JEFFREY J. HARDEN
*University of Notre Dame*
JUSTIN H. KIRKLAND
*University of Virginia*
PATRICK E. SHEA
*University of Houston*

# *Legislative Transparency and Credit Risk*

Open governance requirements are designed to improve accountability, which implies that transparent governments are more trustworthy stewards of their publicly invested power. However, transparency may also reduce institutional effectiveness and inhibit political compromise, diminishing the capacity to manage resources responsibly. We assess empirical support for these competing perspectives in the context of American state legislatures, many of which have become exempt from state sunshine laws in recent decades. We leverage variation in the timing of these legislative exemptions to identify the effect of removing transparency in a crucial governing institution on investors' risk perceptions of states' general obligation bonds. Our analysis of these data during the period 1995–2010 suggests that removing legislative transparency *reduces* state credit risk. We conclude that while openness in government may be normatively desirable, shielding legislative proceedings from public view may actually be better for states' debt repayment capacity, improving their overall fiscal health.

## Introduction

Transparency in politics reduces moral hazard problems. If voters can monitor elite behavior, politicians have less incentive to pursue policies that are inconsistent with public preferences. To facilitate this accountability mechanism, many governments around the world have adopted open governance reforms, such as so-called sunshine laws in the United States. In an effort to promote openness, every state requires some form of transparency in governmental proceedings and meetings, such as open records laws, disclosure requirements, and proactive record publication. Importantly, however, states exhibit variation in the application of

these reforms to various institutions of state government. For example, some states do not subject records of the governor's office to freedom of information requirements while others exempt state legislatures from open governance obligations. In this article, we leverage this variation in requirements. We examine how turning off transparency rules in state legislatures influences states' perceived ability to repay debts, which is a key indicator of the quality of life that a state can provide its citizens.

Specifically, we assess how exemption from sunshine laws in state legislatures impacts states' perceived credit risk. In doing so, we develop and adjudicate between two competing theoretical explanations. According to one, an open legislative process lowers credit risk and signals increased debt-repayment capacity because transparency provides investors with more information and certainty to anticipate potential change in the policy environment. On the other hand, open governance shines light on the details of the policymaking process, such as coalition building, compromise, and deal making across party lines. Legislators may feel constrained if these behaviors are observable by the public and, consequently, fail to make optimal fiscal decisions out of fear that compromise could threaten their electoral fortunes (Harbridge and Malhotra 2011; Harden and Kirkland 2018). In other words, transparency creates a tension between the challenging work of real policymaking and the ability to credit claim and promote legislative accomplishment.

The former perspective—that opening governmental proceedings lowers credit risk—may initially seem to be the most plausible. Indeed, existing research supports the claim that transparency and reduced uncertainty generally improve sovereign creditworthiness (e.g., Kim and O'Neill 2017). Moreover, credit rating agencies themselves directly state that they value transparency when evaluating credit. Standard & Poor's (S&P) published methodology for evaluating governments' credit risk, for instance, includes a section on "institutional assessment," which specifically mentions "the transparency and accountability of its institutions, data, and processes" (2017, 1) as elements that can improve a state's rating.[1]

However, that very same document also identifies institutional effectiveness, the ability to maintain sustainable public finances, and the capacity to quickly respond to crises as pivotal elements in credit evaluations (Standard & Poor's 2017). A robust literature in political science suggests that transparency may counteract these institutional traits (e.g., Arnold 1990; Groseclose

2001; Mixon, Hobson, and Upadhyaya 2001; Stasavage 2004). Transparent institutions may make bargaining more challenging, promote grandstanding, and increase the influence of lobbyists and organized interests (Longley and Oleszek 1989; Mixon, Gibson, and Upadhyaya 2003; Meade and Stasavage 2008). As Heald describes: "some transparency is needed to deter fraud and corruption … [but] too much leads to losses in effectiveness through … excessive politicization" (2003, 727). Groseclose and McCarty even contend that closed negotiations may be better for bargaining because sunshine laws reduce efficiency in the process (2001, 114). Legislators themselves have expressed similar sentiment. Washington state senator Mark Schoesler, for example, defended his legislature's 2018 exemption from Washington's Public Records Act by stating that "[j]ust as important as transparency is the ability of lawmakers to effectively work on behalf of those who sent us here" (La Corte 2018).

In what follows, we test these competing arguments empirically to ascertain whether institutions that enforce transparency in state legislatures help or hinder state credit evaluations. Using data on state credit risk from Fortunato and Turner (2018) and novel temporal data on legislative exemptions from state sunshine laws, our analysis supports the latter perspective: shielding the legislature from sunshine laws leads to a moderate, but discernible, *decrease* in the risk associated with state governments' general obligation bonds. Thus, while open governance initially appears normatively positive (e.g., Florini 2007) and is even appealing to the very agencies that conduct states' credit reviews, our work indicates that it carries tangible adverse effects on states' capacity to repay debt.

## The Sovereign Creditworthiness of the American States

How do governments, such as the American states, convince investors to lend them money? There is a rich literature dedicated to this question given the importance of credit for fiscal policy and political survival (Morrison 2009; DiGiuseppe and Shea 2016, 2018). Beyond fiscal policy, sovereign credit represents governments' credibility, a concept that extends into all policy realms. Therefore, determining how states build and maintain credibility in credit markets can impart information about governance in general. Much of the US state sovereign credit literature focuses on institutional constraints (Alt and Lowry 1994; Lowry and Alt 2001; Johnson and Kriz 2005; Crain 2009; Kelemen and Teo 2014). For

example, Lowry and Alt (2001) examine how deficit spending influences general obligation bond interest rates, conditional on balanced budget laws and divided government. These rules may have different effects under different political parties (Lowry and Alt 2001) or under divided government (Alt and Lowry 1994). Usually, this institutional research focuses on the constraining power of such rules, although Kelemen and Teo (2014) argue that they act as focal or coordination points for markets, facilitating punishment for states that deviate.

Besides internal institutions, other research has focused on the relationship between states and the US federal government. The federal government has helped state governments in previous financial crises because of institutional incentives to commit to the solvency of state economies (Rodden 2002). While federal bailouts help avoid default during crises, the expectations of bailouts may lead state governments to engage in behavior that leads to a crisis in the first place. Wibbels (2000) argues that federal systems incentivize subnational governments to forego fiscal discipline, akin to the moral hazard problem of insurance (see also Wibbels 2003). Additionally, the presence of federal institutions does not necessarily prevent the occurrence of extreme fiscal crises (von Hagen 1991). However, some argue that this moral hazard dynamic does not apply to state politicians because fiscal policy does not appear sensitive to expectations of bailouts in the future (Rodden 2012). Others have demonstrated that the influence of the federal government depends on the economic performance of the state (McBrayer, Shea, and Kirkland 2018).

