Electoral Institutions and Electoral Cycles in Foreign Direct Investment: A Field Experiment on Over 3,000 U.S. Municipalities

Nathan M. Jensen, Associate Professor
Department of International Business
The George Washington School of Business
natemjensen@gwu.edu

Michael G. Findley, Associate Professor
Department of Government
University of Texas at Austin
Austin, TX 78712
mikefindley@utexas.edu

Daniel L. Nielson, Professor
Department of Political Science
Brigham Young University
Provo, UT 84604
dan_nielson@byu.edu

03 April 2016

Abstract:
Through a field experiment and audit study we test prominent arguments about the political-business and political-budget cycles. We explore how electoral timing, direct elections, and party composition affect local governments' offers of investment incentives to outside firms. To minimize deception, 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 suggests that election timing matters in this most likely set of locales. Some observational findings include mixed evidence on how direct elections of executives and the seasons in which elections occur are related to incentives. Finally, our evidence suggests that larger, Republican-controlled municipalities more readily offer investment incentives than their Democratic counterparts. Our results suggest limited support for political cycles in driving incentive policies, but uncover other political factors that shape economic development practices.

Keywords:
Electoral Cycles; Political-business Cycle; Political-budget Cycle; Financial Incentives; Foreign Direct Investment; Field Experiment; Pre-registration; Causal Effects; Internal Validity; External Validity

Acknowledgements

We thank Aaron Chatterji, Susan Hyde, Stephen Meier, and participants at various conference and workshop presentations, including the International Studies Association (2013), Midwest Political Science Association (2013), the International Political Economy Society (2013) for comments on the research design, and the Stanford Graduate School of Business for comments on this manuscript.

IRB Clearances were received on February 22, 2013 (BYU), April 2, 2013 (Washington University in St. Louis), and May 8, 2013 (UT-Austin IRB). The research design for this experiment was registered on July 31, 2013 with the Experiments in Governance and Politics registry as study [28] 20130731, which can be found at http://egap.org/design-registration/registered-designs/. Of the interventions registered, in this paper we report on Hypotheses 1 and 2. In other work (Chatterji et al. 2016), we report on the results for Hypothesis 3. Between the papers, we report fully on all aspects outlined in the original registered design document as stated. At the encouragement of some readers we added additional analyses, including the investigation of some interaction effects, and some other observational analyses including partisan effect, but note that we report them *in addition to* what was registered *not in place of*. Thus, the papers provide a full accounting of everything registered in the original July 31, 2013 design. We registered an addendum for a follow-up experiment (Numbered [51] 20140131 on January 31, 2014) and those findings will be presented elsewhere once analyzed.

Funding for this project was provided by Richard and Judy Finch and the Department of Political Science; the College of Family, Home, and Social Sciences; the David M. Kennedy Center for International Studies at Brigham Young University, the Weidenbaum Center and Center for New Institutional Social Sciences at Washington University in St. Louis, and the Bannister Chair at the University of Texas at Austin.

1. INTRODUCTION

Impending elections purportedly make politicians more short sighted and more likely to use economic policy to pander compared to when their careers are not so immediately and publicly at stake. Indeed, it seems logical that elected officials should especially attend to key constituency interests when voters are contemplating their removal than when elections are distant. This might naturally lead to increased spending on public services, tax breaks for valued supporters, or other economic policies that cater to powerful or vocal factions in the months leading up to an election. Some decry such behavior as inimical to the common good; others see this merely as evidence of democracy in action. Regardless, few problems cut so centrally at the question of whether or not representative government functions in the public interest.

Seminal studies in political science, economics, and public policy have theorized about so-called political-business or political-budget cycles and have argued 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 findings also face well-known challenges to causal inference using observational statistics, including potential confounds and omitted variables.

This article adopts a different empirical approach, employing the first pre-registered audit study and field experiment probing how electoral timing shapes a prominent lever of economic policy: in this case the use of targeted financial 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 U.S. cities spend in excess of $80 billion each year attempting to lure new investment or to reward existing firms for expanding their production (Story 2012). 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 could be negotiated between the firm and the municipality in a way that politicians presumably do what they can to assure that the credit for the use of these incentives is timed to the electoral calendar. Officials reputedly stage ribbon cutting ceremonies and photo opportunities at the endogenously chosen announcement date to enhance their electoral prospects. But this possibility has never been investigated in a systematic way capable of estimating the causal effects of election timing on incentives.

We develop a novel research design capable of causal identification to examine how the timing of elections and electoral institutions shape incentive offers. For the purpose of the study, we legally incorporated an actual financial-incentive consulting company that offered site location services for other firms. 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 these questions of how elections shape economic policy.

We test two links between elections and the offering of targeted financial incentives. First, we directly address the literature on political-budget cycles, which provides clear expectations on 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 hypothesized that whether a municipal government has a directly elected executive or an appointed executive may condition the use of incentives: directly elected executives should be more likely to overprovide incentives as a means of credit claiming for economic development.

The study produces two primary findings. Experimentally, municipalities randomly assigned to receive the possibility of an investment announcement in the few months preceding the next municipal election were not significantly more responsive to the firm's inquiry nor were they significantly more likely to offer incentives than municipalities assigned to a post-election announcement. In dollar terms, the offers were also statistically indistinguishable across experimental conditions. These null results are precisely estimated. Municipalities that identified manufacturing as an economic development focus, however, were significantly more likely to respond and offer incentives if they were assigned to the before-election treatment compared to the after-election control.[1] This effect was unanticipated (and not part of our registration prior to fielding our survey) and thus we are hesitant to offer any strong conclusions from this subgroup effect. Our primary results indicate no impact of election timing on incentive offers.

We believe that this is a notable null result for a large literature that emphasizes the timing of various incumbent economic-policy behaviors relative to elections.[2] Our dependent variable was derived from the study's unobtrusive audit of actual municipal incentives presented to a real potential investor and therefore offers strong internal and external validity, especially compared to most prior studies of investment incentives and investigations of political-budget cycles.

Another primary finding indicates mixed results that municipalities with direct elections for mayor, as opposed to appointed municipal executives, on the impact of incentives. Our evidence on the impact of elections varies by specification. These results are observational, as we obviously cannot manipulate form of government.

In the final part of our paper we deviate from the preregistration plan and examine other relationships in the rich set of data generated from the study. The most striking pattern in the data finds that municipalities with Republican leanings are significantly more active in offering incentives than Democratic municipalities. This result is robust across many, though not all, additional specifications.

---

[1] We thank Alex Debs and Jonathan Rodden for suggesting this alternative test.

[2] This is especially relevant for the political-business cycle and political-budget cycle literatures we address below.

