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Natural disasters and clientelism: The case of floods and landslides in Colombia

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ABSTRACT

What are the effects of natural disasters on electoral results? Some authors claim that catastrophes have a negative effect on the survival of leaders in a democracy because voters have a propensity to punish politicians for not preventing or poorly handling a crisis. In contrast, this paper finds that these events might favor incumbents. Disasters are linked to leader survival through clientelism: they generate an in-flow of resources in the form of aid, which increases money for buying votes. Analyzing the rainy season of 2010–2011 in Colombia, considered its worst disaster in history, I use a difference-in-differences strategy to show that in the local election incumbent parties benefited from the disaster. The result is robust in regard to different specifications and alternative explanations. Moreover, I present evidence that goes against other common explanations found in the literature and that goes in line with the clientelism mechanism.

1. Introduction

What are the effects of natural disasters on incumbency advantage? On the one hand, some authors purport that the occurrence of natural disasters reduces incumbents' reelection prospects, since voters punish their leaders even if the events are considered “acts of God” (Achen and Bartels, 2016; Quiroz and Smith, 2013). On the other hand, in line with other studies (Healy and Malhotra, 2009; Gasper and Reeves, 2011; Bechtel and Hainmueller, 2011; Cole et al., 2012), this paper finds that natural disasters might be beneficial from an electoral perspective for democratic leaders in developing countries. The mechanism proposed in this paper, that has not been fully explored in the literature so far, links disasters to incumbency advantage through clientelism: disasters generate an in-flow of resources in the form of donations and humanitarian aid, which increases cash and resources available for buying votes. Hence, disasters might increase subsequent vote shares and the probability of reelection for those holding office, if the institutional setting in which the disaster takes place favors patronage and vote-buying.

Why is it that disasters might be beneficial for incumbents? There are at least two reasons why this may be true. First, if the politicians in office are efficient at handling the crisis, an adequate provision of humanitarian assistance and relief might lead voters to reward this efficiency. In other words, a catastrophe could serve as a good opportunity for incumbents to signal their quality to voters (Ashworth, 2005; Besley, 2007; Ashworth et al., 2018), and this effect might be higher in places where voters are more responsive and better informed (Besley and Burgess, 2002; Lazarev et al., 2014). Second, incumbents might use aid and relief to buy votes. After the disaster, victims are expected to increase the propensity to sell their votes. An increased budget for buying votes and a higher propensity to sell them will result in more clientelism in favor of the incumbent. In developing countries, where institutions overseeing public spending tend to be weaker, incumbents face more favorable conditions to allocate strategically aid in order to increase the odds of staying in power. In this paper, I present evidence in favor of the clientelism mechanism and against alternative explanations.

To support these claims, between 2010 and 2011 Colombia suffered the worst rainy season of its history, while some weeks later—in October of 2011—local elections were held. More than 3 million people (about 8% of the Colombian population) were affected by the disaster and around 3.5 billion dollars had been allocated to ameliorate its consequences. Naturally, some municipalities were more affected than others and some received more resources from the central government, which created a source of variation in terms of the levels of victimization and the distribution of disaster relief. Consequently, the empirical strategy proposed in this paper utilizes a difference-in-differences (DID) estimator to calculate the causal effect of exposure to the disaster on the probability of reelection of the incumbent party.

In the simplest specification, municipalities' level of exposure to the disaster is represented by the number of victims per capita caused by floods and landslides. Other specifications include an exogenous

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measure of extreme rainfall shocks, quantified by the change in the maximum level of precipitation in 24 h during the six months in which the rainy season was more intense, for the 2006–2007 and 2010–2011 electoral periods. This measure of rainfall differs from other precipitation variables used in the development literature, which certainly tend to be correlated to important economic and institutional features of municipalities or countries. The strategy compares incumbents' probability of reelection in the 2007 (before the disaster) and the 2011 (after the disaster) mayoral races as a function of exposure to the disaster. Under a common trend assumption—and compelling evidence is presented to support it—the DID estimator provides a causal estimation of the effect of the disaster on incumbents' electoral performance.

In order to account for potential endogeneity concerns—given that the number of victims or the amount of aid received by a municipality are not completely exogenous to the political process or to municipality-level unobservable variables—besides exploiting events of extreme rainfall, this paper employs an instrumental variables approach to identify the causal effect of the disaster on electoral outcomes. Pre-disaster surface runoff at the municipal level, which is a measure of water supply per capita (or the average volume of water contained by major water bodies), is utilized as an instrument for levels of victimization. The basic argument is that municipalities surrounded by voluminous water bodies (major rivers, lakes, ponds, etc.) are more likely to experience floods during days of heavy rainfall and consequently are predicted to have a higher number of victims.

These results are robust in regard to several specifications and falsification tests. I conducted a placebo test that strengthened the causal argument posed by this paper. A second rainy season took place after the October 2011 election, which was less devastating than the first one, but still caused substantial damage and left in its wake a significant number of victims. Using the rate of victimization associated with this post-election event, no relationship was found between this disaster and electoral performance. The second rainy season was not helpful for incumbents, because any aid associated with it was disbursed after the election. This supports the argument that the first rainy season—which took place before the election—had an impact on electoral outcomes, because incumbents strategically used aid to acquire votes.

If the lethality of disasters has a negative impact on leaders' survival in democracies, and given that Colombia has a long tradition of democratic rule, why is it then the case that exposure to floods and landslides had a positive impact on the incumbent parties in the 2011 election? As previously stated, the basic mechanism proposed in this paper links disasters to political survival through clientelism—the exchange of private benefits for political support usually through brokers. Data on the Colombian disaster allows for the disaggregation of disaster aid between relief and reconstruction, something that traditionally has not been done within this literature (Besley and Burgess, 2002; Cole et al., 2012; Bodet et al., 2016). Municipalities receiving higher amounts of relief per capita—which is highly particularistic—exhibit a higher increase in the probability of reelection for the incumbent party. An equivalent association with reconstruction aid—which is more collective in nature—is not found. Furthermore, once I control for vote buying irregularities, the effect of both exposure to the disaster and humanitarian aid allocated to victims on reelection is absorbed by this measure.

As previously mentioned, other explanations could account for the fact that there is a positive association between exposure to the disaster and electoral performance of the incumbent party. For instance, voters could reward the incumbent—or at least not punish him—because he efficiently handled the crisis (Cole et al., 2012; Lazarev et al., 2014; Bodet et al., 2016). However, using different measures of state capacity, media penetration, literacy, and disaster response, no relationship is found between incumbency diligence and subsequent electoral performance. Moreover, the results also suggest that the positive effect of the disaster on incumbency advantage is not driven by changes in the dynamics of civil conflict in Colombia.

An additional potential concern of the identification strategy is that the disaster might affect agricultural production. Income shocks would have an impact on political preferences, altering the electoral performance of incumbents. To rule out this alternative channel, total planted and harvested land per capita are controlled for at the municipality level. In every specification, the effect of the disaster on the probability of reelection remains positive and significant and no heterogeneous effects are found among agricultural and nonagricultural places. Therefore, even if the disaster affects agricultural production, a positive and alternative effect on political survival still persists.

This paper is comprised of seven sections, including this introduction. Section 2 reviews the literature on the political economy of natural disasters, with particular emphasis on how these events affect electoral outcomes and, therefore, can be used strategically by political actors. Background information pertaining to the 2010–2011 rainy season in Colombia, as well as information on the 2011 local elections is provided in section 3. Section 4 describes the empirical strategy employed within this paper, founded on a difference-in-differences estimation complemented by an instrumental variables approach. Section 5 describes the data and its sources. Meanwhile, the main results of this paper are reported in section 6, including the basic specification, the instrumental variables estimations, some tests of the mechanism, and the validity of alternative explanations. Finally, section 7 provides the concluding discussion for the paper, as a whole.

2. Disasters, preparedness, and clientelism

The political economy of natural disasters has focused on explaining the variation in the lethality of these events (Anbarci et al., 2005; Cohen and Werker, 2008; Kahn, 2005; Quiroz and Smith, 2013; Raschky and Schwindt, 2016; Stromberg, 2007; Toya and Skidmore, 2007). In general, these sources argue that the nature and quality of political institutions greatly affect the number of victims caused by a disaster. While some authors focus on cross-national data that includes many types of disasters (Kahn, 2005; Stromberg, 2007; Quiroz and Smith, 2013), others focus on specific events, such as earthquakes (Escaleras et al., 2007; Brancati, 2007; Keefer et al., 2011), hurricanes (Abney and Hill, 1966; Chen, 2013a; b), or floods (Mustafa, 2003; Congleton, 2006; Eisensee and Stromberg, 2007; Fair et al., 2017).

