Impact of Corporate Subsidies on Borrowing Costs of Local Governments: Evidence From Municipal Bonds *

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Abstract

We analyze the impact of $38 billion of corporate subsidies given by U.S. local governments on their borrowing costs. We find that winning counties experience a 13.6 bps increase in bond yield spread as compared to the losing counties. The increase in yields is higher ($16 - 21$ bps) when the subsidy deal is associated with a lower jobs multiplier or when the winning county has a lower debt capacity. However, a high jobs multiplier does not seem to alleviate the debt capacity constraints of local governments. Our results highlight the potential costs of corporate subsidies for the local governments.

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JEL Classification: G12, H25, H74

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1 Introduction

State and local governments in the United States compete intensely, by offering subsidies in the form of tax abatement and grants, to attract firms to their regions.[1] Targeted business incentives may help job creation and economic development through potential multiplier effects. However, the foregone revenue and the additional demand for public services may require local governments, especially those that are financially constrained, to raise additional resources either by increasing taxes or by issuing additional debt, or both. Alternatively, local governments may cut spending on public services (Bartik, 2019). In this paper, we analyze the financial and real impact of winning large corporate investment deals on the local communities.

A direct assessment of the economic impact of corporate subsidies on the local community is challenging given the significant uncertainty about the level and timing of the proposed investment, the number and type of jobs created[2], wages offered, and potential multiplier effects of these jobs (Moretti, 2010). Moreover, confounding events during the long gestation period complicate the measurement of multiplier effects and the associated costs of these corporate subsidy deals. In this paper, we shed light on the potential economic impact of large corporate subsidy deals by documenting their effects on the borrowing costs of local governments.

The $3.8 trillion municipal bond market is a significant source of financing for local governments. Property taxes and sales taxes account for over one-third of local government revenue. So, any direct or indirect jobs created following a subsidized corporate investment impact local governments' future revenue streams. Despite a low (0.08%) default rate, default risk accounts for at least 74% of the average municipal bond spread (Schwert, 2017). As a result, municipal yields are likely to incorporate changes in the expected future cash flow streams of the local governments and consequently any changes in their default risk (Gao, Lee, and Murphy, 2020). Hence, the municipal bond market is an ideal setting to understand the impact of corporate subsidies on the local economies.

Using hand-collected county-level data on the winners and runner-up bidders for 127 large

[1] Recently, 238 cities made bids for Amazon HQ2 that promised $5 billion investment. The winners, New York City and Northern Virginia, offered tax rebates and other incentives totaling $5.5 billion.

[2] For example, in 2017, Wisconsin announced $4.1 billion in subsidies to Foxconn. However, there was still significant uncertainty about the actual investment and the number of jobs it will create.

corporate investment deals during 2005–2018, we find that the bond yields of winning counties increase by 7.5 basis points (bps) in the secondary market within one year after the announcement of the subsidy. However, the losing counties do not experience a significant change. Within thirty-six months after the deal, we find an increase of 13.61 bps in after-tax yield spreads. This is equivalent to a 10.5% increase in credit spread or a reduction in bondholders' wealth of about \$4.3 billion[3] in the secondary market. On the other hand, for new issuances by local governments in the primary market, this amounts to \$0.79 billion in additional borrowing cost. However, this magnitude under-estimates the effect given the issuers' ability to time the primary market.

In a standard corporate finance framework (Jensen and Meckling, 1976), positive (negative) NPV projects would increase (decrease) the value of the firm and hence the value for shareholders and bondholders. For local governments, the 'equity holders' are likely to include the local citizens and the taxpayers. Therefore, they are likely to benefit from subsidy deals which bring in greater economic activity including new jobs. Local governments may need to raise capital through new municipal debt to provide for additional infrastructure. However, local governments' debt capacity may affect the value of future investment projects (Myers, 1977; Turnbull, 1979). Therefore, any new subsidy deal may shift value from bondholders to shareholders for low debt capacity counties. This could increase the secondary market bond yields. In such scenario, a subsidy deal with high multiplier benefits may alleviate these debt capacity constraints. Our main result is mostly driven by subsidy deals with low (anticipated) jobs multiplier. Consistent with the cost-benefit trade-offs that counties face, we find that winning counties with a lower debt capacity experience higher borrowing costs. Finally, we find that even a high jobs multiplier does not seem to alleviate the binding debt capacity constraints of local governments.

Identifying the causal impact of the corporate subsidy events on the borrowing costs of local governments is challenging since we cannot observe what would have happened if the winning

[3] The average after-tax credit spread between A- and AAA-rated municipal bonds of winning counties in the sample before the deal equals 130 basis points. For all winning counties, the outstanding municipal debt in the deal year was $\sim$ \$400 billion. The average duration of bonds for winning counties in the year before the deal was 8.01 years, with an average yield of 2.95%. We use the modified duration approach to compute this impact on the bondholders' value for a yield increase of 13.61 bps three years after the deal on a semi-annual basis as \$4.3 billion (=\$400 $\times$ 8.01 $\times$ 0.0013/(1+2.95%/2) billion).

county did not win the bid. So, we follow Greenstone et al. (2010) and Bloom et al. (2019) and consider the closest runner-up bidder for the project (the losing county) as the counterfactual county. We invest significant effort to manually parse through local print media/newspapers to find out the losing county (and state) and earliest date of announcement for the subsidy (See Section IA2 for details). Typically, local governments and economic development board officials maintain secrecy about subsidy offers to avoid other competitors. For example, when Missouri bid for Freightquote's facility in 2012, the project was encoded as "Apple". Even after the project investment is announced, competing local governments may be bound by non-disclosure requirements from releasing details about the subsidy. Given such constraints, it was difficult to collect data on the subsidy packages offered by losing counties. In the absence of such data, we first report evidence supporting the identifying assumption, i.e., the winning county and losing county follow similar economic trends before the deal. We find an upward (downward) trend for aggregate employment (unemployment rate) for both winning and losing counties after the subsidy announcement. Similarly, trends for county ratings and the underlying county risk using local betas (Tuzel and Zhang, 2017) for the winning-losing county pairs are statistically similar. For instance, if a bidding county is too aggressive in the hopes of reversing their fortunes, we expect to find differential pre-trends for economic indicators like unemployment rate, county ratings, etc. before the deal. However, the absence of such trends suggests that the losing county is an appropriate counterfactual for our analysis.

We use secondary market trades for 123,468 municipal bonds of the winning-losing county pairs for the 127 deals. We focus on secondary market trades to avoid any confounding endogeneity due to market-timing in the new municipal bond issuance market. We use tax-adjusted yield spreads as the main dependent variable. We estimate event-study style difference-in-differences regression with winning-losing county-pair fixed effects, county fixed effects, and calendar year-month fixed effects, and include bond-specific controls and county specific controls[4]. We find that the bond yields of the winning counties increase by approximately 13 bps as compared to the losing counties within 36 months after the deal.

[4]We include coupon rate, size of issuance, remaining maturity, callability, bond insurance, and type of security based on bond repayment source (tax revenues for general obligation bonds and project-specific revenues for revenue bonds). For county-specific controls, we include lagged level and changes in the unemployment rate and labor force to control for local economic conditions.

Corporations do not choose between the bidding counties randomly (Greenstone, Hornbeck, and Moretti, 2010). In order to address this concern, we estimate a predictive regression of winner dummy using various county-level ex-ante observable characteristics such as the level and changes in the unemployment rate, level and changes in the labor force, house price index, and income per capita. We do not find any of these observable county-level characteristics systematically predict the probability of winning the deal. Next, we test if the timing of the subsidy announcement confounds with the declining economic health of the winning counties. If this is true, we should expect to see an increase in yields for both *revenue bonds* (bonds supported by revenue from a specific project) and *general obligation (GO) bonds* (bonds that are backed entirely by the issuers' creditworthiness and ability to levy taxes on its residents) among the winning counties of poor economic health. However, we find a strong positive effect on yields of *general obligation bonds* and an insignificant impact on *revenue bonds* of low credit rating counties. These results show that a declining trend in economic conditions of the winning county is unlikely to be driving the higher bond yields after winning the deal.[5]

To shed light on the economic mechanism underlying the higher bond yields of the winning counties, we analyze the winning counties based on the expected benefits and costs of corporate subsidies. To proxy for expected benefits, we use two measures of the expected jobs multiplier for a deal. First, we construct the measure of anticipated jobs multiplier by summing up the proportion of value-added in the upstream and downstream segments of a given industry, weighted by the corresponding county's share of wages. We find that the difference between winners and losers is 22 bps within a year after deals with a low multiplier effect. However, for high multiplier deals, the difference between winners and losers is insignificant. In our second measure, we focus on knowledge spillovers using the economic importance of the aggregate value of prior patents granted to the firm winning the subsidy deal (Kogan, Papanikolaou, Seru, and Stoffman, 2017). We find that deals involving firms with low value patents result in 18–24 bps higher bond yield spreads for the winning counties. These results suggest that deals with a lower expected jobs multiplier lead to a greater increase in bond yields for winners.

[5]We also conduct a falsification test, wherein we consider the impact on the bonds of the winning county that have negligible credit risk (i.e., bonds which are pre-refunded with escrow accounts in state and local government securities). We find that the announcement of the subsidy deal doesn't have an impact on bonds of winning counties with minimal credit risk, further reinforcing our main results.

Following the subsidy deal, apart from the foregone tax revenues, the increased demand for public services may require the winning county to raise more municipal debt. We consider the ex-ante debt capacity of local governments in two ways: a) interest expenditure, and b) county credit ratings. We find that counties with higher interest expenditure (scaled by revenue) show a higher impact on yields (16–27 bps). We also find a higher probability of bond rating downgrades for winning counties with lower debt capacity (or higher interest expenditure). We also show that counties with lower ex-ante credit rating experience higher yield spreads on their existing debt after the subsidy announcement. These results suggest that irrespective of the jobs multiplier associated with the deal, winning counties with a lower debt capacity experience a greater increase in bond yields after the subsidy deal.

Next, we test whether a high jobs multiplier can alleviate debt capacity constraints of the local governments. We divide the winning counties into low and high groups based on the anticipated jobs multiplier and interact with our baseline coefficient. We find that the differential impact due to high-interest expenditure is similar in magnitude across both groups of anticipated jobs multiplier. The results suggest that a high multiplier does not alleviate the debt capacity constraints of local governments.[6]

We also find that compared to a year before the deal, the new municipal issuance for the winning counties increases about three times in the year after the deal. This is driven by counties with more debt capacity, i.e., those with lower interest expenditure (scaled by revenue). However, for the losing counties, this increase is only about 1.5 times. Compared to the losing counties, there is an increase of about 4.5 bps in new issuance offering yields for the winners after the deal. These results suggest that winning counties with low debt capacity anticipate a higher borrowing cost. As a result, they do not issue new municipal debt after the subsidy announcement.

We analyze if winning counties constrained by their debt capacity increase taxes. We find that counties with high interest expenditure experience an increase in property tax revenue per capita without a commensurate increase in the house price index. These results suggest that counties may either be increasing tax rates or reassessing property values.

[6] Our results on bargaining power show how the relative size between the firm and the county also affects the distribution of gains from corporate subsidy deals (Rubinstein, 1982).

To understand the real impact of corporate subsidies on the local economy, we also examine employment at the county level around the deal. Our results suggest a modest increase in employment growth and annual payroll growth but no meaningful change in unemployment rates. Further, we find that there is no significant change in the overall expenditure on public services at the county level. However, for winning counties with lower debt capacity, the per capita expenditure on health care decreases by 11.11% (=\$44/\$396), benchmarked to the year before the deal. For winning counties, the per capita expenditure on healthcare amounts to 10.23% of the total expenditure per capita. Overall, our results suggest that the ex-ante debt capacity of the county may influence the choice of financing the increased demand for public services following a plant subsidy, either by increasing debt or by raising property taxes. Additionally, debt constrained counties appear to reduce their per capita expenditure on healthcare without making meaningful gains in employment.

Our paper relates to the large literature on tax incentives (see the survey by Akcigit and Stantcheva (2020)). Specifically, we contribute to the literature on the economics of location-based tax incentives (Glaeser, 2001; Austin, Glaeser, and Summers, 2018). Our paper builds on the literature[7] about the overall implications of subsidy-based location economics by studying their impact on the yields of municipal bonds, a critical source of financing for local governments. To the best of our knowledge, our paper is the first attempt to use the municipal bond market as a lens to evaluate the impact of corporate subsidies on local communities. In this regard, our approach sheds new light on how policymakers' decisions may affect the wealth of local bondholders and the default risk of local bonds (Schwert, 2017). We also contribute to the recent literature documenting how local shocks affect municipal bond prices (Gao, Lee, and Murphy, 2020, 2019; Goldsmith-Pinkham, Gustafson, Lewis, and Schwert, 2020). Further, our results on the debt capacity constraints of borrowing counties (Adelino, Cunha, and Ferreira, 2017) contribute to the large literature on firms' debt capacity affecting firm outcomes (Myers, 1977; Turnbull, 1979).

[7] Greenstone, Hornbeck, and Moretti (2010) document a 12% increase in total factor productivity (TFP) in incumbents of the winning county five years after the opening of a large plant, suggesting agglomeration gain to the county. Slattery (2020) uses the state-level bidding process to show that the firms capture the welfare gains in subsidy competition.

2 Identification Challenges and Methodology

In this section, we first discuss the challenges in identifying the impact of corporate subsidies and then describe our empirical specification.

2.1 Identification Challenges

The first econometric challenge is that the targeted subsidies are not random. Large corporations usually invite bids on subsidy packages from various counties that wish to attract investment in their jurisdiction. However, if a specific location is endowed with natural resources (Glaeser, 2001) or other strategic advantages pertinent to a specific kind of firms, they are more likely to get repeated investments in that sector or industry. Therefore, the assignment of the *winner* of a corporate subsidy deal may depend on multiple local factors. See Section IA1 for details on corporate subsidies in the U.S.

Greenstone and Moretti (2004) argue that firms' decisions are governed by the expected future supply of inputs and the magnitude of subsidy offered by the county. This results in a two-way matching between government decision-makers and corporate agents to arrive at the 'winner' between the bidding counties. To the extent that local officials cannot fully determine their chances of winning the plant by merely offering the higher subsidy, the assignment of 'winner' is closer to being random. The uncertainty in the final treatment assignment after the subsidy bids provides some support to the causal effect.

The next challenge is to identify the control group. Following Greenstone, Hornbeck, and Moretti (2010), we denote a 'winner' as the bidding county that was chosen by the firm to locate their project and use the closest runner-up bidder, the 'losing' county, as a counterfactual. In an ideal experiment, we would like to have the same incentive package offered by the competing locations. However, it is difficult to obtain the data of subsidy offer made by the losing county because of the inherent secrecy maintained by local governments (see Figure IA1). Regardless, there is some anecdotal evidence in support of a bidding process involving competitive subsidy bids offered by both the bidders[8].

[8] For example, Kansas and Missouri arrived at a subsidy armistice only in August 2019 after a history of shuffling jobs across the border: `https://www.wsj.com/articles/the-kansas-missouri-subsidy-armistice-11565824671`

Finally, another potential threat to our identification stems from the local economic conditions resulting in a negative selection. The underlying assumption in our identification strategy requires that the winning and corresponding losing county follow similar economic trends before the subsidy deal announcement. If the winning county is in worse economic shape, then its bond yields should be higher, which implies that our main effect is over-estimated. We plot the trends for bond yields, county-level aggregate employment, unemployment rate, bond rating, and local beta around the subsidy announcement. We do not find supporting evidence for negative selection (see Section 4.1.2 for details.)

2.2 Methodology

Our baseline event study focuses on the impact of corporate subsidies on the borrowing costs of local governments. Consistent with Greenstone, Hornbeck, and Moretti (2010), we rely on the stakeholders' expertise to identify the closest bidder as the counterfactual. This approach has the advantage of not introducing any researcher-specific biases in choosing the counterfactual. We carefully read newspaper articles to identify 127 winner-loser deal pairs at the county level spanning 39 states during 2005-2018 (See Section 3.1 for details). We use a three-year window before and after the subsidy announcements.[9] We use secondary market trades as the baseline case because these bonds are already trading in the winner-loser county pairs at the time of the deal announcement (and mitigate any concerns with deal related bond issuance driving our results).

We use a standard difference-in-differences approach between the treatment and control counties' bond yields in the secondary market for municipal bonds. This results in the baseline specification as below:

$$\begin{aligned} y_{i,c,p,t} = \alpha + \beta_0 * Winner_{i,c,p} * Post_{i,c,t} + \beta_1 * Winner_{i,c,p} + \beta_2 * Post_{i,c,t} \\ + BondControls + CountyControls + \eta_p + \gamma_c + \kappa_t + \epsilon_{i,c,p,t} \end{aligned} \tag{1}$$

where index i refers to bond, c refers to county, p denotes the (winner-loser) county pair and t indicates the year-month. After-tax yield spread is the dependent variable in $y_{i,c,p,t}$ obtained

[9] Our results are robust to using other windows, as shown in Panel B of Table 3

from secondary market trades in local municipal bonds (described in Section 3.2). We also use the raw average yield, after-tax yield, and yield spread as dependent variables in additional tests. *Winner* corresponds to a dummy set to one for a county that ultimately wins the subsidy deal. This dummy equals zero for the runner-up county in that subsidy deal. *Post* represents a dummy that is assigned a value of one for months after the deal is announced and zero otherwise. The main coefficient of interest is β_0 which comes from the interaction term, *Winner* $\times$ *Post*. The baseline specification also includes three sets of fixed effects: η_p, denoting county pair fixed effects to ensure that the comparisons are within bonds mapped to a winner-loser pair; γ_c, denoting county fixed effects to absorb unobserved heterogeneity at the county-level; and, κ_t, denoting year-month fixed effects to control for time trends. We follow Bergstresser, Cohen, and Shenai (2013); Gao, Lee, and Murphy (2020) to include amount issued, coupon rate, dummy for status of insurance and dummy based on general obligation versus revenue bond security type, collectively represented as *BondControls*. *CountyControls* refers to a vector of county level measures to control for local economic conditions. It includes the lagged value of log of labor force in the county, lagged county unemployment rate, the percentage change in the annual labor force level, and the percentage change in the annual unemployment rate. In all our specifications we double cluster standard errors at the county-specific bond issuer and year-month level, unless specified otherwise.

Our difference-in-differences approach following Greenstone, Hornbeck, and Moretti (2010) affords us some advantages over previously used methods in the literature. First, we do not compare the winning counties with all other counties in the US. Such a regression is likely to lead to biased estimates due to unobserved heterogeneity between the two sets of counties. Counties that offer large subsidies could be fundamentally different from the rest of the counties within the US. Plausibly, a county that is likely to gain substantially from a particular firm locating within it is more likely to attract the project with greater incentives. Simultaneously, a county with a greater need to increase jobs is likely to offer an aggressive incentives package. By doing so, it could try to overcome its inherent disadvantages and influence the firms' location decisions. These omitted factors may also be correlated with the bond yields of the respective local issuers. By restricting the sample to only those that were also involved in bidding for the same corporation at the same time, we reduce the bias from such unobserved heterogeneity.

3 Data

In this section, we provide details about the data used in this paper. First, in Section 3.1, we describe our data on corporate subsidies. In Section 3.2, we discuss the data used from the municipal bond market. Finally, we describe some other variables used in this study in Section 3.3.

3.1 Corporate Subsidies

The *Good Jobs First Subsidy Tracker* (Mattera, 2016) provides a starting point with its compilation on establishment-level spending data. As shown in Figure 1, states and the federal government spent more than USD 10 billion every year in corporate subsidies after the financial crisis of 2009. Further, there has been an increase in the portion of subsidies offered by state governments during the sample period of 2005-2018. States differ in the amount of subsidy they have offered in the past, with New York, Louisiana, and Michigan ranking among the top three (see Figure 2 for a ranking among states). On a per-capita basis, Washington, Oregon, and Louisiana spent over USD 1,500 during this period. Specifically, Figure IA2 depicts the subsidy value per capita using a choropleth map with five breaks shown in the legend.

One of the challenges that previous studies faced in evaluating the impact of corporate subsidies was the lack of comprehensive data at the county-level. We discuss the literature on location-based incentives in Internet Appendix IA1. The identification used in this paper relies on close-bidding auctions where two cities compete against each other to attract a firm. Their respective states may back local governments in sponsoring the subsidy. However, there is no published data source documenting such competing bids based on subsidy. One contribution of our paper is to provide the first records of winning and losing counties for large subsidy (defined as those exceeding USD 50 million) deals in the United States. We detail the construction of the data in the Internet Appendix IA2. In Table IA1, we show a comparison of the original data set from *Good Jobs First Subsidy Tracker* versus the one constructed after the hand-collection of relevant variables. Hand-collection was especially difficult due to inherent secrecy maintained by the bidding local governments. For example, when Missouri bid for Freightquote's facility in 2012, the project was encoded as "Apple" (see Table IA2). Further, it is difficult to obtain all the bidders in a given subsidy project due to multiple stages involved in the negotiations.

We can identify 127 winner-loser deal pairs at the county level, which we define as consisting of our final sample with subsidy over USD 50 million in each deal[10]. Of these, only 39 deal pairs overlap with those used in Bloom, Brynjolfsson, Foster, Jarmin, Patnaik, Saporta-Eksten, and Van Reenen (2019). We provide a summary of the subsidy deals in our final sample in Table 1. Panel A shows the distribution across all deals. The mean subsidy amount in the deals is USD 301 million, whereas the median amount is USD 138 million. Comparing this to the proposed investment, we find that the median deal gets a 38% subsidy as a proportion of investment. The median deal involved 925 jobs promised by the firm. The average subsidy per job promised by the firm amounts to nearly USD 499,000. Panel B shows that most of these deals are for new/expansion projects with about half the deals in manufacturing (Panel C). In Table IA3, we evaluate probable metrics in the data, which may help predict the level of subsidy offered by the winning counties. We find the amount of investment and jobs promised to be strongly correlated. Figure IA3 provides a distribution of the subsidy amounts over different buckets. Each bin worth less than USD 500 million has at least 20 deals each.

