Declared Support and Clientelism*

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*The authors thank Andy Baker, Jordan Gans-Morse and Pietro Ortoleva for comments, as well as Inbok Rhee, Mariana Carvalho-Barbosa, Anselm Rink, and Henrique Barbosa for outstanding research assistance. In addition, we appreciate feedback from participants at Stanford University's Conference on Contemporary Challenges to Inclusion and Representation in Latin America and at the 2015 Annual Meeting of the American Political Science Association. Simeon Nichter acknowledges support from the Hellman Foundation, the National Science Foundation, and the Harvard Academy for International and Area Studies. Salvatore Nunnari acknowledges financial support from the European Research Council (POPULIZATION Grant No. 852526).

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June 29, 2021

Abstract

Recent studies of clientelism predominantly focus on how elites use rewards to influence vote choices and turnout. This article shifts attention towards citizens and their choices beyond the ballot box. Under what conditions does clientelism influence citizens' decisions to express political preferences publicly? When voters can obtain post-election benefits by declaring support for victorious candidates, their choices to display political paraphernalia on their homes or bodies may reflect more than just political preferences. We argue that various factors — such as the size of rewards and punishments, the competitiveness of the election, and whether multiple candidates employ clientelism — affect citizens' propensity to declare support in response to clientelist inducements. Building on insights from fieldwork, formal analyses reveal how and why such factors can distort patterns of political expression observed during electoral campaigns. We conduct an experiment in Brazil, which predominantly corroborates predictions about declared support and clientelism.

1 Introduction

In many parts of the world, citizens receive material benefits in contingent exchange for providing political support. A cross-national survey of 1,400 experts found that such patterns of "clientelism" exist to some degree in over 90 percent of countries, with clientelist efforts reaching "moderate" or "major" levels in nearly three-fourths of nations (Kitschelt, 2013). This phenomenon is widely recognized to have a broad range of consequences for democracy and development. Clientelism often exacerbates political inequalities by allowing those with resources to buy votes from impoverished citizens, and undermines representation when vote choices no longer reflect recipients' political preferences (Stokes et al., 2013). Moreover, clientelism is frequently linked to numerous maladies that can stifle development, such as the under-provision of public goods, increased rent seeking and expanded public deficits (Hicken, 2011).

The literature on clientelism tends to focus on the choices of elites more than those of citizens. Traditional studies explored patron-client relationships involving highly asymmetric power. Voters in these relationships often had limited autonomy to make choices of their own volition, due to various factors such as restrictive land-tenure arrangements and the lack of ballot secrecy (Scott, 1972; Baland and Robinson, 2008). While some contemporary studies recognize clients' heightened autonomy (e.g., Hilgers, 2012; Auerbach and Thachil, 2018), the formal and quantitative literature continues to pay far greater attention to the choices of political elites. Particularly emblematic of this tendency, many analysts concentrate on the supply-side logic by which politicians and their representatives target citizens when distributing campaign handouts. For instance, Stokes (2005) contends that elites reward weakly opposed voters for vote-switching, whereas Nichter (2008) argues they reward nonvoting supporters for showing up at the polls. This elite-targeting focus is extended in various studies motivated by Stokes et al. (2013), who argue that party leaders' efforts to target weakly opposed voters are hindered by brokers who channel rewards to supporters.

Other influential examples that predominantly focus on elites include studies on the targeting of reciprocal voters (Finan and Schechter, 2012), on the optimal combination of multiple clientelist strategies (Gans-Morse et al., 2014), and on the broader targeting of distributive benefits (Dixit and Londregan, 1996).

While such research has greatly enhanced our understanding of clientelism, it sheds relatively less light on the role of citizen choices. Voter choice deserves greater attention, especially because some qualitative work underscores citizens' substantial autonomy in various contexts. Indeed, scholars have long argued that politicians' control of contingent exchanges varies and may well be in decline (Scott, 1972). A principal contribution of the present study is its central focus on citizen choices in contexts with clientelism, testing formal predictions with an online experiment in Brazil. In particular, we investigate the following question: Under what conditions does clientelism influence citizens' decisions to express political preferences publicly? Our analysis reveals how and why citizens will often make distinct choices in different contexts, and thus underscores that their willingness to participate in clientelism should not be taken for granted.

In addition to this emphasis on citizen choices, another key contribution is investigating how clientelism can induce political expression *beyond* the ballot box. Unlike some qualitative studies, formal and quantitative work on clientelism tends to focus more narrowly on voting. By contrast, we examine why citizens publicly express support for political candidates, through actions such as displaying campaign paraphernalia on their homes, on their bodies, and at rallies. Many studies consider such activities to be important forms of democratic participation, which enable citizens to express their political preferences and potentially influence the selection of leaders (Verba and Nie, 1972; Huckfeldt and Sprague, 1995). We argue that in much of the world, clientelism presents another understudied motivation. When voters can obtain post-election benefits by declaring support for victorious candidates, their decisions to participate publicly often reflect more than just political preferences. As explored below, various factors affect citizens' propensity to declare support in

response to clientelist inducements. For instance, citizens may deem it especially advantageous to declare support for a clientelist candidate who distributes large rewards, is likely to win the election, and can easily observe declarations. But citizens may also balk at declaring for that candidate if doing so is costly: it might be challenging to obtain campaign materials or travel to rallies, citizens might prefer another candidate ideologically, or they might live in neighborhoods where declaring for that candidate involves social costs. And in some contexts, citizens might even face punishments if they declare support for a candidate who loses the election. By exploring such dynamics of clientelism, the present study not only shifts the lens of analysis to citizen choices, but also broadens its scope to actions beyond voting.

Recent research points to yet another important reason to investigate declared support: it may play a crucial role in the survival of "relational clientelism" — i.e., ongoing exchange relationships that extend beyond election campaigns. Nichter (2018) argues that when the state fails to provide an adequate social safety net, citizens are often motivated to sustain ongoing exchange relationships with politicians who mitigate their vulnerability. By declaring support, citizens alleviate a credibility problem that threatens the viability of relational clientelism: citizens mitigate politicians' concerns about whether to trust their vote promises in contexts with ballot secrecy. Consistent with this link between declared support and vulnerability, Bobonis et al. (2019) find that declarations increase amidst negative rainfall shocks. Despite their contributions, such studies ignore how other contextual factors might affect citizens' declarations; they instead undertake measures to control for contextual variation. By contrast, we advance the literature by investigating how various factors shape citizens' decisions to declare support for local politicians. Given the role of declared support in relational clientelism, our study focuses on post-electoral benefits — which are the hallmark of relational clientelism, and by definition cannot involve electoral clientelism.[1]

Evidence suggests that declared support warrants careful investigation. During our 130 interviews in the Brazilian states of Bahia and Pernambuco, many citizens and politicians

[1]As discussed, our analyses are robust to – and can be used to examine – campaign handouts.

explained that declaring support for a victorious candidate improves access to post-election benefits.[2] In line with such perceptions, quantitative analyses of two surveys suggest that Brazilians declaring for an elected mayor or councilor are more likely to receive benefits (Nichter, 2018). Beyond Brazil, observational analyses in Mexico suggest declarers are significantly more likely to receive clientelist benefits than non-declarers (Nichter and Palmer-Rubin, 2015). In Argentina, qualitative research suggests citizens who demonstrate their support at rallies are more likely to receive handouts (Auyero, 2000). In Ghana, fieldwork reveals citizen perceptions that publicly expressing support will help them obtain future benefits from elected politicians (Michelitch, 2013). And in Lebanon, citizens are likelier to receive benefits if they demonstrate their partisan commitment through various actions such as displaying posters and voting (Cammett, 2014). These studies provide global evidence of declared support, but do not elaborate and test the logic underlying this phenomenon.

To clarify this logic, the present study is the first to develop and test a theoretical model of declared support. This model provides numerous predictions about voters' declaration choices in contexts with clientelism, which we test experimentally. For instance, formalizing the intuition discussed above, it suggests that citizens are most likely to declare support when a clientelist politician: (a) offers larger rewards, (b) is heavily favored to win the election, (c) can be publicly supported without incurring additional material or social costs, (d) can monitor declarations effectively, and (e) is the only candidate offering rewards. Just as important, the model also shows why various factors can depress declarations for non-clientelist candidates, and reveals their aggregate effect on the overall level of declarations. Furthermore, theoretical analyses suggest that rewards induce more declarations when offered by leading rather than trailing candidates, and that rewards are more effective than punishments. Overall, formal analyses yield a rich set of hypotheses about citizen choices and declared support in contexts with clientelism.

[2]The Supplementary Information describes this fieldwork.

Another important contribution of our study is that it employs experimental methods to examine citizen choices in clientelism, more specifically by using an online experiment to test our model's predictions. A key advantage of this approach is that it isolates causal effects by changing exogenously only one aspect of the decision environment at a time; by contrast, testing predictions observationally would require disentangling various reasons why citizens declare and dealing with endogenous changes in the political and social environment. Another advantage is that we sidestep the issue of reverse causality that often bedevils research on clientelism: are citizens rewarded for declaring support, or do citizens declare support because they received rewards? Our experiment controls the order of moves; citizens make declaration choices before an election, and then rewards are distributed by victorious candidates. Thus, the present study is able to focus squarely on how contextual factors affect citizens' decisions to declare support in exchange for post-election benefits.

Our experiment involved 1,259 online participants from 1,061 municipalities across Brazil. To investigate mechanisms, this experiment exposed subjects to 10 distinct treatments, each testing pre-registered theoretical predictions about multiple declaration actions.[3] Various findings are consistent with theory; for example, citizens are more likely to declare support for a clientelist politician who offers larger material rewards or is heavily favored to win the election, and they are less likely to declare support when clientelism is competitive or if it involves both rewards and punishments. Multivariate analyses show that findings hold even when focusing exclusively on within-subject variation. The experiment also reveals empirical patterns not predicted by theory: citizens are insensitive to whether their declarations can be easily monitored or can influence the election, punishments fail to suppress declarations for the opposition, and the *aggregate* number of declarations is unchanged in most treatments.

Even though our theoretical predictions extend beyond local-level politics, we focus our experiment on declarations for mayoral candidates. In many countries, local politicians have

[3]We received IRB approval on 3/2/2016, and pre-registered deterministic predictions with Evidence in Governance and Politics on 9/20/2016 (before the experiment commenced).

substantial resources and discretion to engage in clientelism; for instance, Brazil's government expenditures are among the most decentralized in the world (IMF, 2016). Declared support for local politicians is important to study not only because of their ability to engage in clientelism, but also because these elites often serve as brokers for state, provincial, and national politicians. In Brazil, Novaes (2018) contends that mayoral candidates frequently mobilize their clienteles to support congressional candidates in exchange for benefits. Likewise, local politicians are often clientelist intermediaries for higher-level politicians in Argentina and the Philippines (Stokes et al., 2013; Ravanilla, et al., 2021). Given that declared support often reinforces relational clientelism (Nichter, 2018), the present article's findings reveal the contextual conditions in which citizens are most likely to heighten local politicians' ability to play this brokerage role.

Overall, the present article emphasizes and elucidates the understudied role of citizen choices in clientelism. Building on insights from our fieldwork, a model unpacks the voter calculus of publicly expressing political support when contingent benefits are distributed. Moreover, our experiment tests predictions and yields important insights for future theoretical and empirical research about clientelism's effects on political expression.

2 Motivating Evidence

To motivate our formal and experimental analyses of declared support, we first provide suggestive evidence from Brazil. In an online survey, we randomly exposed 1,995 participants in over a thousand municipalities to one of several vignettes, depicting citizens who had — or had not — declared support during a fictitious mayoral campaign.[4] As shown in Figure 1, subjects who viewed a vignette depicting a declared supporter of the election winner indicated it would be easier for that citizen to obtain post-election benefits, than did subjects who viewed a vignette depicting an undeclared citizen. First, 43.1 percent of participants

[4]Section 4 discusses survey recruitment and representativeness. This survey experiment has a larger sample size, as it includes additional participants recruited similarly.

exposed to the declared-supporter vignette perceived it would be "easy" or "very easy" for the citizen to obtain a medical treatment, compared to only 35.5 percent of those exposed to the undeclared-citizen vignette. Second, 39.7 percent of subjects viewing the declared-supporter vignette believed it would be "easy" or "very easy" to receive a water cistern, compared to just 28.7 percent of those viewing the undeclared-citizen vignette. And third, 38.6 percent of respondents viewing the declared-supporter vignette believed it would be "easy" or "very easy" to obtain employment, compared to only 18.2 percent of those viewing the undeclared-citizen vignette. These differences, which are all statistically significant (at the .01 level), suggest that Brazilian survey participants perceived a link between declared support and clientelism.[5] Our interviews revealed similar perceptions. For example, a mason said if needing a medical treatment, he would ask "the politicians he voted for," who would help "because he declares his vote before voting." Regarding employment, an interviewee indicated that obtaining a job is "much easier" for declared supporters, with another explaining that "of course [a politician] will first help those who were there with him all the time he needed it, backing his victory."[6] Consistent with such perceptions, quantitative analyses of two surveys in Nichter (2018) suggest that Brazilians who declare for an elected mayor or city councilor are more likely to receive post-election benefits — such as medicine, building materials, water deliveries and job assistance from politicians, as well as private benefits from the municipal government.[7]

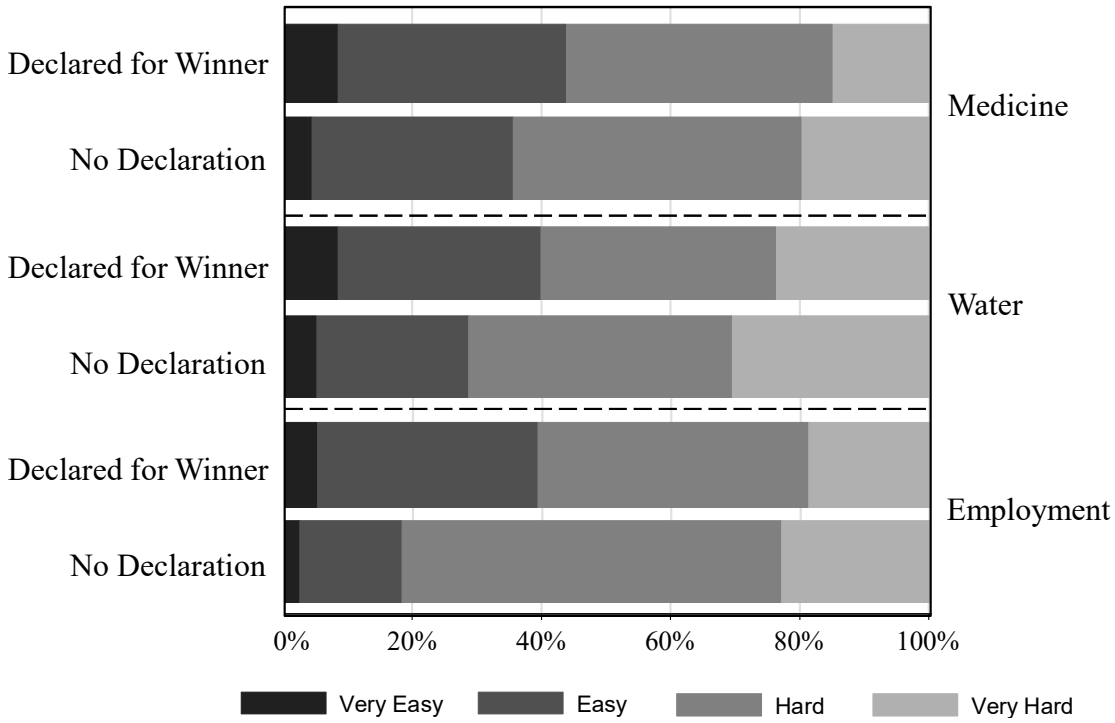
Local politicians are able to monitor and reward declarations in part because they often cultivate clientelist relationships with citizens. In much of Brazil, these relationships build on mayors' direct interactions with constituents, in addition to dense networks of operatives

[5]To explore punishments, others viewed a vignette depicting a declared supporter of the election loser. He was deemed to have more difficulty than the declared-supporter vignette for all benefits, but more difficulty than the undeclared-citizen vignette only for cisterns.

[6]Author's interviews, municipalities in Bahia with 80,000, 10,000 and 15,000 citizens (11/21/2008, 10/16/2008 and 1/12/2009). See Nichter (2018) for further qualitative evidence about the link between declarations and health care, jobs, and water provision.

[7]In addition, its regressions suggest declared supporters are favored with campaign handouts.

Figure 1: Declared Support and Perceived Difficulty of Obtaining Benefits



Source: Authors' survey with 1,995 respondents recruited across Brazil on Facebook (see Footnote 4).

and brokers (Novaes, 2018; Frey, 2020). Amid weak political parties, local notables have long played an important intermediary role in Brazilian municipalities (Nunes Leal, 1949; Novaes, 2018), including city councilor candidates who often monitor declarations on behalf of allied mayoral candidates. As one city councilor explained to us, after a "voter identifies himself" by putting a sign on his house, a clientelist politician rewards "those who declared for him" — "he knows" because "he has the neighborhood on a computer" and "map(s) the city."[8] A 2012 survey in rural Northeast Brazil suggests local politicians reach even relatively isolated households.[9] Over 81 percent of respondents received home visits from representatives of a mayoral candidate during the 2012 municipal campaign, averaging 4.6 visits across the overall sample. Nearly half of respondents reported declaring support: 38 percent placed political

[8]Author's interview in a Bahian municipality with 80,000 citizens (11/18/2008).

[9]Survey conducted by Bobonis et al. (2019) in 40 municipalities with populations between 6,000 and 324,000. N = 3,643 to 3,674.

flags or banners on their homes, 22 percent displayed campaign paraphernalia while attending a rally, and 19 percent wore campaign stickers or t-shirts. Given local politicians' extensive networks, many citizens believe declarations are monitored; as one woman explained, "I think they'll remember you forever."[10] In the 2012 survey, almost two-thirds of respondents believed others would remember who placed campaign flags on their homes — as did 72 percent of citizens engaging in this type of declaration. In addition, over half of respondents believed others would remember rally attendees — as did 68 percent of citizens displaying political paraphernalia at a rally. Building on this motivating evidence, we next explore how contingent benefits affect citizen choices to declare support.

3 Model

3.1 Setup

To investigate declared support, we develop a model with numerous predictions that are tested experimentally. Citizens are modeled as strategic individuals who decide to declare support not only on the basis of political preferences, but also on the basis of inducements and contextual characteristics. Our analysis is decision-theoretic, in that a citizen's behavior depends on exogenous parameters and not actions taken by others.[11] This approach is tailored to the experimental design discussed in Section 4, in which online subjects across Brazil participated asynchronously. The Supplementary Information shows most key predictions are similar when employing a game-theoretic model in which a citizen's action influences other citizens' beliefs about candidates' probability of electoral victory.

In the analysis, each citizen weighs whether to declare support for one of two competing candidates (A or B), or to remain undeclared. A citizen's payoff depends on five factors: (1)

[10] Author's interview in a Bahian municipality with 15,000 citizens (1/15/2009).

[11] Whereas we use decision theory with perfect information to analyze effects of contextual factors on declared support, Nichter's (2018) signaling model clarifies how citizens can use declarations to signal credibility. Both models suggest under specific conditions, some declarations may not represent genuine political preferences.

her political preferences with respect to the election winner, (2) any post-election reward or punishment for declaring, (3) the cost of declaring, (4) expressive utility from declaring, and (5) any impact of her declaration on the election outcome. Given competitive clientelism exists in some contexts (Kitschelt, 2013), the model allows for both candidates to reward and punish declarers; we provide predictions for both monopolistic and competitive clientelism.

The timing of the game is as follows:

1. Citizen i decides whether to declare for candidate A at cost $c_A > 0$, to declare for candidate B at cost $c_B > 0$, or to remain undeclared.
2. Candidates observe citizens' declarations with probability $\gamma \in (0, 1]$.
3. The election winner is decided (potentially influenced by declarations).
4. If candidate A (B) wins, she distributes rewards $r_A \geq 0$ ($r_B \geq 0$) to all citizens observed to declare support for her and imposes punishments $p_A \geq 0$ ($p_B \geq 0$) to all citizens observed to declare support for her opponent.

We make several assumptions based on our fieldwork in Brazil. First, citizens have heterogeneous ideological preferences, ranging from strongly prefer A to strongly prefer B: $x_i \in (-\infty, \infty)$ is citizen i's ideological gain (if positive) or loss (if negative) from A's election victory.[12] Second, candidates distribute rewards (r_A and r_B) after they are elected, to citizens observed (with probability γ) to declare support for them during the campaign. This assumption builds on evidence from our interviews,[13] as well as evidence discussed in Section 2. Third, because politicians in some countries employ negative inducements (Mares and Young, 2016), the model allows candidates to impose punishments (p_A and p_B) on citizens observed to declare support for the opponent.[14] Fourth, we assume declaring involves candidate-specific costs (c_A and c_B). These costs include material costs such as obtaining and placing a banner on one's house, or traveling to demonstrate support at a rally.[15] For ex-

[12]This setup normalizes the ideological gain from B winning to 0; x_i is the amount by which citizen i is better off or worse off when A wins relative to when B wins.

