

# **Supplementary Appendix**

## **After the Flood: Natural Disasters and Electoral Choices in Chile**

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# 1 Appendix A: Behavioral Benchmark

The most relevant critique of conjoint experiments is that participants are evaluating hypothetical choices; in real life they might be making different decisions. Following [Hainmueller, Hangartner and Yamamoto \(2015\)](#) approach, one method of validating the conjoint analysis is to compare it with actual voting behavior: citizens' response to the 2015 flood in the 2016 local elections.

In this behavioral benchmark, the outcome is not the incumbent vote share, as it would be in the case of traditional research studying retrospective voting. First, I analyze the impact of the flood on voting for leftist, rightist, centrist, and independent candidates (welfare candidate hypothesis).<sup>1</sup> Second, I analyze the effect of the flood on voting for older and more educated candidates.<sup>2</sup>

How can I compare affected and unaffected areas? The government declared a state of constitutional exception due to the catastrophe in 11 counties, therefore those municipalities are defined as the exposed units. One empirical strategy is to select 11 unaffected counties that are similar to the exposed municipalities. Ideally, the control group should be similar in terms of (i) unobserved and (ii) observed covariates.

Regarding point (i), I restrict the group of eligible control units to counties located north of Santiago, the capital city. The idea is to have a natural block of eligible counties from the center-north of Chile, and exclude all the municipalities located in the capital and the south of the country because they might have multiple unobserved characteristics if compared to places in northern Chile.

Regarding point (ii), I select from the sample of eligible units 11 control counties that are similar to the affected municipalities in terms of observed characteristics. I use the following pre-treatment covariates to make more credible comparisons: the right, center, left, and independent candidates vote share in the 2012 local election, total population, percentage of rural population,

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<sup>1</sup> It is not possible to test the role of the expectations about distribution of disaster relief in a behavioral benchmark.

<sup>2</sup> This empirical strategy cannot rule out the role of retrospective accountability. However, it is studying the political consequences of disasters by a different dimension since it focuses on the candidates' characteristics rather than the incumbent vote share.

human development index, and poverty levels. These covariates are included because they have been studied as factors explaining voters' behavior in Chile (González, 1999; Altman, 2004; López, 2004; Navia, Izquierdo and Morales, 2008; Luna, 2010; Calvo and Murillo, 2012). It is important to note that the nature of the control and exposed groups are different when comparing the natural experiment with the behavioral benchmark. However, both control groups share a crucial commonality: they include citizens who did not suffer material damage due to the flood. The exposed group in the behavioral benchmark might include people who did not experience damage since the treatment was assigned at the county level. Therefore, any effect can be seen as a conservative estimate.

The control units are obtained using recent advances in mathematical programming (Zubizarreta, Paredes and Rosenbaum, 2014; Zubizarreta and Kilcioglu, 2016). I use cardinality matching to obtain 11 control units that are similar to the 11 exposed counties. In particular, the goal was to achieve the largest matched sample that reduces the standardized differences in means between the groups.

Table A1 shows that covariate balance was achieved for all the pretreatment county characteristics. The algorithm kept the 11 affected counties, and optimally selected 11 other municipalities to reduce the standardized differences between both groups. The standardized differences are below the traditional requirements for illustrating balance, one-fifth of a standard deviation (Silber et al., 2013).

Table A1: Balance of pretreatment covariates

Covariate	Mean exposed	Mean control	Standardized difference
Left-wing candidates	0.60	0.59	0.05
Right-wing candidates	0.18	0.21	0.18
Centrist candidates	0.07	0.07	0.02
Independent candidates	0.15	0.13	0.11
Total population	53,808	47,016	0.08
Percentage of rural population	0.21	0.23	0.07
Human Development Index	0.72	0.72	0.03
Poverty	0.14	0.13	0.15

I use equation 2 to estimate the effect of the flood (disaster declaration) at the county level. The matched sample used for this estimation is not just balanced in terms of observed covariates, but was constructed while attempting to reduce sensitivity to hidden biases by focusing on a natural block to generate credible comparisons (cities to the north of Santiago).

$$Y_c = \alpha + \beta_1 T_c + \sigma_n + \varepsilon_c \quad (2)$$

$Y$  represents the outcome of interest for the 2016 election (vote share of left, right, centrist, independent, more educated,<sup>3</sup> and older candidates.<sup>4</sup>  $T$  depicts the treatment (declaration of emergency).  $\sigma_n$  represents region fixed effects. I expect to find results that go in the same direction as the conjoint experiment, but because of power issues they might not be significant ( $n=22$ ). Tables [A2](#) and [A3](#) summarize the results.

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<sup>3</sup> 0: High school or less, 1: More than high school. Source: public declaration of patrimony.

<sup>4</sup> 0: less than 50 years old, 1: more than 50 years old.

Table A2: Regression results

	Behavioral Benchmark: Welfare Candidates			
	Left	Right	Center	Independent
	(1)	(2)	(3)	(4)
Flood	0.097 (0.203)	-0.360* (0.175)	-0.063* (0.036)	0.327 (0.286)
Region fixed effects	Yes	Yes	Yes	Yes
Observations	22	22	22	22

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Table A3: Regression results

	Behavioral Benchmark: Managerial Candidates	
	Age	Education
	(1)	(2)
Flood	-0.035 (0.279)	0.086 (0.323)
Region fixed effects	Yes	Yes
Observations	22	22

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01

It is important to remember that we cannot directly compare the coefficients of the conjoint experiment with the behavioral benchmarks because the estimates of the former are obtained using a reference category. However, we should pay attention to the size and direction of the estimates. The results show that right-wing and centrist candidates were punished in the affected counties; meanwhile, the estimates for left-wing and independent candidates show a positive but non-significant effect. The large standard errors are probably explained by the small sample size. However, the direction of the coefficients for the welfare candidate perfectly matches the conjoint experiment. There is a positive correlation between disasters and voting for left-wing and independent candidates, and a negative one between disasters and voting for right-wing and centrist ones. The findings are also congruent for the managerial candidates' characteristics. Citizens do not seem particularly focused on selecting more experienced and educated candidates.

