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

Manuel Adelino
Duke University

Igor Cunha
Nova School of Business and Economics

Miguel A. Ferreira
Nova School of Business and Economics, ECGI

This Version: February 2015

Abstract

We study how changes in the supply of local public financing affect economic outcomes by exploring Moody's municipal bond credit ratings scale recalibration. Following the ratings recalibration, upgraded municipalities increase bond issuance and experience a reduction in their borrowing costs relative to non-upgraded municipalities. This exogenous shock to the supply of credit to local governments leads to greater increases in local government employment, private sector employment, and total income of upgraded municipalities relative to otherwise similar municipalities that are not upgraded. Private sector job creation is concentrated in the non-tradable, education, and health sectors, which depend primarily on local demand and government transfers.

JEL classification: E24, G24, G28, H74
Keywords: Municipal bonds, Credit ratings, Public finance, Local demand, Government employment, Private employment, Income

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 bonds and 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 in the supply of credit to local governments affect economic outcomes? Identifying the causal impact of municipals' bond financing on local 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 exogenous variation in municipal bonds ratings due to Moody's recalibration of its municipal bond rating scale (the treatment). Credit rating agencies in particular are an important source of information to investors in the municipal bond market, as it is more opaque than other markets such as the sovereign and corporate bond markets. Before the 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 Scale ratings measured expected losses among corporate bonds, sovereign debt, 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 municipal bonds worth more than $2.2 trillion.

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 were upgraded. Municipalities that were already properly calibrated vis-à-vis other securities serve as control group. 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 (2014) use the Moody's rating 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. This effect is the result of improvements in the information environment and a reduction in ratings-based regulatory compliance costs.[1]

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 sector employment, and total income. Since our event affects bonds issued by counties, as well as by other local governments such as cities, townships, school districts, and special districts, we aggregate the changes in ratings at the county level. Our treatment variables can be interpreted as the fraction of local entities that have bond issues upgraded in each county as a result of the Moody's recalibration.

Whereas most prior studies rely on time series variation to study the economic effects of public financing, we use cross-sectional geographic variation in municipalities' ratings, which

[1] Higher credit ratings translate into lower capital requirements and other costs associated with regulatory and contractual compliance applied to banks, insurance companies, and pension funds.

greatly reduces concerns about confounding effects of public financing from other macroeconomic factors, such as monetary and federal fiscal policy. The identifying assumption is that confounding nationwide factors are uncorrelated with the differential effect on ratings and economic outcomes across municipalities.

We first show that the Moody's recalibration causes an asymmetric effect in municipal ratings. Moody's ratings increase half a notch more for recalibrated municipalities than for non-recalibrated municipalities. Since S&P did not have a dual-class rating system at the time of the Moody's recalibration, we use S&P municipal bond ratings as a placebo test. As expected, we find no significant differences on S&P ratings between Moody's recalibrated bonds and non-recalibrated bond. This placebo test supports the necessary condition for the validity of our tests: the shock to Moody's ratings due to the recalibration is exogenous with respect to local fundamentals.

We then exploit the asymmetric effect of the recalibration on municipalities to identify the effects of ratings on credit supply to municipalities. Consistent with the evidence in Cornaggia, Cornaggia, and Israelsen (2014), we show that upgraded municipalities increase the dollar volume of new bond issues after the recalibration significantly more than non-upgraded municipalities. The differential effect on the dollar volume of bond issues at the county level is about 22% in a period of three years after the recalibration relative to three years before. 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.

These results are consistent with a positive shock to the credit supply of municipalities that

benefit from bond ratings 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, or decrease taxes and therefore improve local economic conditions.

Consistent with local governments using the increase in bond financing to boost local 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 in the year of 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 employees. Indeed, the overall employment effects of the rating recalibration are smaller than in the public sector. We find that total private employment in upgraded municipalities increases about 3% more than in non-upgraded municipalities in the year of 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 in the year of the recalibration. The differential effects are also strong in the health and education sectors at 7% to

12%. In contrast, the corresponding differential effect in tradable sector employment is negative but statistically insignificant. We conclude that the effects on private sector employment are concentrated in the non-tradable, education, and health sectors. In the tradable sector, there is evidence of a crowding out effect due to the increase in local government spending.

There is also a differential effect on total income, 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% in the year of 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. Differences in the post-treatment period can only be attributed to the treatment when this assumption holds. In order to address this concern, we consider the evolution of the economic outcomes variable (employment and income) in the years leading to the treatment separately for the treatment and control groups.

