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The Effect of Social Programs on Voting Decisions: Evidence from a List Experiment in Mexico*

El Efecto de los Programas Sociales Sobre la Decisión del Voto: Evidencia de un Experimento de Lista en México

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ABSTRACT

This paper analyses the effect of an unconditional cash transfer program, provided by a local government in Mexico, on the voting behavior of its direct beneficiaries. Using a list experiment that employs data from an original survey conducted in Guadalajara, Mexico, we explore if being a beneficiary of the program is a significant issue to cast a vote. The analysis shows that voters are indeed influenced in their voting decisions by being beneficiaries of a social program. The main result of this study indicates that 16 percent of the beneficiaries considered the receipt of the social program in their voting decision.

Keywords: Social program, voting behavior, list experiments.

RESUMEN

Este artículo analiza el efecto de un programa de transferencias en efectivo no condicionadas implementado por un gobierno local en México, sobre la decisión del voto de sus beneficiarios directos. Mediante un experimento de lista que emplea datos de una encuesta original levantada en Guadalajara, México, se esplora esta relción. El principal resultado indica que el 16 por ciento de los beneficiarios consideraron los beneficios del programa como una razón en la decisión de su voto.

Palabras clave: *Programa social, comportamiento del voto, experimento de lista.*

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I. INTRTODUCTION

Due to the expansion of social programs and policies that have focused directly on poverty level reduction, which commenced in the 1990s, an important literature has developed regarding the effects that the transfers that comprise these policies can have on the electoral behavior of their beneficiaries.[1] These effects can be responses to patronage strategies or incentivized by programmatic policies that do not seek, at least explicitly, to attract the votes of those who are benefited. While most of the existing analyses report positive effects on voters, that is, that the receipt of transfers provided by these programs affirms the votes of their beneficiaries for the government party, some studies conclude that there is not enough evidence to report such an effect (Anderson, 2007; Pop-Eleches and Pop-Eleches, 2012, Imai, King, and Velasco Rivera, 2017).

By implementing a list experiment for the case of an unconditional transfer program at the municipal level in the state of Jalisco, Mexico, this study found that voters are indeed influenced in their voting decisions by being beneficiaries of a social program. The main result of this study indicates that 16% of the beneficiaries considered the receipt of the social program in their voting decision. Notably, the effects we have found are independent of the level of marginalization where people live. We also observed no differences in responses based on the level of education, age or sex of the beneficiaries.

A list experiment was applied to reduce the selection biases that may exist in the case programs and in the responses of the people we surveyed. In order to carry out the experiment an original survey was conducted among the beneficiaries of the social program.

The analysis focuses on a local social program offered by a specific municipal government. Therefore, although it has the disadvantage of lacking a national vision, this approach reduces the problem of identification that the voters have about the level of government that provides their benefits and reduces biases due to some omitted variables that arise from the heterogeneity of national electoral processes and the operation and provision of social programs.

II. THE ELECTORAL RESPONSE TO SOCIAL PROGRAMS

The idea of a possible positive response by voters to the provision of goods and services, whether public or private, by governments is one of the main starting points for analyses of the political economy of public spending. There is a significant amount of research that has analyzed how governments strategically distribute and use public resources, whether in a patronage or programmatic way, via policies and programs that offer private

[1] In this article, we refer to the "beneficiaries" of a social program, those individuals who are registered in an institutional social program and are the direct recipients of the services or goods provided by the program.

transfers to voters to obtain political–electoral advantages over their competitors (Schady, 2000; Calvo & Murillo, 2004; Brusco, Nazareno, & Stokes, 2004; Stokes, 2005; Magaloni, Díaz-Cayeros, & Estévez, 2007; Nupia, 2011; Brun & Diamond, 2014, are some examples). These studies corroborate the strategic distribution of public spending, and social programs in particular, for political–electoral purposes. This strategic behavior of governments is based on the notion that voters respond positively to the private benefits provided by this type of program.

The theoretical works that seek to explain and analyze this political use of public programs mainly explain the positive response of voters with two factors. The first refers to the political–ideological preferences of people and their proximity to the positions of political competitors within an ideological spectrum. The theoretical developments that follow Downs (1957) and the classic model of probabilistic voting (Lindbek & Weibull, 1987; Hinich & Munger, 1997; Dixit & Londregan, 1996, 1998; Persson & Tabellini, 2002) argue that political competitors seek to capture marginal voters, that is, those who do not have a strong ideological link with any of the competitors and are therefore less costly to coopt or attract than those who have a strong ideological preference for one of the competing parties. In this way, the strategic distribution of public resources favors this group of voters.

With regard to the ideological factor of social programs and the distribution of public spending, it can provide voters with information about the performance of the ruling party and their ideological positions and preferences. This information can be used by voters when deciding the direction of their votes according to their own preferences and interests (Rogoff 1990; Drazen & Eslava 2006, 2010; Manacorda, Miguel, & Vigorito 2011; Healy & Malhotra 2013).

The second factor is the level of income or wealth of people. The idea of a diminishing marginal utility of private resources provides the intuition that those who have fewer resources will value a transfer to their income more than people with a greater amount of resources. People with lower levels of income and wealth therefore respond more easily to a government transfer or even have a greater willingness to exchange their vote or political support for some type of transfer or action that favors them (Scott, 1972; Kitschelt, 2000; Busco, Nazareno & Stokes, 2004). Thus, given the greater political profitability of the provision of benefits and private transfers to lower-income voters, political competitors and ruling parties find attractive mechanisms for conducting political–electoral strategies in social and antipoverty programs. (Dixit & Londregan, 1996; Calvo & Murillo, 2004; Robinson & Verdier, 2013; Díaz-Cayeros, Estévez, & Magaloni, 2016). Different empirical studies have demonstrated how in effect, programs that offer private transfers as well as clientelist strategies tend to be used more frequently in localities with lower incomes (Wantchekon, 2003; Keefer, 2007; Kitschelt & Wilkinson, 2007; Remer, 2007; Busticova & Corduneanu-Huci, 2017).