All these results provide more robust evidence about how disaster victims evaluate candidates' ideological labels and increase the external validity of the conjoint analysis. Voters are more likely to vote for candidates associated with social policies (or punish candidates not associated with them), and these preferences seem to overcome any focus on managerial attributes.

Finally, I expand on the details of the behavioral benchmark in the following paragraphs. The covariates used to select the control units were the following: right-wing parties<sup>5</sup> vote share in the 2012 local elections (Renovación Nacional, and Unión Demócrata Independiente); centrist parties vote share in the 2012 local elections (Partido Regionalista de los Independientes, ChilePrimero, and Fuerza del Norte); left-wing parties vote share in the 2012 local elections (Partido Igualdad, Partido Ecologista Verde, Partido Ecologista Verde del Norte, Partido Progresista, Partido Comunista, Izquierda Cristiana, Partido por la Democracia, Partido Radical Socialdemócrata, Partido Demócrata Cristiano, Partido Socialista, Movimiento Amplio Social, and Partido Humanista); independent candidates vote share in the 2012 local elections; human development index computed by the PNUD in 2003; poverty levels generated by the Ministry of Social Development in 2009, and demographic characteristics obtained from the 2002 national census.<sup>6</sup>

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<sup>5</sup> Center-left wing parties are considered as left-wing, meanwhile center-right are considered as right-wing.

<sup>6</sup> The following are the exposed counties: Antofagasta, Taltal, Copiapó, Caldera, Tierra Amarilla, Chañaral, Diego

The following are the outcome variables: right-wing parties vote share in the 2016 local election (Renovación Nacional, Evolución Política, Partido Regionalista Independiente, and Unión Demócrata Independiente); centrists parties vote share in the 2016 local elections<sup>7</sup> (Partido Regionalista de Magallanes, Amplitud, and Somos Aysén); left-wing parties vote share in the 2016 local election (Partido Ecologista Verde, Poder, Partido Demócrata Cristiano, Partido Socialista, Partido Radical Socialdemócrata, MAS Región, Izquierda Ciudadana, Partido por la Democracia, Partido Comunista, Revolución Democrática, Partido Igualdad, Frente Popular, Fuerza Regional Norte Verde, Partido Progresista, Democracia Regional Patagónica, Frente Regional y Popular, Wallmapuwen, Partido Liberal, Partido Humanista, Movimiento Independiente Regionalista Agrario y Social, and Unión Patriótica); independent candidates vote share in the 2016 local elections; age of candidates; and education of candidates.<sup>8</sup>

The goal of the mathematical algorithm used in the paper was to generate the largest matched sample that is balanced in terms of observed covariates. The balance requirements can be defined beforehand by the researchers. In this case, I focus on mean balance. This means that standardized differences between both groups should be below a particular threshold. I use the `designmatch` package in R ([Zubizarreta and Kilcioglu, 2016](#)) and the Gurobi optimizer to obtain the control group.

## 2 Appendix B: External Validity

The main evidence is coming from a particular natural disaster in the north of Chile. In this section I explore how a different disaster in a different region of the country can produce similar effects.

In 2010, the central-southern regions of Chile were shattered by an earthquake of magnitude

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de Almagro, Vallenar, Alto del Carmen, Freirina, and Huasco. Meanwhile, the following are the selected control counties that meet the covariate balance requirements: Calama, Quilpue, María Elena, Calera, Illapel, San Pedro de Atacama, Quintero, Panquehue, Combarbalá, Algarrobo, and San Antonio.

<sup>7</sup> Some of the parties that were considered in the center of the ideological spectrum in 2012 now are right-wing parties because they joined the list of the center-right coalition.

<sup>8</sup> Binary indicator of more than high school constructed using the public declaration of patrimony.

8.8. This was the 4th strongest earthquake the world had experienced during the previous 50 years. I exploit a national survey conducted four months after the flood to understand how this disaster might affect citizens' political preferences.<sup>9</sup> I follow [Zubizarreta, Cerdá and Rosenbaum \(2013\)](#) strategy to select affected counties by using the intensity of the earthquake at the county level. Counties with peak ground acceleration greater than 0.275g are identified as exposed. Respondents from those counties are assigned to the treatment group. Meanwhile, participants from municipalities that were not part of the reconstruction plan, and therefore were not affected by the earthquake, are categorized as controls. I find the largest matched sample that achieves covariate balance on three placebo covariates (i.e., gender, age, and education) by using cardinality matching. Table A4 reports the standardized differences between both groups, which are below 0.2 ([Silber et al., 2013](#)).<sup>10</sup>

Table A4: Balance of pretreatment covariates

Covariate	Mean exposed	Mean control	Standardized difference
Gender	1.55	1.58	0.07
Age	45.63	45.94	0.02
Education	3.21	3.49	0.15

The survey did not ask about preferences for welfare and social policies. However, the survey included a group of questions that can help us test some implications of the main argument. If victims support the distribution of welfare policies, those measures must be funded from somewhere. As a consequence, it is possible to expect that victims might also be more likely to support a raise in taxes.<sup>11</sup> The survey asked the following question: Do you agree or disagree with the following

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<sup>9</sup> I use the national representative survey conducted by the Centro de Estudios Pùblicos (CEP) in June-July 2010.

