Business Incentives and Economic Activity: Evidence from Alabama

Rebecca Lester[†], Natalie Millar[‡], and Joshua Rauh[§]

The Hoover Institution and the Graduate School of Business

Stanford University

October 25, 2025

[Click here for latest version]

Abstract

Business incentives are a central policy tool of state and local officials to attract and retain firms. However, prior academic work suggests that these incentives come at a large fiscal cost, with approximately 40% of state corporate tax revenues and 70% of local economic development funds spent on these incentives. Earlier studies on the effectiveness and overall impact of incentives on job creation produce mixed results, potentially due to the reliance on incomplete data. In partnership with state and local authorities in Alabama, we assemble a dataset of state and local business incentives for the period 2012 through 2024. Of the 3,500 incentives granted during this period, only approximately 3% were previously observed in academic research. In total, these incentives equal $6.2 billion, with an average incentive value of $4.1 million. Using administrative employment data at the firm-county level, we find that receiving a business incentive increases employment at recipient firms by an average of 61 jobs as compared to non-recipient firms. This effect increases to 247 additional jobs for firms receiving the largest incentives, whereas we find no employment effect for the bottom 30th percentile of incentives. We estimate an average cost per job of $77,000, which is equal to 2.5 years of average per-capita income in Alabama and is lower than estimates from prior work. Our findings provide new insights into the prevalence, characteristics, and economic impacts of business incentives, contributing to the literature on the effectiveness of incentives as an economic development policy tool.

*We are grateful to our Alabama data-sharing partners, including numerous anonymous local economic developers. In particular, we extend our thanks to the Joint Legislative Advisory Committee on Economic Incentives, particularly Alabama Senate president pro tempore Garlan Gudger. We also express our appreciation to Alabama's Secretary of Commerce Ellen McNair, Secretary of Workforce Greg Reed, and Commissioner of Revenue Vernon Barnett.

We appreciate financial support and research assistance from Stanford University's Graduate School of Business and the Hoover Institution's State and Local Governance Initiative, along with assistance from Stanford's Office of Sponsored Research.

The authors of this paper are responsible for the contents of this paper, and the views expressed in this paper do not necessarily represent the views of the Alabama Department of Commerce, the Alabama Department of Workforce, the anonymous local economic developers, or the Joint Legislative Advisory Committee on Economic Incentives.

[†]Rebecca Lester: rlester@stanford.edu

[‡]Natalie Millar: nemillar@stanford.edu

[§]Joshua Rauh: rauh@stanford.edu

1. Introduction

Policymakers have long struggled with how best to address regional disparities in economic development, despite decades of federal programs and extensive academic research. Increasingly, policymakers use place-based policies to target economic development in a particular geographic area. One prominent group of place-based policies includes business incentives, which are tax breaks and government transfers to firms. These incentives are intended to create jobs, develop the workforce, and increase capital investment, with the ultimate goal of stimulating local economic activity. However, prior work produces mixed evidence of the effectiveness of these incentives and suggests a high fiscal cost, ranging from 40% of state corporate tax revenues to 70% of local economic development spending (Bartik, 2017; Slattery, 2025). In this paper, we use extensive, granular data on state and local business incentives to estimate their impact on employment. We then quantify the taxpayer cost-per-job to assess the overall cost-benefit of this development approach.

Place-based policies can potentially increase social welfare in a local area when they address market failures or reduce regional disparities. In this way, business incentives could serve as an effective place-based policy to combat low workforce participation rates and rising geographic inequality within the United States. Such effects are especially important in light of recent findings that highlight disparities in economic conditions and opportunities across regions (Chetty et al., 2014; Austin et al., 2018). Furthermore, place-based policies may be more effective at targeting recipients as compared to broad person-based approaches (Gaubert et al., 2021). However, the effectiveness of business incentives is a contentious topic, as some policymakers have proposed banning incentive competition on the basis that it reflects a "zero-sum" or "race-to-the-bottom" game between jurisdictions. Critics further assert that incentives only shift jobs and investments between jurisdictions without generating new economic activity in the aggregate. They argue that incentive competition merely transfers rents from governments to firms (Badger, 2014; Markell, 2017; Farmer, 2019; Burstein and Rolnick, 1995). Opponents also argue that incentives lead to wasteful spending characterized as a "candy store" and "corporate welfare" (LeRoy, 1997), with many place-based job creation policies viewed as ineffective (Neumark and Simpson, 2015).

Despite the widespread use of business incentives and the contentious debate about their effectiveness, prior work has reached no clear consensus about their impact. One possible reason for the mixed evidence is due to reliance on incomplete data, as many state and local governments are not required to publicly disclose incentives granted to businesses. This use of incomplete data understates total government transfers to businesses and provides only a partial picture. We address this key weakness of prior research by partnering with government officials in Alabama to compile a large, confidential dataset of state and local incentives

for the period 2012 through 2024. During this period, Alabama authorities granted over 3,500 individual incentives across more than 1,500 deals worth $6.2 billion in total. These incentives are granted to 1,008 unique companies. While the average incentive is $4.1 million, incentive amounts are very skewed, with five firms collectively receiving $1.5 billion (25% of the total value). For reference, recent work includes only 15 subsidies for the state of Alabama during a partially overlapping sample period (Slattery, 2025). When we further compare the Alabama data to that of "Subsidy Tracker," a commonly-used public dataset from Good Jobs First, we find that only 3% of incentives provided by state authorities are included, and *none* of the incentives provided by local authorities appear.[1] Thus, our data reveal a substantial increase in the amount and prevalence of incentives as compared to earlier work.

We then quantify the extent to which incentives drive subsequent employment at the recipient firm. We focus on studying employment because this is the most common outcome examined in the prior literature, and it is the key goal often cited by government officials when granting incentives. Examining this question in Alabama provides several empirical features that strengthen our ability to identify the causal effect of business incentives on job creation. First, Alabama has a rich history of offering incentives, including notable deals like those in the automotive and aerospace sectors (Brooks, 2002; Clark, 2015), providing a sufficiently long time-series to examine the effectiveness on employment. Second, Alabama's focus on the manufacturing sector is similar to many other states that similarly rely on business incentives to attract and retain firms, allowing our results to generalize to other jurisdictions. Third, the sample period captures a major legislative shift related to the types and amounts of business incentives. This change, known as the Alabama Jobs Act, is considered "the most significant economic development overhaul in Alabama" since the 1990s (Bradley, 2015), providing exogenous variation to assess optimal policy design. Finally, the state recently passed transparency legislation in 2023, enabling access to incentives previously unobserved.

We merge the state and local incentives data, provided by the Alabama Department of Commerce and numerous localities across the state, with administrative employment data provided by the Alabama Department of Workforce. These administrative data are derived from Unemployment Insurance records and provide total employment at the firm-establishment-quarter level for UI-covered employers. Although state authorities from the Department of Commerce provided us complete data across the state, we focus our analysis on the 15 counties in which data provided by the local authorities are also complete to ensure a complete picture of all incentives. We acknowledge that these 15 counties represent only 22% of all Alabama

[1] Good Jobs First is transparent about the known coverage issues, stating that they focus on the most substantial programs when collecting data across the U.S. and that they can only include in their dataset incentives that either state and local authorities provide to them or that are observed via news reports. While both Good Jobs First and prior research acknowledge the lack of complete coverage, our work is the first to quantify the extent of these coverage issues.

counties, but they include a disproportionately large amount of Alabama businesses and employment (above 60%).[2] We aggregate the employment data to the firm-county-year level and implement event study estimators and a stacked cohort difference-in-difference design that compares changes in employment for recipient firms to other local firms that are similar on observable characteristics.

A key challenge, both in this setting and in the broader literature on business incentives, is to construct a reliable counterfactual for outcomes that would have occurred without the incentive. We first address this issue by using two different control samples. The first control sample compares incentivized firms to non-incentivized firms within the same county. By comparing companies in one county to firms that similarly chose to operate in that county, we control for endogenous spatial sorting related to a firm's decision to locate in particular place. Second, we further tighten this control sample by comparing incentivized firms to non-incentivized firms within both the same industries and county. Within both groups, we also address concerns about differences by entropy balancing incentive recipients with control firms on observable characteristics. Specifically, we balance firms based on pre-incentive employment and the number of business establishments to ensure a similar distribution of firms based on size and scale. The use of multiple control groups and entropy balancing helps improve comparability between the groups along both observable and unobservable dimensions.

Our estimates from the stacked cohort difference-in-differences specification indicate that the first business incentive awarded to a firm increases average employment by 61 jobs in the six years after an incentive was granted. When further testing these effects using the within-industry group, we find a similar count of 59 jobs, confirming the magnitude of the effect. Heterogeneity tests reveal important variation in these effects. For example, effects are largest in new firms versus incumbent firms (103 vs. 46 jobs), and in large versus small firms (117 vs. 16 jobs). The industries with the largest effects include Warehousing (NAICS = 49), Information (51), and Finance and Insurance (52), with average increased employment of 519, 317, and 223 jobs, respectively.

We also observe different effects based on the amount of incentive granted to a company. Specifically, we sort firms into deciles based on the size of incentive provided and observe that the largest effects occur within the top twentieth percentile of incentives, where the average deal ranges from $7.3 million (9th decile) to $35.1 million (top decile), and estimated jobs increase from 151 to 246, respectively. Both the incentive amounts and corresponding employment effects decline substantially below this top 20th percentile: firms in the next five lower deciles (the 40th to 80th percentile of incentives) create 42 jobs in response to the average

[2] Our ongoing analysis at the time of writing includes carefully merging, reviewing, and incorporating incentives information from an additional 26 counties received over the preceding five months.

incentive amount of $1.5 million. We observe no statistically significant increase in jobs in the bottom three deciles. However, incentives in this portion of the distribution are predominantly property and sales/use tax abatements granted by local authorities, which are primarily intended to motivate investment and not necessarily new jobs. That said, over 92 percent of the abatement details include promised job commitments, suggesting that incentives were granted with some expectation of employment effects that do not occur.

We next study whether employment effects of incentives vary based on the policy design. As previously mentioned, the 2015 Alabama Jobs Act ("AJA") substantially changed the types of incentives granted to businesses. Prior to 2015, the state offered a mix of up-front cash incentives and tax abatements for which firms were not required to provide evidence that they met job and investment commitments *ex post*. The AJA formed two new types of incentives: the Jobs Credit and Investment Credit. The Jobs Credit typically provides a refundable tax credit up to 3% of the prior year's gross payroll. The Investment Credit is earned based on qualified capital investment, with the tax credit up to 1.5% of total capital investment. These investment credits may be used to offset state income tax, financial institution excise tax, insurance premium tax, utility tax, and/or utility license tax liability. To qualify for these incentives, companies must either operate within a qualifying industry, as defined by their NAICS code, or meet alternative eligibility criteria, including job and investment benchmarks. Notably, companies must provide evidence that they met their job and investment commitments to claim the tax credits on their Alabama state income tax return. Thus, not only did the AJA introduce new incentives, but it imposed guardrails to ensure taxpayer dollars were spent only if companies met their commitments.

Time series plots of the types of incentives provided from 2012 through 2024 validate a stark change in the amount and mix of state incentives after 2015, confirming that this legislation introduced important variation in incentives. We use this change to compare whether incentives had a differential impact before versus after the AJA. We first separately estimate employment effects for these two time periods. While the event study estimators suggest larger point estimates post-AJA, the estimates are not statistically different from the pre-AJA incentives. These results imply that the job effects did not vary based on whether firms were promised pre- or post-AJA incentives.

However, this analysis does not take into account whether firms were compliant and actually received the promised incentive based on ex-post confirmation of meeting job and investment targets. We obtain access to each recipient's compliance status, as tracked annually by the Department of Commerce. Of all firms receiving AJA incentives, approximately 40% exhibit some form of non-compliance: 14% were non-compliant in at least one year, 11% were never compliant, and Commerce terminated an additional 15% of deals.

Given this substantial proportion of non-compliant firms, we further refine our analyses to study whether employment varies based on the compliance status. We observe very different effects: incentives are associated with 332 jobs among compliant firms, as compared to 106 among non-compliant firms. While certainly we would expect firms with larger job growth to file the compliance reports and claim their promised tax credits, and thus we cannot claim that compliance reporting drove greater effects, the analysis shows the compliance process saved the state from paying out incentives to non-compliant businesses that would have otherwise received incentives. This is important because, while many advocate for increased oversight of incentives via compliance reporting or clawbacks, the literature contains extremely little evidence about these provisions. Our ability to observe variation based on compliance sheds new light on potential policy mechanisms that improve governance over business incentives.

Finally, we estimate the cost per job created, which is equal to the average incentive divided by the estimated employment effect obtained from our earlier analysis. Based on the average estimated effect of 61 jobs created by incentives, the cost per job is $77,000. To put this number into context, the expected cost per job at the time incentives were granted ranged from $46,000 - $61,000 based on promised jobs outlined in the project agreements. While the actual cost per job is above this range, it falls within estimates in the prior literature. These estimates include $40,829, $79,500, to $120,000 (Greenstone et al., 2010; Patrick, 2016; Slattery and Zidar, 2020). For further comparison, this amount is also below estimates for national stimulus spending, which range from $120,000 to $202,000 per job (Wilson, 2012; Conley and Dupor, 2013).

To contextualize, with an average per capita income of $29,588 in Alabama, the cost-per-job estimate is equivalent to paying an individual's income for 2.6 years.[3] That said, our $77,000 cost-per-job estimate is subject to two important caveats. First, the cost is *understated* to the extent that the state provides additional incentives that we do not observe, including support for workforce training or other statutory income tax credits via the Department of Revenue. Second, the cost is *overstated* to the extent that incentives have spillover employment effects in the local community.[4] Such spillovers may occur through industry agglomeration or economic growth outside of the recipient industry (more restaurants, retail, etc.). Ultimately, the overall impact of these two depends on which effect dominates.[5]

Given that incentives are highly skewed, we also calculate the cost-per-job based on the size of the incentive. This reveals very high costs among both the largest and smallest incentives. Specifically, while the top tenth

[3] Average per capita income is calculated using the 2019–2023 ACS 5-year estimates and adjusted to 2020 dollars for inflation.
[4] Spillover effects are also referred to as "multiplier effects"; we use these terms interchangeably throughout the paper.
[5] Future analysis will include estimates of spillover effects by estimating employment both within industry (at the three-digit NAICS level) and within a more refined geographic area (to capture employment effects of consumer-facing sectors). However, refinement for workforce incentives and other statutory incentives would require both additional administrative data and certain assumptions about the value of workforce training that accrues to the firm versus the economy more generally.

percentile of incentives generates the largest estimated employment effects, the cost per job of $142,474 is almost three times that of the next decile ($45,000). This estimate also exceeds the $120,000 benchmark from prior work. This means that, among the largest incentives, the positive job creation comes at a larger cost to the government, possibly reflecting rent extraction by firms. Among the bottom three deciles of incentive size, we also observe an outsized cost per job. While the average incentive amount in these deciles is relatively small in dollar value ($139,000), these incentives are frequently awarded, with an aggregate amount of $19.2 million. Given that we observe no statistically significant employment effects for this group (zero jobs), the cost per job is undefined (we cannot divide by zero). If even just one job had been created, the cost per job would be $139,000, which is almost as high as that in the top decile. Again, we note that these incentives are predominantly linked to capital investment, but the fact that job commitments often accompany these incentives raises questions about the lack of observed employment effects in the bottom three deciles.

Our paper contributes to the literature on the effectiveness of business incentives, which is highly debated. Some studies suggest that incentive deals can increase employment, investment, output, and total factor productivity (TFP) (Greenstone et al., 2010; Slattery and Zidar, 2020; Freedman et al., 2023; Bernini and Pellegrini, 2011; Patrick, 2016; Criscuolo et al., 2019). However, these benefits may be short-lived, as investment gains tend to dissipate within five years following the final incentive payment (Bronzini and De Blasio, 2006). Other studies find little evidence that incentives drive sustained economic growth at the state and local levels, with some showing no significant effect or even a negative impact on the number of establishments, earnings per worker, county-level revenue, labor productivity, and TFP (Slattery and Zidar, 2020; Patrick, 2016; Bernini and Pellegrini, 2011).

These mixed findings are likely due to several limitations in prior research. First, as described above, significant measurement error arises from incomplete data on incentive details, with much of the existing literature relying on known-incomplete sources. These studies typically capture only a small subset of incentive deals, often focusing on high-profile megadeals exceeding $5 million (Slattery, 2025). Observing a more complete distribution of incentives allows us to measure employment effects across incentive sizes and compare cost estimates. Second, prior findings may be confounded by other incentive deals occurring concurrently with large deals that cannot be separated at the recipient level. Third, U.S.-based research often lacks high-frequency firm-level employment data, hindering efforts to capture the direct impact of place-based policies on key outcomes such as employment. Finally, existing cost-efficiency calculations often rely on promised incentives, jobs, and investment rather than actual outcomes. This paper addresses these gaps by collecting data on all business incentives within a single jurisdiction, measuring their impact on granular

firm-level outcomes, comparing to two different control groups, and utilizing actual data on incentives and jobs. By documenting notable job growth and quantifying the fiscal cost per job, we confirm that incentives can increase employment at recipient firms, with on-average costs that fall below other recent estimates. However, we also document relatively high cost-per-job estimates among subsamples of the largest and smallest incentives, raising questions about the amount of awards provided to firms in these groups.

Further complicating the picture, the literature still lacks clarity on which types of incentives are most effective for different firms. For incentives to be cost-effective, firms must meet their commitments for capital investment, job creation, and wage requirements, and the benefits of these activities must outweigh the taxpayer costs. Amid growing evidence that place-based policies, such as Enterprise Zones (EZ), have not proven cost-effective, policymakers have begun rethinking and restructuring incentive programs (Neumark and Simpson, 2015). For instance, the Texas Enterprise Fund, established in 2003, targeted firms considering investment in other states, with performance-based contracts requiring firms to meet job creation targets or face clawbacks. Similarly, the California Competes Tax Credit was designed to ensure that firms met specific investment and hiring commitments prior to receiving credits. To our knowledge, our study is the first to measure the impact of the compliance report policy mechanism employed by Alabama, offering valuable insights for policymakers seeking to maximize the impact of these programs. By filling key gaps in the literature, our firm-level analysis significantly advances our understanding of how place-based policies affect regional disparities, providing policymakers with more nuanced guidance on the effective use of incentives.

