

# DO MARKETS REWARD CONSTITUTIONAL REFORM? LESSONS FROM AMERICA'S STATE DEBT CRISIS<sup>\*</sup>

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**Abstract:** America's 1840s state debt crisis presents a unique opportunity to identify whether self-imposed fiscal constraints are perceived as credible. After nine states defaulted, 20 states adopted constitutional provisions that constrained the legislature's ability to issue debt. The process of reform generates natural variation in the timing of these reforms that aids identification. Exploiting this variation, I find that only states with tarnished reputations (i.e. states that defaulted during the crisis) were rewarded with lower borrowing costs following the adoption of these reforms. This suggests that institutional constraints are indeed credible and can also help sovereigns reestablish their commitment to debt repayment.

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## I. INTRODUCTION

Douglass North and Barry Weingast's landmark article on the Glorious Revolution has launched a vibrant literature on the role that constitutions and rules might play in promoting sovereign commitment and access to credit.<sup>1</sup> Perhaps the central question raised by this literature is whether constitutional reforms that bind the state can reduce borrowing costs. Put more simply, are self-imposed constraints credible? The literature thus far has been dominated by a mix of case studies and time-series evidence (e.g., Balla and Johnson, 2009; Summerhill 2015; Wells and Wills 2000; Saiegh, 2013). This literature has yielded conflicting evidence as to the importance of self-imposed constraints, reflecting the difficulty associated with identifying a causal effect with these empirical approaches.

In this paper, I contribute to this literature by exploiting an historical episode that is well known to American economic historians: the wave of constitutional reforms adopted by some American states in the aftermath of 1840s debt crisis (see English, 1996; Wallis, 2005; and Wallis et al., 2004). Because states varied in the timing of their constitutional reforms and some states never reformed, I am able to use a difference-in-differences strategy to identify and measure the extent to which financial markets reward sovereign borrowers for adopting institutional constraints on their behavior. In this way, I build on a growing literature in economic history that uses similar identification strategies to isolate the causal impact of institutions and institutional reform on economic performance (e.g., Gregg, 2017; Nunn and Wantchekon, 2011; Cantoni and Yuchtman, 2014. See also Johnson and Koyoma, 2017 for a recent review of this literature.)

Following the default of nine US states and territories between 1841 and 1843, 20 states reformed their constitutions to adopt debt limits, require that new debt issues

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<sup>1</sup> North and Weingast argue that England was rewarded with lower interest rates and increased access to credit, laying the foundation for England's future economic success, once it adopted a constitution that constrained the power of the monarch. This interpretation sparked considerable academic debate with Greg Clark (1996 and 2008) arguing that the reforms did nothing to alter a long-run decline in borrowing costs and David Stasavage (2002 and 2007) arguing that interest rates did not fall until capital owners were better represented within parliament. The current consensus emphasizes the importance of reforms relating to political representation over the explicit protection of private property (Cox, 2012 and Jha, 2015).

be accompanied by new taxes, and in general, restrict the legislatures ability to unilaterally issue new debt. While American states are, by definition, sub-sovereign, to the extent that repayment cannot be forced their debts can be thought of as sovereign. The United States Constitution precludes suits against states to enforce payment. Consequently, attempts to use the Supreme Court to compel payment have been unsuccessful.<sup>2</sup> This feature, paired with variation in both the adoption of the reforms as well as the timing of the reforms lends itself to the use of a differences-in-differences empirical approach.<sup>3</sup> Constitutions are, of course, self-imposed constraints and thus subject to change, which may undermine the extent to which the commitment is perceived as credible. To assess whether the constraints were seen as credible, I construct a panel of state bond prices to examine how financial markets responded to their adoption. I find that that, on average, the price of bonds issued by reforming states increased by two percent following reform, which indicates that markets valued these reforms.

There is reason to believe that markets might have responded differentially to the reforms adopted by states that defaulted during the crisis relative to reforming states that did not default. The act of default tarnished the state's reputation by illustrating a willingness to impose large costs on creditors. Therefore, to the extent that these reforms helped convey commitment to debt repayment, defaulting states stood to benefit the most from reforming their constitutions. Accordingly, I find the largest effects for these states; bonds issued by states that defaulted during the crisis appreciated by 4 to 12 percent following reform.

For reforming states with untarnished reputations there are two competing effects. Similar to defaulting states, if markets view constitutional constrains as a credible commitment then bonds issued by reforming states should be viewed as more secure than bonds issued by non-defaulting states that didn't reform, and so reform should increase bond prices. Working in the opposite direction is the possibility that

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<sup>2</sup> English (1996) provides a detailed discussion of the sovereignty of state debts during this period.

<sup>3</sup> Gregg (2017) also uses standard methodology from the applied microeconomics literature to analyze stock market data. As in that paper, the identifying assumptions are discussed further in the methodology section, Section III.b.

markets perceive reform as a negative information shock. A skeptical investor, unsure of why states that managed to avoid default are now adopting these reforms, might interpret a non-defaulting state's choice to reform its constitution as a signal that its fiscal situation is unstable. Results indicate that the second effect completely counteracts the positive benefits of establishing a credible commitment to repaying existing and future debts. Specifically, I find that the price of bonds issued by non-defaulting states fell by 1 to 4 percent following reform.

These results lend support to the idea that sovereigns can adopt self-imposed constraints to signal commitment and are particularly relevant for sovereigns with recently tarnished reputations. In their survey of sovereign debt and default, Michael Tomz and Mark Wright (2013) document 251 defaults by 107 distinct sovereigns between 1820 and 2010.<sup>4</sup> Thus sovereign default is by no means a unique phenomenon. Further, sovereign defaults impose considerable costs on both creditors and defaulters. A typical restructuring of debt results in creditor losses on the order of 40 percent, with heavily indebted and low-income countries imposing the largest costs on creditors (Benjamin and Wright, 2013; Cruces and Trebesch, 2012). Those that default are also punished with restricted access to credit and higher borrowing costs. The results in this paper indicate that fiscal constraints have the potential to help sovereigns regain access to credit at favorable terms.

## **II. THE ORIGINS OF DEFAULT AND REFORM**

Total debt issued by American states increased from roughly 20 million dollars in 1830 to nearly 200 million dollars in 1840.<sup>5</sup> This increase in borrowing came to an abrupt end following the suspension of payments by Florida and Mississippi in the beginning of 1841. Between 1841 and 1843, nine of the 29 existing US states and territories defaulted on their debts. Four of those states eventually repaid their debts while the remaining five repudiated all or part of their debts. To better understand why

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<sup>4</sup> Sovereign defaults typically come in three forms: (1) an outright refusal to pay, (2) an implicit default by devaluing the currency in which the debt is to be paid, or (3) a renegotiation of payment terms.

