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**WHAT DRIVES FINANCIAL REFORM?  
ECONOMICS AND POLITICS OF THE  
STATE-LEVEL ADOPTION OF MUNICIPAL  
BANKRUPTCY LAWS**

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***FINANCIAL ECONOMICS***



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# WHAT DRIVES FINANCIAL REFORM? ECONOMICS AND POLITICS OF THE STATE-LEVEL ADOPTION OF MUNICIPAL BANKRUPTCY LAWS<sup>†</sup>

## Abstract

We investigate economic and political theories of financial reform to analyze state-level adoption of municipal bankruptcy laws (Chapter 9). Using a dynamic Cox hazard model, we find that interest group factors related to the relative strength of potential losers (labor unions) and winners (bond investors), courts' efficiency, and trust in non-opportunistic behavior by local government explain the timing of Chapter 9 adoptions between 1980 and 2012. Similar factors also explain congressional voting on municipal bankruptcy law. After Chapter 9 adoption, municipal bond spreads decrease and firms experience higher revenues, profits, and investments, particularly in states in which more bond proceeds are used by the private sector. Our findings support political and economic theories of financial reform, and highlight a novel spillover channel from the public to the private sector.

JEL Classification: D86, G33, G34 and K22

Keywords: bankruptcy, chapter 9, judicial efficiency, labor union and municipal bonds

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## *I Introduction*

There is a near consensus in economics that financial reforms such as changes in bankruptcy and corporate governance legislation reduce borrowing costs and foster economic and financial development (e.g., La Porta, Lopez-De-Silanes, and Shleifer (2008); Djankov, Hart, McLiesh, and Shleifer, (2008)). It is also well understood in theory that powerful incumbents and other political economy factors can delay and even block beneficial reforms (Rajan and Zingales (2003a and 2003b), Caselli and Gennaioli (2008)), and that an efficient legal infrastructure is a necessary prerequisite for successful financial reforms (Ayotte and Yun (2009); Gennaioli and Rossi (2010 and 2013); Ponticelli (2014)). Other approaches emphasize the role of worsening economic conditions in fostering financial reform (e.g., Stiglitz (2000)). But can these theories of financial reform help understand the data?

A recent and growing literature examines the effect of financial reform in individual countries on various firm-level outcomes,<sup>1</sup> but the drivers of financial reform have received less attention. In general, financial reform improves the ability of agents to enter into financial contracts with each other ex ante and to restructure such contracts ex post.<sup>2</sup> In this paper we focus on the state-level adoption of bankruptcy law for municipalities in the U.S., known as Chapter 9. Chapter 9 of the U.S. Bankruptcy Code, enacted in 1978, regulates the bankruptcy process for municipalities. However, the application of the code is not automatic; for municipalities to be able to file for Chapter 9, their states first need to vote its authorization into state law. In the absence of Chapter 9, municipalities face limited options to resolve financial distress (McConnell and Picker (1993), Gillette (2012a)). Debt restructuring would typically require unanimity, which can be hard to achieve with many dispersed bondholders. As a result, often the only remaining possibility is to

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<sup>1</sup> Scott and Smith (1986), Araújo, Ferreira, and Funchal (2012), Vig (2013), Assunção, Benmelech, and Silva (2013), Cerqueiro, Ongena, and Roszbach (JF forthcoming), Hackbarth, Haselmann, and Schoenherr (RFS forthcoming), Ponticelli (2014), and Rodano, Serrano-Velarde, and Tarantino (2015). Claessens and Klapper (2005) examine how provisions in the bankruptcy code correlate with the number of firm bankruptcies around the world.

<sup>2</sup> In this sense, examples of financial reform include changes in corporate-governance and bankruptcy legislation, such as improvements in the protection of minority investors (e.g., La Porta, Lopez-De-Silanes, and Shleifer (2008)); changes in the regulation of financial intermediaries, such as bank branching deregulation (e.g., Kroszner and Strahan (1999)), interest-rate liberalization and liberalization of entry into the financial sector (e.g., Abiad and Mody (2005)); or indeed any legal or administrative reform, such as increases in the hiring and the training of judges, thereby increasing the likelihood that intertemporal contracts will be enforced.

levy new taxes, which can both be of limited effectiveness in times of economic recessions and have drawbacks such as creditor runs and fire sales (McConnell and Picker (1993)).

The law and economics of municipal borrowing, financial distress, and bankruptcy are relatively underexplored and quite interesting in their own right. In addition, our focus on the adoption of Chapter 9 in U.S. states to study the relative explanatory power of theories of financial reform presents four clear advantages over alternative settings, e.g., cross-country comparisons or other U.S. reforms. First, unlike other U.S. reforms, such as for example the bankruptcy reforms for corporations (Chapter 11) and individuals (Chapter 13) that together became effective in all states at the same time in 1978, the staggered passage of Chapter 9 is both largely free of confounding events that might cloud inference and also provides a larger set of counterfactuals and a more effective identification opportunity of the drivers and consequences of financial reform. At the same time, focusing on variation within the U.S. allows to hold fixed a broad set of financial and political institutions that are common across U.S. states. Second, unlike firms, municipalities cannot ‘forum shop’ to a court located in a different state, which implies that states’ decisions regarding bankruptcy law are binding for their municipalities. Third, Chapter 9 clearly means the same thing in all U.S. states. By contrast, cross-country comparisons of the likelihood and timing of financial reform likely suffer from an “apples and oranges” problem, as both the initial conditions differ, for example between developed and emerging countries, and the very content of financial reform also typically differs across countries.<sup>3</sup> Fourth, Chapter 9 was fully developed by the time of the enactment of the 1978 Bankruptcy Code. As a result, we can focus on the explanatory power of theories of financial reform and abstract from issues related to the drafting costs of devising and writing up a bankruptcy code from scratch, which may also differ across countries.

At the same time we note that, unlike firms, municipalities provide public goods. As a result, one central goal of municipal bankruptcy law is to allow financially distressed

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<sup>3</sup> Berkovitch and Israel (1999), Povel (2003), Ayotte and Yun (2009), and Gennaioli and Rossi (2013) show theoretically that very different bankruptcy laws emerge in different economic and legal environments and at different stages of financial and economic development. Empirically, see the bankruptcy reforms in Hungary, Italy, France, Spain, and Brazil (e.g., Franks and Loranth (2014), Rodano, Serrano-Velarde, and Tarantino (2015), and Ponticelli (2014)), all inspired by U.S. Chapter 11 law, but containing also very different idiosyncratic components; for example, France, Spain, and Brazil also reformed the liquidation code at the same time (Chapter 7-style), while Hungary strengthened considerably the role of the trustee.

municipalities to restructure their debt and successfully emerge from bankruptcy with a viable budget. By contrast, corporate bankruptcy law recognizes that some firms are no longer economically viable and should therefore be liquidated.<sup>4</sup>

We begin by documenting the large variation to date in the likelihood and speed of adoption of Chapter 9. For example, New York, New Jersey, and Colorado were among the early adopters, while Delaware, Massachusetts, and Indiana, among others, have yet to adopt it. Georgia and Iowa, on the other hand, have explicitly banned municipal bankruptcy.<sup>5</sup>

To investigate the drivers of financial reform, we use a dynamic Cox hazard model to explain the timing of the intrastate adoption of Chapter 9. We incorporate proxies for interest-group, legal, political-institutional, and economic factors to understand how changes in these variables over time and across states affect the likelihood of Chapter 9 adoption.

We find that Chapter 9 is authorized earlier in states in which labor unions have more limited coverage, the judiciary is more efficient, outstanding state debt is larger, and the population identifies more closely with the local community relative to the national one. In contrast, general economic conditions, other interests such as the local insurance and banking industry, and whether the Democratic or Republican party control both state houses and governorship do not explain Chapter 9 authorization. Interest group factors related to the relative strength of potential losers (labor unions) and winners (muni bond investors, particularly mutual funds), the extent of courts' efficiency, and the extent of the public's trust in non-opportunistic behavior by the local government *ex post* (e.g., strategic bankruptcy filings), thus explain the timing of Chapter 9 authorizations between 1980 and 2012. Similar interest group and economic and political factors also illustrate the litigation process in court in recent high-profile municipal bankruptcy cases (e.g., San Bernardino, Detroit), and explain congressional voting on the first federal municipal bankruptcy law in the 1930s.

We proceed to examine municipal bond financing and firm outcomes in the aftermath of Chapter 9 authorizations. Despite the large literature on the effects of financial reforms on firm-

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<sup>4</sup> In other words, Chapter 9 can be thought of as a “Chapter 11 for municipalities” (although with important differences that we outline in Section II), as both Chapter 9 and 11 ‘improve the ability of agents to enter into financial contracts with each other *ex ante* and to restructure such contracts *ex post*’. By contrast, Chapter 7 is concerned with winding up a firm *ex post* rather than restructure it.

<sup>5</sup> As of 2012, only 27 states have authorized Chapter 9.

level and contract-level outcomes reviewed above, the effects of Chapter 9 authorizations have yet to be assessed empirically. It is important to establish whether, in line with the theories of financial reform mentioned above, Chapter 9 authorizations are followed by beneficial outcomes for economic agents. We find that the credit spreads of newly issued municipal bonds are 10 basis points lower following the adoption of Chapter 9, relative to bonds issued in states that do not adopt Chapter 9. These effects are concentrated in revenue bonds that are backed by a cash flow stream from public projects, e.g., highway toll roads, and are absent in general obligation bonds that are backed by the tax revenues that Chapter 9 may limit or prevent access to. These findings support the idea that municipal bankruptcy reform reduces borrowing costs for municipalities.

We also find that firms operating in a single state experience increases in sales, profitability, investments, and dividend payouts after their state adopts Chapter 9. These effects are stronger in states in which a larger fraction of the proceeds of newly issued municipal bonds are used by the private (non-government) sector, highlighting a novel spillover channel from the public to the private sector, consistent with the idea that the reduced costs of public financing benefit the local firms that use these bond proceeds to finance their investment projects.

To the best of our knowledge, we are the first to examine empirically the drivers of the adoption of bankruptcy law, and to gauge the explanatory power of theories of financial reform. Kroszner and Strahan (1999) use a hazard model like us, but they examine the timing of bank branching deregulation across U.S. states, and unlike us they are interested in gauging the explanatory power of public and private interest theories of regulation. Abiad and Mody (2005) construct an index aggregating six financial liberalization policies, use an ordered logit model to examine the index's variation across countries and over time, and document a strong status quo bias. In line with Kroszner and Strahan (1999), we find that powerful interest groups can successfully delay and even prevent reform. Our focus on legal factors such as court efficiency and on the potential for opportunistic behavior by local governments are novel.

The roadmap of the paper is as follows. Section II describes municipal bankruptcy law and Chapter 9 of the 1978 Bankruptcy Code. Section III describes the theoretical hypotheses and the variables for the empirical analysis. Section IV presents the results. Section V concludes.

## II Institutional Background of Municipal Bankruptcy and Chapter 9

This section provides a brief overview of municipal bankruptcy law in the U.S. and shows that the introduction of Chapter 9 of the 1978 Bankruptcy Code has improved municipalities' options to resolve financial distress. The application of the code, however, is not automatic; for municipalities to be able to file for Chapter 9, states need to explicitly vote to authorize it into state law.<sup>6</sup>

### II.A Resolving Municipal Distress without a Bankruptcy Law

Prior to the Great Depression, there was no federal bankruptcy law and municipalities financed their operations mostly through property taxes, but also through municipal bonds. When unpaid taxes led to delayed repayments to municipal bondholders and defaults, creditors sought a number of remedies in court, including i) the seizure of city property; ii) the request of judicial oversight of city financial affairs, such as explicit limitations on expenditures away from debt service; (iii) the seizure of private property within the city (analogue to 'piercing the corporate veil'); (iv) the state assumption of municipal indebtedness (analogue to requiring a parent company to assume the debts of its subsidiaries); (v) obtaining a lien on future tax revenues; or (vi) the *mandamus*, i.e., the imposition of new taxes earmarked for debt service.

The first five remedies echoed similar remedies available to private creditors, but they all proved ineffective, because they were either strongly limited or outright denied by courts.<sup>7</sup> Only the sixth remedy (*mandamus*), unavailable to private creditors, was explicitly allowed by the U.S.

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<sup>6</sup> This section is based on McConnell and Picker (1993) and Gillette (2012a). Additional sources include Feldstein and Fabozzi (2008) and Freyberg (1997).

<sup>7</sup> No court permitted seizure of city property (e.g., *City of Chicago v Hasley* 1861; Louisiana Supreme Court in *Town of Farmerville v Commercial Credit Co.*, 1931), even though some courts claimed that "cities are like businesses" (e.g., Ohio Supreme Court in *Comm'rs of Hamilton Co. v Mighels*, 1857; Supreme Court of Florida in *Kaufman v City of Tallahassee*, 1922). In addition, courts had no authority to limit the discretion of city officials to control the operating budget of the municipality (e.g., U.S. Supreme Court in *East St. Louis v Zebley*); in the absence of express statutory authorization by the state, courts held that they could not appoint a receiver to enforce taxes levied to secure payments of municipal debts. The seizure of private property within the city, although unthinkable today, used to be the rule in most of the New England states. The U.S. Supreme Court rejected this New England doctrine in *Meriwether v Garrett*, 1880, bringing about the analogy with limited liability for individuals. State assumption of municipal indebtedness was already ruled out during the railroad debt crises of the 1860s and 1870s. Finally, liens on future tax or other revenues were also ruled out explicitly. In a typical case, *Ellis v Pratt City*, 1896, the Alabama Supreme Court held that garnishment was barred by the "public purpose doctrine" (even though they provided an exception by ordering garnishment in *Murphree v City of Mobile*, 1895). Similarly, garnishment of tax receipts was in general not allowed barring exceptions (e.g., in *Underhill v Calhoun* it was ruled "contrary to moral sense but necessitated by public policy"). The U.S. Supreme Court in *Meriwether v Garrett* ruled that out forever, establishing that until taxes are collected, they cannot be seized.