2. Institutional Background

State and local governments across the United States use a variety of business incentives to attract and retain businesses and stimulate economic growth. Business incentives refer to a wide grouping of government transfers, including tax incentives, cash grants, in-kind transfers, infrastructure support, workforce development training, and low-interest loans. The extent to which a particular jurisdiction uses any of these incentives is a function of the jurisdiction's taxing authority, state and local laws, and preferences of government leaders. A brief description of these categories is as follows:

- Income tax incentives: reductions in income tax, either through tax deductions or credits for certain activity (capital investment, R%D investment, hiring), or reductions in tax rates, such as through a tax holiday for a specified period of time.
- Non-income tax incentives: reductions or abatements of property taxes, sales/use taxes, or other excise

taxes levied at the state or local level, often tied to a firm's investment in real or tangible property.

- Grants: direct financial assistance for specific projects or activities, either through up-front cash transfers or ex-post reimbursement.
- In-kind transfers: non-monetary government transfers, such as reduced purchase or rental rates on government-owned land and buildings. Other examples include waivers for fees and regulations, as well as accelerated approval through the local permitting process.
- Infrastructure support: investment by the government in roads, utilities, and facilities that directly benefit a particular business. Examples include installation of stop lights, turn lanes, exit ramps, or railroad extensions to a particular property.
- Workforce development support: reduced or waived tuition at local colleges or training centers for current and potential employees. Training could range from general education curriculum to company-specific skills-based courses.

Use of business incentives gained prominence in the mid-20th century as state and local governments began competing for industrial investment within their jurisdictions. This competition intensified over the decades, particularly as globalization and technological advancements reshaped the economic landscape. Consequently, firms increasingly sought incentives as part of their location choice, and governments responded by devising more targeted incentive packages.

In Alabama, the practice of local governments offering incentives to attract firms traces back at least to 1955 in Cullman, Alabama, when a group of fifty men known as the "Flying Fifty" successfully incentivized King Edward Cigar to build a factory in Cullman. The plant agreed to locate in the city if the community developed a suitable plot of land. The city, county, and community members provided the company with land and $87,000 in cash in exchange for a commitment to create 500 local jobs (The Cullman Tribune, 2024)[6].

Today, most states and many localities have a professional economic development organization to recruit and retain businesses, which includes negotiating business incentives. The process of granting incentives varies by jurisdiction and includes a number of actors, including state and local government officials, private firms, community stakeholders, and intermediaries. State governments often set the broad framework for incentives through legislation and policy, such as targeting specific sectors of industry and setting eligibility

[6]The $87,000 in cash in 1955 translates to $1,051,000 in 2025 dollars.

requirements. Local governments set their own framework based on priorities of the city council and other local leaders. Private firms negotiate and receive incentives, at times working with intermediaries such as consultants, site selectors, and legal advisors who assist in structuring and negotiating incentive agreements. Other parties include community stakeholders (residents, advocacy groups, and local organiations), as well as Industrial Development Boards (IDBs), who often own and lease out land or buildings to businesses. The interplay among all of these actors shapes how incentives are awarded and monitored.

The process of awarding incentives to new companies typically begins with an initial request for information (RFI) from a company representative or site selector. The RFI outlines the company's specific location needs and preferences, including real estate, infrastructure, workforce quality, corporate tax rates, and potential incentive packages. Submitting an RFI does not guarantee that incentives will be offered. At that point, the recipient government organization (whether the state or a locality) must assess whether the company's needs align with their resources and economic priorities. For larger incentives, this assessment often involves coordination and discussion across multiple levels of government and numerous agencies.

To the extent that the state or locality wants to proceed, the government and business will enter negotiations about the terms of the agreement. This process can be very lengthy, ranging from months to years, as businesses often simultaneously evaluate multiple locations, both within and outside the state. At the same time, government parties perform cost-benefit analyses and prepare incentive packages that weigh company commitments against the estimated fiscal cost. If the parties move forward, the incentive-granting jurisdictions and the company will execute a project agreement, which details the amount of incentive, the type of incentive, and the company's commitments, which often relate to job creation, wages, and investment. The project agreement also outlines any other performance conditions for receiving incentives.

In response to recent concerns over fiscal sustainability and competitiveness, many governments have restructured their incentive systems. One notable change is including guardrails or "clawback" provisions to limit transfers to firms that do not meet their commitments. A second example includes increased public disclosures about incentives. The demand for greater transparency reflects growing public and academic scrutiny about the amount of taxpayer dollars spent on these programs. For example, several states, including Alabama, North Carolina, Florida, Kentucky, Louisiana, Tennessee, and Virginia, have recently launched public disclosure portals to provide greater visibility into the distribution and impact of economic incentives. These efforts aim to balance the pursuit of economic competitiveness with responsible fiscal management and public accountability.

In Alabama, business incentives are a key component of the state's economic development strategy. The

state and local governments utilize a variety of tools similar to those employed nationally, including tax abatements, grants, in-kind incentives, and workforce training programs. The state's business incentive policies are administered and managed by the Alabama Department of Commerce and the Alabama Department of Revenue, often in collaboration with local governments. If a company seeks some of the state's incentives, such as the Jobs Credit and Investment Credit available under the AJA, a project manager at the Department of Commerce formally reviews each project. Projects are only authorized based on factors such as job creation, capital investment, and average hourly wage; competition with other states; local discretionary commitments; strategic value; and net revenue to the state. If deemed appropriate, the request is elevated to the Secretary of Commerce and the Governor for final approval. Once reviewed, the state issues an acceptance or rejection letter.[7] At the local level, incentives are administered and managed by local economic development authorities and county/municipal governments. These officials have some discretion and flexibility to tailor incentives to the needs of their jurisdiction, such as by offering tax abatetments or infrastructure-related incentives.[8] All large state incentives require local authorities to participate by offering incentives as well. Conversely, when local authorities provide abatements, there is typically an accompanying abatement of the state portion of the tax. Overall, the incentive process is a coordinated effort across multiple levels of government, with localities, states, and private entities working together to attract and retain businesses.

3. Data and Descriptive Statistics

3.1. Data Collection

State Incentives Data: This research project involved an extensive data collection effort across multiple state agencies and localities. The state partners include the Alabama Department of Commerce, the Alabama Department of Workforce, and the Alabama legislature. The Alabama legislature passed the 2023 Transparency in Incentives Act ("TIA"), which enabled access to the historical state incentives data. In the subsequent 18 months following the passage of the TIA, the Department of Commerce provided detailed data on business incentives.

[7] For existing industries, the application process is more streamlined, as these companies typically engage directly with local economic developers to negotiate incentives.

[8] Some of the most common types of incentives that the local authorities grant includes property, sales, and use tax abatements. Under Alabama law, only the non-educational portion of the property tax can be abated, which means only the portion not ear-marked for schools can be abated. Of the remaining tax, there is both a state and a local portion, meaning that if the locality abates their portion of the tax, the state portion can also be abated. In cases where a company is seeking abatements, such as property or sales tax abatements, the locality must notify the state's Department of Revenue through a formal letter. Further, the state requires companies seeking abatements for major additions to meet minimum investment thresholds, such as investing at least $2 million in capital or 30% of the book value of the company. There is no investment threshold for new projects.

The data included the names of recipients, the types of incentives, and the amount of awards for all recipients across the state from 2012 through 2024. Additionally, the data provide information about promised jobs, capital investment, average hourly wages for promised jobs, and the state's estimated 10- and 20-year returns on investment. Finally, the data included information about compliance status based on reports companies filed under the terms of the AJA.

Local Incentives Data: Starting in 2022, we visited with multiple local economic developers to ask for their partnership in sharing data. Where possible, we visited with the economic developers at least once in person and engaged in numerous follow-up discussions to better understand local operations and build trust with partners. The initial group of local partners included 15 counties. This represents only 22% of Alabama counties, these counties host a disproportionately large amount of private sector activities: they account for 63% of businesses, 59% of employees, and 65% of GDP in the state.[9]

After obtaining initial agreement from these 15 counties, representatives from both the Department of Commerce and the Department of Revenue sent emails to other counties to further encourage support of the project. As of writing, we currently have commitments from 26 additional counties, with much of the data received to date. At the time of writing, we are working to merge these data into our analysis so that we can include information on incentives across all 41 counties.[10]

The datasets provided by the participating localities include information on the types and amounts of incentives awarded to firms, such as property tax abatements, sales tax abatements, use tax abatements, cash incentives, in-kind incentives, and tax credits. The format of the data provided varied: some partners provided Excel spreadsheets, while others required digitization of meeting notes, confidential reports, and project agreements. To gather further context, we conducted site visits, met plant owners, and toured training facilities.

Employment Data: The Department of Workforce provided firm-level employment data. These data are derived from Unemployment Insurance (UI) records that firms are required to provide to federal and state authorities on a quarterly basis. The data contain quarterly individual-level employment and earnings data

[9]Sixty-three percent of businesses is calculated using the 2017 ACS. Fifty-nine percent of employees is calculated using June 2022 BLS. Sixty-five percent of GDP is calculated using 2023 real GDP data from BEA.

[10]In total, there are 67 counties in Alabama. Some localities are excluded from the analysis or chose not to participate due to three primary constraints: (1) the absence of local business incentive programs in some areas, (2) insufficient data availability to construct a longitudinal panel, or (3) the locality's decision to opt out of participation. Structural disparities, including limited infrastructure and workforce capacity, prevented some counties from attracting firms eligible for such incentives. Additionally, fiscal constraints hindered certain counties from implementing incentive programs altogether. The fragmented and often non-digitized nature of incentive data introduced further complexities, particularly for records spanning over a decade. Nonetheless, the 41 counties that have shared data cover 81% of Alabama's population, 84% of Alabama businesses, and 87% of Alabama employment, ensuring our inferences reflect most of the private sector activity in the state.

at each firm establishment, which we aggregate to the firm-county-year level. These data also include the six-digit NAICS code identifying the employer's industry. The administrative data are checked and cleaned internally by the Department of Workforce, thereby ensuring that the data are high quality.

Control Variables: We supplement the incentives and employment data with county-year-level measures of economic activity from the American Community Survey (ACS), including population, unemployment rates, poverty rates, education levels, median household income, and race.

3.2. Descriptive Statistics using All Incentives Data

Table 1, Panel A provides descriptive statistics about the total number and amount of state and local incentives granted from 2012 through 2024. In the data, we observe all state incentives granted during this period, as well as incentives granted by 41 localities. In total, there are 1,008 firms who receive 1,566 deals composed of 3,511 individual incentives during this time period. The aggregate amount of promised incentives is $6.2 billion, with an average incentive of $4.1 million. Untabulated analysis shows that approximately $3.7 billion of the aggregate amount was spent on state incentives. For context, total state corporate income tax receipts for the state from 2012 through 2024 were $12.3 billion, so approximately 30% of total corporate tax revenues for this period were spent on incentives.

The distribution of incentive amounts is highly skewed, with a few companies receiving very large incentives. Untabulated analysis to preserve firm confidentiality show that 5 firms receive $1.5 billion of incentives, or 25% of the total amount. The skewness of the distribution is also reflected in the much lower median value of incentives as compared to the mean ($544,000) in the last column of Panel A.

Figure 1 depicts the count of incentives across time in the blue bars (corresponding to the left axis) and the amount of incentives in the red line (corresponding to the right axis). There are around 300 incentives per year, with the number and amount of incentives the greatest in 2018. The relatively lower number in 2024 is due to the delay in obtaining updated incentives from some partners.

Table 1, Panel B presents the amount by type of incentive. Over 2,700 (77%) of incentives granted are property, sales, and use tax abatements. However, these incentives account for only 35% of the total promised incentive value. Aside from these deals, the largest incentives by deal value include the state's Investment Credit (43% of value) and the state's Jobs Credit (12% of value).

Figure 2, Panel A presents additional details about the dollar value of incentives based on the type of award. The distribution matches that of the red line in Figure 1, Panel A. The blue bars and dark orange bars

capture the amount of property, sales, and use tax abatements. While local authorities provided the data for most of these incentives, the value includes the amount promised to abate both the state and local tax. The appearance of the purple and light blue bars in 2015 correspond to the Jobs Credit and Investment Credit that were passed in the 2015 Alabama Jobs Act. Cash and, to a lesser extent, infrastructure and in-kind incentives are provided throughout the sample period.

4. Methodology

4.1. Estimation

To estimate the impact of receiving an incentive on employment, we employ a stacked cohort difference-in-differences design in which we compare incentivized firms to two control groups, described further below. We construct cohorts c that include firms i receiving their first incentive in year t. We include all observations in a seven-year window centered around each cohort year for both the incentivized firms and control firms. We then stack the cohorts in event time and estimate the following specification to measure the average effect of first incentive receipt on employment:

$$Y_{ict} = \beta Incentive_{ic} * Post_{ct} + \theta X_{ict} + \gamma_{ic} + \tau_{ct} + \epsilon_{ict}, \tag{1}$$

Y_{ict} represents employment for firm i, in cohort c, in year t and is equal to the number of jobs reported by a firm at the firm-county-year level in the UI data. $Incentive_{ic}$ is a binary indicator equal to one for firm i in cohort c that receives the first incentive in cohort year t, and zero for firms that do not receive incentives. For purposes of our analysis, we treat the first incentive we observe as the first incentive a firm receives.[11] $Post$ is an indicator variable equal to one beginning in year t of incentive receipt. In any across-county analysis, we include time-varying county control variables such as population, educational attainment above a high school diploma, median household income, and poverty rate, denoted as X_{ict}. We include firm-by-cohort fixed effects, denoted by γ_{ic}, to control for time-invariant firm characteristics, as well as year-by-cohort fixed effects, denoted by τ_{ct}, to control for time-specific shocks to employment. The firm-by-cohort and year-by-cohort fixed effects subsume the main effects for $Incentive_{ic}$ and $Post_{ct}$. ϵ_{ict} is an additive error term. Our coefficient of interest, β, measures the difference in employment between incentivized firms and control firms after the first incentive receipt relative to any difference in the pre-period. Standard errors are clustered at

[11] Since incentives have been awarded since the 1950s, it is likely that we do not observe the first incentive for firms that we observe in existence at the beginning of the panel. Unfortunately, state and local records limit our ability to observe incentives prior to 2012. Thus, for our analysis, we assume that the first year in our sample period is the first possible year a firm can receive an incentive.

the firm level.

To further study the year-specific effects of incentives and also evaluate whether the treatment and control firms exhibit parallel trends in outcomes prior to receipt of an incentive, we replace the static specification in Equation 1 with an event-study model:

$$Y_{ict} = \alpha + \sum_{t-3}^{t+5} \beta I * Incentive_{ic} + \theta' X_{ict} + \gamma'_{ic} + \tau'_{ct} + \epsilon'_{ict}, \tag{2}$$

where t is the year of incentive receipt for cohort c, I is an indicator equal to one for particular year t, γ'_{ic} are firm-by-cohort fixed effects, τ'_{jc} are year-by-cohort fixed effects, and ϵ'_{ijc} is an additive error term. β measures year-specific effects relative to $t-1$ (omitted baseline). The lack of significant effects before treatment would lend credibility to the assumption of parallel trends.

4.2. Identification

This paper seeks to estimate the causal effect of receiving a business incentive on employment outcomes. In an ideal scenario, such an effect would be identified through a natural experiment or randomized controlled trial, where a firm is randomly selected to receive an incentive. However, local governments do not utilize randomization or natural experiments in their incentive allocation processes. Instead of a structured or randomized assignment, these firms may differ from other firms (i.e., size, industry, political connections).

The absence of randomization introduces substantial challenges in accurately identifying the causal impact of incentives. Specifically, identifying an appropriate control group for firms receiving incentives is difficult, as firms receiving incentives are likely to differ systematically from those that do not.

We construct two distinct groups of control firms to address these challenges. The first control group consists of firms located within the same county as the treatment firms, but these firms do not receive incentives. By comparing firms located in one county to other firms located in the same county, we control for firms' endogenous preferences that affect spatial sorting. Furthermore, firms within the same region are likely to face similar unobserved shocks, making them more comparable to treatment firms than those located further away. This control group, along with the treatment firms, is called the "within-county" sample.

While the first sample compares firms to other locally-situated firms, one concern is that the potential control firms in an area differ substantially from those firms that receive incentives. For example, firms across different industries may face distinct market conditions, regulatory environments, and business cycles that could confound our results. Thus, the second control sample is comprised of non-incentivized firms

within the same county and within the same industry as our treatment firms. By using firms from the same industry and location as controls, we restrict the number of dimensions along which control and treatment firms differ and minimize the concern that control firms are impacted by differential industry-specific shocks or local economic conditions. This approach provides a cleaner comparison. While this strategy may reduce the pool of potential control firms, it creates the closest possible comparable firms—those operating in the same market environment but without receiving incentives, allowing for more precise identification of the causal effect.

We further address concerns about differences in the incentivized and non-incentivized firms by entropy balancing within each cohort. Entropy balancing reweights control observations to ensure a similar distribution of covariates between the treatment and control firms. We implement entropy balancing in two ways. We entropy balance within each cohort on employment and establishments in year *t-1* to ensure similarity based on a firm's pre-incentive size.

Finally, we leverage the AJA as an exogenous shock to Alabama's incentive landscape; see Section 5.3.

4.3. Sample Selection and Descriptive Statistics for Estimation

Table 2 presents the steps to merge the incentives data with the administrative employment data from the Department of Workforce. We start with over 2,319,672 observations for 282,349 private sector firms. We then drop out-of-state firms, firms that only ever report zero employment, and firms with missing county information before collapsing the observations to the firm-county-year level (n=1,386,647 observations). After further dropping firms with long gaps in employment reporting (suggesting errors in reporting) and firms outside of the 15 counties for which we conduct the employment analysis, we are left with 846,680 firm-county-year observations (107,819 firms), of which 7,010 are for incentive recipients (495 unique firms).

The remaining steps relate to creating the cohorts for estimation of Eq. 1. Incentivized firms are only permitted to be in one cohort, but control firms can enter multiple cohorts. Thus, when constructing and stacking the cohorts, the number of observations grows. We drop firms first incentivized after 2020 because we lack a sufficient post-period in which to create jobs, and we backfill observations for new firms entering the sample to ensure that we capture this business formation within the cohort window. Retaining these new firms is important given that over 27% of incentive recipients are new. We require firms to be present for at least four years in the cohort window, from t-1 through t+2, and we drop any observations for which entropy balancing lacks convergence. The final sample for estimation includes 4,767,025 firm-county-year observations, of which 4,134 relate to incentivized companies.