These two factors are essential, but the degree of voter response is also determined by the development and strengthening of a democratic culture and the construction of democratic institutions that inhibit clientelistic relations and vote buying. The process of social development not only leads to an increase in the income and wealth of people that weakens the effectiveness of clientelist strategies (Magaloni, Díaz-Cayeros, & Estévez, 2007). It also drives the expansion of a middle class with democratic values that can strengthen positive responses to programmatic policies and inhibit the willingness to exchange or sell political–electoral support for transfers that provide private benefits to voters (Kitschelt & Wilkinson, 2007; Weitz-Shapiro, 2014). One result of the development and strengthening of democratic institutions is the institutionalization and shielding of social programs to minimize their discretionary operation and prevent their political–electoral use. However, a decrease in the positive electoral response of beneficiaries is not necessarily expected, even when the institutionalization of the programs manages to prevent the development of patronage and patronage relationships. In that case, voters use their votes to punish or reward the government in power, regardless of whether they receive transfers from social programs (Imai et al., 2017).

Accordingly, there can also be gratitude and reciprocity among those who benefit from social programs toward those who implement them (Sobel, 2005; Finan and Schechter 2012; Lawson and Greene, 2014), reinforcing the response of voters even when they have not been conditioned to provide it. However, another mechanism that explains positive voter response is not necessarily related to rewarding the government that implements the program but to search for the permanence of the program and avoid the risk of its cancellation if an opposition party takes control of the government (Shedler, 2000; Cornelius, 2004).

The empirical evidence for these voter responses, however, is not entirely conclusive. Several studies have found positive responses to the benefits of institutionalized social programs and programmatic policies. In turn, other studies conclude that some programs have had no effect on electoral behavior and some cases even provide counterintuitive results, where beneficiaries actually increased their support for the opposition.

In Colombia, the provision of conditional transfers has been shown to have fostered votes for the ruling party and increased electoral participation in the 2010 presidential elections (Nupia, 2011; Conover et al., 2020). Similar positive effects have been observed in the percentage of votes for ruling parties in Indonesia, the Philippines and Brazil as a result of conditional transfer programs (Labonne, 2013; Zucco, 2013; Tobias, Sumarto, and Moody 2014). Based on information from individual self-report surveys in Uruguay, Manacorda et al. (2011) found that the benefits of a temporary unconditional transfer program had an important impact on the support of its beneficiaries for the party that implemented the program. Galiani et al. (2019) have identified an analogous positive effect on votes for the ruling party during the 2013 presidential elec-

tions in Honduras whose magnitude differed according to the temporal distance between the provision of transfers and the moment of the election.

In Romania, the beneficiaries of a program that offered vouchers for the purchase of computers responded with greater support for the coalition of ruling parties (Pop-Eleches and Pop-Eleches, 2012). As this effect resulted from a unique transfer to each beneficiary, the effect was likely motivated by a reaction of gratitude or reciprocity, as proposed by Green and Lawson (2014).

In contrast, regarding a program to support the development of productive projects in Uganda, the probability of supporting the opposition increased among its beneficiaries. This reaction could be because the development of productive capacities and the economic improvement of the people in the program provided them with greater economic and financial independence, inhibiting their dependence and patronage ties with the ruling party (Blattman, et al. 2018).

Regarding Mexico, the analyses that have been carried out thus far also show mixed results.

Various studies have focused on the impact of the national conditional cash transfer program, PROGRESA, reporting positive effects on support for the ruling party. Using the Mexico 2000 Panel Study, Cornelius (2004), for example, find that for the federal election of 2000, those who were beneficiaries of PROGRESA were 12% more likely to vote for the ruling party (PRI) than for the right-wing party (PAN) and 26% more likely to do so than for the leftist opposition party (PRD). Based on exit polls during the presidential elections of 2000 and 2006, Díaz-Cayeros et al. (2016) observe that the probability of PROGRESA beneficiaries voting for the party in the government was higher than that of the rest of the voters: 17% greater in 2000 and 11% in 2006.

Taking advantage of the fact that the selection process at the beginning of the PROGRESA program was carried out randomly, De la O (2013) compares the information from communities incorporated into PROGRESA at different times and reports an increase of 9% in the vote for the ruling party, attributable to the social program. The study concludes that this increase in votes for the ruling party was mainly due to a mobilization effect. However, based on the same data and by correcting matching inconsistencies, Imai, King, and Velasco Rivera (2017) find that De la O's observed effects do not exist and argue that the program has no effects on the decisions of its participant voters to either reward or punish the responsible party. On the other hand, Rodríguez-Chamussy (2015) determine that increases in the coverage of the same program prior to the electoral moment have increased the percentage of votes in favor of the governing party at the municipal level, even when this party does not coincide with the governing party at the federal level. These contradictory results may stem from the fact that beneficiaries may not correctly distinguish what level of government actually provides the program. Although the Progresa–Oportuni-

dades–Prospera program was dependent on the federal government, its administration fell largely to the staff of local administrations; thus, people could erroneously identify municipal governments as the providers of these benefits, not necessarily the federal government. This confusion was strengthened through municipal governments' strategies to signal themselves as the provider of the program and its benefits, as argued by Rodríguez-Chamussy (2015).

All the studies discussed above compare how people who benefited from the social program voted with how voters outside the program voted. Their datasets are, in some cases, aggregate data regarding the number of affiliates of the program and electoral results at the local or district level; in other cases, they comprise data obtained from surveys, conducted either at poll exit or on the day after an election, which ask voters directly if they are affiliated with a program and how they voted. In contrast, for this study, we implemented a list experiment to investigate the beneficiaries of an unconditional social program at a municipal level to directly analyze whether being participants in this program influenced how the beneficiaries voted in an electoral process involving the government that was responsible for providing and administering the program.

A bias problem than analyses and estimations with aggregated data can occur when a program is used and distributed strategically by a government such results (Levitt and Snyder, 1997; Conover et al. 2020). Analyses using this type of data introduce additional variables to control for these biases. In our case, obtaining the counterfactual within the same group of beneficiaries through the experiment also helps mitigate this type of bias. In the experiment, responses from groups of beneficiaries belonging to the same program are compared, reducing the problem of selection bias that arises from the potential strategic use of the program when comparing individuals inside and outside of it. However, although in this study we considered a local program that is restricted to one municipality, it could have been selectively used according to electoral expectations based on different criteria. For example, on one hand, to meet the expectations of party supporters, and on the other hand, to try to attract indifferent voters or even those affiliated with the opposition party. Nevertheless, the experiment focuses on individual responses based on random sampling, which helps reduce selection biases.

