Do targeted business tax subsidies achieve expected benefits?

Lisa De Simone[*]
Stanford University

Rebecca Lester
Stanford University

Aneesh Raghunandan
London School of Economics

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We examine the association between thousands of state and local firm-specific tax subsidies and business activity in the surrounding county, measured as the number of employees, aggregate wages, per capita employment, per capita wages, and number of business establishments. Using three different matched control groups, we find a positive association between subsidies and the employment measures. However, we show that local information – measured based on subsidy-specific disclosures, public awareness, and local press coverage – plays an important role in the effectiveness of subsidies. We also demonstrate that (i) receipt of multiple or subsequent subsidies in the same counties is critical for these employment outcomes and (ii) results are concentrated in the largest subsidy packages by dollar value. In addition, we observe mixed evidence for the relation between subsidies and business establishments and find little to no local effects for over 1,000 subsidies that cost approximately $99.8 million in aggregate. By providing a large-scale empirical analysis of the relation between firm-specific tax subsidies and aggregate economic activity at the county level, we extend a literature that generally focuses on the real effects of statutory tax policies that impact all firms in a jurisdiction. We also contribute to the accounting literature by examining the role of the local information environment in subsidy effectiveness.

Keywords: Business Taxes, Subsidies, Employment, Local Information
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[*] 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 and Pengjie Gao, Chang Lee, and Dermot Murphy for sharing newspaper closure data. We appreciate helpful comments from Eric Allen, John Core, Merle Erickson, Michael Faulkender (discussant), John Gallemore, Joao Granja, Pradeep Gupta, Russ Hamilton, Joe Max Higgins, Philip Joos, Tsafrir Livne, Dan Lynch, Marcel Olbert, Terry Shevlin, Nemit Shroff, Eddie Watts, Ann-Catherin Werner, Mark Wolfson (discussant), University of Connecticut Tax Readings Group, and workshop participants at the University of Chicago, Indiana University, London Business School, 2019 Minnesota Accounting Conference, Northwestern University, the 2018 Stanford Accounting Summer Camp, the University of California at Irvine, the 2019 UNC Tax Symposium, and the University of Mannheim. 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.

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1. Introduction

This paper studies the relation between targeted business tax subsidies and local economic activity and how this relation varies based on information publicly available in the local economy. Understanding corporate responses to tax policies is important because these companies are responsible for three-quarters of aggregate investment (IRS, 2013), over half of private sector employment (Tax Foundation, 2014), and approximately one-third of per capita GDP growth (John, Litov, and Yeung, 2008). In addition to altering statutory tax policies (such as corporate tax rates), governments increasingly use firm-specific tax subsidies to stimulate local business activity. We study whether and to what extent these tax subsidies are associated with greater local employment, higher wages, and more business establishments in the years after subsidy grant. Further, we study whether the association between these subsidies and local business activity varies with the information environment of granting jurisdictions.

Prior work studies corporate responses to changes in tax policies across jurisdictions and over time, with most studies examining the real effects of changes in statutory corporate income tax rates affecting all firms in a particular jurisdiction (Hines, 1997; Papke, 1991; Suarez Serrato and Zidar, 2016; Fuest, Peichl, and Siegloch, 2018; Giroud and Rauh, 2019). However, in recent years, state and local governments provided thousands of firm-specific tax subsidies, generally under the premise of creating jobs and boosting the local economy. The aggregate dollar value of tax subsidies awarded amounts to nearly $11 billion in 2014 alone. While some subsidies provide multi-million dollar tax benefits, the most prevalent subsidies – and the ones we focus on in this paper – are smaller packages typically ranging from a few thousand dollars to less than $1 million in value. Despite the large aggregate dollar value of these subsidies and their growing prevalence, there is limited empirical research on their economic effects. Possible reasons for this lack of evidence include limited data on a broad sample of subsidies and difficulties identifying an appropriate control sample against which to measure outcomes. Consequently, prior and concurrent work often studies small samples of large subsidies (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Moretti and Wilson, 2014; Slattery, 2019; Slattery and Zidar, 2019), examining productivity outcomes or generating estimates unlikely to generalize to the smaller, more prevalent subsidies we study. Other work examines smaller subsidies but focuses on the role of local demographics and within-state competition in the subsidy granting process, rather than measuring economic outcomes (Felix and Hines, 2013; Ossa, 2018; Mast, 2019). Further, none of these studies focus on how subsidy disclosures and local public awareness impact subsidy effectiveness, despite the rising implementation of disclosure regulation as an enforcement mechanism. We address these issues by (i) using a large dataset of subsidies across 540 counties from 2004 to 2016, (ii) leveraging econometric approaches from recent literature to construct three different control samples that address state- and local-level factors related to subsidy grant, and (iii) employing local information environment measures developed by the accounting and finance literatures.

Prior literature motivates a positive relation between tax subsidies and local economic activity. For example, international studies generally find 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 (2019) find decreases in state tax rates lead to increases in employment and new business establishments. 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. First, the literature examining the role of taxes in fostering economic growth yields largely inconsistent, and even surprising, results. Two studies 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 employment and new firm formations. Second, recipient firms may not be required to provide evidence they increased their local employment and investment 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 minimal 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 neighboring counties.

To test the relation between tax subsidies and local economic activity, we obtain data on firm-specific tax credits and 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.[1] We combine this large sample of tax subsidies granted to a wide range of firms with county-year data from the U.S. Bureau of Labor Statistics and the U.S. Census Bureau to measure subsequent local economic activity and to construct control variables.

We examine the association between subsidy incidence and county-level business activity using a staggered difference-in-difference design and three alternative control samples. A subsidy recipient county could be substantially different from other randomly selected counties along a number of dimensions. Thus, identifying an appropriate control sample is critical to measuring the effect of subsidies on economic outcomes, but is also challenging because the ideal match – jurisdictions that offered or would offer a similar subsidy to the same recipient – is unobservable

[1] 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). We exclude federal subsidies from our tests to focus on the role of state and local incentives.

for the broad range of subsidies we study. Therefore, we use three distinct counterfactual groups to address different county- and state-level determinants of state and local subsidy grants and to mitigate concerns about omitted correlated variables that could be present in one particular control sample. Our first approach uses propensity score matching to identify those jurisdictions most likely to have offered a subsidy based on both state and local characteristics. We match (with replacement) within state to counties with no subsidy recipients during our sample period using county characteristics such as population, whether the county is urban or rural, and economic trends in the years preceding subsidy receipt. By requiring a within-state match, we mitigate concerns about confounding state-year characteristics (such as GDP and state tax rates) and unobservable state-year characteristics (such as private sector connections (Aobdia, Koester, and Petacchi, 2019)) related to subsidy grant.

Our second approach creates synthetic controls with entropy balancing. Although this approach does not require controls to be in the same state, it ensures balanced covariates across all observable characteristics related to subsidy grants. We mitigate concerns about matching across states (and thus unobservable state factors) by including state-by-year fixed effects. Finally, because counties with subsidy recipients could be different from counties with no subsidy recipients across dimensions these first two approaches may not address, our third approach benchmarks the business activities of county-years with subsidy recipients against counties that later receive subsidies similar to Fuest et al. (2018). Although this last approach mitigates selection bias concerns by requiring control counties to also grant subsidies, it produces a smaller sample.

Counties with subsidy recipients have over 16,500 distinct tax subsidies during the sample period, including tax credits and tax abatements. We observe a growing frequency of tax subsidies during the sample period, with nearly 5,600 tax subsidies in 2014. Our tests focus on a large and representative subset of awards to better identify the effects of a tax subsidy on county-level economic factors. 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 based on GJF's assertion that their data is substantially complete beginning in 2004. We therefore focus on the first tax subsidies observed in the GJF data after 2005. Because we require counties to be observable for the entire sample period, the propensity score matched sample is a balanced panel of 12,960 county-years (540 county pairs) for the 12-year period from 2004 to 2015. This sample of county-years is distributed across 26 distinct U.S. states, with the most observations in Colorado, Indiana, Iowa, Kentucky, Tennessee, and Texas. Our resulting sample of 2,832 first-observed tax subsidies across 540 unique counties provide approximately $1.5 billion of benefits.

We test the relation between the incidence of a tax subsidy and the future number of employees, aggregate level of wages, and number of business establishments in the corresponding jurisdiction relative to control counties. We also test number of employees and aggregate wages scaled by working-age population (age 25-64) to measure economic effects relative to the county's labor pool. Although we acknowledge that we cannot construct the perfect counterfactual, observing generally consistent evidence across three different control samples mitigates concerns that any effects are driven by an omitted variable correlated with subsidy grant. Furthermore, it allows us to provide a range of magnitudes and verify or caveat conclusions based on the robustness of results.

We first estimate average increases of 266.7 to 1,633.3 employees beginning in the first two years following subsidy receipt relative to all three control groups. However, on a per-capita basis, we observe no statistically significant effects relative to two control groups, implying that differences in the rate of employment growth to population growth across the different control samples is important when estimating economic magnitudes. Specifically, the estimates range from 0.0-2.7 percent increase in per capita employment following a subsidy. Second, we estimate positive coefficients for aggregate wages, with statistically significant coefficients relative to two of the three control groups. These results imply an increase in aggregate wages in the range of $0 to $135.7 million over the sample period. On a per capita basis, we estimate an increase of $0-$801.7 in per capita income, with effects delayed until the third or fourth year after subsidy receipt. An examination of trends suggests these results are driven by both an increase in employment and wages in treatment counties and a decline in control counties. Third, we estimate an increase of 17.2-89.9 additional establishments during the post-subsidy period, with effects delayed three to five years following subsidy receipt. Collectively, the evidence suggests subsidies are associated with increases in the total number of employees, wages, and establishments, but these effects are often delayed or even offset on a per capita basis by commensurate changes in population.

We next test the role of the local information environment. We first partition the sample based on state-level subsidy disclosure initiatives. We focus on this channel because some governments have attempted to address public concerns about subsidy effectiveness through the passage of subsidy-specific disclosure laws. We also examine two other elements of the local information environment: public attention to subsidies (using state-level Google Trends data following Da, Engelberg, and Gao (2011)) and public monitoring (based on the presence of a local newspaper following Gentzkow, Shapiro, and Sinkinson (2011) and Gao, Lee, and Murphy (2019)). Across all three tests, we observe that the positive association between subsidies and aggregate employment and wages occurs in those counties with *less* information and attention given to subsidies – those without state subsidy disclosure initiatives in place, with fewer subsidy-specific internet searches, and lacking a local newspaper. One possible ex-post explanation relates to the accounting literature on proprietary costs (see Appendix A in Heinle, Samuels, and Taylor (2018)), which shows that mandatory disclosures can harm a firm's competitive position (Graham, Harvey, and Rajgopal, 2005). By extension, the mandatory reporting required under these disclosure initiatives reveals information about which jurisdictions are willing to provide these subsidies and the type of firm that receives them. As a consequence, the effectiveness of subsidies could be *lower* in jurisdictions with greater information about these subsidies, as the subsidies may attract broader scrutiny. For example, local special interest or advocacy groups may pressure firm recipients to divert funds from investment and employment to other community outcomes we cannot observe (e.g., mitigating gentrification or environmental concerns). Alternatively, greater information may result in grants of subsidies that are more politically popular rather than necessarily effective (e.g., favoring local firms over outside or foreign firms). While untestable due to data limitations, these explanations are consistent with a number of examples (Chaney, 2016; Craighead and Manzo, 2017; Story, 2012).

Finally, we study how subsidy effectiveness varies with multiple subsidies and subsidy size. This analysis reveals a number of findings. First, after including all 16,500 awards in empirical tests, we find that employment increases with the number of subsidies received. Second, subsequent subsidies are necessary to achieve positive employment outcomes when there is only one subsidy granted in the first year. Third, we find that increases in the number of employees, wages, employment rate, and per capita wages occur only in the top tercile of deals based on dollar value. In contrast, we observe little to no economic effect for over 1,000 smaller subsidies that cost approximately $99.8 million in aggregate.[2] Finally, when we examine an additional group of 49

[2] These 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 in a location. However, we are unaware of publicly available county-level data to test these outcomes.

multi-million dollar subsidies, we observe increases in the number of employees, but no change in aggregate wages. These results are consistent with firms using tax savings to hire, but such jobs not necessarily being associated with aggregate compensation growth. We also find no change in employment on a per-capita basis, consistent with working-age population growth in those counties.

To evaluate the relative cost of these outcomes, we compare the average direct cost of the first-observed subsidies of $720 thousand to the estimated incremental number of employees (266.7-1,633.3 jobs) and establishments (17.2-89.9). This comparison implies that counties spend approximately $440.60-$2,698.55 per additional worker and at least $8 thousand per establishment. However, this calculation only takes into account these measured outcomes, excluding other potential benefits (such as increased local tax revenue and economic spending) and related costs (increased pollution, rising housing prices for existing residents, traffic, etc). Further, it assumes that the first-time subsidy is effective independent of any subsequent subsidies. Given we show that multiple subsidies are necessary to stimulate additional hiring and compensation, the average estimated cost increases to $881.29-$10,794.22 per worker and $16.0-$32.0 thousand per establishment assuming two to four incentives are provided. Based on the average per capita income in the sample of $18.0 thousand, this calculation suggests that subsidy costs could approach or even exceed a half year of wages for each new job.

We acknowledge that our approach and data do not allow us to directly test the effects of subsidies at the firm- or establishment-level. 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 important costs of these tax policies. Finally, we examine subsidies across over half of all U.S. states, but results could vary based on the inclusion of other jurisdictions. Although the paper does not address these open and interesting questions, we believe our approach is a first step towards understanding the effectiveness of a broad sample of small, targeted tax subsidies in generating purported benefits and the role of local information in those outcomes.

Our study relates to prior and concurrent work in economics that explores different aspects of firm-specific subsidies. Greenstone et al. (2010) examine agglomeration effects following 47 large "million dollar plant" openings from 1973 to 1998; in concurrent work, Slattery (2019) and Slattery and Zidar (2019) examine 500 of the largest subsidies with values exceeding $5 million. While we also separately examine large subsidy packages, we primarily focus on thousands of smaller-value incentives used by an increasing number of state and local governments. Concurrent work on these subsidies is generally limited to understanding the role of competition and political connections as determinants of which firms receive subsidies (e.g., Mast, 2019; Aobdia et al., 2019). In contrast, we provide evidence on real investment and employment outcomes.

Second, we contribute to the accounting literature by documenting the extent to which the local information environment plays a role in observed real effects. Given that disclosure initiatives have been a primary mechanism used by city and state officials to monitor subsidy recipients, it is important to understand the effectiveness of this enforcement tool in achieving intended outcomes. By providing evidence on the role of the local information environment, we extend the literature that has primarily focused on country-level information environments in firm investment (Francis, Huang, Khurana, and Pereira, 2009; Chen, Hope, Li, and Wang, 2011; Shroff, Verdi, and Yu, 2014; Loureiro and Taboada, 2015) and employment decisions (Engel, Gordon, and Hayes, 2002). Further, we add to the nascent accounting literature studying these subsidies, including the relation between subsidy magnitude and future firm performance (Drake, Hess, Wilde, and Williams, 2018) and the role of government capture in the likelihood of subsidized firms engaging in and getting caught engaging in accounting misstatements (Raghunandan, 2018).

Finally, we 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; Chirinko and Wilson, 2008; Busso, Gregory, and Kline, 2013; Giroud and Rauh, 2019; Ljungqvist and Smolyanksky, 2015; Gale et al., 2015; Suarez Serrato and Zidar, 2016; Fuest et al., 2018). 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 an economically material governmental policy tool in a setting that fundamentally differs from those found in the development economics literature, which generally finds limited evidence of subsidy effectiveness (e.g., Clemens, Radelet, Bhavnani, and Bazzi, 2012; Rajan and Subramanian, 2008). We attribute the difference in results primarily to the relatively better and consistent quality of institutions across the states we study. Finally, our analysis facilitates future comparisons of the effectiveness of firm-specific tax policies to other policies that impact all firms in a jurisdiction.

2. 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 robust 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.[3]

A related, but distinct, literature studies the effect of taxes on employment and firm establishments. 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. Suarez 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 state tax changes.[4] Giroud and Rauh (2019) 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. Fuest et al. (2018) re-examine the incidence of the corporate tax using changes in

[3] See also Bartik (1991), Mazerov (2013), and McBride (2012) for reviews of this literature.
[4] A large literature focuses on the incidence of business taxes, generally asking whether and to what extent labor 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.

German local business tax rates. All four of these studies focus on tax policy changes that affect all firms in 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.

Studies examining establishments provide some evidence of a weak association. Bartik (1985) studies the association between aggregate corporate effective tax rates and the creation of new plants 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.

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 (2019) 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, 2019; Suarez 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 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. We bypass these issues by directly observing the incidence of tax subsidies that reduce or otherwise offset a firm's tax burden. This approach provides an improved identification strategy relative to prior work to examine 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 whether and to what the extent tax subsidies achieve increases in local area activity, including both direct and spillover employment and establishment 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 local employment.

H2: Firm-specific tax subsidies are positively associated with the number of local establishments.

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 employment or establishments) 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). There are also significant non-tax determinants of location decisions, such as proximity to production inputs or customer markets, as well as agglomeration effects (e.g., Devereaux, Griffith, and Simpson, 2007) that could dominate the role of business taxes. Finally, because we measure effects at the county-level given the lack of publicly-available establishment-level data, firm-specific or related spillover effects of tax subsidies may not be large enough to detect any differences in the local area.

3. Research Design

3.1 Empirical Approach

We first 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 employees, the amount of aggregate wages, or the number of business establishments in a specific county; we discuss these measures below in Section 3.2. $PostTaxSubsidy_{i,t}$ 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 of subsidy grant, and zero otherwise. Thus, our primary tests analyze the incidence of the first-observed tax subsidy. Subsequent tests (i) examine the role of the information environment, (ii) use a continuous measure that captures the cumulative number of subsidies awarded to firms in each county-year, and (iii) examine the role of subsidy size by partitioning the sample based on the dollar value magnitude of the subsidy.[5] We use GJF data to identify counties with subsidy recipients and discuss the subsidy data further in Section 3.3. We predict that subsidies are positively associated with employment and the number of establishments ($\beta_1 > 0$).

We include a set of control variables ($Controls_{i,j,t}$) motivated by prior literature, further discussed in Section 3.4. Finally, we include county and year fixed effects as proxies for potentially unobservable changes in local 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. Additional specifications replace the county- and year- fixed effects with state-by-year fixed effects. We cluster standard errors by county. Online Appendix, Table 1, Panel C presents robustness of results to clustering by state.

