DISCUSSION PAPER SERIES

DP12430

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FINANCIAL ECONOMICS

ISSN 0265-8003

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Discussion Paper DP12430
Published 08 November 2017
Submitted 08 November 2017

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JEL Classification: D72, G24, H74

Keywords: elections, Credit ratings, Financial constraints, Municipal Bonds, government spending, Economic Conditions

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Can Credit Rating Agencies Affect Election Outcomes?*

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April 12, 2017

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We show that credit rating agency actions 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 an increased likelihood of reelection and higher vote shares. Rating upgrades affect elections by improving voter perceptions of the quality of incumbents, and by producing wealth effects through voters' holdings of municipal bonds. We also establish a link between incumbents' reelection prospects and the improvements in economic conditions that are due to a debt-financed increase in government spending following rating upgrades.

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*We thank Manuel Adelino, Laurent Bach, Taylor Begley, Philip Bond, Sergey Chernenko, Joao Cocco, Alex Edmans, 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, 4th Annual HEC Paris Workshop, 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 Kentucky, and University of Delaware for helpful comments. Financial support from the European Research Council (ERC) and the Fundacao para a Ciencia e Tecnologia (FCT) is gratefully acknowledged.
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1 Introduction

The long-standing debate about the power of credit rating agencies (CRAs) 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 banks and financial institutions have too much power as reported in poll results (e.g., Gallup (2011)). Regulators and academics have expressed similar concerns (Zingales (2015)).[1]

We ask whether CRA actions 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. We identify these effects by exploiting 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., how likely a municipality might be to reach 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, 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

[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)).

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 or with local and nationwide economic conditions.

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 winning odds and the vote share of the incumbent political party in the 2010-2012 elections compared to the 2006-2009 elections at the county level 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 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- or district-level heterogeneity.

We find that incumbent party candidates were more likely to be reelected in upgraded

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

counties than in non-upgraded counties. The incumbent effect persists across different types of elections. Our results for Senate elections show that a 10% increase in the fraction of upgraded local governments (which corresponds to about one standard deviation) in a county is associated with an increase of 1.7% in the likelihood that the incumbent would win the election in that county. For House elections, a 10% increase in upgraded local governments in a district is associated with a 3.9% increase in the likelihood that the incumbent would be reelected (i.e., win the election in that constituency).

We find similar evidence in executive elections. A 10% increase in the fraction of upgraded local governments in a county increases the likelihood that the incumbent would win the election (at the county level) by 4.3% for gubernatorial elections and 5.5% for presidential elections. The corresponding increase in the likelihood of reelection is 26% in the case of mayoral elections in California.

We also find evidence that incumbent politicians obtained more votes in upgraded municipalities than in non-upgraded municipalities, but these estimates are less precise due to the noisier nature of these tests. Voters do not seem to differentiate which level of government is responsible for the positive news, as municipal bond rating upgrades increase the chances that the incumbent politician is reelected in all types of elections. Overall, the results suggest that voters respond to positive news on municipalities' credit quality, and the result is that they choose continuity rather than change.

We show that ratings affect election outcomes through three channels. First, we show that municipal bond ratings affect elections directly through an impact on the candidate's political discourse and voters' perceptions of the incumbent's quality.[3] We study this hypothesis 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 perception about a candidate's quality could be influenced by the recalibration-related upgrades. We find that

[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.

the incumbent effect is stronger in states where there was a surge in ratings-related Google searches.

We also explore the timing of the effects of ratings on election outcomes. While changes in the political discourse and voters' perceptions about the incumbent's quality (direct effect) can affect elections immediately, improvements in local economic conditions due to fiscal policy (indirect effect) take time to materialize, and thus will affect election outcomes 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.

Second, we show that the recalibration produces wealth effects through the voter's holdings of local municipal bonds. Investors who held upgraded municipal bonds experienced an appreciation in the value of their portfolios in 2010, and thus an increase in their overall wealth. Cornaggia, Cornaggia, and Israelsen (2016) estimate that a lower bound for the post-recalibration cumulative abnormal return of upgraded bonds is approximately 50 basis points. Since households held approximately $1.87 trillion in municipal bonds in 2010, the wealth increase was about $9 billion on average. These voters' positive wealth shock can in turn affect their voting behavior. We test this idea by exploring a key feature of the municipal bond market: 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.

Finally, we find that ratings affect elections indirectly through changes in fiscal policy and economic conditions. Municipal bond markets are an important resource for local governments to finance the construction and maintenance 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.

We find that upgraded municipalities experienced a significant reduction in borrowing costs in the municipal bond market after Moody's recalibration (Cornaggia, Cornaggia, and Israelsen (2016)). Reduced borrowing costs would allow municipalities to increase bond issuance and spending (or reduce taxes). These fiscal policy changes had positive spillovers to the private sector in terms of employment and income (Adelino, Cunha, and Ferreira (2017)). We establish a link between improvements in local economic conditions and election outcomes using instrumental variables methods. We show that increases in bond issuance amounts due to the recalibration significantly increased the incumbent's likelihood of winning the election. 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.[4]

To have a detailed picture of the political impact of CRAs, we investigate whether the effect of municipal bond ratings on election outcomes 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 fiscal policy. 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 reduced bond yields, followed by an increase in bond issuance and government spending, and a subsequent increase in private employment and income. Our results indicate that both parties implement similar policies, but the electoral rewards 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. First, we study issuers that have both Moody's and S&P ratings. There

[4]An alternative potential channel for our results is that the upgrades reveal new information about the power of incumbent politicians, but this channel is unlikely to explain our results. The upgrades were identical within issuer type (i.e., county, city, township, school district, and special district) and pre-recalibration rating level. In addition, we find that the incumbent candidates were more likely to win in upgraded counties. Since our regressions are within state and year, in these elections, the candidate is the same across all counties, so our results cannot be explained by cross-sectional differences in incumbent power.

are no changes in their S&P ratings either before or after the recalibration. If the recalibration by Moody's reflected changes in underlying credit quality, the S&P ratings on this sample of bonds would also be affected. Second, we find that house prices of treatment and control groups 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 treatment and control groups differently. Third, our results are also robust to the use of a sample of urban counties. Finally, we consider two alternative definitions of our treatment variable: a dummy variable that takes the value of one when the county has at least one upgraded issuer; and a treatment variable weighted by the amount of bonds issued. The results are robust to these alternative definitions.

Our study contributes to three strands of the literature. First, we provide a novel link between credit ratings 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), and Almeida, Cunha, Ferreira, and Restrepo (2017)). Research also shows that municipal bond ratings affect municipalities' financing and economic condition. (Cornaggia, Cornaggia, and Israelsen (2016), Adelino, Cunha, and Ferreira (2017)). To the best of our knowledge, we are the first to provide causal evidence that the actions of credit rating agencies can influence incumbents' chances of reelection.

Second, we contribute to the literature on the effect of economic conditions on election outcomes. 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), and 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)). In addition, Bagues and Esteve-Volart (2016) show that exogenous good economic conditions (driven by a cash windfall won in a lottery in Spain) have a positive effect on the incumbent's vote share. 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 spending.

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), and Lee, Moretti, and Butler (2004)). Yet Ferreira and Gyourko (2009) find no evidence of partisan influence on local government policies. We contribute to this literature by showing that political partisanship does not affect how incumbent politicians react to a reduction in municipalities' financial constraints. Democratic and Republican politicians implemented similar fiscal policies following the recalibration even in non-closely contested elections. Our results do 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. Its 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%. 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 level, we restrict the analysis of the recalibration to bond issues that can be matched to a county. 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.

We first define the treatment and control groups at the local government unit level (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*).

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 up-

graded in the recalibration, and therefore can be used in the control group. Some local governments were already "properly calibrated" 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 effect of the recalibration on Moody's ratings of new bond issues for the treatment and control groups from three years before the recalibration to three years after it (relative to four years before the recalibration, the baseline year). 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. Figure 3 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. Figure 3 provides evidence that Moody's recalibration does not reflect a change in issuers' credit quality, which is an important

validation of our identification strategy.

We also compare changes in house price index (HPI) of treatment and control groups before and after the recalibration. Figure 4 shows no significant differential effects 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, the U.S. Senate, gubernatorial, and presidential elections at the county level for the 2004-2012 period from David Leip's website.[5] 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 ratings changes on local election outcomes, we collect mayoral election data for California for the 2006-2012 period from the California Elections data archive.[6]

Elections for the House, Senate, and President 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, and presidential elections take place in leap years. Many 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.[7] Senators

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

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

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

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. Presidents serve four-year terms. 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.[8] 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. For presidential elections, the 2004 and 2008 elections are included in the pre-treatment period, and the 2012 election is 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 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 Senate, gubernatorial, and presidential elections, we create a dummy variable (*Incumbent Win*) that takes a value of one if the incumbent party candidate wins the most votes in a county, and zero otherwise. As an additional way to test whether ratings affect election outcomes, we 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 or congressional district (for House elections). We then create the variable Δ*Incumbent Share*, which is the difference between the *Incumbent Share* in the current election and the previous election (in percentage points). This variable is commonly used in the political economy literature to measure the change in vote share for the incumbent (e.g., Bagues and Esteve-Volart (2016)).

We then merge the elections data with the recalibration data to obtain our measure of

[8] Given that the 2010 elections took place on the 2nd of November, 2010 is included in the post-recalibration period.

the degree to which the Moody's changes affected incumbent politicians in a given region. In the case of the Senate, Gubernatorial, and presidential 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.[9] We match each district to the corresponding counties using a bridge provided by the U.S. Census Bureau.[10] 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 use the *Recalibrated* variable of the county where the city is located to measure the treatment intensity.

To control for constituency size, the election outcomes regressions include the total number of votes cast in a county (*County Votes*), congressional district (*District Votes*), or city (*City Votes*) as a control variable in some specifications. The regressions also include the lag *Vote Share* as a control variable in some specifications.

Table 1 presents summary statistics of treatment and control groups for election outcomes and treatment variable in the pre-recalibration period by election type: Senate (Panel A), House (Panel B), gubernatorial (Panel C), presidential (Panel D), and mayoral (Panel E). The treatment group includes counties/districts with above-median *Recalibrated*, and the control group includes counties/districts with below-median *Recalibrated*. In the case of Senate, gubernatorial, and presidential elections, the median of the *Recalibrated* variable is zero. Columns (7) and (8) show the differences between the two groups. One feature of 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 county, congressional district, or city) and state-by-year fixed effects; these controls are also included in our regression tests. Although the raw differences show some statistically significant differences

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

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

between treatment and control groups prior to the recalibration (column (7)), these differences lose statistical significance 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.[11] 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).

