How Clientelism Works: Evidence from the Barinas Special Election

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Abstract
Do politicians target the benefits of social programs to party loyalists or to swing voters? Traditional tests of this question are clouded by an identification problem caused by the simultaneity of politician and voter choices to participate in the exchange of assistance for votes. I use the holding of an unanticipated repeat gubernatorial election in the Venezuelan state of Barinas in January 2022 as a natural experiment to identify the effects of elections on the distribution of government assistance. I estimate that the holding of the election led to a more than doubling in the number of food packages received by households in Barinas relative to the control group of households in the neighboring state of Apure. I also find that moderate opposition voters and voters who claimed to have supported third party candidates received the largest level of benefits, consistent with the predictions of the spatial model of distributive politics.

Keywords: clientelism, distributive politics, Venezuela, natural experiments.

1 Introduction
How do politicians decide who benefits from social programs? The spatial model of voting suggests that if parties can trade access to publicly provided benefits in exchange for political support, they will direct benefits towards electorally pivotal constituencies (Dixit and Londregan, 1996; Lindbeck and Weibull, 1987). The result follows directly from the logic of spatial electoral competition. Just as politicians can set platforms to sway voters who are nearly undecided, they can also use economic resources to provide benefits to those voters who would be likely to vote for their opponents in their absence.

The empirical evidence on whether politicians target publicly provided goods and transfers to swing voters is at best mixed. While there is some evidence that politicians may target non-conditional (pork-barrel) transfers towards swing districts (Berry et al., 2010; Wright, 1974), studies of individualized

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particularistic transfers invariably tend to find that party loyalists are overrepresented among their recipients. Stokes et al. (2013), for example, show that government sympathizers were much more likely to be the recipients of government gifts in Argentina, India, Mexico and Venezuela. Using survey data from Latin America and Africa, Nichter and Peress (2017) found that respondents who identified with parties that were involved in moderate or major clientelistic efforts were significantly more likely to receive handouts. Díaz-Cayeros et al. (2016) find that targeted particularistic transfers through Mexico’s Pronasol program were around 50 percent more likely to go to government-party controlled districts than to contested districts (and around twice as likely as to opposition-controlled districts). Using survey data from Northeast Brazil, Nichter (2018) finds that respondents who declared support for a victorious candidate were three times more likely than non-declarers to receive private benefits from the politician the following year.¹

Evaluating whether politicians seek to direct targeted benefits to their constituents poses a formidable empirical identification problem. As Nichter (2018) has highlighted, clientelar relations reflect bilateral decisions by parties and voters to engage in an exchange of votes for transfers. The receipt of a clientelar benefit thus reflects both a decision by a politician to offer the benefit to a voter and a decision by the voter to accept the offer. While political parties may seek to condition access to benefits on voter characteristics, these characteristics will also influence the willingness of voters to seek access to these programs. A cross-sectional correlation between partisanship and program access may indicate that governments are targeting benefits to political loyalists or that loyalists are more likely to seek access to programs launched and run by parties that they support.

The observed correlation between partisanship and receipt of transfers tells us very little about who politicians are targeting benefits to for the same reason that the observed correlation between prices and quantities in the market for apples tells us very little about how prices affect the willingness of apple growers to grow and sell apples. Like the exchange of apples for money, the exchange of votes for social benefits is a result of the decision by a buyer (government) and a seller (voter) to participate in an exchange. It is impossible to test comparative statics implications about how recipient characteristics affect the willingness of the government to offer clientelar benefits without an adequate identification strategy to ensure that we are estimating the demand, and not the supply, of votes. The empirical tests of theories of clientelism quoted above rely on assessing whether loyalists or swing voters are more likely to be the recipients of targeted transfers. Yet, as I show in section 2, self-selection of government loyalists into government programs can make this correlation positive even if the government is not targeting a specific set of voters. Furthermore, the correlation between the share of program recipients that are government sympathizers and the magnitude of the program can be negative or positive depending on whether the variations are produced by shocks to politician or voter preferences.

In this paper, I tackle this identification problem by using evidence from a natural experiment: the holding of an unanticipated repeat election in the Venezuelan state of Barinas in January 2022. I use a difference-in-differences approach to evaluate how the holding of the election affected the distribution of food packages through the government’s food assistance program, the Local Committees of Production and Supply (Comités Locales de Abastecimiento y Producción, CLAPs) to Barinas voters in comparison to a control group of households in the neighboring state of Apure.

The unanticipated nature of the Barinas election is key to my identification strategy. The difference-

¹Not surprisingly, results vary across levels of development and institutional setting. Chen (2013) finds no evidence that FEMA hurricane aid on Florida was targeted to Republicans during the George W. Bush presidency. Mares and Young (2018) argue that negative inducements implemented through threats to withdraw core voters’ existing entitlements were prevalent in Hungary’s 2014 parliamentary elections. Calvo and Murillo (2004) argue that patronage as a mechanism of redistribution benefits political parties with low-skilled constituencies, such as Argentina’s Peronists.
in-differences approach relies on using changes over time in the trends of the outcome variable before and after treatment and thus presumes similarity of the treatment and control group in the pre-treatment period - which in our case is the campaign for the November 2021 elections. Yet if the government had known that a repeat election would ultimately be held only in Barinas, then it would have likely allocated packages differently in Barinas than in a state where it did not expect there to be another election.²

This paper adds to a small literature that studies the effect of repeat elections on political behavior. Bartels, Horowitz and Kramon (2021) study how the Kenyan Supreme Court’s invalidation of the incumbent’s victory in a 2017 election and its upholding of his repeat-election affected public opinion of the judiciary. Waddilove (2021) uses the same event to study the differences in mobilization by local political leaders during the re-run. Coşkan, Baysu and Koc (2023) consider a 2015 parliamentary election in Turkey that was repeated due to the failure to form a coalition and find that it had differential effects on the motivations driving collective action for secular, liberal and leftist groups. Yarchi and Samuel-Azran (2023) use a period of three consecutive elections in Israel to study the determinants of political efficacy among voters. Lundmark, Oscarsson and Weissenbilder (2010) estimate that the repeat of a regional election in Sweden in 2010 due to procedural mistakes in the vote handling led to a short-term decline in voters’ confidence in the electoral authority.

To the best of my knowledge, this is the first paper to use a repeat election to estimate treatment effects by comparing the evolution of political variables in the jurisdiction in which the election is repeated relative to a control group of voters living in a jurisdiction where it is not repeated. Previous studies have instead focused on the changes in political behavior by comparing outcomes before and after the holding of repeat elections. Such an approach implicitly assumes that observed levels or trends of outcomes prior to the repeat elections can serve as an adequate counterfactual of what would have happened if the election had not been repeated. My approach, in contrast, directly estimates the counterfactual from the data on political outcomes in an explicit control group formed by voters who live in a jurisdiction where no repeat election is held.

The rest of the paper proceeds as follows. Section 2 presents a theoretical model of the supply and demand of clientelism and shows that the signs of the correlations typically used to test theories of clientelism will depend on whether the identifying shocks come from the demand or supply side of targeted benefits. Section 3 discusses the background of the CLAP program and the electoral dispute that led to the holding of the Barinas special election. Section 4 presents my econometric estimates and section 5 discusses the key implications of these findings for the literature.

