



When do firms deliver on the jobs they promise in return for state aid?

Qingkai Dong¹ · Aneesh Raghunandan²  · Shivaram Rajgopal³

Accepted: 23 August 2023
© The Author(s) 2023

Abstract

US state governments frequently provide firms with targeted subsidies. In exchange, recipients promise to create or retain a certain number of jobs in the subsidizing state. Using novel hand-collected data, we address three questions: (i) the extent to which firms meet job creation targets promised in their applications, (ii) the factors that determine which firms meet the targets, and (iii) the benefits to firms from meeting those targets. We find that 63% of subsidies awarded to publicly traded U.S. firms between 2004 and 2015 meet their promised job creation targets. Firms with poorer labor practices are less likely to meet their targets, as are politically connected firms that receive subsidies in election years. Conversely, promised job targets are also more likely to be met for subsidies accompanied by government press releases but less likely to be met for subsidies accompanied by firm press releases; the latter likely reflects the fact that firms put out press releases for larger subsidies with more ambitious job targets. In terms of consequences, firms that meet job targets are more successful at obtaining subsequent subsidies both in and out of subsidizing states. However, while firms' success in meeting job targets is associated with an uptick in positive media coverage, this does not flow through to ESG ratings, even on scores specific to community impact. Our results should be of interest to both academics and policymakers interested in the design of state-level economic incentives.

Keywords Subsidies · Political connections · Transparency · Job creation · Labor practices · Stakeholders · ESG

JEL Classification D72 · M14 · H25 · H71

✉ Aneesh Raghunandan
a.raghunandan@lse.ac.uk

¹ Hong Kong University of Science and Technology, Hong Kong, Hong Kong

² London School of Economics, London, UK

³ Columbia Business School, New York, NY, USA

1 Introduction

US state governments frequently provide firms with economic incentives, via targeted firm-specific subsidies, to retain or create jobs within state borders. These subsidies take the form of either cash grants or, more commonly, firm-specific tax abatements or tax credits. We examine three research questions related to these subsidies: (i) whether the recipients of these economic incentives actually retain or create jobs in line with promised job-creation or retention targets, (ii) what firm- and state-level factors determine which subsidies result in job creation targets being met; and (iii) what the consequences are, if any, for firms that fail to meet job-creation targets.

While our research questions have long been of interest to policymakers, they have been difficult to answer because of a lack of available data on job creation targets associated with *individual* subsidies. As a result, prior research (e.g., Slattery and Zidar 2020; De Simone et al. 2022) studies subsidies from the perspective of the government, testing how subsidies affect county-level measures, such as employment rates, wages, or housing prices in recipient firms' locations. In contrast, little is known about how the characteristics of recipients may contribute to the success or failure of specific subsidies.

We sidestep these issues by focusing on a set of subsidies that are subject to mandatory disclosure of job creation information, both promised and realized.¹ We caution that, in focusing on a subsample chosen via strict data requirements, we cannot address two key issues studied elsewhere: (i) whether or when subsidies create jobs in aggregate and (ii) the overall efficiency of subsidies as a state-level policy tool. Instead, we provide initial evidence on the factors that lead *specific firms* to meet or miss job creation targets. We view this as significant because states concurrently award subsidies to several different firms. However, because the outcome measures used in prior studies are geographic aggregates, those studies can assess the performance of subsidies as a broader policy tool but not of individual subsidies. That is, other work asks whether subsidy-giving is good for governments in aggregate and how governments can improve their subsidy grants. In contrast, we provide initial evidence on *which of the firms that receive subsidies* meet specific goals set during the subsidy-granting process. This question is of interest both to policymakers and to socially conscious investors, who seek to assess which firms are responsible stewards of taxpayer funds.²

We hand-collect job creation data for 70 subsidy programs across 20 states. After merging this with other data from Good Jobs First's Subsidy Tracker database, on subsidy recipient information, we obtain a sample of 1,964 subsidies awarded to publicly traded U.S. firms between 2004 and 2015 that contain ex ante and ex post

¹ For example, the Florida Department of Economic Opportunities launched a publicly available interactive database in 2013, the Economic Development Incentives Portal. The portal contains information on subsidy recipients, the value of incentives awarded, and, crucially, the number of jobs promised by the company as a result of the subsidy and the number of jobs actually created by the specific subsidy.

² See, e.g., <https://tiiproject.com/wp-content/uploads/2022/01/TIIP-Stewardship-Final.pdf>.

job information associated with the subsidy. For each subsidy, we observe whether the number of jobs promised by the subsidy recipient was created as well as other subsidy-related information (dollar value, total number of jobs promised, and actual number of jobs created). We aggregate these data to the firm-state-year level because some firms may receive multiple related subsidies in the same year from the same state, where the relevant target from the awarding state's perspective is the firm's overall performance across all subsidies awarded in that year.

A key innovation of our approach is our focus on firms' performance with respect to subsidy-specific job creation targets.³ Proponents of subsidies frequently argue that it is insufficient to analyze a subsidy on the basis of the number of jobs created by that particular subsidy's recipient, because such analyses may not (i) properly compare the increase in payroll taxes resulting from a subsidy against the cost of the subsidy or (ii) fully account for spillover or fiscal multiplier effects (e.g., a subsidy to one company may spur additional demand for the goods and services of other companies in the local area, leading to an additional increase in local economic activity and tax revenues).⁴ In turn, it has historically been difficult to assess whether a subsidy has succeeded or failed. Focusing on job creation in relation to job targets reduces this concern, because subsidy-related increases in the tax base and potential spillovers should be accounted for in the process of negotiating the targets.

We find, descriptively, that 63% of subsidies (59% of firm-state-years) meet the job targets associated with the subsidies they receive. Given the role of disclosure in facilitating more efficient subsidies (De Simone et al. 2022), the 63% in our sample is likely to be an upper bound with respect to the broader set of subsidy recipients for which we cannot observe information. We observe substantial temporal variation in this figure; the yearly percentage of job targets met in our sample ranges from a low of 55% to a high of 75%.

The fact that 37% of job creation targets associated with subsidies are not met motivates our second research question: which firms deliver on their promises to state governments and, by extension, the taxpayers bearing subsidies' costs. We assess firm-specific and firm-by-state factors potentially affecting job target completion. We begin with financial performance. While prior work (e.g., Cohen et al. 2011) argues that subsidies crowd out private investment, such studies typically focus on subsidies large enough to meet firms' financing needs. However, the subsidies in our sample are smaller, and so the findings from those studies are unlikely to generalize to ours. The subsidies in our sample require complementary private investment by firms to succeed in meeting job targets; we argue that this investment is more likely to occur in financially stronger firms. Consistent with this argument,

³ Some subsidies are accompanied by investment targets. However, we do not consider these for two reasons: (i) they occur less frequently than job targets, and (ii) ex-post investment data is very rarely available, meaning that we do not have the data that would be required to assess ex post performance with respect to ex ante targets.

⁴ For example, the Idaho Department of Commerce explicitly alludes to the need to consider both direct and indirect economic impact as part of the subsidy-giving process. See https://commerce.idaho.gov/content/uploads/2016/06/IDOC_TRI_2019_FINAL.pdf.

we find some evidence that job target completion is more likely in more profitable firms.

We turn to firms' labor-related records as our next potential factor affecting target completion, because subsidies frequently arise from politicians' calls to create high-quality jobs (defined informally as jobs that pay a fair wage and provide good working conditions). Criticism of subsidy recipients that do not create these kinds of jobs has increased in recent years.⁵ In response to these criticisms, many subsidy programs include explicit job quality provisions. For example, Florida's Quick Action Closing Fund requires subsidy recipients to pay employees an average annual wage of at least 125% of the areawide private sector average wage.⁶ We argue that a firm with a history of mistreating employees has revealed itself as an employer that provides lower-quality jobs. An employer that relies upon low-quality jobs must invest more, relative to an employer whose default practice is to provide high-quality jobs, to satisfy job quality criteria. In turn, the low-quality employer may be less likely to fulfill its job obligations. Consistent with this argument, we find that firms with more violations of federal labor laws in the years preceding a subsidy, at facilities located within the subsidizing state, are less likely to meet subsidy-related job targets. Our results suggest that even politicians who are primarily concerned with the financial returns to subsidies might want to consider concerns about potential recipients' labor practices.

We next consider politically motivated subsidies. Slattery (2021) documents that politicians frequently award subsidies for personal political reasons and that politically motivated subsidies arise more frequently in election years. Such subsidies are less likely to arise for bona fide economic reasons, which in turn may make them less effective in creating jobs. We exploit exogenous variation in the timing of state elections to test this assertion. We show that subsidies awarded to politically connected firms (those making contributions to campaigns for state office) during re-election years are 12.6–15.0 percentage points less likely to result in job-creation targets being met. Conversely, subsidies awarded to connected firms in non-election years or firms connected to other (nonsubsidizing) states exhibit no differences from the sample average target completion rate. Our results highlight a potential cost borne by taxpayers when subsidies are awarded for political, rather than purely economic, reasons.

Subsidies are frequently covered in the media, through both independent journalism and voluntary press releases. The literature shows that both types of media can facilitate corporate monitoring and real actions more generally (e.g., Bushee et al. 2010; Blankespoor et al. 2018). We therefore examine whether media coverage facilitates or signals the likelihood of job target completion. To do so, we hand-collect subsidy-specific media coverage for each subsidy in our sample. We classify all coverage into three categories: (i) independent journalism, (ii) government press

⁵ For example, Amazon's 2019 attempt to set up a second headquarters in New York in exchange for \$3 billion in subsidies received criticism from politicians and unions citing Amazon's poor labor practices (Raghuandan 2021). In response, Amazon ultimately withdrew the from the proposed subsidy deal.

⁶ See Florida Statutes Title XIX, Public Business § 288.1088.

releases, and (iii) firm press releases. We find that job targets are more likely to be met for subsidies with voluntary government press releases but less likely to be met for subsidies with firm press releases.⁷ We interpret the former result as evidence of signaling: governments should be more willing to make *ex ante* voluntary disclosures about subsidies that they believe are more likely to succeed. The latter result may reflect the fact that subsidies accompanied by firm press releases have higher job targets (whereas job targets do not differ based on the presence of the other types of media), which may make them more difficult to meet. This result suggests that the benefit to a firm in signaling its intention to grow in a state may outweigh the costs of a more visible failure should the firm not meet the subsidy's job target.

We next turn to the reputational consequences of firms meeting job creation targets. We first assess direct reputational benefits. We argue that the effect of a firm's reputation in the subsidy setting is most salient in the context of future subsidy awards, in that a state government may not award more subsidies to a firm that failed to meet commitments. Consistent with this notion, we find that firms that meet job targets in one state are more likely to receive subsequent subsidies in that state and others. This effect is stronger in-state than out-of-state and in later years when the subsidy outcome is more likely to be known. Our results suggest that governments account for firms' performance in choosing subsidy recipients and that this effect is strongest locally.

Our final set of tests examines indirect reputational benefits. We consider two measures: (i) media coverage of the firm as well as, considering recent interest in viewing subsidies through the lens of corporate social responsibility, (ii) commercial ESG scores. We find that meeting job targets is associated with more positive subsequent news coverage of recipient firms, consistent with the accrual of reputational benefits. We focus on ESG scores as our second measure of reputation in light of recent literature (e.g., Thomas et al. 2022) that highlights the primacy of these scores in developing firms' nonfinancial reputations. A firm that meets a job target can be thought of as having responsibly handled taxpayer funds; if there are reputational benefits to demonstrating responsibility to the community, these should be reflected in higher ESG scores for firms that meet targets. However, we find no relation between commercial ESG scores—both aggregate scores as well as specific sub-scores capturing performance with respect to employees or the community—and job target completion. These results raise two possibilities: either (i) meeting job targets may not result in reputational gains with socially minded investors, or (ii) ESG scores may not fully capture evidence of firms acting responsibly in their communities.

Our study contributes to a rapidly growing literature in accounting on the antecedents and consequences of targeted subsidies awarded to individual firms. Due to data limitations, studies have focused on either (i) aggregate effects of subsidies on awarding jurisdictions (e.g., De Simone et al. 2022) or (ii) post-subsidy differences

⁷ We do not find a link between the presence of independent journalism about a subsidy and the likelihood that job targets are met. This result is consistent with recent work (Chen et al. 2019) highlighting the limited effect of independent media coverage on corporate tax planning decisions more generally.

in subsidized firms' behavior (e.g., Huang 2022; Drake et al. 2022; Raghunandan 2022). In contrast, our goal is not to assess the extent to which subsidies create a net benefit to taxpayers in subsidizing jurisdictions, nor whether states are on net better off by subsidizing individual firms compared to less targeted tax policies, such as corporate tax rate changes. Our hand-collected data instead allow us to assess, *conditional* on states awarding targeted subsidies, the firm and subsidy factors that determine whether a subsidy succeeds with respect to its stated goals.

Two caveats are in order. First, because we focus on between-recipient heterogeneity, conditional on subsidy receipt, we cannot normatively identify an optimal level of subsidies or job creation targets (or the related issue of whether state governments are setting job targets that are too high or too low). Second, the tests we can feasibly conduct on our data may not always yield causal interpretations. Nonetheless, given the historical difficulty of obtaining data on individual subsidy outcomes, our study represents an initial attempt to understand *firm-level* determinants of subsidy outcomes. Our findings complement research on regulatory and demographic characteristics associated with subsidy performance and, as such, are of immediate relevance to state economic policymakers.

Our study is also relevant to socially conscious investors and the ESG community more generally (e.g., regulators, standard setters, data providers, and academics), who have become interested in understanding the payoffs to taxpayer funds consumed by corporations to better measure individual firms' net impacts on their communities. For instance, the Global Reporting Initiative, a standard setter for sustainability reporting, has advocated for greater transparency related to firms' tax payments and taxpayer subsidies.⁸ The initiative and similar parties seek to understand whether corporate subsidy recipients deliver benefits commensurate with funds received. However, assessing this issue has long been difficult because of a lack of data identifying outcomes for specific pieces of funding. By assessing subsidy-specific job targets, our study overcomes this limitation. In doing so, we provide initial evidence on which firms act as responsible stewards of taxpayer funds, providing benefits to the community that exceed the government's cost of providing these benefits.

