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Rules versus Home Rule

Local Government Responses to Negative Revenue Shocks*

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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 revenue shortfalls. 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 react more swiftly by raising taxes or issuing bonds. A regression discontinuity analysis of cities in Illinois emphasizes that this effect of local autonomy is causal. Home rule cities do not abuse their discretion: their bond ratings are more likely to be strong.

Keywords: Home Rule, Local Government, Municipal Debt, Taxation, Retail, Revenue, Spending

JEL: E6, H1, H7, K2, R5

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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. However, even at business cycle frequencies local sales taxes are a volatile source of revenue. 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 order to effectively design local government policy in this context, it is crucial to understand how governments respond to revenue shocks and why some cities fare better than others in the face of such shocks.

In this paper, we use national big-box chain bankruptcies as natural experiments that allow us to answer these questions. 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 (2016) 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 recover less quickly than cities with more discretionary authority, without benefiting even 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.

The paper 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. Section 2 introduces the data we use. After that, in section 3, we assess the size and persistence of the revenue shocks produced by big-box store bankruptcies, while the focus of section 4 is how cities respond to these shocks. How those

responses vary with home rule status is discussed in section 5, while we establish the causal link from home rule to differences in outcomes in section 6. We conclude in section 7.

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. Their financing has not received as much attention, although there is a sizable literature that explores how local governments use intergovernmental grants, that is, how they respond to positive revenue shocks. In general, most research finds that money "sticks where it hits", a phenomenon commonly referred to as the flypaper effect (Gramlich, 1977; Fisher, 1982; Hines and Thaler, 1995).

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. Similarly, Baicker (2004) finds that counties respond to sudden spending increases triggered by a capital crime conviction by raising taxes and cutting expenditures, specifically and in flypaper-type fashion, on public safety.

Negative revenue shocks are not as well understood, despite their importance in the recent past (Lutz et al., 2011). 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 (measured by the difference between actual and forecast 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.

More directly related to the type of revenue shock we study 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 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.

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 (Vanlangdingham, 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

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. We attempt to fill this void in two ways. First, by tying local-government responses to negative shocks to local government home rule status, we find that the leeway afforded by home rule helps cities adjust and recover from shocks swiftly. Second, we examine the causal effect of home rule in a steady state environment by exploiting a discontinuity in a population rule in Illinois that determines home rule status. In this analysis, we find that home rule cities have better bond ratings and a greater degree of financial stability.

Most work in this area has come in the form of case studies at the city or state level. Carroll and Johnson

¹We discuss home rule as a legal and institutional construct in more detail in section 5.

(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 to identify causal responses. We find that cities with home rule recover more rapidly from negative shocks to revenue by drawing from a broader range of revenue sources.

This finding illustrates 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 flexibility 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 local Leviathans in the long run. Kuziemko (2013), however, suggests that the benefits of discretion may outweigh the consequences in her study of parole versus fixed-sentence release schemes for prisoners. She demonstrates that parole officers, in their discretion, set prison time efficiently. This prevents low-risk inmates from spending unnecessary time in prison. Thus, when parole boards are given discretion to set sentence length, the state saves money and ultimately, recidivism falls. The existing empirical evidence regarding these concerns 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 occurrence 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 confirm, using a regression discontinuity design, that concerns about the long-run budget outlook of home rule cities are unfounded: cities with home rule pay lower interest rates than cities without it, demonstrating the power of local control.

2 Data

We ask two basic questions in this paper. First, how do local governments respond to negative revenue shocks? Second, how is that response shaped by the institutional environment? To answer these questions, we use data on big-box stores, on local government revenue, spending, and balance sheets, as well as on local governance.

2.1 Big-Box Stores

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, and Mervyn's stores in each municipality in the U.S. between 2004 and 2012. 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).

2.2 Local Government Revenue and Spending

To analyze changes in local government revenue and expenditures, 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.²³

2.3 Municipality-Level House Prices

In addition to data on local government revenue and spending, we also gather information on local house price from Zillow. First, we obtain the median price of sold houses in each month and in each zip code. Then, the zip code level data is converted to municipality-level data, and we take the mean of the monthly data to get an annual measure.

2.4 Local Government Structure

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

²Appendix Table 7 shows that this does not affect our results.

³In 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%. We drop one city with a population that appears to be miscoded, 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, cities with more sales tax revenue than total revenue, and cities that had zero sales tax revenue during 2004-2007.

these sources to construct four distinct measures of home rule: two at the state level, and two at the city level. Note that our results are quite consistent regardless of when home rule status is measured, as it does not change tremendously often.

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.” ⁴

⁴Table A.1 presents the summary statistics of both home rule and non-home rule cities.

2.5 Home Rule and Financial Data from Illinois

Illinois Comptroller's financial databases provide data on population and revenues of municipalities in Illinois. Bond ratings for years 1994-1996 at the municipality-level are also available in the same databases, and the more recent bond ratings were obtained by scrapping information from Municipal-Bonds.com. We use these data to examine the causal effect of home rule status on a municipality's revenue stability.

3 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 (2016). 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 we 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; that the neighborhoods look similar when we compare characteristics ranging from racial composition to access to public transit; and that a matching approach produces results that are quite similar to the approach followed here.

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 financial characteristics. The financial characteristics tested

include debt outstanding; debt retired; house prices; property tax, charges & fees, and miscellaneous revenue; own-source revenue; state intergovernmental revenue; expenditures; and sales tax revenue. The bankrupt variables (i.e. $BankruptDummy_i$ and $BankruptCount_i$) are not statistically different from zero in the regressions shown in Table 1, 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. Finally, we present two maps of operating and defunct store locations in Figures A.1 and A.2.

There is no reason to suspect that any local trend breaks would be correlated with national chain bankruptcies, and these locally exogenous, national-level bankruptcies therefore allow us to identify the effects of a negative revenue shock on local government finances. We employ a difference-in-difference design, comparing cities that were home to a bankrupt chain to cities that were home to a comparable surviving chain before and after the bankruptcies in 2008. Specifically, we run regressions of the following kind:

$$\ln(Revenue_{it}^h) = \alpha + \beta(BankruptDummy * Post)_{it}^c + \theta(OperatingDummy * Post)_{it}^c + \delta_i + \gamma_t + \varepsilon_{it} \quad (1)$$

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). In addition to the three aforementioned categories, we also create another pooled category by using Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. $Post_t$ 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 $Post_t$. Finally, δ_i represents municipality fixed effects and γ_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.