2.4 Economic Outcomes

The primary economic outcomes we study are local government expenditures, tax rate, government employment, private employment, and income. We obtain data on government 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/district for the same sample period as the bond sample (2007-2013). 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.

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 we use a sample of local

[11] 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.

governments in the other years. Government employment is measured 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).[12]

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. Income (adjusted gross income) is defined as total wages and salaries in a county in a given 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).[13]

In the economic outcomes regressions, we control for other factors that are important determinants of local economic conditions. We include yearly changes in logarithm of house price index (HPI), to capture the severity of the post-2006 downturn in each county, as well as the logarithm of the number of households. Housing prices come from the Federal Housing Finance Agency's (FHFA's) HPI data at the Metropolitan Statistical Area (MSA) level. [14] We obtain county-level information on the number of households from the 2007 Census Bureau Summary Files. The variable *Households* is defined as one or more people that occupy a given housing unit.

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 expenditure, local government employment, private employment, or income. We present both raw differences in

[12]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.

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

[14]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.

means between treatment and control groups, as well as differences after adjusting for county size (using the number of households) and state-by-year fixed effects.[15] 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. More important, 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. Additionally, the growth rates of outcome variables in the pre-treatment period are similar across the two groups, except for government expenditures (although the difference is economically small). We conclude that pre-existing differential trends between treatment and control groups are unlikely to explain our results.

3 The Impact 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 bond ratings. We start the analysis by studying the impact on the likelihood that the incumbent party candidate will win the Senate, House, or gubernatorial elections using the regression model (at the county level or congressional district level in the case of House elections):

$$Incumbent\ Win_{it} = \beta_1 Recalibrated_i \times Post_t + \beta_2 X_{it} + \alpha_i + \gamma_{state,t} + \varepsilon_{it}, \tag{1}$$

where *Recalibrated* is the fraction of upgraded local governments in a county/district; 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 level, the regressions include election-level (county or district) fixed effects (α_i) in all specifications. We also include state-by-year fixed effects ($\gamma_{state,t}$) to take

[15] In the election outcomes regressions, we control for county size using number of votes instead of number of households.

into account any macroeconomic conditions and other time trends that could be affecting election outcomes. 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 an upgraded municipality as compared to the change in a non-upgraded municipality in the same state and year.

In some specifications, we 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), and we also include the number of votes in the county or district. In addition, some regressions are weighted using the number of votes to account for the possibility that size could be correlated with voting behavior. Standard errors are clustered at the election level (county or district).

Table 3 presents the results for Senate (Panel A), House (Panel B), and gubernatorial (Panel C) elections. Columns (1)-(3) present results in which the dependent variable is the *Incumbent Win* dummy variable (i.e., estimates of a linear probability model). In Panel A, we examine the effect of rating upgrades on Senate elections using the regression in equation (1) at the county level. 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 probability of *Incumbent Win* of the treatment group relative to the control group. Columns (2) and (3) show similar differential effects when we include control variables or weight observations by number of votes, although the effect is not statistically significant in column (3). The estimate in column (2) implies that a 10% increase in the fraction of upgraded local governments in a county (which corresponds to about a one-standard deviation increase in the *Recalibrated* variable) leads to an increase of 1.7% in the probability of *Incumbent Win*.

Columns (4)-(6) present results in which the $\triangle$*Incumbent Share* is the dependent variable. The interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant in all specifications. The estimate in column (5) implies that a 10% increase in the *Recalibrated* variable increases the $\triangle$*Incumbent Share* by 0.6%. We conclude that candidates affiliated with the

incumbent party benefited from an increase in the probability that they would win the most votes in the county in Senate elections after the recalibration.

Figure 5 shows the effect of the recalibration on the probability of an incumbent's party win (Panel A) and the incumbent's vote share (Panel B) around the recalibration for the treatment and control groups in Senate elections. The figures show that treatment and control groups followed similar trends before the recalibration. We then see a significant higher *Incumbent Win* and *Incumbent Share* for the treatment group than for the control group after the recalibration.

In Panel B of Table 3, we examine the effect of rating upgrades on House elections using the regression in equation (1) at the congressional district level (the *Recalibrated* variable is now the fraction of upgraded local governments in each district). In columns (1)-(3), the interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant, which indicates that incumbents that run for the House in districts where a higher proportion of local government units were upgraded were more likely to be reelected. The estimate in column (2) implies that a 10% increased in the fraction of local governments upgraded in a district increases the probability of incumbent reelection by 3.9%. While the estimates are not statistically significant in columns (4)-(6), they are of similar size to those in Panel A, and consistently indicate a differential increase in the $\triangle$*Incumbent Share* of the treatment group versus the control group after the recalibration.

Figure 6 shows the effect of the recalibration on the probability of an incumbent's party win (Panel A) and the incumbent's vote share (Panel B) around the recalibration for the treatment and control groups in House elections. Again, the two groups followed similar trends before the recalibration, and we again observe a significant differential effect between treatment and control groups after the recalibration.

In Panel C of Table 3, we examine gubernatorial elections using the regression in equation (1) at the county level. We find that governors affiliated with the incumbent party were more likely to be elected in counties with a higher proportion of upgraded local governments after the recalibration. The estimate in column (2) implies that a 10% increase in the *Recalibrated*

variable increased the probability of reelection by 4.3%.

We perform several robustness checks to ensure that our results are not driven by the treatment variable definition or the sample choice. These robustness checks are shown in the Internet Appendix. Table IA.1 presents the results of regressions similar to those in Table 3, but replacing the treatment variable with a dummy that takes a value of one if at least one local government unit within the county was upgraded. In Table IA.2, we use a treatment variable weighted by the amount of bonds issued. The results using these alternative treatment variables are similar to those in Table 3. In Table IA.3, we restrict the sample to urban counties (counties where the share of urban population is at least 50%) to make sure that our results are not driven by a lack of comparability between treatment and control groups. Although the reduction in sample size makes the results slightly weaker, the estimates are still economically important.

Next, we study the effect of ratings on presidential and mayoral elections. We analyze both the probability that the incumbent wins the election and the change in vote share using the regression in equation (1). Table 4 presents the results. The specifications are similar to those in Table 3. In Panel A, we analyze whether the 2012 presidential election results were affected by the recalibration in 2010. In particular, we study whether Barack Obama (the incumbent) was more likely to win in counties where a higher proportion of local governments were upgraded. In columns (1)-(6), the interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant (with the exception of column (3) in which the coefficient is statistically insignificant). These results indicate that the recalibration had a disproportional effect on the probability of the incumbent presidential candidate winning the election in counties with a higher fraction of upgraded local governments (*Recalibrated*). The estimate in column (2) shows that a 10% increase in the *Recalibrated* variable in a county increases the probability of a Barack Obama win (at the county level) by 5.5%. In addition, the estimate in column (5) implies that a 10% increase in the *Recalibrated* variable in a county increased Barack Obama's vote share by 0.9%.

In Panel B, we examine mayoral elections in California using the regression in equation (1)

at the city level (although the *Recalibrated* variable is defined at the county level). In columns (1)-(3), the interaction term *Recalibrated* $\times$ *Post* coefficient is positive and significant, which indicates a higher probability of reelection for candidates who happened to be in the mayor's office at the time of the recalibration. The estimate in column (2) implies that a 10% increase in the fraction of upgraded local governments increases the probability of reelection by 26%. The estimates of the interaction term coefficient in columns (4)-(6) for the Δ*Incumbent Share* are all positive and economically significant, but imprecisely estimated because of the small sample.

We perform robustness checks of the presidential and mayoral elections results similar to those for the Senate, House, and gubernatorial elections. In Table IA.4, we replace our treatment variable with a dummy that equals one if at least one local government unit upgraded within the county. In Table IA.5, we weight the treatment variable by the amount of bonds issued. The results remain similar with these alternative treatment definitions. In Table IA.6, we restrict the sample to urban counties. The results are similar for both the presidential and 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 probability of reelection and vote share. 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 How Do Credit Ratings Influence Elections?

There are several ways that municipal bond ratings can affect election outcomes. Incumbents might use better ratings as a certification of their quality, in which case ratings could be part of the political discourse during the campaign. Or, ratings might affect election outcomes through a wealth effect attributable to an increase in the value of voters' municipal bonds. Finally, 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 upgrades.

4.1 Political Discourse and Voter Perceptions

The impact of municipal 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 changes in the policies or in economic conditions. This is especially true if voters attribute the change in ratings due to the recalibration to skill (attribution error).

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 an upgrade.[16]

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

[16]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.

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.[17] We create a dummy variable (*Ratings News*) that takes a value of one if the increase in searches for the term "credit rating" from before the recalibration (2006-2009) to after the recalibration (2010-2012) is above the median, and a value of zero if the increase is below the median. States with an increase in searches for ratings are more likely to be the 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 an above-median increase. Table 5 presents the results for Senate, House, and gubernatorial elections. The explanatory variable of interest is the triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Ratings News*, which measures the effect of ratings on election outcomes in states with high news searches versus states with low news searches. The interaction term coefficient is positive in all specifications and statistically significant in the case of Senate and gubernatorial elections.

We perform a robustness test to guarantee that voters are in fact searching for the term "credit rating" to better understand it, and not because of poor economic conditions at the time of the 2007-2009 financial crisis. We repeat our tests using a *Crisis News* dummy variable based on searches for the term "financial crisis" instead of the term "credit ratings." Table IA.7 in the Internet Appendix presents the results. We find that states in which there is an increase in searches for the term "financial crisis" do not experience a stronger effect of rating upgrades on election outcomes. In fact, the coefficient on the triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Crisis News* is insignificant and even negative in some specifications.

We also investigate the timing of the effects of rating upgrades on elections. A direct effect of ratings (i.e., political discourse and voter's perception of the quality of the candidate)

[17]The eleven entities with data on Google Trends are: California, Florida, Illinois, Massachusetts, New Jersey, New York, Oregon, Pennsylvania, Texas, Virginia, and the District of Columbia.

would occur sooner on elections that take place in the year of the recalibration, while an effect through fiscal policy and improvement in economic conditions would occur with a lag.

