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Abstract

We show that credit rating agencies can have a significant effect on election outcomes. 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 experience an increase in their likelihood of reelection and their vote shares. These rating upgrades improve voters' opinions about the incumbent and produce positive wealth effects through voters' holdings of local municipal bonds. In addition, rating upgrades cause an expansion of local governments' debt capacity that allows the incumbent to increase spending and improve local economic conditions.

JEL classification: D72, G24, H74
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1. Introduction

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The long-standing debate about the power of credit rating agencies (CRAs) has recently received additional attention due to the 2007–2009 financial crisis and the 2010–2012 European sovereign debt crisis. In 2012, Leonardo Domenici, a member of the European Parliament, claimed, "The debt crisis in the Eurozone has shown that CRAs 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 indicated by poll results (e.g., Gallup 2011). Regulators and academics have expressed similar concerns (Zingales 2015).[1] In this paper, we address the question of whether CRAs' 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 exogenous variation in municipal bond ratings due to Moody's recalibration of its Municipal Rating Scale in 2010. Before the recalibration, Moody's had a dual-class rating system. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality is to reach a weakened financial position that requires extraordinary support from a higher level of government to avoid default) among municipal bonds. 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 of 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 (i.e., bond issuers), corresponding to bonds worth more than $2.2 trillion in par value (nearly 70,000 bond issues). According to Moody's (2010), the recalibration simply unifies all bond ratings into a single scale and "does not reflect an improvement in credit quality or a change in our opinion [about the issuer]." Thus, the rating upgrades due to the recalibration are

[1] Paradoxically, one of the main reasons for the power of CRAs is rating-based regulations (e.g., Kisgen and Strahan 2010). In addition, investment management policies and practices often rely on ratings by restricting the portfolio holdings of institutional investors (e.g., Chen et al. 2014). In the aftermath of the 2007–2009 financial crisis, several regulatory initiatives have been taken to reduce the mechanical reliance on credit ratings by market participants (e.g., 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act; Financial Stability Board 2010, 2012).

uncorrelated with changes in the municipalities' intrinsic credit quality or with local and nationwide economic conditions.

The variation in ratings due to the recalibration provides us with a unique opportunity to examine the impact of the municipalities' ratings on election outcomes. It allows us to isolate the effects that are exclusively due to changes in municipal bond ratings from other confounding effects. The local governments that were not affected by the recalibration but experienced similar economic conditions to those of recalibrated local governments can be used as a control group. The control group includes local governments that were already properly calibrated vis-à-vis the Global Rating Scale and local governments without a 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 nonupgraded 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 voting share of the incumbent political party in the 2010–2012 elections relative to the 2006–2009 elections at the county level or (in the case of House elections) congressional district level. The recalibration affected bonds issued by counties and districts, as well as by other local government units within a county or district, 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 district level. Our (continuous) treatment variable is the fraction of local government units in each county or district whose outstanding bonds were upgraded due to Moody's recalibration.[3] The regressions also include county and state-year fixed effects to capture local economic conditions and any source of unobserved county-level heterogeneity.

We find that incumbent party candidates are more likely to be reelected in upgraded counties vis-à-vis nonupgraded counties. The incumbent effect is pervasive across different types of

[2] We exclude states as they are a higher-level government than counties and districts (i.e., states include multiple counties and districts).

[3] In the internet appendix that accompanies this paper, we show that our results are robust to alternative definitions of the treatment variable.

elections. Our results for Senate elections show that a 10 percent 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 percent in the likelihood of the incumbent winning the election in that county. For House elections, a 10 percent increase in upgraded local government units in a district is associated with a 3.9 percent increase in the likelihood of an incumbent being reelected. We find similar evidence in executive elections. A 10 percent increase in the fraction of upgraded local governments in a county is associated with an increase in the likelihood of the incumbent winning the election (at the county level) of 4.3 percent in gubernatorial elections and 5.5 percent in presidential elections. The corresponding increase in the likelihood of reelection is 26 percent in the case of mayoral elections in California. We also find evidence that incumbent party candidates receive more votes in upgraded municipalities vis-à-vis nonupgraded municipalities, but the 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 party's candidate is reelected in all types of elections. Overall, the results suggest that voters respond to positive news on the municipalities' credit quality by choosing continuity rather than change.

We find evidence that ratings affect election outcomes through three channels. First, we show that municipal bond ratings affect elections directly through their impact on the candidate's political discourse and the voter's perception of the incumbent's quality.[4] We study this hypothesis by exploring cross-sectional variation on Google searches for the term "credit rating" around the elections. An increase in Google searches for this term suggests that more people in the state are paying attention to ratings and might have their opinion about the candidate influenced by the local government upgrades. Our estimates indicate that the results are stronger in states with a surge in ratings-related Google searches. In addition, we explore the timing of the effects of ratings on

[4] There is anecdotal evidence of political candidates using credit ratings in their political discourse. For example, Donald Trump and Mike Pence referred to the rating Indiana's bonds (which had the maximum attainable rating of Aaa) during the 2016 presidential race.

election outcomes. While changes in the political discourse and voter's perception 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 on election outcomes, we find a significant effect in the year of the recalibration.

Second, we show that the recalibration also affects elections directly through wealth effects in voters' holdings of local municipal bonds. Investors that held upgraded municipal bonds experienced an appreciation in the value of their portfolios in 2010, which translates into an increase in their overall wealth. According to Cornaggia, Cornaggia, and Israelsen (2016), a lower bound for the postrecalibration cumulative abnormal return of upgraded bonds held by retail investors is approximately 50 basis points.[5] They estimate that households held approximately $1.87 trillion in municipal bonds in 2010. Therefore, municipal bond retail investors experienced an increase in wealth of about $9 billion. These voters' positive wealth shocks can in turn affect their voting behavior. We test this idea by exploring a feature of the municipal bond market: municipal bonds are exempt from state income taxes if the bond buyer is a state resident. This feature creates stronger incentives for ownership of municipal bonds in states with higher income tax rates. We find that the impact of the municipal rating upgrades on elections is more pronounced in states with higher income tax rates, which are plausibly those with higher local ownership of municipal bonds.

Finally, we find that ratings affect elections indirectly through local economic conditions. Municipal bond markets are an important source 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 generates cross-sectional variation in ratings

[5] Cornaggia, Cornaggia, and Israelsen (2016) estimate that the average post-recalibration cumulative return of bonds that were upgraded one notch is 50 basis points. Bonds that were upgraded two or three notches present a higher average cumulative return.

across local governments, which significantly affects local governments' financial constraints and debt capacity. Easier and cheaper access to financing can 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 experience a significant decrease in their borrowing costs in the municipal bond market after the recalibration (Cornaggia, Cornaggia, and Israelsen 2016). This decrease in borrowing costs allows local governments to increase bond issuance and spending (or reduce taxes). These changes in fiscal policy had positive spillovers to the private sector (Adelino, Cunha, and Ferreira 2017). We find that upgraded municipalities experience an increase in private employment and income. We establish a link between the improvements in local economic conditions and election outcomes using instrumental variable methods. We show that increases in the amount of bonds issued due to the recalibration significantly improve 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.[6]

To paint 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 Democratic incumbents improve their electoral chances significantly more than Republican incumbents do. However, the differences in election outcomes do not seem to be driven by differences in fiscal policy. Consistent with Ferreira and Gyourko (2009), we do not find significant differences in local level policy reactions to the rating upgrades. Both Democratic and Republican incumbents experience a decrease in bond yield, followed by an increase in municipal bond issues and a subsequent increase in government spending, private employment, and income. Our results indicate that both parties implement similar policies, but the electoral benefits of these

[6] An alternative potential channel for our results is that the upgrades reveal new information about politician power. However, this channel is unlikely to explain our results. The upgrades were identical within issuer type (i.e., municipality, township, school district) and pre-recalibration rating level. In addition, we find that the incumbent presidential candidate is more likely to win in upgraded counties. In presidential elections, the candidate is the same across all counties, and therefore our results cannot be explained by cross-sectional differences in politician power.

policies depend on the type of voter and their 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 find that (Standard and Poor's) S&P ratings of treatment and control groups follow similar trends both before and after the recalibration. If the recalibration by Moody's reflects 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 finding helps to rule out the possibility that the 2007–2009 financial crisis and the subsequent recovery may have affected the 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 a value of one when the county has at least one upgraded issuer, and a treatment variable weighted by the dollar amount of bonds issued. The results are robust to these alternative definitions.

