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

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Abstract

We show that municipalities' credit constraints can have important effects on local economies through a ratings channel. We identify these effects by exploiting exogenous variation on U.S. municipal bond ratings due to Moody's recalibration of its ratings scale in 2010. We find that local governments increase expenditures and employment due to an expansion of their debt capacity following a rating upgrade. These increases in local government spending have a local income multiplier of 2.4 and a cost per job of $21,000. Our findings suggest that debt-financed increases in local government spending can improve economic conditions during recessions.

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Municipal bonds markets are an important source for state and local governments to finance the construction and maintenance of infrastructure and other public projects. Bonds provide cash flow for government needs and services (e.g., education), as well as finance private projects (through the use of "conduit" financing). According to the U.S. Securities and Exchange Commission (SEC, 2012), investors held over one million different municipal bond issues, representing a total outstanding (principal) amount of more than $3.7 trillion as of December 2011, corresponding to 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 the local economy, particularly during recessions. Easier access to financing can have important effects on local economic outcomes when governments face significant financial stress such as during the 2007-2009 Great Recession.[1] Specifically, local governments can use bond financing to alleviate spending cuts (maintain employees and avoid program cuts), to prevent tax and fee increases, or to contribute to their end-of-year balances (which include rainy day funds). These could, in turn, have positive spillover effects to the private sector arising from higher disposable income due to either the wages of the direct hires or lower net taxes. On the other hand, the increase in local government spending could crowd out private consumption and investment.

We identify the real effects of public financing by exploiting exogenous variation in U.S. municipal bonds ratings that is due to Moody's recalibration of its municipal bond rating scale in 2010. Before the ratings 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, on the other hand, measures expected losses (i.e., default probability and loss given default) among sovereign and corporate

[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 (difference between total revenues and total expenditures).

bonds. This dual-class rating system persisted for decades until Moody's recalibrated its municipal ratings to align them with the Global Rating Scale in April-May of 2010, resulting in upgrades by up to three notches of nearly 18,000 local governments (issuers), corresponding to bonds worth more than $2.2 trillion in par value.

This recalibration experiment allows us isolate changes in economic outcomes that are due exclusively to changes in public financing from those that would occur in the absence of these changes. An important aspect of the recalibration is that not all municipal bonds were affected by the recalibration. Local governments that did not have a rating from Moody's or did not have bonds outstanding were not subject to recalibration, by definition. In addition, local governments that were properly calibrated vis-à-vis other securities could also be used as a control group.[2]

Importantly for our study, the upgrades resulting from the recalibration did not reflect changes to the intrinsic quality of the issuers, but rather the goal to align municipal ratings standards with those of sovereign and corporate ratings. In fact, the recalibration algorithm used the expected losses of each municipal rating by sector (i.e., historical default rates by rating category and loss severity by sector), and thus changes in ratings due to recalibration are uncorrelated with changes to specific local government (and nationwide) fundamentals.

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 event. We study how this shock to municipal ratings affects economic outcomes at the county level. Since our event affects bonds issued by counties, as well as by local government units within a county (i.e., other jurisdictions) such as cities, townships, school districts, and public utility districts, we aggregate the changes in ratings to the county level.[3] Our treatment (continuous) variable is the local government units in each county

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[2] Housing, healthcare, and other sectors did not see a change in ratings because they were already well-calibrated with the global scale. Bonds with higher ratings (at or above Aa3) on the municipal scale were also less likely to be recalibrated than those with a lower rating (e.g., bonds with Aaa rating on the municipal scale could not, by definition, be upgraded).

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

whose outstanding bonds were upgraded as a result of the Moody's recalibration. The specifications also include time-varying county-level control variables, as well as state-by-year fixed effects to capture state economic conditions and any source of unobserved state-level heterogeneity that affects counties in a given year, such as changes in transfers from state governments and ballooning of the unfunded pension liabilities of states.

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 2007-2013 period. We begin by mapping the ratings into 22 numerical values, where 22 is the highest rating (Aaa) and one the lowest (default). We find that Moody's ratings increase 0.6 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 with a Moody's rating also have a S&P rating). 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 significantly different changes in the S&P ratings between treatment and the control groups.

We also find that upgraded local governments increase the amount of new bond issues significantly more than non-upgraded local governments following the recalibration. The differential effect on the (dollar) amount of bonds issued (at the local government level) is 19% to 23% per year in the three-year period after the recalibration (April 2010-March 2013) relative to the three-year period before the event (April 2007-March 2010). The offer yield of the new bond issues of the upgraded local governments decreases by 20 to 30 basis points relative to non-upgraded local governments. These findings are consistent with credit ratings playing an important informational role in the municipal bond market, likely due to the significantly higher presence of retail investors relative to other fixed income markets.[4] The effects of changes in

[4] 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%

municipal bond ratings are thus likely to reflect retail investors' reliance on ratings as a source of information. The effects can also be the result of ratings-based regulations and internal policies on institutional investors.[5] 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 changes in credit ratings on municipal bond prices. They find that that upgraded bonds earn abnormal returns in the secondary market, and that upgraded municipalities subsequently benefit from a reduction in offer yields relative to non-upgraded municipalities in the primary market.

Consistent with local governments using the increase in bond financing to improve economic conditions, we find significant effects on local economic outcomes after the ratings recalibration. We find that upgraded local governments' expenditures and employment increase 1%-2% more than non-upgraded local governments following the recalibration, which is both economically and statistically significant. We do not observe a significant relative decrease in local government taxes, which indicates that our multiplier effects appear to be driven by increases in government spending. 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 significant de facto flexibility for local governments to run deficits (at least for limited periods of time).

We find evidence of important 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%-0.5%. We also examine whether the effects on private employment are heterogeneous across sectors. We expect the

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by 2013, but households still have an important share of the municipal bond market. In contrast, households held only 13% of corporate and foreign bonds as of 2010.

[5] 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; Financial Stability Board 2010, 2012).

effects to be more pronounced in the non-tradable sector [retail, food, and accommodation, as in Mian and Sufi (2014)], which depend primarily on local demand, as well as in the healthcare and education sectors, which typically receive transfers or grants from state and local governments. We find that a one-standard deviation increase in the fraction of local governments upgraded in a county increases non-tradable employment by 0.6%-0.8%. The effect in the construction and tradable sectors is statistically insignificant.

We also examine the role of firm entry in job creation in the non-tradable sector. We find that employment in startups (i.e., firms less than two years old) in the non-tradable sector increases 0.8%-1.5% in response to a one-standard deviation shock in the fraction of upgraded local governments in a county. This finding is consistent with the notion that startups play a key role in net job creation (Haltiwanger, Jarmin, and Miranda 2013), in particular in response to local demand shocks (Adelino, Ma, and Robinson 2015).

Finally, 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 in a county increases income by 0.4%-0.8%.

A concern about inferences from the difference-in-differences approach is whether the processes generating the treatment and control group outcomes would have followed parallel trends in the absence of the treatment. We show that bond ratings, local government bond financing, expenditures and employment, as well as private employment and income, follow similar trends across upgraded and non-upgraded local governments in the years before the recalibration. Thus, we identify an economic effect exactly at the time of the recalibration, indicating that local governments have used the positive shock to credit supply to create (or save) jobs in the public and private sector.

Our study contributes to the literature on the use of cross-sectional variation in the estimation of fiscal multipliers (e.g., Cohen, Coval, and Malloy 2011; Chodorow-Reich et al. 2012; Nakamura and Steinsson 2014; 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 ("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 spending, we estimate that a marginal million dollars in local government spending results in an additional 48 jobs, 36 of which are outside of the public sector. This estimate corresponds to a cost per job created of \$21,000. Our estimates also imply an income multiplier of 2.4 (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 2.4 × \$21,000 = \$50,000.

Our estimates of fiscal multipliers are at the upper end of the range 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, thus fiscal multipliers should be larger. Indeed, we find that our effects are concentrated in counties with higher unemployment. This is consistent with empirical work on state-dependent multipliers that finds higher multipliers during depressed economic conditions (e.g., Fishback and Kachanovskaya 2010; Auerbach and Gorodnichenko 2012).[6]

[6] The 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%, unemployment rates was about 9.9% (both drawn from the Bureau of Economic Analysis), and the federal funds rate was 0.12%.

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 (2015) 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.[7] While these authors study the local economic effects of shocks to credit supply to the *private* sector, we study 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.

Finally, we also 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; Almeida et al. 2014).

1. Institutional Background and Data

1.1 Recalibration Event

Moody's had a dual-class rating system up until the ratings recalibration in 2010. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality is to reach a weakened financial position that requires extraordinary support from a higher level of government to avoid default). On the other hand, Moody's Global Rating Scale is designed to measure expected losses (default probability and loss given default) among sovereign bonds, corporate bonds, and structured finance products (Moody's 2007). Moody's (2009) attributes its

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

dual-class rating system to the preferences of the highly risk-averse investors in municipal bonds. Using the Flow of Funds Accounts in 2010, households owned 50% of the 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 resulted in lower municipal bond ratings relative to its competitors. In our sample, Moody's assigned a rating lower than S&P's in 53% of the issues (and the same rating in 40% of the issues) in the year prior to the recalibration, while 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. As a consequence of the more conservative ratings under the dual-class system, Moody's share in the municipal bond market declined, as did their dominant role in the marketplace. In the year prior to the recalibration, Moody's had a market share of 34%, S&P 59%, and Fitch 7%. After the recalibration, Moody's market share increased to more than 40% (2010-2012).[8]

Moody's maintained a sizable market share despite this apparent competitive disadvantage under the dual-class system largely because many regulations [e.g., Basel II and National Association of Insurance Commissioners (NAIC) guidelines] and investment rules require at least two ratings from a nationally recognized statistical rating organization (NRSRO), and employ the lower of two ratings, or the middle of three ratings, to serve as the basis for regulatory benchmarks (e.g., banks' capital requirement).[9] Beyond regulations, local governments' debt management policies and institutional investors' policies also 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

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

[9] 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.

recognized credit rating services, unless the transaction is of a small size."[10] 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 to 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 comment from market participants about the methodology and a potential shift from the municipal scale to the global scale in June of 2006 (Moody's 2006). Moody's planned to implement the ratings recalibration in June and July of 2008, but the financial market's turmoil during the summer and fall of 2008 led to a postponement. Finally, in March of 2010, Moody's announced a recalibration of its Municipal Rating Scale to align it with the Global Rating Scale (Moody's 2010). In April and May of 2010, over a four-week period, Moody's announced how the municipal bonds rated by Moody's would be affected by the recalibration, resulting in a zero-to-three notch upgrade of nearly 70,000 bond ratings.

Moody's recalibration algorithm used the expected losses of each municipal rating by sector (i.e., historical default rates by rating category and loss severity by sector) to map to its equivalent rating on the global scale. An important aspect of this recalibration is that not all municipal bond issues were upgraded as a result of the recalibration and therefore can be used as control group. Some local governments were already properly calibrated vis-à-vis the global scale, in particular housing, healthcare, and other sectors 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

[10] 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."

be recalibrated than those with a lower rating (below Aa3); bonds with the maximum attainable rating (Aaa) in the municipal scale could not be upgraded, by definition. Of course, local governments without bonds rated by Moody's or without outstanding bonds 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 prior to recalibration would remain under review, and would not be lumped into these massive rating changes. Thus, our sample does not include any natural upgrades associated with improving issuer fundamentals that could contaminate our results. Additionally, the fact that the methodology closely follows a discussion that occurred (and was made public) over a period of several years makes it 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 billion). 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 2007 to March 2013, which corresponds to three-year period before the recalibration and the three-year period afterwards. We restrict the sample to new bond issues rated by Moody's and local governments that issue bonds during the three-year period before the recalibration.[11]

Since we measure local 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 public utility districts. We exclude state-level bonds as they cannot be attributed 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 the municipal bonds are uninsured). The results are robust when we include insured bonds 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), and includes more than 90% of the counties in the United States.

We obtain local government employment data from the Census Bureau's Government Employment and Payroll Survey. The Census Bureau conducts a complete census of local government employees every five years (e.g., 2002, 2007, 2012), and 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

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

of each year. The analysis of local government employment is restricted to local government units that are present in all years of our sample (2007-2013), that is, the sample includes only counties that have at least one government unit that is present in all years.[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 obtain county-level income data from the Internal Revenue Service (IRS) Statistics of Income. Income (adjusted gross income) is defined as total wages and salaries in a county in a given calendar year (the sample period for income is 2006-2012). When we analyze private sector employment or income, we use the full CBP or IRS data (i.e., we include all counties).[13]

Data on new firm employment comes 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. It provides total employment in the private sector for five firm age categories: startups (0-1 year), 2-3 years, 4-5 years, 6-10 years, and 11 years or older. The totals are provided by county, quarter, and industry (two-digit NAICS code level).

