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

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Abstract

We study how changes in the supply of local public financing affect economic outcomes by exploring Moody's recalibration of its municipal bond credit ratings scale. Upgraded municipalities increase bond issuance and experience a reduction in their borrowing costs relative to non-upgraded municipalities following the ratings recalibration. This shock to the supply of credit to local governments leads to greater increases in government employment, private employment, and income of upgraded municipalities relative to non-upgraded municipalities. Private sector job creation is concentrated in the non-tradable, education and health sectors, which depend primarily on local demand and government transfers.

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1. Introduction

Municipal bonds markets are an important source of financing of local governments to finance the construction and maintenance of infrastructure and other public projects, provide cash flow for government needs, as well as finance private projects (through the use of "conduit" financing). According to the Securities Exchange Commission (2012), investors held over one million different municipal bond issues representing a total outstanding principal amount of more than $3.7 trillion as of December 2011, which corresponds to about 25% of gross domestic product (GDP).

How do changes to the supply of credit to local governments affect economic outcomes? Identifying the causal impact of municipals' bond financing on economic outcomes is challenging, because changes in credit supply (i.e., investor demand for municipal bonds) are correlated with changes in nationwide and local fundamentals, as well as changes in credit demand by municipalities.

Our identification strategy exploits the variation in municipal bonds ratings due to Moody's recalibration of its municipal bond rating scale (the treatment). Credit rating agencies are an important source of information to investors in the municipal bond market, as it is more opaque than other fixed income markets (e.g., sovereign and corporate bond markets). In addition, the municipal bond market is dominated by retail (unsophisticated) investors, while other fixed income markets are dominated by regulated financial institutions.[1] Thus, the effects of changes in ratings are more likely to reflect investors' reliance on ratings as a source of information, rather than just rating-based regulations.

[1] In contrast with the corporate bond market, which is dominated by institutional investors, households directly own 50% of the outstanding debt in the municipal bond market.

Before the ratings recalibration, Moody's Municipal Rating Scale measured how likely an entity was to require extraordinary support from a higher level of government in order to avoid default, while Moody's Global Rating Scale ratings measures expected losses among sovereign and corporate bond, and other securities. This dual class rating system persisted for decades until Moody's recalibrated its municipal ratings to align them with the Global Scale in April and May of 2010, resulting in upgrades of zero to three notches on more than 600 thousand municipal bonds worth more than $2.2 trillion (in par value).

Crucially, these rating changes do not reflect changes in the intrinsic quality of the issuers, but rather the intention to align municipal ratings standards to those of sovereign and corporate ratings. Thus, this unique event is uncorrelated with changes in local government (and nationwide) fundamentals and allows us to identify the economic effects of changes in municipal bond ratings. A particularly important aspect of this recalibration is that not all municipal bond issues are upgraded by Moody's. Municipalities that are already properly calibrated vis-à-vis other securities can thus be used as control group, as well as municipalities that do not have a rating from Moody's. We employ differences-in-differences estimates that compare outcomes between upgraded municipalities (the treatment group) and non-upgraded municipalities (the control group) around the recalibration event.

Cornaggia, Cornaggia, and Israelsen (2015) use the Moody's ratings recalibration to study whether changes in credit ratings affect municipal bond market prices. They find that upgraded bonds earn abnormal returns. Upgraded municipalities subsequently issue more bonds with lower offer yields than non-upgraded municipalities. The authors argue that this effect is a result of investors' reliance on rating agencies to assess credit risk rather than rating-based regulation.

We study how this shock to the supply of credit to local governments affects local economic

outcomes. We focus our analysis on the effects on county-level government employment, private employment, and income. Since our event affects bonds issued by counties, as well as by other local government units (i.e., jurisdictions) such as cities, townships, school districts, and special districts, we aggregate the changes in ratings to the county level.[2] Our treatment variable is the fraction of local government units that have bond issues upgraded in each county as a result of the Moody's recalibration.

We first show that Moody's recalibration causes an asymmetric effect in ratings of new municipal bond issues. Moody's ratings increase half a notch more for recalibrated municipalities than for non-recalibrated municipalities. Unlike Moody's, S&P has long maintained that it never had a dual-class rating system. If S&P's municipal ratings were already on the same scale as corporate and sovereign ratings, S&P should not update its ratings at the time of the Moody's recalibration. Thus, 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 half of the bonds with a Moody's rating also have a S&P ratings). As expected, we find no significant changes in S&P ratings between Moody's recalibrated bonds and non-recalibrated bond. This placebo test supports the key assumption of our identification strategy that the shock to Moody's ratings due to the recalibration is unrelated to changes in issuer credit quality and local and nationwide fundamentals.

We then exploit the asymmetric effect of the recalibration on municipal bond ratings to identify the effects of ratings on credit supply to municipalities. Consistent with the evidence in Cornaggia, Cornaggia, and Israelsen (2015), we show that upgraded municipalities increase the dollar volume of new bond issues after the recalibration significantly more than non-upgraded

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

municipalities. The differential effect on the dollar volume of bond issues at the county level is about 22% per year in a period of three years after the recalibration (April 2010-March 2013) relative to three years before (April 2007-March 2010). The recalibration affects not only quantities but also prices. We find that the offer yield of new issues of upgraded municipalities decrease more than the offer yield of non-upgraded municipalities following the recalibration. The differential reduction in offer yields at the county level is economically significant at about 40 basis points.[3]

These results are consistent with a positive shock to the credit supply of municipalities that benefit from bond rating upgrades as a consequence of the ratings recalibration. A priori, the positive shock to the supply of municipal bond financing could have a small (or zero) immediate impact on local economic outcomes if municipalities use the funds to increase rainy day funds, without affecting the overall spending of local governments. On the other hand, local governments can use the increase in the availability of financing in bond markets to hire employees, increase spending and transfer programs, and decrease taxes and therefore improve local economic conditions.

Consistent with local governments using the increase in bond financing to boost economic growth, we find important effects on local economic outcomes after the ratings recalibration. We find that upgraded municipalities' government employment increases about 6% more than non-upgraded municipalities following the ratings recalibration, which is both economically and statistically significant.

In the private sector, we expect the direct effects of municipalities' rating upgrades to be less pronounced than in the government sector, as local governments can use funds to directly hire

[3] These economic magnitudes correspond to a shock in which 100% of local government units within a county are upgraded due to the recalibration relative to a county where no entity is recalibrated.

employees. Indeed, the overall employment effects of the ratings recalibration are smaller than in the public sector. We find that private employment in upgraded municipalities increases about 3% more than in non-upgraded municipalities following the ratings recalibration.

We also examine whether the effects on private employment are heterogeneous across sectors. We expect the effects to be more pronounced in the non-tradable sector, which depend primarily on local demand, as well as in the health and education sectors, which receive transfers or grants from local governments. We find that non-tradable sector employment increases by 7% to 12% more in upgraded municipalities than in non-upgraded municipalities following the recalibration. The differential effects are also strong in the health and education sectors employment with a similar magnitude to those the non-tradable sector. In contrast, the corresponding differential effect in tradable sector employment is negative but statistically insignificant, which is consistent with some crowding out due to the increase in local government spending.

There is also a differential effect on income (wage and salaries), as we find that upgraded municipalities' income increases significantly more than non-upgraded municipalities following the ratings recalibration. The differential effect on income is about 10% following the recalibration.

A concern about inferences from the differences-in-differences framework is whether the processes generating the treatment and control group outcomes follow parallel trends prior to the treatment. In order to address this concern, we consider the evolution of the economic outcomes (employment and income) in the years leading to the treatment separately for the treatment and control groups. We show that government employment, private employment, and income follow similar trends across upgraded and non-upgraded municipalities in the years before the

recalibration. Next, in the year of the recalibration, upgraded municipalities' private employment and income increase significantly more than those of non-upgraded municipalities. Two years after the recalibration, the employment and income processes of the two groups follow again similar dynamics. Thus, we identify an effect on employment and income, exactly at the time of the recalibration, indicating that local governments have used the positive shock to credit supply to create (or save) jobs in the public and private sector.

This study contributes to the literature that uses cross-sectional variation to estimate fiscal multipliers, which differs from the traditional empirical macroeconomics literature which relies on time series variation (see Ramey (2011) and Fuchs-Schuendeln (2015) for surveys). The long-standing debate on the effects of public spending on economic outcomes and the size of the fiscal multiplier has been revived by the American Recovery and Reinvestment Act of 2009.