Supreme Court in the *Meriwether v Garrett* decision in 1880.<sup>8</sup> However, at times of general financial distress this remedy had serious drawbacks, first and foremost because in a recession levying new taxes does not necessarily increase revenue. In addition, municipal financial distress during the Great Depression was associated with creditor runs and fire sales in tax sales of property, which made matters worse. Typical remedies available to corporate creditors, such as debt renegotiation or restructuring ex post, often required unanimity, which was very hard to achieve with a large number of municipal creditors.

### *II.B The First Federal Bankruptcy Law for Municipalities*

The large increase in the number of financially distressed municipalities during the Great Depression prompted Congress to introduce a federal bankruptcy law. The law was created in 1933 by amending the Bankruptcy Act of 1898; it was then voted in the House in 1933 and in the Senate in 1934. This bill was designed to deal solely with holdout problems, which arose when individual creditors threatened to halt the negotiated debt plans in the best interests of the debtor and the creditors as a group.<sup>9</sup> However, in 1936 the Supreme Court ruled in a 5-4 decision this first statute to be unconstitutional as it “might materially restrict respondent’s control over its fiscal affairs” (*Ashton v. Cameron County Water Improvement Dist. No. 1*).

In the following year, Congress passed a new municipal bankruptcy statute only slightly different from the previous one. The new statute was again challenged, but this time it was upheld by the Supreme Court without identifying any difference with the previous act, as the Court was impressed by the extent of the financial distress of municipalities and the need for debt relief. The legislation was generally regarded as effective and timely. Hempel (1973) reports that 362 municipal bankruptcy cases were filed between 1938 and 1972. Of that number, 343, or nearly 95%, were filed before 1952 (see also Lehmann (1950)).

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<sup>8</sup> *Meriwether v Garrett*, 102 US 472, 513 (1880). Typically, *mandamus* duties are imposed by state law. In other cases, the borrower commits to levy necessary taxes as part of some covenant in the debt instrument itself. In those cases, such duty was self-imposed and made irrevocable by the force of federal constitutional law under the Contracts Clause. Even absent state law or ex-ante commitment, courts in general have been willing to allow for *mandamus*, i.e., to allow creditors to force the municipality into levying new taxes ex post. However, if state law limited tax rates for the purposes of repaying debt, creditors could not obtain *mandamus* for any additional taxation.

<sup>9</sup> Under the bill, municipalities were permitted (but not required) to negotiate settlements of their debts with their creditors. Only if the settlement was approved by at least 75% of creditors could it be imposed on the remaining minority.

## *II.C Chapter 9 of the 1978 Bankruptcy Code*

The above legislation remained in place until 1978, when the current Bankruptcy Code was passed, and Chapter 9 was introduced to deal with municipal bankruptcy. Relative to the prior legislation, Chapter 9 provides a municipal debtor with two primary benefits: i) the automatic stay, and ii) the power to readjust debts through a bankruptcy plan process, with the explicit definition of a cram-down provision similar to that in Chapter 11. The reorganization plan can include the reduction of municipal debts, the rejection of onerous contracts, and the issuance of new debt to provide capital that might facilitate financial recovery. Among these features, the cram-down provision and the rejection of executory or onerous contracts are particularly important.<sup>10</sup> While the full extent of the latter prohibition is under debate – namely, to what extent it includes collective bargaining agreements, including pension and benefit obligations – it does explain the political opposition of labor unions to the passage of Chapter 9 and to many filings by distressed municipalities (e.g., Detroit and San Bernardino, see Stech (2012)). In fact, unlike private debtors in the context of Chapter 11, municipal debtors might be able to unilaterally abrogate collective bargaining agreements, subject only to liability for damages for breach of contract.

Section 109(c) allows a debtor to file for Chapter 9 if it i) is a municipality, that is, a “political subdivision or public agency or instrumentality of a State,” ii) is authorized by state law or an appropriate governmental officer or organization, iii) is insolvent, iv) desires to effect a plan to adjust its debts, and v) has appropriately negotiated with creditors.<sup>11</sup>

There are two main limitations to the power of municipalities to file for bankruptcy, differentiating from the analogous case of a corporation filing for Chapter 11. First, the municipality has to be authorized “by State law, or by a governmental officer or organization

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<sup>10</sup> The standards for confirming a Chapter 9 plan are quite lengthy and complex, but the cram-down powers are fairly straightforward. In short, Chapter 9 incorporates the Chapter 11 requirement that at least one impaired class of claims approve the plan. It also incorporates Chapter 11's cram-down protections for secured and unsecured creditors. For secured creditors, this means that they are to receive at least the value of the property securing their claims. For unsecured creditors, it means that managers need not pay off unsecured creditors to remain in control, but instead can retain control while confirming a plan giving unsecured creditors very little. In the extreme, the debtor might be able to abrogate the claims of the unsecured creditors altogether, as long as it can find an impaired class of secured claims willing to approve the plan. That is a very real possibility, given the possibility of gerrymandering classes.

<sup>11</sup> More precisely, according to section 109(c), the procedural bar mandates certain pre-filing efforts by the municipal debtor to work out its financial difficulties. It must either have reached an agreement sufficient to confirm a plan under Chapter 9, have failed to do so despite good faith negotiations, or such negotiations must be “impracticable.” These requirements are unique to the Chapter 9 debtor and do not apply in, for example, Chapter 11. Section 921(c) empowers the bankruptcy court to dismiss petitions not filed in ‘good faith’.

empowered by State law to authorize such entity to be a debtor under such chapter”; and, second, the municipality has to be insolvent.<sup>12</sup> On the other hand, unlike Chapter 11, Chapter 9 gives courts very little discretion or explicit authority in municipal bankruptcy. Section 904 prohibits courts from interfering with the political or governmental powers of the debtor municipality, any of its property or revenues, or its use or enjoyment of any income-intervention principle to be constitutionally required without the consent of the debtor. As a result of all of these considerations, Chapter 9 is generally considered to be improving the options of debtors and creditors for restructuring debt (e.g., Skeel (2013)).

#### *II.D State Authorization to File for Chapter 9*

For our purposes, the crucial feature of Section 109 of Chapter 9 is the requirement of state authorization. Authorization can be conditional on specific circumstances such as explicit state approval. We obtain the year of the relevant enactment of Chapter 9 for each state from the original state codes and statutes as posted on the states’ official website and WestlawNext database provided by Thompson Reuters. WestlawNext provides historic information about when a certain code was first introduced and which amendments have been made over time. Table 1 reports the relevant state-level authorization in U.S. states and documents a large variation in the likelihood and speed of adoption of Chapter 9. Before the mid-1990s, some may have believed that the somewhat vague requirement of a “general authorization” implied that states wishing to allow their municipalities to file did not need an explicit vote and a vote was required only to prohibit municipal bankruptcy, as Georgia did. Even then, however, 19 states explicitly authorized Chapter 9 well before 1990. In any event, in 1994 Congress passed a law<sup>13</sup> that clarified the need for an explicit state-level vote to authorize municipalities to file for bankruptcy.

Table I reports the relevant legislation that in each state authorized Chapter 9. As of 2012, twenty-seven states have authorized Chapter 9. While there is some variation in eligibility, Chapter

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<sup>12</sup> For Chapter 9 purposes, insolvency requires that a municipality be unable currently or prospectively to pay its bills as they become due. The first test is straightforward, as it involves the current nonpayment of debts; the second test, which looks at future capacity to pay, is not straightforward and is a source of legal debate. The insolvency test is essentially a cash flow insolvency test, and is arguably a hard one to pass: For example, in the prominent case of Bridgeport, Connecticut, the municipality was running a \$16 million annual deficit, had the highest effective tax rates in the State, and was in dire financial straits. But it had not yet exhausted its borrowing power and thus would not “run out of cash” in the next fiscal year. The court accordingly held that it was not insolvent. The insolvency standard has since been argued to be too strict.

<sup>13</sup> Bankruptcy Reform Act of 1994, Pub L 103394, 108 Stat 4106 (1994).

9 is drafted at the federal level and is uniform across all these states. The remaining twenty-four states (including Washington, DC) have not authorized Chapter 9, and therefore are not covered by bankruptcy protection. Three of them have even put in place severe restrictions that make it practically impossible for municipalities to file for bankruptcy. Kansas requires all of its subdivisions to operate on a cash-only basis, which makes bankruptcy highly unlikely. Georgia explicitly prohibits municipalities from filing Chapter 9, and Iowa also prohibits municipal bankruptcy but allows for a single exception. The remaining twenty-one states have not enacted legislation for dealing with municipal financial distress. For our purposes, in the empirical analysis we focus on the basic dichotomy of Chapter 9 authorization v. no authorization, because even the states that have explicitly banned Chapter 9 can still in principle authorize it just by enacting appropriate legislation.

### *III. Hypotheses and Variable Definitions*

Our empirical tests are based on the timing of state-level adoption of municipal bankruptcy laws (Chapter 9). In this section, we describe our hypotheses, our predictions, and the data sources used to construct the variables. Some of the variables will be consistent with a single theory of financial reform, while others will be consistent with several. Our approach is to assess the relative importance of variables representing each theory in speeding or slowing the adoption of municipal bankruptcy laws. Importantly, sometime theories will have opposite empirical predictions regarding whether some variables should predict earlier or later adoption. These opposing predictions will allow our empirical tests to differentiate somewhat among competing theories.

#### *III.A Hypotheses*

There are three main views of what drives financial reform. Under an *economic efficiency* view, following negative shocks to the economy, financial reform helps overcome contractual rigidities, fostering efficient renegotiation (e.g., Stiglitz (2000)). According to this view, one would expect reform to occur earlier when economic conditions worsen and when the benefits from renegotiation are larger. Under a *political economy* view, it is the relative size of interest groups that shape the likelihood and timing of reform (e.g., Rajan and Zingales (2003a)). In the context of Chapter 9 bankruptcy, potential losers are labor unions and potential winners are bondholders (e.g., Gillette (2012a and 2012c)). Under a *judicial efficiency* view, the efficiency of courts is a

prerequisite for efficient reform (e.g., Ayotte and Yun (2009), Gennaioli and Rossi (2013), Ponticelli (2014)). In the context of Chapter 9, one would expect reform to occur earlier when bankruptcy courts are more efficient. Other approaches emphasize the importance of concerns for strategic bankruptcy filing by neighbor municipalities (Gillette (2012b)), of political science-type conflicts between federal and state law (e.g., Briffault (1990), Williams (1986)), and of ideology, which has been argued to be potentially relevant for most reforms (Poole and Rosenthal (2007)), albeit much less so for bankruptcy (Berglöf and Rosenthal (2000)).

### *III.B Empirical Predictions*

We review the empirical predictions of the above mentioned theories on several (classes of) empirical variables, including economic conditions, labor unions, bondholders' interests, judicial efficiency, local identity/trust in local government, and political/institutional factors. In some cases (e.g., labor unions and judicial efficiency) different theories yield opposite empirical predictions, which will allow our empirical tests to distinguish somewhat among the theories.

*Inertia/economic conditions.* One possibility is that some states did not feel a pressing need to authorize Chapter 9. Congress did not pass a law that clarified the need for an explicit state-level vote to adopt Chapter 9 until 1994. In addition, even if states recognized the need to allow for Chapter 9, it could be that booming general economic conditions suggested that states did not need to vote quickly to authorize Chapter 9. Kimhi (2010) documents that between 1976 and the beginning of 2009 only about 40 general purpose municipalities filed under Chapter 9, and only about thirty filings were approved. During the same period, approximately 180 single-purpose districts – governmental entities, such as water districts or park districts – filed under Chapter 9.

Prima facie, pure inertia is implausible as a comprehensive account of the data, given the large variation in the likelihood and speed of Chapter 9 authorization. Of the 27 states that authorized municipal bankruptcy as of 2012, 19 of them did so by 1990, well before Congress clarified the need for an explicit vote. Only 6 states voted to authorize between 1993 and 1996. And Georgia and Iowa had already voted to explicitly ban municipal bankruptcy. Also, Kimhi's empirical study of municipal bankruptcies is of course inconclusive on whether the low number of filings was due to demand or supply factors. On the other hand, the Great Depression was certainly instrumental in the introduction of the first federal municipal bankruptcy law (e.g., McConnell and Picker (1993); see also Section IV.B below); and Michigan voted to authorize Chapter 9 in 2011

in direct response to the financial crisis and to Detroit's and other cities' financial distress, so it is definitely plausible that general economic conditions may play a systematic role in the decision to authorize Chapter 9. Therefore, to analyze whether differences across states and time in general economic conditions account for the likelihood and timing of the adoption of Chapter 9, in our empirical tests we include a host of variables related to general economic conditions, such as growth in GDP per capita, growth in unemployment, and growth in housing prices. Under the *economic efficiency* view of financial reform, reforms should occur earlier in states where economic conditions deteriorate more, in terms of lower growth in GDP per capita and higher unemployment. Also, reform should occur earlier in states that experience declines in house prices, because house price busts may decrease liquidity for homeowners, which may hamper tax revenues and make debt unsustainable.

