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JEL Classification: E24, G24, G28, H74

Keywords: Public Finance, Local Economy, Municipal Bonds, Credit ratings, Government Employment, Private Employment, Income

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The Economic Effects of Public Financing: Evidence from Municipal Bond Ratings Recalibration*

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Abstract

We show that municipalities' financial constraints can have a significant impact on local employment and growth. We identify these effects by exploiting exogenous upgrades in U.S. municipal bond ratings caused by Moody's recalibration of its ratings scale in 2010. We find that local governments increase expenditures because their debt capacity expands following a rating upgrade. These expenditures have an estimated local income multiplier of 1.9 and a cost per job of $20,000 per year. Our findings suggest that debt-financed increases in government spending can improve economic conditions during recessions.

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Municipal bond markets are an important source for state and local governments to finance the construction and maintenance of infrastructure and other public projects, provide cash flow for government needs and services, and finance private projects (using conduit financing). According to the U.S. Securities and Exchange Commission (SEC 2012), as of December 2011, investors held more than one million municipal bond issues, representing an outstanding (principal) amount of more than $3.7 trillion, or about 25% of the gross domestic product (GDP) of the United States.

In this paper, we examine how changes to the supply of credit to municipalities in the United States affect local economies, particularly during recessions. Easier access to financing can have important effects on economic outcomes when governments face financial distress, such as during the 2007–2009 Great Recession.[1] Specifically, local governments can use bond financing to alleviate spending cuts, prevent tax and fee increases, or contribute to their end-of-year balances. These increases in government spending can either have positive spillover effects in the private sector arising from increased disposable income or crowd out private consumption and investment.

We identify the real effects of public financing by exploiting exogenous variation in U.S. municipal bond ratings due to Moody's 2010 recalibration of its municipal bond rating scale. 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). Moody's Global Rating Scale, in contrast, measures expected losses (i.e., default probability and loss given default) among sovereign and corporate bonds. This dual-class rating system persisted for decades until Moody's recalibrated its Municipal Rating Scale to align it with the Global Rating Scale in April–May 2010. As a result, nearly 18,000 local governments received ratings upgrades of up to three notches, corresponding to bonds worth more than $2.2

[1] According to the 2009 Surveys of State and Local Finances conducted by the Census Bureau, during the 2009 fiscal year, state and local governments faced large budget gaps totaling $900 billion.

trillion in par value (nearly 70,000 bond issues).

Moody's recalibration allows us to isolate changes in economic outcomes that are caused by changes in public financing from those that would occur absent of these changes. The upgrades that resulted from the recalibration reflected not changes to the issuers' intrinsic quality, but rather the goal of aligning municipal ratings standards with those of sovereign and corporate ratings. In fact, the recalibration algorithm used expected losses by rating level and type of government, and thus changes in ratings due to the recalibration are uncorrelated with changes to local government (and nationwide) fundamentals. An important aspect of the recalibration is that it did not affect all local governments. Local governments that were already properly calibrated vis-à-vis the Global Rating Scale can be used as a control group. In addition, local governments without Moody's rating or with the maximum attainable rating (i.e., Aaa rating) were, by definition, not subject to recalibration and can also be used as a control group.

We employ a difference-in-differences approach to compare the outcomes between upgraded local government units (the treatment group) and non-upgraded local government units (the control group) around the recalibration. We study how this shock to municipal ratings affects outcomes at both local government and county levels. Because our event affects bonds issued by any local government unit within a county (i.e., the recalibration can affect bonds issued by counties, cities, townships, school districts, and special districts), we aggregate all changes in ratings in a county whenever we study county-level outcomes.[2] Our treatment (continuous) variable is the fraction of local government units in each county whose outstanding bonds were upgraded because of the recalibration. The specifications include county-level control variables, as well as state-by-year fixed effects to capture any source of time-varying unobserved state-level heterogeneity, such as changes in transfers from state governments and ballooning of unfunded state pension liabilities. We also estimate specifications with county-size decile-by-year fixed effects to account for the possibility that the Great Recession and the subsequent recovery may have affected large and small counties differently.

[2] We exclude states as they are a higher level government than counties.

We first examine whether Moody's recalibration causes an asymmetric effect in the ratings of new municipal bond issues in the primary market during the 2006–2013 period. We find that Moody's ratings increase 0.7 notches more for upgraded local governments than for non-upgraded local governments. We use S&P municipal bond ratings as a placebo test for the sample of bonds that have both Moody's and S&P ratings (about 55% of the bonds). 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. We find no significant changes in the S&P ratings between the treatment and control groups.

We also find that upgraded local governments increase the amount of new bond issues significantly relative to non-upgraded local governments following the recalibration. The differential effect on the amount of bonds issued (at the local government level) is 16%–20% per year in the three-year period after the recalibration (April 2010–March 2013) relative to the four-year period before the event (April 2006–March 2010). At the county level, a one standard deviation increase in the fraction of upgraded local governments increases the amount of bonds issued by 3.1%. The offer yield of the new bond issues of upgraded local governments decreases by 14 basis points relative to non-upgraded local governments. These findings are consistent with credit ratings playing an informational role in the municipal bond market. This may be due to the larger presence of retail investors relative to other fixed-income markets, which means that ratings are more likely to be used as a source of information.[3] The effects may also be the result of ratings-based regulations and internal policies on institutional investors.[4] Our offer yield results are consistent with those in Cornaggia, Cornaggia, and Israelsen (2015), who use the Moody's ratings recalibration to study the effects of credit ratings on municipal bond prices.

[3] According to the U.S. Flow of Funds Accounts quarterly data, the household sector held $1,872 billion of the $3,772 billion of municipal bonds outstanding in 2010 (a share of almost 50%). This share decreased to about 44% by 2013, but households still have an important share of the municipal bond market. In contrast, households held only 19% of corporate and foreign bonds as of 2010.

[4] Beyond official regulations (e.g., Basel II and National Association of Insurance Commissioners (NAIC) guidelines), 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 (the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act; and Financial Stability Board 2010, 2012).

They find that upgraded bonds earn abnormal returns in the secondary market, and that upgraded municipalities subsequently benefit from a reduction in offer yields in the primary market. Our study builds on the results in Cornaggia, Cornaggia, and Israelsen (2015) to study how a shock to local governments' financial constraints, caused by the ratings recalibration, affects the real economy in terms of government spending, employment, and income.

Consistent with local governments using the additional bond financing to improve economic conditions, we find significant effects on local economic outcomes after the recalibration. We find that a one standard deviation increase in the fraction of local governments upgraded in a county increases local governments' expenditures and employment by 0.5%. Even though state and local governments are required to have balanced budgets, court decisions and referendums on borrowing have led to the exclusion of (capital) expenditures funded by long-term debt from deficit calculations as reported by the National Conference of State Legislatures (2003). There is also de facto flexibility for local governments to run deficits (at least for limited periods).

We find evidence of spillover effects to private employment and income measured at the county level. A one standard deviation increase in the fraction of local governments upgraded in a county increases total private employment by 0.3%. We also find that the effects on private employment are heterogeneous across sectors and are concentrated in the non-tradable sector (retail, food, and accommodation), which depends primarily on local demand, and in the education sector, which typically receive transfers or grants from local governments. A one standard deviation increase in the fraction of local governments upgraded in a county increases non-tradable employment by 0.9% and education sector employment by 1%. The effect in the tradable sector is statistically insignificant. Last, we find that county-level income increases in response to the recalibration event. A one standard deviation increase in the fraction of upgraded local governments increases income by 0.5%.

The effect of the recalibration is heterogeneous across municipalities. The results are more pronounced in the sample of high leverage, low rating, and small local governments units, consistent with the recalibration playing a particularly important role for financially constrained

local governments. We also find that results are stronger in counties with higher unemployment rate, consistent with the idea that expansionary government spending is less likely to crowd out private consumption or investment in an economy with greater economic slack.

Our results are robust to a series of alternative sample definitions and specifications. We obtain consistent estimates when we restrict the sample to counties located in urban areas, counties with multiple bond issuers, and counties with higher amount of bonds issued. We also obtain consistent estimates when we include group-specific (treatment and control) trends, which mitigates concerns about pre-existing differential trends.

Our study contributes to the literature that relies on cross-sectional variation in the estimation of local fiscal multipliers (e.g., Cohen, Coval, and Malloy 2011; Chodorow-Reich et al. 2012; Nakamura and Steinsson 2014; and Suarez-Serrato and Wingender 2014), which differs from the traditional empirical macroeconomics literature, where time series variation is employed (see Ramey 2011 for a survey). The long-standing debate on the effects of public spending on economic outcomes and the size of the fiscal multiplier has received additional attention due to the American Recovery and Reinvestment Act (ARRA) of 2009.

Given that we exploit a cross-sectional regional shock to government financing and expenditures, we can provide estimates of local fiscal multipliers (the "open economy relative multiplier")—that is, the effect that a relative increase in government spending in one region relative to another has on relative output or employment. A caveat of this approach is that it ignores general equilibrium effects, which could change the interpretation of the overall effect of the stimulus spending and national multiplier (the "closed economy aggregate multiplier").

Using the ratings recalibration as an instrument for local government expenditures, we estimate that a marginal million dollars in local government expenditures results in an additional 51 jobs, 45 of which are outside the public sector. This estimate corresponds to a cost per job created of $20,000 per year. Our estimates also imply an income multiplier of 1.9 (i.e., dollar change in local income produced by a one-dollar change in local government spending). Combining the income and employment multipliers, we estimate that the jobs created have a

remuneration of 1.9 × $20,000 = $38,000 per year.

Our estimates of fiscal multipliers are in line with the estimates of local fiscal multipliers in the literature. This is consistent with Keynesian models that predict high multipliers during periods when the marginal propensity to consume is high. Intuitively, in periods of factor underutilization and when interest rates are near zero, government spending shocks are less likely to crowd out private consumption and investment, and fiscal multipliers should thus be larger (e.g., Auerbach and Gorodnichenko 2012; and Fishback and Kachanovskaya 2015).[5]

We also contribute to the literature on the effect of credit market shocks on economic outcomes. Mian and Sufi (2011, 2014) and Mian, Sufi, and Rao (2013) focus on the role of household leverage in explaining the severity of the Great Recession in 2007–2009, and Giroud and Mueller (2016) focus on the role of firm leverage. Chodorow-Reich (2014) shows that firms with pre-crisis lending relationships with weaker banks face restrictions in credit supply and reductions in employment following the collapse of Lehman Brothers in 2008. Greenstone, Mas, and Nguyen (2014) and Bentolila et al. (2015) find that shocks to the supply of bank credit to (small) businesses during the Great Recession are associated with reductions in employment.[6] Whereas these authors study the economic effects of credit supply shocks to the *private* sector, we study the economic effects of credit supply shocks to the *public* sector. To the best of our knowledge, we are the first to provide causal evidence of the real effects of municipal bond markets.

Last, we contribute to the literature on the real effects of credit ratings. Credit ratings matter for firm investment and financial policy (Faulkender and Petersen 2006; Kisgen 2006, 2009; Sufi 2009; Tang 2009; Kisgen and Strahan 2010; Chernenko and Sunderam 2012; Manso 2013; and Almeida et al. 2016).

[5] The ratings recalibration coincided with a period with significant slack in the economy and short-term interest rates near zero. In December 2009, the real GDP annual growth was -2.8%, the unemployment rate was about 9.9% (both drawn from the Bureau of Economic Analysis), and the federal funds rate was 0.12%.

[6] Others study the effect of credit expansions (through mortgage origination) on house prices and (non-tradable) employment (e.g., Adelino, Schoar, and Severino 2014; and Di Maggio and Kermani 2015).

1. Institutional Background and Data

1.1 Recalibration Event

Moody's had a dual-class rating system before the ratings recalibration in 2010. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality was to reach a position that required support from a higher level of government to avoid default). In contrast, Moody's Global Rating Scale is designed to measure expected losses (default probability and loss given default) among sovereign bonds, corporate bonds, and 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 Flow of Funds Accounts in 2010, households owned 50% of municipal bonds, followed by money market funds with 10% and insurance companies with 9%. In contrast, households held only 19% of corporate and foreign bonds.

Moody's dual-class rating system produced lower ratings for municipal bonds relative to its competitors. In our sample, Moody's assigned a rating lower than S&P in 53% of the issues (and the same rating in 40% of the issues) in the year before the recalibration; this number drops to only 17% in the year after the recalibration (reflecting the upgrades). In addition, Moody's (2007) shows that default rates in municipal bonds are significantly lower than those experienced by comparable corporate bonds. Because of the more conservative ratings under the dual-class system, Moody's share in the municipal bond market declined. In the year before the recalibration, Moody's had a market share of 34%, compared with S&P's share of 59% and Fitch's share of 7%. After the recalibration, Moody's market share increased to more than 40% (2010–2012).[7]

Moody's maintained a sizable market share despite this apparent competitive disadvantage under the dual-class system likely because many regulations (e.g., Basel II and National

[7] Moody's also faced lawsuits over its dual-class rating system arguing that harsher standards imposed on municipalities resulted in higher borrowing costs for taxpayers.

Association of Insurance Commissioners (NAIC) guidelines) and investment rules require at least two ratings from a nationally recognized statistical rating organization (NRSRO), and use the lower of two ratings, or the middle of three ratings, as the basis for regulatory benchmarks (e.g., banks' capital requirement).[8] Beyond regulations, local governments' debt management policies and institutional investors' policies often require two ratings. For example, the County of Alameda (2014), California, debt management policy stipulates that "at least two credit ratings should be procured from any of the nationally recognized credit rating services, unless the transaction is of a small size."[9] The Government Finance Officers Association (2015) (GFOA) also writes that "historically, many issuers have sought separate ratings from at least two credit rating agencies. In addition, many institutional investors require a minimum of two ratings." Market participants also emphasize the importance of two ratings. Timothy Cox, executive director of debt capital markets at Mizuho Securities, said in an interview with Bloomberg (2011): "If I don't have two ratings on a bond, I cannot sell it. No money manager is going to buy it."

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. Moody's issued a request for comments from market participants about the methodology and a potential shift from the municipal scale to the global scale in June 2006 (Moody's 2006). It planned to implement the ratings recalibration in June and July 2008, but the financial market turmoil during the summer and fall of 2008 led to a postponement. 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 2010, over a four-week period, Moody's announced how individual bonds would be affected by the recalibration, resulting in a zero-to-

[8] Bongaerts, Cremers, and Goetzmann (2012) find that Fitch typically plays the role of a "third opinion" (in addition to Moody's and S&P ratings), which matters primarily for regulatory purposes, rather than providing additional information about credit quality. Becker and Milbourn (2011) find that increased competition from Fitch is associated with higher and less informative ratings from the incumbents (Moody's and S&P).

[9] As another example, in 2008 the attorney general of the state of Connecticut stated in a letter to Barney Frank (chairman, House Committee on Financial Services): "The credit rating market is highly concentrated and most issuers require two ratings from a NRSRO to make their bond marketable under SEC rules."

three-notch upgrade of nearly 70,000 bond ratings.

Moody's recalibration algorithm used the expected losses by rating level and type of government (i.e., historical default rates by rating level and loss severity by government type) 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 and therefore can be used as control group. Some local governments were already properly calibrated vis-à-vis the global scale, in particular special districts related to housing and health-care did not see a change in ratings. In addition, bonds with higher ratings (at or above Aa3) on the municipal scale were less likely to be recalibrated than those with a lower rating (below Aa3); bonds with the maximum attainable rating (Aaa) in the municipal scale could, by definition, not be upgraded. Of course, local governments without bonds rated by Moody's were not subject to recalibration and can also be used as a control group.

Moody's (2010) clarifies that the recalibration is intended to enhance the comparability of ratings across asset classes, and it does not indicate a change in the credit quality of the issuer: "Our benchmarking … 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 … (The recalibration) does not reflect an improvement in credit quality or a change in our opinion."

Moody's (2010) also reports that any ratings under review for upgrade or downgrade before recalibration would remain under review and would not be lumped into these rating changes. Thus, our sample does not include any natural upgrades associated with improving issuer fundamentals that could contaminate our results. In addition, because the methodology closely follows a discussion that occurred (and was made public) over a period of several years, it is especially unlikely that the rating changes could include information about individual local governments.

1.2 Data

We obtain 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).

The municipal bond market data come from the Ipreo i-Deal new issues database. The sample period is from April 2006 to March 2013, which corresponds to the four-year period before the recalibration and the three-year period afterward. We restrict the sample to local governments that issued bonds during the four-year period before the recalibration.[10] We exclude from the sample bond issues from special districts in the housing and health-care sectors because they were already properly calibrated relative to the global rating scale and did not see a change in ratings.[11] Bonds issued by upgraded local governments (i.e., an issuer that experienced an upgrade in any of its outstanding bonds due to Moody's recalibration) represent about 74% of the sample of new issues (53% were upgraded by up to one notch, 19% by two notches, and 2% by three notches).

Because we measure economic outcomes (private employment and income) 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 counties (including boroughs and parishes), cities, townships (including towns and villages), school districts, and special districts. We exclude state-level bonds because they cannot be linked to a specific county. Because credit ratings on insured bonds reflect the credit quality of the *insurer* rather than the *issuer*, we include only uninsured bonds in our analysis (roughly 60% of bonds).

[10] We obtain numerically identical differential effects when we include all new issues or restrict the sample of new issues to local governments that issue bonds both before and after the recalibration, given that only local governments that issue bonds *both* before and after can be identified with the difference-in-differences estimator.
[11] These sectors represent about 21% of the bond issues from special districts. We obtain similar estimates when we include these sectors in the sample.

The primary economic outcome variables are local government expenditures, government employment, private employment (total and for sectors), and income. We obtain data on government expenditures from the U.S. Census Bureau's Annual Survey of State and Local Government Finances. The data include revenues and expenditures of individual local government units within each county. The sample includes local governments that are present in all years of our sample period (2007–2013).

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 within each county as of the week of March 12 of each year. The analysis of local government employment is restricted to local government units that are present in all years of our sample period (2007–2013).[12]

We obtain data on private-sector employment by industry (National American Industry Classification System (NAICS)) and 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 also present results using data on quarterly employment from the Census Bureau's Quarterly Workforce Indicators (QWI). The QWI is derived from the Longitudinal Employer-Household Dynamics (LEHD) program at the Census Bureau. We obtain county-level income data from the Internal Revenue Service (IRS) Statistics of Income. Income is defined as total wages and salaries in a given county and calendar year (the sample period for income is 2006–2012).