Our research contributes to three strands of the literature. First, we contribute to the literature on the effect of economic conditions on election outcomes. In particular, there is a long-standing debate about whether voters penalize or reward budget deficits and government spending. The literature has traditionally provided evidence of a negative correlation between government spending and election outcomes (e.g., Niskanen 1975; Peltzman 1993; Matsusaka 2004). More recent research finds that voters reward government spending (e.g., Levitt and Snyder 1997; Akhmedov and Zhuravskaya 2004; Veiga and Veiga 2007; Sakurai and Menezes-Filho 2008; Jones, Meloni, and Tommasi 2012; Litschig and Morrison 2013). In addition, Bagues and Esteve-Volart (2016) show that exogenous good economic conditions (driven by a cash windfall brought by 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 deficit-financed spending. Our findings also raise the possibility of "pay-for-luck" in the electoral

process as the ratings recalibration is outside of the politicians' control.[7]

Second, we provide a novel link between credit ratings and political elections. There is vast evidence that ratings affect corporate actions (e.g., Kisgen 2006; Kisgen and Strahan 2010; Baghai, Servaes, and Tamayo 2014; Begley 2015; Almeida et al. 2017). Previous research has shown that municipal bond ratings affect municipalities' financing and economic condition (Adelino, Cunha, and Ferreira 2017; Cornaggia, Cornaggia, and Israelsen 2016). To the best of our knowledge, we are the first to provide causal evidence that CRAs can influence incumbents' chances of reelection.

Finally, we contribute to the literature on the effects of political partisanship on public policies and voting behavior. The literature provides evidence that the legislative power is highly partisan (Besley and Case 2003; Lee, Moretti, and Butler 2004). However, Ferreira and Gyourko (2009) find no evidence of a 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 implement similar government spending increases and tax cuts following the recalibration even in nonclosely contested elections. However, our results suggest that Democratic voters react more favorably to a fiscal expansion than their Republican counterparts.

2. Methodology and Data

2.1 Recalibration

Moody's had a dual-class rating system until the ratings recalibration in 2010. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality is to reach a weakened financial position that requires extraordinary support from a higher level of government to avoid default). On the other hand, Moody's Global Rating Scale is designed to measure expected losses (i.e., default probability and loss given default) among sovereign bonds, corporate bonds, and

[7] A large literature studies the effects of compensation for luck in the pay of corporate executives (e.g., Bertrand and Mullainathan 2001). However, "pay for luck" may not necessarily be inefficient (e.g., Edmans and Gabaix 2009; Gopalan, Milbourn, and Song 2010).

structured finance products (Moody's 2007). Moody's (2009) attributes its dual-class rating system to the preferences of the highly risk-averse investors in municipal bonds. According to the US flow of funds accounts in 2010, households owned 50 percent of municipal bonds, followed by money market funds with 10 percent and insurance companies with 9 percent. In contrast, households owned only 19 percent of corporate and foreign bonds.

Moody's intention to map municipal bond ratings into the Global Rating Scale dates back to at least 2002 (Moody's 2002) and is mentioned in a variety of publications over the years. Finally, in March 2010, Moody's announced a recalibration of its Municipal Rating Scale to align it with the Global Rating Scale (Moody's 2010). In April and May of 2010, over a four-week period, Moody's announced how the municipal bond ratings would be affected by the recalibration, resulting in a zero- to three-notch upgrade of nearly 70,000 ratings.

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. An important aspect of this recalibration is that not all municipal bond issues were upgraded in the recalibration, and those bonds can therefore be used as control group. Some sectors were already properly calibrated vis-à-vis the global scale; in particular housing, healthcare did not see a change in ratings. In addition, bonds with higher ratings (at or above Aa3) on the Municipal Rating Scale were less likely to be recalibrated than those with lower ratings (below Aa3); bonds with the maximum attainable rating (Aaa) in the municipal scale could not be upgraded. Of course, local governments without Moody's ratings or outstanding bonds were not subject to recalibration and can also be used as a control group.

Moody's (2010) explains that the recalibration is intended to enhance the comparability of ratings across asset classes, and does not indicate a change in the credit quality of the 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 1 shows the effect of the recalibration on Moody's ratings 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 undergoes a sharp increase in Moody's ratings after 2010, a difference that persists for up to three years. In contrast, the S&P ratings for the exact same municipalities do not experience any differential changes 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. In short, the figure provides evidence that Moody's recalibration does not reflect a change in issuers' credit quality and therefore is an important validation of our identification strategy.

To validate our exclusion restriction further, we compare the changes in the house price index of treatment and control groups before and after the recalibration. Figure 2 shows that there are no significant differential effects in the Federal Housing Finance Agency's House Price Index 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.

We obtained a list of recalibrated bond issues from Moody's. The list contains the rating of each bond issue before and after the recalibration, with the change in rating ranging from zero to three notches. The recalibration comprised 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 local 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 (state, city, townships, school district, etc.) level. The treatment group contains local government units whose outstanding bonds were upgraded by at least one notch during the Moody's 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 congressional district that were upgraded during the Moody's recalibration (*Recalibrated*).

Figure 3 shows a map of the United States with the terciles of the treatment variable (*Recalibrated*), among those counties with nonzero values. There is a variation both in the intensity of the treatment variable and the location of treated counties across the United States.

2.2 Election Outcomes

We obtain voting data for US House, Senate, gubernatorial, and presidential elections at the county level for the 2004–2012 period from David Leip's website.[8] These data have been used in previous research (e.g., Gentzkow, Shapiro, and Sinkinson 2011). The data contain information on total votes by political party or candidate. There is 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.[9]

Elections for the House of Representatives, 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 purposes.

[8] The data are available at http://uselectionatlas.org.
[9] The data are available at https://www.csus.edu/calst/california_election_data_archives.html.

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 serves a two-year term. There is large variation in the number of congressional districts by state, as some states contain several congressional districts, while others states contain only one.[10] Senators are elected at the state level and serve six-year terms. The terms are staggered so that approximately one-third of the seats are up for election every two years. Presidents serve four-year terms. In the case of House elections, the 2006 and 2008 elections are included in the pretreatment period, and the 2010 and 2012 elections are included in the posttreatment period.[11] In the case of Senate elections, the 2004, 2006, and 2008 elections are included in the pretreatment period, and the 2010 and 2012 elections are included in the posttreatment period. In the case of presidential elections, the 2004 and 2008 elections are included in the pretreatment period, and the 2012 election is included in the posttreatment period.

Elections for governors and mayors do not occur in even years only. Governors are elected by state and serve four-year terms (with the exception of Vermont and New Hampshire, where terms are two years long). Mayors are elected by city and serve four-year terms. In the case of gubernatorial and mayoral elections, the pretreatment period is 2006–2009, and the posttreatment 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 is the most-voted candidate 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

[10] For example, California contains 53 congressional districts. But Alaska, Delaware, Montana, North Dakota, South Dakota, Vermont, and Wyoming each have one congressional district.
[11] Given that the 2010 elections took place on November 2, it is included in the post-recalibration period.

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 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 election data to the recalibration data to obtain our measure of the degree to which the upgrades 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. However, there is no one-to-one mapping between counties and congressional districts.[12] In some cases, a county contains several districts, while in other cases, districts encompass multiple counties. We match each district to the corresponding counties using a bridge provided by the US Census Bureau.[13] 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 (*Cong. Distr. 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 for election outcomes and treatment variables of treatment and control groups 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 and districts with above-median *Recalibrated*, and the control group includes

[12] For example, the 53 congressional districts of California are associated with 58 counties.
[13] This bridge can be obtained at https://www.census.gov/geo/maps-data/data/cd_state.html.

counties and districts with below-median *Recalibrated*. In the case of Senate, gubernatorial, and presidential elections, the median of the *Recalibrated* variable is zero. In columns 7 and 8, we present 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 county size (number of county votes, congressional district votes and city votes) and state-by-year fixed effects; these controls are included in our regression tests. Although the raw differences show some statistically significant differences between treatment and control groups prior to the event (column 7), these differences lose statistical power as well as economic significance when we control for size and state-by-year fixed effects (column 8). This leads us to conclude that treatment and control groups were comparable prior to the Moody's recalibration.