Several recent papers use cross-sectional geographic variation in government spending to estimate government spending multipliers, exploiting the fact that nationwide factors are independent of the differential effects on spending and economic outcomes across regions. Nakamura and Steinsson (2014) use regional variation in U.S. military spending and estimate a state-level multiplier of 1.5. Serrato and Wingender (2014) exploit variation in federal spending directed to counties due to changes in the count of local population after each Decennial Census and estimate a local income multiplier of 1.57 and a cost per job of $30,000. Shoag (2013, 2015) uses differences in returns to state pension funds as windfall shocks to state finances that predict subsequent spending patterns, and estimate a state-level spending multiplier above 2 and a cost per job of $35,000.[4] Chodorow-Reich, Feiveson, Liscow, and Woolston (2012) use pre-crisis

[4] The literature uses two other interesting natural experiments to identify the impact of government spending on the private sector. Cohen, Coval, and Malloy (2011) use changes in congressional committee chairmanships as a source of exogenous variation in state-level federal expenditures. Acconcia, Corsetti, and Simonelli (2014) uses a law

state-level Medicaid spending to extract the exogenous component of state fiscal relief during the 2009 American Recovery and Reinvestment Act (ARRA), and find a cost per job of $25,000 and a local spending multiplier of 2.[5]

We use the exogenous variation in local government spending identified by the ratings recalibration to measure the causal impact of local government spending on economic outcomes at the local level. Our estimates suggest that a marginal million dollars in local government financing or spending results in 26 net jobs, 23 of which are outside of the government sector. This estimate corresponds to a cost per job created of $38,000. Our estimates also indicate that each marginal dollar of financing or spending raises local income by $4. Our estimates, especially for income, are at the upper end of the range in the literature, consistent with recent empirical work on state dependent multipliers that finds higher multipliers during depressed demand conditions such as the one prevailing during our sample period (Auerbach and Gorodnichenko (2012)).

This study is also related to papers that examine the role of municipal bonds and local government spending in providing infrastructure and public services. Cellini, Ferreira, and Rothstein (2010) estimate the valuation of investments in school facilities in California by comparing housing prices on school districts where referenda on municipal bond issues that passed and failed by narrow margins. They find treatment effects of 6% or more, and implied valuations of $1.50 or more for $1 in school capital spending.

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passed to combat political corruption and Mafia infiltration of city councils in Italy as source of variation in local public spending.

[5] A few other papers 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. He finds a cost per job created of around $125,000. Conley and Dupor (2013) find a positive effect of ARRA transfers on government employment, but no positive effect on employment outside of government. Mian and Sufi (2012) find that the relatively small ($3 billion) "Cash for Clunkers" program (which was separate from the ARRA but implemented concurrently during the summer of 2009) had little net effect on purchases.

We also contribute to the literature on the effect of credit market shocks on economic outcomes. Chodorow-Reich (2014) examines disruptions in the syndicated loan market following the collapse of Lehman Brothers in 2008. He shows that firms with pre-crisis lending relationships with weaker banks face restrictions in credit supply, which lead to reductions in employment. Greenstone, Mas, and Nguyen (2014) find that shocks to the supply of bank credit to small businesses during the 2007-2009 financial crisis are associated with reductions in county-level employment. While these papers study local economic effects of shocks to *private* sector credit supply, we study shocks to *public* sector credit supply.

Finally, we contribute to the literature on the real effects of credit ratings. Credit ratings matter to issuing firms including cost of capital, capital structure, and investment decisions (Faulkender and Petersen (2006), Kisgen (2006, 2009), Sufi (2009), Tang (2009), Kisgen and Strahan (2010), Chernenko and Sunderam (2012), Almeida, Cunha, Ferreira, and Restrepo (2014)). While these papers study the effects of corporate ratings on *firm* outcomes, we study the effects of local government ratings on *economic* outcomes.

2. Sample and Variables Description

2.1 Recalibration Event

Moody's Municipal Rating Scale historically measures how likely a municipality (i.e., a local government unit) is to require extraordinary support from a higher level of government in order to avoid default. In contrast, Moody's Global Rating Scale is designed to measure expected losses among sovereign bonds, corporate bonds, and structured finance products (Moody's (2007)). Moody's (2009) attributes its dual rating system to the preferences of the highly risk averse investors in municipal bonds. Using U.S. Flow of Funds Accounts in 2010, the household

sector owns 50% of the municipal debt market, followed by money market funds with 10% and insurance companies with 9%. In contrast, households hold only 19% of corporate and foreign bonds.

In March 2010, Moody's announced a recalibration of its Municipal Rating Scale to align it with the Global Rating Scale. In April and May of 2010, over a four week period, Moody's announced a zero-to-three notch upgrade of all municipal bond issues it rated. Moody's (2010) clarifies that the ratings revision is intended to enhance the comparability of ratings across asset classes, and that it does not indicate a change in intrinsic 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 indicates that any ratings under review for upgrade or downgrade prior to recalibration would remain under review, and would not be lumped into these massive rating changes. Thus, our sample does not include any natural upgrades associated with improving issuer fundamentals that could contaminate our estimates.

We obtain a list of recalibrated bonds issues from Moody's. The list contains the rating of each issue before and after the recalibration, with the change in rating ranging from zero to three notches. Cornaggia, Cornaggia, and Israelsen (2015) provide a detailed description of the Moody's municipal bond ratings recalibration. They report that 645,130 municipal bonds (with a total par amount of $2,211 billion) were recalibrated by Moody's of which about 79% were

upgraded.[6]

2.2 Data and Summary Statistics

The first set of tests study the effects of the ratings recalibration on municipal bond new issues. We estimate the equivalent of first-stage tests in our setting, where the dependent variables are the Moody's credit rating, the dollar amount, and offer yield of new bond issues in the municipal bond market at the issue level. 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 period starting three years before the recalibration and until three years after. The sample of new bonds issues is restricted to issues rated by Moody's and municipalities that issue bonds in the three years before the recalibration.[7]

Since we measure local economic outcomes (employment and income) at the county level, we restrict the analysis of the recalibration to issues that can be matched to a county. These include issues by counties (including boroughs and parishes), cities, townships (including towns, and villages), school districts, and special districts. We exclude state-level bond issues as they cannot be attributed to a specific county. The treatment group is composed of issuers that had any of its outstanding bonds recalibrated by Moody's between April and May of 2010. Because credit ratings on insured bonds reflect the credit quality of the insurer rather than the issuer, we follow Cornaggia, Cornaggia, and Israelsen (2015) and include only uninsured bonds in our analysis (roughly 60% of the municipal bonds are uninsured).

Panel A of Table 1 presents summary statistics of the issue amount and offer yield in the

[6] About one-third of the municipal bond issues have a Moody's rating, one-third has an S&P rating, and one-third has both Moody's and S&P ratings.

[7] We obtain similar estimates when we restrict the sample of new issues to municipalities that issue bonds in both the period of three years before the recalibration and the period of three years after, independent of which agency assigns them a rating.

sample of new issues. Issues by upgraded municipalities represent about 75% of the sample of new bond issues. The average bond issue in the sample (from April 2007 to March 2013) has a par amount of $4.5 million, but the distribution is highly skewed with a median of $0.8 million. The offer yield is 2.8% on average, with a median of 2.9%.

The primary outcome variables are county-level government employment, private employment, and income. We obtain local government employment data from the Census 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 (i.e., counties, cities, townships, school districts, and special districts) within each county as of the week of March 12 of each year. Our analysis is restricted to counties that are present in all years of our sample period (2007-2012).[8]

We obtain data on private-sector employment by industry (NAICS) and county from the County Business Patterns (CBP) published by the U.S. 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 is defined as total wages and salaries in the county in a given calendar year.

Panel B of Table 1 presents summary statistics on our outcome variables (local government employment, private employment, and income at the county level) both in levels and growth (log change) from 2007 to 2012. Counties in the sample have an average of 4.5 thousand government employees, and the median is 0.7. Government employment average annual growth rate during this period is negative at -0.4%. Private employment is much larger than government

[8] Our sample of counties represents about half of the universe of counties.

employment at 66 thousand employees in total. We separately track tradable and non-tradable employment using the Mian and Sufi (2014) classification based on four-digit NAICS industry codes.[9] Employment in the tradable sector (NAICS 31-33; manufacturing) is about 5% of total in these counties (3.4 thousand employees), and employment in the non-tradable sector (NAICS 44-45 and 72; retail, food and accommodation) is about 17% of total (11 thousand employees). The average drop in employment over the sample period is more severe in the non-tradable sector (-14.8%) than in the tradable sector (-4.7%). The income average annual growth rate is about 2.6%.

The final three rows present summary statistics on county-level explanatory variables. The main explanatory variable is the fraction of local government units in a given county that has been upgraded during the Moody's recalibration event (*Recalibrated*). In our tests, we control for other factors that are important determinants of employment and income. The housing prices come from the Federal Housing Finance Agency (FHFA) *House Price Index* (HPI) data at the Metropolitan Statistical Area (MSA) level. The FHFA HPI is a weighted, repeat-sales index, and measures the average price changes in repeat sales or refinancing on the same properties.[10] We obtain county-level information on the number of households from the 2007 Census Bureau Summary Files. The *Households* variable is defined as one or more people that occupy a given housing unit.