Our results on firms' labor practices are also relevant to socially conscious investors and academics. While job target completion data is not publicly available for most subsidies, our results suggest that firms that demonstrate responsibility along another key social dimension may also be more likely to act as responsible stewards of taxpayer funds. Our findings build on recent work that links firms' tax planning practices to other dimensions of ESG performance (e.g., Hoi et al. 2013; Garcia 2016; Kanagaretnam et al. 2018).

⁸ The Global Reporting Initiative is supported by several large institutional investors and consortia. As examples of its call for transparency related to subsidies, see: <https://www.globalreporting.org/standards/standards-development/topic-standard-project-for-tax/>, <https://www.globalreporting.org/news/news-center/backing-for-gri-s-tax-standard/>, and <https://www.globalreporting.org/news/news-center/pwc-supports-global-standards-fund/>.

2 Background and related literature

2.1 Background on subsidy giving

US state governments frequently provide firms with economic incentives to retain or create quality jobs within state borders. The most common type of subsidy targeted to individual firms, in terms of both frequency and the value of incentives awarded, is tax abatements. These tax abatements primarily reflect local- or state-level property tax exemptions, sales tax abatements, or state-level corporate income tax rebates.

The process varies across states, but the key steps are broadly similar for each state. An economic development agency, housed within the state governor's office and run by an individual director appointed by the governor, oversees, and approves the vast majority of subsidies.⁹ The agency typically devises a set of economic incentive programs meant to align with the state's economic priorities. These priorities can include, for example, luring employers in certain industries to locate in the state or providing incentives for companies to create jobs in specific cities or counties within the state. In many cases, the relevant economic priority is simply job creation, irrespective of other factors. For example, Florida's Quick Action Closing Fund is not limited to specific industries or locations but is generally reserved for the highest-profile relocation or retention deals.

For each subsidy program, the state agency provides details on the types of incentives that are provided, the amount of funding available, the structure (e.g., tax break or cash grant), and the typical duration (e.g., whether a tax break lasts for three, five, or 10 years). Applications contain information about the business entity (e.g., name and industry), project information (e.g., the project's purpose, whether the project reflects within-state expansion or relocation from another state, how many jobs the project will create or retain, and a project timeline). While firms submit formal applications to the state agency for these incentives, these typically occur after a period of consultation or negotiation with the agency—and, in many cases, after it has already solicited specific firms.¹⁰ Job targets as well as other subsidy requirements (e.g., with respect to investment) are also typically agreed upon during this consultation period.

Jobs created to fulfill a subsidy's requirements must be full-time and permanent, and contractors do not count toward the number of jobs a firm is deemed to have created (i.e., for a new employee to count toward the job target, that employee must be directly on the subsidized firm's payroll rather than on the payroll of a third-party contractor hired by the firm). Firms are aware of this requirement upon signing

⁹ Some of the largest subsidies awarded by individual states—referred to in the Subsidy Tracker database as “megadeals”—are directly approved by the state legislature and signed into law by the governor, rather than going through the state economic development agency.

¹⁰ Conversations with Good Jobs First and with an anonymous director of a state economic development agency (who directly oversees subsidy grants) suggest that, in most cases, especially for larger firms or subsidies, applications are formally submitted after a period of informal consultation, where the firm and state agree upon the number of jobs the recipient firm will create.

subsidy agreements. Not all economic incentive programs require an explicit job-creation target to be agreed upon, a practice that has invited outside criticism in many cases.¹¹ In many cases, a job creation or retention target is agreed upon by the firm and the state EDA, but this information is not publicly disclosed; these cases do not enter our sample. Variation in disclosure typically occurs across programs rather than across subsidies within a single program, an institutional feature arising from differential program-level disclosure requirements codified into state law (De Simone et al. 2022). Nonetheless, we acknowledge that our sample subsidies may therefore have different target completion rates from those in states or programs where we cannot observe data, for example, because a subsidy program's disclosure requirements may influence which firms it chooses to award subsidies to. Still, the fact that subsidy programs that disclose job target completion do so for *all* recipients—individual firms cannot opt out of providing this data—mitigates at least some of the potential sample selection-related bias in our analyses.

2.2 Related literature

We draw upon research on the causes and consequences of the subsidy-giving process. One strand of this literature examines determinants. For example, Mast (2020) finds that spatial competition by local governments in a region increases the amount of tax breaks firms receive from those local governments. Modeling subsidy grants as an auction, Slattery (2021) quantifies the welfare effect of competition among states and finds that governors facing reelection are more willing to subsidize business relocations. Many other studies find evidence consistent with this reelection effect. For instance, Aobdia et al. (2021) show that firms with more political campaign contributions (PAC) to state elected officials receive more frequent and higher-value subsidies. Aobdia et al. (2021) document that this relation is stronger when state officials face reelection.

Another strand of the literature investigates the consequences of subsidies. Most of these studies focus on employment effects. For example, Slattery and Zidar (2020) find minimal evidence that the largest subsidy packages (i.e., megadeals) result in job creation, while De Simone et al. (2022) find that the success of subsidies in creating jobs varies with the quality of governmental transparency surrounding the subsidy-giving process. In the European setting, Criscuolo et al. (2019) exploit changes in area-specific subsidy eligibility criteria to document that an increase in the maximum allowable subsidy for a particular region improves that region's manufacturing employment. However, the causal effect is concentrated entirely among small

¹¹ As an example, in 2013 Boeing received a subsidy package valued at a total of \$8.7 billion—the single largest subsidy ever awarded as per the Subsidy Tracker database. However, this subsidy did not come with any job targets attached; when Boeing cut the size of its workforce in Washington within a few years of the subsidy being awarded, legislators tried—and failed—to retroactively add job targets to the subsidy (e.g., <https://www.seattletimes.com/seattle-news/politics/bills-would-link-boeing-tax-breaks-to-the-size-of-its-workforce-in-washington/>).

firms rather than the large public firms that we study. Our study also builds on a wide literature in accounting on the operational consequences of tax incentives. For example, Lester (2019) studies the effectiveness of the Domestic Production Activities Deduction in stimulating domestic investment and employment. Our work complements these studies by focusing on firm-specific, rather than statutory, incentive programs.

A theme in this literature is that researchers typically use indirect measures, such as county-level employment growth, new establishments, and housing price, to quantify the economic impact of subsidies. This approach does not differentiate *direct* job creation by subsidy recipients from *indirect* job creation resulting from the subsidy (e.g., food and clothing stores opening in a town where a large new plant commences operations). As a result, while there is a substantial body of work on the geographic and political factors that facilitate successful subsidies in terms of the *overall* effect (direct plus indirect), this literature is largely silent on the role of individual recipient firm characteristics. By studying job-creation targets for individual subsidies given to specific firms, our study fills this void.

We also build on an emerging literature that studies other consequences of subsidies and the extent to which they correlate with and directly reflect other aspects of firms' stakeholder-centric behavior. For example, Raghunandan (2022) finds that subsidy recipients are more likely to subsequently commit misconduct against local stakeholders, arguing that this reflects regulatory capture of the politicians responsible for awarding subsidies. Other studies consider subsidy-led changes in financial reporting and disclosures. For instance, Pappas et al. (2022) find that subsidized firms smooth income more aggressively, which they argue reflects an effort to avoid public scrutiny. Huang (2022) finds that subsidized firms provide more voluntary disclosures of business activity, arguing that this reflects an attempt by firms to show both officials and the public how taxpayers' funds are being used.

Finally, we contribute to an emerging literature—spanning both academia and practice—that views firms' tax planning practices and stewardship of taxpayer funds as an increasingly important aspect of their overall ESG performance (e.g., Fonseca 2020) and attractiveness to socially minded investors. In support of this, recent whitepapers by KPMG and Ernst & Young, among others, highlights the need to consider ESG-minded investors when engaging in tax planning and accepting targeted tax relief.¹² Our study adds to this emerging conversation by documenting substantial heterogeneity, even after accounting for geographic and temporal factors, in the extent to which firms responsibly steward taxpayer funds. In doing so, we also add to the literature examining how firms' tax practices relate to their ESG performance more generally (e.g., Hoi et al. 2013; Kanagaretnam et al. 2018).

¹² See, e.g., <https://home.kpmg/xx/en/home/insights/2021/09/esg-and-tax.html> or <https://www.pwc.com/gx/en/services/tax/publications/tax-is-a-crucial-part-of-esg-reporting.html>.

3 Data and research design

3.1 Subsidy Tracker

We obtain state-level subsidy data from the Subsidy Tracker database, provided by the nonprofit organization Good Jobs First. Subsidy Tracker collects subsidy data from online sources, such as state government websites or media reports, as well as through direct requests (e.g., Freedom of Information Act requests) for subsidy-related information not publicly posted by awarding governments. Subsidy Tracker contains detailed information, including the recipient's name, awarding government agency, subsidy program name, subsidy value, year of the award, location of subsidy, and primary data source from which subsidy information was obtained. Subsidy Tracker also provides information on the type of subsidy, which we aggregate to three primary categories: tax breaks, cash grants, and other. The final category reflects a small proportion of our sample. If the recipient is a subsidiary of a public company, Subsidy Tracker also links the subsidiaries to their public parent companies.¹³ Because of the other data we incorporate, we restrict our sample to firms with publicly traded parents.

3.2 Subsidy job data

Even though the nominal purpose of state-level subsidies is within-state job creation or retention, only some subsidy agreements stipulate the number of jobs to be created or retained. For subsidies that include (and disclose) job requirements, Subsidy Tracker typically contains the number of jobs committed to by a recipient but not the actual number of jobs created or retained. We therefore supplement the subsidy data outlined in Section 3.1 with hand-collected data on the actual number of jobs created or retained. In many cases, we use the data source listed in Subsidy Tracker, whereas in other cases we directly obtain these through internet searches. We can identify this information for 1,964 subsidies awarded by 70 distinct subsidy programs across 20 states from 2004 to 2015. Table 1 lists subsidy programs in our sample across all states, the number of subsidies in each program, and the data source that we obtained job creation information from.

We obtain job creation data from two different types of primary sources. The first type of source is subsidy-specific databases maintained by state governments and posted online. One example is the [Economic Development Incentives Portal](#), launched by the Florida Department of Economic Opportunity in 2014. The portal provides information on subsidy recipients' progress in meeting job creation and retention goals, containing both the number of jobs committed to and the actual number of jobs created by subsidy recipients. The second type of primary source is subsidy programs' annual reports, either published directly by the programs or by

¹³ Subsidy Tracker provides *current* parent-subsidiary linkages. We therefore manually inspect every observation and, in cases where the historical parent-subsidiary linkage differs not exist at the time a subsidy was received, we update the entry to reflect the parent company at the time of subsidy.

Table 1 Data sources of subsidy programs

State	Program	Num	Source
AZ	Arizona Competes Fund	5	AZ CA
CT	Manufacturing Assistance Act	7	CT DECD
	Small Business Express	2	CT DECD
FL	Brownfield Redevelopment Bonus	21	FL DEO
	Brownfield Voluntary Cleanup Tax Credits	3	
	Capital Investment Tax Credit	8	
	High Impact Performance Incentive	5	
	Qualified Defense and Space Contractor Tax Refund	5	
	Qualified Target Industry Tax Refund	159	
	Quick Action Closing Fund	42	
	Community Economic Betterment Account	12	IA EDA
IA	Economic Development Set-Aside	5	
	Enterprise Zones	34	
	Grow Iowa Values Fund	21	
	High Quality Job Creation Program	42	
	High Quality Jobs Program	30	
	New Capital Investment Program	8	
	Physical Infrastructure Assistance	17	
	Value-Added Agricultural Products & Processes Financial Assistance	6	
ID	Tax Reimbursement Incentive	4	ID Commerce
IL	Business Development Public Infrastructure Program	3	IL Accountability
	Corporate Headquarters Relocation Program	1	
	EDGE	200	
	Employee Training Investment Program	72	
	Enterprise Zone	16	
	High Impact Business Designation	6	
	IDOT Economic Development Program	30	
	Large Business Development Assistance Program	20	
KY	Economic Development Bonds	2	KY CED
	High-Tech Investment/Construction Pools	2	
	Incentives for Energy Independence Act	1	
	Kentucky Business Investment Program	86	
	Kentucky Enterprise Initiative Act	54	
	Kentucky Industrial Development Act	27	
	Kentucky Industrial Revitalization Act	2	
	Kentucky Jobs Development Act	31	
Kentucky Jobs Retention Act	1		
LA	Kentucky Rural Economic Development Act	13	
	Enterprise Zone	242	LA Fastlane
	Industrial Tax Exemption	125	
	Quality Jobs	32	
	Restoration Tax Abatement	2	

Table 1 (continued)

State	Program	Num	Source
MD	Job Creation Tax Credit	39	MD Commerce
MI	Business Development Program	30	MI EDC
	Michigan Business Development Program	1	
	Michigan Business Tax–Compensation Tax Credit	1	
MN	Job Opportunity Building Zone	10	MN DEED
	Minnesota Business Assistance	18	
	Minnesota Financial Assistance	1	
MO	Quality Jobs Program	22	MO DED
NC	JDIG	79	NC EOC
	JMAC	2	
	One NC	81	
NJ	Business Employment Incentive Program (BEIP)	65	NJ DEA
NV	Catalyst Fund Grants	2	NV GOED
	TAX ABATEMENT	4	
NY	Excelsior Jobs Program–Investment Track	1	NY ESD
OH	Job Creation Tax Credit	93	OH DSA
	JRTC–NR Job Retention Tax Credit Non-Refundable	2	
	JRTC–R Job Retention Tax Credit Refundable	1	
TX	Texas Economic Development Act	52	TX Comptroller
VA	Commonwealth’s Development Opportunity Fund	17	VA EDP
	Commonwealth’s Development Opportunity Fund (formerly Governor’s Opportunity Fund)	1	
	Virginia Investment Partnership	8	
	Virginia Investment Partnership and Major Eligible Employer Grant	1	
WI	Business Expansion and Retention Investment	1	WI EDC
	Economic Development Tax Credit	21	
	Enterprise Zone	1	
	Jobs Tax Credit	4	
	Workforce Training Grants	2	
Sum		1964	

This table lists the subsidy programs in each state, the number of subsidies in each program, and the data source of the programs

state Departments of Economic Development. For example, the Arizona Commerce Authority administers the Arizona Competes Fund and monitors whether firms that receive subsidies through this fund meet job creation or retention goals over time. The authority releases annual reports specifically about the fund, listing new recipients and updating job creation and retention performance on prior-year recipients.¹⁴ For both types of primary sources, information is disclosed by state law rather than

¹⁴ https://www.azcommerce.com/media/1542324/acf_annualreport_fy17_v7_final.pdf.