2.3 Municipal Bond Markets

The municipal bond issues (primary market) data come from the Ipreo i-Deal new issues database. The sample period is from April 2007 to March 2013, which corresponds to the three-year period before the recalibration and the three-year period afterward. We restrict the sample to new bond issues rated by Moody's and local government units that issued bonds during the three-year period before the recalibration.[14] 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 percent 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 US Census Bureau's Annual Survey of State and Local Government

[14] 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.

Finances. The data include revenues and expenditures of individual local government units within each county and district. The sample period is from 2007 to 2013, which corresponds to the three-year period before the recalibration and the three-year period afterward. The sample includes local government units that are present in all years of the sample period and covers more than 90 percent 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 a sample of local governments is used in the other years. Government employment is measured as full-time equivalent employees at local government units in each county as 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).[15]

We obtain data on private employment by county from the 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).[16]

In the economic outcomes regressions, we control for other factors that are important determinants of local economic conditions. We include yearly changes in house prices (to capture the severity of the post-2006 downturn in each county), as well as the number of households. The housing prices come from the Federal Housing Finance Agency's House Price Index data at the

[15] 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 only 1,618 counties, which corresponds to about half of the counties in the United States.

[16] 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.

metropolitan statistical area level. The HPI is a weighted repeat-sales index that measures the average price changes in repeat sales or refinancing on the same properties.[17] 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. The treatment group includes counties with above-median *Recalibrated*, and the control group includes counties with below-median *Recalibrated*. The table also presents the differences between treatment and control groups. Consistent with table 1, counties in the treatment group are larger than counties in the control group as measured by local government expenditure, local government employment, private employment, or income. We present raw differences in means between treatment and control groups, as well as differences after adjusting for county size (logarithm of the number of households) and state-by-year fixed effects; these controls are included in our regression tests.[18] This adjustment controls for size and regional heterogeneity in a given year between treatment and control group, and after making it, the differences in levels of economic variables are no longer positive and statistically significant. More importantly, the treatment variable (*Recalibrated*) is not affected by this adjustment, which indicates that differences in size do not seem to be influencing the treatment selection. Additionally, the growth rates of outcome variables in the pretreatment period are similar across the two groups, except for government expenditures (although economically small). We conclude that preexisting differential trends between treatment and control groups are unlikely to explain our results.

[17] Whenever the MSA house price index is missing information, we complement the data with state-level house price indices from the FHFA.

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

3. The Impact of Credit Ratings on Election Outcomes

We estimate (reduced form) regression models in which we use rating changes due to Moody's recalibration of its Municipal Rating Scale as a source of exogenous variation in municipal bond ratings. We start our analysis by studying the impact of ratings upgrades on the likelihood of the incumbent party candidate wining the Senate, House, or gubernatorial elections using the following regression model (at the county level or congressional district level in the case of House elections):

$$Incumbent\ Win_{it} = \beta \times Recalibrated_i \times Post_{it} + X_{it} + \alpha_i + \gamma_{state,t} + \varepsilon_{it}, \quad (1)$$

where *Recalibrated* is the fraction of upgraded local governments in a county or district, and *Post* is a dummy variable that takes a value of one after the recalibration event in April–May 2010, and zero before the recalibration event. To account for any time-invariant unobserved heterogeneity at the county or 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 into account any macroeconomic conditions and other time trends that could be affecting election outcomes. The interaction term *Recalibrated* × *Post* is the difference-in-differences estimate of the effect of ratings upgrades on election outcomes. Specifically, we estimate the change in election outcomes within an upgraded municipality relative to the change in a nonupgraded 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). In addition, some regressions are weighted using the number of votes in a county/district to account for the possibility that size could be correlated with voting behavior. Standard errors are clustered at the county or district levels.

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., the estimates of a linear probability model). In panel A, we examine the

effect of ratings upgrades on Senate elections using the regression in equation (1) at the county level. In column 1, the interaction term *Recalibrated* × *Post* coefficient is positive and significant, which indicates that the recalibration has a disproportionate 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 the lag share 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 percent increase in the fraction of upgraded local governments in a county (which corresponds to about one standard deviation increase in the *Recalibrated* variable) leads to an increase of 1.7 percent in the probability of *Incumbent Win*. Columns 4–6 present results in which the change in *Incumbent Share* is the dependent variable. The interaction term *Recalibrated* × *Post* coefficient is positive and significant in all specifications. The estimate in column 5 implies that a 10 percent increase in the *Recalibrated* variable increases the *Incumbent Share* by 0.6 percentage points. We conclude that candidates affiliated with the incumbent party benefit from an increase in their probability of being the most-voted candidate in the county in Senate elections after the recalibration.

Figure 4 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 follow similar trends before the recalibration. After the recalibration, we see a significantly higher probability of *Incumbent Win* and *Incumbent Share* for the treatment group versus the control group.

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 congressional district). In columns 1–3, the interaction term *Recalibrated* × *Post* coefficient is positive and significant, which indicates that incumbents that enter a House election in districts with a higher fraction of local government units upgraded are more likely to be reelected. The estimate in column (2) implies that a 10 percent increase in the fraction of local governments upgraded in a district leads to a 3.9 percent increase

in the probability of incumbent reelection. While the estimates are not statistically significant in columns 4–6, the estimates have similar magnitudes to those in panel A and consistently indicate a differential increase in the *Incumbent Share* of the treatment group versus the control group 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 House elections. The two groups follow similar trends before the recalibration, and we then 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 are more likely to be elected in counties with a higher fraction of upgraded local governments after the recalibration. The estimate in column 2 implies that a 10 percent increase in in the *Recalibrated* variable leads to a 4.3 percent increase in the probability of reelection.

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 our treatment variable with a dummy that takes a value of one if at least one local government unit within the county is upgraded. In table IA.2, we use a treatment variable weighted by the dollar amount of bonds upgraded. The results using the alternative treatment variables are quantitatively similar to those in table 3. In table IA.3, we restrict the sample to urban counties (counties with a fraction of urban population above the median) to make sure that our results are not driven by a lack of comparability between treatment and control counties. Although the reduction in sample size makes the results slightly weaker, the estimates are still economically significant.

Next, we study the effect of ratings on presidential (panel A) and mayoral (panel B) 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 with a higher fraction of upgraded local governments. In columns 1–6, the interaction term *Recalibrated* × *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 has a disproportionate 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 percent increase in the *Recalibrated* variable in a county increases the probability of a Barack Obama win (at the county level) by 5.5 percent. In addition, the estimate in column 5 implies that a 10 percent increase in the *Recalibrated* variable in a county increases Barack Obama's vote share by 0.9 percentage points.

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* × *Post* coefficient is positive and significant, which indicates that the probability of reelection increases for candidates that happen to be in the mayoral office at the time of the recalibration. The estimate in column 2 implies that a 10 percent increase in the fraction of upgraded local governments increases the probability of reelection by 26 percent. 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 due to 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 the county has one local government unit upgraded. In table IA.5, we weight the treatment variable by the dollar amount of bonds upgraded. The results remain quantitatively similar to the alternative treatment definitions. In table IA.6, we restrict the sample to urban counties. The results are qualitatively similar for both the presidential and mayoral elections with the exception of the mayoral election results in the sample of urban counties.

Overall, we show that CRAs affect election outcomes. We find that candidates affiliated with

political parties that are in power at the time of the recalibration experience an increase in their probability of reelection and vote share. Our results suggest that politicians are rewarded for luck, as the changes in ratings due to the recalibration are exogenous and outside of their control.

4. How Do Credit Ratings Influence Elections?

In this section, we investigate how municipal bond ratings affect election outcomes. Ratings may affect election outcomes through a direct wealth effect due to an increase in the value of municipal bonds held by voters. In addition, politicians could use better ratings as a certification of their own quality, in which case ratings could be part of the political discourse during the campaign. 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 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 portfolios at 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 postrecalibration cumulative abnormal return of upgraded bonds held by retail investors is approximately 50 basis points, and that households held approximately $1.87 trillion of municipal bonds in 2010. Therefore, municipal bond investors experienced an increase in their wealth of about $9 billion. The wealth effects are economically meaningful. These voters' positive wealth shocks can in turn affect their voting behavior, which may explain our results.

