Field Experiment on Incentives for Direct Investment: Research Registration Report and Data Analysis Plan

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Note: This is a data analysis plan. It outlines the motivation and research design, identifies testable hypotheses, and uses mock tables to delineate specific statistical analyses to be performed. This will serve as the registration document, and it pre-commits us as authors to follow this structure through to publication.

Abstract

Existing studies of economic competition for investment are hampered by the use of observational data, generally examining either how country or state corporate tax rates vary in response to competitor's reducing tax rates or case study evidence on the use of firm-specific tax incentives. This field experiment sidesteps many of the limits of observational studies by testing how politicians at the city level in the United States respond with investment promotion to sincere interest by potential investors. Specifically, this experiment examines how institutions, electoral timing, and national origin affect both cities' offered incentives for direct investment and their interest in prospective investors captured by activity on a company website. Working as real consultants for an actual firm, we randomly assign the projected timing of the firm's investment announcement along with its implied national origin (in the United States, Japan, or China) to learn the causal effects on property tax abatements, grants, and other investment incentives offered by 4,000 United States cities block randomized by size, region, GDP per capita, and level of professionalization. We also track subjects' activity on our consulting company's website as additional outcomes of interest. Subgroup analysis by cities' level of professionalization – elected mayors vs. council-managers – should reveal heterogeneous treatment effects across types of local institutions. Difference-in-means tests between outcomes for experimental conditions varying the investment announcement for two months before or one month after the next municipal election should reflect on treatment effects for how timing of investment alters offered incentives and website activity. Likewise, difference-in-means tests for incentives and website activity between the U.S. and the two foreign conditions should reveal causal effects of national vs. foreign origin. Mean differences in outcomes between the Japan and China treatment groups should suggest the effects of the two countries' relative attractiveness to cities as investment originators.

Introduction

In 2009 the benighted city of Pontiac, Michigan successfully lured Hollywood executives to convert an abandoned General Motors plant into a state-of-the art motion picture studio. The Disney film *Oz: The Great and Powerful* was filmed there in 2011 and received a $40 million payout from the State of Michigan in investment incentives. The state's pension fund backed $18 million in municipal bonds for constructing the studio, and an additional $14 million in incentives went to the studio under the "Film and Digital Media Infrastructure Investment Tax Credit." The studio later failed to pay most of the interest on the bonds, sticking the pensioners with the liability. In the end, the studio created only 14 permanent jobs, by our estimate costing taxpayers an astounding $1.95 million per job created.

This example of a failed incentive program is just one of the literally thousands of incentives offered every year, many of which are extremely costly to taxpayers. In our own data, Jensen, Malesky and Walsh (2013) document over 5,000 incentives between 2010-2012 in the United States. Using a longer time period and more sources for incentives, an exposé by the *New York Times* uncovered more than $80 billion in incentives to firms.[1]

These figures are more than fodder for journalists or academic studies of economic development projects. The use of these incentive programs to attract investment can provide a window into negotiations between governments and firms, allowing us to examine how globalization and domestic politics both contribute to government policy and impact citizens. Numerous studies have argued that the need to attract capital leads to a "race to the bottom," with governments slashing tax rates, lowering environmental protection, or more generally tilting economic policy towards the attraction and retention of mobile capital. These studies include classics such as Cerny (1990) and Andrews (1994).[2] Many of these studies argue that the competition for investment is best characterized

[1] http://www.nytimes.com/interactive/2012/12/01/us/government-incentives.html?_r=0

[2] The majority of studies examining tax competition and the race to the bottom have focused on the OECD. See Rudra (2002, 2008) for work on developing countries

as a prisoner's dilemma, where governments are forced into a competition that leads all countries, states and cities to become worse off.

In this paper we specifically examine the competition for capital using an experimental approach that we outline later in the paper. We specifically approach 4,000 U.S. cities as an incorporated consultancy with a (real) client interested in relocating. Before we outline our research design we briefly review the literature and develop a series of testable hypotheses.

Literature Review

In American Politics, the literature on the relationship between firms and governments tends to focus on how companies and individuals can swing policy decisions through campaign contributions and lobbying. The comparative and international political economy literatures focus on how the competition for capital can sway policy decisions.