Institutions can have constraining or coordinating effects on sovereign creditworthiness. Additionally, institutions such as transparency requirements can provide information to lenders that they might not otherwise possess, reducing uncertainty surrounding fiscal policy.[2] We define transparency as accessibility to information, and we focus our attention on sunshine laws that increase the accessibility of information on deliberative legislative processes.[3] Thus, transparency may improve perceptions of creditworthiness by reducing uncertainty about fiscal choices, or it may hamper creditworthiness by creating a more exposed and challenging policymaking environment, in which several of legislators' principals can observe and react to lawmakers' choices. In the next section, we develop these competing arguments and demonstrate how institutions requiring transparent decision-making in state legislatures might improve or harm perceptions of a state's creditworthiness.

## Information, Credibility, and Credit

The central principle of sovereign borrowing is lenders assessing a government's risk of default. This risk assessment is a function of a state's ability and willingness to repay its debts. The former is a function of the state's revenue streams and legislative capacity (Fortunato and Turner 2018). States with more fiscal resources and more ability to affect policy changes are better able to meet debt obligations. Willingness to repay debt, however, is more difficult to infer. Willingness represents a government's credibility to maintain debt obligations, even if economic or political conditions worsen. Given politicians' incentives to maintain office, unpopular policies such as spending cuts or higher taxes may be difficult to enact if a budget crisis arises. Moreover, legislators who are mindful of the next election focus on the short term, but fiscal policies may take more time to realize their intended effects. As a result, governments face a credibility problem in the credit market. Investors are only willing to lend if they are convinced that the benefits of lending (interest payments, plus principal) outvalue the risks (default). To be convinced, investors need assurances that the government is politically willing to repay debt obligations.

Assessing political willingness is a challenge, especially because individual investors have little incentive to monitor. Instead, investors free ride on the information acquisition of other investors, following the investment decisions of the well-informed lenders (Hauswald and Marquez 2006). One solution to this collective action problem is the emergence of credit rating agencies. These financial intermediaries monitor lenders and provide risk assessments in the form of "grades." In exchange for their assessments, credit rating agencies receive fees from both a borrowing state (if the assessment was requested) and from investors for access to the assessment information. The key question we seek to answer centers on how opening or closing the proceedings of a major institution of government influences these agencies' assessments of state credit. We detail our two competing perspectives below.

### Transparency Improves Creditworthiness

Despite their incentive to monitor lenders, credit rating agencies still face information asymmetries with lenders. Only governments know their true willingness to repay debt when economic conditions worsen, though all governments will claim they are

willing. How do US states convince investors or credit rating agencies of their creditworthiness? Previous sovereign credit literature has focused on the constraints facing state legislatures. While these rules do constrain states from committing fiscal abuse, they leave room for fiscal discretion (Bohn and Inman 1996). In addition, these institutions are generally static, and thus they do not help explain the temporal variation exhibited by states' credit ratings.

In addition to focusing on the constraining role of institutions, we argue that institutions also provide information and help reduce the information asymmetry between borrower and lender. Governmental transparency requirements, such as state sunshine laws, reduce the uncertainty surrounding the states' fiscal policies. This information gain may lead to better risk assessments from credit rating agencies. Indeed, markets generally prefer to minimize uncertainty, even if it means revealing risk (Ellsberg 1961; Segal 1987).

As part of the reduction of uncertainty, transparency requirements provide politicians the ability to signal effort. Not all fiscal distress is equal. Creditors will give lenders the benefit of the doubt if all policy options are utilized to maintain debt obligations, even if the fiscal outcome is still suboptimal. Conversely, lenders that avoid politically unpopular policy options will be "down graded" by credit rating agencies. Only in a fully transparent setting with open records and open meetings can credit rating agencies fully observe the government's effort in maintaining fiscal discipline. Accordingly, from this perspective transparency improves credit ratings and reduces the credit risk of states. Thus, exempting a legislature from a sunshine law would exert the opposite effect, as we state in the first of our two competing hypotheses.

*H1:* Exempting its legislature from a sunshine law increases a state's perceived credit risk by the major ratings agencies.

*Transparency Diminishes Creditworthiness*

The previous argument about information provision requires that state policymakers maintain the same approach to fiscal policy in both transparent and opaque settings. That is, it assumes that a state's willingness to repay its debts is unchanged by the transparency institutions themselves, and all that transparency does is provide otherwise difficult to obtain information. However, politicians themselves are often concerned about the

potential perverse incentives of transparency on decision-making (Harden and Kirkland 2019). A willingness to undertake a long-term commitment to repay old debt often requires difficult political choices that may hamper politicians' electoral performance in the short term. Those difficult choices may be easier to make in closed-door settings, where more open-minded bargaining can occur (Groseclose and McCarty 2001; Stasavage 2004). Indeed, Arnold (1990) notes that open-door meetings made it difficult for members of the US House's Finance Committee to make quality tax policy and often resulted in the expansion of preferred status within the tax code regardless of whether such expansions were fiscally appropriate.

One particular manifestation of the concerns about transparency is that institutions emphasizing legislative openness assist lobbying organizations in distorting public policy more than they assist the public in holding members accountable. For example, Longley and Oleszek (1989, 59) discuss a 1981 farm bill directly influenced by the mere presence of an agriculture lobbyist in the conference committee. Thus, granting an intense policy demander access may constrain legislators' behavior in ways that would not be possible in less transparent environments. Furthermore, given that citizens generally know very little about politics and have limited interest in the details of policymaking (Hibbing and Theiss-Morse 2002; Rogers 2017), transparency in practice may not actually serve as a mechanism for good governance and representation.[4] After all, if citizens pay little attention to politics, it is not at all clear how providing them with more information would translate into improved policy.[5]

In short, a policymaking body captured by lobbying organizations and run by legislators unwilling to alter policy positions is less likely to have the capacity or willingness to repay debts. To repay past debts or avoid new debts, politicians often must raise taxes or cut spending, policy options much less likely to be realized under pressure from interest organizations. Shielding the legislature from transparency may reduce these pressures, leading to a decrease in the state's credit risk. This alternative logic leads to our second hypothesis.

*H2:* Exempting its legislature from a sunshine law reduces a state's perceived credit risk by the major ratings agencies.

