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Pass the Bucks: Credit, Blame, and the Global Competition for Investment[1]

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Both countries and subnational governments commonly engage in competition for mobile capital, offering generous incentives to attract investment. Existing economics research has suggested that these tax incentives have a limited ability to affect investment patterns and are often excessively costly when measured against the amount of investment and jobs created. In this paper, we argue instead that the "competition" for capital can be politically beneficial to incumbent politicians. Building off work on electoral pandering, we argue that incentives allow politicians to take credit for firms' investment decisions. We test the empirical implications of this theory using a nationwide Internet survey, which employs a randomized experiment to test how voters evaluate the performance of incumbent US governors. Our findings illustrate a critical political benefit of offering such incentives. Politicians can use these incentives to take credit for investment flowing into their districts and to minimize the political fallout when investors choose to locate elsewhere.

The competition between governments for international capital is fierce, with cash-strapped governments often providing generous tax holidays, abatements, and other forms of incentives to reduce the tax burdens of individual firms. Between 2010 and 2012, there have been more than 5,000 documented cases of countries, states, provinces, and cities using such investment incentives to lure new projects, encourage expansion of sites, or retain companies after threats to move—all in the name of creating or saving jobs. *ICAincentives*, a for-profit incentive tracking company, finds that such financial incentives are far from trivial, averaging more than 20 percent of capital investment and amounting to more than $58,000 per job created.[2] In fact, their figures understate the total universe of incentives, as many countries do not provide the same level of transparency in their incentive programs as the United States, Canada, and European Union members do. As we document in "Competing for Mobile Capital" of this paper, investment incentives are widespread across the developed and developing world and are becoming increasingly costly.

Investment incentives are not new. The first documented tax incentive package dates back to 1160, when Italian local governments bid for a textile production facility (Wells 1999). In the United States, the first recorded incentive occurred with New Jersey's luring of Alexander Hamilton's manufacturing company in 1791 (Bernstein 1984). What has changed since these incidents is how common this strategy has become for countries, states, provinces, and cities.

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[2] ICAincentives.com 2010–2012.

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The growing use of incentives is prima facie evidence that the increased mobility of capital has enhanced the ability of firms to pit governments against each other. Yet, as we document in the next section, the assumption that markets "demand" these incentives is doubtful at best. It is not at all clear that communities that avoid the use of incentives will be punished with less investment and worse economic performance.

In short, the use of financial incentives is difficult to justify on economic grounds. Fiscally strained governments offer lucrative tax treatment to firms, yet numerous studies have documented that these incentives have limited impact on the investment decisions of businesses or are too generous relative to the ultimate economic benefits (Moran 1998; Head, Ries, and Swenson 1999; Morisset and Pirnia 1999; Oman 2000; Blomstrom and Kokko 2003; Bobonis and Shatz 2007; Buettner and Ruf 2007). As summarized by the Tax Foundation:

> States often overpay, granting such generous tax abatements that their already resident taxpayers must pitch in more just so that state "economic development" officials can make headlines rolling out the red carpet for a newcomer. (Ahern 2010)

What does the competition for capital mean for a country's ability to make taxation and spending decisions? How does it affect state sovereignty? Unlike previous international political economy research that focuses on the competition across countries in slashing tax rates, we focus on the increasing use of special location incentives that allow politicians to discriminate in favor of some types of investor over others. Whether there is a prisoner's dilemma in tax competition or not, we argue that these incentives can actually be used as part of a political strategy for politicians seeking reelection.

We hypothesize that, while financial incentives have uncertain economic benefits (and can even be economically inefficient), they can help politicians win reelection. Specifically, our project examines how voters reward and punish politicians for investment and the offering of incentives. We use a theory of politicians "pandering" to voters—whereby politicians endorse popular policies that provide short-term political gain at the cost of lower economic performance over the long term—to explain the political logic of politicians offering incentives. Thus, even in states that are certain to attract investment or certain not to attract investment, politicians are motivated to offer lucrative incentives.

Our theory does not necessarily contradict work focusing on how competitive pressures affect government policy. Rather, we simply highlight that the electoral calculus makes it such that offering incentives is a winning strategy independent of competition from other locations. Our project illustrates how politicians can harness policy levers like tax incentives to claim credit or avoid blame for economic outcomes. Far from constraining governments, globalization can actually generate more variation in local economic policy choices.

In many countries, both the central government and subnational authorities provide incentives. In the United States, however, states largely control the policy levers necessary to attract investment. Many states have even gone as far as establishing offices in foreign countries. By last count, US states had established 175 foreign offices to promote trade and investment (Le, Miloslavsky, and Shatz 2003; cited in Bobonis and Shatz 2007). Governors of US states, much like the leaders of economically competing countries, have become increasingly active in attempting to woo foreign capital by using financial incentives to distinguish their states from competitors (Davis 2004). Taking advantage of this ability of state politicians to offer special incentives, we use the United States as a laboratory to test our theory.

We specifically test this theory using a survey experiment embedded in a nationwide Internet survey that explores individuals' voting intentions for US governors. This experimental approach has the advantage of testing the net effect of tax incentives and investment on voting intentions. Because the treatment is exogenously assigned, whether a respondent learned their state won a project or offered a tax incentive is designed to be orthogonal to the individual attributes of voters, specifically their previously held views about fairness and taxation. This strategy mitigates the concern that the underlying views of citizens inform both a governor's selection of economic policy and voting intentions, leading to a spurious correlation between the two latter variables. Our treatment not only provides the respondent with a realistic amount of information on the investment, it also tests the impact of incentives and investment on voting behavior, not simply on policy evaluations. In other words, we do not want to know "What do you think?" so much as "What will you do?" This, of course, is what a politician running for reelection most cares about. Our main finding is that, although attracting new investment benefits incumbent governors, the size of the vote bonus is enhanced by the use of incentives and by voter type.

A governor reaps more reward for new investment in his or her state if his or her administration offered tax incentives. In fact, a governor will be rewarded for offering tax incentives even if it does not succeed in luring the intended investment. Even more strikingly, the use of incentives protects against downside risk. The vote bonus for offering greater incentives than competitors do is actually higher for a governor whose state loses the project (about 5.2% from all respondents and 10.7% from political independents). Thus, whether or not the policy was effective, the incumbent gets credit (and votes) for trying. Few other policy actions by a governor can play so immediate a role. The long-term investments in infrastructure, education, and regulatory reform that have been shown to have the most influence on firms' location decisions are likely to bear fruit only years afterward, perhaps after the governor's political career is over, and so are difficult to identify with him or her.

The effect of incentives on winning votes varies greatly across governors and sub-groups of voters. By interacting our treatment with predetermined attributes of voters and the state they are in, we are also able to explore these heterogeneous treatment effects. In particular, we learn that party identification matters a great deal. The use of incentives is most effective among registered independents and least effective in attracting Republican voters. More speculatively, when we interact the treatments with previous measures of governor approval, we find that incentives bear the greatest fruit for unpopular governors. This result indicates that offering incentives may be used as part of a gambling-for-resurrection strategy, whereby unpopular governors have a higher incentive to engage in an economically risky policy.

Our paper proceeds as follows. In "Competing for Mobile Capital," we outline the literature on tax competition and how the contest for investment can lead to economically inefficient policies. We point out that much of

the scholarship has focused on the economic costs and benefits of changing tax rates on capital, but has not explored the political dynamics underlying these changes. In "Globalization and Governors," we review the recent literature on globalization and mass behavior and link this to our study of US governors. We argue that the United States is an excellent laboratory in which to examine how voters blame or reward politicians for economic policy related to globalization. We conclude "Globalization and Governors" by theorizing about the decisions by incumbent politicians to offer firm-specific incentives greater than those being offered by other states. Unlike other models in public finance, we focus directly on the relationship between tax incentives and reelection, while allowing for incentives targeted to specific firms. In "Empirical Analysis" and "Ordered Probit Analysis of Experiment," we introduce our research design, explain the data we gathered by including survey experiments in the Cooperative Congressional Election Survey (CCES), and discuss our empirical results. "Conclusion" concludes by assessing the implications of our findings for the political economy research agenda.