<sup>5</sup> These figures come from William Cost Johnson's 1843 report to the U.S. House of Representatives on state indebtedness.

some states borrowed more heavily than others and why some states defaulted while others did not, it is tempting to consider the individual histories of each state. Indeed, this was the approach that the prominent financial reporter Thomas Kettell took as he wrote a series of articles for Hunt's Merchant Magazine between 1847 and 1852. The goal of this paper, however, is to abstract away from the case-study approach that is prevalent in the existing literature, and instead focus on the common features of default and reform.<sup>6</sup>

As to the causes of default, the revisionist work of John Wallis, Richard Sylla, and Arthur Grinath, (2004) is perhaps the most informative. As the authors argue, traditional narratives of the 1837 panic inducing a revenue short fall where some states were "lucky" to have avoided default does not fit the data. While state debts increased from 20 to 200 million between 1830 and 1841, roughly 45% of this increase occurred between 1838 and 1841, suggesting that the Panic of 1837 did little to curb state borrowing.<sup>7</sup> Figure 1 illustrates this point by plotting cumulative debt authorizations by region. There we see large increases in borrowing following 1837, with the largest increases occurring in the Northeast and the Old West.

As Wallis et al. (2004) argue, the underlying cause of this borrowing was the dramatic land boom that occurred throughout the 1830s. Established states did not rely on property taxes as a source of revenue – by 1835 Massachusetts, New York, Pennsylvania, Maryland, Georgia, and Alabama had all suspended their property tax (Wallis et al. 2004, Table 4). Nevertheless, increasing land prices signaled to these states that they could borrow comfortably knowing that a large untaxed fiscal resource was held in reserve. Western states and Southwestern, however, did rely heavily on property taxes yet they increased their pace of borrowing in the latter half of the 1830s reflecting the fact that the tens of millions of acres that the federal government sold throughout the 1830s would finally be eligible for taxation.<sup>8</sup>

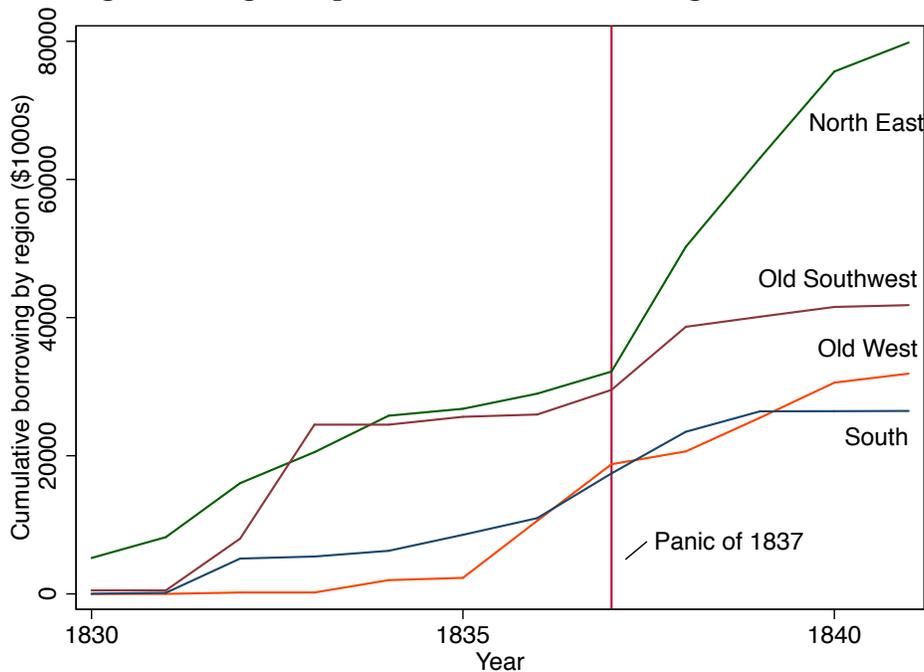
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<sup>6</sup> Wallis, Sylla, and Grinath (2004) provide an excellent overview of the causes of state defaults. See also English (1996), Ratchford (1966), and Thomas Kettell's series of articles analyzing the experience of individual states, which appeared in Hunt's Merchant Magazine between 1847 and 1852.

<sup>7</sup> Underlying data from Table 3 of Wallis et al. (2004).

<sup>8</sup> Federal land sales were exempt from state taxation for the first five years following the sale.

**Figure 1: Regional patterns in state borrowing, 1830-1841**



Underlying data from William Cost Johnson’s 1843 report on state indebtedness. Data retrieved from Table 3 of Wallis et al. (2004). The North East region contains Massachusetts, Maryland, Maine, New York, Ohio, and Pennsylvania. The Old West contains Illinois, Indiana, and Michigan. The South region contains Alabama, Georgia, South Carolina, and Virginia. The Old South West contains Florida, Kentucky, Louisiana, Missouri, Mississippi, and Tennessee. States and territories without any debts as of 1841 are omitted.

Land values fell sharply in the early 1840s, setting the stage for default for Western states. Consider the case of Indiana. Between 1835 and 1841, the amount of taxable land in Indiana rose from 5.2 to 10.2 million acres. The average value of a taxable acre, however, increased from \$5.41 in 1835 to a peak of \$9.87 in 1837 before falling to \$6.20 in 1841 and \$3.73 in 1842. Property tax revenues increased from \$44,537 in 1835 to a peak of \$300,481 in 1840 before falling to \$168,898 the following year.<sup>9</sup> Indiana would default in January of 1841. Many other western states found themselves in a similar situation.

All states were borrowing to finance banks, railroads, and canals. Western states invested heavily with the expectation that debts could be repaid with property tax revenues. Established states intended to repay future debts with revenues from those

<sup>9</sup> All data from Table 6 of Wallis et al. (2004).

projects. When those revenues failed to materialize, however, established states avoided default by quickly reinstating their property tax to makeup the revenue shortfall. Maryland and Pennsylvania – the two established states that did default on their debts – were too slow to reinstate their property tax, which explains why they were forced to temporarily suspend debt payments.

A final group of defaulters never anticipated servicing their debts directly. To better understand why this was the case, it is important to understand the interaction between state legislatures and private corporations during this period.<sup>10</sup> State legislatures chartered private corporations to open new banks, build canals, and build railroads. After chartering these corporations, states borrowed in order to invest in those enterprises. States typically invested in corporations by selling bonds and using those proceeds to buy stock or bonds of the chartered corporation. However, sometimes a state would be restricted in the sense that it was not authorized to sell its bonds below their par value. In this case, a state might circumvent this restriction by exchanging state bonds for company stock or bonds at face value. The private corporation could then sell the state bonds for whatever price the market would bear. States that ultimately repudiated their debts tended to invest in these corporations with the understanding that the private enterprises would service those debts. In other words, the states had no intention of using their own tax revenues to service those debts, and so when the private corporations failed to service the debts the states refused to repay their creditors.

The debt crisis quickly revealed the problems associated with state investment in private enterprises as well as allowing legislatures to borrow as much as they pleased. Reflecting on the origins of the debt crisis, Thomas Kettell wrote, “experience has brought with it the necessity of very clearly and pointedly forbidding the Legislature to exercise such powers of ... grant[ing] charters, ... borrowing money on their own responsibility, ... [as well as] granting special privileges to corporate bodies [and] endowing them with larger credit and less liability ... than is permitted to individual citizens”.<sup>11</sup> Consistent with this, 20 states reformed their constitutions between 1842

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<sup>10</sup> This paragraph is based off of Ratchford (1966) pages 89-90 as well as Wallis (2005) and Wallis et al. (2004).