[13]The Supplementary Information describes this fieldwork.

[14]One interpretation of punishments is terminating benefits that a declarer would have received, had she remained undeclared.

[15] While we focus on post-election rewards, the model can also examine campaign rewards: c_A and c_B can represent *net* costs. They include material and social costs of declaring, less any pre-election rewards for declaring. Such campaign rewards would not affect other

ample, one interviewee complained about repainting his wall and removing bumper stickers, and another indicated that rallies are too time-intensive. They also include any social costs, such as being ostracized if declaring for A in a neighborhood mostly populated by B's supporters; e.g., a maid explained she did not declare because the other candidate's supporters would "complain a lot ... they fight, they get angry."[16] Fifth, citizens may receive expressive utility from the act itself of declaring in accordance with their preferences, regardless of who wins the election. This assumption builds on some Brazilian interviewees' discussion of enjoyment received from displaying political paraphernalia of their preferred candidates. We employ a dampening factor, $\delta \in [0, 1]$, to capture the degree to which declaring provides such expressive utility. Just as x_i is citizen i's ideological gain or loss from A's election victory, δx_i is her ideological gain or loss from declaring for A. And inversely, $-\delta x_i$ is her ideological gain or loss from declaring for B. Sixth, the model allows for the possibility that a citizen's declaration affects the election outcome. In our analysis, candidate A's *ex ante* probability of winning the election is given by $q \in (0, 1)$. We assume that a declaration increases that candidate's probability of victory by $\alpha \in [0, \min\{q, 1-q\}]$. If a citizen declares for A (B), then A's (B's) probability of victory is increased by α, which may equal zero — and given there are two candidates, B's (A's) probability of victory declines by α.[17]

3.2 Expected Utility

Table 1 summarizes citizen i's utility contingent on her declaration action and the election outcome. We now investigate the expected utility from each declaration action. The expected utility of citizen i when declaring support for candidate A is:

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parameters or any comparative statics discussed below, so long as there exist citizens who remain undeclared. By contrast, campaign rewards would affect the level of declarations.

[16] Author's interviews in Bahian municipalities with 10,000, 80,000 and 100,000 citizens, respectively (10/22/2008, 11/20/2008 and 12/22/2008).

[17] If *all* partisans derive no expressive utility from declaring (i.e., $\delta = 0$) — an unlikely scenario, based on our interviews — all predictions hold so long as declarations have some impact on electoral outcomes ($\alpha > 0$). Even less likely, if $\delta = \alpha = 0$: (a) all predictions hold for the stochastic model, and (b) the deterministic model's predictions no longer depend on political preferences and the same declaration action is optimal for all citizens.

Table 1: Citizen i's Utility by Declaration Action and Election Outcome

	i Declared for A	i Declared for B	i Undeclared
Candidate A Wins	$x_i + \delta x_i + \gamma r_A - c_A$	$x_i - \delta x_i - \gamma p_A - c_B$	x_i
Candidate B Wins	$\delta x_i - \gamma p_B - c_A$	$-\delta x_i + \gamma r_B - c_B$	0

$$EU_i(A) = (q + \alpha)(\gamma r_A + x_i) - (1 - q - \alpha)\gamma p_B + \delta x_i - c_A \tag{1}$$

By declaring, the citizen receives three components of utility: clientelist, instrumental, and expressive effects. The clientelist effect is composed of the citizen's expected reward and punishment from declaring for A, which depend on four factors: the size of each reward candidate A distributes to declared supporters (r_A), the size of each punishment candidate B imposes to declared opposers (p_B), the probability candidates observe declarations (γ), and each candidate's probability of victory given the citizen's declaration ($q + \alpha$ for A and $1-q-\alpha$ for B). The instrumental effect is the citizen's expected ideological gain or loss from the election outcome, which depends on her preferences with regards to A (x_i) as well as A's probability of victory given her declaration ($q+\alpha$). The expressive effect is the utility gained (lost) from the act of declaring support in accordance (discordance) with one's ideological beliefs (x_i, discounted by δ). In Equation (1), the first term includes both clientelist and instrumental effects. More specifically, it represents the incremental utility accrued from A's victory — for both clientelist (γr_A) and instrumental (x_i) reasons — weighted by the probability A wins given the citizen's declaration ($q + \alpha$). The second term represents the incremental disutility accrued from B's victory for clientelist reasons (γp_B). The third term (δx_i) captures the expressive effect, and the fourth term (c_A) captures declaration costs.

Next, the expected utility of citizen i when declaring for candidate B is:

$$EU_i(B) = (q - \alpha)(x_i - \gamma p_A) + (1 - q + \alpha)\gamma r_B - \delta x_i - c_B \tag{2}$$

Comparing Equations (1) and (2), α is now subtracted (added) when weighting the consequences of A's (B's) victory, as the citizen reduces A's (increases B's) probability of victory when declaring for B. The sign of the expressive utility term (δx_i) is negative in Equation (2), as the act of declaring for B provides a utility gain (loss) to supporters of B (A).

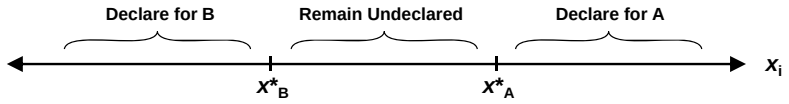
Finally, the expected utility of citizen i from remaining undeclared is: $EU_i(\emptyset) = qx_i$. There are no clientelist or expressive effects of remaining undeclared. The instrumental effect depends on her preferences about A (x_i) and the *ex ante* probability A wins (q).

3.3 Deterministic Choice

Given these expected utilities, we predict citizens' declaration decisions deterministically. To do so, we derive which action provides the highest expected utility to citizens according to their political preferences and other parameter values. We later examine implications if citizens make mistakes during decision making, employing a stochastic choice model.

To simplify exposition, we assume declaration costs and/or clientelist punishments are sufficiently large relative to clientelist benefits, such that there exist citizens who remain undeclared. This assumption is realistic: in real-world campaigns, not every citizen publicly expresses support for a candidate.[18] Citizens with sufficiently intense ideological preferences always declare support for their preferred candidate. For such citizens, the expressive utility from declaring and/or the increased probability of their favored candidate's victory are worth declaration costs and dominate any clientelistic considerations. On the other hand, clientelist considerations and/or declaration costs weigh more heavily on the decisions of citizens with weaker ideological preferences. Figure 2 shows the ideological space along which citizens can be arranged according to their ideological preferences. Moving along the spectrum of ideological preferences, the incentive to declare support for candidate A increases as x_i rises.

Figure 2: Optimal Behavior as a Function of Ideological Preferences



Citizens on the left, with smaller values of x_i, are supporters of B, whereas citizens on the right, with higher values of x_i, are supporters of A. The right cutpoint ($x_A^\star$) represents a citizen whose ideological preferences make her indifferent between declaring for A and remaining undeclared. The left cutpoint ($x_B^\star$) represents a citizen whose ideological preferences

[18]More precisely, we assume the numerator of the right-hand side of Equation (4) is positive.

make her indifferent between declaring for B and remaining undeclared. The assumption that non-declarers exist enables us to focus on the case in which $x_B^\star < x_A^\star$. All citizens to the right of $x_A^\star$ (i.e., who prefer A more strongly than $x_A^\star$) declare support for A. By contrast, all citizens to the left of $x_B^\star$ (i.e., who prefer B more strongly than $x_B^\star$) declare support for B. Citizens between $x_A^\star$ and $x_B^\star$ remain undeclared. Depending on contextual characteristics — i.e., on values of model parameters — both cutpoints may represent supporters of the same party (i.e., $x_A^\star > x_B^\star > 0$). In such cases, clientelist considerations dominate instrumental and expressive considerations for some citizens, who declare for the candidate they dislike.[19]

To derive these two cutpoints, we observe that citizen i prefers declaring for A over remaining undeclared when $EU_i(A) > EU_i(\emptyset)$, and prefers declaring for B over remaining undeclared when $EU_i(B) > EU_i(\emptyset)$. Substituting equations and solving yields:

$$x_A^\star = \frac{c_A - (q+\alpha)\gamma r_A + (1-q-\alpha)\gamma p_B}{\alpha+\delta} \quad x_B^\star = \frac{-c_B + (1-q+\alpha)\gamma r_B - (q-\alpha)\gamma p_A}{\alpha+\delta} \tag{3}$$

Undeclared citizens are those with ideology between these cutpoints. Thus, the fraction of undeclared citizens is proportional to the distance between the cutpoints $(x_A^\star - x_B^\star)$, where:

$$x_A^\star - x_B^\star = \frac{c_A + c_B + \gamma[-(q+\alpha)r_A - (1-q+\alpha)r_B + (q-\alpha)p_A + (1-q-\alpha)p_B]}{\alpha+\delta} \tag{4}$$

Our objective is to derive comparative statics for the effect of increasing each parameter on the fraction of citizens who declare for A, declare for B, or remain undeclared. Formal analysis in the Supplementary Information yields the predictions in Table 2, shown for the cases of monopolistic and competitive clientelism, both with and without punishments.

Moreover, the formal analysis yields additional predictions that do not involve parameter shifts (H8 and H9), or are broadly conditional on parameter values (H10 and H11):

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[19] In such cases, declarations still convey meaningful information about political preferences: A (B) supporters are more likely to declare for A (B) than B (A) supporters are; i.e., the probability a citizen supports A (B) conditional on declaring for A (B) exceeds 50%. For example, when $x_A^\star < 0$, *all* A supporters declare for A, but only *some* B supporters do.

Table 2: Predicted Impact of Parameter Changes on Declarations (Deterministic Model)

	Monopolistic Clientelism					
	Rewards Only			**Rewards and Punishment**		
Parameter Change	**Declare A**	**Declare B**	**Undeclared**	**Declare A**	**Declare B**	**Undeclared**
H1: A's Clientelist Reward Increases	Increases	No Effect	Decreases	Increases	No Effect	Decreases
H2: A's Probability of Winning Increases	Increases	No Effect	Decreases	Increases	Decreases	Conditional
H3: Cost of Declaring for A Increases	Decreases	No Effect	Increases	Decreases	No Effect	Increases
H4: Clientelist Monitoring Increases	Increases	No Effect	Decreases	Increases	Decreases	Conditional
H5: A's Clientelist Punishment Increases	–	–	–	No Effect	Decreases	Increases
	Competitive Clientelism					
	Rewards Only			**Rewards and Punishment**		
Parameter Change	**Declare A**	**Declare B**	**Undeclared**	**Declare A**	**Declare B**	**Undeclared**
H1: A's Clientelist Reward Increases	Increases	No Effect	Decreases	Increases	No Effect	Decreases
H2: A's Probability of Winning Increases	Increases	Decreases	Conditional	Increases	Decreases	Conditional
H3: Cost of Declaring for A Increases	Decreases	No Effect	Increases	Decreases	No Effect	Increases
H4: Clientelist Monitoring Increases	Increases	Increases	Decreases	Conditional	Conditional	Conditional
H5: A's Clientelist Punishment Increases	–	–	–	No Effect	Decreases	Increases
H6: B's Clientelist Reward Increases	No Effect	Increases	Decreases	No Effect	Increases	Decreases
H7: B's Clientelist Punishment Increases	–	–	–	Decreases	No Effect	Increases

H8 *Relative Impact of Rewards vs. Punishments*: Rewards affect declarations relatively more than punishments of comparable magnitude. That is, the marginal effect of a candidate's rewards on increasing declarations for her is greater than the marginal effect of the same candidate's punishments on decreasing declarations for her opponent.

H9 *Relative Impact of Rewards Across Candidates*: The more popular candidate has a greater impact on declarations, using rewards of identical magnitude. That is, the marginal effect of A's rewards on increasing A's declarations is greater than that of B's rewards on increasing B's declarations, if and only if A is more popular than B. If candidates enjoy the same support, the relative impact of their rewards is identical.

H10 *Expressive Utility*: As expressive utility from declaring increases, non-declarations fall. A's declarations increase if $x_A^\star > 0$, decrease if $x_A^\star < 0$, and are unaffected if $x_A^\star = 0$. B's declarations increase if $x_B^\star < 0$, decrease if $x_B^\star > 0$, and are unaffected if $x_B^\star = 0$.

H11 *Election Influence*: As the election influence of declaring increases, A's declarations increase if $x_A^\star > \psi$, decrease if $x_A^\star < \psi$, and are unaffected if $x_A^\star = \psi$ (where $\psi = -\gamma(r_A + p_B)$). B's declarations increase if $x_B^\star < \theta$, decrease if $x_B^\star > \theta$, and are unaffected if $x_B^\star = \theta$ (where $\theta = \gamma(p_A + r_B)$). Non-declarations increase if $x_A^\star - x_B^\star < \phi$, decrease if $x_A^\star - x_B^\star > \phi$, and are unaffected if $x_A^\star - x_B^\star = \phi$ (where $\phi = -\gamma(r_A + p_B + p_A + r_B)$).

Several conditional predictions warrant further discussion. First, observe in Table 2 that H4 is always conditional in the case of competitive clientelism with rewards and punishment. The reason is that a marginal increase in monitoring (γ) not only increases the probability of a reward from the candidate for whom the citizen declares, but also increases the probability of punishment from the other candidate. Thus, the effect of monitoring is conditional on the size of rewards and punishments (weighted by the electoral odds). Second, observe H10

and H11 are broadly conditional on parameter values. Regarding H10, assume citizen j has ideology $x_A^\star$; recall this threshold indicates indifference between declaring for A and remaining undeclared. If he is a supporter of A (i.e., $x_A^\star > 0$), a marginal increase in δ increases his expressive gain from declaring; and a marginal increase in α strengthens his clientelist and instrumental incentives to declare. Thus, in both cases, j becomes strictly better off declaring for A, meaning another citizen — who more weakly supports A — lies on the threshold $x_A^\star$. On the other hand, if j is a supporter of B (i.e., $x_A^\star < 0$), a marginal increase in δ increases his expressive loss from declaring. Thus, he becomes strictly worse off declaring for A, meaning another citizen — who more weakly supports B — lies on the threshold $x_A^\star$. Regarding H11, a marginal increase in α strengthens the clientelist incentive to declare for A, by heightening the probability of a reward by A and decreasing the probability of a punishment by B. However, it also increases the instrumental incentive to remain undeclared, as declaring is more likely to clinch the victory of his disfavored candidate. If j is a sufficiently strong B supporter, the latter effect dominates, so he is strictly better off remaining undeclared. This means that another citizen — who more weakly supports B — lies on the threshold $x_A^\star$.

3.4 Stochastic Choice

Despite offering many predictions, one might be concerned that the deterministic model above is unrealistic, because citizens do not always make optimal choices. For instance, someone nearly indifferent between two actions may make minute errors in judgments about rewards, declaration costs, electoral odds, or other factors, leading to a suboptimal decision. Given such concerns, the Supplementary Information derives predictions employing a probabilistic choice model, following many prominent studies testing theoretical predictions with laboratory experiments (e.g., Harless and Camerer, 1994; Levine and Palfrey, 2007). In particular, we allow a small degree of bounded rationality and assume that instead of optimizing, citizens make decisions according to a standard Logit stochastic choice rule: they choose with positive probability all available actions, but are more likely to choose "bet-

ter" alternatives. That is, in their randomization, they place more (less) weight on actions providing higher (lower) payoffs.

Section 5 below compares experimental findings to predictions of both the deterministic and stochastic models. Table 4 shows the two models' predictions are similar. Two sources of discrepancies arise. First, for most hypotheses, whenever the deterministic model predicts a change, the stochastic model's predictions are unambiguously in the same direction. But in some cases, the deterministic model predicts no effect, whereas the stochastic choice model does. The reason is that citizens in the deterministic model choose with certainty the action delivering the *highest* expected utility, while citizens in the stochastic model are more likely to choose actions delivering *higher* relative expected utility.[20]

A second source of discrepancies is that the stochastic model predicts heterogeneous treatment effects for Hypotheses 8-11. Depending on citizens' political preferences, shocks to these factors have differential effects on the propensity of citizens to take each declaration action. For these hypotheses, the stochastic model makes ambiguous predictions for average treatment effects, which depend crucially on the value of other parameters and the sensitivity of choices to relative payoffs.[21] Given the stochastic model's ambiguity, the experimental section exclusively tests the deterministic model for these four hypotheses.

4 Experimental Design

The theoretical analyses developed above provide intriguing hypotheses, but to what extent do they offer meaningful predictions about human behavior? If citizens are exposed to the model's conditions, their declaration choices might be entirely unaffected by rewards, political competition and other factors — or they may change in unpredicted ways. Such

[20]Consider a slight perturbation of any model parameter that changes only the expected utility from declaring for A (B). Given non-declarers exist, in the deterministic model, declarations for A (B) are affected while declarations for B (A) are not. By contrast, perturbing any such parameter affects all of the stochastic model's predictions, because changing any action's expected utility influences the relative expected utility of all actions.

[21]The Supplementary Information derives these predictions.

findings would cast serious doubt on our modeling assumptions. By contrast, if citizens tend to respond as predicted, it would heighten confidence in its theoretical insights about how and why clientelism influences political expression beyond the ballot box. To investigate whether the model provides meaningful predictions about human behavior, we developed an experiment to test our pre-registered hypotheses.[22]

Given that external validity is always an important concern with experiments, we focused testing efforts on Brazil, where our fieldwork revealed patterns of declared support. We sought a large subject pool from across the nation, and thus recruited 1,259 participants from 1,061 municipalities. To recruit participants, we broadcast advertisements on Facebook in October-December 2016, following an established strategy employed in Brazil (Samuels and Zucco, 2014; Boas, 2014). Facebook's impressive reach in the nation makes it a particularly useful tool for recruiting subjects. Brazil is Facebook's third-largest market globally, with 123 million registered users during our research period, compared to an overall population of 207 million.[23] A 2019 survey by the Latin American Public Opinion Project found that 60 percent of Brazilian respondents had a Facebook account, of which 92 percent accessed it repeatedly each week. Of course, Facebook is by no means perfectly representative of Brazil's population, and certain types of users may be more inclined to click on advertisements. As such, we advertised more extensively to specified demographic subgroups, particularly women and the elderly. As shown in the Supplementary Information, our sample mirrors Brazil's overall population fairly closely with respect to gender, age, region and urban/rural mix, but is considerably more educated.[24] Given the paucity of research on clientelism outside of Brazil's largest cities, we displayed advertisements in both urban and rural areas of municipalities with populations up to 250,000 citizens. This

[22]See Footnote 3 for pre-registration details.

[23]*Financial Times*, 5/15/2017; www.statista.com/statistics/268136/top-15-countries-based-on-number-of-facebook-users; IBGE, 2017.

[24]Research suggests online samples tend to be disproportionately more educated in various countries (e.g., Boas et al. 2020).

inclusion criterion captures 98.3 percent of all municipalities, with 59.7 percent of Brazil's population. Of 2,433 citizens who consented to participate, 16.6 percent exited before the experiment began, and an average of 3.5 percent exited after each round, yielding 1,259 participants who completed all ten rounds. The Supplemental Information shows findings are robust to including subjects who terminated the survey early, and finds some evidence of differential attrition.[25] Our experimental design is unaffected by such attrition; we return to external validity considerations in the Discussion. Our sample proved to be quite familiar with clientelism: 87.3 percent of participants thought clientelist benefits were distributed "frequently" or "very frequently" by candidates in their municipalities, and 14.4 percent reported receiving such handouts in 2016.

As incentives in the experiment, subjects earned and accumulated lottery tickets, which increased their probability of winning one of four awarded iPhones. This incentive mechanism followed prior studies and facilitated our online experiment, given that many Brazilians do not use electronic payments.[26] Facebook users clicking on our advertisement were redirected to a consent page and then commenced the experiment. Following Berinsky, Margolis and Sances (2014), the survey included two screener questions, enabling us to control for participant attentiveness in multivariate analyses.

The experiment elicited participants' willingness to declare support for fictitious candidates, employing incentives to manipulate clientelist inducements and preferences about candidates. Subjects could expend a small number of lottery tickets to declare support for one of two candidates (A or B) by displaying a corresponding flag on her fictitious home, potentially affecting the election outcome. We induced political preferences using a standard

[25] Attrition is significantly higher among younger respondents. Less-educated and female respondents also have higher attrition, but only if excluding controls.