Competing for Mobile Capital

There is a large literature on the competition for international capital. Within the public finance literature, a number of influential articles model how governments, maximizing the utility of the representative household, set taxes in a world of mobile capital (Tiebout 1956; Oates 1972; Wilson 1986; Zodrow and Mieszkowski 1986). This competition for capital can lead to downward pressure on tax rates and revenues, yet it does not require all governments to charge the same tax rate.[3] Such competition can indirectly result in the under-provision of public goods (Wildasin 1989), but it can also help constrain excessive government (Brennan and Buchanan 1980).

Political scientists have built upon this public finance literature, explaining corporate tax-rate setting under different institutional settings and different partisan compositions of government. This includes work identifying that statutory corporate taxes have converged across OECD countries, while the effective tax rates and corporate tax receipts have remained relatively stable over time (Swank and Steinmo 2002). Hays and Swank find evidence for tax policy competition; yet, this form of tax competition and diffusion is more nuanced than that presented in the traditional race-to-the-bottom literature.[4] Basinger and Hallerberg find that domestic political institutions, operationalized by the number of veto players and the partisan composition of government, temper tax competition (Basinger and Hallerberg 2004).

We believe that the public finance literature on tax incentives has largely ignored two related points. First, governments can offer tax incentives specific to individual firms. In an influential paper on the provision of tax incentives, Li examines 58 developing countries and their use of six different types of firm-specific incentives in 1990.[5] These countries, on average, used 1.3 types of programs; just under a quarter of the countries offered no tax incentives at all and only one offered all six types. The 1996 *U.S. Commercial Guides* provide some descriptive evidence on the increasing use of these incentives. Of the 78 developed and developing countries discussed, 80 percent offer at least one type of incentive, with many offering more than one (averaging just more than 2.5 incentives per country). The most common of these incentives are corporate income tax holidays (69%) and exemptions from import duties (62%). These data clearly document that most countries offer incentives and have increased the activity over time. Other scholars document that these programs are becoming more costly, although systematically documenting this rise requires some care (Thomas 2011). These stylized facts suggest that targeted, firm-specific incentives have become the norm in investment promotion, yet they have been largely ignored by the political science literature.

Second, as the political science literature has demonstrated, politicians will use tax and other economic policies not necessarily to maximize growth and investment, but to maximize the probability of staying in power.[6] Indeed, political science has identified two benefits of politicians favoring reelection over economic performance for policy outcomes: correcting moral hazard and correcting adverse selection problems (Maskin and Tirole 2004). But the use of economic policies to attract votes comes with costs, the most important being the pandering problem, whereby politicians choose popular policies that are contrary to voters' interests (Canes-Wrone, Herron, and Shotts 2001). Harrington has shown that voters' uncertainty about the efficacy of the different policy choices and about the incumbent's future intentions will trigger pandering behavior by the incumbent (Harrington 1993). The key insight from the pandering literature is that even if the incumbent begins the reelection campaign with a strategy based on his or her personal beliefs about the effectiveness of a particular policy, reelection pressures create a bias toward policies that are more popular with voters, even if they are less effective. Thus, politicians with sincere concern about the welfare of voters and perfect information about the ineffectiveness of incentives may still choose the "bad" policy due to reelection pressures.

These observations help us understand a number of puzzles in the literature on tax incentives. Specifically, a growing economics consensus points out that (i) tax incentives are ineffective in luring investment to a particular location, and (ii) the large incentives being offered are economically suboptimal for states, sometimes amounting to a net transfer to investors.[7]

The use of these incentives, despite the well-documented limits and uncertainty of their effectiveness, perplexes economists. One high-profile study is Wells et al.'s examination of the how the repeal of incentives in Indonesia, even in the face of growing regional competition for investment, had no adverse consequences (Wells, Allen, Morisset, and Pirnia 2001). Most recently, Bobonis and Shatz (2007) explore the agglomeration effects of investment, by which attracting new investment has a positive impact on attracting more investment. This find-

[3] See Wilson (1991) and Wilson and Wildasin (2004) for a review of this literature.

[4] Hays (2003) and Swank (2002, 2006). Recent work by Pinto and Pinto (2008) explores how the complementariness of foreign capital with domestic labor and domestic capital affect tax policy.

[5] Li (2006) These are (i) incentives related to value added taxes, (ii) corporate income taxes, (iii) property taxes, (iv) licensing fees, (v) import duties, and (vi) sale taxes.

[6] The political science arguments cited above include discussions about how veto players and levels of partisanship affect tax policy setting. Most recently, Plümper, Troeger, and Winner (2009) demonstrate formally that corporate tax rates are constrained by domestic political considerations.

[7] For an overview, see Oman (2000).

ing seemingly supports the use of tax incentives to prime the pump. Yet, the authors directly test the impact of tax policies on investment location and find that incentives have "little influence over the location of FDI [foreign direct investment]" (Bobonis and Shatz 2007:39).

Tax incentives, even if they are purported to sway location decisions, often appear out of proportion to their direct benefits (Buettner and Ruf 2007). In an extensive review of the literature, Morisset and Pirnia find that tax incentives cannot overcome major obstacles to investment, such as poor infrastructure, and the costs often exceed the benefits (Morisset and Pirnia 1999). Blomstrom and Kokko argue further that, while scholars have documented the positive spillovers of foreign direct investment, government incentive policies to maximize these spillovers are inefficient (Blomstrom and Kokko 2003).

Examples of excessive incentives are easy to find. Moran documents a number of high-profile incentives to attract manufacturing investment in the United States (Moran 1998). Alabama successfully attracted a Mercedes-Benz plant, for example, by offering subsidies that exceeded $200,000 per employee. Thomas and Wishlade (2009) show that, since 2002, US states have offered 17 incentive packages in excess of $100 million each; the authors argue that these incentives are not only expensive but economically inefficient. With sky-high numbers such as these, it is no surprise that Head et al. (1999) concluded that the incentives are often larger than the gains.

For us, however, the debate between economists and practitioners is merely a starting point. While we note that there is some evidence regarding the ineffectiveness of tax incentives, we do not take a stand on the relative economic benefits of incentives under all circumstances. Certainly, readers can think of occasions when incentives have been or may be fruitful. Of greater interest to us is their widespread use despite clear uncertainty about their efficacy.

Something else is at work here, and we argue that it is political pandering. We argue that for politicians to pander, voters need not actually hold incorrect views about the benefits of incentives; they only need to be unsure enough to be persuaded in a campaign. This less stringent assumption means that two politicians can have opposite opinions about the actual economic utility of investment incentives, yet both will find such incentives helpful for attracting votes in the next election.

Globalization and Governors

In this paper, we examine how attracting investment affects the political fortunes of incumbent governors of US states. Why focus on US states? Our theory generalizes across countries, yet we believe American states are an excellent laboratory for our inquiry.

Similar to countries, many of the important determinants of investment at the state level, such as size of the market, quality of infrastructure, or the level of human capital, are difficult to change in the short to medium term. Moreover, it is difficult for a politician to claim that these features are associated with a specific investment within their borders. Consequently, offering firm-specific financial incentives to projects has become one of the main short-term economic development strategies of countries and subnational government around the world (Thomas 2011). Tax incentives have two obvious benefits for executives at all level of government: (i) they can be issued immediately; (ii) they are directly linked to an individual project. While the European Union has institutional constraints that limit the amount of incentives offered, and Canada and Australia have laws limiting tax competition across provinces, many countries and their subnational units compete with one another for individual firms by using tax incentives.[8] Fiscal wars across Brazilian states have led to extremely lucrative packages to auto producers, while countries as diverse as Costa Rica, Egypt, Ghana, and Israel are known to provide handsome inducements (Thomas 2007). Although it is difficult to compare the size of these incentives across countries,[9] Easson (2004) argues that incentives in the United States and across borders have increased dramatically in recent years.