In our tests, 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 (FHFA's) House Price Index (HPI) data at the

[12] The sample of counties with government employment data includes only 1,618 counties, which corresponds to about half of the counties in the United States.

Metropolitan Statistical Area (MSA) level. The HPI is a weighted repeat-sales index that measures the average price changes in repeat sales or refinancing on the same properties.[13] We obtain county-level information on the number of households, defined as one or more people who occupy a given housing unit, from the 2007 Census Bureau Summary Files.

1.3 Summary Statistics

Panel A of Table 1 presents summary statistics on county-level outcome variables from 2007 to 2013. Our sample consists of counties in which at least one local government issued bonds in the municipal bond market in the four-year period before the recalibration (April 2006–March 2010). Thus, the local governments in the control group either do not have a rating from Moody's (i.e., they were rated only by S&P or Fitch) or have a Moody's rating that was not affected in the recalibration. This sample of bond issuers comprises 1,768 counties for which we also have employment and income data, which correspond to about 60% of the counties in the United States.[14]

We map the ratings into 22 numerical values, where 22 is the highest rating (Aaa), 21 the second highest (Aa1), and one the lowest (default). The average numerical rating by Moody's is 17.9, corresponding to a rating between A1 and A2. The median is 18 (A1). About half of our sample of new issues rated by Moody's is simultaneously rated by S&P. The average numerical rating by S&P is 18.6, confirming that Moody's municipal bond ratings were lower than S&P ratings before the recalibration. Counties in the sample issue an average of $170 million in bonds each year, but the distribution is highly skewed with a median of $15 million. The offer yield is 2.8% on average.

Counties in the sample have an average of 6,200 government employees and a median of 1,370 government employees. The average county in the sample has local government

[13] Whenever the MSA HPI is missing, we complement the data with state-level house price indices from the FHFA.
[14] The number of counties included in each regression varies according to the availability of sector-level employment-by-county data in the CBP. The Census Bureau often omits observations, or includes only broad ranges, for confidentiality reasons.

expenditures of $766 million. The distribution is also heavily skewed, with a median of $143 million dollars.

Private employment in each county is much larger than government employment at 60,170 employees on average. We separately track tradable and non-tradable employment based on two-digit NAICS codes. Average employment in the tradable sector (manufacturing; NAICS codes 31–33) is 4,390 employees, while average employment in the non-tradable sector (retail, food and accommodation; NAICS codes 44–45 and 72) is about 14,020 employees.

The final row of Table 1 presents summary statistics on the main explanatory variable (*Recalibrated*) at the county level. We first define the treatment and control groups at the local government level. The treatment group contains local governments whose outstanding bonds were upgraded during the Moody's recalibration event. We then calculate our treatment (continuous) variable at the county level as the fraction of all local government units in a given county that were upgraded due to the Moody's recalibration (*Recalibrated*). Figure 1 shows a map of the United States with the terciles of the *Recalibrated* variable (among those counties with non-zero value), which are well spread across the United States.

Panel B of Table 1 presents summary statistics on county-level outcome variables for an alternative sample. Specifically, we focus on the sample of urban counties (those counties with more than 50,000 people following the Census Bureau definition). This sample of urban counties comprises 933 counties, and the counties are, on average, bigger than those in Panel A. For example, counties in the sample of urban counties have an average of 93,620 households versus an average of 53,280 in the sample of bond issuers.

Table 2 provides a comparison of counties with recalibrated local governments (i.e., counties with non-zero *Recalibrated* variable–the treated counties) and counties without recalibrated local governments (i.e., counties with *Recalibrated* variable equal to zero−the control counties) in the pre-recalibration period. Because the median of *Recalibrated* is zero, recalibrated counties correspond to the counties with above-median *Recalibrated*, whereas non-recalibrated counties correspond to the counties with below (or at)-median *Recalibrated*. The sample in Panel A

consists of counties with bond issuers, and the sample in Panel B consists of urban counties.

Panel A shows that one feature of the data is that counties in the treatment group are larger than counties in the control group. The average number of households is 82,120 for the treatment group versus 19,410 for the control group. The average total private employment presents a similar pattern with 97,520 and 18,620 for treatment and control groups, respectively. We present both raw differences in means between treatment and control groups, as well as differences after adjusting for state-by-year and county-size decile-by-year fixed effects. This adjustment controls for regional and size heterogeneity in a given year between treatment and control groups, and the differences in number of households and private employment are no longer statistically (or economically) significant. We use this strategy in the regression tests to ensure comparability of treatment and control groups along the regional and size dimensions (as well as to absorb any time-varying factors that could affect counties of distinct sizes differently). Panel A also shows that the treatment and control groups have similar economic structures in terms of relative importance of local government employment versus private employment. Additionally, the growth rates (annual log change) of outcome variables in the pre-treatment period are similar across the two groups, except for government expenditures. These differences become relatively smaller in Panel B, when we make the comparisons within urban counties.

2. Effect on Municipal Bond Market

We start by examining the effects of the ratings recalibration on the access of local governments to the municipal bond market. We study the effect of Moody's recalibration on bond ratings, as well as on quantities and prices in the municipal bond market. We estimate the equivalent of a first stage in our setting (given that we are primarily interested in economic outcomes) by comparing the ratings, amount of bonds issued, and offer yield of upgraded local governments (the treatment group) and non-upgraded local governments (the control group) in the three-year period after the recalibration relative to the four-year period before.

We first estimate the effects of the recalibration on ratings aggregated to the county level. We

aggregate the new bond issues data using the average rating across all issues in each county and event year. We estimate the following (reduced form) regression:

$$Rating_{it} = \beta_1 Recalibrated_i \times Post_t + \beta_2 X_{i,t} + \eta_i + \eta_{state,t} + \eta_{size,t} + \varepsilon_{it} \quad (1)$$

The analysis is conducted within-county—that is, we include county fixed effects (η_i) in all regressions. The regressions include state-by-year fixed effects ($\eta_{state,t}$), as well as county-size decile-by-year fixed effects in some specifications ($\eta_{size,t}$). The explanatory variable of interest is the fraction of upgraded local governments in a county (*Recalibrated*) interacted with a dummy variable that takes a value of one for April 2010–March 2013 (*Post*). The fixed effects (county and state-by-year fixed effects) absorb both the *Recalibrated* and the *Post* dummy variables. We also control for issue characteristics, including whether the bond is a general obligation (GO) bond or revenue bond, whether the bond is part of the Build America Bonds (BAB) program, and duration.[15] Standard errors are clustered at the county level to correct for within-county residual correlation.

Panel A of Table 3 presents the results of county-level regressions of the effects on ratings. Column (1) presents the results in which the dependent variable is the Moody's rating. We find that the coefficient of the interaction term *Recalibrated* × *Post* is positive and significant. Column (2) shows a similar differential effect on ratings when we include county-size decile-by-year fixed effects in the regression. When we use the sample of urban counties in columns (5) and (6), the effects are slightly weaker but statistically significant.

About half of our sample of new issues rated by Moody's is simultaneously rated by S&P. We can use the S&P ratings as a placebo test, because S&P does not have a dual-class rating system. Specifically, this allows us to test whether the effect on Moody's ratings is the result of information about the creditworthiness of the upgraded municipalities (in which case S&P

[15] The BAB program ran from April 2009 to December 2010 to help state and local governments regain access to the bond markets and invest in infrastructure projects after the financial crisis. Our sample contains 4% of the bonds that are part of the BAB program. General obligation bonds represent 50% of the sample of bonds, and revenue bonds represent the other 50%.

ratings should also react). Columns (3) and (4) of Table 3 show no significant differential effect on S&P ratings between the treatment and control groups following the recalibration. When we use the sample of urban counties in columns (7) and (8), the effects on S&P ratings are also statistically insignificant. Although the exclusion restriction is not directly testable, we view this finding as an important validation of our identification strategy.[16]

Panel B of Table 3 presents the results of local government–level regressions of the effects on ratings. The dependent variable is the average rating across all issues of a given local government in each event year. We estimate the following (reduced form) regression:

$$Rating_{jt} = \beta_1 Recalibrated\ Dummy_j \times Post_t + \beta_2 X_{j,t} + \eta_j + \eta_{type,t} + \eta_{county,t} + \varepsilon_{jt} \quad (2)$$

The main explanatory variable of interest is the interaction term *Recalibrated Dummy* × *Post*. The *Recalibrated Dummy* takes a value of one if local government *j* experienced an upgrade in any of its outstanding bonds during the Moody's recalibration event. The analysis is conducted within local governments (issuer)—that is, we include local government fixed effects (η_j) in the regressions. The regressions also include government type-year fixed effects ($\eta_{type,t}$) and county-year fixed effects ($\eta_{county,t}$). Standard errors are clustered at the local government level to correct for within-local-government residual correlation.

The local-government-level results in Panel B are similar to the county-level results in Panel A. We find that the coefficient of the interaction term *Recalibrated Dummy* × *Post* is positive and significant, which indicates that the recalibration has a disproportional effect of about 0.7 notches on the Moody's ratings of the treatment group relative to the control group. In addition, there is no evidence of a significant differential effect on S&P ratings.

Figure 2 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). Treatment and control groups show no

[16] We find that the interaction term *Recalibrated* × *Post* coefficient remains positive and significant in the sample of issues with both Moody's and S&P ratings (the same sample used to generate the results of the placebo test).

significant differential changes before the recalibration. The treatment group undergoes a significantly larger increase in ratings at the time of the recalibration, a difference that persists for up to three years. Figure 3 also shows that there are no significant changes in the S&P ratings of the treatment and control groups either before or after the recalibration, confirming that channels other than ratings do not seem to drive our effects.

We also estimate the effect of the recalibration on ratings at the bond issue level. Table IA.1 in the Internet Appendix presents the results, which are similar to the county-level and local-government-level results in Table 3. Table IA.2 shows that the results are robust when we use a shorter a sample period of two years before and two years after the recalibration (i.e., the sample period is April 2008 to March 2012).

Next, we study how the recalibration-related upgrades affect the municipal bond primary market. We run regressions similar to those in equation (1), where the dependent variables are the amount of bonds issued and the average offer yield instead of the average rating. Panel A of Table 4 presents the results of county-level regressions. Columns (1) and (2) present results in which the dependent variable in the regression is the logarithm of the total amount of bonds issued by local governments (in millions of dollars) in each county and event year (*Issue Amount*). Columns (3) and (4) present results in which the dependent variable is the average of offer yields (in percentage) in each county and event year (*Offer Yield*). In column (1), the interaction term *Recalibrated* × *Post* coefficient is positive and significant, and indicates that the recalibration has a disproportional effect of 3.1% on the *Issue Amount* of the treatment group relative to the control group for a one standard deviation increase in the *Recalibrated* variable. Column (2) shows a similar differential effect on the *Issue Amount* when we include county-size decile-by-year fixed effects. In columns (3) and (4), we find that the *Offer Yield* of the treatment group experiences a larger reduction after the recalibration than the *Offer Yield* of the control group.

We also study the effects on the municipal bond primary market at the local government level (similar to equation (2)). Panel B in Table 4 presents the results of the regression of the

logarithm of the *Issue Amount* (columns (1) and (2)) and *Offer Yield* (columns (3) and (4)). The dependent variables are the total amount of bonds issued and the average offer yield across all new issues of a given local government in each event year. The local-government-level results in Panel B are similar to the county-level results in Panel A. The treatment group has a large and statistically significant relative increase in the *Issue Amount* following the recalibration. The treatment group increases the *Issue Amount* after the recalibration 16%–20% more than the control group. We also find that the *Offer Yield* of the treatment group decrease significantly more than that of the control group, at the local government level, following the recalibration. The estimated reduction in the *Offer Yield* is 13–14 basis points. The magnitude of the differential effect on offer yields is similar to that in Cornaggia, Cornaggia, and Israelsen (2015). When we use the sample of urban counties, the effects on the *Issue Amount* (columns (5) and (6)) and *Offer Yield* (columns (7) and (8)) are slightly stronger at both the county level (Panel A) and local government level (Panel B).

Figure 4 shows the effect of the recalibration on the amount of bonds issued by the treatment and control groups from three years before the recalibration up to three years after. The figure shows no evidence of significant pre-existing differential trends between treatment and control groups. We then see a significantly higher *Issue Amount* in the year of the recalibration and in subsequent years for the treatment group versus the control group. The control groups suffers a decline in the *Issue Amount* after the recalibration, while the treatment group avoids this decline and even increases slightly the *Issue Amount*.

We perform several robustness checks of the effects on the *Issue Amount* and *Offer Yield* variables. These robustness checks are shown in the Internet Appendix. Table IA.3 presents the results of regressions of the logarithm of the *Issue Amount* and *Offer Yield* at the bond issue level. The bond-issue-level results are qualitatively similar to the county-level and local-government-level results in Table 4. We also estimate the bond-issue-level and local-government-level regressions using: (1) a sample period with only two years before and two years after the recalibration (i.e., the sample period is April 2008–March 2012); (2) a sample of

issues with both S&P and Moody's ratings; (3) a sample of issues excluding BAB; (4) a sample of all local governments, including those that have not issued bonds in the pre-treatment period; and (5) a sample with both uninsured and insured bonds. These robustness checks are shown in Table IA.4. In particular, we find similar effects on the *Issue Amount* and *Offer Yield* in the sample of issues with both S&P and Moody's ratings in columns (3) and (4). In this sample, the information channel is likely to be less important because investors have access to S&P ratings on the same bonds. The regulatory channel thus seems to play a role, at least in the sample with both Moody's and S&P ratings, which typically corresponds to larger issues (likely those with a larger share of institutional investors). Figure IA.1 shows the effect of the recalibration on the average offer yield of the treatment and control groups.

We also explore whether the magnitude of the effect on the *Issue Amount* and *Offer Yield* is different according to the magnitude of the upgrade. Table IA.5 shows that the effect is generally larger in magnitude for upgrades of two or three notches relative to those of one notch, although the differences are not always statistically significant. Last, Table IA.6 shows that the effects on the *Issue Amount* and *Offer Yield* are similar when the county-level *Recalibrated* variable is calculated using the amount of bonds issued by each local government during the pre-recalibration period.

3. Effect on Local Government Expenditures and Employment

In this section, we study the effect of the ratings recalibration on local government expenditures and employment. The evidence in Section 2 shows a reduction in the offer yield of approximately 14 basis points, which implies a saving in local governments' interest expenses. However, the potential increase in government expenditures may not be limited to the savings in interest expenses. A reduction in the cost of funding can make potentially large projects net present value (NPV) positive, and lead to a large increase in expenditures. At the same time, it is also possible that local governments over-react to the reduction in cost of funding and spend more than a pure NPV rule might justify, therefore we do not have a clear prediction for the size

of expenditures increase given this reduction in the cost of funding.

To estimate the impact of the ratings recalibration on local government outcomes, we first estimate difference-in-differences regressions at the county level of local government expenditures and employment. We estimate panel regressions using the logarithm of the outcome in each county and year as the dependent variable and the specification in equation (1). In these tests, the explanatory variable of interest is the interaction *Recalibrated* × *Post*. *Post* takes a value of one in 2011, 2012, and 2013, because the fiscal year ends in June 30 (just one month after the recalibration in 2010) for local government expenditures, and employment in the Census of Government is measured as of the week of March 12 of each year (just before the recalibration in 2010). The regressions include county-level controls, county fixed effects, state-by-year fixed effects, and, in some specifications, county-size group-by-year fixed effects. Standard errors are clustered at the county level.

The regressions consider two alternative sample periods: 2007–2013 and 2009–2012. We also consider two samples of counties. The first sample includes counties in which at least one local government issued bonds in the four-year period leading up to the recalibration (April 2006–March 2010). This sample of bond issuers excludes counties without any bond-issuing entities from the control group. The second sample is restricted to urban counties.

We also estimate difference-in-differences regressions at the local government level of expenditures and employment (akin to equation (2)). Here, the explanatory variable of interest is the interaction of the *Recalibrated Dummy* with the *Post* dummy variable. The regressions include local government fixed effects, as well as local government type-by-year fixed effects (which accounts for potential differences in the response to the 2007-2009 financial crisis and the subsequent economic recovery by type of local government) and county-by-year fixed effects (which absorbs local economic shocks). This means that comparisons are made between groups of local governments within-type (i.e., county, city, township, school district, or special district) and within-county in each year.

3.1 Local Government Expenditures

We test whether the positive shock to the supply of municipal bond financing affected government expenditures and employment. Table 5 presents the results of difference-in-differences regressions using the logarithm of local government expenditures as the dependent variable. Panel A shows results at the county level, and Panel B shows results at the local government level. Columns (1) and (2) present the results using the 2007–2013 period, and columns (3) and (4) present the results using the 2009–2012 period.[17]

The county-level estimates indicate that the recalibration is associated with a relative increase in local government expenditures of 2.7%–6.1%.[18] A one standard deviation increase in the fraction of upgraded local governments in a county (a change of 0.094 in the *Recalibrated* variable, as shown in Table 1, Panel B1) increases local government expenditures by about 0.5% using the estimate in column (3), Panel A. When we use the sample of urban counties in columns (5)-(8), Panel A, the effects are larger in magnitude and statistically significant.

The local-government-level estimates in Panel B are consistent with those using county-level data. In column (3) of Table 5, Panel B, the differential increase in local government expenditures is 2.2% when we include local government type-by-year fixed effects and county-by-year fixed effects, and it is stronger at 2.9% when we include local government type-by-county-by-year fixed effects in column (4). Notice that, in this specification, we compare expenditures before and after the recalibration for the same type of local government within the same county and year, eliminating many alternative explanations for the observed effects. The corresponding estimates in columns (1) and (2) where a longer event window is used are smaller in magnitude at 0.7%–1.9%. In the sample of urban counties in columns (5)-(8), Panel B, the

[17] Local governments are responsible for many services and infrastructures. According to the Census Bureau's 2010 Survey of Public Employment and Payroll, local governments employ about 11 million people, of which about 60% work in the education sector. The recalibration took place at a time when local governments were facing severe financial constraints as a consequence of the 2007–2009 recession. Global Research (2010), among many others, reports: "Confronting massive budget deficits, school districts throughout the country have been sending out notices ('pink slips') to employees this spring, warning them that they are unlikely to have a job in the fall."

[18] This economic magnitude corresponds to a shock in which 100% of local government units within a county are upgraded.

effects are slightly stronger at 2.3%–3.8%, and statistically and economically significant.

