The Spoils of Victory: Campaign Donations and Government Contracts in Brazil

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When firms give money to candidates for public office, what return can they expect on their investment? Prior studies have been inconclusive, due to both methodological challenges and unique features of the U.S. political context on which they have focused. Using data from Brazil, we employ a regression discontinuity (RD) design to identify the effect of an electoral victory on government contracts for a candidate's corporate donors. Firms specializing in public-works projects can expect a substantial boost in contracts—at least 14 times the value of their contributions—when they donate to a federal-deputy candidate from the ruling Workers' Party (PT) and that candidate wins office. We find no effects among allied parties, indicating that the PT prioritizes this form of state spending for party strengthening rather than coalition management.

When firms give money to candidates for public office, what return can they expect on their investment?[1] In many democracies, public sentiment takes the buying and selling of policy as a basic political fact. By contrast, the largely U.S.-focused empirical literature on corporate donations and policymaking is inconclusive (Milyo 1999; Stratmann 2005). In challenging the popular consensus, scholars have identified methodological problems with the traditional means of demonstrating the influence of corporate money on policy outcomes. They have also pointed to features of the U.S. electoral environment, such as mass partisanship and the predominance of small, individual donors, that may weaken firms' ability to "buy" elected politicians (Ansolabehere, de Figueiredo, and Snyder 2003, 124). Yet the applicability of these findings beyond the United States remains largely unexplored. In a different political and economic environment, and with a research design that addresses common methodological challenges, will the

perception that corporate money buys policy outcomes be supported or rejected?

In this analysis of Brazil, we find that political investment by corporations does reap large public policy returns. Using a regression discontinuity (RD) design, we compare candidates for the Chamber of Deputies who barely won or barely lost the 2006 election, examining the effect of an electoral victory on government contracts for the firms who donated to their campaigns. Among candidates from the ruling Workers' Party (PT) who received donations from public-works firms, we find that, on average, an electoral victory brings an additional 138,601 to 346,267 *reais* (US $73,921 to $184,676) in government contracts for each corporate donor—14 to 39 times the average contribution. We find no effect among other parties' candidates, including the PT's legislative allies.

Our research design circumvents several methodological problems that have plagued prior studies. A common approach—regressing a measure of policy

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outcomes, such as legislators' roll-call votes, on the amount of corporate donations received—risks endogeneity bias because shared ideological proclivities may be the cause of both campaign contributions and legislators' votes (Ansolabehere, de Figueiredo, and Snyder 2003; Bronars and Lott 1997). We examine a different question than most prior studies—the effect of an electoral victory versus the effect of an additional dollar donated—but one that better lends itself to making causal inferences. In the context of the regression discontinuity design, our treatment of interest—whether the recipient of corporate donations barely wins or barely loses an election—can be considered "as if" it were randomly assigned and thus independent of either ideology or donations received.

Our choice of dependent variable—government contracts for a candidate's donor firms—also improves upon the existing literature in terms of both measurement and causal inference. Specific roll-call votes and other legislative actions by politicians are often difficult to link back to the interests of individual donors, since multiple firms may profit from a particular policy (Gordon, Hafer, and Landa 2007). In contrast, government contracts benefit the specific firms that are hired to do the work, not the industry at large.[2] Furthermore, allocating state spending for public-works projects is a less ideologically charged activity than voting on industry regulation or corporate taxation, for example. Hence, our estimates are less likely to suffer bias from shared ideological affinities than studies that analyze roll-call votes or interest-group scores.

Our finding that an electoral victory brings increased contracts for the candidate's donor firms—but only among candidates from the governing party, and not its legislative allies—reinforces existing arguments that the ruling PT has taken an unconventional approach to managing its governing coalition (Amorim Neto 2007; Samuels 2008; Pereira, Power, and Raile 2011). While previous presidents generously shared the spoils of office with coalition members, PT president Luiz Inácio Lula da Silva hoarded many of the most valuable benefits of incumbency, such as bureaucratic appointments, for his own party. We show that the related and highly lucrative resource of direct government spending was similarly used to benefit the ruling party—rewarding donors to victori-

ous PT candidates in a way that could encourage repeat contributions during future electoral bids.

Background and Theory

Trading corporate donations for public policy requires a strong demand for particularistic benefits by individual firms, and this demand is shaped by political and economic institutions. At a macro level, varieties of capitalism condition firms' incentives to donate to politicians (Hall and Soskice 2001; McMenamin 2013). In coordinated market economies, strong business associations give firms collective input into the policy process, and coalition governments often include at least one party that will exercise veto power over antibusiness policies. As a result, corporate funding of political parties is limited in countries such as Germany, Norway, and Denmark, and patterns of support suggest ideological motivations rather than pragmatic efforts to influence policy. In liberal market economies, individual firms have greater demand for political influence because business associations are weaker and supply fewer collective goods, interfirm competition is more intense, and majoritarian political institutions allow for more drastic policy swings. In countries like Australia, Canada, the United States, and the United Kingdom, firms give more money to parties and politicians, and they do so pragmatically, taking into consideration who can deliver their preferred policies (McMenamin 2013; Schneider 2013).

The nature of political economies in Latin America also suggests that individual firms should demand input into the policy process and be willing to pay for it. Latin American economies are dominated by multinational corporations and highly diversified domestic conglomerates that are often controlled by single families. Sectoral business associations tend to provide a weak basis for political influence because subsidiaries in a given sector have little autonomy to coordinate with other firms and pursue collective policy preferences. Economy-wide business associations are strong in a few countries but weak in many others, including Brazil. Interfirm relations are less competitive and more oligopolistic than in liberal market economies, but industry leaders have an incentive to seek political influence in order to maintain their dominance, and smaller firms may need political connections in order to compete with them (Schneider 2004, 2013).

Latin America's political institutions similarly suggest that firms should seek particularistic policy benefits. Most Latin American countries combine presidentialism with proportional representation.

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[2] Claessens, Feijen, and Laeven (2008) examine the effect of campaign contributions on Brazilian firms' stock-market valuations and access to bank financing. These outcomes also benefit specific firms, but only bank financing can be influenced by politicians, and only indirectly via their influence over state-owned banks, part of a lending market also served by private banks.

Presidentialism allows for greater volatility in policies of interest to businesses, giving them an incentive to cultivate direct ties to politicians. Meanwhile, proportional representation makes legislative coalitions the norm, exerting a check on executive power and offering a variety of parties and politicians that firms can pay off to gain policy influence (Schneider 2013).

