The Political Economy of State Economic Development Incentives: A Case of Rent Extraction*

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Abstract

There is a large literature examining the macroeconomic effects of state economic development incentives on employment, income, tax revenue, and growth. At best, these incentives are found to be weakly effective at job creation, but inefficient due to the distortions, secondary effects, and increased rent-seeking they encourage, with little public accountability. Given the evidence on their inefficiency, what explains their continued popularity? We find that large development incentives create substantial benefits for incumbent politicians in the form of both higher campaign contributions (particularly from business, labor, and construction sectors) and higher margins of victory at election time. Thus, political rent extraction may be the best explanation for the continued existence and popularity of these relatively ineffective incentive programs in states.

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1 Introduction

State and local governments spend more than $30 billion per year on economic development incentives intended to influence location, expansion, or job retention decisions of private sector firms (Bartik 2019).[1] These incentives take many forms, including job development and training tax credits, tax abatements, infrastructure financing, and even loans of public funds. Despite widespread use, the literature concludes that they do little, if anything, to promote meaningful improvements in economic outcomes. Even studies finding positive effects on job creation show the gains come at an economic cost of hundreds of thousands of dollars per job.[2]

To the extent there are welfare gains, they are captured by the recipient firms (Jensen and Malesky 2018) and construction workers who build the incentivized plants (Hicks and LaFaive 2011). These incentives result in significant effort and resources being devoted toward securing them by both the 'winning' and 'losing' firms (Coyne and Moberg 2014; Mitchell et al. 2018; Hicks and Shughart 2007; Felix and Hines 2013; Baumol 1990; Sobel 2008).[3] Estimates suggest an average of three firms compete for each incentive granted, with firms that devote more resources toward the political process being more likely to secure them.[4]

While economic development incentives may at worst seem to be a zero-sum transfer of firms between states, or between taxpayers and firms, they are socially negative-sum due to the resources wasted in securing them (Baumol 1990; Tullock 1967). Incentives also cause secondary distortions by leading to tax increases on other activities and reducing the likelihood states adopt structural tax reforms that would benefit all firms (Bartik 2019; Calcagno and Hefner 2018). Transparency is also a concern because states rarely perform follow-up assessments (Hinkley et al. 2000; Pew Charitable Trusts 2016).[5]

[1] Hicks and LaFaive (2011), Hicks and Shughart (2007), Fox and Murray (2004), Gabe and Kraybill (1998), Peters and Fisher (2004), Bingham and Bowen (1994), Bundrick and Snyder (2018), Mitchell et al. (2018), Bartik (2018) and Bartik (2019), Jensen and Malesky (2018), and Jensen (2018).

[2] The cost per job created for the incentive to Mercedes in Alabama, for example, was $192,730 (Calcagno and Hefner 2009), and $170,000 for Cabela's incentive in West Virginia (Hicks and Shughart 2007). In Michigan, Hicks and LaFaive (2011) find a cost of $123,000 per job created in construction, with 75% of these jobs lasting for one year or less.

[3] It is unknown whether the total value of these government provided 'rents' are fully dissipated through competitive expenditures. Regardless, resources devoted to capturing 'rents' are substantial (Payson 2020; Mateer and Lawson 1995; Wagner and Elder 2021; Ansolabehere et al. 2003; Sobel and Garrett 2002).

[4] For a three-year period in Ohio, Gabe and Kraybill (1998) find incentives received by 156 of 494 eligible firms. Aobdia et al. (2021) find that politically connected companies are roughly four times more likely to receive an incentive award, and when they do it is a significantly larger amount.

[5] Development incentives also have other, less visible, secondary effects. Dove and Sutter (2018) find a negative

With so much evidence against their continued use, why are these incentives so pervasive and growing in size and frequency? When government policies cannot be justified on grounds of economic efficiency, the culprit is likely to be political incentives (Buchanan and Tullock 1962; Weingast et al. 1981; McCormick and Tollison 1981).[6]

Economically inefficient policies may be enacted in cases of concentrated benefits and widespread (dispersed) costs because they increase political support for re-election seeking incumbents (Tullock et al. 2002; Shugart and McChesney 2010; Mueller 2003). State economic development incentives may be one such case. After all, firms that receive the incentives, employees of these firms (including unions that may represent them), and other firms eager to land an incentive package are all potential sources of political donations and support.

In this paper, we examine how the initial offering of 'large' development incentives in a state affects campaign contributions and electoral outcomes for politicians. We do this by utilizing a large database of individual state incentive awards and a difference-in-differences approach that compares states after their first 'large' incentive to states who have never awarded a 'large' incentive. Our results are robust to several definitions of 'large' incentive size, to controlling for leading effects, and to randomization inference techniques. Our preferred specifications define a state's first 'large' incentive as one that is 3500 times larger than their historical median incentive, although our results are similar for other thresholds.

Once a state begins offering 'large' incentives, we find that annual campaign contributions increase by approximately 38.4% (or $738,100) in the average state from construction and labor unions, 20.5% (or $158,600) from lobbyists and lawyers who represent large firms in the political process, and 106.8% (or $122,000) from large business advocacy and trade organizations. We also find that an average incumbent legislator is rewarded with a 7-percentage point increase in their

relationship between incentives and state economic freedom (Gwartney and Lawson 2003, and reductions in economic freedom can cause slower economic growth (Stansel and Tuszynski 2018; Hall and Lawson 2014, more severe economic crises (Bjørnskov 2016), smaller income shares for labor versus capital (Young and Lawson 2014, and even reductions in perceptions of individual control for state citizens (Nikolaev and Bennett 2016. Development incentives may also result in greater income inequality and corruption by increasing a state's reliance on industries that are subjected to higher federal regulations (Chambers and O'Reilly 2021; Dincer and Gunalp 2020).

[6]State rainy day funds, for example, are largely ineffective at preventing state fiscal stress during recessions, but they are in widespread use because they allow politicians to evade fiscal constraints such as balanced budget or tax and expenditure limit laws (Wagner and Sobel 2006). Similarly, Skidmore et al. (2013) find evidence that political considerations play a significant role in the adoption of state government subsidies/tax credits specifically for ethanol, while Young et al. (2013) find evidence that even IRS audit rates across congressional districts are influenced by political considerations.

margin of electoral victory.[7] This suggests that the battle over the future existence of economic development incentives may lie more in overcoming the political benefits that perpetuate their existence than in demonstrating a lack of worthwhile economic effects. We briefly review the literature on economic development incentives and then proceed to our empirical analysis.

2 Literature Review

It is widely recognized that the competitive incentive game merely reallocates economic activity between states (Burstein and Rolnick 1995; Calcagno and Thompson 2004). Some claim that benefits may not outweigh the costs even for the individual states involved (Mauey and Spiegel 1995; Ellis and Rogers 2000; Bartik 2002). The largest meta-analysis of the literature, conducted by Hicks and Shughart (2007), concludes that targeted development incentives have little measurable effect on economic outcomes. In addition, Bartik (2018) and Jensen and Malesky (2018) both argue that incentives are a small part of the decision-making process for firms and most would locate in these states without the use of incentives.

A few studies find small, positive partial-equilibrium effects. For instance, Goss and Phillips (1994) find economic development agency spending to be positively correlated with state employment growth, while Hoyt et al. (2009) find that job training incentives in Kentucky boosted employment in counties bordering other states. The promise to create jobs has also been linked to incentives (Gabe and Kraybill 2002), with scant evidence that firms fulfill those promises. To the extent that small employment gains occur, Bartik (1994) and Bartik (2019) finds the jobs are filled by new in-migrants rather than local unemployed workers, and that governments fail to target high-unemployment areas where incentives could potentially be most beneficial.[8]

The direct and indirect costs of these incentive programs are substantial. For instance, Wang (2015) finds that incentives crowd out net spending on productive public goods by $18.60 for every $100 spent. Bartik (2019) finds that the population growth associated with incentivized jobs

[7] The use of incentives to increase re-election chances is similar to Stratmann (2013)'s finding that federal pork-barrel earmark spending in congressional districts results in increases in vote shares for incumbents. For comparison, he finds that roughly a $10 million increase in earmarks leads a one percentage point increase in incumbent vote shares.

[8] In prior literature, development incentives' lack of effectiveness is sometimes blamed on deficiencies in the knowledge, skills, data, or information available to those designing, overseeing, or implementing the programs rather than the political incentives involved (Poole et al. 1999). For example, Hinkley et al. (2000) call for increased audits of incentive awards, while Bartik (2019) advocates for improved training for policymakers to prevent them from overestimating the benefits of targeted economic incentives. These arguments suffer from failing to recognize the knowledge problems in government central planning popularized by Hayek (1945).

increases government costs at least as much as it increases tax revenue. This in turn often leads to increased taxes on other economic activities to finance the infrastructure needs of incentivized firms. Finally, if incentives are used to compensate for uncompetitive tax structures (such as high taxes on capital equipment) and delay broad-based reforms that would benefit all firms in a state, they indirectly harm output and productivity (Calcagno and Hefner 2018; Thuronyi 1988).

Most prior literature ignores the political economy aspects of incentives and, therefore, omits the public-choice-related social costs that should be included in any cost-benefit analysis.[9] Exceptions include Coyne and Moberg (2014), Jansa and Gray (2017), Jensen (2018), and Aobdia et al. (2021), who argue that targeted-incentive policies lead to rent-seeking and a reallocation of entrepreneurial effort and resources from productive to unproductive uses.[10] Once a state begins offering large incentives to firms, existing firms may threaten to leave to extract similar economic incentives.[11] Some firms have even received multiple rounds of incentives through repeated threats to move (Morgan et al. 2013).

This behavior reflects Buchanan (1986)'s point that when policymakers begin offering targeted incentives, firms will invest resources in rent-seeking to capture and influence them–becoming subsidy entrepreneurs (Gustafsson et al. 2020). Large development incentives thus create a policy-induced rent-seeking contest (Clark and Riis 1996; Nitzan 1994) among these subsidy entrepreneurs. To a large extent the political behavior of firms that is induced by the presence of large development incentives typifies how an increased size of government and the associated poor institutional environment can induce cronyism (Holcombe 2013) and unproductive entrepreneurship (Baumol 1990). Perhaps more troubling is the mounting evidence of linkages between incentives and corruption of public officials (Glaeser et al. 2006; Felix and Hines 2013).

