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

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Abstract

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

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

In this paper, we examine how changes to the supply of credit to municipalities in the United States affect local economies, particularly during recessions. Easier access to financing can have important effects on local economic outcomes when governments face financial distress, such as during the 2007–2009 Great Recession.[1] Specifically, through bond financing local governments can alleviate spending cuts (maintaining employees and avoiding program cuts), prevent tax and fee increases, or contribute to their end-of-year balances. These can in turn have positive spillover effects in the private sector arising from increased disposable income. 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 bond ratings due to Moody's 2010 recalibration of its municipal bond rating scale. Before the recalibration, Moody's had a dual-class rating system. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality is to reach a weakened financial position that requires extraordinary support from a higher level of government to avoid default). Moody's Global Rating Scale, in contrast, measures expected losses (i.e., default probability and loss given default) among sovereign and corporate bonds. This dual-class rating system persisted for decades until Moody's recalibrated its Municipal Rating Scale to align it with the Global Rating Scale in April–May 2010. As a result, nearly 18,000 local governments

[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).

received ratings upgrades of up to three notches, corresponding to bonds worth more than $2.2 trillion in par value (nearly 70,000 bond issues).

Moody's recalibration allows us to isolate changes in economic outcomes that are caused by changes in public financing from those that would occur absent of these changes. An important aspect of the recalibration is that not all local governments were affected. Local governments that were already properly calibrated vis-à-vis the Global Rating Scale can be used as a control group. In addition, local governments without Moody's rating or outstanding bonds were by definition not subject to recalibration and can also be used as a control group.[2]

Important for our study, the upgrades that resulted from the recalibration reflected not changes to the issuers' intrinsic quality, but rather the goal of aligning municipal ratings standards with those of sovereign and corporate ratings. In fact, the recalibration algorithm used expected losses of each municipal rating by type of government (i.e., historical default rates by rating level and loss severity by type of government), and thus changes in ratings due to the recalibration are uncorrelated with changes to individual 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 both local government and county levels. Because our event potentially affects bonds issued by any local government unit within a county (i.e., the recalibration can affect bonds issued by counties, townships, municipalities, school districts, or special districts), we aggregate the changes in ratings at the county level.[3] Our treatment (continuous) variable is the fraction of local government units in each county whose outstanding bonds were upgraded because of the recalibration. The specifications include county-level

[2] Certain special districts in the housing and health-care sectors did not see a change in ratings because they were already well calibrated relative to 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).

control variables, as well as state-by-year fixed effects to capture any source of time-varying unobserved state-level heterogeneity, such as changes in transfers from state governments and ballooning of unfunded state pension liabilities. We also estimate specifications with county-size decile-by-year fixed effects to account for the possibility that large and small counties may have been affected differently by the Great Recession and the subsequent recovery, which might alter our results.

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.[4] We find that Moody's ratings increase 0.7 notches more for upgraded local governments than for non-upgraded local governments. We use S&P municipal bond ratings as a placebo test for the sample of bonds that have both Moody's and S&P ratings (about 55% of the bonds). If the recalibration by Moody's reflects changes in underlying credit quality, the S&P ratings on this sample of bonds would also be affected. We find no significantly different changes in the S&P ratings between the treatment and control groups.

We also find that upgraded local governments increase the amount of new bond issues significantly 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 about 20% 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 upgraded local governments decreases by 20–30 basis points relative to non-upgraded local governments. These findings are consistent with credit ratings playing an informational role in the municipal bond market. This may be due to the larger presence of retail investors relative to other fixed-income markets, which means that ratings are more likely to be used as a source of information.[5] The effects may also be the result of ratings-based regulations and internal policies

[4] We map the ratings into 22 numerical values, where 22 is the highest rating (Aaa) and 1 is the lowest (default).
[5] 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%

on institutional investors.[6] Our offer yield results are consistent with those in Cornaggia, Cornaggia, and Israelsen (2015), who use the Moody's ratings recalibration to study the effects of credit ratings on municipal bond prices. 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 in the primary market. Our study builds on the results in Cornaggia, Cornaggia, and Israelsen (2015) to study how a shock to local governments' financial constraints, caused by the ratings recalibration, affects the real economy in terms of government spending, employment, and income.

Consistent with local governments using the 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%–3% more than non-upgraded local governments following the recalibration. 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 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%–0.6%. We also find that the effects on private employment are heterogeneous across sectors and are concentrated in the non-tradable sector (retail, food, and accommodation), which depends primarily on local demand, as well as in the health-care and education sectors, which typically receive transfers or grants from state and

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by 2013, but households still have an important share of the municipal bond market. In contrast, households held only 19% of corporate and foreign bonds as of 2010.

[6] Beyond official regulations (e.g., Basel II and National Association of Insurance Commissioners (NAIC) guidelines), investment management policies and practices often rely on ratings by restricting the portfolio holdings of institutional investors (e.g., Chen et al. 2014). In the aftermath of the 2007–2009 financial crisis, several regulatory initiatives have been taken to reduce the mechanical reliance on credit ratings by market participants (the 2010 Dodd-Frank Wall Street Reform and Consumer Protection Act; and Financial Stability Board 2010, 2012).

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 0.4%–0.8%. This increase partly reflects a higher rate of firm entry in the non-tradable sector, consistent with the role of new firms in job creation, especially in response to local demand shocks (Haltiwanger, Jarmin, and Miranda 2013; Adelino, Ma, and Robinson 2016). The effect in the tradable sector is statistically insignificant. Last, we find that county-level income increases in response to the recalibration event. A one standard deviation increase in the fraction of upgraded local governments in a county increases income by 0.3%–0.7%.

The effect of the recalibration is heterogeneous across municipalities. The results are more pronounced in the sample of small and highly levered local governments, consistent with the recalibration playing a particularly important role for financially constrained local governments. We also find that results are stronger in counties with greater economic slack as proxied by the unemployment rate or the change in house prices. Our results are robust to alternative definitions of the treatment and control groups. We obtain consistent estimates when we restrict the control group to local governments with outstanding bonds, counties with multiple bond issuers, and counties located in urban areas.

Our study contributes to the literature that relies on 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; and Suarez-Serrato and Wingender 2014), which differs from the traditional empirical macroeconomics literature, where time series variation is employed (see Ramey 2011 for a survey). The long-standing debate on the effects of public spending on economic outcomes and the size of the fiscal multiplier has received additional attention due to the American Recovery and Reinvestment Act (ARRA) of 2009.

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

Using the ratings recalibration as an instrument for local government expenditures, we estimate that a marginal million dollars in local government expenditures results in an additional 48 jobs, 36 of which are outside the public sector. This estimate corresponds to a cost per job created of \$21,000 per year. 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 per year.

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, and fiscal multipliers should thus 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., Auerbach and Gorodnichenko 2012; and Fishback and Kachanovskaya 2015).[7]

We also contribute to the literature on the effect of credit market shocks on economic outcomes. Mian and Sufi (2011, 2014) and Mian, Sufi, and Rao (2013) focus on the role of household leverage in explaining the severity of the Great Recession in 2007–2009, and Giroud and Mueller (2016) focus on the role of firm leverage. Chodorow-Reich (2014) shows that firms with pre-crisis lending relationships with weaker banks face restrictions in credit supply and reductions in employment following the collapse of Lehman Brothers in 2008. Greenstone, Mas, and Nguyen (2014) and Bentolila et al. (2015) find that shocks to the supply of bank credit to

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

(small) businesses during the Great Recession are associated with reductions in employment.[8] Whereas 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.

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

1. Institutional Background and Data

1.1 Recalibration Event

Moody's had a dual-class rating system before the ratings recalibration in 2010. Moody's Municipal Rating Scale measured distance to distress (i.e., how likely a municipality was to reach a position that required support from a higher level of government to avoid default). In contrast, Moody's Global Rating Scale is designed to measure expected losses (default probability and loss given default) among sovereign bonds, corporate bonds, and structured finance products (Moody's 2007). Moody's (2009) attributes its dual-class rating system to the preferences of the highly risk-averse investors in municipal bonds. According to the Flow of Funds Accounts in 2010, households owned 50% of municipal bonds, followed by money market funds with 10% and insurance companies with 9%. In contrast, households held only 19% of corporate and foreign bonds.

Moody's dual-class rating system produced lower ratings for municipal bonds relative to its competitors. In our sample, Moody's assigned a rating lower than S&P in 53% of the issues (and the same rating in 40% of the issues) in the year before the recalibration; this number drops to

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

only 17% in the year after the recalibration (reflecting the upgrades). In addition, Moody's (2007) shows that default rates in municipal bonds are significantly lower than those experienced by comparable corporate bonds. Because of the more conservative ratings under the dual-class system, Moody's share in the municipal bond market declined, as did its dominant role in the marketplace. In the year before the recalibration, Moody's had a market share of 34%, compared with S&P's share of 59% and Fitch's share of 7%. After the recalibration, Moody's market share increased to more than 40% (2010–2012).[9]

Moody's maintained a sizable market share despite this apparent competitive disadvantage under the dual-class system likely because many regulations (e.g., Basel II and National Association of Insurance Commissioners (NAIC) guidelines) and investment rules require at least two ratings from a nationally recognized statistical rating organization (NRSRO), and use the lower of two ratings, or the middle of three ratings, as the basis for regulatory benchmarks (e.g., banks' capital requirement).[10] Beyond regulations, local governments' debt management policies and institutional investors' policies often require two ratings. For example, the County of Alameda (2014), California, debt management policy stipulates that "at least two credit ratings should be procured from any of the nationally recognized credit rating services, unless the transaction is of a small size."[11] The Government Finance Officers Association (2015) (GFOA) also writes that "historically, many issuers have sought separate ratings from at least two credit rating agencies. In addition, many institutional investors require a minimum of two ratings." Market participants also emphasize the importance of two ratings. Timothy Cox, executive director of debt capital markets at Mizuho Securities, said in an interview with Bloomberg (2011): "If I don't have two ratings on a bond, I cannot sell it. No money manager is

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

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

[11] 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."

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 2006 (Moody's 2006). It planned to implement the ratings recalibration in June and July 2008, but the financial market turmoil during the summer and fall of 2008 led to a postponement. Finally, in March 2010, Moody's announced a recalibration of its Municipal Rating Scale to align it with the Global Rating Scale (Moody's 2010). In April and May 2010, over a four-week period, Moody's announced how individual bonds would be affected by the recalibration, resulting in a zero-to-three-notch upgrade of nearly 70,000 bond ratings.

Moody's recalibration algorithm used the expected losses of each municipal rating by type of government (i.e., historical default rates by rating level and loss severity by government type) to map to its equivalent rating on the global scale. An important aspect of this recalibration is that not all municipal bond issues were upgraded and therefore can be used as control group. Some local governments were already properly calibrated vis-à-vis the global scale, in particular special districts related to housing and health-care did not see a change in ratings. In addition, bonds with higher ratings (at or above Aa3) on the municipal scale were less likely to be recalibrated than those with a lower rating (below Aa3); bonds with the maximum attainable rating (Aaa) in the municipal scale could, by definition, not be upgraded. Of course, local governments without bonds rated by Moody's 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 before recalibration would remain under review and would not be lumped into these rating changes. Thus, our sample does not include any natural upgrades associated with improving issuer fundamentals that could contaminate our results. In addition, because the methodology closely follows a discussion that occurred (and was made public) over a period of several years, it is especially unlikely that the rating changes could include information about individual local governments.

1.2 Data

We obtain a list of recalibrated bond issues from Moody's. The list contains the rating of each bond issue before and after the recalibration, with the change in rating ranging from zero to three notches. The recalibration comprised 69,657 municipal bonds (with a total par amount of $2.2 trillion). Almost all the bonds had an investment-grade rating before the recalibration (only 56 municipal bonds had a speculative-grade rating).

The municipal bond market data come from the Ipreo i-Deal new issues database. The sample period is from April 2007 to March 2013, which corresponds to the three-year period before the recalibration and the three-year period afterward. We restrict the sample to new bond issues rated by Moody's and local governments that issued bonds during the three-year period before the recalibration.[12]

Because 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

[12] 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.

(including towns and villages), school districts, and special districts. We exclude state-level bonds because they cannot be linked to a specific county. Because credit ratings on insured bonds reflect the credit quality of the *insurer* rather than the *issuer*, we include only uninsured bonds in our analysis (roughly 60% of bonds). 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 comprises 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 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 local government unit present in all years.[13]

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

[13] 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.

calendar year (the sample period for income is 2006–2012).

We also present results using data on quarterly employment from the Census Bureau's Quarterly Workforce Indicators (QWI). The QWI is derived from the Longitudinal Employer-Household Dynamics (LEHD) program at the Census Bureau. 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, defined as one or more people who occupy a given housing unit, from the 2007 Census Bureau Summary Files.

1.3 Summary Statistics

Panel A of Table 1 presents summary statistics 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 (April 2007–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.

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

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 of Aa3, confirming that Moody's municipal bond ratings were more conservative than S&P ratings before the recalibration.

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 county in the sample has local government 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 based on two-digit NAICS codes. Average employment in the tradable sector (manufacturing; NAICS codes 31–33) is 3,400 employees, while average employment in the non-tradable sector (retail and restaurant; NAICS codes 44–45 and 72) is about 8,600 employees.

The final row of Table 1 presents summary statistics on the main explanatory variable (*Recalibrated*) at the county level. We first define the treatment and control groups at the local government level. The treatment group contains local governments whose outstanding bonds were upgraded by at least one notch during the Moody's recalibration event. We then calculate our treatment (continuous) variable as the fraction of all 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. Panels B1 and B2 provide 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 equal to zero−control counties). Because the median of *Recalibrated* is zero, recalibrated counties correspond to the counties with above-median *Recalibrated*, whereas non-recalibrated counties correspond to the counties with below (or at)-median *Recalibrated*. The sample in Panel B1 consists of all counties in the United States.[15] The sample in Panel B2 consists of counties in which at least one local government issued bonds in the municipal bond market in the three-year period before the recalibration (April 2007–March 2010). In the case of the sample of bond issuers, the local governments in the control group either do not have a rating from Moody's (i.e., they were rated only by S&P or Fitch) or have a Moody's rating that was not affected in the recalibration.