Table 6 presents the effects of rating upgrades on elections by year. The coefficients of interest are the interaction variables between the *Recalibrated* variable and 2010, 2011, and 2012 calendar-year dummies. These interactions allow 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 and Gubernatorial elections. Overall, these results suggest that one channel through which municipal rating upgrades may affect elections is in use as a political weapon to influence voter perceptions of the quality of the candidate.

4.2 Wealth Effects

The recalibration may generate wealth effects that can directly affect local economic conditions. 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. Cornaggia, Cornaggia, and Israelsen (2016) estimate that the lower bound for the cumulative abnormal return of upgraded bonds held by retail investors was approximately 50 basis points, and that households held approximately $1.87 trillion of municipal bonds in 2010. Therefore, investors in municipal bonds experienced an increase in their wealth of about $9 billion, an economically meaningful effect. Such a wealth shock could affect voting behavior, which may explain our results.

The effects would be stronger if the ownership of municipal bonds is segmented in a specific region, and thus upgrades led to a large local wealth effect. We test this idea by exploring a key 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 particular state. This creates stronger incentives for local ownership of municipal bonds in states with higher income tax rates (e.g., California), while the incentives are less strong 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, that it is in states with lower income taxes.

Table 7 presents the results. Columns (1) and (2) presents results for Senate elections; columns (3) and (4) present results for House elections; and columns (5) and (6) present results for gubernatorial elections. The triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Tax*, where *Tax* is the top marginal state income tax rate in 2010. The coefficient on the triple interaction measures the effect on election outcomes in states with high income taxes versus states with low income taxes. We find that the effects of ratings upgrades on elections are more pronounced in states with high income taxes, as indicated by the positive and significant coefficient on the triple interaction term in the case of Senate and Gubernatorial elections; in the House elections the coefficient is positive and economically significant but statistically insignificant. Therefore, the evidence supports the idea that CRAs influence election outcomes through an effect in the distribution of wealth effects across states.

Democratic states traditionally have higher income taxes. To rule out the possibility that Democratic party voters respond more positively to expenditures is driving our results, we perform a robustness test in which we restrict our sample to counties in Republican states. Table IA.8 in the Internet Appendix presents the results for the sample of Republican states. Although the results are slightly weaker due to a smaller sample, the triple interaction *Recalibrated* $\times$ *Post* $\times$ *Tax* coefficient indicates that ratings have a stronger impact on election outcomes in Republican constituencies with high income taxes than in Republican constituencies with low income taxes.

4.3 Fiscal Policy and Local Economic Conditions

We test the hypothesis that political gains in elections are due to a reduction in local governments' financial constraints. Local governments facing lower borrowing costs are able to expand bond financing and adopt an expansionary fiscal policy (increasing spending and/or reducing taxes). In turn, this fiscal policy can improve local economic conditions, which might

translate into more votes for the incumbent candidate.

To test this channel, we first study the effect of the recalibration on economic outcomes. Table 8 presents the results of difference-in-differences regressions of local economic outcomes around the recalibration. We find that the recalibration is associated with economically large and statistically significant effects. The recalibration is associated with a significant decline in the average offer yield (*Offer Yield*) of upgraded municipalities (column (1)), consistent with the evidence in Cornaggia, Cornaggia, and Israelsen (2016). Reduced borrowing costs would allow local governments to increase the amount of bonds issued (column (2)). Columns (3)-(5) show that the increase in the amount of bonds issued allowed local governments to increase their expenditures and employment, and reduce taxes. Columns (6)-(9) show that the increase in government expenditures had positive spillovers to the private sector as recalibrated counties experienced an increase in private employment and income. The effect is particularly strong in the non-tradable sector (retail, food, and accommodation; NAICS codes 44–45 and 72), which is more dependent on local demand (Mian and Sufi (2014), and Adelino, Ma, and Robinson (2017)). These findings are consistent with those in Adelino, Cunha, and Ferreira (2017).

To study the relation between municipal bond financing and election outcomes, we implement instrumental variables methods in which we analyze the impact of the recalibration on election outcomes through its effect on the amount of bonds issued in the municipal bond market. We use the amount of bonds issued as an overall measure of the changes in fiscal policy (government expenditures and taxes) due to the recalibration at the county or congressional district level. In the first-stage regression, we test whether upgraded municipalities increase the amount of bonds issued. We estimate a regression model at the county/district level (first stage):

$$Issue\ Amount_{it} = \beta_1 Recalibrated_i \times Post_t + \beta_2 X_{it} + \alpha_i + \gamma_{state,t} + \varepsilon_{it}, \tag{2}$$

where variables are defined as in equation (1). *Issue Amount* is the average yearly amount of

bonds issued (by local government units within a county) during the electoral term divided by number of votes (in tens of thousands of dollars). The coefficient of interest is β, which measures the impact of the recalibration on the amount of bonds issued per voter. In the second-stage regression, we measure the impact of the increase in bond financing due to the recalibration on election outcomes.

Table 9, Panel A, presents the estimates of the first-stage regression. Columns (1) and (2) present the results for Senate elections, columns (3) and (4) for House elections, columns (5) and (6) for gubernatorial elections, and columns (7) and (8) for presidential elections. The estimates of β indicate a positive and significant effect of the recalibration-related upgrades on the amount of bonds issued (*Issue Amount*).

We then study whether the increase in bond financing (which allows local governments to increase spending and/or cut taxes) is the channel through which rating upgrades affect elections. Table 9, Panel B, presents the estimates of the second-stage regression. In columns (1), (3), (5), and (7), we estimate the impact of bond financing on the probability of an incumbent win in Senate, House, gubernatorial, and presidential elections. We find a positive and economically significant effect of the *Issue Amount* on the *Incumbent Win*. The effect is also statistically significant with exception of Senate elections. We find that a $1,000 increase in *Issue Amount* is associated with an increase in the likelihood of an incumbent win of 1.2% in House elections, 1.1% in gubernatorial elections, and 3.5% in presidential elections. Columns (2), (4), (6), and (8) present estimates in which the dependent variable is the $\triangle$*Incumbent Share*. Although the results are imprecisely estimated, they are consistent with the idea that rating upgrades have a positive impact on the electoral chances of the incumbent.

5 Political Parties

Does political partisanship shape the effect of municipal bond ratings on elections? To investigate this question we start by studying the differences in election outcomes between Democratic and Republican incumbents following Moody's recalibration in 2010. Next we

study how the differential electoral responses may be 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 the recalibration on election outcomes among Democratic and Republican incumbent candidates. The regressions include a triple interaction term *Recalibrated* $\times$ *Post* $\times$ *Democrat*, where *Democrat* is a dummy variable that takes a value of one if the Democratic party is in power at the county level (based on the last election prior to the recalibration), and zero otherwise. The coefficient on the interaction term measures whether the effect on election outcomes differs between Democratic incumbent candidates and Republican incumbent candidates.

Table 10 presents these estimates. Columns (1) and (2) present the results for Senate elections. We find that rating upgrades have a more pronounced impact in Democratic counties, both in terms of the likelihood of an incumbent win and vote share. The effect is both economically and statistically significantly stronger for Democratic incumbents than for Republican incumbents. Columns (3) and (4) show that Democratic incumbents also benefit more from rating upgrades in terms of the likelihood of winning than Republicans in House elections. Columns (5) and (6) show a stronger effect of the upgrades for Democratic incumbents than Republican incumbents in gubernatorial elections. The results for the $\triangle$*Incumbent Share* show that in fact incumbents in Republican counties are losing in terms of vote shares, and incumbents in Democratic counties experience a significantly stronger increase in their vote shares than Republican incumbents.

In short, our results suggest that incumbent Democratic candidates benefit more from the recalibration-related upgrades than incumbent Republican candidates. Democratic incumbents in upgraded municipalities experience a higher chance of winning, as well as more of an increase in their vote share.

5.2 Political Parties and Economic Outcomes

We also examine whether the differences in the effects of the recalibration on the election outcomes of Democratic counties relative to Republican counties are due to differences in policies implemented by politicians and their spillovers to the private sector. 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 had more votes in that county than other candidates 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.

First, we study whether 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 and Republican counties using the interaction term *Recalibrated* $\times$ *Post* $\times$ *Democrat*. Columns (1) and (2) of Table 11 present the results. We do not find any significant difference between Democratic and Republican counties in the access to municipal bond markets.

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 *Government Expenditures*, *Tax Rate*, and *Government Employment*, respectively. We do not find any 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 each party may lead 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 the *Construction Employment*, as the recalibration is associated with a significant increase only in Democratic counties. We do not find any 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 and their subsequent consequences. We do find that voters react differently at the time of elections even though policies are similar across parties. 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 Conclusion

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.

We identify three channels through which credit ratings of local governments affect elections. First, ratings act as a sort of certification that helps to shape voter opinions of the quality of incumbent politicians. Second, rating upgrades generate wealth effects through voter holdings of local municipal bonds. Finally, upgraded local governments can and do increase bond financing and spending. This increase in government spending leads to an improvement in economic conditions, which enhances the incumbent's electoral prospects.

Our results suggest that incumbent politicians are rewarded at the polls when positive shocks benefit their constituents, even if the shock is in no way in the incumbent's control (attribution error). This could be due to rational inattention as the average voter has little way to separate political skill from luck. Moreover, voters may simply not be able to make such judgments.

Our results highlight the power of credit rating agencies (CRAs). Credit ratings of local

governments can affect elections outcomes. We conclude that CRAs have an important effect on markets beyond simply reducing information asymmetry. Regulators should be aware of the role of CRAs in shaping the architecture of financial systems, as their actions could have implications beyond the financial markets; they can affect the political process. Although our results suggest that CRAs may have an outsize power, we see a potential 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.