Table 3 presents descriptive statistics for the sample, both before and after cohort construction, in Panels A and B, respectively. The descriptive statistics reveal a highly skewed distribution across firm characteristics. Focusing on Panel A, the average (median) firm has 18 (4) employees, with the firms in the top 1 percent reporting between 217 and 6,311 employees. The average (median) annual payroll is approximately $900,000 ($148,000), with average earnings per worker of $43,000. Most companies have 1 establishment location, with 5 establishments at the 99th percentile.

Due to the statutory requirements of incentives, only a small fraction (0.8%) of companies receive these awards. The average amount of award in this sample is $4.6 million, which is only slightly larger than the average amount of $4.1 million reported in Table 1 for the broader sample of incentives across the state and the larger sample of 41 counties. Panel B of both Figure 1 and Figure 2 map the count, amount, and type of incentives within this 15-county sample, further confirming similar variation within this subset of incentives.

Figures 3 and 4 compare the number of employees and establishments for incentivized versus non-incentivized firms. As expected, these firms appear quite different, reflecting that larger firms – measured with either employment or locations – receive more incentives. Table 4 presents descriptive statistics comparing the incentivized firms to the non-incentivized firms, both before and after entropy balancing. Incentivized companies report 138.7 employees versus 11.3 at non-incentivized companies and have 1.27 versus 0.7 establishments. While differences in these characteristics and others are statistically significant prior to balancing, we observe no statistical differences on these two characteristics after balancing. While we note that the balancing reduces the differences in other characteristics, such as *Total Payroll ($)*, the differences are statistically significant.

Table 5 provides descriptive statistics about the incentives included within the estimation sample. In total, the 472 incentivized firms receive 1,038 incentives valued at $1.6 billion. This amount is smaller than that in Table 1 for several reasons, including the fact that i) the current sample includes only 15 counties, ii) we focus on incentives through 2020 (and not after) to allow a sufficient post-period to create jobs, and iii) some incentive recipients are not merged with the UI data due to name mis-matches, account number changes, and filing under a general payroll provider. While the amount and number of incentives is smaller in this sample, we note that the distribution across type remains quite similar to the full sample based on statistics presented in Panel B. Finally, untabulated analyses show that the firms in the sample actually receive a total of $2.3 billion of incentives over time, implying that they account for approximately one-third of the total incentives granted.

Table 6 presents the terms of incentive deals and compares them to those in Slattery (2025). In her

manuscript, she studied 387 total deals across the U.S., of which 15 were in Alabama during her sample period of 2002 through 2017, which is partially overlapping with our sample period. By comparison, the full sample in Table 1 includes over 3,511 incentives, and the sample used in estimation (as seen in the third and fourth columns of Table 6) includes 660 incentives, of which 472 are first-time awards. Thus, even after restricting our sample to the counties with most complete coverage, our sample includes substantially more incentives than in earlier work.

When comparing incentive amounts, we note that Slattery's 2024 manuscript focuses predominantly on larger incentives, with an average value ranging from $150 million (across the U.S.) to $184 million (in Alabama). By comparison, the average value within our estimation sample declines to approximately $3.6 million, implying many smaller incentive deals. These deals are also accompanied by much smaller job commitments, of approximately 100 jobs as compared to about 1,400. The *ex ante* cost per job based on the terms of the project agreement is much higher, at $356,000, whereas the expected cost per job in our sample ranges from $46,000 to $61,000. This suggests that our sample includes many more smaller incentives than that previously observed, allowing us to study employment effects across the award distribution.

5. Results

5.1. Average Effects

Table 7 presents results from estimating Eq. 1, which regresses firm-country-year employment on the interaction of $Incentive * Post$ to measure the relation between first-time incentive receipt and subsequent employment. Column 1 reports results for the within-county sample; Column 2 reports results for the within-county sample after limiting the possible control observations to those in the same industries as the incentivized firms. Both columns reflect entropy balancing within each cohort on establishments and employment in year $t-1$. The coefficient estimates for β range from 58.64 to 61.28, and both are statistically significant at the 1% level. This means that the first incentive leads to an increase of approximately 59 to 61 more jobs at incentivized firms as compared to non-incentivized firms located in the same county. Notably, the coefficients in columns 1 and 2 are not statistically different from one another, implying that employment gains at incentivized firms are consistent across both samples.

Figure 5 plots year-specific treatment effects obtained from estimating Equation (2). Panel A corresponds to the within-county sample using all control firms; Panel B corresponds to the within-industry sample. These graphs confirm that the two samples exhibit parallel trends in the years preceding incentive receipt. We also

observe statistically significant increases in employment after the incentive, consistent with the results in Table 7. The plots show that these effects occur relatively quickly, growing up to over 100 jobs by year $t+5$.

5.2. Heterogeneous Effects

5.2.1. New and Existing Firms

We analyze the heterogeneous impacts, first distinguishing between new entrants and incumbent firms. By definition, new firms should increase employment as they open a new facility in a jurisdiction, and so we should expect to observe an employment effect within those companies. However, the effect for incumbent firms is unclear. Incumbent firms could receive incentives for a number of reasons, including expansion, potential threats of relocation, or potential threats of closure. Proponents of providing incentives to incumbents argue that these awards are important for the retention of local firms and that they aid economic growth within the jurisdiction. However, this raises an important counterfactual: to what extent would these expansions or retentions have occurred in the absence of subsidies? Given the high costs associated with relocation and the inherent inertia of production processes, it is plausible that many expansions would have proceeded without public intervention, raising questions about the efficiency of such allocations.

To quantify these differential impacts, Table 8 presents regression estimates of employment changes attributable to business incentives separately for new and existing firms. We define existing firms as those first observed in the cohort at any point within the three years prior to incentive receipt ($t-3$ to $t-1$), whereas new firms are defined as those only first observed in year t.

The effect for existing firms is captured by the coefficient on $Incentive*Post$, whereas the incremental effect for new firms is the coefficient on the triple interaction for $Incentive*Post*New$. The coefficient in Column 1 implies that the first incentive increases employment by approximately 46 workers at existing firms that receive incentives, as compared to other existing firms within the same county. The coefficient of 103 on the triple interaction means that new firms create 103 jobs, as compared to other new firms in the county. The difference of 57 jobs for new firms is statistically significant at the 10% level, as tabulated in the middle of the table. The bottom of the table provides mean values for the dependent variables and the observation counts by group.

Column (2) extends the analysis to compare incentivized firms with other firms within the same industry and county. The estimated effect for existing firms is 45 jobs, which is statistically significant at the 1% level. For new firms the estimate is 107 jobs, which is also significant. Observing similar effects across the two

samples confirms the magnitude of impact and demonstrates robustness to controlling for industry effects via sample construction. Figure 6 plots the effects for the two samples, showing the large difference in effects between new versus existing firm.

One important caveat to the results for existing firms is that it is not possible to disentangle the effects within this group. Existing firms include a mix of expanding firms, firms that were retained in the jurisdiction, and firms that simply maintained their operations but did not grow. Unfortunately, it is not possible to separately identify and quantify the distinct impacts of these groups, but observing a positive effect overall implies that these incentives were not only granted to firms that simply maintained the size of their workforce.

Overall, the results underscore the stronger employment effects of incentives for new firms, which account for approximately 41% of firms in our sample based on Table 3. These findings suggest that targeting new entrants may yield greater employment gains relative to incumbent firms, though the latter group does demonstrate increased employment as well.

5.2.2. Firm Size

We next examine whether the effects vary across firm size categories. Some states prioritize incentives for large firms to maximize employment gains, yet it remains unclear whether subsidizing a single large firm generates greater job growth than supporting multiple smaller firms. While larger firms may be more stable recipients of incentives due to greater resources and potentially lower failure rates, the economic consequences of their underperformance could be more severe.

To test variation based on firm size, we split firms into groups using the numer of employees in year $t-1$ relative to the year of first incentive receipt. Requiring observation of a firm's employment count in this pre-period year means that this analysis is only conducted on incumbent firms. We separate firms into three groups: small (1–49 employees), medium (50–249 employees), and large (250+ employees).

Table 9 presents estimates for each of the three groups in Column (1). We observe increases of 16, 47, and 177 jobs across the small, medium, and large groups, respectively. Tests of differences in the coefficients tabulated in the middle of the page confirm that the effects are statistically different; for example, large firms create 101.5 more jobs than small companies. Inferences are unchanged when using the alternative control sample in Column 2. Figure 7 presents the effects graphically, confirming both pre-period parallel trends for each of the three groups, as well as a much greater average difference of approximately 70 jobs between large firms and medium firms, as compared to the average difference of 32 jobs between medium and small firms.

5.2.3. Incentive Size

We next study whether the effects vary based on the size of incentive granted to a business. We conduct this analysis in three steps.

First, we group deals based on whether they are a “megadeal," which is a deal exceeding $5 million of value as categorized by Good Jobs First. We use this split because these deals have been the focus of much prior work, and thus assessing variation based on this type of deal provides a comparison with earlier estimates. Table 10 presents the findings in Columns (1) and (4), with corresponding plots in Figure 8. Consistent with earlier work documenting increased job at megadeal recipients, which are often new firms moving into a jurisdiction, we observe a statistically significant increase of 276 jobs. Notably, we also find a statistically significant increase of 39 jobs among the non-megadeal recipients in the sample. While the difference of 237 jobs is statistically significant (as seen in the middle of the table), it is notable that jobs occur in both types of incentives. The effect in the second control sample is of similar size (255 more jobs).

Second, we further parse deals into quartiles to assess how the effects vary across the distribution. Column (2) and (5) of Table 10 present the coefficients for these four groups. We find that the effects monotonically increase, with no statistically significant jobs in the bottom quartile, increasing up to 177 jobs in the top quartile. Figure 9, Panel A plots the effects by quartiles. The plots confirm no employment in the bottom 25th percentile. The second and third quartiles exhibit similar effects, and we observe a notably higher effect in the top quartile.

A natural question is at what size of incentive do the large employment effects occur? To address this, we conduct a third analysis in which we separate firms into deciles based on the size of the first incentive received. Coefficients are reported in Columns (3) and (6). First, we observe no statistically significant job effects in the first two deciles. We observe relatively weak effects in the third decile, but only in Column (3) and only significant at the 10 percent threshold. These results suggest incentives in the bottom three deciles have limited impact on employment. Of incentives in this portion of the distribution, 95% are abatements for property, sales, and use taxes awarded by localities. It is unclear ex ante whether these types of abatements would indeed result in increased employment at recipient firms. These types of abatements typically only indirectly affect jobs through the complementarity of capital and labor. Furthermore, Alabama law does not specify any requirements beyond capital investment to qualify for abatements. However, we find that 92% of abatements in the bottom three deciles include some amount of jobs promised by the firm.[12] Thus,

[12]This statistic is conditional on the local authority reporting jobs and investment promised. We measure job commitments as (i) formal job commitments with clawback provisions, (ii) job promises, and (iii) good-faith job estimates as reported directly

observing no employment impact among this subsample of incentives suggests that these promised jobs do not manifest. In contrast, the fourth through eighth deciles report estimates ranging from 27 to 65 jobs, with mixed significance. The ninth and tenth deciles report increased employment of 161 and 247 jobs, respectively.

Panels C and D of Figure 9 plot the effects from the deciles. For ease of review, we group the first three deciles together, then the next five deciles (groups 4-8), and then separately plot the top two deciles in which the largest effects occur. In these groups, the average first incentive ranges from $2.95 million to $29.68 million, as reported in Table 10.

Using a dataset that includes a full distribution of incentives allows us to separately quantify the job effects and identify the potential inflection point for job creation. Our analysis points to substantially larger effects for incentives above $1 - $2 million, with almost double the employment effects in the top decile.

5.2.4. Industry

Table 11 examines how the results vary by industry. We include interaction terms for all of the two-digit NAICS industries with at least one incentivized firm. We identify firms' NAICS industry based on the industry reported in the Department of Workforce UI data.

We observe that the largest effects are in Transportation and Warehousing - Storage and Final Delivery (NAICS = 49), Information (NAICS = 51), Finance and Insurance (NAICS = 52), with estimated employment effects of of 529, 317, and 223, respectively. Manufacturing industries collectively report statistically significant effects of 104 jobs in Column 1 (for NAICS 32 and 33) and 129 jobs in Column 2 (for NAICS 31-33).

The effects by industry are plotted in Figure 10. Aside from the large positive effects, the plot also shows a number of industries with incentive recipients for which there is zero employment effect, including Other Services (81), Retail Trade (45), Professional Services (54), Construction (23), Real Estate (17), Management of Companies (55), and Health Care (62).

Overall, Table 11 highlights the stronger employment effects of incentives for warehousing, information, and finance firms, with slightly smaller effects for manufacturing. These findings suggest that targeting specific industries may generate greater employment gains than broad-based incentive programs.

by local officials, abatement applications, or abatement resolutions/agreements.

5.3. Alabama Jobs Act

We extend our analysis by studying an exogenous shock that reshaped Alabama's incentive landscape at the state level. As previously mentioned, the Alabama Jobs Act of 2015 shifted incentives from being largely up-front cash incentives to a reimbursement-based tax credit approach. The Jobs Credit provides annual cash rebates of up to 3% of the previous year's gross payroll.[13] The Investment Credit is up to 1.5% annually of the qualified capital investment, which can be applied against Alabama income tax, financial institution excise tax, insurance premium tax, utility tax, and/or utility license tax liability.[14] This policy change effectively alters the types of both firms and incentives granted. Figure 2 provides evidence consistent with this shift, based on the stark increase in Investment and Jobs Credits (as seen with the purple and blue bars).

Ideally, we would estimate the AJA's impact by comparing firms exposed to incentives both before and after the policy change. This approach would provide the strongest causal evidence by controlling for firm-specific factors and enabling a clear before-and-after comparison. However, this is infeasible because too few firms receive state incentives in both periods, making the sample too small to yield reliable estimates. Moreover, the subset of firms that do meet this criterion may not be representative of the broader population affected by the AJA.

Given these limitations, we re-estimate Eq. 1 to include a term that identifies whether an incentive was an AJA incentive (Investment or Jobs Credit). We retain only those firms that receive state incentives in the pre- and post-AJA period. This means that we drop firms we identified as incentive recipients based only on data provided by local authorities. Practically, this means that we exclude firms that only receive property, sales, and use tax abatements. While these abatements typically include some abatement of the state portion of these taxes as well, we exclude them because these are not the predominant type of incentives impacted by the AJA.

We present results in Table 12. In Column (1), the statistically significant coefficient of 217.8 means that state incentives provided prior to the AJA are associated with 218 more jobs at incentivized firms. The statistically significant coefficient of 187.17 on $Incentive * Post * AJA$ means that post-AJA incentive recipients created 187 jobs. The difference in jobs is not statistically significant. In Column (3) using the industry control

[13] Qualifying projects can receive a cash rebate of up to 4% if they are located in a particular targeted or "jumpstart" county, or if the employer is a technology company or a research and development-focused company. A county is targeted if it has a population of 60,000 or fewer in that year. A county is "jumpstart" if it is not a targeted county, has experienced negative population growth over the last five years, and contains no more than two Opportunity Zones. Additional rebates are available for the wages of veterans and former active duty military. See more details in Appendix A.

[14] For comprehensive details on these credits, refer to AL Code § 40-18-375 to -376 (2024).

sample, we observe similar effects as well.

Figure 11, Panel A plots year-specific treatment effects. In the initial years, it appears that the pre-AJA incentives had a slightly higher effect on employment, but the overall difference across these groups does not differ.

One key part of the AJA is that firms are required to prove their compliance with job and investment targets before claiming Jobs Credits and Investment Credits. To do so, firms must file annual compliance reports with the Department of Commerce. This change is a potentially important guardrail to ensure that companies meet their commitments before providing taxpayer funds in the form of the Jobs and Investment credit.

The Department of Commerce shared details as to each deal's compliance status on an annual basis. Firms begin filing compliance reports after the "build out" phase, which on average is approximately three years to build or open a facility and hire workers. In the full sample, across all counties, we observe 276 deals awarded under the AJA. Of this number, almost 40% exhibit some non-performance. Specifically, 14% are non-compliant in at least one year, 11% are never compliant, and 15% are terminated. Of the remaining 60% of AJA deals, half were always compliant (83), and the remaining (85) appear to be in the build out phase. This latter group mostly includes firms that more recently obtained AJA incentives, and we define this group as any deals that have not been terminated and have not filed a compliance report yet.

We then focus within our sample of 15 counties for which we have merged incentives data with employment data. That sample includes 63 AJA recipent firms. The distribution of non-compliance, compliance, and build-out is 44%, 40%, and 16%, reflecting that the sample ends with 2020 incentive recipients, of which most have completed the build-out phase.

We use this sample to further parse the post-AJA effects based on compliance status as seen in Columns (2) and (4) of Table 12. We find substantially different impacts among compliant vs. non-compliant firms. For example, the compliant firms created on average 332 to 345 jobs, as compared to non-compliant firms, who created 106 jobs. As expected, the build out firms have no statistically significant job creation effects, given that they remain in the early phases of operations.

Panels C and D of Figure 11 show the differential effects graphically. The blue line matches that of the before-AJA effects also shown in Panels A and B. The red line from Panels A and B is now split in Panels C and D into a red line (Fully Compliant), orange line (Non-compliant and/or Terminated), and yellow line (Build Out). Through $t + 2$, we observe very similar effects across these three groups, consistent with firms

undergoing the build-out phase. However, in years $t+3$ through $t+5$, we observe a divergence between the compliant and non-compliant firms. While the different effects are only statistically significant in year $t+5$, possibly due to the relatively small sample size, the patterns point to differing effects based on compliance status.

Filing a compliance report is an endogenous choice, in the sense that only firms meeting their targets are likely to do so to claim the promised Jobs Credits and Investment Credits. That said, this appears to be an important policy design that ensures that the state does not pay the tax incentives to the approximately 30-40% of firms that are non-compliant. Said differently, the state likely would have otherwise provided government transfers to these firms if not for observing the non-performance of these companies.

These results shed new light on possible policy mechanisms to ensure companies meet their commitments. The combination of AJA features that i) shift incentives from up-front to ex-post via the tax system and ii) require compliance reporting provide notable examples of how to ensure company performance on stated job and investment objectives.