Panel B1 shows that one feature of the data is that counties in the treatment group are larger than counties in the control group. The average number of households is 82,000 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 and control groups, respectively. This indicates that smaller counties issue bonds less frequently and/or are less likely to have a Moody's rating. We present both raw differences in means between treatment and control groups, as well as differences after adjusting for state-by-year and county-size decile-by-year fixed effects. This adjustment controls for regional and size heterogeneity in a given year between treatment and control groups, and the differences are no longer statistically (or economically) significant. We use this strategy in the regression tests to ensure comparability of treatment and control groups along the regional and size dimensions (as well as to absorb any time-varying factors that could affect regions or counties of distinct sizes differently). Panel B1 also shows that the treatment and control groups have similar economic structures in terms of relative importance of local government versus private employment. Additionally, the growth rates (annual log change) of outcome variables in the pre-treatment period are similar across the

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

two groups, except for government expenditures. This difference becomes smaller in Panel B2, when we make the comparisons only within counties with at least one bond issuer in the April 2007–March 2010 period. The remaining statistics in Panel B2 using the sample of bond issuers present qualitatively similar differences between treatment and control groups.

2. Effect on Municipal Bond Market

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

We first estimate the effects of the recalibration on ratings aggregated to the county level because our private-sector employment and income measures are measured only at the county level. We aggregate the new bond issues data using the average rating across all issues in each county and event year. The sample is restricted to counties in which at least one local government issued bonds in the municipal bond market in the three-year period before the recalibration (April 2007–March 2010). We estimate the following (reduced form) regression:

$$Rating_{it} = \beta_1 Recalibrated_i \times Post + \beta_2 X_{i,t} + \eta_i + \eta_{state,t} + \eta_{size,t} + \varepsilon_{it} \tag{1}$$

The analysis is conducted within-county—that is, we include county fixed effects (η_i) in all regressions. The regressions include state-by-year fixed effects ($\eta_{state,t}$), as well as county-size decile-by-year fixed effects in some specifications ($\eta_{size,t}$). The explanatory variable of interest is the fraction of upgraded local governments in a county (*Recalibrated*) interacted with a dummy variable that takes a value of one for April 2010–March 2013 (*Post*). Both the *Recalibrated* and the *Post* dummy variables are absorbed by the fixed effects (county fixed effects and the state-by-year 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.[16] Standard errors are clustered at the county level to correct for within-county residual correlation.

Panel A of Table 3 presents the results of county-level regressions of the effects on ratings. Column (1) presents the results in which the dependent variable is the Moody's rating. We find that the coefficient of the interaction term *Recalibrated* × *Post* is positive and significant, and indicates that the recalibration has a disproportional effect of 0.3 notches on the ratings of the treatment group relative to the control group for a one standard deviation increase in the *Recalibrated* variable. Column (2) shows a similar differential effect on ratings when we include county-size decile-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. We can use the S&P credit ratings as a placebo test, because S&P does not have a dual-class rating system. Specifically, this allows us to test whether the effect on Moody's ratings can be the result of information about the creditworthiness of the upgraded municipalities (in which case S&P ratings should also react). Columns (3) and (4) of Table 3 show no significant differential effect on S&P ratings between the treatment and control groups following the recalibration. Although the exclusion restriction is not directly testable, we view this finding as an important validation of the identification strategy.

Columns (5) and (6) 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 (3) and (4)). We find that the interaction term *Recalibrated* × *Post* coefficient remains positive and significant in this sample, though somewhat smaller in magnitude.

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

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 sample is restricted to local governments that issued bonds in the three-year period before the recalibration. We estimate the following (reduced form) regression:

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

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

The local-government-level results in Panel B are similar to the county-level results in Panel A. We find that the coefficient of the interaction term *Recalibrated Dummy* × *Post* is positive and significant, which indicates that the recalibration has a disproportional effect of about 0.7 notches on the ratings of the treatment group relative to the control group.

Figure 2 shows the effect of the recalibration on Moody's ratings for the treatment and control groups from two years before the recalibration to two years after it. Treatment and control groups show no significant differential changes in the two years before the recalibration. The treatment group undergoes a significantly larger increase in ratings at the time of the recalibration, a difference that persists for up to two years. 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 effects are not related to channels other than ratings.

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

Panel A of Table 4 presents the results of county-level regressions of the effects on the municipal bond primary market. We run regressions like those in equation (1), where the dependent variables are the amount of bonds issued and the average offer yield instead of the average rating. Columns (1) and (2) present results in which the dependent variable in the regression is the logarithm of the total amount of bonds issued by local governments (in millions of dollars) in each county and event year (*Issue Amount*). Columns (3) and (4) present results in which the dependent variable is the average of offer yields (in percentage) in each county and event year (*Offer Yield*). In column (1), the interaction term *Recalibrated* × *Post* coefficient is positive and significant, and indicates that the recalibration has a disproportional effect of 8% on the *Issue Amount* of the treatment group relative to the control group for a one standard deviation increase in the *Recalibrated* variable. Column (2) shows a similar differential effect on issue amounts when we include county-size decile-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 columns (1)-(2) of Table 4, the differential effect in the *Offer Yield* corresponds to a 15−19 basis point reduction for a one standard deviation increase in the *Recalibrated* variable.

Panel B in Table 4 presents the results of the regression of the logarithm of the *Issue Amount* (columns (1) and (2)) and *Offer Yield* (columns (3) and (4)) at the local government level (similar to equation (2)). The dependent variables are the logarithm of the total amount of bonds issued and the average offer yield across all issues of a given local government in each event year. The local-government-level results in Panel B are similar to the county-level results in Panel A. The treatment group has a large and statistically significant increase in the *Issue Amount* following the recalibration. The treatment group increases the *Issue Amount* after the recalibration 19%–23% more than the control group.

We also find that the *Offer Yield* of the treatment group decrease significantly more than that of the control group, at the local government level, following the recalibration. The estimated reduction in the *Offer Yield* is 20–30 basis points. The magnitude of the differential effect on offer yields is similar to that in Cornaggia, Cornaggia, and Israelsen (2015).

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 no evidence of significant pre-existing differential trends between treatment and control groups. We then see a significantly higher *Issue Amount* in the year of the recalibration and in subsequent years for the treatment group versus the control group.

We perform several robustness checks of the effects on the *Issue Amount* and *Offer Yield* variables in Table 4. These robustness checks are shown in the Internet Appendix. Table IA.3 presents the results of regressions of the logarithm of the *Issue Amount* and *Offer Yield* at the bond issue level. The bond-issue-level results are qualitatively similar to the county-level and local-government-level results in Table 4. We also estimate the issue-level and local-government-level regressions using: (1) a sample period with only two years before and two years after the recalibration (i.e., the sample period is April 2008–March 2012); (2) a sample of issues with both S&P and Moody's ratings; (3) a sample of issues excluding BAB; (4) a sample of all local governments, including those that have not issued bonds in the pre-treatment period; (5) a sample with both uninsured and insured bonds. These robustness checks are shown in Table IA.4 in the Internet Appendix. In particular, we find similar effects on the *Issue Amount* and *Offer Yield* in the sample of issues with both S&P and Moody's ratings in columns (3) and (4). In this sample, the information channel is likely to be less important because investors have access to S&P ratings on the same bonds. The regulatory channel thus seems to play 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).[17] Figure IA.1 in the Internet

[17] To 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

Appendix shows the effect of the recalibration on the average offer yield of the treatment and control groups.

We also explore whether the magnitude of the effect on the *Issue Amount* and *Offer Yield* is different according to the magnitude of the upgrade. Table IA.5 in the Internet Appendix shows that the effect is generally larger in magnitude for upgrades of two or three notches relative to those of one notch, although the differences are not always statistically significant.

Last, Table IA.6 in the Internet Appendix shows that the effects on the *Issue Amount* and *Offer Yield* are similar when the county-level *Recalibrated* variable is calculated using the amount of bonds issued by each local government during the pre-recalibration period.

3. Effect on Local Government Expenditures and Employment

In this section, we study the effect of the ratings recalibration on local government spending and employment. The evidence in Section 2 shows a reduction in the offer yield of approximately 20–30 basis points, which implies a saving in local governments' interest expenses. However, the potential increase in government expenditures may not be limited to the savings in interest expenses. A reduction in the cost of funding can make potentially large projects net present value (NPV) positive, and lead to a large increase in spending. At the same time, it is also possible that local governments over-react to the reduction in cost of funding and spend more than a pure NPV rule might justify, so it is unclear what size of spending increase to expect given this reduction in the cost of funding.

To estimate the impact of the ratings recalibration on local government outcomes, we first estimate difference-in-differences regressions at the county level of local government expenditures and employment. We estimate panel regressions using the logarithm of the outcome in each county and year as the dependent variable and the specification in equation (1). In these

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.

tests, the explanatory variable of interest is the interaction *Recalibrated* × *Post*. *Post* takes a value of one in 2011, 2012, and 2013, because the fiscal year ends in June 30 (just one month after the recalibration in 2010) for local government expenditures, and employment in the Census of Government is measured as of the week of March 12 of each year (just before the recalibration in 2010). The regressions include county-level controls, county fixed effects, state-by-year fixed effects, and, in some specifications, county-size-group-by-year fixed effects. Standard errors are clustered at the county level.

The regressions consider two alternative sample periods: 2007–2013 and 2009–2012. We also consider two samples of counties. The first sample includes all counties in the United States with available government expenditures data regardless of whether they issue bonds or have bonds with a rating from Moody's. The second sample is restricted to counties in which at least one local government issued bonds in the three-year period leading up to the recalibration (April 2010–March 2013). This sample of bond issuers uses a different control group by excluding counties without any bond-issuing entities.

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

3.1 Local Government Expenditures

We test whether the positive shock to the supply of municipal bond financing affected government expenditures and employment in the aftermath of the 2007–2009 recession. Table 5 presents the results of difference-in-differences regressions using the logarithm of local government expenditures as the dependent variable. Panel A shows results at the county level, and Panel B shows results at the local government level. Columns (1) and (2) present the results using the 2007–2013 period, and columns (3) and (4) present the results using the 2009–2012 period.[18]

The county-level estimates indicate that the recalibration is associated with an increase in local government expenditures of 3%–12%.[19] 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 0.7% using the estimate in column (3), Table 5, Panel A.

The local-government-level estimates in Panel B are consistent with those in Panel A using county-level data. In column (1) of Table 5, Panel B, the differential increase in local government expenditures is 2.3% when we include local government type-by-year fixed effects and county-by-year fixed effects, and it is stronger at 3.4% when we include local government type-by-county-by-year fixed effects in column (2). Notice that in this specification we compare expenditures before and after the recalibration for the *same type* of local government within the same county and year, eliminating many alternative explanations for the observed effects. The corresponding estimates in columns (3) and (4) are smaller in magnitude at 1.0%–1.8%, but still statistically significant. When we use the sample to bond issuers in columns (5)-(8), the effects are slightly stronger at 2.1%–3.3%, and statistically significant.

Figure 4 shows the evolution of local government expenditures before and after the ratings recalibration for the treatment and control groups to account for the possibility of pre-existing

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

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

trends. The two groups follow similar trends before the recalibration, although the difference between the two groups becomes smaller right before the recalibration event. This suggests that there may have been some anticipation of the recalibration on the part of governments (who may have limited some expenses before the shock). Expenditures then increase for the treatment group in the year of the recalibration, whereas they continue their negative trend for the control group.

Tables IA.7 and IA.8 in the Internet Appendix show additional results for local government expenditures and revenues. Table IA.7 shows separate results for county-level current expenditures and capital outlays. Current expenditures represent on average about 80% of local government total expenditures, whereas capital outlays represent about 20%. We find positive effects in both components of total expenditures. The recalibration is associated with a positive and significant increase in current expenditures, in line with estimates for total expenditures in Panel A of Table 5. In addition, there is a large effect for capital outlays, but the estimates are imprecisely estimated (and we cannot reject the hypothesis of no effects on capital outlays).

Table IA.8 shows the evolution of local government taxes (mostly property taxes) between upgraded and non-upgraded counties. We find negative and significant effects in local government taxes due to the recalibration in some specifications. This suggests that local governments used the proceeds from municipal bond issues to both alleviate spending cuts (or increase spending), and to prevent contemporaneous tax and fee increases (or reduce taxes). Figure IA.2 shows the evolution of local government taxes before and after the ratings recalibration for the treatment and control groups. The two groups follow similar trends before the recalibration. Taxes then decrease for the treatment group relative to the control group in the year of the recalibration.

3.2 Local Government Employment

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

Panel A of Table 6 shows the results of county-level regressions. We find that the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at between 4% and 9%. A one standard deviation increase in the fraction of upgraded local governments in a county increases local government employment at the county level by about 0.54% (using the estimate in column (3), Table 6, Panel A).

Panel B of Table 6 shows the results of local-government-level regressions. We find increases of 2% in employment for the treatment group relative to the control group. When we limit the sample to bond issuers, the effects range from 0.2% to 1.9% (statistically insignificant).[20]

Figure 5 shows the evolution of government employment before and after the ratings recalibration for the treatment and control groups to account for the possibility of pre-existing trends. The two groups follow similar trends before the recalibration. Further, government employment increases for the treatment group in the year of the recalibration, and the gap continues to expand in the subsequent years.

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 regressions of private employment and income. We use county-level employment because of potential for spillovers across smaller geographic units. Although an upgraded local government can hire (and thus we can measure its employment creation), it is unlikely that the local private-sector effects would be limited to a small area such as a ZIP code. We use counties as a compromise between even larger units (e.g., metropolitan

[20] 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. Table 6 is thus restricted to local government units present in all years 2007–2013. In unreported regressions we use the growth rate in local government employment from 2007 to 2012 using all counties in the United States. We find that the interaction term *Recalibrated* × *Post* coefficient is 0.06 and significant at the 1% level, within the range of estimates in Panel B of Table 6.

statistical areas) and smaller ones (e.g., ZIP codes or census tracts). We estimate panel regressions using the logarithm of employment or income in each county and year as the dependent variables. The specifications are equivalent to those in Panel A of Tables 5 and 6 for local government outcomes at the county level (akin to equation (1)).[21]

4.1 Private Employment

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

Table 7 presents the results of difference-in-differences regressions using the logarithm of private employment as the dependent variable. Panel A shows results using the sample of all counties, and Panel B shows results using the sample of counties in which at least one local government issued bonds in the three-year period before the recalibration.

