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, transparency problems inhibit clear assessments about whether subsidies achieve their intended outcomes. We first develop a parsimonious model, which demonstrates the conditions under which subsidy disclosure facilitates job growth. We then empirically test the model's predictions to examine whether there is a differential relation between subsidies and local employment in the states where governments provide more information about granted subsidies. We measure subsidy information 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 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 prior work. Furthermore, we offer policy-relevant evidence for the increasing number of states implementing subsidy disclosure regimes.

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1 Introduction

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 subsidy awards is a central problem; the opaque nature of subsidies inhibits public monitoring, thereby impeding an assessment of whether employment and investment increases following subsidy grant (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, a growing stream of research examines the subsidy granting process, which is inherently political in nature (Slattery, 2021; Aobdia et al., 2020). Second, there is little information available to the general public, such as information about granting agencies, recipient firms, amounts awarded, intended outcomes, and resulting effects. This lack of transparency is exacerbated by the fact that subsidy

[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).

giving is highly decentralized, often occurring across multiple state-level agencies and in thousands of municipalities and counties. Concerns about transparency have become so great that a growing number of governments have recently implemented subsidy disclosure regulations in an effort to improve subsidy effectiveness. Furthermore, the U.S. Governmental Accounting Standards Board recently required all state and local governments to disclose information about foregone tax revenues attributable to subsidy abatements in their annual financial statements (GASB 77). These disclosure regimes reflect the sentiment 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).

It is this transparency concern 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. To formalize the intuition behind our empirical analyses, we begin with a parsimonious model that illustrates the role of disclosure in facilitating subsidy-related job creation. We model the role of three parties: the government, a representative firm, and the public. The government awards a subsidy to the representative firm as a way to stimulate economic growth, but the government can be penalized by taxpayers if the subsidy is deemed to be wasteful *ex-post*. To minimize these penalties, the government engages in private monitoring and can also disclose information about awarded subsidies to facilitate public monitoring. The public can use any information disclosed by the government to hold both the government and the firm accountable if it deems the subsidy wasteful. The model demonstrates the conditions under which full, partial, or no disclosure are optimal. Because we do not empirically observe jurisdictions that provide full disclosure of all subsidy details, and because it is impossible to evaluate jurisdictions with no disclosure, we focus our predictions and empirical tests on studying the role of partial disclosure, which characterizes the actual disclosure status of most states.

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 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

[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), Dong et al. (2021), Slattery (2020, 2021) and Slattery and Zidar (2020).

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 also 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 periods.

Having demonstrated positive employment effects in our sample, we turn to examining our primary research question on the role of subsidy disclosures. Our model demonstrates that the effectiveness of disclosure can increase in its precision. Thus, we use three measures of subsidy disclosure with varying levels of precision: (i) whether the state had a subsidy disclosure regime in place at the time of the first observed subsidy, (ii) whether information about the specific subsidy program was disclosed online, and (iii) whether – in addition to online disclosure – the subsidy recipient was required to provide specific job commitments prior to subsidy receipt. We observe positive employment effects when the subsidy recipient provided specific job commitments, suggesting that these detailed 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 are consistent with contrasting predictions from the model for partial disclosure and suggest that 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 setting suffers from substantial information

problems about whether subsidies achieve their intended outcomes (Greenstone and Moretti, 2004; Good Jobs First, 2010; Pew Charitable Trusts, 2013; Slattery, 2020). 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, we add to the nascent accounting literature also 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; Dong et al., 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, 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 provide an analytical model that outlines the role of subsidy information in Section 2.2 and use this model to derive our empirical predictions.

[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.

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 examining the role of subsidy disclosures in subsidy effectiveness.

2.2 The Role of the Information Environment in Subsidy Effectiveness

2.2.1 Overview of Model

To formalize the intuition behind our empirical analyses, we present a parsimonious model that illustrates the role of disclosure in facilitating subsidy-related job creation. Our model incorporates three parties: the government, a representative firm, and the public. The government awards a subsidy to the representative firm to stimulate economic growth, but the government can be penalized by taxpayers if the subsidy is deemed to be wasteful *ex-post*. To minimize these penalties, the government engages in private monitoring and can also disclose information that facilitates public monitoring. The firm seeks to maximize its profits, subject to the potential penalties it may

face from non-compliance with the subsidy terms (i.e., not creating enough jobs). The firm can face consequences for non-compliance as a result of both public and private monitoring.[4] The public is motivated by a desire to have its tax dollars spent efficiently. The public can use information disclosed by the government to hold both the government and the firm accountable if it deems the subsidy wasteful.

The model proceeds in three stages. In the first stage, the state government awards a subsidy worth s dollars to the firm. We assume for simplicity that s is fixed rather than a choice undertaken by the government.[5] In this stage, the government also chooses (i) the degree to which it will directly monitor the firm's compliance and (ii) a public disclosure regime. In the second stage, the firm hires employees necessary for producing its goods and then sells its outputs. In the third stage, the public potentially observes the number of jobs the firm created and, if dissatisfied, can punish both the firm and the government through reputational damage and political costs, respectively.