*Transparency Is Unrelated to Creditworthiness*

While we have good reason to suspect that transparency efforts ought to influence financial markets, it is worth considering the plausibility of the null hypothesis. We suggest that voters are unlikely to notice changes in state legislative transparency, and thus, are unlikely to alter the degree to which they hold legislators accountable for the fiscal choices they make before and after changes in transparency levels. It is also possible that credit rating agencies indicate that they care about transparency as a normative goal, but in practice they are almost exclusively worried about balance sheets. Indeed, to the degree that credit ratings are mostly about ability to repay debt rather than willingness to repay debt, transparency institutions that alter willingness may be unrelated to credit ratings. Finally, agencies may also suspect that state governments that are both unwilling and unable to repay debts may be bailed out by the federal government, and thus, state legislative institutions are unrelated to credit (Wibbels 2003; McBrayer, Shea, and Kirkland 2018).

Additionally, selection pressures could conceivably generate a null result. Fiscal discipline often requires partisan compromise, but such compromise could result in electoral punishment from constituents (Harden and Kirkland 2018). States that have already demonstrated credibility in credit markets and enjoy easy access to credit receive marginal benefit from ending legislative exemptions to transparency laws. With credibility already established, politicians can exempt themselves from sunshine laws and decrease scrutiny from constituents. Conversely, state governments that have historically demonstrated poor fiscal management and are deemed more of a default risk would benefit by exempting their legislature from sunshine policies, but they cannot plausibly make an argument to citizens that exemptions are beneficial precisely because of that government's history of poor performance. As such, "good governments" do not need exemptions, but have them, and "bad governments" need exemptions but cannot have them. This sort of theoretical environment would create a clear between-state difference in credit for states with and without exemptions, but it would show no meaningful differences within states before and after their adoption of exemptions to sunshine laws.

Finally, as we detail in our section on research design, the implementation of legislative exemptions to sunshine laws is uncommon. Ultimately, the rarity of this event may make detecting

an effect of transparency changes exceedingly difficult. All three of these perspectives suggest that a null result is quite plausible in this research.

> *H0:* Exempting its legislature from a sunshine law has no effect on a state's perceived credit risk by the major ratings agencies.

### Research Design

Our primary goal is to evaluate how transparency in state legislatures influences credit rating agencies' evaluations of state credit risk. We do so by building on the work of Fortunato and Turner (2018), who construct a temporal measure of latent credit risk—a state's capacity to repay debt obligations—among the 40 states that issue general obligation bonds.[6] They generate this indicator from a Bayesian hierarchical-ordered probit model, which takes as its inputs ratings from the three major rating houses (Fitch, Moody's, and S&P) and yields a posterior distribution for the latent credit risk of each state in each year from 1995 to 2010. This approach is useful because it (1) places states' credit risk on a common scale in both space and time, (2) automatically incorporates missing credit rating data into its estimates, and (3) does not force a linearity assumption in the use of credit ratings, which vary across rating houses (Fortunato and Turner 2018, 628). We model this measure of credit risk as a function of transparency in the state legislature and several other variables, as we describe below.

Our key variable of interest comes from data we collected on sunshine laws as they apply to state legislatures' meetings and proceedings in a given year. A legislature is covered by such a law if the state adopted one prior to that year and did not exempt the legislature. A legislature is not covered if no sunshine law existed in the state in a given year *or* if a critical part of the legislative process was exempt from an existing law. We searched legislative records to obtain the specific name, statute, and adoption dates for sunshine laws as well as the legislative exemption dates, if applicable. In some cases, exemption only applied to certain groups or points in the process. We coded a state as exempt if the legislative exemption covered any of the following institutions:

•Subcommittees;
•Committees outside of committees of the whole;

- Partisan caucuses;[7]
- Conference committees, personnel committees, and/or committees considering legislation not yet proposed before the entire chamber;
- Political committees, conferences, and caucuses;
- Ethics caucuses; and
- Political parties, groups, caucuses, rules, or sifting committees.

These circumstances represent substantively important situations for policymaking—those in which legislators might alter a bill, deliberate over a bill, or engage in coalition building.[8]

Additionally, there are some cases in which the state legislature was not exempt due to a rule outside of the state's sunshine law. For example, some states' open meetings laws do not mention the state legislature specifically, but transparency for the legislature is encoded in the state constitution. We also coded states as requiring legislative transparency if one of the following conditions were in place during a given year:[9]

- The presence of another statute that requires transparency in the legislature but is not part of the state's open meetings law;
- The state constitution;
- Chamber rules; and
- Court decision.

*Modeling Strategy*

Our empirical analysis combines the information on sunshine law adoptions and exemptions described above with Fortunato and Turner's (2018) data, which cover the period 1995–2010. These data allow us to leverage both cross-sectional and temporal variation in a series of panel regression models. Our outcome variable is the posterior mean of Fortunato and Turner's (2018) data latent credit risk measure, which is coded such that larger (smaller) values represent more (less) risk. The treatment variable is an indicator for the presence of a legislative exemption to a state's sunshine law.[10] Our first hypothesis predicts a positive coefficient on this variable (i.e., removing legislative transparency increases credit risk) while negative estimates are consistent with the second hypothesis, that removing transparency reduces risk. A coefficient near zero would provide favorable evidence for the null hypothesis.

Our coding of the treatment variable is the reverse of what might be the more intuitive choice—legislative exposure to a sunshine law. We consider exposure the "control" condition because nearly all states adopted some form of a sunshine law by the time these data begin (1995).[11] This approach—as opposed to the reverse coding—allows us to better leverage the temporal variation in the data because three states (Pennsylvania, Ohio, and Massachusetts) experienced legislative exemptions during 1995–2010.[12] Thus, we have pre- and posttreatment data for those states under this conceptualization of treatment.[13] Figure 1 displays the variation in our treatment variable. While we present a pooled specification that models cross-sectional variance below, our preferred model includes state fixed effects to control for time-invariant state-level confounders and year fixed effects to mitigate baseline trends in state credit risk over time.

More specifically, we first examine the effect of legislative exemption on credit risk through several panel models specified with our treatment indicator as well as several political and economic variables informed by the literature. We include all variables that Fortunato and Turner (2018) use in their own models with these data.[14] We also add Berry et al.'s (1998) measure of unidimensional governmental ideology and a folded Ranney index of partisan competition, although results are not dependent on their inclusion.