3.2 Employment and Investment Measures

[5] Directly examining the relation between subsidy amount and subsequent economic activity is difficult in our setting because tax subsidies are generally awarded over a duration longer than one year, but GJF does not provide the dollar value of each award by year.

We obtain the number of employees, aggregate wages, and number of establishments from the U.S. Bureau of Labor Statistics (BLS) website. The BLS 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.[6] From the quarterly filings, we obtain a county-level calendar-year average, computed by the BLS, of the number of employees, aggregate annual wages paid, and number of establishments. We take the logarithm of these amounts to construct the dependent variables $Ln(Employees)_{i,t+1}$, $Ln(Wages)_{i,t+1}$, and $Ln(Establishments)_{i,t+1}$. Because the subsidy data capture subsidies to private-sector firms, we use private-sector employees, wages, and establishments (i.e., we exclude public-sector employees and 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 channel through which employment benefits arise following subsidies and abatements, we test both the number of employees and aggregate wages.

As alternative measures of employment and wages, we also scale employees and wages by working age population (age 25-64) to create $(Employees/Population)_{i,t+1}$ and

[6] The BLS website states that these data reflect over 95 percent of all US jobs.

(Wages/Population)$_{i,t+1}$. 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 per capita measures also more closely represent economic outcomes such as reduced unemployment and higher-paying jobs in a jurisdiction, which are often stated goals of many tax subsidies.

3.3 Subsidy Data

GJF data is available publicly on the organization's webpage. However, because certain subsidies are omitted from the website, we obtain Subsidy Tracker data directly from GJF to ensure completeness. The subsidy data begin in 2004, as GJF focused on ensuring relatively complete coverage – for example by using Freedom of Information Act requests – 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 three categories of firm-specific awards that likely have long-term effects on firms' marginal costs for the long-term investment and employment decisions we study. The first two categories include (i) Tax Credits, which are *dollar value* awards that reduce a firm's tax liability dollar-for-dollar, and (ii) Tax Abatements, which provide a *percentage* reduction of a firm's tax liability. These two categories represent the most prevalent subsidies, accounting for approximately 70.0-75.0 percent of all subsidies in the dataset.[7] Subsequent tests examine a third category known as "megadeals,"

[7] See additional details from the GJF website: https://www.goodjobsfirst.org/accountable-development/corporate-income-tax-credits; we rely on the GJF classification of these subsidy awards. Remaining subsidies in 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 and Reimbursements because they represent one-time cash transfers or diversions of public resources less likely to change the marginal cost of long-term labor and capital investments. We do not study Tax Increment Financing or Enterprise Zones because these provide benefits to any eligible firm in the designated area, as opposed to specific firm recipients. Section 5.3 discusses additional analyses related to these other groups of incentives. We are unable to distinguish between subsidies granted

a term coined by GJF to reflect the largest deals based on dollar value and which span multiple categories of subsidy type.[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. We hand-match subsidy data to BLS establishment and employment data at the county-year level.

3.4 Control Variables

We control for determinants of employment and location decisions. These control variables include the minimum wage 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 state GDP level, measured using aggregate data from the Bureau of Economic Analysis, and 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 measures from the Census Bureau's American Community Survey. Following Giroud and Rauh (2019), 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

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to incentivize continued operations in a jurisdiction and subsidies granted to incentivize new operations in the jurisdiction.

[8] Good Jobs First defines megadeals as those packages that generally exceed $50-$75 million in value. Given the large dollar values of these incentives relative to other tax credits and abatements in our sample, as well as some prior evidence about these large subsidies (Greenstone et al., 2010), we study these subsidies separately in tests discussed in Section 5.2 and presented in Table 7.

[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.

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]

3.5 Control Groups

We use three control samples to identify the effects of tax subsidies on the economic activities of jurisdictions with subsidy recipients. The goal across these three different control samples is to identify jurisdictions likely to have also offered a subsidy to the same recipients. Because this set of control counties is unobservable, we identify the benchmark groups in three ways that take into account both state and county-level factors related to subsidy grant.

Our first approach propensity-score matches (with replacement) counties with subsidy recipients to counties in the same state with no subsidy recipients at any point during our sample period. By requiring a within-state match, this approach mitigates concerns about observable state-year and possible unobservable state-specific confounds. Our second approach uses entropy balancing to ensure balanced covariates across all observable county-year characteristics related to subsidy grant on both the first and second moment. We allow for matches in other states, thereby minimizing concerns that effects are due to within-state shifts in employment or establishments, which could possibly overstate results.[11] To mitigate concerns that differences across states could be driving the results, we include state-by-year fixed effects. 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 to further address the selection concern that counties with subsidy recipients could be different from counties with no subsidy recipients across

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

[11] Criscuolo et al. (2019) find no evidence of a reallocation of effects across geographies in an examination of U.K. investment subsidies for firms in disadvantaged areas. Nonetheless, we do not measure effects relative to a neighboring county as an alternative control sample because declines in the outcome variables in neighboring counties could be attributable to a shift in employment and establishments across county lines, which would overstate the economic effect of tax subsidies.

observable and unobservable dimensions that we are unable to address with the first two approaches. Thus, the collective evidence across three different benchmark groups allows us to provide a range of magnitudes and verify or caveat conclusions based on the robustness of results.

3.6 Main Sample Selection and Description

Table 1 describes the sample selection process for propensity score matching. We use this as our main approach because it permits us to directly address both across-state characteristics related to subsidy grant (by requiring matches within the same state) and within-state characteristics (by matching on local economic characteristics identified in prior literature (Heider and Ljungqvist, 2015; Giroud and Rauh, 2019)). As detailed in Panel A, we start with all county-years with available employment and establishment data for the 12 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)).[12] We acknowledge that firms could receive subsidies in years prior to the start of our sample period in 2004 that would not be captured in the GJF data due to the organization's focus on the period since 2004. To better isolate the effect of observable subsidies, 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.[13] Of the remaining 29,220 county-year observations, 9,384 observations relate to 783 distinct counties with at least one tax subsidy recipient during the sample period.

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

[13] This results in the exclusion of California, Maine, and Vermont, as all counties in these states had subsidy recipients in 2004 and/or 2005. We exclude Montana counties because they provide non-tax subsidies that are not the focus of this paper.

We propensity score match each of these counties to the set of counties in the same state 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 observed subsidy in our sample.[14] We present results of our propensity model in Panel B. The sample of 14,136 county-years is composed of 589 subsidy-control pairs, including both megadeal and non-megadeal subsidies. Given the extreme size of megadeal subsidies as compared to the other subsidies in our sample, as well as some prior work on megadeals (Greenstone, Hornbeck, and Moretti, 2010), we first focus on non-megadeal subsidies. We present descriptive statistics for this sample of treatment counties with non-megadeal subsidies (and their matched control counties (n=12,960)) in the remaining panels of Table 1 and use this as the basis for the treatment sample in Tables 2 through 6. Table 7 presents results from separately examining megadeals using the sample of 1,176 matched observations.

Panel C provides tests of differences in the mean values of the dependent variables for non-megadeal subsidy counties and the propensity-matched control counties in the year of matching. This panel shows that there are statistically significant differences in the levels of three of the five measures.[15] Because a key assumption of our difference-in-differences research design is that the

[14] The propensity score matched sample of 22,032 includes 9,384 subsidy county-year observations and 12,648 possible control observations. The matching approach results in the use of 200 distinct control counties. For 193 of the possible 782 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 in the state. We drop all counties in Louisiana, Ohio, Oregon, and Wisconsin due to a lack of sufficient matched control counties.

[15] Because the independent variables of interest are state-level measures and we propensity score match within state, there are no statistically significant differences in these variables across the subsidy and control groups.

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 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 variables and fixed effects. Consistent with the descriptive evidence in Panel C, we observe that subsidy and control counties exhibit different levels across these 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 21,083 employees with aggregate wages of approximately $905 million annually, or approximately $18,000 per working age (age 25-64) resident. The average county has approximately 1,598 business establishments. All of these amounts are skewed, with counties in the 99th percentile reporting over 268,000 employees, $14.0 billion of aggregate wages, and 19,880 establishments (untabulated). The average minimum wage for the sample period is $6.57/hour. Approximately 26.4 percent of the working-age population has at least a bachelor's degree, and 5.74 percent are union members. The average state corporate and personal income tax rates are 5.84 and 3.79 percent, respectively.

Panel B provides sample composition by type of subsidy. We first show descriptive statistics on the 540 county-years with first-observed subsidies granted during our sample period, as these are the primary subsidies we examine in empirical tests. We observe 2,832 distinct tax subsidies in this sample. Tax credits and abatements comprise 57.8 percent and 42.2 percent of the sample, respectively. Over 2,000 of these tax subsidies include data on the dollar value of the award, aggregating to almost \$1.5 billion in total. Although we focus on the first-observed subsidies in each county, Panel B also provides information on all subsidies reported by counties with subsidy recipients. In total, 1,457 county-years report over 16,000 distinct tax subsidies.[16] As with the sample of first-observed subsidies, we see that tax credits comprise approximately 60 percent of the sample and have a dollar value that is approximately four to five times that of abatements. Figure 2 maps the number of subsidy counties by state.

Panel C shows the number of tax subsidies by year. The majority of first tax subsidies we observe for the 540 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 have multiple subsidies. We test the role of all 16,000 subsidies in Section 5.1. Table 3 presents the correlation matrix.

4. Main Results

4.1 Relation between Tax Subsidies and Local Activity

Panel A of Table 4 presents results of our hypothesis tests using the propensity score matched sample. Columns (1), (3), (5), (7), and (9) present results for regressing $Ln(Employees)_{i,t+1}$, $Ln(Wages)_{i,t+1}$, $(Employees/Population)_{i,t+1}$, $(Wages/Population)_{i,t+1}$, and $Ln(Establishments)_{i,t+1}$ on $PostTaxSubsidy_{i,t}$ and control variables, respectively. Columns (2), (4), (6), (8), and (10) repeat these analyses after including an indicator for specific years following tax subsidy grant.

[16] 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.

We observe positive and statistically significant effects in Columns (1) and (3) 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. Figure 1 provides graphical evidence of these effects by plotting both raw values and residual values from regressing our outcome variables on the control variables and fixed effects. Based on Panels A and B, these effects appear to be attributable to both an 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 0.0594 on *PostTaxSubsidy* in Column (1) suggests that counties experience a 5.9 percent increase in the number of employees following a first-time tax subsidy to a firm in the county, relative to matched control counties. Given that the average number of employees in subsidy counties in the year prior to the first-observed tax subsidy is 27,496 (untabulated), this result translates to 1,633.3 additional employees. The positive and significant coefficient of 0.1212 on *PostTaxSubsidy* in Column (3) suggests that counties experience a 12.1 percent increase in wages following a first-time tax subsidy to a firm in the county, relative to matched control counties. Given that average aggregate wages in subsidy counties in the year prior to the first-observed tax subsidy are $1.12 billion (untabulated), this result translates to $135.7 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 (5) and (7). The positive and significant coefficient of 0.0137 in Column (5) suggests that, relative to control counties, counties with tax subsidy recipients experience a 1.4 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 prior to the first subsidy (0.52), this result represents an increase of 2.7 percent. Figure 1, Panel C suggests that this is driven by both 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 0.8017 in Column (7) suggests counties experience a $801.70 increase in annual per-capita income following a first-time tax subsidy to a firm in the county, relative to control counties.

Columns (2), (4), (6), and (8) provide more insight into the timing of these employment effects. For *Employees* and *Employees/Population*, the effects begin within two years following subsidy receipt. For *Wages* and *Wages/Population*, we observe delayed effects. Although we observe positive coefficients in the years immediately following the first-observed subsidy, these effects are not statistically significant until years t+3 to t+4. Further, the magnitude of statistically significant coefficients on the staggered post-subsidy indicator variables across Columns (6) and (8) monotonically increase, suggesting that the per capita economic impact of a tax subsidy on employees 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.

We observe a positive but statistically insignificant effect for $Ln(Establishments)_{i,t+1}$ in Column (9), suggesting that subsidies on average throughout the post-tax subsidy period are not associated with new facilities or offices in the county. Figure 1, Panel E demonstrates graphically that the number of establishments appears to increase in counties with subsidy recipients, but that an increase also occurs in matched control counties in the two years following subsidy receipt, resulting in no statistically significant effect in Column (9). Effects are increasing from year *t-2* to *t-1* and slightly declining from year *t-1* to year *t*. The trends are similar but we acknowledge that, unlike with the employment graphs presented in Figure 1 Panels A-D, they are not entirely parallel, and thus the establishments results should be interpreted with caution. We observe an increase in control counties in year *t+1* but also a decrease in year *t+2*, whereas subsidy counties demonstrate the opposite effects, likely contributing to the lack of significant effect in the two years following subsidy receipt. However, in Column (10) we observe statistically significant effects beginning three years following subsidy receipt. The coefficient of 0.0166 in Column (2) implies a 1.6 percent increase in establishments following subsidy receipt. Given that the average number of establishments in subsidy counties in the year prior to the first-observed tax subsidy is 2,025 (untabulated), this coefficient implies an increase of 33.6 establishments three years following subsidy receipt. There are at least three explanations for the delayed effect, none of which are mutually exclusive. First, recipient firms use tax subsidies to open new facilities, but it takes time to build or open these establishments. Second, other non-recipient businesses in the community open new establishments, but these spillover effects occur with a lag. Third, the effect is driven by subsequent subsidies; we test this effect in Section 5.1 below.

One concern is that Eq. (1) does not appropriately capture unobservable characteristics that could be correlated with the relation between subsidies and local economic effects. Therefore, we re-estimate Eq. (1) after replacing the state-year control variables and year fixed effects with state-by-year fixed effects. Online Appendix Table 1, Panel A shows that results are robust to this alternative specification. Further, because the control group is critical to testing the relation between subsidies and local economic effects, we present results in Table 4, Panel B using an alternative entropy-balanced matched sample that ensures balanced covariates between treatment and matched control counties across observable characteristics on both the first and second moments. While we do not require that the control county be in the same state for purposes of optimizing the balance across the sample, we limit the sample of possible control counties to those in the same 26 states represented in the PSM sample. We re-estimate Eq. (1) using the entropy balanced sample after replacing the state-year control variables and year fixed effects with state-by-year fixed effects.[17]

Results presented in Panel B are similar to the propensity score matched results presented in Panel A and Online Appendix Table 1, Panel A, with the exception of the *Employees/Population* effect. The statistically significant coefficients for the other four outcomes imply slightly lower economic magnitudes than those estimated previously, thereby providing a range of estimated magnitudes across the two panels. For example, for *Ln(Employees),* the coefficient of 0.0318 on *PostTaxSubsidy* in Column (1) implies a 3.18 percent increase in the number of employees, or approximately 874.4 more employees. The coefficient in Column (3) for *Ln(Wages)* of 0.0618 suggests that, relative to control counties, counties with tax subsidy recipients experience a 6.18 percent increase in wages, or $69.2 million in aggregate, following the receipt of a first-time tax subsidy by a firm in the county. The positive and significant coefficient of 0.4946 in Column (7) suggests counties experience a $494.6 increase in annual per-capita income following a first-time tax subsidy to a firm in the county, relative to control counties. The coefficient for *Ln(Establishments)* of 0.0139 in Column (10) implies a 1.4 percent delayed increase in establishments, or 28.1 establishments, beginning five years after subsidy grant.

The primary difference in the results in Table 4, Panel B as compared to Panel A relates to the lack of per capita employment effect. The positive association between subsidies and total number of employees in Columns (1)-(2) of both panels suggests that the lack of result for per capita employment in Columns (5)-(6) is a function of the growth in employment relative to changes in the working age population. Further, this employment growth in the entropy-balanced

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[17] Online Appendix Table 1, Panel B presents results of estimating Eq. (1) on the entropy balanced sample using state-year controls and year fixed effects, as in Table 4, Panel A.

matched controls must outpace wage growth on a per capita basis, resulting in the different effects observed in Columns (5)-(6) as compared to Columns (7)-(8).[18]

We also test our hypothesis using a third control group: other subsidy recipient counties. We first restrict the sample to only those counties with first-time subsidy recipients during our sample period because counties with subsidy recipient firms may be different from other counties across additional unobservable characteristics that propensity-score matching and entropy balancing do not capture. In doing so, we measure 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 drop any counties with first-time subsidy recipients in 2014 and 2015 because there is no post-period for these subsidies. This results in a much smaller sample for these tests (n=6,216), which is a limitation of this approach when comparing results across the three different samples.

Table 4, Panel C presents results. We show only the period-specific effects given well-known concerns about estimation error for the average effect in this sample.[19] We continue to observe a positive and statistically significant relation for three of the outcome variables: *Ln(Employees*), *Wages/Population*, and *Ln(Establishments)*.

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[18] In untabulated tests we find that the matched control counties in the PSM sample (Table 4, Panel A) exhibit employment growth that is commensurate with population growth; in contrast, we find that the subsidy counties' employment growth outpaces population growth, which contributes to the positive and statistically significant effect that we attribute to subsidy receipt. Thus, the lack of effect in Table 4, Panel B Columns (5)-(6) implies that the entropy-balanced control sample exhibits a rate of employment growth relative to population growth that is more similar to that of subsidy counties. While the relatively higher employment growth to population growth in the entropy-balanced matched control counties (as compared to the PSM-matched counties) contributes to the lack of a statistically significant effect in Columns (5)-(6), results for income per capita persist in Columns (9)-(10). That suggests that growth in per capita wages among entropy-balanced matched controls occurs at a rate less than the growth in per capita employment.

[19] Borusyak and Jaravel (2017) show that, when a sample contains firms that are all "treated" at some point in the sample period, the coefficient capturing the average effect for the full post-period can be estimated with error due to an underweighting of long-run effects and overweighting of short-run effects.