In column (1), the interaction term *Recalibrated* × *Post* coefficient is 7.1%, significant at the 1% level, when we include state-by-year fixed effects, which controls for time-varying regional economic shocks. The differential increase in private employment when we include county-size decile-by-year fixed effects is 5.4% (column (2)). The corresponding results in columns (3) and (4) where a shorter event window is used are smaller in magnitude at 3.2%–3.5%, but still statistically significant. The results indicate that a one standard deviation increase in the fraction of upgraded local governments in a county increases private employment by 0.3% (using the estimate in column (3), Table 7, Panel A). The estimates in Panel B using the sample of bond issuers are generally smaller but still statistically significant.

Figure 6 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.

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

Private employment increases for the treatment group in the year of the recalibration, and the gap keeps expanding in the subsequent years, consistent with the multiplier effect of government spending taking some time to play out fully. Figure 7 shows the evolution of private employment before and after the recalibration for the treatment and control groups using quarterly data. The evolution of private employment at the quarterly frequency is consistent with that of the annual frequency in Figure 6.

4.2 Private Employment by Sectors

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

The results in columns (1) and (2) for tradable sector employment (manufacturing; NAICS codes 31–33) are statistically insignificant, consistent with this sector's dependence on non-local, more dispersed demand. Columns (3) and (4) present the results for the non-tradable sector employment (retail and restaurants; NAICS codes 44–45 and 72), which is more dependent on local demand (Mian and Sufi 2014; Adelino, Ma, and Robinson 2016). In column (3), the interaction term *Recalibrated* × *Post* coefficient is 0.10, significant at the 1% level, when we include state-by-year fixed effects in the regression. The interaction term coefficient in column (4) is 0.055, significant at the 10% level, when we include county-size decile-by-year fixed effects in the regression. These results imply that a one standard deviation increase in the fraction of upgraded local governments in a county increases non-tradable employment by 0.4%–0.8%. Panel B shows similar estimates using the sample of bond issuers.

Government spending is more likely to occur in such sectors as construction, education, and health-care. Table 8 also presents difference-in-differences results for employment in the construction sector (columns (5) and (6)), health-care sector (columns (7) and (8)), and education sector (columns (9) and (10)).

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

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

Table IA.9 in the Internet Appendix shows that the increase in employment has a significant effect on employment in new firms in the non-tradable sector (firms less than two years old), which is consistent with the role of startups on the net creation of employment (Haltiwanger, Jarmin, and Miranda 2013; Adelino, Ma, and Robinson 2016). A one standard deviation increase in the fraction of upgraded local governments in a county is associated with a 1.0%–1.7% increase in non-tradable employment in new firms. Figure IA.4 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.

4.3 Income

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

Table 9 presents the results of regressions that are equivalent to those in Table 7 for private employment. In column (1), the interaction term *Recalibrated* × *Post* coefficient is 0.09, significant at the 1% level, when we include state-by-year fixed effects in the regression. In column (2), the differential increase is 0.05, significant at the 1% level, when we include county-size decile-by-year fixed effects in the regression. The corresponding results in columns (3) and (4) where a shorter event window is used are smaller in magnitude at 3.5%–6.5%, but still statistically significant. A one standard deviation increase in the fraction of upgraded local governments in a county increases income by 0.5% using the estimate in column (3), Table 9, Panel A. Panel B shows estimates of similar magnitude using the sample of bond issuers with a range of 3%–6% and statistically significant.

Figure 8 shows the evolution of income in the two-year periods before and after the recalibration for the treatment and control groups. The income processes of the two groups follow similar trends before the recalibration, and a gap emerges in 2010. In the subsequent two-year period, the gap persists and the income processes again follow similar dynamics.

Table IA.10 in the Internet Appendix shows the results of specifications that include separate time trends for the counties in the treatment and control groups. The dependent variables are the logarithm of government expenditures, government employment, private employment, and income. This specification allows treatment and control groups to follow different linear trends, and thus mitigates concerns about pre-existing differential trends. The estimates are smaller in magnitude but remain statistically significant.

5. Fiscal Multipliers

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

We use instrumental variables methods to estimate the fiscal multipliers. We instrument for local government expenditures at the county level using the exogenous variation due to Moody's 2010 recalibration. The instrument is the interaction variable *Recalibrated* × *Post*. We estimate the effect of government spending on government employment, private employment, and income using two-stage least squares in the 2009–2012 county-year panel with county, state-year, and, in some specifications, county-size decile-year fixed effects.

The first-stage regressions are similar to columns (3) and (4) of Panel A of Table 5 and are reproduced in Panel A of Table 10. The sample of counties in each regression is constrained by data availability for each outcome variable (i.e., government employment, private employment, and income data). The variable *Recalibrated* × *Post* is positive and significant in all regressions. *F*-statistics are above 10 for the private employment and income regressions in columns (3) and (5) but are smaller for government employment and when we include county size-by-year fixed effects.

Panel B of Table 10 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 expenditures evaluated at the mean of the data. Following the literature, we estimate the multipliers using the increase in government spending instead of the increase in bond financing.[22]

The creation of local government jobs is calculated as the product of the estimate in column (1) of Table 10, 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.

[22] The additional government spending after the recalibration, at the county level, can be calculated as the product of the estimate in column (5) of Table 10, Panel A, 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 (1) of Table 4, Panel A, of 20.9% by the average amount of bonds issued of \$180 million by county).

The elasticity in column (3) of Table 10 can be translated into the corresponding increase in private sector jobs by multiplying it by the ratio of private employment to government spending. The results indicate that a marginal million dollars in local government spending results in 36 jobs (= 0.467 × 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 (5) 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.

We obtain similar estimates for government employment, private employment, and income in columns (2), (4), and (6), respectively, when we include county-size decile-by-year fixed effects in the regressions.

Although we use a different setting, our estimates are similar to those in the recent literature that exploit cross-sectional (geographic) variation. Cohen, Coval, and Malloy (2011) use changes in congressional committee chairmanships as a source of variation in state-level federal expenditures and find that public spending crowds out private sector investment over a long period of time. Suarez-Serrato and Wingender (2014) exploit variation in federal spending directed to counties due to changes in the local population count after each decennial census and estimate a local income multiplier of 1.57 and a cost per job of $30,000. Shoag (2015) uses differences in returns to state pension funds as windfall shocks to state finances that are predictive of subsequent spending patterns, and estimates a state-level spending multiplier of 2.1 and a cost per job of $35,000. Nakamura and Steinsson (2014) use regional variation in U.S. military spending and estimate a state-level multiplier of 1.5, although they find larger multipliers during high slack periods.[23] Chodorow-Reich et al. (2012) use pre-crisis state-level

[23] 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

Medicaid spending to extract the exogenous component of state fiscal relief during the 2009 American Recovery and Reinvestment Act and estimate a cost per job of $26,000.[24]

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 (2014) 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 coincides with the national multiplier only when nominal interest rates are unresponsive.[25]

Our multipliers are based on deficit-financed subnational government spending, which tend to be lower than multipliers based on windfall-financed (federal) government spending if private consumption and investment are crowded out. However, this crowding-out effect is likely to have been muted by the low-interest-rate environment during our sample period. In addition, in a neoclassical model, output multipliers based on deficit-financed spending could be larger than multipliers based on windfall spending, because households increase labor supply and hence output as they recognize that increased government spending requires increased future taxes.[26]

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facilities in California by comparing housing prices in school districts where referenda on municipal bond issues passed or failed by narrow margins.

[24] 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.

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

[26] 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 lower and upper end of the range in this literature, respectively. 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.[27]

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 three and five. 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%. Further, the ratings recalibration took place when state and local governments were facing severe financial constraints from the 2007–2009 recession.[28] Our estimates of the fiscal multiplier are also consistent with work on state-dependent multipliers that finds higher multipliers during depressed economic conditions such as the one that prevailed during our sample period (e.g., Auerbach and Gorodnichenko 2012; and Fishback and Kachanovskaya 2015).

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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.

[27] 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.

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

6. Effect Heterogeneity

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

6.1 Economic Slack

We investigate whether the effects of government spending on the local economy are larger in counties with greater economic slack. Specifically, we estimate panel regressions like those in Tables 5, 6, 7 and 9, and include a triple interaction term, *Recalibrated* × *Post* × *High Slack*, where *High Slack* is a dummy variable that takes a value of one in regions with more economic slack. The coefficient on the triple interaction term measures the differential effect between counties of high and low economic slack. We define low-slack and high-slack regions based on (pre-treatment) unemployment rate below or above the median, and based on (2007-2009) change in house prices below or above the median.

Table 11 presents the results using the logarithm of government expenditures, government employment, private employment, and income as dependent variables. We use specifications with county and size decile-by-year fixed effects (we do not include state-by-year fixed effects because much of the variation in the response to the economic slack variables occurs at the state level). Panels A and B present the results using slack defined by unemployment and real estate prices, respectively.

Panel A shows that our effect is driven by those counties with high unemployment (the first measure of economic slack) because 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 with lower economic according to the unemployment). Panel B shows similar results for private employment and income, but the estimates for the triple interaction are not significant for local government expenditures and employment, suggesting that the government response was not significantly different across counties with high- and low-house-price changes. Columns (2), (4), (6), and (8) present similar estimates when we include county-size decile-by-year fixed effects. Taken together, the results support the idea that the multiplier effects of government spending are larger when local economies have greater economic slack.

6.2 Local Government Financial Constraints

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

Panel A of Table 12 presents results for the sample split into small and large local governments based on number of employees (as of 2007) below or above the median. Small local governments are more likely to be financially constrained than large local governments. Panel B presents results for the sample split into low-leverage and high-leverage local governments based on the ratio of total debt-to-revenues (as of 2007) below or above the median. We expect high-leverage local governments to be more constrained in raising additional debt than low-leverage local governments.

We find that the effect of the recalibration on expenditures is more pronounced among small and high-leverage local governments. We conclude that the recalibration had a more pronounced effect on financially constrained local governments. The ratings upgrade due to the recalibration allowed constrained local governments to issue more debt and increase expenditures.

6.3 County size

As a final set of tests, we consider how the recalibration affects the main outcomes of interest in different types of counties. Specifically, we focus on the sample of urban counties (those counties with more than 50,000 people following the Census Bureau definition) and on the sample of multiple issuers (those counties with two or more issuers of municipal bonds in the period before the recalibration).

Table 13 presents the results. Panel A presents the results for the sample of urban counties, and Panel B presents the results for the sample of multiple issuers. We find that the point estimates are generally in line with those in the main tables across all the specifications. This analysis further mitigates the concern that the comparison of counties that differ along unobservable characteristics correlated with county size may explain our findings.

7. Conclusion

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

This upgrades lead to increases in local government expenditures and employment. There are also positive spillover effects to the private sector as local governments obtain easier 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 expenditures. The private employment increase is concentrated in the non-tradable sector, which is more directly dependent on local demand.

We show that increases in the supply of financing to local governments can have important effects on the local economy. The effects are driven specifically by changes in ratings of municipal bonds, and not by changes in local or nationwide fundamentals. Our findings are consistent with the New Keynesian view of the economy in which aggregate demand shocks, such as government spending shocks, have large output multipliers when the economy is in a liquidity trap. Specifically, our findings suggest that debt-financed increases in government spending can improve economic conditions during periods of factor underutilization and near-zero interest rates, such as those observed in many countries in recent years.

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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 Ipreo i-Deal municipal new bond issues in the April 2007–March 2013 period. The sample in Panel B consists of counties in the 2007–2013 period.

	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							
Households (thousand)	33.94	9.57	104.62	0.14	3,133.77	21,647	3,117
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
Tradable Employment (thousand)	3.41	0.45	13.78	0.00	417.55	11,249	2,033
Non-Tradable Employment (thousand)	8.56	1.71	27.72	0.00	778.39	21,797	3,115
Construction Employment (thousand)	1.89	0.31	6.61	0.00	171.09	21,750	3,116
Health-Care Employment (thousand)	5.65	1.01	18.69	0.00	504.70	21,689	3,103
Education Employment (thousand)	1.15	0.00	5.83	0.00	140.50	17,796	2,725
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 (raw and adjusted) in means between treatment and control groups. The sample in Panel A consists of Ipreo i-Deal municipal new bond issues in the April 2007–March 2010 period. The sample in Panel B1 consists of counties in the 2007–2009 period. The sample in Panel B2 consists of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). Panel A shows the difference in means adjusted by county-year fixed effects. Panels B1 and B2 show the difference in means adjusted by state-year fixed effects and county-size decile-year fixed effects. Robust standard errors clustered at the local government level (Panel A) and county level (Panels B1 and B2) and are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Issue-Level Variables

	Mean				Number of Observations	
	Recalibrated Dummy = 1	Recalibrated Dummy = 0	Difference (raw)	Difference (adjusted)	Recalibrated Dummy = 1	Recalibrated Dummy = 0
Issue Amount ($ million)	4.75	6.07	-1.33	-2.22*	67,268	23,860
Offer Yield (%)	3.46	3.42	0.04	0.06	67,268	23,860
Moody's Ratings	17.96	18.72	-0.761***	-0.95***	67,268	23,860
S&P Ratings	19.04	19.41	-0.368***	-0.36**	40,949	11,189

Panel B1: County-Level Variables

	Mean		Difference	Difference	Number of Observations	
	Recalibrated > 0	Recalibrated = 0	(raw)	(adjusted)	Recalibrated > 0	Recalibrated = 0
Households (thousands)	81.68	12.39	69.29***	4.10	2,895	6,448
Private Employment (thousands)	96.73	10.65	86.08***	7.32	2,898	6,379
Fraction of Local Government Employment	0.059	0.061	-0.001	0.000	2,298	2,522
Local Government Expenditures Growth	0.054	0.044	0.010***	0.011***	1,904	4,020
Local Government Employment Growth	0.011	0.006	0.005	0.011*	1,542	1,683
Local Private Employment Growth	-0.026	-0.028	0.002	-0.001	1,933	4,221
Income Growth	-0.008	-0.009	0.001	0.001	1,930	4,296