Our empirical strategy described above leverages the timing of legislative exemptions in the three states that receive treatment during the time period under study in what is essentially a difference-in-differences design. However, it is not a straightforward case because the data include variation in treatment timing as well as some states that were exempt (treated) in all years. The implication of the former issue relates to the specific quantity of interest that we can estimate. Rather than the typical average treatment effect on the treated (ATT) identified by such designs, our two-way fixed effects estimator gives a variance-weighted average of ATTs from all possible two-group (treated versus untreated units), two-period (before versus after) comparisons in the data (Goodman-Bacon 2018).[15] Additionally, as Figure 1 shows, our sample contains several states that were treated prior to 1995 and had legislative exemptions in place throughout our data. While these units do not pose problems for statistical identification of our panel models, they are not necessarily useful for causal identification in the context of a difference-in-differences design. Thus,

FIGURE 1
Variation in State Sunshine Law Legislative Exemptions

in our results below we present specifications with and without these states included in the estimation sample.

The difference-in-differences framework also carries the key parallel trends identifying assumption. We address this assumption in two ways. First, in the online supporting information we compare pretreatment trends in the treated states (i.e., those that became exempt during 1995–2010). Second, after discussing our panel models below we report similar results using the generalized synthetic control method (Xu 2017), which relaxes that assumption. Thus, with appropriate caution, we contend that our analysis represents a feasible means of gaining insight into how transparency—operationalized as legislative exemption from sunshine laws—affects states' perception as borrowers.

## Results

Table 1 presents coefficients and standard errors from our panel models of credit risk.[16] Model (1) is Fortunato and Turner's (2018) pooled feasible generalized least squares (FGLS) model with the treatment variable, governmental ideology, and the Ranney index added. Fortunato and Turner employ this estimator for its robustness to autocorrelation and heteroskedasticity in the error term (2018, 632).[17] In model (2) we add state fixed effects to the specification, which removes the time-invariant variables from the estimation. Finally, models (3)–(5) are our favored specifications; they include state and year fixed effects to control for time-invariant confounders and baseline time trends. Model (3) is estimated with FGLS, and models (4) and (5) are estimated with ordinary least squares (OLS).[18] These latter two models are identical in specification, but they differ in the estimation sample; model (5) excludes the 14 states with exempt legislatures during the entire 1995–2010 period.[19]

The coefficient on legislative exemption from sunshine laws is negative across the five specifications, but it exhibits some variation in magnitude. The outcome variable has a standard deviation of 1. Thus, the estimated effects of the treatment indicator are interpretable as proportions of a standard deviation in credit risk. Pooling cross-sectional and temporal variance (model (1)) leads to a substantively small effect (5% of a standard deviation decrease in credit risk) that is not statistically significant. Modeling within-state variance with state fixed effects in the latter specifications yields estimates that are relatively larger and statistically significant

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TABLE 1

The Effect of Legislative Exemption from Sunshine Laws on
Credit Risk in General Obligation Bond-Issuing States, 1995–2010

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Exempt | −0.05 | −0.25* | −0.36* | −0.32* | −0.38* |
| | (0.12) | (0.12) | (0.12) | (0.11) | (0.11) |
| Governmental Ideology | 0.01* | 0.01* | 0.01* | 0.01* | 0.01* |
| | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Ranney Index | −0.45 | 0.40 | 0.20 | 0.16 | −0.57 |
| | (0.32) | (0.28) | (0.27) | (0.42) | (0.51) |
| Squire Index | 2.44* | −2.25 | −1.22 | −0.87 | 1.22 |
| | (0.65) | (1.16) | (1.23) | (1.11) | (1.21) |
| Term Limits | 0.13* | −0.04 | −0.02 | 0.02 | 0.07 |
| | (0.07) | (0.06) | (0.06) | (0.05) | (0.07) |
| Divided Government | 0.11* | −0.07* | −0.05 | −0.03 | −0.02 |
| | (0.04) | (0.03) | (0.03) | (0.06) | (0.08) |
| Historical Turnover | 1.16 | −1.22 | −1.39 | 0.02 | −1.47 |
| | (0.93) | (0.83) | (0.88) | (1.11) | (1.50) |
| Unemployment Rate | 0.06* | 0.03 | 0.03 | 0.02 | 0.05 |
| | (0.02) | (0.02) | (0.03) | (0.05) | (0.07) |
| Per Capita Income | −0.24* | −0.11* | −0.42* | −0.62* | −0.85* |
| | (0.05) | (0.04) | (0.09) | (0.11) | (0.15) |
| Average Tax Burden | −0.67 | 7.08 | −2.37 | −1.05 | −13.21 |
| | (3.57) | (3.65) | (3.98) | (5.87) | (9.47) |
| Per Capita Spending | 0.19 | −0.20 | −0.05 | −0.36* | 1.83* |
| | (0.18) | (0.16) | (0.17) | (0.14) | (0.42) |
| Per Capita Revenue | −0.03 | 0.01 | 0.00 | 0.00 | −0.53* |
| | (0.06) | (0.05) | (0.05) | (0.07) | (0.27) |
| Per Capita Debt | −0.04 | 0.16 | 0.05 | 0.25* | −0.58* |
| | (0.10) | (0.09) | (0.09) | (0.09) | (0.21) |
| ACIR Lax | 1.03* | | | | |
| | (0.31) | | | | |
| Revenue Limit | −0.32 | | | | |
| | (0.25) | | | | |
| Spending Limit | 0.34 | | | | |
| | (0.20) | | | | |
| Debt Restriction | −0.27 | | | | |
| | (0.26) | | | | |
| Intercept | −0.56 | | | | |
| | (0.51) | | | | |
| State Fixed Effects | | ✓ | ✓ | ✓ | ✓ |
| Year Fixed Effects | | | ✓ | ✓ | ✓ |
| N | 640 | 640 | 640 | 640 | 416 |
| States | 40 | 40 | 40 | 40 | 26 |
| Adjusted R2 | 0.30 | 0.83 | 0.84 | 0.85 | 0.86 |

*Note*. Cell entries report coefficient estimates and standard errors (in parentheses). The outcome is credit risk (Fortunato and Turner 2018). Models (1)–(3) are estimated by FGLS, and models (4) and (5) are estimated by OLS. The sample used for models (1)–(4) includes all states that issue general obligation bonds. The sample for model (5) excludes the 14 states with exempt legislatures during the entire 1995–2010 period.

*$p < 0.05$ (two-tailed).