<sup>10</sup> The affected counties selected by the algorithm are: Arauco, Buin, Bulnes, Cabrero, Casablanca, Cauquenes, Chanco, Chiguayante, Chillan, Chillan Viejo, Concepcion, Constitucion, Coronel, Curanilahue, El Quisco, Graneros, Las Cabras, Linares, Litueche, Los Angeles, Lota, Maria Pinto, Melipilla, Ninhue, Penco, Renaico, Retiro, San Carlos, San Javier, San Pedro de la Paz, San Vicente, Santa Cruz, Talca, and Talcahuano. The control counties selected by the algorithm are: Antofagasta, Arica, Calama, Calbuco, Castro, Copiapo, Coquimbo, Coyhaique, Curaco de Velez, Illapel, Iquique, La Serena, Lago Ranco, Maullin, Natales, Osorno, Ovalle, Paillaco, Panguipulli, Puerto Montt, Punta Arenas, Quemchi, Rio Negro, San Pablo, Tocopilla, Valdivia, and Vallenar.

<sup>11</sup> This analysis is based on the assumption that Chilean citizens can connect more taxes, welfare policies, and

measures to fund the reconstruction efforts after the earthquake? (1) to raise taxes, and (2) to raise taxes on mining companies.<sup>12</sup> I use equation 3 to estimate the effect of the earthquake on victims' preferences regarding taxation. I cluster the standard errors at the municipality level.

$$Y_c = \alpha + \beta_1 T_c + \sigma_n + \varepsilon_c \quad (3)$$

$Y$  represents the outcome of interest (support a raise on taxes for the reconstruction efforts).  $T$  depicts the treatment (respondent living in a county affected by the earthquake).  $\sigma_n$  represents region fixed effects. This natural disaster should increase support for these measures because these can be linked to the implementation of welfare policies to improve citizens' living conditions after the earthquake. Table A5 summarizes the results.

Table A5: Regression results

	Policy Preferences	
	Mores taxes 1	More taxes 2
	(1)	(2)
Earthquake	0.437*** (0.000)	0.104*** (0.000)
County fixed effects	Yes	Yes
Observations	478	478

\* $p < 0.1$ ; \*\* $p < 0.05$ ; \*\*\* $p < 0.01$

As expected, affected citizens are more likely to support a raise in taxes. The framing of the question directly links the taxes with the reconstruction efforts. Affected citizens have instrumental

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left-wing politicians.)

<sup>12</sup> There are other questions that are less relevant, such as to raise taxes on cigarettes.

motivations, mainly based on improving their living conditions, and are more likely to support policies that reduce the gap between how they used to live before the earthquake and their living conditions after the earthquake. This particular post-disaster context provides a natural advantage to left-wing candidates. This analysis provides evidence about the causal mechanisms; disaster victims are changing their policy preferences because of the catastrophe (as the interviews also illustrated). However, it is not showing that victims are more likely to vote for left-wing politicians. I did not include that analysis, first because that survey did not ask about electoral preferences (it was not implemented in an electoral year), and second because that question does not help us to disentangle retrospective and prospective voting as the conjoint experiment does.

### 3 Appendix C: Pre-analysis Plan

I pre-registered a preliminary theory and design before any research activity. In the pre-analysis plan I described the characteristics of the conjoint experiment, in particular, the candidates' attributes that would be randomized. The following is an excerpt from the preregistration: "*The experiment will ask a population of citizens living in the city of Copiapo to decide between two (non-real) candidates that will be competing for the position of mayor in the 2016 local elections. The respondents will see information about six attributes of these two candidates: ideology, gender, previous political experience, profession, age and proposal for affected citizens (proxy of distribution). These attributes will be randomly chosen to generate the candidates profiles. This experimental design allows for the comparison of the explanatory power of different treatments (Hainmueller and Hopkins 2014). (...) The outcome will be the answer to the following question: if you have to vote for one of these two candidates, whom do you prefer for mayor? Each of the respondents will have to evaluate 8 pairs of profiles. Therefore, in the analysis it will be necessary to cluster the standard errors by respondent.*"

The preliminary design intended to use flood damage as a covariate instead of a treatment. The pre-analysis plan said: the "*empirical design will allow me to study the interactions between candidates attributes and respondents' characteristics. In particular I will focus on how the damage produced by the floods at the individual level (pretreatment covariate) affects the way people make electoral decisions.*" I learned about the natural experiment in the field. After having this new information, I decided to interpret the results as the treatment effect of flood damage.

In the pre-analysis plan I registered the following preliminary theoretical framework: "*What explains voters' political preferences? There are multiple factors that affect voters' electoral behavior, but these can be aggregated in two main categories (Adams et al. 2005, Calvo and Murillo 2015). The first relies on the role of ideology, and assumes that voters and parties locate themselves along an ideal point on some ideological continuum. Voters prefer the candidate/party that minimizes ideological distance. The second category emphasizes the existence of non-ideological*

*considerations in voters' decision making. This may involve voters taking into account some non-policy-related factors when they are deciding to vote for a particular candidate, such as descriptive representation (e.g. race, gender or social class), targeted distribution (e.g. vote-buying or patronage) and retrospective voting (reward/punish the incumbent when economic condition improve/worsen), among other non-programmatic variables. Adams et al. (2015) attempt to reconcile both groups of arguments by proposing a unified model of voting behavior, which integrates the behavioralist's perspective into the spatial-modeling framework. Therefore, the combination of the programmatic and non-programmatic components will explain voters' electoral decisions. However, all the theories that unified the spatial and sociological explanations assume that voters have fixed preferences regardless of the social and economic context. Ideology will have the same importance for voter  $i$  when she votes during adverse conditions (e.g. natural disaster or an economic crisis) and normal times. This project challenges this view, arguing that the importance of the ideological and non-ideological determinants of the vote are conditional to the context. Simply put, adverse conditions produced by natural disasters will affect the role of the ideological and non-ideological factors that explain voters' political preferences."*

From this framework, I presented three hypotheses: (1) Political preferences are conditional on the magnitude of the negative shock. (2) Ideology (i.e. ideological congruence) will be less relevant to voters' preferences where the damage from the disaster was higher. (3) Future distribution of financial relief will be more important for explaining voters' preferences where the damage from the disaster was higher.

