Review of Finance, 2022, 937–969
https://doi.org/10.1093/rof/rfac039
Advance Access Publication Date: 9 June 2022

OXFORD

Do Credit Rating Agencies Influence Elections?*

Igor Cunha[1], Miguel A. Ferreira[2], and Rui C. Silva[3]

[1]University of Kentucky, USA, [2]Nova School of Business and Economics, ECGI, CEPR, Portugal, and [3]Nova School of Business and Economics and CEPR, Portugal

Abstract

We show that credit rating agencies can influence political elections. We find that incumbent political parties experience an increase in their vote shares following municipal bond upgrades. The evidence is consistent with rating agencies affecting elections indirectly by expanding local governments' debt capacity and directly through an impact on voters' perceptions of the quality of incumbent politicians. To identify these effects, we examine election outcomes within neighboring counties by exploiting exogenous variation in municipal bond ratings due to Moody's recalibration of its scale in 2010.

Keywords: Elections, Credit ratings, Financial constraints, Municipal bonds, Government spending, Economic conditions

JEL classification: D72, G24, H74

Received June 23, 2019; accepted May 12, 2021 by Editor Geoffrey Tate.

* We thank Manuel Adelino, Laurent Bach, Bo Becker, Taylor Begley, Kizkitza Biguri, Philip Bond, Sergey Chernenko, João Cocco, Alex Edmans, Fernando Ferreira, Mariassunta Giannetti, Francisco Gomes, Christopher Hennessy, Christopher Hrdlicka, Stephen Karolyi, Stefan Lewellen, Felipe Restrepo, Francesco Sangiorgi, Breno Schmidt, Amit Seru, Henri Servaes, Elena Simintzi, Jason Sturgess, David Thesmar, Vikrant Vig, and Luigi Zingales; participants at the European Finance Association Annual Meeting, University of Washington Summer Finance Conference, Carnegie Mellon Conference on the Economics of Credit Rating Agencies, Annual Conference on Taxation, Annual HEC Paris Workshop, Copenhagen Business School Conference on Financial Frictions, Portuguese Economic Journal Annual Meeting, and Portuguese Finance Network Conference; and seminar participants at Koc University, London Business School, Stockholm School of Economics, Universidade de Sao Paulo, University of Delaware, University of Kentucky, and University of Melbourne for helpful comments. Financial support from the European Research Council (ERC) and the Fundação para a Ciência e Tecnologia (FCT) is gratefully acknowledged.

© The Author(s) 2022. Published by Oxford University Press on behalf of the European Finance Association.
All rights reserved. For permissions, please email: journals.permissions@oup.com

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

1. Introduction

The long-standing debate about the role of financial markets in society has recently received additional attention due to the 2007–09 global financial crisis and the 2010–12 European sovereign debt crisis. The fear that financial institutions may have too much power has been expressed not only by regulators and academics (Zingales, 2015) but also in public opinion polls (Gallup, 2011). Politicians have voiced similar concerns. In 2012, Leonardo Domenici, a Member of the European Parliament, claimed that: "The debt crisis in the Eurozone has shown that credit rating agencies (CRAs) have gained too much influence, to the point of being able to influence the political agenda."

We ask whether credit ratings influence the electoral prospects of incumbent political parties. We examine this question by studying the effects of municipal bond ratings on gubernatorial and mayoral elections in the USA. Credit rating upgrades can have meaningful impacts on voting through improved economic conditions. Incumbent politicians can take advantage of the reduced financing costs generated by the upgrades to increase government spending and decrease taxes. Ratings can also directly affect voting through voters' perceptions of the quality of incumbent politicians. For example, ratings can serve as a certification mechanism of the incumbent's fiscal responsibility.

Our identification strategy exploits the exogenous variation in municipal bond ratings that occurred when Moody's recalibrated its municipal rating scale in 2010. Before the recalibration, Moody's used a dual-class rating system. Moody's Municipal Rating Scale measured distance to distress for municipal bonds (i.e., the likelihood that a municipality reaches a financial position that would require extraordinary support from a higher level of government to avoid default). In contrast, Moody's Global Rating Scale measured expected losses (i.e., default probability and loss given default) among sovereign and corporate bonds. This dual-class rating system persisted for decades. In April–May 2010, Moody's recalibrated its Municipal Rating Scale to align it with the Global Rating Scale. The recalibration resulted in upgrades by up to three notches of nearly 18,000 local governments, corresponding to bonds worth more than $2.2 trillion in par value (about 70,000 bond issues). According to Moody's Investors Services (2010), the recalibration unifies all bond ratings on a single scale and "does not reflect an improvement in credit quality or a change in our opinion [about the issuer]." Thus, rating upgrades due to the recalibration are uncorrelated with changes in local governments' intrinsic credit quality, the incumbent's past actions, and local and nationwide economic conditions.[1]

The variation in ratings due to the recalibration provides us a unique opportunity to examine the impact of ratings on election outcomes. It allows us to isolate effects due exclusively to municipal bond ratings from other confounding effects. Municipalities that were not affected by the recalibration can serve as a control group.[2] Our empirical setting also exploits the geographic distribution of the recalibration for identification purposes. While

1 We further validate the exclusion restriction by showing no differential variation in S&P ratings (for issuers rated by both Moody's and S&P) and house prices between treated and non-treated counties.

2 We employ several different control groups in our tests. The baseline control group includes both municipalities that were already adequately calibrated vis-à-vis the Global Rating Scale and municipalities with no bonds outstanding or Moody's rating. We confirm that our main findings also hold when we restrict the sample to counties with outstanding bonds and counties with bonds rated by Moody's.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

two counties that are geographically distant from each other may experience different (and potentially unobservable) economic conditions around the recalibration, this is less likely among neighboring counties (i.e., the set of control counties that border with a treated county). By comparing upgraded counties with non-upgraded *neighboring* counties, our approach alleviates concerns that our results are driven by differences in local economic conditions, culture, or other unobservable characteristics.

We employ a difference-in-differences approach to compare the election outcomes between upgraded local government units (the treatment group) and non-upgraded local government units (the control group) around the recalibration in 2010. Specifically, we study how this shock to municipal bond ratings affects the incumbent political party's vote shares in the 2010–12 elections compared with the 2006–09 elections at the county level. The recalibration affected bonds issued by states, counties, cities, and local government units, such as townships, school districts, and special districts (e.g., public utility districts). For this reason, we aggregate the changes in ratings to the county level. Our (continuous) treatment variable is the fraction of local government units in each county whose outstanding bonds were upgraded because of the Moody's recalibration (*Recalibrated*).

We find that incumbent party candidates obtained a higher vote share in upgraded counties than in non-upgraded counties. Our results for gubernatorial elections show that a 10% increase (approximately one standard deviation) in the fraction of upgraded local government units in a county is associated with a 0.7 percentage point increase in the incumbent's vote share relative to non-upgraded neighboring counties.

We study what factors increase the voting share of the incumbent party. First, we explore whether the recalibration energizes the incumbent party's base and increase their likelihood of voting. Our estimates imply that a 10% increase in the fraction of local governments upgraded in a county increases the number of votes cast in the county by 0.6 percentage points. Second, we explore the possibility that upgrades resulted in changes in the composition of candidates running for office. A powerful incumbent candidate can discourage candidates from running against him/her. Our estimates imply that a 10% increase in the fraction of local governments upgraded in a county decreases the probability that the race will have more than two candidates by 1.1 percentage points.

Credit rating upgrades can affect voting indirectly through improvement in economic conditions resulting from the relaxation of local governments' financial constraints.[3] Municipal bond markets are an important resource for local governments to finance the construction and maintenance of infrastructure and other public projects. The recalibration generated cross-sectional variation in ratings across municipalities, affecting municipalities' financial constraints and debt capacity. Cheaper access to financing can, in turn, have significant effects on local economic conditions. Cornaggia, Cornaggia, and Israelsen (2018) show that municipalities upgraded due to the recalibration experienced a significant reduction in borrowing costs in the municipal bond market after Moody's recalibration. Adelino, Cunha, and Ferreira (2017) show that reduced borrowing costs allowed municipalities to

3 A reason why exogenous rating changes can influence public policy is rating-based regulations (Kisgen and Strahan, 2010). Investment management policies and practices also often depend on ratings in that they restrict the portfolio holdings of institutional investors (Chen *et al.*, 2014). Besides, investors are more likely to rely on credit ratings for information about credit risk in the municipal bond market because this market is relatively opaque (Cornaggia, Cornaggia, and Israelsen, 2018).

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

increase bond issuance and spending (or reduce taxes) and that these fiscal policy changes had positive spillovers to the private sector in terms of employment and income.

We confirm in our setting that local governments upgraded during the recalibration subsequently increase their bond issuance amounts. We use a two-step procedure to estimate the upper bound of municipal bond issuances' impact on incumbent voting share. We find that the incumbent party's voting share increases by 1.45 percentage points when the amount issued in the county increases by 10%. These estimates provide an upper bound for an expansionary fiscal policy's impact on incumbent party voting share because the recalibration may affect voting not only through an increase in external financing.

CRAs can also directly affect political outcomes if ratings affect voters' perceptions of the quality of incumbent politicians. For example, they can serve as a certification mechanism of the incumbent's fiscal responsibility. We provide several examples of incumbent politicians using rating upgrades to certify the economy's strength and the quality of their fiscal policies.[4] These examples reveal that while incumbent candidates know that they are not responsible for the recalibration upgrades, they still try to use it as a selling point with voters and spin the event as a positive signal about their government.

We start our analysis of the direct effects of ratings on elections, providing evidence that improvements in economic conditions do not entirely explain the impact of ratings on elections. Our baseline tests offer evidence consistent with channels other than local governments' financial constraints affecting elections. We compare election outcomes across neighboring counties, which are typically subject to similar government resource constraints and local economic conditions due to proximity. Our results remain statistically and economically significant when we include economic controls, which proxy for local governments' fiscal policy, commonly used to explain election outcomes (e.g., local unemployment rate, income, and tax rates).

We also study the timing of the effects of ratings on election outcomes. While the direct effect of ratings through voters' perceptions should have an immediate impact on voting outcomes, improvements in local economic conditions due to fiscal policy changes take time to materialize and affect elections with a lag. Consistent with a direct effect of ratings, we find a positive and significant impact of ratings on incumbent party vote shares in the same year as the recalibration. This timing could also be consistent with CRAs directly affecting voters' wealth due to an increase in the value of the municipal bond portfolio of voters following the recalibration. While expectations about the future expansionary fiscal policy may affect voting immediately in 2010, if voters did not believe that the incumbent was responsible for the upgrade, there is no reason for those expectations to attach specifically to the incumbent party. To change their behavior, voters must also update their opinion about the incumbent's management abilities.[5] We also find an effect in 2012 (2 years after the recalibration), which we interpret as evidence of an indirect impact of ratings on voting outcomes through improvements in local economic conditions.

Next, we test whether the influence of credit ratings on election outcomes is stronger in cases where voters are more attentive to ratings. We explore variation in Google news

4 One notable example is President Donald Trump and Mike Pence drawing attention to the rating of the state of Indiana (which had the maximum attainable rating of Aaa) during the 2016 presidential race.