We show that government employment, private sector employment, and income follow similar trends across upgraded and non-upgraded municipalities in the two years before the recalibration. Furthermore, the local government employment for the treated and control groups have different evolutions in the year of the recalibration. Upgraded municipalities' government employment increases in the year of the recalibration, while non-upgraded municipalities experience a slight reduction. 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 save jobs in the public and private sector.

Our paper contributes to the literature on the effect of credit market shocks on economic outcomes. Chodrow-Reich (2014) examines disruptions in the syndicated loan market following the collapse of Lehman Brothers in 2008. Firms that had pre-crisis lending relationships with weaker banks faced restrictions in credit supply, which caused reductions in employment at these firms. Greenstone, Mas, and Nguyen (2014) find that shocks to the supply of credit from banks 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.

We also contribute to the literature on the real effects of credit ratings. Credit ratings matter to issuing firms including access to capital, 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.

Finally, we contribute to the long-standing debate on the effects of public spending on economic outcomes (see Ramey (2001) for a survey). The policy debate on the effects of the American Recovery and Reinvestment Act of 2009 has revived the interest of researchers over the size of the fiscal multiplier. Recent papers use cross-sectional geographic variation in government spending to identify fiscal effects, exploiting the fact that nationwide factors are independent of the differential effects on spending and economic outcomes across regions (e.g., Nakamura and Steinsson (2010), Shoag (2011), Suarez-Serrato and Wingender (2011), Chodrow-Reich, Feiveson, Liscow, and Woolston (2012), Wilson (2012)).

2. Sample and Variables Description

2.1 Recalibration Event

Moody's Municipal Rating Scale historically measured how likely an entity is to require extraordinary support from a higher level of government in order to avoid default. Moody's Global Scale ratings, on the other hand, are designed to measure expected losses among corporate bonds, sovereign debt, 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.

In March 2010, Moody's announced a recalibration of its municipal ratings scale to align it with the Global Scale. In April and May of 2010, over a four week period, Moody's announced a zero-to-three notch upgrade of municipal bond issues associated with the recalibration. Moody's (2010) clarifies that the ratings revision is intended to enhance the comparability of ratings across asset classes, not to indicate a change in intrinsic credit quality of the issuer. Finally, and importantly for our study, Moody's (2010) indicates that any ratings under review for upgrade or downgrade prior to recalibration would remain under review, and are not lumped into these massive ratings changes. Thus, our sample does not include any natural upgrades associated with improving issuer fundamentals that could contaminate the estimates generated by our tests. Cornaggia, Cornaggia, and Israelsen (2014) provide a detailed description of the Moody's municipal bond ratings recalibration.

2.2 Data and Summary Statistics

The first tests study the effects of the ratings recalibration on municipal bond market 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 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 of three years before and the three years after the recalibration event.

We obtain data on recalibrated bond issues from Moody's. The data contains the rating of each issue before and after the recalibration. Since we measure local economic outcomes (employment and income) at the county level, we restrict the analysis of upgrades to issues that can be matched to a county such as issues of counties, cities, townships (these include boroughs, towns, villages, and parishes), 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. As our analysis focus on economic outcomes, rather than on municipal bond market outcomes, we take into account any municipal bond issue, whether uninsured or insured, in order to classify an issuer as recalibrated.[2]

Panel A of Table 1 presents summary statistics of the issue amount and offer yield in the sample of new issues. Issues by upgraded municipalities represent about 75% of the sample of new bond issues.[3] The average 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 yields are 2.8% on average, with a median of 2.9%.

The primary outcome variables of interest are county-level employment and income. We

[2] We obtain similar estimates when we define an issuer as recalibrated using only uninsured issues as in Cornaggia, Cornaggia, and Israelsen (2014).
[3] This figure would be about 60% when we define an issuer as recalibrated using only uninsured issues.

obtain local government employment data from the Census Government Employment and Payroll survey. Government employment is measured as the full time equivalent employees at counties, cities, townships, school districts, and special districts. The Census Bureau conducts a complete census of local government employees every five years (e.g., 1997, 2002, 2007), and a sample of local governments is used in the other years. Our analysis is restricted to entities that are present in all years in our sample. 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 in level and growth (log change): local government, private employment, and income at the county level 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 is negative at -0.4%. Private employment is much larger than government employment at 66 thousand employees in total. 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 issuers 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.[4] The FHFA HPI is a weighted, repeat-sales index, and it measures the average price changes in repeat sales or refinancing on the same properties. We obtain county-level information on the number of households from the 2000 Census Bureau Summary Files. The *Households* variable is defined as all the people that occupy a given housing unit.