In Columns (1) and (4), we observe positive and significant relations for employees (beginning in the period immediately following subsidy receipt) and per capita income effects (beginning in the third year after subsidy receipt). As before, the coefficient magnitudes on the staggered post-subsidy indicator variables for employees and per capita income increase monotonically over time. The significant coefficients of 0.0097 to 0.0375 on the *PostTaxSubsidy* period indicators when using *Ln(Employees)*$_{i,t+1}$ as the dependent variable implies a 1.0-3.8 percent increase in the number of employees, or approximately 266.7- 1,031.1 more employees. The significant coefficients on the *PostTaxSubsidy* period indicators when using *Wages/Population*$_{i,t+1}$ of 0.5547-0.5551 suggests that, relative to control counties, counties with current tax subsidy recipients experience a $554.7-$555.1 increase in annual per-capita income, following the receipt of a first-time tax subsidy by a firm in the county. The coefficients on *PostTaxSubsidy* for *Ln(Establishments)* are positive throughout the sample period, ranging from 0.0085 in the first post-subsidy years to 0.0444 at least seven years post-subsidy, suggesting an increase in 17.2 to 89.9 establishments in counties with current tax subsidy recipients relative to counties that later have tax subsidy recipients.

As in Panel B, we find that the coefficients for *Employees/Population* are positive in Column (3), but not statistically significant. We also observe positive, but statistically insignificant coefficients for *Ln(Wages)*. The differing results for aggregate wages and per capita employment across the three panels demonstrate that the statistical significance of the relation for two of the outcomes is sensitive to the control group selected, which may be one reason prior literature has found mixed evidence. For example, the lack of effect for per capita employment growth again implies the rate of employment growth to population growth in these later-subsidy control counties must be similar to that in the earlier-subsidy counties.

In summary, results presented in Table 4 demonstrate a range of establishment and employment effects. For employment, we observe average increases of 266.7 to 1,633.3 employees occurring in the first two years following subsidy receipt across all three empirical specifications. However, on a per-capita basis, we observe no statistically significant effects in two specifications, meaning that differences in the rate of employment growth to population growth across the different control samples is important when estimating the magnitude of effects. Thus, we estimate a range from 0.0-2.7 percent increase in per capita employment following a subsidy. We observe positive and increasing coefficients for *Ln(Wages)*, with statistically significant coefficients in two of the three specifications. These results imply a range from $0 to $135.7 million in the aggregate level of wages over the sample period. On a per capita basis, our results suggest an increase of $0-$801.7 in per capita income, with effects delayed until the third or fourth year after subsidy receipt. Finally, we observe an increase of 17.2-89.9 establishments, but in two of the three tests, we also find that the effects are delayed three to five years following subsidy receipt.

4.2 The Role of Information in Subsidy Effectiveness

We next examine whether subsidy effectiveness varies based on the local information environment, which we expect will affect the public's ability to monitor subsidy recipients and granting jurisdictions. We explore three specific channels through which we expect the information environment to affect monitoring and subsidy effectiveness. Across these tests, we benchmark results against the propensity-score matched sample of counties that never receive a subsidy.

First, we expect that more effective subsidies occur in more transparent jurisdictions, because more information about the subsidies should improve the public's awareness of these incentive packages. We measure local area transparency based on whether the state has public disclosure initiatives about subsidies. In some states, these include disclosure websites or economic development funds with transparency provisions; in other states, these include specific laws requiring disclosure of subsidy grant. We obtain these data from GJF studies in 2007, 2010, and 2014 and partition subsidy counties based on whether an initiative was in place in the year prior to the first-observed subsidy.[20] We partition the sample of subsidy counties and their matched controls based on whether the treatment county was located in a state with a disclosure initiative in place, and then re-estimate Eq. (1). We then calculate differences in coefficients across the two partitions (those with a disclosure initiative less those without a disclosure initiative) and present these coefficient differences in Table 5, Panel A. The top numbers in the panel reflects differences in coefficients that capture the average effect over the post-subsidy period, whereas the bottom numbers capture differences in specific post-subsidy periods. These differences are negative in 17 out of the 25 regression specifications, indicating that the coefficient for the partition with disclosure initiatives in place is less than the coefficient in the partition without disclosure initiatives. These results suggest subsidies are less effective in states with disclosure initiatives. For example, the statistically significant differences in Columns (1) and (2) mean that the positive association between subsidies and *Ln(Employees)* and *Ln(Wages)* from Table 4, Panel A occurs in *less* transparent counties – those without a disclosure initiative in place – and that these effects persist up to six years after subsidy receipt.[21] Results are similar when we partition the sample

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[20] In 2007, GJF published a report called *The State of State Disclosure: An Evaluation of Online Public Information About Economic Development Subsidies, Procurement Contracts, and Lobbying Activities*. GJF also published two follow-up subsidies in 2010 (*Show Us the Subsidies*) and in 2014 (*Show us the Subsidized Jobs*). In the 2010 and 2014 studies, GJF lists the states and years in which states created disclosure initiatives and in some cases passed disclosure laws. We identify states that had disclosures initiatives or laws prior to 2010 based on their inclusion in the 2007 report.

[21] Online Appendix Table 2 presents the full results of these tests. For example, Online Appendix Table 2 shows that when testing *Ln(Employees)*, the coefficient for *PostTaxSubsidy* in the sample with a disclosure initiative is 0.0175 (t=1.37), whereas the coefficient in the sample without a disclosure initiative is 0.1853 (t=2.02), for a difference of -0.1678.

based on the existence of a disclosure initiative in the same year as the first-observed subsidy, the disclosure grade that GJF assigns to each state, or whether the subsidy is in a state viewed as a "Leading State" based on a review of state disclosure laws in a 2017 Pew Charitable Trusts study (untabulated).

Second, we study whether subsidy effectiveness varies based on public attention to subsidies. We examine public attention using state-level Google Trends data, similar to the approach taken in Da et al. (2011).[22] We obtain state-year normalized data (on a scale of 0-100) on cumulative Google search volume for the terms "subsidy," "subsidies," "tax break," and "tax breaks" relative to all other searches conducted in that state-year.[23] Table 5, Panel B presents differences in coefficients from estimating Eq. (1) on the sample partitioned based on the median within-year Google Trends score for the initial subsidy year. We continue to observe that the differences are predominantly negative, with negative amounts in 22 out of the 25 specifications. These differences are again statistically significant for *Ln(Employees)* and *Ln(Wages)*: we observe a 7.8 percent higher increase in the number of employees and a 17.2 percent higher increase in wages in the sample with below-median Google Trends scores in Columns (1) and (2), respectively. However, these differences appear to weaken over time.

Third, we examine whether subsidy effectiveness varies based on the existence of a local newspaper in the subsidy recipient county at the time of subsidy grant. We study local newspapers because they have important roles in both disseminating information relevant for the political process (Gentzkow et al., 2011) and monitoring local governments (Gao et al., 2019). Furthermore,

[22] Google Trends search volume data is not publicly available at the county level.
[23] For example, if 50 total Google searches were conducted in a given state-year, and 7 of these included any of the four subsidy search terms, the relative interest of subsidies would be 14 percent for that state-year. We also search several other terms – e.g., "tax subsidy", "tax abatement", "tax credit" – but search volume for these is negligible relative to the four terms we list above.

unlike the prior two measures constructed at the state level, this measure captures county-level information. However, one trade-off of using this more local measure is that we only observe the existence of newspapers rather than actual press coverage about subsidies. We obtain a list of current daily local newspapers from Editor & Publisher and a list of newspaper closures beginning in 2004 from Gao et al. (2019) to construct an indicator variable equal to one if a local newspaper is operational (and zero otherwise) by county-year over our sample period.[24] Table 5, Panel C presents the differences in coefficients from re-estimating Eq. (1) after partitioning the sample based on the existence of a local newspaper in the county in the year the subsidy was granted. Results are generally consistent with our other information tests in that we continue to observe that the more effective subsidies occur in counties with less information: all 25 of the coefficient differences presented are negative. We estimate a statistically significant 1.7 percentage point higher increase in per capita employment in jurisdictions without a newspaper, relative to jurisdictions with a newspaper. We also observe a higher increase in per capita wages of $1,043 in counties without a newspaper relative to counties with a newspaper when using the propensity-score matched sample. These statistically significant differences persist for six and four years after subsidy receipt, respectively.[25]

[24] We thank Pengjie Gao, Chang Lee, and Dermot Murphy for graciously sharing their newspaper closure data. Gentzkow et al. (2011) find that the first newspaper in a market generates the greatest effects in the political process (with diminishing effects of a second and third newspaper), and Gao et al. (2019) focus on jurisdictions with fewer than four local newspapers in studying municipal bond yields. In our sample, 323 counties have no newspaper, 191 have one newspaper, and only 26 counties have more than one newspaper. Therefore, we partition based on whether the county has a newspaper rather than using a continuous newspaper count. Ideally, we would also use newspaper closures as a plausibly exogenous shock to the county-year information environment following Gao et al. (2019); however, there are only 19 counties with a newspaper closure in our sample, and of those only three closures occurred in the year the first-observed subsidy was granted. We partition based on newspaper presence to keep our control sample consistent across disclosure tests; however, we acknowledge this approach does not incorporate the existence of a newspaper when matching counties with subsidy recipients to control counties.

[25] Although the collective evidence across all panels is that the association between subsidy receipt and economic outcomes is greater in less transparent jurisdictions, the statistically significant effects in Panel C occur in the per capita measures, as opposed to *Ln(Employees)* and *Ln(Wages)* in Panels A and B. There are two possible reasons for this. First, the newspaper measure is constructed at the county level, unlike the disclosure and Google trends measures, which are a function of state initiatives and state-level subsidy-specific attention. Second, in untabulated analysis we

The tests above suggest that the positive association between subsidies and economic outcomes occurs in jurisdictions with less disclosure and public attention.[26] There are three possible ex-post explanations for observing these results. First, the positive effects of information may simply be delayed. If this were the case, we would expect to see (i) little to no effectiveness in the jurisdictions with less information (no disclosure initiatives, below median Google Trends searches, and no newspapers), and (ii) an increasingly positive and significant relation in high information jurisdictions. We test this explanation by examining the timing of effects; see Online Appendix Table 2. For employees, wages, and establishments, we generally do not observe increases in the statistical significance of positive coefficients in high-information jurisdictions. Instead, we observe that the coefficients remain statistically significant or even lose significance across the sample period. Further, estimated coefficients in the low information jurisdictions remain positive and statistically significant throughout the sample period, rather than having no effect. Thus, these results are inconsistent with a greater but delayed effectiveness of tax subsidies granted in high information environments.

A second possible explanation is that greater information and awareness of a subsidy could attract the attention of local groups. That is, greater information about subsidies could cause some of the funds to be spent on outcomes other than employment and establishments, resulting in a weaker relation between subsidies and the economic outcomes than we observe in locations with

find that partitioning based on the presence of a newspaper produces treatment and control groups with much starker differences in population – and therefore, also changes in population – which contributes to the statistically significant effects in Panel C.

[26] Subsequent tests presented in Tables 6 and 7 demonstrate the roles of subsidy size and count as determinants of subsidy effectiveness. To rule out these potential confounding factors in our information tests, we confirm there is no systemic difference in subsidy size or count between jurisdictions with more versus less subsidy information across the existence of a disclosure initiative and above/below median Google Trends searches. We find larger subsidies in counties with a local newspaper relative to counties without a local newspaper, biasing against our finding that the positive association between subsidies and economic outcomes occurs in jurisdictions with less disclosure and public attention.

less local information and attention. This may occur in at least two ways. First, subsidy recipients may divert some funds to support local organizations as a way to convey that the firm is a good corporate citizen (which could also indirectly and positively influence the likelihood of obtaining subsequent subsidies). Second, special interests such as community or environmental activism groups may exert pressure on the granting jurisdictions or the recipients themselves to use some granted funds to remediate local concerns (e.g., local school funding, gentrification, homelessness, waste, and preserving natural habitats of native species). These actions could create other positive outcomes for local jurisdictions, but at the same time explain the effects observed in Table 5. Unfortunately, data are not available to explicitly test the extent to which firm recipients spend on these other outcomes. However there are numerous examples of this occurring in several jurisdictions that our partitioning approaches classify as "transparent." For example, in a press release announcing the opening of a facility partly funded by a North Carolina subsidy, MetLife described how the company had made grants exceeding $2.0 million to support a number of community programs (Metlife, 2017). As another example, Samsung donated a portion of its property tax subsidy back to a Texas school district following public concerns over local tax breaks used to lure Samsung to the state (Story, 2012).

Third, a more transparent information environment could also result in granting jurisdictions choosing the most politically palatable grant recipients, potentially at the expense of lower expected outcomes from the project. For example, a jurisdiction may grant a tax subsidy to a local company rather than an outside or foreign company for political reasons, even if the latter project had higher expected benefits absent these political costs. Although we are also unable to test this explanation, there is anecdotal support for this argument. Officials in Missouri often emphasized the importance of subsidizing incumbent companies to preserve existing jobs, even though several subsidized establishments ultimately failed (Mhire, 2011; Famuliner, 2013). Furthermore, prioritizing job retention could come at the cost of foregoing higher-return projects from outside companies. As another example, Texas Governor Rick Perry touted his ability to use tax incentives to "win" head-to-head bids against other states to lure outside companies to Texas, though some argue these politically popular "wins" were not worth the costs (Story, 2012). North Carolina and Illinois have been criticized for granting a disproportionate amount of subsidies to larger public firms, even though these firms were investing in metro areas that arguably would benefit less relative to other communities (Chaney, 2016; Craighead and Manzo, 2017).[27]

In summary, the tests in this section suggest that employment outcomes occur in jurisdictions with less information and attention to subsidies. This finding extends and applies evidence from the accounting literature on proprietary costs into the public finance space. For example, mandatory reporting required under disclosure initiatives reveals information about which jurisdictions grant subsidies and the types of firms that receive them. As a consequence, subsidy effectiveness could be *lower* in these jurisdictions, as the subsidies may attract broader scrutiny. A 2010 GJF study stated that business groups have often been opposed to mandatory disclosures due precisely to these proprietary concerns. However, to further validate this possible explanation, additional data at the establishment-level that includes other local economic outcomes would be necessary. Several jurisdictions also have recently passed stricter disclosure initiatives that require better enforcement of subsidy outcomes. Understanding the extent to which the effectiveness varies based on these more recent and tighter initiatives will be important once data become available.

[27] Aobdia et al. (2018) focus on whether politically connected firms have a higher likelihood of receiving a subsidy. This is distinct from the explanation we propose that local government officials may grant subsidies based on *constituents'* concerns, as opposed to their own political connections (though both are plausible explanations). We also examine this question in the context of the local information environment, which Aobdia et al. (2018) do not test.

5. Additional Tests

5.1 Multiple Subsidies

The tests discussed up to this point all focus on measuring economic activity following the first subsidies received in a county. However, as seen in Table 2 Panel C, many of the 540 counties with a subsidy recipient report multiple subsidies in a given year, as well as subsidies in subsequent years.[28] Therefore, in Table 6, we study the relation between the *number* of subsidies and the five economic outcomes using the propensity score matched sample. We test the effect of multiple subsidies in two ways.

First, we examine whether and to what extent business activities increase with the number of subsidies provided. Specifically, we re-estimate Eq. (1) after replacing $PostTaxSubsidy_{i,t}$ with $Ln(CountSubsidy_{i,t})$, which is equal to the log of one plus the total number of cumulative subsidies received in that county to date.[29] As with the subsidy incidence tests presented in Table 4, Panel A, we find a positive association between the cumulative number of subsidies in a jurisdiction and *Ln(Employees)*, *Ln(Wages)*, and *Employees/Population* in Columns (1), (3), and (5). We also observe a positive, but insignificant, coefficient for per capita wages in Column (7) (t=1.51). These results imply that employment measures increase with the number of subsidies received in a county. We observe no statistically significant relation between the number of subsidies and the number of business establishments for the full post subsidy period.

We next explicitly test the incremental effect of multiple subsidies relative to the incidence of the first subsidies received. To do so, we re-estimate Eq. (1) including both $PostTaxSubsidy_{i,t}$ and $Ln(CountSubsidy_{i,t})$. In these specifications, *PostTaxSubsidy* captures the effect attributable to

[28] These could be multiple subsidies to the same firm (for example, different property tax abatements for different facilities on a firm's campus) or multiple subsidies to different firms. Given our unit of measurement is at the county-level, we aggregate all of these for the tests in this section.

[29] For example, if three subsidies were received in a county in 2006, no subsidies in 2007, and two new subsidies in 2008, *Ln(CountSubsidy)* would be equal to ln(4) in 2006 and 2007 and ln(6) in 2008.

the first subsidies received (i.e., the extensive margin), whereas *Ln(CountSubsidy)* measures the effect attributable to the number of subsidies (i.e., the intensive margin). We observe positive coefficients for *PostTaxSubsidy*$_{i,t}$, but the coefficients are not statistically significant in Columns (2), (4), (6), and (10). Instead, we observe statistically significant coefficients on *Ln(CountSubsidy*$_{i,t}$*)* for *Ln(Employees)* and *Employees/Population* in Columns (2) and (6), suggesting that the number of subsidies – whether in the first year or in subsequent years – is important for these employment outcomes.

Because counties can grant multiple subsidies in the first year, we conduct an additional test (untabulated) in which we partition the sample based on whether the county initially gave one subsidy or multiple subsidies. We then re-estimate Eq. (1) including both *PostTaxSubsidy* and *Ln(CountSubsidy)*. For the subsample of subsidy counties (and their matched controls) that give more than one subsidy in the first year, we generally observe positive but insignificant coefficients for both *PostTaxSubsidy* and *Ln(CountSubsidy)*. However, for the subsample of counties that only provide one subsidy in the first year, we observe that subsequent subsidies are extremely important. Specifically, the coefficients for *PostTaxSubsidy* are generally positive but insignificant, whereas the coefficients on *Ln(CountSubsidy)* for *Ln(Establishments)*, *Ln(Employees)*, and *Wages/Population* are positive and significant. These results imply that some of the documented effects in Table 4 for these three outcome variables are attributable to counties providing multiple subsequent subsidies. This result means that the actual direct cost of a subsidy could in fact be much greater than one discrete incentive if multiple subsidies are necessary to achieve employment outcomes.

5.2 Deal Size

Descriptive statistics on tax subsidies indicate significant skewness with respect to deal size. Further, 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 therefore test the role of deal size in two ways, both presented in Table 7.

First, we re-estimate Eq. (1) after partitioning the sample with requisite data on monetary value (a reduction in observations from 12,960 to 9,072) into terciles based on the dollar value of tax subsidies awarded. We present results in Table 7, Panel A. For parsimony, we do not tabulate coefficients on control variables.

