Do targeted business tax subsidies achieve expected benefits?

Lisa De Simone[*]
Stanford University

Rebecca Lester
Stanford University

Aneesh Raghunandan
London School of Economics

March 2019

We examine the association between state and local firm-specific tax subsidies and business activity in the surrounding county, measured as the number of business establishments, number of employees, aggregate wages, per capita employment, and per capita wages. Using propensity score matching and entropy balancing, we find evidence of a positive association between tax subsidies and these measures of business activity. These results are generally robust to a third control sample of counties that later receive subsidies. We also observe an increase in the number of employees, wages, and per capita wages among the 48 "megadeal" subsidy packages that include a variety of state and local benefits; however, population growth following such megadeals appears to negate per capita employment effects. Finally, the effectiveness of subsidies varies based on state-level disclosure about these awards, demonstrating that increased disclosures enhance public monitoring of tax subsidy efficiency. We provide a large-scale empirical analysis of the relation between firm-specific tax subsidies and aggregate economic activity at the county level, thus extending a literature that generally focuses on the real effects of statutory tax policies that impact all firms within a jurisdiction.

[*] Corresponding author: De Simone can be contacted at Lnds@stanford.edu or (650) 723-3874. We thank Good Jobs First for access to the Subsidy Tracker dataset. We appreciate helpful comments from Eric Allen, John Core, John Gallemore, Philip Joos, Terry Shevlin, Ann-Catherin Werner, Mark Wolfson (discussant), and workshop participants at London Business School, the University of Mannheim, Indiana University, the University of California at Irvine, and the 2018 Stanford Accounting Summer Camp. Lester and De Simone gratefully acknowledge financial support from the Stanford Graduate School of Business; Raghunandan appreciates financial support from the London School of Economics.

I. Introduction

This paper studies the relation between targeted business tax subsidies and local economic activity. Understanding the role of taxes and governmental policies in firm investment and employment decisions, as well as the effect of tax policies on economic development, has been the topic of much academic research and is a central question for policy makers. Prior papers generally focus on differences in firms' tax burdens across jurisdictions and over time, with most studies examining the real effects of changes in tax policies, such as statutory corporate income tax rates, that affect all firms operating in a particular jurisdiction (Hines, 1997; Papke, 1991; Suarez Serrato and Zidar, 2016; Giroud and Rauh, 2018).

In addition to altering statutory tax regimes, governments use firm-specific tax subsidies as tools to increase business activity in their jurisdictions. In recent years, state and local governments have provided thousands of these firm-specific tax subsidies, generally under the premise of creating local jobs and boosting the local economy.[1] The aggregate dollar value of tax subsidies awarded is material, amounting to nearly $11 billion in 2014 alone. Despite the large dollar amount of tax subsidies granted and the growing prevalence of this practice across jurisdictions, empirical research on the effects of these tax subsidies is generally limited to studying specific jurisdictions. We study to what extent these tax subsidies are associated with more business establishments, greater local employment, and higher wages in the years after the subsidy is granted. Further, we study how tax subsidy size and disclosures by the granting jurisdictions impact the association between these subsidies and local business activity.

[1] We examine some potential benefits of tax subsidies awarded by state and local governments, excluding federal subsidies. For example, Boeing received subsidies equivalent to approximately $8.7 billion in 2013 from the state of Washington to retain production of the 777X airplane near Seattle. Amazon selected two jurisdictions to share their second headquarters location in November 2018, and most of the 20 finalist localities included some form of local tax and other benefits in their offer package (Leroy, 2018). The company cancelled plans for its New York location on February 14, 2019, forgoing approximately $3 billion in tax benefits. Most tax subsidies, however, are much smaller than these "megadeal" examples; this study examines both these larger awards and almost 3,000 smaller subsidies.

Prior literature motivates a positive relation between tax subsidies and the number of local establishments and employees. For example, international studies generally find that firms respond to lower country-level tax rates when making production location decisions (e.g., Hines, 1997; Grubert and Mutti, 2000; Hines and Rice, 1994). Similarly, Giroud and Rauh (2018) find that business establishments and employment are negatively associated with state corporate income tax rates.

However, other studies of tax regime differences across U.S. states produce conflicting results, suggesting at least two reasons we may not find an association between targeted tax subsidies and greater local economic activity. First, the literature examining the role of taxes in fostering economic growth yields largely inconsistent, and even surprising, results. Two studies re-examine multiple models in the literature and document an overall *positive* relation between tax rates and personal incomes (Pjesky, 2006; Gale, Krupkin, and Rueben, 2015). Furthermore, Gale, et al. (2015) and Ljunqvist and Smolyansky (2015) find either no statistical relation or very small economic magnitudes when studying the association between tax policy changes and new firm formations and employment. Second, recipient firms may not be required to provide evidence that they increased their local investment and employment spending, and thus they could receive a tax subsidy despite little subsequent change in the firm's local presence. In addition to these two reasons, we may observe little economic effect if the firm-specific or related spillover effects are not large enough to detect at the county level, or if the tax subsidy is awarded by a state government with the primary goal of spillover effects in counties neighboring the location of the recipient firm.

To test the relation between tax subsidies and local economic activity, we obtain data on the incidence of individual firm tax credits and tax abatements from Good Jobs First (GJF). Good Jobs First is a nonprofit organization that compiles Subsidy Tracker, the most comprehensive

dataset available on state and local subsidies since 2004, covering over 600 development programs across all 50 states.[2] We combine the sample of tax subsidies with county-year data from the U.S. Census Bureau and the U.S. Bureau of Labor Statistics to measure subsequent local economic activity and to construct control variables used in empirical tests.

We examine the association between subsidy incidence and business activity using a staggered difference-in-difference design and three alternative control samples. Our main approach matches (with replacement) county-years with subsidy recipients to counties in the same state with no subsidy recipients during our sample period. We perform propensity score matching to model the likelihood of a first-time subsidy in a county based on county characteristics, including population, whether the county is urban or rural, and economic trends in the years preceding subsidy receipt. By requiring a within-state match, this approach has the benefit of controlling for state-year characteristics such as GDP and state tax rates as well as unobservable state-year factors, such as state-level preferences or private sector connections related to subsidy grant (Aobdia, Koester, and Petacchi, 2018). Our second approach uses entropy balancing to match the same county-years with subsidy recipients to counties with no subsidy recipients during our sample period. Although this approach relaxes the requirement that controls be within the same state, the advantage of this method is that it ensures balanced covariates across all observable state- and county-year characteristics. Finally, because counties with subsidy recipients could be different from counties with no subsidy recipients across observable and unobservable dimensions that these other two approaches may not address, we also benchmark the business activities of county-years with subsidy recipients against counties that later receive subsidies.

[2] Good Jobs First is a Washington, D.C. based nonprofit that describes its mission as "...seeking to make economic development subsidies more accountable and effective" (from www.goodjobsfirst.org).

Counties with subsidy recipients have over 26,000 distinct tax subsidies during the sample period, including tax credits, tax abatements, and "megadeals," which tend to be the largest deals in dollar value of incentives provided. We observe a growing frequency of tax subsidies during the sample period, with over 6,600 tax subsidies in 2014 alone. Although we are interested in all of these tax subsidies, our tests focus on a subset of awards to better identify the effects of a tax subsidy on county-level economic factors. First, we require sample counties to have no observable subsidies in 2004 or 2005 to provide a "clean" pre-subsidy period of at least two years within GJF coverage. Second, we focus on the first tax subsidies observed in the GJF data after 2005. Because we require counties to be observed for the entire sample period, the propensity-score matched sample is a balanced panel of 14,112 county-years (558 county pairs) for the twelve year period from 2004 to 2015. This sample of county-years is distributed across 31 distinct U.S. states with requisite data, with the most observations in Colorado, Indiana, Iowa, Kentucky, Tennessee, and Texas. The 588 unique counties with tax subsidy recipients represent a total of 2,859 subsidies in these county-years. These first-observed tax subsidies provide approximately $11.1 billion of benefits to recipient firms.

We test the relation between the incidence of a tax subsidy and the future number of establishments, number of employees, and aggregate level of wages in the corresponding jurisdiction relative to control counties. We also test employment and aggregate wages scaled by working age population (age 25-65) as measures of economic effects relative to the county's labor pool. Regression analysis provides some evidence of an increase in the number of business establishments using the entropy balanced sample. We estimate a 0.7 percent increase in establishments following a first-time tax subsidy to a recipient firm within the county relative to matched control counties, or approximately 12.4 additional business establishments. We find

robust evidence of increases in employees, wages, and per capita employees and wages using both the within-state propensity-matched sample and the entropy balanced sample. We estimate a 1.9-4.8 percent increase in employees and a 1.6-7.9 percent increase in wages relative to matched control counties. Per capita employment increases by 0.83-1.1 percentage points, or 3.0-4.0 percent, relative matched control counties, and per capita income increases by $574.1-$781.6. These results are generally supported by additional analyses that restrict the sample to only those counties with subsidy recipients, using the pool of counties that later receive subsidies as a third alternative control sample.

We next examine the role of subsidy size in the relation between tax subsidies and local area activity. In particular, we study "megadeals," which are subsidies that range from $12.4 million to $1.07 billion in size. These subsidies are often composed of several types of awards from multiple jurisdictions (state plus city/county). Although our sample of the first subsidies observed in the GJF data after 2005 includes only 50 of these megadeal subsidies (or 1.7 percent of the subsidies studied), they represent approximately 86.1 percent of the total dollar value for the subsidies with disclosed award amounts ($9.6 billion). We observe that the subset of counties with a megadeal recipient report an increase in the number of employees and wages, consistent with firms using the tax savings to subsequently hire. Interestingly, we find no change in employment on a per-capita basis in these counties, suggesting that megadeals are not only positively associated with jobs but also with commensurate population growth. Counties without megadeal recipients also exhibit increases in the number of employees, wages, employment rate, and per capita wages.

To provide additional evidence on the role of deal size in the relation between tax subsidies and local economic activity, we sort our non-megadeal sample into quartiles based on the dollar

amount of the tax subsidies. We continue to find no change in the number of business establishments across all four quartiles. Across employment, wages, and per capita wages, the point estimates increase with deal size, suggesting that the economic effects may be monotonically increasing in the amount of the subsidy. However, most of these estimated coefficients are not statistically significant, nor do we find evidence of statistically significant differences in the estimated coefficients across the four quartiles.[3] We acknowledge that partitioning our sample into quartiles to examine the role of deal size likely reduces the power of these tests.

Our final test focuses on the information environment of jurisdictions with subsidy recipients to further study variation in the effectiveness of the subsidies in achieving local economic activity. Specifically, we study whether the quality of a state's public disclosures about awarded subsidies is associated with more or less effective tax subsidies. We partition the county-year sample based on their state's GJF transparency ranking. Consistent with disclosures allowing more effective monitoring, the effect of tax subsidies on a per capita basis is strongest in the states with more transparent subsidy disclosures.

We acknowledge that our approach and the data we use does not allow us to directly test at the firm level the reporting or real effects of subsidies on firm recipients. This paper also does not consider general equilibrium effects of tax subsidies on employment levels and growth. For example, a tax subsidy in one county may impact supply or demand for labor, capital, or outputs in another county. Furthermore, we are unable to evaluate the local tax revenue net gain or loss associated with the granting of a tax subsidy, or other costs of these tax policies. Although the paper does not address these open and interesting questions, we believe our approach is an

[3] The subsidies may generate other types of business activity that we are unable to test at the county-level. For example, certain subsidies may motivate capital expenditures by an existing business or attract specific industries to create an agglomeration of firms within a location; however, we are unaware of available county-level data to test these outcomes.

important step towards understanding the effectiveness of targeted tax subsidies in generating purported benefits.

Our study relates to four recent studies that explore different aspects of firm-specific subsidies. Aobdia et al. (2018) examine whether grants of regional subsidies are associated with contributions by firms to state political candidates. They find a positive association between corporate contributions and subsidies, but no effect of subsidies granted to politically connected firms on future state-wide job growth. Mast (2018) also explores the government's decision to offer firm-specific tax breaks but focuses on the competitive effects between localities. Using data on property and sales tax subsidies offered by towns in New York, the study predicts and finds a positive association between a town offering exemptions and the likelihood and frequency of those being offered by a neighboring town. Drake, Hess, Wilde, and Williams (2018) examine the relation between subsidy magnitude and future firm performance measured as sales growth, sales, gross profits, operating income, and net income. They find a positive effect as predicted. Subsequent tests suggest that future performance is not impounded into price by investors when subsidies are awarded. Finally, Raghunandan (2018) uses subsidies as a setting to examine the role of government capture on the likelihood of subsidized corporations both engaging in and getting caught engaging in accounting misstatements and other indicators of poor financial reporting quality. Our paper contributes to this nascent literature by providing a large-scale empirical analysis of the relation between firm-specific tax subsidies and increased aggregate economic activity at the county level.

We also contribute to the broader literature on the role of state business climate characteristics (such as tax rates, state apportionment factors, characteristics of the tax base, etc.) in firms' production decisions (Papke, 1991; Hines, 1997; Goolsbee and Maydew, 2000; Holcombe

and Lacombe, 2004; Giroud and Rauh, 2015; Ljungqvist and Smolyanksky, 2015; Gale et al., 2015; Serrato and Zidar, 2016). None of these prior papers examine firm-specific tax subsidies, which permit a more precise evaluation of how taxes affect firms' investment and employment decisions. Our analysis improves our understanding of these economically material governmental policy tools and facilitates future comparisons of the effectiveness of firm-specific tax policies relative to other policies that impact all firms within a jurisdiction.

II. Prior Literature and Hypotheses

A large literature examines whether tax policies impact general economic outcomes and growth, generating mixed and often conflicting results. Early models and empirical tests fail to find a relation (e.g., Bloom, 1955; Thompson and Mattila, 1959; Carlton, 1979 and 1983). Using a relatively broad panel of state personal incomes as a proxy for overall economic health, Helms (1985) documents a negative relation between tax revenues and personal incomes. However, the negative coefficients on tax collections are more than offset by coefficients on state spending on public services, suggesting that both the revenue and spending effects of state tax policies must be taken into account. Pjesky (2006) revisits the empirical models of Helms (1985), along with those of four other studies (Vedder, 1996; Becsi, 1996; Mofidi and Stone, 1990; and Carroll and Wasylenko, 1994). Using a constant sample period and per capita personal income as the dependent variable, Pjesky (2006) confirms an overall positive relation in many specifications between taxes and income. In contrast, Reed (2008) concludes there is a consistent and robustly negative effect of taxes on real personal incomes from 1970 to 1999. Gale et al. (2015) also employ multiple models from the prior literature to re-examine the role of taxes on growth, concluding there is an overall positive relation.[4]

[4] See also Mazerov (2013) and McBride (2012) for reviews of this literature.