Campaign Donations and Government Contracts in Brazil

If Latin America provides a general context in which firms should demand particularistic policy benefits, Brazil is a case where politicians should be especially motivated to seek corporate campaign contributions. Thanks to weak party loyalties and partisan cues, campaigns in Brazil are likely to influence electoral outcomes, giving candidates strong incentives to raise money for them. Around 40–45% of Brazilians claim to identify with a party, though most of these identify with a single party, the PT. Other parties boast no more than 6–7% of the electorate as sympathizers; most have less than 1% (Samuels 2006). Nor does a candidate's party label serve as an effective voting cue. In the 2006 federal-deputy election, an average of 15.3 parties presented candidates in each electoral district, and 7.3 of these parties won seats. Inferring meaningful distinctions among candidates from so many parties is a challenging task. Party switching after the election is also common (Desposato 2006), making affiliation on the ballot an even less useful heuristic.

Campaigns are also intensely competitive, expensive, and require extensive fundraising from private donors. Open-list proportional representation for federal-deputy elections pits candidates against their list mates as well as those from opposing parties. As a result, they cannot rely on party organizations to get them elected and must develop a capacity for individual campaigning. Federal deputies are chosen in statewide districts, meaning that they face an overwhelming number of competitors and must spend lavishly to stand out of the crowd (Samuels 2001b). Public funding is allocated only to parties, and quantities are small in relation to the cost of campaigns (Bourdoukan 2010). Parties can transfer money to candidates, but they tend to concentrate on executive races.[3]

Brazil's electoral regulations are especially permissive toward corporate donations, facilitating firms' efforts to buy political influence. Businesses are allowed to give money directly to candidates, up to 2% of their gross annual revenues. In contrast to the United States, where the sum of individual contributions dwarfs those from Political Action Committees (Ansolabehere, de Figueiredo, and Snyder 2003), corporate money constitutes the predominant source of campaign financing in Brazil. In 2006, 55% of funds raised by federal-deputy candidates came from corporate donors, versus 34% from individuals.[4]

Finally, Brazil's political system allows candidates and donors to make credible commitments to one another. Career politicians are more common than one-shot amateurs, so repeat interactions between candidate and donor are likely. Among incumbents in the 2003–2006 legislature, 76% ran for reelection, and 59% of these rerunners were returned to office. These numbers are lower than in the United States, but many politicians move back and forth between positions at the municipal, state, and federal levels, allowing donor firms to follow them throughout their careers and exploit their influence over the spending decisions of different governments (Samuels 2003). Because of the potential long-term nature of candidate-donor relationships, each actor has the capacity to sanction the other if he or she reneges (Samuels 2001a).

Traditionally, donors and politicians have taken their reciprocal obligations quite seriously, setting up carefully specified agreements about the exchange of legislative benefits for cash payments or campaign donations. In a 1993 scandal, senior members of the congressional Joint Budget Committee were found to have been accepting payment from a cartel of construction firms for getting pork-barrel projects inserted into the federal budget. Internal documents from one firm specified kickbacks of up to 3% of the project's value for each budget-committee member who helped get it approved. Initial payments were made in cash, but when corporate campaign donations were legalized in 1993, compensation continued through this less risky route (Krieger, Rodrigues, and Bonassa 1994, 185-205). More recently, in 2011, the federal deputy who headed the government-allied Party of the Republic (PR) was alleged to have used his party's control over the Ministry of Transportation to solicit kickbacks from construction firms. Firms were required to pay 4–5% of the contracts' value to

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[3]In 2006, donations from parties and other candidates' committees accounted for only 6.5% of total fundraising for federal deputies.

[4]Officially registered donations are only part of the campaign-finance picture in Brazil; off-the-books contributions are also quite common. However, substantial sums of money are reflected in the official figures, so regardless of how much more is given under the table, legal campaign financing is still a high-stakes game.

the PR to guarantee that funding would be approved and released. Firms in good standing with the PR also received increases in the value of ongoing contracts with the National Department of Transportation Infrastructure (DNIT) (Folha de São Paulo 2011).

The specific payback sought by corporate campaign donors is likely to depend upon their industry. Firms that are subject to government oversight, such as private banks, may prefer loose regulation; those that rely heavily on credit, such as agricultural producers, may seek subsidized loans from the National Economic and Social Development Bank (BNDES). As is suggested by Brazil's history of campaign-finance scandals, however, the real action seems to lie with firms in those industries, such as construction, that do substantial amounts of work for the federal government. For public-works firms such as these, payback for campaign donations can take the form of direct federal spending on important national development priorities such as building highways or maintaining ports and navigable waterways. Unlike favorable regulation, government contracts can be targeted to specific firms, and in contrast to BNDES loans (controlled by the executive branch), individual legislators have direct say via the annual budgeting process.

Legislators can influence federal-government spending through a variety of mechanisms. The annual budgeting process begins with the executive sending a bill to congress. Before being scheduled for a vote, the bill is reviewed and amended by the Joint Budget Committee and then the entire Senate and Chamber of Deputies, giving legislators multiple opportunities to propose spending that could benefit their campaign donors (Tollini 2009). Once the budget is signed into law, the game enters a new phase focused on the executive, who during the fiscal year can choose to reduce spending for any line item in the budget or to eliminate it entirely. Though lacking decision-making power at this point, legislators play a key role in lobbying the executive for budgeted funds to be spent and for their preferred contractors to be hired. They also can lobby the executive at the front end of the process, proposing that their spending priorities be inserted into the initial budget bill.

While most existing studies have focused on legislators' individual budget amendments (e.g., Ames 2001; Limongi and Figueiredo 2007; Pereira and Mueller 2004; Samuels 2002), our research examines direct federal-government expenditures, a more lucrative source of funds. Individual amendments, which often fund pork-barrel projects in a deputy's bailiwick, typically authorize transfers (convênios) to state or municipal governments, which carry out the contracting on their own. By contrast, direct federal-government expenditures are normally proposed in the executive's initial budget bill or via amendments by congressional committees and state-based congressional delegations. Unlike individual amendments, these funding sources are not subject to spending limits, so they can propose massive contracts. Payments from these sources are also more likely to be released by the executive during the fiscal year (Limongi and Figueiredo 2007).

For firms that stand to benefit from government contracts, getting funds authorized and disbursed by the federal government is often a more crucial game than getting hired in the first place. The budgeting process is technically prior to the awarding of contracts, but there is often little question as to who will be hired to carry out a particular project. It is not uncommon for the winning bid to have been chosen before federal funding is actually approved (Santos, Machado, and Rocha 1997); the firm in question may even draft the amendment that authorizes the spending (Krieger, Rodrigues, and Bonassa 1994). Moreover, once hired for a particular project, firms are likely to be rehired in subsequent years if the project extends over a 12-month period, as large-scale infrastructure projects typically do. Renewed spending for multiyear projects is proposed annually as a matter of course, but the *amount* of spending in a particular year—essentially, the payment to a firm that has already been selected—can easily be adjusted up or down.