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 15% of local sales tax revenue and gross receipt revenue, depending on the chain type under study. While a single bankruptcy, even the bankruptcy of a big-box retailer, is unlikely to cause such a large decline, Shoag and Veuger (2016) 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 with every big-box store going bankrupt, a municipality's total sales tax and gross receipt revenue will go down by about 1.6% to 5.6%. 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 3.4% and 5.8%. Panel D paints a similar picture; for every big-box store going bankrupt, a municipality's own-source revenue will go down by about 1.5% to 3%.⁵

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 three years after. Panel A from Table A.2 shows that the shocks to sales tax revenue (i.e. the reduction of total sales tax and gross receipt revenue of about 5% to 12%) persisted even three years after the bankruptcy, perhaps because municipalities struggled to fill empty store fronts, or because customers switched to online shopping permanently. In

⁵In Table A.6, 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.

fact, the effect of bankruptcy becomes 1% to 3% more severe from year 1 to year 3 after the bankruptcies. Interestingly, Panel B from Table A.2 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.

4 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 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:

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

where $\text{Expenditure}_{it}^h$ 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).⁶ 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 own-source revenue immediately after the bankruptcy year, as implied by Panels C and D from Table 2. However, when examining the more specific components of municipalities' own-source 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 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. Both panels mask heterogeneity by city home rule status. We explore this in Section 5 and find cities with greater autonomy have large and statistically significant increases in property tax and charges & fees revenue. We now turn to an analysis of how the broader institutional environment affects these responses.

⁶In Table A.6, 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.

5 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.⁷ If neither the state constitution nor the state law provides a clear answer, state courts decide.⁸ 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 en-

⁷This paragraph and the following three rely heavily on Richardson et al. (2003), and quotations originate there.

⁸A 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.

joyed 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. As discussed in the data section, we will use a number of different measures of home rule to categorize these various combination.

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

$$\begin{aligned} \ln(Revenue_{it}^h) = & \alpha + \phi(BankruptCount * Post * HomeRule)_{it}^r + \beta(BankruptCount * Post)_{it} \\ & + \rho(HomeRule * Post)_{it}^r + \theta(OperatingCount * Post)_{it} + \delta_i + \gamma_t + \varepsilon_{it} \end{aligned} \quad (3)$$

where $Revenue_{it}^h$ 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 $HomeRule_i^r$ 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 introduced in the data section. 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 ($\theta + \beta$) 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 (β) 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. There is reason, however, to believe that it 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 from Panel A of Table 5 show that out of the municipalities that experience bankruptcies, municipalities with home rule status experience reduction that is 60% less severe than their counterparts in terms of own-source revenue. Furthermore, columns 2, 3, and 4 from Panel B of Table 5 show that having home rule status mitigates the negative effect the big-box bankruptcy has on the own-source revenue.

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 the impact of big-box bankruptcy

on total expenditure is the same for home rule cities and non-home rule cities: the differential response is limited to categories directly affected by home rule.⁹

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.5. 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.

6 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. 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

⁹Note that for additional robustness checks, we re-run regressions from Tables 4, 5, 6, and 7 but with Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. The results are shown in Table A.3 and Table A.4.

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). This is therefore not necessarily the result of strategic manipulation à la Eggers (2015). We also create Figure 3 to 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 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.

With those considerations in mind, we use whether or not a city is above the population cutoff as an instrument for whether or not the city has home rule. 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 $\pm 12,500$ to $\pm 20,000$), and in all cases, the results are qualitatively the same. Specifically, we estimate the following first stage regression on the sample of municipalities near the discontinuity:

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

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:

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

where $RevFall_i$ is the largest percent fall in revenue from 2010 to 2015 in municipality i . 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 volatile 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. These regression results show municipalities with home rule are about 35% more likely to have an extremely strong bond rating.¹⁰

7 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. In addition, we demonstrate that municipalities with less discretionary decision-making experience a sharper drop in revenue and recover less quickly than municipalities with more discretionary authority.

The downside of autonomy typically considered in the literature on rules versus discretion is a lack of

¹⁰Figures 5a, 5b, and 6 are graphical representations of the regression results from Panels B, C, and E, respectively, from Table 8.

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 succumbing to temptations in their bond ratings, 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.

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 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.

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

Panel A	(1) '05-'07 Debt Outstanding	(2) '05-'07 Debt Retired	(3) '05-'07 House Prices	(4) '05-'07 Property Tax, Charges & Fees, Misc. Rev.
Bankrupt Dummy	0.0422 (0.0361)	0.0321 (0.0883)	-0.0163 (0.0141)	0.0266 (0.0231)
Observations	448	446	322	450
	(5) '05-'07 Own-Source Revenue	(6) '05-'07 State Intergov. Revenue	(7) '05-'07 Expenditures	(8) '05-'07 Sales Tax Revenue
Bankrupt Dummy	0.0235 (0.0145)	-0.0477 (0.1710)	0.0213 (0.0176)	-0.0283 (0.0517)
Observations	450	442	450	423
Panel B	(1) '05-'07 Debt Outstanding	(2) '05-'07 Debt Retired	(3) '05-'07 House Prices	(4) '05-'07 Property Tax, Charges & Fees, Misc. Rev.
Bankrupt Count	0.0005 (0.0068)	0.0276 (0.0242)	-0.0016 (0.0032)	-0.0025 (0.0057)
Observations	448	446	322	450
	(5) '05-'07 Own-Source Revenue	(6) '05-'07 State Intergov. Revenue	(7) '05-'07 Expenditures	(8) '05-'07 Sales Tax Revenue
Bankrupt Count	0.0017 (0.0031)	0.0180 (0.0429)	-0.0030 (0.0052)	-0.0112 (0.0094)
Observations	450	442	450	423

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

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

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:

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

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: Matching	(5) Pooled: State by Year FEs
Panel A Total Sales Tax and Gross Receipt Revenue					
Bankrupt Dummy	-0.1542*** (0.0467)	-0.1252*** (0.0338)	-0.1575*** (0.0383)	-0.1327*** (0.0376)	-0.0981*** (0.0291)
Adjusted R^2	0.939	0.970	0.947	0.929	0.969
Panel B Total Sales Tax and Gross Receipt Revenue					
Bankrupt Count	-0.0433*** (0.0144)	-0.0405*** (0.0151)	-0.0307*** (0.0088)	-0.0564*** (0.0136)	-0.0167** (0.0054)
Adjusted R^2	0.939	0.970	0.947	0.929	0.969
Observations	3473	2767	4346	3802	4346
Panel C Own-Source Revenue					
Bankrupt Dummy	-0.0423*** (0.0153)	-0.0584*** (0.0164)	-0.0499*** (0.0142)	-0.0341** (0.0144)	-0.0405** (0.0174)
Adjusted R^2	0.986	0.989	0.988	0.986	0.989
Panel D Own-Source Revenue					
Bankrupt Count	-0.0258*** (0.0057)	-0.0302*** (0.0088)	-0.0186*** (0.0040)	-0.0159*** (0.0052)	-0.0180*** (0.0054)
Adjusted R^2	0.986	0.989	0.988	0.986	0.989
Observations	3477	2768	4350	3806	4350