Focusing on US states allows us to examine the setting of incentives while holding constant political institutions, culture, and other difficult to measure factors that can lead to unobserved heterogeneity and bias results in a cross-country analysis. Like many countries, US states have a diverse array of automatic incentive programs and the ability to provide discretionary incentives to firms. Some states have "deal closing funds," while others provide incentives through the normal legislative process. The mixture of discretionary and non-discretionary incentives mirrors that observed in countries and creates variation in the tools available to incumbent leaders (Jensen 2006).

We focus on governors rather than members of the House or Senate for two reasons. First, while members of Congress certainly engage in activities to increase investment, their efforts predominantly take the form of informal suasion. They have little control over formal state-level policy levers that voters can see and associate with an investment project. Secondly, voters have a limited ability to assign credit when multiple politicians operate within the same state. Large investment projects create revenue, employment opportunities, and business spillovers that reach beyond an individual congressman's electoral district and the policies that attract these activities are often the results of legislation that requires the support of many legislators.

By contrast, governors' activities to attract investment are clearly attributable by voters to a single actor, allowing voters to assign responsibility to the governor for her performance.[10] Even existing tax incentive programs can be retailored to attract or retain specific investments. For example, New Jersey's Urban Transit Hub Tax Credit was added to a previous package "by Governor Corzine in January 2008, largely to stop financial services giant, BlackRock Private Equity Partners, from moving more than 1,000 jobs from Plainsboro to Pennsylvania" (Morley 2011).

Third, governors across the country are visibly active in attempting to attract investment, traveling abroad on commercial visits, and using the powers of their office to offset investor start-up costs with tax holidays, property tax reductions, and other incentives. Governors are perceived by voters to be the actors most responsible for attracting investment, and voters can reward them for their performance.

[8] See Thomas and Wishlade (2009) for a discussion of EU laws limiting competition and the decrease in the size of investment incentives since 2002.

[9] As Thomas (2011) points out, the details of these incentives are often hard to come by and the forms of accounting for the value of these incentives varies across the country.

[10] See Hellwig and Samuels (2008) for a cross-national analysis of how institutions affect the attribution of responsibility.

Finally, 30 US states have "contingency funds" or "deal closing funds," averaging between $7 and $10 million in additional incentives that can be added to existing tax incentive packages. The largest of these funds is the behemoth $250 million Texas Enterprise Fund, which provides incentives authorized by a three-person board made up of the Governor, Lieutenant Governor, and Director of Economic Development.

Scholars have argued that voters reward governors for economic outcomes that reflect their actions and tend not to take into account economic events that reflect influences outside the governor's control (Wolfers 2002:1). Cohen and King make a similar point, arguing that voters recognize the limitations of state governments to affect the economy and often focus their attention on the governor's (perceived) ability to generate jobs (Cohen and King 2004). For Cohen and King (2004), the level of unemployment *relative* to other states has the largest effect on voting. More broadly, Arceneaux (2006) finds that in federal systems, such as the United States, citizens sanction politicians for policy decisions over which their level of government has responsibility. These studies all identify the importance of state economic conditions for the reelection prospects of governors and find that voters are reasonably sophisticated in assigning credit and blame to specific politicians. Combining these insights about attribution with the established theories of electoral pandering cited above, we derive three hypotheses of how voters will respond when politicians use investment incentives to claim credit or deflect blame for the investment projects their states do or do not get.

Building on the pandering literature, we argue that voters largely reward politicians based on economic outcomes and policy. Some work, such as Canes-Wrone et al. (2001) focus on how economic outcomes signal an incumbent's "type". Alternatively, some of the pandering models argue that voters select politicians based on the popularity of a policy position taken (Maskin and Tirole 2004). In both cases, this can lead politicians to select policies that actually are not in the interests of voters.

In our paper, following Harrington (1993), we argue that voters have an intrinsic interest in economic outcomes (attracting investment) and have beliefs about the effectiveness of policy (incentives) in achieving these outcomes. Thus, voters observe a policy choice (incentives) and the outcome (investment), but they cannot directly observe how important the policy choice was in determining the outcome. Their prior beliefs about the effectiveness of the policy, however, shape how they reward incumbents. We argue that even if such tax policies prove to be economically inefficient, politicians can make use of them to signal their own alignment with the voters' interests.

Our pandering theory rests on three reasonable assumptions. First, we must assume that voters generally believe that incentives can be an effective way to attract investment. This assumption is not controversial and is supported by a 2005 Polimetrix survey that found that more than 70 percent of US respondents believed tax incentives were a very important determinant of firm location choice.[11]

Second, we assume that voters can directly observe the offering of incentives. This assumption is difficult to document systematically, but numerous governors' campaign Web sites and press releases tout the use of incentives to attract firms. Far from hiding these incentive programs, governors advertise their use.

Third, we assume that voters have imperfect information on how pivotal a specific incentive was to attracting a firm. Again, this is not controversial, since voters cannot observe the counterfactual of no incentive, while both governors and firms have reasons to lie about the importance of incentives for the firm. Governors want to claim credit for the investment and firms want to minimize their tax burden by maintaining a threat of exit. In short, these are three reasonable assumptions that fit our US case, but are also likely to hold in other countries around the world.

In other words, governors can claim credit for investment through the use of incentives. If voters believe higher incentives are a good policy choice and are unable to evaluate whether or not a particular incentive really was pivotal in attracting a particular firm, voters will be systematically more likely to reward politicians for using incentives than to punish politicians for overusing them. This leads to our first hypothesis:

Hypothesis 1 (Credit-claiming): *Voters are more likely to vote for an incumbent governor who attracted a firm to the state with incentives than for an incumbent governor who attracted the same firm without incentives.*

Taking this logic a step further, pandering allows politicians to escape blame for poor economic performance by claiming that they enacted the policies favored by voters and made a clear effort to improve the economy, even if the effort was unsuccessful. Pandering logic dictates that voters should be more willing to accept policy choices that did not succeed as long as they are in line with the voters' beliefs about such policies.

Hypothesis 2 (Blame-avoidance): *Voters are more likely to reelect an incumbent governor who failed to attract a firm despite offering incentives than an incumbent governor who failed to attract the same firm and did not use incentives.*

Both of these hypotheses make the assumption that voters prefer the use of tax incentives to attract investment. Nevertheless, substantial literature on the political economy of international trade has focused on how politicians use policies as an "inefficient redistribution" from citizens to firms.[12] Work by Kono (2006) shows that electoral institutions shape the form of these policies. Democratically elected politicians hide these redistributive policies from voters, by focusing on policy instruments that are too complex for voters to understand and for opposition politicians to use as fodder for political campaigns.

Firm-level incentives are one of the more transparent policies that politicians can use to attract firms. In fact, they have to be for the politician to claim credit. On the other hand, transparency also facilitates the ability of opposition groups to resist the use of public money for "corporate welfare" that allocates state funds to companies

[11] See Polimetrix (2005) for details. For further analysis of the knowledge of voters regarding tax incentives, see online Appendix S3, in which we document the self-reported knowledge of partisan and independent respondents.

[12] See Acemoglu and Robinson (2001) for a helpful review of these arguments. Seminal work by Magee, Brock, and Young (1989) argue that electoral pressure leads to "optimal obfuscation" of policies. See Rodrik (1995) for a review of the policy economy of different trade instruments.

rather than to citizens. Examples include recent public backlash against state film incentive programs,[13] NGOs that track and criticize incentives such as *Texans for Public Justice* and *Good Jobs First*, and a recent scathing *New York Times* series on US investment subsidies (Story 2012).