3.2 Municipal Bonds

Municipal bond characteristics are obtained from the Municipal Bonds dataset by FTSE Russell (formerly known as Mergent MBSD). We retrieve the key bond characteristics such as CUSIP, dated date, amount issued, size of the issue, state of the issuing authority, name of the issuer, offering yield, status of tax exemption, insurance status, pre-refunding status, type of bid, coupon rate, and maturity date for the bonds. We also use S&P credit ratings for these bonds by reconstructing the time-series of the most recent ratings from the history of CUSIP-level rating changes. We encode character ratings into numerically equivalent values ranging from 28 for the highest quality to 1 for the lowest.

An important step in our data construction is to link the bonds issued at the local level to the counties that make the subsidy bids. This geographic mapping allows us to study the

[10]As such, there are 120 unique firm-year level subsidy deals among bidding states. For one deal-pair, we do not have information on the jobs promised. There were 12 pairs for which we could not gather data on the size of investment for the proposed project.

implications on other economic variables using data on demographics and county-level financial metrics. Since the FTSE Municipal Bonds dataset does not have the county name for each bond, we need to supplement this information from other sources, such as Bloomberg. However, in light of Bloomberg's download limit, it is not feasible to search for information on each CUSIP individually. Therefore, we first extract the first six digits of the CUSIP to arrive at the issuer's identity[11]. Out of 63,754 unique issuer identities (6-digit CUSIPs), Bloomberg provides us with county-state names on 59,901 issuers. For these issuers, we match the Federal Information Processing Standards (FIPS) code. The FIPS code is then used as the matching key between bonds and bidding counties involved in offering corporate subsidies. We also match the names of issuers to the type of (issuer) government (state, city, county, other) on Electronic Municipal Market Access (EMMA) provided by Municipal Securities Rulemaking Board. We use this information to distinguish local bonds from state-level bonds because we are interested in the non-state bonds.

We use the Municipal Securities Rulemaking Board (MSRB) database on secondary market transactions during 2005-2019. Our paper closely follows Gao, Lee, and Murphy (2020) in aggregating the volume-weighted trades to a monthly level. Following Downing and Zhang (2004); Gao, Lee, and Murphy (2019), we only use customer buy trades to eliminate the possibility of bid-ask bounce effects. Table IA4 summarizes each step of the sample construction (Schwert, 2017). Given our primary focus on the borrowing cost from secondary market yields, our sample is derived from the joint overlap between the bond characteristics and bond trades at the CUSIP level. In matching the bond transactions from secondary market data to their respective issuance characteristics (from FTSE Russell), we rely on the CUSIP as the key identifier. In Table 2, we provide descriptive statistics on bond features pertaining to the primary market and secondary market. The average bond in the sample has a weighted average yield of 2.8% in the secondary market, with a remaining maturity of 10.6 years and 11.3 years for the winning and losing counties, respectively. We describe the key variables in Table A1.

The primary outcome variable used in Equation (1) is the tax-adjusted spread over the risk-free rate. We calculate the bond's coupon-equivalent risk-free yield as in Gao, Lee, and Murphy

[11]The 9-digit CUSIP consists of the first six characters representing the base that identifies the bond issuer. The seventh and eighth digits identify the type of the bond or the issue. The ninth digit is a check digit that is generated automatically.

(2020)[12]. Tax adjustment follows Schwert (2017) wherein the marginal tax rate impounded in the tax-exempt bond yields is assumed to be the top statutory income tax rate in each state. This is consistent with the broad base of high net worth individuals and households who form a major section of investors in the US municipal bond market (often through mutual funds). A detailed study on tax segmentation across states by Pirinsky and Wang (2011) shows significant costs on both issuers and investors in the form of higher yields. In particular, we use:

$$1 - \tau_{s,t} = (1 - \tau_t^{\text{fed}}) * (1 - \tau_{s,t}^{\text{state}})$$

To compute the tax-adjusted spread on secondary market yields:

$$spread_{i,t} = \frac{y_{i,t}}{(1 - \tau_{s,t})} - r_t,$$

where r_t corresponds to the maturity–matched coupon-equivalent risk-free yield for a bond traded at time t. Similar to Schwert (2017), we use the top federal income tax rate as 35% from 2005 to 2012, 39.6% from 2013 to 2017, and 37% from 2018 to 2019. We also consider tax-exemption at county-level and discuss this in Section 4.2.

3.3 Other Variables

We use data on county finances from the Census Bureau Annual Survey of Local Government Finances to get details on revenue, property tax, expenditures, and indebtedness of the local bodies. This gives us detailed constituents of revenue and tax components at the local level, which we use in additional tests to examine the implications for our main results. Our data on county-level household income is from the Internal Revenue Service (IRS) and is used as the total personal income at the county level. Our unemployment data comes from the Bureau of Labor Statistics. We use input-output tables from the Bureau of Labor Statistics (BLS). For the county-level population, we use data from the Surveillance, Epidemiology, and End

[12]First, we calculate the present value of coupon payments and the face value of a municipal bond using the US treasury yield curve based on zero-coupon yields as given by Gürkaynak, Sack, and Wright (2007). Using this price of the coupon-equivalent risk-free bond, the coupon payments, and the face-value payment, we get the risk-free yield to maturity. Finally, the yield spread is calculated as the difference between the municipal bond yield observed in the trades and the risk-free yield to maturity calculated. This yield spread calculation is similar to Longstaff, Mithal, and Neis (2005).

Results (SEER) Program under the National Cancer Institute. We obtain county-level data on the number of establishments and annual payroll growth from County Business Pattern (CBP). As a proxy for the risk-free rate, we use zero-coupon yield provided by FEDS, which provides continuously compounded yields for maturities up to 30 years. To get tax-adjusted yield spreads, we use the highest income tax bracket for the corresponding state of the bond issuer from the Federation of Tax Administrators.

4 Results

We discuss our baseline results (Section 4.1) for Equation (1), including evidence from the dynamics using the raw data on secondary market municipal yields and evidence on parallel pre-trends assumption. Section 4.2 shows robustness tests for our baseline specification. We propose the potential mechanism to explain our results in Section 4.3. Finally, we discuss the impact on the primary market of municipal bonds (Section 4.4) and then on the local economy, property taxes, and public expenditure (Section 4.5).

4.1 Impact on Borrowing Costs of Local Governments

4.1.1 Dynamics and Baseline Results

We begin our analysis by plotting the average yields observed in the secondary market between the winning and losing counties. Our event window comprises three years before and three years after the subsidy deal announcement. We use the quarter before the event window (T=-37 to T=-39 months) as the benchmark period to evaluate the pre-trends between the treatment and control groups. We depict the observations aggregated to a quarterly scale to mitigate the inherent limitations of liquidity in the municipal bond market. We plot the average yields based on Equation (2) below:

$$y_{i,c,p,t} = \alpha + \beta_q * \sum_{q=-12}^{q=12} Winner_{i,c,q} * Post_{i,c,q} + \delta_q * \sum_{q=-12}^{q=12} Loser_{i,c,q} * Post_{i,c,q} \tag{2}$$
$$+ \eta_p + \gamma_c + \kappa_t + \epsilon_{i,c,p,t}$$

where index i refers to bond, c refers to county, p denotes the (winner-loser) county pair, t indicates the event month, and q refers to the quarter corresponding to the event month t. Average yield is the dependent variable in $y_{i,c,p,t}$ obtained from secondary market trades in local municipal bonds. η_p represents the (winner-loser) county pair fixed effects; γ_c represents the county fixed effects; and, κ_t represents the calendar year-month fixed effects. We also add county specific year-month trends to control for time trends for a given county. The coefficients β_q and δ_q represent the average change in yields with respect to the benchmark period for the winning and losing counties, respectively. We depict the coefficient estimates from the regression in Figure 3.

In Figure 3, the dashed line with circles plots the average yields over the 3-year window for winning counties, depicted quarterly. The losing counties are depicted using a dashed line with diamonds. We plot the corresponding differences with 95% confidence intervals in the figure. We notice that in the pre-period, the difference between the average yields for winners and losers is negligible. Based on the fitted line in the pre-period, we find supporting evidence for the parallel pre-trends assumption between the winning and losing counties (See Section 4.1.2 for additional evidence on parallel trends assumption). Second, the secondary market average yields for the winning counties appear to be higher than those of the losing counties in the first year after the deal. This difference increases to 7.5 bps by the fourth quarter after the subsidy deal and is statistically significant. The fitted line for the post-period suggests an increase in difference between the winning and losing counties after the subsidy announcement.[13]

Note that the above results only represent the raw difference in average yields between the two groups by stacking the 127 deal-pairs in our sample into an aggregated set. These findings do not control for differences in bond characteristics and local economic conditions over time. Next, we estimate our difference-in-differences using our baseline Equation (1). Here, the coefficient β_0 of the interaction term, $Winner \times Post$, identifies the differential effect after the subsidy deal announcement on average yield spreads. We compare the winning counties to the losing counties while controlling for observable characteristics. To revisit our identifying assumption: the losing county serves as an adequate counterfactual to map how the winner's yield spreads would have changed in the absence of the deal announcement. The county–pair

[13]We extend the post-window up to five years and find increasing effect in Panel B of Table 3.

fixed effects ensure estimation from within each deal pair. Using county fixed effects helps us absorb any unobserved variation due to the bidding county itself. The calendar year-month fixed effects control for declining yield spreads in the overall municipal bond market during our sample period, over and above the spread adjustment for coupon-equivalent risk-free yields.

Table 3, Panel A reports the effect of winning a subsidy deal on the municipal bond yield spreads using Equation (1). In Column (1) - Column (3), we estimate the regression equation using the raw average yield as the dependent variable. Specifically, Column (1) denotes the estimates without using any controls. We use bond level controls in Column (2), which consist of the coupon (%); log(amount issued in USD); dummies for callable bonds, bond insurance, general obligation bond, and competitively issued bonds; remaining years to maturity; and inverse years to maturity. We describe the key variables in Table A1. In Column (3), we use county controls for levels and trends in the local economy. We use the lagged values (to the year of deal announcement) for log(labor force) and unemployment rate, and the percentage change in the unemployment rate and labor force, respectively. Since subsidies are often motivated by job creation, we use these measures at the county level consistent with the previous literature.[14] We follow the same scheme and show our results using after-tax yield spread as a dependent variable in Column (4) - Column (6). Consistent with Schwert (2017), we adjust the yield spread for taxes because most municipal bonds in our sample are tax-exempt securities. For robustness, we report our results using after-tax yield and yield spread as dependent variables in Section 4.2.

Using Column (6) of after-tax yield spread as our baseline case implies that the yield spread for winning counties increases by 13.61 bps after the subsidy announcement, in comparison to the losing counties. The 13.61 bps is equivalent to a reduction in bondholders' wealth amounting to 11.3% (=4.29/38) of the total subsidy ($38 billion) offered during the sample period. To arrive at this magnitude, we start with the outstanding municipal debt of the winning counties in our sample. We find that this amount is $\sim$ \$400 billion in the deal year. The average duration of bonds for winning counties in the year before the deal was 8.01 years, with an average yield of 2.95%. Using a modified duration approach, we compute the aggregate impact for three years after the deal on a semi-annual basis as $\$400 \times 8.01 \times 0.001361/(1+2.95\%/2)$ billion =

[14] We report the coefficients for bond level and county level controls in Table IA5.

$4.29 billion.

Next, in Panel B, we show the baseline result of Column (6) using different forward windows, keeping the pre-event window the same as three years. We find that the magnitude of the differential impact increases from seven bps within the first twelve months after the event (Column (1)) to about 19 bps in 5 years (Column(7)). There seems to be a gradual increase in magnitude, which likely persists beyond the immediate near-term. To evaluate the sensitivity of our results against the choice of the window used, we discuss robustness to our main result in Section 4.2.[15] In the next sub-section, we provide more evidence on the parallel trends assumption.

4.1.2 Do bond yield spreads respond to underlying local economic differences?

Our baseline comparison between winning and losing counties' yield spreads assumes similar local economic conditions between the treatment and control groups during the event window around the deal. The results in Section 4.1.1 suggest that winning and losing counties exhibit parallel pre-trends in their bond yield spreads. However, as discussed before, the decision by local governments to engage in the bidding process to attract firms may not be random. The local administration may be attempting to create new jobs or to retain existing ones by offering incentives. It could be the case that bondholders from these counties are responding to underlying differences between the winning and losing counties. We test such underlying economic differences based on some relevant observable economic indicators. We present the comparison of the average trends at the county-level in a) aggregate employment, b) unemployment rate, c) county-level municipal bond rating, and d) local beta between winners and losers in Figure 4. In each of these subplots, we use the annualized version of Equation (2).

In Figures 4a and 4b, we find that the aggregate employment shows an upward trend while the unemployment rate decreases. Both winning and losing counties seem to follow a similar trajectory with no statistical difference between them. This supports our parallel trends assumption on these key metrics related to employment. However, it is worth noting that after the subsidy deal, the increase in employment in the winning county is similar to that

[15] We find an increase in volume traded for both customer buy and customer sell trades and report our results in Table IA6.

in the losing county.

Further, Figures 4c and 4d provide a comparison of the county level credit-worthiness and riskiness, respectively. We use credit ratings from GO bonds aggregated up to the county-year to get the county level ratings. As shown in the figure, the two groups do not show any difference in trends. Since the rating of the winners is not worse than that of the losers, this also helps against the concern of negative selection. Finally, the local beta is a measure defined in Tuzel and Zhang (2017). Using this as a proxy for the underlying riskiness of the counties, we find that both winning and losing counties had similar local beta during the event window. Overall, the results suggest that winning and losing counties look similar based on local economic conditions during the event period.

Next, we estimate a multivariate linear probability model to understand if the local economic factors jointly determine the probability of winning a deal by the county. We use the local conditions during the three years before the deal as the regressors. In addition to using the four control variables in our baseline specification on unemployment and labor force, we further introduce income per capita and house price index. Table IA7 shows the regression results where we introduce each regressor successively. We plot the coefficients from Column (6) in Figure IA4 and show the confidence intervals at the 95% level. For each metric on the y-axis, we show the explanatory power in determining the 'winner' dummy. We find that the coefficient for none of these key local metrics significantly differs from zero.[16]

Another potential concern in our identification is about the timing of the subsidy announcement. In our baseline specification, we use county-level controls to absorb variation in key economic metrics that may be relevant to the subsidy offer. But unobserved time-varying county-specific changes, that coincide with the deal announcement, may also affect the bond yield spreads. Moreover, local government officials may be responding to undisclosed information about the county's health with the incentive deal. If bondholders are also privy to such private information, that may weaken our main result. However, if this is the case, we should observe an increase in yield spreads for all types of bonds irrespective of the use of proceeds. We

[16]Additionally, in Figure IA5 and Figure IA6, we also verify against improper pairing between winners and losers based on local economic conditions. By dividing the winning counties into low and high groups based on the unemployment rate and household income, we show that there is no statistical difference in the unemployment rates between correspondingly matched winners and losers. Further, even the matching based on county-level credit rating and local beta suggests against any evidence of mismatching good counties with bad ones.

include bond purpose fixed effects and bond purpose × year-month fixed effects in robustness checks to mitigate these concerns. Our results remain similar and statistically significant. In our strictest specification using county-pair × county × year fixed effects, we find similar results (See Section 4.2).

4.2 Robustness Tests

In this section, we test the robustness of our main result in Column (6) of Table 3 (Panel A) to various alternative specifications. We present the results of these robustness checks in Table 4.

4.2.1 Other Observables and Unobservables

Panel A of Table 4 shows results based on observable and unobservable factors. Our baseline specification controls for relevant time-varying county-level observables. We now consider whether our results are robust to a host of unobserved factors at the county and bond levels. First, in Column (1) we use county-pair × county fixed effects to account for unobserved variation at the county level within deals. The resulting magnitude for the baseline effect is 13.51 bps. In Column (2), we show our baseline result by using a stricter specification using county-pair × county × year fixed effects. This absorbs unobserved variation over the years across counties within subsidy deals. We find that the baseline effect reduces to 6.29 bps and remains statistically significant. Our results in Column (3) use an alternative way to absorb unobserved heterogeneity at the county level over year-months. We use indicator dummies to control for monthly county trends and find that after-tax yield spreads increase by 10.16 bps for the winning counties.

Next, to control for issuer-level characteristics among the bidding counties, we show our results with issuer fixed effects introduced to the baseline specification in Column (4). This may be relevant because municipal bonds are issued in a series corresponding to the given issuer's bond sale program. The increase in bond yield spreads is about 11.33 bps under this specification. To control for unobserved heterogeneity based on the use of proceeds (bond's purpose), we add purpose fixed effects to the baseline. We report this coefficient in Column (5) as 12.75 bps. Finally, we also consider the time-varying unobserved heterogeneity based on the purpose of the bonds by incorporating bond purpose × year-month fixed effects. We show our results for this specification in Column (6) as 9.93 bps.

4.2.2 Additional Tax Considerations and Duration

We provide evidence from additional tax considerations and duration in Panel B of Table 4. Municipal bonds are exempt from federal and state-level income taxes in most states, especially for bonds issued within the state itself. However, there are cases where some bonds issued by the local governments may not qualify for tax exemption. Additionally, there are four states which do not offer an exemption on state-level taxes for municipal bonds issued by them, namely: Illinois, Iowa, Oklahoma and Wisconsin. We account for these considerations in Columns (1) and (2) of Table 4. First, we drop bonds that do not qualify for exemption from state-level taxes in any state. Our main result in this case amounts to 16.73 bps in Column (1). Second, we drop subsidy deals involving the four states mentioned above (as either of the bidders). For this consideration, we report our results in Column (2) as 17.99 bps. Since both of these magnitudes are higher than the baseline effect, we argue that accounting for these additional tax considerations does not weaken our baseline result.

Additionally, there may be a concern that local residents may price in expectations of higher local individual income tax rates after the subsidy announcement. Municipal bond interest income may often be exempt from local taxes as well, leading to triple tax exemption. In this case, a higher future expectation of local tax rates may decrease bond yield spreads in winning counties. On the other hand, if the bondholders expect that the local administration may reduce local tax rates to attract more businesses after the subsidy, the bond yield spreads may go up. To understand more about this ambiguity, we next consider the individual income tax rates in Column (3). However, we are limited by the availability of data on local individual income tax rates. For the counties in our sample that can be matched to the Tax Foundation website, we obtain local individual income tax rates for 2011. We assume these values to hold for other years. We apply the local tax rate adjustment to bond yield spreads, over and above the state tax rate. We show our results by assuming zero local individual tax rates for counties with missing information. In Column (3), the main coefficient of interest is estimated as 13.78 bps, which is very similar to our baseline result. Based on these results, we argue that our results are not explained by the bondholders' expectations for a change in local individual income tax rates.

Finally, we consider the non-linearity in bond level payoffs by accounting for duration effects in the baseline specification. In this regard, we modify the baseline specification in Columns (4)-(5). First, in Column (4), we show our main effect by replacing years to maturity and inverse years to maturity at the bond level by the corresponding duration using (pre-tax) average yield for the bond-month observation. This results in a higher impact of 14.02 bps. We show the same result by re-calculating duration based on after-tax yields in Column (5). This tax adjustment further increases the impact to 14.29 bps.

4.2.3 Clustering

Next, in Panel C, we consider alternative ways to cluster standard errors in our baseline specification of Equation (1). We report our results in Columns (1)-(3). In our baseline we double cluster standard errors at county specific bond issuer and year-month level to account for correlations in yields for a given issuer. Here, in Column (1), we show that our main result is not affected by single clustering standard errors at the county-specific bond issuer level. This would be relevant if there is a concern that yield spreads from bonds of the same municipal bond issuer may be correlated with another. We show that our baseline effect is statistically significant under this consideration. In Column (2), we single cluster standard errors by bond issue. Finally, in Column (3), we show our results by double clustering standard errors by county bond issue and year-month. This specification addresses concerns of correlation among standard errors in yield spreads within bond issues over time. The statistical significance in all of these considerations suggests that our results are robust to alternative strategies of clustering.

4.2.4 Alternative Event Window

We use a three-year window in our baseline specification. There might be a concern about how sensitive our results are to the choice of the event window. Specifically, our results in Panel D bring out the robustness of our main effect to the choice of the event window. First, in Column (1), we use a shorter window than the baseline. We find that the bond yield spreads increase by 8.66 bps when using a 24-month event window. In Columns (2) and (3), we show our main result using longer windows of 48 months and 60 months, respectively. We find the magnitude to be higher than our baseline effect and statistically significant. In summary, these alternative definitions of the event window show robustness to our main specification in this regard.

4.2.5 Other Dependent Variables

Finally, for the robustness of our main result to the choice of the dependent variable, we show our results in Panel E. First, in Column (1), we use the after-tax yield (tax-adjusted average yield) as the dependent variable and estimate the coefficient of interest as 15.54 bps without using any controls in the regression framework. Next, we add the bond level and county level controls into the regression. Our results show an increase in yield of 15.00 bps in Column (2). Likewise, we repeat this scheme using yield spread as the dependent variable in Columns (3) and (4). The magnitudes are lower in this case as we do not adjust for tax differentials among states. Overall, we show that we find similar results by using dependent variables in which yields are only adjusted for taxes or only for spread over the risk-free rate.

Overall, we provide results in this section for the robustness of our baseline specification to observable and unobservable factors, additional tax considerations and duration, clustering standard errors, choice of the event window, and the choice of other dependent variables. Separately, we conduct a falsification test, wherein we consider the impact on the bonds of the winning county that have negligible credit risk (i.e., bonds which are pre-refunded with escrow accounts in state and local government securities). We find that the announcement of the subsidy deal doesn't have an impact on bonds of winning counties with minimal credit risk, further reinforcing our main results. Section IA3.1 provides further details. We provide further robustness checks on size of trade, subsidy deals related to the financial crisis of 2009, recently issued bonds, and additional county-level controls in Section IA3.2.