2 Theoretical Framework

Consider a simple model of clientelism in which the government offers every voter a benefit \( b > 0 \) conditional on entering a binding agreement to vote for the incumbent candidate. There are two parties, government and opposition, indexed by \( j = \{g,o\} \), and a continuum of voters, indexed by \( i \) and distinguished by their ideal policy \( \pi_i \). Voters’ utility is additive in the monetary benefits from the government transfer and the squared Euclidean distance between the policy of the party in power and the voter’s ideal policy. Letting \( \pi_j, j = \{g,o\} \) denote the policies that parties implement once they are in power, voter \( i \) will choose to vote in favor of the government party if:

\[
U_{ig} = - (\pi_i - \pi_g)^2 + b > U_{io} = - (\pi_i - \pi_o)^2. \tag{1}
\]

²For the same reason, we cannot simply focus on differences between electoral and non-electoral periods. As Nichter (2018) has convincingly argued, clientelar relations are long-term relations in which the delivery of benefits during non-electoral periods may serve to fulfill the commitments made by politicians during the elections.
Note that every voter who decides to vote for the government will also decide to accept the government’s offer of \( b \), as the utility of voting for the government and receiving the transfer, \(- (\pi_i - \pi_g)^2 + b\) is greater than the utility of voting for the government without receiving the benefit, \(- (\pi_i - \pi_g)^2\). It also follows that any voter who does not vote for the government must also have rejected the transfer, since accepting the transfer would have implied entering into a binding commitment to vote for the government. Therefore equation (1) separates both those who vote for the government from those who vote for the opposition and those who receive the transfer from those who do not.

Assume without loss of generality that \((\pi_o > \pi_g)\). After some algebra, (1) becomes:

\[
\pi_i < \frac{\pi_g + \pi_o}{2} + \frac{b}{2(\pi_o - \pi_g)}.
\]  

(2)

The first term on the right-hand side of (2) is the midpoint between the policy positions of the government and opposition party. In the standard Downsian model of electoral competition, this threshold separates those who vote for one party from those who vote for the other (e.g., Roemer, 2006, Chapter 1). The second term on the right-hand side of (2) represents the additional contribution of the government’s offer to swaying voters. A voter who would otherwise have been ideologically closer to the opposition may be swayed to vote for the government as long as this value is greater than the deviation between the voter’s ideal point and the midpoint between the parties’ positions.

Define those individuals whose ideal policy is closer to that of the government party as government sympathizers and those whose ideal policy is closer to that of the opposition as opposition sympathizers. Denote the fraction of sympathizers of party \( j \) that are receiving benefits as \( s^b_j \) and the fraction of program beneficiaries that are sympathizers of party \( j \) as \( s^b_j b \). Note that both of these shares have been used in the literature to test theories of clientelism. For example, Stokes et al. (2013, Chapter 2) show that the \( s^b_j \) of government sympathizers is systematically higher than that of non-sympathizers in Argentina, Venezuela, Mexico and India and argue that this is inconsistent with the spatial competition model of clientelism. Díaz-Cayeros et al. (2016, Chapter 4), in contrast, regress per capita transfers on a municipality-level measure of the share of core government support in the population of program beneficiaries \( s^b_j b \) and argue that a positive estimated coefficient lends strong support to the core voter model of clientelism.

I now establish:

**Proposition 1.** \( s^b_g \geq s^b_o \) with the strict inequality holding as long as there are some voters in the population that do not receive benefits.

See Appendix A for proofs.

Proposition 1 captures the essence of the self-selection problem. Note that to this point I have said nothing about how the government chooses the benefits level. All we know is that it makes an untargeted offer of \( b \) to all voters in exchange for a binding commitment to support the government candidate. Nevertheless, all government sympathizers will decide to accept this offer, while only some opposition sympathizers will. The simple reason is that for those who would have voted for the government anyway, there is no cost of committing to do so, so there is no reason to turn down the offer of compensation in exchange for their votes.

This result suggests that we need to be extremely cautious in interpreting data on the partisanship of government program beneficiaries as indicating anything about the validity of theories of how governments target social benefits. Even if there is no government targeting of these programs, there are reasons to expect that government sympathizers will be among those most likely to seek to receive these benefits.

I now turn to the discussion of the \( s^b_j \), the shares of sympathizers of different parties among program beneficiaries. To do so, I impose additional structure on voter preferences and model explicitly how the government decides to set benefit levels. This will allow us to understand how shocks to voter and politician preferences affect the political composition of the population of program beneficiaries.
Let voter preferences over policies be equal to the sum of a constant $\alpha$ and an individual-specific policy parameter $\eta_i$ distributed according to the density $f(\eta)$ over the $[0,1]$ interval:

$$\pi_i = \alpha + \eta_i.$$  

We can think of $\alpha$ as an aggregate preference parameter indicating how close voters are to the preferences of either party. When $\alpha$ increases, voters’ ideal points will rise, driving them closer to the ideal policies of the opposition.\(^3\) I assume that $\eta_i$ is distributed according to the density $f(\eta)$ over the $[0,1]$ interval.

Let $P$ denote the government’s vote share. Note that this will be the same as the share of program beneficiaries in the population, that is:

$$P = \int_{\eta}^{\hat{\eta}} f(\eta) \, d\eta.$$ (4)

where

$$\hat{\eta} = \frac{\pi_g + \pi_o}{2} + \frac{b}{2(\pi_o - \pi_g)} - \alpha.$$ (5)

We can express (4) and (5) more compactly as $P(\pi_g, \pi_o, b, \alpha)$ I refer to this function as the supply of votes as it captures voters’ decision to offer their votes in exchange for receiving government-provided benefits.

The government will set $b$ to maximize its objective function which is a weighted average of the vote share and a quadratic loss function representing the opportunity cost of not using the funds invested in clientelism for other public policy purposes.

$$V = P - \theta (bP)^2.$$ (6)

Denote the solution of maximizing $V$ in (6) as:

$$b(\theta, \pi_g, \pi_o, \alpha) = \text{Argmax}_b V(\theta, b, P(b, \pi_o, \pi_g, \alpha)).$$ (7)

I call this function the demand for votes as it captures the determinants of the policymaker’s decision to offer benefits in exchange for votes.\(^4\)

Consider now the share of government sympathizers among all program beneficiaries. This will be equal to:

$$s^g_b = \frac{\int_{\eta}^{\hat{\eta}} f(\eta) \, d\eta}{P},$$ (8)

where

$$\hat{\eta} = \frac{\pi_g + \pi_o}{2} - \alpha.$$ (9)

**Proposition 2.** Increases in $\alpha$ and $\theta$ will both lead to declines in the government’s vote share $P$ but may have effects of opposite sign on the share of government sympathizers receiving benefits, with $\frac{ds}{d\alpha} \leq 0$ and $\frac{ds}{d\theta} > 0$.