Coalition Dynamics and the Donations-for-Contracts Game

While all deputies are likely to seek government contracts for their corporate campaign donors, some should be better positioned than others to secure them. Who can most effectively deliver these benefits depends on the executive's approach to forming and managing a legislative coalition. Brazilian presidents typically construct ideologically diverse, supermajority coalitions to ensure sufficient votes for their legislative agendas, a strategy known as coalitional presidentialism (Power 2010). Initial alliances are purchased "wholesale," through the distribution of cabinet ministries and other patronage to parties (Amorim Neto 2007). However, because of weak party discipline, and because the initial payment in the form of cabinet posts may not have been enough to ensure ongoing loyalty, presidents also have to manage the coalition by making "retail" payments to individual legislators (Raile, Pereira, and Power 2011). Several currencies are available: direct federal spending that benefits their campaign donors, transfers to state and municipal governments for pork-barrel projects in their bailiwicks, and illegal cash payments.

The means by which a president builds a legislative coalition has important implications for which firms receive federal contracts. Under a president whose wholesale distribution of cabinet posts largely satisfied coalition members, and who thus had less need for additional retail spending, one would expect the effect of an electoral victory on contracts for campaign donors to be largest among allied parties. Presidents often delegate specific spending decisions to cabinet ministers, and agencies under their purview sign the actual contracts. A party enticed into the coalition with the control of a pork-laden ministry would typically have free reign to favor its own legislators and their campaign donors in the distribution of funds.

Unlike its predecessors, however, the Lula government was reluctant to share the spoils of office with other parties in the governing coalition, suggesting that allied parties might not be able to reap the full rewards of heading up big-budget ministries. During Lula's first term, his cabinets heavily overrepresented the PT and underrepresented important allies like the Party of the Brazilian Democratic Movement (PMDB), which received a much smaller share of ministries than its share of seats in the legislative coalition (Amorim Neto 2007). Moreover, the PT government sought to influence major policy decisions and retain control over second- and third-level bureaucratic appointments within ministries headed by allied parties (Samuels 2008, 164). In other words, the PT's first-term strategy for coalition management relied relatively little on the wholesale distribution of cabinet posts and their accompanying pork. Rather, the party primarily sought to centralize control over these benefits of incumbency and then parcel out payment to individual legislators in order to purchase (or retain) support as needed.

Given the Lula government's reliance on the retail purchasing of legislative support, the question of which parties benefited most from the donations-for-contracts game depends on how it used the particular currency of direct federal spending versus other instruments. During Lula's first term, one important tool for coalition management, funding legislators' individual budget amendments, was used primarily to compensate deputies from major allied parties, as well as the opposition, whose votes were sought on certain legislative initiatives like pension reform (Pereira, Power, and Raile 2011). If direct federal spending was used in a similar fashion during the second presidential term that we examine, one would expect the effect of an electoral victory on contracts for campaign donors to be large and significant among allied or even opposition legislators. However, PT deputies would presumably not need to be compensated because party

loyalty would ensure their votes. Hence, even if the Lula government targeted certain "pivotal" allied or opposition legislators, average treatment effects should still be larger for these groups than for *petistas*. We can refer to this possibility as the "coalition management" thesis.

Other aspects of the PT's governing strategy, however, lead to the opposite prediction: that corporate donors should extract a larger benefit from PT victories than from those of other parties. Direct federal-government spending is a much more lucrative source of funds than legislators' individual amendments; R$ 891 billion fell into the former category in 2005, versus R$ 50 billion in the latter (Pereira, Power, and Raile 2011, 50). In keeping with its stinginess in the distribution of cabinet ministries, the Lula government may have sought to hoard direct federal spending for benefit of the PT, while reserving smaller pools of money—individual amendments, as well as the illegal cash payments uncovered in the 2005 *mensalão* scandal—for coalition management. Privileging *petistas* and their campaign donors in the distribution of funds would be consistent with a governing strategy of strengthening the party and its elected legislators (Samuels 2008)—in this case, by rewarding major donors and ensuring their continued loyalty in future elections. Lula might also have needed to use direct expenditures simply to maintain unity within his own party's ranks, given internal resistance to the party's shift to the center.

Several of the PT's coalitional choices during Lula's second term favor this alternative, "party strengthening" thesis. While the traditionally pork-laden ministries of National Integration and Transportation went to allied parties, the PT retained control of the Ministry of Planning, responsible for generating the executive's initial budget proposal. And in the congressional Joint Budget Committee, whose composition changes annually, the PT consistently reserved for itself the most powerful position—the general rapporteur (*relator-geral*), who has the ability to protect certain areas of the executive's budget proposal from spending cuts and to increase or decrease funding proposed by collective amendments (Santos, Machado, and Rocha 1997; Tollini 2009).

Data

In order to test the relationship between electoral victory and contracts for a candidate's donor firms, we constructed a unique dataset based on publicly available government data. We began with electoral

data from the Superior Electoral Tribunal (TSE): results of the 2006 federal-deputy elections, demographic information on candidates, and a complete list of each candidate's registered campaign donations. For firms donating to these candidates, we collected additional covariates from the Ministry of Finance, including year of founding, industrial classification code, and the state in which their headquarters is located. We dropped candidates who received no corporate donations, since the outcome—contracts for one's donor firms—is undefined for them. Overall, our dataset includes 7,375 firms donating to 1,504 candidates.

To measure federal-government contracts, we queried the ''Transparência'' database of the Office of the Comptroller General (CGU), using the unique tax-identification numbers (CNPJs) of all donor firms in 2006. We gathered data on all direct federal-government expenditures (*gastos diretos*) to these firms from 2004 to 2010. As a baseline measure of contracts, we aggregated all expenditures by beneficiary firm from January 2004 to September 2006, the 33 months immediately prior to the election of October 1, 2006. To measure the outcome, we pooled all expenditures from January 2008 to September 2010, a similar period in which spending was under the influence of the new congress but could not be affected by the results of the *next* election.[5] We dropped data from 2007, since that year's budget was formulated by the previous congress, while spending took place under the newly-elected one. All annual contract totals were converted to 2006 *reais*.

To categorize firms by industry, we used the first two digits of the National Classification of Economic Activities (CNAE) by Brazil's Finance Ministry.[6] These codes define 21 different industries, three of which—construction, water and sewage, and energy— we grouped together as ''public works.'' The vast majority of these public-works firms—1,050 out of 1,129, or 93%—are involved in the construction industry.

Research Design

The Regression Discontinuity Design

To examine the effect of an electoral victory on government contracts for the politician's donor

firms, we utilize a regression discontinuity design, which exploits the fact that candidates cannot fully control how many votes they receive on Election Day. While electoral performance is broadly determined by a number of structural, institutional, and political factors, precise vote totals are subject to a ''nontrivial random component'' (Lee 2008, 684). In many scenarios, whether a candidate barely wins or barely loses an election can be viewed ''as if'' it were a random event, which permits the credible estimation of the causal effects of this electoral victory. While the validity of the RD design must be carefully assessed on a case-by-case basis (Caughey and Sekhon 2011), use of this design has become quite common in the study of elections because its assumptions are relatively clear and their implications are empirically testable.

