Electoral Institutions and Electoral Cycles in Investment Incentives: A Field Experiment on Over 3,000 U.S. Municipalities[1]

Nathan M. Jensen, University of Texas at Austin
Michael G. Findley, University of Texas at Austin
Daniel L. Nielson, Brigham Young University

Abstract:
Through a field experiment and audit study we test how the electoral calendar affects the use of local economic development policies. We explore how electoral timing along with local political institutions and party composition affect local governments' offers of investment incentives to outside firms. We legally incorporated a consultancy and, on behalf of a real investor in manufacturing, approached roughly 3,000 U.S. municipalities with inquiries. The main experimental results show no greater tendency to offer incentives for investment anticipated prior to than after elections – a null result that is estimated with high precision. Limiting the sample to municipalities that specialize in manufacturing, the relevant subgroup, suggests that election timing matters in this most likely set of locales. Some observational findings include additional evidence on how direct elections of executives and partisanship correlate with incentive offers.

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network, at https://doi.org/10.7910/DVN/8PIIAI

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[1] We pre-registered the research design with the *Evidence in Governance and Politics (EGAP) Network* (www.egap.org) on July 31, 2013, prior to the execution of the experiment in August 2013. The registration documents were embargoed until September 2014 to avoid detection in the field experiment. Anonymized preregistration documents are available from EGAP upon email request. We registered substantial information including the study background, hypotheses, expected analysis procedures, and who would carry out the research. We pre-committed to report certain interventions and results regardless of the outcome, which we have done throughout, and we also note where we deviate from the preregistration document.

1. Introduction

Seminal studies in political science, economics, and public policy have theorized that the election calendar alters politician action toward economic policy or public spending (Nordhaus 1975, Hibbs 1977, Rogoff 1990, Persson and Tabellini 2003). The observational research testing these theories empirically, however, reveals mixed findings (Franzese 2002; de Haan and Klomp 2013). These results also face well-known challenges to causal inference using observational statistics, including potential confounds and omitted variables.

In this article we build on this literature and focus on how the electoral calendar affects politicians' willingness to offer financial incentives to firms. This policy lever is both directly available to elected politicians and allows politicians to use these policies to claim credit for company investment decisions. In line with the literature, we hypothesize that politicians are more willing to make costly concessions to firms (through grants and tax breaks) that announce investments before rather than after elections.

In contrast to prior work, including research on political-business-cycles and political-budget-cycles, our study employs the first audit study and field experiment probing how electoral timing shapes a prominent lever of economic policy: the use of targeted financial investment incentives to firms. In expectation, the randomized intervention balances all potential confounds between treatment and control groups and thus enables the evaluation of causal effects or the precise estimation of their absence.

The ubiquitous tax breaks, grants, and subsidies involved in investment incentives have become key to economic development policies of U.S. cities and states. Some estimates find that states and local governments spend between $45 billion (Bartik 2017) and $80 billion each year (Story 2012) attempting to lure new investment or to reward existing firms for expanding their production. City and state agencies offer these incentives directly to firm managers and owners to publicly *announce* their job creation and expansion plans. The timing of the announcement might be negotiated between the firm and the municipality. This may motivate politicians to do what they can to assure that the credit for the use of these incentives is timed to the electoral calendar.

Our focus centers on the decisions of political leaders, probing whether they see pre-election investments as more politically valuable, and not on the strategic decisions of firms in the timing of their announcements. In the theory section below we argue that there are costs and benefits to offering incentives, which necessarily constrain politicians' responses to firms. The political benefits should increase in the pre-election period and should therefore cause an increase in investment incentives compared to post-election.

We develop a novel research design using a field experiment and audit study to examine how the timing of elections and electoral institutions shape incentive offers. We legally incorporated an actual financial-incentive consulting company that offered site-location services. We then identified a real manufacturing company with express interest in a future expansion and signed a confidentiality agreement with the company allowing us to collect information about local incentives on its behalf. In all, we approached more than 3,000 U.S. municipalities via email and collected information on local interest in providing incentives to our confederate client firm. We use the experimental (election timing) and observational (electoral institutions) evidence to address the questions of how elections shape investment-incentive policy.

We test two links between elections and municipalities' offering of targeted financial inducements. First, we directly address how electoral timing shapes policy behavior: to facilitate credit claiming, politicians should be more likely to offer incentives to companies willing to announce relocation prior to municipal elections. Second, we hypothesize that directly elected, compared to appointed, municipal executives should be more likely to overprovide incentives as a means of credit claiming for economic development.

The study produces three primary findings. First, we theorized that government officials would be more likely to respond to our potential investor pre-election. Our findings found no difference in pre- or post-election incentive offers for the full sample of municipalities. Subgroup analysis of municipalities that identified manufacturing as an economic development focus were, however, significantly more likely to respond and offer incentives to our proposed manufacturing investment if the municipalities were assigned to the before-election treatment compared to the after-election control, and there is some evidence that the amount of those incentives increases in response to the before-election treatment.

A second primary finding indicates mixed results on the impact of incentives for municipalities with elected as opposed to appointed executives. The evidence on the impact of elections varies by model specification, and we are left to conclude that institutional electoral incentives may matter, but further investigation is necessary. These mixed results are observational, of course, as the form of municipal government cannot be manipulated experimentally.

In the final part of our paper we deviate from our pre-registered research design and analysis plan to explore other relationships in the rich set of data generated from the study. A particularly striking pattern in the data demonstrates that municipalities with Republican leanings respond more often and are significantly more active in offering incentives relative to Democratic municipalities. This result is robust across many, though not all, additional specifications.

2. Preregistered Theory and Hypotheses: Electoral Incentive Cycles

In this section we outline our theoretical motivation along with pre-registered hypotheses. We then turn to a discussion of alternative theories of electoral cycles and articulate where our research design allows us to differentiate these alternative theories and where results based on this design would be consistent with multiple theories of electoral cycles of policy making.

Economic performance is so important to incumbent politicians that economic policy can be driven by the electoral calendar. For example, a large literature has examined the existence of political-business cycles – the attempt to use fiscal or monetary policy in the periods prior to elections for short-term electoral gains (Nordhaus 1975; Hibbs 1977).[2]

[2] See Franzese (2002) and Dubois (2016) for a review of the literature. For an example of cross-national studies see Canes-Wrone and Park (2012)

A variant of this literature has focused on political-budget cycles, in which governments expand spending in the period prior to elections (Rogoff 1990; Persson and Tabellini 2003; Brender and Drazen 2005; Alt and Lassen 2006).

