


# Declared Support and Clientelism

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## Abstract

Recent studies of clientelism predominantly focus on how elites use rewards to influence vote choices and turnout. This article shifts attention toward 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.

## Keywords

elections, public opinion, voting behavior, experimental research, corruption and patronage, Latin American politics

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## Introduction

In many parts of the world, citizens receive material benefits in contingent exchange for providing political support. A cross-national survey of 1400 experts found that such patterns of “clientelism” exist to some degree in over 90% 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 & Robinson, 2008). While some contemporary studies recognize clients’ heightened autonomy (e.g., Hilgers, 2012; Auerbach & 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 & Schechter, 2012), on the optimal combination of multiple clientelist strategies (Gans-Morse et al., 2014), and on the broader targeting of distributive benefits (Dixit & 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 & Nie, 1972; Huckfeldt & 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"—that is, 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.<sup>1</sup>

Evidence suggests that declared support warrants careful investigation. During our 130 interviews in the Brazilian states of Bahia and Pernambuco, many citizens and politicians explained that declaring support for a victorious candidate improves access to post-election benefits.<sup>2</sup> 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 & 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 (Cammatt, 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.

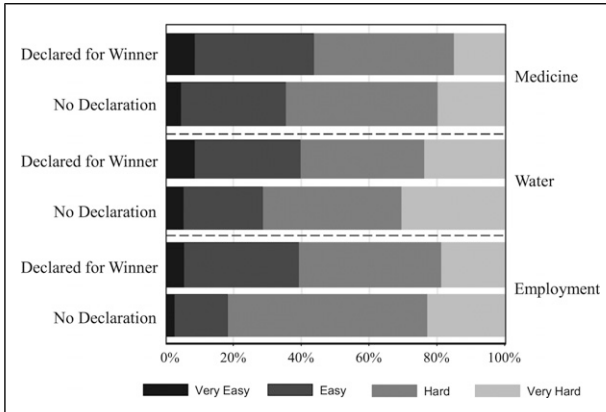
Another important contribution of our study is that it employs experimental methods to examine citizen choices in clientelism, more specifically by using an online experiment to test our model's predictions. A key advantage of this approach is that it isolates causal effects by changing exogenously only 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 1259 online participants from 809 municipalities across Brazil. To investigate mechanisms, this experiment exposed subjects to 10 distinct treatments, each testing pre-registered theoretical predictions about multiple declaration actions.<sup>3</sup> 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 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.



**Figure 1.** Declared Support and Perceived Difficulty of Obtaining Benefits. *Source:* Authors' survey with 1935 respondents recruited across Brazil on Facebook (see Footnote 4).

## 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 1935 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.<sup>4</sup> 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, 43.9% of participants 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 35.7% of those exposed to the undeclared citizen vignette. Second, 40.0% 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% of those viewing the undeclared citizen vignette. And third, 39.4% of respondents viewing the declared supporter vignette believed it would be “easy” or “very easy” to obtain employment, compared to only 18.3% 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.<sup>5</sup> 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.”<sup>6</sup> 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.<sup>7</sup>

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 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.”<sup>8</sup> A 2012 survey in rural Northeast Brazil suggests local politicians reach even relatively isolated households.<sup>9</sup> Over 81% 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% placed political flags or banners on their homes, 22% displayed campaign paraphernalia while attending a rally, and 19% 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.”<sup>10</sup> In the 2012 survey, almost two-thirds of respondents believed others would remember who placed campaign flags on their homes—as did 72% of citizens engaging in this type of declaration. In addition, over half of respondents believed others would remember rally attendees—as did 68% 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.

## Model

### *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.<sup>11</sup> This approach is tailored to the experimental design discussed in *Experimental Design*, 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.<sup>12</sup>

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) 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.<sup>13</sup> Second, candidates distribute rewards ( $r_A$  and  $r_B$ ) after they are elected, to citizens observed (with probability  $\gamma$ ) to declare support for them during the campaign. This assumption builds on evidence from our interviews,<sup>14</sup> as well as the discussion in *Motivating Evidence*. Third, because politicians in some countries employ negative inducements (Mares & Young, 2016), the model allows candidates to impose punishments ( $p_A$  and  $p_B$ ) on citizens observed to declare support for the opponent.<sup>15</sup> 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.<sup>16</sup> For example, 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; for example, a maid explained she did not declare because the other candidate's supporters would "complain a lot ...they fight, they get angry."<sup>17</sup> 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,  $\delta x_i$  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  $\alpha$ , which may equal zero—and given there are two candidates, *B*'s (*A*'s) probability of victory declines by  $\alpha$ .<sup>18</sup>

