Tax Subsidy Information and Local Economic Effects

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We examine whether the relation between business tax subsidies and local employment varies based on the subsidy information environment. State and local business incentives have tripled in size over the past thirty years (Bartik, 2019), now amounting to over 40 percent of total state corporate tax revenues (Slattery and Zidar, 2020). However, the subsidy grant process is notoriously opaque, and transparency problems inhibit clear assessments about whether subsidies achieve their intended outcomes. We evaluate whether there is a differential relation between subsidies and local employment in the states where governments provide more information about granted subsidies, measured based on the existence of state-level subsidy disclosure initiatives, the online publication of subsidy and recipient details, and job commitment disclosures. We find positive employment effects only when the subsidies are accompanied by job commitments, signaling heterogeneity in the extent to which subsidy disclosures facilitate employment and wage growth. Our large-scale empirical analysis on the role of the subsidy information environment provides evidence of an important factor previously discussed, but not empirically tested, in public economics. Furthermore, we offer policy-relevant evidence for the increasing number of states implementing subsidy disclosure regimes.

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1. Introduction

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In recent years, governments have increasingly used firm-specific tax subsidies to stimulate local business activity. These incentives have grown in number and scale, tripling in size since the 1990s (Bartik, 2019); the aggregate dollar value of tax subsidies is nearly $11 billion in 2014 alone. The premise for granting subsidies is that targeted tax benefits induce corporate investment and employment, thereby stimulating local economic growth through both direct and spillover channels. However, prior work suggests that a lack of transparency – about both the subsidy granting process and the subsequent monitoring of subsidy recipients – is a central problem inhibiting subsidies from achieving intended outcomes (Greenstone and Moretti, 2004; Slattery, 2020; Slattery and Zidar, 2020; Slattery, 2021). We test whether the relation between business tax subsidies and local economic activity varies with the subsidy information environment.

Prominent public economists discuss the importance of firm-specific subsidies as a place-based public policy tool (Glaeser, 2001; Austin, Glaeser, and Summers, 2018).[1] These state and local business tax incentives are now the primary U.S. place-based policy, amounting to an average of 40 percent of total state corporate tax revenues (Slattery and Zidar, 2020). Prior empirical work documents notable employment effects of large subsidy awards (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Slattery and Zidar, 2020). However, the literature suggests two important forces that impede an even larger business response. First, the subsidy granting process is inherently political in nature (Slattery, 2021; Aobdia et al., 2020). Second, the subsidy granting process is often opaque, with little information available about granting states, recipient firms, amounts awarded, intended outcomes, or resulting effects. This lack of transparency is exacerbated by the fact that subsidy giving is highly decentralized, often occurring across multiple state-level agencies and in thousands of municipalities and counties. Concerns

[1] A place-based economic policy is one intended to encourage economic activity in a particular micro-area, such as a neighborhood, census tract, city, or region (Moretti and Wilson, 2014).

about transparency have become so great that governments have recently implemented subsidy disclosure regulations in an effort to improve subsidy effectiveness.

It is this second concern about transparency that we focus on in this paper. We study whether government disclosures of subsidy information alter the relation between tax subsidies and future employment in the recipient's county. The prior accounting literature on disclosure motivates two non-mutually exclusive avenues for better subsidy information environments to improve local economic outcomes following subsidy receipt (Badertscher et al., 2013; Shroff et al., 2014). First, better information during the subsidy granting process should result in a more efficient allocation of subsidies. Second, better information about subsidies can facilitate monitoring by relevant stakeholders after subsidies are granted, including both the granting agency and local taxpayers funding these subsidies. Consistent with these two roles of disclosure, several non-governmental groups have advocated for greater subsidy disclosure on the basis that "sunshine is the best antiseptic" and that public information is "essential to the process [of economic development accountability]" (Good Jobs First, 2007, p. 8 and 10). We therefore predict that any positive effects of subsidies on local employment outcomes are stronger in the presence of greater subsidy disclosures.

However, there are a number of reasons we may not observe these effects. First, increased monitoring could induce granting agencies and subsidy recipients to make myopic decisions that result in short-term, salient effects at the expense of more optimal long-term employment. Second, subsidy disclosures could reveal proprietary information about the recipient's market and growth strategies, reducing the recipient's competitive position and economic presence (Graham, Harvey, and Rajgopal, 2005; Heinle, Samuels, and Taylor, 2018). Third, more subsidy disclosures could signal those jurisdictions willing to provide subsidies, potentially driving increased demand for

subsidies that produce declining marginal benefits. Finally, subsidy disclosures could result in recipients redirecting funds towards other outcomes, such as remediating local environmental or gentrification concerns (Story, 2012; Metlife, 2017). While such outcomes could have positive effects on the local community, they would not drive the employment effects we study. Collectively, all of these reasons suggest a role for subsidy disclosures in the relation between subsidies and local employment.

To test our research question, we obtain data on firm-specific subsidies from Good Jobs First (GJF). Good Jobs First is a nonprofit organization that compiles Subsidy Tracker, the most comprehensive dataset available on state and local subsidies since 2004, covering over 600 development programs across all 50 states.[2] We combine this large sample of tax subsidies with county-year data from the U.S. Bureau of Labor Statistics and the U.S. Census Bureau to measure local economic activity and to construct control variables. Focusing on states with the most complete subsidy coverage obtained via local news articles, government websites, and freedom of information (FOIA) requests, our tests examine how subsidy disclosures affect subsidy effectiveness.

We start by evaluating under which conditions our sample of subsides exhibits the same positive relation between subsidies and county-level employment as that observed in prior work (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Slattery and Zidar, 2020). In so doing, we make three extensions relative to the prior findings. First, instead of studying the largest subsidy awards that were the focus of these earlier studies, we examine thousands of smaller awards. These awards include tax credits, tax abatements, tax

[2] Good Jobs First is a Washington, D.C. based nonprofit that describes its mission as "...seeking to make economic development subsidies more accountable and effective" (from www.goodjobsfirst.org). We exclude federal subsidies from our tests to focus on the role of state and local incentives. These data are also used by Raghunandan (2018, 2021), Slattery (2020, 2021) and Slattery and Zidar (2020).

reimbursements, grants, tax increment financing, and enterprise zone benefits. Although these subsidies are on average much smaller than those previously studied, they are also much more prevalent, with almost 85 times the number of awards. The average county has approximately 4.9-24.1 subsidies over the sample period, worth $1.6-$7.8 million. We focus on these subsidies primarily because we expect there to be the greatest variation in public information for these smaller awards, as compared to the multi-million dollar megadeals that often attract substantial press and public attention. Second, we can more directly assess the type of subsidies that are most effective in driving certain outcomes. In contrast to megadeals, which are designed to stimulate both investment and employment upon firm entry into a jurisdiction, our more expansive sample of subsidies permits refined tests that better match specific subsidy criteria (such as increased jobs or worker training) to employment outcomes. Finally, we extend the analysis to evaluate the common strategy of granting multiple subsidies over time.

Counties with subsidy recipients have 49,335 distinct subsidies during the sample period. While our sample allows us to examine subsidies with the most variation in available information, it also presents a research design challenge. Specifically, identification of an appropriate control sample is difficult because the ideal match – jurisdictions that offered or would offer a similar subsidy to the same recipient – is unobservable for the broad range of smaller subsidies we study. Thus, we follow recent research and benchmark the business activities of county-years with subsidy recipients against counties that later receive subsidies (Fuest et al., 2018). This approach mitigates selection bias concerns by requiring control counties to also have subsidy recipients at some point. Because we require counties to be observable for the entire sample period, the sample is a balanced panel of 9,320 county-years (1,165 counties) for the eight-year period from 2008 to 2015. This sample of county-years is distributed across 27 distinct U.S. states, with the most

observations in Indiana, Kentucky, Ohio, and Virginia.

Results of our first tests show some evidence of a positive relation between subsidies and employment outcomes. After exploiting detailed hand-collected data to identify those subsidies specifically targeting increased employment, we observe a 1.0-1.9 percent increase in number of employees. After considering the 42,000 subsequent subsidies received by firms in sample counties, we observe a larger 1.3-3.4 percent increase in number of employees and a 1.2-3.0 percent increase in aggregate wages. These figures imply that the median county has an increase of approximately 276-646 workers following receipt of 3.4-19.4 employment subsidies over a period of 4.4 years. These tests highlight the critical role of subsequent subsidies: to achieve employment outcomes, multiple subsidies must be granted over several subsequent years.

Having demonstrated positive employment effects in our sample, we turn to examining our primary research question on the role of subsidy disclosures. We use three measures of subsidy disclosure: (i) whether the state had a subsidy disclosure regime in place at the time of the first observed subsidy, (ii) whether information about the subsidy was disclosed online, and (iii) whether – in addition to online disclosure – the subsidy recipient was required to provide job commitments prior to subsidy receipt. We observe positive employment effects when the subsidy recipient provided specific job commitments, suggesting that these job disclosures play an important role in fulfilling subsidy employment goals. In contrast, we find a negative incremental effect of disclosure regimes and online disclosure, which in total implies no on average relation between subsidies and employment outcomes in those jurisdictions. These results suggest the existence of state disclosure regimes alone may be insufficient to obtain intended local employment outcomes. Our estimates also permit a break-even assessment of the value of subsidy disclosure: granting agencies could achieve the same employment outcomes after the first subsidy

is awarded by either granting 1.3-4.4 additional subsidies over time or by disclosing job commitments of a first-time subsidy recipient. Given the average subsidy value of $324,000, subsidy-specific job commitment disclosures are associated with saving between $464,000 to $1,381,000 for each county with a subsidy recipient. As our sample contains over 1,100 counties with subsidy recipients, across 27 states, this implies a savings of $510.9 million - $1.52 billion.

Our study contributes to the business subsidy literature by examining the role of disclosure in mitigating concerns about the opaque subsidy granting and monitoring processes. Prior and concurrent work on firm-specific subsidies discusses that the lack of transparency can have a substantial effect on business outcomes (Slattery, 2020). For example, Slattery and Zidar (2020) state that "local discretion reduces transparency" and that "the lack of transparency in subsidy-giving leaves [the subsidy-granting process] more exposed to political capture and noneconomic allocation of funds" (p. 97). The setting also suffers from substantial information problems about whether subsidies achieve their intended outcomes (Greenstone and Moretti, 2004; Good Jobs First, 2010; Pew Charitable Trusts, 2013). We provide empirical evidence on whether and what kind of subsidy disclosures are most helpful in addressing these problems. Furthermore, we demonstrate that the subsidy information environment should be considered in future empirical studies that examine the broad set of subsidies in our sample. In addition to contributing to the literature on business tax subsidies, we also contribute to the broader literature on the role of state business climate characteristics (such as tax rates, state apportionment factors, characteristics of the tax base, etc.) in firms' production decisions (Papke, 1991; Hines, 1997; Goolsbee and Maydew, 2000; Holcombe and Lacombe, 2004; 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).

By providing evidence on the role of the local information environment, we extend the accounting 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). Specifically, our work takes central findings from the accounting literature and applies them in a public economics context. In so doing, we add to the nascent accounting literature studying these subsidies, including the relation between the incidence and relative magnitude of subsidies and recipient firm margins, income, and growth in the following period (Drake, Hess, Wilde, and Williams, 2021) and the role of government capture in the likelihood of subsidized firms engaging in and getting caught engaging in financial and non-financial misconduct (Raghunandan, 2018, 2021).

Finally, the use of business tax subsidies is becoming increasingly controversial. Slattery (2020) shows that subsidies can be welfare-increasing by improving the matching process between firms and locations, but welfare gains accrue primarily to firms rather than the government or the public. Critics suggest that subsidies do not benefit local governments but instead foster a race to the bottom. Such concerns have resulted in a number of governmental actions, including recent legislation in New York and a "truce" agreement between Kansas and Missouri over the use of subsidies to attract business.[3] However, the most popular approach has been for states to implement disclosure initiatives as a first step toward driving intended economic outcomes. While many states adopted disclosure regimes in the early years of our sample period (Good Jobs First,

[3] The New York "End Corporate Welfare Act" proposes an interstate agreement that imposes a cease-fire on states awarding targeted business incentives (Farmer, 2019). The Kansas-Missouri truce was announced in August 2019, with the Missouri governor remarking, "[s]ometimes commonsense does prevail…because you don't have to be a scientist to figure out this was a bad deal for both states" (Hardy, 2019). These state-level actions mirror actions taken at the international level to address cross-border tax competition, such as steps by the European Commission targeting illegal state aid via member state tax policies and G7 efforts towards a global minimum tax.

2007; 2010), over 21 states have approved new or augmented laws since 2012 (Pew Charitable Trusts, 2017). Furthermore, the Governmental Accounting Standards Board mandated that governments disclose subsidies in a new accounting standard effective at the end of 2015. This paper speaks to the effectiveness of these federal and state disclosure regimes and provides policy-relevant evidence about these reporting requirements.

2. Prior Literature and Hypotheses

There are two observed phenomena important for motivating our study of business tax subsidies. First, corporate tax rate reductions are often intended to stimulate investment by lowering the cost of capital. We discuss this literature in Section 2.1. Second, many state governments have attempted to address concerns about subsidy effectiveness by implementing subsidy-specific disclosure regimes, perhaps in recognition of the link between the information environment and investment efficiency. We discuss the accounting literature on disclosure in Section 2.2, which provides the basis for our research question on the role of the subsidy information environment in subsidy effectiveness.

2.1 Real Effects of Subsidies

Firms should invest until the marginal product of capital equals the user cost of capital. A reduction in a firms' cost of capital attributable to a decline in the corporate tax rate should lead to increases in corporate investment (Hall and Jorgenson, 1967). The prior empirical literature documents this negative elasticity of investment to tax rates within U.S. states (e.g., Hines, 1997; Ljunqvist and Smolyansky, 2015; Suarez Serrato and Zidar, 2016; Giroud and Rauh, 2019) and internationally (e.g., Hines and Rice, 1994; Grubert and Mutti, 2000; Devereux, Griffith, and Simpson, 2007; Fuest et al., 2018). The literature finds evidence supporting both complementary

and substitutive effects of investment and employment (Giroud and Rauh, 2019; Lester 2019; Dobridge et al. 2019).

In the context of targeted business subsidies that reduce a firm's corporate tax burden, prior literature provides some limited evidence of investment and employment effects (e.g., Bartik, 1985; Papke, 1991; Gale et al., 2015). The structural model in Slattery (2020) quantifies the welfare effects of subsidy competition, finding that 50 percent of firms would locate in another state if there were no incentive spending and that subsidy competition increases total welfare by 10 percent. The implication is that subsidies can improve the matching process between firms and locations, but a key finding is that welfare gains accrue primarily to the firms via additional awards instead of to the government or the public.

Empirical work focuses on measuring the economic effects of very large subsidies (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Moretti and Wilson, 2014; Slattery, 2020; Slattery and Zidar, 2020). For example, 82 counties who won a "Million Dollar Plant" report a 1.5 percent increase in the average wage bill and a 12 percent increase in incumbent plants' total factor productivity following subsidy grant (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010). Using a sample of 543 businesses receiving discretionary expansion and relocation subsidies in excess of $5 million, Slattery and Zidar (2020) evaluate county-level employment outcomes. They use a difference-in-differences analysis, in which the control group is a hand-collected sample of "runner up" counties that were also competing to attract the specific firm to their locality. They find increases of approximately 1,500 jobs within the three-digit industry of the recipient firm. However, the statistical significance of this effect declines as the industry definition is broadened, implying countervailing spillover effects in the local area.

Additional work has studied another prevalent group of subsidies, which include subsidies that are smaller in size but large in aggregate number. These studies primarily focus on the role of local demographics and within-state competition in the subsidy granting process (Felix and Hines, 2013; Ossa, 2018; Mast, 2019). Coupled with the findings on larger subsidies, these studies suggest that subsidies should drive some local corporate investment. To the extent that employment is either directly targeted through specific job-related subsidies, or indirectly affected through a complementary relationship with investment, we would observe increased employment after subsidy receipt. We build on this literature by being (to our knowledge) the first to empirically examine the role of subsidy disclosures in subsidy effectiveness.

2.2 The Role of the Information Environment in Subsidy Effectiveness

The rise of subsidy disclosure mandates from granting jurisdictions is often attributed to a growing need to ensure firms deliver on targeted outcomes (Pew Charitable Trusts, 2017).[4] These mandates are based on the premise that "the most fundamental reform is disclosure, i.e. the public reporting of which companies are receiving subsidies and how much they are getting" (Good Jobs First, 2010, p. 3). The accounting literature on the role of disclosure addresses the important role of information in achieving certain capital market and real investment outcomes.