Studies focusing on competitive dynamics often build on existing research in public finance. For example, classic works ranging from Tiebout (1956), Oates (1972), Wilson (1986) and Zodrow and Mieszkowski (1986) focus on the tax policies of governments that maximize the welfare of the representative household. Absent "politics," how do governments set tax policy? One of the findings of this literature is that greater mobility of capital lowers this efficiency-maximizing tax rate on capital, although countries can differ in their rates of taxation (Zodrow and Mieszkowski 1986). Countries that have higher rates of return on capital (for example are more capital scarce) can charge higher taxes on capital than countries with lower rates of return on capital.

Despite the differences in tax rates across countries, these studies do point to tax competition across countries, even when we focus only on welfare enhancing taxation. The normative implications of this competition can range from, on the negative side, reducing the provision of public goods (Wildasin

1989) to the positive effect of reigning in excessive government taxation of capital (Brennan and Buchanan 1980).

Political scientists have advanced this literature, largely by adding politics either explicitly into these public finance models or by using their intuitions about the pressures for tax reductions to illuminate how political pressures can exacerbate or mitigate pressure for tax cuts. The empirical evidence from this literature is largely consistent with tax competition being mitigated by politics. For example, the oft-cited statutory corporate tax cuts in the OECD have largely left government tax receipts relatively stable and effective tax rates on firms largely unchanged (Swank and Steinmo 2002).[3]

Even when we assume that there are real market pressures for tax cuts, one finding is that domestic politics can limit this competition. For example, Hays (2003) finds evidence for tax policy competition, yet this form of tax competition and diffusion is more nuanced than the traditional race-to-the-bottom literature suggests. Recent work by Pinto and Pinto (2008) explores how the complementariness of foreign capital with domestic labor and domestic capital affect tax policy. Basinger and Hallerberg (2004) find that domestic political institutions – operationalized by the number of veto players – and the partisan composition of government jointly temper tax competition. The short story is that there are domestic constraints that limit this competition, leading governments to largely maintain their ability to tax capital at the national level. Plümper et al (2009) argue that country size, budget rigidities, and fairness norms all constrain tax competition in the OECD (see, also, Jensen 2006 for a review of this literature).

Less studied is the increasing use of firm-specific investment incentives. A few studies have examined how competition for capital shapes incentive use. One of the better studies is Keene and Mansor's (2009) examination of changes in incentive programs in Africa over time. Other studies such

[3] See also Swank (2002, 2006).

as Thomas (2000, 2007, 2011) largely focus on incentive competition in democratic countries. These studies largely find that the competition for investment drives these incentive decisions.[4]

A small literature in economics has explored these policies, largely focusing on the inefficiency of incentive programs. Many of these studies focus on a single country, such as Wells et al's (2001) examination of the repeal of a major tax incentive program, Bronzini and de Blasio's (2006) examination of the impact of Italian tax incentives on investment decisions, and Bobonis and Shatz's (2007) study of German investments in the United States. These studies all find that firm-specific incentives have very limited impact on investment decisions. These single country studies have been criticized for their lack of external validity. And yet the move towards cross-national studies has yielded very similar results (Klemm and Van Parys 2012).[5] Incentive programs have very limited impact on investment decisions, and they are inefficient.[6]

This body of literature points to competitive forces that lead to the proliferation of inefficient incentive programs. While we believe that competition can shape politicians' economic policies, a rich account of politics can provide more analytical leverage in explaining these programs. In the next section we provide our theory on how elected and unelected officials provide incentives, and how the electoral calendar and country of origin of the firm shape incentive use.

Theory and Hypotheses

Flipping this perspective, we consider how firms seeking incentives may appear to local policy makers. While the actual attraction of investment projects may influence citizen votes for local office, the choice of offering incentives might also affect voting decisions by demonstrating that a politician put

[4] See also Markusen and Nesse (2007). One exception is the cross-national tax holiday study of Klemm and Van Parys (2012). They find a decreasing trend in the use of holidays across 40 developing countries in Africa, Latin America, and the Caribbean.

[5] See Head et al. (1999), Morisset and Pirnia (1999), Blomstrom and Kokko (2003) and Easson (2004) for reviews of the literature.

