IMPACT OF CORPORATE SUBSIDIES ON BORROWING COSTS OF LOCAL GOVERNMENTS*

Sudheer Chava[†] Baridhi Malakar[‡] Manpreet Singh[§]

Abstract

We analyze the impact of $38 billion of corporate subsidies given by U.S. local governments during 2005-2018 on their borrowing costs. We find that winning counties experience an 8 bps (2.85%) increase in their bond yields as compared to the losing counties. Winning counties with a lower debt capacity or a weaker bargaining power relative to the recipient firms experience higher borrowing costs (15–22 bps). In contrast, counties winning deals with a higher potential jobs multiplier experience lower borrowing costs (5–7 bps). Our results highlight some potential costs of corporate subsidies given by local governments ostensibly to create jobs.

Keywords: Corporate Subsidies, Municipal Debt, Public Finance
JEL Classification: G12, H25, H74

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[†]Scheller College of Business, Georgia Tech, *Email:* `schava6@gatech.edu`

[‡]Scheller College of Business, Georgia Tech, *Email:* `baridhi.malakar@scheller.gatech.edu`.

[§]Scheller College of Business, Georgia Tech, *Email:* `msingh92@gatech.edu` (corresponding author).

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 multiplier effects. However, the foregone revenue may require local governments to increase taxes or municipal debt or both to fulfill the additional demand for public services or cut spending (Bartik, 2019). In this paper, we shed light on the net economic impact of large corporate subsidy deals by documenting their effects on the borrowing costs of local governments.

The municipal debt market, at $3.8 trillion, is a significant source of financing for local governments in the U.S. 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, wages offered and the multiplier effects.[2] Moreover, confounding events during the long gestation period complicate the measurement of multiplier effects as they rely on a number of assumptions. To the extent that bond prices reflect investors' expectations about the future revenues and costs of the local governments, municipal bond market provides an ideal setting to study the net economic benefits of the corporate subsidies to the local governments.

The announcement of the subsidized corporate investment on the borrowing costs of local governments is ambiguous. We hypothesize that municipal bondholders react positively to the deal announcement if the expected economic benefits (including multiplier effects) from the promised investment exceed the costs to finance the additional civic burden (including the subsidy itself). Alternatively, bond investors may react negatively to the announcement if the costs outweigh the benefits.

We hand-collect county-level data on competition for large corporate investment deals with $50 million or greater subsidy. These deals involved an aggregate subsidy of $38 billion during 2005–2018 for a total planned investment of $131 billion. Identifying the causal impact of the announcement of a corporate subsidy deal on the borrowing costs of local government is challenging since we cannot observe what would have happened if the winning county did not win the bid. So, we follow Greenstone, Hornbeck, and Moretti

[1] Buss (2001) details that as early as 1844, Pennsylvania had invested over USD 100 million in more than 150 corporations and placed directors on their boards. More recently, 238 cities made bids offering tax incentives 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 (see: `https://tinyurl.com/yy95kmlb`).

[2] For example, in 2017, Wisconsin announced $4.1 billion subsidy to FoxConn, which is $1,774 per household and $230,000 per job. After the announcement, FoxConn has repeatedly changed its investment plans and the number of jobs it will create.

(2010) and Bloom et al. (2019) and consider counties that were the closest runner-up bidder for the project (the losing county) as the counterfactual county.[3]

Using secondary market trades for 123,187 municipal bonds of the winning-losing county pairs for the 127 deals, we estimate event-study style difference-in-differences regression with deal fixed effects (i.e., winning-losing county-pair fixed effects) and calendar-time month fixed effects (to control for declining trends in the yields during our sample period). First, we confirm that the the pre-trends in the key economic indicators (for example, the unemployment rate, aggregate county employment growth, county ratings, and the underlying county risk using local betas (Tuzel and Zhang, 2017)) for the winning-losing county pairs are statistically similar. However, we find that within a quarter after the announcement of the deal, there is an upward trend in the bond yields of winning counties while there is no change in the bond yields for losing counties. The bond yields for winning counties increase by 5.43 to basis points (bps) (8.36 bps) compared to the losing counties, within 6 months (within 36 months) after the deal. The results are similar when yield-spreads or tax-adjusted yield-spreads are used as the dependent variable.

One of the identifying assumptions for our empirical strategy is that the bidding counties follow similar economic trends before the deal. In line with this assumption, we confirm that the bond yields of winning and losing counties follow similar trends, and the difference between the two groups is statistically insignificant before the announcement of the deal. We also find an upward (downward) trend for aggregate employment (unemployment rate) for both winning and losing counties after the deal announcement. These findings suggest that our results are unlikely to be driven by the poor economic condition of bidding counties. However, corporations do not choose their locations randomly (Greenstone, Hornbeck, and Moretti, 2010). So, we estimate a predictive regression of winner dummy using various county-level ex-ante observable characteristics such as the level and changes in unemployment rate, level and changes in the labor force, house price index and personal income. We do not find any of these observable county-level characteristics systematically predict the probability of winning the deal. But, the subsidy offered to the winning county is positively correlated with the jobs promised and investment size, while negatively correlated with winning state's previous year budget surplus. Another possible concern with our identification may be that the timing of the subsidy announcement confounds with the declining economic health of the winning counties.

[3]We provide evidence for validity of the identifying assumption i.e. the winning county and the closest runner-up bidder follow similar economic trends before the deal. Further, the inherent secrecy maintained by local governments alleviates concerns about bond investors anticipating the deal. Meanwhile, the two-way matching between the firm and county reduces the concerns about market-timing by local government issuers.

The evidence that not all types of municipal bonds demonstrate an increase in the yields helps mitigate this concern. In fact, there is a decline in yields for bonds issued for some public services (-5.24 bps) and water-sewer projects (-8.33 bps) after the subsidy announcement.

Local governments face a trade-off in using targeted business incentives for economic development. The corporate subsidy deal may bring in new jobs with possible spillovers (Greenstone, Hornbeck, and Moretti, 2010). However, the increased demand for public services after the new plant opening along with foregone tax revenue may require counties to increase municipal debt. Our main result suggests that on average, the costs outweigh the benefits from the subsidy. However, there is significant heterogeneity across the muni bond response for various municipalities. In order to shed more light on the underlying economic mechanism, we explore the heterogeneity in the deals and among winning counties based on the expected costs and benefits of the deal. Specifically, we analyze the heterogeneity in the potential multiplier effects of the deal based on various deal characteristics and the role of debt capacity of the winning county and how it may impact the cost of existing and additional borrowings of the winning local governments.

First, we consider ex-ante debt capacity of local governments in three ways: a) interest expenditure, b) county credit ratings, and c) tax privilege (similar to Babina, Jotikasthira, Lundblad, and Ramadorai (2019)). We expect that a lower debt capacity of the counties may result in greater increase in bond yields after the deal. We find that counties with higher interest expenditure show a higher impact on yields (15-22 bps). Interestingly, for counties with low interest expenditure, the borrowing cost decreases by at most 8.93 bps. This suggests that for these counties the benefits outweigh the costs. Moreover, counties with lower ex-ante credit rating also end up paying relatively higher yields on their existing debt after the deal. Finally, we also find that lower (higher) tax privilege increases (decreases) the borrowing costs of winning counties.

We use two measures to understand the implications of expected jobs multiplier effects on municipal bond yields after the deal. Deals involving lower jobs multiplier may show a greater increase in bond yields for winning counties. In our first measure, we focus on knowledge spillovers using the economic importance of aggregate value of patents granted to the firm before the deal (Kogan, Papanikolaou, Seru, and Stoffman, 2017) which is receiving the subsidy. We find that subsidy deals involving more (less) innovative firms show a decrease (increase) in municipal yields of at most 5 bps (at most 16.83 bps) after the announcement. We also use industry-level jobs multiplier from Economic Policy Institute to quantify the differential effect on municipal bond yields. We find that deals involving industries with low multiplier effect show a greater impact on borrowing cost of the winning counties. Next, we study the interaction between these two factors: debt capacity and expected multiplier effects. We find that the potential gains from knowledge spillover and jobs multiplier help mitigate the negative effects of debt capacity. For example, with high jobs multiplier, the borrowing cost reduces by about 7 bps, even for winning counties with high ex-ante interest expenditure.

As additional heterogeneity, we study the bargaining power of the winning counties relative to firm involved in the subsidy deal. We use four measures of bargaining power: a) the ratio of firm assets to the county-level revenue, b) ratio of subsidy offered to ex-ante county's budget surplus, c) intensity of competition (the gap between the bidding states budget surplus to revenue ratio) and d) unemployment rate. We expect that counties with a weaker bargaining power observe a higher increase in borrowing costs. For example, based on firm assets to county revenue, we find that counties with low (high) bargaining power observe an increase (decrease) of 14.52 (4.50) bps in bond yields. We find consistent results with the other three measures of bargaining power.

We focus on secondary market trades to avoid any confounding endogeneity due to market-timing in the new municipal bond issuance market. Our baseline results include multiple controls specific to the bond. We control for coupon rate, size of issuance, remaining maturity, callability, status of credit enhancement and type of security based on bond repayment source (tax sources for general obligation bonds and specific revenue stream for revenue bonds). Further, we utilize county-specific controls including lagged level and changes in the unemployment rate and labor force, the average per capita county income, and house prices to control for local economic conditions. As a restrictive specification, we control for bond fixed effects to further control for bond-specific unobservables. To control for any state and county unobservables, in some specifications, we also include the county and state fixed effects. Besides, in a falsification test, 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) and find that, as expected, there is no impact.

Finally, we test the implication of higher borrowing costs reflected by the secondary market yields. First, we find that compared to a year before the deal, the new municipal issuance for the winning counties increases to about 3 times in the year after the deal. Whereas, for the losing counties this increase is only about 1.5 times. Further, we show that this higher issuance is driven by winning counties with more debt capacity i.e., those with lower interest burden. Compared to the losing counties, there is an increase of about 4.7 bps in new issuance yields for the winners after the deal. Next, we analyze if winning counties constrained by their debt capacity increase taxes or not. We find that counties with high interest expenditure experience an increase in property tax revenue without a commensurate increase in house price index. These results suggest that counties may either be increasing the property tax rates or reassessing the property values. Moreover, we do not find any changes in the expenditure on the public services at the county level. Overall, our results suggest that 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.

Our paper relates to the large literature on tax incentives (see 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[4] on the net aggregate implications of subsidy-based location economics by studying their impact on the yields of municipal bonds, a critical source of financing for the local governments. Our results suggest that some winning counties, especially those with a weaker bargaining power vis-a-vis the corporation, experience an increase in their bond yields after winning the deal. 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 the local communities. In this regard, we also contribute to the recent literature on municipal bonds (Adelino, Cunha, and Ferreira, 2017; Schwert, 2017; Gao, Lee, and Murphy, 2019a,b; Babina, Jotikasthira, Lundblad, and Ramadorai, 2019). Our hand collected records of winning and losing counties for large subsidy (defined as those exceeding USD 50 million) deals in the United States may also be useful for future studies.

The rest of the paper proceeds as follows. We discuss our empirical methodology and identification concerns in Section 2. Section 3 describes our data and provides summary statistics. Our main empirical results are presented in Section 4, and we conclude in Section 5.

2 Identification Challenges and Methodology

In this section we firstly discuss the challenges in identifying the impact of location-based policies and then describe our empirical specification.

[4]Greenstone, Hornbeck, and Moretti (2010) document a 12% increase in total factor productivity (TFP) in incumbents of the winning county 5 years after the opening of a large plant suggesting agglomeration gain to the county. Slattery (2018) uses the state-level bidding process to show that the firms capture the welfare gains in subsidy competition. Bartik (2017) and Serrato and Zidar (2018) find tax credits, rather than statutory rates, better explain the variation in corporate tax revenue. Ossa (2015) and Mast (2018) empirically estimate their model to consider the subsidy setting decision using data from New York state.

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 'winner' of a corporate subsidy deal may depend on local economic conditions.

Greenstone and Moretti (2004) argues 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 have been made offers 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 IA10). Regardless, there is adequate anecdotal evidence in support of a bidding process involving competitive subsidy bids offered by both the bidders[5].

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 winning and corresponding losing county follow similar economic trends before the deal announcement. If the winning county is in worse economic shape, then it's bond yields should be higher which implies that our main effect is over-estimated. We plot the trends for bond yield, county-level aggregate employment, unemployment rate, bond rating, and local beta around the subsidy announcement and do not find supporting evidence for negative selection (see Section 4.1.2 for details.)

[5] 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`

2.2 Methodology

Our baseline event study focuses on the impact of corporate subsidies on the borrowing cost 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 the newspaper articles to identify 127 winner-loser deal pairs at the county level spanning 39 states during 2005-2018. We use a three-year window before, and after the subsidy announcements[6]. 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).

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

$$\begin{aligned} y_{i,c,d,t} = \alpha &+ \beta_0 * Winner_{i,c,d} * Post_{i,c,t} + \beta_1 * Winner_{i,c,d} + \beta_2 * Post_{i,c,t} \\ &+ BondControls + CountyControls + \eta_d + \gamma_t + \epsilon_{i,c,d,t} \end{aligned} \tag{1}$$

where, index i refers to bond, c refers to county, d denotes the deal pair and t indicates the event year-month. After-tax yield spread is the dependent variable in $y_{i,d,t}$ obtained from secondary market trades in local municipal bonds (described in Section 3). We also use the raw average yield and yield spread as dependent variables. *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 coefficient of interest is β_0. The baseline specification also includes two sets of fixed effects: deal pair fixed effects (η_d), so the comparisons are within bonds mapped to a winner-loser pair; γ_t denotes year-month fixed effects to control for time trends. We follow Bergstresser, Cohen, and Shenai (2013); Gao, Lee, and Murphy (2019a) 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 follow

[6] Our results are robust to using other windows, as shown in Panel C of Table 2

Gao, Lee, and Murphy (2019a) in clustering standard errors at the issue-year month level, unless specified otherwise. Our results are robust to alternate clustering methods and reported in Section 4.2.5

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 among 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 more from a particular firm locating within it is more likely to attract the project with greater incentives. Simultaneously, a county in 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. To the extent that counties may be bidding for new jobs during specific business cycles, our deals are distributed over different periods in the 14 years during 2005-2018.

3 Data

In this section we provide details about data used in this paper. First, in Section 3.1, we describe our data on corporate subsidies. Section 3.2 discusses 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 IA1, 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 sampler 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 IA2 for a ranking among states). On per capita basis, Washington, Oregon and Louisiana spent over USD 1,500 during this period. Specifically, Figure IA3 depicts the subsidy value per capita using a chloropleth map with five breaks shown in the legend.

One of the challenges that previous studies faced in evaluating the impact of corporate subsidies is the lack of comprehensive data at the county-level. We discuss the literature on location-based incentives in Internet Appendix A. The identification used in this paper relies on close-bidding auctions where two cities compete against each other to attract a firm. Local governments may be backed by their respective states in sponsoring money. 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 B.

We can identify 127 winner-loser deal pairs at the county level, which we define as consisting our final sample with subsidy over USD 50 million in each deal[7]. Of these, 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 300 million, whereas the median amount is USD 123 million. Comparing this to the proposed investment, we find that the median deal gets 37% subsidy as a proportion of investment. The average subsidy per job promised by the firm amounts to nearly USD 468,000. 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 investment amount and jobs promised to be strongly correlated. Figure IA4 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 Municipal Bonds dataset by FTSE Russel (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, yield to maturity, tax status, insurance status, pre-refunding status, type of bid, coupon rate and maturity date for bonds issued after 1990. We also use

[7]As such, there are 120 unique firm-year level subsidy deals among bidding states. For 6 deal-pairs, we do not have information on the jobs promised. There were 13 pairs for which we could not gather data on the size of investment for the proposed project.

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 and 1 for the lowest.