*Labor unions.* Many local government officials are either directly dependent upon labor union support for (re)election, or seeking to avoid union opposition (Moe (2006), Gillette (2011), Trotter (2011)). Furthermore, government officials and public sector employees negotiate for salary increases, pension,<sup>14</sup> and health care benefits and new hires, and these collective bargaining agreements are usually insulated from the overall economic climate, thereby remaining constant during periods of booms and recessions (Moe (2006)). However, debt reorganization plans under Chapter 9 can include the rejection of executory or onerous contracts – such as collective agreements with government officials and public sector employees – and can be crammed down on dissenting classes as long as one impaired class approves the plan. Gerrymandering of classes makes it likely that municipalities undergoing Chapter 9 will be able to find at least one such impaired class. As a result, labor unions are generally seen as the losers in Chapter 9 reorganizations (Gillette (2012b)), which explains the strong and vocal opposition of labor union representatives to recent high profile Chapter 9 filings (e.g., San Bernardino, Detroit; see Stech (2012)).<sup>15</sup>

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<sup>14</sup> Along similar lines as labor unions, another factor that may drive Chapter 9 authorization is thus the extent of underfunding of state pension liabilities (e.g., Spiotto (2014)). Unfortunately, data on the underfunding of state pensions is only available for recent years, and is not considered to provide an accurate depiction of the true extent of state-level pension liabilities (Novy-Marx and Rauh (2011)).

<sup>15</sup> As a recent illustration, Governor Mitch Daniels of Indiana proposed to adopt Chapter 9 to avoid costly labor contracts (Devitt, 2010), but the proposal did not go through. To date, Indiana has not adopted Chapter 9.

Under the *political economy* view of financial reform, therefore, reform should occur later in those states where the strength of labor unions is greater. Under the *economic efficiency* view, on the other hand, reform should occur earlier in those states where the strength of labor unions is greater, as the benefits of debt renegotiation are likely directly related to the size of labor unions.

*Bondholders.* Bondholders are typically secured, organized, and often senior holders of municipal debt claims, so they tend to benefit in reorganization plans. Of course, in any given bankruptcy filing bondholders' claims may be adjusted, which implies that they will argue their cases in court. On the whole, they tend to benefit from a regime with a bankruptcy law, such as Chapter 9, relative to a regime without a bankruptcy law in which their claims would most likely be more impaired (e.g., McConnell and Picker (1993), Gillette (2012a, 2012c)). As a result, municipal bondholders typically support the passage of Chapter 9. Under this *political economy* view, reform should occur earlier when bondholders' demand for bankruptcy law is larger.

*Judicial Efficiency.* Bankruptcy cases are large and complex, particularly in the case of large municipalities. In addition, all bankruptcy cases are heard in federal courts at the state level. Therefore, adopting Chapter 9 and allowing for municipal bankruptcy filings is likely to add a significant burden to already busy bankruptcy courts. Prior empirical studies on judicial efficiency document that the number of cases each judge handles annually is a negative predictor of the efficiency of court decisions (Bermant, Lombard, and Wiggins (1991)).

Ayotte and Yun (2009) and Gennaioli and Rossi (2010, 2013) show theoretically that efficient courts are a prerequisite to successful bankruptcy reform. Ponticelli (2014) finds support for this proposition by documenting that the Brazil bankruptcy reform produced significantly better outcomes in those districts with a less busy / more efficient judiciary. Iverson (2014) similarly finds that busier judges promote less efficient outcomes following the passage of a Bankruptcy Act that exogenously increases caseload. Following this logic, state legislatures may be more comfortable authorizing Chapter 9 and allowing municipal bankruptcy filings after courts become more efficient. Therefore, under this *judicial efficiency* view, financial reform should occur later in states where judges have a larger caseload. Under the *economic efficiency* view, on the other hand, reform should occur earlier in states where judges have a larger caseload, as a larger caseload would indicate a worsening of general economic conditions whenever the number of bankruptcy judges is constant over time.

*Local identity/trust in local government.* Gillette (2012b) underscores that municipalities may seek to adjust debts under Chapter 9 either opportunistically or strategically, for example because they lack the political will to adopt difficult fiscal adjustments. Essentially, local officials of municipalities that enter bankruptcy retain political authority over municipal fiscal affairs, while at the same time imposing externalities on other municipalities or more centralized levels of government. State governments may therefore impose harsh restrictions on local officials as a condition for bailout, up to the point of not authorizing Chapter 9 at all. If concerns for strategic filing are important, Chapter 9 authorization should occur earlier if the extent of local – relative to national – identity, and trust in local – relative to federal – government are higher.

On the other hand, while filing for bankruptcy is an option and not an obligation, adoption of Chapter 9, which is a federal law, may be seen as a diminution of local communities and of the powers and prerogatives of the state, and some state legislatures may therefore oppose it. This has been argued to be the case in Georgia, which explicitly banned municipal bankruptcy (e.g., McConnell and Picker (1993), Briffault (1990), Williams (1986)). Under this political science view, therefore, Chapter 9 adoption should occur later if the extent of local – relative to national – identity and trust in local – relative to federal – government are higher.

*Political/Institutional factors.* Republicans are typically perceived as more likely to favor financial reform than Democrats. However, in the contest of municipal bankruptcy, it was mostly Democrats that supported the introduction of the first federal bankruptcy law in 1933-34—an issue to which we return in Section IV.B below. In general, political-institutional theories imply that reform should occur earlier in states controlled by a party that supports reform (e.g., Poole and Rosenthal (2007)). We investigate whether reform becomes more likely when the same party controls both the houses of state legislatures and the governorship. Of course, any such political party effects may simply reflect the economic interests of the state constituencies (see Peltzman (1984)).<sup>16</sup>

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<sup>16</sup> We also investigate the role of other interests, such as the insurance and the banking industry, and we examine empirically whether reform occurs earlier or later in states with a large size of the insurance industry, or in states with a large demand for banking services. Theoretical predictions are ambiguous in both cases. With respect to the insurance industry, by decreasing bond spreads Chapter 9 may either complement or substitute for bond insurance, so that the prediction on whether insurance companies stand to gain or lose from Chapter 9 is ambiguous. With respect to banking, by providing positive spillovers to the private sector Chapter 9 may either enable more small firms to tap banking services, or alternatively it may crowd out bank lending to those firms. As a result, also the prediction on the effect of Chapter 9 authorization on the demand for banking services is ambiguous.

### *III.C Variable Definitions and Data Sources*

In this section we describe our empirical variables and the data sources used to construct them.

The fraction of labor union coverage among total employees in each state from 1980 to 2014 is obtained from unionstats.com, based on Hirsch, Macpherson, and Vroman (2001) and Hirsch (2008). In our main specifications we focus on public labor unions, as they are direct losers from Chapter 9 authorizations. On the other hand, it is plausible that in some cases both public and private unions may end up on the same side of the reform. Therefore, we also perform tests based on the total membership of public and private unions, and we find similar results.

We measure court efficiency as the number of bankruptcy filings per judge in each state, based on work by Bermant, Lombard, and Wiggins (1991), which finds that the effectiveness of bankruptcy courts depends critically on judges' caseloads. The number of bankruptcy filings from 1980 to 2011 is available from the American Bankruptcy Institute. The availability of this data determines the sample period of our study. We obtain the number of judges from 1984 to 2012 from the Office of the Law Revision Counsel (U.S. Code Title 28, Part I, Chapter 6, Section 152). Since the number of judges in each district typically remains stable over several years, the number of judges is then backfilled to 1980 using 1984 numbers.

To measure the level of local – relative to national – identity, and of trust in local – relative to national – government, we rely on the World Values Survey (WVS), which interviewed varying-sized samples of people in the U.S. in 1995 and 1999. In each of these surveys, at least 1,500 U.S. individuals were asked “To which of these geographical groups would you say you belong first of all?” Crucially for our purposes, i) respondents can choose between their local town, their state, or the U.S. as a whole, and ii) responses are stratified by U.S. region. Therefore, we construct our measure of local identity as the ratio of the number of those who responded either “locality or town where you live” or “state or region where you live”, divided by the number of those who responded either “locality or town,” “state or region,” or “the U.S. as a whole.” We backfill the data to the closest survey date.

While arguably more direct, other measures of trust present shortcomings. In fact, survey questions often focus on social rather than political trust;<sup>17</sup> and questions on political trust either

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<sup>17</sup> Trust in government is identified as a crucial feature of democratic societies by political scientists such as Miller (1974), Citrin (1974), and Lipset and Schneider (1983), among others. On the other hand, sociologists since Banfield

do not distinguish between local and federal government,<sup>18</sup> or are not stratified by U.S. region, or both. As a result, in our empirical analysis we focus on local identity.<sup>19</sup>

Other state-level characteristics are the total outstanding level of debt (state debt), real GDP per capita, the unemployment rate, and housing prices. Information on state debt, which measures the size of the municipal bond market, is obtained from the U.S. Census Bureau. (The U.S. Census Bureau's Compendia database was discontinued in 2012, which limits the availability of state-level data.) Information on real GDP per capita is obtained from the Bureau of Economic Analysis. Information on unemployment rates is obtained from the St. Louis Fed (FRED). Information on housing prices (the average price of all transactions in each state) is obtained from the Federal Housing Finance Agency.

If any of the interest groups identified above is uniquely represented by one political party, then the identity of the party in control of both state houses and the governorship may matter. Accordingly, we include i) an indicator variable for the Democratic Party controlling both state houses and the governorship, and ii) an indicator variable for the Republican Party controlling both state houses and the governorship. Our purpose is to examine whether political control of both state houses and governorship soaks up the effect of any of the interest groups identified above. Information on the identity of the controlling party in both houses and on the party of the governor is obtained from the National Conference of State Legislatures and the National Governors Association websites.

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(1958) have focused on social trust (e.g., Putnam (1995)). Following Coleman (1990, pp. 306-307) and others who have argued that trust is an essential part of social capital, Guiso, Sapienza and Zingales (2004) and others proxy social capital with survey measures for generalized trust. Although the social capital literature often focuses on social trust rather than political trust, other work argues that the concepts of social and political trust are closely related (e.g., Lane (1959); Putnam (1993); Levi (1996); Brehm and Rahn (1997)).

<sup>18</sup> E.g., questions on trust in government include "People have different views about the system for governing this country. Here is a scale for rating how well things are going: 1 means very bad and 10 means very good" and "could you tell me how much confidence you have in government: is it a great deal of confidence, quite a lot of confidence, not very much confidence or none at all?". On the other hand, the question "how do you rate your local government?" was asked in some years in some countries but not in the U.S.

<sup>19</sup> To be sure, identity and trust are also closely linked together (e.g., Akerlof and Kranton (2000), Bowles and Gintis (2004), Landa (1994)). Recently, Freitag (2003) reports that local identity is a strong positive predictor of trust; Michelson (2003) shows that acculturation predicts reduced political trust at the national level, and Wenzel (2006) shows that such a negative correlation between acculturation and political trust vanishes at the local level. In our data, local identity is strongly positively correlated with the fraction of people who answered with a rating of 7 or higher to the question "People have different views about the system for governing this country. Here is a scale for rating how well things are going: 1 means very bad and 10 means very good". The cross-sectional correlation coefficient between these two variables is about 60% in the 1999 survey.

Next, we turn to measuring bondholders interests, which is challenging because we do not observe direct holdings of municipal bonds. In our main specification, we focus on the level of the state debt outstanding – the larger the debt outstanding, the larger the bondholders’ interests. At the same time, however, the level of state debt outstanding may also proxy for general economic conditions. In an effort to probe deeper into the source of the explanatory power of state debt, therefore, we perform additional tests in which we include variables that measure the presence of interest groups that might hold municipal bonds, namely, mutual funds, insurance firms, and small banks. Therefore, we include a dummy variable for the presence of municipal bond mutual funds. Further, we measure the size of the state insurance sector as the value-added share of the insurance industry relative to state GDP. Data on value-added by industry are from U.S. Commerce Department, Bureau of Economic Analysis, Survey of Current Business. And we measure the relative importance of small, bank-dependent borrowers as the proportion of all establishments operating in the state with fewer than twenty employees, from the U.S. Census Bureau.

Finally, we introduce a dummy variable that equals one if a neighbor state authorizes Chapter 9, to capture learning/network effects among neighbor states.

For the bond-level analysis, we consider credit spread, which is the difference between the offering yield of each municipal bond and the T-bill rate of corresponding maturity at the same period. Other bond characteristics include the bond amount, maturity, whether a bond is investment grade, and whether a bond has a rating. Bond amount is the natural logarithm of the bond issuance amount. Maturity is the natural logarithm of maturity measured in number of days. Investment grade is an indicator that equals 1 for investment grade bonds. Credit rating is an indicator that equals 1 for having a credit rating by at least one of S&P, Moody’s, and Fitch. The credit spreads, bond issuance amounts, maturity, and credit ratings are obtained from the Mergent Municipal Bond Database. Information on the T-bill rates is obtained from the St. Louis Fed (FRED). Data prior to 1990 is not comprehensive and our data ends in 2010, which limits our bond-level study to the period 1990-2010. Our final sample comprises 1,402,187 bond issuances.

For the analysis of spillover effects from state finances to firms, we consider the 1,705 Compustat firms that operate in only one of the 33 states (Arkansas, North Dakota, and West Virginia have no single state firms in our sample) that had not adopted Chapter 9 already by 1980. We focus on these single-state firms because they are mainly affected by the economic condition

of the state they belong to. Following García and Norli (2012), we identify single-state firms by collecting firms that only mention one state in their 10-K filings. The dependent variables in our firm-level analysis are sales computed as total revenue divided by assets, profitability computed as net income divided by total assets, investment rate computed as the annual change in the natural logarithm of capital expenditures divided by total assets, and dividends computed as total dividends divided by total assets. The control variables are firm size computed as the natural logarithm of assets, book leverage computed as the sum of long-term debt plus existing debt in current liabilities divided by total assets, and market-to-book ratio computed as the quasi-market value of assets divided by total assets. Quasi-market value of assets is computed as the book value of assets plus the market value of common equity minus the book value of common equity and deferred taxes. Market value of equity is the price (close) times the number of common shares outstanding. Information on assets, revenue, net income, capital expenditures, dividends, long-term and short-term debt, price, the number of common shares, the market value of common equity, and deferred taxes are obtained from Compustat.