Although the disclosure literature primarily focuses on corporate information about firm financial performance, this literature more generally demonstrates two non-mutually exclusive roles of disclosed information, allowing investors to (i) improve their valuation of potential returns

[4] For example, a 2017 Pew study entitled *How States are Improving Tax Incentives for Jobs and Growth* states that "with better information, lawmakers can make better decisions. State officials are using evaluations to help identify incentives that are working well and reform those that are not. As a result, evaluations are helping states save millions of dollars while achieving stronger economic results" (p. 22). Good Jobs First states that "[t]axpayers have a right to know about the ways in which their money is being diverted to private parties. Transparency is not an abstract principle; it helps to promote integrity and efficiency in the awarding of subsidies" (p. 3 in the 2010 and 2014 studies).

to an investment, and (ii) monitor the use of committed capital by the recipient.[5] By extension, this literature suggests that increased government disclosures about subsidies could play an important role in driving local economic outcomes.

First, a better information environment surrounding subsidies permits a better evaluation of the highest return projects a government should "invest" subsidy tax dollars in during the granting process. In a competitive market without frictions, governments should invest in all positive NPV projects, including subsidies targeted at stimulating local economic growth. However, government resources are not without limit, thus requiring a welfare-maximizing granting agency to choose only the highest return investments. Potential subsidy recipients, in turn, have private information about their planned use of funds and expected returns on planned investments. Any number of assumptions would preclude potential subsidy recipients from voluntarily revealing all such private information to the granting agency, such as a non-zero cost to firms of making such disclosures (e.g., Verrecchia, 1983) or variation across granting agencies in how they prioritize or weigh revealed information (e.g., Dutta and Trueman, 2002; Sujis, 2007).

Despite the potential benefits of information in the subsidy granting process, this process has traditionally been opaque. In the early 2000s, there was little public information about how a firm could even request such subsidies. Even now, many local websites provide little formal information about the application process. We are unable to observe private information provided

[5] For example, voluntary disclosures provided by managers reduce mispricing ahead of an IPO (Schrand and Verrechia, 2005; Leone, Rock, and Willenborg, 2007), improve analyst forecasts and reduce forecast dispersion (Bowen, Davis, and Matsumoto, 2002), reduce information asymmetry (Brown, Lo, and Hillegeist, 2004), and increase monitoring by shareholders (Bens, 2002). Further, a wide stream of the disclosure literature links reductions in information asymmetry to more efficient capital allocation, driving positive real investment and employment outcomes. For example, Badertscher et al. (2013) document industries with greater public firm presence are more responsive to investment opportunities due to greater information from publicly disclosing firms. Shroff et al. (2014) find that greater transparency within a foreign subsidiary location permits better monitoring, thereby increasing the firm's responsiveness to local growth opportunities. Cho (2015) finds improved capital allocation within multi-segment firms following an accounting disclosure change. For a review of this literature, see Beyer, Cohen, Lys, and Walther (2010).

by firms to granting agencies during the subsidy granting process. In the absence of such data, we study variation in the existence and type of government disclosures related to the firm recipient, the recipient's intended investment, and subsidy details to measure the subsidy information environment.

Second, a better subsidy information environment can facilitate monitoring of the use of granted funds by relevant stakeholders, including both the granting agency and local taxpayers funding these subsidies. A key concern upon granting a firm a subsidy is whether the firm will deliver on employment commitments. Few firms voluntarily provide disclosures in their own financial statements about subsidy receipt, and generally only megadeals attract widespread media attention. Furthermore, clawback provisions that permit the granting jurisdiction to terminate benefits or require the firm to repay granted funds are still relatively rare and infrequently enforced. Thus, subsidy information disclosed by the granting agency is the predominant source of information that can be used for monitoring purposes. At a minimum, this information provides the parties to the agreement that should be held accountable for perceived or actual misuses of public funds. At the other extreme, disclosed information can detail the amount of the subsidy and the firm's expected commitment for capital investment, new jobs, and/or minimum salaries. Thus, disclosures about granted subsidies made public by granting agencies often fills an information void, allowing for better monitoring of the return on granted funds by public.

We therefore expect increased government disclosures of information to facilitate employment responses through both channels. We predict the following:

H1: The positive relation between subsidies and employment outcomes is stronger for subsidies with greater government disclosures of subsidy information.

However, it could be the case that subsidy disclosure plays no role in subsidy effectiveness for at least two reasons. First, we focus on subsidy information that is disclosed publicly to

stakeholders outside the recipient firm or granting agency. If the granting agency is the primary user of the subsidy information, for both monitoring firm recipients and assessing future subsidy grants, it is possible that only subsidy information possessed privately by the granting agency is important for subsidy effectiveness. Second, any positive effects of subsidy disclosure may be delayed beyond our sample window.

There are also a number of reasons subsidies could be *less* effective in the presence of greater subsidy disclosure. First, increased monitoring resulting from more disclosed information about subsidies could induce granting agencies or subsidy recipients to make myopic investment decisions that benefit the grantor, sacrificing higher long-term local economic growth for more immediate "results."[6] For example, more disclosed information about granted subsidies could result in granting jurisdictions choosing the most politically palatable grant recipients, potentially at the expense of lower expected outcomes from the project (Aobdia et al., 2020; Slattery, 2021).

Second, subsidy disclosures could reveal proprietary information about the recipient's market or growth strategy, for example, undercutting the subsidy recipient's competitive position and resulting in worse local economic outcomes (Graham, Harvey, and Rajgopal, 2005; see also Appendix A in Heinle, Samuels, and Taylor, 2018). On this basis, business groups have pushed back on disclosure regimes, raising public officials' concerns that the private sector would view disclosing jurisdictions to be unfriendly to business (Good Jobs First, 2010 and 2014).

Third, more disclosed information about granted subsidies could signal which jurisdictions are willing to provide these subsidies and the type of firms that receive them. The availability of this disclosed information could encourage more firms to request subsidies from granting jurisdictions, potentially resulting in the granting of many subsidies to existing businesses with

[6] For reviews of the accounting literature on managerial myopia, see Schipper (1989), Healy and Whalen (1999), and Fields, Lys, and Vincent (2001).

little overall marginal benefit on the local economy.

Finally, greater disclosed information could lead to less effective subsidies by causing some of the granted funds to be spent on outcomes other than capital investments and employment. For example, constituents and 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); relatedly, the company itself may divert funds to support local organizations as a way to convey that the firm is a good corporate citizen. While these effects could have positive outcomes for counties, they may not necessarily manifest in the three outcomes we study. Thus, despite increased available information about subsidies, there is little evidence of the extent to which the effectiveness of subsidies varies with government disclosures about these awards. We aim to fill this gap with our empirical tests.

3. Research Design

3.1 Extension of Earlier Models

We begin by examining under which conditions the more expansive sample of subsidies we study (for purposes of testing the role of the subsidy information environment) exhibits similar effects as that in the prior literature. Prior research generally finds that large subsidies are associated with greater employment (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Slattery and Zidar, 2020). Because we are interested in total increases in local area activity, including both direct and spillover effects, we test the relation between firm-specific subsidies and employment outcomes aggregated across all firms at the county level.

To test these effects in our setting, we estimate staggered difference-in-difference regressions (Stevenson and Wolfers, 2006) of the following form:

$$Employment_{i,t+1} = \alpha + \beta_1 Subsidy_{i,t} + Controls_{i,t} + County_i + StateByYear_{j,t} + \varepsilon \quad (1)$$

Subscript *i* denotes the county, *j* denotes the state, and *t* denotes year. The dependent variable $Employment_{i,t+1}$ represents the natural logarithm of the number of employees $Ln(Employees)_{i,t+1}$ or the amount of aggregate wages $Ln(Wages)_{i,t+1}$ in a specific county; we discuss these measures below in Section 3.3. We measure $Subsidy_{i,t}$ two ways. First, to capture the relation between subsidy incidence and employment outcomes, we use $PostSubsidy_{i,t}$, which is an indicator equal to one for county-year observations with at least one company receiving a subsidy in years including and following the year of subsidy grant, and zero otherwise. Because all counties have at least one subsidy recipient at some point during the sample period, inclusion of $PostSubsidy_{i,t}$ captures variation in the timing of when a sample county had its first subsidy recipient. Second, to capture the relation between subsidy intensity and employment outcomes, we use $Ln(\#FirstSubsidies)_{i,t}$. $Ln(\#FirstSubsidies)_{i,t}$ is the natural logarithm of one plus the number of subsidies observed during the first year a county has a subsidy recipient. Measuring subsidy intensity is important in our sample given our relatively more frequent but smaller subsidies (as compared to prior studies) in any given county-year. Some specifications also account for subsequent subsidy activity by including the logarithm of one plus the cumulative number of subsequent subsidies (i.e., not including those granted in the year of first-observed subsidies) in county *i* and year *t*.[7] We use GJF data to identify counties with subsidy recipients and discuss the subsidy data further in Section 3.4. Based on earlier work, we should observe that subsidies are

[7] Counties have on average 2.1 (6.0) subsidies in the first year and 4.9 (24.1) subsidies over the sample period, using logged (raw) mean values. We use the log transformation throughout our empirical tests to account for substantial skewness in subsidy data, and we report ranges of values and economic magnitudes based on logged and raw means for completeness. When logging, we add one to the number of subsidies so that we have non-missing values for county-years prior to subsidy grant. We do not directly test the relation between subsidy dollar values and subsequent economic activity for two reasons. First, the GJF data to not include subsidy dollar value for all subsidies in the sample. Second, subsidies are generally awarded for a duration longer than one year, but the data do not include the dollar value of each award by year.

positively associated with employment outcomes ($\beta_1 > 0$), although the strength of the association may be diminished given that we study relatively smaller awards.

As described above, prior work generally examined the role of large subsidy awards on employment outcomes. These large subsidies typically included several different types of subsidies, including property tax abatements, job credits, and income tax benefits that would stimulate employment either directly (such as through job credits) or indirectly (such as through the complementarily of labor with capital investment). A distinguishing feature of our sample is that, in contrast to megadeals, a firm may receive one type of subsidy rather than a package of different awards. Thus, to improve the power of our tests, particularly our hypothesis tests about the subsidy information environment, we refine the subsidy indicator based on the criteria of the program in subsequent tests. We identify subsidy criteria based on reading the description of each of the 300 (465) state (local) subsidy programs included in the data. In these tests, we replace $PostFirstSubsidy_{i,t}$ with $PostFirstEmploymentSubsidy_{i,t}$, an indicator equal to one for years including and following the first-observed subsidy targeted at increased employment received by a firm in the county, and zero otherwise. Similarly, we replace $Ln(\#FirstSubsidies)_{i,t}$ with $Ln(\#FirstEmploymentSubsidies)_{i,t}$, a continuous variable equal to the logarithm of one plus the number of employment subsidies observed during the first year a county has a subsidy recipient. To ensure that we continue to account for all types of subsidies that could indirectly affect employment, we also include $PostFirstNonEmploymentSubsidy_{i,t}$, an indicator equal to one for years including and following the first-observed subsidy targeted at outcomes other than employment, such as real property improvements, R&D spending, and film tax credits (among others) and zero otherwise. These tests intend to show under which conditions subsidies specifically targeting employment lead to increased local employment and wages.

We include county-level control variables (*Controls*$_{i,t}$) motivated by prior literature, further discussed in Section 3.5. Finally, we include county and state-by-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 state-by-year fixed effects control for state-level macroeconomic trends (such as the 2008-2009 financial crisis) and state-year factors such as GDP, state corporate and personal income tax rates, and other state incentives that affect private sector presence. Main effects for county-years having at least one subsidy recipient are captured by the county and state-by-year fixed effects. We cluster standard errors by county.[8]

3.2 Hypothesis Test

$$Employment_{i,t+1} = \alpha + \beta_1 FirstEmploymentSubsidy_{i,t} + \beta_2 Information_{i,t} * FirstEmploymentSubsidy_{i,t} + \beta_3 PostFirstNonEmploymentSubsidy_{i,t} + Controls_{i,t} + County_i + StateByYear_{j,t} + \varepsilon \quad (2)$$

To test our hypothesis that the relation between subsidies targeting increased employment and employment outcomes are stronger in those areas with greater disclosed information, we augment Eq. (1) to include *Information*$_{i,t}$, which is an indicator equal to one for subsidy counties with government disclosures about subsidies that are granted; see Section 3.5. We construct this measure based on the available subsidy disclosures in the first year that the first subsidies are observed in a county. Because this measure is not time-varying and because we estimate Eq. (2) with county- and state-by-year-fixed effects, the main effect of *Information*$_{i,t}$ is subsumed. The interaction term captures whether there is a differential relation between subsidies targeting employment and employment in the presence of greater disclosed information about the subsidies;

[8] Inferences are similar when alternatively clustering by state.

we predict $\beta_2 > 0$. As before, we separately examine both subsidy incidence and subsidy intensity in a given county-year.

3.3 Measures of Economic Activity

We obtain the number of employees and aggregate wages 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, the data provide relatively comprehensive coverage on U.S. firms.[9] From the quarterly filings, we obtain a county-level calendar-year average, computed by the BLS, of the number of employees and aggregate annual wages paid to construct $Ln(Employees)_{i,t+1}$ and $Ln(Wages)_{i,t+1}$. Because the subsidy data capture subsidies to private-sector firms, we exclude public-sector employees and wages from our measures following other work (Dube, Lester, and Reich, 2010). We include two measures of employment because the expected positive relation could arise either through the hiring of more employees or through higher aggregate wages to existing employees (or both).[10]

3.4 Subsidy Data

GJF data are 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. We use subsidy data beginning in 2008, as this reflects the year with relatively complete coverage across the broadest set of U.S. states. We include all categories of awards in the GJF data to best measure the full incentive package awarded to a firm. These include (i) Tax Credits, which are dollar value awards that reduce a firm's tax liability dollar-for-dollar; (ii) Tax

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

[10] One possible outcome is that we observe more employees in a county 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 or to increase executive compensation.

Abatements, which provide a percentage reduction of a firm's tax liability; (iii) Grants, up front direct cash transfers to the firm from the government; (iv) Reimbursements, *ex post* cash transfers for expenses incurred by a company, such as job training activities; (v) Enterprise Zones, tax credits or abatements tied to a company's decision to locate to a particular neighborhood; and (vi) 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. However, we exclude the extremely large "megadeal" packages of subsidies because they receive a high level of press coverage and public attention, reducing variation in the amount of information available about them.

A key concern with these data is that state and local jurisdictions notoriously provide incomplete data coverage about all subsidies. Thus, we limit the sample to 27 states in which we consistently observe data on specific subsidy programs, implying a relatively complete collection of requisite information. Within these states, there is substantial variation in the extent of information that is provided, particularly because a large amount of subsidy information was obtained via FOIA requests that GJF filed with local authorities. That is, much information in Subsidy Tracker was not directly provided via voluntary or mandatory requirements, permitting tests of whether voluntary or mandated information alters subsidy effectiveness.[11]

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. This step permits inclusion of both public and private

[11] Our tests are conditional on observing indicia of a subsidy in the GJF data. This data requirement is necessary to ensure that the sample reflects jurisdictions with relatively complete subsidy coverage so that an observed lack of detailed subsidy information can be attributed to non-disclosure as opposed to missing subsidies. However, this data limitation implies that our tests speak primarily to whether the subsidy information environment affects subsidy outcomes, conditional on enough information being provided to observe the subsidy in the first place (the intensive margin), as opposed to whether any information about subsidy existence plays a role (extensive margin).

recipients, with private companies comprising approximately 89 percent of the sample. We hand-match subsidy data to BLS employment data at the county-year level.

3.5 Measures of the Subsidy Information Environment

$Information_{i,t}$ is one of three indicators for the availability of disclosed subsidy information, all of which relate to state-level subsidy programs. First, we study the effect of state-level subsidy disclosure initiatives. We focus on this channel because it has been the most prevalent step taken by governments to address public concerns about subsidy effectiveness. We identify whether a state had a disclosure initiative in place based on listings compiled by GJF in three different studies published in 2007, 2010, and 2014.[12] $DisclosureRegime_{i,t}$ is an indicator equal to one for county-years in which the first subsidy was subject to disclosure requirements, and zero otherwise.

Second, $Online_{i,t}$ is an indicator equal to one (and zero otherwise) if information about all subsidy recipients in a given county is made available online. We identify this based on whether the GJF data include a government website link for information about the subsidy, as opposed to an indication from GJF that the subsidy was obtained through a specific FOIA request by GJF. The assumption when using this measure is that those subsidies posted online reflect counties with the most readily available information for public disclosure.

The third measure examines whether subsidy effectiveness varies based on whether the subsidy information disclosed online further includes an estimate by the recipient firm to the granting jurisdiction about the number of jobs the subsidy is expected to create. Although this may originally have been a private disclosure by the subsidy recipient to the granting jurisdiction, we

[12] 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 studies 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 disclosure initiatives or laws prior to 2010 based on their inclusion in the 2007 report.

observe that nearly all subsidies that required a disclosure of job commitments were also disclosed online. Thus, this type of job disclosure reflects the granting state's requirement to estimate and quantify expected job growth, as well as their preference to make these commitments public. $Online{+}Jobs_{i,t}$ is an indicator equal to one (and zero otherwise) if information about all subsidy recipients in a given county is made available online and all subsidy recipients in the county were required to provide an estimate of the number of jobs that would be created as a result of the subsidy.[13]

3.6 Control Variables

In addition to county and state-by-year fixed effects, we control for known county-level determinants of employment decisions. We control for county population, measured using data from the U.S. Census Bureau's American FactFinder. We also control for educational attainment, measured as the percentage of the population with at least a bachelor's degree (e.g., Card, 1999). We obtain data for this measure from the Census Bureau's American Community Survey.