We make several simplifying assumptions. Most notably, we focus on the government's choice along a single dimension (disclosure) but do not endogenize other potential governmental decisions, such as the value of subsidies to be awarded. In addition, for tractability, we model only the actions of a single representative firm rather than a multi-firm competitive market.

2.2.2 The Government

We begin with the government's first-stage optimization problem. We assume that income in the state is taxed at rate T and that all corporate income is passed through to employees.[6] If the firm creates J jobs and pays each employee a wage of w, the government obtains tax revenue of $J \times w \times T$.

[4] For example, a direct consequence is the clawback of a subsidy by the government or negative press and boycotts by the public. Indirect consequences include not receiving future subsidies or having state and local political officials support legislation at odds with the company's interests.

[5] We assume the budget setting process completed prior to the government's fiscal year sets aside $\$S$ for subsidies to be granted in the year. As a result, the government's mandate is to award $\$S$ in subsidies during the year. For simplicity, we ignore bargaining between the firm and the government during the subsidy process.

[6] This simplifying assumption reflects that subsidies are intended to ultimately generate additional tax revenue through the jobs that they create, and is not intended to generate conclusions about the incidence of taxation.

We assume that the government benefits from each job created and that it faces political costs if the subsidy is deemed to be wasteful to the state, where we define wasteful subsidies as those for which the government does not break even. More formally, we assume that the government has two objectives: (i) it would like the firm to create as many jobs as possible, and (ii) it potentially faces political costs if the firm does not create a sufficient number of jobs such that $J \times w \times T \geq s$ or, alternatively, $J \geq \frac{s}{w \times T}$.

If the firm creates an insufficient number of jobs, the government potentially faces political costs of c. The government engages in two actions to mitigate these costs. First, the government directly monitors the firm following subsidy grant. Because monitoring is costly, a firm may not always face consequences even if the number of jobs J it creates is below the government's break-even point. The government chooses a monitoring effort m, for which the (convex) costs of monitoring are given by $\frac{k}{2} \times m^2$ for known constant k.[7]

Second, the government selects a public disclosure regime. The government chooses from one of three regimes: (i) no disclosure of subsidy information, (ii) imprecise disclosure of subsidy-related information, and (iii) precise disclosure of subsidy-related information. The government faces a trade-off in selecting its disclosure choice. On one hand, public disclosure of subsidy-related information induces pressure on the firm to create jobs; on the other hand, public disclosure increases the likelihood that the government incurs political costs if the subsidy fails (e.g., accusations of wasteful spending by media or political opponents). For simplicity, we assume that choosing a disclosure regime is costless.

We incorporate the disclosure choice into the model through the inclusion of p_D, which is the probability that public monitoring will uncover insufficient job creation. The subscript D reflects the fact that p_D is driven by the government's choice of disclosure regime. Thus, the government

[7]For example, monitoring can include requiring recipients to file subsequent reports that provide employee hiring information. Monitoring costs may not necessarily be an explicit monetary cost; they also include the opportunity cost related to time spent on monitoring as opposed to completing other government duties.

solves the following optimization problem:

$$max_m \mathbb{E}[J|m] - c \times p_D \times \mathbb{P}\{J < \frac{s}{w \times T}|m\} - \frac{k}{2} \times m^2 \tag{1}$$

That is, the government wants to maximize job creation J, taking into account both political costs (c) and the costs of monitoring ($\frac{k}{2} \times m^2$). The expected value in the first term, $max_m \mathbb{E}[J|m]$, reflects that the government does not have perfect information about the firm's demand curve, which directly affects J; see Section 2.2.3 below. Thus, the government cannot perfectly predict how many jobs a firm will create for any particular subsidy. The second term $c \times p_D \times \mathbb{P}\{J < \frac{s}{w \times T}|m\}$ captures the expected political costs that the government will incur if the firm does not create enough jobs to offset the subsidy's cost. Under the no-disclosure regime, p_D is (by construction) zero, meaning that Equation (1) reduces to

$$max_m \mathbb{E}[J|m] - \frac{k}{2} \times m^2 \tag{2}$$

Under full disclosure, p_D is (by construction) 1, meaning that Equation (1) can be rewritten as

$$max_m \mathbb{E}[J|m] - c \times \mathbb{P}\{J < \frac{s}{w \times T}|m\} - \frac{k}{2} \times m^2. \tag{3}$$

2.2.3 The Firm

We assume that the single representative firm is a price-taker that faces a standard downward-facing demand curve with demand parameter α. That is, if the firm produces q units of goods, the price it commands per good is given by $\alpha - q$ (which yields a total revenue of $(\alpha - q) \times q$). We assume that the firm knows α. However, the government cannot perfectly observe α and only knows that α is uniformly distributed over the interval $[0, \theta]$ for some positive real number θ. Because the firm is aware of the distribution of α, it also knows the value of m chosen by the government at the

time the firm makes its hiring decision.