The extent of the spillover of public finance to the private sector is measured by the annual aggregate amount of municipal bonds for non-governmental use divided by the total municipal bond issuance amount in each state. Information on municipal bond issuance amounts and non-governmental use are obtained from Mergent Municipal Bond Database.

### *III.D Summary Statistics*

Table 2 describes the summary statistics of the variables used in this paper. Panel A shows the mean, standard deviation, and 5/50/95 percentile values of the variables. On average, 39.3% of states' public employees are members of a union (median is 37.2%). There is substantial variation in union membership, as the standard deviation is 15.5%, the fifth percentile is 16.5%, and the ninety-fifth percentile is 61.8%. State bankruptcy courts have on average 5 judges, again with large variation ranging from 1 judge in the fifth percentile to 13 judges in the ninety-fifth percentile. There is also considerable variation in court inefficiency measured by caseloads per judge. A typical state judge handles 2,559 cases per year on average, ranging from 540 cases in the fifth percentile to 6,138 cases in the ninety-fifth percentile.

In terms of local identity, an average of 51.4% of the people surveyed responded that they first belong to their local city or region rather than to their country, with variation ranging from

37.5% in the fifth percentile to 62.2% in the ninety-fifth percentile. The mean of GDP per capita is 0.04, with standard deviation of 0.01. The unemployment rate has a mean of 6% and a standard deviation of 2.3%.

Moving on to the bond-level variables, credit spread is often negative, which reflects both the fact that municipal bonds are in general safe assets and that in addition they often carry tax exemption benefits (e.g., Green (1993), Chalmers (1998), Poterba and Rueben (1999)). The mean credit spread is -0.42% and the standard deviation is 0.80%. About two-thirds (66.8%) of the municipal bonds considered in this study have an investment grade rating and 66.9% have credit ratings.

With respect to the firm-level variables, the average firm has total revenues close to total assets, with a mean sales of 1.06. Average profitability is negative (-0.27), but the median firm has positive profits (0.013). On average, firms invest 5.5% of their assets in capital expenditures and pay out 2% of their assets in dividends. Firms in this study have a mean book leverage of 0.42 and market-to-book of 4.38.

Panel B shows the mean, standard deviation, and 5/50/95 percentile values of changes in the natural logarithm of variables from the prior year. The fraction of union membership (decile) is reasonably stable, with a growth rate of 0.1%, but there is a large standard deviation of 22.5%. Also, the mean of the growth rate of judicial inefficiency is 3.5%, again with a large standard deviation, 31%. The mean of GDP per capita growth is 1.7%, with a rather small standard deviation of 3.98%. Unemployment rate growth has a mean near zero (0.64%), with a very large standard deviation (18.3%). The growth rates of firm-level variables (sales, profitability, investment, and dividends) typically have low means, around 0 to 5% below or above zero, but the variation is very large, with a standard deviation ranging from 60% to above 90%. The relatively large variation of single-state firm-level growth rates (e.g., sales, net income, investment, and dividends) compared to those of state-level variables (e.g., GDP per capita, unemployment rate, and housing price) suggests that single-state firms experience larger fluctuations across different economic conditions than states. As a result, states are likely to be in a better position to help local firms, especially during the downturns of the business cycle.

Table 3 shows correlations among the key variables that may influence the decision to authorize Chapter 9. Changes in labor union memberships are weakly correlated with other

variables, typically less than 1%. In contrast, caseload per judge has a higher correlation with other variables, such as changes in GDP per capita (-11%), changes in the unemployment rate (12%), and changes in housing prices (-16.7%). The log of state debt is positively correlated with both the likelihood that both houses are controlled by the Democratic Party (18%) and deficit per GDP (11.9%). Local identity (decile) is weakly correlated with most variables, but is strongly negatively correlated with the log of state debt (-18.2%) and positively correlated with state GDP growth (10.3%). Changes in GDP per capita is strongly negatively correlated with changes in the unemployment rate (-43%). Finally, economic variables, such as changes in GDP per capita and changes in the unemployment rate, are very strongly correlated to each other, as expected. Interestingly, changes in caseload per judge are negatively correlated with economic conditions, as the number of bankruptcy cases tends to be countercyclical and the number of judges remains stable over time.

#### *IV Empirical Strategy and Results*

This section describes our empirical strategy and reports our results. Section IV.A reports our main results on the decision to adopt Chapter 9. Section IV.B reports results on congressional voting and Section IV.C reports results on municipal bond spreads and on the spillover effects at the firm level.

##### *IV.A Decision to Adopt Chapter 9*

In this section, we examine the timing of passing Chapter 9. We consider the period from the beginning of our sample (1980) until the adoption of Chapter 9 as the ‘duration of reform,’ or the ‘time until reform’. The hazard rate,  $h(t)$ , is the likelihood that a state adopts Chapter 9 at time  $t$ , given that the state has not yet done so (Kalbfleisch and Prentice (2002), Kiefer (1988) and Greene (2008)). The hazard rate function takes the form

$$h[t, \mathbf{x}(t), \boldsymbol{\beta}] = h_0(t) e^{\mathbf{x}(t) \cdot \boldsymbol{\beta}}$$

where  $\mathbf{x}(t)$  is a vector of time-varying covariates,  $\boldsymbol{\beta}$  is a vector of unknown parameters to be estimated, and  $h_0(t)$  is the baseline hazard function. Following Cox (1972, 1975), we can use the partial-likelihood approach to estimate  $\boldsymbol{\beta}$  without specifying the form of the baseline hazard function  $h_0(t)$ . To estimate the parameters, we thus maximize the following log-likelihood function (Kiefer (1988)):

$$L(\beta) = \sum_{i=1}^N d_i \ln h[t_i, \mathbf{x}(t_i), \beta] - \ln \sum_{i=1}^N \int_0^{t_i} h[v, \mathbf{x}(v), \beta] dv$$

where  $N$  indexes the number of states that have not yet adopted Chapter 9 at the beginning of the sample period;  $d_i$  is an indicator equal to one for the states that adopted Chapter 9 by the end of the sample period and zero for the states that did not (the censored observations); and  $t_i$  is the time of adoption of Chapter 9 for the  $i$ -th state. In order to account for differences in the adoption of Chapter 9 by each state, we use a stratification model, which allows the baseline hazard function,  $h_0(t)$ , to differ across states. We assume that observations of the same state over time are not necessarily independent. We use a robust estimation procedure for the standard errors, following Lin and Wei (1989).

We exclude states that adopted Chapter 9 before 1980 from the analysis, so  $N$  equals 36 states. During the sample period, 13 states out of the 36 adopt Chapter 9, so we have 23 censored observations, or 21 if we consider Georgia and Iowa as having decided not to adopt Chapter 9 and thus exclude them. Our results are not sensitive to the inclusion or exclusion of Georgia and Iowa. In the baseline specification we include both, consistent with the view that both states might still decide to overturn their current legislation and adopt Chapter 9 in the future. Furthermore, our results remain strongly statistically significant if we exclude the censored observations and focus on the 13 states that adopt Chapter 9 during our sample period.<sup>20</sup>

Table 4 reports the results and shows the resulting hazard rate for adopting Chapter 9. Coefficient magnitudes are interpreted relative to the baseline probability of adoption of Chapter 9. Column I shows the hazard rate of changes in public union coverage on adopting Chapter 9. The hazard rate is -1.67 and is statistically significant at the 5% level. This result indicates that an increase in union membership by one standard deviation (22.51%) in one period implies a 31.3% lower likelihood of adopting Chapter 9 in the next period. In Column II, the hazard rate on changes in judicial caseload is -2.38 and statistically significant at the 5% level, which indicates that a one standard deviation increase (30.74%) in judicial caseload implies a 51.8% lower likelihood of passing Chapter 9 by the next period. In Column III, the hazard rate on local identity is 0.20 and

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<sup>20</sup> In these unreported regressions, the economic significance of labor union growth, state debt, and trust in local government is very similar to the results reported in Table 4, while the economic significance of court efficiency is lower. All variables remain statistically significant.

statistically significant at the 5% level, which indicates that a one standard deviation increase (2.79) implies a 73.97% higher likelihood of passing Chapter 9 by the next period. In Column IV, the hazard rate on state debt is 0.60 and statistically significant at the 10% level, which indicates that a one standard deviation (0.107) increase implies a 6.65% higher likelihood of passing Chapter 9 by the next period. Column V includes all four variables together and shows that they maintain the same sign and their statistical significance is, if anything, stronger. Column VI includes other potential factors that could drive the adoption of Chapter 9: GDP per capita growth, unemployment growth, house prices growth, and an indicator for whether the municipal bonds have the triple-A rating. Even after controlling for these additional factors, the first four factors remain statistically significant, with the same sign and similar magnitude as in earlier columns. The additional variables have the predicted sign but are not statistically significant. For example, reforms occur on average earlier when GDP per capita growth is lower, but the p-value is 20%. Column VII investigates political factors that could drive Chapter 9 authorization, such as Democrat control and Republican control. Column VIII reports the results for the full specification. Reform occurs on average earlier when the same party controls both houses and the governorship, but these effects are not statistically significant. Even after controlling for all additional factors, the first four factors remain statistically significant, with the same sign and similar magnitude as in earlier columns. None of the other variables are statistically significant.

To obtain a log expected time metric for the expected time to adoption of Chapter 9 for a given change in the covariates, one needs to impose structure on the baseline hazard rate,  $h_0(t)$ . One popular way to do this is the Weibull model, according to which one can invert the hazard function and map it into the time domain, so that the log of the time to reform  $T$  is a linear function of the economic and political factors and an error term,  $\ln(T) = \mathbf{x}'\mathbf{b}^* + e$ . Under this formulation, the baseline hazard rate is assumed Weibull with a shape  $pt^{p-1}$  characterized by the parameter  $p$ , so that  $\mathbf{b}^* = -\mathbf{b}/p$ . Estimates from a Weibull model (untabulated, available upon request) indicate that a one standard deviation increase in public union membership implies a 40 percent increase in the time until reform, or about 5.5 years; a one standard deviation increase in caseload per judge implies a 30 percent increase in the time until reform, or about 4.25 years; a one standard deviation increase in the log of state debt implies a 6 percent decline in the time until reform, or about 10

months; and a one standard deviation increase in local identity implies a 50 percent decline in the time until reform, or about 7 years.<sup>21</sup>

Overall, the evidence from Table 4 indicates that court efficiency, local identity, and interest groups such as labor unions and bondholders play a significant role in the Chapter 9 adoption process. The results are difficult to reconcile with a view based purely on states' inertia, or with a view of financial reform driven purely by changing economic conditions. At the same time, the fact the reform occurs earlier when state debt outstanding is larger, while consistent with the importance of bondholders' interests, is consistent with the economic efficiency view, in that the same (perhaps unobserved) economic conditions that explain a larger debt may also drive Chapter 9 authorization. To probe into the reasons for the explanatory power of state debt, we include in additional specifications some variables for specific interests that may be holders of municipal bonds. Natural candidate holders of municipal bonds are muni mutual funds, which have entered different states at different times. Therefore, we include an indicator variable that equals 1 if in the state-year at least one muni mutual fund is present.

Table 5 present the results. Column I report the univariate specification with state debt, for comparison with Table 4. Column II adds the indicator variable for the presence of muni mutual funds. Interestingly, now muni mutual fund is positive and significant, and state debt becomes insignificant, indicating that states adopt Chapter 9 earlier if muni mutual funds are present in the state. Columns III, IV, and V add the other drivers of Chapter 9 authorizations from Table 4, namely public labor union growth, judicial caseload growth, and local identity; the economic conditions, such as GDP p.c. growth, unemployment growth, house prices growth, AAA rating; and also adds variables for other interests, such as the insurance industry and the small local banks, which might also be holding municipal bonds. Interestingly, once we control for the presence of muni mutual funds, the presence of a triple-A rating predicts a slower authorization of Chapter 9. Overall, public labor unions, judicial caseload, local identity and muni mutual funds remain stable predictors of Chapter 9, with similar economic and statistical significance as in the prior tests.

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<sup>21</sup> Relative to the Cox model that leaves  $h_0(t)$  unspecified, the cost of the Weibull model is the need to impose additional distributional assumptions on the baseline hazard rate that may not be supported by the data, and to estimate additional parameters. In our data, the coefficients estimated by the Cox and Weibull models have the same sign and are very similar in terms of statistical significance, but somewhat different in terms of magnitude.

Next, we add an indicator variable for whether a neighbor state has authorized Chapter 9, to examine whether there are learning or network effects in financial reform. Consistent with the visual map of Chapter 9 authorizations, which shows no geographic clustering, we find that such learning or network effects are not present (if anything, the sign is negative – a neighbor authorizing Chapter 9 implies slower adoption).

Finally, we turn to labor unions and consider an alternative formulation that includes total membership of both public and private labor unions – while the effect of Chapter 9 on labor unions is direct, it is reasonable to expect that in some cases private unions will be on the same side of the fence as public ones. In Column VII we find that also the new labor union variable is negative and significant. All other results are unchanged.