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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 election outcome variable and treatment intensity for treatment and control groups. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. 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 for House, gubernatorial, and mayoral elections, and from 2004 to 2008 for Senate and presidential elections. Column (7) presents raw differences between treatment and control groups. Column (8) presents difference treatment and control groups adjusted by state-year fixed effects and number of votes in the county, congressional district or city. p-values clustered at the county level (for Senate, gubernatorial, and presidential elections), congressional district level (for House elections), and city (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: Senate Elections								
Incumbent Share	0.566	0.139	1,917	0.590	0.157	4,283	-0.023***	-0.008**
							(0.000)	(0.033)
Incumbent Win	0.412	0.492	1,917	0.445	0.497	4,283	-0.084***	0.012
							(0.000)	(0.172)
County Votes	0.842	1.566	1,917	0.116	0.227	4,283	0.726***	
							(0.000)	
Recalibrated	0.103	0.114	1,917	0.000	0.000	4,283	0.103***	0.100***
							(0.000)	(0.000)
Panel B: House Elections								
Incumbent Share	0.684	0.139	411	0.642	0.126	409	0.041***	0.010
							(0.001)	(0.331)
Incumbent Win	0.939	0.239	411	0.907	0.291	409	0.032*	0.020
							(0.088)	(0.351)
District Votes	2.279	0.738	411	2.421	0.695	409	-0.143***	
							(0.005)	
Recalibrated	0.136	0.091	411	0.022	0.016	409	0.114***	0.105***
							(0.000)	(0.000)
Panel C: Gubernatorial Elections								
Incumbent Share	0.510	0.141	932	0.518	0.160	2,141	-0.009	0.014***
							(0.132)	(0.004)
Incumbent Win	0.267	0.443	932	0.355	0.479	2,141	-0.088***	0.003
							(0.000)	(0.822)
County Votes	0.635	1.022	932	0.100	0.183	2,141	0.535***	
							(0.000)	
Recalibrated	0.104	0.114	932	0.000	0.000	2,141	0.104***	0.099***
							(0.000)	(0.000)
Panel E: Presidential Elections								
Incumbent Share	0.534	0.128	1,922	0.609	0.129	4,297	-0.075***	-0.024***
							(0.000)	(0.000)
Incumbent Win	0.626	0.484	1,922	0.830	0.376	4,297	-0.204***	-0.062***
							(0.000)	(0.001)
County Votes	1.004	1.887	1,922	0.130	0.228	4,297	0.874***	
							(0.000)	
Recalibrated	0.103	0.114	1,922	0.000	0.000	4,297	0.103***	0.100***
							(0.000)	(0.000)
Panel D: Mayoral Elections								
Incumbent Share	0.695	0.213	83	0.649	0.244	70	0.047	0.042
							(0.246)	(0.320)
Incumbent Win	0.892	0.313	83	0.786	0.413	70	0.106	0.111
							(0.119)	(0.130)
City Votes	0.235	0.409	83	0.161	0.333	70	0.074	
							(0.258)	
Recalibrated	0.130	0.053	83	0.028	0.016	70	0.102***	0.092***
							(0.000)	(0.000)

Table 2: Summary Statistics of Economic Outcomes

This table presents pre-recalibration mean, standard deviation, and number of observations for each economic outcome 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 (log). 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)
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)
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 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)
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)
Recalibrated	0.104	0.115	2,868	0.000	0.000	6,475	0.104***	0.098***
							(0.000)	(0.000)

Table 3: The Effect of Municipal Bond Ratings on Election Outcomes: Senate, House and Gubernatorial Elections

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for Senate elections in the 2004-2012 period. Panel B presents congressional district-level estimates for House elections in the 2006-2012 period. Panel C presents county-level estimates for gubernatorial elections in the 2006-2012 period. *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. Robust standard errors clustered at the county level (in Panels A and C) and congressional district level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Senate Elections						
Recalibrated × Post	0.295**	0.171*	0.289	0.059***	0.060***	0.067***
	(0.136)	(0.097)	(0.190)	(0.017)	(0.019)	(0.016)
County Votes		0.051**	0.004		0.000	0.000
		(0.025)	(0.009)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.606***	1.954***		-0.048***	0.026**
		(0.061)	(0.139)		(0.008)	(0.013)
County × Senate Seat Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.729	0.784	0.744	0.892	0.893	0.882
Number of observations	9,884	9,884	9,884	9,884	9,884	9,884
Panel B: House Elections						
Recalibrated × Post	0.371***	0.387***	0.395**	0.005	0.056	0.062
	(0.141)	(0.144)	(0.163)	(0.086)	(0.059)	(0.058)
District Votes		-0.042	-0.035		-0.104***	-0.102***
		(0.049)	(0.052)		(0.011)	(0.011)
Incumbent Share$_{t-1}$		-0.106	-0.122		-0.638***	-0.631***
		(0.089)	(0.093)		(0.033)	(0.032)
District Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.187	0.189	0.191	0.174	0.471	0.472
Number of observations	1,627	1,627	1,627	1,627	1,627	1,627
Panel C: Gubernatorial Elections						
Recalibrated × Post	0.380***	0.427***	0.552**	-0.016	-0.020	0.039
	(0.129)	(0.128)	(0.271)	(0.023)	(0.024)	(0.039)
County Votes		-0.254*	-0.304**		0.001	0.001**
		(0.135)	(0.119)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.253***	1.577***		-0.140***	-0.054***
		(0.079)	(0.130)		(0.010)	(0.013)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.632	0.674	0.62	0.857	0.865	0.863
Number of observations	5,964	5,964	5,964	5,964	5,964	5,964

Table 4: The Effect of Municipal Bond Ratings on Election Outcomes: Presidential and Mayoral Elections

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for presidential elections in the 2004-2012 period. Panel B presents county-level estimates for mayoral elections in California in the 2006-2012 period. *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. Robust standard errors clustered at the county level (in Panel A) and city level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Presidential Elections						
Recalibrated × Post	1.304***	0.547***	0.061	0.096***	0.087***	0.075***
	(0.255)	(0.137)	(0.286)	(0.012)	(0.011)	(0.012)
County Votes		-0.088	0.105**		-0.001	0.000***
		(0.068)	(0.050)		(0.000)	(0.000)
Incumbent Share$_{t-1}$		2.440***	2.883***		0.029***	0.036***
		(0.042)	(0.119)		(0.003)	(0.005)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.464	0.723	0.664	0.698	0.702	0.750
Number of observations	9,329	9,329	9,329	9329	9,329	9,329
Panel B: Mayoral Elections						
Recalibrated × Post	2.537	2.560*	1.700	0.164	0.163	0.382
	(1.539)	(1.508)	(1.592)	(0.577)	(0.495)	(0.549)
City Votes		-0.616*	-0.504*		-0.107**	-0.092
		(0.365)	(0.280)		(0.054)	(0.056)
Incumbent Share$_{t-1}$		0.148	-0.053		-0.679***	-0.868***
		(0.277)	(0.180)		(0.083)	(0.162)
City Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.611	0.626	0.714	0.019	0.304	0.473
Number of observations	266	266	266	266	266	266

Table 5: The Effect of Ratings News Searches

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), (4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Ratings News* is a dummy variable that takes a value of one if the increase in news searches for the term "credit rating" between the pre-recalibration period and the post-recalibration period is above the median, and zero if it is below the median. *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*$_{t-1}$ and *County Votes* (or *District Votes*). Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Elections		House Elections		Gubernatorial Elections	
	Incumbent Win	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	0.472***	0.077**	0.276**	0.082	-0.191	-0.180**
	(0.143)	(0.031)	(0.112)	(0.075)	(0.260)	(0.072)
Recalibrated × *Post* × *Ratings News*	0.359	0.112***	0.121	0.092	1.747***	0.429***
	(0.401)	(0.040)	(0.285)	(0.089)	(0.438)	(0.090)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.628	0.887	0.157	0.451	0.435	0.844
Number of observations	2,626	2,626	807	807	1,410	1,410

Table 6: The Effect of Municipal Bond Ratings on Election Outcomes by Year

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), (4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) presents county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *2010*, *2011* and *2012* are calendar year dummies that take the value of one in the years 2010, 2011 and 2012, 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*$_{t-1}$ and *County Votes* (or *District Votes*). Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Elections		House Elections		Gubernatorial Elections	
	Incumbent Win	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *2010*	-0.015	0.013	0.394**	0.005	0.473***	-0.028
	(0.175)	(0.024)	(0.189)	(0.076)	(0.171)	(0.028)
Recalibrated × *2011*					0.308**	-0.045
					(0.131)	(0.042)
Recalibrated × *2012*	0.301***	0.091***	0.379*	0.113	0.453	0.113*
	(0.115)	(0.024)	(0.210)	(0.082)	(0.533)	(0.063)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.784	0.893	0.189	0.471	0.674	0.865
Number of observations	9,884	9,884	1,627	1,627	5,964	5,964

Table 7: The Effect of State Income Taxes

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), (4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Tax* is the top marginal state income tax rate in 2010. *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*$_{t-1}$ and *County Votes* (or *District Votes*). Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Elections		House Elections		Gubernatorial Elections	
	Incumbent Win	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*	*Incumbent Win*	$\triangle$*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.250	-0.028	-0.005	0.139	-0.165***	0.178
	(0.245)	(0.043)	(0.083)	(0.175)	(0.062)	(0.412)
Recalibrated × *Post* × *Tax*	7.218*	1.514**	1.011	0.944	2.491***	4.345
	(4.094)	(0.604)	(1.232)	(2.941)	(0.840)	(6.650)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.784	0.893	0.473	0.187	0.865	0.674
Number of observations	9,884	9,884	1,620	1,620	5,964	5,964

Table 8: The Effect of Municipal Bond Ratings on Economic Outcomes

This table presents difference-in-differences estimates of county-level economic outcomes 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 statistical significance at the 1%, 5%, and 10% levels, respectively.