5 We find evidence that the economic effects of the recalibration are similar in states where the incumbent party remains in power and where they are ousted.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

searches for the term "credit rating" around election dates. An increase in news searches for this term would suggest that more people in a state were trying to be informed about credit ratings and would be more likely to use this information in their voting decision. We find that the recalibration had a larger effect on elections where voters were more informed about the rating upgrades as proxied by the number of news searches for "credit ratings."[6]

We also exploit geographic variation in the availability of local newspapers at the time of the recalibration. Local newspapers' availability increases the probability that local reporters will cover the upgrades in the news. Consistent with a direct effect of ratings on voters' perceptions, we find that the impact of rating upgrades on incumbent parties' vote share is larger in states with more local newspapers.

Voters may also learn about the changes in municipal bond ratings by directly following the municipal bond markets. We exploit the fact that municipal bonds enjoy a "tax privilege" because they are exempt from state-level taxation in some states. We use the measure of the municipal bond privilege of Babina *et al.* (2021). The tax privilege variable is the difference between the highest state income tax rate applied to income from municipal bonds issued by other states and the highest state income tax rate applied to income from own state municipal bonds. Babina *et al.* (2021) show that municipal bond funds in high privilege states hold more in-state municipal bonds. Therefore, their measure offers a proxy for the in-state holding of municipal bonds. Voters with municipal bonds are more likely to be aware of the rating upgrades because they are more likely to be exposed to the news and experienced an immediate increase in wealth due to the appreciation of their municipal bond holdings. We find that the effect of rating upgrades on incumbents' electoral prospects is stronger in states with higher municipal bond privilege. Overall, the cross-sectional results provide further evidence that changes in local governments' access to credit ratings do not entirely drive the impact of ratings on elections. Our effects are stronger where voters are more likely to be aware of the upgrades. The variation in the results is consistent with ratings directly affecting electoral voting by impacting perceptions of the incumbent's quality.

Finally, we investigate the effects of the recalibration on gubernatorial and mayoral elections using bonds directly issued by states and cities, respectively. Because the incumbent's office directly issues these bonds, they can offer voters a clearer signal of the incumbent's quality. The effects of ratings on elections are stronger for upgrades of bonds directly connected to the governor's office. Incumbent party governors that experienced upgrades in bonds issued by their state obtained a 13 percentage points increase in their vote share relative to incumbent party governors in neighboring counties across the state border who did not experience an upgrade. In the case of mayoral elections, incumbent party candidates of upgraded cities obtained a 9 percentage points increase in their vote share relative to incumbent party candidates of non-upgraded cities in the same state and year. The fact that the economic magnitudes are larger for rating upgrades of bonds directly linked to the governor and mayor's offices is evidence of the direct effects of ratings in elections. While bonds issued by states and cities do not have a disproportionate weight relative to the total

6 To address the possibility that news searches for "credit rating" may proxy for voters' concerns about local economic conditions. We also conduct "placebo" tests using the terms "financial crisis" and "credit score." We do not find evidence that the effects of ratings on elections are stronger in places with a higher volume of news searches for these alternative terms.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

amount issued by other local government units in terms of revenue source, upgrades of bonds issued by states and cities do have a disproportionate electoral effect.[7]

The paper contributes to two strands of the literature. We provide novel evidence of the effects of credit ratings on the real economy. There is vast evidence that ratings affect corporate actions (Kisgen, 2006; Kisgen and Strahan, 2010; Baghai, Servaes, and Tamayo, 2014; Almeida *et al.*, 2017) and relax financial constraints of local governments (Adelino, Cunha, and Ferreira, 2017; Cornaggia, Cornaggia, and Israelsen, 2018). To the best of our knowledge, we are the first to provide evidence that CRAs' actions can affect political outcomes. Our findings suggest that changes to credit ratings may have long-lasting political, social, and economic consequences through their impact on election results.[8]

We also contribute to the literature on the determinants of voting behavior. This literature provides extensive evidence that economic conditions influence voting (see Lewis-Beck and Stegmaier (2000) for a review of the literature). For example, Bagues and Esteve-Volart (2016) show that improvements in economic conditions driven by a cash windfall won in the Spanish Christmas Lottery have a positive effect on the incumbent's vote share, even though voters do not perceive the incumbent as of better quality. We contribute to this literature by showing that voters use financial market information when making their voting decisions. More specifically, we provide evidence of a direct effect of ratings on voting behavior through its impact on voter perceptions of incumbents' quality.

2. Methodology and Data

2.1 Recalibration

Moody's had a dual-class rating system until its rating recalibration in 2010. Moody's Municipal Rating Scale measured distance to distress (when a municipality might reach a financial position that required extraordinary support to avoid default). Moody's Global Rating Scale is designed to measure expected losses (default probability and loss given default) in sovereign bonds, corporate bonds, and structured finance products (Moody's Investors Services, 2007). Moody's Investors Services (2009) attributed its dual-class rating system to the preferences of the highly risk-averse investors in municipal bonds. According to the US Flow of Funds Accounts in 2010, households owned 50% of municipal bonds, followed by money market funds with 10%, and insurance companies with 9%. In contrast, households owned only 19% of corporate and foreign bonds.

Moody's idea of mapping municipal bond ratings into the Global Rating Scale dates back to at least 2002 (Moody's Investors Services, 2002) and is mentioned in various publications over the years. It finally announced a recalibration of the Municipal Rating Scale to align it with the Global Rating Scale in March of 2010 (Moody's Investors Services, 2010). Moody's recalibration algorithm used the expected losses of each municipal rating by sector (i.e., historical default rates by rating category and loss severity by sector) to map to its

7 For example, in 2009, there were 108,624 municipal bond issues, for a total amount of $344 billion. In that year, states made 3,370 issuances for a total of $88 billion, representing 25% of the total amount. Cities made 26,278 issuances for a total of $41 billion, which represents 12% of the total amount.

8 The distortions caused by CRAs in elections could be exacerbated when ratings are influenced by analysts' political affiliation (Kempf and Tsoutsoura, 2018), private interests, and career concerns (Cornaggia, Cornaggia, and Xia, 2016; Kempf, 2020).

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

equivalent rating on the global scale. In April and May of 2010, over 4 weeks, Moody's described how municipal bond ratings would be affected by the recalibration, resulting in a zero-to-three notch upgrade of ratings.

We obtain a list of recalibrated bond issues from Moody's. The recalibration covered 69,657 municipal bonds (with a total par amount of $2.2 trillion). Almost all the bonds had an investment-grade rating before the recalibration (only fifty-six municipal bonds had a speculative-grade rating).

An important aspect of this recalibration is that not all municipal bond issues were upgraded in the recalibration and can be used in the control group. Some local governments were already "properly calibrated" (Moody's Investors Services, 2010) in terms of the global scale. Housing, healthcare, and some other sectors, in particular, did not see a change in ratings. Municipal bonds with higher ratings (at or above Aa3) were also less likely to be recalibrated than those with lower ratings (below Aa3); municipal bonds with the maximum attainable rating (Aaa) could not be upgraded. Of course, local governments without Moody's ratings or with no outstanding bonds were not subject to recalibration and can also be used in the control group.

In our primary analysis of credit ratings' effect on gubernatorial elections, we explore upgrades from all local governments within a county. We identify all recalibrated local governments within a county (i.e., county, city, townships, school district, and special districts) and create a continuous treatment variable *Recalibrated* as the fraction of all local government units in a given county that were upgraded during the Moody's recalibration. Figure 1 shows a map of the USA with the terciles of the treatment variable (*Recalibrated*), among those counties with a non-zero value. There is variation in the intensity of the treatment and the location of counties in the treatment group. Our empirical setting exploits both types of variation.

We also investigate the effects of upgrades of bonds directly issued by the Governor's office. *State Upgrade* is an indicator variable that takes the value of 1 if the state's bonds were upgraded during the recalibration and zero otherwise. In the case of mayoral elections, *City Upgrade* is a dummy variable that takes the value of 1 if the city's bonds were upgraded during the recalibration and zero otherwise.

Moody's (2010) explained that the recalibration was intended to enhance the comparability of ratings across asset classes. It did not indicate any change in the issuer's credit quality: "Our benchmarking analysis of municipal credits against global scale rating across the Moody's rated universe will result in an upward shift for most state and local government long-term municipal ratings by up to three notches. The degree of movement will be less for some sectors . . ., which are largely already aligned with ratings on the global scale. Market participants should not view the recalibration of municipal ratings as rating upgrades, but rather as a recalibration of the ratings to a different scale. This recalibration does not reflect an improvement in credit quality or a change in our opinion."

To validate our exclusion restriction, we study new bond issues that have both Moody's and S&P ratings. Figure 2 shows the evolution of the difference in Moody's and S&P ratings between upgraded local governments (treated) and non-upgraded local governments (control) around the recalibration event. The figure shows no differential changes in Moody's ratings between upgraded local governments and non-upgraded local governments before the recalibration. In contrast, the treatment group presents a sharp increase in Moody's ratings after 2010, which persists for up to 3 years after the recalibration. We do not see any changes in the S&P ratings between upgraded and non-upgraded local

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

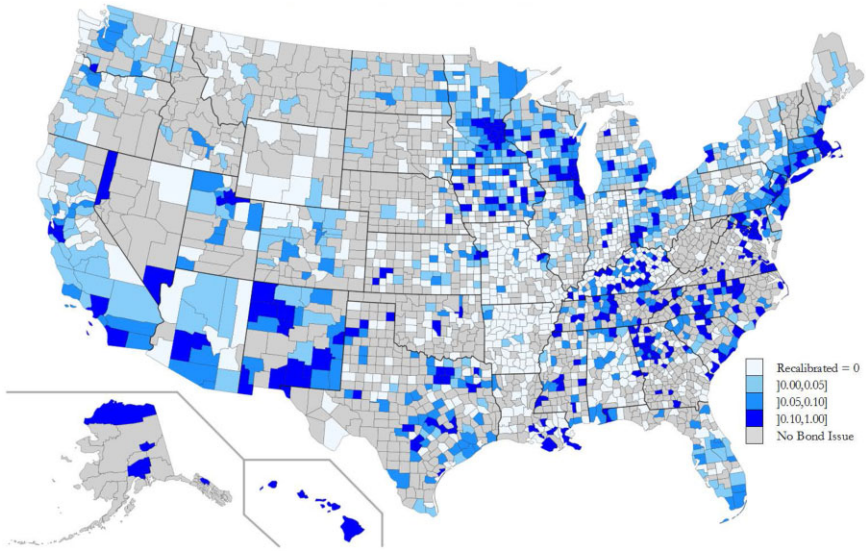

Figure 1. Recalibration by county The map shows the fraction of local government units in a given county upgraded during the recalibration event in April–May 2010 (Recalibrated). Counties in gray have no local government unit issuing bonds in the 3 years before the recalibration (1,365 counties). Counties in white have no upgraded local government unit (812 counties). Counties in light blue, medium blue, and dark blue are in the bottom tercile (322 counties), medium tercile (323 counties), and top tercile (322 counties) of the distribution of the Recalibrated variable (considering non-zero values), respectively.

governments either before or after the recalibration. If the recalibration-related upgrades reflected changes in underlying credit quality, the S&P ratings would also be affected. The figure shows that Moody's recalibration does not reflect a change in issuers' credit quality, which is an important validation of our identification strategy.