#### *IV.B Voting on Municipal Bankruptcy Law in the U. S. House of Representatives and Senate*

We now examine whether the forces driving intrastate authorization of Chapter 9 also drive interstate adoption of the federal municipal bankruptcy law. As noted above, the key votes on the federal bankruptcy law for municipalities in Congress occurred in the 1930s, when the first federal bankruptcy law was created; in the late 1970s, when Chapter 9 was introduced as part of the 1978 Bankruptcy Code; and in 1994, when the requirement for state authorization was made explicit. Some of these key votes concerning municipal bankruptcy were either voice votes, so the voting data is not available, or were not exclusively about municipal bankruptcy (e.g., votes on the 1978 Bankruptcy Code and on the 1994 bankruptcy reform were not only about Chapter 9, but also about the other Chapters, including Chapter 7 and 11 about corporations, and 13 about personal bankruptcy), or extremely lopsided, so we could not estimate a voting model from them.

In addition, a number of bills and amendments related to Municipal Bankruptcy Law have been debated in Congress at various points in time – most recently, an amendment to Chapter 9 to introduce protection for employees and retirees in Chapter 9 (H.R.95, 114<sup>th</sup> Congress, sponsor Rep. John Conyers, Jr. [D-MI-13], introduced 01/06/2015) – but a systematic search of the Congressional Records produced only two roll-call votes related to municipal bankruptcy law. These were the votes in the House on 6/9/1933 and in the Senate on 5/1/1934. The House voted on the Kurtz attempt to recommit to the committee the bill that introduced the first municipal bankruptcy law. The vote rejected the recommitment and thus supported the bill, by 191-172. The House then passed the bill via a voice vote shortly thereafter. One year later, the Senate voted on

the Bankruptcy bill, and approved it with a 45-28 majority. To be sure, the Great Depression provided a major impetus for the bankruptcy bill. At the same time, there was considerable variation in voting behavior that was not dictated by ideology and did not occur through strict party lines, as documented by Poole and Rosenthal (2007) and Berglöf and Rosenthal (2000): while Democrats were more likely to support the bill than Republicans, both parties were significantly split on the issue.

Interestingly, the Congressional Record of the 1933 House vote reports the arguments used for and against the bill in the discussion prior to the House vote (see also Berglöf and Rosenthal (2000)). Arguments for the bill emphasized predominantly the need to avoid holdup by some of the state creditors; and that the contracts clause of the Constitution clearly forbid state intervention in the bond contracts whereas the bankruptcy clause permitted federal interventions. Arguments against the bill emphasized the concern that the bill would encourage strategic default by otherwise solvent municipalities, thereby generating negative externalities on neighboring communities. More broadly, many of the arguments emphasized concerns about the likely wealth effects of a bankruptcy law for municipal bondholders and for workers.

The above discussion already suggests that many of the forces at play at the federal and congressional level for the introduction of the first bankruptcy law in the early 1930s were similar to those operating half a century later for the state-level authorization of Chapter 9. To check more formally for the influence of the factors that we have considered in the state-level authorizations, we examine the actual congressional votes on these matters. One challenge is that data limitations prevent the estimation of a model of congressional voting that exactly mirrors the hazard models reported in Tables 4 and 5 (for example, World Value Surveys on trust and identity did not start until the 1960s).

Table 6 reports estimates of a probit model where the dependent variable equals one if the legislator voted in favor of the amendment and zero otherwise. The coefficients in Table 6 are the marginal effects (“slopes”) of a unit change of each variable on the probability that a legislator will vote for the amendment. Note that a force favoring financial reform has a positive coefficient in the hazard model (Tables 4 and 5) and in the probit model of column (II) of Table 6 (the Senate vote for passing the bill), but a negative coefficient in the probit model of column (I) of Table 6 (the House vote to recommit the bankruptcy bill to the committee).

We use labor unions, judicial caseload, and state debt as main explanatory variables. Labor unions is total union members divided by population in each state as of 1929. The state-level total union members are constructed from union-level membership data from the Handbook of American Trade Unions published by the Bureau of Labor Statistics (1929).<sup>22</sup> This Handbook reports total membership for each national union organization and the number of affiliated local unions in each state. Following Troy and Sheflin (1985), we estimate the number of members of each union in each state by weighting total membership of each union with the fraction of affiliated local unions in each state. Then, we aggregate members of each union for each state to obtain the total number of union members in each state. State-level population data is obtained from the U.S. Census Bureau. Judicial caseload is the number of government units in default in 1932-34 (from Hillhouse (1936)) divided by the number of federal judges in the state district (from the Federal Judicial Center).<sup>23</sup> State debt is the log of state debt in 1932 (from the U.S. Census Bureau).

As additional explanatory variables, we use a party affiliation indicator variable equal to one for Democrats and zero for Republicans as our proxy for political factors. As our proxies for economic conditions, we use the rate of unemployment and the urban share of the population as our proxy for economic conditions, both computed from the U.S. Census Bureau of 1930; and the percent of large cities in default, computed as the number of cities in default with more than 10,000 inhabitants as a percent of the total number of such large cities, computed from Hillhouse (1936).

Column (I) of Table 6 shows that House representatives from states with a smaller labor union presence, a smaller judicial caseload, and a larger debt outstanding were more likely to oppose recommitting the bill to the committee (and thus support the bankruptcy bill); and Column (II) shows that senators from states with smaller labor union presence and a smaller judicial caseload were more likely to vote in favor of the bankruptcy bill. These findings exactly mirror our findings in Table 4 and 5. The evidence also shows that Democrats, as well as representatives from states with a higher level of unemployment, a lower share of population in urban areas and a larger fraction of large cities in default were more likely to support the municipal bankruptcy law.

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<sup>22</sup> The subsequent edition was published in 1936. Hence, we use the 1929 edition which is the most recent edition prior to the 1933-34 votes.

<sup>23</sup> <http://www.fjc.gov>.

Overall, our analysis of the vote on the first municipal bankruptcy bill supports the economic and political theories of financial reform. It provides a consistency check that many of the forces operating at the congressional level for the passage of the first municipal bankruptcy law in the mid-1930s are very similar to those operating on the state legislatures for Chapter 9 authorization over 1980-2012.

#### *IV.C Bond Financing and Firm-level Outcomes Following Chapter 9 Adoption*

In this section, we explore the ex-post consequences of authorizing the municipal bankruptcy law, Chapter 9. Sub-section IV.B.1 reports the bond-level analysis and IV.B.2 the firm-level results.

##### *IV.C.1. Bond-level Analysis*

After Chapter 9 adoptions, municipal bond investors can benefit from a more efficient bankruptcy process. Hence, we expect the credit spreads of municipal bonds issued after the authorization of Chapter 9 to decrease, compared with the credit spreads of bonds issued by non-adopting states.

Our sample consists of 1,402,187 bond issuances from the Mergent database from 1990 to 2010. Bond issuance data prior to 1990 is not comprehensive and excluded from our study. The dependent variable is the credit spread of each issued bond. Credit spread is the difference between the offering yield of each municipal bond and the T-bill rate of corresponding maturity at the same period. The key variable of interest is a Chapter 9 indicator, which equals one if Chapter 9 is authorized in a state in a given year, and zero otherwise. In a panel regression with state and year fixed effects, the Chapter 9 indicator captures the differences-in-differences estimate of the change in credit spreads after Chapter 9 adoptions relative to the change in credit spreads in states that do not pass Chapter 9. We control for the natural logarithm of bond issuance amounts (Bond amount), the natural logarithm of maturity measured in number of days (Maturity), an indicator for investment grade bonds (Investment grade), an indicator for having a credit rating by at least one of S&P, Moody's, and Fitch (Credit rating), and state and year fixed effects. Standard errors are adjusted for heteroskedasticity and clustered at the state level.

Table 7 presents the results. Columns I to III consider bond issuances from 1990 to 2010. Column I shows results using all bond issuances. The parameter estimate of the Chapter 9 indicator is negative and statistically significant at the 5% level: after passing Chapter 9, credit spreads in authorizing states are 9.5 basis points lower relative to spreads in non-authorizing states.

Column II considers a subsample of revenue bonds. Revenue bonds tie a revenue stream to interest payments, and upon default, this revenue stream can serve as collateral. During municipal bankruptcies, the court can readjust interest payments based on the revenue stream backing the revenue bonds. Investors in revenue bonds can thus be expected to benefit from the bankruptcy process becoming more efficient following the authorization of Chapter 9. As a result, we would expect the effect of Chapter 9 on credit spreads to be larger for revenue bonds. Column II shows that this is the case. The parameter estimate is also significantly negative with a larger magnitude: after passing Chapter 9, the credit spreads of revenue bonds in Chapter 9 authorizing states are 10.5 basis points lower relative to the spreads in non-authorizing states.

Column III considers a subsample of general obligation bonds. In contrast with revenue bonds, general obligation bonds are backed by the tax base of municipalities. In the United States, fiscal federalism protects local (state) government fiscal sovereignty and Chapter 9 has limited ability to force local governments' taxing authority. As a result, even with Chapter 9, it is less certain that investors in general obligation bonds can benefit from Chapter 9 during the debt readjustment processes. Consistent with this logic, Column III of Table 7 shows that for the sample of general obligation bonds, the Chapter 9 indicator is statistically insignificant and has a smaller economic magnitude (a 3.1 basis point reduction in credit spreads).

Columns IV to VI repeat the analysis from the earlier columns for 1990-2005, a time period that excludes the recent financial crisis. Results are qualitatively similar to those in the full sample. The credit spreads of municipal bonds issued after the adoption of Chapter 9 are lower relative to those issued in non-authorizing states. The effect is again concentrated in revenue bond issuances.

Overall, Table 7 shows that Chapter 9 authorization is followed by lower credit spreads, particularly for revenue bonds, whose cash flow streams can be used by the state bankruptcy court to readjust interest payments. The results are consistent with the view that municipal bankruptcy law reduces municipalities' borrowing costs.

#### *IV.C.2 Spillover Effects: Firm-level Analysis*

In the previous section, we find that the credit spreads of newly issued municipal bonds are lower following the authorization of Chapter 9. That is, municipalities enjoy lower borrowing costs after Chapter 9 adoptions. In this section, we further explore firm-level outcomes following Chapter 9 adoption by focusing on firms that operate within a single state. The intuition is that these firms

are more likely to be exposed to local economic conditions than firms operating nationwide. As a result, single-state firms are more likely to be influenced by state policies, such as the authorization of Chapter 9. We focus on a particular spillover channel from local governments to single-state firms in the private sector through conduit bonds. Conduit bonds are municipal bonds whose proceeds are used by non-government entities.<sup>24</sup> When the credit spreads of municipal bonds decrease, we would expect that at least some of the benefits will accrue to the private sector firms that use conduit bonds.

We use panel regressions with state and year fixed effects. As our main dependent variables we focus on sales (total revenue divided by assets), profitability (net income divided by assets), investment rate (growth in capital expenditures), and dividends (total dividends divided by assets). The key explanatory variable is a post-Chapter-9 authorization indicator (Chapter 9), which captures the (differences-in-differences) change in the dependent variables after Chapter 9 is authorized relative to the change in the dependent variables for firms operating in states where Chapter 9 is not authorized. To account for other observable heterogeneity across firms, we control for firm size (natural logarithm of total assets), book leverage (long-term plus existing debt in current liabilities divided by total assets), and market-to-book ratio. Standard errors are adjusted for heteroskedasticity and clustered at the state level.

Table 8 shows results from firm-level panel regressions. For each dependent variable, we consider two different samples: the full sample and the conduit subsample. The conduit subsample considers single-state firms in states where a large fraction of the total outstanding municipal bonds are conduit bonds. This subsample focuses on the firms that are most likely to benefit from the reduction of municipal bond issuance costs because a substantial fraction of the municipal bonds issued are used by non-government entities like these firms.

Columns I to III consider sales. For the full sample, Column I shows a positive but statistically insignificant coefficient on Chapter 9. Column II shows that when we focus on the conduit subsample of firms, which are more likely to benefit from lower costs of municipal bond financing, we find that the coefficient of Chapter 9 is large, positive, and strongly statistically

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<sup>24</sup> For example, the Dallas Cowboys stadium in Arlington was financed by conduit bonds, and in turn the bonds were repaid by taxes on ticket sales. The stadium generated significant spillovers to the local economy, which resulted in the bonds being repaid much earlier than anticipated, even despite the Great Recession (e.g., Mosier (2010)).

significant. After a state authorizes Chapter 9, that state's single-state firms' sales increase by 17.9%. Column III controls for the interaction of state and industry fixed effects, with similar results. One possible explanation for the increase in revenue after Chapter 9 is increased local consumption, i.e., when municipalities experience lower borrowing costs, these municipalities may decrease the effective tax burdens, which may spur consumption by the population and in turn the revenues of single-state firms.

Columns IV to VI focus on profitability. For the full sample, Column IV shows that the profitability of single-state firms located in states that authorize Chapter 9 increases by 25.4% relative to firms in non-authorizing states. Column V shows that this increase in profitability is even higher (33.9%) for the conduit sample. Column VI shows that the results are robust to controlling for the interaction of state and industry fixed effects.

Columns VII to IX focus on changes in the investment rate. Column VII shows that the investment rate increases by 3.9% for the full sample following the adoption of Chapter 9 relative to firms in non-authorizing states, and Column VIII shows that the increase is much higher (7.7%) in the conduit bond subsample. As shown in Column IX, the results are robust to controlling heterogeneity in industry composition across states with the interaction of state and industry fixed effects.

Columns X to XII focus on changes in dividends. In general, changes in dividends are smaller in economic magnitude than changes in sales, profitability, and investment rate: for the conduit sample, dividends per assets increase by 1.0% after the authorization of Chapter 9. As shown in Column XII, the effect is robust to the inclusion of state x industry fixed effects.

Overall, the results in Table 8 suggest that municipalities' reduced borrowing costs spill over to firms operating within a single state. These single-state firms experienced increases in sales, profitability, investments, and dividends following their state's Chapter 9 adoption. These effects are mostly concentrated in states where a large fraction of municipal bond issues are used by non-government entities like private firms.

