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 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 an increase in the probability of voters in Barinas receiving food packages in comparison with the control group of voters in the state of Apure. I also find that moderate opposition and third-party voters received larger increases in food benefits. These results are consistent with the predictions of spatial models, according to which elections lead governments to direct more benefits to swing voters instead of core supporters. The findings illustrate why investigation of cross-sectional correlations is insufficient to test the implications of theories of voting if it is not accompanied by a clear identification strategy to help isolate the source of the underlying shocks.

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

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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 those benefits towards electorally pivotal constituencies (Lindbeck and Weibull 1987; Dixit and Londregan 1996). 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 otherwise be likely to vote for their opponents.

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, Burden, and Howell 2010; G. Wright 1974), studies of individualized 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) find that respondents who identified with parties that were involved in moderate or major clientelistic efforts were significantly more likely to receive handouts. Diaz-Cayeros, Estévez, and Magaloni (2016) establish 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, clientelist relations reflect bilateral decisions by parties and voters to engage in an exchange of votes for transfers. The receipt of a clientelist 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

1. Not 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.

may seek to condition access to benefits on voter characteristics, these characteristics will also influence the willingness of voters to seek access to the 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 clientelist 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 size of the program can be negative or positive depending on whether the variations are produced by shocks to politician or voter preferences.

I propose to 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-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, it would have likely allocated packages differently there than in a state where it did not expect there to be another election.[2]

To the best of my knowledge, this is the first paper to use an unexpected repeat election as a natural experiment by comparing the evolution of outcomes in the jurisdiction in which the election is repeated to that of a control group in a jurisdiction where it is not repeated. Previous studies that have used repeat elections have instead focused on before-and-after comparisons of outcomes.[3] 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

2.1 A two-party model

Consider a simple model of clientelism in which the government offers every voter a benefit $b > 0$ conditional on agreeing to vote for the incumbent candidate. I assume that this agreement cannot be perfectly verified, so that those who enter into it will vote for the government with a

2. For the same reason, we cannot simply focus on differences between electoral and non-electoral periods. As Nichter (2018) has convincingly argued, clientelist 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.

3. 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 (2019) uses the same event to study the differences in mobilization by local political leaders during the re-run. Coskan, Baysu, and Koc (2022) 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 (2020) 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.

positive probability which may be smaller than one.[4] Voters have quasi-linear preferences that depend on the monetary value of the benefit received as well as the distance between their ideal policy π_i and the policy enacted by the party that reaches office π_k. There are two parties, government (g) and opposition (o); I assume without loss of generality that $\pi_g < \pi_o$.[5] Thus the utility of voter i if party k is in office and the voter receives a benefit b_i will be

$$U_{ik} = -(\pi_i - \pi_k)^2 + b_i \tag{1}$$

I characterize two groups of voters. *Government sympathizers* (G) prefer the policy platform of the government and would vote for it even in the absence of a benefit. I assume that they will accept the transfer and vote for the government with probability one. *Opposition sympathizers* (O), who would vote for the opposition in the absence of a benefit, will compare the monetary value of the benefit with the loss of utility that they would suffer from having the government instead of the opposition in power. If the value of the benefit is higher, they will accept it and vote for the government with probability $\psi \in (0, 1]$; if it is not, then they will reject the benefit and vote for the opposition with probability one.

Voter preferences over policies are equal to the sum of a constant α and an individual-specific policy parameter η_i distributed according to a uniform distribution on the [0,1] interval. The parameter α captures shocks to the distribution of voter preferences, with higher values corresponding to preference distributions that are uniformly closer to the opposition platform[6].

$$\pi_i = \alpha + \eta_i. \tag{2}$$

Denote p_{ig} as the probability that voter i will vote for the government. Voter choices can be summarized as:

$$\{p_{ig}, b_i\} = \begin{cases} \{1, b\} & \text{if } \eta_i \leq \epsilon \\ \{\psi, b\} & \text{if } \eta_i \in (\epsilon, \epsilon + \beta b) \\ \{0, 0\} & \text{if } \eta_i \geq \epsilon + \beta b. \end{cases} \tag{3}$$

where $\epsilon = \frac{\pi_o + \pi_g}{2} - \alpha$ and $\beta = \frac{1}{2(\pi_o - \pi_g)}$. ϵ is the level of η_i that corresponds to the midpoint

4. The case in which the agreement is perfectly binding can be addressed in our framework by setting $\psi = 1$ in equation (3) below.

5. We treat the party platforms π_k as fixed. In Appendix B we show how these platforms can be endogenized when there is uncertainty about the distribution of voters and politicians care about the policies that are adopted by whichever party wins the elections.

6. Therefore, the share of opposition sympathizers will be a monotonically increasing function of α

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 term βb represents the additional contribution to this threshold of the government's offer. A voter who would otherwise have been ideologically closer to the opposition may be swayed to vote for the government with positive probability as long as this value is greater than the distance between the voter's ideal point and the midpoint between the parties' positions.

Define $q_{bJ} = P(b_i > 0 | i \in J)$ as the probability that an individual receives benefits given that they belong to group J and $q_{Jb} = P(i \in J | b_i > 0)$ the probability that an individual belongs to group J given that they receive benefits, with $J = \{G, O\}$. We can now establish

Proposition 1. *$q_{bG} \geq q_{bO}$ 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 of all propositions).

Proposition 1 captures the essence of the self-selection problem. Note that to this point we 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 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 turn now to specifying government preferences. The government's objective will be to maximize the probability of winning the election minus a term that captures the opportunity cost of the resources invested in distributing benefits

$$\max_{b>0} V = P_v - \theta(bP_b)^2 \tag{4}$$

with P_v denoting the share of votes obtained by the government party and P_b denoting the share

of voters receiving government benefits. The parameter θ captures the relative weight given by the government to the costs associated with benefits spending; alternatively, θ can be interpreted as the inverse of the weight that the government puts on the objective of winning elections. In our empirical exercise, we will treat the holding of elections as a decline in θ, embodying the assumption that as elections are nearer, politicians place relatively more importance on raising the share of voters supporting them and less importance on the deadweight losses associated with raising the resources necessary to pay for distributive benefits programs.

Using the uniform distribution assumption we can write:

$$V = \epsilon + \psi\beta b - \theta \left(b \left(\epsilon + \beta b\right)\right)^2 \tag{5}$$

We now establish

Proposition 2. *Increases in α (the proximity between voter ideal points and the opposition platform) and θ (the relative weight that governments put on the opportunity costs of distributing benefits) will both lead to declines in the the share of voters receiving benefits P_b but will have effects of opposite sign on the probability of being a government sympathizer conditional on receiving benefits, with $\frac{dq_{Gb}}{d\alpha} < 0$ and $\frac{dq_{Gb}}{d\theta} > 0$.*

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

The two propositions illustrate 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. Proposition 1 shows that 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 in the absence of any monetary inducement. Proposition 2 shows that increases in program participation 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.[7]

7. Stokes 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

One way to think about α and θ are as shocks to the supply and demand for votes. Increases in α induce changes in program participation even with no changes in the government offer of services, while increases in θ 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 clientelist 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 θ while controlling for shocks to α.[8]

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 (negative) shock to θ. 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.

2.2 A three-party model

The previous section illustrated by means of a model of two-party competition why conventional tests of theories of clientelism are riddled by a fundamental identification problem. I argued that as a result of this problem, we need clearer identification strategies that allow us to isolate the origin of the shocks that produce the correlations on which we plan to test our theories. In order to develop such a test for the case of Venezuela, we need to use a slightly more complex form of the model which allows for competition between more than two parties. The reason is that the context of the 2021 elections that we study were marked by competition between three large coalitions: the government Great Patriotic Pole, the mainstream opposition Democratic Unity Roundtable, and the dissident opposition Democratic Alliance. These three coalitions obtained respectively 46%, 22% and 20% of the national vote in the November 2021 elections (and 37%, 37% and 24% of the Barinas vote).

I now consider an alternative scenario in which there are three parties — government (g),

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

8. Note 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 P. G. Wright (1928).

opposition (o) and centrist (c) — with policy platforms $\pi_g < \pi_c < \pi_o$. Voters will support the party whose platform is closest to their ideal point π_i unless the value of the benefit b is greater than the utility loss that they would suffer from voting for the government instead of their preferred party. As before, if that value is higher than the utility loss, they will accept the benefit and vote for the government with probability $p_{ig} = \psi$; otherwise, they will vote for their preferred party. In an alternative characterization, c represents a group of voters who decide to abstain given the distance between their preferences and those of either party; when offered a transfer, they may choose to vote with probability ψ if the monetary value of the transfer is sufficiently high to compensate for the disutility of voting relative to abstaining. Voter choices can now be summarized as:

$$\{p_{ig}, b_i\} = \begin{cases} \{1, b\} & \text{if } \eta_i \leq \epsilon_c \\ \{\psi, b\} & \text{if } \begin{cases} \eta_i \in (\epsilon_c, \epsilon_c + \beta_c b) & \text{and } b \leq \bar{b} \\ \eta_i \in (\epsilon_o, \epsilon_o + \beta_o b) & \text{and } b \geq \bar{b} \end{cases} \\ \{0, 0\} & \text{if } \begin{cases} \eta_i > \epsilon_c + \beta_c b & \text{and } b \leq \bar{b} \\ \eta_i > \epsilon_o + \beta_o b & \text{and } b \geq \bar{b} \end{cases} \end{cases} \tag{6}$$

where $\epsilon_o = \frac{\pi_o + \pi_g}{2} - \alpha$, $\epsilon_c = \frac{\pi_c + \pi_g}{2} - \alpha$, $\beta_o = \frac{1}{2(\pi_o - \pi_g)}$ and $\beta_c = \frac{1}{2(\pi_c - \pi_g)}$. Government sympathizers — now defined as voters whose policy preferences are closer to the government platform than to the centrist platform — will accept the transfer and vote for the government with probability one. When b is below a threshold $\bar{b}$, then a fraction of centrist sympathizers and no opposition sympathizers will accept the transfer and vote for the government with probability ψ. Once b surpasses $\bar{b}$ then all centrist sympathizers and a fraction of opposition sympathizers will accept the transfer and vote for the government. Government preferences continue to be characterized by the objective in (4).

In Appendix A, I establish:

Proposition 3. *The average benefit received by all voters as well as the average benefit among those who accept it will be weakly decreasing in θ (the relative weight that governments put on the opportunity costs of distributing benefits). A decline in θ will also lead to a proportionate increase in the expected value of benefits received by centrist sympathizers that is greater than or equal to the rise in the expected value of benefits received by government sympathizers.*

Proposition 3 provides us with the central hypotheses to be tested in section 4. I will treat the holding of an election as an example of a shock that lowers θ as it raises the relative importance for politicians of the vote share relative to the opportunity cost of resources spent on social programs. I will use this shock to test the hypothesis that the benefits received by the average voter will increase when elections are held, and will rise more than proportionately for centrist voters than for government sympathizers.

3 Background and Context

3.1 The Barinas repeat election

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 2021). The process was overseen by the National Electoral Council (CNE), two of whose five board members represented opposition parties.[9]

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[10] 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

9. 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 2021; Observatorio Electoral Venezolano 2021)

10. Throughout this paper, we use the label Supreme Court to refer the country's highest court, the *Tribunal Supremo de Justicia*.