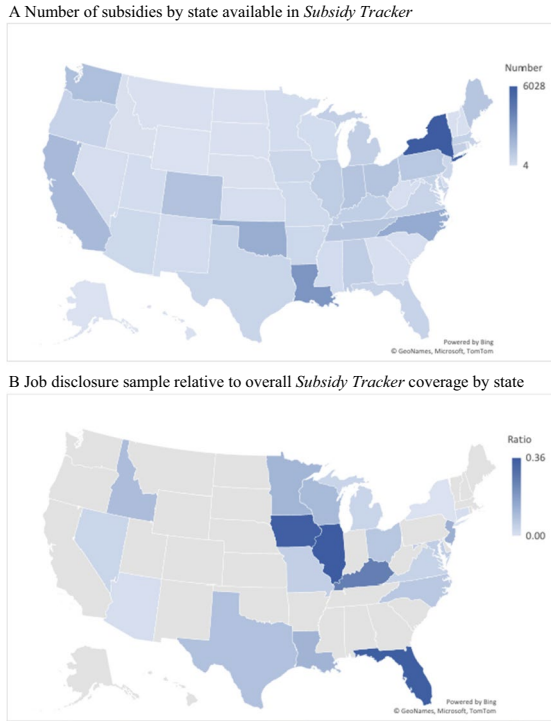


Fig. 1 Distribution of subsidies across the U.S. This figure plots the geographic distribution of state-level subsidies across the United States. Panel A plots the total number of state-level subsidies awarded to U.S. public firms (including their subsidiaries) in each state from 2004 to 2015, as covered by the *Subsidy Tracker* database. To illustrate the representativeness of our subsidy sample coverage, in Panel B, we plot the ratio of the number of subsidies in our sample to that in the broader *Subsidy Tracker* database in each state. As in Panel A, we focus on state-level subsidies awarded to U.S. public firms (including their subsidiaries) from 2004 to 2015. Panels C and D are analogous to Panels A and B but focus on only state-level subsidies *with job targets* awarded to U.S. public firms (rather than all subsidies)

voluntarily by firms, which mitigates potential self-selection bias at the firm level as a driver of our results.

3.3 How representative is the subsidy sample?

Subsidy job disclosure occurs at the behest of state governments and may vary systematically across states and time. To illustrate this point, we compute the ratio of the number of subsidies in our sample to that in the *Subsidy Tracker* database across states and over years. These statistics are summarized in Figs. 1 and 2. Panel A of Fig. 1 plots the total number of state-level subsidies awarded to U.S. public firms (including their subsidiaries) in each state from 2004 to 2015. To give some perspective on how representative our sample is relative to the broader universe of subsidies covered by *Subsidy Tracker* and how this varies cross-sectionally, Panel B of Fig. 1 plots the ratio of the number of the subsidies in our sample to the number of

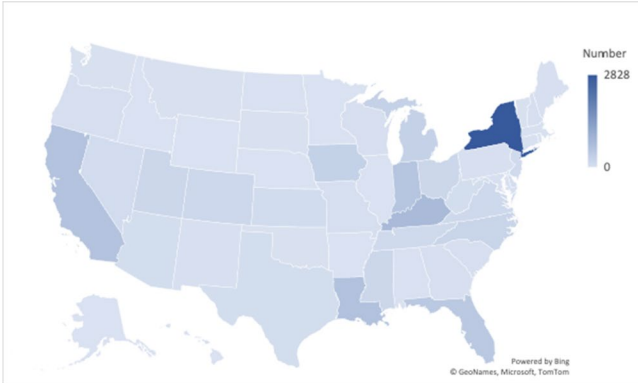
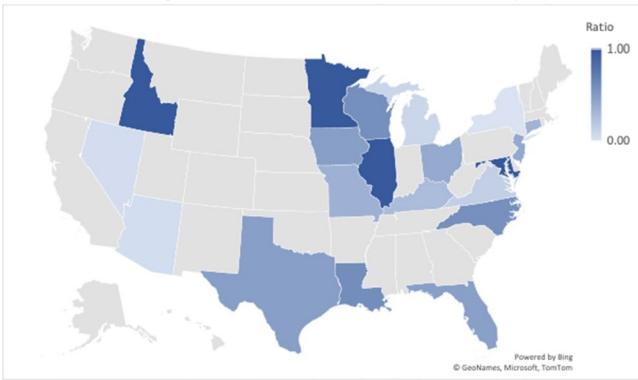
C Number of subsidies with job targets by state available in *Subsidy Tracker*D Job disclosure sample relative to overall *Subsidy Tracker* coverage by state

Fig. 1 (continued)

subsidies in Subsidy Tracker for each state. From this panel, it is apparent that the states where our data is most representative of the broader set of subsidies are Iowa, Illinois, and Florida.

As discussed in Section 2, not all subsidy programs provide information on the job target. Given that our sample is comprised of subsidies that provide both targets and performance, a more natural comparison may be to subsidies that provide the former but not necessarily the latter. We assess representativeness with respect to this set of subsidies in Panels C and D of Fig. 1. From Panel D, it is apparent that the states that appear most representative changes relative to Panel B. While Illinois appears in both, Minnesota, Maryland, and Idaho appear more favorable in Panel D.

In Fig. 2, we summarize the number of public company subsidies as well as the proportion of subsidies to public companies that enter our sample, by virtue of having job disclosure information, in each year. Our data capture 5.1% of all subsidies given to public firms in Subsidy Tracker from 2004 to 2015 (Panel A), highlighting that ex post subsidy-related job disclosure remains an area of improvement for many state economic development agencies. Even when compared to those subsidies with

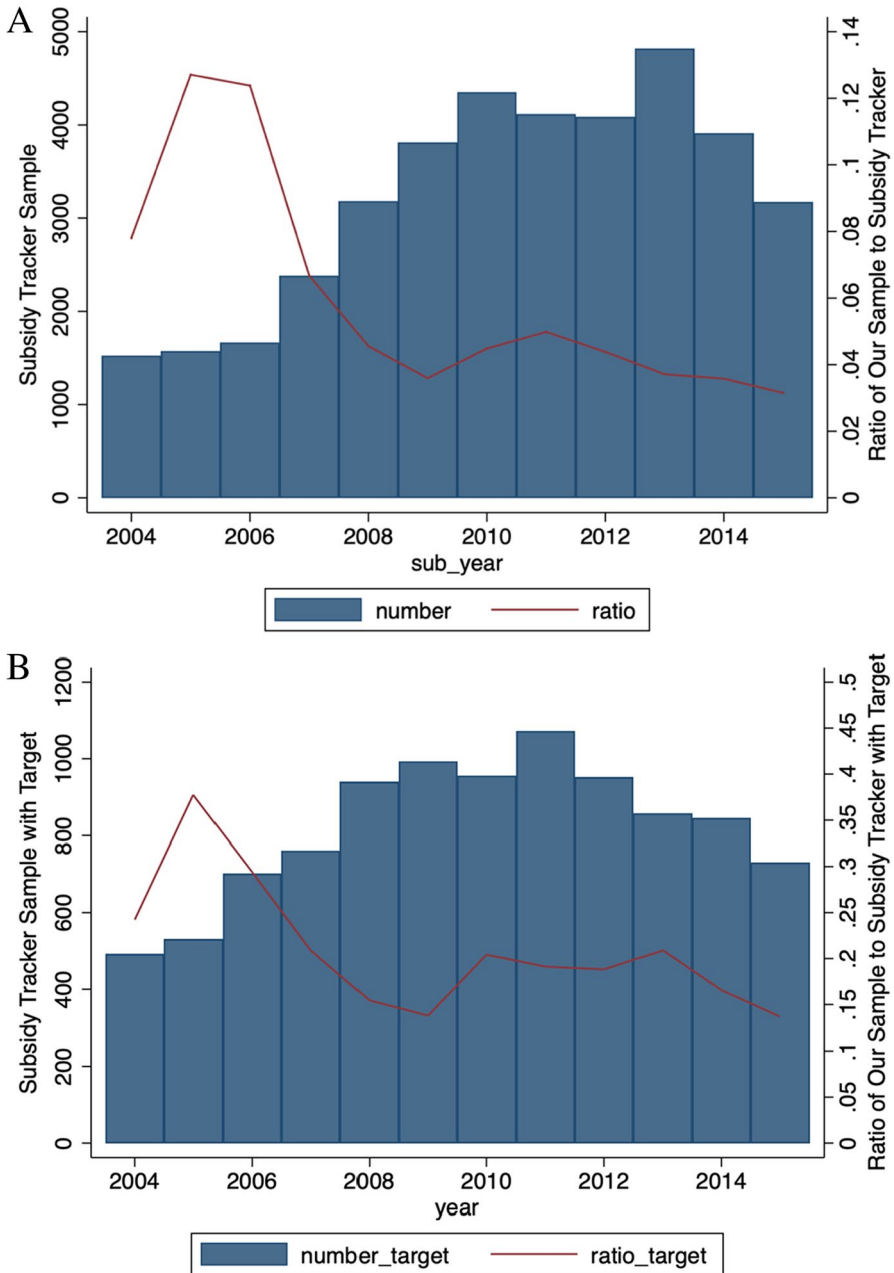


Fig. 2 Time series of subsidies. This figure provides information on the dynamics of the number of state-level subsidies awarded to U.S. public firms (including their subsidiaries) from 2004 to 2015. In Panel A, the bars show the overall number of state-level subsidies awarded to U.S. public firms (including their subsidiaries) from 2004 to 2015, as captured by Subsidy Tracker. The solid line in Panel A represents the ratio of the number of the subsidies in our sample to that in Subsidy Tracker over our sample period. Panel B is analogous but replaces the Subsidy Tracker universe with only those observations in the database that have information on job targets; the solid line in this case represents the proportion of subsidies with both ex ante and ex post job information as a fraction of the number of subsidies that provide job targets

target information (Panel B), our data capture only 20% of subsidies, highlighting a potential inadequacy with many existing disclosure initiatives. The overall number of subsidies awarded per year increases until 2013, where we observe a drop-off. Conversely, the proportion of subsidies with disclosure decreases over time. This may reflect a shift within states to award subsidies through programs with less stringent disclosure requirements, irrespective of whether the disclosure requirements are the primary reason for the shift. Alternatively, this trend could reflect a shift in the composition of states in our sample (e.g., if a state with low disclosure has relatively few subsidies in 2008 but a lot of subsidies in 2014, that will increase the later year's denominator). We caveat that these are only two potential explanations; whether there is a more systematic decrease in state-level subsidy transparency over time is an area potentially ripe for future research by academics or practitioners.

We also examine the extent to which the firms in our sample represent the broader set of publicly traded subsidy recipients. To do so, we assess univariate differences between our sample and the two samples underlying Figs. 1 and 2, that is, all subsidies to public firms as well as those with job target information (though not necessarily target completion information). While disclosure of job target information is driven by state-level policies rather than something state governments can vary on the basis of recipient firm characteristics, the statistics in Panel C of Table 2 suggest the potential need for caution in interpreting our findings; the subsidies with job target completion information in our sample are given to firms that are more profitable, less leveraged, and are growing faster. Differences appear smaller when comparing our sample to the set of subsidies with job targets, relative to the set of all subsidies. Perhaps unsurprisingly, job targets are also lower when ex post performance disclosure is required, relative to subsidies without this requirement.

3.4 Other data

3.4.1 Financial information

To obtain data on subsidy recipients' financial performance and to establish common identifiers for linking with other datasets, we manually match the subsidy data to Compustat. We cannot match 140 (out of 1,549) firm-state-year observations. From Compustat, we obtain data on firm fundamentals, as the likelihood of firms meeting job targets may systematically vary with performance. For instance, larger firms may be more likely to meet job targets (since a given target represents less of their overall workforce), while more leveraged firms may be less likely to meet job targets (as they may be tempted to instead use the funds to pay down debt for instance). In addition to these measures, we control for firms' growth rates and profitability.

3.4.2 Firm-state economic presence

We conduct analyses at the firm-state-year level. A potential issue with this approach is that not every firm operates in every state, and, even in the set of states in which a firm does operate, the extent of its operations may not be the same in each state.