#### *IV.C.3 Parallel Trends in Bond-Level and Firm-Level Results*

As shown in Tables 4 and 5, Chapter 9 adoption is not random. As a result, interpreting the results on bond-level and firm-level outcomes following Chapter 9 adoption in Tables 7 and 8 requires

caution. In fact, several interpretations can be consistent with the findings of Tables 7 and 8 without necessarily implying that Chapter 9 has a causal, beneficial impact on municipal borrowing costs, or on the state-level economy and local firms' outcomes. Indeed, the same economic conditions that drive Chapter 9 authorization may also explain the observed post-Chapter 9 outcomes (*pure selection bias*); alternatively, only states expecting positive outcomes such as reduced muni bond spreads or improved outcomes by local firms may decide to authorize Chapter 9 (*reverse causality*); finally, there may be some unobserved factors that drive both Chapter 9 authorization as well as the observed state and firm level outcomes in the aftermath of Chapter 9 authorization (*omitted variable bias*).

First, we note that all three channels above (pure selection bias, reverse causality, omitted variables) imply that treated and control bonds and firms do not share parallel trends. However, our test results, as well as the graphical depictions in Figures 1 and 2, show that, indeed, the pre-trends of treated and control groups are indistinguishable, which is inconsistent with the predictions of the three channels above.

The remaining question, as in any differences-in-differences setting, is whether post-treatment trends would have continued to be parallel had it not been for the Chapter 9 authorization by the state. As we discuss below, our empirical design takes several steps to mitigate the concern that such trends may not have continued to be parallel.

*Pure Selection Bias.* A pure selection bias interpretation would imply that only states that expect positive effects of bankruptcy law on bond spreads and local firms' performance authorize Chapter 9. To address pure selection bias, we consider the specific channels through which Chapter 9 operates. Among municipal bonds, revenue bonds generate proceeds that can be used as collateral in Chapter 9 proceedings. As a result, if Chapter 9 reduces the borrowing costs of municipalities, this should be particularly the case for revenue bonds.<sup>25</sup> Indeed, this is what we find in columns II and V of Table 7, supporting the view that pure selection bias does not explain our bond-level results.

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<sup>25</sup> Of course, revenue bonds can still be adjusted in Chapter 9 proceedings; and other bonds, too, such as for example general obligation bonds, can benefit from increased tax revenues following Chapter 9 authorization. These two channels would imply, counterfactually, that we should not find a differential effect of Chapter 9 on revenue bond spreads as opposed to general obligation bonds spreads.

Following a similar logic, firm-level spillovers should be more pronounced in states in which a larger fraction of bonds are conduit bonds, because the proceeds of conduit bonds are directly used to finance the private sector. This is precisely what we find in columns II, V, VIII, XI of Table 8. Importantly, conduit loans are industry-specific (e.g., types of conduits housing, infrastructure, etc.), and in turn industry composition may differ across states. Accordingly, we further control for the interaction of state and industry fixed effects in columns III, VI, IX, and XII of Table 8, and find that our results are robust to this possibility. Therefore, pure selection bias does not explain our firm spillover results.

*Reverse Causality.* Reverse causality would imply that expected bond-level and firm-level outcomes drive financial reform. In the context of Tables 7 and 8, reverse causality would imply that Chapter 9 is authorized when states expect lower credit spreads and/or better performance of local firms. Extending this logic, then, Chapter 9 would appear most likely to be authorized when economic conditions and investment opportunities are expected to improve in the future, driving down the demand for bankruptcy law.

However, this interpretation is at odds with the evidence concerning the determinants of Chapter 9 authorization. To begin, Tables 4 and 5 do control for economic conditions directly, in a variety of ways, and the results show that economic conditions do not predict Chapter 9 authorization. Furthermore, and crucially, Tables 4 and 5 show that Chapter 9 is authorized when demand for bankruptcy law is higher, not lower, in a manner that diametrically opposes the reverse causality interpretation of the bond-level and firm-level results.

*Omitted Variables.* The remaining concern regarding the possibility that trends in bond spreads and firm performance would not have been parallel absent Chapter 9 is one of omitted variables. In principle, observable or unobservable omitted variables could account for both Chapter 9 authorization and the subsequent changes in bond prices and firm-level outcomes. Our empirical design takes steps to address these potential omitted factors in our context, namely, both observed and unobserved economic conditions that drive both Chapter 9 authorization and bond-level and firm-level outcomes.

In particular, unlike country-wide changes to bankruptcy law, Chapter 9 authorizations are binding only for a subset of bonds (re. firms) at a time – those issued (re. doing business) in the state concerned. Therefore, the staggered nature of Chapter 9 authorization provides a set of

counterfactuals for how credit spreads and firm performance would have evolved in the absence of Chapter 9, thereby allowing us to disentangle the effect of Chapter 9 on credit spreads and on firm performance from other forces shaping municipal bond pricing and firm behavior.

We also condition on the changes in standard bond-level covariates of bond spreads in Table 7 (such as amount outstanding, maturity, and whether the bond has a credit rating or is investment grade) and firm-level covariates of Table 8 (such as firm size, market-to-book ratio, and book leverage) that could cause trends to diverge after Chapter 9 for reasons unrelated to the bankruptcy reform. We find that adding such controls has very little effect on the estimated effects of Chapter 9. The implication is that Chapter 9 authorization is essentially random at the bond and at the firm level, in the sense that the passage of Chapter 9 does not coincide systematically with changes in bond and firm characteristics.

Moreover, we note that the economic conditions variables in Tables 4 and 5 do not drive Chapter 9 authorization, and as a result they are not plausible omitted factors in Tables 7 and 8. Subsequently, we consider the economic and political drivers of Chapter 9 authorization, as documented in Tables 4 and 5. We re-estimate Tables 7 and 8 including labor union growth, judicial caseload growth, local identity, and state debt, and we find that the coefficients associated with these new variables are small in magnitude and largely statistically insignificant. At the same time, our results on bond-level and firm-level outcomes in the aftermath of Chapter 9 are unaffected. Table A1 in the appendix reports the results for bond spreads.

The remaining challenge to the parallel-trends assumption is that Chapter 9 authorizations might coincide with other important state-level legal and regulatory changes that could affect bond spreads and firms' behavior or performance. To investigate this, we collect data on changes in state corporate taxes, anti-takeover regulation, bank branching deregulation, and labor regulation. Broadly speaking, in principle Chapter 9 authorizations could be part of a "package" of state-level reforms, and some of these reforms have been shown to affect firm financing and performance (e.g., see Garvey and Hanka (1999) for anti-takeover regulation). Therefore, in principle these reforms could directly or indirectly affect bond spreads and firm performance regardless of Chapter 9.

Table A2 in the Appendix presents our findings. Chapter 9 authorizations almost never coincide with other state-level changes. There is only one tax change in the year in which Chapter

9 is authorized. Moreover, in the years before or after the passage of Chapter 9, state taxes are about as likely to go up as down. There is no anti-takeover regulation that coincides with the passage of Chapter 9, and very few in the years before or after. There are but a handful of bank branching deregulations in the two years after Chapter 9 authorizations. Furthermore, there are no changes to labor regulation in the three years around the passage of Chapter 9. Overall, there does not appear to be evidence that corporate tax changes coincide with other state-level important legal or regulatory changes, further assuaging concerns of potential omitted variables.

## *V. Conclusions*

Economic and political theories of financial reform can account for the pattern and timing of Chapter 9 adoption across U.S. states between 1980 and 2012. Interest group factors related to the relative strength of potential losers (labor unions) and winners (bond investors, particularly mutual funds), courts' efficiency, and trust in non-opportunistic behavior by the local government ex-post explain the timing of Chapter 9 adoptions over the study period. On the whole, the results are difficult to reconcile with pure inertia, or purely with the view that worsening economic conditions drive financial reform.

Our results have implications for municipal bankruptcy reform. We find that municipal borrowing costs are lower and single-state firm performance and investment rates are higher in the aftermath of Chapter 9 authorizations, which suggests that Chapter 9 may disproportionately benefit states in which the economy relies more strongly on local firms. If in those states legislators tried but failed to pass Chapter 9, opposition from political and economic interests might trigger welfare losses. As a result, our results can help understand recent attempts to amend the law in such a way as to remove such political opposition, such as for example the recently proposed amendment to introduce some protection for employees and retirees in Chapter 9 (H.R.95, 114<sup>th</sup> Congress, 01/06/2015).

Similarly, our results carry implications for the debate on state bankruptcy. The recent default by Puerto Rico has highlighted the large costs that loom when government entities face large debt obligations and there is no available mechanism for debt restructuring. Our results thus

support recent proposals for removing barriers to municipal as well as state bankruptcy (e.g., Skeel (2013)).

Beyond the municipal bankruptcy setting, our results may also carry potential implications for calls for global reform in the resolution of sovereign debt crises, and in particular the idea of a sovereign debt restructuring mechanism inspired by the U.S. corporate bankruptcy reorganization law under Chapter 11 of the 1978 Bankruptcy Act (Krueger (2002); Bolton and Jeanne (2007)). Our findings show that, if courts are inefficient, such beneficial reforms may be delayed or not happen at all. Therefore, for developing countries it may be crucial to improve the efficiency of courts before considering more ambitious financial reforms that mimic those in more financially developed countries (e.g., Djankov et al. (2008)).

Most broadly, our results relate back to theories of financial contracting and financial development. Many economic models have sought to explain real-world departures from the Coase Theorem by focusing on asymmetric information and imperfect or costly multilateral bargaining. These approaches have generally maintained the implicit assumption of perfect court enforcement of contracts and property rights. Even approaches that have considered departures from perfect enforcement have focused on frictions such as costly state verification or contract incompleteness, without focusing on the role and on the incentives of judges. In these settings, the only hurdle to financial reform is typically represented by the (transaction) cost of devising and drafting an appropriate piece of legislation.

To the contrary, our results highlight that financial reforms do not occur in a vacuum. They are significantly shaped not only or even not primarily by economic conditions, but particularly by the political landscape and by the efficiency of courts. Even reforms that are theoretically and practically sound in financial and economic terms may be delayed or even blocked if interest groups can gather enough political consensus, or if courts are not yet able to handle complex litigation, consistent with recent theories of financial contracting and financial development that focus on political economy factors and on the efficiency and incentives of courts. At the most basic level, enabling courts to enforce the complex legislation of financial markets is one of the main challenges of globalization.

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**Table 1. List of States Adopting Chapter 9**

This table shows the states that authorized or banned Chapter 9 between 1980 and 2012. <1980 indicates states that adopted Chapter 9 prior to 1980. State code is the relevant code that indicates adoption or ban of Chapter 9.

State	Chapter 9	State code
Alabama	Authorization (<1980)	Ala. Code § 11-81-3
Alaska		
Arizona	Authorization	Ariz. Rev. Stat. Ann. §35-603
Arkansas	Authorization	Ark. Code Ann. § 14-74-103
California	Authorization	Cal. Gov't Code §53760
Colorado	Authorization (<1980)	Colo. Rev. Stat. §§32-1-1403, 37-32-102
Connecticut	Authorization	Conn. Gen. Stat. § 7-566
Delaware		
Washington DC		
Florida	Authorization (<1980)	Fla. Stat. § 218.01
Georgia	Ban	Ga. Code Ann. § 36-80-05
Hawaii		
Idaho	Authorization (<1980)	Idaho Code Ann. §67-3903
Illinois	Authorization	50 Ill. Comp. Stat.320/9(b)(4)
Indiana		
Iowa	Ban	Iowa Code § 76.16A
Kansas		
Kentucky	Authorization (<1980)	Ky. Rev. Stat. Ann. §66.400
Louisiana	Authorization (<1980)	La. Rev. Stat. Ann. §§13:4741 and 39:619-620
Maine		
Maryland		
Massachusetts		
Michigan	Authorization	MCL 141.1523
Minnesota	Authorization	Minn. Stat. § 471.831
Mississippi		
Missouri	Authorization	Mo. Ann. Stat.427.100 [MO S.B. 414]
Montana	Authorization (<1980)	Mont. Code Ann. §§7-7-132 and 85-7-2041
Nebraska	Authorization	Neb. Rev. Stat. § 13-402
Nevada		
New Hampshire		
New Jersey	Authorization (<1980)	N.J. Stat. Ann. §52:27-40
New Mexico		
New York	Authorization (<1980)	N.Y. Local Fin. Law §85.80
North Carolina	Authorization (<1980)	N.C. Gen. Stat. § 23-48
North Dakota		
Ohio	Authorization	Ohio Rev. Code Ann.§ 133.36
Oklahoma	Authorization (<1980)	Okla. Stat. tit. 62, §281 & 283 [62 Okl St 286]
Oregon	Authorization	Or. Rev. Stat. §548.705
Pennsylvania	Authorization	53 Pa. Cons. Stat. §11701.261
Rhode Island	Authorization (<1980)	RI Gen Laws §45-9-7
South Carolina	Authorization (<1980)	S.C. Code Ann. § 6-1-10
South Dakota		
Tennessee		
Texas	Authorization	Tex. Loc. Gov't Code Ann. § 140.001
Utah		
Vermont		
Virginia		
Washington	Authorization (<1980)	Wash. Rev. Code §39.64.040
West Virginia		
Wisconsin		
Wyoming		

**Table 2. Summary Statistics**

This table shows summary statistics for the variables used in this paper. Panel A shows mean, standard deviation, and 10/50/90 percentile of variables, and Panel B shows mean, standard deviation, 10/50/90 percentile of the growth rates of these variables. Labor union is the fraction of public labor union coverage among total employees in each state. Number of judges is the number of bankruptcy court judges in each state. Judicial caseload is number of annual bankruptcy filings divided by the number of judges in each state. State debt is the natural logarithm of the outstanding amount of debt in each state. Local identity is obtained from the World Values Survey and is the fraction of people who responded they belong first to their local area or region rather than country. GDP per capita is the real GDP divided by the population of each state. Unemployment rate is the unemployment rate in each state. House price is quarterly from all transaction indexes (fourth quarter). Democrat control is an indicator equal to 1 if the Democratic Party holds a majority in both houses in each state and the governor also belongs to the Democratic Party. Credit spread is the difference between the offering yield of each municipal bond and the T-bill rate of corresponding maturity at the same period. Bond amount is the natural logarithm of the bond issuance amount. Maturity is the natural logarithm of maturity measured in number of days. Investment grade is an indicator equal to 1 for an investment grade bond. Has rating is an indicator equal to 1 for having a credit rating from at least one of S&P, Moody's, and Fitch. Sales is total revenue divided by assets. Profitability is net income divided by total assets. Investments is capital expenditures divided by total assets. Dividends is total dividends divided by total assets. Book leverage is the ratio of the sum of long-term debt plus existing debt in current liabilities to total assets. Market value of equity is the price (at close) times the number of common shares outstanding. Market-to-book ratio is the book value of assets plus the market value of common equity minus the book value of common equity and deferred taxes divided by total assets.