Panel B2: County-Level Variables - Sample of Bond Issuers

	Mean		Difference	Difference	Number of Observations	
	Recalibrated > 0	Recalibrated = 0	(raw)	(adjusted)	Recalibrated > 0	Recalibrated = 0
Households (thousands)	81.68	19.45	62.23***	5.28	2,895	2,436
Private Employment (thousands)	96.73	18.62	78.37***	6.45	2,898	2,424
Fraction of Local Government Employment	0.059	0.052	0.007***	0.005*	2,298	1,184
Local Government Expenditures Growth	0.054	0.050	0.005	0.008*	1,904	1,594
Local Government Employment Growth	0.011	0.019	-0.008*	0.000	1,542	790
Local Private Employment Growth	-0.026	-0.029	0.003*	0.000	1,933	1,612
Income Growth	-0.008	-0.009	-0.001	0.000	1,930	1,624

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 around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the average rating across all issues of local governments of each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the average rating across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P		Rating Moody's (sample S&P)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: County Level						
Recalibrated × Post	0.737***	0.770***	0.058	0.094	0.575***	0.419*
	(0.164)	(0.170)	(0.203)	(0.209)	(0.191)	(0.248)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.36	0.37	0.25	0.29	0.17	0.39
Number of Observations	4,216	4,216	1,902	1,902	1,902	1,902
Number of Counties	1,144	1,144	555	555	555	555
Panel B: Local Government Level						
Recalibrated Dummy × Post	0.699***	0.647***	-0.110	-0.009	0.654***	0.527***
	(0.065)	(0.069)	(0.191)	(0.207)	(0.100)	(0.099)
Year Fixed Effects	Yes	No	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.27	0.52	0.01	0.31	0.23	0.48
Number of Observations	10,061	10,061	4,211	4,211	4,211	4,211
Number of Local Governments	4,335	4,335	1,660	1,660	1,660	1,660

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

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of local governments in each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Panel A: County Level				
Recalibrated × Post	0.209**	0.196*	-0.360***	-0.470***
	(0.096)	(0.101)	(0.114)	(0.118)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.16	0.18	0.39	0.41
Number of Observations	5,504	5,504	5,504	5,504
Number of Counties	1,780	1,780	1,780	1,780
Panel B: Local Government Level				
Recalibrated Dummy × Post	0.188***	0.233***	-0.308***	-0.196**
	(0.059)	(0.069)	(0.077)	(0.078)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.33	0.35	0.57
Number of Observations	10,061	10,061	10,061	10,061
Number of Local Governments	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 (as of July of each year) around the Moody's recalibration in April–May 2010. Panel A presents county-level results using expenditures across all local governments in each county and year. The sample in Panel A, columns (1)–(4), consists of all counties. The sample in Panel A, columns (5)–(8), consists of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results. The sample in Panel B, columns (1)–(4), consists of all local government units. The sample in Panel B, columns (5)–(8), consists of local governments with at least one bond issued in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007–2013		Panel 2009–2012		Panel 2007–2013		Panel 2009–2012	
Panel A: County Level	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated × Post	0.121***	0.080***	0.082***	0.052**	0.062**	0.047*	0.054**	0.030
	(0.025)	(0.027)	(0.024)	(0.026)	(0.029)	(0.028)	(0.026)	(0.026)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.32	0.33	0.11	0.12	0.39	0.39	0.12	0.13
Number of Observations	20,727	20,727	11,844	11,844	12,222	12,222	6,984	6,984
Number of Counties	2,961	2,961	2,961	2,961	1,746	1,746	1,746	1,746
Panel B: Local Government Level	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated Dummy × Post	0.023***	0.034***	0.010*	0.018***	0.021**	0.023*	0.026***	0.033**
	(0.006)	(0.008)	(0.005)	(0.007)	(0.010)	(0.014)	(0.009)	(0.013)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.19	0.27	0.17	0.25	0.28	0.38	0.23	0.34
Number of Observations	112,557	93,571	75,027	61,866	21,752	15,263	14,500	9,861
Number of Local Governments	18,767	16,476	18,764	15,745	3,626	2,763	3,626	2,502

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

This table presents difference-in-differences estimates of regressions of the logarithm of local government employment (as of March of each year) around the Moody's recalibration in April–May 2010. Panel A presents county-level results using employment across all local governments in each county and year. The sample in Panel A, columns (1)–(4), consists of all counties. The sample in Panel A, columns (5)–(8), consists of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results. The sample in Panel B, columns (1)–(4), consists of all local government units. The sample in Panel B, columns (5)–(8), consists of local governments with at least one bond issued in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007–2013		Panel 2009–2012		Panel 2007–2013		Panel 2009–2012	
Panel A: County Level	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated × Post	0.093***	0.094***	0.067***	0.070***	0.039*	0.058**	0.051**	0.058**
	(0.025)	(0.026)	(0.021)	(0.022)	(0.023)	(0.026)	(0.024)	(0.024)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.09	0.09	0.09	0.10	0.13	0.13	0.13	0.13
Number of Observations	11,263	11,259	6,435	6,435	8,116	8,116	4,639	4,639
Number of Counties	1,614	1,612	1,612	1,612	1,161	1,161	1,161	1,161
Panel B: Local Government Level	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated Dummy × Post	0.016*	0.021*	0.015**	0.021*	0.005	0.019	0.002	0.007
	(0.009)	(0.012)	(0.007)	(0.012)	(0.011)	(0.016)	(0.010)	(0.013)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99
Number of Observations	29,518	16,660	16,863	9,512	9,351	4,717	5,343	2,695
Number of Local Governments	4,234	2,390	4,222	2,381	1,336	674	1,336	674

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

This table presents difference-in-differences estimates of regressions of the logarithm of private employment (as of March of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of all counties. The sample in Panel B of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2007–2013		Panel 2009–2012	
Panel A: County Level - Full Sample				
Recalibrated × Post	0.071***	0.054***	0.035***	0.032**
	(0.015)	(0.015)	(0.012)	(0.013)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.21	0.22	0.08	0.08
Number of Observations	21,640	21,640	12,365	12,365
Number of Counties	3,114	3,114	3,110	3,110
Panel B: County Level - Sample of Bond Issuers				
Recalibrated × Post	0.061***	0.038**	0.033**	0.022*
	(0.018)	(0.016)	(0.014)	(0.013)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.27	0.28	0.08	0.09
Number of Observations	10,668	10,668	7,112	7,112
Number of Counties	1,778	1,778	1,778	1,778

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

This table presents difference-in-differences estimates of regressions of the logarithm of tradable (manufacturing, NAICS codes 31–33), non-tradable (retail and restaurant, NAICS codes 44–45 and 72), construction, health-care, and education sectors employment (as of March of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of all counties in the 2009–2012 period. The sample in Panel B of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Tradable		Non-Tradable		Construction		Health-Care		Education	
Panel A: County Level - Full Sample										
Recalibrated × Post	-0.035	0.012	0.100***	0.055*	-0.007	0.028	0.082***	0.039*	0.106**	0.062
	(0.147)	(0.169)	(0.029)	(0.032)	(0.033)	(0.037)	(0.021)	(0.021)	(0.041)	(0.044)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.04	0.05	0.02	0.03	0.18	0.18	0.05	0.06	0.10	0.12
Number of Observations	6,162	6,162	12,125	12,125	10,286	10,286	10,378	10,378	4,152	4,152
Number of Counties	1,681	1,681	3,063	3,063	2,746	2,746	2,685	2,685	1,119	1,119
Panel B: County Level - Sample of Bond Issuers										
Recalibrated × Post	-0.014	0.050	0.089***	0.060*	0.002	0.017	0.051**	0.026	0.107***	0.068
	(0.141)	(0.158)	(0.034)	(0.036)	(0.039)	(0.040)	(0.022)	(0.023)	(0.041)	(0.042)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.06	0.07	0.03	0.05	0.22	0.23	0.08	0.10	0.12	0.13
Number of Observations	4,695	4,695	7,061	7,061	6,503	6,503	6,569	6,569	3,496	3,496
Number of Counties	1,251	1,251	1,771	1,771	1,684	1,684	1,676	1,676	931	931

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

This table presents difference-in-differences estimates of regressions of the logarithm of income (as of December of each year) in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2010 and for each year thereafter. Controls include house price index and number of households. The sample in Panel A consists of all counties. The sample in Panel B of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2006–2012		Panel 2008–2011	
Panel A: County Level - Full Sample				
Recalibrated Dummy × Post	0.090***	0.051***	0.065***	0.035***
	(0.017)	(0.015)	(0.013)	(0.013)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.59	0.61	0.65	0.65
Number of Observations	18,676	18,678	12,449	12,450
Number of Counties	3,114	3,114	3,113	3,113
Panel B: County Level - Sample of Bond Issuers				
Recalibrated × Post	0.063***	0.031**	0.051***	0.031**
	(0.017)	(0.015)	(0.014)	(0.014)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.67	0.67	0.71	0.72
Number of Observations	10,668	10,668	7,112	7,112
Number of Counties	1,778	1,778	1,778	1,778

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

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

	(1)	(2)	(3)	(4)	(5)	(6)
	Government Employment		Private Employment		Income	
Panel A: First Stage - Local Government Expenditures						
Recalibrated × Post	0.055**	0.040**	0.081***	0.054**	0.078***	0.052**
	(0.022)	(0.020)	(0.025)	(0.021)	(0.024)	(0.021)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.15	0.15	0.11	0.12	0.11	0.12
F-statistic of Instrument	6.32	3.93	10.63	6.43	10.27	6.10
Number of Observations	6,371	6,371	11,751	11,751	11,843	11,843
Number of Counties	1,596	1,596	2,955	2,955	2,961	2,961
Panel B: Second Stage						
Local Gov. Expenditures	1.216*	1.745*	0.467**	0.592*	0.636***	0.464**
	(0.625)	(1.013)	(0.215)	(0.316)	(0.245)	(0.235)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Number of Observations	6,371	6,371	11,751	11,751	11,843	11,843
Number of Counties	1,596	1,596	2,955	2,955	2,961	2,961

Table 11

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

This table presents difference-in-differences estimates of regressions of the logarithm of government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* 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 variable takes a value of one when the unemployment rate in 2010 is below the median. In Panel B, the *High Slack* dummy variable takes a value of one when the change in house price index between 2007 and 2009 is below the median. Controls include house price index and number of households. The sample consists of all counties in the 2007–2013 period (2006–2012 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel A: Unemployment Rate								
Recalibrated × Post	-0.035	0.010	0.008	0.048	-0.005	0.006	0.001	0.003
	(0.026)	(0.027)	(0.030)	(0.030)	(0.014)	(0.014)	(0.016)	(0.016)
Recalibrated × Post × High Slack	0.165***	0.156***	0.105*	0.110**	0.084***	0.066**	0.103***	0.081***
	(0.054)	(0.054)	(0.057)	(0.055)	(0.029)	(0.030)	(0.027)	(0.026)
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.24	0.02	0.02	0.15	0.16	0.51	0.53
Number of Observations	20,734	20,734	11,270	11,266	21,640	21,640	18,808	18,804
Number of Counties	2,962	2,962	1,615	1,613	3,114	3,114	3,136	3,135
Panel B: Change in House Prices								
Recalibrated × Post	0.056	0.090**	0.093**	0.119***	0.012	0.022	0.026	0.027
	(0.040)	(0.042)	(0.038)	(0.039)	(0.022)	(0.022)	(0.023)	(0.023)
Recalibrated × Post × High Slack	0.029	0.002	-0.030	-0.035	0.062**	0.039*	0.072**	0.046*
	(0.049)	(0.051)	(0.051)	(0.051)	(0.028)	(0.023)	(0.031)	(0.027)
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.24	0.02	0.03	0.14	0.15	0.51	0.52
Number of Observations	20,734	20,734	11,268	11,266	21,630	21,630	18,810	18,810
Number of Counties	2,962	2,962	1,614	1,613	3,112	3,112	3,136	3,136

Table 12

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

This table presents difference-in-differences estimates of regressions of the logarithm of expenditures (as of July of each year) of each local government unit and year around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Panel A presents estimates for the sample split into small and large local governments based on number of employees (as of 2007) below of above the median. Panel B presents estimates for the sample split into low-leverage and high-leverage local governments based on the ratio of total debt-to-revenues (as of 2007) below or above the median. Controls include house price index and number of households. The sample consists of all local government units. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(1)	(2)	(3)	(4)
	Panel 2007–2013		Panel 2009–2012		Panel 2007–2013		Panel 2009–2012	
Panel A: Sample Split by Local Government Size	*Small*				*Large*			
Recalibrated Dummy × Post	0.049**	0.083***	0.031	0.055**	0.019***	0.027***	0.009*	0.015**
	(0.023)	(0.032)	(0.019)	(0.026)	(0.005)	(0.006)	(0.005)	(0.006)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.96	0.96	0.97	0.97	0.26	0.34	0.21	0.29
Number of Observations	54,444	47,578	36,296	31,603	52,308	38,337	34,872	25,138
Number of Local Governments	9,074	8,466	9,074	8,102	8,718	6,775	8,718	6,341
Panel A: Sample Split by Local Government Leverage	*High Leverage*				*Low Leverage*			
Recalibrated Dummy × Post	0.042***	0.050***	0.026***	0.032***	-0.003	0.022*	-0.004	0.009
	(0.008)	(0.009)	(0.007)	(0.009)	(0.014)	(0.013)	(0.012)	(0.012)
Local Gov. Type-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
County-Year Fixed Effect	Yes	No	Yes	No	Yes	No	Yes	No
Local Gov. Type-County-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.23	0.30	0.20	0.29	0.28	0.36	0.26	0.31
Number of Observations	54,552	43,130	36,363	28,300	51,876	41,736	34,579	27,465
Number of Local Governments	9,096	7,688	9,096	7,212	8,649	7,497	8,647	7,012