Most existing studies of the relationship between corporate campaign donations and public policy have focused not on the expected benefits of an electoral victory but rather on the marginal return for each dollar donated. Such an approach is of obvious theoretical interest; it also typically involves familiar ordinary least squares (OLS) regression techniques applied to a full sample of donors and candidates. However, to interpret such regression results as a *causal* effect, one must control for the many variables that are correlated with donations and also affect the policy outcome of interest—something that is quite difficult to do in a credible fashion (Ansolabehere, de Figueiredo, and Snyder 2003). An instrumental variables approach might offer a solution, but valid instruments for donating to a campaign are rare and often contested (Stratmann 2005). Given these inferential challenges, we opt to focus instead on the effect of an electoral victory, which is amenable to causal inference using an RD design. As discussed below, this approach implies estimating effects only for candidates who barely win or barely lose the election rather than for a broader sample. However, we can proceed with greater confidence that we obtain an unbiased estimate of the quantity of interest.

In this article, we adapt the RD design to the open-list proportional-representation rules used in Brazil's legislative elections. Brazilian voters typically vote for candidates, but seats are first distributed to parties or coalitions according to the D'Hondt formula. The candidates in winning parties or coalitions are then ranked by number of personal votes, and the seats won are given to

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[5]We aggregated by the first eight digits of firms' CNPJs, which are common across subsidiaries and parent companies. We did the same with firms' donations.

[6]Firms could list one main and multiple secondary activities, each with a separate code. We consider all codes when classifying them by sector, so these categories are not mutually exclusive.

those with the highest vote share.[7] We are interested in the effect of *personal* electoral victories on government contracts for those candidates' donors. Hence, we focus on the intracoalition stage of seat allocation, which determines whether individual candidates win or lose.

Formally, a coalition j wins s_j seats. Each candidate is indexed by i, which also denotes intra-coalition rank, as determined by his v_{ij} votes.[8] The candidates with $i \leq s_j$ win office and become incumbents, while those with $i > s_j$ lose. The "last winner" is the candidate with $i = s_j$, whose vote total can be written as $v_{i=s,j}$. The "first loser" is the candidate with $i = s_j + 1$, whose vote total is denoted as $v_{i=s+1,j}$. Candidate i's margin of victory or of defeat, M_{ij}, can be defined as:

$$M_{ij} = \begin{cases} v_{ij} - v_{i=s+1,j} & \text{if } i \leq s_j \\ v_{i=s,j} - v_{ij} & \text{if } i > s_j. \end{cases}$$

In words, a winning (losing) candidate's vote margin will be the difference between his vote total and that of the first loser (last winner). Naturally, vote margin determines the electoral outcome: $I_{ij} = 1$ if $M_{ij} > 0$, and $I_{ij} = 0$ if $M_{ij} < 0$.

We wish to estimate the quantity $\tau = \mathbb{E}[Y_{ij}(1) - Y_{ij}(0)]$, where $Y_{ij}(1)$ and $Y_{ij}(0)$ denote the outcome of interest for candidate i in coalition j when the candidate is a winner and loser, respectively. This estimand is unidentified without further assumptions since we only observe $Y_{ij}(1)|I_{ij} = 1$ and $Y_{ij}(0)|I_{ij} = 0$, but not $Y_{ij}(1)|I_{ij} = 0$ and $Y_{ij}(1)|I_{ij} = 1$.

The RD design allows one to proceed by making the assumption that the distribution of potential outcomes is a smooth function of the vote margin. Under this smoothness assumption about $Y_{ij}(1)$ and $Y_{ij}(0)$, one can identify a local causal effect at $M_{ij} = 0$ since, on either side of the threshold (with a minimum amount of extrapolation), the outcomes of winners are valid counterfactuals for the outcomes of losers (Imbens and Lemieux 2008). Thus, in this article, we focus on the following quantity: $\tau_{RD} = \mathbb{E}[Y_{ij}(1)|M_{ij} = 0] - \mathbb{E}[Y_{ij}(0)|M_{ij} = 0]$. This estimand is a "local" average treatment effect, since

it represents the effect among candidates who barely win or lose.

An implication of the smoothness assumption is that the empirical density of the vote margin should be continuous around $M_{ij} = 0$. If some candidates could exert fine control over their vote totals (e.g., because the governing party manipulates the vote count), one might observe abnormal numbers of observations immediately above or below the threshold. Reassuringly, a histogram of the forcing variable shows no such "lumpiness." Moreover, implementing the McCrary (2008) test for smoothness of the forcing variable for each of our subsamples, we fail to reject the null hypothesis of no difference in density at the cut point. The figure and results are in the online appendix.

In this analysis, we use raw vote margin as our forcing variable rather than vote margin as a share of total votes. The latter is appropriate for measuring close elections in a single-member-district electoral system, but it is much more problematic in a system such as Brazil's, which has substantial variation in district magnitude. Ten small states and the federal district elect eight deputies each, whereas the largest state, São Paulo, has 70 seats. In 2006, the number of candidates, which is partially a function of district magnitude, ranged from 48 to 954. A candidate in São Paulo could expect to receive 0.1% of the vote, on average, whereas one in Sergipe could expect 2.1%. As a result, close elections defined in terms of vote shares occur disproportionately in large states, as shown in the appendix. Overrepresenting large states is problematic because such states are systematically *under*represented in Brazil's malapportioned Chamber of Deputies, and in particular, in the Joint Budget Committee, where legislators from small states dominate (Samuels 2003, 136–37). Small states tend to be rural and poor, meaning that their economies are more reliant on federal government spending, and they have a greater need for infrastructural investment. A sample that underrepresented small states would miss much of the donations-for-contracts action.[9]

Specification

Our analysis is conducted at the level of the candidate rather than the individual firm. Specifically, we sum

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[7]Candidates who leave the legislature, often because they are appointed to bureaucratic positions, are replaced by highly ranked losers (*suplentes*). In our sample, 15 losers served for more than half the legislative session, and nine winners served less than half. In the appendix, we present alternative results using instrumental variables to adjust for this "noncompliance" issue. Our coefficient estimates increase somewhat, and all substantive conclusions are unchanged.

[8]Ties are broken by giving the older candidate the higher rank. Since this rule introduces imbalance in age among winners and losers, we drop candidates with a zero vote margin.

[9]As a robustness check, we calculated effect estimates for several alternative forcing variables, including rank in the coalition list, vote margin as a share of all personal votes, and an "inflated" vote margin that seeks to account for candidates in small states whose margin of victory or loss is large relative to their total votes. In addition, since using raw vote margin tends to underrepresent large states, we estimated a specification that weighted observations by the log of state population. In each case, results (in the appendix) are similar to those reported below.

up total contracts for all of a candidate's donor firms, take the average amount per firm, and use the log of this quantity (plus one) as our dependent variable. We do the same with all firm-level covariates used in balance checking. While there is a certain theoretical attractiveness to a firm-level analysis, the treatment is applied to candidates, so calculating accurate standard errors would require clustering on the candidate. Analyzing contracts by donor-candidate dyad thus introduces substantially more variation in the outcome, without any increase in statistical power, meaning that any effects are less likely to be detected.