A related, but distinct, literature studies the effect of taxes on firm establishments and employment. Bartik (1985) studies the association between aggregate corporate effective tax rates and the creation of new plant establishments by existing firms. While the paper documents a negative association, the magnitude of the relation is small enough that the author cautions that state tax policy changes are unlikely to cause either an economic "miracle" or "wasteland" (Bartik, 1985, p. 21). Papke (1991) extends the analysis to new plant establishments by start-ups in five select industries, finding that plant births are decreasing in effective tax rates but increasing in public services such as fire and police services and local financing assistance. Importantly, the significance and magnitude of results vary materially across industries, suggesting results do not generalize to the economy as a whole. In addition to studying the relation between taxes and economic growth, Gale et al. (2015) also find an economically small but negative association between taxes and firm births and a negative, but insignificant, association between taxes and employment.[5]

Evidence on the effects of tax rate changes on firm-level employment is more limited, largely due to data availability. Ljunqvist and Smolyansky (2015) examine firm-county employment responses to changes in state corporate income tax rates, finding that employment declines following tax rate increases. However, they observe no change in employment following tax rate decreases. The lack of symmetry in these results could be attributable to confounding tax policy changes that often coincide with state rate decreases, such as a change from separate to combined reporting. Serrato and Zidar (2016) use county-level Census data and payroll data from Reference USA to study the incidence of the corporate tax and to evaluate how firms respond to

[5] A large literature focuses on the incidence of business taxes, generally asking whether and to what extent labor, which is a relatively immobile factor of production, bears the burden of business taxes. The purpose of our study is not to estimate how statutory tax regime changes affect specific groups, such as capital-owners or employees, but instead to focus on whether and to what extent firm-specific tax benefits are associated with local area effects.

state tax changes. Giroud and Rauh (2018) use establishment-level Census data and find that an increase in the state corporate tax burden leads to the closing of local corporate establishments and a reduction in firm employment. All three of these studies focus on tax policy changes that affect all firms within a particular jurisdiction. Collectively, these findings suggest that tax changes have important effects on firm employment, but estimated elasticities vary based on the type and location of the tax incentive.

Our study also relates to a large literature that examines the role of taxes in location and, importantly, relocation decisions for investment and employment across borders. In the international context, numerous studies demonstrate that taxes impact investment and location decisions for firms with mobile factors of production (e.g., Hines and Rice, 1994; Grubert and Mutti, 2000). Hines (1997) extends the analysis to foreign multinationals investing in U.S. states, finding that firms based in countries offering a foreign tax credit are less responsive to state tax rates when making U.S. investment decisions. Devereux, Griffith, and Simpson (2007) find that smaller grants are required to entice multi-plant and multinational firms to invest in underperforming regions of the U.K. if there is already an agglomeration of industry in those areas. Giroud and Rauh (2018) demonstrate that, in response to state corporate tax policy changes, some establishments and employees re-locate to other jurisdictions.

A concern common to many studies examining U.S. state tax policies is how to measure the tax burden. With some notable exceptions (e.g., Giroud and Rauh, 2018; Serrato and Zidar, 2016), many studies focus only on statutory income tax rates or revenues, as empirically capturing other tax features, such as state level apportionment factors and non-income taxes such as property taxes, requires additional data and some measurement at the local level. Even within studies focusing only on tax rates, there is significant variation in the measurement of the relevant tax rate,

with papers using a mix of statutory, marginal, or effective tax rates for estimation. We bypass these issues by directly observing the incidence of tax subsidies that reduce or otherwise offset a firm's total tax burden. This approach provides an improved identification strategy relative to prior work to more precisely identify the association between taxes and local firm investment and employment decisions.

In summary, prior research generally finds that lower taxes are associated with more firm employment and, to a lesser extent, more firm establishments. Because we are interested in measuring to what the extent tax subsidies achieve increases in local area activity, including establishment and employment outcomes, we test the relation between firm-specific tax subsidies and these outcomes aggregated across all firms at the county level. We predict the following:

H1: Firm-specific tax subsidies are positively associated with the number of establishments in the associated county.

H2: Firm-specific tax subsidies are positively associated with employment in the associated county.

We may not observe this positive association for several reasons. First, as summarized previously, the prior literature provides mixed evidence on the relation between taxes and economic growth. The number of studies showing a negative, zero, or even a positive relation between taxes and economic outcomes at the state and local level, in addition to the sensitivity of results documented in prior work (e.g., Gale et al. 2015; Pjesky, 2006), suggest that we could observe little association. Furthermore, firms are not always required to demonstrate that they increase their presence in a jurisdiction (via establishments or employment) following receipt of a tax subsidy, and thus tax subsidies could be granted with little or no change in firm activity. Although some states impose a clawback of benefits if the economic activity is not documented, these clawbacks are not frequently enforced (Mattera et al., 2012). Finally, because we measure

effects at the county-level, the firm-specific or related spillover effects of tax subsidies may not be large enough to detect any differences in the local area.

III. Research Design

A. Empirical Approach

We study whether and to what extent employment and investment at the county-level are related to the incidence of a subsidy. We estimate staggered difference-in-difference regressions (Stevenson and Wolfers, 2006) of the following form:

$$EconomicActivity_{i,t+1} = \alpha + \beta_1 PostTaxSubsidy_{i,t} + Controls_{i,j,t} + County_i + Year_t + \varepsilon \quad (1)$$

Subscript i denotes the county, j denotes the state, and t denotes year. The dependent variable $EconomicActivity_{i,t+1}$ represents the number of business establishments, the number of employees, or the amount of aggregate wages in a specific county; we discuss these measures below in Section B. $PostTaxSubsidy_{i,t}$ is an indicator equal to one for observations in counties with at least one company receiving a tax subsidy in years including and following the year that the subsidy is granted, and zero otherwise; that is, we focus our primary analysis on the incidence of the first tax subsidy. Directly examining the relation between subsidy size and economic activities is difficult in our setting because tax subsidies are generally awarded over a duration longer than one year, however GJF does not provide the dollar value of each award by year.[6] We use the GJF data to identify counties with subsidy recipients and discuss the subsidy data further in Section C. We predict that tax credits and abatements are positively associated with the number of establishments and employment; that is, we expect $\beta_1 > 0$.

[6] We test the role of subsidy size in subsequent tests by re-estimating equation (1) after partitioning the sample based on the dollar value magnitude of the subsidy. In future work, we intend to extend these tests.

We include a set of control variables ($Controls_{i,j,t}$) motivated by prior literature, further discussed in Section D. Finally, we include county and year fixed effects as proxies for potentially unobservable changes in local jurisdictional economic conditions. The county fixed effects control for mechanical differences associated with economic activity, whereas year fixed effects control for nationwide macroeconomic trends (such as the 2008-2009 financial crisis) that affect state and county fiscal conditions. Main effects for counties having at least one subsidy recipient and for years in which subsidies are granted are captured by the county and year fixed effects. We cluster standard errors by county.

B. Employment and Investment Measures

We obtain the number of establishments, number of employees, and aggregate wages from the U.S. Bureau of Labor Statistics website. The Bureau of Labor Statistics provides open access to county-level data on business activity by industry. Because the data are derived from firms' mandatory Unemployment Insurance filings, we believe the data provide relatively comprehensive coverage on U.S. firms. From the quarterly filings, we obtain a county-level calendar-year average, computed by the BLS, of the number of establishments, number of employees, and aggregate wages paid on an annual basis. We take the logarithm of these amounts to construct the dependent variables $Ln(Establishments)_{i,t+1}$, $Ln(Employees)_{i,t+1}$, and $Ln(Wages)_{i,t+1}$. As alternative measures of employment and wages, we also scale employees and wages by population and use $(Employees/Population)_{i,t+1}$ and $(Wages/Population)_{i,t+1}$ in our main tests. Gale et al. (2015) state that scaling by population is important because an increase in the number of employees and corresponding wages in a jurisdiction, as captured by $Ln(Employees)_{i,t+1}$ and $Ln(Wages)_{i,t+1}$, could merely reflect population growth. These measures also more closely represent economic outcomes

such as reduced unemployment and specific types of higher-paying jobs within a jurisdiction, which are often included as stated goals of many tax subsidies.

Because the subsidy data capture subsidies to private-sector firms, we construct the outcome variables using private-sector establishments, employees, and wages; that is, we exclude the number of public-sector employees and public sector wages from our measures following other work (Dube, Lester, and Reich, 2010). We confirm that the number of establishments is relatively unaffected by limiting our sample to the private-sector. Although we predict that subsidies are positively associated with employment, the positive relation could arise either through the hiring of more employees or through higher aggregate wages to existing employees (or both). One possible outcome is that we observe more employees in a jurisdiction but lower aggregate wages if the newly employed individuals are hired for lower-paying jobs. Alternatively, we may observe no change in the number of employees but an increase in wages, signaling that the subsidy was used to possibly retain workers at the facility or to increase executive compensation. To more fully explore the mechanism through which employment benefits arise following subsidies and abatements, we test both the number of employees and aggregate wages.

C. Subsidy Data

GJF data is available publicly on the organization's webpage. However, because certain subsidies (such as those from Pennsylvania) are omitted from the website, we obtain the Subsidy Tracker data directly from GJF to ensure completeness. The subsidy data begin in 2004, as GJF have primarily focused on obtaining information on subsidies starting in this year. As discussed previously, we limit our sample to counties with no observed subsidies for at least 2004 and 2005 to better isolate the effect of observable subsidies in our empirical tests. We focus on two categories of awards that account for approximately 70.0-75.0 percent of all subsidies in the

dataset: Tax Credits, which are *dollar value* awards that reduce a firm's tax liability dollar-for-dollar, and Tax Abatements, which provide a *percentage* reduction of a firm's tax liability.[7] Our sample also includes subsidies that span multiple categories of subsidy type and are larger than the average subsidy. GJF separately categorizes these as "megadeals," given their size, mix of subsidy types, and prominence.[8]

We aggregate subsidies to the county-year level using addresses of subsidy recipients provided by GJF. If a subsidy was awarded at the state level, we assign the subsidy to the county in which the recipient firm is located. For example, in 2012 Freightquote, Inc. received combined tax incentives of $64.3 million to relocate its corporate headquarters 15 miles across a state border from Lenexa, Kansas, to Kansas City, Missouri. Of this amount, approximately $33.2 million was awarded by the state of Missouri, while the remaining $31.1 million was provided by Kansas City and the relevant county authority (Jackson County). As the proposed benefits of the subsidy would be realized in Jackson County, we assign all $64.3 million to this county. We hand-match the subsidy data to the BLS establishment and employment data at the county-year level.

D. Control Variables

[7] See additional details from the GJF website: https://www.goodjobsfirst.org/accountable-development/corporate-income-tax-credits. Remaining subsidies in the Subsidy Tracker can be broadly categorized into Grants (up front direct cash transfers to the firm from the government), Reimbursements (ex post cash transfers for expenses incurred by a company, such as job training activities), Enterprise Zones (tax credits or abatements tied to a company's decision to locate to a particular neighborhood), and Tax Increment Financing (which diverts a portion of a company's tax payments to public services that specifically benefit that company, such as maintenance of roads outside a facility). We exclude Grants, Reimbursements, and Tax Increment Financing from our analysis because they represent one-time cash transfers or diversions of public resources likely initiated with the goal of different economic outcomes from those of interest in this study. Because we are interested in how companies respond to direct tax benefits as opposed to statutory tax changes, we focus on tax credits and abatements, which comprise approximately 88.0 percent of all subsidies for our sample.

[8] Some megadeals in the sample have a higher proportion of the total dollar value represented by subsidy types other than the tax credits and tax abatements that are the focus of our study; for example, some include cash grants or reimbursements for job training. However, we retain these megadeals in our sample because the dollar values of the portion of the deals attributable to tax credits and abatements are large relative to those of the average non-megadeal tax credit or abatement in our sample. We discuss robustness to the exclusion of these megadeals in Section IV.D.

We control for determinants of location and employment decisions. These control variables include the minimum wage applicable in each county (e.g., Neumark and Wascher, 2007).[9] We obtain county minimum wage data from the U.C. Berkeley Center for Labor Research and Education. We also control for the state GDP level, measured using aggregate data from the Bureau of Economic Analysis, and the county population, measured using data from the U.S. Census Bureau's American FactFinder. Additional control variables include the percentage of private-sector employees that are union members (e.g., Card, 1996) and educational attainment, measured as the percentage of the population with at least a bachelor's degree (e.g., Card, 1999). We obtain data for both of these from the Census Bureau's American Community Survey. Following Giroud and Rauh (2018), we include a number of additional state-level tax variables, including the state's top marginal corporate tax rate, the personal income tax rate, the property tax share, the log of the state unemployment insurance contribution, the tax incentives index, and the sales tax rate to account for other state tax policy tools that may complement or substitute for tax subsidies.[10]

E. Control Groups and Main Sample Selection and Description

We use three alternative control samples to identify the effects of tax subsidies on the economic activities of counties with subsidy recipients. Our main approach propensity-score matches (with replacement) county-years with subsidy recipients to counties in the same state with no subsidy recipients during our sample period. We describe this procedure and the resulting sample in more detail below. By requiring a within-state match, this approach controls for

[9] States often set a minimum wage different from the federal minimum wage. Because federal law supersedes state law, we replace the state minimum wage with the federal minimum wage if the state minimum wage is lower, indicating that the state did not increase its minimum wage following a federal increase. Further, we replace the state minimum wage with the local minimum wage for seven counties in our sample that impose this additional wage.

[10] We thank Josh Rauh and Xavier Giroud for sharing these data with us. Because their sample period ends in 2011, we augment the data to measure the control variables through 2015.

observable state-year characteristics and possible unobservable state-specific effects related to the likelihood of granting subsidies. Our second approach uses entropy balancing to ensure balanced covariates across all observable state- and county-year characteristics, though it relaxes the requirement that matches be within the same state to improve matches along observable county-year characteristics. Our third approach benchmarks the business activities of county-years with subsidy recipients against counties that later receive subsidies. We use this third control sample because counties with subsidy recipients could be different from counties with no subsidy recipients across observable and unobservable dimensions that our first two approaches may not address.

Table 1 describes the sample selection process for propensity score matching, which we use as our main approach. As detailed in Panel A, we start with all county-years with available establishment and employment data for the twelve years from 2004 to 2015 (n=38,352 observations). We drop any county that has merged with another county during the sample period (n=64), counties in states with missing BLS data (n=464), and counties in states reporting two or fewer years with any subsidy activity (suggesting that subsidy data for those jurisdictions are incomplete (n=3,348)).[11] We acknowledge that firms could receive subsidies in years prior to 2004 that are not captured in the GJF data. To better isolate the effect of observable subsidies in empirical tests, we eliminate from our sample counties with observed tax subsidies in the first two years of the GJF data (2004 and 2005) (n=5,256), thus ensuring a "clean" pre-subsidy period of at least two years. Of the remaining 29,220 county-year observations, 9,396 observations relate to 783 distinct counties with at least one tax subsidy recipient during the sample period.

[11] These states include Alaska, Delaware, Hawaii, Idaho, North Dakota, New Hampshire, Pennsylvania, Rhode Island, South Dakota, and Wyoming.

We use propensity score matching to match each of the counties to the set of counties with no subsidy recipients during the period of GJF coverage (2004 to 2015). We estimate the likelihood a county has firms receiving a subsidy as a function of the county's population, an indicator variable equal to one if the county is classified as rural based on the Consumer Financial Protection Bureau's urban/rural county classifications, all control variables included in our main tests, and trends in the outcome variables over the preceding three years. We match with replacement in the year preceding the first subsidy we observe in our sample, and we require that counties be matched to another county within the same state to control for unobservable state-specific characteristics related to subsidy grants.[12] We present the results of our propensity model in Panel B. The main sample of 14,112 county-years is composed of 588 subsidy-control county pairs.

