Can Credit Rating Agencies Affect Election Outcomes?*

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March 15, 2018

Abstract

We show that credit rating agencies can have a significant effect on elections. We identify these effects by exploiting exogenous variation in municipal bond ratings due to Moody's recalibration of its scale in 2010. We find that incumbent politicians in upgraded municipalities experienced higher vote shares and increased likelihood of re-election. The evidence is consistent with a direct effect of ratings on voting beyond the effect of economic conditions. Rating upgrades affect elections by improving voter perceptions of the quality of incumbents. Democratic and Republican incumbents implement similar policies, but Democratic incumbents obtain more electoral rewards from upgrades.

JEL classification: D72, G24, H74
Keywords: Elections, Credit Ratings, Financial Constraints, Municipal Bonds, Government Spending, Economic Conditions

*We thank Manuel Adelino, Laurent Bach, Bo Becker, Taylor Begley, Philip Bond, Sergey Chernenko, Joao 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, 4th 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 Fundacao para a Ciencia e Tecnologia (FCT) is gratefully acknowledged.

1 Introduction

The long-standing debate about the role of credit rating agencies (CRAs) in society has recently received additional attention due to the 2007-2009 global financial crisis and the 2010-2012 European sovereign debt crisis. In 2012, Leonardo Domenici, a Member of the European Parliament, claimed that: "The debt crisis in the Eurozone has shown that credit rating agencies have gained too much influence, to the point of being able to influence the political agenda." The general public also believes that financial institutions and markets have too much power as reported in poll results (e.g., Gallup (2011)). Regulators and academics have expressed similar concerns (Zingales (2015)).[1]

In this paper we ask whether financial markets influence the electoral prospects of incumbent politicians. We examine this question by studying the effects of municipal bond ratings on election outcomes in the United States. Empirically testing the impact of credit ratings on election outcomes is challenging because credit ratings may reflect improvements in economic conditions that are due to the actions of political incumbents. This could lead to a positive correlation between ratings and the electoral prospects of political incumbents, even if ratings have no causal effect on election outcomes. To overcome this identification challenge, we exploit 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 measures 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,

[1]Paradoxically, one of the main reasons for the power of CRAs is rating-based regulations (see, Kisgen and Strahan (2010)). Investment management policies and practices also often depend on ratings in that they restrict the portfolio holdings of institutional investors (e.g., Chen, Lookman, Schurhoff, and Seppi (2014)). In the aftermath of the 2007-2009 financial crisis, several regulatory initiatives were under taken to diminish market participants' mechanical reliance on credit ratings (e.g., 2010 Dodd-Frank Wall Street reform and the Consumer Protection Act, Financial Stability Board (2010, 2012)).

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 (nearly 70,000 bond issues).

According to Moody's Investors Services (2010), the recalibration simply 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, incumbent's actions, and local and nationwide economic conditions. To further validate our exclusion restriction, we conduct a series of tests to verify that indeed no new information was incorporated in Moody's recalibration. First, we study municipalities that have both Moody's and S&P ratings. If the recalibration by Moody's reflected changes in underlying credit quality, the S&P ratings on this sample of municipalities would also be affected. We find no changes in their S&P ratings either before or after the recalibration. Second, we find that house prices of upgraded and non-upgraded municipalities follow similar trends around the recalibration. This helps to rule out the possibility that the 2007-2009 financial crisis and the subsequent recovery may have affected upgraded and non-upgraded municipalities differently.

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 that are due exclusively to changes in municipal bond ratings from other confounding effects. Local governments that were not affected by the recalibration but that experienced similar economic conditions to those of recalibrated local governments can serve as a control group. The control group includes both local governments that were already properly calibrated vis-à-vis the Global Rating Scale and local governments that had no Moody's rating or bonds outstanding.

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 vote shares and winning odds of the incumbent political party in the 2010-2012 elections compared to the 2006-2009 elections at the county level (in the case of gubernatorial and Senate elections) or congressional district level (in the case of U.S. House elections).

The recalibration affected bonds issued by counties, as well as local government units within a county, such as cities, townships, school districts, and special districts (e.g., public utility districts).[2] Thus, we aggregate the changes in ratings to the county or congressional district level. Our (continuous) treatment variable is the fraction of local government units in each county/district whose outstanding bonds were upgraded because of the Moody's recalibration. The regressions also include county/district and state-year fixed effects to capture local economic conditions and any source of unobserved county/district-level heterogeneity.

We find that incumbent party candidates obtained a higher vote share and were more likely to be reelected in upgraded municipalities than in non-upgraded municipalities. The incumbent effect is pervasive across different types of elections. Our results for House elections show that a 10% increase in the fraction of upgraded municipalities (which corresponds to about one standard deviation) in a district is associated with an increase of 2% in vote share. For gubernatorial elections, a 10% increase in the fraction of upgraded municipalities in a county is associated with a 2% increase in vote share. For Senate elections, the corresponding increase in vote share is 1%. We also explore the effects of ratings on mayoral elections in the California and find that having the city bonds upgraded during the recalibration is associated with a 28% increase in vote share. Our results are stronger for elections with more proximity between candidates and populations, which is consistent with voters giving more credit of a local shock to a candidate.

The evidence is consistent with two non-mutually exclusive mechanisms: (1) an improvement in local economic conditions due to relaxation of financial constraints; and (2) a direct effect of ratings on voters' perceptions of the quality of incumbent politicians. Municipal bond markets are an important resource for local governments to finance the construction and main-

[2]We exclude states as they are a higher-level government than counties (i.e., they cannot be attributed to a specific county).

tenance of infrastructure and other public projects. When municipalities face a shock to their credit supply, the quantity and quality of local public goods provision may change, and therefore affect voting behavior. The recalibration generated cross-sectional variation in ratings across local governments, which could affect local governments' financial constraints and debt capacity. Easier and cheaper access to financing can in turn have important effects on local economic conditions, especially when governments face significant financial distress such as during the 2007-2009 Great Recession. Cornaggia, Cornaggia, and Israelsen (2017) 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 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. Our evidence supports the view that government spending and economic conditions play an important role in voting behavior, in particular by increasing the incumbent's chances of winning the election.

In addition, we show that economic voting alone does not explain our findings. We perform a series of tests to investigate the additional mechanisms behind the effect of rating upgrades on the votes received by the incumbent. First, in order to isolate a direct effect of rating on voting behavior, we include as controls in our regressions several economic variables commonly used in the existing literature to explain election outcomes. Our results remain both statistically and economically significant, which indicates that the effects of ratings on elections go beyond the effect of local economic conditions on voting behavior.

Second, we provide further evidence consistent with direct effects of ratings by exploring the timing of the effects on election outcomes. While the direct effects of ratings should affect elections immediately, improvements in local economic conditions due to changes in fiscal policy take time to materialize, and thus will affect elections with a lag. Consistent with a direct effect of ratings, we find a positive and significant effect of ratings on elections in the same year of the recalibration.

Third, we show that municipal bond ratings affect elections through an impact on voters'

perceptions of the incumbent's quality.[3] We study this mechanism by exploring cross-sectional variation (at the state level) in Google searches for the term "credit rating" around election dates. An increase in Google searches for this term would suggest that more people in a state were paying attention to ratings and that their opinion about a candidate's quality could be influenced by the recalibration-related upgrades. We find that the effect of rating upgrades on the electoral prospects of incumbents is stronger in states where there was a surge in ratings-related Google searches.

Finally, we show that the recalibration also affect voters' perception of the incumbent through the voter's holdings of local municipal bonds. Improvements in the valuation of bond holdings might signal to voters that the local government is being better managed, especially during the financial crisis. We test this idea by exploiting a unique feature of the municipal bond market: municipal bonds are exempt from state-level income taxes if the buyer of the bond is a resident of the state. We find that the incumbent effect is more pronounced in states with higher income tax rates, which are plausibly states with higher local ownership of municipal bonds.

To provide a more detailed picture of the political impact of CRAs, we investigate whether the effect of municipal bond ratings on elections differs across political parties. We find that the electoral chances of Democratic incumbents improve significantly more than those of Republican incumbents. The differences in election outcomes, however, do not seem to be driven by differences in the policies implemented by the different parties. Consistent with Ferreira and Gyourko (2009), we do not find significant differences in policy reactions to the rating upgrades. Both Democratic and Republican incumbents benefited from reductions in bond yields, which allowed incumbents to increase municipal bonds issuance and local government spending. This expansionary fiscal policy led to subsequent increases in private employment and income. Our results indicate that both parties implemented similar policies as a reaction to the reduction in municipalities' financial constraints, but the electoral rewards

[3]There is anecdotal evidence that political candidates use credit ratings in their political discourse. For example, Donald Trump and Mike Pence referred to the rating of the State of Indiana bonds (with the maximum attainable rating of Aaa) during the 2016 presidential race.

of these policies depend on the type of voter and voter preferences.

We perform a series of robustness checks to guarantee that our results are not driven by the lack of comparability between treatment and control groups or the definition of the treatment variable. To guarantee that differences in political affiliation or urbanization rate between our treatment and control groups are not driving our results, we consider two different samples: a sample of Democratic counties, and a sample of Democratic urban counties. Our results are robust to these sample variations. Our results are also robust to an alternative definition of our treatment variable: a treated variable weighted by the amount of bonds issued by local governments in a county or congressional district.

Our study contributes to three strands of the literature. First, we provide a novel link between financial markets and elections. There is vast evidence that ratings affect corporate actions (e.g., Kisgen (2006), Kisgen and Strahan (2010), Baghai, Servaes, and Tamayo (2014), Begley (2016), Almeida, Cunha, Ferreira, and Restrepo (2017)). Research also shows that municipal bond ratings affect municipalities' financing and economic condition (Cornaggia, Cornaggia, and Israelsen (2017), Adelino, Cunha, and Ferreira (2017)). To the best of our knowledge, we are the first to provide causal evidence that financial markets, and specifically the actions of credit rating agencies, can influence voting behavior. This suggests that short-term shocks to financial markets in general, and credit ratings in particular, can have long-lasting consequences through their impact on political outcomes.

Second, we contribute to the literature on the effect of economic conditions on elections. There is a long-standing debate about whether voters penalize or reward budget deficits and government spending. Authors have traditionally provided evidence of a negative correlation between government spending and election outcomes (e.g., Niskanen (1975), Peltzman (1993), Matsusaka (2004)). More recent research finds that voters reward government spending (e.g., Levitt and Snyder (1997), Akhmedov and Zhuravskaya (2004), Veiga and Veiga (2007), Sakurai and Menezes-Filho (2008), Jones, Meloni, and Tommasi (2012), Litschig and Morrison (2013)). Bagues and Esteve-Volart (2016) show that exogenous improvements in economic conditions (driven by a cash windfall won in a lottery in Spain) have a positive effect on the

incumbent's vote share. However, voters do not perceive the incumbent as of better quality in regions where lottery prizes are awarded. We provide causal evidence of the effects of government spending and economic conditions on voting behavior. Whereas the literature studies the election effects of cash windfalls, we show that voters reward debt-financed increases in government spending. In addition, we provide evidence of a direct effect of ratings on voting behavior through its impact on voter perceptions of the quality of incumbents.