Figure 5 shows the evolution of local government expenditures before and after the ratings recalibration for the treatment and control groups to account for the possibility of pre-existing trends. The two groups follow similar trends before the recalibration, although the difference between the two groups becomes smaller right before the recalibration. This suggests that there may have been some anticipation of the recalibration on the part of local governments (who may have limited some expenses before the shock). Expenditures then increase for the treatment group after the recalibration, whereas they continue their negative trend for the control group.

The Internet Appendix shows additional results for local government expenditures. Table IA.7 addresses whether the regulatory channel plays a role in generating the effects on expenditures. We split the sample into governments in which the rating from Moody's is lower than that from S&P and governments in which the rating from Moody's is equal to or higher than that from S&P. The regulatory channel is more likely to play a role for local governments in which Moody's rating is lower because institutional investors have to rely on the lowest rating to calculate capital requirements. We find that the effect on local government expenditures is positive and significant for both groups, but the effects are economically stronger in the sample of issues in which the rating from Moody's is lower than that from S&P.[19]

Table IA.8 shows separate results for county-level current expenditures and capital outlays. Current expenditures represent on average about 80% of local government total expenditures, whereas capital outlays represent about 20%. We find positive effects in both components of expenditures. The recalibration is associated with a positive and significant increase in current expenditures, in line with estimates for total expenditures in Panel A of Table 5. In addition, there is a large effect for capital outlays, and the estimates are statistically significant in the shorter event window and sample of urban counties.

[19] We also find a positive and significant coefficient on the amount of bonds issued (the coefficient is 0.133), and a negative and significant effect on offer yields (the coefficient is -0.321) in the sample of issues with Moody's ratings lower than the S&P rating. In contrast, the effect on offer yields is insignificant in the sample of issues with Moody's ratings equal to or higher than the S&P rating.

Table IA.9 shows the effects of the recalibration on local government taxes (mostly property taxes). We find that upgraded local governments significantly decrease taxes in the sample of bond issuers. The estimates are negative but insignificant in the sample of urban counties. This suggests that local governments may use the proceeds from municipal bond issues to both alleviate spending cuts (or increase spending), and to prevent tax and fee increases (or reduce taxes). Figure IA.2 shows the evolution of local government taxes before and after the ratings recalibration for the treatment and control groups.

3.2 Local Government Employment

One possible use of funds obtained through financing is to directly hire (or maintain) local government employees. Table 6 present the results of difference-in-differences regressions using the logarithm of local government employment as the dependent variable. We present the same specifications as in Table 5 for government expenditures.

Panel A of Table 6 shows the results of county-level regressions. We find that the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at 4%–6%. A one standard deviation increase in the fraction of upgraded local governments in a county increases local government employment at the county level by about 0.5% (using the estimate in column (3), Panel A). Panel B of Table 6 shows the results of local-government-level regressions. The effects range from 0.2% to 1.8% but are statistically insignificant.

Figure 6 shows the evolution of government employment before and after the ratings recalibration. The two groups follow similar trends before the recalibration and there is a modest increase for the treatment group relative to the control after the recalibration.

4. Effect on Private Employment and Income

To estimate the impact of the ratings recalibration on economic outcomes, we estimate county-level difference-in-differences regressions of private employment and income. We use county-level employment because of potential spillovers across smaller geographic units.

Although an upgraded local government can hire (and thus we can measure its employment creation), it is unlikely that the private-sector effects would be limited to a small area. We use counties as a compromise between even larger units (e.g., MSA) and smaller ones (e.g., ZIP code or census tract). We estimate panel regressions using the logarithm of private employment or income in each county and year as the dependent variables. The specifications are equivalent to those in Panel A of Tables 5 and 6 for local government outcomes at the county level (akin to equation (1)).[20]

4.1 Private Employment

When we study the effects of Moody's recalibration on private employment, the *Post* variable takes a value of one in 2011, 2012, and 2013, because employment in the CBP data is measured as of the week of March 12 of each year.

Table 7 presents the results of difference-in-differences regressions using the logarithm of private employment as the dependent variable. Panel A shows results using the sample of counties with bond issuers, and Panel B shows results using the sample of urban counties.

In column (1), Panel A, the interaction term *Recalibrated* × *Post* coefficient is 5.9%, significant at the 1% level, and include state-by-year fixed effects, which controls for time-varying regional economic shocks. The differential increase in private employment when we include county-size decile-by-year fixed effects is 3.7% (column (2)). The corresponding results in columns (3) and (4) where a shorter event window is used are smaller in magnitude at 2.1%–3.2%, but still statistically significant. The results indicate that a one standard deviation increase in the fraction of upgraded local governments in a county increases private employment by 0.3% (using the estimate in column (3), Panel A). The estimates in Panel B using the sample of urban counties are generally smaller but still statistically significant (with the exception of column (3)).

[20] We obtain similar estimates (untabulated) when we estimate cross-sectional regressions using growth rates (the log change in the outcome variable in a given county from 2009 to 2011) as the dependent variable (instead of panel regressions) for the main outcome variables (local government expenditures and employment, private employment, and income).

Figure 7 shows the evolution of private employment before and after the recalibration for the treatment and control groups. The two groups follow similar trends before the recalibration. Private employment increases for the treatment group in the year of the recalibration, and the gap keeps expanding in the subsequent years, consistent with the multiplier effect of government spending taking some time to play out fully. Figure 8 shows the evolution of private employment before and after the recalibration for the treatment and control groups using quarterly data. The evolution of private employment at the quarterly frequency is consistent with that of the annual frequency in Figure 7.

4.2 Private Employment by Sectors

Given the effects on private employment, we next examine the effects of the ratings recalibration on employment by sector. We expect the impact of the expansion in government spending to show up foremost in sectors that depend on local demand (specifically the non-tradable sector) or on transfers from the government sector. We separately track employment by two-digit NAICS codes and show the results in Table 8.

The results in columns (1) and (2) for tradable sector employment (manufacturing; NAICS codes 31–33) are statistically insignificant, consistent with this sector's dependence on non-local and more dispersed demand. Columns (3) and (4) present the results for the non-tradable sector employment (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). In column (3), the interaction term *Recalibrated* × *Post* coefficient is 0.095, significant at the 1% level, when we include state-by-year fixed effects in the regression. The interaction term coefficient in column (4) is 0.065, significant at the 10% level, when we include county-size decile-by-year fixed effects in the regression. These results imply that a one standard deviation increase in the fraction of upgraded local governments in a county increases non-tradable employment by 0.6%–0.9%.

Government spending is more likely to occur in such sectors as construction and education.

Table 8 also presents results for employment in the construction sector (columns (5) and (6)), and education sector (columns (7) and (8)). The *Recalibrated* × *Post* coefficient is insignificant for the construction sector. The effects for the education sector are positive and significant, consistent with the fact that it receives transfers and grants from local governments. A one standard deviation increase in the fraction of upgraded local governments in a county increases education employment by 1%. Panel B shows that sector-level employment estimates are consistent but with lower magnitudes (and imprecisely estimated) when we use the sample of urban counties.

Figure IA.3 in the Internet Appendix shows the evolution of non-tradable employment before and after the recalibration for the treatment and control groups at the quarterly frequency. The two groups follow similar trends before the recalibration. Non-tradable employment increases for the treatment group in the year of the recalibration and keeps increasing in the subsequent years, but it remains constant (or grows at a slower rate) for the control group.

4.3 Income

We also examine the effects of Moody's ratings recalibration on county-level income (i.e., adjusted gross income from the IRS). In the case of the income variable, *Post* takes a value of one in 2010, 2011, and 2012, because the income variable is measured over the 12-month period that ends in December. The sample period is 2006–2012.

Table 9 presents the results of regressions that are equivalent to those in Table 7 for private employment. In column (1), Panel A, the interaction term *Recalibrated* × *Post* coefficient is 0.06, significant at the 1% level, when we include state-by-year fixed effects in the regression. In column (2), the differential increase is 0.028, significant at the 10% level, when we include county-size decile-by-year fixed effects in the regression. The corresponding results in columns (3) and (4) where a shorter event window is used are similar at 2.9%–4.9%, and statistically significant. A one standard deviation increase in the fraction of upgraded local governments in a county increases income by 0.5% using the estimate in column (3), Panel A. Panel B shows estimates of similar magnitude using the sample of urban counties with a range of 1.0%–3.6%

and statistically significant with the exception of column (4).

Figure 9 shows the evolution of income in the three-year periods before and after the recalibration for the treatment and control groups. The income processes of the two groups follow similar trends before the recalibration, and a gap emerges in the year of the recalibration. In the subsequent three-year period, the gap persists and the income processes again follow similar dynamics.

4.4 Robustness

The Internet Appendix presents robustness checks of our results on county-level economic outcomes: government expenditures, government employment, private employment, and income. Table IA.10 shows the results of specifications that include separate time trends for the counties in the treatment and control groups. This specification mitigates concerns about pre-existing differential trends. The estimates are similar in magnitude and remain statistically significant.

We also perform a series of robustness tests to guarantee that the results are not driven by a lack of comparability between treatment and control groups. Table IA.11 presents the results when we restrict the sample to counties that have at least two issuers (Panel A) and when we exclude counties below the 20th percentile of the amount of bonds issued (Panel B). These tests guarantee that our results are not driven by counties with a small exposure to the municipal bond market and may not be a good control group for our treatment counties. Panel C of Table IA.11 presents the results when we restrict the control group to counties without issuers rated by Moody's (i.e., counties where all issuers are rated by S&P or Fitch), which mitigates concerns that our results are driven by a Moody's ex-ante selection of counties based on their creditworthiness. Finally, Figure IA.4 shows that there are no significant differential effects in the house price index of the treatment and control groups before or after the ratings recalibration. Thus, there is no evidence that the 2007-2009 financial crisis had a differential effect on treatment and control groups or that the treatment group recovered faster than the control group from the crisis.

Table IA.12 shows the results using the sample of all counties for which we are able to obtain

data on economic outcomes, which comprises more than 90% of the counties in the United States. In this sample, the control group also includes counties without exposure to the municipal bond market (i.e., counties where local governments did not issue bonds in the municipal bond market in the four-year period before the recalibration). The estimates are similar in magnitude and statistically significant.

5. Fiscal Multipliers

Our results support a positive relation between municipal bond rating upgrades and bond financing, government expenditures and employment, private employment, and income. To interpret the magnitude of the results, we estimate local fiscal multipliers for employment (i.e., the increase in jobs from a marginal million dollars in government spending) and income (i.e., the dollar change in income produced by a one-dollar change in government spending). These multipliers are interpreted as the impact of local policy interventions that include direct impacts of government spending (e.g., purchases or hires), as well as impacts through indirect channels (e.g., economic activity created by additional government spending).

We use instrumental variables methods to estimate the fiscal multipliers. We instrument for local government expenditures at the county level using the exogenous variation due to Moody's 2010 recalibration. The instrument is the interaction variable *Recalibrated* × *Post*. We estimate the effect of government spending on government employment, private employment, and income using two-stage least squares in the 2007–2013 county-year panel with county, state-year, and, in some specifications, county-size decile-by-year fixed effects.

Table 10 presents the instrumental variables estimates. Panel A shows results using the sample of bond issuers, and Panel B shows results using the sample of urban counties. The first-stage regressions are similar to columns (1) and (2) of Panel A of Table 5. *F*-statistics are above 10 in the sample of urban counties in Panel B, but they are usually below 10 in the sample of bond issuers in Panel A. For this reason, we use the estimates in Panel B for the sample of urban counties to generate our estimates of the fiscal multiplier.

The dependent variables are the logarithm of government employment, private employment, and income, so the estimated coefficients are elasticities and must be transformed to recover fiscal multipliers. Per the definition of the elasticity, we multiply the coefficient in each regression by the ratio of government employment, private employment, or income to local government expenditures evaluated at the mean of the data. Following the literature, we estimate the multipliers using the increase in government spending instead of the increase in bond financing.

The creation of local government jobs is calculated as the product of the estimate in column (1) of Table 10, Panel B, by the average ratio of local government employment to government spending by county. The estimates indicate that a marginal million dollars in local government spending results in 6 jobs (= 0.963 × 6.0) in the local government sector.

The elasticity in column (3) of Table 10 can be translated into the corresponding increase in private sector jobs by multiplying it by the average ratio of private employment to government spending. The results indicate that a marginal million dollars in local government spending results in 45 jobs (= 0.440 × 102.7 in the private sector). Overall, our results suggest that $1 million in spending increases total employment (local government and private) by 51 jobs (= 6 + 45), which corresponds to a cost per job created of $20,000 (the inverse of the local employment multiplier).

The marginal increase in income is obtained as the product of the estimate in column (5) by the average ratio of income to government spending by county. This implies that government spending has a local income multiplier of 1.9 (= 0.326 × 5.8). Combining the income and employment multipliers, we estimate that the jobs created have a remuneration of 1.9 × $20,000 = $38,000.

We obtain similar estimates for government employment, private employment, and income in columns (2), (4), and (6), respectively, when we include county-size decile-by-year fixed effects in the regressions. Table IA.13 in the Internet Appendix shows that we also obtain consistent estimates when we use the sample of all counties.

Although we use a different setting, our estimates are similar to those in the recent literature that exploit cross-sectional (geographic) variation. Cohen, Coval, and Malloy (2011) use changes in congressional committee chairmanships as a source of variation in state-level federal expenditures and find that public spending crowds out private sector investment over a long period of time. Suarez-Serrato and Wingender (2014) exploit variation in federal spending directed to counties due to changes in the local population count after each decennial census and estimate a local income multiplier of 1.57 and a cost per job of $30,000. Shoag (2015) uses differences in returns to state pension funds as windfall shocks to state finances that are predictive of subsequent spending patterns, and estimates a state-level spending multiplier of 2.1 and a cost per job of $35,000. Nakamura and Steinsson (2014) use regional variation in U.S. military spending and estimate a state-level multiplier of 1.5, although they find larger multipliers during high slack periods.[21] Chodorow-Reich et al. (2012) use pre-crisis state-level Medicaid spending to extract the exogenous component of state fiscal relief during the 2009 American Recovery and Reinvestment Act and estimate a cost per job of $26,000.[22]

Similar to these papers that exploit cross-sectional variation, we provide estimates of local fiscal multipliers (open economy relative multiplier)—that is, the effect that a *relative* increase in government spending in one region relative to another has on *relative* employment or income. This corresponds closely to contexts in which output and factors of production are at least partially mobile across borders. This approach ignores general equilibrium effects, and it is different from the overall effect of stimulus spending and a national multiplier (closed economy aggregate multiplier). Whether local multipliers are larger or smaller than national multipliers is not clear. Nakamura and Steinsson (2014) study the theoretical mapping from these estimates of

[21] Others examine the role of municipal bonds and local government spending in providing infrastructure and public services. For example, Cellini, Ferreira, and Rothstein (2010) estimate the valuation of investments in school facilities in California by comparing housing prices in school districts where referenda on municipal bond issues passed or failed by narrow margins.

[22] A few researchers have also studied parts of the ARRA. Wilson (2012) use exogenous formulary allocation factors such as federal highway miles in a state or a state's youth share to instrument government spending. Conley and Dupor (2013) find a positive effect of ARRA transfers on government employment but no positive effect on employment outside of government.

local fiscal multipliers to the national multiplier in an open-economy setting. They show that the cross-sectional estimate of the local fiscal multiplier coincides with the national multiplier only when nominal interest rates are unresponsive.[23]

Our estimates of a cost of $20,000 per job and an income multiplier of 1.9 are in line with the estimates of local fiscal multipliers in the literature. Our multipliers are based on deficit-financed subnational government spending, which tend to be lower than multipliers based on windfall-financed (federal) government spending if private consumption and investment are crowded out. However, this crowding-out effect is likely to have been muted by the low-interest-rate environment during our sample period. In addition, in a neoclassical model, output multipliers based on deficit-financed spending could be larger than multipliers based on windfall spending, because households increase labor supply and hence output as they recognize that increased government spending requires increased future taxes.[24]

Our estimates are consistent with Hall (2009), who argues that GDP multipliers are larger during recessions (when marginal propensity to consume is higher) and when nominal interest rates are near zero, as observed in 2010 at the time of the recalibration. Intuitively, in periods of factor underutilization, government spending shocks are less likely to crowd out private consumption or investment and a fiscal multiplier should be larger.[25] Eggertsson (2008) and Christiano, Eichenbaum, and Rebelo (2011) employ general equilibrium models with some Keynesian features, and suggest that the fiscal multiplier in periods with a binding zero lower bound on nominal interest rates could be somewhere between three and five. Intuitively, with the

[23] Moretti (2010) argues that the local multiplier may be an upper bound on the national multiplier in non-tradable sectors (because factor mobility mitigates crowd-out of private sector production) but a lower bound in tradable goods sectors, as the benefits of the local demand shock spill over to other regions. However, labor mobility is likely small over a period of time as short as that we consider.

[24] Clemens and Miran (2012) use state government spending cuts attributable to institutional rules on budget deficits to estimate a spending multiplier. Unlike other studies where spending changes come from windfall shocks that do not lead to changes in tax liabilities for states or regions, their multiplier estimate for income is about 0.8, which is consistent with a Ricardian effect.

[25] In Keynesian macroeconomic models, relatively high multipliers are associated with high marginal propensities to consume, especially in recessions. In contrast, in neoclassical models, low multipliers are indicative of the crowding out of private consumption and investment due to supply-side factors (labor and fixed assets) or anticipation of future tax liabilities. In Neo-Keynesian models that combine neoclassical modeling with frictions in the economy, the multipliers are somewhere in between.

binding zero lower bound, increases in government spending have no effect on interest rates and thus there is no crowding out of private consumption and investment. In December 2009, the real GDP annual growth was −2.8%, unemployment was about 9.9% (both drawn from the Bureau of Economic Analysis, BEA), and the federal funds rate was 0.12%. Further, the ratings recalibration took place when state and local governments were facing severe financial constraints from the 2007–2009 recession.[26] Our estimates of the fiscal multiplier are also consistent with work on state-dependent multipliers that finds higher multipliers during depressed economic conditions such as the one that prevailed during our sample period (e.g., Auerbach and Gorodnichenko 2012; and Fishback and Kachanovskaya 2015).

6. Effect Heterogeneity

In this section, we investigate whether the effect of the ratings recalibration on local economic outcomes is heterogeneous across different types of regions and local governments.