Across *Employees*, *Wages*, and *Wages/Population*, 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 package. However, we only observe statistically significant effects for these three employment measures in the top tercile (coefficients of 0.0493, 0.1201, and 1.4993, respectively), implying that the 1,100 smaller subsidy packages in the bottom two terciles have little employment effect in the local county jurisdictions. Interestingly, we observe that the per capita employment effect is concentrated in the bottom tercile. Untabulated univariate statistics reveal variation across the terciles that helps to reconcile this result to the lack of result for *Ln(Employees)* in the bottom tercile (coefficient=0.0128, t-stat=0.55). Specifically, both treatment and control counties in the bottom tercile report an increase in employment and population, but subsidy counties have a higher rate of employment growth to population growth as compared to matched control counties. In contrast, in the top tercile we observe that, for both subsidy and matched control counties, employment growth occurs at a rate commensurate with population growth, thereby contributing to the lack of per capita effect in that subsample.

For establishments, we continue to observe no statistically significant effects across the different terciles. Untabulated tests confirm these inferences hold when instead partitioning the sample based on subsidy recipient size.

Second, we examine the relation between subsidy receipt and economic outcomes for counties with megadeal recipients. Recall that these counties were included in the original set of 589 subsidy counties (Table 1, Panel A), but we separately examine these incentives in Table 7, Panel B because the megadeals are large in dollar value relative to the rest of our sample and may otherwise skew results. For example, the 49 first-time megadeals across 23 states (including five additional states not included in the main sample) account for nearly $9.5 billion of tax benefits, even though they only represent 1.7 percent of the first-time subsidies we study. Furthermore, by separately estimating megadeals, we can also provide estimates of outcomes given that the prior literature focuses on total factor productivity and local spillover effects (Greenstone, Hornbeck, and Moretti, 2010).

We present results from re-estimating Eq. (1) on the sample of megadeal counties and their matched control observations (n=1,176 county-years) in Table 7, Panel B. In Columns (1) and (2), we observe a positive and statistically significant employment effect. The coefficient of 0.0302 in Column (1) implies a 3.02 percent increase in number of employees (or 4,270.4 more employees) following a megadeal compared to control counties. Based on the average megadeal value of $202.0 million and an estimated 4,270 jobs created, the cost of megadeals is equivalent to subsidizing approximately $47.3 thousand in compensation (or approximately 7.2 thousand hours of minimum wage work) per additional employee.

With respect to *Ln(Wages*), we only observe a statistically significant wage effect in the last post-subsidy period. We also observe no positive effects for per capita employment in Columns (5)-(6) or for per capita wages in Columns (7)-(8) (except for the periods beginning at least seven years after subsidy receipt). Together, these results suggest that megadeals are associated with more jobs but not necessarily overall higher wages, higher per capita employment, or higher per capita income.

We observe no significant relation between tax subsidies and number of establishments on the coefficient that captures the full post-period in Column (9), suggesting that subsidies are not on average associated with a statistically significant increase in the number of offices and business locations. However, we do observe effects for establishments beginning in the third and fourth years following subsidy receipt, consistent with results in Table 4, Panel A. The coefficient for the period *t+3* and *t+4* is 0.0289 with a t-statistic of 1.89, implying an increase of 2.9 percent. Given that the average number of establishments in the megadeal subsample was 8,820 prior to the subsidy, this coefficient implies an increase of 254.9 establishments, as compared to our estimated increase of 33.6 establishments in the non-megadeal sample in Table 4, Panel A.

In summary, the results across Table 7 show that subsidy effectiveness increases with the dollar value of the subsidy package, with employment results concentrated in the megadeals and the top tercile of subsidy packages.[30] The smaller subsidies in the sample generally exhibit no statistically significant association with the level of employees, wages, or establishments.

5.3 Other subsidy types

As discussed in Section 3.3, Good Jobs First collects data on different categories of subsidies. We focus on tax credits and abatements (Tables 4 through Table 7, Panel A), as well as megadeals (Table 7, Panel B) because they (i) should have the most direct effect on firms' long-term decisions related to investment in local employment and establishments (as opposed to cash

[30] 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 Eq. (1) after dropping seven megadeals for which the dollar value of tax subsidies and abatements does not exceed 50 percent of the total subsidy package, finding consistent results across all of the columns.

grants and reimbursements), (ii) comprise the majority of subsidy observations in the GJF data, and (iii) are firm-specific rather than available to any firm in a particular jurisdiction (as opposed to TIFs and Enterprise Zones). One concern, however, is that we could be inadvertently misattributing employment and establishment effects of these other categories of subsidies to the subsidies we study. Therefore, we perform three robustness analyses that target the 131 counties that also have recipients of these other subsidy types. First, we drop any county (and its matched control county) from the sample if the county received one of these other subsidy classes in the same year as the initial subsidy we study. Second, we drop any county (and its matched control county) from the sample if the country received another type of subsidy in the years preceding the initial subsidy we study. Third, we drop any matched pair where there is either a prior or concurrent subsidy. Across all three tests, we find consistent and robust results, supporting that the effects we document are not driven by these other types of subsidies.

6. Conclusions

We test whether and to what extent firm-specific local tax abatements and subsidies are associated with greater local employment and investment, measured with employment, aggregate wages, per capita employment and wages, and business establishments, in counties with tax subsidy recipients. We show that the local information environment plays an important role in this association, finding results consistent with subsidy funds being spent on outcomes other than employment and establishments. Further, we study whether subsidy effectiveness in achieving improvements in local economic conditions varies with the number of subsidies, subsidy size, and the information environment of the granting jurisdictions.

There are four key results from our empirical analysis. First, using three different control groups to benchmark the economic outcomes of counties with subsidy recipients, we generally find evidence of a positive association between tax subsidies and employment outcomes, but mixed evidence for business establishments. In several cases, the effects are delayed or even fully offset on a per capita basis.

Second, we observe that the more effective subsidies appear to be those in jurisdictions that have less information about subsidies. This could be due to jurisdictions granting more politically palatable subsidies in the first place, or to recipients diverting subsidy funds to local organizations or causes to remediate societal effects induced by subsidies. Future research and additional data will be needed to test these possible outcomes and also to determine whether more recent and improved local disclosure initiatives requiring information about firms' use of subsidy dollars facilitate improved monitoring of subsidy use.

Third, some of these positive effects appear attributable to jurisdictions giving multiple subsidies, implying that this policy could be costly if subsequent subsidies are necessary to achieve economic outcomes. When taking into account multiple subsidies, the cost of subsides approaches or even exceeds a half year of wages for each new job.

Finally, effects appear concentrated in the largest subsidy packages by dollar value: although we observe employment effects in the top tercile of subsidies, we observe little to no economic effect for over 1,000 of the smaller first-time subsidies observed that cost approximately $99.8 million in aggregate. Further, megadeals are associated with an increased number of jobs, but we again observe that population growth appears to negate employment effects in those jurisdictions.

These results are subject to several important caveats. First, we acknowledge that counties with subsidy recipients likely differ from other counties. While we address this selection issue by using three alternative control samples, we cannot observe distinct establishments (or their respective political connections) in a particular county or state, and thus are unable to model or control for these effects. Second, we limit the sample to counties with sufficient GJF coverage and without observable tax subsidies prior 2006. However, results may be different with the inclusion of other states and/or could be attributable to subsidies granted prior to 2004 that we cannot observe. Third, there are numerous possible outcomes we could examine, including local-area GDP, tax revenue collections, public services, and costs of these tax policies such as increased pollution, traffic, or housing prices. Given the stated policy benefits of these subsidies and data availability, we focus our analysis on the employment and investment outcomes of number of employees, aggregate wages, per capita rates of employment and wages, and establishments. However, a complete cost-benefit analysis would need to consider all relevant outcomes to assess the net cost or benefit to the local communities. Finally, we cannot consider general equilibrium effects of tax subsidies on employment levels and growth. Nonetheless, we think this work is an important step in understanding the potential effects of these subsidies. We look forward to 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.

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Appendix A

Variable Definitions

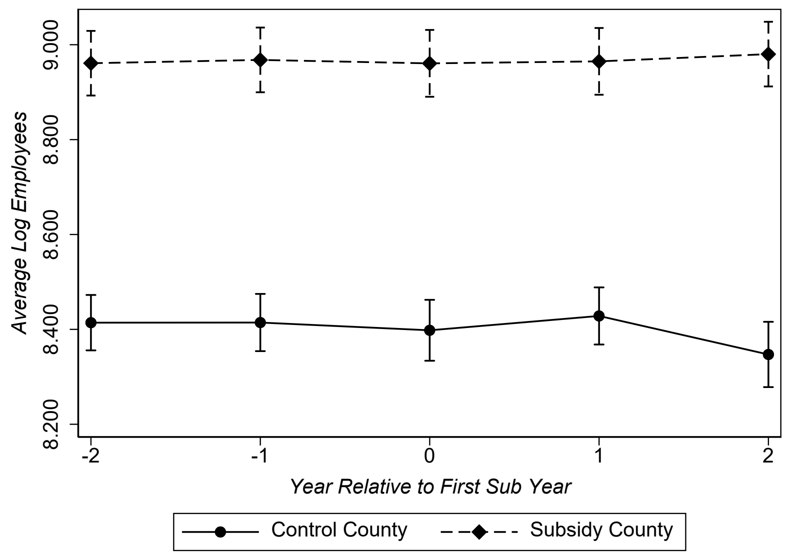
We construct variables using data from the U.S. Census, U.S. Bureau of Labor Statistics, Good Jobs First, and Compustat. We include data codes from Compustat where applicable.

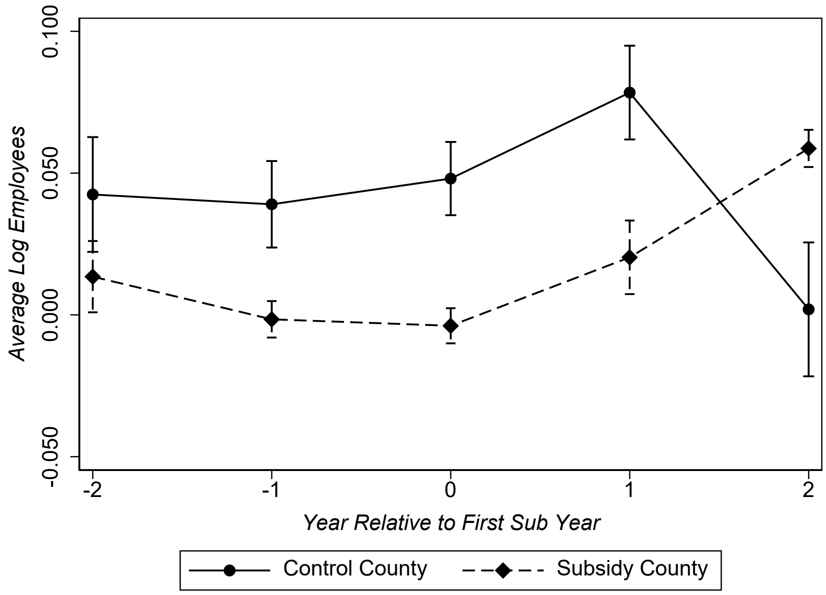
Variable	Definition
Dependent Variables	
$Establishments_{i,t+1}$	Total number of private-sector business establishments in county *i* in year *t*+1; computed by the Bureau of Labor Statistics (BLS) as the average number of active establishments at the end of each quarter.
$Employees_{i,t+1}$	Total number 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-observed 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-observed subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(0-2)}$	Equals 1 for the year of and the two years following the first-observed subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(3-4)}$	Equals 1 for the third and fourth years following the first-observed subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(5-6)}$	Equals 1 for the fifth and sixth years following the first-observed subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$PostTaxSubsidy_{i,(7+)}$	Equals 1 for years including and after the seventh year following the first-observed subsidy received by a firm in county *i* (for the time period 2006-2015) and 0 otherwise.
$Ln(CountSubsidy)_{i,t}$	Log of 1 plus the number of cumulative subsidies in year *t* including and following the first-observed subsidy received by firms in county *i* (for the time period 2006-2015).
$Rural_{i,t}$	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*.
$CorpTaxRate\%_{j,t}$	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*).

Figure 1
Pre-subsidy Trends

Panel A: Ln(Employees)

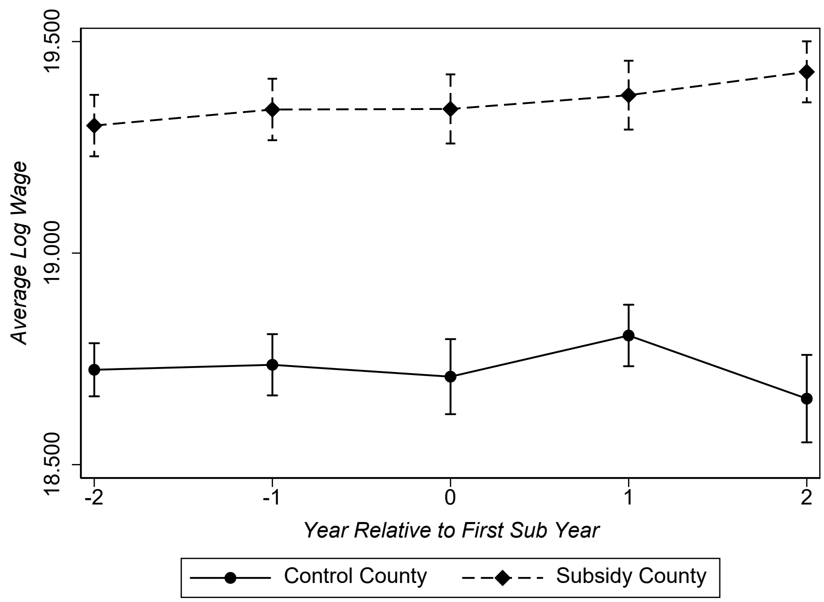


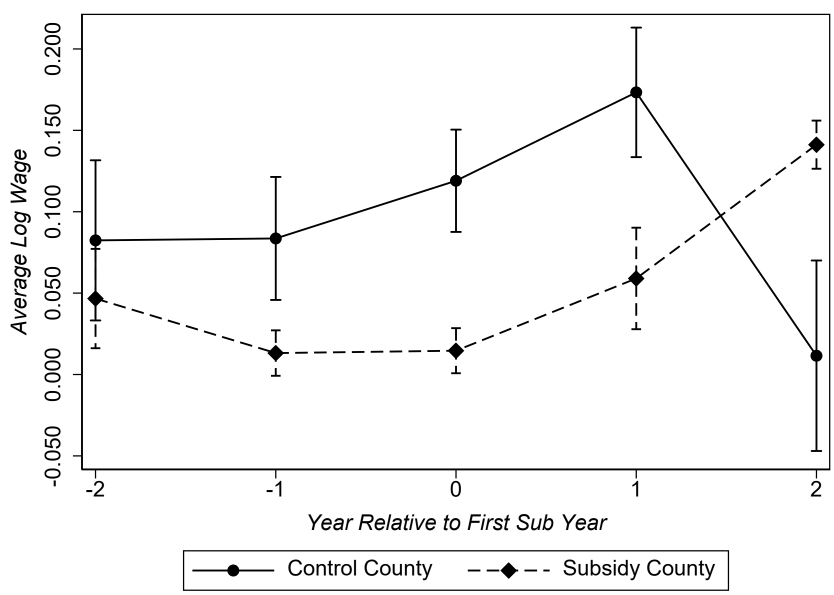


The figures in this panel present trends in the log of the number of total employees at the county-level. The top graph maps the raw value of *Ln(Employees)*, whereas the bottom graph plots residual values from regressing *Ln(Employees)* on control variables and fixed effects.

Figure 1 (continued)
Pre-subsidy Trends

Panel B: Ln(Wages)

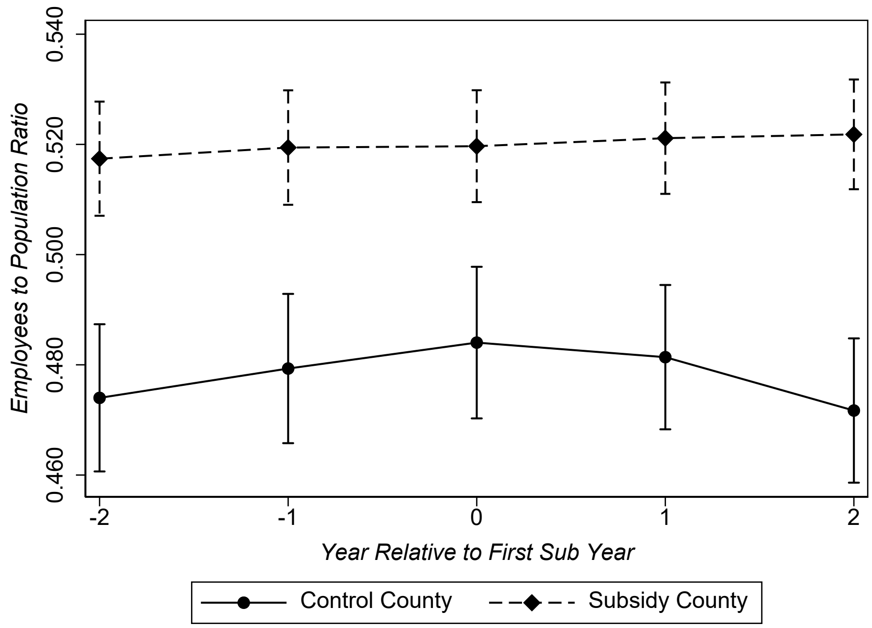


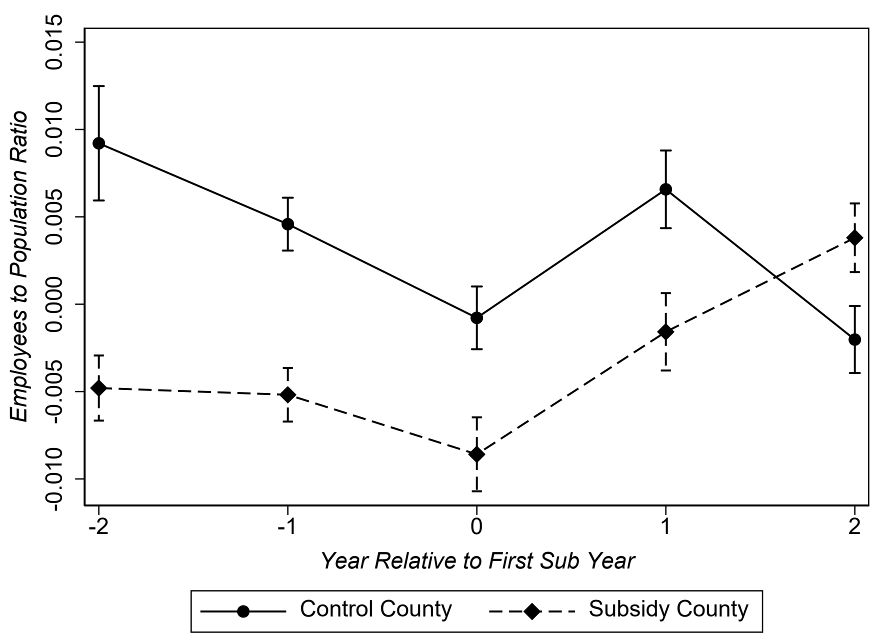


The figures in this panel present trends in the log of aggregate wages at the county-level. The top graph maps the raw value of *Ln(Wages)*, whereas the bottom graph plots residual values from regressing *Ln(Wages)* on control variables and fixed effects.

