Online Appendix

After the Flood: Disasters, Ideological Voting and Electoral Choices in Chile

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1 Appendix A: Pre-analysis Plan

I pre-registered a research 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*[1] *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 in the pre-analysis plan: (1) Political pref-

[1] Paipote is a district within Copiapo.

erences 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) The future distribution of financial relief will be more important for explaining voters' preferences where the damage from the disaster was higher.

I made three main amendments to the pre-analysis plan. I re-conceptualized flood damage as a treatment instead of a covariate. The analysis remains the same (the interaction between flood damage and the conjoint experiment). Second, the preliminary theory mainly focuses on ideological congruence (the difference between voters' self-placement on the left-right spectrum and the ideology of her or his preferred candidate), but I decided to pay attention to a broader concept: ideological preferences (i.e., selecting candidates based on their ideological labels). Third, I moved the discussion about short-term relief and non-programmatic policies (hypothesis 3) to the appendix, which gave me the opportunity to expand on the role of ideological preferences in the manuscript.

The first hypothesis from the pre-registered plan was confirmed: affected citizens have different political preferences from unexposed citizens, in particular, regarding their ideological preferences. The second hypothesis was also confirmed. The results show that disaster victims did not change their ideological placement, but were more likely to vote for left-wing and independent politicians. Consequently, ideological congruence becomes less salient for them. The third hypothesis about the role of short-term financial relief was not confirmed, and I hold that the lack of evidence is explained by the existence of spillovers between exposed and unexposed citizens (i.e., empathic feelings). I expand on this in appendix G.

2 Appendix B: 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, I analyze the impact of the flood on voting for leftist, rightist, centrist, and independent candidates.

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 8 pretreatment covariates to make more credible comparisons.[2] 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 by using the designmatch package in R (Zubizarreta and Kilcioglu, 2016) to obtain 11 control units that are similar to the 11 exposed counties in terms of relevant observed covariates.[3]

[2] The covariates used to select the control units were the following: right-wing parties 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. Center-left wing parties are considered as left-wing, meanwhile center-right are considered as right-wing.

[3] The following are the exposed counties: Antofagasta, Taltal, Copiapó, Caldera, Tierra Amarilla, Chañaral, Diego de Almagro, Vallenar, Alto del Carmen, Freirina, and Huasco. Meanwhile, the following are the selected control counties that meet the covariate balance requirements: Pozo Almonte, Pica, Calama, San Pedro de Atacama, Maria Elena, Andacollo, Illapel, Los Vilos, Ovalle, Monte Patria, and Arica.

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 absolute 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	0.57	0.12
Right-wing candidates	0.18	0.20	0.14
Centrist candidates	0.07	0.06	0.05
Independent candidates	0.15	0.16	0.04
Total population	53,809	50,667	0.04
Percentage of rural population	0.21	0.25	0.13
Human Development Index	0.72	0.72	0.01
Poverty	0.14	0.14	0.00

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).

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

Y represents the outcome of interest for the 2016 election: vote share of left,[4] right,[5] centrist,[6] and independent candidates.[7] T depicts the treatment (declaration of emergency). σ_n represents region fixed effects. I expect to find similar results than with the conjoint experiment, but because of power issues they might not be significant (n=22). Table A2 summarizes the results.

[4] 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).

[5] Right-wing parties vote share in the 2016 local election (Renovación Nacional, Evolución Política, Partido Regionalista Independiente, and Unión Demócrata Independiente).

[6] Centrists parties vote share in the 2016 local elections (Partido Regionalista de Magallanes, Amplitud, and Somos Aysén). 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.

[7] Independent candidates vote share in the 2016 local elections.

Table A2: Behavioral Benchmark

	Left	Right	Center	Independent
	(1)	(2)	(3)	(4)
Flood	0.097	-0.360^{*}	-0.063	0.327
	(0.155)	(0.174)	(0.049)	(0.261)
Region fixed effects	Yes	Yes	Yes	Yes
Observations	22	22	22	22
Note:				*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 benchmark because the estimates of the former are obtained using a reference category. However, this analysis can provide some useful information to validate the conjoint experiment. The results go in the expected direction, a positive correlation between the flood declaration and voting for left and independent candidates, and a negative correlation for right and centrist candidates. The findings are significant for punishing right-wing candidate, which is a natural implication if left-wing politicians become more attractive.