[6] For work on the inefficiency of incentives see Easson (2004), Buettner and Ruf (2007),

forth effort to lure a job or revenue-producing project into the city or state. Even if the ultimate economic effects of such tax policies are inefficient, work on pandering has shown that politicians can make use of such policies to signal alignment with voter interests. In short, a voter who believes higher incentives are a good policy choice is more willing to reelect a politician who offers incentives than a politician who does not enact these incentives. Thus, rather than punishing politicians for offering incentives, voters may be systematically more likely to reward politicians that enact incentives in connection with attracting investment (see Jensen et al. 2011).

We argue that this leads elected politicians to offer investment incentives to firms, but that those incentives will be contingent on local institutions, electoral timing, and origin of the investment.

Hypothesis 1: Electoral Institutions

- **Hypothesis:** Cities with directly elected leaders (mayor-council institutions) are more likely to respond and offer incentives than indirectly elected leaders (council-manager institutions).
- **Treatment:** Electoral institutions cannot be randomly assigned, but we plan to perform sub-group analysis to study the effects of each treatment for cities with elected mayors compared with council-manager systems.
- **Outcome measures:**
    - Measure 1: Email response (yes or no)
    - Measure 2: Size of offer
    - Measure 3: Website hits and activity patterns

Our second hypothesis builds on the voluminous literature on political business cycles.

Scholarship, beginning with the seminal work of Nordhaus (1975) has identified politicians' incentives to manipulate policy prior to elections. The original version of this theory focused on myopic voters, where incumbent politicians can generate an artificial pre-election expansion of the economy in order to win

reelection. More recent work, such as Rogoff and Sibert (1988) and Rogoff (1990) focus on the information asymmetry between voters and politicians. Politicians can be of high or low competence type, and the competent politicians manipulate policies in periods prior to elections to signal competence.

Our theory and hypothesis relate more closely to the original conception of the political business cycle, but with an important twist. Building on Jensen et al (2013) we argue that politicians are using policy levers to claim credit for investment. We specifically design our experiment to test how the *announcement* (rather than the actual investment) affects a politician's decision to offer incentives. We argue that politicians are keen to provide incentives to claim credit for investments made prior to elections. This leads to our second hypothesis.

Hypothesis 2: Election Timing

- **Hypothesis:** Cities with elected leaders are more likely to respond and offer incentives for projects where credit can be claimed prior to an election.
- **Treatment:** Randomly assign the timing of when the investment will be announced: either two months before the next election or one month after. A relocation announcement two months before an election should enable politicians sufficient time to claim credit and benefit electorally from attracting investment.
- **Outcome Measures:**
    - Measure 1: Email response (yes or no)
    - Measure 2: Size of offer
    - Measure 3: Website hits and activity patterns

Our third experiment builds on work that identifies biases against foreign firms. In the management literature this is often called the "liability of foreignness," where foreign firms face

additional social costs of operating abroad (Hymer 1976; Zaheer 1995). This includes discriminatory treatment by host governments.

While our main focus is on how electoral pressures shape incentive use, there is also ample literature that documents a public bias against foreign firms.[7] Specifically, we expect bias against both Japanese but especially Chinese firms and that this anti-foreign bias will be most pronounced in cities with elected mayors.

Figure A to be created and placed here: Country perception index—China vs. Japan vs. U.S.

Hypothesis 3: Country of Origin

- **Hypothesis 3a:** Cities will favor U.S. firms over foreign companies.
- **Hypothesis 3b:** Leaders are less likely to respond positively to Chinese than Japanese investors.
- **Hypothesis 3c:** Cities with elected (mayoral) leaders are less likely to respond positively to Chinese investors than non-elected (council-manager) cities.
- **Treatment:** Randomize the treatment of the implied investor's country of origin (e.g. U.S. vs. Japan vs. China).
- **Outcome Measures:**
    - Measure 1: Email response (yes or no)
    - Measure 2: Size of offer
    - Measure 3: Website hits and activity patterns

Study Design

Experimental design: The experiment will be conducted in several stages, which we describe in turn below. We will approach all 4,000 cities in the United States with a population of 10,000 or greater as a

[7] See Jensen and Lindstädt (2013) for a review.

legally-incorporated consultancy (Globeus Consulting) representing a real client currently based in St. Louis. The approach will vary key treatment variables including the timing of investment (in relation to elections) and the implied country of origin of the investment. The firm we will be representing is the type of firm that could locate in urban or rural areas and employ a moderate amount of workers. All experimental conditions will be contained in emails sent to mayors, city managers, and economic development directors.