This paper is situated within the literature on the political legacies of natural disasters. Previous work suggests that voters punish incumbents who do a poor job at implementing disaster relief policies. Achen and Bartels (2016), for instance, argue that hard times tend to threaten governments, “even in situations where objective observers can find little rational basis to suppose that those incumbents have had any part in producing the voters' pain” (p. 9). However, recent studies (Gasper and Reeves, 2011; Bechtel and Hainmueller, 2011; Cole et al., 2012; Lazarev et al., 2014; Bodet et al., 2016; Ashworth et al., 2018) challenge the “voter irrationality conclusion”, claiming that natural disasters—and in general random shocks—can be informative in regards to government preparedness. Consequently, if voters punish or reward incumbents after a catastrophe, they do so not because they are irrational, but because the event reveals information about the politician's competency at handling a crisis.

In the Colombian case, as I show in this paper, not only do citizens not punish incumbents in the aftermath of a disaster, they reward them. A possible explanation for this phenomenon, as related in Besley and Burgess (2002), is that governments are responsive because citizens are informed and politically sophisticated. Using data from extreme climatic events in India, the authors show that state governments are more responsive to disasters in places where the mass media plays a more important role and where electoral accountability is higher. Hence, this explanation would argue that the disaster signals incumbents' competency (Cole et al., 2012; Bodet et al., 2016), being the quality of the signal a function of voter sophistication, which in turn depends on factors such as education, political competition, and information

(Besley and Burgess, 2002). However, using different measures of voters' levels of information and sophistication, the evidence from the Colombian case provides little support for this explanation. The effect of relief allocation on political survival is not mediated by mass media penetration, state capacity, literacy rates, or incumbent performance after the disaster. On the contrary, it seems to be connected to the strategic (i.e., clientelistic) allocation of relief among the constituency.

Moreover, an additional contribution of this paper with respect to previous studies is that in this case it is possible to disaggregate the electoral effects of various forms of disaster aid, i.e., relief versus reconstruction aid. In a similar fashion, Healy and Malhotra (2009) show that there is a discrepancy between the electorate's reaction towards relief spending and preparedness spending. Voters reward politicians' efforts to ameliorate the consequences of a disaster that already occurred, but are relatively indifferent towards efforts to prepare ex ante for these events. In both cases, the mechanism driving this result seems to be the fact that relief spending takes the form of particularistic goods, while preparedness spending and reconstruction aid, in each case, have a collective nature. Consequently, this paper represents a contribution with respect to Healy and Malhotra (2009) because it shows that even in the case of post-disaster spending, the nature of aid matters in order to understand how voters respond to politicians' actions.

Other studies, in the context of developed countries and especially in the U.S., show how governments might *strategically* use relief spending and humanitarian aid for electoral purposes. If retrospective voters (Fiorina, 1981; Ferejohn, 1986) reciprocate in the polls towards the plight of politicians who pursue actions that increase their welfare, then aid becomes an important instrument for getting votes. In the context of the 2004 hurricane season in Florida, Chen (2013a) and Chen (2013b) show that awarding Federal Emergency Management Agency (FEMA) aid to the challenger party's voters reduces their turnout, while distributing aid to the incumbent party's supporters augments it. These effects are mediated by income and are greater a week before the election. In a similar fashion, using a placebo test I show that pre-election aid has an impact on political outcomes, while post-election relief has no effects. Nonetheless, this paper goes even further because it analyzes the effects of different types of aid and uses a clearer identification strategy. In any case, all of the evidence supports the claim that aid may be strategically allocated by incumbents to increase the odds of winning elections. The difference is that in developing countries, like Colombia, patronage and clientelism may exacerbate these incentives.

Other theories suggest that political institutions mediate the electoral consequences of natural disasters. Quiroz and Smith (2013) frame the analysis in terms of the selectorate theory (Bueno de Mesquita et al., 2003). According to these authors, the occurrence and lethality of disasters have divergent effects on the political survival of autocratic governments on the one hand, and democratic leaders on the other. This is due to the divergent sizes of the coalitions that hold politicians accountable. The authors find that in large coalition systems such as democracies, where mass support is required to retain office, the lethality of a disaster, and not its occurrence per se, has a direct negative effect on political survival. In autocratic regimes, the situation is different, as the occurrence of disasters serves as a coordination device for protests that can overthrow the leader, while lethality of disasters has no significant effects.

However, the situation quite different in developing countries like Colombia. Being a democracy, the winning coalition is certainly bigger than in an autocratic regime. Nonetheless, and in contrast to developed countries, the clientelist nature of political systems in most developing countries makes the size of winning coalitions smaller, especially at the regional and local levels. Clientelism erodes democratic institutions, as it makes politicians less accountable (Stokes, 2005), undermines the quality of public servants (Gallego et al., 2018), increases excessively the size of the public sector (Robinson and Verdier, 2013), and results in the under-provision of public goods (Keefer and Vlaciu, 2008). Hence, in the context of a catastrophe, the delivery of particularistic goods to strategic agents (e.g. the allocation of relief spending to influential local intermediaries) attenuates the pervasive electoral effects of disaster lethality, even making them advantageous for democratic leaders. In other words, when disasters hit broker-mediated democracies (Stokes et al., 2013; Gallego and Wantchekon, 2018), relief may represent a positive shock on the resources incumbents need to discipline intermediaries in charge of delivering votes. In turn, brokers are able of cultivating supporters for their candidates with these additional resources. This behavior is feasible because anti-corruption and electoral agencies, and institutions overseeing public spending, tend to be weaker.

3. Background

3.1. Colombia's 2010–2011 rainy season

President Juan Manuel Santos described the 2010–2011 rainy season as the worst natural disaster in the history of Colombia.[1] Of particular importance for understanding Colombia's current weather is the ocean-atmosphere phenomenon known as La Niña, which forms part of the broader El Niño Southern Oscillation (ENSO) pattern. La Niña corresponds to a drop in the sea surface temperature across the equatorial Eastern Central Pacific Ocean along the coasts of Peru, Ecuador, and Colombia. A significant increase in rainfall takes place, particularly in the Pacific, Caribbean, and Andean regions of the country, as a result of the combined decrease in temperatures and wind patterns.

Even though this phenomenon occurs on a yearly basis, the event that started in the second quarter of 2010 and persisted until at least April of 2011 is considered the most intense in history. During this period, rainfall registered at unprecedented levels with values that presented 170 percent above average norms (Sanchez, 2011). Extreme rainfall did not plague a particular region of the country, but struck with varying intensity throughout 28 of the 32 departamentos and 93 percent of the municipalities.

An increase in the river levels was a direct consequence of the heavy rain. Colombia's hydrology is one of the most abundant and complex around the world, with almost every municipality having access to a significant body of water. As a result, floods were a common event, causing damage and loss throughout the affected population. In addition, given the complex topography of the nation and the fact that an important proportion of its habitants are located in the Andean region, landslides caused by the accumulation of water and sediment in the ground were the other major source of devastation.

According to official records,[2] the 2010–2011 La Niña rainy season phenomenon affected about 4 million citizens (more than 8 percent of the population), with 490 human deaths, 595 people injured, 12,908 houses destroyed, and 441,579 affected, as well as 1,080,000 ha of productive land flooded (Sanchez, 2011). The Colombian disaster was not very lethal in terms of human loss, but it had an enormous social and economic impact when measured by the number of people affected and the total property destroyed. Fig. 1 depicts the spatial distribution of this Colombian disaster. In terms of the number of people affected, this event was particularly intense in the Pacific, Caribbean, and Andean regions of the country, which are also the most populous.

Compared to other natural disasters around the world, this type of impact is common to events that result from heavy rain and floods. For instance, using data from the Emergency Events Database (EM-DAT), Stromberg (2007) documents that from 1980 to 2004, a total of 621

[1] See http://www.semana.com/nacion/peor-tragedia-natural-historia-del-pais/155398-3.aspx.

[2] See http://www.regiones.gov.co/FenomenoNina/Fenomeno-Nina_110714.pdf.