\(^3\)Even if a voter’s initial ideal point $\pi_i$ is higher than that of the opposition, $\pi_o$ and thus an increase in $\alpha$ drives them farther from the position of both parties, there will be a greater increase in its squared Euclidean distance from the government than from the opposition.

\(^4\)There are two important caveats. One is that the distinction between demand and supply is purely semantic in an exchange economy (see Mas-Colell et al., 1995, p. 515-525). Thus I could also have referred to $P(.)$ in (4) and (5) as the demand for clientelism and $b(.)$ in the solution to (6) as as the supply of clientelism. A second caveat is that my model differs from the traditional atomistic supply and demand framework in that politicians internalize the effects of their choice of $b$ on voter decisions. Therefore, my model is similar to that of a monopsonistic price-setting firm. Whether 7 can be called a demand function or simply an input function in that setting is also the source of some controversy but ultimately also semantic (see, e.g., Rabianski and Stone, 1989). I opt for labeling it as a demand function to capture the intuition that it arises from a solution to the policymaker’s optimization problem.
Corollary. Shocks to policymaker preferences will generate negative co-movements between program participation and the share of government sympathizers in program beneficiaries, while shocks to voter preferences may induce positive co-movements between the same variables. See Appendix A for proofs.

These results indicate why it may be futile to attempt to test theories of clientelism by looking at either the cross-sectional or time-series correlation between program participation and the political preferences of program participants. As Proposition 1 shows, the fact that government loyalists are over-represented among program beneficiaries tells us nothing about who the government is targeting, since it arises from the fact that government sympathizers would naturally be more willing to accept an offer from a candidate that they were planning to vote for even in the absence of any monetary inducement. And, as Proposition 2 shows, increases in program participation either across localities or over time can be either positively or negatively associated with changes in the share of government sympathizers participating in the program, depending on whether they arise from variations in government decisions or in voter preferences.5

One way to think about \( \alpha \) and \( \theta \) are as shocks to the supply and demand for votes. Increases in \( \alpha \) induce changes in program participation even with no changes in the government offer of services, while increases in \( \theta \) induce changes in program participation even with no changes in participant preferences. For the purpose of evaluating theories of how governments target groups of voters through clientelar transfers, we are primarily interested in isolating the effect of changes in politician preferences on the distribution of transfers, and thus should seek to isolate the effects of shocks to \( \theta \) while controlling for shocks to \( \alpha \).6

My empirical strategy seeks to do this by focusing on a natural experiment that led to an increase in the government’s interest in buying votes among beneficiaries in the treatment group relative to a control group. I argue that the holding of an unanticipated election in Barinas constitutes such a natural experiment and thus serves as an example of a shock to \( \theta \). Our ability to measure the distribution of food assistance during the period preceding the special election allows us to have a reasonable baseline from which to compare changes in food assistance in the treatment and control groups using a difference-in-differences specification.

3 Background and Context

Venezuela held regional and mayoral elections on November 21, 2021. These marked the first time since 2017 that the mainstream opposition coalition, the Democratic Unity Roundtable (MUD), chose to participate in a national election, reversing its prior strategy of electoral boycotts. The elections counted with the participation of an electoral observation mission from the European Union which raised objections about the fairness of the process, but did not question the integrity of the vote count (EU EOM, 2022). The process was overseen by the National Electoral Council (CNE), two of whose five

5Stokes et al. (2013) consider the hypothesis that the overrepresentation of loyalist among benefit recipients may be a consequence of the endogeneity of political preferences. That is a distinct type of simultaneity problem than the one of selection by loyalists into government programs which I highlight in this section. While Stokes et al. (2013) argue that the large share of "certain loyal" voters among recipients - voters who would not change their support for the government even in the absence of benefits - is evidence against the endogeneity hypothesis, it is precisely these die-hard loyalists that we would expect to be most likely to self-select into the program.

6Note that my approach to identification is based on the comparative statics results of Proposition 2 derived from the reduced form and not on the use of supply shocks to recover the structural form of the demand function popularized by Wright (1928).
board members represent opposition parties.\textsuperscript{7}

Candidates from the governing United Socialist Party of Venezuela (PSUV) won the vote in 19 states, while MUD candidates prevailed in two states and a candidate from the Democratic Alliance (ADem), a coalition of centrist opposition parties, took one governorship. Preliminary results also put MUD candidate Freddy Superlano ahead of incumbent PSUV governor Argenis Chávez in the state of Barinas, the home of the late Venezuelan president and PSUV founder Hugo Chávez (brother to Argenis). However, on November 30, the Supreme Court suspended the final tallying of Barinas votes and ordered that a new election be held after determining that Superlano had been barred from running for office by the Comptroller General’s Office.

The repeat election was held on January 9. The PSUV ran former Foreign Minister Jorge Arreaza, a son-in-law of Hugo Chávez, while the MUD nominated state legislator Sergio Garrido. Garrido won the special election with a 14-point margin. Key to his victory was Garrido’s ability to sway many third-party voters and a dismal performance by third-party candidate Claudio Fermín, whose bid led to a split in the ADem coalition (Rodríguez, 2022a).\textsuperscript{8}

\begin{figure}[h]
\centering
\includegraphics[width=\linewidth]{figure1.png}
\caption{Electoral results in Barinas, 2021 and 2022 Gubernatorial Elections}
\end{figure}

Figure shows that while both the MUD and PSUV candidate gained vote share in the special election, the MUD saw a much greater increase, which suggests its ability to capture votes that had gone to third-party candidates in November. Source: CNE (2022)

Several journalistic accounts have provided anecdotal evidence that the government made significant efforts to improve delivery of publicly provided goods and services in Barinas during the runup to the January 9 special election. These include the distribution of food packages through Venezuela’s public

\textsuperscript{7}Although legal and constitutional restrictions preclude partisanship of CNE board members, in practice board members are appointed after intense political negotiations and are nominated by political party coalitions (EU EOM, 2022; Observatorio Electoral Venezolano, 2021)

\textsuperscript{8}Fermín counted with the support of some ADem parties, but key national figures from the ADem coalition including former presidential candidate Henri Falcón and former Barinas governor Rafael Rosales Peña, who had been ADem’s gubernatorial candidate in the November election, backed Garrido
food assistance program, the Local Committees of Production and Supply (CLAPs)(Zambrano, 2022; Coscojuela, 2022; El Estímulo, 2022; Leizaola, 2022; BBC, 2022).

The CLAP program was created by the Maduro administration in 2016, as falling oil revenues made the country’s previous system of indirect price subsidies unsustainable and marred by arbitrage and corruption. Over time, the system has grown to reach around 90 percent of Venezuelan households (ENCOVI, 2022; Datanálisis, 2022), around half of which report receiving food packages at least once a month. Rodríguez (2022b) estimates the value of food subsidies received by households through the CLAP program at $854 million, or 46% of the country’s food imports and 8% of central government spending in 2021.