Table 2 Percentage of subsidy job targets met across states and over years**Panel A: Sample composition across states**

State	Program	Sub-sidy	Mean job targets	% Met	Firm-state-year obs
AZ	1	5	381	0.80	5
CT	2	9	1,169	0.89	9
FL	7	243	223	0.48	184
IA	9	175	158	0.49	121
ID	1	4	306	0.25	4
IL	8	348	361	0.73	299
KY	10	219	167	0.31	188
LA	4	401	40	0.90	240
MD	1	39	60	1.00	33
MI	3	32	156	0.75	31
MN	3	29	67	0.48	24
MO	1	22	262	0.50	18
NC	3	162	297	0.30	137
NJ	1	65	233	0.58	64
NV	2	6	164	0.83	6
NY	1	1	50	1.00	1
OH	3	96	158	0.85	92
TX	1	52	40	0.96	37
VA	4	27	137	0.67	27
WI	5	29	415	0.55	29
Total	70	1,964	197	0.63	1,549

Panel B: Sample composition over time

Year	Program	Sub-sidy	Mean jobs targets	% Met	Firm-state-year obs
2004	26	119	195	0.75	101
2005	28	200	179	0.65	134
2006	26	206	163	0.65	144
2007	31	159	236	0.55	128
2008	31	145	198	0.61	110
2009	30	137	212	0.68	106
2010	30	195	186	0.72	159
2011	30	205	146	0.59	164
2012	34	179	216	0.55	144
2013	31	179	205	0.67	151
2014	25	140	227	0.61	119
2015	22	100	250	0.56	89
Total		1,964	197	0.63	1,549

Table 2 (continued)

Panel C: Sample compared to Subsidy Tracker universe							
	(1)	(2)	(3)	(4)	Difference		
	Our sample	Subsidy Tracker					
		w/ targets	w/o targets	All	(1)-(2)	(1)-(3)	(1)-(4)
Subsidies	1,964	9,548	28,130	37,678			
Subsidies with all financials	1,832	9,024	26,691	35,715			
Job targets	196.792	320.409	N/A	N/A	-123.618***		
Market to book	3.510	2.926	3.177	3.113	0.584***	0.333**	0.396***
ROA	0.154	0.147	0.129	0.134	0.007***	0.025***	0.021***
Leverage	0.240	0.257	0.265	0.263	-0.017***	-0.025***	-0.023***
Sales growth	0.102	0.082	0.068	0.072	0.020***	0.034***	0.030***
Log market value	9.220	9.242	9.329	9.307	-0.019	-0.109**	-0.087*
Labor intensity	0.004	0.004	0.003	0.004	-0.000	0.001***	0.001***

This table details the composition of our sample across states and over the years. In Panel A, we provide the number of programs, the number of subsidies, the average number of jobs committed to, the percentage of the subsidies that meet their job targets, and the number of firm-state-year observations in each state in our sample. In Panel B, we present the number of programs, the number of subsidies, the average job targets, the percentage of the subsidies that meet their job targets, and the number of firm-year observations in each year. In Panel C, we provide descriptive characteristics for subsidy recipients in our sample compared to two baselines: (i) the set of all subsidies to publicly traded firms in Subsidy Tracker with job target information, and (ii) the set of all subsidies to publicly traded firms in Subsidy Tracker, irrespective of what job target information is available

While we cannot observe firm-state-year level employment or sales, we observe a highly correlated proxy: the number of establishments that a firm has in a given state in a given year. We obtain this data from Infogroup's historical ReferenceUSA database.

3.4.3 Labor practices

We measure firms' labor practices using data on labor violations from Violation Tracker, also provided by Good Jobs First. Violation Tracker is a comprehensive database of corporate misconduct, covering sanctions from nearly all federal agencies (over 50). We argue, following Raghunandan and Rajgopal (2023), that labor violations represent a firm-level measure of labor practices free from the biases of commercial CSR ratings or surveys often used to assess employee satisfaction or workforce quality. Violation Tracker classifies each type of violation into one of nine distinct categories: competition, consumer protection, employment, environment, financial, government contracting, healthcare, workplace safety, and miscellaneous. We classify "employment" and "workplace safety" violations as reflective of poor labor practices and all other types of violations as being unrelated to labor.

3.4.4 Political contributions

We obtain data on political contributions to state-level election campaigns from the National Institute on Money in State Politics, following Aobdia et al. (2021). The data, derived from mandatory filings, represent a comprehensive source of information on political contributions to all types of state politicians (governor, state house and senate, attorney general, etc.). The data provide information at the individual contribution level, which we aggregate to the firm-state-year level across different types of state politicians. The data also contain information on election years by state, allowing us to identify contributions made in election years versus other years (a distinction we exploit later). We fuzzy-match this information to Compustat (and the subsidy data) based on firm names.

4 Are job targets met?

We begin our analyses by addressing a core descriptive question: how frequently do subsidized firms meet job targets? Before doing so, it is helpful to understand the scope of firms' job creation commitments. We provide graphical evidence along these lines in Fig. 3, where we plot the distribution of the number of jobs committed to in our sample. For ease of interpretation, we truncate this figure above at 1,000; subsidies that require more than 1,000 jobs to be created are quite rare in our sample (only 58 out of 1,964 underlying subsidies). Immediately evident from Fig. 3 is the right-skewness of the distribution of commitments by firms: the mean and median jobs promised are 145 and 70, respectively.

To assess how frequently subsidy recipients retain or create the number of jobs they commit to, we provide descriptive evidence in Table 2. The data underlying Table 2 comprise 1,964 individual subsidies in 1,549 distinct firm-state-years, awarded to 696 unique public companies from 2004 to 2015. Panel A of Table 2 shows that around 63% of the job targets in our sample are met. However, we observe substantial geographic variation: of states with more than 10 subsidy observations, the figure is as low as 31% in Kentucky and as high as 100% in Maryland. Panel A of Table 2 shows that the average number of jobs committed to at the individual-subsidy level in our sample is 197. However, we again observe substantial geographic variation: of states with at least 10 subsidy observations, the figure is as low as 40 in Texas and as high as 415 in Wisconsin. This heterogeneity underscores the importance of controlling for state-level factors in our research design.

Panel B of Table 2 provides details on temporal dynamics of the percentage of job targets met over time. These figures range from 75% in 2004 to 55% in 2007. Panel B in Table 2 also displays the time trend of the average number of jobs committed to. These figures range from 250 jobs committed per subsidy in 2015 to 146 jobs committed per subsidy in 2011. Both the percentage of the job targets met, and the average number of jobs committed to vary less over time than across states.

In Fig. 4, we document the extent to which subsidy recipients meet or miss their commitments. Figure 4 shows the distribution of the percentage of the committed number of jobs that are created or retained. In Fig. 4, we only include subsidies

when the actual number created or retained is less than five times the committed number but is strictly greater than zero. The reason for the latter is that we cannot distinguish the case where the actual number of jobs created or retained is zero from the case where the subsidy is interrupted. Two stylized facts emerge from Fig. 4. First, many subsidies have job target completion percentages right above 100%, suggesting that many subsidy recipients receive just enough incentives to marginally meet committed job targets. Second, we observe substantial variation: some recipients seem to create or retain several times more jobs than what they committed to while others only create a small fraction of their commitment.

5 What determines who meets subsidy job targets?

We show in Section 4 that more than one-third of subsidy recipients do not meet job-creation targets. We therefore turn to two natural follow-up questions: which firms fail to meet targets and why? We consider both firm-level factors (financial performance) and firm-state-level factors (labor practices and politically motivated subsidies) as potential determinants. In addition, we provide evidence on the role of voluntary firm- and government-level disclosures in facilitating target completion. In all cases, we measure determinant variables in the year that a subsidy was awarded so as to rely on information that a state government could feasibly have had access to when deciding whether to award a subsidy.

5.1 Firms' financial performance

We begin by considering firms' financial performance as a potential determinant of subsidy job target completion. The subsidies in our sample are not the megadeals, often worth hundreds of millions or even billions of dollars, that states use to lure companies into shifting the location of substantial portion of their operations. While research (e.g., Cohen et al. 2011) argues that subsidies may crowd out private investment, these studies consider subsidies large enough to meet firms' financing needs on a standalone basis. However, for the subsidies we study, the funding from a subsidy alone is likely insufficient to ensure job creation by recipient firms. As a result, these subsidies will only succeed in achieving job-creation targets if firms treat them as complements to their own private investment. We posit that firms with greater capability to make such investment may be more willing to do so, which would translate to firms with stronger financial performance being more likely to meet job creation targets. In this section, we directly test this assertion.

We consider several measures of financial performance at the time when subsidies are granted. These include profitability, measured as return on assets (ROA); growth rate, measured as the ratio of current-year sales to prior-year sales minus one; and leverage, measured as the ratio of debt to assets.¹⁵ In addition, we control

¹⁵ We do not consider other measures, such as firms' capital expenditures (capex), for data availability reasons: given our small sample size, the attrition caused by unavailable capex data would lead to low-powered tests.

for other firm- and firm-state level fundamentals (size, market to book, labor intensity, and the number of establishments the firm has in the given state), also measured as of the time the firm receives the subsidy.

We also control for two key subsidy characteristics: the underlying job target and the type of subsidy (tax break or cash grant). We control for the former to account for the possibility that higher job targets are mechanically harder to meet. We control for subsidy type because tax breaks and cash grants affect firms differently; cash grants are typically paid as a lump sum but do not affect a firm's costs of doing business in a state in future years, while tax breaks are realized over time and reflect a longer-term reduction in the cost of doing business in a subsidizing state. Table 3 provides descriptive statistics, while Appendix Table 10 provides more detail on our variables. We then estimate the following linear probability model¹⁶:

$$\text{Job target met}_{i,s,t} = \beta_0 + \beta_1 \text{Financials}_{i,t} + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \quad (1)$$

The dependent variable in Eq. (1), *Job target met*_{*i,s,t*}, is an indicator variable that equals one if firm *i* meets *all* job targets pertaining to subsidies awarded in year *t* in state *s* and zero otherwise.¹⁷ Because of how we construct this variable, the mean of *Job target met*_{*i,s,t*} as documented in Table 3 is 59%, slightly lower than the 63% figure corresponding to the rate of success in meeting job targets at the individual subsidy level.

Our research design accounts for state-year heterogeneity via state-by-year fixed effects $\delta_{s,t}$. State-by-year fixed effects capture the heterogeneity documented in Section 4 as well as the effects of unobservable state-level policy shifts or economic conditions that may affect the distribution of recipient firms or the likelihood that recipient firms meet job targets. For example, legislation designed to improve subsidy-related transparency and accountability has been introduced at the state (rather than program) level in a staggered fashion over time. However, the effects of such legislation will be subsumed by our state-by-year fixed effects. Some states may also set easier targets, for example, if local politicians have incentives to demonstrate subsidy success. Our state-by-year fixed effects account for this heterogeneity as well as the possibility that it is time-varying. We also employ two-digit NAICS industry-by-year fixed effects $\lambda_{j,t}$ to account for the possibility that job creation and hence target completion may be systematically harder in certain economic sectors at certain times as a result of macroeconomic factors.

Table 4 presents results from estimating Eq. (1). We consider ROA, leverage, and growth separately in columns (1)–(3), while, in column (4), we consider all three measures. We find some evidence of a relation between financial strength and the

¹⁶ We estimate a linear probability model for two reasons. First, our fixed effects structure means that nonlinear methods, such as probit and logit, are susceptible to the incidental parameters problem. Second, we view it as more straightforward to interpret coefficients in terms of marginal effects.

¹⁷ We measure job target completion as an indicator rather than a continuous variable because we do not observe the exact number of jobs created—only an indicator for whether a target was met—for nearly half of our sample.

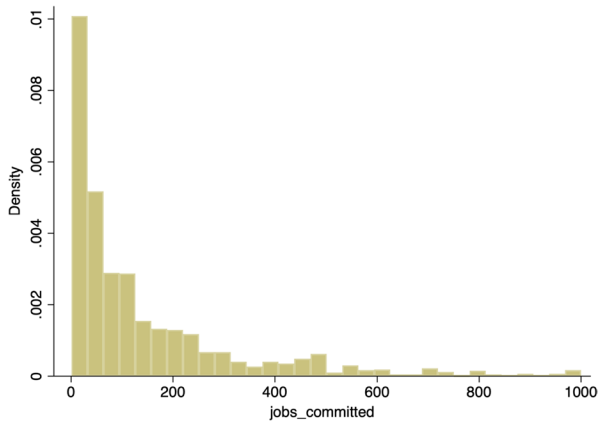


Fig. 3 Distribution of the number of jobs promised. This figure plots the distribution of the number of jobs promised for the subsidies covered by our sample. Our sample focuses on state-level subsidies awarded to U.S. public firms (including their subsidiaries) from 2004 to 2015. For ease of visual presentation, the distribution is truncated at 1,000 jobs because subsidies with more than 1,000 jobs committed are rare in our sample (59 out of 1,964 subsidies). The term density refers to the relative frequency of the given histogram bin

likelihood of meeting job creation targets: job target completion is higher in more profitable firms. Columns (2) and (3) also suggest a potential relation between job target completion and both higher growth and lower leverage, though these variables are insignificant in column (4). Table 4 also suggests that, as expected, job target completion is lower for subsidies that have higher targets. Hence, to the extent that meeting a target is a function of both (i) firms' incentives to meet a target and (ii) the target level, we assess whether our results reflect profitable firms receiving more favorable targets or being more likely to meet otherwise similar targets. In column (5), we estimate an alternative specification of Eq. (1), where we use the natural logarithm of the job target as the dependent variable rather than as a control. We find no relation between our financial metrics and job targets, suggesting that the results in column (4) reflect differences in target completion likelihood rather than the targets themselves.

Our finding that job target completion relates to profitability complements recent studies that focus on the link between firms' receipt of subsidies and their subsequent financial reporting practices and performance (Drake et al. 2022; Huang 2021; Pappas et al. 2022). Those studies' primary focus is on the ex post, rather than ex ante, financial performance of recipient firms; our results suggest a potential selection effect may also be in play. Our results also suggest that politicians responsible for awarding subsidies may need to balance shorter-term incentives (e.g., job creation) against longer-term incentives (e.g., fostering healthy competition). While the results in Table 4 suggest that subsidies are more likely to result in job creation when given to more profitable firms, these are also the firms most likely to establish

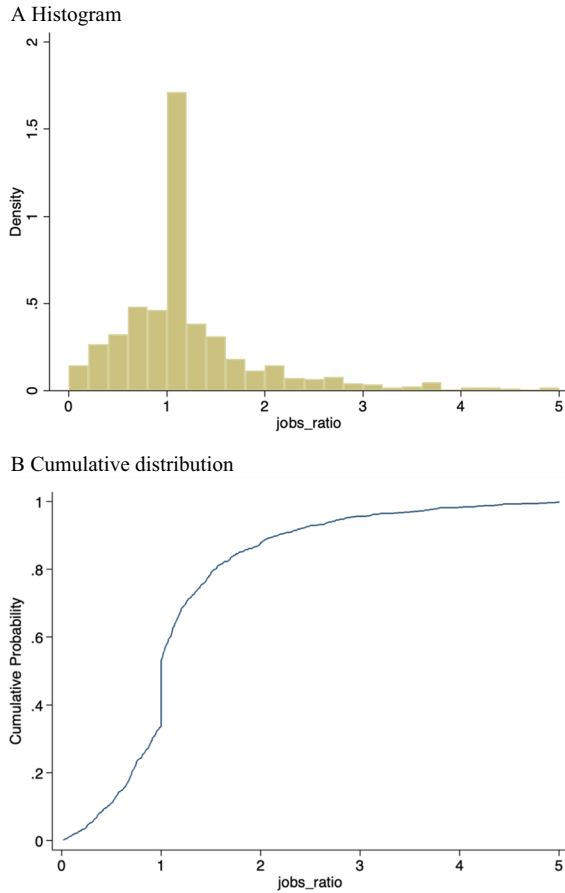


Fig. 4 Distribution of the percentage of promised jobs that are actually created. This figure provides two plots related to the percentage of promised jobs that are actually created, using the subsample of 1,058 subsidies (out of a total of 1,964) where we can obtain data on the precise number of jobs created. Panel A plots the distribution of the ratio of the number of jobs actually created to the number of jobs committed to. Panel B plots the cumulative distribution of this metric. The term density in Panel A refers to the relative frequency of the given histogram bin

market dominance. Unfortunately, we cannot speak to the potential longer-term effects of preferentially awarding subsidies to these firms.