The first hypothesis was confirmed: affected citizens have different political preferences than unexposed citizens, in particular regarding their ideological preferences. The second hypothesis was also confirmed, because ideological congruence is less relevant for exposed citizens. Ideological congruence is the difference between a voter's self-placement in the ideological spectrum and the ideology of her or his preferred candidate. The results show that respondents did not change their ideological placement, but affected citizens are more likely to vote for left-wing politicians. Consequently, ideological congruence becomes less salient for them. The third hypothesis was

not confirmed because of the spillover effects discussed in the paper. This latter discussion was incorporated in the paper after I learned about the empathic feelings in the field.

The previous theoretical framework mainly focused on the role of ideological congruence. Natural disasters do, in fact, reduce the ideological congruence between voters and parties, because the former are willing to vote for new candidates. However, this is a consequence of victims' focus on improving their living conditions. Therefore, the new theoretical framework (i.e., disaster victims are more likely to select political authorities who can increase their well-being after the catastrophe) is taking a step backwards to better understand voters' political preferences after natural disasters. The lack of ideological congruence is now an implication of the main theory.

In summary, I made two main amendments to the pre-analysis plan. First, I re-conceptualized flood damage as a treatment instead of a covariate. The analysis remains the same (interaction between flood damage and the conjoint experiment). Second, the preliminary theory focuses on ideological congruence, but now I develop a more general theory about how citizens modify their political preferences after natural disasters.

## **4 Appendix D: Alternative Hypotheses**

Can traditional sanctioning arguments explain the previous findings? The blind retrospection theory argues that victims do not focus on candidates' ideology, but rather tend to punish incumbents as a way to channel their emotional distress. Consequently, candidates' ideological labels should not be relevant to voters when they make electoral decisions. However, the conjoint experiment shows exactly the opposite. Affected voters are more likely than non-affected voters to choose candidates with a particular ideological label.

The research design attempts to rule out the incumbent evaluation by focusing on hypothetical candidates who voters should not have a reason to punish or reward when making electoral choices. The conjoint experiment, however, can only partially discard the role of sanctioning arguments. For example, if affected citizens are rewarding the mayor and they associate him with the left, they would be more likely to vote for left-wing candidates.

Nevertheless, the evidence from the interviews does not support this alternative hypothesis. The mayor was the most blamed political actor: both affected and non-affected voters had a negative impression of his performance. The responses to the following survey question confirm the qualitative evidence: "Speaking about the floods, how would you rate the job performance of Mayor Maglio Cicardini in handling the disaster? (1) very good, (2) good, (3) neither good nor bad (fair), (4) bad, (5) very bad." The average response was 3.97.

Another option is that the mayor is associated with the right; therefore because he is being punished, victims are more likely to vote for the left. However, the mayor does not hold a clear ideological position. He was a member of the Socialist party (center-left) before running as mayor, but in 2008 he switched to the PRI (center) and in 2012 and 2016 ran as an independent (without party affiliation). Therefore, it does not seem that rewarding left-wing candidates is an alternative way to punish the incumbent mayor.

A different causal mechanism for explaining why affected and unaffected citizens have the same preference regarding distribution of financial relief is that the latter are expecting to also get

a benefit even though they were not materially affected by the flood. The survey shows that only 5 % of non-affected respondents got aid from the state. Therefore, there should be no reason to think that non-victims can expect to get financial aid if they were not exposed to the disaster.

Are disaster victims changing their preferences or they beliefs about left-wing policies and candidates? Table A6 reports the impact of the treatment on self-placement on the ideological scale (from 1 (left) to 10 (right)) and a binary indicator of self-placement. There no significant distinction between both groups. Therefore, affected citizens are modifying their political preferences (i.e., stronger focus on welfare policies, in particular distribution of new housing) but are not changing their political beliefs (i.e., self-placement on the ideological spectrum).

Table A6: Regression results for respondents' ideology

	Outcome:	
	Ideological Position	Ideology Reported
	(1)	(2)
Treatment	-0.242 (0.557)	0.066 (0.071)
Controls	Yes	Yes
Observations	88	200

\*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Are retrospective evaluations of authorities different across exposed and unexposed areas? Can those differences drive the main results? Table A7 shows that the performance evaluations of the mayor and the president handling the disaster are statistically indistinguishable between both groups. Therefore, affected voters are not more likely to vote for a left-wing candidate because they have a worse or better opinion of the mayor or the president. The dependent variable has the following values to measures the performance of political authorities: (1) very good, (2) good, (3) neither good nor bad, (fair) (4) bad, (5) very bad.

Table A7: Regression results for authorities' evaluations

	Performance evaluation:	
	Mayor	President
	(1)	(2)
Treatment	-0.156 (0.140)	-0.101 (0.143)
Controls	Yes	Yes
Observations	195	194

\*p<0.05; \*\*p<0.01; \*\*\*p<0.001

## 5 Appendix E: The Ravine and the Bridge



Figure A1: Paipote's ravine



Figure A2: Paipote's bridge

## 6 Appendix F: Exposed and Unexposed Areas



Figure A3: Map of the affected areas (in red) marked by the local fire department