Finally, we contribute to the literature on the effects of political partisanship on public policies and voting behavior. The literature provides evidence that legislative power is highly partisan (Besley and Case (2003), Lee, Moretti, and Butler (2004)). Recently, Fedaseyeu, Gilje, and Strahan (2015) show that voters become more politically conservative after an economic (shale oil and gas) boom, and Autor, Dorn, Hanson, and Majlesi (2016) show that increased import competition from China has contributed to a shift in congressional voting towards political extremes (i.e., conservative Republican or liberal Democrat). Yet Ferreira and Gyourko (2009) find no evidence of partisan influence on local government policies at the city level. We show that political partisanship does not affect how officeholders react to a reduction in municipalities' financial constraints. While Democratic and Republican politicians implemented similar fiscal policies following the recalibration, our results suggest that Democratic voters react more favorably to a fiscal expansion than Republicans.

2 Methodology and Data

2.1 Recalibration

Moody's had a dual-class rating system until its ratings 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 U.S. 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 a variety of 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 equivalent rating on the global scale. In April and May of 2010, over a four-week period, Moody's described how municipal bond ratings would be affected by the recalibration, resulting in a zero-to-three notch upgrade of nearly 70,000 ratings.

We obtain a list of recalibrated bond issues from Moody's. This list includes the rating of each bond issue before and after the recalibration. 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 56 municipal bonds had a speculative-grade rating).

Since we measure election and economic outcomes at the county/district/city level, we restrict the analysis of the recalibration to bond issues that can be matched to a county/district/city. These include issues by local government units such as counties (including boroughs and parishes), cities, townships (including towns and villages), school districts, and special districts (e.g., public utility districts). We exclude state-level bonds as they cannot be attributed to a specific county/district/city.

We first define the treatment and control groups at the local government unit level (i.e., county, city, townships, school district, and special districts). The treatment group includes local government units whose outstanding bonds were upgraded by at least one notch during the recalibration event. Since our tests are at the county or congressional district level, we then calculate our treatment (continuous) variable as the fraction of all local government units in a

given county or district that were upgraded during the Moody's recalibration (*Recalibrated*). In the case of mayoral elections, *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise.

Figure 1 shows a map of the United States with the terciles of the treatment variable (*Recalibrated*), among those counties with non-zero value. There is a variation both in the intensity of the treatment variable and the location of counties in the treatment group across the United States.

An important aspect of this recalibration is that not all municipal bond issues were upgraded in the recalibration, and therefore can be used in the control group. Some local governments were already "properly calibrated" (Moody's (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.

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 credit quality of an issuer: "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 ratings 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."

Figure 2 shows the evolution of ratings around the recalibration separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel A shows the evolution of Moody's ratings. The figure shows no differential changes before the recalibration.

The treatment group relative to the control group reveals a sharp increase in Moody's ratings after 2010, a difference that persists for up to three years. To validate our exclusion restriction, we study new bond issues that have both Moody's and S&P ratings. Panel B of Figure 2 shows S&P ratings for the treatment and control groups around the recalibration. We do not see any differential changes in the S&P ratings 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 provides evidence that Moody's recalibration does not reflect a change in issuers' credit quality, which is an important validation of our identification strategy. Figure 3 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.

We also compare changes in house price 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. Figure 4 shows no significant differential trends in HPI of treatment and control groups (at the county level) before or after the ratings recalibration. Thus, there is no evidence that our results are driven by differential effects on treatment and control groups of the 2007-2009 financial crisis and subsequent recovery.

2.2 Election Outcomes

We obtain voting data for the U.S. House of Representatives at the congressional district level, and gubernatorial and U.S. Senate elections at the county level for the 2004-2012 period from David Leip's website.[4] These data have been used in previous research (e.g., Gentzkow, Shapiro, and Sinkinson (2011)). The data include information on total numbers of votes by political party or candidate. There are no readily accessible data on mayoral elections across different states. To observe the impact of municipal bond ratings on local election outcomes, we collect mayoral election data for California for the 2006-2012 period from the California

[4]The data are available at: http://uselectionatlas.org.

Elections data archive.[5]

Elections for the House and the Senate are held on the Tuesday immediately following the first Monday in November. House and Senate elections take place every two years in even-numbered years. Many other state and local government officials are also elected on the same day for convenience and cost saving reasons.

In the case of the House, elections are at the congressional district level. Congressional districts are electoral constituencies that elect a Member of Congress, who each serve two-year terms. There is considerable variation in the number of congressional districts by state, as some states have many congressional districts, while others have only one.[6] Senators are elected at the state level and serve six-year terms. The terms are staggered so that approximately one-third of Senate seats are up for election every two years. For House elections, the 2006 and 2008 elections are included in the pre-treatment period, and the 2010 and 2012 elections are included in the post-treatment period.[7] For Senate elections, the 2004, 2006, and 2008 elections are included in the pre-treatment period, and the 2010 and 2012 elections are included in the post-treatment period.

Elections for governors and mayors do not occur in even years only. Governors are elected by states and serve four-year terms (with the exception of Vermont and New Hampshire where terms are two years long). Mayors are elected by cities and serve four-year terms. For gubernatorial and mayoral elections, the pre-treatment period is 2006-2009, and the post-treatment period is 2010-2012.

For each election, we start by identifying the incumbent party as the party that won the previous election in each constituency. We then create the incumbent party vote share (*Incumbent Share*), defined as the number of votes that the incumbent party received divided by the total number of votes in the county (for gubernatorial and Senate elections), congressional district (for House elections), and city (for mayoral elections). As an additional way to test

[5]The data are available at: http://www.csus.edu/isr/reports/california_elections/.

[6]For example, California has 53 congressional districts. Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming have one congressional district each.

[7]Given that the 2010 elections took place on the 2nd of November, 2010 (6 months after the recalibration) is included in the post-recalibration period.

whether ratings affect election outcomes, we create a dummy variable (*Incumbent Win*) that takes a value of one if the incumbent party candidate is reelected in the case of House or mayoral elections, and zero otherwise. In the case of gubernatorial and Senate elections, *Incumbent Win* is a dummy variable that takes a value of one if the incumbent party candidate wins the most votes in a county, and zero otherwise.

We then merge the elections data with the recalibration data to obtain our measure of the degree to which rating upgrades affected incumbent politicians electoral prospects in a given region. In the case of the Gubernatorial, and Senate elections, we directly match the election and the *Recalibrated* variable at the county level. In the case of the House elections, we conduct the analysis at the congressional district level. There is no one-to-one mapping between counties and congressional districts.[8] We match each district to the corresponding counties using a bridge provided by the U.S. Census Bureau.[9] If a district encompasses more than one county, we take the average of the counties that are part of the corresponding district. If a county encompasses multiple districts, all districts within the county are assigned the same value of the *Recalibrated* variable. In the case of mayoral elections, we define *Recalibrated* as a dummy variable that takes the value of one if the city was upgraded during the recalibration, and zero otherwise.

Table 1 presents summary statistics of treatment and control groups for election outcomes and number of votes (in hundreds of thousand) in the pre-recalibration period by election type: House (Panel A), gubernatorial (Panel B), Senate (Panel C), and mayoral (Panel D). The treatment group includes counties/districts/cities with above-median *Recalibrated*, and the control group includes counties/districts/cities with below-median *Recalibrated*. In the case of mayoral elections in Panel D, *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise. In the case of gubernatorial, Senate, and mayoral elections, the median of the *Recalibrated* variable is zero. Columns (7) and (8) show the differences between the two groups in the pre-recalibration period. One feature of

[8]For example, the 53 congressional districts in California are associated with 58 counties.

[9]This bridge can be obtained at the following website: https://www.census.gov/geo/maps-data/data/cd_state.html.

the data is that counties in the treatment group are larger than counties in the control group in terms of voting population. We present both raw differences in means between treatment and control groups, as well as differences after adjusting for size (number of votes in the congressional district, county, or city) and state-by-year fixed effects; these controls are also included in our regression specifications. Although the raw differences between treatment and control groups prior to the recalibration show some statistical significance (column (7)), these differences lose statistical as well as economic significance when we control for size and state-by-year fixed effects (column (8)). This indicates that treatment and control groups are comparable in the pre-recalibration period.

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 through March 2013, which corresponds to the three-year period before Moody's recalibration and the three-year period afterward. We restrict the sample to new bond issues rated by Moody's and to local government units that issued bonds during the three-year period before the recalibration.[10] 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 create the variable *Issue Amount*, defined as the total amount of bonds issued by local governments in each county and year. We also create the variable *Offer Yield*, defined as the average of offer yields (in percentage) of new bond issues in each county and year.

2.4 Economic Outcomes

The primary economic outcomes we study are local government expenditures, taxes, government employment, private employment, and income. We obtain data on government revenues

[10] 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.

and expenditures from the U.S. Census Bureau's Annual Survey of State and Local Government Finances. The data include revenues and expenditures of individual local government units within each county. The sample includes local government units that are present in all years of the sample period, and covers more than 90% of the counties in the United States. The variable *Local Government Expenditures* is defined as the total expenditures of all local governments in each county and year. The variable *Local Tax Rate* is defined as the taxes charged by all local governments divided by income in each county and year.

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 five years (e.g., 2002, 2007, 2012), and uses a sample of local governments in the other years. The variable *Local Government Employment* is defined as full-time equivalent employees at local government units in each county as of the week of March 12 of each year. The analysis of local government employment is restricted to local government units that are present in all years of the sample period (2007-2013).[11]

We obtain 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. We obtain county-level income data from the Internal Revenue Service (IRS) Statistics of Income. The variable *Private Employment* is defined as the number of employees in each county and year. We separately track non-tradable employment (retail, food and accommodation; NAICS codes 4445 and 72) and construction employment. We also create the variable *Unemployment Rate*, defined as the ratio of unemployed workers to total labor force in each county and year from the the Bureau of Labor Statistics. We obtain county-level income data from the Internal Revenue Service (IRS) Statistics of Income. Income is defined as total wages and salaries in a given county and calendar year (the sample period for income is 2006-2012). When we analyze private sector employment or income, we use the full CBP or IRS data (i.e., we include all counties).[12]

[11]The sample includes only counties that have at least one government unit that is present in all years. The resulting sample of counties with government employment data includes 1,618 counties, or about half of the counties in the United States.