### 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

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

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 ( $\gamma$ ), and each candidate's probability of victory given the citizen's declaration ( $q + \alpha$  for *A*

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

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

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 regard 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  $\delta$ ). 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 ( $\gamma r_A$ ) 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 ( $\gamma p_B$ ). The third term ( $\delta x_i$ ) 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

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

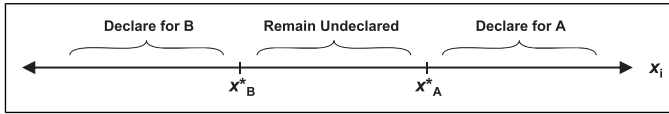
Comparing equations (1) and (2),  $\alpha$  is now subtracted (added) when weighting the consequences of  $A$ 's ( $B$ 's) victory, as the citizen reduces  $A$ 's (increases  $B$ 's) probability of victory when declaring for  $B$ . The sign of the expressive utility term ( $\delta x_i$ ) is negative 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 is 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 ( $q$ ).

### 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.<sup>19</sup> 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



**Figure 2.** Optimal Behavior as a Function of Ideological Preferences.

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.

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 ( $x_A^*$ ) represents a citizen whose ideological preferences make her indifferent between declaring for *A* and remaining undeclared. The left cutpoint ( $x_B^*$ ) represents a citizen whose ideological preferences 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  $x_B^* < x_A^*$ . All citizens to the right of  $x_A^*$  (i.e., who prefer *A* more strongly than  $x_A^*$ ) declare support for *A*. By contrast, all citizens to the left of  $x_B^*$  (i.e., who prefer *B* more strongly than  $x_B^*$ ) declare support for *B*. Citizens between  $x_A^*$  and  $x_B^*$  remain undeclared. Depending on contextual characteristics—that is, on values of model parameters—both cutpoints may represent supporters of the same party (e.g.,  $x_A^* > x_B^* > 0$ ). In such cases, clientelist considerations dominate instrumental and expressive considerations for some citizens, who declare for the candidate they dislike.<sup>20</sup>

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

$$\begin{aligned} x_A^* &= \frac{c_A - (q + \alpha)\gamma r_A + (1 - q - \alpha)\gamma p_B}{\alpha + \delta} \\ x_B^* &= \frac{-c_B + (1 - q + \alpha)\gamma r_B - (q - \alpha)\gamma p_A}{\alpha + \delta} \end{aligned} \quad (3)$$

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

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

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):

**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  $x_A^* > 0$ , decrease if  $x_A^* < 0$ , and are unaffected if  $x_A^* = 0$ . *B*'s declarations increase if  $x_B^* < 0$ , decrease if  $x_B^* > 0$ , and are unaffected if  $x_B^* = 0$ .

**H11 *Election Influence*:** As the election influence of declaring increases, *A*'s declarations increase if  $x_A^* > \psi$ , decrease if  $x_A^* < \psi$ , and are unaffected if  $x_A^* = \psi$  (where  $\psi = -\gamma(r_A + p_B)$ ). *B*'s declarations increase if  $x_B^* < \theta$ , decrease if  $x_B^* > \theta$ , and are unaffected if  $x_B^* = \theta$  (where  $\theta = \gamma(p_A + r_B)$ ). Non-declarations increase if  $x_A^* - x_B^* < \phi$ , decrease if  $x_A^* - x_B^* > \phi$ , and are unaffected if  $x_A^* - x_B^* = \phi$  (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 ( $\gamma$ ) 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  $x_A^*$ ; recall this threshold indicates indifference between declaring for *A* and remaining undeclared. If he is a supporter of *A* (i.e.,  $x_A^* > 0$ ), a marginal increase in  $\delta$  increases his expressive gain from declaring; and a marginal increase in  $\alpha$  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  $x_A^*$ . On the other hand, if *j* is a supporter of *B* (i.e.,  $x_A^* < 0$ ), a marginal increase in  $\delta$  increases his expressive

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

Parameter Change	Rewards Only			Rewards and Punishment		
	Declare A	Declare B	Undeclared	Declare A	Declare B	Undeclared
Monopolistic Clientelism						
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						
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

loss from declaring. Thus, he becomes strictly worse off declaring for  $A$ , meaning another citizen—who more weakly supports  $B$ —lies on the threshold  $x_A^*$ . Regarding H11, a marginal increase in  $\alpha$  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  $x_A^*$ .