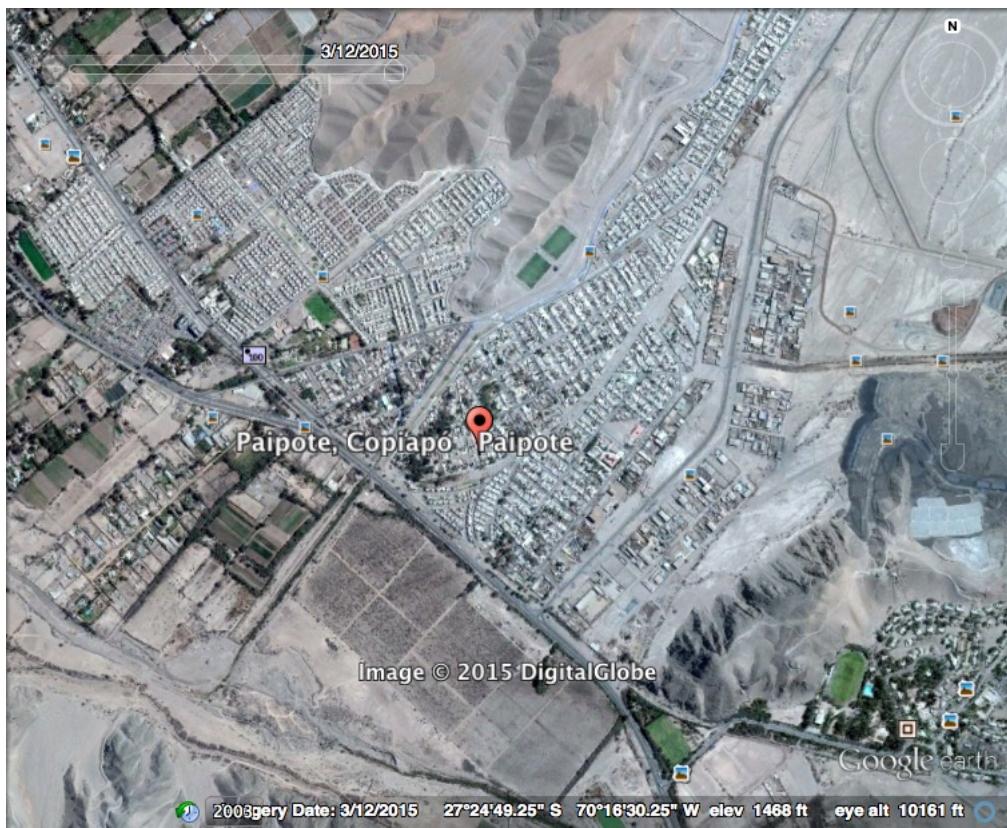


Figure A4: Google Earth; before the floods

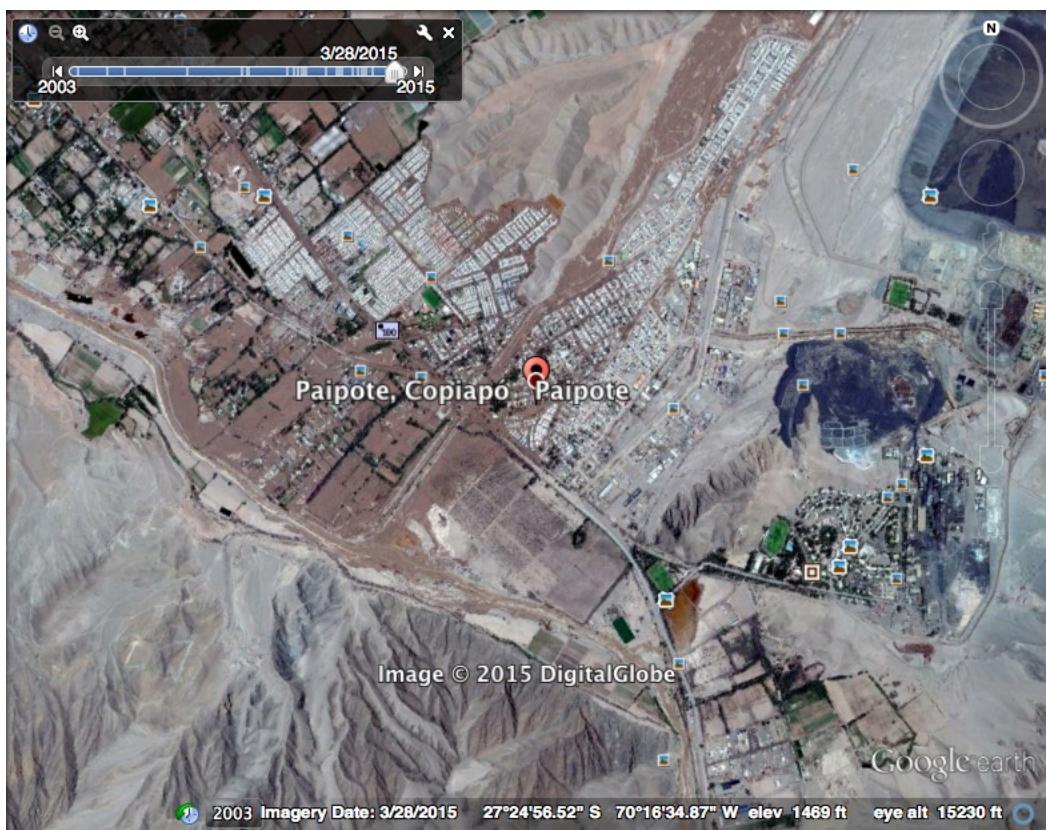


Figure A5: Google Earth; after the floods

## 7 Appendix G: Differential Effects

The comparison between the more and the less affected areas of Paipote is similar to the concept of differential effects developed by [Rosenbaum \(2006\)](#). Differential effects are immune by design to generic unobserved biases, since they should affect different treatment conditions in similar ways. Consequently, it is possible to remove the generic unmeasured biases by studying associated or parallel treatments. For example, if we want to compare the effects of crack cocaine use during pregnancy, a comparison between treated and control subjects is likely to be biased since a woman who uses crack might engage in other unmeasured activities that can also put the fetus at risk. However, we can expect a similar pattern of behavior by a woman who uses marijuana during pregnancy ([Rosenbaum, 2006](#)). Therefore, the comparison of two treatment conditions, crack cocaine and marijuana, and the exclusion of a pure control group, will allow us to rule out the generic unobserved biases common in both treatments. In the case of Paipote, there are two treatment conditions: being directly and being indirectly affected by the flood. Hence, a pure control group constructed with people from a different city that were not affected (directly or indirectly) by the flood might not be as good a comparison as the unexposed citizens from Paipote.