Figure 1 is a map of the United States with counties' shading reflecting the quintiles of the *Recalibrated* variable. The *Recalibrated* variable is well spread across the United States.

[9] The method defines retail- and restaurant-related industries as "non-tradable" (NAICS codes in sectors 44-45 and 72), and industries which show up in global trade data as "tradable" (those in two-digit NAICS sectors 31-33). Remaining industries are classified as "construction" or "other".

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

3. Municipal Bond Market Outcomes

We start by examining the effects of the ratings recalibration on the access of local governments to the municipal bond market. We study the effect of the Moody's recalibration on bond ratings, as well as quantities and prices in the municipal bond market. We compare the bond rating, amount issued, and offer yield of recalibrated municipalities (the treatment group) and non-recalibrated municipalities (the control group) in a period of three years after the recalibration relative to three years before. We first obtain issue-level differences-in-differences estimates using new issues data from Ipreo i-Deal between April 2007 and March 2013. We then estimate county-level regressions for bond market outcomes since our economic outcomes are measured at the county level.

The main explanatory variables are as follows: (1) a dummy variable that takes a value of one if an issuer experienced an upgrade in any of its outstanding (uninsured) bonds during the Moody's recalibration event (*Recalibrated Dummy*); (2) a dummy variable that takes a value of one between April 2010 and March 2013 (*Post Dummy*); and (3) the interaction term *Recalibrated Dummy* × *Post Dummy*. The analysis is done within issuer, i.e., we include issuer fixed effects in all regressions, which means that the direct effect on the *Recalibrated Dummy* is not identified. The regressions also include year-event fixed effects. Standard errors are clustered at the issuer level to correct for within-issuer residual correlation.

Table 2 presents estimates of bond issue-level regressions of the effects on the municipal bond market. Column (1) presents estimates in which the dependent variable is the Moody's rating. In order to perform this test, we map the ratings into 22 numerical values, where 22 is the highest rating (AAA), 21 the second highest (AA+), and one the lowest (default). We find that the interaction term *Recalibrated Dummy* × *Post Dummy* coefficient is positive and significant,

which indicates that the recalibration has a disproportional effect on the Moody's ratings of the treatment group relative to the control group. The estimates suggest that ratings increase 0.5 notches more for the treatment group than for the control group following the recalibration.

About half of our sample of new issues rated by Moody's is simultaneously rated by S&P. Thus, we can use the S&P credit ratings as a placebo test, as S&P does not have a dual-class rating system, and test whether the differential effect on Moody's ratings between treatment and control groups can be explained by other factors besides the recalibration. If the Moody's recalibration does not reflect any change in the intrinsic credit quality of the issuers, we should not find any differential effects on S&P ratings of treatment and control groups around the time of the Moody's recalibration.

Column (2) of Table 2 presents estimates of the placebo test by using the S&P bond ratings for the subsample of bond issues with both S&P and Moody's ratings. We find no significant differential effect on S&P ratings between the treatment and control groups following the recalibration. While the exclusion restriction is not directly testable, this finding is as an important validation of our identification strategy.

Figure 2 compares the effect of the Moody's ratings recalibration on treated and control municipalities ratings from two years before the recalibration up to two years after for both Moody's and S&P ratings. The estimates come from the regressions in columns (1) and (2) of Table 2, replacing the interaction term *Recalibrated Dummy* × *Post Dummy* with dummies for whether a bond issue is in the treated group t years after or t years before the recalibration. Treatment and control groups show no significant differential changes in the two years prior to the recalibration. The treated municipalities then suffer a significantly stronger increase in ratings at the time of the recalibration, a difference that persists for up to two years afterward.

The figure also shows the effect on the S&P ratings of treated and control municipalities at the time of the Moody's recalibration. There are no significant changes in S&P ratings of treated and control municipalities either before or after the recalibration. The evolution of S&P ratings around the ratings recalibration event confirms that the differential effects are not related to channels other than the recalibration.

Column (3) of Table 2 presents estimates in which the dependent variable is the log of the *Issue Amount* (in millions of dollars) and column (4) presents estimates in which the dependent variable is the *Offer Yield* (in percentage). Upgraded municipalities show a large and statistically significant increase in the issue amount after the recalibration. In column (3) the interaction term (*Recalibrated Dummy* × *Post Dummy*) coefficient is 0.113, significant at the 5% level, which indicates that municipalities in the treatment group after the recalibration increase the issue amount 11% more than municipalities in the control group. We find that offer yields of new issues of upgraded municipalities experience a larger reduction after the recalibration than offer yields of non-upgraded issuers. The estimated differential reduction in offer yields is about 14 basis points. The magnitude of the effect on offer yields is similar to that in Cornaggia, Cornaggia, and Israelsen (2015).

Figure 4 compares the effect of the recalibration on the issue amount by treated and control municipalities from two years before the recalibration up to two years after. The estimates come from the regression in column (3), replacing the interaction term *Recalibrated Dummy* × *Post Dummy* with dummies for whether a bond issue is in the treated group *t* years after or *t* years before the recalibration. The figure shows that, in the two years prior to the recalibration, the issue amount of treated and control municipalities is similar. We then see a significantly higher issue amount in the year of the recalibration and in the subsequent years for upgraded

municipalities versus non-upgraded municipalities.

Next we estimate the effects of the recalibration on bond market outcomes at the county level, as the effects on economic outcomes are estimated at the county level (see Section 4). We aggregate the new bond issues data by county and year (years are defined from April of year *t*-1 to March of year *t* to match the recalibration event). The *Dollar Volume* is the sum of the issue amount of new bond issues of local government units in each county and year (in millions of dollars). The *Offer Yield* is the average offer yield across new bond issues of all local government units in each county and year (in percentage). The *Recalibration* variable is the fraction of local government units that has been upgraded in a given county as a result of the recalibration event (columns denoted by "Equal"). Alternatively, the *Recalibration Dummy* is weighted by the dollar volume of new bond issues of each local government unit in the pre-recalibration period (columns denoted by "Amount").

Table 3 presents the estimates of the regression of the log of the *Dollar Volume* (Panel A) and *Offer Yield* (Panel B) at the county level. Columns (1) and (2) present estimates using the equal-weighted *Recalibrated* variable, and columns (3) and (4) present estimates using the *Recalibrated* variable weighted by dollar volume of new bond issues. Upgraded municipalities show a large and statistically significant increase in the issuance volume following the recalibration event. The interaction term (*Recalibrated* × *Post Dummy*) coefficient is 0.226, significant at the 1% level, which indicates that counties in the treatment group increase the dollar volume of new bond issues after the recalibration 23% more than counties in the control group. We find that offer yields of upgraded municipalities decrease significantly more than offer yields of non-upgraded municipalities following the recalibration. The estimated differential reduction in offer yields is 32 to 43 basis points. The county-level estimates in Table

3 are qualitatively similar to the issue-level estimates in Table 2.

Overall, we find that upgraded municipalities after the ratings recalibration raise more bond financing and at a lower cost relative to non-upgraded municipalities

4. Economic Outcomes

To estimate the impact of the ratings recalibration on local economic outcomes, we estimate county-level differences-in-differences regressions of government employment, private employment, and income. We estimate panel regressions using the log of employment or income in each county and year as the dependent variables. The regressions consider two alternative sample periods: 2007-2012 and 2009-2011. The sample includes counties with government employment data in all years of our sample period, and regardless of whether they issue bonds or not during the sample period.

The explanatory variable of interest is the interaction of the *Recalibrated* variable with the *Post Dummy*, which takes a value of one after the recalibration event in April-May 2010. The *Recalibrated* variable is the fraction of local government units in a given county that has been upgraded as a result of the Moody's recalibration event. In the case of the employment variables, the *Post Dummy* variable takes a value of one in 2011 and 2012, as employment in the Census of Government and County Business Patterns data is measured as of the week of March 12 of each year. In the case of the income variable, the *Post Dummy* variable takes a value of one in 2010, 2011, and 2012, as the IRS income variable is measured over the 12-month period that ends in December of each year. The regressions include county fixed effects, as well as year fixed effects and, in some specifications, additional county-level controls. Standard errors are clustered at the county level to account for within-county correlation.