These recent examples illustrate the potential for these incentives to be labeled as "corporate welfare" and therefore resented by voters.[14] Thus, an alternative expectation stresses public scrutiny of the use of government funds for economic development and a preference for lower rather than higher use of incentives.

Hypothesis 3 (Cost–benefit analysis): *Voters are more likely to punish a governor who offers investment incentives that are greater than those offered by competing states.*

We believe these hypotheses are generalizable outside the United States, where models of economic voting at the country level share commonalities with those across subnational locations. Yet, theoretical work has not yet fully explored the relationship between the policies of elected officials and citizens' voting intentions.

Empirical Analysis

Our empirical analysis builds on the observational studies on incentives, yet our research design is intended to correct for biases in previous work. First, given the increasing trend of offering tax incentives to attract investment, it is difficult to harness observational analyses. Theoretical and empirical issues, such as endogeneity, complicate statistical identification. Specifically, germane is the issue of unobserved heterogeneity among governors. Particularly, entrepreneurial governors may be the most popular in general and also the most likely to offer investment incentives, leading to a correlation without a direct causal relationship.

Second, we directly examine whether these policies are relevant for voters. This cannot be taken for granted; for example, in a recent study of trade, Guisinger (2009) finds that voters have little knowledge of individual trade policies and that trade has very little impact on vote choice. We address this question by focusing directly on how the offer of tax incentives affects the *voting intentions* of individual voters.

Limitations of causal inference can be mitigated through an experimental approach. Specifically, we use a survey experiment to randomize treatment across individuals. This allows us to test the causal mechanism linking policy to the perceptions of individual voters. Rather than focusing on satisfaction with the governor, we ask respondents how a policy (tax incentives) *changes* the probability of their voting for the incumbent. Consequently, we can isolate the differential effects of incentives from the effects of the underlying flow of investment. This is an especially rigorous test of our hypotheses.

Our analysis uses a survey experiment placed in the Cooperative Congressional Election Survey (CCES), a nationally representative Internet-based survey of 13,800 Americans administered by *YouGovPolimetrix* in October 2009. Respondents were asked demographic questions, queries on voting and partisanship, and tests of their knowledge of how their elected officials had voted on specific legislation. We added our questions to a nationwide subsample of 1,974 respondents.

Our main question was a survey experiment testing how the respondent's intention to vote for the incumbent governor was affected by the governor's use of tax incentives to attract (or at least try to attract) an investment project. The only information we provided on the project was the number of jobs created, so that the respondent recognized the event as influential on the state's economy. Small investment projects would likely not be considered important enough to alter voting behavior. We offered no other information on the investment—in particular, no negative information—because our ultimate research question is whether tax incentives help politicians claim credit for positive economic outcomes. It was critical for our design that the relatively positive depiction of the investment (the number of jobs) had no impact on the voter's ability to separate his or her approval of the investment from his or her approval of the incentive policy (also randomly assigned), which is the ultimate goal of our research design. In other words, we allow for the response: "I'm glad jobs were created, but I still don't want to reward the governor for offering tax incentives." Thus, our survey experiment is a realistic test of how politicians claim credit for economic performance.

Following our theoretical framework, our experiment positions incentives relative to the competition, rather than in isolation. As we noted above, most states offer some form of tax incentive, so what really matters for both firms and voters is the relative difference between a given state and its competitors for a specific project.[15]

The survey experiment divides the sample into four groups with each respondent having a 25-percent probability of receiving one of four treatments. These treatments consisted of two dimensions, investment attraction and tax incentives. Our question reads as follows, with the two treatments in parentheses:

> Your state competed with a number of other states over a new manufacturing plant that will create 1,000 jobs.
>
> With the support of the governor, your state offered a tax incentive (break/reduction) package *that was* [*greater/equal or less*] than that of the other states. If your state [*receives/does not receive*] this investment, how would this effect your evaluation of your governor's performance in office?
>
> (1) I would be much more likely to vote for the governor in the next election.
> (2) I would be slightly more likely to vote for the governor in the next election.
> (3) My vote choice would not be altered.
> (4) I would be slightly less likely to vote for the governor in the next election.
> (5) I would be much less likely to vote for the governor in the next election.

All survey experiments force researchers to make difficult choices about question wording. We felt obligated to use a non-conventional, hypothetical construction—"how

[13] The most high-profile case was a series of scandals involving Iowa's film tax credits. For an outsider auditor's report on fraud, see www.iowalifechanging.com/downloads/filmreport09.pdf.

[14] See Bartlett and Steele (1998) for an earlier exposé on investment subsidies. See also Whitfield (2001) and Dawkins (2002) for work on corporate welfare and Thomas (1997) for work on public campaigns against corporate welfare.

[15] We would have liked to separate the size of the incentives into three groups (greater than, equal to, or less than competitor), but statistical power considerations required combining the latter two groups.

would this…?"—because investment projects of the scale we reference in our survey are sufficiently rare and well documented that a reasonably knowledgeable respondent would know whether or not there really had been such a large investment in his or her state. We did not want these respondents to feel that they were being overtly misled, so we chose a prospective construction that speculated about a future investment. Public opinion researchers have long debated about whether hypothetical questions lead to inaccurate responses (Converse 1964, 1974; Cummings, Harrison, and Rutström 1995). Yet, some scholars—particularly those in the economics literature who use anchoring vignettes, experiments, and contingent valuation designs—have shown empirically that respondents can give meaningful answers to hypothetical questions if (i) a neutral or "don't know" option is provided for uninformed respondents and (ii) the question allows the respondent to draw upon relevant previous experiences to place the hypothetical in context (Mitchell and Carson 1989:173). We made sure to fulfill both of these conditions.

Our choice of a prospective construction had two downstream effects. First, the answer options needed to be prospective as well, referencing a hypothetical vote choice. Secondly, the prospective construction made it difficult to devise a realistic control group as recommended by Gaines et al., because respondents could not be asked to evaluate how their vote would change if there was *no investment* for which to compete and their governor took *no actions* (Gaines, Kuklinski, and Quirk 2007).

For this design, the natural baseline is the state that offered less tax incentives and did not receive the project. From that cell, which receives a zero on both dichotomous options (0,0), we can calculate the impact of winning the project with incentives less than or equal to those of the competitors (Group 2 [1,0]), offering an incentive that is greater than those of the competitors but nevertheless losing the project (Group 3 [0,1]), and offering an incentive that is greater than those of the competitors and winning the project (Group 4 [1,1]). In short, all marginal effects calculated in the empirical analysis are derived from comparing shifts in incentives and investment to the baseline category (0,0).

A final consideration was whether to use vote choice in the option or to use the standard job approval scale. We opted for vote choice because we wanted to simulate, as closely as possible, the political logic of our theory. Moreover, we were concerned that using approval ratings would overstate the impact of the investment, which might play only a small role in voters' ultimate calculations (Guisinger 2009). We also offered a neutral category of "My vote choice would not be altered" to provide an opt-out opportunity to respondents for whom economic issues are less important than other issues. Both of these are conservative choices that were intended to bias against the pandering hypotheses (H1 and H2). As a robustness check, we included a variable for the respondents' rating of governor approval (drawn from the common use questions in the *Polimetrix* survey, which preceded our experiment in the sequencing of the survey) in the empirical analysis, to approximate the level of change in a respondent's vote choice.

Our randomization process worked with only a few very small exceptions; the four treatment samples do not differ dramatically from one another across factors (see online Appendix S1). This indicates that the treatments were uncorrelated with voter characteristics and can therefore be assessed directly. Mutz and Pemantle argue that the display of balance tables is not necessary for survey experiments, as randomization can occasionally lead to unbalanced covariates on some factors and controlling for these factors can actually bias results more than sampling error would (Mutz and Pemantle 2011). In our case, however, the relatively high noncompletion rate (about 30%) of the Web-based *Polimetrix* survey creates concerns about differential attrition rates in some demographics that could be correlated with the treatments. Fortunately, the balance table demonstrates that this is not a serious concern.