Panel C provides tests of differences in the mean values of the dependent variables for subsidy counties and the propensity-matched control counties in the year of the matching.[13] This panel shows that there are statistically significant differences in the levels of three of the five measures. Because a key assumption of our difference-in-differences research design is that the treatment and control groups would have exhibited similar outcomes after a subsidy, absent any subsidy effect, we examine the pre-period trends to assess the similarity of these groups. In Figure 1, we plot the values for each of the five outcome variables by year across five panels. The top

[12] The propensity score matched sample of 18,370 includes 7,830 subsidy county-year observations and 10,540 possible control observations. The matching approach results in the use of 199 distinct control counties. For 195 subsidy counties, we are unable to identify a suitable match. For example, all counties in Connecticut have a subsidy recipient at some point during the sample period, and thus there are no possible control counties within the state. As another example, we drop all counties in Louisiana, Ohio, Oregon, and Wisconsin due to a lack of sufficient matched control counties; inferences are unchanged using more restrictive thresholds for determining a sufficient number of matched control counties that further reduce sample size.

[13] Because the independent variables of interest are all state-level measures, there are no statistically significant differences in the values of these variables across the subsidy and control groups given that we require matches to be within the same state.

graph in each panel plots the raw values of each variable, and the bottom graph plots the residual values after first regressing the outcome variable on control variable and fixed effects. Consistent with the descriptive evidence in Panel C, we observe that the subsidy and control counties exhibit different levels across all five variables, as seen in the top graph on each page. However, we generally observe similar pre-period trends, confirming the similarity of these groups for purposes of our tests. Table 1, Panel D provides the distribution of county-year observations by state. The states with the most observations for tax subsidy recipients include Colorado, Indiana, Iowa, Kentucky, Tennessee, and Texas.

Table 2 provides descriptive statistics for key variables used in empirical tests. The average county reports approximately 1,926 business establishments and 26,595 employees. These establishments pay aggregate wages of approximately $1.2 billion annually, or approximately $9,790 per working age (age 25-64) resident. All of these amounts are skewed, with counties in the 99th percentile reporting 22,000 establishments, over 333,000 employees, and $16.7 billion of aggregate wages (untabulated). The average minimum wage for the sample period is $6.57/hour. Approximately 26.4 percent of the population has at least a bachelor's degree, and 5.67 percent are union members. The average state corporate and personal income tax rates are 5.92 and 3.87 percent, respectively.

Panel B provides sample composition by type of subsidy. We first show descriptive statistics on the 588 county-years with first observed subsidies granted during our sample period, as these are the primary subsidies we examine in empirical tests. We observe 2,859 distinct tax subsidies in this sample. Tax credits and abatements comprise 57.6 percent and 40.7 percent of the sample. Relative to all government awards in the GJF dataset, tax credits and abatements constitute 49.4 percent and 38.2 percent, respectively (untabulated). Over 2,000 of these tax subsidies include

data on the dollar value of the award, aggregating to over $11.1 billion in total. Approximately 86.1 percent of this aggregate dollar value relates to megadeals. Although we focus on the first observed subsidy in each county in empirical tests, Panel B also provides further information on all subsidies reported by counties with subsidy recipients. In total, 1,575 county-years report over 17,000 distinct tax subsidies.[14] Megadeals continue to reflect a disproportionate share of the dollar value of subsidies awarded – they represent approximately 74.4 percent ($22.1 billion) of the total $29.7 billion in tax subsidies. Figure 2 maps the number of subsidies in the sample by state.

Panel C shows the number of tax subsidies by year. The majority of the first tax subsidies we observe for the 588 distinct counties with tax subsidy recipients are in 2007, 2011, and 2013. For the sample of all county-years with tax subsidy recipients, we observe large and growing numbers of tax subsidies, with over 5,000 in 2014. A comparison of these two columns confirms that counties appear multiple times in the sample.[15] Table 3 presents the correlation matrix.

IV. Results

A. Main Results

Table 4 presents primary results of our hypothesis tests. Columns (1), (3), (5), (7), and (9) present results for regressing $Ln(Establishments)_{i,t+1}$, $Ln(Employees)_{i,t+1}$, $Ln(Wages)_{i,t+1}$, $(Employees/Population)_{i,t+1}$, and $(Wages/Population)_{i,t+1}$ on $PostTaxSubsidy_{i,t}$ and control variables. Columns (2), (4), (6), (8), and (10) repeat these analyses after including an indicator for specific years following tax subsidy grant. We observe a positive but statistically insignificant effect for $Ln(Establishments)_{i,t+1}$ across both specifications, meaning that subsidies are not

[14] We retain GJF tax subsidies with requisite county-level information as the starting point for our sample, which represents approximately 86 (72) percent of the total dollar value of tax (all) subsidies in GJF data.

[15] The effects we document could reflect not only the first tax subsidy observed in our sample period but also subsequent tax subsidies granted by the county. In future work, we intend to address this concern in two ways. We will separately estimate results for counties that have no or very few subsequent tax subsidies, or alternatively, control for the year of subsequent tax subsidies. We also intend to test the effects of tax subsidies using firm-specific data on establishments and employees, which would permit measurement of tax subsidy effects at the recipient firm.

associated with new facilities or offices within the county. Figure 1, Panel A demonstrates graphically that the number of establishments increases in counties with subsidy recipients, but that such increase also occurs within the matched control counties as well.

We observe positive and statistically significant effects when testing $Ln(Employees)_{i,t+1}$ and $Ln(Wages)_{i,t+1}$, suggesting that subsidies are associated with more employees and higher wages in the local jurisdictions. Based on Panels B and C of Figure 1, these effects appear to be attributable to both a slight increase in employees and wages in subsidy recipient counties and a more pronounced decrease in employees and wages in matched control counties.

In terms of economic magnitudes, the positive and significant coefficient of 0.0477 on *PostTaxSubsidy* in Column (3) suggests that counties experience a 4.7 percent increase in the number of employees following a first-time tax subsidy to a firm within the county, relative to matched control counties. Given that the average number of employees in subsidy counties in the year prior to the first tax subsidy is 26,337 (untabulated), this result translates to 1,256 additional employees. The positive and significant coefficient of 0.0793 on *PostTaxSubsidy* in Column (5) suggests that counties experience a 7.9 percent increase in wages following a first-time tax subsidy to a firm within the county, relative to matched control counties. Given that average aggregate wages in subsidy counties in the year prior to the first tax subsidy is $1.1 billion (untabulated), this result translates to $91.0 million in additional aggregate wages.

We also observe positive and statistically significant effects for $(Employees/Population)_{i,t+1}$ and $(Wages/Population)_{i,t+1}$ in Columns (7)-(10). The positive and significant coefficient of 0.0109 in Column (7) suggests that, relative to control counties, counties with tax subsidy recipients experience a 1.1 percentage point increase in per capita employment following the receipt of a first-time tax subsidy by a firm in the county. Relative to the sample

mean of per capita employment for the subsidy sample (0.275), this result represents an increase of 4.0 percent. Figure 1, Panel D suggests that this is driven both by an increase in per capita employment in treatment counties and a decline in per capita employment in control counties.

The positive and significant coefficient of 0.7816 in Column (9) suggests counties experience a $781.60 increase in annual per-capita income following a first-time tax subsidy to a firm within the county, relative to control counties. The graph in Figure 1, Panel E suggests that the effect is driven primarily by an increase in treatment counties.

Columns (4), (6), (8), and (10) provide more insight into the timing of these effects. Positive and significant coefficients on $PostTaxSubsidy_{i,(2\text{-}3)}$ and $PostTaxSubsidy_{i,(4\text{-}5)}$ in Columns (4) and (6) indicate that the employment and wage effects occur the second to fifth years following the subsidy. Positive and significant coefficients on $PostTaxSubsidy_{i,(2\text{-}3)}$, $PostTaxSubsidy_{i,(4\text{-}5)}$, and $PostTaxSubsidy_{i,(6+)}$ in Columns (8) and (10) indicate that it also takes two years for these effects to occur, but that they generally persist over time. The magnitude of statistically significant coefficients on the staggered post-subsidy indicator variables across Columns (4), (6), (8), and (10) are monotonically increasing over time, suggesting that the full economic impact of a tax subsidy on employment, wages, and per capita employment and wages lags the subsidy grant. However, we acknowledge that the increasing magnitude of the yearly indicators over time could also reflect subsequent tax subsidies granted to firms in the same county.

Because the control group is critical to testing these effects, we present results in Panel B using an alternative entropy-balanced matched sample that prioritizes – but does not force – matches within the same state via the inclusion of state fixed effects in the matching procedure.[16]

[16] Future work will employ one additional alternative approach to measure effects relative to a neighboring county, although this will result an even smaller sample.

This approach ensures balanced covariates between the treatment and matched control counties across observable characteristics.

Results presented in Panel B are similar to those presented in Panel A with the exception that we now also observe statistically significant increases in establishments. The positive and significant coefficient of 0.0065 on *PostTaxSubsidy* in Column (1) suggests that counties experience a 0.7 percent increase in the number of establishments following a first-time tax subsidy to a firm within the county, relative to matched control counties. Given that the average number of establishments in subsidy counties in the year prior to the first tax subsidy is 1,906 (untabulated), this result translates to 12.4 additional establishments.

The economic magnitudes of the employment, wage, and per capita employment and wage effects implied by estimates in Panel B are lower than those supported by Panel A, thereby providing a range of estimates across these two panels. The coefficient of 0.0189 on *PostTaxSubsidy* in Column (3) implies a 1.89 percent increase in the number of employees, or approximately 497.8 more employees. The positive and significant coefficient of 0.0164 on *PostTaxSubsidy* in Column (5) suggests that, relative to control counties, counties with tax subsidy recipients experience a 1.6 percent increase in wages, or $18.8 million in aggregate, following the receipt of a first-time tax subsidy by a firm in the county. The positive and significant coefficient of 0.0083 on *PostTaxSubsidy* in Column (7) suggests that, relative to control counties, counties with tax subsidy recipients experience a 0.83 percentage point increase in per capita employment following the receipt of a first-time tax subsidy by a firm in the county. Relative to the sample mean of per capita employment, this result represents an increase of 3.0 percent. The positive and significant coefficient of 0.5741 in Column (9) suggests counties experience a $574.1 increase in

annual per-capita income following a first-time tax subsidy to a firm within the county, relative to control counties.

Finally, because counties with subsidy recipient firms may be different from other counties across additional unobservable characteristics that neither propensity-score matching nor entropy-balancing captures, we also employ a third approach in untabulated tests that uses subsidy recipient counties as an alternate control sample. Following Fuest, Peichl, and Siegloch (2018), we restrict the sample to only those counties with first-time subsidy recipients within our sample period and employ a staggered difference-in-difference design. This design measures the effect of subsidies on business activity relative to the same counties prior to receiving the subsidy and other subsidy recipient counties that have not yet received a subsidy. We continue to find a positive and significant relation between subsidies and the number of establishments, employees, wages, and per capita employment in the first and second years following the award of a subsidy to a firm within the county, and these effects generally persist throughout the remaining sample period. We also find a positive and significant relation between subsidies and per capita wages, however these effects do not occur until the third year following the subsidy. As before, the magnitude of the coefficients on the staggered post-subsidy indicator variables increase monotonically over time. Thus, using three different benchmarking approaches, results described in this section demonstrate economically and statistically significant increases in business activity, measured with employment, wages, and per capita employment and wages. We also find some mixed evidence of increases in business establishments.

B. Results by Deal Size

We expect that the magnitude of the increase in economic activity following a tax subsidy is increasing in the dollar value of the tax subsidy awarded. We test the role of deal size in two ways, both presented in Table 5. First, given that megadeals constitute a disproportionately large

share of the dollar value of subsidies in our sample, we separately test the association between the incidence of these subsidies and local economic activity in Table 5, Panel A. We re-estimate Equation (1) after partitioning the sample into megadeal counties and their matched observations (n=1,200 county-years) versus all other observations (n=12,912).

We observe no significant relation between tax subsidies and number of establishments in either the megadeal or non-megadeal sub-sample, suggesting that subsidies are not associated with an increase in the number of offices and business locations in each of these groups. These results are consistent with the lack of relation between subsidies and number of establishments when using the propensity-matched control sample. Given that megadeals are at times given to lure a business to a jurisdiction, one possible explanation is that some megadeal recipients indeed open new establishments, but that spillover effects in the same jurisdiction are insufficient to result in a statistically significant effect when testing outcomes at the county level.

In Columns (3) and (4), we continue to observe a positive and statistically significant employment effect in both subsamples. The coefficients of 0.0454 in Column (3) and 0.0486 in Column (4) imply a 4.5 percent increase in number of employees following a megadeal compared to control counties, relative to a 4.9 percent increase for other tax subsidies. These coefficients are not statistically different from each other (t-stat = 0.21), but the descriptive statistics for these subsamples imply different economic magnitudes of 4,237.1 employees (990.7 employees) for megadeal (non-megadeal) counties. Similarly, in Columns (5) and (6) we continue to observe a positive and statistically significant wage effect in both subsamples. The coefficients of 0.0488 in Column (5) and 0.0819 in Column (6) imply a 4.9 percent increase in wages following a megadeal compared to control counties, relative to an 8.2 percent increase for other tax subsidies. These coefficients are also not statistically different from each other (t-stat = 0.80), but the differences in

the average aggregate wage across these counties imply different economic effects ranging from $71.8 million for non-mega deal counties to $222.2 million for megadeals.

Interestingly, we only observe a positive and significant relation between tax subsidies and per capita employment in the *non*-megadeal sub-sample (Column (8)). We estimate an increase in per capita employment of 1.2 percentage points following a tax subsidy relative to control counties, or 2.5 percent relative to the sample mean in the sub-sample of non-megadeals. Because the numerator in Columns (7)-(8) is the same as the unlogged values in Columns (3)-(4), these results suggest that although megadeals are associated with more jobs, they are also associated with population growth. The interpretation of results across these columns implies that population growth in megadeal counties is sufficiently large so as to fully negate any employment effects on a *per capita* basis.[17]

We observe positive and statistically significant effects for per capita wages in Columns (9)-(10). The coefficients of 0.4950 in Column (9) and 0.4589 in Column (1) imply that the increase in the level of wages in megadeal and non-megadeal counties outpaces any increases in the overall population following the receipt of a megadeal or other subsidy by a firm in the county.[18]

---

[17] Future tests will study population levels across megadeals and non-megadeal groups to determine whether the effects can be attributed to shifts in population across counties. We also intend to study differences in the levels of working age population to total population to determine whether the effects may be attributable to demographic changes.

[18] Although all megadeals contain tax credits or abatements that are large relative to the average non-megadeal tax credit or abatement in the sample, some megadeals are predominantly composed of other, non-tax types of subsidies such as cash grants and reimbursements for certain company activities (such as job training). We re-estimate Equation (1) after dropping nine megadeals for which the dollar value of tax subsidies and abatements does not exceed 50 percent of the total subsidy package. We find consistent results of the same sign across all columns, with slightly weaker megadeal results for wages (coefficient of 0.0472, t-stat = 1.51), per capita employment (coefficient = -0.0028, t=-0.61), and per capita wages (coefficient = 0.4548, t-stat = 1.36). We also re-estimate results after dropping two counties with megadeals that include no tax break in the package, finding consistent results across all columns. Further, we observe a greater number of establishments within the megadeal sample after excluding these two deals (coefficient = 0.0231, t-stat = 1.74).