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 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.[14] We obtain county-level information on the number of households from the 2007 Census Bureau

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

Summary Files. The *Households* variable is defined as one or more people that occupy a given housing unit.

1.3 Summary Statistics

Panel A of Table 1 presents summary statistics of the issue amount and offer yield of the sample of new issues. The main explanatory variable is a dummy variable that takes a value of one if a local government (issuer) experienced an upgrade in any of its bonds due to Moody's recalibration event (*Recalibrated Dummy*). Bonds issued by upgraded local governments represent about 75% of the sample of new issues (54% were upgraded by up to one notch, 19% by two notches, and 2% by three notches). The average new issue in the sample (from April 2007 to March 2013) has a par amount of $4.5 million, but the distribution is highly skewed with a median of $0.9 million. The offer yield is 2.8% on average, with a median of 2.9%.

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 18.6, corresponding to a rating between Aa3 and A1. The median is 19 (Aa3). 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 19.1, corresponding to a rating (Aa3), which confirms that Moody's municipal bond ratings are more conservative than S&P.

Panel B of Table 1 presents summary statistics on county-level outcome variables from 2007 to 2013. Counties in the sample have an average of 4,600 government employees and a median of 700 government employees. The average local government in the sample has expenditures of $475 million. The distribution is also heavily skewed, with a median of $68 million dollars.

Private employment in each county is much larger than government employment at 37,000 employees on average. We separately track tradable and non-tradable employment using the Mian and Sufi (2014) classification based on four-digit NAICS codes. Average employment in the tradable sector is 3,400 employees, while average employment in the non-tradable sector is about 6,200 employees.[15]

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 (uninsured) bonds were upgraded by at least one notch during the Moody's recalibration event. We then calculate our treatment (continuous) variable as the fraction of local government units in a given county that were upgraded during 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 A of Table 2 provides the results of a comparison of recalibrated and non-recalibrated issues in the pre-recalibration period. Panel B of provides the results of a comparison of counties with recalibrated local governments (i.e., counties with non-zero *Recalibrated* variable − treated counties) and counties without recalibrated local governments (i.e., counties with *Recalibrated* variable equals zero − control counties). Since the median of *Recalibrated* is zero, recalibrated counties correspond to the counties with above-median *Recalibrated*, while non-recalibrated counties correspond to the counties with below-median *Recalibrated*.

One of the features of the data is that counties in the treatment group are larger than counties in the control group. The average number of households is 82,000 for the treatment group versus 12,000 for the control group. The average total private employment presents a similar pattern with 97,000 and 11,000 for treatment group and control group, respectively. This may reflect the fact that smaller counties either issue bonds less frequently and/or are less likely to have a rating from Moody's. Importantly, the treatment and control groups have similar economic structures in terms of private versus local government employment (as a fraction of private employment), and tradable versus non-tradable employment. Additionally, the growth rates of outcome variables in

[15] The retail- and restaurant-related industries are defined as "non-tradable" (NAICS codes 44-45 and 72), and industries that show up in the global trade data (mostly manufacturing) are defined as "tradable" (NAICS codes 31-33). The remaining industries are classified as "construction" or "other."

the pre-treatment period are also similar across the two groups. This indicates that, despite the difference in size, counties are comparable along the other dimensions we consider.[16]

The panel specifications also include county fixed effects and year fixed effects. As an alternative, we estimate cross-sectional regressions using growth rates as the dependent variable. This mitigates concerns that the results are simply picking up different trends or differences in how these counties were affected by the 2007-2009 financial crisis. Additionally, we also include state-by-year fixed effects to capture state economic conditions and any source of unobserved state-level heterogeneity that affects counties in a given year. As a robustness check, we show results with size-by-year fixed effects to assess whether the results are picking up a possible differential impact of the recession and recovery on counties of different sizes.

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 the 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 local economic outcomes) by comparing the bond rating, issue amount, and offer yield of upgraded local governments (the treatment group) and non-upgraded local governments (the control group) in a three-year period after the recalibration relative to a three-year period before. We first estimate issue-level difference-in-differences (reduced form) regressions using new issues data for the April 2007 to March 2013 period. We also estimate local government-level and county-level regressions for bond market outcomes since our main economic outcomes are measured either at the local government or county level.

The explanatory variables are as follows: (1) a dummy variable that takes a value of one if an

[16] In the robustness section, we address the concern that our effects are due to larger counties recovering faster after the recession than smaller counties.

local government experienced an upgrade in any of its outstanding (uninsured) bonds during the Moody's recalibration event (*Recalibrated Dummy*); (2) a dummy variable that takes a value of one for the April 2010 to March 2013 period (*Post*); and (3) the interaction term *Recalibrated Dummy* × *Post*. The analysis is conducted within-local governments (issuer), i.e., we include local government fixed effects in all regressions, which means that the direct effect on the *Recalibrated Dummy* is not identified. The regressions include year-event fixed effects. 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 (in the case of offer yield tests).[17] Standard errors are clustered at the local government level to correct for within-local government residual correlation.

Panel A of Table 3 presents the results of bond issue-level regressions of the effects on ratings. Column (1) presents the results in which the dependent variable in the regression is the Moody's rating. 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 0.6 notches on the Moody's ratings of the treatment group relative to the control group. Columns (2) and (3) show a similar differential effect on ratings when we include control variables and state-by-year fixed effects in the regression.

About half of our sample of new issues rated by Moody's is simultaneously rated by S&P. Thus, we can use the S&P credit ratings as a placebo test, as S&P does not have a dual-class rating system. We test whether the differential effect on Moody's ratings between the treatment and control groups can be due to factors other than the recalibration. If the Moody's recalibration does not reflect any change in the intrinsic credit quality of the issuers, no differential effects on S&P ratings should be found.

Columns (4)-(6) of Table 3 presents the results of the placebo test using S&P ratings for the

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

subsample of bond issues with both S&P and Moody's ratings. We find no significant differential effect on S&P ratings between the treatment and control groups following the recalibration. While the exclusion restriction is not directly testable, this finding is as an important validation of our identification strategy.

Columns (7)-(9) of Table 3 present the results of an examination of the effect on Moody's ratings using the sample of issues with both Moody's and S&P ratings (the same sample used to generate the results of the placebo test in columns (4)-(6)). We find that the interaction term *Recalibrated Dummy* × *Post* coefficient remains positive and significant in this sample.

Figure 2 shows the effect of the recalibration on Moody's ratings for the treatment and control groups from two years before the recalibration up to two years after. The results are from the regression in column (1) of Table 3, Panel A, replacing the interaction term *Recalibrated Dummy* × *Post* with dummies for whether a bond issue is in the treatment group t years after or t years before the recalibration. Treatment and control groups show no significant differential changes in the two years prior to 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 two years afterward. Figure 2 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 the differential effects are not related to channels other than ratings.

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. The local government-level results in Panel B are similar to the issue-level results in Panel A.

Panel A of Table 4 presents the results of bond issue-level regressions of the effects on the municipal bond primary market. Columns (1)-(3) present results in which the dependent variable in the regression is the logarithm of the *Issue Amount* (in millions of dollars). Columns (4)-(6) present results in which the dependent variable in the regression is the *Offer Yield* (in percentage). The treatment group shows a large and statistically significant increase in the amount of each issue after the recalibration. In column (1), the interaction term *Recalibrated Dummy* × *Post* coefficient is 0.113, significant at the 5% level, which indicates that local governments in the treatment group after the recalibration increase the issue amount 11% more than local governments in the control group. Columns (2) and (3) show a similar differential effect on issue amounts when we include control variables and county-year fixed effects.

We find that the offer yields of new issues of the treatment group experience a larger reduction after the recalibration than the offer yields of the control group. In column (1) of Table 4, the differential reduction in offer yields is about 12 basis points. Columns (2) and (3) show a similar effect on offer yields when we include control variables and county-year fixed effects. The magnitude of the differential effect on offer yields is similar to that in Cornaggia, Cornaggia, and Israelsen (2015).

Panel B in Table 4 presents the results of the regression of the logarithm of the *Issue Amount* (columns (1)-(3)) and *Offer Yield* (columns (4)-(6)) at the local-government level. The dependent variables are the logarithm of total amount issued and the average offer yield across all issues of a given local government in each event year. The treatment groups have a large and statistically significant increase in the (dollar) amount of the bonds they issue following the recalibration. The treatment group increases the total issue amount after the recalibration 19% to 23% more than the control group. We find that the average offer yields of the treatment group decreases significantly more than the offer yields of the control group following the recalibration. The estimated reduction in offer yields is 20 to 32 basis points. The local government-level results in Panel B are qualitatively similar to the issue-level results in Panel A.

Figure 3 shows the effect of the recalibration on the amount of bonds issued by the treatment and control groups from two years before the recalibration up to two years after. The figure shows that, in the two years prior to the recalibration, the issue amount of both groups is similar. 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.

We perform several robustness checks of the effects on the issue amount and offer yield in

Table 4. We re-estimate the 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 to 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; (5) a sample with both uninsured and insured bonds. These robustness checks are shown in Table IA.2 in the Internet Appendix. In particular, we find similar effects on the 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 as investors have access to S&P ratings on the same bonds. Thus, the regulatory channel seems to be playing an important 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).[18]

We also explore whether the magnitude of the effect on the amount of bonds issued and the offer yield is different according to the magnitude of the upgrade. Table IA.3 in the Internet Appendix shows that the effect is monotonically increasing with the magnitude of the upgrade, although the differences are not always statistically significant.

We estimate the effects of the recalibration on bond market outcomes aggregated to the county level in the Internet Appendix. We aggregate the new bond issues data using the sum of the amount of bonds issued by local governments (in millions of dollars) and the average of offer yields in each county and event year. The *Recalibration* variable is the fraction of upgraded local governments in a county. Table IA.4 in the Internet Appendix, Panel B presents the results of the regression of the logarithm of the *Issue Amount* (columns (1)-(3)) and *Offer Yield* (columns (4)-(6)) at the county level. The county-level results are qualitatively similar to the issue-level and local government-level results in Table 4.

[18] To further confirm this idea, we estimate the effect on offer yields using the sample of issues where the rating from Moody's is lower than that from S&P in which the regulatory channel is more likely to play a role because institutional investors have to rely on the lowest rating to calculate capital requirements. We find a negative and significant effect on offer yields in this sample (the coefficient is -0.336 with a t-statistic of 3.36). In contrast, the effect is insignificant in the sample of issues with Moody's ratings equal or higher than the S&P rating.

3. Effect on Local Government Expenditures and Employment

To estimate the impact of the ratings recalibration on local government outcomes, we first estimate difference-in-differences (reduced form) regressions at the local government level of expenditures and employment. In these tests, the explanatory variable of interest is the interaction of the *Recalibrated Dummy* with the *Post* dummy variable (which takes a value of one after the recalibration event in April-May 2010). The regressions include local governments fixed effects, as well as local government type-by-year fixed effects and, in some specifications, county-by-year fixed effects. This means that comparisons are made between groups of local governments within-type (county, city, township, school district, or special district) and within-county in each year. Standard errors are clustered at the local government level.

We also estimate regressions using county-level aggregates of local government expenditures and employment. Here, we estimate panel regressions using the logarithm of the outcome in each county and year as the dependent variable. The regressions consider two alternative sample periods: 2007-2013 and 2009-2012. The sample includes all counties regardless of whether they issue bonds or have bonds with a rating from Moody's. In these tests, the explanatory variable of interest is the interaction *Recalibrated* × *Post*. The *Recalibrated* variable is the fraction of upgraded local governments in a county. The *Post* variable takes a value of one in 2011, 2012, and 2013, as the fiscal year ends in June 31 for most local governments, and employment in the Census of Government is measured as of the week of March 12 of each year (so that 2010 still falls just before, or just one month after in the case of expenditures, the recalibration event). The regressions include county fixed effects and, in some specifications, county-level controls and state-by-year fixed effects. Standard errors are clustered at the county level.