The effects would be stronger if the ownership of specific municipal bonds is segmented in a specific region, and thus upgrades lead 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 bond buyer is a state resident. This exemption creates a stronger incentive for municipal

bond ownership in states with higher income tax rates. In states with no state income taxes (e.g., Florida), there are smaller incentives to invest in local municipal bonds than in states with high state income taxes (e.g., California). Thus, we rely on state income taxes as a proxy for the size of wealth effects associated with the recalibration. We test whether the incumbent effect is stronger in states with higher income taxes, and presumably with higher holdings of local municipal bonds, relative to states with lower income taxes.

Table 5 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* × *Post* × *Tax* 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 shows that CRAs influence election outcomes through an effect on the distribution of wealth effects across states.

Democratic states traditionally have higher income taxes. To rule out the possibility that Democrats responding more positively to expenditure is driving our results, we perform a robustness test in which we restrict our sample to counties in Republican states. Table IA.7 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* × *Post* × *Tax* coefficient indicates that the impact of municipal ratings on election outcomes is stronger in Republican constituencies with high income taxes than in Republican constituencies with low income taxes.

4.2 Political Discourse and Voter Perception

The impact of municipal ratings on election outcomes may be due to a change in voters' perception of the incumbent politician's quality. If a higher rating is associated with responsible budgeting

practices and good policies, a rating upgrade could lead to a change in voting behavior even in the absence of any real changes in the economy. This is especially true if voters attribute recalibration-related changes in ratings to skill rather than luck.

Anecdotal evidence suggests that credit ratings are used in political discourse as a way to persuade electors of the economic acumen of candidates. During one of his first interviews as the 2016 Republican presidential nominee (*60 Minutes*, July 17, 2016), Donald Trump pointed out the AAA credit ratings of bonds in Indiana, where Mike Pence was governor, as an indication of the vice presidential candidate's political quality: "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." Pence also used Indiana's credit rating as a selling point when he introduced himself at the Republican National Convention on July 20, 2016: "We in Indiana have . . . the highest credit rating in the nation." Interestingly, Indiana had held this rating since 2008, prior Pence's arrival as governor in 2012, which suggests that politicians may be tempted to use improvements in ratings in their political discourse even if their actions were not the cause of the upgrade.[19]

To test whether municipal bond rating upgrades affect the public's perception of politicians, we collect data from Google Trends on the evolution of news searches for the expression "credit rating" by state in the 2006–2012 period. We focus on May to November, when the searches are most likely to be related to political campaigns. Because the term "credit ratings" is not a popular search term, there are several states with zero searches in all years of the sample. We therefore focus on the 11 states that have nonzero entries on Google Trends in at least one year of our sample.[20] We create a dummy variable (*News*) that takes a value of one if the increase in news searches for the term "credit rating" from before the recalibration (2006–2009) to after the recalibration (2010–2012) is above the median, and takes a value of zero otherwise. States with an

[19] There are other examples of politicians using ratings in their political discourse: 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, Maine, 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, just to name a few.
[20] The 12 jurisdictions with data on Google Trends are California, DC, Florida, Illinois, Massachusetts, New Jersey, New York, Oregon, Pennsylvania, Texas, and Virginia.

increase in news searches for ratings are likely to be the ones where voters pay closer attention to ratings as indicative of the quality of politicians.

We test whether the impact of the recalibration on election outcomes is stronger in regions where news searches related to ratings have an above-median increase. Table 6 presents the results for Senate, House, and gubernatorial elections. The explanatory variable of interest is the triple interaction term *Recalibrated* × *Post* × *News*, which measures the effect of ratings on election outcomes in states with high new searches versus states with low new 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 crisis. We repeat our tests using a *News Crisis* dummy variable defined based on searches for the term "financial crisis" instead of the term "credit ratings." Table IA.8 presents the results. We find that states with an increase in searches for "financial crisis" did not experience a stronger effect of ratings on election outcomes. In fact, the coefficient on the triple interaction term *Recalibrated* × *Post* × *News Crisis* is insignificant and even negative in some specifications.

We also investigate the timing of the effects of ratings on elections. A direct effect of ratings (i.e., wealth effects, political discourse, and voter's perception of candidate quality) would occur faster on elections that take place in the year of the recalibration, while an effect through an expansionary fiscal policy and improvement in economic conditions would occur with a lag.

Table 7 presents the effects of rating upgrades on elections by year. The coefficients of interest are the interaction between the *Recalibrated* variable and the event-year dummies from 2010 to 2012. 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, our results suggest that one of the channels by which municipal rating upgrades may affect election outcomes is through wealth effects and their use as a political

weapon for their impact on voters' perceptions of the candidates' quality.

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 reducing taxes). In turn, this fiscal policy can improve local economic conditions, which translates 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. Moody's recalibration is associated with a significant decrease in the offer yield of an upgraded municipality (column 1), consistent with the evidence in Cornaggia, Cornaggia, and Israelsen (2016). The decrease in borrowing costs allowed local governments to significantly increase their dollar amount of bonds issued (column 2). Columns 3–5 show that the increase in dollar amount of bonds issued allowed local governments to increase their expenditures, increase government employment, and reduce taxes. Columns 7–9 show that the increase in government expenditures had positive spillovers to the private sector. Recalibrated counties experienced an increase in private employment and income. The effect is particularly strong in the nontradable sector (retail, food, and accommodation: NAICS codes 44–45 and 72), which is more dependent on local demand (Mian and Sufi 2014; Adelino, Ma, and Robinson 2016). These findings are consistent with those in Adelino, Cunha, and Ferreira (2017).

To study the relation between government bond financing and election outcomes, we implement an instrumental variables approach 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 encompassing 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 experience an increase in the amount of bonds issued. In the second-stage regression, we measure the impact on election outcomes of the increase in government bond financing. We estimate the following regression model at the county and district levels (first stage):

$$Issue\ Amount_{it} = \alpha + \beta \times Recalibrated_i \times Post_{it} + \propto_i + \gamma_{StateXt} + \varepsilon_{it}, \qquad (2)$$

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

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 dollar amount of bonds issued (*Issue Amount*) in the municipal bond market.

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

having a positive impact on the incumbent's chances of being reelected.

5. Political Parties

In this section, we investigate whether political partisanship shapes the effect of municipal bond ratings on election outcomes. We start by studying the differences in the 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 Democrat and Republican incumbent candidates. The regressions include a triple interaction term *Recalibrated* × *Post* × *Democrat*, where *Democrat* is a dummy variable that takes a value of one if the Democratic party was in power 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 candidates and Republican candidates.

Table 10 presents these estimates. Columns 1 and 2 present the results for Senate elections. We find that the impact of the ratings recalibration is more pronounced among Democratic counties both in terms of the likelihood of the incumbent winning the election and his vote share. The effect is both economic and statistically significantly larger for Democratic incumbents than for Republican incumbents. Columns 3 and 4 show that Democrat incumbents also benefit more from rating upgrades in terms of likelihood of winning than Republicans do, in the case of House elections. Columns 5 and 6 show a stronger effect of the recalibration for Democratic incumbents relative to Republican incumbents in gubernatorial elections. The results for the *Incumbent Share* show that, in fact, incumbents in Republican counties are losing voting shares, and that Democrats experience a significantly larger increase in their voting shares relative to Republican candidates.

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 the election and an increase in their vote share.

5.2 Political Parties and Economic Outcomes

We also examine whether the differences in the recalibration's effects on the election outcomes of Democratic counties relative to Republican counties is due to differences in public policies implemented by politicians and their spillovers to the private sector. In these regressions, because we cannot determine the party of the issuer nor 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 2012 elections. Any 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 *Offer Yield* after the recalibration. We compare the effects of the recalibration on the *Issue Amount* and *Offer Yield* in Democratic counties relative to Republican counties using the interaction term *Recalibrated* × *Post* × *Democrat*. Columns 1 and 2 of table 11 present the results. We do not find any significant difference between Democrat and Republican counties in the access to municipal bond markets.

Second, we study whether partisanships is associated with different fiscal policy shocks. Columns 3–5 examine the differences in the reactions of Democratic and Republican counties in their *Government Expenditure*s, *Tax Rate*, and *Government Employment*, respectively. We do not find any significant differences between the reactions of Democrat 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 parties' differences

in terms of the effect of the recalibration on *Private Employment*, *Non-Tradable Employment*, *Construction Employment*, and *Income*. We only observe a difference in the increase of *Construction Employment*, as the recalibration is associated with an increase in construction employment only in Democratic counties. We do not find any significant differences between Democratic and Republican counties in terms of the effects on *Private Employment* and *Income*.