6.1 Economic Slack

We investigate whether the effects of government spending on the local economy are larger in counties with greater economic slack. Specifically, we estimate panel regressions like those in Tables 5, 6, 7 and 9, and include a triple interaction term, *Recalibrated* × *Post* × *High Slack,* where *High Slack* is a dummy variable that takes a value of one in counties with more economic slack. The coefficient on the triple interaction term measures the differential effect between counties of high and low economic slack. We define low-slack and high-slack counties based on (pre-treatment) unemployment rate below or above the median across counties, respectively.

Table 11 presents the results using the logarithm of government expenditures, government

[26] According to the 2009 Survey of State and Local Finances conducted by the Census Bureau during the 2009 fiscal year, state and local governments faced large budget gaps totaling $900 billion (difference between total revenues and total expenditures), of which more than $200 billion were in local governments. Net savings of state and local governments (difference between current revenues and current expenditures) reached −$217.9 billion in 2009, according to the BEA Survey of Current Business.

employment, private employment, and income as dependent variables. We use specifications with county and size decile-by-year fixed effects (we do not include state-by-year fixed effects because much of the variation in the response to the economic slack variables occurs at the state level). Panels A and B present the results using the sample of bond issuers and the sample of urban counties, respectively.

Panel A of Table 11 shows that our effect is driven by those counties with high unemployment as the triple interaction term coefficient is positive and significant with the exception of the government employment outcome. In addition, the interaction term *Recalibrated* × *Post* is no longer significant, which indicates that the effect is small for counties with lower economic slack. Panel B shows qualitatively similar results for the sample of urban counties but with lower statistical significance for government expenditures and private employment. Taken together, the results support the idea that the multiplier effects of government spending are larger when local economies have greater economic slack.[27]

6.2 Local Government Financial Constraints

We next investigate whether the effects of government spending on the local economy are larger for financially constrained local governments. To investigate the extent to which our results capture financial constraints, we estimate the same regressions as in Panel B of Table 5 (at the local government level) separately for more financially constrained and less constrained local governments. We use local governments' leverage as proxy for financial constraints.

Table 12 presents results including a triple interaction term, *Recalibrated* × *Post* × *High Leverage,* where *High Leverage* is a dummy variable that takes a value of one when the ratio of total debt-to-revenues (as of 2007) is above the median. We expect high-leverage local governments to be more constrained in raising additional debt than low-leverage local governments, and thus benefit more from the recalibration-related upgrades.

We find that the effect of the recalibration on expenditures is more pronounced among high-

[27] Table IA.14 in the Internet Appendix shows that the results are robust when we use the sample of all counties.

leverage local governments, as the triple interaction term coefficient is positive and significant. The interaction term *Recalibrated* × *Post* is insignificant, which indicates that the effect is small for counties with low leverage. Panel B shows qualitatively similar results for the sample of urban counties.

Tables IA.15 and IA.16 in the Internet Appendix present the results for sample splits using alternative measures of financial constraints. We find that the effect of the recalibration is stronger among low-rated local governments (Moody's rating below Aa1), and among school and special districts (as opposed to counties, cities or townships), although the coefficients are imprecisely estimated. This result is consistent with the notion that low-rated local governments and school and special districts are more financially constrained than other type of local governments; for example, because they cannot change taxes.

7. Conclusion

In this paper, we provide estimates of the effect of municipalities' financial constraints on local economies by exploring the exogenous variation in ratings due to the 2010 Moody's recalibration of its U.S. municipal bond ratings scale. The recalibration generates variation in ratings across local governments that is unrelated to local economic conditions, resulting in a zero-to-three-notches upgrade of municipal bonds. Following the recalibration, upgraded local governments raise more bond financing and experience reductions in their borrowing costs relative to non-upgraded local governments.

This upgrades lead to increases in debt-financed government spending as local governments obtain easier access to credit markets. There are positive spillover effects to the private sector. County-level private employment and income respond in a significant way to the positive shock to local government expenditures. The private employment increase is concentrated in the non-tradable sector, which is more directly dependent on local demand.

We show that increases in the supply of financing to local governments can have important effects on the local economy. The effects are driven specifically by changes in ratings of

municipal bonds, and not by changes in local or nationwide fundamentals. Our findings are consistent with the New Keynesian view of the economy in which aggregate demand shocks, such as government spending shocks, have large output multipliers when the economy is in a liquidity trap. Specifically, our findings suggest that debt-financed increases in government spending can improve economic conditions during periods of factor underutilization and near-zero interest rates, such as those observed in many countries in recent years. This improvement in local economic conditions can influence the electoral process and the incumbent's reelection chances. We leave this topic for future research.

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Table 1
Summary Statistics

This table shows mean, median, standard deviation, minimum, maximum, and number of observations for each variable. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The sample period is from April 2006 to March 2013.

	Mean	Median	Standard Deviation	Minimum	Maximum	Nr. of Observations	Nr. of Counties
Panel A: County-Level Variables - Sample of Bond Issuers							
Moody's Ratings	17.86	18.00	1.70	12.00	21.00	4,872	1,319
S&P Ratings	18.61	19.00	1.80	12.00	21.00	2,147	608
Issue Amount ($ million)	170.1	15.3	1,482.7	0.1	66,400.0	6,843	1,859
Offer Yield (%)	2.83	3.03	1.67	0.10	6.60	6,843	1,859
Households (thousand)	53.28	16.41	134.66	0.29	3,133.77	12,348	1,768
Local Government Expenditures ($ million)	765.7	143.1	3,445.8	0.1	108,487.3	12,180	1,740
Local Government Employment (thousand)	6.20	1.37	20.44	0.00	437.54	8,067	1,154
Private Employment (thousand)	60.17	13.59	173.09	0.01	3,910.43	12,346	1,768
Tradable Employment (thousand)	4.39	0.75	15.80	0.00	417.55	8,405	1,407
Non-Tradable Employment (thousand)	14.02	3.57	35.87	0.01	778.39	12,314	1,766
Construction Employment (thousand)	3.09	0.67	8.56	0.00	171.09	12,368	1,768
Education Employment (thousand)	1.75	0.06	7.20	0.00	140.50	11,313	1,676
Income ($ million)	2,924.3	652.6	8,106.7	5.7	197,206.3	12,348	1,768
Recalibrated	0.048	0.000	0.094	0.000	1.000	12,374	1,768
Panel B: County-Level Variables - Sample of Urban Counties							
Moody's Ratings	18.25	18.39	1.61	12.00	21.00	3,168	701
S&P Ratings	18.81	19.00	1.61	12.00	21.00	1,918	485
Issue Amount ($ million)	218.5	45.1	938.8	0. 1	27,388.6	3,912	799
Offer Yield (%)	2.80	3.05	1.70	0.10	6.60	3,912	799
Households (thousand)	93.62	43.39	176.76	1.52	3,133.77	6,501	932
Local Government Expenditures ($ million)	1,400.2	462.3	4,678.8	1.0	108,487.3	6,363	909
Local Government Employment (thousand)	9.28	3.81	24.63	0.00	437.54	5,321	761
Private Employment (thousand)	107.97	42.27	228.00	0.16	3,910.43	6,522	932
Tradable Employment (thousand)	6.23	1.41	18.84	0.00	417.55	5,741	879
Non-Tradable Employment (thousand)	24.85	11.63	46.72	0.03	778.39	6,519	932
Construction Employment (thousand)	5.68	2.28	11.26	0.01	171.09	6,439	932
Education Employment (thousand)	4.06	0.93	10.61	0.00	140.50	4,871	783
Income ($ million)	5,301.9	2,088.0	10,642.4	35.4	197,206.3	6,501	932
Recalibrated	0.066	0.020	0.115	0.000	1.000	6,532	933

Table 2
Treatment and Control Groups Pre-Treatment Characteristics

This table shows pre-treatment means and *p*-values of differences in means (raw and adjusted) between treatment and control groups. The differences are adjusted by state-year fixed effects and county-size decile-by-year fixed effects. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The pre-treatment sample period is from 2006 to 2009. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: County-Level Variables - Sample of Bond Issuers

	Mean			Difference	Number of Observations	
	Recalibrated > 0	Recalibrated = 0	Difference	(adjusted)	Recalibrated > 0	Recalibrated = 0
Moody's Ratings	17.41	17.58	-0.17*	-0.05	1,291	1,498
S&P Ratings	18.66	18.29	0.37**	0.32*	651	576
Issue Amount ($ million)	254.6	104.8	149.8	41.0	1,459	2,483
Offer Yield (%)	3.19	2.97	0.22***	0.29**	1,459	2,483
Households (thousands)	82.12	19.41	62.71***	6.86	2,895	2,868
Private Employment (thousands)	97.52	18.62	78.90***	8.42	2,898	2,866
Fraction of Local Government Employment	0.059	0.052	0.006***	0.005*	2,298	2,278
Growth Local Government Expenditures	0.054	0.050	0.004	0.007*	1,904	1,886
Growth Local Government Employment	0.011	0.019	-0.008*	0.000	1,542	1,518
Growth Local Private Employment	-0.026	-0.029	0.003*	0.000	1,933	1,910
Growth Income	-0.008	-0.009	0.001	0.000	1,930	1,912
Local Government Expenditures ($ million)	1,169.1	287.5	881.6***	-37.6	1,904	2,829
Local Government Employment (thousand)	8.19	2.67	5.52***	-0.47	1,542	2,278
Private Employment (thousand)	97.52	18.62	78.90***	8.42	1,933	2,866
Tradable Employment (thousand)	6.60	1.10	5.49***	1.43**	1,904	2,388
Non-Tradable Employment (thousand)	22.43	4.32	18.11***	2.80**	1,542	2,862
Construction Employment (thousand)	5.69	0.98	4.71***	0.78**	1,933	2,868
Education Employment (thousand)	2.48	0.54	1.94***	-0.08	1,933	2,779
Income ($ million)	4,442. 7	831.7	3,611.0***	456.5	1,930	2,868

Table 2 (continued)

Panel B: County-Level Variables - Sample of Urban Counties

	Mean			Difference	Number of Observations	
	Recalibrated > 0	Recalibrated = 0	Difference	(adjusted)	Recalibrated > 0	Recalibrated = 0
Moody's Ratings	17.87	17.99	-0.12	-0.12	873	927
S&P Ratings	18.83	18.60	0.23	0.13	498	575
Issue Amount ($ million)	213.4	198.8	14.7	-5.3	1,203	1,025
Offer Yield (%)	3.10	2.79	0.31***	0.31**	1,203	1,025
Households (thousands)	124.3	40.4	83.9***	12.8	1,776	1,020
Private Employment (thousands)	150.0	40.4	109.6***	21.0	1,776	1,020
Fraction of Local Government Employment	0.064	0.063	0.001	0.004	1,654	624
Growth Local Government Expenditures	0.051	0.045	0.006	0.011**	1,176	642
Growth Local Government Employment	0.012	0.011	0.001	0.010*	1,104	416
Growth Local Private Employment	-0.026	-0.023	-0.004	-0.004	1,184	680
Growth Income	-0.008	-0.005	-0.002	0.002	1,184	680
Local Government Expenditures ($ million)	1,801.4	666.2	1,135.2***	-117.2	1,764	963
Local Government Employment (thousand)	11.00	5.35	5.65**	-0.58	1,657	624
Private Employment (thousand)	150.00	40.35	109.65***	21.03	1,776	1,020
Tradable Employment (thousand)	9.00	1.67	7.33***	1.96**	1,707	777
Non-Tradable Employment (thousand)	34.13	9.22	24.91***	6.74***	1,776	1,017
Construction Employment (thousand)	8.80	2.25	6.55***	2.10***	1,776	1,020
Education Employment (thousand)	3.83	1.24	2.59***	-0.10	1,771	980
Income ($ million)	6,863.5	1,862.7	5,000.8***	1,070.2**	1,776	1,020

Table 3
Difference-in-Differences of Ratings around the Recalibration

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the average rating across all issues of local governments of each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the average rating across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample in columns (1)–(4) consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of urban counties. The sample period is from April 2006 to March 2013. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P		Rating Moody's		Rating S&P	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: County Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.700***	0.727***	0.150	0.147	0.590***	0.564***	-0.019	0.004
	(0.162)	(0.170)	(0.218)	(0.235)	(0.215)	(0.227)	(0.241)	(0.261)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.31	0.33	0.25	0.30	0.29	0.31	0.25	0.29
Number of Observations	4,872	4,872	2,147	2,147	3,168	3,168	1,918	1,918
Number of Counties	1,319	1,319	608	608	701	701	485	485
Panel B: Local Government Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated Dummy × Post	0.653***	0.666***	0.114	0.167	0.663***	0.666***	0.215	0.167
	(0.067)	(0.076)	(0.199)	(0.289)	(0.071)	(0.077)	(0.224)	(0.289)
Local Gov. Type-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.53	0.61	0.31	0.40	0.52	0.60	0.33	0.40
Number of Observations	11,258	11,258	4,567	4,567	8,941	8,941	4,094	4,094
Number of Local Governments	4,672	4,672	1,720	1,720	3,611	3,611	1,541	1,541

Table 4
Difference-in-Differences of Issue Amount and Offer Yield around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of local governments in each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample in columns (1)–(4) consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of urban counties. The sample period is from April 2006 to March 2013. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield		Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: County Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.334***	0.289***	-0.454***	-0.614***	0.407***	0.408***	-0.591***	-0.600***
	(0.093)	(0.097)	(0.140)	(0.146)	(0.131)	(0.137)	(0.221)	(0.228)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.22	0.24	0.29	0.31	0.32	0.33	0.39	0.41
Number of Observations	6,843	6,843	6,843	6,843	3,912	3,912	3,912	3,912
Number of Counties	1,859	1,859	1,859	1,859	799	799	799	799
Panel B: Local Government Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated Dummy × Post	0.200***	0.164***	-0.141**	-0.127**	0.206***	0.174***	-0.162**	-0.139*
	(0.046)	(0.047)	(0.060)	(0.061)	(0.048)	(0.050)	(0.064)	(0.065)
Local Gov. Type-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.30	0.31	0.53	0.54	0.29	0.30	0.52	0.53
Number of Observations	18,215	18,215	18,215	18,215	13,982	13,982	13,982	13,982
Number of Local Governments	8,114	8,114	8,114	8,114	5,916	5,916	5,916	5,916

Table 5
Difference-in-Differences of Local Government Expenditures around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government expenditures (as of July of each year) around the Moody's recalibration in April–May 2010. Panel A presents county-level results using aggregated expenditures across all local governments in each county and year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. The sample in columns (1)–(4) consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of urban counties. Controls include house price index and number of households. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013		Panel 2009-2012		Panel 2007-2013		Panel 2009-2012	
Panel A: County Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.061**	0.045*	0.053**	0.027	0.095***	0.071**	0.078***	0.044*
	(0.029)	(0.027)	(0.026)	(0.026)	(0.027)	(0.029)	(0.025)	(0.026)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.39	0.39	0.12	0.13	0.47	0.48	0.18	0.20
Number of Observations	12,159	12,159	6,948	6,948	6,349	6,342	3,628	3,624
Number of Counties	1,737	1,737	1,737	1,737	907	906	907	906
Panel B: Local Government Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated Dummy × Post	0.007	0.019	0.022**	0.029**	0.023**	0.028*	0.029***	0.038***
	(0.010)	(0.015)	(0.009)	(0.013)	(0.011)	(0.016)	(0.010)	(0.014)
Local Gov. Type-Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.28	0.39	0.24	0.34	0.24	0.35	0.19	0.31
Number of Observations	21,430	15,054	14,286	9,725	17,756	12,710	11,836	8,222
Number of Local Governments	3,572	2,726	3,572	2,466	2,960	2,271	2,960	2,080

Table 6
Difference-in-Differences of Local Government Employment around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government employment (as of March of each year) around the Moody's recalibration in April–May 2010. Panel A presents county-level results using aggregated government employment across all local governments in each county and year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. The sample in columns (1)–(4) consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of urban counties. Controls include house price index and number of households. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013		Panel 2009-2012		Panel 2007-2013		Panel 2009-2012	
Panel A: County Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.039*	0.057**	0.052**	0.059**	0.075***	0.075***	0.075***	0.072***
	(0.023)	(0.026)	(0.025)	(0.024)	(0.025)	(0.026)	(0.023)	(0.024)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.13	0.13	0.13	0.13	0.19	0.20	0.20	0.21
Number of Observations	8,067	8,067	4,611	4,611	5,300	5,293	3,029	3,025
Number of Counties	1,154	1,154	1,154	1,154	758	757	758	757
Panel B: Local Government Level	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated Dummy × Post	0.006	0.018	0.002	0.006	0.007	0.018	0.004	0.006
	(0.012)	(0.016)	(0.010)	(0.013)	(0.012)	(0.016)	(0.010)	(0.013)
Local Gov. Type-Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.34	0.41	0.31	0.41	0.32	0.40	0.29	0.39
Number of Observations	9,281	4,675	5,303	2,671	8,917	4,633	5,095	2,647
Number of Local Governments	1,326	668	1,326	668	1,274	662	1,274	662

Table 7
Difference-in-Differences of Private Employment around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of private employment (as of March of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2007-2013		Panel 2009-2012	
Panel A: County Level - Sample of Bond Issuers				
Recalibrated × Post	0.059***	0.037**	0.032**	0.021*
	(0.017)	(0.016)	(0.013)	(0.013)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.26	0.27	0.08	0.09
Number of Observations	12,331	12,331	7,050	7,050
Number of Counties	1,765	1,765	1,765	1,765
Panel B: County Level - Sample of Urban Counties				
Recalibrated × Post	0.041***	0.040***	0.014	0.021**
	(0.016)	(0.015)	(0.012)	(0.010)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.38	0.42	0.17	0.27
Number of Observations	6,508	6,501	3,718	3,714
Number of Counties	930	929	930	929

Table 8
Difference-in-Differences of Private Employment by Sector around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of tradable (manufacturing, NAICS codes 31–33), non-tradable (retail, food and accommodation, NAICS codes 44–45 and 72), construction, and education sectors employment (as of March of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The sample period is from 2009 to 2012. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Tradable		Non-Tradable		Construction		Education	
Panel A: County Level - Sample of Bond Issuers								
Recalibrated × Post	-0.023	0.039	0.095***	0.065*	0.001	0.017	0.107***	0.068*
	(0.144)	(0.161)	(0.034)	(0.036)	(0.039)	(0.041)	(0.041)	(0.040)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.06	0.07	0.03	0.05	0.22	0.22	0.12	0.13
Number of Observations	4,670	4,670	7,025	7,025	6,469	6,469	3,480	3,480
Number of Counties	1,244	1,244	1,762	1,762	1,675	1,675	1,675	1,675
Panel B: County Level - Sample of Urban Counties								
Recalibrated × Post	-0.067	-0.085	0.057*	0.033	0.024	0.020	0.083**	0.054
	(0.140)	(0.147)	(0.034)	(0.036)	(0.034)	(0.037)	(0.040)	(0.045)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.08	0.09	0.09	0.17	0.36	0.37	0.15	0.16
Number of Observations	3,229	3,227	3,717	3,715	3,659	3,659	2,722	2,717
Number of Counties	830	829	930	929	923	923	708	707