3.7 Control Group

One concern when studying smaller subsidies is how to construct the appropriate group against which to compare economic effects. 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 not only measuring the effect of subsidies on economic outcomes and the role of the subsidy information environment in those outcomes, but is also

[13] To ensure this test is properly specified, we set $Online{+}Jobs_{i,t}$ equal to zero if $Online_{i,t}$ equals zero such that $Online{+}Jobs_{i,t}$ represents the interaction of $Online_{i,t}$ with an indicator for the existence of a job creation estimate. When we test local subsidy programs in the Online Appendix, we use a fourth measure that also captures the local subsidy information environment using the presence of a local newspaper (Gentzkow et al., 2011; Gao et al., 2019). See Online Appendix Table 2.

challenging because the ideal match – jurisdictions that offered or would offer a similar subsidy to the same recipient – is unobservable for the broad range of subsidies we study.

We mitigate this concern by restricting the sample to counties with a subsidy recipient at some point during the sample period. Thus, we benchmark the employment of county-years with subsidy recipients against counties with recipients that later receive subsidies. To do so, we first restrict the sample to only those counties with first-time subsidy recipients during our sample period. Our tests measure the effect of subsidies on employment relative to the same counties prior to receiving the subsidy and other subsidy recipient counties that have not yet received a subsidy. The use of this control sample provides several benefits. First, this control sample addresses the selection concern that counties with subsidy recipient firms may be different from other counties across unobservable characteristics. By mitigating this selection concern, these tests permit a better assessment of the role of the subsidy information environment in subsidy effectiveness.[14] Second, this control sample mitigates potential control group contamination arising from assigning a county as a control due to a lack of observed subsidies in a county during our sample period, when in fact the county had subsidy recipients not observable due to limited information about the subsidies. Finally, it leverages the "event-study" approach used in recently published work in public economics, thereby extending this identification method to the accounting literature (Fuest et al., 2018). We mitigate concerns raised in Baker et al. (2021) about using later-treated counties in the control sample based on refinement due to the specific subsidy type; see Section 4.

3.8 Sample Selection and Description

[14] Additional selection concerns, such as that granting jurisdictions with better information environments are generally more economically efficient, are mitigated by our focus on state subsidy programs, the use of state-by-year fixed effects, and measurement at the county-level: our tests examine variation in the role of state-level disclosures in subsidy effectiveness at the county-level within state-year.

Table 1 describes the sample selection process to identify counties with a subsidy recipient. We focus primarily on state-level subsidies as opposed to city and county subsidies because the data covering state subsidies are most complete (Good Jobs First, 2013) and permit use of the largest sample. Subsidies granted via state-level programs are also more exogenous to local county economics relative to those granted via local subsidy programs. Furthermore, most initiatives to implement disclosure regimes have occurred at the state level, thereby providing a more powerful setting in which to assess the role of the subsidy information environment in economic outcomes. However, because firms can also receive local (city and county) subsidies, we conduct additional tests on a much smaller sample of observations with necessary local data; see Online Appendix Tables 1 and 2.

As detailed in Table 1 Panel A, we start with all county-years with available BLS employment data for the eight years from 2008 to 2015 (n=25,216 observations for 3,152 counties).[15] We drop counties in 17 states reporting three or fewer years with any subsidy activity (suggesting that subsidy data for those jurisdictions are incomplete), and counties in six states with inconsistent subsidy coverage in the dataset(n=8,568).[16] This step leaves 27 states for which we observe consistent subsidy information throughout the sample period. Because we assume that the subsidy information is complete for this set of states, we drop the 905 counties that never have a

[15] We retain state and local GJF subsidies with requisite county-level information as the starting point for our sample, which represents approximately 91 percent of the total dollar value of all subsidies at the state and local levels in Subsidy Tracker.

[16] The 17 states with fewer than three years of subsidy coverage include Alaska, Arkansas, Delaware, Hawaii, Idaho, Mississippi, Montana, New Hampshire, New Jersey, North Dakota, Rhode Island, South Dakota, Utah, Vermont, Washington, West Virginia, and Wyoming. The six states with large changes in subsidy coverage, implying underlying data issues, include Colorado (8,130.64% increase from 19.8 to 1,629.7 average annual subsidies during the sample period), Iowa (decrease from 176.6 average annual subsidies from 2008-2012 to 51.5 from 2013-2016), Michigan (decrease from 626 average annual subsidies from 2009 to 2011 to 120 in 2012), North Carolina (increase from 357 average annual subsidies in 2009-2010 to 1,485.75 from 2011-2014), Oregon (decrease from 1,577.6 average annual subsidies from 2008-2010 and 2012-2013 to 90 in 2011, 2014, and 2015), and Tennessee (increase from 2.7 average annual subsidies from 2009-2011 to 127.5 from 2012-2015).

subsidy recipient or have megadeal recipients, resulting in a sample of 1,176 distinct counties (9,408 county-year observations). Finally, we drop 88 counties missing requisite data for estimation. The remaining 1,165 distinct counties have at least one subsidy in 5,311 county-years out of 9,320 total observations included in the final sample. Table 1, Panel B provides the distribution of county-years by state. The states with the most observations for subsidy recipients include Indiana, Kentucky, Ohio, and Virginia.

Table 2, Panel A provides descriptive statistics for the sample of state-level subsidy county-years. The average county reports 47,234 employees with aggregate wages of $2.25 billion annually working in approximately 3,552 business establishments. All of these amounts are skewed, with counties in the 99th percentile reporting 605,382 employees and $34.1 billion of aggregate wages (untabulated). Approximately 21 percent of the working-age population has at least a bachelor's degree.

Panel B provides sample composition by type of subsidy. We first show descriptive statistics on the 1,165 distinct county-years with first-observed subsidies granted during our sample period. We observe 6,996 distinct first-observed subsidies in this sample. 6,188 of these first-observed subsidies include data on the dollar value of the award, aggregating to almost $2.1 billion in total. Panel B also provides information on all subsidies reported by counties with subsidy recipients. The aggregate dollar value of the 46,139 subsidies in our sample with requisite data is $15.0 billion.

These statistics imply an average subsidy value of $324,277. For reference, we compare our sample to that of the subsidies studied in prior work. This average value is much smaller as compared to the “megadeal” subsidies studied in Slattery (2020) and Slattery and Zidar (2020) of $149.5 million and $178.4 million, respectively. However, these subsidies are also much more

prevalent, with almost 85 times the number of awards. Together, the descriptive statistics in Panels A and B imply that the average county has approximately 4.9-24.1 total (first-time plus subsequent) subsidies over the sample period each year worth $1.6-$7.8 million. Figure 1 maps the number of subsidy counties by state.

Panel C presents the distribution of subsidies and subsidy-years across time. The first two columns show the number of subsidies based on the first time a subsidy is observed in a county, whereas the second two columns show all years for those counties with subsidies. Panel D shows the number of subsidies by year based on the stated criteria of the program. We manually obtained information on these criteria by reviewing GJF subsidy information, state subsidy program documentation, and program-specific information available via internet search. The descriptive statistics in Panels A and B imply that the average county has approximately 3.4-19.4 total (first-time plus subsequent) employment subsidies over the sample period each year worth $1.1-$6.3 million. Panel E provides a summary of the total first-time and subsequent subsidies, showing that 17.5-25.3 percent of subsequent subsidies are awarded to the same firm. Subsequent subsidies largely appear to be awards from the same type of subsidy programs as those granted in the first year, ranging from 68.0-81.7 percent. Appendix B lists all subsidy programs reviewed, and Appendix C includes examples by the type of subsidy criteria. Non-employment subsidies include awards targeting a variety of activities such as capital investment, infrastructure investment, innovation activity, film tax credits, and historic rehabilitation credits.

4. Main Results

4.1 Extension of Prior Work

Table 3 presents results from estimating Eq. (1). Columns (1)-(3) report results using the number of employees as the dependent variable, and Columns (4)-(6) report results using aggregate wages. In Columns (1) and (4), we examine the average relation between the incidence

of subsidies in a county and these employment outcomes. Recall that this coefficient detects whether county employment differs on average after a subsidy is given, relative to the same county in the pre-subsidy period and those counties that do not yet have a subsidy recipient. We find no evidence of a relation on average for counties that have recipients of the relatively smaller subsidies we study. Figure 2 depicts this effect graphically; specifically, Panel A (C) plots this effect for *Ln(Employment)* (*Ln(Wages)*), showing that there is no observed statistically significant effect when examining the incidence of subsidies. This figure also demonstrates that there are no pre-period differences between the counties that first receive subsidies and the control counties (those that later receive subsidies), mitigating concerns that observable factors are playing a role in estimation.

In Columns (2) and (4), we include the number of first-time subsidies observed to account for subsidy intensity. Because the average recipient county-years reports approximately 2.1-6.0 subsidies in the first subsidy year, these tests examine whether having multiple recipients in the first year alters the relation between subsidies and local economic outcomes. However, after including $Ln(\#FirstSubsidies)_{i,t}$ as a measure of subsidy intensity in the first subsidy year, we continue to observe no statistically significant relation between subsidies and employment across these two columns.

In Columns (3) and (6), we continue to examine the relation between subsidy intensity and employment outcomes by additionally controlling for the number of subsidies granted in subsequent years. Although we continue to find no relation between the number of first-time subsidies granted and employment outcomes in these columns, we observe a positive and significant relation between the number of subsequent subsidies and employment outcomes. Using the average number of subsequent subsidies received by firms in a county-year over our sample

period (2.7-18.1), the coefficients of 0.0076 and 0.0057 on $Ln(\#SubsequentSubsidies)_{i,t}$ in Columns (3) and (6), respectively, suggest that subsequent subsidies lead to a 1.0-2.2 percent increase in number of employees and a 0.8-1.7 percent increase aggregate wages on average. While these employment effects are qualitatively consistent with earlier studies (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Slattery and Zidar, 2020), the results imply that counties need multiple years of subsidies to observe such effects.

One challenge with these tests is that they study the effect of any subsidy in a county on employment and wages. We next refine these tests to more precisely match the criteria of specific programs to employment outcomes. Specifically, we replace $PostFirstSubsidy_{i,t}$ in Eq. (1) with $PostFirstEmploymentSubsidy_{i,t,}$ an indicator equal to one if the stated criteria of the subsidy is to increase employment based on a manual review of subsidy programs. Similarly, we replace $Ln(\#FirstSubsidies)_{i,t}$ with $Ln(\#FirstEmploymentSubsidies)_{i,t,}$ a continuous variable equal to the logarithm of one plus the number of first-time subsidies granted with the stated criteria of increasing employment. To account for other subsidies, in all specifications we also include $PostNonEmploymentSubsidy_{i,t,}$ an indicator equal to one if the stated criteria of the subsidy does not include increased employment. Because this test compares counties with a firm recipient of an employment subsidy against both (i) other counties with firms that later receive an employment subsidy, and (ii) counties with firms that never receive an employment subsidy (instead, they receive for example an investment or a historical rehabilitation subsidy), this research design also addresses potential bias induced by using a sample that only includes treatment counties (Baker, Larcker, and Wang, 2020).

We present results of this refinement in Table 4. We continue to find no relation on average between the incidence of subsidies in a county and employment outcomes. Figure 2 also shows

this effect graphically, with Panel B (D) showing effects for *Ln(Employment)* ((*Ln(Wages)*). While the figure conveys the lack of a statistically significant effect after subsidies are granted, it also shows that there are no differences in the pre-period, mitigating concerns about unobservable factors affecting estimation.[17]

However we find some evidence of a positive relation once we account for subsidy intensity. Specifically, we find a statistically significant coefficient in Column (3) for the relation between the number of first-observed employment subsidies and number of employees. In both Columns (3) and (6), we continue to observe that the number of subsequent employment subsidies affects both outcomes. Using the average county-year number of first-time employment subsidies granted, which ranges 1.6-5.8, the coefficients of 0.0098 on $Ln(\#FirstEmploymentSubsidies)_{i,t}$ in Column (3) suggest first-time employment subsidies lead to a 0.9-1.9 percent increase in number of employees. The coefficient of 0.0128 on $Ln(\#SubsequentEmploymentSubsidies)_{i,t}$ means that subsequent subsidies are associated with 1.3-3.4 percent increase in the number of employees. Related to wages, the coefficient of 0.0111 on $Ln(\#SubsequentEmploymentSubsidies)_{i,t}$ in Column (6) suggests subsequent employment subsidies for the average county-year lead to a 1.2-3.0 percent increase in aggregate wages. Evaluated based on the median county, these estimates imply an increase of 112-229 workers for first-time employment subsidies on average. Similarly, these estimates suggest an increase of 163-417 workers and $4.9-$12.5 million in total wages, but only after 3.4 years of subsequent employment subsidies on average. These employment effects appear reasonable given that they imply an average salary of $30,017, which is similar to the median wage

[17] In untabulated analyses, we also re-estimate Eq. (1) instead using annual time indicators for *t-3*, *t-2*, and *t-1* to further assess if the pre-period trends were different for the first counties receiving Employment subsidies as compared to counties either (i) without an employment subsidy or (ii) later receiving subsidies (both of which are used as control observations). We find that none of the interactions of these pre-period time indicators with our treatment indicator are statistically significant, further mitigating concerns about pre-period differences across these groups.

in the county of $34,600 based on sample descriptive statistics. By hand-collecting data on the underlying criteria of the program, we are able to better match the underlying subsidy type with the intended employment outcome and thus accurately estimate the effects of subsidies on intended outcomes.

One concern is that Eq. (1) does not appropriately capture observable characteristics that could be correlated with the relation between subsidies and local economic effects. Therefore, we re-estimate Eq. (1) after replacing the state-by-year fixed effects with state-year control variables and year fixed effects; see the Online Appendix.

4.2 The Role of the Subsidy Information Environment in Subsidy Effectiveness

We next examine our main research question on the role of government disclosures of subsidy information in subsidy effectiveness. Against the baseline of overall positive effects of employment subsidies on employment outcomes, as discussed above, these tests focus on whether subsidy effectiveness differs with the presence of specific types of subsidy disclosures. Table 5 presents results of estimating Eq. (2). In Panel A, we explore the presence of a disclosure initiative. In Panel B, we examine the role of the availability of subsidy disclosures online. In Panel C, we explore the incremental effect of the provision of an estimate of job creation by the subsidy recipient to the granting agency over the availability of subsidy disclosures online.

In Panel A, we find some evidence that the relation between subsidy receipt and $Ln(Employees)_{i,t+1}$ is different in counties where subsidies are subject to a state disclosure initiative. In Column (1), we find a positive and significant coefficient of 0.0124 on the main effect of $PostFirstEmploymentSubsidy_{i,t}$, but a negative and statistically significant coefficient of -0.0149 on $PostFirstEmploymentSubsidy_{i,t}$*$DisclosureRegime_{i,t}$. This result suggests subsidies are *less* effective at achieving employment outcomes when government disclosures about the subsidies are

mandated under state reporting requirements. The result holds in Column (2) when examining subsidy intensity, though after controlling for subsequent subsidies in Column (3), we find diminished statistical significance of the negative coefficient on the interaction term. The signs of estimated coefficients are consistent when examining the role of disclosure initiatives on aggregate wages across Columns (4)-(6), however the estimated coefficients are not statistically significant. Although the statistical significance of the coefficients of interest varies across columns, in all columns F-tests confirm the aggregate effect of subsidies with government disclosures on employment outcomes is not statistically distinguishable from zero. This means that greater subsidy disclosures do not result in negative employment effects, but rather no change in employment on average. We explore the potential explanations for the negative incremental effect of subsidy disclosures in these tests in Section 4.3.

We find a similar pattern but stronger results in Panel B, where we examine the role of the availability of online disclosures about subsidies when examining employment outcomes. The incremental effect of online disclosure is negative and statistically significant in four of the six columns. However, as in Panel A, F tests reveal the overall effectiveness of subsidies with online disclosures remains positive in one specification and indistinguishable from zero in the remaining columns; that is, the total effect of these subsidies is never negative. We continue to find that having multiple years of subsidy recipients is important for increased employment. This is a costly strategy in that a county must have several years with subsidy recipients before observing any employment effect.