To ensure that our results are robust to different estimation procedures, we estimate treatment effects using three different approaches: a locally weighted regression, or loess; a local linear regression; and a third-order polynomial regression.[10] Each estimator is fit separately on either side of the discontinuity, with the difference in predicted values at the zero vote margin providing an estimate of τ_{RD}. We use the following general specification:

$$\min \sum_{j=1}^{J} \sum_{i=1}^{N_j} K_{\lambda\left(M_{ij}\right)} \cdot 1\left\{-h \geq M_{ij} \leq h\right\} \cdot$$
$$\left(Y_{ij} - \sum_{z=0}^{p} \beta_z \cdot M_{ij}^z - \sum_{z=0}^{p} \gamma_z \cdot M_{ij}^z \cdot I_{ij}\right)^2.$$

Our estimate of τ_{RD} is given by γ_0. For the local linear and loess specifications, $p = 1$;[11] for the polynomial specification, $p = 3$. $K_{\lambda\left(M_{ij}\right)}$ is a tricubic weighting function in the loess specification and a constant in the local linear and polynomial specifications. Because of the weighting function, the loess estimator weights observations closest to the discontinuity more heavily. The bandwidth is given by h, which specifies how much data in a window around 0 is retained for estimating τ_{RD}. Standard errors for the loess estimator are generated using the bootstrap, while the polynomial and local linear standard errors account for heteroskedasticity ("robust standard errors").

Following the hypotheses outlined above, we estimate treatment effects for four different samples of candidates: those receiving any corporate donations, those

receiving donations from public-works firms, PT candidates receiving donations from public-works firms, and non-PT coalition members receiving donations from public-works firms.[12]

Bandwidth Selection and Covariate Balance

An important implication of our identifying assumptions is that bare winners and losers, as well as the firms that donate to them, will be similar on background characteristics. In other words, one should find balance on a range of covariates when comparing the separate treatment and control estimates at the discontinuity. We selected values for h, the discontinuity bandwidth, such that the resulting sample sizes were large enough to yield sufficient statistical power to identify treatment effects, yet also small enough to retain good balance.[13] For the local linear specification, we use observations where the absolute value of raw vote margin is less than 25,000, the 31st percentile for this covariate in the full sample. For the loess and polynomial estimators, we use a window of 40,000 votes (46th percentile) and 100,000 votes (89th percentile), respectively.[14]

To check covariate balance, we examined attributes of both candidates and firms, using the specifications described above to estimate the difference between winners and losers at the discontinuity point. Figure 1 graphically displays balance on 33 covariates for the three estimators and four different samples. Balance is similar to what one would expect if the treatment had been randomly assigned; in each sample, no more than one estimate generates a t-statistic greater than 2.[15]

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[10]Covariate balance was better with a third- than a fourth-order polynomial.

[11]Our loess algorithm uses a linear specification rather than the more common polynomial specification; the former is better suited for boundaries because it is less sensitive to outliers (Hastie, Tibshirani, and Friedman 2009, 196). Furthermore, a cross-validation procedure indicated that the linear fit had a lower prediction error than alternative polynomial specifications. We also adopt a span parameter of 1, based on this cross-validation procedure.

[12]We define non-PT coalition members as the allied parties in Lula's cabinet at the start of his second term: the PMDB, the Progressive Party (PP), the Green Party (PV), the Brazilian Socialist Party (PSB), the Communist Party of Brazil (PC do B), the Democratic Labor Party (PDT), and the PR, which ran as the Liberal Party (PL) and the National Order Reconstruction Party (PRONA) in 2006.

[13]For our data, this approach to bandwidth selection is more conservative than using the algorithms proposed by Imbens and Kalyanaraman (2009) or Imbens and Lemieux (2008), which yield significantly larger bandwidths for the local linear specification. While our results are stronger (more significant) with these larger bandwidths, covariate balance is considerably worse, which is sometimes a pitfall of using these algorithms (Caughey and Sekhon 2011, 405).

[14]The additional flexibility of the loess and polynomial estimators reduces bias, but it can also increase the variance of our estimates. As is standard practice, we use larger bandwidths—and hence more data—for these specifications while still maintaining covariate balance.

[15]Covariate balance using an alternative metric—the difference-in-means divided by the pooled standard deviation, or "standardized bias"—is presented in the appendix.

FIGURE 1 **Balance Statistics for Federal Deputies**
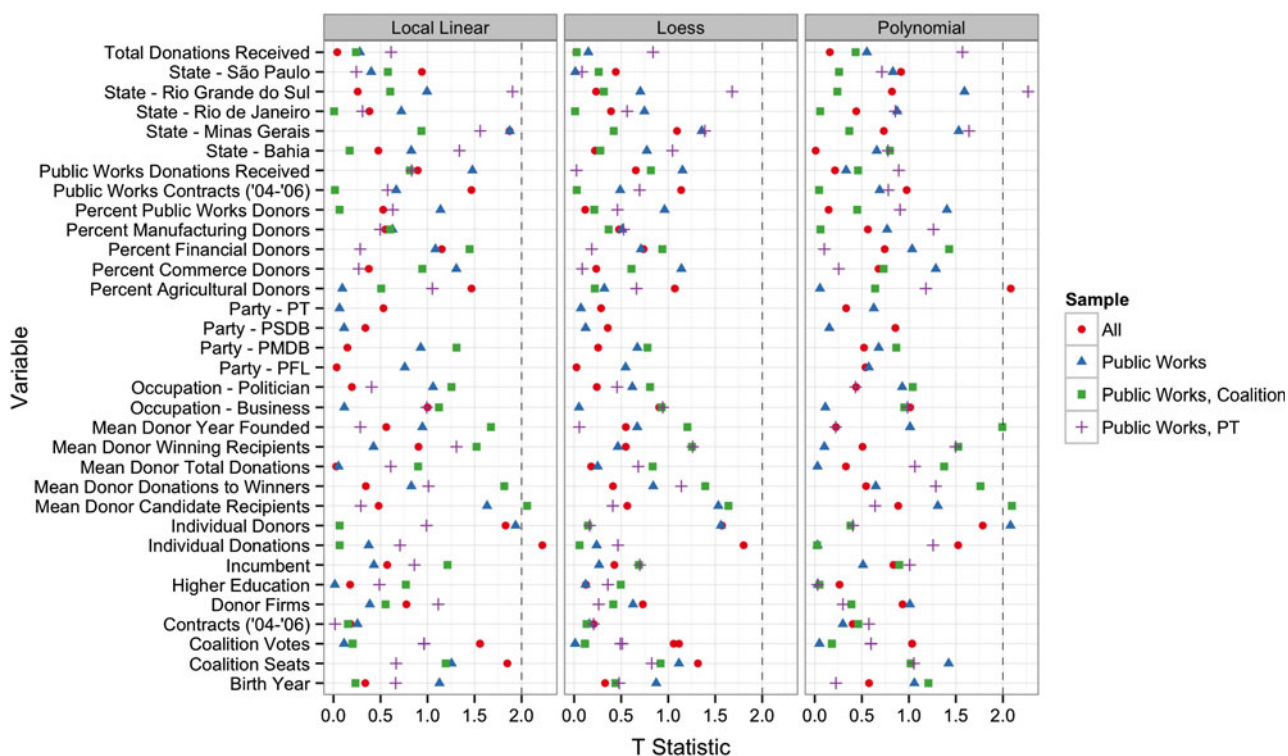
Most importantly, all specifications and samples are well balanced with respect to government contracts previously received by a candidate's donor firms. We also obtain good balance on mean total donations, mean donations to winners, mean number of candidate recipients, and mean number of winning recipients among a candidate's donor firms. This implies that firms giving to candidates who won did not invest more funds, diversify to a greater extent, or pick winners with any greater success than firms giving to candidates who lost.