Table 9
Difference-in-Differences of Income around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of income (as of December of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2010 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2006-2012		Panel 2008-2011	
Panel A: County Level - Sample of Bond Issuers				
Recalibrated × Post	0.060***	0.028*	0.049***	0.029**
	(0.017)	(0.016)	(0.014)	(0.014)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.67	0.67	0.72	0.72
Number of Observations	10,614	10,614	7,076	7,076
Number of Counties	1,769	1,769	1,769	1,769
Panel B: County Level - Sample of Urban Counties				
Recalibrated × Post	0.036***	0.017*	0.029**	0.010
	(0.009)	(0.009)	(0.014)	(0.014)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.77	0.77	0.84	0.84
Number of Observations	5,580	5,574	3,720	3,716
Number of Counties	930	929	930	929

Table 10
Instrumental Variable Estimates of the Elasticity of Employment and Income

This table presents instrumental variables (two-stage least squares) estimates of regressions of the logarithm of government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. Local government expenditures are instrumented with the *Recalibrated* × *Post* interaction variable. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The sample period is from 2007 to 2013 (2006 to 2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Government Employment		Private Employment		Income	
Panel A: County Level - Sample of Bond Issuers						
Local Gov. Expenditures	0.648	1.336*	1.195***	1.026*	0.912***	0.634*
	(0.474)	(0.779)	(0.393)	(0.535)	(0.324)	(0.377)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic of instrument (first stage)	5.34	3.90	10.73	4.60	9.51	4.24
Number of Observations	8,046	8,046	12,135	12,135	12,166	12,166
Number of Counties	1,151	1,151	1,737	1,737	1,738	1,738
Panel B: County Level - Sample of Urban Counties						
Local Gov. Expenditures	0.963***	1.189***	0.440***	0.662***	0.326**	0.272*
	(0.368)	(0.434)	(0.151)	(0.256)	(0.138)	(0.160)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic of instrument (first stage)	12.90	11.15	24.35	11.75	12.23	11.75
Number of Observations	5,300	5,293	6,348	6,341	6,349	6,342
Number of Counties	758	758	907	907	907	907

Table 11

Difference-in-Differences of Economic Outcomes around the Recalibration: Effect of Economic Slack

This table presents difference-in-differences estimates of regressions of the logarithm of government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. *High Slack* is a dummy variable that takes a value of one when the county-level unemployment rate in 2010 is above the median. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The sample period is from 2007 to 2013 (2006 to 2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel A: County Level - Sample of Bond Issuers								
Recalibrated × Post	-0.021	0.008	-0.006	0.033	0.019	0.014	0.022	0.005
	(0.027)	(0.028)	(0.031)	(0.031)	(0.016)	(0.014)	(0.017)	(0.016)
Recalibrated × Post × High Slack	0.108*	0.115**	0.091	0.087	0.047*	0.051*	0.089***	0.079***
	(0.059)	(0.058)	(0.058)	(0.057)	(0.028)	(0.030)	(0.029)	(0.028)
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.29	0.30	0.03	0.21	0.21	0.22	0.60	0.60
Number of Observations	12,180	12,180	8,067	8,067	12,345	12,345	10,626	10,626
Number of Counties	1,740	1,740	1,154	1,154	1,767	1,767	1,771	1,771
Panel B: County Level - Sample of Urban Counties								
Recalibrated × Post	0.017	0.015	0.035	0.047*	0.003	0.004	-0.005	-0.012
	(0.028)	(0.029)	(0.029)	(0.028)	(0.013)	(0.014)	(0.017)	(0.016)
Recalibrated × Post × High Slack	0.067	0.059	0.118*	0.115*	0.039	0.031	0.103***	0.097***
	(0.057)	(0.058)	(0.061)	(0.062)	(0.034)	(0.031)	(0.036)	(0.036)
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.35	0.37	0.07	0.08	0.29	0.33	0.70	0.71
Number of Observations	6,363	6,356	5,314	5,307	6,522	6,515	5,592	5,586
Number of Counties	909	908	760	759	932	931	932	931

Table 12
Difference-in-Differences of Local Government Expenditures around the Recalibration: Effect of Financial Constraints

This table presents difference-in-differences estimates of regressions of the logarithm of expenditures (as of July of each year) of each local government unit and year around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. *High Leverage* is a dummy variable that takes a value of one when the ratio of total debt-to-revenues (as of 2007) is above the median. The sample in Panel A consists of local governments with at least one bond issued in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists local government within urban counties. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2007-2013		Panel 2009-2012	
Panel A: Local Government Level - Sample of Bond Issuers				
Recalibrated Dummy × Post	-0.011	0.005	0.004	0.013
	(0.012)	(0.014)	(0.010)	(0.013)
Recalibrated Dummy × Post × High Leverage	0.054**	0.051	0.050***	0.051*
	(0.023)	(0.035)	(0.019)	(0.030)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.33	0.43	0.27	0.37
Number of Observations	18,954	12,878	12,636	8,225
Number of Local Governments	3,159	2,372	3,159	2,086
Panel B: Local Government Level - Sample of Urban Counties				
Recalibrated Dummy × Post	-0.014	-0.001	0.001	0.009
	(0.013)	(0.015)	(0.011)	(0.014)
Recalibrated Dummy × Post × High Leverage	0.058**	0.067**	0.056***	0.064**
	(0.023)	(0.034)	(0.020)	(0.029)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.31	0.41	0.26	0.36
Number of Observations	15,984	10,953	10,656	7,008
Number of Local Governments	2,664	2,000	2,664	1,776

Figure 1

Local Government Units Upgraded due to the Recalibration by County

The map shows the fraction of local government units in a given county upgraded during the Moody's recalibration (*Recalibrated*). Counties in gray have no local government unit that issued bonds during the four-year period before the recalibration (April 2006–March 2010) (1,373 counties). Counties in white have no upgraded local government unit (811 counties). Counties in light blue, medium blue, and dark blue are in the bottom tercile (327 counties), medium tercile (313 counties), and top tercile (317 counties) of the distribution of the *Recalibrated* variable (considering non-zero values), respectively.

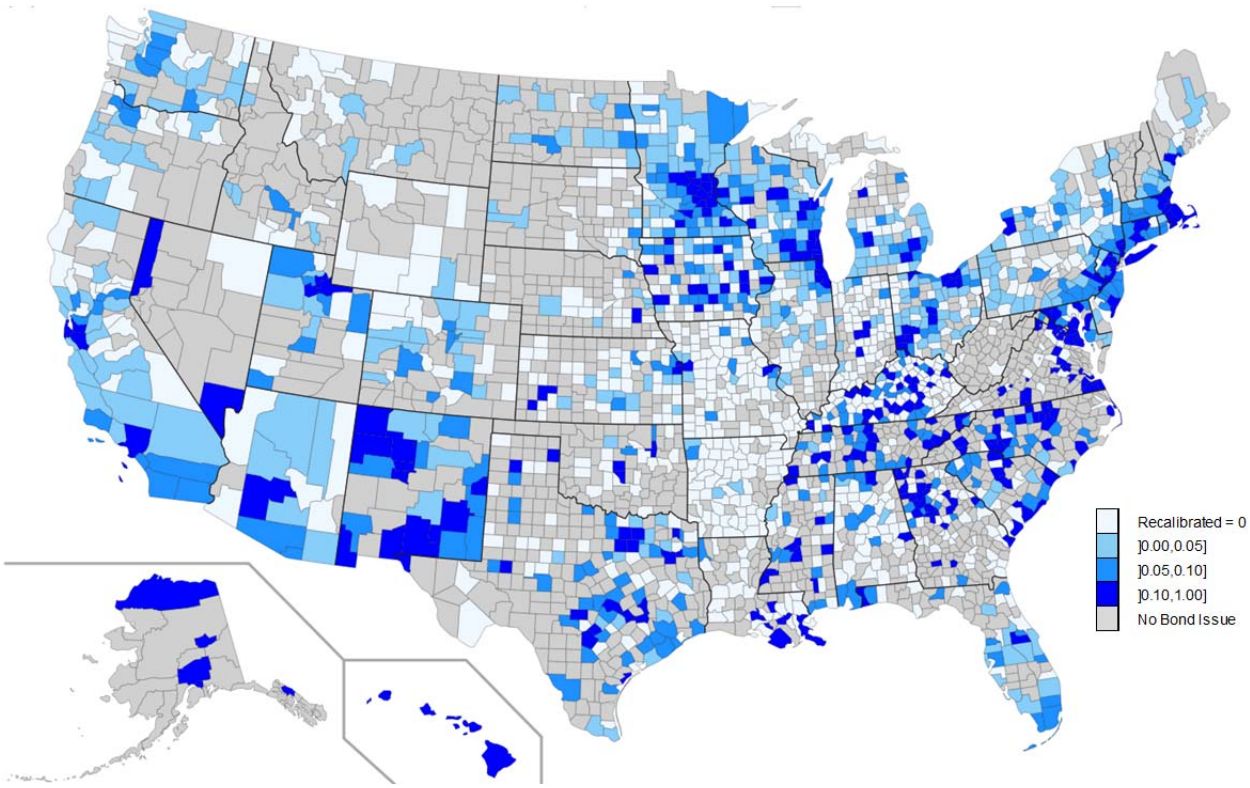
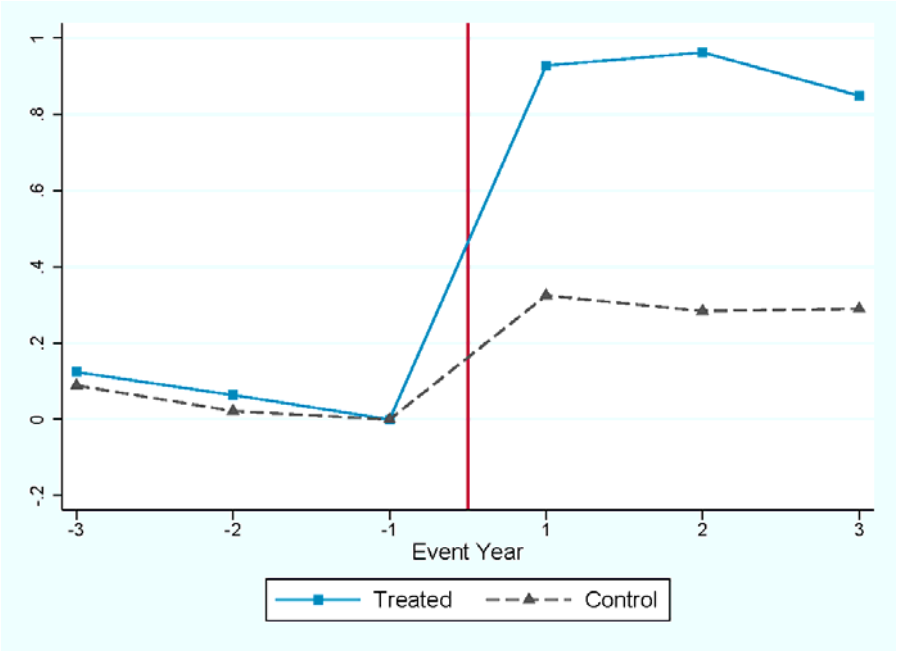


Figure 2
Moody's Ratings around the Recalibration

This figure shows the regression coefficients of Moody's ratings on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in Moody's rating separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Moody's Ratings (relative to year -1)



Panel B: Difference between Treatment and Control Groups

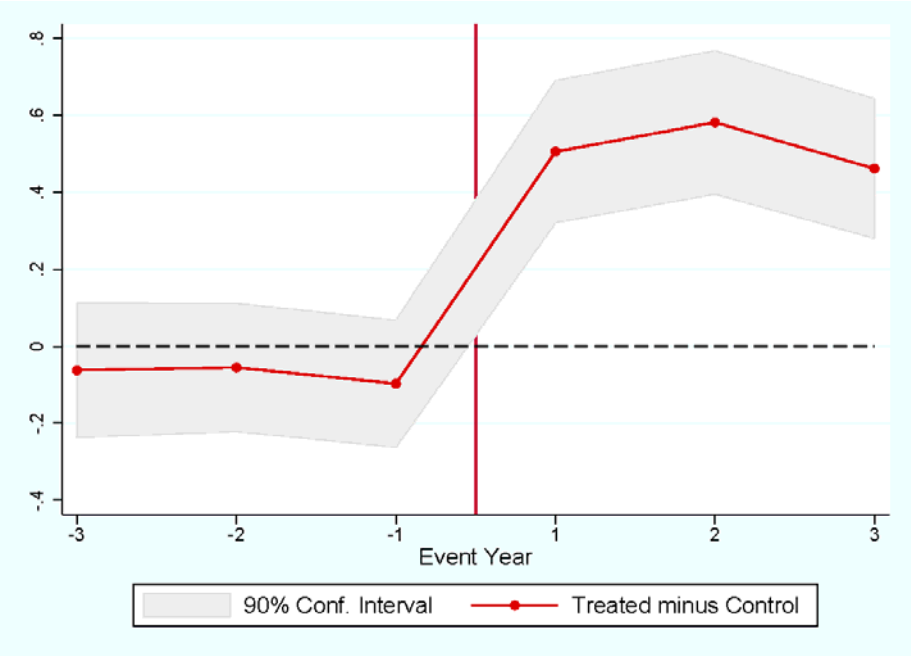


Figure 3
S&P Ratings around the Recalibration

This figure shows the regression coefficients of S&P's ratings on event-year dummies around the Moody's recalibration event in April-May 2010. The figure presents the change in Moody's rating (relative to year -1) separately for upgraded local governments (treated) and non-upgraded local governments (control). The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

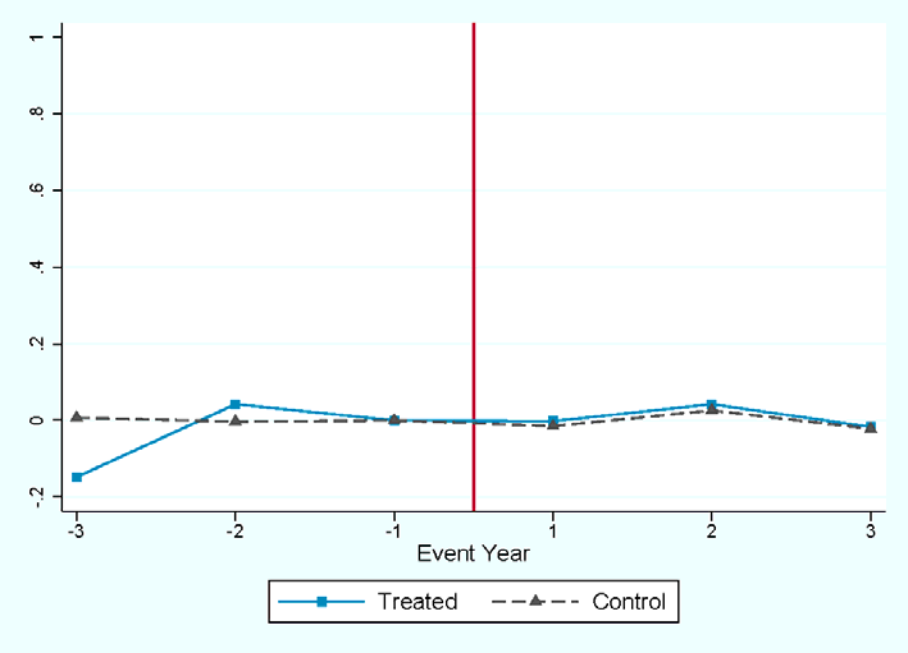
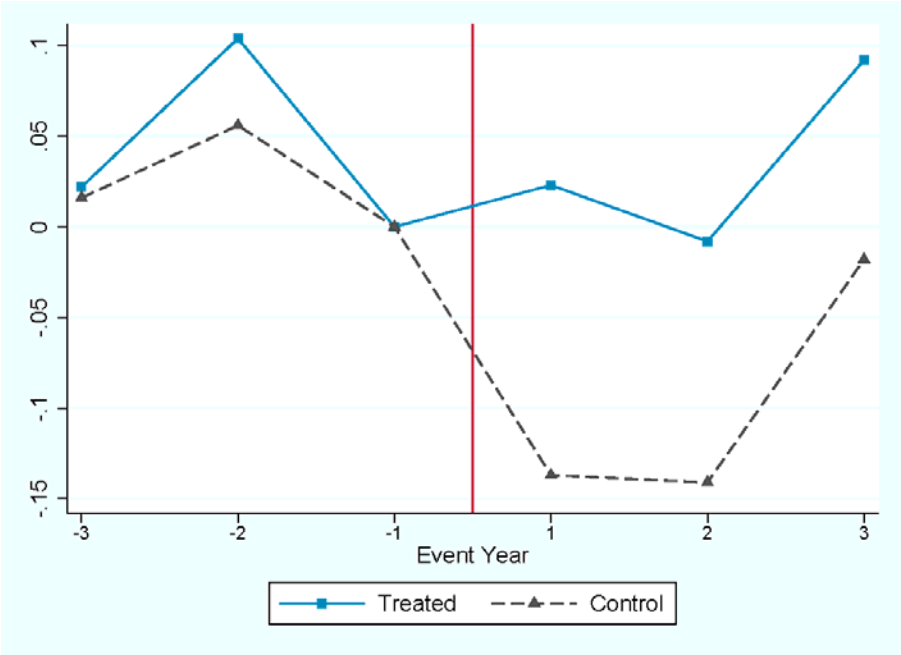


Figure 4
Issue Amount around the Recalibration

This figure shows the regression coefficients of logarithm of the issue amount on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of the issue amount separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Issue Amount (relative to year -1)