In Table 5, Panel C, we examine the incremental effect of the disclosure of expected job creation over the availability of online disclosures. Because jobs estimates are nearly always also disclosed online, we are limited to examining whether job commitment disclosures provide

incremental explanatory power over online disclosure. We generally find the same pattern of results as in Panel A for the effects of first-time subsidies with and without online disclosures. However, across all columns, we estimate a positive and significant coefficient on $PostEmploymentSubsidy_{i,t}$*$Online$+$Jobs_{i,t}$, indicating that the inclusion of expected job figures improves employment subsidy effectiveness. The estimated coefficients in Columns (1) and (4) imply that job creation figures lead to incremental increases of 1.81 percent in number of employees and 2.51 percent in aggregate wages, relative to just the availability of subsidy disclosures online.

Accounting for subsidy intensity allows us to evaluate a hypothetical trade-off between government disclosures of subsidy information on expected job creation and the number of subsequent subsidies required to achieve the same employment effects. The coefficients in Column (3) imply that by requiring job commitments from a first-time subsidy recipient, granting agencies could save the cost of granting 1.43 subsequent subsidies.[18] Similarly, the coefficients in Column (6) imply requiring job commitments from a first-time subsidy recipient achieves the same positive effect on aggregate wages of granting 4.3 subsequent subsidies. Given the average subsidy value of $324,000, subsidy-specific job commitment disclosures are associated with saving between $464,030-$1,381,615.

This specification also allows us to distinguish between the two different roles for the subsidy information environment. Specifically, the results imply that job commitment disclosures used to facilitate the grantor's investment decision are positively associated with subsidy employment outcomes. In contrast, these results are inconsistent with any subsequent monitoring

[18] The average incremental effect of a first-observed employment subsidy with online job commitments is 0.0158*Ln(1+1) = 1.1 percent. The average incremental effect of X subsequent subsidies is 0.0124*Ln(1+X). Thus, the number of subsequent subsidies X required to obtain the same average incremental effect of a first-observed employment subsidy with online job commitments is 1.43 (= $e^{(0.011/0.0124)} - 1$).

of subsidy information provided online facilitating subsidy employment outcomes given that the pure online provision of subsidy disclosures produces no incremental effect. In summary, the tests in this section suggest a nuanced role of the subsidy information environment in employment outcomes.

4.3 Explanations for the Negative Incremental Effect of Disclosure Regimes and Online Government Disclosures

As discussed in Section 2 when describing our hypotheses, there are several possible explanations for a negative effect of subsidy disclosures on subsidy effectiveness. In this section, we explore whether there is evidence consistent with these potential (and non-mutually exclusive) explanations.

First, greater government disclosures of subsidy information could result in myopic investment decisions by the granting agency or by recipient firms. To explore the validity of this explanation, we rely on anecdotal evidence. 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. For 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). Based on these examples, it seems

plausible that myopic investment decisions could contribute to the negative incremental effects of disclosure regimes and online disclosures.

Second, greater government disclosures of subsidy information could result in the granting of a greater number of subsidies with diminishing effectiveness. To assess this explanation, we regress the number of subsidies granted in year *t+1* on the number of subsidies granted in year *t* along with its interaction with $Information_{i,t}$. We observe positive and marginally significant coefficients on the interaction with *DisclosureRegime* (coefficient = 0.0330, t-statistic = 1.11) and *Online* (coefficient = 0.0557, t-statistic = 1.41), versus a smaller positive but insignificant coefficient on the interaction with *Online+Jobs* (coefficient = 0.0070, t-statistic = 0.22). These results are consistent with our heterogenous results on the role of government disclosures in subsidy effectiveness, implying that this second explanation could contribute to the negative incremental effects of disclosure regimes and online disclosures.

Third, government disclosures of subsidy information could increase public pressure on recipient firms to use granted funds for outcomes other than employment. For example, in a press release announcing the opening of a facility partly funded by a North Carolina subsidy, MetLife described how the company 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). Thus, anecdotal evidence also supports this explanation for the negative incremental effects of disclosure regimes and online disclosure. [19]

4.4 Establishments

[19] Measurement at the county-year level permits inclusion of a broad range of subsidies across both public and private firms. However, while the inclusion of both types of subsidy recipients more precisely captures the extent of subsidies received in a county, it precludes firm-specific tests. Specifically, because over 90 percent of firm recipients in our sample are private corporations, we are unable to construct tests related to the release of proprietary information.

Our primary tests focus on employment given earlier work on subsidies, which permits comparison of effects and magnitudes across tests. In additional analyses, we also study establishment outcomes following work more generally examining state tax rates (Giroud and Rauh, 2019). Table 6, Panel A provides the number of subsidies targeting increased investment by year, identified using the same manual review process used to identify subsidies targeting increased employment. We note that subsidies can target both investment and employment, thus the indicators $PostFirstEmploymentSubsidy_{i,t}$ and $PostFirstInvestmentSubsidy_{i,t}$ are not mutually exclusive.

Table 6, Panel B presents results from estimating Eq. (2) evaluating the incremental effect of job commitment disclosures of expected outcomes over the online subsidy disclosures. We focus on this analysis as it is where we found the most positive role of government disclosures in the effectiveness of employment subsidies for employment outcomes. However, we no longer find a positive and statistically significant role of job commitment disclosures in the investment setting. Results using our other measures of government disclosures (untabulated) likewise provide limited evidence of a role of the subsidy information environment on establishment outcomes. One possible explanation for the results relates to the measure of investment used in these tests. Ideally, we would examine the role of the subsidy information environment in subsidy effectiveness using a broad measure of investment like capital expenditures, where a relatively smaller subsidy may motivate spending on tangible fixed assets such as machinery and equipment. However, we are only to observe large-scale investments in the form of number of establishments in a broad enough sample for regression analysis. Thus, the lack of results may be attributable to the fact that we are unable to measure smaller investments that do not constitute new establishments.

5. Conclusion

We examine the role of the subsidy information environment in the relation between firm-specific subsidies and local area employment. We first examine the circumstances under which our expanded sample of thousands of state firm-specific subsidies exhibit the similar positive association between subsidies and employment as prior literature. To improve the power of these tests, we collect data on the different criteria for each subsidy program to best match each subsidy with intended local outcomes. These tests confirm that subsidies granted based on employment criteria are positively associated the number of employees and aggregate wages.

Our main finding is that the local subsidy information environment plays an important and nuanced role in subsidy effectiveness. Motivated by the prior accounting literature on information, disclosure, and monitoring, our results are consistent with heterogeneity in the role of the subsidy information environment in subsidy effectiveness. We only find positive employment outcomes for subsidies with job commitments disclosed online.

Our results are subject to several caveats related to our inability to measure firm-level outcomes, the unobservability of ideal control counties, and the lack of subsidy coverage. Furthermore, a more complete cost-benefit analysis would need to consider all relevant outcomes to assess the net cost or benefit to the local communities, such as the structural model in Slattery (2020). Finally, we cannot consider general equilibrium effects of subsidies on employment levels and growth. Nonetheless, we think this work is a step towards better understanding the effects of these subsidies, as well as the role of subsidy information in their effectiveness. 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	
$Employees_{i,t+1}$	Total number of private-sector employees working in county *i* in year *t*+1 relative to obtaining a subsidy in year *t*; computed by BLS as the average number of private-sector employees at the end of each quarter.
$Establishments_{i,t+1}$	Total number of private-sector business establishments in county *i* in year *t*+1 relative to obtaining a subsidy in year *t*; computed by the Bureau of Labor Statistics (BLS) as the average number of active establishments 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 relative to obtaining a subsidy in year *t*.
Subsidy & Information Variables	
$DisclosureRegime_{i,t}$	Equals 1 for county-years with first-observed subsidies for which the state had a subsidy disclosure initiative or law in place at the time of the subsidy and 0 otherwise.
$Information_{i,t}$	Equal to $DisclosureRegime_{i,t}$, $Online_{i,t}$, $ExAnteJobs_{i,t}$, all defined in this appendix.
$Ln(\#FirstEmploymentSubsidies)_{i,t}$	Log of the number of first-time subsidies targeting increased employment for years including and following the first-observed subsidy received by a firm in county *i*.
$Ln(\#SubsequentEmploymentSubsidies)_{i,t}$	Log of the running count of subsequent subsidies targeting increased employment in county *i* and year *t*.
$Ln(\#FirstInvestmentSubsidies)_{i,t}$	Log of the number of first-time subsidies targeting increased investment for years including and following the first-observed subsidy received by a firm in county *i*.
$Ln(\#SubsequentInvestmentSubsidies)_{i,t}$	Log of the running count of subsequent subsidies targeting increased investment in county *i* and year *t*.
$Ln(\#FirstSubsidies)_{i,t}$	Log of the number of first-time subsidies for years including and following the first-observed subsidy received by a firm in county *i*.
$Ln(\#SubsequentSubsidies)_{i,t}$	Log of the running count of subsequent subsidies in county *i* and year *t*.
$Online_{i,t}$	Equals 1 for county-years with first-observed subsidies for which there is subsidy information disclosed online and 0 otherwise.
$Online+Jobs_{i,t}$	Equals 1 for county-years with first-observed subsidies for which there is subsidy information disclosed online about expected job creation and 0 otherwise.
$PostFirstSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy received by a firm in county *i* and 0 otherwise.
$PostFirstEmploymentSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy targeting increased employment received by a firm in county *i* and 0 otherwise. Subsidies are classified on the basis of having employment criteria observed from manual reviews of each subsidy program.
$PostFirstInvestmentSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy targeting increased capital investment received by a firm in county *i* and 0 otherwise. Subsidies are classified on the basis of having investment criteria observed from manual reviews of each subsidy program.

$PostFirstNonEmploymentSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy targeting goals other than increased employment received by a firm in county *i* and 0 otherwise. Subsidies are classified based on the stated criteria observed from manual reviews of each subsidy program.
$PostFirstNonInvestmentSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy targeting goals other than increased investment received by a firm in county *i* and 0 otherwise. Subsidies are classified based on the stated criteria observed from manual reviews of each subsidy program.
Control Variables	
$Ln(Population)_{i,t}$	Log of total population of county *i* in year *t*+1. County population data is computed by the US Census Bureau.
$\%Educ_{j,t}$	Percentage of people in each county with at least a four-year college degree in year *t*.

Appendix B
Sample of State Subsidy Programs

AL	Industrial Dev. Grant
AZ	21st Century Energy Demonstration Projects Grant Prog.
AZ	Arizona Job Training Prog.
AZ	Enterprise Zone Premium & Income Tax Credits
CA	California Competes
CA	Employment Training Panel
CA	Employment Training Panel
CT	Digital Animation Production Company Tax Credit
CT	Digital Media & Film Tax Credit
CT	Dry Cleaning Establishment Remediation Fund
CT	Enterprise Zone
CT	Film Infrastructure Tax Credit
CT	First Five
CT	Historic Preservation Tax Credit
CT	Historic Structures Rehabilitation Tax Credit
CT	Job Creation Tax Credit
CT	Job Expansion Tax Credit
CT	Manufacturing Assistance Act
CT	Public Act/Special Act
CT	Qualified Small Bus. Job Creation Tax Credit
CT	Small Bus. Express
CT	Small Bus. Revolving Loan Fund
CT	Urban Action Grant
CT	Urban & Industrial Site Reinvestment Tax Credit
FL	Brownfield ReDev. Bonus
FL	Brownfield Voluntary Cleanup Tax Credits
FL	Capital Investment Tax Credit
FL	Econ. Dev. Grant
FL	Econ. Dev. Transportation Fund
FL	Film & Entertainment Incentive
FL	High Impact Performance Incentive
FL	Innovation Incentive Fund
FL	Local Government Distressed Area Matching Grant
FL	Manufacturing & Spaceport Investment Incentive
FL	Qualified Defense & Space Contractor Tax Refund
FL	Qualified Target Industry Tax Refund
FL	Quick Action Closing Fund
FL	Quick Response Training
FL	Semi-Conductor Defense & Space Technology Tax Exemption
FL	Urban Jobs Tax Credit
GA	Econ. Dev., Growth & Expansion (EDGE) Fund
GA	Entrepreneur & Small Bus. Dev. Loan Guarantee Prog.
GA	Regional Econ. Bus. Assistance Prog. (REBA)
IL	Bus. Dev. Public Infrastructure Prog.
IL	Coal Competitiveness Prog.
IL	Coal Demonstration Prog.
IL	Econ. Depressed Areas
IL	EDGE Tax Credit
IL	Electric Vehicle Industry Dev.
IL	Enterprise Zone Expanded M&E Sales Tax Exemption
IL	Enterprise Zone State Utility Tax Exemption
IL	High Impact Bus. Designation
IL	IDOT Econ. Dev. Prog.
IL	Large Bus. Dev. Assistance Prog.
IL	Next Generation Biofuels Grant Prog.
IL	Next Generation Biofuels Production Prog.
IL	River Edge Redev. Zone Grant Prog.
IN	Bus. Dev. Loan Fund
IN	Community Revitalization Enhancement District Tax Credit
IN	Econ. Dev. for a Growing Economy
IN	Headquarters Relocation Tax Credit
IN	Hoosier Bus. Investment Tax Credit
IN	Incentive Payment
IN	Indiana Specific Insurance Related Education
IN	Industrial Dev. Project Guaranty Fund
IN	Skills Enhancement Fund
IN	Skills Enhancement Funds
IN	Technology Enhancement Certification for Hoosiers
IN	Twenty-First Century Research & Technology Fund
KS	Inv. in Major Proj. & Comp. Training Prog. (IMPACT)
KS	Job Creation Prog. Fund (JCF)
KY	Department of Commercialization & Innovation Awards
KY	Econ. Dev. Bonds
KY	Grant-in-Aid Prog.
KY	Incentives for Energy Independence Act
KY	Kentucky Bus. Investment Prog.
KY	Kentucky Econ. Dev. Finance Authority Direct Loan
KY	Kentucky Enterprise Initiative Act
KY	Kentucky Industrial Dev. Act
KY	Kentucky Industrial Revitalization Act
KY	Kentucky Jobs Dev. Act
KY	Kentucky Jobs Retention Act
KY	Kentucky Reinvestment Act
KY	Kentucky Rural Econ. Dev. Act
KY	Kentucky Small Bus. Investment Credit
KY	Kentucky Small Bus. Tax Credit
KY	Skills Investment Credit
KY	Training Tax Credit
LA	Enterprise Zone Prog.
LA	Industrial Tax Exemption
LA	LED FastStart
LA	Motion Picture Investor Tax Credit
LA	Quality Jobs Prog.
MA	Brownfields Tax Credit
MA	Econ. Dev. Incentive Prog.
MA	Historic rehabilitation tax credit
MA	Historic Rehabilitation Tax Credit
MA	Housing Dev. Tax Credit
MD	Biotechnology Commercialization Prog.
MD	Biotechnology Investor Incentive Tax Credit
MD	Cybersecurity Investor Incentive Tax Credit
MD	Enterprise Zone
MD	Export MD Grant
MD	Job Creation Tax Credit
MD	Maryland Industrial Training Prog.
MD	MEDAAF-1 Significant Strategic Econ. Dev. Opportunities
MD	MEDAAF-2 Local Econ. Dev. Opportunities
MD	One Maryland Tax Credit
MD	Partnership for Workforce Quality
MD	Small Bus. Dev. Financing Authority Contract Financing
IN	Community Revitalization Enhancement District Tax Credit
IN	Econ. Dev. for a Growing Economy
IN	Headquarters Relocation Tax Credit
IN	Hoosier Bus. Investment Tax Credit
IN	Incentive Payment
IN	Indiana Specific Insurance Related Education