Because the specifications used to estimate treatment effects require extrapolation to the threshold, they rely on the assumption that balance on potential confounders is improving as one approaches this point. The outcome of close elections is not always "as if" random; balance on key covariates may actually worsen in the immediate vicinity of the zero vote margin. Specifications that extrapolate to the threshold may not detect this sort of imbalance, but simple mean differences in narrow windows typically do. Following Caughey and Sekhon (2011, 395–97), we further examined balance on seven key covariates by calculating the difference of means

between winners and losers in moving windows of 4,000 votes. Balance consistently improves when approaching the threshold, arguing in favor of the "as if" random assumption. Results are in the appendix.

The RD design also implies that unmeasurable characteristics of candidates and donor firms should be balanced between treatment and control groups. In particular, bare winners and bare losers should have received approximately the same amount of money in off-the-books donations. Focusing on officially registered contributions means we are examining only part of the campaign financing picture, but the portion we ignore should not serve as a confounder.

Results

The results of our analysis, consistent across the three specifications, show that an electoral victory brings substantial increases in government contracts for a candidate's campaign donors, but only among public-works firms giving money to candidates from the

TABLE 1   The Effect of Electoral Victory on Future Federal Government Contracts

	All Contracts		DNIT Contracts		Mean Donation	N
	τ_{RD}	τ_{RD}, unlogged	τ_{RD}	τ_{RD}, unlogged		
Loess Estimator						
All	1.74	R$ 41,929	0.94	R$ 12	R$ 10,412	693
	(1.4)		(1.58)			
Public-works donations	3.67	R$ 53,744	1.6	R$ 623	R$ 10,541	354
	(2.55)		(2.6)			
Public works, coalition	1.14	R$ 5,774	-1.46	R$ -1,667	R$ 9,569	155
	(4.45)		(4.42)			
Public works, PT	9.01	R$ 147,249	7.69	R$ 599	R$ 10,497	68
	(4.59)		(5.49)			
Polynomial Estimator						
All	1.1	R$ 25,548	0.64	R$ 8	R$ 58,752	1343
	(1.16)		(1.26)			
Public-works donations	4.15	R$ 98,563	2.49	R$ 1,516	R$ 17,997	576
	(2.1)		(2.15)			
Public works, coalition	0.77	R$ 5,994	-1.07	R$ -1,402	R$ 26,205	241
	(3.35)		(3.48)			
Public works, PT	11.47	R$ 346,267	8.94	R$ 755	R$ 8,774	112
	(4.08)		(4.5)			
Local Linear Estimator						
All	1.92	R$ 54,005	0.98	R$ 15	R$ 62,556	463
	(1.11)		(1.22)			
Public-works donations	4.48	R$ 105,376	1.97	R$ 1,107	R$ 19,158	236
	(2.01)		(2.06)			
Public works, coalition	1.55	R$ 22,612	-1.63	R$ -5,997	R$ 29,013	111
	(3.18)		(3.17)			
Public works, PT	8.67	R$ 138,601	6.98	R$ 466	R$ 8,936	45
	(3.7)		(3.76)			

Note: Dependent variable is the log of the average value of contracts (plus one) received by a candidate's corporate donors, January 2008–September 2010. The left panel shows results using all contracts and the right panel shows results using only contracts issued by the National Department of Transportation Infrastructure (DNIT). "PT" candidates are from the governing Workers' Party. "Coalition" candidates are those from parties listed in footnote 12. The unlogged treatment effect is given by $exp(E[Y_{ij}(1)|M_{ij} = 0]) - exp(E[Y_{ij}(0)|M_{ij} = 0])$. "Mean Donation" is the estimated donation per firm for candidates at the discontinuity. The loess specification uses a bandwidth of 40,000 votes and a span of 1. The local linear and polynomial specifications use bandwidths of 25,000 and 100,000 votes, respectively. Standard errors are in parentheses. Loess standard errors are bootstrapped; others are heteroskedasticity robust.

ruling PT. The left side of Table 1 summarizes these results. The first column presents τ_{RD}, the local average treatment effect, and its standard error. The second column gives the estimated effect in *reais* by unlogging the treatment and control estimates of the outcome and taking their difference. For context, the fifth column estimates the mean contribution per donor firm for candidates at the discontinuity.[16]

Estimated treatment effects are all positive, but for most of the samples, they fail to obtain statistical significance. Several of the unlogged treatment-effect estimates are similar to or smaller than the estimated average donation, suggesting that even if they obtain some benefits, these firms are not profiting much from their contributions. The smallest and least significant estimates we obtain are for public-works donors to candidates whose parties ended up joining the PT in the governing coalition. These results argue strongly against the coalition-management thesis: we find no evidence that the Lula government allocated public-works spending in a way that might entice legislators

[16]These figures were calculated by estimating the log of the average donation on each side of the discontinuity and then taking the mean of the separate unlogged estimates.

from other allied parties to continue voting with the PT.[17]

We find strikingly different results when examining public-works donors to PT candidates: treatment effect estimates are large, positive, and consistently significant.[18] Depending on the specification, a PT electoral victory means that each public-works donor can expect between 138,601 and 346,267 *reais* (US $73,921 to 184,676) in additional government contracts over 33 months during the ensuing legislative term. This figure amounts to between 14 and 39 times the estimated average public-works donations to that candidate. Over the full four-year term, the ratio would likely be even higher, though we obtain a cleaner estimate of the treatment effect by focusing on the period in which only these elected deputies, and not those from a previous or subsequent legislature, were able to influence federal-government spending.