[12]The number of counties included in each regression varies according to the availability of sector-level

In the economic outcomes regressions, we control for other factors that are important determinants of local economic conditions. We include yearly changes in house prices, to capture the severity of the post-2006 downturn in each county, and the number of households. The housing prices come from the Federal Housing Finance Agency's (FHFA's) House Price Index (HPI) data at the Metropolitan Statistical Area (MSA) level.[13] We obtain county-level information on the *Number of Households*, defined as one or more people who occupy a given housing unit, from the 2007 Census Bureau Summary Files.

Table 2 provides a comparison of economic outcomes between treatment and control groups in the pre-recalibration period. Consistent with Table 1, 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 in the pre-recalibration period, as well as differences after adjusting for size (using the *Number of Households*) and state-by-year fixed effects. After adjustment for size and regional heterogeneity in a given year between treatment and control group, the differences in levels of economic variables are no longer positive and statistically significant. Additionally, the growth rates of outcome variables in the pre-treatment period are similar across the two groups, except for *Local Government Expenditures* (although the difference is economically small). Importantly, the treatment variable (*Recalibrated*) is not affected by this adjustment, which indicates that differences in size do not seem to be influencing the treatment selection. We conclude that pre-existing differential trends between treatment and control groups are unlikely to explain our results.

employment-by-county data in the CBP. The Census Bureau often omits observations, or includes only broad ranges for confidentiality reasons.

[13]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.

3 The Effect of Credit Ratings on Elections

To study the impact of credit ratings on election outcomes, we estimate (reduced form) regression models that use rating upgrades due to Moody's recalibration of its Municipal Rating Scale as a source of exogenous variation in municipal bond ratings. We start the analysis by studying the impact on the vote share and on the likelihood that the incumbent party candidate wins the House (at the congressional district level), gubernatorial (at the county level), Senate (at the county level), and mayoral elections (at the city level) using the following regressions model:

$$Y_{it} = \beta_1 Recalibrated_i \times Post_t + \beta_2 X_{it} + \alpha_i + \gamma_{state,t} + \varepsilon_{it}, \quad (1)$$

where Y is the logarithm of the number of votes that the incumbent received divided by the total number of votes (*Incumbent Share*) or a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*). *Recalibrated* is the fraction of upgraded local governments in a county/district or, in the case of mayoral elections, a dummy variable that takes the value of one if the city was upgraded; and *Post* is a dummy variable that takes a value of one after the recalibration in April-May 2010, and zero before the recalibration. To account for any time-invariant unobserved heterogeneity at the county/district/city level, the regressions include election-level (county, district, or city) fixed effects (α_i) in all specifications. We also include state-by-year fixed effects ($\gamma_{state,t}$), which absorbs all shocks that are common to regions within each state and year (i.e., only within-state and year variation is used for identification). The interaction term *Recalibrated* $\times$ *Post* is the difference-in-differences estimate of the effect of bond ratings on election outcomes. Specifically, we estimate the change in election outcomes within a county/district/city with upgraded municipal bonds as compared to the change in a non-upgraded county/district/city in the same state and year.

To control for constituency size, the regressions include the total number of votes cast in a county/district/city (*Number of Votes*) as a control variable. We also include the lag of the incumbent vote share (*Incumbent Share*$_{t-1}$) as a control to take into account the possibility

that parties that had a high vote share in the past election are more likely to experience a high vote share in the current election (incumbent effect). In addition, the regressions are weighted using the number of votes to account for the possibility that size could be correlated with voting behaviour around 2010, and to guarantee that our results are not driven by a few small (in terms of population) counties, districts or cities, and are instead representative of the entire population. Standard errors are clustered at the election level (county, district or city).

Table 3 presents the results for House, gubernatorial, Senate, and mayoral elections. The dependent variable is the logarithm of the *Incumbent Share* in columns (1), (3), (5), and (7), and the *Incumbent Win* dummy variable (i.e., estimates of a linear probability model) in columns (2), (4), (6), and (8). Columns (1) and (2) examine the effect of rating upgrades on House elections using the regression in equation (1) at the congressional district level. The *Recalibrated* variable is the fraction of upgraded local governments in each district. In column (1), the interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant, which indicates that the recalibration had a differential effect on the incumbent vote share of the treatment group relative to the control group. Similarly, in column (2), we find that congressional districts with more upgraded local governments experienced an increased probability of *Incumbent Win*, relative to districts with fewer local governments upgraded during the recalibration. The estimates imply that a 10% increase in the fraction of local governments upgraded in a district increases the vote share by 2% and the probability of incumbent reelection by 4%.

Figure 5 shows the evolution of the difference in the likelihood of an incumbent's party win around the recalibration between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) in House elections. The two groups followed similar trends before the recalibration, and we observe a significant differential effect between treatment and control groups after the recalibration.

In columns (3) and (4), we estimate the effect of rating upgrades on gubernatorial elections using the regression in equation (1) at the county level (the *Recalibrated* variable is now the

fraction of upgraded local governments in each county). The interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant in column (3), which indicates that governors affiliated with the incumbent party saw an increase in their vote share post-recalibration, relative to the vote share pre-recalibration. A 10% increase in the *Recalibrated* variable in a county increases the incumbent vote share by 2%. In column (4), we test whether the recalibration also affects the probability of *Incumbent Win* and find a positive but statistically insignificant coefficient.

Figure 6 shows the evolution of the difference in the likelihood of an incumbent's party win around the recalibration between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) in gubernatorial elections. The two groups followed similar trends before the recalibration, and we observe a significant differential effect between treatment and control groups after the recalibration.

In columns (5) and (6), we examine the effect of rating upgrades on Senate elections using the regression in equation (1) at the county level. The estimate in column (5) implies that a 10% increase in the *Recalibrated* variable in a county leads to an increase of 1% in the incumbent party vote share. At these higher-level (more distant) elections not only the effect of the recalibration on vote share is economic smaller than in the case of House and Governor, we also do not find an effect on the probability of *Incumbent Win*. This suggests that voters are more likely to reward politicians that they perceived as having a more direct responsibility for rating upgrades and local economic conditions.

Figure 7 shows the evolution of the difference in the likelihood of an incumbent's party win around the recalibration between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) in Senate elections. The figure shows that, if anything, the probability of reelection was decreasing for the treatment group relative to the control group before the recalibration. We then see a significant higher *Incumbent Win* for the treatment group than for the control group after the recalibration.

Finally, in columns (7) and (8), we study the effect of rating upgrades on mayoral elections in California. In these two columns, in which the unit of observation is a city-year, the

variable *Recalibrated* is a dummy that takes the value of one if the city was upgraded during the recalibration, and zero otherwise. The estimate in column (7) implies that the vote share of incumbents increased by roughly 28% in recalibrated cities, relative to the control group. In column (8), we find that incumbent politicians in recalibrated cities experience an increase in the probability of reelection of 8%. Due to the small sample size, the coefficients are imprecisely estimated. In addition, we do not have enough variation to explore the mechanisms that drive our results in mayoral elections.

Overall, we show that credit rating agency actions affect election outcomes. We find that candidates affiliated with political parties that were in power at the time of Moody's recalibration had a higher vote share and probability of reelection. Our results suggest that incumbents are rewarded for positive news (exogenous rating upgrades due to the recalibration in our experiment) even if the news is beyond their control.

4 Mechanisms: How Do Credit Ratings Affect Elections?

There are several ways through which municipal bond ratings can affect election outcomes. 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. In addition, rating upgrades may have a direct effect on voting behavior. In the context of asymmetric information, voters may interpret an exogenous rating upgrade event (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 perceptions of the quality of the incumbent. If a higher bond rating is associated with responsible budgeting practices and good economic policies, a rating upgrade could lead to a change in voting behavior even in the absence of any real change in the policies or in economic conditions.

4.1 Fiscal Policy and Local Economic Conditions

The recalibration is associated with economically large and statistically significant effects on the local economy. Cornaggia, Cornaggia, and Israelsen (2017) 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 the reduced borrowing costs allowed local governments to increase the amount of bonds issued. The proceeds of the increase in debt financing was used to increase local governments expenditures and employment, and to reduce taxes. The increase in local government expenditures had positive spillovers to the private sector as counties with more upgraded local governments experienced an increase in 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 (2017) in Table IA.1 in the Internet 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, which translated into more votes for the incumbent candidate.

Next, we explore whether the ratings have a direct effect on voting behavior (i.e., beyond the existence of voting driven by economic fundamentals). Table 4 shows that the effect of rating upgrades on election outcomes are robust to the inclusion of several economic variables as controls. The economic variables are *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income* at the county/district level. We also include the *Offer Yield* at the county/district level (average offer yield across all issues of local governments in each county/district) as a control variable. The *Offer Yield* controls for other sources of improvements in the credit quality of the local government, which can improve its access to financial markets beyond the recalibration.[14] The estimates of the the interaction term *Recalibrated* $\times$ *Post* are similar to those in Table 3. Overall, we find the effect of credit ratings

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

on election outcomes remains economically and statistically significant when we include a battery of controls for local economic conditions (in addition to the state-year fixed effects) in the regressions.

We also investigate the timing of the effect of rating upgrades on elections. A direct effect of ratings on elections would occur shortly after the recalibration, as voter's perceptions would be altered by the recalibration event itself. In contrast, an effect through fiscal policy would occur with a lag because improvements in local economic conditions take time to materialize.

Table 5 presents the effects of rating upgrades on elections by year. The coefficients of interest are the interactions between the *Recalibrated* variable and the 2010, 2011, and 2012 calendar-year dummies. This table allows us to observe how much of the effect is incorporated according to the timing of the election. Consistent with a direct effect of ratings, we find that ratings start affecting election outcomes in November 2010 (the year of the recalibration) for House, gubernatorial, and Senate elections. There is also evidence of effects taking place with a lag in 2012, which is consistent with improvements in local economic conditions affecting voting behavior. The results in Tables 4 and 5 indicate that the effects of the recalibration go beyond improvements in local economic conditions. Next, we provide evidence of mechanisms through which ratings can have a direct effect on election outcomes.

4.2 Voter Perceptions

4.2.1 Political Discourse Effect

Anecdotal evidence suggests that credit ratings are used in the political discourse as a way to persuade voters of the economic acumen of candidates. 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 political 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

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, prior to Mike Pence's election as governor in 2012, which suggests that politicians may be tempted to tout high credit ratings, even if they had nothing to do with the upgrade.[15]

To test whether municipal bond rating upgrades affect the public perception about the quality of politicians, we collect data from Google Trends on the evolution of news searches for the term "credit rating" by state in the 2006-2012 period. We focus on the months May to November when the searches might most likely be related to political campaigns. Because the term "credit ratings" is not a very popular search term, there are several states with zero searches in all years of the sample. We therefore focus on the eleven states that have non-zero searches in at least one year of our sample.[16] We create the *Rating News* variable, defined as the increase in news searches for the term "credit rating" from before the recalibration (2006-2009) to after the recalibration (2010-2012). States with an increase in searches for ratings are more likely to be those where voters pay closer attention to ratings as indicative of the quality of politicians.