We next evaluate the role of deal size for all tax subsidies other than megadeals in our sample. Table 5, Panel B reports results of re-estimating Equation (1) after partitioning the sample into quartiles based on the dollar value of the tax subsidies awarded. Because we separately examined megadeals in Panel A, we estimate these quartile regressions after dropping megadeals and after dropping subsidies without requisite data on monetary value, resulting in a smaller sample (n=9,048). For parsimony, we do not tabulate coefficients on control variables.

For establishments, we continue to observe no statistically significant effects across the different quartiles. Furthermore, we find that the coefficients are not statistically different from each other across each of these groups. Similarly, we find no statistically significant effect of subsidies on employment or per capita wages across these different quartiles, however we acknowledge that the reduced sample size of partitioning the non-megadeal sample into four groups could impair the power of these tests. For wages, we observe a positive and statistically significant effect in the top quartile. Across employment, wages, and per capita wages, we note that the point estimates increase with deal size, suggesting that the economic effects may be monotonically increasing in the amount of the subsidy. However, tests of differences in estimated coefficients across each quartile suggest that the coefficients are not statistically different from each other.

C. Results by State Transparency

Government officials, the media, and the public at large all serve as important monitoring mechanisms to hold governments and firms accountable for the efficient use of public funds that are awarded in the form of tax subsidies. However, states vary in the level of disclosures typically issued about firm-specific tax subsidies. For example, some states generally provide significant disclosures about the intended outcomes of a particular subsidy, whereas other states only provide

public information related to subsidy type and dollar value. To incorporate the potential monitoring role of the media and public, we re-estimate Equation (1) after partitioning the sample based on the relatively transparency of the state. We measure a state's subsidy transparency using GJF's ranking of states by the quality of disclosures provided about granted subsidies.[19] We define *Transparent* to be an indicator variable equal to one for the top ten states in our sample based on their GJF ranking (and zero otherwise). Transparent states include Colorado, Illinois, Indiana, Iowa, Kentucky, Michigan, Missouri, North Carolina, Texas, and Utah. We characterize the remaining states (i.e., the bottom 21 in our sample) as *Opaque*.

Table 6 presents results after partitioning the sample based on the relative transparency of a county's state. Consistent with disclosures allowing more effective monitoring, the effect of tax subsidies on a per capita basis is strongest in the states with more transparent subsidy disclosures. We continue to observe no statistically significant effect for establishments, regardless of the state level of transparency, in Columns (1) and (2). In Columns (3) and (4), we observe a positive and significant effect for the number of employees across both subsamples. However, in Column (7), we observe only a positive employment effect on a per capita basis in the transparent jurisdictions. The coefficient of 0.0191 implies an increase in per capita employment of 1.9 percentage points, representing a 3.6 percent increase over the sample mean per capita employment. In contrast, we observe no change in per capita employment in the more opaque jurisdictions, suggesting possible population shifts that result in no effective increase in the employment rate.

Across Columns (5)-(6), both coefficients for the level of wages are positive, although the effect for transparent states is not statistically significant (t=1.47). However, in Columns (9)-(10),

[19] In December 2010, GJF published a report called *Show Us the Subsidies*, which ranked all 50 states based on the transparency of the states' subsidy-related disclosures. Transparency is based on a number of factors including (among other things) the ease of accessing information about the subsidy, whether a job-creation number is disclosed, and whether estimated subsidy dollar values are disclosed.

we observe positive and statistically significant coefficients for both subsamples, with a larger coefficient for transparent states. One interpretation is that these state-level disclosures are an important mechanism to disseminate information about subsidy packages within these jurisdictions. These results suggest that better disclosure improves public monitoring, and therefore, the effectiveness of subsidies in stimulating local activity.

V. Conclusions and Future Work

We test to what extent firm-specific local tax abatements and subsidies are associated with greater local investment, employment, aggregate wages, and per capita employment and wages in counties with tax subsidy recipients. Further, we study whether the effectiveness of these subsidies in achieving improvements in local economic conditions varies with the size of the subsidy or the information environment of the jurisdiction. Using three different control groups to benchmark the economic outcomes of counties with subsidy recipients, we generally find robust evidence of a positive association between tax subsidies and employment, wages, and per capita employment and wages. These effects generally begin more than one year after tax subsidy grant and persist for at least five years. We find limited evidence of increases in the number of business establishments and that economic outcomes vary with the dollar value of the tax subsidy awarded. However, we acknowledge that these latter tests may suffer from low power. Finally, we find some evidence that better disclosures about these subsidies improve public monitoring, and therefore, the effectiveness of subsidies in stimulating local activity.

These results are subject to several important caveats. First, we acknowledge that counties with subsidy recipients likely differ from other counties. We address this selection issue by creating and using three alternative control samples. In two of the approaches, we use matching procedures to benchmark economic outcomes in counties with subsidy recipients against those in

other counties that are similar along numerous observable and unobservable characteristics. In our third approach, we use counties with subsidy recipients as the control group. However, concurrent literature suggests that the subsidy granting process reflects political connections between individuals and firms. As we cannot currently observe distinct firms (or their respective political connections) in a particular county or state, we are unable to model or control for these effects. Second, we limit the sample to counties without observable tax subsidies prior 2006. However, some results may be attributed to either unobserved subsidies granted prior to 2004 or to subsequent tax subsidies. Although we attempt to mitigate this concern in empirical tests, additional work at both the firm- and county-level could better identify and measure the effect of subsequent subsidies. Third, there are numerous possible outcomes we could examine, including local-area GDP, tax revenue collections, and costs of these tax policies such as increased traffic or housing prices. Given data availability, we focus our analysis on the investment and employment outcomes of number of establishments, number of employees, aggregate wages, and per capita rates of employment and wages. Finally, we cannot consider general equilibrium effects of tax subsidies on employment levels and growth. For example, a tax subsidy in one county may impact supply or demand for labor, capital, or outputs in another county, but we are unable to capture these effects in this paper. Nonetheless, we think this work is an important step in understanding the potential effects of these subsidies.

In future work, we intend to extend our analysis to better examine how the relation between tax subsidies and economic outcomes varies with deal size by directly testing subsidy magnitudes. We also plan to provide more granular analyses of the state and local information environments that are associated with the greatest local effects. Future tests may incorporate establishment-level or firm-level data to more precisely identify non-spillover effects or to evaluate the types of jobs

created from these subsidies. We look forward to additional future research that adds to and complements our understanding of the economic effects of these subsidies, particularly given their large and growing prevalence as a tool to compete for private sector activity.

References

Aobdia, D., A. Koester, and R. Petacchi. 2018. Political connections and government subsidies: State-level evidence. Working paper.

Bartik, T. 1985. Business location decisions in the United States: Estimates of the effects of unionization, taxes, and other characteristics of states. *Journal of Business and Economic Statistics* 3: 14-22.

Becsi, Z. 1996. Do state and local taxes affect relative state growth? *Economic Review* 81 (2), 18–36.

Bloom, C. 1955. *State and Local Tax Differentials and the Location of Manufacturing.* Iowa City: Bureau of Business and Economic Research.

Card, D. 1996. The effect of unions on the structure of wages: a longitudinal analysis. *Econometrica* 64 (4), 957-979.

Card, D. 1999. The causal effect of education on earnings. In *Handbook of Labor Economics*, ed. D. Card and O. Ashenfelter, Amsterdam: Elsevier, 1999.

Carroll, R. and M. Wasylenko. 1994. Do state business climates still matter? Evidence of a structural change. *National Tax Journal* 47: 19-37.

Carlton, D. 1979. Why new firms locate where they do: An econometric model. In *International Movements and Regional Growth,* ed. W. Wheaton, Washington, D.C.: Urban Institute: 13-50.

Carlton, D. 1983. The location and employment choice of new firms: An econometric model with discrete and continuous endogenous variables. *Review of Economics and Statistics* 65: 440-449.

Costello, A., Petacchi, R., and Weber, J. 2017. The Impact of Balanced Budget Restrictions on States' Fiscal Actions. *The Accounting Review* 92: 51-71.

Devereux, M., R. Griffith, and H. Simpson. 2007. Firm location decisions, regional grants and agglomeration externalities. *Journal of Public Economics* 91: 413-435.

Drake, M., R. Hess, J. Wilde, and B. Williams. 2018. The relevance and pricing of non-income tax relief. Working paper.

Dube, A., T. Lester, and M. Reich. 2010. Minimum wage effects across state borders: estimates using contiguous counties. *Review of Economics and Statistics* 92 (4), 945-964.

Fuest, C., Peichl, A., and Siegloch, S. 2018. Do Higher Corporate Taxes Reduce Wages? Micro Evidence from Germany. *American Economic Review* 108(2), 393-418.

Gale, W., A. Kruepkin, and K. Rueben. 2015. The relationship between taxes and growth at the state level: New evidence. *National Tax Journal* 68: 919-942.

Giroud, X. and Rauh, J. State taxation and the reallocation of business activity: Evidence from establishment-level data. *Journal of Political Economy*, forthcoming.

Goolsbee, A. and Maydew, E. 2000. Coveting Thy Neighbor's Manufacturing: The Dilemma of State Income Apportionment, *Journal of Public Economics* 75, 125-143.

Grubert, H. and Mutti, 2000. Do Taxes Influence Where U.S. Corporations Invest? *National Tax Journal* 53, 825-839.

Hassett, K., and G. Hubbard. "Tax policy and business investment." In: Auerbach, A., Feldstein, M. (Eds.), *Handbook of Public Economics*, vol. 3. Elsevier Science, Amsterdam (2002): 1294–1343.

Hassett, K., and K. Newmark. "Taxation and business behavior: a review of the recent literature." In: Diamond, J., Zodrow, G. (Eds.), *Fundamental Tax Reform: Issues, Choices and Implications.* MIT Press, Cambridge (2008): 191–214.

Helms, J. 1985. The effect of state and local taxes on economic growth: A time-series cross section approach. *Review of Economics and Statistics* 32: 292-312.

Hines, J. 1997. Altered states: Taxes and the location of foreign direct investment in America. *American Economic Review* 86: 1076-1094.

Hines, J. and E.M. Rice. 1994. Fiscal paradise: Foreign tax havens and American business. *Quarterly Journal of Economics* February: 149-182.

Holcombe, R.G. and D.J. Lacombe. 2004. The effect of state income taxes on per capita income growth. *Public Finance Review* 32: 292-312.

Leroy, G. 2018. Public auction, private dealings: Will Amazon's HQ2 veer to secrecy create a missed opportunity for inclusive, accountable development?

Lester, R. Made in the U.S.A.? A study of firm responses to domestic production incentives. Working paper.

Ljunqvist, A. and M. Smolyansky. 2015. To cut or not to cut? On the impact of corporate taxes on employment and income. Working paper.

Mast, E. 2018. Race to the bottom? Local tax break competition and business location. Working paper.

Mattera, P., Cafcas, T., McIlvaine, L., Seifter, A., and Tarczynska, K. 2012. Money Back Guarantee for Taxpayers: Clawbacks and Other Enforcement Safeguards in State Economic Development Subsidy Program. Accessed at http://www.goodjobsfirst.org/sites/default /files/docs/pdf/moneyback_0.pdf.

Mofidi, A. and J. Stone. 1990. Do state and local taxes affect economic growth? *Review of Economics and Statistics* 72: 686-691.

Neumark and Wascher, 2007

Papke, L. 1991. Interstate business tax differentials and new firm location. *Journal of Public Economics* 45: 47-68.

Pjesky, R. 2006. What do we know about taxes and state economic development? A replication and extension of five key studies. *Journal of Economics* 32: 25-40.

Raghunandan, A. 2018. Government Subsidies and Corporate Fraud. *Working paper.*

Reed, R. 2008. The robust relation between taxes and U.S. state income growth. *National Tax Journal* 51: 57-80.

Serrato, J.C., and O. Zidar. 2016. "Who Benefits from State Corporate Tax Cuts? A Local Labor Markets Approach with Heterogenous Firms." *American Economic Review*, forthcoming.

Stevenson, B. and J. Wolfers. 2006. Bargaining in the shadow of the law: Divorce laws and family distress. *Quarterly Journal of Economics* February 2006: 267-288.

Thompson, W.R. and J.M. Mattila. 1959. *An Econometric Model of Postwar State Industrial Development*. Detroit: Wayne State University Press.

Vedder, R. 1996. Taxation and economic growth: Lessons for Oklahoma. Office of State Finance, Oklahoma City, OK.

Appendix A
Variable Definitions

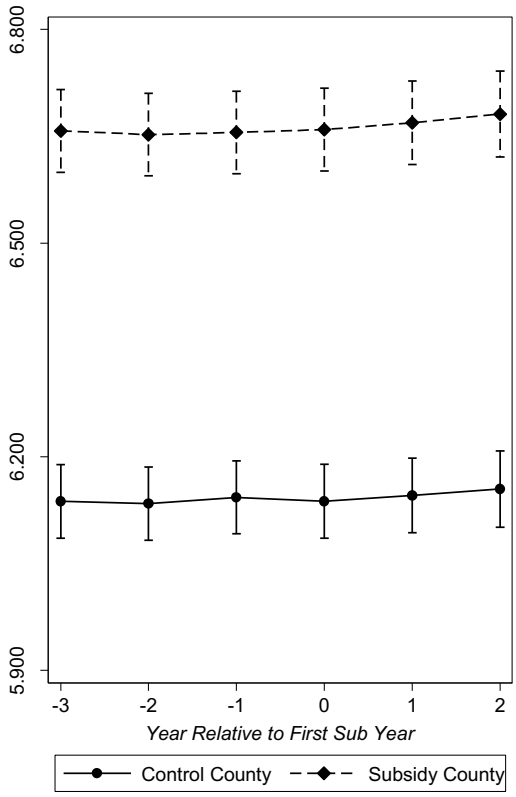
We construct variables using data from the U.S. Census, Good Jobs First, and Compustat. We include data codes from Compustat where applicable.