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 estimate differences-in-differences regressions using new issues data from Ipreo from April 2007 to March 2013. In this test, the sample is restricted to municipalities that issue bonds both in the three year period before and the three year period after the recalibration event. We apply this filter to mitigate selection bias, as upgraded municipalities may disproportionately participate in the primary market after the recalibration.

We study the effect of the Moody's recalibration on bond ratings, as well as quantities and prices in the bond market. We compare the rating, issue amount, and offer yield in the municipal bond markets of upgraded municipalities (the treatment group) and non-upgraded municipalities (the control group), before and after the recalibration.

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 bonds (insured or uninsured

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

issue) 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 main effect on the *Recalibrated Dummy* is not identified. The regression also includes control variables and year fixed effects. Standard errors are clustered at the issuer level to correct for within-issuer residual correlation.

Table 2 presents the estimates of the issue-level regressions of the effects on municipal bond markets. Column (1) presents estimates in which the dependent variable is the Moody's bond issue rating. In order to perform this test, we map the ratings into 22 numerical values, where 22 corresponds to the highest rating (Aaa) and one to 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. The estimates suggest that ratings increase 0.51 notches more for the treatment group than for the control group following the recalibration.

We can use the S&P credit ratings as a placebo test, as S&P did not have a dual-class rating system. 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. This placebo test can detect whether the differential effect on Moody's ratings between treatment and control groups is explained by other factors besides the recalibration.

Column (2) of Table 2 presents estimates of the placebo test by using the S&P bond ratings of the same issues as in column (1). We find no significant differential effect between the treatment and control groups following the recalibration using S&P ratings. This finding is

important to validate our identification strategy.

Figure 1 compares the effect of the Moody's rating recalibration on treated and control municipalities ratings from two years before the recalibration up to two years after. The figure shows the effect on both Moody's and S&P ratings. The estimates come from the regressions in columns (1) and (2) of Table 2, including yearly leads and lags of the interaction term *Recalibrated Dummy* × *Post Dummy*. Treatment and control groups have similar Moody's ratings before the recalibration, and there are no significant changes in the two years prior to the recalibration. The treated municipalities then suffer a significantly greater upgrade 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 before and after the recalibration. The evolution of 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 0.111, 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 10

basis points. The magnitude of the effect on offer yields is similar to that in Cornaggia, Cornaggia, and Israelsen (2014).

Figure 2 compares the effect of the recalibration on the amount issued 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), including yearly leads and lags of the interaction term *Recalibrated Dummy* × *Post Dummy*. 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.

We also 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. We aggregate the new bond issues data by county and year (years are defined from April to March to match the recalibration event). The *Issuance Volume* is the sum of the issue amount of all new bond issues of municipalities in each county and year (in millions of dollars). The *Offer Yield* is the average of offer yields across all new bond issues of municipalities in each county and year (in percentage). The *Recalibration* variable is the average of the *Recalibration Dummy* or the weighted average by issue amount of the *Recalibration Dummy* across all new bond issues of municipalities in each county and year. The *Recalibration* variable can be interpreted as the fraction of municipalities that has been upgraded in a given county as a result of the recalibration event.

Table 3 presents the estimates of the regression of the log *Issuance Volume* (Panel A) and *Offer Yield* (Panel B) at the county level. Columns (1) and (2) present estimates using the *Recalibrated* variable calculated using equal weights and columns (3) and (4), using the issue

amount as weights. 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 0.22, significant at the 1% level, which indicates that counties in the treatment group increase the dollar volume of new bond issues after the recalibration 22% more than counties in the control group. We find that offer yields of upgraded municipalities decrease significantly more than offer yield of non-upgraded municipalities following the recalibration. The estimated differential reduction in offer yields is 32 to 42 basis points. The county-level estimates in Table 3 are qualitatively similar to the issue-level estimates in Table 2.

4. Economic Outcomes

In order to estimate the impact of the ratings recalibration on local economic outcomes, we estimate differences-in-differences regressions of government employment, private employment, and total income. We estimate county-year panel regressions using the logarithm of employment or income as the dependent variables. The sample includes all counties. The panel regressions consider two alternative sample periods: 2007-2012 and 2009-2011.