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

4.1 Local Government Employment

We expect the effects of the municipalities' ratings recalibration to be more pronounced in government employment than in private employment, as local governments can use funds to directly hire (or maintain) employees. Columns (1)-(4) of Table 4 present the estimates of differences-in-differences regressions using the log of local government employment as the dependent variable. Columns (1) and (2) present the estimates using the 2007-2012 period and columns (3) and (4) using the 2009-2011 period. Column (5) presents the estimates of the cross-sectional regression using the growth rate (2009-2011) in government employment as the dependent variable.

In column (1), the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at 4%.[11] The estimated differential increase in government employment is higher at 4.9% in column (2) when we include county-level controls. These estimates are, however, statistically insignificant. As expected, the corresponding estimates in columns (3) and (4) that use the shorter window around the recalibration are stronger in magnitude and more precisely estimated. The point estimate of the differential effect on local government employment is 5.8%, significant

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

at the 5% level. The cross-sectional regression in column (5) estimate is also similar at 5.8%.

The estimates in Table 4 indicate that counties in the treatment group increase local government employment by nearly 6% more after the ratings recalibration relative to counties in the control group. The effect is stronger in a shorter window (2009-2011) than on a longer window (2007-2012) around the recalibration event, which indicates that our effect is driven by the recalibration, and that non-treated counties seem to catch up to some extent a few years after the recalibration. The evidence suggests that the recalibration helped upgraded counties to mitigate the large decline in employment during the 2007-2009 Great Recession (Mian and Sufi (2014)), and non-upgraded counties caught up some time after the recession.

A concern about inferences from the treatment effects framework is whether the processes generating the treatment and control group outcomes would have followed parallel trends in the absence of the treatment. Differences in the post-treatment period can only be attributed to the treatment when this assumption holds. While this is not a direct test of the parallel trends assumption, it is standard in the literature to examine the evolution of the outcome variable in the years leading to the treatment separately for the treatment and control groups. If trends are not parallel prior to the event, it is unlikely that post-event differences can be attributed to the treatment.

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

the recalibration. There is no indication of statistical significant pre-existing differential trends.

In short, we find robust evidence of a positive effect of the exogenous credit rating changes of municipal bonds on local government employment. The differential effect between upgraded and non-upgraded counties is about 6% and is both statistically and economically important. The effects seem be unique at the time of the ratings recalibration, which supports a causal interpretation of the effect of shocks to the supply of local public financing on economic outcomes.

To interpret the magnitude of the estimates, we can translate the regression coefficients into the increase in jobs from a marginal million dollars in local government spending. The additional government spending in the period of two years after the recalibration can be calculated as the product of the estimate in column (2) of Table 3, Panel A, by the average annual dollar volume of municipal bond debt issued by the county (see Table 1): 22.4% × $180 million × 2 = $80.6 million. The government job creation in in the period of two years after the recalibration can be calculated as the product of the estimate in column (5) of Table 4 by the average government employment by county: 5.8% × 4,500 = 261 jobs. These estimates indicate that a marginal million dollars in local government spending results in 3 jobs (= 261 / 80.6) in the public sector.

4.2 Private Employment

Next, we study the effects of Moody's municipal bond ratings recalibration on private employment. Columns (1)-(4) of Table 5 present the estimates of differences-in-differences regressions using the log of private employment as dependent variable. Column (5) presents the estimates of the cross-sectional regression using the growth rate in private employment as dependent variable.

In column (1) the interaction term *Recalibrated* × *Post Dummy* coefficient is 5%, significant

at the 1% level. The estimated differential increase in private employment is slightly lower in column (2) when we include county-level controls, but still statistically significant at the 5% level. The corresponding estimates in columns (3) and (4) that use the shorter event window are similar at about 3.5% and statistically significant at the 1% level. The cross-sectional regression in column (5) estimate is similar at 2.8%, and statistically significant at the 10% level.

The estimates indicates that private employment in counties in the treatment group increase by about 3% more after the ratings recalibration relative to counties in the control group. In short, we find evidence of a positive effect of the exogenous credit rating changes of municipal bonds on private employment. The corresponding shock to the supply of public financing seems to generate spillover effects to the private sector with an increase in employment. The magnitude of the effect on private sector employment is lower than the one in the public sector.

Figure 5, Panel A, shows the evolution of the log of private employment before and after the recalibration event for the treatment and control groups. The two groups follow similar trends before the recalibration event. Furthermore, we can see that private employment increases for the treatment group in the year of the recalibration, but stays constant for the control group. Figure 5, Panel B, shows the differential effect of the recalibration between treated and control municipalities from two years before the recalibration up to two years after the recalibration. There is no indication of statistical significant pre-existing differential trends. Additionally, the effect on private employment seems to persist for at least two year, while the effect on government employments seems to be short lived.

We can again translate the regression coefficients into the increase in jobs from a marginal million dollars in local government spending. The private job creation in in the period of two years after the recalibration can be calculated as the product of the estimate in column (5) of

Table 5 by the average private employment by county: 2.8% × 65,700 = 1,840 jobs. These estimates indicate that a marginal million dollars in local government spending results in 23 jobs (= 1,840 / 80.6) in the private sector.

Thus, our estimates suggest that $1 million of spending increase county total employment (government and private) by 26 jobs (= 3 + 23), which corresponds to a cost per job created of $38,000. This estimate is in line with other estimates of local job multipliers in the literature.

4.3 Non-Tradable and Tradable Private Employment

We also study the effects of municipalities' rating upgrades on non-tradable versus tradable sector employment. We expect that the impact of the expansion in government spending due to the rating upgrades and, corresponding expansion in debt capacity, should show up foremost in non-tradable employment. The non-tradable sector in a county depends primarily on local demand, while the tradable sector is more diversified in its geographic origins of demand. We therefore separately track tradable and non-tradable employment using the same four-digit industry classification as in Mian and Sufi (2014).

Panel A of Table 6 presents the estimates for non-tradable sector employment, and Panel B for tradable sector employment. Columns (1)-(4) of Table 6 present the estimates of differences-in-differences regressions, using the log of non-tradable employment and the log of tradable employment as dependent variables. Column (5) presents the estimates of the cross-sectional regression using the growth rate in non-tradable and tradable employment as dependent variable.

In column (1), Panel A, the interaction term *Recalibrated* × *Post Dummy* coefficient is 0.241, significant at the 1% level. The estimated differential increase in non-tradable employment is lower at 0.168 in column (2) when we include county-level controls. The corresponding estimates in columns (3) and (4) that use the shorter event window are lower but remain

economically and statistically significant at about 0.07. The cross-sectional regression in column (5) estimate is 12% and statistically significant at the 1% level. In short, the estimates in Table 6 indicate that non-tradable employment in counties in the treatment group increases about 7% to 12% more after the ratings recalibration relative to the control group.

Panel B presents the estimates for tradable sector employment. In column (1), the interaction term *Recalibrated* × *Post Dummy* coefficient is negative, but statistically insignificant. The estimates are similar in the other specifications. The point estimates are negative but the effect on tradable employment is statistically insignificant.

Overall, we find robust evidence of a positive effect of the exogenous credit rating changes of municipal bonds on non-tradable employment. The differential effect between upgraded and non-upgraded municipalities is 7% to 12% and is statistically and economically important. This is consistent with the notion that the expansion in local government spending mainly benefits the non-tradable sector employment. The shock to non-tradable and government employment can have a crowding-out effect on employment in other sectors, in particular in the tradable sector (as well as lead to higher wages). Indeed, we find a negative differential effect of the ratings recalibration on tradable employment but estimates are imprecisely estimated. Thus, the evidence supports that workers who move into the government sector and non-tradable sector move out from the tradable sector. Of course, there may also be mobility across counties and transfers into and out of the labor force.

The effects of government spending are more likely to occur in sectors that receive transfers and grants from local governments such as the educational services sector and the health care services sector (these sectors are classified as “other”). Table 7 presents differences-in-differences estimates using the 2009-2011 panel and growth in employment in the education and

health sectors separately. Columns (1) and (2) present estimates for the educational services sector, and columns (3) and (4) present estimates for the health services sector.

The *Recalibrated* × *Post Dummy* dummy coefficient is positive and significant in all specifications. The differential effect between upgraded and non-upgraded municipalities is 7% to 11% and is statistically and economically important in the education sector employment. There is also a significant differential effect on health sector employment at 8% to 10%. The differential effects on these two sectors are more pronounced than those in local government employment.

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

4.4 Income

We then study the effects of municipalities' ratings recalibrations on county-level income (i.e., salaries and wages). We expect that the expansion in government and private employment has a positive effect on salaries and wages. Columns (1)-(4) of Table 8 present the estimates of differences-in-differences regressions, using the log of income as dependent variable. Column (5) presents the estimates of the cross-sectional regression using the growth rate in income as dependent variable.