3 Appendix C: 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 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.[8] I follow Zubizarreta, Cerdá and Rosenbaum (2013) and Visconti and Zubizarreta (2018) 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 (Zubizarreta, Paredes and Rosenbaum, 2014). Table A3 reports the standardized differences between both groups, which are below 0.2 (Silber et al., 2013).[9]

Table A3: 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/or social policies. However, the survey included a group of questions that can help us test some implications of the main results. If victims are more likely to vote for left-wing candidates because they are prone to 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. The survey asked the following question: Do you agree or disagree with the following measures to fund the reconstruction efforts after the earthquake? (1) to raise taxes, and (2) to raise taxes on mining companies.[10] 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.

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

[8] I use the national representative survey conducted by the Centro de Estudios Públicos (CEP) in June-July 2010.

[9] 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.

[10] There are other questions that are less relevant, such as to raise taxes on cigarettes.

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). σ_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 A4 summarizes the results.

Table A4: Regression results

	Policy Preferences	
	Mores taxes 1	More taxes 2
	(1)	(2)
Earthquake	0.437^{***}	0.104^{***}
	(0.000)	(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 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 to the claim that 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 because that survey did not ask about electoral preferences.

4 Appendix D: Fieldwork and Satellite Pictures

Figure A1: Paipote's ravine

Figure A2: Paipote's bridge

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

5 Appendix E: Evidence of a Natural Experiment

The story of Carmen, a 21-year-old mother, is a good example of the two points mentioned above. Carmen lived in an unexposed area where the flood did not enter her house. On the night of the flood she heard firefighters in the streets saying that people needed to evacuate because that area would be affected by mudslides. She decided to go with her baby to her grandparents' house located near the bridge. After a few hours her new refuge was completely flooded, and they barely escaped. Her own house, however, was not affected at all since it was located in an area where water did not enter homes. The decision to move from an unexposed to an exposed area reflects the lack of information about the possible trajectory of the flood.

6 Appendix F: Spillovers

In natural experiments, the identification of causal effects relies on two core (untestable) assumptions. The first is geographic treatment ignorability (Keele and Titiunik, 2016), which means that the distribution of potential outcomes should be the same for the control and exposed areas. The second is non-interference: in other words, potential outcomes for any subject do not vary with the treatment assigned to other subjects.

Because the unexposed and exposed people live in the same community, potential spillovers are part of the design itself. Thus, if we find a significant difference between the ideological preferences of each group, it can be interpreted in two ways: 1) both groups have changed their preferences in the same direction but exposed individuals are strongly modifiying them, or 2) the groups have changed their preferences in opposite directions. Qualitative evidence helps us rule out the second alternative: because non-victims express empathy toward affected citizens (see appendix G for details), both groups' preferences should therefore move in the same direction. Consequently, any violation of the non-interference assumption should bias the effects toward zero, and any effect can be seen as a conservative estimate (Keele, Titiunik and Zubizarreta, 2015). In other words, any significant result can be seen as strong evidence of a treatment effect because this is a strong case for finding any result at all.

7 Appendix G: Evidence of Emphatic Feelings

Affected citizens suffered extensive material damage and getting help from the state was an urgent concern. Non-exposed citizens witnessed the suffering of their neighbors and realized how close the flood was to their own homes. People living in non-affected areas helped their neighbors and welcomed them in their homes after the flood. Also non-affected citizens helped clean up the streets and damaged houses. Additionally, the distribution of resources to disaster victims did not imply that non-affected citizens would stop receiving social benefits not related to the disaster. The benefits were distributed to affected people contingent on disaster damage. A good example is the "bono de ensures," which consists of a benefit of 1 million pesos (1.4k US dollars) delivered to families that lose their houses or belongings during a natural disaster. These resources come from the Emergency Fund, allocated by the Ministry of the Interior. These funds are independent of the funds allocated to each public agency in Chile. They do not have a set limit and can be used as needed (CITRID, 2017). Finally, the interviews implemented in the area provided even more evidence about empathy among non-victims.