We also know that, when faced with a direct question, people tend to deny having participated in a political–patronage relationship that involves exchanging their vote for some type of perk. Although in this case the objective of the research was not to detect the effects derived from patronage relationships or vote buying, it is very likely that by asking each voter directly if his or her voting decision depended on receiving the benefits of the social program, the voter related the question to a form of exchange of his or her vote for his or her permanence or integration into the social program, which not only civically incorrect but also classified as an electoral crime in Mexico. This interpretation is further reinforced in a context where patronage policy has had a strong

presence for many years (Magaloni, 2006; Greene, 2007) and at a time when there has been a strong dissemination of policies against the exchange and sale of a vote. In this context, the probability of biased responses to a direct question about whether the benefit that the social program offers to a person is a determinant in his or her voting decision is high. This is the first reason why the people surveyed might not want to publicly express their true opinion if receiving the benefits of the program actually induced them to direct their vote in favor of the ruling party. On the other hand, risk can also bias their answers, since people may tend to answer affirmatively for fear that their benefit from the program could be withdrawn if they do not report having voted for the party in government.

These biases have been recorded in several analyses focused on the clientelist exchange of votes and have been identified through list experiments. These studies show that when voters are asked directly if they exchange their vote for a favor or perk, they report a lower degree of involvement in clientelist exchanges than the experiments reveal (Gonzalez-Ocantos et al., 2012; Imai, Park and Greene, 2015; Corstange, 2018). In Mexico, using data from the Mexico 2012 Panel Study, Imai et al. (2015) show that when directly questioned about having exchanged their vote for a favor or perk, only 5.5% of the people surveyed answered affirmatively, while the authors' list experiment revealed that approximately 19.4% of people exchanged their vote for a gift or favor.

Accordingly, to carry out the list experiment in this research, we conducted an original survey among the participants of a social program administered by the municipal government of Zapopan in the state of Jalisco in Mexico. The survey was carried out during the weeks following the election day of 2015, when the municipal government was elected for the 2015–2018 period. The social program that we considered in this analysis is provided by a municipal government, and the beneficiaries have full knowledge of it due to how it is operated and an evident strategy of the municipal government to signal itself as the provider of the program. This offers the advantage of ensuring that the beneficiaries clearly identify the provider of the benefits, which does not necessarily occur with programs provided by federal governments, e.g., the Oportunidades program (UNDP, 2007). In addition, in 2015, the elections were intermediate for the federal and state governments and coincided with those of the municipal governments in Jalisco. Therefore, although state and federal congressmen were elected, there was no influence of a presidential campaign, which tends to attract much of the attention of voters. Moreover, the campaigns of the congressmen are operated and promoted mainly through local party organizations. This focuses the attention of voters on the local electoral process, mainly on the election of the municipal president. Hence, the people surveyed for the experiment could clearly relate the question concerning the social program with the vote they specifically cast for the municipal government election.

Although the experiment does not have national coverage, in addition to eliminating biases via the identification of the benefit provider, it offers the advan-

tage of controlling for heterogeneity in the conditions and local dynamics that impact the same electoral process at the national level and for other variables that can impact behavior at the regional, state or local level.

The results of this study show that indeed the beneficiaries, 16% of them, took into account the reception of the social program when deciding how to vote. These results do not necessarily contradict the findings of Imai, King, and Velasco Rivera (2017), as the fact of benefiting from a social program does not necessarily determine the direction of the vote in favor of the incumbent. The findings of this study only indicate that the benefit of a social program is taken into account when deciding the vote, but we do not have information on whether its effect is strong enough to change the vote's direction. However, these findings also do not oppose the hypothesis of a positive response in voting to the benefits of social programs, particularly considering the evidence from Rodríguez-Chamussy (2015) focusing on the dynamics at the municipal level.

We also found no differences between the responses of the beneficiaries who live in areas with very high marginalization and those who live in areas with high marginalization. This result could contradict the hypothesis that people with a lower income value the benefits of a program more strongly. Moreover, neither the level of education, sex, or age of the recipients impacted the probability that the benefits of the program affected the direction of their vote.

This analysis on the behavior of beneficiaries of social programs contribute to the literature focused on voter behavior and empirically strengthen the hypothesis of a positive voting response to the benefits of social programs, particularly in the case of Mexico. Furthermore, our results contribute to studies focused on the analysis and design of social policies that point to the problems derived from existing political–electoral incentives among both those who administer them and those who are directed by them (Kitchelt, 2000; BID, 2006; Mares and Carnes, 2009, are some examples), as well as to the literature focused on the political–patronage use of public resources.

III. EXPERIMENTAL DESIGN

The experiment was designed to analyze the effect of a person being enrolled in a social program and receiving a direct transfer on their voting decision in an electoral process. To this end, we conducted a survey among the beneficiaries of a social program implemented by the municipal government of Zapopan, one of the local governments in the Guadalajara metropolitan area, in the state of Jalisco, Mexico.[2]

[2] The Guadalajara metropolitan area contains what is known as the city of Guadalajara, which has just over 5 million inhabitants and is composed of 8 municipalities. Zapopan is the second most populated municipality in the area, with just over 1.5 million inhabitants.

The experiment involved administering a survey that included a list experiment question to a random sample of beneficiaries of the social program, resulting in a control group and a treatment group. All the people surveyed were beneficiaries of the "Support for female-headed households in vulnerable conditions"[3] program at the time the experiment was conducted. In August 2015, this program had just over 14,300 people enrolled, each of whom was a head of household (both male and female).

The program was created in 2011 to provide support to single-parent families with a female head of household and dependent children or grandchildren who live in the same household and attend school. The program's coverage was restricted to families living in one of the 74 most marginalized neighborhoods in the municipality who were not enrolled in any other social program, in particular Oportunidades-Prospera.[4] According to the marginalization index defined by the Institute of Statistical and Geographic Information of Jalisco (*Instituto de Información Estadística y Geográfica de Jalisco - IIEGJ*), the 74 neighborhoods where the program was offered were classified as having a medium to high level of marginalization.