We also present results of cross-sectional regressions using the growth rate of the outcome variables as the dependent variable in alternatives to the panel regressions. We define growth rates as the log change in the outcome variable in a given county from 2009 to 2011. In these tests, the explanatory variable of interest is the *Recalibrated* variable (as there are no pre and post periods in this cross-sectional specification using growth rates).

3.1 Local Government Expenditures

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% are in the education sector. We test whether the positive shock to the supply of municipal bond financing affected government expenditures and employment in the aftermath of the 2007-2009 recession.[19]

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 local government level and Panel B shows results at the county level. Columns (1)-(3) present the results using the 2007-2013 period and columns (4)-(6) using the 2009-2012 period. Columns (7) and (8) present the results of the cross-sectional regression using the 2009-2011 growth rate of expenditures as the dependent variable.

In column (1) in Table 5, the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at 2.4% when we include local government type-by-year fixed effects. This accounts for potential differences in the response to the financial crisis and the subsequent economic recovery by type of local government. The differential increase in local government expenditures is similar at 2.3% in column (2) when we include county-by-year fixed effects (which absorbs local economic shocks), and stronger at 3.4% when we include local government type-by-county-by-year fixed effects in column (3). 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 results in columns (4)-(6) are slightly weaker in magnitude at 1% to 1.8%

[19] Anecdotal evidence in the press supports the notion that the recalibration took place at time when local governments were facing severe financial constraints as a consequence of the 2007-2009 recession. Global Research (2010) 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."

and remain statistically significant. The cross-sectional regression results in columns (7) and (8) are also similar at about 1.5%.

Panel B in Table 5 presents results using county-level data. The results show that treatment group increases local government expenditures by 9% to 12% more than the control group after the ratings recalibration when we include state-year fixed effects.[20] The cross-sectional regressions produce similar results to the panel regressions. A one-standard deviation increase in the fraction of upgraded local governments in a county (a change of 0.08 in the *Recalibrated* variable, as shown in Table 1, Panel B) increases local government expenditures by about 0.8% (using the estimate in column (8), Panel B).

Tables IA.5 and IA.6 in the Internet Appendix show additional results for local government expenditures and revenues. Table IA.5 shows separate results for local government current expenditures and capital outlays. Current expenditures represent on average about 80% of local government total expenditures, while capital outlays represent about 20%. We find positive and significant effects in both components of total expenditures. There is a large difference between groups in capital outlays (at 22% to 61%), which is consistent with the idea that a significant fraction of the increase in bond financing due to the recalibration was invested in capital and infrastructure projects. Table IA.6 shows the evolution of local government taxes (mostly property taxes) between upgraded and non-upgraded counties. In most specifications, we find no significant effects in local government taxes due to the recalibration. This means that local governments mostly used the proceeds from municipal bond issues to alleviate spending cuts (or increase spending), rather than prevent tax and fee increases (or reduce taxes).

3.2 Local Government Employment

One of the likely uses of funds obtained through financing is to directly hire (or maintain) local government employees. Columns (1)-(6) of Table 6 present the results of difference-in-

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

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 local government-level regressions. We find increases of 1% to 2% in employment for the treatment group relative to control group. The most demanding specifications using county-by-type-by-year fixed effects produce even larger effects.

Panel B of Table 6 shows the results of county-level regressions. We find that the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at between 4% and 6% in most specifications. The point estimate results using state-by-year fixed effects generally produce larger magnitudes at 6% to 9%. The cross-sectional regressions produce similar results to the panel regressions. A one-standard deviation increase (using the standard deviation of 0.08 of the *Recalibrated* variable in Table 1, Panel B) in the fraction of upgraded local governments in a county increases local government employment at the county level by about 0.36% (using the estimate in column (8), Table 6, Panel B).

The Census of Governments is conducted every five years, and it was conducted in 2007 and 2012. In the intervening years, only a sample of local governments is collected. Thus, the sample of local government employment is restricted to counties that have at least one local government unit that is present in all years between 2007 and 2013. This raises concerns that the regressions in Table 6 may miss an important part of the variation. To address this concern, we estimate cross-sectional regressions using the logarithm growth rate in local government employment between 2007 and 2012, which avoids using intervening years and therefore includes all counties in the United States. The results in Table IA.7 of the Internet Appendix are consistent with those using the smaller sample of counties in Table 6.

Figure 4 shows the evolution of government employment before and after the ratings recalibration for the treatment and control group to account for the possibility of pre-trends. The two groups follow similar trends before the recalibration. Furthermore, we can see that government employment increases for the treatment group in the year of the recalibration, while it continues its negative trend for the control group.

4. Effect on Private Employment and Income

To estimate the impact of the ratings recalibration on local economic outcomes, we estimate county-level difference-in-differences (reduced form) regressions of private employment and income. We use county-level employment because smaller geographic units would not be appropriate. While an upgraded local government can hire (and thus we can measure its own employment creation), it is unlikely that the local private sector spillover effects would be limited to a small area such as ZIP Code. We use county as a compromise between even larger units (e.g., metropolitan statistical areas) and smaller ones (e.g., ZIP Codes). We estimate panel regressions using the logarithm of employment or income in each county and year as the dependent variables, as well as the corresponding 2009-2011 growth rates. The specifications are equivalent to those in Panel B of Tables 5 and 6 for government expenditures and employment.

4.1 Private Employment

We study the effects of Moody's recalibration on private employment. In the case of the employment variables, the *Post* variable takes a value of one in 2011, 2012, and 2013, as employment in the CBP data are measured as of the week of March 12 of each year (so that the 2010 observation falls immediately before the recalibration).

Columns (1)-(6) of Table 7 present the results of difference-in-differences regressions using the logarithm of private employment as dependent variable. Columns (7) and (8) present the results of the cross-sectional regression using the 2009-2011 growth rate of private employment as the dependent variable.

In column (1), the interaction term *Recalibrated* × *Post* coefficient is 4.8%, significant at the 1% level. The differential increase in private employment is higher in column (3) at 7.1% when we include state-by-year fixed effects in the regression, which controls for time-varying regional economic shocks. The corresponding results in columns (4)-(6) where a shorter event window is used are slightly smaller in magnitude at 2.2%-3.5%, but still statistically significant at the 5% level. The cross-sectional regressions produce similar results, as shown in columns (7) and (8).

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% to 0.5%. This is evidence of positive spillover effects to the private sector due to local governments having better access to credit markets.

Figure 5 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, but remains constant for the control group.

4.2 Non-Tradable and Tradable Private Employment

We also examine the effects of local governments' rating upgrades on non-tradable versus tradable sector employment. We expect that the impact of the expansion in government spending to show up foremost in non-tradable employment. The non-tradable sector in a county depends primarily on local demand, while demand for the tradable sector is more dispersed. We therefore separately track tradable and non-tradable employment using the same four-digit industry classification as in Mian and Sufi (2014).

Panel A of Table 8 presents the results for the non-tradable sector employment, while Panel B presents the results for tradable sector employment. In column (1) of Panel A, the interaction term *Recalibrated* × *Post* coefficient is 0.224, significant at the 1% level. The results in column (3) show that the increase in non-tradable employment is similar when we include state-by-year fixed effects in the regression. The corresponding results in columns (4)-(6) for the shorter event window are lower but remain economically and statistically significant. These results imply that a one-standard deviation increase in the fraction of upgraded local governments in a county increases non-tradable employment by 0.5%-0.8%. In Panel B, the results for tradable sector employment are negative but imprecisely estimated and statistically insignificant. The results are consistent with the notion that the expansion in local government spending mainly affects local demand and the non-tradable sector.

Government spending is more likely to occur in sectors such as construction, education, and healthcare [these sectors are not classified as tradable or non-tradable according to the definition in Mian and Sufi (2014) given that demand can be both local and more geographically dispersed]. Table 9 presents difference-in-differences results for employment in the construction (columns (1) and (2)), education (columns (3) and (4), and healthcare (columns (5) and (6) sectors.

The *Recalibrated* × *Post* coefficient is insignificant for the construction sector, indicating that the impact of the recalibration on private employment is not driven by exposure to construction-related sectors. The *Recalibrated* × *Post* coefficient is positive and significant in all specifications for the education and healthcare sectors. The effects on these two sectors are consistent with the notion that they receive transfers and grants from local governments.

Table IA.8 in the Internet Appendix shows that the increase in employment has a large effect on employment in new firms in the non-tradable sector (firms less than two years old), which is consistent the role of startups on the net creation of employment (Haltiwanger, Jarmin, and Miranda 2013; Adelino, Ma, and Robinson 2015). A one-standard deviation increase in the fraction of upgraded local governments in a county is associated with a 1.9% increase in employment in new firms. Figure IA.1 shows the evolution of employment in new firms in the two-year periods before and after the recalibration event for the treatment and control groups.

We conclude that the effects of the expansion on local public financing are not restricted to the public sector. We find evidence on important effects on private sector employment, especially in the case of the non-tradable, education, and healthcare sectors. In contrast, there is some evidence of a crowding out effect on employment in the tradable sector.

4.3 Income

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

Table 10 present the results of regressions that are equivalent to those in Tables 7-9 for private employment. In column (1), the interaction term *Recalibrated* × *Post* coefficient is 0.053, significant at the 1% level. The differential increase in income is similar in column (2) when we include county-level controls in the regression, and increases to 0.09 when we include state-by-year fixed effects in the regression. These results are statistically and economically important. A one-standard deviation increase in the fraction of upgraded local governments in a county increases income 0.4%-0.8%.

Figure 6 shows the evolution of income in the two-year periods before and after the recalibration event for the treatment and control groups. The income processes of the two groups follow similar trends before the recalibration. Furthermore, income increases significantly for the treatment group in the year of the recalibration, although the increase is much lower for the control group. In the two-year period following the recalibration, the income processes again follow similar dynamics.

5. Fiscal Multipliers

Our results support a positive and robust 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., increase in jobs from a marginal million dollars in government spending) and income (i.e., dollar change in income produced by a one dollar change in government spending). These multipliers are interpreted as the total 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 new government employees).

We use instrumental variables methods to estimate the fiscal multipliers. We instrument for local government spending at the county level using the exogenous variation due to the Moody's recalibration in 2010. 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, and the 2009-2012 county-year panel with county and state-year fixed effects.

The first-stage regression is the same as that in column (6) of Panel B of Table 5, and reproduced in Panel A of Table 11. The sample of counties is defined by data availability in each regression (government employment, private employment, and income data). The variable *Recalibrated* × *Post* is positive and significant in all regressions. F-statistics are above ten for the private employment and income regressions, suggesting that our instrument is not subject to the weak instrument problem. For the government employment regression, the F-statistic is close to six, which may partly explain the noisier second-stage estimate.

Panel B of Table 11 presents the results of the second-stage regressions. 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. Given 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 spending 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.[21]

The creation of local government jobs is calculated as the product of the estimate in column (1) of Table 11, Panel B, by the ratio of local government employment to government spending by county. The estimates indicate that a marginal million dollars in local government spending results in 12 jobs (= 1.216 × 9.7) in the local government sector.

The elasticity in column (2) of Table 11 can be translated into the corresponding increase in private sector jobs by multiplying it by the ratio of private employment to government spending.

[21] The additional government spending after the recalibration, at the county level, can be calculated as the product of the estimate in columns (3) of Table 11, Panel B, by the average of the annual local government expenditures by the county (see Table 1): 7.8% × \$475 million = \$37 million. This increase is supported by an average annual increase in the amount issued of \$38 million (obtained by multiplying the coefficient estimate in column (3) of Table IA.4, Panel A, of 20.9% by the average amount issued of \$180 million by county).

The results indicate that a marginal million dollars in local government spending results in 36 jobs (= 1.467 × 77) in the private sector. Overall, our results suggest that $1 million in spending increases total employment (local government and private) by 48 jobs (= 12 + 36), which corresponds to a cost per job created of $21,000 (the inverse of the local employment multiplier).

The marginal increase in income is obtained as the product of the estimate in column (3) by the ratio of income to government spending by county. This implies that government spending has a local income multiplier of 2.4 (= 0.636 × 3.8). Combining the income and employment multipliers, we estimate that the jobs created have a remuneration of 2.4 × $21,000 = $50,000.

