Tax Subsidy Information and Local Economic Effects

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We examine whether the relation between business tax subsidies and local economic activity varies based on publicly available subsidy information. State and local business incentives have tripled in size over the past thirty years (Bartik, 2019), now amounting to over 40 percent of total state corporate tax revenues (Slattery and Zidar, 2020). However, the subsidy grant process is notoriously opaque, and transparency problems inhibit clear assessments about whether subsidies achieve their intended outcomes. We find that public information about these incentives – measured based on the existence of state-level subsidy disclosure initiatives, the online publication of subsidy details, and ex ante job commitment disclosures – plays a critical role in subsidy effectiveness. Across all three measures, we observe that the positive association between subsidies and establishments occurs only in those counties with greater information. However, we find that employment effects occur only when the subsidies are accompanied by ex ante job commitments, signaling heterogeneity in the extent to which information facilitates job and wage growth. Our large-scale empirical analysis on the role of public subsidy information provides evidence of an important factor previously discussed, but not empirically tested, in public economics. Furthermore, we offer policy-relevant evidence for the increasing number of states implementing subsidy disclosure regimes.

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1. Introduction

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 underlying premise for granting subsidies is that targeted tax benefits will induce corporate investment and employment, thereby stimulating local economic growth through both direct and spillover channels. However, prior work suggests that a lack of transparency – about both the subsidy granting process and the *ex post* monitoring of subsidy recipients – is a central problem inhibiting subsidies from achieving intended outcomes (Greenstone and Moretti, 2004; Slattery, 2020; Slattery and Zidar, 2020; Slattery, 2021). This paper directly tests whether the relation between business tax subsidies and local economic activity varies based on publicly available subsidy information.

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, despite these findings, the literature suggests two important forces that mitigate or offset the business response. The first, examined in concurrent work, is that the subsidy granting process is inherently political in nature (Slattery, 2021; Aobdia et al., 2020). Second, the subsidy granting process is extremely opaque, with little information available about granting jurisdictions, recipient firms, amounts

[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).

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awarded, intended outcomes, and actual *ex post* realizations. This lack of transparency is exacerbated by the fact that subsidy giving is highly decentralized, often occurring across multiple state-level agencies and at thousands of local municipal and county levels. Concerns about transparency have become so great that governments have recently implemented subsidy disclosure regulations in an effort to improve local economic outcomes.

It is this last concern that we focus on in this paper. Specifically, we study whether information alters the relation between the incidence of a tax subsidy and the future number of employees, aggregate level of wages, and number of business establishments in the recipient's local jurisdiction. The prior accounting literature on disclosure motivates two important roles for information in achieving real local economic effects following subsidy receipt. First, better information *ex ante* during the subsidy granting process should result in a more efficient allocation of subsidies by granting agencies. Second, better public information about subsidies *ex post* can facilitate monitoring by relevant stakeholders, including both the granting agency and local taxpayers funding these subsidies. Indeed, several non-governmental groups have advocated for greater subsidy information on the basis that "sunshine is the best antiseptic" and that public information about subsidy awards is "essential to the process [of economic development accountability]" (Good Jobs First, 2007, p. 8 and 10). We therefore predict that any positive effects of subsidies on local economic outcomes are stronger for subsidies for which there is greater information.

However, there are a number of reasons we may not observe these effects. First, increased monitoring could induce subsidy recipients to make myopic decisions that result in short term, salient effects but at the expense of more optimal long-term investment and employment decisions. Second, subsidy disclosures could reveal proprietary information about the recipient's market and

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growth strategies, thereby reducing a firm's competitive position and economic presence. Third, more public information signals those jurisdictions willing to provide subsidies, potentially resulting in grants of more and more subsidies with declining marginal benefit to the local areas. Finally, greater information could result in recipients redirecting subsidy funds towards spending on other local outcomes, such as remediating local environmental or gentrification concerns. While such outcomes could have positive effects on the local community, they would not drive the effects studied in this manuscript. Collectively, all of these reasons suggest differing effects of information for the relation between business subsidies and local outcomes.

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

We start by validating that our sample of subsides exhibits the same positive relation between subsidy incidence and county-level employment activity 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, in addition to

[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), Slattery (2020, 2021) and Slattery and Zidar (2020).

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studying the largest subsidy awards ("megadeals") that were the focus of these earlier studies, we also include thousands of additional smaller awards. These additional awards include tax credits, tax abatements, tax reimbursements, grants, and tax increment financing, and enterprise zone benefits. We include these primarily because we expect subsidy information to play the greatest role in these smaller awards, as compared to the multi-million dollar megadeals that are naturally accompanied by substantial national press and public attention. Second, we can more directly assess the type of subsidies that are most effective in driving certain outcomes. In contrast to megadeals, which are designed to stimulate both investment and employment upon firm entry into a jurisdiction, our more expansive sample of subsidies permits refined tests that better match specific subsidy criteria (such as capital spending or worker training) to intended outcomes. Finally, we extend the set of examined outcomes to also include business establishments following other work that studies local economic effects of tax policies (Giroud and Rauh, 2019).

Counties with subsidy recipients have over 49,517 distinct subsidies during the sample period. While inclusion of a broad set of smaller awards allows us to examine those awards where information should play the greatest role, 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. Because we require counties to be observable for the entire sample period, the sample is a balanced panel of 10,104 county-years (1,263 counties) for the eight-year period from 2008 to 2015. This sample of county-years is distributed across 27 distinct U.S. states,

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with the most observations in Indiana, Kentucky, Ohio, and Virginia. Our resulting sample of 7,021 first-observed tax subsidies provide approximately $2.1 billion of benefits.

Results of our first validation tests show some evidence of a positive relation between subsidies and economic outcomes: subsidies are associated with a 1.18 (1.25) percent increase in the number of business establishments when including (excluding) megadeals in the sample. After exploiting detailed hand-collected data on subsidy criteria, we also find a positive relation between subsidy grant and employment outcomes, consistent with the relation observed in prior literature. We find that subsidies are associated with a 1.95 percent increase in employees and a 4.16 percent increase in aggregate wages. This implies that the median county has an increase of approximately 234-254 workers, which is reasonable when compared to the 1,500 employees found in Slattery and Zidar (2020) for larger subsidy awards.

Having validated our sample, we turn to examining our primary research question on the role of subsidy information. We use three measures of information: (i) whether the state had a subsidy disclosure regime in place at the time of the first observed subsidy, (ii) whether information about the subsidy was disclosed online, and (iii) whether the subsidy recipient was required to provide job commitments prior to subsidy receipt. Across all three measures, we observe that the positive association between subsidies and establishments occurs in those counties with greater information. However, employment effects occur only when the subsidies have *ex ante* job commitments, meaning that specific up front job disclosure plays an important role in the firm fulfilling its subsidy commitment. The variation across the information measures implies that the existence of state disclosure regimes alone may be insufficient to obtain the intended local outcomes.

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We acknowledge that our approach and data do not allow us to directly test the effects of subsidies at the firm- or establishment-level. This paper also does not consider general equilibrium effects of tax subsidies on employment levels and growth. For example, a tax subsidy in one county may impact supply or demand for labor, capital, or outputs in another county. Furthermore, we are unable to evaluate the local tax revenue net gain or loss associated with the granting of a tax subsidy, or other costs of these tax policies. Finally, our tests are conditional on observing indicia of a subsidy in the GJF data, where the details were obtained either via voluntary or mandatory disclosure, from local press coverage of an award, or by a FOIA request. 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 information 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). Results could vary based on the inclusion of other jurisdictions with relatively worse subsidy coverage.

Although the paper cannot address these open and interesting questions, we believe our study contributes towards understanding the role of information in mitigating concerns about the opaque process in which subsidies are granted and monitored. Prior and concurrent work on firm-specific subsidies discusses that the lack of transparency can have a substantial effect on business outcomes. For example, Slattery and Zidar (2020) state that "local discretion reduces transparency" and that "the lack of transparency in subsidy-giving leaves [the subsidy-granting process] more exposed to political capture and noneconomic allocation of funds" (p. 97). Not only is the subsidy-setting process opaque (Slattery, 2020), but the setting suffers from substantial

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information problems about whether subsidies achieve their intended outcomes *ex post* (Greenstone and Moretti, 2004; Good Jobs First, 2010; Pew Charitable Trusts, 2013). We provide empirical evidence on whether and what kind of information is most helpful in addressing these information problems. Furthermore, we demonstrate that subsidy information should be considered in future empirical studies that examine the broad and prevalent set of subsidies included in our sample. In addition to contributing to the literature on business tax subsidies, we also contribute to the broader literature on the role of state business climate characteristics (such as tax rates, state apportionment factors, characteristics of the tax base, etc.) in firms' production decisions (Papke, 1991; Hines, 1997; Goolsbee and Maydew, 2000; Holcombe and Lacombe, 2004; Chirinko and Wilson, 2008; Busso, Gregory, and Kline, 2013; Giroud and Rauh, 2019; Ljungqvist and Smolyanksky, 2015; Gale et al., 2015; Suarez Serrato and Zidar, 2016; Fuest et al., 2018).

By providing evidence on the role of the local information environment, we extend the accounting literature that has primarily focused on country-level information environments in firm investment (Francis, Huang, Khurana, and Pereira, 2009; Chen, Hope, Li, and Wang, 2011; Shroff, Verdi, and Yu, 2014; Loureiro and Taboada, 2015) and employment decisions (Engel, Gordon, and Hayes, 2002). Specifically, our work takes central findings from the accounting literature and applies them in a public economics context. In so doing, we add to the nascent accounting literature studying these subsidies, including the relation between subsidy magnitude and future firm performance (Drake, Hess, Wilde, and Williams, 2018) and the role of government capture in the likelihood of subsidized firms engaging in and getting caught engaging in accounting misstatements (Raghunandan, 2018).

Finally, the use of business tax subsidies is becoming increasingly controversial. Slattery

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(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. Indeed, 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, with the more extreme 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 the sample (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 specifically speaks to 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. The first relates to the prior literature in public economics: corporate tax rate reductions are often intended to stimulate investment by lowering the cost of capital. Prior literature offers evidence about the impact of tax rate reductions on investment by studying tax rate changes at the federal and state levels. More recent work also studies large firm-specific subsidies; because this work finds some evidence of average employment effects, the implication is that the smaller

[3] The New York legislation is the "End Corporate Welfare Act." The Kanas-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 also address cross-border competition, such as steps by the European Commission targeting illegal state aid via member state tax policies.

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subsidies we study should similarly motivate local economic activity, although to a lesser extent. We discuss this literature in Section 2.1.

The second observed phenomenon relates to a substantial movement towards increased information and transparency about business tax subsidies. Perhaps in recognition of the link between information and investment efficiency, many state governments have attempted to address concerns about subsidy effectiveness by implementing subsidy-specific disclosure regimes. We discuss the accounting literature on disclosure regimes in Section 2.2, which is the basis for examining our primary research question related to the role of information in subsidy effectiveness.

2.1 Real Effects of Subsidies

Firms should invest until the marginal product of capital equals the user cost of capital. Therefore, a reduction in a firms' cost of capital attributable to a decline in the corporate tax rate should lead to increases in corporate investment. 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 also finds similar effects when studying corporate employment, implying that on average investment and employment are complements.

Prior literature provides some limited evidence of this relation in the context of targeted business subsidies that reduce a firm's corporate tax burden (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

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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). The evidence from the earlier work on larger subsidies, coupled with the evidence in this other work, suggests that subsidies should drive some local corporate investment and employment. We build on this literature by being (to our knowledge) the first to empirically examine the role of information in subsidy effectiveness.