Figure 1 (continued)
Pre-subsidy Trends

Panel C: Employees/Population

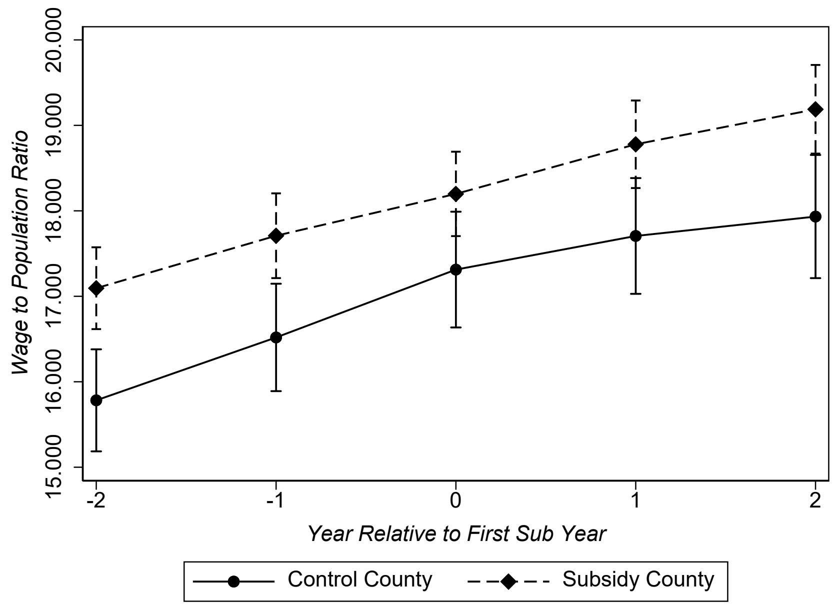


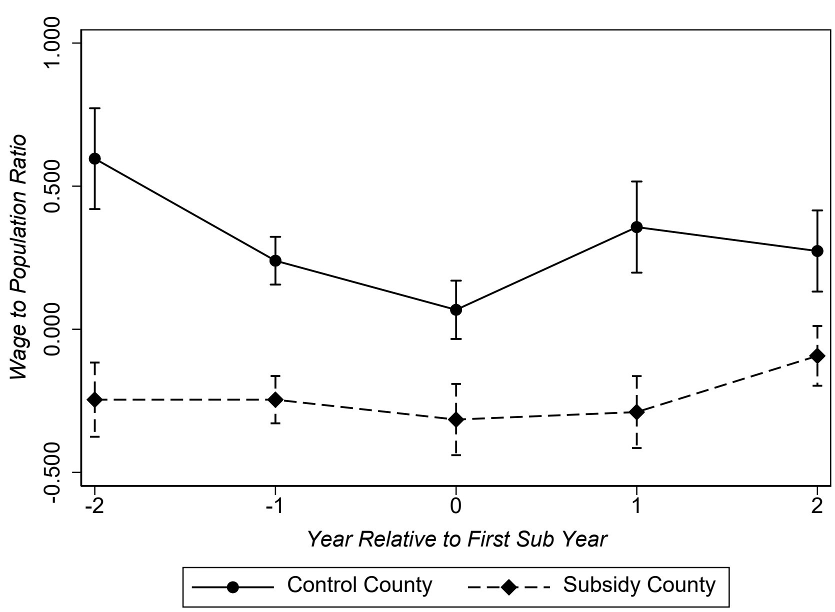


The figures in this panel present trends in the ratio of employees to the working age population at the county-level. The top graph maps the raw value of *Employees/Population*, whereas the bottom graph plots residual values from regressing *Employees/Population* on control variables and fixed effects.

Figure 1 (continued)
Pre-subsidy Trends

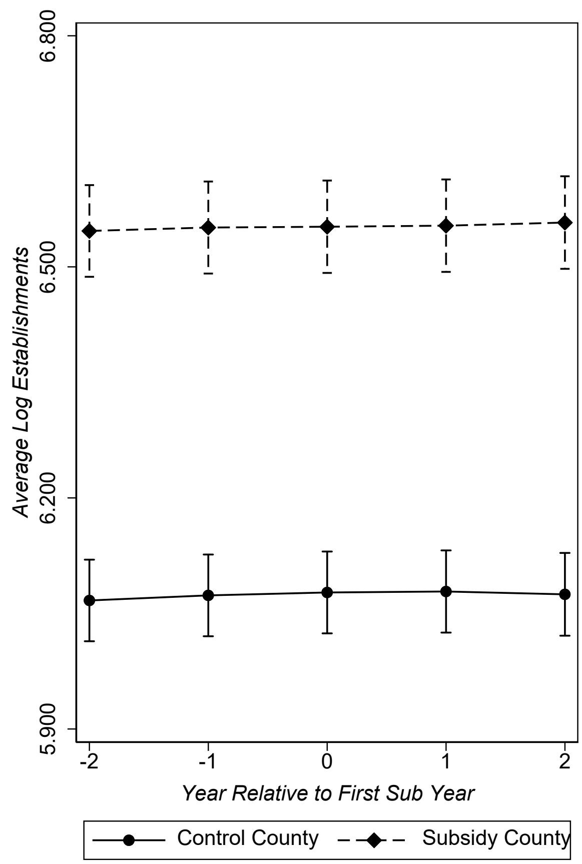
Panel D: Wages/Population

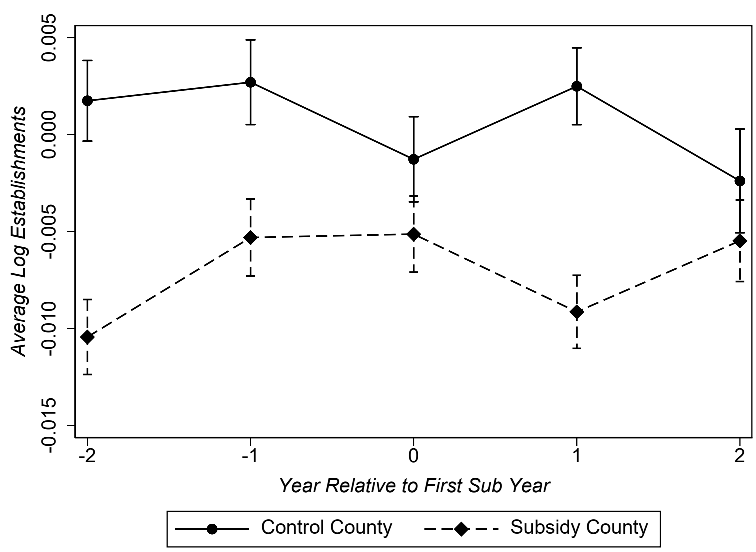




The figures in this panel present trends in the ratio of wages to the working age population at the county-level. The top graph maps the raw value of *Wages/Population*, whereas the bottom graph plots residual values from regressing *Wages/Population* on control variables and fixed effects.

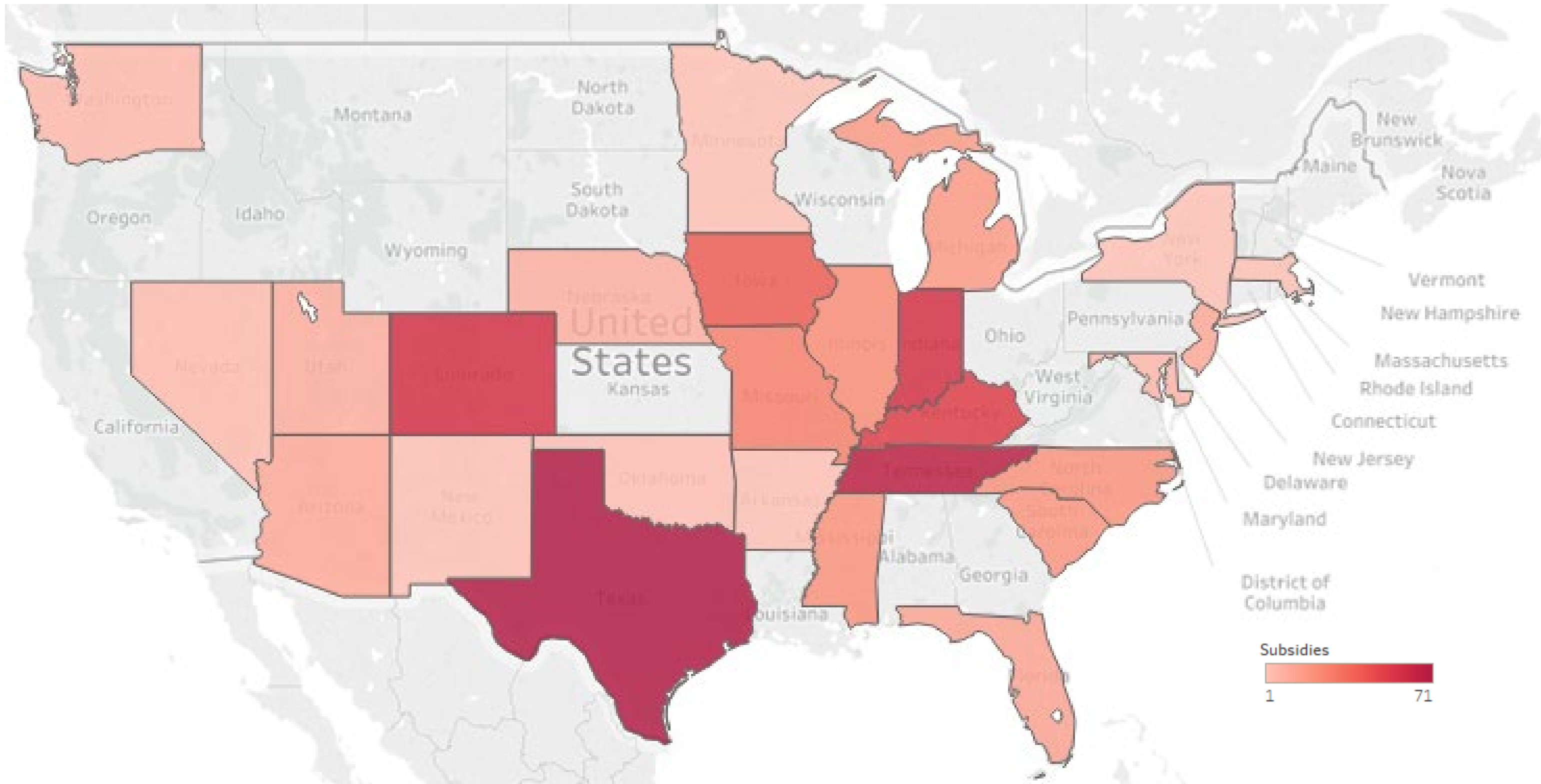
Figure 1 (continued)
Pre-subsidy Trends
Panel E: Establishments





The figures in this panel present 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 2
Map of Number of Subsidy Counties in each State



This figure presents the distribution of non-megadeal subsidy counties across states. We aggregate county-level subsidies to the state level for presentation purposes in this figure. The first-observed subsidies in Alabama, Georgia, Kansas, Virginia, and West Virginia include megadeals and thus are omitted from this figure and from the sample of 12,960 observations used in Tables 2 through 6; see Table 7 for an analysis of these deals. 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	(19,836)	9,384	(1,653)	783
Less: Counties without a matched control	(2,316)	7,068	(193)	589
Matched observations (w. replacement)	7,068	7,068	200	200
Total		**14,136**		**789**
Sample for Tables 1-6		12,960		692
Sample for Table 7 (megadeals)		1,176		97

Panel B: First-Stage Propensity Score Matching Model

	FirstTaxSubsidy
$Rural_{i,t}$	-0.1077***
	[-2.78]
$Ln(Population)_{i,t}$	0.6282***
	[37.35]
$MinWage_{i,t}$	0.3530***
	[20.74]
$Ln(GDP)_{j,t}$	-0.0427
	[-1.44]
$\%Educ_{j,t}$	-0.1181***
	[-23.89]
$\%Union_{j,t}$	0.0236***
	[4.30]
$CorpTaxRate\%_{j,t}$	0.1952***
	[24.34]
$Ln(UIContrib)_{j,t}$	-0.6955***
	[-18.92]
$PropertyTax_{j,t}$	7.8105***
	[13.62]
$PersonalTaxRate\%_{j,t}$	-0.0991***
	[-11.77]
$TaxIncentivesIndex_{j,t}$	-0.0292***
	[-6.00]
$SalesTaxRate\%_{j,t}$	0.0846***
	[9.87]
$Ln(Establishments)_{i,t-3\_t-1}$	-1.4334***
	[-5.31]
$Ln(Employees)_{i,t-3\_t-1}$	1.7445***
	[6.12]
$Ln(Wages)_{i,t-3\_t-1}$	-0.5851***
	[-5.32]
Year FE	Y
Observations	22,032
Pseudo R-squared	0.1907

Table 1
Sample Selection

Panel C: Differences in treatment and control counties

	Distinct Tax Subsidy Counties	Matched Control Counties	Difference	t-stat
Ln(Employees)$_{i,t+1}$	8.956	8.425	0.531	(4.04)
Ln(Wages)$_{i,t+1}$	19.372	18.805	0.567	(3.82)
(Employees/Population)$_{i,t+1}$	0.518	0.480	0.038	(0.77)
(Wages/Population)$_{i,t+1}$	18.779	17.725	1.054	(0.45)
Ln(Establishments)$_{i,t-1}$	6.542	6.075	0.467	(3.66)

Panel D: County-year observations by state

State	#County-Year Observations	State	#County-year Observations
Arizona	264	Missouri	648
Arkansas	24	Nebraska	192
Colorado	1,368	Nevada	72
Florida	312	New Jersey	264
Illinois	528	New Mexico	24
Indiana	1,368	New York	24
Iowa	960	North Carolina	480
Kentucky	1,248	Oklahoma	48
Maryland	120	South Carolina	456
Massachusetts	24	Tennessee	1,608
Michigan	408	Texas	1,704
Minnesota	24	Utah	216
Mississippi	480	Washington	96
		Total	**12,960**

This table presents sample selection details and descriptive statistics. 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 7,068 county-years (representing 589 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 14,136 county-year observations, 12,960 of which represent the non-megadeal sample used in Tables 1 to 6 and 1,176 of which are the megadeals sample studied in Table 7. We use propensity score matching with replacement to pair treatment counties with control counties in the same state based on state and county characteristics, as well as 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 local economic outcomes are all measured at the state-level, propensity score matching within state ensures that these covariates are balanced across the groups. Panel D presents the distribution of non-megadeal county-year observations across 26 state jurisdictions. The first-observed subsidies in Alabama, Georgia, Kansas, Virginia, and West Virginia include megadeals and thus are omitted from the sample of 12,960 observations. 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 in 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
$Employees_{i,t+1}$	21,083.00	4,820.00	58,588.64	2,162.00	14,495.50
$Ln(Employees)_{i,t+1}$	8.65	8.48	1.62	7.68	9.58
$Wages_{i,t+1}$ ($M)	905.47	152.46	3,309.26	66.33	501.53
$Ln(Wages)_{i,t+1}$	18.98	18.84	2.19	18.01	20.03
$(Employees/Population)_{i,t+1}$	0.50	0.45	0.28	0.30	0.64
$(Wages/Population)_{i,t+1}$	18.00	14.20	13.74	8.87	22.57
$Establishments_{i,t+1}$	1,597.91	444.00	4,150.45	215.00	1,198.00
$Ln(Establishments)_{i,t+1}$	6.31	6.10	1.31	5.38	7.09
$PostTaxSubsidy_{i,t}$	0.26	0.00	0.44	0.00	1.00
Control Variables					
$MinWage_{i,t}$	6.57	7.25	1.02	5.15	7.25
$Ln(GDP)_{j,t}$	12.62	12.48	0.73	12.10	12.94
$\%Educ_{j,t}$	26.41	25.30	4.87	23.00	27.80
$\%Union_{j,t}$	5.74	5.20	3.05	3.00	7.60
$CorpTaxRate\%_{j,t}$	5.84	6.25	2.76	4.63	7.50
$Ln(UIContrib)_{j,t}$	11.29	11.26	0.47	10.95	11.50
$PropertyTax_{j,t}$	0.12	0.12	0.04	0.09	0.15
$PersonalTaxRate\%_{j,t}$	3.79	4.66	2.73	0.00	6.04
$TaxIncentivesIndex_{j,t}$	25.82	27.00	3.11	24.00	28.00
$SalesTaxRate\%_{j,t}$	4.30	6.00	2.67	1.49	6.25

Panel B: Types of Subsidies

	Sample of First Subsidy County-Years (n=540)				Sample of All Subsidy County-Years (n=1,457)			
	# 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	1,637	57.8%	1,507	$1,085	9,899	60.0%	9,585	$6,229
Tax Abatements	1,195	42.2%	523	$376	6,608	40.0%	3,603	$1,254
Total	**2,832**	**100.0%**	**2,030**	**$1,461**	**16,507**	**100.0%**	**13,188**	**$7,483**

Table 2 (cont'd)

Panel C: Subsidies by year

Year	Sample of First Subsidy County-Years (n=540)	Sample of All Subsidy County-Years (n=1,457)
2006	123	123
2007	582	663
2008	64	623
2009	46	686
2010	55	637
2011	518	1,343
2012	61	661
2013	1,346	2,994
2014	32	5,592
2015	5	3,185
Total	**2,832**	**16,507**