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

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

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, 4, and 5 pool both electronics and department stores. Post_t is an indicator for whether or not the year is after 2008, the bankruptcy year. $\text{OperatingDummy} * \text{Post}$ controls for whether or not municipality i contains any operating stores in category c after the bankruptcy year. δ_i represents municipality fixed effects and γ_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: Matching" uses Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. Column 5 "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 Ad- ministration	(5) Total Debt Outstanding	(6) Cash Securities
Panel A						
Bankrupt Dummy	-0.0336** (0.0164)	-0.0266* (0.0148)	-0.0274 (0.0586)	-0.1016* (0.0532)	-0.0715 (0.0455)	-0.0717* (0.0389)
Adjusted R^2	0.986	0.984	0.828	0.827	0.961	0.955
Panel B						
Bankrupt Count	-0.0145*** (0.0040)	-0.0076*** (0.0029)	-0.0297** (0.0134)	-0.0324** (0.0144)	-0.0125 (0.0094)	-0.0208** (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. Trans- actions
Panel C						
Bankrupt Dummy	0.0289 (0.0417)	0.0061 (0.0256)	0.1346* (0.0716)	0.0931*** (0.0324)	0.0296 (0.0198)	0.0319 (0.0207)
Adjusted R^2	0.965	0.974	0.857	0.966	0.985	0.983
Panel D						
Bankrupt Count	-0.0038 (0.0089)	0.0025 (0.0057)	0.0196 (0.0183)	0.0046 (0.0083)	0.0020 (0.0045)	0.0021 (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:

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

where $\text{Expenditure}_{it}^h$ 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. $\text{OperatingDummy}_i * \text{Post}_t$ controls for whether or not municipality i contains any operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_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 $\text{Expenditure}_{it}^h$ with Revenue_{it}^h , 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*** (0.0118)	-0.0668** (0.0276)	-0.0741** (0.0345)	-0.0732** (0.0343)	-0.0762** (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: Bankrupt+(HR x Bankrupt)		-0.0344*** (0.0116)	-0.0344*** (0.0113)	-0.0347*** (0.0113)	-0.0213* (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*** (0.0421)	-0.1325 (0.0907)	-0.1415 (0.1331)	-0.1546 (0.1327)	-0.2077* (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: Bankrupt+(HR x Bankrupt)		-0.1471*** (0.0404)	-0.1360*** (0.0358)	-0.1335*** (0.0355)	-0.1095** (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:

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

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_i^r 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 introduced in the data section. $\text{OperatingCount} * \text{Post}$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on BankruptCount and the coefficient on the interaction term $\text{HomeRule} * \text{BankruptCount}$. 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*** (0.0058)	-0.0369*** (0.0074)	-0.0407*** (0.0081)	-0.0401*** (0.0081)	-0.0312*** (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** (0.0063)	-0.0150** (0.0061)	-0.0152** (0.0061)	-0.0105 (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** (0.0160)	-0.0721*** (0.0265)	-0.1149*** (0.0321)	-0.1177*** (0.0322)	-0.0561** (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 (0.0185)	-0.0134 (0.0170)	-0.0128 (0.0169)	-0.0075 (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:

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

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^r 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 introduced in the data section. $\text{OperatingCount} * \text{Post}$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on BankruptCount and the coefficient on the interaction term $\text{HomeRule} * \text{BankruptCount}$. 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 (0.0065)	-0.0068 (0.0160)	-0.0232** (0.0117)	-0.0236** (0.0116)	-0.0044 (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 (0.0059)	0.0110* (0.0066)	0.0112* (0.0067)	0.0134 (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* (0.0205)	-0.0108 (0.0369)	-0.0686** (0.0346)	-0.0613* (0.0352)	0.0067 (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*** (0.0238)	0.0696*** (0.0229)	0.0683*** (0.0232)	0.0705** (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:

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

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). $HomeRule_i^r$ 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 introduced in the data section. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount$ and the coefficient on the interaction term $HomeRule * BankruptCount$. 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*** (0.0065)	-0.0274*** (0.0093)	-0.0297*** (0.0107)	-0.0299*** (0.0106)	-0.0182* (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: Bankrupt+(HR x Bankrupt)		-0.0145** (0.0072)	-0.0142** (0.0071)	-0.0142** (0.0071)	-0.0170** (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 (0.0170)	-0.0495* (0.0266)	-0.0441 (0.0342)	-0.0458 (0.0338)	-0.0379 (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: Bankrupt+(HR x Bankrupt)		-0.0102 (0.0217)	-0.0131 (0.0194)	-0.0127 (0.0194)	-0.0004 (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:

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

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_i^r 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 introduced in the data section. $\text{OperatingCount} * \text{Post}$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on BankruptCount and the coefficient on the interaction term $\text{HomeRule} * \text{BankruptCount}$. 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 \geq 25,000	0.591*** (0.116)	0.592*** (0.106)	0.578*** (0.096)	0.594*** (0.090)
Observations	148	183	259	314
Panel B. Largest Percent Fall in Revenue from 2010-2015.				
Municipality has Home Rule	-0.100** (0.048)	-0.081* (0.048)	-0.081* (0.047)	-0.085* (0.044)
Observations	148	183	257	312
Panel C. Percent Fall in Revenue Greater Than 10%.				
Municipality has Home Rule	-0.293** (0.136)	-0.228* (0.131)	-0.197 (0.129)	-0.177 (0.124)
Observations	148	183	258	313
Panel D. Percent Fall in Revenue Greater Than 30%.				
Municipality has Home Rule	-0.198** (0.092)	-0.182** (0.090)	-0.185** (0.086)	-0.192** (0.081)
Observations	148	183	258	313
Panel E. Municipality has Extremely Strong Bond Rating.				
Municipality has Home Rule	0.328 (0.253)	0.330 (0.231)	0.353* (0.210)	0.346* (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 Benefits are Capitalized in Housing Price

	House Price			
	(1)	(2)	(3)	(4)
Bankrupt Dummy	-0.1130*** (0.0358)			
Bankrupt Count		-0.0212*** (0.0080)		-0.0463*** (0.0166)
One Year After Bankruptcy			-0.0835* (0.0438)	
Two Years After Bankruptcy			-0.0753 (0.0458)	
Three Years After Bankruptcy			-0.0826** (0.0396)	
Krane et al. Home Rule 1 x Bankrupt Count				0.0343* (0.0185)
Adjusted R ²	0.903	0.902	0.902	0.906
Observations	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** reports estimates of regressions of the following form:

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

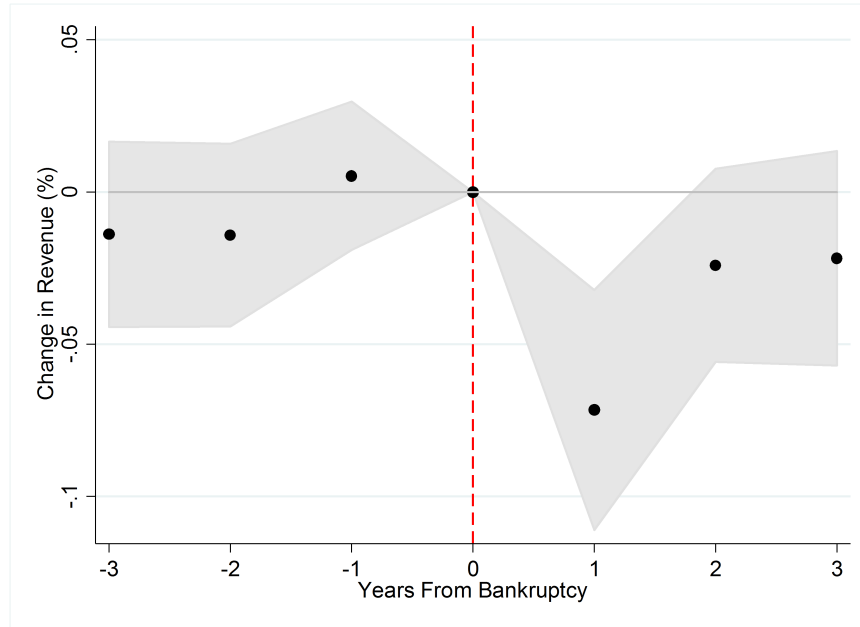
where $YearDummy_t=1$ represents dummy variables for each of the three years after the bankruptcy. $OperatingDummy * YearDummy$ controls for whether or not municipality i contains any operating stores of interest during the corresponding year. **Column 4** reports estimates of regressions in the following form:

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

where $HomeRuleKrone_i$ 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. δ_i represents municipality fixed effects and γ_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.

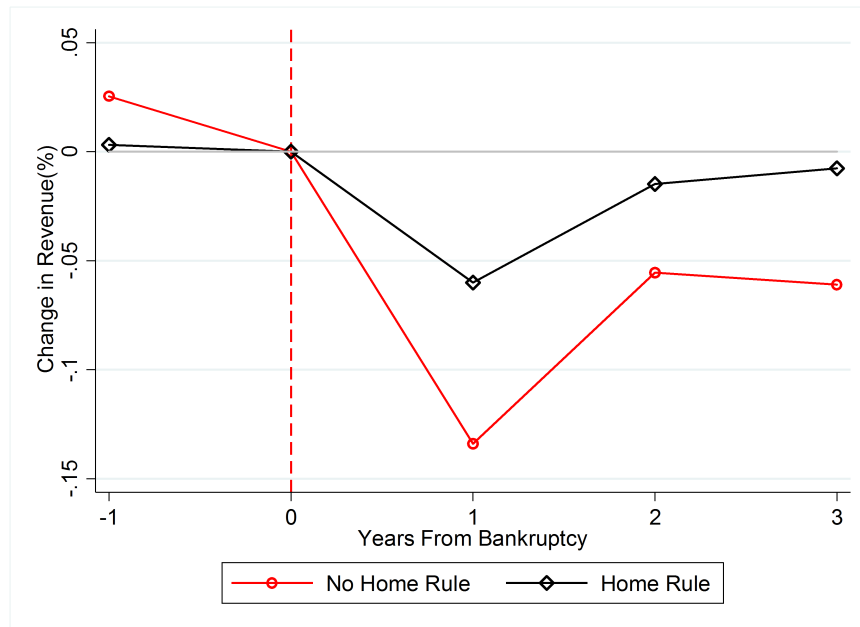
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1a: Own-Source Revenue Event Study from 2005-2011



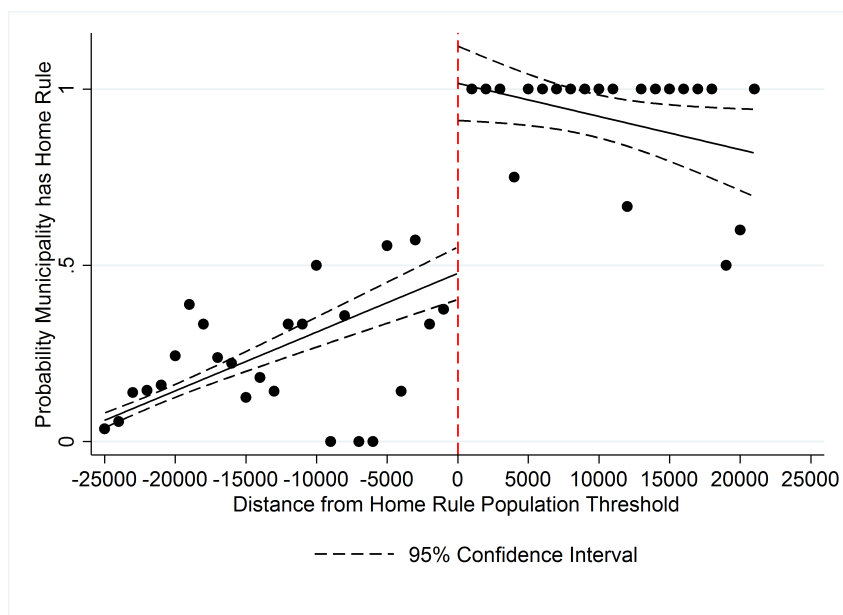
Note: The figure plots coefficients and confidence intervals from following regression: $\ln(\text{OwnSourceRev}_{it}) = \alpha + \beta(\text{BankruptDummy} * \text{YearDummy})_{it}^c + \delta_i + \gamma_t + \varepsilon_{it}$ where δ_i represents municipality fixed effects and γ_t represents time fixed effects.

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



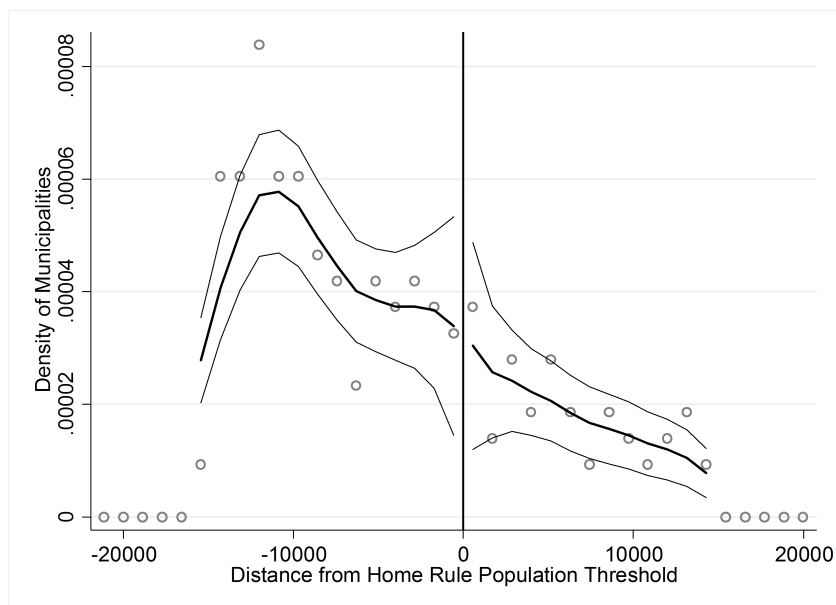
Note: The figure plots coefficients from the following regression : $\ln(\text{OwnSourceRev}_{it}) = \alpha + \beta(\text{BankruptDummy} * \text{YearDummy})_{it}^c + \delta_i + \gamma_t + \varepsilon_{it}$ where δ_i represents municipality fixed effects and γ_t represents time fixed effects, with two different sets of data. One set only includes home rule municipalities, and the other set only includes non-home rule municipalities.