Overall, our results suggest that there are no significant differences between the public policies implemented by Democrat and Republican politicians and their subsequent consequences. However, we find that voters react differently at the time of elections even though the policies are similar across parties. Democratic voters seem to respond more positively to increases in government spending and improvements in economic conditions than Republican voters. This result can shed light on the long-standing debate on whether voters punish or reward deficit-financed increases in government spending. The mixed results in the literature may be driven by the differences in voters' tastes based on their political affiliations.

6. Conclusion

We provide causal estimates of the effect of credit ratings on election outcomes. We explore exogenous variation in credit ratings due to Moody's recalibration of its US municipal bond ratings scale in 2010. The recalibration generates cross-sectional variation in ratings across local governments, resulting in a zero- to three-notch upgrade of municipal bonds. We find significant electoral rewards to incumbent candidates of upgraded municipalities.

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

Our findings raise the possibility of "pay-for-luck" embedded in the political system.

Politicians that are in power when positive shocks affect their constituents are rewarded even if the shock is outside of their control (attribution error). This could be the result of rational inattention. The amount of time and effort necessary to separate political skill from luck may be too high for the median voter. In addition, voters may simply not have the cognitive capacity to make such judgments.

Our results highlight the power of CRAs. The impact of ratings on local governments' financial constraints and in the public opinion perception seems to be sufficiently important to affect elections outcomes. Taken together with the evidence of the effects of ratings on firm-level outcomes, we conclude that CRAs have an important effect on markets and that their actions do more than simply reduce information asymmetry. Regulators should be aware of the role of CRAs in shaping the architecture of financial systems, as their actions could have implications that transcend financial markets and affect the political process. Although our results suggest that CRAs may have "too much power," there is also a potential bright side. Democracy is an imperfect form of market competition, as it is typically difficult to fire a politician during their 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 device that limits spending by ill-intentioned politicians.

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Table 1. Summary Statistics of Election Outcomes

This table presents the mean, standard deviation, and number of observations for each election outcome variable separated by the treatment group and control group prior to 2010 (the pre-recalibration period). The treatment group includes counties with an above-median fraction of upgraded local government (the median *Recalibrated* is zero except for House and mayoral elections). The control group includes counties with a below-median fraction of upgraded local government. The sample 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 the raw difference between treated and control counties. Column 8 presents the difference adjusted by state-year fixed effects and a continuous control of county, congressional district, and city size, respectively. The *p*-values in columns 7 and 8 are calculated based on standard errors clustered by county for the case of Senate, governor, and president; by congressional district for the case of House of Representatives; and by city for the case of mayor.

	Treatment Group			Control Group			Differences	
	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*** (0.000)	–0.008** (0.033)
Incumbent Win	0.412	0.492	1,917	0.445	0.497	4,283	–0.084*** (0.000)	0.012 (0.172)
Recalibrated	0.103	0.114	1,917	0.000	0.000	4,283	0.103*** (0.000)	0.100*** (0.000)
County Votes	0.842	1.566	1,917	0.116	0.227	4,283	0.726*** (0.000)	
Panel B: House Elections								
Incumbent Share	0.684	0.139	411	0.642	0.126	409	0.041*** (0.001)	0.010 (0.331)
Incumbent Win	0.939	0.239	411	0.907	0.291	409	0.032* (0.088)	0.020 (0.351)
Recalibrated	0.136	0.091	411	0.022	0.016	409	0.114*** (0.000)	0.105*** (0.000)
Cong. Distr. Votes	2.279	0.738	411	2.421	0.695	409	–0.143*** (0.005)	
Panel C: Gubernatorial Elections								
Incumbent Share	0.510	0.141	932	0.518	0.160	2,141	–0.009 (0.132)	0.014*** (0.004)
Incumbent Win	0.267	0.443	932	0.355	0.479	2,141	–0.088*** (0.000)	0.003 (0.822)
Recalibrated	0.104	0.114	932	0.000	0.000	2,141	0.104*** (0.000)	0.099*** (0.000)
County Votes	0.635	1.022	932	0.100	0.183	2,141	0.535*** (0.000)	
Panel D: Presidential Elections								
Incumbent Share	0.534	0.128	1,922	0.609	0.129	4,297	–0.075*** (0.000)	–0.024*** (0.000)
Incumbent Win	0.626	0.484	1,922	0.830	0.376	4,297	–0.204*** (0.000)	–0.062*** (0.001)
Recalibrated	0.103	0.114	1,922	0.000	0.000	4,297	0.103*** (0.000)	0.100*** (0.000)
County Votes	1.004	1.887	1,922	0.130	0.228	4,297	0.874*** (0.000)	
Panel E: Mayoral Elections								
Incumbent Share	0.695	0.213	83	0.649	0.244	70	0.047 (0.246)	0.042 (0.320)
Incumbent Win	0.892	0.313	83	0.786	0.413	70	0.106 (0.119)	0.111 (0.130)
Recalibrated	0.130	0.053	83	0.028	0.016	70	0.102*** (0.000)	0.092*** (0.000)
City Votes	0.235	0.409	83	0.161	0.333	70	0.074 (0.258)	

Table 2. Summary Statistics of Economic Outcomes

This table presents the mean, standard deviation, and number of observations for each economic outcome variable separated for the treatment group and control group prior to 2010 (the pre-recalibration period). The treatment group includes counties with an above-median fraction of upgraded local government (the median *Recalibrated* is zero except for House and mayoral elections). The control group includes counties with a below-median fraction of upgraded local government. The sample period is from 2007 to 2009. Column 7 presents the raw difference between treated and control counties. Column 8 presents the difference adjusted by state-year fixed effects and a continuous control of county size. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Treatment Group			Control Group			Differences	
	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 ($ millions)	1,169.071	3,302.450	2,829	151.097	2,188.747	6,057	1,017.974***	–424.731
							(0.000)	(0.191)
Local Government Employment (thousands)	8.127	20.147	2,297	1.606	15.095	2,547	6.521***	–5.368**
							(0.000)	(0.035)
Private Employment (thousands)	97.252	220.873	2,874	10.742	51.743	6,403	86.510***	–15.937**
							(0.000)	(0.041)
Income ($ millions)	4,442.666	9,582.727	2,868	490.147	1,901.400	6,475	3,952.519***	–524.074*
							(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 an incumbent party win (columns 1–3) and the change in incumbent party vote share (columns 4–6) around the recalibration event (April–May 2010). Panel A presents the results for Senate elections, panel B for House elections, and panel C for gubernatorial elections. *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. In panel A, the sample consists of counties in the 2004–2012 period. Panel B consists of congressional districts in the 2006–2012 period. In panel C, the sample consists of counties in the 2006–2012 period. Robust standard errors are clustered at the county level in panels A and C, and at the congressional district level in panel B, 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)
Cong. Distr. 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)
Cong. Distr. 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 an incumbent party win (columns 1–3) and the change in incumbent party vote share (columns 4–6) around the recalibration event (April–May 2010). Panel A presents the results for presidential elections, panel B for California mayoral elections. *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. In panel A, the sample consists of counties in the 2004–2012 period. Panel B consists of California cities in the 2006–2012 period. Robust standard errors are clustered at the county level in panel A and at the city level in panel B, 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 Election						
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: California Mayoral Election						
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 State Income Taxes

This table presents difference-in-differences estimates of the likelihood of an incumbent party win (columns 1, 3, and 5) and the change in incumbent party vote share (columns 2, 4, and 6) around the recalibration event (April–May 2010) separated by the state tax level. *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. *Tax* is the top marginal state income tax rate in 2010. In columns 1 and 2, the sample consists of counties in the 2004–2012 period. In columns 3 and 4, the sample consists of congressional districts in the 2006–2012 period. In columns 5 and 6, the sample consists of counties in the 2006–2012 period. We control for *Incumbent Share*$_{t-1}$ in all columns, as well as *County Votes* in columns 1, 2, 5, and 6, and congressional district votes in columns 3 and 4. Robust standard errors, clustered at the county or congressional district level, 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*	–0.250	–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
Cong. Distr. 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 6. The Effect of Ratings News Searches

