressions that include state by year fixed effects. Standard errors are clustered at the county level. We also estimate similar regressions that contain counts of the number of bankrupt big-box stores instead of the dummy variable shown in equation 1. Note that the identifying assumption here is that, conditional on the included covariates, fiscal outcomes would have evolved similarly across the two types of localities in the sample had the national-chain bankruptcies not occurred. As we saw, Table 1 suggests that this would indeed have been the case.[5]

Table 2 shows our estimates of the effect the bankruptcies had on local revenue. Panel A of Table 2 shows that municipalities suffered a loss of between 9% and 16% of local sales tax revenue and gross receipt revenue, depending on the chain type under study.[6] While a single bankruptcy, even the bankruptcy of a big-box retailer, is unlikely to cause such a large decline, Shoag and Veuger (2018) show that significant numbers of stores located close to a Circuit City, CompUSA, or Mervyn's store shut down as a consequence of their disappearance. Panel B of the same table shows that for each big-box store going bankrupt, a municipality's total sales tax and gross receipt revenue will go down by about

[4]As Appendix Table A.3 shows, whether we include this control or not does not affect our results qualitatively.

[5]These results, and those in the rest of the paper that rely on the same approach, are not qualitatively different when we use a matching estimator that relies on Coarsened Exact Matching based on the municipalities' population and the number of big-box stores.

[6]Our unit of observation is the city-year. We have 539 cities and 9 years of data. The total sample is 4,350 city-year observations. Note: the sample is not 539 x 9 = 4,851 because our sample restriction only requires that cities have at least 5 years of data and be present pre- and post-2008.

1.6% to 4.3%. Panel C from Table 2 shows that because the municipalities in our sample rely heavily on local sales tax revenue, this shock actually translates into a significant dent in total own-source revenue, with decreases of between 4.1% and 5.8%. Panel D paints a similar picture; for each big-box store going bankrupt, a municipality's own-source revenue will go down by about 1.8% to 3.0%. These results are robust to the inclusion of state-by-year fixed effects, which is of particular interest because it demonstrates that they are not driven by the differential impact of the Great Recession across the country.[7]

We then test the persistence of the shocks to revenue by interacting the bankruptcy dummy variable with dummy variables for the year before the bankruptcy and the four years after. Panel A from Table A.4 shows that the shocks to sales tax revenue (i.e. the reduction of total sales tax and gross receipt revenue of about 7% to 12%) persisted even four years after the bankruptcy, perhaps because municipalities struggled to fill empty store fronts, or because customers switched to online shopping permanently. In fact, the effect of bankruptcy becomes 1% to 3% more severe from year 1 to year 4 after the bankruptcies. Interestingly, Panel B from Table A.4 shows that the decline in total own-source revenue decays within one or two years, as municipalities turn towards other sources of revenue for the loss. The next section sheds light on that development; in that and all following sections, we pool our two types of stores, as our findings in this section have shown that there appear to be no qualitative differences between the shocks produced by the bankruptcies of the different chain types.

## 3 Local Government Responses

We now turn our attention to the way in which local policymakers respond to the drops in revenue estimated in the previous section. As discussed, we focus on pooled estimates for the remainder of the

[7] In Table A.3, we demonstrate that our main results are robust to eliminating the $OperatingDummy$ term, the addition of state-level controls, and the addition of county-level finance data.

paper, and, as a consequence, our $BankruptDummy$ variable will now equal 1 if a city contained either a Circuit City, a CompUSA, or a Mervyn's, and 0 otherwise. Analogously, $BankruptCount$ will now be the sum of Circuit City, CompUSA, and Mervyn's stores in a city.

Let us first look at spending. We run regressions of the following form:

$$ln(Expenditure_{it}^{h}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it} \tag{2}$$

where $Expenditure_{it}^{h}$ is the amount of local government expenditures in category $h$, where the categories are total expenditures, police protection, capital outlays, financial administration, debt outstanding, and cash securities. Panel A from Table 3 shows estimates of the drop in four of the six categories, with the most severe reductions in financial administration (about 10%) and cash securities (about 7%). The estimate for total expenditures, a 3.36% decrease, is only slightly smaller than the effect we found on total own-source revenue (3.41% decrease).[8] As to the type of expenditures that are cut, we confirm the findings of Baicker (2004) and Cromwell and Ihlandfeldt (2015): cities decrease spending on police protection and administrative services. Panel B of Table 3, which replaces Bankrupt Dummy with Bankrupt Count, shows similar results; the more big-box stores went bankrupt, the higher the reduction on various expenditures.

Turning back to the revenue side, Figure 1a shows that there is a statistically significant reduction in sales tax revenue and own-source revenue generally immediately after the bankruptcy year, as implied by Panels C and D from Table 2. The pre-trends suggests that this a causal consequence of the bankruptcies. However, when examining the more specific components of municipalities' own-source

[8] In Table A.3, we demonstrate that our main results are robust to the specification, the addition of state-level controls, and the addition of county-level finance data.

revenues, Panel C of Table 3 shows that big-box shocks actually have a positive effect on financial transactions or property tax combined with financial transactions. We see here how municipalities immediately attempt to turn to other sources of revenue as their sales and gross receipt tax revenue declines. This result partially explains why the effect of the bankruptcies on own-source revenues is not as negative as the effect on total sales tax and gross receipt revenue. This difference in magnitude is also partially mechanical: sales tax revenue is only a fraction of own-source revenue, thus any decrease in sales tax revenue should lead to a smaller and proportional decrease in own-source revenue. In Panel D, we show estimates of the effect of bankrupt count on these revenues. The coefficients are positive but smaller in magnitude and not statistically distinguishable from zero. We now turn to an analysis of how the broader institutional environment affects these responses.

# 4 The Institutional Environment

In the U.S. context, “home rule" is a term that generally refers to a greater level of autonomy local governments receive from their respective state through the state constitution, state legislation, or local charter (Richardson et al, 2003). As implied by this definition, local governments, such as counties, municipalities, and townships, derive their existence and power solely from their respective states, and home rule does not give them complete autonomy (Vanlandingham, 1968). This view is in line with the Tenth Amendment of the U.S. Constitution, which states that “[t]he powers not delegated to the United States by the Constitution, nor prohibited by it to the States, are reserved to the States respectively, or to the people." In addition, the U.S. Constitution does not contain any reference to local governments (ACIR, 1993). Therefore, only a state has the power to grant home rule to its local governments (ACIR, 1993), and each state’s definition and implementation of home rule may differ from each other (National League of Cities, 2013).

What does this mean in practice? When questions regarding a local government's authority arise, the state constitution is consulted first, and if examining the state constitution does not resolve the issue, the courts will turn to the laws set by the state legislature.[9] If neither the state constitution nor the state law provides a clear answer, state courts decide.[10] There are two basic methods of interpretation: strict construction (also known as "Dillon's Rule") and liberal construction. Under strict construction, if a power is not enumerated among those granted to a local government, the local government does not have that power. On the other hand, under liberal construction, a local government possesses a certain power as long as it is not expressly taken from it. State constitutions and statutes can mandate either strict or liberal construction for different types of local governments.

These distinctions often date back to quite some time ago. In the early republic, local governments enjoyed significant leeway in determining their own economic policies, which gave rise to "the patronage-based awarding of utility franchises; and (..) the deliberate creation and extinguishment of municipalities to avoid accumulated debt." This widespread corruption prompted Judge John F. Dillon of Iowa to formulate Dillon's Rule in Clark v. City of Des Moines (1865) and in City of Clinton v. Cedar Rapids and Missouri River Railroad (1868). Dillon's Rule established the guiding principle of strictly interpreting the scope of the local governments' power, and its growing popularity sparked debates over the level of autonomy local governments should have. Partially in response to these new strict construction practices, the home rule movement gained momentum in the late 19th century as states like Missouri (1875), California (1879), and Washington (1889) adopted constitutional home rule provisions that gave more autonomy to local governments. Over time, the back and forth over local autonomy has produced a range of combinations of devolved powers under home rule and of conditions under which localities qualify for home rule.

[9] This paragraph and the following three rely heavily on Richardson et al. (2003), and quotations originate there.

[10] A court's interpretation can of course be overruled if an amendment to the state constitution or if a new law enacted by the state legislature provides clear instructions on how to solve the issue.

For information on the organization of local governments, we draw from an ICMA (1974) survey, U.S. Advisory Commission on Intergovernmental Relations (ACIR) (1993), and Krane et al. (2002). We use these sources to construct four distinct measures of home rule: two at the state level, and two at the city level.

First, ACIR (1993) reports whether a state has granted structural home rule authority and/or broad functional home rule authority to the cities in that state. Cities with structural home rule authority are given the power to choose their own form of government, while those with functional home rule authority are given autonomy over local government functions such as taxation. According to ACIR, by 1993, forty states had granted their cities structural home rule authority, while only twenty-eight had granted them functional home rule authority. In our analysis, we use the functional home rule measure, as this type of home rule grants autonomy that is more important to the type of decision making that we focus on.

Second, Krane et al. (2002) also include information that indicates which states have structural, functional, and limited functional home rule. They report that thirty-one states had granted functional or limited functional home rule authority as of 2002. We use their data to construct a third measure as well, this one city-specific. Krane et al. (2002) detail the population each state requires a city to reach before it can be granted home rule status. Using these population limits, we can exclude non-home rule cities that are in home rule states but that have not met the requirements for home rule authority.

Finally, the ICMA (1974) survey gives us a city-level answer to the question "Within what type of charter or basic law does your city operate?," where the options were "unique charter", "uniform charter", "classification charter", "optional charter", "home rule", and "other." [11]

To explore the consequences of these differences in policy instrument availability, we run regressions of the following type:

[11] Table A.5 presents the summary statistics of both home rule and non-home rule cities.

$$ln(Revenue^h_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule)^r_{it} + \beta(BankruptCount * Post)_{it}$$
$$+\rho(HomeRule * Post)^r_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it} \quad (3)$$

where $Revenue^h_{it}$ is total local government revenue in category $h$, where $h$ is either total sales tax and gross receipts revenue or total own-source revenue. $BankruptCount_i$ is the total number of bankrupt stores in city $i$, $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year), while $HomeRule^r_i$ is a dummy variable equal to 1 if the city has home rule status according to measure $r$, where $r$ is one of the four home rule measures. The triple interaction is our variable of interest. In addition, we control for all other interactions either directly or through municipalities fixed effects. We also control for the number of still-operating stores a city has after 2008 and we include year fixed effects.

Panel A of Table 4 shows no robust, statistically significant difference in sales tax revenue between municipalities that enjoy home rule and municipalities that do not, which is unsurprising: there is no reason why policy flexibility should shield you from the kind of negative revenue shock that a big-box store bankruptcy triggers. For ease of comparison, we include a row labeled "Combined Effect" that represents the total effect of the bankruptcy ($\phi + \beta$) in home rule municipalities. This can be compared to the row labeled "Bankrupt Count" or "Bankrupt Dummy" that represents the total effect of the bankruptcy ($\beta$) in municipalities without home rule. Panel B of Table 4, which presents the results for robustness tests that replace Bankrupt Count with Bankrupt Dummy, supports the same conclusion.

However, there is reason to believe that home rule allows you to recover more swiftly, and we see evidence of that in Table 5. Cities with home rule status face smaller declines in total own-source revenue after the shock. In fact, columns 2, 3, and 4 in Panel A of Table 5 show municipalities with home rule status experience a reduction in own-source revenue that is 60% less severe than that experienced by those without home rule when we rely on our bankruptcy count estimator. Panel B shows that when we focus purely on the extensive margin - that is, experiencing any number of bankruptcies versus zero bankruptcies - the impact of home rule is similar if not even stronger.

Table 6 shows the mechanism through which this happens, at least partially: through property taxes, financial-market revenue, and miscellaneous revenue. Out of all the municipalities experiencing bankruptcies, the ones with home rule are able to raise about 3% more property tax, charges & miscellaneous revenue than the ones without. Figure 1b shows this dynamically, as revenue in home rule cities recovers more quickly than in cities without such flexibility. Table 7 shows that home rule and non-home rule cities are not statistically different in terms of post-bankruptcy expenditures. However, the estimated effect of bankruptcy on spending in home rule cities is approximately one-third to one-half of the estimated effect of bankruptcy on spending in non-home rule cities. The magnitude of this difference is similar to the difference in own-source revenue loss for home rule versus non-home rule cities.