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. The *Recalibrated* variable is the fraction of issuers 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 County Business Patterns data is measured as of March 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 a 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 sector employment than in private sector 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 in government employment as the dependent variable.

In column (1), the interaction term *Recalibrated* × *Post Dummy* coefficient is positive at 0.04, but imprecisely estimated. The estimated differential increase in government employment is higher at 0.049 in column (2) when we include county-level controls. As expected, the corresponding estimates in columns (3) and (4) that use the shorter window around the recalibration are slightly stronger and more precise. The point estimate of the differential effect on local government employment is 0.058, significant at the 5% level. The cross-sectional regression in column (5) estimate is also similar at 0.058.

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 local governments 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.

Figure 3, Panel A, shows the evolution of the log of government employment in the two years 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 3, Panel B, shows the differential effect of the recalibration between treated and control counties from two years before the recalibration up to two years 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 upgrades 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 rating recalibration, which supports a causal interpretation of the effect of shocks to the supply of local public financing on economic outcomes.

4.2 Private Employment

Next, we study the effects of municipalities' ratings recalibration on private sector 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 0.050, 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 higher at about 0.035 and statistically significant at the 1% level. The cross-sectional regression in column (5) estimate is similar at 0.028, 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 upgrades of municipal bonds on total 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 4, Panel A, shows the evolution of the log of private employment in the two years 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 4, 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.

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 sector 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 four-digit industry employment 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 increase 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 sector employment is imprecisely estimated.

Overall, we find robust evidence of a positive effect of the exogenous credit rating upgrades of municipal bonds on non-tradable sector 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 higher wages). Indeed, we find a negative differential effect of the rating recalibration on tradable employment but estimates are imprecise. Thus, the evidence supports that workers who move into the government sector and non-tradable sector may 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 transfer and grants from local governments such as the education and health sectors. 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 education sector, and columns (3) and (4) present estimates for the health 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 slightly 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, education, and health sectors. 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' rating recalibrations on local 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 local 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 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 5, 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 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 significant pre-existing differential trends, and the differential effect becomes significant after the recalibration.

In short, we find robust evidence of a positive effect on local income of the exogenous credit rating upgrades 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 rating recalibration, which supports a causal interpretation of the effect of shocks to the supply of local public financing on local income.

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 includes counties that may be 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 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. This estimate is similar to that in Table 4. Column (2) presents the estimates for total 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 are quantitatively similar to our baseline estimates.

A second robustness tests consists of restricting the sample to municipalities that have at least one bond issue rated by Moody's. This filter will restrict the control group only to municipalities rated by Moody's.

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 3. 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 total 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 estimates in Table 8.

In short, 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 credit 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 are able to obtain more new bond financing and experience reductions in their borrowing costs than non-upgraded municipalities.

This asymmetric effect to municipalities' credit ratings leads to greater increases in government and private sector employment of upgraded municipalities relative to non-upgraded municipalities. The private sector 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, education, and health sectors. Income (wages and salaries) also responds in a significant manner 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 seem to be 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 contributed to an improvement in the information environment and a reduction in ratings-based regulatory compliance costs, which expanded the debt capacity of upgraded 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 municipal bond issues from April 2007 to March 2013. The sample in Panel B consists of observations on counties from 2007 to 2012.

	Mean	Median	Std. Dev.	Minimum	Maximum	Obs.
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						
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 of Credit Ratings, Issue Amount and Offer Yield around the Recalibration

This table presents the estimates of differences-in-differences regressions of Moody's ratings, S&P ratings, 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 municipal bond issues 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	Offer Yield
Recalibrated Dummy × Post Dummy	0.511***	-0.068	0.111**	-0.095**
	(0.053)	(0.064)	(0.045)	(0.048)
Post Dummy	0.321***	0.005	-0.083**	-0.982***
	(0.034)	(0.034)	(0.038)	(0.040)
Year fixed effects	Yes	Yes	Yes	Yes
Issuer fixed effects	Yes	Yes	Yes	Yes
R-squared	0.830	0.820	0.570	0.310
Observations	220,109	118,145	202,615	220,109

Table 3: Differences-in-Differences of Issuance Volume and Offer Yield of New Bond Issues around the Recalibration