In what follows, we first review relevant theoretical considerations, including those that motivated the research design and pre-analysis plan. We then outline the experimental context and protocol. In the results that follow, we first report on what we preregistered ahead of conducting the experiment, followed by a discussion of additional possibilities that we investigated after the study's preregistration and execution.

2. ELECTORAL INCENTIVE CYCLES

Economic performance is so important to incumbent politicians that 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).[3] A variant of this literature has focused on political-budget cycles, where governments expand spending in the period prior to elections (Rogoff 1990; Persson and Tabellini 2003; Brender and Drazen 2005; Alt and Lassen 2006). We extend the work on political budget cycles to include the use of targeted financial "incentives" to attract direct investment in the United States.

As argued by Alt and Rose (2007), focusing the study in the United States allows us to hold many contextual factors constant, allowing for a clearer comparison across local governments. In our policy area of firm-specific incentives, essentially every city has the ability to offer some form of incentives. Moreover, we argue that the form of government is the key contextual factor that should affect the political benefits related to the use of

[3] 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)

incentives. The rich institutional variation in the U.S. can thus be used to examine how democratic institutions affect economic policy (Besley and Case 2003).

An increasingly important tool of economic policy is investment incentives. 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 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]

---

[4] Klein and Moretti (2013) provide and 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).

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] 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 mostly easily achieved when spending is "visible and easily targeted" (de Haan and Klomp 2013, 389).

Jensen et al (2014) argues 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

---

[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)

"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 add to this work, but in a way that broadens 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. 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 how firm-specific fiscal policies (incentives) can be used either for the *purpose* of job creation or for the *perception* of job creation.

How do elites make choices about the provision of incentives for the purpose of job creation? Ideally, politicians would use obvious cost-benefit analysis metrics, attempting to ascertain how much incentives cost relative to the number of jobs created,

[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) finds evidence for credit claiming and even stronger evidence for blame avoidance in how voters evaluate investment and incentives policies.

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

and consequently the degree to which the incentives propel economic growth.[12] And yet the research on investment incentives suggests that there may be a political logic that trumps the economic calculus. One possibility is that politicians use incentives to claim credit for investment that was already coming to their district (Jensen, Malesky and Walsh 2015). Voters would thus reward politicians for violating simple cost-benefit metrics, giving politicians more credit for investment that occurred alongside – without necessarily being caused by – incentives.

Our first hypothesis considers that the political 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 expanding fiscal policies in the period prior to elections. 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.

This theoretical link between elections and fiscal policy has generated a number of influential theories, but mixed empirical results. For example, de Haan and Klomp (2013) review the now extensive cross-national empirical evidence on the existence of political-business cycles and conclude that the results are mixed and the possible mechanisms far from established. One potential reason for differences in the empirical results may be that the theoretical models of political-budget cycles are very nuanced. A number of influential

[12] Jensen, Malesky and Walsh (2015) document that a large number of cities do not conduct a cost-benefit analyses of incentives.

works, such as Rogoff and Sibert (1998) and Persson and Tabellini (2003), focus on the use of fiscal policy to exploit imperfect information, where incumbents attempt to employ fiscal policy to signal competence. Other models, such as those emphasizing the use of fiscal policy for redistribution to favored groups, could be tied to the electoral cycle but would have different empirical implications. A second limitation is that variation across countries in policies, institutions, and economic conditions make not only the detection of cycles difficult, but suggest that the very effectiveness of these policies is context dependent (Franzese and Jusko 2006). Every government may be engaging in political-budget cycles, but the form and magnitude could be sufficiently heterogenous that identifying cycles in cross-national studies is extremely difficult.

Although most of this work has focused on political-budget cycles by national level politicians, some work has also explored cycles in subnational elections. In countries as diverse as Canada (Kneebone and McKenzie 2001), India (Khemani 2004), Italy (Cioffi et al 2012), the United States (Alt and Rose 2007) and even non democratic regimes such as China (Guo 2009), Egypt (Blades 2011), and Malaysia (Pepinsky 2007), scholars have found mixed evidence for the manipulation of fiscal policy based on the electoral calendar.

These theoretical works and empirical tests consider the manipulation of policies in periods prior to elections as key to utilizing political-budget cycles. In our context we explore how politicians can harness the location decisions of firms for electoral gain. 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 in the quarter prior to elections ought to be more valuable to politicians than investment that will be announced post-election.

In the example related to our study, a firm could announce its intentions to invest in a municipality two months prior to an election or one month after. In both scenarios the actual timing of the announcement would likely have a minimal impact on job creation and capital invested. In most cases this timing decision is in the same fiscal year, having no impact on the actual cost of incentives for the government. Thus, the key difference is when the information is revealed to the public.

We argue that the context enables a clear test of political-budget cycles in which we can examine the willingness of politicians to provide incentives to firms pre- and post-election and estimate the size of these incentives across periods.[13]

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 the next election rather than following the election.

Hypothesis 1 develops the key experimental intervention that we are able to randomize and test. It is of course predicated on an institutional framework that allows for elections. 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-

---

[13] Some studies of political business cycles, such as Schultz (1995) and Alt and Rose (2007) account for the level of popularity of incumbents. See Dubois (2016, 242-244) for a review. These studies theorize that the incentives for PBCs increase vary according to the reelection prospects of incumbents. Unfortunately, approval and voting data for our 3,000 municipalities are not readily available.

manager systems (See Feiock et al (2003)). In the former, mayors are directly elected by voters, which leads to direct electoral pressures on mayors (Vlaicu and Whalley 2014).

We thus contend that politicians who are subject to electoral pressures are more likely to offer more and bigger incentives. Along with others, we argue that incentives are more likely to be provided by directly elected mayor-council governments (Jensen, Malesky, and Walsh 2015). This particularly helps facilitate in the claiming of credit for investment.

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. STUDY DESIGN

3.1 Study Context

Our study focuses on the use of incentives by U.S. municipalities. The United States is unique compared to most developed and developing countries in its 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.[14] This largely leaves economic development policies to the state and local levels, and a key policy is the provision of incentives for investment promotion. Although the measurement of incentives is notoriously tricky, some have estimated that subnational incentives in the U.S. total as much as $80 billion per year (Story 2012).

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 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).

3.2 Experimental Protocol

Given our hypothesis that electoral pressures may affect economic incentives, we could not approach mayoral economic development offices as researchers, which likely

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

would have induced debilitating social desirability bias. Instead, we needed 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).[15] Perhaps the most important element, and also the most challenging, is that subjects must not know they are participating in the experiment. In "Get Out the Vote (GOTV)" experiments in American politics, for example, researchers attempt to mobilize potential voters who are not aware they are participating in a study.[16]

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

[15] Field experiments also preserve many of the internal-validity advantages of lab experiments: because the experimental conditions are randomly assigned and thus in expectation all observable and unobservable confounds are balanced across conditions. Experiments can therefore reveal causal effects rather than mere statistical correlations. Moreover, by occurring in a natural setting where subjects behave in their normal day-to-day routines and do not know they are being studied, field experiments also provide strong advantages in ecological validity relative to survey experiments (Findley et al 2015).