Our many interviews with economic development officials in different U.S. states and cities suggest that email is a very common method that firms use to approach cities to inquire after tax incentives. While telephone calls are also used, email is quite common and thus makes the approach generally realistic.

- **Main Experiment**
    - **Pre-treatment Blocking:** We will generate control and treatment groups based on the following characteristics:
        - Form of city government
        - Size of city
        - Region
        - GDP Per Capita
    - **Randomly Assign Inquiries:** We will email each mayor/executive and the chief of staff/vice mayor with an inquiry. This email will specifically ask about the types of local incentives that can be offered to the client firm on top of any existing state incentives. We provide our consultancy website in the email.
    - **Analyze Results:** With the completed experiment data, we will compare results across treatment and control groups and draw appropriate conclusions.

Outcome Measures

The initial outcome of interest will focus on response rates. That is, city officials' decisions to respond in the first place ought to indicate interest in the inquiry and therefore be responsive to experimental conditions. The response rate itself is therefore a key outcome of interest, but can also be included in multinomial and selection models – where response is the first stage outcome and incentives or web activity are second stage outcomes – to make use of more sophisticated data analysis techniques in robustness checks. We will also be computing the intent to treat effects (ITT) and the treatment effects on the treated, otherwise known as the complier average causal effects (CACE), which can likewise help account for selection effects. More discussion of analytical approach appears below.

The primary outcome of interest will be the data city officials provide on the web form to be included with the approach email. The web form will include prompts for level of tax abatements, number of years the abatements would be in place, any local grants and loans offered, earnings tax abatements, and an open-ended box for additional information. See example below.

In addition to the main web form, the email will provide subjects with a link to the consulting company's website, where additional information might be obtained. Information about GLOBEUS Consulting, its management team, its board of directors, and its mission will be available on the website and accessible through clicking on various links. Additionally, the web site will provide two brief "White Papers" that discuss different topics of potential interest to cities, including "Maximizing Jobs to Maximize Incentives" and "Political Contributions and Firm Strategies." Subjects will be able to request that a copy of the papers be sent to them by uploading their contact information. Thirteen different websites were created to track web activity: twelve for each of the six treatment conditions subdivided again by each of the two main blocks for elected mayors and council-manager systems and a thirteenth for subjects who choose to do a web search for the consulting company's name. We will then track

subjects as they arrive at the website by matching them to their locale using their IP addresses, which should provide us a loose indication of location that can be connected to the assigned treatment condition. By tracking IP addresses associated with specific localities, we will be able to assess the experimental conditions' effects on subjects' level of interest evinced by time spent on the website, number of links clicked, and material requested.

Subjects

Subjects are mayors, city managers, economic development directors, and their agents in 4,000 U.S. cities. The cities and their operating procedures are the key units of analysis. We are not interested in the individual, idiosyncratic preferences of the persons occupying the various targeted positions but rather in the standard processes and procedures they follow.

While some coordination between cities and state offices occurs as they discuss incentives, our informants claim that very little coordination occurs among cities themselves, so the risk of contamination, detection, or spillover is relatively low. Given that cities compete against one another to attract investment, we do not expect much sharing of information across subjects. We will continue to investigate whether or not contamination or spillover is a significant risk. An alternative design would be to provide a link in the email and then invite subjects to click on the link to learn the specific details of the client firm. If we determine that there is a real risk of contamination under the initial plan, we will shift to the backup design.

Example Approaches

Main Experiment:

"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 [two months before next election / 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 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/index.html?id=301

Web Survey:

Globeus Consulting
Selection & Incentives Department

Introduction

This data you enter into this webform will be used by our client to narrow down their location decision. Your answers are not binding, but any concrete details you can provide will help us evaluate the feasibility of your ${e://Field/type} as a site for the plant relocation.