4.3 Mechanism

As we discussed before, the local governments face a trade-off while using targeted business incentives, i.e., anticipated jobs multiplier benefit (see Greenstone, Hornbeck, and Moretti (2010)) versus foregoing future tax revenue. We motivate our subsequent analysis from a standard corporate finance framework in which positive NPV projects increase the firm's value and that of its bondholders and shareholders. In our setting, we think of local citizens/taxpayers as ‘equity holders’ with local governments undertaking corporate subsidy deals to create value. Our results in the previous section suggest that bondholders’ value reduces on average after the subsidy announcement. However, it is unclear whether this reduction is due to lower anticipated benefits (jobs) from the project or the higher cost of financing the deal. This section sheds light on the mechanism by arguing that the anticipated jobs multiplier and the underlying debt capacity drive our main result.

4.3.1 Anticipated Jobs Multiplier Effects

First, we evaluate the heterogeneity of our results based on potential benefits after the subsidy deal. As noted before, a direct assessment of the future economic impact of the subsidies on the local community is challenging. Most of these projects have a long gestation period and the benefits get realized over a longer horizon. We hypothesize that the municipal bond prices reflect the expected local benefit from the subsidy deal. We measure the anticipated multiplier effects using two proxies: a) anticipated jobs multiplier using input-output tables and b) knowledge spillover using firm patents.

We construct the measure of anticipated jobs multiplier effect by summing up the proportion of value-added in the upstream and downstream segments of the firm’s industry, weighted by the corresponding county’s share of wages. We obtain sector-level data on upstream and downstream value-added fractions from real-valued input-output tables provided by the Bureau of Labor Statistics (BLS). Our sector-level NAICS mapping comes from the BLS crosswalk. See Table A1 for variables description. In Figure 5, we provide dynamic evidence from bond yield spreads corresponding to the interaction effect over groups of winning counties based on the ex-ante anticipated jobs multiplier effect. To ensure that our measure for anticipated jobs multiplier is sufficiently outside the event window, we construct it one year before the event window of (-3,+3) years around the subsidy announcement.

We modify Equation (2) to include interaction terms of the winner dummies only. We also introduce group year-month fixed effects in this specification. Therefore, the coefficients represent the difference-in-differences estimates over time for the high and low groups, benchmarked to the losing counties. Comparing sub-figures (a) and (b) shows that the differential impact on bond yield spreads is predominantly driven by winning counties with low anticipated jobs multiplier. Specifically, the coefficients are small and insignificant in the quarters before the deal announcement. We find that the differential impact between winners and losers in the low multiplier group increases from about 11.9 bps after the deal to 35.8 bps at the end of three years. The above results further shed light on the potential economic impact of large corporate subsidy deals by documenting their effects on the borrowing costs of local governments. We provide tabular results for the coefficient estimates in Table IA8.

Next, to measure knowledge spillover, we follow Kogan, Papanikolaou, Seru, and Stoffman (2017) to quantify the economic importance of patents originating from firms receiving subsidies. Specifically, we use the aggregated dollar value of innovation for patents granted over three (and five) years before the deal[17]. We use our baseline Equation (1) interacted with dummies corresponding to values above and below the median for the winning counties. We additionally control for group $\times$ year-month fixed effects. We show our results in Figure 6. We find that municipal bond yield spreads increase by 18-24 bps more for winning counties involving subsidy deals of low patent value.

Overall, we highlight the mechanism based on expected benefits from the anticipated jobs multiplier. We show that the impact on bond yields is higher and statistically different for winning counties that attract/retain firms in industries with lower anticipated jobs multiplier or lower valued innovation. These results are consistent with municipal bond investors incorporating the expected benefits of the deal in their valuation. Next, we discuss the impact of a county's debt capacity on the cost of borrowing.

4.3.2 County Debt Capacity

Myers (1977) and Turnbull (1979) argue that a firm's debt capacity affects the value of future investment projects. Therefore, even a non-negative NPV project may shift value from bondholders to shareholders for local governments with low debt capacity. Thus, any non-negative NPV subsidy deal may increase the secondary bond yields for low debt capacity local governments. We consider the ex-ante debt capacity of local governments using three proxies: a)

[17]We are able to match 59 deal-pairs in our subsidy database to the patents granted. Following Kogan, Papanikolaou, Seru, and Stoffman (2017), we use the issuing year of patents. For deals that can be linked to the patent-CRSP (firm) database but do not have any patents associated with them, we assign their value of innovation as zero.

interest expenditure, b) net debt, and c) county credit ratings. First, we expect the secondary market impact to be higher for winning counties with large ex-ante interest expenditure. To this end, we use the ex-ante interest on general debt scaled by three measures of revenue, and one measure of total debt, namely: (i) Revenue_1, (ii) Revenue_2, (iii) Revenue_3, and (iv) total long term debt outstanding. We define the three approaches of calculating revenue in Table A1. We use interest expenditure in the year preceding the deal, scaled by the corresponding fiscal metric two years before the deal. A high value of the interest expenditure measure corresponds to a low debt capacity.

We divide the winning counties into two bins based on the median of the interest expenditure measures (as defined above). Using our baseline Equation (1) with interactions for the bins, we estimate the differential impact on high versus low debt capacity counties. We also include group-month fixed effects in the regression. We present our results in Table 5, which shows the coefficient of the interaction term for each group. First, Column (1) shows that for counties with a high $\textit{Interest/Revenue}_1$ ratio, the bond yield spreads increase by 17.55 bps. This corresponds to the higher debt burden these counties face since more of their revenue is devoted to meeting general debt interest costs. Similarly, Column (2) suggests that the borrowing cost for counties with higher $\textit{Interest/Revenue}_2$ ratio goes up by 26.68 bps. We find a similar result in Column (3) using $\textit{Revenue}_3$. Column (4) shows the impact on bond yield spreads due to differential interest to debt ratio. A higher value of the measure results in an increase in borrowing cost by 27.31 bps. The difference between the two groups is economically meaningful and statistically significant when using the scaled measure of interest expenditure in each approach.

Next, we use the county's ex-ante measure of net debt outstanding to uncover differences in debt capacity. We define the net debt outstanding as the difference between the total debt outstanding and the amount of debt retired. In Column (5) of Table 5, we show that counties with higher ex-ante net debt experience an increase in bond yield spreads of 16.49 bps. A higher net debt burden implies a lower debt capacity. As before, the difference between the two groups is statistically significant.

Finally, we consider our third proxy for debt capacity based on county credit ratings. Table 6 shows the results for our analysis. We interact our baseline Equation (1) with dummy variables corresponding to ex-ante high credit rating versus low credit rating winning counties[18]. We divide the winning counties based on the median value of the credit rating numeral. As before, we control for the average effect within a group in a given month using the relevant fixed effects. We find that a lower credit rating is associated with a higher increase in the winning county's municipal bond yield spreads. In Column (1), using all bonds (with a rating) in our sample, we find that yield spreads increase by 23.27 bps for counties with a rating below the median.

Next, we consider if the timing of the subsidy announcement confounds with the declining economic health of the winning counties. If this is true, we should expect to see an increase in yields for both *revenue bonds* (bonds supported by revenue from a specific project) and *general obligation bonds* (bonds that are backed entirely by the issuers' creditworthiness and ability to levy taxes on its residents) among the winning counties of poor economic health. We find a strong positive effect on yields of *general obligation bonds* (Column 2) with yield spreads increasing by 22.02 bps for low rated (below the median) counties. The difference between the two groups is economically and statistically significant. Meanwhile, there is an insignificant impact on *revenue bonds* of counties with low credit rating.

To summarize, our results in this subsection provide evidence suggesting the ex-ante debt capacity of winners as the underlying channel. We show that winning counties with a higher interest expenditure are associated with up to 27 bps as additional borrowing cost. Similarly, winning counties with high net debt (low debt capacity) experience a greater increase in their bond yield spreads. Further, low rated counties also pay more for their debt after the deal.

4.3.3 Does a high multiplier alleviate debt capacity constraint?

Our results in Sections 4.3.1 and 4.3.2 provide evidence based on the anticipated jobs multiplier and county-level debt capacity. These results motivate our evaluation of the interaction effect between debt capacity and anticipated jobs multiplier to address: Does a high multiplier alleviate debt capacity constraints of local governments? We divide the winning counties into two groups based on the median value of the anticipated jobs multiplier. We modify the

[18]The county-level credit rating is based on the average S&P municipal bond rating obtained from the FTSE Russell Municipal Bonds dataset. To use a clean period before the event, we focus on ratings of corresponding bonds issued from 12 to 24 months before the deal month. We use the numeric equivalent for the bond ratings with AAA representing the highest value.

baseline Equation (1) to interact with dummies corresponding to the ex-ante county-level debt capacity based on *Interest/Revenue*$_1$. We additionally control for group-month fixed effects in the regression. Figure 7 shows our results from this analysis. For deals with low anticipated multiplier, we find that bond yield spread increase by 30.19 bps when the *Interest/Revenue*$_1$ ratio is above median. The corresponding impact on the low *Interest/Revenue*$_1$ ratio group is 15.28 bps. Meanwhile, for subsidy deals involving a high anticipated jobs multiplier, the bond yield spreads increase by 14.49 bps when the interest expenditure is high, but there is no significant impact when the value is low.

Regardless of whether the anticipated jobs multiplier is low or high, our analysis shows that the differential impact due to a high *Interest/Revenue*$_1$ ratio is 13–14 bps. This evidence seems to suggest that a high multiplier effect does not seem to alleviate the debt capacity constraints of local governments.

In addition to debt capacity of the county and anticipated jobs multiplier effects, the amount of subsidy given to attract or retain firms to the county relative to the projected benefits is likely to influence the response of municipal bond investors after the deal. To assess the relative bargaining power between the county and the firm, we use the following: a) *Proposed Value*, b) ratio of investment to state revenue, c) intensity of bidding competition, and d) county's unemployment rate. A lower bargaining power causes a greater increase in yields (between 16-22 bps). We provide details in Section IA3.5.

Taken together, our results in Section 4.3 suggest that a lower anticipated jobs multiplier and lower debt capacity (higher interest expenditure or higher net debt) increase the impact on borrowing cost. The combined effect is dominated by the debt capacity, with the jobs multiplier not attenuating the effect. In Section IA3.4, we find consistent evidence in terms of the probability of bond rating downgrades for the winning counties using our measures of debt capacity. In the next section, we consider the impact on issuance of new municipal bonds.

4.4 Impact on New Issuance of Municipal Bonds

First, we consider the volume of municipal debt issued in the form of bonds. Given that some of the additional economic activity/expansion would have to be financed through borrowings, we expect the winning counties to issue more debt. This especially could be the case when the winners need to create the infrastructure required to support the large plant. Instead of diverting cash from regular sources of revenue (which may have been already earmarked for dedicated uses), borrowing in the public market could be a feasible option. In this light, we present our results in Figure 8a where we compare the volume of bond issuance at the county level between the winning and losing counties after the deal announcement.

For each county, we calculate the total par value of bonds issued during the six months before the corresponding deal event window (comprising T=-13 to T=-18 months). We normalize this value to one and compute the total par value of new issuances relative to this amount in subsequent half years. The ratio represents the relative growth in issuance among winners, compared to the corresponding growth of losers. The vertical bars in the figure show the upper and lower limits based on the standard error of the mean values. We find that the winning counties issue nearly 2-3 times more debt in each of the half years immediately following the deal up to three years after the deal. Using Figure 8b, we find evidence consistent with our proposed mechanism. Counties with a low $Interest/Revenue_1$ ratio (higher debt capacity) are able to issue more debt than their counterparts. Specifically, in the year after the deal their issuance of municipal bonds increases to about 4-6 times with respect to pre-event benchmark.

As a final step in our analysis of the primary market of new municipal bonds, we evaluate the impact of the subsidy announcement on offering yields. The aggregate new issuance among the winning counties three years after the deal announcement is \$190 billion. Our coefficient of interest is similar to the difference-in-differences estimate in the secondary market using Equation (1). However, we additionally introduce issuer fixed effects. We also control for bond ratings at the time of issuance. We double cluster standard errors at the county specific bond issuer and dated month level. We show our results in Table 7. In Column (1), we estimate the difference-in-differences coefficient from within the same county-pair, absorbing for the county fixed effect and issuer fixed effect. We report an increase of 11.97 bps. We show our results in Column (4) after controlling for bond characteristics, local economic conditions, and ratings where the main estimate is 4.55 bps. This is equivalent to an increased borrowing cost for local governments amounting to 2.08% (=0.79/38)[19] of the total subsidy (\$38 billion) offered during

[19]The new municipal debt issued by the winning counties in the three years after the deal is $\sim$ \$190 billion. The average maturity of bonds issued by them in the three years after the deal was 9.3 years, with an average offering yield of 3.01%. Based on a modified duration approach, we compute the aggregate impact for three years

the sample period.

Overall, our results suggest an increase in offering yields of new municipal issuance by about 4.55 bps after the subsidy announcement. However, one caveat to these results is that counties may rationally expect a higher borrowing cost following the deal announcement and may try to time the market in raising new debt. That is one of the reasons why we focus on the secondary market trades of existing bonds in our baseline analysis to evaluate the impact of the corporate subsidy deal on borrowing cost.

4.5 Impact on Local Economy, Property Taxes and Public Expenditure

So far, we find evidence suggesting an increase in the borrowing cost of local governments after the deal both in the primary and secondary bond market. We also show that the new municipal bond issuance increases for winning counties (compared to the losing counties), but only for counties with low-interest expenditure, i.e., high debt capacity. Keeping these results in mind, we now investigate the impact on local property taxes and local public expenditure similar to Adelino, Cunha, and Ferreira (2017).

Following a large plant subsidy, the local governments may be faced with additional/new demand for public services or risk cutting quality/services. They may choose to finance this growth by raising taxes, increasing borrowing, or both. To evaluate the relative preference between the choices of financing, we extend our analysis to property taxes using the annualized version of Equation (2), while replacing year-month fixed effects with event-year fixed effects.

In Figure 9a, we find evidence that winning counties with high interest expenditure which could not issue new debt showed an increase in property tax revenue. Winning counties with a high $Interest/Revenue_1$ ratio (low debt capacity) increase their property tax revenue per capita by about USD 100 after the deal. Interestingly, we do not find a commensurate increase in the house price index for these counties, as shown in Figure 9b. Meanwhile, this effect is relatively muted (and statistically insignificant) for winning counties with a low $Interest/Revenue_1$ ratio. Taken together, these results seem to suggest that local governments with debt capacity constraints may be increasing tax rates or reassessing property values to realize this higher property tax revenue.

after the deal on a semi-annual basis as \$190 $\times$ 9.3 $\times$ 0.000455/(1+3.01%/2) billion = \$0.79 billion.

Given that the reported motivation behind offering corporate subsidies is to promote county-level economic growth, we also consider the impact on the local economy and local public expenditure. First, in Panel A of Table 8, we show the implications for county-level employment growth from QCEW and the unemployment rate from the Bureau of Labor Statistics. We use the annualized version of Equation (1). Here, we do not include county controls because they may be persistent and bias our regression coefficients. Column (1) shows that winning counties experience a 0.6% growth in employment when compared to the losing counties. Column (4) suggests that there is no meaningful overall effect on the unemployment rate after a subsidy deal. Further, we interact the main equation with dummies corresponding to above-median (high) and below-median (low) values of the anticipated jobs multiplier effect and $Interest/Revenue_1$ ratio among winning counties. Based on Column (2), we find weak evidence toward a 0.95% increase in employment growth among winning counties with a high anticipated jobs multiplier effect. However, the difference between the two groups is not statistically significant. Column (3) suggests weak evidence toward a 1.06% increase in employment growth among winning counties with a high $Interest/Revenue_1$ ratio.

We also study the impact on the local economy based on entrepreneurship and payroll growth. We present our analysis in Panel B of Table 8. In Column (1), we report the aggregate effect on all winners when compared to the losing counties in the three years after the subsidy deal. Our dependent variable is logged number of establishments at the county-level obtained from the County Business Pattern. We do not find a meaningful change in entrepreneurship, as measured from the number of establishments. There is no differential impact between winning counties based on the anticipated jobs multiplier effect and the debt capacity measure (Columns (2)-(3)). Further, we use the annual payroll growth (%) as the dependent variable for our analysis in Columns (4)-(6). Column (4) suggests that the annual payroll growth for winning counties increases by about 1% when compared to the losers. We do not find statistical difference between winning counties of low and high values of anticipated jobs multiplier and debt capacity. However, Column (6) suggests weak evidence for a higher effect among winning counties with low $Interest/Revenue_1$ ratio. This provides some evidence for high debt capacity (low $Interest/Revenue_1$ ratio) winners experiencing annual payroll growth.

Finally, we show the impact on county-level expenditures around the corporate subsidy deal announcement in Table 9. We use the annualized version of Equation (1) as the primary specification for this table. First, in Columns (1)-(4), we show the aggregate effect for the coefficient of interest from our difference-in-difference setting. We scale all the dependent variables by the county population of the corresponding year to get the per capita impact. Our measure of *Health Expenditure* consists of per capita expenditure on health, hospitals, and public welfare. For *Police and Protection Expenditure*, we use per capita spending on police protection, fire protection, correctional expenditure, and judicial expenditure. In Columns (1)-(4), we find that there is no significant change in total expenditure, elementary education expenditure, health expenditure, and police and protection expenditure. Next, we show the interaction effects in Columns (5)-(8) by using our proxy for debt capacity in the form of the $Interest/Revenue_1$ ratio. Column (7) shows a significant decline in health expenditure per capita of USD 44.54 for winning counties with a high $Interest/Revenue_1$ ratio. This amounts to 11.11% (=$44/$396) of the average healthcare expenditure per capita for the winning counties before the subsidy deal. For the average winning county, the per capita expenditure on healthcare amounts to 10.23% of the total spending on a per capita basis in our sample.

Overall, we find evidence suggesting that there is an increase in borrowing costs for the winning county after the subsidy announcement. This is driven by counties with low anticipated jobs multiplier effect and low debt capacity. As a result, only counties with high debt capacity are able to issue new debt after the deal. On the other hand, counties with low debt capacity seem to rely on property tax revenue to finance the increased demand for public services. We find weak evidence of employment growth but no significant changes in total public expenditure on local public services after the deal. However, debt constrained counties appear to reduce their spending on healthcare on a per capita basis.

5 Conclusion

Corporate subsidies have recently attracted much attention in the United States. Some policymakers favoring corporate incentives highlight the importance of creating more jobs, while others worry about the costs of financing the incentives and the additional burden on public services. In light of this divergence, our paper evaluates how subsidy deals impact the borrowing costs of local governments and their expenditure on civic services. Counties face a trade-off while using targeted business incentives for economic development, i.e., foregoing future tax revenue versus anticipated jobs multiplier gains. If the additional civic burden requires local governments to raise more debt, the underlying debt capacity may impact the borrowing cost. On the other hand, the overall benefit from the expected jobs multiplier may attenuate the impact of debt capacity.

Using detailed hand-collected data on corporate subsidy deals worth USD 38 billion during 2005-2018, we provide new evidence through the lens of municipal bond yields. We find that the cost of municipal debt in the secondary market increases for the winning counties compared to the losing counties. This amounts to a reduction in bondholders' wealth of about $4.3 billion, which is 11.3% of the total subsidy offered. We propose a mechanism based on the ex-ante debt capacity of the winning counties and the anticipated jobs multiplier effects. We find a more significant increase in yield spreads after the deal for counties with lower debt capacity and a lower anticipated jobs multiplier. We also document additional debt issuance by counties that have a high debt capacity. In contrast, counties with low debt capacity observe an increase in property tax revenue per capita without a corresponding change in the house price index. This seems to suggest that these counties likely finance the additional civic burden after the deal by raising property tax rates (or assessments) while not observing meaningful growth in employment.

To the best of our knowledge, our paper is the first to document the impact of corporate subsidies on the borrowing cost of local governments. Our results highlight that the costs of some of the corporate subsidy deals to some of the counties may be more than the benefits from attracting or retaining firms through the subsidy deal.

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Table A1: Description of Key Variables

This table reports variable definitions. Data sources include the municipal bond transaction data from the Municipal Securities Rulemaking Board (MSRB), FTSE Russell's Municipal Bond Securities Database (FTSE, formerly known as Mergent MBSD), zero coupon yield provided by FEDS, highest income tax bracket for the corresponding state of the bond issuer from the Federation of Tax Administrators (FTA), Census data from the Census Bureau Annual Survey of Local Government Finances (CLGF), input-output tables from Bureau of Labor Statistics (BLS), wages and employment data from Quarterly Census of Employment and Wages (QCEW), and subsidy data from Subsidy Tracker which was enhanced through hand-collection (ST-HC).