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2.2 The Role of Information in Subsidy Effectiveness

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

Although the disclosure literature primarily focuses on corporate information about firm financial performance, this literature more generally demonstrates two key roles of information, allowing investors to (i) improve their *ex ante* valuation of potential returns to an investment, and (ii) monitor the *ex post* use of committed capital by the recipient.[5] For example, voluntary disclosures provided by managers reduce mispricing ahead of an IPO (Schrand and Verrechia, 2005; Leone, Rock, and Willenborg, 2007), improve analyst forecasts and reduce forecast dispersion (Bowen, Davis, and Matsumoto, 2002), reduce information asymmetry (Brown, Lo, and Hillegeist, 2004), and increase monitoring by shareholders (Bens, 2002). Further, a wide stream of the disclosure literature specifically links reductions in information asymmetry to more efficient capital allocation, thus driving positive real investment and employment outcomes.[6] For example, Badertscher et al. (2013) document industries with greater public firm presence are more

[4] For example, a 2017 Pew study entitled *How States are Improving Tax Incentives for Jobs and Growth* states that "with better information, lawmakers can make better decisions. State officials are using evaluations to help identify incentives that are working well and reform those that are not. As a result, evaluations are helping states save millions of dollars while achieving stronger economic results" (p. 22). Good Jobs First states that "[t]axpayers have a right to know about the ways in which their money is being diverted to private parties. Transparency is not an abstract principle; it helps to promote integrity and efficiency in the awarding of subsidies" (p. 3 in the 2010 and 2014 studies).
[5] For a review of this literature, see Beyer, Cohen, Lys, and Walther (2010).
[6] For a review of this literature, see Roychowdhury, Shroff, and Verdi (2019).

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responsive to investment opportunities due to greater information from the publicly disclosing firms. Shroff et al. (2014) find that greater transparency within a foreign subsidiary location permits better monitoring, thereby increasing the firm's responsiveness to local growth opportunities. Cho (2015) finds improved capital allocation within multi-segment firms following an accounting disclosure change.

By extension, this literature suggests that increased disclosures about subsidies could play an important role in driving local economic outcomes. A better information environment surrounding these subsidies permits both a better *ex ante* evaluation of the highest return projects a government should "invest" subsidy tax dollars in and better *ex post* monitoring of the use of granted funds to achieve targeted outcomes. We therefore expect increased information about subsidies to facilitate investment and employment responses through both channels.

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

Despite the potential benefits of information in the subsidy granting process, this process

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has traditionally been opaque. In the early 2000s, there was little public information about how a firm could even request such subsidies. Even now, many local websites provide little formal information about the application process. We are unable to observe private information provided by firms to granting agencies during the subsidy granting process. In the absence of such data, we study variation in the existence and type of information that is available related to the firm recipient, the recipient's intended investment, and subsidy details to measure the subsidy information environment.

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

Based on the findings of the accounting disclosure literature, we predict the following:

H1: The positive relation between subsidies and investment and employment outcomes is stronger for subsidies with greater public information.

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However, it could be the case that public information plays no role in subsidy effectiveness for at least two reasons. First, we focus on information that is publicly available to stakeholders outside the recipient firm or granting agency. If the granting agency is the primary user of the information, for both monitoring firm recipients and assessing future subsidy grants, it is possible that only information possessed privately by the granting agency is important for subsidy effectiveness. Second, any positive effects of information may be delayed beyond our sample window.

There are also a number of reasons subsidies could be *less* effective in the presence of greater public information. First, increased monitoring resulting from more public information about subsidies could induce granting agencies or subsidy recipients to make myopic investment decisions that benefit the grantor, sacrificing higher long-term local economic growth for more immediate "results."[7] For example, more public information about granted subsidies could result in granting jurisdictions choosing the most politically palatable grant recipients, potentially at the expense of lower expected outcomes from the project.[8] In concurrent work, Aobdia et al. (2020) and Slattery (2021) examine some of these effects in the tax subsidy setting.

Second, subsidy disclosures could reveal proprietary information about the recipient's market or growth strategy, for example, undercutting the subsidy recipient's competitive position and resulting in worse local economic outcomes (Graham, Harvey, and Rajgopal, 2005; see also

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

[8] Officials in Missouri often emphasized the importance of subsidizing incumbent companies to preserve existing jobs, even though several subsidized establishments ultimately failed (Mhire, 2011; Famuliner, 2013). Furthermore, prioritizing job retention could come at the cost of foregoing higher-return projects from outside companies. As another example, Texas Governor Rick Perry touted his ability to use tax incentives to "win" head-to-head bids against other states to lure outside companies to Texas, though some argue these politically popular "wins" were not worth the costs (Story, 2012). North Carolina and Illinois have been criticized for granting a disproportionate amount of subsidies to larger public firms, even though these firms were investing in metro areas that arguably would benefit less relative to other communities (Chaney, 2016; Craighead and Manzo, 2017).

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Appendix A in Heinle, Samuels, and Taylor, 2018). On this basis, business groups have pushed back on disclosure regimes, raising public officials' concerns that the private sector would view disclosing jurisdictions to be unfriendly to business (Good Jobs First, 2010 and 2014).

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

Finally, greater public information could lead to less effective subsidies by causing some of the granted funds to be spent on outcomes other than capital investments and employment. For example, constituents and special interests such as community or environmental activism groups may exert pressure on the granting jurisdictions or the recipients themselves to use some granted funds to remediate local concerns (e.g., local school funding, gentrification, homelessness, waste, and preserving natural habitats of native species); relatedly, the company itself may divert funds to support local organizations as a way to convey that the firm is a good corporate citizen. While these effects could have positive outcomes for local jurisdictions, they may not necessarily manifest in the three outcomes we study.[9] Thus, despite increased available information about subsidies, there is little evidence of the extent to which the effectiveness of subsidies varies with public information about these awards. We aim to fill this gap with our empirical tests.

[9] For example, in a press release announcing the opening of a facility partly funded by a North Carolina subsidy, MetLife described how the company had made grants exceeding $2.0 million to support a number of community programs (Metlife, 2017). As another example, Samsung donated a portion of its property tax subsidy back to a Texas school district following public concerns over local tax breaks used to lure Samsung to the state (Story, 2012).

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3. Research Design

3.1 Validation and Extension of Earlier Models

We begin by validating that the more expansive sample of subsidies we study for purposes of testing our research question exhibits similar effects as that in the prior literature. Prior research generally finds that 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 tax subsidies and employment outcomes aggregated across all firms at the county level. We also test establishment outcomes following other related work (Giroud and Rauh, 2019).

To test these effects in our setting, we first show that establishments and employment are related to the incidence of a subsidy. We estimate staggered difference-in-difference regressions (Stevenson and Wolfers, 2006) of the following form:

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

Subscript *i* denotes the county, *j* denotes the jurisdiction, and *t* denotes year. The dependent variable $EconomicActivity_{i,t+1}$ represents the number of business establishments, the number of employees, or the amount of aggregate wages in a specific county; we discuss these measures below in Section 3.3. $PostSubsidy_{i,t}$ is an indicator equal to one for county-year observations with at least one company receiving a tax subsidy in years including and following the year of subsidy grant, and zero otherwise. Thus, our primary tests analyze the incidence of the first-observed tax subsidy.[10] Subsequent tests refine the indicator based on the criteria of the program, which we

[10] Directly examining the relation between subsidy amount and subsequent economic activity is difficult in our setting 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

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identify based on reading the description of each subsidy program included in the data. 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 economic outcomes ($\beta_1 > 0$), although the strength of the association may be diminished given that we study relatively smaller awards. These validation tests intend to show that subsidies specifically targeting capital expenditures lead to increases in the number of establishments and, similarly, that subsidies targeting employment lead to increased local employment and wages.

We include a set of control variables ($Controls_{i,j,t}$) motivated by prior literature, further discussed in Section 3.5. Finally, we include county and year fixed effects as proxies for potentially unobservable changes in local economic conditions. The county fixed effects control for mechanical differences associated with economic activity, whereas year fixed effects control for nationwide macroeconomic trends (such as the 2008-2009 financial crisis) that affect state and county fiscal conditions. Main effects for counties having at least one subsidy recipient and for years in which subsidies are granted are captured by the county and year fixed effects. We cluster standard errors by county.[11]

3.2 Hypothesis Test

$$EconomicActivity_{i,t+1} = \alpha + \beta_1 PostSubsidy_{i,t} + \beta_2 Information_{i,t} * PostSubsidy_{i,t} + Controls_{i,j,t} + County_i + Year_t + \varepsilon \quad (2)$$

To test our hypothesis that the relation between subsidies and investment and employment outcomes are stronger in those areas with greater public information, we augment Eq. (1) to include $Information_{i,t}$, which is an indicator equal to one for subsidy counties with public information about subsidies that are granted; see Section 3.5. We construct this measure based on the available

each award by year.

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

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information in the first year that the first subsidies are observed in a jurisdiction. Thus, because this measure is not time-varying and because we estimate Eq. (2) with county- and year-fixed effects, the main effect of $Information_{i,t}$ is subsumed. The interaction term captures whether there is a differential relation between subsidies and the economic outcomes we study in the presence of greater public information about the subsidies; we predict $\beta_2 > 0$.

3.3 Measures of Economic Activity

We obtain the number of establishments, 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.[12] From the quarterly filings, we obtain a county-level calendar-year average, computed by the BLS, of the number of establishments, number of employees, and aggregate annual wages paid. We take the logarithm of these amounts to construct the dependent variables $Ln(Establishments)_{i,t+1}$, $Ln(Employees)_{i,t+1}$, and $Ln(Wages)_{i,t+1}$. Because the subsidy data capture subsidies to private-sector firms, we exclude public-sector employees and wages from our measures following other work (Dube, Lester, and Reich, 2010). We include two measures of employment because the expected positive relation could arise either through the hiring of more employees or through higher aggregate wages to existing employees (or both).[13]

3.4 Subsidy Data

GJF data is available publicly on the organization's webpage. However, because certain

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

[13] One possible outcome is that we observe more employees in a jurisdiction but lower aggregate wages if the newly employed individuals are hired for lower-paying jobs. Alternatively, we may observe no change in the number of employees but an increase in wages, signaling that the subsidy was used to possibly retain workers at the establishment or to increase executive compensation.

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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.

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 those 27 states in which we consistently observe data on specific subsidy programs, implying a relatively complete collection of requisite information. Within these states, there is substantial variation in the extent of information that is provided, particularly because a large amount of subsidy information was obtained via FOIA requests that GJF filed with local authorities. That is, much information in Subsidy Tracker was not directly provided via voluntary or mandatory requirements.

Given the prior and concurrent literature's focus on the largest subsidy packages, we estimate tests with and without megadeals, a term coined by GJF to reflect the largest deals based on dollar value and which span multiple categories of subsidy type.[14] Use of both samples permits

[14] Good Jobs First defines megadeals as those packages that generally exceed $50-$75 million in value. Given the large dollar values of these incentives relative to other subsidies in our sample, the number of other studies focused on these large subsidies, and the overall high level of press coverage and public attention related to these awards, we exclude these subsidies from some samples. See Section 4.

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an assessment of whether those deals exhibit the same employment effects as that documented in the prior literature and the extent to which these effects vary based on the type of the subsidy award.

We aggregate subsidies to the county-year level using addresses of subsidy recipients provided by GJF. If a subsidy was awarded at the state level, we assign the subsidy to the county in which the recipient firm is located. We hand-match subsidy data to BLS establishment and employment data at the county-year level.

3.5 Measures of Public Information

$Information_{i,t}$ is one of three indicators for the availability of public subsidy information, all of which relate to state-level subsidy programs. First, we study the effect of state-level subsidy disclosure initiatives. We focus on this channel because it has been the most prevalent step taken by governments to address public concerns about subsidy effectiveness. We identify whether a state had a disclosure initiative in place based on listings compiled by GJF in three different studies published in 2007, 2010, and 2014.[15] $DisclosureRegime_{i,t}$ is an indicator equal to one for county-year observations in which the first subsidy was subject to disclosure requirements, and zero otherwise.

Second, $Online_{i,t}$ is an indicator equal to one (and zero otherwise) if information about all subsidy recipients in a given county is made available online. We identify this based on whether the GJF data include a website link for the information about the subsidy, as opposed to an indication from GJF that the subsidy was obtained through a specific FOIA request by GJF. The

[15] 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.

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assumption when using this measure is that those subsidies posted online reflect jurisdictions with the most readily available information for public consumption.

The third measure examines whether subsidy effectiveness varies based on the provision of an *ex ante* estimate by the recipient firm to the granting jurisdiction about the number of jobs the subsidy is expected to create. Although this may originally have been a private disclosure by the subsidy recipient to the government, we observe that nearly all subsidies that required an *ex ante* job disclosure were also made available publicly online. Thus, this type of job disclosure reflects the granting jurisdiction's requirement to estimate and quantify the expected job growth, as well as their preference to make these *ex ante* commitments public. $ExAnteJobs_{i,t}$ is an indicator equal to one (and zero otherwise) if all subsidy recipients in the county were required to provide an *ex ante* estimate of the number of jobs that would be created as a result of the subsidy. The online appendix also presents results using a fourth measure of local information: the presence of a local newspaper (Gentzkow et al., 2011; Gao et al., 2019). While this measure captures the amount of local information in a community, it does not specifically capture business subsidy information and thus is not tabulated in the main text. See Online Appendix Table 2.