We also observe, in Table 4, a negative relation between labor intensity and job target completion. This result is consistent with the idea that firms with more employees (relative to size and accounting for industry via our fixed effects) may also have less room to add additional jobs. More generally, the negative relation between labor intensity and job target completion also highlights the potential importance of a firm’s labor practices to the likelihood it will meet job creation targets, a point we examine in greater detail next.

Table 3 Summary statistics

	Mean	Median	SD	p10	p25	p75	p90	N
Outcome variables								
Job target met	0.594	1	0.490	0	0	1	1	1549
Job target	4.492	4.575	1.494	2.398	3.526	5.521	6.342	1549
Same-state future subsidies (t+1 to t+4)	0.381	0	0.486	0	0	1	1	1549
Same-state future subsidies (t+1 to t+2)	0.329	0	0.470	0	0	1	1	1549
Same-state future subsidies (t+3 to t+4)	0.259	0	0.438	0	0	1	1	1549
Out-of-state future subsidies (t+1 to t+4)	0.308	0	0.462	0	0	1	1	1549
Out-of-state future subsidies (t+1 to t+4)	0.262	0	0.440	0	0	1	1	1549
Out-of-state future subsidies (t+1 to t+4)	0.208	0	0.406	0	0	0	1	1549
KLD overall (t+1 to t+4)	1.044	0.250	3.635	-2.667	-1.333	3.250	6	816
Asset4 overall (t+1 to t+4)	50.390	50.779	19.702	22.135	35.260	66.608	76.073	1062
Sentiment (t+1 to t+4)	0.91	1	0.286	1	1	1	1	1549
Controls								
Same-state lobby	0.118	0	0.323	0	0	0	1	1549
Out-of-state lobby	0.250	0	0.433	0	0	1	1	1549
Same-state labor penalties (t-2 to t)	0.141	0	0.349	0	0	0	1	1549
Same-state non-labor penalties (t-2 to t)	0.067	0	0.250	0	0	0	0	1549
Out-of-state labor penalties (t-2 to t)	0.434	0	0.496	0	0	1	1	1549
Out-of-state non-labor penalties (t-2 to t)	0.269	0	0.443	0	0	1	1	1549
Reelection	0.195	0	0.396	0	0	0	1	1549
Log # same-state estabs	1.715	1.386	1.696	0	0	2.708	4.304	1549
Log # out-of-state estabs	3.511	3.761	2.930	0	0	5.704	7.585	1549
Government press release	0.246	0	0.431	0	0	0	1	1549
Corporate press release	0.040	0	0.196	0	0	0	0	1549
Independent journalism	0.097	0	0.297	0	0	0	0	1549
Market to book	4.085	2.640	11.035	1.178	1.714	4.029	6.602	1409
ROA	0.155	0.145	0.089	0.054	0.100	0.198	0.262	1409
Leverage	0.238	0.223	0.156	0.027	0.124	0.338	0.454	1409
Sales growth	0.109	0.074	0.238	-0.064	0.010	0.162	0.313	1409
Log market value	9.106	9.170	1.847	6.833	7.816	10.466	11.524	1409
Labor intensity	0.004	0.003	0.005	0.000	0.001	0.005	0.010	1409
Cash grant	0.249	0	0.432	0	0	0	1	1549
Tax break	0.728	1	0.445	0	0	1	1	1549

This table provides descriptive statistics for the dependent and independent variables used in our tests. The statistics include the mean, median, standard deviation, 10th percentile, 25th percentile, 75th percentile, 90th percentile, and number of observations. Continuous variables are winsorized at first and 99th percentiles in each year

Table 4 Financial performance and job targets

Column	(1)	(2)	(3)	(4)	(5)
Dependent variable	Job target met				Job target
ROA	0.363** (2.828)			0.265* (1.925)	-0.055 (-0.089)
Leverage		-0.190* (-1.906)		-0.162 (-1.573)	0.339 (1.129)
Sales growth			0.093** (2.188)	0.063 (1.244)	0.144 (1.052)
Log # same-state estabs	0.020* (1.830)	0.020* (1.760)	0.020* (1.774)	0.021* (1.938)	0.001 (0.049)
Market to book	0.000 (0.690)	0.001 (1.696)	0.001 (1.349)	0.001 (1.388)	-0.005*** (-3.806)
Log market value	-0.006 (-0.785)	-0.004 (-0.460)	-0.002 (-0.252)	-0.005 (-0.613)	0.128*** (5.117)
Labor intensity	-10.523* (-2.048)	-10.126* (-2.024)	-8.293 (-1.550)	-10.934** (-2.248)	19.805 (1.399)
Cash grant	0.049 (0.525)	0.043 (0.462)	0.048 (0.533)	0.052 (0.587)	1.061*** (6.514)
Tax break	0.010 (0.134)	0.009 (0.111)	0.018 (0.229)	0.014 (0.187)	0.353 (1.177)
Job target	-0.011* (-1.977)	-0.010* (-1.959)	-0.011* (-1.964)	-0.010* (-1.929)	
Constant	0.625*** (5.145)	0.695*** (6.104)	0.618*** (5.350)	0.653*** (5.403)	2.677*** (7.591)
Observations	1,345	1,345	1,345	1,345	1,345
R-squared	0.373	0.373	0.372	0.375	0.481
State-by-Year FE	YES	YES	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.194	0.194	0.192	0.196	0.333

This table examines whether firms with stronger financial performance are more likely to meet job targets. The dependent variable in columns (1) to (4) is an indicator variable that equals one if firm i meets all job targets of the subsidies awarded in year t in state s and zero otherwise. The independent variables of interest are three financial measures: firms' return on assets (ROA), leverage, and sales growth rate. In columns (1) to (3), we consider these three variables one at a time, while, in column (4), we consider all three financial measures simultaneously. In column (5), we follow the specification in column (4) but replace the outcome variable with *Job target*, the logarithm of the number of jobs promised. All variables are defined in the Appendix Table 10. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t-statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively

5.2 Firms' labor practices

Politicians often claim that subsidy recipients create high-quality jobs, with such statements frequently supported by subsidy program requirements (e.g., Florida's Quick Action Closing Fund as discussed in the introduction). While there is no formal definition of a high quality job, in practice these sorts of jobs are characterized by two things: (i) fair compensation and (ii) safe workplaces. We examine these characteristics through the lens of workplace misconduct, which captures the *lack* of either fair compensation or a safe environment. We argue that firms with a history of violations do not provide high quality jobs. If these firms are required to adhere to job quality provisions in exchange for receiving a subsidy, their lack of a track record may make job creation—and hence meeting job targets—harder. To empirically test this argument, we estimate the following:

$$\begin{aligned} \text{Job target met}_{i,s,t} = & \beta_0 + \beta_1 \text{Same state labor penalties } (t-2 \text{ to } t)_{i,s,t} \\ & + \beta_2 \text{Same state nonlabor penalties } (t-2 \text{ to } t)_{i,s,t} \quad (2) \\ & + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \end{aligned}$$

The dependent variable in Eq. (2), *Job target met*_{*i,s,t*}, is as in Eq. (1). The independent variables of interest are *Same-state labor penalties (t-2 to t)*_{*i,s,t*} and *Same-state nonlabor penalties (t-2 to t)*_{*i,s,t*}. *Same-state labor penalties (t-2 to t)*_{*i,s,t*} is an indicator that equals one if firm *i* incurs labor penalties in the awarding state *s* in the three years prior to and including the subsidy year (i.e., *t-2* to *t*). *Same-state non-labor penalties (t-2 to t)*_{*i,s,t*} is defined analogously for nonlabor penalties in the awarding state. Labor violation data, obtained from Violation Tracker, primarily reflects firms being sanctioned by the Wage and Hour Division and the Occupational Safety and Health Administration, for wage violations and workplace safety violations. Control variables *X*_{*i,s,t*} include *Log # same-state estabs*_{*i,s,t*}, *Market to book*_{*i,t*}, *ROA*_{*i,t*}, *Leverage*_{*i,t*}, *Sales growth*_{*i,t*}, *Log market value*_{*i,t*}, *Labor intensity*_{*i,t*}, *Cash grant*_{*i,s,t*}, *Tax break*_{*i,s,t*}, and *Job target*_{*i,s,t*}. All variables are defined in Appendix Table 10.

We provide results from estimating Eq. (2) in Table 5. In column (1), we include our full sample of subsidy firm-state-years by omitting financial control variables that limit the sample size. We augment this model with financial performance variables in column (2). Consistent with our prediction, the coefficient on *Same-state labor penalties (t-2 to t)*_{*i,s,t*} in column 2 is statistically significantly negative and implies that subsidy recipients that incur labor penalties for violations committed in state *s* during the three years before receiving the subsidies in year *t* (i.e., *t-2* to *t*) are 10.2 percentage points less likely to meet job targets. The coefficient on *Same-state nonlabor penalties (t-2 to t)*_{*i,s,t*} is insignificant. This result, in conjunction with our results for labor violations, supports our argument that poor labor practices inhibit a firm's ability to create quality jobs, which, in turn, reduces that firm's likelihood of meeting subsidy-related job targets.

We conduct a falsification test in column (3). We replace the same-state violation variables with indicators for the presence of violations committed in other states:

Table 5 Are firms with a record of misconduct less likely to meet job targets?

Column	(1)	(2)	(3)	(4)
Dependent variable	Job target met			Job target
Same-state labor penalties (t-2 to t)	-0.128*** (-7.975)	-0.102*** (-5.016)	-0.097*** (-3.897)	0.044 (0.589)
Same-state non-labor penalties (t-2 to t)	-0.001 (-0.014)	-0.004 (-0.077)	0.003 (0.059)	-0.201** (-2.220)
Out-of-state labor penalties (t-2 to t)			-0.001 (-0.046)	-0.047 (-0.847)
Out-of-state non-labor penalties (t-2 to t)			-0.039 (-1.357)	-0.081 (-0.708)
Log # same-state estabs	0.021** (2.273)	0.024** (2.266)	0.025** (2.549)	0.010 (0.341)
Market to book		0.001 (1.374)	0.001 (1.271)	-0.005*** (-3.156)
ROA		0.254* (1.932)	0.228* (1.853)	-0.135 (-0.216)
Leverage		-0.141 (-1.371)	-0.139 (-1.382)	0.352 (1.114)
Sales growth		0.068 (1.340)	0.077 (1.543)	0.179 (1.096)
Log market value		-0.004 (-0.426)	-0.001 (-0.068)	0.143*** (4.798)
Labor intensity		-10.759** (-2.171)	-10.828** (-2.155)	19.528 (1.469)
Cash grant		0.050 (0.591)	0.051 (0.605)	1.070*** (6.830)
Tax break		0.010 (0.133)	0.010 (0.134)	0.359 (1.227)
Job target		-0.010* (-1.767)	-0.011* (-1.846)	
Constant	0.581*** (65.903)	0.651*** (5.358)	0.636*** (4.928)	2.586*** (7.132)
Observations	1,495	1,345	1,345	1,345
R-squared	0.374	0.379	0.380	0.483
State-by-Year FE	YES	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.213	0.199	0.199	0.333
Labor = Non-labor (F Statistics)	15.53***	4.09*		
Same-state = Out-of-state (F Statistics)			4.53**	

This table examines whether firms with a history of workplace misconduct (measured using federal penalties) are less likely to meet subsidy-related job targets. The dependent variable in columns (1) to (3) is an indicator variable that equals one if the firm meets all job targets of the subsidies awarded in year t in state s and zero otherwise. The independent variables of interest are four penalty measures. Same-

Table 5 (continued)

state labor penalties ($t-2$ to t) $_{i,s,t}$ is an indicator variable that equals one if firm i incurs labor penalties in the award states during the three years before receiving the subsidy in year t ($t-2$ to t) and zero otherwise. Same-state nonlabor penalties ($t-2$ to t) $_{i,s,t}$ is defined analogously for nonlabor penalties in the award state. Out-of-state labor penalties ($t-2$ to t) $_{i,t}$ is an indicator variable that equals one if firm i incurs labor penalties in any non-award states during three years before receiving the subsidy in year t ($t-2$ to t) and zero otherwise. Out-of-state non-labor penalties ($t-2$ to t) $_{i,t}$ is defined analogously for the nonlabor penalties in the non-award states. In columns (1) and (2), we examine whether receiving penalties in the award state is correlated with the likelihood of meeting job targets of the subsidies in the same state. In column (3), we further add penalties in the non-awarding states as a placebo test. In column (4), we follow the specification in column (3) but replace the outcome variable with *Job target*, the logarithm of the number of jobs promised. All variables are defined in the Appendix Table 10. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t-statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. We provide, at the bottom of columns (1) and (2), test statistics comparing the coefficient on *Same-state labor penalties* to the coefficient on Same-state non-labor penalties for each specification separately. We also provide, at the bottom of column (3), the test statistic comparing the coefficient on Same-state labor penalties to the coefficient on *Out-of-state labor penalties*

Out-of-state labor penalties (t-2 to t) $_{i,t}$ and *Out-of-state nonlabor penalties (t-2 to t)* $_{i,t}$. *Out-of-state labor penalties (t-2 to t)* $_{i,t}$ is an indicator that equals one if firm i incurs labor penalties in any non-award states during the three years before receiving the subsidy in year t ($t-2$ to t). *Out-of-state nonlabor penalties (t-2 to t)* $_{i,t}$ is defined analogously for nonlabor penalties in non-awarding states. This approach addresses whether the relevant construct is a firm's labor practices *in the subsidizing state* versus more generally in any nonsubsidizing states (allowing for within-firm heterogeneity in labor practices). We find that the coefficient on *Out-of-state labor penalties (t-2 to t)* $_{i,t}$ is statistically insignificant, suggesting that a firm's history of labor practices within a subsidizing state may be more informative than its history of labor practices in other states.