Variable	Description	Source
Winner	Dummy set to one for a county that ultimately wins the subsidy deal. This dummy equals zero for the runner-up county in that subsidy deal.	ST-HC
Post	Dummy that is assigned a value of one for months after the deal is announced and zero otherwise.	ST-HC, MSRB
Average Yield	Volume-weighted average yield for a CUSIP in a given month. Volume refers to the par value of the trade.	MSRB
Yield Spread	Calculated as the difference between the *Average Yield* and the coupon-equivalent risk free yield. The risk free yield is based on the present value of coupon payments and the face value of the municipal bond using the US treasury yield curve based on maturity-matched zero-coupon yields as given by Gürkaynak et al. (2007). This yield spread calculation is similar to Longstaff et al. (2005).	MSRB, FEDS
After-tax Yield Spread	Calculated as the difference between the tax-adjusted *Average Yield* and the coupon-equivalent risk free yield. The risk free yield is based on the present value of coupon payments and the face value of the municipal bond using the US treasury yield curve based on maturity-matched zero-coupon yields as given by Gürkaynak et al. (2007). This yield spread calculation is similar to Longstaff et al. (2005). We follow Schwert (2017) in applying the tax adjustment. It is calculated as below: $$spread_{i,t} = \frac{y_{i,t}}{(1-\tau_t^{\text{fed}})*(1-\tau_{s,t}^{\text{state}}))} - r_t$$	MSRB, FEDS, FTA
Competitive Bond Dummy	Dummy variable that equals 1 if the issue is sold to underwriters on a competitive basis and is 0 otherwise	FTSE

Variable	Description	Source
GO Bond Dummy	Dummy variable for general obligation bond. A GO bond is a municipal bond backed by the credit and taxing power of the issuing jurisdiction rather than the revenue from a given project.	FTSE
Log(Amount)	Log transformation of the dollar amount of the individual bond's (9-digit CUSIP) original offering.	FTSE
Callable Dummy	Dummy variable that equals 1 if the issue is callable and is 0 otherwise.	FTSE
Insured Dummy	Dummy variable that equals 1 if the issue is insured and is 0 otherwise.	FTSE
Remaining Maturity	Individual bond maturity measured in years.	FTSE, MSRB
Inverse Maturity	Inverse of the value of *Remaining Maturity*; to account for non-linearity.	FTSE, MSRB
Anticipated Jobs Multiplier	This metric represent the county's exposure in the industry (j) using the upstream (or downstream) sector (s) based on the fraction of total wages in that sector ($\eta_{s,t}^{county}$), derived from QCEW. The input-out tables from BLS also provide us with a share of value added by upstream sectors in a given industry ($w_{s,t}^{j}$). To arrive at the county's sector level exposure for a given industry (j) ($e_{s,j,t}^{county}$) by summing up the upstream sector linkages, we follow: $$e_{s,j,t}^{county} = \sum_{s} w_{s,t}^{j} * \eta_{s,t}^{county}$$	BLS, QCEW
Revenue$_2$	(Total Revenue - State Inter-Governmental Transfers) - (Total Expenditure - Interest on Total Debt)	CLGF
Revenue$_3$	(Total Revenue - State Inter-Governmental Transfers) - (Total Expenditure - Interest on General Debt)	CLGF
Interest/Revenue$_1$	Interest/Revenue$_1$=$\frac{\text{Interest on general debt}}{\text{Total Revenue}}$	CLGF
Interest/Revenue$_2$	Interest/Revenue$_2$=$\frac{\text{Interest on general debt}}{\text{Revenue}_2}$	CLGF
Interest/Revenue$_3$	Interest/Revenue$_3$=$\frac{\text{Interest on general debt}}{\text{Revenue}_3}$	CLGF
Interest to debt	Ratio of interest on general debt to total long term debt outstanding for the county.	CLGF

Variable	Description	Source
Interest to expenses	Ratio of interest on general debt to total expenditure for the county.	CLGF
Net debt	Difference between total debt outstanding and total debt retired.	CLGF

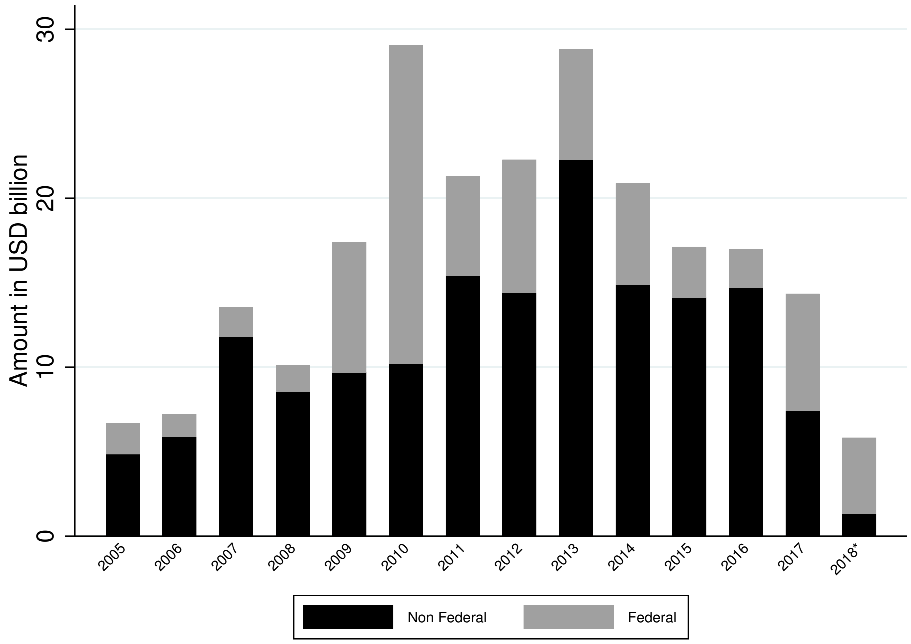


Figure 1: Total Subsidy: The vertical bars show the aggregated value of total subsidy offered by federal and non-federal (state and local) governments for each year during 2005-2018. This does not include federal loans. Calculated based on Source: Good Jobs First, Subsidy Tracker. *Denotes incomplete data for the year, until June 2018

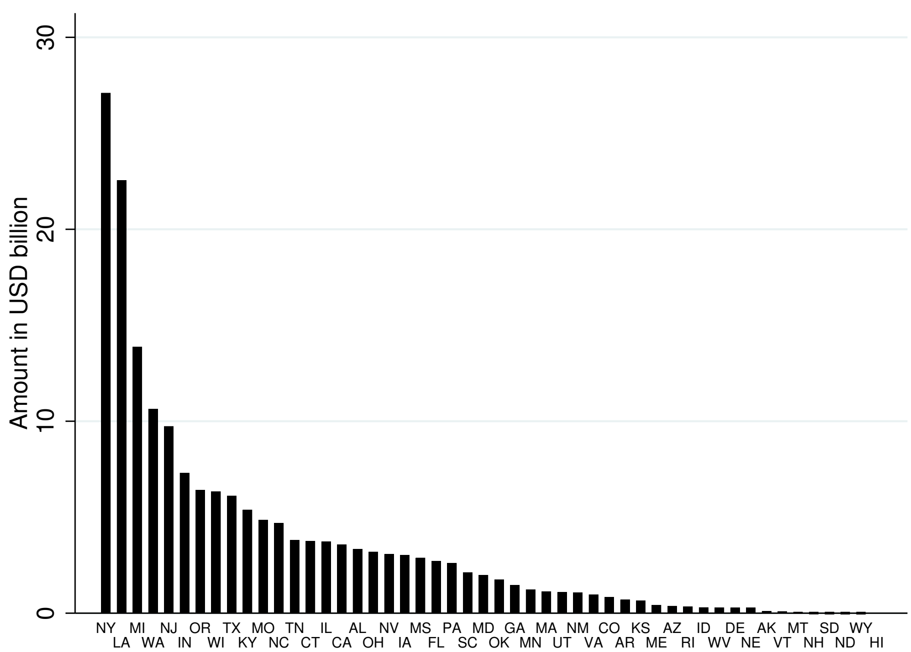


Figure 2: Total Subsidy by States: The figure shows ranking among US states based on total non-federal subsidy offered during 2005-2018. Calculated based on Source: Good Jobs First, Subsidy Tracker

Average Yield (bp)

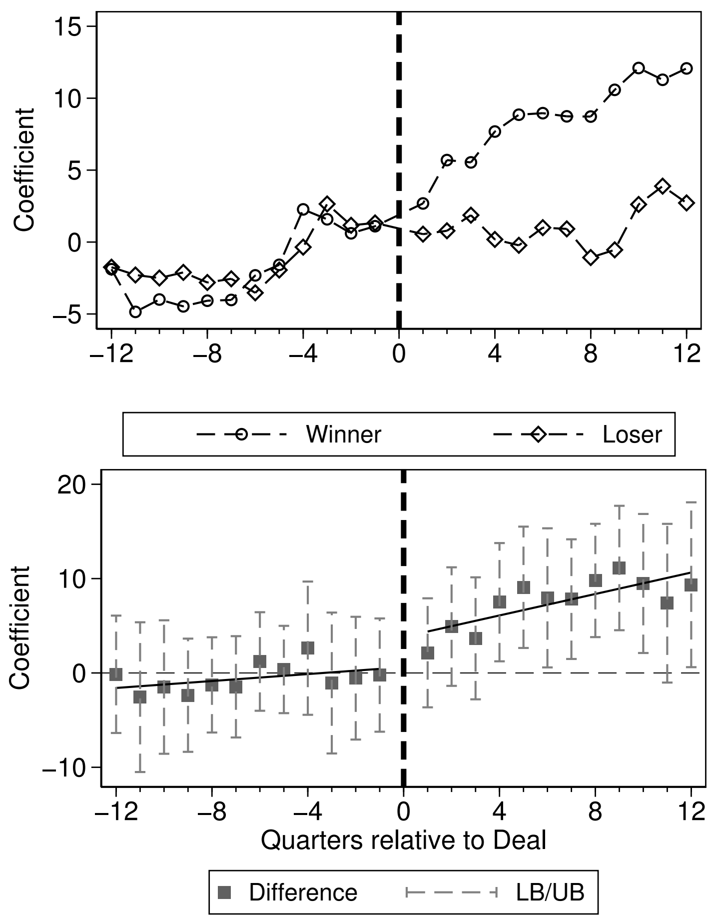


Figure 3: Baseline Result - Winner vs Loser: In this figure, we plot the average yield for municipal bonds traded using Equation (2). We also show the differences between the yields of winning and losing counties. See Table A1 for variables description. The coefficients are shown in basis points. We regress the average yields on monthly interaction dummies for winner and loser using county-pair (winner-loser pair) fixed effects, county fixed effects, and year-month fixed effects. We also add county specific year-month trends. We depict the coefficients on a quarterly scale on the x-axis, where 0 corresponds to the month of the subsidy deal announcement. The first quarter dummy after announcement subsumes the month of announcement. The omitted benchmark period is a quarter before the event window around the deal, i.e., (-12,12) quarters. Standard errors are double clustered by county bond issuer and year-month. The dashed lines represent 95% confidence intervals.

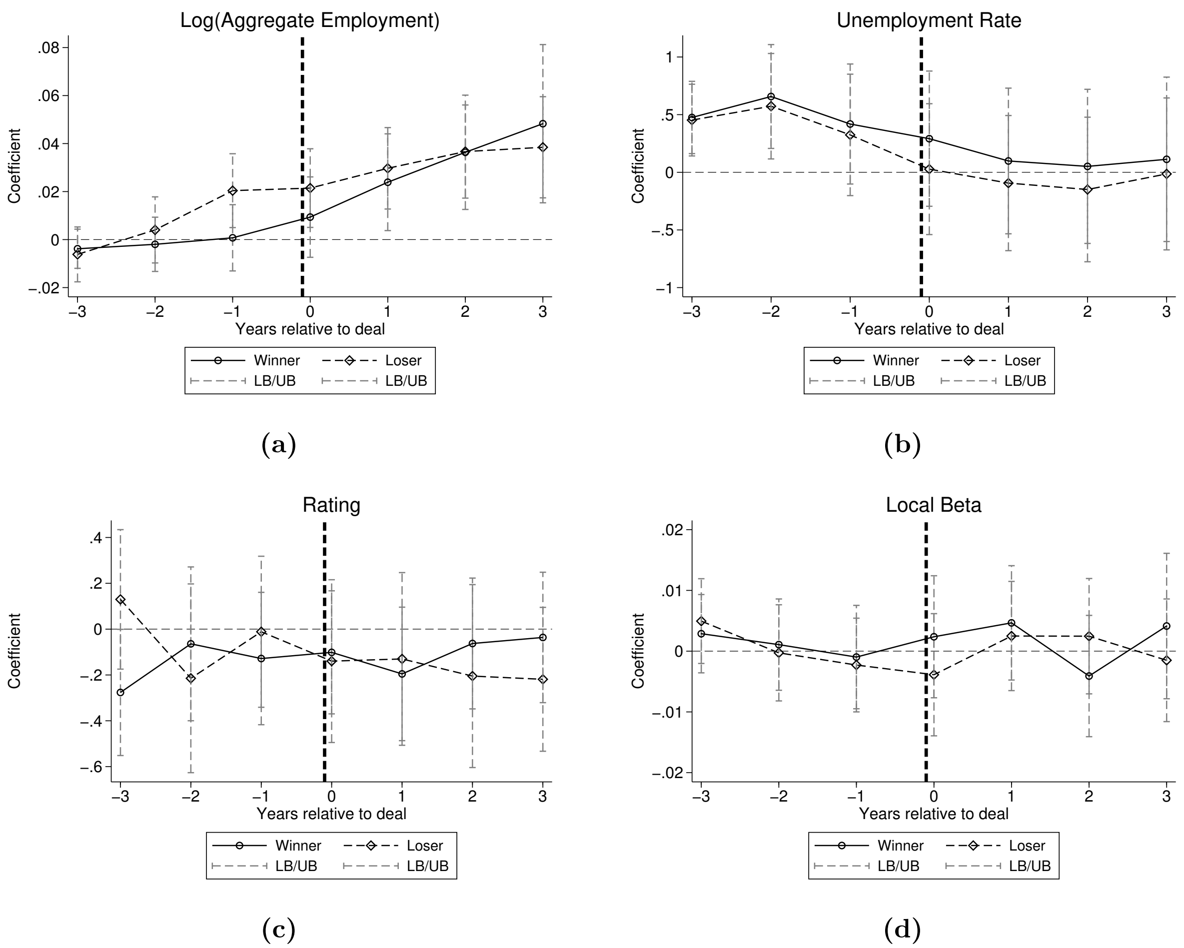
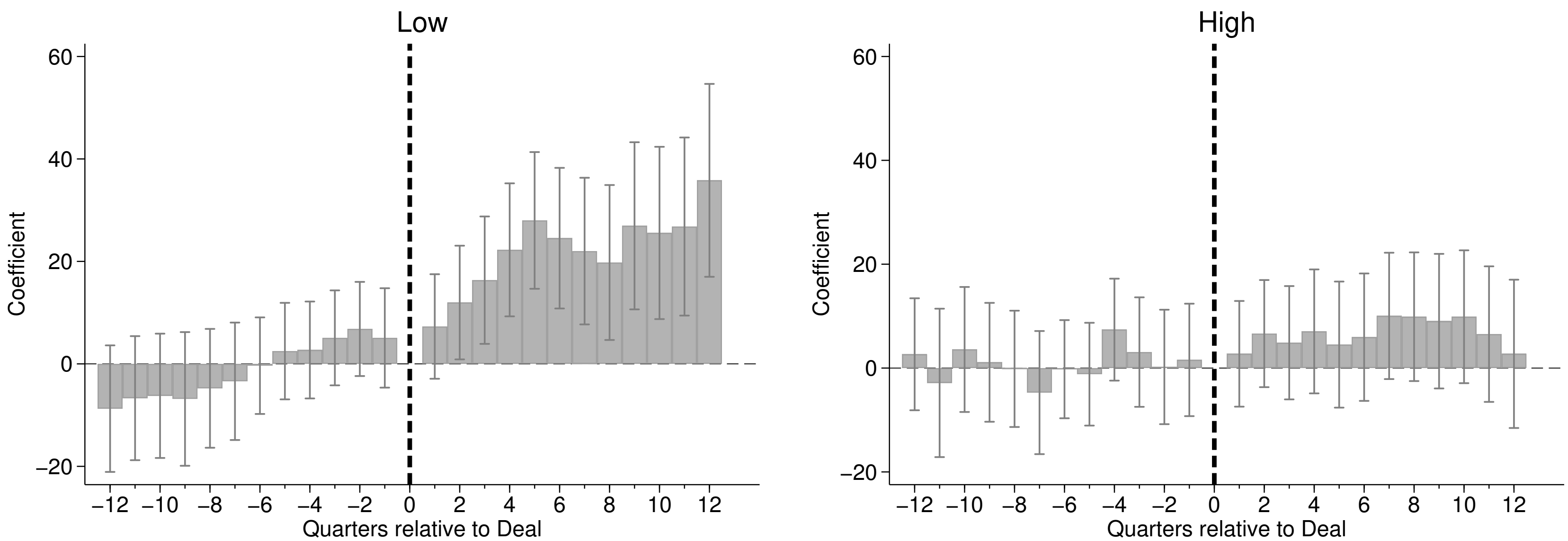


Figure 4: Identifying Assumption - Local Economy: The figure shows the local economic conditions at the county level between the bidding counties, around the event of subsidy announcement. We use the annualized version of Equation (2). Here, we cluster standard errors at the deal level. The benchmark period is the year before the window (-3,3) years. The dashed lines represent 95% confidence intervals.

After-tax Yield Spreads (bp) by Ex-Ante Anticipated Jobs Multiplier



(a) (b)

Figure 5: Effect of Anticipated Jobs Multiplier (I): This figure shows regression coefficients for after-tax yield spreads from modified Equation (2), interacted over groups of winning counties based on the ex-ante anticipated jobs multiplier effect. We only include the interaction terms of the winner dummies. Therefore, the coefficients represent the difference-in-differences estimates over time for the high and low groups, benchmarked to the losing counties. We construct the measure of anticipated jobs multiplier effect by summing up the proportion of value-added in the upstream and downstream segments of a given industry, weighted by the corresponding county's share of wages. See Table A1 for variables description. We modify the equation to include group-year-month fixed effects. The coefficients are shown in basis points. In sub-figures (a) and (b), we show the coefficients from deals below and above the median value of the anticipated multiplier measure, respectively. We depict the coefficients on a quarterly scale on the x-axis, where 0 corresponds to the month of subsidy deal announcement. The omitted benchmark period is a quarter before the event window around the deal, i.e. (-12,12) quarters. Standard errors are double clustered by county bond issuer and year-month. The solid lines represent 95% confidence intervals.

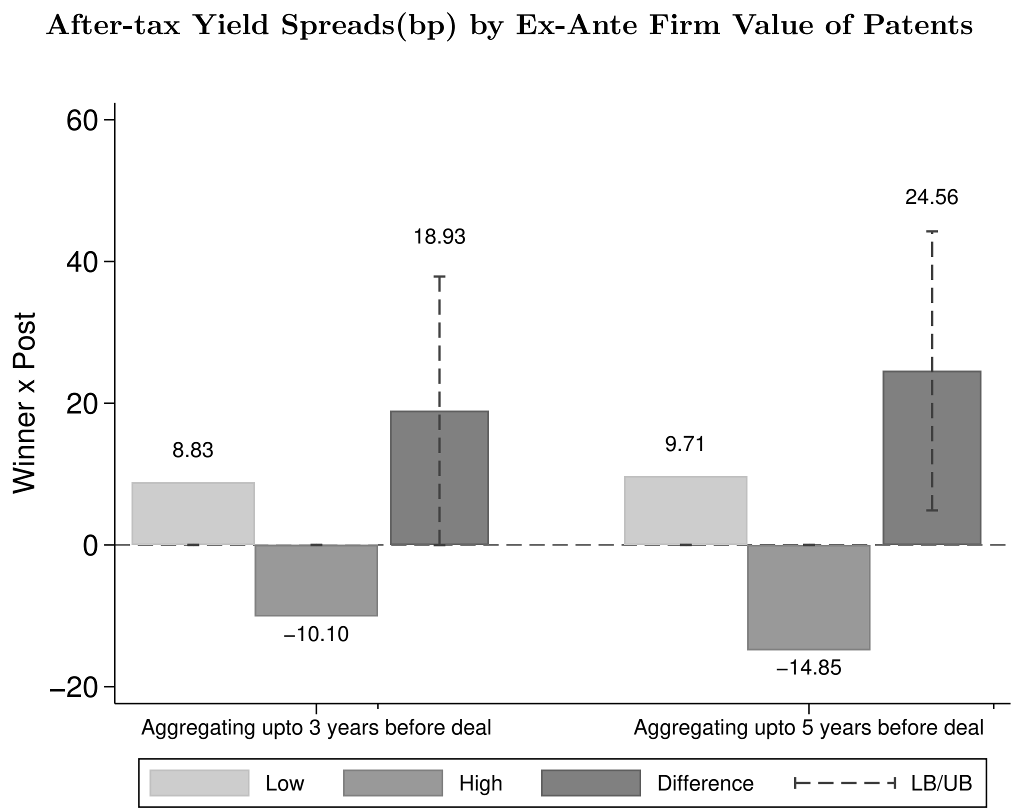


Figure 6: Effect of Anticipated Jobs Multiplier (II): The figure shows results for our main interaction term, β_0, from Equation (1). We modify the baseline equation to interact with dummies corresponding to the ex-ante firm value of patents. We additionally control for group-month fixed effects in the regression. We show results by aggregating the value of patents at the firm level 3 years and 5 years before the deal, respectively. Standard errors are double clustered by county bond issuer and year-month. The dashed lines represent 95% confidence intervals.