### 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 & Camerer, 1994; Levine & 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 “better” alternatives. That is, in their randomization, they place more (less) weight on actions providing higher (lower) payoffs.

*Experimental Results* 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.<sup>21</sup>

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

**Table 3.** Parameters for Experimental Design.

Treatment	$r_A$	$q$	$c_A$	$\Gamma$	$p_A$	$r_B$	$\delta$	$\alpha$	$c_B$
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

Red text indicates parameters differing from *Baseline Clientelism*.

sensitivity of choices to relative payoffs.<sup>22</sup> Given the stochastic model's ambiguity, the experimental section exclusively tests the deterministic model for these four hypotheses.

## 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 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.<sup>23</sup>

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 1259 participants from 809 municipalities. To recruit participants, we broadcast advertisements on Facebook in October–December 2016, following an established strategy employed in Brazil (Samuels & 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.<sup>24</sup> A 2019 survey by the Latin American Public Opinion Project found that 60% of

Brazilian respondents had a Facebook account, of which 92% 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.<sup>25</sup> 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 inclusion criterion captures 98.3% of all municipalities, with 59.7% of Brazil's population. Of 2433 citizens who consented to participate, 16.6% exited before the experiment began, and an average of 3.5% exited after each round, yielding 1259 participants who completed all 10 rounds. The Supplemental Information shows findings are robust to including subjects who terminated the survey early, and finds some evidence of differential attrition.<sup>26</sup> 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% of participants thought clientelist benefits were distributed "frequently" or "very frequently" by candidates in their municipalities, and 14.4% 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.<sup>27</sup> Facebook users clicking on our advertisement were redirected to a consent page and then commenced the experiment. Following [Berinsky et al. \(2014\)](#), 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 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*.<sup>28</sup>



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.<sup>29</sup> 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 versus 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 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 10 distinct treatments, each involving a fictitious election with different contextual characteristics. Using a within-subject design, each participant was exposed to all 10 treatments.<sup>30</sup> 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 10 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 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%, 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.<sup>31</sup> Testing Hypothesis 4, *Low Monitoring* adapts *Baseline Clientelism* to consider the case in which the clientelist candidate has a lower ability to observe declarations.<sup>32</sup> Next, we employ *Punishment and Reward* and *Punishment Only* to investigate effects when the clientelist candidate, if elected, punishes citizens who declared for the opposition—testing Hypothesis 5. *Punishment and 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.<sup>33</sup> 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 *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.

## Experimental Results

Each of 1259 participants who completed the experiment made 10 declaration decisions. Figure 3 presents a descriptive overview of declaration decisions, by partisan type, for the 10 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 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.<sup>34</sup> 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 utility 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  $x_A^*$  and  $x_B^*$  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% in *Baseline Clientelism* to 37.1% in *No Clientelism* ( $p = .01$ ), declarations for *B* increase from 30.5 to 33.8% ( $p = .04$ ), and non-declarations increase from 23.5 to 29.1% ( $p = .01$ ). 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.

Table 4. Declaration Choices by Treatment.