We contribute to the literature on electoral cycles in economic policies by examining the use of targeted financial "incentives" to attract direct investment in the United States.[3] We argue that an increasingly important tool of economic policy is investment incentives, which are targeted at individual firms. Proposed incentives for firm-level investment can take many forms, including long tax holidays, worker retraining grants, low-interest loans, favorable property leases, and infrastructure improvements in addition to the conventional tools of tax reductions, grants, and subsidies. All such incentives are meant to sway a company's decision to invest, expand, or stay in a given location. And as noted, a large share of U.S. states and cities are now in the business of offering incentives to lure investment.

On the surface, the use of incentives for electoral gain is perplexing. Much of the literature in political science and economics highlights that these incentives have a very

[3] As argued by Alt and Rose (2007), focusing the study in the United States allows us to hold many contextual factors constant, enabling a clearer comparison across local governments. The rich institutional variation in the U.S. can thus be used to examine how democratic institutions affect economic policy (Besley and Case 2003).

limited ability to affect firm location choice,[4] are exceedingly expensive relative to the benefits,[5] and can have unintended consequences, such as the encouragement of rent seeking.[6] Thus, while the consensus is that these policies are far from ideal, and many central governments worldwide are beginning to regulate their practice, use of incentives in the United States remains widespread.[7]

Given these criticisms of investment incentives, why do they persist? The literature makes at least two distinct theoretical arguments. First, many non-governmental organizations (NGOs) champion the argument that campaign contributions and lobbying shape government economic policy in this domain as in many others.[8] Investors, along with other vested local interests (landowners, developers, construction companies), push for economic development policies that essentially transfer taxpayer money to firms.[9]

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[4] Klein and Moretti (2013) provide an excellent theoretical overview of the rationale for the use of local economic development policies. Most of the empirical literature is critical of these programs. See Easson (2004) for an excellent review of the literature and Buss (2001) for a meta-analysis of 300 studies of incentives in the United States. For a recent contribution see Patrick (2014).

[5] For example, see Wells et al (2001), Fox and Murray (2004), Peters and Fisher (2004) and Bartik (2005).

[6] For example see Zee, Stotsly, and Ley (2002).

[7] For one of the most comprehensive treatments of the issue see Thomas (2011).

[8] One example is the work of Good Jobs First (goodjobsfirst.org). This NGO has collected original data on incentives and has begun to link these incentives to campaign contributions. See also LeRoy (2005).

[9] Weber and O'Neill-Kohl (2013)

Although the mechanisms of influence can vary, the key is that that these local economic development policies could be captured by special interests.

A second argument is that electoral connections between economic development policies and incumbent politicians can drive incentive use. Grimmer, Messing, and Westwood (2012) argue that politicians can use fiscal spending as a mechanism for electoral gain, but this requires politicians to actively seek credit for this spending. This credit claiming is most easily achieved when spending is "visible and easily targeted" (de Haan and Klomp 2013, 389).

Jensen et al (2014) argue that economic development incentives can be an effective mechanism for credit claiming or blame avoidance, where a politician can take credit for investment that was coming into her district by offering an incentive, linking a concrete government policy to an individual investment. These incentives, and the jobs "created" by these incentives, are touted on Governors' websites and are part of press releases.[10] Thus, these policies provide opportunities for credit claiming for incumbents.

We build on this work in ways that broaden the theoretical and empirical scope. Specifically, we investigate the motivations and constraints, including electoral cycles and institutions, that policymaking elites face as they consider allocating investment incentives.

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[10] Conversely, the politicians can show effort in trying to attract investment by offering an incentive and diffusing blame if the investment locates in another district. Using a series of survey experiments, Jensen et al (2014) find evidence for credit claiming and even stronger evidence for blame avoidance in how voters evaluate investment and incentives policies.

In the United States, local governments have the ability to offer incentives to firms, providing a clear link between government policies and economic outcomes.

Building on work in political economy, including rich literatures on political-budget cycles, we begin with an assumption that voters are imperfectly informed about the impact of policies on outcomes.[11] Unlike the political-business-cycle models that largely focus on the relationship between inflation and unemployment (i.e., the Phillips Curve), literature on political-budget cycles highlights the electoral use of fiscal policy in periods prior to elections. We focus on a politician's willingness to use incentives for electoral gain.

We argue that there are costs to offering incentives, most of which do not vary across the electoral calendar. First, the ineffectiveness of incentives leads to very high costs-to-jobs ratios.[12] Second, the administrative costs of these programs can be high, and they can crowd out other government policies.[13] Third, backlashes from citizens, watchdog groups, and competing firms can further increase the political costs of these programs.[14]

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[11] Much of the political-business cycle literature assumes that voters do not know a politician's "type."

[12] See Jensen (2017) for a review of the recent evidence.

[13] One example is the State of Kentucky's commissioned study that examined the tradeoffs between incentives and other economic development policies. See Anderson Economic Group (2012).

[14] A recent example includes fights over the transparency of economic development programs. Numerous watchdog NGOs, public sector unions, and small business associations submitted comments for a new rule (GASB 77) requiring additional disclosure of the costs of tax abatements. For a discussion from Good Jobs First, see: http://www.goodjobsfirst.org/gasb-statement-no-77

Despite these costs, locations have extensively used incentives. In a way that is consistent with the work of Jensen, Malesky and Walsh (2015), electoral pressures stemming from the form of local government can lead to greater use of incentives and less use of cost-benefit analysis.[15] We extend this work by focusing on how the use of incentives may be largely shaped by electoral mechanisms, especially the timing of elections. Politicians can take advantage of poorly informed or myopic voters by enacting policies for short-term electoral gains. Thus, the electoral benefits of using incentives are especially powerful in the run-up to the polls. Politicians may more readily pursue investment from firms willing to announce their investments before elections, which might help maximize the electoral returns of perceived job creation.

Specifically, we explore how the timing of the announcement of a firm's investment (before or after an election) shapes the willingness of a government to exert effort and offer incentives to the firm. We argue that investments that are announced prior to elections ought to be more valuable to politicians than investments that will be announced post-election. This is a straightforward application of the core political-budget cycle logic that politicians are more likely to pursue visible economic benefits to voters before rather than after elections.