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

This table presents difference-in-differences estimates of regressions of the logarithm of local government expenditures, government employment, private employment, and income in each county and year around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2011 (2010 in the case of income) and for each year thereafter. The sample in Panel A consists of urban areas of 50,000 or more people. The sample in Panel B consists of counties in which at least two local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). Controls include house price index and number of households. The sample consists of all counties in the 2009–2012 period (2008–2011 in the case of income). Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Panel A: Sample of Urban Counties								
Recalibrated × Post	0.095***	0.071**	0.075***	0.076***	0.041***	0.040***	0.038**	0.020
	(0.027)	(0.029)	(0.025)	(0.026)	(0.016)	(0.015)	(0.019)	(0.019)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.47	0.48	0.19	0.20	0.38	0.42	0.77	0.77
Number of Observations	6,349	6,342	5,300	5,293	6,508	6,501	5,580	5,574
Number of Counties	907	906	758	757	930	929	930	929
Panel B: Sample of Counties with Multiple Issuers								
Recalibrated × Post	0.043	0.053*	0.079**	0.093***	0.053**	0.035*	0.090***	0.052**
	(0.030)	(0.028)	(0.033)	(0.033)	(0.025)	(0.021)	(0.024)	(0.024)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.48	0.40	0.15	0.16	0.34	0.36	0.79	0.80
Number of Observations	6,733	6,607	5,339	5,222	6,718	6,466	5,790	5,622
Number of Counties	1,057	979	818	779	1,062	978	1,063	979

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

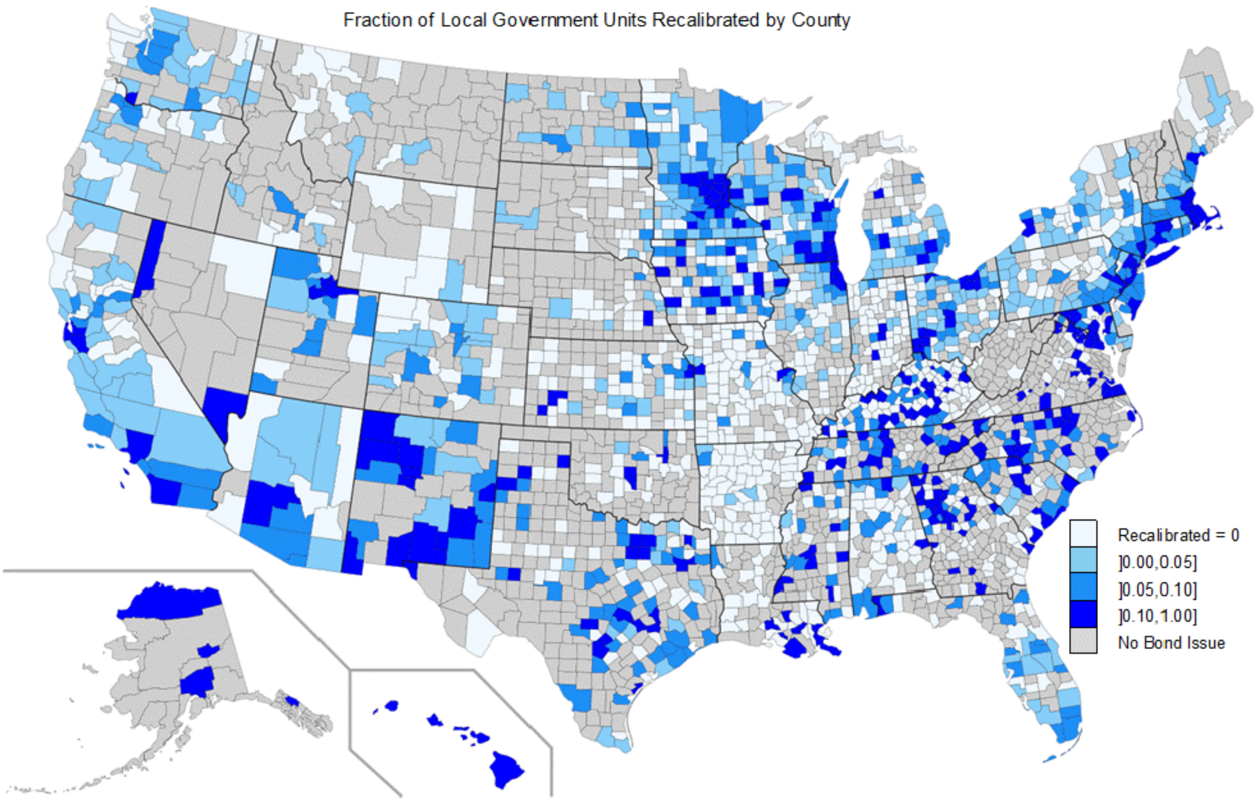
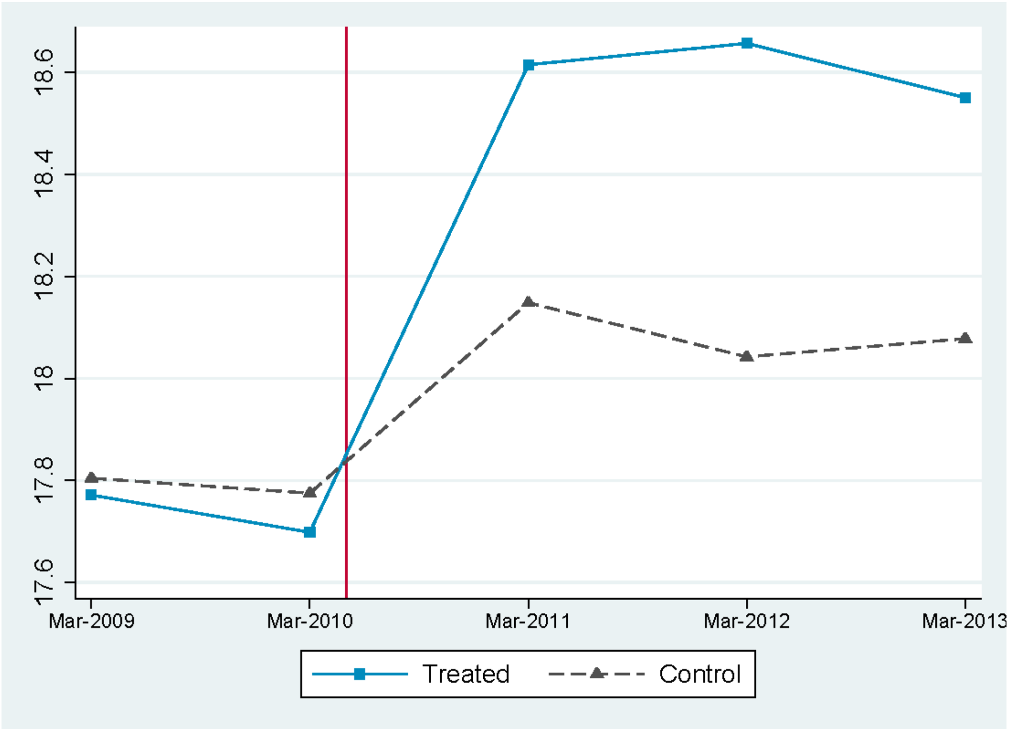


Figure 2
Moody's and S&P Ratings around the Recalibration

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



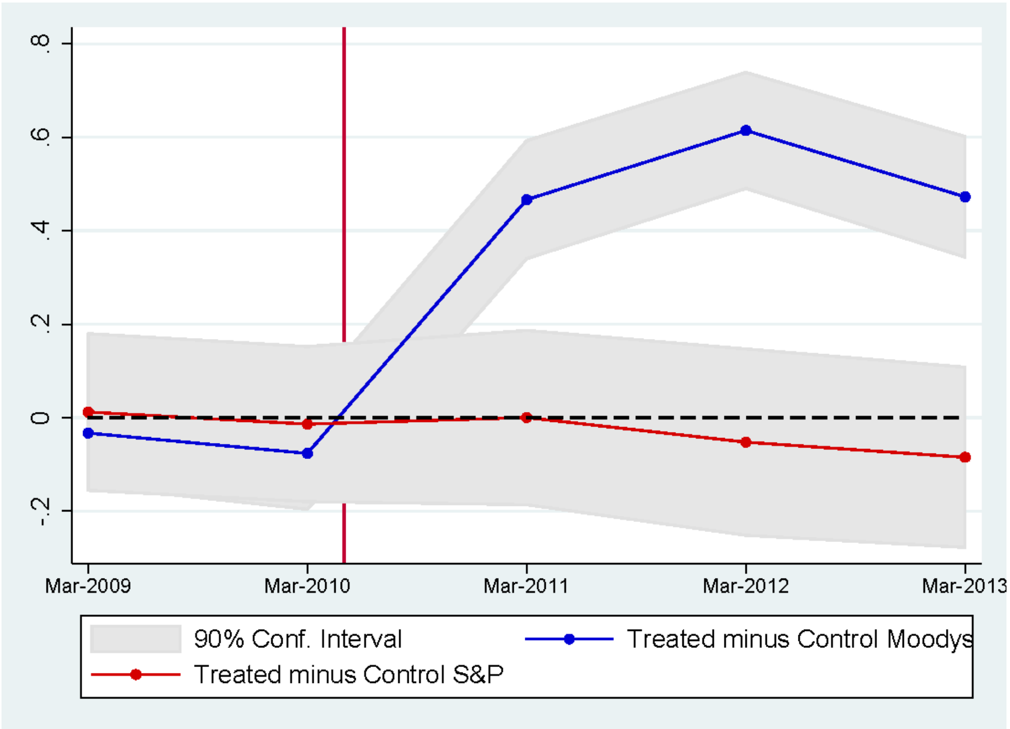


Figure 3
Issue Amount around the Recalibration

This figure shows 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) and its difference during the Moody's recalibration event in April–May 2010.

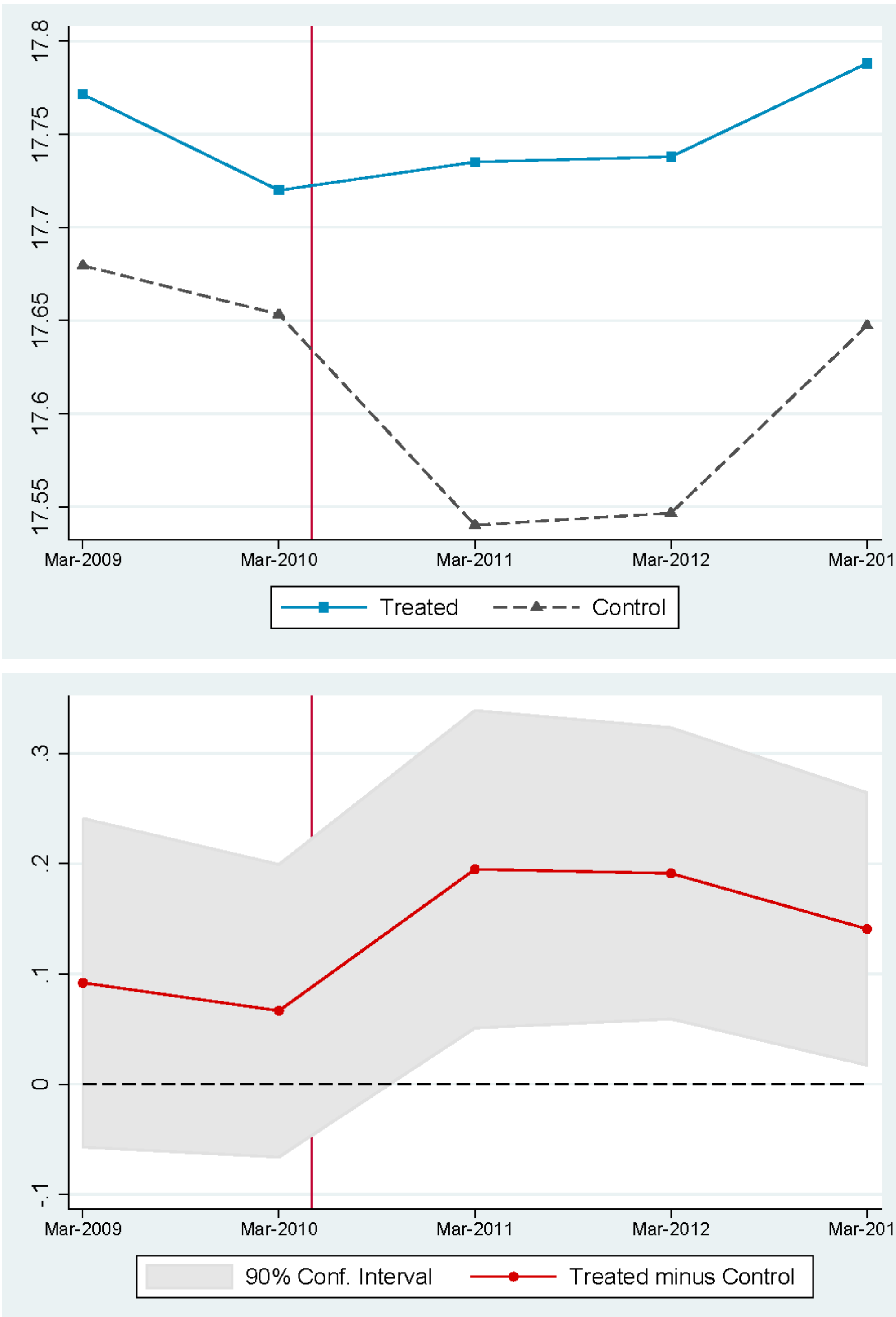


Figure 4

Local Government Expenditures around the Recalibration

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of local government expenditures (as of July of each year) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April–May 2010.