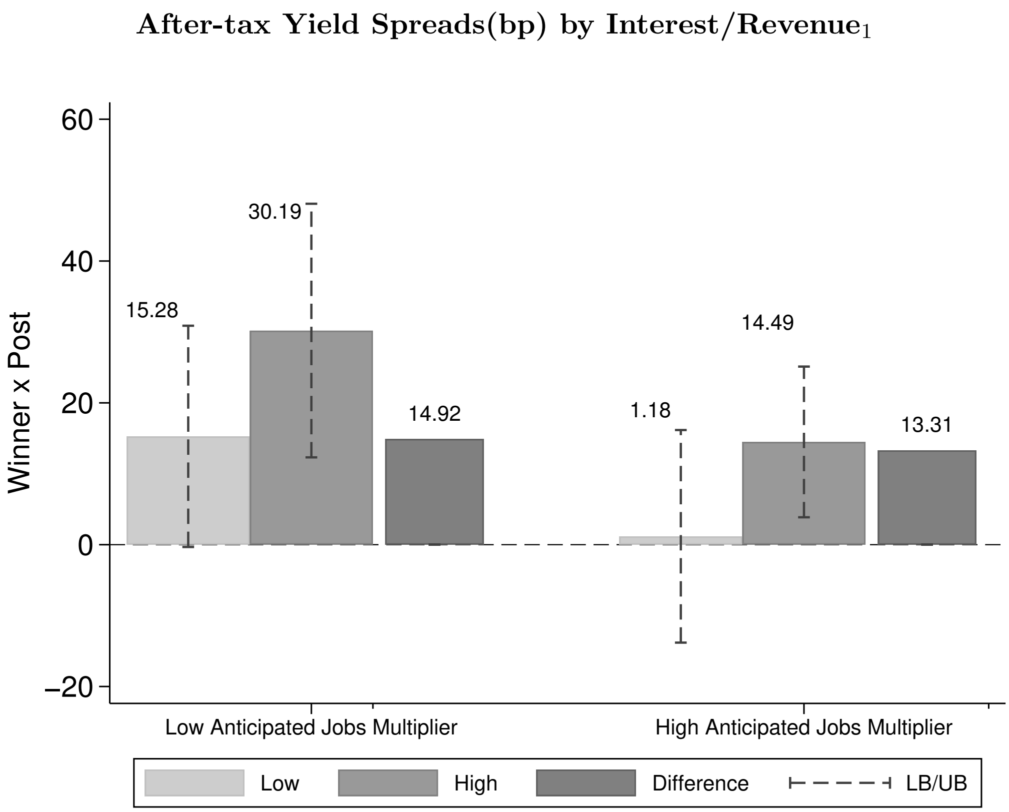
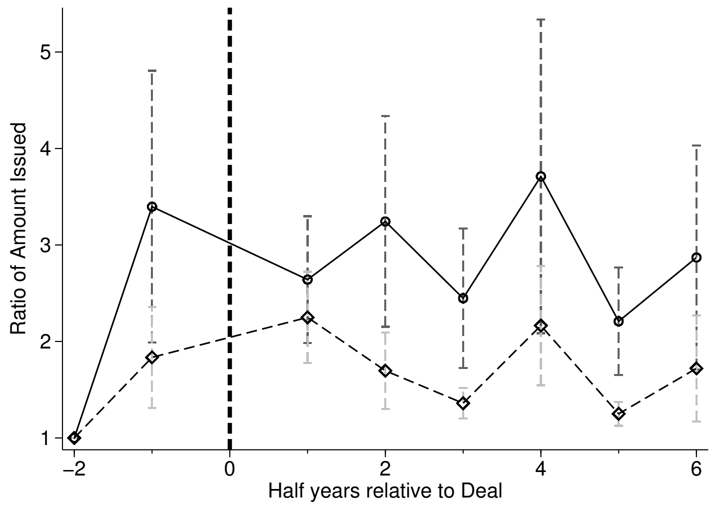


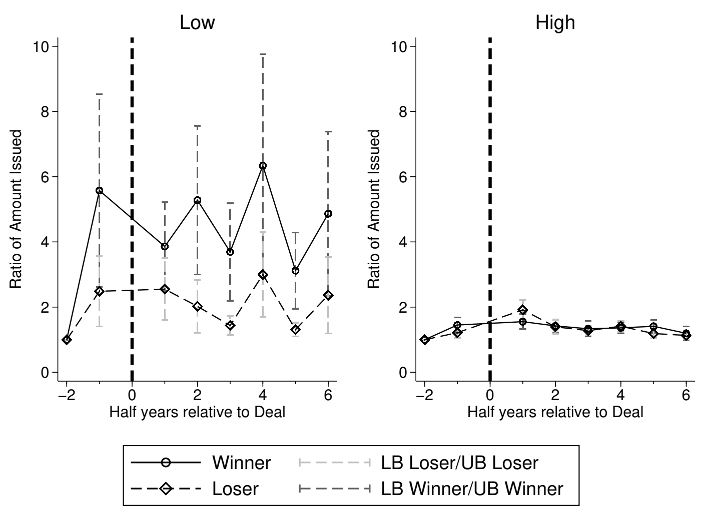
Figure 7: Effect of Anticipated Jobs Multiplier and Debt Capacity: The figure shows results for our main interaction term, β_0, from Equation (1). We modify the baseline equation to interact with dummies corresponding to the ex-ante county-level debt capacity based on *Interest/Revenue*$_1$. We additionally control for group-month fixed effects in the regression. We show results by using sub-samples across subsidy deals involving low and high anticipated jobs multiplier effect, respectively. See Table A1 for details on the construction of anticipated jobs multiplier effect. Standard errors are double clustered by county bond issuer and year-month. The dashed lines represent 95% confidence intervals.

New Municipal Bond Issuance



(a)

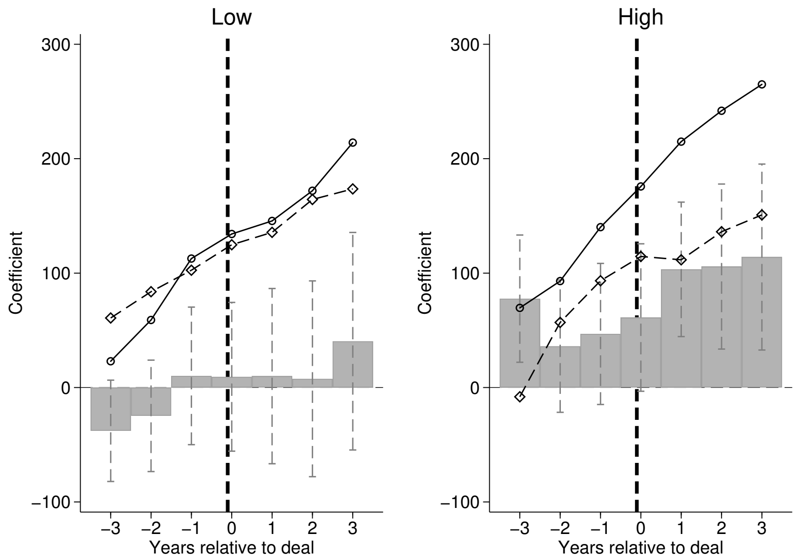
By Interest/Revenue$_1$:



(b)

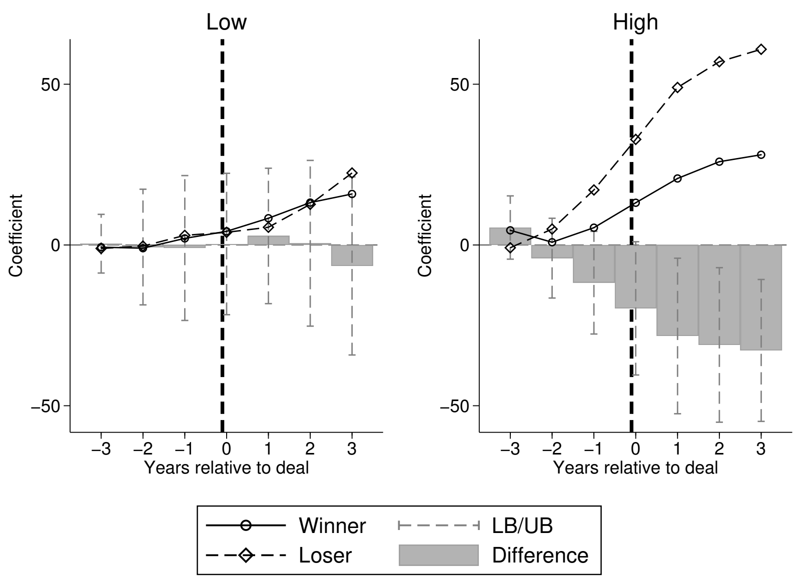
Figure 8: New Municipal Bond Issuance: The figure shows the county level aggregate volume of bond issuance for winners and losers after the deal announcement. For each county, we calculate the total par value of bonds issued during the six months before the corresponding deal event window (comprising T=-13 to T=-18 months). We normalize this value to one and compute the total par value of new issues relative to this amount in the half years after the announcement. The ratio represents the relative growth in issuance among winners, compared to the corresponding growth of issuance among losers. In sub-figure (a), we show the total issuance and in sub-figure (b), we split the sample based on the *Interest/Revenue*$_1$ ratio (defined as the ratio of interest on general debt to total revenue of the county). A low value of *Interest/Revenue*$_1$ ratio suggests a high debt capacity for the county. The vertical bars show the upper and lower limits based on the standard errors of the mean values.

Property Tax Revenue per capita By Interest/Revenue$_1$:



(a)

House Price Index By Interest/Revenue$_1$:



(b)

Figure 9: Impact on Local Property Taxes and House Price Index: The figure represents the relative changes in property taxes among winners sub-grouped based on *Interest/Revenue*$_1$ ratio, compared to the corresponding losers. In sub-figure (a), we show the property tax revenue per capita and in sub-figure (b), we represent the house price index obtained from the Federal Housing Finance Agency (FHFA). We use the annualized version of Equation (2), but additionally introduce event-year fixed effects. Here, we cluster standard errors at the deal level. The omitted benchmark period is the year before the event window of (-3,3) years. The dashed lines represent 95% confidence intervals.

Table 1: Summary Statistics: Subsidy Deals

This table summarizes the deal level characteristics on subsidy in our sample during 2005-2018. In Panel A, we provide summary statistics on all deals together. In Panel B, the deals are sub-divided based on the purpose for which the subsidy was offered. Panel C shows the subsidy amount across various industry groups of the subsidy firms, based on NAICS classification, which are recombined further. Non-tradeables include wholesale trade and retail trade. Data centers include information, finance, and insurance sectors, professional and scientific, and management and administrative services. Transportation includes transportation and warehousing, and real estate sectors. Mining is a combination of mining and energy, utilities, and construction sectors.

Panel A: All Deals

	Count	Mean	Median	Std. Dev.
Subsidy (USD million)	127	301.9	138.8	552.2
Investment (USD million)	115	1,141.6	550.0	1,852.3
Subsidy/Investment (%)	115	74.2	38.0	154.0
Jobs promised	126	1,746.7	925.5	2,845.7
Subsidy (USD) per job	126	499,468.5	162,000.0	1,162,208.7

Panel B: By Purpose of Subsidy

	Subsidy ($ million)			
	Count	Mean	Median	Std. Dev.
Relocation	25	100.6	84.8	54.8
New/Expansion	84	394.0	182.8	659.2
Retention	18	151.7	120.4	91.9

Panel C: By Industry of Firms

	Subsidy ($ million)			
	Count	Mean	Median	Std. Dev.
Manufacturing	58	343.5	155.9	682.5
Non-Tradeables	18	536.2	134.6	649.6
Data Centres	26	164.4	108.5	111.7
Transportation	7	83.7	67.6	40.0
Mining-Energy-Utilities	9	291.4	92.6	513.0
All Other	9	143.0	89.5	123.8

Table 2: Summary Statistics: Municipal Bonds

This table summarizes the municipal bond level characteristics during 2005-2019 for our sample of bonds linked to corporate subsidies. Panel A reports the secondary market attributes. Panel B reports the primary market features. The key variables are described in Table A1.

Panel A: Secondary market

	Count	Mean	Median	Std. Dev.
Winner				
Wtd. Avg. Yield (%)	935,797	2.8	2.9	1.4
Yield Spread (%)	935,797	1.5	1.6	2.1
After-tax Yield Spread (%)	935,797	3.4	3.0	2.5
Remaining Maturity (years)	935,797	10.6	9.3	6.9
Loser				
Wtd. Avg. Yield (%)	1,709,644	2.8	3.0	1.4
Yield Spread (%)	1,709,644	1.4	1.6	2.1
After-tax Yield Spread (%)	1,709,644	3.3	3.0	2.5
Remaining Maturity (years)	1,709,644	11.3	9.8	7.4
Observations	2,645,441			

Panel B: Primary market

	Count	Mean	Median	Std. Dev.
Winner				
Offering Yield (%)	133,401	2.8	2.9	1.4
Offering Price (USD)	133,400	103.7	101.7	7.4
Coupon (%)	133,401	3.6	4.0	1.2
Years to Maturity	133,401	9.3	8.2	6.4
Years to Call	54,361	9.0	9.7	1.7
Amount (USD million)	133,401	3.0	0.7	16.6
Issue Size (USD million)	133,401	42.0	11.4	117.6
Loser				
Offering Yield (%)	212,177	2.9	3.0	1.4
Offering Price (USD)	212,175	103.6	101.8	9.3
Coupon (%)	212,177	3.7	4.0	1.3
Years to Maturity	212,177	9.9	8.9	6.7
Years to Call	93,652	8.8	9.7	2.1
Amount (USD million)	212,177	4.9	0.8	22.0
Issue Size (USD million)	212,177	81.9	14.1	189.9
Observations	345,578			

Table 3: Impact on Borrowing Costs of Local Governments: Evidence from Municipal Bonds Secondary Market

This table reports the baseline results for our sample using Equation (1) estimating the differential effect on municipal bond yields of winning and losing counties after the subsidy announcement. The primary coefficient of interest, β_0, is captured by the interaction term of Winner × Post. Panel A compares winners and losers in the secondary market around an equal window of three years of the event. Columns (1)-(3) show the results for monthly average yield as the dependent variable. Specifically, Column (1) reports the effect using county-pair fixed effects, county fixed effects and year-month fixed effects. In Column (2), we also introduce bond level controls consisting of coupon (%); log(amount issued in $); dummies for callable bonds, bond insurance, general obligation bond and competitively issued bonds; remaining years to maturity; and inverse years to maturity. We provide the description of key variables in Table A1. In Column (3), we additionally control for the county-level variation in unemployment rate and labor force. We use the lagged values (to the year of deal announcement) for log(labor force) and unemployment rate, and the percentage change in unemployment rate and labor force, respectively. We use a similar scheme for the remaining columns. In Columns (4)-(6), the dependent variable is after-tax yield spread (see Section 3.2 for details). Our baseline specification comes from Column (6) in Panel A. In Panel B, we report the baseline specification with incremental duration after the subsidy, holding the pre-event window constant at 36 months before the subsidy announcement. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year-month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Three-year Window

Dependent Variable:	Average Yield			After-tax Yield Spread		
	(1)	(2)	(3)	(4)	(5)	(6)
Winner × Post	9.77***	9.11***	9.44***	13.41***	13.18***	13.61***
	[3.23]	[3.57]	[3.63]	[2.79]	[2.95]	[2.99]
Winner	-0.92	2.02	2.14	0.84	2.61	2.61
	[-0.30]	[0.73]	[0.75]	[0.17]	[0.56]	[0.53]
Post ($t \geq 0$)	-2.61*	-2.16	-1.48	-1.72	-1.57	-0.63
	[-1.80]	[-1.59]	[-1.03]	[-0.70]	[-0.65]	[-0.25]
County-pair FE	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓
County Controls			✓			✓
Adj.-R^2	0.323	0.575	0.576	0.546	0.602	0.603
Obs.	2,645,441	2,645,441	2,645,441	2,645,441	2,645,441	2,645,441

Panel B: Different Forward Windows (in months)

Dependent Variable:	After-tax Yield Spread						
Window (months):	[-36,+12]	[-36,+18]	[-36,+24]	[-36,+30]	[-36,+36]	[-36,+48]	[-36,+60]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Winner × Post	7.25*	9.21**	11.37***	12.85***	13.61***	17.57***	18.85***
	[1.80]	[2.13]	[2.67]	[2.95]	[2.99]	[3.76]	[3.88]
Winner	0.52	2.57	3.90	5.42	2.61	-0.93	-7.39
	[0.12]	[0.52]	[0.77]	[1.09]	[0.53]	[-0.21]	[-1.57]
Post ($t \geq 0$)	0.79	-0.52	-0.74	-0.53	-0.63	-4.33	-6.49**
	[0.32]	[-0.20]	[-0.29]	[-0.21]	[-0.25]	[-1.43]	[-2.06]
County-pair FE	✓	✓	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.630	0.625	0.616	0.610	0.603	0.602	0.593
Obs.	1,642,378	1,890,208	2,139,974	2,389,775	2,645,441	3,161,737	3,676,680

Table 4: Robustness Tests

In this table we report results for various robustness tests on our baseline specification, i.e., Column (6) of Panel A in Table 3. In Panel A, we present evidence to control for other observables and unobservables. First, Columns (1)-(3) report results controlling for unobserved factors at the county. Specifically, in Column (1), we introduce county-pair × county fixed effect to the baseline. We show our strictest specification in Column (2) by including county-pair × county year fixed effects. In Column (3), we control for county specific year-month trends. Next, we report results controlling for unobserved factors at the bond level in Columns (4)-(6). Column (4) shows results with issuer fixed effect added to the baseline. In Column (5), we add bond purpose fixed effects to the baseline to control for the purpose for which the money was borrowed by the county. Column (6) shows the main result with bond purpose × year-month fixed effects added to the baseline. Panel B corresponds to further tax considerations and duration. First, in Column (1), we show the effect of using the sub-sample of tax-exempt municipal bonds only in the sample. Next, in Column (2), we show results by dropping deals involving Illinois, Iowa, Oklahoma and Wisconsin. These states do not offer tax exemption on municipal bonds issued even by the respective states. Column (3) shows the result using yield spreads adjusted for local (county) level individual income tax rates, over and above the state tax rate. We use individual income tax rates from 2011 for deals where we could find the data, while assuming zero local individual income tax rates for the remaining deals. Finally, we show results after controlling for duration to account for non-linear effects in remaining maturity. In Column (4), we use duration in the controls by replacing years to maturity and inverse of years to maturity. Thereafter, in Column (5), we use tax-adjusted duration to replace years to maturity and inverse of years to maturity. In Panel C, we show robustness to the choice of clustering used in the baseline specification. Column (1) shows results by single clustering at the county-specific issuer level. In Column (2), we single cluster standard errors at the county-specific bond issue level. Finally, Column (3) uses double clustering at the county-specific bond issue and year-month level. We show robustness to the choice of event window in Panel D, by using alternative windows. First, in Column (1), we use a shorter window of 24 months. In Columns (2)-(3) we use a longer event window of 48 months and 60 months around the subsidy announcement, respectively. Panel E shows results using other dependent variables. Columns (1)-(2) show the results for after-tax yield as the dependent variable. In Column (1), we do not use any bond or county level controls, whereas Column (2) uses all controls as in the baseline specification. We follow the same strategy for Columns (3)-(4) using yield spread as the dependent variable. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year-month level, unless otherwise specified. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$

Panel A: Other Observables and Unobservables

Dependent Variable:	After-tax Yield Spread					
	Other Unobservables					
	County-pair × county FE (1)	County-pair × county × year FE (2)	Control for monthly county trends (3)	Add Issuer FE (4)	Add Bond Purpose FE (5)	Add Bond Purpose-YM FE (6)
Winner × Post	13.51***	6.29***	10.16**	11.33**	12.75***	9.93**
	[2.93]	[11.34]	[2.04]	[2.51]	[2.86]	[2.32]
Adj.-R^2	0.603	0.612	0.607	0.685	0.620	0.633
Obs.	2,645,441	2,645,428	2,645,441	2,645,347	2,645,441	2,645,169

Panel B: Additional Tax Considerations and Duration

Dependent Variable:	After-tax Yield Spread				
				Controlling for Duration	
	Tax Exempt Bonds Only (1)	Drop States w/o Tax Exemption (2)	Adjusting Spreads for Local Taxes (3)	Use Duration (4)	Use After-tax Duration (5)
Winner × Post	16.73***	17.99***	13.78***	14.02***	14.29***
	[3.85]	[3.87]	[3.03]	[2.97]	[2.93]
Adj.-R^2	0.611	0.621	0.603	0.591	0.578
Obs.	2,413,447	2,268,034	2,645,441	2,637,247	2,637,247

Panel C: Clustering

Dependent Variable:	After-tax Yield Spread		
	By Issuer (1)	By Issue (2)	By Issue and Year-month (3)
Winner × Post	13.61***	13.61***	13.61***
	[3.07]	[5.35]	[4.75]
Adj.-R^2	0.603	0.603	0.603
Obs.	2,645,441	2,645,441	2,645,441

Panel D: Alternative Window

Dependent Variable:	After-tax Yield Spread		
	[-24,+24 months] (1)	[-48,+48 months] (2)	[-60,+60 months] (3)
Winner x Post	8.66**	18.55***	19.65***
	[2.29]	[4.08]	[4.13]
Adj.-R^2	0.605	0.605	0.604
Obs.	1,817,425	3,450,139	4,220,257

Panel E: Other Dependent Variables

Dependent Variable:	After-tax yield		Yield Spread	
	No controls (1)	All controls (2)	No controls (3)	All controls (4)
Winner × Post	15.54***	15.00***	7.46***	7.88***
	[3.03]	[3.41]	[2.61]	[2.76]
Adj.-R^2	0.299	0.563	0.752	0.772
Obs.	2,645,441	2,645,441	2,645,441	2,645,441

Table 5: County Debt Capacity: Evidence based on Interest Expenditure and Local Government Debt