Although the program's name did not change, as time went by, male-headed and not necessarily single-parent households also began to be included, but the restriction of living in one of the 74 most marginalized neighborhoods was maintained, as well as the condition that the dependent children or grandchildren must attend school.

The benefits offered by this program at the time of this study, and until 2016, was a cash transfer of 500 Mexican pesos and a food bundle, which was delivered to families every two months. In July 2015, the total value of the transfer, considering both the cash and the value of the food items, was approximately 45 US dollars (720 Mexican pesos) at the current exchange rate. These transfers were delivered directly to the beneficiaries by municipal government staff, either at government facilities or directly to the neighborhoods. This is important because the way in which people enrolled in the program and the delivery of the transfers enabled the beneficiaries to clearly identify that the municipal government was the entity that was providing them with this aid; this was constantly reinforced, as on many occasions, the municipal president, or a high-level official, would be present at these deliveries. The delivery of the food items and cash was carried out by grouping the beneficiaries based on the zone or neighborhood in which they lived.

[3] In Spanish, "Apoyo a mujeres jefas de familia en condición vulnerable." For more details on this program, please see Appendix 1 and Gobierno Municipal de Zapopan (2011).

[4] Progresa-Oportunidades-Prospera was Mexico's main social program. It was a conditional transfer program by the federal government to combat poverty. It began in 1997 with the name Progresa. In 2000, its name was changed to Oportunidades, and in 2012, its name was changed again to Prospera. In 2019, it dissolved under the new federal government.

To measure the program's effect on the voting direction of the beneficiaries, the survey was conducted in July and August 2015, during the first delivery of aid following the electoral process that year. On June 7, 2015, elections were held for federal deputies and, in the state of Jalisco, local deputies and municipal government officials were also elected. In particular, during those elections, the municipal government for the 2015-2018 term was selected, beginning its duties in October of that same year.[5]

Accordingly, the people surveyed were beneficiaries who had voted several weeks earlier in the elections for the next municipal government, and thus, the voting decision had already been made. At that point, the election results had already been announced, and the respondents therefore knew that their response to the survey could not affect or influence their continued enrollment in the program.

To calculate the sample size, a statistical power of 80% and a significance of 5% were considered. Based on what was reported by Gonzalez-Ocantos et al. (2012), an R^2 of 0.38 was also considered. Unlike surveys that ask questions directly, list experiments produce larger standard errors, and it is therefore necessary to have larger samples; samples of at least 1000 observations are recommended (Corstange, 2009).

To design the sample, an attempt was made to balance the levels of marginalization where the beneficiaries lived. The randomization was carried out by balancing the sample between zones with a very high and high level of marginalization, and the surveys were administered during the delivery of the aid. A total of 1,000 beneficiaries of the program, of legal age, who had voted in the previous municipal election, were randomly selected, and the surveys were administered to them. The sample was also randomly divided to construct a control group and a treatment group.

After the observations with incomplete responses and inconsistencies were eliminated, the final sample consisted of 958 observations: 481 in the control group and 477 in the treatment group.

When the survey was conducted, the pollsters specified that the questions referred to the election for municipal president.

[5] In the 2015 elections, in the municipality of Zapopan, the incumbent party during the previous two terms (2010-2012 and 2013-2015), the PRI (Institutional Revolutionary Party), lost the election to MC (Citizens' Movement). This also occurred in two other major municipalities in the Guadalajara metropolitan area, including the municipality of Guadalajara, where the state capital is located. The other municipality where MC defeated the PRI was San Pedro Tlaquepaque. Additionally, MC remained the incumbent party in a fourth municipality in the metropolitan area: Tlajomulco. Thus, MC won the elections in 4 out of the 6 municipalities in the metropolitan area, which are the 4 most important municipalities in terms of economy and population. Similarly, in the same electoral process, MC won the state governorship, which was previously held by the PRI.
In the case of Zapopan, in that 2015 mayoral election, the MC party obtained 42.21% of the votes. The party with the closest number of votes was the PRI with 27.58%. The third party was the PAN (National Action Party) with 14.87%.

The survey that was administered to people in both groups differed only in terms of the list question. In this question, voters were presented with a list of possible reasons or responses that might have determined their vote in that year's election.

The list for the control group included four items: control items.

For the treatment group, in addition to those same four items, the list included a fifth item that constituted the sensitive item.

The complete text corresponding to the list question is presented below.

> "I am going to show you a card with a list of reasons that you may or may not have considered when deciding who to vote for in the last election. I would like you to read them and tell me how many of these reasons were important for you when deciding to vote for the person you ultimately selected.

PLEASE DO NOT TELL ME WHICH REASONS—ONLY HOW MANY."

- Because of campaign posters/banners posted in your locality.
- Because of the proposals presented by the different candidates.
- Because they visited your home to ask for your vote.
- Because someone suggested you should vote for a certain candidate at the religious activities you attend.
- The sensitive item that was added to the list for the treatment group was:
- Because I am a beneficiary of the "Support for Heads of Households" program.

When designing the list, an attempt was made to ensure that the sensitive item would not stand out from the others, which might bias the responses of the people surveyed (Glynn, 2013; Aronow, Coppock, Crawford and Green, 2015). The selection of the items on the list was also made in an attempt to minimize the probability of obtaining responses that considered all or none of the reasons as determinants of the voting decision, thereby minimizing the bias and variance in the estimators (Tsuchiya, Hirari and Ono, 2007; Glynn, 2013). With this same objective and following Glynn (2013), two reasons that had a high probability of impacting the voters' decision were included on the list, such that the mode of the response to the list question was equal to 2. These reasons were i) the publicity campaigns of the parties and their candidates in the neighborhood in which the voter lives; and ii) the proposals put forward by the candidates. However, even when an attempt was made to maintain a distribution with a more centered mode, the resulting mode was still 1 (see Table 2).

The survey considered four control variables that might influence people's responses to the list: gender, age, education level and level of marginalization of

the zone in which the respondents lived. Table 1 presents the variables used and their characteristics.

Table 1. Description of Variables.