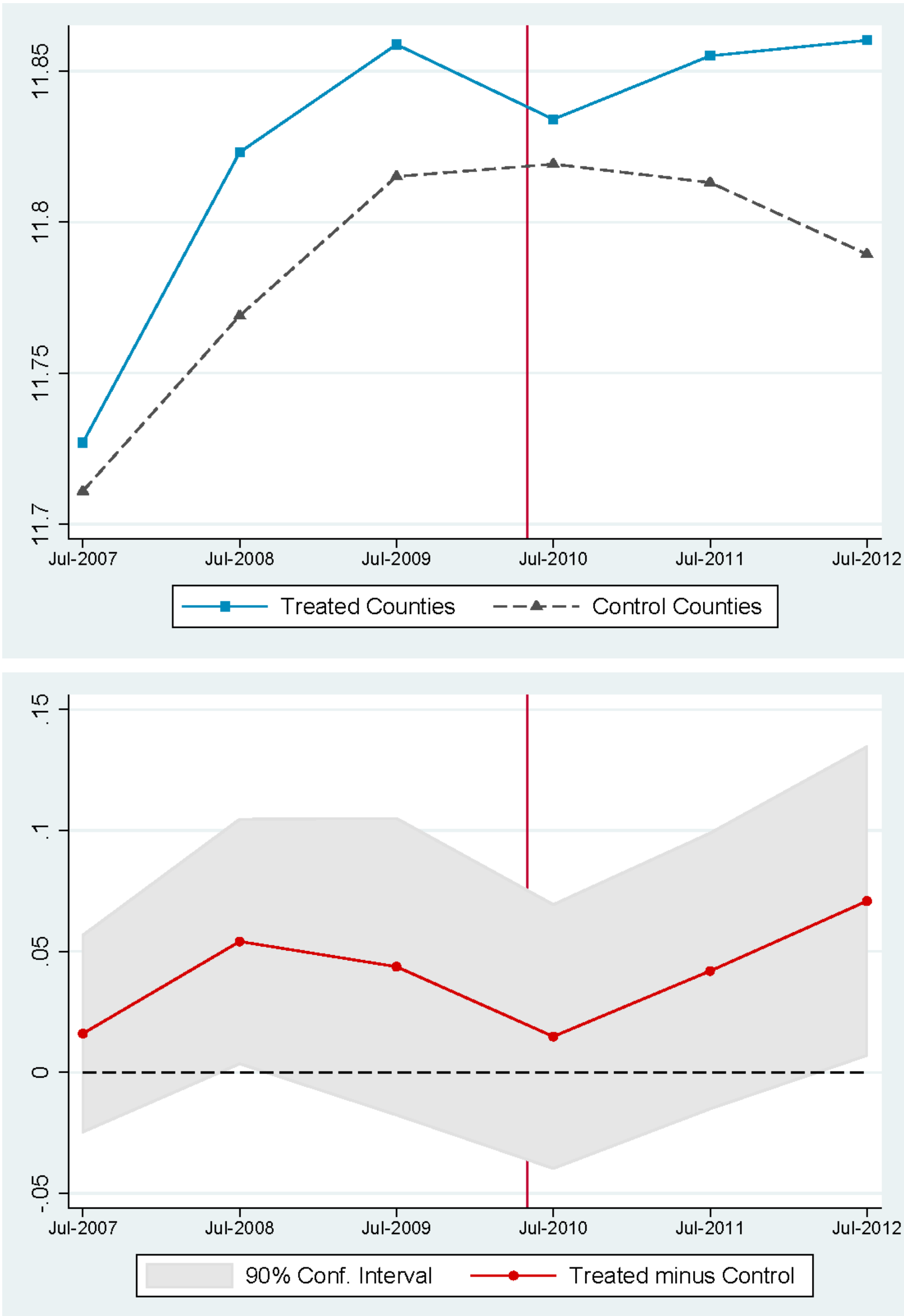


Figure 5
Local Government Employment around the Recalibration

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of local government employment (as of March of each year) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April–May 2010.

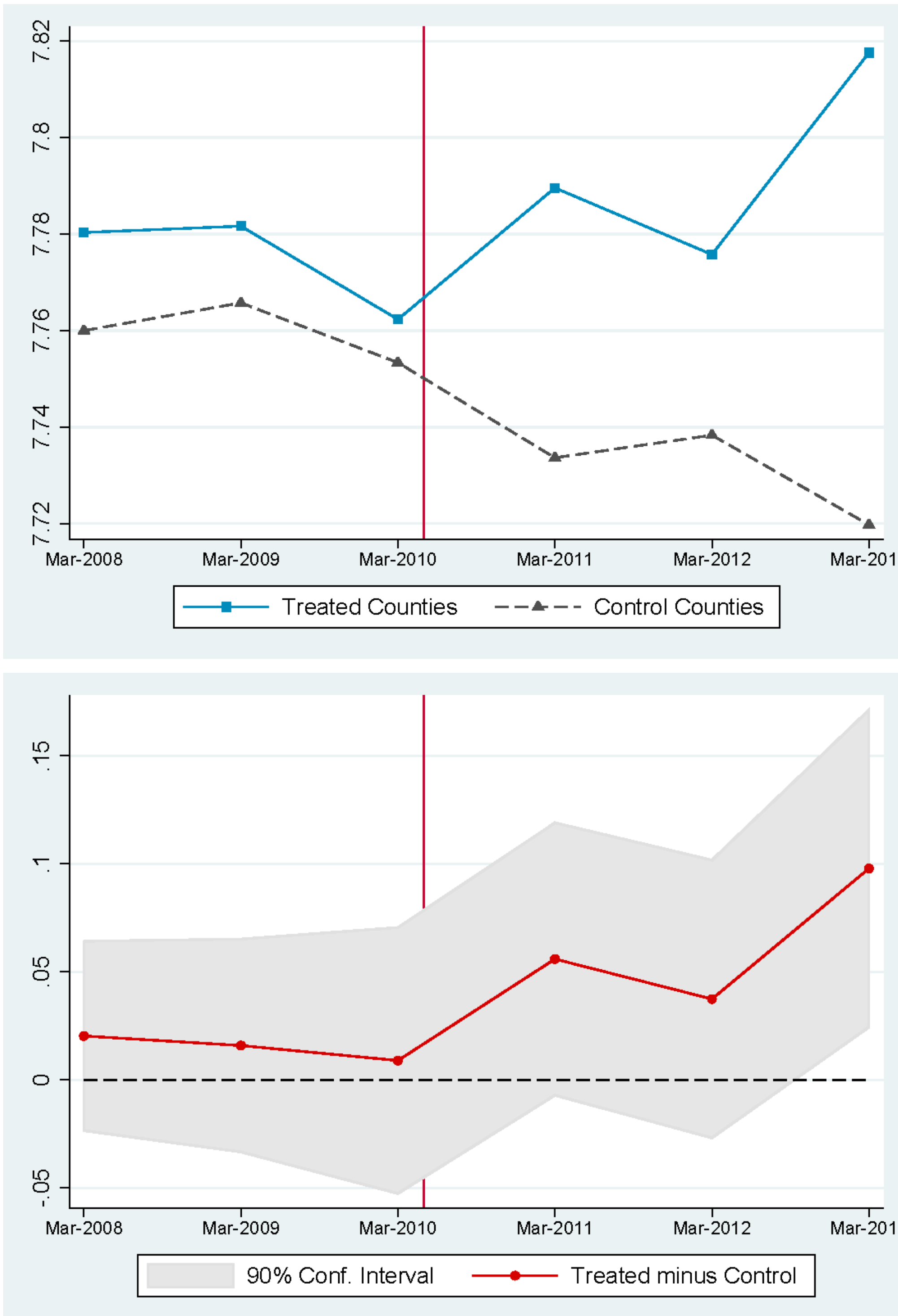
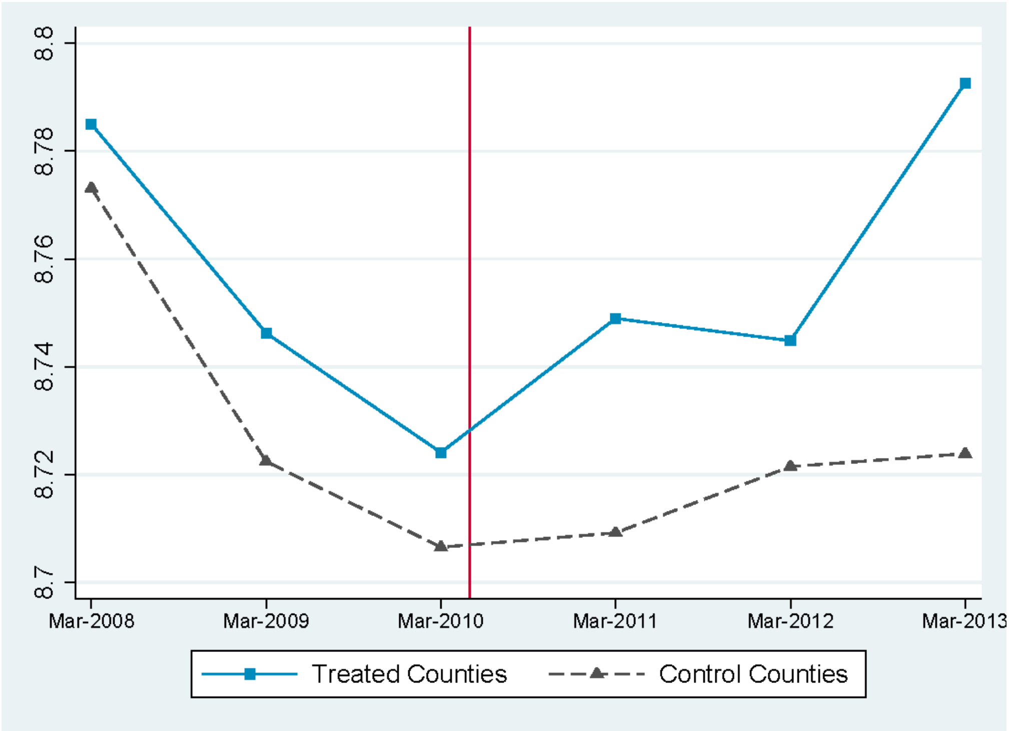


Figure 6
Private Employment around the Recalibration

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of private employment (as of March of each year) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April–May 2010.



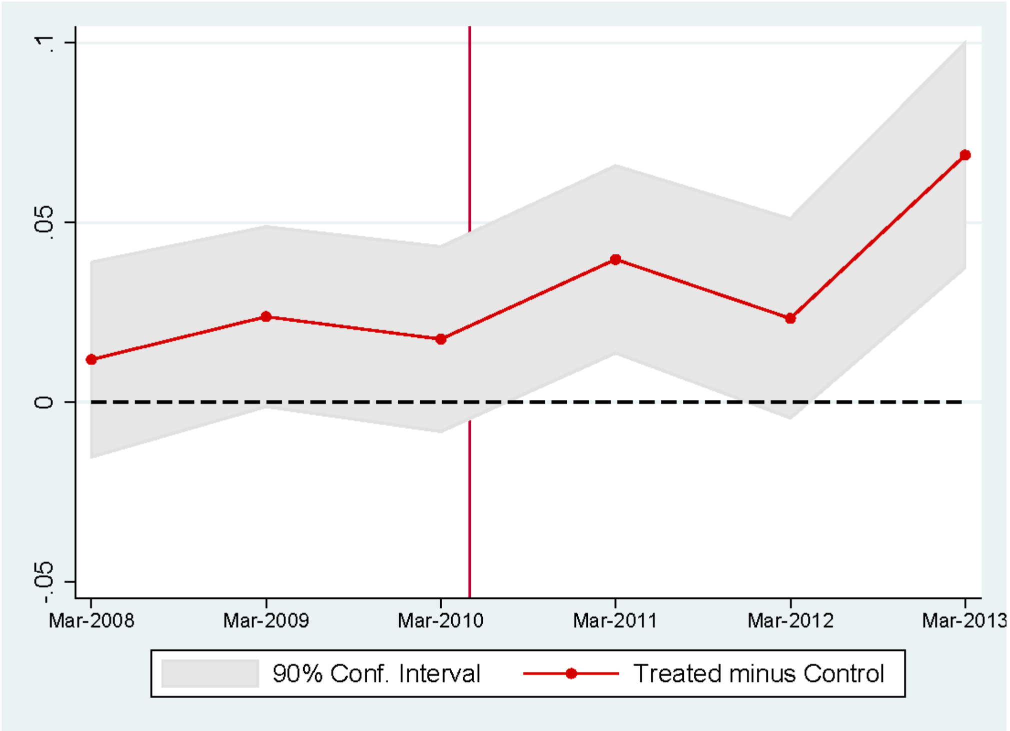


Figure 7

Private Employment around the Recalibration: Quarterly Frequency

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of private employment (at the end of each quarter) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April–May 2010.