This table describes the sample of county-year observations and tax subsidies. Panel A presents descriptive statistics for the dependent variables, variable of interest $PostTaxSubsidy_{i,t}$, and control variables for the sample of 12,960 county-year observations. Panel B provides information on sample composition by type of tax subsidy included in the sample of 540 county-years for which we observe the first post-2005 tax subsidy and for all 1,457 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 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) $Ln(Employees)_{i,t+1}$		**0.94**	**0.52**	**0.50**	**0.95**	**0.14**	**0.06**	**0.05**	**0.23**	**0.08**	**0.09**	**0.18**	**0.11**	**0.12**	**-0.10**	**-0.05**
(2) $Ln(Wages)_{i,t+1}$	**0.99**		**0.48**	**0.49**	**0.92**	**0.14**	**0.08**	**0.07**	**0.22**	**0.09**	**0.07**	**0.16**	**0.11**	**0.05**	**-0.03**	**-0.08**
(3) $(Employees/Population)_{i,t+1}$	**0.69**	**0.71**		**0.90**	**0.55**	**0.06**	**0.06**	**0.02**	**0.43**	**0.05**	**-0.02**	**0.08**	**0.20**	**0.12**	**-0.06**	**-0.15**
(4) $(Wages/Population)_{i,t+1}$	**0.67**	**0.73**	**0.95**		**0.53**	**0.10**	**0.17**	**0.07**	**0.46**	0.01	**-0.08**	**0.10**	**0.21**	**0.07**	**-0.08**	**-0.21**
(5) $Ln(Establishments)_{i,t-1}$	**0.91**	**0.75**	**0.39**	**0.40**		**0.13**	**0.09**	**0.06**	**0.26**	**0.06**	**0.08**	**0.22**	**0.14**	**0.17**	**-0.18**	**-0.04**
(6) $PostTaxSubsidy_{i,t}$	**0.15**	**0.17**	**0.12**	**0.19**	**0.14**		**0.34**	**0.13**	0.00	0.01	**-0.04**	**0.09**	**0.07**	-0.02	**0.03**	**-0.24**
(7) $MinWage_{i,t}$	**0.06**	**0.11**	**0.08**	**0.18**	**0.07**	**0.33**		**0.18**	**0.35**	**0.05**	-0.01	**0.31**	**0.25**	**0.03**	**-0.09**	**-0.46**
(8) $Ln(GDP)_{j,t}$	**0.10**	**0.11**	**0.08**	**0.11**	**0.11**	**0.15**	**0.27**		**0.24**	**-0.15**	**-0.54**	**-0.13**	**0.66**	**-0.55**	**0.17**	**-0.07**
(9) $\%Educ_{j,t}$	**0.26**	**0.28**	**0.39**	**0.40**	**0.26**	**0.05**	**0.42**	**0.44**		**0.03**	**-0.11**	**0.15**	**0.49**	**0.15**	**-0.28**	**-0.40**
(10) $\%Union_{j,t}$	**0.04**	**0.02**	**0.11**	**0.08**	**0.05**	0.00	**0.06**	**-0.14**	**-0.07**		**0.32**	**0.16**	**0.10**	**0.33**	**0.14**	**0.03**
(11) $CorpTaxRate_{j,t}$	**0.11**	**0.09**	**-0.02**	**-0.04**	**0.13**	**-0.03**	**-0.06**	**-0.29**	**-0.23**	**0.41**		**0.24**	**-0.23**	**0.49**	**0.12**	**0.03**
(12) $Ln(UIContrib)_{j,t}$	**0.14**	**0.15**	**0.07**	**0.10**	**0.19**	**0.12**	**0.33**	**-0.08**	**0.18**	**0.17**	**0.22**		**0.02**	**0.31**	**-0.20**	**-0.20**
(13) $PropertyTax_{j,t}$	**0.11**	**0.12**	**0.29**	**0.29**	**0.12**	**0.06**	**0.25**	**0.62**	**0.60**	**0.06**	**-0.17**	**-0.07**		**-0.15**	**0.11**	**-0.09**
(14) $PersonalTaxRate_{j,t}$	**0.12**	**0.10**	**0.07**	**0.04**	**0.14**	-0.01	0.00	**-0.54**	0.01	**0.24**	**0.34**	**0.39**	**-0.27**		**-0.24**	**-0.14**
(15) $TaxIncentivesIndex_{j,t}$	**-0.11**	**-0.11**	**-0.02**	**-0.03**	**-0.10**	**0.05**	**-0.16**	**0.10**	**-0.35**	**0.13**	**0.24**	**-0.16**	**0.15**	**-0.23**		**0.09**
(16) $SalesTaxRate_{j,t}$	**-0.06**	**-0.09**	**-0.18**	**-0.21**	**-0.07**	**-0.17**	**-0.38**	0.00	**-0.47**	**-0.04**	**0.10**	**-0.25**	**-0.11**	**-0.34**	**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:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0594****		**0.1212***		**0.0137****		**0.8017****		**0.0050**	
	[2.11]		**[1.90]**		**[2.42]**		**[1.97]**		**(0.63)**	
$PostTaxSubsidy_{i,(0\text{-}2)}$		**0.0563***		**0.1181**		**0.0114****		**0.5227**		**0.0002**
		[1.73]		**[1.56]**		**[2.16]**		**[1.46]**		**[0.02]**
$PostTaxSubsidy_{i,(3\text{-}4)}$		**0.0643*****		**0.1224****		**0.0189*****		**1.4459*****		**0.0166***
		[2.96]		**[2.58]**		**[2.62]**		**[2.74]**		**[1.77]**
$PostTaxSubsidy_{i,(5\text{-}6)}$		**0.0783*****		**0.1472****		**0.0229*****		**1.8248*****		**0.0207**
		[2.70]		**[2.30]**		**[2.61]**		**[2.77]**		**[1.58]**
$PostTaxSubsidy_{i,(7+)}$		**0.0568**		**0.0716**		**0.0290*****		**2.3186*****		**0.0301***
		[1.10]		**[0.54]**		**[2.64]**		**[2.90]**		**[1.84]**
$MinWage_{i,t}$	0.0033	0.0029	0.0134	0.0129	-0.0028	-0.0030	0.2641	0.2474	-0.0006	-0.0009
	[0.30]	[0.27]	[0.62]	[0.61]	[-0.42]	[-0.44]	[1.05]	[0.97]	(-0.21)	[-0.31]
$Ln(GDP)_{j,t}$	0.9079***	0.9009***	1.6683**	1.6560**	0.2016***	0.1991***	20.6837***	20.3831***	0.3679***	0.3632***
	[3.00]	[2.96]	[2.24]	[2.20]	[4.30]	[4.30]	[5.43]	[5.43]	(8.16)	[8.08]
$\%Educ_{j,t}$	0.0011	0.0013	0.0105	0.0111	-0.0001	-0.0002	-0.0842	-0.0891	-0.0051**	-0.0052**
	[0.11]	[0.12]	[0.40]	[0.40]	[-0.08]	[-0.11]	[-1.11]	[-1.17]	(-2.30)	[-2.31]
$\%Union_{j,t}$	-0.0199*	-0.0196*	-0.0431*	-0.0429	-0.0058***	-0.0056**	-0.2753*	-0.2495	0.0087***	0.0092***
	[-1.80]	[-1.72]	[-1.67]	[-1.59]	[-2.59]	[-2.55]	[-1.67]	[-1.57]	(3.53)	[3.76]
$CorpTaxRate_{j,t}$	-0.0062	-0.0061	-0.0106	-0.0108	0.0004	0.0006	0.0715	0.0970	-0.0012	-0.0007
	[-1.17]	[-1.08]	[-0.86]	[-0.82]	[0.38]	[0.58]	[0.99]	[1.33]	(-0.82)	[-0.52]
$Ln(UIContrib)_{j,t}$	-0.0499	-0.0492	-0.1048	-0.1045	-0.0231**	-0.0224**	-0.9610	-0.8720	-0.0344**	-0.0328**
	[-1.17]	[-1.11]	[-1.23]	[-1.16]	[-2.18]	[-2.15]	[-1.54]	[-1.44]	(-2.22)	[-2.14]
$PropertyTax_{j,t}$	0.2765	0.2821	0.0834	0.0741	-0.1727	-0.1635	-1.9336	-0.9473	0.0515	0.0678
	[0.98]	[0.93]	[0.15]	[0.12]	[-1.40]	[-1.33]	[-0.39]	[-0.20]	(0.35)	[0.47]
$PersonalTaxRate_{j,t}$	0.0727	0.0721	0.1827	0.1823	0.0042	0.0038	0.3196**	0.2678*	0.0068*	0.0059
	[1.05]	[1.02]	[1.07]	[1.05]	[1.55]	[1.37]	[2.08]	[1.73]	(1.75)	[1.52]
$TaxIncentivesIndex_{j,t}$	0.0616	0.0616	0.1424	0.1420	0.0003	0.0003	0.0161	0.0233	0.0051**	0.0052**
	[1.23]	[1.24]	[1.16]	[1.17]	[0.19]	[0.24]	[0.19]	[0.29]	(2.34)	[2.40]
$SalesTaxRate_{j,t}$	-0.0328	-0.0324	-0.0857	-0.0864	-0.0056	-0.0050	0.4322	0.5038*	0.0023	0.0035
	[-0.82]	[-0.76]	[-0.87]	[-0.83]	[-1.23]	[-1.09]	[1.40]	[1.65]	(0.42)	[0.63]
Adj. R-squared	0.919	0.919	0.724	0.724	0.968	0.968	0.949	0.949	0.998	0.998

This panel presents results of testing the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)). for the sample of 12,960 non-megadeal 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 with State-by-year Fixed Effects

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0318****		**0.0618****		**0.0005**		**0.4946****		**0.0003**	
	[2.38]		**[2.18]**		**[0.16]**		**[2.34]**		**(0.10)**	
$PostTaxSubsidy_{i,(0\text{-}2)}$		**0.0253****		**0.0524****		**-0.0008**		**0.2545**		**-0.0029**
		[2.34]		**[1.99]**		**[-0.37]**		**[1.63]**		**[-1.08]**
$PostTaxSubsidy_{i,(3\text{-}4)}$		**0.0417*****		**0.0745****		**0.0039**		**1.0137*****		**0.0056**
		[2.85]		**[2.44]**		**[0.68]**		**[2.78]**		**[1.53]**
$PostTaxSubsidy_{i,(5\text{-}6)}$		**0.0664****		**0.1219***		**0.0025**		**1.1793*****		**0.0139*****
		[2.15]		**[1.72]**		**[0.39]**		**[3.19]**		**[2.62]**
$PostTaxSubsidy_{i,(7+)}$		**0.0306****		**0.0155**		**0.0055**		**1.6838*****		**0.0237*****
		[1.98]		**[0.40]**		**[0.78]**		**[3.71]**		**[3.63]**
Adj. R-squared	0.979	0.979	0.902	0.902	0.970	0.970	0.962	0.963	0.999	0.999

This panel presents results of using an entropy-balanced matched sample to test the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the unweighted (weighted) sample of 15,324 (12,960) county-year observations. We define all variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-year fixed effects, and standard errors are clustered by county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 4
Relation between Tax Subsidies and Local Activity

Panel C: Staggered Design

Dep Var.	$Ln(Employees)_{i,t+1}$	$Ln(Wages)_{i,t+1}$	$(Employees / Population)_{i,t+1}$	$(Wages / Population)_{i,t+1}$	$Ln(Establishments)_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
$PostTaxSubsidy_{i,(0-2)}$	**0.0097****	**0.0084**	**0.0011**	**0.0060**	**0.0085*****
	[1.98]	**[0.94]**	**[0.41]**	**[0.05]**	**[2.89]**
$PostTaxSubsidy_{i,(3-4)}$	**0.0313***	**0.0485**	**0.0063**	**0.5547****	**0.0277*****
	[1.94]	**[1.30]**	**[1.34]**	**[2.17]**	**[4.91]**
$PostTaxSubsidy_{i,(5-6)}$	**0.0334**	**0.0527**	**0.0035**	**0.5551***	**0.0346*****
	[1.63]	**[1.16]**	**[0.59]**	**[1.71]**	**[4.06]**
$PostTaxSubsidy_{i,(7+)}$	**0.0375***	**0.0569**	**0.0033**	**0.6814**	**0.0444*****
	[1.67]	**[1.22]**	**[0.45]**	**[1.61]**	**[3.75]**
$MinWage_{i,t}$	0.0150***	0.0314***	0.0075***	0.4307***	-0.0008
	[3.34]	[3.34]	[4.65]	[4.09]	[-0.32]
$Ln(GDP)_{j,t}$	0.5276***	0.8215***	0.2712***	23.3741***	0.3621***
	[5.58]	[5.00]	[4.86]	[5.43]	[7.07]
$\%Educ_{j,t}$	-0.0085**	-0.0147*	0.0013	-0.0146	-0.0034***
	[-2.22]	[-1.70]	[0.60]	[-0.19]	[-2.72]
$\%Union_{j,t}$	-0.0004	0.0009	-0.0030***	-0.2038***	0.0087***
	[-0.20]	[0.32]	[-2.64]	[-3.36]	[4.95]
$CorpTaxRate_{j,t}$	-0.0078	-0.0175	-0.0004	0.1033*	-0.0001
	[-1.14]	[-1.05]	[-0.42]	[1.75]	[-0.10]
$Ln(UIContrib)_{j,t}$	-0.0125	-0.0433*	-0.0182*	-1.1790**	-0.0330***
	[-0.75]	[-1.68]	[-1.73]	[-2.29]	[-3.82]
$PropertyTax_{j,t}$	-0.1075	0.2142	-0.2077***	-2.7946	0.0595
	[-0.85]	[0.98]	[-3.33]	[-0.87]	[0.78]
$PersonalTaxRate_{j,t}$	-0.0046	0.0006	0.0041*	0.0336	0.0052
	[-1.11]	[0.11]	[1.68]	[0.26]	[1.59]
$TaxIncentivesIndex_{j,t}$	0.0028**	0.0017	-0.0016***	-0.0444	0.0042***
	[2.00]	[0.65]	[-2.94]	[-1.30]	[4.71]
$SalesTaxRate_{j,t}$	0.0003	0.0027	-0.0006	0.0559	-0.0005
	[0.08]	[0.41]	[-0.23]	[0.44]	[-0.20]
Adj. R-squared	0.990	0.956	0.937	0.955	0.999

This panel presents results testing the relation between subsidies and the log of the number of employees (Column (1)), the log of aggregate wages (Column (2)), employees scaled by population (Column (3)), aggregate wages scaled by population (Column (4)), and the log of the number of establishments (Column (5)) for the sample of 6,216 non-megadeal county-year observations with at least one subsidy recipient. 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5
Disclosure and Information Dissemination

Panel A: Summary of Differences in Coefficients across Partitions of Having vs. Not Having a Disclosure Initiative

Dep Var:	$Ln(Employees)_{i,t+1}$	$Ln(Wages)_{i,t+1}$	$(Employees / Population)_{i,t+1}$	$(Wages / Population)_{i,t+1}$	$Ln(Establishments)_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
$PostTaxSubsidy_{i,t}$	**-0.1678***	**-0.3931***	**0.0065**	**-1.0214**	**0.0018**
	[-1.82]	[-1.78]	[0.62]	[-1.41]	**[0.09]**
$PostTaxSubsidy_{i,(0-2)}$	-0.1714*	-0.4065*	0.0050	-1.0501	0.0003
	[1.70]	[-1.65]	[0.52]	[-1.64]	[0.02]
$PostTaxSubsidy_{i,(3-4)}$	-0.1314*	-0.3007*	0.0120	-0.6485	0.0154
	[-1.78]	[-1.70]	[0.94]	[-0.72]	[0.69]
$PostTaxSubsidy_{i,(5-6)}$	-0.3199*	-0.7656*	0.0122	-0.6883	-0.0097
	[-1.65]	[-1.65]	[0.72]	[-0.63]	[-0.31]
$PostTaxSubsidy_{i,(7+)}$	-0.1027	-0.2167	0.0128	-1.1898	-0.0335
	[-1.03]	[-0.83]	[0.60]	[-0.83]	[-0.96]

Panel B: Summary of Differences in Coefficients across Partitions of Above vs. Below Median Google Trends

Dep Var:	$Ln(Employees)_{i,t+1}$	$Ln(Wages)_{i,t+1}$	$(Employees / Population)_{i,t+1}$	$(Wages / Population)_{i,t+1}$	$Ln(Establishments)_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
$PostTaxSubsidy_{i,t}$	**-0.0777***	**-0.1721***	**0.0089**	**-0.6844**	**-0.0105**
	[-1.77]	**[-1.67]**	**[0.83]**	**[-0.88]**	**[-0.75]**
$PostTaxSubsidy_{i,(0-2)}$	-0.0758*	-0.0175	0.0153	-0.3447	-0.0102
	[-1.71]	[-1.64]	[1.44]	[-0.47]	[-0.77]
$PostTaxSubsidy_{i,(3-4)}$	-0.0836	-0.1758	-0.0044	-1.2351	-0.0053
	[-1.62]	[-1.48]	[-0.36]	[-1/36]	[-0.34]
$PostTaxSubsidy_{i,(5-6)}$	-0.0704	-0.1286	-0.0054	-1.5627	-0.0196
	[-1.27]	[-1.07]	[-0.35]	[-1.32]	[-0.91]
$PostTaxSubsidy_{i,(7+)}$	-0.0273	-0.0348	0.0055	-1.2424	-0.0156
	[-0.38]	[-0.21]	[0.29]	[-0.85]	[-0.61]

Table 5
Disclosure and Information Dissemination

Panel C: Summary of Differences in Coefficients across Partitions of Having vs. Not Having a Local Newspaper

Dep Var:	$Ln(Employees)_{i,t+1}$	$Ln(Wages)_{i,t+1}$	$(Employees / Population)_{i,t+1}$	$(Wages / Population)_{i,t+1}$	$Ln(Establishments)_{i,t+1}$
	(1)	(2)	(3)	(4)	(5)
$PostTaxSubsidy_{i,t}$	**-0.0223**	**-0.0536**	**-0.0171****	**-1.0429***	**-0.0020**
	[1.13]	**[1.20]**	**[2.17]**	**[1.89]**	**[0.21]**
$PostTaxSubsidy_{i,(0-2)}$	-0.0166	-0.0389	-0.0157**	-1.0059*	-0.0017
	[-0.83]	[0.86]	[-2.12]	[-2.03]	[-0.19]
$PostTaxSubsidy_{i,(3-4)}$	-0.0366	-0.0857	-0.0216**	-1.2618*	-0.0024
	[-1.37]	[-1.38]	[-2.02]	[-1.67]	[-0.20]
$PostTaxSubsidy_{i,(5-6)}$	-0.0456	-0.1101	-0.0249*	-1.4472	-0.0200
	[-1.25]	[-1.31]	[-1.94]	[-1.59]	[-1.23]
$PostTaxSubsidy_{i,(7+)}$	-0.0551	-0.1563*	-0.0134	-0.3478	-0.0394*
	[-1.32]	[-1.79]	[-0.85]	[-0.34]	[-1.92]

This table presents results of testing differences in the relation between subsidies and local economic activity for the propensity-score matched sample of 12,960 non-megadeal county-year observations based on whether the state has a subsidy disclosure initiative (Panel A), the amount of attention given to subsidies (Panel B), and the existence of a local newspaper in the county (Panel C). In Panel A, we partition subsidy counties based on whether the corresponding state had an initiative in place at the time of subsidy grant that required disclosure at the state level. In Panel B, we partition based on within-year Google search interest for four primary subsidy-related search terms ("subsidy", "subsidies", "tax break", and "tax breaks"), relative to all other Google searches conducted in those states. We partition above and below the median relative search volume based on the initial subsidy year (that is, whether subsidy-related searches, as a fraction of all Google searches, were higher or lower than the median across all states for that year). In Panel C, we partition counties based on the existence of a local newspaper using newspaper closure data from Gao et al. (2019) and 2019 data on local newspapers from Editors & Publishers. We present differences in the coefficients across these partitions in each of the panels above; the full regression results for each partition are presented in the Online Appendix, Table 2. 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.