($p < 0.05$, two-tailed). We place more confidence in these models because they control for both observed and unobserved state-level confounders.[20]

Indeed, models (3)–(5) show support for the perspective that legislative transparency is problematic for state creditworthiness. States whose legislatures are exempt from sunshine laws show a reduction in risk compared to those whose legislatures are open. The magnitude of the effect is moderate in size—about one-third of a standard deviation in the observed outcome variable—but statistically distinguishable. In sum, these models provide support for the second of our two competing hypotheses; despite the credit agencies' declared methodology, shielding legislative proceedings from public view, not opening them, decreases the perceived risk of states' bond obligations.[21]

*Generalized Synthetic Control Method*

We also estimate the effect of exemption using Xu's generalized synthetic control method (GSC). A central goal of GSC is to "estimate the average treatment effect on the treated using time-series cross-sectional (TSCS) data when the 'parallel trends' assumption is not likely to hold" (2017, 57). It does so by estimating the ATT in each of the posttreatment time periods.[22] This approach allows us to relax the parallel trends assumption. However, it also reduces statistical power because the 14 states with legislative exemptions in place during the entire 1995–2010 time period drop out of the estimation, as in model (5) from Table 1.

We present results from the GSC model (which includes all time-varying covariates from Table 1) in Figure 2.[23] The top panel graphs the ATT and its bootstrapped 95% confidence interval (y-axis) over time since treatment (x-axis). That graph also reports the proportion of bootstrap estimates at each time point greater than zero along the x-axis. The effect is consistently negative, although the confidence interval includes zero for the entire range. This increase in uncertainty is not surprising given the reduced sample size. Nonetheless, the average ATT over all of the time points since treatment is −0.46 (nearly one-half of a standard deviation of the outcome) with a 95% confidence interval that is bounded away from zero: [−0.78, −0.06]. This result is consistent with the findings from models (3)–(5) in Table 1.

FIGURE 2
Estimated Exemption Effects Using Generalized Synthetic
Control Method
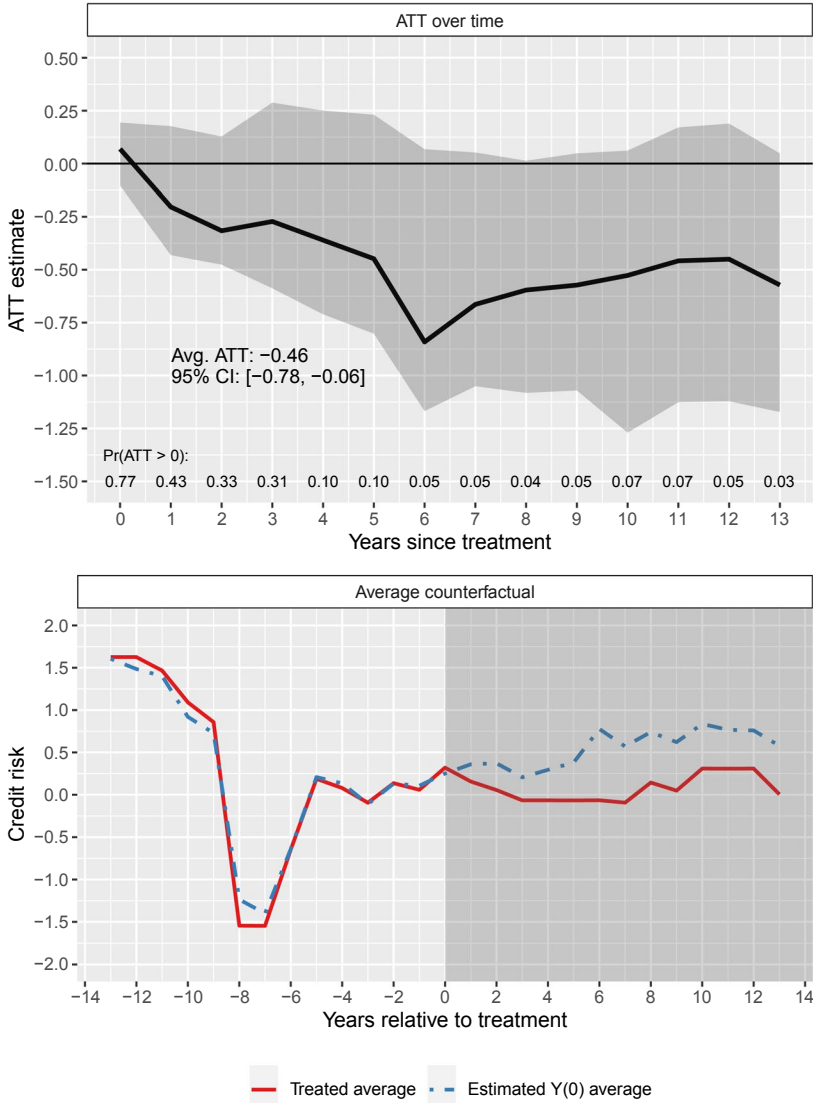
The bottom panel of Figure 2 plots time relative to treatment on the *x*-axis against the outcome variable (credit risk) on the *y*-axis. The lines denote the average outcome for the treated states (solid red) and the average counterfactual outcome as constructed by the GSC model (dot-dash blue). Note that in the pretreatment time periods (unshaded portion) the counterfactual average is quite close to the treated average, suggesting that the model provides a valid counterfactual comparison.[24] This similarity provides favorable evidence for identification of the effect of legislative exemption.

Finally, we consider the treatment effects within each treated state. Figure 3 graphs the average outcome and the counterfactual estimated by our GSC model for each of the states that instituted legislative exemptions during 1995–2010. All three show close counterfactual fits in the pretreatment time periods, which again bolsters our confidence in the model. Additionally, each state shows the negative treatment effect that appears in Figure 2. Taken together, these graphs confirm the findings from our panel models in Table 1; we conclude that removing transparency from state legislatures exerts a moderate, but detectable, decrease in states' credit risk.

*A Closer Look at the Treated States*

As we note above, three states—Pennsylvania, Massachusetts, and Ohio—exempted themselves from sunshine requirements during the time frame of our data sample. Our results shows that exemptions generally reduce credit risk, but are those results consistent with the experiences of these three states? To answer this question, we briefly discuss the fiscal and credit circumstances surrounding these states at the time of the exemptions. We would *not* expect credit rating agencies to explicitly applaud these exemptions. Agencies are highly regulated and often draw the ire of Congress (Younglai and Lynch 2011). Therefore, if they admitted to preferences for specific actions, they might risk facing even more scrutiny from the federal government.

Despite the incentives of credit rating agencies to remain quiet about the benefits of sunshine exemptions, we find evidence that the credit rating agencies responded positively to these three states' exemptions. Pennsylvania's legislative exemption corresponded directly with a credit rating upgrade from S&P in 1998.