The effect of large development incentives on the political fortunes of elected officials who authorize them has received little attention. Incentive programs may allow political actors to improve their electoral chances by providing highly visible, concentrated benefits at the expense

[9]In addition to the papers discussed specifically, other papers that at least recognize the political economy aspects of incentives in passing are Bennett and DiLorenzo (1983), Dewar (1998), Wiewel (1999), Finkle (1999), Esinger (1989), Buss (1999a), Buss (1999b) and Buss (2001), and Bartik (2005).

[10]See Baumol (1990) and Sobel (2008) for a better understanding of the difference between productive and unproductive entrepreneurship, and how government policies may reallocate effort between them. In addition, some may refer to the environment created by incentives as cronyism, although rent-seeking may not be conceptually distinct from cronyism, see Klein et al. (2022).

[11]Pennsylvania awarded a firm that had been in the state for more than 70 years a $1 million incentive to remain in-state and move its headquarters from one county to another (Gannon and Belko (2015); Sheehan (2015)).

of difficult to identify, widespread costs.[12] Indeed, government actors themselves constitute an interest group that must be considered when evaluating public policy (McCormick and Tollison 1981). Recipient firms, employees of those firms, and other firms that compete unsuccessfully for incentives are all potential sources of political support and contributions. Considering that large incentives are frequently awarded to manufacturing firms, labor unions may also be a source of political support if they perceive those workforces as opportunities for expansion.

Despite being socially wasteful, incentives may exist for government agents to encourage and maximize rent-seeking for their benefit to win subsequent elections. The notion that governments structure policies to purposely increase (or maximize) rent-seeking is known as 'rent extraction' (McChesney 1987; McChesney 1997). Holcombe (1998) argues that discretionary programs, such as incentives, create more rent-seeking than broad-based policies. Having individual 'favors' to distribute generates additional campaign contributions that give incumbents an advantage over challengers.

3 Data and Identification Strategy

3.1 Defining 'Large' Incentives

We employ the comprehensive Subsidy Tracker database from Good Jobs First to identify economic development incentives awarded by states.[13] The database contains more than 230,000 incentive awards granted by state governments.[14]

The first time a state awarded an incentive valued at more than a billion dollars to a single firm was in 2003. This single award, valued at \$32.4 billion, was 670% larger than the previous record. There are now at least 16 additional single incentive awards to private companies valued

[12]See Hicks and Shughart (2007), Calcagno and Hefner (2007), Jensen et al. (2015), and Coyne and Moberg (2014).

[13]We thank Philip Mattera from Good Jobs First for providing us with access to their full database. A limited set of items are publicly available at no cost from https://www.goodjobsfirst.org/subsidy-tracker. The database includes all incentives in state public records, as well as additional incentives compiled from news and media accounts and official freedom of information requests. The values reported reflect the value of the incentive package, which as noted in the introduction might include a variety of economic development tools.

[14]The full Good Jobs First database contains 672,248 unique observations, where 43.1% are state-granted awards, 36.2% are federal-granted awards, and 20.7% are local-granted awards. Of the 289,611 state-granted awards, the state, year, and value of the incentive package are present in the 231,885 observations that serve as our sample. A closer inspection of observations with missing values revealed three potential states of concern. New Jersey and Washington accounted for more than half of all observations with unreported values. Colorado had one undated targeted incentive valued at \$14.5 million, which is 4200 times larger than the state's median award, that might potentially affect the state's treatment status or timing. As a robustness check, we excluded New Jersey, Washington, and Colorado from our regressions and the magnitude and statistical significance of the results are unchanged. These additional regressions are available upon request.

at \$1 billion or more. Prior to 2003, more than 99% of awards were less than \$15 million, and the median award was \$31,000. Collectively, states granted only a few hundred incentives annually in the mid-1990s, but this figure has grown rapidly, reaching a peak of almost 24,000 awards by 2013.[15] The aggregate value of awards across all states went from less than \$500 million per year in the mid-1990s to over \$20 billion per year by 2009. In two decades, state awards of these large ('megadeal') incentives went from non-existent to commonplace in about a third of the states.

If firms perceive the initial awarding of a 'megadeal' incentive as a shift in policy, then this could induce additional rent-seeking. Identifying the timing of when states began awarding 'large' incentives would be a simple matter if states were required to pass legislation that revealed a clear policy position shift. In the real-world however, defining the exact timing of a states first 'large' incentive is complicated by numerous factors.

Consider the competition for Amazon's second headquarters (HQ2) as a recent example. As Jensen (2019) notes, of the 26 cities whose bids were publicly released, only two states (New Jersey and Maryland) passed specific legislation to bolster their offers. All other existing (publicly-released) bids, each estimated to be in the billions, were authorized under an existing program or a combination of programs. In other words, cities and states were very "creative" in working within existing laws to offer Amazon these incredibly large incentives.

Moreover, elected officials often fail to disclose firms with whom they are negotiating or details of pending incentive offers. The argument for shielding the public is that it helps a state's competitiveness in attracting businesses. In Amazon's HQ2 bidding war, only 26 out of 238 total bids (about 11%) were publicly released, suggesting that most bidders believed it was in their best interest to keep details of their offer private (Jensen 2019).

Considering that 'large' incentives are often hidden from the public's eye until they are actually awarded, we employ a data-driven algorithm to identify "treated states." Our approach identifies if and when a state has ever experienced an "extraordinarily large" increase in the size of the incentives it awards relative to its own historical norm. Doing so would be a signal of a policy shift toward a willingness to begin offering significantly larger incentives. As a result we might expect

[15] Our empirical analysis uses the entire history of incentive awards for each state. Given that the increase in awards began around 2001, we also performed the analysis excluding all pre-2001 incentives. This had no meaningful effect on either the magnitude or statistical significance of our findings. These additional regressions are available upon request.

an increase in the political activities of incentive-seeking firms in the state.

The empirical dating algorithm is triggered if there is a single incentive awarded to one firm in state i in year t that is X times greater than what the state normally offers. Because there is arbitrariness in what constitutes an "extraordinarily large" jump, we use thresholds that are 2500, 3500, and 4500 times larger than the state's historical median incentive (expressed as a share of state GDP) to examine the sensitivity of our approach.[16]

[Table 1 about here]

For each threshold, Table 1 shows the initial year in which each state began awarding 'large' economic development incentives. If no year is reported, then the state never awarded an extraordinarily large incentive to one firm above the minimum threshold we consider (2500 times greater than its historical median incentive as a share of state GDP).

Consider Arizona. It never awarded a single incentive to one firm (in any year) that was 2500 times larger than its own historical median award value. Consequently, Arizona is not considered to be "treated" using this threshold (or higher thresholds) because the largest incentives the state has ever awarded never constituted an extraordinarily large jump from past behavior. Over the sample, Arizona's median incentive was \$96,800 (and its single largest incentive was \$1.25 million (13 times larger than the median).

In contrast, Pennsylvania gave a single incentive to one firm in 2012 (valued at \$1.65 billion) that did constitute an extraordinarily large jump from its past median award value. At the time of this award, Pennsylvania's median historical incentive was \$23,588. This one incentive was 69,900 times larger than the historical median, so Pennsylvania is "treated" in 2012 using every threshold because this single award easily surpasses every threshold we consider.

Based on the midpoint 3500X threshold that we consider, roughly one-third of states (17) began offering 'large' incentives in our sample period. These states constitute the "treatment group" in the empirical analysis. States like Arizona, which never awarded a single extraordinarily large incentive to date, form the "comparison group." To be clear, comparison group states do award incentives;

[16]State GDP normalizes for differences in the size of state economies and economic growth. A \$500 million incentive is a much larger share of state GDP in Arkansas than it is in California. It is worth explicitly noting that our algorithm does not depend on the size of a state's incentives relative to other states, only a state's own past incentives.

they are simply the subset of states that have never significantly altered the size of the incentives they award. In other words, comparison group states have never given a single "extraordinarily large" incentive to one firm that might signal a policy shift to other incentive-seeking firms. The level of incentive driven rent-seeking in these comparison group states, therefore, would not be expected to change.

For most "treated" states, like Pennsylvania, a single 'large' incentive is sufficient to be 2500, 3500, and 4500 times larger than their historical norm. In other states, like Arkansas, this is not the case. Arkansas awarded an $87.1 million dollar incentive to Lockheed Martin in 2015, which was more than 2500 times the state's historical median award but *less* than 3500 or 4500 times their historical median. Thus, based on the dating algorithm thresholds, Arkansas is part of the "treatment" group using the 2500X threshold and part of the comparison group using the 3500X and 4500X thresholds. Using several thresholds allows us to explore the sensitivity of our results.

It is worth examining if "treated states" continue to offer 'large' incentives after giving the initial large incentive. Doing so sheds light on whether the treatment dates in Table 1 signal the start of a permanent shift in policy, which one would also expect to be related to how strongly incentive-seeking firms respond. If incentive-seeking firms perceive an announcement of an extraordinary large award to be a one-time event, they may not alter their rent-seeking behavior. However, if firms believe the first 'large' incentive signals a policy shift to larger or more numerous future incentives (or both), they would be expected to devote additional resources to pursuing these larger 'rents' that have now become available.

If the initial large incentive does indeed represent a policy shift, then a state's largest (maximum) incentives in future years (after the initial large incentive) should continue to surpass the largest incentives made before the initial large incentive was awarded.