The CLAP program works through the direct distribution of food packages by local committees to participant families. The local committees, which are supposed to represent communities, work alongside the food ministry to distribute the packages sold at subsidized prices to program participants. The committees are strongly integrated into the structure of local government associations known as communal councils, community self-organization bodies that work parallel to the elected structure of local governments and are funded by transfers from the central government(García-Guadilla and Torrealba, 2019). In contrast to the elected communal councils, CLAP members are primarily designated by pro-government political organizations (Rodríguez, 2022b). Once a CLAP committee is established in the community, it carries out a house-by-house census to determine how many families will receive food packages. In principle, all families in the community are eligible to receive one package with every delivery. Some families are eligible to receive more than one package depending on their needs (e.g., families with a pregnant member, large-sized families, and families with members who show symptoms of undernourishment). Families must designate a head of household who is entitled to receive the subsidized food package and is in charge of paying for it. Participant households are encouraged, but not required, to register for access through the Fatherland System, an electronic registry of beneficiaries of various government programs.

Upon delivery of package shipments to a central depot, representatives from the local committee bring them to the community in military-escorted trucks. Once these arrive at the community, the committee assigns the boxes to manzaneros—community members in charge of distributing packages among families in areas close to their households (a manzana is a street block) (Transparencia Venezuela, 2018). The manzaneros deliver the boxes and collect cash payments or tallies of bank deposits made in payment of the boxes by the family member. Typically one manzano will oversee distribution for no more than 15 families.

Accusations of political bias in the distribution of CLAP boxes are frequent. According to an opposition community organizer from Caracas cited in a report published by local human rights group PROVEA, the government systematically targets distribution to pro-government strongholds (PROVEA, 2016). Another resident cited in the same study denounced that the local head of the communal council in his district would single out opposition supporters in food distribution lines and ordered that they not receive food bags. A CLAP committee member interviewed by another NGO defended the allocation to Maduro supporters as a reasonable rationing mechanism in the presence of insufficient supplies (Transparencia Venezuela, 2017). Government officials have denied claims of political discrimination, alleging that they form part of disinformation campaigns designed by the opposition. Freddy Bernal, who served as National Coordinator of the CLAP system from 2016 to 2020, told a government newspaper that the accusation of political bias was ”a complete lie”, as opposition supporters ”are part of the people, too,” (Correo del Orinoco, 2016).

Some researchers have claimed that the use of CLAP boxes has allowed the government to increase the effectiveness of its mobilization of voters and is one of the factors behind its improved electoral performance. There is evidence that the government distributed food bags at pro-government mobilization booths, also known as ”red points” located outside of voting centers (Penfold, 2018). Provision of bags
Figure 2: Receipt of CLAP Packages by Frequency, 2022

Around one half of the population receive CLAP packages irregularly, while around one-third receive them at least once a month. Source: ENCOVI (2022)

appears to have been prevalent in around one-fourth of voting centers, according to a survey carried out during the December 2017 municipal elections (Rodríguez and Navarro, 2018).

4 Data and Results

I use data collected by Oil for Venezuela, a non-profit organization, covering CLAP recipients in the states of Barinas and Apure. I use the responses of 2500 heads of households in the states of Barinas and Apure between March 7 and 26 of 2022, collecting data in 4 out of 7 municipalities in Apure and 7 of 12 municipalities in Barinas. The data excludes municipalities whose main cities were more than 100 km (62 miles) from the state capitals, making the samples representative of the metropolitan area of state capitals rather than of the states as a whole. Within those municipalities, data collectors chose 62 sampling points through simple random sampling in each state. At each sampling location, they chose 20 households through systematic sampling, interviewing respondents in housing units that were at least eight units apart from the previous one, following the random route method (Etikan and Bala, 2017).

Given a sampling universe of 58 thousand households in Barinas and 137 thousand in Apure, we calculate a sampling error of 2.7 percent for Apure and 2.8 percent for Barinas.

There are two basic reasons for using Apure respondents as our control group. One of them is the similarity of both states in terms of key socio-demographic characteristics. According to the most recent census (Instituto Nacional de Estadística, 2011), Apure was the closest among 22 states to Barinas in terms of its urbanization rate, the second closest in terms of the quality of dwellings and the third closest in terms of its human development index. As in Barinas, the November 21 gubernatorial election in Apure was also very tight, with the PSUV candidate Eduardo Piñate eking out victory over MUD candidate Luis Lippa by a 44.2-42.0% margin.9

9The only other state which was as similar demographically to Barinas across all these characteristics is Portuguesa. However, in contrast to Apure and Barinas, Portuguesa’s November 21 election was a landslide victory for the PSUV candidate, who more than doubled the MUD candidate’s vote share (CNE, 2022).
The survey questionnaire included 23 questions. Eight of these were socio-demographic, 10 were related to the CLAP program, 3 were about past voting or political self-identification, and 2 were about participation in other programs. Regarding the CLAP program, we asked respondents to indicate the frequency with which they generally received CLAP packages, how many packages they had received in total during the prior five months (November 2021-February 2022) and how many they had received in each of those months. We followed up responses to the November and January questions with another question asking whether respondents had received the packages on those months before or after the respective election dates. We separately asked whether the delivery of packages in December had occurred earlier or later than normal. We also asked respondents who claimed to have received boxes what conditions they believed they had to comply with to receive the boxes, while we asked non-recipients what were the reasons they thought they had not received the boxes. We further asked respondents what political group they identified with, as well as who they had voted for in the November and January elections.

For the purposes of the regression analysis, I distinguish between three different time periods: the month of October, the days in November previous to the November 21 elections, and the period between the November 21 elections and the January 9 elections. The change between the first two periods allows us to estimate the pre-treatment trend in government benefits, while the change between the last two periods allows us to estimate the post-treatment trend. I distinguish between the group of respondents for which the answers to the questions of how many packages were received in each month are consistent with the total that they indicate having received for the whole period (which we label the high-quality sample) and those for which the answers are inconsistent (the low-quality sample).

Figure 2 shows the evolution of food packages received per household in Apure and Barinas both during the elections period.
prior to the November 21 election (held in both states) and in the period between the November election and the special January 9 election (held only in Barinas). In both states, we observe an increase in the number of food packages received by families between the month of October and the part of November prior to the election, with the absolute magnitudes of increases being remarkably similar. In the period between the first election and the special Barinas election, we see the number of boxes continue to rise in Barinas, but stabilize in Apure.

In columns 1-4 of Table 1, I present the results of panel fixed effects regressions where the dependent variable is the number of food packages received by households and controls include both time and individual-specific effects. Given my use of count data for our dependent variable, I use a Poisson fixed effects pseudomaximum likelihood estimator (Correia et al., 2020). Columns 5-8 present results for conditional fixed-effects logistic regressions where the dependent variable is a binary measure of whether the household in question received any food packages during the period in question. In both specifications, the treatment effect is captured by the coefficient on an indicator variable $B_i$ that takes the value 1 in the state of Barinas in the period between the November and January elections and 0 at all other times and places.