<sup>11</sup> Kettell (1851, pg. 5).

and 1860 in order to restrict the legislature's ability to issue new debt and charter corporations. Ohio, for instance, adopted a provision stating "The credit of the State shall not, in any manner, be given or loaned to, or in aid of, any individual, association, or corporation whatever; nor shall the State ever hereafter become a joint owner, or stockholder in any company or association in this State or elsewhere, formed for any purpose whatever." As another example, Illinois restricted the legislature's ability to borrow by adopting the following provision: "The State may, to meet casual deficits or failures in revenue, contract debt never to exceed in aggregate fifty thousand dollars; ... and no other debt except for the purpose of repelling invasion ... shall be contracted, unless the law authorizing the same shall, at a general election ... receive a majority of all the votes cast." These provisions are demonstrative of the broader reforms adopted by many states following the defaults of the early 1840s.

The provisions each state adopted are presented in Table 1. States constrained their ability to unilaterally increase debt, imposed debt limits, and required new debt issues be accompanied by new taxes. States also prohibited investment in private corporations, adopted general incorporation laws, and prohibited the granting of special privileges to corporations. The main takeaway from Table 1 is that the adoption of these reforms was widespread. As Table 1 illustrates, the types of reforms that states adopted were neither decided by a state's indebtedness nor whether the state defaulted. In fact, the lessons of the debt crises were so salient that states joining the Union after the debt crisis (e.g. California, Kansas, Minnesota, Oregon, and Texas) also adopted constitutions that limited the legislature's ability to borrow and prevented state investment in private enterprises. These provisions were also innovative at the time of their adoption. To demonstrate this point I compare the text of the reformed constitutions to the text of the pre-existing constitution.<sup>12</sup> The word "debt" appears 152 times in the reformed constitutions but only appears eight times in pre-existing constitutions. The word "corporation" appears 74 times in the new constitutions while it only appeared six times in the earlier constitutions. Lastly, the word "tax" appears 120 times in the reformed constitutions, but it only appeared 60 times in the previous

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<sup>12</sup> This exercise relies on the digitized constitutions available from Wallis' state constitutions website.

constitution. These comparisons illustrate that constraining the state's ability to borrow and charter corporations was of central importance when states rewrote their constitutions. John Wallis' 2005 article on the adoption of these reforms provides much more context for how the debt crisis necessitated the adoption of these provisions.

Although these reforms were widely adopted, it should be noted that not all states adopted these reforms. Alabama, Massachusetts, Missouri, and Vermont did not alter their constitutions at all between 1842 and 1860. Arkansas, Connecticut, Delaware, Florida, Mississippi, New Hampshire, North Carolina, South Carolina, and Tennessee amended their constitutions during this time period but did not adopt any of the previously mentioned reforms. The amendments adopted by these states typically addressed term limits, defined who could vote in general elections, and specified who could run for public office. Only two states appearing in Table 1 amended their constitutions (Pennsylvania and Maine). The remaining states decided to either write their first constitution or replace their existing constitution. All reforming states restricted the legislature's ability to borrow and every state except Iowa and Kansas prohibited state invest in private corporations.

**Table 1: Constitutional reforms enacted between 1840 and 1860**

	1841 Debt per capita	Year of reform	Defaulted during crisis	Procedural restrictions	Debt limits	Ways and means	No investment in corporations	General laws
Louisiana	\$68.14	1845	Y	Y	Y	Y	Y	Y
Maryland	\$32.37	1851	Y	Y	Y	Y	Y	Y
Illinois	\$28.42	1848	Y	Y	Y	Y	Y	Y
Michigan	\$26.47	1850	Y	Y	Y	Y	Y	Y
Pennsylvania	\$19.32	1857	Y	Y	Y		Y	
Indiana	\$18.59	1851	Y	Y	Y		Y	Y
New York	\$8.97	1846		Y	Y	Y	Y	Y
Ohio	\$7.19	1851		Y	Y	Y	Y	Y
Wisconsin	\$6.45	1848		Y	Y	Y	Y	Y
Kentucky	\$3.96	1850		Y	Y	Y	Y	
Maine	\$3.46	1847		Y	Y		Y	
Virginia	\$3.23	1851				Y	Y	
Iowa	\$0.00	1846		Y	Y	Y		Y
New Jersey	\$0.00	1844		Y	Y	Y	Y	Y
Rhode Island	\$0.00	1842		Y	Y		Y	
California	--	1849		Y	Y	Y	Y	Y
Kansas	--	1859		Y	Y	Y		Y
Minnesota	--	1857		Y	Y	Y	Y	Y
Oregon	--	1857			Y		Y	Y
Texas	--	1845		Y	Y		Y	Y

Outstanding debts in 1841 obtained from "The report on valuation, taxation, and public indebtedness" which was published in volume seven of the 1880 census. California, Kansas, Minnesota, Oregon, and Texas were not established as official states or territories prior to 1841 and data on indebtedness is not available. "Procedural restrictions" relate to the types of borrowing that are allowed or specific requirements that must be met for new debt to be issued (e.g. a 2/3 majority in both houses or approved by referenda). "Debt limits" can be either a limit on short-term debt or an absolute debt limit. "No investment in corporations" refers to any provision that prevents either explicit investment (loaning of money) or implicit (loaning of credit) to individuals or corporations. "Ways and means" refers to the requirement that new debt issues be accompanied by tax increases or the establishment of a sinking fund. "General laws" refers to general incorporation laws or the prohibition of granting corporations and individuals special privileges. The text of these constitutions is freely available from Wallis' state constitution database.

### III. THE MARKET RESPONSE TO REFORM

#### *III.a. Data*

While there is evidence that fiscal rules constrain government behavior in the long run (Alesina et al., 1999; Auerbach, 2008; and Poterba, 1994 and 1996), whether markets perceive fiscal rules as credible at the time of their adoption is unclear. An ideal test of market perception would be to analyze whether the cost of borrowing or the availability of credit were affected by the adoption of these constitutional constraints, indicating whether markets perceive self-imposed constitutional constraints to be a credible commitment. Unfortunately, while the William Cost Johnson report on

indebtedness provides a complete picture of state borrowing throughout the 1830s an equivalent source covering state borrowing from 1842 to 1859 (the year in which Kansas, the last state, modifies its constitution) and beyond does not exist. The closest source is a report in the tenth annual Census, which includes outstanding debt for 1853, 1860, 1870, and 1880. My own efforts to fill in the missing information have been largely unsuccessful. Borrowing information is reported in state auditor and treasurer reports, however, these reports are difficult to come by during the period of interest and when they do exist the data are not consistently reported across time or between states. Without additional data it is impossible to precisely identify the effect of these constitutional reforms on access to credit.

It is, however, possible to identify the extent to which the adoption of these reforms affected the cost of capital. To do so, I rely on data from Richard Sylla, Jack Wilson, and Robert Wright's Early American Securities Database. Sylla et al. gathered price quotations for publicly traded government and corporate securities between 1790 and 1860. These prices were retrieved from historical newspapers and magazines that were circulated in the following financial hubs: Alexandria, VA; Baltimore, MD; Boston, MA; Charleston, SC; London, England; New Orleans, LA; New York, NY; Norfolk, VA; Philadelphia, PA; Richmond, VA. The authors consulted every available edition for roughly 200 different historical newspapers and magazines to obtain these data. The exhaustive list of sources that were consulted suggests that these data characterize the market for state securities between 1840 and 1860. Consistent with this, my own consultations with primary sources (e.g. Hunt's Merchant Magazine and The New York Daily Tribune) have failed to yield any observations that are not already reported in the Sylla et al. (2002) database.