We also compare the evolution of house prices of treatment and control groups before and after the recalibration using the Federal Housing Finance Agency's (FHFA's) House Price Index (HPI) data at the Metropolitan Statistical Area (MSA) level.[9] Figure 3 shows no significant differential trends in HPI of treatment and control groups (at the county level) before or after the rating recalibration. If anything, the treatment group house prices declined more than those of the control group around the recalibration. Thus, there is no evidence that our results are driven by differential effects on treatment and control groups of the 2007–09 financial crisis and subsequent recovery.

2.2 Election Outcomes

We obtain voting data for gubernatorial elections at the county level for the 2004–12 period from David Leip's website.[10] These data have been used in previous research (Gentzkow, Shapiro, and Sinkinson, 2011). The data include information on the total

9 The HPI is a weighted repeat-sales index that measures the average price changes in repeat sales or refinancing on the same properties. Whenever the MSA HPI is missing information, we complement the data with state-level house price indices from the FHFA.

10 The data are available at http://uselectionatlas.org.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

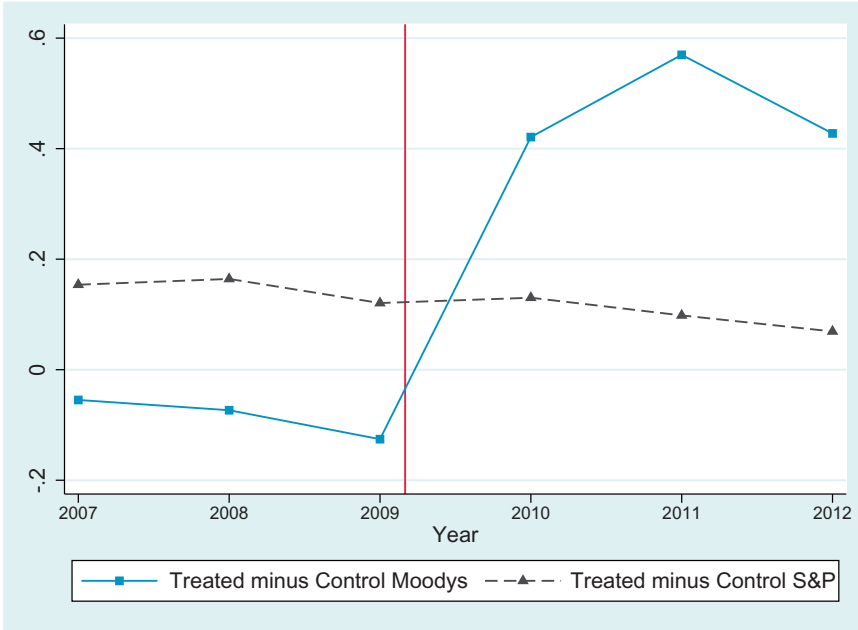

Figure 2. Difference in ratings around the recalibration. This figure shows the regression coefficients of Moody's and S&P ratings on event-year dummies around the recalibration event from April to May 2010. The figure presents the difference between upgraded local governments (treated) and non-upgraded local governments (control). The sample consists of counties where at least one local government-issued bonds in the municipal bond market during the 4 years before the recalibration (April 2006–March 2010).

numbers of votes separately for each political party. There are no readily accessible data on mayoral elections across different states. We collect mayoral election data for California for the 2006–12 period from the California Elections data archive.[11] We supplement the mayoral election data by manually collecting information from online sources. In particular, we obtain information on the total number of votes by each candidate of the mayoral races in the largest cities in each state from the "OurCampaigns" website.[12] Some states have data on the ten largest cities (e.g., Illinois, North Carolina, and Ohio), but other states only have data on one city (e.g., Arkansas, Idaho, and Montana). Our mayoral election data contain information on forty-two states with an average of five cities per state.

We start by identifying the incumbent party as the party that won the previous election for each election. We then create the incumbent party vote share (*Incumbent Party Share*), defined as the number of votes that the incumbent party received divided by the total number of votes in the county (for gubernatorial elections) or city (for mayoral elections). Governors and Mayors serve 4-year terms (except for Vermont and New Hampshire's governors, where terms are 2 years long). The pre-treatment period is 2006–09 and the post-treatment period is 2010–12.

11 The data are available at http://www.csus.edu/isr/reports/california_elections/.

12 The data are available at https://www.ourcampaigns.com/.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

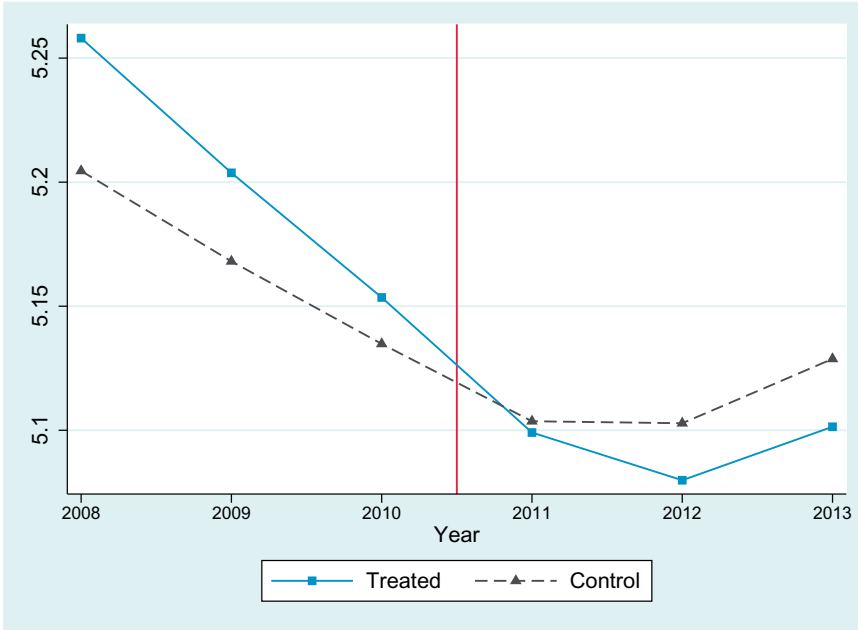

Figure 3. House prices around the recalibration. This figure shows the HPI for counties in the treatment and control groups around the recalibration event (April–May 2010). Recalibrated is the fraction of upgraded local government units in each county. The treatment group includes counties with at least one municipal bond upgraded during the recalibration (Recalibrated > 0) and the control group comprises counties with no municipal bond upgraded (Recalibrated $= 0$).

Table I presents summary statistics of treatment and control groups for election outcomes and the treatment variable in the pre-recalibration period by election type. In Panel A, the treatment group includes counties with at least one municipal bond upgraded during the recalibration (Recalibrated > 0) and the control group comprises all neighbors of treated counties with no municipal bond upgraded (Recalibrated $= 0$). Because a county may be a neighbor of multiple counties, some control counties may be repeated in this sample. In Panel B, the treatment group includes counties where the state's bonds were upgraded during the recalibration. In this sample, the control neighboring counties must be in a neighboring state where the state's bonds were not upgraded. Thus, this sample provides a comparison between neighboring counties separated by a state border. In the case of mayoral elections, in Panel C, we only have data for the largest cities in each state. The treatment group includes cities whose bonds were upgraded during the recalibration.

Columns (7) and (8) show the differences between the two groups in the pre-recalibration period. Counties in the treatment group are larger than counties in the control group regarding the voting population. We present both raw differences in means between treatment and control groups and differences after adjusting for size using the number of votes in the county and county neighborhood-by-year-fixed effects; a neighborhood includes a treated county and all its neighboring counties. In the case of mayoral elections, we adjust differences for size using the number of votes in the city- and state-by-year-fixed effects. These controls are also included in our regressions.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 O

Table I. Summary statistics of election outcomes

This table presents the pre-treatment mean, standard deviation, and the number of observations for each variable for treatment and control groups. Incumbent Party Share is the number of votes that the incumbent party received divided by the total number of votes in the county. Recalibrated is the fraction of upgraded local government units in each county. State Upgrade is a dummy variable that takes the value of 1 if the bonds issued by the state were upgraded, and zero otherwise. City Upgrade is a dummy variable that takes the value of 1 if the bonds issued by the city were upgraded and zero otherwise. In Panel A, the treatment group includes counties with at least one local government unit upgraded and the control group all their non-treated neighboring counties. In Panel B, the treatment group includes counties where the state's bonds were upgraded. The control group comprises neighboring counties across the state border whose bonds were not upgraded. In Panel C, the treatment group includes cities with upgraded bonds and the control group includes cities in the same state whose bonds were not upgraded. The pre-treatment period is from 2006 to 2009. Column (7) presents the raw differences between treatment and control groups. Column (8) presents the difference between treatment and control groups adjusted by neighborhood-by-year-fixed effects and number of votes (Panels A and B) or state-year-fixed effects and number of votes (Panel C). *P*-values clustered at the county level (for gubernatorial elections) and city level (for mayoral elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Treatment group			Control group			Difference	
	Mean	Standard deviation	Number of observations	Mean	Standard deviation	Number of observations	Raw difference (*P*-value)	Adjusted difference (*P*-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Gubernatorial elections (sample of neighboring counties)								
Incumbent Party Share	0.508	0.135	3,661	0.503	0.142	2,603	0.005	0.009**
							(0.443)	(0.044)
Recalibrated	0.104	0.103	3,661	0.000	0.000	2,603	0.104***	0.088***
							(0.000)	(0.000)
Number of Votes (hundreds of thousand)	0.759	1.18	3,661	0.131	0.214	2,603	0.628***	
							(0.000)	

(continued)

Table I. Continued

	Treatment group			Control group			Difference	
	Mean	Standard deviation	Number of observations	Mean	Standard deviation	Number of observations	Raw difference (*P*-value)	Adjusted difference (*P*-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel B: Gubernatorial elections (sample of neighboring counties across state border)								
Incumbent Party Share	0.55	0.17	146	0.55	0.135	578	0.000	−0.024
							(0.999)	(0.252)
State Upgrade	1.000	0.000	146	0.000	0.000	578		
Number of Votes (hundreds of thousand)	0.221	0.435	146	0.267	0.653	578	−0.047	
							(0.401)	
Panel C: Mayoral Elections								
Incumbent Party Share	0.630	0.173	98	0.681	0.226	125	−0.050*	−0.068*
							(0.083)	(0.088)
City Upgrade	1.000	0.000	98	0.000	0.000	125		
Number of Votes (hundreds of thousand)	0.348	0.473	98	0.125	0.191	125	0.223***	
							(0.000)	

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 O

2.3 Municipal Bond Markets

The municipal bond issues (primary market) data come from the Ipreo i-Deal new issues database. The sample period runs from April 2007 to March 2013, which corresponds to the 3 years before Moody's recalibration and the 3 years afterward. We restrict the sample to new bond issues rated by Moody's and local government units that issued bonds during the 3 years before the recalibration.[13] Because credit ratings on insured bonds reflect the credit quality of the *insurer* rather than the *issuer*, we include only uninsured bonds in our analysis (roughly 60% of the municipal bonds are uninsured).