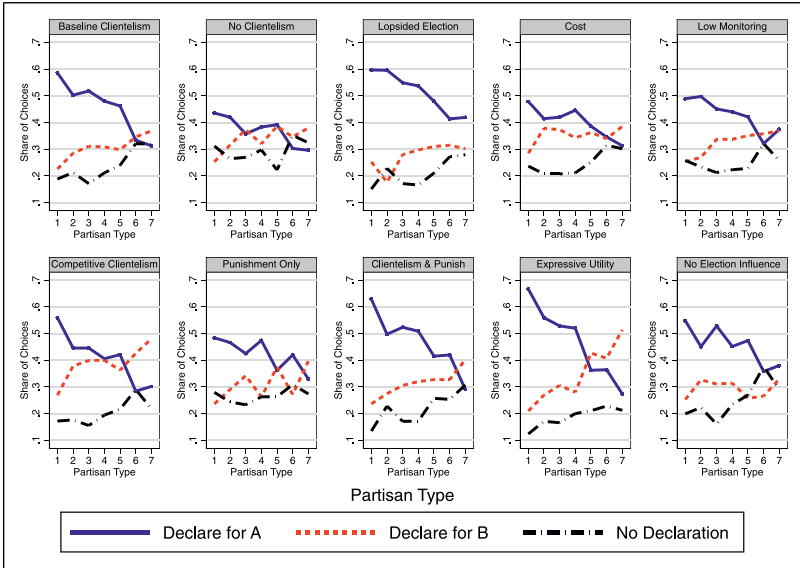
Treatment	Proportion	Difference	p-value	Deterministic Prediction	Stochastic Prediction
Panel A: Declaration for Clientelist Candidate A					
Baseline Clientelism	0.460	Baseline	Baseline	Baseline	Baseline
No Clientelism	0.371	−0.089	.001	Decreases ✓	Decreases ✓
Lopsided Election	0.516	0.056	.002	Increases ✓	Increases ✓
Cost	0.402	−0.058	.002	Decreases ✓	Decreases ✓
Low Monitoring	0.431	−0.029	.069	Decreases [X]	Decreases [X]
Punishment Only	0.424	0.053	.003	No effect X	Increases ✓
Clientelism and Punishment	0.472	0.048	.008	Increases ✓	Increases ✓
Competitive Clientelism	0.412	−0.048	.008	No effect X	Decreases ✓
Expressive Utility	0.473	0.013	.739	Decreases X	—
No Election Influence	0.458	−0.002	.468	Increases X	—
Panel B: Declaration for Opposition Candidate B					
Baseline Clientelism	0.305	Baseline	Baseline	Baseline	Baseline
No Clientelism	0.338	0.033	.037	No effect X	Increases ✓
Lopsided Election	0.274	−0.031	.043	No effect X	Decreases ✓
Cost	0.353	0.048	.006	No effect X	Increases ✓
Low Monitoring	0.323	0.018	.162	No effect ✓	Increases X
Punishment Only	0.311	−0.028	.068	Decreases [X]	Decreases [X]
Clientelism and Punishment	0.312	0.002	.466	No effect ✓	Decreases X
Competitive Clientelism	0.387	0.082	.001	Increases ✓	Increases ✓
Expressive Utility	0.342	0.037	.025	Increases ✓	—

(continued)

Table 4. (continued)

Treatment	Proportion	Difference	p-value	Deterministic Prediction	Stochastic Prediction
No Election Influence	0.295	−0.010	.714	Decreases X	—
Panel C: No Declaration					
Baseline Clientelism	0.235	Baseline	Baseline	Baseline	Baseline
No Clientelism	0.291	0.056	.001	Increases ✓	Increases ✓
Lopsided Election	0.210	−0.025	.063	Decreases X	Decreases X
Cost	0.245	0.010	.272	Increases X	Increases X
Low Monitoring	0.246	0.011	.257	Increases X	Increases X
Punishment Only	0.265	−0.025	.077	Increases X	Increases X
Clientelism and Punishment	0.216	−0.049	.002	Decreases ✓	Decreases ✓
Competitive Clientelism	0.201	−0.034	.019	Decreases ✓	Decreases ✓
Expressive Utility	0.186	−0.049	.001	Decreases ✓	—
No Election Influence	0.247	0.012	.758	Decreases X	—

Total, 1259 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 X if at the .10 level; X otherwise. All results are robust to using two-tailed tests, with the exception of No Clientelism in Panel B (p-value: .073) and Lopsided Election in Panel B (p-value: .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.



**Figure 3.** Declaration Choices of Participants, by Treatment. *Note:* Figures reflect the share of experimental participants ( $N = 1259$ ) 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.

### *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% in *Baseline Clientelism* to 51.6% in *Lopsided Election* ( $p = .01$ ), *B*'s declarations decrease from 30.5 to 27.4% ( $p = .04$ ), and non-declarations decrease from 23.5 to 21.0% ( $p = .06$ ). 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% 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 & Sprague, 1995; Makse & 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% in *Baseline Clientelism* to 40.2% in *Cost* ( $p = .01$ ), *B*'s declarations increase from 30.5 to 35.3% ( $p = .01$ ), and non-declarations increase from 23.5 to 24.5% ( $p = .27$ ). 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 regard 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% in *Baseline Clientelism* to 43.1% in *Low Monitoring* ( $p = .07$ ), *B*'s declarations increased from 30.5% to 32.3% ( $p = .16$ ), and non-declarations increased from 23.5% to 24.6% ( $p = .26$ ). 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, only the deterministic model predicts the observed insignificant effects on *B*'s declarations, and neither model predicts the observed insignificant effects on 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 & 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 & Ho, 1994; Tversky & Kahneman, 1992), then they may perceive minimal differences between the two treatments.<sup>35</sup>