Now, one may worry that home rule amendments are common to states in a particular region of the country, and that cities in that region rapidly recovered from the big-box store bankruptcies for other reasons. We estimate a series of regressions where we interact regional dummies with the bankruptcy count variable. Results are presented in Table A.6. Column 1 replicates column 3 from Panel A of Table 5. Column 2 controls for (interactions with) Census region, column 3 for Census division, while column 4 controls for state. The coefficients remain statistically significant and around 2% to 3%, except for the coefficient from column 4 that goes up to about 5.7%. Overall, the results are similar and our conclusions remain unchanged. In the next section we demonstrate that these results are unlikely to be driven by unobserved, systematically different features of cities with and without home rule.

# 5 Regression Discontinuity Analysis of Home Rule Status and Revenue Stability

The constitution of the State of Illinois states in Article VII, Section 6, that any municipality with a population above 25,000 is automatically given home rule authority. Municipalities with populations under this population cutoff can still elect via referendum to become home rule municipalities. Conversely, a municipality with a population above the cutoff can, by referendum, elect to remove its home rule authority. Illinois Comptroller's financial databases provide data on home rule status, population, and revenues of municipalities in Illinois.

Even though the population rule does not strictly determine home rule status, Figure 2 demonstrates that the probability a municipality has home rule does jump dramatically at the population cutoff of 25,000. We exploit this clear discontinuity in home rule status in a fuzzy regression discontinuity design. Note that there are two legitimate reasons why a number of towns below the threshold have home rule: towns generally do not lose home rule when their population decreases, and a town with a population below 25,000 can acquire home rule via referendum. Between 1970 and 2000 there were 191 referenda in Illinois, of which 97 passed and 94 failed (some of these latter towns also passed the 25,000 threshold during the same period). The existence of towns with home rule below the threshold is therefore not necessarily the result of strategic manipulation à la Eggers (2015). Appendix Figures 1 and 2 display the density of municipalities with a range of populations from 10,000 to 40,000 centered at the home rule population threshold in Illinois. The figures do not show a statistical break in the density of municipalities near the cutoff, which is evidence against endogenous sorting or manipulation of the running variable (McCrary, 2008).[12]

With those considerations in mind, we use whether or not a city is above the population cutoff

[12] Table A. 7 shows summary statistics by population.

as an instrument for whether or not the city has home rule. The Illinois Comptroller database contains reports from 1994-2015. While we focus our analysis on recent years (2010-2015), we use the maximum population from 1994-2009 as our measure of population, as the maximum population is the relevant population for home rule determination. It is rare for home rule authority to be revoked, and therefore a city need only cross the threshold once to become a home rule city. We ignore population post-2010 since it is endogenous to revenue changes. Our preferred specification is a local linear fuzzy regression discontinuity design with a triangular kernel, which places the most weight on those cities closest to the population cutoff. We estimate the model using a range of population bandwidths (from +\- 12,500 to +\- 20,000), and in all cases, the results are qualitatively the same.[13] Specifically, we estimate the following first stage regression on the sample of municipalities near the discontinuity:

$$HomeRule_i = \alpha + \beta(Above25000)_i + \theta(Population)_i + \rho(Above25000 * Population)_i + \varepsilon_i \tag{4}$$

Results are shown in Panel A of Table 8. We see that municipalities with a population over 25,000 are about 60% more likely to have home rule authority.

The second stage now produces an estimate of the causal effect home rule status has on revenue stability, as follows:

$$RevFall_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i \tag{5}$$

where $RevFall_i$ is the largest percent fall in revenue from 2010 to 2015 in municipality $i$. Note that $RevFall_i$ is strongly correlated with the standard deviation in per capita revenue from 2010-2015; the correlation is about 0.5 after excluding outliers above the 99th percentile in both variables. In other words, places with bigger revenue falls also have more variation in per-capita revenue over the full time

[13]Table A.8 shows a number of additional bandwiths as robustness checks.

period. We believe that $RevFall_i$ is a better measure of volatility since we argue and find that home rule municipalities are good at forestalling and curtailing revenue shocks. Case in point: Panel B shows that home rule makes revenue reduction 8% to 10% less severe. Panel C replaces $RevFall_i$ with $RevFall10_i$, which is a dummy variable that equals 1 if municipality $i$ has a fall in revenue larger than 10% at any point from 2010 to 2015. Municipalities with home rule are about 20% to 30% less likely to experience revenue reduction greater than 10%. Panel D replaces $RevFall_i$ with $RevFall30_i$, which is a dummy variable that equals 1 if municipality $i$ had a fall in revenue larger than 30% at any point from 2010 to 2015. Again, municipalities with home rule status are significantly less likely (18% to 19% less likely) to experience a dramatic fall in revenue.

Taken together, these results suggest that home rule municipalities are not as vulnerable to sharp revenue downturns as non-home rule municipalities. Last but not least, Panel E replaces $RevFall_i$ with $StrongBond_i$, which is a dummy variable that equals 1 if municipality $i$ has an extremely strong (triple A) bond rating. Bond ratings for years 1994-1996 at the municipality-level are from the Illinois Comptroller's financial databases, and the more recent bond ratings were obtained by scrapping information from MunicipalBonds.com. In the data we use, 653 Illinois cities issue bonds (approximately half of the cities in the state). The percentage is even greater for cities from 5,000 to 45,000 in population: 80% of the sample of cities in that range, or 266 total. Bond ratings are not available for all of these cities. About 240 cities in the bond data do not have a bond rating available and 67 cities in our sample bandwidth do not have a bond rating available. We code these cities as not having an extremely strong bond rating. This decision does not affect the results. First, the probability that a city is missing a bond rating does not jump discretely at the 25,000 home rule threshold. Second, if we instead code those cities as missing for the bond analysis, the results are qualitatively similar to our reported results.

The regression results in Panel E show that municipalities with home rule are about 35% more likely to have an extremely strong bond rating.[14] This inference is, of course, only valid locally, but over 40% of the population in Illinois lives in municipalities with populations between 5,000 and 45,000 (i.e. populations within 20,000 of the threshold). Excluding Chicago, over 60% of people in Illinois live in those municipalities. With nearly half of the state's population (over 5 million people as of 2010) living in municipalities close to the home rule threshold, we believe that even the local inference is important and relevant for policy.[15]

If, in fact, home rule cities fare better after a shock, a model of spatial equilibrium à la Roback-Rosen would suggest that house prices will fall in all affected cities, but more so in non-home rule cities than in home rule cities. We test this using the median sale price of houses by zip code and month provided by Zillow (aggregated up to the city level and averaged within each year). Our estimates are shown in Table 9: the results are indeed consistent with our expectation. House prices decline substantially everywhere, but less so in home rule cities, which are both more insulated from and more effective at responding to shocks. This is precisely what one would expect if it were indeed home rule, not stringent rules imposed by higher levels of government, that are best suited to let localities manage the inevitable ebbs and flows of tax revenue.[16]

## 6 Discussion

As e-commerce becomes more dominant, local governments are likely to continue experiencing revenue shifts similar to those produced by the bankruptcies of big-box retail chains. The results above offer robust estimates of the effect of such shocks on revenue and expenditures, of local governments' responses, and of the importance of the legal framework cities operate in.

[14]Figures 3a, 3b, and 4 are graphical representations of the regression results from Panels B, C, and E, respectively, from Table 8.

[15]Tables A.8 and A.9 show that our estimates are reasonably robust to different specifications.

[16]Table A.10 shows that these results are reasonably robust to the inclusion of regional trends as well as the exclusion of boom-and-bust states.

In addition, we demonstrate that municipalities with less discretionary decision-making, i.e. no home rule, experience a sharper drop in revenue and a slower rebound in revenue than municipalities with more discretionary authority. The downside of autonomy typically considered in the literature on rules versus discretion is a lack of credibility and self-control. In the results of our regression discontinuity analysis of cities in Illinois we do not see evidence of home rule towns' bond ratings being worse, while our regression results suggest that their spending bounces back faster from negative shocks. This suggests to us that home rule cities are not more likely to live beyond their means: if anything, they are more fiscally responsible, suggesting that in this case, discretion trumps rules. This may be the case, in part, because of the constitutional restrictions placed by cities on their own spending and taxing abilities that Brooks et al. (2016) analyze. They find that these self-imposed home rule-type restrictions do indeed reduce municipal revenue growth.

These findings illustrate the upside of granting policymakers discretion, as opposed to tying their hands. In that sense, and despite the fact that this paper deals most directly with a question about local government responses to revenue shocks, we contribute to a larger literature on rules versus discretion started by Kydland and Prescott (1977). This is important in the context of both federalist systems like the United States and supranational organizations like the European Monetary Union. In that literature, with the upside of flexiblity comes potential downsides: a loss of focus on the long run and a loss of credibility. Similarly, while state governments recognize that home rule status can be a source of helpful flexibility in times of crisis, this is often coupled with concerns that giving local politicians too much leeway will result in financial distress in the long run. Our results suggest that one ought not worry about that too much.

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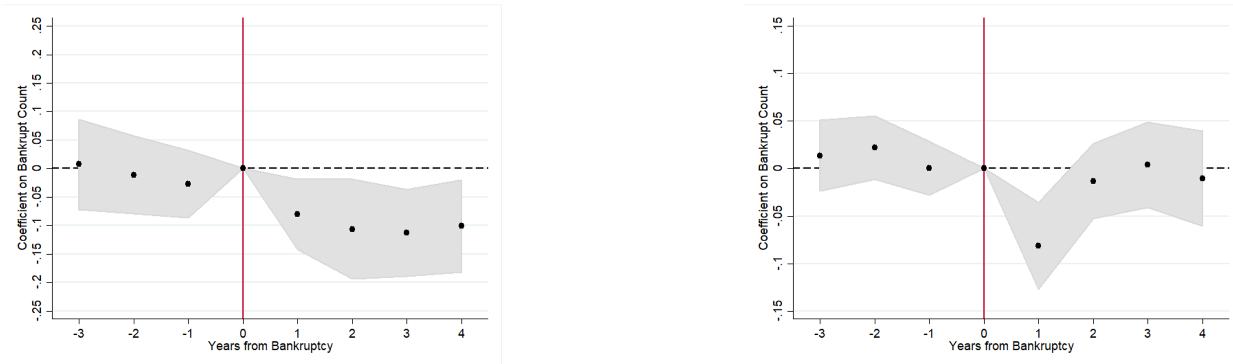
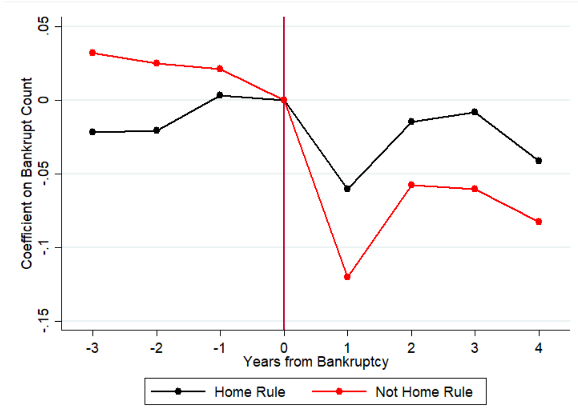
Figure 1a: Event Study of Bankruptcy Coefficients from 2005-2011

(a) Sales Tax and Gross Receipts (b) Own Source Revenue



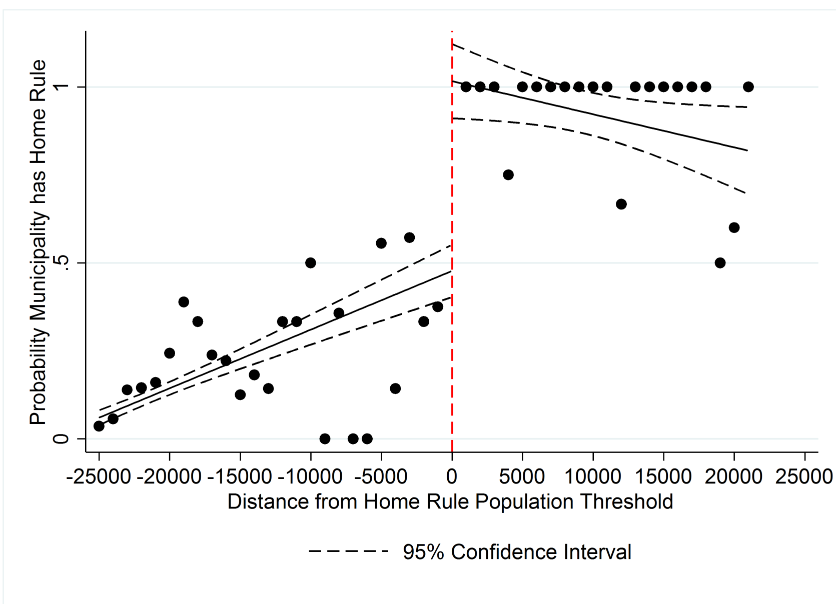

Note: The figure plots coefficients and confidence intervals from the following regression: $ln(Revenue_{it}) = \alpha + \beta(BankruptcyDummy)_i + \lambda(YearDummy)_t + \rho(BankruptcyDummy * YearDummy)_{it} + \theta(OperatingDummy * YearDummy)_{it} + \delta_i + \gamma_t + \sigma_{jt} + \varepsilon_{it}$ where $Revenue_{it}$ stands for total sales tax and gross receipts revenue in municipality $i$ in year $t$ in **subfigure a**. In **subfigure b** $Revenue_{it}$ represents total own source revenue for municipality $i$ in year $t$. In both panels, $BankruptcyDummy_i$ is an indicator that has a value of 1 if any big-box stores go bankrupt in municipality $i$. $YearDummy_t$ is a series of dummies taking a value of 1 for each of the years in our sample, centered at 2008. $(BankruptcyDummy * YearDummy)_{it}$ is an interaction between the bankruptcy dummy variable and a series of dummies for each year in our sample. $OperatingDummy_{it}$ takes a value of 1 if any opperating big-box stores are still in the municipality and this is interacted with $YearDummy_t$. $(OperatingDummy * YearDummy)_{it}$ is an interaction between the indicator for any operating big box stores and a series of dummies for each of the sample years. $\delta_i$ represents municipality fixed effects, $\gamma_t$ represents time fixed effects, and $\sigma_{jt}$ represents state-year fixed effects.