Panel A. Levels

	N	Mean	Std Deviation	5 <sup>th</sup> perc.	Median	95 <sup>th</sup> perc.
<i>State-level variables</i>						
Labor Union	910	39.33	15.52	16.50	37.15	61.80
Number of judges	942	4.58	5.14	1	3.00	13.00
Judicial caseload	942	2,559	1,841	540	2,100	6,138
State debt (\$1,000)	877	15.10	1.06	13.39	15.16	16.96
Local identity	910	4.88	2.79	1.00	4.50	9.00
GDP p. c., 2009 \$m	910	0.037	0.01	0.02	0.04	0.059
Unemployment rate	910	6.01	2.29	3.10	5.60	10.30
House prices	910	203.88	106.44	101.36	174.13	443.37
Democrat control	910	0.495	0.500	0.00	0.00	1.00
<i>Bond-level variables</i>						
Credit spread	1,402,187	-0.421	0.80	-1.60	-0.51	1.08
Bond amount	1,402,187	12.93	1.54	10.60	12.83	15.66
Maturity	1,402,187	7.88	0.80	6.25	8.04	8.88
Investment grade	1,402,187	0.67	0.471	0.00	1.00	1.00
Credit rating	1,402,187	0.67	0.470	0.00	1.00	1.00
<i>Firm-level variables</i>						
Sales	21,269	1.07	1.41	0.07	0.91	2.02
Profitability	21,269	-0.28	22.76	-0.61	0.01	0.13
Investments	20,999	0.06	0.10	0.00	0.03	0.12
Dividends	21,206	0.02	0.20	0.00	0.00	0.03
Firm size, \$m	21,309	4.32	2.38	1.49	4.17	7.51
Book leverage	21,309	0.42	10.30	0.00	0.11	0.50
Market-to-book ratio	21,309	4.38	59.64	0.89	1.63	5.55

Panel B. Growth Rates

	N	Mean	Std Deviation	5 <sup>th</sup> perc.	Median	95 <sup>th</sup> perc.
<i>State-level variables</i>						
Labor Union	874	-0.01	0.10	-0.17	0.00	0.17
Judicial caseload	905	0.03	0.31	-0.26	0.06	0.36
State debt (\$1,000)	809	0.08	0.11	-0.06	0.06	0.24
GDP p. c., 2009 \$m	910	0.02	0.04	-0.04	0.02	0.07
Unemployment rate	910	0.01	0.18	-0.24	-0.03	0.36
House prices	910	0.04	0.07	-0.06	0.04	0.14
<i>Firm-level variables</i>						
Sales	20,283	0.00	0.62	-0.40	0.00	0.42
Profitability	10,128	-0.05	0.93	-0.95	-0.01	0.79
Investments	19,760	-0.06	0.97	-1.12	-0.03	0.98
Dividends	6,514	-0.04	0.80	-0.56	-0.00	0.45

### Table 3. Correlations

This table shows correlations among the variables used in this paper. Description of the variables are provided in the caption of Table 2. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

	Public Labor Union Growth	Judicial Caseload Growth	State Debt	Local Identity	Democrat Control	GDP p.c. Growth	Unemployment Growth	House Prices Growth
Judicial Caseload Growth	0.0137							
State Debt	0.0026	-0.0233						
Local Identity	-0.0105	-0.0128	-0.1819***					
Democrat Control	0.0317	0.0284	0.1811***	-0.0393				
GDP p.c. Growth	-0.0058	-0.1138***	-0.0307	0.1029***	0.0241			
Unemployment Growth	-0.0064	0.1281***	0.0819	-0.0071	-0.0222	-0.4348***		
House Prices Growth	-0.0012	-0.1669***	-0.0101	-0.0176	-0.0389	0.2175***	-0.1115***	
AAA Muni Bond Rating	-0.0067	0.0030	0.1645***	-0.1132***	0.1394***	0.0050	0.0220	-0.0008

**Table 4. Hazard Model of Decision to Authorize Chapter 9**

This table shows the expected time (hazard rate) to authorizing Chapter 9 using a dynamic Cox hazard model with state-level stratification. The dependent variable is years since 1980. We drop all observations one year after the state authorizes Chapter 9. The independent variables are public labor union growth, the changes in the natural logarithm of the fraction of public labor union coverage (deciles) from the prior year; judicial caseload growth, the changes in caseload per judge from the prior year; state debt, the natural logarithm of state debt outstanding; local identity (deciles); Democrat control and Republican control, dummy variables equal to one if the Democratic (re. Republican) party controls both state houses and the governorship; GDP per capita growth; Unemployment growth, the changes in the natural logarithm of the unemployment rate ; house prices growth, the changes in the natural logarithm of housing prices, and AAA muni bond rating, a dummy equal to one if the muni bonds have AAA rating. The t-statistics are shown in parentheses and robust standard errors are used. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Year-1980 (I)	Year-1980 (II)	Year-1980 (III)	Year-1980 (IV)	Year-1980 (V)	Year-1980 (VI)	Year-1980 (VII)	Year-1980 (VIII)
Public Labor Union Growth	-1.67* [-1.96]				-2.97*** [-2.80]	-2.90*** [-3.00]	-3.01*** [-2.96]	-2.83*** [-3.21]
Judicial Caseload Growth		-2.32** [-2.16]			-3.04** [-2.52]	-2.98** [-2.31]	-3.03** [-2.27]	-3.10** [-2.24]
Local Identity			0.20* [1.86]		0.42** [2.03]	0.47** [1.99]	0.42** [2.09]	0.47** [1.99]
State Debt				0.60** [2.02]	1.45*** [2.73]	1.43*** [2.60]	1.47*** [3.23]	1.47*** [3.30]
GDP p.c. Growth						-25.57 [-1.23]		-26.80 [-1.28]
Unemployment Growth						-2.49 [-0.91]		-2.50 [-0.93]
House Prices Growth						4.94 [0.85]		4.73 [0.85]
AAA Muni Bond Rating						-0.45 [-0.81]		-0.47 [-0.84]
Democrat Control							-0.00 [-0.01]	0.22 [0.39]
Republican Control							0.11 [0.09]	0.21 [0.16]
No Observations	857	857	893	861	825	825	825	825
Log pseudo-likelihood	-39.56	-39.84	-42.30	-38.73	-26.35	-25.35	-26.35	-25.31
Wald Chi-Square	3.83	4.68	3.47	4.10	13.82	16.21	17.48	22.46
p-value	0.050	0.031	0.062	0.043	0.008	0.039	0.008	0.013

**Table 5. Hazard Model of Decision to Authorize Chapter 9: Alternative Specifications**

This table shows the expected time (hazard rate) to authorizing Chapter 9 using a dynamic Cox hazard model with state-level stratification. The dependent variable is years since 1980. We drop all observations one year after the state authorizes Chapter 9. The independent variables are labor union growth, the changes in the natural logarithm of the fraction of public labor union coverage (deciles) from the prior year; judicial caseload growth, the changes in caseload per judge from the prior year; state debt, the natural logarithm of state debt outstanding; local identity (deciles); Democrat control and Republican control, dummy variables equal to one if the Democratic (re. Republican) party controls both state houses and the governorship; GDP per capita growth; Unemployment growth, the changes in the natural logarithm of the unemployment rate ; house prices growth, the changes in the natural logarithm of housing prices, and AAA muni bond rating, a dummy equal to one if the muni bonds have AAA rating. The t-statistics are shown in parentheses and robust standard errors are used. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	Year-1980 (I)	Year-1980 (II)	Year-1980 (III)	Year-1980 (IV)	Year-1980 (V)	Year-1980 (VI)	Year-1980 (VII)
Public Labor Union Growth			-3.15*** [-2.93]	-3.46*** [-2.85]	-3.39*** [-2.72]	-3.99*** [-3.34]	
Public + Private Labor Union Growth							-2.22* [-1.77]
Judicial Caseload Growth			-3.62*** [-2.72]	-4.06*** [-3.10]	-4.21*** [-3.41]	-4.38*** [-3.80]	-4.34*** [-3.49]
Local Identity			0.44*** [2.98]	0.47** [2.42]	0.49** [2.28]	0.57** [2.28]	0.55** [2.45]
State Debt	0.60** [2.02]	0.17 [0.47]	0.12 [0.16]	0.25 [0.31]	0.35 [0.39]	0.13 [0.11]	-0.11 [-0.12]
Muni Mutual Funds		1.50** [1.97]	3.81** [2.00]	3.49** [2.07]	3.47** [2.16]	4.13* [1.84]	3.89** [2.21]
GDP p.c. Growth			-22.62 [-0.66]	-22.29 [-0.61]	-25.65 [-0.64]	-27.03 [-0.58]	-27.05 [-0.78]
Unemployment Growth			-2.74 [-0.99]	-2.57 [-0.90]	-2.51 [-1.02]	-3.75 [-1.57]	-3.63 [-1.46]
House Prices Growth			-1.73 [-0.32]	-0.30 [-0.04]	-0.87 [-0.12]	-1.56 [-0.23]	-1.02 [-0.18]
AAA Muni Bond Rating			-2.26** [-2.01]	-2.51** [-2.48]	-2.61*** [-2.73]	-3.77*** [-2.59]	-3.61*** [-2.65]
Small Firms Share				-17.61 [-0.76]	-20.94 [-0.83]	-31.75 [-1.28]	-22.92 [-1.06]
Relative Size of Insurance				1.67 [0.51]	2.08 [0.83]	2.29 [0.79]	0.31 [0.12]
Democrat Control					0.47 [0.69]	0.81 [1.25]	1.15 [1.35]
Republican Control					0.32 [0.28]	0.11 [0.08]	-0.58 [-0.52]
Neighbor State Authorizes Chapter 9						-1.43** [-2.03]	-1.00 [-1.38]
No Observations	861	861	825	825	825	825	825
Log pseudo-likelihood	-38.73	-36.84	-21.83	-21.46	-21.35	-20.63	-22.26
Wald Chi-Square	4.10	5.98	64.69	64.64	109.14	58.78	87.37
p-value	0.043	0.050	0.000	0.000	0.000	0.000	0.000

**Table 6. Marginal Effects from a Probit Model of the Influence of Economic and Political Factors on the Votes on Municipal Bankruptcy Law at the House, June 9, 1933, and at the Senate, May 1, 1934**

The dependent variable is one if the legislator votes yes and zero if no. The reported coefficients are the effects of a unit change of the independent variable (from its mean) on the probability of voting for the Bill. Labor unions is the number of labor union members divided by population. Judicial caseload is the number of government units in default divided by the number of judges in the state district. State debt is the log of state debt. Unemployment is the sum of unemployed and laid off workers as a percent of total workforce. Urban share of the population is the fraction of the population that lives in urban areas. Percent of large cities in default is the percent of cities with more than 10,000 inhabitants in default. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Dependent Variable	House Vote, 6/9/33 Recommit to Committee (Yes 172 – No 191) (1)	Senate Vote, 5/1/34 Pass Bankruptcy Bill (Yes 45 – No 28) (2)
Labor Unions	14.22** (6.72)	-36.51*** (13.61)
Judicial Caseload	0.010*** (0.004)	-0.058* (0.035)
State Debt	-0.123** (0.059)	0.137 (0.152)
Indicator 1 if Democrat	-1.48*** (0.19)	1.205*** (0.465)
Unemployment	-33.21 (26.33)	259.3*** (76.69)
Urban Share of Population	2.81*** (0.97)	-8.67*** (2.33)
Percent of Large Cities in Default	-2.77*** (0.71)	43.80 (34.92)
Constant	1.07 (0.65)	-1.10 (1.60)
Number of Observations	361	66
Number of Yes Votes	172	40
Pseudo-R <sup>2</sup>	0.273	0.483
Likelihood Ratio $\chi^2$	133.53	42.72
p-value of $\chi^2$ for regression	0.000	0.000