An important step in our data construction is to link the bonds issued at the local level to the counties which make the subsidy bids. This geographic mapping would allow us to study the 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 of each bond, we need to supplement this information from other sources like Bloomberg. However, in light of Bloomberg's download limit, it is not feasible to search information on each CUSIP individually. Therefore, we first extract the first 6 digits of the CUSIP to arrive at the issuer's identity[8]. Out of 63,754 unique issuer identity (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 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 Municipal Securities Rulemaking Board (MSRB) database on secondary market transactions during 2005-2018. Our paper closely follows Gao, Lee, and Murphy (2019a) in aggregating the volume-weighted trades to a monthly level. Following Downing and Zhang (2004); Gao et al. (2019b), 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 IA5, we provide descriptive statistics on bond features pertaining to the primary market and secondary market, respectively. We provide the description of key variables in Table A1.

The primary outcome variable used in Equation 1 is the tax-adjusted spread over risk free rate. We match the bond's remaining maturity to that of zero coupon yields

[8]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.

to get the reference benchmark for risk free rate following Gürkaynak, Sack, and Wright (2007). Tax adjustment follows Schwert (2017) wherein 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 even). 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:

$$1 - \tau_{s,t} = (1 - \tau_t^{\text{fed}}) * (1 - \tau_{s,t}^{\text{state}}) \tag{2}$$

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

$$spread_{i,t} = \frac{y_{i,t}}{(1 - \tau_{s,t})} - r_t, \tag{3}$$

where r_t corresponds to the maturity matched zero coupon yield for a bond traded at time t. From Schwert (2017), we use the top federal income tax rate as 35% during 2005 to 2012, and 39.6% for 2013 to 2015. Extending the last segment further, we use the same rate for 2016 as well. Subsequently, we match the municipal bond yields to similar maturity zero coupon yields (ZCY) to get the corresponding proxy for risk free rates. We round the bond maturity in years to the nearest integer. Since ZCY is not available for maturity over 30 years, we can not match those bond observations with risk free rate and they do not feature in the final analysis. However, since the average remaining maturity for the sample is 10 years, this should not create a significant bias.

3.3 Other Variables

We use Census data 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. We use county level household income from Internal Revenue Service (IRS) to get total personal income at the county level. Our unemployment data comes from Bureau of Labor Statistics. For county-level population, we use data from Surveillance, Epidemiology, and End Results (SEER) Program under the National Cancer Institute. As a proxy for 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 from the raw data on yields, evidence on parallel trends assumption and a falsification test. Section 4.2 shows robustness tests for our baseline specification. We propose the potential mechanism to explain our results in Section 4.3. In Section 4.4, we analyze the heterogeneity in relative bargaining power between the county and the firm involved in the subsidy deal. Finally, we discuss the impact on primary market of municipal bonds (Section 4.5) and local property taxes and public expenditure (Section 4.6).

4.1 Impact on Borrowing Costs of Local Governments

4.1.1 Dynamics and Baseline Results

We begin our analysis by plotting the yields observed in the secondary market between the winning and losing counties. Our event window comprises three years before and after the subsidy deal announcement. We use the 12 months before the event window (T=-37 to T=-48 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 raw yields based on Equation 4 below.

$$\begin{aligned} y_{i,c,d,t} = \alpha + \beta_q * \sum_{q=-12}^{q=12} Winner_{i,c,q} * Post_{i,c,q} + \delta_q * \sum_{q=-12}^{q=12} Loser_{i,c,q} * Post_{i,c,q} \\ + \eta_d + \kappa_t + \epsilon_{i,c,d,t} \end{aligned} \tag{4}$$

where, index i refers to bond, c refers to county, d denotes the deal pair, t indicates the event year-month and q refers to the quarter corresponding to the event month t. After-tax yield spread is the dependent variable in $y_{i,c,d,t}$ obtained from secondary market trades in local municipal bonds. η_d represents the deal-pair fixed effects; κ_t represents the year-month fixed effects. 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.

In Figure 1a, the solid line with circles plots the pre-tax average yield over the 3-year window for winning counties on average. The losing counties are depicted using a dashed line with diamonds. The figure reveals no statistical difference between the two groups before the deal announcement. The treatment and control groups exhibit parallel trends in terms of bond yields. Second, the average yields for the winning counties appears to be higher than that of the losing counties in the first quarter after the deal. Finally, we find that the difference between the two groups persists for at least 10 quarters. The trends look similar for yield spread as the dependent variable (Figure 1b). Consistent with Schwert (2017), we adjust the yield and yield spread for taxes because many municipal bonds are tax-exempt securities. We find similar results for after-tax yield and after-tax yield spread (Figures 1c and 1d) as our dependent variable. We use the after-tax yield spread as our primary dependent variable in the subsequent analysis.

Note that the above results only represent the raw difference in 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, $Winner_{i,d} * Post_{i,t}$, identifies the differential effect after the subsidy deal announcement on average yields of winning counties in comparison to the losing counties where we control for observable characteristics. To revisit our identifying assumption: the losing county serves as an adequate counterfactual to map how the winner's yields would have changed in the absence of the deal announcement. The deal fixed effects ensure estimation from within each deal pair. The year-month fixed effects control for declining yields in the overall municipal market during our sample period, over and above the treasury adjustment for spreads.

Table 2, Panel A reports the effect of winning a subsidy deal on the municipal bond yields 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 coupon (%), log(amount issued in USD), dummies for callable bonds, additional credit enhancement, 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 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. Since subsidies are often motivated by job creation, we use these measures at the county level consistent with previous literature.[9] Thereafter, in each successive triplet of columns, we follow the same scheme and show our results using yield spread as a dependent variable, followed by after-tax yield spread.

Using Column (9) of after-tax yield spread as our baseline case implies that the yield spread for winning counties increases by 8.36 bps after the subsidy announcement, in comparison to the losing counties. The 8.36 bps is equivalent to additional borrowing cost amounting to 7.5% (=2.8/38) 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. We find that this amount is ∼ $400 billion in the deal year. The increase of 8.36 bps amounts to $334 million (=0.000836*400*1,000) in additional interest costs per annum. The average outstanding maturity of bonds is 10 years. We discount this additional monetary cost for 10 years using the average yield (winners) of 2.8% to get $2.8 billion.

Next, in Panel B we show the baseline result of Column (9) using different forward windows, keeping the pre-event window same as three years. We find that the magnitude of the differential impact increases from about 5 bps within the first six months after the event (Column (1)) to about 12 bps in 5 years (Column(8)). 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 window used, we show our main result in Panel C using different pre- and post-windows. While the effect is relatively small in magnitude and weak in significance for the 12 month window in Column (1), we see qualitatively similar results for other windows in the remaining columns. We argue that a longer period is needed both in the preceding and succeeding periods around the announcement to arrive at sharper estimates of the effect, especially given the limited trading in the municipal bond market. We also observe an increase in trading activity after the deal announcement. We find an increase in both customer buy and customer sell trades and report results in Table IA8. In the next sub-section, we provide evidence on the parallel trends assumption.

4.1.2 Do bond yields respond to underlying local economic differences?

Our baseline comparison between winning and losing counties' yields assumes similar local economic conditions between the treatment and control groups during the event window around the deal. The results in Section 4.1 suggests that winning and losing

[9] We report these coefficients for bond level and county level controls in Table IA7. For robustness, we further include the lagged values of log(personal income) and log(house price index). The magnitude of our main effect remains similar reported in Section 4.2

counties exhibit parallel trends in their bond yields. However, as discussed before, the decision by local governments to engage in the bidding process to attract firms may not be random. Thee 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) S&P municipal bond rating and d) local beta between winners and losers in Figure IA5. In each of these subplots we use the annualized version of Equation 4, but additionally introduce county fixed effects. Here, we cluster standard errors at the deal level.

In Figures IA5a and IA5b, 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 in the losing county.

Further, Figures IA5c and IA5d provide a comparison of the county level creditworthiness and riskiness, respectively. We use bond level ratings aggregated up to the event-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 the 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 3 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 the median income and house price index. Table IA9 shows the regression results where we introduce each regressor successively. We plot the coefficients from Column (6) in Figure IA6 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.

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 it does not control for unobserved time-varying county specific changes that may be happening simultaneously as the deal is announced, which may also affect the bond yields. 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 increase in yields for all types of bonds irrespective of use of proceeds. We find that not all types of municipal bonds demonstrate a change in yields as shown in Figure IA7. The effect is primarily driven by bonds raised to finance primary education (5.11 bps), economic development (7.06 bps) and infrastructure (21.19 bps). In fact, the negative effect on other public services (-5.24 bps) and water-sewer (-8.33 bps) shows that the results are not driven by the declining health of the winning county. Consistently, we also find that bonds with longer duration show a higher increase in yields. For example, bonds with duration less than 5 years do not seem to get affected. There is a 7.11 bps increase in bonds with duration of 5-10 year and 12.44 bps increase for bonds with duration over 10 years.

4.1.3 Falsification Tests: Pre-Refunded Bonds

It is common 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 risk free (Fischer, 1983; Chalmers, 1998; Schwert, 2017). Local governments may choose to pre-refund their bonds thereby offering a clean change of the 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 yields 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, exclude bonds that are not exempt from federal within-state income taxes; 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, called current refunding. Table IA6 shows the results of the falsification test. In Column (1), we find that the average yield for winners goes up by a marginally small magnitude of about

1.5 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) and (4) report the effect on after-tax yield spreads without and with county controls, respectively. The magnitude is even smaller than the previous two columns and statistically insignificant again. Thus, we do not find any impact on these pre-refunded bonds as they have been secured against the escrow of funds deemed 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 market.

4.2 Robustness Tests

In this section, we test the robustness of our main result in Column (9) of Table 2 to various alternative specifications. We present the results of these robustness checks in Table 3.

4.2.1 Is the effect driven by large institutional 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 [10]. Column (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 9 bps in each of these sub-samples. This suggests that our main result is also present in smaller transactions.

4.2.2 Are results driven by newly issued bonds?

Even though our data from MSRB on secondary market bond yields is cleaned for primary-market transactions recorded therein, we assume further precaution in favor of seasoned bonds. In Columns (4)-(7), we report our baseline results by dropping bonds that were recently issued i.e., within 6, 12, 24 and 36 months of the subsidy announcements. By doing so, we remove bonds from the sample that have been newly issued and may demonstrate unusual trading in the initial phases. The magnitude ranges from 7.46 to 5.01 bps, which is lower than our baseline case, but still statistically and economically

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

significant. This shows that our results are not only driven by trading activity in newly issued bonds.

4.2.3 Financial Crisis of 2009

Another potential worry may be due to the sample period spanning the financial crisis of 2009. Understandably, this was a period of major volatility in the financial markets across asset classes and municipal bonds were not immune to this. As a result, we report our findings by excluding this period from our data. We consider two approaches: first, in Column (8), we show our results by only keeping bond transactions after 2009. This reduces the number of observations and the magnitude goes down to 5.83 bps. Second, in Column (9), we show the results by retaining only those deal events that were announced after 2009. In this case, the borrowing cost increases by 8 bps. Taken together, we argue that our regression estimates in the baseline specification are not entirely driven by unusual activity in the municipal bond market due to the financial crisis of 2008-09.

4.2.4 Other Trades

In our baseline estimates, we use only customer buy trades at the local level. We test our specification using a different set of trades/bonds in our baseline. First, to evaluate if our results are sensitive to the choice of customer-buy trades, we run the baseline regression using only the customer-sell trades (Column (10)). There is still an increase of about 6 bps. Second, in Column (11), we document the impact on state level bonds only (otherwise excluded from our main sample) among the bidding states. As expected, the impact is much lower (1.39 bps) at the state-level as the primary impact of many of the corporate subsidy deals is at the local county level.

4.2.5 Assumptions on correlation of standard errors

In our baseline specification, we cluster standard errors by issue-year month following Gao, Lee, and Murphy (2019a,b). In Columns (12)-(15), we show our main result using alternative definitions for clustering. First, we consider the possibility that the standard errors may be correlated across different bond issuers over the calendar months (see Column (12)). Further, it could also be that the error term in our main specification is correlated over specific bond issues and over event months (instead of year month), i.e. relative to the subsidy announcement date. This accounts for the fact that the bond market response may be sharper and correlated in the first few months just after the news of the deal. We also use state-year month clustering in Column (13) and deal-year month clustering in Column (14). These specifications help to address the concern that the cross-sectional variation in bond yields may be correlated across the state by which the bond is issued or by the specific deal event itself. The last specification in Column (15) is rather restrictive as it attributes correlations at the deal-pair level. Here, we use only 127 deal pairs in the cross-section to adjust the standard errors. In all of these specifications, we find statistical significance similar to our baseline specification. Thus, our results are robust to these alternative choices of clustering standard errors.

4.2.6 Additional Bond and County Level Controls

In the baseline specification we do not include bond ratings so that we could analyze both rated and unrated bond transactions. Here, we check on 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 (16) of Table 3 by introducing the numeric equivalent of bond level ratings among the regressors. The magnitude goes up to over 11 bps and the result is statistically significant.

In Column (17), we present our results by introducing some more county level time-varying covariates. We introduce the lagged values of log(personal income) and log(house price index) to account for any changes in these metrics that may be simultaneously changing as the subsidy announcement. Indeed, there is some evidence that firm decisions to locate in a region may increase house prices locally [11]. Hence, we control for that and still find an increase of 6.67 bps.

In Section 4.1.2, we document that bonds with higher duration show a greater increase in yields. To verify the robustness of our results to duration effects of bonds, we modify the baseline specification in Columns (18)-(20). First, in Column (18), 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. We report a higher impact of 10.21 bps. Next, in Column (19), we show the same result by calculating duration based on after-tax yields. This tax adjustment further increases the impact to 10.57 bps. Finally, in Column (20), we use duration as a control variable but also change the dependent variable. Here, we match the treasury yields based on the duration instead of remaining years to maturity. We thus get the after-tax yield spreads after adjusting for duration (instead of remaining maturity). We find the baseline effect as 10.19 bps and it remains statistically significant. These combinations help address any concerns that duration effects are driving our primary result.

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

4.2.7 Unobserved heterogeneity at county and state levels

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 state level. First, we absorb all county level, time-invariant variation beyond the deal fixed effects in Column (21). This yields a baseline effect of 10.54 bps. Next, we impose county-year fixed effects to absorb county-level variation that changes over calendar years. In Column (22), we report an increase of 2.87 bps which is statistically significant at the 1% level. Given the buy and hold nature of the investors in this market, we argue that this is a very restrictive setting. This results in a substantially muted effect. Next, in Column (23) we show the result by dropping counties that may get repeated as a winner or a loser within 6 years of the previous deal. Here, the number of observations reduces substantially but even so the magnitude increases to 13.42 bps. Next, to control for state level characteristics among the bidding counties, we show our results with state fixed effects and state-year fixed effects in Columns (24) and (25) respectively. This may be relevant because the competition often manifests at the level of the state governments. We find a significant impact even though the magnitude drops to 3.16 bps in Column (25). Overall, our results suggest that time-varying unobservables at county and state levels may not be the primary driver of our main results.

4.2.8 Alternative specifications

Finally, we also consider some other specifications. We start with a very restrictive specification in Column (26) where we impose bond fixed effects. We also require that the same bond was trading at least once before and after the announcement of subsidy. These requirements are over and above the baseline specification which seeks to tease out variation between winning county and losing county from within a deal pair. Here, the magnitude drops to about 4 bps, but is still significant. We argue that this bond-specific impact may not represent the average borrowing cost prevailing for the overall county in the secondary market. Thus, it is an under-estimation of the main effect and may correspond to the lower-bound.

Next, we address the concern that the relative age of the transaction with the respect to the deal announcement may vary over time. We introduce event month fixed effects over and above the year-month fixed effects in Column (27). The magnitude remains similar to our baseline case. Next, in Column (28) we replace the year-month fixed effects with event-month fixed effects. Our magnitude goes up to 9.31 bps, thereby indicating that our choice of using the year month fixed effects is more conservative.

In Column (29), we use the event month fixed effects as in the Column (28), but now cluster the standard errors by issue-event month. Our results are still robust to this choice of clustering for the alternative specification in fixed effects. In Column (30), we replace the month fixed effects with calendar year fixed effects and find that a higher magnitude of 11.25 bps for our key variable. This again reinforces our rationale to use the more conservative year month fixed effects in the baseline specification to absorb for month-specific variation over and above the year-specific variation.