Figure 1: Electoral results in Barinas, 2021 and 2022 Gubernatorial Elections

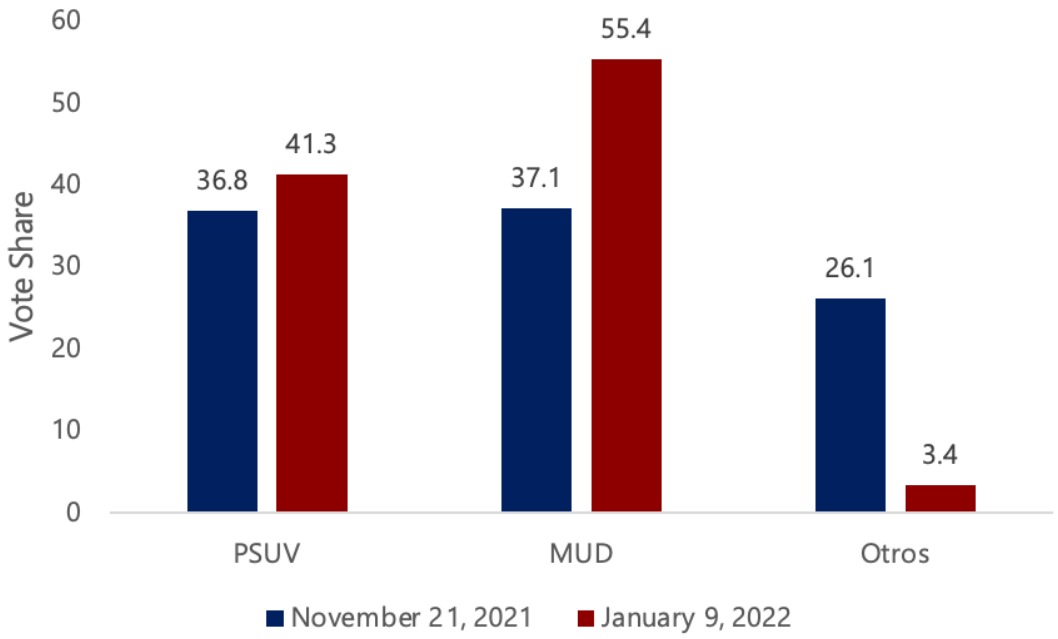


Figure shows that while both the MUD and PSUV candidate gained vote share in the special election, the MUD saw a much greater increase, which suggests its ability to capture votes that had gone to third-party candidates in November. Source: CNE (2022)

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).[11]

Repeat elections are relatively rare events in Venezuela. They can occur for one of two reasons: the absolute absence of the elected official (due, for example, to death or criminal conviction), or the annulment of the election. Of 2178 elections for national, state or municipal executives held since the approval of the 1999 Constitution, only 19 have been repeat elections (Table 1). Among these, a large majority (15) have been due to absolute absences; only a handful (5) have been due to annulment of prior elections. The case of Barinas studied in this paper marked the first time that a gubernatorial election had been repeated due to annulment since the Amazonas state election of 2000.

11. 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

Table 1: Repeat elections for executive positions held in Venezuela, 1999-2021

Year	Jurisdiction	Level of government	Initial election date	Repeat election date	Cause of repeat election	Details
2021	Barinas	Governor	11/21/21	01/09/22	Annulment	Disqualification
2017	Zulia	Governor	10/15/17	12/10/17	Absolute absence	Did not take oath of office
2014	San Cristobal (Táchira)	Mayor	04/14/13	05/25/14	Absolute absence	Criminal conviction
2013	San Diego (Carabobo)	Mayor	04/14/13	05/25/14	Absolute absence	Criminal conviction
2012	Venezuela	President	10/07/12	04/14/23	Absolute absence	Death
2010	Guarico	Governor	11/23/08	12/05/10	Absolute absence	Death
2010	Panamericano (Táchira)	Mayor	11/23/08	12/05/10	Absolute absence	Death
2010	Boconó (Trujillo)	Mayor	11/23/08	12/05/10	Absolute absence	Death
2010	Maracaibo (Zulia)	Mayor	11/23/08	12/05/10	Absolute absence	Abandonment of position
2010	Guarico	Governor	11/23/08	12/05/10	Absolute absence	Death
2010	Arismendi (Nueva Esparta)	Mayor	11/23/08	12/05/10	Absolute absence	Death
2007	Catatumbo (Zulia)	Mayor	10/31/04	06/03/07	Annulment	Ineligibility
2007	Alto Orinoco (Amazonas)	Mayor	10/31/04	06/03/07	Absolute absence	Arrest warrant
2004	Miranda (Trujillo)	Mayor	10/31/04	10/22/06	Absolute absence	Death
2005	Achaguas (Apure)	Mayor	10/31/04	08/07/05	Absolute absence	Death
2002	Rosario de Perijá (Zulia)	Mayor	07/30/00	08/01/02	Absolute absence	Death
2000	Amazonas	Governor	07/30/00	02/11/01	Annulment	Partial annulment due to fraud
2000	Nirgua (Yaracuy)	Mayor	07/30/00	11/04/01	Annulment	Partial annulment due to fraud
2000	Carrizal (Miranda)	Mayor	07/30/00	01/27/02	Annulment	Partial annulment due to fraud

This table contains an exhaustive list of all elections for local, regional and national executives that were held outside of regular constitutional terms. An election is repeated if a prior election has already been held to elect the head of the executive for that government, the person elected in it was either unable or not allowed to complete their term, and electoral authorities proceeded to fill that vacancy by calling a new election.

The repeat of the Barinas 2021 election was also exceptional in that it was the first time in the

country's democratic history that an election has been annulled because the Comptroller General failed to notify electoral authorities that a candidate had been disqualified from running before the election could be held. Since 1975, the Comptroller General's Office — an autonomous organism tasked with oversight over the use of public funds and resources — has had the authority to bar public officials found to have incurred in administrative irregularities from being appointed to public office. Yet after Venezuela approved a new Constitution in 1999, the Comptroller General's Office began to apply the norm to elected —- instead of just appointed — officials (Brewer-Carías 2011). The constitutionality of this controversial use of its power to bar persons from running for public office was affirmed by the Supreme Court in a 2008 decision (Tribunal Supremo de Justicia 2008).

In the case of the Barinas 2021 election, the Supreme Court justified its decision to annul the election on its having received notice after the election that the winning candidate had been barred from running for office by the Comptroller General (Tribunal Supremo de Justicia 2021; Contraloría General de Venezuela 2021). Candidate bans imposed by the Comptroller General are supposed to be published and notified to the electoral council ahead of the election, so disqualified candidates are typically unable to get their name on the ballot (Contraloría General de Venezuela 2009, Article 113); the Comptroller General never clarified why in this instance his office had failed to comply with this legal provision.

The only precedent in Venezuela's post-1999 history that in some sense resembles the case of the 2021 Barinas election is the annulment of the 2004 mayoral elections in the Catatumbo municipality of Amazonas state (Tribunal Supremo de Justicia 2006). In that case, the Electoral Chamber of the Supreme Court found that the elected mayor had held public office at the time of his election, contravening a law that required mayoral candidates to resign from those offices before registering their candidacies. In contrast to the case of Barinas studied in this paper, the reason that the winner in Catatumbo had been allowed to run was that there were unresolved differences at the time in the interpretation of what type of public positions the bar on candidates holding public office applied to.

Some observers saw the Court's decision to annul the Barinas 2021 election as a blatant act of interference through which Maduro loyalists used their institutional control to overturn what had been an embarassing loss for the government against a hardline opposition candidate in its symbolic stronghold of Hugo Chávez's home state[12] Yet while it was not the first time that the

12. Freddy Superlano was at the time a member of the Popular Will (VP) party, which many government

government had sought to invalidate the effects of an important election, it was the first time it did so through the scheduling of a repeat election.[13]

One possible reason for the government's use of a repeat election to overturn a result is that the board of the National Electoral Council (CNE) at the time, born out of negotiations that aimed at convincing the MUD to participate in elections after several years of boycotts, was relatively independent. Enrique Márquez, who served as one of the opposition's two representatives on the board at the time, explained the thesis that the scheduling of the repeat election was due to resistance in the CNE to alter the vote count. In his response to an interview for this paper, he stated:

> *The repeat of the elections in Barinas...was a completely unexpected and unforeseen event. In fact, the government's first option was not to repeat the election, but to force the proclamation of the government candidate Argenis Chávez in the CNE. This ended up not being politically viable, and the government...forced the repeat of the elections using its control of the Supreme Court.*

To gather more systematic evidence of the unexpected nature of the event, I interviewed ten prominent journalists who covered Venezuelan elections at the time. I asked them whether they were surprised by the repeat of the 2012 elections and whether there was any indication prior to the first election that the Supreme Court would end up annulling the results. Eight respondents indicated that the scheduling of the Barinas repeat election in 2021 was a surprise. One respondent claimed that the repeat of the election was not surprising given that the opposition had won the November election, but that the opposition victory in that election had been

leaders have characterized as the most hardline wing of the opposition. Among the party's main leaders are former Chacao mayor Leopoldo López, who was sentenced to 14 years in prison for allegedly leading violent anti-government protests, and former National Assembly President Juan Guaidó, who proclaimed himself interim president in 2019 on the argument that Maduro's 2018 re-election was invalid (La Fuente 2015; Rosati and Vásquez 2019). In April 2019, López and Guaidó openly called on the military to rise up against the government (López 2019). PSUV vicepresident Diosdado Cabello has claimed that VP is "made up of pure criminals and violent people" (Con el Mazo Dando 2023). In 2020, the pro-government Prosecutor General asked the Supreme Court to classify VP as a terrorist organization (the Supreme Court declined to hear the case) (Reuters 2020; Tribunal Supremo de Justicia 2020). Therefore, while it may be argued that the government was willing to tolerate an opposition leader as Barinas governor, it may have been unwilling to tolerate a representative of what it has claimed is the most extreme faction of the opposition.

13. In December 2015, the Supreme Court suspended the election of four legislators in the state of Amazonas pending a trial on fraud allegations against the opposition party; the suspension of those legislators ensured that the opposition would fall below the two-thirds threshold necessary to claim a powerful supermajority (Tribunal Supremo de Justicia 2015). In October 2017, opposition candidate Andrés Velásquez accused the CNE of altering the results of tally sheets for the Bolívar gubernatorial election to hand victory to his pro-government opponent (Moreno 2017). While these constitute two cases in which officials are credibly accused of having undermined the integrity of elections in order to favor the government, in neither of them did the actions of the government lead to the holding of a repeat election.

unexpected. Anotherrespondent indicated no clear recollection either way. All respondents who answered the second question said that there were no indications prior to the first election that the Supreme Court would annul the results. Details on these interviews and responses are given in Appendix C.

In sum, during the campaign for the 2021 regional elections, there were no reasons to expect that the Barinas election would end up being repeated. Repeat elections were rare in Venezuela and were usually held only in exceptional cases such as death or conviction of the elected official. And while the government was believed to have sometimes interfered with the results of elections that it held to be symbolically or strategically important, it had never done so by ordering a repeat election. The holding of the special election in Barinas appears to have resulted from the confluence of two factors: the appointment that year of a relatively independent CNE, and the victory of a hardline opposition leader in a symbolic government stronghold.

3.2 The CLAP food packages program

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. 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).

A majority of local CLAP committee members is designated by pro-government political organizations. Of the nine members in each committee, four are appointed by social or political movements which are not formally part of the structure of the state and one of them is appointed

by the Bolivarian militias, which is formally affiliated with the armed forces.[14] Two are elected in local assemblies, one is appointed by the communal council, and the last one is elected by the eight designated members.