MD	Small Bus. Dev. Financing Authority Guaranty Fund
MD	Sunny Day Fund
ME	Bus. Equipment Tax Reimbursement (BETR)
ME	Employment Tax Increment Financing
ME	Governor's Training Initiative
ME	Pine Tree Dev. Zones
MN	Fiscal Disparities Pool
MN	Job Opportunity Building Zones (JOBZ)
MN	Minnesota Investment Fund
MN	Special Incumbent Worker Training
MO	Build - Bus. Use Incentives for Lare Scale Dev.
MO	Bus. Facility Tax Credit
MO	Dev. Tax Credits
MO	Enhanced Enterprise Zone
MO	Film Production Company Tax Credit
MO	Missouri Automotive Manufacturing Jobs
MO	Missouri Works
MO	Quality Jobs Program
MO	Rebuilding Communities - 1.5% Employee
MO	Rebuilding Communities - 40% Equipment
MO	Wine & Grape Tax Credit
NE	LB 775/Employment & Investment Growth Act
NE	Nebraska Advantage Act
NE	Nebraska Advantage Rural Dev. Act
NM	Film Production Tax Credit
NM	Job Training Incentive Program
NV	Bus. Tax Abatement
NV	Catalyst Fund
NV	Personal Property Tax Abatement
NV	Real Property Tax Abatement
NV	Sales & Use Tax Abatement
NV	Train Employees Now
NY	Brownfield Cleanup Program Tax Credit
NY	Brownfield Redev. Credit
NY	Empire State Jobs Retention Program
NY	Empire Zones
NY	Environmental Remediated Insurance Credit
NY	Excelsior Jobs Program-Job Growth Track
NY	Film Tax Credit Program
NY	Job Dev. Authority Direct Loan Program
NY	Jobs Now
NY	Manufacturing Assistance Program
NY	Remediated Brownfield Credit for Real Property Taxes
NY	Start-up NY
NY	State Grant
OH	Bus. Dev. Grant
OH	Bus. Incentive Grant
OH	Community Reinvestment Areas
OH	Econ. Dev. Contingency Grant
OH	Energy Sector Training Grant
OH	Enterprise Zone Tax Credit Program
OH	Facilities Establishment Fund
OH	Incumbent Workforce Training Voucher
OH	Industrial Training Grant
OH	Innovation Ohio
OH	Investment in Training Expansion
OH	Job Creation Tax Credit
OH	Job Retention Tax Credit
OH	Logistics & Distribution Stimulus Program
OH	Minority Bus. Enterprise Loan
OH	Motion Picture Tax Credit
OH	Third Frontier
OH	Thomas Edison Program
OH	Workforce Dev. Initiatives
OH	Workforce Training Grant
OK	Community Econ. Dev. Pooled Finance Program
OK	Oklahoma Investment/New Jobs Credit
OK	Oklahoma Opportunity Fund
OK	Quality Jobs Incentive Payment
PA	Infrastructure Dev. Program Grant
PA	Job Creation Tax Credit
PA	Keystone Innovation Zone Tax Credit
PA	Machinery & Equipment Loan Fund
PA	Opportunity Grant Program
PA	Pennsylvania First - Grant
PA	Research & Dev. Tax Credit
PA	special state tax credits
SC	Econ. Dev. Set-Aside Program
SC	Enterprise Zone Job Dev. Credit
SC	Enterprise Zone Job Retraining Credit
SC	Film Production Incentives
SC	Film Productive Incentives
SC	Governor's Closing Fund
SC	infrastructure assistance
SC	readySC Training
SC	Rural Infrastructure Fund
TX	Tax Refund for Econ. Dev.
TX	Texas Enterprise Fund
VA	Aerospace Engine Manuf. Performance Grant Program
VA	Commonwealth's Dev. Opportunity Fund
VA	Governor's Opportunity Fund
VA	Enterprise Zone Real Property Investment Grant
VA	Enterprise Zone Real Property Investment Grant Program
VA	Major Eligible Employer Grant
VA	Special Performance Grants
VA	Virginia Investment Partnership
VA	VA Investment Partnership & Major Eligible Employer Grant
WI	Blight Elimination & Brownfield Redev. Program
WI	Brownfield Program
WI	Brownfield Redev. Financial Assistance
WI	Bus. Employees Skills Training (RED)
WI	Bus. Employees Skills Training (WDF)
WI	Bus. Expansion & Retention Investment
WI	Bus. Opportunity Loan Fund
WI	Buy Local, Buy Wisconsin Grants
WI	Community Dev. Zone
WI	Customized Labor Training
WI	Dairy Manufacturing Facility Investment Credit
WI	Dairy Manufacturing Facility Investment Tax Credit
WI	Dev. Opportunity Zone
WI	Econ. Dev. Program
WI	Econ. Dev. Tax Credit
WI	Econ. Dev. Tax Credit Program
WI	Enterprise Dev. Zone
WI	Enterprise Zone
WI	Enterprise Zone Jobs Tax Credit
WI	Film Production Company Investment Credit
WI	Film Production Services Credit
WI	Food Processing Plant & Food Warehouse Credit
WI	Food Processing Plant & Food Warehouse Investment Credit
WI	Historic Preservation Tax Credit
WI	Historic Preservation Tax Credit - Qualified Rehab
WI	Industrial Revenue Bond
WI	International Market Access Grant

WI Jobs Tax Credit
WI Major Economic Development
WI Meat Processing Facility Investment Credit
WI Qualified New Business Venture
WI Special Project Loan Fund
WI Technology Assistance Grant
WI Technology Bridge Grant
WI Technology Business Development Investments
WI Technology Development Fund
WI Technology Development Loan
WI Technology Matching Grant
WI Technology Venture Fund Loan
WI Technology Zone
WI Transportation Economic Assistance
WI Workforce Training Grants

Appendix C
Examples of State Subsidy Program Criteria

Examples of Employment Subsidies

Ex. 1: Wisconsin Jobs Tax Credit
Source: https://www.revenue.wi.gov/Pages/TaxPro/2010/jobtaxcr.pdf

"To qualify for the 2010 Wisconsin jobs tax credit, you must meet all of the following conditions:

- The claimant must be certified by the Wisconsin Department of Economic Development that the claimant is operating or intends to operate a business in Wisconsin and that a contract has been entered into with the Wisconsin Department of Economic Development.
- The claimant has received from the Wisconsin Department of Economic Development a notice of eligibility to receive tax benefits that reports the amount of tax benefit for which the claimant is eligible.

… The credit is based on the amount of wages paid to eligible employees in the taxable year, subject to a maximum amount of ten percent of such wages, and the costs incurred by the claimant to undertake training activities in the current year."

Ex. 2: Kansas Promoting Employment Across Kansas (PEAK)
Source: https://www.kansascommerce.gov/program/business-incentives-and-services/peak/

"PEAK is a business incentive where companies may retain or be refunded 95% of the payroll withholding tax of qualified employees for new jobs created in Kansas. Basic projects that create at least ten new jobs in metropolitan areas, or at least five new jobs in non-metropolitan areas, within two years may be eligible for up to seven years of payroll withholding tax savings. Larger, high-impact projects that create at least 100 new jobs within two years may be eligible for up to ten years of payroll withholding tax savings.

Who can qualify:
- Existing Kansas Businesses
- New Startups
- Out-of-State Businesses
- International Business
- Non-profit Headquarters Locations

…Qualified companies must meet program requirements, including:
- Be a for-profit entity with a qualifying NAICS code, or a not-for-profit entity headquartered in Kansas
- Be creating new jobs in Kansas resulting from locating, relocating or expanding a business

- Meet minimum employment requirements (at least ten new metro jobs or five new non-metro jobs)
- Must pay wages that meet or exceed the county's median wage
- Must provide adequate health care coverage for full-time employees and pay at least 50% of the health insurance premium
- Must submit an application and be approved for benefits by the Secretary of Commerce"

Examples of Investment Subsidies

Ex. 1: Arizona Government Property Lease Excise Tax (GPLET)
Source: https://www.azcommerce.com/incentives/lease-excise/

"The Government Property Lease Excise Tax ('GPLET') has been established by the State of Arizona and is a redevelopment tool to initiate development by reducing a project's operating costs by replacing the real property tax with an excise tax. Under the state statute an excise tax is established for the building type of use and is calculated on the gross square footage of the building. The use of the excise tax cannot continue for more than twenty-five years and requires that the land and improvements conveyed to a government entity and leased back for private use. The excise tax rate can be abated for the first eight years after a certificate of occupancy on the building is issued if the property is located within a Central Business District and a Redevelopment Area."

Ex. 2: Ohio Minority Business Enterprise Loan
Source: Ohio Revised Code Sec. 122.7(A), accessed at https://codes.ohio.gov/ohio-revised-code/section-122.76

"The director of development services, with controlling board approval, may lend funds to minority business enterprises and to community improvement corporations, Ohio development corporations, minority contractors business assistance organizations, and minority business supplier development councils for the purpose of loaning funds to minority business enterprises, for the purpose of procuring or improving real or personal property, or both, for the establishment, location, or expansion of industrial, distribution, commercial, or research facilities in the state, and for the purpose of contract financing, and to community development corporations that predominantly benefit minority business enterprises or are located in a census tract that has a population that is sixty per cent or more minority…"

Examples of Other Subsidies

Ex. 1: Indiana Twenty-First Century Research and Technology Fund
Source: Indiana Code Sec. 5-28-16-2, accessed at https://law.justia.com/codes/indiana/2017/title-5/article-28/chapter-16/section-5-28-16-2/
"The Indiana twenty-first century research and technology fund is established within the state treasury to provide grants or loans to support proposals for economic development in one (1) or more of the following areas:

(1) To increase the capacity of Indiana postsecondary educational institutions, Indiana businesses, and Indiana nonprofit corporations and organizations to compete successfully for federal or private research and development funding.
(2) To stimulate the transfer of research and technology into marketable products.
(3) To assist with diversifying Indiana's economy by focusing investment in biomedical research and biotechnology, information technology, development of alternative fuel technologies, development and production of fuel efficient vehicles, and other high technology industry clusters requiring high skill, high wage employees.
(4) To encourage an environment of innovation and cooperation among universities and businesses to promote research activity."

Ex. 2: Missouri Qualified Beef Tax Credit Program
Source: https://agriculture.mo.gov/abd/financial/qualifiedbeef.php

"The Missouri Agricultural and Small Business Development Authority (MASBDA) provides Missouri tax credits to Missouri's qualified beef producers who raise and background or finish Missouri born qualified beef animals to a weight of one hundred (100) pounds or more over their established baseline weight."

Examples of Subsidies Meeting More than One Criteria

Ex. 1: Missouri Business Use Incentives for Large-Scale Development (BUILD)
Categorized as: Employment and Investment
Source: https://ded.mo.gov/programs/business/BUILD

"To help companies embark on major investment and job creation expansions, provides low-interest loans to qualified borrowers through the issuance of tax-exempt revenue bonds for the acquisition, construction, and equipping of qualified manufacturing production facilities and/or equipment.

Eligibility requirements:

- An eligible industry in manufacturing, processing, assembly, research and development, agricultural processing or services in interstate commerce must invest a minimum of $15 million; or $10 million for an office industry(regional, national or international headquarters, telecommunications operations, computer operations, insurance companies or credit card billing and processing centers) in an economic development project; and
- Create a minimum of one hundred new jobs at the project facility within 3 years or a minimum of five hundred jobs if the project is an office industry or a minimum of two hundred new jobs if the project is an office industry located within a distressed community…"

Ex. 2: New York Manufacturing Assistance Program
Categorized as: Employment and Investment
Source: https://www1.nyc.gov/nycbusiness/description/manufacturing-assistance-program

“The Manufacturing Assistance Program provides monetary grants for manufacturing businesses. Grants are for projects to improve productivity or competitiveness. Grants are up to $1 million. Businesses must be in New York State, employ 50-1,000 workers, and export at least 30% of their production outside of New York State. The grant is for a project. The project must include:

- an investment of at least $1 million;
- major improvements to production; and
- retention of 85% of workers for five years.

The project can be for increased or more environmentally friendly production, new equipment, training, or other costs.”

Appendix D
State Disclosure Regimes

The table below provides details of the disclosure regimes identified for each state. Sources include *The State of State Disclosure: An Evaluation of Online Public Information About Economic Development Subsidies, Procurement Contracts, and Lobbying Activities* (Good Jobs First, 2007); *Show Us the Subsidies* (Good Jobs First, 2010); *Show Us the Subsidized Jobs* (Good Jobs First, 2014); and *How States Are Improving Tax Incentives for Jobs and Growth* (Pew Research, 2017).

State	**Year**	**Details about Required Disclosures**
Alabama	2016	Classified based on Pew Research Study
Arizona	2011	Arizona Competes Fund created with transparency provisions
California	2011; 2013	CA legislature gives final approval to a bill that would have required creation of a public database containing names of public firms claiming tax credits; vetoed by Schwarzenegger; In 2011, extends Film Production Tax Credit and requires Film Commission to make public the recipients of that credit in Assembly Bill 1069. In July 2013, Jerry Brown signs Assembly Bill 93, overhauling the Enterprise Zone hiring credit and requiring online transparency and improved accountability standards for that and newly enacted business subsidies.
Connecticut	2007; 2013	CT Gov. Malloy issues executive order to make economic development subsidy data more accessible to the public. Soon thereafter the state posts an interactive map of economic development deals with downloadable information.
Florida	2013	
Georgia	None found	
Illinois	Prior to 2007	Classified has having disclosure based on Good Jobs First 2007 Report
Indiana	Prior to 2007	Classified has having disclosure based on Good Jobs First 2007 Report
Kansas	None found	
Kentucky	2007; 2008	Classified has having disclosure based on Good Jobs First 2007 Report; expanded in 2008 when Cabinet for Economic Development creates Financial Incentives Project Database

Louisiana	2010; 2013	Louisiana begins posting Board of Commerce & Industry Incentive Approvals for several major subsidy programs; in 2013, starts posting online recipients of the state's film and digital media subsidies.
Maine	Prior to 2007; 2010	Classified has having disclosure based on Good Jobs First 2007 Report
Maryland	2007; 2011	Classified has having disclosure based on Good Jobs First 2007 Report; in 2011, Finance Tracker Database released
Massachusetts	2010	In 2010, state passed law that requires disclosure of names of recipients of refundable, salable, and transferable tax credits starting in 2011; Releases first report in 2013 that includes company-specific disclosure for subsidies such as its broadly used Economic Development Incentive Program
Minnesota	Prior to 2007; 2013	Classified has having disclosure based on Good Jobs First 2007 Report
Missouri	Prior to 2007	Classified has having disclosure based on Good Jobs First 2007 Report
Nebraska	Prior to 2007	Classified has having disclosure based on Good Jobs First 2007 Report
Nevada	Prior to 2007	Classified has having disclosure based on Good Jobs First 2007 Report
New Mexico	None found	
New York	2007; 2010; 2013; 2012	In 2010, Governor signs law that creates Excelsior Jobs Program and mandates disclosure of recipients; in 2012, New York State's Empire State Development begins posting online quarterly reports on the Excelsior Jobs Program, in compliance with program's transparency requirements. In 2013, NY Governor announced Start-up NY Program, creating tax-free zones in which participating companies pay no tax for 10 years. Empire State Development is required by the program to publish an annual report beginning in Dec. 2014 listing names and addresses of businesses in the zones and # of jobs created
Ohio	Prior to 2007; 2008; 2009	Classified has having disclosure based on Good Jobs First 2007 Report. In 2008; Passes bill for creation of public database that discloses the names of economic development grants and

		loan recipients; in 2009 House Bill passed that mandates the creation of a public database with the names of the recipients of economic development tax credits.
Oklahoma	2007; 2008; 2009	Taxpayer Transparency Act passed in 2007; launched site in 2008; Tax credit information added to Open Books fiscal transparency site in 2009
Pennsylvania	Prior to 2007; 2008	Classified has having disclosure based on Good Jobs First 2007 Report
South Carolina	None found	
Texas	Prior to 2007; 2013	Classified has having disclosure based on Good Jobs First 2007 Report
Virginia	None found	
Wisconsin	Prior to 2007; 2011	Classified has having disclosure based on Good Jobs First 2007 Report; later regime required disclosure of all subsidy recipients receiving $100,000 or more

Appendix E

Online Disclosure Example

Figure E1 provides an example of subsidies that qualify as having "online job disclosure". The specific example in question comes from the Florida Department of Economic Opportunity (DEO)'s 2013 annual report, which is posted online at the DEO's website at the conclusion of each year.[20] The annual report contains subsidy-level information for subsidies agreed upon by Florida's state-level subsidy programs, housed under the DEO, during the year. Information includes the recipient firm, the maximum potential dollar value of the subsidy awarded, the county of the recipient firm, and the state program (e.g., Quick Action Closing Fund – QACF in the example below).

The column labeled "Contracted New Jobs" reflects the number of jobs Florida expects the recipient company to create. The existence of such data for a given subsidy program would lead us to label that program as providing online disclosures of job commitments.