4.3 Mechanism

As we discussed before, the local governments face a trade-off while using targeted business incentives i.e., foregoing future tax revenue versus anticipated jobs multiplier benefit (see Greenstone and Moretti (2004)). The increased demand for public services after the new plant opening while simultaneously foregoing revenue in the form of subsidy may require local governments to increase municipal debt. The underlying debt capacity of the winning counties may impact the cost of existing debt and additional borrowing. Whereas, the large multiplier effect of new plant on incumbent businesses may attenuate the effect of debt capacity. In this section, we discuss the effect of debt capacity on municipal borrowing cost and its interaction with the expected multiplier effects.

4.3.1 County Debt Capacity

We consider ex-ante debt capacity of local governments using three proxies: a) interest expenditure, b) county credit ratings, and c) tax privilege. First, we expect the secondary market impact to be higher for winning counties with large ex-ante interest expenditure. To this end, we use three measures based on the ex-ante interest expenditure, namely: (i) interest burden (ratio of interest on general debt to total revenue), (ii) interest to debt (ratio of interest on general debt to total long term debt outstanding) and (iii) interest to expenses (ratio of interest on general debt to total expenditure). We use interest 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. The results are depicted in Figure 2 which shows the coefficient of interaction term for each group. First, among counties with high interest burden ratio, we find the bond yields going up by 15.34 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, in the next bar graphs, we find that the borrowing cost for counties with higher ex-ante interest to debt ratio goes up by 22.13 bps. In contrast, counties with low interest to debt see a reduction in yields of about 8.93 bps. The difference between the two groups is economically and statistically significant. We find similar results by using interest to expense ratio.

Next, we consider debt capacity using county credit ratings. Table 4 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[12]. As before, we control for the average effect within a group in a given month using the relevant fixed effects. Our estimation results show that a lower credit rating is associated with a higher increase in the winning county's municipal bond yields. In Column (1), we show the results corresponding to all bonds (with a rating) in our sample. We find an increase of 8.45 bps for counties rated as A+ and below; whereas, the increase in yields is 5.89 bps for counties rated as AA- and above. Similarly, within the subset of general obligation bonds, the yields go up by 16.69 bps among the low rated counties. On the other hand, the yields decrease by 6.74 bps for high rated counties. The difference between the two groups is economically and statistically significant. Column (3) shows the results corresponding to the subset of revenue bonds, which are qualitatively similar to the full sample results of Column (1).

Finally, drawing upon the demand side of municipal bonds, we explore the debt capacity of the county with respect to the local investor base. This argument is based on the retail clientele/ownership for municipal bonds, the local home biasand the unique tax-exemption from investment into this market. These features support the idea that if a county has many potential subscribers to local bonds, then it should have a higher debt capacity. As a result it should find better cushion against adverse shocks to its bond prices. To test this, we adapt the tax privilege measure in Babina, Jotikasthira, Lundblad, and Ramadorai (2019). It is defined as the highest state income tax rate applied to income from municipal bonds issued by other states minus the highest state income tax rate applied to income from the winning state-issued municipal bonds. Since

[12]The county level credit rating is based on the average S&P rating for the corresponding bonds as obtained from MBSD. To use a clean period before the event, we focus on ratings during 12 to 24 months prior to the deal month. Here, our subset of general obligation bonds is obtained by specifically using"Unlimited Tax GO" bonds. All others are classified as revenue bonds. We separately obtain county level rating corresponding to all bonds, general obligation bonds only and revenue bonds bonds only respectively.

different states in the US have different state level tax rates, it creates a distortion in the incentives to buy/hold municipal bonds. States with lower tax privileges are more likely to have a smaller investor base.

In Table 5, we document the variation among different terciles of tax privilege among the winning counties. We use the baseline specification from Equation 1, after controlling for average group-month effects. Column (1) shows the results for the full sample where we find a 21.61 bps increase for the lowest tercile, alongside a contrasting 19.49 bps decline in yields for the highest group. Column (2) shows a similar effect for tax-exempt bonds. Consistent with Babina, Jotikasthira, Lundblad, and Ramadorai (2019), we further control our baseline specification with the lagged values of debt-to-income at the county level. We obtain the county level total personal income from the Internal Revenue Service data. We show our results in Column (3) with magnitudes being similar to the previous two columns. Finally, we use a measure of tax privilege gap as the difference in tax privilege between the winning and losing counties. Following a similar scheme, we show the results in Columns (4) - (6). Once again, the low tax incentive winners experience a much larger increase in borrowing costs than their medium-placed counterparts. For winners in the high tax privilege bracket, there is a reduction in yields by 8-19 bps. The evidence supports that a low debt capacity measured by a low tax privilege results in a greater increase in bond yields.

To summarize, our results in this subsection provide evidence in favor of the ex-ante debt capacity of winners as the underlying channel. We show that winning counties which face a higher interest burden are associated with up to 21 bps as additional borrowing cost. Similarly, low rated counties also pay more for their debt after the deal. Finally, a lower tax privilege reflects lower debt capacity for a county resulting in up to 27 bps increase in bond yields.

4.3.2 Expected Multiplier Effects

In this sub-section we evaluate the heterogeneity in 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 expected multiplier effects using two proxies: a) knowledge spillover using firm patents, and b) national industry-specific jobs multiplier.

First, to measure knowledge spillover, we follow Kogan, Papanikolaou, Seru, and Stoffman (2017) to quantify the economic importance of each patent of the firm receiving the subsidy. Specifically, we use the dollar value of innovation associated with the firm receiving the subsidy. We aggregate this value over patents granted three (and five) years before the deal[13]. We create three equal terciles among winners in the matched deals. Using our baseline Equation 1 interacted with the corresponding group of the tercile, we get the results as depicted in Figure 3a. We also control for group-month fixed effects. By aggregating up to three years before the deal, we find an increase in yields of 16.83 bps among deals involving firms with the lowest tercile of patent value. On the other hand, the borrowing cost seems to go down in deals involving firms with the highest tercile of patent value. The difference between these two extremes of 19.31 bps is economically and statistically significant. Further, we find a similar impact when we use a longer history of the firms and aggregate the value of patents up to five years before the deal. The lowest group shows a statistically significant increase in yields of 14.71 bps, while the yields drop significantly by 5.06 bps among the high patent values. Once again, the difference of 19.77 bps is statistically and economically significant. Taken together, the evidence suggests that the borrowing cost decreases for deals involving high innovation/patent value (see Moretti (2012)).

The economic benefit reported in the press releases of many subsidy deals includes the prospect of many indirect jobs getting created. Proponents of corporate subsidy often refer to this multiplicative effect on jobs created in the value chain of a given industry (based on upstream or downstream linkages). As a result, we consider the variation across deals based on the jobs multiplier associated with a given industry. For this, we hand match the industry (and/or project description) of the 127 deals in our sample to employment multipliers by industry provided by the Economic Policy Institute[14]. We divide the winners of the subsidy deals into terciles based on the employment multipliers associated with the industry of the firm. Then, we use our baseline Equation 1 interacted with the corresponding group. We also control for group-month fixed effects. Figure 3b shows the differential effect across the three groups. Among deals in the lowest tercile, the impact is highest: borrowing cost goes up by 7.01 bps. The effect diminishes to 3.12 bps and 1.71 bps in the medium and high jobs multiplier groups. The difference between the impact on the lowest and the highest terciles is 5.30 bps. This statistically significant result points toward the economic interpretation of smaller increase in the borrowing cost after the subsidy deal if the expected employment multiplier for the deal is higher. Further, we find variation at the industry level of the deal, as reported in Figure 3c. We

[13]We are able to match 60 deal-pairs in our subsidy database to the patents granted until 2010. For deals which 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.

[14]`https://www.epi.org/publication/updated-employment-multipliers-for-the-u-s-economy/`

find an increase in bond yields for data centers (13.76 bps), warehouses (7.05 bps), and mining and utilities (8.16 bps). On the other hand, the impact on yields for plants in the manufacturing sector is -2.71 bps and non-tradeables sector is -8.72 bps.

Overall, we highlight the variation in expected benefits from innovation spillovers and employment multipliers. We show that subsidy deals that attract or retain firms with higher valued innovation or attract or retain firms in industries with a higher job multiplier effects show a decrease in borrowing cost for the winning counties. These results are consistent with municipal bond investors incorporating the expected benefits of the deal in their valuation.

4.3.3 Interaction of County Debt Capacity with Expected Multiplier Effects

In Section 4.3.1, we find evidence for a higher increase in borrowing cost for winning counties with lower debt capacity. Thereafter, in Section 4.3.2, we show that a higher multiplier effect (from innovation or jobs) results in a decline in borrowing cost after the subsidy deal. Finally, in this section we interact the debt capacity with the expected multiplier effects.

In order to further understand how the main result is driven by the relative strength of debt capacity versus the expected multiplier effects, we interact these two factors. We first categorize the deals into high and low groups based on the corresponding values of debt capacity measures (interest burden, interest to debt and interest to expenses) defined in Section 4.3.1. Then, we further divide each group into terciles based on the multiplier effects using the economic value of patents (aggregated over three years before the deal) and employment multipliers (See Section 4.3.2). We thus create a two-step sorting among the subsidy deals to uncover the nuanced variation in debt capacity as discussed below. After grouping the deal-pairs into six categories, we merge the corresponding bond-month transactions as before. We use the following basic equation in this sub-section:

$$\begin{aligned} y_{i,c,d,t} = \alpha &+ \beta_0 * Winner_{i,c,d} * Post_{i,c,t} * High_{d,t} + \beta_1 * Winner_{i,c,d} * Post_{i,c,t} \\ &+ \beta_2 * Post_{i,c,t} * High_{d,t} + \beta_3 * Winner_{i,c,d} * High_{d,t} + \beta_4 * Winner_{i,c,d} \\ &+ \beta_5 * Post_{i,c,t} + BondControls + CountyControls + \eta_d + \mu_{dt} + \epsilon_{i,c,d,t} \end{aligned} \tag{5}$$

where, *High* denotes a dummy one for deals with high interest burden or high level of interest to debt or high interest to expenses (all representing lower debt capacity). This dummy takes zero otherwise. μ_{dt} denotes a fixed effect for the group-month year to absorb the average effect for bond yields in the *High* group for a given month. All other variables follow the same meaning as in Equation 1. As a final step, we interact the equation above with corresponding dummies at the deal level pertaining to terciles in value of patents and employment multipliers. We suitably modify μ_{dt} to control for the average effect of the tercile sub-group in a given month.

In Figure 4a, we show how debt capacity interacts with knowledge spillover. For example, using interest burden, we find that the differential effect of high interest burden is muted due to a high value of patents (-0.04 bps). We find similar results using other measures of debt capacity, like interest to debt and interest to expenses. Next, we interact the debt capacity with jobs multiplier. We find similar results. We show that the marginal impact of high interest burden is significantly reduced for deals with high jobs multiplier (-7.87 bps). Our results show similar effect with other measures using interest to debt and interest to expenses.

Taken together, our results in Section 4.3 suggest that lower debt capacity increases the impact on borrowing cost. Whereas, expected multiplier attenuates this impact. Further, the interaction of debt capacity with expected multiplier suggests that the potential gains from knowledge spillover and expected jobs may help mitigate the negative effects of debt capacity.

4.4 Bargaining Power: County versus Firm

So far, we have considered the impact of the debt capacity of the county and potential multiplier effects of the deal on the municipal bond yields. But, the amount of subsidy given to attract or retain firms to the county relative to the projected benefits is likely to be a major factor in the price reaction to the deal. The offered subsidy is in turn likely to depend on the relative bargaining power between the parties involved in the subsidy deal. We argue that while firms may hire site consultants to conduct their search through a bidding mechanism[15], 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) ratio of firm's assets to county's revenue, b) ratio of subsidy to county's budget surplus, c) intensity of bidding competition, and d) county's unemployment rate.

First, we divide the winning counties based on the ratio of firm assets to county-level revenue. We hypothesize that if the firm (size represented by assets) is larger than the county (size represented by revenue), then the county's relative bargaining power is likely to be lower. Therefore, the impact on borrowing cost would be higher. In Column (1) of

[15] For instance, The Wall Street Journal reported on a cadre of consultants which 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

Table 6, we report our results for the baseline Equation 1 interacted with terciles of this measure. We also control for group-year month fixed effects. We find that yields go up by 14.52 bps for deals with high firm assets to county revenue ratio. On the other hand, the yields actually decrease by 4.5 bps when this ratio is low. The difference between the two sub-groups is significant. We find similar impact by using our second measure of subsidy to county budget surplus ratio. As shown in Column (2), the cost increases by 19.37 bps for the high group, but decreases by 11.53 bps when the corresponding ratio is low.

Since the competition for these firm investments is often supported by the state level governments, we construct our next measure at the state level. We calculate the ratio of state level budget surplus to revenue and use the gap between the winning and losing state as a measure of intensity of competition. As the gap between states increases, the competition is likely to be lower and the county's bargaining power is likely to be higher. In Column (3), we show our results based on the interaction with intensity of competition. We find that the secondary market yields go up by 20.86 bps when the intensity of competition is high (surplus to revenue gap is low). Whereas, the yields decrease by 5.73 bps when the intensity of competition is low. Finally, we show our results based on county level unemployment rate in Column (4). For counties with high unemployment rate, we expect them to have lower bargaining power in the bidding process causing a greater impact on yields (22.32 bps). Whereas, for counties with low unemployment rate, the yields actually go down by about 4 bps. The resultant differential effect is 26.42 bps.

Overall, we argue that the four measures of bargaining power highlight the differential impact of relative bargaining power on the amount of subsidy offered and through it, the market reaction to the deal. A lower bargaining power causes a greater increase in yields (between 14-22 bps). Our results also bring out that the municipal borrowing cost responds favorably (reduction of 4-11 bps) to subsidy announcements when the bargaining power of the local government is high.

4.5 Impact on New Issuance of Municipal Bonds

The results in the previous sections suggest that the borrowing cost increases for the winning counties in the secondary bonds market. This is especially so in counties with low debt capacity, with low bargaining power and in deals involving low expected multiplier effect. In this Section, we test the implications of higher secondary market yields on new issuance.

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 could especially be the case when the winners need to create the infrastructure needed to support the large plant. Instead of diverting cash from regular sources of revenue (which may already be earmarked for dedicated uses), borrowing in the public market could be a feasible option. In this light, we present our results in Figure 5a 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 in the six month rolling window during T=-6 to T=-12 months before the corresponding deal event. We normalize this value to one and compute total par value of new issuances 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 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. Specifically, the difference shows up after 12, 18 and 30 months after the deal. Further, to understand how debt capacity may influence new issuance, we split the winners based on this metric. In Figure 5b, we find evidence consistent with our proposed mechanism. Counties with low interest burden (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.

As a final step in our analysis in the primary market of municipal bonds, we evaluate the impact of the event on the primary market using new bonds issuance data. The average new issuance in the sample is $43 million for the winners. Therefore, it becomes important to understand the borrowing cost implications in raising the additonal funds. To this end, we use a similar estimate as the difference-in-differences Equation 1 of the secondary market. However, we only include deal fixed effects and issuer fixed effects. We also control for bond ratings at the time of issuance. We cluster standard errors at the issue-dated month level. Table 7 shows our results from the primary market yields. In Column (1), we estimate the difference-in-differences coefficient from within the same deal-pair, absorbing for the issuer fixed effect. We report an increase of 11.61 bps. We show our results in Column (5) after controlling for bond characteristics, local economic conditions and ratings where the main estimate is 4.71 bps.

Overall, our results suggest that the primary market bond issuance is associated with an increase in yields by about 4.5 bps after the subsidy announcement. However, one caveat to these results is that counties and local governments may rationally expect a higher borrowing cost following the deal announcement and may try to time the market and raise funds well before or after the event. Thats one of the reasons where we can focus on secondary market trades of exisitng bonds in our baseline analysis to evaluate the impact on borrowing cost of the corporate subsidy deal.

4.6 Impact on Property Taxes and Public Expenditure

So far we find evidence suggesting an increase in borrowing cost of local governments after the deal both in the secondary and primary 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 burden i.e. high debt capacity. Keeping these results in mind, we now investigate the impact on public expenditure and local property taxes.

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 or 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 4, but additionally introduce county fixed effects. Here, we cluster standard errors at the deal level.