This table shows the evidence based on interest expenditure and outstanding local government debt, using the baseline Equation (1). We modify the equation to interact with dummies for high and low values of ex-ante county level measures of debt capacity using interest expenditure and debt. Specifically, we use the *Interest/Revenue*$_1$ ratio in Column (1). In Column (2), we use the *Interest/Revenue*$_2$ ratio, followed by the *Interest/Revenue*$_3$ ratio in Column (3). We show results using interest to debt (ratio of interest on general debt to total long term debt outstanding) in Column (4). Finally, in Column (5), we use the net debt. See Table A1 for variables description. We additionally control for group-month fixed effects in these regressions. T-statistics are reported in brackets and standard errors are double clustered by county bond issuer and year-month. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread				
Interaction Variable:	$\frac{\text{Interest}}{\text{Revenue}_1}$	$\frac{\text{Interest}}{\text{Revenue}_2}$	$\frac{\text{Interest}}{\text{Revenue}_3}$	$\frac{\text{Interest}}{\text{Debt}}$	Net Debt
Winner × Post	(1)	(2)	(3)	(4)	(5)
× Low dummy	1.97	0.19	0.91	-3.42	-4.15
	[0.27]	[0.03]	[0.14]	[-0.61]	[-0.56]
× High dummy	17.55***	26.68***	26.16***	27.31***	16.49***
	[3.41]	[3.63]	[3.59]	[4.53]	[3.19]
Difference	15.58	26.49	25.25	30.74	20.63
p-val	0.07	0.01	0.02	0.00	0.02
County-pair FE	✓	✓	✓	✓	✓
Year-month FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓
Group-Month FE	✓	✓	✓	✓	✓
Adj.-R^2	0.603	0.603	0.603	0.604	0.603
Obs.	2,636,342	2,636,342	2,636,342	2,636,342	2,636,342

Table 6: County Debt Capacity: Evidence based on County Credit Ratings

This table shows the evidence based on ex-ante county level credit ratings among winning counties, using the baseline Equation (1). We interact the main equation with dummies corresponding to the ex-ante average S&P municipal bond rating group of the county. We use municipal bonds issued before the subsidy announcement to assess the county level credit rating. The rating group *Above Median* corresponds to higher credit rating quality, while *Below Median* represents lower credit quality. We additionally control for the average effect within a particular group for that month by adding group-month fixed effects. First, in Column (1), we show the impact on the full sample of bonds. Second, in Column (2), we show the results using a sub-sample of general obligation (GO) bonds only. Finally, Column (3) shows the impact on revenue (RV) bonds alone. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread		
Type of Bonds:	All Bonds	GO Bonds	RV Bonds
Winner × Post	(1)	(2)	(3)
× Above Median	-2.91	-8.02	5.28
Rating dummy	[-0.41]	[-0.85]	[0.71]
× Below Median	23.27***	22.02***	12.21
Rating dummy	[3.42]	[2.97]	[1.58]
Difference	26.17	30.04	6.93
p-val	0.01	0.01	0.55
County-pair FE	✓	✓	✓
Year-month FE	✓	✓	✓
County FE	✓	✓	✓
County Controls	✓	✓	✓
Group-Month FE	✓	✓	✓
Adj.-R^2	0.596	0.586	0.616
Obs.	2,454,715	874,855	1,579,860

Table 7: Impact on Offering Yields of Municipal Bonds

This table shows the effect of subsidy announcement on offering yields of new bond issuances using a difference-in-differences estimate based on Equation (1). Here, we also introduce the issuer fixed effects in each of the specifications. In Column (1), we show the result by using only the county-pair, county and issuer fixed effects in the baseline equation. Next, in Column (2), we introduce bond level controls. Column (3) shows the results with county controls. Finally, Column (4) shows the results with S&P credit rating controls at the time of issuance also added to the specification. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and dated month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Offering Yield			
	(1)	(2)	(3)	(4)
Winner × Post	11.97***	6.67***	4.69**	4.55**
	[4.36]	[3.18]	[2.41]	[2.08]
Winner	-6.13	-7.15**	-2.37	-2.07
	[-1.24]	[-1.99]	[-0.71]	[-0.56]
Post (t $\geq$ 0)	-45.88***	-37.50***	-31.56***	-31.61***
	[-15.27]	[-15.73]	[-13.56]	[-12.45]
County-pair FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
Issuer FE	✓	✓	✓	✓
Bond Controls		✓	✓	✓
County Controls			✓	✓
Rating Controls				✓
Adj.-R^2	0.418	0.823	0.828	0.831
Obs.	341,662	341,662	341,628	220,163

Table 8: Impact on Local Economy

This table shows the impact of subsidy on employment growth (%) from QCEW and unemployment rate (%) from the BLS at the county level in Panel A. In Panel B, we show results using the number of establishments and annual payroll growth (%) from the County Business Pattern. We use the annualized version of Equation (1) without county controls as the primary specification for this table. In each panel, Columns (1) and (4) report the overall effect. Columns (2) and (5) show the results by interacting the equation with dummies corresponding to ex-ante county level anticipated jobs multiplier effect among winning counties. We construct this measure by summing up the proportion of value-added in the upstream and downstream segments of a given industry, weighted by the corresponding county's share of wages. See Table A1 for variables description. Finally, Columns (3) and (6) report the results by interacting Equation (1) with a dummy variable (*Low Int./Rev.*$_1$ and *High Int./Rev.*$_1$) based on the median value of the *Interest/Revenue*$_1$ ratio among winning counties. In these interacted specifications, we replace the event-year fixed effects with group-event year fixed effects. T-statistics are reported in brackets and standard errors are clustered at the deal level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A

Dependent Variable:	Employment Growth (%)			Unemployment Rate (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Winner × Post	0.64*			-0.02		
	[1.67]			[-0.18]		
Low Multiplier × Winner × Post		0.31			0.12	
		[0.59]			[0.60]	
High Multiplier × Winner × Post		0.95*			-0.16	
		[1.73]			[-0.82]	
Low Int./Rev$_1$ × Winner × Post			0.28			0.22
			[0.53]			[1.29]
High Int./Rev$_1$ × Winner × Post			1.06*			-0.33
			[1.88]			[-1.45]
Difference		0.63	0.78		-0.28	-0.54
P-value		0.41	0.31		0.32	0.05
County-pair FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Event-Year FE	✓	✓	✓	✓	✓	✓
Group-Event-Yr. FE		✓	✓		✓	✓
R^2	0.150	0.153	0.152	0.364	0.365	0.373
Obs.	2,494	2,494	2,483	2,574	2,574	2,563

Panel B

Dependent Variable:	Log(Number of establishments)			Annual Payroll Growth (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Winner × Post	-0.00			1.05*		
	[-0.57]			[1.80]		
Low Multiplier × Winner × Post		-0.01			1.13	
		[-1.49]			[1.35]	
High Multiplier × Winner × Post		0.01			0.97	
		[0.65]			[1.20]	
Low Int./Rev_1 × Winner × Post			-0.01			1.43*
			[-1.27]			[1.74]
High Int./Rev_1 × Winner × Post			0.00			0.63
			[0.38]			[0.75]
Difference		0.02	0.02		0.15	0.80
P-value		0.14	0.24		0.90	0.49
County-pair FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Event-Year FE	✓	✓	✓	✓	✓	✓
Group-Event-Yr. FE		✓	✓		✓	✓
R^2	0.999	0.999	0.999	0.078	0.080	0.079
Obs.	2,670	2,670	2,648	2,670	2,670	2,648

Table 9: Impact on Local Public Expenditure

This table shows the impact of subsidy on local government expenditure per capita. We use the annualized version of Equation (1) as the primary specification for this table. Columns (1)-(4) show the aggregate impact between winners and losers, while Columns (5)-(8) present the results based on sub-groups of the *Interest/Revenue*$_1$ ratio. Columns (1) and (5) show the impact on total expenditure at the local level. *Health Expenditure* in Columns (3) and (6) consists of per capita expenditure on health, hospitals, and public welfare. *Police and Protection Expenditure* in Columns (4) and (8) consists of per capita expenditure on police protection, fire protection, correctional expenditure, and judicial expenditure. Specifically, we show the interacted form of the difference-in-differences estimate using a dummy variable (*Low Int./Rev.*$_1$ and *High Int./Rev.*$_1$) based on the median value of the *Interest/Revenue*$_1$ ratio among winning counties. In these interacted specifications, we replace the event-year fixed effects with group-event year fixed effects. T-statistics are reported in brackets and standard errors are clustered at the deal level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Total	Elementary Education	Health	Police and Protection	Total	Elementary Education	Health	Police and Protection
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner × Post	0.02	-2.02	-12.23	3.61				
	[0.00]	[-0.09]	[-0.81]	[0.59]				
Winner × Post					1.18	-17.76	19.46	4.40
×Low Int./Rev.$_1$					[0.02]	[-0.46]	[0.70]	[0.51]
Winner × Post					-2.58	11.28	-44.54***	2.46
×High Int./Rev.$_1$					[-0.04]	[0.39]	[-3.66]	[0.28]
Difference					-3.76	29.03	-64.00	-1.94
P-value					0.97	0.55	0.04	0.88
County-pair FE	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Event-Year FE	✓	✓	✓	✓				
Group Event-Year FE					✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.972	0.943	0.987	0.975	0.971	0.943	0.987	0.975
Obs.	2,508	2,508	2,508	2,508	2,497	2,497	2,497	2,497

For Online Publication–Internet Appendix

IA1 Corporate Subsidies in the U.S.

Since colonial times, businesses have been offered tax-related incentives (Eisinger and La, 1988; Taylor, 1993). Buss (2001) provides some interesting details about the history of subsidy competition. As early as 1800, states financed infrastructure and offered capital to businesses. For example, Pennsylvania had invested USD 100 million in more than 150 corporations and placed directors on their boards by 1844. While intense rivalry between Pittsburgh and Philadelphia led to substantial investment in public infrastructure, widespread corruption also ensued. As a result, constitutional amendments outlawed some of these practices (Watson, 1995). Relevant to many deals in our setting, Mississippi pioneered tax-exempt municipal bonds to attract industries in 1936. Subsequently, by 1959, 21 states had established state-level business development corporations. Much of this new economic development was often financed through debt. In the latter part of the 20^{th} century, the unemployment crises of the 1970s and the recessions in the early 1980s resulted in an aggressive war between states to win/retain jobs.

Chi and Leatherby (1997) document 15 of the most common business tax incentives ranging from corporate and personal tax exemption to various forms of tax credits related to job creation or research and development. Hanson (2019) provides some broad conclusions about the usefulness of different types of tax incentives. Property taxes and tax concessions are fully capitalized into property values. As a result, tax increment financing (TIF) is not an effective economic redevelopment tool. On the other hand, increasing the corporate tax rate reduces employment and decreases business entry. Even so, there has been justification for such tax incentives with various motivations: protecting (retaining) businesses from being lost to other states, shielding businesses from competition, revitalizing failing firms (Ambrosius, 1989; Burnier, 1992; Wolman, 1988) or attracting new firms from outside. When most states offer such subsidy bids and incentives, other states also make room for such developmental tools (Gilbert, 1995). There is also an argument made in favor of subsidies since they are revenues forgone but not actual cash paid out. Another justification comes from Noll and Zimbalist (2011): if society has underemployed resources, then said resources could be used more productively through corporate incentive programs. The primary difficulty in understanding the overall impact of using subsidies for local economic development stems from the endogeneity: policy changes/moves are directly correlated with outcomes of interest (Hanson, 2019). In this regard, Hanson and Rohlin (2018) provide a detailed toolkit of methods and best practices in evaluating spatially targeted urban redevelopment incentives. We try to incorporate some of those recommendations in our methodology and identification.

IA2 Data on Corporate Subsidies in the U.S.

IA2.1 Subsidy Deals

The *Good Jobs First Subsidy Tracker* (Mattera, 2016) provides a starting point with its compilation on establishment-level spending data. It sources these data from the state level dossiers on revenue foregone/credit offered in the Tax Expenditure Reports. Further, states also report incentives allocated through various programs provided by their respective economic development offices. Such disclosures are usually cited in the annual (or biennial) state-level budgets. The state Department of Revenue or Budget Office may be responsible for updating and maintaining such (web) archives. States that do not report establishment-level monetary spending through subsidies in their financial data also are present in the Subsidy Tracker dataset. News articles, press releases, and Freedom of Information Act (FOIA) requests are used/cited in the dataset for these additional deals. However, *Good Jobs First* does not contain an exhaustive list of all the subsidy programs launched and run by various states. At best, it may be most relevant for the larger set of discretionary subsidies floated by states and local governments. Overall, the existing dataset reports state-year level observations. For our purposes, the dataset needs to be enhanced with key variables that are not already recorded.

As of June 2018, the Subsidy Tracker files contained 606,899 records of subsidy items listed in their full dataset. We focus on records after 1990, wherein the year of subsidy is not missing. Also, our setting requires bidding competition between non-federal governments. Hence, we omit deals where the money is sponsored by the federal government of the United States. This further omits 221,000 records. (Figure 1 brings out the proportion of federal versus non-federal incentives.) As a further check to verify for deals containing federal sponsorship, we check the raw data included in our sample for loans granted by the US government. While some of the composite subsidy packages may contain components offered by the US government, the total subsidy listed under state-level deals excludes these federal loans. In order to focus on large, economically meaningful deals for the local governments, we restrict our sample to subsidies with values exceeding USD 50 million. After dropping records below this threshold, we are left with 573 observations, which have to be manually parsed further because they include repetitions at the firm or parent company level. Due to a lack of consistent nomenclature of firms, we parse this information through careful reading. Specifically, a "Megadeal" may include various incentives stitched together from money/tax abatement offered by the city, town, county and state governments. The Subsidy Tracker data may or may not include overlapping items at the state level. For instance, in 2006 the state of Florida offered USD 310 million as subsidy to Burnham Institute for Medical Research to locate their medical research facility in Orlando (Orange County), which included USD 155.3 million from the Innovation Incentive Fund. Given the existing overlap in the raw Subsidy Tracker data, both these observations show up after the above filters. Florida statutes list an Innovation Incentive Program[20] which is intended 'to respond expeditiously to extraordinary economic opportunities and to compete effectively for high-value research and development,

[20] `https://tinyurl.com/y2jze7ys`

innovation business, and alternative and renewal energy projects.' In archival reports, grants approved under the scheme date back to 1995–96.

Narrowing down to 2005–2018 for our sample period, we get 437 records. This imposition of calendar years chosen is based on the availability of secondary market transactions in municipal bonds, described in Section 3.2. From the variables listed by *Good Jobs First*, we are primarily interested in the company name, parent firm, firm location, year, subsidy amount, subsidy adjusted, level of subsidy (based on the government level), city/county of the facility, number of jobs promised and total investment. There are cases of missing information. Additional data is gathered on the FIPS code for the county, NAICS code for the proposed facility or firm, and the purpose of the subsidy: new plant/expansion, retention, or relocation. To distinguish between retention versus expansions, we rely on documented evidence in newspaper articles. A retention must be for a facility already operating in a location, while expansions may be a new unit/assembly line. Understandably, retentions are often without any fresh investments made by the firms. However, significant effort is devoted to comprehensively parse through local print media/newspapers to find out the losing county (and state) and earliest date of announcement for the subsidy/plant. These two variables were the most painstaking aspects of the data collection procedure. In this context, our dataset construction is more granular and focused than Slattery (2020), who uses state-level bidding competition. The Subsidy Tracker dataset never provides information on the losing county nor the precise announcement date. In Table IA1, we show a comparison of the original dataset versus the one constructed after hand-collection of relevant variables.

Through a careful manual reading of newspaper articles, we can identify 127 winner-loser deal pairs at the county level, which we define as our final sample[21]. Of these, 39 deal pairs overlap with those used in Bloom, Brynjolfsson, Foster, Jarmin, Patnaik, Saporta-Eksten, and Van Reenen (2019). Where it was not possible to reasonably align a winning/losing city to a single county, all counties were included. Since losing county information in these articles is worded differently, it is challenging to automate the process through a programmed algorithm. We argue that given the incongruity between the size of the state and the subsidy offered, the local governments' lens would be more relevant as a setting, in terms of proportion. Motivated by reasons similar to those cited in Bertrand and Mullainathan (2003) on using the exact dates of law passage for announcement effects of anti-takeover provisions, we rely on the earliest available dates for a given deal. Specifically, if the date of incentive approval/announcement is before the plant announcement date, we use this date because the market already learns of the potential subsidy offer. However, occasionally the facility announcement predates the disclosures of all incentives that may have been offered to attract the firm.

There is inherent secrecy maintained by local governments and economic development board officials about such subsidy offers. The underlying assumption is that disclosures would invite other competitors; alternatively, it could invite moral hazard problems for counties that may be desperate to win new jobs. For instance, consider the case of Burnham Institute for Medical Research choosing Florida in 2006. Local officials refused to share details of the economic incentives bid in the public media for fear of instigating more competition from other

[21] As such, there are 120 unique firm-year level subsidy deals among bidding states.

locations/bidders (See Figure IA1). A snapshot of some project names attributed to subsidy deals is provided in Table IA2 of the Appendix.

IA2.2 Caveats

State economic development boards often revamp their (web) archives when the officer/Governor in charge loses power. This also becomes a hurdle in collecting information. As indicated before, we do not claim to have collected the full universe of the subsidies offered to corporations. In fact, doing so may complicate the task of identifying the impact of corporate subsidies on the local governments, based on insignificant amounts waived off in abatement. Also, there is no way to ascertain what subsidy bid was offered by the runner-up county/location. Only in some cases do newspaper stories carry information about the competing bid offered. Largely, this remains unobserved in the current setting - and we acknowledge this as a major limitation in the data. This is especially true in cases where more than one city is known to have competed. For deals with multiple losers, there is no direct way to ascertain which of the losers was the closest to getting the deal. We base our judgment on a subjective assessment of grammatical hints available in the article documenting the story. Therefore, this is not a robust way to identify the closest runner-up location. To replicate the interstate competition, wherever possible, priority is offered to a location outside the winning state in assigning the runner-up county (for multiple runners-up).

IA3 Additional Results

In Section IA3.1, we provide results of falsification tests. Next, in Section IA3.2, we provide robustness of our baseline specifications. We provide supplementary evidence on anticipated multiplier effects using BLS input-output tables in Section IA3.3. Our results in Section IA3.4 show the impact on the probability of rating downgrades. In Section IA3.5, we analyze the heterogeneity in relative bargaining power between the county and the firm involved in the subsidy deal.

IA3.1 Falsification Tests: Pre-Refunded Bonds

It is typical for municipal bond issuers to pre-refund bonds before the call date by issuing new debt and holding the proceeds in a trust to fund remaining payments until the call date. This would effectively render the pre-refunded bonds nearly risk free (Fischer, 1983; Chalmers, 1998; Schwert, 2017). Local governments may choose to pre-refund their bonds, thereby offering a clean change of said bonds from risky to risk-free. We exploit this argument to claim that bonds which have been thus "insured" would not see any significant change in their yield spreads in our setting of Equation (1).

To construct the sample of pre-refunded bonds for this test, we follow Ang, Green, Longstaff, and Xing (2017) and Schwert (2017). We apply the following filters to our sample: keep only the pre-refunded bonds, excluding bonds that are not exempt from federal and within-state income taxes; and exclude pre-refunded bonds that are not escrowed by Treasury securities, State and Local Government Series (SLGS), or cash. Finally, we exclude bonds that are pre-refunded within 90 days of the call date since the Internal Revenue Service treats these transactions as different from pre-refunding, instead classifying them as current refunding. Table IA9 shows the results of the falsification test. In Column (1), we find that the average yield for winners goes up by a small magnitude of about 5.15 bps, but the measure is not statistically significant. Likewise, in Column (2), we do not find any significant change to the yield spread as the outcome variable among the pre-refunded bonds. Finally, Columns (3)-(5) report the effect on after-tax yield spreads without and with county controls, respectively. The magnitude is slightly higher than the previous two columns but again is statistically insignificant. Thus, we do not find any impact on these pre-refunded bonds as they have been secured against the escrow of funds earmarked for their outstanding payments. The absence of any marginal impact in the subset of pre-refunded bonds suggests that our main effect is not driven by overall market conditions in the US municipal bond market.

IA3.2 Further Robustness Tests

In Table IA10, we report results from additional considerations of robustness to our baseline specification using Equation (1) as reported in Column (6) of Panel A in Table 3.

IA3.2.1 Is the effect driven by the size of trades?

In 2018, about USD 0.96 trillion out of USD 3.25 trillion of the municipal bond holdings was managed by money market mutual funds and exchange-traded funds. One potential concern is that few large institutional trades may be driving our main result. We separate our results into sub-samples of trades constituting various buckets. Retail-sized transactions usually correspond to \$100,000 or less [22]. Columns (1)-(3) depict the main effect from Equation (1), as derived from trade sizes worth $\leq$ \$25,000, $\leq$ \$50,000, and $\leq$ \$100,000. The increase in borrowing cost is over 14 bps in each of these sub-samples, which is higher than our baseline estimate. This suggests that our main result is also present in smaller transactions.

On the other hand, the lack of information among retail investors may be driving our results. To address this, we now use bond-month observations based on trade sizes worth $>$ \$25,000, $>$ \$50,000, and $>$ \$100,000 in Columns (4)-(6). As before, we still report an increase in bond yield spreads of over 15 bps in each of these sub-samples. Based on this evidence, we argue against the size of trades explaining our main result.

IA3.2.2 Does the financial crisis of 2009 drive results?

Another potential worry is that the sample period spans the financial crisis of 2009. Understandably, this was a period of major volatility in the financial markets across asset classes, and municipal bonds were no exception. As a result, we report our findings by showing our results for periods before and after 2009. In Columns (7)-(8), we

[22] `http://www.msrb.org/~/media/Files/Resources/Mark-Up-Disclosure-and-Trading.ashx?`

report the main coefficient of interest, β_0, by interacting the baseline Equation (1) with dummies corresponding to events before and after 2009, respectively. We find that the increase in yield spreads is 17.35 bps for subsidy deals before 2009 and 11.12 bps after 2009. The difference between these two coefficients is statistically insignificant. This suggests that our results are not singularly driven by events belonging to either side of the financial crisis of 2009.

IA3.2.3 Are results driven by newly issued bonds?