Table 6
Multiple Subsidies

Propensity Score Matching within State with Number of Subsidies

Dep Var:	*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*		*Ln(Establishments)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ln(CountSubsidy)$_{i,t}$	**0.0290*** **	**0.0161***	**0.0500** **	**0.0143**	**0.0078** **	**0.0060***	**0.3313**	**0.0963**	**0.0024**	**0.0013**
	[2.92]	**[1.93]**	**[2.37]**	**[0.74]**	**[2.45]**	**[1.71]**	**[1.51]**	**[0.44]**	**[0.60]**	**[0.37]**
PostTaxSubsidy$_{i,t}$		**0.0364**		**0.1007**		**0.0051**		**0.6636***		**0.0031**
		[1.10]		**[1.28]**		**[0.94]**		**[1.80]**		**[0.48]**
MinWage$_{i,t}$	0.0012	0.0024	0.0094	0.0126	-0.0033	-0.0032	0.2375	0.2587	-0.0008	-0.0007
	[0.11]	[0.22]	[0.42]	[0.60]	[-0.49]	[-0.46]	[0.93]	[1.01]	[-0.27]	[-0.24]
Ln(GDP)$_{j,t}$	0.9157***	0.9119***	1.6824**	1.6719**	0.2037***	0.2031***	20.7772***	20.7079***	0.3685***	0.3682***
	[3.01]	[3.02]	[2.24]	[2.25]	[4.36]	[4.36]	[5.45]	[5.45]	[8.15]	[8.16]
%Educ$_{j,t}$	0.0008	0.0009	0.0101	0.0103	-0.0002	-0.0002	-0.0869	-0.0857	-0.0051**	-0.0051**
	[0.08]	[0.09]	[0.38]	[0.39]	[-0.14]	[-0.13]	[-1.13]	[-1.12]	[-2.30]	[-2.30]
%Union$_{j,t}$	-0.0218*	-0.0210*	-0.0463*	-0.0440*	-0.0063***	-0.0062***	-0.2972*	-0.2820	0.0086***	0.0086***
	[-1.88]	[-1.92]	[-1.72]	[-1.74]	[-2.72]	[-2.67]	[-1.68]	[-1.62]	[3.25]	[3.35]
CorpTaxRate$_{j,t}$	-0.0057	-0.0059	-0.0097	-0.0103	0.0006	0.0005	0.0775	0.0737	-0.0011	-0.0011
	[-1.08]	[-1.10]	[-0.80]	[-0.83]	[0.52]	[0.50]	[1.03]	[1.00]	[-0.78]	[-0.79]
Ln(UIContrib)$_{j,t}$	-0.0595	-0.0548	-0.1221	-0.1091	-0.0256**	-0.0249**	-1.0760*	-0.9904	-0.0352**	-0.0348**
	[-1.35]	[-1.33]	[-1.37]	[-1.33]	[-2.42]	[-2.31]	[-1.73]	[-1.57]	[-2.19]	[-2.18]
PropertyTax$_{j,t}$	0.2826	0.2897	0.0757	0.0952	-0.1688	-0.1678	-1.9833	-1.8542	0.0520	0.0526
	[1.00]	[1.02]	[0.14]	[0.17]	[-1.36]	[-1.36]	[-0.39]	[-0.37]	[0.36]	[0.36]
PersonalTaxRate$_{j,t}$	0.0710	0.0718	0.1797	0.1818	0.0038	0.0039	0.2997*	0.3140**	0.0066*	0.0067*
	[1.03]	[1.03]	[1.06]	[1.06]	[1.37]	[1.40]	[1.89]	[1.98]	[1.72]	[1.73]
TaxIncentivesIndex$_{j,t}$	0.0613	0.0615	0.1419	0.1423	0.0002	0.0002	0.0130	0.0152	0.0051**	0.0051**
	[1.23]	[1.23]	[1.16]	[1.16]	[0.13]	[0.15]	[0.16]	[0.18]	[2.34]	[2.35]
SalesTaxRate$_{j,t}$	-0.0346	-0.0341	-0.0880	-0.0869	-0.0062	-0.0061	0.4163	0.4242	0.0022	0.0022
	[-0.86]	[-0.85]	[-0.89]	[-0.89]	[-1.35]	[-1.34]	[1.30]	[1.32]	[0.38]	[0.39]
Adj. R-squared	0.919	0.919	0.724	0.724	0.968	0.968	0.949	0.949	0.998	0.998

This table presents results of testing the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the sample of 12,960 non-megadeal county-year observations. We examine the relation between local economic activity and the number of subsidies (*Ln(CountSubsidy$_{i,t}$)* measured as the logarithm of one plus the number of cumulative subsidies) and the incidence of subsidies (*PostTaxSubsidy$_{i,t}$*). 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.

Panel A: Propensity Score Matching by Tercile of Subsidy Size ($)

	Bottom Tercile (1)	Middle Tercile (2)	Top Tercile (3)
		$Ln(Employees)_{i,t+1}$	
$PostTaxSubsidy_{i,t}$	0.0128	0.0310	0.0493*
	[0.55]	[1.40]	[1.92]
Adj. R-squared	0.0158	0.0663	0.0223
	Difference	t-statistic	
1st Tercile vs. 2nd	0.0182	0.64	
1st Tercile vs. 3rd	0.0365	1.17	
2nd Tercile vs. 3rd	0.0183	0.85	
		$Ln(Wages)_{i,t+1}$	
$PostTaxSubsidy_{i,t}$	0.0046	0.0477	0.1201**
	[0.09]	[1.27]	[1.99]
Adj. R-squared	0.0127	0.0924	0.00724
	Difference	t-statistic	
1st Tercile vs. 2nd	0.0431	0.72	
1st Tercile vs. 3rd	0.1155	1.52	
2nd Tercile vs. 3rd	0.0724	1.33	
		$(Employees/Population)_{i,t+1}$	
$PostTaxSubsidy_{i,t}$	0.0309**	0.0111*	0.0045
	[2.05]	[1.69]	[0.43]
Adj. R-squared	0.0523	0.0380	0.0409
	Difference	t-statistic	
1st Tercile vs. 2nd	-0.0198	-1.30	
1st Tercile vs. 3rd	-0.0264	-1.47	
2nd Tercile vs. 3rd	-0.0066	-0.64	
		$(Wages/Population)_{i,t+1}$	
$PostTaxSubsidy_{i,t}$	0.1538	0.7080	1.4993**
	[0.22]	[1.39]	[2.02]
Adj. R-squared	0.0189	0.0942	0.0746
	Difference	t-statistic	
1st Tercile vs. 2nd	0.5542	0.88	
1st Tercile vs. 3rd	1.3455	1.53	
2nd Tercile vs. 3rd	0.7913	1.17	
		$Ln(Establishments)_{i,t+1}$	
$PostTaxSubsidy_{i,t}$	0.0026	0.0034	0.0001
	[0.22]	[0.37]	[0.01]
Adj. R-squared	0.107	0.179	0.0598
	Difference	t-statistic	
1st Tercile vs. 2nd	0.0008	0.07	
1st Tercile vs. 3rd	-0.0025	-0.20	
2nd Tercile vs. 3rd	-0.0033	-0.31	

This panel presents results of 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 9,072 county-year observations). We partition the sample into terciles based on subsidy dollar value (each tercile contains on average 3,024 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.

Panel B: Propensity Score Matching within State for Megadeals Only

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0302***		**0.0319**		**-0.0071**		**0.5492**		**0.0140**	
	[1.96]		**[1.31]**		**[-1.02]**		**[1.25]**		**[1.15]**	
$PostTaxSubsidy_{i,(0-2)}$		**0.0168**		**0.0204**		**-0.0102***		**0.3089**		**0.0059**
		[1.37]		**[1.08]**		**[-1.76]**		**[0.93]**		**[0.56]**
$PostTaxSubsidy_{i,(3-4)}$		**0.0499****		**0.0466**		**-0.0035**		**0.8070**		**0.0289***
		[2.38]		**[1.42]**		**[-0.32]**		**[1.21]**		**[1.89]**
$PostTaxSubsidy_{i,(5-6)}$		**0.0756*****		**0.0740**		**0.0043**		**1.4956**		**0.0370***
		[2.68]		**[1.54]**		**[0.33]**		**[1.59]**		**[1.72]**
$PostTaxSubsidy_{i,(7+)}$		**0.1250*****		**0.1402****		**0.0233**		**3.2285****		**0.0461**
		[3.36]		**[2.29]**		**[1.25]**		**[2.32]**		**[1.63]**
$MinWage_{i,t}$	0.0132*	0.0166**	0.0262**	0.0302**	0.0038	0.0049	0.8192*	0.9184**	-0.0052	-0.0040
	[1.87]	[2.26]	[2.22]	[2.49]	[0.88]	[1.08]	[1.81]	[2.09]	[-0.88]	[-0.66]
$Ln(GDP)_{j,t}$	0.4270***	0.4233***	0.7629***	0.7573***	0.0732	0.0715	16.0103***	15.8511***	0.3925***	0.3927***
	[3.02]	[3.16]	[3.61]	[3.47]	[0.96]	[0.90]	[3.15]	[2.92]	[2.72]	[2.82]
$\%Educ_{j,t}$	-0.0028	-0.0028	-0.0066	-0.0065	0.0026	0.0026	-0.0938	-0.0912	-0.0013	-0.0013
	[-0.55]	[-0.57]	[-0.92]	[-0.91]	[1.15]	[1.17]	[-0.60]	[-0.58]	[-0.39]	[-0.41]
$\%Union_{j,t}$	-0.0067	-0.0063	-0.0122	-0.0118	-0.0033	-0.0032	-0.2382	-0.2291	-0.0046	-0.0044
	[-1.14]	[-1.14]	[-1.45]	[-1.48]	[-1.55]	[-1.54]	[-1.49]	[-1.48]	[-0.88]	[-0.85]
$CorpTaxRate_{j,t}$	0.0034	0.0039	0.0018	0.0027	0.0020	0.0023	-0.0351	-0.0115	0.0049	0.0050
	[0.63]	[0.84]	[0.28]	[0.46]	[0.74]	[0.80]	[-0.17]	[-0.06]	[1.29]	[1.48]
$Ln(UIContrib)_{j,t}$	-0.0115	-0.0145	0.0128	0.0089	0.0090	0.0079	0.7811	0.6825	-0.0267	-0.0274
	[-0.40]	[-0.54]	[0.36]	[0.26]	[0.58]	[0.52]	[0.91]	[0.81]	[-1.46]	[-1.51]
$PropertyTax_{j,t}$	0.5964	0.7566*	0.7831	0.9596*	-0.2094	-0.1601	8.9712	13.2804	0.6314**	0.6905**
	[1.38]	[1.93]	[1.29]	[1.71]	[-1.04]	[-0.84]	[0.65]	[1.03]	[2.41]	[2.58]
$PersonalTaxRate_{j,t}$	0.0024	0.0095	0.0001	0.0077	0.0026	0.0047	0.4569	0.6401	-0.0065	-0.0037
	[0.29]	[1.08]	[0.01]	[0.62]	[0.55]	[0.98]	[1.14]	[1.64]	[-0.94]	[-0.55]
$TaxIncentivesIndex_{j,t}$	0.0022	0.0018	0.0006	0.0002	0.0002	0.0001	-0.0954	-0.1033	-0.0007	-0.0009
	[0.76]	[0.62]	[0.17]	[0.07]	[0.12]	[0.05]	[-1.20]	[-1.36]	[-0.37]	[-0.48]
$SalesTaxRate_{j,t}$	0.0214*	0.0238**	0.0382*	0.0409*	0.0040	0.0048	0.7415**	0.8099**	0.0166*	0.0174**
	[1.86]	[2.23]	[1.70]	[1.86]	[1.22]	[1.48]	[2.13]	[2.38]	[1.99]	[2.17]
Adj. R-squared	0.998	0.998	0.997	0.997	0.982	0.983	0.977	0.978	0.999	0.999

This panel presents results of testing the relation between subsidies and local economic activity, measured as the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the propensity-score matched sample of megadeals representing 1,176 treatment and control counties. 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.

Online Appendix

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Table 1
Robustness of Table 4 to Alternative Fixed Effects Structures, Matched Samples, and Clustering

Panel A: Propensity Score Matching with State-by-year Fixed Effects

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0730*** **		**0.1587** **		**0.0127** **		**0.8317** **		**0.0042**	
	[2.60]		**[2.41]**		**[2.23]**		**[2.09]**		**(0.57)**	
$PostTaxSubsidy_{i,(0-2)}$		**0.0433** **		**0.0935** **		**0.0099***		**0.4157**		**-0.0011**
		[2.13]		**[1.99]**		**[1.74]**		**[1.07]**		**[-0.15]**
$PostTaxSubsidy_{i,(3-4)}$		**0.1208*** **		**0.2693** **		**0.0171** **		**1.5149*** **		**0.0115**
		[2.72]		**[2.52]**		**[2.40]**		**[3.11]**		**[1.47]**
$PostTaxSubsidy_{i,(5-6)}$		**0.2101** **		**0.4717** **		**0.0203** **		**1.8875*** **		**0.0188***
		[2.51]		**[2.31]**		**[2.40]**		**[3.28]**		**[1.91]**
$PostTaxSubsidy_{i,(7+)}$		**0.0103**		**-0.0701**		**0.0261** **		**2.6605*** **		**0.0365*** **
		[0.17]		**[-0.47]**		**[2.40]**		**[3.83]**		**[3.06]**
Adj. R-squared	0.952	0.952	0.837	0.838	0.97	0.97	0.953	0.953	0.999	0.999

This panel presents results of using a propensity-score matched sample to test the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the unweighted (weighted) sample of 12,960 county-year observations. We define all variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-year fixed effects, and standard errors are clustered by county. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix
Table 1
Robustness to Alternative Fixed Effects Structures, Matched Samples, and Clustering

Panel B: Entropy-balancing

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0225****		**0.0354***		**0.0027**		**0.3131**		**-0.0039**	
	[2.28]		**[1.77]**		**[1.02]**		**[1.63]**		**[-1.10]**	
$PostTaxSubsidy_{i,(0-2)}$		**0.0174***		**0.0265**		**0.0010**		**0.1178**		**-0.0076**
		[1.91]		**[1.30]**		**[0.49]**		**[0.97]**		**[-1.62]**
$PostTaxSubsidy_{i,(3-4)}$		**0.0335*****		**0.0539*****		**0.0069**		**0.7783*****		**0.0054**
		[4.06]		**[3.46]**		**[1.50]**		**[2.62]**		**[0.98]**
$PostTaxSubsidy_{i,(5-6)}$		**0.0424*****		**0.0728*****		**0.0070**		**0.9854*****		**0.0070**
		[3.32]		**[2.98]**		**[1.37]**		**[3.04]**		**[0.95]**
$PostTaxSubsidy_{i,(7+)}$		**0.0361*****		**0.0514*****		**0.0069**		**1.1218*****		**0.0100**
		[3.03]		**[2.60]**		**[1.52]**		**[3.46]**		**[1.08]**
$MinWage_{i,t}$	0.0062	0.0058	0.0190	0.0182	0.0041**	0.0040**	0.4718***	0.4604***	-0.0073**	-0.0075**
	[0.62]	[0.59]	[0.90]	[0.87]	[2.05]	[2.02]	[7.65]	[7.81]	[-2.31]	[-2.44]
$Ln(GDP)_{j,t}$	0.5864***	0.5742***	0.9206***	0.8986***	0.1878***	0.1847***	18.1386***	17.6915***	0.3978***	0.3901***
	[7.71]	[7.31]	[6.31]	[5.91]	[4.00]	[4.02]	[5.18]	[5.26]	[14.48]	[13.80]
$\%Educ_{j,t}$	-0.0013	-0.0013	-0.0038	-0.0037	0.0006	0.0006	-0.0001	-0.0031	-0.0016**	-0.0016**
	[-0.35]	[-0.35]	[-0.62]	[-0.60]	[0.66]	[0.67]	[-0.00]	[-0.09]	[-2.04]	[-2.03]
$\%Union_{j,t}$	-0.0078	-0.0075	-0.0141	-0.0136	-0.0030***	-0.0029***	-0.2018***	-0.1921***	0.0049***	0.0051***
	[-1.59]	[-1.49]	[-1.41]	[-1.33]	[-4.91]	[-4.69]	[-5.91]	[-6.16]	[3.88]	[3.81]
$CorpTaxRate_{j,t}$	-0.0073*	-0.0069*	-0.0114	-0.0107	-0.0002	-0.0000	0.0380	0.0556	-0.0041***	-0.0037**
	[-1.83]	[-1.80]	[-1.09]	[-1.07]	[-0.30]	[-0.07]	[1.00]	[1.39]	[-2.85]	[-2.41]
$Ln(UIContrib)_{j,t}$	-0.0223**	-0.0226**	-0.0585***	-0.0587***	-0.0133**	-0.0132**	-1.2334***	-1.2514***	-0.0264***	-0.0265***
	[-2.18]	[-2.15]	[-3.20]	[-3.20]	[-2.21]	[-2.24]	[-4.00]	[-4.02]	[-3.50]	[-3.56]
$PropertyTax_{j,t}$	0.1381	0.1541	0.5322**	0.5578**	-0.2139***	-0.2095***	-2.0510	-1.3434	0.1955*	0.2077**
	[0.88]	[1.03]	[2.00]	[2.18]	[-6.05]	[-5.94]	[-1.11]	[-0.75]	[1.88]	[2.16]
$PersonalTaxRate_{j,t}$	0.0147*	0.0141*	0.0318**	0.0308**	0.0016	0.0014	0.2163**	0.1957**	0.0082***	0.0078***
	[1.83]	[1.70]	[2.42]	[2.27]	[0.85]	[0.79]	[2.50]	[2.07]	[5.59]	[5.46]
$TaxIncentivesIndex_{j,t}$	0.0115*	0.0115*	0.0233	0.0234	-0.0006	-0.0005	-0.0312	-0.0256	0.0030***	0.0031***
	[1.85]	[1.87]	[1.58]	[1.59]	[-0.90]	[-0.85]	[-1.20]	[-0.96]	[3.12]	[3.12]
$SalesTaxRate_{j,t}$	-0.0019	-0.0007	-0.0076	-0.0057	-0.0010	-0.0007	0.3277	0.3793*	0.0065	0.0074
	[-0.72]	[-0.27]	[-1.34]	[-0.98]	[-0.71]	[-0.49]	[1.59]	[1.95]	[1.21]	[1.49]
Adj. R-squared	0.978	0.978	0.899	0.899	0.960	0.960	0.968	0.968	0.999	0.999