Panel B: Difference between Treatment and Control Groups

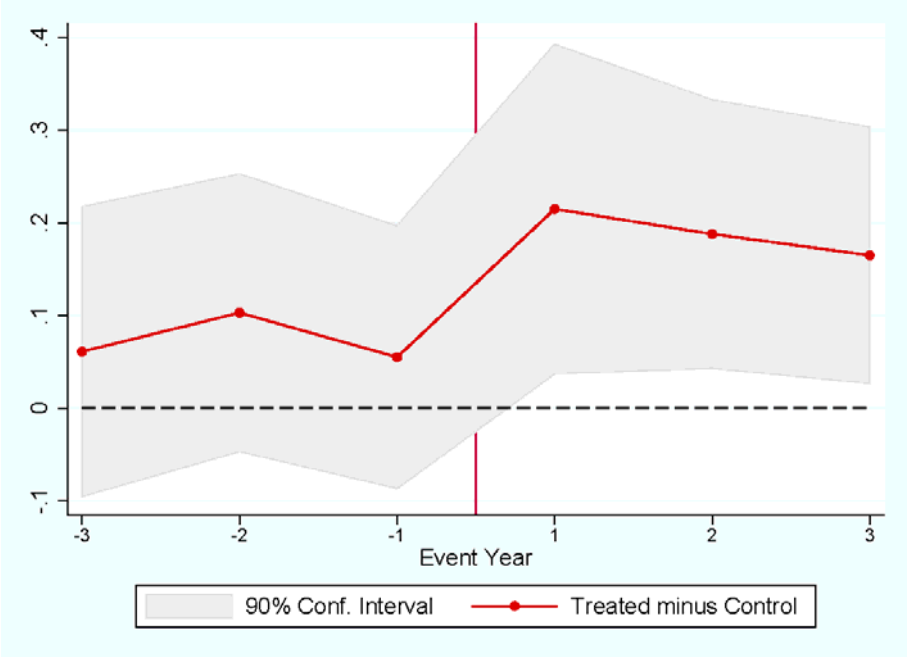
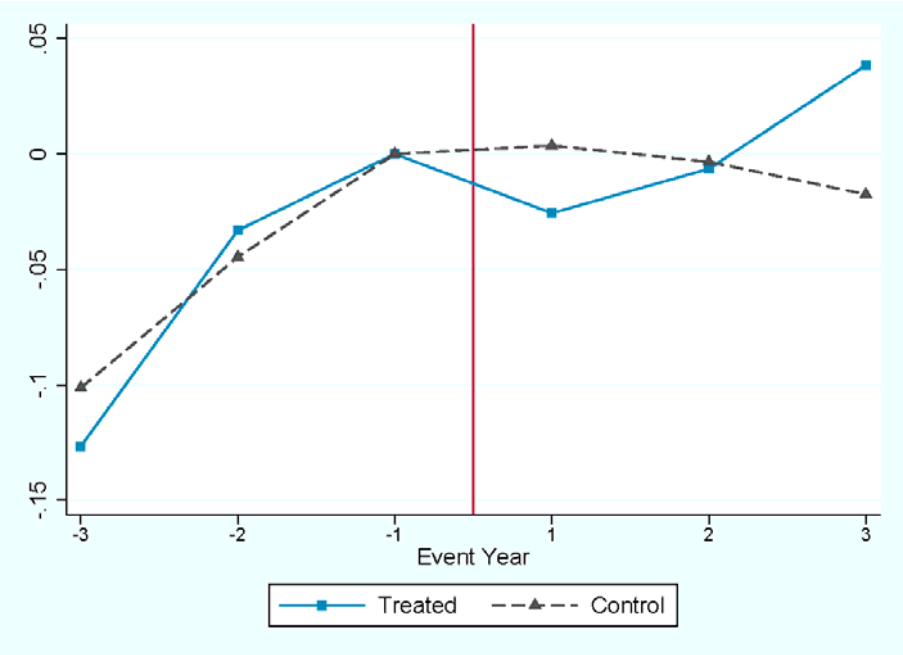


Figure 5
Local Government Expenditures around the Recalibration

This figure shows the regression coefficients of logarithm of local government expenditures (as of July of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of local government expenditure separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Local Government Expenditure (relative to year -1)



Panel B: Difference between Treatment and Control Groups

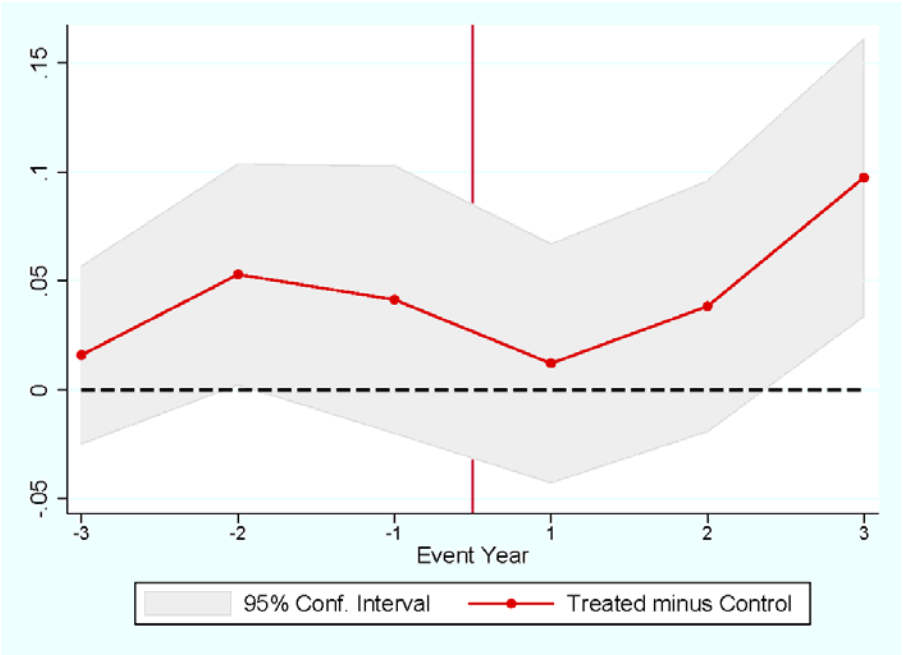
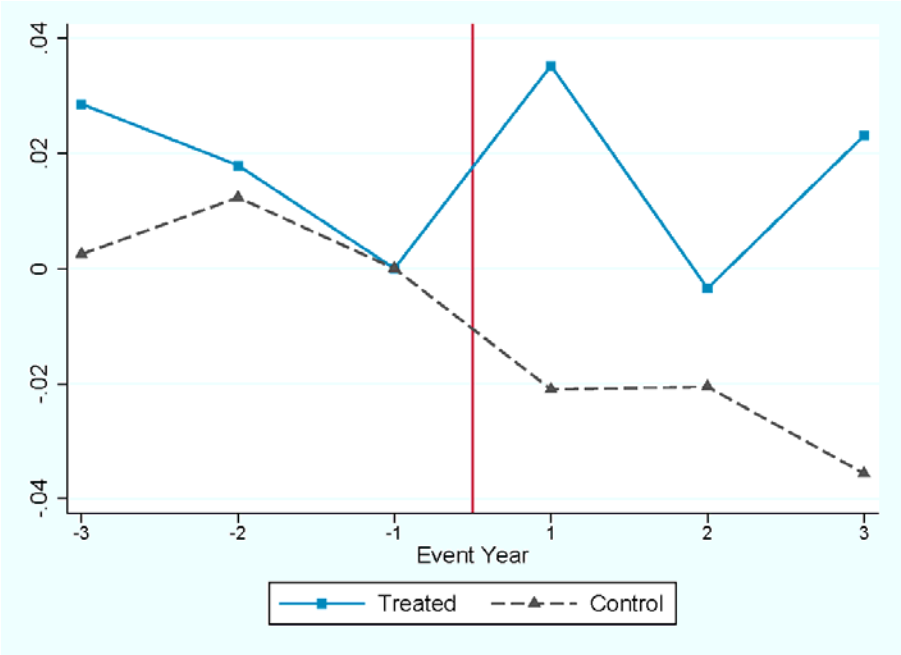


Figure 6
Local Government Employment around the Recalibration

This figure shows the regression coefficients of logarithm of local government employment (as of March of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of local government employment separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Local Government Employment (relative to year -1)



Panel B: Difference between Treatment and Control Groups

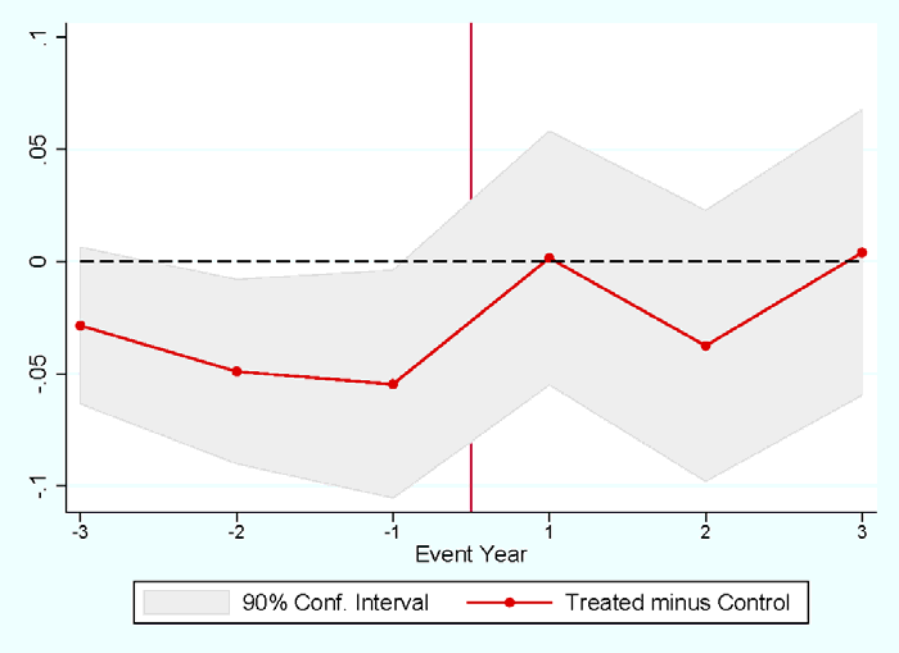
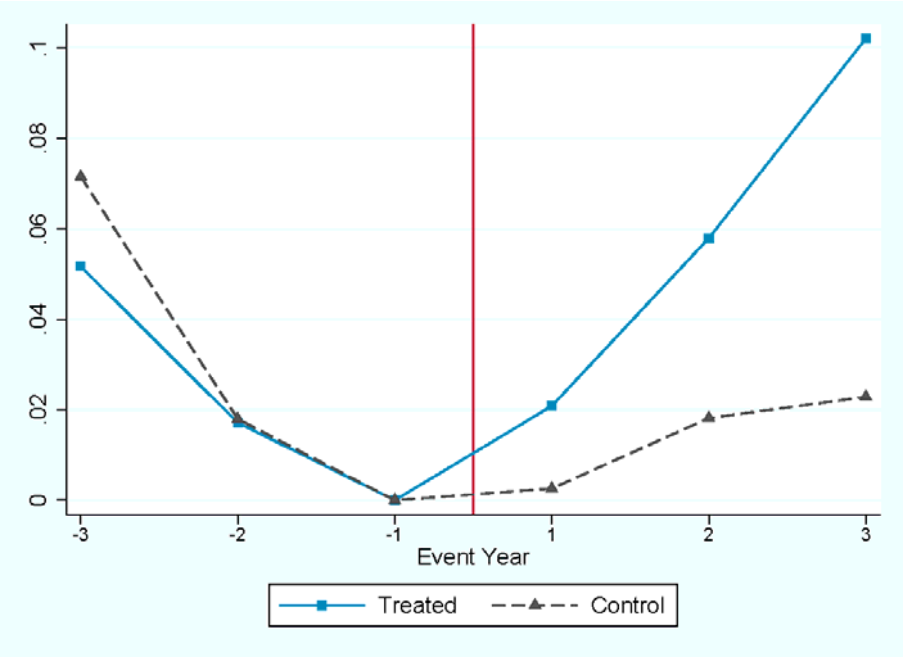


Figure 7
Private Employment around the Recalibration

This figure shows the regression coefficients of logarithm of private employment (as of March of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of private employment separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Private Employment (relative to year -1)



Panel B: Difference between Treatment and Control Groups

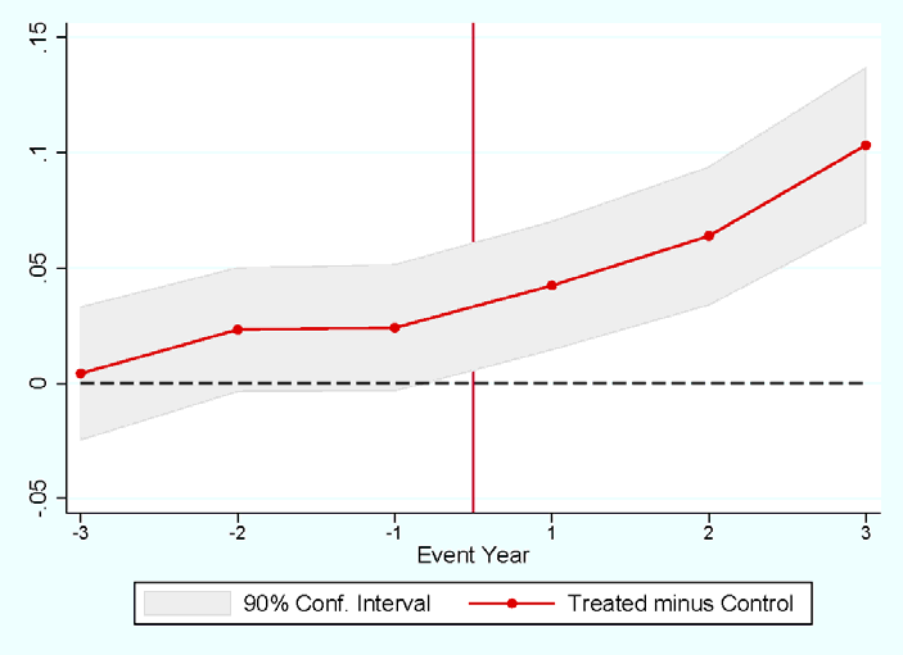
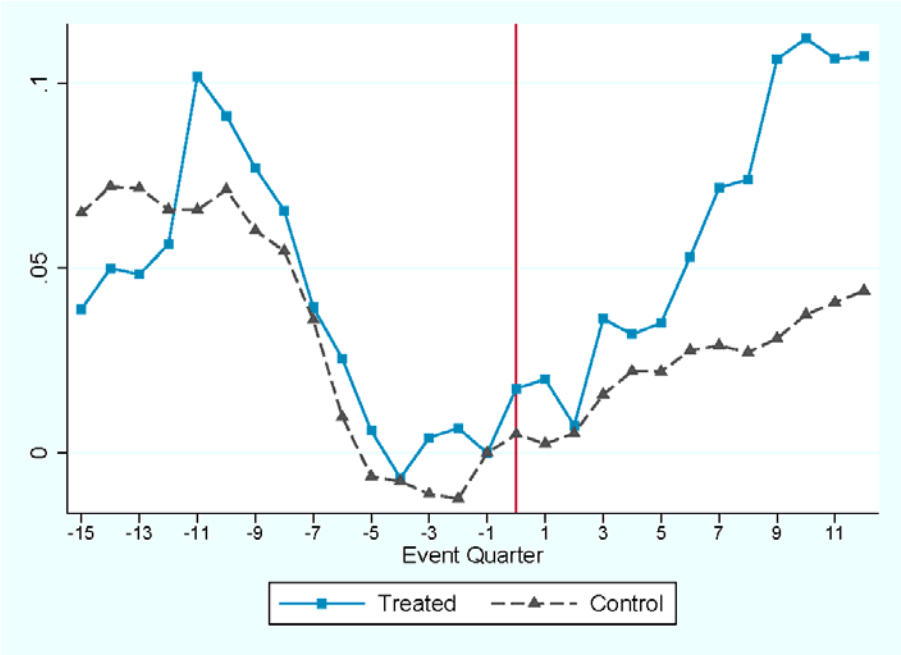


Figure 8
Private Employment around the Recalibration: Quarterly Frequency

This figure shows the regression coefficients of logarithm of private employment (at the end of each quarter) on event-quarter dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of private employment separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Private Employment (relative to quarter -1)



Panel B: Difference between Treatment and Control Groups

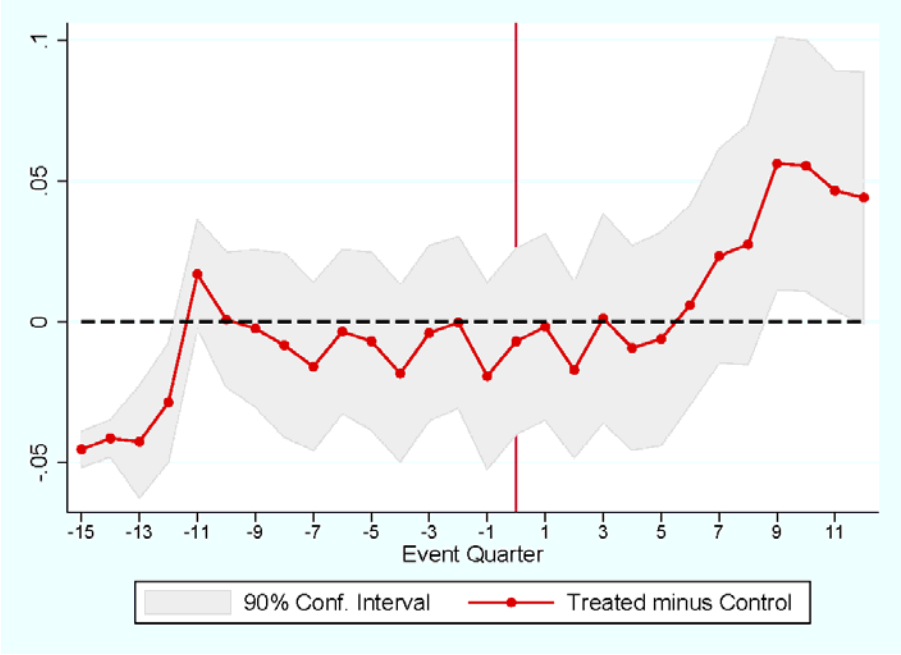
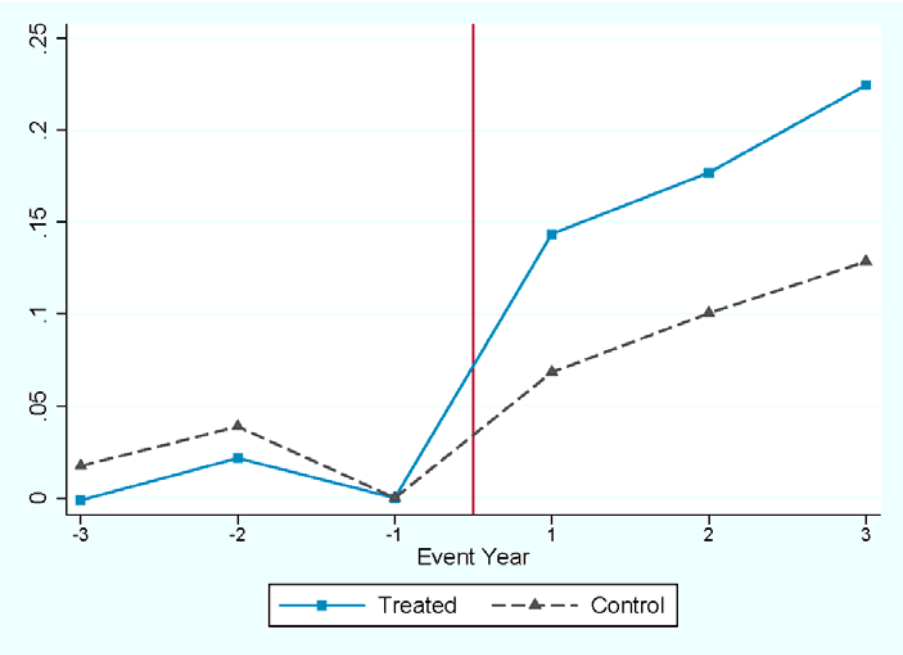