Variable	Description
list	Number of items chosen from the list: 0 to 4 for the control group and 0 to 5 for the treatment group.
treatment	Dummy variable; 0 for control group, 1 for the treatment group.
sex	Dummy variable; 0 if respondent is female, 1 if male.
margin	Dummy variable; 0 for medium level of marginalization y 1 for high level of marginalization.
age	Age of respondent.
primary	Dummy variable: 1 if the respondents' highest level of education is primary school, 0 in other case.
secondary	Dummy variable: 1 if the respondents' highest level of education is secondary school, 0 in other case.
high_school	Dummy variable: 1 if the respondents' highest level of education is high school or equivalent, 0 in other case.
college	Dummy variable: 1 if the respondents' highest level of education is college, equivalent or grater, 0 in other case.

Considering that the survey was conducted among the beneficiaries of the program and to avoid obtaining responses biased toward the ruling party, the questionnaire did not ask which party the person had voted for in the most recent election.

IV. EVALUATION OF THE DESIGN

Although an effort was made to obtain the most centered distributions possible for the response to the list question and although the distributions for the two groups were unimodal, the mode is 1 for both cases. Similarly, as seen in Table 2, for both the control and treatment groups, the frequency for a response of 2 on the list question is higher than that for the response of 0.

Table 2. List Question Answers' Distribution.

Response Value	Control Group		Treatment Group		Total	
	Frequency	Percentage	Frequency	Percentage	Frequency	Percentage
0	120	24.95%	110	23.06%	230	23.91%
1	149	30.98%	138	28.93%	287	29.83%
2	125	25.99%	124	26.00%	249	25.88%
3	68	14.14%	67	14.05%	135	14.03%
4	18	3.74%	24	5.03%	42	4.37%
5			14	3.13%	14	1.46%
Total	481	100%	477	100%	958	100%

By testing for differences in means between the two groups and excluding from the treatment group the cases that we knew were sensitive to the treatment question (those with a response equal to 5 for the list question), a p-value of 0.035 is obtained, and we can thus reject the hypothesis of different means at 95% and argue that there is no design effect and that the estimators for the differences between the groups are valid (Imai, 2011).

Table 3 shows the balance between the control group and the treatment group, considering the control variables. In general, none of the covariates are significantly different between the control group and the treatment group. For education level, only in the set with a high school education is there a difference in the size of the control and treatment groups that is significant at 95%; there were 71 people with a high school education in the control group and 93 in the treatment group. Similarly, for all the variables, there were no differences between the variances for the control and treatment groups, with a 99% probability.

Table 3. Balance Between Control and Treatment Groups

Variable	Control Group			Treatment Goroup			Differen-ce	T- test Ho: diff =0		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean Diff.	Ha: diff < 0	Ha: diff¹ 0	Ha: diff > 0
Age	481	42.5447	15.5983	477	41.6226	15.7276	0.9221	0.81875	0. 3625	0.1813
Sex	481	0.4469	0.4976	477	0.4297	0.4955	0.0128	0.7041	0.5918	0.2959
Margin	481	0.5031	0.5005	477	0.4968	0.5005	0.0062	0.5268	0.8465	0.4232
Primary	481	0.3700	0.4824	477	0.3668	0.4824	0.0032	0.5407	0.9187	0.4593
Secundary	481	0.3056	0.4611	477	0.2872	0.4529	0.0084	0.7333	0.5334	0.2667
High_school	481	0.1476	0.3550	477	0.1949	0.3965	-0.0073	0.0259	0.0518	0.9741
College	481	0.0686	0.2530	477	0.0712	0.2575	-0.0026	0.4357	0.8714	0.5643

Table 4 shows the results of the individual regressions for each covariate on the treatment variable. The *p*-values indicate that, in general, no covariate explains the treatment variable, with the exception of high school education, which cannot be rejected with a 90% probability, reflecting the slight imbalance in the sizes of the control and treatment subsets for those with a high school education.

Tabla 4. Results of logistic regressions on the treatment.

Variable	Coeficiente Logit	Desviación Estandar	Valor p
Age	-0.0037	0.0041	0.362
Sex	-0.06976	0.1300	0.592
Margin	-0.01659	0.1289	0.898
Primary	-0.01350	0.1334	0.919
Secundary	-0.08815	0.1414	0.533
High_school	0.33510	0.1727	0.052
College	0.04102	0.2533	0.871

Despite the small imbalance in the group with a high school education, for the other variables, the control and treatment groups are balanced, and none of these covariates affect the probability of belonging to either group.

V. RESULTS

Table 5 shows that the conditional mean of the experiment's dependent variable—the number of responses selected from the list—is significantly lower for the control group than for the treatment group, with a 95% probability. This indicates that, on average, 16% of the program's beneficiaries would have considered the benefits of the social program as a reason that would determine their vote.

Even though the control and treatment groups are fairly well balanced, we know that there is a significant difference for the subgroup with a high school education. Similarly, based on the results of the ordinary least squares regression of the list variable versus the treatment variable, which is presented in Table 6, the coefficient of determination is very low. Consequently, I chose to perform the analysis by introducing the covariates described in the previous section.

Tabla 5. Mean Differences.

Variable	Control Group			Treatment Group			Difference	T- test Ho: diff =0		
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Mean diff.	Ha: dif < 0	Ha: dif¹ 0	Ha: dif > 0
list	481	1.41372	1.12977	477	1.57861	1.28198	-0.16489	0.0174	0.0349	0.9826

The standard deviations presented in Table 5 correspond to variances of 1.27639 for the list response in the control group and 1.64349 in the treatment group. Based on conditional means, there does not seem to be any problem of overdispersion in the dependent variable. Considering the above, a Poisson model could be used for the regression analysis. However, the variance for the control group is slightly greater than its mean, and given its small range, I decided to use a negative binomial distribution model, which is a generalization of the Poisson model and would, in any case, control for any problem of overdispersion, if it existed. Robust methods were used in all the regressions.

Tabla 6. Models without interactions.