Although we use a different setting, our estimates are close 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. Acconcia, Corsetti, and Simonelli (2014) use a law passed to combat political corruption and Mafia infiltration of city councils in Italy as source of variation in local public spending. Suarez-Serrato and Wingender (2014) exploit variation in federal spending directed to counties due to changes in the count of the local population 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. The author estimates a state-level spending multiplier above 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.[22] Chodorow-Reich et al. use pre-crisis state-level Medicaid spending to extract the exogenous component of state fiscal relief during the 2009 American

[22] 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.

Recovery and Reinvestment Act (ARRA), and find a cost per job of $26,000.[23]

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 they are larger or smaller than national multipliers is not clear. Nakamura and Steinsson (2011) study the theoretical mapping from these estimates of local fiscal multipliers to the national multiplier in an open economy setting. They show that the cross-sectional estimate of the local fiscal multiplier will coincide with the national multiplier only when nominal interest rates are unresponsive.[24]

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 standard neoclassical model, output multipliers based on deficit-financed spending could be larger than multipliers based on windfall spending, as households increase labor supply and hence output as they recognize that increased government spending requires increased future taxes.[25]

[23] 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.

[24] 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 spillover to other regions. However, labor mobility is likely small over a period of time as short as that we consider.

[25] 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

Our estimates of a cost of $21,000 per job and an income multiplier of 2.4 are at the upper end of the range in this literature. 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.[26]

Eggertsson (2008) and Christiano, Eichenbaum, and Rebelo (2011), among others, employ general equilibrium models with some Keynesian features. They suggest that the fiscal multiplier in periods with a binding zero lower bound on nominal interest rates (which are recessionary times) could be somewhere between 3 and 5. Intuitively, with the 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%. Furthermore, the ratings recalibration took place when state and local governments were facing severe financial constraints from 2007-2009 recession.[27] 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 prevailing during our sample period (e.g., Fishback and Kachanovskaya 2010; Auerbach and Gorodnichenko 2012).

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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.

[26] 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.

[27] 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 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.

6. Effect at High versus Low Economic Slack

We next investigate whether the effects of government spending on the local economy are larger in counties with greater economic slack. To investigate the extent to which our results capture counties with high versus low slack, we estimate the difference-in-differences panel regressions in Tables 5, 6, 7, and 10 including a triple interaction term *Recalibrated* × *Post* × *High Slack,* where *High Slack* is a dummy variable that takes a value of one in periods of high economic slack. The coefficient on the triple interaction term measures the differential effect between counties of high and low economic slack. We define the high and low slack periods in terms of the pre-treatment unemployment rate and change in house prices at the county level. A first definition considers that *High Slack* takes a value of one if the county-level unemployment rate in 2010 (as of March) is above the median across all counties. A second definition considers that *High Slack* takes a value of one if the county-level 2007-2009 change in house prices is above the median across all counties.

Table 12 presents the results using the logarithm of government employment, private employment, and income as dependent variables. We use the 2007-2013 panel and specifications with year and county fixed effects (instead of state-year fixed effects) to have sufficient time series and cross-sectional variation. Panels A and B present the results using slack defined by unemployment and real estate prices, respectively.

Panel A shows that our effect is driven by those counties with greater economic slack as the triple interaction term coefficient is positive and significant; the interaction term *Recalibrated* × *Post* is no longer significant (i.e., the effect is small for counties that are doing well according to the unemployment and house price measures). Panel B shows similar results for private employment and income, but the estimates are less precise in the case of government employment. In short, the results support the idea that the multiplier effects of government spending are larger when local economies have greater economic slack.

7. Robustness

We first address the concern that counties in the treatment group are significantly larger than the counties in the control group.[28] Although the analysis of the pre-trends of our main outcome variables indicate that treatment and control counties do not behave differently prior to the recalibration, we investigate the possibility that the difference in county size is driving our results. We do that in two different ways. First, we include county size groups-by-year fixed effect in our main specifications. Counties are ranked into groups according to the terciles of the distribution of the number of households, and each tercile indicator is interacted with year fixed effects. Second, we run a placebo test in which we replace our treatment variable by dummies indicating the size group of the county.

Panel A of Table 13 presents the results of the effect of the recalibration on our main outcome variables when we include county size groups-by-year fixed effect in the 2009-2012 panel. Column (1) presents the results for government employment, which indicate an effect of 5%-7%. Column (2) presents the results for private employment effects at 3%-4%. Finally, column (3) presents the results for the income effects at 4%-5%. Therefore, the results are both qualitatively and quantitatively similar to our baseline results.[29]

Table IA.10 in the Internet Appendix presents the results of a placebo test in which we replace our *Recalibrated* variable with size group dummies (defined by the terciles of the distribution of the number of households) of the county. If our results are driven by any unobservable characteristics that are common to large counties, we should observe larger coefficients for larger counties around the date of the recalibration in 2010. The differential effects on our main outcome variables are both statistically and economically insignificant. These results help to rule out the possibility that our findings are driven by a size differential

[28] This result is expected because, even though the upgrades were random, an increase in the number of local governments increases the likelihood that the county will have at least one upgraded entity.
[29] Panel A of Table IA.9 in the Internet Appendix shows similar results when we include county size groups fixed effects in the 2009-2011 cross-sectional growth rate regressions.

between the treatment and control counties.

We estimate the impact of the ratings recalibration on local economic outcomes using a sample that includes all counties regardless of whether or not they issue new bonds in the municipal bond market during our sample period. Thus, the control group may include counties that are less financially constrained as they have no need to issue debt. This should bias against finding an effect of the recalibration, as the control group includes higher quality and less financially constrained counties. To further address this concern, we run the regressions of government employment, private employment, and income by using a sample that includes only counties with local entities with at least one new bond issue in the municipal bond market in the three-year period before the recalibration.

Panel B of Table 13 presents the results using the 2009-2012 panel. Columns (1) and (2) present the results for government expenditures and employment, respectively. The point estimate on the *Recalibrated* variable when using local government expenditures as the dependent variable is 4% in Panel A (panel regression) and 9% in Panel B (cross-sectional growth regression). This is lower in magnitude compared to the full sample results in Table 5, but still statistically significant. The results on local government employment are close to those in Table 6, at 6% and 4% for the panel and the cross-sectional specifications, respectively. Column (3) presents the results for private employment. The private employment results are lower than those in Table 7 with a differential effect of 2.8%-3.6%. Column (4) shows that the differential effect on income is positive at about 5%, which is similar in magnitude to the effect in Table 10, and statistically significant.[30] In short, the results using a sample restricted to local governments that issue bonds during the pre-recalibration period are similar to our baseline estimates.

In order to eliminate any concerns that our results might be affected by the fact that we have

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[30] Panel B of Table IA.9 in the Internet Appendix shows similar estimates when we estimate 2009-2011 cross-sectional growth rate regressions using the sample restricted to counties with new bond issues in the pre-recalibration period.

more counties in the control group than in the treatment group, we also run our regressions using a matched sample (with replacement). For each county in the treatment group, we find a county in the control group within the same state with the closest (nearest neighbor) number of households as of 2009. The sample consists of 892 treated and control counties. The matched sample also helps to reduce the difference in size between treated and control counties relative to the baseline sample (see Table 2, Panel B). The average number of households is 75,000 for the treatment group versus 37,000 for the control group, while the median is 30,000 versus 22,000. The matched sample results in Panel C of Table 13 are both qualitatively and quantitatively similar to our baseline results.

8. Conclusion

In this paper, we provide causal estimates of the effect of municipalities' credit constraints on local economies by exploring the exogenous variation in ratings due to the Moody's recalibration in 2010 of its U.S. municipal bond ratings scale. The recalibration generates cross-sectional variation in ratings across local governments, 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 asymmetric effect on local governments' credit ratings leads to increases in local government expenditures and employment. There are also positive spillover effects to the private sector as local governments experience an increase in their access to credit markets, as well as a debt-financed increase in government spending. County-level private employment and income respond in a significant way to the positive shock to local government liquidity. 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. We provide new evidence of a link between municipal bond markets and the real economy. Our findings are consistent with the New Keynesian view of the economy in which aggregate demand shock, 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.

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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 observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. The sample in Panel B consists of observations on counties from 2007 to 2013.

	Mean	Median	Standard Deviation	Minimum	Maximum	Number of Observations	Number of Counties
Panel A: Issue-Level Variables							
Issue Amount ($ million)	4.48	0.85	24.13	0.00	3,000.00	202,615	1,781
Offer Yield (%)	2.83	2.88	1.41	0.30	6.65	202,615	1,781
Moody's Ratings	18.55	19.00	1.65	12.00	21.00	202,615	1,781
S&P Ratings	19.10	19.00	1.60	12.00	21.00	111,367	585
Recalibrated Dummy	0.751	1.000	0.432	0.000	1.000	202,615	1,781
Panel B: County-Level Variables							
Local Government Expenditures ($ million)	475.41	68.10	2,664.74	0.04	108,487.30	20,734	2,962
Local Government Employment (thousand)	4.62	0.71	17.48	0.00	437.54	11,287	1,618
Private Employment (thousand)	36.80	6.61	133.69	0.01	3,910.43	21,649	3,117
Non-Tradable Employment (thousand)	6.19	0.94	21.47	0.00	685.64	20,567	3,065
Tradable Employment (thousand)	3.41	0.45	13.78	0.00	417.55	11,249	2,033
Income ($ million)	1,817.97	329.31	6,383.17	0.66	197,206.30	18,683	3,116
Recalibrated	0.032	0.000	0.080	0.000	1.000	22,327	3,195

Table 2
Treatment and Control Groups Pre-Treatment Characteristics

This table shows pre-treatment means and *p*-values of differences in means between treatment and control groups. The sample in Panel A consists of observations of Ipreo i-Deal municipal new bond issues from April 2007 to March 2010. The sample in Panel B consists of observations on counties from 2007 to 2009.

Panel A: Issue-Level Variables

	Mean		Difference p-value	Number of Observations	
	Recalibrated Dummy = 1	Recalibrated Dummy = 0		Recalibrated Dummy = 1	Recalibrated Dummy = 0
Issue Amount ($ million)	4.75	6.07	0.11	67,268	23,860
Offer Yield (%)	3.46	3.42	0.18	67,268	23,860
Moody's Ratings	17.96	18.72	0.00	67,268	23,860
S&P Ratings	19.04	19.41	0.01	40,949	11,189

Panel B: County-Level Variables

	Mean		Difference	Number of Observations	
	Recalibrated > 0	Recalibrated = 0	p-value	Recalibrated > 0	Recalibrated = 0
Households (thousands)	81.68	12.39	0.000	2,895	6,448
Private Employment (thousands)	96.73	10.65	0.000	2,898	6,379
Fraction of Local Government Employment	0.065	0.068	0.246	2,298	2,522
Fraction of Non-Tradable Employment	0.189	0.164	0.000	2,411	2,525
Fraction of Tradable Employment	0.044	0.047	0.287	2,411	2,548
Growth Local Government Expenditures	0.055	0.044	0.000	2,050	4,020
Growth Local Government Employment	0.011	0.006	0.282	1,542	1,683
Growth Private Employment	-0.026	-0.028	0.197	1,933	4,221
Growth Non-Tradable Employment	-0.036	-0.040	0.417	1,930	3,973
Growth Tradable Employment	-0.093	-0.120	0.137	1,543	1,458
Growth Income	-0.008	-0.009	0.421	1,930	4,296

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

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings of new bond issues around the Moody's recalibration in April-May 2010. Panel A presents issue-level results. Panel B presents local government-level results using the average rating across all issues of a given local government in each event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one for the April 2010 to March 2013 period. Controls include a dummy for general obligation bonds, and a dummy for Build America Bonds. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***,**,* indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's			Rating S&P			Rating Moody's - Sample S&P		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Issue Level									
Recalibrated Dummy × Post	0.562***	0.580***	0.594***	-0.043	-0.034	-0.052	0.612***	0.626***	0.584***
	(0.049)	(0.049)	(0.060)	(0.059)	(0.059)	(0.086)	(0.064)	(0.063)	(0.081)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.83	0.85	0.89	0.83	0.83	0.86	0.81	0.83	0.86
Number of Observations	202,615	202,615	202,615	111,367	111,367	111,367	111,367	111,367	111,367
Panel B: Local Government Level									
Recalibrated Dummy × Post	0.692***	0.699***	0.647***	-0.126	-0.110	-0.009	0.692***	0.699***	0.647***
	(0.065)	(0.065)	(0.069)	(0.191)	(0.191)	(0.207)	(0.065)	(0.065)	(0.069)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.89	0.89	0.91	0.73	0.73	0.76	0.89	0.90	0.91
Number of Observations	10,061	10,061	10,061	4,211	4,211	4,211	10,061	10,061	10,061
Number of Local Governments	4,335	4,335	4,335	1,660	1,660	1,660	4,335	4,335	4,335