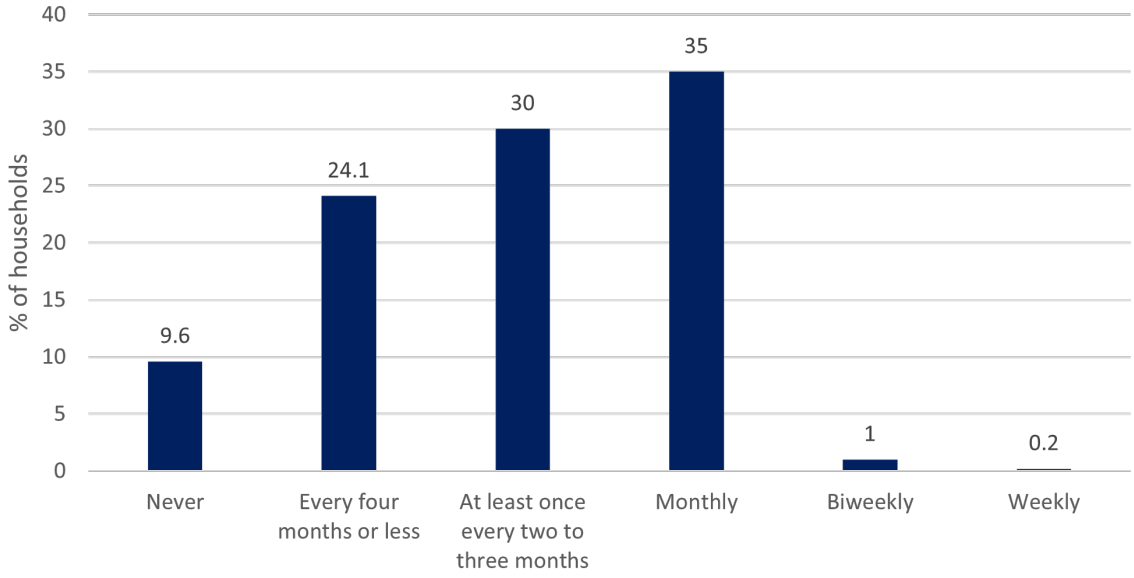
Once a CLAP committee is established in the community, it carries out a house-by-house census to determine how many families will receive food packages. In principle, all families in the community are eligible to receive one package with every delivery. Some families are eligible to receive more than 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 *manzanero* 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 order 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:

14. The entities that appoint CLAP committee members are the National Women's Union (UNAMUJER), a feminist organization created by Maduro; the Francisco de Miranda Front, an organization created by former-president Hugo Chavez and former Cuban leader Fidel Castro to support and survey social programs, and the Units of Battle Hugo Chávez (which appoints two members), civilian groups created to "defend the revolution" and affiliated to the governing Socialist Party. These three organizations are either known or believed to receive significant government funding and are active participants in pro-government proselytism.

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)

> *"it's impossible for there to be enough for everyone. Thus, we always favor more the one that lends more support, that helps, that is there when one must be with the President, and it's not because this is political, but because if we're getting this it's because of him [Maduro], so we have to be grateful, no? Those people who are against, well then they shouldn't ask for the bags because it's not fair"* (Transparencia Venezuela 2017)

Another CLAP committee member from Miranda state also cited the idea that rationing to favor government supporters when food packages were insufficient was morally justified:

> *"If enough food arrives for everyone, good. But if we have to prioritize, we prioritize...I am not going to stop giving food to a revolutionary to give it to someone who is then going to end up band-mouthing the government."* (Analítica 2016)

Because CLAP distribution is segmented by communities and because each *manzanero* oversees distribution for no more than 15 families, CLAP distribution teams can get to know the families receiving packages quite well. While it appears unlikely that coordinators would have direct information on how recipients cast their votes in elections, they will have direct information on many of their public political activities, which are of course imperfect proxies for their voting behavior. CLAP coordinators belong to the same political organizations that are in charge of mobilizing voters to government candidate rallies and would thus know whether recipient took part — or refused to participate — in government or opposition campaign ac-

tivities. For example, a recipient in the Caracas parish of La Pastora claimed that a group of residents stopped receiving packages after receiving a visit from opposition legislators to the community. Another community member claimed that "opposition supporters are left in last place and if the bag has a package of milk, they take it away" (León 2016).

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). Despite these denials, the government has not shied away from publicly using the threat to condition access to the CLAP and other social programs on voting. During a 2018 presidential campaign rally, President Nicolás Maduro said:

> *"You give, I give. I am thinking of giving a prize to the people of Venezuela who come out and vote [on election day], with the Card of the Fatherland, I'm thinking of it. For democracy, for liberty. You give, I give....What do you think if we give a special award to the democratic people of Venezuela? Do you like the idea?"* (Maduro 2018)

While Maduro backtracked on this particular commitment — which would have violated provisions in Venezuelan law that forbid explicit vote-buying — the government has routinely distributed food bags on election day at pro-government mobilization booths, also known as "red points" located outside of voting centers (Penfold 2018; Rodríguez and Navarro 2018). 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. (Zambrano 2022; Coscojuela 2022; Leizaola 2022; BBC 2022).

The evidence presented is thus consistent with the existence of both explicit and implicit quid pro quos in which the government makes offers to voters of access to CLAP benefits in exchange for political support. This suggests that the theories of clientelism discussed in sections 1 and 2, premised on conditionality and contingency of access to benefits, are well-suited to study their distribution. As in many political systems, explicit vote buying is formally illegal in Venezuela, and the government has neither the authority nor the capacity to monitor individual votes. Nevertheless it is clear that both government leaders and program administrators devote significant efforts to signaling that access to publicly provided benefits is tied to electoral support, and that many Venezuelans believe this to be the case.

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.[15] 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, the etsimated sampling error is 2.7 percent for Apure and 2.8 percent for Barinas.

Figure 3: Index of similarity to Barinas

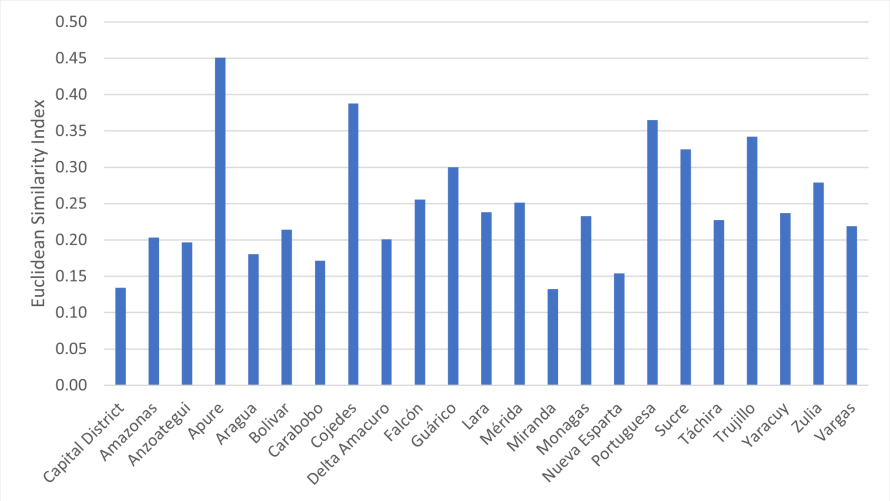


Figure represents the Euclidean similarity index between Barinas and other Venezuelan states based on data on indicators of access to public services, schooling, poverty, dependency ratios, employment, infant mortality and food insecurity from ENCOVI (2021). See Appendix B for calculation details

There are two basic reasons in favor of using Apure respondents as our control group. One is the similarity of both states in terms of key socio-demographic characteristics. As Figure 3 shows, Apure is by far the state closest to Barinas in terms of a proximity metric based on indicators of social and economic development.[16] The other reason is that Apure, like Barinas

15. See Rodríguez, Bravo, and Guerrero (2023) for a description of the data.

16. See Appendix B for details of indicators and calculation as well as alternative metrics. Because the Oil for Venezuela survey only covered the states of Apure and Barinas, the tests presented in this paper cannot be

(and unlike most other states) saw a very tight race in the November gubernatorial election, with the PSUV candidate eking out victory over his MUD opponent by a 1.8 percentage point margin. This suggests that a key potential determinant of CLAP allocations prior to the first election — the degree to which the election is effectively contested — was also similar in both states. Furthermore, since the government candidate won in Apure, both Barinas and Apure had a government party governor during our treatment period. In contrast, in the state of Cojedes (the second closest to Barinas after Apure in terms of the Figure 3 similarity measure), the opposition candidate won the election and assumed office during the treatment period, while in Portuguesa (the third closest), the PSUV candidate won the November election by a landslide, more than doubling the votes of his MUD opponent (CNE 2022).

Table 2: Sample moments and balance tests of respondent characteristics unweighted and entropy-balance weighted

	Barinas			Apure			Difference in means	Standardized difference in means
	Mean	Variance	Skewness	Mean	Variance	Skewness		
Unweighted								
Male	0.31	0.22	0.80	0.32	0.22	0.77	-0.006	0.012
Age	48.7	207.7	-0.15	47.6	220.6	-0.04	1.136*	-0.078
Non-slum	0.16	0.14	1.82	0.15	0.13	1.93	0.011	-0.031
Income	135.5	25982.0	1.45	168.1	39095.0	0.95	-32.608***	0.181
Secondary schooling or higher	0.72	0.20	-0.99	0.64	0.23	-0.61	0.078***	-0.167
Employed	0.52	0.25	-0.08	0.47	0.25	0.13	0.052***	-0.104
Weighted								
Male	0.31	0.22	0.80	0.31	0.22	0.80	0.000	0.000
Age	48.7	207.7	-0.15	48.7	216.1	-0.13	0.001	0.000
Non-slum	0.16	0.14	1.82	0.16	0.14	1.82	0.000	0.000
Income	135.5	25982.0	1.45	135.5	32241.0	1.37	-0.024	0.000
Secondary schooling or higher	0.72	0.20	-0.99	0.72	0.20	-0.99	0.000	0.000
Employed	0.52	0.25	-0.08	0.52	0.25	-0.08	0.000	0.000

Table shows sample moments for key respondent characteristics in the states of Apure and Barinas, as well as t tests for comparisons in means and standardized differences in means comparisons. Significance tests reported only for the diffference in means comparison. Levels of significance: *-10%, **-5%, ***-1%

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, the survey asked

applied to other states using their data.

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. They 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. They separately asked whether the delivery of packages in December had occurred earlier or later than normal. They also asked respondents who claimed to have received boxes what conditions they believed they had to comply with to receive the boxes, and asked non-recipients what were the reasons they thought they had not received the boxes. They further asked respondents what political group they identified with, as well as who they had voted for in the November and January elections.[17]

I explore differences between Apure and Barinas respondents in Table 2, which reports sample moments and balance tests for differences in means. Note that, given that I plan to use a difference-in-differences approach, covariate balancing is not necessarily relevant to assess the validity of the research design (Wing, Simon, and Bello-Gomez 2018; Zeldow and Hatfield 2021). Nevertheless, covariate balancing may be reassuring as it suggests a lower risk that time-varying factors may be affecting outcomes and thus generating artificial treatment effects.

The results in Table 2 show no systematic differences across states in terms of gender, neighborhood type and age. In fact, sample moments in the first of these two characteristics are nearly identical. However, they do show some differences in nominal income, level of schooling and employment rates. The last column of Table 2 shows standardized difference in means tests; according to this criterion, only these three variables exceed the Austin (2009) threshold of a standardized differences greater than 0.1 (the last of them just barely so). From a quantitative standpoint,the differences in means of the last two variables (8% higher rate of schooling and 5% higher employment rate in Barinas than Apure) may not be large enough to drive substantial differences in outcomes, while nominal income differences could simply be reflecting cross-state differences in price levels instead of real income.[18]

As an additional robustness test, I apply an entropy balancing estimator suggested by Hainmueller (2012) to address problems of selection on observables. The method relies on reweighting the control group observations so that they will replicate the characteristics of the sanctioned

17. Details of the precise wording of questions and construction of variables are provided in Appendix E

18. Regrettably, there is no publicly available price data allowing us to measure price level differences at the state level.

observations, allowing us to reasonably replicate the similarity of control and treatment group characteristics that we would obtain in a pure experimental setting. The bottom panel of 2 shows how reweighting using entropy balancing weights eliminates any systematic differences between sample moments. Entropy balancing results corresponding to those found in the main text can be found in appendix tables A11, A12, and A15.