We test whether the impact of the recalibration on elections is stronger in regions where news searches related to ratings have a larger increase. Table 6 presents the results for House, gubernatorial, and Senate elections. The explanatory variable of interest is the triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Rating News*, which measures the effect of ratings on election outcomes in states with high news searches versus states with low news searches. For House elections the triple interaction coefficients are positive but statistically insignificant, and for Senate elections the coefficient is positive and significant for *Incumbent Share*. The interaction term coefficients are positive, statistically significant for *Incumbent Share*, and

[15]There are other examples of this pattern. 2012 Ohio Senate candidate Josh Mandel was accused of falsely claiming that Ohio's ratings improved while he was the treasurer; Paul LePage, mayor of Waterville, ME (now 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.

[16]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.

economically large in the case of gubernatorial elections, probably because the perception effect is stronger for executive than for legislative offices.[17]

We perform two robustness exercises to guarantee that voters are in fact searching for the term "credit rating" and not for terms that reflect poor economic conditions at the time of the 2007-2009 financial crisis. We repeat our tests using a *Crisis News* variable based on searches for the term "financial crisis" instead of the term "credit ratings." We also perform a similar test using the term "credit score" to rule out the possibility that people might be searching for their own credit score. Table IA.2 in the Internet Appendix shows the results. Incumbents in states in which people were searching for these alternative terms do not benefit more from the recalibration. These results help us alleviate concerns that our results are driven by measurement errors in public information or are instead proxying for local economic conditions.[18]

4.2.2 Municipal Bond Holdings Effect

Voters may also learn about the candidate's quality by following the municipal bond market. Increases in the value of their bond holdings, might signal to voters that the local government is being well-managed. This could be especially important during the financial crisis. In addition, investors holding upgraded municipal bonds experienced an increase in the value of their portfolio at the time of the recalibration in 2010, which translates into an increase in their overall wealth. Therefore, we expect the effects to be stronger in places with higher ownership of municipal bonds.

We test this idea by exploring a unique feature of the municipal bond market. Municipal bonds are exempt from state income taxes if the buyer of the bond is a resident of the respective state. This creates strong incentives for local ownership of municipal bonds in

[17] An alternative potential mechanism for our results is that the upgrades reveal new information about the power of incumbent politicians, but this mechanism is unlikely to explain our results, for several reasons. The upgrades were identical within issuer type and pre-recalibration rating level. In addition, since our regressions exploit variation within state and year, we are holding fixed the same set of candidates across counties in the case of gubernatorial and Senate elections. Therefore, our results cannot be explained by cross-sectional differences in incumbent power.

[18] We also study whether education plays a role on voters' attribution. Table IA.3 in the Internet Appendix shows no evidence that education (at the county level) plays a role in voters' response to the recalibration.

states with high income tax rates (e.g., California), while the incentives are weaker in states with low income taxes (e.g., Florida). Thus, we use state income taxes as a proxy for the extent of the local wealth effects associated with Moody's recalibration. We test whether the incumbent effect is stronger in states with higher income taxes, and presumably with higher holdings of local municipal bonds, than it is in states with lower income taxes.

Table 7 presents the results. Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) present county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. The coefficient of interest is the one associated with the triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Income Tax*, where *Income Tax* is the average state income tax rate in 2010. The coefficient on the triple interaction measures the effect on election outcomes in states with higher income taxes. We find that the effects of ratings upgrades on elections are more pronounced in states with higher income taxes. The effects are economically important for House elections but the triple interaction coefficient is statistically significant only for *Incumbent Share*. The estimates are positive and significant for *Incumbent Share* in the case of gubernatorial elections and for *Incumbent Win* in the case of Senate elections.[19] Overall, these results suggest that one mechanism through which municipal rating upgrades may affect elections is its use as a political weapon to influence voter perceptions of the quality of incumbents.

5 Political Parties

The results so far show a positive impact of rating upgrades on the electoral prospects of incumbent politicians. However, this average effect may be heterogeneous, as different political preferences can ultimately lead to a differential effect of credit ratings on elections. In this

[19]There is a potential concern that these results are driven by Democratic states since they typically have higher income tax rates. However, Democratic states cannot explain these results because the regressions include state-by-year fixed effects, which would absorb Democratic-by-year fixed effects in the case of gubernatorial and Senate elections. In the case of House elections, these two types of fixed effects are similar and yield similar results.

section we ask the question: does political partisanship shape the effect of municipal bond ratings on elections? To answer this question we investigate whether there are differences in election outcomes between Democratic and Republican incumbents following Moody's recalibration in 2010, and whether electoral responses are determined by differences in the policies enacted by the two parties.

5.1 Political Parties and Election Outcomes

We estimate regressions that allow for a differential effect of rating upgrades on election outcomes among Democratic and Republican incumbent candidates. The regressions include a triple interaction term *Recalibrated* × *Post* × *Democrat*, where *Democrat* is a dummy variable that takes a value of one if the Democratic party was in power in the respective constituency (based on the last election prior to the recalibration), and zero otherwise. The coefficient on the interaction term tests whether the effect of ratings on election outcomes differs between Democratic and Republican incumbents.

Table 8 presents the estimates. Columns (1) and (2) present congressional district-level estimates for House elections. Columns (3) and (4) present county-level estimates for gubernatorial elections. Columns (5) and (6) present county-level estimates for Senate elections. Our results suggest that incumbent Democratic candidates benefit more from the recalibration-related upgrades than incumbent Republican candidates, as Democratic incumbents in upgraded municipalities experience a larger increase in their vote share and chance of winning.

5.2 Political Parties and Economic Outcomes

We also examine whether the differences in the effects of rating upgrades on the election outcomes of Democratic counties relative to Republican counties are due to differences in policies implemented by different parties. In these regressions, because we cannot determine the party of the issuer or the party in power, we classify a county as Democratic if the Democratic presidential candidate was the most voted candidate in that county in both the 2008 and the 2012 elections. Similarly, we classify a county as Republican if the Republican

presidential candidate was the most voted candidates in that county in both the 2008 and the 2012 elections. A county that switches between the Democratic and Republican candidates (and vice-versa) as the most voted party in the 2008 and 2012 presidential elections is excluded from the sample for this analysis. Counties in which neither the Republican or Democratic parties are the most voted party in 2008 or 2012 are also excluded.

First, we study whether political partisanship affects the amount of bonds issued (*Issue Amount*) and the average offer yield (*Offer Yield*) after the recalibration. We compare the effects of the recalibration on the *Issue Amount* and *Offer Yield* in Democratic counties relative to Republican counties using the interaction term *Recalibrated* $\times$ *Post* $\times$ *Democrat*. Columns (1) and (2) of Table 9 present the results. We do not find any statistically significant difference between Democratic and Republican counties in the access to municipal bond markets although Democratic counties seem to experience lower borrowing costs and issue more bonds.

Second, we study whether partisanship is associated with differences in fiscal policy following the recalibration. Columns (3)-(5) examine the differences in the reactions of Democratic and Republican counties in their *Local Government Expenditures*, *Local Tax Rate*, and *Local Government Employment*. We do not find any statistically significant differences between the reactions of Democratic and Republican politicians in terms of the local fiscal policies they implement, which is consistent with the evidence in Ferreira and Gyourko (2009).

Finally, we analyze whether the similar fiscal policies implemented by these parties may have led to different spillovers to the private sector. Columns (6)-(9) present estimates of the differences among parties in terms of the effect of the recalibration on *Private Employment*, *Non-Tradable Employment*, *Construction Employment*, and *Income*. We observe a difference only in *Construction Employment*, as rating upgrades are associated with a significant increase only in Democratic counties. We do not find any statistically significant differences between Democratic and Republican counties in terms of the effects on *Private Employment* or *Income*.

Overall, our results suggest that there are no significant differences between the policies implemented by Democratic and Republican politicians. However, Democratic voters seem to

respond more positively to increases in government spending and to improvements in economic conditions than Republican voters. This result might shed light on the long-standing debate on whether voters punish or reward debt-financed increases in government spending. The mixed results in the literature may be driven by differences in voters' taste according to their political affiliation.

6 Robustness

We perform several robustness checks to ensure that our results are not driven by the lack of comparability between treatment and control groups. We estimate the election outcomes regressions in Table 3 using two alternative samples. Table 10 presents the estimates. In Panel A, the sample is restricted to counties or congressional districts in which the Democratic party is the incumbent party in 2010. In Panel B, the sample is restricted to counties and congressional districts in which the share of urban population in 2010 is above 50%, and the Democratic party is the incumbent party in 2010. Despite the reduction in sample size, the estimates remain statistically and economically significant. We conclude that differences in political affiliation or differences in urbanization rates between treatment and control groups cannot explain our results.

In addition, Table IA.4 in the Internet Appendix presents the results of regressions similar to those in Table 3, but with observations equally weighted. Table IA.5 in the Internet Appendix presents the results using a different definition of the treatment variable. Specifically, we replace the *Recalibrated* variable with a new treatment variable in which upgraded local governments within a county/district are weighted by the amount of bonds issued. The results using these alternative samples and treatment variable are similar to those in Table 3.

7 Conclusion

We study the effects of financial markets on election outcomes. We explore exogenous variation in credit ratings due to Moody's recalibration of its U.S. municipal bond ratings scale

in 2010. The recalibration generated cross-sectional variation in ratings across local governments, resulting in a zero-to-three notch upgrades of municipal bonds. We find significant electoral rewards to incumbent candidates of upgraded municipalities versus non-upgraded municipalities. Local governments take advantage of the reduction in financial constraints and lower borrowing costs in the municipal bond market after the recalibration by increasing bond financing and spending. This increase in local government spending leads to an improvement in economic conditions, which enhances the incumbent's electoral prospects.

The recalibration-related upgrades also have a direct effect on voting behavior. Indeed, we show that rating upgrades affect elections even after controlling for local economic conditions. The effect of rating upgrades is more pronounced when voters pay more attention to credit rating news and local ownership of municipal bonds is higher. These findings suggest that incumbent politicians are rewarded at the polls when positive shocks benefit their constituents, even if the shock is outside of 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 simply not have the ability to make such judgments.

Our results highlight the influence of credit rating agencies (CRAs) beyond credit markets. The findings suggest that CRAs may have an outsize power, as they can affect the outcomes of elections and therefore alter public policy choices. However, there is also a bright side. Democracy is an imperfect form of market competition, as it is typically difficult to oust a politician during his or her term for taking actions that favor their own interests at the expense of society at large. CRAs can help solve this problem by acting as a disciplining force that limits the actions of politicians of ill will. Regulators should be aware that financial markets can affect the political process when shaping the architecture of the financial system.