[Table 2 about here]

Using treatment dates from the 3500X threshold, we compare the largest annual incentives granted in each state before and after the initial large incentive. This information is displayed in Table 2. In Mississippi, for example, the average of the largest annual post-treatment incentives (2011-2016) is $110.9 million (or 892.5%) larger than the average of the state's largest annual pre-treatment incentives (pre-2010). Moreover, 78% of Mississippi's largest annual post-treatment incentives were larger than the single largest award the state granted in the pre-treatment period. While the figures differ by state, they all indicate a significant jump in the provision of large incentives after the first extraordinarily large incentive was awarded.[17]

Relative to the average of the largest annual pre-treatment incentives, the average maximum annual incentives increase (after the initial large incentive) by almost $170 million (624%) in the typical state. These data suggest that the treatment dates represent the onset of permanent changes in the willingness of states to offer significantly larger incentives, which we argue could potentially induce significant changes in the amount of rent seeking and political contributions.

Additional evidence that the timing of the first "extraordinary large" award signals a policy shift is visible in Figure 1. Averaging across treated states, Panel A shows the fraction of post-treatment incentive awards that exceed a state's 90th percentile award in the pre-treatment period. The results show a clear upward trend through the first 6 post-treatment period before flattening (around the Great Recession). Six years after the first 'large' incentive, almost 40% of a state's incentives are *larger* than the 90th percentile award made before the first large award. Incentives are measured as a share of state GDP so differences in economic climates have been taken into consideration.

[Figure 1 about here]

Another striking feature worth noting is illustrated in Panel B of Figure 1. This panel shows the aggregate number of incentive awards granted by "treated" states. In the four years prior to granting their first 'large' incentive, these states collectively awarded an average of 5,076 incentives annually. In the first 8 years after granting the first 'large' incentive, these same states gave an annual average of 9,575 awards, an increase of more than 88%! When Panels A and B are considered together, not only are states giving more incentive awards after their first "extraordinary large" incentive, approximately one-third of those awards are larger than their 90th percentile pre-treatment award. In absolute numbers, the typical state is giving more *and larger* incentives after their initial 'large' award.[18]

[17] Replicating Table 2 using the 2500X or 4500X thresholds supports the permanent shift toward awarding larger incentives. Using the 2500X threshold, the average state's largest annual post-treatment incentives are $154.5 million (or 600.3%) larger than the largest annual pre-treatment incentives. For the 4500X thresholds, the average differential grows to $207.9 million (or 734.0%). In each case, and in Table 2, only states with at least four years of pre-treatment and post-treatment data are included.

[18] As an additional test, we examined the equality of the full *distribution* of pre- and post-treatment incentive

A related issue worth discussing is the direction of causality between political contributions and political favors (incentives). This is an area of debate in the public choice literature. Political contributions may be given in advance to secure a favorable policy, or as a reward to politicians who provide favors after the fact (e.g., a politician may call on a firm they provided favors to in subsequent campaigns). We stand agnostic on this issue, as either possibility lends support to our hypothesis that incentive programs create political benefits for the policymakers who offer them.

From an empirical perspective, however, one might be concerned if an increase in political activities caused the initial large incentive to be awarded. We test for, and reject, the presence of anticipation (or pre-treatment) effects in every political outcome we explore (discussed in detail in Section 3.3). Since this includes campaign contributions, the lack of anticipation effects suggests that there were no meaningful differences in contributions between the treated and comparison states prior to the year of the initial large incentive awards. Interestingly, while the presence of a significant difference would signal bias and other concerns, it would also indirectly lend support to our hypothesis that political gains do indeed occur.

[Figure 2 about here]

To illustrate our logic that large incentives and perceived shifts in incentive-giving behavior may create political benefits for politicians, we show raw contribution data in Figure 2 (these data are described in detail in Section 3.3). Michigan awarded its first extraordinarily large state incentive to Ford Motor Company in 2010. Panel A shows Ford's contributions in Michigan over time, with a large spike occurring in 2014. More importantly, once Michigan signaled its willingness to give these large incentives to auto manufacturers, contributions from other auto manufacturing firms (excluding Ford) and transportation unions also increased. Their increased giving is depicted in Panel B, with large spikes in 2012, 2014, and 2016 relative to past contribution amounts.

A similar data pattern occurs in Oregon and Missouri following their 2005 and 2014 large incentives to Intel and Boeing. While these are anecdotal cases, they illustrate what we are attempting to capture in our models – whether contributions and other political variables change in a state after it begins offering these 'large' incentives as many other firms seek to secure them.

awards using a Kolmogorov-Smirnov test. Using the 3500X threshold definition for treated states, we find that the post-treatment distributions of awards are significantly different than the pre-treatment distribution in 15 of the 17 treated states. This is further evidence of a behavioral shift in incentive-giving.

3.2 Empirical Specification

Given a sample of states that have begun to award significantly larger incentives and those that have not, we can estimate the effect on a desired measure of a state's political activity ($P_{i,t}$) using a difference-in-differences strategy. With panel data, such a model may be expressed as:

$$P_{i,t} = \alpha + \beta \, post_{i,t} + \delta \, X_{i,t} + \psi_i + \theta_t + \varepsilon_{i,t} \tag{1}$$

where $X_{i,t}$ is a vector of time-varying control variables, $\varepsilon_{i,t}$ is the error term, and ψ_i and θ_t are state- and year-specific fixed effects. $P_{i,t}$ denotes the political activity outcome of interest in state i at time t, either a measure of electoral support or campaign donations (described in detail in Section 3.3). The year-specific fixed effects adjust for shocks common to all states in a given year, while the state-specific fixed effects adjust for any unobserved time-invariant factors unique to each state. The difference-in-differences variable of interest, $post_{i,t}$, is equal to unity in the treatment period for the treated states and zero otherwise. For the treated states that began offering large incentives, $post_{i,t}$ is equal to unity in the first year an extraordinarily large incentive was granted and in all subsequent years.[19]

Given the staggered timing of states' awarding their first extraordinarily large incentives, the difference-in-differences parameter of interest (β) will be biased if equation (1) is estimated using two-way fixed effects if treatment dynamics or heterogeneity across treatment groups are present (Goodman-Bacon 2021). This occurs because states treated 'early' in the sample end up serving as comparison units for states treated later in the sample. To mitigate this source of bias, we use the stacked regression difference-in-differences estimator proposed by Cengiz et al. (2019). As Baker et al. (2022) show, the stacked estimator yields an unbiased estimate of the average treatment effect in the presence of treatment heterogeneity or dynamics.

The idea underlying the stacked regression approach is the formation of a comparison group for each treatment group cohort so that treated states do not later contaminate the comparison group. Each treatment group cohort's dataset is then re-centered, or converted to relative time, so that treatment dates align across cohorts. If the treatment date for any treated cohort g is re-defined

[19] We seek to uncover how within-state changes in incentives affect changes in political activity. Since the comparison group states never awarded a single incentive that was extraordinarily large relative to their historical norm, we would not expect any within-state changes in political activity.

as period 0, then cohort g's dataset uses treated and comparison observations from period t-6 (to ensure at least one election cycle) up to a maximum of t+13, depending on the timing of the first large incentive.

Using the 3500X threshold from Table 1, there are 17 treated states awarding their first large incentive in nine distinct years. This means we must create a total of nine cohort datasets. The final dataset is formed by "stacking" (row concatenating) the individual cohort datasets to ensure that every treated cohort is treated in the same relative time period (period 0). As Cengiz et al. (2019) note, an important factor in the stacked difference-in-differences design is the need to fully saturate the state- and year-fixed effects for each cohort dataset.

Within the difference-in-differences framework, the main identifying assumption (which is inherently untestable) is that the treated and comparison groups follow parallel trends post treatment.This allows the comparison group to represent the unobserved potential outcome in the treatment group had the treated states not dramatically increased the size of their incentives.

An event study is one of the most common approaches for providing evidence in favor of parallel trends holding in the pre-treatment sample. Given the large number of political outcome variables we explore (discussed in Section 3.3) and the fact that we use three different treatment dating algorithm thresholds, event studies are not feasible. As an alternative, we pursue two approaches to lend support to the parallel trend assumption holding in the pre-treatment period. The first, popularized by Autor (2003), includes 'lead' terms for the treated states in the pre-treatment period to test for anticipatory effects. Formally, this model can be expressed as:

$$P_{i,t} = \alpha + \beta \; post_{i,t} + \delta \; X_{i,t} + \psi_i + \theta_t + \sum_{t=-m}^{-M} \varphi^t \; pre_{i,t} + \varepsilon_{i,t} \tag{2}$$

where $pre_{i,t}$ is an indicator variable that equals unity for the treated states in the pre-treatment period. Four pre-treatment lead terms are included to capture the maximum length of any state's election cycle. If one fails to reject the null hypothesis that the lead terms jointly equal zero, then this suggests that there were no meaningful differences between political outcomes in the treated and comparison group states in the years just prior to states' awarding their first large incentive (conditional, of course, on the fixed effects and covariates).

Alternatively, studies such as Muralidharan and Prakash (2017) and Antwi et al. (2013) propose testing for the validity of the parallel trends assumption by explicitly allowing for differential pre-treatment trends for treated units. This formulation is given by:

$$P_{i,t} = \alpha + \beta \, post_{i,t} + \delta \, X_{i,t} + \psi_i + \theta_t + \omega_1 \, (pre_{i,t} \cdot T) + \omega_2 \, (pre_{i,t} \cdot T^2) + \varepsilon_{i,t} \tag{3}$$

where $pre_{i,t}$ is an indicator variable that equals unity for the treated states in the pre-treatment period and T is a linear trend. If the treated and comparison groups share *identical* pre-treatment trends, then $\omega_1 = \omega_2 = 0$ and equation (3) reduces to equation (1). If the pre-treatment trends do not differ, conditional on the covariates and fixed effects, then we have more confidence that the comparison states are a valid counterfactual for the unobserved potential outcomes of the treated states. For each equation we estimate, we provide the results of both parallel-trends tests.