For the purposes of the regression analysis, I will distinguish between five different time periods: the month of October, the days in November previous to the November 21 elections, the period between the November 21 elections and the January 9 elections, the days of January after the January 9 elections, and the mot of February. 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 I label the high-quality sample) and those for which the answers are inconsistent (the low-quality sample).

Figure 4: Share of families receiving packages, Apure and Barinas

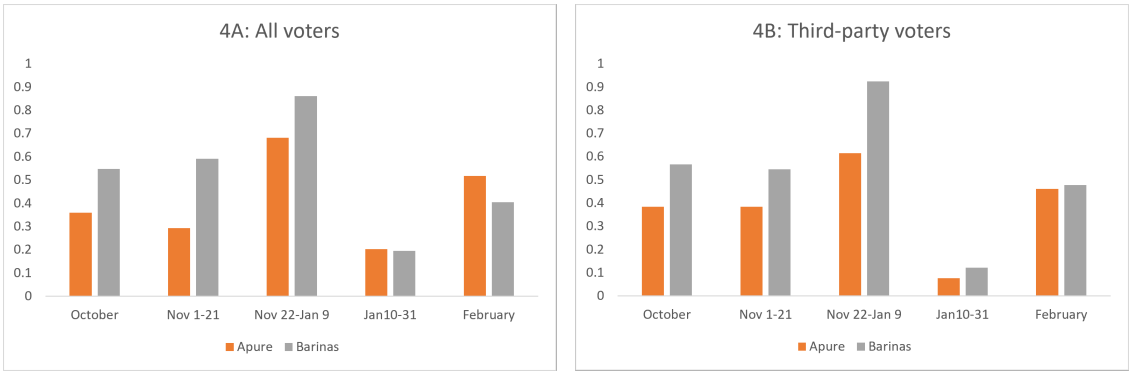


Figure represents the share of families that report receiving packages in each state in the periods of October, November 1-21 (before the regular election), November 22-January 9 (after the regular election and before the special Barinas election), January 10-31 (after the special election) and February.

Figure 4 shows the evolution of the share of families receiving packages in Apure and Barinas over these periods, both for all voters and for voters who supported third-party candidates in the first election. Both panels show a significant increase in participation in both states in the period between the first and the second election. In the case of all voters, however, the increase is lower in absolute magnitude for the state of Barinas, contrary to what would be expected. On the other hand, after the second election is held, participation declines much more rapidly in Barinas than in Apure, suggesting that part of the reason they may have been so high was the holding of the special election. In the case of the second panel, we get a more unequivocal pattern: beneficiaries increase more rapidly in Barinas during the campaign for the special

election, and also drop more rapidly after the special election.

I go on to evaluate the hypotheses laid out in section 2 more systematically using two-way fixed effects regressions. In the upper panel of Table 3, I present the results of panel 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,[19] I use a Poisson fixed effects pseudomaximum likelihood estimator (Correia, Guimarães, and Zylkin 2020). The lower panel presents 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.[20] 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.

My baseline specification in the first column of Table 3 (upper panel) controls for individual and time effects and uses the high quality sample with standard errors clustered at the municipality level. The point estimate of the treatment effect is positive yet borderline significant. In column 2 I introduce a treatment group trend. Controlling for a treatment group trend is needed when the parallel trends assumption does not hold; even when it holds, a treatment group trend reduces bias and maintains reasonable power to detect a treatment effect (Bilinski and Hatfield 2018). Doing so does not meaningfully affect the point estimate nor the statistical significance of the treatment effects. A similar result holds after I control for municipality trends (column 3) or restrict to high-quality observations (columns 4-6). The lower panel of Table 3 reports logistic panel estimates, where I use the receipt of at least one food package as the dependent variable. The estimated treatment effect in columns 1 and 4 is positive and significant, indicating that the odds of receiving a box in Barinas was 3.9 times higher in Barinas relative to Apure in the treatment period. This estimate is robust to the inclusion of treatment or municipality trend and restricting to the high-quality sample.

The first three columns of Table 4 show the results of estimating separate treatment effects by the political self-identification of respondents. Note that the treatment effect reported in

19. Given 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.

20. I 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 3.

Table 3: Treatment effect estimates

	1	2	3	4	5	6
Dependent variable	Number of food packages received					
Treatment effect	.245*	.28*	.27*	0.248	.282*	.272*
	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
N	10,941	10,941	10,941	10,662	10,662	10,662
Pseudo R2	0.15	0.16	0.16	0.15	0.16	0.16
Dependent variable	Receipt of at least one food package					
Treatment effect	1.37***	1.48***	1.49***	1.4***	1.52***	1.53***
	(0.35)	(0.36)	(0.36)	(0.36)	(0.36)	(0.36)
N	10,212	10,212	10,212	9,962	9,962	9,962
Pseudo R2	0.25	0.28	0.29	0.26	0.29	0.30
Sample	All observations	All observations	All observations	High Quality	High Quality	High Quality
Treatment group trend	No	Yes	No	No	Yes	No
Municipality trends	No	No	Yes	No	No	Yes

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 packafes. 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%

the first row refers to the incremental packages received by the base category (pro-Maduro chavistas). This is positive and borderline significant in some of the specifications shown. Note that the point estimate of the anti-MUD opposition group is the highest one on the table. While it is not statistically different from the base category treatment effect, the sum of the two coefficients (which captures the effect of being an anti-MUD opposition voter in Barinas relative to Apure) is significant at p=.051 in column 1 and p=.036 in column 2.

One possible source of concern regards potential heterogeneity of treatment effects by categories distinct from political self-identification. For example, the fact that respondents do need to pay for the boxes suggests that some low-income families may not have the capacity to ac-

quire them even if they would benefit from access to subsidized food.[21] In the third column of Table 4 I introduce treatment interaction effects with income, education group and neighborhood type to control for this heterogeneity. While the treatment effect for the base category declines, the one for anti-MUD opposition voters strengthens mildly. The interactions between the treatment effect and education and neighborhood type groups are jointly significant, but that for income levels is not.

One striking result of Table 4 is that the point estimates for non-aligned voters are negative and significant in some specifications. This means that the point estimate for the treatment effect of being non-aligned is lower than that of the base category (pro-Maduro chavistas) and thus lower than that of any other political group. 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).

In columns 4-6 I estimate separate treatment effects for groups of voters according to how they report having voted in the November elections. This specification is closer to that of the three-party model presented in section 2.2. A predicted by Proposition 3, the strongest treatment effect in this specification corresponds to voters who claim to have voted for third-party candidates in the November election. The difference between the effect of being a third-party voters and the base category is statistically significant at $p < .01$ in all three specifications. Controlling for municipality trends and interactions between treatment and socioeconomic controls, someone who reports having voted for a third-party candidate in the November election in Barinas was likely to receive 26% more packages than someone who claimed to have voted for the PSUV candidate.

21. The price of CLAP packages in the period of study oscillated between VED 3.5 and VED 7 (USD 0.76-USD 1.52), equal to 35-70% of the minimum legal remuneration of VED 10 at the time. See Redacción El Pitazo (2022)

Table 4: Treatment effect estimates by political group

	1	2	3	4	5	6
Dependent Variable	Number of food packages received					
Treatment Effect	.249*	.276**	0.187	.253*	.28*	0.190
	(0.14)	(0.14)	(0.22)	(0.14)	(0.15)	(0.23)
Effect by political self-identification (base category: pro-Maduro chavistas)						
Anti-Maduro chavistas	0.054	0.061	0.045			
	(0.06)	(0.07)	(0.07)			
Pro-MUD opposition	0.032	0.025	0.014			
	(0.06)	(0.05)	(0.04)			
Anti-MUD opposition	0.060	0.057	.0567*			
	(0.04)	(0.04)	(0.03)			
Non-aligned	-0.066	-.0697*	-.0697**			
	(0.04)	(0.04)	(0.03)			
Did not answer	00.122	0.116	0.138			
	(0.21)	(0.21)	(0.22)			
Effect by November election vote (base category: voted for PSUV candidate)						
Voted for MUD candidate				-0.0066	-0.0087	0.0047
				(0.02)	(0.02)	(0.02)
Voted for third-party candidate				.209***	.204***	.234***
				(0.08)	(0.08)	(0.09)
Did not vote				0.023	0.024	0.030
				(0.07)	(0.07)	(0.08)
Did not answer				-.0709**	-.076**	-.0815**
				(0.04)	(0.04)	(0.04)
N	10941	10941	10941	10941	10941	10941
Adjusted R2	0.149	0.159	0.16	0.149	0.159	0.16
Sample	All observations	All observations	All observations	All observations	All observations	All observations
Municipality trends	No	Yes	Yes	No	Yes	Yes
Treatment * Socio-economic interactions	No	No	Yes	No	No	Yes

Columns 1-3 estimate separate treatment effects according to political self-identification, while 4-6 estimate them according to reported vote in the November election. All estimates use the complete sample; columns 2-3 and 5-6 control for municipality trends. Columns 3 and 6 include interaction effects between treatment and income, education group and neighborhood type. See Appendix Table A13 for results restricted to high-quality sample and Appendix Table A12 for entropy balance reweighted results. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%.

I turn now to the evidence on whether the distribution of boxes was brought forward or delayed during the month of December. 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 5: Timing of December boxes and political conditioning

	1	2	3	4	5	6
Dependent variable	December boxes arrived late		December boxes arrived early		Access to boxes politically conditioned	
Treatment effect	-0.353	-0.368	1.34***	1.32***	-0.513	-0.475
	(0.31)	(0.29)	(0.35)	(0.34)	(0.51)	(0.50)
N	1,879	1,879	1,786	1,786	2,065	2,065
Pseudo R2	0.02	0.03	0.08	0.09	0.03	0.03
Sample	All	All	All	All	All	All
Social and demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Political identification controls	No	Yes	No	Yes	No	Yes

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 A14 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 Table 5 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. The last two columns use the likelihood of voters claiming that access to boxes was politically conditioned as a dependent variable (see Appendix E 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 6 I consider whether there is evidence that changes in the number of food packages received are correlated with voters' decisions to switch their vote to support the government between the November and the January elections in Barinas. I consider whether voters who voted against the government in November switched to voting in favor of it in January. 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. However, the variable is not significant after controlling for municipality fixed effects. Centering on the point estimates, these suggest that receiving an additional food package is associated with an increase of between 9 and 33 percent in the odds of recipients switching their vote in favor of the government. 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, could have made sense from a strategic viewpoint.

Table 6: Vote-switching behavior and packages received

	1	2	3
Dependent variable	Switched vote in favor of government		
Change in food packages received	.090	.286**	.281
	(0.14)	(0.12)	(0.19)
N	789	620	620
Pseudo R2	0.06	0.20	0.23
Sample	All observations	All observations	All observations
Social and demographic controls	Yes	Yes	Yes
Political identification controls	No	Yes	Yes
Municipality fixed effects	No	No	Yes

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. Controls include gender, age group, income group, education, employment, and neighborhood type. Column 2 adds controls for political self-identification while column 3 adds municipality fixed effects. All estimates use the complete sample; see Appendix Table A16 for results restricted to high-quality sample. Measure of packages received is adjusted for seasonality using coefficients from linear fixed effects regression of packages received in time effects as in Appendix Table A10. Standard errors are clustered at the municipality level. Levels of significance: *-10%, **-5%, ***-1%.