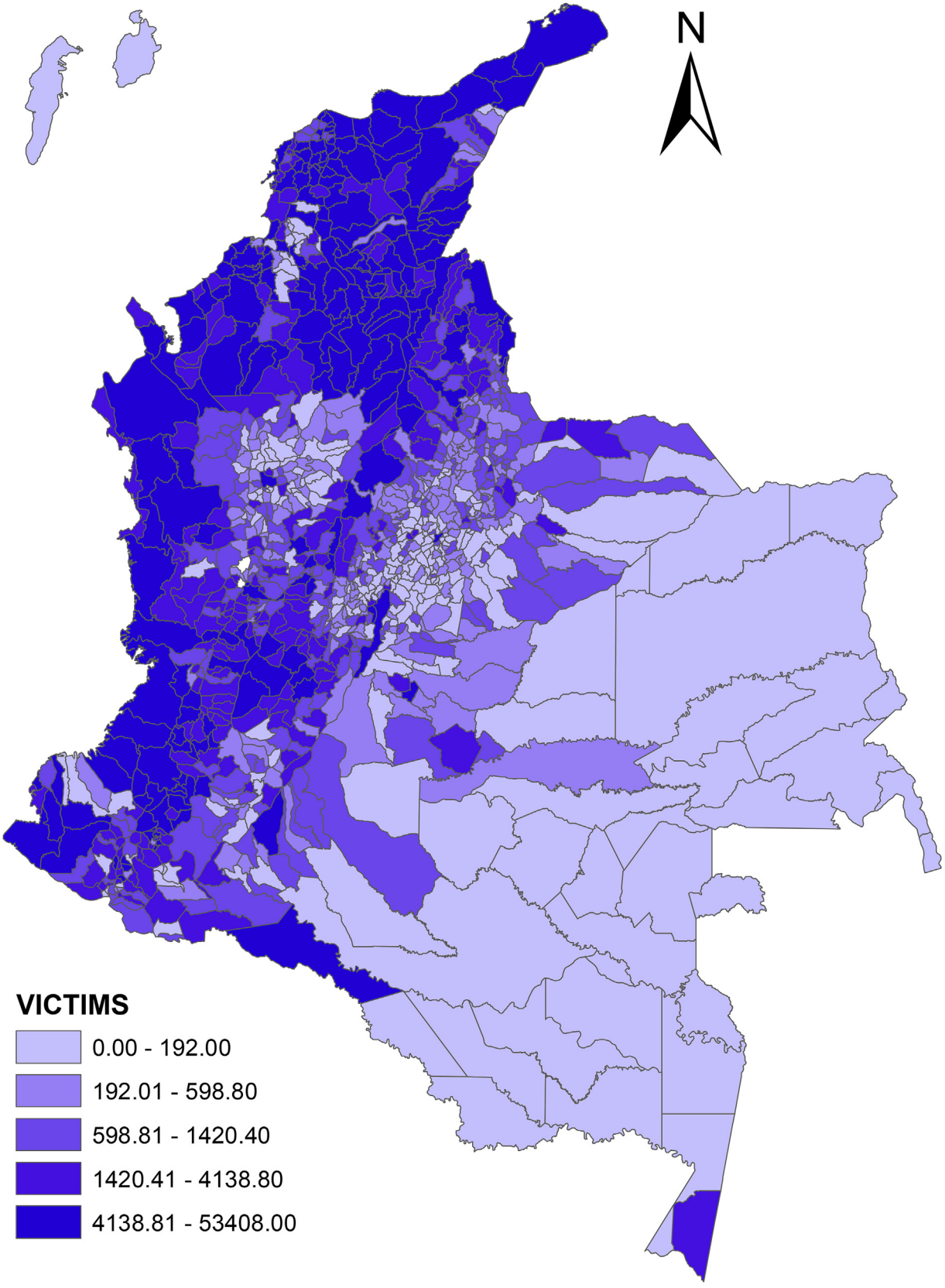

Fig. 1. Spatial distribution of exposure to the disaster (No. Of victims).

earthquakes occurred worldwide, causing approximately 215 million deaths and affecting 78 million people. In contrast, 2102 floods took place during the same period, causing 171 million deaths but affecting 2490 million people. Hence, while other disasters are more lethal in terms of fatalities, floods like the ones that took place in Colombia have higher levels of affectation.

The significant impact of La Niña led President Santos to create "Colombia Humanitaria", a National-Level Agency in charge of raising aid and distributing it to the affected communities. For this purpose, the government implemented a three-stage strategy that included humanitarian aid, reconstruction, and rehabilitation. More than U.S. $3.5 billion were raised and subsequently distributed from the central government to local authorities at both the departmental and municipal levels. To a great extent, mayors were in charge of allocating relief, and in the case of reconstruction aid, 47% of aid resources was allocated to mayors. Needless to say, mayors had a lot of discretion over these resources. In fact, President Santos signed a decree that was later approved by the Constitutional Court, that authorized mayors to leap-frog certain steps in the execution of these contracts in order to fasten the procurement process (Garbiras, 2012). Naturally, these simplifications to legal requirements expedited the processes, but in some cases fostered malfeasance and clientelism.

3.2. The 2011 local elections

This season of above-average rainfall and the associated number of floods and landslides continued through the middle of 2011. Later that year, on October 30th, local and regional elections took place with more than 130,000 citizens running for governor, mayor, or local councils. Elections for these offices took place in the country's 32 departamentos in over 1100 municipalities. Until 1988, mayors in Colombia used to be centrally appointed, but now they are elected by universal suffrage. Since then, local governments have been given increased responsibilities and powers to raise local revenues. In this sense, the mayor is a central figure in advancing the interests of the municipality. Being a multiparty system, the immediate reelection of mayors (the main focus of this paper) is prohibited in Colombia. For this reason, reelection of the incumbent party is used as a measure of political survival.

Even if mayors cannot be immediately reelected in Colombia, in 2007, and especially in 2011, they had strong incentives to support their official party. First, in 2003 a political reform was approved to deter the fragmentation of candidacies into a plethora of parties and political movements, as it used to be the case.[3] Hence, compared to previous years, in 2007 and 2011 the number of party options for candidates reduced significantly (Arevalo et al., 2013). More importantly, before the 2011 local election, another reform was approved to punish party indiscipline. Among other things, the reform punishes members of one party—especially previously elected officials—that support candidates from other parties. Hence, by law, *former* mayors cannot publicly support candidates from other parties, unless they officially defect, a decision that is both rare and costly. These reforms have been effective: in 2002, 28% of the elected congressmen had switched parties. This number increased to 51% in 2006 but decreased to 10% in 2010 and 2% in 2014. While equivalent data for mayors is hard to construct, given that these laws are valid for all corporations, it is reasonable to believe that the trend is similar in those elections. Hence, one would expect higher levels of party discipline during the 2011 post-disaster election.

Finally, qualitative and journalistic evidence supports the claim that mayors tend to select and support a successor, who usually belongs either to the same party or to the government (Lopez, 2010). As a consequence, mayors tend to be affiliated to political and economic groups that usually persist in power. UNDP (2011) calculated the 2007 political persistence index—a measure of the extent to which the same group governed continuously in a given place—for a sample of Colombian municipalities. The report found high levels of persistence in a large number of municipalities. Hence, being part of these groups that want to stay in power, it is common for mayors to influence the result of the elections in favor of their parties.[4] After all, non-consecutive reelection of mayors is allowed in this country.

Election day was relatively calm, although illegal armed groups (comprised by guerrillas and paramilitaries) tried to interfere and alter the normal conduct of elections (Gallego, 2018). Given the overlap between the political campaign associated with this election and the humanitarian efforts aimed at ameliorating the impact of the disaster, several institutions monitoring the process warned about the possibility of candidates using aid strategically for electoral purposes. On March of 2011, more than seven months before the election, the Electoral Observation Mission—a traditional NGO that monitors democracy in Colombia—launched an official document in which it warned about the risk of mayors using relief to buy votes.[5] Similarly, the Office of the Comptroller General, which is the institution in charge of fiscal control in the country, on May of that same year warned about the same risks and announced its strategy against these crimes.[6] Despite these calls and actions, mayors and different party members were not completely deterred from allocating resources strategically. After the election, in more than 15% of Colombian municipalities at least one public servant—in most cases the mayor—ended up being investigated by the Office of the Inspector General, the institution that oversees the public conduct of these officials, for "irregularities" associated to the allocation of relief[7].

4. Empirical strategy

To determine the causal effect of the natural disaster on party survival in Colombia, this paper emphasizes the fact that the 2010–2011 rainy season was not concentrated on a particular region, but instead exhibited spatial variation across the country. Therefore, it is possible to claim that the intensity of rainfall and, to some extent, the occurrence of disasters, such as floods and landslides, are exogenous to the political process and to electoral results. Different approaches will be used in order to measure a municipality's level of exposure to the disaster. In the two basic specifications used in this study, the *changes* in the levels of abnormal rainfall during the rainy season and the rate of victims per capita are used.

In this paper, a difference-in-differences estimator is utilized, in order to compare party survival in municipalities heavily and weakly affected by the disaster, before and after it occurred. For this purpose, this research controls for municipality and year fixed effects in a linear regression. Municipality fixed effects control for any specific characteristics that do not change over time and that might affect the probability of reelection or the vote share of an incumbent party, such as historical conditions or geographic characteristics of the municipalities. Time effects control for specific events that occurred in a particular year and that equally affect every municipality, such as any political reform that might have changed the rules of the game for a particular election or certain economic characteristics that caused an impact across the country. In this context, the basic specifications estimated are:

[3] The reform established that parties need to obtain at least a 2% vote share in Congress elections in order to be able to run in future elections. Through another reform, approved in 2009, this threshold increased to 3%.

[4] Should mayors support candidates from *other* parties, measurement error would flaw our dependent variable. If this form of unloyalty of mayors is orthogonal to the predictors of party reelection, estimates would not be biased. If it is not, the identification strategy (in particular the IV approach) should solve any biasedness.