Karina is a 32-year-old housewife who was not materially affected by the flood. She said: "Emotionally it was shocking, it was shocking to see the destruction of the houses, so much material destruction. Thank God nothing happened to us [. . .] I was affected by what happened to other people, people who lost their home or people who lost their lives, it was the most shocking." She also described people's reactions in her community: "after the floods I saw the solidarity of the people, how they sent help [. . .] What was created after was a connection between neighbors. People who never exchanged a word before, but suddenly due to a catastrophe or misfortune, strong ties are formed, friends are made, and we met people we never thought could react so well."

Ana, a 33-year-old housewife, described her experienced with the floods thusly: "It was distressing, but I am a Christian and I clung to God, I clung to God in that instant and I thanked him that nothing happened to me, like many families who lost relatives, and the situations I saw were terrible." She also described how people helped each other: "Many people with shovels helping, many young people there, young people helping. My husband had to go to help a co-worker and the first floor was covered with mud. He lost everything, the car, he lost everything, and they made it up to the second floor, otherwise he would have lost his family too. I made him some lunch and went to drop it off."

Jaime, a 53-year-old mine worker, was not exposed to the floods. He expressed his frustration about how friends who were affected did not received anything: "I have friends who lost everything, and they did not get anything."

Andrea, 18 years old, reported a similar problem of people who were affected but did not get help: "Because I know of a case of a gentleman who has a liquor store, and because his house did not appear on the plan or something like that they did not give him anything (...). I find it ridiculous. The house is on the ground, and they tell you no because your house doesn't appear on the map."

Tania, a 40-year-old housewife and also a non-victim, provides the following anecdote: "I remember that when I was on the bus, I met a couple of grandparents who were going to the store. I helped them to walk back to their house, and the grandmother told me she'd lost everything, and her daughter lives with them, but only the daughter got relief benefits. What do you think about that—if they are two families, they should get two benefits, but got only one?"

8 Appendix H: Sensitivity to Hidden Biases

A powerful strategy for reducing sensitivity to hidden biases is comparing units from the same natural block. This is a desirable approach in observational studies because unmeasured covariates may be more similar within the block (Pimentel, Kelz, Silber and Rosenbaum, 2015). In this case, Paipote is a homogeneous low-middle income town—for example, 90% of the survey respondents do not have any higher education—which makes the more and the less affected citizens comparable because they are drawn from the same "natural block." Any additional data that increases heterogeneity can also increase bias (Keele, 2015). Rosenbaum (2005) shows that reducing unit heterogeneity decreases sensitivity to unmeasured biases. In particular, when there is less unit heterogeneity, there needs to be larger unmeasured biases to explain away a given effect (Sekhon, 2009). This benefit cannot be achieved by merely increasing the sample size. As Keele (2015, p.325) summarizes: "there are reasons for focusing on small samples where differences across treated and control units are reduced not by statistical means but by the design."

An additional strategy for reducing sensitivity to hidden biases relies on 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.

9 Appendix I: 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.

The main goal of the survey was to generate two groups that look very similar to each other, where the main difference between them is that one group was exposed to the flood and the other was not. To do that, the sampling strategy was exactly the same across both areas of the town: All of the streets were included in the sample covering the entire town, and on a given street all households were invited to participate in the study. The door was knocked on and the person who opened it was asked to respond to the survey. Because Paipote's population is highly homogenous, using the same sampling approach should produce comparable groups, which was the case in terms of observed characteristics. The survey was implemented over three consecutive weeks from 10 am to 6 pm.

Therefore, the sampling strategy was not trying to generate a representative sample but to produce comparable groups of respondents. Additionally, there is no available information about the population that could allow us to check whether or not the sample looks like Paipote. However, due to the low level of heterogeneity in the town's population, there is good reason to believe that the sample should look very similar to the population because units are coming from a homogenous natural block where it would be difficult to select subjects who are very different from the median respondent.[11]

Even though constructing a representative sample of Paipote was not the goal and there is no information about the population, I do provide evidence that the sample looks as we would expect. Paipote is low-middle income town that inhabitants call "the backyard of Copiapo." As a result, when comparing this working-class area with all of Copiapo, we would expect to see lower education levels. According to data from the 2017 Census, 73% of people completed high school in Copiapo. In contrast, in the Paipote sample, only 44% of respondents did so. Therefore, there is evidence that the sample looks as we would have expected (i.e., lower levels of education) when compared with Copiapo.