It is worth underscoring a number of caveats that could limit the generalizability of our results. One obvious concern regards the external validity of our findings outside of the setting of a special election. The same elements that made the 2022 Barinas election an unprecedented and atypical event also make it systematically different from many elections. One important difference is that, precisely because the repeat election occurred in only one state, the government may have had more resources to spend in it than it would have had if elections were being

held across the whole nation. Furthermore, the fact that the election was held so soon after a prior election may have given government party intermediaries access to better information on the political leanings of recipients than they would have in a regularly scheduled election.

At a broader level, during the period of study, Venezuela was undergoing the largest peacetime economic collapse documented in modern economic history and had transitioned from being one of the region's stellar democracies to a hybrid authoritarian regime (Corrales 2023; Rodríguez 2025). We should thus exercise caution in extending the findings of the study to institutional settings with greater constraints on executive power and stronger economic performance. Alternatively, however, one could also argue that a setting with fewer constraints on executive power may be an ideal one to study clientelism in its pure form precisely because it allows us to abstract from the effects of restrictions that impede governments from doing what they intend to do.

An additional source of concern regards the potential biases that may be introduced when asking respondents to report past actions or events. This should be a particularly relevant concern given evidence that respondents may have reason to fear government retribution as a result of revelation of their political preferences. For example, Hsieh et al. (2011) found evidence that the publication by the Venezuelan government of the list of signers of a recall referendum petition in 2004 led to a decline in the wages and employments prospects of petition signers.

These concerns notwithstanding, it is worth noting that our results do not conform to what one might expect if experimenter demand bias was a serious issue. For example, if respondents were more likely to claim to have voted in favor of the government after having received the boxes, we would expect the non-base category effects in Table 4 to be negative (i.e, for those who claim to have supported other candidates to get less boxes than those who claim to have supported the government). In contrast, they are almost all positive in sign and several are significantly so. More specifically, experimenter demand bias does not seem capable of explaining why respondents who received more boxes claim to have supported third-party candidates in the November vote.

One reason why experimenter demand bias may not be that severe in this setting is that the subsidy implicit in CLAP packages, while relevant, is moderate in comparison to recipient's spending. The market value of CLAP products consumed accounts for 7.1% of the average household's food consumption.[22] As Mummolo and Petersen (2019) and Diva, Jain, and Jay-

22. Own calculations based on the ENCOVI 2020 national survey. The average for Apure and Barinas is 7.0%

achandran (2022) have shown recently, concerns regarding experimenter demand bias may not always be as prominent as feared in many applied settings.

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 are 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 clientelist 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 the probability of receiving food packages rose in Barinas relative to Apure during the treatment period. I also find that the government targeted food packages to swing voters, in particular those who had supported third-party candidates in the prior election. Voters who supported a third party candidate in Barinas were likely to receive 26 percent more packages than die-hard Maduro supporters. Barinas voters were also more likely to report that packages had arrived earlier during the campaign for the special election and more likely to switch their votes in favor of the government when they received more food packages.

The results are consistent with the prediction of the classic spatial model of voting according to which parties will direct benefits that can be traded for political support towards swing voters. In contrast, many established theories of clientelism emphasize the idea that governments will direct benefits to their core supporters. I have argued that the conventional cross-sectional correlations often used in support of these theories could reflect other influences different from the intention of governments to win elections.

This does not of course mean that the evidence of a cross-sectional correlation between party affiliation and receipt of government benefits should be disregarded. It is certainly the

case that core government supporters are systematically overrepresented in clientelist programs. What our results suggest is that winning elections does not appear to be the main driver of that correlation, because the holding of elections makes governments less, not more, likely to transfer resources to core supporters relative to pivotal voters.

More generally, the results in this paper 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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Appendix A: Proof of Propositions

Proposition 1. *$q_{bG} \geq q_{bO}$ with the strict inequality holding as long as there are some voters in the population that do not receive benefits.*

Proof. The fraction of government sympathizers in the population is ϵ, whereas the fraction of opposition sympathizers is $1-\epsilon$. By (3), the number of government sympathizers receiving benefits is ϵ while that of opposition sympathizers receiving benefits is βb as long as $\epsilon + \beta b < 1$. It follows that $q_{bG} = 1$ and $q_{bO} = \frac{\beta b}{1-\epsilon} < 1$ as long as $\epsilon + \beta b < 1$. If $\epsilon + \beta b = 1$ then $s_S = s_O = 1-\epsilon$ so that $q_{bO} = 1$. □

Proposition 2. *Increases in α and θ will both lead to declines in the the share of voters receiving benefits P_b but will have effects of opposite sign on the probability of being a government sympathizer conditional on receiving benefits, with $\frac{dq_{Gb}}{d\alpha} < 0$ and $\frac{dq_{Gb}}{d\theta} > 0$.*

Proof. From (3),

$$P_b = \epsilon + \beta b \tag{7}$$

so that

$$\frac{dP_b}{d\theta} = -\beta \frac{db}{d\theta} \tag{8}$$

and

$$\frac{dP_b}{d\alpha} = -1 + \beta \frac{db}{d\alpha}. \tag{9}$$

We derive the first-order condition for the government's maximization problem from (5):

$$\frac{dV}{db} = -4\theta\beta^2 b^3 - 6\theta\beta\epsilon b^2 - 2\theta\epsilon^2 b + \psi\beta = 0 \tag{10}$$

with second order condition:

$$\frac{d^2V}{db^2} = -12\theta\beta^2 b^2 - 12\theta\beta\epsilon b - 2\theta\epsilon^2 < 0 \tag{11}$$

Differentiating the first-order condition gives:

$$\frac{db}{d\theta} = -\frac{\left[4\beta^2 b^3 + 6\beta\epsilon b^2 + 2\theta\epsilon^2\right]}{\left[12\theta\beta^2 b^2 + 12\theta\beta\epsilon b + 2\theta\epsilon^2\right]} < 0 \tag{12}$$

and

$$\frac{db}{d\alpha} = \frac{\left[6\theta\beta b^2 + 4\theta b\epsilon\right]}{\left[12\theta\beta^2 b^2 + 12\theta\beta\epsilon b + 2\theta\epsilon^2\right]} > 0 \tag{13}$$

Note that

$$\beta\frac{db}{d\alpha} = \frac{\left[6\theta\beta^2 b^2 + 4\theta\beta b\epsilon\right]}{\left[12\theta\beta^2 b^2 + 12\theta\beta\epsilon b + 2\theta\epsilon^2\right]} \in [0, 1] \tag{14}$$

as

$$6\theta\beta^2 b^2 + 4\theta\beta b\epsilon < 12\theta\beta^2 b^2 + 12\theta\beta\epsilon b + 2\theta\epsilon^2 \tag{15}$$

(10) shows that the solution to b is the root of a third degree polynomial of the form

$$p(b) = Ab^3 + Bb^2 + Cb + D = 0 \tag{16}$$

with $A < 0$, $B < 0$, $C < 0$, $D > 0$. Let b_1, b_2 and b_3 be the three roots to this cubic polynomial. By Vieta's formulas:

$$b_1 + b_2 + b_3 = -\frac{B}{A} < 0 \tag{17}$$

$$b_1 b_2 b_3 = -\frac{D}{A} > 0 \tag{18}$$

To establish uniqueness, we prove that there is exactly one real positive root to this equation. We know that there is at least one real root because the limit of (10) as $b \to \infty$ is $-\infty$ and as $b \to -\infty$ is ∞. By the intermediate value theorem there must then exist a real number at which $P(b) = 0$. Suppose there were no real positive roots. Then either all three roots are real and negative, or there are non-real complex roots. If all three roots were real and negative, then (18) cannot hold. Thus there must be complex non-real roots. Since these roots must come in conjugate pairs of the form $(t + ri, t - ri)$, then their product, $t^2 + r^2$ is positive. But then the real root must be positive by (18). Now, if there were more than one positive real root, then they would all have to be positive by (18). But that would contradict (17).

Since the fraction of government sympathizers in the population is ϵ, then by (3)

$$q_{Gb} = \frac{\epsilon}{\epsilon + \beta b}, \tag{19}$$

so that

$$\frac{dq_{Gb}}{d\theta} = -\frac{\epsilon}{(\epsilon+\beta b)^2}\beta\frac{db}{d\theta} > 0 \tag{20}$$

and

$$\frac{dq_{Gb}}{d\alpha} = -\frac{\beta b}{(\epsilon+\beta b)^2} - \frac{\epsilon}{(\epsilon+\beta b)^2}\left(\beta\frac{db}{d\alpha}\right) < 0 \tag{21}$$

since $\frac{db}{d\alpha} > 0$ from (13). □

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 (9) and (21), an increase in α will generate a decline in P_b and a decline in q_{Gb} while from (8) and (20), an increase in θ will generate a decline in P_b and an increase in q_{Gb}. □

Proposition 3. *The average benefit received by all voters as well as the average benefit conditional on accepting the benefit will be weakly decreasing in θ. A decline in θ will also lead to a proportionate increase in the expected value of benefits received by centrist sympathizers that is greater than or equal to the rise in the expected value of benefits received by government sympathizers.*

Proof. We begin by establishing that b^* is weakly monotonically decreasing in θ. From (6) we can write

$$P_b = \begin{cases} \beta_c b + \epsilon_c & \text{if } b \leq \bar{b} \\ \beta_o b + \epsilon_o & \text{if } b \geq \bar{b} \end{cases} \tag{22}$$

and

$$P_v = \begin{cases} \epsilon_c + \psi\beta_c b & \text{if } b \leq \bar{b} \\ \epsilon_c + \psi(\beta_o b + \epsilon_o - \epsilon_c) & \text{if } b \geq \bar{b} \end{cases} \tag{23}$$

The government's objective will then be:

$$V = \begin{cases} V_1 = \epsilon_c + \psi\beta_c b - \theta(b(\beta_c b + \epsilon_c))^2 & \text{if } b \leq \bar{b} \\ V_2 = \epsilon_c + \psi(\beta_o b + \epsilon_o - \epsilon_c) - \theta(b(\beta_o b + \epsilon_o))^2 & \text{if } b \geq \bar{b} \end{cases} \tag{24}$$

Note that V_1 and V_2 are identical to (5) up to a constant with β and ϵ replaced respectively by β_c and ϵ_c or β_o and ϵ_o. Therefore their solutions will be defined by first-order conditions identical to (10). It also follows that they will be unique, single-valued functions of θ (see proof of Proposition 2).