As a first cut, we provide a comparison of means of these four groups based on collapsing our dependent variable from a five-point Likert scale. We invert this scale with a score of 5 representing "much more likely to vote for the governor" and 1 as "much less likely." As is to be expected, respondents who learned that their state had won the investment project were more likely to reward their governor than those who did not. Winning the investment increases the likelihood of voting for the governor (checking 4 or 5 on our scale) by about 20 percent (changing the mean score from 2.85 to 3.37). This result was statistically significant at the 95-percent level of confidence. More importantly, offering an investment incentive has a statistically significant but substantively small impact in isolation. Confirming H1, 2.4 percent more voters say they are likely to vote for the governor after hearing an incentive was offered (a change in the mean score from 3.05 to 3.15), whether or not the state won the project.

These results are interesting but do not take into account, the conditioning effect of incentives, depending on whether the project was won. To this end, we construct Table 1, based on the two different treatments for the entire sample of 1,974 respondents. The vertical axis displays the investment treatment, while the horizontal axis displays the incentive treatment. The parentheses in each cell depict 95-percent confidence intervals. Panel A of Table 1 reveals that the impact of incentives is in fact conditional on whether or not the project was won. For states that attracted investment, the difference in the likelihood of voting for the governor barely exceeds zero, which is inconsistent with H1. For states that lost the project, however, 3.5 percent of voters would shift their vote in favor of a governor who offered an investment incentive, raising the average score from 2.76 to 2.95, lending tentative support for H2. This result is significant at the 0.05 level and appears to indicate that voters, as a whole, are supportive of greater investment, but may not be responsive to credit-claiming on the part of elected officials. Rather, they respond more positively to visible effort in a lost cause, offering tentative confirmation of H2. We find no support for H3 in this analysis.

Testing our theory on the aggregate sample, however, may be too ambitious. Party affiliation is quite strong among American voters, and, depending on the state, a significant portion of the electorate may not be motivated to alter their vote choice at all, much less because of a single incident of attracting investment, as indicated by the thresholds in Figure 1. According to our data, about 62 percent of voters self-identify as strong or weak members of a particular party—35 percent as Democrats and 27 percent as Republicans. Repeated analyses at national and local levels have found party identification to be among the strongest determinants of vote choices (Bartels 2000; Hetherington 2001). As every political consultant worth

TABLE 1. Likelihood of Vote for Governor (By Party Affiliation)

	Tax Incentive Greater Than Competitors							
	All (N = 1,974)		*Independents* (N = 453)		*Democrats* (N = 580)		*Republicans* (N = 505)	
	Yes	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*
Received Investment								
Yes	3.38 (3.284–3.469)	3.37 (3.279–3.458)	3.51 (3.323–3.706)	3.38 (3.211–3.558)	3.41 (3.295–3.620)	3.35 (3.262–3.564)	3.19 (3.067–3.396)	3.33 (3.157–3.520)
No	2.95 (2.856–3.037)	2.76 (2.664–2.848)	3.09 (2.923–3.253)	2.72 (2.526–2.919)	2.92 (2.807–3.123)	2.78 (2.618–2.937)	2.91 (2.726–3.083)	2.78 (2.550–2.938)

(*Note.* Comparison of mean response to question: … "How would this effect your evaluation of your governor's performance in office? (i) Much less likely; (ii) Slightly less likely; (iii) Vote choice would not be altered; (iv) Slightly more likely; (v) Much more likely." Ninety percent CI in parentheses.)

his paycheck knows, the battle for election lies in the middle of the distribution among voters who are likely to change their mind based on policy choices. True independents are only about 12.5 percent of the electorate, but if we include those who self-identify as independent but lean toward a particular party (8% lean Democrat and 15% lean Republican), we can carve out a reasonable subsample of 453 observations for our analysis.[16]

Panels 2, 3, and 4 analyze the impact of the treatments individually on Democrats, Republicans, and independents, respectively. While all three groups reward the attraction of investment (Republicans more weakly than the others), the impact of investment incentives is only significant among independent voters.

Among voters who were asked about their states winning the investment project, 44 percent of independents whose state offered incentive packages larger than those of competing states claimed they were more likely to vote for the governor, as opposed to 39 percent of independents whose states offered an incentive equal to or smaller than those of competing states. This leads to a 0.13 (3.51–3.38) difference in mean scores on our Likert scale, which is just shy of the traditional standard of statistical significance.

Incentives offered in losing efforts also pay off with independent voters. While such efforts have a marginal impact on favorable votes (about 2.2%), they do shift 19 percent of the negative voters to the neutral category, significantly increasing the average score on the five-point scale by 0.37 (3.09–2.72) points. Substantively, this is an important result which lends further credibility to our experimental treatment. All voters were told how many new jobs had been created and no one was given any negative information about the project, yet only independent voters significantly responded to tax incentives.

Ordered Probit Analysis of Experiment

While the use of a mean score across the Likert scale is an illustrative first cut, it assumes that one-point shifts across each level of the scale are equivalent. This assumption may be cavalier, as a shift from a score of 2 (slightly less likely to vote) to 3 (no difference) may be very different from a shift from 4 (slightly more likely) to 5 (much more likely). To address this problem, many social science scholars have adopted the use of the ordered probit (*oprobit*) specification for regression analysis (McKelvey and Zavoina 1975), which also helps addresses problems of heteroskedasticity in multiple regression and eliminates the possibility that a predicted probability for a particular unit on the scale will be larger than one.

Table 2 displays an ordered probit analysis of the two treatments on voter choice. Model 1 presents the results for the entire sample. Both the investment and incentives treatments are significant at the 95-percent level. Receiving an investment project increases the probability of a vote for governor by 18.2 percent (0.0672 for option 4

[16] Petrocik (2009) distinguishes between true independents and *leaners*, who are just as partisan and unlikely to be persuaded as those who self-identify as weak members of a particular party. Including leaners, therefore, is akin to including partisans. Unfortunately, there is little we can do to resolve this problem. Self-identified independents are a very small group (179 observations), accounting for fewer than 50 observations per cell. Randomized treatments on such small groups offer insufficient statistical power. As a second-best alternative, we use only independents who registered to vote. This choice should bias against finding differences between independent and partisan voters.

and 0.115% for option 5), while offering an incentive increases the likelihood of a vote for governor by 3.4 percent (0.0113 for option 4 and 0.0215 for option 5).

While these results are quite strong, one concern is that responses to the treatment may be correlated with characteristics of the respondent, leading to omitted-variable bias. By design, randomized survey experiments are meant to sidestep this problem by ensuring that voter characteristics are orthogonal to the treatment and we confirm this.[17] Nevertheless, while the treatments are randomly assigned, governors are not. The treatments may therefore have differential effects depending on the interaction between voters and the governor.

To address this problem, Model 2 adds two state-level variables that are critical in the electoral performance of incumbent governors: the governor's state-level approval ratings and state unemployment for the previous year.[18] In states with highly unpopular governors, the treatment may be too weak to sway vote choices, while the opposite may be true in states with very popular governors or excellent economic performance over the past year. Under the latter happy circumstances, how much more certain can a voter be that he or she would vote for the incumbent? The state-level approval rating does prove statistically significant, but substantively small. Each one-percent increase in prior approval ratings increases the likelihood of a vote increase by 0.02 percent. The effect of unemployment, however, is not statistically different from zero. Most importantly, our treatment variables are not affected.