## 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.<sup>36</sup> As shown in Table 4, *A*'s declarations increase from 37.1% in *No Clientelism* to 42.4% in *Punishment Only* ( $p = .01$ ), *B*'s declarations decrease from 33.8 to 31.1% ( $p = .07$ ), and non-declarations decrease from 29.1 to 26.5% ( $p = .08$ ). 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 & 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% in *Punishment Only* to 47.2% in *Clientelism and Punishment* ( $p = .01$ ), *B*'s declarations are virtually unchanged, and non-declarations decrease from 26.5 to 21.6% ( $p = .01$ ). 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% in *Baseline Clientelism* minus 37.1% in *No Clientelism*), while the punishment  $p_A$  decreases *B*'s declarations by 2.8 percentage points (31.1% in *Punishment Only* minus 33.8% 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% in *Baseline Clientelism* to 41.2% in *Competitive Clientelism* ( $p = .01$ ), *B*'s declarations increase from 30.5 to 38.7% ( $p = .01$ ), and non-declarations decrease from 23.5 to 20.1% ( $p = .02$ ). 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 regard to Hypothesis 9, we can also test predictions about the relative effectiveness of rewards offered by competing candidates with comparable political support.<sup>37</sup> 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% in *Baseline Clientelism* minus 37.1% in *No Clientelism*), while increasing  $r_B$  raises *B*'s declarations by 8.2 percentage points (38.7% in *Competitive Clientelism* minus 30.5% in *Baseline Clientelism*). As expected, the difference between the two treatment effects is statistically insignificant using a chi-square test ( $p = 0.75$ ).

### 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.<sup>38</sup> 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% in *Baseline Clientelism* vs. 47.3% in *Expressive Utility*,  $p = 0.74$ ), non-declarations

decrease (23.5% vs. 18.6%,  $p = 0.01$ ), and  $B$ 's declarations increase (30.5% vs. 34.2%,  $p = 0.03$ ).

While research suggests that many US survey respondents believe lawn signs can influence votes (Makse & 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 decrease 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% in *Baseline Clientelism* vs. 45.8% in *No Election Influence*,  $p = 0.47$ ), non-declarations (23.5% vs. 24.7%,  $p = 0.76$ ), or  $B$ 's declarations (30.5% vs. 29.5%,  $p = 0.71$ ). While not dispositive, one interpretation of these surprising results is that voters ignore their declarations' potential impact on 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.<sup>39</sup> 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.<sup>40</sup> These covariates were described in *Experimental Design*. 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 (0–2), 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

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

Treatment	Declare for A		Declare for B		No Declaration	
	(1)	(2)	(3)	(4)	(5)	(6)
No Clientelism	-.088** (0.017)	-.115** (0.020)	.032* (0.015)	.044* (0.020)	.056** (0.015)	.095** (0.023)
Lopsided Election	.058** (0.016)	.076** (0.020)	-.032* (0.015)	-.045* (0.020)	-.025 (0.013)	-.043* (0.022)
Cost	-.058** (0.016)	-.077** (0.020)	.047** (0.016)	.066** (0.020)	.010 (0.014)	.018 (0.022)
Low Monitoring	-.027 (0.017)	-.035 (0.020)	.016 (0.015)	.022 (0.020)	.011 (0.014)	.019 (0.022)
Competitive Clientelism	-.046** (0.016)	-.061** (0.020)	.080** (0.016)	.112** (0.021)	-.034** (0.013)	-.058** (0.021)
Expressive Utility	.013 (0.016)	.017 (0.020)	.037* (0.015)	.051* (0.021)	-.049** (0.013)	-.084** (0.021)
No Election Influence	-.001 (0.016)	-.001 (0.020)	-.011 (0.015)	-.015 (0.020)	.012 (0.013)	.020 (0.022)
Round	.007** (0.002)	.010** (0.002)	-.007** (0.001)	-.011** (0.002)	.000 (0.001)	.001 (0.002)
Partisan Type	.004** (0.000)		-.002** (0.000)		-.002** (0.000)	
Screeners	.013 (0.010)		-.025** (0.009)		.012 (0.009)	
Subject Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	10072	7656	10072	7192	10072	5888

\*  $p < .05$ , \*\*  $p < .01$ . 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).