Finally, in column (4) of Table 5, we assess whether our results relate to target completion or the underlying targets themselves. For example, firms with a history of labor violations could receive more ambitious targets by states that are more wary of these firms' labor practices, which in turn makes them more likely to fail to meet their targets. To test this possibility, we estimate a modified version of Eq. (1) that uses log job targets as the dependent variable rather than a control. We find no relation between prior labor violations and underlying job targets, suggesting that the results in columns (1)–(4) reflect a relation between labor practices and job target meeting, rather than between labor practices and job target setting.

5.3 Politically motivated subsidies

We next test whether firms are less likely to meet job targets associated with politically motivated subsidies. Existing literature (Jensen and Malesky 2018; Jensen et al. 2015) finds that politicians frequently award subsidies for political reasons, such as personal career concerns. Slattery (2021) additionally documents that more

subsidies are awarded in years that an incumbent governor is seeking reelection. Some of these election-year subsidies are likely to be politically motivated: a form of quid pro quo in exchange for recipient firms' campaign support. We argue that subsidies that are not awarded because of compelling economic need may yield fewer economic benefits. To test this assertion, we first identify as potentially politically motivated those subsidies given to firms that contribute to campaigns in re-election years.¹⁸ We then exploit cross-state variation in election years to test whether politically motivated subsidies are less likely to succeed in job target completion. We estimate the following regression:

$$\begin{aligned} \text{Job target met}_{i,s,t} = & \beta_0 + \beta_1 \text{Same state lobby}_{i,s,t} \\ & + \beta_2 \text{Same state lobby}_{i,s,t} \times \text{Reelection}_{s,t} \\ & + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \end{aligned} \quad (3)$$

The independent variables of interest in Eq. (3) are *Same-state lobby*_{*i,s,t*} as well as the interaction term *Same-state lobby*_{*i,s,t*} × *Reelection*_{*s,t*}. *Same-state lobby*_{*i,s,t*} is an indicator that equals one if firm *i* makes political contributions to politicians in state *s* in year *t*. *Reelection*_{*s,t*} is an indicator that equals one when the incumbent governor in state *s* runs for reelection. We use the same set of controls as in Table 5.

We present results from estimating Eq. (3) in Table 6. Consistent with our prediction, the coefficient on *Same-state lobby*_{*i,s,t*} × *Reelection*_{*s,t*} in columns (1) and (2) is statistically significantly negative and suggests that subsidy recipients that make political contributions to officials in state *s* in year *t* are 12.6–15.0 percentage points less likely to meet their job targets when the subsidies are awarded by incumbents seeking reelection during reelection years. Notably, the coefficient on *Same-state lobby*_{*i,s,t*} is insignificant, suggesting that potential quid pro quo is concentrated in reelection years when politicians' incentives are strongest. Our results highlighting the role of connections in reelection years are also consistent with those of Slattery (2021) and Jensen et al. (2015).

As a falsification test, in column (3) we include *Out-of-state lobby*_{*i,t*}, an indicator that equals one if firm *i* makes political contributions to officials in any non-awarding states (i.e., states other than *s*) in year *t*. We argue that political contributions made to other states cannot form part of an in-state quid pro quo arrangement that may result in less effective subsidies; if the results in columns (1) and (2) of Table 6 reflect the negative impact of politically motivated subsidies, we should not see similar results for out-of-state campaign contributions. The results in column (3) support this argument.

In column (4) of Table 6, we replace the dependent variable in Eq. (3) with the natural logarithm of the number of jobs promised. When estimating this specification, the coefficient on *Same-state lobby*_{*i,s,t*} × *Reelection*_{*s,t*} is insignificant. This result

¹⁸ Because Subsidy Tracker only contains the year but not the exact date a subsidy was awarded, we cannot precisely identify whether a subsidy preceded a campaign contribution or vice versa within a given year.

Table 6 Are politically motivated subsidies less likely to succeed?

Column	(1)	(2)	(3)	(4)
Dependent variable	Job target met			Job target
Same-state lobby	0.019 (0.386)	0.004 (0.097)	0.024 (0.561)	0.230** (2.794)
Same-state lobby*Reelection	-0.126* (-1.968)	-0.150** (-2.454)	-0.129* (-2.078)	0.057 (0.242)
Out-of-state lobby			-0.032 (-0.706)	-0.037 (-0.468)
Out-of-state lobby*Reelection			-0.026 (-0.501)	0.008 (0.058)
Log # same-state estabs	0.018** (2.596)	0.023** (2.343)	0.023** (2.321)	-0.008 (-0.329)
Market to book		0.000 (0.844)	0.001 (1.197)	-0.005*** (-3.054)
ROA		0.258* (1.808)	0.248 (1.574)	-0.075 (-0.122)
Leverage		-0.157 (-1.513)	-0.151 (-1.522)	0.326 (1.029)
Sales growth		0.065 (1.299)	0.068 (1.382)	0.145 (0.980)
Log market value		-0.003 (-0.360)	-0.000 (-0.029)	0.118*** (4.130)
Labor intensity		-11.089** (-2.300)	-10.954** (-2.187)	19.776 (1.359)
Cash grant		0.055 (0.631)	0.054 (0.619)	1.059*** (6.287)
Tax break		0.017 (0.219)	0.018 (0.232)	0.367 (1.193)
Job target		-0.010* (-1.998)	-0.010* (-2.005)	
Constant	0.569*** (44.605)	0.628*** (5.712)	0.611*** (5.248)	2.764*** (7.014)
Observations	1,495	1,345	1,345	1,345
R-squared	0.369	0.377	0.377	0.483
State-by-Year FE	YES	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.207	0.196	0.195	0.333
Same-state = Out-of-state interactions (F Statistics)			1.26	

This table examines whether firms that make political contributions to state election campaigns are less likely to meet job targets. The dependent variable in columns (1) to (3) is an indicator variable that equals one if firm i meets all job targets of the subsidies awarded in year t in state s and zero otherwise. The independent variables of interest are *Same-state lobby* _{i,s,t} , *Out-of-state lobby* _{i,t} , and their interaction

Table 6 (continued)

terms with $Reelection_{s,t}$. $Same\text{-}state\ lobby_{i,s,t}$ is an indicator variable that equals one if firm i makes any political contributions to officials in state s in year t and zero otherwise. $Out\text{-}of\text{-}state\ lobby_{i,t}$ is an indicator variable that equals one if firm i makes any political contributions to any of states other than s in year t and zero otherwise. $Reelection_{s,t}$ is an indicator variable that equals one when an incumbent governor in state s campaigns for reelection and zero otherwise. In columns (1) and (2), we study whether making political contributions to the award state s in year t is correlated with the likelihood of meeting job targets of the subsidies in the same state. In column (3), we further add same-state political contributions with out-of-state political contributions as a placebo test. In column (4), we follow the specification in column (3) but replace the outcome variable with $Job\ target$, the logarithm of the number of jobs promised. All variables are defined in the Appendix Table 10. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t-statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively. We provide, at the bottom of column (3), the test statistic comparing the coefficient on $Same\text{-}state\ lobby * Reelection$ to the coefficient on $Out\text{-}of\text{-}state\ lobby * Reelection$ for each specification separately

suggests that our findings documented in Table 6 are unlikely to be driven by systematic differences in subsidies given to politically connected firms (for example, politicians awarding subsidies that are more ambitious in terms of job promises, knowing that the success or failure of the subsidy will occur after the election). Moreover, the insignificant coefficient on $Same\text{-}state\ lobby_{i,s,t}$ in columns (1)–(3) of Table 6 is inconsistent with the alternative explanation that political contributions can facilitate weaker ex post monitoring, which in turn leads to lower target completion rates. Were this the case, the effect of campaign contributions should not be strongest in election years; in fact, we would expect the effect to strengthen for subsidies given *prior* to reelection years (as ex post monitoring would then occur during reelection years).

5.4 The role of media

A wide literature in accounting studies how media can influence public firms' actions, both in terms of the disciplinary role of independent media and the potential dissemination role of media coverage resulting from voluntary press releases. Building on this literature, we test whether subsidy-specific media coverage is associated with job target completion.

We hand-collect data on media coverage of each of the individual subsidies in our sample. We do so by manually searching Lexis-Nexis for articles about each subsidy, using the firm-state-year triple plus the words “subsidy,” “subsidies,” “tax break,” or “tax breaks” as keywords for our searches. We search for articles from either the subsidy year or the year after to allow for potential variation in the timing of subsidy awards within a year. Using this approach, we identify news coverage for 33.6% of subsidies. We then manually read each article and classify it into one of three types: (i) a government-issued press release, (ii) a company-issued press release, or (iii) independent journalism.

Government-issued press releases account for most of our sample, whereas company-initiated press releases and independent journalism are rarer: despite the fact that all subsidy recipients in our sample are relatively large publicly traded firms, 4.0% of subsidies in our sample are accompanied by a corporate press release while 9.7% are written about independently by local or national media. In contrast, 24.6% of subsidies are accompanied by a government press release. These figures are not mutually exclusive, as some subsidies are accompanied by multiple forms of media coverage.

Because independent journalism, government press releases, and corporate press releases serve very different purposes, in our empirical tests, we construct separate indicators for each of these types of coverage. We test the impact of the different types on job target completion in the following model:

$$\begin{aligned} \text{Job target met}_{i,s,t} = & \beta_0 + \beta_1 \text{Media}_{i,s,t} \\ & + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \epsilon_{i,s,t}. \end{aligned} \quad (4)$$

The independent variable of interest in Eq. (4), $\text{Media}_{i,s,t}$, reflects one of the three measures of media detailed above. We tabulate results from estimating Eq. (4) in Panel A of Table 7. We observe a positive but statistically insignificant coefficient on the independent journalism indicator in column (1). We also document, in column (2), that voluntary government disclosure—in the form of press releases—is associated with a higher likelihood of job target completion. This result is consistent with a selection effect: state governments should be more likely to voluntarily reveal information for subsidies with expected better outcomes. Thus, although voluntary disclosure may not be a primary mechanism for maximizing the success of a subsidy, it may signal the government's ex ante beliefs about specific subsidies. Conversely, in column (3), we find that job target completion is less likely for subsidies accompanied by corporate press releases. These results hold in column (4), where we include all three types of media in a single specification.

In column (5), we examine whether the results in columns (1)–(4) are likely to be a numerator or a denominator effect. We find that corporate press releases are associated with higher job targets. We do not find evidence of differences in job targets for subsidies covered by independent journalism or government press releases, relative to those without coverage. The latter result supports our interpretation that government press releases reflect signaling, while the former suggests that our results on firm-level press releases may reflect a selection effect. That is, firms are more likely to publicize subsidies that are expected to create more jobs, suggesting growth on the firm's part. However, the fact that firms are more likely to issue press releases to accompany larger job targets, despite larger targets being harder to meet, suggests that firms view the expected short-term benefits of announcing a subsidy as greater than any multiplier effect that putting out a press release may have on the expected longer-term costs of not meeting the job target associated with that subsidy.

Table 7 The role of media coverage

Column	(1)	(2)	(3)	(4)	(5)
Dependent variable	Job target met				Job target
Independent journalism	0.063 (1.304)			0.061 (1.251)	0.252 (1.649)
Government press release		0.072** (2.142)		0.078** (2.112)	0.135 (1.611)
Corporate press release			-0.149*** (-2.994)	-0.154*** (-3.084)	0.566*** (3.788)
Log # same-state estabs	0.022* (2.005)	0.022* (1.978)	0.021* (1.941)	0.022* (2.046)	0.004 (0.173)
Market to book	0.000 (1.191)	0.001 (1.191)	0.001 (1.305)	0.000 (0.873)	-0.005*** (-3.368)
ROA	0.273* (1.906)	0.243* (1.744)	0.255* (1.965)	0.239 (1.736)	-0.024 (-0.040)
Leverage	-0.158 (-1.580)	-0.155 (-1.490)	-0.157 (-1.496)	-0.146 (-1.395)	0.347 (1.223)
Sales growth	0.064 (1.282)	0.064 (1.277)	0.065 (1.241)	0.068 (1.299)	0.140 (1.108)
Log market value	-0.005 (-0.589)	-0.004 (-0.557)	-0.004 (-0.502)	-0.004 (-0.414)	0.125*** (4.878)
Labor intensity	-10.982** (-2.271)	-10.547** (-2.222)	-10.380** (-2.156)	-9.987** (-2.156)	17.969 (1.343)
Cash grant	0.050 (0.569)	0.046 (0.531)	0.046 (0.505)	0.038 (0.426)	1.054*** (6.295)
Tax break	0.014 (0.177)	0.009 (0.122)	0.008 (0.101)	0.001 (0.016)	0.361 (1.213)
Job target	-0.011* (-1.941)	-0.011* (-2.058)	-0.008* (-1.790)	-0.010* (-1.896)	
Constant	0.649*** (5.311)	0.638*** (5.561)	0.648*** (5.307)	0.628*** (5.340)	2.618*** (7.476)
Observations	1,345	1,345	1,345	1,345	1,345
R-squared	0.376	0.378	0.378	0.381	0.488
State-by-Year FE	YES	YES	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.196	0.198	0.198	0.201	0.340

This table examines the relation between media coverage, in the form of both independent journalism and government and firm press releases, and job target completion. The dependent variable in columns (1) to (4) is an indicator variable that equals one if firm i meets all job targets of the subsidies awarded in year t in state s and zero otherwise. The independent variables of interest are *Corporate press release* $_{i,s,t}$, *Government press release* $_{i,s,t}$, and *Independent journalism* $_{i,s,t}$. *Corporate press release* $_{i,s,t}$ is a dummy variable that equals one if firm i provided a press release, in year t or $t+1$, about a subsidy received from state s in year t . *Government press release* $_{i,s,t}$ and *Independent journalism* $_{i,s,t}$ are defined analogously for government press releases and independent journalism, respectively. In columns (1) to (3), we examine the correlation between the likelihood of meeting job targets and each media measure one at a time. In column (4), we include all three media variables concurrently. All variables are defined in the Appendix

Table 7 (continued)

Table 10. In column (5), we follow the specification in column (4) but replace the outcome variable with *Job target*, the logarithm of the number of jobs promised. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t-statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively

5.5 Robustness

In this section, we outline two robustness checks we undertake. First, we note that, in Tables 5, 6 and 7, we do not include as control variables prior tables' treatment variables, so that our results do not depend on the order of presentation. Nonetheless, the inclusion of some of these variables may affect the estimated effect of others. Hence, in an additional analysis (see in columns (1) and (2) of Online Appendix Table OA1), we include all treatment variables in a single specification. Our results continue to hold with similar coefficient magnitudes, with one exception: in this specification, ROA is now marginally statistically insignificant.