We argue that random assignment of the timing of investments enables a clear test of this specific electoral-cycle empirical implication in which we can examine the

[15] See also Feiock and Kim (2001)

willingness of politicians to provide incentives to firms pre- and post-election and also estimate the size of these incentives across experimental conditions.

This leads to a first hypothesis, which we can test experimentally.

Hypothesis 1: Municipalities are more likely to respond to inquiries, and to offer incentives, if the investment will be announced prior to rather than following the next election.

Our empirical analysis also tests existing theoretical work on the use of economic development incentives. Hypotheses 1 is of course predicated on an institutional framework that allows for direct elections. Work such as Rickard (2018) explores how electoral institutions shapes the use of incentives. She argues that economic geography drives the employment of subsidies, where majoritarian institutions can encourage subsidies to firms concentrated within electoral districts.

Our second hypothesis explores how the electoral pressures on leaders shape economic development policies. In the United States, while there are many forms of local government, the majority of municipalities can be classified as either mayor-council systems or executive-manager systems (See Feiock et al (2003)). Mayors and city managers all represent the same electoral districts, and thus we hold economic geography constant. But these institutional systems have different types of political accountability and relationships between politicians and voters.

Mayors are directly elected by voters, which leads to direct electoral pressures on them (Vlaicu and Whalley 2016). Previous research argues that mayor-council governments provide larger incentives (in dollar terms) and this is made possible by the weaker oversight of these programs (Jensen, Malesky, and Walsh 2015). Mayors dramatically "overpay" for incentives.

We contribute to this literature by arguing that these electoral mechanisms also lead to the overprovision in the number of incentives, making directly elected politicians more likely to exert effort to attract companies through the *offering* of firm-specific incentives.

This leads to a second, observational, hypothesis:

Hypothesis 2: Municipalities with directly elected leaders are more likely to respond to inquiries, and to offer incentives, than indirectly elected leaders.

3. Alternative Arguments

Our first two hypotheses were developed prior to fielding our experiment and thus provide pre-registered tests of our theory that cannot be altered post-hoc. In retrospect, after seeing results, suppositions arose suggesting alternative interpretations of the analysis. These alternatives are, by necessity, speculative and exploratory and thus do not carry the weight of pre-experiment prediction. Nevertheless, exploratory consideration of other conjectures may help to interpret the results and to provide a foundation for future work. In the remainder of this section, we thus speculate on a key alternative explanation, link our work to existing theories that were not part of our pre-registered design, and outline other potential theories in relation to our empirical analysis.

Hypothesis 1 develops the key experimental intervention that we are able to randomize and test in which we focused on the overall impact of elections on incentive activity across all municipalities. This is a general hypothesis that does not specify the specialized industries, such as manufacturing, of specific municipalities. When firms from those industries propose investment to the municipalities, then we might expect the political-business-cycle effect to be most pronounced there. Indeed, one might reason that, because of the noted costs of investment incentives, no effect should be expected when pooling municipalities of all types, but inducements should come mainly when a firm and municipality focus on the same sector.[16] This suggests a subgroup-variation hypothesis.

> *Hypothesis 3: Municipalities specializing in certain industries are more likely to respond to inquiries, and to offer incentives, from firms in the matched industries if the investment will be announced prior to rather than following the next election.*

The three hypotheses are tested using an experimental research design. Our first hypothesis, our main test, focuses on how the timing of elections shapes local governments' efforts in attracting investment. Finding increased willingness to offer incentives prior to elections is consistent with our theory, although alternative theories could also explain this pattern. We briefly outline some of these possibilities below.

Klein and Sakurai, examining budget cycles in Brazil, find that politicians with a limited ability to run fiscal deficits are more likely to shift spending to more visible programs. In contrast, Alt and Lassen (2006) and Hallerberg et al. (2007) find increases in public debt in periods prior to elections. Our analysis is unable to differentiate governments' shifting efforts to more visible reforms (incentives) from economic development efforts that are increasing across the board through debt financing. This does not affect our theory or research design, but the implications for the local economy and

[16] This subgroup analysis was suggested to us after data collection was complete.

the costs of these pre-electoral cycles depend on whether these programs are funded through debt or spending cuts.

Equally important, economic development efforts could increase in periods prior to elections, but these policy changes could apply more broadly to local economies than individual incentives. Foremny and Riedel (2014) find evidence of electoral cycles in the levies of business taxes in German municipalities. It is unclear to us whether we would expect the same pressures to use incentives in locations already enacting business tax cuts in periods prior to elections. Clingermayer and Feiock (2001) further broaden this perspective to examine how electoral institutions shape the time horizons of leaders, affecting numerous policies including the decision to borrow. They argue that mayor-council institutions lead to more electorally motivated spending and thus to greater borrowing.

These broader theories of how electoral environments shape policy are consistent with our theory, but they can challenge our ability to causally identify the impact of elections and institutions on incentive policy. For example, finding a null result on our incentives measure could be due to a lack of motivation to attract investment, or it could be due to politicians' already enacting broad policies – such as business or property tax reductions – to attract companies in the periods prior to the elections. Our research design does not allow us to test broad patterns of changes in economic development efforts and thus we cannot rule out that any null result on incentives could be due to other, broader economic development policies. We only note that our work theorizes that incentives are the most visible policy that can be used for credit claiming for an individual firm's investment decision.

A final point is that our empirical work can contribute to important questions on the implications of fiscal federalism for economic performance. There is a rich theoretical debate over the impact of federalism on government functioning and, most relevant for our work, how the direct elections of subnational politicians affect economic performance. For example, Blanchard and Shleifer (2001) argue that directly elected politicians are more likely to be captured by special interests, leading to less efficient economic policies. Enikolopov and Zhuravskaya (2007) find no support for this relationship, although a meta-analysis of cross-national studies by Baskaran et al (2016) finds that political institutions have a major impact on the link between fiscal federalism and growth, although the authors note the sensitivity of these observational studies to the controls included in the analysis.

We contribute to this literature by examining how local elections vary within a single country, holding other institutions constant, allowing us to examine whether we observe less efficient policy (more incentives) from municipalities with direct elections versus appointed politicians. Our experimental design also allows us to side-step the thorny issues of control variables through pre-treatment blocking and through the expected balance across conditions that randomization affords.