This table presents the estimates of differences-in-differences of county-year panel regressions of the issuance volume and offer yield of new bond issues around the Moody's recalibration event in April and May 2010. *Recalibrated* is the fraction of issuers that has been upgraded in each county using equal weights or weighted by the issue amount 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 municipal bond issues 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)
Panel A: Issuance Volume				
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 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 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 the estimates of differences-in-differences regressions of the log of government employment around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of county-year panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of issuers 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. 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 the estimates of differences-in-differences regressions of the log of private employment around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of county-year panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of issuers 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. 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	0.050***	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 the estimates of differences-in-differences regressions 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 county-year panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of issuers 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. 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 the estimates of differences-in-differences regressions 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 county-year panel regressions and columns (2) and (4) present estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of issuers 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. 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 the estimates of differences-in-differences regressions of the log of income around the Moody's recalibration event in April and May 2010. Columns (1)-(4) present estimates of county-year panel regressions and column (5) presents estimates of cross-sectional growth (log change) regressions. *Recalibrated* is the fraction of issuers 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. 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	0.122***	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 the estimates of differences-in-differences of county-year 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 issuers 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. The sample is restricted to countries with new bond issues in Ipreo 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 the estimates of differences-in-differences of county-year 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 issuers 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. The sample is restricted to countries with new bond issues in Ipreo and Moody's rating 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

This figure shows 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.