I extract all state bond observations occurring between 1843 and 1860. I choose 1843 as the start because it is the first year after states default (all defaults occur between 1841 and 1842).<sup>13</sup> By extracting observations from each exchange I am able to

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<sup>13</sup> As discussed below, it is important to analyze post-default data because it allows the asset fixed effects to better capture the systematic differences between states.

fully capture the market for state securities.<sup>14</sup> The frequency that observations occur varies by asset but is typically monthly. Some assets are reported weekly or bi-monthly, and for these assets I take the average price for each month. This ensures that each asset appears at the same frequency. Table 2 presents summary statistics by state. On average, the sample includes 10 assets for each state and each of those assets appears for an average of 25 months. The sample includes a total of 24 states. 15 reforming states appear in the sample, but only nine states have both pre and post-reform observations (those states are: Illinois, Indiana, Kentucky, Michigan, New York, Ohio, Pennsylvania, and Virginia).

**Table 2: Summary statistics**

	Number of assets	Mean observations per asset	Median observations per asset	Total observations	Reformed constitution	Defaulted during crisis
Alabama	5	20.4	8	102		
Arkansas	1	31	31	31		Y
California	4	19.8	18.5	79	Y	
Georgia	3	15	7	45		
Illinois	23	14.1	4	325	Y	Y
Indiana	17	26.5	20	451	Y	Y
Iowa	1	6	6	6	Y	
Kentucky	15	30.8	21	462	Y	
Louisiana	9	11.2	6	101	Y	Y
Maine	4	23.5	23.5	94	Y	
Maryland	9	63.7	40	573	Y	Y
Massachusetts	9	43.8	41	394		
Michigan	2	18	18	36	Y	Y
Minnesota	1	11	11	11	Y	
Mississippi	5	3	3	15		Y
Missouri	1	39	39	39		
New York	75	10.5	7	788	Y	
North Carolina	2	19	19	38		
Ohio	21	26.4	7	554	Y	
Pennsylvania	23	25.5	11	586	Y	Y
South Carolina	3	70.7	60	212		
Tennessee	8	22.3	13	178		
Texas	3	2.3	2	7	Y	
Virginia	6	50.5	63.5	303	Y	

Data retrieved from Sylla, Wilson, and Wright's Early American Securities Database. Sample restricted to the years 1843-1860.

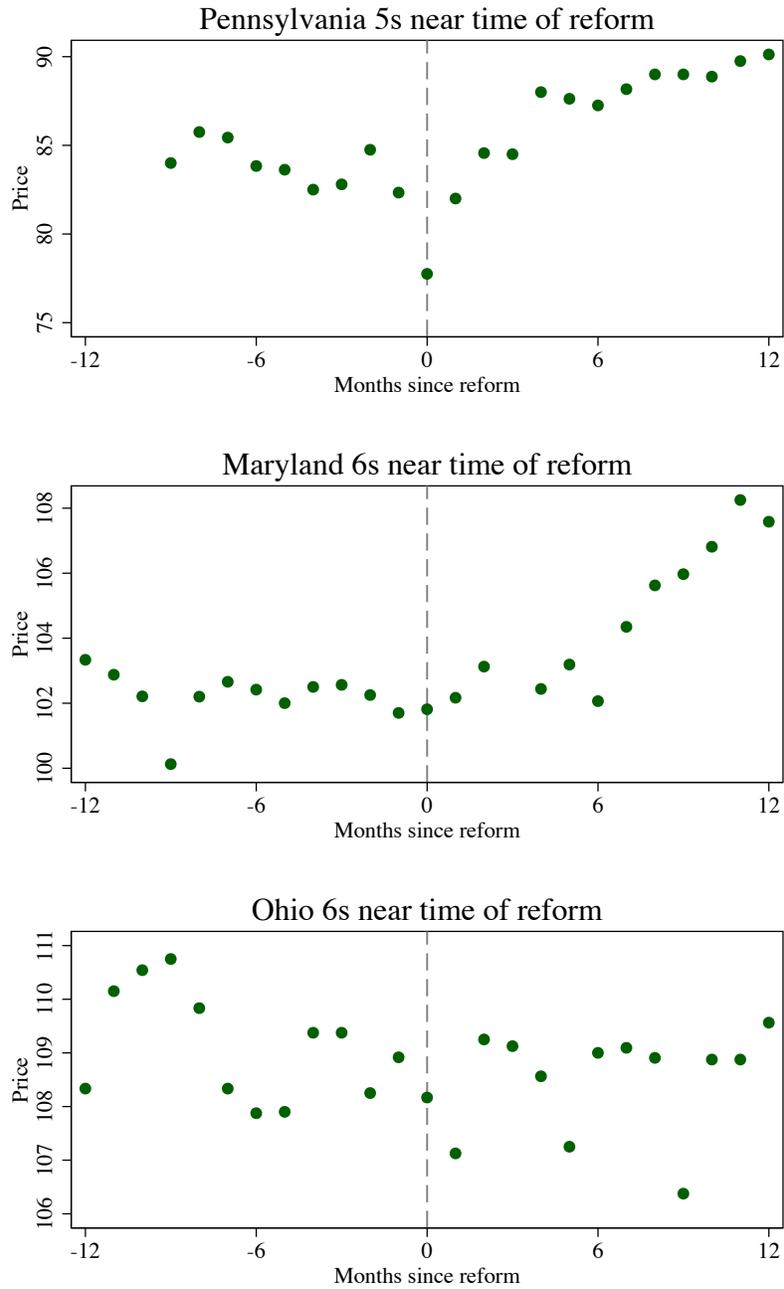
<sup>14</sup> When an asset appears on more than one exchange, I only keep the prices from the exchange with the most observations. One might be concerned about the integration of capital markets during this time period. The integration of early capital markets is well documented in Neal (1992, 1993) and Sylla et al (2006). Wright (2002), in particular, shows that American markets were integrated in the antebellum period. Furthermore, my estimating equation will include asset fixed effects which will eliminate any systematic differences between exchanges.

The features of some bonds (coupon rate and maturity date) can sometimes be deduced from the name of the bond. For instance, “Illinois Canal Bonds, 1870” refers to bonds issued by Illinois that mature in 1870, and “Alabama 6s” refers to bonds that pay a six percent coupon. When both the interest rate and the maturity date can be deduced, one can then calculate the asset’s yield to maturity. The yield to maturity is the rate of return that the investor will receive from holding the asset until it matures. Unfortunately, the interest rate and maturity date are only reported for 49 percent of my sample. Thus, as in Wells and Wills (2000) and Stasavage (2002; 2007), I use price quotations to proxy for the cost of capital instead of the yield to maturity. Price is an appropriate proxy for the cost of capital, as an asset’s price and yield to maturity are inversely related. If constitutional reforms reduce payment uncertainty, then the price of assets issued by the reforming state should increase (reflecting that the asset has become less risky) and the yield to maturity will fall as a result. Therefore, the magnitude of the price change is indicative of the magnitude of the change in the cost of capital.