4. Study Design

4.1 Study Context

Our study focuses on the use of incentives by U.S. municipalities, which are unusual in their economic development policies. In most countries a strong national investment-promotion agency comprises the primary interlocutor for firms seeking to enter a market. Although there is tremendous variation in the capacities and professionalism of these agencies, few countries allow subnational units (states and municipalities) to play much of a direct and active role in the attraction of investment. The United States only recently established a national investment promotion agency, and it has few tools to attract investment directly.[17] This largely leaves economic development policies to the state and local levels, and a key policy is the provision of incentives for investment promotion.

The uniqueness of the United States suggests caution against attempts to generalize the findings to the international arena. Most localities across the world do not have the same unconstrained economic development tools at their disposal. Yet U.S. municipalities provide an interesting laboratory to examine the interactions between firms and governments. Specifically, the large number of U.S. municipalities with a population above 10,000 – 3,117 in our study – provides rich variation in municipality size, electoral

[17] See: http://selectusa.commerce.gov/

institutions, and the ability to couple a large observational and experimental approach in auditing direct firm-government interactions. By moving away from the well-known limitations of cross-national data in the study of investment incentives internationally, this experiment capitalizes on a larger pool of actors capable of being studied with experimental techniques (Hyde 2010; Findley et al 2013).

4.2 Experimental Protocol

Approaching mayoral economic development offices as researchers would likely have induced debilitating social desirability bias (Findley et al. 2017). Instead, we sought to approach local officials in their actual operating environments in which they were unaware that their normal day-to-day actions were being studied. Accordingly, we conducted the study as a randomized natural field experiment (Harrison and List 2004). The major advantage of natural field experiments is that they maximize the realism of the experimental treatments and unobtrusively measure behavior (Gerber and Green 2012).

To maximize the realism of the study, and to minimize deception, we first legally incorporated an actual consultancy. Then, we identified a real firm that was interested in investing in another state, and our consultancy formed an agreement to represent the confederate client firm as detailed below. The confederate firm we identified was willing to provide concrete details on its potential investment in a proposed municipality including projected numbers for job creation and capital investment that were modeled on the operations of its existing plants. Our investment proposal exactly matched the real proposal given to us by the client and thus sharply decreased deception.[18]

The consulting company mimicked existing U.S.-based investment promotion and incentive management companies. These companies are generally small operations and often do not publish their client lists on their websites. We incorporated our company, Globeus Consulting, as an LLC in Delaware in 2013.[19] We created a company website and a board of consultants – all academics willing to lend their names for the purposes of the experiment.[20]

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[18] Our client indicated that there was no latitude for changes in the amount of capital invested or number of employees, which we honored fully in the approach emails. Our client did authorize us to vary the timing of announcement of the investment decision. The client also authorized us to randomly vary the implied country of origin of the investment, the results of which are comprehensively reported elsewhere. Thus, this is a real investor with fixed preferences that is evaluating a relocation decision. We signed a confidentiality agreement with the investor, assuring that the name of the individual or company would not be used in the experiment. In return for collaborating as a confederate in the study, the investor was offered an analysis of potential locations for the investment based on the conclusions of our study.

[19] We made it clear in our approach email that all negotiations would occur between the municipal and the client firm and that we were only collecting information at the beginning of the process. The consulting company was built solely for research purposes and did not collect any fees or generate any revenues.

[20] One author was listed as the company president. Three paid research assistants served as "Associates" that directly contacted cities through email addresses registered through our website. We created Internet phone lines for use by our research assistants if cities required follow-up calls.

The key experimental treatment consisted of directly emailing the executive, the chiefs of staff, and any economic development directors, in 3,117 municipalities with the details of a proposed investment. Our client provided the plans for the future investment that would include $2 million in capital investment and 19 full-time employees. This investment is relatively small, where incentive data collected by *Incentive Monitor* finds that this would put this investment at the bottom 25 percent of incentives from 2010-2014 in terms of jobs created and the bottom 30 percent of capital expenditures. Selecting an investor at the bottom quartile may have led some larger municipalities to ignore the inquiry because it was from a small investor, and thus this may have suppressed the response rate among large municipalities. On the other hand, the relatively small firm size is a more realistic possibility for the smaller municipalities contacted in the study, which are the modal type in our sample, perhaps heightening interest among these smaller locales. The fact that this is a plausible investment for small and large municipalities alike provides for a realistic treatment in line with a large share of investment opportunities for governments.

In our approach, we contacted all municipalities by email and asked them to fill out a Qualtrics webform wherein we could track response rates and collect information including whether and how much they proposed in incentives, which comprised the central outcome of interest.[21] The exact wording of our email appears below and in Appendix A and our Qualtrics questions in Appendix B. Field experiments require special care in the ethical treatment of subjects. We document these issues in Appendix C.

As outlined in the previous section, the experiment included two treatments. First, we randomly assigned the timing of the investment announcement. We learned the dates of the next mayoral elections in all subject municipalities and then randomly assigned an indication that the announcement of the investment would occur either two months prior to the given election or one month after. For example, if the next mayoral election in the subject municipality was set to occur in November of the following year, we indicated that the investment announcement would take place either in September (treatment) or December (control).[22] We only included formal election dates for municipalities with elected mayors, and we did not code for elections of city councils or the appointment of managers. In our robustness tests we also include dummy variables for the quarter of the

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[21] We made clear that these incentive details were not binding and that we expected cities would interact directly with the investor if there were mutual interest. We estimated that subjects would on average spend 10-15 minutes answering the emails, often with material that had previously prepared for such exchanges. Given that such requests are part of their normal day-to-day routines, costs of responding would be low on the one hand and on the other there would be the potential benefit of attracting the interest of the actual investor.

[22] Municipalities without mayoral election dates were treated with one of four dates corresponding to the four quarters of the calendar year.

proposed investment, allowing us to examine if the fourth quarter (with the most elections of any period) leads to greater effort in the allocation of incentives.