Placebo and Robustness Tests

To rule out several threats to the internal validity of our research design, we conduct a series of placebo and robustness tests. A first concern is that our estimated treatment effect is due to some unmeasured attribute of candidates, such as their political skills, that covaries with vote margin and remains unbalanced between treatment and control groups. To test for this possibility, we estimate the treatment effect on public-works contracts for PT donors at several nonzero thresholds: 15,000, 7500, −7500, and −15,000 votes. The assumptions of our research design would predict a zero effect at each of these thresholds; winning (or losing) by a slightly greater or lesser margin should not matter for one's ability to steer contracts to campaign donors. On the other hand, if unmeasured political skills are driving the relationship, one would expect estimates at

these thresholds to be similar to those at the zero vote margin. As shown in the appendix, all placebo estimates are much smaller than the nonplacebo estimate, several are negative, and none is statistically significant.

A second possible concern is that campaign contributions do not actually matter for the amount of government contracts—that public-works donors to victorious PT candidates would have received the same payout even if they had given no money in 2006. Strictly speaking, our design cannot rule out this possibility; we identify the effect of an electoral victory rather than the effect of a donation. However, one way to test for it is to examine the effect of electoral victory on government contracts for firms that gave only to other candidates. Our theory would not predict any benefits for firms failing to donate to a bare winner versus those failing to donate to a bare loser. As shown in the appendix, none of the estimates is statistically significant.

A third possible concern is that our results are partially or wholly driven by residual imbalance in key covariates. Although estimated differences on pre-treatment covariates between bare winners and losers are almost always insignificant, even slight variation in potential confounders can sometimes affect results. As a robustness test, therefore, we estimated treatment effects under the polynomial and local linear specifications while controlling for incumbency, number of donor firms (total and public-works), and the log of prior contracts, prior public-works contracts, total donations, and public-works donations. Results, reported in the appendix, are as significant or more significant than the unadjusted results, while still remaining indistinguishable from zero for public-works donations to the PT's coalition partners.

Contextualizing the Effects

The estimated effect of a PT electoral victory would have a noticeable impact on the bottom line of most public-works donor firms. While large construction conglomerates get most of the media attention, the average public-works donor is much smaller. Out of 1,129 public-works donors in our dataset, only 68 ranked among Brazil's highest grossing construction firms, according to the industry's trade publication (O Empreteiro 2007). Thus, 94% of our firms presumably had revenues of less than R$ 6.5 million (the cutoff to make this list) in 2006. For these firms, our treatment-effect estimate for PT candidates would represent, at minimum, 1–2% of revenues over the corresponding 33-month period. For the median firm, the benefit could easily be around 10%.

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[17]When examining contracts for all firms, insignificant effects are found among candidates from each of the major parties, including the PT. For public-works firms, we also obtain insignificant estimates when separately examining results for the largest allied party, the PMDB, and the two largest opposition parties, the Party of Brazilian Social Democracy (PSDB) and the Democrats/Liberal Front Party (DEM/PFL).

[18]We formally tested the null hypothesis that the treatment effect among public-works donors to the PT's coalition partners is the same as among donors to the PT itself. For the local linear and polynomial specifications, we included treatment interactions for PT and coalition candidates, testing equality of coefficients with an *F*-test. For the loess specification, we used the bootstrap distribution to calculate the standard error of the difference in coefficients. Effects differ significantly at the 10% level under the loess specification and the 5% level under the polynomial specifications (*p*-values of 0.06 and 0.04, respectively). The null is not rejected in the local linear specification (*p*-value of 0.16), but given the smaller sample size, this test is relatively low powered.

Our treatment-effect estimates give the expected increase in contracts when a single federal-deputy candidate wins election; as such, they represent only one piece of the overall relationship between politicians and corporate donors. Firms may donate to more than one federal-deputy candidate, as well as to candidates for other offices and to political parties as a whole. Payback may include not only direct federal-government spending, but also contracts awarded by state and municipal governments, preferential access to credit from state-owned banks, votes on key legislation, and other perks. Total return on investment in a particular election would have to account for all of these costs and benefits, some of which cannot easily be quantified.

For most public-works donors to winning federal-deputy candidates, however, total return on investment should be higher than the ratio of our treatment effect to the average donation. Diversifying one's portfolio is not particularly common; of all public-works firms that gave money to federal-deputy candidates in 2006, 81% gave to only a single candidate for this office, and 54% gave to no other candidates at any level. As shown in the appendix, the effective number of federal-deputy candidate recipients, which accounts for the relative size of donations, is even more concentrated at the low end of the scale. Moreover, fewer than 10% of these firms gave separately to a political party. Most firms bet on a single federal-deputy candidate, so while their overall payoff from all government sources may be greater, they do not invest more than is accounted for here.

If the PT prioritizes direct federal-government spending for party-strengthening purposes, does this strategy have the desired effect? To address this question, we examined candidates who received corporate donations in 2006 and then ran again in 2010 with the same party and in the same state. For all 223 candidates who fall into this category, the change in contracts for their corporate donors from before to after the 2006 election is positively correlated with the change in corporate donations from 2006 to 2010 ($r = 0.16$, $p = 0.018$). For the 50 PT candidates, the relationship is even stronger ($r = 0.23$), though less significant ($p = 0.111$) given the low power. Thus, firms appear to credit candidates for the contracts they receive and to contribute more generously the next time around. Moreover, the PT seems to benefit the most from this quid pro quo relationship.

Evidence of the Causal Mechanism

We hypothesized that the effect of an electoral victory on contracts for campaign donors operates through a variety of causal mechanisms in the budgeting and contracting processes. Budget-related mechanisms are, unfortunately, quite difficult to observe through written documentation (Mognatti 2008). Even if our spending data could be readily linked to appropriations data, drilling down to identify the influence of a particular legislator would be a near impossible task.

By contrast, mechanisms related to the awarding of contracts by particular ministries are more readily testable. Under a president whose strategy for coalition management relied on the "wholesale" distribution of cabinet posts, one would expect the effect of an electoral victory on government contracts for a particular party's donors to operate primarily through ministries controlled by that party. However, as noted above, the Lula government relied much more on "retail" payments to individual legislators and did not share the spoils of office with allied parties as generously as other presidents had done in the past. In particular, it sought to influence spending decisions and second- and third-level bureaucratic appointments even in ministries that were headed up by other coalition members. We also know, from the results presented above, that direct federal spending under the PT favored donors to the party's own legislative candidates. Hence, we would expect to see effects of an electoral victory on contracts for PT donor firms when examining spending by any of the top public-works ministries, even when the ministerial appointment went to another party.

To test this hypothesis, we examine spending by the National Department of Transportation Infrastructure (DNIT) within the Ministry of Transportation, the top ministry involved in public-works spending. Throughout Lula's second term, the Minister of Transportation and the Director General of DNIT were both politicians from the PR, a small but crucial partner in the governing coalition. Yet the PT named one of its own members, Hideraldo Luiz Caron, as DNIT's Director of Highway Infrastructure. Since the bulk of Transportation Ministry funds flow through DNIT, and the vast majority of its contracts are for highway construction, Caron oversaw substantial spending.