3.3 Measures of Political Behavior and Other Control Variables

Our political variables of interest ($P_{i,t}$) include campaign contributions from the large businesses competing for the incentives, plus associated groups such as construction firms and labor unions. We also investigate several electoral outcomes for members of the state legislature. State-level campaign contribution data are derived from individual campaign finance reports collected and compiled by the National Institute on Money in Politics (NIMP) from 2000 to 2016.[20]

In addition to the dollar contribution, NIMP classifies donors as individuals or organizations. Organizations are further stratified into broad sectors. Recipients of large incentives tend to be large entities such as Boeing, Ford, and Amazon (Jansa and Gray 2017). We, therefore, expect possible increases in contributions from lawyers and professional lobbyists who might represent these firms in the political process, labor unions who may view the firms as opportunities for expansion, business advocacy organizations such as chambers of commerce and trade associations, and the construction industry that would help to build a new facility. Prior research has shown that expanded incentives correlate with a temporary increase in construction jobs (Hicks and LaFaive 2011).

In addition to contributions, we explore how the electoral prospects of members of the legislature are affected once states begin offering large incentives. If voters perceive these incentives as leading

[20] We thank Pete Quist from the National Institute on Money in Politics for providing us with access to their full database. Because our campaign contribution data (described in Section 3.3) are only available from 2000-2016, we must omit cases if the treatment date occurred before 2001 or after 2016 under that specific threshold. This only occurred in one case, Colorado, using the lowest 2500X Median threshold.

to additional jobs and income, they might reward incumbent politicians at election time.

We investigate several measures of electoral success. The first is the re-election rate of incumbents, defined as the percentage of all lower and upper house incumbents re-elected in state i at time t. Next, using all lower and upper house seats up for election in state i at time t, we investigate the median margin of victory across all seats, the median margin of victory in races with incumbents, and the median margin of victory in races without an incumbent.[21]

Because only an incumbent can claim credit for the incentive awards, the winning margins in races excluding incumbents serves as a falsification outcome test. We would not expect to see any changes in the winning margins in these races.[22] Descriptive statistics for all variables are provided in Appendix A for our preferred 3500X midpoint threshold.[23]

We rely on prior literature for our independent variables. To adjust for differences in economic conditions and other policies, we include per capita state GDP (in thousands), the index of state economic freedom, the state's unemployment rate, share of population age 65 and older, and an indicator variable for gubernatorial election years.

To control for electoral competitiveness, we include measures of electoral competitiveness for the lower and upper houses.[24] To adjust for partisanship and political engagement, we include an indicator variable for divided party control of the legislature and the differential in ideology scores between the state's elected officials and citizens (Berry et al. 1998). Finally, we include each state's incentive environment index developed by Patrick (2014). This index measures the constitutional provisions in each state regarding the granting of public monies, credit, and property to private firms. This will control for differences across states in the ease with which state governments can offer incentives to private firms.[25]

[21] The margin of victory is defined as the difference in vote share versus the second-place candidate.

[22] There are some ways in which non-incumbents may benefit, including referendum effects (Atkeson and Partin 1995), and the possibility that voters could reward a member of the incumbent's party, particularly if the incumbent campaigns for them. However, empirical evidence on the latter has not been strong in the literature, including (Bennett and Long 2019), who provide evidence that policy changes do not affect the election prospects of the incumbent governor's party in the next election, suggesting that voters at the state level do attribute policy to the sitting politicians, not necessarily their party.

[23] Descriptive statistics for other thresholds are available upon request, with no significant differences of note.

[24] We calculate the inverse of the Herfandahl-Hirschman Index (HHI) of vote shares for every seat in both chambers. If three candidates receive an equal vote share for a given legislative seat, the inverse HHI has a value of 0.00306. Our measures of electoral competitiveness in each chamber are the median value of the inverse HHI across all seats. Larger values indicate that the median race is more competitive.

[25] Higher values of the index correspond to a state government having more freedom to grant public assistance to private firms through credit clauses, current appropriation clauses, and stock clauses.

4 Empirical Results

Our main specifications involve estimating 66 different regression equations. Complete results, including all diagnostic tests, are in Appendix A. Table 3 presents a summary of our results in a manner that is easier to compare across specifications. Each shaded row shows six difference-in-differences point estimates (from distinct regressions) for a political outcome of interest.[26]

Columns (1), (2), and (3) report point estimates for the 2500X, 3500X, and 4500X thresholds excluding additional control variables. Columns (4), (5), and (6) report the same point estimates, by threshold, when all of the control variables described in the previous section are included.[27]

[Table 3 about here]

Below each coefficient estimate, we report two p-values. The first, shown in normal parentheses, is the conventional cluster-robust p-value when standard errors are clustered by state. The second p-value, denoted $RI-t$ and shown in curly brackets, is a randomization inference p-value following MacKinnon and Webb (2020). The randomization inference p-value is a useful robustness check when the number of treated states is small (less than 40) because conventional standard errors can be misleading (MacKinnon and Webb 2020).[28]

Across all outcomes, we find no meaningful differences between the magnitude or statistical significance in the regressions with and without the control variables. The coefficient estimates are also stable across the three threshold definitions of what constitutes the first extraordinarily large incentive in each state.

Our most consistent results show that contributions increase significantly following the first large development incentive in the 'construction and labor unions', 'lobbyists and lawyers', and 'business advocacy' categories. This is what we anticipated because the large firms receiving these

[26] Alaska and Hawaii must be dropped from the comparison group in the specifications that include covariates because Patrick (2014)'s incentive environment index is not available for those states.

[27] We also explored lower thresholds, such as 500x and 1000x, and our baseline results are robust to these alternatives. As the incentive threshold is lowered, more states move from the "comparison" group to the "treatment" group which also reduces statistical power.

[28] The $RI-t$ p-value, shown in curly brackets, is the proportion of re-randomization t-statistics for the parameter of interest that are greater in absolute value than the conventional cluster-robust t-statistic. MacKinnon and Webb (2020) note that it is always more conservative than a traditional cluster-robust t-statistic. To illustrate the methodology, suppose 19 and 31 states are in the treatment and comparison groups, respectively. We randomly select 19 states (without replacement) to be treated and then randomly assign them an observed treatment date. The stacked dataset is constructed so every treated state is treated in the same relative period. Every regression is re-run with the randomized dataset, retaining the t-statistic of interest. The process is repeated 100,000 times to form distributions to which the baseline estimated parameters are compared.

incentives are usually represented by lawyers and lobbyists in the political process, create easier-to-organize workforces for labor unions, and often require new or expanded facilities that boost construction jobs.

For construction and organized labor, we find an increase in contributions of \$0.097 to \$0.158 per capita (depending on the model). The 3500X midpoint threshold produces an estimate of \$0.118 with no covariates, and \$0.121 with covariates included. The effect is significant in 8 of the 12 possible p-values. Based on the mean contributions from construction and organized labor in the treated states (\$0.315), our estimates imply an increase between 31% and 50% once a state begins offering incentives significantly larger than in the past. With an average population of 6.1 million, the 3500X threshold point estimate including covariates (0.121) suggests that annual contributions increase by 38.4% or \$738,100 from the construction and organized labor sector once large incentives start being offered.

We find similar results for lobbyists and lawyers. The estimated magnitude is tightly clustered across incentive thresholds at roughly \$0.02 per capita and significant based on nine of the 18 p-values. For the midpoint 3500X threshold, the estimate is \$0.020 without covariates, and \$0.026 with covariates included, with all four p-values being significant. Based on the mean contributions from lobbyists and lawyers in the treated states (\$0.127), the 3500X threshold point estimate (0.026) suggests that contributions increase approximately 20.5% once a state begins offering incentives significantly larger than in the past. In dollar terms, this equates to an annual increase in contributions of \$158,600 for the average state.

Consistent with the theory, the results also indicate that contributions from business advocacy organizations increase significantly following the first large incentive award. Across all specifications, these point estimates are clustered tightly around \$0.02 and are significant using all 12 p-values. Using the mean contributions from the treated states for this sector (\$0.02), these estimates suggest that business advocacy contributions increase 106.8% (or \$122,000) for the average state. Although the percentage response is sizable, contributions from business advocacy groups represent less than 2% of total contributions from organizations.

The point estimates for total organizational contributions range between \$0.135 and \$0.294 per capita (\$0.135 and \$0.175 for our 3500X threshold). This is a broad category reflecting all contributions (including finance, energy, and large manufacturing, etc.). We are able to reject the null hypothesis that the coefficients equal zero using 11 of the 12 p-values. Based on the mean total organizational contributions in the treated states ($1.274), the 3500X threshold point estimate (0.175) suggests that contributions increase approximately 13.7% once a state begins offering incentives significantly larger than in the past. In dollar terms, this equates to an annual increase in contributions of $1,067,500 for the average state.

Using the 3500X threshold with control variables (column 5), adding the point estimates from construction and organized labor, business advocacy organizations, and lobbyists, and lawyers yields $0.167. From these three sectors alone, our estimates suggest annual campaign funding increases by $1,018,700 in the typical state once it begins offering large incentives. Recall that the average state's maximum annual incentive increases by $168.8 million following the initial large incentive (Table 2). A back-of-the-envelope calculation implies that every $166 in additional economic development incentives leads to one additional dollar of campaign contributions from these three sectors.

Focusing on electoral activity, we find robust evidence of a boost for incumbents across all incentive thresholds. The median incumbent margin of victory expands by approximately 7% percentage points, and as expected, we find no evidence that margins of victory change in non-incumbent races, suggesting our results are not spurious.

We find only one instance of a significantly different pre-treatment trend (p-value of 0.05 or less) from the 66 regressions estimated to create Table 3. This violation occurred in Model 4 of the incumbent re-election rate (Appendix Table A.5). Similarly, we find only one instance in which the lead terms for the treated states are significantly different from the comparison states. This violation occurred in Model 4 of the total organization contributions (Appendix Table A.6). Overall, the diagnostic tests suggest the comparison group states provide a suitable counterfactual for the treatment states. The p-values for all diagnostic tests and outcome variables are reported in the individual regression tables in the Appendix for readers interested in the results for any specific model or threshold.[29]

[29]Since states awarded their first large incentives at different points in time, it is possible to expand the comparison group to also include states that are not-yet-treated to the comparison group. For example, if state A awarded its first large incentive in 2010 and state B awarded its first large incentive in 2015, then state B is potentially a valid comparison for state A from 2010 through 2014. We explored the robustness of our results using an alternative comparison group that also includes states that awarded their first large incentive two years or more in the future. Our baseline results and conclusions are unchanged when using this expanded comparison group. These additional robustness checks are available upon request.