The following email prompt displays the precise wording where the elements in boldface are the two possible dates for the announcement of investment:

"I am an associate with GLOBEUS Consulting (see our website here [insert hyperlink]). GLOBEUS is a new consulting firm that specializes in matching cities with prospective firms. I work in the GLOBEUS group focusing on investors based in [the United States / Japan / China] and am contacting you to see if your city would be a good match for a client I am representing.[23]

Our client is considering an expansion of a manufacturing plant producing electrical grounding products. The company is looking to make a decision and announce the investment in ["**Month" of "Year" {corresponding to two months before municipality's next scheduled election for executive} / "Month" of "Year" {corresponding to one month after next election}**]. Based on specs from another facility, we project that the plant would create 19 full-time hourly jobs at around $12 an hour plus benefits and 6 salaried jobs at around $40,000 per year.

The company is looking to buy or lease a 15,000 to 20,000 square-foot building. The total investment would be $2,000,000 ($1,750,000 on building and equipment and $250,000 on other various moving expenses). Previous plants have taken 6 months from the time of the announcement to being fully operational.

To examine the feasibility of your city for this proposed project we are asking for you to fill out this web form (available here [insert hyperlink]) on the type of incentives you could potentially offer this investor and what types of incentives you have offered in the past.

As you might expect, this offer is not binding and we realize any formal offer would require due diligence and direct interaction with our client. Our goal at this stage is to present a detailed analysis to our client on the feasibility of relocating to your [city / town / village].

We regret that we are not authorized to provide any more details about our client at this point, but if you have any questions please feel free to contact us via email. We look forward to your response.

[Associate Name]
[us / japan / china]_client_team@globeusconsulting.com

[23] Results for the country-of-origin treatment are available upon request.

Selection & Incentives Associate Globeus Consulting—[U.S. / Japan / China] Client Team Team www.globeusconsulting.com

To achieve balance across experimental conditions, prior to assignment we block randomized using the following criteria: population (above or below the median), form of government (council-manager or other), directly elected or appointed executive, quarter of next election, and state.[24]

5. Analysis of Incentive Offers

We focus on three outcome indicators. First, we examined response rates to our inquiry. Second, we considered whether the subject indicated that the respondent municipality would be willing to offer financial incentives to our client firm. Finally, we present evidence about actual offers provided to the consulting company on the size of incentives measured as the log of grant dollars per job.

To test Hypothesis 1 on electoral cycles, in Table 1 (and Figure D1 in Appendix D) we present difference-in-means results for the randomized treatment conditions compared to control. None of the results for the Before-Election treatment are significant as shown in the first two columns, and throughout Appendix Tables D1. This null result holds across all three dependent variables of response, offer, and amount offered. Of

[24] See Appendix Table D7 where we report randomization checks and show that the experimental conditions are balanced, at least for as many observables as we have access to.

course, this is not tantamount to “proving” the null is true. Rather, we merely fail to reject the null hypothesis of no mean difference between treatment and control at standard significance levels. We also note that these null results are quite precisely estimated: even with greatly increased statistical power, detectable effects would likely be very small substantively – as indicated by the relatively tight confidence intervals bounding the mean differences.

<Insert Table 1 about here>

These main results are robust to a wide variety of alternative statistical analyses. Most notably, in Appendix Table D1 we present six models, a reduced and full model for each dependent variable, where the key independent variable is the Before-Election treatment condition. These regression results corroborate the difference-in-means tests reported in Table 1. Across all three dependent variables using models including different dummy variables for locale – region or state – we find no support for the hypothesis that incentive offers are affected by the timing of an investment in the period immediately before elections compared to after.

Because municipalities could only offer incentives if they responded in the first place, these results are subject to potential selection bias and may require alternative specifications to check robustness. We therefore estimate a selection model and a multinomial probit model and report the results in Appendix D, Tables D2 and D3. We performed a number of additional subgroup analyses that had little impact on our substantive findings. The Before-Election treatment does not appear to have a significant effect on cities with elections immediately pending either within a few months, roughly one year or roughly two years (see Appendix Tables D1A-D1C).[25] Likewise, neither larger nor smaller cities in terms of population appear significantly more sensitive to the Before-Election condition across all three outcomes (see Appendix Tables D1D-D1E). Additionally, the electoral timing treatment had no significant effect for cities holding elections in any of the four quarters of the year considered separately. Further, neither more conservative nor liberal cities (or, alternatively, cities in which a majority voted for or against Barack Obama in 2008) proved significantly more sensitive to the Before-Election treatment. Likewise, incentives were no more likely in the Before-Election condition compared to control in cities with elected mayors or appointed executives.

The robust null result prompts us to speculate about its origin. In addition to subgroup exploration discussed below in which evidence might be found for the effects of electoral timing in specific data subsets, it is possible that municipalities respond to investment opportunities in scripted ways that reflect standard operating procedures (Allison 1971; Ashforth and Fried 1988; Feldman and Pentland 2003). That is, officials within local governments may be following normal, habitual routines allowing little discretion in which types of firms or investments – or their timing – receive the standard

[25] An exception reported in Appendix Table D1C occurs for the log of incentive dollars offered for cities with elections pending within 28 months. This may be an artifact of multiple testing in subgroups, so we hesitate to place too much weight on this result.

incentive offer. Such organizational scripts are common in governments at all levels and thus may help account for the lack of a detectible treatment effect.

Further, the comparison of all municipalities' responses to incentive offers masks considerable variation in local economic development strategies. Our confederate firm is a manufacturing firm that would generate economic development for most municipalities, but other locales that are either suburban with little manufacturing, or with a focus on finance or biotech, would unlikely see major political benefits to attracting this type of firm. Rather than speculate on the economic goals of firms, in a deviation from the pre-analysis plan, we harness a question on local economic development goals in the International City/County Management Association (ICMA) Economic Development survey. From the survey we were able to code whether attracting investment in manufacturing was the main focus of its economic development activities for a subset of municipalities in our sample.[26] In these three models we find strong support that municipalities with a manufacturing focus were more likely to respond to our inquiry, although we are careful in our interpretation due to the reduced sample size.