### *III.b. Methodology*

As mentioned in the introduction, the previous literature aimed at understanding whether markets value institutional constraints has largely relied on individual case studies. Figure 2 illustrates how difficult it is to disentangle the effect of reform by studying only one sovereign. Specifically, I plot prices for three state bonds near the time of constitutional reform. The three states are Pennsylvania, Maryland, and Ohio, and each bond is presented on a common axis (12 months before and after reform). In the first panel it appears that the price of Pennsylvania 5-percent bonds rose by \$5 following reform relative to their pre-reform average price of \$85. In the second panel, it appears that Maryland 6-percent bonds appreciated following the adoption of constitutional reforms, but the effect appears to be delayed by about six months. In the final panel there is, perhaps, weak evidence that reform stopped a downward trend in the price of Ohio 6-percent bonds. Of course, none of these panels control for general market conditions, which further complicates a causal interpretation but is, again, consistent with the methodology employed by much of the existing literature.

**Figure 2: Bond prices near the time of reform**



In contrast to the existing literature, I employ a difference-in-differences methodology to study the relationship between borrowing costs and constitutional reform. This approach offers two primary improvements over the existing literature. First, it allows me to separate the effect of reform from general market conditions. Second, while it is true that the experience of each sovereign is to some extent unique, by considering the experience of many sovereigns, I am able to better understand what is common about the market response. My estimating equation is as follows:

$$P_i(t) = \alpha + \beta_1 newcon(t) + bond\ FE's + time\ FE's + \varepsilon_i(t) \quad (1)$$

where  $P_i(t)$ , denotes the log of the price of bond  $i$  during month  $t$ . The variable  $newcon(t)$  is an indicator variable, which equals one if bond  $i$  was issued by a state that reformed its constitution by time  $t$ . States reform their constitutions at different times. Thus, I limit the treatment effect to the first year to ensure that the estimate isn't biased towards states that reform at an earlier time.<sup>15</sup> I include time fixed effects to control for any general market conditions. In contrast to the finance literature, which would compare the evolution of each bond price to a market control, time fixed effects remove general market conditions as a source of bias by removing common movements across assets. This also limits the extent to which results can be explained by incorrectly specifying the market control asset.

The estimating equation also includes bond-specific fixed effects. These fixed effects, which normalize the price data, remove as a source of bias the fact that the price of bonds issued by each group (defaulters that reform their constitution, defaulters that do not reform, non-defaulters that reform, and non-defaulters that do not reform) may be systematically different. Because bonds are inherently state specific, the inclusion of bond fixed effects also removes systematic state-level variation.

The primary variable of interest in equation (1) is  $newcon(t)$ , which measures the extent to which the price of a state's bonds change following reform. The time and bond fixed effects ensure that estimates of  $newcon(t)$  are not biased by general market

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<sup>15</sup> More specifically, I interact each bond with an indicator for whether the observation occurs more than 12 months after constitutional reform. The inclusion of these interactions effectively re-normalizes the post reform data, which allows me to include these observations to more precisely estimate general market conditions.

fluctuations or by systematic state-level changes. Identification is further aided by the fact that states reformed their constitutions at different times.

Because reforms were not randomly assigned, we may worry that the price of bonds issued by defaulting states were evolving in a systematic way prior to reform. For instance, states that defaulted during the crisis may have chosen to adopt reforms after exhausting other efforts to establish credibility. To the extent that these efforts also affect bond prices, then we would be concerned that any estimates of reform are confounded by these policies. One feature of the setting I study is that, while reforms were not randomly assigned, there is plausibly exogenous variation in the timing of the reforms.

Variation in the timing comes from two sources: (1) procedures that dictated when and how a convention could be called, and (2) the actual length of the convention. States typically held conventions to discuss the content of the new constitution, and only after the convention agreed on the language of the proposed constitution would the new constitution would then be approved either by the state legislature or by ballot in a general election. However, existing constitutions often dictated when and how a convention could be called. For many states, state legislatures could not call for a convention unless they received approval in a general election. Once approved, the legislature could call for a convention in its next session. Some constitutions specified the amount of time that could pass between approving and hosting a convention (usually the convention was to be held within three to six months) but this was not universal.

Once a convention was called there was no set end date and so debates related to any aspect of the constitution (debt provisions, term limits, voting procedures, etc.) would naturally delay the process of reform. Consequently, the length of time between hosting a convention and reforming the constitution varied from state to state. New Jersey was able to reform its constitution within four months of hosting a convention, while it took 13 months for Ohio. On average, the delay between hosting a convention and adopting a new constitution was about eight months. Because reform was a slow-moving process, there were often large delays between calls for reform and reform

itself. On this point, the New York state legislature first called for a limit on state borrowing in 1842, but the constitution establishing that debt limit was not enacted until 1846 (Wallis, 2005). Similarly, the governor of Indiana recommended the adoption of borrowing limits and procedural restrictions as early as 1848, but those reforms were not adopted until November of 1851 (Dove, 2012).

With this source of variation in hand, it must be the case that any confounding variables must not only be systematic but must also match the precise timing of the reform. To further alleviate concerns on this front, my preferred specification includes state-specific linear time trends. If one is concerned that the price of bonds issued by defaulting states were trending up prior to reform, say because the states were slowly regaining credibility in the market by continuing to borrow and repay debts, the inclusion of these time trends, which effectively de-trend the price data, remove this source of bias. Under this specification, it must be the case that any competing story has to occur systematically across only reforming states, must precisely match the timing of these reforms, and has to manifest as a discrete change in bond prices.

### *III.c. Constitutional reform and the cost of borrowing*

Table 3 presents difference-in-difference estimates of the effect of constitutional reform on the log of asset prices. Because the sample does not have enough states to cluster standard errors at the state-level, I instead adjust standard errors using the wild bootstrap procedure discussed in Cameron et al. (2008).<sup>16</sup> The first column of Table 3 estimates equation (1). This specification indicates that bond prices increased by 1.9 percent (p-value of 0.06) following constitutional reform. This suggests that markets responded favorably to the adoption of these reforms.

Of course, if these estimates truly reflect the market response to constitutional reform, then we would expect the market response for states that defaulted during the crisis to be different than the response for states that did not default. This is because the act of reform likely conveyed different information based on the state's current

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<sup>16</sup> The wild bootstrap is preferred in this context as it does not assume regression errors are independently and identically distributed and it relaxes requirements of a balanced sample.

reputation. The act of default demonstrated a willingness to impose large costs on creditors; bondholders were never fully compensated for missed payments and many states adjusted the terms of repayment.<sup>17</sup> Accordingly, these states stood to benefit the most from reducing any lingering uncertainty as to whether bondholders might experience a similar loss in the future. Thus, to the extent that markets perceive constitutional reforms as a credible commitment, the response should be largest for these states.

How markets should respond to the adoption of constitutional reforms by non-defaulting states is less clear. Because states incurred large costs to avoid default, avoiding default might have already demonstrated commitment to debt repayment.<sup>18</sup> If so, the act of reform would not convey new information and consequently markets would not react. Alternatively, the act of reform might have the unintended consequence of *introducing* uncertainty. This is because markets might interpret the adoption of debt limits and other procedural restrictions as a signal that the state is concerned about its fiscal situation. This is particularly likely since not all non-defaulting states reformed their constitutions. Therefore, a skeptical investor might be concerned about why one non-defaulting state chose to adopt these seemingly beneficial provisions other non-defaulting states did not.