Even though our data from MSRB on secondary market bond yield spreads is cleaned for primary-market transactions recorded therein, we assume further precaution in favor of seasoned bonds. In Columns (10)-(14), we report our baseline results by dropping bonds that were recently issued with respect to the deal i.e., within 6, 12, 18, 24, and 36 months of the subsidy announcement date, respectively. By doing so, we remove bonds from the sample that have been newly issued before or after the subsidy and thus may demonstrate unusual trading in the initial phases. Our main effect still shows up as nearly 12 bps in each of these columns, even as the sample size reduces in Column (14). This shows that our results are not solely driven by trading activity in newly issued bonds around the subsidy announcement dates. Moreover, in Columns (15)-(17), we use the complementary sub-samples by only keeping bonds that were recently issued around the subsidy announcement dates. Understandably, the sample size shrinks substantially in these analyses focusing on bonds issued within 18, 24, and 36 months of the subsidy dates. Even so, our results show that we still find the baseline effect to be over 8 bps. This provides further evidence that our results are not affected by including or dropping newly issued bonds.

IA3.2.4 Additional County and Bond Level Considerations

In Column (18), we report our results from more county-level economic considerations. There is some evidence that firms' decisions to locate in a region may increase house prices locally[23]. This may also be correlated with local household incomes. In this regard, we report our results in Columns (18) by additionally controlling for the values of log(household income) and log(house price index). We find that the main result is 9.50 bps, which is economically meaningful and statistically significant.

In the baseline specification, we do not include bond ratings so that we can analyze both rated and unrated bond transactions. Here, we check on the robustness of our main results using only those bonds for which the most recent bond ratings are available from S&P's credit ratings. We show this result in Column (19) of Table IA10 by introducing the numeric equivalent of bond level ratings among the regressors. The magnitude goes up to over 14.54 bps, and the result is statistically significant.

[23]https://www.wsj.com/articles/amazon-primed-to-boost-property-prices-in-winning-hq2-cities-1542715200

IA3.3 Anticipated Jobs Multiplier Effects

Our coefficient estimates from the dynamic regression in Figure 5b are informative, but do not control for bond characteristics and county-level features. To formalize our analysis in this regard, we interact our baseline Equation (1) with dummies corresponding to high and low values of anticipated jobs multiplier effect, based on the median value. We additionally control for the average impact within a particular group for that year-month by adding group × year-month fixed effects. Table IA8 shows the results of our analysis. Using total, upstream,+ and downstream segments, our results in Columns (1)-(3) show that the bond yield spreads increase by 16-21 bps for deals involving a low anticipated jobs multiplier. The difference between the winning counties with low and high values is statistically significant in Column (1). We find qualitatively similar results in Columns (2) and (3).

IA3.4 Probability of Rating Downgrades

The results in the previous subsections highlight that winning counties with a lower debt capacity and a lower jobs multiplier observe an increase in yield spreads and thus a reduction in the value for municipal bondholders. Next, we test if rating agencies react to such an event.[24] Specifically, we evaluate the impact on bond ratings by considering the probability of rating downgrades. Our municipal bond ratings come from S&P ratings, as provided by FTSE Russell's municipal bonds database. We use a dummy variable that switches to one after a rating downgrade (and zero otherwise) as the dependent variable in Equation (1). Given the nature of our county-pair based cohort fixed effect, we rely on a simple ordinary least squares regression instead of a logistic regression (Neyman and Scott, 1948). Similar to our baseline regression, we expect that the coefficient of interest, β_0, would capture the differential probability of rating downgrades on winning counties after the deal. We report our results in Figure IA7.

Rating downgrades in the municipal bond market are not very frequent. On average, the probability of rating downgrades in our sample is 17.82%. First, we show that the probability of downgrade across all bonds for winning counties in the sample increases by 3.79% after the subsidy announcement. After that, we show the effect based on high and low values of the ex-ante debt capacity among winners using interest expenditure and net debt. Using the $\textit{Interest/Revenue}_1$ ratio, we find that the probability of rating downgrade increases by 4.02% after the subsidy announcement for winners in the high group. This represents 22.6% (=4.02/17.82) of the average likelihood of rating downgrades. The differential impact between the high and low groups is 0.46 percentage points, which is statistically insignificant. Subsequently, we find that the differential increase of the probability of a rating downgrade is 8.06% for winning counties with a high $\textit{Interest/Revenue}_2$ ratio. Using our third measure of debt capacity based on interest expenditure, the $\textit{Interest/Revenue}_3$ ratio, the winning counties

[24]Moody's downgraded Racine County's credit worthiness in Wisconsin after the announcement of Foxconn's incentives. For details, see: `https://www.bondbuyer.com/news/foxconn-incentives-costly-for-a-wisconsin-countys-rating`

with high values are 7.19% more likely to see a bond rating downgrade in comparison to the losing counties.[25] Similarly, our results using the ratio of interest to debt suggest a 6.33% higher probability of rating downgrades among winning counties with above median values of this metric. Finally, we find qualitatively similar results using net debt as a measure of debt capacity.

IA3.5 Relative Bargaining Power of the County versus the Firm

So far, we have considered the impact of the debt capacity of the county and the anticipated jobs multiplier effects of the subsidy deal on the municipal bond yield spreads. The amount of subsidy offered likely could depend on the relative bargaining power between the counties and the firm involved in the deal. We argue that while firms may hire site consultants to conduct their search through a bidding mechanism[26], local governments may not have access to such sophisticated resources. To assess the relative bargaining power between the county and the firm, we use the following: a) *Proposed Value*, b) ratio of investment to state revenue, c) intensity of bidding competition, and d) county's unemployment rate. We present our results in Table IA11.

First, we divide the winning counties based on the median value of *Proposed Value*. Our measure is obtained by taking the ratio of the differential between proposed investment and subsidy to the county's lagged revenue. We hypothesize that if the excess value proposed from the investment, beyond the subsidy, is small relative to the county (size represented by revenue), then the county's relative bargaining power is likely to be lower. Therefore, the impact on borrowing costs would be higher. In Column (1) of Table IA11, we report our results for the baseline Equation (1) interacted with dummies of this measure. We also control for group-year month fixed effects. We find that yield spreads go up by 16.30 bps for deals with low proposed value. We find a similar impact by using our second measure of investment to state revenue ratio. We argue that when the value of the investment to be made by the firm is relatively larger than the state's revenue, the county has a lower bargaining power. As shown in Column (2), the cost increases by 22.54 bps for the high group with a relatively milder effect when the corresponding ratio is low.

Given that state-level governments often support the competition for firms' investments, we construct our next measure at the state level. We calculate the ratio of the state-level budget surplus to revenue and use the gap between the winning and losing states as a measure of the intensity of competition. As the gap between states widens, the competition is likely to be lower, and the county's bargaining power is expected to be higher. In Column (3), we show our results based on the interaction with the intensity of competition. We find that the secondary market yields go up by 20.66 bps when the intensity of competition is high (surplus to revenue gap is low). Finally, we show our results based on the county-level unemployment rate in Column (4). We expect

[25]The increase in bond yield spreads after the subsidy announcement could be associated with agency costs whereby politicians seek short-term outcomes for future elections. While this is interesting by itself, it is difficult to measure the agency problems due to data limitations. In this paper, we focus on the underlying debt capacity as the mechanism.

[26]For instance, The Wall Street Journal reported on a cadre of consultants who help companies decide the location of their projects: https://www.wsj.com/articles/meet-the-fixers-pitting-states-against-each-other-to-win-tax-breaks-for-new-factories-11558152005

counties with a high unemployment rate to have a lower bargaining power in the bidding process, resulting in a greater impact on yields (17.86 bps).

Overall, we argue that the four measures of bargaining power highlight the differential impact of relative bargaining power between the county and the firm. The municipal bond market reaction to the deal is linked to this bargaining power, which is also related to the amount of subsidy offered. A lower bargaining power causes a greater increase in yields (between 16-22 bps).

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Bid to woo biomed firm: $90 million: Leaders hope to lure a California research center to Orlando

Mark Schlueb and David Damron . Knight Ridder Tribune Business News ; Washington [Washington]11 Apr 2006: 1.

Local officials have refused to reveal details of the economic- development bid they've dubbed "Project Power," fearing that doing so would tip off as many as a dozen other suitors. Public records and those involved indicate the total value of the bid from Orange County, Orlando and the developer of Lake Nona -- the sprawling development where the institute's satellite operation would be built -- was still in flux Monday.

"This is a competition, and it is something of a long shot. But I believe we have assembled here a very strong team," Orange County Mayor Rich Crotty said. He cited estimates by local economic- development officials that the

Figure IA1: Project Secrecy: This figure provides an excerpt representing an instance of secrecy maintained by local officials in their process to bid for a project by offering incentives.

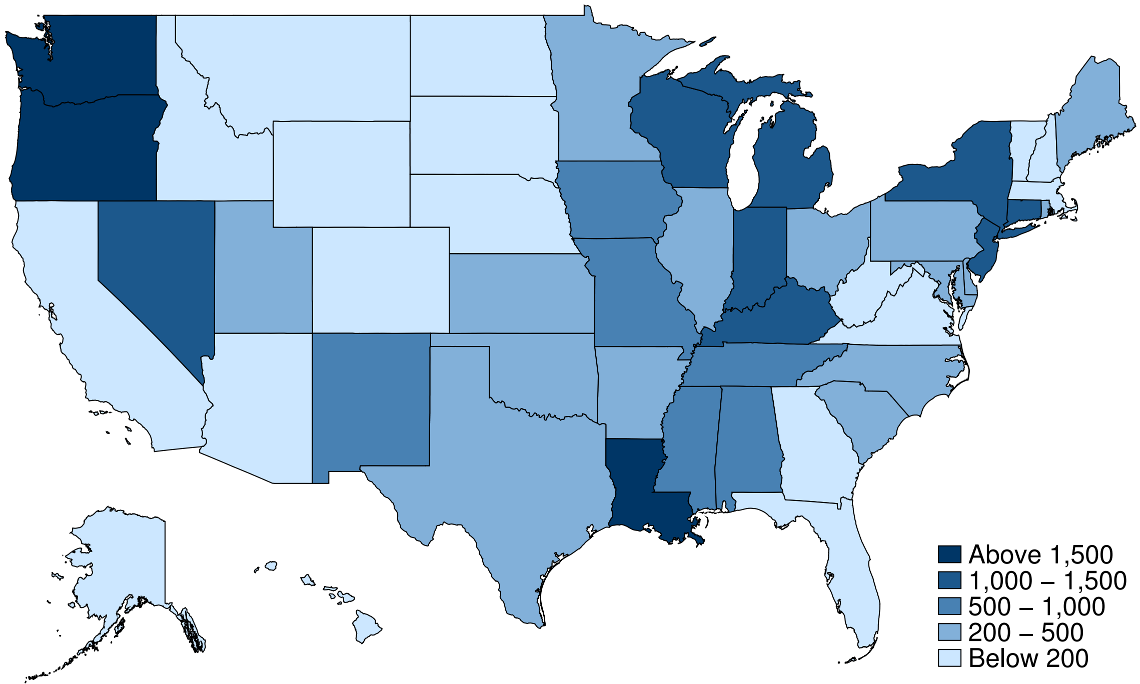


Figure IA2: Subsidy per Capita: The state-level distribution of subsidy per capita (in USD) is shown for the period 2005-2018. Calculated based on Source: *Good Jobs First, Subsidy Tracker*

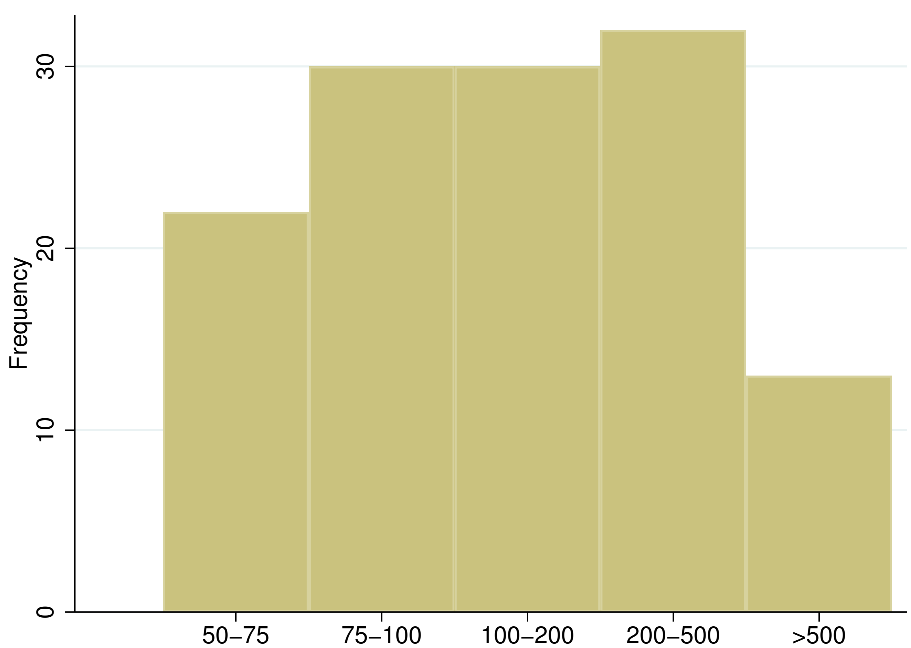


Figure IA3: Distribution of Subsidy (USD million): In this figure, we plot the number of deals in our sample of winner-loser pairs during 2005-2018 across different ranges of subsidy bins. The horizontal axis shows the subsidy bins (in USD million).

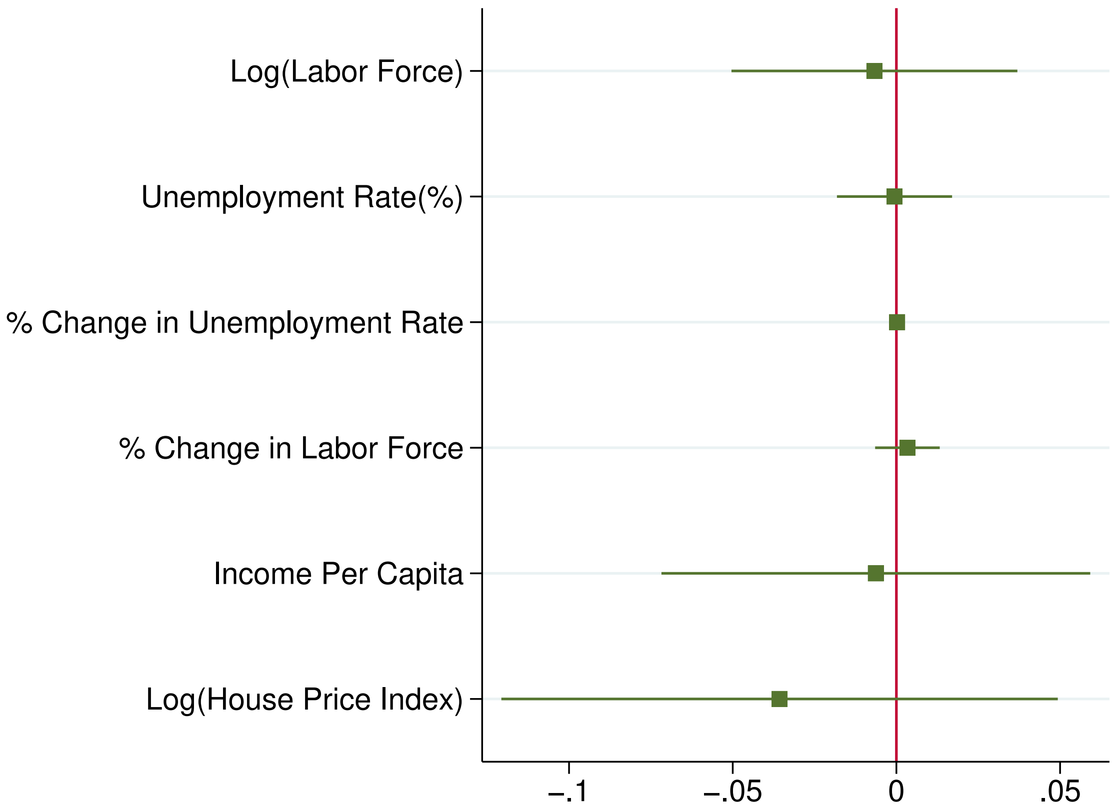
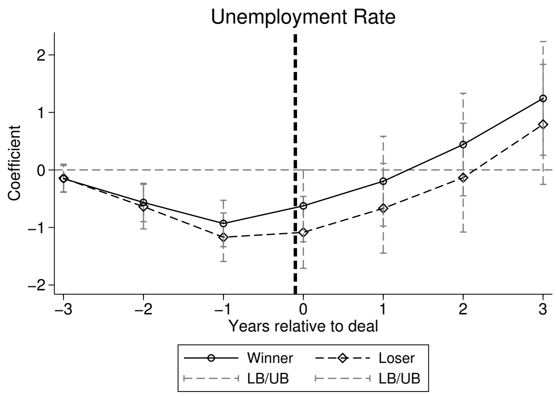
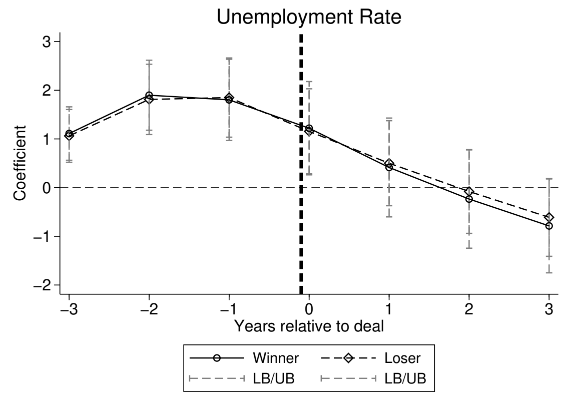


Figure IA4: Predicting Winners: This figure reports the regression coefficients of a linear probability model predicting the winners using local economic variables. We use local economic variables three years before the deal. Confidence intervals at the 95% level are plotted.

Panel A: By Unemployment Rate

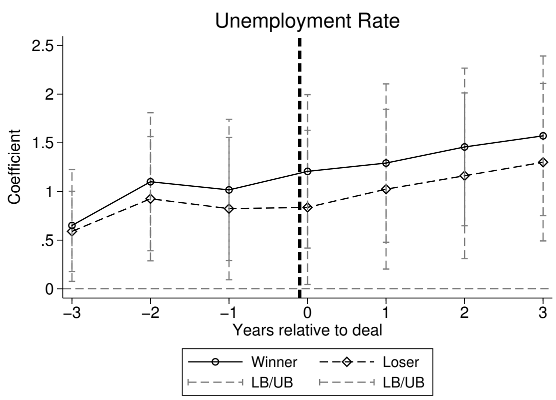


(a) Low

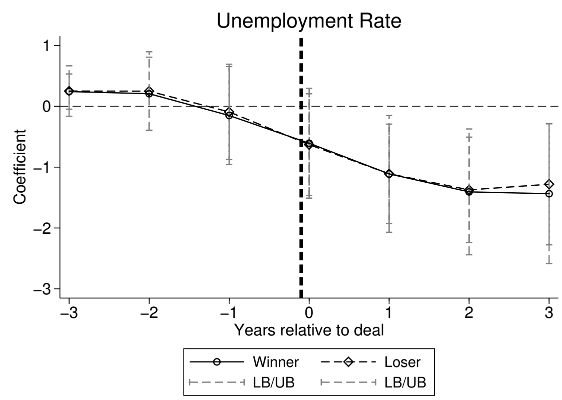


(b) High

Panel B: By Household Income



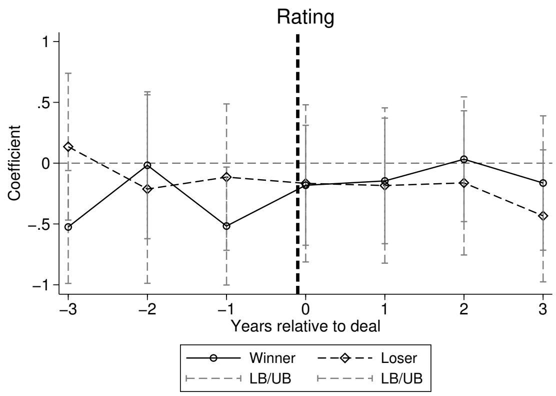
(c) Low



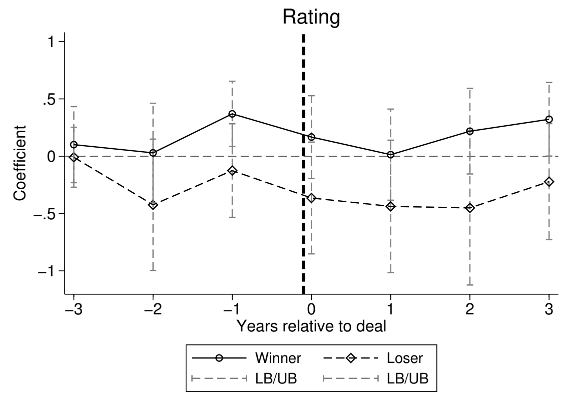
(d) High

Figure IA5: Winner vs Loser - Local Economy: The figure shows the unemployment rate at the county level for the bidding counties split into two groups (high and low), around the time of subsidy announcement. Panel A uses ex-ante unemployment rate among winning counties to divide the sample, while Panel B uses ex-ante median household income. We regress the unemployment rate against a set of interaction dummies for Winner $\times$ Post, split into half-yearly periods. We use deal and county fixed effects; standard errors are clustered by deal-pair. The benchmark period is from a year before the event window.