In Table 2 we test Hypothesis 3 in a sub-group analysis using only those municipalities that claimed the manufacturing focus. Note that the number of

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[26] The ICMA survey reveals a wide range of economic development efforts. Municipalities can choose agriculture, retail, institutional (government, military, etc), high tech, as well as manufacturing focus.

municipalities that completed the ICMA survey and indicating a manufacturing focus is only 168, which significantly decreases statistical power. Nevertheless, of these manufacturing-focused municipalities, the number offering an incentive in the treatment group was 21, compared to 11 in the control group – suggesting that the Before-Election treatment caused the incentives offered to nearly double compared to the base rate. Both a difference-in-means test on the partitioned sample and a regression model on the subgroup reporting a focus on manufacturing indicate that municipalities were significantly more likely to respond and to offer incentives in the condition projecting an announcement to invest before the election compared to after. With covariates included in Models 2 and 4, significance attenuates to the 0.1 level (p = .070 and p = .052, respectively). Municipalities with a manufacturing focus also appeared to offer more lucrative incentives in the treatment group, but this result is not significant nor robust to the inclusion of control covariates in regression analysis. That said, these results hold in regression analysis with interaction terms (on interaction models, see Hainmueller et al. 2016) and, even more, the logged dollars variable capturing the amount of incentives offered becomes significant at the standard 0.05 level in the interaction models (See Appendix Table D8), suggesting that there could indeed be subgroup treatment effects for all three of our dependent variables.

<Insert Table 2 about here>

To test Hypothesis 2, we present observational evidence on whether direct elections for mayor make incentive offers more likely and of greater value. Table 3 presents a total of six models each using response rate as the dependent variable. While in expectation experimental conditions are balanced across covariate values – and indeed we forced this result with the variables used for blocking – our hypothesis on the form of local government requires us to include control variables because we were obviously unable to manipulate government type. We specifically include a variable for the size of the municipality's population, since mayoral forms of government may have a different population distribution than council-mayors, as well as dummy variables for region and states. Population, direct elections, election quarter, and state were used for blocking criteria and thus best practice dictates their inclusion in regression models.

Across the models we test our hypothesis on how direct elections for municipal executive relate to incentives using information we collected on the presence or absence of executive elections for most of our sample of 3,000 municipalities. We find mixed support for the notion that local elections affect response rates.[27] In the simplest specification in Model 1, the results suggest that elected leaders have a greater propensity than appointed officials to respond to our inquiries. However, this result is not robust to the inclusion of logged municipal population (Model 2), or dummy variables for election quarter (Model 3), region (Model 4), and the combinations of quarters and regions (Model 5) and quarters

[27] Note that we cannot include the Before-Election treatment alongside the Elected variable in estimation because the two are perfectly collinear (only cities with elected mayors were assigned to the Before-Election treatment).

and states (Model 6). Indeed, the sign flips from positive to negative and becomes statistically significant in Model 5 controlling for election quarters and regions.

These inconclusive results are partially due to our reliance on government form, in which unelected executives are significantly clustered in the Northeast region. Government form is also influenced by city size and by state laws dictating the default form of government (Jensen, Malesky, and Walsch 2015). Adding control variables can thus lead to bias in coefficient estimates (Lenz and Sahn 2018). Other observational studies are better suited to address this question, and our focus remains on leveraging the benefits of our experimental approach.

<Insert Table 3 about here>

The strongly and consistently significant results for the log of population across response and incentive offered (though not logged dollars) suggests that larger municipalities are, not surprisingly, more likely to respond to firms' inquiries and to offer incentives. In terms of election quarter, roughly two thirds (65%) of municipalities hold their elections in the fourth quarter of the year. The next largest share of municipalities (28%) go to the polls in the second quarter, which we use as the reference group. Compared to spring elections, the few municipalities with winter elections (124 of 2,712, or 4.6%) were significantly less likely to offer incentives, which may have resulted from the relatively small sample in the category (so an anomalous result is more likely). Also, municipalities in the Northeast are significantly less likely to offer incentives compared to Western municipalities, which served as the reference group.

We replicate these same sets of tests for the other dependent variables in Tables 4 and 5. In Table 4 we coded the dependent variable as 1 if the municipality made a potential offer of incentives through the Qualtrics form and 0 otherwise. In Table 5 we estimate OLS regressions with the log of grant dollars offered as the dependent variable only for the municipalities that provided some sort of incentive offer. All three sets of tests recover the same basic empirical pattern: a positive and significant coefficient for direct elections for the simplest model(s), then attenuation, change in sign, and even statistical significance for the unexpected result. This pattern appears to stem from the obvious fact that whether municipal executives are elected or appointed is not assigned at random and significantly co-varies with the other control variables. Municipalities with larger populations, located in the South and Midwest (compared to West), and holding elections in the winter and fall (compared to spring) were significantly more likely to have elected executives; Northeastern municipalities were significantly less likely to elect directly. These confounds make the effects of direct elections difficult to disentangle from the other covariates and will require future exploration elsewhere.[28]

<Insert Tables 4 & 5 about here>

[28] To further probe the sensitivity of the direct election results, we merged data from the 2009 International City/County Management Association and National League of Cities. The ICMA database codes municipalities as mayor-council, executive-council, or other forms. Although including these data dramatically reduces the sample size, we find no statistically significant relationship between local institutions and incentives for all three dependent variables. Results available from the authors.

We deviated from our pre-analysis plan to consider the independent effects of partisanship, a powerful explanatory variable in the U.S. political context. The impact of partisanship on incentive use isn't theoretically clear. The Republican Party is often associated with stronger ties to business and has considerable support of business associations. Yet the main critics of incentives come from both progressive organizations on the left and many libertarian groups on the right. In many ways the right has been more active in attacking "corporate welfare," including recent criticisms of U.S. Export-Import Bank. Our analysis is driven by the unexplored question of the relationship between partisanship and incentives, and we are not claiming that this work is testing a theory of partisanship.

In Table D4 of Appendix D, we included a measure of partisanship, which accounts for the share of municipalities' population that voted for President Obama in the 2008 election. Note that including this variable, along with the 2009 survey data, dramatically reduces our number of observations to almost 10% of our original sample. In this reduced sample we find that partisanship is a strong predictor of responding to our email, where Republican municipalities were more likely to respond, although this does not appear to affect the size of incentives. For response to our inquiry, moving from a locality with the highest level of voting for President Obama in the 2008 election to the lowest Obama-support level, we observe an increased probability of responding to our inquiry from a predicted response of 6.58% from heavily Democratic municipality to 45.48% in the Republican locale. Estimating a more modest change from a municipality with support for President Obama at half a standard deviation above the mean support for President

Obama to half a standard deviation below the mean, the probability of a response increases from 18.51% to 26.53%, or a 30% increase from the base response rate. Thus, moving from a Democratic to a Republican municipality appears to have a substantial impact.