Figure 2: Home Rule First Stage



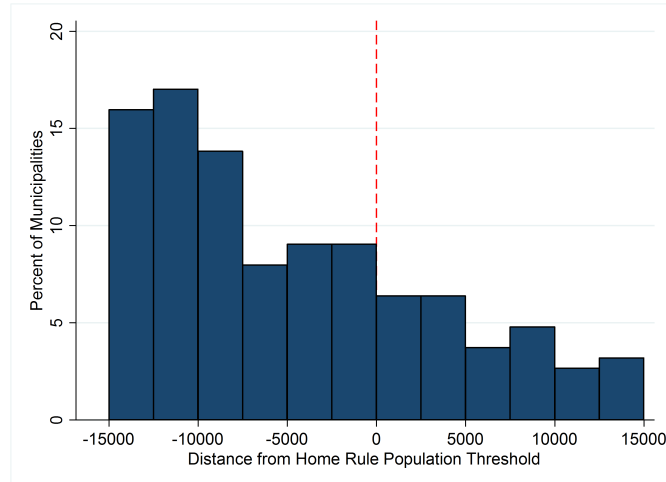
Note: The figure plots linear fit from two regressions of home rule (=1 if IL data indicates the city ever has home rule from 2010-2015) on the city's max population from 1994-2009: the first regression is run on cities with population under 25,000 and the second is run on cities with population above 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 3: Formal Test of Change in Density at Home Rule Population Threshold



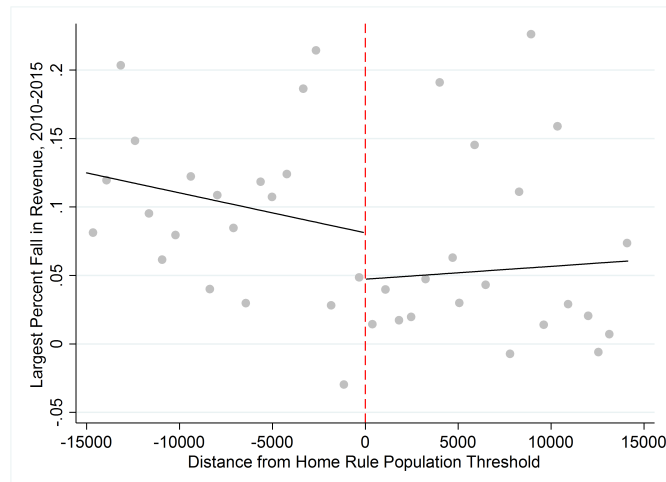
Note: The figure displays the density of municipalities with a range of populations from 10,000 to 40,000 centered at the home rule population threshold in Illinois (population=25,000). The figure does not show a statistical break in the density of municipalities near the cutoff - this is evidence against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

Figure 4: Home Rule, Population Histogram–No Sorting at Cutoff



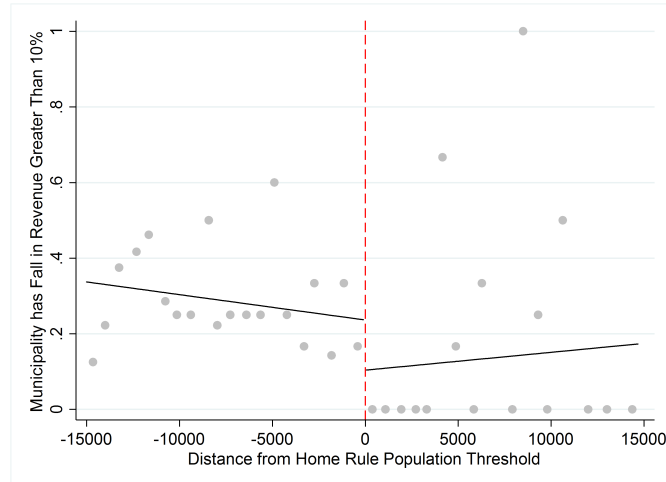
Note: The figure plots histogram showing percent of municipalities falling in each population bin. The size of the bins is 2,500.

Figure 5a: Effect of Home Rule on Revenue Stability



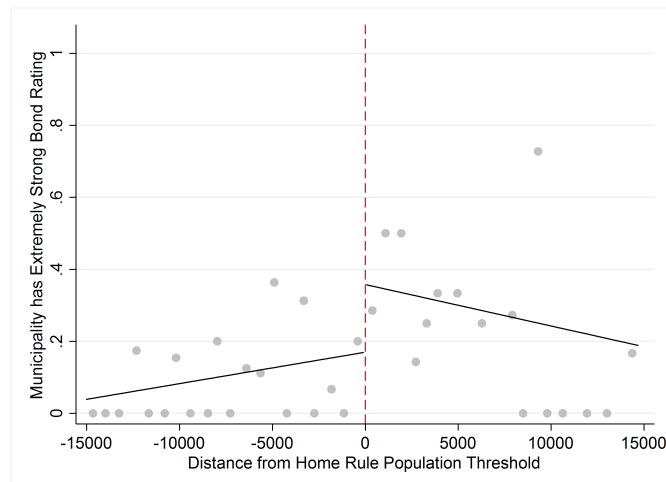
Note: The figure plots linear fit from fuzzy RD regression described below under **Table 9 - Panel B**

Figure 5b: Effect of Home Rule on Revenue Stability



Note: The figure plots linear fit from fuzzy RD regression described below under **Table 9 - Panel C**

Figure 6: Effect of Home Rule on Bond Rating



Note: The figure plots linear fit from fuzzy RD regression described below under **Table 9 - Panel E**

Table A.1: 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 (191,603)	121,944 (203,985)	125,610 (248,466)	142,936 (196,893)
Number of Big-Box	2.759 (2.689)	3.541 (3.461)	3.054 (2.126)	4.438 (4.558)
Total Revenue	335,228 (928,556)	282,942 (691,208)	301,118 (754,940)	317,511 (563,074)
Total Own-Source Revenue	274,666 (667,519)	236,797 (561,002)	250,754 (633,135)	275,638 (469,592)
Total Taxes	109,427 (313,337)	101,092 (208,953)	109,049 (224,677)	102,044 (148,985)
Total Spending	327,327 (885,429)	290,631 (695,875)	310,513 (793,521)	325,852 (577,036)
Total Debt Outstanding	337,014 (896,226)	483,514 (1,450,430)	464,548 (1,722,903)	540,872 (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.2: Effect of Bankruptcy on Sales Tax and Gross Receipts Revenue Over Time