[26] For a review of studies using similar lotteries as experimental rewards, and a large-scale experiment demonstrating their effectiveness, see Conn et al. (2019). Kirchkamp et al. (2021) suggest lottery tickets motivate behavior similar to cash payments. Using two prominent experimental methods for measuring risk attitudes, it finds paying subjects with lottery tickets instead of cash has no significant effect on choices or elicited risk preferences.

methodology in experimental social sciences: a reward mechanism in which election outcomes generate different monetary values (Induced Value Theory, Smith 1976). In particular, we induced a stronger ideological affiliation with a candidate by increasing the iPhone lottery tickets a citizen received from that candidate's victory (regardless of whether she declared support). Subjects were assigned randomly to one of seven partisan types — each type induced to have distinct preferences, ranging from strongly prefer A to strongly prefer B.[27]

We introduced clientelist rewards by increasing the lottery tickets received if a citizen declared for clientelist candidate A and A won the election; competitive clientelism is also explored below. As shown in the Supplementary Information, before subjects chose whether and for whom to declare, they viewed a simple vignette communicating information associated with each choice. Once a subject submitted her choice, the election winner was determined by the computer using the odds resulting from the citizen's declaration decision. The identity of the election winner and the resulting clientelistic rewards (if any) determined the subject's earnings for each election.

In order to facilitate subjects' understanding, information in the vignettes was also represented graphically.[28] For each declaration choice, subjects viewed an urn with 10 balls. The election outcome and lottery tickets earned were determined by the random draw of a ball out of the urn chosen by the subject. Balls in the urn were green or yellow (corresponding to which candidate won if selected) and were imprinted with numbers (corresponding to their earnings if selected). We controlled the probability a candidate won the election by manipulating how many of the 10 balls were green vs. yellow. We controlled subjects' partisan types by manipulating the numbers displayed on each ball. To illustrate how we induced partisan types, consider the *No Clientelism* treatment, in which a subject's earnings

[27]The sample includes 186 subjects assigned to be strong A supporters, 193 moderate A supporters, 193 weak A supporters, 175 indifferent citizens, 171 weak B supporters, 162 moderate B supporters, and 179 strong B supporters (with $x_i = 30, 15, 5, 0, -5, -15$, and -30, respectively).

[28]The Online Appendix includes examples of vignettes and their graphical representation.

are determined only by her partisan type, the election outcome, and declaration costs (2 tickets if declaring). Subjects randomly assigned to be strong supporters earned 94 tickets if their preferred candidate won and 34 otherwise; moderate supporters earned 64 tickets if their preferred candidate won and 34 otherwise; weak supporters earned 44 tickets if their preferred candidate won and 34 otherwise; and indifferent citizens earned 34 tickets regardless who won. All other treatments used identical earnings by partisan type, increased or decreased by clientelist rewards or punishments as described below.

To test model predictions, the experiment employed ten distinct treatments, each involving a fictitious election with different contextual characteristics. Using a within-subject design, each participant was exposed to all ten treatments.[29] This design has important advantages over a between-subject design. First, it enables us to control for unobserved individual characteristics that may affect citizens' choices, such as risk aversion and cognitive abilities, through the use of fixed effects in regression analyses. And second, it increases the statistical power of our analyses because each participant contributes an observation for each of the ten treatments. However, observations are not independent in a within-subject design, and participants could potentially behave differently in later treatments. We address this concern by randomizing the order in which subjects observe treatments, and by controlling for within-subject error correlation in regression analyses.

In the *Baseline Clientelism* treatment, subjects were presented with a close election between a clientelist candidate who delivers rewards to declared supporters once elected, and a non-clientelist candidate who delivers no rewards. We use behavior in this baseline as a benchmark to evaluate the effect of treatment variables. A fundamental attribute of the experimental setup is that all other treatments (italicized below) modify only one factor at a time, thereby leaving all other elements of the decision environment constant. Such

[29]The Supplemental Information shows the identical number of findings are significant when including subjects who terminated the survey early (i.e., viewing only some treatments).

comparisons enable us to draw inferences about the causal link between contextual factors and citizen behavior.

In *No Clientelism*, we remove clientelist rewards for declaration, but maintain all other elements identical to *Baseline Clientelism*. Comparing these two treatments identifies the causal impact of clientelism on the prevalence of declared support, testing Hypothesis 1. Next, *Lopsided Election* tests Hypothesis 2 by increasing the clientelist candidate's probability of victory from 50 to 80 percent, before declarations of support. To examine Hypothesis 3, *Cost* investigates the scenario in which declaring for the clientelist candidate involves greater costs than declaring for the opposition.[30] Testing Hypothesis 4, *Low Monitoring* adapts *Baseline Clientelism* to consider the case in which the clientelist candidate has a lower ability to observe declarations.[31] Next, we employ *Punishment & Reward* and *Punishment Only* to investigate effects when the clientelist candidate, if elected, punishes citizens who declared for the opposition — testing Hypothesis 5. *Punishment & Reward* leaves in place the baseline's rewards, while *Punishment Only* eliminates them. With *Competitive Clientelism*, we examine Hypothesis 6 by considering the scenario in which both candidates distribute rewards to their own declared supporters.[32] To test Hypothesis 8, we compare the treatment effect of adding A's punishments (i.e., the difference in behavior between *Punishment Only* and *No Clientelism*) and the treatment effect of adding A's rewards (i.e., the difference in behavior between *No Clientelism* and *Baseline Clientelism*). To test Hypothesis 9, we compare the treatment effect of adding B's rewards (i.e., the difference in behavior between *Baseline Clientelism* and *Competitive Clientelism*) and the treatment effect of adding

[30]Though we do not consider campaign handouts, Footnote 15 explains the model can incorporate them using net costs. They can thus be examined comparing *Cost* to *Baseline Clientelism*.

[31]For this treatment, the text indicates the candidate rewards the flag "if he sees it," and explains how many tickets are earned if he sees vs. does not see the flag. In the urn, some of the candidate's balls show only the baseline earnings for the corresponding partisan type, and others show these earnings augmented by the clientelist reward.

[32]The experiment does not test Hypothesis 7 (punishments by B).

Table 3: Parameters for Experimental Design

Treatment	r_A	q	c_A	γ	p_A	r_B	δ	α	c_B
Baseline Clientelism	5	.5	2	1	0	0	0	.1	2
No Clientelism	0	.5	2	1	0	0	0	.1	2
Lopsided Election	5	.8	2	1	0	0	0	.1	2
Cost	5	.5	4	1	0	0	0	.1	2
Low Monitoring	5	.5	2	.2	0	0	0	.1	2
Punishment Only	0	.5	2	1	5	0	0	.1	2
Clientelism and Punishment	5	.5	2	1	5	0	0	.1	2
Competitive Clientelism	5	.5	2	1	0	5	0	.1	2
Expressive Utility	5	.5	2	1	0	0	.5	.1	2
No Election Influence	5	.5	2	1	0	0	0	0	2

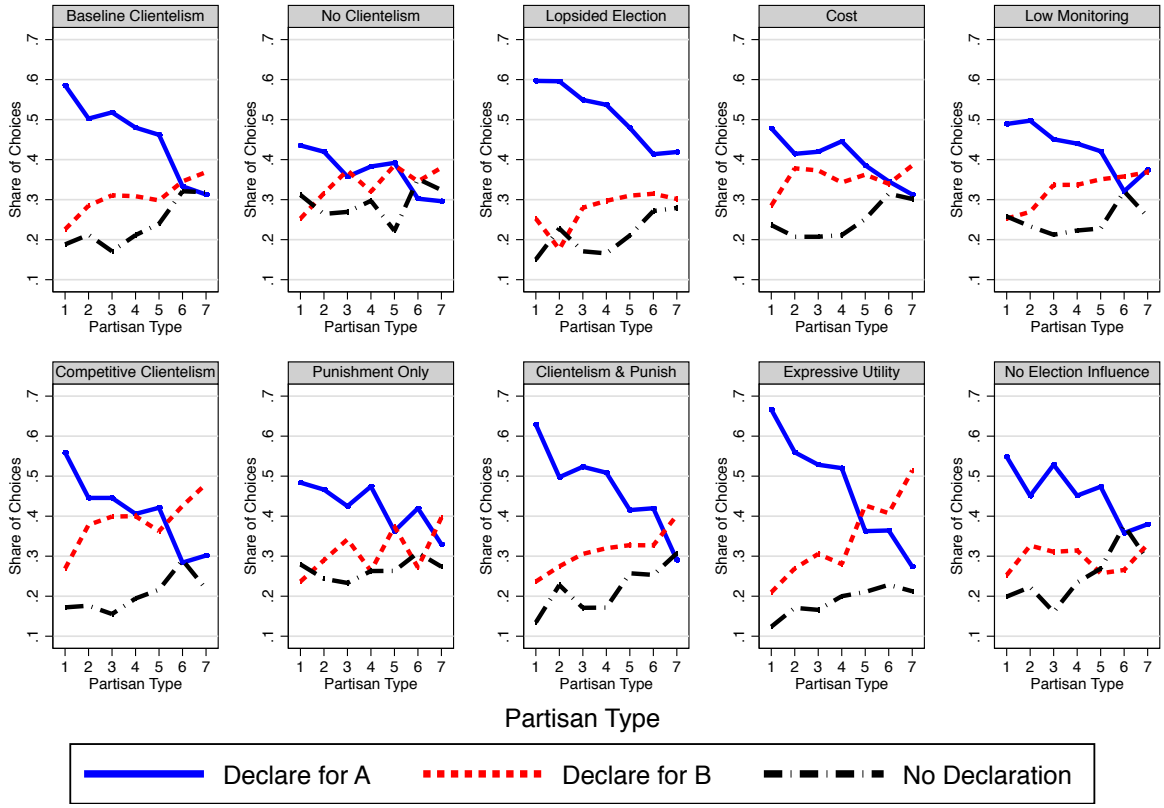
Note: Red text indicates parameters differing from *Baseline Clientelism*.

A's rewards (i.e., the difference in behavior between *No Clientelism* and *Baseline Clientelism*). In *Expressive Utility*, we examine Hypothesis 10 by introducing a benefit from declaring in accordance with one's preferences and a cost from declaring against one's preferences (regardless of the election outcome). In *No Election Influence*, we study the case where declaring for a candidate has no effect on that candidate's probability of winning the election, testing Hypothesis 11. To summarize the experimental design, Table 3 shows the parameters used in these treatments.

5 Experimental Results

Each of 1,259 participants who completed the experiment made ten declaration decisions. Figure 3 presents a descriptive overview of declaration decisions, by partisan type, for the ten experimental treatments. In line with the structure of incentives, participants induced to prefer candidate A were most likely to declare for A (i.e., the solid lines slope downward). In addition, participants induced to prefer candidate B were most likely to declare for B (i.e., the dashed lines slope upward). To test predictions, we first examine how the overall proportion of subjects undertaking each declaration action varies across treatments. These

Figure 3: Declaration Choices of Participants, by Treatment



Note: Figures reflect the share of experimental participants (N = 1,259) who declared for A, declared for B, and did not declare. Shares are shown by partisan type labeled on horizontal axes: (1) strong A supporter, (2) moderate A supporter, (3) weak A supporter, (4) indifferent citizen, (5) weak B supporter, (6) moderate B supporter, and (7) strong B supporter.

proportions are shown in Table 4, with declarations for clientelist candidate A in Panel A, declarations for opposition candidate B in Panel B, and non-declarations in Panel C. We discuss these results below.[33] After employing differences in proportions to assess predictions, we demonstrate findings remain robust when conducting multivariate regressions and examining within-subject variation across treatments.

While experimental findings conform with many theoretical expectations, primary sources of discrepancies warrant consideration at the outset. With respect to the deterministic model, discrepancies primarily arise because it predicts that treatments changing the expected util-

[33]Tests of Hypotheses 8 and 9 compare two treatment effects; they are thus excluded from Table 4 but discussed below.

Table 4: Declaration Choices by Treatment

	Panel A: Declaration for Clientelist Candidate *A*						
Treatment	*Proportion*	*Difference*	*p-value*	*Deterministic Prediction*		*Stochastic Prediction*	
Baseline Clientelism	0.460	Baseline	Baseline	Baseline		Baseline	
No Clientelism	0.371	-0.089	0.000	Decreases	✓	Decreases	✓
Lopsided Election	0.516	0.056	0.002	Increases	✓	Increases	✓
Cost	0.402	-0.058	0.002	Decreases	✓	Decreases	✓
Low Monitoring	0.431	-0.029	0.069	Decreases	✗	Decreases	✗
Punishment Only	0.424	0.053	0.003	No Effect	✗	Increases	✓
Clientelism and Punishment	0.472	0.048	0.008	Increases	✓	Increases	✓
Competitive Clientelism	0.412	-0.048	0.008	No Effect	✗	Decreases	✓
Expressive Utility	0.473	0.013	0.739	Decreases	✗	—	
No Election Influence	0.458	-0.002	0.468	Decreases	✗	—	
	Panel B: Declaration for Opposition Candidate *B*						
Treatment	*Proportion*	*Difference*	*p-value*	*Deterministic Prediction*		*Stochastic Prediction*	
Baseline Clientelism	0.305	Baseline	Baseline	Baseline		Baseline	
No Clientelism	0.338	0.033	0.037	No Effect	✗	Increases	✓
Lopsided Election	0.274	-0.031	0.043	No Effect	✗	Decreases	✓
Cost	0.353	0.048	0.006	No Effect	✗	Increases	✓
Low Monitoring	0.323	0.018	0.162	No Effect	✓	Increases	✗
Punishment Only	0.311	-0.028	0.068	Decreases	✗	Decreases	✗
Clientelism and Punishment	0.312	0.002	0.466	No Effect	✓	Decreases	✗
Competitive Clientelism	0.387	0.082	0.000	Increases	✓	Increases	✓
Expressive Utility	0.342	0.037	0.025	Increases	✓	—	
No Election Influence	0.295	-0.010	0.714	Increases	✗	—	
	Panel C: No Declaration						
Treatment	*Proportion*	*Difference*	*p-value*	*Deterministic Prediction*		*Stochastic Prediction*	
Baseline Clientelism	0.235	Baseline	Baseline	Baseline		Baseline	
No Clientelism	0.291	0.056	0.001	Increases	✓	Increases	✓
Lopsided Election	0.210	-0.025	0.063	Decreases	✗	Decreases	✗
Cost	0.245	0.010	0.272	Increases	✗	Increases	✗
Low Monitoring	0.246	0.011	0.257	Increases	✗	Increases	✗
Punishment Only	0.265	-0.025	0.077	Increases	✗	Increases	✗
Clientelism and Punishment	0.216	-0.049	0.002	Decreases	✓	Decreases	✓
Competitive Clientelism	0.201	-0.034	0.019	Decreases	✓	Decreases	✓
Expressive Utility	0.186	-0.049	0.001	Decreases	✓	—	
No Election Influence	0.247	0.012	0.758	Decreases	✗	—	

Notes: 1,259 observations. Predictions aggregate across all partisan types. *p*-values refer to one-tailed difference in proportions Z tests. Treatments marked ✓if consistent with predictions at the .05 level and ✗ if at the .10 level; ✗ otherwise. All results are robust to using two-tailed tests, with the exception of *No Clientelism* in Panel B (*p*-value: 0.073) and *Lopsided Election* in Panel B (*p*-value: 0.087). To ensure analyses isolate the effect of a single parameter change: (1) *Punishment Only* is compared to *No Clientelism*, and (2) *Clientelism and Punishment* is compared to *Punishment Only*.

ity from declaring for A (B) will affect A's (B's) declarations but not B's (A's) declarations. In numerous cases, we observe significant treatment effects on declarations for both candidates, largely because subjects close to $x_A^\star$ and $x_B^\star$ made suboptimal choices that are not random but reflect the shock to relative incentives. By contrast, the stochastic model captures such errors in decision making, so it conforms with more experimental findings. Some discrepancies arise, however, from the stochastic model's sensitivity: it predicts some change to the probability distribution over actions after any parameter change. In several cases, subjects did not alter their declaration choices enough to yield significant effects, even when signs corresponded with predictions. Notwithstanding such discrepancies, the experiment predominantly corroborates predictions about declared support.

No Clientelism (H1)

The first treatment examines how citizens' public expressions of political support change in response to clientelist inducements. Table 4 shows that declarations for A decrease from 46.0 percent in *Baseline Clientelism* to 37.1 percent in *No Clientelism* ($p = .01$), declarations for B increase from 30.5 to 33.8 percent ($p = .04$), and non-declarations increase from 23.5 to 29.1 percent ($p = .01$). These findings confirm that clientelist rewards successfully induced participants to alter their declarations in our experiment, consonant with fieldwork and surveys in Brazil and elsewhere. Two of these three experimental findings are consistent with predictions of the deterministic model; all three findings are consistent with predictions of the stochastic model.

Lopsided Election (H2)

We next turn to political competition, a contextual characteristic that has important effects on clientelism. Amidst inducements, how might low political competition influence citizens' choices about whether to express political preferences publicly? To explore this question, we test how declarations change if clientelist candidate A is heavily favored to win the election. Table 4 demonstrates that A's declarations increase from 46.0 percent in

Baseline Clientelism to 51.6 percent in *Lopsided Election* ($p = .01$), B's declarations decrease from 30.5 to 27.4 percent ($p = .04$), and non-declarations decrease from 23.5 to 21.0 percent ($p = .06$). These experimental findings suggest uncompetitive elections amplify the effect of clientelism on political expression. Two of these three results conform with predictions of the deterministic model, and all three conform with the stochastic model (though the effect on non-declarations is only significant at the 10 percent level).

Cost (H3)

Even in contexts without clientelism, the social context of a neighborhood can discourage or encourage various forms of political expression (Huckfeldt, 1979), and citizens' decisions to display yard signs and bumper stickers in particular can be influenced by neighbors (Huckfeldt and Sprague, 1995; Makse and Sokhey, 2014). What if citizens who declare for clientelist candidate A suffer social costs imposed by their neighbors? As shown in Table 4, A's declarations decrease from 46.0 percent in *Baseline Clientelism* to 40.2 percent in *Cost* ($p = .01$), B's declarations increase from 30.5 to 35.3 percent ($p = .01$), and non-declarations increase from 23.5 to 24.5 percent ($p = .27$). Participants were thus responsive to this asymmetric increase in declaration costs, suggesting that machines' well-known challenges when obtaining votes in opposition territory (Stokes, 2005) may also extend beyond the ballot box. Findings comport with both models' predictions regarding A's declarations, but are less consistent with other expectations. While the stochastic model predicts the observed increase in B's declarations, the deterministic model expects no effect. With regards to non-declarations, the observed increase is consistent with both models, but is statistically insignificant.

Low Monitoring (H4)

Machines can monitor citizens to minimize opportunistic defection, and the effectiveness of these efforts depends in part on their organizational infrastructure and resources (Stokes, 2005). How does reducing this monitoring capability affect voters' decisions to express political preferences publicly? Table 4 reports results from this experimental treatment: A's

declarations decreased from 46.0 percent in *Baseline Clientelism* to 43.1 percent in *Low Monitoring* ($p = .07$), B's declarations increased from 30.5 percent to 32.3 percent ($p = .16$), and non-declarations increased from 23.5 percent to 24.6 percent ($p = .26$). The decrease in A's declarations is consistent with both models and suggests that monitoring has effects on clientelism extending beyond vote choices and turnout (Stokes et al., 2013). Beyond these marginally significant findings, neither model predicts the observed insignificant effects on B's declarations and non-declarations. Although signs follow predictions, one might be surprised by this weak evidence regarding monitoring, a vigorously debated topic in the clientelism literature (Hicken and Nathan, 2020). A potential explanation for citizens' insensitivity to the likelihood of monitoring is probability distortion, a key pillar of prospect theory (Kahneman, 1979). If citizens overweight small probabilities and underweight large probabilities, as much research suggests (e.g., Camerer and Ho, 1994; Tversky and Kahneman, 1992), then they may perceive minimal differences between the two treatments.[34]

Punishments (H5 and H8)

Although studies of clientelism tend to focus on positive inducements, elites in some contexts employ punishments to influence voter behavior. The role of negative inducements in clientelism is emphasized by Mares and Young (2016), who note that substantially more Afrobarometer respondents report fear of punishments than offers of rewards. When a machine metes out punishments, how does this affect public expressions of political support? We investigate this question experimentally with *Punishment Only*, in which A exclusively punishes citizens who declared for B. Comparing results to *No Clientelism* isolates the causal effect of introducing punishments; that is, any differences between the two treatments reflects declarations altered by negative inducements.[35] As shown in Table 4, A's declarations increase from 37.1 percent in *No Clientelism* to 42.4 percent in *Punishment*

[34]Given probability distortion, they may underweight the probability of receiving a reward conditional on declaring in *Baseline Clientelism*, and overweight it in *Low Monitoring*.

[35]This comparison isolates the effect because it changes only one parameter value (see Table 3). A is assumed to observe all declarations, as in all treatments except *Low Monitoring*.

Only ($p = .01$), B's declarations decrease from 33.8 to 31.1 percent ($p = .07$), and non-declarations decrease from 29.1 to 26.5 percent ($p = .08$). Surprisingly, and contrary to both models' predictions, punishments thus neither suppress the opposition's declarations nor increase non-declarations. On the other hand, in line with the stochastic model, they boost declarations for the candidate employing punishments.