	Offer Yield (1)	*Issue Amount (log)* (2)	*Government Expenditures* (3)	*Tax Rate* (4)	*Government Employment* (5)	*Private Employment* (6)	*Non-Tradable Employment* (7)	*Construction Employment* (8)	*Income* (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	0.0873	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 9: Instrumental Variable Estimates

This table presents difference-in-difference instrumental variable estimates of the average yearly amount of bonds issued (by local government units within a county) during the electoral term divided by number of votes (*Issue Amount*) on election outcomes around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for gubernatorial elections in the 2006-2012 period. Columns (7) and (8) present county-level estimates for presidential elections in the 2004-2012 period. Panel A presents first-stage regression estimates of the *Issue Amount* on the *Recalibrated* × *Post* interaction variable. *Recalibrated* is the fraction of upgraded local government units in each county 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. Panel B presents second-stage regression estimates of the likelihood of incumbent win (columns (1),(3), (5) and (7)), and the difference between the incumbent vote share in the current election and the previous election (columns (2), (4), (6) and (8)). Controls include *Incumbent Share*$_{t-1}$ and *County Votes* (or *District Votes*). Robust standard errors clustered at the county (for Senate, gubernatorial and presidential elections) or congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: First Stage - Issue Amount

	Senate Elections		House Elections		Gubernatorial Elections		Presidential Elections	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Recalibrated × *Post*	0.640	2.805***	3.218**	4.102**	3.851**	6.272***	1.562**	1.626***
	(0.761)	(0.681)	(1.532)	(1.775)	(1.498)	(1.507)	(0.640)	(0.629)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No	No	No
County Fixed Effects	No	No	No	No	Yes	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-statistic	0.71	16.38	4.42	5.34	6.61	17.33	5.96	6.67
R^2	0.025	0.109	0.131	0.349	0.043	0.115	0.078	0.113
Number of observations	7,831	7,831	1,627	1,627	5,964	5,964	9,329	9,329

Panel B: Second Stage - Election Outcomes

	Senate Elections		House Elections		Gubernatorial Elections		Presidential Elections	
	Incumbent Win	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Issue Amount	0.375	0.025***	0.120*	0.014	0.111**	-0.003	0.350**	0.054***
	(0.446)	(0.009)	(0.071)	(0.014)	(0.053)	(0.004)	(0.158)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No	No	No
County Fixed Effects	No	No	No	No	Yes	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	3,264	7,831	1,627	1,627	5,666	5,964	9,329	9,329

Table 10: The Effect of Partisanship

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), 4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) present county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Democrat* is a dummy variable that takes a value of zero if the Democratic party is in power in 2010, and zero if the Republican party is in power in 2010. *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*$_{t-1}$ and *County Votes* (or *District Votes*). Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Elections		House Elections		Gubernatorial Elections	
	Incumbent Win	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.402***	-0.078**	-0.230	-0.169	0.257	-0.198***
	(0.140)	(0.037)	(0.301)	(0.109)	(0.195)	(0.034)
Recalibrated × *Post* × *Democrat*	0.865***	0.201***	0.891***	0.380***	0.282	0.294***
	(0.193)	(0.038)	(0.297)	(0.117)	(0.260)	(0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.772	0.886	0.196	0.494	0.674	0.867
Number of observations	6,806	6,806	1,627	1,627	5,964	5,964

Table 11: The Effect of Municipal Bond Ratings on Economic Outcomes by Political Party

This table presents difference-in-differences estimates of county-level economic outcomes 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 is the most voted in the county in the 2008 and 2012 elections, and zero if the Republican presidential candidate is the most voted in the county in the 2008 and 2012 elections. Counties with a change in the presidential candidate winner in the county between 2008 and 2012 ("swing" counties) are excluded from the sample. Controls include house price index and number of households. ***, **, and * indicate significance at the 1%, 5% and 10% level respectively.

	Offer Yield (1)	*Issue Amount (log)* (2)	*Government Expenditures* (3)	*Tax Rate* (4)	*Government Employment* (5)	*Private Employment* (6)	*Non-Tradable Employment* (7)	*Construction Employment* (8)	*Income* (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

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.