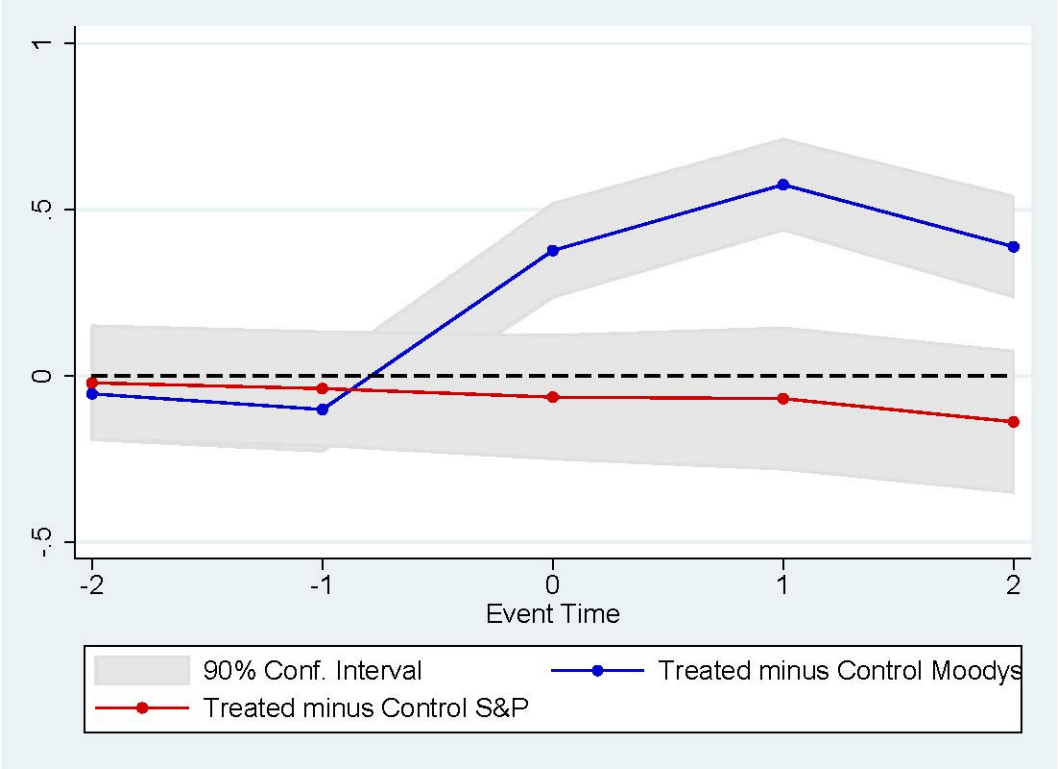


Figure 2: Bond Issue Amount around the Recalibration

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

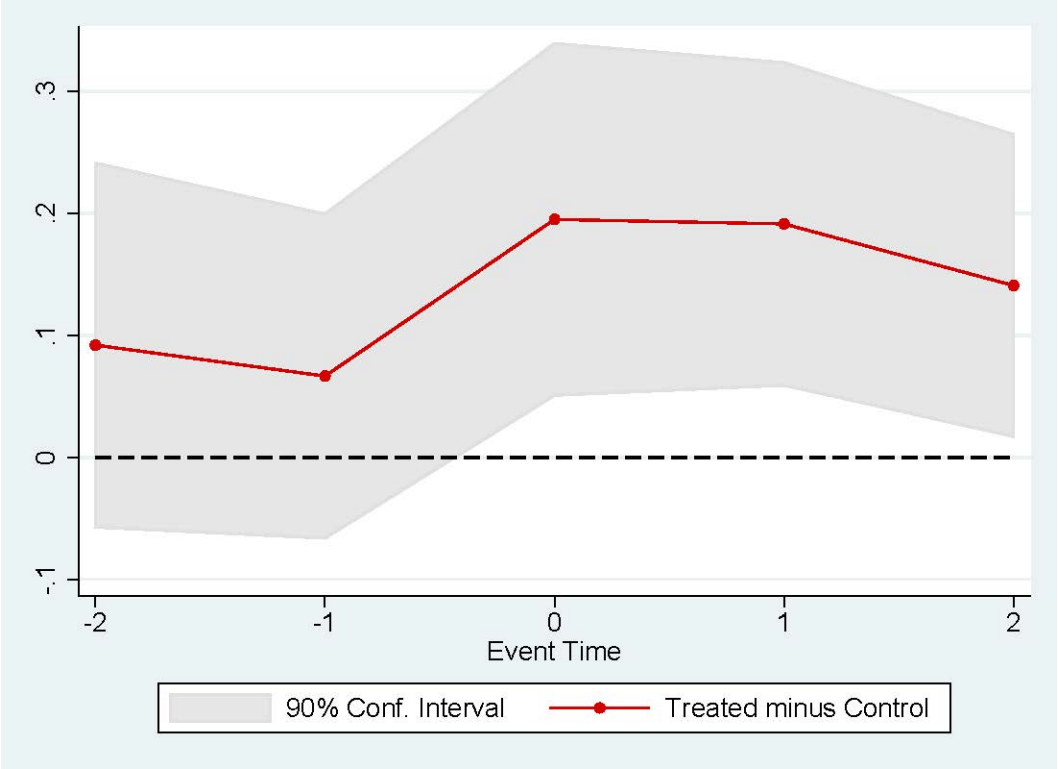
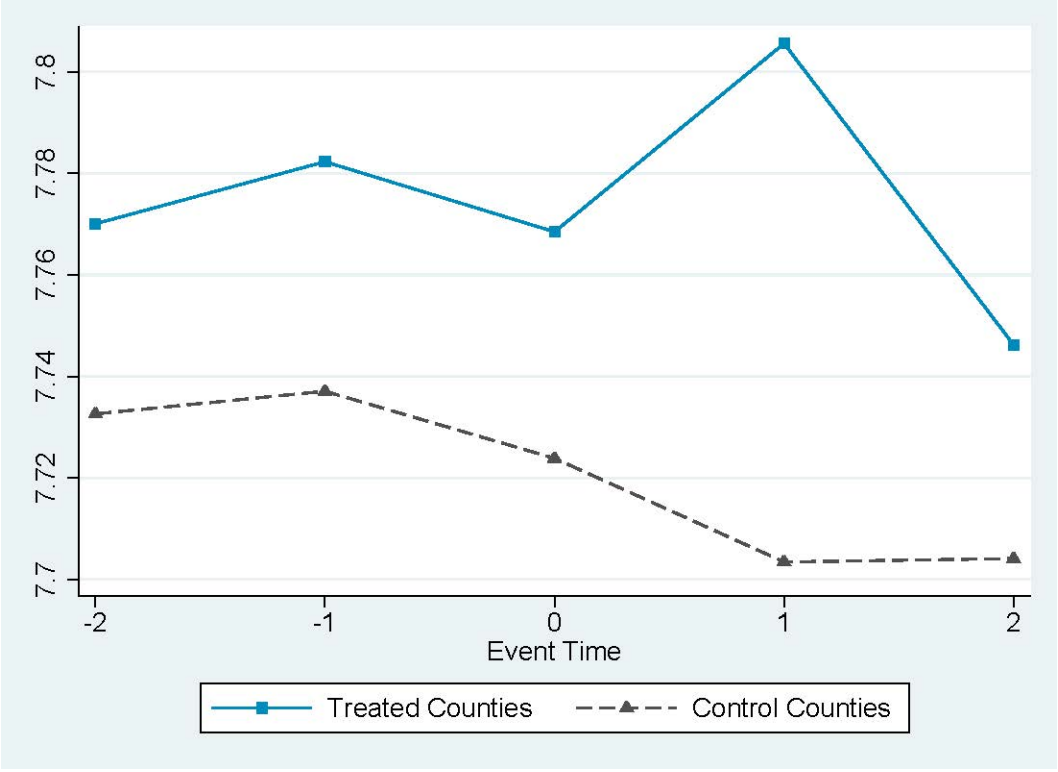