[16] Alternatively, in other field experiments subjects are made aware but only as part of what feels like their normal everyday routine. As some international development organizations implement development programs, for example, villages are randomly assigned to treatment after being selected in a lottery process of which they are aware.

operations of its existing plants. Our investment proposal exactly matched the real proposal given to us by the client and thus sharply decreased deception.[17]

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.[18] We created a company website and a board of consultants – all academics willing to lend their names for the purposes of the experiment.[19]

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

---

[17] 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 and to vary attributes about our consulting company, including framing our assigned company team as representing U.S., Japanese, or Chinese firms. Thus while our study introduces interesting variation to provide insights into the decision-making process of municipal officials and investment promotion agencies, 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.

[18] Having a real investor provide us information on prospective investment raises the question of whether the researcher eventually would be part of the negotiation of an incentive offer between a municipality and the firm. To guard against this, we both made clear in our approach email that all negotiations would be between the municipal and the client firm and that we were only collecting information at the beginning of the process. We also made clear through our IRB process that our consulting company was built solely for research purposes and does not collect any fees or generate any revenues.

[19] Nathan Jensen 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.

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 lead some larger municipalities to ignore the inquiry because it was from a small investor, and thus this may suppress the response rate among large municipalities. On the other hand, the relatively small firm size is a more realistic possibility for the many small municipalities contacted in the study, 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.[20] 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 randomized the timing of the investment announcement, proposing a date either two

[20] 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.

months before or one month after the next local elections. More specifically, we randomized the timing of the election for all municipalities for which we could find election dates. For all other municipalities, we randomized the dates.[21] This was an especially conservative choice, where 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 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.

Second, as authorized by our client, we varied the implied country of origin for the investment, which we report extensively elsewhere (Chatterji et al 2016).[22] The following email prompt shows the precise wording where the elements in boldface are the three potential country sources and 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.

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

[21] For municipalities without mayoral election dates were treated with one of four dates corresponding to the four quarters of the calendar year.

[22] This treatment included different treatments for different groups in the first paragraph of our email to the municipalities. The second aspect of this treatment is the email signature. Municipalities that were treated with "China", for example, were contacted by a research assistant that signed off as part of the "China Client Team".

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

To achieve balance across different treatment groups, 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.

3.3 Preregistration of the Research Design

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 request. We registered substantial information including our names, affiliations, contact information, study background, hypotheses, expected analysis procedures, and who would carry out the research. Because we preregistered the design, we pre-committed to report certain interventions and results regardless of the outcome. We thus turn to a discussion of the proposed analysis we preregistered.

4. PRE-REGISTERED ANALYSIS

Following from the pre-analysis plan, we focus on three outcome indicators. First, we examined response rates to our inquiry in the form of filling out the online web form. 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 1 we present difference-in-means results for the randomized treatment conditions compared to control. We note that only one of the comparisons – for a single condition on one outcome – the Japan country-of-origin treatment on the response outcome – is statistically significant at conventional levels ($p = 0.086$). We add that, given the nine comparisons reported, in expectation roughly one of the results should have shown statistical significance at the 0.1 level by chance alone. We therefore note that this result is tentative at best and also not fully robust to alternative regression specifications reported below (compare, for example,

Table 2). We do not discuss the country of origin results in this study (see instead Chatterji et al. 2016 2016), but we do present the results for the interested reader.

<Insert Table 1 about here>

<Insert Figure 1 about here>

None of the other treatments caused significant changes to any of the three outcome measures. Of 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. Here, the small confidence intervals bounding the minimal effects provide helpful guides for interpretation. For example, the estimated 0.003 effect (Cohen's $d = 0.012$) of the electoral-timing treatment on incentive offered is precisely estimated: it might be as high as .027 or as low as -.020 (representing a change in either direction of roughly one fourth from the base rate or an effect size/Cohen's d of 0.079), but it is unlikely to be larger. This suggests that, even if statistical power were dramatically increased and statistically significant effects appeared, they would very likely be small substantively. This is true for the effects of electoral timing (mean difference = 0.009, Cohen's $d = 0.025$) on response rate as well; even with much greater power any substantive effect would in all likelihood be relatively small (between .039 and -.020, or a change of less than one fifth from the base rate equal to an upper effect size/Cohen's d of 0.078). The effect of electoral timing on the log of dollars in inducements is less precisely estimated, which is likely the result of the high variance in the monetary values of offered incentives, but the effect size is nonetheless small, with a Cohen's d of .037 and the 95 percent confidence intervals suggesting an upper effect size of 0.078. The range of effects sizes suggested by 95 percent confidence intervals of less than 0.08 across all three dependent variables indicates very precisely estimated null findings.

These main results are robust to a wide variety of alternative statistical analyses. Most notably, in Table 2 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. We present our results on the country of origin for completeness, but note that these results have been discussed at length in Chatterji et al. (2016).

These null findings may be important for reflecting on whether the electoral calendar results in sub-optimal economic policymaking before elections. The manipulation was relatively subtle, having randomly assigned subjects to receive a cue that the investment announcement would occur either two months before or one month after the date of the next mayoral election. It is therefore possible that the intervention was too understated for the average city official to note. However, as described below, both municipalities that – in a separate survey – self-identified as focusing on manufacturing in their economic development strategy and municipalities in the Northeast region appeared sensitive to the intervention, suggesting that the treatment was strong enough to provoke responses among key subgroups. In speculating about the reason for the null result, to our minds the most likely is that municipalities on average simply have a standard incentive offer they propose to all inquiries regardless of the particulars. Across the subject pool generally this may indicate a relatively high level of professionalism and non-discrimination in municipalities' interaction with (small) potential investors.

<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.[23] 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 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.

<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.

[23] 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).

One interesting finding is that, although we see no clear evidence of electoral cycles, we do find a significant calendar effect. Government officials were much more likely to respond to queries for investors that would announce their investment in the first quarter, and less likely to respond in announcements in the fourth quarter. We can only speculate on the meaning of this result, but it is important to note that this is unrelated to the timing of election in our municipalities.

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.[24]

<Insert Table 4 about here>

<Insert Table 5 about here>

To summarize, we found little support for the propositions that local elections, election timing, or country of origin shape municipalities offered investment incentives. Only electoral institutions have some impact on incentives, but this finding varies wildly based on the specification. What then explains the promotion of investment incentives?