5 Conclusion

Starting in the early 2000s, state governments began spending billions of dollars per year on targeted economic development incentives intended to affect firms' decisions about location, expansion, or job retention. Depending upon the definition employed, roughly one-third of states now routinely offer incentives to a single firm that can range in the billions. The published literature questions the economic efficiency and effectiveness of these programs and suggests that any benefits are captured by recipient firms involved in a case of concentrated benefits and widespread costs.

Potential beneficiary firms and associated groups are willing to spend significant resources to compete for, and capture, these incentives. This could provide significant electoral benefits for incumbents who design and fund the continued existence of these programs. The literature on 'rent extraction' suggests that politicians often design and implement programs precisely to maximize the amount of rent-seeking political support directed at them for their use in upcoming election campaigns. Economic development incentives may be one such case.

We examine individual state incentive data to identify the first year that a state awarded a single incentive to one firm that was an extraordinarily large jump from their historical norms. Identifying three thresholds for measuring the large jumps in development incentives, we then use a difference-in-differences estimation strategy to estimate how political support in these states changes relative to states that never awarded a single extraordinarily large incentive to one firm. Our results are robust to all our threshold measures of large incentives, suggesting that states that begin offering these large incentive packages are able to extract additional rents from the policy shift.

Once a state begins offering substantially larger development incentives, our results show that total organizational campaign contributions increase by approximately $1,067,500 in the average state. The gains come from construction and organized labor, business advocacy groups, and lobbyists and lawyers. These sectors either stand to directly benefit from the awards or represent firms in the political process. We also find a sizable electoral benefit for incumbent politicians; their median margin of victory increases by 7 percentage points after the large economic development incentives become commonplace.

This paper extends the literature on state economic development incentives by focusing on the political ramifications of these programs rather than the macroeconomic effects. While studies sometimes allude to these political economy aspects, empirical evidence has been lacking. Given the weak justification for incentives on efficiency grounds, our results provide an alternative explanation for why states continue to use, and even expand, these programs. Simply put, incumbent politicians benefit from the enhanced political support that flows from the concentrated benefits they create. Therefore, the push to end large economic development incentives may not lie in finding additional evidence of their economic ineffectiveness, but rather in finding ways to overcome the concentrated benefits to the incumbent politicians who award these incentives.

Our paper also provides empirical support for the idea that an increased size of government transfers and involvement in economic affairs, and the associated reduction in economic freedom and poor institutional environment it creates, induces higher levels of cronyism (Holcombe 2013) and unproductive entrepreneurship (Baumol 1990; Sobel 2008).

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Table 1: Year of First 'Large' Economic Development Incentive by Threshold

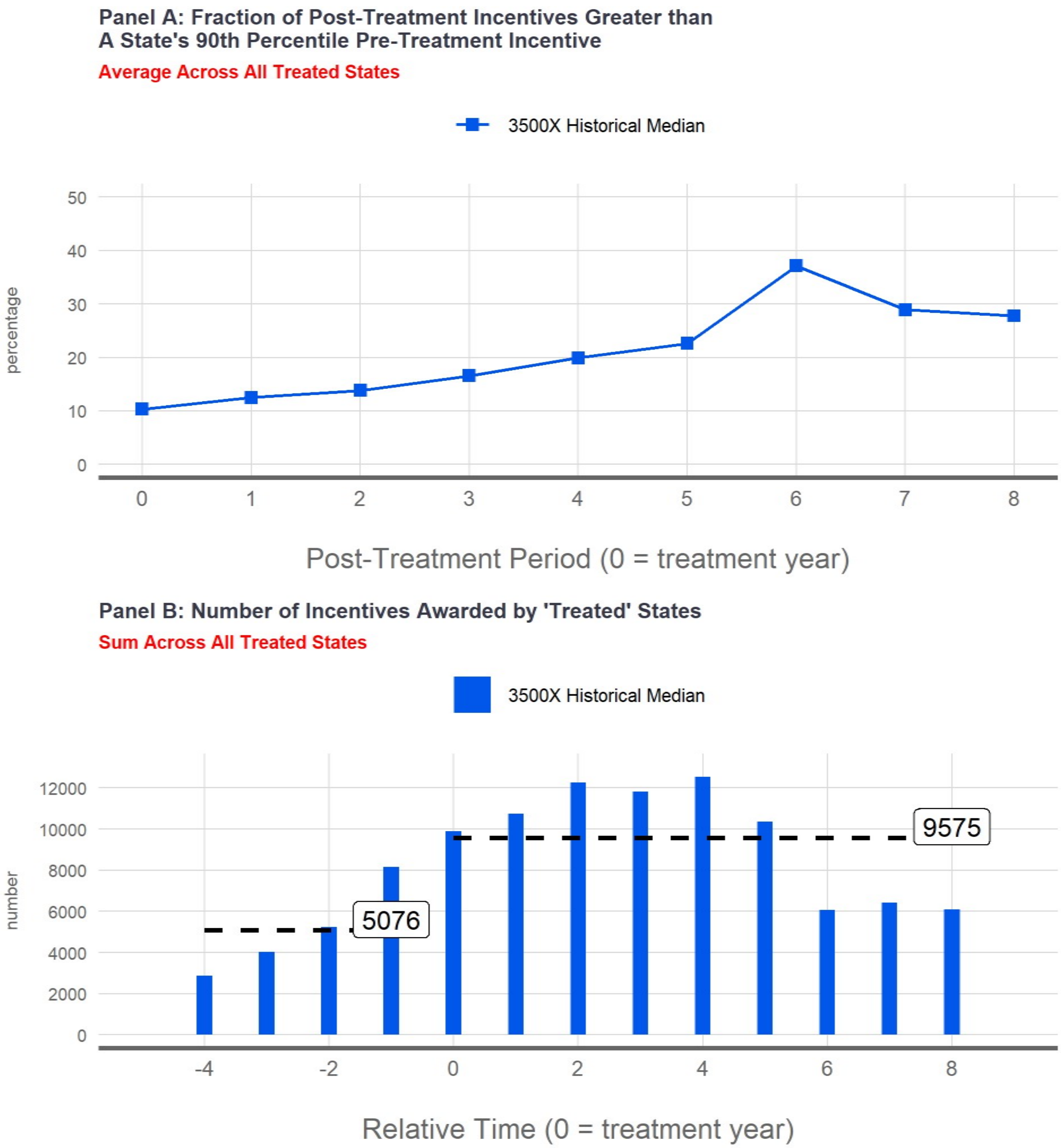
	"Treatment" Threshold		
	2500X	3500X	4500X
Alabama			
Alaska			
Arizona			
Arkansas	2015		
California	2014	2014	2014
Colorado	2016		
Connecticut	2014	2014	2014
Delaware			
Florida			
Georgia			
Hawaii			
Idaho			
Illinois			
Indiana			
Iowa			
Kansas	2009	2009	
Kentucky			
Louisiana	2011	2011	2011
Maine			
Maryland			
Massachusetts			
Michigan	2010	2010	2010
Minnesota			
Mississippi	2009	2010	2010
Missouri	2010	2010	2014
Montana			
Nebraska			
Nevada	2015	2015	2015
New Hampshire			
New Jersey			
New Mexico			
New York	2002	2002	2002
North Carolina			
North Dakota			
Ohio			
Oklahoma	2007	2012	2013
Oregon	2005	2005	2005
Pennsylvania	2012	2012	2012
Rhode Island	2010	2010	
South Carolina			
South Dakota			
Tennessee			
Texas	2012	2012	2012
Utah	2009	2009	2009
Vermont			
Virginia			
Washington	2006	2006	2006
West Virginia	2012	2012	
Wisconsin			
Wyoming			
Number of states	**19**	**17**	**14**

Notes: Each column shows the first year that a state awarded an economic development incentive that was X times larger than their historical median incentive. For example, the column 2500X is the first year a state awarded a single incentive to one firm that was 2500 times larger (or more) than their historical median award (as a share of state GDP). A blank cell indicates that the state never awarded a single incentive exceeding the threshold. The value of incentives is from the Subsidy Tracker database maintained by Good Jobs First.

Table 2: Changes in Largest Annual Incentives Awarded After Threshold Year

State	3500X Threshold Year	Change in Annual Average Maximum Incentive, Pre-Post ($ Change, (in millions)	(% Change)	Percent of Years After Treatment in Which Maximum Incentive Exceeded Maximum Prior Incentive
California	2014	$74.5	1743.4%	100.0%
Connecticut	2014	$49.0	68.1%	16.7%
Kansas	2009	$6.9	33.9%	14.3%
Louisiana	2011	$960.4	1232.6%	83.3%
Michigan	2010	$97.4	27.9%	20.0%
Missouri	2010	$37.4	347.7%	30.0%
Mississippi	2010	$110.9	892.5%	77.8%
Nevada	2015	$179.5	3112.3%	25.0%
New York	2002	$482.3	776.3%	41.2%
Oklahoma	2012	$69.4	146.1%	100.0%
Oregon	2005	$248.1	245.8%	21.4%
Pennsylvania	2012	$166.7	305.3%	12.5%
Rhode Island	2010	$6.1	48.4%	44.4%
Texas	2012	$27.1	122.1%	12.5%
West Virginia	2012	$15.6	249.9%	20.0%
Average		**$168.8**	**623.5%**	**41.3%**

Figure 1: Shifts in Incentive Awards Among 'Treated' States



Note: Authors' calculations using data from Good Jobs First. Averaging across states, Panel A shows the fraction of incentives each post-treatment year that exceeds a state's 90th percentile pre-treatment incentive. Summing across states, Panel B shows that 'treated' states (collectively) awarded an annual average of 5,076 incentives in the four years prior to the first extraordinarily large incentive award. The dashed black lines indicate the aggregate average number of pre- and post treatment incentives awarded by treated states. After the first extraordinarily large incentive award, treated states collectively awarded an average 9,575 awards. Panels A and B seem to suggest that treated states began offering more *and* larger incentives after their first extraordinarily large incentive award than they did in the years preceding these awards.