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

This table presents difference-in-differences estimates of regressions of the logarithm of issue amount and offer yield of new bond issues around the Moody's recalibration in April-May 2010. Panel A presents issue-level results. Panel B presents local government-level results using the logarithm of total issue amount and the average offer yield across all issues of a given local government in each event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one for April 2010 to March 2013 period. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)			Offer Yield		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Issue Level						
Recalibrated Dummy × Post	0.113**	0.121***	0.166**	-0.119***	-0.101***	-0.090**
	(0.045)	(0.045)	(0.069)	(0.033)	(0.032)	(0.046)
Controls	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.57	0.57	0.61	0.36	0.38	0.42
Number of Observations	202,615	202,615	202,615	202,615	202,615	202,615
Panel B: Local Government Level						
Recalibrated Dummy × Post	0.187***	0.188***	0.233***	-0.320***	-0.308***	-0.196**
	(0.059)	(0.059)	(0.069)	(0.079)	(0.077)	(0.078)
Controls	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.86	0.86	0.89	0.69	0.71	0.75
Number of Observations	10,061	10,061	10,061	10,061	10,061	10,061
Number of Local Governments	4,335	4,335	4,335	4,335	4,335	4,335

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 around the Moody's recalibration in April-May 2010. Panel A presents local government-level results. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. Panel B presents county-level results using the aggregated local government expenditures in each county and year. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on local government units from 2007 to 2013 (as of July of each year). Robust standard errors clustered at the local government level (in Panel A) and county level (in Panel B) are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Panel A: Local Government Level								
Recalibrated Dummy × Post	0.024***	0.023***	0.034***	0.012***	0.010*	0.018***		
	(0.005)	(0.006)	(0.008)	(0.004)	(0.005)	(0.007)		
Recalibrated Dummy							0.015***	0.016**
							(0.005)	(0.006)
Local Gov. Type-Year Fixed Effect	Yes	Yes	No	Yes	Yes	No	No	No
Local Gov. Type Fixed Effect	No	No	No	No	No	No	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
County-Year Fixed Effects	No	Yes	No	No	Yes	No	No	No
Local Gov. Type-County-Year Fixed Effect	No	No	Yes	No	No	Yes	No	No
County Fixed Effects	No	No	No	No	No	No	No	Yes
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.19
Number of Observations	115,563	115,563	115,563	77,034	77,034	77,034	19,251	19,251
Number of Local Governments	19,270	19,270	19,270	19,269	19,269	19,269	19,251	19,251
Panel B: County Level								
Recalibrated × Post	0.048**	0.029	0.121***	0.026	0.053***	0.086***		
	(0.023)	(0.023)	(0.025)	(0.021)	(0.020)	(0.024)		
Recalibrated							0.055*	0.109***
							(0.029)	(0.034)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.22	0.22	0.32	0.01	0.08	0.11	0.04	0.12
Number of Observations	20,734	20,734	20,734	11,848	11,848	11,848	2,962	2,962
Number of Counties	2,962	2,962	2,962	2,962	2,962	2,962	2,962	2,962

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 around the Moody's recalibration in April-May 2010. Panel A presents local government-level results. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. Panel B presents county-level results using the aggregated local government employment in each county and year. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on local government units from 2007 to 2013 (as of March of each year). Robust standard errors clustered at the local government level (in Panel A) and county level (in Panel B) are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Panel A: Local Government Level								
Recalibrated Dummy × Post	0.007	0.016*	0.021	0.006	0.015**	0.021*		
	(0.006)	(0.009)	(0.013)	(0.005)	(0.007)	(0.012)		
Recalibrated Dummy							0.011**	0.019*
							(0.006)	(0.010)
Local Gov. Type-Year Fixed Effect	Yes	Yes	No	Yes	Yes	No	No	No
Local Gov. Type Fixed Effect	No	No	No	No	No	No	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
County-Year Fixed Effects	No	Yes	No	No	Yes	No	No	No
Local Gov. Type-County-Year Fixed Effect	No	No	Yes	No	No	Yes	No	No
County Fixed Effects	No	No	No	No	No	No	No	Yes
R^2	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.30
Number of Observations	34,711	34,711	34,711	19,832	19,832	19,832	4,952	4,952
Number of Local Governments	4,979	4,979	4,979	4,970	4,970	4,970	4,952	4,952
Panel B: County Level								
Recalibrated × Post	0.037	0.045*	0.093***	0.029	0.036*	0.064***		
	(0.025)	(0.024)	(0.025)	(0.022)	(0.021)	(0.022)		
Recalibrated							0.074***	0.045
							(0.026)	(0.031)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.02	0.02	0.09	0.02	0.02	0.09	0.00	0.09
Number of Observations	11,287	11,270	11,270	6,451	6,439	6,439	1,609	1,609
Number of Counties	1,618	1,615	1,615	1,616	1,613	1,613	1,609	1,609

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

This table presents county-level difference-in-differences estimates of regressions of the logarithm of private employment in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2007 to 2013 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Recalibrated × Post	0.048***	0.030**	0.071***	0.025**	0.022**	0.035***		
	(0.013)	(0.013)	(0.015)	(0.011)	(0.010)	(0.012)		
Recalibrated							0.029**	0.043***
							(0.013)	(0.016)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.13	0.13	0.21	0.03	0.03	0.08	0.00	0.05
Number of Observations	21,649	21,649	21,649	12,375	12,375	12,375	3,074	3,074
Number of Counties	3,117	3,117	3,117	3,117	3,117	3,117	3,074	3,074

Table 8
Difference-in-Differences of Non-Tradable and Tradable Sectors Employment around the Recalibration

This table presents county-level difference-in-differences estimates of regressions of the logarithm of non-tradable and tradable employment in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2007 to 2013 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Panel A: Non-Tradable Employment								
Recalibrated × Post	0.224***	0.163***	0.214***	0.141***	0.101**	0.122**		
	(0.057)	(0.056)	(0.060)	(0.045)	(0.044)	(0.048)		
Recalibrated							0.099**	0.069
							(0.040)	(0.046)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.63	0.63	0.65	0.64	0.64	0.65	0.00	0.03
Number of Observations	20,567	20,567	20,567	11,738	11,738	11,738	2,922	2,922
Number of Counties	3,065	3,065	3,065	3,033	3,033	3,033	2,922	2,922
Panel B: Tradable Employment								
Recalibrated × Post	-0.064	-0.027	-0.008	-0.093	-0.055	-0.035		
	(0.106)	(0.106)	(0.119)	(0.136)	(0.137)	(0.146)		
Recalibrated							-0.375	-0.230
							(0.231)	(0.233)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.05	0.05	0.09	0.01	0.02	0.04	0.02	0.04
Number of Observations	11,249	11,249	11,249	6,363	6,363	6,363	1,451	1,451
Number of Counties	2,033	2,033	2,033	1,879	1,879	1,879	1,451	1,451

Table 9
Difference-in-Differences of Construction, Education, and Health Sector Employment around the Recalibration

This table presents county-level difference-in-differences estimates of panel regressions of the logarithm of construction, education, and healthcare sectors employment in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2009 to 2012 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Construction		Educational Services		Health Care and Social Assistance	
Recalibrated × Post	-0.027	0.013	0.075**	0.106***	0.089***	0.082***
	(0.030)	(0.032)	(0.036)	(0.041)	(0.018)	(0.021)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No	No	No	No
State-Year Fixed Effects	No	Yes	No	No	No	No
State Fixed Effects	No	No	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	No	No	No	No
R^2	0.09	0.18	0.04	0.10	0.02	0.05
Number of Observations	10,463	10,463	4,325	4,325	10,471	10,471
Number of Counties	2,920	2,920	1,289	1,289	2,775	2,775

Table 10
Difference-in-Differences of Income around the Recalibration

This table presents county-level difference-in-differences estimates of regressions of the logarithm of income in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2010 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2006 to 2012 (as of December of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2006-2012			Panel 2008-2011			Growth 2008-2010	
Recalibrated × Post	0.053***	0.047***	0.090***	0.046***	0.052***	0.072***		
	(0.015)	(0.014)	(0.017)	(0.012)	(0.012)	(0.013)		
Recalibrated							0.069***	0.081***
							(0.013)	(0.014)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.48	0.48	0.58	0.48	0.48	0.55	0.01	0.14
Number of Observations	18,683	18,683	18,815	12,453	12,453	12,541	3,112	3,135
Number of Counties	3,116	3,116	3,138	3,115	3,115	3,137	3,112	3,135

Table 11
Instrumental Variable Estimates of the Elasticity of Local Employment and Income

This table presents county-level instrumental variable estimates of panel regressions of the logarithm of government employment, private employment, and income in each county and year. Local government expenditures are instrumented with the *Recalibrated* × *Post* interaction variable. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable 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 observations on counties 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 * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Government Employment	Private Employment	Income
Panel A: First Stage - Local Government Expenditures			
Recalibrated × Post	0.055**	0.081***	0.078***
	(0.022)	(0.025)	(0.024)
Controls	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
R^2	0.99	0.99	0.99
F-statistic	6.316	10.63	10.27
Number of Observations	6,371	11,751	11,843
Number of Counties	1,596	2,959	2,961
Panel B: Second Stage			
Local Gov. Expenditures	1.216*	0.467**	0.636***
	(0.625)	(0.215)	(0.245)
Controls	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.99	0.99	0.99
Number of Observations	6,371	11,751	11,843
Number of Counties	1,596	2,959	2,961

Table 12
Difference-in-Differences of Economic Outcomes around the Recalibration: High versus Low Slack Regions

This table presents county-level difference-in-differences estimates of panel 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. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. In Panel A, the *High Slack* dummy takes a value of one when the county unemployment rate in 2010 is below the median across counties. In Panel B, the *High Slack* dummy takes a value of one when the county change in house price index between 2007 and 2009 is below the median across counties. Controls include house price index and number of households. The sample consists of observations on counties 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 * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Government Employment	Private Employment	Income
Panel A: Unemployment Rate			
Recalibrated × Post	0.008	-0.005	0.001
	(0.030)	(0.014)	(0.016)
Recalibrated × Post × High Slack	0.105*	0.084***	0.103***
	(0.057)	(0.029)	(0.027)
Controls	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.018	0.147	0.507
Number of Observations	11,270	21,642	18,809
Number of Counties	1,615	3,116	3,137
Panel B: Change in House Prices			
Recalibrated × Post	0.093**	0.012	0.026
	(0.038)	(0.022)	(0.023)
Recalibrated × Post × High Slack	-0.030	0.062**	0.072**
	(0.051)	(0.028)	(0.031)
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.021	0.140	0.507
Number of Observations	11,268	21,632	18,810
Number of Counties	1,614	3,114	3,136

Table 13
Difference-in-Differences of Economic Outcomes around the Recalibration: Robustness

This table presents county-level difference-in-differences estimates of panel 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. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. Regressions in Panel A include county size group-by-year fixed effects. Counties are ranked into groups according to the terciles of the distribution of the number of households. The sample in Panel B is restricted to counties with at least one local government unit with a new bond issue in the Ipreo i-Deal database from April 2007 to March 2010. Panel C uses a matched sample of counties. For each county in the treatment group, we select a county from the control group within the same state that best match (nearest neighbor) the treated one on number of households as of 2009. Controls include house price index and number of households. The sample consists of observations on counties 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 * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Government Employment	Private Employment	Income
Panel A: County Size Group-by-Year Fixed Effects			
Recalibrated × Post	0.067***	0.026**	0.042***
	(0.020)	(0.011)	(0.011)
Controls	Yes	Yes	Yes
Size Group-Year Fixed Effects	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.001	0.002	0.002
Number of Observations	4,828	9,261	9,402
Number of Counties	1610	3097	3135
Panel B: Sample of Counties with New Bond Issues			
Recalibrated × Post	0.060**	0.028**	0.047***
	(0.024)	(0.011)	(0.013)
Controls	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.128	0.123	0.702
Number of Observations	3,480	5,312	5,334
Number of Counties	1,160	1,772	1,778
Panel C: Matched Sample			
Recalibrated × Post	0.046	0.027**	0.043***
	(0.044)	(0.013)	(0.015)
Controls	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.999	0.999	0.999
Number of Observations	5,494	7,127	7,136
Number of Counties	879	1,222	1,223

Figure 1
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 grey color have no local government unit issuing bonds in the three years prior to the recalibration in the Ipreo i-Deal database (1,365 counties). Counties in white color have no local government unit upgraded (812 counties). Counties in light blue, medium blue, and dark blue color are in the bottom tercile (322 counties), medium tercile (323 counties), and top tercile (322 counties) of the distribution of the *Recalibrate* variable (considering non-zero values), respectively.