We define the variable *Issue Amount* as the total amount of bonds issued by local governments in each county and year. We also define the variable "Offer Yield" as the average offer yields (in percentage) of new bond issues in each county and year.

2.4 Economic Outcomes

In our analysis of the effects of ratings on incumbent party share, we control for variables commonly used to explain election outcomes such as unemployment rate, income, local tax rate, and local government expenditures. We obtain annual data for unemployment from the Bureau of Labor Statistics. "Unemployment Rate" is the ratio of unemployed workers to the total labor force in each county and year.[14] We obtain annual county-level income data from the Internal Revenue Service (IRS) Statistics of Income. "Income" is total wages and salaries in a given county and calendar year (the sample period for income is 2006–12). We obtain data on government revenues and expenditures from the US Census Bureau's Annual Survey of State and Local Government Finances. The data include revenues and expenses of individual local government units within each county. The sample comprises local government units present in all years of the sample period and covers more than 90% of the counties in the USA. "Local Tax Rate" is the ratio of total taxes charged by all local government units to income in each county and year. "Local Government Expenditures" is the total expenditures of all local government units in each county and year.

We also use data on the labor market characteristics to compare treated and control counties. We obtain local government employment data from the Census Bureau's Government Employment and Payroll Survey. The Census Bureau conducts a complete census of local government employees every 5 years (e.g., 2002, 2007, and 2012) and uses a sample of local governments in the other years. "Local Government Employment" is the number of full-time equivalent employees of local government units in each county as of the week of March 12 of each year. The local government employment analysis is restricted to local government units present in all years between 2007 and 2013.[15]

13 We obtain numerically identical differential effects when we include all new issues or restrict the sample of new issues to local governments that issue bonds both before and after the recalibration, given that only local governments that issue bonds *both* before and after can be identified with the difference-in-differences estimator.

14 The number of counties included in each regression varies according to the availability of sector-level employment-by-county data in the CBP. The Census Bureau often omits observations or includes only broad ranges for confidentiality reasons.

15 The sample used in the analysis of government employment includes only counties with at least one government unit present in all years. The resulting sample of counties with government employment data consists of 1,618 counties.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

We obtain annual data on private employment by county from County Business Patterns (CBP) published by the Census Bureau. The data include employment in the week of March 12 of each year. "Private Employment" is the number of employees in each county and year.

Table II provides a comparison of economic outcomes between treatment and control groups in the year before the recalibration (2009). Consistent with Table I, counties in the treatment group are larger than counties in the control group in terms of "Local Government Expenditures," "Local Government Employment," "Private Employment," "Income," and "Number of Households." We present both raw differences in means between treatment and control groups for the year before the recalibration and differences after adjusting for size (using the "Number of Votes") and county neighborhood-by-year-fixed effects. After adjusting for size and regional heterogeneity, the differences in economic variables' levels are no longer positive and statistically significant. Besides, the growth rates of outcome variables in the pre-treatment period are similar across the two groups, even before the adjustments. We conclude that pre-existing differential trends between treatment and control groups are unlikely to explain our results.

3. Effect of Credit Ratings on Elections

To study the impact of credit ratings on election outcomes, we explore Moody's Municipal Rating Scale's recalibration as a source of exogenous variation in municipal bond ratings. We study the impact of the upgrades generated by the recalibration on the incumbent vote share in gubernatorial elections (at the county level) using the following regression model:

$$Y_{ihst} = \beta \text{Recalibrated}_{ihs} \times \text{Post}_t + \mathbf{X}'_{ihst}\boldsymbol{\Theta} + \alpha_i + \gamma_{ht} + \lambda_{st} + \varepsilon_{ihst}, \quad (1)$$

where Y_{ihst} is "Incumbent Party Share," the number of votes that the incumbent party received divided by the total number of votes cast in the respective county i, neighborhood h, state s, at time t. Our treatment variable is "Recalibrated," defined as the fraction of upgraded local governments in a county. "Post" is a dummy variable that takes the value of 1 after the recalibration in 2010, and 0 before the recalibration. $\mathbf{X}_{ihst}$ is a vector of time-varying control variables including the total number of votes cast in the county to control for constituency size and economic controls (Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures) in some specifications. The regressions include county-fixed effects (α_i) in all specifications. The interaction term Recalibrated $\times$ Post is the difference-in-differences estimate of the effect of bond ratings on election outcomes (i.e., the change in election outcomes in a region with upgraded municipal bonds when compared with a region with no upgraded municipal bonds).

We exploit the geographic distribution of the treatment. For each treated county (Recalibrated > 0), we identify the set of all neighboring counties and use them to provide a counterfactual evolution of "Incumbent Party Share" in the absence of the recalibration. This procedure may lead to a control county to be included in more than one neighborhood. We then run our regressions within state-year and the neighboring group and year by including state-by-year-fixed effects (λ_{st}) and neighborhood-by-year-fixed effects (γ_{ht}) and in our regressions; a neighborhood includes the treated county and all its control neighboring counties.

Table III presents the estimates of the effects of ratings on elections. In Panel A, the dependent variable is "Incumbent Party Share." The estimates imply that a 10% increase in

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 O

Table II. Summary statistics of economic outcomes

This table presents the mean, standard deviation, and the number of observations for economic variables separately for treatment and control groups for the year before the recalibration. The treatment group includes counties with at least one local government unit upgraded and the control group comprises all their non-treated neighboring counties. Column (7) presents the raw differences between treatment and control groups. Column (8) presents the difference between treatment and control groups adjusted by neighborhood-by-year-fixed effects and the number of votes. *P*-values clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Treatment group			Control group			Difference	
	Mean	Standard deviation	Number of observations	Mean	Standard deviation	Number of observations	Raw difference (*P*-value)	Adjusted difference (*P*-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Offer Yield (%)	3.155	1.208	2,791	3.046	1.718	1,066	0.109	0.217*
							(0.322)	(0.066)
Issue Amount ($ million)	197.3	503.5	2,791	32.0	73.3	1,066	165.3***	−7.9
							(0.000)	(0.635)
Local Government Expenditures ($ million)	1,314.5	2,716.6	3,048	192.2	388.9	3,074	1,122.2***	−133.2*
							(0.000)	(0.069)
Local Tax Rate (%)	0.058	0.027	3,052	0.048	0.046	3,079	0.011***	0.009***
							(0.000)	(0.000)
Local Government Employment (thousand)	8.714	16.668	2,499	1.820	3.778	1,463	6.894***	−0.460
							(0.000)	(0.326)
Private Employment (thousand)	111.7	209.0	3,045	17.1	33.7	3,064	94.6***	−4.6
							(0.000)	(0.102)

(continued)

Table II. Continued

	Treatment group			Control group			Difference	
	Mean	Standard deviation	Number of observations	Mean	Standard deviation	Number of observations	Raw difference (*P*-value)	Adjusted difference (*P*-value)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemployment Rate (%)	8.833	2.351	3,052	9.495	3.252	3,079	−0.662***	−0.780***
							(0.000)	(0.000)
Income ($ million)	5,179.7	8,921.2	3,048	844.3	1,588.1	3,074	4,335.4***	−107.8
							(0.000)	(0.352)
Growth Local Government Expenditures	0.044	0.092	3,048	0.038	0.117	3,074	0.005	0.010*
							(0.288)	(0.082)
Growth Local Government Employment	0.006	0.082	2,499	0.011	0.094	1,462	−0.005	−0.004
							(0.334)	(0.519)
Growth Private Employment	−0.054	0.042	3,045	−0.055	0.072	3,046	0.001	0.003
							(0.833)	(0.222)
Growth Income	−0.038	0.029	3,048	−0.042	0.046	3,074	0.003*	0.007***
							(0.052)	(0.000)
Number of Households (thousands)	93.3	161.1	3,048	20.0	30.7	3,074	73.2***	−3.8
							(0.000)	(0.290)

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 O

Table III. Effect of municipal bond ratings on incumbent party share

This table presents difference-in-difference estimates of regressions of the incumbent party vote share and incumbent candidate vote share in gubernatorial elections around the recalibration event (April–May 2010). In Panel A, the dependent variable is the number of votes that the incumbent party received divided by the total number of votes cast in the respective county. In Panel B, the dependent variable is the number of votes that the incumbent candidate received divided by the total number of votes cast in the respective county. In Panel B, the sample is restricted to candidates running for reelection. Recalibrated is the fraction of upgraded local government units in each county. Post is a dummy variable that takes the value of 1 for the 2010–12 period and zero for the period before 2010. In Columns (1)–(3), the sample consists of treated counties and all their non-treated neighboring counties. In Column (4), the sample is restricted to counties where at least one local government has bond issues. In Column (5), the sample is limited to counties where at least one local government is rated by Moody's. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Panel A: Incumbent Party Share					
Recalibrated × Post	0.074**	0.068*	0.057*	0.078**	0.089**
	(0.036)	(0.038)	(0.033)	(0.038)	(0.041)
Number of Votes	−0.029	−0.031	−0.074***	−0.083***	−0.082***
	(0.030)	(0.029)	(0.024)	(0.028)	(0.030)
Unemployment Rate		0.001	−0.002	0.000	0.002
		(0.003)	(0.002)	(0.004)	(0.005)
Income		0.041	0.009	0.034	0.041
		(0.050)	(0.041)	(0.067)	(0.078)
Local Tax Rate		−0.044	0.086	0.066	0.150
		(0.107)	(0.087)	(0.130)	(0.204)
Local Government Expenditures		0.005	−0.008*	−0.015*	−0.018*
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.887	0.887	0.930	0.921	0.912
Number of observations	12,448	12,448	12,448	9,506	7,749
Sample	All	All	All	Bond issuers	Moody's rated
Panel B: Incumbent Candidate Share					
Recalibrated × Post	0.693**	0.710**	0.632**	0.731**	0.858**
	(0.272)	(0.281)	(0.281)	(0.321)	(0.342)
Number of Votes	−0.549***	−0.553***	−0.529***	−0.596***	−0.562***
	(0.120)	(0.119)	(0.123)	(0.138)	(0.140)
Unemployment		0.019	0.015	0.004	0.009
		(0.017)	(0.018)	(0.025)	(0.029)
Income		0.223	0.162	0.211	0.379
		(0.373)	(0.375)	(0.564)	(0.666)
Local Tax Rate		0.331	0.371	1.149	2.520*
		(0.298)	(0.301)	(0.767)	(1.286)
Local Government Expenditures		−0.047	−0.042	−0.122	−0.268*
		(0.067)	(0.068)	(0.109)	(0.137)
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.816	0.816	0.821	0.804	0.791
Number of observations	4,113	4,113	4,113	3,153	2,679
Sample	All	All	All	Bond issuers	Moody's rated

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

the fraction of local governments upgraded in a county increases the vote share by 0.7 percentage points. In Columns (2)–(5), we include controls for contemporaneous economic outcomes.[16] In Columns (3)–(5), we include state-by-year-fixed effects. In Column (4), the sample is restricted to counties where at least one local government-issued bonds in the municipal bond market. In Column (5), the sample is restricted to counties where at least one local government-issued bond is rated by Moody's. The effect is robust across all specifications.[17] The reduction in the magnitude of the estimates when we include controls for local economic activity indicates that rating upgrades seem to partly operate through changes in local economic conditions (within a neighborhood). However, the bulk of the effect is still present even after including these controls.[18]

Figure 4 shows the evolution of the difference in "Incumbent Party Share" in gubernatorial elections around the recalibration between counties in the treatment group (Recalibrated > 0) and control group (Recalibrated $= 0$). The two groups follow similar trends before the recalibration and we observe a significant differential effect between treatment and control groups after the recalibration.