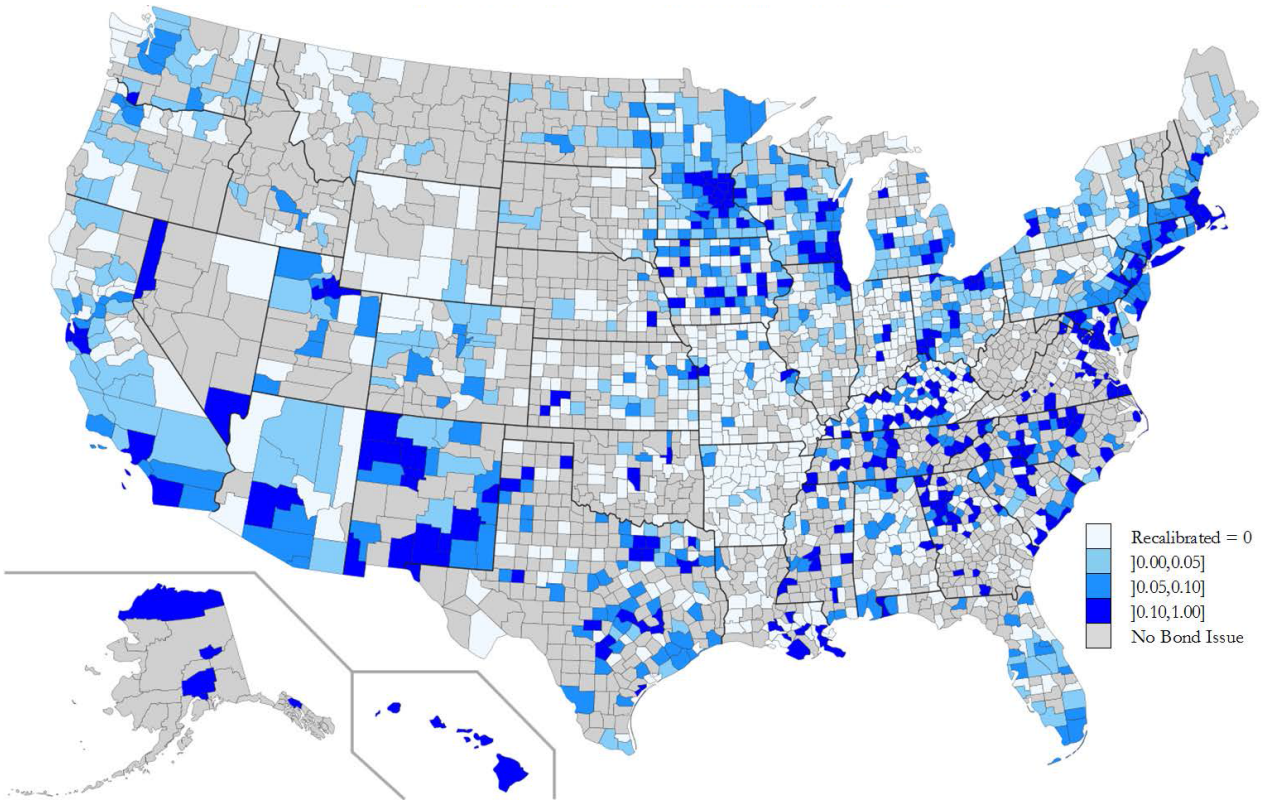


Figure 2: Moody's Ratings around the Recalibration

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

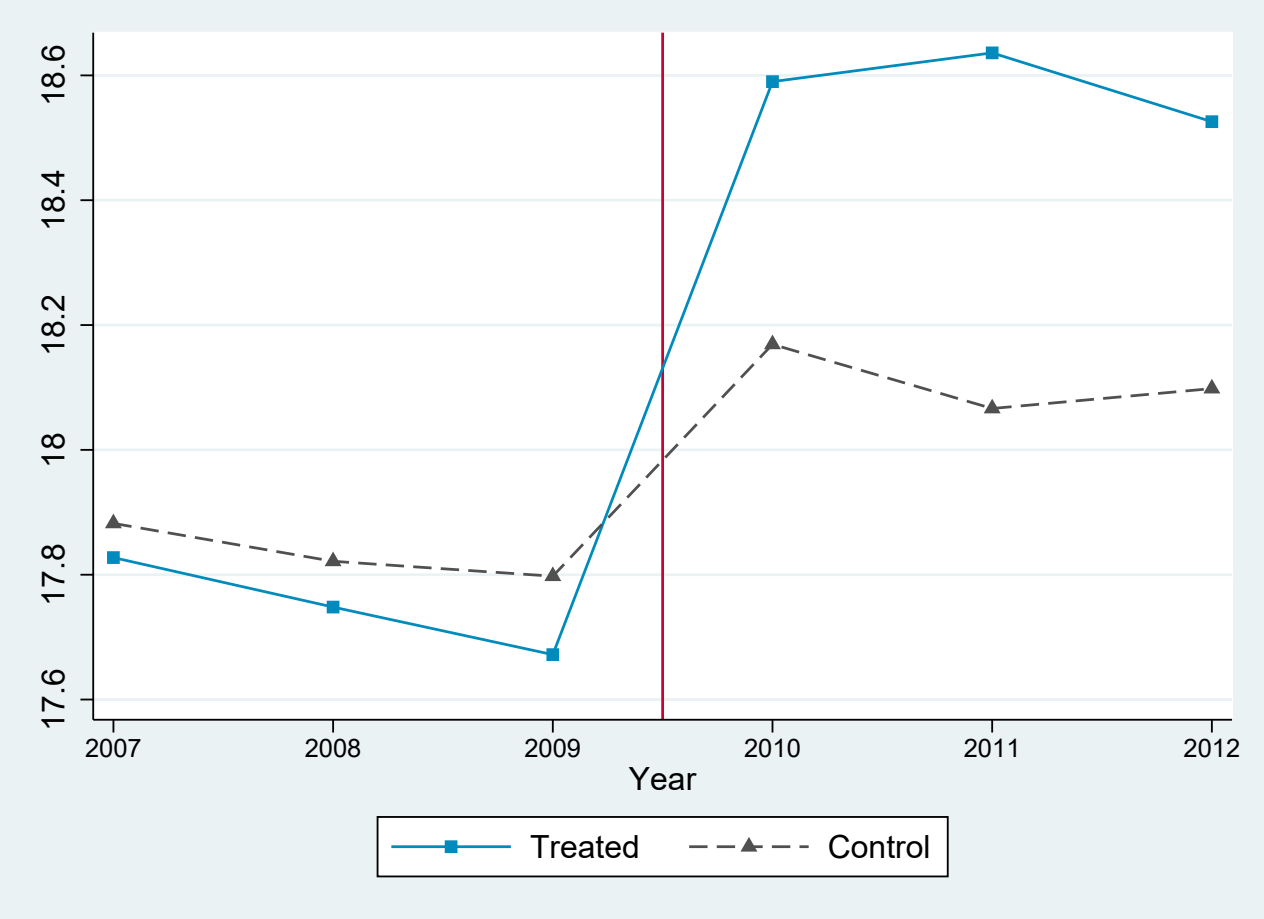


Figure 3: S&P Ratings around the Recalibration

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

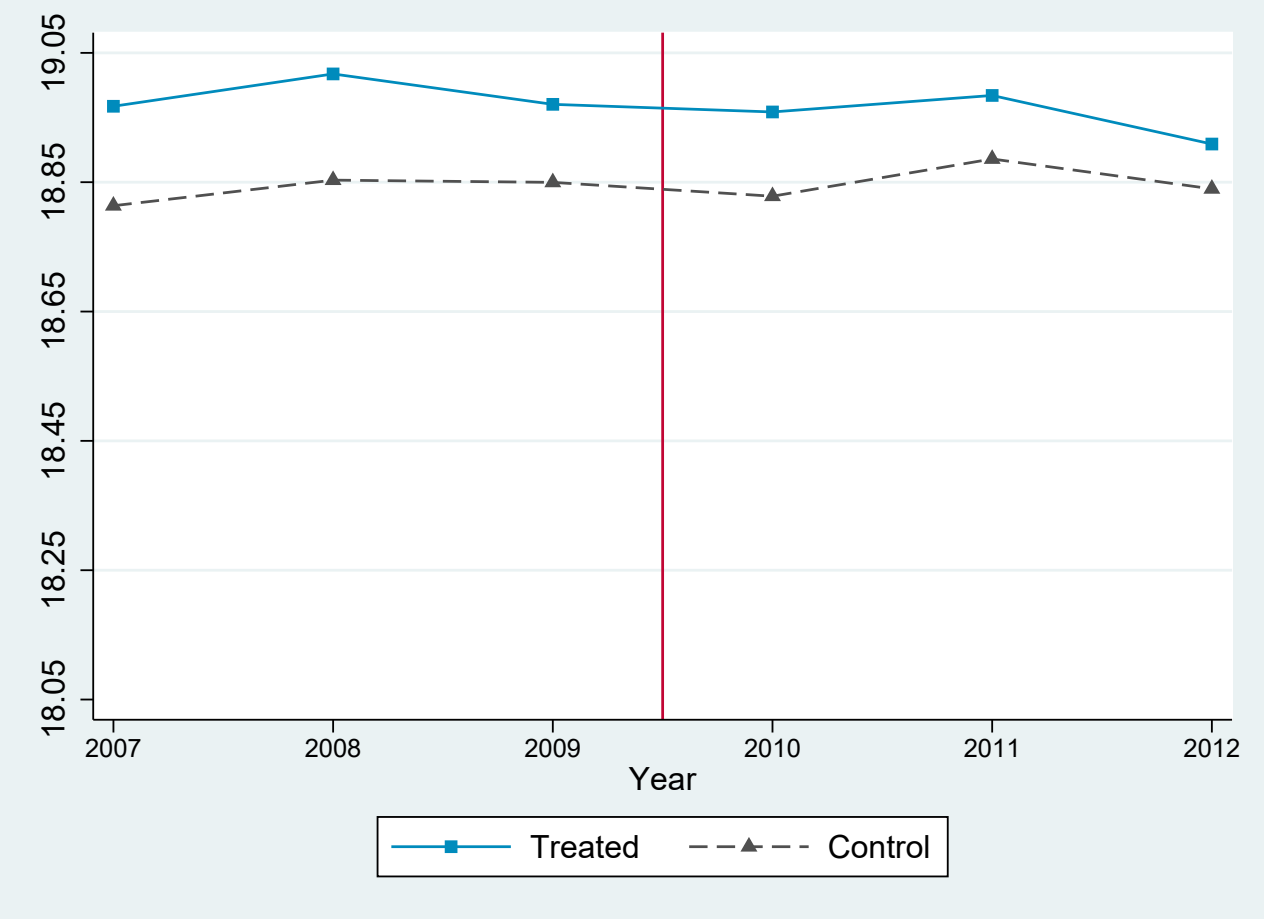


Figure 4: House Prices around the Recalibration

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. The sample consists of counties in the 2006-2012 period.

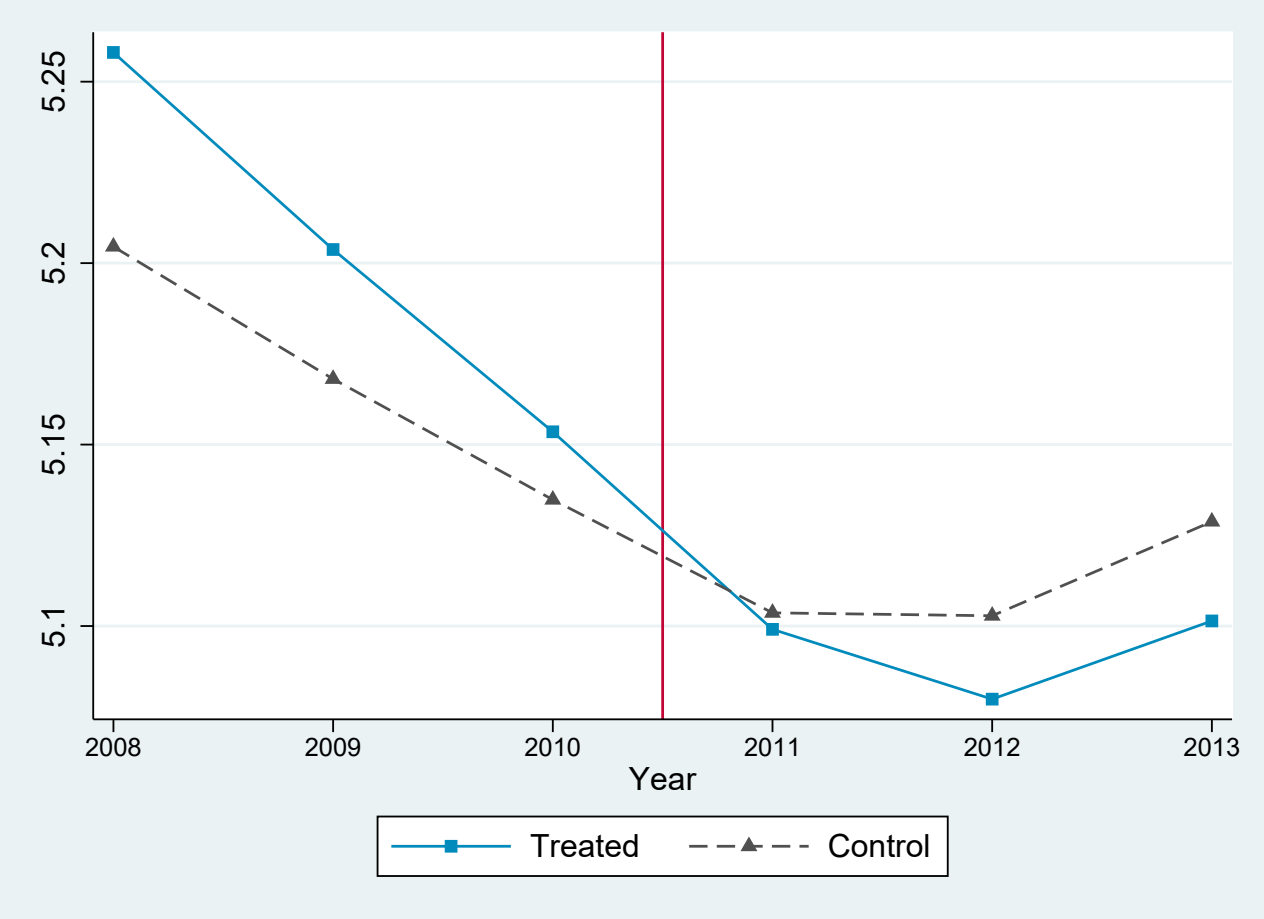
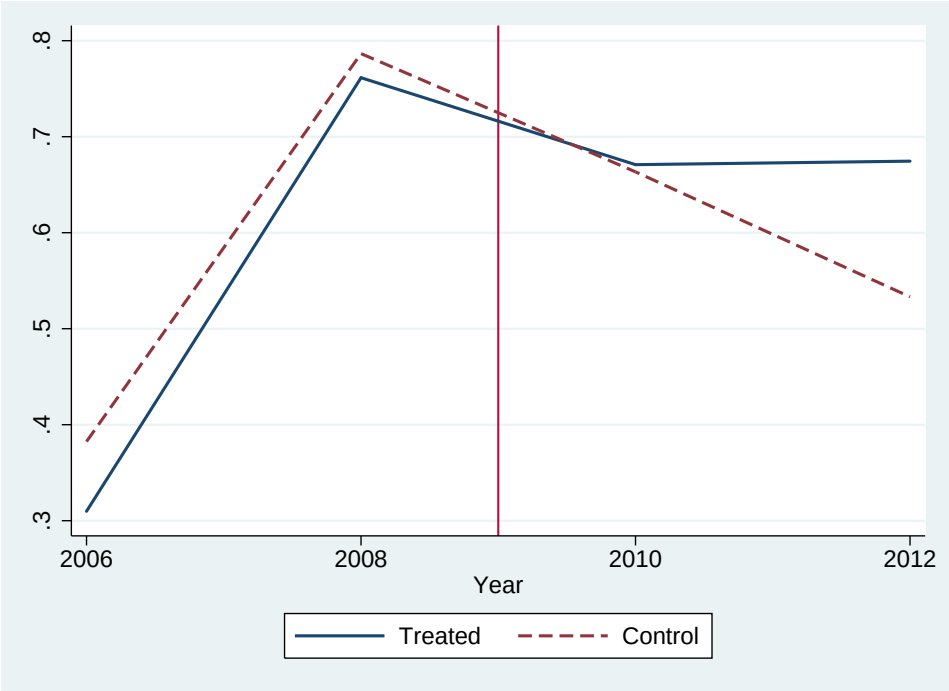


Figure 5: Senate Election Outcomes around the Recalibration

This figure shows the likelihood of incumbent win and the incumbent vote share in Senate elections 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. The sample consists of counties in the 2004-2012 period.