3.6 Control Variables

We control for determinants of employment and location decisions. These control variables include the minimum wage in each county (e.g., Neumark and Wascher, 2007).[16] We obtain county minimum wage data from the U.C. Berkeley Center for Labor Research and Education. We also control for state GDP level, measured using aggregate data from the Bureau of Economic Analysis, and county population, measured using data from the U.S. Census Bureau's American FactFinder.

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

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Additional control variables include the percentage of private-sector employees that are union members (e.g., Card, 1996) and educational attainment, measured as the percentage of the population with at least a bachelor's degree (e.g., Card, 1999). We obtain data for both of these measures from the Census Bureau's American Community Survey. Following Giroud and Rauh (2019), we include a number of additional state-level tax variables, including the state's top marginal corporate tax rate, the personal income tax rate, the property tax share, the log of the state unemployment insurance contribution, the tax incentives index, and the sales tax rate to account for other state tax policy tools that may complement or substitute for tax subsidies.[17]

3.7 Control Group

A natural concern when studying these smaller subsidies is how to construct the appropriate group against which to compare the 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 information 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*. Specifically, we benchmark the business activities of county-years with subsidy recipients against counties 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. Thus, our tests measure the effect of subsidies on business activity relative to the same counties prior to receiving the subsidy and other subsidy recipient counties that have not yet received a subsidy.

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

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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 additional unobservable characteristics. By mitigating the selection concern, these tests permit a better assessment of the role of firm information within the sample of subsidy recipients. Second, it 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 an approach used in recently published work in public economics, thereby extending this identification method to the accounting literature (Fuest et al., 2018).

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 information 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 establishment and employment data for the eight years from 2008 to 2015 (n=25,216 observations for 3,152 counties). 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

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states with inconsistent subsidy coverage in the dataset(n=8,568).[18] This step leaves 27 states for which we observe consistent subsidy information throughout the sample period. Because we assume that the subsidy information is complete for this set of states, we drop the 818 counties that never have a subsidy recipient, resulting in a sample of 1,263 distinct counties (10,104 county-year observations). Although we include megadeals in our initial validation tests to permit a better comparison to prior work, we drop the 87 counties (696 county-year observations) in our main tests for the role of local information given their extreme size. The remaining 1,176 distinct counties have at least one tax subsidy in 5,596 county-year observations out of the total 9,408 observations for these counties that are included in the final sample. Table 1, Panel B provides the distribution of county-year observations by state. The states with the most observations for tax subsidy recipients include Indiana, Kentucky, Ohio, and Virginia.

Table 2, Panel A provides descriptive statistics for key variables used in empirical tests for the sample of state-level subsidy county-years, both with and without megadeal subsidies. In the sample including (excluding) megadeals, the average county has approximately 4,720 (3,524) business establishments. Similarly, the average county reports approximately 62,213 (46,810) employees with aggregate wages of approximately $3.14 ($2.22) billion annually. All of these amounts are skewed, with counties in the 99th percentile including (excluding) megadeals reporting over 54,595 (50,737) establishments, 720,455 (604,893) employees, and $43.5 ($34.0) billion of aggregate wages (untabulated). The average minimum wage for the sample period is $7.26/hour.

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

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Approximately 21 percent of the working-age population has at least a bachelor's degree, and 6.4 percent are union members. The average state corporate and personal income tax rates are 6.5 and 5.3 percent, respectively.

The remainder of this table describes the sample excluding the counties with megadeal recipients. Panel B provides sample composition by type of subsidy. We first show descriptive statistics on the 1,176 distinct county-years with first-observed subsidies granted during our sample period, as these are the primary subsidies we examine in empirical tests. We observe 7,021 distinct first-observed subsidies in this sample. Over 6,200 of these first-observed subsidies include data on the dollar value of the award, aggregating to almost $2.1 billion in total. Although we focus on the first-observed subsidies in each county, Panel B also provides information on all subsidies reported by counties with subsidy recipients. The average magnitude of the 46,310 subsidies in our sample that report a dollar value is $324,500. While the average subsidy is much smaller in dollar value as compared to the very large subsidies studied in prior work (e.g., $149.5 million in Slattery (2020) and $178.4 million in Slattery and Zidar (2020)), they are much more prevalent with almost 85 times the number of awards. In total, 5,596 county-years in our sample report over 49,000 distinct tax subsidies, with approximately 8.75 average annual subsidies each year worth approximately $2.67 million.[19] 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 tax subsidies by year based on the stated criteria of the

[19] 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.

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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. These categories are not mutually exclusive, such that a single subsidy can entail spending by a firm on (for example) both investment (capital equipment, establishments, etc.) and employment (hiring, job retention, wages, and worker training, etc.). The "Other" subsidies include a variety of categories such as infrastructure investment, innovation activity, film tax credits, and historic rehabilitation credits.

4. Main Results

4.1 Validation and Extension of Prior Work

Table 3 presents results from estimating Eq. (1). Columns (1), (3), and (5) report results using the sample of 10,104 county-year observations, including the 696 observations (87 counties) with megadeal subsidies. Columns (2), (4), and (6) present results for the sample of 9,408 county-year observations excluding megadeals. We observe limited evidence of a positive relation, primarily for $Ln(Establishments)_{i,t+1}$ in Columns (1) and (2). The coefficients of 0.0118 and 0.0125 in Columns (1) and (2), respectively, imply a 1.18-1.25 percent increase in establishments following subsidy receipt, suggesting that state-level subsidies on average throughout the post-tax subsidy period are associated with new facilities or offices in the county. Given that the average number of establishments in subsidy counties in the year prior to the first-observed tax subsidy is 4,639 (untabulated), this coefficient implies an increase of 54.7-57.9 establishments following subsidy receipt. However, we observe statistically insignificant effects for employees and wages across both samples, implying that the results (or lack thereof) do not appear to be driven by counties with megadeal recipients. This lack of employment effect is inconsistent with earlier studies (Greenstone and Moretti, 2004; Greenstone, Hornbeck, and Moretti, 2010; Slattery and Zidar, 2020), suggesting that either the expanded sample of smaller subsidies does not exhibit the

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same effects as in prior literature, or that the use of the *PostSubsidy* indicator is not measured precisely enough to detect effects.

To assess this issue, we refine the initial tests to more precisely match the criteria of specific programs to their intended outcomes. Specifically, we extend Eq. (1) to include subsidy indicators identifying the stated criteria of each subsidy based on a manual review of subsidy information. Because this test compares counties with a firm recipient of a particular type of subsidy (for example, an employment subsidy) against both (i) other counties with firms that later receive that particular type of subsidy (an employment subsidy), and (ii) counties with firms that never receive that particular type of subsidy (instead, they receive an investment or a historical rehabilitation subsidy), this research design also addresses potential bias induced by using a sample of treatment counties (Baker, Larcker, and Wang, 2020).

We present results of this refinement of the validation tests in Table 4. We now find more evidence of the expected positive relation between subsidies and outcomes when we refine our tests to match subsidies with their stated criteria. In Columns (1) and (2) examining $Ln(Establishments)_{i,t+1}$ as the outcome, we observe positive and significant coefficients on $PostInvestmentSubsidy_{i,t}$. The magnitude of this coefficient suggests subsidies granted based on the stated criteria of increasing investment lead to a 1.07-1.14 percent increase in the number of establishments, or approximately 49.6-52.9 businesses.

In Columns (3) through (6), we observe positive and significant coefficients in the state subsidy sample on $PostEmploymentSubsidy_{i,t,}$ when $Ln(Employees)_{i,t+1}$ and $Ln(Wages)_{i,t+1}$ are the outcome measures. The magnitudes of these coefficients suggest subsidies granted based on the stated criteria of increased employment lead to a 1.93-1.95 percent increase in number of employees and a 3.99-4.16 percent increase aggregate wages. Evaluated based on the median

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county, this suggests an increase of 234-254 workers and $17.2-$19.5 million in total wages. The employment affect appears reasonable given that much larger megadeal packages are associated with an increase of 1,500 workers based on Slattery and Zidar (2020). By hand-collecting data on the underlying criteria of the program, we are able to better match the underlying subsidy type with the intended investment and employment outcome and thus more accurately estimate the effects of these subsidies on the intended outcomes.

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

4.2 The Role of Information in Subsidy Effectiveness

We next examine our main research question on the role of public information in subsidy effectiveness. Table 5 presents results of estimating Eq. (2) on the non-megadeal sample. In each panel, we explore one of three different measures of public information: the presence of a disclosure initiative (Panel A), the availability of subsidy information online (Panel B), and the *ex ante* provision of an estimate of job creation by the subsidy recipient to the granting agency (Panel C). In each panel, we present results for $Ln(Establishments)_{i,t+1}$ as the outcome variable in Column (1), $Ln(Employees)_{i,t+1}$, in Column (2), and $Ln(Wages)_{i,t+1}$ in Column (3).

In Panel A, we find that the relation between subsidy receipt and $Ln(Establishments)_{i,t+1}$ is different in the counties where subsidies are subject to a state disclosure initiative. Specifically, we find an insignificant coefficient of 0.0050 on the main effect of $PostSubsidy_{i,t}$, but a positive and statistically significant coefficient of 0.0129 on $PostSubsidy_{i,t}$*$DisclosureRegime_{i,t}$. This result suggests subsidies are only effective in stimulating corporate investment (in the form of new

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establishments) when information about the subsidies is mandated under state reporting requirements.

In Figure 1, we plot the residual values from estimating Eq. (2), with Panel A demonstrating the differential effects for those subsidies subject to disclosure regimes (dashed lines) as compared to no disclosure regime (solid line). We measure these effects for the first subsidy receipt in a county in year *t*, with all effects estimated relative to year *t-1*. The graph in Figure 2, Panel A demonstrates there is no statistically significant difference in the number of establishments across subsidy counties based on disclosure regimes in the three years preceding subsidy grant. We observe that those counties for which the subsidies are subject to a disclosure regime appear to have a higher number of establishments in year *t+1*, suggesting a relation between disclosed information and the number of establishments after subsidy grant. However, while we continue to observe a divergence in the other post-period years, the effects do not appear to be statistically significant.

Columns (2) and (3) of Table 5, Panel A suggest little role of these disclosure regimes in generating the necessary employment outcomes; we observe no statistically significant difference based on those areas where recipients are subject to reporting requirements. This lack of effects is observed in Panel A of Figures 3 and 4 for $Ln(Employees)_{i,t+1}$ and $Ln(Wages)_{i,t+1}$, respectively.

In Table 5, Panel B, we find the same pattern of results when examining variation based on whether the subsidy information was online. Specifically, we estimate a positive and significant coefficient on $PostSubsidy_{i,t}$*$Online_{i,t}$, indicating that subsidies with information posted online are positively associated with number of establishments. Figure 2, Panel B demonstrates the divergence across information similar to that observed with $DisclosureRegime_{i,t}$.

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Table 5 Panel B, Columns (2) and (3) again suggest little role of online information in the effectiveness of subsidies at generating employment outcomes. For example, in Column (2), we observe an insignificant coefficient of -0.0104 on the interaction term, and we similarly see no statistically significant effect in Column (3) when testing wages. Panels B of both Figures 3 and 4 depict this graphically, showing overall lower employment across both samples. While there are lightly higher levels of employment and wages in years *t+3* and *t+4* in jurisdictions where there is online disclosure, the divergence is not statistically different.

Table 5, Panel C, presents results with the third measure of local information, *ex ante* job disclosure. In Panel C, Column (1), we observe a coefficient of 0.0395 that again suggests an economically significant role of information in subsidy effectiveness for $Ln(Establishments)_{i,t+1}$. Figure 2, Panel C confirms that the difference in establishments appears to be present much more quickly when refining the measure of $Information_{i,t}$ to those subsidies with *ex ante* job commitments. This implies that the provision of more up front and precise information is associated with more pronounced and earlier investment effects.