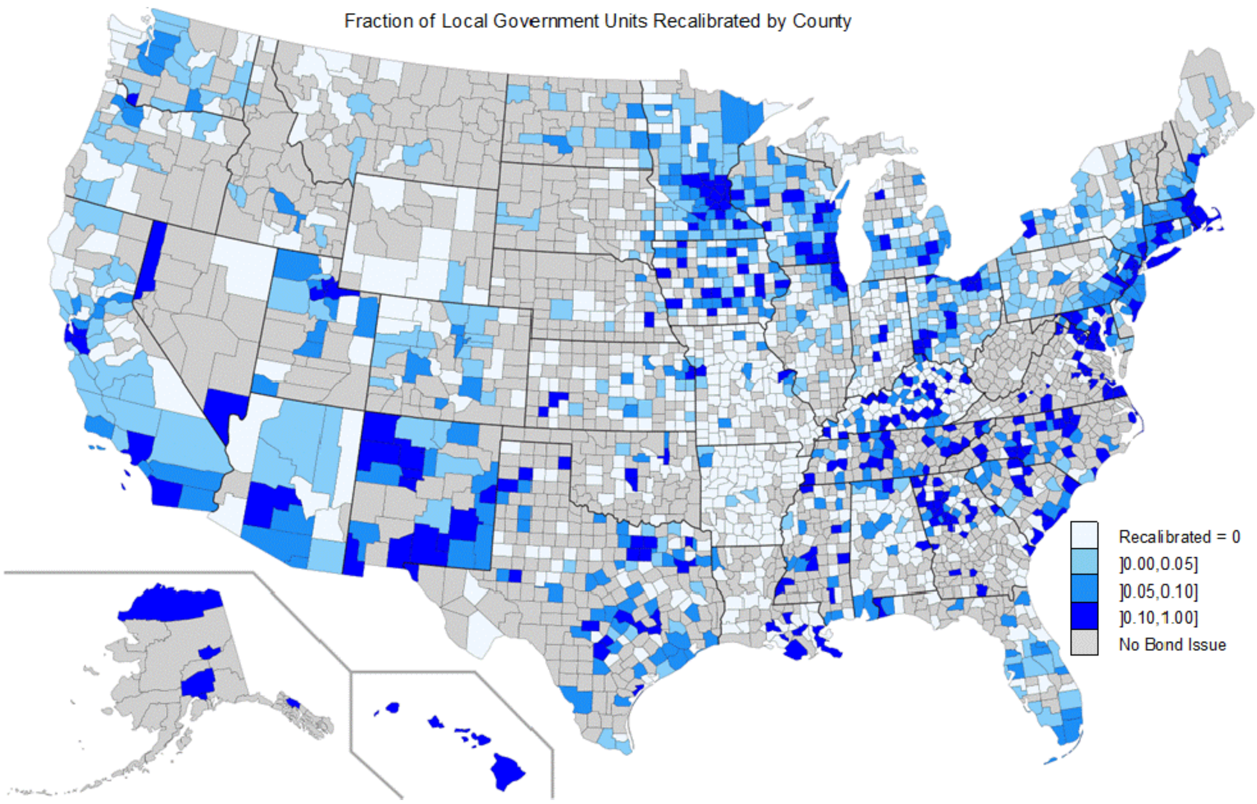


Figure 2
Moody's and S&P Ratings around the Recalibration

This figure shows issue-level point estimates and 90% confidence intervals for the effect on the S&P and Moody's ratings of upgraded local governments (treated) relative to non-upgraded local governments (control) during the Moody's recalibration event in April-May 2010.

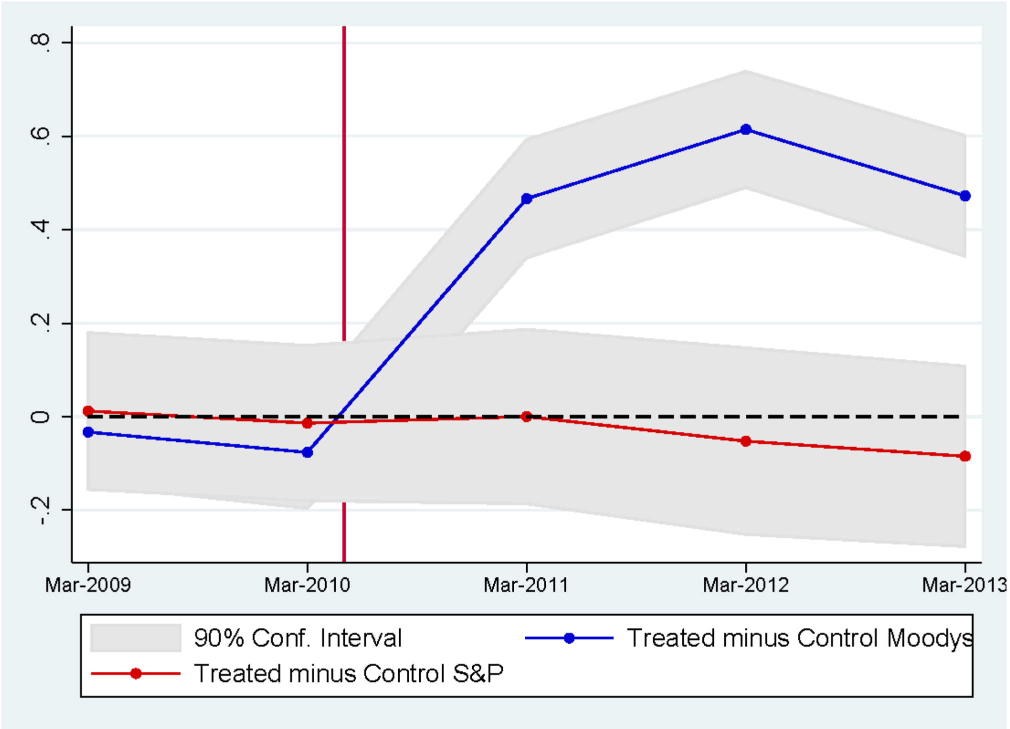


Figure 3
Issue Amount around the Recalibration

This figure shows issue-level point estimates and 90% confidence intervals for the effect on the logarithm of the issue amount of upgraded local governments (treated) relative to non-upgraded local governments (control) during the Moody's recalibration event in April-May 2010.

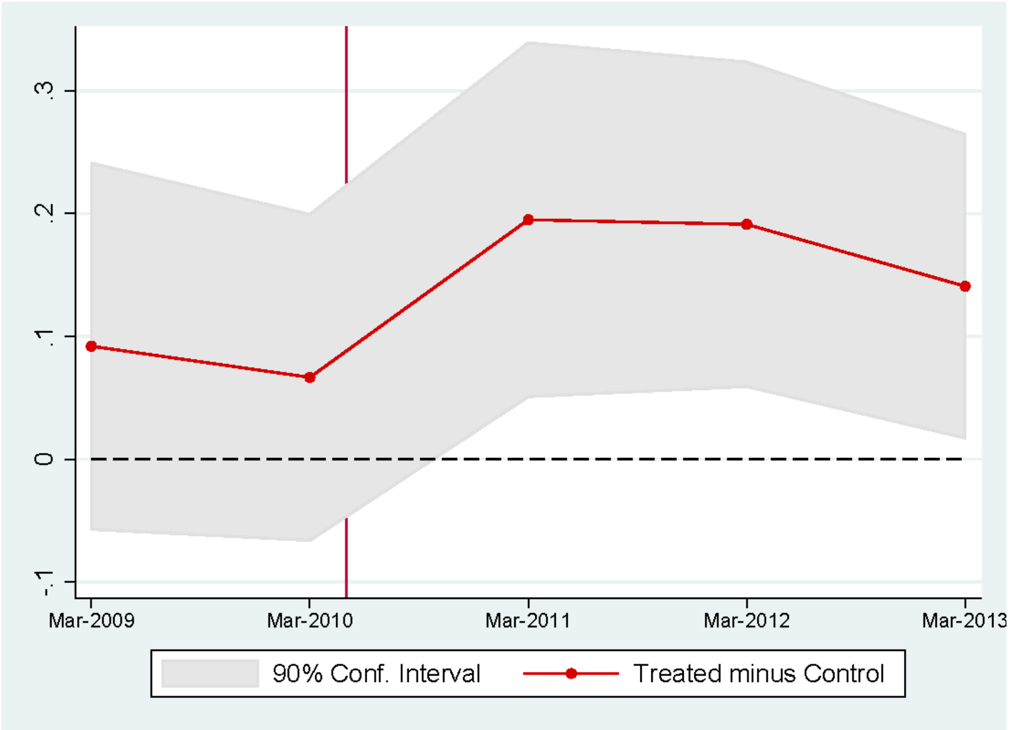


Figure 4
Local Government Employment around the Recalibration

This figure shows county-level parallel trends (top panel) and point estimates and 90% confidence intervals (bottom panel) of the logarithm of local government employment (as of March of each year) of counties with upgraded local governments (treated counties − non-zero *Recalibrated* variable) and counties without upgraded local governments (control counties − *Recalibrated* variable equals zero) during the Moody's recalibration in April-May 2010.

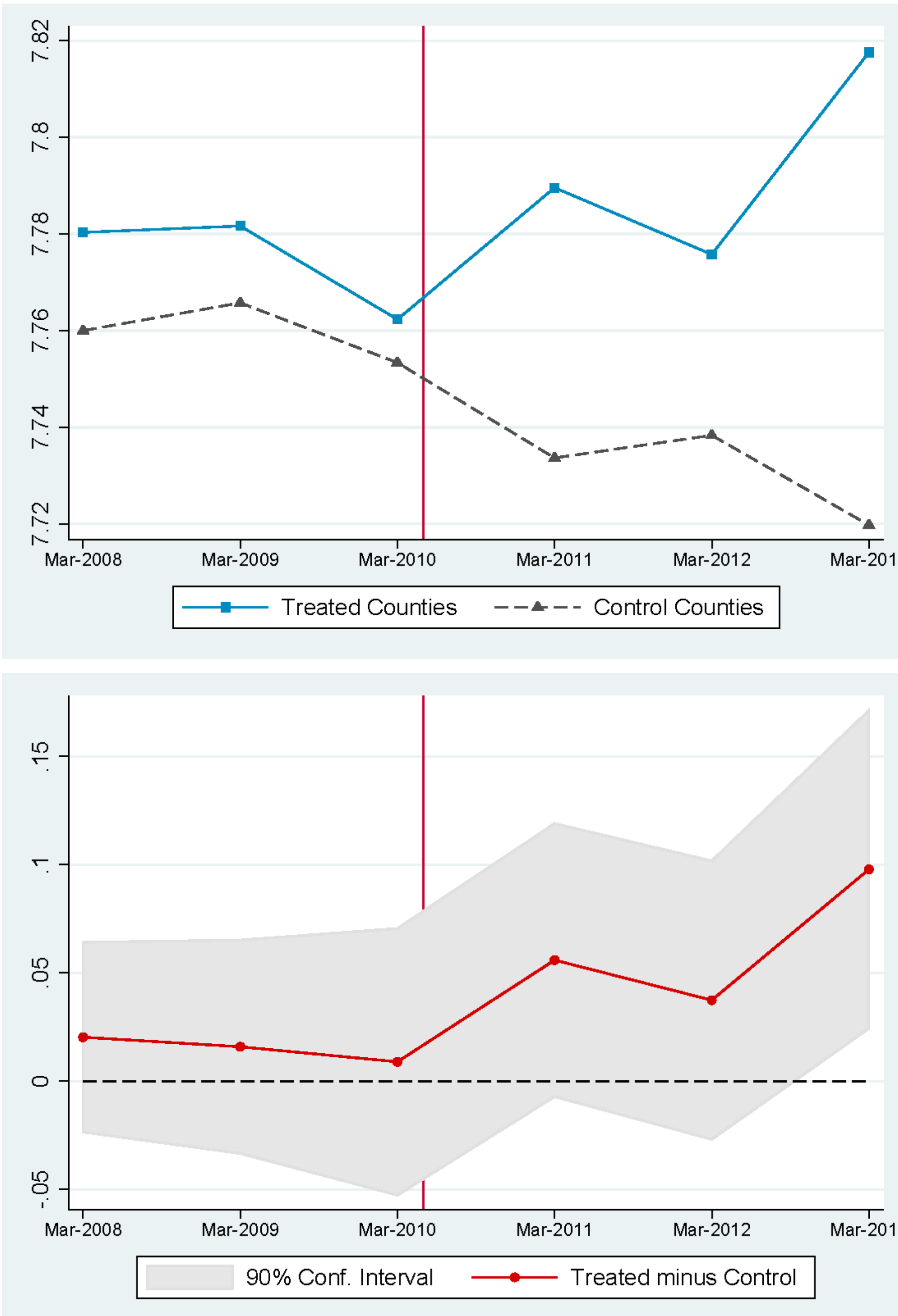


Figure 5
Private Employment around the Recalibration

This figure shows county-level parallel trends (top panel) and point estimates and 90% confidence intervals (bottom panel) of the logarithm of private employment (as of March of each year) of counties with upgraded local governments (treated counties − non-zero *Recalibrated* variable) and counties without upgraded local governments (control counties − *Recalibrated* variable equals zero) during the Moody's recalibration in April-May 2010.