In summary, this research design exploits two features to decrease sensitivity to hidden biases: the low heterogeneity in Paipote, since both groups are coming from the same natural block, as well as the differential effects generated by the comparison of two associated treatment conditions.

## **8 Appendix H: Survey Implementation**

The survey was implemented in Copiapó during June 2015, three months after the disaster. The affected and unaffected areas were defined through conversations with the local police, firefighters, and citizens. It was confirmed by official government images, a map marked by the local fire department after the flood, and satellite images. Half of the questionnaires were implemented in the exposed areas, and the other half in the unexposed areas.

Regarding the conjoint experiment, the candidates profiles were generated in advance to the implementation using *R*. Each questionnaire had eight pair of candidates attached at the end. The survey and conjoint were implemented in paper.

The sampling strategy was the same across the more and less affected areas. This is a key part of the design because the differences between both sectors cannot be explained by differences in the implementation of the survey. On a given street, all households were invited to participate in the study. The door was knocked and the person that opened it was asked to answer the survey. This strategy did not attempt to build a representative sample since this is not a probabilistic sampling strategy, but it attempted to cover all the town and maximize the number of respondents. By the end of the survey, almost all the streets in town were included in the study. Only one sector was not incorporated in the design (Los Llanos 2) since it was partially affected and it is a relatively new area so that it could bring unwanted heterogeneity.

## **9 Appendix I: Local vs. National Elections**

I focus on local elections because they were to be held in 2016, closer to the survey time, while national elections would not be held until 2017. In other words, it will not be realistic to make voters think about elections that will happen in two years.

It is important to evaluate whether we expect different results based on the type of leader selected (e.g., mayors vs. presidents). Are voters evaluating politicians at the local or national level?

In the case of Chile, social programs originate in the national government, but mayors play an active role in the implementation of these programs. For instance, even though a mayor cannot directly provide new housing, he or she plays a crucial role in asking the national government for more resources and coordinating their delivery. This is illustrated in figure [A6](#), from a local newspaper in Paipote, which shows how the mayor, Maglio Cicardini (fourth from the left), participates in the ceremony transferring a house delivered by the national government to victims of the flood. Consequently, because of this complex relationship between the local and national governments, citizens have a hard time identifying who is actually providing these benefits.



Figure A6: Distribution of emergency houses (Source: Norte Noticias Diario Digital)

The following interview quote provides further support to this idea. Pamela, a Paipote resident, was selected to receive emergency housing from the national government. When she did not receive the new house on time, she went to the municipality to demand for her new housing: "I was supposed to receive emergency housing, and I have not got it. I went to the municipal community center, and even went to the municipality to talk about it." This shows how, even though the resources are allocated by the national government, local mayors play a role in their distribution. Therefore, the results of the conjoint experiment should be the same regardless if it focuses on mayors or presidents.

## 10 Appendix J: Conjoint Diagnostics

Table A8 checks the randomization of attributes by regressing respondents' gender on the candidates' attributes.

Table A8: Balance test

	Outcome
	Respondent's gender
Center	−0.018 (0.024)
Independent	0.018 (0.022)
Left	−0.002 (0.024)
Teacher	−0.012 (0.021)
Engineer	−0.011 (0.020)
Female	0.001 (0.017)
40	−0.0005 (0.018)
50	−0.006 (0.015)
Council member	−0.015 (0.019)
Mayor	−0.008 (0.019)
Will distribute a financial relief	−0.015 (0.017)
Constant	1.765*** (0.039)

Note:

\*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Table A9 checks that the results are not conditional to candidate order (within a pair).

Table A9: Profile order effects

	Outcome
	Electoral Choice
Center*Candidate 2	−0.030 (0.051)
Independent*Candidate 2	−0.047 (0.051)
Left*Candidate 2	−0.075 (0.049)
Teacher*Candidate 2	0.057 (0.044)
Engineer*Candidate 2	0.028 (0.042)
Female*Candidate 2	−0.045 (0.034)
40*Candidate 2	0.029 (0.039)
50*Candidate 2	0.042 (0.044)
Council member*Candidate 2	0.044 (0.042)
Mayor*Candidate 2	0.065 (0.039)
Will distribute a financial relief*Candidate 2	−0.004 (0.036)

*Note:* Only reporting interaction terms. \*p<0.05; \*\*p<0.01; \*\*\*p<0.001

Table A10 checks that the results are not conditional to candidate order (across the eight pairs).

Table A10: Carryover effects

	Outcome
	Electoral Choice
Center*Pair 2	0.016 (0.099)
Independent*Pair 2	−0.045 (0.094)
Left*Pair 2	−0.036 (0.094)
Center*Pair 3	0.077 (0.097)
Independent*Pair 3	−0.122 (0.090)
Left*Pair 3	0.054 (0.098)
Center*Pair 4	0.126 (0.098)
Independent*Pair 4	0.086 (0.100)
Left*Pair 4	0.031 (0.094)
Center*Pair 5	−0.061 (0.100)
Independent*Pair 5	−0.063 (0.087)
Left*Pair 5	−0.027 (0.098)
Center*Pair 6	−0.004 (0.097)
Independent*Pair 6	0.070 (0.092)
Left*Pair 6	0.032 (0.093)
Center*Pair 7	0.105 (0.102)
Independent*Pair 7	0.082 (0.094)
Left*Pair 7	0.152 (0.100)
Center*Pair 8	−0.046 (0.104)
Independent*Pair 8	0.041 (0.093)
Left*Pair 8	−0.024 (0.104)

Note: Only reporting interaction terms of the main attribute of interest. \*p<0.05; \*\*p<0.01; \*\*\*p<0.001

## 11 Appendix K: Robustness Checks

I have conducted two different robustness checks to test the sensitivity of my results to using a different treatment and sample.