Half of the surveys and conjoint experiments were conducted in the more affected areas of Paipote. The affected and unaffected areas were defined through conversations with the local police, firefighters, and residents. This preliminary definition was confirmed by official government images, a map marked by the local fire department after the flood, and satellite images (see appendix D). Finally, the candidates' profiles were generated in advance of the implementation using R. Each questionnaire had eight pairs of candidates attached at the end. The survey and conjoint were implemented on paper.

Regarding the possibility of biases, I compare respondents' self-report of material damage with the affected and unaffected areas defined using satellite images. Self-reported damage is almost perfectly correlated with the area where subjects were living. In fact, I used both measures of

[11] 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.

damage (i.e., self-report and living in an affected area) and found the same results (see appendix M). As a result, I did not find evidence that people were over/underreporting material damage due to the floods.

10 Appendix J: Local vs. National Elections

As mentioned in the paper, it is important to discuss 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 55-year-old 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.

11 Appendix K: Conjoint Experiment

Table A5 reports all the labels for each attribute and table A6 illustrate a possible pair of profiles evaluated by a respondent.

Table A5: Profile of candidates

Attributes	Values
Ideology	Right
	Center
	Independent
	Left
Profession	Gardener
	Teacher
	Engineer
Gender	Male
	Female
Age	30
	40
	50
Previous Political Experience	No experience
	Council Member
	Mayor
Proposal for affected citizens	Will NOT distribute a financial relief
	Will distribute a financial relief

Table A6: Example of experimental design

Attributes	Candidate 1	Candidate 2
Ideology	Left	Right
Gender	Female	Male
Previous Political Experience	No experience	Council Member
Profession	Gardener	Engineer
Age	30	50
Proposal for affected citizens	Will NOT distribute a financial relief	Will distribute a financial relief

12 Appendix L: Conjoint Diagnostics

Table A7 checks the randomization of attributes by regressing respondents' gender on the candidates' attributes.

Table A7: Balance test

	Outcome
	Respondent's gender
Right	0.018
	(0.024)
Left	0.016
	(0.022)
Independent	0.035
	(0.021)
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)
Note:	*p<0.05; **p<0.01; ***p<0.001

Table A8 checks that the results are not conditional to candidate order (within a pair).

Table A8: Candidate order effects

	Outcome
	Electoral Choice
Right*Candidate 2	0.029
	(0.051)
Left*Candidate 2	−0.045
	(0.046)
Independent*Candidate 2	−0.018
	(0.050)
Teacher*Candidate 2	0.057
	(0.044)
Engineer*Candidate 2	0.028
	(0.043)
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 A9 checks that the results are not conditional to profile order (across the eight pairs).

Table A9: Profile order effects

	Outcome
	Electoral Choice
Right*Pair 2	−0.016
	(0.099)
Left*Pair 2	−0.052
	(0.102)
Independent*Pair 2	−0.061
	(0.101)
Right*Pair 3	−0.077
	(0.097)
Left*Pair 3	−0.023
	(0.094)
Independent*Pair 3	−0.199*
	(0.093)
Right*Pair 4	−0.126
	(0.098)
Left*Pair 4	−0.095
	(0.098)
Independent*Pair 4	−0.039
	(0.103)
Right*Pair 5	0.061
	(0.100)
Left*Pair 5	0.034
	(0.096)
Independent*Pair 5	−0.003
	(0.095)
Right*Pair 6	0.004
	(0.097)
Left*Pair 6	0.036
	(0.093)
Independent*Pair 6	0.075
	(0.092)
Right*Pair 7	−0.105
	(0.102)
Left*Pair 7	0.047
	(0.099)
Independent*Pair 7	−0.023
	(0.093)
Right*Pair 8	0.046
	(0.104)
Left*Pair 8	0.022
	(0.099)
Independent*Pair 8	0.088
	(0.088)
Note: Only reporting interaction terms of ideology.	*p<0.05; **p<0.01; ***p<0.001

13 Appendix M: 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 A10 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 δ coefficients (interactions) for left-wing and independent candidates (in comparison to centrist ones). The findings are consistent with the previous results: affected voters are rewarding candidates with a left-wing and independent label.