Figure 1b: Own-Source Revenue Event Study by Home Rule Status



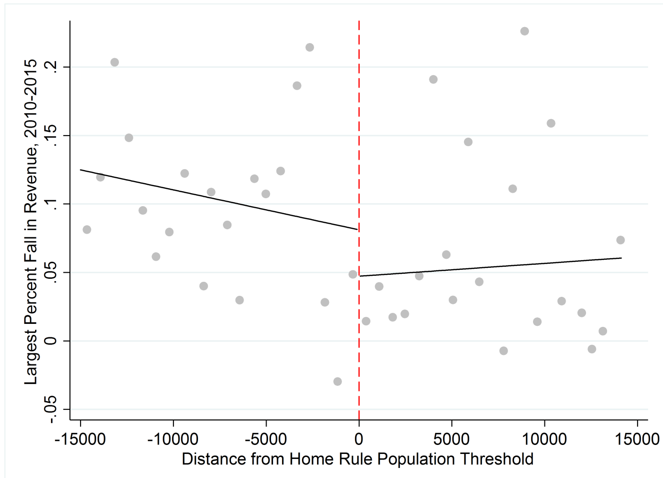

Note: The figure plots results of the following regression for Home Rule and Not Home Rule municipalities separately: $ln(OwnSource_{it}) = \alpha + \beta(BankruptcyDummy)_i + \lambda(YearDummy)_t + \rho(BankruptcyDummy * YearDummy)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \varepsilon_{it}$ where $OwnSource_{it}$ stands for total own source revenue in municipality $i$ in year $t$. $BankruptcyDummy_i$ is an indicator that has a value of 1 if any big-box stores go bankrupt in municipality $i$. $YearDummy_t$ is a series of dummies taking a value of 1 for each of the years in our sample, centered at 2008. $BankruptcyDummy * YearDummy)_{it}$ is an interaction between the bankruptcy dummy variable and a series of indicators for each year centered at 2008. $OperatingDummy$ takes a value of 1 if any opperating big-box stores are still in the municipality and this is interacted with $Post_t$ which takes the value of 1 after 2008. $\delta_i$ represents municipality fixed effects. Home Rule designations follow from Krane et. al (2002)

Figure 2: Home Rule First Stage



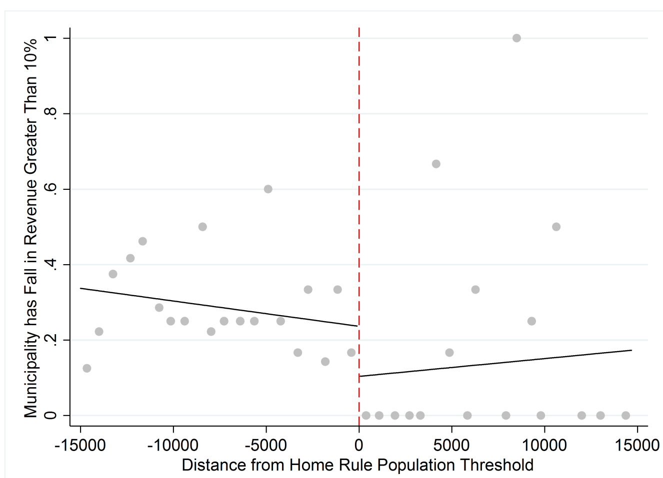

Note: The figure plots linear fit from two regressions of home rule (with $HomeRule$ taking a value of one if IL data indicates the city ever has home rule from 2010-2015) on the city's max population from 1994-2009: the first regression is run on cities with population under 25,000 and the second is run on cities with population above 25,000. The linear fits and confidence intervals are plotted over a binned scatterplot of home rule on population where each dot represents the average of the variable "home rule" for all cities within that bin. The size of the bins is 1,000.

Figure 3a: Effect of Home Rule on Revenue Stability



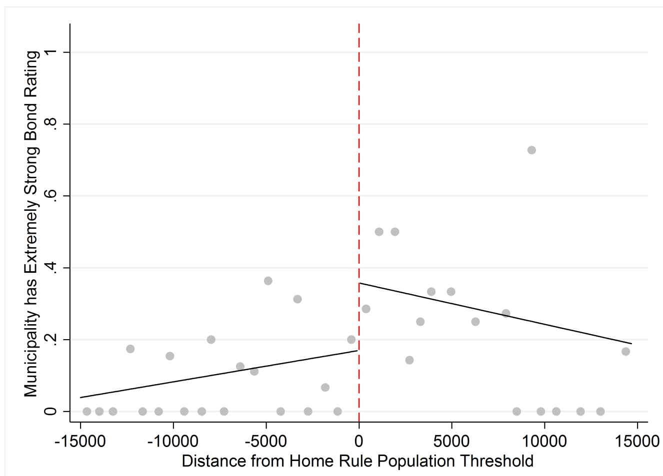

Note: The figure plots linear fit from fuzzy RD regression described below under **Table 8 - Panel B**

Figure 3b: Effect of Home Rule on Revenue Stability




Note: The figure plots linear fit from fuzzy RD regression described below under **Table 8 - Panel C**

Figure 4: Effect of Home Rule on Bond Rating




Note: The figure plots linear fit from fuzzy RD regression described below under **Table 8 - Panel E**

Table 1: Evidence of Parallel Pre-trends from 2005-2007

| **Panel A** | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **'05-'07 Debt Outstanding** | **'05-'07 Debt Retired** | **'05-'07 House Prices** | **'05-'07 Property Tax, Charges & Fees, Misc. Rev.** |
| Bankrupt Dummy | 0.0422 | 0.0321 | -0.0163 | 0.0266 |
| | (0.0361) | (0.0883) | (0.0141) | (0.0231) |
| Observations | 448 | 446 | 322 | 450 |
| | (5) | (6) | (7) | (8) |
| | **'05-'07 Own-Source Revenue** | **'05-'07 State Intergov. Revenue** | **'05-'07 Expenditures** | **'05-'07 Sales Tax Revenue** |
| Bankrupt Dummy | 0.0235 | -0.0477 | 0.0213 | -0.0283 |
| | (0.0145) | (0.1710) | (0.0176) | (0.0517) |
| Observations | 450 | 442 | 450 | 423 |

| **Panel B** | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | **'05-'07 Debt Outstanding** | **'05-'07 Debt Retired** | **'05-'07 House Prices** | **'05-'07 Property Tax, Charges & Fees, Misc. Rev.** |
| Bankrupt Count | 0.0005 | 0.0276 | -0.0016 | -0.0025 |
| | (0.0068) | (0.0242) | (0.0032) | (0.0057) |
| Observations | 448 | 446 | 322 | 450 |
| | (5) | (6) | (7) | (8) |
| | **'05-'07 Own-Source Revenue** | **'05-'07 State Intergov. Revenue** | **'05-'07 Expenditures** | **'05-'07 Sales Tax Revenue** |
| Bankrupt Count | 0.0017 | 0.0180 | -0.0030 | -0.0112 |
| | (0.0031) | (0.0429) | (0.0052) | (0.0094) |
| Observations | 450 | 442 | 450 | 423 |

Note: **Panel A** of this table reports estimates of regressions of the following form:

$$(ln(X_{2007}) - ln(X_{2005})) = \alpha + \beta BankruptDummy_i + \varepsilon_i$$

where $X_t$ is a financial characteristic of a municipality measured in year $t$. Eight financial characteristics are included, which are debt outstanding; debt retired; house prices; property tax, charges & fees & miscellaneous revenue; own-source revenue; state intergovernmental revenue; expenditures; and sales tax revenue. $BankruptDummy_i$ is equal to 1 if municipality $i$ has a big-box store that will go bankrupt (i.e. Circuit City, CompUSA, or Mervyns) and equal to 0 if it does not have a big-box store that will go bankrupt but does have one of the comparison stores (i.e. Kohls, JC Penney, or Best Buy). **Panel B** reports estimates of regressions of the following form:

$$(ln(X_{2007}) - ln(X_{2005})) = \alpha + \beta BankruptCount_i + \varepsilon_i$$

where $BankruptDummy_i$ is replaced with $BankruptCount_i$, which is equal to the number of big-box stores that will go bankrupt in municipality $i$ and equal to zero if the municipality does not have a big-box store that will go bankrupt but does have one of the comparison stores. Standard errors are clustered at the county-level in all of the regressions in Table 2, except for the regression with state intergovernmental revenue (column 6) where the standard errors are clustered at the state-level.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 2: Effect of Bankruptcy on Sales Tax and Gross Receipts Revenue

| | (1) Electronics | (2) Department | (3) Pooled | (4) Pooled: State by Year FEs |
|---|---|---|---|---|
| **Panel A** | **Total Sales Tax and Gross Receipt Revenue** | | | |
| Bankrupt Dummy | -0.1542*** | -0.1252*** | -0.1575*** | -0.0981*** |
| | (0.0467) | (0.0338) | (0.0383) | (0.0291) |
| Adjusted $R^2$ | 0.939 | 0.970 | 0.947 | 0.969 |
| **Panel B** | **Total Sales Tax and Gross Receipt Revenue** | | | |
| Bankrupt Count | -0.0433*** | -0.0405*** | -0.0307*** | -0.0167** |
| | (0.0144) | (0.0151) | (0.0088) | (0.0054) |
| Adjusted $R^2$ | 0.939 | 0.970 | 0.947 | 0.969 |
| Observations | 3473 | 2767 | 4346 | 4346 |
| **Panel C** | **Own-Source Revenue** | | | |
| Bankrupt Dummy | -0.0423*** | -0.0584*** | -0.0499*** | -0.0405** |
| | (0.0153) | (0.0164) | (0.0142) | (0.0174) |
| Adjusted $R^2$ | 0.986 | 0.989 | 0.988 | 0.989 |
| **Panel D** | **Own-Source Revenue** | | | |
| Bankrupt Count | -0.0258*** | -0.0302*** | -0.0186*** | -0.0180*** |
| | (0.0057) | (0.0088) | (0.0040) | (0.0054) |
| Adjusted $R^2$ | 0.986 | 0.989 | 0.988 | 0.989 |
| Observations | 3477 | 2768 | 4350 | 4350 |

Note: This table reports estimates of regressions of the following form:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptDummy * Post)_{it}^{c} + \theta(OperatingDummy * Post)_{it}^{c} + \delta_i \gamma_t + \varepsilon_{it}$$

where $Revenue_{it}$ is total sales tax and gross receipt revenue for **Panels A and B** and own-source revenue in **Panels C and D** in municipality $i$, in year $t$; $BankruptDummy_i^c$ in **Panels A and C** equals 1 when municipality $i$ contains one or more of the treatment chains of type $c$. The store type of column 1 is electronics store; the store type of column 2 is department store; columns 3 and 4 pool both electronics and department stores. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post$ controls for whether or not municipality $i$ contains any operating stores in category $c$ after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. **Panels B and D** of this table replaces $BankruptDummy_i^c$ with $BankruptCount_i^c$, which is the number of bankrupt big-box store of category $c$ in municipality $i$.