**Table 1: Treatment effect estimates**

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<th>Number of food packages received</th>
<th>Treatment effect</th>
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<th>4</th>
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<th>6</th>
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<td>(0.12)</td>
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<th>Dependent variable</th>
<th>Receipt of at least one food package</th>
<th>Treatment effect</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.34)</td>
<td>(0.31)</td>
<td>(0.34)</td>
<td>(0.34)</td>
<td>(0.29)</td>
<td>(0.29)</td>
</tr>
<tr>
<td>Treatment effect</td>
<td></td>
<td></td>
<td>1.58***</td>
<td>1.72***</td>
<td>2.13***</td>
<td>1.66***</td>
<td>1.83***</td>
<td>2.27***</td>
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<td></td>
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<td>1,331</td>
<td>1,331</td>
<td>1,284</td>
<td>1,284</td>
<td>1,284</td>
</tr>
<tr>
<td>Pseudo R2</td>
<td></td>
<td></td>
<td>.25</td>
<td>.25</td>
<td>.31</td>
<td>.26</td>
<td>.26</td>
<td>.32</td>
</tr>
</tbody>
</table>

The upper panel represents coefficient estimates from a Poisson fixed effects pseudomaximum likelihood estimator with individual and time-specific effects where the dependent variable is the number of food packages received. The lower panel represents results from a logistic fixed effects model with the same controls where the dependent variable is an indicator that takes the value 1 if the household received at least one package and 0 if it received no packages. High quality sample refers to those respondents whose estimates of the number of boxes received in each specific month is consistent with the total that they report receiving over the whole period. Columns 2 and 5 control for a treatment group trend while 3 and 6 introduce full set of municipality trends. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%

My baseline specification in the first column of Table 1 (upper panel) controls for individual and time effects and uses the high quality sample with standard errors clustered at the municipality level. I estimate that treatment is associated with an incidence risk ratio of 1.20, or an increase of 20% in

---

11Given the procedure for estimating boxes when recipients were uncertain as to the timing, a very small number of our observations (0.4%) are fractional values. Verdier (2018) finds that the Poisson fixed effects estimator is locally efficient even if the dependent variable is not a count variable.

12I opt for a logistic regression because there does not exist a sufficient statistic allowing the fixed effects to be conditioned out of the likelihood in the probit specification. In Appendix B, I show that the results of a fixed effects linear probability specification are consistent with the baseline estimates reported in Table 1.
the number of boxes received per household relative to the control group. This estimate is statistically significant at $p=.097$. In column 2 I introduce a treatment group trend. Despite the lack of visual evidence of non-parallel trends, controlling for a treatment group trend reduces bias and maintains reasonable power to detect a treatment effect (Blinkski and Hatfield, 2018). Once I do this, the treatment effect rises significantly, with the incidence ratio rising to 2.11, suggesting that a Barinas household was likely to receive more than twice the packages received by an Apure household during the campaign for the special election. This coefficient estimate remains stable and statistically significant even after we control for municipality trends (column 3). If we restrict only to high-quality observations (columns 4-6), we obtain slightly higher coefficient estimates and lower standard errors, making the estimated treatment effects significant at conventional levels in all specifications. Similar results emerge from the logistic panel estimates in the lower panel of Table 1, where I use the receipt of at least one food package as the dependent variable. My results indicate that the odds of receiving at least one package for a typical Barinas household during the campaign for the special election were between 5 and ten times larger than the odds of receiving one in Apure.

Table 2 shows the results of estimating separate treatment effects by the political self-identification of respondents. I now find that the coefficient estimate is consistently strongest for those voters who self-identify as being pro-opposition while also claiming to be against the leadership of the mainstream opposition coalition MUD. In fact, although all coefficient point estimates are positive, only the coefficients on these anti-MUD opposition voters and the one on anti-Maduro chavistas are significant in the baseline specification with no municipality trends. When I introduce municipality trends, all coefficients become significant, but the anti-MUD coefficient remains the largest one.

Strikingly, some of the lowest point estimates in these specifications correspond to non-aligned voters. In fact, both the hardline pro-government voters and the unaligned voters seem to do worst, with the highest level of benefits going to the voters that self-identify as sympathizers of one of the main two political groups but not of their leaderships. One possible interpretation for this result is that rather than being potential swing voters, those who self-identify as non-aligned are disaffected with the whole political establishment and are thus unlikely to be swayed to support mainstream politicians from either side.

An alternative way of measuring voters’ political leanings is by considering how voters from different groups claim to have voted in the November election. In fact, respondents who see themselves as non-aligned are also much more likely to have abstained in both the November and January elections. For example, 50% of respondents who claimed to be non-aligned abstained from voting in November and 56% of them abstained in the Barinas January election, as opposed to just 10% and 12% of other voters, respectively. It is understandable that politicians may not want to target voters whom they perceive as unlikely to turn out to vote (Stokes, 2005; Nichter, 2008).

In columns 3 and 4 of Table 2 I estimate separate treatment effects for groups of voters according to how they report having voted in the November elections. I find the strongest effects for those who claim to have voted for third-party candidates. In the specification in which I control for municipality trends, someone who reports having voted for a third-party candidate in the November election in Barinas was likely to receive 29% more packages than someone who claimed to have voted for the PSUV candidate and nearly three times as many packages as those received by a third-party voter in Apure.

I turn now to the evidence on whether the distribution of boxes was brought forward or delayed during the month of December. Since we posed this question separately from those concerning the number of boxes received, its results serve as a consistency check of our panel estimates. Given that this question refers only to distribution at one point in time, I estimate cross-sectional logistic regressions in which I include a battery of social and demographic controls, as well as controls for political self-identification of respondents.
Table 2: Treatment effect estimates by political group

<table>
<thead>
<tr>
<th>By political self-identification</th>
<th>Number of food packages received</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
</tr>
<tr>
<td>Pro-Maduro chavistas</td>
<td>0.159</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Anti-Maduro chavistas</td>
<td>0.208*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Pro-MUD opposition</td>
<td>0.159</td>
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<td></td>
<td>(0.12)</td>
</tr>
<tr>
<td>Anti-MUD opposition</td>
<td>0.296***</td>
</tr>
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<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Non-aligned</td>
<td>0.144</td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
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<tr>
<td>Did not answer</td>
<td>0.121</td>
</tr>
<tr>
<td></td>
<td>(0.14)</td>
</tr>
</tbody>
</table>

By vote in November election

<table>
<thead>
<tr>
<th></th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
</tr>
</thead>
<tbody>
<tr>
<td>Voted for government candidate</td>
<td>0.161</td>
<td>0.764***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Voted for MUD candidate</td>
<td>0.241**</td>
<td>0.857***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Voted for third-party candidate</td>
<td>0.372***</td>
<td>1.019***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Did not vote</td>
<td>0.169</td>
<td>0.793***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
<td>(0.19)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Did not answer</td>
<td>0.108</td>
<td>0.726***</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
<td>(0.14)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

| N                                | 2957 | 2957 | 2957 | 2957 |
| Adjusted R2                      | 0.136 | 0.144 | 0.136 | 0.145 |
| Sample                           | All observations | All observations | All observations | All observations |
| Municipality trends              | No | Yes | No | Yes |

Columns 1 and 2 estimate separate treatment effects according to political self-identification, while 3 and 4 estimate them according to reported vote in the November election. All estimates use the complete sample; columns 2 and 4 control for municipality trends. See Appendix Table A2 for results restricted to high-quality sample. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%.