Table 5, Panel C, Columns (2) and (3) present a similar – albeit statistically weaker – pattern of results when evaluating the role of information in the effectiveness of subsidies at generating employment outcomes. We observe a positive coefficient on $PostSubsidy_{i,t}$*$Information_{i,t}$ of 0.0261, with a t-statistic of 1.60. While not statistically significant at a two-tailed level, Figure 3, Panel C suggests an earlier and more persistent divergence across effects for subsidies with *ex ante* job disclosure. We observe similar effects in Column (3) when studying $Ln(Wages)_{i,t+1}$– a positive coefficient of 0.0550, with a t-statistic of 1.59. These results support the inference that subsidies are most effective when provided along with public information about the subsidy; however we find that the effects diminish as we shift from

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investment outcomes to the two employment outcomes, with the weakest results related to wages.

Finally, we aim to provide more insight regarding the two possible information channels that could be driving subsidy investment and employment outcomes: *ex ante* selection of grant investment and *ex post* monitoring of invested funds. To do so, we use the two measures of information that are the most granular and, thus, most representative of the existence of disclosed information: $Online_{i,t}$ and $ExAnteJobs_{i,t}$. Because *ex ante* jobs estimates are nearly always also disclosed online, we are limited to examining whether *ex ante* jobs disclosures provide incremental explanatory power over online disclosure when examining the effectiveness of subsidies on our outcomes of interest. Specifically, we re-estimate Eq. (2) after including both $Online_{i,t}$, an indicator equal to one if an *ex post* disclosure about the subsidy (that facilitates monitoring) is made available online, and a new indicator variable $OnlineJobs_{i,t}$, which is equal to one if the online disclosure includes an *ex ante* estimate of job creation (that facilitates selection of grant recipients).[20] In untabulated tests across all three outcomes, we estimate a positive and statistically significant coefficient on $OnlineJobs_{i,t}$ but insignificant coefficients on $Online_{i,t}$. These results imply that e*x ante* jobs disclosures used to facilitate the grantor's investment decision are positively correlated with subsidy investment and employment outcomes. In contrast, these results are inconsistent with any *ex post* monitoring of subsidy information provided online facilitating subsidy investment and employment outcomes.

In summary, the tests in this section suggest that establishments and, to a lesser extent, employment outcomes, occur when there is more publicly available information about subsidies. The relative strength of results when evaluating establishment outcomes mirrors the relative strength of establishment results presented for our baseline validation tests in Table 3. Similarly,

[20] To ensure this test is properly specified, we set $OnlineJobs_{i,t}$ equal to zero if $Online_{i,t}$ equals zero such that $OnlineJobs_{i,t}$ represents the interaction of $Online_{i,t}$ with an indicator for the existence of an *ex ante* jobs estimate.

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the relative weakness of results when evaluating employment outcomes is not surprising given the lack of evidence that subsidies affect employment presented in Table 3. Future tests will further examine whether the relations documented vary based on the quantity of subsidies in a county and the relative dollar amount of the incentive packages.

5. Conclusion

We examine the role of information in the relation between firm-specific subsidies and local area investment and employment. We first validate our expanded sample of thousands of state and local firm-specific subsidies by demonstrating that these subsidies exhibit the similar positive association between subsidies and employment as prior literature. To do so, we collect data on the different criteria for each subsidy program to best match each subsidy with intended local outcomes. The validation tests confirm that subsidies granted based on investment criteria are associated with new establishments, and similarly that subsidies granted based on employment criteria are positively associated the number of employees and aggregate wages.

Our main finding is that the local information environment plays an important role in subsidy effectiveness. Motivated by the prior accounting literature on information, disclosure, and monitoring, our results are consistent with public information about subsidies improving establishment outcomes, and *ex ante* job commitments (weakly) improving the number of jobs and aggregate wages.

Our results are subject to several caveats related to our inability to measure firm-level outcomes, the inobservability 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 tax subsidies on employment

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

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Appendix A
Variable Definitions

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

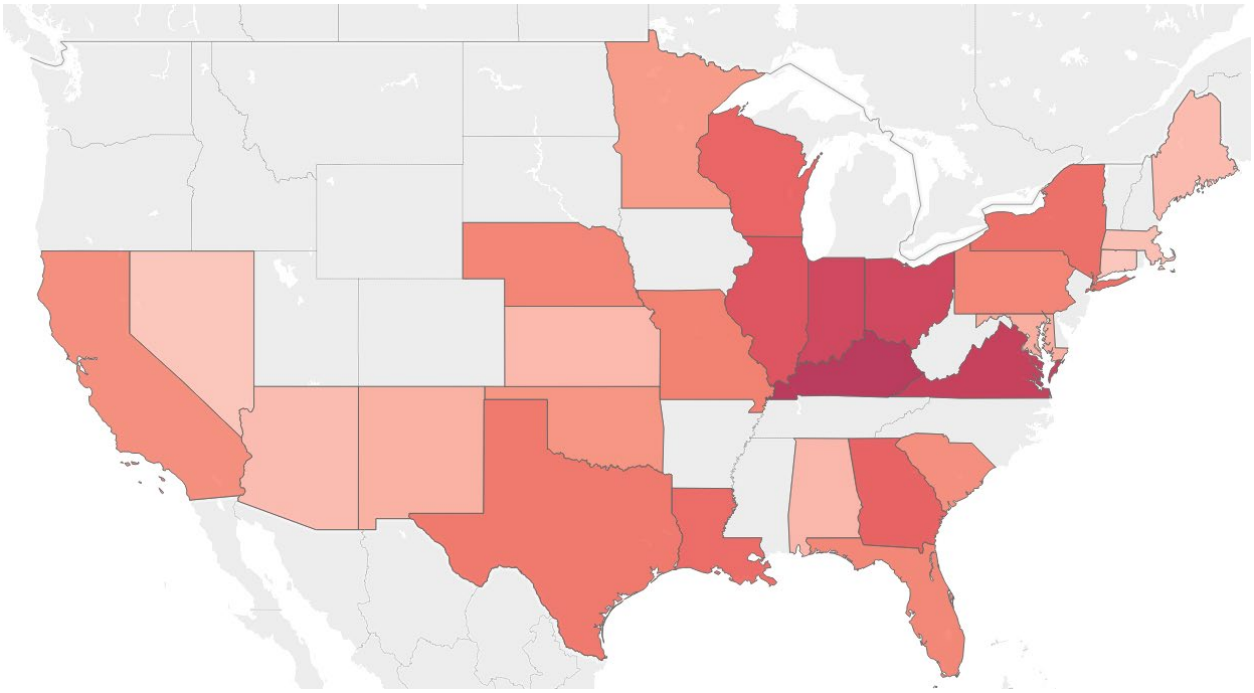
Variable	Definition
Dependent Variables	
$Establishments_{i,t+1}$	Total number of private-sector business establishments in county i in year t+1 relative to obtaining a subsidy in year t; computed by the Bureau of Labor Statistics (BLS) as the average number of active establishments at the end of each quarter.
$Employees_{i,t+1}$	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.
$Wages_{i,t+1}$ *($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	
$PostSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy received by a firm in county i and 0 otherwise.
$PostEmploymentSubsidy_{i,t}$	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.
$PostInvestmentSubsidy_{i,t}$	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.
$PostOtherSubsidy_{i,t}$	Equals 1 for years including and following the first-observed subsidy targeting goals other than increased employment or capital 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.
$Information_{i,t}$	Equal to $DisclosureRegime_{i,t}$, $Online_{i,t}$, $ExAnteJobs_{i,t}$, or $Newspaper_{i,t}$, all defined below.
$DisclosureRegime_{i,t}$	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.
$Online_{i,t}$	Equals 1 for county-years with first-observed subsidies for which there is public information available online and 0 otherwise.
$ExAnteJobs_{i,t}$	Equals 1 for county-years with first-observed subsidies for which there is public information about expected job creation and 0 otherwise.
$Newspaper_{i,t}$	Equals 1 for county-years that have a local newspaper according to newspaper closure data from Gao et al. (2019) and 2019 data on local newspapers from Editors & Publishers, and 0 otherwise.
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.
$MinWage_{i,t}$	Minimum wage applicable to county i in year t. In most cases this is the state-mandated minimum wage or federal minimum wage; at varying points between 2004 and 2015, seven counties introduced their own minimum wage that supersede the state's minimum wage.
$Ln(GDP)_{j,t}$	Log of total GDP in year t for state j.

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$\%Educ_{j,t}$	Percentage of people in state *j* with at least a four-year college degree in year *t*.
$\%Union_{j,t}$	Percentage of private-sector employees in state *j* who are union members in year *t*.
$CorpTaxRate\%_{j,t}$	Top marginal corporate state tax rate for state *j* in year *t*.
$Ln(UIContrib)_{j,t}$	The log of top unemployment insurance (UI) tax rate multiplied by the maximum base wage (i.e., the maximum amount of wages taxable for UI purposes) for state *j* in year *t*.
$PropertyTax_{j,t}$	Ratio of total property taxes (collected by state and local governments) to total revenues (collected by state and local governments) for state *j* in year *t.*
$PersonalTaxRate\%_{j,t}$	Top marginal personal *state* income tax rate for state *j* in year *t*.
$TaxIncentivesIndex_{j,t}$	Index of tax incentives potentially available to businesses that locate in/relocate to state *j* in year *t* as compiled by *Site Selection* magazine. There are 33 possible incentives; this variable adds one index point for each incentive (e.g., if state *j* could offer 22 incentives in year *t*, this variable equals 22).
$SalesTaxRate\%_{j,t}$	Sales tax rate assessed by state *j* in year *t* (does not include any additional sales tax that may be collected by county *i*).

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Figure 1
Map of Number of Subsidy Counties in each State

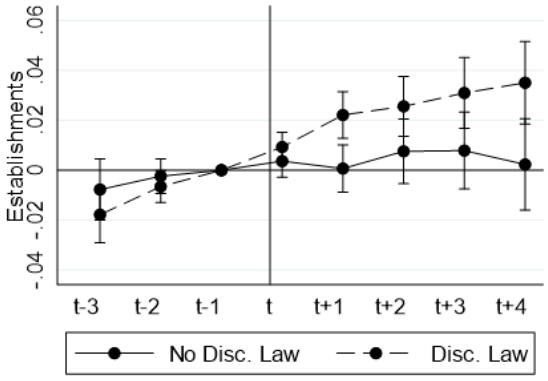


This figure presents the distribution of non-megadeal subsidy counties across states. We aggregate county-level subsidies to the state level for presentation purposes in this figure.

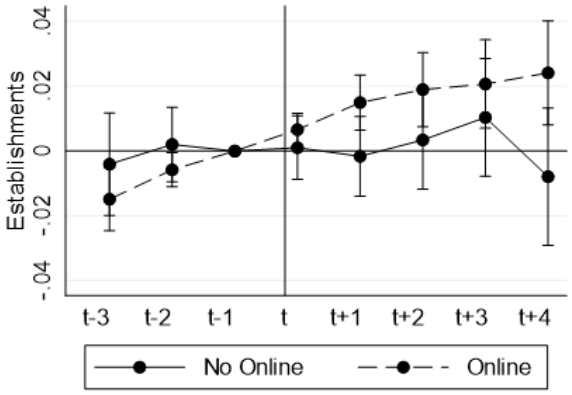
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Figure 2
Role of Information in Subsidy Outcomes: Business Establishments

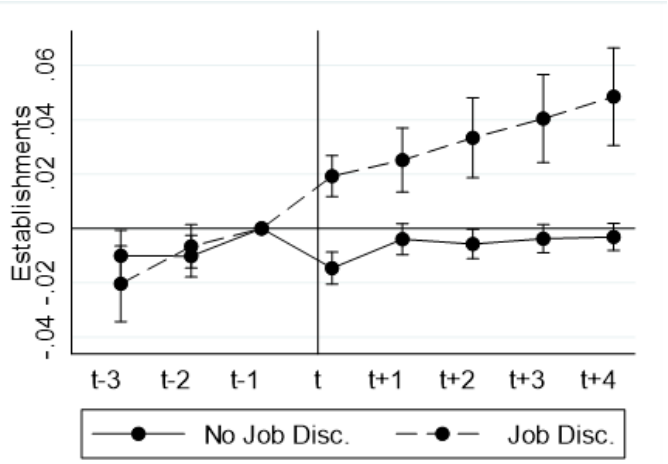
Panel A: Disclosure Regime



Panel B: Online Information



Panel C: Ex Ante Job Disclosure

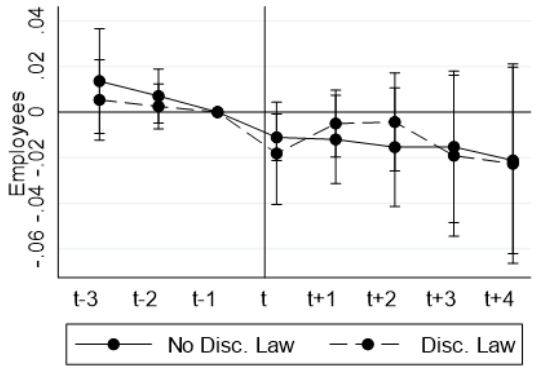


The figures present the effect of subsidy receipt on the log of the number of total business establishments at the county-level based on whether the subsidy was subject to a disclosure regime (Panel A), whether there was online information available about each subsidy at the time of subsidy grant (Panel B), or if there was *ex ante* job commitment for each subsidy award (Panel C). The graphs plot the residual values from regressing *Ln(Establishments)* on control variables and fixed effects. We measure time relative to the first subsidy receipt in a county in year *t*, with year *t-1* omitted from the regression.