In column (1) the interaction term *Recalibrated* × *Post* coefficient is 0.122, significant at the 1% level. The estimated differential increase in income is similar in column (2) when we include county-level controls. The corresponding estimates in columns (3) and (4) that use the shorter event window are similar. The point estimate of the differential effect on local income is slightly

lower at 11% but remains strongly economically and statistically significant. The cross-sectional regression estimate in column (5) is also similar. The point estimates indicate that income in counties in the treatment group increase by about 10% more after the ratings recalibration relative to the control group.

Figure 6, Panel A, shows the evolution of the log of income in the two years before and after the recalibration event for the treatment and control groups. The income processes of the two groups follow similar trends before the recalibration. Furthermore, income increases significantly for the treatment group in the year of the recalibration, but the increase is much lower for the control group. In the two years following the recalibration, the income processes again follow similar dynamics. Figure 6, Panel B, shows the differential effect of the recalibration between treated and control municipalities from two years before the recalibration up to two years after the recalibration. There is no indication of significant pre-existing differential trends, and the differential effect on income becomes significant after the recalibration and seems to persist for at least two years.

In short, we find robust evidence of a positive effect on local income of the exogenous credit rating changes of municipal bonds. The differential effect between upgraded and non-upgraded municipalities is about 10% and is statistically and economically important. The effects seems be unique at the time of the ratings recalibration, which supports a causal interpretation of the effect of shocks to the supply of local public financing on local income.

The marginal increase in income in the period of two years after the recalibration can be calculated as the product of the estimate in column (5) of Table 8 by the average county income: $9.9\% \times \$3{,}267 = \$323$ million. We find an estimate of the local income multiplier, the change in local aggregate income produced by a one dollar change in local government spending, of 4.

This estimate is at the upper end of the range in the literature, consistent with the notion that fiscal multipliers are higher during depressed demand conditions such as the one prevailing during our sample period.

4.5 Robustness

We estimate the impact of the ratings recalibration on local economic outcomes using a sample that includes all counties regardless of whether they issue new bonds in the municipal bond market during our sample period. Thus, the control group may include counties that are less financially constrained as they have no need to issue debt. This should bias against finding an effect of the recalibration, as the control group includes higher quality and less financially constrained counties. To further address this concern, we run the regressions of government employment, private employment, and income by using a sample of counties that includes only those with at least one local government unit that issue new bonds in the municipal bond market in both the three year period before and the three year period after the recalibration event.

Table 9 presents the results using only the 2009-2011 panel to conserve space. Column (1) presents the estimates for government employment. The estimates indicate that counties in the treatment group increase local government employment by nearly 7% more after the ratings recalibration relative to counties in the control group. As expected, this estimate is similar to that in Table 4. Column (2) presents the estimates for private employment, and columns (3) and (4) for non-tradable and tradable employment, respectively. The private employment estimates are consistent with those in Table 5 with a differential effect of about 3% between treatment and control group. This increase in private employment is concentrated in the non-tradable sector with a differential effect of about 8%, which is again consistent with the estimate in Table 6. Column (5) shows that the differential effect on income is positive at 11%, which is similar to

the estimate in Table 8. In short, the estimates using a sample restricted to municipalities that issue new bonds during the sample period are quantitatively similar to our baseline estimates.

A second robustness tests consists of restricting the sample to counties that have at least one local government unit with a bond issue rated by Moody's. In this test, we also restrict the control group to counties with at least one local government unit that issue new bonds in the municipal bond market in both the three year period before and the three year period after the recalibration event.

Table 10 presents the results using only the 2009-2011 panel to conserve space. We find that the estimates in column (1) of the effect of the ratings recalibration on government employment are similar to those in Table 4. Government employment at upgraded municipalities increases by 7% more than in non-upgraded municipalities. The effect is statistically significant at the 5% level. Column (2) shows that the magnitude of the effect on private employment is lower but still economically sizable at 1%. Columns (3) and (4) show estimates for non-tradable and tradable employment. The differential effect on non-tradable employment is 5%, which is similar to the estimate in Table 6. Column (5) shows that the differential effect on income is important at about 10%, which is similar to the estimate in Table 8.

Overall, the estimates using a sample restricted to municipalities rated by Moody's are quantitatively similar to our baseline estimates with the exception of private employment. When we restrict the sample to municipalities rated by Moody's, private sector employment presents a lower differential effect due to a larger drop in tradable employment

5. Conclusion

We provide causal estimates of the effect of shocks to the supply of local governments'

financing on economic outcomes by exploring the exogenous variation in credit ratings due to the Moody's recalibration of its municipal bond ratings scale. The recalibration generates cross-sectional variation in ratings across municipalities, with a zero-to-three notch upgrade of municipal bond issues. Following the ratings recalibration, upgraded municipalities raise more bond financing and experience reductions in their borrowing costs than non-upgraded municipalities.

This asymmetric effect to municipalities' ratings leads to greater increases in government employment and private employment of upgraded municipalities relative to non-upgraded municipalities. The private employment differential increase is concentrated in the non-tradable sector, which is more directly dependent on local demand, and education and health sectors, which typically receive government transfers. In contrast, we find greater reductions in tradable employment of upgraded municipalities than non-upgraded municipalities. Thus, there is evidence of a shift in jobs from the tradable sector to the local government, as well as non-tradable, educational services, and health services sectors. Income also responds in a significant way to the positive shock to local government liquidity.

Our findings show that changes in the supply of financing to local governments have important effects on the local economy. The effects are driven specifically by changes in credit ratings of municipal bonds, and not by changes in local or nationwide fundamentals. The recalibration of the municipal bond rating scale contributes to an improvement in the information environment and a reduction in ratings-based regulatory compliance costs, which expands the debt capacity of municipalities.

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Table 1
Summary Statistics

This table shows mean, median, standard deviation, minimum, maximum, and number of observations for each variable. The sample in Panel A consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013. The sample in Panel B consists of observations on counties from 2007 to 2012.

	Mean	Median	Standard Deviation	Minimum	Maximum	Observations
Panel A: Issue-Level Variables						
Issue Amount ($ million)	4.5	0.8	24.1	0.0	3,000.0	202,615
Offer Yield (%)	2.8	2.9	1.5	0.0	11.0	202,615
Panel B: County-Level Variables						
Dollar Volume ($ million)	180.5	16.1	1,557.7	0.0	66,400.0	5,974
Offer Yield (%)	2.8	2.9	1.6	0.0	9.1	5,974
Government Employment (thousand)	4.5	0.7	14.7	0.0	380.7	8,791
Private Emploment (thousand)	65.7	16.8	178.2	0.0	3,910.4	8,791
Tradable Employment (thousand)	3.4	0.2	14.5	0.0	417.5	8,791
Non-Tradable Employment (thousand)	11.3	2.7	29.8	0.0	685.6	8,791
Income ($ thousand)	3,266.9	790.1	8,677.0	11.0	197,206.3	8,791
Growth Government Employment	-0.004	0.000	0.137	-3.584	1.427	7,269
Growth Private Employment	-0.009	-0.006	0.057	-0.660	0.632	7,283
Growth Tradable Employment	-0.047	-0.032	0.537	-5.370	5.348	4,833
Growth Non-Tradable Employment	-0.148	-0.038	0.338	-3.835	3.288	7,196
Growth Income	0.026	0.027	0.054	-1.387	1.417	7,323
Recalibrated	0.050	0.000	0.084	0.000	1.000	8,791
Households (thousand)	58.4	20.2	142.0	0.5	3,133.8	8,791
House Price Index	252.7	243.5	87.2	101.4	684.5	8,791

Table 2
Differences-in-Differences Estimates of Ratings, Issue Amount and Offer Yield around the Recalibration – Issue Level

This table presents issue-level differences-in-differences estimates of Moody's ratings, S&P ratings, log issue amount and offer yield around the Moody's recalibration event in April and May 2010. The *Recalibrated Dummy* takes a value of one if an issuer experienced an upgrade in any of its outstanding bonds during the Moody's recalibration event. The *Post Dummy* takes a value of one between April 2010 and March 2013. The sample consists of observations on Ipreo i-Deal municipal new bond issues rated by Moody's from April 2007 to March 2013. Robust standard errors clustered at the issuer level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	Rating Moody's	Rating S&P	Issue Amount (log)	Offer Yield
Recalibrated Dummy × Post Dummy	0.502***	-0.067	0.113**	-0.141***
	(0.053)	(0.064)	(0.045)	(0.045)
Year-event fixed effects	Yes	Yes	Yes	Yes
Issuer fixed effects	Yes	Yes	Yes	Yes
R-squared	0.830	0.820	0.570	0.35
Observations	220,109	118,145	202,615	220,109

Table 3
Differences-in-Differences of Volume and Offer Yield of New Bond Issues around the Recalibration – County Level