FIGURE 3
Treated and Counterfactual Comparisons for States Exempted in
1995–2010
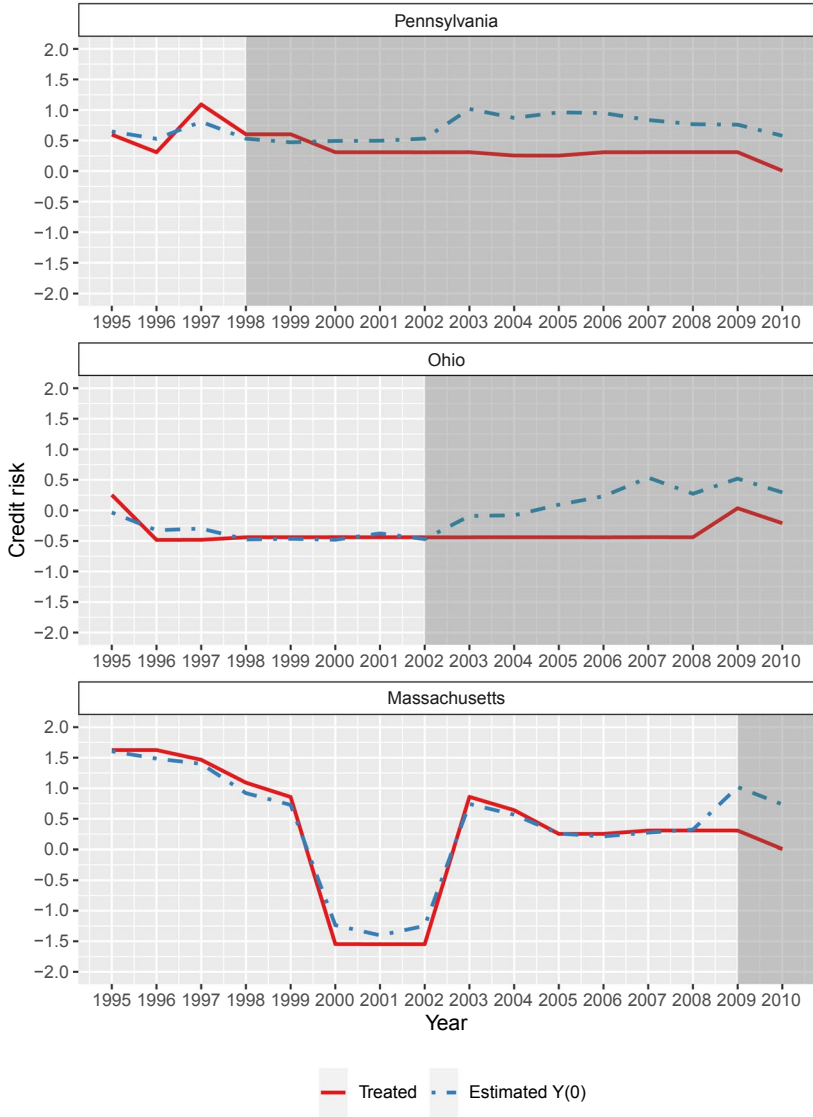
This was Pennsylvania's first upgrade in 14 years, despite S&P's concern that Pennsylvania's growth significantly lagged behind the rest of the country (Demenchuk 1998; Business Editors 1998). Even with the slow growth, S&P upgraded its credit outlook on Pennsylvania in late 1998 to AA because of the state legislature's spending restraint (Business Editors 1998). This restraint contrasted with Pennsylvania's previous fiscal strategy, which was described as "during the good times we've spent every penny we've had, and then when a downturn hit we've needed to raise taxes at the worst time" (Demenchuk 1998, 40). As a result of its newfound fiscal constraint, the credit rating agencies believed that Pennsylvania would be "insulate[d] it from the next economic downturn" (Demenchuk 1998, 40). This expectation proved to be correct during the recession following 9/11 and during the Great Recession, in which Pennsylvania avoided credit downgrades.

Unlike Pennsylvania, Massachusetts and Ohio waited until economic downturns to exempt their legislatures from sunshine requirements. Ohio instituted its sunshine exemption in 2002, in the midst of the post-9/11 recession. The rating agency Fitch notes that this downturn, along with the more recent Great Recession, "disproportionately affected" Ohio because employment growth did not follow as it did in the rest of the country (Wireless Editors 2009). For example, from 2004 to 2007, employment grew only 0.3% in Ohio versus 4.7% in the United States as a whole. The years 2009 and 2010 also produced weak economic recovery in Ohio (Wireless Editors 2009). Despite slow recovery, credit rating agencies praised Ohio's fiscal management during the recessions, which required the state to implement both unpopular spending cuts for Medicaid and temporary tax increases (Egan 2003). While there are no explicit statements from credit rating agencies nor legislators (to our knowledge) that support the notion that exemptions from sunshine helped politicians reach these difficult agreements, it is important to note that Ohio survived both recessions with no credit downgrades, despite slow recoveries.

Massachusetts' legislature also exempted itself from its sunshine laws during a recession, this time during the Great Recession in 2009. At that time, the state budget was in crisis. Governor Deval Patrick described the situation as follows:

The historic downturn is hitting state government especially hard, leaving us with tough choices among miserable options. We must take these painful but necessary steps today if we are going to position Massachusetts for recovery and long-term economic success. (Kaske and Phillips 2009, 9)

The legislature implemented the necessary difficult policy options, particularly reducing popular spending programs under the cover of a closed deliberation process. Massachusetts not only avoided a credit downgrade during the Great Recession, it ultimately received an upgrade shortly thereafter in 2011. Fitch cites the 2009 decision to reform the Commonwealth Transportation Fund as the reason for its upgrade (Fitch 2011). Before the reform, Fitch criticized the Fund for failing "to act decisively and reliably to protect the interests of bondholders" (Business Editors 2009). The reform, introduced by the state senate, allowed the Fund to generate the necessary revenue to cover its debt (Fitch 2011).

As with Ohio, there are no statements (to our knowledge) from credit rating agencies or politicians that explicitly connect closed-door proceedings with credit risk, but we note that Massachusetts mitigated the effects of the Great Recession after its exemption. Recently, the state has reconsidered the role of transparency, prompting several legislators to defend the exemption because "lawmakers have said the ability to confer in private, set priorities and have frank exchanges of ideas out of public view is vital to the smooth operation of government" (Salsberg 2011, 2). Representative Russell Holmes of Boston admits that the process lacks transparency and that "most decisions … are being made behind backrooms," but that "this is what folks find efficient" (Lannan and Young 2019). House Speaker Robert DeLeo defended the status quo exemption and urged voters to "focus on the end result rather than the process used to get there" (Lannan and Young 2019). These statements are consistent with our findings that exemptions from sunshine requirements allow politicians to enact better fiscal policies for their states, reducing credit risk. While we do not find direct statements from credit rating agencies directly supporting sunshine exemptions (which would be unlikely), the rating actions of these agencies for these three states are consistent with our overall results.