This table presents difference-in-differences estimates of the likelihood of an incumbent party win (columns 1, 3, and 5) and the change in incumbent party vote share (columns 2, 4, and 6) around the recalibration event (April–May 2010) separated by the volume of Google searches for the term "credit ratings." *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. *News* is a dummy variable that takes a value of one if the increase in news searches for the term "credit rating" from before the recalibration (2006–2009) to after the recalibration (2010–2012) is above the median, and takes a value of zero otherwise. In columns 1 and 2, the sample consists of counties in the 2004–2012 period. In columns 3 and 4, the sample consists of congressional districts in the 2006–2012 period. In columns 5 and 6, the sample consists of counties in the 2006–2012 period. We require that the state has a record of news searches for credit ratings in at least one of the sample years. We control for *Incumbent Share*$_{t-1}$ in all columns, as well as *County Votes* in columns 1, 2, 5, and 6, and *Cong. Distr. Votes* in columns 3 and 4. Robust standard errors clustered at the county or congressional district level 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*	0.472***	0.077**	0.276**	0.082	–0.191	–0.180**
	(0.143)	(0.031)	(0.112)	(0.075)	(0.260)	(0.072)
Recalibrated × *Post* × *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
Cong. Distr. 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 7. The Effect of Municipal Bond Ratings on Election Outcomes by Year

This table presents difference-in-differences estimates of the likelihood of an incumbent party win (columns 1, 3, and 5) and the change in incumbent party vote share (columns 2, 4, and 6) around the recalibration event (April–May 2010) by year. *Recalibrated* is the fraction of upgraded local government units in each county or congressional district. The variables 2010, 2011, and 2012 are year dummies that take a value of one in the years 2010, 2011, and 2012, respectively, and take a value of zero otherwise. In columns 1 and 2, the sample consists of counties in the 2004–2012 period. Columns 3 and 4 consist of congressional districts in the 2006–2012 period. In columns 5 and 6, the sample consists of counties in the 2006–2012 period. We control for *Incumbent Share*$_{t-1}$ in all columns, as well as *County Votes* in columns 1, 2, 5, and 6, and *Cong. Distr. Votes* in columns 3 and 4. Robust standard errors clustered at the county or congressional district level 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 × 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
Cong. Distr. 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 8. The Effect of Municipal Bond Ratings on Economic Outcomes

This table presents difference-in-differences estimates of county-level economic outcomes around the Moody's 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 from April 2010 to March 2013 and zero from April 2007 to March 2010. 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	*Issue Amount (log)*	*Government Expenditure*	*Tax Rate*	*Government Employment*	*Private Employment*	*Non-Trade Employ ment*	*Construction Employ- ment*	*Income*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recalibrated × *Post*	–0.360***	0.209**	0.064**	–0.143***	0.093***	0.071***	0.128**	0.000	
		0.084***	(0.114)	(0.096)	(0.029)	(0.029)		(0.025)	
		(0.015)	(0.059)	(0.036)	(0.021)				
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.391	0.165	0.387	0.423	0.0873	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
Number of Counties	1,370	1,370	1,749	1,741	1,614	3,115	3,063	2,909	3135

Table 9. Instrumental Variable Estimates

This table presents instrumental variable estimates of the municipal debt issues (*Issue Amount*) on election outcomes. Panel A presents first-stage regression estimates in which *Issue Amount* is instrumented with 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 2006–2009 period (2004–2009 in the case of presidential elections). Panel B presents second-stage regression estimates in which the dependent variable is the likelihood of an incumbent party win (columns 1, 3, 5, and 7) and the change in incumbent party vote share (columns 2, 4, 6, and 8). The sample consists of counties or congressional districts in the 2006–2012 period (2004–2012 in the case of presidential elections). We control for *Incumbent Share*$_{t-1}$ in all columns, as well as *County Votes* in columns 1, 2, 5, 6, 7, and 8, and *Cong. Distr. Votes* in columns 3 and 4. Robust standard errors clustered at the county or congressional district level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: First Stage – Amount of Bonds Issued

	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
Cong. Distr. 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
Cong. Distr. 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 an incumbent party win (columns 1–3) and the change in incumbent party vote share (columns 4–6) around the recalibration event (April–May 2010) separated by party. *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 2006–2009 period. *Democrat* is an indicator variable that takes the value of one if the party in power in 2010 is Democratic in the respective election, and takes a value of zero if the party in power in 2010 is Republican. All other incumbent parties are excluded from the sample. In columns
1 and 2, the sample consists of counties in the 2004–2012 period. Columns 3 and 4 consist of congressional districts in the 2006–2012 period. In columns 5 and 6, the sample consists of counties in the 2006–2012 period. We control for *Incumbent Share*$_{t-1}$ in all columns, as well as *County Votes* in columns 1, 2, 5, and 6, and *Cong. Distr. Votes* in columns 3 and 4. Robust standard errors clustered at the county or congressional district level 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
Cong. Distr. 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 Moody's recalibration event (April–May 2010) separated by party. *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 from April 2010 to March 2013 and zero from April 2007 to March 2010. *Democrats* is a dummy variable that takes a value of one if the Democratic presidential candidate was the most voted in the county in 2008 and 2012 and takes a value of zero if the Republican presidential candidate was the most voted in the county in 2008 and 2012. All counties that experienced a change in the presidential candidate winner ("swing" counties) are excluded from the sample. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	Offer Yield	Issue Amount (log)	Government Expenditure	Tax Rate	Government Employment	Private Employment	Non-Trade	Construction	Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Recalibrated × *Post*	–0.163	0.220	0.065	–0.125***	0.079**	0.085***	0.034	–0.059	0.109***
	(0.191)	(0.178)	(0.042)	(0.043)	(0.038)	(0.021)	(0.076)	(0.050)	(0.031)
Recalibrated × *Post* × *Democrats*	–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	Yes	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
Number of Counties	1,634	1,634	1,604	1,596	1,475	2,886	2,835	2,698	2,905

Figure 1. Moody's and S&P Ratings around the Recalibration

This figure shows the evolution of Moody's ratings and S&P ratings around the recalibration event (April–May 2010) for issuers that have both Moody's and S&P ratings.