	(1) Electronics	(2) Department	(3) Pooled	(4) Pooled: Matching	(5) Pooled: State by Year FEs
Panel A	Total Sales Tax and Gross Receipt Revenue				
One Year After Bankruptcy	-0.1044*** (0.0394)	-0.1005*** (0.0259)	-0.1013*** (0.0314)	-0.0875*** (0.0280)	-0.0565** (0.0189)
Two Years After Bankruptcy	-0.1082*** (0.0402)	-0.1043*** (0.0257)	-0.1053*** (0.0327)	-0.0934*** (0.0321)	-0.0821** (0.0277)
Three Years After Bankruptcy	-0.1224*** (0.0388)	-0.1030*** (0.0266)	-0.1192*** (0.0314)	-0.0921*** (0.0327)	-0.0885*** (0.0231)
R^2	0.939	0.970	0.946	0.928	0.969
Observations	3473	2767	4346	3802	4346
Panel B	Own-Source Revenue				
One Year After Bankruptcy	-0.0816*** (0.0249)	-0.0631** (0.0248)	-0.0681*** (0.0221)	0.0203 (0.0355)	-0.0875*** (0.0236)
Two Years After Bankruptcy	-0.0190 (0.0143)	-0.0297** (0.0147)	-0.0189 (0.0124)	-0.0354* (0.0184)	-0.0178 (0.0150)
Three Years After Bankruptcy	-0.0119 (0.0155)	-0.0339** (0.0140)	-0.0168 (0.0136)	-0.0576 (0.0390)	-0.0059 (0.0156)
R^2	0.986	0.989	0.988	0.986	0.989
Observations	3477	2768	4350	3806	4468

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

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

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_i^c 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; $\text{YearDummy}_t = 1$ represents dummy variables for each of the three years after the bankruptcy. $\text{OperatingDummy} * \text{YearDummy}$ controls for whether or not municipality i contains any operating stores in category c during the corresponding year. δ_i represents municipality fixed effects and γ_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.3: Post-Bankruptcy Comparison on Revenues and Spendings with Matching Specification (Bankrupt Count)

Panel A	Sales Tax and Gross Receipts-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Count	-0.0543*** (0.0152)	-0.0504 (0.0313)	-0.0954** (0.0442)	-0.0946** (0.0435)	-0.0830** (0.0366)
ACIR 1993 Home Rule x Bankrupt Count		0.0039 (0.0350)			
Krane et al. 2002 Home Rule 1 x Bankrupt Count			0.0492 (0.0465)		
Krane et al. 2002 Home Rule 2 x Bankrupt Count				0.0480 (0.0458)	
ICMA 1974 Home Rule x Bankrupt Count					0.0658 (0.0450)
Combined Effect: Bankrupt+(HR x Bankrupt)		-0.0465*** (0.0173)	-0.0462*** (0.0146)	-0.0465*** (0.0147)	-0.0172 (0.0251)
Adjusted R^2	0.929	0.929	0.930	0.930	0.906
Observations	3802	3802	3802	3802	2340

Panel B	Own-Source Revenue-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Count	-0.0161*** (0.0059)	-0.0337*** (0.0089)	-0.0545*** (0.0101)	-0.0537*** (0.0100)	-0.0287*** (0.0107)
ACIR 1993 Home Rule x Bankrupt Count		0.0273** (0.0110)			
Krane et al. 2002 Home Rule 1 x Bankrupt Count			0.0456*** (0.0120)		
Krane et al. 2002 Home Rule 2 x Bankrupt Count				0.0449*** (0.0118)	
ICMA 1974 Home Rule x Bankrupt Count					0.0249 (0.0159)
Combined Effect: Bankrupt+(HR x Bankrupt)		-0.0064 (0.0069)	-0.0089 (0.0064)	-0.0089 (0.0064)	-0.0038 (0.0117)
Adjusted R^2	0.986	0.986	0.986	0.986	0.987
Observations	3806	3806	3806	3806	2340

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

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

where $PublicFin_{it}$ is one of four public-finance variables in municipality i , in year t . The four variables are total sales tax and gross receipts revenue (**Panel A**), own-source revenue (**Panel B**), revenue from property taxes and charges & fee (**Panel C**), and total expenditure (**Panel D**). $BankruptCount_i$ 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). $HomeRule_i^r$ 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 introduced in the data section. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount$ and the coefficient on the interaction term $HomeRule * BankruptCount$. This gives us the total effect of the bankruptcy on home rule municipalities. All four panels use Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. 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.3: Post-Bankruptcy Comparison on Revenues and Spendings with Matching Specification (Bankrupt Count)

Panel C	Property Tax + Charges & Misc. Rev.-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Count	-0.0008 (0.0123)	-0.0416** (0.0186)	-0.0284* (0.0168)	-0.0288* (0.0166)	-0.0074 (0.0121)
ACIR 1993 Home Rule x Bankrupt Count		0.0624*** (0.0204)			
Krane et al. 2002 Home Rule 1 x Bankrupt Count			0.0329 (0.0226)		
Krane et al. 2002 Home Rule 2 x Bankrupt Count				0.0334 (0.0227)	
ICMA 1974 Home Rule x Bankrupt Count					0.0314* (0.0161)
Combined Effect: Bankrupt+(HR x Bankrupt)		0.0208** (0.0084)	0.0044 (0.0147)	0.0046 (0.0148)	0.0240** (0.0105)
Adjusted R^2	0.982	0.983	0.982	0.982	0.984
Observations	3806	3806	3806	3806	2340

Panel D	Total Expenditure-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Count	-0.0265** (0.0125)	-0.0490*** (0.0174)	-0.0272* (0.0160)	-0.0276* (0.0155)	-0.0188 (0.0125)
ACIR 1993 Home Rule x Bankrupt Count		0.0348 (0.0214)			
Krane et al. 2002 Home Rule 1 x Bankrupt Count			0.0009 (0.0218)		
Krane et al. 2002 Home Rule 2 x Bankrupt Count				0.0013 (0.0215)	
ICMA 1974 Home Rule x Bankrupt Count					0.0099 (0.0204)
Combined Effect: Bankrupt+(HR x Bankrupt)		-0.0141 (0.0132)	-0.0263* (0.0148)	-0.0263* (0.0149)	-0.0089 (0.0168)
Adjusted R^2	0.982	0.982	0.982	0.982	0.984
Observations	3806	3806	3806	3806	2340