In Panel B of Table III, we perform a robustness test focusing on governors running for reelection. The dependent variable is the number of votes that the incumbent candidate received divided by the county's total number of votes ("Incumbent Candidate Share"). Our results are stronger for this subsample. The estimates in all specifications imply that a 10% increase in the fraction of local governments upgraded in a county increases the vote share by approximately 7 percentage points.

In our analysis, we focus on the incumbent party share. We believe that this measure offers a complete picture of the effects of ratings on elections because it allows us to study the impact of ratings on succession. In our sample, twenty-five governors had a term limit after the recalibration. Out of the remaining governors, only sixteen decided to run for reelections.[19] Using the incumbent party share, we can see the effects of ratings on elections even if a governor cannot run due to a term limit or because he/she decided to follow a different career path.

Overall, the results show that CRAs' actions influence voting outcomes. We find that candidates affiliated with political parties in power at the time of the Moody's recalibration obtained a higher vote share. Our results suggest that incumbents are rewarded for positive

16 To mitigate concerns that our results are contaminated by a "Bad Control" problem, we replicate the results in Table III without including any controls in Online Appendix Table IA.1. The results are qualitatively similar without controls.

17 Online Appendix Table IA.2 presents estimates separately for the samples of general obligation bonds and revenue bonds. We find positive effects on elections using both types of issuances, but the effects are stronger in the sample of revenue bonds.

18 To guarantee that the re-sampling does not affect our standard errors, we estimate the standard errors in several ways. Online Appendix Table IA.3 replicates the results in Table III with standard errors clustered at the neighborhood level and state level, rather than at the county level. In Online Appendix Table IA.4, we re-estimate the standard errors using bootstrap methods. In Panel A, we bootstrap at the county level and in Panel B, we bootstrap at the neighborhood level. Our results are robust to these different ways of estimating the standard errors.

19 Some candidates chose not to run due to a political scandal, like Connecticut governor Jodi Rell. Others decided to take a different path in their careers, such as Florida Governor Charlie Christ, who chose to run for the US Senate instead of reelection in 2010.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

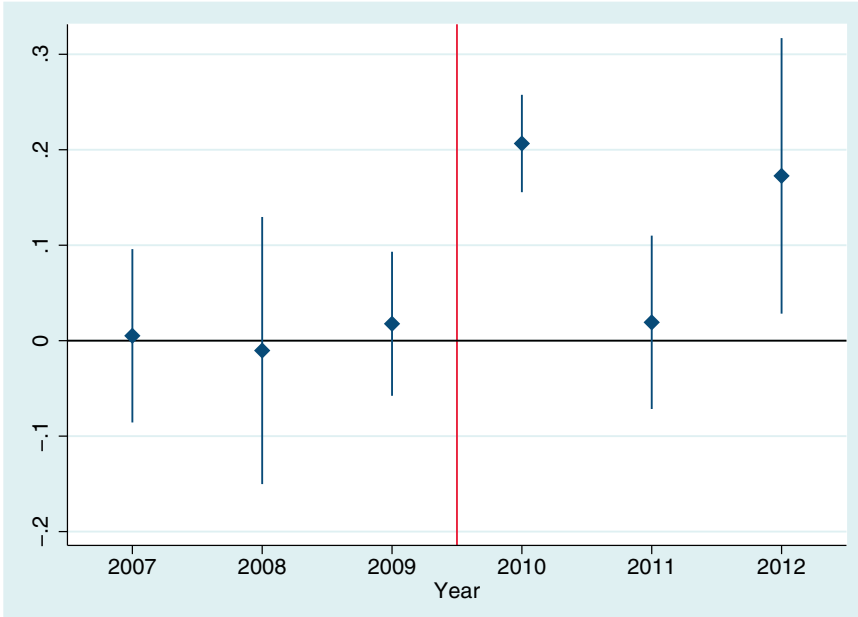

Figure 4. Difference in gubernatorial election outcomes around the recalibration. This figure shows the difference in incumbent party vote share between counties in the treatment and control groups and 90% confidence intervals in gubernatorial elections in the 2006–12 period around the recalibration event (April–May 2010). The estimates are regression coefficients of Incumbent Party Share on event-year dummies interacted with the Recalibrated variable. Recalibrated is the fraction of upgraded local government units in each county. The treatment group includes counties with at least one municipal bond upgraded during the recalibration (Recalibrated > 0) and the control group includes counties with no municipal bond upgraded (Recalibrated $= 0$). The sample consists of treated counties and all their non-treated neighboring counties. The regression includes "Number of Votes," county-fixed effects, neighborhood-by-year-fixed effects as controls.

news (exogenous rating upgrades due to the recalibration, in our experiment) even if the news is beyond their control.[20]

To shed additional light on municipal rating upgrades' political impacts, we explore whether the recalibration energizes the incumbent party's base and increases their likelihood of voting. Table IV shows the effect of the recalibration on the number of votes cast in the county. The estimates imply that a 10% increase in the fraction of local governments upgraded increases turnout between 0.5% and 1.4%.

We also analyze the impact of the recalibration-generated upgrades on the contestability of elections. A stronger incumbent candidate might discourage candidates from running against him/her. We test whether municipal rating upgrades affect the probability of the

20 We analyze the role of political affiliation in response to Moody's recalibration in the Online Appendix. Online Appendix Table IA.5 shows that incumbent Democratic candidates seem to benefit more from the recalibration-related upgrades than Republican candidates, as Democratic incumbents experienced a larger increase in their vote share. However, Online Appendix Table IA.6 shows no significant differences in policies and economic outcomes between Democratic and Republican counties following the upgrades.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table IV. Effect of municipal bond ratings on total number of votes

This table presents difference-in-difference estimates of regressions of the total number of votes in gubernatorial elections around the recalibration event (April–May 2010). The dependent variable is the logarithm of the total number of votes cast in the county. Recalibrated is the fraction of upgraded local government units in each county. Post is a dummy variable that takes the value of 1 for the 2010–12 period and zero for the period before 2010. In Columns (1)–(3), the sample consists of treated counties and all their non-treated neighboring counties. In Column (4), the sample is restricted to counties where at least one local government has bond issues. In Column (5), the sample is restricted to counties where at least one local government is rated by Moody's. Economic controls include Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Recalibrated × Post	0.146***	0.102***	0.079***	0.060**	0.048*
	(0.025)	(0.025)	(0.023)	(0.025)	(0.027)
Economic controls	No	Yes	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.998	0.998	0.999	0.999	0.999
Number of observations	12,448	12,448	12,448	9,506	7,749
Sample	All	All	All	Bond issuers	Moody's rated

election having more than two candidates. Because the number of candidates is at the state level, we cannot include state-by-year-fixed effects in these regressions. The estimates in Table V imply that a 10% increase in the fraction of local governments upgraded in a county decreases the probability that the race will have more than two candidates by between 0.7% and 1.1%. The results are statistically insignificant when we use the entire sample and become stronger when we restrict the sample to counties where at least one local government-issued bonds.

4. How Do Credit Ratings Affect Elections?

Municipal bond ratings can affect election outcomes through two non-mutually exclusive channels. Incumbents can improve local economic conditions by adopting an expansionary fiscal policy, taking advantage of the relaxation of financial constraints and lower borrowing costs following the recalibration-related rating upgrades. Rating upgrades may also have a direct effect on voting behavior. In the context of asymmetric information, voters may interpret an exogenous rating upgrade (over which incumbents have no control) as a signal of an incumbent's ability or effort. Thus, the impact of municipal bond ratings on election outcomes might occur because voters change their incumbent quality perceptions. Suppose a higher bond rating is interpreted as evidence of responsible budgeting practices and sound economic policies. In that case, a rating upgrade could lead to a change in voting behavior even in the absence of any real change in policies or real economic conditions.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table V. Effect of municipal bond ratings on number of opposing candidates

This table presents difference-in-difference estimates of regressions of the number of opposing candidates in gubernatorial elections around the recalibration event (April–May 2010). The dependent variable is a dummy variable that takes the value of 1 when the election had a third candidate and zero otherwise. Recalibrated is the fraction of upgraded local government units in each county. Post is a dummy variable that takes the value of 1 for the 2010–12 period and zero for the period before 2010. In Columns (1) and (2), the sample consists of treated counties and all their non-treated neighboring counties. In Column (3), the sample is restricted to counties where at least one local government has bond issues. In Column (4), the sample is restricted to counties where at least one local government is rated by Moody's. All regressions include Number of Votes as control (coefficient not shown). Economic controls (coefficients not shown) include Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Recalibrated × Post	−0.072	−0.077	−0.112**	−0.102*
	(0.049)	(0.049)	(0.052)	(0.054)
Economic controls	No	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes
R^2	0.977	0.977	0.981	0.980
Number of observations	12,448	12,448	9,506	7,749
Sample	All	All	Bond issuers	Moody's rated

4.1 Fiscal Policy and Local Economic Conditions

Previous research has shown that the recalibration is associated with economically and statistically significant real effects on the local economy due to expansionary fiscal policies. Cornaggia, Cornaggia, and Israelsen (2018) show that the recalibration is associated with a significant decline in the average offer yield of municipal bonds. Adelino, Cunha, and Ferreira (2017) show that reduced borrowing costs allowed local governments to increase the amount of bonds issued. The proceeds of the increase in debt financing were used to increase local government expenditures and employment and reduce taxes. The increase in local government expenditures had positive spillovers to the private sector as counties with more upgraded local governments experienced increased private employment and income. The effect is particularly strong in the non-tradable sector, which is more dependent on local demand (Mian and Sufi, 2014; Adelino, Ma, and Robinson, 2017). We replicate the findings in Adelino, Cunha, and Ferreira (2017) and Cornaggia, Cornaggia, and Israelsen (2018) in Table IA.7 of the Online Appendix. Local governments facing lower borrowing costs were able to expand bond financing and adopt an expansionary fiscal policy. In turn, this fiscal policy improved local economic conditions.