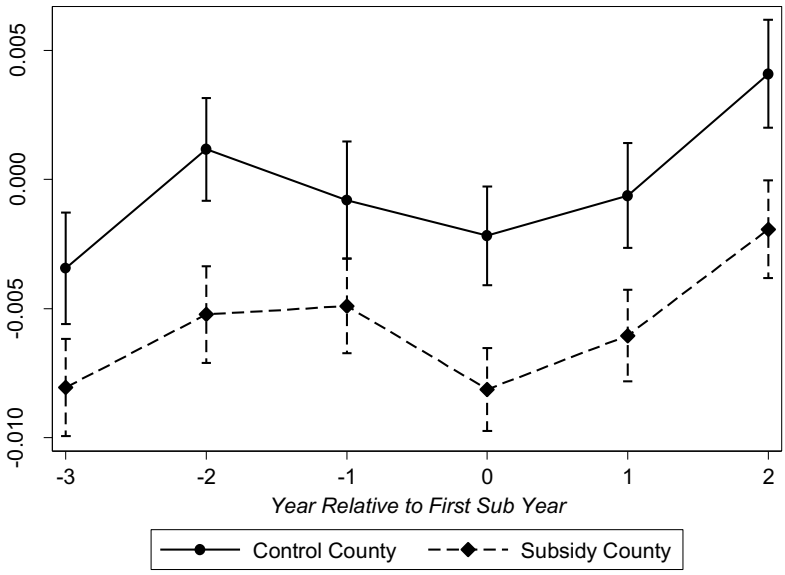
Variable	Definition
Dependent Variables	
$Establishments_{i,t+1}$	Total number of private-sector business establishments in county *i* in year *t*+1; computed by the Bureau of Labor Statistics (BLS) as the average number of active establishments at the end of each quarter.
$Employees_{i,t+1}$	Total number of private-sector employees working in county *i* in year *t*+1; computed by BLS as the average number of private-sector employees at the end of each quarter.
$Wages_{i,t+1}$ *($M)*	Total wages paid to all private-sector employees working in county *i* in year *t*+1.
$(Employees/Population)_{i,t+1}$	$Employees_{i,t+1}$ divided by population ages 25-64 of county *i* in year *t*+1. County population data is computed by the US Census Bureau.
$(Wages/Population)_{i,t+1}$	$Wages_{i,t+1}$ divided by total population of ages 25-64 of county *i* in year *t*+1. County population data is computed by the US Census Bureau.
Subsidy & Control Variables	
$FirstTaxSubsidy_{i,t}$	Equals 1 in the year of the first subsidy received by a firm in county *i* 2006-2015 and 0 otherwise.
$PostTaxSubsidy_{i,t}$	Equals 1 for years including and following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,t}$	Equals 1 for years including and following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(0-1)}$	Equals 1 for the year of and the year following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(2-3)}$	Equals 1 for the second and third years following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(4-5)}$	Equals 1 for the fourth and fifth years following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(6+)}$	Equals 1 for years including and after the sixth year following the first subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$Rural_{i,t}$	Equals 1 if county *i* is classified as "rural" by the Consumer Financial Protection Bureau and 0 otherwise.
$Ln(Population)_{i,t}$	Log of total population of county *i* in year *t*+1. County population data is computed by the US Census Bureau.
$MinWage_{i,t}$	Minimum wage applicable to county *i* in year *t*. In most cases this is the state-mandated minimum wage or federal minimum wage; at varying points between 2004 and 2015, seven counties introduced their own minimum wage that supersede the state's minimum wage.
$Ln(GDP)_{j,t}$	Log of total GDP in year *t* for state *j*.
$\%Educ_{j,t}$	Percentage of people in state *j* with at least a four-year college degree in year *t*.
$\%Union_{j,t}$	Percentage of private-sector employees in state *j* who are union members in year *t*.
$CorpTaxRate\%_{0j,t}$	Top marginal corporate state tax rate for state *j* in year *t*.
$Ln(UIContrib)_{j,t}$	The log of top unemployment insurance (UI) tax rate multiplied by the maximum base wage (i.e., the maximum amount of wages taxable for UI purposes) for state *j* in year *t*.

PropertyTax$_{j,t}$	Ratio of total property taxes (collected by state and local governments) to total revenues (collected by state and local governments) for state *j* in year *t*.
PersonalTaxRate%$_{j,t}$	Top marginal personal *state* income tax rate for state *j* in year *t*.
TaxIncentivesIndex$_{j,t}$	Index of tax incentives potentially available to businesses that locate in/relocate to state *j* in year *t* as compiled by *Site Selection* magazine. There are 33 possible incentives; this variable adds one index point for each incentive (e.g., if state *j* could offer 22 incentives in year *t*, this variable equals 22).
SalesTaxRate%$_{j,t}$	Sales tax rate assessed by state *j* in year *t* (does not include any additional sales tax that may be collected by county *i*).
Transparent	Equals 1 if the county is in a state ranked in the top 10 of disclosure quality by GJF, and 0 otherwise.
Opaque	Equals 1 if the county is in a state ranked in the bottom 40 of disclosure quality by GJF, and 0 otherwise.

Figure 1
Parallel Trends

Panel A: Establishments

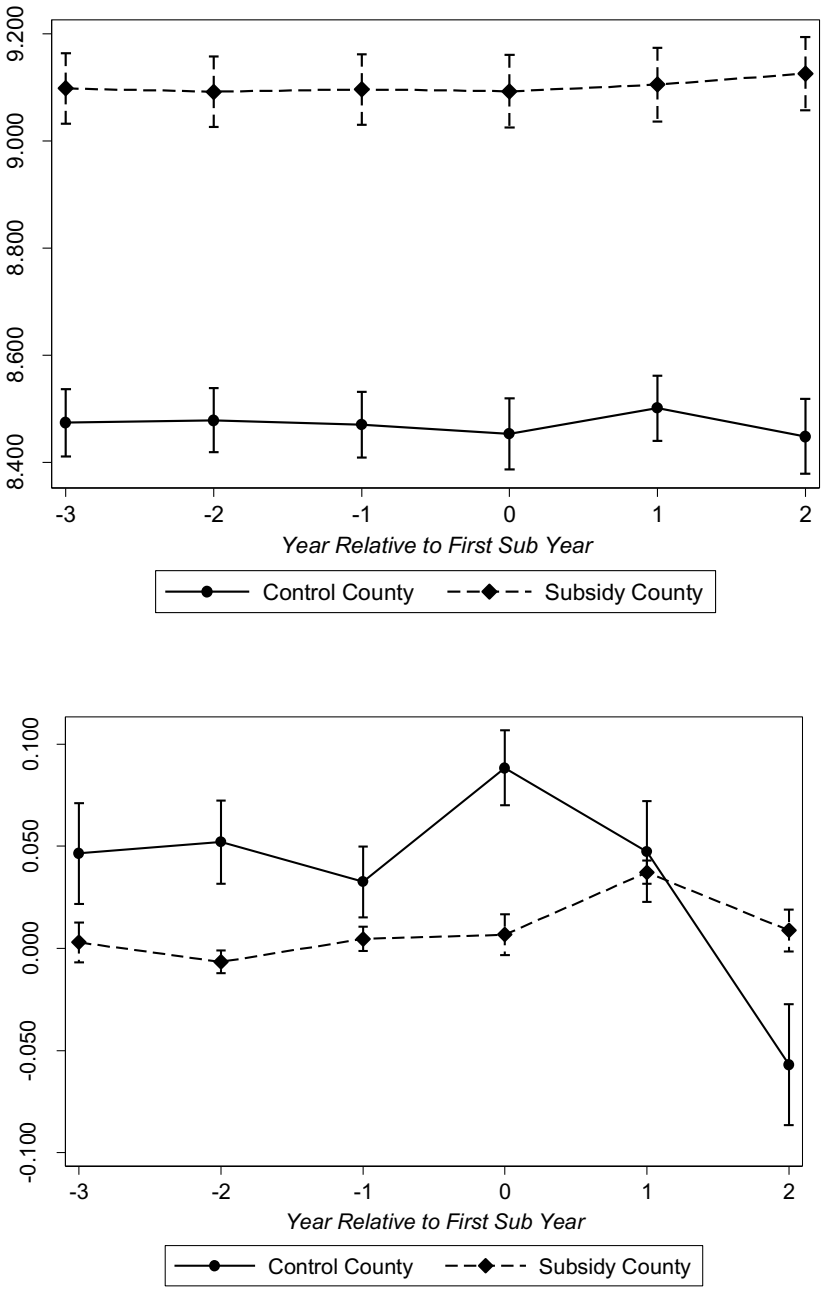




The figures in this panel demonstrate pre-period trends in the log of the number of local establishments. The top graph maps the raw value of *Ln(Establishments)*, whereas the bottom graph plots residual values from regressing *Ln(Establishments)* on control variables and fixed effects.

Figure 1 (continued)
Parallel Trends

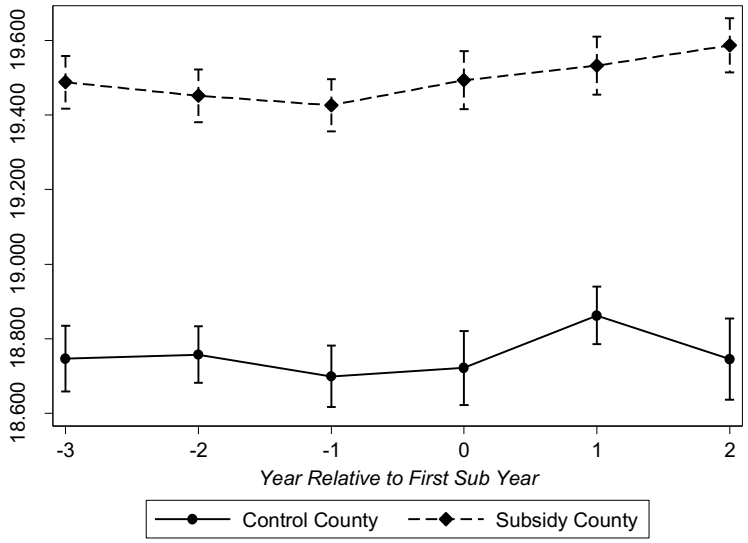
Panel B: Log(Employees)

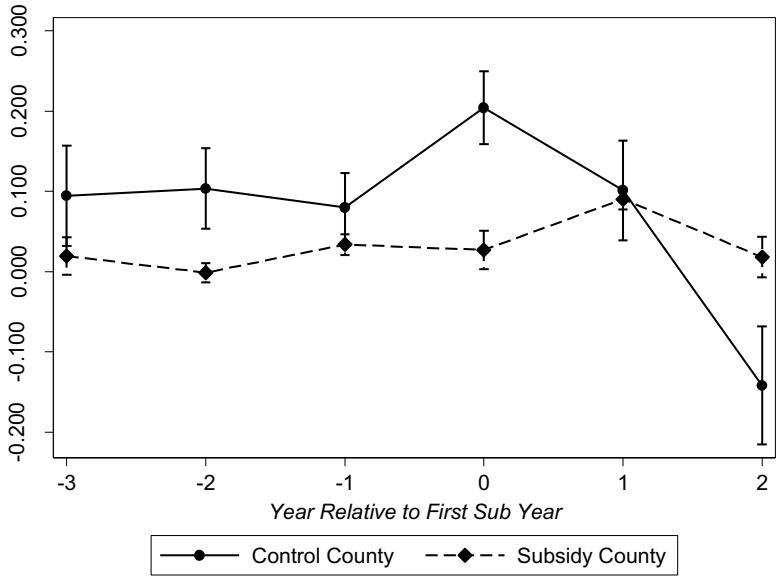


The figures in this panel demonstrate pre-period trends in the log of the number of total employees at the county-level. The top graph maps the raw value of *Ln(Employees)*, whereas the bottom graph plots residual values from regressing *Ln(Employees)* on control variables and fixed effects.

Figure 1 (continued)
Parallel Trends

Panel C: Log(Wages)

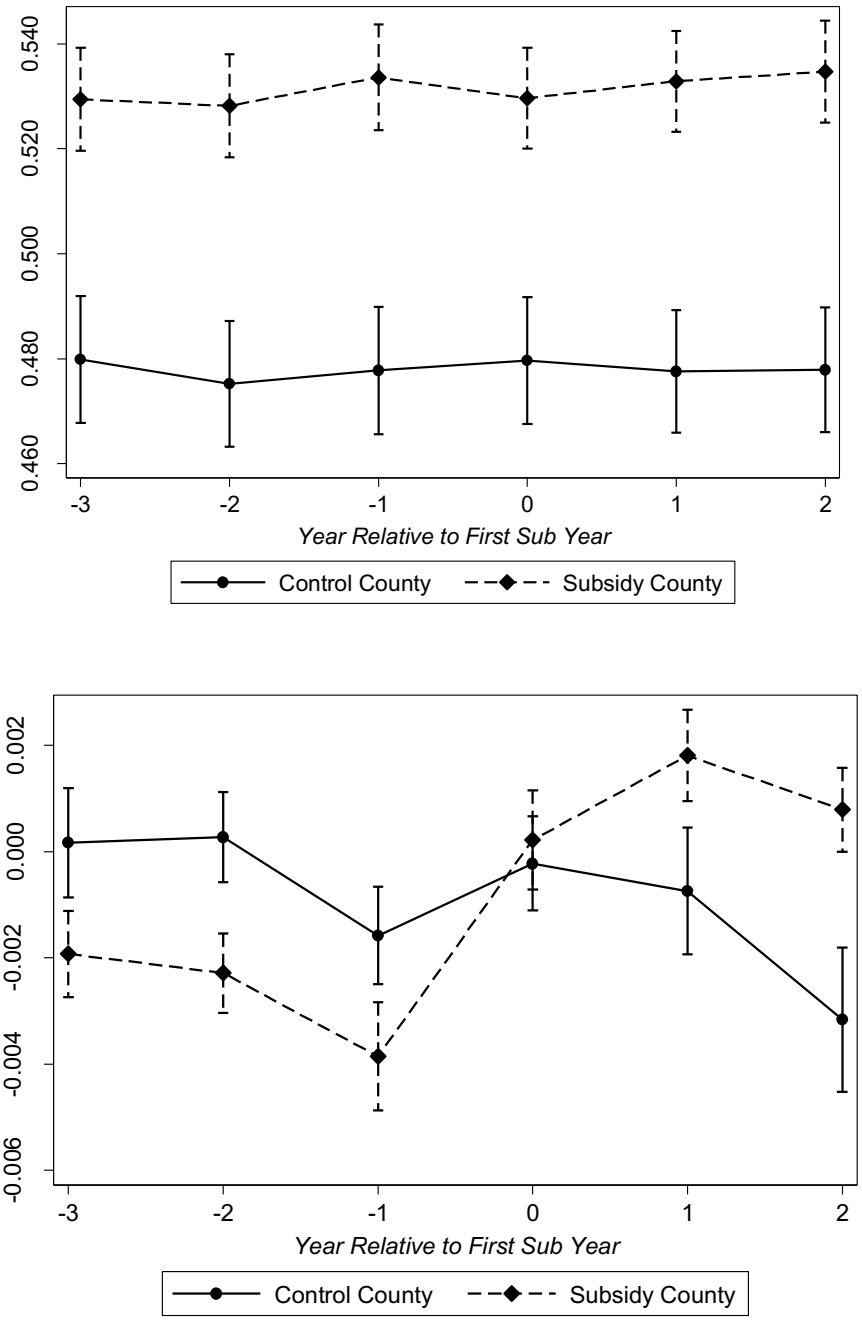




The figures in this panel demonstrate pre-period trends in the log of aggregate wages at the county-level. The top graph maps the raw value of *Ln(Wages)*, whereas the bottom graph plots residual values from regressing *Ln(Wages)* on control variables and fixed effects.