The results reported in 3 show that Barinas voters claim that boxes delivered in December arrived earlier than usual relative to Apure voters, with the effect being strongly statistically significant. On average, the odds of Barinas voters reporting having received early delivery of boxes in December was nearly four times as large as in Apure. Barinas voters are also less likely to report having received late boxes, though the estimate is not statistically significant.
Table 3: Timing of December boxes and political conditioning

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>December boxes arrived late</th>
<th>December boxes arrived early</th>
<th>Access to boxes politically conditioned</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment effect</td>
<td>-0.367</td>
<td>1.35***</td>
<td>-0.561</td>
</tr>
<tr>
<td></td>
<td>(0.31)</td>
<td>(0.35)</td>
<td>(0.51)</td>
</tr>
<tr>
<td>N</td>
<td>1,879</td>
<td>1,786</td>
<td>2,065</td>
</tr>
<tr>
<td>Pseudo R2</td>
<td>0.02</td>
<td>0.08</td>
<td>0.03</td>
</tr>
<tr>
<td>Sample</td>
<td>All</td>
<td>All</td>
<td>All</td>
</tr>
<tr>
<td>Social and demographic controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Political identification controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
</tr>
</tbody>
</table>

Regressions estimate the coefficient on a Barinas dummy in logistic regressions with a full set of demographic controls. The dependent variable is respondents’ claim that December boxes arrived late (columns 1 and 2), early (3 and 4) and that access was politically conditioned (5 and 6). Controls include gender, age group, income group, education, employment and neighborhood type. Columns 2, 4 and 6 add controls for political self-identification. All estimates use the complete sample; see Appendix Table A3 for results restricted to high-quality sample. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%.

The last two columns of Table 3 report regressions in which we use the likelihood of voters claiming that access to boxes was politically conditioned as a dependent variable (see Appendix C for details on the construction of this indicator). I find that Barinas voters were less likely to claim that they had experienced political conditioning in access to boxes than Apure voters, although the coefficient estimates are not statistically significant.

In Table 4 I consider whether there is evidence that changes in the number of food packages received are correlated with voters’ decisions to switch their vote between the November and the January elections in Barinas. I consider separately the question of whether voters who voted against the government in November switched to voting in favor of it in January (whether benefits helped sway swing voters) from that of whether those who voted for the government switched to voting against it (whether benefits helped avert defection). My explanatory variable of interest is the change in the number of boxes received between the campaign for the November elections and the campaign for the January special election.

The results show that voters who saw a larger increase in the number of boxes received during the campaign for the special election relative to that of the November election were more likely to switch their vote in favor of the government. I estimate that receiving an additional food package is associated with an increase of between 43 and 73 percent in the odds of recipients switching their vote in favor of the government. In contrast, I do not find that receiving less boxes made it more likely for those who voted for the government to switch in favor of other candidates or abstaining in the special election. These results suggest that the government’s decision to focus on targeting swing voters, and especially those who had failed to vote for the government candidate in the November election, may have made more sense from a strategic viewpoint than the alternative of using those boxes to try to avoid defections among loyalists.
Table 4: Vote-switching behavior and packages received

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>Switched vote in favor of government</th>
<th>Switched vote against government</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>1</td>
<td>2</td>
</tr>
<tr>
<td>Change in food packages received</td>
<td>.357***</td>
<td>.547***</td>
</tr>
<tr>
<td>N</td>
<td>314</td>
<td>261</td>
</tr>
<tr>
<td>Pseudo R2</td>
<td>0.11</td>
<td>0.24</td>
</tr>
</tbody>
</table>

Regressions estimate the coefficient on the change in packages received between the campaign for the regular election (November 1-21) and the campaign for the special election (November 22-January 9). Dependent variable is a dummy that takes the value 1 for voters who switched their votes in favor of the government (columns 1-3) or against the government (columns 4-6). Controls include gender, age group, income group, education, employment, and neighborhood type. Columns 2 and 5 add controls for political self-identification while 3 and 6 add municipality fixed effects. All estimates use the complete sample; see Appendix Table A4 for results restricted to high-quality sample. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%.

5 Concluding Comments

The decision by governments to provide benefits through social programs and the decision of potential recipients to seek to receive these benefits jointly determine participation in social programs. Attempts to test theories of clientelism by examining the cross-sectional or time-series correlations between political affiliation and program participation is clouded by this simultaneity of impacts. Greater program participation by government sympathizers may simply reflect the lower cost for these voters of agreeing to vote for the incumbents. Expansions in clientelar social programs could lead to increases or declines in the share of participants who are government sympathizers depending on whether they reflect a greater willingness of voters to sign up for the programs or a decision by the government to increase the provision of benefits.

I address this identification problem by studying changes in the provision of food packages through Venezuela’s CLAP program in the period prior to the holding of an unanticipated special gubernatorial election in the state of Barinas in January 2022. I find that the government significantly ratcheted up the provision of food packages in Barinas relative to the control group of Apure, a state with similar demographic and political characteristics in which no repeat election occurred.

I also find that the increase in the government provision of benefits induced by the holding of the special election was targeted primarily towards swing voters, consistent with the predictions of spatial models of voting. During the campaign for the Barinas special election, Venezuela’s government was more likely to target voters who self-identified as pro-opposition but expressed dissatisfaction with the current opposition leadership, as well as voters who had supported third party candidates in the past. Voters who supported a third party candidate in Barinas were likely to receive 29 percent more packages.
than die-hard Maduro supporters - and nearly three times as many as Apure voters. Barinas voters were also more likely to report that packages had arrived earlier during the campaign for the special election. They were also more likely to switch their votes in favor of the government when they received more food packages, suggesting that the government strategy of targeting potential vote-switchers was the correct one from the standpoint of vote maximization.

These results underscore the need to design theoretically-grounded identification strategies to evaluate theories of politics. Examination of cross-sectional or time series patterns is generally insufficient to test comparative statics implications when the variables of interest are determined through the interaction of actors with diverse interests, motivations and constraints. In the absence of randomized control trials, exploiting natural experiments such as unanticipated elections can allow us to isolate the effects of preference and policy shocks and provide cleaner tests of model predictions.
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19
A Appendix 1: Proof of Propositions

**Proposition 1.** \( s^b_g \geq s^b_o \) with the strict inequality holding as long as there are some voters in the population that do not receive benefits.