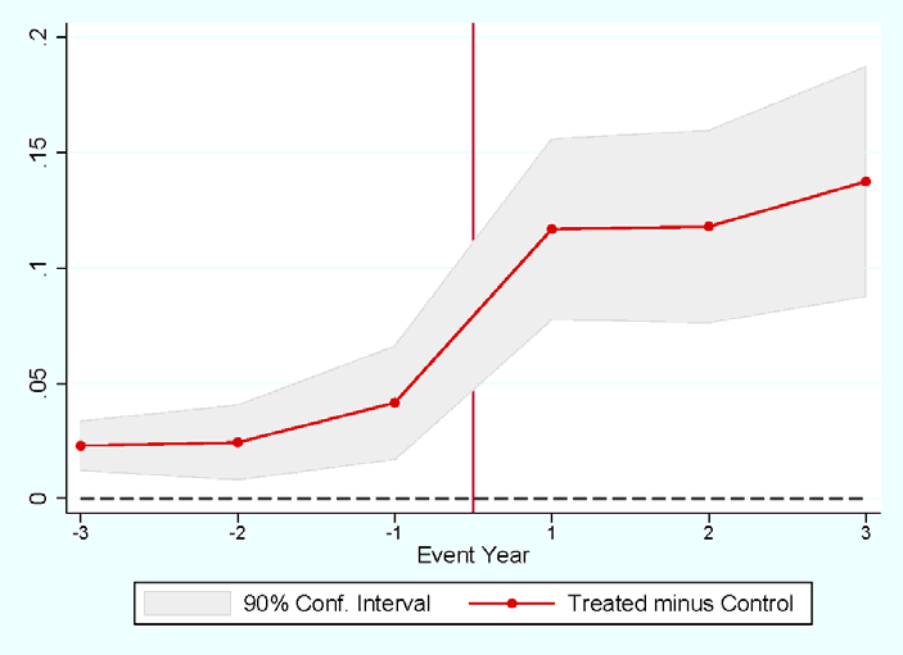
Figure 9
Income around the Recalibration

This figure shows the regression coefficients of logarithm of income (as of December of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of income separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Income (relative to year -1)



Panel B: Difference between Treatment and Control Groups



Internet Appendix to

"The Economic Effects of Public Financing: Evidence from Municipal Bond Ratings Recalibration"

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This Version: January 2017

Table IA.1

Difference-in-Differences Estimates of Ratings around the Recalibration: Bond Issue Level

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of municipal new bond issues in the April 2006–March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P	
	(1)	(2)	(3)	(4)
Recalibrated Dummy × Post	0.562***	0.595***	-0.002	-0.029
	(0.052)	(0.064)	(0.066)	(0.093)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.24	0.46	0.04	0.21
Number of Observations	219,202	219,202	118,376	118,376

Table IA.2
Difference-in-Differences of Ratings around the Recalibration: Sample Period 2008–2012

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the average rating across all issues of local governments of each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the average rating across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2012. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample period is from April 2008 to March 2012. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P	
	(1)	(2)	(3)	(4)
Panel A: County Level				
Recalibrated × Post	0.702***	0.715***	0.062	0.077
	(0.125)	(0.230)	(0.225)	(0.229)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.42	0.42	0.25	0.30
Number of Observations	2,924	2,924	1,377	1,377
Number of Counties	1,166	1,166	562	562
Panel B: Local Government Level				
Recalibrated Dummy × Post	0.628***	0.673***	-0.276	-0.249
	(0.078)	(0.089)	(0.237)	(0.337)
Local Gov. Type-Year Fixed Effects	No	Yes	No	Yes
County-Year Fixed Effects	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.33	0.58	0.01	0.33
Number of Observations	7,081	7,081	3,083	3,083
Number of Local Governments	3,781	3,781	1,508	1,508

Table IA.3
Difference-in-Differences of Issue Amount and Offer Yield around the Recalibration: Bond Issue Level

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of municipal new bond issues in the April 2006–March 2013 period. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Recalibrated Dummy × Post	0.107**	0.177***	-0.151***	-0.205**
	(0.043)	(0.068)	(0.045)	(0.086)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.01	0.10	0.38	0.47
Number of Observations	219,202	219,202	219,202	219,202

Table IA.4
Difference-in-Differences of Issue Amount and Offer Yield around the Recalibration: Robustness

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents bond-issue-level results. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of a given local government in each event year. In columns (1) and (2), the sample is restricted to two years before and after the recalibration (2008–2012). In columns (3) and (4), the sample is restricted to bond issues that have both Moody's and S&P ratings. In columns (5) and (6), the sample excludes bonds issued under the "Build for America" government program. In columns (7) and (8), the sample consists of all local governments (instead of just those that issue bonds in the four years before the recalibration). In columns (9) and (10), the sample consists of insured and uninsured bond issues. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of municipal new bond issues in the April 2006–March 2013 period. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sample Period 2008-2012		Sample with S&P Ratings		Sample Excluding BAB		Sample of All Issuers		Sample of All Bonds	
	Issue Amount (log)	Offer Yield	Issue Amount (log)	Offer Yield	Issue Amount (log)	Offer Yield	Issue Amount (log)	Offer Yield	Issue Amount (log)	Offer Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Bond Issue Level										
Recalibrated Dummy × Post	0.173**	-0.296**	0.289***	-0.233**	0.194**	-0.229***	0.185***	-0.221**	0.107*	-0.126**
	(0.082)	(0.117)	(0.079)	(0.099)	(0.068)	(0.085)	(0.068)	(0.088)	(0.062)	(0.050)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.08	0.39	0.08	0.43	0.10	0.44	0.09	0.46	0.08	0.54
Number of Observations	146,248	145,141	119,257	118,474	212,650	210,960	255,276	253,162	378,864	378,864
Panel B: Local Government Level										
Recalibrated Dummy × Post	0.135**	-0.191**	0.216***	-0.186***	0.195***	-0.187*	0.198***	-0.136*	0.091*	-0.214***
	(0.057)	(0.078)	(0.052)	(0.071)	(0.046)	(0.063)	(0.045)	(0.082)	(0.048)	(0.036)
Local Gov. Type-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.32	0.48	0.36	0.51	0.36	0.54	0.35	0.58	0.27	0.60
Number of Observations	12,198	12,198	12,940	12,940	18,123	18,123	23,564	23,564	32,465	32,465

Table IA.5
Difference-in-Differences of Issue Amount and Offer Yield around the Recalibration: Upgrade Notches

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents bond-issue-level results. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of a given local government in each event year. *Recalibrated 1 Notch*, *Recalibrated 2 Notches*, and *Recalibrated 3 Notches* take a value of one if a local government experienced a maximum upgrade of one notch, two notches, and three notches respectively in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of municipal new bond issues in the April 2006–March 2013 period. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Panel A: Bond Issue Level				
Recalibrated 1 Notch × Post	0.106**	0.148**	-0.178***	-0.224***
	(0.042)	(0.065)	(0.042)	(0.078)
Recalibrated 2 Notches × Post	0.176***	0.131*	-0.270***	-0.168*
	(0.052)	(0.079)	(0.054)	(0.101)
Recalibrated 3 Notches × Post	0.350	0.321	-0.095	-0.068
	(0.233)	(0.330)	(0.103)	(0.147)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.01	0.10	0.38	0.46
Number of Observations	219,202	219,202	219,202	219,202
Panel B: Local Government Level				
Recalibrated 1 Notch × Post	0.124***	0.162***	-0.227***	-0.175***
	(0.047)	(0.050)	(0.063)	(0.064)
Recalibrated 2 Notches × Post	0.205***	0.250***	-0.227***	-0.064
	(0.055)	(0.059)	(0.073)	(0.075)
Recalibrated 3 Notches × Post	0.088	0.286	-0.512***	-0.250
	(0.142)	(0.181)	(0.158)	(0.164)
Local Gov. Type-Year Fixed Effects	No	Yes	Yes	No
County-Year Fixed Effects	Yes	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.05	0.30	0.31	0.53
Number of Observations	18,215	18,215	18,215	18,215
Number of Local Governments	8,114	8,114	8,114	8,114

Table IA.6
Difference-in-Differences of Issue Amount and Offer Yield of New Bond Issues around the Recalibration: Weighted by Amount of Bonds Issued

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. The dependent variables are the logarithm of the amount of bonds issued and the average offer yield across all issues of local governments in each county and event year. *Recalibrated* is the fraction of upgraded local government units in each county (weighted by the amount of bonds issued) during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample period is from April 2006 to March 2013. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Recalibrated × Post	0.330***	0.296***	-0.222*	-0.339**
	(0.081)	(0.085)	(0.121)	(0.127)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.22	0.24	0.29	0.31
Number of Observations	6,364	6,364	6,364	6,364
Number of Counties	1,784	1,784	1,784	1,784

Table IA.7

Difference-in-Differences of Local Government Expenditures around the Recalibration: Effect of Regulatory Constraints

This table presents difference-in-differences estimates of regressions of the logarithm of expenditures (as of July of each year) of each local government unit and year around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. The sample in columns (1)–(4) consists of local governments with at least one bond issued in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of local government within urban counties. The sample period is from April 2006 to March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Rating Moody's < Rating S&P		Rating Moody's >= Rating S&P		Rating Moody's < Rating S&P		Rating Moody's >= Rating S&P	
	Sample of Bond Issuers				*Sample of Urban Counties*			
Recalibrated Dummy × Post	0.080*	0.132**	0.059**	0.078***	0.079*	0.132**	0.059**	0.078***
	(0.045)	(0.059)	(0.027)	(0.025)	(0.045)	(0.059)	(0.027)	(0.025)
Local Gov. Type-Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.32	0.40	0.49	0.44	0.31	0.37	0.50	0.44
Number of Observations	1,260	613	936	467	1,216	585	928	467
Number of Local Governments	315	155	234	118	304	147	232	118

Table IA.8

Difference-in-Differences of Local Government Current Expenditures and Capital Outlays around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government current expenditures and capital outlays (as of July of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in columns (1)–(4) consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (5)–(8) consists of urban counties. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013		Panel 2009-2012		Panel 2007-2013		Panel 2009-2012	
Panel A: Current Expenditures	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.102***	0.072***	0.042*	0.024	0.069***	0.060**	0.027	0.014
	(0.029)	(0.028)	(0.023)	(0.022)	(0.025)	(0.026)	(0.020)	(0.020)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.54	0.55	0.19	0.20	0.61	0.61	0.21	0.23
Number of Observations	12,166	12,166	6,952	6,952	6,349	6,342	3,628	3,624
Number of Counties	1,738	1,738	1,738	1,738	907	906	907	906
Panel B: Capital Outlays	*Sample of Bond Issuers*				*Sample of Urban Counties*			
Recalibrated × Post	0.039	0.047	0.323*	0.188	0.441***	0.318*	0.495***	0.365*
	(0.161)	(0.166)	(0.170)	(0.178)	(0.151)	(0.171)	(0.167)	(0.187)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.10	0.08	0.10	0.14	0.16	0.14	0.16
Number of Observations	12,155	12,155	6,944	6,944	6,341	6,334	3,623	3,619
Number of Counties	1,738	1,738	1,738	1,738	907	906	907	906

Table IA.9
Difference-in-Differences of Local Government Taxes around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government taxes (as of July of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2007-2013		Panel 2009-2012	
Panel A: County Level - Sample of Bond Issuers				
Recalibrated × Post	-0.082***	-0.048*	-0.052**	-0.045*
	(0.025)	(0.025)	(0.022)	(0.024)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.59	0.59	0.44	0.45
Number of Observations	12,090	12,090	6,912	6,912
Number of Counties	1,730	1,730	1,730	1,730
Panel B: County Level - Sample of Urban Counties				
Recalibrated × Post	-0.011	-0.021	-0.014	-0.011
	(0.022)	(0.024)	(0.020)	(0.024)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.47	0.48	0.37	0.37
Number of Observations	6,307	6,300	3,604	3,600
Number of Counties	901	900	901	900

Table IA.10

Difference-in-Differences of Economic Outcomes around the Recalibration: Group-Specific Trends

This table presents difference-in-differences estimates of regressions of the logarithm of local government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. The specification includes group-specific trends for the counties in the treatment and control groups. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B consists of urban counties. The sample period is from 2007 to 2013 (2006 to 2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel A: County Level - Sample of Bond Issuers								
Recalibrated × Post	0.033*	0.033*	0.075***	0.086***	0.024***	0.021**	0.041***	0.028***
	(0.019)	(0.019)	(0.016)	(0.016)	(0.008)	(0.009)	(0.009)	(0.008)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.29	0.30	0.03	0.04	0.20	0.21	0.57	0.57
Number of Observations	12,180	12,180	8,067	8,067	12,345	12,345	10,626	10,626
Number of Counties	1,740	1,740	1,154	1,154	1,767	1,767	1,771	1,771
Panel B: County Level - Sample of Urban Counties								
Recalibrated × Post	0.034*	0.033*	0.069***	0.064***	0.022***	0.022***	0.028***	0.023***
	(0.018)	(0.019)	(0.016)	(0.017)	(0.008)	(0.009)	(0.009)	(0.009)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.34	0.36	0.06	0.07	0.27	0.31	0.67	0.68
Number of Observations	6,363	6,356	5,314	5,307	6,522	6,515	5,592	5,586
Number of Counties	909	908	760	759	932	931	932	931

Table IA.11
Difference-in-Differences of Economic Outcomes around the Recalibration: Robustness

This table presents difference-in-differences estimates of regressions of the logarithm of local government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of counties in which at least two local governments issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in Panel B excludes counties below the 20th percentile of the amount of bonds issued. The control group in Panel C is restricted to counties without issuers rated by Moody's. The sample period is from 2009 to 2012 (2008 to 2011 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel A: Sample of Counties with Multiple Issuers								
Recalibrated × Post	0.027	0.043*	0.085***	0.097***	0.038**	0.028	0.075***	0.038*
	(0.027)	(0.023)	(0.031)	(0.030)	(0.019)	(0.018)	(0.021)	(0.021)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.14	0.02	0.14	0.16	0.12	0.13	0.85	0.86
Number of Observations	3,646	3,670	2,963	2,963	3,661	3,661	3,664	3,664
Number of Counties	973	979	779	779	978	978	979	979
Panel B: Sample Excluding Counties with Low Amount of Bonds Issued								
Recalibrated × Post	0.043*	0.038*	0.084***	0.085***	0.033**	0.023	0.066***	0.036**
	(0.024)	(0.019)	(0.028)	(0.027)	(0.015)	(0.014)	(0.016)	(0.015)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.13	0.02	0.15	0.17	0.11	0.12	0.83	0.84
Number of Observations	4,866	4,708	3,389	3,313	4,925	4,759	4,932	4,766
Number of Counties	1,406	1,325	929	891	1,427	1,344	1,430	1,347

Table IA.11 (continued)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel C: Control Group Restricted to Counties without Issuers Rated by Moody's								
Recalibrated × Post	0.054*	0.041*	0.074**	0.088***	0.035**	0.029*	0.082***	0.048***
	(0.031)	(0.022)	(0.032)	(0.032)	(0.017)	(0.016)	(0.019)	(0.018)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.12	0.02	0.14	0.15	0.13	0.14	0.79	0.79
Number of Observations	5,528	5,540	3,855	3,855	5,568	5,568	5,584	5,584
Number of Counties	1,382	1,385	965	965	1,393	1,393	1,396	1,396

Table IA.12

Difference-in-Differences of Economic Outcomes around the Recalibration: Sample of All Counties

This table presents difference-in-differences estimates of regressions of the logarithm of local government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. Controls include house price index and number of households. The sample consists of all counties in the 2009–2012 period (2008–2011 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Recalibrated × Post	0.080***	0.049*	0.069***	0.072***	0.035***	0.031**	0.065***	0.038***
	(0.025)	(0.026)	(0.022)	(0.022)	(0.012)	(0.013)	(0.013)	(0.013)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.11	0.12	0.09	0.10	0.08	0.08	0.65	0.65
Number of Observations	11,844	11,844	6,435	6,435	12,365	12,365	12,449	12,449
Number of Counties	2,961	2,961	1,612	1,612	3,110	3110	3,113	3,113

Table IA.13
Instrumental Variable Estimates of the Elasticity of Employment and Income: Sample of All Counties

This table presents instrumental variables (two-stage least squares) estimates of regressions of the logarithm of government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. Local government expenditures are instrumented with the *Recalibrated* × *Post* interaction variable. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. Controls include house price index and number of households. The sample consists of all counties in the 2007–2013 period (2006–2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Government Employment		Private Employment		Income	
Local Gov. Expenditures	1.292*	1.912*	0.470**	0.602*	0.643**	0.468*
	(0.680)	(1.152)	(0.222)	(0.335)	(0.255)	(0.258)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic of instrument (first stage)	5.75	3.39	10.13	5.88	9.71	5.48
Number of Observations	6,371	6,371	11,751	11,751	11,843	11,843
Number of Counties	1,596	1,596	2,955	2,955	2,961	2,961