Figure 1 (continued)
Parallel Trends

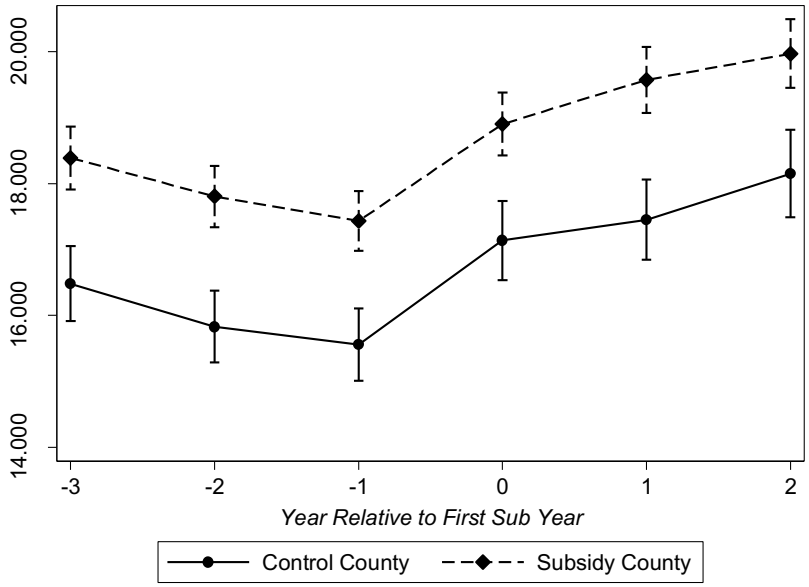
Panel D: Employees/Population

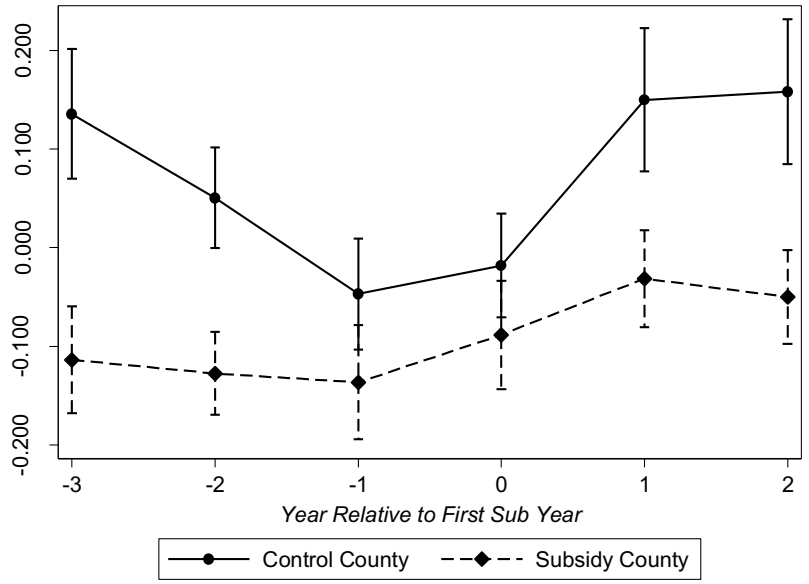


The figures in this panel demonstrate pre-period trends in the ratio of employees to the working age population at the county-level. The top graph maps the raw value of *Employees/Population*, whereas the bottom graph plots residual values from regressing *Employees/Population* on control variables and fixed effects.

Figure 1 (continued)
Parallel Trends

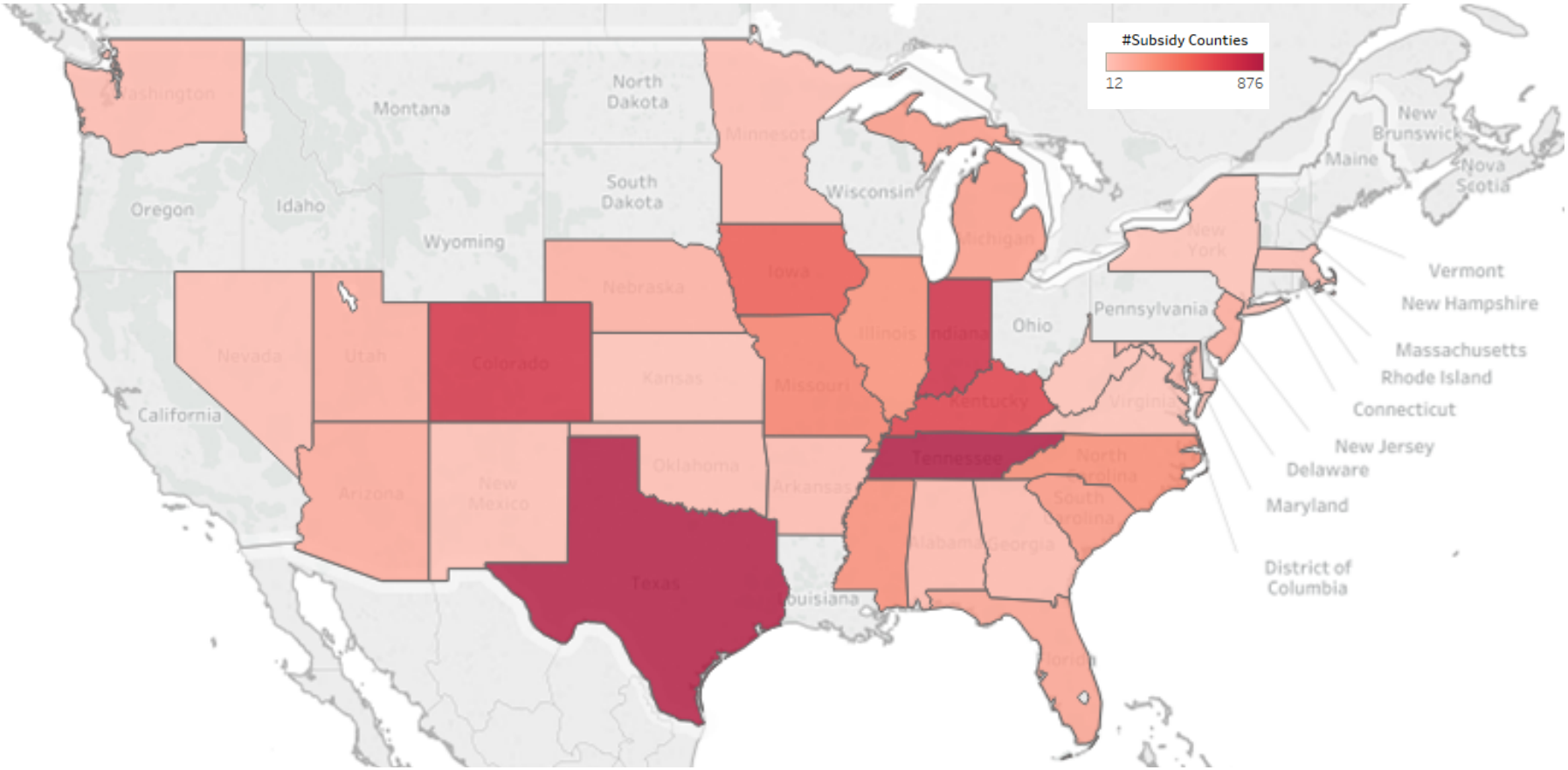
Panel E: Wages/Population





The figures in this panel demonstrate pre-period trends in the ratio of wages to the working age population at the county-level. The top graph maps the raw value of *Wages/Population*, whereas the bottom graph plots residual values from regressing *Wages/Population* on control variables and fixed effects.

Figure 2
Map of Number of Subsidy Counties within each State



This figure presents the distribution of same subsidies across states. We aggregate county-level subsidies to the state level for presentation purposes in this figure. We exclude Alaska, Delaware, Hawaii, Idaho, North Dakota, New Hampshire, Pennsylvania, Rhode Island, South Dakota, Wyoming, and Washington, D.C. because the subsidy data for those jurisdictions are incomplete. Additionally, we omit California because all counties with subsidies also had subsidies in the 2004 and 2005 period, and we cannot study counties in Maine and Vermont because we do not observe any tax subsidies in these jurisdictions after 2005. Connecticut, Louisiana, Ohio, Oregon, and Wisconsin do not have a sufficient number of non-subsidy counties for possible matches, and we exclude Montana because all counties provide non-tax subsidies that are not the focus of this paper.

Table 1
Sample Selection

Panel A: Data requirements

	County-Years		Distinct Counties	
	Obs. Dropped	**Obs. Remaining**	**Obs. Dropped**	**Obs. Remaining**
Initial sample		38,352		3,208
Less: observations for merged or split counties	(64)	38,288	(12)	3,196
Less: Counties in states with < 3 years of nonzero subsidy data or counties in states without BLS data	(3,812)	34,476	(323)	2,873
Less: Counties with tax subsidy recipients in 2004 or 2005	(5,256)	29,220	(438)	2,435
Less: Observations without a tax subsidy	(19,824)	9,396	(1,652)	783
Less: Counties without a matched control	(2,340)	7,056	(195)	588
Matched observations (w. replacement)	7,056	7,056	199	199
Total county-years (counties)		**14,112**		**787**

Panel B: First-Stage Propensity Score Matching Model

	FirstTaxSubsidy
$Rural_{i,t}$	-0.0839**
	[-2.02]
$Ln(Population)_{i,t}$	0.6467***
	[35.52]
$MinWage_{i,t}$	-0.1417***
	[-6.13]
$Ln(GDP)_{j,t}$	-0.0479
	[-1.41]
$\%Educ_{j,t}$	-0.0887***
	[-16.28]
$\%Union_{j,t}$	0.0673***
	[10.83]
$CorpTaxRate\%_{0j,t}$	0.1343***
	[15.95]
$Ln(UIContrib)_{j,t}$	-0.2132***
	[-5.25]
$PropertyTax_{j,t}$	4.1863***
	[7.35]
$PersonalTaxRate\%_{0j,t}$	-0.1372***
	[-15.73]
$TaxIncentivesIndex_{j,t}$	-0.0295***
	[-5.47]
$SalesTaxRate\%_{0j,t}$	-0.0006
	[-0.07]
$Ln(Establishments)_{i,t-3\_t-1}$	-3.3445***
	[-10.22]
$Ln(Employees)_{i,t-3\_t-1}$	1.0072***
	[3.41]
$Ln(Wages)_{i,t-3\_t-1}$	-0.3504***
	[-3.05]
Year FE	Y
Observations	18,370
Pseudo R-squared	0.1614

Table 1 (cont'd)

Panel C: Differences in treatment and control counties

	Distinct Tax Subsidy Counties	Matched Control Counties	Difference	t-stat
Ln(Establishments)$_{i,t-1}$	6.661	6.143	0.518	-4.37
Ln(Employees)$_{i,t+1}$	9.098	8.497	0.601	-5.13
Ln(Wages)$_{i,t+1}$	19.526	18.858	0.668	-4.80
(Employees/Population)$_{i,t+1}$	0.275	0.255	0.02	-0.70
(Wages/Population)$_{i,t+1}$	10.175	9.42	0.755	-0.58

Panel D: County-year observations by state

State	#Subsidy County-Year Observations	# Control County-Year Observations	Total County-year Observations
Alabama	60	60	120
Arizona	132	132	264
Arkansas	48	48	96
Colorado	696	696	1,392
Florida	168	168	336
Georgia	60	60	120
Illinois	264	264	528
Indiana	732	732	1,464
Iowa	504	504	1,008
Kansas	12	12	24
Kentucky	624	624	1,248
Maryland	72	72	144
Massachusetts	36	36	72
Michigan	204	204	408
Minnesota	48	48	96
Mississippi	276	276	552
Missouri	336	336	672
Nebraska	108	108	216
Nevada	36	36	72
New Jersey	132	132	264
New Mexico	24	24	48
New York	12	12	24
North Carolina	288	288	576
Oklahoma	36	36	72
South Carolina	228	228	456
Tennessee	876	876	1,752
Texas	864	864	1,728
Utah	108	108	216

Virginia	12	12	24
Washington	48	48	96
West Virginia	12	12	24
Total County-Year Observations	**7,056**	**7,056**	**14,112**

This table presents more information about our sample selection. Panel A outlines the sample selection process. We construct a balanced panel of county-year observations from 2006 to 2015, excluding counties that merged or split during this period. The treatment sample is composed of 7,056 county-years (representing 588 distinct counties) with firms that receive at least one subsidy during the sample period but no subsidies in 2004 or 2005. We match each treatment county to a control county that never receives a subsidy 2004-2015 for a total sample of 14,112 county-year observations. We use propensity score matching with replacement to pair treatment counties with control counties in the same state based on population, whether the county is rural or urban, and three-year trends in aggregate number of establishments, number of employees, and wages. Panel B presents results from the propensity score matching model. Panel C presents tests of differences in means for the dependent variables across treatment versus control counties after matching. Because control variables in our second-stage tests of investment and employment outcomes are all measured at the state-level, matching within state ensures that these covariates are balanced across the groups. Panel D presents the distribution of county-year observations across 31 state jurisdictions. We exclude observations from Alaska, Delaware, Hawaii, Idaho, North Dakota, New Hampshire, Pennsylvania, Rhode Island, South Dakota, Wyoming, and Washington, D.C. because the subsidy data for those jurisdictions are incomplete. Additionally, we omit California because all counties with subsidies also had subsidies in the 2004 and 2005 period, and we cannot study counties in Maine and Vermont because we do not observe any tax subsidies in these jurisdictions after 2005. We exclude Connecticut, Louisiana, Ohio, Oregon, and Wisconsin because we are unable to find a sufficient number of suitable matched control counties within these states. Finally, Montana counties provide non-tax subsidies that are not the focus of this paper. We define all variables in Appendix A.

Table 2
Descriptive Statistics

Panel A: Sample descriptive statistics

Variables	Mean	Median	Std. Dev.	P25	P75
Establishments$_{i,t+1}$	1,925.81	479.00	5,361.43	236.00	1,378.00
Ln(Establishments)$_{i,t+1}$	6.41	6.17	1.36	5.47	7.23
Employees$_{i,t+1}$	26,595.02	4,953.00	81,613.53	2,378.00	17,200.00
Ln(Employees)$_{i,t+1}$	8.77	8.51	1.67	7.77	9.75
Wages$_{i,t+1}$ *($M)*	1,190.03	164.80	4,490.00	79.47	618.30
Ln(Wages)$_{i,t+1}$	19.12	18.92	2.23	18.19	20.24
(Employees/Population)$_{i,t+1}$	0.51	0.46	0.27	0.30	0.65
(Wages/Population)$_{i,t+1}$	18.51	14.60	13.79	9.36	23.39
PostTaxSubsidy$_{i,t}$	0.22	0.00	0.41	0.00	0.00
Control Variables					
MinWage$_{i,t}$	6.57	7.25	1.02	5.15	7.25
Ln(GDP)$_{j,t}$	12.60	12.48	0.73	12.08	12.92
%Educ$_{j,t}$	26.37	25.30	4.91	23.00	27.80
%Union$_{j,t}$	5.67	5.00	3.02	3.00	7.60
CorpTaxRate%$_{0j,t}$	5.92	6.25	2.71	4.95	7.50
Ln(UIContrib)$_{j,t}$	11.29	11.25	0.48	10.91	11.50
PropertyTax$_{j,t}$	0.12	0.12	0.04	0.09	0.14
PersonalTaxRate%$_{0j,t}$	3.87	4.68	2.73	-	6.04
TaxIncentivesIndex$_{j,t}$	25.84	27.00	3.14	24.00	28.00
SalesTaxRate%$_{0j,t}$	4.29	6.00	2.66	1.49	6.25

Panel B: Types of Subsidies

	Sample of First Subsidy County-Years (n=588)				**Sample of All Subsidy County-Years (n=1,575)**			
	# Subs.	% of Total Subs.	# Subs. w. $Value	Total $Value of Subs. ($M)	Number of Subs.	% of Total Subs.	# Subs. w. $Value	Total $Value of Subs. ($M)
Tax Credits	1,646	57.6%	1,516	$1,054	10,111	57.6%	9,796	$6,220
Tax Abatements	1,163	40.7%	524	$495	7,346	41.9%	3,606	$1,382
Megadeals	50	1.7%	48	$9,567	83	0.5%	78	$22,121
Total	**2,859**	**100.0%**	**2,088**	**$11,115**	**17,540**	**100.0%**	**13,480**	**$29,722**

Table 2 (cont'd)

Panel C: Subsidies by year

Year	Sample of First Subsidy County-Years (n=588)	Sample of All Subsidy County-Years (n=1,575)
2006	136	136
2007	590	673
2008	66	636
2009	52	699
2010	60	651
2011	529	1,486
2012	64	763
2013	1,320	3,431
2014	34	5,825
2015	8	3,240
Total	**2,859**	**17,540**

This table describes the sample of county-year observations and tax subsidies studied. Panel A presents descriptive statistics for the dependent variables, variable of interest $PostTaxSubsidy_{i,t}$, and control variables for the sample of 14,112 county-year observations. Panel B provides information on sample composition by type of tax subsidy included in the sample of 588 county-years for which we observe the first post-2005 tax subsidy and for all 1,575 county-years with tax subsidies. *Tax Credits* are dollar value awards that reduce a firm's tax liability dollar-for-dollar, and *Tax Abatements* provide a percentage reduction of a firm's tax liability. *Megadeals* are certain subsidies that span multiple categories of subsidy type and are larger than the average subsidy. In addition to the number and percentage of observations by subsidy type, we present the number of observations reporting the dollar value of the tax subsidy and, for those observations, the total magnitude of tax subsidies awarded. Panel C presents the total number of tax subsidies by year. We define all variables in Appendix A.