And what if, as is commonly observed (Mares and Young, 2016), politicians mix positive and negative inducements? To isolate effects of this portfolio approach from that of punishments alone, we compare the aforementioned *Punishment Only* treatment with *Clientelism and Punishment*, in which A rewards its own declared supporters and punishes citizens who declared for B. Table 4 shows that A's declarations increase from 42.4 percent in *Punishment Only* to 47.2 percent in *Clientelism and Punishment* ($p = .01$), B's declarations are virtually unchanged, and non-declarations decrease from 26.5 to 21.6 percent ($p = .01$). These findings comport with all three predictions of the deterministic model, and two of three predictions of the stochastic model; the latter predicted a fall in B's declarations, which was not observed.

The experiment also sheds light on the relative effectiveness of rewards and punishments. In particular, the deterministic model predicts the effect of r_A on increasing A's declarations is strictly larger than the effect of p_A on decreasing B's declarations. We test this prediction comparing the observed effect of adding A's rewards (i.e., the difference in behavior between *Baseline Clientelism* and *No Clientelism*) with the observed effect of adding A's punishments (i.e., the difference in behavior between *Punishment Only* and *No Clientelism*). The reward r_A increases A's declarations by 8.9 percentage points (46.0 percent in *Baseline Clientelism* minus 37.1 percent in *No Clientelism*), while the punishment p_A decreases B's declarations by 2.8 percentage points (31.1 percent in *Punishment Only* minus 33.8 percent in *No Clientelism*). This finding is consistent with our theoretical prediction, and the difference between these two treatment effects is statistically significant ($p = 0.01$ using a chi-square test).

Competitive Clientelism (H6 and H9)

In numerous countries, clientelism is not a monopolistic phenomenon, but rather involves multiple machines providing rewards in the same localities. For instance, Kitschelt's (2013) cross-national survey reveals competitive clientelism in nations such as Hungary, Ghana, Indonesia and Nigeria. How would competitive clientelism affect citizens' declaration choices? To investigate this question, Table 4 shows results for a treatment in which both candidates provide contingent rewards: A's declarations decrease from 46.0 percent in *Baseline Clientelism* to 41.2 percent in *Competitive Clientelism* ($p = .01$), B's declarations increase from 30.5 to 38.7 percent ($p = .01$), and non-declarations decrease from 23.5 to 20.1 percent ($p = .02$). These findings complement the broader literature's discussion of various challenges facing dominant machines amidst the rise of competitive clientelism: their public declarations also fall as alternative providers of benefits emerge. Moreover, these results corroborate two of three predictions of the deterministic model, and all three predictions of the stochastic model.

With regards to Hypothesis 9, we can also test predictions about the relative effectiveness of rewards offered by competing candidates with comparable political support.[36] In particular, the deterministic model predicts the impact of A's rewards on increasing A's declarations is the same as the impact of B's rewards on increasing B's declarations. In the experiment, increasing r_A raises A's declarations by 8.9 percentage points (46.0 percent in *Baseline Clientelism* minus 37.1 percent in *No Clientelism*), while increasing r_B raises B's declarations by 8.2 percentage points (38.7 percent in *Competitive Clientelism* minus 30.5 percent in *Baseline Clientelism*). As expected, the difference between the two treatment effects is statistically insignificant using a chi-square test ($p = 0.75$).

[36]In the treatments enabling this test, $q = \frac{1}{2}$.

Expressive Utility and Election Influence (H10 and H11)

Studies of voting behavior often point to expressive utility as a reason why citizens vote even if they are unlikely to influence the election outcome. We examine how these two factors — expressive utility and election influence — affect citizens' decisions to express political preferences beyond the ballot box in contexts with clientelism. For these two treatments, unlike for other factors we manipulate, theoretical analyses only provide unambiguous predictions about the aggregate distribution of actions if parameter values are specified.[37] Given the experiment's parameter values, the deterministic model predicts that as citizens obtain greater expressive utility from declaring in accordance with their preferences, A's declarations decrease, non-declarations decrease, and B's declarations increase. Experimental results comport with two of these predictions: although A's declarations are unchanged (46.0 percent in *Baseline Clientelism* versus 47.3 percent in *Expressive Utility*, $p = 0.52$), non-declarations decrease (23.5 percent versus 18.6 percent, $p = 0.01$), and B's declarations increase (30.5 percent versus 34.2 percent, $p = 0.05$).

While research suggests that many US survey respondents believe lawn signs can influence votes (Makse and Sokhey, 2014), what effects are expected in contexts where such forms of declaration have no influence whatsoever on the election outcome? Given the experiment's parameter values, the deterministic model also predicts that as declarations' electoral impact falls: declarations for A increase, non-declarations increase and declarations for B decrease. Contrary to these predictions, subjects' choices in the experiment were insensitive to the degree to which declarations influence outcomes. No significant change was observed in A's declarations (46.0 percent in *Baseline Clientelism* versus 45.8 percent in *No Election Influence*, $p = 0.94$), non-declarations (23.5 percent versus 24.7 percent, $p = 0.48$), or B's declarations (30.5 percent versus 29.5 percent, $p = 0.57$). While not dispositive, one interpretation of these surprising results is that voters ignore their declarations' potential impact on

[37] As discussed in Section 3.4, we focus on the deterministic model for these factors, given its homogeneous predictions across partisan types (unlike the stochastic model).

electoral outcomes, and instead focus on expressive utility and clientelist rewards. This interpretation would further validate our experimental design, which employs a decision-theoretic framework.[38] Moreover, this finding is relevant for future theoretical work on clientelism, as it suggests citizens may concentrate on their actions' direct consequences (e.g., receiving rewards) rather than indirect consequences (e.g., affecting the election).

Multivariate Regressions

Thus far, we have estimated treatment effects by comparing differences in proportions; such findings do not rely on parametric assumptions. To show robustness, we next conduct multivariate regressions that control for key variables and examine within-subject variation across treatments. This step involves pooling observations across treatments and adopting a basic parametric structure. More specifically, we employ logistic regressions and assume that declaration decisions are a function of each treatment as well as political preferences, survey round, and screener performance.[39] These covariates were described in Section 4. Recall that political preferences (x_i) about fictitious candidates were induced with payoffs. Survey round is included to control for the possibility that experience within the experiment affects declaration decisions. Screener performance, which refers to how many screener questions the subject answered correctly (0 to 2), controls for respondents' level of attentiveness. Some specifications include subject fixed effects to investigate within-subject variation across treatments. This step controls for any characteristics that do not vary across treatments for a given participant, such as age, education and gender.

Nearly all findings from differences of proportions are robust to using multivariate analyses, both with respect to the accuracy of predictions and statistical significance. Table 5 focuses on rewards; coefficients for each treatment indicate marginal effects and are shown in comparison to *Baseline Clientelism* (the excluded treatment category). The only discrepancy pertains the effect of *Lopsided Election* on non-declarations, which is negative across

[38]The Supplementary Information also includes a game-theoretic model.

[39]Findings are also robust when using multinomial logit.

Table 5: Estimates of Average Treatment Effects, Rewards (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.088**	-0.115**	0.032*	0.044*	0.056**	0.095**
	(0.017)	(0.020)	(0.015)	(0.020)	(0.015)	(0.023)
Lopsided Election	0.058**	0.076**	-0.032*	-0.045*	-0.025	-0.043*
	(0.016)	(0.020)	(0.015)	(0.020)	(0.013)	(0.022)
Cost	-0.058**	-0.077**	0.047**	0.066**	0.010	0.018
	(0.016)	(0.020)	(0.016)	(0.020)	(0.014)	(0.022)
Low Monitoring	-0.027	-0.035	0.016	0.022	0.011	0.019
	(0.017)	(0.020)	(0.015)	(0.020)	(0.014)	(0.022)
Competitive Clientelism	-0.046**	-0.061**	0.080**	0.112**	-0.034**	-0.058**
	(0.016)	(0.020)	(0.016)	(0.021)	(0.013)	(0.021)
Expressive Utility	0.013	0.017	0.037*	0.051*	-0.049**	-0.084**
	(0.016)	(0.020)	(0.015)	(0.021)	(0.013)	(0.021)
No Election Influence	-0.001	-0.001	-0.011	-0.015	0.012	0.020
	(0.016)	(0.020)	(0.015)	(0.020)	(0.013)	(0.022)
Round	0.007**	0.010**	-0.007**	-0.011**	0.000	0.001
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
Partisan Type	0.004**		-0.002**		-0.002**	
	(0.000)		(0.000)		(0.000)	
Screener	0.013		-0.025**		0.012	
	(0.010)		(0.009)		(0.009)	
Subject Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	10072	7656	10072	7192	10072	5888

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors are reported, clustered by subject in columns 1, 3, and 5.

Table 6: Estimates of Average Treatment Effects, Punishment Only (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.053**	0.152**	-0.027	-0.094**	-0.026	-0.098*
	(0.016)	(0.034)	(0.015)	(0.036)	(0.014)	(0.039)
Round	0.007*	0.013	-0.010**	-0.023**	0.003	0.007
	(0.003)	(0.008)	(0.003)	(0.008)	(0.003)	(0.009)
Partisan Type	0.002**		-0.002**		-0.000	
	(0.001)		(0.001)		(0.001)	
Screener	-0.009		-0.026*		0.034**	
	(0.013)		(0.013)		(0.012)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2518	862	2518	746	2518	648

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors are reported, clustered by subject in columns 1, 3 and 5.

Table 7: Estimates of Average Treatment Effects, Clientelism & Punishment (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.047*	0.133**	0.002	0.006	-0.049**	-0.198**
	(0.020)	(0.033)	(0.018)	(0.035)	(0.017)	(0.039)
Round	0.009**	0.036**	-0.009**	-0.039**	-0.001	-0.005
	(0.003)	(0.007)	(0.003)	(0.008)	(0.003)	(0.010)
Partisan Type	0.004**		-0.002**		-0.001**	
	(0.001)		(0.000)		(0.000)	
Screener	-0.005		-0.025*		0.029**	
	(0.011)		(0.011)		(0.010)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2518	896	2518	780	2518	628

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors are reported, clustered by subject in columns 1, 3 and 5.

all tests, but is only statistically significant in a logistic regression with subject fixed effects.[40] Turning to punishments, Tables 6 and 7 employ the identical methodology as Table 5, but compare to different base treatments to ensure that only one aspect of the decision environment is changed at a time. These multivariate regressions confirm all findings discussed above for *Clientelism and Punishment* (Table 7). For *Punishment Only*, multivariate regressions concord with findings above for the effect on A's declarations; effects on B's declarations and on non-declarations are negative across all tests but statistically significant only in logistic regressions with subject fixed effects (Table 6).[41]

6 Discussion

Overall, the present study emphasizes four key points. First, the choices of citizens play an important role in clientelism. While often depicted as passive actors who merely accept offers and follow instructions, many citizens undertake actions of their volition that influence contingent exchanges. Second, the scope of citizen choices in clientelism extends well beyond casting a ballot. Third, citizens often respond to clientelist incentives when deciding whether to declare support during campaigns. When voters can obtain post-election benefits by declaring support for victorious candidates, their decisions to express political support publicly often reflect more than just political preferences. And fourth, citizens will often make distinct declaration choices in different contexts, so their willingness to participate in clientelism should not be taken for granted.

Theoretical predictions are tested with an online experiment involving 1,259 participants across Brazil. Subjects responded as predicted in numerous treatments, involving shifts in

[40] As shown in Table 4, the p-value for a difference in proportions Z test is 0.063.

[41] The p-values for difference in proportions Z tests are 0.068 and 0.077, respectively (see Table 4). Regarding H8 and H9, which compare two treatment effects: (a) for H8, the difference between relevant coefficients has the predicted sign with and without subject fixed effects, but is significant only in the latter case; and (b) for H9, findings are robust to using multivariate analyses with and without subject fixed effects.

factors such as: (a) benefits from declaring for a victorious candidate, (b) the competitiveness of the election, (c) material or social costs of declaring, (d) whether multiple candidates engage in clientelism, and (e) the combined use of rewards and punishments. Experimental results are most consistent with predictions for clientelist candidate A, and especially with our stochastic model. With regards to B's declarations, all treatment effects have the correct sign but are not always significant. One reason is that several treatments only *directly* affect the expected utility from declaring for A. Nevertheless, the stochastic model also predicts weak *indirect* effects on B's declarations: because citizens may make suboptimal decisions, changing any action's expected utility influences the relative expected utility of all actions. With regards to non-declarations, although both models always predict changes in how many citizens remain undeclared — and hence, in the *aggregate* number of declarations — no such changes are observed in half of treatments. Some treatments may have failed to deter participants from declaring due to subjects' usual propensity to undertake rather than abstain from actions in laboratory and survey experiments (e.g., Levine and Palfrey, 2007). The experiment also yields other empirical patterns not predicted by theory: citizens are insensitive to whether their declarations can be easily monitored or can influence the election, and punishments fail to suppress declarations for the opposition. Overall, these results provide valuable insights about how and why clientelism influences political expression beyond the ballot box.

This study not only elaborates and tests the logic of declared support, but also lays the groundwork for further investigation into the role of citizens in clientelism. On the theoretical side, future work should complement our analysis of citizens' declarations by investigating the endogenous provision of rewards by politicians within a *general equilibrium* theory of clientelism, involving both citizens and politicians who make strategic choices as a mutual best response. Our study provides numerous insights for further exploration in such models. For instance, both theoretically and empirically, we find that rewards affect declared support relatively more than punishments do. This finding has important implications for politicians'

clientelistic strategies, and may well provide one reason why rewards are more prevalent than punishments in various countries. Another result warrants investigation in future studies: although theoretically we expect both to inform citizens' choices, experimental subjects were responsive to declarations' direct effects but not to indirect effects on electoral outcomes.

On the empirical side, further experimental and observational work should investigate the external validity of our findings. Our experiment may have been more cognitively complex than citizens' usual decisions regarding clientelism and elections, so robustness should be examined with less-educated subjects.[42] Another task is to test the dynamics of declared support if politicians make spot-market payments for declarations; though rare in Brazil, this modality may be observed in some contexts including Argentina (Auyero, 2000; Nichter, 2018). For instance, future work should test our model's finding that such campaign handouts would have no effect on its predictions, even though they would influence the *level* of declarations. Likewise, our predictions should be tested in countries with stronger political parties, as partisan operatives beyond Brazil may engage in far more coordinated efforts to encourage and monitor declared support, as well as to distribute clientelist rewards.

Future research should examine patterns of declared support in various countries, for both local and higher-level politicians. As discussed, our fieldwork and recent observational work suggest a relationship between declarations and clientelism in Brazil, and more limited evidence from Argentina, Ghana, Lebanon and Mexico reveals similar patterns. Such data would facilitate rigorous testing of our predictions employing data from actual instead of fictitious elections. This point notwithstanding, we observe a link between participants' preferences and behavior in real-world elections and decisions in our experiment. For example, citizens who reported being partisans or displaying campaign paraphernalia during Brazil's 2016 municipal elections were significantly more likely to declare support for the experiment's fictitious candidates — even though these factors had no impact on their experimental earn-

[42]A lab-in-the-field study could employ more detailed instructions and reduce attrition.

ings.[43] Another important direction for future research is exploring the various modalities by which citizens can shape clientelist exchanges, and examining the conditions under which citizens are more or less motivated to undertake such actions. After all, declared support is an important phenomenon but is by no means the only action that citizens can take to influence their receipt of contingent benefits. These topics also warrant close attention in contexts where citizens face substantial constraints to autonomous decision making, such as under some authoritarian regimes.

The link between declared support and collective behavior also deserves further theoretical and empirical investigation. As discussed above, the Supplementary Information shows that most predictions hold with a game-theoretic model in which a citizen's action influences other citizens' beliefs about candidates' probability of electoral victory. It would be fruitful to extend this model to explore how declarations may involve a "tipping point" dynamic, similar to those examined in studies of anti-regime mobilization and protest behavior (e.g., Kuran, 1991; Lohmann, 1994). For example, in an environment with asymmetric information and dispersed knowledge about the candidates' competence or ideology, the sequential decision of whether to declare support might trigger an information cascade in which early movers have a disproportionate impact on late movers' political behavior. Given that collective benefits in some contexts are distributed in contingent exchange for political support, it also warrants investigation whether declared support affects the distribution of club goods. While Nichter (2018) finds that Brazilians who declare support are no more or less likely to receive club goods, this relationship is important to explore in wider contexts.

Overall, a broader analytical lens that considers the choices of citizens — and not just those of politicians — holds substantial promise to deepen our understanding of contingent exchanges. Given the various consequences of clientelism for both democracy and development, improving our knowledge about this phenomenon would be a significant contribution.

[43]These findings are significant at the 5 percent level in a regression with various controls and state/partisan type fixed effects.

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Supplementary Information for
“Declared Support and Clientelism”

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A Comparative Statics for Deterministic Model

As discussed in Section 3.3, to determine the marginal effect of each variable on the share of citizens who declare for B (A), we determine the sign of the partial derivative of cutpoint $x_B^\star$ ($x_A^\star$) with respect to that variable. To determine the marginal effect of each variable on the fraction of citizens who remain undeclared, we consider the partial derivatives of $(x_B^\star - x_B^\star)$.

$$x_B^\star = \frac{-c_B - (q-\alpha)\gamma p_A + (1-q+\alpha)\gamma r_B}{\alpha+\delta} \qquad x_A^\star = \frac{c_A + (1-q-\alpha)\gamma p_B - (q+\alpha)\gamma r_A}{\alpha+\delta}$$

$$x_A^\star - x_B^\star = \frac{c_A + c_B + \gamma q(r_B + p_A - r_A - p_B) - \gamma\alpha(p_A + p_B + r_A + r_B) + \gamma(p_B - r_B)}{\alpha+\delta}$$

H1 *A's Reward Size*: $x_B^\star$ does not depend on r_A. The derivative of $x_A^\star$ with respect to r_A is $\frac{\partial x_A^\star}{\partial r_A} = -\frac{\gamma(\alpha+q)}{\alpha+\delta}$ which is always strictly negative. Regarding non-declarations, increasing r_A strictly decreases the numerator of $(x_A^\star - x_B^\star)$ and does not affect its denominator.

H2 *A's Support*: The derivative of $x_A^\star$ with respect to q is $\frac{\partial x_A^\star}{\partial q} = -\frac{\gamma(r_A+p_B)}{\alpha+\delta}$ which is always strictly negative. The derivative of $x_B^\star$ with respect to q is $\frac{\partial x_B^\star}{\partial q} = -\frac{\gamma(p_A+r_B)}{\alpha+\delta}$ which is always weakly negative. The derivative of $(x_A^\star - x_B^\star)$ with respect to q is $\frac{\partial (x_A^\star - x_B^\star)}{\partial q} = \frac{\gamma(r_B+p_A-r_A-p_B)}{\alpha+\delta}$ which is positive if $r_B + p_A > r_A + p_B$, 0 if $r_B + p_A = r_A + p_B$, and negative if $r_B + p_A < r_A + p_B$.

H3 *Cost of Declaring for A*: $x_B^\star$ does not depend on c_A. The derivative of $x_A^\star$ with respect to c_A is $\frac{\partial x_A^\star}{\partial c_A} = \frac{1}{\alpha+\delta}$ which is always strictly positive. Regarding non-declarations, increasing c_A strictly increases the numerator of $(x_A^\star - x_B^\star)$ and does not affect its denominator.

H4 *Monitoring*: The derivative of $x_A^\star$ with respect to γ is $\frac{\partial x_A^\star}{\partial \gamma} = \frac{(1-q-\alpha)p_B - (q+\alpha)r_A}{\alpha+\delta}$ which is positive if $(1-q-\alpha)p_B > (q+\alpha)r_A$, 0 if $(1-q-\alpha)p_B = (q+\alpha)r_A$ and negative if $(1-q-\alpha)p_B < (q+\alpha)r_A$. The derivative of $x_B^\star$ with respect to γ is $\frac{\partial x_B^\star}{\partial \gamma} = \frac{-(q-\alpha)p_A + (1-q+\alpha)r_B}{\alpha+\delta}$ which is positive if $(1-q+\alpha)r_B > (q-\alpha)p_A$, 0 if $(1-q+\alpha)r_B = (q-\alpha)p_A$ and negative otherwise. The derivative of $(x_A^\star - x_B^\star)$ with respect to γ is $\frac{\partial (x_A^\star - x_B^\star)}{\partial \gamma} = \frac{q(r_B+p_A-r_A-p_B) - \alpha(p_A+p_B+r_A+r_B) + p_B - r_B}{\alpha+\delta}$ which is positive if $(q-\alpha)p_A + (1-q-\alpha)p_B > (q+\alpha)r_A + (1-q+\alpha)r_B$, 0 if $(q-\alpha)p_A + (1-q-\alpha)p_B = (q+\alpha)r_A + (1-q+\alpha)r_B$ and negative if $(q-\alpha)p_A + (1-q-\alpha)p_B < (q+\alpha)r_A + (1-q+\alpha)r_B$.