Figure 2: Campaign Contribution in Michigan after Ford's 2010 Incentive

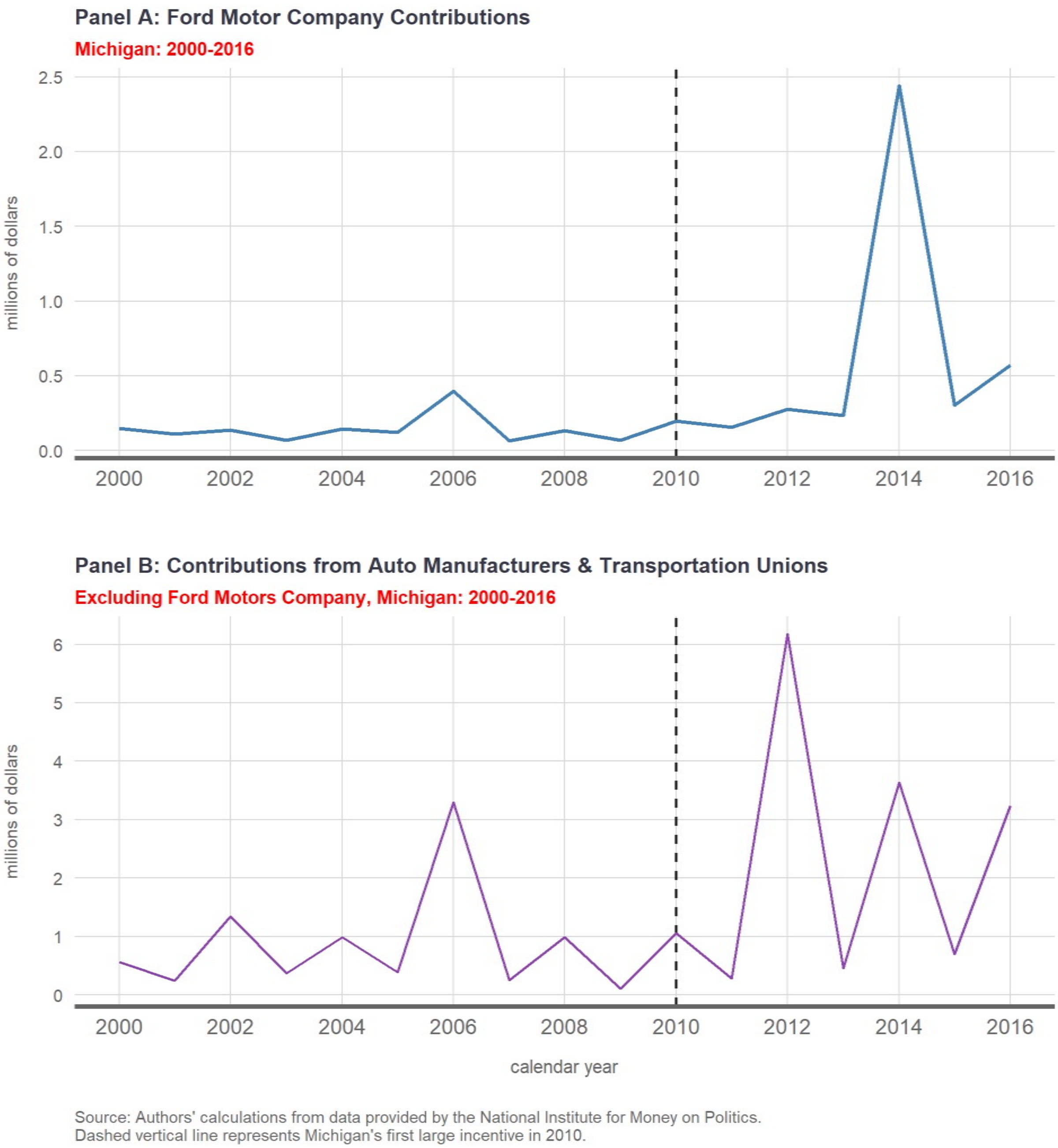


Table 3: Difference-in-Differences Coefficient Estimates of Interest and Randomization Inference p-values

	No Covariates in Regressions			Covariates Included in Regressions		
	(1)	(2)	(3)	(4)	(5)	(6)
Political Outcome	2500X Threshold	3500X Threshold	4500X Threshold	2500X Threshold	3500X Threshold	4500X Threshold
median margin of victory: all races	5.041	5.172	5.679	5.614	5.556	5.222
cluster-robust p-value	(0.006)***	(0.008)***	(0.039)**	(0.011)**	(0.022)**	(0.081)*
$RI-t$ p-value	{0.019}**	{0.022}**	{0.076}*	{0.040}**	{0.065}*	{0.163}
median margin of victory: incumbents	6.535	6.942	6.870	6.731	7.203	6.174
cluster-robust p-value	(0.013)**	(0.013)**	(0.005)***	(0.023)**	(0.027)**	(0.026)**
$RI-t$ p-value	{0.062}*	{0.064}*	{0.031}**	{0.106}	{0.121}	{0.101}
median margin of victory: non-incumbents	-0.012	-0.198	0.380	-0.418	-0.644	-0.939
cluster-robust p-value	(0.997)	(0.935)	(0.906)	(0.887)	(0.821)	(0.774)
$RI-t$ p-value	{0.997}	{0.934}	{0.906}	{0.897}	{0.832}	{0.790}
incumbent re-election rate	-0.750	-0.402	0.347	-0.373	0.058	0.007
cluster-robust p-value	(0.194)	(0.536)	(0.690)	(0.588)	(0.941)	(0.995)
$RI-t$ p-value	{0.381}	{0.672}	{0.767}	{0.719}	{0.960}	{0.997}
total organization contributions	0.234	0.135	0.226	0.294	0.175	0.279
cluster-robust p-value	(0.038)**	(0.062)*	(0.090)*	(0.007)***	(0.022)**	(0.051)*
$RI-t$ p-value	{0.033}**	{0.061}*	{0.102}	{0.003}***	{0.011}**	{0.034}**
energy and large manufacturer contributions	0.024	-0.062	-0.056	0.074	-0.005	0.008
cluster-robust p-value	(0.769)	(0.143)	(0.311)	(0.320)	(0.889)	(0.863)
$RI-t$ p-value	{0.915}	{0.377}	{0.621}	{0.538}	{0.932}	{0.929}
finance contributions	0.007	0.004	0.003	0.008	0.003	0.002
cluster-robust p-value	(0.071)*	(0.356)	(0.470)	(0.039)**	(0.497)	(0.646)
$RI-t$ p-value	{0.114}	{0.350}	{0.501}	{0.083}*	{0.520}	{0.677}
labor and construction contributions	0.097	0.118	0.142	0.100	0.121	0.158
cluster-robust p-value	(0.126)	(0.021)**	(0.013)**	(0.091)*	(0.019)**	(0.009)***
$RI-t$ p-value	{0.297}	{0.106}	{0.059}*	{0.245}	{0.100}*	{0.052}*
business advocacy contributions	0.019	0.020	0.022	0.018	0.020	0.026
cluster-robust p-value	(0.001)***	(0.000)***	(0.003)***	(0.002)***	(0.000)***	(0.011)**
$RI-t$ p-value	{0.010}***	{0.000}***	{0.004}***	{0.024}**	{0.000}***	{0.031}**
lobbyist and lawyer contributions	0.021	0.020	0.013	0.028	0.026	0.022
cluster-robust p-value	(0.026)**	(0.022)**	(0.224)	(0.004)***	(0.012)**	(0.084)*
$RI-t$ p-value	{0.022}**	{0.022}**	{0.285}	{0.010}**	{0.013}**	{0.128}
other business contributions	0.067	0.035	0.102	0.066	0.010	0.063
cluster-robust p-value	(0.480)	(0.715)	(0.127)	(0.483)	(0.911)	(0.308)
$RI-t$ p-value	{0.382}	{0.663}	{0.108}	{0.406}	{0.900}	{0.282}

Notes: The shaded rows show the difference-in-differences coefficient of interest point estimate. Each column is a point estimate from a separate regression. For each point estimate, two different p-values are reported. Conventional cluster-robust p-values are shown in parentheses. Randomization inference (RI) t p-values following MacKinnon and Webb (2020) are shown in curly brackets. *** denotes significance at 1 percent, ** at 5 percent, and * at 10 percent. Complete regression results for each outcome variable may be found in Appendix A. Randomization inference p-values were formed from 100,000 re-randomized samples. All contribution variables are in per capita terms.

A Appendix: Descriptive Statistics and Regression Results

Table A.1: Descriptive Statistics

Variable	Treatment Group Mean	Treatment Group Std Deviation	Comparison Group Mean	Comparison Group Std Deviation
total organization contributions (per capita)	1.274	1.552	0.796	1.162
energy and large manufacturer contributions (per capita)	0.103	0.257	0.095	0.700
finance contributions (per capita)	0.046	0.038	0.042	0.060
labor and construction contributions (per capita)	0.315	0.435	0.209	0.251
business advocacy contributions (per capita)	0.020	0.052	0.021	0.120
lobbyist and lawyer contributions (per capita)	0.127	0.136	0.084	0.171
other business contributions (per capita)	0.664	1.062	0.345	0.496
median margin of victory: all races	42.091	24.518	38.761	33.416
median margin of victory: incumbents	45.423	25.801	42.528	35.713
median margin of victory: non-incumbents	31.128	17.441	29.251	21.457
incumbent re-election rate	95.723	3.590	94.793	5.611
divided political control (yes=1)	0.519	0.501	0.513	0.500
gubernatorial election year (yes=1)	0.260	0.439	0.262	0.440
electoral competitiveness, lower house	0.863	0.969	1.611	3.744
ideology gap	-4.209	13.886	-5.789	13.990
incentive environment index	100.419	21.435	96.421	23.949
per capita GDP ($000s)	48.738	9.051	49.083	9.784
share of population 65+	13.202	1.984	13.499	2.002
electoral competitiveness, upper house	0.756	0.850	1.806	5.665
index of state economic freedom	7.121	0.478	7.152	0.530
unemployment rate	6.293	2.032	5.504	1.923

Notes: Sample includes 17 treatment group states and 31 comparison group states over the period from 2000 to 2016. States are classified as treatment or comparison using the 3500X median threshold definition described in Section 3.1. All contribution variables were obtained from the National Institute on Money in Politics. The median margin of victory for all candidates, incumbents, non-incumbents, incumbent re-election rates (defined in Section 3.3), and lower/upper house electoral competitiveness are from Carl Klarner, "State Legislative Election Returns, 1967-2016" available at Harvard's Dataverse https://doi.org/10.7910/DVN/3WZFK9). Divided political control and gubernatorial election years are from various issues of The Book of the States. Ideology gap, which is the difference between government and citizen ideology in the state, is from Berry et al. (1998) and available at: https://rcfording.com/state-ideology-data/. Population share 65 and older is from the Census Bureau. Per capita GDP ($000s) is from the Bureau of Economic Analysis. State unemployment rates are from the Bureau of Labor Statistics. The index of state economic freedom (overall score) is from the Fraser Institute's Economic Freedom of North America report. The incentive environment index is from Patrick (2014). Alaska and Hawaii do not have incentive environment index values.