[5] See http://www.semana.com/enfoque/emergencia-invernal-riesgo-electoral/153227-3.

[6] See https://www.dinero.com/economia/articulo/contraloria-advierte-posible-desviacion-recursos-para-ola-invernal/119887.

[7] See https://www.procuraduria.gov.co/portal/media/file/070911procesos.pdf.

$$Reelection_{it} = \alpha_i + \beta_t + (Victims_i \times Post2010_t)\delta + \mathbf{X}_{it}\phi + \varepsilon_{it} \quad (1)$$

$$Reelection_{it} = \alpha_i + \beta_t + (Rainfall_i \times Post2010_t)\gamma + \mathbf{X}_{it}\phi + \varepsilon_{it} \quad (2)$$

where $Reelection_{it}$ is a dummy variable[8] indicating if the incumbent party won the mayoral election in municipality i at election year t, for $t = 2007,2011$. Furthermore, α_i controls for municipality-level fixed effects while β_t for time effects. $\mathbf{X}_{it}$ is a vector representing time-varying municipality level controls. In this case and to avoid potential endogeneity caused by the inclusion of post-treatment covariates, I include pre-disaster fixed values of different controls, and interact them with a post-disaster time dummy ($Post2010_t$). This vector includes demographic controls (population and population density), a climatological control (temperature), and a socio-economic control (poverty). Finally, ε_{it} represents the error term for municipality i at election-year t.

The main variables of interest in (1) and (2) are the interaction terms $Victims_i \times Post2010_t$ and $Rainfall_i \times Post2010_t$. Here, $Victims_i$ is the number of victims per capita in municipality i, as a consequence of the 2010–2011 rainy season in Colombia. $Post2010_t$ is a dummy variable, which indicates if the event (election) analyzed took place before or after the disaster. Given that we only have two years, $Post2010_t$ also represents the year effects. Therefore, the coefficient of interest in (1), δ, measures the effect of the disaster on incumbent party reelection in 2011, as compared to the 2007 contest. Note that the constituent terms of the interaction, $Victims_i$ and $Post2010_t$, are absorbed by the fixed and time effects respectively, and do not appear separately in (1). In all specifications, standard errors are clustered at the municipality level, to control for potential serial correlation at such level. Section A7 in the Online Appendix shows that the main results of this paper are robust to clustering at the departamento level.[9]

It can be claimed that $Victims_i$ is not exogenous to municipality-level observable or unobservable characteristics. For instance, it is likely that in poor and unequal municipalities, more families will live in zones prone to floods and landslides, such as riverbanks or hillsides. Therefore, municipalities highly affected by the disaster would differ from weakly affected ones on other (possibly) non-observable characteristics, making the identification of causal effects more difficult. This fact would be troublesome if such characteristics exert a differential impact after 2010. An example of this would be if poorer places voted differently in 2011. In order to account for this potential endogeneity concerns, I employed a primary identification strategy to utilize a plausibly more exogenous measure of municipality exposure from the disaster. Landslides and floods are more likely to occur when extreme climatological events take place in short periods of time, such as episodes of heavy rain.

For this reason, the second basic specification is the one described by (2). The difference between (1) and (2) is that in the second specification, the exposure to the disaster is captured by the variable $Rain\ fall_i$, which quantifies events of extreme rainfall at the municipal level. This variable measures, originally in millimeters, the *change* in the maximum level of rainfall in 24 h experienced by municipality i during the six months between October of 2010 and March of 2011, compared to the same period for years 2006 and 2007.[10] The rationale behind this measure, similar in nature to the one employed by Burgess et al. (2014) in their analysis of the effects of abnormally hot days in India, is that in those places in which it rained more during a whole day, the probability of having subsequent floods and landslides (and hence more victims) is also higher. In other words, $Rain\ fall_i$ measures, for each municipality, how much more it rained the day that it rained the most during the 2010–2011 rainy season, compared to the 2006–2007 period.

The claim in this case is that a variable like $Rain\ fall_i$ is much more exogenous than the rate of victims caused by the disaster and than traditional measures of precipitation used in other studies (Benson and Clay, 1998; Miguel et al., 2004; Barrios et al., 2010; Bruckner and Ciccone, 2011). In essence, $Rain\ fall_i$ captures outliers, as it filters for days in which precipitation was extreme, causing catastrophes such as floods or landslides. The literature on meteorology suggests that these types of events are more likely when episodes of abnormal rainfall take place during short periods of time rather than being the result of continued days of heavy rainfall. Hence, it is less likely that this measure is correlated to economic or social variables, which usually is the case for other measures of rainfall, such as monthly deviations from historical levels.

Nonetheless, the rate of victims caused by this disaster remains to be an endogenous variable. Therefore, this paper employs an instrumental variables (IV) approach, using an exogenous source of variation correlated with the number of victims per capita and uncorrelated with the error term. For this purpose, *pre-disaster* water supply per capita at the municipal level is used to predict the number of victims, as measured by hydrologists for the 1998's National Water Study (IDEAM, 1998), *more than a decade before the catastrophe*. Hence, our instrument is by no means affected by the 2010–2011 rainfall levels. This variable measures surface runoff, which is an estimation of the volume of water contained in nonoceanic water bodies (rivers, lakes, ponds, etc.) and groundwater. Naturally, municipalities with bigger rivers or lakes tend to have a higher supply.

Water supply (interacted with the time dummy) is a valid instrument for victims of floods and landslides (also interacted) if it is not weak and if the exclusion restriction is satisfied. As shown below, it is not weak being that an increase in supply predicts an increased likelihood of floods and, hence, more victims. In terms of the correlation with agricultural activities, it is not expected to be as high as in the case of rainfall, because this variable simply measures the volume of water contained in rivers. An interesting feature of Colombia's geography is that almost every municipality has a river; therefore, this variable is not an indicator of whether a population has access to a body of water as a transportation or economic source. It simply measures the volume of water surrounding a place, which in part is the result of topographic characteristics, such as the size of the hydrographic basins that, perhaps, are exogenous to covariates affecting any political outcome. A full discussion on the validity of the exclusion restriction, and a test based on observable characteristics, are provided in section A9 of the Online Appendix.

Defining $Water_i$ as the average level of water supply per capita in municipality i before the disaster, the first stage of the IV approach estimates the following:

$$Victims_i \times Post2010 = \alpha_i + \beta_t + (Water_i \times Post2010_t)\eta + \mathbf{X}_{it}\phi + \omega_{it} \quad (3)$$

While the second stage is given by:

$$Reelection_{it} = \alpha_i + \beta_t + (Victims_i \hat{\times} Post2010_t)\rho + \mathbf{X}_{it}\phi + \varepsilon_{it} \quad (4)$$

where $Victims_i \hat{\times} Post2010_t$ are the predicted values that result from equation (3). Naturally, η is expected to be positive, as floods are more likely to occur in places with more water surrounding. Additionally, in order to test the mechanism—that disasters generate an inflow of resources to local authorities that is used to buy votes and consequently augments the probability of reelection—the following specification is estimated:

$$\begin{aligned} Reelection_{it} \quad &= \alpha_i + \beta_t + (Relief_i \times Post2010_t)\delta_1 + \\ &(Reconstruction_i \times Post2010_t)\delta_2 + \mathbf{X}_{it}\phi + \varepsilon_{it} \end{aligned} \quad (5)$$

[8] See section A1 in the Online Appendix for an explanation of the coding scheme of this variable.

[9] As posed by Dube and Vargas (2013), this test is quite stringent given that the cross-sectional variation in the key explanatory variables is at the municipality level, which are grouped into 32 departamentos.

[10] These months, in both periods, where characterized by high levels of precipitation. In fact, the 2006–2007 also caused victims, although the intensity was much lower. In fact, the 2010–2011 event was so intense, that for the first time an agency like Colombia Humanitaria was created to handle the crisis.

where $Relief_i$ and $Reconstruction_i$ are measures of the amount of relief (mainly food) and reconstruction aid per capita received by municipality i after the 2010–2011 rainy season and before the election. In this way, δ_1 and δ_2 test if these two versions of disaster aid have differential effects on the probability of reelecting the incumbent party. Subsequent estimations and alternative specifications include measured irregularities in the allocation of aid, media penetration, state capacity, literacy rates, agricultural production, and civil conflict, yielding evidence in favor of the clientelism mechanism and against alternative explanations, such as government responsiveness, civil conflict dynamics, or electoral consequences of income shocks. Additionally, a placebo test is conducted, showing that disaster aid disbursed *after* the election has no effect whatsoever on the probability of reelecting the incumbent party.

Summing up, this section describes the basic identification strategy used in this paper, which employs a difference-in-differences estimation of the effect of the disaster on party survival. An instrumental variables approach complements this strategy and different estimations are used to test the robustness of the results and the validity of the mechanism.