Nevertheless, the two controls above are insufficient to rule out state-level effects. There is a possibility that our results may be driven by unobservable factors among a small group of states where changes in voter choice are most likely. State fixed effects in Model 3 allow us to address unobserved state-level heterogeneity, ensuring that our results are not an artifact of large swings in only a handful of states. Our results are robust to this change in specification (see Model 3), but it is worth noting that the inclusion of state effects increases the substantive effect of the investment treatment.[19]

Finally, Model 4 controls for a respondent's current governor-approval rating, which is asked in the CCES before our experimental question. Controlling for pretreatment governor approval does not offer a perfect baseline comparison or diff-in-diff analysis, but it does allow us to differentiate a respondent's change in views about the governor that occurs as a result of the experimental treatment from the respondent's pretreatment assessment. The coefficient on this control variable, which is substantively large and statistically significant, indicates that many respondents maintained their opinions of the governor despite the new information. Nevertheless, this additional variable strengthens the marginal effects of both the incentives and the FDI attraction treatments.

The next three panels of Table 2 rerun all four models, but restrict the analysis to Independents, Democrats, and Republicans, respectively. All three groups are positively affected by the attraction of new investment, but the effect is greater for independents than it is for the other two groups. The impact of investment on Republican voting is less than the national average. When it comes to incentives, there are even starker differences. Only among independent voters does offering incentives greater than those of the competitors have a positive impact on voting for the governor. In Model 5, the marginal effect of incentives on the probability of independents voting for a governor is 9.2 percent (4.2% for option 4 and 5.0% for option 5). The impact for Democrats and Republicans is not significantly different from zero.

Conditional Effects of Incentives

H2 stated that voters will respond favorably to investment incentives in a losing effort. Drawing on the economics literature, voters may be unwilling to pay incentives ("provide giveaways" to corporations) for investment they believe their state would have won anyway, an insight that underlies H3. Tentative support for this conclusion was found in the comparison of means in Table 1. To test the conditional effect of investment incentives more rigorously, we interact the two treatments in Table 3. Model 1 performs the analysis for the full sample, while Model 2 limits the observations to independents. Models 3 and 4 demonstrate that the results survive state fixed effects, while Models 5 and 6 add the pretreatment, respondent-level governor-approval rating.

Caution should be taken in interpreting the interaction effects in Table 3, as Norton, Wang, and Ai (2004) have shown that the calculation of interaction effects in nonlinear models is not analogous to that in linear models and that coefficients and even the sign of effects can therefore not be read directly off regression tables.[20] The marginal effects of incentives under different conditions are displayed in Table 4.

Beginning with the baseline treatment in the full sample (no project, no incentive), we find that the predicted probability of options 4 and 5 (higher likelihood of voting for the governor) is 14.5 percent. Respondents who were asked about their state winning the investment without offering a greater incentive had a predicted probability of voting for the governor of 35 percent. The 20-percent marginal effect of investment attraction without incentives is statistically significant at the 0.05 level. For states that offered a greater incentive but still did not attract investment, the predicted probability of voting for the governor was 19.6 percent. Investment lured (or at least believed to have been lured) by greater incentives increased the predicted probability of voting for the governor to 42.7 percent. Thus, the marginal effect of investment attraction in the presence of incentives (shifting from Group 3 to Group 4) is 23.1 percent.

[17] See online Appendix S1 for balance tests.

[18] The governor approval data come from SurveyUSA 50 State Tracking of Job Approval numbers (http://www.surveyusa.com/50StateTracking.html) for the month of September of 2009 (when our survey was in the field). We supplemented this with data from http://www.pollster.com for states that did not have September approval data.

[19] A substantial econometric literature has expressed concerns about fixed effects leading to bias in probit and logit estimates. See Abrevaya (1997), Katz (2001), Greene (2004), and Coupe (2005). This bias, however, is thought to be most pronounced when the number of dummy variables is less than twenty. With 50 states, the bias should be limited. Out of caution, however, we interpret the substantive effects from the unadjusted Model 1.

[20] Norton et al. (2004). Their prepackaged program for analyzing nonlinear interactions, inteff, is only applicable to dichotomous variables, but its intuition has been built into STATA's prvalue, which can be used with the oprobit model. Using this command, we calculate the predicted effects for all four treatment possibilities, which are displayed graphically in online Appendix S2.

TABLE 2. Ordered Probit Model of Likelihood of Voting for Governor

Dependent Variable: Likelihood of Voting for Governor in Next Election	Full Sample				Independents				Democrats				Republicans			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
Incentive > Competitor	0.109	0.126	0.127	0.153	0.270	0.195	0.307	0.333	0.112	0.194	0.119	0.140	−0.00520	−0.0278	−0.00793	−0.0124
	(0.0493)**	(0.0550)**	(0.0497)**	(0.0516)***	(0.103)***	(0.117)*	(0.110)***	(0.112)***	(0.0916)	(0.103)*	(0.0948)	(0.0984)	(0.0985)	(0.109)	(0.107)	(0.110)
Received Investment	0.576	0.570	0.570	0.582	0.622	0.629	0.688	0.728	0.588	0.585	0.599	0.611	0.445	0.461	0.491	0.541
	(0.0516)***	(0.0575)***	(0.0520)***	(0.0545)***	(0.108)***	(0.123)***	(0.113)***	(0.114)***	(0.0958)***	(0.107)***	(0.102)***	(0.106)***	(0.102)***	(0.114)***	(0.111)***	(0.115)***
Governor Approval 2010 (state-level)		0.00538				0.000669				0.00539				0.0121		
		(0.00219)**				(0.00410)				(0.00433)				(0.00463)***		
State Unemployment 2009–2010 (state-level)		0.0121				0.0411				−0.0418				0.0846		
		(0.0189)				(0.0402)				(0.0341)				(0.0393)**		
Pretest Governor Approval (individual-level)				0.463				0.465				0.440				0.418
				(0.0322)***				(0.0750)***				(0.0676)***				(0.0856)***
State Fixed Effects	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes	No	No	Yes	Yes
Cut Point 1	−0.961	−0.586	−0.830	0.216	−1.044	−0.622	−1.461	−0.398	−0.960	−1.105	−0.986	−0.163	−0.939	0.420	−0.592	0.645
	(0.0480)***	(0.250)**	(0.172)***	(0.220)	(0.107)***	(0.504)	(0.315)***	(0.415)	(0.0885)***	(0.466)**	(0.142)***	(0.175)	(0.0962)***	(0.525)	(0.353)*	(0.458)
Cut Point 2	−0.641	−0.286	−0.507	0.570	−0.588	−0.236	−0.970	0.132	−0.645	−0.793	−0.637	0.204	−0.724	0.634	−0.354	0.905
	(0.0459)***	(0.249)	(0.173)***	(0.220)***	(0.0986)***	(0.506)	(0.317)***	(0.414)	(0.0842)***	(0.467)*	(0.136)***	(0.170)	(0.0959)***	(0.523)	(0.359)	(0.464)*
Cut Point 3	1.010	1.375	1.175	2.375	1.005	1.390	0.761	1.975	1.040	0.931	1.196	2.150	0.976	2.316	1.540	2.896
	(0.0500)***	(0.251)***	(0.174)***	(0.224)***	(0.107)***	(0.514)***	(0.317)**	(0.419)***	(0.0932)***	(0.470)**	(0.145)***	(0.195)***	(0.103)***	(0.533)***	(0.363)***	(0.477)***
Cut Point 4	1.521	1.906	1.698	2.942	1.678	2.111	1.512	2.813	1.492	1.383	1.686	2.640	1.370	2.702	1.988	3.363
	(0.0572)***	(0.253)***	(0.175)***	(0.226)***	(0.125)***	(0.526)***	(0.321)***	(0.427)***	(0.106)***	(0.474)***	(0.157)***	(0.206)***	(0.112)***	(0.539)***	(0.369)***	(0.482)***
Observations	1,974	1,595	1,974	1,874	453	356	453	441	580	468	580	552	505	411	505	491
States	50	50	50	50	50	50	50	50	50	50	50	50	50	50	50	50
Pseudo R^2	0.0270	0.0283	0.0389	0.0890	0.0353	0.0347	0.0981	0.145	0.0281	0.0340	0.0844	0.118	0.0161	0.0238	0.0929	0.125
Log Likelihood	−2523	−2029	−2523	−2400	−593.2	−459.0	−593.2	−579.0	−732.6	−584.7	−732.6	−692.4	−618.3	−508.8	−618.3	−601.3
Chi-squared	127.6	107.5	208.5		36.71	28.27	1328		38.23	37.35	–	–	19.07	21.67	–	–