Evidence suggests that Caron's spending decisions were swayed by the lobbying efforts of fellow party members, such that PT influence within DNIT could serve as a plausible mechanism connecting legislative electoral victories to contracts for campaign donors. During the 2011 scandal, the press uncovered a close relationship between Caron and Paulo Pimenta, a PT deputy from Caron's home state and one of the bare winners in our dataset (Costa 2011). Two of Pimenta's donors were public-works firms that had received government contracts in 2004–2006; one firm earned

six times as much during 2008–10, and the other experienced a fourfold gain. Nearly all of these firms' post-election contracts were paid by DNIT—99% and 96%, respectively—so Caron would have been directly responsible for the increases.

Our estimates of the effect of an electoral victory on contracts awarded by DNIT support the hypothesis of PT influence throughout the federal bureaucracy. As shown in the right side of Table 1, we find a similar pattern of coefficients as when examining all contracts, including a dramatic contrast between estimates for public-works firms donating to candidates from the PT (large and positive in each specification) versus other allied parties (slightly negative and insignificant).[19] Our null result for non-PT coalition members does not mean that PR deputies extracted no benefits from the party's top-level control of Transportation and DNIT. There are too few bare winners and losers from this small party for us to estimate the effects of its candidates winning office, though the 2011 scandal does suggest that such effects exist. However, we do show that the PT did not write a blank check when granting leadership of Brazil's most pork-laden ministry to an allied party.

Indeed, the PT's use of lower-level bureaucratic appointments to control spending was an excuse specifically offered by DNIT's PR-affiliated director when he came under fire during the 2011 scandal (Seabra 2011). Though one might be tempted to discount such a charge as simple blame shifting by an official under investigation, our analysis supports the claim.

Conclusion

Our analysis has shown that, for public-works firms that contribute to ruling-party legislative candidates in Brazil, campaign donations can indeed buy policy outcomes. Giving to a winner is hardly a prerequisite for gaining contracts—government could not function if it only bought goods and services from campaign donors—but helping elect a ruling-party candidate does boost the amount that a firm can expect to be paid. In this sense, our study confirms long-standing conventional wisdom in Brazil, as well as many other democracies, while simultaneously challenging skep-

tical academic views about the influence of corporate money on public policy. Previous inconclusive or null findings on this question may be due to features of the U.S. political landscape that limit the buying power of corporate donations, as well as to research designs that suffer from endogeneity bias due to shared ideological proclivities between donor and recipient. In Brazil, where candidates rely heavily on corporate money and where campaign expenditures should be more likely to win votes, donors may be able to extract greater policy concessions. Examining an area of public policy that is less subject to ideological leanings, and using a regression discontinuity design to deal with any lingering endogeneity concerns, we are able to demonstrate these large causal effects.

The relationship between corporate donations and policy outcomes in Brazil seems likely to be replicated in much of the developing world. The political economy features that incentivize firms to donate to politicians, such as weak interfirm coordination to pursue collective interests, are shared among other Latin American countries as well as Turkey, South Africa, and much of Southeast Asia (Schneider 2013). Economies in East Central Europe also have similar characteristics (Nölke and Vliegenthart 2009). The multinational corporations that dominate these economies might be inclined toward ''exit'' rather than ''voice'' (McMenamin 2013), but domestic firms that compete with them should be likely to seek policy favors from legislators. In all of these regions, democracies are newer than in Europe and the United States, meaning that partisan identities should be more fluid and campaigns will tend to matter more for electoral outcomes (Greene 2011). Thus, candidates should be motivated to fund them. Outside of advanced democracies, limits on campaign financing tend to be weak, weakly enforced, or nonexistent, allowing corporate money to flow freely into politics (Austin and Tjernström 2003). Finally, the rule of law is generally weaker as well, facilitating practices like bid rigging for government contracts.

While confirming the use of public-works contracts to reward campaign donors in Brazil, our study shows that the PT government did not allocate this valuable resource for coalition management as much of the literature would suggest. Instead of parceling out the spoils of office via its allocation of cabinet positions, the PT awarded few ministries to allied parties and sought to control spending decisions and lower-level bureaucratic appointments even in those ministries, such as Transportation, headed up by a coalition member. It then used direct federal spending to benefit donors to its own deputies, rather than to purchase support ''retail'' from other parties' legislators.

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[19]Unlogged treatment effects are smaller across the board because there are numerous zeros in the outcome for both treatment and control observations; many public-works firms do not specialize in highway construction. Effects for PT candidates are also somewhat less significant than in the main analysis (at the 0.05 level for the polynomial estimator, the 0.1 level for local linear, and insignificant for loess), possibly for the same reason.

Our findings do not invalidate arguments that the PT, like prior governing parties, used a largely separate form of spending—individual amendments that typically transfer funds to state and municipal governments for small-scale infrastructure projects—for the purposes of coalition management (Pereira, Power, and Raile 2011). However, we do show that other, more substantial forms of spending may be used for different political purposes than individual amendments.

Our findings for the PT government raise questions about how other ruling parties—in Brazil and elsewhere—might respond to campaign donations. The limited availability of data on government contracts precludes extending our study to earlier periods when other parties governed at the federal level. However, a recent RD analysis of campaign donations and spending by eight state governments shows similar patterns to those that we identify (Arvate, Barbosa, and Fuzitani 2013). While these authors do not test the hypothesis that governors privilege copartisans over other coalition members when awarding contracts to campaign donors, they do show larger effects for the coalition as a whole than for opposition legislators. These results suggest that our findings may have broader applicability, within as well as outside of Brazil. The states examined by Arvate, Barbosa, and Fuzitani (2013) are quite heterogeneous in terms of both economic development and the type of party that governs them. Hence, the link between donations and contracts that we have uncovered is likely to extend beyond the sample examined in this article.

Finally, the campaign donations of Brazil's largest public-works firms are potentially relevant for government spending decisions in the many other Latin American countries where they routinely give money and win contracts. In the early months of Peru's 2011 presidential campaign, the top three donors to then-frontrunner Alejandro Toledo were Brazilian construction firms that had won numerous state contracts in the past, including during Toledo's prior presidential term (Lachini 2011). Likewise, in Panama's 2009 election, Brazil's top-grossing construction firm Odebrecht was a major donor to the presidential campaign of Ricardo Martinelli (Panamá América 2011). After Martinelli's victory, Odebrecht was awarded its largest Panamanian contracts to date, including for the new Panama City Metro (Jiménez 2012). The lack of systematic campaign finance data in these and other countries in the region rules out extending our statistical analysis beyond Brazil. However, firms that have learned to play the donations-for-contracts game in a fairly transparent system should not be deterred from doing so where there is much less scrutiny.

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