Column 4 "Pooled: State by Year FEs" includes state-by-year fixed effects in the regression specifications. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 3: Effect of Bankruptcy on Spending and the Revenue Recovery Thereafter

| | (1) Total Expenditure | (2) Police Protection | (3) Capital Outlays | (4) Financial Administration | (5) Total Debt Outstanding | (6) Cash Securities |
|---|---|---|---|---|---|---|
| **Panel A** | | | | | | |
| Bankrupt Dummy | -0.0336** | -0.0266* | -0.0274 | -0.1016* | -0.0715 | -0.0717* |
| | (0.0164) | (0.0148) | (0.0586) | (0.0532) | (0.0455) | (0.0389) |
| Adjusted $R^2$ | 0.986 | 0.984 | 0.828 | 0.827 | 0.961 | 0.955 |
| **Panel B** | | | | | | |
| Bankrupt Count | -0.0145*** | -0.0076*** | -0.0297** | -0.0324** | -0.0125 | -0.0208** |
| | (0.0040) | (0.0029) | (0.0134) | (0.0144) | (0.0094) | (0.0087) |
| Adjusted $R^2$ | 0.986 | 0.984 | 0.828 | 0.827 | 0.960 | 0.955 |
| Observations | 4350 | 4348 | 4329 | 4205 | 4340 | 4348 |

| | (1) Property Tax | (2) Total Charges & Misc. Revenue | (3) Fin. Transactions | (4) Property Tax+ Fin. Transactions | (5) Property Tax+Charges | (6) Property Tax+Charges+ Fin. Transactions |
|---|---|---|---|---|---|---|
| **Panel C** | | | | | | |
| Bankrupt Dummy | 0.0289 | 0.0061 | 0.1346* | 0.0931*** | 0.0296 | 0.0319 |
| | (0.0417) | (0.0256) | (0.0716) | (0.0324) | (0.0198) | (0.0207) |
| Adjusted $R^2$ | 0.965 | 0.974 | 0.857 | 0.966 | 0.985 | 0.983 |
| **Panel D** | | | | | | |
| Bankrupt Count | -0.0038 | 0.0025 | 0.0196 | 0.0046 | 0.0020 | 0.0021 |
| | (0.0089) | (0.0057) | (0.0183) | (0.0083) | (0.0045) | (0.0047) |
| Adjusted $R^2$ | 0.965 | 0.974 | 0.857 | 0.965 | 0.984 | 0.983 |
| Observations | 4208 | 4350 | 4348 | 4349 | 4350 | 4350 |

Note: **Panel A** of this table reports estimates of regressions of the following form:

$$ln(Expenditure^h_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $Expenditure^h_{it}$ is the amount of local government expenditures in category $h$. There are eight categories, which are total expenditures, police protection, capital outlays, financial administration, debt outstanding, and cash securities in municipality $i$, in year $t$. $BankruptDummy_i$ equals 1 when municipality $i$ contains one or more of the treatment chains. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post_{it}$ controls for whether or not municipality $i$ contains any operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. **Panel B** of this table replaces $BankruptDummy_i$ with $BankruptCount_i$, which is the number of bankrupt big-box store in municipality $i$. **Panel C** and **Panel D** replace $Expenditure^h_{it}$ with $Revenue^h_{it}$, which is the amount of local government revenue in category $h$. There are six categories, which are property taxes; total charges & miscellaneous revenue; financial transactions; property tax & financial transactions; property tax & charges; and property tax & charges & financial transactions in municipality $i$, in year $t$. As in Panel A, Panel C includes $BankruptDummy_i$, but in Panel D, $BankruptDummy_i$ is replaced with $BankruptCount_i$. Standard errors clustered at the county level are in parentheses. This table uses the pooled sample of municipalities. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 4: Post-Bankruptcy Comparison on Sales Tax between Home Rule & Non-Home Rule Cities

| **Panel A** | **Sales Tax and Gross Receipts** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Count | -0.0418*** | -0.0668** | -0.0741** | -0.0732** | -0.0762** |
| | (0.0118) | (0.0276) | (0.0345) | (0.0343) | (0.0331) |
| ACIR 1993 Home Rule x Bankrupt Count | | 0.0324 | | | |
| | | (0.0275) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Count | | | 0.0396 | | |
| | | | (0.0345) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Count | | | | 0.0385 | |
| | | | | (0.0343) | |
| ICMA 1974 Home Rule x Bankrupt Count | | | | | 0.0549 |
| | | | | | (0.0353) |
| Combined Effect: | | -0.0344*** | -0.0344*** | -0.0347*** | -0.0213* |
| Bankrupt+(HR x Bankrupt) | | (0.0116) | (0.0113) | (0.0113) | (0.0115) |
| Adjusted $R^2$ | 0.947 | 0.946 | 0.946 | 0.946 | 0.927 |
| Observations | 4346 | 4337 | 4337 | 4337 | 2739 |

| **Panel B** | **Sales Tax and Gross Receipts** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Dummy | -0.1494*** | -0.1325 | -0.1415 | -0.1546 | -0.2077* |
| | (0.0421) | (0.0907) | (0.1331) | (0.1327) | (0.1137) |
| ACIR 1993 Home Rule x Bankrupt Dummy | | -0.0146 | | | |
| | | (0.0970) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Dummy | | | 0.0056 | | |
| | | | (0.1352) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Dummy | | | | 0.0211 | |
| | | | | (0.1343) | |
| ICMA 1974 Home Rule x Bankrupt Dummy | | | | | 0.0982 |
| | | | | | (0.1257) |
| Combined Effect: | | -0.1471*** | -0.1360*** | -0.1335*** | -0.1095** |
| Bankrupt+(HR x Bankrupt) | | (0.0404) | (0.0358) | (0.0355) | (0.0551) |
| Adjusted $R^2$ | 0.947 | 0.946 | 0.947 | 0.946 | 0.928 |
| Observations | 4346 | 4337 | 4337 | 4337 | 2739 |

Note: This table reports estimates of regressions of the following form:

$$ln(SalesTax_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule)^r_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRule * Post)^r_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $SalesTax_{it}$ is the total sales tax and gross receipts revenue in municipality $i$, in year $t$. $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $HomeRule^r_i$ is a dummy variable equal to 1 if the municipality has home rule status according to measure $r$, where $r$ is one of the four home rule measures discussed in section 4. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount*Post$ and the coefficient on the interaction term $HomeRule * BankruptCount * Post$. This gives us the total effect of the bankruptcy on home rule municipalities. Panel B of this table replaces $BankruptCount_i$ with $BankruptDummy_i$, which equals 1 when municipality $i$ contains one or more of the treatment chains. (Note that from Table 4, the $BankruptCount_i$ variable appears in Panel A, before $BankruptCount_i$ in Panel B.) Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 5: Post-Bankruptcy Comparison on Own-Source Revenue between Home Rule & Non-Home Rule Cities

| **Panel A** | **Own-Source Revenue** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Count | -0.0191*** | -0.0369*** | -0.0407*** | -0.0401*** | -0.0312*** |
| | (0.0058) | (0.0074) | (0.0081) | (0.0081) | (0.0101) |
| ACIR 1993 Home Rule x Bankrupt Count | | 0.0221** | | | |
| | | (0.0087) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Count | | | 0.0257*** | | |
| | | | (0.0089) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Count | | | | 0.0250*** | |
| | | | | (0.0088) | |
| ICMA 1974 Home Rule x Bankrupt Count | | | | | 0.0207 |
| | | | | | (0.0127) |
| Combined Effect: Bankrupt+(HR x Bankrupt) | | -0.0149** | -0.0150** | -0.0152** | -0.0105 |
| | | (0.0063) | (0.0061) | (0.0061) | (0.0079) |
| Adjusted $R^2$ | 0.988 | 0.987 | 0.987 | 0.987 | 0.987 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |

| **Panel B** | **Own-Source Revenue** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Dummy | -0.0368** | -0.0721*** | -0.1149*** | -0.1177*** | -0.0561** |
| | (0.0160) | (0.0265) | (0.0321) | (0.0322) | (0.0231) |
| ACIR 1993 Home Rule x Bankrupt Dummy | | 0.0526* | | | |
| | | (0.0309) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Dummy | | | 0.1015*** | | |
| | | | (0.0354) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Dummy | | | | 0.1049*** | |
| | | | | (0.0351) | |
| ICMA 1974 Home Rule x Bankrupt Dummy | | | | | 0.0486 |
| | | | | | (0.0327) |
| Combined Effect: Bankrupt+(HR x Bankrupt) | | -0.0196 | -0.0134 | -0.0128 | -0.0075 |
| | | (0.0185) | (0.0170) | (0.0169) | (0.0254) |
| Adjusted $R^2$ | 0.988 | 0.987 | 0.987 | 0.987 | 0.987 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |

Note: This table reports estimates of regressions of the following form:

$$ln(OwnSourceRev_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule)^r_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRule * Post)^r_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $OwnSourceRev_{it}$ is own-source revenue in municipality $i$, in year $t$. $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $HomeRule^r_i$ is a dummy variable equal to 1 if the municipality has home rule status according to measure $r$, where $r$ is one of the four home rule measures discussed in section 4. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount * Post$ and the coefficient on the interaction term $HomeRule * BankruptCount * Post$. This gives us the total effect of the bankruptcy on home rule municipalities. Panel B of this table replaces $BankruptCount_i$ with $BankruptDummy_i$, which equals 1 when municipality $i$ contains one or more of the treatment chains. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 6: Post-Bankruptcy Comparison on Property Taxes and Charges & Fees between Home Rule & Non-Home Rule Cities

| **Panel A** | **Property Tax + Charges & Misc. Rev.** | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Count | 0.0056 | -0.0068 | -0.0232** | -0.0236** | -0.0044 |
| | (0.0065) | (0.0160) | (0.0117) | (0.0116) | (0.0108) |
| ACIR 1993 Home Rule x Bankrupt Count | | 0.0156 | | | |
| | | (0.0150) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Count | | | 0.0342*** | | |
| | | | (0.0122) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Count | | | | 0.0348*** | |
| | | | | (0.0122) | |
| ICMA 1974 Home Rule x Bankrupt Count | | | | | 0.0178 |
| | | | | | (0.0112) |
| Combined Effect: Bankrupt+(HR x Bankrupt) | | 0.0088 | 0.0110* | 0.0112* | 0.0134 |
| | | (0.0059) | (0.0066) | (0.0067) | (0.0085) |
| Adjusted $R^2$ | 0.984 | 0.984 | 0.984 | 0.984 | 0.983 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |
| **Panel B** | **Property Tax + Charges & Misc. Rev.** | | | | |
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Dummy | 0.0384* | -0.0108 | -0.0686** | -0.0613* | 0.0067 |
| | (0.0205) | (0.0369) | (0.0346) | (0.0352) | (0.0285) |
| ACIR 1993 Home Rule x Bankrupt Dummy | | 0.0736* | | | |
| | | (0.0428) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Dummy | | | 0.1383*** | | |
| | | | (0.0409) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Dummy | | | | 0.1295*** | |
| | | | | (0.0424) | |
| ICMA 1974 Home Rule x Bankrupt Dummy | | | | | 0.0638 |
| | | | | | (0.0419) |
| Combined Effect: Bankrupt+(HR x Bankrupt) | | 0.0628*** | 0.0696*** | 0.0683*** | 0.0705** |
| | | (0.0238) | (0.0229) | (0.0232) | (0.0322) |
| Adjusted $R^2$ | 0.985 | 0.984 | 0.984 | 0.984 | 0.983 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |

Note: This table reports estimates of regressions of the following form:

$$ln(PropTax_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule)^r_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRule * Post)^r_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $PropTax_{it}$ is revenue from property taxes and charges & fees in municipality $i$, in year $t$. $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $HomeRule^r_i$ is a dummy variable equal to 1 if the municipality has home rule status according to measure $r$, where $r$ is one of the four home rule measures discussed in section 4. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount * Post$ and the coefficient on the interaction term $HomeRule * BankruptCount * Post$. This gives us the total effect of the bankruptcy on home rule municipalities. Panel B of this table replaces $BankruptCount_i$ with $BankruptDummy_i$, which equals 1 when municipality $i$ contains one or more of the treatment chains. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* p < 0.1$, $** p < 0.05$, $*** p < 0.01$