H5 *A's Punishment Size*: $x_A^\star$ does not depend on p_A. The derivative of $x_B^\star$ with respect to p_A is $\frac{\partial x_B^\star}{\partial p_A} = \frac{(q-\alpha)\gamma}{\alpha+\delta}$ which is always strictly positive. The derivative of $x_A^\star - x_B^\star$ with respect to p_A is $\frac{\partial x_A^\star - x_B^\star}{\partial p_A} = -\frac{(q-\alpha)\gamma}{\alpha+\delta}$ which is always strictly negative.

H6 *B's Reward Size*: $x_A^\star$ does not depend on r_B. The derivative of $x_B^\star$ with respect to r_B is $\frac{\partial x_B^\star}{\partial r_B} = \frac{(1-q+\alpha)\gamma}{\alpha+\delta}$ which is always strictly positive. The derivative of $x_A^\star - x_B^\star$ with respect to r_B is $\frac{\partial x_A^\star - x_B^\star}{\partial r_B} = -\frac{(1-q+\alpha)\gamma}{\alpha+\delta}$ which is always strictly negative.

H7 *B's Punishment Size*: $x_B^\star$ does not depend on p_B. The derivative of $x_A^\star$ with respect to p_B is $\frac{\partial x_A^\star}{\partial p_B} = \frac{(1-q-\alpha)\gamma}{\alpha+\delta}$ which is always strictly positive. The derivative of $x_A^\star - x_B^\star$ with respect to p_B is $\frac{\partial x_A^\star - x_B^\star}{\partial p_B} = \frac{(1-q-\alpha)\gamma}{\alpha+\delta}$ which is always strictly positive.

H8 *Relative Impact of Rewards vs. Punishments*: $\left|\frac{\partial x_A^\star}{\partial r_A}\right| - \left|\frac{\partial x_B^\star}{\partial p_A}\right| = \frac{\gamma(\alpha+q)}{\alpha+\delta} - \frac{\gamma(q-\alpha)}{\alpha+\delta} = \frac{\gamma(2\alpha)}{\alpha+\delta}$, which is weakly greater than 0 for any $\alpha \geq 0$ (and strictly for any $\alpha > 0$). Similarly, $\left|\frac{\partial x_B^\star}{\partial r_B}\right| - \left|\frac{\partial x_A^\star}{\partial p_B}\right| = \frac{(1-q+\alpha)\gamma}{\alpha+\delta} - \frac{(1-q-\alpha)\gamma}{\alpha+\delta} = \frac{\gamma(2\alpha)}{\alpha+\delta}$, which is weakly greater than 0 for any $\alpha \geq 0$ (and strictly for any $\alpha > 0$).

H9 *Relative Impact of Rewards by A vs. Rewards by B*: $\left|\frac{\partial x_A^\star}{\partial r_A}\right| - \left|\frac{\partial x_B^\star}{\partial r_B}\right| = \frac{\gamma(\alpha+q)}{\alpha+\delta} - \frac{\gamma(1-q+\alpha)}{\alpha+\delta} = \frac{\gamma(2q-1)}{\alpha+\delta}$, which is greater than 0 if and only if $q > \frac{1}{2}$ and equal to 0 if and only if $q = \frac{1}{2}$.

H10 *Expressive Utility*: The derivative of $x_A^\star$ with respect to δ is $\frac{\partial x_A^\star}{\partial \delta} = -\frac{x_A^\star}{(\alpha+\delta)}$ which is positive if $x_A^\star < 0$, 0 if $x_A^\star = 0$ and negative if $x_A^\star > 0$. The derivative of $x_B^\star$ with respect to δ is $\frac{\partial x_B^\star}{\partial \delta} = -\frac{x_B^\star}{(\alpha+\delta)}$ which is positive if $x_B^\star < 0$, 0 if $x_B^\star = 0$ and negative if $x_B^\star > 0$. Regarding non-declarations, increasing δ strictly increases the denominator of $(x_A^\star - x_B^\star)$ and does not affect its numerator, which is always positive since, by assumption, $c_A + c_B > \gamma(q+\alpha)r_A$.

H11 *Election Influence*: The derivative of $x_A^\star$ with respect to α is $\frac{\partial x_A^\star}{\partial \alpha} = -\frac{\gamma(r_A+p_B)+x_A^\star}{\alpha+\delta}$ which is positive if $x_A^\star < -\gamma(r_A+p_B)$, 0 if $x_A^\star = -\gamma(r_A+p_B)$, and negative if $x_A^\star > -\gamma(r_A+p_B)$. The derivative of $x_B^\star$ with respect to α is $\frac{\partial x_B^\star}{\partial \alpha} = \frac{\gamma(p_A+r_B)-x_B^\star}{(\alpha+\delta)}$ which is positive if $x_B^\star < \gamma(p_A+r_B)$, 0 if $x_B^\star = \gamma(p_A+r_B)$ and negative if $x_B^\star > \gamma(p_A+r_B)$. The derivative of $(x_A^\star - x_B^\star)$ with respect to α is $\frac{\partial(x_A^\star - x_B^\star)}{\partial \alpha} = -\frac{\gamma(r_A+p_B+p_A+r_B)+(x_A^\star - x_B^\star)}{\alpha+\delta}$ which is positive if $(x_A^\star - x_B^\star) < -\gamma(r_A+p_B+p_A+r_B)$, zero if $(x_A^\star - x_B^\star) = -\gamma(r_A+p_B+p_A+r_B)$, and negative if $(x_A^\star - x_B^\star) > -\gamma(r_A+p_B+p_A+r_B)$.

B Comparative Statics for Stochastic Choice Model

As described in Section 3.4, we assume that citizens choose according to a Logit stochastic choice rule. The probability that citizen i chooses declaration action $j = \{A, B, \emptyset\}$ is:

$$\pi_j = \frac{exp(\lambda U_j)}{exp(\lambda U_A) + exp(\lambda U_B) + exp(\lambda U_\emptyset)} \tag{1}$$

where, using a compact notation, $U_A = EU_i(A)$ as in equation (1) in the paper, $U_B = EU_i(B)$ as in equation (2) in the paper, $U_\emptyset = EU_i(\emptyset)$ as in equation (3) in the paper and $\lambda \in [0, \infty)$ measures responsiveness to expected payoffs. The partial derivative of π_j with respect to parameter y is:

$$\frac{\partial \pi_j}{\partial y} = \frac{\lambda exp(\lambda U_j)\frac{\partial U_j}{\partial y}\left[\sum_{i \neq j} exp(\lambda U_i)\right] - exp(\lambda U_j)\left[\sum_{i \neq j} \lambda exp(\lambda U_i)\frac{\partial U_i}{\partial y}\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2} \tag{2}$$

The denominator of (2) is always positive. Thus, $\frac{\partial \pi_j}{\partial y}$ is positive if and only if the numerator is positive. The text below refers to "Case 1" as one case in which it is easy to determine the sign of $\frac{\partial \pi_j}{\partial y}$: when parameter y only affects the expected utility from action j—that is, $\frac{\partial U_j}{\partial y} \neq 0$

for action j and $\frac{\partial U_i}{\partial y} = 0$ for both actions $i \neq j$. In this case, $\frac{\partial \pi_j}{\partial y} > 0$ if and only if $\frac{\partial U_j}{\partial y} > 0$ and $\frac{\partial \pi_{i \neq j}}{\partial y} > 0$ if and only if $\frac{\partial U_j}{\partial y} < 0$.

H1 *A's Reward Size*: As A provides larger rewards, declarations for A increase, declarations for B decrease, and non-declarations decrease.

The derivative of the expected utility from each action with respect to r_A is:

$$\frac{\partial U_A}{\partial r_A} = (q+\alpha)\gamma > 0 \qquad \frac{\partial U_B}{\partial r_A} = 0 \qquad \frac{\partial U_\emptyset}{\partial r_A} = 0$$

Marginally increasing r_A increases U_A for all citizens but does not affect U_B or $U_\emptyset$. Thus, we fall in "Case 1," and we have $\frac{\partial \pi_A}{\partial r_A} > 0$, $\frac{\partial \pi_B}{\partial r_A} < 0$, and $\frac{\partial \pi_\emptyset}{\partial r_A} < 0$.

H2 *A's Support*: Consider $r_B = p_A = p_B = 0$. As A's probability of winning increases, declarations for A increase, declarations for B decrease, and non-declarations decrease.

The derivative of the expected utility from each action with respect to q is:

$$\frac{\partial U_A}{\partial q} = x_i + \gamma r_A \qquad \frac{\partial U_B}{\partial q} = x_i \qquad \frac{\partial U_\emptyset}{\partial q} = x_i$$

Marginally increasing q increases U_A by $x_i + \gamma r_A$, U_B by x_i and $EU_\emptyset$ by x_i. We have:

$$\frac{\partial \pi_A}{\partial q} = \frac{\lambda exp(\lambda U_A)(\gamma r_A)\left[\sum_{i \neq A} exp(\lambda U_i)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2} \qquad \frac{\partial \pi_B}{\partial q} = \frac{-exp(\lambda U_B)\lambda exp(\lambda U_A)\gamma r_A}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2} \qquad \frac{\partial \pi_\emptyset}{\partial q} = 0\frac{-exp(\lambda U_\emptyset)\lambda exp(\lambda U_A)\gamma r_A}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

$\frac{\partial \pi_A}{\partial q}$ is always positive; $\frac{\partial \pi_B}{\partial q}$ is always negative; and $\frac{\partial \pi_\emptyset}{\partial q}$ is always negative.

H3 *Cost of Declaring for A*: As the cost of declaring for candidate A increases, declarations for A decrease, declarations for B increase, and non-declarations increase.

The derivative of the expected utility from each action with respect to c_A is:

$$\frac{\partial U_A}{\partial c_A} = -1 < 0 \qquad \frac{\partial U_B}{\partial c_A} = 0 \qquad \frac{\partial U_\emptyset}{\partial c_A} = 0$$

Marginally increasing c_A decreases U_A for all citizens but does not affect U_B or $U_\emptyset$. Thus, we fall in "Case 1," and we have $\frac{\partial \pi_A}{\partial c_A} < 0$, $\frac{\partial \pi_B}{\partial c_A} > 0$, and $\frac{\partial \pi_\emptyset}{\partial r_A} > 0$.

H4 *Monitoring*: Consider $r_B = p_A = p_B = 0$. As A's monitoring ability increases, declarations for A increase, declarations for B decrease, and non-declarations decrease.

The derivative of the expected utility from each action with respect to γ is:

$$\frac{\partial U_A}{\partial \gamma} = (q+\alpha)r_A > 0 \qquad \frac{\partial U_B}{\partial \gamma} = 0 \qquad \frac{\partial U_\emptyset}{\partial \gamma} = 0$$

Marginally increasing γ increases $EU_i(A)$, and does not affect $EU_i(B)$ and $EU_i(\emptyset)$. Thus, we fall in "Case 1," and we have $\frac{\partial \pi_A}{\partial \gamma} > 0$, $\frac{\partial \pi_B}{\partial \gamma} < 0$, and $\frac{\partial \pi_\emptyset}{\partial \gamma} < 0$.

H5 *A's Punishment Size*: As candidate A imposes greater punishments, declarations for A increase, declarations for B decrease, and non-declarations increase.

The partial derivatives of the expected utility from each action with respect to p_A are:

$$\frac{\partial U_A}{\partial p_A} = 0 \qquad \frac{\partial U_B}{\partial p_A} = -(q-\alpha)\gamma < 0 \qquad \frac{\partial U_\emptyset}{\partial p_A} = 0$$

Thus, we fall in "Case 1," and have $\frac{\partial \pi_A}{\partial p_A} > 0$, $\frac{\partial \pi_B}{\partial p_A} < 0$, and $\frac{\partial \pi_\emptyset}{\partial p_A} > 0$.

H6 *B's Reward Size*: As candidate B provides larger rewards, declarations for A decrease, declarations for B increase, and non-declarations decrease.

The partial derivatives of the expected utility from each action with respect to r_B are:

$$\frac{\partial U_A}{\partial r_B} = 0 \qquad \frac{\partial U_B}{\partial r_B} = (1-q+\alpha)\gamma > 0 \qquad \frac{\partial U_\emptyset}{\partial r_B} = 0$$

We fall in "Case 1," and have $\frac{\partial \pi_A}{\partial r_B} < 0$, $\frac{\partial \pi_B}{\partial r_B} > 0$, and $\frac{\partial \pi_\emptyset}{\partial r_B} < 0$.

H7 *B's Punishment Size*: As candidate B imposes greater punishments, declarations for B increase, declarations for A decrease, and non-declarations increase.

The partial derivatives of the expected utility from each action with respect to p_B are:

$$\frac{\partial U_A}{\partial p_B} = -(1-q-\alpha)\gamma \qquad \frac{\partial U_B}{\partial p_B} = 0 \qquad \frac{\partial U_\emptyset}{\partial p_B} = 0$$

Thus, we fall in "Case 1," and have $\frac{\partial \pi_A}{\partial p_B} < 0$, $\frac{\partial \pi_B}{\partial p_B} > 0$, and $\frac{\partial \pi_\emptyset}{\partial p_B} > 0$.

H8 *Relative Impact of Rewards vs. Punishments*: Consider $r_A = p_A = r$, $c_A = c_B = c$, $\delta = 0$, $q \geq 1/2$. Among A's supporters, neutral citizens and weak B's supporters (that is, for $x_i > -\frac{q\gamma r}{\alpha}$), the marginal effect of r_A on increasing declarations for A is strictly larger than the marginal effect of p_A on decreasing declarations for B.

$$\left|\frac{\partial \pi_A}{\partial r_A}\right| = \frac{\lambda exp(\lambda U_A)(q+\alpha)\gamma\left[\sum_{i\neq A} exp(\lambda U_i)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2} > \left|\frac{\partial \pi_B}{\partial p_A}\right| = \frac{\lambda exp(\lambda U_B)(q-\alpha)\gamma\left[\sum_{i\neq j} exp(\lambda U_i)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

Rearranging we get:

$$\frac{exp[\lambda(2\alpha\gamma r + 2qx_i - 2c)] + exp[\lambda((\gamma r + x_i)\alpha + \gamma q r + 2qx_i - c)]}{exp[\lambda(2\alpha\gamma r + 2qx_i - 2c)] + exp[\lambda((\gamma r - x_i)\alpha - \gamma q r + 2qx_i - c)]} \quad > \quad \frac{q-\alpha}{q+\alpha}$$

Since $q \geq 1/2$, the RHS is smaller than or equal to 1. If $\alpha = 0$, the LHS is greater than 1 and the inequality holds for any x_i. If $\alpha > 0$ and $x_i > -\frac{\gamma q r}{\alpha}$, the LHS is > 1.

H9 *Relative Impact of Rewards across Candidates*: Consider $r_A = r_B = r$, $c_A = c_B = c$, $\delta = 0$, $q \geq 1/2$. Among A's supporters, neutral citizens and weak B's supporters (that is, for $x_i > -\gamma r$), the marginal effect of r_A on increasing declarations for A is strictly larger than the marginal effect of r_B on increasing declarations for B.

$$\frac{\partial \pi_A}{\partial r_A} = \frac{\lambda exp(\lambda U_A)(q+\alpha)\gamma\left[\sum_{i\neq A} exp(\lambda U_i)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2} \quad > \quad \frac{\partial \pi_B}{\partial r_B} = \frac{\lambda exp(\lambda U_B)(1-q+\alpha)\gamma\left[\sum_{i\neq j} exp(\lambda U_i)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

Rearranging we get:

$$\frac{exp[\lambda(2q\gamma r + 2qx_i - 2c)] + exp[\lambda(q\gamma r + 2qx_i - c + \alpha(\gamma r + x_i)]}{exp[\lambda(2q\gamma r + 2qx_i - 2c)] + exp[\lambda(q\gamma r + 2qx_i - c - \alpha(\gamma r + x_i)]} \quad > \quad \frac{1-q+\alpha}{q+\alpha}$$

Since $q \geq 1/2$, the RHS is smaller than or equal to 1. If $\alpha = 0$, the LHS is equal to 1 and the inequality holds for any x_i. If $\alpha > 0$ and $x_i > -\gamma r$, the LHS is strictly greater than 1.

H10 *Expressive Utility*: As the utility of declaring in accordance with preferences increases, declarations for A increase among A's supporters, but decrease among B's supporters. Declarations for B increase among B's supporters, but decrease among A's supporters. Declarations by indifferent citizens are unaffected. The aggregate effect is ambiguous.

The derivative of the expected utility from each action with respect to δ is:

$$\frac{\partial U_A}{\partial \delta} = x_i \qquad \frac{\partial U_B}{\partial \delta} = -x_i \qquad \frac{\partial U_\emptyset}{\partial \delta} = 0$$

Marginally increasing δ increases the expected utility any citizen derives from supporting her favorite candidate, decreases the expected utility she derives from supporting the other candidate, and does not affect the expected utility from remaining undeclared. We have:

$$\frac{\partial \pi_A}{\partial \delta} \quad = \quad \frac{\lambda exp(\lambda U_A) x_i \left[\sum_{i\neq j} exp(\lambda U_i)\right] + exp(\lambda U_A)\left[\lambda exp(\lambda U_B) x_i\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

which is positive if $x_i > 0$, equal to 0 if $x_i = 0$, and negative if $x_i < 0$.

$$\frac{\partial \pi_B}{\partial \delta} \quad = \quad \frac{-\lambda exp(\lambda U_B) x_i \left[\sum_{i\neq j} exp(\lambda U_i)\right] - exp(\lambda U_B)\left[\lambda exp(\lambda U_A) x_i\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

which is positive if $x_i < 0$, equal to 0 if $x_i = 0$, and negative if $x_i > 0$.

$$\frac{\partial \pi_\emptyset}{\partial \delta} \quad = \quad \frac{-exp(\lambda U_\emptyset)\lambda x_i \left[exp(\lambda U_A) - exp(\lambda U_B)\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

Since $f(x) = exp(\lambda x)$ is strictly increasing in x, $exp(\lambda U_A) - exp(\lambda U_B)$ is positive if and only if $U_A - U_B$ is positive. Thus, $\frac{\partial \pi_\emptyset}{\partial \delta}$ is positive if and only if $x_i(U_A - U_B)$ is positive.

H11 *Election Influence*: Consider $r_B = p_A = p_B = 0$. As the election influence of declaring increases: declarations for A increase and declarations for B decrease among A's supporters and sufficiently weak B's supporters (that is, if $x_i > -\frac{\gamma r_A}{2}$); declarations for A decrease and declarations for B increase among sufficiently strong B's supporters (that is, if $x_i < -\gamma r_A$). The aggregate effect is ambiguous.

The derivative of the expected utility from each action with respect to α is:

$$\frac{\partial U_A}{\partial \alpha} = x_i + \gamma r_A \qquad \frac{\partial U_B}{\partial \alpha} = -x_i \qquad \frac{\partial U_\emptyset}{\partial \alpha} = 0$$

We have:

$$\frac{\partial \pi_A}{\partial \alpha} = \frac{\lambda exp(\lambda U_A) exp(\lambda U_B)(2x_i + \gamma r_A) + \lambda exp(\lambda U_A) exp(\lambda U_\emptyset)(x_i + \gamma r_A)}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

A sufficient condition for this to be positive is $x_i > -\frac{\gamma r_A}{2}$ and a sufficient condition for this to be negative is $x_i < -\gamma r_A$. For $x_i \in [-\gamma r_A, -\frac{\gamma r_A}{2}]$, the sign depends on other parameters.

$$\frac{\partial \pi_B}{\partial \alpha} = -\frac{\lambda exp(\lambda U_B) exp(\lambda U_A)(2x_i + \gamma r_A) + \lambda exp(\lambda U_B) exp(\lambda U_\emptyset) x_i}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

A sufficient condition for this to be negative is $x_i > -\frac{\gamma r_A}{2}$ and a sufficient condition for this to be positive is $x_i < -\gamma r_A$. For $x_i \in [-\gamma r_A, -\frac{\gamma r_A}{2}]$, the sign depends on other parameters.

$$\frac{\partial \pi_\emptyset}{\partial \alpha} = \frac{-exp(\lambda U_\emptyset)\left[\lambda exp(\lambda U_A)(x_i + \gamma r_A) - \lambda exp(\lambda U_B) x_i\right]}{\left[\sum_{i=\{A,B,\emptyset\}} exp(\lambda U_i)\right]^2}$$

A sufficient condition for this to be negative is $x_i \in (-\gamma r_A, 0)$. For other values of x_i, the sign depends on other parameters.

C Strategic Model of Declared Support

To clarify the logic by which declarations can affect other citizens' vote intentions and, thus, electoral outcomes, we analyze the stylized case in which there are two voters, V_1, V_2. Ideological preferences are a voter's private information but their distribution is common knowledge: x_1, x_2 are IID draws from $f \sim U[-k,k]$.[1] Voters derive "joy of winning" if they vote for the election winner, $R > 0$. This is the timing of the game:

1. V_1 decides whether to declare support for A (at cost $c_A > 0$), declare support for B (at cost $c_B > 0$) or remain undeclared.

2. V_2 observes V_1's decision.

3. On election day, V_2 decides whether to vote for A, vote for B, or to abstain. If he votes, V_2 incurs cost $c_2 > 0$. V_1 votes according to his declaration.