We now establish that V is continuous at $\bar{b}$. Setting $V_1 = V_2$ gives us:

$$\psi(\beta_c - \beta_o)b - \psi(\epsilon_o - \epsilon_c) = \theta(b(\beta_c b + \epsilon_c))^2 - \theta(b(\beta_o b + \epsilon_o))^2 \tag{25}$$

Evaluating the left side of this equation at $b = \bar{b} = \frac{1}{4\beta_c\beta_o}$ gives:

$$\psi(\beta_c - \beta_o)b - \psi(\epsilon_o - \epsilon_c) = \psi\left[\frac{\pi_o - \pi_c}{2} + \frac{\pi_c - \pi_o}{2}\right] = 0 \tag{26}$$

while the right-hand collapses to:

$$\theta b^2\left[\frac{\beta_c^2}{16\beta_c^2\beta_0^2} + 2\frac{\beta_c\epsilon_c}{4\beta_c\beta_o} - \frac{\beta_o^2}{16\beta_c^2\beta_0^2} - 2\frac{\beta_o\epsilon_o}{4\beta_c\beta_o} + \epsilon_c^2 - \epsilon_o^2\right] = 0 \tag{27}$$

which demonstrates that V is continuous in b. Therefore:

$$b^* = \begin{cases} b_c & \text{if } \begin{cases} b_c < \bar{b}, b_o > \bar{b}, V_1(b_c) > V_2(b_o) \\ b_c < \bar{b}, b_o < \bar{b} \end{cases} \\ b_o & \text{if } \begin{cases} b_c < \bar{b}, b_o > \bar{b}, V_1(b_c) < V_2(b_o) \\ b_c > \bar{b}, b_o > \bar{b} \end{cases} \\ \bar{b} & \text{if } b_c > \bar{b}, b_o < \bar{b}. \end{cases} \tag{28}$$

By (10) we can see that as $\theta \to 0$ then $b_c \to \infty$ and $b_o \to \infty$. Thus, for the lowest values of θ, $b_c > \bar{b}, b_o > \bar{b}$ and $b^* = b_o$. By (12), both b_c and b_o are monotonically decreasing in θ. Let θ_k for $k = c, o$ be defined by the equation

$$b_k(\theta_k) = \bar{b} \tag{29}$$

Consider first the case in which $\theta_o < \theta_c$. As long as $\theta < \theta_o$ then $b_c > \bar{b}$ and $b_o > \bar{b}$ and by (28), $b^* = b_o > \bar{b}$. For $\theta \in (\theta_o, \theta_c)$ then $b_c > \bar{b}, b_o < \bar{b}$ and $b^* = \bar{b}$. For $\theta > \theta_o$, $b_c < \bar{b}, b_o < \bar{b}$ and $b^* = b_c < \bar{b}$. Therefore b^* declines monotonically with θ. Now consider the case in which

$\theta_c < \theta_o$. As long as $\theta < \theta_c$ then $b_c > \bar{b}$ and $b_o > \bar{b}$ and $b^* = b_o$. For $\theta \in (\theta_c, \theta_o)$ then $b_c < \bar{b}, b_o > \bar{b}$ and, by (28), whether b^* is equal to b_o or b_c will depend on whether $V_1(b_c) < V_2(b_o)$ or not. However, at θ_c, $V_1(b_c)$ must be less than $V_2(b_o)$ since $V_1(b_c(\theta_c)) = V_1(\bar{b}) = V_2(\bar{b}) < V_2(b_o(\theta_c))$ where the last step follows from the fact that $b_o(\theta_c)$ maximizes V_2 at θ_c. If $V_1(b_c) < V_2(b_o)$ for all $\theta \in (\theta_c, \theta_o)$ then $b = b_o$ over that range and then drops to $b_c < \bar{b}$ at $\theta = \theta_c$. Therefore b^* also declines monotonically with θ in this case. If, instead, $V_1(b_c) > V_2(b_o)$ for some $\theta_s \in (\theta_c, \theta_o)$ then b drops to b_c at the lowest θ_s for which this condition hold. Denote this threshold $\bar{\theta_s}$. As long as $V_1(b_c) > V_2(b_o)$ for all $\theta \in (\bar{\theta_s}, \theta_o)$ then $b = b_c$ for all $\theta > \theta_s$ so that b^* declines monotonically with θ. A sufficient condition for $V_1(b_c) > V_2(b_o)$ for all $\theta \in (\bar{\theta_s}, \theta_o)$ is for $V_1(b_c(\theta)) = V_2(b_o(\theta))$ at most at one point in (θ_c, θ_o). This is because we have already shown that $(V_1(b_c(\theta_c)) < V_2(b_o(\theta_c))$ and $\theta_c < \theta_s$. Begin by applying the envelope theorem to (24) so that:

$$\frac{dV_1}{d\theta} = \frac{\partial V_1}{\partial \theta} = -(b(\beta_c b + \epsilon_c))^2 \tag{30}$$

$$\frac{dV_2}{d\theta} = \frac{\partial V_2}{\partial \theta} = -(b(\beta_o b + \epsilon_o))^2 \tag{31}$$

We note that

$$\frac{\partial^2 V_1}{\partial \theta \partial b} = -2b(\beta_c b + \epsilon_c)^2 - 2b^2(\beta_c b + \epsilon_c)\beta_c < 0 \tag{32}$$

$$\frac{\partial^2 V_2}{\partial \theta \partial b} = -2b(\beta_o b + \epsilon_o)^2 - 2b^2(\beta_o b + \epsilon_o)\beta_o < 0 \tag{33}$$

From (30) and (31)

$$\frac{dV_1}{d\theta} < \frac{dV_2}{d\theta} \Leftrightarrow \beta_c b + \epsilon_c > \beta_o b + \epsilon_o \Leftrightarrow b > \bar{b} \tag{34}$$

Therefore:

$$\left.\frac{dV_1}{d\theta}\right|_{b=\bar{b}} = \left.\frac{dV_2}{d\theta}\right|_{b=\bar{b}} \tag{35}$$

Now note that by (32)

$$\left.\frac{dV_1}{d\theta}\right|_{b=\bar{b}} < \left.\frac{dV_1}{d\theta}\right|_{b_c<\bar{b}} \tag{36}$$

and by (33)

$$\left.\frac{dV_2}{d\theta}\right|_{b=\bar{b}} > \left.\frac{dV_2}{d\theta}\right|_{b_o>\bar{b}} \tag{37}$$

so that

$$\left.\frac{dV_2}{d\theta}\right|_{b_o(\theta)} < \left.\frac{dV_1}{d\theta}\right|_{b_c(\theta)} \tag{38}$$

Therefore, V_2 declines more rapidly than V_1 for any $\theta > \theta_o$. establishing that the functions can cross at most once in that range. This completes the proof that b^* is weakly monotonically decreasing in θ.

Since P_b is monotonically increasing in b) and does not depend on θ other than through b, then $P_b b$ will also be weakly monotonically decreasing in θ.

The expected value of benefits received by sympathizers of party J is defined as

$$E(b|i \in J) = bq_{bJ}. \tag{39}$$

By (6), $q_{bG} = 1$. Using (22), we can derive

$$q_{bC} = \begin{cases} \frac{\beta_c b}{\epsilon_o - \epsilon_c} & \text{if } b < \bar{b} \\ 1 & \text{if } b \geq \bar{b} \end{cases} \tag{40}$$

Taking derivatives of the logarithms of both of these expressions, we get

$$\frac{d \ln E(b|i \in G)}{d\theta} = \frac{1}{E(b|i \in G)} \frac{dE(b|i \in G)}{d\theta} = \frac{1}{b} \frac{db}{d\theta}. \tag{41}$$

$$\frac{d \ln E(b|i \in C)}{d\theta} = \frac{1}{E(b|i \in C)} \frac{dE(b|i \in C)}{d\theta} = \begin{cases} \frac{2}{b} \frac{db}{d\theta} & \text{if } b < \bar{b} \\ \\ \frac{1}{b} \frac{db}{d\theta} & \text{if } b \geq \bar{b} \end{cases} \tag{42}$$

We note that these expressions are only defined when $\frac{db}{d\theta}$ exists. To study changes around a point of discontinuity at $b^*(\theta_0)$ consider two levels of θ, θ_1 and θ_2, such that $\theta_1 < \theta_0 < \theta_2$. By (28), it must be that $b(\theta_1) \geq \bar{b}$. In that case, $q_{bC}(\theta_1) = 1$ and the proportionate changes can be calculated by assessing:

$$\frac{1}{E(b_1|i \in G)} \left[E(b_2|i \in G) - E(b_1|i \in G)\right] = \frac{1}{b_1}(b_2 - b_1). \tag{43}$$

$$\frac{1}{E(b_1|i \in C)} \left[E(b_2|i \in C) - E(b_1|i \in C)\right] = \frac{1}{b_1}(b_2 - b_1). \tag{44}$$

□

Appendix B: Endogenous platforms under state-space uncertainty

I now assume that policymakers define their platforms ahead of the election. As shown by Wittman (1973) and Roemer (1994; 2006, ch, 3), uncertainty about voter choices does not in itself generate policy divergence even if combined with non-opportunistic politician preferences. This is because probabilistic voter choices don't necessarily generate uncertainty over the distribution of votes. In order for vote uncertainty to lead to policy divergence, politicians must have preferences over policy outcomes and there must be uncertainty regarding the distribution of votes. I now consider a variant of the model where this occurs. Let the share of votes obtained by the incumbent party be

$$S_v = \begin{cases} 0 & \text{if } P_v + \zeta < 0 \\ P_v + \zeta & \text{if } P_v + \zeta \in (0,1) \\ 1 & \text{if } P_v + \zeta > 1 \end{cases} \tag{45}$$

where $\zeta \sim U[-\frac{1}{2}, \frac{1}{2}]$. It follows that the probability that the incumbent's vote share is greater than one-half will be given by

$$P\left(S_v > \frac{1}{2}\right) = P_v \tag{46}$$

Let preferences of party $k = \{g, o\}$ be given by:

$$V_k = P_v(-(\pi_g - \pi_{k^*})^2) + (1 - P_v)(-(\pi_o - \pi_{k^*})^2) - \theta(bP_b)^2. \tag{47}$$

The game proceeds in two stages. First, government and opposition politicians simultaneously set their policy platforms π_g, π_o to maximize their objectives as given by (47). Then the government sets b to maximize (47) in a second stage. Note that voters are assumed to either receive the transfer prior to the election, or for these transfers to be legislated in a way such that they cannot be changed after the election. In either case, there is no election-related uncertainty regarding the level of b nor the associated deadweight losses in (47). I solve the game by backward induction. In stage 2, the government takes π_g and π_o as given and sets b

to maximize (47). We can rewrite this expression as:

$$V = P_v\left(-(\pi_g - \pi_{g^*})^2 + (\pi_o - \pi_{g^*})^2\right) - (\pi_o - \pi_{g^*})^2 - \theta(bP_b)^2 \tag{48}$$

Note first that the middle term in (48) is a constant that is unaffected by b and thus can be ignored. From (3) $P_v = \epsilon + \psi\beta b$, so that $\frac{dP_v}{db} = \psi\beta > 0$. Thus if $|\pi_o - \pi_{g^*}| \leq |\pi_g - \pi_{g^*}|$, $\frac{dV}{db} < 0$ in (48) and thus the only solution to the government's second-stage maximization problem is $b = 0$. On the other hand, if $|\pi_o - \pi_{g^*}| > |\pi_g - \pi_{g^*}|$, then we can rewrite the government's maximization problem in the second stage as

$$\max_b V = P_v - \theta^*(bP_b)^2 \tag{49}$$

where

$$\theta^* = \frac{\theta}{(\pi_o - \pi_{g^*})^2 - (\pi_g - \pi_{g^*})^2} \tag{50}$$

The problem is thus identical to that in (4) and the solution will thus be identical to (10) if we substitute θ^* for θ.

Let $\{\pi_o^*, \pi_g^*, b\}$ denote the strategies corresponding to a subgame perfect equilibrium of the state-space uncertainty game describes in this appendix. Then we can establish

Proposition 4. *In any pure-strategy subgame perfect equilibrium of the state-space uncertainty game,* $\pi_o^* \neq \pi_g^*$.