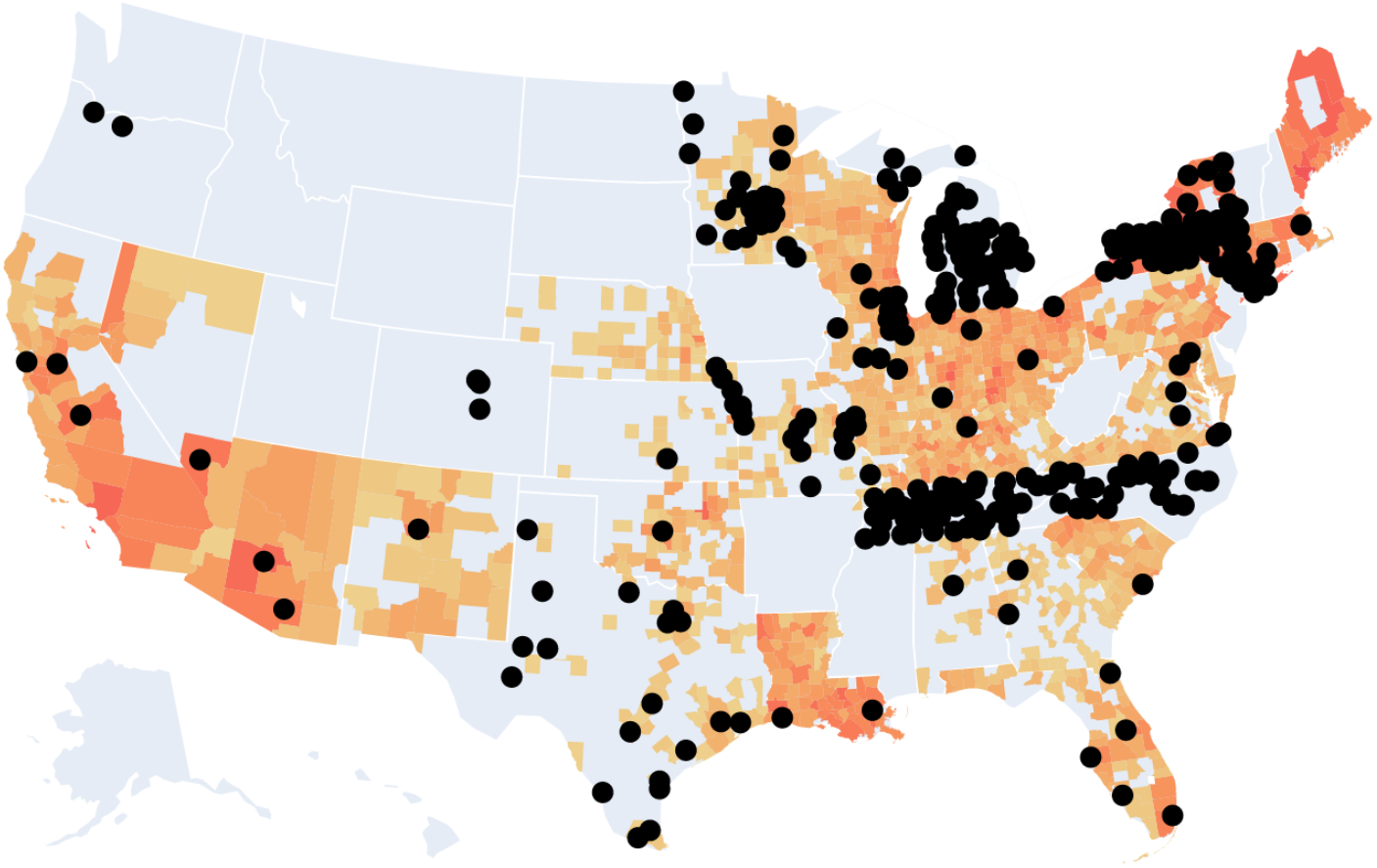
Figure E1: Florida DEO Annual Report Example

Table 33
INCENTIVE AGREEMENTS EXECUTED – FY 2013

Company	County	Industry	Contracted New Jobs	Expected Capital Investment	State Program	Maximum State Incentive Payment	Required Local Financial Support	Contracted Annual Average Wage
1Brick, LLC	Pasco	Financial/Prof Svcs	200	$210,000	QTI	$480,000	$120,000	$36,319
Absolute Consulting, Inc	Santa Rosa	Financial/Prof Svcs	30	$1,310,000	QTI	$96,000	$24,000	$60,000
Accuform Manufacturing Inc.	Hernando	Emerging Tech	271	$4,925,000	QTI	$650,400	$162,600	$28,984
Accurate Information Systems	Sarasota	Financial/Prof Svcs	30	$120,000	QTI	$72,000	$18,000	$41,711
Actavis, Inc.	Broward	Life Sciences	220	$40,500,000	QACF	$690,000	$0	$53,994
ADS Waste Holdings, Inc.	St. Johns	Corporate HQ	85	$8,200,000	QACF	$200,000	$0	$112,000
ADS Waste Holdings, Inc.	St. Johns	Corporate HQ	85	$8,200,000	QTI	$476,000	$119,000	$81,110
ADT LLC	Palm Beach	Corporate HQ	120	$400,000	QACF	$400,000	$0	$81,110
ADT LLC	Palm Beach	Corporate HQ	120	$400,000	QTI	$672,000	$168,000	$81,110
Advanced Sawmill Machinery	Okaloosa	Emerging Tech	15	$1,275,612	QTI	$36,000	$9,000	$50,750
Amtech LLC	Bay	Emerging Tech	90	$895,000	QTI	$216,000	$54,000	$34,000
Ansafone Contact Centers LLC	Marion	Not in Target Sector	300	$2,913,000	BFR	$600,000	$0	$21,800
Embraer Eng.& Tech Center USA, Inc.	Brevard	Engineering Services	200	$24,520,000	IIF	$6,000,000	$6,000,000	$70,000

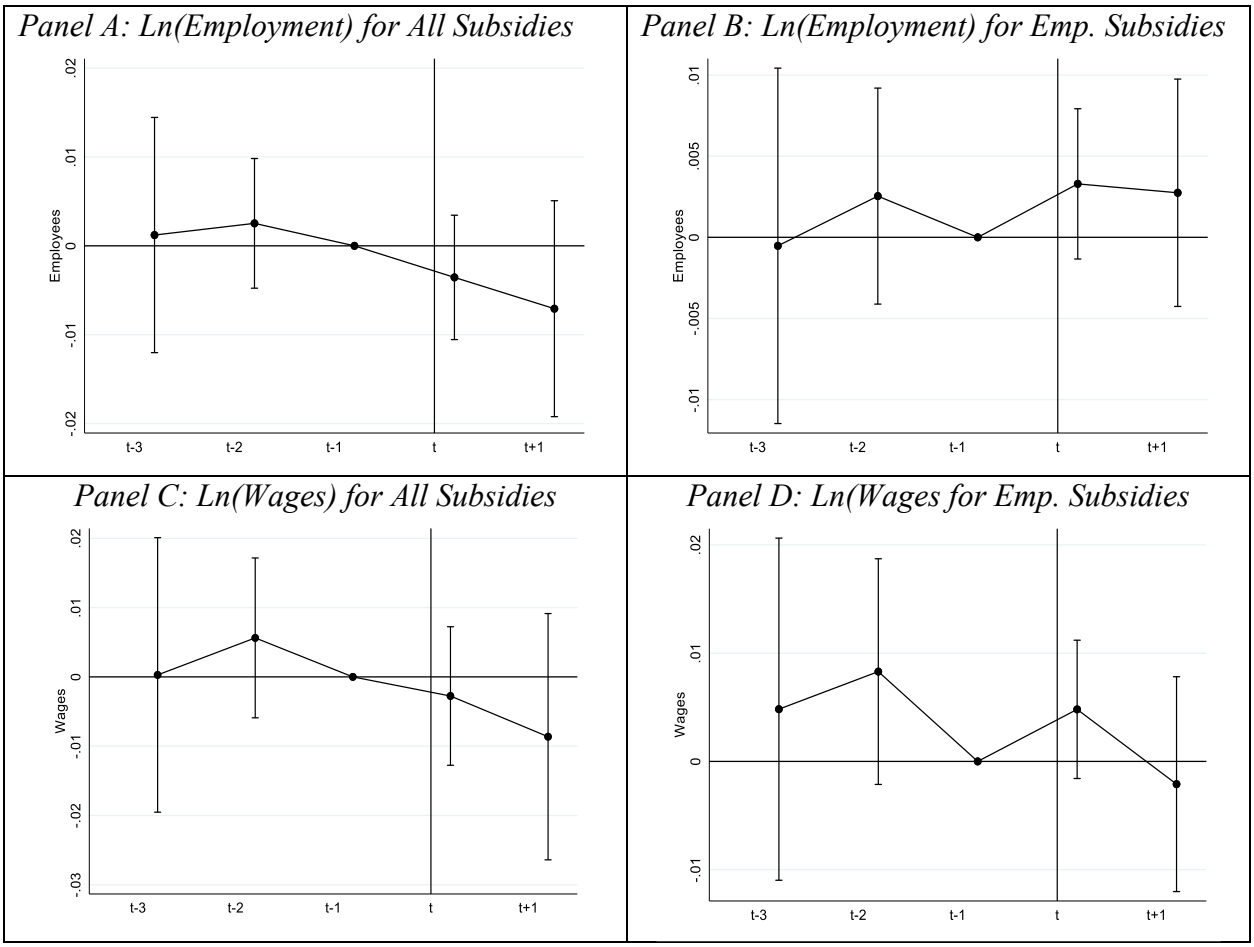
[20] http://www.floridajobs.org/business/EDP/EconomicDevelopmentIncentivesReport.pdf

Figure 1
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 shading shows the natural logarithm of total state subsidies, where lighter (darker) colors reflect fewer (more) subsidies. The dots indicate the counties with local subsidies separately tested in the Online Appendix.

Figure 2
Pre-Subsidy Trends



The figures present trends for the relation between *PostSubsidy* and the log of the number of total employees (Panel A and B) and the log of total wages (Panel C and D) at the county-level. Panels A and C present trends for the sample of all subsidies; Panels B and D present trends for the sample of employment-specific subsidies.

Table 1
Sample Selection for State Subsidy Sample

Panel A: Data requirements for state subsidy sample

	County-Years		Distinct Counties	
	Obs. Dropped	Obs. Remaining	Obs. Dropped	Obs. Remaining
Initial sample for 2008-2015		25,216		3,152
Less: Counties in 23 states with insufficient state subsidy coverage	(8,568)	16,648	(1,071)	2,081
Less: Counties that never receive a subsidy or have a megadeal subsidy not studied in this manuscript	(7,240)	9,408	(905)	1,176
Less: Counties without requisite BLS employment/wages data	(88)	9,320	(11)	1,165
Total state subsidy sample		**9,320**		**1,165**

Panel B: County-year observations of state subsidy sample by state

State	# County-Years	State	# County-Years
Alabama	96	Minnesota	256
Arizona	112	Missouri	384
California	336	Nebraska	384
Connecticut	40	Nevada	56
Florida	336	New Mexico	168
Georgia	512	New York	416
Illinois	592	Ohio	632
Indiana	672	Oklahoma	304
Kansas	128	Pennsylvania	384
Kentucky	776	South Carolina	296
Louisiana	400	Texas	408
Maine	120	Virginia	752
Maryland	176	Wisconsin	504
Massachusetts	80	**Total**	**9,320**

This table presents sample selection details and descriptive statistics for the state subsidy sample. Panel A outlines the sample selection process. We construct a balanced panel of county-year observations from 2008 to 2015 with requisite data for the empirical tests. The sample is composed of 9,320 county-years representing 1,165 distinct counties with firms that receive at least one subsidy during the sample period. Panel B presents the distribution of county-year observations (n=9,320) across 27 states. We exclude data from the remaining 23 states not shown above because of insufficient data coverage in Subsidy Tracker. We define all variables in Appendix A.

Table 2
Descriptive Statistics for State Subsidy Sample

Panel A: State subsidy sample descriptive statistics

Variables	Mean	Median	Std. Dev.	P25	P75
Establishments$_{i,t+1}$	3,552.44	970.00	9,187.77	463.00	2,627.00
Ln(Establishments)$_{i,t+1}$	7.08	6.88	1.32	6.14	7.87
Employees$_{i,t+1}$	47,233.81	12,162.50	128,332.90	5,386.00	35,202.50
Ln(Employees)$_{i,t+1}$	9.56	9.41	1.44	8.59	10.47
Wages$_{i,t+1}$ ($M)	2,245.18	421.00	7,672.94	172.90	1,300.66
Ln(Wages)$_{i,t+1}$	20.05	19.86	1.56	18.97	20.99
PostFirstSubsidy$_{i,t}$	0.86	1.00	0.35	1.00	1.00
Ln(#FirstSubsidies)$_{i,t}$	1.14	0.69	0.94	0.69	1.61
Ln(#SubsequentSubsidies)$_{i,t}$	1.32	0.69	1.51	0.00	2.20
PostFirstEmploymentSubsidy$_{i,t}$	0.74	1.00	0.44	0.00	1.00
Ln(#FirstEmploymentSubsidies)$_{i,t}$	0.94	0.69	0.94	0.00	1.39
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$	1.05	0.00	1.40	0.00	1.79
PostFirstInvestmentSubsidy$_{i,t}$	0.64	1.00	0.48	0.00	1.00
Ln(#FirstInvestmentSubsidies)$_{i,t}$	0.79	0.69	0.95	0.00	1.10
Ln(#SubsequentInvestmentSubsidies)$_{i,t}$	0.80	0.00	1.32	0.00	1.39
PostFirstNonEmploymentSubsidy$_{i,t}$	0.45	0.00	0.50	0.00	1.00
PostFirstNonInvestmentSubsidy$_{i,t}$	0.53	1.00	0.50	0.00	1.00
DisclosureRegime$_{i,t}$	0.59	1.00	0.49	0.00	1.00
Online$_{i,t}$	0.78	1.00	0.41	1.00	1.00
OnlineJobs$_{i,t}$	0.26	0.00	0.44	0.00	1.00
Control Variables					
Ln(Population.)$_{j,t}$	10.24	10.05	1.25	9.40	11.00
%Educ$_{j,t}$	20.68	18.40	9.64	13.80	25.40

Panel B: Types of subsidies in state subsidy sample

	First Subsidy County-Years by Type (n=1,165)				All Subsidy County-Years by Type (n=5,311 out of 9,320 total county-years)			
	# Subs.	% of Total Subs.	# Subs. w. $Value	Total $Value of Subs. ($M)	# Subs.	% of Total Subs.	# Subs. w. $Value	Total $Value of Subs. ($M)
Tax credit	1,155	16.51%	933	768.10	9,831	19.93%	8,822	6,695.90
Tax abatement	308	4.40%	217	223.54	6,839	13.86%	6,748	1,741.00
Reimbursement	810	11.58%	711	77.39	5,137	10.41%	4,499	512.37
Enterprise zone	3,727	53.27%	3,428	558.04	21,532	43.64%	20,571	3,620.39
Grant	914	13.06%	899	454.69	5,538	11.23%	5,499	2,392.17
Financing	82	1.17%	0	0	458	0.93%	0	0
TOTAL	6,996	100.00%	6,188	2,081.76	49,335	100.00%	46,139	14,961.83

Table 2, continued
Descriptive Statistics for State Subsidy Sample

Panel C: Distribution of subsidy-years and subsidies in state subsidy sample by year

Year	First Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years	All Subsidy County-Years
	Subsidy-Years	Subsidies	Subsidy-Years	Subsidies
2008	689	6,143	689	6,143
2009	159	357	669	5,888
2010	86	117	636	6,171
2011	80	115	638	6,231
2012	61	84	705	6,610
2013	36	59	679	6,686
2014	32	48	667	6,068
2015	22	73	628	5,538
Total	**1,165**	**6,996**	**5,311**	**49,335**

Panel D: Distribution of first subsidy-years in state subsidy sample by stated criteria

Year	Employment Subsidies			Non-Employment Subsidies		
	First Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years	First Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years
	Subsidy-Years	Subsidies	Subsidies	Subsidy-Years	Subsidies	Subsidies
2008	555	5,366	5,366	777	777	777
2009	149	229	5,041	260	847	260
2010	86	116	4,662	922	1,509	922
2011	73	89	4,384	216	1,847	216
2012	70	88	4,618	73	1,992	73
2013	34	49	4,813	62	1,873	62
2014	46	81	4,182	34	1,886	34
2015	39	93	3,718	23	1,820	23
Total	**1,052**	**6,111**	**36,784**	**2,367**	**12,551**	**2,367**

Panel E: Multiple Subsidies

			% of Subsequent Subsidies	
Types of Subsidies	First-time Subsidies	Subsequent Subsidies	Awarded to Same Company	Awarded from Same Program
All	6,996	42,339	25.3%	68.0%
Employment	6,111	30,673	17.5%	81.7%

This table describes the county-year observations and subsidies for the state subsidy sample. Panel A presents descriptive statistics for the dependent variables, variables of interest, and control variables for the sample of 9,320 county-year observations. Panel B shows the number and percentage of observations by subsidy type, the number of observations reporting the dollar value of the tax subsidy and, for those observations, the total magnitude of subsidies. *Tax Credits* are dollar value awards that reduce a firm's tax liability dollar-for-dollar. *Tax Abatements* provide a percentage reduction of a firm's tax liability. *Reimbursements* are *ex post* cash transfers for expenses incurred by a company, such as job training activities. *Enterprise Zones* are tax credits or abatements tied to a company's decision

to locate to a particular neighborhood. *Grants* are up front direct cash transfers to the firm from the government. *TIF*, which stands for *Tax Increment Financing*, 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. Panel C and D present the total number of subsidies by year, where Panel D presents the number of subsidies based on whether the subsidy program is intended to result in increased employment or if it targets other outcomes. Panel E provides descriptive statistics about the number and proportion of subsequent subsidies. We define all variables in Appendix A.