5. Data

5.1. Data sources

Victim-related data used in this study comes from the National Administrative Department of Statistics (DANE). A few weeks after the disaster took place, and for the purpose of quantifying its effects, DANE developed the Unique Record of Victims. According to this record, victims are "people that have suffered serious damage associated directly to the event: partial or total loss of goods (estate, livestock or crops) and/or disappearance, injury or death of family members" (DANE, 2011). In order to control for municipality size, the total number of victims is divided by the total population. Therefore, the variable $Victims_i$ used below corresponds to the number of victims per capita in municipality i as a consequence of the 2010–2011 wet season in Colombia.

Rainfall data comes from the Hydrology, Meteorology, and Environmental Studies Institution (IDEAM), which is the official Colombian government environmental agency. This institution registers rainfall on a daily basis, using more than 2500 pluviometric stations located across the country. Given that in Colombia there are about 1200 municipalities, and for some there is more than one station, while for others there are none, this study geo-referenced both stations and municipalities, calculated the distance between each,[11] and determined the nearest one. As it was described above, the variable $Rainfall_i$ is a measure of extreme climatological events and corresponds to the change in the maximum level of rainfall in 24 h, measured in millimeters, registered for municipality i from October through March, for the periods corresponding to both elections.[12] Water supply, which is an estimate of surface runoff of major water bodies and groundwater of Colombian municipalities in 1998, was measured by IDEAM and is reported in the National Water Study (IDEAM, 1998). Measured in millions of cubic meters, this variable represents the average volume of water available for Colombian municipalities during a regular year.[13]

Another disaster-related variable includes the total aid allocated to affected municipalities. This information comes from Colombia Humanitaria, a national agency created by the central government after the crisis for the purpose of facilitating assistance to victims.[14] Two types of humanitarian aid are analyzed in this paper: relief (measured as the number of grocery and hygiene kits per capita allocated to affected municipalities during or weeks after the disaster) and reconstruction aid (measured as the amount of money per capita in Colombian pesos allocated to municipalities to construct or rebuild infrastructure assets destroyed by the disaster). Information on the timing of reconstruction projects comes from Garbiras (2012). To measure the efficacy of local governments in facilitating these projects, a dummy variable called Delays is employed, which is based on information provided by the Office of the Inspector General of Colombia.[15] The report lists municipalities that were investigated in September of 2011 (one month before the election), because of delays and other irregularities in the execution of these projects. Additional information on vote-buying allegations for the 2007 and 2011 local elections was obtained from the Office of the Attorney General of Colombia.[16]

Municipality-level covariates include pre-disaster values for demographic, climatic, and socioeconomic controls. Demographic controls include levels of population and population density in 2007, as measured by DANE. The climatic covariate is the average temperature in Celsius and is also provided by DANE. Poverty is used to control for socioeconomic characteristics, measured as the proportion of citizens with unsatisfied basic needs.[17] This index was compiled by the National Planning Department (DNP). Electoral variables for the 2007 and 2011 local elections are derived from the National Registry, the official Colombian government electoral agency. Information on radio stations comes from the Ministry of Information Technologies and Communication, newspaper data from Camacho and Conover (2011), and measures of state capacity and literacy rates come from the DNP. Finally, information on agricultural production comes from the Ministry of Agriculture.

6. Results

6.1. Basic specification: difference-in-differences approach

I begin by presenting a couple of graphs that summarize the basic empirical strategy used in this paper. The main idea is to compare the proportion of municipalities in which the incumbent party got reelected in 2007 and 2011, in places more and less "affected" by the 2010–2011 disaster. In Fig. 2, municipalities affected by the disaster are those whose number of victims per capita is above the 50th percentile.[18] The graph shows that in 2007, before the disaster occurred, a similar proportion of municipalities in both groups again saw the incumbent party win the mayoral election. However, the situation is quite different in the months after the disaster took place. In 2007, there was a sharp increase in the proportion of municipalities heavily affected by the disaster where the incumbent party won, whereas this proportion remains stable (decreases a little bit) for municipalities not affected by the catastrophe. Similar results were found when comparing places exposed to higher changes in levels of extreme rainfall (above the 50th percentile) with places less exposed to this situation, as shown in Fig. 3.

The rationale behind this analysis is that if the disaster had not occurred, reelection patterns in municipalities affected and not affected would have followed a common trend and, consequently, the difference

[11] For this purpose, Stata's *Vincenty* package was used (Nichols, 2003).

[12] This particular range is chosen because of it corresponds to the most intense period. Results are robust to other rainfall specifications.

[13] Regular years are defined as those in which surface runoff corresponds to the average multi-year value of historical series of representative flows (IDEAM, 1998).

[14] See http://www.colombiahumanitaria.gov.co/Paginas/QueesColombiaHumanitaria.aspx.

[15] Procuraduría General de la Nación.

[16] Fiscalía General de la Nación.

[17] In Spanish the index capturing this number is called NBI, for *Índice de Necesidades Basicas Insatisfechas*.

[18] The shape of the figure and the intuition holds for lower and higher thresholds.

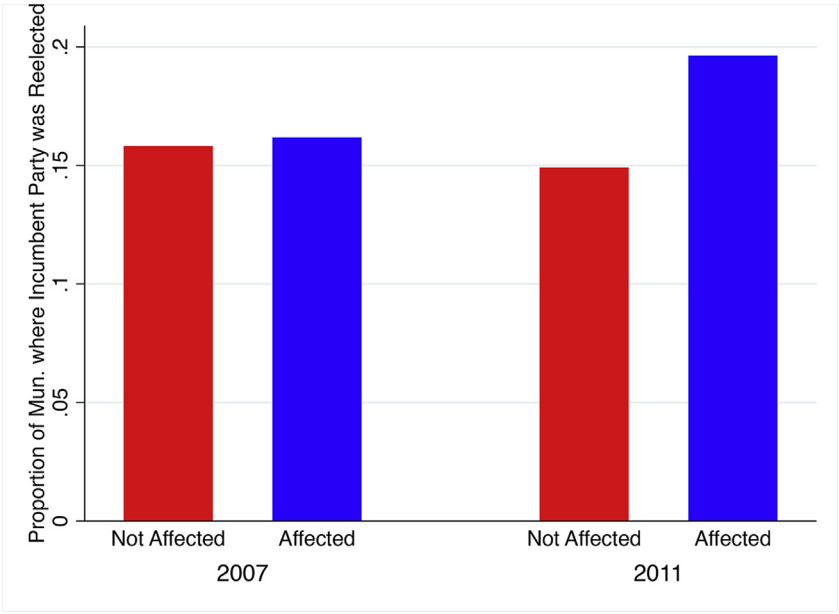

Fig. 2. Victims and reelection.

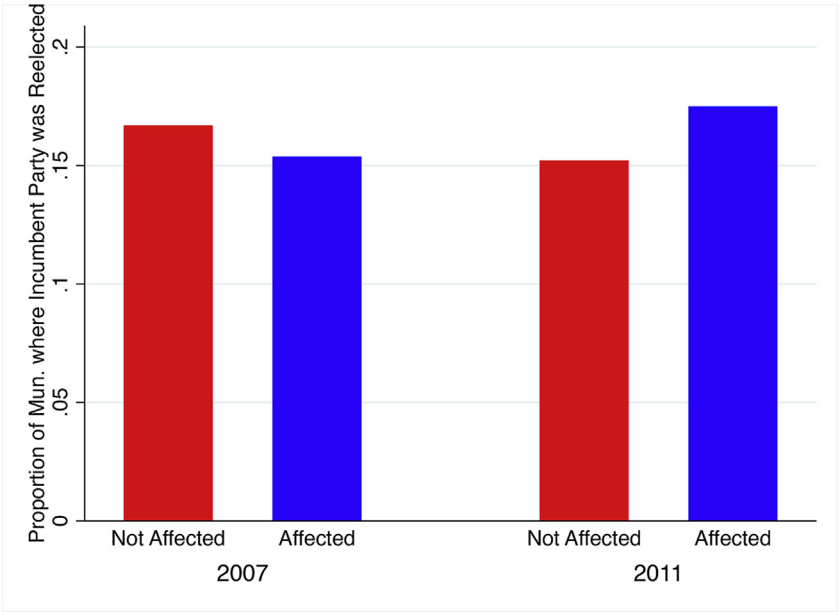

Fig. 3. Extreme rainfall and reelection.

in the actual trajectories is the causal effect of the disaster that wants to be determined. Figs. 2 and 3 suggest that the disaster caused an increase in the probability of reelection in affected municipalities compared to those not affected in 2011, relative to 2007.

In fact, Table A1 in the Online Appendix yields evidence in support for the common trend assumption. In this case, I use an extended period of analysis, by including electoral years 2003, 2007, and 2011. In this way, two pretreatment periods are included, making feasible the conventional test, in which the treatment variables—the number of victims, the change in extreme rainfall, and surface runoff in the reduced form—interact with the year dummies. Note that in these estimations the dummy variable for the year 2003 is left as the benchmark.