We implement a two-stage procedure in which we analyze the impact of the recalibration on election outcomes through its effect on the amount issued in the municipal bond market. We use the issuance amount as an overall measure of fiscal policy changes (government expenditures and taxes) due to the county's recalibration. In the first-stage regression,

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

we test whether upgraded municipalities increase the amount of bonds issued. We estimate the following regression model:

$$I_{ibst} = \beta \text{Recalibrated}_{ibs} \times \text{Post}_t + \mathbf{X}'_{ibst}\boldsymbol{\Theta} + \alpha_i + \gamma_{bt} + \lambda_{st} + \varepsilon_{ibst}, \quad (2)$$

where I_{ibst} is the logarithm of the total amount of bonds issued by local governments in each county and year ("Issue Amount"); other variables are defined as in Equation (1). The coefficient of interest is β, which measures the impact of the recalibration on the municipal bond issue amount. In the second-stage regression, we measure the impact of the increase in bond financing explained by the recalibration on election outcomes.

Table VI, Panel A, presents the estimates of the first-stage regression. The estimates of β indicate a positive and significant effect of the recalibration-related upgrades on the amount of bonds issued ("Issue Amount"). We then study whether the increase in bond financing is associated with a change in voting outcomes. Table VI, Panel B, presents the estimates of the second-stage regression. We find a positive and economically significant effect of the Issue Amount on the Incumbent Party Share. Using the estimates in Column (3), we find that a 10% increase in Issue Amount is associated with an increase in the winning margin of an incumbent party of 1.45 percentage points. These estimates provide an upper bound for an expansionary fiscal policy's impact on incumbent party voting share. The recalibration may have affected voting through an increase in external financing by municipalities and a change in perceptions. In that case, these coefficients would be biased upward, as they would capture the effect attributable to the change in bond issuance and the effect of the change in voters' perceptions. In the next subsection, we study the direct effect of municipal ratings on elections.[21]

4.2 Direct Effect of Ratings

Rating upgrades may directly affect voting behavior beyond the effect attributable to better access to the municipal bond market and economic conditions. Table III presents the first test of this hypothesis. The estimates of the interaction term Recalibrated × Post remain economically and statistically significant after the inclusion of economic controls such as Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. This result suggests that the relaxation of local governments' financial constraints is insufficient to explain the changes in voting behavior. This section provides additional evidence of the direct effect of municipal bond ratings on election outcomes.

4.2.a. *Political discourse*

Anecdotal evidence suggests that credit ratings are used in the political discourse to persuade voters of the candidates' economic acumen. During one of his first interviews as the 2016 Republican presidential nominee (interview on "60 Minutes" on CBS television on July 17, 2016), Donald Trump pointed to the AAA credit rating of the State of Indiana bonds, where Mike Pence was governor, as an indication of the quality of the vice-president candidate: "I looked at the numbers. Unemployment? What a great job he did. Jobs? What a great job he did. Triple-A rating on his bonds." Mike Pence also used the

21 The results in Table VI do not quantify the precise contribution of bond issuances to the total effect. The reduced form regressions are mechanically connected with the estimates in the 2SLS regressions. The purpose of Table VI is to provide evidence that bond issuance is an important potential channel for the relationship between rating upgrades and election outcomes.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table VI. Effect of fiscal policy on voting outcomes

This table presents estimates of regressions of the logarithm of the total amount of bonds issued by local governments in the county (Issue Amount) on the incumbent party share using a two-stage procedure. Panel A presents first-stage regression estimates of the Issue Amount on the Recalibrated × Post interaction variable. Recalibrated is the fraction of upgraded local government units in each county. Post is a dummy variable that takes the value of 1 for the 2010–12 period and zero for the period before 2010. Panel B presents second-stage regression estimates. The dependent variable is the number of votes that the incumbent party received divided by the total number of votes cast in the respective county. In Columns (1)–(3), the sample is restricted to counties where at least one local government has bond issues. In Column (4), the sample is restricted to counties where at least one local government is rated by Moody's. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Panel A: First stage—issue amount (log)				
Recalibrated × Post	0.841***	1.103***	1.103***	0.864***
	(0.234)	(0.254)	(0.256)	(0.266)
Number of Votes	−0.600**	−0.377	−0.377	−0.551*
	(0.278)	(0.308)	(0.310)	(0.320)
Unemployment Rate		−0.023	−0.023	−0.010
		(0.034)	(0.034)	(0.035)
Income		−0.610	−0.610	0.597
		(0.739)	(0.744)	(0.803)
Local Tax Rate		2.670	2.670	7.339*
		(3.546)	(3.570)	(4.264)
Local Government Expenditure		−0.688***	−0.688***	−0.864***
		(0.193)	(0.194)	(0.244)
County-fixed effects	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes
R^2	0.987	0.987	0.987	0.987
Number of observations	3,434	3,427	3,427	3,159
F-test	12.92	18.83	18.59	10.56
Sample	Bond issuers	Bond issuers	Bond issuers	Moody's rated
Panel B: Second stage—incumbent party share				
Issue Amount (log)	0.092***	0.145***	0.145***	0.217***
	(0.034)	(0.036)	(0.037)	(0.069)
Number of Votes	−0.090**	−0.138***	−0.138***	−0.041
	(0.040)	(0.053)	(0.053)	(0.087)
Unemployment Rate		0.035***	0.035***	0.031***
		(0.005)	(0.006)	(0.008)
Income		0.545***	0.545***	0.247
		(0.114)	(0.114)	(0.195)
Local Tax Rate		−2.119***	−2.119***	−3.280***
		(0.585)	(0.589)	(1.168)
Local Government Expenditure		−0.014	−0.014	0.014
		(0.038)	(0.038)	(0.076)
County-fixed effects	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes
R^2	0.959	0.930	0.930	0.861
Number of observations	3,434	3,427	3,427	3,159
Sample	Bond issuers	Bond issuers	Bond issuers	Moody's rated

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

rating of Indiana as a selling point when he introduced himself at the Republican National Convention on July 20, 2016: "We in Indiana have a $2 billion surplus, the highest credit rating in the nation, even though we have cut taxes every year since I became governor four years ago." Interestingly, Indiana had enjoyed this rating since 2008, before Mike Pence's election as governor in 2012, which suggests that politicians may be tempted to tout credit ratings, even if they are not responsible for the upgrade.[22]

Mike Pence and Donald Trump are not alone in using credit ratings to persuade voters of their quality as a politician and fiscal responsibility. Indeed, this behavior seems to be widespread. To illustrate this, we perform searches for quotes made by US Governors in office at the time of the recalibration in 2010. Even though these governors know that the recalibration was just a change of rating scale, they still use it as a selling point with voters. For example, Arkansas governor Mike Beebe mentioned Moody's high credit rating without ever referring to the recalibration:

> The strength of Arkansas's economy received yet another confirmation recently, this time from Moody's Investor Services, one of the Big Three credit-rating agencies worldwide. Recently, Moody's gave its second-highest rating on a $44 million bond series issued by the State of Arkansas. The rating indicates to investors that the bonds are of high quality and that they carry very low credit risk.[23]

Similarly, Governor Earl Ray Tomblin made the following statements to the press in 2011:

> Two weeks ago Fitch Ratings increased its rating of West Virginia's general obligation (GO) bonds from "AA" to "AA+." This follows similar increases in the last two years from the other two major bond raters. Moody's Investor Service increased it's rating for West Virginia GO bonds in 2010, following Standard and Poor Rating Services 2009 increase.[24]

> Our Rainy Day Fund is one of those things that are pots of money out there that has really helped the bond rating agencies that upgrade our bond rating to AA-1, which is one step away from their highest ratings, especially with Moody's.[25]

While the media could, in principle, provide additional background on these claims and help voters distinguish rating changes due to the recalibration from regular upgrades, this does not necessarily happen. In an article mentioning the record of several Republican candidates, the newspaper The Hill describes Mississippi's recalibration-driven upgrade as a regular rating change:

> Moody's bumped Mississippi's rating from Aa3 up to Aa2 last April, while S&P has left the state's rating at AA during Barbour's time in office. S&P put Mississippi on a watch list for a

22 There are other examples of this pattern. 2012 Ohio Senate candidate Josh Mandel was accused of falsely claiming that Ohio's rating improved while he was the treasurer; Paul LePage, mayor of Waterville, ME (later Maine's governor), was credited with a miracle in the local news for improving the city's rating; and Hawaiian governor David Ige made an official press announcement of a two-notch upgrade of state bonds.

23 https://www.carrollconews.com/blogs/1364/entry/48610

24 http://shepherdstownchronicle.com/page/content.detail/id/502438/West-Virginia-s-fiscal-progress-continues.html?nav=5094

25 https://www.register-herald.com/news/gubernatorial-candidate-profiles-democrat-earl-ray-tomblin/article_5669fee2-ee66-547d-a355-4c4058b6858e.html

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

revised negative outlook on its credit at the height of Hurricane Katrina in August of 2005, but restored Mississippi to a stable outlook that November.[26]

These are just a few examples of the multitude of cases in which governors tout credit rating upgrades as a political triumph. In the Online Appendix, we provide additional quotes of governors exhibiting this behavior.

4.2.b. *Timing of the effect*

To shed further light on the mechanism through which rating upgrades impact elections, we study the timing of the effect of the recalibration on voting outcomes. The effect's timing is informative because a direct effect of ratings on elections would occur immediately after the recalibration as voter's perceptions would be altered immediately by the recalibration event. In contrast, an effect through fiscal policy would happen with a lag because improvements in local economic conditions take time to materialize.

Table VII presents the effects of rating upgrades on elections by year. The coefficients of interest are the interactions between the treatment variable "Recalibrated" and the 2010, 2011, and 2012 calendar-year dummy variables. This table allows us to observe when the effect of ratings is incorporated in the voting outcomes. Consistent with a direct effect of ratings, Columns (1) and (2) show that rating upgrades start affecting electoral voting in November 2010 (the year of the recalibration), just a few months after the recalibration. Although the economic magnitudes are similar, the results in Columns (3)–(5) are statistically insignificant due to decreased statistical power when we include state-by-year-fixed effects. Although it is unlikely that an expansionary fiscal policy would have affected the local economy in such a short period, news about rating upgrades could likely have an immediate effect on voters' perceptions of the incumbent's quality. While expectations about the future expansionary fiscal policy may affect voting immediately in 2010, if voters do not believe that the incumbent was responsible for the upgrade, there is no reason for those expectations to attach specifically to the incumbent party. Easier access to financial markets allows whoever is in power (be it an incumbent or not) to increase spending. Therefore, if the only way credit rating upgrades affected voting was through a change in expectation of future economic conditions driven by an increase in government spending, this should not necessarily lead to the rise in voting for the incumbent party. To change their behavior, voters must also update their opinion about the incumbent's managerial ability.[27] We also observe a significant effect in 2012, which is prevalent across all specifications. This confirms that improved local economic conditions due to better access to debt financing are also a channel explaining the effect of ratings on voting behavior.