Marginal Probability of Voting for Governor from Ordered Probit

Sample (Value Selected)	Full Sample		Independents		Democrats		Republicans	
	(4)	(5)	(4)	(5)	(4)	(5)	(4)	(5)
Received Investment	0.0672 (0.00744)***	0.115 (0.0105)***	0.0930 (0.0194)***	0.117 (0.0209)***	0.0608 (0.0125)***	0.122 (0.0203)***	0.0410 (0.0112)***	0.0918 (0.0207)***
Incentive > Competitor	0.0131 (0.00597)**	0.0215 (0.00974)**	0.0416 (0.0161)***	0.0501 (0.0195)**	0.0120 (0.00995)	0.0229 (0.0187)	−0.000488 (0.00924)	−0.00108 (0.0204)

(Notes. Robust standard errors, clustered at state level, in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$). Dependent variable measured on a five-point scale from (1) Much less likely to (5) Much more likely. Observations drop in models 2, 6, 10, and 14 as approval ratings were not available for all states. Chi-squared test statistic of overall model fit could not be calculated in the presence of state fixed effects in some models.
Marginal probabilities derived from models 1, 5, 9, and 13. These figures represent the change in probability of selecting values 4 (More likely) and 5 (Much more likely) from a one-unit change in the treatment variables.)

Table 3. Conditional Effects of Experimental Treatments on Vote

	Ordered Probit (5-Point Scale)						*Probit (Dichotomous)*
Dependent Variable: Likelihood of Voting for Governor in Next Election	*Full Sample* (1)	*Independents* (2)	*Full Sample* (3)	*Independents* (4)	*Full Sample* (5)	*Independents* (6)	*Full Sample* (7)
Incentive > Competitor	0.204 (0.073)***	0.391 (0.102)***	0.222 (0.076)***	0.523 (0.128)***	0.278 (0.081)***	0.632 (0.139)***	0.170 (0.081)**
Received Investment	0.673 (0.072)***	0.740 (0.153)***	0.668 (0.076)***	0.906 (0.183)***	0.711 (0.061)***	1.031 (0.173)***	0.156 (0.134)
Incentive*Investment	−0.195 (0.099)**	−0.244 (0.180)	−0.197 (0.099)**	−0.448 (0.220)**	−0.258 (0.090)***	−0.616 (0.230)***	−0.082 (0.102)
Pretest Governor Approval (individual-level)					0.465 (0.036)***	0.475 (0.063)***	
Governor Approval 2010 (state-level)							0.002 (0.002)
Incentive*Approval							−0.003 (0.002)*
Investment_Approval							0.002 (0.003)
Incentive*Investment*Approval							0.001 (0.002)
Unemployment 2009–2010							0.002 (0.009)
State Fixed Effects	No	No	Yes	Yes	Yes	Yes	No
Cut Point 1	−0.916 (0.057)***	−0.990 (0.099)***	−0.774 (0.053)***	−1.370 (0.087)***	0.289 (0.109)***	−0.270 (0.160)*	
Cut Point 2	−0.596 (0.048)***	−0.531 (0.088)***	−0.450 (0.041)***	−0.872 (0.085)***	0.644 (0.099)***	0.272 (0.163)*	
Cut Point 3	1.058 (0.070)***	1.066 (0.127)***	1.235 (0.066)***	0.871 (0.109)***	2.454 (0.102)***	2.140 (0.199)***	
Cut Point 4	1.569 (0.072)***	1.737 (0.145)***	1.758 (0.069)***	1.621 (0.139)***	3.022 (0.110)***	2.978 (0.218)***	
Observations	1,974	453	1,974	453	1,874	441	1,595
States	50	50	50	50	50	50	50
Pseudo R^2	0.0278	0.0365	0.0396	0.102	0.0903	0.152	0.0538
Log Likelihood	−2523	−593.2	−2523	−593.2	−2400	−579.0	176.9

(*Note.* Robust standard errors, clustered at state level, in parentheses (***$p < 0.01$, **$p < 0.05$, *$p < 0.1$). For Models 1–6, the dependent variable is measured on a five-point scale from (1) Much less likely to (5) Much more likely. Model 7 uses a dichotomous variable if the ordinal scale is equal to 4 or 5. The table depicts marginal effects for model 7. Observations drop in model 7, as approval ratings were not available for all states.)

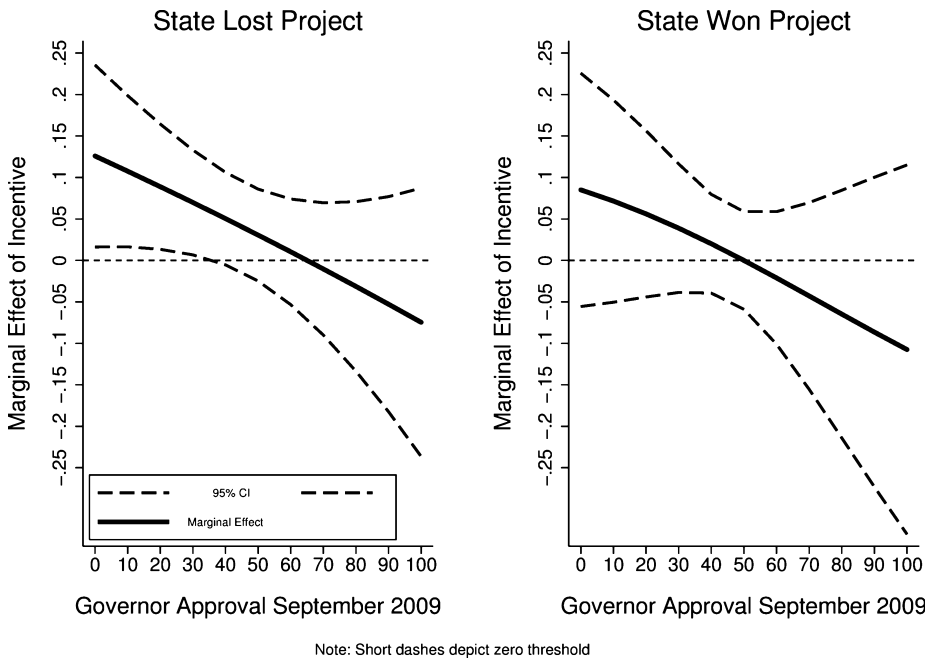

FIG 1. Marginal Effect of Offering Tax Incentive Greater Than Competitors, Conditioned by Approval Rating of Governor. Derived from Model 7, Table 3. Calculated using Stata's *prvalue* function (Long and Freese (2005)).

The marginal effect of investment attraction on voter behavior is not surprising. Far more interesting from our perspective is the marginal effect of greater tax incentives on voter behavior, which we depict in Table 4. The results clearly indicate that incentives do less to generate votes when the state has successfully attracted the project. In terms of our theory, the blame-avoidance effect (H2) is stronger than the credit-claiming effect (H1). When the project has been won, incentives only provide the incumbent governor 0.032 percent (0.0009 + 0.0023) of the vote among all respondents, which is not significantly different from zero. Incentives offered in a losing effort, however, are far more beneficial, providing the governor with a statistically significant vote bonus of 5.15 percent (0.0237 + 0.0278), consistent with H2. Among independents, incentives offer even greater opportunities for credit-claiming, increasing votes for the governor by 5.65 percent (0.0184 + 0.0381) in a winning effort. But this is only about half of their impact on votes garnered in a losing effort (10.65%, 0.0586 + 0.0479), a statistically significant difference.