In Figure 6a, we find evidence wherein counties which could not increase debt show an increase in property tax revenue. For winning counties with high interest burden (low debt capacity), the property tax revenue per capita goes up 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 6b. We obtain house price index from Federal Housing Finance Agency (FHFA). Taken together, these results seem to suggest that these local governments may be increasing tax rates or reassessing property values to realize this higher property tax revenue. In the Internet Appendix, we show similar results by using other measures of interest expenditure, namely: interest to debt (Figure IA8) and interest to expense (Figure IA9).

Finally, in Table IA10 we show the impact on county level expenditures around the corporate subsidy deal. 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 population of the corresponding year to get the per capita impact. 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 interest burden. We do not find any significant change for any of the expenditures. However, Column (7) does show a significant decline in health expenditure per capita of USD 91.11 for counties with high interest burden.

Overall, we find evidence suggesting that there is an increase in borrowing cost for the the winning county after the subsidy announcement. This is driven by counties with low debt capacity, low bargaining power and low expected multiplier effects. As a result, only counties with high debt capacity are able to issue new debt after the deal. Whereas, counties with low debt capacity seem to rely on property tax revenue to finance the increased demand for public services. We do not find any significant changes in expenditure on local public services.

5 Conclusion

Corporate subsidies have recently attracted much attention[16] in the United States even though this practice can be traced back to 1791 (Buss, 2001). Some policy makers favoring corporate incentives highlight the importance of creating more jobs. Whereas, 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 net benefit from the expected multiplier effect may attenuate the effect of debt capacity.

Using detailed hand-collected data on corporate welfare 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 an additional 7.5% of the total subsidy offered. We propose a mechanism based on the ex-ante debt capacity of the winning counties and its interaction with expected jobs multiplier. We find a greater increase in yields after the deal for counties with lower debt capacity and lower bargaining power. Interestingly, the yields decrease for counties with high multiplier effect, measured using ex-ante firm value of patents and jobs multiplier. Further, the debt capacity effect

[16] https://www.nytimes.com/2018/04/24/opinion/amazon-hq2-incentives-taxes.html

is attenuated by the multiplier effect.

As a result of our main effect, we document evidence supporting greater issuance by counties which have high debt capacity. Whereas, counties with low debt capacity observe an increase in property tax revenue without similar change in house price index. This seems to suggest that these counties likely finance the additional civic burden after the deal by raising property tax rates. To our knowledge, our paper is the first to document the impact of corporate subsidies on the borrowing cost of local governments. Our results suggest 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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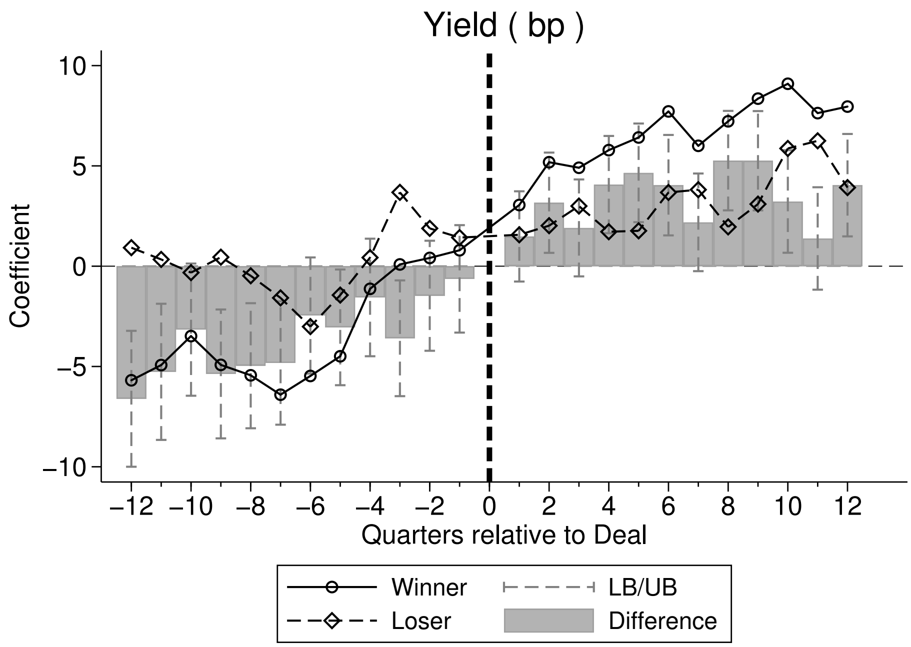
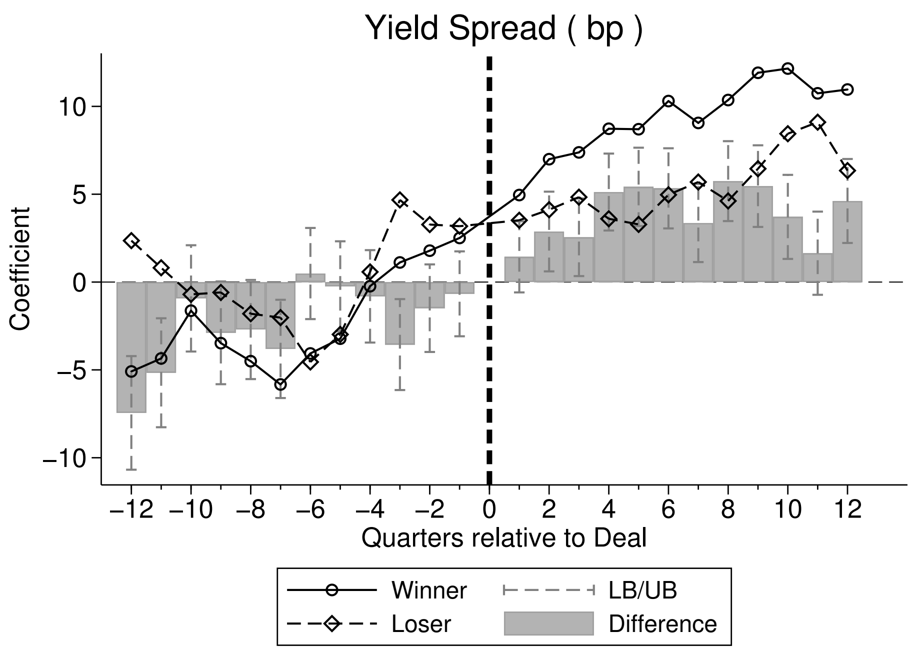
Panel A: Before Tax



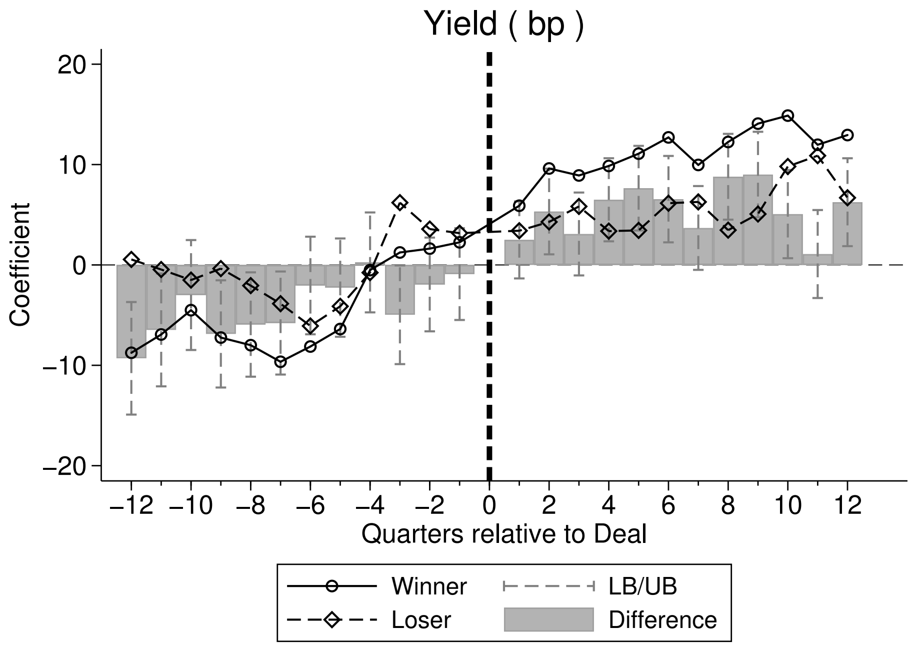
(a)



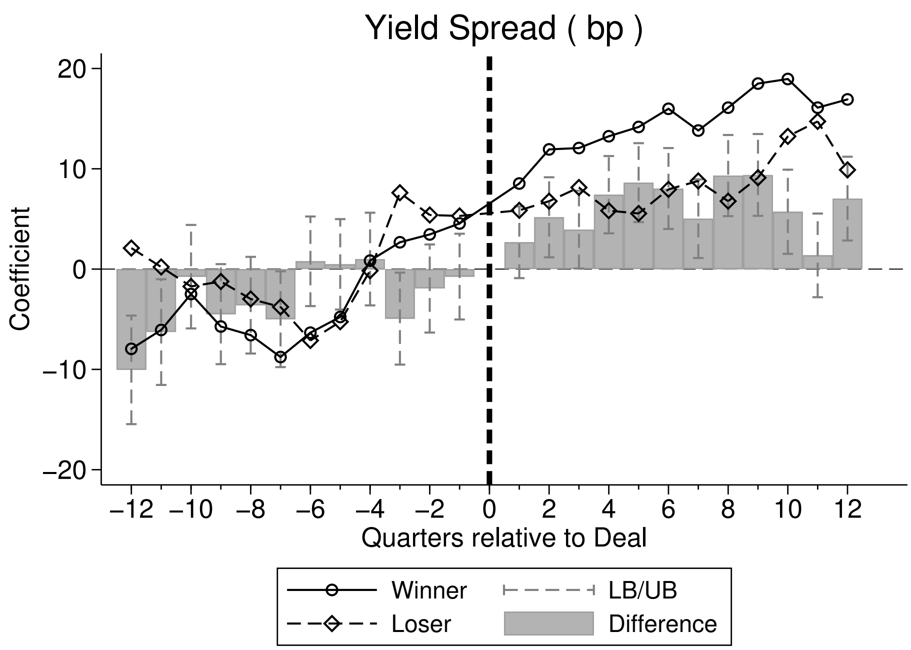
(b)

Figure 1: Baseline Result - Winner vs Loser: In this figure, we plot the yields and yield spreads (before and after tax) for municipal bonds traded before and after the subsidy announcement. Panel A plots the before-tax yield and yield spread. Panel B shows the after-tax yield and spread. Refer Table A1 for variables description. The coefficients are shown in basis points. We regress the yield and spreads on monthly interaction dummies for winner and loser using fixed effects for deal and year month as in Equation 4. The benchmark period is the quarter preceding the window (-12,12). Standard errors are clustered by issue-year month. The dashed line represents 95% confidence intervals.

Panel B: After Tax



(c)



(d)

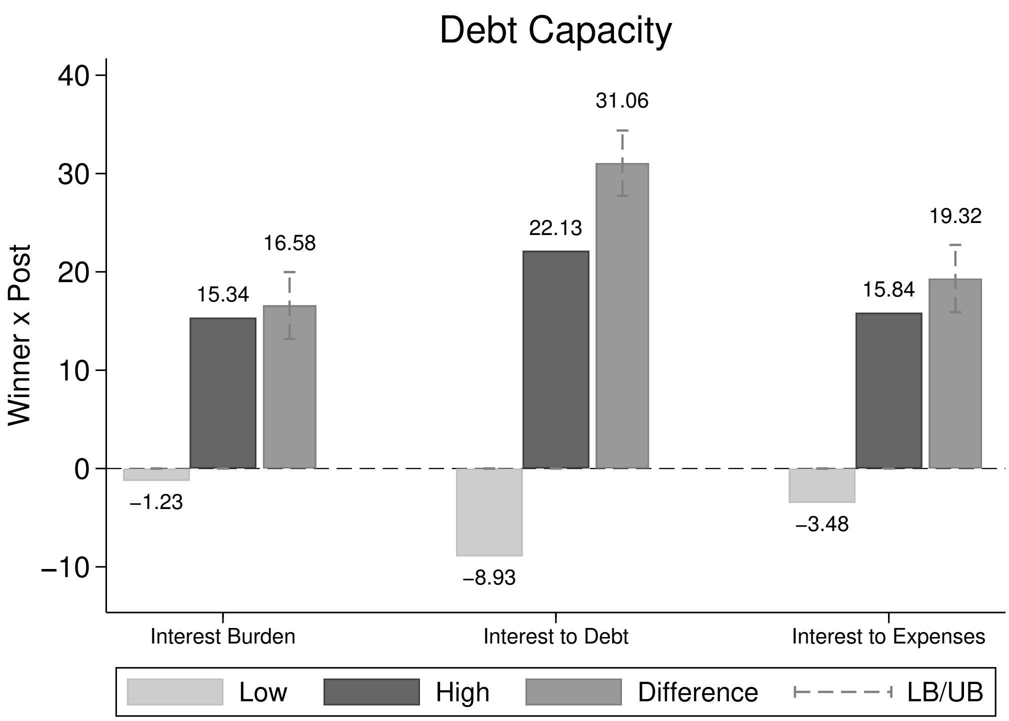
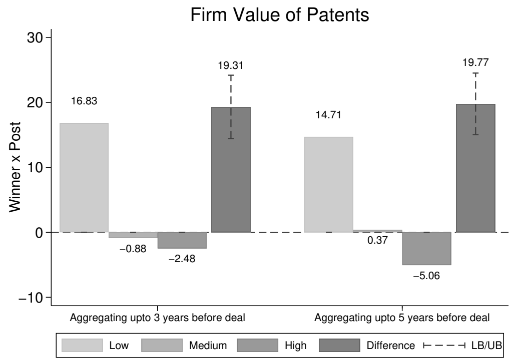
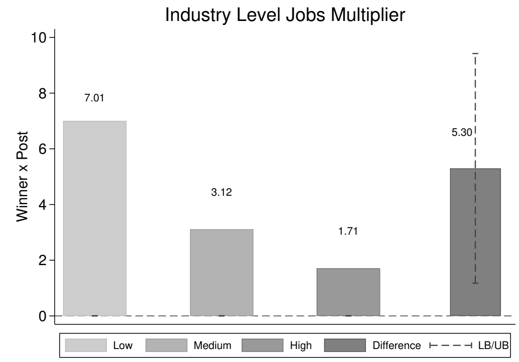


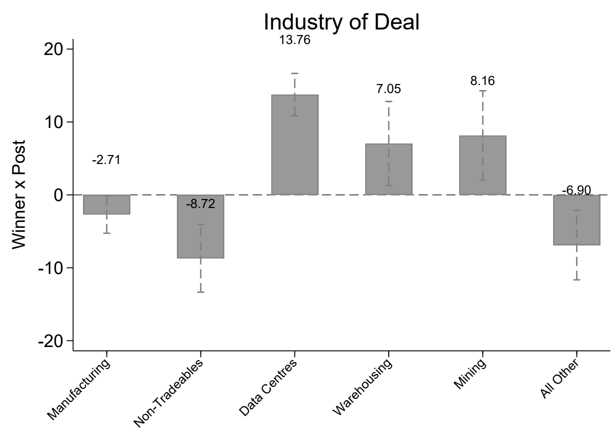
Figure 2: County Debt Capacity - Interest Expenditure: The figure shows results for our main interaction term, β_0, from Equation 1. We modify the baseline equation to interact with dummies for high and low values of ex-ante (one year before the deal) county level measures of debt capacity using interest expenditure, namely: a) interest burden (ratio of interest on general debt to total revenue), b) interest to debt (ratio of interest on general debt to total long term debt outstanding) and c) interest to expenses (ratio of interest on general debt to total expenditure). We additionally control for group-month fixed effects in the regression. The corresponding bars are indicated in the legend. Standard errors are clustered by issue-year month. The dashed line represents 95% confidence intervals.