Panel A: Probability of Incumbent Win of Treatment and Control Groups



Panel B: Incumbent Vote Share for Treatment and Control Groups

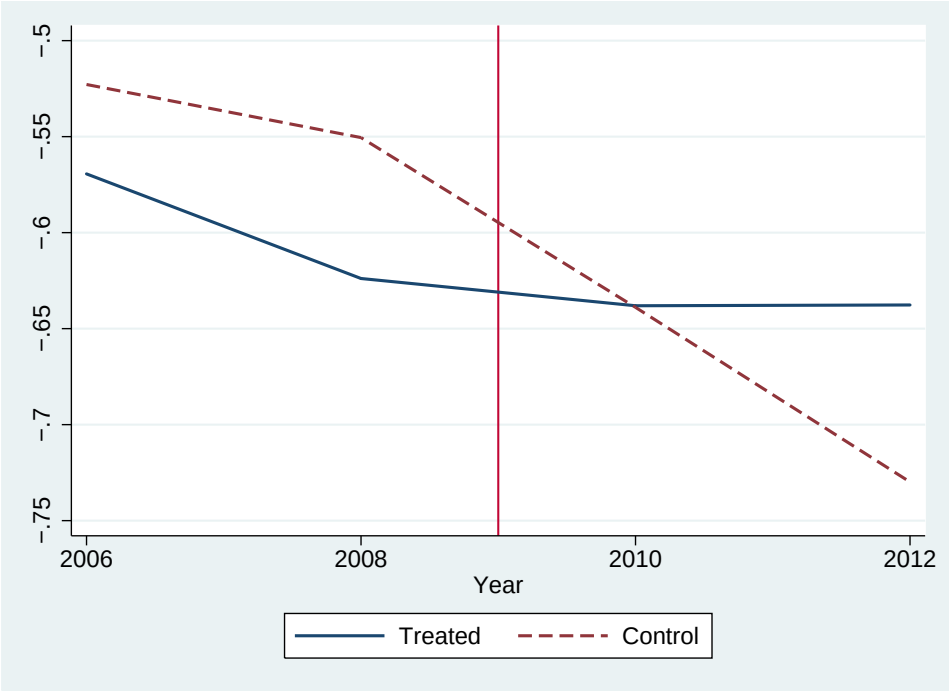
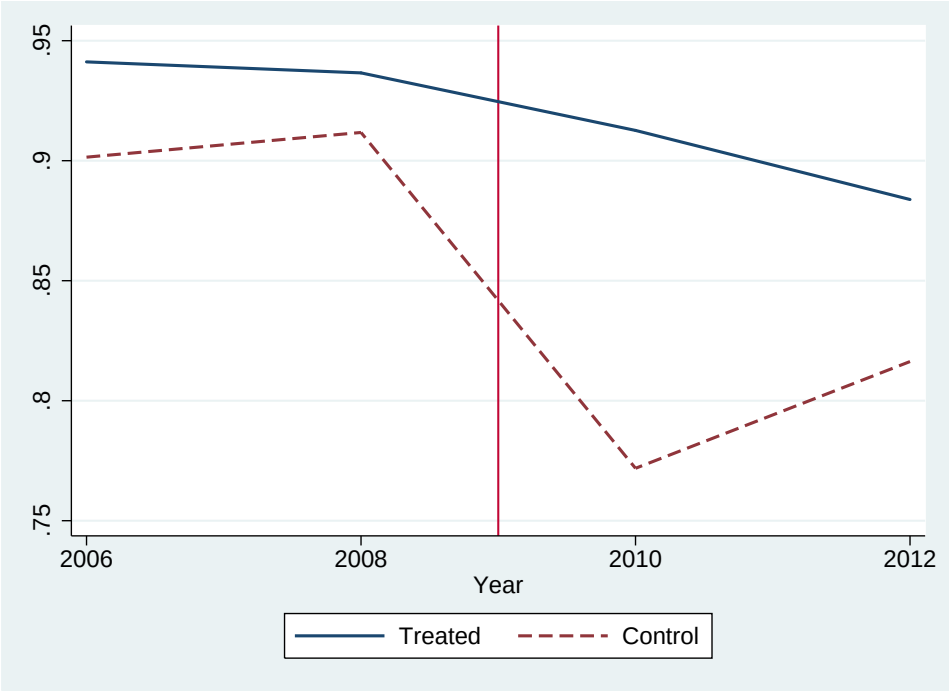


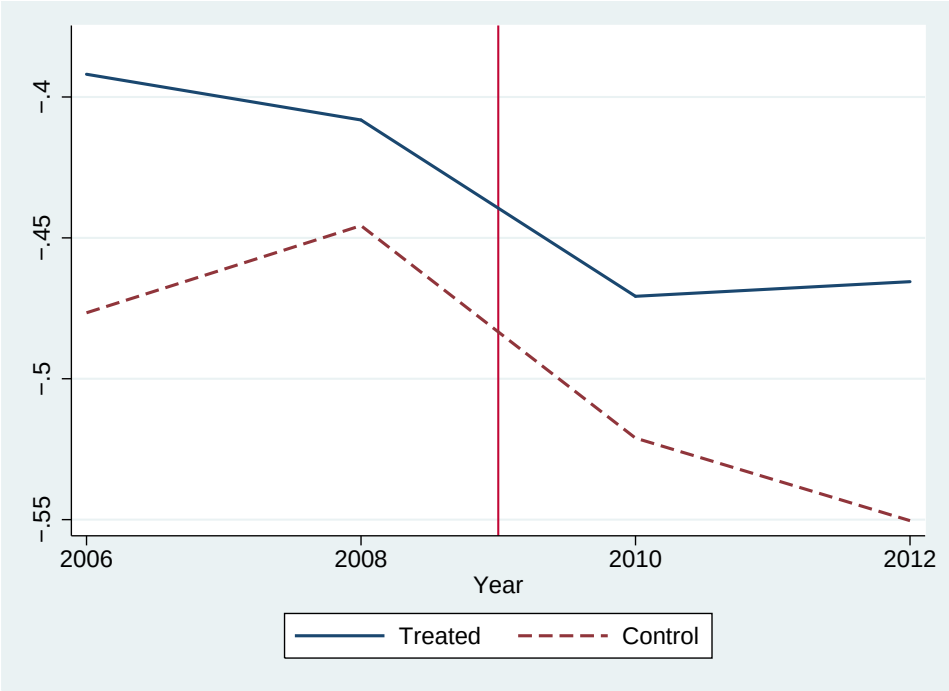
Figure 6: House Election Outcomes around the Recalibration

This figure shows the likelihood of incumbent win and the incumbent vote share in House elections 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. The sample consists of counties in the 2006-2012 period.

Panel A: Probability of Incumbent Win of Treatment and Control Groups



Panel B: Vote Share for Treatment and Control Groups



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

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April 12, 2017

Table IA.1: Senate, House and Gubernatorial Elections - Recalibrated Dummy

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for Senate elections in the 2004-2012 period. Panel B presents congressional district-level estimates for House elections in the 2006-2012 period. Panel C presents county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated Dummy* is a dummy variable that takes a value of one if there is at least one local government unit upgraded in the county or congressional district, 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. Robust standard errors clustered at the county level (in Panels A and C) and congressional district level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Senate Elections						
Recalibrated Dummy × Post	0.069***	0.040***	0.081***	0.008***	0.009***	0.010**
	(0.016)	(0.014)	(0.024)	(0.002)	(0.002)	(0.004)
County Votes		0.052**	0.005		0.000	0.001
		(0.025)	(0.009)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.601***	1.990***		-0.049***	0.028**
		(0.062)	(0.124)		(0.008)	(0.013)
County × Senate Seat Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.730	0.784	0.744	0.892	0.893	0.882
Number of observations	9,884	9,884	9,884	9,884	9,884	9,884
Panel B: House Elections						
Recalibrated Dummy × Post	-0.018	-0.018	0.004	0.032	0.041	0.051
	(0.085)	(0.086)	(0.082)	(0.034)	(0.033)	(0.032)
District Votes		-0.038	-0.030		-0.105***	-0.103***
		(0.049)	(0.052)		(0.010)	(0.011)
Incumbent Share$_{t-1}$		-0.102	-0.117		-0.631***	-0.625***
		(0.089)	(0.094)		(0.035)	(0.033)
District Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.184	0.186	0.187	0.173	0.466	0.467
Number of observations	1,627	1,627	1,627	1,627	1,627	1,627
Panel C: Gubernatorial Elections						
Recalibrated Dummy × Post	0.060***	0.060***	0.058	-0.004	-0.004	-0.011*
	(0.018)	(0.017)	(0.036)	(0.004)	(0.004)	(0.006)
County Votes		-0.248*	-0.293**		0.001	0.001***
		(0.142)	(0.129)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.248***	1.571***		-0.140***	-0.053***
		(0.079)	(0.127)		(0.010)	(0.013)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.632	0.674	0.615	0.857	0.865	0.863
Number of observations	5,964	5,964	5,964	5,964	5,964	5,964

Table IA.2: Senate, House and Gubernatorial Elections - Recalibrated Weighted by Amount of Bonds Issued

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for Senate elections in the 2004-2012 period. Panel B presents congressional district-level estimates for House elections in the 2006-2012 period. Panel C presents county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated Amount* is the fraction of upgraded local government units in each county or congressional district weighted by the amount of bonds issued by each local government unit. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Robust standard errors clustered at the county level (in Panels A and C) and congressional district level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Senate Elections						
Recalibrated Amount × Post	0.169*	0.103*	0.159	0.034***	0.034**	0.053***
	(0.089)	(0.055)	(0.146)	(0.012)	(0.013)	(0.011)
County Votes		0.052**	0.005		-0.000	0.000
		(0.025)	(0.009)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.607***	1.959***		-0.048***	0.025**
		(0.061)	(0.136)		(0.008)	(0.013)
County × Senate Seat Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.729	0.784	0.743	0.892	0.893	0.882
Number of observations	9,884	9,884	9,884	9,884	9,884	9,884
Panel B: House Elections						
Recalibrated Amount × Post	0.257**	0.271**	0.267**	-0.021	0.026	0.029
	(0.112)	(0.115)	(0.129)	(0.064)	(0.045)	(0.044)
District Votes		-0.042	-0.035		-0.103***	-0.102***
		(0.049)	(0.052)		(0.011)	(0.011)
Incumbent Share$_{t-1}$		-0.106	-0.122		-0.639***	-0.632***
		(0.089)	(0.093)		(0.033)	(0.032)
District Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.186	0.188	0.190	0.174	0.471	0.472
Number of observations	1,627	1,627	1,627	1,627	1,627	1,627
Panel C: Gubernatorial Elections						
Recalibrated Amount × Post	0.240***	0.266***	0.496***	-0.018	-0.020	0.021
	(0.078)	(0.076)	(0.179)	(0.015)	(0.016)	(0.032)
County Votes		-0.238*	-0.301**		0.001	0.001**
		(0.139)	(0.119)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.254***	1.582***		-0.140***	-0.054***
		(0.079)	(0.128)		(0.010)	(0.013)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.632	0.674	0.622	0.857	0.865	0.863
Number of observations	5,964	5,964	5,964	5,964	5,964	5,964