6. Cost-effectiveness

Having quantified the estimated job effects and documented heterogeneity based on firm characteristics, incentive size, and AJA reporting, we next study the cost effectiveness of the jobs. Table 13 presents estimates of the cost for each job created from incentives. This cost-per-job amount is computed as the average incentive amount per firm, divided by the number of jobs created. The average incentive amount per firm is $4.7 million. Based on the average of 61.2 jobs created, this equates to a cost-per-job of $77,000.

To put this number into context, we compare this estimate to several others. First, recall that firms promise to create an average of 102-108 jobs as stated from the project agreements; see Table 6, Columns 3 and 4. Based on incentives provided to these firms, this equates to an estimated *expected* cost per job ranging from $46,114 to $61,494. The key reason for the divergence between the expected and actual amount relates to the fact that firms on average create fewer jobs than those promised up front.

While the actual cost-per-job is greater than the expected amount, it is notable that the cost is within the range of other estimates in recent literature. These figures include Greenstone et al. (2010) at $40,829, Patrick (2016) at $79,500, and, most recently, Slattery and Zidar (2020) at $120,000. The $77,000 cost-per-job estimate is also well below that of direct-transfer policies such as federal stimulus spending, which range from $125,000 to $202,000 per job (Wilson, 2012; Conley and Dupor, 2013). This suggests that the overall

cost is within range and lower than other relevant programs.

The calculations above reflect average incentive amounts and average job creation. However, our findings also highlight the highly skewed distribution of incentive awards and effects. Thus, Table 13 also estimates the cost-per-job across the four groups depicted in Figure 9: Deciles 1-3 (no jobs effect[15]), Deciles 4-8 (average 42 jobs), Decile 9 (161 jobs), and Decile 10 (246 jobs). We report the calculations of the cost-per-job in Panel A (for the four groups) and Panel B (the underlying decile detail).[16]

Figure 12 summarizes the key takeaways from this analysis. The figure first plots the average cost-per-job in the sample of $77,000. The next three groups, which contain up to the 90th percentile of incentive size, have even lower cost-per-job estimates of $62,000 (bottom 30th percentile), $34,000 (30th to 80th percentile), and $46,000 (90th percentile). However, the cost per job then almost quadruples to $142,000 in the top decile. Thus, while the largest incentives create the most jobs, the average cost is much higher because the overall incentive package is much larger.

In addition to concerns about the high cost-per-job among the largest incentives, Panel B of the table also reveals that the bottom three deciles of incentives produce no statistically significant jobs. It is not possible to calculate the cost per jobs in this case (as we cannot divide by zero), but if we were to assume even just one job was created, the cost would range from $7,064 to $84,877, which is the average incentive paid to these firms. While these incentives are relatively small, it is an open question as to what effects these incentives are intended to drive and whether granting these are effective.

There are two important caveats about these estimates. First, the cost per job estimates do not reflect any spillover effects that might occur, as we measure only the firm-level employment outcomes. Spillovers could occur both within industry in the form of agglomeration benefits, or within the local labor market in other businesses such as restaurants, retail, etc. To the extent that such spillovers occur, this would increase the jobs estimate and lower the cost-per-job.

Second, the cost per job estimates are based solely on the incentives data provided and do not include other transfers, whether monetary or in-kind, to firms. For example, to the extent that governments support firms by subsidizing workforce training or providing other statutory tax credits, our estimates will be understated. The extent of this bias is unknown and will ultimately be a function of which effect dominates.

[15] The table reports 2.25, but this amount is not statistically significant. Thus, we interpret the effect as no jobs.

[16] The mean incentive amount reported in the first row of Panel B represents the average of the total incentives received by a firm, which is slightly different than the mean values reported at the bottom of Table 10, which reflects the mean of only the first incentive received.

7. Conclusion

Our analysis of business incentives in Alabama from 2012 to 2024 provides new evidence on the labor market effects of state and local economic development policies. In partnership with state and local authorities, we assemble the most comprehensive dataset to date on business incentives in a single state, documenting 3,500 incentive awards totaling over $6 billion to 1,008 firms. We then focus on 15 counties with the most complete data coverage and use administrative employment data to study the job creation impacts of these incentives.

We document that receipt of the first incentive increases firm-level employment by 58 to 61 jobs. Effects are the greatest in new firms, the largest firms, and firms receiving the top 20th percentile of incentive value. In terms of industry, effects are concentrated in Transportation and Warehousing, Information, Finance and Insurance, and Manufacturing. We find that the design and compliance features of the Alabama Jobs Act are also important for ensuring state incentives are only provided to compliant firms that meet their jobs commitments.

Our cost-effectiveness estimates suggest that the mean incentive generates employment at a cost of approximately $77,000 per job, a figure in range of prior estimates and below the most recent estimate of $120,000. However, the estimates for the largest incentives exceed $142,000, and there are also no observed jobs for the smallest deals. These raise questions about the size of incentive packages granted to large firms and the efficacy of the small incentives granted.

These findings contribute to the broader debate on the efficacy of place-based policies and firm-targeted subsidies. We document positive and notable employment effects, demonstrating employment growth for most incentives. Besides documenting these employment effects and quantifying in which groups the effects are the largest, we also evaluate whether policy design such as compliance reports provides necessary guardrails on government transfers to firms. The evidence points to compliance reporting as an important mechanism that aids the state in preserving incentives otherwise paid to 30%-40% of state incentive recipients.

Our ongoing and short-run future analysis will build on these findings in several ways. First, we will use two additional control samples of firms, including firms in other counties and later-treated firms, to further assess the magnitude of the employment effects. Second, we will merge in data obtained from 26 additional counties and expand our analysis to include all 41 counties. Third, we will calculate spillover effects at both the industry and local area level to refine cost-per-job estimates. Fourth, we will extend our analysis to examine the effects of subsequent incentive awards, which are often tied to firm expansions. Given that many

firms receive multiple awards over time, distinguishing the effects of initial versus follow-up incentives will yield a more comprehensive assessment of policy efficacy. Finally, we will examine worker effects. Specifically, using the administrative employment records, we will measure whether workers make more when they move to incentivized firms. Furthermore, we will study from which companies the new workers came or whether the workers were previously unemployed, thereby providing insight into workforce issues connected with business incentives. Broadly, this work will provide further insight into the long-run effects of business incentives on firm growth and local labor markets.

References

Daniel Aobdia, Allison Koester, and Reining Petacchi. The politics of government resource allocation: Evidence from us state government awarded economic incentives. *Available at SSRN 3127038*, 2021.

Benjamin A Austin, Edward L Glaeser, and Lawrence H Summers. Jobs for the heartland: Place-based policies in 21st century america. Technical report, National Bureau of Economic Research, 2018.

Emily Badger. Should we ban states and cities from offering big tax breaks for jobs? *The Washington Post Accessed at: https://www. washingtonpost. com/news/wonk/wp/2014/09/15/should-we-ban-states-and-cities-from-offering-big-tax-breaks-for-jobs*, 2014.

Keith Barnatchez, Leland Dod Crane, and Ryan Decker. An assessment of the national establishment time series (nets) database. 2017.

Timothy J Bartik. A new panel database on business incentives for economic development offered by state and local governments in the united states. 2017.

Timothy J Bartik and Nathan Sotherland. Local job multipliers in the united states: Variation with local characteristics and with high-tech shocks. Technical report, Upjohn Institute working paper, 2019.

Cristina Bernini and Guido Pellegrini. How are growth and productivity in private firms affected by public subsidy? evidence from a regional policy. *Regional Science and Urban Economics*, 41(3):253–265, 2011.

Nicholas Bloom, Erik Brynjolfsson, Lucia Foster, Ron Jarmin, Megha Patnaik, Itay Saporta-Eksten, and John Van Reenen. What drives differences in management practices? *American Economic Review*, 109 (5):1648–1683, 2019.

Bradley. House Bill 58, Alabama Jobs Act, Gives State Jobs Tax Credit and Capital Investment Credit. https://www.jdsupra.com/legalnews/house-bill-58-alabama-jobs-act-gives-s-69673/, 2015. Accessed 1-15-2025.

Raffaello Bronzini and Guido De Blasio. Evaluating the impact of investment incentives: The case of italy's law 488/1992. *Journal of urban Economics*, 60(2):327–349, 2006.

Rick Brooks. Big incentives won alabama a piece of the auto industry. *The Wall Street Journal*, 2002.

Melvin Burstein and Arthur J Rolnick. Congress should end the economic war between the states. *Annual Report*, 9(Mar):2–20, 1995.

Business Facilities. Business Facilities' 20th Annual Rankings Report. https://businessfacilities.com/20th-annual-rankings-report-2024-state-rankings/, 2024. Accessed 1-18-2025.

Doruk Cengiz, Arindrajit Dube, Attila Lindner, and Ben Zipperer. The effect of minimum wages on low-wage jobs. *The Quarterly Journal of Economics*, 134(3):1405–1454, 2019.

Raj Chetty, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. Where is the land of opportunity? the geography of intergenerational mobility in the united states. *The Quarterly Journal of Economics*, 129 (4):1553–1623, 2014.

Nicola Clark. First u.s. airbus factory gives wings to revival in mobile, ala. *The New York Times*, 2015.

Timothy G Conley and Bill Dupor. The american recovery and reinvestment act: Solely a government jobs program? *Journal of Monetary Economics*, 60(5):535–549, 2013.

Leland D. Crane and Ryan A. Decker. Business dynamics in the national establishment time series (nets). 2019.

Chiara Criscuolo, Ralf Martin, Henry G Overman, and John Van Reenen. Some causal effects of an industrial policy. *American Economic Review*, 109(1):48–85, 2019.

Matthieu Crozet, Thierry Mayer, and Jean-Louis Mucchielli. How do firms agglomerate? a study of fdi in france. *Regional science and urban economics*, 34(1):27–54, 2004.

E Mark Curtis and Ryan Decker. Entrepreneurship and state taxation. 2018.

Lisa De Simone, Charles McClure, and Bridget Stomberg. Examining the effects of the tax cuts and jobs act on executive compensation. *Contemporary Accounting Research*, 39(4):2376–2408, 2022.

Michael P Devereux, Rachel Griffith, and Helen Simpson. Firm location decisions, regional grants and agglomeration externalities. *Journal of public economics*, 91(3-4):413–435, 2007.

Erin Edgemon. 'The deal is done:' Amazon to bring 1,500 jobs to Bessemer. https://www.al.com/news/birmingham/2018/06/the_deal_is_done_amazon_to_bri.html, 2018. Accessed 12-17-2024.

Pablo D Fajgelbaum, Eduardo Morales, Juan Carlos Suárez Serrato, and Owen Zidar. State taxes and spatial misallocation. *The Review of Economic Studies*, 86(1):333–376, 2019.

Liz Farmer. With amazon out of new york, some lawmakers seek multistate ban on corporate tax breaks. *Governing*, 14, 2019.

Matthew Freedman, Shantanu Khanna, and David Neumark. Combining rules and discretion in economic development policy: Evidence on the impacts of the california competes tax credit. *Journal of Public Economics*, 217:104777, 2023.

Clemens Fuest, Andreas Peichl, and Sebastian Siegloch. Do higher corporate taxes reduce wages? micro evidence from germany. *American Economic Review*, 108(2):393–418, 2018.

Cecile Gaubert, Patrick M Kline, and Danny Yagan. Place-based redistribution. Technical report, National Bureau of Economic Research, 2021.

Xavier Giroud and Joshua Rauh. State taxation and the reallocation of business activity: Evidence from establishment-level data. *Journal of Political Economy*, 127(3):1262–1316, 2019.

Michael Greenstone and Enrico Moretti. Bidding for industrial plants: Does winning a million dollar plant increase welfare? 2003.

Michael Greenstone, Richard Hornbeck, and Enrico Moretti. Identifying agglomeration spillovers: Evidence from winners and losers of large plant openings. *Journal of Political Economy*, 118(3):536–598, 2010.

Howard Koplowitz. Bessemer City Council gives mayor go-ahead to make deal with Amazon on facility. https://www.al.com/news/2018/06/bessemer_city_council_gives_ma.html, 2018. Accessed 12-17-2024.

R Glenn Hubbard and Michael R Strain. Has the paycheck protection program succeeded? *Brookings Papers on Economic Activity*, 2020.

Greg LeRoy. No more candy store, 1997.

Alexander Ljungqvist and Michael Smolyansky. To cut or not to cut? on the impact of corporate taxes on employment and income. Technical report, National Bureau of Economic Research, 2014.

Jack Markell. Let's stop government giveaways to corporations. *The New York Times*, 2017.

Evan Mast. Race to the bottom? local tax break competition and business location. *American Economic Journal: Applied Economics*, 12(1):288–317, 2020.

Enrico Moretti. Local multipliers. *American Economic Review*, 100(2):373–377, 2010.

Enrico Moretti and Per Thulin. Local multipliers and human capital in the united states and sweden. *Industrial and Corporate Change*, 22(1):339–362, 2013.

David Neumark and Helen Simpson. Place-based policies. In *Handbook of Regional and Urban Economics*, volume 5, pages 1197–1287. Elsevier, 2015.

Carlianne Patrick. Identifying the local economic development effects of million dollar facilities. *Economic Inquiry*, 54(4):1737–1762, 2016.

Cailin Slattery. Bidding for firms: Subsidy competition in the united states. *Journal of Political Economy*, 133(8):000–000, 2025.

Cailin Slattery and Owen Zidar. Evaluating state and local business incentives. *Journal of Economic Perspectives*, 34(2):90–118, 2020.

Stephanie Hoops. Incentives package caused early controversy. https://www.tuscaloosanews.com/story/news/2003/09/21/incentives-package-caused-early-controversy/27848187007/, 2003. Accessed 12-17-2024.

Juan Carlos Suárez Serrato and Owen Zidar. Who benefits from state corporate tax cuts? a local labor markets approach with heterogeneous firms. *American Economic Review*, 106(9):2582–2624, 2016.

Liyang Sun and Sarah Abraham. Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics*, 225(2):175–199, 2021.

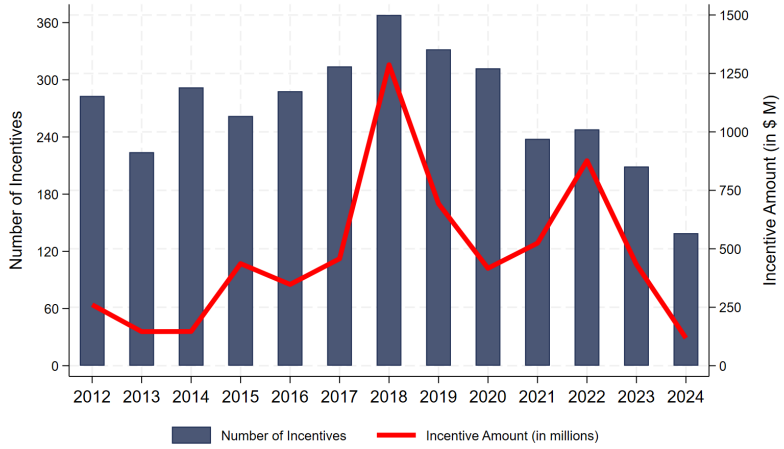
The Cullman Tribune. Flying Fifty Hotel coming to Cullman in 2024. https://www.cullmantribune.com/2023/12/25/flying-fifty-hotel-coming-to-cullman-in-2024/#:~:text=The%20following%20morning%2C%20a%20group,All%20Rights%20Reserved., 2024. Accessed 12-16-2024.

Jasper Jacob Van Dijk. Local employment multipliers in us cities. *Journal of Economic Geography*, 17(2): 465–487, 2017.

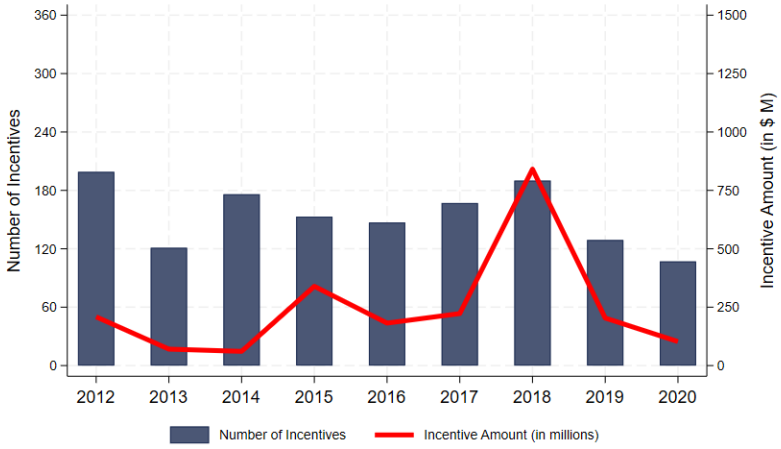
Damian Vergara. *Essays on Public and Labor Economics*. University of California, Berkeley, 2023.

Daniel J Wilson. Fiscal spending jobs multipliers: Evidence from the 2009 american recovery and reinvestment act. *American Economic Journal: Economic Policy*, 4(3):251–282, 2012.

Figure 1: Count and Amount of Incentives by Year



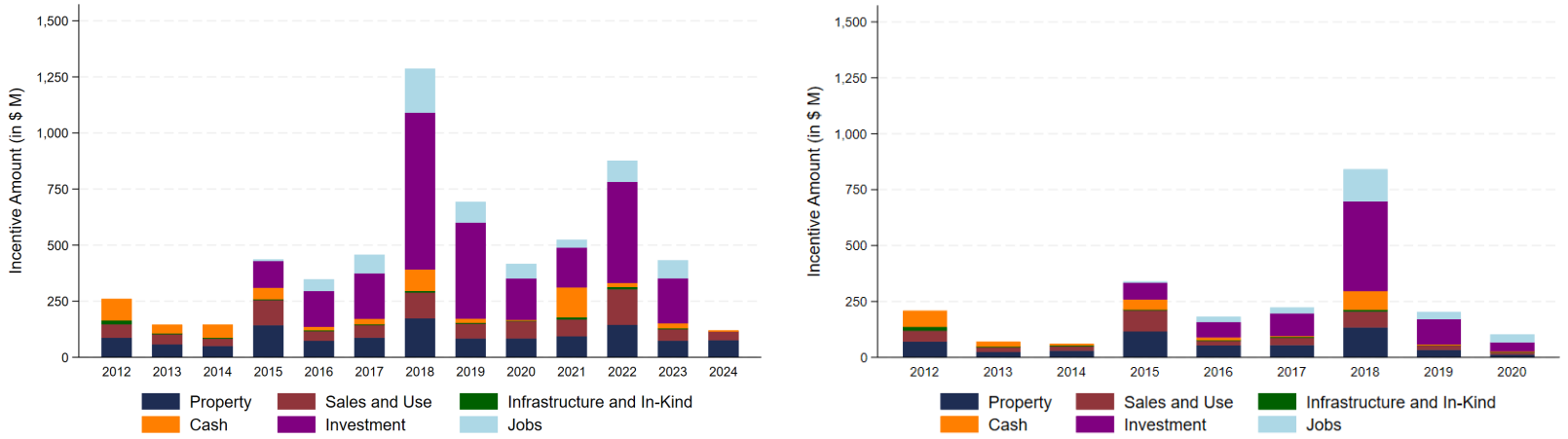
(a) All Data Collected



(b) 15 County Sample

Panel (a) presents the number of incentives awarded each year (blue bars) and the total amount (in nominal $) of incentives awarded (red line) for the full dataset. Panel (b) displays the same information for our 15-county sample. All figures were constructed by the authors using incentive data from the Alabama Department of Commerce and local economic developers. Section 2 defines incentives, and Section 4.3 describes the sample selection procedure.