(a)

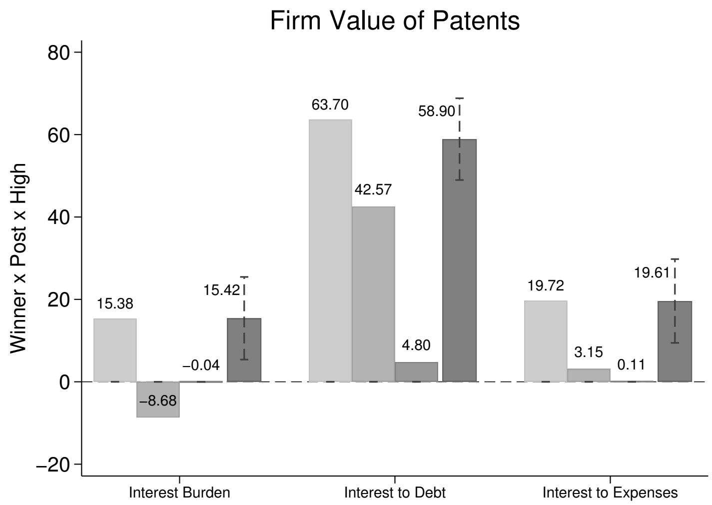


(b)

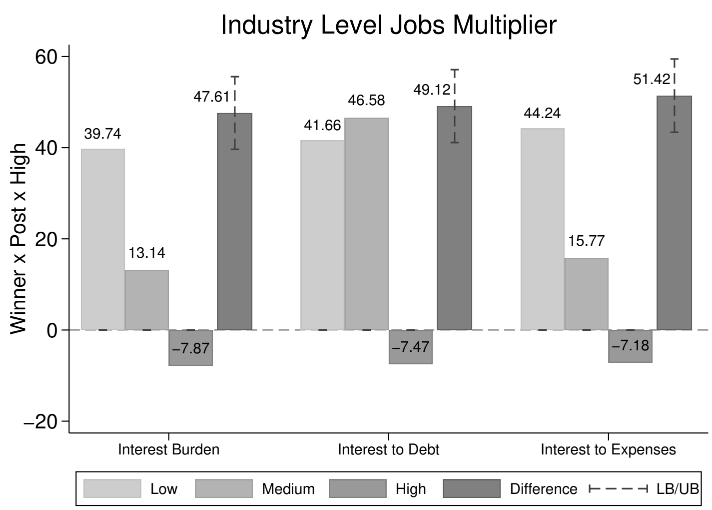


(c)

Figure 3: Expected Multiplier Effect: 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 firm value of patents (sub-figure(a)), industry level jobs multiplier (sub-figure(b)) and the industry of the deal (sub-figure(c)). We additionally control for group-month fixed effects in the regression. In sub-figure (a), we show results by aggregating the value of patents at the firm level 3 years and 5 years before the deal, respectively. Jobs multipliers in sub-figure (b) are obtained from Economic Policy Institute. Industry groups in sub-figure (c) of the recipient firms are based on NAICS classification, which are recombined further. Specifically, non-tradeables include wholesale trade and retail trade. Data centers include professional and scientific, finance and insurance, information, and management and administrative sectors. Mining is a combination of mining and energy, utilities, and construction. Warehousing includes transportation and warehousing, and real estate. Standard errors are clustered by issue-year month. The dashed line represents 95% confidence intervals.

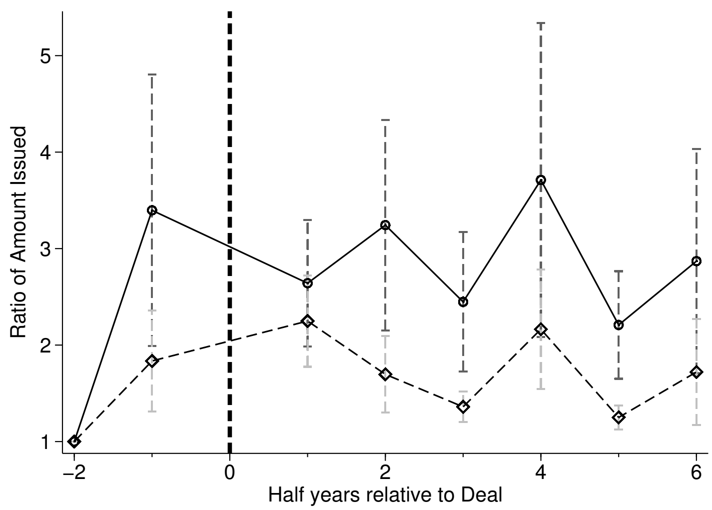


(a)



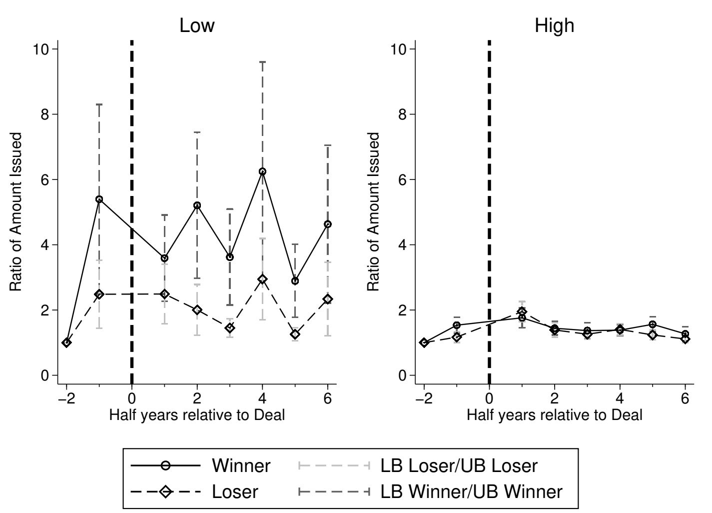
(b)

Figure 4: Interaction of Debt Capacity with Multiplier Effect: The figure shows results for the triple interaction term, β_0, from Equation 5, interacted with the corresponding tercile of multiplier effect. Specifically, we modify the equation to interact with dummies corresponding to the sub-group of firm value of patents (sub-figure(a)) and industry level jobs multiplier (sub-figure(b)) at the deal level. We additionally control for group-month fixed effects in the regression. In each figure, the triple interaction coefficient on the y-axis is shown with respect to the corresponding debt capacity measure. As before, we use interest burden, interest to debt and interest to expenses. A higher value of interest represents lower debt capacity. Standard errors are clustered by issue-year month. The dashed line represents 95% confidence intervals.



(a)

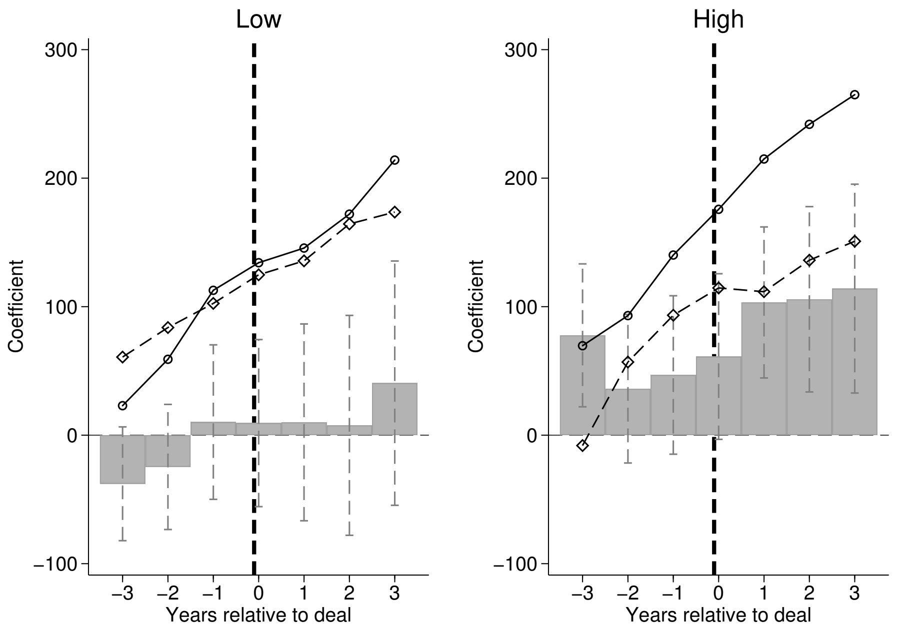
By Interest Burden:



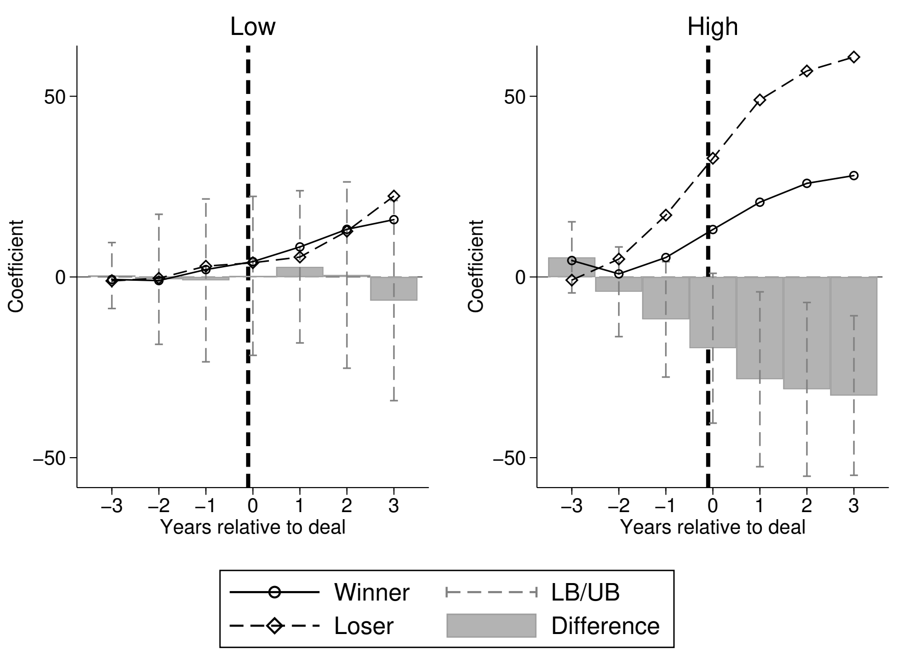
(b)

Figure 5: 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 in the six month rolling window during T=-6 to T=-12 months before the corresponding deal event. We normalize this value to one and compute 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 losers' issuance. In sub-figure (a), we show the total issuance and in sub-figure (b), we split the sample by the interest burden (defined as the ratio of interest on general debt to total revenue of the county). The vertical bars show the upper and lower limits based on the standard errors of the mean values.

By Interest Burden:



(a) Property Tax Revenue per capita



(b) FHFA House Price Index

Figure 6: Impact on Local Property Taxes: The figure represents the relative changes in property taxes among winners split by the interest burden, compared to the corresponding losers. In sub-figure (a), we show the property tax revenue and in sub-figure (b), we represent the house price index obtained from Federal Housing Finance Agency (FHFA). we use the annualized version of Equation 4, but additionally introduce county and event-year fixed effects. Here, we cluster standard errors at the deal level. The benchmark period is the year before the window (-3,3). The dashed line represents 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. Data centers include professional and scientific, finance and insurance, information, and management and administrative sectors. Non-tradeables include wholesale trade and retail trade. Mining is a combination of mining and energy, utilities, and construction. Warehousing includes transportation and warehousing, and real estate.

Panel A: All Deals

	Count	Mean	Median	Std. Dev.
Subsidy ($ million)	127	300.2	123.0	552.6
Investment ($ million)	114	1,151.1	560.3	1,857.2
Subsidy/Investment(%)	114	73.5	37.8	154.5
Jobs promised	121	1,761.9	951.0	2,886.3
Subsidy ($) per job	121	468,341.9	158,500.0	1,132,961.4
Observations	127			

Panel B: By Purpose of Subsidy

	Subsidy ($ million)			
	Count	Mean	Median	Std. Dev.
Relocation	25	98.8	82.0	51.8
New/Expansion	84	392.0	163.0	659.9
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
Warehousing	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: 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 winners versus losers after the subsidy announcement. The primary coefficient of interest, β_0, is captured by the interaction term of Winner x Post. Panel A compares winners and losers in the secondary market around an equal window of 3 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 deal-pair 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, additional credit enhancement, 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. Columns (4)-(6) show the results using yield spread as the dependent variable. In Columns (7)-(9), the dependent variable is after-tax yield spread which is calculated using Equation 2 and 3. Our baseline specification comes from Column (9) in Panel A. In Panel B, we report the baseline specification with incremental effect, holding the pre-event window constant at 36 months before the subsidy announcement. Panel C shows robustness to the baseline specification to using different choices of windows for our preferred specification. P-values are reported in brackets and standard errors are clustered at issue-year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Panel A: Three-year Window

Dependent Variable:	Average Yield			Yield Spread			After-tax yield spread		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner x Post	6.80***	6.38***	6.60***	6.22***	6.07***	6.28***	8.94***	8.36***	8.36***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Winner	-3.88***	-3.11***	-4.63***	-2.72***	-3.07***	-4.58***	-3.50***	-3.35***	-5.80***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Post ($t \geq 0$)	-1.22**	-0.70	0.20	-0.16	0.17	0.98**	0.07	0.90	2.38***
	[0.03]	[0.12]	[0.66]	[0.76]	[0.71]	[0.04]	[0.93]	[0.24]	[0.00]
Deal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓		✓	✓
County Controls			✓			✓			✓
Adj.-R^2	0.301	0.559	0.565	0.627	0.687	0.689	0.373	0.531	0.536
Obs.	2,612,055	2,471,373	2,442,115	2,612,055	2,471,373	2,442,115	2,610,761	2,470,129	2,440,871

Panel B: Different Forward Windows (in months)

Dependent Variable:	After-tax yield spread							
Window (months):	[-36,+6]	[-36,+12]	[-36,+18]	[-36,+24]	[-36,+30]	[-36,+36]	[-36,+48]	[-36,+60]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner x Post	5.43***	5.82***	6.10***	7.32***	8.07***	8.36***	10.79***	12.00***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Winner	-0.64	-1.06	-2.33***	-4.08***	-5.29***	-5.80***	-6.46***	-7.46***
	[0.44]	[0.19]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Post ($t \geq 0$)	2.30**	1.86**	1.44	1.67*	1.88**	2.38***	-0.68	-2.96***
	[0.03]	[0.04]	[0.10]	[0.05]	[0.02]	[0.00]	[0.34]	[0.00]
Deal FE	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.547	0.547	0.544	0.540	0.539	0.536	0.543	0.540
Obs.	1,314,869	1,539,676	1,768,570	1,996,344	2,218,969	2,440,871	2,842,002	3,201,652

Panel C: Different Windows (in months)

Dependent Variable:	After-tax yield spread				
Window (months):	[-12,+12] (1)	[-24,+24] (2)	[-36,+36] (3)	[-48,+48] (4)	[-60,+60] (5)
Winner x Post	0.68	5.02***	8.36***	12.01***	12.26***
	[0.60]	[0.00]	[0.00]	[0.00]	[0.00]
Winner	1.84	-3.31***	-5.80***	-5.77***	-4.85***
	[0.11]	[0.00]	[0.00]	[0.00]	[0.00]
Post ($t \geq 0$)	1.19	-1.36	2.38***	1.78***	-0.01
	[0.38]	[0.17]	[0.00]	[0.01]	[0.98]
Deal FE	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓
Adj.-R^2	0.548	0.530	0.536	0.546	0.547
Obs.	890,384	1,689,777	2,440,871	3,125,405	3,734,706