	OLS	Negative Binomial Regression					
	I	II		III		IV	
		Coefficient	IRR	Coefficient	IRR	Coefficient	IRR
Constant	1.41372***	0.34622***	1.41372**	1.11621***	3.05327***	1.011078***	3.036752***
	(0.051513)	(0.03641)	(0.05148)	(0.13014)	(0.39736)	(0.13581)	(0.41242)
Treatment	0.16489**	0.11032**	1.11663**	0.10878**	1.11492**	0.10827**	1.114356**
	(0.078096)	(0.05203)	(0.05810)	(0.05066)	(0.05649)	(0.05058)	(0.05637)
Age				-0.01059***	0.98945***	-0.01033***	0.989720***
				(0.00186)	(0.00184)	(0.00191)	(0.00189)
Primary				-0.43372***	0.64809***	-0.43000***	0.650505***
				(0.09522)	(0.06171)	(0.09539)	(0.06205)
Secunday				-0.32530***	0.72230***	-0.32153***	0.725036***
				(0.09782)	(0.07066)	(0.09811)	(0.07113)
High_ school				-0.33173***	0.72230***	-0.31701***	0.728320***
				(0.10852)	(0.07066)	(0.11075)	(0.08066)
College				-0.40082***	0.66977***	-0.38767***	0.678631***
				(0.13317)	(0.08919)	(0.13458)	(0.09133)
Sex						-0.03933	0.961425
						(0.05340)	(0.05134)
Margin						0.011253	1.011317
						(0.05192)	(0.05251)
Obs. 958	R^2 0.0046	Wald χ^2 4.50		Wald χ^2 55.62		Wald χ^2 55.71	
		ln-alpha -37.1045		ln-alpha -37.10459		ln-alpha -37.10459	

Robust standard errors in parentheses.
***p < 0.01, **p < 0.05, *p < 0.1.

In all the models analyzed, the treatment variable is significant at 95%. With regard to the covariates, age is also significant at 99% in all the models, while the respondents' gender and level of marginalization of the neighborhood in which they live do not have statistically significant effects on the number of responses they selected from the list. With regard to the education variables, they are also significant at 99%.

Table 6 presents the results for three binomial regression models with robust methods: Model II only includes the treatment variable, while Model III includes the covariates that are consistently significant. Model IV includes all the covariates, but sex and the level of marginalization are not significant.

The goodness-of-fit for the model that includes the covariates is quite high, while for Model II, it is lower, although, as mentioned above, significant at 95%.

For both models, the estimated coefficients of the negative binomial regression are presented, as well as the incidence rate ratios (IRRs). Based on those results, for one response chosen by a person in the control group, an average of 1.1149 responses would be chosen in the treatment group, controlling for the education level and age of the respondents.

This effect is consistent with the results of the difference in means test. Considering that the mean number of responses selected in the control group is 1.41372, the IRR estimated in Model III implies that the mean number of responses selected in the treatment group is 1.57618, which is very close to the unconditional mean in the treatment group. The difference in these two means is determined solely by the inclusion of the sensitive item on the list of the control group and does not depend on variables such as education level, age, gender or level of marginalization of the zone in which the voters live.

Accordingly, the results indicate that 16.246% of the people receiving benefits from the social program considered that being a recipient of those benefits was a reason for deciding who to vote for, with a 95% probability.

A relevant question in the analysis is whether the voters' income level as well as their education level are determinants of a type of behavior, in which their voting direction can be influenced by transfers such as those received as part of a social program as described herein. To test for these effects, I analyzed models that included interactions between the covariates and the treatment variable. For those analyses, negative binomial regression models are used, as well as the methodology proposed by Imai (2011), to obtain more efficient estimators by including interactions between the treatment variable and the covariates.[6]

As seen in Table 7, which shows some of the models we have analyzed, by including the interactions of the control variables and the treatment variable, the results of the models without interactions were maintained. The treatment variable continued to be significant at 95%, while age and education level were significant at 99%. Once again, the degree of marginalization and sex were discarded.

Regarding the interactions, in practically all cases, the control variables had no effect on the probability of considering the sensitive response of the list. This indicates that in this case, variables such as the age or sex of the beneficiaries do not increase or decrease the positive effect that a social program has on people to define the direction of their vote.

[6] Based on Blair and Imai (2012), the *List* package of R code was used, which estimates logistic regression models by maximum likelihood and nonlinear least squares in two stages, using the expectation-maximization algorithm.

Table 7. Models with Interactions.

	Negative Binomial Regression				NLLS (2 stages)		Maximum Likelihood	
	I	II	III	IV	I	II	I	II
Constant	0.29497***	1.22631***	1.37615***	1.33242***	0.52389***	0.58049***	0.28941	0.25744
	(0.05712)	(0.15050)	(0.17857)	(0.18660)	(0.29212)	(0.27961)	(0.18525)	(0.20815)
Treatment	0.17241	-0.08928	-0.39115	-0.32270	-6.54331**	-6.92198**	-2.26517**	-2.01482**
	(0.07793)	(0.14478)	(0.25909)	(0.26857)	(3.92796)	(4.1031)	(1.3299)	(1.62993)
Age		-0.01260***	-0.01439***	-0.01392***	-0.02001***	-0.02054***	-0.01713***	-0.01666***
		(0.00256)	(0.00277)	(0.00282)	(0.00403)	(0.00391)	(0.00274)	(0.00287)
Primary		-0.43856***	-0.46578***	-0.45947***	-0.67945***	-0.68738***	-0.61032***	-0.5973***
		(0.01546)	(0.12889)	(0.12352)	(0.19251)	(0.19162)	(0.13305)	(0.14586)
Secundary		-0.41217***	-0.49879***	-0.49021***	-0.74507***	-0.75644***	-0.56288***	-0.54933***
		(0.11085)	(0.12889)	(0.12982)	(0.20512)	(0.20389)	(0.14233)	(0.14586)
High_school		-0.33155***	-0.51518***	-0.49194***	-0.75229***	-0.78252***	-0.58672***	-0.55995***
		(0.10862)	(0.14923)	(0.15193)	(0.23451)	(0.22980)	(0.15989)	(0.16491)
College		-0.40103***	-0.59228***	-0.56734***	-0.81864***	-0.84710***	-0.71417***	-0.68912***
		(0.13268)	(0.16654)	(0.17113)	(0.25785)	(0.25006)	(0.19210)	(0.20092)
Sex				-0.02682	-0.02244			-0.03583
				(0.07426)	(0.10502)			(0.08100)
Margin	0.09940			0.05032	0.06242			0.02780
	(0.07344)			(0.07259)	(0.10301)			(0.08200)
Interactions								
Age_Treat		0.00372	0.00714*	0.00669	0.07864	0.07541	0.01757	0.01684
		(0.00333)	(0.00377)	(0.00385)	(0.04785)	(0.04645)	(0.01833)	(0.02022)
T_Primary			0.00600	0.05745	-1.35940	-0.97489	-0.84653	-0.95343
			(0.01109)	(0.19309)	(3.94407)	(3.46172)	(1.17683)	(1.24606)
T_Secundary		0.15775	0.33684*	0.32970*	3.39433	3.41202*	-0.68419	0.60751
		(0.10682)	(0.19798)	(0.19850)	(2.56801)	(2.75444)	(1.03888)	(1.09114)
T_High			0.34478	0.32817	3.81173	3.51571	-0.78619	0.73975
			(0.21934)	(0.22346)	(2.81382)	(2.92734)	(1.10412)	(1.15628)
T_College			0.37439	0.35075	3.96958	3.83600	1.02501	0.96209
			(0.26118)	(0.26439)	(2.90776)	(3.11199)	(1.21765)	(1.34199)
Sex_T				-0.00971	-0.68543			-0.20958
				(0.10620)	(1.01427)			(0.61809)
Margin_T	-0.12136			-0.07615	-0.57437			-0.15764
	(0.10446)			(0.10337)	(0.97871)			(0.64233)
Obs. 958	R^2 0.0643	Wald c^2 58.25	Wald c^2 62.37	Wald c^2 62.47	Err. St. Res. 1.26208	Err. St. Res. 1.25974	Log-Lh -1457.851	Log-Lh -1457.48