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Table 1: Summary Statistics of Election Outcomes

This table presents pre-recalibration mean, standard deviation, and number of observations for each variable for treatment and control groups. In Panels A-C, *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. In Panel D, *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise. The treatment group includes counties/districts/cities with above-median *Recalibrated*. The control group includes counties/districts/cities with below median *Recalibrated*. The pre-treatment period is from 2006 to 2009 for House elections, gubernatorial elections, and California mayoral elections, and from 2004 to 2008 for Senate elections. Column (7) presents raw differences between treatment and control groups. Column (8) presents difference between treatment and control groups adjusted by state-year fixed effects and number of votes. *p*-values clustered at the congressional district level (for House elections), county level (for gubernatorial and Senate 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: House Elections (congressional district level)								
Incumbent Share	0.684	0.139	411	0.643	0.127	410	0.040***	0.010
							(0.001)	(0.329)
Incumbent Win	0.939	0.239	411	0.907	0.290	410	0.032*	0.020
							(0.090)	(0.348)
Number of Votes (hundreds of thousand)	2.279	0.738	411	2.419	0.697	410	-0.140***	
							(0.006)	
Panel B: Gubernatorial Elections (county level)								
Incumbent Share	0.510	0.141	932	0.518	0.160	2,141	-0.009	0.014***
							(0.132)	(0.004)
Incumbent Win	0.621	0.485	932	0.631	0.483	2,141	-0.009	0.050***
							(0.626)	(0.008)
Number of Votes (hundreds of thousand)	0.635	1.022	932	0.100	0.183	2,141	0.535***	
							(0.000)	
Panel C: Senate Elections (county level)								
Incumbent Share	0.566	0.139	1,917	0.590	0.157	4,283	-0.023***	-0.008**
							(0.000)	(0.033)
Incumbent Win	0.746	0.435	1,917	0.749	0.434	4,283	-0.003	0.012
							(0.840)	(0.359)
Number of Votes (hundreds of thousand)	0.842	1.566	1,917	0.116	0.227	4,283	0.726***	
							(0.000)	
Panel D: Mayoral Elections (city level)								
Incumbent Share	0.632	0.197	34	0.686	0.236	119	-0.053	-0.034
							(0.181)	(0.427)
Incumbent Win	0.824	0.387	34	0.849	0.360	119	0.106	-0.008
							(0.119)	(0.916)
Number of Votes (hundreds of thousand)	0.399	0.611	34	0.145	0.254	119	0.254***	
							(0.000)	

Table 2: Summary Statistics of Economic Outcomes

This table presents pre-recalibration mean, standard deviation, and number of observations for each variable for treatment and control groups. *Recalibrated* is the fraction of upgraded local government units in each county. The treatment group includes counties with above-median *Recalibrated*. The control group includes counties with below median *Recalibrated*. The pre-treatment period is from 2006 to 2009. Column (7) presents raw differences between treatment and control groups. Column (8) presents difference between treatment and control groups adjusted by state-year fixed effects and number of households. *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 (%)	3.084	1.698	2,148	2.956	2.118	1,053	0.128	0.333***
							(0.106)	(0.001)
Issue Amount ($ million)	161.128	542.219	2,148	76.356	917.730	1,053	84.772*	-221.618
							(0.096)	(0.119)
Local Government Expenditures ($ million)	1,169.1	3,302.5	2,829	151.1	2,188.7	6,057	1,018.0***	-424.7
							(0.000)	(0.191)
Local Tax Rate (%)	5.599	5.199	2,844	4.761	6.379	6,054	0.838***	1.779***
							(0.000)	(0.000)
Local Government Employment (thousand)	8.127	20.147	2,297	1.606	15.095	2,547	6.521***	-5.368**
							(0.000)	(0.035)
Private Employment (thousand)	97.252	220.873	2,874	10.742	51.743	6,403	86.510***	-15.937**
							(0.000)	(0.041)
Unemployment Rate (%)	6.459	2.738	2,891	6.659	3.153	6,472	-0.200**	-0.576***
							(0.012)	(0.000)
Income ($ million)	4,442.7	9,582.7	2,868	490.1	1,901.4	6,475	3,952.5***	-524.1*
							(0.000)	(0.062)
Growth Local Government Expenditures	0.054	0.094	1,886	0.045	0.123	4,038	0.009***	0.009**
							(0.000)	(0.016)
Growth Local Government Employment	0.011	0.091	1,530	0.006	0.175	1,695	0.005	0.011*
							(0.288)	(0.059)
Growth Private Employment	-0.026	0.049	1,917	-0.028	0.085	4,237	0.002	0.000
							(0.198)	(0.820)
Growth Income	-0.008	0.041	1,912	-0.009	0.111	4,314	0.001	0.003*
							(0.434)	(0.072)
Recalibrated	0.104	0.115	2,868	0.000	0.000	6,475	0.104***	0.098***
							(0.000)	(0.000)
Number of Households (thousands)	82.119	171.320	2,868	12.479	36.105	6,475		

Table 3: The Effect of Municipal Bond Ratings on Election Outcomes

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. Columns (7) and (8) present city-level estimates for mayoral elections in California in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). In columns (7) and (8), *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Regressions include *Incumbent Share*$_{t-1}$ and *Number of Votes* as controls. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)), county level (in columns (3)-(6)), and city level (in columns (7)-(8)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections		Mayoral Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recalibrated × *Post*	0.203**	0.404**	0.206**	0.329	0.098**	-0.111	0.278*	0.083
	(0.094)	(0.164)	(0.097)	(0.214)	(0.041)	(0.205)	(0.166)	(0.265)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes	No	No
City Fixed Effects	No	No	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.289	0.193	0.847	0.558	0.890	0.546	0.874	0.709
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429	266	266

Table 4: The Effect of Municipal Bond Ratings on Election Outcomes Controlling for Economic Conditions

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. Columns (7) and (8) present city-level estimates for mayoral elections in California in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). In columns (7)-(8), *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, *Income*, and *Offer Yield*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)), county level (in columns (3)-(6)), and city level (in columns (7)-(8)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections		Mayoral Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recalibrated × *Post*	0.184*	0.311*	0.205*	0.450*	0.094*	-0.370	0.301**	0.181
	(0.098)	(0.181)	(0.107)	(0.271)	(0.051)	(0.305)	(0.144)	(0.201)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes	No	No
City Fixed Effects	No	No	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.299	0.195	0.880	0.618	0.928	0.579	0.888	0.752
Number of observations	1,586	1,586	1,667	1,667	2,790	2,790	248	248

Table 5: The Effect of Municipal Bond Ratings on Election Outcomes by Year

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *2010*, *2011* and *2012* are calendar year dummy variables that take the value of one in the years 2010, 2011 and 2012, and zero otherwise. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *2010*	0.061	0.379*	0.254**	0.495**	0.093**	-0.039
	(0.095)	(0.210)	(0.110)	(0.226)	(0.047)	(0.173)
Recalibrated × *2011*			-0.238	0.410**		
			(0.149)	(0.176)		
Recalibrated × *2012*	0.365***	0.433*	0.380	-0.719	0.101	-0.175
	(0.131)	(0.228)	(0.263)	(0.659)	(0.068)	(0.324)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.292	0.193	0.848	0.560	0.890	0.546
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429

Table 6: The Effect of Rating News Searches

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Rating News* is the increase in news searches for the term "credit rating" between the 2010-2012 period and the period before 2010. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	0.230*	0.497**	0.256	0.730***	0.116**	-0.349
	(0.122)	(0.196)	(0.156)	(0.275)	(0.051)	(0.280)
Recalibrated × *Post* × *Rating News*	0.010	0.016	0.033**	0.034	0.021***	0.017
	(0.013)	(0.017)	(0.015)	(0.036)	(0.007)	(0.029)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.280	0.169	0.763	0.532	0.922	0.550
Number of observations	802	802	1,328	1,328	2,390	2,390

Table 7: The Effect of State Income Taxes

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Income Tax* is the average state income tax rate in 2010. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.156	-0.049	-0.366	0.855**	0.119	-0.856*
	(0.190)	(0.237)	(0.225)	(0.419)	(0.090)	(0.506)
Recalibrated × *Post* × *Income Tax*	7.586**	8.164	13.379***	-13.832	0.099	17.072*
	(3.839)	(6.924)	(3.820)	(8.990)	(2.212)	(10.258)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.293	0.195	0.851	0.560	0.892	0.551
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429

Table 8: The Effect of Partisanship

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Democrat* is a dummy variable that takes a value of one if the Democratic party was in power in the respective constituency in 2010 (last election prior to the recalibration), and zero otherwise. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.224	-0.389	-0.410***	-0.066	-0.044	-0.664*
	(0.181)	(0.353)	(0.123)	(0.286)	(0.117)	(0.390)
Recalibrated × *Post* × *Democrat*	0.631***	0.952***	0.871***	0.497	0.200	1.098**
	(0.192)	(0.317)	(0.151)	(0.381)	(0.127)	(0.434)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.357	0.204	0.854	0.559	0.896	0.532
Number of observations	1,616	1,616	5,736	5,736	6,437	6,437

Table 9: The Effect of Municipal Bond Ratings on Economic Outcomes by Political Party

This table presents difference-in-differences estimates of county-level *Offer Yield*, logarithm of *Issue Amount*, logarithm of *Local Government Expenditures*, *Local Tax Rate*, logarithm of *Local Government Employment*, logarithm of *Private Employment*, logarithm of *Non-Tradable Employment*, logarithm of *Construction Employment*, and logarithm of *Income* around the recalibration event (April-May 2010). *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Democrat* is a dummy variable that takes a value of one if the Democratic presidential candidate was the most voted in the county in the 2008 and 2012 elections, and zero if the Republican presidential candidate was the most voted in the county in the 2008 and 2012 elections. All other counties are excluded from the sample. Controls include house price index and number of households. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5% and 10% level respectively.

	Offer Yield	*Issue Amount*	*Local Government Expenditures*	*Local Tax Rate*	*Local Government Employment*	*Private Employment*	*Non-Tradable Employment*	*Construction Employment*	*Income*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recalibrated × *Post*	-0.163	0.220	0.065	-0.125***	0.079**	0.085***	0.034	-0.059	0.109***
	(0.191)	(0.178)	(0.042)	(0.043)	(0.038)	(0.021)	(0.076)	(0.050)	(0.031)
Recalibrated × *Post* × *Democrat*	-0.285	0.319	0.029	0.009	0.040	-0.012	0.159	0.141**	-0.038
	(0.323)	(0.264)	(0.050)	(0.053)	(0.051)	(0.030)	(0.108)	(0.070)	(0.045)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Tes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.462	0.248	0.391	0.417	0.0922	0.221	0.569	0.400	0.666
Number of Observations	5,456	5,456	11,228	11,152	10,303	20,038	24,566	17,025	23,235

Table 10: Alternative Samples

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. The sample in Panel A consists of counties or congressional districts in which the Democratic party was in power in the respective constituency in 2010 (last election prior to the recalibration). The sample in Panel B consists of counties or congressional districts in which the share of urban population was above 50% in 2010, and the Democratic party was in power in the respective constituency in 2010. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Sample of Democratic Counties						
Recalibrated × *Post*	0.397***	0.530**	0.416***	0.325	0.170***	0.459**
	(0.107)	(0.208)	(0.092)	(0.285)	(0.060)	(0.201)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.553	0.321	0.880	0.612	0.906	0.494
Number of observations	954	954	3,164	3,164	3,232	3,232
Panel B: Sample of Democratic Urban Counties						
Recalibrated × *Post*	0.182*	0.210	0.372***	0.324	0.129**	0.397*
	(0.108)	(0.185)	(0.103)	(0.318)	(0.063)	(0.214)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.470	0.254	0.889	0.625	0.922	0.498
Number of observations	689	689	1,312	1,312	1,558	1,558

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 grey have no local government unit issuing bonds in the three years before the recalibration in the Ipreo i-Deal database (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.