4.3 Information and Voters' Perceptions

This section tests whether the effect on elections is stronger when voters are more likely to be exposed to information about credit ratings. To test the hypothesis that voters use ratings to make voting decisions, we explore cross-sectional variation on whether voters are likely to be informed about ratings. If voters are to be persuaded directly by ratings, they

26 https://thehill.com/blogs/blog-briefing-room/news/157095-most-gop-presidential-hopefuls-saw-their-states-credit-ratings-rise

27 We provide empirical support for these economic arguments in Online Appendix Table IA.8. The results show that whether the incumbent party remains in control has no bearing on economic conditions' reaction to rating upgrades.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table VII. Effect of municipal bond ratings on election outcomes by year

This table presents annual difference-in-difference estimates of regressions of the incumbent party vote share in gubernatorial elections around the recalibration event (April–May 2010). Recalibrated is the fraction of upgraded local government units in each county. 2010, 2011, and 2012 are calendar year dummy variables that take the value of 1 in the years 2010, 2011, and 2012, and zero otherwise. In Columns (1)–(3), the sample consists of treated counties and all their non-treated neighboring counties. In Column (4), the sample is restricted to counties where at least one local government has bond issues. In Column (5), the sample is restricted to counties where at least one local government is rated by Moody's. All regressions include Number of Votes as control (coefficient not shown). Economic controls (coefficients not shown) include Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Recalibrated × 2010	0.079*	0.074*	0.047	0.062	0.074
	(0.044)	(0.045)	(0.037)	(0.045)	(0.049)
Recalibrated × 2011	0.014	0.010	0.032	0.080	0.089
	(0.093)	(0.093)	(0.087)	(0.082)	(0.085)
Recalibrated × 2012	0.184***	0.175***	0.215***	0.267***	0.288***
	(0.060)	(0.062)	(0.061)	(0.091)	(0.087)
Economic controls	No	Yes	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.887	0.887	0.930	0.921	0.912
Number of observations	12,448	12,448	12,448	9,506	7,749
Sample	All	All	All	Bond issuers	Moody's rated

would need to be aware of them. One way through which voters would be exposed to news about ratings is by searching online. We collect data from Google Trends on the evolution of news searches for the term "credit rating." We focus on the interval between May and November when the searches are most likely related to political campaigns. Because the term "credit ratings" is not a popular news search term, the finest level at which Google Trends provides information is at the state level. There are also several states with zero searches in all years of the sample.[28] Therefore, we focus on the eleven states that have non-zero searches in at least 1 year of our sample.[29] "Rating News" is a dummy variable that takes the value of 1 for states with an above-median increase in news searches for the term "credit rating" from before the recalibration (2006–09) to after the recalibration

28 According to the Google Trends website, its measure is obtained as follows: "Google Trends adjusts search data to make comparisons between terms easier. Each data point is divided by the total searches of the geography and time range it represents to compare relative popularity." Therefore, to appear in Google Trends, a search term must obtain a minimum threshold of searches among all searched terms.

29 The eleven states with data on Google Trends are California, Florida, Illinois, Massachusetts, New Jersey, New York, Oregon, Pennsylvania, Texas, Virginia, and the District of Columbia.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

(2010–12) and zero otherwise. States with an increase in rating news searches are more likely to be those where voters are paying closer attention to ratings as an indicator of politicians' quality. We perform these tests using gubernatorial elections as rating news searches are available only at the state level.

Panel A of Table VIII presents the results for rating news searches. The explanatory variable of interest is the triple interaction term Recalibrated × Post × Rating News, which measures the effect of ratings on election outcomes in counties in states with high news searches versus neighboring counties in states with low news searches. The interaction term coefficient is positive and significant in all specifications. Therefore, the effect of rating upgrades on elections is stronger in regions with a high volume of searches about ratings.

We perform two robustness checks to guarantee that voters are searching for information on "credit rating" and not for terms that reflect general local economic conditions during the 2007–09 financial crisis. We repeat our tests using the "Crisis News" dummy variable, which is based on news searches for the term "financial crisis" instead of the term "credit ratings." We also perform a similar test using the dummy variable "Credit Score News," which is based on news searches for the term "credit score," to rule out the possibility that people might be searching for their credit score. Table IA.9 in the Online Appendix shows the results. Incumbent politicians in counties in states in which people were searching for these alternative terms do not benefit more on elections from the recalibration.[30]

To further test whether voters who are more likely to be informed about ratings have a stronger electoral response to the recalibration, we exploit cross-sectional variation on local newspapers' availability at the recalibration time. Voters in states with more local newspapers are more likely to be exposed to news about municipal bond ratings.[31] In turn, this makes it more plausible that voters are reacting directly to rating news, instead of responding solely to local economic conditions (Snyder and Strömberg, 2010). We define "Local Newspapers" as the number of local newspapers available in a given state in 2009 (the year before the recalibration) per 10,000 inhabitants.

Panel B of Table VIII presents the results. The explanatory variable of interest is the triple interaction term Recalibrated × Post × Local Newspapers. The triple interaction coefficient estimates are positive and significant in Columns (1)–(4), which implies that the effects of ratings on election outcomes are more pronounced in counties with more local newspapers. We observe a reduction in the triple interaction term estimates when we include state-by-year-fixed effects. When we restrict the sample to counties rated by Moody's, the estimate becomes statistically insignificant. However, the estimate remains economically similar. The smaller sample reduces the statistical power of our tests and makes our estimates less precise. The estimates of the interaction Recalibrated × Post become insignificant when we include the triple interaction term, which indicates that information is a key channel.

Voters may also learn about changes in municipal bond ratings by directly following the municipal bond market. Voters who have invested their savings in municipal bonds issued by their local governments are more likely to learn about the upgrades. We use municipal

30 We also study whether education plays a role in voters' attribution. Online Appendix Table IA.10 shows no evidence that education plays a role in voters' response to the recalibration.

31 We thank Pengjie Gao, Chang Lee, and Dermot Murphy for sharing the local newspaper data used in Gao, Lee, and Murphy (2018).

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table VIII. Effect of rating news searches, local newspapers, and municipal bond tax privilege

This table presents difference-in-difference estimates of regressions of the incumbent party vote share in gubernatorial elections around the recalibration event (April–May 2010). Recalibrated is the fraction of upgraded local government units in each county. Post is a dummy variable that takes the value of 1 for the 2010–12 period and zero for the period before 2010. Rating News is a dummy variable that takes the value of 1 if the increase in news searches for the term "credit rating" from 2006–09 to 2010–10 is above the median and zero otherwise. Local Newspapers is the total number of local newspapers in the state in the year before the recalibration (2009) per 10,000 inhabitants. Tax Privilege is a dummy variable that takes the value of 1 if the state municipal bond tax privilege measure (Babina *et al.*, 2021) is above the median, and zero otherwise. In Columns (1)–(3), the sample consists of treated counties and all their non-treated neighboring counties. In Column (4), the sample is restricted to counties where at least one local government has bond issues. In Column (5), the sample is restricted to counties where at least one local government is rated by Moody's. All regressions include Number of Votes as control (coefficient not shown). Economic controls (coefficients not shown) include Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
Panel A: News Searches					
Recalibrated × Post	−0.247***	−0.257***	−0.252***	−0.267***	−0.168
	(0.071)	(0.070)	(0.070)	(0.091)	(0.123)
Recalibrated × Post × Rating News	0.709***	0.694***	0.671***	0.660***	0.513***
	(0.136)	(0.128)	(0.123)	(0.135)	(0.158)
Economic controls	No	Yes	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.950	0.950	0.956	0.948	0.941
Number of observations	3,160	3,160	3,160	2,384	1,906
Sample	All	All	All	Bond issuers	Moody's rated
Panel B: Number of state's local newspapers per 10,000 inhabitants					
Recalibrated × Post	−0.075	−0.086	−0.061	−0.036	0.004
	(0.069)	(0.071)	(0.058)	(0.072)	(0.077)
Recalibrated × Post × Local Newspapers	2.034**	2.067**	1.613***	1.499**	1.116
	(0.834)	(0.842)	(0.608)	(0.701)	(0.757)
Economic controls	No	Yes	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.895	0.895	0.930	0.921	0.912
Number of observations	12,448	12,448	12,448	9,506	7,749

(continued)

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table VIII. Continued

	(1)	(2)	(3)	(4)	(5)
Sample	All	All	All	Bond issuers	Moody's rated
Panel C: Municipal bond tax privilege					
Recalibrated × Post	0.038	0.034	−0.002	0.031	0.047
	(0.043)	(0.044)	(0.041)	(0.046)	(0.050)
Recalibrated × Post × Tax Privilege	0.120	0.118	0.221***	0.186**	0.163*
	(0.086)	(0.086)	(0.072)	(0.089)	(0.095)
Economic controls	No	Yes	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes	Yes
State × year-fixed effects	No	No	Yes	Yes	Yes
R^2	0.887	0.887	0.931	0.921	0.912
Number of observations	12,448	12,448	12,448	9,506	7,749
Sample	All	All	All	Bond issuers	Moody's rated

bonds' local ownership as a proxy for the likelihood that voters learn about the rating upgrades by following the news and tracking the value of their bond holdings. An increase in the value holdings' value to the rating upgrades can signal that the local government is well managed. Thus, CRAs could directly affect the wealth of those voters who hold a significant amount of municipal bonds in their portfolios. Because changes in municipalities' fiscal policies would not drive this increase in wealth, they also represent a direct effect of CRAs.

We test this idea by exploiting a unique feature of the municipal bond market. Municipal bonds are exempt from state income taxes if the bond buyer is a resident of the respective state. Therefore, there are strong incentives for local ownership of municipal bonds in states with high-income taxes (e.g., California) versus states with low-income taxes (e.g., Florida). We use the measure of the municipal bond privilege of Babina *et al.* (2021), who define tax privilege as the highest state income tax rate applied to income from municipal bonds issued by other states minus the highest state income tax rate applied to income from own state municipal bonds.[32] This measure captures the comparative advantage of holding local versus out-of-state municipal bonds. Babina *et al.* (2021) show that states with the highest privilege have the highest marginal state income tax rates, confirming that much of the variation in privilege comes from the variation in the tax rate. However, some high-tax states do not tax-exempt municipal bonds, while others exempt both in and out-of-state municipal bonds. The latter states, therefore, have a low tax privilege. Babina *et al.* (2021) show that municipal bond funds in high privilege states hold more in-state municipal bonds. Thus, this measure is a proxy for the in-state holdings of municipal bonds.

Panel C of Table VIII presents the results. "Tax Privilege" is an indicator variable that takes the value of 1 if the state municipal bond tax privilege is above the median and zero

32 We thank Tania Babina, Pab Jotikasthira, Christian Lundblad, and Tarun Ramadorai for graciously sharing their data with us. See Babina *et al.* (2021) for additional details on this measure.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

otherwise. The explanatory variable of interest is the triple interaction Recalibrated × Post × Tax Privilege. The triple interaction coefficient estimates are positive and significant in Columns (3)–(5), in which we include state-by-year-fixed effects. These results imply that the recalibration effects on election outcomes are more pronounced within states with high tax privilege. When we do not saturate the regressions with state-by-year-fixed effects, our results are statistically insignificant. This could be due to differences in state-level characteristics unaccounted for by our control variables. Similarly to Panel B, the estimates of the main effect Recalibrated × Post become insignificant in all specifications in which we include the triple interaction term. This indicates that state taxes on municipal bond holdings are an important channel for the effects of ratings in elections.