Table A10: Robustness checks

	Outcome: Electoral Choice	
	Area as Treatment (1)	Matched Sample (2)
Left*Area	0.131* (0.054)	
Independent*Area	0.122* (0.049)	
Left*Treatment		0.115* (0.057)
Independent*Treatment		0.119* (0.050)
Respondents	200	188
Observations	3200	3008
Note:	*p<0.05; **p<0.01; ***p<0.001	

14 Appendix N: Multiple Comparisons

Conjoint experiments are suitable for multiple comparison problems since many outcomes are evaluated at the same time. This issue can increase the chances of making a Type I error and concluding that there is an observed difference when there is not one (Coppock, 2015). In table A11, I report the p-values of the interaction between candidates' ideological attributes and exposure to treatment (i.e., the estimates of interest) using a Benjamini-Hochberg (BH) and a Bonferroni (Bon) correction to address concerns about multiple comparisons. As a reminder, left-wing and independent candidates become more attractive to voters after exposure to the floods, and those results are robust to both corrections (p-values < 0.05).

Table A11: P-values before and after multiple comparison correction

Attributes	No correction	BH correction	Bon. correction
Right∗Treatment	0.9024	0.9024	1
Left∗Treatment	0.0162	0.0243	0.0486
Independent∗Treatment	0.0124	0.0243	0.0372

15 Appendix O: Regression Results

Table A12 shows the regression results for the conjoint experiment (i.e., implementation of equation 1).

Table A12: Regression results

	Outcome
	Electoral Choice
Right	-0.018
	(0.037)
Left	-0.078^{*}
	(0.038)
Independent	-0.013
	(0.035)
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	-0.054
	(0.066)
Treatment*Right	0.007
	(0.054)
Treatment*Left	0.131^{*}
	(0.054)
Treatment*Independent	0.122^{*}
	(0.049)
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)

$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

In the pre-registered design, I mentioned a third hypothesis about the attribute: "will distribute a financial relief." I hold that exposed citizens should become more likely to support the distribution of these type of benefits, such as food baskets, since they needed urgent relief to address the conce-quences of the disaster. However, the results show that there is no difference between the exposed and control group. Both exposed and unexposed citizens are highly likely to prefer candidates who want to distribute financial relief to disaster victims, even though unexposed respondents were not affected.[12] Why would victims and non-victims have similar preferences regarding the distribution of short-term benefits? This is not a pure null result because this characteristic is the most important factor explaining voters' decisions in each subgroup, but there is no difference between the exposed group and the control. This is congruent with a spillover hypothesis. Non-victims display empathic feelings towards their neighbors because they are seeing them suffer. Qualitative evidence supports this argument. Also, there are no reasons to believe that all the other attributes that report null results within each subgroup and between the subgroups (e.g., gender) are evidence of spillover effects.

The discussion about spillovers can be seen as a post hoc theorizing. In appendix A I discuss the pre-registration of the study, which did not consider the role of interference. The importance of the empathic feelings was something learned in the field during the interviews. I believe that we should use this new information in the interpretation of the conjoint experiment and not omit it because it was not part of the pre-fieldwork theory.[13]

[12] An alternative option is that both groups had the same preference regarding the distribution of short-term benefits before the natural disaster, and material damage due to the flood did not change those preferences. That option seems very unlikely based on the magnitude of the catastrophe.

[13] This discussion about spillovers corresponds to THARKing (Transparently Hypothesizing After Results Are Known), which can "promote the effectiveness and efficiency of both scientific inquiry and cumulative knowledge creation" (Hollenbeck and Wright, 2017, p.5).

16 Appendix P: Alternative Explanations

One of the main alternative explanations for why left-wing and independent candidates became more attractive is that affected citizens are rewarding the mayor and they associate him with the left or see him as an independent. Nevertheless, the qualitative and quantitative 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 or center; 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 without party affiliation. Therefore, it does not seem that rewarding left-wing or independent candidates is an alternative way to punish the incumbent mayor since it is very hard to place him on the ideological spectrum.