Figure 2: Amount of Incentives by Year and Type

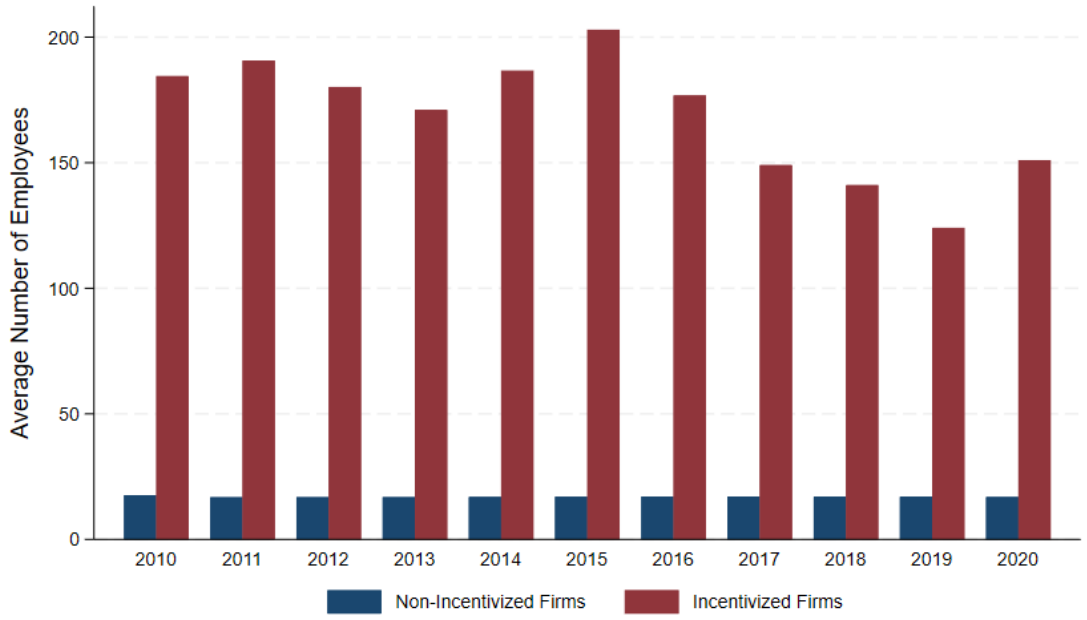


(a) All Data Collected

(b) 15 County Sample

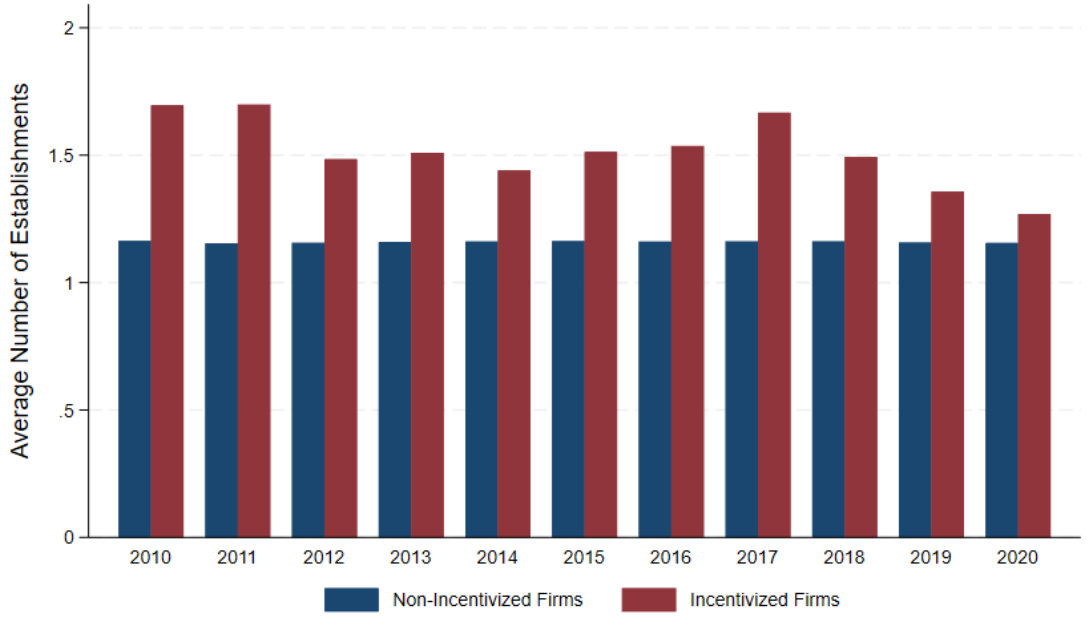
Panel (a) presents the total value (in nominal $) of incentives by year and incentive type for the full dataset. Panel (b) presents the same breakdown for our 15-county sample. Incentive types include property tax abatements, sales and use tax abatements, jobs tax credits, investment tax credits, cash grants, infrastructure support, and in-kind contributions. All panels were created by the authors using incentive data from the Alabama Department of Commerce and local economic developers. Section 2 defines incentive types, and Section 4.3 describes the sample selection procedure.

Figure 3: Average Number of Employees per Firm



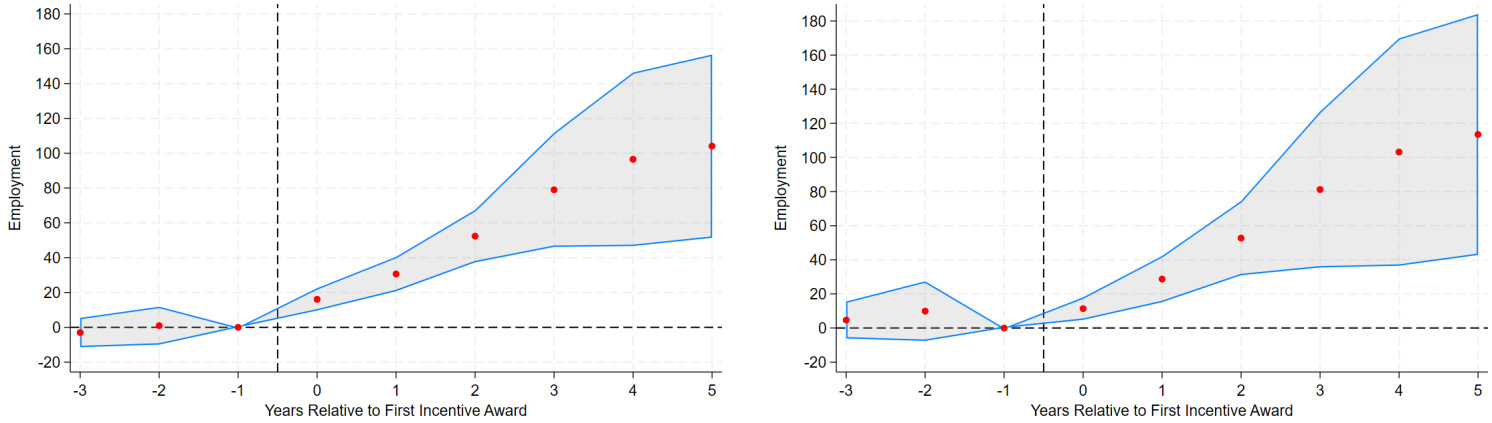
This figure presents the average number of employees from 2010 to 2020 at incentivized and non-incentivized firms. For incentivized firms, employment is measured prior to the receipt of the incentive. The figure was created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 4.3 describes the data and sample selection procedure.

Figure 4: Average Number of Establishments per Firm



This figure plots the average number of establishments from 2010 to 2020 at incentivized and non-incentivized firms. For incentivized firms, the number of establishments is measured prior to the receipt of the incentive. The figure was created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 4.3 describes the data and sample selection procedure.

Figure 5: Event Studies

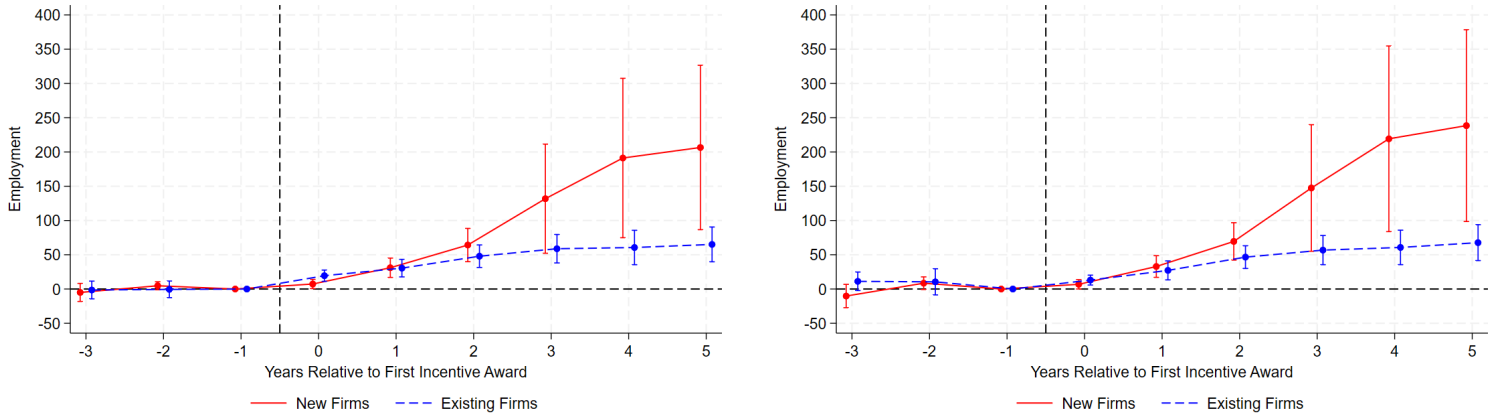


(a) All Control Firms

(b) Within Industry Control Firms

These figures present coefficients from event study regressions estimating Equation 2, comparing incentivized firms to non-incentivized firms. The dependent variable is an indicator for incentive receipt interacted with relative time indicators, where year t denotes the year of incentive receipt. Panel (a) compares incentivized firms to non-incentivized firms within the same county, while Panel (b) compares incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

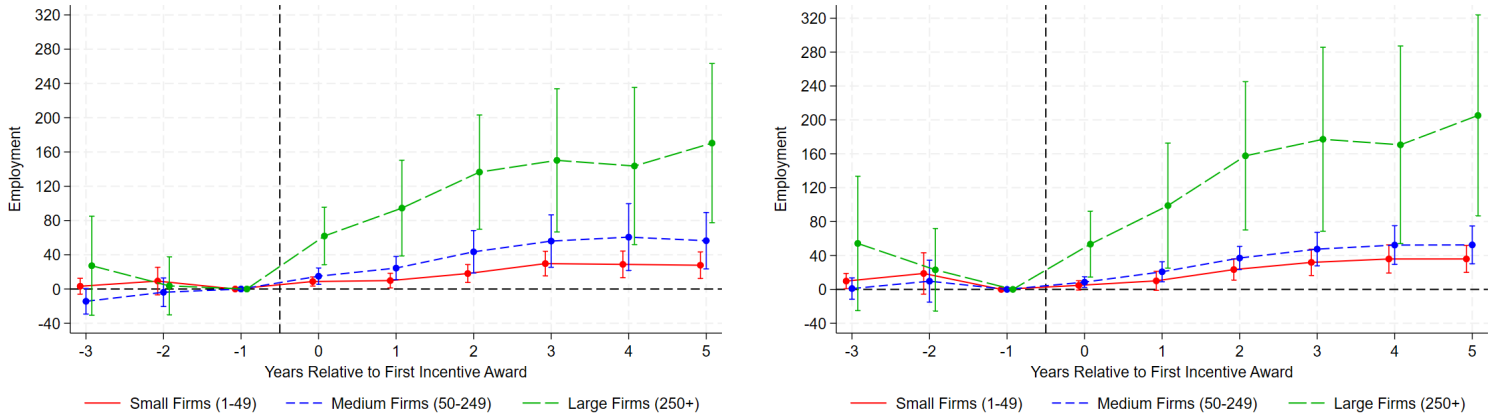
Figure 6: Event Studies - New and Existing Firms



(a) All Control Firms **(b) Within Industry Control Firms**

These figures present coefficients from event study regressions estimating Equation 2 for new and existing firms. The dependent variable is an indicator for incentive receipt interacted with relative time indicators and an indicator for "new", where year t denotes the year of incentive receipt. Panel (a) compares incentivized firms to non-incentivized firms within the same county, while Panel (b) compares incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. Red solid lines plot treatment effect coefficients for new firms, and blue dashed lines plot treatment effect coefficients for existing firms. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

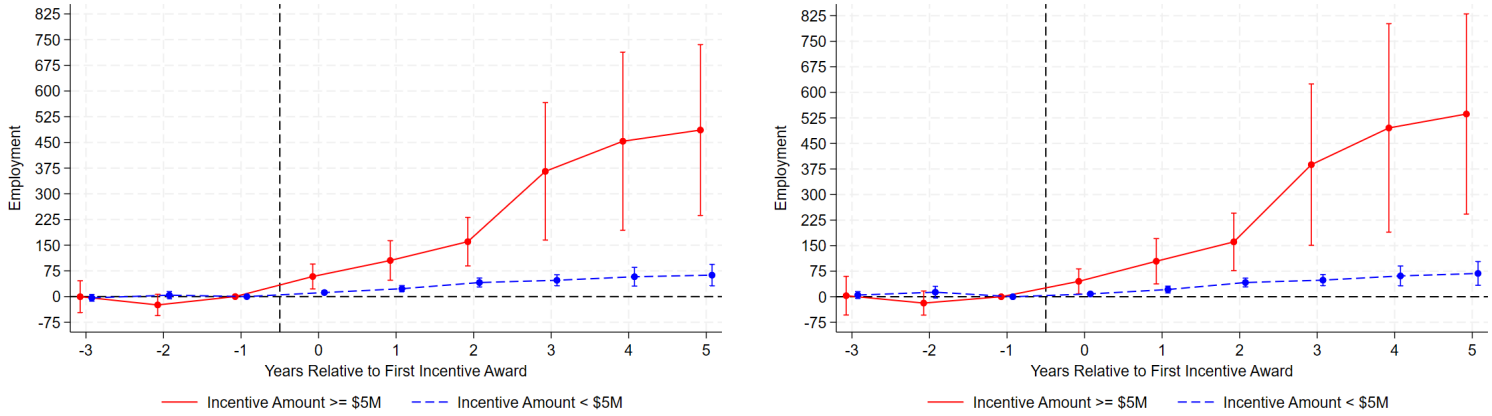
Figure 7: Event Studies - Firm Size



(a) All Control Firms **(b) Within Industry Control Firms**

These figures present coefficients from event study regressions estimating Equation 2 by firm size. Each firm is assigned to one of three firm size bins based on average employment in the pre-period. As a result, the sample is restricted to existing firms because we must observe pre-period employment. The dependent variable is an indicator for incentive receipt interacted with relative time indicators and firm size bin, where year t denotes the year of incentive receipt. Panel (a) compares incentivized firms to non-incentivized firms within the same county, while Panel (b) compares incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. Green dashed lines plot treatment effect coefficients for large firms, blue dashed lines plot treatment effect coefficients for medium firms, and the red solid lines plot treatment effect coefficients for small firms. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

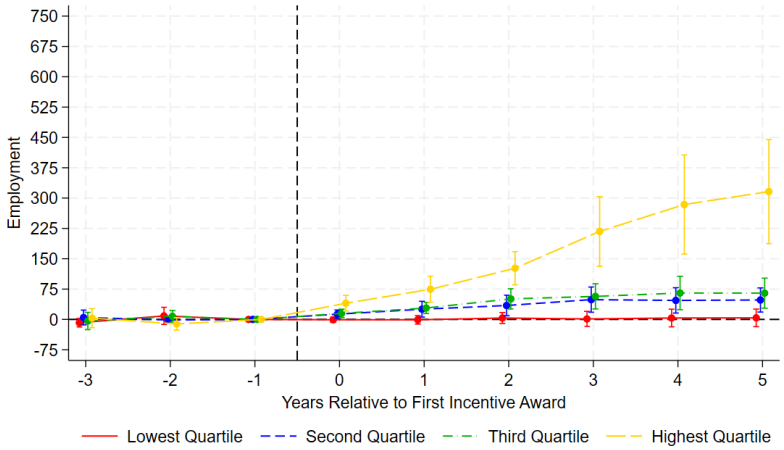
Figure 8: Event Studies - Large Deals (Above $5 Million)



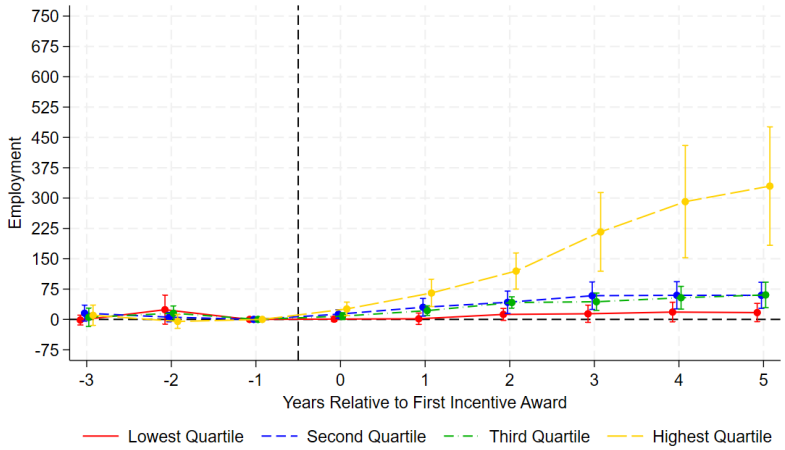
(a) All Control Firms **(b) Within Industry Control Firms**