5. NON-PREREGISTERED ANALYSIS

5.1 Probing Possible Effects of Municipalities With a Manufacturing Focus

Given the results thus far are largely null, we decided to conduct a validity check that we had not anticipated and thus not preregistered. If the email approach was plausible, then we should expect that municipalities that prioritize manufacturing jobs should be more likely to respond and offer incentives. If even manufacturing municipalities did not express interest, then it suggests that perhaps the email approach was not on target. To test this, we include a dummy variable if the municipality indicated a focus on manufacturing in the ICMA/ICL survey, which was fielded to economic development professionals and

[24] 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/ILC 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.

provides some of the highest quality information on economic development goals, conditions on the ground, and attributes of the local communities. 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. In these three models we find strong support that municipalities with a manufacturing focus were more likely to respond to our inquiry.

<Insert Table 6 about here>

Given the strong relationship between having a manufacturing focus and responding as well as offering incentives, we next consider whether the electoral cycles treatment stated in Hypothesis 1 holds in a sub-group analysis using only those municipalities that claimed the manufacturing focus. Note that the number of municipalities that completed the ICMA/ICL 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. Municipalities with a manufacturing focus also appeared to offer more lucrative incentives (at the 0.1 level of significance) in the treatment group, but this result is not robust to the inclusion of control covariates in regression analysis. See Table 7, which reports the subgroup effects of the Before Election treatment across the three outcomes. As shown, the result is largely robust across model specifications. These results also hold in regression analysis with interaction terms, but we opt here for the more conservative estimation and note that interaction models may face difficult methodological challenges (Hainmueller et al. 2016).

<Insert Table 7 about here>

Because municipalities could only offer incentives if they responded in the first place, these results are subject to potential selection bias and require alternative specifications to check robustness. Identification of a two-stage model is difficult in this case, however, given the challenges in locating an instrument that predicts response but not incentive offered (except through the response mechanism) and that thus satisfies the exclusion restriction. We employed two strategies. First, the results are robust to a selection model whose assumptions allow for the same independent variables to predict both the selection and the outcome stages (Sartori 2003). Second, we employed multinomial probit specifications with three alternative outcomes: municipalities could decide not to respond (coded 0), to respond and decline the request (1), or to offer incentives (2). The fact that municipal officials likely decided how to respond – with incentives or not – at the same moment they decided whether or not to respond helps to justify employing the multinomial model here. And, indeed, the results suggest that only the municipalities that offered incentives were significantly more compliant in the treatment group compared to those not responding and to those responding but declining the request. The results of the selection model and multinomial probit are reported in Appendix Tables A1 and A2.

In retrospect it perhaps should not surprise that, if the Before Election condition provoked a response anywhere, it would be in the municipalities explicitly purporting to pursue a manufacturing strategy matching our confederate firm's profile. This provides some evidence that the treatment was strong enough to cause an effect in the most likely set of cases.[25]

5.2 Partisanship as a Possible Explanation for Incentives

We now consider the independent effects of partisanship, a powerful explanatory variable in the U.S. political context. In Table 6 above 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

[25] We preformed a number of additional subgroup analyses that had little impact on our substantive findings. To further check robustness of the main findings, we probed for additional subgroup effects. The Before Election treatment does not appear to have a significant effect on cities with elections immediately pending either within one year or two years. Likewise, neither larger nor smaller cities in terms of population appear significantly more sensitive to the Before Election condition across all three outcomes. 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.

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 level of support for President Obama, 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. A more modest change from a municipality with support for President Obama at half a standard deviation above the mean to support for President Obama half a standard deviation below the mean, the probably of a response increases from 18.51% to 26.53%, or a 30% increase in the response rate. Thus moving from a Democratic to a Republican municipality appears to have a substantial impact.

We explore the robustness of this result in Table 8 with a model that allows for a larger sample size and an alternative measure of partisanship. In Models 1, 3, and 5 we test the impact of partisanship on our three dependent variables controlling for population, directly elected politicians, and state fixed effects. In all three models we achieve samples sizes of roughly 1,000 municipalities (which is largely limited by the partisanship data). For this second measure of partisanship (Models 2, 4, and 6), we utilize Tausanovitch and Warshaw (2013)'s measure of local level partisanship using item response theory based on survey items on the policy preferences of 275,000 voters. This measure of constituency preferences ranges from -1 (most liberal) to 1 (most conservative) and is highly correlated with our measure of presidential vote share. Additional variations of the partisanship measure are reported in Appendix Table A3 (Appendix D) and demonstrate the robustness of this measure.

<Insert Table 8 about here>

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. The finding that municipalities whose majorities of citizens are Republicans are more likely to offer incentives may sound intuitive. 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. Thus we believe that this finding, while observational, provides a new insight into the offering of incentives.[26]

6. CONCLUSION

[26] To explore other possible explanations, we included a variable on the evaluation of the previous 5 years of local economic performance on a 1-7 scale. While our priors were that more distressed communities would be more willing to offer an incentive, we simply find no evidence of this in the results. We also coded an alternative measure, which is a projection of the expected growth in the next 5 years. The results remain unchanged. We find no impact of economic growth on incentive responses or dollars.

Finally, we include a dummy measure for mayor-council institutions. We find mixed evidence on mayor council-institutions. Mayors are not associated with more responses, but we do see that mayors offer more dollars in incentives, which is consistent with the observational findings of Jensen, Malesky and Walsh (2015). We are cautious in our interpretation of this result since a few outliers appear to be largely driving it.

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. Building on existing work on electoral pandering along with political-budget cycles we examined whether politicians are motivated to provide more incentives in periods prior to reelection (Hypothesis 1) and more generally when the politicians face direct elections rather than appointment (Hypothesis 2).

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, sectors, and from different home countries.

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 elements of the approach that (randomly) varied were the timing of the investment (before or after elections) and the investing firm's perceived country of origin.

As part of our commitment to an ethical experiment and scientific accumulation, not only were we careful in minimizing deception, but we also pre-registered the protocol and analysis plan. Our specific hypotheses were pre-specified before the fielding of the experiment, and experimental protocols and coding decisions were made prior to data collection.

The main results reporting the pre-registered analysis are largely null findings. Hypotheses on both the electoral timing (experimental) and direct elections (observational) were not supported in the data analysis. We found that municipal leaders and economic development professionals had very similar responses to firms before or after elections and limited evidence that the form of municipal government had an impact on the allocation of incentives. Though not anticipated in the pre-analysis plan, we do find significant and largely robust effects for the subgroup of municipalities focused on manufacturing, which responded to inquiries and offered incentives at significantly higher rates for investments before compared to after elections

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 subsequent studies.