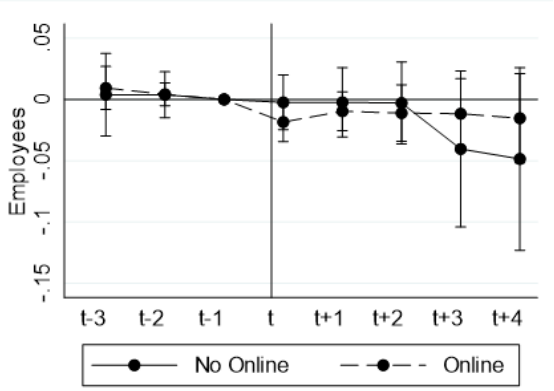
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Figure 3
Role of Information in Subsidy Outcomes: Private Sector Employment

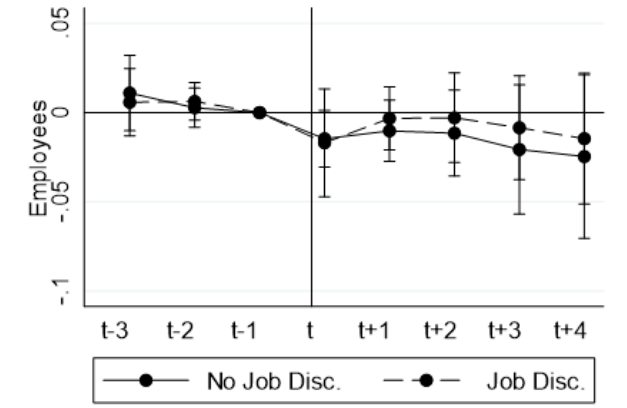
Panel A: Disclosure Regime



Panel B: Online Information



Panel C: Ex Ante Job Disclosure

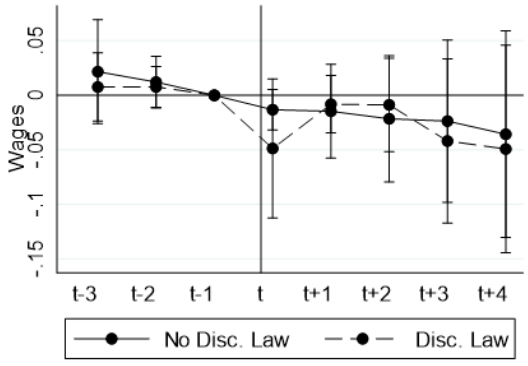


The figures present the effect of subsidy receipt on the log of the number of total business establishments at the county-level based on whether the subsidy was subject to a disclosure regime (Panel A), whether there was online information available about each subsidy at the time of subsidy grant (Panel B), or if there was *ex ante* job commitment for each subsidy award (Panel C). The graphs plot the residual values from regressing *Ln(Employees)* on control variables and fixed effects. We measure time relative to the first subsidy receipt in a county in year *t*, with year *t-1* omitted from the regression.

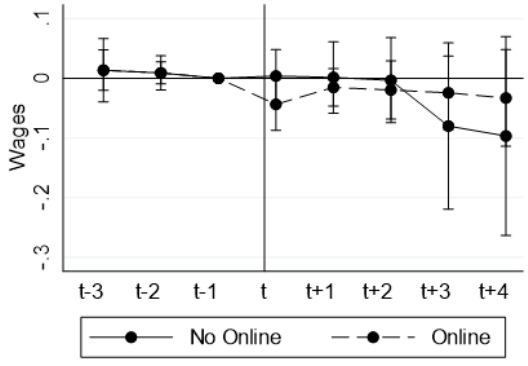
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Figure 4
Role of Information in Subsidy Outcomes: Private Sector Wages

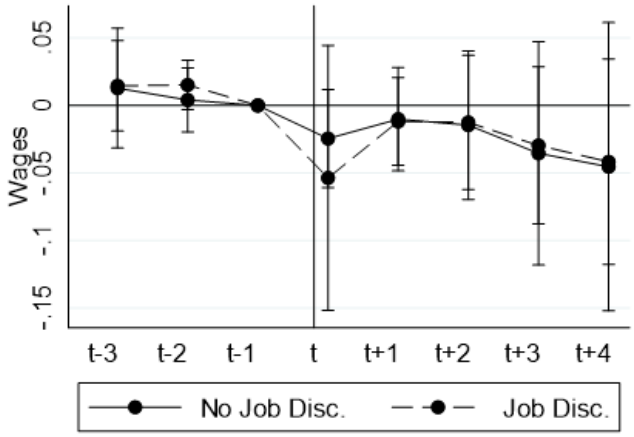
Panel A: Disclosure Regime



Panel B: Online Information



Panel C: Ex Ante Job Disclosure



The figures present the effect of subsidy receipt on the log of the number of total business establishments at the county-level based on whether the subsidy was subject to a disclosure regime (Panel A), whether there was online information available about each subsidy at the time of subsidy grant (Panel B), or if there was *ex ante* job commitment for each subsidy award (Panel C). The graphs plot the residual values from regressing *Ln(Wages)* on control variables and fixed effects. We measure time relative to the first subsidy receipt in a county in year *t*, with year *t-1* omitted from the regression

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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 tax subsidy	(6,544)	10,104	(818)	1,263
Total state subsidy sample		**10,104**		**1,263**
Counties with megadeal subsidies		**696**		**87**
Counties with non-megadeal subsidies		**9,408**		**1,176**

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

State	**# Non-Megadeal County-Years**	**# All County-Years**	**State**	**# Non-Megadeal County-Years**	**# All County-Years**
Alabama	96	144	Minnesota	264	288
Arizona	120	120	Missouri	384	416
California	336	344	Nebraska	384	392
Connecticut	48	64	Nevada	56	72
Florida	344	384	New Mexico	168	176
Georgia	512	544	New York	416	488
Illinois	592	616	Ohio	648	696
Indiana	680	696	Oklahoma	304	320
Kansas	128	136	Pennsylvania	384	392
Kentucky	776	808	South Carolina	296	344
Louisiana	408	504	Texas	408	448
Maine	128	128	Virginia	752	752
Maryland	176	192	Wisconsin	512	536
Massachusetts	88	104	**Total**	**9,408**	**10,104**

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 including (excluding) megadeals is composed of 10,104 (9,408) county-years representing 1,263 (1,176) distinct counties with firms that receive at least one subsidy during the sample period. Panel B presents the distribution of non-megadeal county-year observations (n=9,408) and total county-year observations (n=10,104) across 27 states. We exclude data from the remaining 23 states not shown above because of insufficient data coverage in Subsidy Tracker. We define all variables in Appendix A.

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Table 2
Descriptive Statistics for State Subsidy Sample

Panel A: State subsidy sample descriptive statistics

	Including Megadeals (n=10,104)			Excluding Megadeals (n=9,408)		
Variables	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**
Establishments$_{i,t+1}$	4,719.50	1,054.50	16,437.42	3,523.61	963.00	9,149.82
Ln(Establishments)$_{i,t+1}$	7.19	6.96	1.42	7.07	6.87	1.33
Employees$_{i,t+1}$	62,212.57	13,142.00	187,221.40	46,810.47	11,988.50	127,805.80
Ln(Employees)$_{i,t+1}$	9.67	9.48	1.57	9.53	9.39	1.49
Wages$_{i,t+1}$ *($M)*	3,142.73	462.70	12,076.23	2,224.96	414.18	7,639.82
Ln(Wages)$_{i,t+1}$	20.15	19.95	1.81	20.00	19.84	1.73
PostSubsidy$_{i,t}$	0.86	1.00	0.35	0.85	1.00	0.35
PostEmploymentSubsidy$_{i,t}$	0.75	1.00	0.43	0.74	1.00	0.44
PostInvestmentSubsidy$_{i,t}$	0.65	1.00	0.48	0.63	1.00	0.48
PostOtherSubsidy$_{i,t}$	0.25	0.00	0.44	0.25	0.00	0.43
Online$_{i,t}$	0.77	1.00	0.42	0.78	1.00	0.41
ExAnteJobs$_{i,t}$	0.25	0.00	0.43	0.26	0.00	0.44
Control Variables						
Ln(Population.)$_{j,t}$	10.34	10.10	1.33	10.23	10.04	1.25
%Educ$_{j,t}$	21.20	18.90	9.94	20.65	18.30	9.67
MinWage$_{i,t}$	7.26	7.25	0.59	7.26	7.25	0.59
Ln(GDP)$_{j,t}$	12.81	12.69	0.79	12.81	12.69	0.79
%Union$_{j,t}$	6.40	6.90	3.26	6.40	6.90	3.22
CorpTaxRate%$_{j,t}$	6.47	7.00	2.61	6.50	7.00	2.60
Ln(UIContrib)$_{j,t}$	11.23	11.25	0.45	11.23	11.25	0.45
PropertyTax$_{j,t}$	0.13	0.13	0.04	0.13	0.13	0.04
PersonalTaxRate%$_{j,t}$	5.29	5.71	2.50	5.31	5.71	2.49
TaxIncentivesIndex$_{j,t}$	26.46	27.00	3.33	26.48	27.00	3.30
SalesTaxRate%$_{j,t}$	3.46	4.23	2.76	3.48	4.50	2.77

Panel B: Types of subsidies in state subsidy sample (excluding megadeals)

	First Subsidy County-Years by Type (n=1,176)				All Subsidy County-Years by Type (n=5,596 out of 9,408 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	1,164	16.58%	942	769.89	9,881	19.95%	8,871	6,701.11
Tax abatement	310	4.42%	219	223.83	6,894	13.92%	6,803	1,744.35
Reimbursement	813	11.58%	714	77.92	5,142	10.38%	4,504	513.43
Enterprise zone	3,731	53.14%	3,429	558.28	21,539	43.50%	20,573	3,620.64
Grant	919	13.09%	904	455.56	5,598	11.31%	5,559	2,401.76
TIF	84	1.20%	0	0	463	0.94%	0	0
TOTAL	7,021	100.00%	6,208	2,085.49	49,517	100.00%	46,310	14,981.29

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Panel C: Distribution of subsidy-years and subsidies in state subsidy sample (excluding megadeals) by year

Year	First Subsidy County-Years	First Subsidy County-Years	All County-Years with a Subsidy	All Subsidies by County-Year
	Subsidy-Years	Subsidies	Subsidy-Years	Subsidies
2008	694	6,162	712	6,162
2009	160	358	716	5,896
2010	86	117	663	6,184
2011	81	116	700	6,242
2012	62	85	741	6,636
2013	38	61	711	6,741
2014	33	49	690	6,096
2015	22	73	663	5,560
Total	**1,176**	**7,021**	**5,596**	**49,517**

Panel D: Distribution of first subsidy-years in state subsidy sample (excluding megadeals) by stated criteria

Year	Employment		Investment		Other	
	First Subsidy County-Years	All Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years	First Subsidy County-Years	All Subsidy County-Years
2008	560	560	431	431	134	134
2009	149	564	148	415	61	144
2010	86	548	91	392	49	155
2011	75	521	86	419	38	169
2012	70	572	66	482	45	186
2013	36	539	46	467	27	164
2014	47	536	44	472	32	182
2015	39	533	23	428	19	163
Total	**1,062**	**4,373**	**935**	**3,506**	**405**	**1,297**

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 10,104 (9,408) county-year observations including (excluding) megadeals. The remaining panels describe the sample of 9,408 county-year observations excluding megadeals given that we primarily focus on this sample in the empirical tests. 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 by targeted outcome. We define all variables in Appendix A.