To accommodate the fact that reform likely conveyed different information for reforming states that defaulted during the crisis (relative to those that did not default), I allow the effect to vary by reputation in the second column of Table 3. Once states are grouped by whether or not they defaulted during the crisis, I find that bonds issued by states that defaulted during the crisis appreciated by approximately 12 percent following the adoption of constitutional reforms while bonds issued by non-defaulting states fell by about 4 percent. In the third column, I include state-specific linear trends. Perhaps unsurprisingly, the inclusion of both time fixed effects and state-specific linear trends

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<sup>17</sup> Ratchford (1966) discusses both repudiation and debt adjustment in chapter five of his book *American state debts*.

<sup>18</sup> For instance, New York avoided default by suspending its projects and reinstating its property tax; Alabama liquidated several branches of its state bank and reinstated its property tax; Ohio continued to finance its projects but raised property taxes dramatically – from 0.235 percent in 1837 to 0.5 percent in 1843 and 0.8 percent in 1845; and Tennessee increased its tax rate by 50 percent in order to meet its debt obligations.

attenuates the results. Nevertheless, results are largely consistent: bonds issued by defaulting states appreciated by 4 percent following reform while bonds issued by non-defaulting states fell by 1 percent.<sup>19</sup>

As an alternative to state-specific linear trends, I have also run the analysis using group-specific splines for reforming states. Specifically, I include a continuous linear trend for defaulters and non-defaulters and then I include a second linear trend (for defaulters and non-defaulters) that turns on at the time of constitutional reform. This specification is effectively a compromise between state-specific trends and no trends at all. Under this specification, I find that the price of bonds issued by defaulting states increased by 7.6% (significant at the 1-percent level) following reform, while the price of bonds issued by non-defaulting states fell by 4.3% (also significant at the 1-percent level). Because this specification varies substantially from the earlier specifications, I do not report the results in Table 3.

**Table 3: Effect of constitutional reform on ln(bond prices)**

	(1)	(2)	(3)
Post constitutional reform indicator	0.019* (0.010)		
Post reform indicator (States that defaulted during the crisis)		0.124*** (0.019)	0.041** (0.020)
Post reform indicator (States that did not default during the crisis)		-0.045*** (0.007)	-0.014** (0.006)
Asset fixed effects	Y	Y	Y
Time fixed effects	Y	Y	Y
State specific time trends			Y
R-squared	0.455	0.470	0.595
Observations	5431	5431	5431

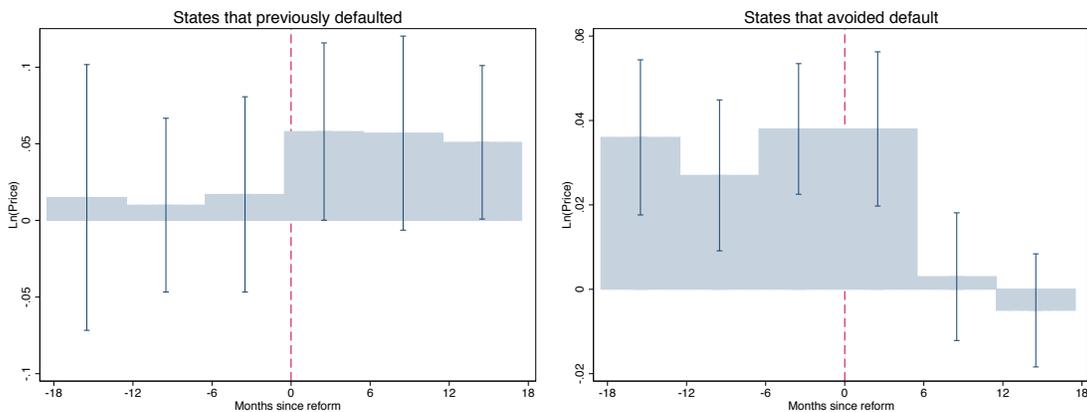
Bootstrapped standard errors reported in parenthesis.

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

<sup>19</sup> These results seemingly contradict the findings of Dove (2012). Using a cross section of bond prices from October of 1850, 1855, and 1860, Dove documents a positive relationship between debt provisions and average bond prices, regardless of default status. These findings are not necessarily mutually exclusive, as a simple cross section (after reform) does not allow one to identify how bond prices changed in response to reform.

Figures 3 and 4 present results that help validate my empirical approach. Specifically, I take the estimating equation from column 3 of Table 3 (bond fixed effects, time fixed effects, and state-specific linear trends) but instead of modeling reform as a simple indicator I include a series of indicators that turn on for each six month bin in the 18 months preceding and following constitutional reform. As in Table 3, I generate separate indicators for states that defaulted and states that avoided default. This event study approach allows me to more precisely identify the timing of the market response. It also allows me to visualize whether prices were trending prior to reform. If there is a trend in the pre-reform data then we would be concerned that the identifying assumption of parallel trends does not hold. Results for states that defaulted during the crisis appear in the first panel of Figure 3 while results for states that avoided default appear in the second panel. Neither panel indicates the presence of pre-existing trends. For states that defaulted, we see a jump in bond prices of roughly 4 percent immediately following the adoption of the new constitution. For states that avoided default, we see that prices were trading at a roughly 4 percent premium prior to reform, but about 6 months after reform that premium disappears. Because states adopted reforms at different times it is important to note that the systematic decline cannot be explained by general market conditions. The delayed response combined with the pre-existing price premium does suggest that we may want to interpret the effects for non-defaulting states with caution.

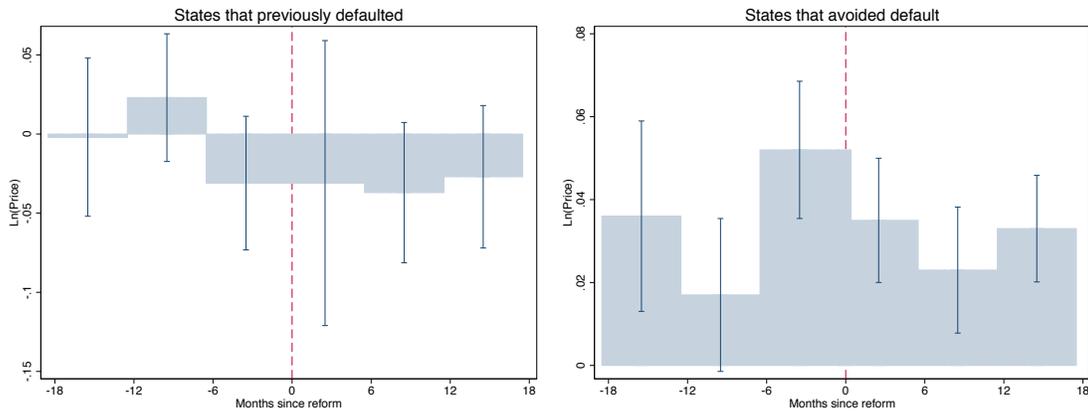
**Figure 3: Event study**



In addition to the coefficients being displayed, the full regression also includes bond fixed effects, time fixed effects, and state-specific linear trends. Confidence intervals represent bootstrapped standard errors.