Table A.2: median margin of victory: all races

	Dependent variable: median margin of victory: all races					
	(1)	(2)	(3)	(4)	(5)	(6)
post	5.041***	5.172***	5.679**	5.614**	5.556**	5.222*
	(1.843)	(1.960)	(2.755)	(2.202)	(2.424)	(2.995)
per capita GDP ($000s)				0.264***	0.243***	0.109**
				(0.051)	(0.056)	(0.051)
divided political control (yes=1)				-1.033***	-1.401***	-1.533***
				(0.376)	(0.349)	(0.275)
incentive environment index				-9.112***	-8.408***	-8.034***
				(1.609)	(1.802)	(1.886)
index of state economic freedom				5.176***	5.419***	5.085***
				(1.069)	(0.912)	(0.816)
gubernatorial election year (yes=1)				1.424***	1.366***	2.018***
				(0.273)	(0.259)	(0.203)
ideology gap				0.060***	0.053***	0.081***
				(0.014)	(0.014)	(0.013)
electoral competitiveness, lower house				-0.128***	-0.175***	-0.116*
				(0.030)	(0.028)	(0.063)
electoral competitiveness, upper house				0.364***	0.316***	-0.021
				(0.057)	(0.053)	(0.062)
unemployment rate				-0.779***	-0.758***	-1.282***
				(0.132)	(0.098)	(0.242)
share of population 65+				2.722***	2.566***	2.436**
				(0.625)	(0.690)	(0.980)
N	2314	2069	2406	2166	1945	2272
Adj. R-squared	0.866	0.869	0.855	0.874	0.876	0.863
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.819	0.826	0.971	0.703	0.671	0.825
p(joint F pre-treatment trend)	0.390	0.401	0.771	0.158	0.157	0.328
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.3: median margin of victory: incumbents

	Dependent variable: median margin of victory: incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
post	6.535**	6.942**	6.870***	6.731**	7.203**	6.174**
	(2.628)	(2.778)	(2.470)	(2.966)	(3.248)	(2.772)
per capita GDP ($000s)				0.282***	0.281***	0.089***
				(0.068)	(0.078)	(0.034)
divided political control (yes=1)				-0.750	-1.424***	-1.204***
				(0.465)	(0.452)	(0.356)
incentive environment index				-4.748***	-4.772***	-4.544***
				(1.151)	(1.296)	(1.392)
index of state economic freedom				3.117*	4.785***	4.656***
				(1.737)	(1.490)	(1.773)
gubernatorial election year (yes=1)				0.404	0.238	0.922***
				(0.319)	(0.276)	(0.238)
ideology gap				0.045**	0.033	0.077***
				(0.021)	(0.023)	(0.017)
electoral competitiveness, lower house				-0.140***	-0.202***	-0.130***
				(0.026)	(0.018)	(0.040)
electoral competitiveness, upper house				0.423***	0.435***	0.089
				(0.084)	(0.082)	(0.095)
unemployment rate				-1.367***	-1.087***	-1.660***
				(0.223)	(0.172)	(0.323)
share of population 65+				3.303***	3.600***	3.194***
				(0.590)	(0.637)	(0.742)
N	2314	2069	2406	2166	1945	2272
Adj. R-squared	0.875	0.874	0.863	0.882	0.881	0.870
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.964	0.937	0.851	0.872	0.789	0.703
p(joint F pre-treatment trend)	0.505	0.492	0.619	0.251	0.242	0.336
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.4: median margin of victory: non-incumbents

	Dependent variable: median margin of victory: non-incumbents					
	(1)	(2)	(3)	(4)	(5)	(6)
post	-0.012	-0.198	0.380	-0.418	-0.644	-0.939
	(2.923)	(2.435)	(3.222)	(2.932)	(2.855)	(3.265)
per capita GDP ($000s)				0.668***	0.668***	0.744***
				(0.109)	(0.092)	(0.089)
divided political control (yes=1)				-1.821***	-1.757***	-2.298***
				(0.435)	(0.288)	(0.260)
incentive environment index				-1.344***	-1.442***	-0.828**
				(0.298)	(0.313)	(0.376)
index of state economic freedom				-14.625***	-14.488***	-14.681***
				(1.425)	(1.570)	(1.539)
gubernatorial election year (yes=1)				1.759***	1.673***	1.485***
				(0.354)	(0.352)	(0.295)
ideology gap				-0.137***	-0.140***	-0.155***
				(0.021)	(0.013)	(0.014)
electoral competitiveness, lower house				-0.009	0.021	0.019
				(0.176)	(0.180)	(0.179)
electoral competitiveness, upper house				-7.259***	-7.227***	-6.530***
				(0.412)	(0.451)	(0.455)
unemployment rate				-2.512***	-2.402***	-2.172***
				(0.207)	(0.228)	(0.270)
share of population 65+				5.201***	5.088***	5.439***
				(0.640)	(0.578)	(0.617)
N	2235	2003	2335	2087	1879	2201
Adj. R-squared	0.542	0.551	0.534	0.585	0.593	0.577
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.530	0.307	0.614	0.559	0.329	0.624
p(joint F pre-treatment trend)	0.167	0.196	0.739	0.152	0.161	0.637
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.5: incumbent re-election rate

	Dependent variable: incumbent re-election rate					
	(1)	(2)	(3)	(4)	(5)	(6)
post	-0.750	-0.402	0.347	-0.373	0.058	0.007
	(0.578)	(0.650)	(0.871)	(0.688)	(0.782)	(1.028)
per capita GDP ($000s)				0.343***	0.478***	0.424***
				(0.054)	(0.045)	(0.043)
divided political control (yes=1)				-1.812***	-2.111***	-1.909***
				(0.106)	(0.194)	(0.158)
incentive environment index				-3.017***	-2.838***	-3.076***
				(0.639)	(0.575)	(0.565)
index of state economic freedom				1.828***	3.927***	4.775***
				(0.440)	(0.471)	(0.443)
gubernatorial election year (yes=1)				0.765***	0.465***	0.561***
				(0.060)	(0.045)	(0.044)
ideology gap				-0.066***	-0.075***	-0.069***
				(0.007)	(0.006)	(0.004)
electoral competitiveness, lower house				-0.327***	-0.258***	-0.254***
				(0.026)	(0.036)	(0.033)
electoral competitiveness, upper house				0.513***	0.544***	0.440***
				(0.044)	(0.045)	(0.030)
unemployment rate				-0.068	0.550***	0.406***
				(0.101)	(0.060)	(0.033)
share of population 65+				2.734***	3.813***	3.445***
				(0.218)	(0.150)	(0.160)
N	2314	2069	2406	2166	1945	2272
Adj. R-squared	0.375	0.369	0.351	0.477	0.469	0.446
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.143	0.780	0.994	0.057	0.680	0.844
p(joint F pre-treatment trend)	0.084	0.294	0.852	0.038	0.185	0.378
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.6: total organization contributions (per capita)

	Dependent variable: total organization contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.234**	0.135*	0.226*	0.294***	0.175**	0.279*
	(0.113)	(0.072)	(0.133)	(0.110)	(0.077)	(0.143)
per capita GDP ($000s)				-0.001	-0.010***	-0.010***
				(0.002)	(0.002)	(0.002)
divided political control (yes=1)				0.068***	0.052***	0.069***
				(0.015)	(0.016)	(0.015)
incentive environment index				-0.062***	-0.089***	-0.098***
				(0.022)	(0.030)	(0.029)
index of state economic freedom				0.005	0.044	-0.026
				(0.036)	(0.045)	(0.044)
gubernatorial election year (yes=1)				0.492***	0.492***	0.455***
				(0.018)	(0.027)	(0.017)
ideology gap				0.005***	0.007***	0.008***
				(0.001)	(0.001)	(0.000)
electoral competitiveness, lower house				-0.006***	-0.008***	-0.008***
				(0.001)	(0.001)	(0.002)
electoral competitiveness, upper house				-0.005***	-0.005***	-0.005***
				(0.001)	(0.001)	(0.001)
unemployment rate				-0.041***	-0.100***	-0.123***
				(0.009)	(0.011)	(0.012)
share of population 65+				-0.011	-0.030**	-0.047***
				(0.011)	(0.012)	(0.016)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.245	0.251	0.233	0.447	0.398	0.350
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.152	0.418	0.620	0.002	0.055	0.249
p(joint F pre-treatment trend)	0.591	0.799	0.594	0.158	0.616	0.468
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post.* Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.7: energy and large manufacturer contributions (per capita)