Taken as a whole, our results suggest that credit ratings affect election outcomes through two (non-mutually exclusive) channels: (1) a direct effect of ratings, as they provide information on the economic acumen of government officials (certification role), which affects the voter perceptions of the quality of politicians and (2) an indirect effect of ratings through relaxation of financial constraints that local governments are exposed to, which impacts local economic conditions in the following years.

4.4 Effect of State and City Upgrades

Bonds issued directly by the incumbent candidate's office may play a critical role in affecting election outcomes. Indeed, the quotes we provide in Section 4.2 and the Online Appendix reflect governors touting the credit ratings of bonds issued by the state. In this section, we study the effects of the recalibration of bonds directly issued by states and cities on gubernatorial and mayoral elections, respectively.

Table IX presents the estimates of the effects of the recalibration of bonds directly issued by states in Panel A and cities in Panel B on voting outcomes. "State Upgrade" is a dummy variable that takes the value of 1 if a county is part of a state whose bonds were upgraded during the recalibration and zero otherwise. Governors are directly responsible for the issuance of state bonds, and therefore, these upgrades can be interpreted by voters as direct evidence of the politician's quality. When we use the "State Upgrade" dummy variable, we cannot use neighbors within the state as control counties because all counties within a state are assigned the same treatment status. In the case of state bond upgrades, there is no variation in treatment across counties within-state. Thus, in these regressions, we focus on counties that are at the state border. We compare counties in which the state's bonds were upgraded with neighboring counties across the state border in which bonds issued by the state were not upgraded. Our regressions still include neighborhood-by-year-fixed effects, but in this case, the treated and control neighbors are separated by state borders. The estimates in Columns (1) and (2) indicate that governors of states with their bonds upgraded experienced an increase in their vote share of about 13 percentage points relative to non-upgraded neighboring counties across the state border. The estimates in Columns (3) and (4) are statistically insignificant, but the sample is much smaller.

Panel B presents the results for mayoral elections. "City Upgrade" is a dummy variable that takes the value of 1 for cities whose bonds were upgraded during the recalibration and zero otherwise. We do not have enough observations to run our regressions within groups of contiguous cities as we only have data for the larger cities in each state. We include state-by-year-fixed effects, which absorb all shocks that are common to regions within each state and year (i.e., only within-state and year variation is used for

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Table IX. Effect of state and city upgrades

This table presents difference-in-difference estimates of regressions of the incumbent party vote share in gubernatorial elections in Panel A and mayoral elections in Panel B around the recalibration event (April–May 2010). State Upgrade is a dummy variable that takes the value of 1 if the bonds issued by the state were upgraded, and zero otherwise. City Upgrade is a dummy variable that takes the value of 1 if the bonds issued by the city were upgraded, and zero otherwise. Post is a dummy variable that takes the value of 1 for the 2010–12 period, and zero for the period before 2010. In Columns (1) and (2) of Panel A, the sample consists of treated counties and all their nontreated neighboring counties. In Column (3) of Panel A, the sample is restricted to counties where at least one local government has bond issues. In Column (4) of Panel A, the sample is restricted to counties where at least one local government is rated by Moody's. In Columns (1) and (2) of Panel B, the sample consists of cities with available mayoral election data (largest cities in the state). In Column (3) of Panel B, the sample is restricted to cities with bond issuances. In Column (4) of Panel B, the sample is restricted to cities rated by Moody's. All regressions include Number of Votes as control (coefficient not shown). Economic controls (coefficients not shown) include Unemployment Rate, Income, Local Tax Rate, and Local Government Expenditures. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
Panel A: Gubernatorial Elections				
State Upgrade × Post	0.136***	0.129***	0.043	0.016
	(0.025)	(0.025)	(0.043)	(0.055)
Economic controls	No	Yes	Yes	Yes
County-fixed effects	Yes	Yes	Yes	Yes
Neighborhood × year-fixed effects	Yes	Yes	Yes	Yes
R^2	0.862	0.866	0.860	0.846
Number of observations	1,435	1,435	576	380
Sample	All	All	Bond issuers	Moody's rated
Panel B: Mayoral elections				
City Upgrade × Post	0.094*	0.089*	0.115**	0.102**
	(0.048)	(0.046)	(0.057)	(0.046)
Economic controls	No	Yes	Yes	Yes
City-fixed effects	Yes	Yes	Yes	Yes
State × year-fixed effects	Yes	Yes	Yes	Yes
R^2	0.622	0.626	0.627	0.637
Number of observations	394	394	356	367
Sample	All	All	Bond issuers	Moody's rated

identification).[33] The estimates imply that incumbent party candidates of cities that had their bonds upgraded during the recalibration increase their vote share by 9–11 percentage points relative to incumbent party candidates in other cities in the same state and year that did not have their bonds upgraded.

33 In the case of mayoral elections, we assign to each city the economic variables associated with the respective county.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

The fact that the economic magnitudes are larger for rating upgrades of bonds directly linked to the Governor and mayor's offices suggests that economic conditions do not solely drive the electoral rewards. While bonds issued by states and cities do not have a disproportionate revenue weight relative to bonds issued by other local government units, upgrades of bonds issued by states and cities do have a disproportionate electoral effect. For instance, in 2009, there were 108,624 municipal bond issues, for a total amount of $344 billion. States made 3,370 issuances for a total of $88 billion, representing 25% of the total amount. Cities made 26,278 issuances for a total of $41 billion, which represents 12% of the total amount. Although states and cities only represent a fraction of the total amount issued, these upgrades' effects on elections are one order of magnitude bigger than the local government upgrades.

5. Conclusion

We study the effects of financial markets on elections by analyzing the impact of municipal bond ratings on gubernatorial and mayoral elections in the USA. We exploit exogenous variation in credit ratings due to Moody's recalibration of its US municipal bond rating scale in 2010. The recalibration generated cross-sectional variation in ratings across local governments uncorrelated with local economic conditions, resulting in a zero-to-three notch upgrade of municipal bonds. We find significant electoral rewards to incumbent party candidates following municipal bond upgrades.

We provide evidence of two channels through which CRAs influence political outcomes. First, local governments take advantage of the lower borrowing costs in the municipal bond market to increase bond financing and spending. This increase in local government spending leads to improved economic conditions, enhancing the incumbent party's electoral prospects. Second, the recalibration-related upgrades directly affect voting behavior by improving voters' perceptions of incumbent politicians. Rating upgrades affect elections even after controlling for local economic conditions. Besides, the effect of rating upgrades is more pronounced when voters are better informed about the ratings as proxied by online news searches, availability of local newspapers, and attention to municipal bond markets.

These findings suggest that political incumbents are rewarded at the polls when positive shocks benefit their constituents, even if it is outside the incumbent's control (attribution error). This could be due to rational inattention, as the average voter has little incentive to separate political skill from luck, or voters may lack the ability to make such judgments.

Our results highlight the influence of financial markets on the political process. CRAs may have an outsize power, as they can affect elections' outcomes and, therefore, alter public policy choices. However, there is also a potential bright side. Democracy is an imperfect system. It is typically challenging to oust a politician during his or her term for taking actions that favor their interests at the expense of society at large. CRAs can act as a disciplining force that limits the actions of politicians of ill will. Our findings suggest that regulators should be aware that financial markets can affect the political process when designing the financial system's rules.

Data Availability Statement

The data underlying this article will be shared on reasonable request to the corresponding author.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022

Supplementary Material

Supplementary data are available at *Review of Finance* online.

References

Adelino, M., Cunha, I., and Ferreira, M. (2017): The economic effects of public financing: evidence from municipal bond ratings recalibration, *Review of Financial Studies* 30, 3223–3268.

Adelino, M., Ma, S., and Robinson, D. (2017): Firm age, investment opportunities, and job creation, *Journal of Finance* 72, 999–1038.

Almeida, H., Cunha, I., Ferreira, M., and Restrepo, F. (2017): The real effects of credit ratings: the sovereign ceiling channel, *Journal of Finance* 72, 249–290.

Babina, T., Jotikasthira, C., Lundblad, C., and Ramadorai, T. (2021): Heterogeneous taxes and limited risk sharing: evidence from municipal bonds, *Review of Financial Studies* 34, 509–568.

Baghai, R., Servaes, H., and Tamayo, A. (2014): Have rating agencies become more Conservative? Implications for capital structure and debt pricing, *Journal of Finance* 69, 1961–2005.

Bagues, M. and Esteve-Volart, B. (2016): Politicians' luck of the draw: evidence from the Spanish Christmas lottery, *Journal of Political Economy* 124, 1269–1294.

Chen, Z., Lookman, A., Schurhoff, N., and Seppi, D. (2014): Rating-based investment practices and bond market segmentation, *Review of Asset Pricing Studies* 4, 162–205.

Cornaggia, J., Cornaggia, K., and Israelsen, R. (2018): Credit ratings and the cost of municipal financing, *Review of Financial Studies* 31, 2038–2079.

Cornaggia, J., Cornaggia, K. J., and Xia, H. (2016): Revolving doors on Wall Street, *Journal of Financial Economics* 120, 400–419.

Gallup. (2011): Americans decry power of lobbyists, corporations, banks, feds, April.

Gao, P., Lee, C., and Murphy, D. (2018): Financing dies in darkness? The impact of newspaper closures on public finance. Working paper, University of Illinois at Chicago.

Gentzkow, M., Shapiro, J., and Sinkinson, M. (2011): The effect of newspaper entry and exit on electoral politics, *American Economic Review* 101, 2980–3018.

Kempf, E. (2020): The job rating game: revolving doors and analyst incentives, *Journal of Financial Economics* 135, 41–67.

Kempf, E. and Tsoutsoura, M. (2018): Partisan professionals: evidence from credit rating analysts, *Journal of Finance* 76, 2805–2856.

Kisgen, D. (2006): Credit ratings and capital structure, *Journal of Finance* 61, 1035–1072.

Kisgen, D. and Strahan, P. (2010): Do regulations based on credit ratings affect a firm's cost of capital?, *Review of Financial Studies* 23, 4324–4347.

Lewis-Beck, M. and Stegmaier, M. (2000): Economic determinants of electoral outcomes, *Annual Review of Political Science* 3, 183–220.

Mian, A. and Sufi, A. (2014): What explains the 2007–2009 drop in employment?, *Econometrica* 82, 2197–2223.

Moody's Investors Services. (2002): Moody's US municipal bond rating scale, November.

Moody's Investors Services. (2007): The U.S. municipal bond rating scale: Mapping to the global rating scale and assigning global scale ratings to municipal obligations, March.

Moody's Investors Services. (2009): Moody's rating symbols and definitions, June.

Moody's Investors Services. (2010): Recalibration of Moody's U.S. municipal ratings to its global rating scale, March.

Snyder, J., Jr and Strömberg, D. (2010): Press coverage and political accountability, *Journal of Political Economy* 118, 355–408.

Zingales, L. (2015): Presidential address: does finance benefit society?, *Journal of Finance* 70, 1327–1363.

Downloaded from https://academic.oup.com/rof/article/26/4/937/6604765 by UNL-Nova School of Business and Economics user on 14 October 2022