Table 3
The Relation between Subsidies and Local Activity

Dependent Var:	***Ln(Employees)$_{i,t+1}$***			***Ln(Wages)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
PostFirstSubsidy$_{i,t}$	**-0.0019**			**0.0004**		
	[-0.47]			**[0.08]**		
Ln(#FirstSubsidies)$_{i,t}$		**-0.044**	**-0.0010**		**-0.0042**	**-0.0016**
		[-1.15]	**[-0.26]**		**[-0.76]**	**[-0.28]**
Ln(#SubsequentSubsidies)$_{i,t}$			**0.0076*** **			**0.0057** **
			[3.74]			**[2.03]**
Ln(Population)$_{i,t}$	0.5507***	0.5484***	0.5127***	0.5009***	0.4964***	0.4694***
	[8.09]	[8.06]	[7.44]	[5.70]	[5.66]	[5.26]
%Educ$_{j,t}$	0.0004	0.0004	0.0004	0.0009**	0.0009**	0.0008**
	[1.46]	[1.45]	[1.22]	[2.21]	[2.20]	[2.07]
County FE?	Y	Y	Y	Y	Y	Y
State-by-Year FE?	Y	Y	Y	Y	Y	Y
Observations	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999	0.998	0.999	0.999

This panel presents results of testing the relation between subsidies and the log of the number of employees (Columns (1)-(3)) and the log of aggregate wages (Columns (4)-(6)) for the 9,320 county-year observations in the state subsidy sample. *PostSubsidy$_{i,t}$* is an indicator equal to one for years including and following the first-observed subsidy received by a firm in county *i* and zero otherwise. *Ln(#FirstSubsidies)$_{i,t}$* is the log of total number of subsidies received by firms in the first year a county has an observed subsidy. *Ln(#SubsequentSubsidies)$_{i,t}$* is the log of the cumulative number of subsequent subsidies for year *t* in county *i*. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-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

The Relation between Subsidies and Local Activity by Stated Criteria

Dependent Var:	***Ln(Employees)$_{i,t+1}$***			***Ln(Wages)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
PostFirstEmploymentSubsidy$_{i,t}$	**0.0031**			**-0.0001**		
	[0.81]			**[-0.01]**		
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**0.0049**	**0.0098****		**0.0001**	**0.0044**
		[1.19]	**[2.39]**		**[0.02]**	**[0.75]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$			**0.0128*****			**0.0111*****
			[5.66]			**[3.50]**
PostFirstNonEmploymentSubsidy$_{i,t}$	-0.0012	-0.0012	-0.0015	0.0022	0.0022	0.0020
	[-0.29]	[-0.29]	[-0.36]	[0.39]	[0.39]	[0.35]
Ln(Population)$_{i,t}$	0.5545***	0.5544***	0.4915***	0.5011***	0.5012***	0.4465***
	[8.13]	[8.13]	[7.21]	[5.71]	[5.71]	[5.04]
%Educ$_{j,t}$	0.0004	0.0004	0.0003	0.0009**	0.0009**	0.0008**
	[1.46]	[1.47]	[1.13]	[2.21]	[2.21]	[1.98]
County FE?	Y	Y	Y	Y	Y	Y
State-by-Year FE?	Y	Y	Y	Y	Y	Y
Observations	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999	0.998	0.998	0.999

This panel presents results of testing the relation between employment subsidies and the log of the number of employees (Columns (1)-(3)) and the log of aggregate wages (Column (4)-(6)) for the 9,320 county-year observations in the state subsidy sample. *PostFirstEmploymentSubsidy$_{i,t}$* is an indicator equal to one for years including and following the first-observed subsidy targeting increased employment received by a firm in county *i* and zero otherwise. *Ln(#FirstEmploymentSubsidies)$_{i,t}$* is the log of total number of employment subsidies received by firms in the first year a county has an observed subsidy. *Ln(#SubsequentEmploymentSubsidies)$_{i,t}$* is the log of the cumulative number of subsequent subsidies targeting increased employment in county *i* and year *t*. *PostFirstNonEmploymentSubsidy$_{i,t}$* is an indicator equal to one for years including and following the first-observed subsidy not targeting increased employment received by a firm in county *i* and zero otherwise. Subsidies are classified on the basis of having employment criteria observed from manual reviews of each subsidy program. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-year and county fixed effects, and we cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5

The Role of the Subsidy Information Environment in Subsidy Effectiveness

Panel A: Disclosure Regimes

Dependent Variable:	***Ln(Employees)$_{i,t+1}$***			***Ln(Wages)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
PostFirstEmploymentSubsidy$_{i,t}$	**0.0124***			**0.0095**		
	[1.89]			**[1.19]**		
PostFirstEmploymentSubsidy$_{i,t}$*DisclosureRegime$_{i,t}$	**-0.0149***			**-0.0153**		
	[-1.86]			**[-1.46]**		
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**0.0150****	**0.0179****		**0.0107**	**0.0132**
		[2.05]	**[2.47]**		**[1.27]**	**[1.57]**
Ln(#FirstEmploymentSubsidies)$_{i,t}$*DisclosureRegime$_{i,t}$		**-0.0156***	**-0.0125**		**-0.0163**	**-0.0137**
		[-1.77]	**[-1.44]**		**[-1.45]**	**[-1.23]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$			**0.0126*****			**0.0109*****
			[5.59]			**[3.44]**
PostFirstNonEmploymentSubsidy$_{i,t}$	-0.0011	-0.0011	-0.0014	0.0023	0.0023	0.0020
	[-0.27]	[-0.27]	[-0.34]	[0.41]	[0.41]	[0.36]
Ln(Population)$_{i,t}$	0.5534***	0.5550***	0.4929***	0.4999***	0.5018***	0.4480***
	[8.11]	[8.15]	[7.23]	[5.70]	[5.72]	[5.06]
%Educ$_{j,t}$	0.0004	0.0004	0.0003	0.0008**	0.0008**	0.0008**
	[1.44]	[1.44]	[1.11]	[2.20]	[2.19]	[1.97]
F Test Statistic	0.28	0.02	1.19	0.73	0.57	0.00
County FE?	Y	Y	Y	Y	Y	Y
State-by-Year FE?	Y	Y	Y	Y	Y	Y
Observations	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999	0.999	0.999	0.999

This panel presents results of testing the role of disclosure regimes in the relation between employment subsidies and the log of the number of employees (Columns (1)-(3)) and the log of aggregate wages (Column (4)-(6)) for the 9,320 county-year observations in the state subsidy sample. *DisclosureRegime$_{i,t}$* is an indicator equal to one if the state had a subsidy disclosure initiative at the time the first company in a county received a subsidy and zero otherwise. The F Test statistic in Columns (1) and (4) (Columns (2)-(3) and (5)-(6)) evaluates whether the sum of the coefficients on *PostFirstEmploymentSubsidy$_{i,t}$* and *PostFirstEmploymentSubsidy$_{i,t}$*DislosureRegime$_{i,t}$* (*Ln(#FirstEmploymentSubsidies)$_{i,t}$* and *Ln(#FirstEmploymentSubsidies$_{i,t}$*DislosureRegime$_{i,t}$*) is statistically distinguishable from zero. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-year and county fixed effects, and we cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5

The Role of the Subsidy Information Environment in Subsidy Effectiveness

Panel B: Online Subsidy Disclosure

Dependent Variable:	***Ln(Employees)$_{i,t+1}$***			***Ln(Wages)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
PostFirstEmploymentSubsidy$_{i,t}$	**0.0291****			**0.0268**		
	[2.05]			**[1.57]**		
PostFirstEmploymentSubsidy$_{i,t}$*Online$_{i,t}$	**-0.0298****			**-0.0307***		
	[-2.01]			**[-1.73]**		
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**0.0300****	**0.0332****		**0.0250**	**0.0279**
		[1.99]	**[2.23]**		**[1.39]**	**[1.56]**
Ln(#FirstEmploymentSubsidies)$_{i,t}$*Online$_{i,t}$		**-0.0281***	**-0.0263***		**-0.0279**	**-0.0264**
		[-1.78]	**[-1.68]**		**[-1.48]**	**[-1.41]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$			**0.0126*****			**0.0110*****
			[5.62]			**[3.46]**
PostFirstNonEmploymentSubsidy$_{i,t}$	-0.0010	-0.0011	-0.0014	0.0024	0.0023	0.0021
	[-0.24]	[-0.25]	[-0.33]	[0.43]	[0.42]	[0.37]
Ln(Population)$_{i,t}$	0.5496***	0.5510***	0.4889***	0.4960***	0.4978***	0.4439***
	[8.03]	[8.06]	[7.15]	[5.63]	[5.65]	[5.00]
%Educ$_{j,t}$	0.0004	0.0004	0.0003	0.0008**	0.0008**	0.0008*
	[1.38]	[1.40]	[1.07]	[2.15]	[2.17]	[1.94]
F Test Statistic	0.03	0.19	2.60	0.54	0.24	0.06
County FE?	Y	Y	Y	Y	Y	Y
State-by-Year FE?	Y	Y	Y	Y	Y	Y
Observations	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999	0.999	0.998	0.999

This panel presents results of testing the role of online disclosure in the relation between employment subsidies and the log of the number of employees (Columns (1)-(3)) and the log of aggregate wages (Column (4)-(6)) for the 9,320 county-year observations in the state subsidy sample. *Online$_{i,t}$* is an indicator equal to one for county-years for which there is subsidy information disclosed online about the first-observed subsidies and zero otherwise. The F Test statistic in Columns (1) and (4) (Columns (2)-(3) and (5)-(6)) evaluates whether the sum of the coefficients on *PostFirstEmploymentSubsidy$_{i,t}$* and *PostFirstEmploymentSubsidy$_{i,t}$*Online$_{i,t}$* (*Ln(#FirstEmploymentSubsidies)$_{i,t}$* and *Ln(#FirstEmploymentSubsidies)$_{i,t}$*Online$_{i,t}$*) is statistically distinguishable from zero. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-year and county fixed effects, and we cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5

The Role of the Subsidy Information Environment in Subsidy Effectiveness

Panel C: Online and Job Commitment Disclosure

Dependent Var:	***Ln(Employees)$_{i,t+1}$***			***Ln(Wages)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
PostFirstEmploymentSubsidy$_{i,t}$	**0.0277***			**0.0249**		
	[1.96]			**[1.45]**		
PostFirstEmploymentSubsidy$_{i,t}$*Online$_{i,t}$	**-0.0389****			**-0.0434****		
	[-2.55]			**[-2.32]**		
PostFirstEmploymentSubsidy$_{i,t}$*Online+Jobs$_{i,t}$	**0.0181****			**0.0251****		
	[2.49]			**[2.33]**		
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**0.0287***	**0.0321****		**0.0232**	**0.0261**
		[1.91]	**[2.16]**		**[1.29]**	**[1.46]**
Ln(#FirstEmploymentSubsidies)$_{i,t}$*Online$_{i,t}$		**-0.0371****	**-0.0340****		**-0.0414****	**-0.0387***
		[-2.29]	**[-2.11]**		**[-2.06]**	**[-1.93]**
Ln(#FirstEmploymentSubsidies)$_{i,t}$*Online+Jobs$_{i,t}$		**0.0188****	**0.0159****		**0.0278****	**0.0254****
		[2.31]	**[1.98]**		**[2.34]**	**[2.13]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$			**0.0124*****			**0.0106*****
			[5.50]			**[3.33]**
PostFirstNonEmploymentSubsidy$_{i,t}$	-0.0007	-0.0007	-0.0010	0.0028	0.0029	0.0026
	[-0.17]	[-0.17]	[-0.25]	[0.51]	[0.52]	[0.47]
Ln(Population)$_{i,t}$	0.5494***	0.5520***	0.4910***	0.4957***	0.4993***	0.4473***
	[8.06]	[8.09]	[7.19]	[5.65]	[5.68]	[5.05]
%Educ$_{j,t}$	0.0004	0.0004	0.0003	0.0008**	0.0008**	0.0008**
	[1.43]	[1.43]	[1.10]	[2.20]	[2.19]	[1.97]
County FE?	Y	Y	Y	Y	Y	Y
State-by-Year FE?	Y	Y	Y	Y	Y	Y
Observations	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999	0.999	0.999	0.999

This panel presents results of testing the role of the subsidy information environment in the relation between employment subsidies and the log of the number of employees (Columns (1)-(3)) and the log of aggregate wages (Column (4)-(6)) for the 9,320 county-year observations in the state subsidy sample. *Online$_{i,t}$* is an indicator equal to one for county-years with first-observed subsidies for which there is subsidy information disclosed online and zero otherwise. *Online+Jobs$_{i,t}$* is an indicator equal to one for county-years with first-observed subsidies for which, conditional on having subsidy information disclosed online, there are also online disclosures about expected job creation and zero otherwise. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-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: Descriptive Statistics on Investment Subsidies

Year	Investment Subsidies		Non-Investment Subsidies	
	First Subsidy County-Years	All Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years
2008	428	428	393	393
2009	147	412	95	382
2010	90	390	74	406
2011	86	417	49	387
2012	65	478	38	394
2013	46	462	22	404
2014	43	466	15	352
2015	23	423	22	358
Total	**928**	**3,476**	**708**	**3,076**

Panel B: Regression Analyses of Investment Subsidies

Dependent Var:	***Ln(Establishments)$_{i,t+1}$***		
	(1)	**(2)**	**(3)**
PostFirstInvestmentSubsidy$_{i,t}$	-0.0052		
	[-0.88]		
PostFirstInvestmentSubsidy$_{i,t}$*Online$_{i,t}$	0.0043		
	[0.59]		
PostFirstInvestmentSubsidy$_{i,t}$*Online+Jobs$_{i,t}$	0.0031		
	[0.54]		
Ln(#FirstInvestmentSubsidies)$_{i,t}$		-0.0098	-0.0098
		[-1.37]	[-1.36]
Ln(#FirstInvestmentSubsidies)$_{i,t}$*Online$_{i,t}$		0.0104	0.0105
		[1.19]	[1.19]
Ln(#FirstInvestmentSubsidies)$_{i,t}$*Online+Jobs$_{i,t}$		0.0013	0.0019
		[0.19]	[0.28]
Ln(#SubsequentInvestmentSubsidies)$_{i,t}$			0.0022
			[1.10]
PostFirstNonInvestmentSubsidy$_{i,t}$	-0.0038	-0.0035	-0.0032
	[-1.42]	[-1.37]	[-1.26]
Ln(Population)$_{i,t}$	0.4421***	0.4417***	0.4326***
	[8.22]	[8.23]	[7.71]
%Educ$_{j,t}$	0.0003*	0.0003*	0.0003*
	[1.87]	[1.87]	[1.84]
County FE?	Y	Y	Y
State-by-Year FE?	Y	Y	Y
Observations	9,320	9,320	9,320
Adj. R-squared	0.999	0.999	0.999

This table reports results of tests examining the role of the subsidy information environment in the relation between investment subsidies and the number of establishments. Panel A presents descriptive statistics on the number of subsidies by targeted outcome by year. Panel B presents results of testing the role of the subsidy information environment in the relation between investment subsidies and the log of the number of establishments for the 9,320 county-year observations in the state subsidy sample. *Online$_{i,t}$* is an indicator equal to one for county-years with first-observed subsidies for which there is subsidy information disclosed online and zero otherwise. *Online+Jobs$_{i,t}$* is an indicator equal to one for county-years with first-observed subsidies for which, conditional on having subsidy information disclosed online, there are also online disclosures about expected job creation and zero otherwise. We define all other variables in Appendix A and present t-statistics in brackets. Each specification includes state-by-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

Not intended for print publication

Online Appendix Table 1
Local Subsidy Sample Selection and Descriptive Statistics

Panel A: Data requirements for local subsidy sample

	County-Years		Distinct Counties	
	Obs. Dropped	Obs. Remaining	Obs. Dropped	Obs. Remaining
Initial sample for 2008-2015		25,216		3,152
Less: Counties with insufficient local subsidy coverage or with megadeals	(23,216)	2,000	(2,902)	283
Singleton counties dropped due to use of state-by-year fixed effects	(56)	1,944	(7)	243
Final local subsidy sample		**1,944**		**243**

Panel B: County-year observations of local subsidy sample by state

State	# County-Years
Arizona	16
California	24
Colorado	24
Florida	24
Georgia	16
Illinois	104
Indiana	16
Michigan	352
Minnesota	176
Missouri	112
New York	384
North Carolina	168
Oregon	16
Tennessee	360
Texas	120
Virginia	32
Total	**1,944**

This table presents sample selection details and descriptive statistics for the local subsidy sample. Panel A outlines the sample selection process. We construct a balanced panel of county-year observations from 2008 to 2015. The sample is composed of 1,944 county-years representing 243 distinct counties with firms that receive at least one subsidy during the sample period. Panel B presents the distribution of county-year observations across 16 states. We retain localities if they had three or more years of observed subsidies within the eight year sample window and did not have substantial changes in the number of subsidy observations across the period which would imply erroneous data. We define all variables in Appendix A.