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Table 3
The Relation between Subsidies and Local Activity

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*	
Subsidy Sample	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals
	(1)	(2)	(3)	(4)	(5)	(6)
PostSubsidy$_{i,t}$	**0.0118*** [3.74]**	**0.0125*** [3.89]**	**-0.0105 [-1.11]**	**-0.0086 [-0.90]**	**-0.0130 [-0.63]**	**-0.0109 [-0.51]**
Ln(Population)$_{i,t}$	0.4719*** [9.80]	0.4247*** [8.45]	0.4029*** [3.07]	0.3735*** [2.65]	0.1147 [0.44]	0.0378 [0.13]
%Educ$_{j,t}$	0.0008*** [3.37]	0.0007*** [3.03]	0.0023** [2.09]	0.0020* [1.76]	0.0041 [1.58]	0.0039 [1.42]
MinWage$_{i,t}$	-0.0119*** [-5.97]	-0.0111*** [-5.36]	0.0090 [0.69]	0.0075 [0.53]	0.0178 [0.66]	0.0168 [0.56]
Ln(GDP)$_{j,t}$	0.2920*** [9.38]	0.3010*** [9.12]	-0.0357 [-0.24]	0.0051 [0.03]	-0.0406 [-0.14]	-0.0148 [-0.05]
%Union$_{j,t}$	0.0096*** [10.69]	0.0102*** [10.84]	-0.0023 [-0.86]	-0.0023 [-0.80]	-0.0037 [-0.62]	-0.0036 [-0.57]
CorpTaxRate$_{j,t}$	0.0047*** [10.05]	0.0046*** [9.32]	-0.0018 [-1.20]	-0.0019 [-1.22]	-0.0011 [-0.36]	-0.0011 [-0.33]
Ln(UIContrib)$_{j,t}$	-0.0341*** [-8.30]	-0.0368*** [-8.65]	0.0090 [0.75]	0.0079 [0.63]	0.0166 [0.67]	0.0150 [0.57]
PropertyTax$_{j,t}$	-0.0411 [-1.07]	-0.0362 [-0.92]	0.0309 [0.18]	-0.0050 [-0.03]	0.1677 [0.48]	0.1205 [0.33]
PersonalTaxRate$_{j,t}$	0.0036*** [3.24]	0.0040*** [3.51]	0.0036 [1.49]	0.0041 [1.63]	0.0097* [1.94]	0.0106* [1.93]
TaxIncentivesIndex$_{j,t}$	-0.0023*** [-3.25]	-0.0026*** [-3.59]	0.0048*** [3.37]	0.0045*** [3.15]	0.0044** [2.28]	0.0042** [2.09]
SalesTaxRate$_{j,t}$	0.0043*** [3.25]	0.0050*** [3.64]	-0.0031 [-0.74]	-0.0039 [-0.94]	0.0012 [0.14]	0.0003 [0.04]
Adj. R-squared	0.999	0.999	0.984	0.981	0.941	0.931

This panel presents results of testing the relation between subsidies and the log of the number of establishments (Columns (1)-(2)), the log of the number of employees (Columns (3)-(4)), and the log of aggregate wages (Columns (5)-(6)). Columns (1), (3), and (5) include the 696 county-year observations for counties with a megadeal subsidy (n=10,104); Columns (2), (4), and (6) exclude these observations (n=9,408). 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.

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Table 4

The Relation between Subsidies and Local Activity by Stated Criteria

Dep Var:	*Ln(Establishments)*$_{i,t+1}$		*Ln(Employees)*$_{i,t+1}$		*Ln(Wages)*$_{i,t+1}$	
	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals
	(1)	(2)	(3)	(4)	(5)	(6)
***PostInvestmentSubsidy*$_{i,t}$**	**0.0107*** **	**0.0114*** **	-0.0212	-0.0209	-0.0428	-0.0443
	[3.36]	**[3.48]**	[-1.60]	[-1.48]	[-1.49]	[-1.44]
***PostEmploymentSubsidy*$_{i,t}$**	0.0053	0.0053	**0.0193***	**0.0195***	**0.0399***	**0.0416***
	[1.58]	[1.55]	**[1.89]**	**[1.82]**	**[1.80]**	**[1.78]**
PostOtherSubsidy$_{i,t}$	-0.0030	-0.0034	-0.0034	-0.0016	-0.0100	-0.0080
	[-1.05]	[-1.15]	[-0.37]	[-0.16]	[-0.52]	[-0.38]
Ln(Population)$_{i,t}$	0.4713***	0.4253***	0.4064***	0.3731***	0.1131	0.0292
	[9.87]	[8.52]	[3.10]	[2.63]	[0.43]	[0.10]
%Educ$_{j,t}$	0.0008***	0.0007***	0.0023**	0.0020*	0.0042	0.0040
	[3.34]	[3.02]	[2.11]	[1.78]	[1.59]	[1.44]
MinWage$_{i,t}$	-0.0120***	-0.0112***	0.0090	0.0077	0.0181	0.0174
	[-6.05]	[-5.48]	[0.70]	[0.54]	[0.67]	[0.58]
Ln(GDP)$_{j,t}$	0.2887***	0.2972***	-0.0350	0.0065	-0.0421	-0.0160
	[9.34]	[9.07]	[-0.24]	[0.04]	[-0.14]	[-0.05]
%Union$_{j,t}$	0.0097***	0.0102***	-0.0023	-0.0023	-0.0037	-0.0036
	[10.74]	[10.87]	[-0.86]	[-0.79]	[-0.62]	[-0.56]
CorpTaxRate$_{j,t}$	0.0049***	0.0048***	-0.0024*	-0.0025*	-0.0023	-0.0022
	[10.35]	[9.61]	[-1.84]	[-1.81]	[-0.86]	[-0.78]
Ln(UIContrib)$_{j,t}$	-0.0349***	-0.0374***	0.0105	0.0092	0.0196	0.0175
	[-8.38]	[-8.70]	[0.91]	[0.75]	[0.82]	[0.68]
PropertyTax$_{j,t}$	-0.0464	-0.0416	0.0339	-0.0010	0.1702	0.1239
	[-1.21]	[-1.06]	[0.20]	[-0.01]	[0.48]	[0.33]
PersonalTaxRate$_{j,t}$	0.0037***	0.0041***	0.0031	0.0037	0.0086*	0.0095*
	[3.37]	[3.67]	[1.33]	[1.52]	[1.81]	[1.82]
TaxIncentivesIndex$_{j,t}$	-0.0024***	-0.0027***	0.0044***	0.0043***	0.0039**	0.0038**
	[-3.43]	[-3.74]	[3.32]	[3.13]	[2.36]	[2.18]
SalesTaxRate$_{j,t}$	0.0044***	0.0052***	-0.0040	-0.0048	-0.0006	-0.0016
	[3.39]	[3.80]	[-0.97]	[-1.16]	[-0.08]	[-0.18]
Adj. R-squared	0.999	0.999	0.984	0.981	0.941	0.931

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 (3)), and the log of aggregate wages (Column (5)) for the 9,408 county-year observations in the sample of state subsidies, excluding megadeals. Subsidies are categorized as targeting capital investment, employment, and/or other outcomes based on a manual review of subsidy information. 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.

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Table 5
The Role of Information in Subsidy Effectiveness

Panel A: Disclosure Regimes

Dep Var:	$Ln(Establishments)_{i,t+1}$ (1)	$Ln(Employees)_{i,t+1}$ (2)	$Ln(Wages)_{i,t+1}$ (3)
$\mathbf{PostSubsidy_{i,t}}$	**0.0050**	**-0.0059**	**-0.0124**
	[1.07]	**[-0.88]**	**[-1.27]**
$\mathbf{PostSubsidy_{i,t}*Information_{i,t}}$	**0.0129****	**-0.0047**	**0.0027**
	[2.03]	**[-0.31]**	**[0.08]**
$Ln(Population)_{i,t}$	0.4285***	0.3722***	0.0386
	[8.49]	[2.60]	[0.13]
$\%Educ_{j,t}$	0.0008***	0.0020*	0.0039
	[3.07]	[1.76]	[1.42]
$MinWage_{i,t}$	-0.0109***	0.0075	0.0169
	[-5.28]	[0.52]	[0.56]
$Ln(GDP)_{j,t}$	0.2890***	0.0094	-0.0173
	[9.12]	[0.06]	[-0.05]
$\%Union_{j,t}$	0.0106***	-0.0024	-0.0036
	[11.64]	[-0.89]	[-0.58]
$CorpTaxRate_{j,t}$	0.0046***	-0.0019	-0.0010
	[9.41]	[-1.24]	[-0.33]
$Ln(UIContrib)_{j,t}$	-0.0362***	0.0077	0.0151
	[-8.43]	[0.61]	[0.57]
$PropertyTax_{j,t}$	-0.0355	-0.0053	0.1206
	[-0.90]	[-0.03]	[0.33]
$PersonalTaxRate_{j,t}$	0.0038***	0.0042	0.0105*
	[3.39]	[1.64]	[1.90]
$TaxIncentivesIndex_{j,t}$	-0.0021***	0.0044***	0.0043**
	[-2.85]	[3.16]	[2.28]
$SalesTaxRate_{j,t}$	0.0047***	-0.0038	0.0003
	[3.45]	[-0.91]	[0.03]
Adj. R-squared	0.999	0.981	0.931

This panel presents results of testing the role of information in 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 9,408 county-year observations in the state subsidy sample excluding megadeals. $Information_{i,t}$ is measured with $DisclosureRegime_{i,t}$, an indicator equal to one if the state had a subsidy disclosure initiative during the sample period and zero otherwise. We define all other 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.

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Table 5
The Role of Information in Subsidy Effectiveness

Panel B: Online Availability of Subsidy Information

Dep Var:	$Ln(Establishments)_{i,t+1}$ (1)	$Ln(Employees)_{i,t+1}$ (2)	$Ln(Wages)_{i,t+1}$ (3)
PostSubsidy$_{i,t}$	**-0.0032**	**0.0002**	**-0.0088**
	[-0.54]	**[0.02]**	**[-0.51]**
PostSubsidy$_{i,t}$*Information$_{i,t}$	**0.0185*****	**-0.0104**	**-0.0025**
	[2.66]	**[-0.67]**	**[-0.10]**
$Ln(Population)_{i,t}$	0.4259***	0.3729***	0.0377
	[8.50]	[2.64]	[0.13]
$\%Educ_{j,t}$	0.0008***	0.0020*	0.0039
	[3.08]	[1.76]	[1.42]
$MinWage_{i,t}$	-0.0108***	0.0074	0.0168
	[-5.29]	[0.51]	[0.56]
$Ln(GDP)_{j,t}$	0.3002***	0.0055	-0.0147
	[9.17]	[0.03]	[-0.04]
$\%Union_{j,t}$	0.0103***	-0.0023	-0.0037
	[10.98]	[-0.83]	[-0.57]
$CorpTaxRate_{j,t}$	0.0046***	-0.0019	-0.0011
	[9.29]	[-1.22]	[-0.33]
$Ln(UIContrib)_{j,t}$	-0.0354***	0.0072	0.0148
	[-8.35]	[0.57]	[0.56]
$PropertyTax_{j,t}$	-0.0327	-0.0070	0.1200
	[-0.83]	[-0.04]	[0.33]
$PersonalTaxRate_{j,t}$	0.0040***	0.0041	0.0106*
	[3.51]	[1.63]	[1.94]
$TaxIncentivesIndex_{j,t}$	-0.0022***	0.0043***	0.0041**
	[-3.03]	[3.10]	[2.12]
$SalesTaxRate_{j,t}$	0.0049***	-0.0038	0.0003
	[3.57]	[-0.92]	[0.04]
Adj. R-squared	0.999	0.981	0.931

This panel presents results of testing the role of information in 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 9,408 county-year observations in the state subsidy sample excluding megadeals. $Information_{i,t}$ is measured with $Online_{i,t}$, an indicator equal to one if information about subsidies granted to firms in the county-year is made available online and zero otherwise. We define all other 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.