Panel A: By County Rating

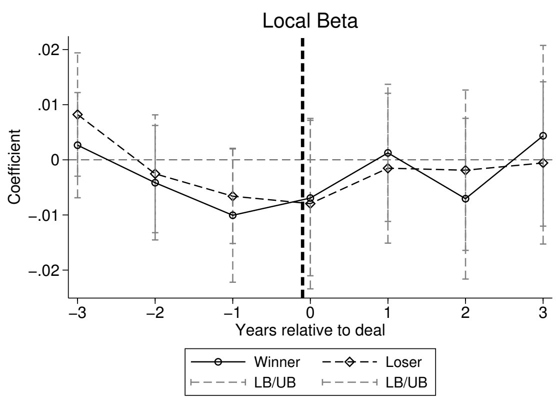


(a) Low

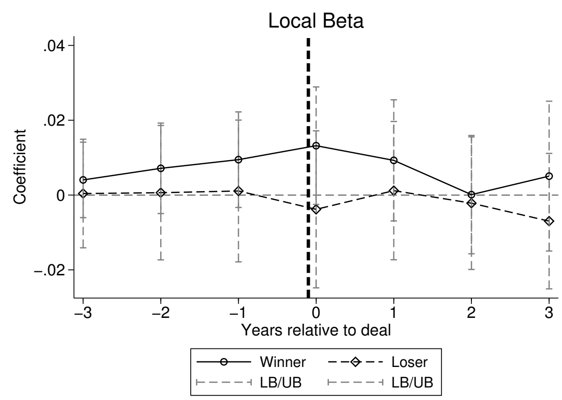


(b) High

Panel B: By Local Beta



(c) Low



(d) High

Figure IA6: Winner vs Loser - Local Economy: The figure shows the rating at the county level for the bidding counties split into two groups (high and low), around the time of subsidy announcement. Panel A uses ex-ante county rating among winning counties to divide the sample, while Panel B uses ex-ante local beta. We regress the county rating in Panel A and county local beta in Panel B against a set of interaction dummies for Winner × Post, split into half-yearly periods. We use deal and county fixed effects; standard errors are clustered by deal-pair. The benchmark period is from a year before the event window.

Probability of Bond Rating Downgrade by County Debt Capacity

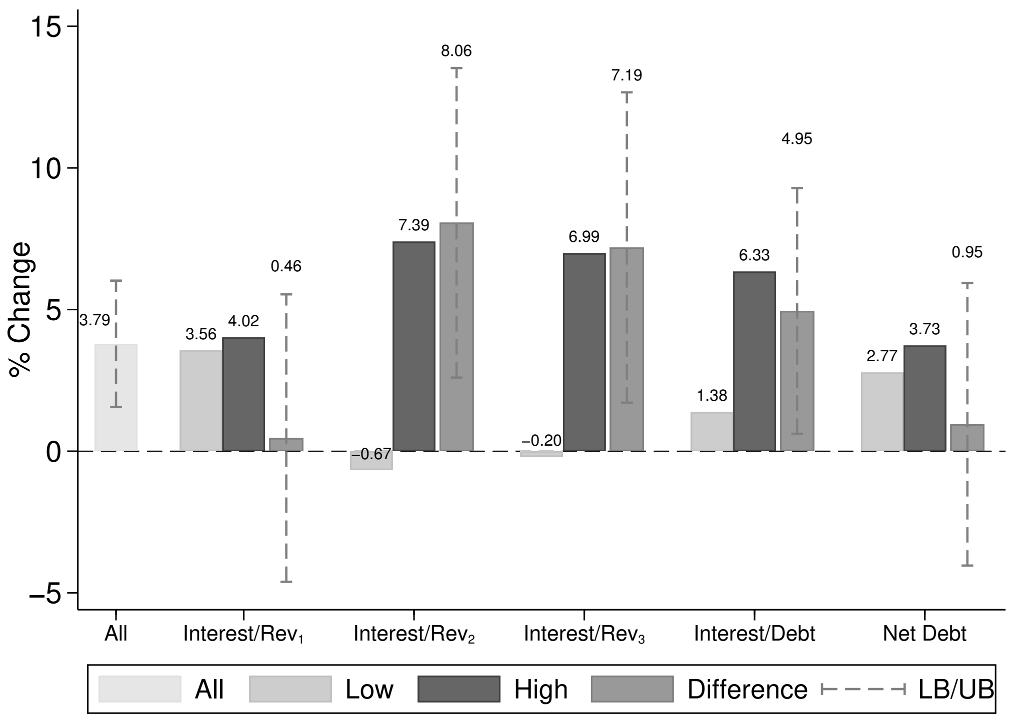


Figure IA7: Probability of Bond Rating Downgrade: The figure shows results for our main interaction term, β_0, from Equation 1. The dependent variable is a dummy variable equal to one indicating a bond rating downgrade (and zero otherwise). First, we show the baseline effect across all bonds in our sample. Next, we modify the baseline equation to interact with dummies for high and low values of ex-ante county level measures of debt capacity using interest expenditure and debt, namely: a) Interest/Revenue$_1$, b) Interest/Revenue$_2$, c) Interest/Revenue$_3$, d) Interest/Debt, e) Net Debt. See Table A1 for variables description. We additionally control for group-month fixed effects in these regressions. The corresponding bars for the low versus high groups and differences are indicated in the legend. Standard errors are double clustered by county bond issuer and year-month. The dashed lines represent 95% confidence intervals.

Table IA1: Comparison of Subsidy Datasets

This table provides a snapshot comparison of the information on subsidy deals between the original data from *Good Jobs First Subsidy Tracker* and the completed dataset prepared after hand-collection. Panel A shows a sample of data available from *Good Jobs First*. Panel B shows the information available in our completed dataset. “???” denotes that some information may be available, while “×” denotes that no information was available.

Panel A: Good Jobs First

					Winner		Loser			
Company	Year	Date	Subsidy ($ mil)	Investment ($ mil)	State	County	State	County	Jobs	Purpose
Baxter International	2012	×	211	???	GA	???	×	×	???	???
Foxconn	2017	×	4,792	10,000	WI	Racine	×	×	13,000	???
Vertex Pharmaceuticals	2011	×	72	???	MA	???	×	×	500	???

Panel B: Completed Dataset

					Winner		Loser			
Company	Year	Date	Subsidy ($ mil)	Investment ($ mil)	State	County	State	County	Jobs	Purpose
Baxter International	2012	4/19/2012	211	1,000	GA	Newton	NC	Durham	1,500	New
Foxconn	2017	7/26/2017	4,792	10,000	WI	Racine	MI	Wayne	13,000	New
Vertex Pharmaceuticals	2011	9/15/2011	72	2,500	MA	Suffolk	MA	Middlesex	500	Relocation

Table IA2: Names of Projects (Amounts in $ million)

This table shows some examples of project names under which the respective bidding processes were encoded by the winning local governments in order to maintain secrecy.

Company	Year	State	Project Name	Investment	Subsidy
Eastman Chemical	2007	TN	Reinvest	1,300	100
Burnham Institute for Medical Research	2006	FL	Power	90	310
Freightquote	2012	MO	Apple	44	64
Airbus (EADS)	2012	AL	Hope	600	158.5
Northrop Grumman	2014	FL	Magellan	500	471
Benteler Steel/Tube	2012	LA	Delta	900	81.75

Table IA3: Determinants of Subsidy

This table reports a linear regression of the amount of subsidy in our sample of deals from 2005-2018 on metrics potentially linked to the incentive. P-values are reported in brackets and standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Subsidy (USD million)					
Jobs (1000)	133.64***	89.79***	88.90***	110.31***	67.69***	64.23***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Investment (USD mil)		0.13***	0.13***	0.10***	0.10***	0.10***
		[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
State Expenditure (USD mil)			-0.03	-10.49*		
			[0.97]	[0.06]		
Median HH-Income$_{t-1}$(1000)					-2.25*	-2.39*
					[0.08]	[0.06]
State Surplus Gap (USD mil)						-3.53*
						[0.09]
Constant	68.70	8.34	8.37	534.37*	156.36**	165.04**
	[0.11]	[0.84]	[0.90]	[0.07]	[0.03]	[0.02]
State FE				✓	✓	✓
Event-Year FE				✓	✓	✓
Adj.-R^2	0.466	0.594	0.590	0.761	0.824	0.829
Obs.	126	117	116	108	100	100

Table IA4: Sample Generation: Secondary Market

This table summarizes the construction of the municipal bond transactions sample. The steps involved in cleaning the transaction data include: removal of data errors such as dropping bonds with missing information in the MSRB data, coupons greater than 20%, maturities over 100 years, and fewer than 10 trades in the sample period; as well as dropping individual trades occurring at prices below 50 and above 150.

	Number of CUSIPs	Number of Transactions
Customer Purchase trades (2005-2019)	2,499,014	59,890,438
Drop if maturity (days) $>$ 36,000 or $<$ 0 or missing	2,496,350	59,877,834
Drop if missing coupon or maturity	2,434,644	56,312,228
Drop if USD price $<$ 50 or $>$ 150	2,427,575	55,680,832
Drop primary market trades	1,711,814	44,073,138
Drop trades within 15 days after issuance	1,663,827	41,754,985
Drop trades with less than 1 year to maturity	1,556,152	40,151,034
Drop if yield $<$ 0 or $>$ 50%	1,543,510	39,394,883
Drop if $<$ 10 transactions	572,392	36,154,927
Match CUSIPs from MSRB txns to MBSD features	572,285	
Matching to FIPS using Bloomberg	564,517	
Matching to corporate subsidy locations by FIPS	218,377	14,358,884
Aggregating to CUSIP-month txns and plugging tax rates	215,184	4,465,916
Get tax-adjusted spread for event panel of 3 years using local bonds	123,468	2,645,441
- Winner	64,519	935,797
- Loser	82,629	1,709,644

Table IA5: Baseline Table with All Controls

This table reports the baseline results of Panel A in Table 3 for our sample using Equation (1) estimating the differential effect on municipal bond yields of winners versus losers after the subsidy announcement. Columns (1)-(3) show the results for monthly average yield as the dependent variable. In Columns (4)-(6), the dependent variable is after-tax yield spread (see Section 3.2). Our preferred specification comes from Column (6). T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Average Yield			After-tax yield spread		
	(1)	(2)	(3)	(4)	(5)	(6)
Winner x Post	9.77***	9.11***	9.44***	13.41***	13.18***	13.61***
	[3.23]	[3.57]	[3.63]	[2.79]	[2.95]	[2.99]
Winner	-0.92	2.02	2.14	0.84	2.61	2.61
	[-0.30]	[0.73]	[0.75]	[0.17]	[0.56]	[0.53]
Post ($t \geq 0$)	-2.61*	-2.16	-1.48	-1.72	-1.57	-0.63
	[-1.80]	[-1.59]	[-1.03]	[-0.70]	[-0.65]	[-0.25]
Coupon (%)		11.28***	11.28***		26.32***	26.31***
		[6.02]	[6.01]		[8.83]	[8.83]
Competitive bond dummy		4.77*	4.69*		7.60*	7.49*
		[1.85]	[1.84]		[1.76]	[1.76]
GO bond dummy		-29.50***	-29.52***		-46.85***	-46.88***
		[-8.19]	[-8.18]		[-8.21]	[-8.20]
Log(Amount)		-9.22***	-9.23***		-15.98***	-16.00***
		[-8.46]	[-8.48]		[-9.14]	[-9.16]
Callable dummy		-31.54***	-31.53***		-47.94***	-47.94***
		[-12.13]	[-12.20]		[-11.19]	[-11.23]
Insured dummy		-17.45***	-17.60***		-26.69***	-26.91***
		[-5.21]	[-5.28]		[-4.89]	[-4.94]
Remaining Maturity (years)		8.92***	8.92***		9.49***	9.50***
		[25.52]	[25.60]		[12.53]	[12.55]
Inverse Maturity (years)		-132.67***	-132.58***		60.36***	60.49***
		[-17.19]	[-17.24]		[4.90]	[4.91]
$\triangle$ Unemployment Rate (%)			3.61***			4.30*
			[3.02]			[1.96]
$\triangle$ Labor Force			0.50			0.61
			[1.45]			[1.06]
Log(Labor Force$_{t-1}$)			-84.69**			-115.09*
			[-2.14]			[-1.70]
Unemployment Rate$_{t-1}$			8.88***			12.84***
			[5.47]			[4.71]
County-pair FE	✓	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓
County Controls			✓			✓
Adj.-R^2	0.323	0.575	0.576	0.546	0.602	0.603
Obs.	2,645,441	2,645,441	2,645,441	2,645,441	2,645,441	2,645,441

Table IA6: Trading Volume

This table reports the baseline results similar to Table 3 for our sample using Equation (1), with trading volume as the dependent variable. Columns (1)-(2) show the results for a sub-sample of customer buy trades. Columns (3)-(4) show the results for a sub-sample of customer sell trades. Finally, Columns (5)-(6) use the sum total of buy and sell trades (wherever both are available) as the dependent variable in the trading volume. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Trading volume (bond-month)					
	Customer Buy		Customer Sell		Total	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner x Post	41,499.88*	42,914.84*	44,996.58*	47,968.45*	88,019.49	94,028.38*
	[1.75]	[1.81]	[1.66]	[1.75]	[1.59]	[1.68]
Winner	47,240.70*	44,044.81*	12,3697.44**	106,756.94**	242,087.05**	213,199.68**
	[1.86]	[1.93]	[2.08]	[2.08]	[2.10]	[2.16]
Post ($t \geq 0$)	-20,787.62	-21,476.14	-6,901.54	-6,477.56	-24,616.11	-24,170.04
	[-1.05]	[-1.09]	[-0.29]	[-0.27]	[-0.53]	[-0.52]
Deal FE	✓	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓
County Controls		✓		✓		✓
Adj.-R^2	0.050	0.050	0.048	0.048	0.055	0.055
Obs.	2,645,441	2,645,441	1,794,922	1,794,922	1,794,922	1,794,922

Table IA7: Predicting Winner

This table shows the results from a linear probability model using the 'winner' dummy as the dependent variable. We use the three-year event window, before the subsidy deal announcement. T-statistics are reported in brackets and standard errors are robust to heteroskedasticity. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Winner					
	(1)	(2)	(3)	(4)	(5)	(6)
Log(Labor Force)	-0.021	-0.021	-0.021	-0.021	-0.023	-0.007
	[-1.60]	[-1.57]	[-1.57]	[-1.57]	[-1.43]	[-0.30]
Unemployment Rate(%)		0.001	0.000	0.000	0.002	-0.001
		[0.08]	[0.03]	[0.05]	[0.32]	[-0.06]
△ Unemployment Rate(%)			0.000	0.000	0.000	0.000
			[0.12]	[0.12]	[0.05]	[0.24]
△ Labor Force				0.001	0.001	0.003
				[0.15]	[0.14]	[0.77]
Income Per Capita					-0.018	-0.006
					[-0.52]	[-0.18]
Log(House Price Index)						-0.036
						[-0.83]
Constant	0.753^{***}	0.747^{***}	0.751^{***}	0.751^{***}	0.770^{***}	1.010^{**}
	[4.81]	[4.28]	[4.23]	[4.23]	[3.64]	[2.27]
R^2	0.003	0.003	0.003	0.003	0.003	0.003
Obs.	738	738	738	738	735	680

Table IA8: Anticipated Jobs Multiplier: Evidence Based on Input-Output Tables

This table shows the evidence based on ex-ante county level expected jobs multiplier effect among winning counties, using the baseline Equation (1). We construct the measure of anticipated jobs multiplier effect by summing up the proportion of value-added in the upstream and downstream segments of a given industry, weighted by the corresponding county's share of wages. See Table A1 for variables description. We additionally control for the average impact within a particular group for that month by adding group-month fixed effects. Specifically, we show results by using the total measure in Column (1). In Column (2), we show results based on the upstream measure only. Finally, Column (3) shows the effect based on downstream measure. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread		
Interaction Variable:	Total	Upstream	Downstream
Winner × Post	(1)	(2)	(3)
× High dummy	5.60	11.20**	8.56*
	[1.27]	[2.19]	[1.87]
× Low dummy	21.07***	19.77***	16.70**
	[3.14]	[2.99]	[2.04]
Difference	15.46	8.57	8.14
p-val	0.03	0.26	0.37
County-pair FE	✓	✓	✓
Year-month FE	✓	✓	✓
County FE	✓	✓	✓
County Controls	✓	✓	✓
Group-Month FE	✓	✓	✓
Adj.-R^2	0.603	0.603	0.603
Obs.	2,645,441	2,645,441	2,645,441

Table IA9: Falsification Tests: Pre-refunded bonds

This table shows a falsification test based on Equation (1) using transactions from bonds that have been pre-refunded. We provide detailed steps for creating this sample in Section IA3.1. Column (1) shows the results using average yield as the dependent variable. In Column (2), we use yield spread as the outcome variable. Columns (3)-(5) report our results using only the subset of pre-refunded bonds with after-tax yield spread as the dependent variable. Specifically, Column (5) corresponds to our baseline specification. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Average Yield	Yield Spread	After-tax yield spread		
	(1)	(2)	(3)	(4)	(5)
Winner x Post	5.15	4.74	6.30	6.51	6.64
	[1.11]	[1.15]	[0.89]	[1.02]	[1.05]
Winner	3.64	4.19	7.12	8.13	7.16
	[0.79]	[1.03]	[1.01]	[1.22]	[1.06]
Post ($t \geq 0$)	-1.00	0.29	1.24	1.34	2.01
	[-0.47]	[0.14]	[0.36]	[0.41]	[0.60]
County-pair FE	✓	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓
Bond Controls				✓	✓
County Controls					✓
Adj.-R^2	0.478	0.780	0.602	0.648	0.649
Obs.	534,711	534,711	534,711	534,711	534,711

Table IA10: More Robustness Tests

In this table we report results for additional robustness tests for our baseline specification, i.e., Column (6) of Table 3 (Panel A). In Columns (1)-(6), we report results using only customer-buy trades with transaction size $\leq$ \$25,000, $\leq$ \$50,000, $\leq$ \$100,000, $>$ \$25,000, $>$ \$50,000, and $>$ \$100,000, respectively. We consider robustness to events based on the financial crisis of 2009 in Columns (7)-(8). Specifically, Column (7) reports the coefficient for the baseline equation from events before 2009. Column (8) corresponds to the main effect for events after 2009. There is no statistical difference between these coefficients. Columns (10)-(14) show results by dropping bonds that were issued close to the announcement of the subsidy. We consider periods of 6, 12, 18, 24, and 36 months on either side of the announcement date, respectively. Similarly, in Columns (15)-(17), we show our results by only focusing on a sub-sample of bonds issued within 18, 24, and 36 months of the subsidy announcement date, respectively. Columns (18)-(20) report results with other considerations, namely: controlling for log household income and log house price index (HPI) in Column (18), and controlling for bond level credit rating from S&P in Column (19). Column (19) shows the results for bonds with the most recent non-missing S&P credit ratings for a given CUSIP. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year-month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread							
	Transaction Size						Financial Crisis (2009)	
	≤25,000	≤50,000	≤100,000	>25,000	>50,000	>100,000	Before	After
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner x Post	14.81***	14.38***	14.21***	15.26***	15.84***	15.99**	17.35**	11.12*
	[3.10]	[3.11]	[3.12]	[2.97]	[2.81]	[2.54]	[2.44]	[1.94]
Adj.-R^2	0.621	0.614	0.610	0.608	0.600	0.590	0.603	
Obs.	1,878,324	2,256,876	2,451,519	1,541,971	965,832	562,657	2,645,441	

Dependent Variable:	After-tax Yield Spread									
	Drop Recently Dated Bonds Within					Keep Recently Dated Bonds Within			Other	
	6 months	12 months	18 months	24 months	36 months	18 months	24 months	36 months	More Controls	Add Rating
	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)
Winner x Post	12.62***	11.47**	12.08**	12.34**	12.72**	12.05***	11.91***	8.36**	9.50**	14.54**
	[2.73]	[2.44]	[2.47]	[2.41]	[2.26]	[3.00]	[3.01]	[2.30]	[2.15]	[2.53]
Adj.-R^2	0.595	0.588	0.581	0.575	0.557	0.790	0.785	0.778	0.618	0.583
Obs.	2,468,635	2,306,682	2,131,450	1,976,419	1,654,567	513,991	669,022	990,872	2,342,709	1,854,638

Table IA11: Bargaining Power of Winning Counties

This table shows the heterogeneity in bargaining power across counties and states, using the baseline Equation (1). We interact the main equation with dummies corresponding to the economic variables in each column, as described hereafter. Group-month fixed effects are added to control for the average effect within a particular group for that month. Column (1) shows results based on *Proposed Value* which is obtained by taking the ratio of the differential between proposed investment and subsidy to the county's lagged revenue. A low value of the ratio indicates low bargaining power of the county. In Column (2), we use the $\frac{Investment}{StateRevenue}$ ratio to create two bins based on the median value among the winning counties. Counties which received a large investment from the firm compared to their state's revenue would represent higher desperation and lower bargaining power. Column (3) shows the interactions based on *Intensity of Competition*. We use the gap between winning and losing states in their budget surplus to state revenue ratio as a proxy for intensity of bidding competition. A low gap in ratios denotes high bidding competition, leading to low bargaining power for the winner. In Column (4), we provide evidence from the ex-ante *Unemployment Rate* of the winning counties in the year before the deal. A high value of the unemployment rate would signify a lower bargaining power for the winning county. T-statistics are reported in brackets and standard errors are double clustered at county bond issuer and year-month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread			
Interaction Variable:	Proposed Value	$\frac{\text{Investment}}{\text{State Revenue}}$	Intensity of Competition	Unemployment Rate
Winner x Post	(1)	(2)	(3)	(4)
Low	16.30***	10.17*	7.32	-0.63
	[2.84]	[1.74]	[1.43]	[-0.10]
High	6.23	22.54***	20.66***	17.86***
	[1.04]	[3.54]	[2.85]	[2.71]
Difference	10.06	12.37	13.34	18.49
P-value	0.20	0.14	0.14	0.05
County-pair FE	✓	✓	✓	✓
Year-Month FE	✓	✓	✓	✓
County FE	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
Group-Month FE	✓	✓	✓	✓
Adj.-R^2	0.613	0.613	0.603	0.604
Obs.	2,297,669	2,306,768	2,641,062	2,641,062