These figures present coefficients from event study regressions estimating Equation 2 for incentives above and below $5 Million (Slattery, 2025). The dependent variable is an indicator for incentive receipt interacted with relative time indicators and an indicator for "large deal", where year t denotes the year of incentive receipt. Panel (a) compares incentivized firms to non-incentivized firms within the same county, while Panel (b) compares incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. Red solid lines plot treatment effect coefficients for firms awarded incentive deals of $5 million or more, and the dashed blue lines plot treatment effect coefficients for firms awarded incentive deals of less than $5 million. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

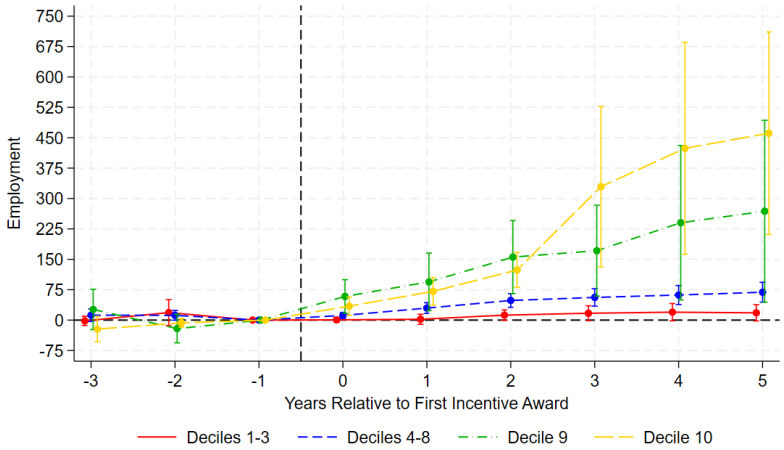
Figure 9: Event Studies - Incentive Size



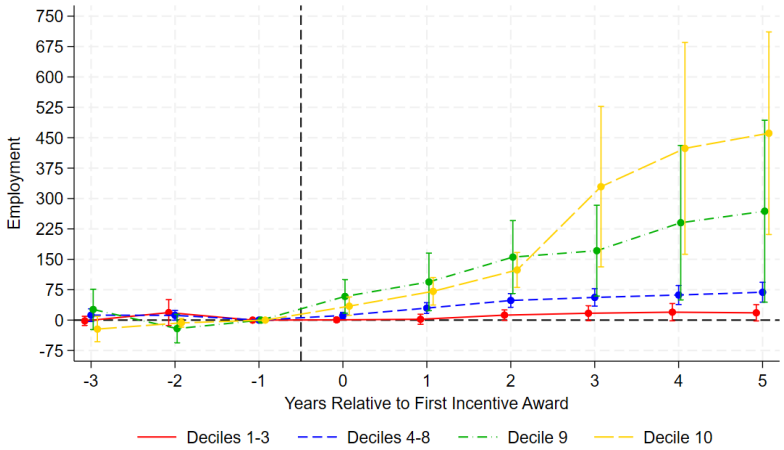
(a) All Control Firms - Quartiles



(b) Within Industry Control Firms - Quartiles



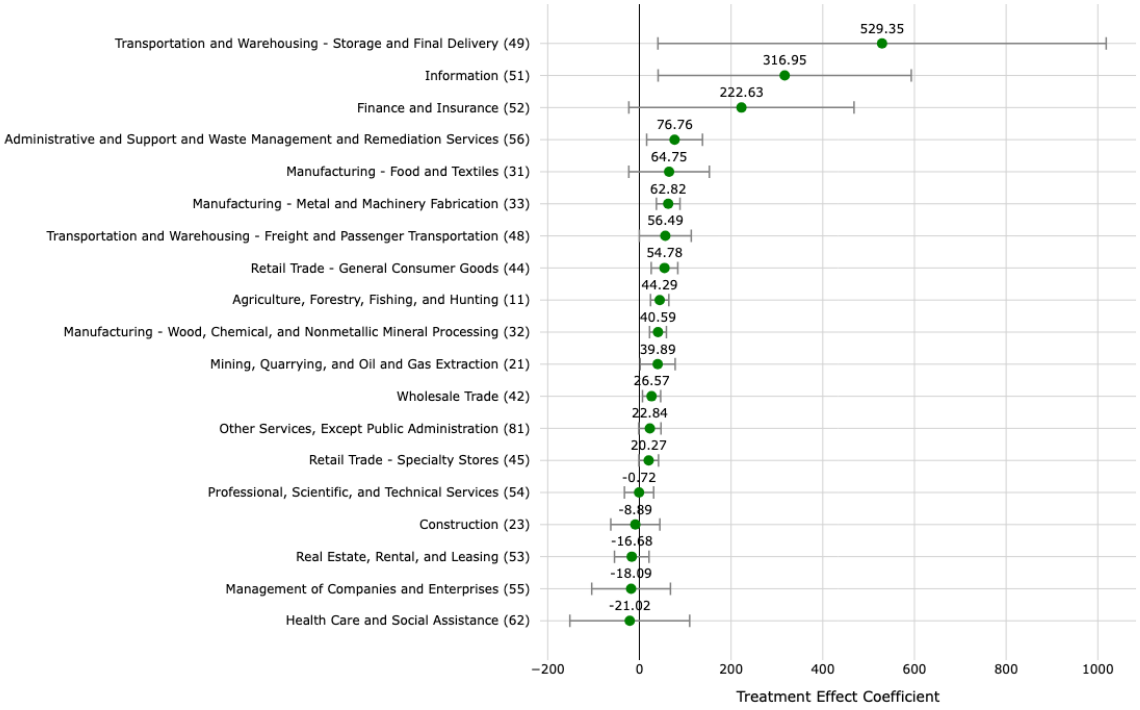
(c) All Control Firms - Decile Groups



(d) Within Industry Control Firms - Decile Groups

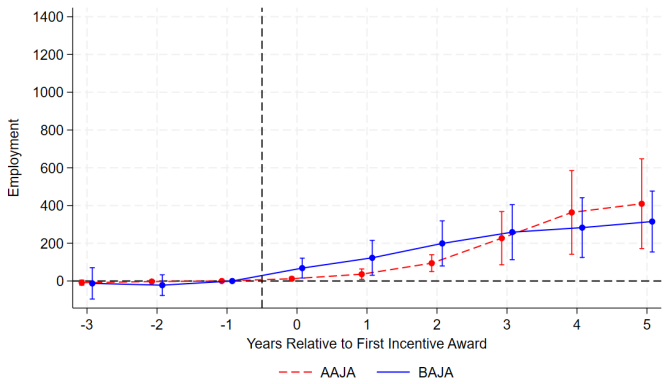
These figures present coefficients from event study regressions estimating Equation 2 by incentive size. The dependent variable is an indicator for incentive receipt interacted with relative time indicators and incentive size indicators, where year t denotes the year of incentive receipt. Panels (a) and (c) compare incentivized firms to non-incentivized firms within the same county, while Panels (b) and (d) compare incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. Panels (a) and (b) present results by incentive size quartiles. Panels (c) and (d) present results by incentive size deciles, where deciles with similar effects were grouped together ex-post. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using administrative employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

Figure 10: Employment Effects by Industry

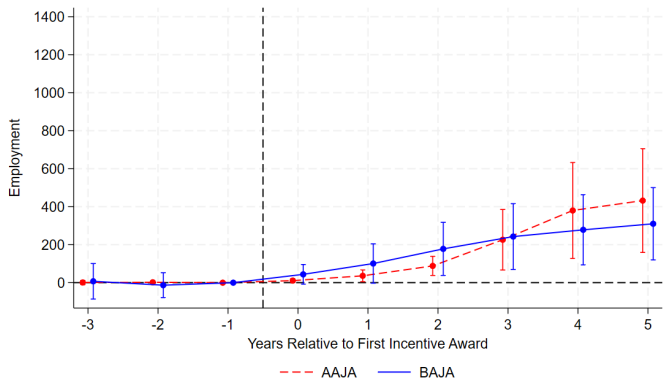


This figure presents coefficients from a difference-in-differences regression estimating Equation 1 by industry (2-digit NAICS code), comparing incentivized firms to non-incentivized firms within the same county. The dependent variable is an indicator for incentive receipt interacted with industry indicators, where year t denotes the year of incentive receipt. Horizontal lines show 95 percent confidence intervals based on robust standard errors clustered at the firm level. Control firms are entropy balanced on period $t-1$ employment and establishments. Industries are ordered by estimated effect size. Confidence intervals that do not cross zero correspond to statistically significant effects at the 5 percent level. Estimates for multi-digit NAICS sectors (e.g., Manufacturing 31–33, Transportation & Warehousing 48–49) are presented separately. All figures were created by the authors using administrative employer data from the Alabama Department of Workforce, incentives data from the Alabama Department of Commerce, and local economic developers.

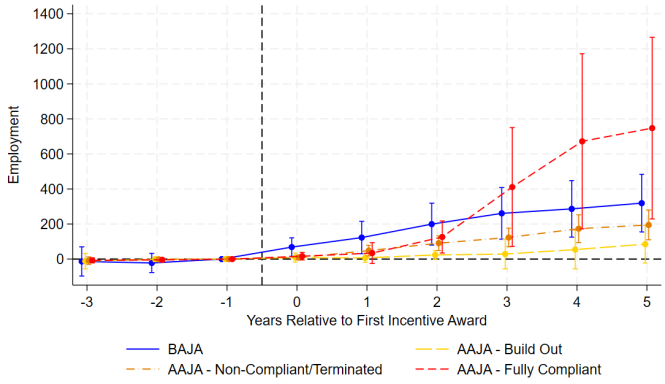
Figure 11: Event Studies - Alabama Jobs Act



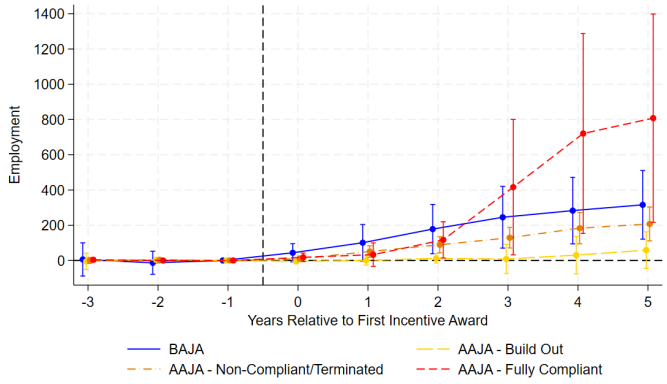
(a) All Control Firms



(b) Within Industry Control Firms



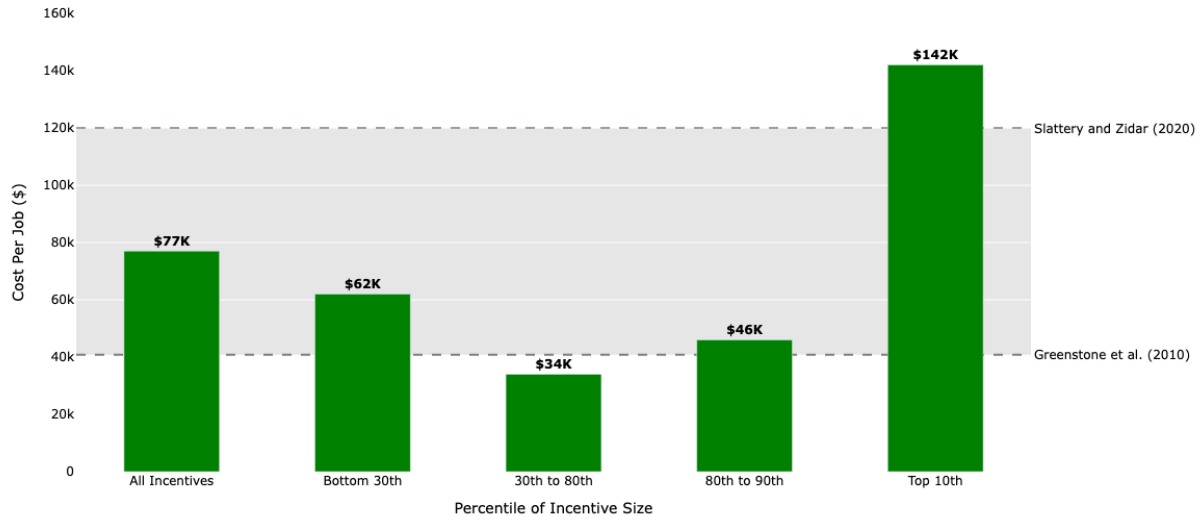
(c) All Control Firms - Compliance



(d) Within Industry Control Firms - Compliance

These figures present coefficients from event study regressions estimating Equation 2 for state deals before and after the Alabama Jobs Act (AJA). In Panels (a) and (b), the dependent variable is an indicator for incentive receipt interacted with relative time indicators and an indicator for AJA. In Panels (c) and (d), the dependent variable is an indicator for incentive receipt interacted with relative time indicators and an indicator for AJA, further interacted with compliance status. Year t denotes the year of incentive receipt. Panels (a) and (c) compare incentivized firms to non-incentivized firms within the same county, while Panels (b) and (d) compare incentivized firms to non-incentivized firms within the same county and industries. Both figures present year-specific treatment effect coefficients relative to $t-1$, along with 95 percent confidence intervals based on robust standard errors clustered at the firm level. Panels (a) and (b) present results for firms receiving state incentives before and after the AJA. Blue solid lines plot treatment effect coeffcients for firms that were awarded incentives before the AJA, and the red dashed lines correspond to firms awarded incentives after AJA. Panels (c) and (d) further parse out results by compliance status for firms receiving incentives under the AJA. Blue solid lines plot treatment effect coefficients for that were awarded incentives before the AJA, yellow lines correspond to firms that were not compliant or terminated under the AJA, orange lines correspond to firms awarded incentives under the AJA that are still in the build-out phase, and red solid lines correspond to firms fully compliant under the AJA. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. All figures were created by the authors using employment data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 3 describes the data and Section 4.3 describes the sample selection procedure.

Figure 12: Cost Per Job Across Incentive Size Percentile Groups



This figure plots the average cost per job created through business incentives, grouped by percentiles of incentive size. Green bars display the cost per job for each percentile category, with exact values labeled above each bar. The dashed horizontal lines and shaded region indicate benchmark cost-per-job estimates from prior research. The lower bound of $40,829 is from Greenstone et al. (2010), and the upper bound of $119,972 is from Slattery and Zidar (2020). All Incentives denotes the average cost per job across all incentive deals in our sample. All figures were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers. Section 4.3 describes the data and sample selection procedure.

Table 1: Statistics for All Incentive Awards - All Data Collected

Panel A: Statistics by Year

Year	Incentivized Firms	Incentives Awarded	Total Amount of Incentives Awarded	Average Incentive Amount	Median Incentive Amount
2012	151	283	$260,623,106	$1,692,358	$253,258
2013	109	224	$145,720,871	$1,324,735	$319,375
2014	140	292	$146,346,536	$1,030,609	$391,772
2015	120	262	$436,505,302	$3,577,912	$389,991
2016	118	288	$347,576,579	$2,896,471	$571,216
2017	135	314	$457,521,787	$3,389,050	$679,700
2018	150	368	$1,286,994,379	$9,127,620	$887,330
2019	138	334	$753,178,059	$5,705,894	$937,928
2020	119	312	$416,588,876	$3,686,627	$923,108
2021	106	238	$524,194,638	$5,139,163	$781,350
2022	101	248	$876,790,362	$9,133,233	$841,096
2023	94	209	$432,890,103	$4,975,748	$1,176,800
2024	65	139	$118,127,809	$1,968,797	$364,779
Total	1,008	3,511	$6,203,058,407	$4,097,132	$544,486

Panel B: Statistics by Incentive Type

Type Of Incentive	Number of Incentives	Percent of Total Incentives	Total Incentive Amount	Percent of Total Incentive Amount
Property Tax Abatement	1,484	42%	$1,225,551,203	20%
Sales and Use Tax Abatement	1,238	35%	$946,885,900	15%
Cash Incentive	214	6%	$579,769,655	9%
In-Kind Incentive	90	3%	$58,927,646	1%
Jobs Credit	259	7%	$720,096,567	12%
Investment Credit	226	6%	$2,671,827,436	43%
Total	3,511	100%	$6,203,058,407	100%

Table 1 presents descriptive statistics for all incentives in this paper's study period. Panel A shows annual statistics including the number of incentivized firms, total incentives awarded, and incentive amounts (measured by full package value at award, not annual payments), along with corresponding averages and medians by year. Total incentivized firms corresponds to unique incentivized firms. Panel B shows summary statistics by incentive type, including the number and percentage of each incentive type, total amounts, and percentage of total incentive value. For example, if a firm receives a property tax abatement and a cash incentive, we count these as two incentives. Sales and use tax abatements are combined as some counties could not separate them. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 2: Sample Selection