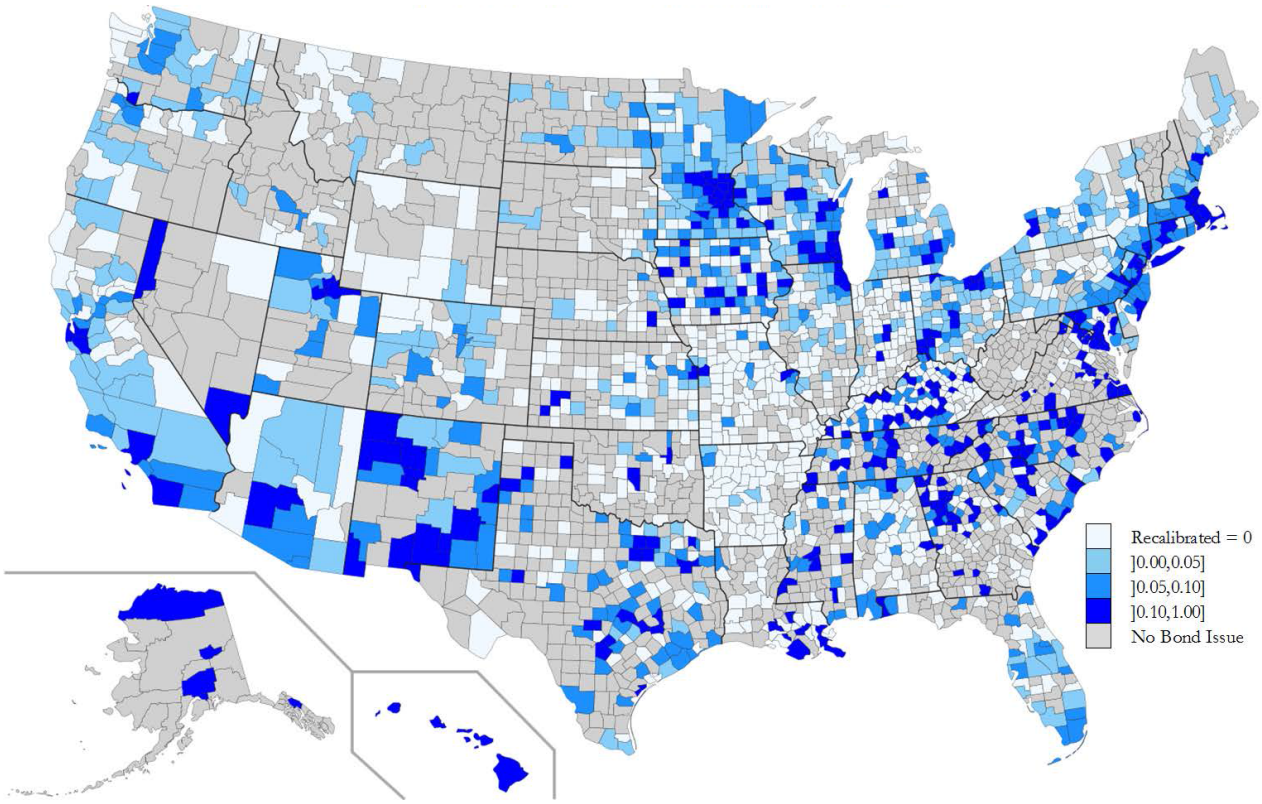
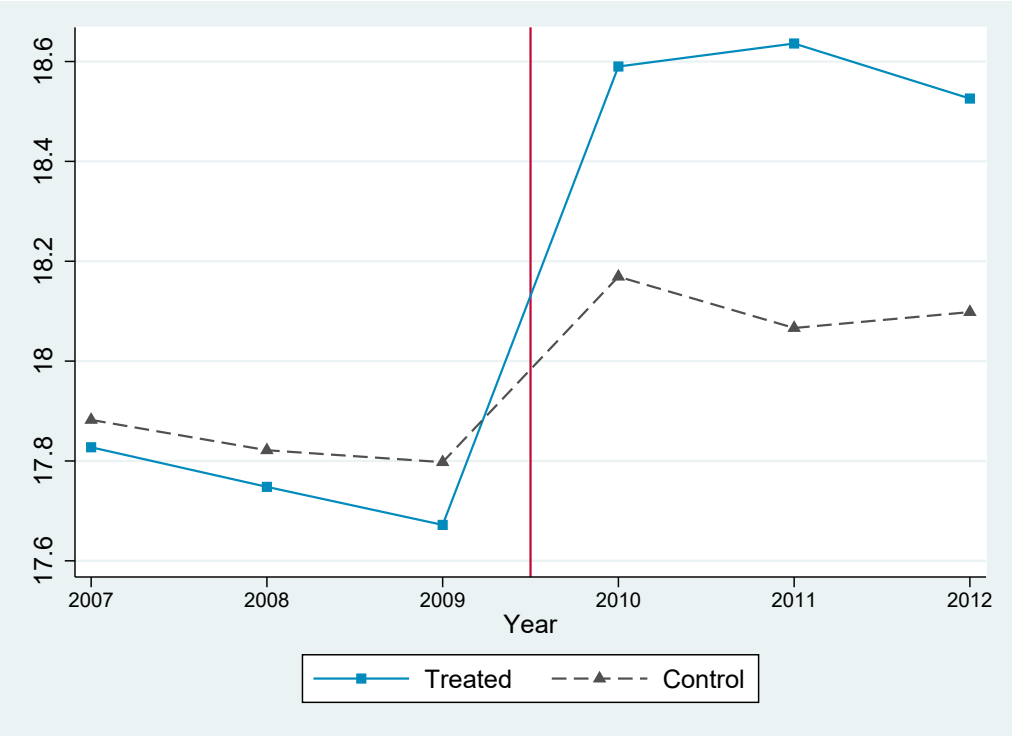


Figure 2: Ratings around the Recalibration

This figure shows the evolution of ratings around the recalibration event (April-May 2010) separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel A presents Moody's ratings. Panel B presents S&P ratings.

Panel A: Moody's Ratings



Panel B: S&P Ratings

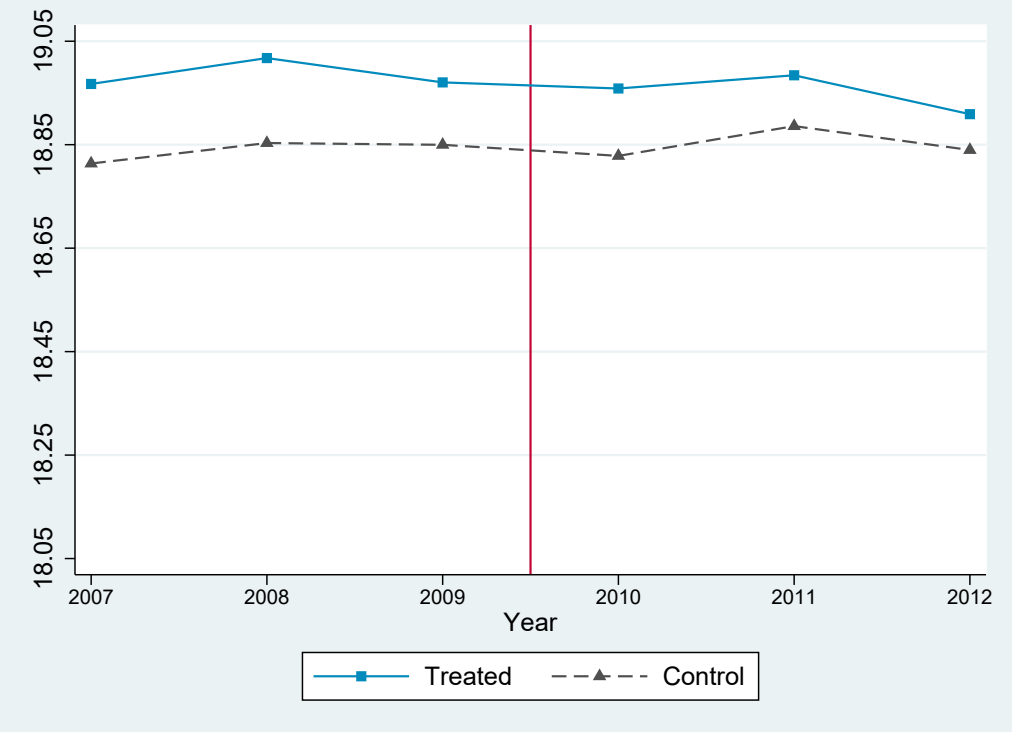


Figure 3: Difference in Ratings around the Recalibration

This figure shows the evolution of the differences in Moody's and S&P ratings between upgraded local governments (treated) and non-upgraded local governments (control) around the recalibration event (April-May 2010).