We assume that each additional employee enables the firm to produce one additional unit of goods. Thus, if the firm hires J employees, it obtains revenue $(\alpha - J) \times J$. The firm must pay each employee a wage w. Finally, we assume that the firm faces some fixed cost of production F. In practice, F could represent relocation costs or one-time costs associated with initial construction of a facility and hiring employees. For subsidies targeted at retaining existing firms and employment levels, F is close to zero.[8] The firm's optimization problem in the absence of a subsidy would therefore be:

$$max_J(\alpha - J)J - wJ - F \tag{4}$$

meanting that the firm wants to maximize revenue after wages and fixed costs. Incorporating subsidies into this optimization provides additional cash flow (s), as well as commensurate costs. The optimization problem with subsidies can be written as:

$$max_J(\alpha - J)J - wJ - F + s - (m + p_D)(\frac{s}{w \times T} - J)\mathbb{I}\{J < \frac{s}{w \times T}\} \tag{5}$$

The last term captures the costs that accompany subsidy receipt. Specifically, if the firm accepts a subsidy s, it is aware that the government expects it to create a certain number of jobs for the government to at least break even on the funds awarded; that is, the government would like the firm to set $\{J \geq \frac{s}{w \times T}\}$. If the firm does not create at least $\frac{s}{w \times T}$ jobs, it faces potential punishment from both the government and the public. This punishment is proportional to the extent the firm fell short of the break-even point; that is, a firm missing its target by 10 jobs will be punished less than a firm that missed its target by 100 jobs. These costs are a function of m, the monitoring

[8]The fixed cost F also illustrates the firm's rationale for accepting a subsidy (and the government's rationale for awarding one to encourage the firm to expand in its state). In the presence of high fixed costs, there may be no value of J for which expression (2) is positive, impeding the firm from opening a new facility or expanding operations in the state. By offsetting some of the firm's fixed costs, the subsidy encourages expanded production in its jurisdiction.

effort chosen by the government in Stage 1, and p_D, the probability that the public punishes the firm for missing the break-even number. The term $(m + p_D)$ is additive because the firm can face consequences from both the government and the public.[9]

2.2.4 The Public

The public serves a largely passive, singular role: it can potentially punish both the firm and government when it perceives that tax dollars have been inefficiently spent on subsidies. The likelihood that the public punishes the firm and government depends on the disclosure regime chosen in the first stage, as public monitoring is a function of the information environment.

Recall the government can choose three disclosure regimes: no disclosure, partial disclosure, or full disclosure. When the government has chosen no disclosure, we assume that the public does not learn about the subsidy, and there is no third stage of the game.[10] When the government has chosen partial disclosure, the public learns limited information about the subsidy and observes an imprecise signal of what has occurred. Thus, the public does not always choose to investigate the firm and examines with probability p_D. A public investigation only leads to negative consequences for the firm (and political costs for the government) if the number of jobs created is less than the break-even number $\{J < \frac{s}{w \times T}\}$). If the public investigates the subsidy and learns that the firm has created at least the break-even number of jobs, there are no negative consequences to the firm or government.

[9] For tractability, we assume that government monitoring and public monitoring do not directly interact.

[10] We acknowledge that this does not perfectly reflect the actual subsidy information environment, because some subsidies are observable through channels other than disclosure (for example, FOIA requests). Altering the model to reflect that the public can (imperfectly) learn about the subsidy in the no-disclosure case does not alter our inferences, as long as the signal in the no-disclosure case is noisier than the signal in the partial disclosure case.

2.2.5 Equilibrium and Hypothesis

The game is solved by backward induction. The public (potentially) has some information about the likelihood the firm created a socially acceptable number of jobs following receipt of a subsidy. Knowing this, the firm selects the number of jobs J to create, comparing the costs of creating an insufficient number of jobs (given the government's existing monitoring and disclosure regime) to the firm's optimal level of production. Thus, the government's first-stage decisions with respect to the disclosure regime and monitoring effort informs the firm's choice of J. The model yields a critical value for J^*, which is the optimal number of jobs:

$$J^* = \frac{\alpha - w + m + p_D}{2} \tag{6}$$

Equation (4) shows that the number of jobs created J^* is increasing in the demand parameter α and decreasing in the per-employee wage w. The number of jobs created J^* is also increasing in the government's monitoring effort m, meaning that government monitoring facilitates greater employment. Furthermore, J^* is increasing in p_D, reflecting that public monitoring risk to the firm increases as the public's signal becomes more precise. For full and non-disclosure, we replace p_D with 0 and 1 respectively. The detailed solution is included in Appendix A.

Let $\beta \equiv \frac{s}{w \times T}$ denote the break-even number of jobs for the subsidy. Further analysis shows that the firm chooses the break-even number of jobs when

$$\frac{\alpha - w + m + p_D}{2} > \beta \tag{7}$$

That is, the government can choose a monitoring effort m that is high enough such that the firm will always create a break-even number of jobs.

We turn next to the government's decision to motivate our empirical predictions. Our goal

is not to precisely characterize the government's monitoring effort m; rather, it is to highlight the conditions under which the government makes different disclosure choices and to study the resulting consequences of the disclosure choice on local employment.