Table IA.3: Senate, House and Gubernatorial Elections - Sample of Urban Counties

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for Senate elections in the 2004-2012 period. Panel B presents congressional district-level estimates for House elections in the 2006-2012 period. Panel C presents county-level estimates for gubernatorial elections in the 2006-2012 period. *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. The sample is restricted to counties or congressional districts where the share of urban population is at least 50%. Robust standard errors clustered at the county level (in Panels A and C) and congressional district level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Senate Elections						
Recalibrated × Post	0.116	0.125	0.276	0.038*	0.039*	0.045***
	(0.169)	(0.122)	(0.210)	(0.020)	(0.020)	(0.017)
County Votes		0.035	0.001		-0.000	0.001
		(0.023)	(0.008)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.701***	1.915***		0.004	0.033***
		(0.088)	(0.158)		(0.010)	(0.013)
County × Senate Seat Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.688	0.749	0.746	0.900	0.900	0.901
Number of observations	4,045	4,045	4,045	4,045	4,045	4,045
Panel B: House Elections						
Recalibrated × Post	0.227*	0.211	0.217	0.009	0.059	0.063
	(0.135)	(0.133)	(0.138)	(0.107)	(0.074)	(0.072)
District Votes		0.043	0.063		-0.095***	-0.091***
		(0.056)	(0.060)		(0.012)	(0.013)
Incumbent Share$_{t-1}$		0.060	0.035		-0.586***	-0.572***
		(0.122)	(0.132)		(0.043)	(0.040)
District Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.197	0.199	0.204	0.175	0.430	0.437
Number of observations	1,071	1,071	1,071	1,071	1,071	1,071
Panel C: Gubernatorial Elections						
Recalibrated × Post	0.231	0.377**	0.618**	0.044	0.036	0.059
	(0.169)	(0.173)	(0.291)	(0.027)	(0.028)	(0.042)
County Votes		-0.308**	-0.285**		0.001	0.001**
		(0.137)	(0.121)		(0.001)	(0.000)
Incumbent Share$_{t-1}$		1.233***	1.532***		-0.116***	-0.044***
		(0.129)	(0.157)		(0.016)	(0.015)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.601	0.646	0.626	0.858	0.864	0.874
Number of observations	2,344	2,344	2,344	2,344	2,344	2,344

Table IA.4: Presidential and Mayoral Elections - Recalibrated Dummy

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for presidential elections in the 2004-2012 period. Panel B presents county-level estimates for mayoral elections in California in the 2006-2012 period. *Recalibrated Dummy* is a dummy variable that takes a value of one if there is at least one local government unit upgraded in the county, 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. Robust standard errors clustered at the county level (in Panel A) and city level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Presidential Elections						
Recalibrated Dummy × Post	0.017***	0.015***	0.023***	0.262***	0.075***	0.183***
	(0.001)	(0.001)	(0.002)	(0.033)	(0.022)	(0.051)
County Votes		-0.001	0.000***		-0.074	0.099*
		(0.000)	(0.000)		(0.069)	(0.051)
Incumbent Share$_{t-1}$		0.028***	0.038***		2.439***	2.813***
		(0.003)	(0.004)		(0.041)	(0.124)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.699	0.703	0.758	0.467	0.723	0.668
Number of observations	9,329	9,329	9,329	9,329	9,329	9,329
Panel B: Mayoral Elections						
Recalibrated Dummy × Post	0.168	0.174	0.456	0.061	0.013	0.065
	(0.338)	(0.330)	(0.299)	(0.131)	(0.108)	(0.090)
City Votes		-0.575	-0.460		-0.093*	-0.086
		(0.385)	(0.284)		(0.054)	(0.053)
Incumbent Share$_{t-1}$		0.180	-0.019		-0.682***	-0.875***
		(0.290)	(0.187)		(0.084)	(0.171)
City Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.596	0.611	0.712	0.014	0.298	0.470
Number of observations	266	266	266	266	266	266

Table IA.5: Presidential and Mayoral Elections - Recalibrated Weighted by Amount of Bonds Issued

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for presidential elections in the 2004-2012 period. Panel B presents county-level estimates for mayoral elections in California in the 2006-2012 period. *Recalibrated Amount* is the fraction of upgraded local government units in each county weighted by the amount of bonds issued by each local government unit. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Robust standard errors clustered at the county level (in Panel A) and city level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Presidential Elections						
Recalibrated Amount × Post	0.053***	0.049***	0.048***	0.642***	0.306***	-0.089
	(0.008)	(0.007)	(0.007)	(0.172)	(0.075)	(0.216)
County Votes		-0.001**	0.000***		-0.067	0.106**
		(0.000)	(0.000)		(0.068)	(0.050)
Incumbent Share$_{t-1}$		0.031***	0.038***		2.449***	2.910***
		(0.003)	(0.005)		(0.041)	(0.118)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.695	0.700	0.746	0.460	0.723	0.664
Number of observations	9,329	9,329	9,329	9,329	9,329	9,329
Panel B: Mayoral Elections						
Recalibrated Amount × Post	2.004	2.005	1.570	0.096	0.135	0.337
	(1.244)	(1.216)	(1.276)	(0.470)	(0.401)	(0.452)
City Votes		-0.608*	-0.506*		-0.102*	-0.088
		(0.362)	(0.270)		(0.054)	(0.056)
Incumbent Share$_{t-1}$		0.148	-0.070		-0.679***	-0.872***
		(0.280)	(0.180)		(0.084)	(0.165)
City Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.611	0.625	0.717	0.017	0.301	0.470
Number of observations	266	266	266	266	266	266

Table IA.6: Presidential and Mayoral Elections - Sample of Urban Counties

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1)-(3)) and the difference between the incumbent vote share in the current election and the previous election (columns (4)-(6)) around the recalibration event (April-May 2010). Panel A presents county-level estimates for presidential elections in the 2004-2012 period. Panel B presents county-level estimates for mayoral elections in California in the 2006-2012 period. *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. The sample is restricted to counties or congressional districts where the share of urban population is at least 50%. Robust standard errors clustered at the county level (in Panel A) and city level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Incumbent Win			*△Incumbent Share*		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Presidential Elections						
Recalibrated × Post	1.007***	0.259	-0.219	0.059***	0.048***	0.050***
	(0.327)	(0.178)	(0.318)	(0.012)	(0.011)	(0.012)
County Votes		-0.029	0.120**		-0.000	0.000***
		(0.068)	(0.051)		(0.000)	(0.000)
Incumbent Share$_{t-1}$		2.795***	2.877***		0.040***	0.033***
		(0.066)	(0.141)		(0.004)	(0.005)
County Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.325	0.688	0.663	0.741	0.750	0.789
Number of observations	3,746	3,746	3,746	3,746	3,746	3,746
Panel B: Mayoral Elections						
Recalibrated × Post	2.645	2.674*	1.701	0.189	0.187	0.384
	(1.615)	(1.581)	(1.613)	(0.593)	(0.507)	(0.550)
City Votes		-0.622*	-0.505*		-0.105*	-0.092
		(0.367)	(0.281)		(0.054)	(0.056)
Incumbent Share$_{t-1}$		0.148	-0.053		-0.674***	-0.867***
		(0.280)	(0.182)		(0.083)	(0.162)
City Fixed Effects	Yes	Yes	Yes	No	No	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Weighting	Equal	Equal	Votes	Equal	Equal	Votes
R^2	0.611	0.626	0.714	0.018	0.302	0.473
Number of observations	262	262	262	262	262	262

Table IA.7: The Effect of Crisis News Searches

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), (4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) presents county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Crisis News* is a dummy variable that takes a value of one if the increase in news searches for the term "financial crisis" between the pre-recalibration period and the post-recalibration period is above the median, and zero if it is below the median. *Post* is a dummy variable that takes a value of one for the 2010-2012 period, and zero for the period before 2010. Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Election		House Election		Gubernatorial Elections	
	Incumbent Win	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibration × *Post*	0.253	0.063**	0.155	0.087	0.307*	-0.056
	(0.174)	(0.029)	(0.134)	(0.059)	(0.185)	(0.035)
Recalibration × *Post* × *Crisis News*	-0.140	-0.002	0.098	-0.088	0.233	0.066
	(0.206)	(0.031)	(0.228)	(0.070)	(0.264)	(0.040)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.779	0.882	0.183	0.470	0.658	0.864
Number of observations	9,177	9,177	1,600	1,600	5,531	5,531

Table IA.8: The Effect of State Income Taxes - Sample of Republican Counties

This table presents difference-in-differences estimates of the likelihood of incumbent win (columns (1), (3) and (5)) and the difference between the incumbent vote share in the current election and the previous election (columns (2), 4) and (6)) around the recalibration event (April-May 2010). Columns (1) and (2) present county-level estimates for Senate elections in the 2004-2012 period. Columns (3) and (4) present congressional district-level estimates for House elections in the 2006-2012 period. Columns (5) and (6) presents county-level estimates for gubernatorial elections in the 2006-2012 period. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. *Tax* is the top marginal state income tax rate in 2010. *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*$_{t-1}$ and *County Votes* (or *District Votes*). The sample is restricted to counties or congressional districts where the Republican party is in power in 2010 for each election. Robust standard errors clustered at the county level (for Senate and gubernatorial elections) and congressional district level (for House elections) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Senate Elections		House Elections		Gubernatorial Elections	
	Incumbent Win	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*	*Incumbent Win*	△*Incumbent Share*
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated × *Post*	-0.199	-0.103**	-0.182	-0.128	0.109	-0.208***
	(0.308)	(0.051)	(0.185)	(0.107)	(0.377)	(0.072)
Recalibrated × *Post* × *Tax*	-3.337	1.687*	0.287	2.230	3.201	0.878
	(5.678)	(0.992)	(3.844)	(1.958)	(6.827)	(1.050)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
County × Senate Seat Fixed Effects	Yes	No	No	No	No	No
District Fixed Effects	No	No	Yes	No	No	No
County Fixed Effects	No	No	No	No	Yes	No
State × Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.877	0.818	0.435	0.623	0.732	0.814
Number of observations	3,335	3,335	662	662	2,653	2,653