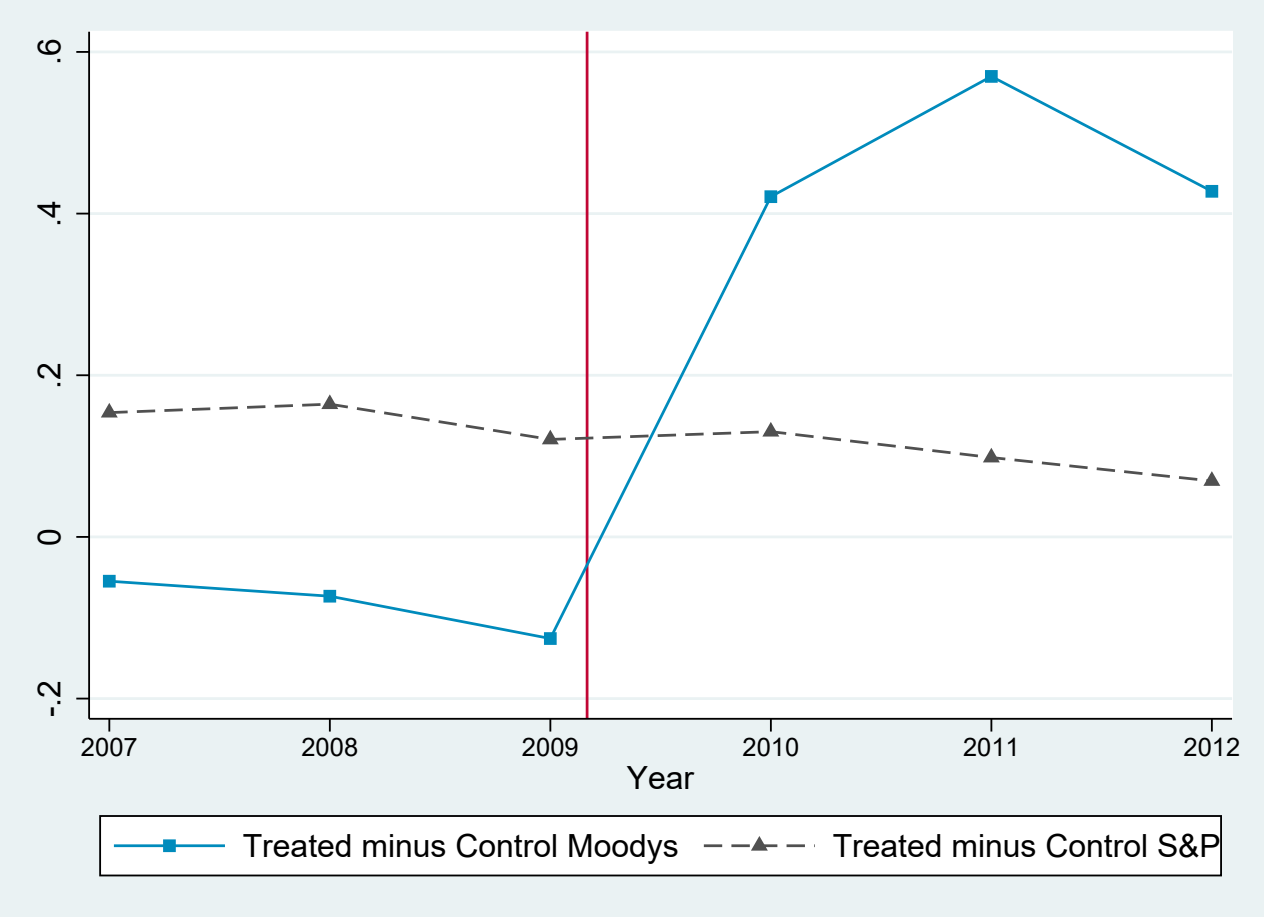


Figure 4: House Prices around the Recalibration

This figure shows the house price index for counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) around the recalibration event (April-May 2010). *Recalibrated* is the fraction of upgraded local government units in each county.

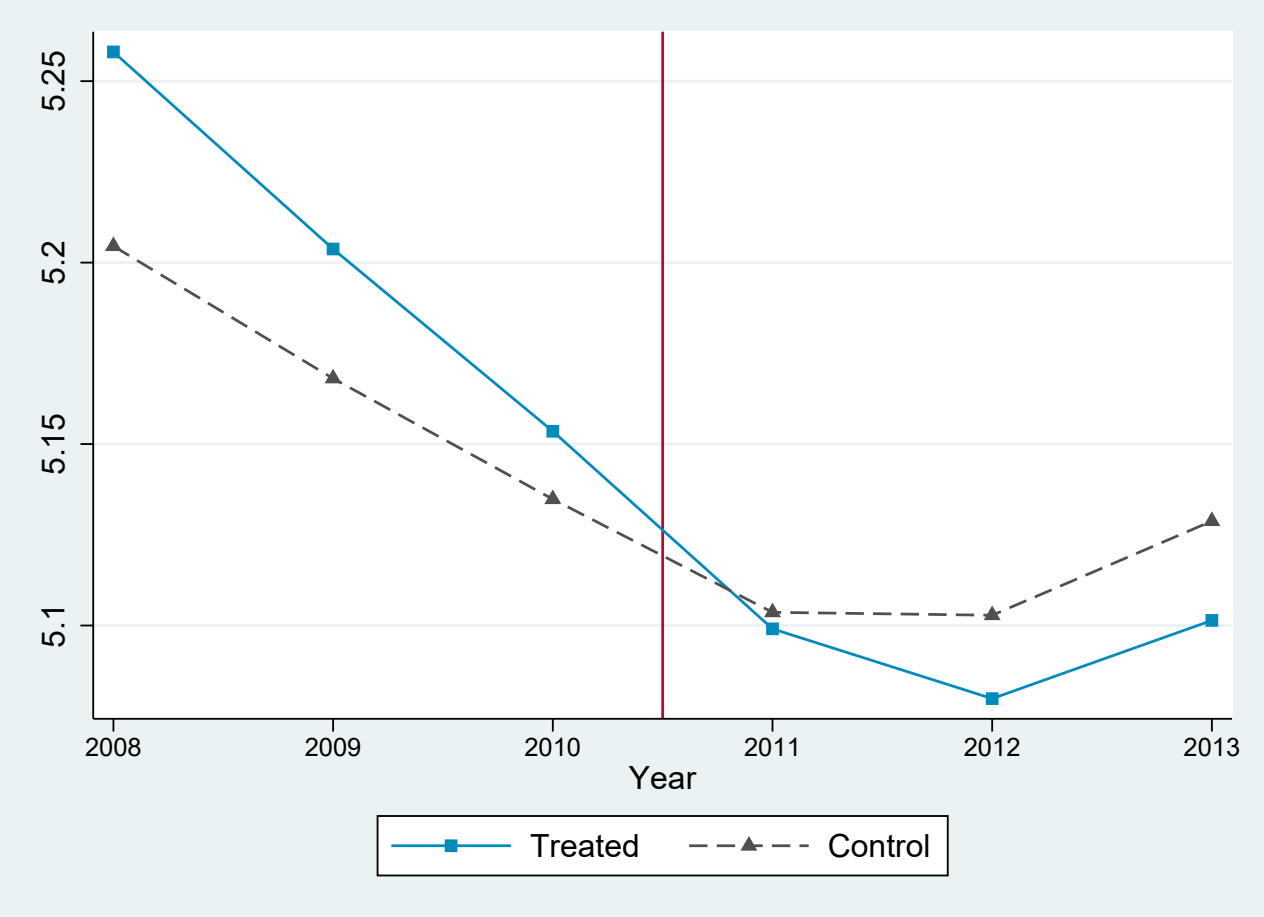


Figure 5: Difference in House Election Outcomes around the Recalibration

This figure shows the evolution of the difference in the likelihood of *Incumbent Win* in House elections between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) around the recalibration event (April-May 2010). *Recalibrated* is the fraction of upgraded local government units in each county. The sample consists of counties in the 2006-2012 period.

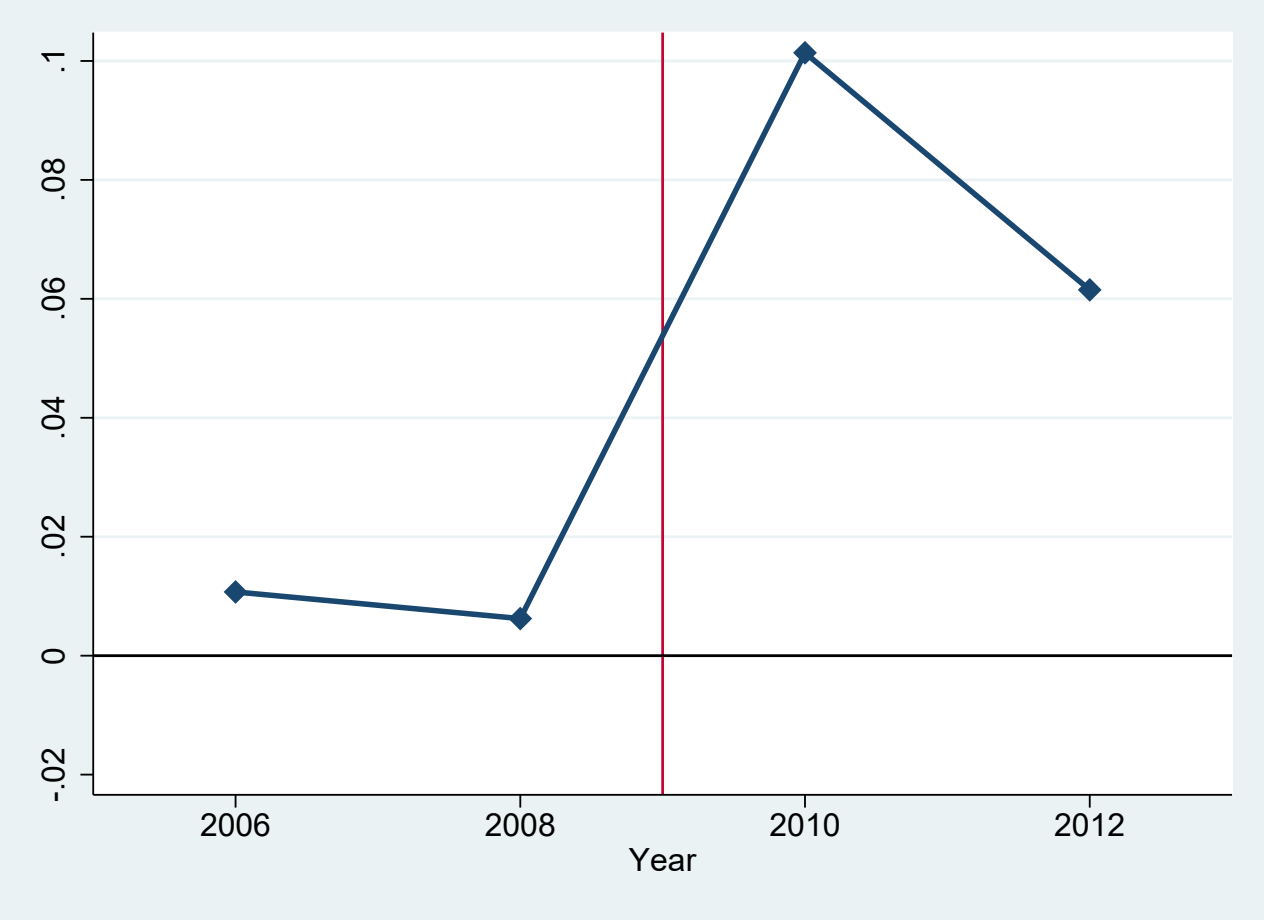


Figure 6: Difference in Gubernatorial Election Outcomes around the Recalibration

This figure shows the evolution of the difference in the likelihood of *Incumbent Win* in gubernatorial elections between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) around the recalibration event (April-May 2010). *Recalibrated* is the fraction of upgraded local government units in each county. The sample consists of counties in the 2006-2012 period.