This table presents county-level differences-in-differences estimates of panel regressions of the log of dollar volume and offer yield of new bond issues around the Moody's recalibration event in April and May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county using equal weights or issue amount weights during the Moody's recalibration event. The *Post Dummy* takes a value of one between April 2010 and March 2013. The sample consists of observations on Ipreo i-Deal municipal new bond issues from April 2007 to March 2013 aggregated by county and event year. Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)
Panel A: Dollar Volume (log)				
Recalibrated × Post Dummy	0.226***	0.224***	0.228***	0.227***
	(0.072)	(0.072)	(0.067)	(0.068)
Weights	Equal	Equal	Amount	Amount
Controls	No	Yes	No	Yes
Year-event fixed effects	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes
R-squared	0.830	0.830	0.830	0.830
Observations	5,974	5,968	5,974	5,968
Panel B: Offer Yield				
Recalibrated × Post Dummy	-0.421***	-0.426***	-0.316***	-0.318***
	(0.104)	(0.105)	(0.099)	(0.099)
Weights	Equal	Equal	Amount	Amount
Controls	Yes	Yes	Yes	Yes
Year-event fixed effects	Yes	Yes	Yes	Yes
County fixed effects	No	Yes	No	Yes
R-squared	0.500	0.500	0.490	0.490
Observations	5,974	5,968	5,974	5,968

Table 4
Differences-in-Differences of Government Employment around the Recalibration

This table presents county-level differences-in-differences estimates of the log of government employment around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011 and 2012. The sample consists of observations on counties from 2007 to 2012 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
					Growth
	Panel 2007-2012		Panel 2009-2011		2009-2011
Recalibrated × Post Dummy	0.040	0.049	0.058**	0.058**	
	(0.035)	(0.034)	(0.028)	(0.028)	
Recalibrated					0.058*
					(0.035)
Controls	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No
County fixed effects	Yes	Yes	Yes	Yes	No
R-squared	0.011	0.011	0.022	0.022	0.001
Observations	8,757	8,751	4,380	4,377	1,458
Number of counties	1,462	1,461	1,462	1,461	

Table 5
Differences-in-Differences of Private Employment around the Recalibration

This table presents county-level differences-in-differences estimates of the log of private employment around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011 and 2012. The sample consists of observations on counties from 2007 to 2012 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
	Panel 2007-2012		Panel 2009-2011		Growth 2009-2011
Recalibrated × Post Dummy	0.050***	0.033**	0.034***	0.037***	
	(0.016)	(0.015)	(0.012)	(0.012)	
Recalibrated					0.028*
					(0.014)
Controls	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No
County fixed effects	Yes	Yes	Yes	Yes	No
R-squared	0.260	0.266	0.070	0.078	0.016
Observations	8,777	8,771	4,389	4,386	1,462
Number of counties	1,467	1,466	1,465	1,464	

Table 6
Differences-in-Differences of Non-Tradable and Tradable Sectors Employment around the Recalibration

This table presents county-level differences-in-differences estimates of the log of non-tradable and tradable sectors employment around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011 and 2012. The sample consists of observations on counties from 2007 to 2012 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
	Panel 2007-2012		Panel 2009-2011		Growth 2009-2011
Panel A: Non-Tradable Employment					
Recalibrated × Post Dummy	0.241***	0.168***	0.066*	0.071*	
	(0.056)	(0.054)	(0.040)	(0.040)	
Recalibrated					0.122***
					(0.047)
Controls	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No
County fixed effects	Yes	Yes	Yes	Yes	No
R-squared	0.663	0.667	0.139	0.139	0.005
Observations	8,696	8,690	4,361	4,358	1,446
Number of counties	1,466	1,465	1,464	1,463	
Panel B: Tradable Employment					
Recalibrated × Post Dummy	-0.103	-0.078	-0.121	-0.114	
	(0.154)	(0.153)	(0.224)	(0.225)	
Recalibrated					-0.295
					(0.283)
Controls	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No
County fixed effects	Yes	Yes	Yes	Yes	No
R-squared	0.061	0.061	0.012	0.012	0.01
Observations	6,179	6,173	3,074	3,071	959
Number of counties	1,184	1,183	1,116	1,115	

Table 7
Differences-in-Differences of Education and Health Sectors Employment around the Recalibration

This table presents county-level differences-in-differences estimates of the log of non-tradable and tradable sectors employment around the Moody's recalibration event in April and May 2010. Columns (1) and (3) present estimates of panel regressions and columns (2) and (4) present estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011. The sample consists of observations on counties from 2009 to 2011 (as of March of each year). Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)
	Educational Services		Health Care and Social Assistance	
Recalibrated × Post Dummy	0.070*		0.077***	
	(0.040)		(0.018)	
Recalibrated		0.119**		0.095***
		(0.052)		(0.028)
Controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	No	Yes	No
County fixed effects	Yes	No	Yes	No
R-squared	0.033	0.006	0.041	0.010
Observations	2,392	746	4,050	1,322
Number of counties	871		1,379	

Table 8
Differences-in-Differences of Income around the Recalibration

This table presents county-level differences-in-differences estimates of the log of income around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2010, 2011, and 2012. The sample consists of observations on counties from 2007 to 2012 (calendar year). Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
					Growth
	Panel 2007-2012		Panel 2009-2011		2009-2011
Recalibrated × Post Dummy	0.122***	0.117***	0.106***	0.107***	
	(0.019)	(0.019)	(0.016)	(0.016)	
Recalibrated					0.099***
					(0.019)
Controls	No	Yes	No	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	No
County fixed effects	Yes	Yes	Yes	Yes	No
R-squared	0.611	0.612	0.700	0.700	0.016
Observations	8,814	8,808	4,407	4,404	1,469
Number of counties	1,469	1,468	1,469	1,468	

Table 9
Sample of Counties with New Bond Issues

This table presents county-level differences-in-differences estimates of panel regressions of the log of government employment, private employment, non-tradable employment, tradable employment and income around the Moody's recalibration event in April and May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011 in the case of employment and 2010 and 2011 in the case of income. The sample consists of observations on counties from 2009 to 2011 (measured as of March of each year for employment and using calendar year for income). The sample is restricted to counties with local government units with new bond issues in Ipreo i-Deal from April 2007 to March 2013. Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
	Government Employment	Private Employment	Non-Tradable Employment	Tradable Employment	Income
Recalibrated × Post Dummy	0.069**	0.026**	0.084***	-0.163	0.109***
	(0.029)	(0.012)	(0.031)	(0.234)	(0.017)
Controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.038	0.125	0.020	0.016	0.805
Observations	3,160	3,164	3,163	2,559	3,171
Number of counties	1,054	1,055	1,056	905	1,057

Table 10
Sample of Counties with New Issues Rated by Moody's

This table presents county-level differences-in-differences of panel regressions of the log of government employment, private employment, non-tradable employment, tradable employment and income around the Moody's recalibration event in April and May 2010. *Recalibrated* is the fraction of local government units that has been upgraded in each county during the Moody's recalibration event. The *Post Dummy* takes a value of one in 2011 in the case of employment and 2010 and 2011 in the case of income. The sample consists of observations on counties from 2009 to 2011 (measured as of March of each year for employment and using calendar year for income). The sample is restricted to counties with local government units with new bond issues in Ipreo i-Deal rated by Moody's from April 2007 to March 2013. Robust standard errors clustered at the county level are reported in parentheses. ***,**,* indicates significance at the 1%, 5% and 10% level respectively.

	(1)	(2)	(3)	(4)	(5)
	Government Employment	Private Employment	Non-Trade Employment	Trade Employment	Income
Recalibrated × Post Dummy	0.068**	0.012	0.053*	-0.237	0.095***
	(0.026)	(0.013)	(0.029)	(0.259)	(0.020)
Controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
County fixed effects	Yes	Yes	Yes	Yes	Yes
R-squared	0.054	0.116	0.022	0.0506	0.801
Observations	2,536	2,535	2,539	2,164	2,541
Number of counties	846	845	847	757	847

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

The map shows quintiles of the fraction of local government units in a given county that has been upgraded during the Moody's recalibration event (*Recalibrated*). Regions range from white (no local government unit in the county is recalibrated) to dark blue (highest quintile of the recalibration variable). Grey counties are those with no local government unit issuing bonds in the three years prior to the recalibration event in Ipreo i-Deal database.