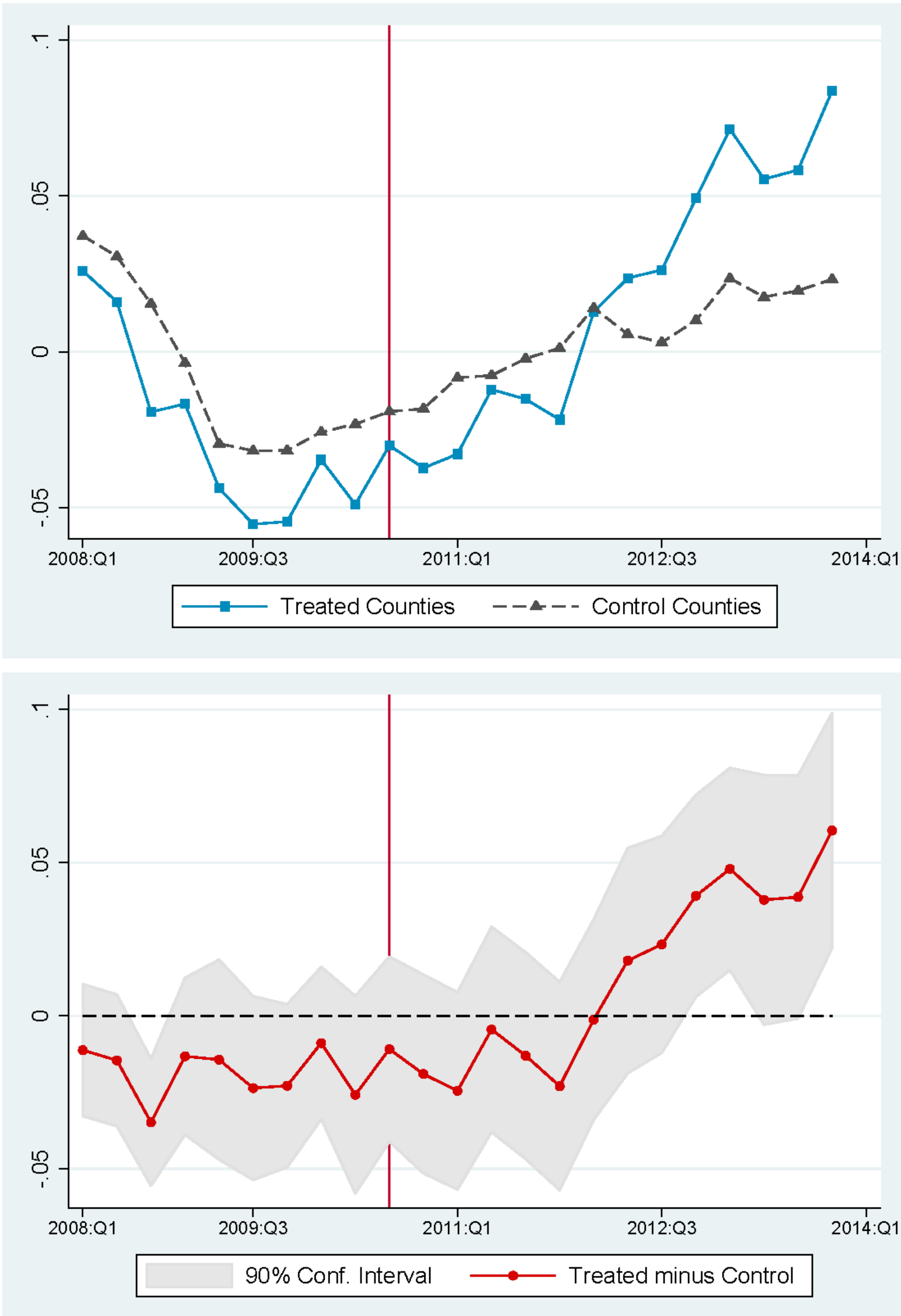
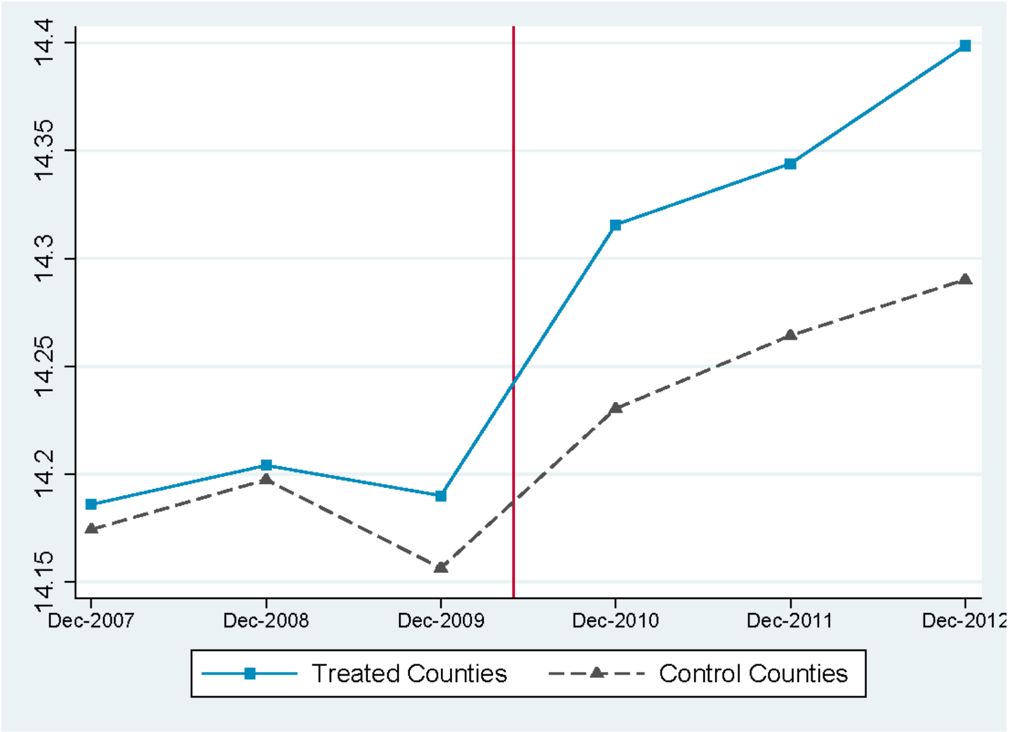
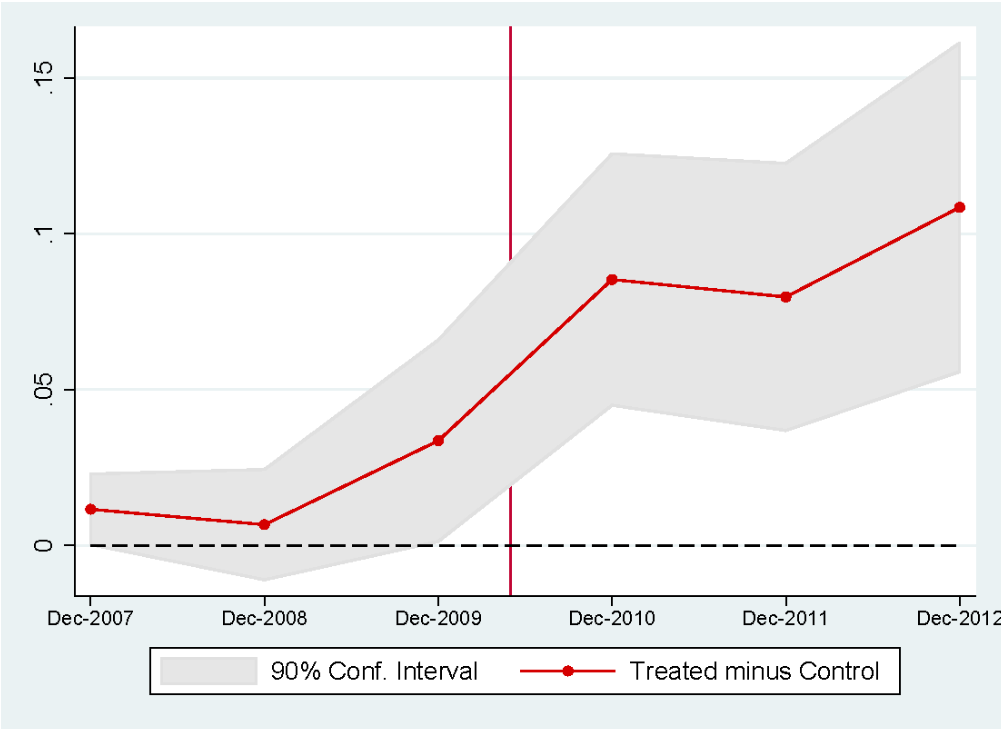


Figure 8
Income around the Recalibration

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of income (as of December of each year) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event 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: Bond Issue Level

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007–March 2013. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P		Rating Moody's (sample S&P)	
	(1)	(2)	(3)	(4)	(5)	(6)
Recalibrated Dummy × Post	0.580***	0.594***	-0.034	-0.052	0.626***	0.584***
	(0.049)	(0.060)	(0.059)	(0.086)	(0.063)	(0.081)
Year Fixed Effects	Yes	No	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.24	0.44	0.04	0.18	0.21	0.38
Number of Observations	202,615	202,615	111,367	111,367	111,367	111,367

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

This table presents difference-in-differences estimates of regressions of Moody's and S&P ratings around the Moody's recalibration in April–May 2010. Panel A presents county-level results using the average rating across all issues of local governments of each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. Panel B presents local-government-level results using the average rating across all issues of each local government and event year. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2008-March 2012 period. Robust standard errors clustered at the county level (in Panel A) and local government level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Rating Moody's		Rating S&P		Rating Moody's (sample S&P)	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: County Level						
Recalibrated × Post	0.802***	0.825***	0.047	0.016	0.545**	0.454**
	(0.206)	(0.210)	(0.222)	(0.223)	(0.273)	(0.229)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.43	0.44	0.25	0.30	0.45	0.51
Number of Observations	2,490	2,490	1,121	1,114	1,121	1,114
Number of Counties	1,144	1,144	555	555	555	555
Panel B: Local Government Level						
Recalibrated Dummy × Post	0.665***	0.631***	-0.419	-0.271	0.715***	0.558***
	(0.084)	(0.078)	(0.255)	(0.235)	(0.124)	(0.122)
Year Fixed Effects	Yes	No	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.33	0.56	0.02	0.34	0.33	0.56
Number of Observations	7,004	7,004	3,109	3,109	3,109	3,109
Number of Local Governments	4,335	4,335	1,660	1,660	1,660	1,660

Table IA.3
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 the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* is a dummy variable that takes a value of one between April 2010 and March 2013. Controls include a dummy for general obligation bonds, a dummy for Build America Bonds, and duration. The sample consists of Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Recalibrated Dummy × Post	0.121***	0.166**	-0.187***	-0.228**
	(0.045)	(0.069)	(0.044)	(0.082)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.01	0.09	0.45	0.52
Number of Observations	202,615	202,615	202,615	202,615

Table IA.4

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 the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents bond-issue-level results. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of a given local government in each event year. In columns (1) and (2), the sample is restricted to two years before and after the recalibration (2008–2012). In columns (3) and (4), sample is restricted to bond issues that have both Moody's and S&P ratings. In columns (5) and (6), the sample excludes bonds issued under the "Build for America" government program. In columns (7) and (8), the sample consists of all local governments (instead of just those that issue bonds in the three years before the recalibration). In columns (9) and (10), the sample consists of insured and uninsured bond issues. *Recalibrated Dummy* takes a value of one if a local government experienced an upgrade of any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the local government level (in Panel A) and county level (in Panel B) are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Sample Period 2008–2012		Sample with S&P Ratings		Sample Excluding BAB		Sample of All Issuers		Sample of All Bonds	
	Issue Amount	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: Bond 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)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.08	0.46	0.08	0.50	0.09	0.51	0.08	0.54	0.08	0.26
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)
County-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.32	0.48	0.33	0.52	0.34	0.54	0.33	0.58	0.28	0.59
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.5
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 the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. Panel A presents bond-issue-level results. Panel B presents local-government-level results using the logarithm of the amount of bonds issued and the average offer yield across all issues of a given local government in each event year. *Recalibrated 1 Notch*, *Recalibrated 2 Notches*, and *Recalibrated 3 Notches* take a value of one if a local government experienced a maximum upgrade of one notch, two notches, and three notches respectively in any of its outstanding bonds during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the local government level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Panel A: Bond Issue Level				
Recalibrated 1 Notch × Post	0.114***	0.137**	-0.184***	-0.232***
	(0.044)	(0.066)	(0.040)	(0.074)
Recalibrated 2 Notches × Post	0.179***	0.119	-0.298***	-0.201*
	(0.054)	(0.083)	(0.053)	(0.103)
Recalibrated 3 Notches × Post	0.321	0.211	-0.227**	-0.211
	(0.283)	(0.401)	(0.094)	(0.140)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.00	0.09	0.45	0.52
Number of Observations	202,615	202,615	201,039	201,039
Panel B: Local Government Level				
Recalibrated 1 Notch × Post	0.152**	0.189***	-0.185***	-0.079
	0.061	(0.070)	0.069	(0.081)
Recalibrated 2 Notches × Post	0.275***	0.232**	-0.270***	-0.008
	0.077	(0.094)	0.090	(0.112)
Recalibrated 3 Notches × Post	0.272	0.219	-0.457**	-0.315
	0.250	(0.300)	0.227	(0.288)
Year Fixed Effects	Yes	No	Yes	No
County-Year Fixed Effects	No	Yes	No	Yes
Local Gov. Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.04	0.33	0.43	0.61
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.6
Difference-in-Differences of Issue Amount and Offer Yield of New Bond Issues around the Recalibration: Weighted by Amount of Bonds Issued

This table presents difference-in-differences estimates of regressions of the logarithm of the *Issue Amount* and *Offer Yield* around the Moody's recalibration in April–May 2010. The dependent variables are the logarithm of the amount of bonds issued and the average offer yield across all issues of local governments in each county and event year. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* 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 Ipreo i-Deal municipal new bond issues in the April 2007-March 2013 period. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Issue Amount (log)		Offer Yield	
	(1)	(2)	(3)	(4)
Recalibrated × Post	0.226***	0.217**	-0.210**	-0.293**
	(0.084)	(0.090)	(0.096)	(0.100)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.17	0.18	0.39	0.41
Number of Observations	5,504	5,504	5,504	5,504
Number of Counties	1,780	1,780	1,780	1,780

Table IA.7
Difference-in-Differences of Local Government Current Expenditures and Capital Outlays around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government current expenditures and capital outlays (as of July of each year) in each county and year around the Moody's recalibration in April–May 2010. The sample in columns (1)–(4) consists of all counties. The sample in columns (5)–(8) consists of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Panel 2007–2013		Panel 2009–2012		Panel 2007–2013		Panel 2009–2012	
Panel A: Current Expenditures	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated × Post	0.150***	0.090***	0.068***	0.038**	0.102***	0.072***	0.042*	0.025
	(0.025)	(0.024)	(0.019)	(0.018)	(0.029)	(0.027)	(0.023)	(0.022)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.45	0.46	0.18	0.19	0.54	0.55	0.19	0.20
Number of Observations	20,727	20,727	11,844	11,844	12,229	12,229	6,988	6,988
Number of Counties	2,961	2,961	2,961	2,961	1,747	1,747	1,747	1,747
Panel B: Capital Outlays	*Full Sample*				*Sample of Bond Issuers*			
Recalibrated × Post	0.247*	0.214	0.323**	0.228	0.050	0.064	0.335**	0.206
	(0.150)	(0.168)	(0.162)	(0.182)	(0.160)	(0.164)	(0.169)	(0.176)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size Decile-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.07	0.08	0.06	0.07	0.09	0.10	0.08	0.09
Number of Observations	20,640	20,640	11,793	11,793	12,218	12,218	6,980	6,980
Number of Counties	2,958	2,958	2,957	2,957	1,747	1,747	1,747	1,747

Table IA.8

Difference-in-Differences of Local Government Taxes around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of local government taxes (as of July of each year) in each county and year around the Moody's recalibration in April–May 2010. The sample in Panel A consists of all counties. The sample in Panel B consists of counties in which at least one local government issued bonds in the municipal bond market during the three-year period before the recalibration (April 2007–March 2010). *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2011 and for each year thereafter. Controls include house price index and number of households. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2007–2013		Panel 2009–2012	
Panel A: County Level - Full Sample				
Recalibrated Dummy × Post	-0.051**	-0.018	-0.042**	-0.030
	(0.024)	(0.024)	(0.020)	(0.022)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.47	0.48	0.37	0.37
Number of Observations	20,531	20,531	11,736	11,736
Number of Counties	2,936	2,936	2,936	2,936
Panel B: County Level - Sample of Bond Issuers				
Recalibrated × Post	-0.080***	-0.045*	-0.050**	-0.044*
	(0.025)	(0.024)	(0.022)	(0.023)
State-Year Fixed Effects	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.59	0.59	0.44	0.45
Number of Observations	12,153	12,153	6,948	6,948
Number of Counties	1,739	1,739	1,739	1,739