We explore the robustness of this result with models that allow for a larger sample size and an alternative measure of partisanship and report those results in Appendix Table D5 and D6. Taken together, the results from these observational models point to partisanship as one of the main drivers of incentive offers. We find that both measures of partisanship are related to whether the municipalities responded to our query and their willingness to offer an incentive. It could be argued that these observational results for partisanship could be driven by other factors including economic conditions in the municipalities. We thus included a control for the unemployment rate, which is coded from the Bureau of Labor Statistics reports on metropolitan centers.[29] The results for partisanship are qualitatively the same as those reported in Tables D4-D6, thereby offering some evidence that partisanship could be the operative dynamic. We also included a control for whether the municipality has an independent daily newspaper (de Benedictis-

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[29] We are not aware of unemployment data on all municipalities. The BLS reports unemployment data for about 400 metropolitan centers, which constitute groups of municipalities. We disaggregated those centers into specific municipalities and applied the metropolitan center code to the included municipalities. While the BLS measure masks within-metropolitan-center variation, we are unaware of other possible data sources.

Kessner and Warshaw 2016), which may capture some level of accountability of officials, and the results do not change qualitatively.

6. Conclusion

Many governments around the world, and the United States in particular, have turned to offering targeted financial incentives to individual firms in order to create jobs locally. We examined whether the use of these programs can be understood by examining the electoral benefits of incentives. We argue that politicians can harness incentives to affect the perceptions of economic development, namely through company investment announcements. Unlike previous work on political business cycles that concentrates on politicians' attempt to manipulate the real economy, our work focused on company announcements and how the timing of these announcements shapes the allocation of government economic development policies. We specifically examined whether politicians are motivated to provide more incentives in periods prior to reelection.

Exploring how electoral timing drives incentives is extremely difficult using observational data alone. Simply examining data on the incentives firms receive (if available) suffers from serious selection bias. We only observe the incentives that were both offered and accepted by firms. Equally problematic is the difficulty in comparing the incentives offered to firms of different sizes and sectors.

We sidestep many of these hurdles through an experimental approach in which we contacted more than 3,000 U.S. municipalities on behalf of a confederate firm. This allows for a standard comparison across municipalities because every municipality was interacting with a firm in the same industry, of the same size, and promising the exact same investment. The only element of the approach that (randomly) varied was the timing of the investment (before or after elections).

Our main results find that municipalities' investment incentives do not respond to variations in electoral timing generally. Cities and towns were no more likely to offer incentives to investments whose announcements were anticipated before elections compared to after – a null result estimated with relatively high precision. So, the experiment uncovered little evidence in support of the political-budget-cycle logic in the main. However, in exploratory subgroup analysis we find some evidence that municipalities seeking to attract manufacturing investment, the investment type of our confederate firm, are more likely to respond to and allocate dollars to an investment project that would be announced prior to an election. While focusing on manufacturing-seeking municipalities is the correct subsample for our analysis, this deviates from our original preregistration plan. We interpret this finding in two ways. First, we believe that additional nuance is in order in the understanding of the exertion of effort by politicians in periods prior to reelection. Only in the cases where an investment clearly maps onto a government's economic development strategy do we observe additional effort. Thus, existing works focusing on electoral cycles may be too broad to detect nuanced strategies. Second, given our results are far from conclusive, and this result was a non-pre-registered finding, caution is in order. Governments may be willing to exert this additional effort in periods prior to reelection, but this is a strategy choice made by an economic development agency on behalf of a government.

Results on the impact of direct elections on incentive allocations offers mixed evidence and are generally not supported in the data analysis. A final unanticipated result indicates that Republican-dominated or conservative municipalities are more likely to respond and offer incentives to potential investors. We can only offer conjectures on why this is the case, but this finding is robust across alternative specifications and worthy of future consideration.

In sum, this study makes a theoretical, empirical, and methodological contribution to the study of electoral cycles in shaping economic policy. We identify incentives as a policy lever that is not only under the control of politicians, but also delivers clarity of responsibility in a way that allows for credit claiming. Broadly, the results are mixed though with strong support in the most relevant sub-sample of interest.

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Tables

Table 1: Main Treatment Effects

Outcome	**Response**	
Condition	After Election Control	Before Election Treat.
N	1248	1252
Outcome Count	205	217
Mean	0.165	0.174
Difference from Control		0.009
p Value		0.546
Lower 95% Confid. Int.		-0.020
Upper 95% Confid. Int.		0.039

Outcome	**Incentive**	
Condition	After Election Control	Before Election Treat.
N	1248	1252
Outcome Count	122	126
Mean	0.098	0.101
Difference from Control		0.003
p Value		0.810
Lower 95% Confid. Int.		-0.020
Upper 95% Confid. Int.		0.027

Outcome	**ln(Dollars)**	
Condition	After Election Control	Before Election Treat.
N	1248	1252
Outcome Count	100	101
Mean	0.167	0.210
Difference from Control		0.041
p Value		0.419
Lower 95% Confid. Int.		-0.058
Upper 95% Confid. Int.		0.139

All analytical statistics estimated using difference-in-means tests.
Significance Level: **p < 0.05, ***p < 0.01

Table 2: Effects of Before-Election Treatment in Subgroup of Municipalities with Self-Identified Manufacturing Focus

	Model 1 Response	Model 2 Response	Model 3 Incentives	Model 4 Incentives	Model 5 Ln(Dollars)	Model 6 Ln(Dollars)
Before Election	0.423**	0.408	0.455**	0.470	0.470	0.437
	(0.213)	(0.226)	(0.228)	(0.242)	(0.282)	(0.289)
Ln(Population)		0.263**		0.234		-0.230
		(0.126)		(0.127)		(0.165)
Quarter 1		4.759		4.759		1.964
		(835.709)		(698.337)		(1.614)
Quarter 2		4.079		3.819		0.446
		(835.708)		(698.337)		(1.367)
Quarter 4		4.406		4.114		0.380
		(835.708)		(698.337)		(1.355)
Midwest		4.812		4.744		0.381
		(388.406)		(319.159)		(0.677)
South		4.729		4.621		0.638
		(388.032)		(318.377)		(0.705)
West		4.718		4.635		1.207
		(388.032)		(318.377)		(0.724)
Constant	-0.876***	-12.616	-1.129***	-12.360	0.167	1.464
	(0.158)	(922.400)	(0.173)	(767.489)	(0.199)	(2.068)
N	167	167	169	169	169	169
Pseudo R^2	0.021	0.087	0.025	0.081		
R^2					0.016	0.069