Second, we conduct our primary analyses at the firm-state-year level rather than the individual subsidy level because we view subsidies awarded to the same firm at the same time by the same state as non-independent; the subsidizing state likely cares about the firm's total job creation in the state. However, we acknowledge two potential drawbacks to this approach. First, if our non-independence assumption is incorrect (e.g., because some government officials may face differential consequences for the success or failure of specific subsidies), we may be incorrectly capturing how state governments and subsidy programs assess success. Second, because we do not observe the exact number of jobs created for nearly half our sample, for firms with multiple subsidies in a single state-year, we require *all* of those subsidies to succeed with respect to job targets to treat the firm-state-year as successful. This may introduce measurement error. To ensure that these issues do not drive our findings, in columns (3)–(4) of Online Appendix Table OA1, we replicate columns (1)–(2)—that is, including all determinant variables in a single specification—and re-run our analyses at the individual subsidy level. Our subsidy-level results are consistent with our firm-state-year level results.

6 Reputational consequences of meeting or missing targets

Firms may be incentivized to meet job targets to accrue reputational gains, an argument consistent with our observation in Fig. 4 that more firms just barely meet their promised targets, compared to those that just miss. We focus on two main issues in this section: (i) the direct benefits to meeting job targets,

realized in the form of future subsidies from the awarding state and others, and (ii) indirect benefits, realized through media coverage and ESG ratings.¹⁹

6.1 Direct benefits: future subsidies

Firms that meet job targets may earn reputations for keeping promises. The impact of this reputational capital matters most in the context of future subsidies. Put another way, a firm with a record of meeting its job target obligations may benefit by attracting future subsidies from economic development agencies tasked with job creation. Consistent with this argument, we hypothesize that firms that meet job targets are more likely to receive future subsidies. To that end, we test the effect of job target completion on the likelihood a firm obtains future subsidies. Specifically, we estimate:

$$\begin{aligned} \text{FutureSubsidies}_{i,s,t+1\text{to}t+4} = & \beta_0 + \beta_1 \text{Job target met}_{i,s,t} \\ & + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \end{aligned} \quad (5)$$

In Eq. (5), the dependent variable reflects two measures of future subsidy receipt, depending on whether these subsidies are awarded in state s . *Same-state future subsidies (t+1 to t+4)* _{i,s,t} is an indicator variable that equals one if firm i receives new subsidies in the award state s during the four years after receiving a state- s subsidy in year t ($t+1$ to $t+4$) and zero otherwise. *Out-of-state future subsidies (t+1 to t+4)* _{i,s,t} is an indicator variable that equals one if firm i receives new subsidies in any state other than s during the four years after receiving a state- s subsidy in year t ($t+1$ to $t+4$) and zero otherwise. We expect that the reputational gains to subsidy job target completion will be strongest in the same state, because a previously subsidizing state is most likely to be aware of a firm's record.

Table 8 provides results from estimating Eq. (5). The dependent variables in column (1) (column (2)) reflect same-state (out-of-same) future subsidies. We find in columns (1) and (2) that successful job target completion is associated with a higher likelihood of receiving future subsidies both in and out of state; a seemingly unrelated regressions approach confirms that the effect strengthens in the state that initially awarded the subsidy. In column (3), we examine whether the effect documented in column (1) varies as a function of the extent to which the firm beat (or missed) the job target, replacing the primary independent variable with the ratio of the number of actual jobs created to the target for the subset of observations where we observe the actual number of jobs created. We find that this ratio is positively associated with the receipt of future subsidies, suggesting that firms obtain greater reputational rewards when the extent of success is greater.

¹⁹ We do not focus on legal consequences of missing job targets, e.g., clawbacks. Enforcement actions are rare in practice (e.g., Brockmyer et al. 2012); moreover, when they do occur, they are typically not publicly disclosed. (e.g., Mattera et al. 2012).

Table 8 Are firms that meet job creation targets more likely to get future subsidies?

Column	(1)	(2)	(3)
	Same state	Out of state	Same state
	Year 1–4	Year 1–4	Year 1–4
Dependent variable	Future subsidies		
Job target met	0.103*** (3.922)	0.071** (2.831)	
Job target met (Ratio)			0.124** (2.936)
Same-state lobby	0.147*** (3.167)		0.155* (1.862)
Log # same-state estabs	0.015 (1.168)		-0.020 (-1.283)
Out-of-state lobby		0.083* (1.835)	
Log # out-of-state estabs		0.004 (0.488)	
Market to book	0.003*** (3.114)	0.002* (1.971)	0.003*** (4.819)
ROA	-0.263 (-1.311)	-0.393* (-1.985)	-0.218 (-1.053)
Leverage	0.067 (0.787)	0.165** (2.310)	-0.132 (-1.574)
Sales growth	-0.036 (-0.613)	-0.003 (-0.060)	-0.057 (-0.740)
Log market value	0.042*** (5.850)	0.069*** (6.850)	0.060*** (8.297)
Labor intensity	9.663** (2.224)	12.132** (2.621)	12.462* (2.065)
Cash grant	0.132 (1.112)	0.047 (0.394)	0.063 (0.651)
Tax break	-0.066 (-1.149)	-0.070 (-1.014)	-0.085 (-1.225)
Job target	0.025 (1.393)	0.013 (0.758)	0.062*** (3.204)
Constant	-0.203* (-1.782)	-0.428*** (-4.567)	-0.480*** (-3.031)
Observations	1,345	1,345	669
R-squared	0.404	0.414	0.481
State-by-Year FE	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.232	0.244	0.226

Table 8 (continued)

This table examines whether firms that meet job targets are more likely to receive subsidies in the future in the initially awarding state s and in other states. The dependent variables are measures of future subsidies: *Same-state future subsidies* $(t+1 \text{ to } t+4)_{i,s,t}$ and *Out-of-state future subsidies* $(t+1 \text{ to } t+4)_{i,s,t}$. For example, *Same-state future subsidies* $(t+1 \text{ to } t+4)_{i,s,t}$ is an indicator variable that equals one if firm i receives new subsidies in the awarding state s during the four years after receiving a subsidy in year t ($t+1$ to $t+4$) and zero otherwise. *Out-of-state future subsidies* $(t+1 \text{ to } t+4)_{i,s,t}$ is defined analogously for the future subsidies received by firm i in states other than s . The independent variables of interest in column (1) and (2) are *Job target met* $_{i,s,t}$. *Job target met* $_{i,s,t}$ is an indicator variable that equals one if firm i meets all job targets of the subsidies awarded in year t in state s and zero otherwise. The independent variables of interest in column (3) is the continuous version of *Job target met* $_{i,s,t}$. In the first and third (second) columns, the outcome variable is the future subsidies in the award state (non-award states). All variables are defined in the Appendix Table 10. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t-statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively

6.1.1 Timing

The reputational benefit from job target completion is unlikely to be felt immediately upon receiving a subsidy, because it is not yet known whether the firm will meet its job obligations to the state. In columns (2) and (3) of Online Appendix Table OA 2, we partition the post-subsidy period into one to two years and three to four years after subsidy receipt for same-state future subsidies. We find that the results in column (1) hold in the third and fourth years after subsidy initiation but not in the first two years post subsidy. A seemingly unrelated regressions approach confirms that these coefficients are statistically significantly different ($p < 0.01$). The stronger impact in later years is consistent with the idea that the relation between subsidies and future firm outcomes is likely to strengthen after the outcome of the initial subsidy is known. In columns (5) and (6) of Table OA2, we find analogous results for out-of-state subsidies. Finally, to rule out the possibility that our results simply reflect future subsidies being given to firms that are growing in the state, in untabulated analyses, we further control for the percentage change in the number of establishments firm i has in state s between years t and either $t+3$ or $t+4$. Our results remain unchanged.

6.2 Indirect benefits: media coverage and ESG ratings

6.2.1 Future media coverage

Meeting a job target reflects a beneficial action by the firm for taxpayers and society; we examine, in this section, whether such an action appears to result in reputational gains with stakeholders beyond the state itself via more positive coverage in the media. Because ex post media coverage of specific subsidies is

rare, except in the case of significant perceived failure (e.g., the Boeing example in Section 2.1), we consider the sentiment of all media coverage about the firm more broadly. We caveat that this approach prioritizes statistical power over precision, because the media sentiment data we use capture other firm-level actions. Nonetheless, our findings in this section may be informative for future work that may be able to refine this test.

We obtain media sentiment data from Ravenpack's Dow Jones edition. We then estimate the following regression:

$$\text{Sentiment}_{i,t+1tot+4} = \beta_0 + \beta_1 \text{Job target met}_{i,s,t} + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \quad (6)$$

In Eq. (6), the dependent variable is an indicator for whether firm i 's average media sentiment measured over a four-year period post subsidy ($t+1$ to $t+4$) is positive, based on Ravenpack's Combined Sentiment Score for all articles about the firm during that window. We present results from estimating Eq. (6) in column (1) of Table 9, where we find that meeting job targets is associated with an increase in positive post-subsidy media coverage of the firm. While we caveat that our conclusions from this specific analysis are limited by the data available to us, we view this as evidence consistent with the idea that meeting job targets can result in media-driven reputational benefits.

6.2.2 ESG ratings

Given recent interest by ESG investors and standard setters in understanding whether firms responsibly steward government subsidies (e.g., Global Reporting Initiative), we focus on a metric that has traditionally reflected reputational gains with ESG investors: commercial ESG scores. Recent research (e.g., Raghunandan and Rajgopal 2022; Thomas et al. 2022) argues that, while ESG scores do not always capture firms' underlying ESG performance, their importance with investors makes them a reasonable measure of firms' reputations for social responsibility. We thus examine whether meeting subsidy-specific job creation targets—demonstrating the type of responsibility sought by ESG investors—is rewarded by higher ESG scores. Our analyses in this section may be of use to practitioners because it is especially difficult to measure firms' impacts in their local communities (relative to other social and environmental constructs, such as labor practices or pollution).

Our analyses rely on ESG scores from two of the most common data vendors used by investors during our sample period: KLD and Asset4. We estimate the following regression:

$$\text{ESG}_{i,t+1tot+4} = \beta_0 + \beta_1 \text{Job target met}_{i,s,t} + \gamma X_{i,s,t} + \delta_{s,t} + \lambda_{j,t} + \varepsilon_{i,s,t}. \quad (7)$$

In Eq. (7), the dependent variable reflects firm i 's average ESG score from either KLD or Asset4 measured over four years post subsidy ($t+1$ to $t+4$). For KLD scores, we consider both overall scores as well as sub-scores related

Table 9 Job target completion and future reputation

Column	(1)	(2)	(3)
	Ravenpack Sentiment	ESG Score	
Dependent variable:		KLD	Asset4
Job target met	0.039* (1.964)	0.134 (0.559)	-0.202 (-0.174)
Same-state lobby	0.058 (1.433)	-1.182 (-1.323)	0.066 (0.027)
Same-state lobby*Reelection	-0.030 (-0.451)	0.971 (1.076)	-0.539 (-0.204)
Same-state labor penalties (t-2 to t)	0.045** (2.429)	0.446 (0.938)	-1.577 (-1.071)
Log # same-state estabs	-0.015 (-1.251)	-0.487 (-1.442)	-1.603* (-2.038)
Log # out-of-state estabs	0.006 (0.763)	0.005 (0.039)	1.820*** (3.366)
Market to book	-0.002*** (-4.463)	0.014** (2.629)	-0.010 (-0.218)
ROA	0.361*** (3.177)	-1.867 (-0.849)	-11.072** (-2.191)
Leverage	-0.123 (-0.956)	1.492 (0.861)	4.586 (0.761)
Sales growth	0.013 (0.397)	-0.606 (-0.577)	-6.971 (-1.598)
Log market value	-0.027** (-2.160)	0.987*** (6.697)	8.216*** (12.440)
Labor intensity	-9.845* (-2.087)	-30.655 (-0.455)	432.254** (2.357)
Cash grant	-0.022 (-0.499)	0.096 (0.224)	4.607*** (3.106)
Tax break	-0.040 (-1.467)	-0.205 (-0.502)	2.768 (1.387)
Job target	0.007 (0.878)	0.199 (1.572)	0.052 (0.152)
Constant	1.155*** (7.236)	-7.468*** (-5.941)	-35.014*** (-5.077)
Observations	1,345	735	946
R-squared	0.245	0.515	0.716
State-by-Year FE	YES	YES	YES
Industry-by-Year FE	NAICS-2	NAICS-2	NAICS-2
Cluster	State & Ind	State & Ind	State & Ind
Adjusted R-squared	0.0238	0.328	0.600

Table 9 (continued)

This table examines whether firms that meet job targets are more likely to subsequently enjoy reputational benefits. We measure firms' reputation based on media sentiment and ESG scores. In column (1), the dependent variable, Ravenpack Sentiment, is one if firm i 's average composite sentiment score is positive over the four years post subsidy. In columns (2) and (3), the dependent variables are ESG scores measured four years after the subsidy. Column (2) uses ESG scores from MSCI's KLD database, while column (3) uses scores from Asset4 (now Refinitiv). The independent variable of interest is Job target met $_{i,s,t}$, an indicator variable that equals one if firm i meets all job targets of the subsidies awarded in year t in state s and zero otherwise. All variables are defined in the Appendix Table 10. All specifications include state-year and industry-year fixed effects (two-digit NAICS industry classification). Robust t -statistics are shown in parentheses, and standard errors are double-clustered at the state and two-digit NAICS industry levels. ***, **, and * denote significance at 1%, 5%, and 10% levels, respectively

to firms' treatment of employees, the community, and an overall social score (i.e., specific to the S in ESG). We examine employee and community scores because subsidies' ultimate goal, as frequently stated by politicians, is to create high-quality jobs in specific communities; when successful, such job creation should create benefits felt most strongly by the firm's new employees within the subsidizing area. We follow prior literature (e.g., Kim et al. 2012) and define firms' KLD ESG scores as the number of strengths related to a topic minus the number of weaknesses, where strengths and weaknesses are binary indicators identified by KLD in relation to specific topics. For Asset4 scores, we use Asset4's overall ESG score as well as its social pillar score (which scores a firm's overall S). In both cases, we use a firm's average ESG score over the four-year post-subsidy period (i.e., $t + 1$ to $t + 4$ relative to a subsidy awarded in year t), although our results are insensitive to instead using only the score at $t + 4$.