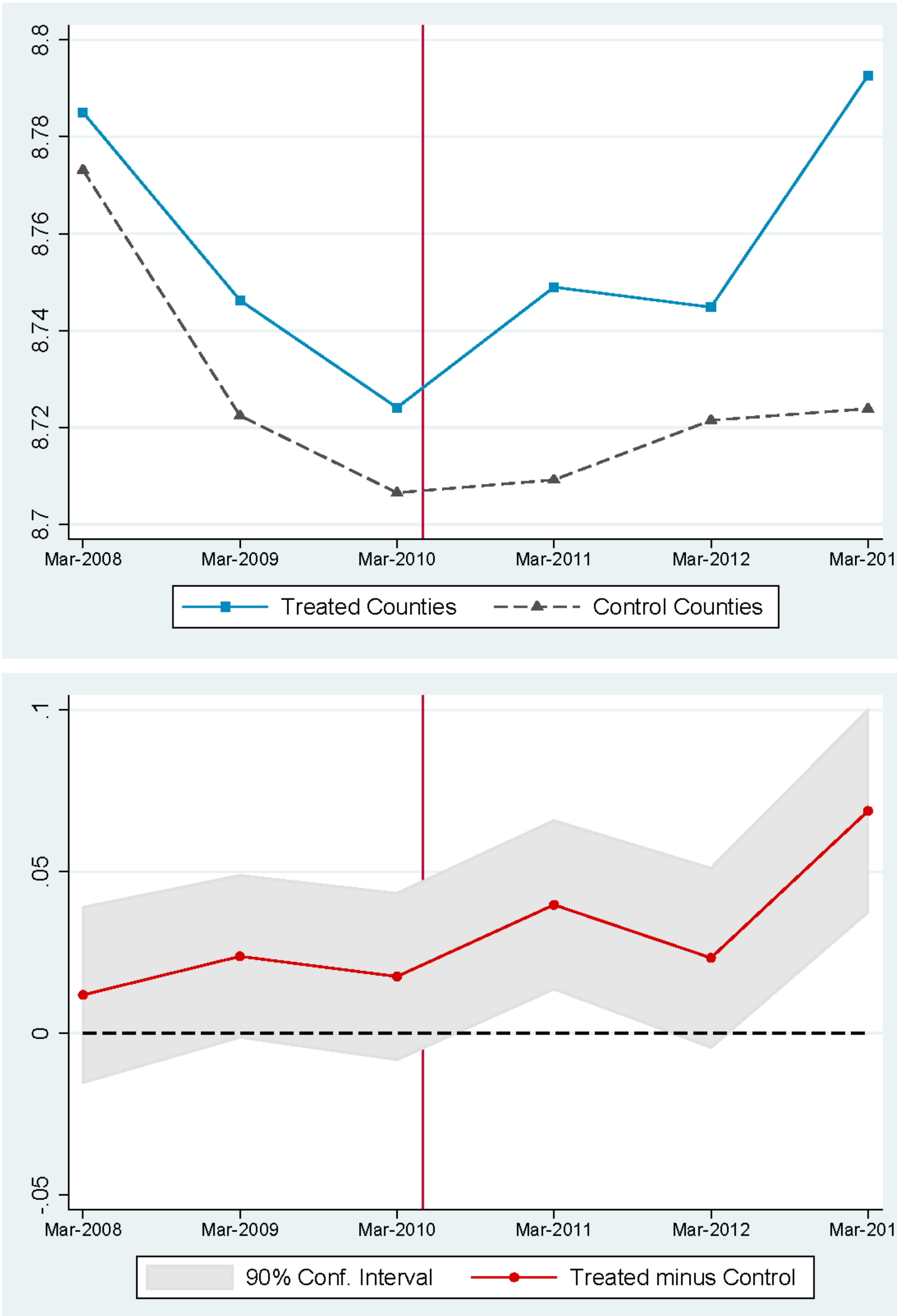
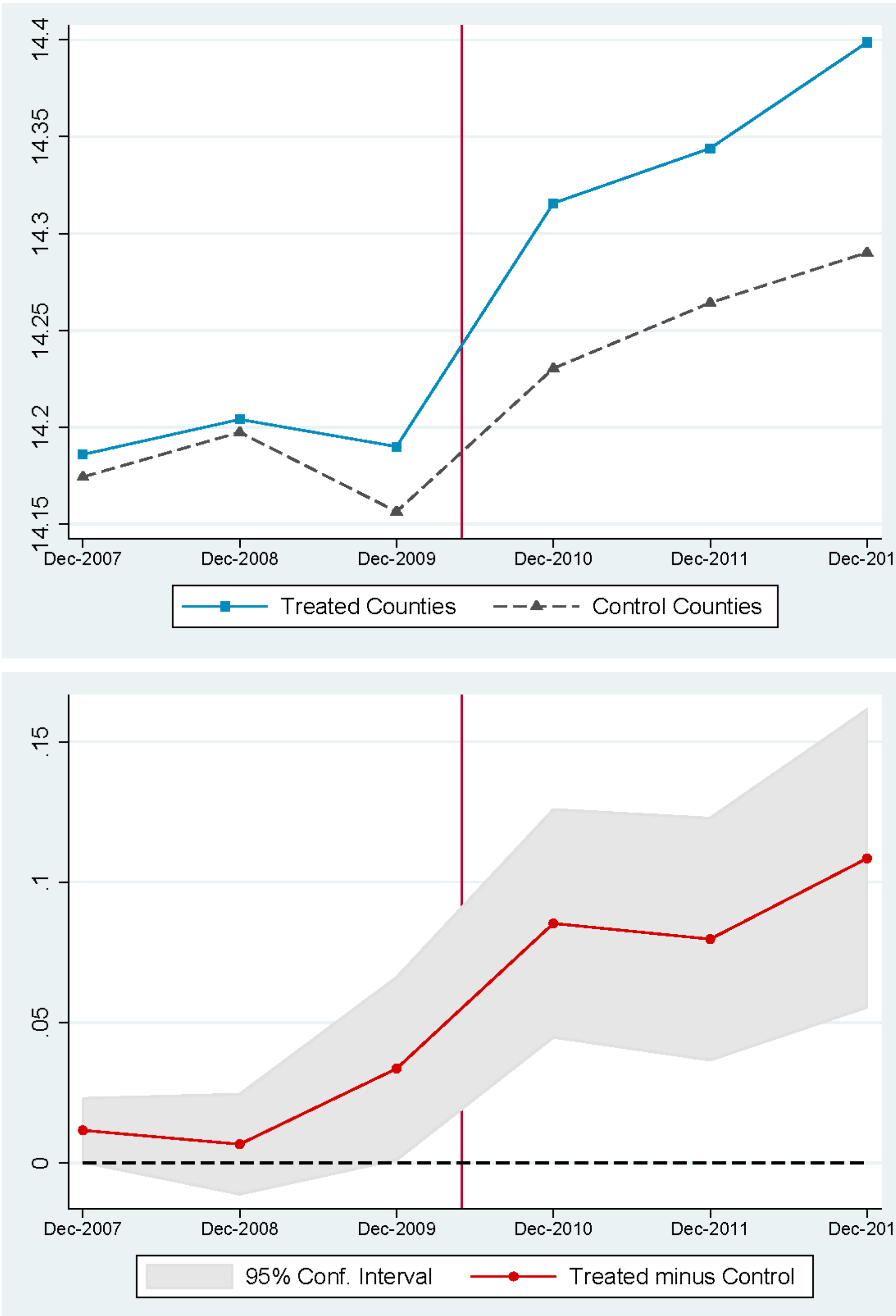


Figure 6
Income around the Recalibration

This figure shows county-level parallel trends (top panel) and point estimates and 90% confidence intervals (bottom panel) of the logarithm of income (as of December of each year) of counties with upgraded local governments (treated counties − non-zero *Recalibrated* variable) and counties without upgraded local governments (control counties − *Recalibrated* variable equals zero) during the Moody's recalibration in April-May 2010.



Internet Appendix to

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

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Table IA.1
Difference-in-Differences Estimates of Ratings around the Recalibration: April 2008-March 2012

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings of new bond issues around the Moody's recalibration in April-May 2010. Panel A presents issue-level results. Panel B presents local government-level results using the average rating across all issues of a given local government in each event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable takes a value of one between April 2010 and March 2012. Controls include a dummy for general obligation bonds, and a dummy for Build America Bonds. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2008 to March 2012. Robust standard errors clustered at the local government level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's			Rating S&P			Rating Moody's - Sample S&P		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Issue Level									
Recalibrated Dummy × Post	0.601***	0.618***	0.601***	-0.035	-0.026	-0.066	0.694***	0.707***	0.597***
	(0.052)	(0.052)	(0.064)	(0.058)	(0.057)	(0.082)	(0.067)	(0.067)	(0.089)
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.85	0.87	0.90	0.84	0.85	0.86	0.84	0.85	0.88
Number of Observations	144,322	144,322	144,322	81,881	81,881	81,881	81,881	81,881	81,881
Panel B: Local Government Level									
Recalibrated Dummy × Post	0.656***	0.665***	0.631***	-0.434*	-0.419	-0.271	0.656***	0.665***	0.631***
	0.084	0.084	0.078	0.255	0.255	0.235	0.084	0.084	0.078
Controls	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
County-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.91	0.91	0.92	0.79	0.79	0.81	0.91	0.91	0.92
Number of Observations	7,004	7,004	7,004	3,109	3,109	3,109	7,004	7,004	7,004
Number of Local Governments	4,335	4,335	4,335	1,660	1,660	1,660	4,335	4,335	4,335

Table IA.2
Difference-in-Differences of Issue Amount and Offer Yield of New Bond Issues around the Recalibration: Robustness