Table A1 shows that before the disaster, in 2007, there was no significant change in the effect of any of the three treatment variables on the probability of reelecting the incumbent party. In contrast, the coefficient for the interaction between treatment and the 2011 year dummy is positive and significant in the majority of the models. In the analysis that follows, the sample is restricted to the 2007 and 2011 timeframe, given that from 2003 backward, the political system in Colombia was extremely fragmented, exhibiting a plethora of parties. Because of this, tracking reelection is, at best, very noisy.[19] Nonetheless, robustness tests to prove that the main results of the paper hold for the extended period of analysis are reported in Appendix section A8.

Table 1, which presents estimations for different specifications based on models (1) and (2), corroborates the intuition suggested by Figs. 2 and 3. Every specification includes municipality-level fixed effects, time effects, and standard errors clustered at the municipal level. Recall that the coefficients of interest are δ and γ, the parameters

[19] For this same reason, the main outcome of interest is used in each period as a level, rather than as a change.

Table 1
Victims, extreme rainfall, and reelection.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
Victims × Post2010	0.225***	0.212***	0.163*	0.151*				
	(0.0767)	(0.0770)	(0.0840)	(0.0879)				
Rainfall × Post2010					0.806**	0.833**	0.783**	0.744**
					(0.347)	(0.348)	(0.346)	(0.345)
Constant	0.138***	0.145***	0.0608	0.0620	0.151***	0.157***	0.0785	0.0606
	(0.0136)	(0.0148)	(0.0674)	(0.0716)	(0.00927)	(0.0102)	(0.0678)	(0.0714)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2200	2196	2144	2134	2113	2111	2111	2102

Notes: Standard errors clustered at the municipality level are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Victims* is the number of victims per capita. *Rain fall* is the change in the highest level of precipitation in 24 h, measured in cubic meters, from 2006 to 2007 to 2010–2011. *Post*2010 equals 1 for 2011 observations and 0 otherwise. OLS Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

associated with $Victims_i \times Post2010$ and $Rain\ fall_i \times Post2010$. As columns 1–8 reveal, the positive and significant coefficients of the different specifications suggest that in 2011 the increase in the probability of reelection is higher in municipalities more affected by the disaster. In other words, an increase in the rate of victims, or in the change in the level of extreme rainfall, significantly increases the probability of reelection in 2011, relative to 2007. Columns 1 and 5 report the estimates when municipality-level controls are not used; columns 2 and 6 show that these results are robust to the inclusion of demographic controls, such as population and population density; columns 3 and 7 include temperature; and columns 4 and 8 incorporate poverty. In every case, the result is the same: a positive and significant coefficient for the interaction between the rate of victims per capita, or the measure of extreme rainfall, and the post-disaster time dummy.[20]

The estimates imply substantial effects. For instance, the coefficient in column 1 suggests that a one percentage point increase in the rate of victims per capita increases the probability of reelection by approximately 0.23% in 2011. Thus, compared to a situation with no victims, a municipality in which the whole population was affected experiences an increase in the probability of reelecting the incumbent party by 23 percentage points higher in 2011. Similarly, the coefficient in column 5 suggests that an increase in extreme rainfall of one cubic meter translates into an increase in the probability of reelection by 0.8 percentage points higher.[21]

Endogeneity could bias the results presented above if our main variable of interest, $Victims \times Post2010$, is correlated with error term ε_{it} in (1). This situation may occur, for instance, if municipalities with higher levels of income inequality have more inhabitants living in zones prone to floods and landslides, such as riverbanks or hillsides. If inequality also predicts whether an incumbent party is reelected, estimates based on (1) would be biased. Another cause of endogeneity and biasedness may be measurement error. For instance, it could be the case that municipalities governed by corrupt politicians tend to over-report the number of victims just for the sake of obtaining more aid. If there is a correlation between this source of measurement error (more victims registered than real victims in "corrupt municipalities") and the likelihood of reelection, the estimates based on (1) would be biased.

6.2. Instrumental variables approach: surface runoff and victims

To account for potential endogeneity bias, in this section I present an alternative approach: the use of a *pre-disaster* hydrotopographic variable as an instrument for the levels of victimization associated with the disaster. Colombia is a great power in terms of water supply. Besides being the only South American country with access to both the Atlantic and Pacific oceans, its territory contains several rivers and lakes that supply water to the population. In fact, it is difficult to find municipalities that do not have access to water bodies. Naturally, accessibility to this important natural resource is not only a function of rainfall. It also depends on topographic characteristics, which determine the size of river basins and soil accumulation, which in turn determine the levels of water supply available for each municipality. Given that most victims analyzed in this paper struggled with floods that destroyed their homes, crops, land and other properties, and that these events were not only a function of rainfall but also the existence and abundance of surrounding water bodies, surface runoff per capita will be used as an instrument for victimization.

Naturally, for the 1998 level of surface runoff to be a valid instrument, the only channel through which the interaction of runoff and the post-disaster time dummy affect reelection results is through their effect on the interaction of victims and its corresponding time dummy. It may be claimed that runoff predicts in which places people choose to live. It is for this reason that I use per capita levels in this analysis; in fact, the correlation between runoff per capita and population density is weak and statistically insignificant (results not reported). It can also be claimed that surface runoff affects economic activities, as is the case with rainfall. Nonetheless, given the nature of the instrument described in (3), for this concern to be valid it would be necessary to explain why is there a differential effect of runoff on these activities in 2011 as

[20] Note that all these models use OLS fixed effects estimations, despite having a dichotomous dependent variable. This approach is preferred, instead of a non-linear Logit or Probit model, for at least three reasons. First, to ease the interpretation of the results; second, because in the context of panel data, a linear probability model seems better suited to incorporate different methodological tools, such as fixed effects estimators, instrumental variables, and/or clustered standard errors (Greene, 2002; Arellano and Hahn, 2013); and third, because Logit or Probit models lead to an important number of municipalities being dropped because their outcome has no variation across time. However, section A10 in the Online Appendix shows that these basic results are robust to estimations based on Logit fixed effects models.

[21] In the case of extreme rainfall, note that the treatment variable accounts for the *change* in the maximum level of rainfall when going from the 2006–2007 period to the 2010–2011 one. These results, however, are robust to other measures of rainfall, such as the cross-sectional *level* of rainfall during the disaster or deviations from *historical* (average) levels of rainfall (results not shown).

Table 2
Disasters and reelection: Instrumental variables approach.

Second Stage				
	Reelection	Reelection	Reelection	Reelection
Victims × Post2010	0.449**	0.441**	0.424*	0.457*
	(0.208)	(0.209)	(0.241)	(0.272)
Post2010	−0.0613*	−0.0556	−0.0802	−0.0520
	(0.0345)	(0.0357)	(0.0706)	(0.0833)
First Stage				
	Vic × P2010	Vic × P2010	Vic × P2010	Vic × P2010
Water × Post2010	0.024***	0.024***	0.022***	0.019***
	(0.003)	(0.003)	(0.003)	(0.003)
Post2010	0.132***	0.136***	−0.018	−0.103***
	(0.006)	(0.006)	(0.023)	(0.024)
Population × Post2010	N	Y	Y	Y
Popdensity × Post2010	N	Y	Y	Y
Temperature × Post2010	N	N	Y	Y
Poverty × Post2010	N	N	N	Y
Fixed Effects	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y
First Stage F-Stat	342.15	171.85	143.62	137.40
KP Wald F-Stat	70.28	69.32	51.01	40.40
N	2136	2134	2128	2122

Notes: Standard errors clustered at the municipality level are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Victims* is the number of victims per capita. *Post*2010 equals 1 for 2011 observations and 0 otherwise. *Water* is the 1998 level of surface runoff per capita. IV Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

compared to 2007, for a reason different from the disaster itself. Besides, as it was mentioned before, water supply is not an indicator of whether a Colombian municipality has access to a river or lake for agricultural or transportation purposes, because most municipalities have access. Consequently, the basic assumption behind the use of *Water* × *Post*2010 as an instrument for *Victims* × *Post*2010 is that water supply measures the size and presence of water bodies that may cause flooding after high levels of precipitation. A full discussion on the validity of the exclusion restriction in the case, including a test on observable characteristics, is provided in section A9 of the Online Appendix.

Table 2 reports the results of the instrumental variables estimation based on models (3) and (4). Second stage results show that in 2011, as compared to 2007, higher rates of victimization caused a higher increase in the probability of reelecting the incumbent party. This finding is robust to the inclusion of demographic controls (column 2), climate (column 3), and poverty (column 4). The coefficient in column 1 suggests that a one percentage point increase in the rate of victims per capita increases the probability of reelection by approximately 0.45% more in 2011. Thus, compared to a situation with *no victims*, one in which *the whole population* is affected implies an increase in the probability of reelecting the incumbent party 45 percentage points higher in 2011.