Robust standard errors in parentheses.
***p < 0.01, **p < 0.05, *p < 0.1.

VI. CONCLUSIONS

This article offers empirical evidence for the positive effect of a social program on its beneficiaries' voting behavior using a list experiment. The experiment was conducted among the beneficiaries of a social program implemented by a local government in the metropolitan area of Guadalajara, Mexico, which offers unconditional cash transfers for families living in poverty and marginalization. The program, at the time of the experiment, covered more than 14,000 families.

The results of the experiment show that being a beneficiary of the social program had a significant impact on the voting decisions of its participants. The evidence thus suggests that receiving the benefits of a social program is a factor that the beneficiaries consider when defining their votes.

In contrast, variables, such as the sex of the head of the household receiving the transfer or age, had no effect on voting decision.

The positive effect on the vote of the beneficiaries could come from three mechanisms: the aim of the beneficiaries to guarantee the continuity of the program by maintaining the ruling party in the next period; beneficiaries' gratitude or reciprocity; and voters rewarding a policy that they consider a good practice, regardless of whether they are its direct beneficiaries. These mechanisms are not mutually exclusive, and all three could have determined the behavior of the voters. Finally, the weight that each of these mechanisms can have in the influence of a social programs on voting decisions is a question that remains open for future analysis.

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APPENDIX

Appendix 1. General description of the "Support for female-headed households in vulnerable conditions" program

The "Support for female-headed households in vulnerable conditions" program is an unconditional cash and in-kind transfer program that aimed to improve the economic conditions of women in vulnerable situations, as well as their families. It focused on individuals who were not beneficiaries of any other social program. The program targeted families living in poverty who were unable to access Oportunidades. Although Oportunidades was a program with universal coverage that included households with the highest levels of poverty, especially those in extreme poverty, it left out many families with significant needs.

The program was created in 2011, and the initial program rules stated that its objective was to "support vulnerable women in Zapopan to develop and enhance their capacities, skills, and abilities by providing them with opportunities for training and protection through access to economic support, which can be in the form of cash or in-kind benefits." (Municipal Government of Zapopan, Jalisco, 2011).

The rules also stated that it aimed to support women in vulnerable situations, even if they were not heads of the family. Particularly during its first phase, from 2010 to 2013, the families who benefited from the program did not necessarily have female household heads.

The operational rules were modified over time due to difficulties in their implementation (Flores, 2017). An important problem with the original operational rules was that they did not consider the periodic review and evaluation of the beneficiaries' situation to determine whether they should continue in the program. There was also no maximum time period established for being a program beneficiary. Thus, the program grew while maintaining enrolled families from the program's inception, many of them with male household heads. This explains why the sample includes households with male household heads, even though the program is targeted at women, particularly female heads of households. Flores (2017) provides a more detailed description of the program and its evolution.

To be accepted as a beneficiary, individuals had to fill out a questionnaire that included information about household income and socio-economic characteristics.[7] The questionnaire responses were assigned predetermined scores, and households with higher scores were selected as program beneficiaries. The

[7] The questionnaire can be consulted in the rule's manual (Gobierno de Zapopan, 2011).

quota and growth of the number of beneficiaries were determined by the budgetary capacity allocated to the program each year. These procedures institutionalize this program as a programmatic policy.

The money transfers and the food package were distributed every two months. Beneficiaries would go to one of the municipal government facilities to receive the support. Specific delivery days were defined for beneficiaries based on the neighborhood they lived in. This way, all beneficiaries residing in the same neighborhood would go to the same location on the same day to receive the support.

To conduct the survey, we had access to the delivery schedule, and we were allowed to administer the questionnaires during the support distribution process. This enabled us to select beneficiaries based on their neighborhood to conform our sample.

Appendix 2. Description of the sampling process

As specified in the main text of the article, to determine the sample size, we considered a statistical power of 80% and a significance level of 5%, with an effect size of 0.38.

The first random selection was that of the sample of neighborhoods. We randomly choose 20 neighborhoods of the 72 where the beneficiaries resided; 10 neighborhoods with high levels of marginalization and 10 neighborhoods with medium levels of marginalization, according to the marginalization index defined by the Institute of Statistical and Geographic Information of Jalisco (Instituto de Información Estadística y Geográfica de Jalisco - IIEGJ).