**Proof.** By definition, all government sympathizers have ideal policies such that \( \pi_i < \frac{\pi_g + \pi_o}{2} \). Therefore equation (2) must hold for all government sympathizers, making \( s^b_g = 1 \). If \( \pi_g + \pi_o + b_i \pi_o - \pi_g < 1 \), then voters with \( \pi_i \in \left[ \frac{\pi_g + \pi_o}{2} + \frac{b_i}{2(\pi_o - \pi_g)} , 1 \right] \) will decide to turn down the benefits. Because all of these voters are opposition sympathizers, \( s^b_o \) will be strictly less than one. \( \square \)

**Proposition 2.** Increases in \( \alpha \) and \( \theta \) will both lead to declines in the government’s vote share \( P \) but will have effects of opposite sign on the share of government sympathizers receiving benefits, with \( \frac{ds^b_g}{d\alpha} < 0 \) and \( \frac{ds^b_g}{d\theta} > 0 \)

**Proof.** Solving the first order condition for maximization of (6) we get a quadratic equation with roots:

\[
b = \frac{-P}{2P^r} \pm \frac{\sqrt{P^r + 2P^2P\theta^{-1}}}{2PP^r}
\]

where \( P^r = \frac{dP}{d\theta} \). Note that we can discard the negative root of this expression given the condition \( b > 0 \). Therefore:

\[
\frac{db}{d\theta} = \frac{P^r}{2} (P^4 + 2P^2P\theta^{-1})^{-1/2} \left( -\frac{1}{\theta^2} \right) < 0
\]

Consider now the effects of shocks to \( \theta \) and \( \alpha \) on \( P \). From (4),(5) and (12) we can derive:

\[
\frac{dP}{d\alpha} = -f(\hat{\eta}) < 0
\]

and

\[
\frac{dP}{d\theta} = f(\hat{\eta}) \frac{1}{2(\pi_o - \pi_g)} \frac{db}{d\theta} < 0
\]

Using (8) we can calculate:

\[
\frac{ds^b_g}{d\theta} = -\frac{s^b_g f(\hat{\eta})}{2(\pi_o - \pi_g)} \frac{db}{d\theta} > 0
\]

Regarding the shock to \( \alpha \), from (5)-(8) we can calculate:

\[
\frac{ds^b_g}{d\alpha} = -\frac{1}{\int_0^{\hat{\eta}} f(\eta) d\eta} \left( -f(\hat{\eta}) + s^b_g f(\hat{\eta}) \right).
\]

A sufficient condition for \( \frac{ds^b_g}{d\alpha} < 0 \) is \( f(\eta) \leq f(\hat{\eta}) \). This will be satisfied by any distribution with non-increasing density, including the uniform, gamma and Pareto distributions. \( \square \)

**Corollary.** Shocks to policymaker preferences will generate negative co-movements between program participation and the share of government sympathizers in program beneficiaries, while shocks to voter preferences may induce positive co-movements between the same variables.

**Proof.** From (13) and (16), an increase in \( \alpha \) will generate a decline in \( P \) and a decline in \( s^b_g \) while from (14) and (15), an increase in \( \theta \) will generate a decline in \( P \) and an increase in \( s^b_g \). \( \square \)

---

13The second order condition for this problem is

\[
P'' - 2bP'(2P' + bP'') - \theta(P + bP'')(2P + 2bP') < 0
\]

where \( P'' = \frac{dP}{d\eta} \). Given that \( P > 0 \) and \( P' > 0 \), a sufficient condition for this condition to hold is \( P'' = 0 \). This will be the case, for example, if \( f(\eta) \) is the density function of the uniform distribution.
### Appendix 2: Linear probability estimates

#### Table A1: Linear probability estimates

<table>
<thead>
<tr>
<th>Treatment group trend</th>
<th>Municipality trends</th>
<th>Sample</th>
<th>Dependent variable</th>
<th>Treatment effect</th>
<th>N</th>
<th>Pseudo R2</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td></td>
<td>Number of food packages received</td>
<td></td>
<td></td>
<td></td>
<td>.493***</td>
<td>.76***</td>
<td>.747***</td>
<td>.546***</td>
<td>.862***</td>
<td>.867***</td>
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<td>(0.07)</td>
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<td>0.37</td>
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<tr>
<td></td>
<td></td>
<td></td>
<td>Receipt of at least one food package</td>
<td></td>
<td></td>
<td></td>
<td>-0.009</td>
<td>.171**</td>
<td>.174***</td>
<td>-0.015</td>
<td>.194**</td>
<td>.207***</td>
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<td></td>
<td>(0.04)</td>
<td>(0.07)</td>
<td>(0.05)</td>
<td>(0.06)</td>
<td>(0.07)</td>
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<td>0.10</td>
<td>0.11</td>
<td>0.14</td>
<td>0.11</td>
<td>0.12</td>
<td>0.16</td>
</tr>
</tbody>
</table>
Table A2: Treatment effects by political groups, high-quality sample

<table>
<thead>
<tr>
<th>By political self-identification</th>
<th>Number of food packages received</th>
</tr>
</thead>
<tbody>
<tr>
<td>By political self-identification</td>
<td>1</td>
</tr>
<tr>
<td>Pro-Maduro chavistas</td>
<td>0.203*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Anti-Maduro chavistas</td>
<td>0.262**</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td>Pro-MUD opposition</td>
<td>0.21*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Anti-MUD opposition</td>
<td>0.337***</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td>Non-aligned</td>
<td>0.187*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Did not answer</td>
<td>0.162</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
</tr>
<tr>
<td>By vote in November election</td>
<td>5</td>
</tr>
<tr>
<td>Voted for government candidate</td>
<td>0.208*</td>
</tr>
<tr>
<td></td>
<td>(0.11)</td>
</tr>
<tr>
<td>Voted for MUD candidate</td>
<td>0.293***</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td>Voted for third-party candidate</td>
<td>0.413***</td>
</tr>
<tr>
<td></td>
<td>(0.12)</td>
</tr>
<tr>
<td>Did not vote</td>
<td>0.21</td>
</tr>
<tr>
<td></td>
<td>(0.16)</td>
</tr>
<tr>
<td>Did not answer</td>
<td>0.148</td>
</tr>
<tr>
<td></td>
<td>(0.10)</td>
</tr>
<tr>
<td>N</td>
<td>2957</td>
</tr>
<tr>
<td>Adjusted R2</td>
<td>0.136</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Sample</th>
<th>High Quality</th>
<th>High Quality</th>
<th>High Quality</th>
<th>High Quality</th>
</tr>
</thead>
<tbody>
<tr>
<td>Municipality trends</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
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</table>

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### Table A3: Timing of December boxes and political conditioning, high-quality sample

<table>
<thead>
<tr>
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<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable</td>
<td>December boxes arrived late</td>
<td>December boxes arrived early</td>
<td>Access to boxes politically conditioned</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Treatment effect</td>
<td>-0.405</td>
<td>-0.412</td>
<td>1.48***</td>
<td>1.45***</td>
<td>-0.583</td>
</tr>
<tr>
<td>(0.31)</td>
<td>(0.29)</td>
<td>(0.32)</td>
<td>(0.29)</td>
<td>(0.51)</td>
<td>(0.51)</td>
</tr>
<tr>
<td>N</td>
<td>1,788</td>
<td>1,788</td>
<td>1,699</td>
<td>1,699</td>
<td>1,951</td>
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<tr>
<td>Pseudo R2</td>
<td>0.03</td>
<td>0.03</td>
<td>0.09</td>
<td>0.10</td>
<td>0.03</td>
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<td>Sample</td>
<td>Social and demographic controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Political identification controls</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
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### Table A4: Vote-switching behavior and packages received, high-quality sample