Proof. The first-order condition for maximization of (50) in the second stage will be:

$$\frac{dV}{db} = \psi\beta - \theta^* 2b^2(\epsilon + \beta b)\beta - \theta^* 2b(\epsilon + \beta b)^2 = 0 \tag{51}$$

Let $b^* = b(\psi, \beta, \theta^*, \epsilon)$ denote the solution to (51). Let

$$V^* = P_v(b^*) - \theta^*(b^* P_b(b^*))^2 \tag{52}$$

Maximizing (52) with respect to π_g gives first-order condition:

$$\frac{dV}{d\pi_g} = -2(\pi_g - \pi_{g^*})P_v + \left(-(\pi_g - \pi_{g^*})^2 + (\pi_o - \pi_{g^*})^2\right)\left(\frac{dP_v}{d\pi_g}\right) - 2\theta b^2 P_b \frac{dP_b}{d\pi_g} = 0 \tag{53}$$

where I have used the envelope theorem. Substituting the definitions of P_v and P_b gives:

$$-2(\pi_g - \pi_{g^*})\left(\frac{\pi_o + \pi_g}{2} + \frac{\psi b}{2(\pi_o - \pi_g)}\right) + \left(-(\pi_g - \pi_{g^*})^2 + (\pi_o - \pi_{g^*})^2\right)\left(\frac{1}{2} + \frac{\psi b}{2(\pi_o - \pi_g)^2}\right)$$
$$-2\theta b^2\left(\frac{\pi_o + \pi_g}{2} + \frac{b}{2(\pi_o - \pi_g)}\right)\left(\frac{1}{2} + \frac{b}{2(\pi_o - \pi_g)^2}\right) = 0 \tag{54}$$

Multiplying by $(\pi_o - \pi_g)^3$ and evaluating at $\pi_o = \pi_g$, yields:

$$-\frac{\theta b^4}{2} < 0 \tag{55}$$

establishing that at any interior solution there must be policy divergence, i.e. $\pi_o \neq \pi_g$. Now assume that there was a subgame perfect equilibrium in which $|\pi_o - \pi_{g^*}| \leq |\pi_g - \pi_{g^*}|$. Maximizing (47) subject to this restriction yields the solutions $\pi_{g'} = \pi_o$ and $\pi_{g''} = 2\pi_{g^*} - \pi_o$. Inserting these into (48) gives:

$$V(\pi_{g'}) = V(\pi_{g''}) = -(\pi_o - \pi_{g^*})^2. \tag{56}$$

Assume instead that the government sets $\pi_g = \pi_{g^*}$. Then by (48):

$$V = (P_v - 1)(\pi_o - \pi_{g^*})^2) - \theta(bP_b)^2 \tag{57}$$

Note that in the second stage, the government can always set $b = 0$ and achieve

$$V(b = 0) = (\epsilon - 1)(\pi_o - \pi_{g^*})^2 \tag{58}$$

Therefore,

$$V(b = b^*) \geq V(b = 0) = (\epsilon - 1)(\pi_o - \pi_{g^*})^2 \geq V(\pi_{g'}) = V(\pi_{g''}) = -(\pi_o - \pi_{g^*})^2, \tag{59}$$

showing that neither $pi_{g'}$ nor $pi_{g''}$ can be best responses for the government in stage 1. Therefore at any pure-strategy subgame perfect Nash equilibrium it must hold that $|\pi_o - \pi_{g^*}| > |\pi_g - \pi_{g^*}|$. □

Appendix C: Political journalist interviews

I selected 12 journalists from national or international news agencies that covered Venezuelan politics at the time of the election. I posed two questions to them: (i) Do you consider that the calling to extraordinary elections made by the National Electoral Council for the governorship of Barinas on January 9 of 2022 was a surprise event? (ii) During the campaign for the first election on November 21 of 2022, was there any indication that would have led one to expect that the Supreme Court would end up annulling the results of that election and ordering the National Electoral Council to hold new elections? One respondent declined to answer the survey, indicating that they did not have sufficient knowledge of the case. One respondent did not respond to initial or follow-up requests. Respondent 1 answered only the first question, while respondent 5 requested anonymity. Table C summarizes responses to the interview. Full text of responses is available upon request.

Table A7: Summary of responses to political journalist interviews

No.	**Respondent**	**Position**	**Was the scheduling of the new election a surprise?**	**Prior to the first election, was there any indication that the Supreme Court would annul the results?**
1	Alejandro Hernández	Editor, La Gran Aldea opinion and news portal	Without a doubt the special elections for the Barinas governorship in January of 2022 constituted a surprising political event for Venezuelan political actors. The disqualification of the winning candidate and the scheduling of new elections could have been considered as a predictable coup given the undemocratic environment and the long history of non-acceptance of the results by the Chavista regime; however, the form in which the events developed were without a doubt unforeseen.	N/A

Summary of responses to political journalist interviews (continued)

No.	Respondent	Position	Was the scheduling of the new election a surprise?	Prior to the first election, was there any indication that the Supreme Court would annul the results?
2	Maripili Hernández	Radio Host, Union Radio	Yes, it was obviously a surprising event, as it was not foreseen that, after the election having been held, the winning candidate could be disqualified. Much less when several candidates, Superlano among them, had been "qualified" by the Comptroller General's Offices specifically so they could participate in this process.	During the campaign there was no indication that there was a possibility that the Supreme Court could annul the results motivated by the disqualification of the candidate because . . . they had been specially permitted to participate in the election. . . There were candidates who remained barred (despite the lack of any procedure for disqualification)...But candidates from the Unitary Platform, like Superlano and Guanipa, were qualified expressly so they could participate.
3	Eugenio Martínez	Director, Votoscopio electoral analysis portal	Surprising and unprecedented ("inédito"). It was unprecedented because the CNE suspended the vote tallying during one week. . . Surprising because a decision of this nature was not foreseen.	No. there were no indications of this because the Supreme Court decision came after the political disqualification of candidate Superlano. Nor were there indications that Superlano would be disqualified after the CNE had admitted his candidacy.

Summary of responses to political journalist interviews (continued)

No.	**Respondent**	**Position**	**Was the scheduling of the new election a surprise?**	**Prior to the first election, was there any indication that the Supreme Court would annul the results?**
4	Tony Frangie Mawad	Editor, Caracas Chronicles opinion portal	Yes, without a doubt it was surprising. There were many expectations that the elections would be cleaner than usual. Several barred political figures had been pardoned and the European Union had been invited [as observers]. The decision of the CNE to repeat the elections was a surprise for voters, the opposition and the European Union observers.	No, there was no [indication]. Rather, before the start of the electoral process there was a presidential pardon of previously disqualified figures and they were allowed to participate. There was also a backtracking with the appearance on the ballot of [opposition party] Justice First. There was thus no indication; the indications were to the contrary. However, the attitude of the TSJ was not a shocking element for Venezuelan observers, because we have seen in Venezuela what has been called authoritarian judicial activism.
5	Reporter	International News Agency	Not completely. What was surprising was the initial victory of the opposition. It was evident that losing Barinas, a state of great symbolic relevance for chavismo, was an important political defeat. Therefore, the calling of a new election...fits in a universe of possibilities under chavista logic after receiving a very significant electoral blow.	No. Chavismo felt confident postulating Argenis Chávez, brother of the late Hugo Chávez in his home state. I don't recall his campaign to have been particularly extraordinary in comparison with the campaigns of chavismo in other states with more legislators, leading me to think that the government overestimated the control that it had over the entity.

Summary of responses to political journalist interviews (continued)

No.	Respondent	Position	Was the scheduling of the new election a surprise?	Prior to the first election, was there any indication that the Supreme Court would annul the results?
6	Luz Mely Reyes	Director, Efecto Cocuyo news portal	I don't remember if it was a surprise. But I assume it was because he was not expected to win. Or it wasn't on the radar screen of the [media's] informative agenda.	There wasn't much reason to assume that there would occur anything different [from a normal election outcome]. Barinas becomes an issue after the results of November 21. Before that, the general vision was on the fact that the electoral route was being reconstructed.
7	Enderson Sequera	Director, Politiks political analysis portal	In one hypothesis, given the small margin of victory of Superlano, the government's control over electoral institution and its technical and logistical capacity to overturn the result, we can conclude that the holding of a repeat election was a surprise event. Alternatively, the close election result could have been considered as an opportunity that the governing coalition saw to sideline one of the last representatives of the Chávez family.	I don't recall any indication, and even less in the contest for the state of Barinas... There were few elements to foresee (i) a victory of Superlano, (ii) a victory of a VP candidate, and (iii) an annulment of the results by the TSJ followed by the holding of a repeat election by the CNE.
8	Ricardo Vaz	Editor, VenAnalysis news and opinion portal	The convening of extraordinary elections was undoubtedly a surprising and unexpected event.	Not only was there no indication that the TSJ could intervene to annul the election, but also the days after the vote were marked by the uncertainty given that the tallying of Barinas votes did not come to a close.

Summary of responses to political journalist interviews (continued)

No.	**Respondent**	**Position**	**Was the scheduling of the new election a surprise?**	**Prior to the first election, was there any indication that the Supreme Court would annul the results?**
9	Blanca Vera	Director, HispanoPost news portal	Yes, it was a surprising event. In fact, what was happening was not well understood. After some time, we realized that the government candidate was not well seen in the higher echelons of power.. That's why there is the proposal to repeat the elections with a different candidate.	No, there was no indication whatsoever.
10	José Gregorio Yépez	Director, Contrapunto news portal	Definitively, yes. Typically, facing this situation, the decision would have been to [declare the government candidate as winner and] ignore appeals of opposition sectors. The criterion that would have prevailed would have been a victory of the government in a dubious process.	No. It was not expected for the Barinas governorship to be among the possibilities of victory for the opposition. . . If the CNE had not recognized a Superlano victory, nothing would have happened.

Note: when necessary to summarize arguments, I have edited and moderately paraphrased them.

Complete text of responses are available from author upon request. Respondent 6 requested anonymity.

Appendix D: Additional estimates

Table A8: Social and economic development indicators for Venezuelan states, 2021

State	Dwellings with inadequate public service access (%)	Adults with complete secondary schooling (%)	Poverty (%)	Dependency ratio (%)	Employment (%)	Infant mortality	Food insecurity (%)
Capital District	11.2	97.4	87.3	47.0	54.0	19.6	85.3
Amazonas	73.0	79.6	96.8	67.0	38.2	35.1	100.0
Anzoategui	44.1	94.5	92.0	54.0	41.2	21.8	93.3
Apure	64.0	87.1	91.5	72.0	52.4	31.0	100.0
Aragua	43.3	93.9	92.6	47.0	51.0	20.3	90.8
Barinas	67.1	89.8	94.0	72.0	50.1	29.2	99.6
Bolivar	49.8	94.2	94.1	52.0	45.6	22.5	93.5
Carabobo	34.8	93.9	89.2	49.0	48.7	19.8	90.4
Cojedes	49.7	90.5	92.3	64.0	51.6	28.1	99.9
Delta Amacuro	78.1	81.5	93.3	59.0	37.7	34.9	100.0
Falcón	42.4	93.6	94.6	59.0	43.5	25.3	97.6
Guárico	42.7	92.4	92.6	59.0	50.7	27.4	100.0
Lara	46.6	87.2	90.0	58.0	49.9	22.5	93.8
Mérida	66.9	91.4	88.8	61.0	54.2	23.2	93.8
Miranda	25.9	90.4	84.6	43.0	51.0	20.8	83.1
Monagas	48.1	93.1	94.9	52.0	46.6	23.3	98.6
Nueva Esparta	22.7	96.9	88.4	47.0	52.0	20.3	90.3
Portuguesa	56.1	87.7	93.4	64.0	45.1	27.5	100.0
Sucre	59.2	90.8	95.4	64.0	45.7	23.1	100.0
Táchira	76.4	91.7	87.0	61.0	50.6	22.3	93.2
Trujillo	68.9	89.6	94.2	59.0	51.8	25.5	100.0
Yaracuy	26.2	90.9	96.5	61.0	46.1	23.4	100.0
Zulia	67.4	90.0	93.3	59.0	42.8	25.5	95.6
Vargas	58.8	95.6	90.8	56.0	49.1	21.2	92.6

Source: ENCOVI (2021)

Table A9: Distance and similarity to Barinas in economic and social development indicators, 2021