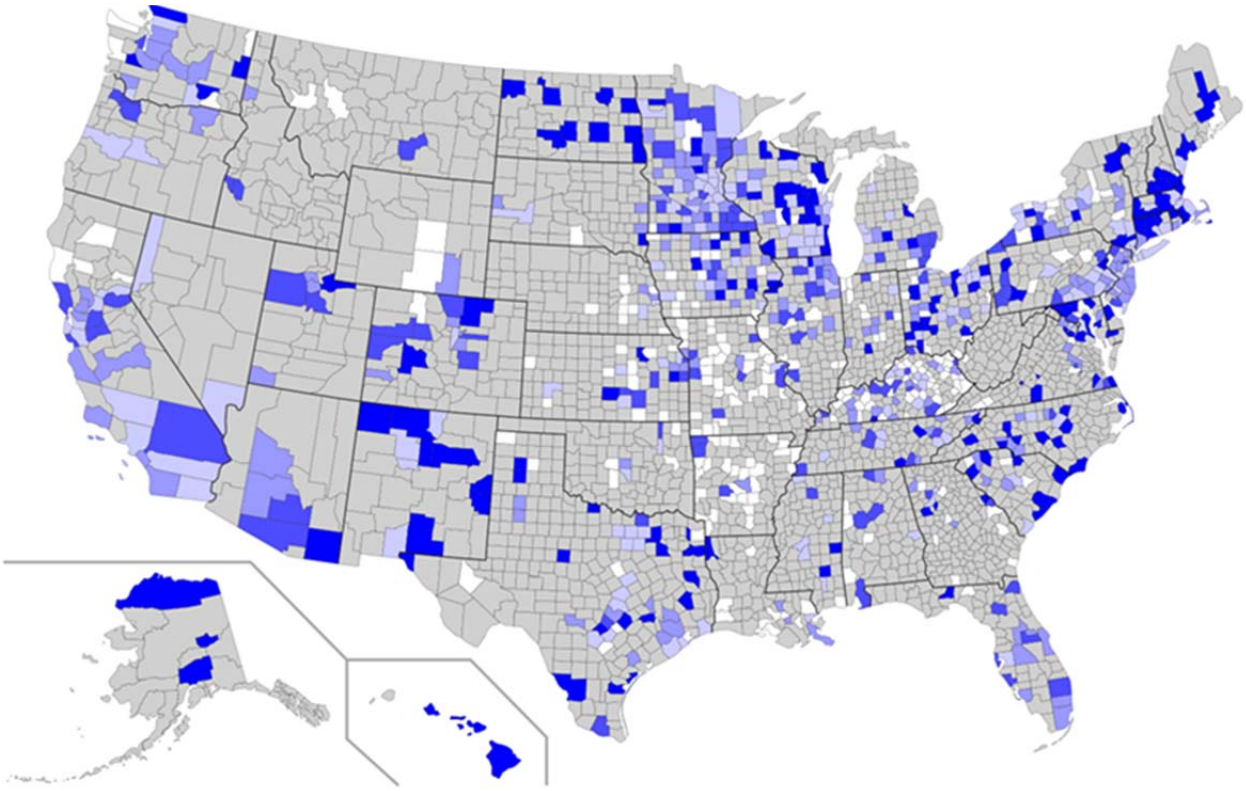

Figure 2
Moody's and S&P Rating around the Recalibration

This figure shows issue-level point estimates and 90% confidence intervals for the effect on the S&P and Moody's ratings of upgraded municipalities (treated) relative to non-upgraded municipalities (control) during the Moody's recalibration event in April and May 2010 (between event year -1 and event year 0).

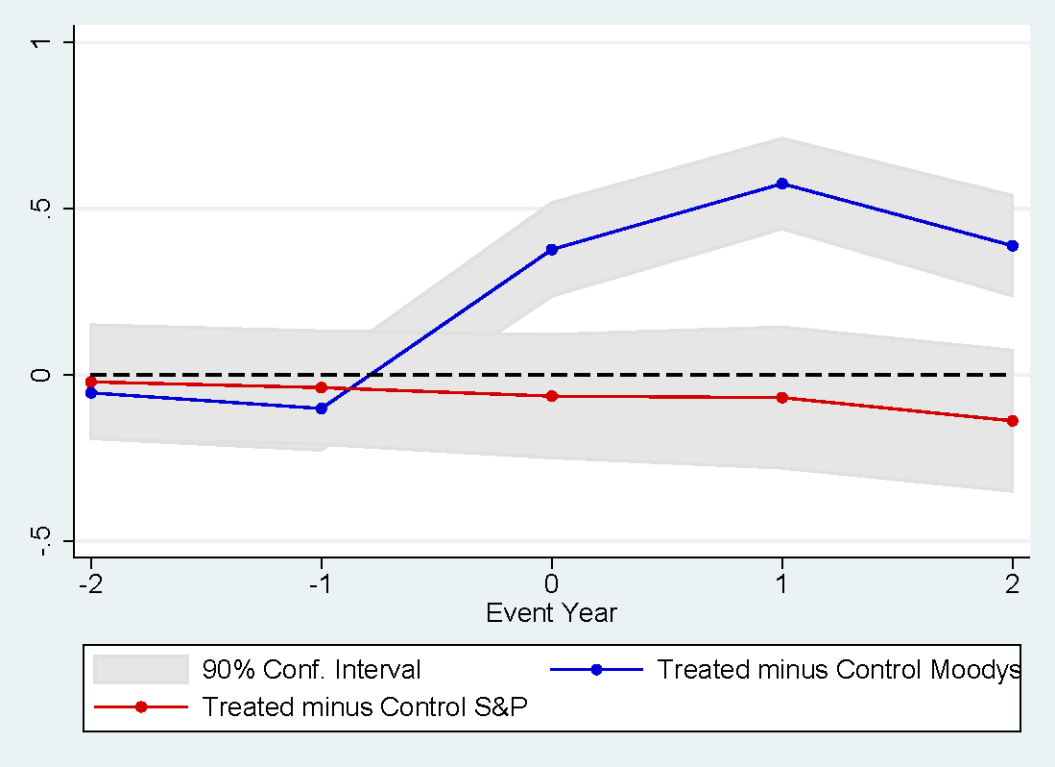

Figure 3
Issue Amount around the Recalibration

This figure shows issue-level point estimates and 90% confidence intervals for the effect on the log of issue amount of upgraded municipalities (treated) relative to non-upgraded municipalities (control) during the Moody's recalibration event in April and May 2010 (between event year -1 and event year 0).

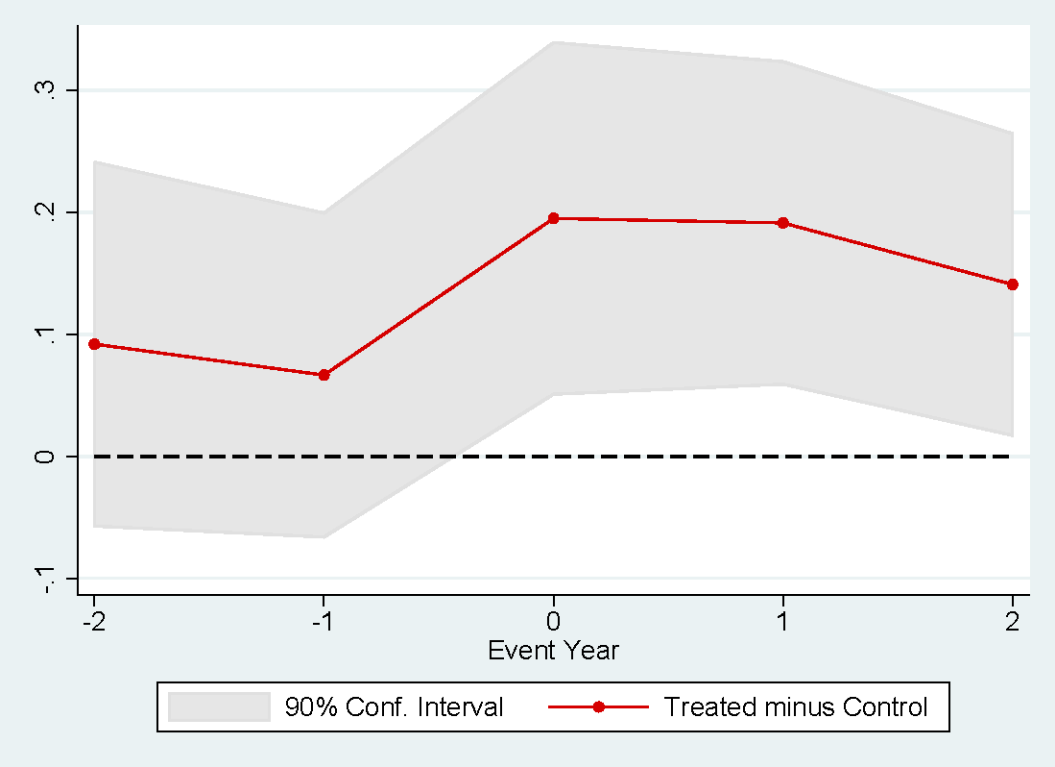

Figure 4
Government Employment around the Recalibration

This figure shows county-level parallel trends (Panel A) and point estimates and 90% confidence intervals (Panel B) for the effect on log government employment of upgraded counties (treated) relative to non-upgraded counties (control) during the Moody's recalibration event in April and May 2010 (between event year -1 and event year 0).