Step	Action	Observations		Unique Firms		Unique Firm-Counties	
		Change	After	Change	After	Change	After
1	All UI account observations for the private sector with non-missing ownership status	-	2,319,672	-	282,349	-	311,070
2	Drop out of state locations	-3,967	2,315,705	-433	281,916	-665	310,405
3	Split payroll firms into their individual client firms	5	2,315,710	15,152	297,068	13,276	323,681
4	Drop firms that only ever report 0 employment	-172,052	2,143,658	-59,266	237,802	-62,246	261,435
5	Drop leading and trailing 0s for employment	-383,635	1,760,023	0	237,802	0	261,435
6	Drop firms with missing NAICS codes	-74	1,759,949	-74	237,728	-74	261,361
7	Resolve account numbers (link together account numbers which correspond to the same firm)	4	1,759,953	-526	237,202	-747	260,614
8	Drop firm-counties only in the sample for 1 year (that entered before 2025)	-44,133	1,715,820	-41,076	196,126	-43,271	217,343
9	Drop firms with missing county (county = 999)	-136,860	1,578,960	-24,463	171,663	-25,517	191,826
10	Collapse to firm-county year level	-192,313	1,386,647	0	171,663	0	191,826
11	Drop firm-counties with spurious 0s	-19,647	1,367,000	-1,765	169,898	-1,880	189,946
12	Drop firm-counties with year gaps > 2	-3,920	1,363,080	-33	169,865	-522	189,424
13	Augment gaps of 2 years	375	1,363,455	0	169,865	0	189,424
14	Drop firms outside 41 sample counties	-219,720	1,143,735	-25,791	144,074	-30,493	158,931
15	Drop firms outside 15 sample counties	-297,369	846,366	-36,255	107,819	-41,696	117,235
16	Merge in incentives data	309	846,675	0	107,819	0	117,235
17	Fill in year gaps created by merging in incentives	5	846,680	0	107,819	0	117,235
	Final	-1,681,777	846,680	-179,409	107,819	-197,948	117,235
	Incentivized	-	7,010	-	495	-	508
	Non-Incentivized	-	839,670	-	107,324	-	116,727
18	Construct and stack cohorts	-	3,914,471	-799	107,020	-874	116,361
19	Drop firms first incentivized after 2020	-29,470	3,885,001	-29	106,991	-35	116,326
20	Backfill within cohort window	1,389,958	5,274,959	0	106,991	0	116,326
21	Drop firms not observed in t-1 through t+2	-454,098	4,820,861	-10,367	96,624	-11,360	104,966
22	Drop cohort(s) that don't converge when entropy balancing	-53,641	4,767,220	-1	96,623	-1	104,965
23	Drop firms with 0 weight after balancing	-195	4,767,025	0	96,623	0	104,965
	Final	852,749	4,767,025	-11,196	96,623	-12,270	104,965
	Incentivized	-	4,134	-	462	-	472
	Non-Incentivized	-	4,762,891	-	96,161	-	104,493

Table 2 reports selection steps for the treatment sample of incentivized firms and the control sample of non-incentivized firms. See Section 4.3 for details. The final panel is collapsed to the firm–county–year level. Entropy balancing weights are applied within each cohort using two approaches: (1) balancing on employment and establishments in year t-1 to ensure similarity based on pre-incentive firm size, and (2) balancing on employment and industry composition using complete sets of major industry classification indicators in year t-1 to account for industry-specific incentive patterns. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 3: Descriptive Statistics for Baseline and Stacked Samples

	N	Mean	SD	Min	p1	p25	p50	p75	p99	Max
Panel A: Baseline Sample										
Firm Characteristics										
Employment	846,680	18.14	84.73	0	0	2	4	12	217	6,311
Log(1 + Employment)	846,680	1.929	1.165	0.000	0.000	1.099	1.609	2.565	5.384	8.750
Total Payroll ($)	846,680	901,313	6,765,357	0	0	50,036	148,846	470,984	11,108,980	681,011,580
Earnings/Employee ($)	837,268	42,782	39,356	3,240	3,240	18,322	32,000	52,292	244,152	244,152
Establishments	846,680	1.16	1.15	0	1	1	1	1	5	87
Incentive Statistics										
Incentivized	846,680	0.008	0.091	0.000	0.000	0.000	0.000	0.000	0.000	1.000
Incentive × Post	846,680	0.006	0.078	0.000	0.000	0.000	0.000	0.000	0.000	1.000
Lump Sum Incentive Award	777	4,590,939	22,283,854	3	505	118,235	435,020	1,980,300	80,454,815	450,637,781
Lump Sum First Incentive Award	493	4,278,674	25,772,510	3	76	85,950	333,265	1,374,000	62,000,000	450,637,781
County Level Controls										
Population	846,680	323,999	220,560	54,377	54,495	119,352	226,955	656,912	672,550	672,550
Median Household Income	846,680	53,780	11,005	37,661	38,703	45,415	51,169	59,082	90,618	93,543
High School Education or More	846,680	87.71	3.879	73.700	76.900	85.300	87.700	90.600	96.900	97.400
Poverty Rate	846,680	15.68	3.538	6.700	6.800	14.500	16.500	18.200	21.200	21.900
Percent White	846,680	66.05	16.253	31.800	32.800	52.400	63.600	81.400	95.700	96.100
Panel B: Within County Stacked Sample										
Firm Characteristics										
Employment	4,767,025	12.87	65.39	0	0	0	2	9	168	5,659
Log(1 + Employment)	4,767,025	1.82	1.127	0.000	0.693	0.693	1.609	2.485	5.176	8.641
Total Payroll ($)	4,767,025	606,137	5,115,446	0	0	0	70,908	304,572	7,816,960	671,795,344
Earnings/Employee ($)	3,395,303	42,039	38,110	3,240	3,584	18,546	31,937	51,197	237,053	244,152
Establishments	4,767,025	0.84	1.14	0	0	0	1	1	4	87
New Firm	4,767,025	0.41	0.492	0.000	0.000	0.000	0.000	1.000	1.000	1.000
Incentive Statistics										
Incentivized	4,767,025	0.001	0.029	0.000	0.000	0.000	0.000	0.000	0.000	1.000
Incentive × Post	4,767,025	0.001	0.024	0.000	0.000	0.000	0.000	0.000	0.000	1.000
Lump Sum Incentive Award	637	3,576,491	20,292,804	3	318	105,586	384,265	1,576,272	52,404,000	450,637,781
Lump Sum First Incentive Award	458	3,505,035	22,801,032	3	76	85,950	313,875	1,230,113	43,085,536	450,637,781
County Level Controls										
Population	4,767,025	338,223	222,563	54,377	54,771	123,102	227,197	658,464	672,550	672,550
Median Household Income	4,767,025	51,603	9,647	37,661	38,971	45,358	49,321	55,088	85,895	93,543
High School Education or More	4,767,025	87.23	3.461	73.700	77.500	85.200	87.400	89.700	94.000	97.400
Poverty Rate	4,767,025	16.12	3.376	6.700	6.800	15.100	17.000	18.400	21.400	21.900
Percent White	4,767,025	65.32	16.495	31.800	33.900	52.400	60.600	81.200	95.700	96.100

Table 3 reports descriptive statistics for the sample both before (Panel A) and after (Panel B) cohort construction. See Section 4.3 for details. Incentivized equals one if a firm ever receives an incentive in the sample period. Incentive × Post equals one for firm-years after initial incentive receipt. Lump-sum variables capture the nominal value of incentive awards in U.S. dollars. County-level controls, matched to each firm's location, include demographic and economic characteristics: population, median household income, educational attainment (share of adults with at least a high school education), poverty rate, and racial composition (percent white). These variables are derived from the annual Census American Community Survey. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 4: Incentivized vs Non-Incentivized Firm Characteristics and Entropy Balance Weights

	Incentivized Firms (n=472)		Non-Incentivized Firms (n=557,804)		Difference in means, unweighted		Difference in means, EB weights	
	Mean unweighted	Mean weighted	Mean unweighted	Mean weighted	Difference (3)-(1)	*p*-value	Difference (4)-(2)	*p*-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employment	138.74	138.74	11.31	137.07	-127.43***	0.00	-1.67	0.94
Log(1 + Employment)	3.20	3.20	1.79	2.32	-1.41***	0.00	-0.88***	0.00
Total Payroll ($)	9,145,989	9,145,989	498,018	6,332,920	-8,647,971***	0.000	-2,813,069*	0.060
Earnings/Employee ($)	57,381	57,381	39,320	37,500	-18,061***	0.00	-19,881***	0.00
Establishments	1.27	1.27	0.70	1.27	-0.57***	0.00	0.00	0.989
New Firm	0.27	0.27	0.40	0.39	0.13***	0.00	-0.12*	0.00

Table 4 compares firm characteristics for incentivized and non-incentivized firms both before and after entropy balancing. Columns (1) and (2) present unweighted and entropy balanced weighted means of characteristics for incentivized firms, while Columns (3) and (4) present analogous means for non-incentivized firms. Columns (5) and (6) present the difference in unweighted means between the two samples and the p-value of this difference. Columns (7) and (8) present analogous statistics after entropy balancing. Statistical significance indicated by asterisks (*** p<0.01; ** p<0.05; * p<0.10). All variables are calculated at time t-1. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 5: Statistics for First-Time Incentive Awards - In Sample

Panel A: Statistics by Year

Year	Incentivized Firms	Incentives Awarded	Total Amount of Incentives Awarded	Average Incentive Amount	Median Incentive Amount
2012	108	199	$208,656,959	$1,950,065	$208,750
2013	53	112	$65,061,188	$1,327,779	$300,000
2014	59	133	$42,894,923	$739,568	$350,411
2015	49	112	$191,057,792	$3,899,139	$356,150
2016	44	90	$101,618,399	$2,363,219	$276,000
2017	42	102	$81,353,261	$1,984,226	$471,100
2018	50	127	$690,115,303	$14,683,304	$381,314
2019	38	84	$160,791,011	$4,594,029	$410,681
2020	29	79	$63,757,088	$2,198,520	$463,623
Total	472	1,038	$1,605,305,924	$3,505,035	$313,875

Panel B: Statistics by Incentive Type

Type Of Incentive	Number of Incentives	Percent of Total Incentives	Total Incentive Amount	Percent of Total Incentive Amount
Property Tax Abatement	442	43%	$354,679,593	22%
Sales/Use Tax Abatement	372	36%	$218,462,626	14%
Cash Incentives	80	8%	$245,411,046	15%
In-kind Incentives	57	5%	$30,358,637	2%
Jobs Credit	46	4%	$230,835,590	14%
Investment Credit	41	4%	$525,558,432	33%
Total	1,038	100.00%	$1,605,305,924	100.00%

Table 5 shows statistics for first-time incentive recipients. Panel A shows annual statistics including the number of incentivized firms, total incentives awarded, and incentive amounts (measured by full package value at award, not annual payments), along with corresponding averages and medians by year. Panel B shows summary statistics by incentive type, including the number and percentage of each incentive type, total amounts, and percentage of total incentive value. For example, if a firm receives a property tax abatement and a cash incentive, we count these as two incentives. Sales and use tax abatements are combined as some counties could not separate them. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 6: Terms of Incentive Deals

	Slattery (2024)		Lester, Millar, and Rauh (2025)	
	U.S. Sample	AL Sample	Full Sample	First-Incentivized
Number of Deals	387	15	660	472
Average Incentive ($)	$149,500,000	$184,170,440	$3,576,491	$3,505,035
Average Promised Jobs	1,399	1,462	102	108
Average Promised Investment	$1,084,000,000	$932,500,000	$43,139,876	$38,223,101
Cost/Job Promised	$107,000	$356,153	$61,494	$46,114

Table 6 presents descriptive statistics for incentive deals compared to previous literature. Column 1 outlines the summary statistics from Table 1 in Slattery (2025) where the sample period is 2002 - 2017. Column 2 outlines summary statistics based on our recreation of Slattery (2025)'s sample of incentives from Alabama. Columns 3 and 4 outline summary statistics for our full sample of incentives and our first-incentivized sample from 2010 - 2020, respectively. The mean and median for jobs promised, investment promised, and cost per job promised are based on 311/472 first-time deals and 440/660 overall deals for which this data was provided. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 7: Difference-in-differences results

Variables	(1) *Firm Employment*	(2) *Firm Employment*
Incentive × Post	61.28***	58.64***
	(11.90)	(16.44)
Firm-by-Cohort FE	Yes	Yes
Year-by-Cohort FE	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort
R^2	0.91	0.85
Observations	4,767,025	603,845

Table 7 presents regression results for the impact of business incentives on employment. We estimate Equation 1 on stacked cohorts, where each cohort corresponds to a calendar year when the first incentive is awarded. Each incentivized firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+5$ around the event year. Regressions include firm-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the firm-by-cohort level and are in parentheses. In all regressions, control firms are entropy balanced on period $t-1$ employment and establishments. In Column (1), the control sample includes all non-incentivized firms in the same county. In Column (2), we restrict the control sample to only firms in the same industries as the incentivized firms in that cohort. ***, **, * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. All tables were created by the authors using administrative employer data from the Alabama Department of Workforce and incentives data from the Alabama Department of Commerce and local economic developers.

Table 8: Difference-in-differences results - New and Existing Firms

Variables	(1) *Firm Employment*	(2) *Firm Employment*
Incentive × Post	46.37***	37.12***
	(8.89)	(9.02)
Incentive × Post × New	103.24***	116.62***
	(29.09)	(33.96)
County Controls	-	-
Firm-by-Cohort FE	Yes	Yes
Year-by-Cohort FE	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort
R^2	0.91	0.85
Observations	4,767,025	603,845
Triple Difference:		
Incentive × Post × New	56.87*	79.49**
− Incentive × Post	(31.02)	(35.38)
Dependent variable mean values $(t-1)$:		
Incentivized New Firms	0.00	0.00
Incentivized Existing Firms	191.49	159.40
Observations by group:		
Incentivized Firms:		
New	1,143	972
Existing	2,991	2,634
	4,134	3,606
Non-Incentivized Firms:		
New	1,945,717	225,721
Existing	2,817,174	374,518
	4,762,891	600,239
Total observations	4,767,025	603,845

Table 8 presents the regression results for the estimated employment effects of business incentives in new and existing firms. Equation 1 is estimated on stacked cohorts, where each cohort corresponds to a calendar year when the first incentive is awarded. Each incentivized firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+5$ around the event year. Regressions include firm-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the firm-by-cohort level and are in parenthesis. In all regressions, entropy balancing is used. In Column (1), the control sample includes all non-incentivized firms in the same county. In Column (2), we restrict the control sample to only firms in the same industries as the incentivized firms in that cohort. ***, **, * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 9: Difference-in-differences results - Firm Size

Variables	(1)	(2)
	Firm Employment	
Incentive × Post × Small	15.54***	13.43**
	(5.87)	(7.32)
Incentive × Post × Medium	47.09***	31.89***
	(13.70)	(9.10)
Incentive × Post × Large	117.07***	121.54***
	(33.56)	(44.08)
Firm-by-Cohort FE	Yes	Yes
Year-by-Cohort FE	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort
R^2	0.96	0.95
Observations	2,820,165	377,152
Triple Difference:		
Incentive × Post × Large	101.53***	108.10**
− Incentive × Post × Small	(33.72)	(43.39)
Incentive × Post × Large	69.97*	89.64**
− Incentive × Post × Medium	(37.78)	(44.83)
Incentive × Post × Medium	31.55**	18.46**
− Incentive × Post × Small	(12.78)	(8.37)
Dependent variable mean values $(t-1)$:		
Incentivized Small Firms	8.72	8.71
Incentivized Medium Firms	100.88	100.54
Incentivized Large Firms	606.45	603.93
Observations by group:		
Incentivized Firms:		
Small	1,272	1,201
Medium	1,125	1,026
Large	594	407
	2,991	2,634
Non-Incentivized Firms:		
Small	2,614,867	347,781
Medium	179,752	23,475
Large	22,555	3,262
	2,817,174	374,518
Total observations	2,820,165	377,152

Table 9 presents the regression results for the estimated employment effects of business incentives in small, medium, and large firms. We define small firms as those with 1-49 employees, medium firms as those with 50-249 employees, and large firms as those with more than 250 employees. Equation 1 is estimated on stacked cohorts, where each cohort corresponds to a calendar year when the first incentive is awarded. Each incentivized firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+5$ around the event year. Regressions include firm-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the firm-by-cohort level and are in parenthesis. In all regressions, entropy balancing is used. In Column (1), the control sample includes all non-incentivized firms in the same county. In Column (2), we restrict the control sample to only firms in the same industries as the incentivized firms in that cohort. ***, **, * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 10: Difference-in-differences results - Incentive Size

Variables	(1)	(2)	(3)	(4)	(5)	(6)
	Firm Employment					
Incentive × Post	38.98***			33.99***		
	(8.85)			(8.90)		
Incentive × Post × Mega Deal	276.00***			288.96***		
	(65.60)			(78.16)		
Incentive × Post × 1st Quartile		-0.57			0.70	
		(7.59)			(9.68)	
Incentive × Post × 2nd Quartile		34.02***			36.39***	
		(9.85)			(10.87)	
Incentive × Post × 3rd Quartile		43.75***			29.65***	
		(14.20)			(9.94)	
Incentive × Post × 4th Quartile		176.69***			170.39***	
		(32.81)			(36.51)	
Incentive × Post × 1st Decile			-10.993			-13.91
			(12.76)			(18.70)
Incentive × Post × 2nd Decile			1.449			4.91
			(11.47)			(10.38)
Incentive × Post × 3rd Decile			17.127*			17.82
			(9.86)			(11.50)
Incentive × Post × 4th Decile			47.823**			34.02***
			(19.79)			(10.96)
Incentive × Post × 5th Decile			29.011***			46.32**
			(11.00)			(21.33)
Incentive × Post × 6th Decile			48.478			13.18
			(31.04)			(9.30)
Incentive × Post × 7th Decile			27.935*			31.59*
			(14.44)			(16.67)
Incentive × Post × 8th Decile			65.841***			61.33***
			(14.34)			(15.72)
Incentive × Post × 9th Decile			161.048***			141.45**
			(58.00)			(61.50)
Incentive × Post × 10th Decile			246.72***			255.43***
			(62.46)			(72.68)
Firm-by-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-by-Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort
R^2	0.91	0.91	0.91	0.86	0.85	0.86
Observations	4,766,900	4,766,900	4,766,900	603,774	603,774	603,774
Triple Difference:						
Incentive × Post × Mega Deal	237.02***			254.97***		
− Incentive × Post	(66.72)			(79.33)		
Incentive × Post × 4th Quartile		177.25***			169.68***	
− Incentive × Post × 1st Quartile		(34.51)			(38.11)	
Incentive × Post × 10th Decile			257.716***			269.34***
− Incentive × Post × 1st Decile			(62.44)			(73.69)