This panel presents results of using an entropy-balanced sample to test the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the unweighted (weighted) sample of 19,224 (14,136) 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. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix
Table 1
Robustness to Alternative Fixed Effects Structures, Matched Samples, and Clustering

Panel C: Robustness of Results to Alternative Clustering

Dep Var:	*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*		*(Employees / Population)$_{i,t+1}$*		*(Wages / Population)$_{i,t+1}$*		*Ln(Establishments)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PostTaxSubsidy$_{i,t}$	0.0594*		0.1212		0.0137*		0.8017**		0.0050	
	[1.84]		[1.57]		[1.85]		[2.54]		(1.33)	
PostTaxSubsidy$_{i,(0-2)}$		0.0563		0.1181		0.0114		0.5227*		0.0002
		[1.63]		[1.44]		[1.39]		[1.77]		[0.04]
PostTaxSubsidy$_{i,(3-4)}$		0.0643**		0.1224		0.0189**		1.4459***		0.0166***
		[2.13]		[1.62]		[2.51]		[3.14]		[3.22]
PostTaxSubsidy$_{i,(5-6)}$		0.0783*		0.1472		0.0229***		1.8248***		0.0207**
		[1.70]		[1.45]		[2.90]		[3.66]		[2.68]
PostTaxSubsidy$_{i,(7+)}$		0.0568		0.0716		0.0290**		2.3186***		0.0301**
		[0.96]		[0.53]		[2.51]		[3.03]		[2.47]
R-squared	0.919	0.919	0.724	0.724	0.968	0.968	0.949	0.949	0.998	0.998

This panel presents the robustness of results presented in Table 4 to alternative standard error clustering approaches using a propensity-score matched sample to test the relation between subsidies and the log of the number of employees (Columns (1)-(2)), the log of aggregate wages (Columns (3)-(4)), employees scaled by population (Columns (5)-(6)), aggregate wages scaled by population (Columns (7)-(8)), and the log of the number of local establishments (Columns (9)-(10)) for the unweighted (weighted) sample of 12,960 county-year observations. We define all variables in Appendix A and present t-statistics in brackets. Each specification includes county and year fixed effects. In the first five rows, we present standard errors that are clustered by county; in the bottom five rows, we present standard errors clustered by state.. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Online Appendix
Table 2
Full Results of Table 5 Disclosure and Information Dissemination Tests

Panel A: Partitions of Having vs. Not Having a Disclosure Initiative

Dep Var:	*Ln(Employees)*$_{i,t+1}$		*Ln(Wages)*$_{i,t+1}$		*(Employees / Population)*$_{i,t+1}$		*(Wages / Population)*$_{i,t+1}$		*Ln(Establishments)*$_{i,t+1}$	
	Disclosure Initiative	No Disclosure Initiative	Disclosure Initiative	No Disclosure Initiative	Disclosure Initiative	No Disclosure Initiative	Disclosure Initiative	No Disclosure Initiative	Disclosure Initiative	No Disclosure Initiative
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
***PostTaxSubsidy*$_{i,t}$**	**0.0175**	**0.1853****	**0.0253**	**0.4184***	**0.0168****	**0.0103**	**0.5714**	**1.5928*****	**0.0042**	**0.0025**
	[1.37]	**[2.02]**	**[0.98]**	**[1.90]**	**[2.40]**	**[1.26]**	**[1.16]**	**[2.79]**	**[0.52]**	**[0.14]**
MinWage$_{i,t}$	0.0059	-0.1227	0.0169	-0.2873	-0.0032	-0.0054	0.3348	-0.2140	0.0077***	0.0038
	[0.59]	[-1.08]	[1.29]	[-0.99]	[-0.40]	[-1.32]	[1.07]	[-1.17]	[3.21]	[0.52]
Ln(GDP)$_{j,t}$	0.5943***	0.5242	0.8620***	1.1118	0.2355***	-0.0275	20.3241***	5.2862	0.3548***	0.1100
	[5.65]	[0.80]	[3.35]	[0.68]	[4.90]	[-0.39]	[4.71]	[1.25]	[7.07]	[0.88]
%Educ$_{j,t}$	-0.0051	0.0264	-0.0096	0.0784	-0.0017	0.0044	-0.0994	-0.0198	-0.0019	-0.0091**
	[-1.19]	[0.69]	[-1.35]	[0.81]	[-1.12]	[1.17]	[-0.97]	[-0.15]	[-1.04]	[-1.99]
%Union$_{j,t}$	-0.0050	-0.0917	-0.0045	-0.2300	-0.0052**	-0.0057*	-0.0769	-0.4837**	0.0047*	0.0161***
	[-1.08]	[-1.33]	[-0.62]	[-1.31]	[-2.38]	[-1.70]	[-0.46]	[-2.52]	[1.94]	[4.52]
CorpTaxRate$_{j,t}$	-0.0069	-0.0702	-0.0096	-0.1795	0.0000	-0.0003	0.0209	0.0975	0.0033**	-0.0182***
	[-1.36]	[-1.11]	[-0.81]	[-1.12]	[0.01]	[-0.12]	[0.25]	[0.63]	[2.44]	[-4.89]
Ln(UIContrib)$_{j,t}$	-0.0324	-0.1027	-0.0477	-0.2839	-0.0203	-0.0081	-1.1369	0.3937	-0.0445***	-0.0288
	[-1.14]	[-0.69]	[-1.23]	[-0.84]	[-1.55]	[-0.55]	[-1.46]	[0.42]	[-3.68]	[-1.02]
PropertyTax$_{j,t}$	0.3756	8.2884	0.2627	20.5438	-0.1785	0.1057	-3.2689	6.2142	-0.0421	0.4088
	[1.44]	[1.23]	[0.89]	[1.21]	[-1.34]	[0.44]	[-0.56]	[0.72]	[-0.25]	[1.64]
PersonalTaxRate$_{j,t}$	-0.0015	0.2709	-0.0011	0.6917	0.0024	-0.0094	0.2193	-0.5685	0.0022	0.0263**
	[-0.30]	[1.08]	[-0.13]	[1.11]	[1.12]	[-0.87]	[1.51]	[-0.89]	[0.69]	[2.08]
TaxIncentivesIndex$_{j,t}$	0.0048*	0.1412	0.0052	0.3389	-0.0015	0.0054**	-0.1057	0.2606***	0.0032***	0.0050*
	[1.78]	[1.44]	[1.18]	[1.39]	[-1.29]	[2.16]	[-1.31]	[3.03]	[2.66]	[1.76]
SalesTaxRate$_{j,t}$	0.0082	0.0448	0.0102	0.1543	-0.0067	0.0111	0.3014	1.0610**	0.0027	-0.0260
	[1.15]	[0.47]	[0.99]	[0.60]	[-1.31]	[1.38]	[0.88]	[2.03]	[0.51]	[-1.30]
Observations	9,072	3,888	9,072	3,888	9,072	3,888	9,072	3,888	9,072	3,888
Adj. R-squared	0.989	0.823	0.945	0.596	0.967	0.960	0.947	0.946	0.998	0.998

Online Appendix
Table 2
Full Results of Table 5 Disclosure and Information Dissemination Tests

Panel B: Partitions of Above vs. Below Median Google Trends

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	Above Median Searches	Below Median Searches	Above Median Searches	Below Median Searches	Above Median Searches	Below Median Searches	Above Median Searches	Below Median Searches	Above Median Searches	Below Median Searches
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$PostTaxSubsidy_{i,t}$	**0.0207**	**0.0984****	**0.0400**	**0.2121***	**0.0169***	**0.0080**	**0.3452**	**1.0295****	**0.0010**	**0.0115**
	[0.99]	**[2.08]**	**[0.94]**	**[1.88]**	**[1.78]**	**[1.42]**	**[0.53]**	**[2.23]**	**[0.09]**	**[1.14]**
$MinWage_{i,t}$	-0.0115	-0.0218	-0.0017	-0.0580	-0.0119	0.0100**	-0.2921	0.8897**	-0.0018	-0.0016
	[-0.59]	[-0.41]	[-0.06]	[-0.43]	[-1.36]	[2.28]	[-0.74]	[2.33]	[-0.44]	[-0.31]
$Ln(GDP)_{j,t}$	0.7264***	0.9416***	1.3649**	1.7868**	0.1239**	0.2160***	19.0417***	19.8104***	0.2902***	0.4485***
	[2.85]	[2.74]	[2.40]	[2.08]	[2.19]	[3.86]	[3.69]	[4.71]	[5.05]	[7.29]
$\%Educ_{j,t}$	-0.0092*	0.0210	-0.0174	0.0590	0.0004	0.0000	0.0036	-0.1236	-0.0057**	-0.0038
	[-1.70]	[0.73]	[-1.51]	[0.80]	[0.18]	[0.02]	[0.04]	[-1.20]	[-2.30]	[-1.50]
$\%Union_{j,t}$	-0.0128	-0.0425	-0.0244	-0.1007	-0.0060**	-0.0040*	-0.3124	-0.2519**	0.0142***	-0.0008
	[-1.36]	[-1.46]	[-1.35]	[-1.44]	[-2.53]	[-1.76]	[-1.48]	[-2.14]	[5.21]	[-0.23]
$CorpTaxRate_{j,t}$	-0.0037	-0.0242	-0.0073	-0.0511	0.0021*	-0.0024	0.2006**	-0.1483	-0.0007	-0.0035*
	[-0.56]	[-1.49]	[-0.48]	[-1.32]	[1.80]	[-1.24]	[1.97]	[-1.36]	[-0.37]	[-1.69]
$Ln(UIContrib)_{j,t}$	-0.0190	-0.0753	-0.0648	-0.1264	-0.0291*	-0.0191	-0.7669	-1.2898*	-0.0180	-0.0365*
	[-0.50]	[-1.61]	[-0.95]	[-1.28]	[-1.95]	[-1.55]	[-0.99]	[-1.69]	[-1.02]	[-1.69]
$PropertyTax_{j,t}$	0.3405	0.2207	0.2861	-0.0357	-0.2544*	-0.0305	-4.1001	2.2541	-0.1210	0.2021
	[1.29]	[0.46]	[0.92]	[-0.03]	[-1.71]	[-0.17]	[-0.65]	[0.36]	[-0.62]	[1.12]
$PersonalTaxRate_{j,t}$	0.0362	0.0402	0.0909	0.1202	0.0023	0.0070*	0.2592	0.2666*	0.0030	0.0013
	[1.14]	[0.72]	[1.18]	[0.85]	[0.80]	[1.89]	[1.39]	[1.74]	[0.73]	[0.25]
$TaxIncentivesIndex_{j,t}$	0.0283	0.0777	0.0608	0.1839	0.0004	-0.0002	0.0489	-0.0586	0.0018	0.0077***
	[1.45]	[1.24]	[1.28]	[1.20]	[0.30]	[-0.13]	[0.52]	[-0.60]	[1.10]	[3.62]
$SalesTaxRate_{j,t}$	-0.0029	-0.0735	-0.0210	-0.1690	-0.0023	-0.0092*	0.6389	-0.0102	-0.0033	-0.0014
	[-0.15]	[-1.30]	[-0.46]	[-1.32]	[-0.37]	[-1.66]	[1.50]	[-0.03]	[-0.47]	[-0.13]
Observations	6,336	6,624	6,336	6,624	6,336	6,624	6,336	6,624	6,336	6,624
Adj. R-squared	0.889	0.967	0.665	0.866	0.948	0.978	0.920	0.967	0.998	0.998

Online Appendix
Table 2
Full Results of Table 5 Disclosure and Information Dissemination Tests

Panel C: Partitions of Having vs. Not Having a Newspaper

Dep Var:	$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$		$(Employees / Population)_{i,t+1}$		$(Wages / Population)_{i,t+1}$		$Ln(Establishments)_{i,t+1}$	
	Has Newspaper	No Newspaper	Has Newspaper	No Newspaper	Has Newspaper	No Newspaper	Has Newspaper	No Newspaper	Has Newspaper	No Newspaper
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\boldsymbol{PostTaxSubsidy_{i,t}}$	0.0453	0.0676**	0.0869	0.1405**	0.0035	0.0206***	0.1805	1.2234***	0.0033	0.0053
	[1.53]	[2.38]	[1.30]	[2.20]	[0.73]	[2.68]	[0.33]	[2.97]	[0.30]	[0.67]
$MinWage_{i,t}$	0.0061	-0.0004	0.0102	0.0131	0.0034	-0.0100	0.4971***	0.0233	0.0011	-0.0018
	[0.70]	[-0.02]	[0.62]	[0.38]	[1.06]	[-0.87]	[3.07]	[0.06]	[0.34]	[-0.51]
$Ln(GDP)_{j,t}$	0.8358***	0.9249***	1.6803**	1.6092**	0.0911	0.2550***	14.6916***	23.5998***	0.4207***	0.3452***
	[2.81]	[3.03]	[2.37]	[2.11]	[1.58]	[4.43]	[3.26]	[5.10]	[6.84]	[6.11]
$\%Educ_{j,t}$	-0.0014	0.0052	0.0062	0.0202	-0.0009	0.0004	-0.0259	-0.1517	-0.0049	-0.0052**
	[-0.20]	[0.35]	[0.37]	[0.51]	[-0.56]	[0.19]	[-0.28]	[-1.37]	[-1.60]	[-2.57]
$\%Union_{j,t}$	-0.0141	-0.0203**	-0.0356	-0.0423*	-0.0026	-0.0074***	0.0823	-0.4159***	0.0068*	0.0096***
	[-1.14]	[-2.11]	[-1.26]	[-1.88]	[-1.20]	[-3.18]	[0.35]	[-2.87]	[1.66]	[4.09]
$CorpTaxRate_{j,t}$	0.0003	-0.0111	0.0056	-0.0223	0.0012	0.0004	0.0053	0.1097	0.0021	-0.0030*
	[0.08]	[-1.32]	[0.71]	[-1.10]	[1.05]	[0.29]	[0.05]	[1.35]	[1.12]	[-1.92]
$Ln(UIContrib)_{j,t}$	-0.0785	-0.0179	-0.1517	-0.0365	-0.0221*	-0.0203*	-1.2764*	-0.9052	-0.0324*	-0.0340**
	[-1.58]	[-0.49]	[-1.49]	[-0.50]	[-1.71]	[-1.68]	[-1.69]	[-1.30]	[-1.96]	[-2.04]
$PropertyTax_{j,t}$	-0.0120	0.7024**	-0.4015	0.8758	-0.3046*	-0.0278	-3.4537	0.9070	0.1087	0.0003
	[-0.03]	[2.00]	[-0.54]	[1.36]	[-1.96]	[-0.26]	[-0.54]	[0.19]	[0.65]	[0.00]
$PersonalTaxRate_{j,t}$	0.0485	0.0976	0.1174	0.2501	0.0059***	0.0019	0.5715***	0.0856	0.0066	0.0073
	[1.14]	[1.00]	[1.12]	[1.03]	[2.88]	[0.49]	[2.70]	[0.54]	[1.54]	[1.46]
$TaxIncentivesIndex_{j,t}$	0.0412	0.0828	0.0924	0.1954	-0.0001	0.0008	-0.0357	0.0225	0.0060***	0.0037
	[1.21]	[1.24]	[1.12]	[1.20]	[-0.11]	[0.44]	[-0.41]	[0.24]	[3.48]	[1.26]
$SalesTaxRate_{j,t}$	-0.0186	-0.0429	-0.0545	-0.1071	-0.0020	-0.0073	0.7089	0.2428	0.0080	-0.0025
	[-0.55]	[-0.97]	[-0.67]	[-0.98]	[-0.78]	[-1.16]	[1.51]	[1.24]	[0.95]	[-0.57]
Observations	5,208	7,752	5,208	7,752	5,208	7,752	5,208	7,752	5,208	7,752
Adj. R-squared	0.932	0.865	0.766	0.623	0.979	0.960	0.971	0.927	0.999	0.997

This table presents results of testing the relation between subsidies and local economic activity for the propensity-score matched sample of 12,960 county-year observations after partitioning based on the home state's disclosure of subsidies (Panel A), the amount of attention given to subsidies in a state (Panel B), and the existence of a local newspaper in the county (Panel C). In Panel A, we partition states based on whether they had an initiative in place at the time of subsidy grant, requiring disclosure at the state level about subsidies; we identify states with a disclosure initiative based on Good Jobs First studies produced in 2007, 2010, and 2014. In Panel B, we partition states based on their within-year Google search interest for four primary subsidy-related search terms ("subsidy", "subsidies", "tax break", and "tax breaks"), relative to all other Google searches conducted in those states. We partition above and below the median relative search volume based on the initial subsidy year (that is, whether subsidy-related searches, as a fraction of all Google searches, were higher or lower than the median across all states for that year). In Panel C, we partition counties based on the existence of a local newspaper using newspaper closure data from Gao et al. (2019) and 2019 data on local newspapers from Editors & Publishers. We define all other 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.