Online Appendix Table 1 (cont'd.)
Local Subsidy Sample Selection and Descriptive Statistics

Panel C: Local subsidy sample descriptive statistics

Variables	Mean	Median	Std. Dev.	P25	P75
$Establishments_{i,t+1}$	5,620.58	1,314.00	12,752.97	685.00	3,964.00
$Ln(Establishments)_{i,t+1}$	7.45	7.18	1.45	6.53	8.29
$Employees_{i,t+1}$	82,001.59	17,002.50	204,675.40	8,246.00	60,433.00
$Ln(Employees)_{i,t+1}$	9.99	9.74	1.57	9.02	11.01
$Wages_{i,t+1}$ *($M)*	4,116.07	606.41	12,158.50	268.73	2,531.17
$Ln(Wages)_{i,t+1}$	20.51	20.22	1.72	19.41	21.65
$PostFirstSubsidy_{i,t}$	0.86	1.00	0.34	1.00	1.00
$PostFirstEmploymentSubsidy_{i,t}$	0.16	0.00	0.37	0.00	0.00
$PostFirstInvestmentSubsidy_{i,t}$	0.77	1.00	0.42	1.00	1.00
$PostFirstNonEmploymentSubsidy_{i,t}$	0.72	1.00	0.45	0.00	1.00
$PostFirstNonInvestmentSubsidy_{i,t}$	0.10	0.00	0.30	0.00	0.00
$Online_{i,t}$	0.85	1.00	0.36	1.00	1.00
$Newspaper_{i,t}$	0.13	0.00	0.33	0.00	0.00
Control Variables					
$Ln(Population.)_{j,t}$	10.67	10.46	1.32	9.75	11.45
$\%Educ_{j,t}$	23.11	20.60	10.45	15.60	28.90

This table presents sample selection details and descriptive statistics for the local subsidy sample. This panel presents descriptive statistics for the dependent variables, variables of interest, and control variables for the sample of 1,944 county-year observations. We define all variables in Appendix A.

Online Appendix Table 2
Local Subsidy Analyses

Panel A: Relation between Subsidies and Local Activity

Dep Var:	*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)
PostSubsidy$_{i,t}$	**-0.0016**		**-0.0051**	
	[-0.22]		**[-0.53]**	
Ln(#FirstSubsidies)$_{i,t}$		**0.0034**		**-0.0022**
		[0.41]		**[-0.21]**
Ln(#SubsequentSubsidies)i,t		**-0.0009**		**-0.0040**
		[-0.22]		**[-0.73]**
Ln(Population)$_{i,t}$	0.7200***	0.7228***	0.6825***	0.7009***
	[5.00]	[5.11]	[3.52]	[3.72]
%Educ$_{j,t}$	0.0016*	0.0016*	0.0022*	0.0022*
	[1.70]	[1.67]	[1.66]	[1.66]
County FE?	Yes	Yes	Yes	Yes
State-by-Year FE?	Yes	Yes	Yes	Yes
Observations	1,944	1,944	1,944	1,944
Adj. R-squared	0.9994	0.9994	0.9991	0.9991

This panel presents results of testing the relation between subsidies and the log of the number of employees (Columns (1)-(2)) and the log of aggregate wages (Columns (3)-(4)). 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 Table 2
Local Subsidy Analyses

Panel B: Relation between Subsidies and Local Activity by Stated Criteria

Dep Var:	*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)
PostEmploymentSubsidy$_{i,t}$	**0.0032**		**0.0119**	
	[0.37]		**[1.15]**	
Ln(#FirstEmploymentSubsidies)$_{i,t,t}$		**0.0071**		**0.0118***
		[1.28]		**[1.74]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$		**-0.0142*****		**-0.0102**
		[-2.60]		**[-1.24]**
PostNonEmploymentSubsidy$_{i,t}$	-0.0033	-0.0031	-0.0108	-0.0062
	[-0.33]	[-0.44]	[-0.82]	[-0.66]
Ln(Population)$_{i,t}$	0.7207***	0.7183***	0.6851***	0.6817***
	[5.00]	[5.07]	[3.54]	[3.54]
%Educ$_{j,t}$	0.0016*	0.0016*	0.0022	0.0022
	[1.68]	[1.70]	[1.61]	[1.63]
County FE?	Yes	Yes	Yes	Yes
State-by-Year FE?	Yes	Yes	Yes	Yes
Observations	1,944	1,944	1,944	1,944
Adj. R-squared	0.9994	0.9994	0.9991	0.9991

This panel presents results of testing the relation between subsidies and the log of the number of establishments (Column (1)), the log of the number of employees (Column (2)), and the log of aggregate wages (Column (3)) for the 250 counties without a megadeal subsidy (n=1,944). 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 Table 2
Local Subsidy Analyses

Panel C: The Role of the Subsidy Information Environment in Subsidy Effectiveness

Dependent Var:	*Ln(Employees)$_{i,t+1}$*				*Ln(Wages)$_{i,t+1}$*			
Type of Information$_{i,t}$	*Online*		*Newspaper*		*Online*		*Newspaper*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostEmploymentSubsidy$_{i,t}$	**-0.0199**		**0.0060**		**0.0017**		**0.0144**	
	[-1.41]		**[0.66]**		**[0.09]**		**[1.34]**	
PostEmploymentSubsidy$_{i,t}$*Information$_{i,t}$	**0.0405****		**-0.0327**		**0.0179**		**-0.0288**	
	[2.34]		**[-1.48]**		**[0.83]**		**[-0.94]**	
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**-0.0092**		**0.0095**		**0.0043**		**0.0147****
		[-1.13]		**[1.59]**		**[0.37]**		**[1.98]**
Ln(#FirstEmploymentSubsidies)$_{i,t}$*Information$_{i,t}$		**0.0288****		**-0.0186***		**0.0132**		**-0.0220**
		[2.42]		**[-1.88]**		**[0.82]**		**[-1.41]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$		**-0.0129****		**-0.0138****		**-0.0096**		**-0.0096**
		[-2.46]		**[-2.47]**		**[-1.19]**		**[-1.15]**
PostNonEmploymentSubsidy$_{i,t}$	-0.0038	-0.0024	-0.0037	-0.0029	-0.0110	-0.0059	-0.0111	-0.0059
	[-0.37]	[-0.34]	[-0.36]	[-0.41]	[-0.83]	[-0.62]	[-0.84]	[-0.63]
Ln(Population)$_{i,t}$	0.7126***	0.7125***	0.7213***	0.7163***	0.6815***	0.6790***	0.6856***	0.6794**
	[4.94]	[5.03]	[5.00]	[5.06]	[3.50]	[3.51]	[3.54]	[3.53]
%Educ$_{j,t}$	0.0016	0.0016*	0.0016*	0.0016*	0.0021	0.0021	0.0021	0.0021
	[1.65]	[1.66]	[1.66]	[1.67]	[1.60]	[1.62]	[1.60]	[1.60]
County FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State-by-Year FE?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,944	1,944	1,944	1,944	1,944	1,944	1,944	1,944
Adj. R-squared	0.9994	0.9994	0.9991	0.9994	0.9991	0.9991	0.9991	0.9991

This panel presents results of testing the role of the local subsidy information environment for the relation between subsidies and the log of the number of employees (Columns (1)-(4)) and the log of aggregate wages (Columns (5)-(8)) for the 243 counties with requisite subsidy information (n=1,944). Because job commitment disclosure is not available for these subsidies, we measure *Information* using *Online* and the presence 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, unlike the other measures, 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 *Newspaper$_{i,t,}$* an indicator variable equal to one if a local newspaper is operational (and zero otherwise) by county-year over our sample period. We thank Pengjie Gao, Chang Lee, and Dermot Murphy for graciously sharing their newspaper closure data. 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 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 Table 3
Correlations for State Subsidy Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) *Ln(Employees)*$_{i,t+1}$	**1.00**	**0.99**	**0.19**	**0.45**	**0.43**	**0.16**	**0.39**	**0.40**	**-0.17**	**-0.26**	**-0.20**	**0.97**	**0.61**
(2) *Ln(Wages)*$_{i,t+1}$	**0.99**	**1.00**	**0.21**	**0.45**	**0.44**	**0.17**	**0.39**	**0.41**	**-0.17**	**-0.26**	**-0.20**	**0.96**	**0.63**
(3) *PostFirstSubsidy*$_{i,t}$	**0.20**	**0.21**	**1.00**	**0.50**	**0.36**	**0.69**	**0.41**	**0.31**	**0.03**	**-0.06**	**-0.16**	**0.18**	**0.11**
(4) *Ln(#FirstSubsidies)*$_{i,t}$	**0.43**	**0.44**	**0.63**	**1.00**	**0.70**	**0.43**	**0.90**	**0.69**	**-0.12**	**-0.30**	**-0.29**	**0.45**	**0.25**
(5) *Ln(#SubsequentSubsidies)*$_{i,t}$	**0.38**	**0.40**	**0.43**	**0.63**	**1.00**	**0.41**	**0.68**	**0.92**	**-0.08**	**-0.17**	**-0.23**	**0.41**	**0.26**
(6) *PostFirstEmploymentSubsidy*$_{i,t}$	**0.16**	**0.17**	**0.69**	**0.52**	**0.47**	**1.00**	**0.60**	**0.45**	**-0.02**	**0.04**	**-0.05**	**0.15**	**0.11**
(7) *Ln(#FirstEmploymentSubsidies)*$_{i,t}$	**0.36**	**0.37**	**0.54**	**0.81**	**0.61**	**0.79**	**1.00**	**0.75**	**-0.15**	**-0.26**	**-0.20**	**0.39**	**0.24**
(8) *Ln(#SubsequentEmploymentSubsidies)*$_{i,t}$	**0.35**	**0.37**	**0.37**	**0.58**	**0.89**	**0.54**	**0.69**	**1.00**	**-0.09**	**-0.20**	**-0.20**	**0.39**	**0.25**
(9) *DisclosureRegime*$_{,t}$	**-0.16**	**-0.15**	**0.03**	**-0.03**	**-0.04**	**-0.02**	**-0.05**	**-0.05**	**1.00**	**0.36**	**0.09**	**-0.22**	**-0.13**
(10) *Online*$_{i,t}$	**-0.24**	**-0.24**	**-0.06**	**-0.19**	**-0.09**	**0.04**	**-0.11**	**-0.11**	**0.36**	**1.00**	**0.31**	**-0.29**	**-0.11**
(11) *Online+Jobs*$_{i,t}$	**-0.19**	**-0.19**	**-0.16**	**-0.30**	**-0.22**	**-0.05**	**-0.19**	**-0.18**	**0.09**	**0.31**	**1.00**	**-0.20**	-0.01
(12) *Ln(Population.)*$_{j,t}$	**0.96**	**0.95**	**0.19**	**0.43**	**0.37**	**0.15**	**0.35**	**0.34**	**-0.20**	**-0.27**	**-0.20**	**1.00**	**0.56**
(13) %Educ$_{j,t}$	**0.63**	**0.64**	**0.11**	**0.26**	**0.24**	**0.12**	**0.24**	**0.23**	**-0.09**	**-0.14**	-0.01	**0.57**	**1.00**

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.

Online Appendix Table 4
Robustness to Estimation with State Control Variables

Dependent Var:	*Ln(Employees)$_{i,t+1}$*				*Ln(Wages)$_{i,t+1}$*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
PostFirstEmploymentSubsidy$_{i,t}$	**0.0005**		**0.0288***		**-0.0005**		**0.0238**	
	[0.14]		**[1.95]**		**[-0.10]**		**[1.34]**	
PostFirstEmploymentSubsidy$_{i,t}$*Online$_{i,t}$			**-0.0381****				**-0.0397****	
			[-2.38]				**[-2.01]**	
PostFirstEmploymentSubsidy$_{i,t}$*Online+Jobs$_{i,t}$			**0.0098**				**0.0196****	
			[1.46]				**[2.02]**	
Ln(#FirstEmploymentSubsidies)$_{i,t}$		**0.0066***		**0.0327****		**0.0024**		**0.0238**
		[1.72]		**[2.02]**		**[0.44]**		**[1.22]**
Ln(#FirstEmploymentSubsidies)*Online$_{i,t}$				**-0.0355****				**-0.0371***
				[-2.04]				**[-1.72]**
Ln(#FirstEmploymentSubsidies)*Online+Jobs$_{i,t}$				**0.0109**				**0.0225****
				[1.52]				**[2.11]**
Ln(#SubsequentEmploymentSubsidies)$_{i,t}$		**0.0088*****		**0.0088*****		**0.0058*****		**0.0059*****
		[5.82]		**[5.80]**		**[2.75]**		**[2.79]**
PostFirstNonEmploymentSubsidy$_{i,t}$	-0.0052	-0.0021	-0.0052	-0.0024	-0.0046	-0.0009	-0.0040	-0.0010
	[-1.15]	[-0.53]	[-1.16]	[-0.60]	[-0.75]	[-0.16]	[-0.65]	[-0.19]
Ln(Population)$_{i,t}$	0.5798***	0.5573***	0.5762***	0.5134***	0.5107***	0.4970***	0.5083***	0.4956***
	[8.95]	[8.66]	[8.90]	[6.16]	[6.14]	[5.98]	[6.12]	[5.96]
%Educ$_{j,t}$	0.0006**	0.0006**	0.0006**	0.0008**	0.0008**	0.0008**	0.0008**	0.0008**
	[2.41]	[2.31]	[2.32]	[2.29]	[2.30]	[2.25]	[2.27]	[2.23]
MinWage$_{i,t}$	0.0044*	0.0067***	0.0047*	0.0137***	0.0126***	0.0142***	0.0135***	0.0152***
	[1.83]	[2.76]	[1.92]	[3.60]	[3.32]	[3.71]	[3.56]	[3.98]
Ln(GDP)$_{j,t}$	0.2441***	0.2377***	0.2436***	0.4964***	0.4969***	0.4937***	0.4930***	0.4903***
	[6.53]	[6.38]	[6.53]	[9.28]	[9.26]	[9.18]	[9.21]	[9.14]
%Union$_{j,t}$	-0.0025**	-0.0026**	-0.0026**	-0.0054***	-0.0053***	-0.0054***	-	-0.0055***
	[-2.12]	[-2.27]	[-2.24]	[-3.28]	[-3.23]	[-3.28]	[-3.34]	[-3.38]
CorpTaxRate$_{j,t}$	-0.0031***	-0.0022***	-0.0031***	-0.0034***	-0.0032**	-0.0026**	-	-0.0027**
	[-4.15]	[-2.87]	[-4.20]	[-2.82]	[-2.56]	[-2.09]	[-2.69]	[-2.28]
Ln(UIContrib)$_{j,t}$	0.0086	0.0060	0.0078	0.0126	0.0123	0.0104	0.0128	0.0112
	[1.39]	[0.99]	[1.33]	[1.62]	[1.50]	[1.28]	[1.64]	[1.45]
PropertyTax$_{j,t}$	-0.1694***	-0.1392***	-0.1732***	-0.2285***	-0.2339***	-0.2133***	-	-0.2104***
	[-3.67]	[-3.04]	[-3.82]	[-3.92]	[-3.97]	[-3.63]	[-3.97]	[-3.61]
PersonalTaxRate$_{j,t}$	0.0008	0.0005	0.0006	0.0024	0.0025	0.0023	0.0023	0.0022
	[0.64]	[0.37]	[0.48]	[1.34]	[1.40]	[1.29]	[1.27]	[1.19]
TaxIncentivesIndex$_{j,t}$	0.0047***	0.0052***	0.0041***	0.0045***	0.0048***	0.0051***	0.0044***	0.0048***
	[3.55]	[3.95]	[3.23]	[2.92]	[3.11]	[3.37]	[2.83]	[3.18]
SalesTaxRate$_{j,t}$	-0.0063***	-0.0070***	-0.0063***	-0.0058***	-0.0059***	-0.0063***	-	-0.0062***

	[-3.78]	[-4.20]	[-3.78]	[-2.58]	[-2.63]	[-2.82]	[-2.61]	[-2.79]
Observations	9,320	9,320	9,320	9,320	9,320	9,320	9,320	9,320
Adj. R-squared	0.9990	0.9984	0.9990	0.9991	0.9984	0.9980	0.9984	0.9984

This table reports robustness of results to replacing state-by-year fixed effects with year fixed effects and state-level control variables. $Ln(Population)_I$ is the logarithm of total population of county i in year t+1. County population data is computed by the US Census Bureau. $\%Educ_{i,tj}$ is the percentage of people in county i with at least a four-year college degree in year t. $MinWage_{i,t}$ is the 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. $Ln(GDP)_{j,t}$ is the log of total GDP in year t for state j. $\%Union_{j,t}$ is the percentage of private-sector employees in state j who are union members in year t. $CorpTaxRate\%_{j,t}$ is the top marginal corporate state tax rate for state j in year t. $Ln(UIContrib)_{j,t}$ is the log of the 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. $PropertyTax_{j,t}$ is the 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. $PersonalTaxRate\%_{j,t}$ is the top marginal personal *state* income tax rate for state j in year t. $TaxIncentivesIndex_{j,t}$ is an 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). $SalesTaxRate\%_{j,t}$ is the sales tax rate assessed by state j in year t (does not include any additional sales tax that may be collected by county i). We cluster standard errors by county. The asterisks *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively. We define all variables in Appendix A.