Use of the Envelope Theorem to compare the expected payoffs to the government shows that, regardless of the disclosure regime (where p_D equals 0 and 1 for the non- and full-disclosure cases, respectively), the probability of the firm creating less than the break-even number of jobs is decreasing in m. When local demand is sufficiently high ($\frac{\alpha-w}{2} \geq \beta$, the firm creates enough jobs irrespective of the government's choice of disclosure policy.[11] Conversely, when local demand is not high enough for the firm to create the break-even number of jobs ($\frac{\alpha-w}{2} < \beta$), the government's monitoring and disclosure choices affect the amount of job creation.

Further analysis in Appendix A shows that the probability that the firm chooses to create the break-even number of jobs is higher when the disclosure regime is more precise. However, the fact that the expected number of jobs created J, given the government's choice of monitoring effort m, increases with the quality of disclosure does not imply that the government will always choose full disclosure.[12] Thus, we evaluate the conditions under which the three disclosure regimes (full, partial, or no disclosure) can arise in equilibrium. We find a unique solution p^* (i.e., a single critical value) for the probability that the public detects and punishes a below-break-even number of jobs. This generates two propositions:

Proposition 1 (Full or No Disclosure): If political costs are sufficiently high and p^* is large (small), then no disclosure (full disclosure) is optimal.

[11] In this case, the government's monitoring effort is "wasted" in the sense that it does not change the firm's chosen number of jobs. However, the government does not know *ex ante* that its monitoring level has no impact because it does not perfectly know the demand curve for the firm's goods.

[12] The scenarios in which the government will prefer either full or no disclosure are a function of the firm's political costs and monitoring costs. For example, if political costs c are sufficiently large, any type of disclosure is too risky for the government because in expectation it will incur substantial political punishment, even when there is a low probability that a firm will miss its job targets.

Proposition 2 (Partial Disclosure): If political costs are low and p^* is close or equal to p_D, then partial disclosure is optimal. Partial disclosure can have heterogeneous effects on job creation:

(a) For firms with stronger demand (higher α), job creation will be higher after subsidy receipt for governments with a partial disclosure regime.

(b) For small subsidies, job creation will be lower after subsidy receipt for governments with a partial disclosure regime.

(c) For governments with limited monitoring capabilities, job creation will be lower for governments with a partial disclosure regime.

Our empirical predictions are derived from these two propositions. Ideally, we would test the role of full, partial, and no disclosure. However, no jurisdictions have full disclosure of all subsidy amounts. Furthermore, it is impossible to empirically evaluate jurisdictions with no disclosure given the inability to observe subsidy receipt. Thus, our empirical tests focus on studying the role of partial disclosure, using measures with varying amounts of precision (p_D).

As outlined above, partial disclosure can have different effects on job creation. On one hand, partial disclosure can facilitate job creation, particularly among subsidy recipients that have relatively higher demand. On the other hand, partial disclosure can be associated with lower job creation, particularly for small subsidies or for governments that otherwise have limited ability to monitor. Therefore, we predict our hypothesis in null form:

H1: There is no variation in the positive relation between subsidies and employment outcomes based on government disclosures of subsidy information.

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:

$$Employment_{i,t+1} = \alpha + \beta_1 Subsidy_{i,t} + Controls_{i,t} + County_i + StateByYear_{j,t} + \varepsilon \qquad (8)$$

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 $PostSubsidy_{i,t}$, 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.[13] 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 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 complementary relation between labor and capital). 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 re-

[13] 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.

place $PostFirstSubsidy_{i,t}$ 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.[14]

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

3.2 Hypothesis Test

$$\begin{aligned} Employment_{i,t+1} =& \alpha + \beta_1 FirstEmploymentSubsidy_{i,t} + \beta_2 Information_{i,t} * \\ & FirstEmploymentSubsidy_{i,t} + \beta_3 PostFirstNonEmploymentSubsidy_{i,t} \\ & + Controls_{i,t} + County_i + StateByYear_{j,t} + \varepsilon \end{aligned} \tag{9}$$

To test our hypothesis about the role of disclosure in the relation between subsidies and employment outcomes, we augment Eq. (8) 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. (9) 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 in the presence of greater disclosed information about the subsidies. 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.[15] 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 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

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

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).[16]

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 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 more complete col-

[16] 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.

lection 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.[17]

We aggregate subsidies to the county-year level using addresses of subsidy recipients provided by GJF. This step permits inclusion of both public and private 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. These measures reflect an increasing level of precision, consistent with the model's predictions about p_D. 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.[18] $DisclosureRegime_{i,t}$ is an indicator equal to one for county-years in which the first subsidy was subject to disclosure requirements, and zero otherwise.

[17] 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). Thus, the tests map directly to the partial disclosure predictions from the model (Proposition 2).

[18] 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.

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 the subsidy, as opposed to an indication from GJF that the subsidy was obtained through a specific FOIA request.

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 these job disclosures may originally have been a private disclosure by the subsidy recipient to the granting jurisdiction, we observe that nearly all subsidies with requisite 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.[19]

3.6 Control Variables

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

[19]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.