<table>
<thead>
<tr>
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</thead>
<tbody>
<tr>
<td>Dependent variable</td>
<td>Switched vote in favor of government</td>
<td>Switched vote against government</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Change in food packages received</td>
<td>.485***</td>
<td>.914***</td>
<td>.872***</td>
<td>0.211</td>
<td>0.23</td>
</tr>
<tr>
<td>(0.08)</td>
<td>(0.18)</td>
<td>(0.21)</td>
<td>(0.20)</td>
<td>(0.24)</td>
<td>(0.21)</td>
</tr>
<tr>
<td>N</td>
<td>242</td>
<td>206</td>
<td>178</td>
<td>140</td>
<td>122</td>
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<tr>
<td>Pseudo R2</td>
<td>0.18</td>
<td>0.31</td>
<td>0.33</td>
<td>0.06</td>
<td>0.36</td>
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<tr>
<td>Sample</td>
<td>Social and demographic controls</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td>Political identification controls</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td>Municipality fixed effects</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>No</td>
<td>No</td>
</tr>
</tbody>
</table>
Appendix 3: Variable definitions

The survey questionnaire included 23 questions. Eight of these were socio-demographic, 10 were related to the CLAP program, 3 were about past voting or political self-identification, and 2 were about participation in other programs. The precise wording of some of the questions is as follows.\(^\text{14}\)

**clap1**: In your household, have you at some moment received bags or boxes of food from the CLAP (Local Committees of Supply and Production)
- 1. Yes
- 2. No

**clap2**: Can you tell me with what frequency you received the CLAP bags or boxes?
- 1. Weekly
- 2. Every two weeks
- 3. Monthly
- 4. Every two months
- 5. Every quarter
- 6. Every six months
- 7. It’s irregular
- 8. Don’t know

**clap3**: And thinking specifically in the month of December of 2021, would you say that:
- 1. The delivery of the CLAP bags or boxes was delayed.
- 2. The delivery of the CLAP bags or boxes was made within the foreseen time.
- 3. The delivery of the CLAP bags or boxes was made earlier than the foreseen time.
- 4. Did not receive CLAP bags or boxes in that period.
- 5. Don’t remember
- 6. Don’t know/Don’t Answer

**clap3**: And specifically between the months of October and February, do you remember having received CLAP boxes or bags? How many CLAP boxes or bags did you receive in this period, counting from October 1?
- 1. None
- 2. One
- 3. Two
- 4. Three
- 5. Four
- 6. Five
- 7. Six
- 8. Seven or more
- 9. Don’t remember
- 10. Don’t know

**clap5oct-clap5feb**: Please tell me how many CLAP bags or boxes did you receive per month for each month
- 1. None
- 2. One
- 3. Two or more
- 4. Don’t remember

\(^{14}\text{The complete questionnaire in Spanish will be published with the paper’s replication files.}\)
**clap6:** [Asked of those who indicate having received packages in November] Specifically, were the CLAP bags or boxes received before or after the elections of November 21?

1. Before
2. After
3. Don’t remember
4. Don’t know/Don’t Answer

**clap7:** [Asked of those who indicate having received packages in January] Specifically, were the CLAP bags or boxes received before or after January 9?

1. Before
2. After
3. Don’t remember
4. Don’t know/Don’t answer

**cond1:** [Asked of those who indicate having received packages] Which of these conditions did you have to comply with to receive the CLAP bag or box?

1. Register in the Fatherland System.
2. Attend government party demonstrations.
3. Vote in elections.
4. Participate in the Communal Council.
5. Be registered in the government party (PSUV).
7. Another condition not related to my needs.
8. None. I was not asked to satisfy any condition other than paying its cost.
9. Don’t answer

**cond2:** [Asked of those who indicate not having received packages] Can you indicate the reasons why you didn’t receive CLAP boxes in your home?

1. We don’t need them.
2. We didn’t ask for them.
3. I/we did not register in the Fatherland System.
4. I don’t attend government demonstrations
5. I am not registered in the government party
6. I don’t participate in the Communal Council
7. I am/we are against the government
8. Other

**elecnov:** Can you indicate if you remember which of the candidates for governor you voted for on November 21, 2021?

**elecjan:** [Asked only of Barinas residents] And do you remember who you voted for in the elections for governor of the state of Barinas on January 9 2022?

**political:** Politically speaking, with which of the following options do you feel more identified, chavismo or opposition? [If response is chavismo] Chavista with Maduro or distanced from Maduro? [If response is opposition] But do you or don’t you support the MUD?

1. Opposition and supports MUD
2. Opposition and does not support MUD
3. Chavista pro-Maduro
4. Chavista not pro-Maduro
5. None
6. Don’t answer
I define the following time periods for our analysis: \( t = 1 \) corresponds to the month of October, \( t = 2 \) to the period from November 1 to November 21, and \( t = 3 \) to the period from November 21 to November 22. I define the panel variable \( \text{panelclap} \) as equal to the numerical responses to \( \text{clap5oct} \) for \( t = 1 \). For \( t = 2 \), I use \( \text{clap5nov} \) only if respondent indicates in \( \text{clap6} \) having received it before the election. For \( t = 3 \) I use the sum of \( \text{clap5dec}, \text{clap5nov} \) (only if respondent indicates in \( \text{clap6} \) having received after the elections) and \( \text{clap5jan} \) (only if respondent indicates in \( \text{clap7} \) having received it before the election). \( \text{panelclap} \) is the variable used in the specifications reported in Tables 1 and 2. I label as high-quality those observations for which the sum of the numerical answers to \( \text{clap5oct-clap5feb} \) is consistent with the response to \( \text{clap3} \).

I use the response to \( \text{clap3} \) to create two binary indicators: one for respondents who claim that December deliveries were late (\( \text{claplate} \), used in columns 1 and 2 of Table 3), and another one for those who claim that they were early (\( \text{clapearly} \), used in columns 3 and 4 of Table 3). Responses 4-6 were coded as missing, while other responses were coded either as zero or one. Thus, \( \text{claplate} = 1 \) if \( \text{clap3} = 1|0 \) if \( \text{clap3} = 2|3 \) and missing if \( \text{clap3} = 4|5|6|\text{missing} \).

I use the response to \( \text{cond1} \) to create \( \text{polcond} \), a binary indicator of political conditioning of CLAP access. I code \( \text{polcond} \) as 0 if \( \text{cond1} = 1|8 \), 1 if \( \text{cond1} = 2|3|4|5|6|7 \), and missing otherwise. \( \text{polcond} \) is the dependent variable in columns 5 and 6 of Table 3.

I use the responses to \( \text{elecnov} \) and \( \text{elecjan} \) to create two binary indicators, \( \text{winvote} \) and \( \text{losevote} \). I code \( \text{winvote} \) as 0 if the respondent indicates that they did not vote for the PSUV candidate in November nor in January, and 1 if they indicate that they voted for the PSUV candidate in January but not in November. I code \( \text{losevote} \) as 0 if respondent indicates that they voted for the PSUV candidate in both November and January, and 1 if respondent indicates voting for the PSUV candidate in November and not in January. These variables are only defined for Barinas residents.