Table 7: Post-Bankruptcy Comparison on Spending between Home Rule & Non-Home Rule Cities

| **Panel A** | | | **Total Expenditure** | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Count | -0.0170*** | -0.0274*** | -0.0297*** | -0.0299*** | -0.0182* |
| | (0.0065) | (0.0093) | (0.0107) | (0.0106) | (0.0107) |
| ACIR 1993 Home Rule x Bankrupt Count | | 0.0130 | | | |
| | | (0.0101) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Count | | | 0.0155 | | |
| | | | (0.0114) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Count | | | | 0.0157 | |
| | | | | (0.0114) | |
| ICMA 1974 Home Rule x Bankrupt Count | | | | | 0.0012 |
| | | | | | (0.0114) |
| Combined Effect: | | -0.0145** | -0.0142** | -0.0142** | -0.0170** |
| Bankrupt+(HR x Bankrupt) | | (0.0072) | (0.0071) | (0.0071) | (0.0081) |
| Adjusted $R^2$ | 0.986 | 0.986 | 0.986 | 0.986 | 0.985 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |

| **Panel B** | | | **Total Expenditure** | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Dummy | -0.0228 | -0.0495* | -0.0441 | -0.0458 | -0.0379 |
| | (0.0170) | (0.0266) | (0.0342) | (0.0338) | (0.0280) |
| ACIR 1993 Home Rule x Bankrupt Dummy | | 0.0393 | | | |
| | | (0.0339) | | | |
| Krane et al. 2002 Home Rule 1 x Bankrupt Dummy | | | 0.0310 | | |
| | | | (0.0389) | | |
| Krane et al. 2002 Home Rule 2 x Bankrupt Dummy | | | | 0.0332 | |
| | | | | (0.0385) | |
| ICMA 1974 Home Rule x Bankrupt Dummy | | | | | 0.0375 |
| | | | | | (0.0384) |
| Combined Effect: | | -0.0102 | -0.0131 | -0.0127 | -0.0004 |
| Bankrupt+(HR x Bankrupt) | | (0.0217) | (0.0194) | (0.0194) | (0.0250) |
| Adjusted $R^2$ | 0.986 | 0.986 | 0.986 | 0.986 | 0.985 |
| Observations | 4350 | 4341 | 4341 | 4341 | 2739 |

Note: This table reports estimates of regressions of the following form:

$$ln(Expenditure_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule)^r_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRule * Post)^r_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $Expenditure_{it}$ is total expenditure in municipality $i$, in year $t$. $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $HomeRule^r_i$ is a dummy variable equal to 1 if the municipality has home rule status according to measure $r$, where $r$ is one of the four home rule measures discussed in section 4. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount * Post$ and the coefficient on the interaction term $HomeRule * BankruptCount * Post$. This gives us the total effect of the bankruptcy on home rule municipalities. Panel B of this table replaces $BankruptCount_i$ with $BankruptDummy_i$, which equals 1 when municipality $i$ contains one or more of the treatment chains. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table 8: Home Rule Regression Discontinuity Analysis

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A. First Stage, Municipality has Home Rule.** | | | | |
| Population≥25,000 | 0.591*** | 0.592*** | 0.578*** | 0.594*** |
| | (0.116) | (0.106) | (0.096) | (0.090) |
| Observations | 148 | 183 | 259 | 314 |
| **Panel B. Largest Percent Fall in Revenue from 2010-2015.** | | | | |
| Municipality has | -0.100** | -0.081* | -0.081* | -0.085* |
| Home Rule | (0.048) | (0.048) | (0.047) | (0.044) |
| Observations | 148 | 183 | 257 | 312 |
| **Panel C. Percent Fall in Revenue Greater Than 10%.** | | | | |
| Municipality has | -0.293** | -0.228* | -0.197 | -0.177 |
| Home Rule | (0.136) | (0.131) | (0.129) | (0.124) |
| Observations | 148 | 183 | 258 | 313 |
| **Panel D. Percent Fall in Revenue Greater Than 30%.** | | | | |
| Municipality has | -0.198** | -0.182** | -0.185** | -0.192** |
| Home Rule | (0.092) | (0.090) | (0.086) | (0.081) |
| Observations | 148 | 183 | 258 | 313 |
| **Panel E. Municipality has Extremely Strong Bond Rating.** | | | | |
| Municipality has | 0.328 | 0.330 | 0.353* | 0.346* |
| Home Rule | (0.253) | (0.231) | (0.210) | (0.193) |
| Observations | 283 | 331 | 434 | 498 |
| Clusters | 135 | 162 | 218 | 257 |
| **Pop. Bandwidth** | $\pm 12,500$ | $\pm 15,000$ | $\pm 18,000$ | $\pm 20,000$ |

Note: **Panel A** of this table reports estimates of first-stage regressions of the following form:

$$HomeRule_i = \alpha + \beta(Above25000)_i + \theta(Population)_i + \rho(Above25000 * Population)_i + \varepsilon_i$$

where $HomeRule_i$ is a dummy variable equal to 1 if municipality $i$ ever had home rule status between 2010 and 2015. $Above25000_i$ is a dummy variable that equals to 1 if municipality's population exceeded 25,000 and equals to 0 otherwise. $Population$ is the maximum number of population municipality $i$ had sometime between 1994 and 2009. $Above25000 * Population$ is an interaction variable between $Above25000$ and $Population$. This regression establishes a link between the home rule population threshold in Illinois and a city's actual home rule status. Panels B, C, D, and E show the results of regressions with fuzzy regression discontinuity design, using instrumented $HomeRule$ variable to estimate various public-finance-related variables. **Panel B** reports estimates of regressions of the following form:

$$RevFall_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall_i$ is the largest percent fall in revenue from 2010 to 2015 in municipality $i$. **Panel C** reports estimates of regressions of the following form:

$$RevFall10_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall10_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 10% at any point from 2010-2015. **Panel D** reports estimates of regressions of the following form:

$$RevFall30_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall30_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 30% at any point from 2010-2015. **Panel E** reports estimates of regressions of the following form:

$$StrongBond_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $StrongBond_i$ is a dummy variable that equals to 1 if municipality $i$ has extremely strong bond rating in IL data (from 1994 to 1996) or in scraped data (2015).

Regressions from all five panels are run with four different population bandwidths. Column 1 includes cities with populations between 12,500 and 37,500; column 2 includes cities with populations between 10,000 and 40,000; column 3 includes cities with populations between 7,000 and 43,000; and column 4 includes cities with populations between 5,000 and 45,000. Standard errors clustered at the municipality level are in parentheses.

$* \; p < 0.1$, $** \; p < 0.05$, $*** \; p < 0.01$

Table 9: Home Rule and House Prices Over Time

| | House Price | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Dummy | -0.1130*** | | | | |
| | (0.0358) | | | | |
| Bankrupt Count | | -0.0253*** | | | -0.0463*** |
| | | (0.0097) | | | (0.0166) |
| One Year After Bankruptcy | | | -0.1109** | -0.0200** | |
| | | | (0.0434) | (0.0096) | |
| Two Years After Bankruptcy | | | -0.1030** | -0.0191 | |
| | | | (0.0511) | (0.0116) | |
| Three Years After Bankruptcy | | | -0.1111** | -0.0249** | |
| | | | (0.0456) | (0.0111) | |
| Four Years After Bankruptcy | | | -0.1252*** | -0.0363** | |
| | | | (0.0396) | (0.0177) | |
| Krane et al. Home Rule 1 x Bankrupt Count | | | | | 0.0343* |
| | | | | | (0.0185) |
| Adjusted $R^2$ | 0.903 | 0.902 | 0.902 | 0.902 | 0.906 |
| Observations | 3169 | 3169 | 3169 | 3169 | 3160 |

Note: **Column 1** of this table reports estimates of regressions of the following form:

$$ln(HousePrice_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $HousePrice_{it}$ is house prices in municipality $i$, in year $t$. $BankruptDummy_i$ equals 1 when municipality $i$ contains one or more of the treatment chains. **Column 2** replaces $BankruptDummy$ with $BankruptCount$, which is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post$ controls for whether or not municipality $i$ contains any operating stores of interest after the bankruptcy year. **Column 3** and **Column 4** report estimates of regressions of the following form:

$$ln(HousePrice_{it}) = \alpha + \beta(BankruptDummy * YearDummy)_{it} + \theta(OperatingDummy * YearDummy)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

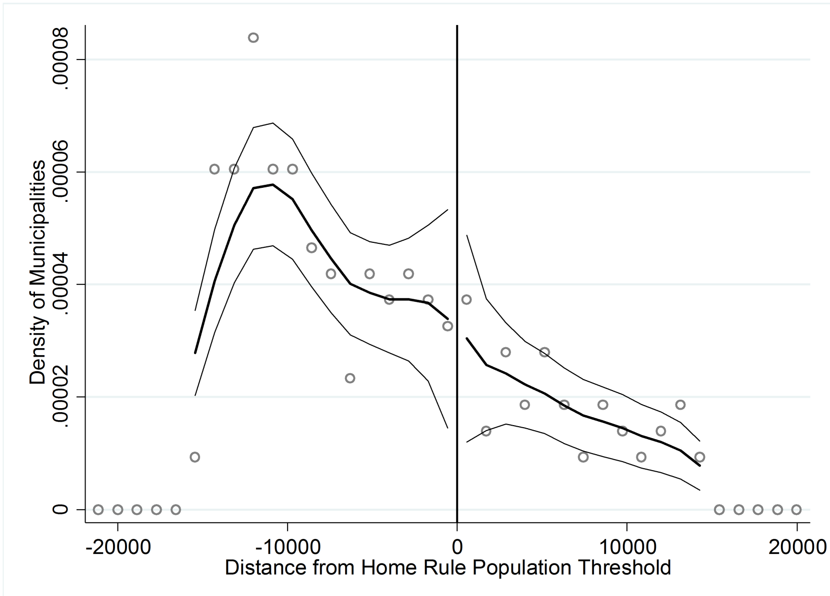
where $YearDummy_t$ represents dummy variables for each of the four years after the bankruptcy. Column 4 replaces $BankruptDummy$ with $BankruptCount$. $OperatingDummy * YearDummy$ controls for whether or not municipality $i$ contains any operating stores of interest during the corresponding year. **Column 5** reports estimates of regressions in the following form:

$$ln(HousePrice_{it}) = \alpha + \phi(BankruptCount * Post * HomeRuleKrane)_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRuleKrane * Post)_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $HomeRuleKrane_i$ is a dummy variable equal to 1 if the municipality has home rule status according to Krane et al. (2002). $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

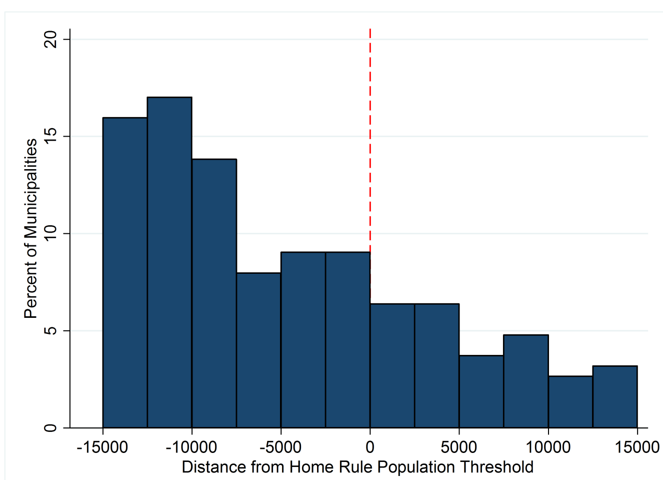
$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Figure A.1: Formal Test of Change in Density at Home Rule Population Threshold




Note: The figure displays the density of municipalities with a range of populations from 10,000 to 40,000 centered at the home rule population threshold in Illinois (population=25,000). The figure does not show a statistical break in the density of municipalities near the cutoff - this is evidence against endogenous sorting or manipulation of the running variable.

Figure A.2: Home Rule, Population Histogram–No Sorting at Cutoff




Note: The figure plots histogram showing percent of municipalities falling in each population bin. The size of the bins is 2,500.