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 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.[20] 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

[20] 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.

extending this identification method to the accounting literature (Fuest et al., 2018). We mitigate concerns raised in Baker et al. (2021) about the use of 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

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).[21] 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).[22] This step leaves 27 states for which we observe consistent

[21]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.

[22]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

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 subsidy recipient or have megadeal recipients, resulting in a sample of 1,176 distinct counties (9,408 county-year observations). Finally, we drop 11 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 approximately $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

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).

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, 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 C and D 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 C lists all subsidy programs reviewed, and Appendix D 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. (8). 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, 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. (8) 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, 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.[23]

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 employment outcomes. Using the average county-year number of first-time employment subsidies granted, which ranges from 1.6 to 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 in the county of

[23] In untabulated analyses, we also re-estimate Eq. (8) 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.

$34,600 based on sample descriptive statistics.

One concern is that Eq. (8) does not appropriately capture observable characteristics that could be correlated with the relation between subsidies and local economic effects. Therefore, we re-estimate Eq. (8) 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. (9), with Panel A using state disclosure initiatives, Panel A using online subsidy disclosures, and Panel C using online disclosures of job counts.

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 result in no change in employment on average.

We find a similar pattern but stronger results in Panel B, where we examine the role of the availability of online subsidy disclosures. 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. We continue to find that having multiple years of subsidy recipients is important for increased employment.

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.[24] 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.

4.3 Establishments

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. (9) 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

[24]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 ($= e^{(0.011/0.0124)} - 1$).

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 analytical model, 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
Details of Model

This Appendix provides additional details for the analytical model presented in Section 2.2.

Derivation of J^*

The analysis below demonstrates how Eq. (6) was derived:

Let $\beta \equiv \frac{s}{w \times T}$ denote the break-even number of jobs for the subsidy. If the firm creates at least β jobs, it does not face any potential consequences due to government monitoring or public scrutiny. Hence, the optimization problem for $J > \beta$ is:

$$max_J(\alpha - J)J - wJ - F + s \tag{10}$$

which yields an optimum number of jobs to be created of $J^* = \frac{\alpha - w}{2}$. Thus, if $\frac{\alpha - w}{2} > \beta$ (which will be true if either demand is strong enough, wages are low enough, or the subsidy is small enough), it is optimal for the firm to create more jobs than needed to break even, irrespective of public monitoring or the government's choices regarding monitoring or disclosure.

Suppose instead that $\frac{\alpha - w}{2} < \beta$. In this case, the firm compares two possible outcomes: (i) the expected profits from creating fewer jobs than the break-even point, and (ii) the expected profits from creating exactly enough jobs to break even (which ensures against any costs arising from government or public monitoring). The firm's optimization problem can then be re-written as:

$$max_J(\alpha - J)J - wJ - F + s - (m + p_D)(\beta - J)\mathbb{I}\{J < \frac{s}{w \times T}\} \tag{11}$$

The optimal number of jobs the firm creates solves the following first-order condition:

$$(\alpha - w) - 2J + (m + p_D) = 0 \tag{12}$$

Re-arranging the first-order condition yields a critical value for J^*, which corresponds to Equation (6) in Section 2.2:

$$J^* = \frac{\alpha - w + m + p_D}{2} \tag{13}$$

Government's Disclosure Choice

Recall the government's optimization problem from Equation (1) is:

$$max_m \mathbb{E}[J|m] - c \times p_D \times \mathbb{P}\{J < \frac{s}{w \times T}|m\} - \frac{k}{2} \times m^2 \tag{14}$$

To highlight the conditions under which the government makes different disclosure choices, we use the Envelope Theorem to compare the equilibrium expected payoffs that the government obtains under each disclosure regime, as a function of the model's parameters.

We separately identify the first and second terms in the government's problem above for each of the three regimes. To identify $\mathbb{P}\{J < \frac{s}{w \times T}|m$, i.e., the probability that the firm does not create enough jobs for the government to break even, we use the inequality from Equation (7):

$$\mathbb{P}\{J < \frac{s}{w \times T}|m\} = \mathbb{P}(\frac{\alpha - w + m + p_D}{2} < \beta) = \frac{2\beta + w - m - p_D}{\theta} \tag{15}$$

where the latter expression above arises from the uniform distribution of α. Here, as above, we replace p_D with 0 and 1 for the non- and full-disclosure cases, respectively. Regardless of the disclosure regime, it is immediately evident from the equation that the probability of the firm creating less than the break-even number of jobs is decreasing in m.