Are disaster victims changing their preferences or they beliefs about left-wing policies and candidates? Table A13 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 A13: Regression results for respondents' ideology

	Outcome:	
	Ideological Position	Ideology Reported
	(1)	(2)
Treatment	−0.242	0.066
	(0.557)	(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 A14 shows that, even though victims seem to have stronger negative views about the performance of the mayor and the president handling the disaster, the difference between both groups is not statistically significant. Therefore, affected voters should not be more likely to vote for a left-wing and independent 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.

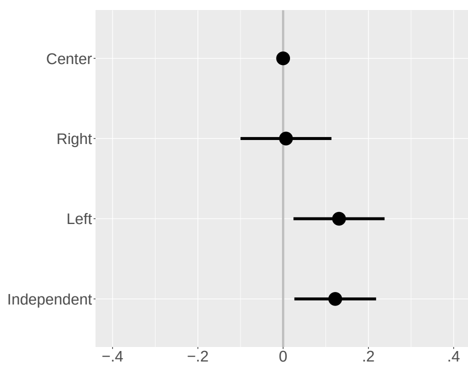
Table A14: Regression results for authorities' evaluations

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

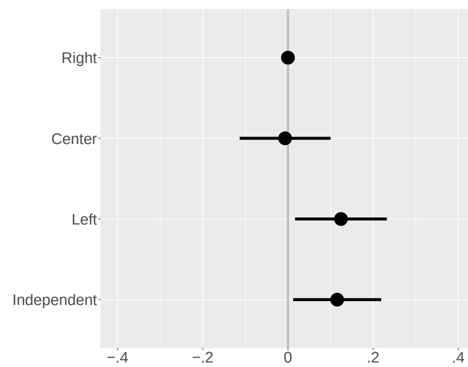
$^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

17 Appendix Q: Other Reference Categories

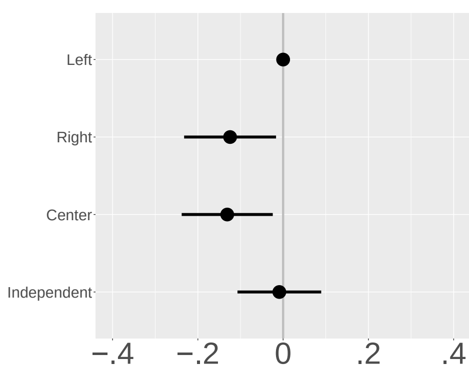
The main results were based on using a centrist candidate as the reference category, but it is also possible to observe voters' preferences using the different ideological positions of the candidates as the baseline categories. Figure A7 reports the results for the interactions (δ coefficients) but now also using right, left, and independent as reference categories.



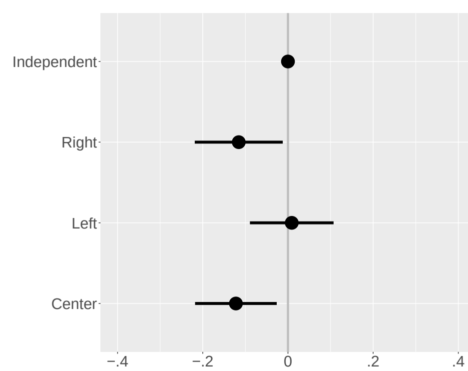
(a) Reference: centrist candidate



(b) Reference: right-wing candidate



(c) Reference: left-wing candidate



(d) Reference: independent candidate

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

18 Appendix R: Covariate Balance for Previous Vote

I also report covariate balance for previous electoral choices (2013 presidential election). I generated four new variables based on the question of who respondents voted for in the last presidential election: “Voted for Bachelet,” “Voted for Matthei,” “Voted for Enriquez-Ominami,” and “Voted for Parisi.” These were the four candidates who received the largest share of the vote, encompassing more than 92% of all votes. The results shows that both groups are comparable in terms of their previous electoral choices (i.e., standardized differences below 0.2). However, it is important to read these results with caution because they might be affected by exposure to the disaster (i.e., they might not be a placebo covariate).

Table A15: Balance of electoral choices in the 2013 presidential election

Covariate	Mean exposed	Mean control	Standardized difference
Voted for Bacheelt	0.45	0.43	0.05
Voted for Matthei	0.08	0.05	0.09
Voted for Enriquez-Ominami	0.03	0.03	0.02
Voted for Parisi	0.05	0.05	0.03

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