Dependent variable mean values ($t-1$):						
Incentivized Non-Mega Deal Firms	112.74			95.06		
Incentivized Mega Deal Firms	375.21			316.82		
Incentivized 1st Quartile Firms		53.25			45.19	
Incentivized 2nd Quartile Firms		96.21			86.70	
Incentivized 3rd Quartile Firms		155.01			128.93	
Incentivized 4th Quartile Firms		257.16			209.94	
Incentivized 1st Decile Firms			46.13			50.68
Incentivized 2nd Decile Firms			69.72			45.80
Incentivized 3rd Decile Firms			45.04			44.49
Incentivized 4th Decile Firms			114.52			53.88
Incentivized 5th Decile Firms			98.29			133.61
Incentivized 6th Decile Firms			148.72			138.08
Incentivized 7th Decile Firms			177.72			135.34
Incentivized 8th Decile Firms			119.09			101.78
Incentivized 9th Decile Firms			287.65			192.61
Incentivized 10th Decile Firms			298.13			279.65
Incentive size mean values:						
Incentivized Non-Mega Deal Firms	$618,571			$621,397		
Incentivized Mega Deal Firms	$28,160,250			$30,504,472		
Incentivized 1st Quartile Firms		$30,753			$28,036	
Incentivized 2nd Quartile Firms		$189,551			$180,485	
Incentivized 3rd Quartile Firms		$631,625			$619,392	
Incentivized 4th Quartile Firms		$13,223,891			13,794,303	
Incentivized 1st Decile Firms			$7,064			$5,726
Incentivized 2nd Decile Firms			$36,333			$32,738
Incentivized 3rd Decile Firms			$84,876			$78,416
Incentivized 4th Decile Firms			$162,626			$152,121
Incentivized 5th Decile Firms			$261,424			$249,869
Incentivized 6th Decile Firms			$395,415			$389,899
Incentivized 7th Decile Firms			$659,105			$639,461
Incentivized 8th Decile Firms			$1,302,138			$1,270,372
Incentivized 9th Decile Firms			$2,950,733			$2,865,151
Incentivized 10th Decile Firms			$29,689,344			$31,142,084

Observations by group:						
Incentivized Firms:						
Non-Mega Deal	3,589			3,178		
Mega Deal	420			357		
1st Quartile Firms		994			878	
2nd Quartile Firms		1,002			884	
3rd Quartile Firms		1,010			886	
4th Quartile Firms		1,003			884	
1st Decile Firms			387			343
2nd Decile Firms			403			348
3rd Decile Firms			407			362
4th Decile Firms			405			354
5th Decile Firms			394			355
6th Decile Firms			406			353
7th Decile Firms			403			358
8th Decile Firms			405			352
9th Decile Firms			402			361
10th Decile Firms			397			349
	4,009	4,009	4,009	3,535	3,535	3,535
Non-Incentivized Firms	4,762,891	4,762,891	4,762,891	600,239	600,239	600,239
Total observations	4,766,900	4,766,900	4,766,900	603,774	603,774	603,774

Table 10 presents regression results for the impact of business incentives on employment based on the size of the incentive package. We split the incentive deals first by whether or not the package is above or below the $5 million "mega-deal" threshold. Then, we split the incentives into quartiles of monetary value. Finally, we compare the deciles of incentive size. Equation 1 is estimated on stacked cohorts, where each cohort corresponds to a calendar year when the first incentive is awarded. Each incentivized firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+5$ around the event year. Regressions include firm-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the firm-by-cohort level and are in parenthesis. In all regressions, entropy balancing is used. In Columns (1), (2), and (3), control firms are balanced on period $t-1$ employment and establishments. In Columns (4), (5), and (6), control firms are balanced on period $t-1$ establishments and industry mix. ***, **, * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 11: Difference-in-differences results - Industry

Panel A: Results

Variables	(1) *Firm Employment*	(2) *Firm Employment*
Incentivize × Post × NAICS Sector:		
(11) Agriculture, Forestry, Fishing, and Hunting	44.29***	59.195***
	(10.18)	(14.46)
(21) Mining, Quarrying, and Oil and Gas Extraction	39.89**	30.859
	(19.47)	(29.93)
(23) Construction	-8.89	-2.688
	(27.31)	(28.11)
(31) Manufacturing - Food and Textiles	64.75	34.927**
	(44.31)	(15.26)
(32) Manufacturing - Wood, Chemical, and Nonmetallic Mineral Processing	40.58***	36.746***
	(9.37)	(10.26)
(33) Manufacturing - Metals and Machinery Fabrication	62.82***	57.155***
	(13.13)	(13,97)
(42) Wholesale Trade	26.56***	26.707**
	(10.09)	(11.77)
(44) Retail Trade - General Consumer Goods	54.78***	41.224**
	(14.80)	(16.11)
(45) Retail Trade - Specialty Stores	20.26*	24.153*
	(10.93)	(13.66)
(48) Transportation and Warehousing - Freight and Passenger	56.49*	45.602
	(28.90)	(36.95)
(49) Transportation and Warehousing - Storage and Final Delivery	529.34**	620.764**
	(249.45)	(284.06)
(51) Information	316.94**	309.145**
	(140.88)	(138.09)
(52) Finance and Insurance	222.62*	309.145
	(125.31)	(138.09)
(53) Real Estate, Rental, and Leasing	-16.68	-
	(19.22)	-
(54) Professional, Scientific, and Technical Services	-0.72	-3.794
	(16.26)	(17.68)
(55) Management of Companies and Enterprises	-18.08	-84.139
	(43.82)	(72.51)
(56) Administrative and Support and Waste Management and Remediation Services	76.76**	78.622**
	(31.06)	(33.66)
(62) Health Care and Social Assistance	-21.01	-138.218
	(66.66)	(106.83)
(81) Other Services, Except Public Administration	22.83*	17.715
	(12.37)	(14.68)
County Controls	-	-
Firm-by-Cohort FE	Yes	Yes
Year-by-Cohort FE	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort
R^2	0.9136	0.8638
Observations	4,767,025	603,845

Panel B: Descriptives

Dependent variable mean values (t-1):	(1)	(2)
(11) Agriculture, Forestry, Fishing, and Hunting	39.00	39.00
(21) Mining, Quarrying, and Oil and Gas Extraction	381.00	381.00
(23) Construction	116.30	124.00
(31) Manufacturing - Food and Textiles	218.66	132.71
(32) Manufacturing - Wood, Chemical, and Nonmetallic Mineral Processing	106.72	103.49
(33) Manufacturing - Metals and Machinery Fabrication	156.14	125.19
(42) Wholesale Trade	48.43	45.28
(44) Retail Trade - General Consumer Goods	13.90	11.67
(45) Retail Trade - Specialty Stores	56.43	65.83
(48) Transportation and Warehousing - Freight and Passenger	52.50	52.50
(49) Transportation and Warehousing - Storage and Final Delivery	389.50	467.40
(51) Information	56.50	56.50
(52) Finance and Insurance	501.50	587.60
(53) Real Estate, Rental, and Leasing	0.00	-
(54) Professional, Scientific, and Technical Services	35.35	35.35
(55) Management of Companies and Enterprises	436.83	445.50
(56) Administrative and Support and Waste Management and Remediation Services	95.25	74.57
(62) Health Care and Social Assistance	484.00	195.00
(81) Other Services, Except Public Administration	13.71	13.71

	Incentivized		**Non-Incentivized**	
Observations by Industry Group	(1)	(2)	(1)	(2)
(11) Agriculture, Forestry, Fishing, and Hunting	9	9	30,502	217
(21) Mining, Quarrying, and Oil and Gas Extraction	34	34	9,293	583
(23) Construction	86	77	424,767	49,340
(31) Manufacturing - Food and Textiles	257	212	25,181	4,596
(32) Manufacturing - Wood, Chemical, and Nonmetallic Mineral Processing	837	739	65,234	31,643
(33) Manufacturing - Metal and Machinery Fabrication	1,637	1,404	114,343	74,470
(42) Wholesale Trade	518	491	309,720	133,676
(44) Retail Trade - General Consumer Goods	86	77	316,340	14,480
(45) Retail Trade - Specialty Stores	61	52	340,443	15,169
(48) Transportation and Warehousing - Freight and Passenger	36	36	127,833	12,291
(49) Transportation and Warehousing - Storage and Final Delivery	108	90	23,319	4,140
(51) Information	18	18	85,624	8,832
(52) Finance and Insurance	49	40	278,979	29,220
(53) Real Estate, Rental, and Leasing	9	-	221,093	-
(54) Professional, Scientific, and Technical Services	176	176	568,792	165,790
(55) Management of Companies and Enterprises	53	17	27,673	2,392
(56) Administrative and Support and Waste Management and Remediation Services	71	63	269,328	18,377
(62) Health Care and Social Assistance	27	9	590,999	19,530
(81) Other Services, Except Public Administration	62	62	421,494	15,493
Total	4,134	3,606	4,726,891	600,239

Table 11 presents treatment effect estimates of incentivized versus non-incentivized firms across various NAICS industry categories. The triple interaction term reflects the impact of the incentive treatment on employment levels, indicating an increase in employment associated with the incentivized firms. Coefficients are reported with standard errors in parentheses. Statistical significance is denoted by asterisks: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Observations are categorized into incentivized and unincentivized firms, and means are calculated with and without entropy balance weights for comparison.

Table 12: Difference-in-differences results - Alabama Jobs Act

Variables	(1)	(2)	(3)	(4)
	Firm Employment			
Incentive × Post	217.80***	219.59***	194.68**	197.03**
	(65.81)	(66.33)	(75.59)	(76.37)
Incentive × Post × AJA	187.17***		186.80***	
	(51.60)		(58.55)	
Incentive × Post × AJA × Build Out		35.42		14.14
		(26.88)		(28.78)
Incentive × Post × AJA		106.40***		106.23***
× Non-Compliant/Terminated		(23.94)		(27.19)
Incentive × Post × AJA × Fully Compliant		332.23***		345.23**
		(117.78)		(133.60)
Firm-by-Cohort FE	Yes	Yes	Yes	Yes
Year-by-Cohort FE	Yes	Yes	Yes	Yes
Clustering	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort	Firm-by-Cohort
R^2	0.90	0.90	0.83	0.83
Observations	4,763,650	4,763,650	600,909	600,909
Triple Difference:				
Incentive × Post × AJA	-30.62		-7.88	
− Incentive × Post	(83.54)		(95.47)	
Incentive × Post × AJA × Fully-Compliant		112.63		148.15
− Incentive × Post		(134.63)		(153.10)
Dependent variable mean values $(t-1)$:				
Incentivized Pre-AJA Firms	228.26	228.26	242.78	242.78
Incentivized AJA Firms	162.07		147.07	
Incentivized AJA - Build Out Firms		148.90		165.44
Incentivized AJA		83.42		71.76
- Non-Compliant/Terminated Firms				
Incentivized AJA - Always Compliant Firms		255.44		228.54
Observations by group:				
Incentivized Firms:				
Pre-AJA	197	197	162	162
AJA	562		508	
AJA - Build Out		90		81
AJA - Non-Compliant/Terminated		247		229
AJA - Fully Compliant		225		198
	759	759	670	670
Non-Incentivized Firms:	4,762,891	4,762,891	600,239	600,239
Total observations	4,763,650	4,763,650	600,909	600,909

Table 12 presents regression results for the impact of business incentives on employment. Equation 1 is estimated on stacked cohorts, where each cohort corresponds to a calendar year when the first incentive is awarded. Each incentivized firm is included in only one cohort, and each cohort includes observations from $t-3$ to $t+5$ around the event year. Regressions include firm-by-cohort and year-by-cohort fixed effects. Standard errors are clustered at the firm-by-cohort level and are shown in parentheses. All regressions use entropy balancing. In Columns (1), control firms are balanced on period $t-1$ employment and establishments. In Columns (2), control firms are balanced on period $t-1$ establishments and industry mix. ***, **, * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 13: Cost Per Job based on Total Promised Lump-Sum Incentives

Panel A: Grouped Deciles

	Full Sample (1)	Decile 1-3 (2)	Decile 4-8 (3)	Decile 9 (4)	Decile 10 (5)
Mean Incentive Amount Per Firm	$4,727,251	$139,046	$1,450,225	$7,329,257	$35,062,892
Incentive × Post	61.28	2.25	42.38	160.52	246.1
Cost per job	$77,141	$61,798	$34,220	$45,659	$142,474
Lower Bound	$55,875	$9,228	$24,670	$26,728	$95,150
Upper Bound	$124,545	-	$55,832	$156,540	$283,456

Panel B: Individual Deciles

	Full Sample (1)	Decile 1 (2)	Decile 2 (3)	Decile 3 (4)	Decile 4 (5)	Decile 5 (6)	Decile 6 (7)	Decile 7 (8)	Decile 8 (9)	Decile 9 (10)	Decile 10 (11)
Mean Incentive Amount per Firm	$4,727,251	$20,590	$202,342	$194,206	$929,016	$612,647	$860,172	$1,217,577	$3,611,229	$7,329,257	$35,062,892
Incentive × Post	61.28	-10.99	1.45	17.12	47.82	29.01	48.47	27.93	65.84	161.05	246.72
Cost per Job	$77,142	-	$140,515	$11,344	$19,427	$21,118	$17,746	$43,594	$54,849	$45,512	$142,116
Lower Bound	55,875	1,469	$8,459	$5,329	$10,727	$12,115	$7,869	$21,653	$38,439	$26,679	$94,985
Upper Bound	$124,546	-	-	-	$102,863	$82,234	-	-	$95,703	$154,756	$282,086

Table 13 reports cost per job estimates based on total promised lump-sum incentives. Panel A shows results for grouped deciles: Column (1) shows the full sample, Column (2) for the bottom 30% of incentives (deciles 1-3), Column (3) for the middle 50% (deciles 4-8), Column (4) for the 90th percentile, and Column (5) for the top 10% of incentives. Panel B shows results by individual deciles, with Column (1) showing the full sample and Columns (2)–(11) showing deciles from smallest to largest incentive packages. Cost per job equals the mean incentive amount per firm divided by the average employment gain per firm in each subgroup. A (-) indicates the cost-per-job estimate is negative and thus not interpretable.

Appendix A. Alabama Jobs Act summary

Updated: 03/25

ALABAMA JOBS ACT:
JOBS CREDIT AND INVESTMENT CREDIT

Sections 40-18-370 through 40-18-383 of the Code of Alabama 1975 provide for incentives to qualifying projects to stimulate job creation and capital investment through the Jobs Credit and Investment Credit. The incentives are discretionary based on various aspects of the project.

For more information regarding eligibility, qualifications, and procedures for a new or expansion project, please contact the Alabama Department of Commerce.

BENEFITS:

1. **Jobs Credit**
   - Cash rebate of up to 3% annually of the previous year's gross payroll (not including fringe benefits) for eligible employees who are Alabama residents. The Jobs Credit is available for up to 10 years. Enhanced benefits include:
     - Up to 4% rebate for qualifying projects locating in a Targeted[1] or Jumpstart[2] County, as defined and listed below.
     - Up to 4% rebate for qualifying projects by a Technology Company[3], as defined below.
     - Up to 4% for qualifying projects of companies engaged in pharmaceutical, biomedical, medical technology or medical supplies or related research and development activities.

     In addition to the maximum percentages provided above, an additional 0.5% rebate is available on the wages of the following:
     - Eligible employees who are veterans, for a company whose eligible workforce is at least 12% veterans.
     - All eligible employees, for qualifying projects locating within a former active duty military installation closed by the Base Realignment and Closure process (BRAC).

2. **Investment Credit**
   - Alabama tax credit of up to 1.5% annually of the qualified capital investment in a qualifying project for up to 10 years; or up to 15 years for Qualifying projects locating in a Targeted or Jumpstart County
   - The Investment Credit may be taken against Alabama income tax, financial institution excise tax, insurance premium tax, utility tax and/or utility license tax liability.
   - The credit can be carried forward for five years.
   - At the discretion of the Governor, the first 5 years of the Investment Credit may be transferred to another taxpayer for at least 85% of its face value.

ELIGIBILITY:

Project must have a qualifying business activity listed in Code of Alabama Section 40-18-372(1).

Job Creation Threshold:

Type of County/Project	Job Threshold Requirement
Non-Targeted / Non-Jumpstart County	50 net new full-time jobs
Targeted or Jumpstart County	10 net new full-time jobs
Technology Company	10 net new full-time jobs
Chemical Manufacturing, Data Center, Renewable Energy Generation, Engineering, Design, or Research, Metal/Machining Technology or Toolmaking	Net new full-time job(s)

Capital Investment Threshold:

Type of County/Project	Capital Investment Threshold Requirement
Non-Targeted / Non-Jumpstart County	None
Targeted or Jumpstart County	$2,000,000, direct or indirect

---

1. "**TARGETED COUNTY**" is defined as an Alabama County with a population of 60,000 or fewer.

2025 TARGETED COUNTIES					
Barbour	Clarke	Crenshaw	Hale	Marion	
Bibb	Clay	Dale	Henry	Monroe	Tallapoosa
Bullock	Cleburne	Dallas	Jackson	Perry	Washington
Butler	Coffee	Escambia	Lamar	Pickens	Wilcox
Chambers	Colbert	Fayette	Lawrence	Pike	Winston
Cherokee	Conecuh	Franklin	Lowndes	Randolph	
Chilton	Coosa	Geneva	Macon	Russell	
Choctaw	Covington	Greene	Marengo	Sumter	

2. "**JUMPSTART COUNTY**" is defined as any Alabama County that (1) does not qualify as a Targeted County, (2) has experienced negative population growth over the last 5 years, **and** (3) contains no more than two Opportunity Zones.

No county qualifies as a "Jumpstart County" in 2025.

3. "**TECHNOLOGY COMPANY**" is defined as any company that meets the following criteria:

- Alabama is or will become the company's headquarters; *and*
- Alabama is or will become the place of residence for the top 3 executives; *and*
- Alabama is or will become the place of residence for at least 75% of employees; *and*
- Company must earn at least 75% of its revenue from one of the following business activities:
    - NAICS Codes:
        - 5112 (Software Publishers)
        - 5121 (other than 51213) (Motion Picture & Video Industries (other than Motion Picture Video Exhibition))
        - 518 (Data Processing, Hosting, & Related Services)
        - 51913 (Internet Publishing & Broadcasting & Web Search Portals)
        - 5415 (Computer Systems Design & Related Services)
        - 5417 (Scientific Research & Development Services)

    - The use of technology to develop new coding or processes for the creation or delivery of goods or services in at least one of these fields:
        - Education
        - Nutrition
        - Aerospace
        - Automotive
        - Financial Services
        - STEM
        - Robotics
        - Healthcare
        - Energy
        - Agriculture
        - Infrastructure

    - Educating and mentoring early-stage Technology Companies recruited to Alabama, with the goal of accelerating their development and growth.