To identify $\mathbb{E}[J|m]$, we must separately consider the cases where $\frac{\alpha-w}{2} \geq \beta$ and where $\frac{\alpha-w}{2} < \beta$. In the former case, the firm always creates more than β jobs irrespective of the government's or public's actions and, hence, its choice of J is constant with respect to both m and the disclosure regime. Conversely, in the latter case (when $\frac{\alpha-w}{2} < \beta$ meaning that demand is insufficient for the firm to naturally create the break-even number of jobs), the government's monitoring choice and choice of disclosure regime affects the amount of job creation. In the partial-disclosure case, when Equation (7) holds, the firm creates exactly β jobs; otherwise, the firm creates the number of jobs outlined in Equation (6). Therefore, maximizing $\mathbb{E}[J|m]$ is equivalent to maximizing

$$\mathbb{P}(\alpha < 2\beta + w - m - p_D) \times \mathbb{E}[J^*|\alpha < 2\beta + w - m - p_D] + \mathbb{P}(2\beta + w - m - p_D \leq \alpha \leq 2\beta + w) \times \beta \quad (16)$$

From this expression, several things are apparent. First, $\mathbb{E}[J|m]$ is increasing in p_D. The government's political costs are also increasing in p_D. Thus, it is not immediately apparent which regime and monitoring level the government would choose. In addition, because $\beta \geq J$ whenever $\frac{\alpha-w}{2} < \beta$, and the probability that the firm chooses to create the break-even number of jobs β is higher when the disclosure regime is more precise, it is also the case that for any level of government monitoring m the ex-ante number of jobs increases as disclosure becomes more precise.

However, the fact that $\mathbb{E}[J|m]$ increases with the quality of disclosure does not imply that the government will always choose full disclosure. Recall that the government faces potential political costs c if a "bad" subsidy comes to light. Consider one extreme case, when c is close to zero; in this scenario, the government finds full disclosure optimal. Consider another extreme case, when c is sufficiently large; in this case, any form of disclosure is too risky for the government because even a small probability that $J < \beta$ will incur large political costs. For example, political costs c may be lower (higher) in states without (with) politically competitive elections. Moreover, if k is sufficiently high – meaning that government monitoring capability is minimal – then the threshold for which the government finds the no-disclosure regime optimal is lower. This is because the likelihood of the firm creating less than the break-even number of jobs increases.

While these cases establish scenarios in which the government would prefer either full or no disclosure, is there an equilibrium in which the government would find partial disclosure optimal? To address this issue, we first explicitly derive the equilibrium levels of monitoring the government chooses under each regime. Given that the non-disclosure and full-disclosure regime simply reflect the special cases $p_D = 0$ and $p_D = 1$, it suffices to maximize a single expression:

$$\frac{4\beta^2 - (w - m - p_D)^2}{4\theta} + \frac{\beta}{\theta}(m + p_D) - \frac{c \times p_D}{\theta}(2\beta + w - m - p_D) - \frac{k}{2} \times m^2 \quad (17)$$

This, in turn, yields the following solution for the government's optimal level of monitoring, conditional on all other parameters:

$$m^* = \frac{2\beta + w - (1 - 2c)p_D}{2k\theta + 1} \quad (18)$$

This equation demonstrates that whether government monitoring is a complement or substitute to public monitoring depends on c. When $c < 1/2$, m^* is decreasing in p_D. That is, when political costs are low, public monitoring disciplines the firm without also causing substantial harm to the

government. In turn, the government is happy to delegate monitoring to the public. Conversely, when $c > 1/2$, public monitoring that uncovers a "bad" subsidy also creates sufficient political costs that the government has a stronger incentive to engage in its own monitoring. Recalling that $\beta \equiv \frac{s}{w \times T}$), it is also apparent from the equation above that monitoring increases as the size of the subsidy s increases.

To verify that any of the three regimes – full, partial, or no disclosure – can arise in equilibrium, we apply the Envelope Theorem. That is, we first compute the partial derivative of the government's optimization problem with respect to p_D. We then plug in the expression given for m^*. Noting that E[J|m] and $\mathbb{P}\{J < \frac{s}{w \times T}|m\}$ are functions of p_D, we have:

$$\frac{\partial}{\partial p_D}(\mathbb{E}[J|m] - c \times p_D \times \mathbb{P}\{J < \frac{s}{w \times T}|m\} - \frac{k}{2} \times m^2 = \frac{(1-2c)(2\beta + w - m) - (1-4c)p_D}{2\theta} \quad (19)$$

Plugging in this last expression, it is immediately apparent that the equation is linear in p_D, which implies there is a unique solution p^* (i.e., a single critical value) when that expression is set to zero. This critical value is given by:

$$p^* = \frac{2k\theta(2\beta + 2)(2c - 1)}{4c^2 + 2k\theta(4c - 1)} \quad (20)$$

Crucially, the value of p^* given in this equation may not lie within the feasible range for a probability, i.e., [0,1]. In turn, this allows for the existence of both the full-disclosure and non-disclosure equilibria, as discussed below. Moreover, at the optimal values of m^*, the government's payoff is quadratic in p_D. It therefore follows that p^* reflects a global maximum when $c < \frac{1}{4}$ and a global minimum when $c > \frac{1}{4}$.