The fact that incentives pay political dividends even in losing efforts is important, because it creates a dominant strategy for governors under certain conditions. An incumbent worried about reelection prospects can rest assured that he or she will receive a large vote bonus from the investment coming to his or her state and a slightly smaller vote bonus just for offering the incentive. If the state loses the project, however, voters will still reward the governor for his or her effort; that is, offering the incentive. Electorally, an incumbent cannot lose by offering a tax incentive to a highly visible project.

Caution is always in order in the interpretation of survey experiments, as modest changes in the framing of questions can substantially alter the responses. Once concern for our project is the fact that incentives are tested in isolation; we do not present potential trade-offs with other policies that voters may appreciate, such as social spending, infrastructure enhancements, education, and tax cuts for individuals. For the incentives question in this paper, we demurred to test the trade-off directly for three reasons. First, we wanted, as much as possible, to avoid priming the respondent in a particular direction. Of course, some voters may already hold an opinion that tax incentives are a form of "corporate welfare" that should instead be spent on citizens. Our experiment did nothing to dissuade them of this opinion and simply captures their response (H3). As the results reveal, Republican voters appear to hold this view. Secondly, the notion of presenting incentives without trade-offs is not artificial. In fact, this is exactly the way they are presented by politicians. Press releases announcing tax incentives rarely say, "Boeing received a 5-year tax holiday, which will be paid for with cuts in educational spending." To get a clean result on how politicians use tax incentives, it was important to frame the issue the way voters actually encounter it.

In this project, we treated all firms as equal, but there is reason to believe that voters may respond differently to incentives intended for foreign investors. Consequently, in follow-up survey, we conducted an additional robustness test on whether governors receive less support by offering tax incentives to foreign firms.[21] Our preliminary evidence finds that offering incentives to foreign firms dampens support for the governor's credit-claiming.

A final empirical consideration is whether it is safe to generalize across states. The large amount of variance explained by state fixed effects in these models leads to the hypothesis that the impact of politicians' use of incentives to claim credit may be contingent on state-level economic and political factors. In Model 7 of Table 3, we explore this conditional hypothesis by interacting state

[21] In a 2010 CCES survey experiment, we varied whether or not the investment was "foreign," "out-of-state," or "in-state." This allows us to compare the effect of foreign firms with that of domestic (in-state or out-of-state) firms and to compare the effect of local firms with that of mobile (foreign and out-of-state) firms. Our experiment was not designed to test blame-avoidance, and thus, all three treatments indicate the state won the investment project.

Table 4. Marginal Effects of Tax Incentive Greater Than Competing States (Change in Probability of Voting for Governor in Next Election)

All Respondents		*Independents*	
State Won Investment Project		*State Won Investment Project*	
Much Less Likely	−0.001 [−0.0162, 0.0143]	Much less likely	−0.0115 [−0.0340, 0.0110]
Slightly Less Likely	−0.0006 [−0.0007, −0.0005]	Slightly less likely	−0.0122 [−0.0165, −0.0078]
No Change	−0.0016 [−0.0017, −0.0016]	No change	−0.0328 [−0.0352, −0.0304]
Slightly More Likely	0.0009 [0.0008, 0.0010]	Slightly more likely	0.0184 [0.0136, 0.0232]
Much More Likely	0.0023 [−0.0342, 0.0388]	Much more likely	0.0381 [−0.0365, 0.1126]
State Lost Investment Project		*State Lost Investment Project*	
Much Less Likely	−0.0485 [−0.0811, −0.0159]	Much less likely	−0.0774 [−0.1364, −0.0183]
Slightly Less Likely	−0.0153 [−0.0176, −0.0130]	Slightly less likely	−0.042 [−0.0537, −0.0303]
No Change	0.0122 [0.0083, 0.0161]	No change	0.0128 [−0.0025, 0.0282]
Slightly More Likely	0.0237 [0.0214, 0.0261]	Slightly more likely	0.0586 [0.0487, 0.0686]
Much More Likely	0.0278 [0.0088, 0.0468]	Much more likely	0.0479 [0.0112, 0.0845]

(Note. 95% confidence intervals in parentheses. Results derived from Table 3 (Models 1 and 2). Marginal effects for interactions calculated with STATA's *prvalue* function (Long and Freese 2005).)

governor-approval numbers at the time of the survey with the two treatments.[22] The model demonstrates that governor approval negatively conditions investment incentives. In other words, highly popular governors benefit less from investment incentives than unpopular governors do.

Figure 1 charts the interaction effect between incentives and approval. The *y*-axis plots the marginal effect of incentives on changing one's vote for governor. The *x*-axis plots governor approval in 2009. The solid line charts the predicted change in the probability of changing one's vote in favor of the governor when a greater incentive is offered, while dashed lines depict 95-percent confidence intervals. Panels are separated by whether the state won or lost the investment. The chart shows that whether a state wins or loses the project, the vote bonus of offering the incentive declines with popularity. The positive effect of the incentives, however, is statistically significant only for unpopular governors (those with lower than 50-percent approval) who make losing efforts.[23]

The result indicates that offering incentives works best as part of a gambling-for-resurrection strategy, whereby unpopular governors have a higher incentive to engage in an economically risky policy that might play well with voters and at least won't turn voters against them. This finding is speculative, as the interaction was not included in our original experimental protocol, but it certainly points to an interesting direction for future research.

Conclusion

In this paper, we address the political motivations of politicians offering generous incentives to attract investment projects despite the uncertainty of the economic benefits of such incentives. Rather than focusing on uninformed politicians or distributional considerations, we build on a "pandering" theory, in which politicians provide incentives even if they have perfect information that they are ineffective. They use these incentives to take credit for new investment or to deflect blame for hoped-for investment that ends up in other states, reaping electoral rewards for an economically inefficient policy.

Our work does not falsify the existing theories on how competitive pressures can lead governments into a prisoner's dilemma competition for capital. But it does show that the incentive "wars" can have a positive impact on a politicians reelection prospects. Offering incentives to firms has political benefits to incumbent politicians.

We test our theory through the use of a survey experiment with a nationally representative sample of Americans. We find strong evidence that voters, especially independents, are more likely to vote for incumbent politicians who use tax incentives to attract investment. Politicians are rewarded more strongly if they offer incentives in a losing effort, leading to a dominant strategy under certain economic conditions. Whether or not a state has a chance to win a project, the governor should publicly demonstrate some effort by offering an incentive package better than that of other states. Finally, we show that the incentive for governors to offer investment is mediated by their approval ratings. Popular governors have less need to gamble for resurrection by pursuing economically damaging but politically advantageous policies. In addition, we highlight a number of other provocative findings on the relationship between tax incentives, voters, and credit-claiming.

Our findings have broader implications concerning the relationship between globalization and the nation-state. While numerous scholars have examined how trade and investment can constrain politicians' behavior, little work has focused on how globalization affects political accountability. We find evidence that the movement of capital can provide opportunities for politicians to pander to the public and take credit for new investment flowing into their states or districts. Thus, globalization can make domestic politics more rather than less relevant, leading to increased political activity aimed at taking credit or avoiding blame for the local outcomes of globalization. As politicians become more constrained in their ability to make policy choices, such as monetary policy, we may see an increased incentive for highly visible policies aimed at taking credit for the workings of global market forces.

[22] Interactions with state unemployment figures were substantively small and not statistically different from zero. Note that approval numbers are only available for 33 states.

[23] Unfortunately, this same effect cannot be estimated efficiently for independents, as there are simply too few observations in each cell generated by the two treatments and the two interactions.

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