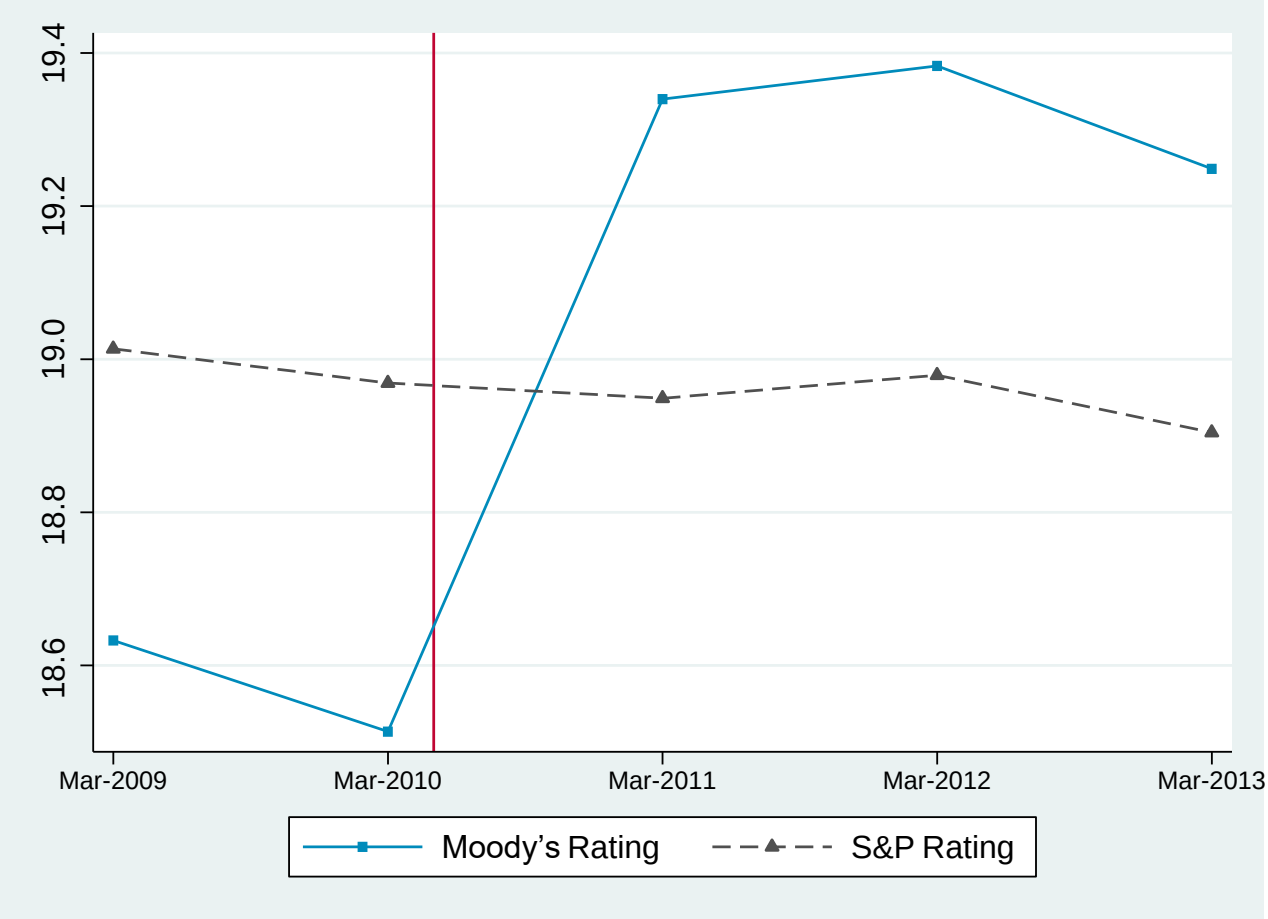


Figure 2. House Prices around the Recalibration

This figure shows the evolution of house prices (relative to 2008) around the recalibration event (April–May 2010), separated for upgraded local governments (treated) and nonupgraded local governments (control). House prices are the Federal Housing Finance Agency's House Price Index data at the metropolitan statistical area level.

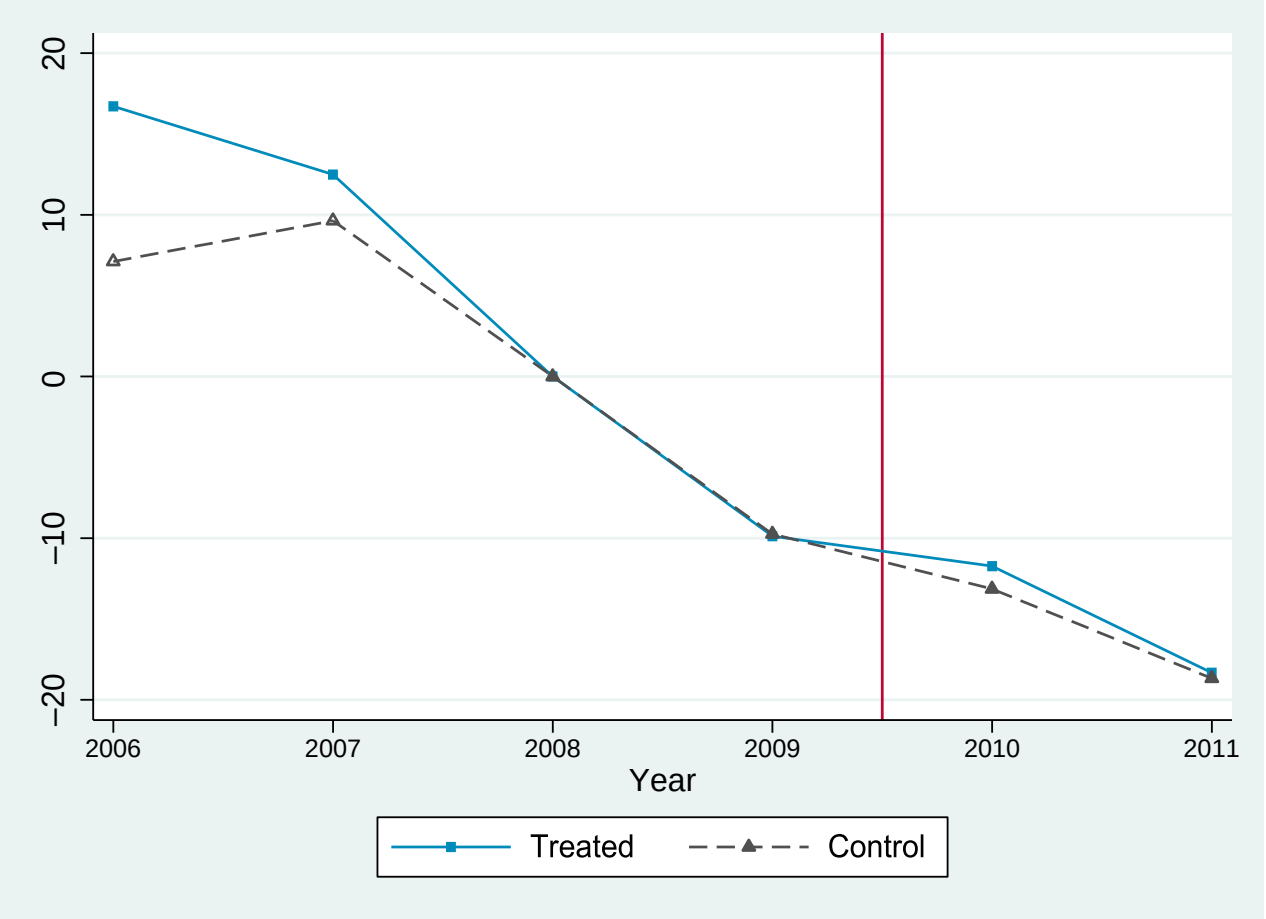


Figure 3. Fraction of Local Government Units Recalibrated by County

The map shows the fraction of local government units in a given county that were upgraded during the Moody's recalibration (*Recalibrated*). Counties in gray 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 nonzero values), respectively.