When $c > \frac{1}{4}$, there are three possibilities. First, if $p^* \in (0, 1)$, then either the no-disclosure or full-disclosure regime is optimal because the government's payoff increases on both sides of p^*. Second, if $p^* > 1$, then the government's payoff monotonically decreases on $[0, p^*)$ which, in turn, implies a monotone decrease over the interval [0,1]; hence, the no-disclosure case is optimal. Finally, if $p^* < 0$ (which occurs for $\frac{1}{4} < c < \frac{1}{2}$) then, by a similar argument, the full-disclosure case is optimal. An important implication of the equilibria discussed above is that when $c > \frac{1}{4}$ – i.e., when political costs are sufficiently high – the partial disclosure regime is never optimal for the government. From the expressio above, it is evident that when $c > \frac{1}{4}$, a sufficiently high value of β or w will lead to the non-disclosure equilibrium (because we will obtain $p^* > 1$). That is, when the subsidy s is sufficiently high – as reflected by high β – the government prefers the nondisclosure regime. Similarly, the government prefers nondisclosure when the per-employee wage w is high (increasing the risk that the firm does not find it worthwhile to hire enough employees for the subsidy to break even.)

Suppose instead that $c > \frac{1}{4}$. There are two possibilities. If p^*1, then the full-disclosure regime is optimal. However, if $p^* \in (0, 1)$, then partial disclosure can be optimal if p_D is sufficiently close to p^* (and is always optimal if $p_D = p^*$). From the expression above, it is evident that an interior solution $p^* \in (0, 1)$ – reflecting the partial-disclosure equilibrium – will obtain if either (i) $2\beta + w$ is not too high, or (ii) $k\theta$ is sufficiently high. Put another way, the government may find partial disclosure optimal if political costs are low and (i) subsidy values are not too high , (ii) expected demand for the firm's output is strong enough (reflected by a higher θ); or (iii) the government's monitoring capabilities are limited (reflected by a high k). If (i) or (iii) is true, we would expect job creation to be lower for governments that choose a partial-disclosure regime, while if (ii) is true we would expect job creation to be higher for governments that choose a partial-disclosure regime.

Appendix B
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 C
Sample of State Subsidy Programs

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
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

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
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
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
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

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
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

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
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
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

Appendix D
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 E
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 F
Online Disclosure Example

Figure F1 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.[25] 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.

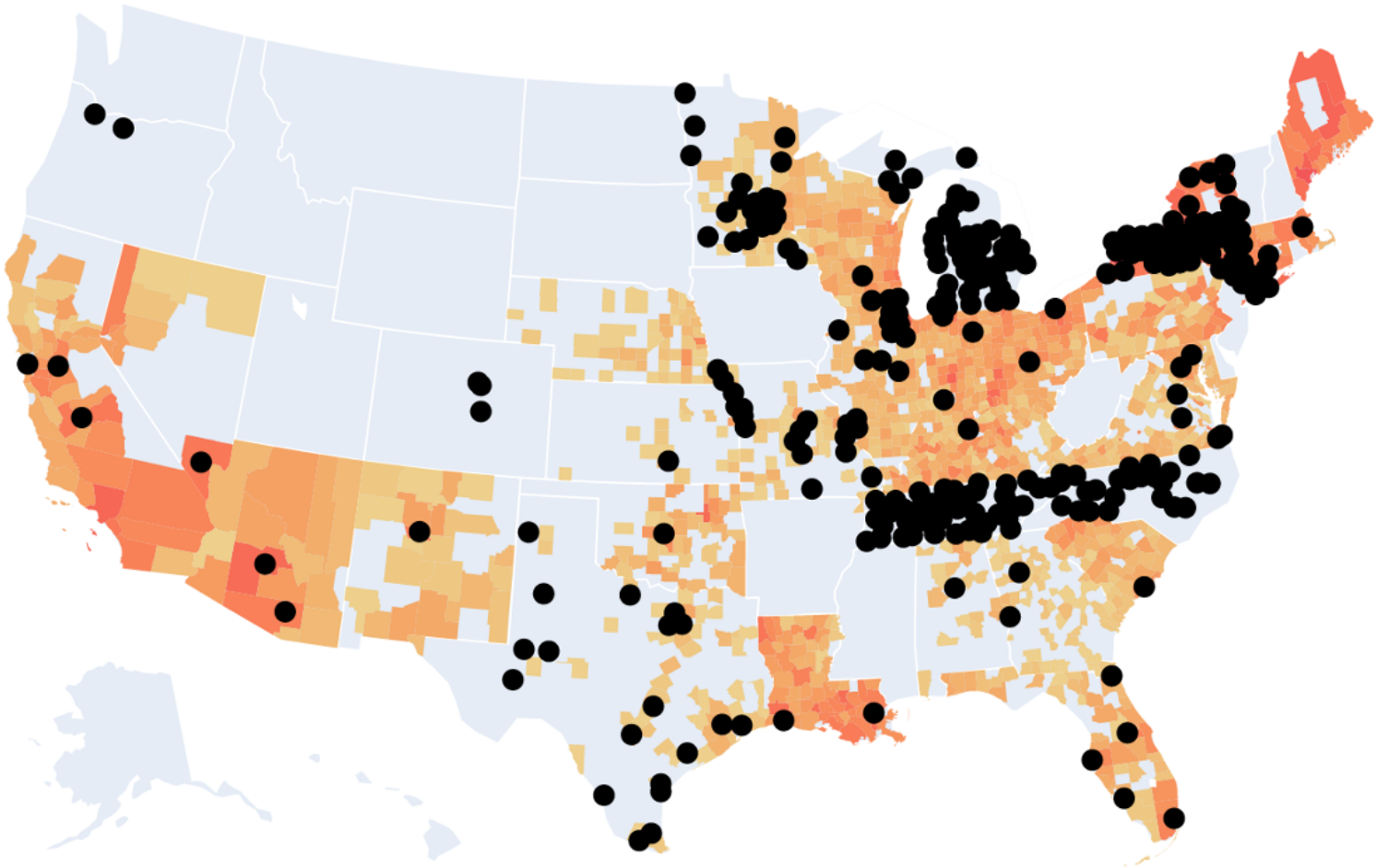
Figure F1: Florida DEO Annual Report Example