Figure 4 presents the results of a placebo event study. Specifically, I run the exact same regression as in Figure 3, but instead of using the actual month of constitutional reform I use a false reform date of 18 months before the reform takes place. Again, the first panel presents results for states that defaulted during the crisis and the second panel presents results for states that avoided default. In both panels we see no evidence of pre-existing trends and we also do not see a discrete change in price following our false reform date. This lends further support to the identifying assumptions employed in Table 3.

**Figure 4: Placebo event study**



In addition to the coefficients being displayed, the full regression also includes bond fixed effects, time fixed effects, and state-specific linear trends. Confidence intervals represent bootstrapped standard errors. Reform date is 18 months before the actual month of constitutional reform.

As discussed in Section III.b, states often held constitutional conventions to discuss the new constitution. Consequently, markets might have anticipated the provisions that would be included. In Table 4 I capture the anticipated response by including an indicator (interacted with default status) that is equal to one if the asset was issued by a state that hosted a constitutional convention by time  $t$ . In this specification, bond prices for states that defaulted during the crisis increased by 8 percent between the hosting of the constitutional convention and the enactment of reform. Prices then increased by an additional 9 percent following the adoption of the constitution. For non-defaulting states, bond prices fell by 3 percent during the convention and they fell an additional 2 percent after enactment of the reforms. As in Table 3, the inclusion of state-

specific trends attenuates these results, but the qualitative story remains: defaulting states were rewarded for adopting these reforms, while non-defaulting states were slightly punished. Using a group-specific spline (as discussed in the previous paragraph) I find that bonds issued by defaulting states appreciated by a total of 9 percent (significant at the 10-percent level) while the price of bonds issued by non-defaulting states fell by a statistically insignificant 1.4 percent.

**Table 4: Anticipated vs. unanticipated effect of constitutional reform**

	(1)	(2)
Post convention indicator (States that defaulted)	0.084*** (0.033)	-0.024 (0.034)
Post reform indicator (States that defaulted)	0.093*** (0.018)	0.048** (0.021)
Post convention indicator (States that did not default)	-0.033*** (0.009)	0.002 (0.010)
Post reform indicator (States that did not default)	-0.019*** (0.007)	-0.016*** (0.006)
Asset fixed effects	Y	Y
Time fixed effects	Y	Y
State-specific time trends		Y
R-squared	0.476	0.596
Observations	5431	5431

Bootstrapped standard errors reported in parenthesis.

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

### *III.d. Putting the magnitude of the market response in perspective*

The results thus far indicate that markets responded favorably when states with tarnished reputations adopted constitutional reforms. While it would be useful to relate this to a precise decline in the cost of borrowing, that exercise is not possible because the necessary details (time until maturity and coupon rate) are not available for a majority of the bonds in the sample. However, one way to put the magnitude of the response in perspective is to compare the market responses to both constitutional reform

and to the resumption of payments. When states suspended their payments the value of the bonds dropped dramatically, reflecting the increased risk to holding the asset. At the time it was unclear whether states would make investors whole or whether they would partially or fully repudiate their debts. Thus, the price of the bonds fell to reflect the uncertainty in repayment that was introduced by the state's decision to temporarily suspend payments. Once a state resumed payments, however, the return from holding the bond became more certain and so bond prices should increase to reflect that reduction in uncertainty.

In Table 5 I regress  $\ln(\text{bond prices})$  on asset and time fixed effects as well as the same convention and reform indicators included in Table 4. In addition to each of these variables, I add an indicator variable that turns on when a defaulting state resumes payment. The first column of Table 5 presents results without state-specific time trends. There we see that following the resumption of payments, bond prices increased by roughly 14 percent. Relative to Table 4, the post convention for indicator is smaller in magnitude, reflecting the fact that, for states that defaulted during the crisis, constitutional conventions typically followed the resumption of payments. Nevertheless, the post-reform indicator for states that defaulted during the crisis is largely unaffected by the inclusion of the resumption variable – the magnitude is roughly 9 percent in both tables. In Column 2, when state-specific time trends are included the magnitude of the resumption indicator is roughly 5.5 percent while the post-reform indicator is nearly identical at approximately 5.3%. These results suggest that the market response to reform was indeed a meaningfully large response – roughly on par with the response to the resumption of payments. Both the resumption of payments and the adoption of a new constitution increased bond prices, which is consistent with the market interpreting both actions as a reduction in payment uncertainty. That the magnitudes of both actions are similar suggests that markets interpreted constitutional reform as a credible and meaningful commitment to debt repayment.

**Table 5: Was the magnitude of the market response meaningful?**

	(1)	(2)
Post resumption of payment indicator	0.144*** (0.009)	0.056*** (0.008)
Post convention indicator (States that defaulted)	0.038 (0.029)	-0.030 (0.031)
Post reform indicator (States that defaulted)	0.091*** (0.021)	0.053** (0.021)
Post convention indicator (States that did not default)	-0.009 (0.010)	0.002 (0.009)
Post reform indicator (States that did not default)	-0.020*** (0.006)	-0.017** (0.007)
Asset fixed effects	Y	Y
Time fixed effects	Y	Y
State-specific time trends		Y
R-squared	0.506	0.599
Observations	5431	5431

Bootstrapped standard errors reported in parenthesis.

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

#### IV. THE ROLE OF PARTIES

The results presented thus far have shown that states that defaulted during the crisis benefited the most from reforming their constitutions. As illustrated in Table 1, defaulting and non-defaulting states adopted very similar provisions, and so it is not simply the case that only defaulting states adopted provisions that markets valued. One might be concerned, however, that unobserved institutional changes (correlated with default status) are driving these results. The composition of the state legislature is one of the more plausible mechanisms, as constitutions are politically devised constraints. David Stasavage's work speaks directly to this point. Stasavage has shown that political composition has the potential to affect the perceived commitment to constitutional constraints. In his critique of North and Weingast's interpretation of the Glorious Revolution, Stasavage (2002 and 2007) argues that interest rates in England did not fall until capital owners were better represented within parliament. Relatedly, in his analysis