Table 3
Correlations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
(1) *Ln(Establishments)*$_{i,t-1}$		**0.92**	**0.77**	**0.36**	**0.41**	**0.13**	**0.09**	**0.05**	**0.26**	**0.03**	**0.10**	**0.22**	**0.11**	**0.18**	**-0.15**	**-0.04**
(2) *Ln(Employees)*$_{i,t+1}$	**0.95**		**0.94**	**0.49**	**0.51**	**0.14**	**0.06**	**0.04**	**0.22**	**0.05**	**0.12**	**0.18**	**0.08**	**0.12**	**-0.07**	**-0.04**
(3) *Ln(Wages)*$_{i,t+1}$	**0.93**	**0.99**		**0.46**	**0.49**	**0.14**	**0.07**	**0.06**	**0.21**	**0.06**	**0.09**	**0.16**	**0.09**	**0.06**	-0.01	**-0.08**
(4) *(Employees/Population)*$_{i,t+1}$	**0.58**	**0.70**	**0.72**		**0.90**	**0.04**	**0.04**	0.00	**0.41**	**0.04**	-0.01	**0.05**	**0.15**	**0.12**	**-0.04**	**-0.13**
(5) *(Wages/Population)*$_{i,t+1}$	**0.55**	**0.68**	**0.74**	**0.95**		**0.09**	**0.15**	**0.05**	**0.45**	0.01	**-0.05**	**0.08**	**0.17**	**0.08**	**-0.04**	**-0.19**
(6) *PostTaxSubsidy*$_{i,t}$	**0.13**	**0.15**	**0.17**	**0.11**	**0.18**		**0.35**	**0.14**	0.01	0.00	**-0.05**	**0.09**	**0.08**	**-0.02**	**0.03**	**-0.27**
(7) *MinWage*$_{i,t}$	**0.06**	**0.05**	**0.10**	**0.05**	**0.16**	**0.34**		**0.18**	**0.35**	**0.05**	-0.01	**0.31**	**0.25**	**0.03**	**-0.06**	**-0.46**
(8) *Ln(GDP)*$_{j,t}$	**0.10**	**0.09**	**0.10**	**0.05**	**0.09**	**0.16**	**0.26**		**0.26**	**-0.14**	**-0.53**	**-0.12**	**0.66**	**-0.53**	**0.14**	**-0.07**
(9) *%Educ*$_{j,t}$	**0.26**	**0.24**	**0.26**	**0.33**	**0.34**	**0.07**	**0.41**	**0.46**		**0.04**	**-0.09**	**0.18**	**0.50**	**0.16**	**-0.24**	**-0.38**
(10) *%Union*$_{j,t}$	**0.02**	0.01	0.00	**0.11**	**0.08**	-0.01	**0.05**	**-0.15**	**-0.06**		**0.32**	**0.16**	**0.11**	**0.30**	**0.16**	**0.04**
(11) *CorpTaxRate*$_{j,t}$	**0.14**	**0.14**	**0.13**	**0.02**	0.01	**-0.03**	**-0.05**	**-0.27**	**-0.20**	**0.41**		**0.25**	**-0.23**	**0.49**	**0.13**	**0.03**
(12) *Ln(UIContrib)*$_{j,t}$	**0.19**	**0.15**	**0.15**	**0.07**	**0.09**	**0.11**	**0.33**	**-0.06**	**0.19**	**0.16**	**0.23**		**0.03**	**0.32**	**-0.15**	**-0.19**
(13) *PropertyTax*$_{j,t}$	**0.10**	**0.07**	**0.08**	**0.23**	**0.23**	**0.08**	**0.25**	**0.62**	**0.60**	**0.07**	**-0.16**	**-0.06**		**-0.15**	**0.10**	**-0.09**
(14) *PersonalTaxRate*$_{j,t}$	**0.15**	**0.13**	**0.12**	**0.08**	**0.04**	-0.01	0.00	**-0.50**	**0.03**	**0.22**	**0.33**	**0.39**	**-0.25**		**-0.23**	**-0.14**
(15) *TaxIncentivesIndex*$_{j,t}$	**-0.08**	**-0.08**	**-0.08**	**0.02**	**0.02**	**0.04**	**-0.13**	**0.08**	**-0.31**	**0.16**	**0.26**	**-0.12**	**0.15**	**-0.20**		**0.09**
(16) *SalesTaxRate*$_{j,t}$	**-0.07**	**-0.06**	**-0.09**	**-0.14**	**-0.17**	**-0.20**	**-0.38**	-0.02	**-0.46**	**-0.02**	**0.10**	**-0.22**	**-0.11**	**-0.32**	**0.19**	

This table reports correlations for dependent and independent variables. We report Pearson coefficients above the diagonal and Spearman coefficients below the diagonal. Numbers in bold indicate statistical significance at the 5% level. We define all variables in Appendix A.

Table 4

Relation between Tax Subsidies and Local Activity

Panel A: Propensity Score Matching within state

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostTaxSubsidy$_{i,t}$	**0.0072**		**0.0477*****		**0.0793****		**0.0109****		**0.7816****	
	[1.01]		**[2.58]**		**[2.10]**		**[1.97]**		**[2.10]**	
PostTaxSubsidy$_{i,(0-1)}$		**-0.0018**		**0.0488**		**0.0963**		**0.0081**		**0.4124**
		[-0.26]		**[1.53]**		**[1.30]**		**[1.49]**		**[1.18]**
PostTaxSubsidy$_{i,(2-3)}$		**0.0118**		**0.0543*****		**0.0853****		**0.0149****		**1.1738****
		[1.31]		**[2.67]**		**[2.10]**		**[2.01]**		**[2.42]**
PostTaxSubsidy$_{i,(4-5)}$		**0.0143**		**0.0656****		**0.0989***		**0.0183****		**1.5023****
		[1.20]		**[2.51]**		**[1.87]**		**[1.99]**		**[2.45]**
PostTaxSubsidy$_{i,(6+)}$		**0.0191**		**0.0443**		**0.0157**		**0.0241****		**2.0135*****
		[1.19]		**[0.98]**		**[0.14]**		**[2.13]**		**[2.71]**
MinWage$_{i,t}$	-0.0019	-0.0022	0.0016	0.0014	0.0119	0.0118	-0.0017	-0.0018	0.2195	0.2090
	[-0.62]	[-0.70]	[0.14]	[0.12]	[0.47]	[0.47]	[-0.29]	[-0.30]	[1.04]	[0.99]
Ln(GDP)$_{j,t}$	0.3599***	0.3573***	0.9602***	0.9548***	1.9108**	1.9026**	0.2313***	0.2298***	23.1641***	22.9905***
	[6.95]	[6.90]	[2.91]	[2.89]	[2.39]	[2.37]	[4.60]	[4.58]	[6.43]	[6.42]
%Educ$_{j,t}$	-0.0050*	-0.0050*	0.0019	0.0018	0.0153	0.0155	0.0007	0.0006	-0.0267	-0.0320
	[-1.90]	[-1.90]	[0.18]	[0.17]	[0.55]	[0.55]	[0.41]	[0.36]	[-0.38]	[-0.45]
%Union$_{j,t}$	0.0094***	0.0097***	-0.0189	-0.0188	-0.0428	-0.0432	-0.0050**	-0.0048**	-0.2847*	-0.2657*
	[3.88]	[4.02]	[-1.63]	[-1.56]	[-1.55]	[-1.50]	[-2.29]	[-2.25]	[-1.81]	[-1.73]
CorpTaxRate$_{j,t}$	-0.0010	-0.0008	-0.0053	-0.0055	-0.0095	-0.0106	0.0000	0.0002	0.0281	0.0436
	[-0.69]	[-0.53]	[-1.09]	[-1.09]	[-0.88]	[-0.94]	[0.03]	[0.14]	[0.39]	[0.61]
Ln(UIContrib)$_{j,t}$	-0.0343**	-0.0335**	-0.0453	-0.0468	-0.1004	-0.1043	-0.0206*	-0.0205*	-0.7609	-0.7315
	[-2.12]	[-2.06]	[-1.12]	[-1.13]	[-1.27]	[-1.27]	[-1.92]	[-1.94]	[-1.24]	[-1.22]
PropertyTax$_{j,t}$	0.1014	0.1068	0.2289	0.2204	0.0719	0.0328	-0.2109*	-0.2061*	-1.3629	-0.8286
	[0.66]	[0.70]	[0.75]	[0.69]	[0.12]	[0.05]	[-1.88]	[-1.85]	[-0.33]	[-0.21]
PersonalTaxRate$_{j,t}$	0.0053	0.0050	0.0603	0.0602	0.1517	0.1521	0.0049*	0.0048*	0.3892**	0.3724**
	[1.50]	[1.42]	[1.02]	[1.01]	[1.04]	[1.03]	[1.87]	[1.83]	[2.40]	[2.30]
TaxIncentivesIndex$_{j,t}$	0.0046**	0.0046**	0.0542	0.0541	0.1244	0.1238	0.0003	0.0004	0.0194	0.0226
	[2.31]	[2.31]	[1.17]	[1.18]	[1.10]	[1.10]	[0.25]	[0.28]	[0.25]	[0.29]
SalesTaxRate$_{j,t}$	-0.0007	0.0002	-0.0350	-0.0362	-0.0955	-0.0999	-0.0052	-0.0048	0.3774	0.4214
	[-0.12]	[0.03]	[-0.84]	[-0.81]	[-0.93]	[-0.92]	[-1.15]	[-1.07]	[1.24]	[1.38]
Adj. R-squared	0.1525	0.1553	0.0516	0.0516	0.0435	0.0436	0.1472	0.1488	0.2716	0.2756

This panel presents results of testing the relation between subsidies and the log of the number of local establishments (Columns (1)-(2)), the log of the number of employees (Columns (3)-(4)), the log of aggregate wages (Columns (5)-(6)), employees scaled by population (Columns (7)-(8)), and aggregate wages scaled by population (Columns (9)-(10)) for the full sample of 14,112 county-year observations. We define all variables in Appendix A and present t-statistics in brackets. Each specification includes year and county fixed effects, and we cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4

Relation between Tax Subsidies and Local Activity

Panel B: Entropy-balancing

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostTaxSubsidy$_{i,t}$	0.0065*		0.0189***		0.0164*		0.0083***		0.5741***	
	[1.95]		[3.55]		[1.93]		[2.84]		[2.90]	
PostTaxSubsidy$_{i,(0-1)}$		-0.0027		0.0070		-0.0051		0.0051**		0.3232**
		[-0.89]		[0.94]		[-0.32]		[2.15]		[2.23]
PostTaxSubsidy$_{i,(2-3)}$		0.0110**		0.0264***		0.0251**		0.0129***		0.9220***
		[2.41]		[3.62]		[2.20]		[2.75]		[2.96]
PostTaxSubsidy$_{i,(4-5)}$		0.0161**		0.0294***		0.0227		0.0141***		1.1437***
		[2.37]		[3.01]		[1.57]		[2.65]		[3.27]
PostTaxSubsidy$_{i,(6+)}$		0.0191**		0.0363***		0.0272		0.0176***		1.3993***
		[2.08]		[3.06]		[1.53]		[3.22]		[3.70]
MinWage$_{i,t}$	-0.0091***	-0.0091***	0.0062	0.0063	0.0214	0.0215	0.0025*	0.0026*	0.3652***	0.3696***
	[-4.54]	[-4.50]	[0.97]	[0.98]	[1.47]	[1.48]	[1.86]	[1.88]	[3.98]	[4.05]
Ln(GDP)$_{j,t}$	0.3498***	0.3488***	0.5035***	0.5026***	0.7910***	0.7887***	0.2612***	0.2611***	18.8086***	18.7957***
	[10.83]	[10.88]	[7.91]	[7.88]	[6.65]	[6.57]	[8.67]	[8.75]	[8.64]	[8.76]
%Educ$_{j,t}$	0.0017*	0.0016*	-0.0046*	-0.0047*	-0.0070	-0.0069	-0.0018	-0.0019	-0.0437	-0.0515
	[1.92]	[1.89]	[-1.65]	[-1.72]	[-1.14]	[-1.15]	[-1.47]	[-1.53]	[-0.97]	[-1.13]
%Union$_{j,t}$	0.0028**	0.0030**	-0.0021	-0.0018	-0.0012	-0.0006	-0.0025***	-0.0024***	-0.1587***	-0.1510***
	[2.03]	[2.21]	[-1.14]	[-0.99]	[-0.33]	[-0.18]	[-3.74]	[-3.61]	[-4.32]	[-4.12]
CorpTaxRate$_{j,t}$	-0.0011	-0.0008	-0.0075	-0.0072	-0.0131	-0.0127	-0.0004	-0.0003	0.0402	0.0498
	[-1.09]	[-0.83]	[-1.61]	[-1.58]	[-1.16]	[-1.15]	[-0.65]	[-0.51]	[0.91]	[1.15]
Ln(UIContrib)$_{j,t}$	-0.0310***	-0.0313***	-0.0139	-0.0149	-0.0359	-0.0344	-0.0004	-0.0013	-0.3551	-0.4378
	[-5.79]	[-5.59]	[-1.11]	[-1.17]	[-1.47]	[-1.38]	[-0.07]	[-0.22]	[-1.09]	[-1.35]
PropertyTax$_{j,t}$	0.1673***	0.1760***	0.0115	0.0214	0.2208	0.2140	-0.2265***	-0.2169***	-1.9311	-1.0366
	[2.82]	[2.92]	[0.10]	[0.18]	[0.89]	[0.87]	[-4.63]	[-4.44]	[-0.80]	[-0.42]
PersonalTaxRate$_{j,t}$	-0.0063***	-0.0059***	0.0009	0.0015	0.0012	0.0016	0.0030***	0.0033***	0.1513**	0.1781**
	[-3.41]	[-3.22]	[0.36]	[0.56]	[0.25]	[0.32]	[2.77]	[2.97]	[2.21]	[2.57]
TaxIncentivesIndex$_{j,t}$	0.0003	0.0005	0.0026*	0.0028*	0.0017	0.0018	-0.0000	0.0001	-0.0287	-0.0192
	[0.51]	[0.72]	[1.74]	[1.87]	[0.54]	[0.58]	[-0.01]	[0.20]	[-1.22]	[-0.79]
SalesTaxRate$_{j,t}$	0.0033**	0.0046***	0.0006	0.0020	0.0003	0.0020	-0.0028*	-0.0022	-0.0786	-0.0239
	[1.98]	[2.83]	[0.20]	[0.65]	[0.04]	[0.33]	[-1.88]	[-1.45]	[-1.14]	[-0.34]
Adj. R-squared	0.1306	0.1344	0.0279	0.0281	0.0484	0.0130	0.1688	0.1699	0.3329	0.3352