In this form we will ask about:
a) Grants and loans for relocation provided on a per job basis.
b) Tax abatements (on property and earnings taxes).
c) Any other local incentives provided.

Grants and Loans

Please indicate the availability of grants and loans.

Local grant dollars for relocation (dollars per job)

Local loans for relocation (dollars per job)

Please enter additional comments or information about grants and loans below.

Real Property Tax

Does your ${e://Field/type} have local real property taxes?

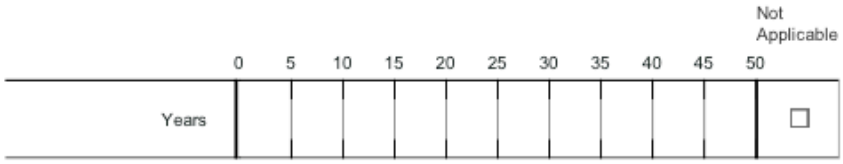
- ◯ Yes
- ◯ No

Please indicate below the local real property tax abatement or refund your ${e://Field/type} is able to offer.

Not Applicable

0 10 20 30 40 50 60 70 80 90 100

Please indicate below the number of years you are able to offer this abatement or refund.



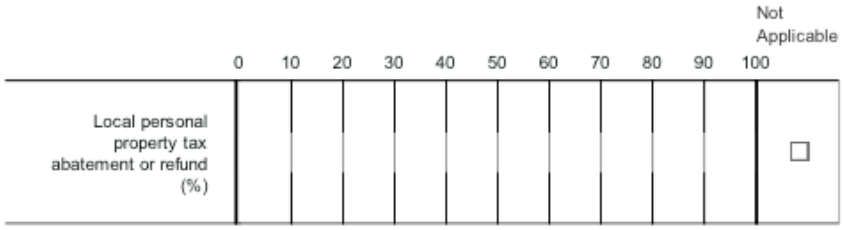
Please enter additional comments about real property taxes below.

Personal Property Tax

Does your ${e://Field/type} have personal property taxes?

- ○ Yes
- ○ No

Please indicate below the local personal property tax abatement or refund your ${e://Field/type} is able to offer.



Please indicate below the number of years you are able to offer this abatement or refund.

Not

	0	5	10	15	20	25	30	35	40	45	50	Not Applicable
Years												☐

Please enter additional comments about personal property taxes below.

Local Earnings Tax

Does your ${e://Field/type} have local earnings taxes?

- ◯ Yes
- ◯ No

Please indicate below the local local earnings tax abatement or refund your ${e://Field/type} is able to offer.

	0	10	20	30	40	50	60	70	80	90	100	Not Applicable
Local real property tax abatement or refund (%)												☐

Please indicate below the number of years you are able to offer this abatement or refund.

	0	5	10	15	20	25	30	35	40	45	50	Not Applicable
Years												☐

Please enter additional comments about local earnings taxes below.

Other Financial Incentives

Please provide information below on any other local financial incentives you are able to offer.

Globeus Consulting

Data Analysis

Descriptive Statistics tables

Note: None of these tables contain real data. This is only an analysis plan with some examples.

Group Breakdown Descriptions:

Election Status Breakdown		
	# of Mayors	**Percent**
Elected	3024	76%
Non-elected	976	24%
Total	**4000**	**100%**

Treatment 1: FDI & Election Timing			Treatment 2: Country of Origin		
	# of Mayors	**Percent**		**# of Mayors**	**Percent**
Pre-Election	1512	50%	**Chinese FDI**	2000	0.5
Post-Election	1512	50%	**Other**	2000	50%
Total	**3024**	**100%**	**Total**	**4000**	**100%**

Breakdown of Subjects and Treatments:				
		Pre-election	**Post-election**	**Total**
Elected	**Chinese FDI**	504	504	**1008**
	Japanese FDI	504	504	**1008**
	US DI	504	504	**1008**
Non-Elected	**Chinese FDI**	163	162	**325**
	Japanese FDI	162	163	**325**
	US DI	**163**	**163**	**325**
Total		**2000**	**2000**	**4000**

Survey Response Tables:

Election Status and Response Rates			
	Responded	**No Response**	**Total**
Elected	2308	716	3024
Non-Elected	356	620	976
Total	**2664**	**1336**	**4000**

Election Timing and Response Rates			
	Responded	**No Response**	**Total**
Pre-Election	12	1500	1512
Post-Election	716	796	1512
Total	**728**	**2296**	**3024**

Country of Origin and Response Rates			
	Responded	**No Response**	**Total**
Chinese FDI	530	804	**1334**
Japanese FDI	801	532	**1333**
US DI	**588**	**745**	**1333**
Total	**1919**	**2081**	**4000**

Response Comparisons:

	Elected	**Non-Elected**	**Pre-Election**	**Post-Election**	**Chinese Firm**	**Other Firm**
% Responded	76%	36%	82%	47%	60%	73%
Average Offer						
S.D. of offers						
Range of offers						
Average website hits						

Difference in Means for Response and the three Dependent Variables

Condition	*N*	Responses	Response Rate	Tax Abatement %	Grant Dollars %	Grant Loans %
Pre-Elect.	2000	1000	57.1%	53.6%	53.6%	53.6%
Post-Elect.	2000	1000	54.3%	52.4%	52.4%	52.4%
U.S.	1333	800	50.2%	47.5%	47.5%	47.5%
China	1333	700	44.9%***	46.8%	46.8%	46.8%
Japan	1334	600	46.8%**	60.3%	60.3%	60.3%
US – Pre	268	153	57.1%	53.6%	53.6%	53.6%
US – Post	232	126	54.3%	52.4%	52.4%	52.4%
Jap – Pre	235	118	50.2%	47.5%	47.5%	47.5%
Jap – Post	276	124	44.9%***	46.8%	46.8%	46.8%
Chin – Pre	248	116	46.8%**	60.3%	60.3%	60.3%
Chin - Post	248	116	46.8%**	60.3%	60.3%	60.3%

Intent to Treat and Complier Average Causal Effects

Condition	*N*	Resp.	Response ITT	Response CACE	Tax Abate. ITT	Tax Abate. CACE	Grant Loans ITT	Grant Loans CACE	Grant Dollars ITT	C D C
Pre-Elect.	2000	1000	57.1%	57.1%	53.6%	53.6%	53.6%	53.6%	53.6%	53
Post-Elect.	2000	1000	54.3%	54.3%	52.4%	52.4%	52.4%	52.4%	52.4%	52
U.S.	1333	800	50.2%	50.2%	47.5%	47.5%	47.5%	47.5%	47.5%	47
China	1333	700	44.9%***	44.9%***	46.8%	46.8%	46.8%	46.8%	46.8%	46
Japan	1334	600	46.8%**	46.8%**	60.3%	60.3%	60.3%	60.3%	60.3%	60
US – Pre	268	153	57.1%	57.1%	53.6%	53.6%	53.6%	53.6%	53.6%	53
US – Post	232	126	54.3%	54.3%	52.4%	52.4%	52.4%	52.4%	52.4%	52
Jap – Pre	235	118	50.2%	50.2%	47.5%	47.5%	47.5%	47.5%	47.5%	47
Jap – Post	276	124	44.9%***	44.9%***	46.8%	46.8%	46.8%	46.8%	46.8%	46
Chin – Pre	248	116	46.8%**	46.8%**	60.3%	60.3%	60.3%	60.3%	60.3%	60
Chin - Post	248	116	46.8%**	46.8%**	60.3%	60.3%	60.3%	60.3%	60.3%	60

Treatment Effects for Website Outcomes

Condition	*N*	Web Hits	Time on Site in Seconds	Number of Links Clicked	Intn'l Clients Link Clicked	U.S. Clients Link Clicked	White Paper Requested	Contact Sent
Pre-Elect. ITT	2000	1000	135**	3.5***	53.6%	53.6%*	53.6%	53.6%
Post-Elect.	2000	1000	102	2.3	52.4%	52.4%	52.4%*	52.4%
U.S. ITT	1333	800	145**	3.6**	47.5%	47.5%*	47.5%	47.5%
China ITT	1333	700	101*	2.4*	46.8%	46.8%	46.8%*	46.8%
Japan ITT	1334	600	125	3.2	60.3%	60.3%	60.3%	60.3%
Pre-Elect. CACE	268	153	143	3.5	53.6%	53.6%	53.6%	53.6%
Post-Elect. CACE	232	126	137	2.3	52.4%	52.4%	52.4%	52.4%
U.S. CACE	235	118	128	3.6	47.5%	47.5%	47.5%	47.5%
China CACE	276	124	118	2.4	46.8%	46.8%	46.8%	46.8%
Japan CACE	248	116	103	2.0	60.3%	60.3%	60.3%	60.3%