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

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

where $PublicFin_{it}$ is one of four public-finance variables in municipality i , in year t . The four variables are total sales tax and gross receipts revenue (**Panel A**), own-source revenue (**Panel B**), revenue from property taxes and charges & fee (**Panel C**), and total expenditure (**Panel D**). $BankruptCount_i$ 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). $HomeRule_i^r$ 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 introduced in the data section. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptCount$ and the coefficient on the interaction term $HomeRule * BankruptCount$. This gives us the total effect of the bankruptcy on home rule municipalities. All four panels use Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. 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.4: Post-Bankruptcy Comparison on Revenues and Spendings with Matching Specification (Bankrupt Dummy)

Panel A	Sales Tax and Gross Receipts-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Dummy	-0.1176*** (0.0429)	-0.0463 (0.0845)	-0.1131 (0.1496)	-0.1319 (0.1484)	-0.1984* (0.1129)
ACIR 1993 Home Rule x Bankrupt Dummy		-0.0835 (0.0935)			
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy			-0.0027 (0.1522)		
Krane et al. 2002 Home Rule 2 x Bankrupt Dummy				0.0191 (0.1496)	
ICMA 1974 Home Rule x Bankrupt Dummy					0.1574 (0.1355)
Combined Effect: Bankrupt+(HR x Bankrupt)		-0.1297*** (0.0454)	-0.1158*** (0.0359)	-0.1128*** (0.0349)	-0.0410 (0.0728)
Adjusted R^2	0.929	0.930	0.930	0.930	0.906
Observations	3802	3802	3802	3802	2340

Panel B	Own-Source Revenue-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Dummy	-0.0304* (0.0160)	-0.0517** (0.0248)	-0.1165*** (0.0352)	-0.1207*** (0.0352)	-0.0570** (0.0244)
ACIR 1993 Home Rule x Bankrupt Dummy		0.0399 (0.0311)			
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy			0.1033*** (0.0392)		
Krane et al. 2002 Home Rule 2 x Bankrupt Dummy				0.1085*** (0.0381)	
ICMA 1974 Home Rule x Bankrupt Dummy					0.0681* (0.0390)
Combined Effect: Bankrupt+(HR x Bankrupt)		-0.0118 (0.0199)	-0.0131 (0.0173)	-0.0122 (0.0170)	0.0112 (0.0317)
Adjusted R^2	0.986	0.986	0.986	0.986	0.987
Observations	3806	3806	3806	3806	2340

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

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

where $PublicFin_{it}$ is one of four public-finance variables in municipality i , in year t . The four variables are total sales tax and gross receipts revenue (**Panel A**), own-source revenue (**Panel B**), revenue from property taxes and charges & fee (**Panel C**), and total expenditure (**Panel D**). $BankruptDummy_i$ equals 1 when municipality i contains one or more of the treatment chains. $Post_t$ is an indicator for whether or not year t is after 2008 (the bankruptcy year). $HomeRule_i^r$ 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 introduced in the data section. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptDummy$ and the coefficient on the interaction term $HomeRule * BankruptDummy$. This gives us the total effect of the bankruptcy on home rule municipalities. All four panels use Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. 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.4: Post-Bankruptcy Comparison on Revenues and Spendings with Matching Specification (Bankrupt Dummy)

Panel C	Property Tax + Charges & Misc. Rev.-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Dummy	0.0068 (0.0347)	-0.0797* (0.0437)	-0.0685* (0.0380)	-0.0587 (0.0392)	0.0072 (0.0294)
ACIR 1993 Home Rule x Bankrupt Dummy		0.1473*** (0.0491)			
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy			0.0900 (0.0577)		
Krane et al. 2002 Home Rule 2 x Bankrupt Dummy				0.0785 (0.0594)	
ICMA 1974 Home Rule x Bankrupt Dummy					0.0742* (0.0414)
Combined Effect: Bankrupt+(HR x Bankrupt)		0.0677*** (0.0229)	0.0215 (0.0430)	0.0198 (0.0432)	0.0814*** (0.0284)
Adjusted R^2	0.982	0.983	0.982	0.982	0.984
Observations	3806	3806	3806	3806	2340

Panel D	Total Expenditure-Matching Specification				
	(1)	(2)	(3)	(4)	(5)
Bankrupt Dummy	-0.0365 (0.0306)	-0.1013*** (0.0373)	-0.0384 (0.0381)	-0.0407 (0.0375)	-0.0416 (0.0293)
ACIR 1993 Home Rule x Bankrupt Dummy		0.1092** (0.0461)			
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy			0.0027 (0.0528)		
Krane et al. 2002 Home Rule 2 x Bankrupt Dummy				0.0053 (0.0525)	
ICMA 1974 Home Rule x Bankrupt Dummy					0.0788 (0.0502)
Combined Effect: Bankrupt+(HR x Bankrupt)		0.0079 (0.0276)	-0.0358 (0.0369)	-0.0354 (0.0370)	0.0372 (0.0398)
Adjusted R^2	0.982	0.982	0.982	0.982	0.984
Observations	3806	3806	3806	3806	2340

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

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

where $PublicFin_{it}$ is one of four public-finance variables in municipality i , in year t . The four variables are total sales tax and gross receipts revenue (**Panel A**), own-source revenue (**Panel B**), revenue from property taxes and charges & fee (**Panel C**), and total expenditure (**Panel D**). $BankruptDummy_i$ equals 1 when municipality i contains one or more of the treatment chains. $Post_t$ is an indicator for whether or not year t is after 2008 (the bankruptcy year). $HomeRule_i^r$ 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 introduced in the data section. $OperatingCount * Post$ controls for the number of operating stores after the bankruptcy year. δ_i represents municipality fixed effects and γ_t represents time fixed effects. The "Combined Effect" row shows the sum of the coefficient on $BankruptDummy$ and the coefficient on the interaction term $HomeRule * BankruptDummy$. This gives us the total effect of the bankruptcy on home rule municipalities. All four panels use Coarsened Exact Matching based on the municipalities' population and the number of big-box stores. 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: Home Rule Results are Robust to Regional Controls

Panel A	Total Own-Source Revenue			
	(1)	(2)	(3)	(4)
Bankrupt Count	-0.0407*** (0.0081)	-0.0552*** (0.0158)	-0.0501*** (0.0158)	-0.0483*** (0.0097)
Krane et al. 2002 Home Rule 1 x Bankrupt Count	0.0257*** (0.0089)	0.0301*** (0.0101)	0.0248*** (0.0088)	0.0578*** (0.0103)
Bankrupt+(HR x Bankrupt)	-0.0150** (0.0061)	-0.0251* (0.0136)	-0.0253* (0.0143)	0.0095** (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*** (0.0321)	-0.1535*** (0.0439)	-0.0948** (0.0386)	-0.1038*** (0.0276)
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy	0.1015*** (0.0354)	0.1203*** (0.0386)	0.0615** (0.0312)	0.1460*** (0.0305)
Bankrupt+(HR x Bankrupt)	-0.0134 (0.0170)	-0.0332 (0.0277)	-0.0334 (0.0282)	0.0422** (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:

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

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^r 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 introduced in the data section. $\text{OperatingCount} * \text{Post}$ controls for the number of operating stores after the bankruptcy year. $\text{BankruptCount} * \text{RegionalDummy}$ controls for the number of bankrupt stores in different census regions or states. δ_i represents municipality fixed effects and γ_t represents time fixed effects. $\text{Bankrupt} + (\text{HR} * \text{Bankrupt})$ shows the sum of the coefficient on BankruptCount and the coefficient on the interaction term $\text{HomeRule} * \text{BankruptCount}$. **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. **Panel C** is the same as Panel A, except that Panel C's regressions utilize Coarsened Exact Matching when the municipalities with $\text{BankruptDummy} = 0$ to municipalities with $\text{BankruptDummy} = 1$ based on their respective population and the number of big-box stores. **Panel D** is the same as Panel C, except that BankruptCount is replaced with BankruptDummy . 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: Home Rule Results are Robust to Regional Controls

Panel C	Total Own-Source Revenue-Matching Specification			
	(1)	(2)	(3)	(4)
Bankrupt Count	-0.0545*** (0.0101)	-0.0770*** (0.0177)	-0.0510*** (0.0179)	-0.0492*** (0.0106)
Krane et al. 2002 Home Rule 1 x Bankrupt Count	0.0456*** (0.0120)	0.0553*** (0.0139)	0.0293** (0.0129)	0.0605*** (0.0111)
Bankrupt+(HR x Bankrupt)	-0.0089 (0.0064)	-0.0217* (0.0131)	-0.0217 (0.0138)	0.0113*** (0.0042)
Controls x Bankrupt Count	-	CENSUS REGION	CENSUS DIVISION	STATE
Adjusted R^2	0.986	0.986	0.987	0.987
Observations	3806	3806	3806	3806
Panel D	Total Own-Source Revenue-Matching Specification			
	(1)	(2)	(3)	(4)
Bankrupt Dummy	-0.1165*** (0.0352)	-0.1585*** (0.0481)	-0.0837** (0.0423)	-0.1107*** (0.0298)
Krane et al. 2002 Home Rule 1 x Bankrupt Dummy	0.1033*** (0.0392)	0.1247*** (0.0423)	0.0498 (0.0343)	0.1494*** (0.0321)
Bankrupt+(HR x Bankrupt)	-0.0132 (0.0173)	-0.0339 (0.0284)	-0.0339 (0.0287)	0.0387*** (0.0125)
Controls x Bankrupt Dummy	-	CENSUS REGION	CENSUS DIVISION	STATE
Adjusted R^2	0.986	0.986	0.987	0.987
Observations	3806	3806	3806	3806

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

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

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^r 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 introduced in the data section. $\text{OperatingCount} * \text{Post}$ controls for the number of operating stores after the bankruptcy year. $\text{BankruptCount} * \text{RegionalDummy}$ controls for the number of bankrupt stores in different census regions or states. δ_i represents municipality fixed effects and γ_t represents time fixed effects. $\text{Bankrupt} + (\text{HR} * \text{Bankrupt})$ shows the sum of the coefficient on BankruptCount and the coefficient on the interaction term $\text{HomeRule} * \text{BankruptCount}$. **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. **Panel C** is the same as Panel A, except that Panel C's regressions utilize Coarsened Exact Matching when the municipalities with $\text{BankruptDummy} = 0$ to municipalities with $\text{BankruptDummy} = 1$ based on their respective population and the number of big-box stores. **Panel D** is the same as Panel C, except that BankruptCount is replaced with BankruptDummy . 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.6: 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*** (0.0386)	-0.0447*** (0.0145)	-0.0336** (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*** (0.0405)	-0.0480*** (0.0158)	-0.0302* (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*** (0.0290)	-0.0692*** (0.0180)	-0.0394** (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. 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 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). The specific controls we include 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 Tables 2 and 3 but 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.

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

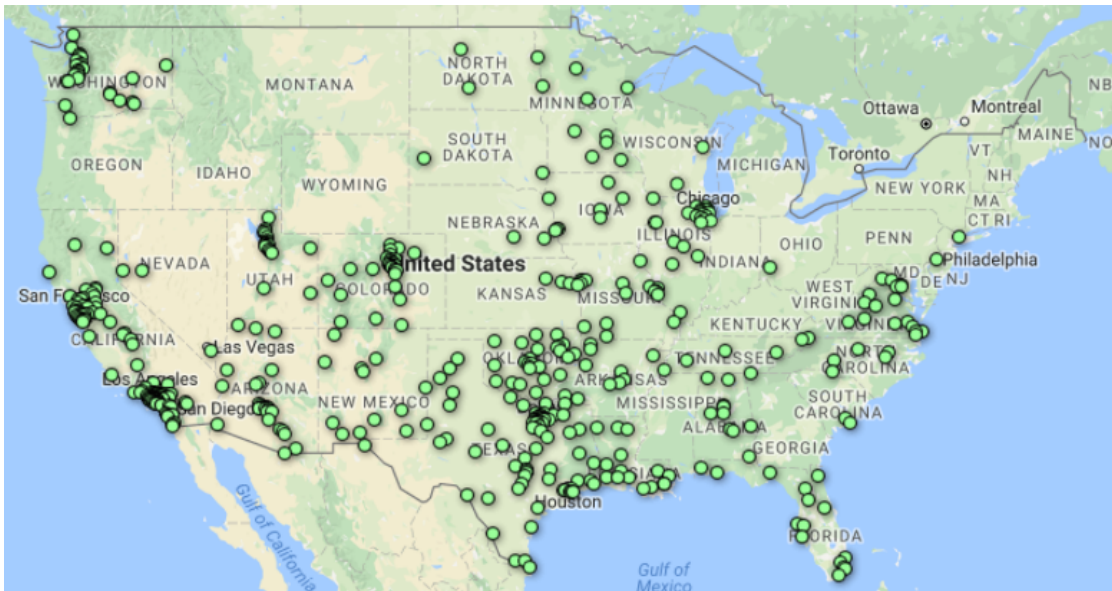
Table A.7: Main Results are Robust to Sample Restriction

	(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*** (0.0381)	-0.0488*** (0.0142)	-0.0330** (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*** (0.0381)	-0.0488*** (0.0142)	-0.0330** (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*** (0.0408)	-0.0528*** (0.0150)	-0.0349** (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*** (0.0422)	-0.0527*** (0.0155)	-0.0317* (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.

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

Figure A.1: Map of the Operating Stores in the US



Note: The green dots on the map shows the locations of the stores that continued to operate within the time frame of the study.

Figure A.2: Map of the Defunct Stores in the US



Note: The red dots on the map shows the locations of the stores that closed down after the bankruptcies.