Table A.1: Main Results are Robust to Sample Restriction

| | (1) Sales Tax & Gross Receipts Revenue | (2) Total Own-Source Revenue | (3) Total Expenditures |
|---|---|---|---|
| **Panel A. Cities in Data Three or More Years** | | | |
| Bankrupt Dummy | -0.1582*** | -0.0488*** | -0.0330** |
| | (0.0381) | (0.0142) | (0.0164) |
| Observations | 4363 | 4367 | 4367 |
| Adjusted $R^2$ | 0.948 | 0.988 | 0.986 |
| **Panel B. Cities in Data Four or More Years.** | | | |
| Bankrupt Dummy | -0.1582*** | -0.0488*** | -0.0330** |
| | (0.0381) | (0.0142) | (0.0164) |
| Observations | 4363 | 4367 | 4367 |
| Adjusted $R^2$ | 0.948 | 0.988 | 0.986 |
| **Panel C. Cities in Data Six or More Years.** | | | |
| Bankrupt Dummy | -0.1599*** | -0.0528*** | -0.0349** |
| | (0.0408) | (0.0150) | (0.0172) |
| Observations | 3928 | 3929 | 3929 |
| Adjusted $R^2$ | 0.944 | 0.987 | 0.986 |
| **Panel D. Cities in Data Seven or More Years.** | | | |
| Bankrupt Dummy | -0.1598*** | -0.0527*** | -0.0317* |
| | (0.0422) | (0.0155) | (0.0176) |
| Observations | 3712 | 3713 | 3713 |
| Adjusted $R^2$ | 0.941 | 0.986 | 0.985 |

Note: Typically, we allow municipalities in our sample if they are in the data for 5 or more years. In this table, we change that sample restriction as a robustness check. In all panels, we run the same regressions listed in Tables 2 and 3. In **Panel A**, we allow municipalities in our sample if they are in the data for 3 or more years. In **Panel B**, we allow municipalities in our sample if they are in the data for 4 or more years. In **Panel C**, we allow municipalities in our sample if they are in the data for 6 or more years. In **Panel D**, we allow municipalities in our sample if they are in the data for 7 or more years. This sample restriction does not affect the results.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.2: Per Capita Differences Between Cities with Defunct and Operational Chains, pre-2008

| | No Bankrupt Stores | Bankrupt Stores | Difference: (No Bankrupt - Bankrupt) |
|---|---|---|---|
| Total Revenue | 1.991 | 2.049 | -0.058 |
| | (1.487) | (1.280) | (0.120) |
| Total Own-Source Revenue | 1.714 | 1.738 | -0.024 |
| | (1.192) | (1.067) | (0.098) |
| Total Taxes | 0.729 | 0.809 | -0.080 |
| | (0.643) | (0.514) | (0.050) |
| Total Sales Taxes | 0.427 | 0.386 | 0.041 |
| | (0.357) | (0.266) | (0.027) |
| Total Property Taxes | 0.217 | 0.327 | -0.110*** |
| | (0.236) | (0.339) | (0.026) |
| Total Charges & Misc. Revenue | 0.558 | 0.491 | 0.067 |
| | (0.594) | (0.332) | (0.040) |
| Financial Transactions | 0.085 | 0.074 | 0.011 |
| | (0.336) | (0.136) | (0.021) |
| State Intergov. Revenue | 0.174 | 0.207 | -0.033 |
| | (0.235) | (0.300) | (0.024) |
| Total Spending | 1.947 | 2.035 | -0.088 |
| | (1.446) | (1.321) | (0.120) |
| Police Spending | 0.214 | 0.240 | -0.026** |
| | (0.100) | (0.088) | (0.008) |
| Capital Outlays | 0.413 | 0.420 | -0.007 |
| | (0.394) | (0.329) | (0.031) |
| Financial Administration | 0.039 | 0.041 | -0.002 |
| | (0.038) | (0.036) | (0.003) |
| Cash Securities | 2.440 | 2.672 | -0.232 |
| | (7.841) | (4.037) | (0.519) |
| Total Debt Outstanding | 2.620 | 2.682 | -0.062 |
| | (7.686) | (4.035) | (0.512) |
| Within-City Std. Dev. Sales Tax | 0.059 | 0.043 | 0.016** |
| | (0.064) | (0.042) | (0.005) |
| Within-City Std. Dev. Own-Source Revenue | 0.209 | 0.215 | -0.006 |
| | (0.184) | (0.177) | (0.017) |
| Within-City Std. Dev. Expenditures | 0.247 | 0.269 | -0.022 |
| | (0.281) | (0.267) | (0.026) |
| Observations | 228 | 310 | 538 |

NOTE: This table reports summary statistics for variables used in this paper. It reports differences in means for vairables used on a per capita basis between municipalities with a bancrupt chain and those without a bankrupt chain before 2008. For per-capita analysis, we remove nine cities with populations below 1,000. This is not important in other analyses since we account for city fixed effects, but for per-capita analysis, these outliers drastically change the means. mean coefficients; standard deviations in parentheses.

Table A.3: Main Results are Robust to Covariates and Inclusion of County Finances

| | (1) Sales Tax & Gross Receipts Revenue | (2) Total Own-Source Revenue | (3) Total Expenditures |
|---|---|---|---|
| **Panel A. Omitting Controls for Operating Stores** | | | |
| Bankrupt Dummy | -0.1467*** | -0.0447*** | -0.0336** |
| | (0.0386) | (0.0145) | (0.0164) |
| Observations | 4346 | 4350 | 4350 |
| Adjusted $R^2$ | 0.947 | 0.988 | 0.986 |
| **Panel B. Including Controls for State-Level Finances and Unemployment** | | | |
| Bankrupt Dummy | -0.1269*** | -0.0480*** | -0.0302* |
| | (0.0405) | (0.0158) | (0.0166) |
| Observations | 4346 | 4350 | 4350 |
| Adjusted $R^2$ | 0.947 | 0.988 | 0.986 |
| **Panel C. County-Level Finances (not including School & Other Special Districts)** | | | |
| Bankrupt Dummy | -0.0933*** | -0.0692*** | -0.0394** |
| | (0.0290) | (0.0180) | (0.0181) |
| Observations | 3175 | 3174 | 3175 |
| Adjusted $R^2$ | 0.975 | 0.986 | 0.988 |

Note: In **Panel A**, we run the same regressions listed in Tables 2 and 3 but without the $OperatingDummy * Post$ term. This regression takes the following form:

$$ln(Outcome_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

Column 1 shows this result for sales tax & gross receipt revenue, column 2 shows this for own-source revenue, and column 3 shows this for expenditures. The inclusion or exclusion of this covariate does not affect the results. $BankruptDummy_i$ takes a value of 1 if a municipality has a bankrupt chain and $OperatingDummy_i$ if there is any operating chain in that municipality. These are both interacted with $Post_t$ which equals 1 after 2008. In **Panel B**, we run the same regressions listed in Tables 2 and 3 but with state-level finance and unemployment rate controls added (these vary at the state-year level). This regression takes the following form:

$$ln(Outcome_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it}$$
$$+\sigma_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

The specific controls state-level controls we include in $\sigma_{it}$ are: total revenue, total taxes, total expenditures, and total debt outstanding (from Census of Local Government Finance) and annual unemployment rate (from BLS Local Area Unemployment Statistics and Current Population Survey). Column 1 shows this result for sales tax & gross receipt revenue, column 2 shows this for own-source revenue, and column 3 shows this for expenditures. The inclusion or exclusion of this covariate does not affect the results. In **Panel C**, we run the same regressions listed in **Panel B** but wothout the state control vector and on data that is aggregated to the county-level instead of the municipality-level. Now, the finance measures (sales tax, total own-source revenue, and expenditures) include finances at township, municipality, and county level all aggregated to county as the unit of observation. This does not include school district finances or other special district finances since reporting does not appear to be as consistent from year to year. Column 1 shows this result for sales tax & gross receipt revenue, column 2 shows this for own-source revenue, and column 3 shows this for expenditures. Including county-level finances does not affect the results.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.4: Effect of Bankruptcy on Sales Tax and Own-Source Revenue Over Time

| | (1) Electronics | (2) Department | (3) Pooled | (4) Pooled: State by Year FEs |
|---|---|---|---|---|
| **Panel A** | **Total Sales Tax and Gross Receipt Revenue** | | | |
| One Year After Bankruptcy | -0.1230*** | -0.1417*** | -0.1469*** | -0.0771*** |
| | (0.0333) | (0.0466) | (0.0379) | (0.0244) |
| Two Years After Bankruptcy | -0.1272*** | -0.1489*** | -0.1501*** | -0.1029*** |
| | (0.0333) | (0.0493) | (0.0402) | (0.0339) |
| Three Years After Bankruptcy | -0.1268*** | -0.1666*** | -0.1663*** | -0.1086*** |
| | (0.0344) | (0.0479) | (0.0392) | (0.0302) |
| Four Years After Bankruptcy | -0.1238*** | -0.1591*** | -0.1651*** | -0.1029*** |
| | (0.0384) | (0.0484) | (0.0405) | (0.0343) |
| $R^2$ | 0.970 | 0.939 | 0.947 | 0.969 |
| Observations | 2,767 | 3,473 | 4,346 | 4,346 |
| **Panel B** | **Own-Source Revenue** | | | |
| One Year After Bankruptcy | -0.0766*** | -0.0885*** | -0.0803*** | -0.0929*** |
| | (0.0265) | (0.0255) | (0.0227) | (0.0232) |
| Two Years After Bankruptcy | -0.0432** | -0.0259 | -0.0311** | -0.0301* |
| | (0.0173) | (0.0168) | (0.0150) | (0.0179) |
| Three Years After Bankruptcy | -0.0473*** | -0.0189 | -0.0290* | -0.0133 |
| | (0.0170) | (0.0186) | (0.0166) | (0.0203) |
| Four Years After Bankruptcy | -0.0650*** | -0.0364* | -0.0581*** | -0.0278 |
| | (0.0200) | (0.0207) | (0.0183) | (0.0232) |
| $R^2$ | 0.989 | 0.986 | 0.988 | 0.989 |
| Observations | 2,768 | 3,477 | 4,350 | 4,350 |

Note: This table reports estimates of regressions of the following form:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptDummy * YearDummy)^c_{it} + \theta(OperatingDummy * YearDummy)^c_{it} + \\ +\delta_i + \gamma_t + \varepsilon_{it}$$

where $Revenue_{it}$ is total sales tax and gross receipt revenue for **Panel A** and own-source revenue in **Panel B** in municipality $i$, in year $t$; $BankruptDummy^c_i$ equals 1 when municipality $i$ contains one or more of the treatment chains of type $c$, where the store type is electronics, department store, or both; $YearDummy_t$ represents dummy variables for each of the four years after the bankruptcy. $OperatingDummy*YearDummy$ controls for whether or not municipality $i$ contains any operating stores in category $c$ during the corresponding year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \, p < 0.1$, $** \, p < 0.05$, $*** \, p < 0.01$

Table A.5: Summary Statistics by Home Rule Status

| | Krane et al. (2002) State-Level Measure | | ICMA (1974) Municipality-Level Measure | |
|---|---|---|---|---|
| | Not Home Rule | Home Rule | Not Home Rule | Home Rule |
| Population | 109,659 | 121,944 | 125,610 | 142,936 |
| | (191,603) | (203,985) | (248,466) | (196,893) |
| Number of Big-Box | 2.759 | 3.541 | 3.054 | 4.438 |
| | (2.689) | (3.461) | (2.126) | (4.558) |
| Total Revenue | 335,228 | 282,942 | 301,118 | 317,511 |
| | (928,556) | (691,208) | (754,940) | (563,074) |
| Total Own-Source Revenue | 274,666 | 236,797 | 250,754 | 275,638 |
| | (667,519) | (561,002) | (633,135) | (469,592) |
| Total Taxes | 109,427 | 101,092 | 109,049 | 102,044 |
| | (313,337) | (208,953) | (224,677) | (148,985) |
| Total Spending | 327,327 | 290,631 | 310,513 | 325,852 |
| | (885,429) | (695,875) | (793,521) | (577,036) |
| Total Debt Outstanding | 337,014 | 483,514 | 464,548 | 540,872 |
| | (896,226) | (1,450,430) | (1,722,903) | (1,284,031) |
| Observations | 783 | 3558 | 1326 | 1413 |

This table shows summary statistics for the municipalities in our sample based on whether or not they are defined as "home rule" municipalities. In the first two columns, the "home rule" breakdown is based on a state-level measure from Krane et al. (2002). In the last two columns, the "home rule" breakdown is based on a municipality-level measure from the 1974 International City/County Management Association (ICMA) survey. Standard deviations are in parentheses. The statistics provided are mean and standard deviation of municipality's population, the number of big-box stores (the number of Best Buy, Circuit City, CompUSA, Mervyns, Kohls, and JC Penney stores), municipality's total revenue, total own-source revenue, total taxes, total spending, and total debt outstanding.