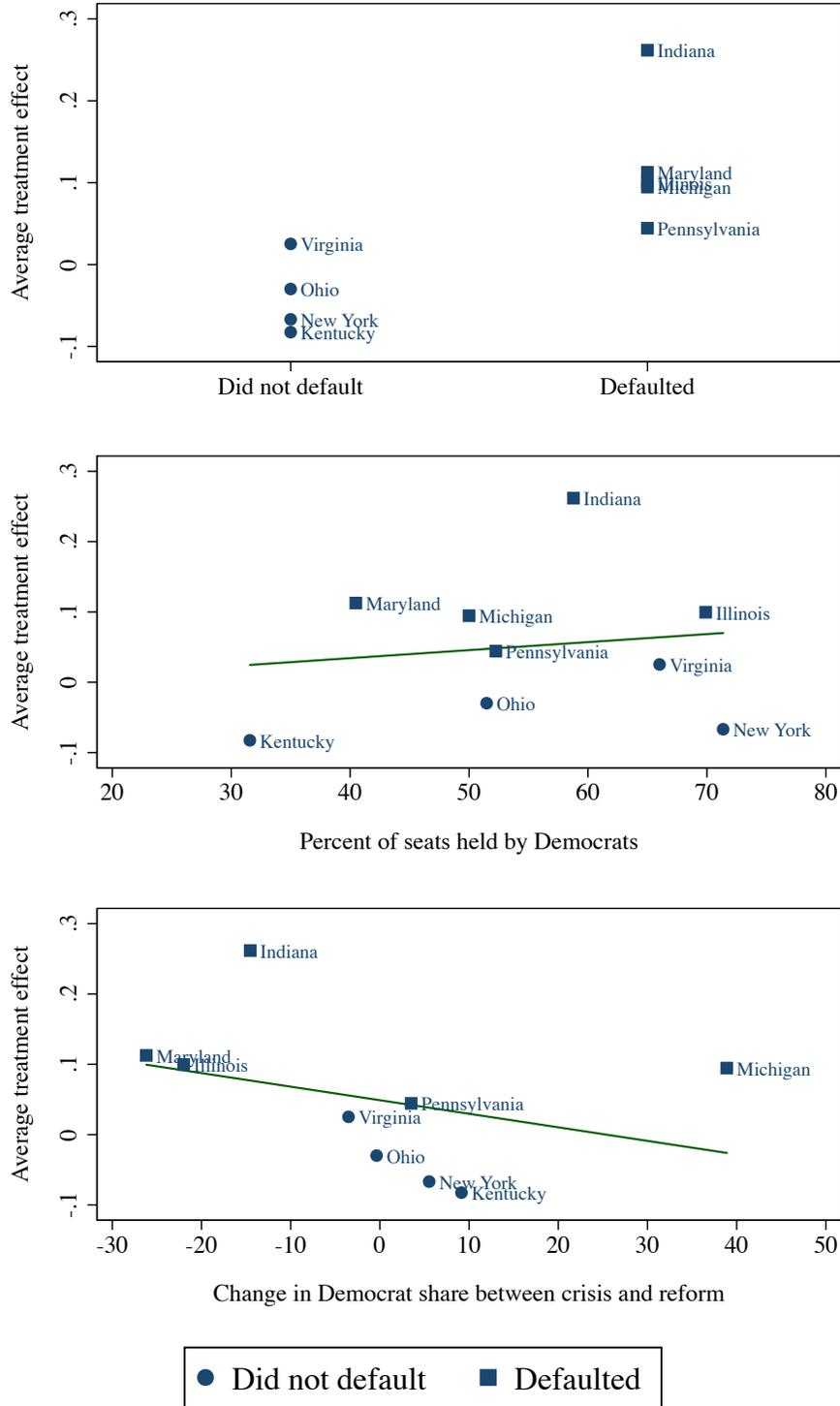
of Europe during the early modern period, Stasavage (2008) finds that interest rates were lower for sovereigns whose creditors wielded political power.

Political composition is an appealing alternative hypothesis, particularly since states have had varied experiences with the stability of their constitutions (Berkowitz and Clay, 2005). The two political parties during this time period were the Whigs and the Democrats, with the Whigs opposing the writing of new constitutions in several states (Wallis, 2005). Thus, it may seem intuitive that states with a stable Democratic majority were rewarded because investors were assured that the reforms would not be overturned in the future. But constraining the powers of the legislature was not a party issue. Louisiana's experience more precisely illustrates this point. Louisiana first replaced its constitution in 1845 when the Democratic Party controlled the legislature and again in 1852 when the Whig Party controlled the legislature. Yet when the Whig-controlled legislature was in charge of re-writing the constitution, they kept all of the debt-related constraints that were adopted by the Democrat-controlled legislature. Both constitutions constrained the legislature's ability to issue new debt, imposed limits on borrowing, required the legislature to provide adequate ways for financing new borrowing, and prohibited investment in corporations.

I rely on Dean Burnham's Partisan Division database to better study the extent to which the political composition of state legislatures interacts with the market response to constitutional reform. This dataset reports the number of legislators in the upper and lower house of each state that belong to each party. The data are reported annually or biennially from 1834-1985. The frequency could vary because observations are genuinely missing or because there was not an election in the year (and thus party composition didn't change). Because it is unclear why the frequency varies, I focus on each state's average composition in the five years prior to reform. I then estimate state-specific treatment effects by estimating a variation of equation (1) where I use state-specific indicators for hosting a convention and implementing reform (bond fixed effects, time fixed effects, and state-specific linear trends are still included). Each reforming state's treatment effect is then defined as the linear combination of the state specific coefficient for hosting a convention and implementing reform.

The results of this exercise are presented in Figure 5. In the first panel of the figure, I organize each state's treatment effect based on whether the state defaulted during the crisis. Consistent with the results presented in Section III, this panel shows that the treatment effect for every defaulting state dominates the treatment effect for every non-defaulting state. In the second panel, I organize the treatment effects based on the average share of the upper house that was controlled by the Democratic Party in the five years preceding reform. While there appears to be a slight positive relationship between average Democratic share and the size of the treatment effect, this is largely driven by the fact that so many defaulting states had a Democratic majority. Of course, the Democratic share is only a potential confounder if states that defaulted are becoming more Democratic at the same time that they are reforming their constitutions. Thus, in the final panel of Figure 5 I organize the treatment effects based on the change in the Democratic share between the debt crisis and the time of reform and find a *negative* relationship between the size of the treatment effect and whether the Democratic party was becoming better represented within the house. Together, the second and third panels of Figure 5 indicate that the treatment effects were higher for (1) states with a high Democratic share at the time of reform and (2) states with a lower Democratic share relative to before the crisis. The inconsistency of panels two and three suggests that any relationship between political composition and the size of the treatment effect is likely spurious.

**Figure 5: State-specific treatment effects**



Each state's treatment effect is obtained using a variation of equation (1) where I include state-specific indicators for hosting a constitutional convention and for reforming the constitution. The treatment effect is the linear combination of these two indicators.

## VI. CONCLUSION

Do markets value the adoption of institutional constraints that reduce payment uncertainty? Previous empirical work has relied on case studies that analyze a single time series. Consequently, existing empirical work is inconclusive. America's 1840s state debt crisis, however, presents a unique opportunity to analyze this question with a panel of sovereigns. This setting, which allows me to control for general market trends, is better suited for inferring causality. By exploiting the plausibly exogenous timing in adoption of reform, I find that defaulting states were rewarded with lower borrowing costs following reform. Non-defaulting states, on the other hand, were slightly punished, possibly because skeptical investors were concerned that the adoption of these reforms signaled that the state was worried about its ability to repay future debts. These results cannot be explained by differences in the content of the constitutions, as the types of reforms that were adopted were largely universal. The results cannot also be explained by differences in political composition (and thus a perceived difference in commitment to the constitution), as these were not party issues.

While the results in this paper indicate that fiscal constraints may help sovereigns with tarnished reputations regain access to credit at favorable terms, there are, of course, long-run consequences that are not considered in this paper. For instance, how do these provisions affect a state's ability to act during a future economic crisis? Although balanced budget rules and debt constraints lower borrowing costs, if those constraints become binding during a downturn the state will be unable to implement fiscal policies that might help mitigate the crisis. As another example, how might these constraints affect the provision of public goods? With states constrained in their ability to borrow, investment in waterworks and other public utilities was often made at the city-level (Troesken, 2015). While some cities were able to access credit markets, others relied on private investment. The extent to which this arrangement was efficient remains unclear. Analysis of these and other long-run consequences remains an important avenue for future research.

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