Figure 3: Government Employment around the Recalibration

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



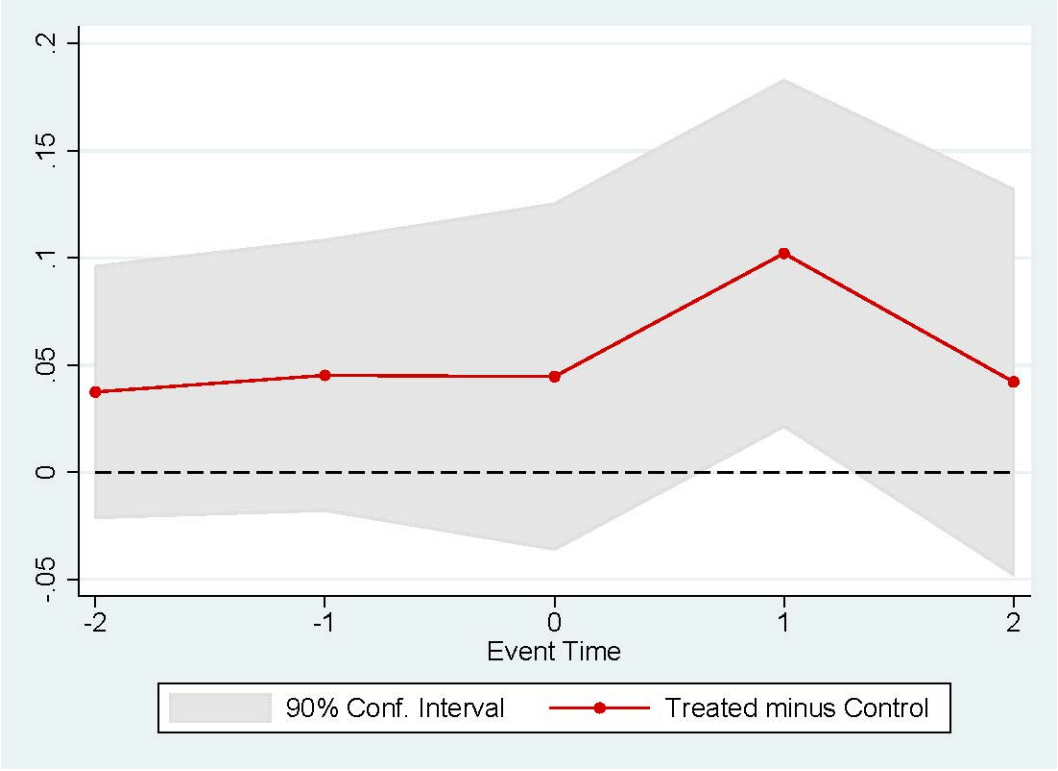
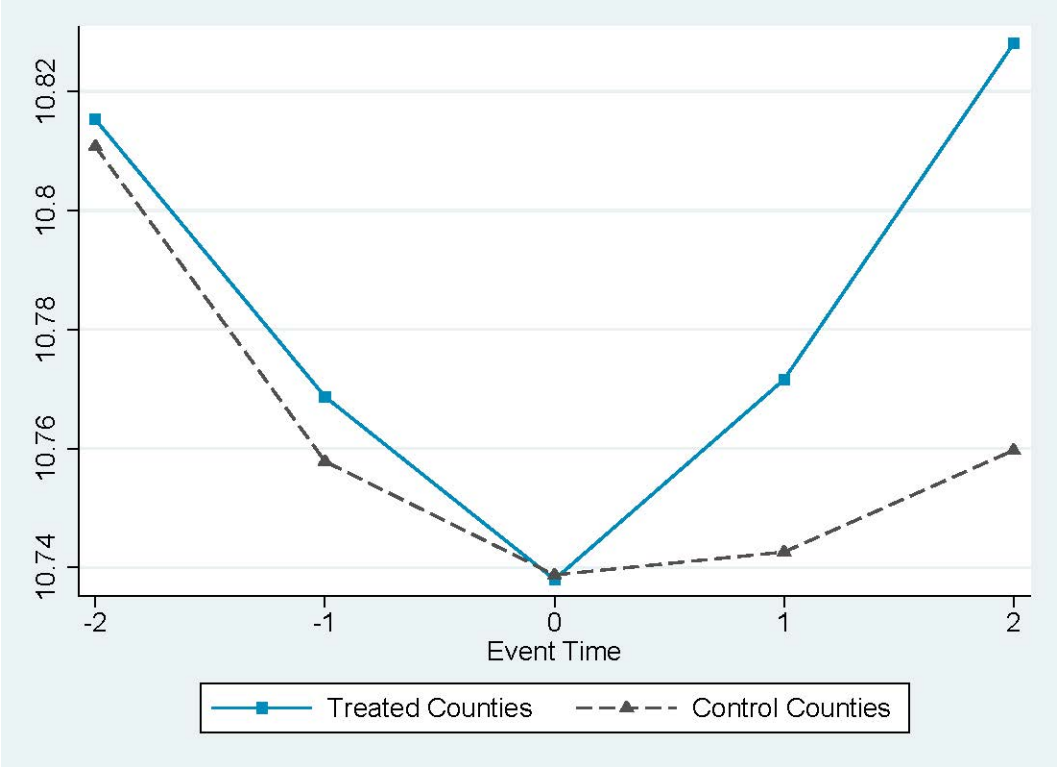