Treatment	Declare for A		Declare for B		No Declaration	
	(1)	(2)	(3)	(4)	(5)	(6)
Punishment Only	.053** (0.019)	.152** (0.034)	-.027 (0.019)	-.094** (0.036)	-.026 (0.018)	-.098* (0.039)
Round	.007* (0.003)	.013 (0.008)	-.010** (0.003)	-.023** (0.008)	.003 (0.003)	.007 (0.009)
Partisan Type	.002** (0.001)		-.002** (0.001)		-.000 (0.000)	
Screeners	-.009 (0.011)		-.026* (0.011)		.034** (0.010)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2518	862	2518	746	2518	648

\*  $p < .05$ , \*\*  $p < .01$ . 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 and Punishment (Logit).

Treatment	Declare for A		Declare for B		No Declaration	
	(1)	(2)	(3)	(4)	(5)	(6)
Clientelism and Punishment	.047* (0.020)	.133** (0.033)	.002 (0.018)	.006 (0.035)	-.049** (0.017)	-.198** (0.039)
Round	.009** (0.003)	.036** (0.007)	-.009** (0.003)	-.039** (0.008)	-.001 (0.003)	-.005 (0.010)
Partisan Type	.004** (0.001)		-.002** (0.000)		-.001** (0.000)	
Screeners	-.005 (0.011)		-.025* (0.011)		0.029** (0.010)	
Subjects Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	2518	896	2518	780	2518	628

\*  $p < .05$ , \*\*  $p < .01$ . 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.

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 all tests, but is only statistically significant in a logistic regression with subject fixed effects.<sup>41</sup> 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).<sup>42</sup>

## 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 1259 participants across Brazil. Subjects responded as predicted in numerous treatments, involving shifts in 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 regard 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 regard 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 & 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.<sup>43</sup> 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 ([Szwarcberg, 2012](#); [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 earnings.<sup>44</sup> 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.

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### Notes

1. As discussed, our analyses are robust to—and can be used to examine—campaign handouts.
2. The Supplementary Information describes this fieldwork.
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). Replication materials and code can be found at [Nichter and Nunnari \(2021\)](#).
4. *Experimental Design* discusses survey recruitment and representativeness. This survey experiment has a larger sample size as it includes additional participants recruited similarly.
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.

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 6000 and 324,000.  $N = 3643$  to 3674.
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.
12. The Supplementary Information is available at: [https://snunnari.github.io/nn\\_declaredsupport\\_onlineapp.pdf](https://snunnari.github.io/nn_declaredsupport_onlineapp.pdf).
13. 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.
14. The Supplementary Information describes this fieldwork.
15. One interpretation of punishments is terminating benefits that a declarer would have received, had she remained undeclared.
16. 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 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.
17. 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).
18. 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.
19. More precisely, we assume the numerator of the right-hand side of equation (4) is positive.
20. 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; that is, the probability a citizen supports  $A$  ( $B$ ) conditional on declaring for  $A$  ( $B$ ) exceeds 50%. For example, when  $x_A^* < 0$ , *all*  $A$  supporters declare for  $A$ , but only *some*  $B$  supporters do.
21. 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.
22. The Supplementary Information derives these predictions.

23. See Footnote 3 for pre-registration details.
24. *Financial Times*, 5/15/2017; [www.statista.com/statistics/268136/top-15-countries-based-on-number-of-facebook-users](http://www.statista.com/statistics/268136/top-15-countries-based-on-number-of-facebook-users); IBGE, 2017.
25. Research suggests online samples tend to be disproportionately more educated in various countries (e.g., Boas et al., 2020).
26. Attrition is significantly higher among younger respondents. Less-educated and female respondents also have higher attrition, but only if excluding controls.
27. 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.
28. 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).
29. The Supplemental Information includes examples of vignettes and their graphical representation.
30. 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).
31. 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*.
32. 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 versus 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.
33. The experiment does not test Hypothesis 7 (punishments by *B*).
34. Tests of Hypotheses 8 and 9 compare two treatment effects; they are thus excluded from Table 4 but discussed below.
35. Given probability distortion, they may underweight the probability of receiving a reward conditional on declaring in *Baseline Clientelism* and overweight it in *Low Monitoring*.
36. 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*.
37. In the treatments enabling this test,  $q = 1/2$ .
38. As discussed in *Stochastic Choice*, we focus on the deterministic model for these factors, given its homogeneous predictions across partisan types (unlike the stochastic model).

39. The Supplementary Information also includes a game-theoretic model.
40. Findings are also robust when using multinomial logit.
41. As shown in Table 4, the  $p$ -value for a difference in proportions Z test is 0.063.
42. 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.
43. A lab-in-the-field study could employ more detailed instructions and reduce attrition.
44. These findings are significant at the 5% level in a regression with various controls and state/partisan type fixed effects.

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