Heterogeneous Treatment Effects by System: Directly Elected Mayors vs. Council-Managers

Condition	*N*	Resp.	Response ITT	Response CACE	Tax Abate. ITT	Tax Abate. CACE	Grant Loans ITT	Grant Loans CACE	Grant Dollars ITT	C D C
Directly Elected Mayors										
Pre-Elect.	2000	1000	57.1%	57.1%	53.6%	53.6%	53.6%	53.6%	53.6%	53
Post-Elect.	2000	1000	54.3%	54.3%	52.4%	52.4%	52.4%	52.4%	52.4%	52
U.S.	1333	800	50.2%	50.2%	47.5%	47.5%	47.5%	47.5%	47.5%	47
China	1333	700	44.9%***	44.9%***	46.8%	46.8%	46.8%	46.8%	46.8%	46
Japan	1334	600	46.8%**	46.8%**	60.3%	60.3%	60.3%	60.3%	60.3%	60
Council-Managers										
Pre-Elect.	2000	1000	57.1%	57.1%	53.6%	53.6%	53.6%	53.6%	53.6%	53
Post-Elect.	2000	1000	54.3%	54.3%	52.4%	52.4%	52.4%	52.4%	52.4%	52
U.S.	1333	800	50.2%	50.2%	47.5%	47.5%	47.5%	47.5%	47.5%	47
China	1333	700	44.9%***	44.9%***	46.8%	46.8%	46.8%	46.8%	46.8%	46
Japan	1334	600	46.8%**	46.8%**	60.3%	60.3%	60.3%	60.3%	60.3%	60

Outcome analysis:

For all hypotheses, we will employ the following methods of data analysis. We use the following measures to analyze differences in reactions:

Measure 1: Email response (yes or no)

-We will use difference-in-means and difference of proportions analysis as well as logit regression analysis to examine response rates for each hypothesis, both with and without covariates using the blocking criteria as controls.

Measure 2: Size of offer

-We will employ statistical adjustments to examine both the main effects – the intent to treat (ITT) – and the treatment effects on the treated or complier average treatment effects (CACE). This will provide the initial baseline against which to compare the sensitivity analysis. The main challenge with the sensitivity analysis accounting for the selection effects manifest in response rates. To address, we will consider using Heckman and Tobit selection models where we identify the models with the number of email prompts to a city. We expect, and will verify before using, that the number of prompts affects response rates, but not the size of the incentives offered.

Additionally, we will employ propensity-score matching. First, we will use the treatment conditions and covariates to predict dichotomized offers or not for the three types of tax incentives – real property tax abatements, personal property tax abatements, local earnings tax abatements – along with grant dollars per job and loan dollars per job. This will be the selection stage. We will then group the cities by propensity score into bins ranked from most to least likely to offer each of the five incentives. Then, we will use the same models to predict the continuous level of incentives offered for the cities in each bin. This will produce a treatment effect for each of the seven bins, which can be combined to calculate the average treatment effect for each outcome over the range of propensities.

Measure 3: Website hits

-As with the size of offer analysis, we will employ statistical adjustments to examine both the main effects – the intent to treat (ITT) – and the treatment effects on the treated or complier average treatment effects (CACE). This will provide the initial baseline against which to compare the sensitivity analysis.

Additionally, we will employ Heckman and Tobit selection models to account for response and then substantive outcomes, with the instrument employed as discussed above. Further, we will use multinomial models to capture the different types of website activity where those activities can be mutually exclusive. And we will further use count models to capture numbers of page hits and duration models to consider amount of time spent on pages. Finally, we will once more employ the propensity-score matching/binning for the continuous outcomes of website activity.

Conclusions

TBD

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