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

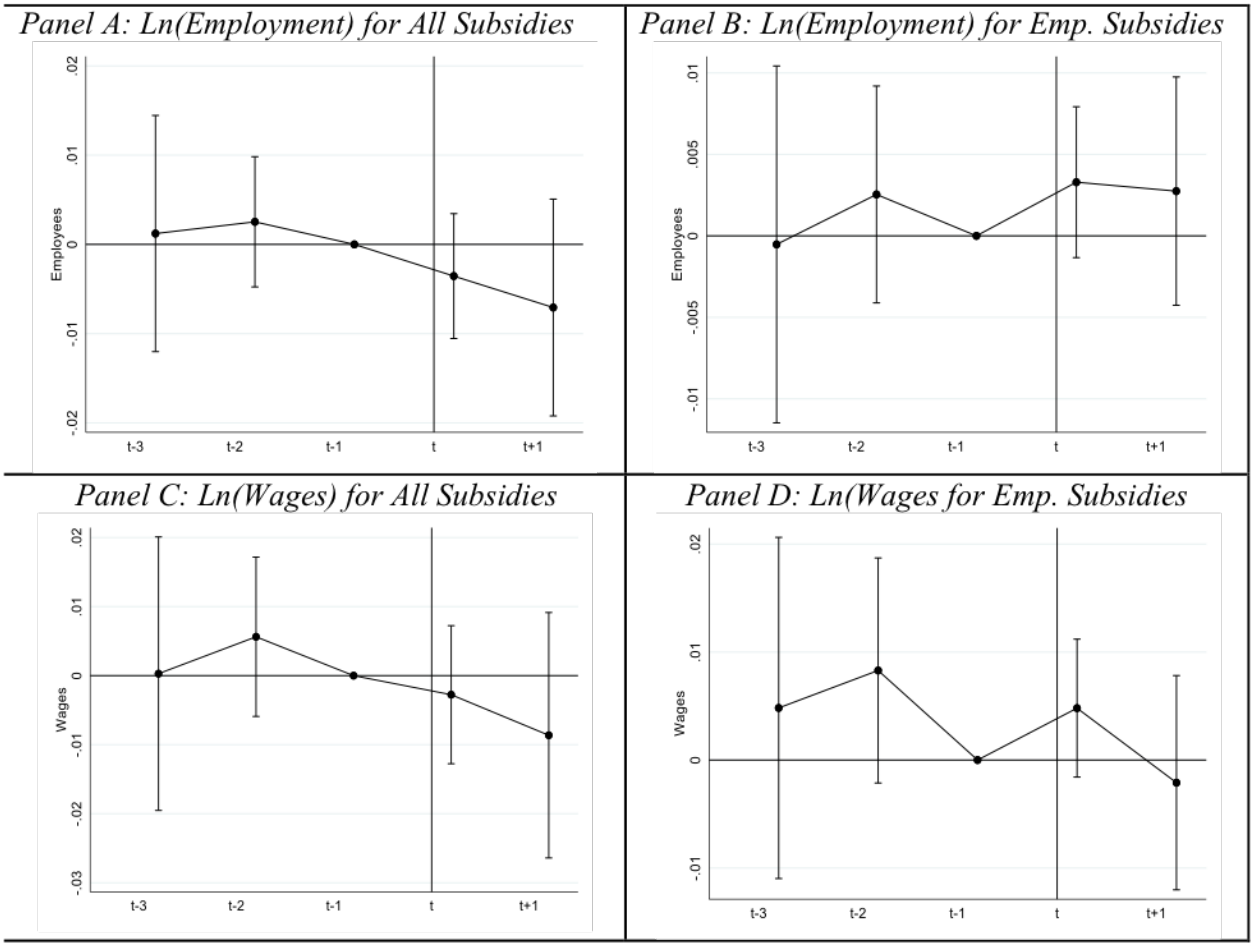
[25] http://www.floridajobs.org/business/EDP/EconomicDevelopmentIncentivesReport.pdf

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 B.

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 B.

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 B 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)$_{i,}$			**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 B 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 B 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 B 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 B 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 6
The Role of the Subsidy Information Environment in Subsidy Effectiveness

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 B 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***	-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**	-0.0033***	-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***	-0.2314***	-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***	-0.0059***	-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.