What should we make of the findings on the whole? Although it might be tempting to view the null results as largely uninformative, we caution against such an interpretation. We emphasize instead that the hypotheses originated in well-established literatures on political-business and political-budget cycles. In light of existing expectations about incumbent political behavior, precisely estimated null results from a large-scale randomized natural field experiment raise important questions – and potentially have serious implications – for standard models. Indeed, it may be the case that political institutions and business cycles are less important than underlying partisan dynamics (at least in the United States). Taken together, we expect that research on the political incentives attracting direct investment could benefit from more refinement coupled with more rigorous methodological examination.

The fact that the first field experiment to directly test observable implications of the political-business and -budget cycles recovered null results bounded by relatively tight confidence intervals should weigh heavily in the empirical literature on the question. Of course, it might well be the case that electoral timing may matter in other economic-policy domains. This experiment targeted only a single – albeit important – policy area, so concerns for external validity suggest caution in generalizing beyond investment incentives.

Still, the study reports some of the best-identified findings on political-business and -budget cycles to date and therefore provides a foundation on which other experimental and quasi-experimental research can build. In expectation, experiments are unbiased, so results can effectively be pooled with other relevant research in later meta-analysis. Future research should consider other notable economic policies that might be consciously manipulated according to the election calendar. While many of these policies may not be susceptible to experimental intervention, future research might seek other sources of exogeneity, including application deadlines or threshold criteria in regression discontinuity or as-if-at random variation in regional or national economic crises to which policies might respond differently contingent on election timing.

There are many possibilities. The key will be greater attention to causal identification. If results continue to accumulate suggesting that election timing matters only for a narrow set of politicians in a circumscribed group of locales and not in the main, this may be read as good news for the functioning of democratic processes.

REFERENCES

Alt, James E, and David D. Lassen. 2006. Transparency, political polarization and political-budget cycles in OECD countries. *American Journal of Political Science* 50: 530-550.

Alt, James E., Shana S. Rose. 2007. Context-conditional political-budget cycles. In C. Boix & S. C. Stokes (Eds.), The Oxford handbook of comparative politics. Oxford: Oxford University Press.

Bartik, Timothy J. 2005. Solving the problems of economic development incentives. *Growth and Change* 36 (2): 139–166.

Besley, Timothy and Anne Case. 2003. Political competition and policy choices: evidence from the United States. *Journal of Economic Literature* 41: 7-73.

Blades, Lisa. 2011. *Elections and Distributive Politics in Mubarak's Egypt.* Cambridge, UK: Cambridge University Press.

Brender, Adi and Allan Drazen. 2005. Political budget cycles in new versus established democracies. *Journal of Monetary Economics* 52 (7): 1271-1295.

Buss, Terry F. 2001. The Effect of State Tax Incentives on Economic Growth and Firm Location Decisions: An Overview of the Literature. *Economic Development Quarterly* 15 (1): 90–105.

Canes-Wrone, Brandice and Jee-Kwang Park. 2012. Electoral Business Cycles in OECD Countries. *American Political Science Review* 106: 103-122.

Chatterji, Aaron, Michael Findley, Nathan M. Jensen, Stephan Meier, and Daniel Nielson. 2016. Field Experiments in Strategy. *Strategic Management Journal*. 37 (1): 116-132.

Cioffi, Marika, Giovanna Messina and Pietro Tommasino. 2012. Parties, institutions and political-budget cycles at municipal level: evidence from Italy. Working Paper.

de Haan, Jakob and Jeroen Klomp. 2013. Conditional Political Budget Cycles: A Review of Recent Evidence. *Public Choice* 157: 387-410.

Dubois, Eric. 2016. Political Business Cycles 40 Years after Nordhaus. *Public Choice* 166: 235-259.

Easson, Alex. 2004. *Tax Incentives for Foreign Direct Investment.* The Hague: Kluwer Law International.

Feiock, Richard C., Moon-Gi Jeong, and Jaehoon Kim. 2003. Credible Commitment and Council-Manager Government: Implications for Policy Instrument Choices. Public Administration Review 63: 616-25.

Findley, Michael G., Daniel L. Nielson, and J.C. Sharman. 2013. "Using Field Experiments in International Relations: A Randomized Study of Anonymous Incorporation." *International Organization* 67(4): 657-693.
Findley, Michael G., Brock Laney, Daniel L. Nielson, and J.C. Sharman. 2015. "Deceptive Studies or Deceptive Answers: Competing Field and Survey Experiments of Anonymous Incorporation." Unpublished Manuscript: University of Texas at Austin.
Fox, William F. and Matthew N. Murray. 2004. Do Economic Effects Justify the Use of Fiscal Incentives? *Southern Economic Journal* 71, no. 1 (2004): 78–92.
Franzese, Robert J. 2002. Electoral and partisan cycles in economic policies and outcomes. *Annual Review of Political Science*, 5, 369–421.
Franzese, Robert J., and Karen Jusko. 2006. Political-economic cycles. In D. Witten & B. Weingast (Eds.), Oxford handbook of political economy. Oxford: Oxford University Press.
Gerber, Alan, and Don Green. 2012. *Field Experiments: Design, Analysis, and Interpretation*. W.W. Norton.
Guo, Gang. 2009. China's Local Political Budget Cycles. *American Journal of Political Science* 53 (3): 621-632.
Grimmer, Justin Solomon Messing, and Sean J. Westwood. 2012. How Words Cultivate the Personal Vote: The Effect of Legislator Credit Claiming on Constituent Credit Allocation. *American Political Science Review* 106 (4): 703-719.
Hainmueller, Jens and Mummolo, Jonathan and Xu, Yiqing, 2016. How Much Should We Trust Estimates from Multiplicative Interaction Models? Simple Tools to Improve Empirical Practice. Working Paper. http://ssrn.com/abstract=2739221 or http://dx.doi.org/10.2139/ssrn.2739221
Harrison, Glenn W., and John A. List. 2004. Field Experiments. *Journal of Economic Literature* 42: 1009-1055.
Hibbs, Douglas. 1977. Political parties and macroeconomic policy. *American Political Science Review* 71:1467-1487.
Hyde, Susan. 2010. "The Future of Field Experiments in International Relations." *Annals of the American Academy of Political and Social Science* 628: 72-84.
International City/County Management Association and National League of Cities. 2009. *Economic Development 2009 Datasets*. http://bookstore.icma.org/Data_Sets_C42.cfm
Jensen, Nathan M., Edmund Malesky, Mariana Medina, Ugur Ozdemir. 2014. Pass the Bucks: Credit, Blame and the Global Competition for Investment. *International Studies Quarterly* 58 (3): 433-447.
Jensen, Nathan M., Edmund Malesky, and Matthew Walsh. 2015. Competing for global capital or local voters? The politics of business location incentives. *Public Choice* 164 (3): 331-356
Khemani, Stuti. 2004. Political cycles in a developing economy: effect of elections in the Indian states. *Journal of Development Economics*, 73, 125–154.
Klein, Patrick, and Enrico Moretti. 2013. People, places and public policy: some simple welfare economics of local economic development programs. National Bureau of Economic Research Working Paper 19659.