Table A.6: Home Rule Results are Robust to Regional Controls

| **Panel A** | **Total Own-Source Revenue** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Bankrupt Count | -0.0407*** | -0.0552*** | -0.0501*** | -0.0483*** |
| | (0.0081) | (0.0158) | (0.0158) | (0.0097) |
| Krane et al. 2002 Home | 0.0257*** | 0.0301*** | 0.0248*** | 0.0578*** |
| Rule 1 x Bankrupt Count | (0.0089) | (0.0101) | (0.0088) | (0.0103) |
| Bankrupt+(HR x Bankrupt) | -0.0150** | -0.0251* | -0.0253* | 0.0095** |
| | (0.0061) | (0.0136) | (0.0143) | (0.0043) |
| Controls x Bankrupt Count | - | CENSUS REGION | CENSUS DIVISION | STATE |
| Adjusted $R^2$ | 0.987 | 0.987 | 0.988 | 0.988 |
| Observations | 4341 | 4341 | 4341 | 4341 |

| **Panel B** | **Total Own-Source Revenue** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Bankrupt Dummy | -0.1149*** | -0.1535*** | -0.0948** | -0.1038*** |
| | (0.0321) | (0.0439) | (0.0386) | (0.0276) |
| Krane et al. 2002 Home | 0.1015*** | 0.1203*** | 0.0615** | 0.1460*** |
| Rule 1 x Bankrupt Dummy | (0.0354) | (0.0386) | (0.0312) | (0.0305) |
| Bankrupt+(HR x Bankrupt) | -0.0134 | -0.0332 | -0.0334 | 0.0422** |
| | (0.0170) | (0.0277) | (0.0282) | (0.0135) |
| Controls x Bankrupt Dummy | - | CENSUS REGION | CENSUS DIVISION | STATE |
| Adjusted $R^2$ | 0.987 | 0.988 | 0.988 | 0.988 |
| Observations | 4341 | 4341 | 4341 | 4341 |

Note: This table reports estimates of regressions of the following form:

$$ln(OwnSourceRev_{it}) = \alpha + \phi(BankruptCount * Post * HomeRule * RegionalDummy)_{it} +$$
$$\beta(BankruptCount * Post * RegionalDummy)_{it} + \lambda(BankruptCount * RegionalDummy)_{it} +$$
$$\rho(HomeRule * Post)_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $OwnSourceRev_{it}$ is own-source revenue in municipality $i$, in year $t$. $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $HomeRule_i$ is a dummy variable equal to 1 if the municipality has home rule status according to Krane et al. (2002). $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $BankruptCount * RegionalDummy$ controls for the number of bankrupt stores in different census regions or states. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. $Bankrupt + (HR * Bankrupt)$ shows the sum of the coefficient on $BankruptCount$ and the coefficient on the interaction term $HomeRule * BankruptCount$. **Panel B** of this table replaces $BankruptCount_i$ with $BankruptDummy_i$, which equals 1 when municipality $i$ contains one or more of the treatment chains. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.7: Summary Statistics by Municipality Population Category

| | Municipality Population Category | | |
|---|---|---|---|
| | 1,000-5,000 | 5,000-45,000 | 45,000-200,000 |
| **Panel A** | **Municipality Finance Statistics** | | |
| Per Capita Revenue | 1,723.705 | 2,199.596 | 2,760.967 |
| | (2,484.743) | (2,085.808) | (1,123.868) |
| Per Capita Own-Source Revenue | 1,345.909 | 1,781.923 | 2,276.569 |
| | (2,287.647) | (1,994.934) | (1,078.800) |
| Per Capita Local Sales Tax | 5.008 | 42.259 | 112.063 |
| | (52.630) | (120.489) | (78.475) |
| Per Capita Property Tax | 219.788 | 301.350 | 410.851 |
| | (585.384) | (246.540) | (156.184) |
| Per Capita Charges & Fines | 503.115 | 574.656 | 637.184 |
| | (779.569) | (927.799) | (494.625) |
| Per Capita Financial Transactions | 11.003 | 55.182 | 106.967 |
| | (41.129) | (70.010) | (76.563) |
| Home Rule Status | 0.122 | 0.409 | 0.973 |
| | (0.328) | (0.492) | (0.164) |
| Annual Percent Change in Per Capita Revenue | 0.032 | 0.032 | 0.030 |
| | (0.246) | (0.133) | (0.121) |
| Observations | 1797 | 1503 | 146 |
| Unit of Observation | Municipality-Year | Municipality-Year | Municipality-Year |
| **Panel B** | **Revenue Stability Statistics** | | |
| Rev. Fall | 0.199 | 0.098 | 0.089 |
| | (0.263) | (0.143) | (0.117) |
| Per Capita Total Revenue in 2010 | 1,572.405 | 1,985.039 | 2,489.151 |
| | (2,437.439) | (1,538.591) | (982.864) |
| Home Rule Status | 0.127 | 0.415 | 0.970 |
| | (0.334) | (0.493) | (0.174) |
| Rev. Fall > 10 % | 0.558 | 0.302 | 0.273 |
| | (0.497) | (0.460) | i(0.452) |
| Rev. Fall > 30 % | 0.193 | 0.071 | 0.061 |
| | (0.395) | (0.257) | (0.242) |
| Observations | 362 | 311 | 33 |
| Unit of Observation | Municipality | Municipality | Municipality |
| **Panel C** | **Bond Rating Statistics** | | |
| Extremely Strong | 0.062 | 0.168 | 0.113 |
| | (0.242) | (0.374) | (0.318) |
| Very Strong | 0.048 | 0.164 | 0.425 |
| | (0.215) | (0.371) | (0.497) |
| Strong | 0.166 | 0.352 | 0.412 |
| | (0.373) | (0.478) | (0.495) |
| Adequate or Less | 0.097 | 0.192 | 0.075 |
| | (0.296) | (0.394) | (0.265) |
| Missing Bond Rating | 0.697 | 0.212 | 0.000 |
| | (0.461) | (0.409) | (0.000) |

| | | | |
|---|---|---|---|
| Home Rule Status | 0.207 | 0.496 | 0.950 |
| | (0.406) | (0.500) | (0.219) |
| Observations | 145 | 500 | 80 |
| Unit of Observation | Municipality-Year | Municipality-Year | Municipality-Year |

This table reports summary statistics for our sample of municipalities by population category. **Panel A** shows summary statistics variables relating to municipal taxes and revenue at the municipality-year observation level. **Panel B** shows summary statistics for measures of revenue stability at the municipality level. Finally, **Panel C** Shows summary statistics for variables relating to municipal bond ratings at the municipality-year level. It is important to note that observations with missing values for bond ratings were coded as 0 for $ExtremelyStrong$. The probability of a missing value does not change around the cutoffs. For all panels, mean coefficients are presented, with standard deviations in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.8: Home Rule Regression Discontinuity Analysis with Alternate Bandwidths

| | (1) | (2) |
|---|---|---|
| **Panel A** | **First Stage, Municipality has Home Rule.** | |
| Population≥25,000 | 0.594*** | 0.572*** |
| | (0.169) | (0.141) |
| Observations | 79 | 111 |
| **Panel B** | **Largest Percent Fall in Revenue from 2010-2015.** | |
| Municipality has | -0.044 | -0.088* |
| Home Rule | (0.038) | (0.045) |
| Observations | 79 | 111 |
| **Panel C** | **Percent Fall in Revenue Greater Than 10%.** | |
| Municipality has | -0.276 | -0.318** |
| Home Rule | (0.177) | (0.151) |
| Observations | 79 | 111 |
| **Panel D** | **Percent Fall in Revenue Greater Than 30%.** | |
| Municipality has | -0.060 | -0.161* |
| Home Rule | (0.066) | (0.085) |
| Observations | 79 | 111 |
| **Panel E** | **Municipality has Extremely Strong Bond Rating.** | |
| Municipality has | 0.405 | 0.360 |
| Home Rule | (0.309) | (0.276) |
| Observations | 176 | 234 |
| Clusters | 75 | 106 |
| **Pop. Bandwidth** | $\pm 7,500$ | $\pm 10,000$ |

Note: **Panel A** of this table reports estimates of first-stage regressions of the following form:

$$HomeRule_i = \alpha + \beta(Above25000)_i + \theta(Population)_i + \rho(Above25000 * Population)_i + \varepsilon_i$$

where $HomeRule_i$ is a dummy variable equal to 1 if municipality $i$ ever had home rule status between 2010 and 2015. $Above25000_i$ is a dummy variable that equals to 1 if municipality's population exceeded 25,000 and equals to 0 otherwise. $Population$ is the maximum number of population municipality $i$ had sometime between 1994 and 2009. $Above25000 * Population$ is an interaction variable between $Above25000$ and $Population$. This regression establishes a link between the home rule population threshold in Illinois and a city's actual home rule status. Panels B, C, D, and E show the results of regressions with fuzzy regression discontinuity design, using instrumented $HomeRule$ variable to estimate various public-finance-related variables. **Panel B** reports estimates of regressions of the following form:

$$RevFall_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall_i$ is the largest percent fall in revenue from 2010 to 2015 in municipality $i$. **Panel C** reports estimates of regressions of the following form:

$$RevFall10_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall10_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 10% at any point from 2010-2015. **Panel D** reports estimates of regressions of the following form:

$$RevFall30_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $RevFall30_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 30% at any point from 2010-2015. **Panel E** reports estimates of regressions of the following form:

$$StrongBond_i = \alpha + \beta(HomeRule)_i + \rho(Population)_i + \lambda(HomeRule * Population)_i + \varepsilon_i$$

where $StrongBond_i$ is a dummy variable that equals to 1 if municipality $i$ has extremely strong bond rating in IL data (from 1994 to 1996) or in scraped data (2015).

Regressions from all five panels are run with two different population bandwidths. Column 1 includes cities with populations between 17,500 and 32,500, and column 2 includes cities with populations between 15,000 and 35,000. Standard errors clustered at the municipality level are in parentheses.

$* \; p < 0.1$, $** \; p < 0.05$, $*** \; p < 0.01$

Table A.9: Polynomial Robustness Checks for Home Rule Regression Discontinuity Analysis

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| **Panel A** | **First Stage, Municipality has Home Rule.** | | | |
| Population≥25,000 | 0.575*** | 0.588*** | 0.605*** | 0.573*** |
| | (0.188) | (0.168) | (0.148) | (0.140) |
| Observations | 148 | 183 | 259 | 314 |
| **Panel B** | **Largest Percent Fall in Revenue from 2010-2015.** | | | |
| Municipality has | -0.038 | -0.089* | -0.089* | -0.085 |
| Home Rule | (0.042) | (0.047) | (0.048) | (0.053) |
| Observations | 148 | 183 | 257 | 312 |
| **Panel C** | **Percent Fall in Revenue Greater Than 10%.** | | | |
| Municipality has | -0.319 | -0.356* | -0.331** | -0.332** |
| Home Rule | (0.212) | (0.183) | (0.159) | (0.161) |
| Observations | 148 | 183 | 258 | 313 |
| **Panel D** | **Percent Fall in Revenue Greater Than 30%.** | | | |
| Municipality has | -0.039 | -0.141* | -0.160* | -0.160 |
| Home Rule | (0.071) | (0.085) | (0.092) | (0.102) |
| Observations | 148 | 183 | 258 | 313 |
| **Panel E** | **Municipality has Extremely Strong Bond Rating.** | | | |
| Municipality has | 0.419 | 0.366 | 0.325 | 0.344 |
| Home Rule | (0.327) | (0.313) | (0.289) | (0.293) |
| Observations | 283 | 331 | 434 | 498 |
| Clusters | 135 | 162 | 218 | 257 |
| **Pop. Bandwidth** | $\pm 12,500$ | $\pm 15,000$ | $\pm 18,000$ | $\pm 20,000$ |