Table 3: Robustness Tests

In this table we report results for various robustness test on our baseline specification, i.e., Column (9) of Table 2. In Columns (1)-(3), we report results using only customer-buy trades with transaction size $\leq$ \$25,000, $\leq$ \$50,000 and $\leq$ \$100,000, respectively. Columns (4)-(7) report regression results where we drop bonds that are dated within 6 months, 12 months, 24 months and 36 months, respectively. In Column (8), we use transactions after the financial crisis of 2009. Likewise, in Column (9), we show results with deals after 2009. Column (10) shows the results with customer sell trades only. In Column (11), we document the impact on state-level bonds for the bidding counties. We replace county-level controls with lagged values of state revenue and state budget surplus. Columns (12)-(15) report results with alternative choices of clustering the standard errors, namely: issuer-year month, issue-event month, state-year month and deal-year month. In Column (16), we show the results for bonds with non-missing S&P credit ratings. We use the most recent ratings for a given CUSIP. In Column (17), we introduce additional county level time-varying covariates using lagged values of log(personal income) and log(house price index). In Columns (18)-(20), we control for duration. First, in Column (18) we use duration in the controls by replacing years to maturity and inverse of years to maturity. Second, in Column (19), we use tax-adjusted duration to replace years to maturity and inverse of years to maturity. Finally, Column (20) shows the result by using after-tax yield spread based on treasury yields matched to duration (instead of remaining years to maturity) while simultaneously controlling for duration instead of years to maturity and inverse of years to maturity. Columns (21)-(25) report results controlling for unobserved factors at the county and state level. Specifically, in Column (21), we introduce county fixed effect to the baseline. Column (22) shows results with county-year fixed effects added to the baseline. In Column (23), we drop counties that may have overlapping bonds across deal events. Any county which features in two or more deals as a bidder (winner or loser) over less than 6 years is dropped. Column (24) shows the results with state fixed effects added to the baseline. In Column (25), we introduce state-year fixed effects. Columns (26)-(30) show the results with alternative specifications. Specifically, Column (26) shows the results with bond fixed effects. We also require that the bond should have at least one transaction before and after the deal announcement. The bond level time-invariant controls get dropped. We also have to omit the years to maturity and inverse years to maturity for consistent estimation of the variance co-variance matrix given our set of fixed effects. Column (27) we introduce event-month fixed effects to the baseline. In Column (28), we drop the year month fixed effects and replace it with the event-month fixed effects in the baseline. For Column (29), we change the clustering of Column (28) from the baseline definition (of issue-year month) to issue-event month. In Column (30), we show the results by replacing year-month fixed effects in the baseline with year fixed effects. P-values are reported in brackets and standard errors are clustered at issue-year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax yield spread										
	Transaction Size ($)			Drop Bonds Dated Within				Financial Crisis		Other Trades	
	≤25,000 (1)	≤50,000 (2)	≤100,000 (3)	6 months (4)	12 months (5)	24 months (6)	36 months (7)	Use Trades>2009 (8)	Use Deals>2009 (9)	Sell Trades (10)	State-level Bonds (11)
Winner x Post	10.94***	10.19***	9.82***	7.46***	6.67***	5.93***	5.01***	5.83***	8.03***	5.90***	1.39***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Adj.-R^2	0.561	0.559	0.557	0.528	0.520	0.501	0.480	0.461	0.479	0.511	0.579
Obs.	1,671,466	2,006,739	2,176,372	2,286,940	2,148,443	1,860,059	1,575,190	1,690,678	1,687,088	2,173,656	5,004,498

	Clustering				Additional Controls		Controlling for Duration		
	Issuer-YM (12)	Issue-Event Month (13)	State-YM (14)	Deal-YM (15)	Add Ratings (16)	More County Controls (17)	Use Duration (18)	Use after-tax duration (19)	Duration adjusted spread (20)
Winner x Post	8.36***	8.36***	8.36***	8.36***	11.87***	6.67***	10.21***	10.57***	10.19***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Adj.-R^2	0.536	0.536	0.536	0.536	0.589	0.544	0.526	0.499	0.758
Obs.	2,440,871	2,440,871	2,440,871	2,440,871	1,878,730	2,190,155	2,339,993	2,339,993	2,339,993

	Unobservables: County and State					Other Specifications				
	Use County FE (21)	Use County-Year FE (22)	Drop Counties with Overlap (23)	Use State FE (24)	Use State-Year FE (25)	Use Bond FE (26)	Add Event-Month FE (27)	Replace YM FE to event-month FE (28)	Cluster by Issue-event month (29)	Replace month FE with year FE (30)
Winner x Post	10.54***	2.87***	13.42***	11.48***	3.16***	3.95***	8.35***	9.31***	9.31***	11.25***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Adj.-R^2	0.545	0.554	0.584	0.542	0.551	0.827	0.536	0.353	0.353	0.451
Obs.	2,440,871	2,440,860	936,385	2,440,871	2,440,871	1,912,603	2,440,871	2,440,871	2,440,871	2,440,871

Table 4: County Debt Capacity: Evidence based on Credit Ratings

This table shows the evidence based on ex-ante county level ratings among winning counties, using the baseline Equation 1. To control for the average impact within a particular group for that month we add group-month fixed effects. We then interact the main equation with dummies corresponding to the average S&P rating group of the county during -12 to -24 months before the deal. The rating group belonging to *AA- and above* corresponds to higher credit rating quality, while *A+ and below* represent lower credit quality. First, in Columns (1), we show the impact on the full sample of bonds for which county level ratings are available in the benchmark period (-12 to -24 months) using all bonds. Second, in Column (2), we replicate this approach using a sub-sample of general obligation (GO) bonds only. Finally, Column (3) shows the impact on revenue (RV) bonds alone. P-values are reported in brackets and standard errors are clustered at issue-year month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread		
	All Bonds	GO Bonds	RV Bonds
Winner x Post	(1)	(2)	(3)
AA- and above	5.89***	-6.74***	5.35**
	[0.00]	[0.00]	[0.01]
A+ and below	8.45***	16.69***	7.31***
	[0.00]	[0.00]	[0.00]
Difference	2.552	23.43	1.955
p-val	0.18	0.00	0.45
Deal FE	✓	✓	✓
Month-Year FE	✓	✓	✓
County Controls	✓	✓	✓
Group-Month FE	✓	✓	✓
Adj.-R^2	0.524	0.544	0.526
Obs.	2,140,846	702,235	1,410,426

Table 5: County Debt Capacity: Evidence based on Tax Privilege

This table shows the heterogeneity in tax privileges among winning counties from different states, using the baseline Equation 1. To control for the average impact within a particular group for that month we add group-month fixed effects. We then interact the main equation with dummies corresponding to the economic variables in each column, as described hereafter. Following Babina, Jotikasthira, Lundblad, and Ramadorai (2019), tax privilege measures how income from in-state municipal bonds are tax exempt for state residents. Tax privilege is defined as the highest state income tax rate applied to income from municipal bonds issued by other states minus the highest state income tax rate applied to income from the winning state-issued municipal bonds. A *high* tax privilege means greater benefit. First, in Columns (1)-(3), we use the difference in highest individual income tax rate for all other states and the candidate state. Second, in Columns (4)-(6), we replicate this measure as a difference in the tax privilege between winning and losing states. Specifically, Columns (1) and (4) show the results for all bonds in our sample. In Columns (2) and (5), we focus on tax-exempt bonds only. Finally, Columns (3) and (6), we additionally control for county-level total debt to total income. P-values are reported in brackets and standard errors are clustered at issue-year month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread					
	Tax Privilege			Tax Privilege Gap		
	All bonds	Tax-exempt Bonds	Add Debt to Income	All bonds	Tax-exempt Bonds	Add Debt to Income
Winner x Post	(1)	(2)	(3)	(4)	(5)	(6)
Low	21.61***	21.46***	26.18***	20.30***	26.05***	27.55***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Medium	4.89***	15.06***	18.02***	7.36***	4.53***	9.65***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
High	-19.49***	-19.12***	-21.08***	-17.79***	-11.53***	-8.89***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Low vs High	41.10	40.59	47.26	38.09	37.57	36.44
P-value	0.00	0.00	0.00	0.00	0.00	0.00
Deal FE	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓
Group-Month FE	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.539	0.550	0.540	0.540	0.550	0.540
Obs.	2,440,871	2,242,597	2,102,452	2,440,871	2,242,597	2,102,452

Table 6: Bargaining Power of Winning Counties

This table shows the heterogeneity in bargaining power across counties and states, using the baseline Equation 1. Group-month fixed effects are added in addition, to control for the average impact within a particular group for that month. We then interact the main equation with dummies corresponding to the economic variables in each column, as described hereafter. Column (1) shows $\frac{FirmAssets}{CountyRevenue}$ denoting the ratio of the lagged value of firm assets to lagged county revenue of the winner. A high value of the ratio indicates low bargaining power of the county. In Column (2), we use the ratio of subsidy to lagged value of county-level budget surplus to create the three groups. Counties which had to pay a large amount of subsidy in comparison to their budget surplus 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 revenue coverage ratios as 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. P-values are reported in brackets and standard errors are clustered at issue-year month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	After-tax Yield Spread			
	$\frac{\text{Firm Assets}}{\text{County Revenue}}$	$\frac{\text{Subsidy}}{\text{County Surplus}}$	Intensity of Competition	Unemployment Rate
Winner x Post	(1)	(2)	(3)	(4)
Low	-4.50***	-11.53***	-5.73***	-4.10***
	[0.00]	[0.00]	[0.00]	[0.00]
Medium	5.73***	16.15***	10.76***	-1.07
	[0.00]	[0.00]	[0.00]	[0.51]
High	14.52***	19.37***	20.86***	22.32***
	[0.00]	[0.00]	[0.00]	[0.00]
High vs Low	19.02	30.91	26.59	26.42
P-value	0.00	0.00	0.00	0.00
Deal FE	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓
County Controls	✓	✓	✓	✓
Group-Month FE	✓	✓	✓	✓
Adj.-R^2	0.543	0.537	0.538	0.537
Obs.	2,030,786	2,427,378	2,438,182	2,438,182

Table 7: Impact on Primary Market of Municipal Bonds

This table shows the effect of subsidy announcement on new bond issuances using a difference-in-differences estimate similar to the baseline specification. It is based on primary market bonds in Equation 1, for offering yields. In Column (1), we show the result by using only the deal fixed effects 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 rating controls also added to the specification. We use S & P credit ratings at the time of issuance. P-values are reported in brackets and standard errors are clustered at issue-dated month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Offering Yield			
	(1)	(2)	(3)	(4)
Winner x Post	11.61***	6.21***	4.59***	4.71***
	[0.00]	[0.00]	[0.00]	[0.01]
Winner	-5.56	-6.18**	-0.42	-0.33
	[0.14]	[0.04]	[0.89]	[0.92]
Post ($t \geq 0$)	-46.18***	-36.93***	-35.78***	-36.59***
	[0.00]	[0.00]	[0.00]	[0.00]
Deal FE	✓	✓	✓	✓
Issuer FE	✓	✓	✓	✓
Bond Controls		✓	✓	✓
County Controls			✓	✓
Rating Controls				✓
Adj.-R^2	0.421	0.824	0.833	0.835
Obs.	343,519	340,241	336,559	217,414

For Online Publication–Internet Appendix

A 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. Even 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 later part of the 20^{th} century, the unemployment crises of the 1970s and the recessions in early 1980s resulted in an aggressive war between states to win/retain jobs.

Chi and Leatherby (1997) document 15 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. Whereas, 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 these are revenues forgone but not actual cash paid out. Another justification comes from Noll and Zimbalist (2011): if society has underemployed resources then that 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) provides 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.

B Data on Corporate Subsidies in the U.S.

Subsidy Deals

The *Good Jobs First* Subsidy Tracker (Mattera, 2016) provides a starting point with its compilation on establishment-level spending data. They source 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. Still other states that do not report establishment-level monetary spending through subsidies in their financial data are also 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, the *Good Jobs First* does not exhaustively contain 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 United States. This further omits 221,000 records. (Figure IA1 brings out the proportion of federal versus non-federal incentives.) As a further check to verify against federal money sponsoring some of these deals, 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 subsidy values exceeding USD 50 million. After dropping such records, 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[17] 'to respond expeditiously to extraordinary economic opportunities and to compete effectively for high-value research and development, 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 later. 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 to plug in 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 the newspaper articles. A retention must be for a facility already operating in a location; whereas, 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 (2018), 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 the newspaper articles, we can identify 127 winner-loser deal pairs at the county level, which we define as consisting our final sample[18]. 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 very 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 earliest available dates for a given deal. Specifically, if 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 precise disclosures on all incentives that may have been offered to lure in the firm.

There is inherent secrecy maintained by local government and economic development board officials about such subsidy offers. The underlying assumption being that disclosures would invite other competitors; alternatively, invite moral hazard problems for counties that may be desperately keen to win new jobs. For instance, consider the case of Burnham Institute for Medical Research in 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 locations/bidders (See Figure IA10). A snapshot of some project names attributed to subsidy deals is provided in Table IA2 of the Appendix.

Caveats

Moreover, 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, the newspaper stories carry the competing bid offered. Largely, this remains unobserved in the current setting - and we acknowledge this as a major limitation in the data. Especially so, in cases where more than one city is known to have competed. For deals with multiple losers, there is no direct way to ascertain as to which among those was the closest to getting the deal. We base our judgment on 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 inter-state competition, wherever possible, priority is offered to a location outside the winning state in assigning the runner-up county (for multiple runners-up).

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

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

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 blanks represent no information 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		4792	10000	WI	Racine			13000	???
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	1000	GA	Newton	NC	Durham	1500	New
Foxconn	2017	7/26/2017	4792	10000	WI	Racine	MI	Wayne	13000	New
Vertex Pharmaceuticals	2011	9/15/2011	72	2500	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 Grunman	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 for 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)	136.58***	95.22***	93.88***	111.42***	68.25***	64.92***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Investment (USD mil)		0.12**	0.12**	0.10***	0.10***	0.10***
		[0.04]	[0.03]	[0.00]	[0.00]	[0.00]
State Expenditure (USD mil)			-0.25	-12.55		
			[0.80]	[0.17]		
Median HH-Income$_{t-1}$(1000)					-2.77	-3.09*
					[0.14]	[0.07]
State Surplus Gap (USD mil)						-3.81*
						[0.08]
Constant	52.61	-1.37	9.06	628.83	181.16*	200.18**
	[0.25]	[0.98]	[0.90]	[0.15]	[0.06]	[0.03]
State FE				✓	✓	✓
Event-Year FE				✓	✓	✓
Adj.-R^2	0.507	0.617	0.613	0.761	0.825	0.832
Obs.	121	112	111	104	96	96

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 involves 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 individual trades occurring at prices below 50 and above 150.