First stage results also conform to the expectations. After the disaster, municipalities with higher levels of water supply in 1998 are predicted to have higher levels of victims per capita in 2010–2011. In every specification, an F value higher than 40 on the Kleibergen-Paap test provides evidence to reject the null hypothesis that the instrument is weak. Overall, results reported in Table 2 suggest that in 2011, municipalities surrounded by more voluminous rivers and lakes are more likely to experience floods, and that places with a higher number of victims exhibit a higher increase in the probability of reelecting the incumbent party.

In this section, an instrumental variables approach has been used to complement the difference-in-differences strategy proposed above and to determine the causal effect of exposure to a natural disaster on the probability of reelection of the incumbent party. Using the 1998 level of surface runoff, interacted with the post-disaster time dummy as an instrument for the number of victims, I found that in 2011, the disaster caused an increase in the incumbent parties' survival in municipalities heavily affected by floods and landslides. In the next section, I strengthen the argument through a placebo test and some robustness checks.

6.3. A placebo test and two robustness checks

First, I present the results of a placebo test performed to corroborate the hypothesis that a disaster increases the incumbent party's chance of reelection. So far, the measure of exposure to the catastrophe includes the number of people affected by the disaster, which corresponds to the victims of the so-called first rainy season, which took place from the second quarter of 2010 through April of 2011. However, after the election took place in October 2011, a second rainy season occurred. Although it was less intense and severe than the first one, it also caused damage and high levels of victimization. For the placebo test, the number of victims per capita from November 2011 through June 2012 is utilized. Consequently, *VictimsAfter* is interacted with the *Post*2010 dummy variable to estimate models equivalent to those reported in Table 1.

If the mechanism proposed in this paper is correct—that the disaster implies an in-flow of resources (aid) that can be used for buying votes—no effects whatsoever on the probability of reelection should be found for these new specifications. The rate of post-electoral victims should have no impact on the probability of reelecting the incumbent party, because any relief allocated for these new victims cannot be used to buy votes.

Table 3 corroborates the intuition. Columns 1–4 report the coefficients for the DID specification, using the rate of post-election victims as the treatment variable, while columns 5–8 report the results for the IV approach. In any case, the results reveal that the level of exposure to the post-election disaster has no effect on the probability of reelecting the incumbent party. In municipalities more affected by the second rainy

Table 3
Placebo test (post election victims and reelection).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
VictimsAfter × Post2010	0.0532	0.0505	−0.0326	−0.154*	7.961	7.984	−30.50	−8.983
	(0.0840)	(0.0841)	(0.112)	(0.0866)	(9.933)	(10.21)	(143.9)	(13.39)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2200	2196	2144	2134	2136	2134	2128	2122

Notes: Standard errors clustered at the municipality level are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *VictimsAfter* is the number of victims per capita during the second rainy season. *Post*2010 equals 1 for 2011 observations and 0 otherwise. OLS Fixed Effects estimation is used in columns 1–4. IV Fixed Effects estimation is used in columns 1–4. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 4
Mechanism: Relief, reconstruction, vote-buying, and reelection.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
Relief × Post2010	0.945**	0.941**	0.736*	0.633	0.341	0.395	0.310	0.270
	(0.370)	(0.370)	(0.415)	(0.421)	(0.585)	(0.587)	(0.617)	(0.618)
Reconstruction × Post2010	−0.00338	−0.00334	−0.00183	−0.00163	−0.00349	−0.00369	−0.00228	−0.00205
	(0.00529)	(0.00531)	(0.00523)	(0.00522)	(0.00529)	(0.00533)	(0.00526)	(0.00525)
Victims × Post2010					0.184	0.167	0.137	0.126
					(0.126)	(0.126)	(0.131)	(0.137)
VoteBuying × Post2010					0.0187**	0.0249**	0.0248**	0.0250**
					(0.00912)	(0.0117)	(0.0116)	(0.0114)
Constant	0.160***	0.161***	0.0899	0.0768	0.137***	0.141***	0.0840	0.0794
	(0.0118)	(0.0140)	(0.0708)	(0.0753)	(0.0162)	(0.0171)	(0.0705)	(0.0749)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2114	2110	2058	2050	2114	2110	2058	2050

Notes: Standard errors are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Relief* is the number grocery and hygiene kits distributed per capita. *Reconstruction* is the amount of Colombian thousand hundred pesos per capita disbursed for reconstruction projects. *Post*2010 equals 1 for 2011 observations and 0 otherwise. *Victims* is the number of victims per capita. *VoteBuying* is the number of vote buying allegations in the 2011 election, as reported by the Office of the Attorney General of Colombia. OLS Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

season—from November 2011 through June 2012—the change in the probability of reelection is not higher than in municipalities less affected. Hence, this placebo test corroborates the idea that what matters is the pre-electoral impact of the disaster. In future subsections, I will claim that this is true because relief is used by incumbents to buy votes.

Two additional robustness checks of the main results are reported in the Online Appendix. First, I explore more deeply the variation in the reelection rates of parties in mayoral elections in Colombia. Given that Colombian politics has been characterized in past decades by a multiplicity of parties, especially at the local level, it cannot be taken for granted that a party holding office will run for reelection in the next period. For this reason, I construct a dummy variable called $Continuity_{it}$, which equals 1 whenever the incumbent party runs for reelection, and zero otherwise. Not surprisingly, only 57% of incumbent parties ran for reelection in 2007 and 2011.

Moreover, Table A2 in the Online appendix shows that the exposure to the disaster—measured through the rate of victims (cols. 1–4), extreme rainfall (cols. 5–8), or the IV approach (cols. 9–12)—does not affect the probability of running for reelection. In contrast, Appendix Table A3 shows that exposure to the disaster has the same positive effect on the probability of reelection, even if we control for whether or not the incumbent party runs for reelection. Similar results (not reported and available upon request) are obtained if we code for whether political parties disappear for at least one sample period.

For the second robustness check presented in this subsection, I extend the period of analysis to include additional pre-disaster periods. As claimed above, in the past decade the Colombian political system has been highly fragmented and unstable, exhibiting a multiplicity of parties that make it difficult to track reelection rates. This situation is particularly evident in 2000 and 2003, before the approval of political reform that disciplined politicians and significantly reduced the number of contesting parties. Nonetheless, to prove the robustness of the estimations, Table A10 reports the results of models based on (1), (2), and (4), in which electoral years 2003, 2007, and 2011 are included in the analysis. In this case, for every specification, once more it is true that higher exposure to the disaster implies a higher increase in the probability of reelecting the incumbent party. In fact, the estimated coefficients are similar in magnitude to those reported in Tables 1 and 2.

6.4. Mechanism: relief versus infrastructure aid

If a disaster diminishes the well-being of those affected, why would it be beneficial for a democratic leader? The mechanism proposed in this paper suggests that after a disaster incumbents will have more resources to buy votes. In order to test this hypothesis, this paper presents two pieces of evidence, based on information provided by Colombia Humanitaria, on the allocation of different types of humanitarian aid among affected municipalities.

During the disaster and for weeks after it ended, the central government, through Colombia Humanitaria, distributed relief to the affected population in the form of food, hygiene kits, and other private goods. But the disaster not only affected citizens directly through the loss of human lives, homes, or livestock. It also destroyed roads, bridges, and other infrastructure assets, which naturally increased economic costs to the affected communities. For this reason, Colombia Humanitaria allocated a fair amount of humanitarian aid for the purpose of reconstructing local public goods destroyed by the disaster, and for the construction of dikes and other structures that could prevent future floods and landslides.

Is it necessarily true that high levels of reconstruction aid increased the likelihood of reelection for incumbent parties in 2011? Facts and intuition suggest that in this case, in contrast to the allocation of relief, things are not so simple. On the one hand, mayors could allocate contracts and jobs associated to reconstruction projects strategically. In fact, orders of mayors concerning reconstruction projects represented almost half of the total investment/spending in infrastructure in Colombian municipalities. But on the other hand, although Colombia Humanitaria allocated resources before the election took place, most of these projects, for different reasons, were not executed before October 30, 2011. Therefore, not all of the money was available during the electoral campaign, as opposed to relief, that was distributed prior to election day. Columns 1–4 in Table 4 report the estimates of a series of OLS Fixed Effects models showing the effect of these different forms of aid on the probability of reelecting the incumbent party. These results must be interpreted with caution, as these allocations cannot be expected to be uncorrelated with unobservable characteristics affecting electoral outcomes. Consequently, they should be interpreted as *tentative* evidence in favor of the mechanism proposed in this paper. The results suggest that municipalities receiving higher levels of relief exhibit higher increases in the probability of reelecting the incumbent party.