Note: **Panel A** of this table reports estimates of first-stage regressions of the following form:

$$HomeRule_i = \alpha + \beta(Above25000)_i + \theta(Population)_i + \delta(Population^2)_i + \gamma(Above25000 * Population)_i + \pi(Above25000 * Population^2)_i + \varepsilon_i$$

where $HomeRule_i$ is a dummy variable equal to 1 if municipality $i$ ever had home rule status between 2010 and 2015. $Above25000_i$ is a dummy variable that equals to 1 if municipality's population exceeded 25,000 and equals to 0 otherwise. $Population$ is the maximum number of population municipality $i$ had sometime between 1994 and 2009. $Above25000 * Population$ is an interaction variable between $Above25000$ and $Population$. This regression establishes a link between the home rule population threshold in Illinois and a city's actual home rule status. Panels B, C, D, and E show the results of regressions with fuzzy regression discontinuity design, using instrumented $HomeRule$ variable to estimate various public-finance-related variables. **Panel B** reports estimates of regressions of the following form:

$$RevFall_i = \alpha + \beta(HomeRule)_i + \theta(Population)_i + \delta(Population^2)_i + \gamma(HomeRule * Population)_i + \pi(HomeRule * Population^2)_i + \varepsilon_i$$

where $RevFall_i$ is the largest percent fall in revenue from 2010 to 2015 in municipality $i$. **Panel C** reports estimates of regressions of the following form:

$$RevFall10_i = \alpha + \beta(HomeRule)_i + \theta(Population)_i + \delta(Population^2)_i + \gamma(HomeRule * Population)_i + \pi(HomeRule * Population^2)_i + \varepsilon_i$$

where $RevFall10_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 10% at any point from 2010-2015. **Panel D** reports estimates of regressions of the following form:

$$RevFall30_i = \alpha + \beta(HomeRule)_i + \theta(Population)_i + \delta(Population^2)_i + \gamma(HomeRule * Population)_i + \pi(HomeRule * Population^2)_i + \varepsilon_i$$

where $RevFall30_i$ is a dummy variable that equals to 1 if municipality $i$ has a fall in revenue larger than 30% at any point from 2010-2015. **Panel E** reports estimates of regressions of the following form:

$$StrongBond_i = \alpha + \beta(HomeRule)_i + \theta(Population)_i + \delta(Population^2)_i + \gamma(HomeRule * Population)_i + \pi(HomeRule * Population^2)_i + \varepsilon_i$$

where $StrongBond_i$ is a dummy variable that equals to 1 if municipality $i$ has extremely strong bond rating in IL data (from 1994 to 1996) or in scraped data (2015).

Regressions from all five panels are run with four different population bandwidths. Column 1 includes cities with populations between 12,500 and 37,500; column 2 includes cities with populations between 10,000 and 40,000; column 3 includes cities with populations between 7,000 and 43,000; and column 4 includes cities with populations between 5,000 and 45,000. Standard errors clustered at the municipality level are in parentheses.

$* \, p < 0.1$, $** \, p < 0.05$, $*** \, p < 0.01$

Table A.10: Effect of Home Rule and Chain Bankruptcy on House Prices

| | House Price | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Bankrupt Count | -0.0478*** | -0.0262 | -0.0166 | -0.0362** | -0.0465** |
| | (0.0171) | (0.0175) | (0.0148) | (0.0153) | (0.0223) |
| Krane et al. Home Rule | 0.0349* | 0.0258 | 0.0244 | 0.0440** | 0.0403 |
| 1 x Bankrupt Count | (0.0184) | (0.0194) | (0.0152) | (0.0197) | (0.0246) |
| Constant | 12.2360*** | 12.2290*** | 12.2223*** | 11.8858*** | 12.3009*** |
| | (0.0124) | (0.0116) | (0.0108) | (0.0164) | (0.0134) |
| Region x Year Fixed | CENSUS | CENSUS | STATE | - | - |
| Effects | REGION | DIVISION | | | |
| Removing States | - | - | - | AZ, NV, FL, | ND, NE, IA, |
| | | | | MI, CA | OK, SD |
| Observations | 2981 | 3132 | 3160 | 1813 | 2875 |
| Adjusted $R^2$ | 0.933 | 0.936 | 0.937 | 0.864 | 0.900 |

This table reports estimates of regressions in the following form:

$$ln(HousePrice_{it}) = \alpha + \phi(BankruptCount * Post * HomeRuleKrane)_{it} + \beta(BankruptCount * Post)_{it} + \rho(HomeRuleKrane * Post)_{it} + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $HomeRuleKrane_i$ is a dummy variable equal to 1 if the municipality has home rule status according to Krane et al. (2002). $BankruptCount_i$ is the total number of bankrupt stores in municipality $i$. $OperatingCount_i$ is the total number of operating stores in municipality $i$ after 2008. $Post_t$ is an indicator for whether or not year $t$ is after 2008 (the bankruptcy year). $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. Column 1 includes Census region by year fixed effects, excluding the Northeast region, which only includes five cities. Column 2 includes Census division by year fixed effects, excluding the Northeast region, which only includes five cities. Column 3 includes state by year fixed effects. Column 4 omits states with housing markets that had the largest peak-to-trough falls, and Column 5 omits states with housing markets that had the smallest peak-to-trough falls (information on peak-to-trough from: https://www.corelogic.com/downloadabledocs/corelogic-peak-totrough-final-030118.pdf). $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. Standard errors clustered at the county level are in parentheses. Municipalities are excluded if they have over 50 big-box stores, have a 500% change between their maximum total revenue (or sales tax or total expenditure) and their minimum total revenue (or sales tax or total expenditure), or if they are in the data for less than 5 years.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.11: Sales Tax and Gross Receipts Robustness Checks for Different Operating Store Controls

| **Panel A** | | **Sales Tax and Gross Receipts** | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Bankrupt Count | -0.0307*** | -0.0418*** | -0.0552*** | -0.0308*** |
| | (0.0088) | (0.0118) | (0.0196) | (0.0087) |
| Constant | 9.8210*** | 9.8212*** | 9.8212*** | 9.8211*** |
| | (0.0260) | (0.0257) | (0.0257) | (0.0259) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| Adjusted $R^2$ | 0.947 | 0.947 | 0.947 | 0.946 |
| Observations | 4346 | 4346 | 4346 | 4346 |
| **Panel B** | | **Sales Tax and Gross Receipts** | | |
| | (1) | (2) | (3) | (4) |
| Bankrupt Dummy | -0.1575*** | -0.1494*** | -0.1465*** | -0.1467*** |
| | (0.0383) | (0.0421) | (0.0456) | (0.0386) |
| Constant | 9.8201*** | 9.8206*** | 9.8205*** | 9.8205*** |
| | (0.0254) | (0.0252) | (0.0253) | (0.0252) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| Adjusted $R^2$ | 0.947 | 0.947 | 0.947 | 0.947 |
| Observations | 4346 | 4346 | 4346 | 4346 |

Note: This table reports estimates of regressions of the following form:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptCount * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $Revenue_{it}$ is total sales tax and gross receipt revenue for municipality $i$, in year $t$; $BankruptCount_i$ in **Panel A** equals the number of bankruptcies of treatment chains in municipality $i$. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post$ controls for whether or not municipality $i$ contains any operating stores in the treatment category after the bankruptcy year. In Column 2 $OperatingDummy$ is replaced by $OperatingCount$. Similarly, in Columns 3 and 4 $BigBoxCount$ and no controls are used instead of $OperatingDummy$, respectively. **Panel B** follows the below specification:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

Where $BankruptDummy_i$ is substituted for $BankruptCount_i$ in **Panel A** and equals 1 when municipality $i$ contains one or more of the treatment chains. In both panels, $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. **Panel B** of this table replaces $BankruptDummy_i$ with $BankruptCount_i$, which is the number of bankrupt chain stores in municipality $i$. Similarly to panels A, Columns 1-4 show different operating specifications and no control. Standard errors are in parentheses.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.12: Own-Source Revenue Robustness Checks for Different Operating Store Controls

| **Panel A** | **Own-Source Revenue** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Bankrupt Count | -0.0182*** | -0.0191*** | -0.0203** | -0.0182*** |
| | (0.0040) | (0.0058) | (0.0096) | (0.0040) |
| Constant | 11.3663*** | 11.3664*** | 11.3664*** | 11.3663*** |
| | (0.0070) | (0.0070) | (0.0070) | (0.0070) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| | 0.988 | 0.988 | 0.988 | 0.988 |
| | 4350 | 4350 | 4350 | 4350 |
| **Panel B** | **Own-Source Revenue** | | | |
| | (1) | (2) | (3) | (4) |
| Bankrupt Dummy | -0.0499*** | -0.0368** | -0.0247 | -0.0447*** |
| | (0.0142) | (0.0160) | (0.0177) | (0.0145) |
| Constant | 11.3665*** | 11.3665*** | 11.3663*** | 11.3667*** |
| | (0.0071) | (0.0071) | (0.0071) | (0.0071) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| Adjusted $R^2$ | 0.988 | 0.988 | 0.988 | 0.988 |
| Observations | 4350 | 4350 | 4350 | 4350 |

Note: This table reports estimates of regressions of the following form:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptCount * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $Revenue_{it}$ is total own-source revenue for municipality $i$, in year $t$; $BankruptCount_i$ in **Panels A** equals the number of bankruptcies of treatment chains in municipality $i$. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post$ controls for whether or not municipality $i$ contains any operating stores in the treatment category after the bankruptcy year. In Column 2 $OperatingDummy$ is replaced by $OperatingCount$. Similarly, in Columns 3 and 4 $BigBoxCount$ and no controls are used instead of $OperatingDummy$, respectively. **Panels B** follows the below specification:

$$ln(Revenue_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

Where $BankruptDummy_i$ is substituted for $BankruptCount_i$ in **Panels A** and equals 1 when municipality $i$ contains one or more of the treatment chains. equals 1 when municipality $i$ contains one or more of the treatment chains. In both panels, $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. Similarly to panels A, Columns 1-4 show different operating specifications and no control. Standard errors are in parentheses.

$* \ p < 0.1$, $** \ p < 0.05$, $*** \ p < 0.01$

Table A.13: Total Expenditure Robustness Checks for Different Operating Store Controls

| **Panel A** | **Total Expenditure** | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Bankrupt Count | -0.0145*** | -0.0170*** | -0.0201* | -0.0200* |
| | (0.0040) | (0.0065) | (0.0111) | (0.0111) |
| Constant | 11.5216*** | 11.5218*** | 11.5218*** | 11.8138*** |
| | (0.0091) | (0.0091) | (0.0091) | (0.0086) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| | 0.986 | 0.986 | 0.986 | 0.986 |
| | 4350 | 4350 | 4350 | 4350 |
| **Panel B** | **Total Expenditure** | | | |
| | (1) | (2) | (3) | (4) |
| Bankrupt Dummy | -0.0336** | -0.0228 | -0.0119 | -0.0282* |
| | (0.0164) | (0.0170) | (0.0176) | (0.0164) |
| Constant | 11.5219*** | 11.5220*** | 11.5218*** | 11.5221*** |
| | (0.0091) | (0.0091) | (0.0091) | (0.0091) |
| Operating Control | Operating Dummy x Post | Operating Count x Post | Big Box Count x Post | No Control |
| Adjusted $R^2$ | 0.986 | 0.986 | 0.986 | 0.986 |
| Observations | 4350 | 4350 | 4350 | 4350 |

Note: This table reports estimates of regressions of the following form:

$$ln(Expenditure_{it}) = \alpha + \beta(BankruptCount * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it}$$

where $Expenditure_{it}$ is the total expenditures for municipality $i$, in year $t$; $BankruptCount_i$ in **Panels A** equals the number of bankruptcies of treatment chains in municipality $i$. $Post_t$ is an indicator for whether or not the year is after 2008, the bankruptcy year. $OperatingDummy * Post$ controls for whether or not municipality $i$ contains any operating stores in the treatment category after the bankruptcy year. In Column 2 $OperatingDummy$ is replaced by $OperatingCount$. Similarly, in Columns 3 and 4 $BigBoxCount$ and no controls are used instead of $OperatingDummy$, respectively. **Panels B** follows the below specification:

$$ln(Expenditure_{it}) = \alpha + \beta(BankruptDummy * Post)_{it} + \theta(OperatingDummy * Post)_{it} + \delta_i\gamma_t + \varepsilon_{it}$$

Where $BankruptDummy_i$ is substituted for $BankruptCount_i$ in **Panels A** and equals 1 when municipality $i$ contains one or more of the treatment chains. In both panels, $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents year fixed effects. Similarly to panels A, Columns 1-4 show different operating specifications and no control. $\delta_i$ represents municipality fixed effects and $\gamma_t$ represents time fixed effects. Standard errors are in parentheses.

$* \, p < 0.1$, $** \, p < 0.05$, $*** \, p < 0.01$