	Number of CUSIPs	Number of Transactions
MSRB CUSIPs (Customer Purchase) (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 $<$5 0 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
Creating event panel for 3 years using local bonds	123,187	2,612,055
- Winner	60,579	872,016
- Loser	82,118	1,740,039

Table IA5: Summary Statistics: Municipal Bonds

This table summarizes the bond level characteristics for our sample of bonds linked to corporate subsidies during 2005-2018. 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				
Average Yield(%)	872,016	2.8	2.9	1.5
Yield Spread(%)	872,016	1.4	1.2	1.8
After-tax Yield Spread (%)	870,722	3.2	2.6	2.4
Remaining Maturity (years)	872,016	10.0	8.8	7.1
Loser				
Average Yield(%)	1,740,039	2.7	2.8	1.5
Yield Spread(%)	1,740,039	1.3	1.2	1.8
After-tax Yield Spread (%)	1,740,039	3.2	2.6	2.4
Remaining Maturity (years)	1,740,039	10.8	9.3	7.6
Observations	2,612,055			

Panel B: Primary market

	Count	Mean	Median	Std. Dev.
Winner				
Offering Yield(%)	134,475	2.8	2.9	1.4
Offering Price ($)	134,499	103.7	101.7	7.4
Coupon(%)	137,374	3.6	4.0	1.3
Years to Maturity	137,374	9.3	8.2	6.5
Years to Call	55,316	9.0	9.7	1.7
Amount ($ million)	135,465	3.1	0.7	16.6
Issue Size ($ million)	137,374	43.3	11.5	122.8
Loser				
Offering Yield(%)	214,434	3.0	3.0	1.4
Offering Price ($)	214,462	103.6	101.8	9.4
Coupon(%)	218,222	3.7	4.0	1.3
Years to Maturity	218,222	10.0	8.9	6.8
Years to Call	95,862	8.8	9.7	2.1
Amount ($ million)	214,503	5.0	0.8	22.6
Issue Size ($ million)	218,222	84.1	14.3	196.0
Observations	355,596			

Table IA6: 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 4.1.3. 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)-(4) report the baseline specification using after-tax yield spread on only the subset of pre-refunded bonds. P-values are reported in brackets and standard errors are clustered at issue-year month level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent Variable:	Average Yield	Yield Spread	After-tax yield spread	
	(1)	(2)	(3)	(4)
Winner x Post	1.48	1.13	0.28	0.42
	[0.20]	[0.29]	[0.86]	[0.79]
Winner	3.48***	3.23***	0.53	-9.24***
	[0.00]	[0.00]	[0.72]	[0.00]
Post ($t \geq 0$)	1.07	2.31**	4.43***	5.46***
	[0.01]	[0.00]	[0.00]	[0.00]
Deal FE	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓
Bond Controls			✓	✓
County Controls				✓
Adj.-R^2	0.473	0.692	0.602	0.603
Obs.	481,723	481,723	470,443	467,740

Table IA7: Baseline Table with All Controls

This table reports the baseline results of Table 2 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. Columns (4)-(6) show the results using yield spread as the dependent variable. In Columns (7)-(9), the dependent variable is after-tax yield spread which is calculated using Equation 2 and 3. Our preferred specification comes from Column (9). P-values are reported in brackets and standard errors are clustered at issue-year month level, unless otherwise specified. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

	Average Yield			Yield Spread			After-tax yield spread		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Winner x Post	6.80***	6.38***	6.60***	6.22***	6.07***	6.28***	8.94***	8.36***	8.36***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Winner	-3.88***	-3.11***	-4.63***	-2.72***	-3.07***	-4.58***	-3.50***	-3.35***	-5.80***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Post ($t \geq 0$)	-1.22**	-0.70	0.20	-0.16	0.17	0.98**	0.07	0.90	2.38***
	[0.03]	[0.12]	[0.66]	[0.76]	[0.71]	[0.04]	[0.93]	[0.24]	[0.00]
Coupon (%)		0.71***	0.63***		0.87***	0.78***		1.19***	1.03***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Competitive bond dummy		0.80**	2.53***		-0.12	1.66***		0.65	3.51***
		[0.02]	[0.00]		[0.73]	[0.00]		[0.26]	[0.00]
GO bond dummy		-27.63***	-26.71***		-28.25***	-27.25***		-45.55***	-43.94***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Log(Amount)		-10.26***	-10.36***		-10.36***	-10.45***		-17.82***	-17.93***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Callable dummy		-25.73***	-25.96***		-24.89***	-25.17***		-43.25***	-43.71***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Insured dummy		-6.37***	-6.97***		-5.91***	-6.48***		-9.52***	-10.49***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Remaining Maturity (years)		11.25***	11.30***		6.95***	6.98***		14.72***	14.79***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
Inverse Maturity (years)		-8.78***	-8.74***		-8.65***	-8.61***		-14.88***	-14.83***
		[0.00]	[0.00]		[0.00]	[0.00]		[0.00]	[0.00]
$\triangle$ Unemployment Rate (%)			4.77***			4.48***			6.60***
			[0.00]			[0.00]			[0.00]
$\triangle$ Labor Force			0.71***			0.53***			0.71***
			[0.00]			[0.00]			[0.00]
Log($LaborForce_{t-1}$)			-1.18***			-1.34***			-2.95***
			[0.00]			[0.00]			[0.00]
Unemployment $Rate_{t-1}$			12.95***			13.05***			21.12***
			[0.00]			[0.00]			[0.00]
Constant	275.86***	344.21***	270.64***	134.13***	252.04***	181.73***	317.67***	489.02***	384.09***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Deal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Bond Controls		✓	✓		✓	✓		✓	✓
County Controls			✓			✓			✓
Adj.-R^2	0.301	0.559	0.565	0.627	0.687	0.689	0.373	0.531	0.536
Obs.	2612055	2471373	2442115	2612055	2471373	2442115	2610761	2470129	2440871

Table IA8: Trading Volume

This table reports the baseline results similar to Table 2 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 available) as the dependent variable in the trading volume. P-values are reported in brackets and standard errors are clustered at issue-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	36,860.38***	38,538.15***	44,894.96***	47,995.37***	87,101.09***	92,656.04***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Winner	-22,669.41***	-19,363.06***	-24,175.89***	-25,659.89***	-45,544.56***	-48,348.35***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Post ($t \geq 0$)	-24,621.30***	-24,900.02***	-11,735.60	-11,983.65	-33,091.52	-33,498.17
	[0.01]	[0.01]	[0.31]	[0.30]	[0.12]	[0.12]
Deal FE	✓	✓	✓	✓	✓	✓
Month-Year FE	✓	✓	✓	✓	✓	✓
Bond Controls	✓	✓	✓	✓	✓	✓
County Controls		✓		✓		✓
Adj.-R^2	0.049	0.049	0.047	0.047	0.053	0.053
Obs.	2,471,373	2,442,115	1,971,026	1,951,056	1,971,026	1,951,056

Table IA9: Predicting Winner

This table shows the results from a linear probability model using the 'winner' dummy as the dependent variable. P-values 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)	-0.016	-0.017	-0.016	-0.016	0.000	0.001
	[0.20]	[0.20]	[0.21]	[0.23]	[0.98]	[0.95]
Unemployment Rate(%)		-0.001	-0.000	-0.001	0.002	0.000
		[0.93]	[0.99]	[0.87]	[0.82]	[1.00]
% Change in Unemployment Rate			-0.005	-0.005	-0.008	-0.012
			[0.70]	[0.72]	[0.53]	[0.39]
% Change in Labor Force				-0.007	-0.009	-0.010
				[0.32]	[0.17]	[0.16]
Log(Median Income)					-0.132*	-0.028
					[0.06]	[0.76]
Log(House Price Index)						-0.097
						[0.11]
Constant	0.703***	0.709***	0.706***	0.707***	1.929***	1.988***
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Adj.-R^2	0.000848	-0.000436	-0.00146	-0.00150	0.00384	0.0111
Obs.	775.00	775.00	773.00	773.00	706.00	680.00

Table IA10: Impact on Public Expenditure

This table shows the impact of subsidy on local government expenditures scaling each metric at the per capita level. We use the annualized version of Equation 1 as the primary specification for this table. Columns (1)-(4) show the aggregate impact per capita, while Columns (5)-(8) present the results based on sub groups of the interest burden. Specifically, we show the interacted form of the difference-in-differences estimate based on a dummy variable (*Low* and *High*) based on the median value among winning counties. In these interacted specifications, we replace the event-year fixed effects with group-event year fixed effects to absorb the underlying group specific yearly variation. P-values 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 Expenditure	Elementary Education	Health	Police and Protection	Total	Elementary Education	Health	Police and Protection
					By Interest Burden			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner x Post	-24.59	3.80	-38.58	-1.85				
	[0.63]	[0.86]	[0.25]	[0.74]				
Winner x Post x Low					-14.09	-10.05	6.60	0.49
					[0.84]	[0.76]	[0.91]	[0.95]
Winner x Post x High					-42.27	17.77	-91.11***	-5.36
					[0.56]	[0.54]	[0.00]	[0.51]
Low vs High					28.18	-27.81	97.71	5.844
P-value					0.78	0.53	0.14	0.61
Deal FE	✓	✓	✓	✓	✓	✓	✓	✓
County FE	✓	✓	✓	✓	✓	✓	✓	✓
Event-Year FE	✓	✓	✓	✓				
Group Event-Year FE					✓	✓	✓	✓
County Controls	✓	✓	✓	✓	✓	✓	✓	✓
Adj.-R^2	0.971	0.956	0.982	0.973	0.971	0.956	0.982	0.972
Obs.	1,629	1,629	1,629	1,629	1,618	1,618	1,618	1,618

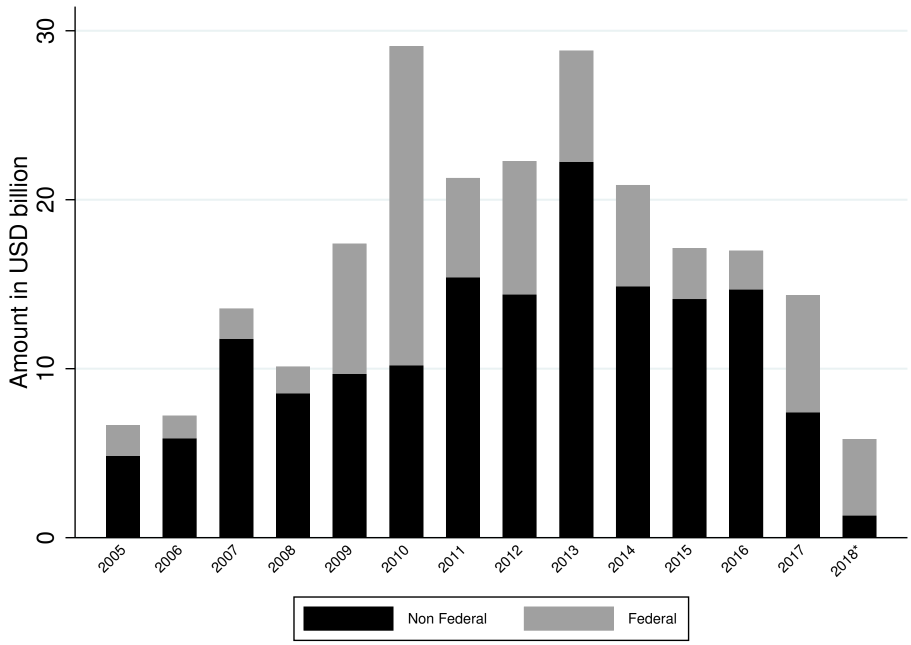


Figure IA1: Total Subsidy: The vertical bars show the aggregated value of total subsidy offered by federal and non-federal (state and local bodies) governments for each year during 2005-2018. This does not include federal loans. Calculated based on Source: Good Jobs First, Subsidy Tracker. *Denotes incomplete data for the year, till June 2018

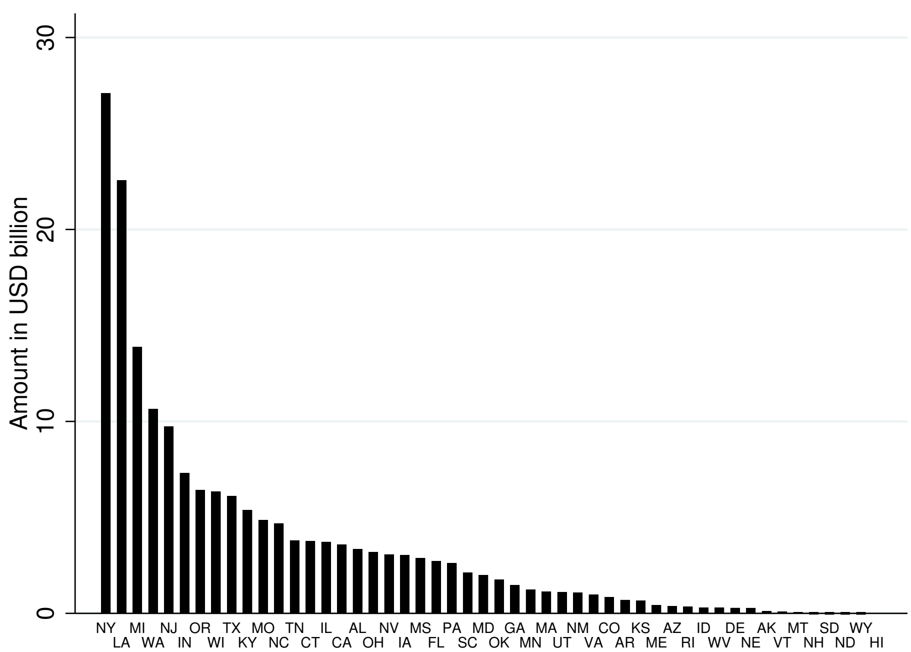


Figure IA2: Total Subsidy by States: The figure shows ranking among US states based on total non-federal subsidy offered during 2005-2018. Calculated based on Source: Good Jobs First, Subsidy Tracker

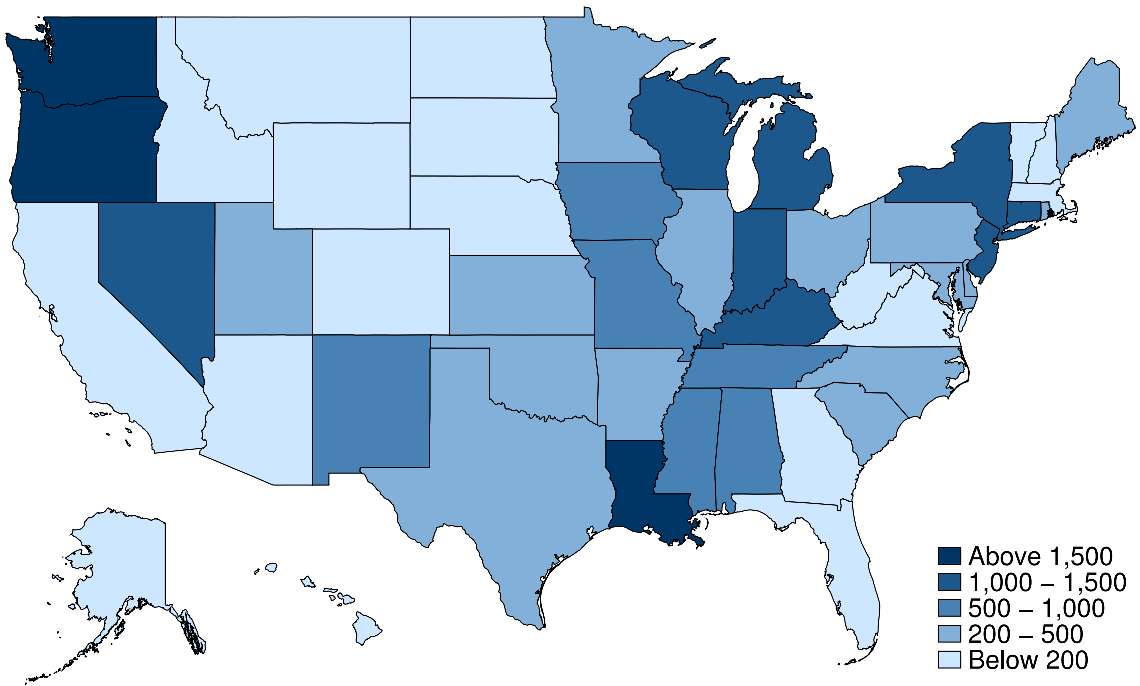


Figure IA3: Subsidy per Capita: The state-level distribution of subsidy per capita (in USD) is shown for the period 2005-2018. Calculated based on Source: Good Jobs First, Subsidy Tracker

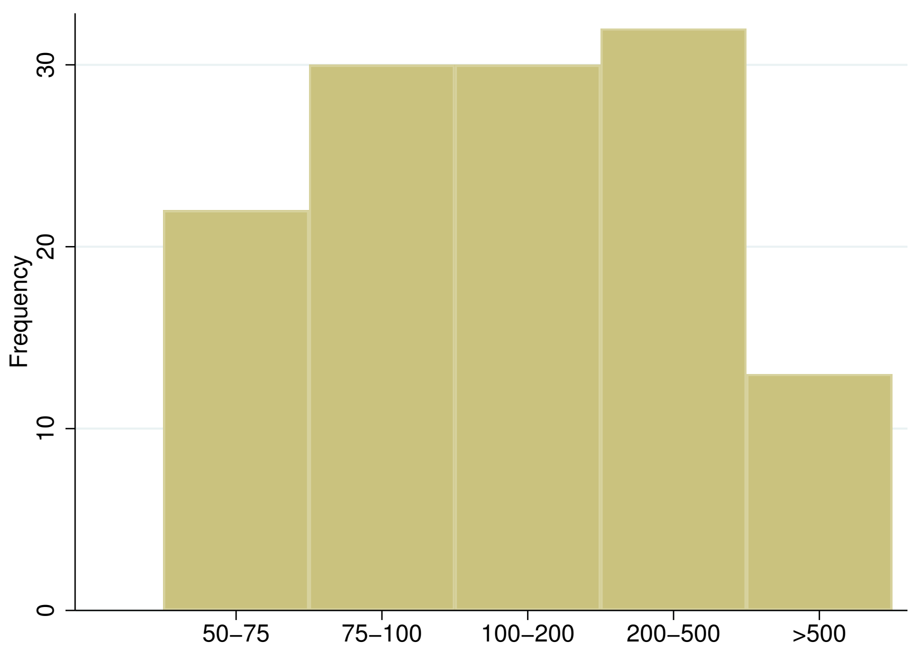
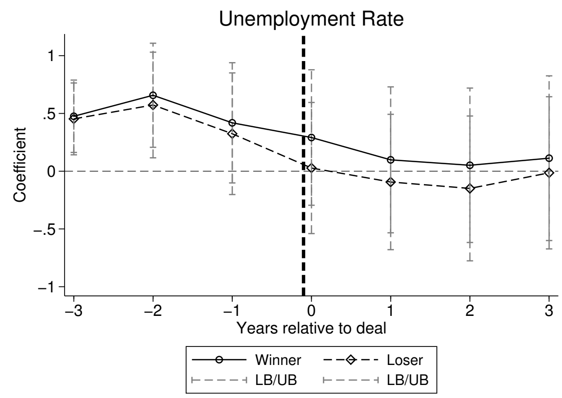
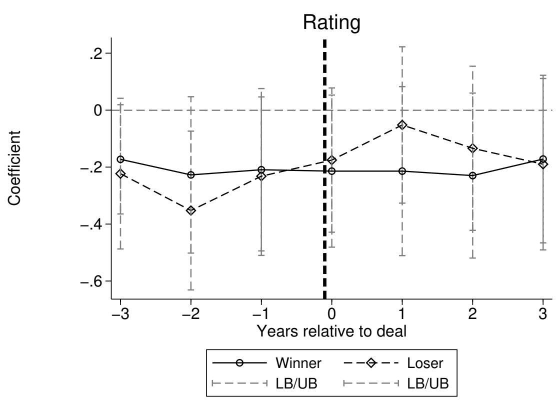


Figure IA4: Distribution of Subsidy (USD million): In this figure, we plot the number of deals in our sample of winner-loser pairs during 2005-2018 across different ranges of subsidy bins.