Table IA.9
Difference-in-Differences of New Firms Employment around the Recalibration

This table presents difference-in-differences estimates of regressions of the logarithm of new firms (firms less than two years old) employment in the non-tradable sector (as of March of each year) in each county and quarter around the Moody's recalibration in April–May 2010. *Recalibrated* is the fraction of local government units upgraded in each county during the Moody's recalibration. *Post* takes a value of one in 2010:Q3 and for each quarter thereafter. Controls include house price index and number of households. Robust standard errors clustered at the county level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)
	Panel 2006:Q1–2012:Q4		Panel 2008:Q1–2011:Q4	
Recalibrated × Post	0.216***	0.210**	0.122	0.128
	(0.083)	(0.091)	(0.084)	(0.088)
Quarter Fixed Effects	Yes	No	Yes	No
State-Quarter Fixed Effects	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes
R-squared	0.09	0.14	0.06	0.11
Number of Observations	73,712	73,664	41,558	41,508
Number of Counties	3,017	2,996	2,960	2,925

Table IA.10
Difference-in-Differences of Economic Outcomes around the Recalibration: Robustness

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Government Expenditures		Government Employment		Private Employment		Income	
Recalibrated × Post	0.066***	0.051**	0.074***	0.078***	0.033***	0.029***	0.034***	0.023*
	(0.020)	(0.020)	(0.020)	(0.021)	(0.011)	(0.011)	(0.012)	(0.012)
State-Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size-Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.11	0.12	0.09	0.10	0.08	0.08	0.67	0.67
Number of Observations	11,844	11,844	6,435	6,435	12,365	12,365	12,537	12,534
Number of Counties	2,961	2,961	1,612	1,612	3,110	3,110	3,135	3,134

Figure IA.1
Offer Yield around the Recalibration

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

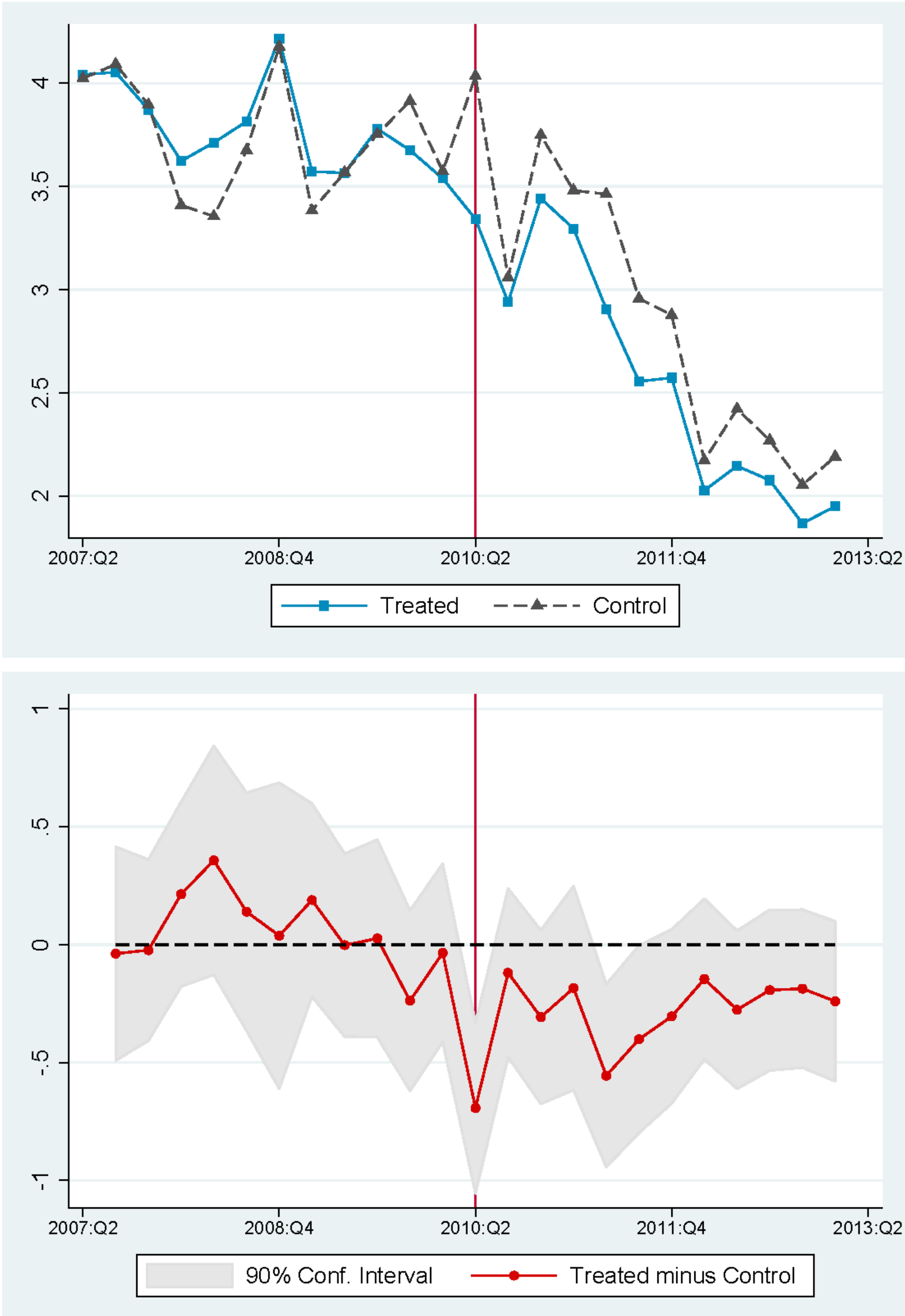

Figure IA.2
Local Government Taxes around the Recalibration

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of local government taxes (as of July of each year) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April-May 2010.

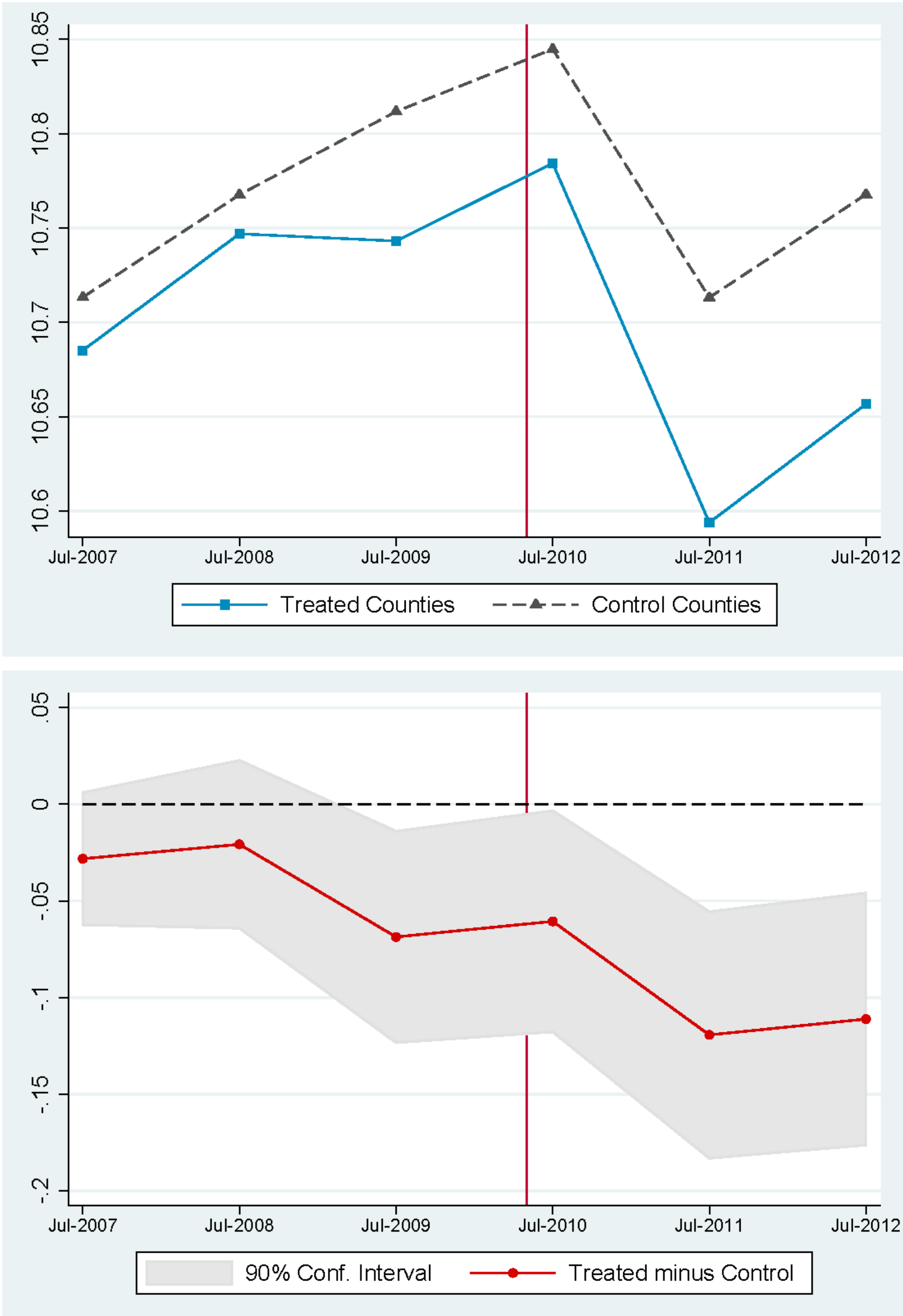

Figure IA.3
Non-Tradable Employment around the Recalibration: Quarterly Frequency

This figure shows point estimates and 90% confidence intervals for the effect on the logarithm of non-tradable (NAICS 44–45 and 72) private employment (at the end of each quarter) of upgraded local governments (treated) relative to non-upgraded local governments (control) and its difference during the Moody's recalibration event in April–May 2010.

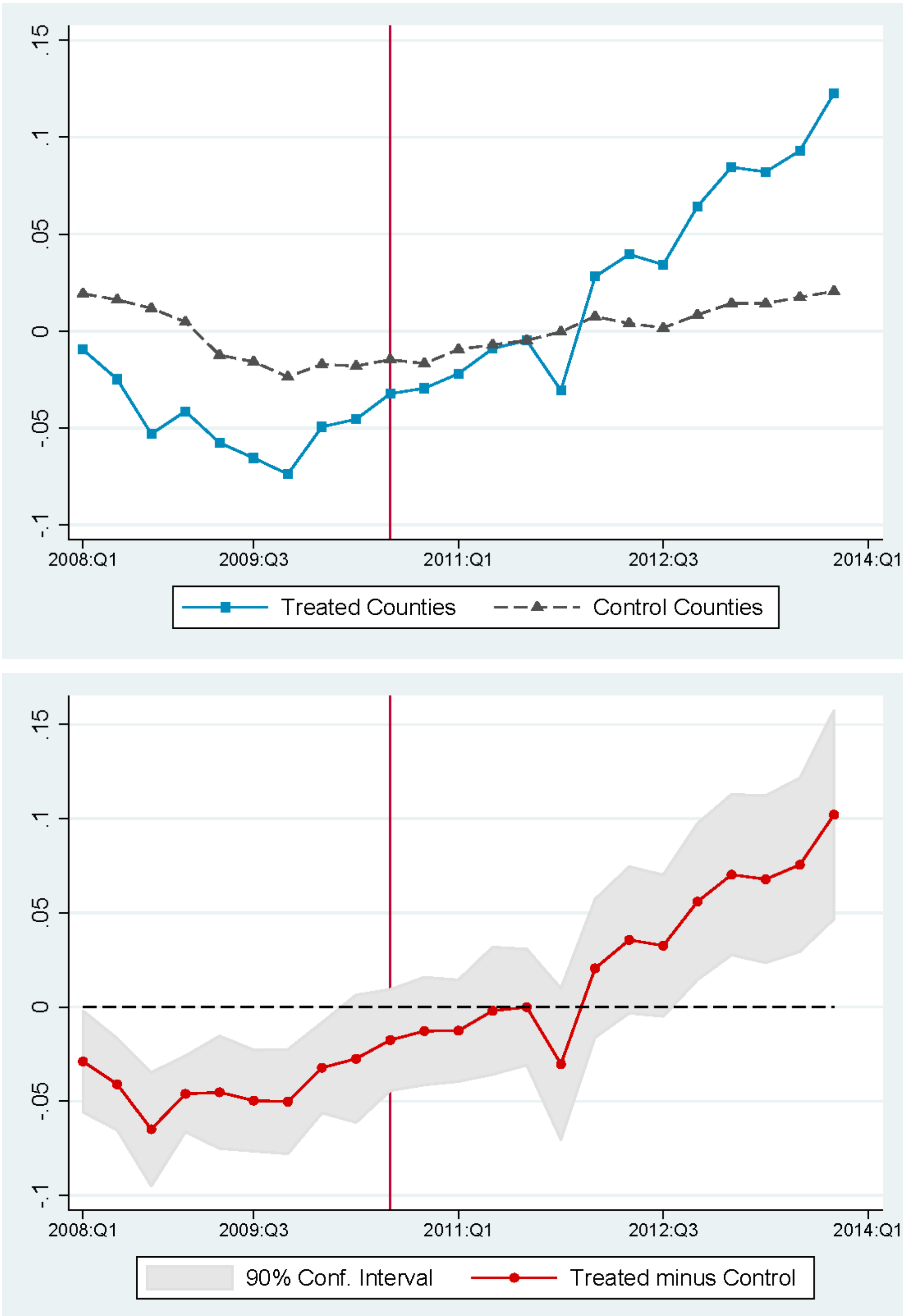

Figure IA.4
New Firms Non-Tradable Employment around the Recalibration

This figure shows point estimates for the effect on the logarithm of new firms (firms less than two years old) employment in the non-tradable sector (at the end of each quarter) of upgraded local governments (treated) relative to non-upgraded local governments (control) during the Moody's recalibration event in April–May 2010.