Kneebone, Ronald, and Kenneth McKenzie. 2001. Electoral and partisan cycles in fiscal policy: an examination of Canadian provinces. *International Tax and Public Finance*, 8, 753–774.

LeRoy, Greg. 2005. *The Great American Job Scam: Corporate Tax Dodging and the Myth of Job Creation*. San Francisco: Berrett-Koehler Publishers.

Nordhaus, William. 1975. The political-business cycle. *Review of Economic Studies* 42:169-190.

Patrick, Carlianne Elizabeth. 2014. Does Increasing Available Non-Tax Economic Development Incentives Result in More Jobs? *National Tax Journal* 67 (2): 351–386.

Pepinsky, Thomas. 2007. Autocracy, Elections, and Fiscal Policy: Evidence from Malaysia. *Studies in Comparative International Development*. 42 (1-2): 136-163.

Persson, T. and Tabellini, G. 2003. *The Economic Effects of Constitutions*. Cambridge, MA: MIT Press.

Peters, Alan and Peter Fisher. 2004. The failures of economic development incentives. *Journal of the American Planning Association,* 70(1): 27–37.

Rogoff, Kenneth. 1990. Equilibrium Political Budget Cycles, American Economic Review 80: 21-36.

Rogoff, Kenneth, and Anne Sibert. 1998. Elections and Macroeconomic Policy Cycles. *Review of Economic Studies* 55(1): 1-16.

Sartori, Anne E. 2003. "An Estimator for Some Binary-Outcome Selection Models without Exclusion Restrictions." *Political Analysis* 11(2).

Schultz, Kenneth A. 1995. The Politics of the Political Business Cycle. *British Journal of Political Science* 25 (1): 79-99.

Story, Louise. 2012. As Companies Seek Tax Deals, Governments Pay High Price. *New York Times*. Dec 1, 2012. < http://www.nytimes.com/2012/12/02/us/how-local-taxpayers-bankroll-corporations.html>

Tausanovitch, Chris, and Christopher Warshaw, 2013. Measuring Constituent Policy Preferences in Congress, State Legislatures, and Cities. *The Journal of Politics* 75 (2): 330-342.

Thomas, Kenneth P. 2011. *Investment Incentives and the Global Competition for Capital*. New York: Palgrave Macmillian.

Vlaicu, Razvan and Alexander Whalley. 2014. Hierarchical accountability in government. SSRN Working Paper No. 1925005.

Weber, Rachel and Sara O'Neill-Kohl. 2013. The Historical Roots of Tax Increment Financing, or How Real Estate Consultants Kept Urban Renewal Alive. *Economic Development Quarterly* 27 (3): 193-207.

Wells, Louis T., Nancy J. Allen, Jacques Morisset, and Neda Pirnia. 2001. "Using Tax Incentives to Compete for Foreign Investment: Are They Worth the Costs?" FIAS Occasional Paper 15.

Zee, Howard H., Janet G. Stotsly and Eduardo Ley. 2002. "Tax Incentives for Business Investment: A Primer for Policy Makers in Developing Countries," *World Development* 30 (9): 1497–1516.

TABLES

Table 1: Main Treatment Effects

Outcome			**Response**		
Condition	After Election Control	Before Election Treat.	US Control	Japan Treat.	China Treat.
N	1265	1261	1042	1027	1043
Outcome Count	207	218	183	152	165
Mean	0.164	0.173	0.176	0.148	0.159
Difference from Control		0.009		-0.028	-0.017
p Value		0.535		0.086*	0.291
Lower 95% Confid. Int.		-0.020		-0.060	-0.049
Upper 95% Confid. Int.		0.039		0.004	0.015

Outcome			**Incentive**		
Condition	After Election Control	Before Election Treat.	US Control	Japan Treat.	China Treat.
N	1265	1261	1042	1027	1043
Outcome Count	123	127	102	101	87
Mean	0.097	0.101	0.098	0.098	0.083
Difference from Control		0.003		0.000	-0.014
p Value		0.770		0.972	0.250
Lower 95% Confid. Int.		-0.020		-0.025	-0.039
Upper 95% Confid. Int.		0.027		0.026	0.010

Outcome			**ln(Dollars)**		
Condition	After Election Control	Before Election Treat.	US Control	Japan Treat.	China Treat.
N	1265	1261	1042	1027	1043
Outcome Count	101	101	83	71	83
Mean	0.164	0.211	0.157	0.157	0.166
Difference from Control		0.047		-0.001	0.009
p Value		0.349		0.992	0.866
Lower 95% Confid. Int.		-0.051		-0.098	-0.092
Upper 95% Confid. Int.		0.145		0.097	0.110

All analytical statistics estimated using difference-in-means tests.
Significance Level: *p < 0.1, **p < 0.05, ***p < 0.01