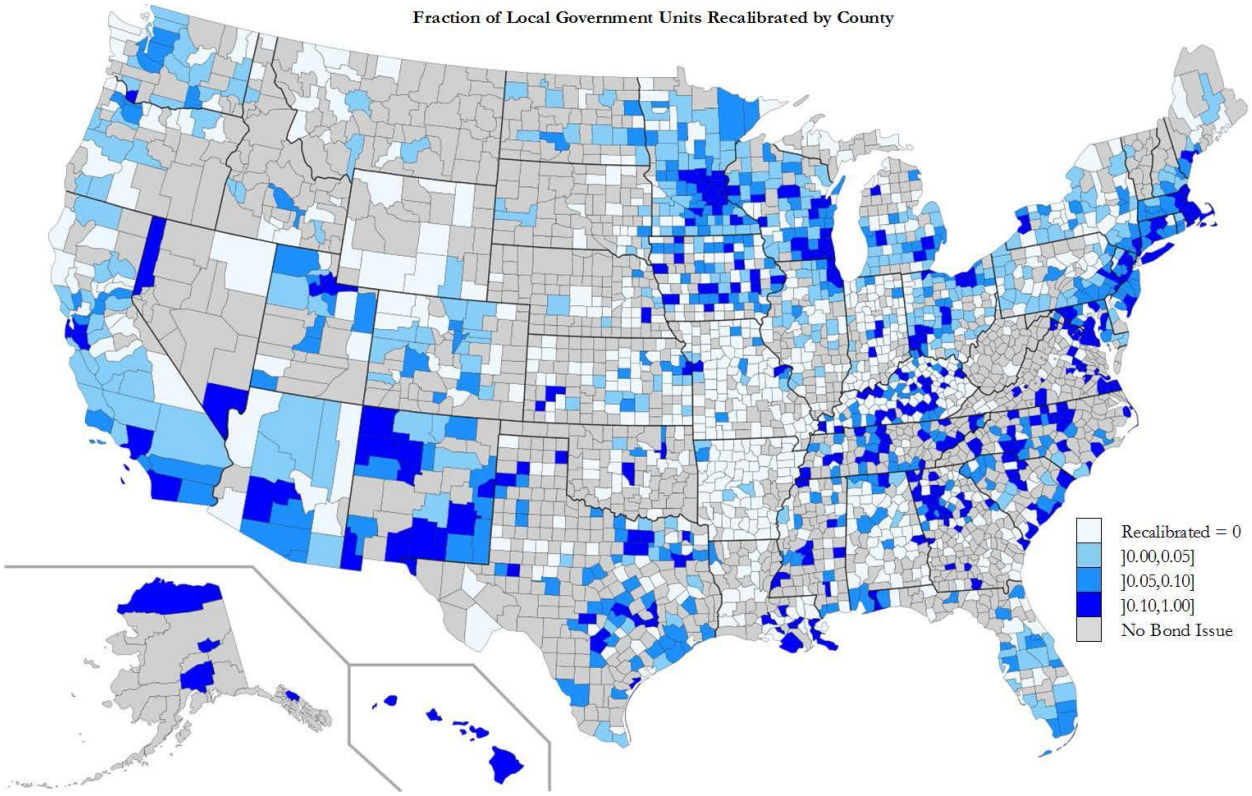
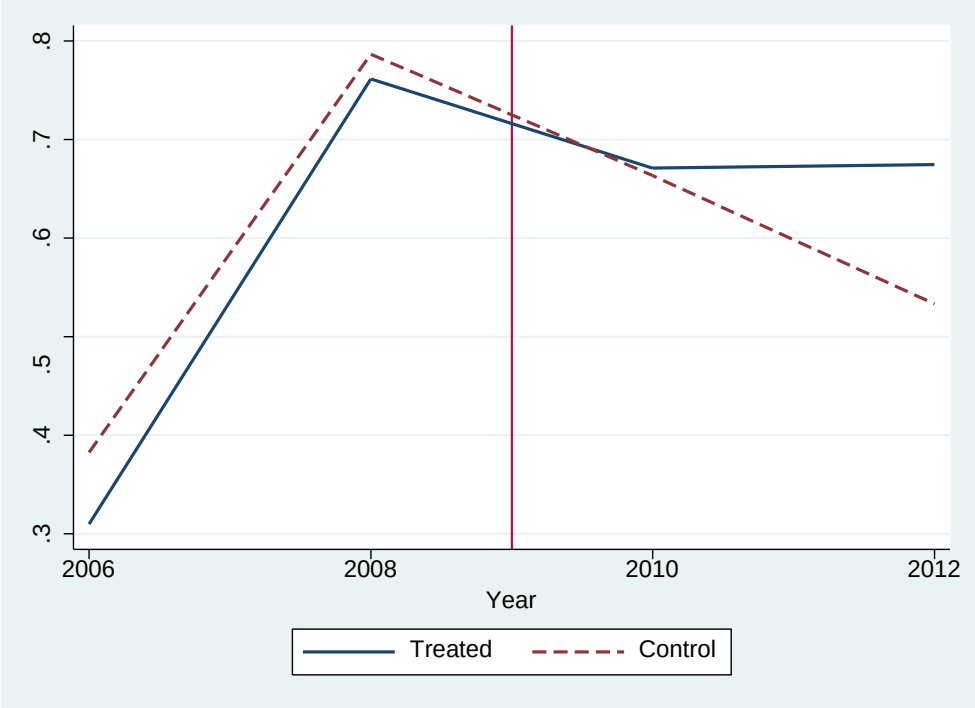


Figure 4. Senate Election Outcomes around the Recalibration

This figure shows the probability of an incumbent party win (*Incumbent Win*) and incumbent voting share for counties in the treatment group (*Recalibrated* above the median) and control group (*Recalibrated* below the median) around the Moody's recalibration event (April–May 2010) for Senate election outcomes. Panel A shows the probability of the *Incumbent Win* for the treatment group and control group separately. Panel B shows the logarithm of the incumbent's vote share for the treatment group and control group separately. 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

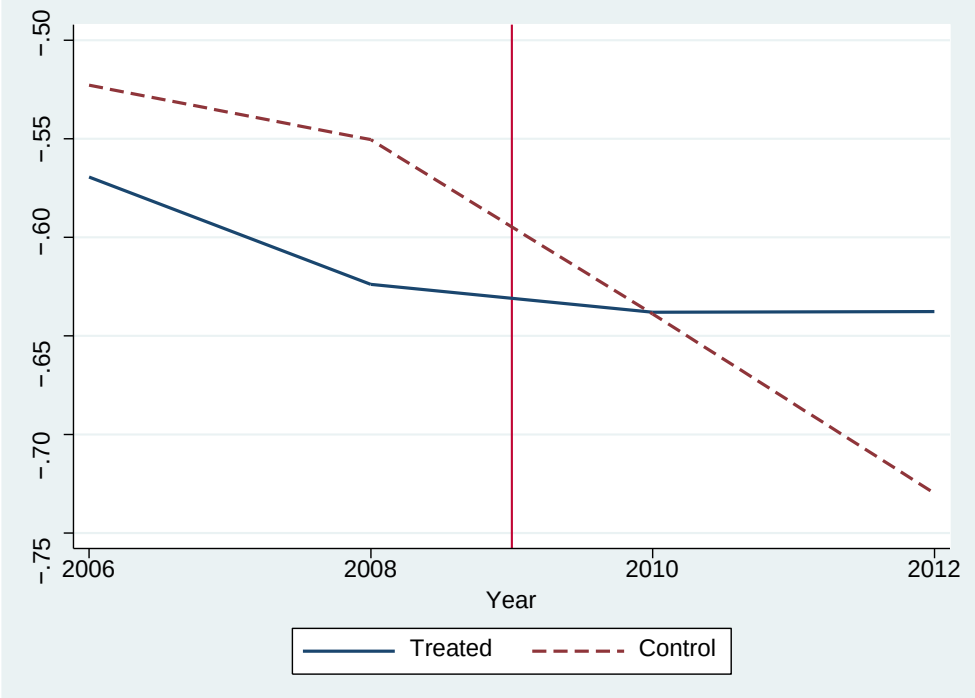
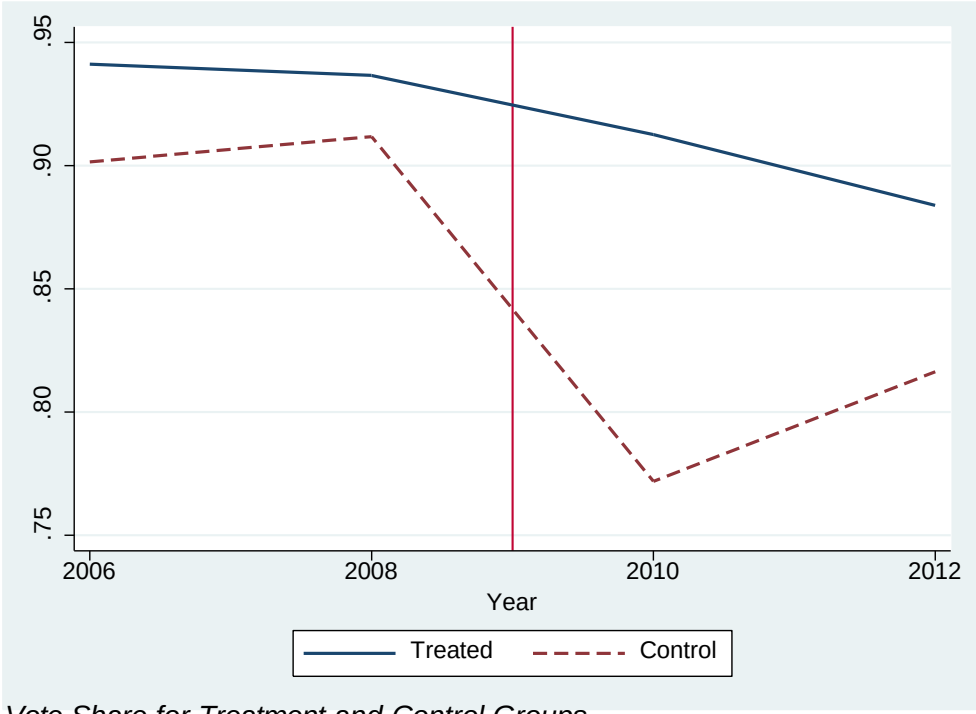


Figure 5. House Election Outcomes around the Recalibration

This figure shows the probability of an incumbent party win (*Incumbent Win*) and incumbent voting share for congressional districts in the treatment group (*Recalibrated* above the median) and control group (*Recalibrated* below the median) around the Moody's recalibration event (April–May 2010) for House election outcomes. Panel A shows the probability of the Incumbent Win for the treatment group and control group separately. Panel B shows the logarithm of the incumbent's vote share for the treatment group and control group separately. The sample consists of congressional districts 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