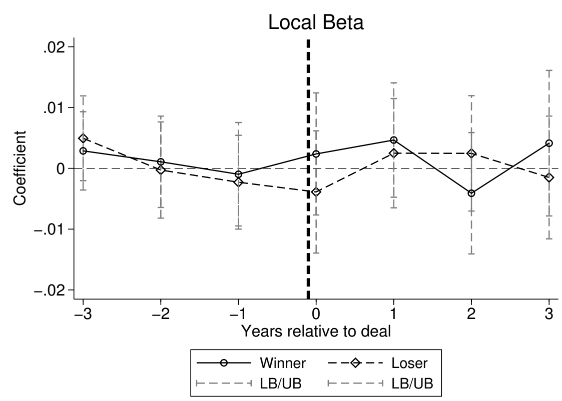
(a)



(b)



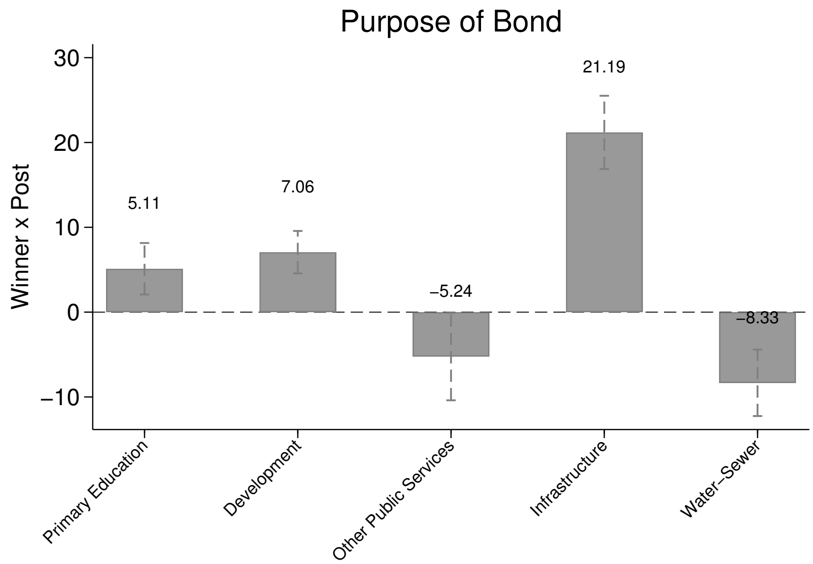
(c)



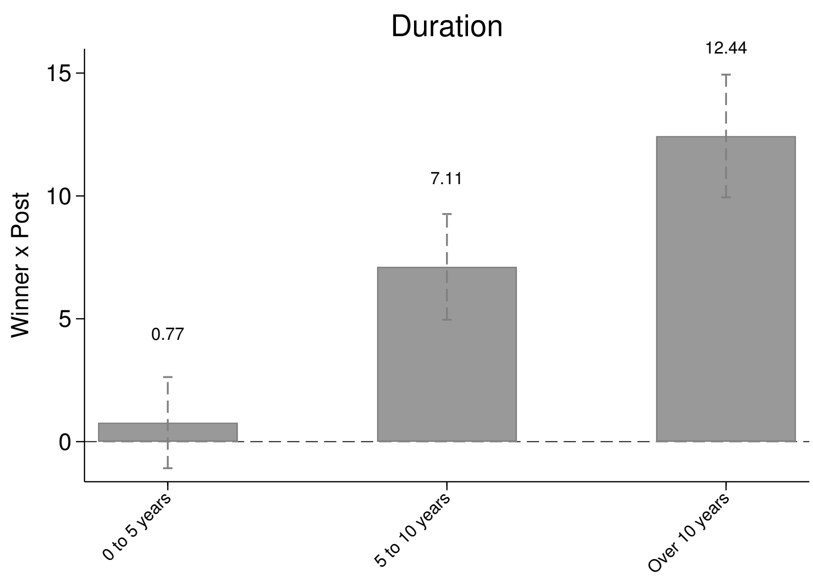
(d)

Figure IA5: 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 4, but additionally introduce county fixed effects. Here, we cluster standard errors at the deal level. The benchmark period is the year before the window (-3,3). The dashed line represents 95% confidence intervals.

Figure IA6: 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 3 years before the deal. Confidence intervals at the 95% level are plotted.



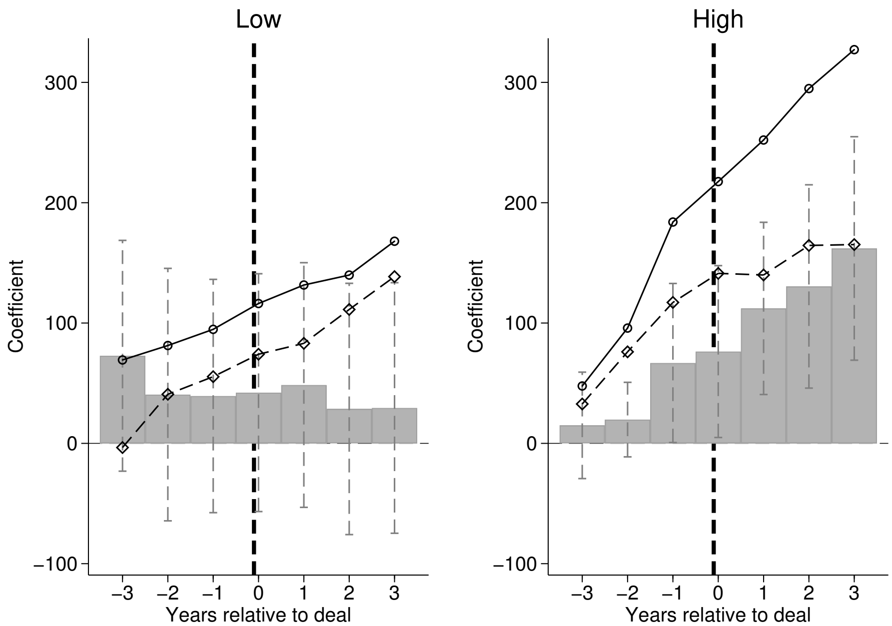
(a)



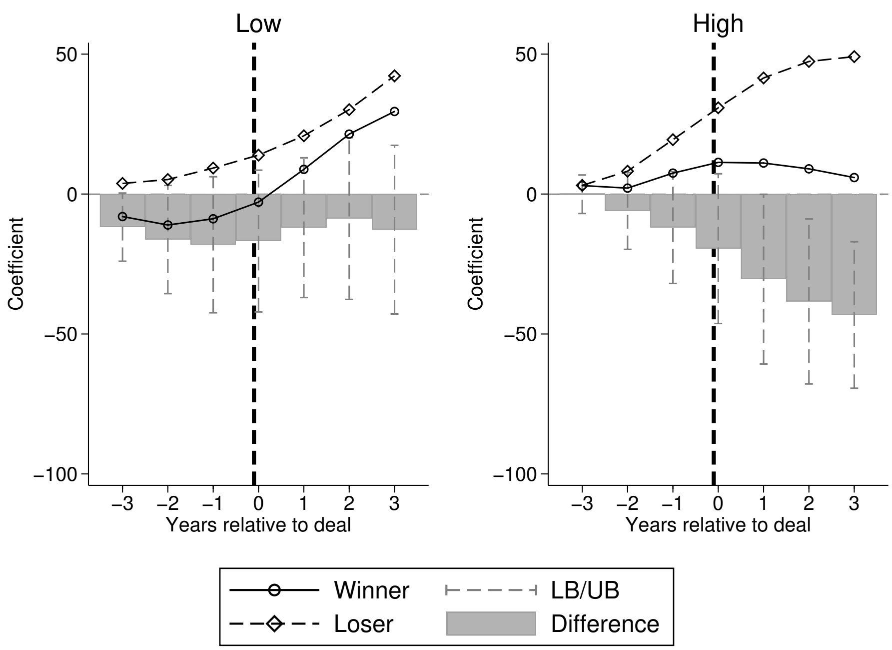
(b)

Figure IA7: Use of Proceeds: 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 use of proceeds (sub-figure(a)) and duration (sub-figure(b)). We additionally control for group-month fixed effects in the regression. The classification of purpose of bond is based on use of proceeds similar to Gao, Lee, and Murphy (2019b)

By Interest to Debt:



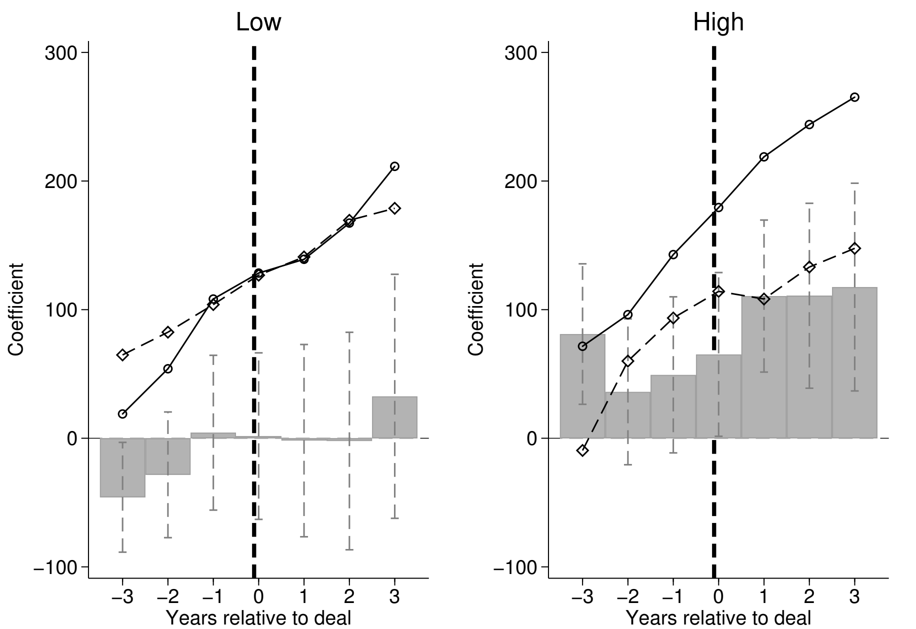
(a) Property Tax Revenue per capita



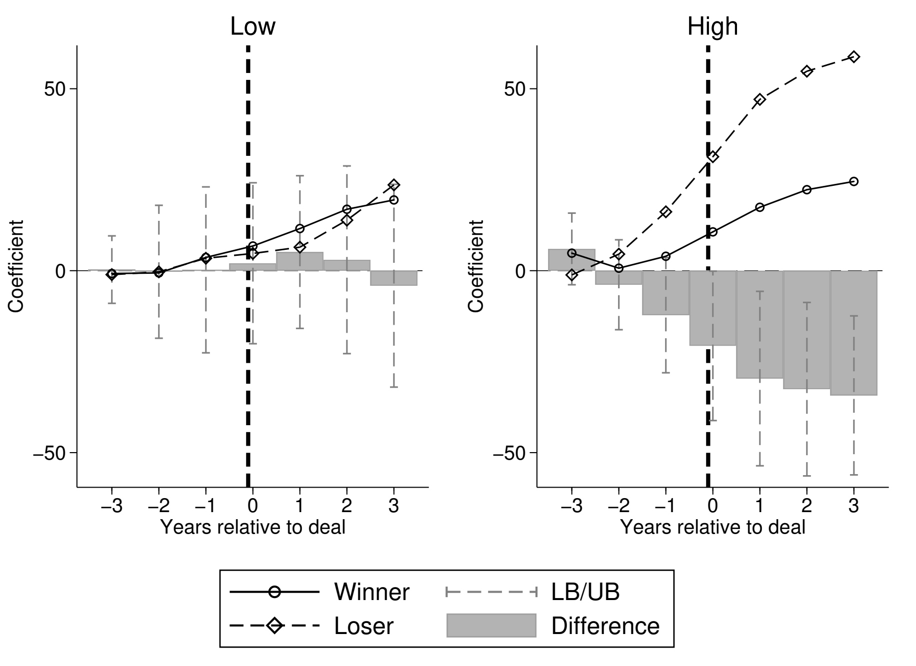
(b) FHFA House Price Index

Figure IA8: Impact on Local Property Taxes: The figure represents the relative changes in property taxes among winners split by the interest to debt, compared to the corresponding losers. In sub-figure (a), we show the property tax revenue and in sub-figure (b), we represent the house price index obtained from Federal Housing Finance Agency (FHFA). we use the annualized version of Equation 4, but additionally introduce county and event-year fixed effects. Here, we cluster standard errors at the deal level. The benchmark period is the year before the window (-3,3). The dashed line represents 95% confidence intervals.

By Interest to Expenses:



(a) Property Tax Revenue per capita



(b) FHFA House Price Index

Figure IA9: Impact on Local Property Taxes: The figure represents the relative changes in property taxes among winners split by the interest to expenses, compared to the corresponding losers. In sub-figure (a), we show the property tax revenue and in sub-figure (b), we represent the house price index obtained from Federal Housing Finance Agency (FHFA). we use the annualized version of Equation 4, but additionally introduce county and event-year fixed effects. Here, we cluster standard errors at the deal level. The benchmark period is the year before the window (-3,3). The dashed line represents 95% confidence intervals.

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 IA10: Project Secrecy: This figure provides an excerpt representing an instance of secrecy maintained by local officials in their process to bid for the said project.

Table A1: Description of Key Variables

This table reports variable definitions. Data sources include the municipal bond transaction data from the Municipal Security 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), 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 risk free rate. We use the maturity matched zero coupon yields as the benchmark for risk free rate following Gürkaynak, Sack, and Wright (2007).	MSRB, FEDS
After-tax Yield Spread	Calculated as the difference between the tax-adjusted *Average Yield* and the risk free rate. We use the maturity matched zero coupon yields as the benchmark for risk free rate following Gürkaynak, Sack, and Wright (2007). 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
Interest burden	Ratio of interest on general debt to total revenue for the county.	CLGF
Interest to debt	Ratio of interest on general debt to total long term debt outstanding for the county.	CLGF
Interest to expenses	Ratio of interest on general debt to total expenditure for the county.	CLGF
Tax privilege	The highest state income tax rate applied to income from municipal bonds issued by other states minus the highest state income tax rate applied to income from the winning state-issued municipal bonds.	FTA
Tax privilege gap	Difference in *tax privilege* between the winning and losing counties. The tax privilege gap results from these counties belonging to different states.	FTA