This table presents difference-in-differences estimates of regressions of the logarithm of issue amount and offer yield of new bond issues around the Moody's recalibration in April-May 2010 at the issue-level. Panel A presents issue-level results. Panel B presents local government-level results using the logarithm of total issue amount and the average offer yield across all issues of a given local government in each event year. In the regressions for columns (1) and (2), we restrict the sample to two years before and after the recalibration (2008-2012). In the regressions for columns (3) and (4), we restrict the sample to issues that have both Moody's and S&P ratings. In the regressions for columns (5) and (6), we exclude bonds issued under the "Build for America" government program. In the regressions for columns (7) and (8), we use the sample of all local governments (not just those that issue in the three years before the recalibration). In the regressions for columns (9) and (10), we include insured and uninsured bonds. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, and a dummy for Build America Bonds. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. Robust standard errors clustered at the local government level (in Panel A) and county level (in Panel B) are reported in parentheses. ***,**, and * indicates 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	Offer Yield	Issue Amount	Offer Yield	Issue Amount	Offer Yield	Issue Amount	Offer Yield	Issue Amount	Offer Yield
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A: Issue Level										
Recalibrated Dummy × Post	0.174**	-0.264**	0.281***	-0.233**	0.178**	-0.255***	0.169**	-0.241***	0.153**	-0.214***
	(0.082)	(0.105)	(0.079)	(0.097)	(0.070)	(0.084)	(0.069)	(0.086)	(0.061)	(0.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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^2	0.61	0.58	0.51	0.58	0.61	0.61	0.61	0.65	0.57	0.63
Number of Observations	144,322	143,241	111,367	110,650	194,803	193,265	240,744	238,749	305,013	305,013
Panel B: Local Government Level										
Recalibrated Dummy × Post	0.204**	-0.253**	0.265***	-0.264**	0.220***	-0.233***	0.231***	-0.194**	0.119**	-0.259***
	(0.082)	(0.105)	(0.080)	(0.118)	(0.066)	(0.090)	(0.066)	(0.087)	(0.065)	(0.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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^2	0.91	0.75	0.87	0.72	0.89	0.73	0.89	0.76	0.88	0.76
Number of Observations	7,004	7,004	5,261	5,261	9,874	9,874	12,685	12,685	15,036	15,036
Number of Local Governments	3,701	3,701	2,087	2,087	4,306	4,306	6,470	6,470	6,516	6,516

Table IA.3
Difference-in-Differences of Issue Amount and Offer Yield of New Bond Issues around the Recalibration: Upgrade Notches

This table presents difference-in-differences estimates of regressions of the logarithm of issue amount and offer yield of new bond issues around the Moody's recalibration in April-May 2010 at the issue-level. Panel A presents issue-level results. Panel B presents local government-level results using the logarithm of total issue amount 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 1 notch, 2 notches, and 3 notches respectively in any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable 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 observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Panel A: Issue Level				
Recalibrated 1 Notch × Post	0.114***	0.114***	0.137**	-0.232***
	(0.044)	(0.044)	(0.066)	(0.074)
Recalibrated 2 Notches × Post	0.179***	0.179***	0.119	-0.201*
	(0.054)	(0.054)	(0.083)	(0.103)
Recalibrated 3 Notches × Post	0.321	0.321	0.211	-0.211
	(0.283)	(0.283)	(0.401)	(0.140)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.57	0.57	0.61	0.62
Number of Observations	202,615	201039	202,615	201,039
Panel B: Local Government Level				
Recalibrated 1 Notch × Post	0.152**	-0.185***	0.189***	-0.079
	0.061	0.069	(0.070)	(0.081)
Recalibrated 2 Notches × Post	0.275***	-0.270***	0.232**	-0.008
	0.077	0.090	(0.094)	(0.112)
Recalibrated 3 Notches × Post	0.272	-0.457**	0.219	-0.315
	0.250	0.227	(0.300)	(0.288)
Controls	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R^2	0.86	0.74	0.89	0.77
Number of Observations	10,061	10,061	10,061	10,061
Number of Local Governments	4,335	4,335	4,335	4,335

Table IA.4
Difference-in-Differences of Issue Amount and Offer Yield of New Bond Issues around the Recalibration: County Level

This table presents county-level difference-in-differences estimates of panel regressions of the logarithm of issue amount and offer yield of new bond issues around the Moody's recalibration in April-May 2010. The dependent variables are the logarithm of total issue amount and the average offer yield across all issues of a given county in each event year. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration using equal weights (Panel A) or the total issue amount in a given county during the pre-recalibration period (Panel B). *Post* is a dummy variable takes a value of one between April 2010 and March 2013. Controls include house price index and number of households. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)			Offer Yield		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: County Level - Equal Weights						
Recalibrated × Post	0.229***	0.227***	0.209**	-0.377***	-0.377***	-0.360***
	(0.072)	(0.072)	(0.096)	(0.083)	(0.083)	(0.114)
Controls	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.83	0.83	0.84	0.54	0.54	0.59
Number of Observations	5,974	5,968	5,968	5,974	5,968	5,968
Number of Counties	1,781	1,780	1,780	1,781	1,780	1,780
Panel B: County Level - Weighted by Issue Amount						
Recalibrated × Post	0.228***	0.227***	0.226***	-0.275***	-0.274***	-0.210**
	(0.067)	(0.068)	(0.084)	(0.077)	(0.077)	(0.096)
Controls	No	Yes	Yes	No	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.83	0.83	0.84	0.54	0.54	0.59
Number of Observations	5,974	5,968	5,968	5,974	5,968	5,968
Number of Counties	1,781	1,780	1,780	1,781	1,780	1,780

Table IA.5
Difference-in-Differences of Local Government Current Expenditures and Capital Outlays around the Recalibration

This table presents county-level difference-in-differences estimates of regressions of the logarithm of aggregated local government current expenditures (Panel A) and capital outlays (Panel B) and in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on local government units from 2007 to 2013 (as of July of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Panel A: Current Expenditures								
Recalibrated × Post	0.083***	0.065***	0.150***	0.017	0.036***	0.069***		
	(0.020)	(0.020)	(0.025)	(0.014)	(0.014)	(0.019)		
Recalibrated							0.028	0.052**
							(0.018)	(0.022)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.324	0.327	0.447	0.008	0.0813	0.184	0.051	0.198
Number of Observations	20,734	20,734	20,734	11,848	11,848	11,848	2,962	2,962
Number of Counties	2,962	2,962	2,962	2,962	2,962	2,962	2,962	2,962
Panel B: Capital Outlays								
Recalibrated × Post	0.019	-0.030	0.247	0.218	0.307*	0.346**		
	(0.144)	(0.146)	(0.152)	(0.159)	(0.160)	(0.161)		
Recalibrated							0.522**	0.613**
							(0.253)	(0.243)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.018	0.019	0.073	0.014	0.0577	0.063	0.018	0.065
Number of Observations	20,647	20,647	20,647	11,798	11,798	11,798	2,940	2,940
Number of Counties	2,959	2,959	2,959	2,959	2,959	2,959	2,940	2,940

Table IA.6
Difference-in-Differences of Local Government Taxes around the Recalibration

This table presents county-level difference-in-differences estimates of regressions of the logarithm of aggregated local government taxes in each county and year around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on local government units from 2007 to 2013 (as of July of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007-2013			Panel 2009-2012			Growth 2009-2011	
Recalibrated × Post	0.008	0.025	-0.051**	-0.005	0.018	-0.040**		
	(0.023)	(0.024)	(0.024)	(0.019)	(0.019)	(0.020)		
Recalibrated							0.032	-0.025
							(0.024)	(0.027)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Year Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.26	0.26	0.47	0.08	0.08	0.37	0.00	0.40
Number of Observations	20,540	20,540	20,540	11,740	11,740	11,740	2,933	2,933
Number of Counties	2,939	2,939	2,939	2,937	2,937	2,937	2,933	2,933

Table IA.7
Difference-in-Differences of Government Employment around the Recalibration: Growth 2007-2012

This table presents county-level cross-sectional growth (log change) regressions of government employment around the Moody's recalibration in April and May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. Controls include house price index and number of households. The sample consists of observations on counties in 2007 and 2012 (the two years with a complete Census of local governments). Employment is measured as of March of each year. Robust standard errors clustered at the county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)
Recalibrated	0.046**	0.060***
	(0.022)	(0.022)
Controls	Yes	Yes
State Fixed Effects	No	Yes
R^2	0.00	0.09
Number of Observations	3,134	3,134
Number of Counties	3,134	3,134

Table IA.8
Difference-in-Differences of New Firms Employment around the Recalibration

This table presents county-level difference-in-differences estimates of regressions of the logarithm of new firms (firms less than two years old) employment in the non-tradable sector in each county and quarter around the Moody's recalibration in April-May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration. *Post* is a dummy variable takes a value of one in 2010:Q3 and for each quarter thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2006:Q1 to 2012:Q4. Robust standard errors clustered at county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2006:Q1-2012:Q4			Panel 2008:Q1-2011:Q4			Growth 2009:Q2-2011:Q2	
Recalibrated × Post	0.256***	0.216***	0.210**	0.126	0.122	0.128		
	(0.084)	(0.083)	(0.091)	(0.084)	(0.084)	(0.088)		
Recalibrated							0.246**	0.242*
							(0.117)	(0.127)
Controls	No	Yes	Yes	No	Yes	Yes	Yes	Yes
Quarter Fixed Effects	Yes	Yes	No	Yes	Yes	No	No	No
State-Quarter Fixed Effects	No	No	Yes	No	No	Yes	No	No
State Fixed Effects	No	No	No	No	No	No	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	No	No
R^2	0.09	0.09	0.00	0.06	0.06	0.00	0.00	0.06
Number of Observations	73,805	73,712	73,712	41,613	41,558	41,558	2,394	2,394
Number of Counties	3,023	3,017	3,017	2,966	2,960	2,960	2,394	2,394

Table IA.9
Difference-in-Differences of Economic Outcomes around the Recalibration: Robustness

This table presents county-level cross-sectional growth (log change) regressions of 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 that has been upgraded in each county during the Moody's recalibration. Regressions in Panel A include county size group fixed effects. Counties are ranked into groups according to the terciles of the distribution of the number of households. The sample in Panel B is restricted to counties with at least one local government unit with a new bond issue in the Ipreo i-Deal database from April 2007 to March 2010. Panel C uses a matched sample of counties. For each county in the treatment group, we select a county from the control group within the same state that best match (nearest neighbor) the treated one on number of households as of 2009. Controls include house price index and number of households. The sample consists of observations on counties from 2009 to 2011 (2008 to 2010 in the case of income). Robust standard errors clustered at county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Government Employment	Private Employment	Income
Panel A: County Size Group-by-Year Fixed Effects			
Recalibrated × Post	0.049*	0.035***	0.045***
	(0.026)	(0.014)	(0.013)
Controls	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Size Group Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
R^2	0.091	0.055	0.145
Number of Observations	1,609	3,074	3,135
Number of Counties	1,609	3,074	3,135
Panel B: Sample of Counties with New Bond Issues			
Recalibrated × Post	0.044	0.036***	0.049**
	(0.029)	(0.011)	(0.023)
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
R^2	0.118	0.067	0.163
Number of Observations	1,162	1,770	1,780
Number of Counties	1,162	1,770	1,780
Panel C: Matched Sample			
Recalibrated × Post	0.043*	0.020*	0.069***
	(0.024)	(0.011)	(0.016)
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
R^2	0.001	0.001	0.019
Number of Observations	1,373	1,780	1,784
Number of Counties	1,373	1,780	1,784

Table IA.10
Difference-in-Differences of Economic Outcomes around the Recalibration: Placebo using County Size

This table presents county-level difference-in-differences estimates of panel 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. *Low Size* (omitted group), *Medium Size*, and *Big Size* correspond to the first, second, and third terciles of the distribution of the number of households in a county, respectively. *Post* is a dummy variable takes a value of one in 2010 and for each year thereafter. Controls include house price index and number of households. The sample consists of observations on counties from 2007 to 2013 (measured as of June of each year for expenditures, as of March for employment and as of December for income). Robust standard errors clustered at county level are reported in parentheses. ***,**, and * indicates statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)
	Government Employment	Private Employment	Income
Panel A: Panel 2009-2012			
Medium Size × Post	0.001	-0.001	-0.002
	(0.008)	(0.004)	(0.002)
Big Size × Post	0.010	0.003	0.010***
	(0.008)	(0.004)	(0.002)
Controls	Yes	Yes	Yes
State-Year Fixed Effects	Yes	Yes	Yes
County Fixed Effects	Yes	Yes	Yes
R^2	0.001	0.002	0.004
Number of Observations	4,828	9,261	9,402
Number of Counties	1,610	3,097	3,135
Panel B: Growth 2009-2011			
Medium Size	0.010	-0.004	-0.003
	(0.013)	(0.005)	(0.005)
Big Size	0.017	-0.002	0.011
	(0.012)	(0.006)	(0.006)
Controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes
R^2	0.091	0.055	0.143
Number of Observations	1,609	3,074	3,135
Number of Counties	1,609	3,074	3,135

Figure IA.1
New Firms Employment around the Recalibration

This figure shows county-level parallel trends of the logarithm of new firms (firms less than two years old) employment (at the end of each quarter) in the non-tradable sector of counties with upgraded local governments (treated counties − non-zero *Recalibrated* variable) and counties without upgraded local governments (control counties − non-zero *Recalibrated* variable) during the Moody's recalibration in April-May 2010.