4. The election winner is determined as a function of the citizens' votes. We assume that the probability a candidate wins is increasing in the absolute amount of votes received by V_1 and V_2. This is meant to capture the fact that, while we model the strategic interaction between a subset of voters (e.g., two neighbors who can monitor each other's declarations), the electorate is potentially larger. In particular, we make the following assumptions:

    - If A receives 2 votes more than B, A wins with probability 1.
    - If A receives 1 vote more than B, A wins with probability $q \in (1/2, 3/4)$.
    - If A and B receive the same number of votes, A wins with probability $1/2$.
    - If A receives 1 vote less than B, A wins with probability $(1-q) \in (1/4, 1/2)$.

[1] As long as k is sufficiently large, all probabilities presented below are between 0 and 1.

- If A receives 2 votes less than B, A wins with probability 0.

5. If A wins and V_1 is observed to declare support for A, A distributes rewards r_A to V_1; if A wins and V_1 is observed to declare support for B, A doles out punishment p_A to V_1; if B wins and V_1 is observed to declare support for B, B distributes rewards r_B to V_1.

Since this is a sequential game, we solve it with backward induction.

Stage 2: V_2's Voting Decision

CASE 1: V_1 did not declare support for either candidate in Stage 1

The expected utility that V_2 derives from the three actions are:

$$EU_2(A) = q(x_2+R) - c_2 \qquad EU_2(B) = qR + (1-q)x_2 - c_2 \qquad EU_2(\emptyset) = 0.5x_2$$

V_2 prefers to vote for A rather than abstaining if and only if $x_2 > x_{2A}^1 = \frac{c_2 - qR}{q-0.5}$.

V_2 prefers to vote for B rather than abstaining if and only if $x_2 < x_{2B}^1 = -\frac{c_2 - qR}{q-0.5}$.

We assume $c_2 > qR$ so that have $x_{2A}^1 > x_{2B}^1$ and, thus, some abstention: V_2 votes for A if $x_2 \geq x_{2A}^1$, votes for B if $x_2 \leq x_{2B}^1$ and abstains if $x_2 \in (x_{2A}^1, x_{2B}^1)$.

From the perspective of V_1 — after he decides to remain undeclared but before the election — the probability that V_2 votes for A is equal to the probability that x_2 is greater than x_{2A}^1; the probability that V_2 votes for B is equal to the probability that x_2 is lower than x_{2B}^1; and the probability that V_2 abstains is equal to the probability that x_2 is between x_{2B}^1 and x_{2A}^1. Since x_2 is distributed uniformly between $-k$ and k, we have:

$$\begin{aligned}
Pr[V_2 \text{ votes for A} | V_1 \text{ abstains}] &= 1 - F(x_{2A}^1) = \frac{k(2q-1) + 2qR - 2c_2}{(4q-2)k} \\
Pr[V_2 \text{ votes for B} | V_1 \text{ abstains}] &= F(x_{2B}^1) = \frac{k(2q-1) + 2qR - 2c_2}{(4q-2)k} \\
Pr[V_2 \text{ abstains} | V_1 \text{ abstains}] &= F(x_{2A}^1) - F(x_{2B}^1) = \frac{-2Rq + 2c_2}{(2q-1)k}
\end{aligned}$$

CASE 2: V_1 declared support for A in Stage 1

The expected utility that V_2 derives from the three actions are:

$$EU2(A) = (x_2+R) - c_2 \qquad EU2(B) = \frac{R}{2} + \frac{x_2}{2} - c_2 \qquad EU2(\emptyset) = qx_2$$

V_2 prefers to vote for A rather than abstaining if and only if $x_2 > x_{2A}^2 = \frac{c_2 - R}{1-q}$.

V_2 prefers to vote for B rather than abstaining if and only if $x_2 < x_{2B}^2 = -\frac{c_2 - R}{1-q}$.

We compute the distribution of V_2's actions from V_1's perspective as above and we get:

$$\begin{aligned}
Pr[V_2 \text{ votes for A} | V_1 \text{ declares for } A] &= 1 - F(x_{2A}^2) = \frac{k(q-1) - R + c_2}{2(q-1)k} \\
Pr[V_2 \text{ votes for B} | V_1 \text{ declares for } A] &= F(x_{2B}^2) = \frac{-2c_2 + R + k(2q-1)}{(4q-2)k} \\
Pr[V_2 \text{ abstains} | V_1 \text{ declares for } A] &= F(x_{2A}^2) - F(x_{2B}^2) = \frac{Rq - c_2}{4kq^2 - 6kq + 2k}
\end{aligned}$$

CASE 3: V_1 declared support for B in Stage 1

The expected utility that V_2 derives from the three actions are:

$$EU2(A) = \frac{x_2 + R}{2} - c_2 \qquad EU2(B) = R - c_2 \qquad EU2(\emptyset) = (1-q)x_2$$

V_2 prefers to vote for A rather than abstaining if and only if $x_2 > x_{2A}^3 = \frac{c_2 - 0.5R}{q - 0.5}$.

V_2 prefers to vote for B rather than abstaining if and only if $x_2 < x_{2B}^3 = -\frac{c_2 - R}{1-q}$.

We compute the distribution of V_2's actions from V_1's perspective as above and we get:

$$\begin{aligned}
Pr[V_2 \text{ votes for A}|V_1 \text{ declares for } B] &= 1 - F(x_{2A}^3) = \frac{-2c_2 + R + k(2q-1)}{(4q-2)k} \\
Pr[V_2 \text{ votes for B}|V_1 \text{ declares for } B] &= F(x_{2B}^3) = \frac{k(q-1) - R + c_2}{2(q-1)k} \\
Pr[V_2 \text{ abstains}|V_1 \text{ declares for } B] &= F(x_{2A}^3) - F(x_{2B}^3) = \frac{Rq - c_2}{4kq^2 - 6kq + 2k}
\end{aligned}$$

Summing up the results from Stage 2, we have:

$$\begin{aligned}
Pr[V_2 \text{ votes for A}|V_1 \text{ undeclared}] = Pr[V_2 \text{ votes for B}|V_1 \text{ undeclared}] &= \frac{k(2q-1) + 2qR - 2c_2}{(4q-2)k} \\
Pr[V_2 \text{ abstains}|V_1 \text{ undeclared}] &= \frac{-2Rq + 2c_2}{(2q-1)k} \\
Pr[V_2 \text{ votes for A}|V_1 \text{ declared for A}] = Pr[V_2 \text{ votes for B}|V_1 \text{ declared for } B] &= \frac{k(q-1) - R + c_2}{2(q-1)k} \\
Pr[V_2 \text{ votes for B}|V_1 \text{ declared for A}] = Pr[V_2 \text{ votes for A}|V_1 \text{ declared for B}] &= \frac{-2c_2 + R + k(2q-1)}{(4q-2)k} \\
Pr[V_2 \text{ abstains}|V_1 \text{ declared for A or B}] &= \frac{Rq - c_2}{4kq^2 - 6kq + 2k}
\end{aligned}$$

It is evident that $Pr[V_2 \text{ votes for A}|V_1 \text{ declared for A}] > Pr[V_2 \text{ votes for A}|V_1 \text{ undeclared}]$ and that $Pr[V_2 \text{ votes for A}|V_1 \text{ undeclared}] > Pr[V_2 \text{ votes for A}|V_1 \text{ declared for B}]$.

Stage 1: V_1's Declaration Decision

From the perspective of V_1: the probability that A wins the election if he does not declare support for either candidate is $\frac{1}{2}$; the probability that A wins the election if he declares support for A is $\frac{R+3k}{4k}$; the probability that A wins the election if he declares support for B is $\frac{-R+k}{4k}$. Since $R > 0 > -k$, we have that Pr[A wins if V_1 declares for A] > Pr [A wins if V_1 undeclared] = $\frac{1}{2}$ > Pr [A wins if V_1 declares for B].[2] Consider now V_1's decision.

V_1 prefers to declare support for A rather than remaining undeclared if and only if:

$$\begin{aligned}
EU(A) &> EU(\emptyset) \\
x_1 &> x_{1A}^\star = \frac{k(-3\gamma r_A - 3R + 4c_A) - R\gamma r_A - R^2}{(4\delta+1)k + R}
\end{aligned}$$

V_1 prefers to declare support for B rather than remaining undeclared if and only if:

$$\begin{aligned}
EU(B) &> EU(\emptyset) \\
x_1 &< x_{1B}^\star = \frac{k(3R + (-p_A + 3r_B)\gamma - 4c_B) + (R + \gamma(p_A + r_B))R}{(4\delta+1)k + R}
\end{aligned}$$

[2]Note that this model endogenizes the probability that A wins as a function of declarations and shows one channel that can lead to $\alpha > 0$ in a strategic environment with multiple voters.

If we assume $r_B = p_A = 0$ as in most experimental treatments, the cutoff becomes:

$$x_1 < x^\star_{1B} = \frac{k(3R-4c_B)+R^2}{(4\delta+1)k+R}$$

We obtain comparative statics by taking the partial derivatives of each cutoff. Hypotheses below are enumerated to facilitate comparison with main paper's hypotheses; since in this model the probability A wins is endogenous and (as in the experiment) B does not impose punishments, there are no counterparts of H2, H7, H9 and H11:

H1 *A's Reward Size*: As r_A increases, declarations for A increase $\left(\frac{\partial x^\star_{1A}}{\partial r_A} < 0\right)$; declarations for B are unaffected $\left(\frac{\partial x^\star_{1B}}{\partial r_A} = 0\right)$; non-declarations decrease $\left(\frac{\partial (x^\star_{1A}-x^\star_{1B})}{\partial r_A} < 0\right)$.

H3 *Cost of Declaring for A*: As c_A increases, declarations for A decrease $\left(\frac{\partial x^\star_{1A}}{\partial c_A} > 0\right)$; declarations for B are unaffected $\left(\frac{\partial x^\star_{1B}}{\partial c_A} = 0\right)$; non-declarations increase $\left(\frac{\partial (x^\star_{1A}-x^\star_{1B})}{\partial c_A} > 0\right)$.

H4 *Monitoring*: As γ increases, declarations for A increase $\left(\frac{\partial x^\star_{1A}}{\partial \gamma} < 0\right)$; declarations for B are unaffected $\left(\frac{\partial x^\star_{1B}}{\partial \gamma} = 0\right)$; and non-declarations decrease $\left(\frac{\partial (x^\star_{1A}-x^\star_{1B})}{\partial \gamma} < 0\right)$

H5 *A's Punishment Size*: Assume $k > R$. As p_A increases, declarations for A are unaffected $\left(\frac{\partial x^\star_{1A}}{\partial p_A} = 0\right)$, declarations for B decrease $\left(\frac{\partial x^\star_{1B}}{\partial p_A} < 0\right)$, and non-declarations increase $\left(\frac{\partial (x^\star_{1A}-x^\star_{1B})}{\partial p_A} > 0\right)$.

H6 *B's Reward Size*: As r_B increases, declarations for A are unaffected $\left(\frac{\partial x^\star_{1A}}{\partial r_B} = 0\right)$; declarations for B increase $\left(\frac{\partial x^\star_{1B}}{\partial r_B} > 0\right)$; and non declarations decrease $\left(\frac{\partial (x^\star_{1A}-x^\star_{1B})}{\partial r_B} < 0\right)$.

H8 *Relative Impact of Rewards vs. Punishments*: Rewards affect declarations relatively more than punishments of comparable magnitude. $\left(\left|\frac{\partial x^\star_{1A}}{\partial r_A}\right| - \left|\frac{\partial x^\star_{1B}}{\partial p_A}\right|\right) > 0$.

H10 *Expressive Utility*: As δ increases, declarations for A increase if and only if $x^\star_{1A} > 0$ $\left(\frac{\partial x^\star_{1A}}{\partial \gamma} < 0 \text{ if and only if } x^\star_{1A} > 0\right)$; declarations for B increase if and only if $x^\star_{1B} < 0$ $\left(\frac{\partial x^\star_{1B}}{\partial \gamma} > 0 \text{ if and only if } x^\star_{1B} < 0\right)$. The effect on non-declarations depend on the parameters.

D Characteristics of Online Sample vs. Brazil Overall

Table 1: Characteristics of Online Sample vs. Brazil Overall

	Online Sample	**Brazil Overall**
Gender		
Female	46.2%	49.0%
Male	53.8%	51.0%
Age		
18-29	34.7%	31.0%
30-39	15.6%	22.3%
40-49	18.3%	18.5%
50-59	21.8%	13.6%
60-69	6.7%	8.1%
70+	1.8%	6.4%
Region		
Center-West	6.2%	7.4%
North	4.9%	8.3%
Northeast	30.6%	27.8%
South	20.0%	14.4%
Southeast	38.4%	42.1%
Urban		
Rural	19.2%	15.6%
Urban	80.8%	84.4%
Education		
No Education	1.4%	11.2%
Incomplete Primary	11.4%	30.6%
Complete Primary	9.3%	9.1%
Incomplete Secondary	10.0%	3.9%
Complete Secondary	26.4%	26.3%
Incomplete Tertiary	19.3%	3.4%
Complete Tertiary	22.3%	15.3%

Notes: Characteristics of online sample are self-reported by participants in the declared support experiment. These participants were recruited through Facebook advertisements, as described in Section 4 of the paper. Characteristics of Brazil overall reflect data from Brazil's census bureau (*Instituto Brasileiro de Geografia e Estatística*); more specifically, from its 2010 census (gender, age, region and urban) and its 2016 PNAD Continua (education).

E Robustness Across Education Level

Table 2: Estimates of Heterogeneous Treatment Effects, Rewards (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.097**	-0.082**	0.038	0.025	0.053**	0.052*
	(0.024)	(0.029)	(0.022)	(0.026)	(0.020)	(0.022)
Lopsided Election	0.038	0.065*	-0.015	-0.024	-0.025	-0.045*
	(0.022)	(0.026)	(0.023)	(0.026)	(0.020)	(0.022)
Cost	-0.051*	-0.056*	0.035	0.045	0.016	0.011
	(0.024)	(0.027)	(0.023)	(0.026)	(0.019)	(0.022)
Low Monitoring	-0.043	-0.016	0.032	0.008	0.011	0.008
	(0.024)	(0.028)	(0.022)	(0.026)	(0.020)	(0.022)
Expressive Utility	-0.009	0.025	0.048*	0.043	-0.041*	-0.073**
	(0.022)	(0.026)	(0.022)	(0.025)	(0.020)	(0.023)
No Election Influence	0.004	0.015	-0.023	-0.020	0.017	0.004
	(0.023)	(0.026)	(0.024)	(0.026)	(0.019)	(0.022)
Competitive Clientelism	-0.039	-0.051*	0.075**	0.078**	-0.039	-0.030
	(0.023)	(0.026)	(0.023)	(0.025)	(0.020)	(0.022)
Round	0.008**	0.004	-0.008**	-0.003	0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Partisan Type	0.003**	0.004**	-0.002**	-0.003**	-0.002**	-0.002*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Screener	0.011	0.015	-0.010	-0.041*	-0.001	0.025
	(0.014)	(0.016)	(0.013)	(0.016)	(0.012)	(0.016)
Subjects Fixed Effects	No	No	No	No	No	No
Above-Median Education	No	Yes	No	Yes	No	Yes
N	4976	3536	4976	3536	4976	3536

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

Table 3: Estimates of Heterogeneous Treatment Effects, Punishments Only (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.048*	0.066**	-0.027	-0.024	-0.020	-0.040
	(0.022)	(0.025)	(0.021)	(0.023)	(0.019)	(0.021)
Round	0.008*	0.008*	-0.012**	-0.006	0.004	-0.002
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Partisan Type	0.002**	0.002**	-0.002**	-0.002**	-0.000	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Screener	-0.005	-0.020	-0.024	-0.018	0.028*	0.037**
	(0.014)	(0.015)	(0.014)	(0.014)	(0.013)	(0.014)
Subjects Fixed Effects	No	No	No	No	No	No
Above-Median Education	No	Yes	No	Yes	No	Yes
N	1881	1701	1881	1701	1881	1701

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

Table 4: Estimates of Heterogeneous Treatment Effects, Clientelism & Punishment (Logit)

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.050*	0.052*	-0.005	0.014	-0.046*	-0.069**
	(0.022)	(0.024)	(0.021)	(0.023)	(0.019)	(0.022)
Round	0.008*	0.011**	-0.010**	-0.008*	0.001	-0.003
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Partisan Type	0.003**	0.004**	-0.002**	-0.003**	-0.001*	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Screener	-0.007	-0.010	-0.022	-0.024	0.028*	0.033*
	(0.014)	(0.015)	(0.013)	(0.014)	(0.012)	(0.013)
Subjects Fixed Effects	No	No	No	No	No	No
Above-Median Education	No	Yes	No	Yes	No	Yes
N	1881	1701	1881	1701	1881	1701

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

Table 5: Estimates of Heterogeneous Treatment Effects, Rewards (Logit), with Subject Fixed Effects

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.125**	-0.115**	0.051	0.035	0.089**	0.090*
	(0.030)	(0.040)	(0.030)	(0.038)	(0.032)	(0.039)
Lopsided Election	0.048	0.089*	-0.020	-0.035	-0.041	-0.078*
	(0.029)	(0.036)	(0.031)	(0.038)	(0.033)	(0.038)
Cost	-0.065*	-0.080*	0.048	0.065	0.026	0.019
	(0.030)	(0.037)	(0.030)	(0.038)	(0.031)	(0.038)
Low Monitoring	-0.055	-0.022	0.042	0.011	0.018	0.014
	(0.030)	(0.038)	(0.030)	(0.037)	(0.032)	(0.038)
Expressive Utility	-0.012	0.035	0.064*	0.060	-0.067*	-0.129**
	(0.029)	(0.035)	(0.029)	(0.037)	(0.032)	(0.040)
No Election Influence	0.005	0.020	-0.030	-0.029	0.028	0.007
	(0.030)	(0.036)	(0.032)	(0.038)	(0.031)	(0.038)
Competitive Clientelism	-0.050	-0.071*	0.101**	0.113**	-0.063	-0.053
	(0.030)	(0.036)	(0.031)	(0.035)	(0.032)	(0.039)
Round	0.010**	0.007*	-0.012**	-0.005	0.003	-0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Subjects Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Above-Median Education	No	Yes	No	Yes	No	Yes
N	3880	2552	3704	2432	3032	2024

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

Table 6: Estimates of Heterogeneous Treatment Effects, Punishments Only (Logit), with Subject Fixed Effects

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.161*	0.153	-0.067	-0.134	-0.138	-0.069
	(0.063)	(0.080)	(0.069)	(0.086)	(0.073)	(0.095)
Round	0.016	0.017	-0.040**	0.005	0.032	-0.032
	(0.016)	(0.019)	(0.015)	(0.020)	(0.017)	(0.020)
Subjects Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Above-Median Education	No	Yes	No	Yes	No	Yes
N	444	278	406	250	338	216

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

Table 7: Estimates of Heterogeneous Treatment Effects, Clientelism & Punishment (Logit), with Subject Fixed Effects

	Declare for A		**Declare for B**		**No Declaration**	
	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.110	0.182*	-0.035	0.099	-0.102	-0.359**
	(0.064)	(0.075)	(0.068)	(0.087)	(0.078)	(0.063)
Round	0.025	0.047*	-0.035*	-0.044	0.018	-0.043
	(0.015)	(0.019)	(0.015)	(0.023)	(0.019)	(0.026)
Subjects Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Above-Median Education	No	Yes	No	Yes	No	Yes
N	462	292	426	242	316	222

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors clustered by subject are reported. Above-median education indicates respondents above high-school completion (the median education level in our sample).