Figure 1: Treatment Effects for Before Election on Incentives Offered

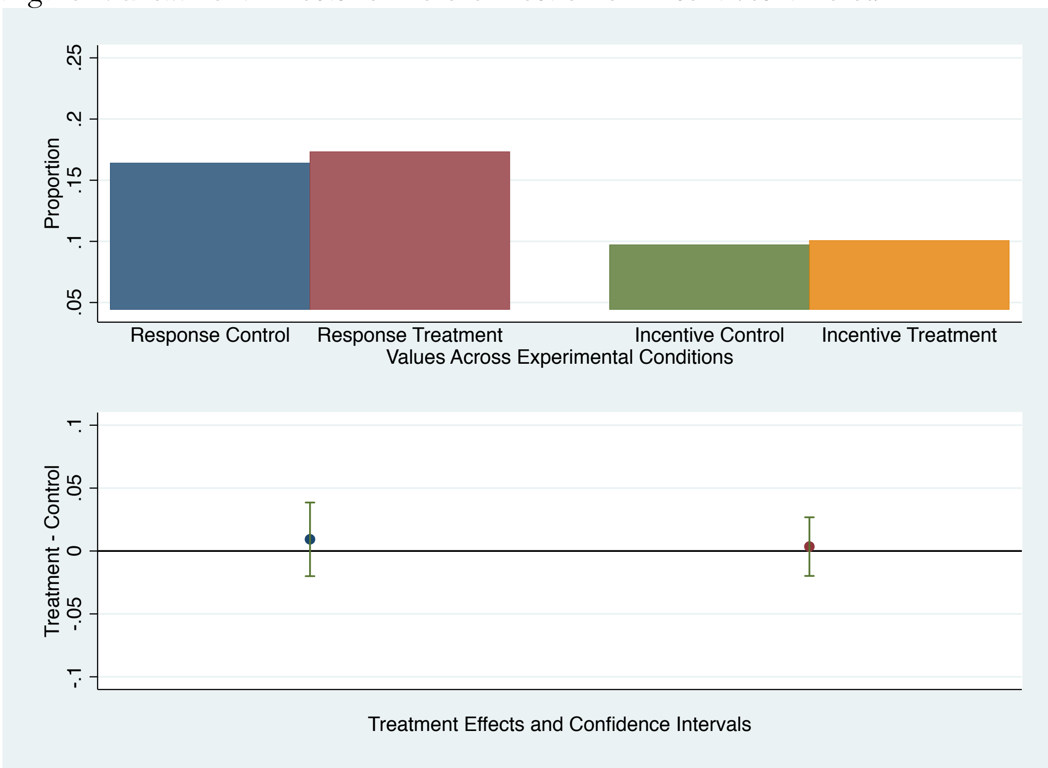

Table 2: Response Rate, Incentive Offered, and Logged Dollars with Treatments and Main Control Variables

	Response		**Incentive Offered**		**Ln(Dollars)**	
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Before Election	0.027	0.032	0.012	0.025	0.471	1.026
	(0.060)	(0.062)	(0.070)	(0.073)	(0.554)	(0.627)
Japan	-0.120	-0.122	0.015	0.012	0.297	0.279
	(0.074)	(0.076)	(0.085)	(0.088)	(0.677)	(0.762)
China	-0.071	-0.071	-0.065	-0.073	0.500	0.725
	(0.073)	(0.075)	(0.086)	(0.089)	(0.662)	(0.715)
Ln(Population)	0.151***	0.153***	0.122***	0.116***	-0.210	-0.275
	(0.033)	(0.034)	(0.038)	(0.040)	(0.291)	(0.326)
Quarter 1	-0.508***	-0.513**	-0.484**	-0.628**	-1.755	-2.076
	(0.187)	(0.231)	(0.237)	(0.311)	(1.842)	(2.310)
Quarter 3	-0.303	0.099	-0.147	0.287	-3.271*	-1.659
	(0.229)	(0.309)	(0.253)	(0.356)	(1.821)	(2.164)
Quarter 4	0.025	0.260**	0.092	0.294**	-0.274	1.126
	(0.072)	(0.122)	(0.082)	(0.145)	(0.648)	(1.182)
Northeast	-0.480***		-0.382***		-1.460	
	(0.112)		(0.140)		(1.152)	
South	0.178**		0.353***		-1.143	
	(0.087)		(0.103)		(0.798)	
Midwest	0.110		0.269***		0.103	
	(0.087)		(0.104)		(0.786)	
Constant	-2.508***	-2.092***	-2.752***	-2.615***	4.874	7.186
	(0.374)	(0.592)	(0.437)	(0.731)	(3.471)	(5.484)
State Dummies	No	Yes	No	Yes	No	Yes
N	2516	2502	2522	2440	202	202
Pseudo R^2	0.034	0.066	0.036	0.087		
R^2					0.045	0.240

Notes: Coefficients of Probit models (for response rate and incentive offered) and OLS regression (for logged dollars). 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 < 0.1, **p < 0.05, ***p < 0.01

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

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
Japan	-0.113*	-0.119*	-0.142**	-0.119*	-0.142**	-0.143*
	(0.066)	(0.067)	(0.071)	(0.067)	(0.072)	(0.074)
China	-0.069	-0.080	-0.085	-0.083	-0.088	-0.087
	(0.065)	(0.066)	(0.070)	(0.066)	(0.071)	(0.072)
Elected	0.178**	0.120	0.098	-0.087	-0.309**	-0.273
	(0.072)	(0.074)	(0.130)	(0.081)	(0.146)	(0.172)
Ln(Population)		0.151***	0.145***	0.156***	0.150***	0.154***
		(0.031)	(0.032)	(0.032)	(0.033)	(0.034)
Quarter 1			-0.550***		-0.460***	-0.483**
			(0.174)		(0.178)	(0.220)
Quarter 3			-0.296		-0.295	0.095
			(0.225)		(0.229)	(0.306)
Quarter 4			-0.080		0.025	0.230**
			(0.065)		(0.069)	(0.115)
Northeast				-0.339***	-0.458***	
				(0.093)	(0.108)	
South				0.206***	0.176**	
				(0.079)	(0.086)	
Midwest				0.083	0.107	
				(0.080)	(0.087)	
Constant	-1.078***	-2.585***	-2.414***	-2.476***	-2.166***	-1.790***
	(0.075)	(0.315)	(0.339)	(0.346)	(0.379)	(0.593)
State Dummies	No	No	No	No	No	Yes
N	3107	3107	2683	3103	2679	2673
R^2	0.003	0.012	0.015	0.028	0.034	0.066

Notes: Coefficients of probit regressions. Dependent variable: 1 responded and 0 otherwise. Coefficients for fixed effects for individual states were included in the regression as noted 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 < 0.1, **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
Japan	0.003	-0.002	-0.013	-0.002	-0.013	-0.013
	(0.076)	(0.076)	(0.081)	(0.077)	(0.082)	(0.085)
China	-0.090	-0.098	-0.095	-0.098	-0.094	-0.101
	(0.077)	(0.077)	(0.082)	(0.079)	(0.083)	(0.087)
Elected	0.203**	0.165*	0.010	-0.064	-0.431***	-0.365*
	(0.086)	(0.088)	(0.144)	(0.097)	(0.166)	(0.194)
Ln(Population)		0.098***	0.101***	0.116***	0.121***	0.116***
		(0.035)	(0.036)	(0.037)	(0.038)	(0.040)
Quarter 1			-0.540**		-0.403*	-0.545*
			(0.212)		(0.218)	(0.284)
Quarter 3			-0.151		-0.149	0.286
			(0.247)		(0.252)	(0.351)
Quarter 4			-0.064		0.073	0.264*
			(0.075)		(0.079)	(0.136)
Northeast				-0.268**	-0.377***	
				(0.116)	(0.134)	
South				0.369***	0.347***	
				(0.094)	(0.101)	
Midwest				0.232**	0.256**	
				(0.095)	(0.103)	
Constant	-1.463***	-2.443***	-2.244***	-2.566***	-2.265***	-2.204***
	(0.090)	(0.364)	(0.389)	(0.402)	(0.441)	(0.731)
State Dummies	No	No	No	No	No	Yes
N	3113	3113	2689	3109	2685	2602
R^2	0.004	0.008	0.009	0.032	0.035	0.085