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Table 5
The Role of Information in Subsidy Effectiveness

Panel C: Ex Ante Job Disclosure

Dep Var:	$Ln(Establishments)_{i,t+1}$ (1)	$Ln(Employees)_{i,t+1}$ (2)	$Ln(Wages)_{i,t+1}$ (3)
PostSubsidy$_{i,t}$	**-0.0036**	**-0.0192**	**-0.0332**
	[-1.01]	**[-1.58]**	**[-1.25]**
PostSubsidy$_{i,t}$*Information$_{i,t}$	**0.0395*****	**0.0261**	**0.0550**
	[5.84]	**[1.60]**	**[1.59]**
$Ln(Population)_{i,t}$	0.4280***	0.3757***	0.0425
	[8.46]	[2.67]	[0.15]
$\%Educ_{j,t}$	0.0007***	0.0020*	0.0039
	[3.13]	[1.77]	[1.42]
$MinWage_{i,t}$	-0.0103***	0.0080	0.0180
	[-5.10]	[0.57]	[0.61]
$Ln(GDP)_{j,t}$	0.2927***	-0.0004	-0.0264
	[9.24]	[-0.00]	[-0.08]
$\%Union_{j,t}$	0.0097***	-0.0026	-0.0043
	[10.16]	[-0.95]	[-0.70]
$CorpTaxRate_{j,t}$	0.0044***	-0.0020	-0.0012
	[8.93]	[-1.27]	[-0.38]
$Ln(UIContrib)_{j,t}$	-0.0309***	0.0118	0.0232
	[-7.23]	[0.86]	[0.79]
$PropertyTax_{j,t}$	-0.0335	-0.0032	0.1243
	[-0.87]	[-0.02]	[0.34]
$PersonalTaxRate_{j,t}$	0.0036***	0.0039	0.0100*
	[3.31]	[1.58]	[1.91]
$TaxIncentivesIndex_{j,t}$	-0.0019***	0.0050***	0.0051**
	[-2.70]	[3.41]	[2.43]
$SalesTaxRate_{j,t}$	0.0050***	-0.0039	0.0003
	[3.66]	[-0.94]	[0.03]
Adj. R-squared	0.999	0.981	0.931

This panel presents results of testing the role of information in 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)) employees for the 9,408 county-year observations in the state subsidy sample excluding megadeals. $Information_{i,t}$ is $ExAnteJobs_{i,t}$, an indicator equal to one if information about anticipated job creation was provided for subsidies granted to firms in the county-year and zero otherwise. We define all other 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.

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Online Appendix

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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	(22,952)	2,264	(2,869)	283
Total local subsidy sample		2,264		283
Counties with megadeal subsidies		264		33
Counties with non-megadeal subsidies		2,000		250

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

State	# Non-Megadeal County-Years	# All County-Years	State	# Non-Megadeal County-Years	# All county-years
Alabama	8	8	Minnesota	176	192
Arizona	16	16	Missouri	112	136
California	24	24	Nevada	8	8
Colorado	24	24	New Mexico	0	8
Connecticut	8	24	New York	384	432
Florida	24	40	North Carolina	168	168
Georgia	16	16	Ohio	0	16
Illinois	104	120	Oklahoma	0	8
Indiana	16	24	Oregon	16	16
Kansas	8	8	South Carolina	0	8
Kentucky	0	8	Tennessee	360	376
Louisiana	8	16	Texas	120	152
Maryland	8	16	Virginia	32	32
Massachusetts	0	8	Wisconsin	8	8
Michigan	352	352	**Total**	**2,000**	**2,264**

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 including (excluding) megadeals is composed of 2,264 (2,000) county-years representing 283 (250) distinct counties with firms that receive at least one subsidy during the sample period. Panel B presents the distribution of non-megadeal county-year observations across 29 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.

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

Panel C: Local subsidy sample descriptive statistics

	Including Megadeals (n=2,264)			Excluding Megadeals (n=2,000)		
Variables	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**
$Employees_{i,t+1}$	116,661.40	22,339.00	224,053.20	88,936.04	18,511.50	208,217.70
$Ln(Employees)_{i,t+1}$	10.32	10.01	1.72	10.06	9.83	1.61
$Wages_{i,t+1}$ *($M)*	5,974.91	865.07	13,080.58	4,449.74	646.32	12,224,47
$Ln(Wages)_{i,t+1}$	20.89	20.58	1.89	20.60	20.29	1.77
$(Employees/Population)_{i,t+1}$	0.58	0.54	0.23	0.55	0.51	0.21
$(Wages/Population)_{i,t+1}$	24.78	20.42	15.96	22.45	19.00	13.17
$Establishments_{i,t+1}$	7,951.44	1,876.00	14,588.08	6,099.98	1,454.50	13,076.07
$Ln(Establishments)_{i,t+1}$	7.76	7.54	1.59	7.52	7.28	1.48
$PostSubsidy_{i,t}$	0.87	1.00	0.34	0.86	1.00	0.34
$PostEmploymentSubsidy_{i,t}$	0.19	0.00	0.39	0.17	0.00	0.38
$PostInvestmentSubsidy_{i,t}$	0.78	1.00	0.42	0.77	1.00	0.42
$PostOtherSubsidy_{i,t}$	0.04	0.00	0.21	0.04	0.00	0.20
$Online_{i,t}$	0.80	1.00	0.40	0.83	1.00	0.37
$Newspaper_{i,t}$	0.14	0.00	0.35	0.12	0.00	0.33
Control Variables						
$Ln(Population.)_{j,t}$	10.94	10.76	1.45	10.73	10.49	1.36
$\%Educ_{j,t}$	24.70	22.23	11.04	23.49	20.90	10.71
$MinWage_{i,t}$	7.35	7.25	0.60	7.34	7.25	0.59
$Ln(GDP)_{j,t}$	13.11	12.94	0.68	13.11	12.94	0.66
$\%Union_{j,t}$	7.85	8.30	4.60	7.91	8.40	4.65
$CorpTaxRate\%_{j,t}$	6.30	6.50	2.38	6.33	6.50	2.29
$Ln(UIContrib)_{j,t}$	11.47	11.42	0.43	11.48	11.43	0.42
$PropertyTax_{j,t}$	0.13	0.13	0.04	0.13	0.13	0.03
$PersonalTaxRate\%_{j,t}$	4.78	5.00	3.29	4.79	5.00	3.32
$TaxIncentivesIndex_{j,t}$	26.14	26.00	2.29	26.06	26.00	2.27
$SalesTaxRate\%_{j,t}$	3.52	4.00	2.86	3.52	4.00	2.88

This table presents sample selection details and descriptive statistics for the local subsidy sample. The sample include localities that 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. Panel C presents descriptive statistics for the dependent variables, variables of interest, and control variables for the sample of 2,264 (2,000) county-year observations including (excluding) megadeals. We define all variables in Appendix A.

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Online Appendix Table 2
Local Subsidy Analyses

Panel A: Relation between Subsidies and Local Activity

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln(Wages)$_{i,t+1}$*	
	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals	Including Megadeals	Excluding Megadeals
	(1)	(2)	(3)	(4)	(5)	(6)
PostSubsidy$_{i,t}$	**0.0010**	**0.0034**	**0.0205**	**0.0075**	**0.0201**	**0.0045**
	[0.21]	**[0.63]**	**[1.36]**	**[1.06]**	**[1.09]**	**[0.47]**
Ln(Population)$_{i,t}$	0.8058***	0.7357***	0.5479***	0.5865***	0.5304***	0.5790***
	[9.62]	[7.49]	[4.65]	[5.33]	[3.49]	[4.01]
%Educ$_{j,t}$	0.0010	0.0012	0.0017	0.0012	0.0021	0.0015
	[1.02]	[1.04]	[1.62]	[1.36]	[1.49]	[1.28]
MinWage$_{i,t}$	-0.0216***	-0.0205***	-0.0052	0.0035	0.0056	0.0172***
	[-6.27]	[-5.48]	[-0.57]	[0.80]	[0.50]	[2.93]
Ln(GDP)$_{j,t}$	0.1426	0.2013	-0.0300	0.3454***	0.1637	0.6127***
	[1.40]	[1.55]	[-0.09]	[2.67]	[0.38]	[3.19]
%Union$_{j,t}$	0.0056**	0.0056**	-0.0006	-0.0043	-0.0020	-0.0064*
	[2.37]	[2.11]	[-0.17]	[-1.61]	[-0.49]	[-1.77]
CorpTaxRate$_{j,t}$	0.0002	-0.0005	-0.0002	0.0000	-0.0017	-0.0016
	[0.12]	[-0.35]	[-0.12]	[0.01]	[-0.91]	[-0.80]
Ln(UIContrib)$_{j,t}$	0.0148	0.0167	-0.0167	-0.0098	-0.0233	-0.0244
	[1.24]	[1.17]	[-1.11]	[-0.59]	[-1.19]	[-1.19]
PropertyTax$_{j,t}$	0.1834**	0.1877**	0.0466	-0.4016***	-0.0201	-0.6009***
	[2.03]	[2.05]	[0.10]	[-4.53]	[-0.04]	[-4.80]
PersonalTaxRate$_{j,t}$	0.0025	0.0030	0.0021	-0.0024	0.0041	-0.0013
	[1.38]	[1.53]	[0.44]	[-1.16]	[0.67]	[-0.40]
TaxIncentivesIndex$_{j,t}$	-0.0013*	-0.0011	-0.0023	-0.0010	-0.0019	-0.0001
	[-1.72]	[-1.39]	[-1.40]	[-0.95]	[-0.95]	[-0.11]
SalesTaxRate$_{j,t}$	0.0036	0.0041	-0.0039	-0.0081**	-0.0037	-0.0085*
	[1.34]	[1.46]	[-0.72]	[-2.58]	[-0.55]	[-1.85]
Adj. R-squared	0.999	0.999	0.999	0.999	0.998	0.999

This panel presents results of testing the relation between subsidies and the log of the number of establishments (Columns (1)-(2)), the log of the number of employees (Columns (3)-(4)), and the log of aggregate wages (Columns (5)-(6)). Columns (1), (3), and (5) include the 264 county-year observations for counties with a megadeal subsidy (n=2,264); Columns (2), (4), and (6) exclude these observations (n=2,000). 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.

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Online Appendix Table 2
Local Subsidy Analyses

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

Dep Var:	$Ln(Establishments)_{i,t+1}$ (1)	$Ln(Employees)_{i,t+1}$ (2)	$Ln(Wages)_{i,t+1}$ (3)
PostInvestmentSubsidy$_{i,t}$	**0.0115****	**0.0070**	**0.0004**
	[2.26]	**[0.86]**	**[0.03]**
PostEmploymentSubsidy$_{i,t}$	**-0.0266*****	**-0.0028**	**0.0116**
	[-3.48]	**[-0.32]**	**[1.00]**
PostOtherSubsidy$_{i,t}$	**0.0560***	**-0.0026**	**-0.0098**
	[1.91]	**[-0.09]**	**[-0.18]**
$Ln(Population)_{i,t}$	0.7474***	0.5894***	0.5731***
	[7.62]	[5.31]	[3.92]
$\%Educ_{j,t}$	0.0012	0.0013	0.0015
	[1.11]	[1.43]	[1.27]
$MinWage_{i,t}$	-0.0185***	0.0043	0.0165***
	[-5.22]	[0.97]	[2.84]
$Ln(GDP)_{j,t}$	0.2276*	0.3428***	0.6020***
	[1.74]	[2.62]	[3.08]
$\%Union_{j,t}$	0.0049*	-0.0044	-0.0061
	[1.79]	[-1.64]	[-1.65]
$CorpTaxRate_{j,t}$	-0.0010	-0.0002	-0.0014
	[-0.65]	[-0.10]	[-0.73]
$Ln(UIContrib)_{j,t}$	0.0175	-0.0097	-0.0248
	[1.21]	[-0.58]	[-1.20]
$PropertyTax_{j,t}$	0.1817**	-0.3961***	-0.5919***
	[2.03]	[-4.53]	[-4.87]
$PersonalTaxRate_{j,t}$	0.0040**	-0.0021	-0.0015
	[2.07]	[-0.98]	[-0.47]
$TaxIncentivesIndex_{j,t}$	-0.0010	-0.0010	-0.0002
	[-1.25]	[-0.96]	[-0.13]
$SalesTaxRate_{j,t}$	0.0057**	-0.0080***	-0.0092**
	[1.99]	[-2.60]	[-2.07]
Adj. R-squared	0.999	0.999	0.999

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=2,000). 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.