Figure 4: Private Employment around the Recalibration

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



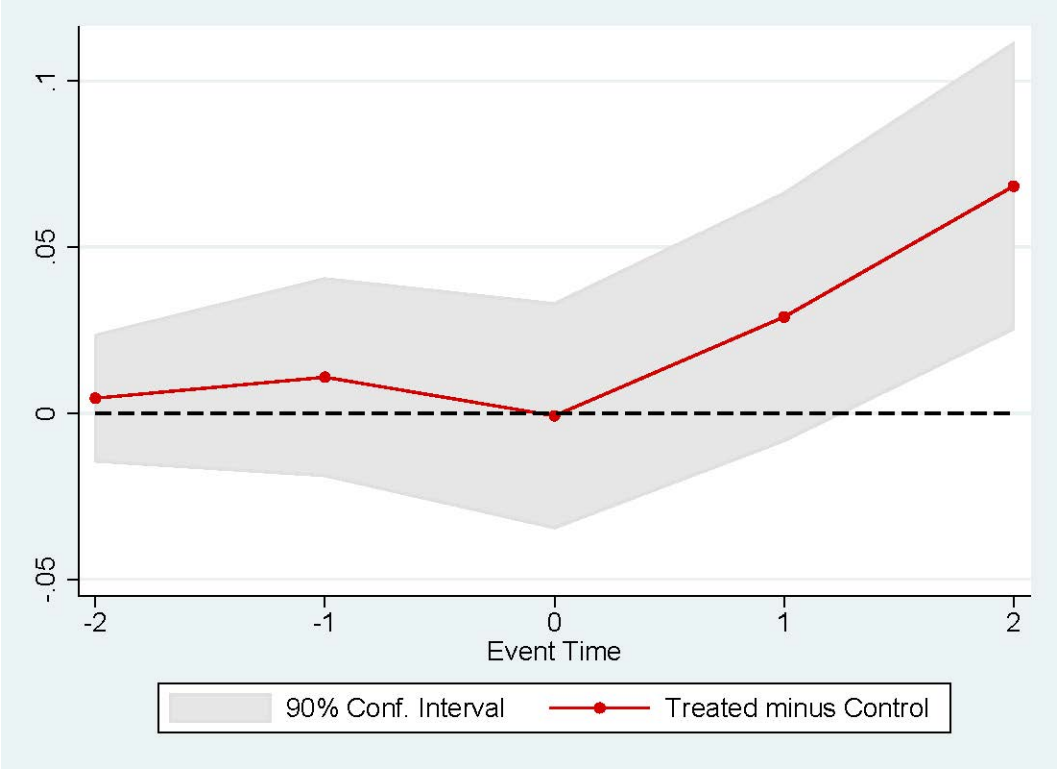


Figure 5: Income around the Recalibration

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