Once we had the neighborhoods defined, we attended the benefit distribution processes corresponding to these neighborhoods.

Fifty individuals from each of the 20 selected neighborhoods were interviewed, with 25 respondents completing the control questionnaire and the other 25 completing the treatment questionnaire.

The survey questionnaires were administered during the transfers delivery processes, which were organized in groups according to the neighborhood of residence. The interviewers attended the support distribution events that corresponded to the pre-selected neighborhoods and randomly selected beneficiaries to administer the questionnaires. The randomization of the beneficiary sample in each neighborhood was done on-site at the time of survey administration.

To do administer the questionnaires, interviewers identified themselves as members of an academic institution using their official identification cards. Each interviewee was informed that the requested information was confiden-

tial and would only be used for an academic study. It was clarified that their name would not be asked at any time, nor would any identification be requested, in order to guarantee the anonymity of their responses.

The first question asked was whether the person had voted in the recent municipal election. The questionnaire was only administered to individuals who responded affirmatively to having voted in that election.

As indicated in the main text of the article, once questionnaires with capture errors or missing responses were eliminated, the final sample size was 958, with 481 observations in the control group and 477 in the treatment group.

Table 3 in the main text displays the balance between the control and treatment groups for the different variables in the sample. Figures A1-A4 also show frequency histograms for the variables of sex, age, education, and level of marginalization for the control and treatment groups. Tables A1 and A2 show the sample size for the different categories within each variable and their distribution by group. Table A3 shows the descriptive statistics for the age.

Table A1. Sex: distribution by group.

	Control Group		Treatment Group	
	Obs.	Percentage	Obs.	Percentage
Female	271	56.31	272	57.03
Male	210	43.69	205	42.97
Total	481	100	477	100

Table A2. Age

Control Group						Treatment Group					
Obs.	Mean	Median	Std. Dev.	Min.	Max.	Obs.	Mean	Median	Std. Dev.	Min.	Max.
481	42.5447	41	15.5983	18	89	477	41.6226	39	15.7276	18	85

Table A3. Level of education: distribution by group.

Level of education	Control Group		Treatment Group	
	Obs.	Percentage	Obs.	Percentage
None	52	10.81	38	7.97
Primary	178	37.00	175	36.68
Secondary	147	30.56	137	28.72
High school	71	14.76	93	19.49
College	33	6.86	34	7.12
Total	481	100	477	100

Figure A1. Frequency distribution by sex.

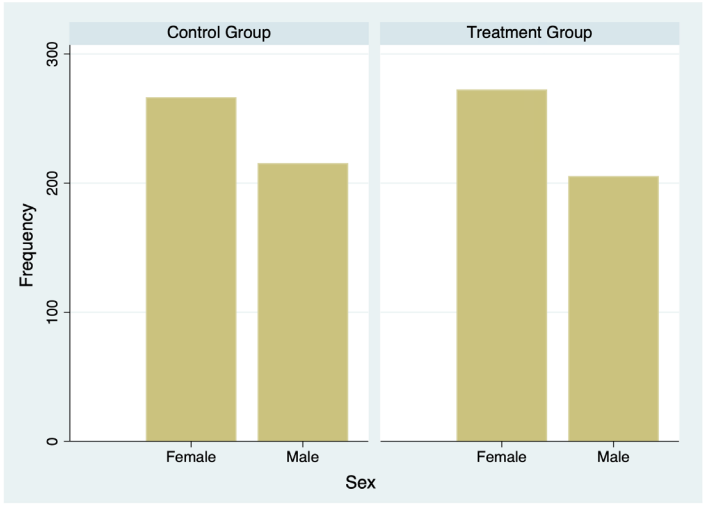

Figure A2. Frequency distribution by age.

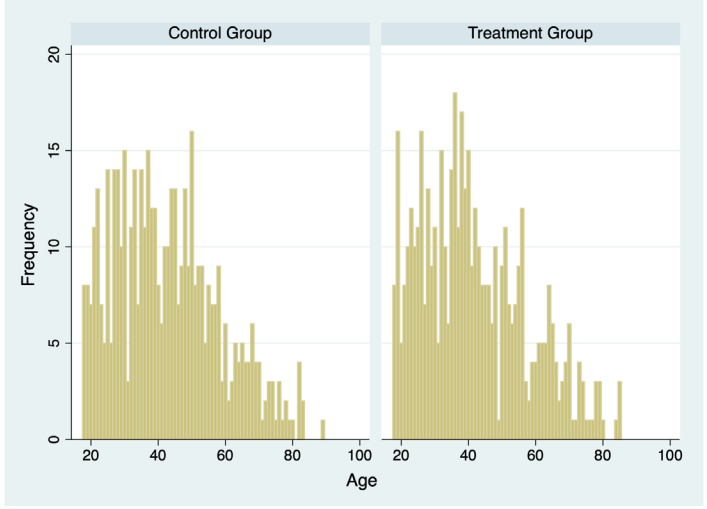

Figure A3. Frequency distribution by marginalization.

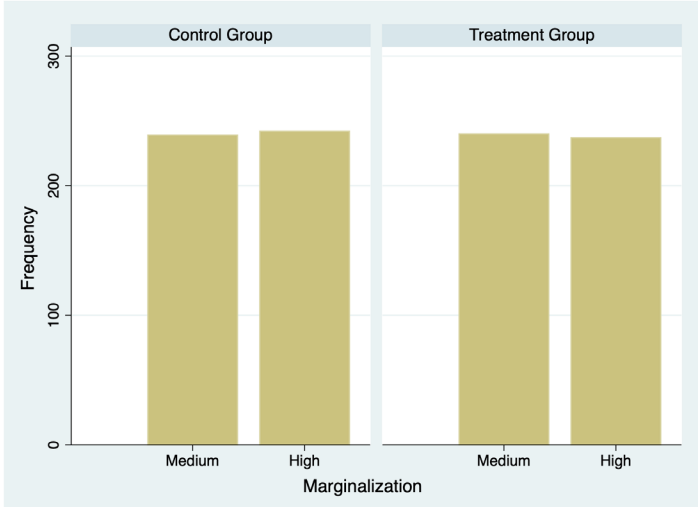

Figure A4. Frequency distribution by level of education.