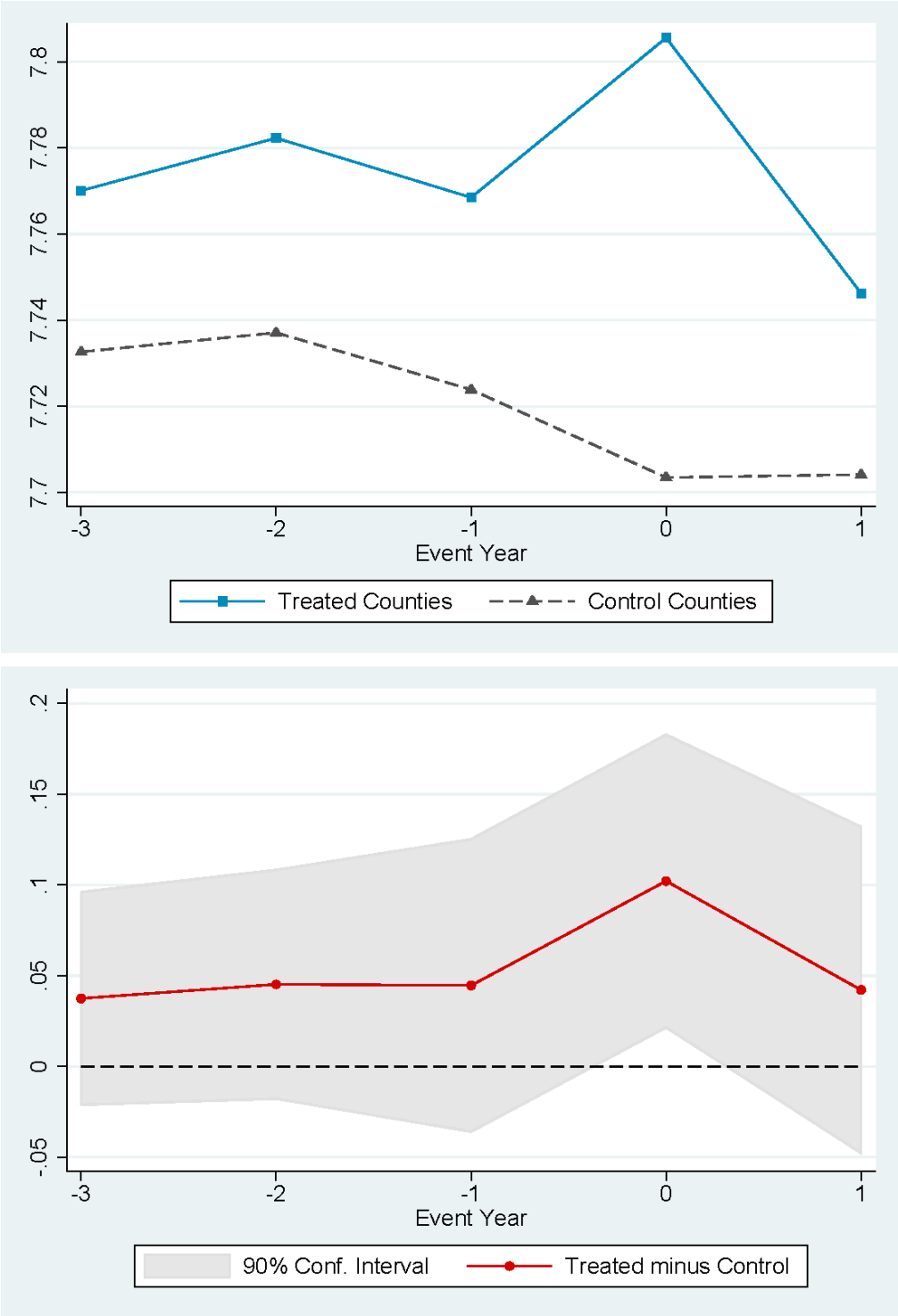

Figure 5
Private Employment around the Recalibration

This figure shows county-level parallel trends (Panel A) and point estimates and 90% confidence intervals (Panel B) for the effect on log private employment of upgraded counties (treated) relative to non-upgraded counties (control) during the Moody's recalibration event in April and May 2010 (between event year -1 and event year 0).

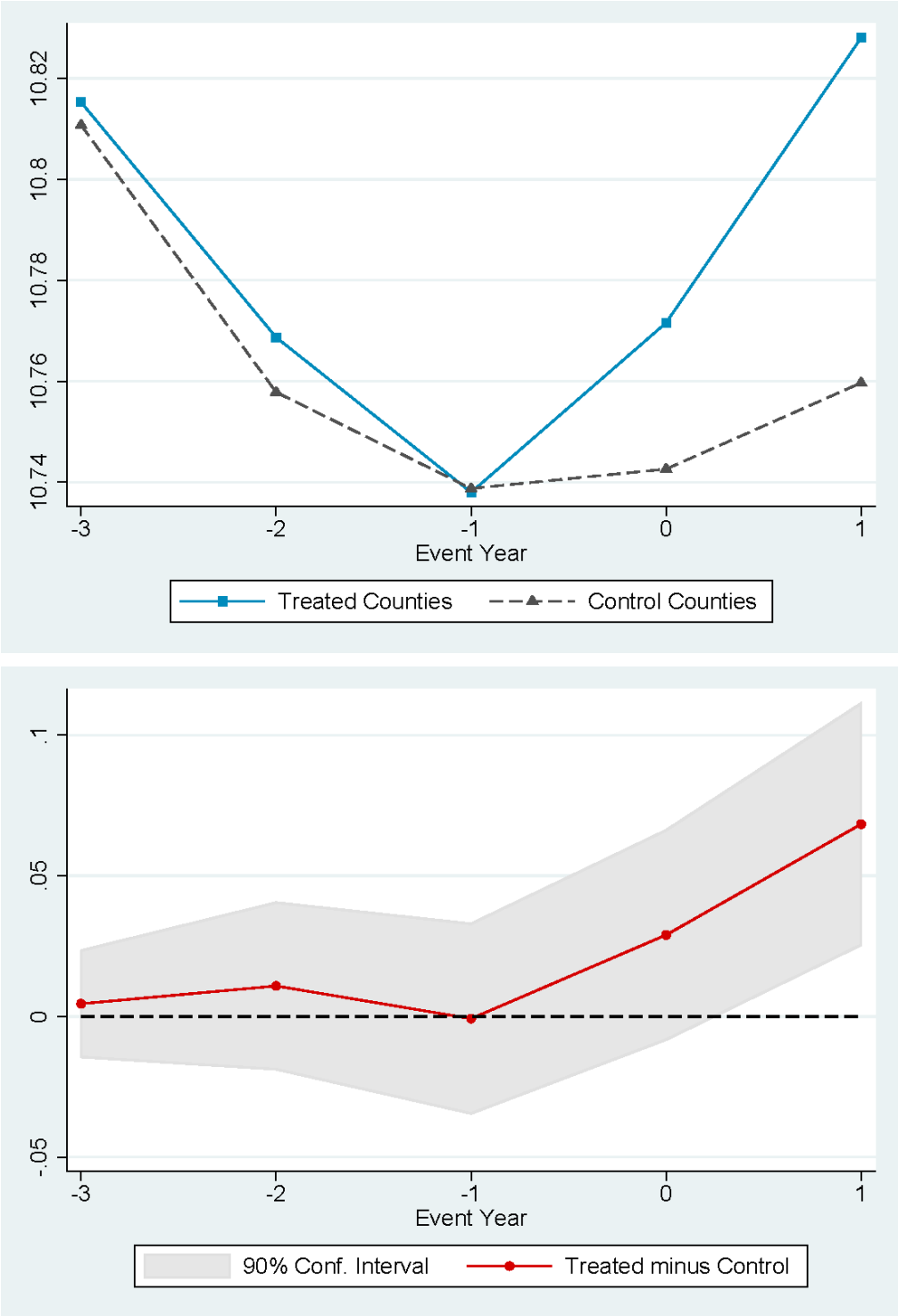

Figure 6
Income around the Recalibration

This figure shows county-level parallel trends (Panel A) and point estimates and 90% confidence intervals (Panel B) for the effect on log income of upgraded counties (treated) relative to non-upgraded counties (control) during the Moody's recalibration event in April and May 2010 (between event year -1 and event year 0).