When using the original treatment, 1 refers to reporting material damage, and 0 otherwise. In this robustness check, I redefine the treatment to make 1 equal to living in the area affected by the flood and 0 to living in an unexposed area.

The second robustness check tests the original specification in a matched sample. I used the `designmatch` package ([Zubizarreta and Kilcioglu, 2016](#)) to select the largest matched sample that reduces the standardized differences of the placebo covariates to be lower than 0.05. The new matched sample has 188 subjects; therefore, the matching procedure pruned 12 respondents to achieve the balance constraints defined beforehand.

Table [A11](#) reports the results of the two robustness checks. The first model uses the original sample but an alternative treatment (area), while the second model uses the original treatment but an alternative sample (matched sample). I only report the  $\delta$  coefficients (interactions) for left-wing candidates (in comparison to right-wing ones). The findings are consistent with the previous results: affected voters are rewarding candidates with a left-wing label.

Table A11: Robustness checks

Outcome:		
	Electoral Choice	
	Area as Treatment	Matched Sample
	(1)	(2)
Left*Area	0.119*	
	(0.055)	
Left*Treatment		0.130*
		(0.057)
Respondents	200	188
Observations	3200	3008

\*p<0.05; \*\*p<0.01; \*\*\*p<0.001

## 12 Appendix L: Regression Results

Table A12: Regression results

	Outcome
	Electoral Choice
Center	0.018 (0.037)
Independent	0.005 (0.033)
Left	−0.060 (0.037)
Teacher	0.060 (0.034)
Engineer	0.009 (0.036)
Female	0.029 (0.023)
40	−0.002 (0.034)
50	−0.034 (0.033)
Council member	0.006 (0.032)
Mayor	0.046 (0.036)
Will distribute a financial relief	0.260*** (0.033)
Treatment	−0.047 (0.066)
Treatment*Center	−0.007 (0.054)
Treatment*Independent	0.116* (0.053)
Treatment*Left	0.124* (0.055)
Treatment*Teacher	−0.032 (0.048)
Treatment*Engineer	−0.036 (0.050)
Treatment*Female	−0.029 (0.032)
Treatment*40	0.013 (0.046)
Treatment*50	0.028 (0.045)
Treatment*Council member	0.060 (0.044)
Treatment*Mayor	0.004 (0.047)
Treatment*Will distribute a financial relief	−0.002 (0.046)
Constant	0.292*** (0.047)

\*p<0.05; \*\*p<0.01; \*\*\*p<0.001

## 13 Appendix M: Other Reference Categories

The main results were based on using a right-wing candidate as the reference category, but it is also possible to observe voters' preferences using the different ideological positions of the candidates as the baseline categories. Figure A7 reports the results for the interactions ( $\delta$  coefficients) but now also using independent, center, and left as reference categories. Only the results for the ideological attributes are reported.

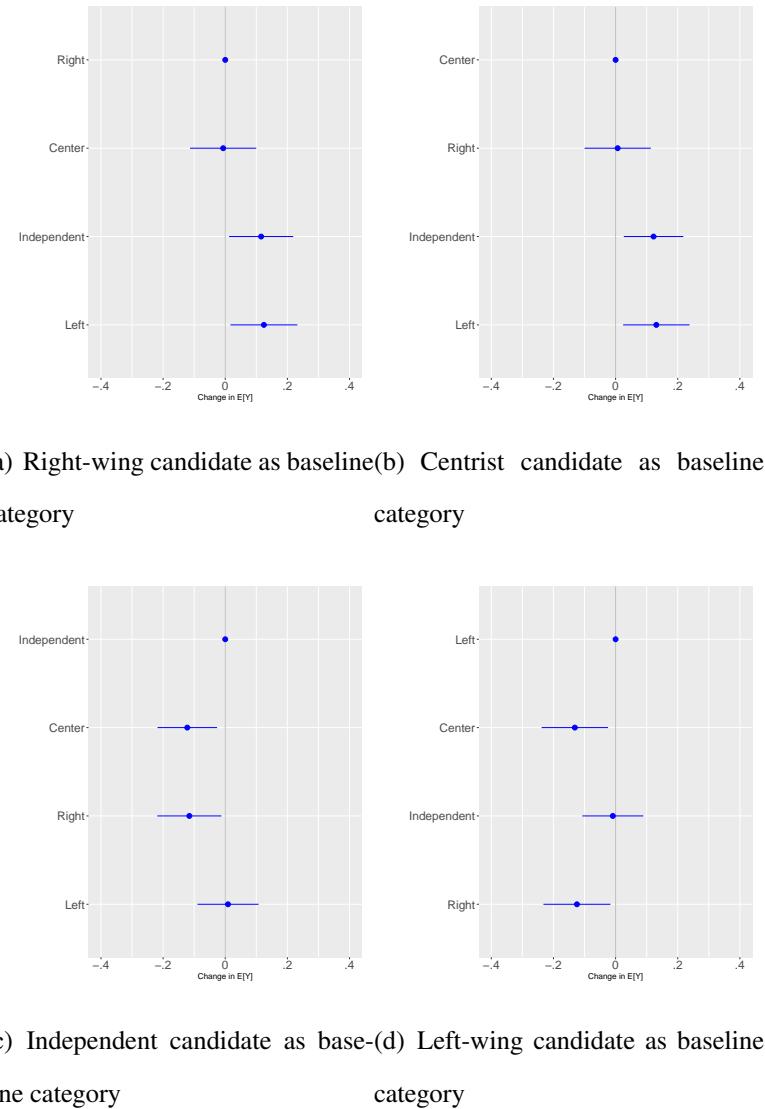


Figure A7: Effects of the flood using different reference categories for ideology