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Online Appendix Table 2
Local Subsidy Analyses

Panel C: The Role of Information in Subsidy Effectiveness

Dep Var:	$Ln(Establishments)_{i,t+1}$		$Ln(Employees)_{i,t+1}$		$Ln(Wages)_{i,t+1}$	
$Information_{i,t}$	*Online*	*Newspaper*	*Online*	*Newspaper*	*Online*	*Newspaper*
	(1)	(2)	(3)	(4)	(5)	(6)
PostSubsidy$_{i,t}$	**-0.0145**	**0.0010**	**-0.0100**	**0.0063**	**-0.0042**	**0.0059**
	[-1.34]	**[0.18]**	**[-0.92]**	**[0.79]**	**[-0.28]**	**[0.55]**
PostSubsidy$_{i,t}$*Information	**0.0225***	**0.0139**	**0.0219**	**0.0067**	**0.0109**	**-0.0088**
	[1.83]	**[1.56]**	**[1.62]**	**[0.51]**	**[0.60]**	**[-0.52]**
$Ln(Population)_{i,t}$	0.7446***	0.7362***	0.5951***	0.5868***	0.5833***	0.5787***
	[7.75]	[7.51]	[5.54]	[5.34]	[4.08]	[4.01]
$\%Educ_{j,t}$	0.0011	0.0012	0.0011	0.0012	0.0015	0.0015
	[0.99]	[1.06]	[1.30]	[1.38]	[1.25]	[1.26]
$MinWage_{i,t}$	-0.0210***	-0.0203***	0.0031	0.0037	0.0170***	0.0170***
	[-5.55]	[-5.39]	[0.69]	[0.81]	[2.86]	[2.89]
$Ln(GDP)_{j,t}$	0.2040	0.2039	0.3480***	0.3466***	0.6140***	0.6110***
	[1.55]	[1.56]	[2.70]	[2.68]	[3.20]	[3.17]
$\%Union_{j,t}$	0.0054**	0.0057**	-0.0045*	-0.0042	-0.0066*	-0.0065*
	[2.01]	[2.13]	[-1.72]	[-1.60]	[-1.82]	[-1.78]
$CorpTaxRate_{j,t}$	-0.0006	-0.0005	-0.0000	0.0000	-0.0016	-0.0015
	[-0.39]	[-0.36]	[-0.03]	[0.00]	[-0.82]	[-0.80]
$Ln(UIContrib)_{j,t}$	0.0173	0.0165	-0.0092	-0.0099	-0.0241	-0.0243
	[1.23]	[1.15]	[-0.56]	[-0.59]	[-1.18]	[-1.18]
$PropertyTax_{j,t}$	0.1733*	0.1851**	-0.4156***	-0.4028***	-0.6079***	-0.5992***
	[1.86]	[2.02]	[-4.66]	[-4.54]	[-4.90]	[-4.79]
$PersonalTaxRate_{j,t}$	0.0033*	0.0031	-0.0021	-0.0024	-0.0011	-0.0013
	[1.72]	[1.57]	[-1.02]	[-1.13]	[-0.36]	[-0.42]
$TaxIncentivesIndex_{j,t}$	-0.0006	-0.0012	-0.0005	-0.0010	0.0001	-0.0001
	[-0.77]	[-1.48]	[-0.50]	[-0.99]	[0.07]	[-0.08]
$SalesTaxRate_{j,t}$	0.0052*	0.0041	-0.0071**	-0.0081**	-0.0079*	-0.0084*
	[1.75]	[1.43]	[-2.22]	[-2.58]	[-1.76]	[-1.86]
Adj. R-squared	0.999	0.999	0.999	0.999	0.999	0.999

This panel presents results of testing the role of local information for the relation between subsidies and the log of the number of establishments (Columns (1) and (2)), the log of the number of employees (Columns (3) and (4)), and the log of aggregate wages (Columns (5) and (6)) for the 250 counties without a megadeal subsidy (n=2,000). Because ex ante job 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.

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Online Appendix Table 3
Correlations for State Subsidy Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)
(1) $Ln(Establishments)_{i,t+1}$	**1.00**	**0.96**	**0.88**	**0.18**	**0.15**	**0.06**	**0.18**	**-0.17**	**-0.28**	**0.97**	**0.63**	**0.20**	**0.38**	**0.09**	0.02	**-0.08**	**0.15**	**0.03**	**0.11**	**0.03**
(2) $Ln(Employees)_{i,t+1}$	**0.98**	**1.00**	**0.98**	**0.19**	**0.16**	**0.09**	**0.19**	**-0.19**	**-0.25**	**0.94**	**0.59**	**0.16**	**0.36**	**0.09**	-0.01	**-0.06**	**0.13**	-0.01	**0.09**	0.02
(3) $Ln(Wages)_{i,t+1}$	**0.96**	**0.99**	**1.00**	**0.19**	**0.16**	**0.09**	**0.17**	**-0.17**	**-0.24**	**0.87**	**0.56**	**0.16**	**0.35**	**0.09**	-0.02	**-0.06**	**0.11**	-0.01	**0.09**	-0.01
(4) $PostSubsidy_{i,t}$	**0.18**	**0.20**	**0.21**	**1.00**	**0.69**	**0.54**	**0.24**	**-0.16**	**-0.07**	**0.19**	**0.11**	**0.31**	**0.11**	**0.12**	**-0.03**	**0.10**	**-0.02**	**0.03**	**0.05**	**-0.23**
(5) $PostEmpSubsidy_{i,t}$	**0.15**	**0.16**	**0.17**	**0.69**	**1.00**	**0.44**	**0.03**	**-0.05**	**0.04**	**0.15**	**0.11**	**0.31**	-0.01	**0.15**	**0.04**	**0.09**	**-0.08**	**0.20**	**-0.04**	**-0.19**
(6) $PostInvestmentSubsidy_{i,t}$	**0.09**	**0.11**	**0.13**	**0.54**	**0.44**	**1.00**	**0.19**	**-0.06**	**-0.08**	**0.06**	**0.07**	**0.18**	**0.08**	**0.06**	**-0.18**	**0.09**	**0.17**	**-0.08**	**-0.15**	**-0.23**
(7) $PostOtherSubsidy_{i,t}$	**0.19**	**0.21**	**0.21**	**0.24**	**0.03**	**0.19**	**1.00**	-0.01	**0.16**	**0.18**	**0.14**	**0.10**	**0.03**	**0.07**	**0.03**	**0.06**	**0.05**	**-0.14**	**-0.04**	**-0.13**
(8) $ExAnteJobs_{i,t}$	**-0.17**	**-0.19**	**-0.19**	**-0.16**	**-0.05**	**-0.06**	-0.01	**1.00**	**0.29**	**-0.20**	-0.01	**0.04**	**-0.12**	**-0.12**	**0.03**	0.01	**0.18**	**-0.11**	-0.02	0.00
(9) $Online_{i,t}$	**-0.26**	**-0.24**	**-0.24**	**-0.07**	**0.04**	**-0.08**	**0.16**	**0.29**	**1.00**	**-0.29**	**-0.11**	**-0.06**	**-0.34**	-0.01	**0.10**	**0.19**	**-0.12**	**-0.15**	**0.10**	0.00
(10) $Ln(Population.)_{j,t}$	**0.97**	**0.96**	**0.94**	**0.19**	**0.15**	**0.09**	**0.18**	**-0.20**	**-0.27**	**1.00**	**0.56**	**0.17**	**0.39**	**0.09**	**-0.03**	**-0.09**	**0.12**	-0.01	**0.08**	**0.04**
(11) $\%Educ_{j,t}$	**0.65**	**0.63**	**0.63**	**0.11**	**0.12**	**0.09**	**0.15**	-0.01	**-0.14**	**0.57**	**1.00**	**0.15**	**0.14**	**0.06**	**0.11**	**0.06**	**0.18**	**0.09**	**0.11**	0.01
(12) $MinWage_{i,t}$	**0.15**	**0.14**	**0.16**	**0.29**	**0.31**	**0.18**	**0.07**	**0.07**	-0.02	**0.13**	**0.15**	**1.00**	**0.37**	**0.30**	**0.05**	**0.22**	**0.14**	**0.18**	-0.01	**-0.31**
(13) $Ln(GDP)_{j,t}$	**0.35**	**0.35**	**0.36**	**0.11**	0.00	**0.12**	**0.07**	**-0.08**	**-0.29**	**0.37**	**0.16**	**0.36**	**1.00**	**0.31**	**-0.18**	**-0.19**	**0.36**	**-0.07**	**0.09**	**-0.03**
(14) $\%Union_{j,t}$	**0.07**	**0.08**	**0.08**	**0.11**	**0.14**	0.02	**0.07**	**-0.10**	**0.08**	**0.06**	**0.07**	**0.29**	**0.24**	**1.00**	**0.23**	**0.36**	**0.09**	**0.28**	**0.03**	**0.07**
(15) $CorpTaxRate\%_{i,t}$	**0.08**	**0.07**	**0.06**	**0.04**	**0.05**	**-0.12**	0.00	**-0.05**	**0.11**	**0.03**	**0.15**	**0.08**	**-0.03**	**0.47**	**1.00**	**0.13**	**0.02**	**0.32**	**0.33**	**0.05**
(16) $Ln(UIContrib)_{i,t}$	**-0.06**	**-0.05**	**-0.05**	**0.09**	**0.06**	**0.08**	**0.10**	**0.06**	**0.22**	**-0.08**	**0.09**	**0.24**	**-0.18**	**0.40**	**0.17**	**1.00**	0.01	**0.21**	0.02	**-0.10**
(17) $PropertyTax_{i,t}$	**0.17**	**0.16**	**0.16**	**-0.03**	**-0.09**	**0.18**	**0.04**	**0.19**	**-0.15**	**0.13**	**0.23**	**0.09**	**0.46**	**0.03**	**0.08**	-0.01	**1.00**	**-0.13**	**-0.19**	**0.05**
(18) $PersonalTaxRate\%_{i,t}$	-0.01	**-0.04**	**-0.04**	**0.05**	**0.21**	**0.03**	**-0.10**	**-0.10**	**-0.20**	**-0.05**	**0.08**	**0.04**	**-0.18**	**0.20**	**0.11**	**0.24**	**-0.07**	**1.00**	**-0.12**	**-0.06**
(19) $TaxIncentivesIndex_{i,t}$	**0.09**	**0.07**	**0.08**	**0.03**	**-0.06**	**-0.17**	**-0.12**	**-0.06**	**0.06**	**0.07**	**0.08**	**-0.09**	0.02	-0.02	**0.39**	**-0.13**	**-0.21**	**-0.28**	**1.00**	**0.02**
(20) $SalesTaxRate\%_{i,t}$	**0.05**	**0.05**	**0.03**	**-0.17**	**-0.15**	**-0.22**	**-0.10**	0.00	**0.02**	**0.06**	0.01	**-0.17**	**0.02**	**0.16**	**0.14**	**-0.09**	**0.08**	**-0.13**	**0.10**	**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.

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Online Appendix Table 4
Robustness to Estimation with State-by-Year Fixed Effects

Dep Var:	*Ln(Establishments)$_{i,t+1}$*		*Ln(Employees)$_{i,t+1}$*		*Ln (Wages)$_{i,t+1}$*	
	(1)	(2)	(3)	(4)	(5)	(6)
PostInvestmentSubsidy$_{i,t}$	-0.0013		-0.0047		-0.0059	
	[-0.43]		[-0.76]		[-0.48]	
PostEmploymentSubsidy$_{i,t}$	0.0019		0.0192*		0.0343	
	[0.54]		[1.80]		[1.41]	
PostOtherSubsidy$_{i,t}$	-0.0048*		-0.0111		-0.0184	
	[-1.68]		[-0.80]		[-0.66]	
PostSubsidy$_{i,t}$		-0.0010		-0.0156		-0.0252
		[-0.29]		[-1.17]		[-0.86]
PostSubsidy$_{i,t}$*Information$_{i,t}$		0.0059		0.0342**		0.0623*
		[0.81]		[2.04]		[1.78]
Ln(Population)$_{i,t}$	0.4543***	0.4544***	0.3962***	0.3818***	0.1136	0.0899
	[8.22]	[8.15]	[3.34]	[3.07]	[0.48]	[0.36]
%Educ$_{j,t}$	0.0005*	0.0005*	0.0018	0.0018	0.0042	0.0042
	[1.94]	[1.93]	[1.56]	[1.56]	[1.47]	[1.46]
Adj. R-squared	0.999	0.999	0.983	0.983	0.935	0.935

This table reports robustness of results to replacing state-level control variables with state-by-year fixed effects. 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.

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