We present results from estimating Eq. (7) in columns (2) and (3) of Table 9. For brevity, we tabulate results using only overall ESG scores from KLD and Asset4; results for topic-specific sub-scores yield the same qualitative conclusions. We consistently find no relation between firms meeting job targets and ESG scores. While we cannot assess whether meeting job targets yields direct reputational benefits with investors, our results suggest that firms do not realize indirect benefits via ESG ratings. This result raises questions about how data vendors assess firms' impacts on their communities, given that a fundamental measure of community responsibility—stewardship of taxpayer funds—appears to not be rewarded.

7 Conclusion

State governments frequently provide subsidies to firms, for which recipients promise to create or retain a certain number of jobs within the awarding state. Using a novel, hand-collected dataset, we provide initial evidence on three research questions: how frequently subsidy recipients meet job targets, what determines the likelihood of meeting job targets, and the benefits firms attain when they meet targets. We show that 63% of job targets in our sample are met and that job targets are less likely to be met when recipient firms have poor labor track records or when subsidies are given for political reasons. Firms that meet job targets are more likely to receive follow-up subsidies and appear to shift political activity out of nonsubsidizing states into subsidizing states. We acknowledge that data limitations prevent us from assessing how representative these states are in terms of the overall success rate of subsidies; that is, it is difficult to assess whether the 63% cited above reflects the reality in states with weaker subsidy disclosure practices. Nonetheless, because *states'* disclosure decisions are orthogonal to *specific firms'* characteristics, we believe our main findings on which firm-level determinants affect subsidy success should be generalizable.

Our study contributes to both academia and practice. First, we provide large-sample evidence on what makes targeted subsidies more or less likely to achieve their stated goals of job creation. Notably, our study does not focus exclusively on the large megadeals, as studied, for example, by Slattery (2021); we instead focus on smaller but still meaningful deals that are more likely to receive regional scrutiny. To that end, our study is also important to policymakers involved in the creation of subsidy programs, as these are the types of subsidies most commonly awarded in practice. We highlight the importance of due diligence on subsidy recipients and of ensuring that safeguards are in place to prevent quid pro quo between politicians and firms during electoral cycles. Our results also highlight the importance to economic development agencies of good disclosure practices and echoes calls by practitioners (e.g., Good Jobs First, Pew Charitable Trusts, Global Reporting Initiative) for greater subsidy-related transparency.

Appendix

Table 10 Variable definitions

Outcome variables	Source	Description
Job target met	See Table 1	Indicator variable that equals one if firm i meets all job targets of the subsidies awarded in the state s in year t and zero otherwise
Job target	See Table 1	The natural logarithm of the average job targets for all subsidies that firm i receive from state s in year t
Same-state future subsidies (t+1 to t+4)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the award state s during the four years after receiving the subsidy in year t (t to t+4) and zero otherwise
Same-state future subsidies (t+1 to t+2)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the award state s during the two years after receiving the subsidy in year t (t to t+2) and zero otherwise
Same-state future subsidies (t+3 to t+4)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the award state s in the third and fourth years after receiving the subsidy in year t (t+3 to t+4) and zero otherwise
Out-of-state future subsidies (t+1 to t+4)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the any non-award state during the four years after receiving the subsidy in year t (t to t+4) and zero otherwise
Out-of-state future subsidies (t+1 to t+4)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the any non-award state during the two years after receiving the subsidy in year t (t to t+2) and zero otherwise
Out-of-state future subsidies (t+1 to t+4)	Subsidy Tracker	Indicator variable that equals one if firm i receives new subsidies in the any non-award state in the third and fourth years after receiving the subsidy in year t (t+3 to t+4) and zero otherwise
KLD overall score	KLD	Number of overall ESG-related strengths minus number of overall ESG-related weaknesses, across all categories of firms' practices and policies covered by KLD. Averaged for firm i during the four years after receiving the subsidy in year t (t+1 to t+4)
Asset4 overall score	Asset4	Overall ESG score (1–100 scale) given by Asset4. Averaged for firm i during the four years after receiving the subsidy in year t (t+1 to t+4)
Sentiment	Ravenpack	A dummy variable that equals one if firm i 's average composite sentiment score in year t is positive and zero otherwise. We focus on news coverage when relevant score is greater than 0.75. Averaged for firm i during the four years after receiving the subsidy in year t (t+1 to t+4)

Table 10 (continued)

Outcome variables	Source	Description
Controls		
Same-state lobby	NIMSP	A dummy variable that equals one if firm i makes any political contributions to the award state s in year t and zero otherwise
Out-of-state lobby	NIMSP	A dummy variable that equals one if firm i makes any political contributions to any of the non-award states in year t and zero otherwise
Same-state labor penalties (t-2 to t)	Violation Tracker	A dummy variable that equals one if firm i incurs labor penalties in the award state s during the three years before receiving the subsidy in year t (t-2 to t) and zero otherwise
Same-state nonlabor penalties (t-2 to t)	Violation Tracker	A dummy variable that equals one if firm i incurs nonlabor penalties in the award state s during the three years before receiving the subsidy in year t (t-2 to t) and zero otherwise
Out-of-state labor penalties (t-2 to t)	Violation Tracker	A dummy variable that equals one if firm i incurs labor penalties in any non-award states during three years before receiving the subsidy in year t (t-2 to t) and zero otherwise
Out-of-state nonlabor penalties (t-2 to t)	Violation Tracker	A dummy variable that equals one if firm i incurs nonlabor penalties in any non-award states during three years before receiving the subsidy in year t (t-2 to t) and zero otherwise
Corporate press release	LexisNexis	A dummy variable that equals one if the source of the press release about the subsidy that firm i receives in state s during the awarding year or the year after that (t to t + 1) is the firm i and zero otherwise
Government press release	LexisNexis	A dummy variable that equals one if the source of the press release about the subsidy that firm i receives in state s during the awarding year or the year after that (t to t + 1) is the government in state s and zero otherwise
Independent journalism	LexisNexis	A dummy variable that equals one if the source of the press release about the subsidy that firm i receives in state s during the awarding year or the year after that (t to t + 1) is independent journalism and zero otherwise
Reelection	NIMSP	A dummy variable that equals one when incumbent governors in the award state s campaign for reelections and zero otherwise
Log # same-state estabs	ReferenceUSA	The natural logarithm of the number of firm i 's establishments in state s in year t
Log # out-of-state estabs	ReferenceUSA	The natural logarithm of the number of firm i 's establishments in all non-award states in year t
Market to book	Compustat	prcc_f/bkvlps
ROA	Compustat	ebitda/lag_at

Table 10 (continued)

Outcome variables	Source	Description
Leverage	Compustat	$(dltt + dlc) / at$
Sales growth	Compustat	$(sale_lag_sale) / lag_sale$
Log market value	Compustat	The natural logarithm of $prcc_f * esho$
Labor intensity	Compustat	emp / at
Cash grant	See Table 1	A dummy variable that equals one if any subsidies received in by firm i in state s and year t provide cash grants and zero otherwise
Tax break	See Table 1	A dummy variable that equals one if any subsidies received in by firm i in state s and year t provide tax benefits and zero otherwise

Supplementary Information The online version contains supplementary material available at <https://doi.org/10.1007/s11142-023-09804-6>.

Acknowledgements For helpful comments and suggestions, we thank the editor (Russell Lundholm) and an anonymous referee as well as Oliver Binz, Lisa De Simone, Brandon Gipper (discussant), Becky Lester, Maureen McNichols, Shiwon Song, and Colin Zeng. We also thank workshop participants at Columbia University, Fordham University, Hong Kong Polytechnic University, Tilburg University, University of Wyoming, the 2021 Yale Fall Accounting Conference, and the 2022 FARS conference. We thank Good Jobs First for sharing subsidy data and providing institutional context regarding subsidy-related disclosure as well as Joachim Rillo, Neel Shah, and Min Jun Song for excellent research assistance. Finally, we thank the Richman Center for Business, Law, and Public Policy for financial support. Any errors are ours.

Data Availability All data are obtained from publicly available sources.

Open Access This article is licensed under a Creative Commons Attribution 4.0 International License, which permits use, sharing, adaptation, distribution and reproduction in any medium or format, as long as you give appropriate credit to the original author(s) and the source, provide a link to the Creative Commons licence, and indicate if changes were made. The images or other third party material in this article are included in the article's Creative Commons licence, unless indicated otherwise in a credit line to the material. If material is not included in the article's Creative Commons licence and your intended use is not permitted by statutory regulation or exceeds the permitted use, you will need to obtain permission directly from the copyright holder. To view a copy of this licence, visit <http://creativecommons.org/licenses/by/4.0/>.

References

- Aobdia, D., A. Koester, and R. Petacchi, R. 2021. The politics of government resource allocation: Evidence from U.S. state government awarded economic incentives. Working paper, available at https://papers.ssrn.com/abstract_id=3127038.
- Blankespoor, E., E. deHaan, and C. Zhu. 2018. Capital market effects of media synthesis and dissemination: Evidence from robo-journalism. *Review of Accounting Studies* 23: 1–36.
- Brockmyer, B., J. Chapman, J. Goodman, D. Wilson, W. Wilson, and R. Zahradnik. 2012. *Evidence counts: Evaluating state tax incentives for jobs and growth*. Washington, DC: Pew Center on the States.
- Bushee, B., J. Core, W. Guay, and S. Hamm. 2010. The role of the business press as an information intermediary. *Journal of Accounting Research* 48 (1): 1–19.
- Chen, S., K. Schuchard, and B. Stomberg. 2019. Media coverage of corporate taxes. *The Accounting Review* 94 (5): 83–116.
- Cohen, L., J. Coval, and C. Malloy. 2011. Do powerful politicians cause corporate downsizing? *Journal of Political Economy* 119 (6): 1015–1060.
- Crisuolo, C., R. Martin, H. Overman, and J. Van Reenen. 2019. Some causal effects of an industrial policy. *American Economic Review* 109 (1): 48–85.
- De Simone, L., R. Lester, and A. Raghunandan. 2022. Tax subsidy information and local economic effects. Working paper, available at https://papers.ssrn.com/abstract_id=3482207.
- Drake, M., R. Hess, J. Wilde, and B. Williams. 2022. The relevance of non-income tax relief. *Contemporary Accounting Research* 39 (3): 1797–1833.
- Fonseca, J. 2020. The rise of ESG investing: How aggressive tax avoidance affects corporate governance & ESG analysis. *Illinois Business Law Journal* 25: 1–18.
- Garcia, J. 2016. The influence of corporate social responsibility on lobbying effectiveness: Evidence from effective tax rates. Working paper, available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=2745506.
- Hoi, C., Q. Wu, and H. Zhang. 2013. Is corporate social responsibility (CSR) associated with tax avoidance? Evidence from irresponsible CSR activities. *The Accounting Review* 88 (6): 2025–2059.

- Huang, Y. 2022. Government subsidies and corporate disclosure. *Journal of Accounting and Economics* 74 (1): 101480.
- Jensen, N., and E. Malesky. 2018. *Incentives to pander: How politicians use corporate welfare for political gain*. Cambridge: Cambridge University Press.
- Jensen, N., E. Malesky, and M. Walsh. 2015. Competing for global capital or local voters? The politics of business location incentives. *Public Choice* 164: 331–356.
- Kanagaretnam, K., J. Lee, C. Lim, and G. Lobo. 2018. Societal trust and corporate tax avoidance. *Review of Accounting Studies* 23: 1588–1628.
- Kim, Y., M. Park, and B. Wier. 2012. Is earnings quality associated with corporate social responsibility? *The Accounting Review* 87 (3): 761–796.
- Lester, R. 2019. Made in the U.S.A.? A study of firm responses to domestic production incentives. *Journal of Accounting Research* 57 (4): 1059–1114.
- Mast, E. 2020. Race to the bottom? Local tax break competition and business location. *American Economic Journal: Applied Economics* 12 (1): 288–317.
- Mattera, P., C. Cafcas, L. McIlvaine, A. Seifter, and K. Tarczynska. 2012. Money Back Guarantees for Taxpayers: Clawbacks and Other Enforcement Safeguards in State Economic Development Subsidy Programs. Available on Good Jobs First website at http://www.goodjobsfirst.org/sites/default/files/docs/pdf/moneyback_0.pdf.
- Pappas, K., M. Walker, L. Xu, and C. Zeng. 2022. Do government subsidies affect income smoothing? Working paper, available at <https://ssrn.com/abstract=3061933>.
- Raghunandan, A. 2022. Government subsidies and corporate misconduct. Working paper, available at https://papers.ssrn.com/sol3/papers.cfm?abstract_id=3035254.
- Raghunandan, A., and S. Rajgopal. 2022. Do ESG funds make stakeholder-friendly investments? *Review of Accounting Studies* 27 (3): 867–905.
- Raghunandan, A., and S. Rajgopal. 2023. Do socially responsible firms walk the talk? Working paper, available at <https://ssrn.com/abstract=3609056>.
- Slattery, C. 2021. The political economy of subsidy-giving. Working paper, available from author.
- Slattery, C., and O. Zidar. 2020. Evaluating state and local business incentives. *Journal of Economic Perspectives* 34 (2): 90–118.
- Thomas, J., W. Yao, F. Zhang, and W. Zhu. 2022. Meet, beat, and pollute. *Review of Accounting Studies* 27 (3): 1038–1078.

Publisher's Note Springer Nature remains neutral with regard to jurisdictional claims in published maps and institutional affiliations.