## 14 Appendix N: Interviews in Spanish

**Carmen:** "Cuando ocurrió el aluvión yo estaba acá en mi casa, y a las 4 de la mañana llegan los bomberos con la sirena informando que teníamos que evacuar toda esta calle, porque se podría ver toda esta parte inundada. Y de ahí nosotros evacuamos hacia cerca de la plaza, un poco más allá, y como a las 6 de la mañana ya empezó a llover más fuerte. Y después como a las 12, ya en la casa de mis abuelos, se vino toda el agua encima, ya no pudimos arrancar ni nada. **Investigador:** Donde queda la casa de sus abuelos, por que parte? **Carmen:** Por 21 de mayo con Juan López, por la avenida principal. **Investigador:** Y esa parte fue afectada, por las ...? **Carmen:** Si, toda esa parte fue afectada. Nosotros evacuamos mejor allá para tener más resguardo, porque yo tengo un bebe. **Investigador:** Usted esperaba que esta parte iba ser más afectada que allá abajo, pero fue al revés al final? **Carmen:** Claro, porque aca no paso nada. Como a las 12 del día se empezo a salir toda el agua de la defensa, y ya no teníamos nada que hacer. Tuvimos que empezar a subir algunas cosas de nuestros abuelos, salvarles ropa, y la comida, porque sabíamos que se venían varios días sin luz sin agua. Y tuvimos que arrancar por el patio, alcanzamos a rescatar un escalera y un primo la puso y tuvimos que salir arrancarnos todos por los patios. Ahí nos resguardamos en una casa de una vecina de mis abuelos, pero igual el agua hasta por aca, mi bebe quedo flotando en la cama. Entonces fue como super angustiante. Y en un momento yo igual pense que me iba a morir, era tanta la fuerza del agua, y con tanta rapidez que llego y entro que nosotros no sabíamos que hacer. Y habíamos muchos más ahí porque todos nos fuimos para allá. **Investigador:** Usted vive acá, esta es su casa? Y aqui que fue lo que sucedió? **Carmen:** Aca la lluvia no más, igual vino un poco de barro, porque más arriba igual se salio un poco de la defensa y todo. Pero justo esta parte como que no fue mucho, solamente barro pero que no alcanzo a entrar a las casas."

**Daniela:** (1) "Todos los proyectos que uno tenía tuvieron que cambiar, tuvieron que retroceder. Muchos se cortaron y cambiarlos por otros. El arreglo de la casa, porque ayuda no hemos tenido. (...) La prioridad en este momento es la casa, lo otro paso a segundo plano." (2) "Dar solución

a los problemas y no poner parches, es lo principal (...). Basarse en cosas más importantes o darle prioridad a las cosas que realmente tiene relevancia (...) Es más principal arreglar una casa donde un niño necesita su hogar para vivir que un paradero donde parar una locomoción colectiva."

**Pedro:** "No es tanto económico sino yo diría una ayuda más física, más como dice mi hermano, un cierre, unas casas, una vivienda definitiva. En construcciones, porque en lo económico no sirve (...) Así que la mejor ayuda sería en forma física, cierres, casas. O sea no estamos pidiendo la media casa tampoco, sino algo así como para que nos construyan algo para nosotros seguir construyendo de frente, una idea fija."

**Rosa:** "Luego de las inundaciones cambia todo. (...) Yo tenía aspiraciones, tenía sueños, y eso quedo ahí (...) Para mi ha sido complicado, mi hijo tuvo que dejar la Universidad, para mi ha sido fuerte también. (...) No pense que después del 25 de marzo me iba a cambiar tanto la vida. No pense. Tampoco creía que me podía suceder una cosa así. Despues del 25 de marzo yo pense que era un sueño. El 26 de marzo yo veía mi casa llena de barro, y decía chuta, va a llegar la noche y donde voy a dormir. Mañana voy a despertar y estoy no va a estar, porque esto es un sueño."

**Manuel:** "Yo creo que cuando uno elige a alguien no es por esas situaciones, o cosas puntuales o específicas (beneficios), así yo le voy a dar esto. No po, tiene que ser un compromiso más general con la comunidad (...) El bono tanto para este, son dos lucas, tres lucas, y a quien le sirve eso, en el tiempo a nadie. Tiene que ser algo concreto, porque yo le puedo decir le voy a dar este beneficio pero la gente va seguir en lo mismo. No son soluciones definitivas, son de momento".

**Claudia:** "A mi me gustaría que el alcalde que saliera o fuese electo se preocupara de la calidad de vida de las personas (...). Pero si, que se preocupe de la calidad de vida en todo aspecto, en todo aspecto, no que me entreguen una bolsa con comida, no, otras cosas, también."

**Ana:** "Y después escuchar los testimonios de la gente, escuchar que pudieron salvar sus vidas, amarrarse a las rejas para que el agua no los llevara (...) niñitos que perdieron todo."

**Tania:** "Yo una vez me acuerdo que me vine en la micro y había una pareja de abuelitos que habían ido a comprar, y la micro los dejó y yo me ofrecí a encaminarlos, y la abuelita me dijo sabe que hija perdimos todo, la casa, me dijo la casa era mía dijo, y yo tenía de allegada a mi hija, a mi hija le dieron y a nosotros no (...) Como veis tu, si se supone que la casa era de la abuelita, y eran dos familias, deberían darles a los dos por iguales y le dieron a una."

**Pamela:** "Me tenían que dar la vivienda de emergencia, y hasta ahora no me la han dado. Yo fui aquí hay un centro comunitario de la muni, fui y conversé, incluso fui con él a conversar a la alcaldía."

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