Our empirical findings imply a tension between rating agencies' perceived effects of transparency and the actual effects on credit risk. One possible explanation is that agencies claim to value transparency to induce states to reveal information about their fiscal proceedings, but they do not actually place value on the institution in practice. Alternatively, the type of transparency offered by sunshine laws may differ from what agencies have in mind in making the claim. Transparency in fiscal policy specifically includes

institutional components such as binding revenue forecasts, non-partisan staff writing appropriations bills, accounting standards, and an annual budget cycle (e.g., Alt, Lassen, and Rose 2006). Transparency from sunshine laws, such as open meetings and proceedings, gives the public a window to the deliberative process where bargaining occurs, more generally.[25] To our knowledge, the ratings agencies have never publicly stated which form of transparency they value most. Nonetheless, we maintain that our focus on general transparency here is relevant and useful for understanding how an accountability institution impacts a key component of states' fiscal health.

## Conclusions

Sovereign credit markets play a key role in state fiscal health in the United States. Changes in credit ratings alter both the sale of state debt and the interest associated with that debt. In recent years, Illinois has seen its credit rating plummet to nearly junk bond status, while California has climbed out of deep fiscal difficulty and seen S&P raise its credit rating four times since the fiscal crisis of 2007–2008. These changes can cost (or benefit) states millions of dollars in interest. Indeed, Connecticut's low credit rating in 2015 and the demand for higher yields from investors on their 10-year bonds meant that for every extra $1 billion borrowed, the state paid an extra $4.7 million compared to states with perfect credit ratings (Chappata 2015). By the end of 2015, Connecticut carried $22.5 billion in long-term bond debt—a considerable fiscal burden for the citizens of the state (Lembo 2015).

Indeed, we report a drop of about one-third of a standard deviation in credit risk as a function of legislative exemptions to sunshine laws. A simple regression modeling S&P ordinal credit scores as a function of Fortunato and Turner's (2018) latent credit risk variable indicates that a decrease in credit risk of one-third of a standard deviation in the latent variable is associated with credit score increase of about 0.398. If we examine this increase in credit scores in the context of Johnson and Kriz's (2005) analysis of the relationship between credit ratings and borrowing costs, a "back of the envelope" calculation suggests that true borrowing costs to voters would decrease by 2.4% in the wake of legislatures enacting sunshine exemptions. These calculations indicate that legislative exemptions to sunshine laws do come with real changes to borrowing costs for a state's citizens.[26]

Thus, the importance of understanding the processes underlying evaluations of state credit is critical to determining how states can overcome fiscal difficulties. While credit rating agencies purposely obfuscate much of the methodology behind their assessments, our research reveals the surprising role of transparency in ratings. Pairing data on state credit ratings from the three major credit rating agencies with novel data on legislative exemptions from state sunshine laws, we find strong evidence that shielding the legislative process from the public decreases credit risk, while opening it is associated with more risk. Although credit rating agencies claim that transparency is a key factor in their assessments of states' creditworthiness, our analysis indicates that in reality open governance is problematic. Overall, we conclude that legislators operating behind closed doors can be more productive and efficient, which improves the institution's credibility in credit markets.

This conclusion presents proponents of transparency as a mechanism for good governance with an important normative dilemma. Representative governance plays two key roles; one of representing citizens' interests and one of managing public resources and solving problems (i.e., the creation of public policy). Our research suggests that these two key features may be at odds with one another. The transparency necessary for citizens to hold legislators accountable, and thus, encourage representation also harms legislators' ability to manage public resources effectively. Transparency provides an easy mechanism to monitor politicians, but at the same time it makes fiscal stewardship more challenging. On the other hand, a full lack of transparency will almost certainly result in poor policy representation in governments and runs counter to the normative ideals of accountable governance. Of course, state credit is only one element of fiscal responsibility and public policy more generally. Thus, more study is warranted to explore these competing dynamics across public policy areas in order to fully appreciate the role of transparency in the creation or inhibition of responsible, representative government, and its role in tangibly improving citizens' lives.

*Jeffrey J. Harden* <jeff.harden@nd.edu> *is Associate Professor of Political Science, University of Notre Dame, 2055 Jenkins Nanovic Halls, Notre Dame, IN 46556. Justin H. Kirkland* <jhk9y@virginia.edu> *is Associate Professor of Politics, University of Virginia, S162 Gibson Hall, Charlottesville, VA 22904. Patrick E. Shea* <pshea@uh.edu>

*is Associate Professor of Political Science, University of Houston, 447 Philip Guthrie Hoffman Hall, Houston, TX 77204.*

## NOTES

1. This statement is particularly significant because rating agencies are (perhaps ironically) generally opaque about their own methodology due to its proprietary nature; thus, any indication of how they evaluate governments is an important consideration.

2. Alt, Lassen, and Rose (2006) suggest that evolutions in fiscal transparency institutions can be linked to a variety of political and fiscal outcomes. While their study focuses on fiscal transparency rather than general transparency in the legislature, it suggests that the implementation of transparency institutions is often a result of efforts to tie future politicians' hands when prior politicians have had poor performances.

3. We note here that our definition of transparency may differ from that of others, including credit rating agencies (which do not explicitly define transparency). Our focus on sunshine laws and exemptions is more general than other treatments of transparency in the literature. For example, Alt, Lassen, and Rose (2006) focus on the formalized budget process, while sunshine laws are focused on the predecision deliberative process as a whole.

4. Beyond their general disinterest in politics, there is ample evidence that citizens are particularly problematic principals for elected officials when it comes to fiscal policy. Social science has shown at least since Citrin (1979) that citizens often advocate for more government spending without favoring increases in revenue, a preference scholars have come to refer to as a "something for nothing" mentality (Sears and Citrin 1982; Welch 1985; Edlund and Johansson Sevä 2013; Winter and Mouritzen 2001). Even if citizens' fiscal preferences do not fully advocate something for nothing, there is a large body of evidence in economics that citizens underprice the costs of government services, and thus, do not support adequate revenue for government. See Turnbull (1998), Logan (1986), Baekgaard, Serritzlew, and Blom-Hansen (2016), Abbott and Jones (2016), and Pommerehne and Schneider (1978) for examples. Harden (2016) also points out that in general citizens have no constraints on their preferences for government action. Extending this logic suggests that it is entirely reasonable for citizens to support increased government spending and lowering government revenue. Of course, policymakers are then left to deal with the consequences of those incompatible demands.