Note: Models 1-4 report Probit regression coefficients with standard errors in parentheses. Models 5 and 6 report OLS regression coefficients with standard errors in parentheses. Models 1-5 suggest significant treatment effects for the Before Election treatment on municipalities that, in an independent survey, self-reported a manufacturing focus for their economic development strategy. The 3rd Quarter is the omitted category for the quarterly dummies and the Northeast is the omitted category for the region dummies. Different comparison categories were used for these regressions due to the Northeast region's and the 3rd Quarter's collinearity with the dependent variables, which forces their omission as covariates. Results are substantively similar if relevant observations are dropped and other categories used as comparisons.[30] *Significance Level*: **p < 0.05, ***p < 0.01

[30] Models were estimated including the country-of-origin treatments but those coefficients are omitted here for simplicity's sake.

Table 3: *Response Rate* with Dummies for Elected, Quarter, Region, and State

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Elected	0.182**	0.123	0.098	-0.083	-0.305**	-0.275
	(0.074)	(0.074)	(0.130)	(0.082)	(0.146)	(0.172)
Ln(Population)		0.151***	0.145***	0.156***	0.150***	0.155***
		(0.031)	(0.032)	(0.032)	(0.033)	(0.034)
Quarter 1			-0.546***		-0.456***	-0.473**
			(0.174)		(0.179)	(0.220)
Quarter 3			-0.300		-0.300	0.094
			(0.225)		(0.229)	(0.307)
Quarter 4			-0.084		0.023	0.239**
			(0.065)		(0.069)	(0.116)
Northeast				-0.333***	-0.452***	
				(0.093)	(0.108)	
South				0.205***	0.176**	
				(0.080)	(0.086)	
Midwest				0.086	0.110	
				(0.080)	(0.087)	
Constant	-1.081***	-2.585***	-2.415***	-2.479***	-2.171***	-1.808***
	(0.076)	(0.317)	(0.342)	(0.348)	(0.382)	(0.596)
State Dummies	No	No	No	No	No	Yes
N	3075	3075	2656	3071	2652	2646
R^2	0.003	0.012	0.015	0.028	0.033	0.067

Notes: Coefficients of probit regressions. Dependent variable: 1 responded and 0 otherwise. Coefficients for fixed effects for individual states and the country-of-origin treatments were included in the regression but omitted here for simplicity in presentation. The 4th Quarter is the omitted category for the quarterly dummies and Region 4 (West) is the omitted category for the region dummies.
Significance Level: **p < 0.05, ***p < 0.01

Table 4: *Incentives Offered* with Dummies for Elected, Quarter, Region, and State

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Elected	0.212**	0.173	0.009	-0.054	-0.429**	-0.371
	(0.087)	(0.088)	(0.145)	(0.098)	(0.167)	(0.194)
Ln(Population)		0.098***	0.101***	0.115***	0.121***	0.118***
		(0.035)	(0.037)	(0.037)	(0.038)	(0.040)
Quarter 1			-0.537**		-0.400	-0.537
			(0.213)		(0.218)	(0.285)
Quarter 3			-0.155		-0.158	0.290
			(0.247)		(0.252)	(0.352)
Quarter 4			-0.068		0.070	0.276**
			(0.075)		(0.079)	(0.138)
Northeast				-0.260**	-0.372***	
				(0.116)	(0.134)	
South				0.363***	0.344***	
				(0.094)	(0.102)	
Midwest				0.239**	0.263**	
				(0.095)	(0.103)	
Constant	-1.472***	-2.452***	-2.255***	-2.572***	-2.275***	-2.234***
	(0.091)	(0.366)	(0.391)	(0.405)	(0.444)	(0.734)
State Dummies	No	No	No	No	No	Yes
N	3081	3081	2662	3077	2658	2576
R^2	0.004	0.008	0.009	0.032	0.035	0.086

Notes: Coefficients of probit regressions. Dependent variable: 1 incentive offered and 0 otherwise. Coefficients for fixed effects for individual states and country-of-origin treatments were included in the regression but omitted here for simplicity in presentation. The 4th Quarter is the omitted category for the quarterly dummies and Region 4 (West) is the omitted category for the region dummies.
Significance Level: **p < 0.05, ***p < 0.01

Table 5: *Logged Dollars* with Dummies for Elected, Quarter, Region, and State

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Elected	1.618**	1.666**	1.688	1.080	0.577	-0.037
	(0.673)	(0.689)	(1.249)	(0.814)	(1.426)	(1.727)
ln(Population)		-0.084	-0.022	-0.199	-0.187	-0.190
		(0.254)	(0.269)	(0.266)	(0.284)	(0.319)
Quarter 1			-1.096		-1.585	-2.223
			(1.628)		(1.660)	(2.274)
Quarter 3			-2.679		-3.196	-1.722
			(1.773)		(1.781)	(2.129)
Quarter 4			-0.399		-0.380	0.751
			(0.577)		(0.626)	(1.121)
Northeast				-1.346	-1.835	
				(0.919)	(1.081)	
South				-0.773	-1.181	
				(0.694)	(0.781)	
Midwest				0.083	0.096	
				(0.688)	(0.771)	
Constant	0.632	1.473	1.118	3.625	4.481	4.058
	(0.689)	(2.649)	(3.045)	(3.021)	(3.555)	(5.475)
State Dummies	No	No	No	No	No	Yes
N	236	236	211	236	211	211
R^2	0.025	0.025	0.022	0.042	0.051	0.234

Notes: Coefficients of OLS regressions. Dependent variable: logged dollars offered as incentives. Coefficients for fixed effects for individual states and country-of-origin treatments were included in the regression but omitted here for simplicity in presentation. The 2nd Quarter is the omitted category for the quarterly dummies and Region 4 (West) is the omitted category for the region dummies.

Significance Level: **p < 0.05, ***p < 0.01