F Robustness to Outcomes in Prior Rounds

Table 8: Estimates of Average Treatment Effects, Rewards (Logit), Robustness to Controlling for Tickets Won in Previous Round

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.086**	-0.121**	0.024	0.043*	0.062**	0.107**
	(0.018)	(0.022)	(0.016)	(0.021)	(0.016)	(0.025)
Lopsided Election	0.057**	0.084**	-0.034*	-0.051*	-0.022	-0.046*
	(0.017)	(0.022)	(0.016)	(0.021)	(0.014)	(0.023)
Cost	-0.050**	-0.077**	0.036*	0.070**	0.014	0.015
	(0.018)	(0.021)	(0.017)	(0.022)	(0.015)	(0.023)
Low Monitoring	-0.019	-0.035	0.011	0.025	0.007	0.015
	(0.018)	(0.021)	(0.016)	(0.021)	(0.015)	(0.024)
Competitive Clientelism	-0.037*	-0.055*	0.070**	0.104**	-0.033*	-0.056*
	(0.017)	(0.021)	(0.017)	(0.022)	(0.014)	(0.023)
Expressive Utility	0.011	0.009	0.037*	0.062**	-0.048**	-0.086**
	(0.017)	(0.021)	(0.016)	(0.022)	(0.014)	(0.023)
No Election Influence	0.003	0.001	-0.014	-0.011	0.011	0.013
	(0.017)	(0.022)	(0.016)	(0.021)	(0.015)	(0.023)
Round	0.008**	0.011**	-0.006**	-0.009**	-0.002	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Partisan Type	0.004**		-0.002**		-0.002**	
	(0.000)		(0.000)		(0.000)	
Screener	0.014		-0.027**		0.012	
	(0.010)		(0.009)		(0.009)	
Tickets in Prior Round	0.000	-0.000	-0.001**	-0.000	0.001**	0.001
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Subject Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	9055	6737	9055	6291	9055	5176

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 9: Estimates of Average Treatment Effects, Punishment Only (Logit), Robustness to Controlling for Tickets Won in Previous Round

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.056**	0.179**	-0.026	-0.116**	-0.030	-0.115**
	(0.021)	(0.038)	(0.019)	(0.042)	(0.019)	(0.044)
Round	0.008	0.000	-0.006	0.000	-0.002	-0.006
	(0.004)	(0.010)	(0.004)	(0.010)	(0.004)	(0.011)
Partisan Type	0.002**		-0.002**		-0.000	
	(0.001)		(0.001)		(0.001)	
Screener	-0.013		-0.023*		0.035**	
	(0.012)		(0.011)		(0.011)	
Tickets in Prior Round	-0.000	-0.004**	-0.001	0.004**	0.001*	0.001
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.002)
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2253	670	2253	560	2253	506

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 10: Estimates of Average Treatment Effects, Clientelism & Punishment (Logit), Robustness to Controlling for Tickets Won in Previous Round

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.042*	0.123**	0.008	0.008	-0.050**	-0.182**
	(0.021)	(0.036)	(0.019)	(0.039)	(0.018)	(0.043)
Round	0.011**	0.048**	-0.008*	-0.045**	-0.004	-0.012
	(0.004)	(0.009)	(0.004)	(0.010)	(0.003)	(0.012)
Partisan Type	0.004**		-0.002**		-0.001**	
	(0.001)		(0.001)		(0.000)	
Screener	-0.008		-0.021		0.028**	
	(0.012)		(0.011)		(0.010)	
Tickets in Prior Round	-0.000	-0.003	-0.001	0.002	0.001*	0.002
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.00)
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2275	718	2275	630	2275	528

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 11: Estimates of Average Treatment Effects, Rewards (Logit), Robustness to Controlling for Total Tickets Won in All Previous Rounds

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.088**	-0.114**	0.033*	0.043*	0.055**	0.095**
	(0.017)	(0.020)	(0.016)	(0.020)	(0.015)	(0.023)
Lopsided Election	0.059**	0.076**	-0.034*	-0.046*	-0.025	-0.042*
	(0.016)	(0.020)	(0.015)	(0.020)	(0.013)	(0.022)
Cost	-0.057**	-0.076**	0.047**	0.065**	0.010	0.018
	(0.016)	(0.020)	(0.016)	(0.020)	(0.014)	(0.022)
Low Monitoring	-0.026	-0.035	0.015	0.020	0.011	0.019
	(0.017)	(0.020)	(0.015)	(0.020)	(0.014)	(0.022)
Competitive Clientelism	-0.045**	-0.060**	0.079**	0.112**	-0.034**	-0.059**
	(0.016)	(0.020)	(0.016)	(0.021)	(0.013)	(0.021)
Expressive Utility	0.013	0.017	0.035*	0.050*	-0.049**	-0.084**
	(0.016)	(0.020)	(0.015)	(0.021)	(0.013)	(0.021)
No Election Influence	-0.001	-0.000	-0.011	-0.016	0.011	0.020
	(0.016)	(0.020)	(0.015)	(0.020)	(0.013)	(0.022)
Round	0.004	0.022**	0.009	-0.023**	-0.011*	0.001
	(0.005)	(0.006)	(0.005)	(0.006)	(0.004)	(0.006)
Partisan Type	0.004**		-0.002**		-0.002**	
	(0.000)		(0.000)		(0.000)	
Screener	0.013		-0.025**		0.012	
	(0.010)		(0.009)		(0.009)	
Cumulated Tickets	0.000	-0.000*	-0.000**	0.000*	0.000**	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Subject Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	10061	7646	10061	7176	10061	5881

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 12: Estimates of Average Treatment Effects, Punishment Only (Logit), Robustness to Controlling for Total Tickets Won in All Previous Rounds

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.053**	0.152**	-0.027	-0.096**	-0.026	-0.101**
	(0.019)	(0.034)	(0.019)	(0.036)	(0.018)	(0.039)
Round	0.010	0.004	0.001	-0.071**	-0.009	0.085**
	(0.007)	(0.027)	(0.007)	(0.027)	(0.006)	(0.031)
Partisan Type	0.002**		-0.002**		-0.001	
	(0.001)		(0.001)		(0.000)	
Screener	-0.009		-0.026*		0.033**	
	(0.011)		(0.011)		(0.010)	
Cumulated Tickets	-0.000	0.000	-0.000	0.001	0.000*	-0.002**
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2517	862	2517	744	2517	646

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 13: Estimates of Average Treatment Effects, Clientelism & Punishment (Logit), Robustness to Controlling for Total Tickets Won in All Previous Rounds

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.047*	0.125**	0.001	0.014	-0.048**	-0.195**
	(0.020)	(0.033)	(0.018)	(0.035)	(0.017)	(0.039)
Round	0.008	0.099**	0.003	-0.118**	-0.009	0.004
	(0.007)	(0.025)	(0.007)	(0.027)	(0.006)	(0.032)
Partisan Type	0.004**		-0.002**		-0.002**	
	(0.001)		(0.001)		(0.000)	
Screener	-0.005		-0.025*		0.029**	
	(0.011)		(0.011)		(0.010)	
Cumulated Tickets	0.000	-0.001*	-0.000*	0.002**	0.000	-0.000
	(0.000)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2517	896	2517	780	2517	628

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 14: Estimates of Effects of Outcomes in Prior Rounds

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Declare for A			**Declare for B**			**No Declaration**		
Declared for Winner in Prior Round	0.0192			-0.0076			-0.0115		
	(0.0107)			(0.0100)			(0.0082)		
Tickets in Prior Round		-0.0000			-0.0008**			0.0007**	
		(0.0003)			(0.0003)			(0.0002)	
Cumulated Tickets			0.0000			-0.0003**			0.0002**
			(0.0001)			(0.0001)			(0.0001)
Round	0.0118**	0.0086**	0.0056	-0.0068**	-0.0060**	0.0079	-0.0050**	-0.0026	-0.0108**
	(0.0018)	(0.0016)	(0.0046)	(0.0018)	(0.0015)	(0.0047)	(0.0013)	(0.0014)	(0.0042)
Partisan Type	0.0035**	0.0037**	0.0037**	-0.0026**	-0.0022**	-0.0021**	-0.0009**	-0.0015**	-0.0016**
	(0.0005)	(0.0004)	(0.0004)	(0.0005)	(0.0004)	(0.0004)	(0.0003)	(0.0004)	(0.0004)
Screener	0.0139	0.0101	0.0091	-0.0288**	-0.0255**	-0.0247**	0.0144*	0.0151	0.0152
	(0.0110)	(0.0096)	(0.0095)	(0.0109)	(0.0092)	(0.0091)	(0.0061)	(0.0086)	(0.0084)
Subjects Fixed Effects	No	No	No	No	No	No	No	No	No
Observations	8671	11330	12578	8671	11330	12578	8671	11330	12578

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. Robust standard errors clustered by subject are reported in parentheses.

G Attrition

Table 15: Completion of Treatments, by Respondent Characteristics

	Number of Treatments Completed					Completed All 10 Treatments				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Below-Median Education	-0.200				-0.124	-0.061**				-0.031
	(0.13)				(0.08)	(0.02)				(0.02)
Female		-0.306**			-0.113		-0.021			-0.010
		(0.12)			(0.07)		(0.02)			(0.02)
Age			0.016***		0.007**				0.004***	0.002***
			(0.00)		(0.00)				(0.00)	(0.00)
Income				0.000	-0.000			0.000		0.000
				(0.00)	(0.00)			(0.00)		(0.00)
Constant	9.234***	9.314***	8.661***	9.100***	9.559***	0.842***	0.831***	0.798***	0.705***	0.828***
	(0.07)	(0.07)	(0.14)	(0.24)	(0.20)	(0.01)	(0.01)	(0.05)	(0.03)	(0.05)
Observations	1296	1294	1480	1495	1156	1296	1294	1495	1480	1156
R^2	0.002	0.005	0.018	0.000	0.016	0.006	0.001	0.001	0.024	0.020

Note: *: $p < 0.05$, **: $p < 0.01$. Analyses are OLS regressions with robust standard errors. Dependent variable for the left panel is the number of treatments completed by the respondent. Dependent variable for the right panel is a binary variable coded 1 if the respondent completed all 10 treatments; 0 otherwise. Right panel is robust using logit specifications. Independent variables are self-reported. "Below-Median Education" is a binary variable coded 1 if the respondent reported having no education, incomplete or complete primary education, or incomplete secondary education; 0 if reporting higher educational attainment. Results are robust to using a continuous measure of education. Female is coded as 1 for female; 0 for male. Age is a continuous variable in years. Income is reported using a ten-point scale.

Table 16: Completion of Treatments, by Respondent Characteristics

	Number of Treatments Completed					Completed All 10 Treatments				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Primary Education or Below	-0.359*				-0.116	-0.079**				-0.020
	(0.15)				(0.09)	(0.03)				(0.02)
Female		-0.306**			-0.108		-0.021			-0.009
		(0.12)			(0.07)		(0.02)			(0.02)
Age			0.016***		0.007***				0.004***	0.002***
			(0.00)		(0.00)				(0.00)	(0.00)
Income				0.000	-0.000			0.000		0.000
				(0.00)	(0.00)			(0.00)		(0.00)
Constant	9.252***	9.314***	8.661***	9.100***	9.531***	0.840***	0.831***	0.798***	0.705***	0.817***
	(0.06)	(0.07)	(0.14)	(0.24)	(0.19)	(0.01)	(0.01)	(0.05)	(0.03)	(0.05)
Observations	1296	1294	1480	1495	1156	1296	1294	1495	1480	1156
R^2	0.005	0.005	0.018	0.000	0.015	0.008	0.001	0.001	0.024	0.019

Note: *: $p < 0.05$, **: $p < 0.01$. Analyses are OLS regressions with robust standard errors. Dependent variable for the left panel is the number of treatments completed by the respondent. Dependent variable for the right panel is a binary variable coded 1 if the respondent completed all 10 treatments; 0 otherwise. Right panel is robust using logit specifications. Independent variables are self-reported. "Primary Education or Below" is a binary variable coded 1 if the respondent reported having no education, incomplete primary education, or having finished primary education; 0 if reporting higher educational attainment. Results are robust to using a continuous measure of education. Female is coded as 1 for female; 0 for male. Age is a continuous variable in years. Income is reported using a ten-point scale.

Table 17: Estimates of Average Treatment Effects, Rewards (Logit), Robustness to Including Attrited Respondents

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-0.059**	-0.087**	0.011	0.017	0.037**	0.075**
	(0.012)	(0.015)	(0.011)	(0.016)	(0.010)	(0.018)
Lopsided Election	0.040**	0.057**	-0.027**	-0.041**	-0.021*	-0.039*
	(0.012)	(0.016)	(0.010)	(0.015)	(0.009)	(0.017)
Cost	-0.045**	-0.064**	0.031**	0.050**	0.009	0.018
	(0.011)	(0.015)	(0.011)	(0.016)	(0.009)	(0.018)
Low Monitoring	-0.024*	-0.034*	-0.001	-0.001	0.004	0.011
	(0.012)	(0.016)	(0.011)	(0.015)	(0.010)	(0.018)
Competitive Clientelism	-0.037**	-0.054**	0.044**	0.068**	-0.025**	-0.049**
	(0.011)	(0.015)	(0.011)	(0.016)	(0.009)	(0.017)
Expressive Utility	0.008	0.011	0.023*	0.037*	-0.030**	-0.059**
	(0.011)	(0.015)	(0.011)	(0.016)	(0.009)	(0.017)
No Election Influence	-0.001	-0.004	-0.016	-0.023	0.012	0.023
	(0.012)	(0.016)	(0.011)	(0.016)	(0.010)	(0.018)
Round	-0.012**	-0.018**	-0.019**	-0.030**	-0.008**	-0.016**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
Partisan Type	0.003**		-0.002**		-0.001**	
	(0.001)		(0.001)		(0.001)	
Screener	0.084**		0.038**		0.047**	
	(0.007)		(0.007)		(0.006)	
Subject Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	17040	11808	17040	10880	17040	8568

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. *Baseline Clientelism* is the excluded treatment category, so that coefficients report differences from that baseline. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 18: Estimates of Average Treatment Effects, Punishment Only (Logit), Robustness to Including Attrited Respondents

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	0.026	0.094**	-0.018	-0.077**	-0.016	-0.087**
	(0.014)	(0.028)	(0.013)	(0.029)	(0.012)	(0.033)
Round	-0.011**	-0.042**	-0.019**	-0.065**	-0.009**	-0.037**
	(0.002)	(0.006)	(0.002)	(0.006)	(0.002)	(0.007)
Partisan Type	0.002**		-0.001**		-0.001	
	(0.001)		(0.001)		(0.001)	
Screener	0.062**		0.035**		0.072**	
	(0.008)		(0.008)		(0.007)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	4260	1220	4260	1062	4260	906

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *No Clientelism* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

Table 19: Estimates of Average Treatment Effects, Clientelism & Punishment (Logit), Robustness to Including Attrited Respondents

	Declare for A		**Declare for B**		**No Declaration**	
Treatment	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism & Punishment	0.040**	0.128**	-0.001	-0.001	-0.031**	-0.145**
	(0.014)	(0.027)	(0.013)	(0.029)	(0.012)	(0.033)
Round	-0.012**	-0.030**	-0.019**	-0.073**	-0.011**	-0.051**
	(0.002)	(0.006)	(0.002)	(0.006)	(0.002)	(0.007)
Partisan Type	0.003**		-0.002**		-0.001**	
	(0.001)		(0.001)		(0.001)	
Screener	0.074**		0.037**		0.058**	
	(0.008)		(0.008)		(0.007)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	4260	1300	4260	1060	4260	876

Note: *: $p < 0.05$, **: $p < 0.01$. Coefficients report marginal effects from logistic regressions. Each observation corresponds to a decision in the experiment. To isolate causal effects, the regressions in this table employ *Punishment Only* as the excluded category. Robust standard errors, clustered by subject in columns 1, 3 and 5, are reported in parentheses.

H Screenshot Examples

Instructions (Page 1 of 2)

Obrigado por participar! Você já tem 50 FICHAS para o sorteio do iPhone 5S. Você agora vai jogar 10 jogos para ganhar mais fichas. Quanto mais fichas você tiver, mais chances você vai ter de ganhar o iPhone.

Cada jogo tem uma eleição. Dois candidatos concorrem para a prefeitura – o candidato amarelo e o candidato verde.

Em cada jogo, você vai ter a opção de colocar uma bandeira amarela ou verde na sua casa. Se você colocar uma bandeira, você aumenta as chances do seu candidato ganhar a eleição. Você pode escolher não colocar nenhuma bandeira.

>>

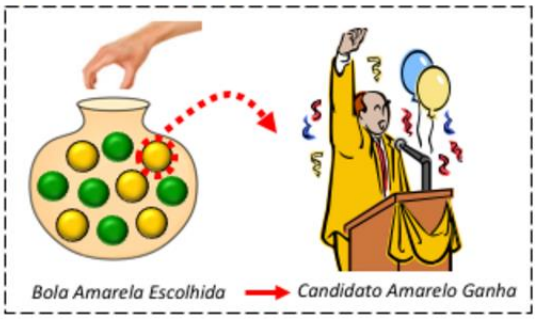
TRANSLATION: "Thank you for participating! You already have 50 TICKETS for the IPhone 5S lottery. You will now play 10 games to earn more tickets. The more tickets you have, the more chances you will have to win an Iphone. Every game has an election. Two candidates run for mayor — the yellow candidate and the green candidate. In each game, you will have the option to place a yellow or green flag on your house. If you put up a flag, you increase the chances of that candidate winning the election. You can also choose to place no flag on your house."

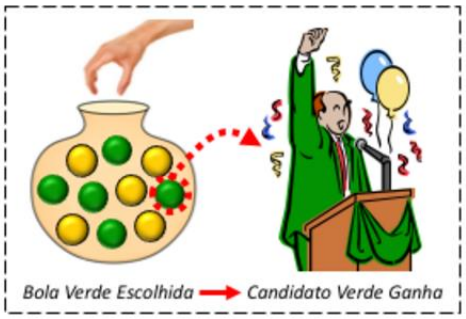
Instructions (Page 2 of 2)

Leia as instruções com cuidado. As fichas que você ganha para cada escolha podem mudar de uma questão para outra.

Em alguns jogos, o candidato que ganha pode te recompensar se você colocou a bandeira dele na sua casa, ou pode te prejudicar se você colocou a bandeira do rival.

Depois de cada jogo, o computador escolhe o vencedor.





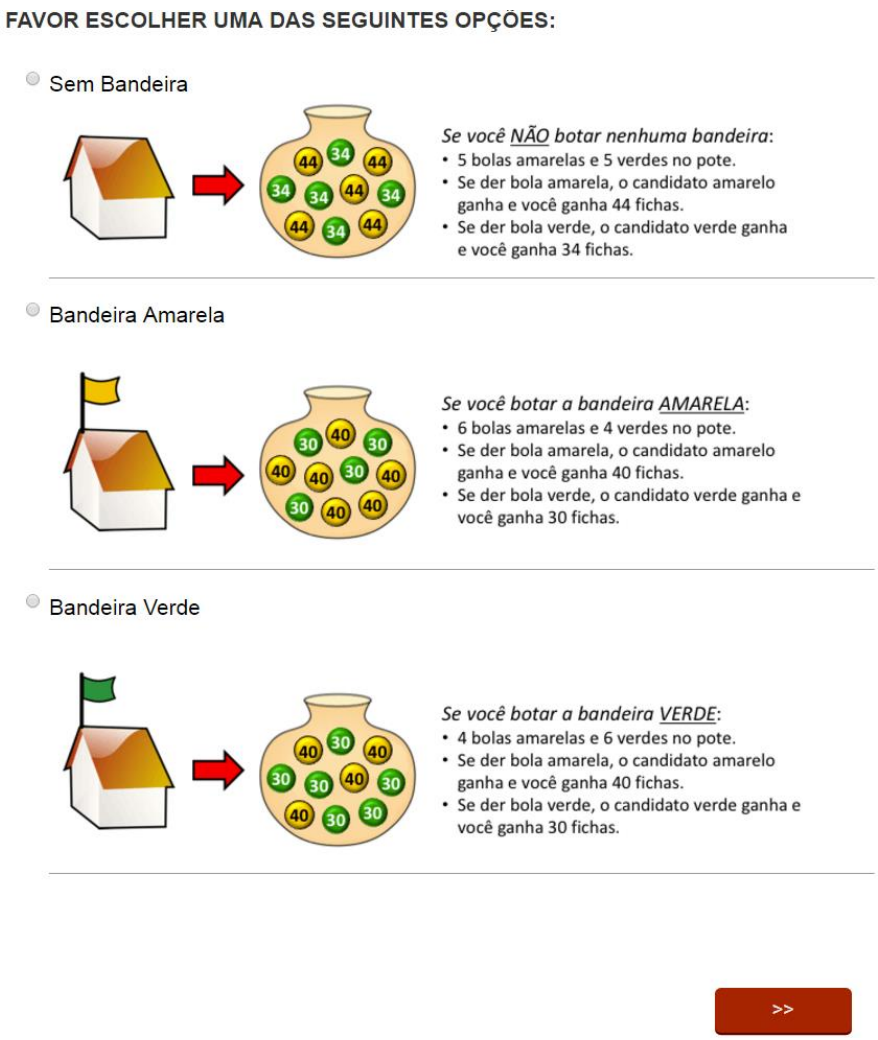
O número de fichas iPhone que você vai ganhar depende de quem ganhar a eleição e da sua decisão sobre a bandeira.

Lembre-se que os candidatos e a bandeira não são reais! Clique quando você estiver preparado para jogar.

>>

TRANSLATION: "Read the instructions carefully. The tickets you earn for each choice can change from one question to another. In some games, the candidate who wins may reward you if you placed his flag on your house, or he may penalize you if you placed his opponent's flag. After each game, the computer chooses the winner. [IMAGE: Yellow Ball Chosen → Yellow Candidate Wins. Green Ball Chosen → Green Candidate Wins.] The number of IPhone tickets you will earn depends on who wins the election and your decision about the flag. Remember that the candidates and the flags are not real! Click when you are ready to play."