State	Euclidean	Squared Euclidean	Absolute Value	Minkowski (∞)	Canberra	Euclidean (1)	Euclidean (2)	Correlation	Angular
Capital District	6.4	41.5	16.1	3.2	6.5	0.13	9.9E-19	-0.67	-0.74
Amazonas	3.9	15.4	8.2	2.6	3.0	0.20	2.1E-07	0.72	0.58
Anzoategui	4.1	16.7	10.2	2.3	7.0	0.20	5.4E-08	-0.45	-0.64
Apure	1.2	1.5	2.6	0.8	2.2	0.45	2.3E-01	0.88	0.91
Aragua	4.5	20.5	9.9	3.2	5.7	0.18	1.2E-09	-0.86	-0.78
Barinas	0.0	0.0	0.0	0.0	0.0	1.00	1.0E+00	1.00	1.00
Bolivar	3.7	13.4	8.3	2.6	6.0	0.21	1.5E-06	-0.73	-0.59
Carabobo	4.8	23.3	11.5	3.0	6.5	0.17	7.3E-11	-0.83	-0.91
Cojedes	1.6	2.5	3.3	1.0	3.0	0.39	8.3E-02	0.55	0.81
Delta Amacuro	4.0	15.8	8.5	2.7	3.5	0.20	1.4E-07	0.59	0.42
Falcón	2.9	8.5	6.8	1.7	5.1	0.26	2.0E-04	-0.13	0.07
Guárico	2.3	5.4	4.7	1.7	3.8	0.30	4.4E-03	-0.15	0.44
Lara	3.2	10.2	7.6	1.8	5.5	0.24	3.6E-05	0.45	-0.26
Mérida	3.0	8.9	6.9	1.7	5.1	0.25	1.4E-04	0.03	0.17
Miranda	6.5	42.8	14.6	3.8	5.5	0.13	2.5E-19	-0.52	-0.80
Monagas	3.3	10.9	7.0	2.6	5.3	0.23	1.9E-05	-0.61	-0.26
Nueva Esparta	5.5	30.2	13.4	3.2	6.3	0.15	7.7E-14	-0.79	-0.79
Portuguesa	1.7	3.0	3.9	1.1	2.8	0.36	4.8E-02	0.79	0.74
Sucre	2.1	4.3	4.6	1.4	3.7	0.32	1.3E-02	0.38	0.60
Táchira	3.4	11.5	7.6	2.2	5.0	0.23	9.8E-06	0.09	0.05
Trujillo	1.9	3.7	3.2	1.7	2.0	0.34	2.5E-02	0.20	0.67
Yaracuy	3.2	10.3	7.0	2.3	4.9	0.24	3.2E-05	0.06	0.12
Zulia	2.6	6.7	5.2	1.7	3.8	0.28	1.3E-03	0.36	0.29
Vargas	3.6	12.7	8.3	2.1	5.6	0.22	3.1E-06	-0.63	-0.42

Source: Own calculations based on data in Table A8

Table A10: Linear probability estimates

	1	2	3	4	5	6
Dependent variable	Number of food packages received					
Treatment effect	.2044**	.2043**	.2043**	.2227**	.2227**	.2227**
	(0.07205)	(0.07201)	(0.07202)	(0.07812)	(0.07813)	(0.07816)
N	11,850	11,850	11,850	10,864	10,864	10,864
Pseudo R2	0.08	0.10	0.11	0.09	0.11	0.12
Dependent variable	Receipt of at least one food package					
Treatment effect	0.08216	0.08208	0.08206	0.09332	0.09332	0.09332
	(0.06848)	(0.06844)	(0.06846)	(0.07342)	(0.07342)	(0.07345)
N	11,850	11,850	11,850	10,864	10,864	10,864
Pseudo R2	0.19	0.21	0.22	0.20	0.23	0.24
Sample	All observations	All observations	All observations	High Quality	High Quality	High Quality
Treatment group trend	No	Yes	No	No	Yes	No
Municipality trends	No	No	Yes	No	No	Yes

This table is identical to Table 3 in text but estimated through linear two-way fixed effects.

Table A11: Treatment effects, entropy balancing estimates

	1	2	3	4	5	6
Dependent variable	Number of food packages received					
Treatment effect	.276*	.311**	.302**	.278*	.312**	.304**
	(0.28)	(0.31)	(0.30)	(0.28)	(0.31)	(0.30)
N	10,941	10,941	10,941	10,662	10,662	10,662
Pseudo R2	0.15	0.16	0.16	0.15	0.16	0.16
Dependent variable	Receipt of at least one food package					
Treatment effect	1.42***	1.54***	1.55***	1.46***	1.57***	1.59***
	(1.42)	(1.54)	(1.55)	(1.46)	(1.57)	(1.59)
N	10,212	10,212	10,212	9,962	9,962	9,962
Pseudo R2	0.25	0.28	0.29	0.25	0.29	0.30
Sample	All observations	All observations	All observations	High Quality	High Quality	High Quality
Treatment group trend	No	Yes	No	No	Yes	No
Municipality trends	No	No	Yes	No	No	Yes

This table is identical to Table 3 in text but weighted using entropy balancing weights.

Table A12: Treatment effects by political group, entropy balancing estimates

	1	2	3	4	5	6
Dependent Variable	Number of food packages received					
Treatment Effect	.28**	.308**	0.219	.284**	.312**	0.222
	(0.14)	(0.14)	(0.22)	(0.14)	(0.14)	(0.23)
Effect by political self-identification (base category: pro-Maduro chavistas)						
Anti-Maduro chavistas	0.054	0.061	0.045			
	(0.06)	(0.07)	(0.07)			
Pro-MUD opposition	0.032	0.025	0.014			
	(0.06)	(0.05)	(0.04)			
Anti-MUD opposition	0.060	0.057	.0567*			
	(0.04)	(0.04)	(0.03)			
Non-aligned	-0.066	-.0697*	-.0697**			
	(0.04)	(0.04)	(0.03)			
Did not answer	0.122	0.116	0.138			
	(0.21)	(0.21)	(0.22)			
Effect by November election vote (base category: voted for PSUV candidate)						
Voted for MUD candidate				-0.0066	-0.0087	0.0047
				(0.02)	(0.02)	(0.02)
Voted for third-party candidate				.209***	.204***	.234***
				(0.08)	(0.08)	(0.09)
Did not vote				0.023	0.024	0.030
				(0.07)	(0.07)	(0.08)
Did not answer				-.0709**	-.076**	-.0815**
				(0.04)	(0.04)	(0.04)
N	10941	10941	10941	10941	10941	10941
Adjusted R2	0.149	0.159	0.159	0.149	0.159	0.159
Sample	All observations	All observations	All observations	All observations	All observations	All observations
Municipality trends	No	Yes	Yes	No	Yes	Yes
Treatment * Socio-economic interactions	No	No	Yes	No	No	Yes

This table is identical to Table 4 in text but weighted using entropy balancing weights.

Table A13: Treatment effects by political groups, high-quality sample

	1	2	3	4	5	6
Dependent Variable	Number of food packages received					
Treatment Effect	.254*	.28*	0.176	.26*	.286*	0.185
	(0.14)	(0.14)	(0.22)	(0.15)	(0.15)	(0.23)
Effect by political self-identification (base category: pro-Maduro chavistas)						
Anti-Maduro chavistas	0.061	0.071	0.054			
	(0.05)	(0.07)	(0.06)			
Pro-MUD opposition	0.037	0.031	0.022			
	(0.05)	(0.05)	(0.03)			
Anti-MUD opposition	0.037	0.035	0.036			
	(0.03)	(0.03)	(0.03)			
Non-aligned	-.0657*	-.0674*	-.0643*			
	(0.04)	(0.04)	(0.03)			
Did not answer	0.116	0.111	0.136			
	(0.21)	(0.21)	(0.22)			
Effect by November election vote (base category: voted for PSUV candidate)						
Voted for MUD candidate				-0.0060	-0.0072	0.0059
				(0.02)	(0.02)	(0.02)
Voted for third-party candidate				.2**	.196**	.223***
				(0.08)	(0.09)	(0.09)
Did not vote				0.023	0.025	0.032
				(0.08)	(0.08)	(0.09)
Did not answer				-.0867***	-.0906***	-.0931**
				(0.03)	(0.03)	(0.04)
N	10662	10662	10662	10662	10662	10662
Adjusted R2	0.15	0.16	0.16	0.15	0.16	0.16
Sample	High-quality	High-quality	High-quality	High-quality	High-quality	High-quality
Municipality trends	No	Yes	Yes	No	Yes	Yes
Treatment * Socio-economic interactions	No	No	Yes	No	No	Yes

This table is identical to Table 4 in text but estimated only on the high-quality sample.

Table A14: Timing of December boxes and political conditioning, high-quality sample

	1	2	3	4	5	6
Dependent variable	December boxes arrived late		December boxes arrived early		Access to boxes politically conditioned	
Treatment effect	-0.388	-0.397	1.461***	1.437***	-0.517	-0.456
	(0.31)	(0.29)	(0.32)	(0.30)	(0.51)	(0.50)
N	1,788	1,788	1,699	1,699	1,951	1,951
Pseudo R2	0.02	0.03	0.09	0.10	0.03	0.04
Sample	High Quality	High Quality	High Quality	High Quality	High Quality	High Quality
Social and demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Political identification controls	No	Yes	No	Yes	No	Yes

This table is identical to Table 5 in text but estimated using only the high-quality sample.

Table A15: Timing of December boxes and political conditioning, entropy balancing estimates

	1	2	3	4	5	6
Dependent variable	December boxes arrived late		December boxes arrived early		Access to boxes politically conditioned	
Treatment effect	-0.313	-0.332	1.41***	1.39***	-0.535	-0.495
	(0.30)	(0.28)	(0.37)	(0.35)	(0.50)	(0.49)
N	1,879	1,879	1,786	1,786	2,065	2,065
Pseudo R2	0.02	0.03	0.08	0.09	0.03	0.04
Sample	All observations	All observations	All observations	All observations	All observations	All observations
Social and demographic controls	Yes	Yes	Yes	Yes	Yes	Yes
Political identification controls	No	Yes	No	Yes	No	Yes

This table is identical to Table 5 in text but weighted using entropy balancing weights.

Table A16: Vote-switching behavior and packages received, high-quality sample

	1	2	3
Dependent variable	Switched vote in favor of government		
Change in food packages received	0.203	.403***	.402**
	(0.14)	(0.14)	(0.19)
N	715	561	561
Pseudo R2	0.07	0.21	0.23
Sample	High quality	High quality	High quality
Social and demographic controls	Yes	Yes	Yes
Political identification controls	No	Yes	Yes
Municipality fixed effects	No	No	Yes

This table is identical to Table 6 in text but estimated only on the high-quality sample.

Appendix E: 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.

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

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).
6. Be in favor of the government.
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, $t = 3$ to the period from November 21 to January 9, $t = 4$ to the period from January 10 to January 31, and $t = 5$

to the month of February. I define the panel variable ***panelclap*** as equal to the numerical responses to ***clap5oct*** for $t = 1$. For $t = 2$, I use ***clap5nov*** only if respondent indicates in ***clap6*** having received it before the election. For $t = 3$ I use the sum of ***clap5dec***, ***clap5nov*** (only if respondent indicates in ***clap6*** having received after the elections) and ***clap5jan*** (only if respondent indicates in ***clap7*** having received it before the election). ***panelclap*** is the variable used in the specifications reported in Tables 3 and 4. I label as high-quality those observations for which the sum of the numerical answers to ***clap5oct-clap5feb*** is consistent with the response to ***clap3***.

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

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

I use the responses to ***elecnov*** and ***elecjan*** to create the binary indicator ***winvote***. I code ***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. This variables is only defined for Barinas residents.