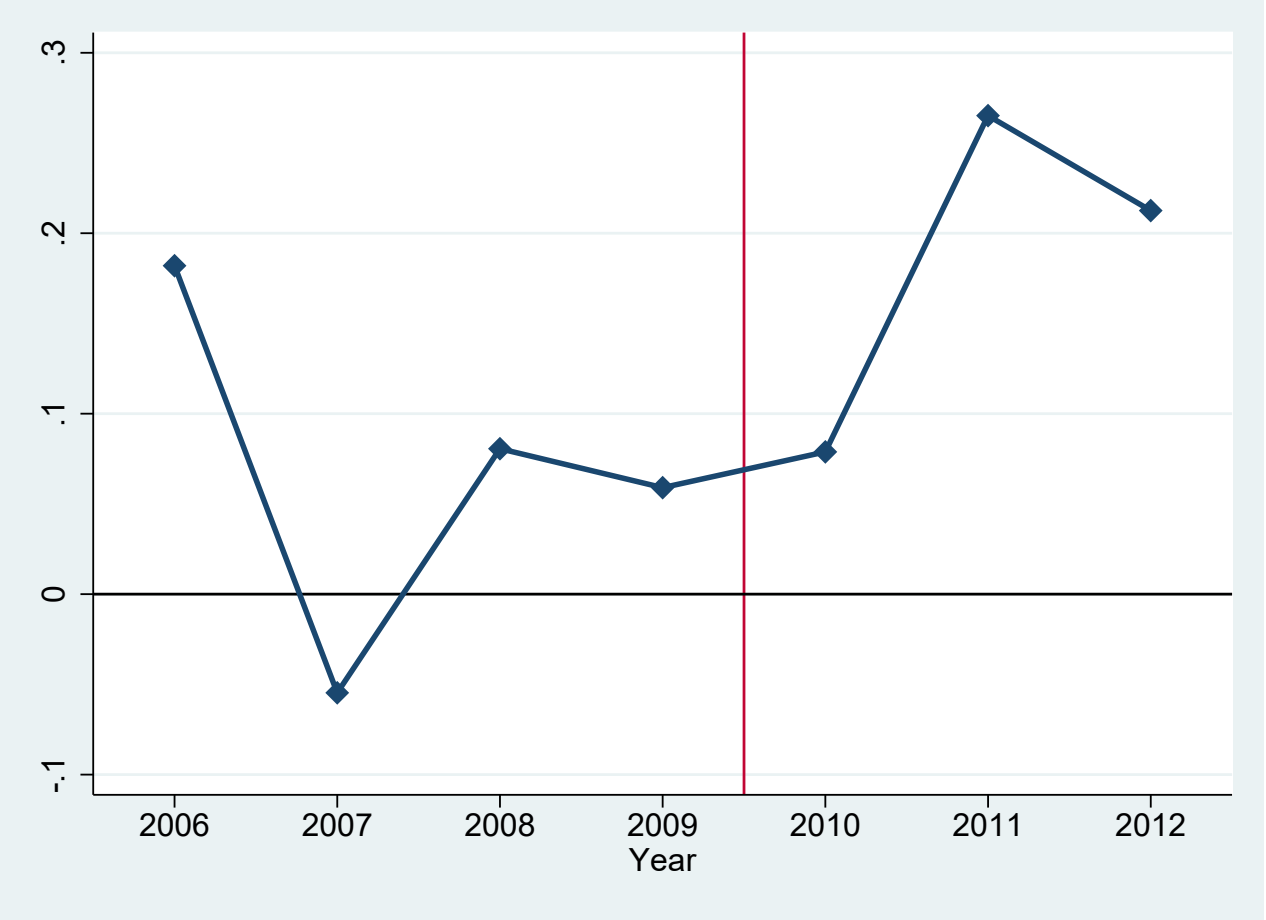


Figure 7: Difference in Senate Election Outcomes around the Recalibration

This figure shows the evolution of the difference in the likelihood of *Incumbent Win* in Senate elections between counties in the treatment group (above-median *Recalibrated*) and control group (below-median *Recalibrated*) around the recalibration event (April-May 2010). *Recalibrated* is the fraction of upgraded local government units in each county. The sample consists of counties in the 2004-2012 period.

Internet Appendix to: "Can Credit Rating Agencies Affect Election Outcomes?"

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March 15, 2018

Table IA.1: The Effect of Municipal Bond Ratings on Economic Outcomes

This table presents difference-in-differences estimates of county-level *Offer Yield*, logarithm of *Issue Amount*, logarithm of *Local Government Expenditures*, *Local Tax Rate*, logarithm of *Local Government Employment*, logarithm of *Private Employment*, logarithm of *Non-Tradable Employment*, logarithm of *Construction Employment*, and logarithm of *Income* 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 a value of one for the 2010-2012 period, and zero for the period before 2010. Controls include house price index and number of households. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5% and 10% level respectively.

	Offer Yield	*Issue Amount*	*Local Government Expenditures*	*Local Tax Rate*	*Local Government Employment*	*Private Employment*	*Non-Tradable Employment*	*Construction Employment*	*Income*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recalibrated × *Post*	-0.360***	0.209**	0.064**	-0.143***	0.093***	0.071***	0.128**	0.000	0.084***
	(0.114)	(0.096)	(0.029)	(0.029)	(0.025)	(0.015)	(0.059)	(0.036)	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.391	0.165	0.387	0.423	0.087	0.213	0.564	0.396	0.671
Number of observations	5,504	5,504	12,243	12,167	11,263	21,632	26,544	18,371	25,069

Table IA.2: The Effect of Alternative News Searches

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Crisis News* is the increase in news searches for the term "financial crisis" between the 2010-2012 period and the period before 2010. *Credit Score News* is the increase in news searches for the term "credit score" between the 2010-2012 period and the period before 2010. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Financial Crisis News Searches						
Recalibrated × *Post*	0.329	0.694	0.239	1.117**	-0.100	-0.193
	(0.251)	(0.563)	(0.170)	(0.440)	(0.066)	(0.273)
Recalibrated × *Post* × *Crisis News*	0.011	0.026	0.002	0.049*	-0.014***	-0.003
	(0.015)	(0.033)	(0.009)	(0.025)	(0.004)	(0.021)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.284	0.186	0.850	0.562	0.893	0.546
Number of observations	1,580	1,580	5,142	5,142	8,561	8,561
Panel B: Credit Score News Searches						
Recalibrated × *Post*	0.210*	0.398*	0.201	0.120	0.107***	0.144
	(0.124)	(0.240)	(0.149)	(0.319)	(0.039)	(0.190)
Recalibrated × *Post* × *Credit Score News*	0.007	0.015	0.001	-0.018	-0.003	0.054**
	(0.014)	(0.023)	(0.015)	(0.031)	(0.004)	(0.021)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.291	0.195	0.847	0.559	0.892	0.552
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429

Table IA.3: The Effect of Education

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. *Education* is the fraction of adults with a bachelor's degree or higher between 2009 and 2012. Controls include *Incumbent Share*$_{t-1}$, *Number of Votes*, *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, and *Income*. Observations are weighted by the number of votes. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.106	-0.709	0.457	0.279	0.154	-0.933
	(0.603)	(1.018)	(0.566)	(1.456)	(0.140)	(0.964)
Recalibrated × *Post* × *Education*	0.009	0.035	-0.010	-0.001	-0.001	0.028
	(0.022)	(0.040)	(0.018)	(0.049)	(0.004)	(0.030)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.294	0.197	0.849	0.561	0.892	0.549
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429

Table IA.4: Equally-Weighted Observations

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. Columns (7) and (8) present city-level estimates for mayoral elections in California in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district (columns (1)-(2)) and county (columns (3)-(6)). In columns (7)-(8), *Recalibrated* is a dummy variable that takes the value of one if the city was upgraded, and zero otherwise. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Controls in Panel A include *Incumbent Share*$_{t-1}$ and *Number of Votes*. Controls in Panel B also include *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, *Income*, and *Offer Yield*. Robust standard errors clustered at the congressional district level (in columns (1)-(2)), county level (in columns (3)-(6)), and city level (in columns (7)-(8)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections		Mayoral Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Basic Controls								
Recalibrated × *Post*	0.191**	0.403***	0.005	0.105	0.065	0.027	0.242	0.170
	(0.092)	(0.145)	(0.064)	(0.135)	(0.043)	(0.109)	(0.157)	(0.206)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes	No	No
City Fixed Effects	No	No	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.302	0.192	0.827	0.547	0.869	0.616	0.713	0.613
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429	266	266
Panel B: Economic Controls								
Recalibrated × *Post*	0.178*	0.302*	0.184**	0.492**	0.013	-0.090	0.230	0.192
	(0.097)	(0.159)	(0.090)	(0.201)	(0.075)	(0.192)	(0.157)	(0.210)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes	No	No
City Fixed Effects	No	No	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.311	0.194	0.872	0.621	0.856	0.534	0.740	0.638
Number of observations	1,586	1,586	1,667	1,667	2,790	2,790	248	248

Table IA.5: Recalibration Weighted by Amount of Bonds Issued

This table presents difference-in-differences estimates of the logarithm of the incumbent vote share (*Incumbent Share*) and a dummy variable that takes the value of one if the incumbent wins the most votes (*Incumbent Win*) around the recalibration event (April-May 2010). Columns (1) and (2) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (3) and (4) presents county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for Senate elections in the 2004-2012 period. *Recalibrated* is the fraction of upgraded local government units in each congressional district or county with each unit weighted by the corresponding amount of bonds issued. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Controls in Panel A include *Incumbent Share*$_{t-1}$ and *Number of Votes*. Controls in Panel B also include *Local Government Expenditures*, *Local Tax Rate*, *Unemployment Rate*, *Income*, and *Offer Yield*. Robust standard errors clustered at the congressional district level (in columns (1)-(2)) and county level (in columns (3)-(6)) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	House Elections		Gubernatorial Elections		Senate Elections	
	Incumbent Share	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*	*Incumbent Share*	*Incumbent Win*
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Basic Controls						
Recalibrated × *Post*	0.135*	0.275**	0.132*	0.297**	0.087***	-0.155
	(0.074)	(0.130)	(0.071)	(0.143)	(0.029)	(0.159)
Basic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.289	0.192	0.846	0.558	0.890	0.547
Number of observations	1,616	1,616	5,736	5,736	9,429	9,429
Panel B: Economic Controls						
Recalibrated × *Post*	0.117	0.209	0.161**	0.416**	0.071*	-0.387*
	(0.076)	(0.139)	(0.075)	(0.188)	(0.037)	(0.218)
Economic Controls	Yes	Yes	Yes	Yes	Yes	Yes
District Fixed Effects	Yes	Yes	No	No	No	No
County Fixed Effects	No	No	Yes	Yes	No	No
County × Senate Seat Fixed Effects	No	No	No	No	Yes	Yes
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.299	0.195	0.880	0.620	0.928	0.583
Number of observations	1,586	1,586	1,667	1,667	2,790	2,790