Notes: Coefficients of probit regressions. Dependent variable: 1 incentive offered and 0 otherwise. Coefficients for fixed effects for individual states 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 < 0.1, **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
Japan	0.202	0.196	0.161	0.186	0.178	0.182
	(0.594)	(0.596)	(0.654)	(0.595)	(0.651)	(0.744)
China	0.092	0.094	0.225	0.161	0.344	0.548
	(0.570)	(0.571)	(0.631)	(0.571)	(0.630)	(0.694)
Elected	1.635**	1.690**	1.703	1.096	0.566	-0.048
	(0.673)	(0.690)	(1.250)	(0.815)	(1.429)	(1.732)
ln(Population)		-0.095	-0.033	-0.205	-0.189	-0.181
		(0.254)	(0.269)	(0.267)	(0.284)	(0.320)
Quarter 1			-1.095		-1.537	-2.188
			(1.630)		(1.663)	(2.281)
Quarter 3			-2.677		-3.161*	-1.707
			(1.775)		(1.784)	(2.135)
Quarter 4			-0.361		-0.314	0.796
			(0.577)		(0.626)	(1.124)
Northeast				-1.338	-1.836*	
				(0.921)	(1.083)	
South				-0.711	-1.089	
				(0.693)	(0.779)	
Midwest				0.082	0.118	
				(0.689)	(0.772)	
Constant	0.617	1.575	1.197	3.655	4.436	1.680
	(0.689)	(2.650)	(3.048)	(3.026)	(3.563)	(5.655)
State Dummies	No	No	No	No	No	Yes
N	237	237	212	237	212	212
R^2	0.026	0.026	0.022	0.042	0.049	0.230

Notes: Coefficients of OLS regressions. Dependent variable: logged dollars offered as incentives. Coefficients for fixed effects for individual states were included in the regression as indicated 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 < 0.1, **p < 0.05, ***p < 0.01

Table 6: Response rate, incentive offered, and logged dollars with observational tests

IVs	Response rate	Incentive offered	Logged dollars	Response rate	Incentive offered	Logged dollars
Partisanship	—	—	—	-1.875*** (0.634)	-0.751 (0.727)	-0.256 (0.33)
Population (logged)	0.195*** (0.066)	0.211*** (0.078)	-0.084 (0.063)	0.365*** (0.112)	0.259** (0.126)	0.087 (0.062)
Manufacturing focus	0.289** (0.12)	0.575*** (0.14)	0.267** (0.114)	0.33* (0.184)	0.537*** (0.207)	0.101 (0.106)
Economic growth	-0.023 (0.041)	-0.053 (0.05)	-0.044 (0.038)	-0.037 (0.06)	-0.019 (0.072)	0.008 (0.033)
Mayor vs. Exec. Council	-0.031 (0.14)	-0.218 (0.183)	-0.267** (0.128)	0.212 (0.235)	0.091 (0.282)	-0.076 (0.133)
Region 1 (Northeast)	-0.038 (0.226)	0.065 (0.335)	-0.226 (0.195)	-0.222 (0.633)	—	-0.01 (0.282)
Region 2 (Midwest)	0.216 (0.154)	0.593*** (0.205)	-0.049 (0.141)	0.257 (0.215)	0.431 (0.269)	-0.021 (0.116)
Region 3 (South)	0.231 (0.149)	0.626*** (0.197)	0.128 (0.137)	0.186 (0.204)	0.668*** (0.248)	-0.072 (0.112)
Constant	-2.919*** (0.748)	-3.763*** (0.901)	1.286* (0.7)	-3.803*** (1.303)	-4.06*** (1.496)	-0.755 (0.734)
Pseudo R-squared	0.027	0.084	0.015	0.077	0.088	0.017
N	654	648	648	313	302	311

Notes: Coefficients of Probit models (for response rate and incentive offered) and OLS regression (for logged dollars). Robust standard errors in parentheses clustered at the individual level. Region 4 (West) is the omitted category for the region dummies.
Significance Level: *p < 0.1, **p < 0.05, ***p < 0.01

Table 7: 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	0.431**	0.418*	0.462**	0.480**	0.472*	0.437
	(0.212)	(0.225)	(0.227)	(0.241)	(0.280)	(0.287)
Japan		-0.081		0.135		-0.007
		(0.266)		(0.283)		(0.344)
China		-0.150		0.016		0.233
		(0.274)		(0.291)		(0.348)
Ln(Population)		0.265**		0.236*		-0.230
		(0.126)		(0.127)		(0.165)
Quarter 1		4.747		4.759		1.964
		(837.054)		(699.558)		(1.608)
Quarter 2		4.075		3.814		0.446
		(837.054)		(699.558)		(1.363)
Quarter 4		4.394		4.103		0.380
		(837.054)		(699.558)		(1.350)
Midwest		4.805		4.741		0.382
		(388.406)		(319.159)		(0.674)
South		4.731		4.626		0.638
		(388.406)		(319.159)		(0.703)
West		4.719		4.639		1.207*
		(388.406)		(319.159)		(0.722)
Constant	-0.884***	-12.633	-1.136***	-12.377	0.165	1.464
	(0.157)	(922.778)	(0.172)	(768.925)	(0.197)	(2.062)
N	168	168	170	170	170	170
Pseudo R^2	0.022	0.088	0.026	0.081		
R^2					0.017	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.

Table 8: Partisanship on Response Rate, Incentive Offered, and Logged Dollars Robustness Checks

IVs	Response rate		Incentive offered		Logged dollars	
Partisanship	-1.111*** (0.377)	—	-1.139** (0.459)	—	-0.361 (0.311)	—
Partisanship (alternate)	—	0.795*** (0.223)	—	0.95*** (0.278)	—	0.189 (0.187)
Elected	-0.004 (0.187)	-0.032 (0.167)	-0.023 (0.23)	0.024 (0.216)	0.092 (0.155)	0.099 (0.14)
Population (logged)	0.205*** (0.06)	0.233*** (0.06)	0.097 (0.072)	0.122* (0.072)	0.075 (0.054)	0.057 (0.054)
State dummies	YES	YES	YES	YES	YES	YES
Constant	-1.224 (0.995)	-2.052** (0.999)	-0.94 (1.074)	-1.804* (1.087)	-0.578 (1.44)	-0.801 (1.478)
Pseudo R^2	0.095	0.093	0.103	0.114		
R^2					0.046	0.051
N	1085	1247	989	1123	1097	1259

Notes: Coefficients of Probit models (for response rate and incentive offered) and OLS regression (for logged dollars). Robust standard errors in parentheses clustered at the individual level.
Significance Level: *p < 0.1, **p < 0.05, ***p < 0.01