Table IA.14
Effect of Economic Slack: Sample of All Counties

This table presents difference-in-differences estimates of regressions of the logarithm of government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. *High Slack* is a dummy variable that takes a value of one when the county-level unemployment rate in 2010 is above the median. Controls include house price index and number of households. The sample consists of all counties in the 2007–2013 period (2006–2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Recalibrated × Post	-0.035	0.010	0.008	0.048	-0.005	0.006	0.001	0.003
	(0.026)	(0.027)	(0.030)	(0.030)	(0.014)	(0.014)	(0.016)	(0.016)
Recalibrated × Post × High Slack	0.165***	0.156***	0.105*	0.110**	0.084***	0.066**	0.103***	0.081***
	(0.054)	(0.054)	(0.057)	(0.055)	(0.029)	(0.030)	(0.027)	(0.026)
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Size Group-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.24	0.02	0.03	0.15	0.16	0.51	0.53
Number of Observations	20,734	20,734	11,270	11,266	21,640	21,640	18,808	18,808
Number of Counties	2,962	2,962	1,615	1,613	3,114	3,114	3,136	3,136

Table IA.15
Difference-in-Differences of Local Government Expenditures around the Recalibration: Effect of Rating Level

This table presents difference-in-differences estimates of regressions of the logarithm of expenditures (as of July of each year) of each local government unit and year around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. *Low Rating* is a dummy variable that takes a value of one when the local government Moody's rating is below Aa1. Controls include house price index and number of households. The sample in columns (1)–(2) consists of local governments with at least one bond issued in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (3)–(4) consists of local government within urban counties. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Sample of Bond Issuers		*Sample of Urban Counties*	
Recalibrated Dummy × Post	0.020**	0.028**	0.023**	0.030**
	(0.010)	(0.012)	(0.010)	(0.013)
Recalibrated Dummy × Post × Low Rating	0.018	0.026	0.015	0.042
	(0.021)	(0.034)	(0.024)	(0.040)
Local Gov. Type-Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.24	0.34	0.20	0.31
Number of Observations	14,286	9,725	11,630	8,086
Number of Local Governments	3,572	2,466	2,908	2,044

Table IA.16
Difference-in-Differences of Local Government Expenditures around the Recalibration: Effect of Local Government Type

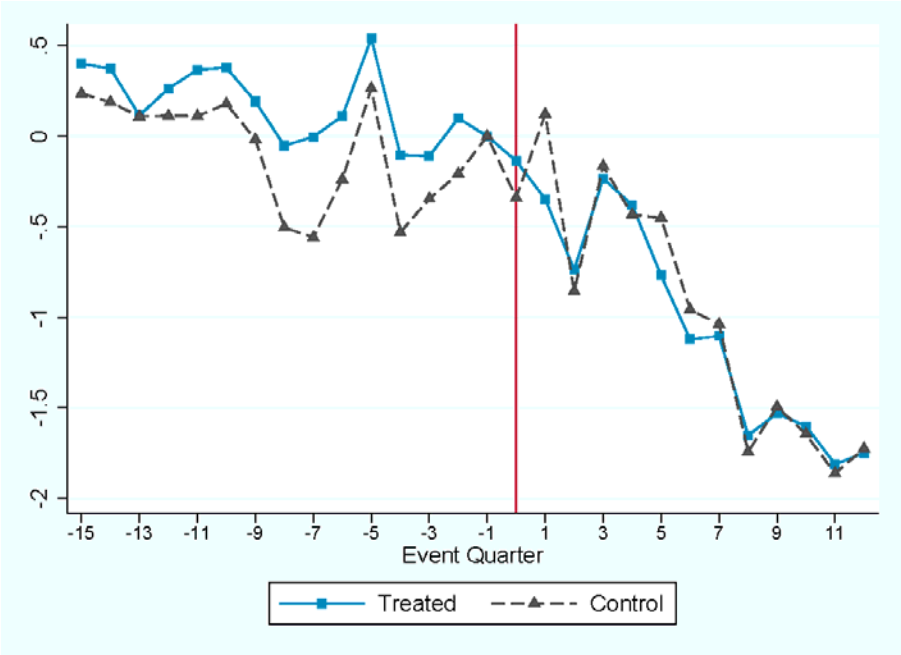
This table presents difference-in-differences estimates of regressions of the logarithm of expenditures (as of July of each year) of each local government unit and year around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one in 2011 and for each year thereafter. *District* is a dummy variable that takes a value of one when the local government is a school district or special district. The sample in columns (1)–(2) consists of local governments with at least one bond issued in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010). The sample in columns (3)–(4) consists of local government within urban counties. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Sample of Bond Issuers		Sample of Urban Counties	
Recalibrated Dummy × Post	0.013	0.015	0.004	0.016
	(0.013)	(0.017)	(0.013)	(0.016)
Recalibrated Dummy × Post × District	0.013	0.018	0.029	0.023
	(0.017)	(0.023)	(0.018)	(0.024)
Local Gov. Type-Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.24	0.34	0.20	0.31
Number of Observations	14,286	9,725	11,630	8,086
Number of Local Governments	3,572	2,466	2,908	2,044

Figure IA.1
Offer Yield around the Recalibration

This figure shows the regression coefficients of offer yield on event-quarter dummies around the Moody's recalibration event in April-May 2010. Panel A present the changes in offer yields separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Offer Yields (relative to quarter -1)



Panel B: Difference between Treatment and Control Groups

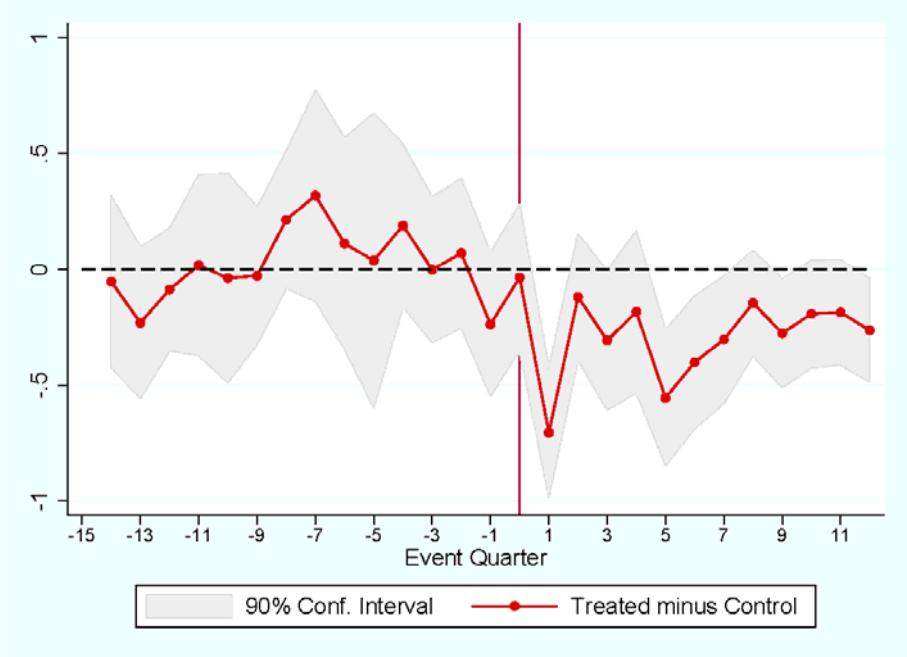
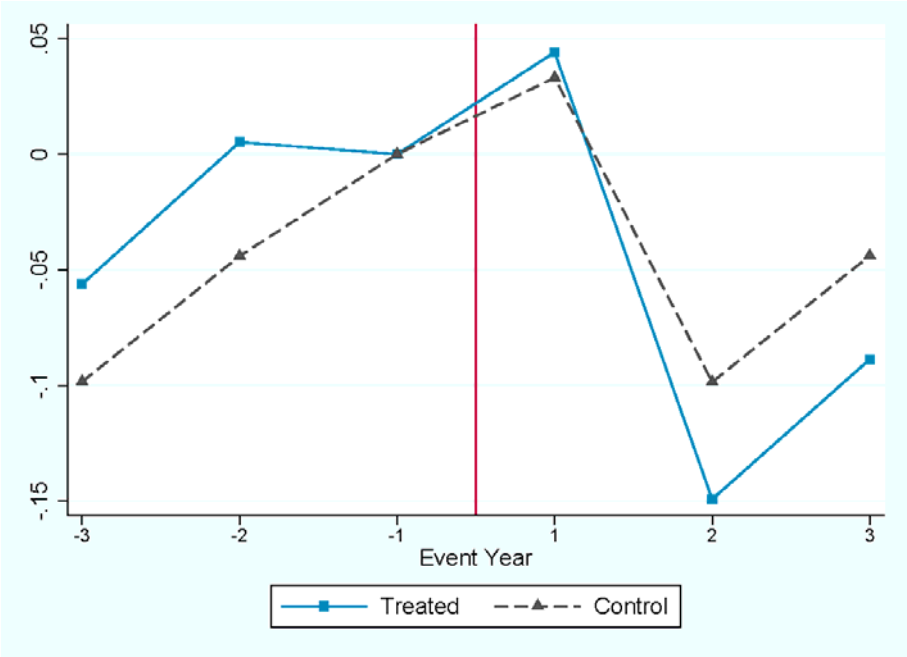


Figure IA.2
Local Government Taxes around the Recalibration

This figure shows the regression coefficients of logarithm of local government taxes (as of July of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A presents the changes in logarithm of local government expenditure separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the differences between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Local Government Taxes (relative to year -1)



Panel B: Difference between Treatment and Control Groups

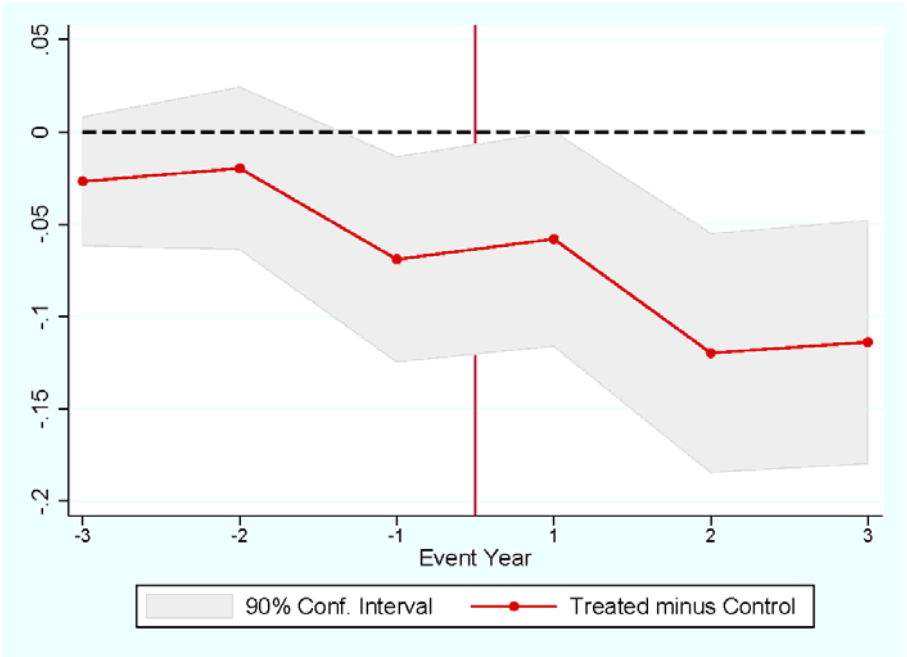
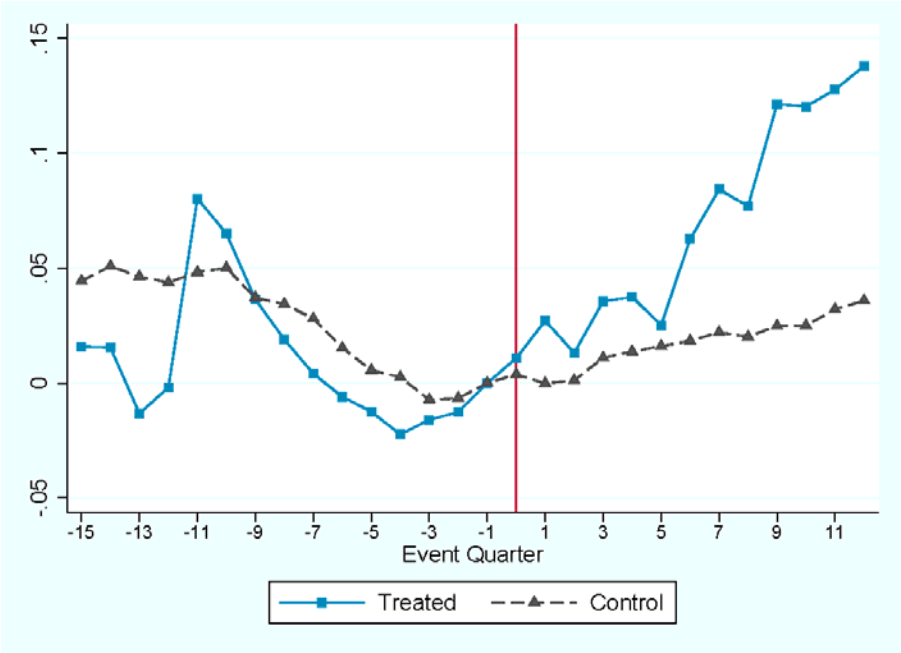


Figure IA.3
Non-Tradable Employment around the Recalibration: Quarterly Frequency

This figure shows the regression coefficients of logarithm of non-tradable (NAICS 44–45 and 72) private employment (at the end of each quarter) on event-quarter dummies around the Moody's recalibration event in April-May 2010. Panel A presents the change in logarithm of non-tradable private employment separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in Non-Tradable Private Employment (relative to quarter -1)



Panel B: Difference between Treatment and Control Groups

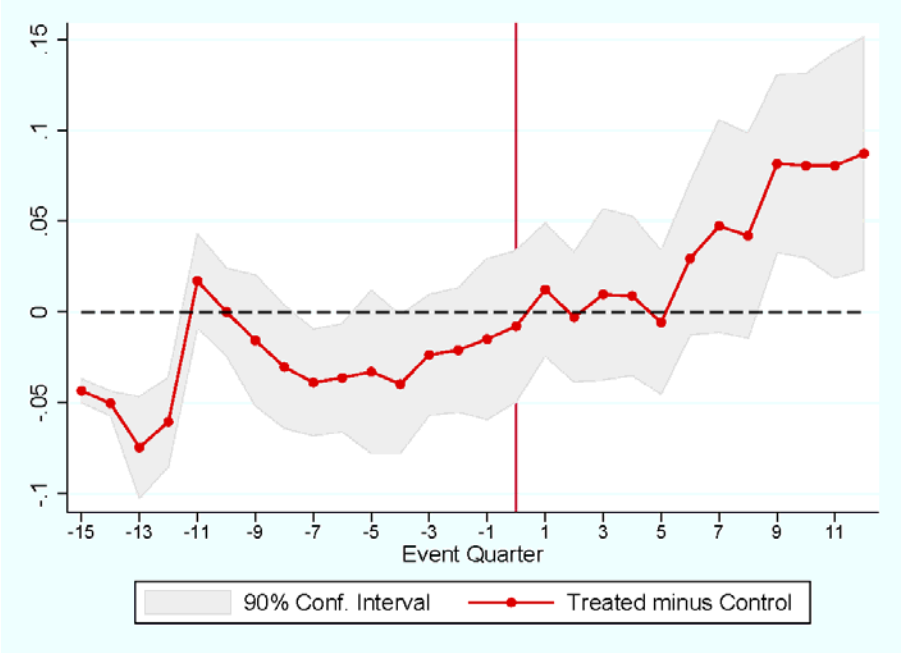
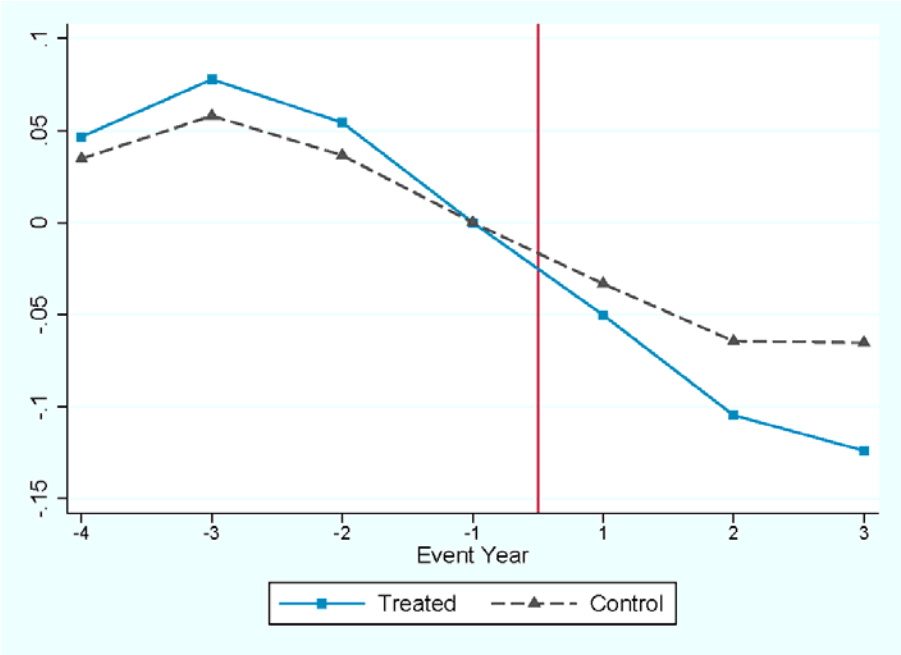


Figure IA.4
House Price Index around the Recalibration

This figure shows the regression coefficients of logarithm of the house price index (as of December of each year) on event-year dummies around the Moody's recalibration event in April-May 2010. Panel A present the changes in logarithm of the house price index separately for upgraded local governments (treated) and non-upgraded local governments (control). Panel B presents the difference between the treatment and control groups and a 90% confidence interval. The sample consists of counties in which at least one local government issued bonds in the municipal bond market during the four-year period before the recalibration (April 2006–March 2010).

Panel A: Change in House Price Index (relative to year -1)



Panel B: Difference between Treatment and Control Groups