	Dependent variable: energy and large manufacturer contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.024	-0.062	-0.056	0.074	-0.005	0.008
	(0.080)	(0.042)	(0.055)	(0.074)	(0.035)	(0.047)
per capita GDP ($000s)				-0.004***	-0.007***	-0.006***
				(0.000)	(0.001)	(0.000)
divided political control (yes=1)				-0.018***	0.022***	0.024***
				(0.005)	(0.007)	(0.006)
incentive environment index				-0.008***	-0.024***	-0.020***
				(0.003)	(0.004)	(0.004)
index of state economic freedom				0.047***	0.041**	0.040**
				(0.015)	(0.020)	(0.019)
gubernatorial election year (yes=1)				0.012***	-0.007*	-0.004
				(0.004)	(0.004)	(0.003)
ideology gap				0.000	0.001***	0.001***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				0.000	0.002***	0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, upper house				0.000	-0.001***	-0.001***
				(0.000)	(0.000)	(0.000)
unemployment rate				-0.010***	-0.040***	-0.036***
				(0.003)	(0.003)	(0.003)
share of population 65+				-0.013***	-0.022***	-0.014***
				(0.005)	(0.005)	(0.004)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.051	0.048	0.047	0.214	0.125	0.125
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.994	0.994	0.993	0.214	0.427	0.425
p(joint F pre-treatment trend)	0.916	0.930	0.919	0.490	0.361	0.335
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.8: finance contributions (per capita)

	Dependent variable: finance contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.007*	0.004	0.003	0.008**	0.003	0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
per capita GDP ($000s)				0.001***	0.001***	0.001***
				(0.000)	(0.000)	(0.000)
divided political control (yes=1)				0.001	-0.001*	-0.001**
				(0.000)	(0.001)	(0.001)
incentive environment index				0.000	0.001	0.001
				(0.001)	(0.001)	(0.001)
index of state economic freedom				0.009***	0.000	0.000
				(0.002)	(0.002)	(0.002)
gubernatorial election year (yes=1)				0.035***	0.032***	0.030***
				(0.001)	(0.001)	(0.001)
ideology gap				0.000	0.000	0.000
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				-0.002***	-0.002***	-0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, upper house				0.000***	0.000***	0.000***
				(0.000)	(0.000)	(0.000)
unemployment rate				0.004***	0.002***	0.002***
				(0.001)	(0.001)	(0.001)
share of population 65+				0.001	-0.002*	-0.002**
				(0.001)	(0.001)	(0.001)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.341	0.338	0.344	0.373	0.364	0.369
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.922	0.914	0.856	0.800	0.874	0.855
p(joint F pre-treatment trend)	0.948	0.991	0.997	0.970	0.993	0.998
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.9: construction and labor union contributions (per capita)

	Dependent variable: construction and labor union contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.097	0.118**	0.142**	0.100*	0.121**	0.158***
	(0.064)	(0.051)	(0.057)	(0.059)	(0.051)	(0.061)
per capita GDP ($000s)				0.003***	0.003***	0.004***
				(0.000)	(0.000)	(0.001)
divided political control (yes=1)				0.043***	0.028***	0.034***
				(0.004)	(0.004)	(0.003)
incentive environment index				0.011***	0.005	0.010**
				(0.004)	(0.004)	(0.005)
index of state economic freedom				-0.074***	-0.047***	-0.074***
				(0.011)	(0.012)	(0.007)
gubernatorial election year (yes=1)				0.140***	0.134***	0.115***
				(0.003)	(0.005)	(0.003)
ideology gap				0.002***	0.002***	0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				0.005***	0.005***	0.006***
				(0.001)	(0.001)	(0.001)
electoral competitiveness, upper house				-0.004***	-0.004***	-0.005***
				(0.000)	(0.000)	(0.000)
unemployment rate				-0.017***	-0.015***	-0.012***
				(0.002)	(0.003)	(0.001)
share of population 65+				-0.018***	-0.017***	-0.012***
				(0.003)	(0.003)	(0.004)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.503	0.460	0.462	0.549	0.489	0.487
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.352	0.744	0.631	0.440	0.746	0.596
p(joint F pre-treatment trend)	0.846	0.602	0.543	0.916	0.642	0.586
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post.* Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.10: business advocacy contributions (per capita)

	Dependent variable: business advocacy contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.019***	0.020***	0.022***	0.018***	0.020***	0.026**
	(0.006)	(0.004)	(0.007)	(0.006)	(0.005)	(0.010)
per capita GDP ($000s)				-0.001***	-0.001***	-0.001***
				(0.000)	(0.000)	(0.000)
divided political control (yes=1)				0.019***	0.015***	0.015***
				(0.003)	(0.003)	(0.002)
incentive environment index				-0.007***	-0.008***	-0.007***
				(0.001)	(0.001)	(0.001)
index of state economic freedom				0.006	0.013***	0.010
				(0.005)	(0.005)	(0.006)
gubernatorial election year (yes=1)				0.025***	0.021***	0.019***
				(0.001)	(0.001)	(0.001)
ideology gap				0.001***	0.001***	0.001***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				-0.001***	-0.001***	-0.001***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, upper house				0.000	0.000**	0.000**
				(0.000)	(0.000)	(0.000)
unemployment rate				-0.012***	-0.013***	-0.011***
				(0.001)	(0.002)	(0.002)
share of population 65+				-0.001	-0.001	-0.001
				(0.001)	(0.001)	(0.001)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.224	0.215	0.216	0.240	0.230	0.230
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.991	0.999	0.996	0.997	1.000	0.999
p(joint F pre-treatment trend)	0.987	0.985	0.999	0.991	0.991	0.982
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parantheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression tesing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression tesing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.11: lobbyists and lawyers contributions (per capita)

	Dependent variable: lobbyists and lawyers contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.021**	0.020**	0.013	0.028***	0.026**	0.022*
	(0.009)	(0.009)	(0.011)	(0.010)	(0.010)	(0.013)
per capita GDP ($000s)				-0.002***	-0.002***	-0.002***
				(0.000)	(0.000)	(0.000)
divided political control (yes=1)				0.029***	0.026***	0.025***
				(0.004)	(0.004)	(0.004)
incentive environment index				0.024***	0.022***	0.023***
				(0.003)	(0.003)	(0.003)
index of state economic freedom				0.084***	0.078***	0.076***
				(0.018)	(0.017)	(0.017)
gubernatorial election year (yes=1)				0.070***	0.068***	0.063***
				(0.004)	(0.005)	(0.004)
ideology gap				0.002***	0.002***	0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				-0.002***	-0.003***	-0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, upper house				0.000***	0.000***	0.000***
				(0.000)	(0.000)	(0.000)
unemployment rate				0.007***	0.004**	0.005**
				(0.002)	(0.002)	(0.002)
share of population 65+				0.013***	0.009***	0.010***
				(0.001)	(0.002)	(0.002)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.306	0.313	0.314	0.332	0.337	0.338
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.919	0.946	0.771	0.960	0.930	0.785
p(joint F pre-treatment trend)	0.680	0.778	0.421	0.808	0.796	0.489
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in parentheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.

Table A.12: other business contributions (per capita)

	Dependent variable: other business contributions (per capita)					
	(1)	(2)	(3)	(4)	(5)	(6)
post	0.067	0.035	0.102	0.066	0.010	0.063
	(0.094)	(0.096)	(0.067)	(0.095)	(0.092)	(0.062)
per capita GDP ($000s)				0.002*	-0.004***	-0.006***
				(0.001)	(0.001)	(0.001)
divided political control (yes=1)				-0.006	-0.039***	-0.027***
				(0.011)	(0.008)	(0.005)
incentive environment index				-0.082***	-0.085***	-0.105***
				(0.021)	(0.027)	(0.026)
index of state economic freedom				-0.067***	-0.040*	-0.077***
				(0.025)	(0.023)	(0.024)
gubernatorial election year (yes=1)				0.211***	0.244***	0.232***
				(0.011)	(0.016)	(0.011)
ideology gap				0.001*	0.002***	0.002***
				(0.000)	(0.000)	(0.000)
electoral competitiveness, lower house				-0.005***	-0.008***	-0.011***
				(0.001)	(0.001)	(0.001)
electoral competitiveness, upper house				-0.001***	0.000**	0.000
				(0.000)	(0.000)	(0.000)
unemployment rate				-0.013*	-0.039***	-0.072***
				(0.007)	(0.008)	(0.010)
share of population 65+				0.006	0.002	-0.028**
				(0.006)	(0.006)	(0.011)
N	4561	4071	4732	4279	3835	4476
Adj. R-squared	0.297	0.229	0.174	0.317	0.247	0.187
Threshold	2500X Median	3500X Median	4500X Median	2500X Median	3500X Median	4500X Median
Number of treated states	19	17	14	19	17	14
Number of comparison states	31	33	36	29	31	34
p(joint F lead terms)	0.000	0.003	0.068	0.000	0.002	0.090
p(joint F pre-treatment trend)	0.011	0.427	0.184	0.006	0.362	0.297
Comparison group	Never treated	Never treated	Never treated	Never treated	Never treated	Never treated

This table presents the effect of states awarding their first large business incentive on the outcome denoted. The difference-in-differences parameter of interest is *post*. Treated states are those awarding a single business incentive that is 2500, 3500, or 4500 times larger than their historical median incentive award. States not awarding these unusually large incentives are the comparison group. The sample period is based on the stacked design proposed by Cenzig et al. (2019); see Section 3.2 of the main paper for complete details. Standard errors clustered at the state level in partntheses. *** denotes significance at the 1 percent level, ** at the 5 percent level, and * at the 10 percent level. The row p(joint F for lead terms) shows the p-value from a separate regression testing the null hypothesis that four lead terms for the treatment group are jointly equal to zero. Failing to reject the null is evidence against anticipation effects. The row p(joint F pre-treatment trend) shows the p-value from a separate regression testing the null hypothesis that the treatment group follows a different pre-treatment trend from the comparison group in the pre-treatment period. Failing to reject the null is evidence in support of the parallel trends assumption holding in the pre-treatment period. Models were estimated with a constant term, state-cohort fixed effects, and year-cohort fixed effects that are not reported. Alaska and Hawaii are excluded from the comparison group in the regressions with covariates because they have no incentive environment index values.