This panel presents results of using an entropy-balanced sample to test the relation between subsidies and the log of the number of local establishments (Columns (1)-(2)), the log of the number of employees (Columns (3)-(4)), the log of aggregate wages (Columns (5)-(6)), employees scaled by population (Columns (7)-(8)), and aggregate wages scaled by population (Columns (9)-(10)) for the sample of 14,112 county-year observations. We define all variables in Appendix A and present t-statistics in brackets. Each specification includes year & county fixed effects, and standard errors are clustered by county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5

Deal Size and Local Activity

Panel A: Megadeals

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*	
	Megadeals	Other deals	Megadeals	Other deals	Megadeals	Other Deals	Megadeals	Other Deals	Megadeals	Other Deals
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostTaxSubsidy$_{i,t}$	**0.0181**	**0.0068**	**0.0454****	**0.0486*****	**0.0488***	**0.0819****	**0.0025**	**0.0124****	**1.0022***	**0.8119****
	[1.37]	**[0.92]**	**[2.57]**	**[2.59]**	**[1.77]**	**[2.15]**	**[0.29]**	**[2.13]**	**[1.83]**	**[2.08]**
MinWage$_{i,t}$	-0.0090	-0.0006	0.0071	0.0021	0.0181	0.0143	0.0005	-0.0020	0.0010	-0.0020
	[-1.46]	[-0.17]	[0.91]	[0.17]	[1.46]	[0.55]	[0.16]	[-0.56]	[0.20]	[-0.33]
Ln(GDP)$_{j,t}$	0.3215*	0.3495***	0.4034**	0.9412***	0.7221***	1.8556**	0.0788**	0.1357***	0.1060	0.2243***
	[1.96]	[6.68]	[2.56]	[2.96]	[3.15]	[2.40]	[2.03]	[5.68]	[1.29]	[4.49]
%Educ$_{j,t}$	0.0010	-0.0055**	-0.0045	0.0020	-0.0089	0.0159	-0.0004	0.0002	-0.0001	0.0008
	[0.29]	[-2.04]	[-0.81]	[0.19]	[-1.16]	[0.55]	[-0.30]	[0.25]	[-0.05]	[0.46]
%Union$_{j,t}$	-0.0041	0.0105***	-0.0036	-0.0189*	-0.0082	-0.0424	-0.0011	-0.0039**	-0.0015	-0.0052**
	[-0.75]	[4.35]	[-0.53]	[-1.65]	[-0.84]	[-1.57]	[-0.89]	[-2.25]	[-0.62]	[-2.29]
CorpTaxRate$_{j,t}$	0.0043	-0.0008	0.0040	-0.0064	0.0060	-0.0129	0.0020	0.0005	0.0026	0.0002
	[1.11]	[-0.54]	[0.57]	[-1.17]	[0.59]	[-1.04]	[1.16]	[0.77]	[0.87]	[0.19]
Ln(UIContrib)$_{j,t}$	-0.0270	-0.0318*	-0.0063	-0.0453	0.0165	-0.1117	0.0096	-0.0127**	0.0162	-0.0220*
	[-1.33]	[-1.85]	[-0.20]	[-0.98]	[0.42]	[-1.21]	[1.10]	[-2.18]	[0.99]	[-1.95]
PropertyTax$_{j,t}$	0.5529**	0.0666	0.5285	0.2151	0.7507	0.0532	-0.1125	-0.1403**	-0.3179	-0.2111*
	[2.07]	[0.43]	[1.22]	[0.68]	[1.23]	[0.08]	[-1.04]	[-2.19]	[-1.55]	[-1.86]
PersonalTaxRate$_{j,t}$	-0.0073	0.0074*	0.0056	0.0744	0.0055	0.1896	0.0003	0.0016	0.0016	0.0052*
	[-1.12]	[1.86]	[0.69]	[1.01]	[0.44]	[1.04]	[0.12]	[0.87]	[0.34]	[1.84]
TaxIncentivesIndex$_{j,t}$	0.0000	0.0050**	0.0038	0.0619	0.0029	0.1437	0.0001	0.0004	0.0007	0.0003
	[0.01]	[2.32]	[1.22]	[1.17]	[0.75]	[1.11]	[0.18]	[0.53]	[0.48]	[0.17]
SalesTaxRate$_{j,t}$	0.0163*	-0.0023	0.0221*	-0.0358	0.0363	-0.0957	0.0018	-0.0061	0.0028	-0.0056
	[1.86]	[-0.42]	[1.95]	[-0.88]	[1.60]	[-0.96]	[0.95]	[-1.29]	[0.77]	[-1.17]
Observations	1,200	12,912	1,200	12,912	1,200	12,912	1,200	12,912	1,200	12,912
Adj. R-squared	0.1685	0.1580	0.2005	0.0550	0.4217	0.0471	0.3382	0.1434	0.3863	0.2660

This panel presents results of OLS regressions testing the relation between subsidies and local economic activity, measured as the log of the number of local establishments (Columns (1)-(2)), the log of the number of employees (Columns (3)-(4)), and the log of aggregate wages (Columns (5)-(6)),)), employees scaled by population (Columns (7)-(8)), and aggregate wages scaled by population (Columns (9)-(10)) for the propensity-score matched sample of 14,112 observations partitioned by megadeals (Columns (1), (3), (5), and (7)) and all other types of subsidies (Columns (2), (4), (6), and (8)). We define all variables in Appendix A and present t-statistics in brackets. Each specification includes year and county fixed effects, and we cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5 (cont'd)

Deal Size and Local Activity

Panel B: By quartile of subsidy size ($), excluding megadeals

	1st Quartile (1)	2nd Quartile (2)	3rd Quartile (3)	4th Quartile (4)
		Ln(Establishments)$_{i,t+1}$		
PostTaxSubsidy$_{i,t}$	0.0129	0.0077	0.0094	0.0035
	[1.04]	[0.76]	[1.07]	[0.28]
Diff (1st Quartile vs. 2nd)	-0.0060			
Diff (1st Quartile vs. 3rd)	-0.0060			
Diff (1st Quartile vs. 4th)	-0.0111			
Diff (2nd Quartile vs. 3rd)		0.0002		
Diff (2nd Quartile vs. 4th)		-0.0047		
Diff (3rd Quartile vs. 4th)			-0.0065	
Adj. R-squared	0.1967	0.1967	0.1967	0.1967
		Ln(Employees)$_{i,t+1}$		
PostTaxSubsidy$_{i,t}$	0.0044	0.0259	0.0314	0.0393
	[0.16]	[1.42]	[1.02]	[1.33]
Diff (1st Quartile vs. 2nd)	0.0232			
Diff (1st Quartile vs. 3rd)	0.0246			
Diff (1st Quartile vs. 4th)	0.0356			
Diff (2nd Quartile vs. 3rd)		0.0052		
Diff (2nd Quartile vs. 4th)		0.0136		
Diff (3rd Quartile vs. 4th)			0.0064	
Adj. R-squared	0.0345	0.0345	0.0345	0.0345
		Ln(Wages)$_{i,t+1}$		
PostTaxSubsidy$_{i,t}$	-0.0093	0.0280	0.0255	0.0766*
	[-0.14]	[0.97]	[0.40]	[1.66]
Diff (1st Quartile vs. 2nd)	0.0413			
Diff (1st Quartile vs. 3rd)	0.0317			
Diff (1st Quartile vs. 4th)	0.0878			
Diff (2nd Quartile vs. 3rd)		0.0000		
Diff (2nd Quartile vs. 4th)		0.0501		
Diff (3rd Quartile vs. 4th)			0.0464	
Adj. R-squared	0.0290	0.0290	0.0290	0.0290
		(Employees/Population)$_{i,t+1}$		
PostTaxSubsidy$_{i,t}$	0.0295	0.0128*	0.0115	0.0062
	[1.62]	[1.74]	[1.22]	[0.43]
Diff (1st Quartile vs. 2nd)	-0.0162			
Diff (1st Quartile vs. 3rd)	-0.0181			
Diff (1st Quartile vs. 4th)	-0.0228			
Diff (2nd Quartile vs. 3rd)		-0.0022		
Diff (2nd Quartile vs. 4th)		-0.0062		
Diff (3rd Quartile vs. 4th)			-0.0038	
Adj. R-squared	0.2023	0.1978	0.1174	0.1098
		(Wages/Population)$_{i,t+1}$		
PostTaxSubsidy$_{i,t}$	0.2603	0.5766	0.9674	1.5332
	[0.41]	[1.38]	[1.61]	[1.47]
Diff (1st Quartile vs. 2nd)	0.3601			
Diff (1st Quartile vs. 3rd)	0.6556			
Diff (1st Quartile vs. 4th)	1.3181			
Diff (2nd Quartile vs. 3rd)		0.3155		
Diff (2nd Quartile vs. 4th)		0.9791		
Diff (3rd Quartile vs. 4th)			0.6466	
Adj. R-squared	0.2924	0.2601	0.3077	0.2297

This panel presents results of OLS regressions testing the relation between subsidies and local economic activity for a sample of non-megadeal tax subsidy county-years that report the dollar value of tax subsidies, and their propensity-score matched control counties (total sample of 9,048 county-year observations). We partition the sample into quartiles based on subsidy dollar value (each quartile contains approximately 2,280 county-year observations). We define all variables in Appendix A and present t-statistics in brackets. Each specification includes year and county fixed effects, and standard errors are clustered by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 6

Transparency: Subsidies and Local Activity

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*	
	Transparent	Opaque	Transparent	Opaque	Transparent	Opaque	Transparent	Opaque	Transparent	Opaque
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostTaxSubsidy$_{i,t}$	0.0100	0.0044	0.0319*	0.0825**	0.0443	0.1550*	0.0191***	-0.0015	0.9825*	0.6507**
	[1.20]	[0.34]	[1.86]	[2.14]	[1.47]	[1.84]	[2.59]	[-0.23]	[1.84]	[2.14]
MinWage$_{i,t}$	0.0026	-0.0138***	0.0026	-0.1120	0.0231	-0.2641	-0.0033	-0.0023	0.1421	0.2782
	[0.73]	[-2.74]	[0.15]	[-1.01]	[0.84]	[-0.93]	[-0.39]	[-0.70]	[0.41]	[1.56]
Ln(GDP)$_{j,t}$	0.2814***	0.5454***	0.7813***	0.6850	1.3189***	1.6402	0.2716***	0.0631	27.5719***	9.7678***
	[4.40]	[6.99]	[4.14]	[1.31]	[3.75]	[1.24]	[4.77]	[0.95]	[5.58]	[3.56]
%Educ$_{j,t}$	-0.0035	-0.0057	-0.0037	0.0232	-0.0012	0.0656	-0.0011	0.0045*	-0.0156	0.0995
	[-1.47]	[-1.40]	[-0.59]	[0.71]	[-0.08]	[0.78]	[-0.69]	[1.71]	[-0.19]	[0.98]
%Union$_{j,t}$	0.0137***	-0.0033	-0.0122	-0.0599	-0.0210	-0.1486	-0.0058**	-0.0019	-0.3397	-0.2334**
	[5.50]	[-0.88]	[-1.31]	[-1.28]	[-1.21]	[-1.26]	[-2.37]	[-0.81]	[-1.59]	[-2.59]
CorpTaxRate$_{j,t}$	-0.0003	-0.0165**	-0.0045	0.0576	-0.0070	0.1333	0.0006	0.0012	0.0581	-0.2945
	[-0.21]	[-2.23]	[-0.96]	[1.02]	[-0.70]	[0.95]	[0.47]	[0.15]	[0.74]	[-0.53]
Ln(UIContrib)$_{j,t}$	-0.0354**	-0.0388	-0.0116	0.0354	-0.0405	0.1053	-0.0151	-0.0053	-0.0519	-0.4211
	[-2.04]	[-1.47]	[-0.27]	[0.63]	[-0.63]	[0.78]	[-0.86]	[-0.49]	[-0.05]	[-0.68]
PropertyTax$_{j,t}$	0.1112	0.0194	0.4033	4.1216	0.2565	10.3303	-0.2357**	0.0845	-1.9858	9.2605
	[0.57]	[0.09]	[1.33]	[1.31]	[0.63]	[1.34]	[-2.04]	[0.43]	[-0.39]	[1.53]
PersonalTaxRate$_{j,t}$	0.0047	-0.0051	-0.0026	-0.0597	-0.0010	-0.1466	0.0034	-0.0045	0.3459*	-0.0617
	[1.01]	[-1.12]	[-0.33]	[-1.14]	[-0.07]	[-1.13]	[1.14]	[-1.27]	[1.66]	[-0.22]
TaxIncentivesIndex$_{j,t}$	0.0014	0.0100***	0.0041	0.1201	0.0031	0.2858	-0.0015	0.0041**	-0.0765	0.1932***
	[1.07]	[3.40]	[1.20]	[1.39]	[0.47]	[1.35]	[-1.12]	[2.36]	[-0.80]	[2.76]
SalesTaxRate$_{j,t}$	0.0024	-0.0132	0.0149	-0.0594	0.0189	-0.1476	-0.0054	0.0034	0.4011	0.6141***
	[0.37]	[-1.33]	[1.53]	[-1.27]	[1.28]	[-1.44]	[-0.88]	[0.90]	[0.93]	[2.60]
Difference	0.0056		-0.0506		-0.1107		0.0126*		0.1907	
P-Value	[0.36]		[-1.22]		[-1.26]		[1.91]		[0.55]	
Observations	9,240	4,872	9,240	4,872	9,240	4,872	9,240	4,872	9,240	4,872
Adj. R-squared	0.1417	0.2619	0.0382	0.1074	0.0561	0.0943	0.1366	0.2323	0.2692	0.3365

This table presents results of testing the relation between subsidies and local economic activity for the propensity-score matched sample of 14,112 county-year observations after partitioning based on the home state's ranking of disclosure transparency as measured by GJF. *Transparent* is an indicator equal to one for states in the top ten by disclosure transparency, and *Opaque* includes all other observations. We define all other variables in Appendix A and present t-statistics are presented in brackets. Each specification includes year and county fixed effects, and standard errors are clustered by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.