Weak Supporter of Candidate A (Partisan Type 3)
No Clientelism Treatment, Options Page

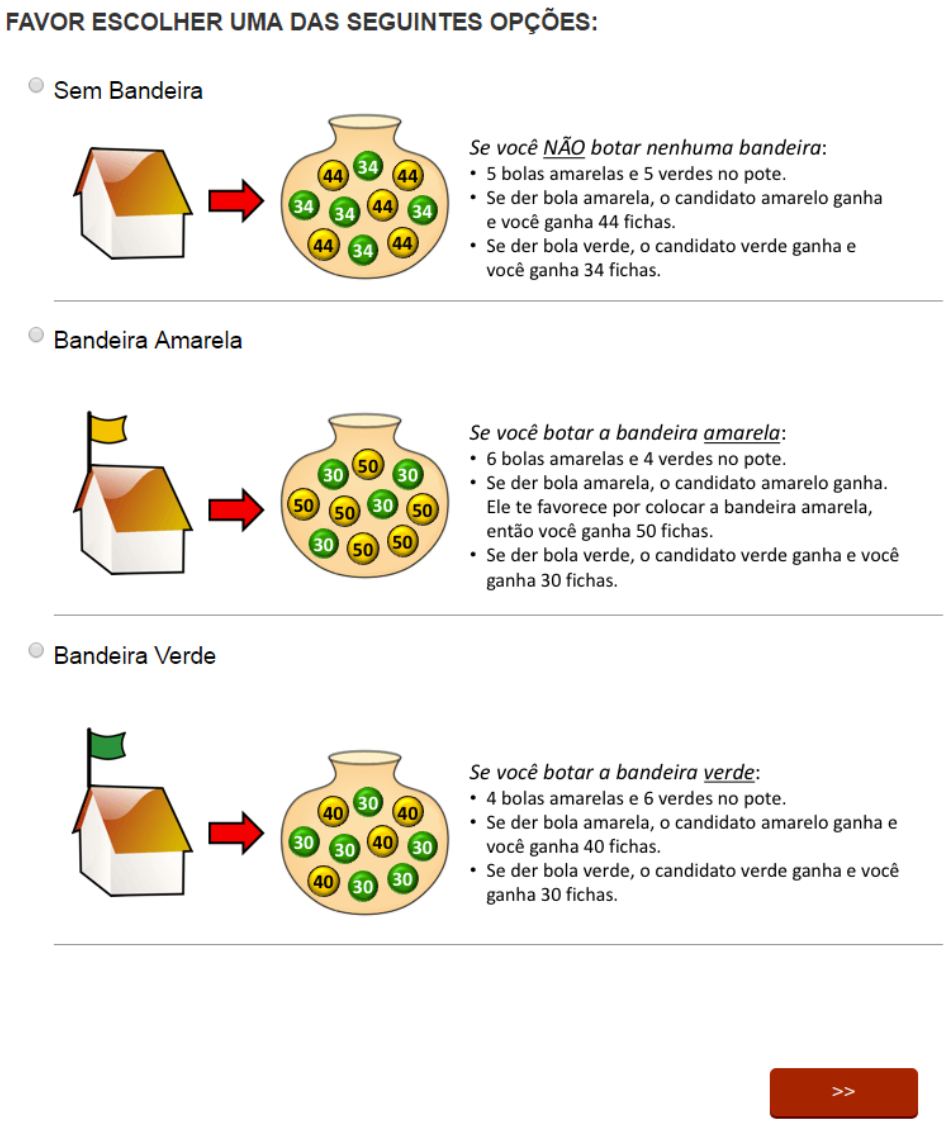


TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 44 tickets; If the green ball is chosen, the green candidate wins and you earn 34 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets."

Weak Supporter of Candidate A (Partisan Type 3)
***No Clientelism* Treatment, Outcome Page**
Yellow Flag Chosen, Yellow Candidate Wins

TRANSLATION: "YOU EARNED 40 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: Yellow flag selected, Yellow ball chosen.] You earned 40 tickets! You placed a yellow flag. The computer chose a yellow ball, so the yellow candidate won. [BUTTON: CLICK TO EARN MORE TICKETS!]."

Weak Supporter of Candidate A (Partisan Type 3)
Baseline Clientelism Treatment, Options Page



TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 44 tickets; If the green ball is chosen, the green candidate wins and you earn 34 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins. He rewards you for placing a yellow flag, so you earn 50 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets."

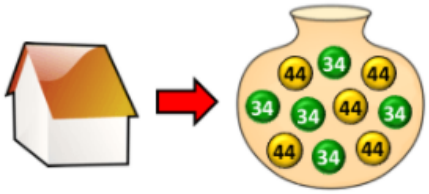
Weak Supporter of Candidate A (Partisan Type 3)
***Baseline Clientelism* Treatment, Outcome Page**
No Flag Chosen, Green Candidate Wins

TRANSLATION: "YOU EARNED 34 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: No flag selected, Green ball chosen.] You earned 34 tickets! You did not place a flag. The computer chose a green ball, so the green candidate won. [BUTTON: CLICK TO EARN MORE TICKETS!]."

Weak Supporter of Candidate A (Partisan Type 3)
***Low Monitoring* Treatment, Options Page**

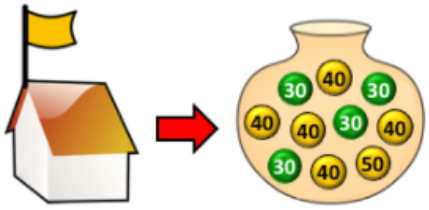
FAVOR ESCOLHER UMA DAS SEGUINTES OPÇÕES:

○ Sem Bandeira



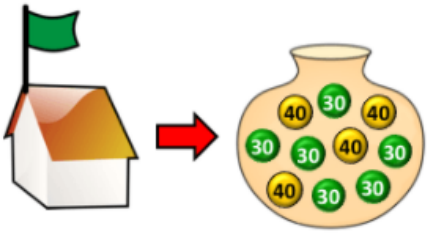
Se você <u>NÃO</u> botar nenhuma bandeira:
- 5 bolas amarelas e 5 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha e você ganha 44 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 34 fichas.

○ Bandeira Amarela



Se você botar a bandeira <u>amarela</u>:
- 6 bolas amarelas e 4 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha. Se ele não ver a sua bandeira amarela, você ganha 40 fichas. Se ele ver, ele te favorece e você ganha 50 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 30 fichas.

○ Bandeira Verde



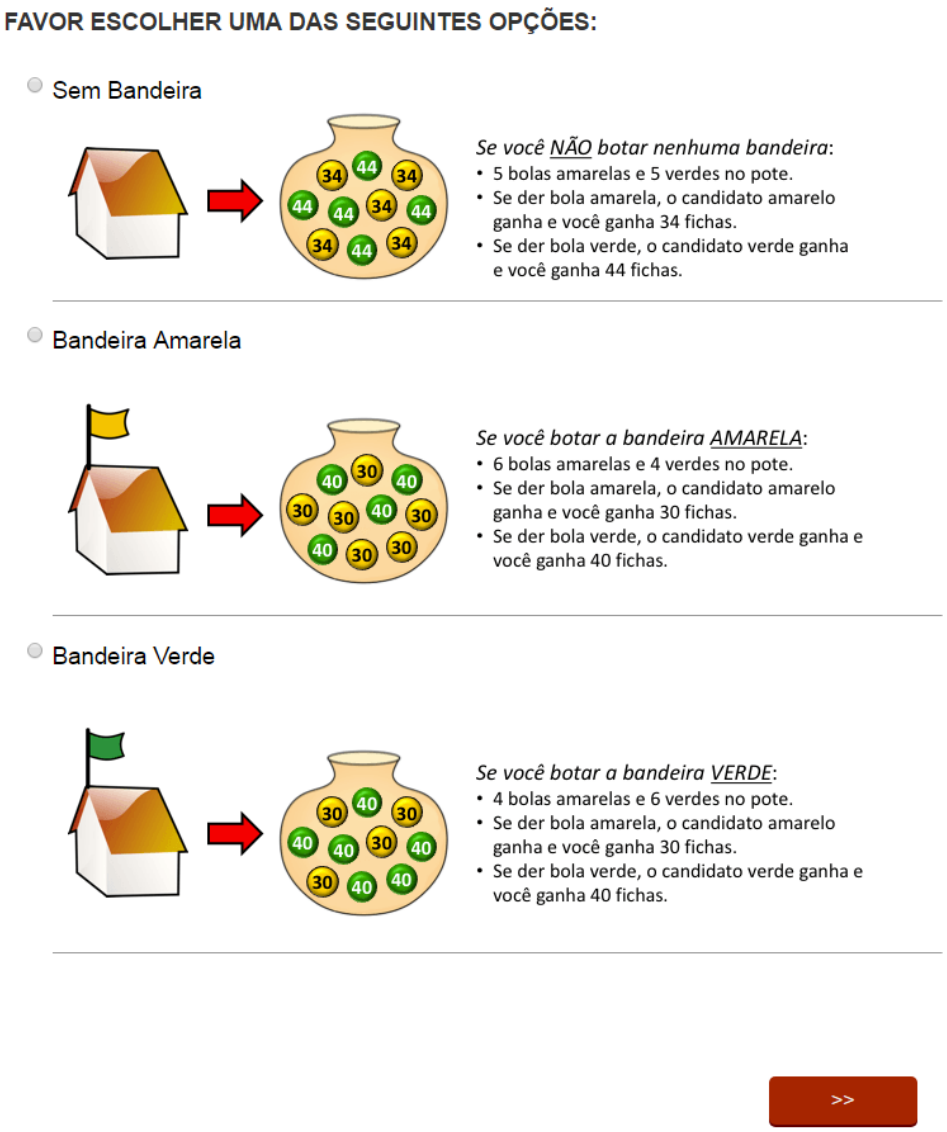
Se você botar a bandeira <u>verde</u>:
- 4 bolas amarelas e 6 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha e você ganha 40 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 30 fichas.

TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 44 tickets; If the green ball is chosen, the green candidate wins and you earn 34 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins. If he doesn't see your yellow flag, you earn 40 tickets. If he sees it, he rewards you and you earn 50 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 30 tickets."

Weak Supporter of Candidate A (Partisan Type 3)
***Low Monitoring* Treatment, Outcome Page**
Yellow Flag Chosen, Yellow Candidate Wins and Sees Flag

TRANSLATION: "YOU EARNED 50 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: Yellow flag selected, Yellow ball chosen.] You earned 50 tickets! You placed a yellow flag; The computer chose a yellow ball, so the yellow candidate won; The yellow candidate saw your flag, so he rewards you; You earn 50 tickets. [BUTTON: CLICK TO EARN MORE TICKETS!]."

Weak Supporter of Candidate B (Partisan Type 5)
No Clientelism Treatment, Options Page

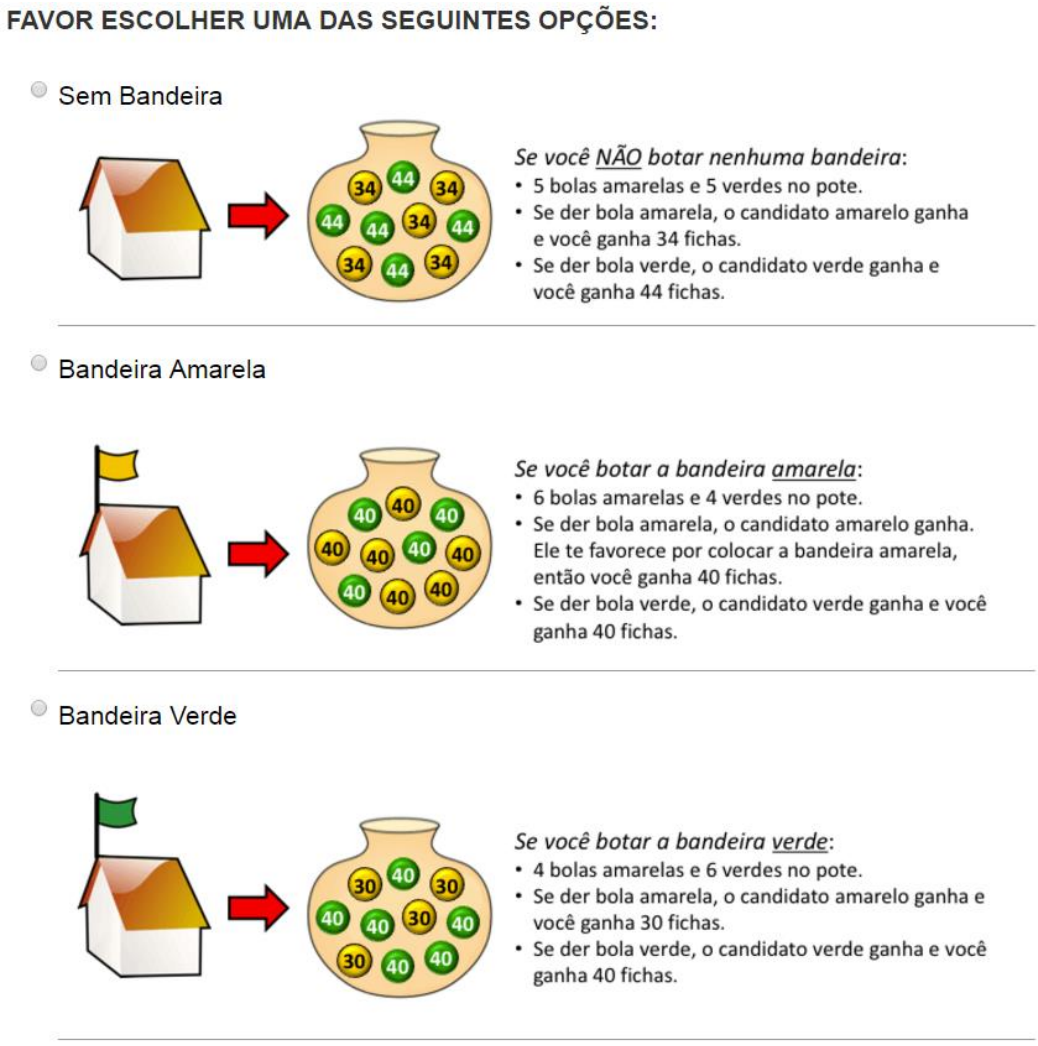


TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 34 tickets; If the green ball is chosen, the green candidate wins and you earn 44 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 30 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 30 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets."

Weak Supporter of Candidate B (Partisan Type 5)
***No Clientelism* Treatment, Outcome Page**
Green Flag Chosen, Yellow Candidate Wins

TRANSLATION: "YOU EARNED 30 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: Green flag selected, Yellow ball chosen.] You earned 30 tickets! You placed a green flag. The computer chose a yellow ball, so the yellow candidate won. [BUTTON: CLICK TO EARN MORE TICKETS!]."

Weak Supporter of Candidate B (Partisan Type 5)
***Baseline Clientelism* Treatment, Options Page**



TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 34 tickets; If the green ball is chosen, the green candidate wins and you earn 44 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins. He rewards you for placing a yellow flag, so you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 30 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets."

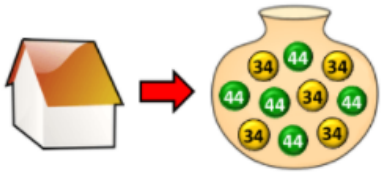
Weak Supporter of Candidate B (Partisan Type 5)
***Baseline Clientelism* Treatment, Outcome Page**
Yellow Flag Chosen, Yellow Candidate Wins

TRANSLATION: "YOU EARNED 40 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: Green flag selected, Yellow ball chosen.] You earned 40 tickets! You placed a yellow flag. The computer chose a yellow ball, so the yellow candidate won. He rewards you for placing a yellow flag, so you earn 40 tickets. [BUTTON: CLICK TO EARN MORE TICKETS!]."

Weak Supporter of Candidate B (Partisan Type 5)
Low Monitoring Treatment, Options Page

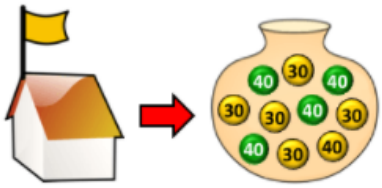
FAVOR ESCOLHER UMA DAS SEGUINTES OPÇÕES:

○ Sem Bandeira



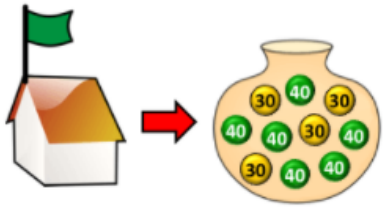
Se você NÃO botar nenhuma bandeira:
- 5 bolas amarelas e 5 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha e você ganha 34 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 44 fichas.

○ Bandeira Amarela



Se você botar a bandeira amarela:
- 6 bolas amarelas e 4 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha. Se ele não ver a sua bandeira amarela, você ganha 30 fichas. Se ele ver, ele te favorece e você ganha 40 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 40 fichas.

○ Bandeira Verde



Se você botar a bandeira verde:
- 4 bolas amarelas e 6 verdes no pote.
- Se der bola amarela, o candidato amarelo ganha e você ganha 30 fichas.
- Se der bola verde, o candidato verde ganha e você ganha 40 fichas.

TRANSLATION: "PLEASE CHOOSE ONE OF THE FOLLOWING OPTIONS: [NO FLAG] If you place NO flag: 5 yellow balls and 5 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 34 tickets; If the green ball is chosen, the green candidate wins and you earn 44 tickets. [YELLOW FLAG] If you place a YELLOW flag: 6 yellow balls and 4 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins. If he doesn't see your yellow flag, you earn 30 tickets. If he sees it, he rewards you and you earn 40 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets. [GREEN FLAG] If you place a GREEN flag: 4 yellow balls and 6 green balls in the jar; If the yellow ball is chosen, the yellow candidate wins and you earn 30 tickets; If the green ball is chosen, the green candidate wins and you earn 40 tickets."

Weak Supporter of Candidate B (Partisan Type 5)
***Low Monitoring* Treatment, Outcome Page**
Yellow Flag Chosen, Yellow Candidate Wins and Does Not See Flag

TRANSLATION: "YOU EARNED 30 TICKETS FOR THE IPHONE LOTTERY! [IMAGE: Green flag selected, Yellow ball chosen.] You earned 30 tickets! You placed a yellow flag; The computer chose a yellow ball, so the yellow candidate won; The yellow candidate doesn't see your flag, so he doesn't reward you. You earn 30 tickets. [BUTTON: CLICK TO EARN MORE TICKETS!]."

I Description of Fieldwork

Fieldwork on clientelism in Brazil was conducted by the author for over 18 months. Prior to and after the October 2008 municipal elections, a total of 110 formal interviews on clientelism were conducted in the state of Bahia. These formal interviews included 55 interviews of community members and 55 interviews of elites. Each of these interviews was conducted in Portuguese, and lasted an average of 70 minutes. Each interview was taped and transcribed, totaling over 4,500 pages of typed transcripts. In addition, informal interviews were conducted of another 350 citizens and elites, as well as three focus groups of citizens. In addition, this fieldwork was supplemented in Pernambuco in mid-2012 with additional interviews of 16 elites and 6 rural citizens.

All interviews were conducted in small municipalities, as defined by those with 100,000 citizens or fewer. In Brazil, 45 percent of the population lives in municipalities with 100,000 citizens or fewer. In addition, 95 percent of Brazilian municipalities are this size (IBGE 2010). The primary field site, Bahia, is the most populous state in the Northeast region of Brazil with 14.0 million citizens (IBGE 2010). Pernambuco is also in the Northeast region with 8.8 million citizens. The Northeast is the poorest region of Brazil and one of the most unequal regions in the world.

In order to identify potential themes, develop interview questions, and field test the citizen and elite interview protocols, the author began qualitative research in a municipality of 10,000 citizens in central Bahia, where he lived for approximately five months. During this time, a stratified random sample of six additional municipalities was selected to conduct further interviews. Overall, the municipalities spanned each of Bahia's seven "mesoregions," which are defined by Brazil's national census bureau (IBGE) as areas that share common geographic characteristics. The sample was stratified to include municipalities with both first-term and second-term mayors. The population sizes of the seven municipalities selected were approximately: 10,000; 15,000; 30,000; 45,000; 60,000; 80,000, and nearly 100,000.

Within each selected municipality, individuals for community member interviews were selected randomly using stratified sampling. Inclusion / exclusion criteria for individuals included the following: (1) at least sixteen years of age (the voting age in Brazil), (2) had lived in the municipality since the previous mayoral election in 2004, and (3) not a member of the same household as any other interviewee. The sample was stratified to ensure balanced representation across gender, age, and urban/rural mix.

Interview protocols consisted of both open-ended and closed-ended questions. An iterative research design was employed; pertinent themes emerging during thematic analysis were investigated during ongoing interviews. While the original, core questions in the interview protocols were asked of all respondents, probes about emerging themes were included in later interviews.

Including both Bahia and Pernambuco, total interviews included 71 elites (primarily mayors and councilors) and 61 citizens (both urban and rural residents). Total interviewed elites included 14 mayors and former mayors, 34 city councilors, three vice-mayors, six party heads, five heads of social services, and several other elites. Interviews were balanced to include a combination of elites both allied and opposed to the current administration.