5. Indeed, if transparent institutions are intended to increase accountability in elections, Fortunato and Loftis (2018) would suggest that increased

accountability, and thus decreased duration of government control, would result in less fiscal discipline for state governments.

   6. Fortunato and Turner limit their sample to these states because only general obligation bonds are backed by the full faith and credit of the state and are repaid from tax revenue. Thus, these bonds provide "a unique opportunity to study market responses to policy, or more precisely, policy change" (2018, 626).

   7. We are focused here on legislatures in their policymaking capacity. Many partisan caucus meetings are exempt from open meetings rules if they are conducting party business (electing leaders, etc.), but they are not exempt if they are discussing roll-call votes or committee business. In the context of partisan caucuses, we only code a legislature as exempt if the caucus is exempt when discussing legislative business, like voting. The legislature is not coded as exempt if the caucus meetings can be closed for internal party business.

   8. We should note that no states *increased* transparency during our study time frame. States largely enacted broad-reaching government transparency rules long before credit agencies began consistently rating state governments. Consequently, over the course of the study time frame, the common consideration for policymakers has been whether those initial transparency initiatives went too far in opening the deliberative process to outside observers.

   9. See the online supporting information for a complete set of results from this coding scheme.

   10. It is tempting to consider using several treatment variables (one for each legislative institution that could be exempt from sunshine laws), but with a limited number of dynamic degrees freedom, we cannot plausibly disentangle the treatment effect of each exempt institution. Additionally, state governments rarely enact exemptions one institution at at time, meaning the roll out of institutional exemptions usually covers entire groups of institutions like committees, caucuses, and parties. Sorting out which of these potentially exempt bodies "really matters" for credit risk is beyond the scope of our data. Thus, we stick to a binary coding scheme for any exemption.

   11. As is noted above, this empirical pattern reflects the fact that most states instituted wide-reaching sunshine laws much earlier than 1995, then exempted their legislatures later. Including cases in which a state's legislature was unexposed, then eventually exposed to a sunshine law, would require going back to the 1970s and 1980s, prior to the advent of consistent annual state credit ratings from the major ratings agencies.

   12. To be sure, our temporal variation in treatment is somewhat limited; an ideal setup would include more than three states experiencing treatment and more pre- and posttreatment data for each treated state. Thus, we encourage appropriate caution in interpretation of our results.

   13. Of course, coding treatment as exposure to sunshine would be mathematically equivalent. However, pretreatment data bolster the credibility of our design overall and are necessary to estimate our model with the generalized synthetic control method (see below). Thus, we rely on exemption as treatment given the time period coverage of the credit risk data.

14. Covariates that vary over time are Squire's (2007) index of legislative professsionalism, indicators for legislative term limits in effect and divided government, the frequency of partisan turnover in recent elections, the unemployment rate, per capita income, the average tax burden, per capita spending, per capita revenue, and per capita debt. Time-invariant covariates are indicators for lax budgetary guidelines according to the Advisory Council on Intergovernmental Relations (ACIR) and the presence of revenue limits, spending limits, and debt restriction. See Fortunato and Turner (2018) for complete details.

15. States select into treatment in this case because legislatures exempt themselves from sunshine laws. Thus, to the extent that our design identifies a causal quantity, that quantity is an ATT.

16. We employ two-tailed tests of statistical significance throughout our analyses due to the competing hypotheses we posit in our theoretical framework.

17. Fortunato and Turner (2018) also account for measurement error in the latent credit risk measure. They first simulate 1,000 draws for each state-year from the posterior distribution of the measure, resulting in 1,000 realizations of the outcome variable. They estimate their model on each of these 1,000 outcome variables and draw 100 estimates from the posterior of the model's coefficient estimates each time. This procedure yields a total of 100,000 (1,000 × 100) estimates for each coefficient included in the model. Finally, they summarize these distributions of estimates to report model results (for complete details, see Fortunato and Turner 2018, 632). As we show in the online supporting information, repeating this procedure for the models discussed here yields substantively identical results to what we report in Table 1, in which we simply regress the posterior mean of the credit risk variable on our treatment variable and the covariates.

18. The standard errors in models (4) and (5) are clustered by state.

19. Specifically, this sample includes the three states that became exempt in the time frame of our data and the 23 states that were not exempt during that time (see Figure 1). The states that drop out are: Alaska, Delaware, Hawaii, Mississippi, New Hampshire, New Mexico, New York, North Carolina, Oklahoma, Oregon, Rhode Island, Utah, Vermont, and Wisconsin.

20. A Lagrange multiplier test indicates that there is significant unexplained cross-sectional and temporal variation in the pooled model (model (1)), lending credibility to the models with state and year fixed effects.

21. These results are robust to several alternative specifications, including controls for short-term state fiscal conditions. See the online supporting information for details on these additional results.

22. The typical implementation of GSC employs an interactive fixed effects (IFE) model to estimate counterfactual outcomes for treated units. One disadvantage to this approach is that it requires more pretreatment data than a standard difference-in-differences estimator (Xu 2017, 59). In our case, we only have a few years of pretreatment data for some states. Thus, we estimate GSC with Athey et al.'s (2017) matrix completion method, which may be preferred in such a case (Xu and Liu 2019). Results are very similar with the IFE estimator. However, one of our states (Pennsylvania) is not usable in that case due to a lack of pretreatment data, which increases estimates of uncertainty.

23. The results in Figure 2 come from the same reduced sample of 26 states used to estimate model (5) in Table 1: the three states that became exempt during 1995–2010 and the 23 states that were not exempt during that time (see Figure 1). See the online supporting information for further diagnostics of our GSC model.

24. The difference between the two lines in the shaded portion of the graph is the ATT over time plotted in the top panel of Figure 2.

25. We thank an anonymous reviewer for bringing this point to our attention.

26. We should note that there is considerable uncertainty around these calculations. We are using three different estimates to develop this estimate: (1) our estimate of the treatment effect, (2) our estimate of the relationship between the treatment effect and observed credit ratings, and (3) the Johnson and Kriz (2005) estimate of the relationship between credit ratings and borrowing costs. We hope to emphasize the importance of our results here, but we caution readers about taking these calculations as definitive.

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## Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's web site:

**Figure S1**: Pretreatment Trends for States Exempted in 1995–2010
**Figure S2**: Treated and Control State Comparisons Over Time
**Table S1**: State Sunshine Laws and Legislative Exemptions
**Table S2**: Alternative Model Specifications
**Table S3**: Treatment Effects Before and After Accounting for Measurement Error in the Outcome
The supplemental information provide additional robustness checks, model specifications, and descriptive details of our data and results.