**Table 7. Ex-Post Consequences of Chapter 9: Bond-level Analysis**

The dependent variable is credit spread, the difference between the offering yield of each municipal bond and the T-bill rate of corresponding maturity at the same period. The key explanatory variable is Chapter 9, a dummy that equals one if Chapter 9 is authorized in a state-year, and zero otherwise. Additional explanatory variables are bond amount, the natural logarithm of the bond issuance amount; maturity, the natural logarithm of the number of days to maturity; investment grade, a dummy that equals one if the bond is investment grade; and credit rating, a dummy that equals one if the bond has a credit rating from at least one of S&P, Moody's, and Fitch. Credit spread, bond amount, and maturity are winsorized at 0.5% and 99.5%. Year and state fixed effects are included but not reported. Columns I to III considers from the sample period 1990 to 2010 and Columns IV to VI consider the period 1990-2005 before the financial crisis. The t-statistics are shown in parentheses and standard errors are clustered at the state level. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Sample Period Dependent variable	1990-2010			1990-2005		
	Credit spread (I)	Credit spread (II)	Credit spread (III)	Credit spread (IV)	Credit spread (V)	Credit spread (VI)
Chapter 9	-0.095** [-2.05]	-0.105*** [-4.12]	-0.031 [-0.73]	-0.117* [-1.84]	-0.090** [-2.55]	-0.031 [-0.48]
Bond Amount	0.003 [0.39]	0.007 [1.08]	-0.011* [-1.86]	-0.001 [-0.24]	0.003 [0.48]	-0.011** [-2.13]
Maturity	0.159*** [7.80]	0.151*** [10.07]	0.139*** [7.55]	0.168*** [9.42]	0.159*** [11.54]	0.149*** [9.38]
Investment Grade	-0.573*** [-11.95]	-0.663*** [-11.82]	-0.171** [-2.60]	-0.563*** [-12.65]	-0.631*** [-10.68]	-0.129** [-2.20]
Credit Rating	0.378*** [5.08]	0.481*** [6.81]	0.078 [1.14]	0.405*** [5.80]	0.463*** [6.36]	0.092 [1.50]
Constant	-2.916*** [-31.23]	-2.424*** [-31.43]	-2.702*** [-23.11]	-2.926*** [-34.51]	-2.874*** [-30.77]	-2.776*** [-24.82]
Sample	Full	Revenue Bonds	G.O. Bonds	Full	Revenue Bonds	G.O. Bonds
No Observations	1,402,187	249,206	771,589	946,910	179,780	513,756
R-squared	0.539	0.529	0.583	0.401	0.393	0.473

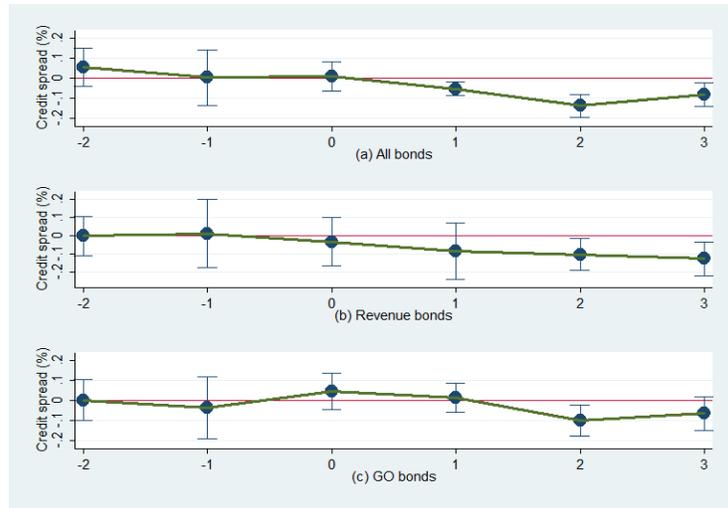
**Table 8. Ex-Post Consequences of Chapter 9: Firm-Level Spillover Effects**

We consider 1705 firms that operate in only one of the 33 states (AK, ND, and WV have no single state firms in our sample) considered in Table 4 from 1980 to 2014. The dependent variables are sales, profitability, investment rate, and dividends. Key explanatory variables are the indicator for being post Chapter 9 authorization (Chapter 9), the first year of authorization (Chapter 9 (year 1)), the second year of authorization (Chapter 9 (year 2)), and the third and later years of authorization (Chapter 9 (year 3+)), and the Chapter 9 indicator, which is one if Chapter 9 is authorized in a state in a given year, and zero otherwise. Chapter 9 (year 1) is an indicator which is one for observations in the first year following Chapter 9 authorization. Chapter 9 (year 2) is an indicator which is one for observations in the second year following Chapter 9 authorization. Chapter 9 (year 3+) is an indicator which is one for observations in the third or later years following Chapter 9 authorization. We control for firm size, book leverage, and market-to-book ratio. For each dependent variable, we consider two different samples: the full sample and the conduit sample. The conduit sample considers single state firms in states with a large fraction of non-governmental purpose municipal bonds (excluding the bottom tercile of non-governmental purpose to total municipal bond annual issuance ratio observations). Year and state fixed effects are included but not shown in table. The t-statistics are shown in parentheses and standard errors are clustered at the state level. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Dependent variable	Sales			Profitability			Investment rate			Dividends		
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)	(IX)	(X)	(XI)	(XII)
Chapter 9	0.087 [1.44]	0.179*** [4.37]	0.109*** [2.85]	0.254* [1.88]	0.339** [2.49]	0.405* [1.87]	0.039** [2.06]	0.077** [2.47]	0.077** [2.69]	0.008** [2.29]	0.010** [2.26]	0.009** [2.09]
Firm Size	-0.084*** [-5.10]	-0.092*** [-5.63]	-0.101*** [-5.19]	0.222 [1.68]	0.217 [1.11]	0.240 [1.04]	0.003** [2.12]	0.005* [1.95]	0.004 [1.29]	-0.008*** [-3.88]	-0.011*** [-4.03]	-0.011*** [-4.22]
Book Leverage	-0.003* [-1.79]	-0.003* [-1.75]	-0.003 [-1.68]	-0.017 [-0.09]	-0.010 [-0.05]	-0.008 [-0.04]	-0.010 [-0.78]	0.000 [0.03]	0.001 [0.06]	0.000 [0.62]	0.000 [0.44]	0.000 [0.37]
Market-to-Book	0.000 [0.62]	0.000 [0.50]	0.000 [0.53]	-0.015 [-0.24]	-0.014 [-0.23]	-0.015 [-0.22]	0.007** [2.41]	0.006** [2.19]	0.006** [2.09]	0.000 [1.01]	0.000 [0.93]	0.000 [0.89]
Constant	1.739*** [29.75]	1.494*** [18.23]	1.431*** [10.68]	-1.685** [-2.68]	-1.032 [-1.60]	-0.899 [-1.22]	-0.036 [-0.45]	-0.339*** [-6.44]	-0.336*** [-6.62]	0.061*** [4.80]	0.094*** [2.75]	0.068*** [7.58]
Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State x Industry FE?			Yes			Yes			Yes			Yes
Sample	Full	Conduit	Conduit	Full	Conduit	Conduit	Full	Conduit	Conduit	Full	Conduit	Conduit
No Observations	21,269	14,785	14,785	21,269	14,785	14,785	19,760	13,756	13,756	21,206	14,779	14,779
R-squared	0.036	0.037	0.133	0.004	0.004	0.011	0.020	0.021	0.029	0.015	0.017	0.038

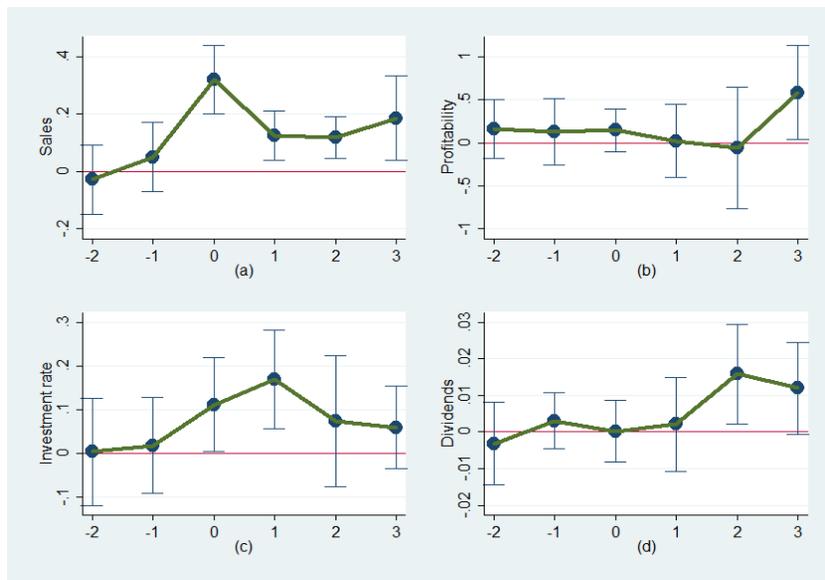
**Figure 1. Bond spreads in treatment and control groups before and after Chapter 9 authorization.**

The graph reports the coefficients on Chapter 9 in the bond spreads regressions in the years before and after Chapter 9 authorization. On the x-axis, the years are measured relative to Chapter 9 authorization (year 0). The regression includes state and year fixed effects. For each firm-level outcome, we report the point estimate and the two-standard deviation interval.



**Figure 2. Firm outcomes in treatment and control groups before and after Chapter 9 authorization.**

The graph reports the coefficients on Chapter 9 in the firm outcomes regressions in the years before and after Chapter 9 authorization. On the x-axis, the years are measured relative to Chapter 9 authorization (year 0). The regression includes state and year fixed effects. For each firm-level outcome, we report the point estimate and the two-standard deviation interval.



### Appendix: Table A1. Bond-level Analysis Controlling for Drivers of Chapter 9

The dependent variable is credit spread, the difference between the offering yield of each municipal bond and the T-bill rate of corresponding maturity at the same period. The key explanatory variable is Chapter 9, a dummy that equals one if Chapter 9 is authorized in a state-year, and zero otherwise. Additional explanatory variables are bond amount, the natural logarithm of the bond issuance amount; maturity, the natural logarithm of the number of days to maturity; investment grade, a dummy that equals one if the bond is investment grade; and credit rating, a dummy that equals one if the bond has a credit rating from at least one of S&P, Moody's, and Fitch. Credit spread, bond amount, and maturity are winsorized at 0.5% and 99.5%. Year and state fixed effects are included but not reported. Columns I to III considers from the sample period 1990 to 2010 and Columns IV to VI consider the period 1990-2005 before the financial crisis. The t-statistics are shown in parentheses and standard errors are clustered at the state level. \*\*\*, \*\*, and \* denote significance at the 1%, 5%, and 10% level, respectively.

Sample Period Dependent variable	1990-2010			1990-2005		
	Credit spread (I)	Credit spread (II)	Credit spread (III)	Credit spread (IV)	Credit spread (V)	Credit spread (VI)
Chapter 9	-0.105** [-2.15]	-0.106*** [-4.06]	-0.033 [-0.73]	-0.111* [-1.77]	-0.081** [-2.54]	-0.018 [-0.29]
Bond Amount	0.003 [0.40]	0.007 [1.10]	-0.011* [-1.89]	-0.001 [-0.21]	0.004 [0.51]	-0.011** [-2.10]
Maturity	0.159*** [7.84]	0.151*** [10.10]	0.139*** [7.62]	0.169*** [9.39]	0.159*** [11.55]	0.149*** [9.39]
Investment Grade	-0.572*** [-11.97]	-0.663*** [-11.71]	-0.171** [-2.51]	-0.563*** [-12.69]	-0.630*** [-10.45]	-0.135** [-2.22]
Credit Rating	0.376*** [5.09]	0.480*** [6.75]	0.078 [1.12]	0.405*** [5.81]	0.461*** [6.25]	0.098 [1.57]
Public Labor Union	0.000 [0.01]	-0.012 [-1.05]	0.003 [0.21]	-0.008 [-0.70]	-0.008 [-0.45]	-0.016 [-1.12]
Judicial Caseload Growth	0.091 [1.14]	0.023 [0.48]	0.117 [1.18]	0.054 [1.15]	0.029 [0.38]	0.075 [1.19]
Local Identity	0.005 [1.56]	-0.002 [-0.50]	0.004 [1.34]	-0.002 [-0.64]	-0.006* [-1.75]	-0.00250 [-0.78]
State Debt	0.050 [1.06]	0.053 [1.28]	0.077 [1.62]	0.071 [1.25]	0.096 [1.65]	0.085** [2.19]
Constant	-2.916*** [-31.23]	-2.424*** [-31.43]	-2.702*** [-23.11]	-2.926*** [-34.51]	-2.874*** [-30.77]	-2.776*** [-24.82]
Sample	Full	Revenue Bonds	G.O. Bonds	Full	Revenue Bonds	G.O. Bonds
No Observations	1,396,182	247,966	769,266	940,905	178,540	511,433
R-squared	0.535	0.525	0.581	0.392	0.385	0.466

**Appendix: Table A2. State-Level Legal and Regulatory Changes Around Chapter 9 Authorizations**

The table reports state-level changes in corporate income state taxes, antitakeover regulation, bank branching deregulation, and labor regulation in the years surrounding Chapter 9 authorizations in the 13 states that authorized Chapter 9 between 1980 and 2011. Year 0 is the year of Chapter 9 authorizations.

State-level change	Year -2	Year -1	Year 0	Year 1	Year 2
<i>Corporate taxes</i>					
Increase	2	1	0	1	2
Decrease	2	2	1	1	3
<i>Antitakeover regulation</i>					
Business combination	1	1	0	1	2
Fair price	0	0	0	1	2
Control share acquisition	0	0	0	0	1
<i>Bank branching deregulation</i>					
Intrastate through M&A	1	0	1	2	2
Full intrastate	0	0	1	3	3
Interstate	1	1	1	1	1
Multibank holding permitted	0	0	0	0	1
<i>Labor regulation</i>					
Public policy	0	0	0	0	0
Implied contract	1	0	0	0	1
Implied covenant	0	0	0	0	0
Right-to-work	0	0	0	0	1