In contrast, no significant effect on reelection is found for reconstruction aid. In fact, to take into account that the timing of reconstruction projects is different and some of them were completed after the election, Table A14 in the Online Appendix reports the results of analogous models in which alternative and more contemporaneous measures of reconstruction aid are used instead. The results are substantively the same. Reconstruction aid has no effect on the probability of reelecting the incumbent party, while relief has a positive effect. Hence, these estimations reveal that particularistic benefits such as relief have a positive impact on party survival, while collective goods, such as reconstruction aid, have no effect. These results support the mechanism proposed in this paper. As defined and discussed in Gallego (2015), political clientelism is a relationship in which a politician (the patron) offers private goods or services to the voter (the client) in exchange for political support, which generally includes the vote. In this case, voters receive private rewards in the form of relief. Incumbents, in the meantime, are more likely to win in places more affected by the disaster and where more relief was allocated. Does it mean, then, that relief is being allocated strategically to gain votes and win elections? In the following subsections, I present more tentative evidence in support of this claim.

6.5. Disaster aid and clientelism

So far I have shown that in places more affected by the disaster—both in terms of the number of victims or through the occurrence of more severe episodes of extreme rainfall—there is a higher increase in the probability of the incumbent party being reelected. Furthermore, Table 4 suggests that reelection becomes more likely in places that receive more relief and that the same does not hold for reconstruction aid. The first piece of suggestive evidence in favor of the clientelism mechanism incorporates evidence from the Office of the Attorney General of Colombia (*Fiscalía General de la Nación*). After every election, this office reports the number of electoral crimes committed in each municipality, using a typology comprised of eleven types of crimes. For the purposes of this study, I use the fourth category, “voter corruption”, which occurs when someone “promises, pays or handles money or gifts to a citizen in exchange for his vote.” Accordingly, I construct the variable $VoteBuying_i$, which corresponds to the count of vote buying allegations in municipality i, as reported by the Office of the Attorney General after the 2011 local elections.

This measure, which has been used elsewhere to capture clientelism (Rueda, 2017), is far from perfect as it may suffer from report bias, but I believe it provides a proxy of the intensity of vote buying at the local level. Nonetheless, the results that follow should be interpreted with caution. To test if clientelism plays any role at determining the effect of the disaster—and of the allocation of relief—on political survival, I extend the previous estimation by including both the vote buying measure and the rate of victims. Columns 5–8 in Table 4 report the results of these estimations. In every specification, once we control for vote buying, the effects of both the number of victims and relief allocation diminish and become insignificant. In contrast, vote buying has a positive and significant effect on the probability of reelecting the incumbent party.

Therefore, these results tentatively suggest that the mechanism at play is clientelism, as the explanatory power of disaster exposure and relief allocation diminish once we control for vote buying allegations. This goes in line with the abundant anecdotal and journalistic evidence revealed during the election, which claimed that parties and political groups allocated relief strategically to increase their chances of staying in power.[22] But this raises an additional question strongly connected to the literature on clientelism: whether incumbents are using aid to buy vote choices (Stokes, 2005) or turnout (Nichter, 2008). Instead of intending to switch preferences, parties may try to mobilize voters that otherwise would abstain. Following this theory, we should expect differential effects of the disaster on voter turnout. However, section A4 in the Online Appendix shows that this is not the case. Correlational and causal evidence reveals that there is no connection between exposure to the disaster and turnout. This tentative evidence suggests that incumbents bought vote choices, not participation.

6.6. Alternative mechanisms

So far this paper has shown that, in comparison to the 2007 mayoral elections, incumbent parties in the 2011 election had a higher probability of being reelected in municipalities heavily affected by the disaster. Moreover, a basic mechanism has been proposed. The disaster is beneficial for incumbent politicians because it generates an inflow of aid and resources that can be used to buy votes. The patterns exhibited by food aid allocation yield tentative evidence in support of this hypothesis. Nonetheless, alternative explanations may justify these findings. In particular, these results could be justified through theories of electoral accountability and government responsiveness, the effects of agricultural income shocks, or the dynamics of civil conflict. In this

[22] See, for instance, http://www.elespectador.com/noticias/politica/proselitismo-subsidios-articulo-302748.

Table 5
Alternative mechanisms: Accountability and responsiveness.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
Delays × Post2010	0.0409	0.0397	0.0347	0.0327	0.0181	0.0190	0.0161	0.0122
	(0.0347)	(0.0349)	(0.0350)	(0.0347)	(0.0410)	(0.0413)	(0.0417)	(0.0415)
Relief × Post2010					1.100**	1.097**	0.894*	0.766
					(0.432)	(0.432)	(0.474)	(0.477)
Reconstruction × Post2010					−0.0138	−0.0138	−0.0114	−0.0112
					(0.00839)	(0.00845)	(0.00851)	(0.00850)
Delays × Relief × Post2010					−0.485	−0.492	−0.450	−0.370
					(0.856)	(0.857)	(0.945)	(0.944)
Delays × Recons × Post2010					0.0178*	0.0177*	0.0162	0.0162
					(0.0107)	(0.0107)	(0.0107)	(0.0108)
Constant	0.165***	0.170***	0.0456	0.0336	0.163***	0.164***	0.100	0.0852
	(0.0109)	(0.0119)	(0.0677)	(0.0711)	(0.0157)	(0.0177)	(0.0719)	(0.0767)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2200	2196	2144	2134	2114	2110	2058	2050

Notes: Standard errors are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Delays* is a dummy variable that equals 1 if the municipality was investigated by the Office of the Inspector General of Colombia for delays in infrastructure projects. *Relief* is the number grocery and hygiene kits distributed per capita. *Reconstruction* is the amount of Colombian thousand hundred pesos per capita disbursed for reconstruction projects. *Post*2010 equals 1 for 2011 observations and 0 otherwise. OLS Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

subsection, I present evidence against these alternative explanations.

6.6.1. Electoral accountability and government responsiveness

A first possibility would be that incumbent parties increase their probability of reelection after the disaster because they are efficient at alleviating the pervasive consequences associated to the tragedy and, consequently, voters reward this efficiency at the polls. This alternative explanation is closely related to the retrospective voting theory, in which voters punish incumbents that they consider diminished their well-being through their actions. On the contrary, if they consider that the incumbent's policies have increased in some way their welfare—and this would be the case of citizens affected by the disaster whose suffering is ameliorated or whose losses are diminished thanks to the efficient strategies implemented by the authorities—a natural way to reciprocate would be to support the party in office.

I present several tests to rule out this explanation. First, in September 2011, about a month before the election, the Attorney's Office published a list of municipalities that were being investigated because relief and prevention projects presented delays in their execution. Therefore, under the assumption that voters have knowledge of this situation—either because they have access to the list or because they are aware of the delays in their municipality—incumbent governments investigated because of their inefficiency in alleviating the crisis should have exhibited a lower probability of reelection. Table 5 reports evidence against this hypothesis. Incorporating the variable $Delays_i$ — an indicator of whether a municipality had any delayed projects— into the basic specification, columns 1–4 show that there is no differential effect of this variable on the probability of reelection. Moreover, columns 5–8 show that the effect of relief allocation remains strong and positive even when we control for delays and that there are no heterogeneous effects at this level. The positive effect of relief on political survival is not lower (higher) in places where incumbents were less (more) efficient.

But perhaps citizens are not aware of these delays. It could be the case that citizens in fact reward more responsive governments. Hence, as stated in Besley and Burgess (2002), incumbents will have electoral incentives to respond to the disaster, and responsiveness will be higher in places where voters are more sophisticated. Consequently, according to these arguments, the positive effect of relief on reelection should be higher in places with greater levels of state capacity, where voters have more access to the media and higher levels of education. Tables A5-A7 in the Online Appendix show that in the Colombian case, these assertions are not true. First, using the number of radio stations in each municipality[23] or the level of circulation of the main newspaper in Colombia (Camacho and Conover, 2011),[24] Table A5 shows that the effect of relief does not vary heterogeneously across places with higher or lower levels of media penetration.

Similarly, using the Integral Performance Index, compiled by the National Planning Department to measure state capacity, Table A6 shows that the effect of relief is robust to the inclusion of this indicator (columns 1–4), and that this impact is not higher in places with more state capacity (columns 5–8). Finally, using the average years of schooling as a measure of literacy, Table A7 again shows that the effect of relief is robust to the inclusion of this measure and that there are no heterogeneous effects at this level. In sum, all this evidence supports the argument that voters are not rewarding more responsive governments, since there is no association between efficiency (measured through the occurrence of delayed projects) and reelection. The effect of relief also is not higher in places with stronger states, higher levels of media penetration, or greater levels of literacy. Furthermore, the effect of relief survives to the inclusion of all these variables.

6.6.2. Agricultural income shocks

A third possibility is that the income shocks caused by the disaster, especially among those families that heavily depend on agricultural production, alter political preferences and hence exert some influence on electoral results. Retrospective voting theory suggests that negative income shocks should harm incumbents. Consequently, it would be

[23] In this case, $Radio_i$ measures the number of FM radio stations per capita in 2011. Similar results are obtained if AM stations are included as well.

[24] In Colombia, Periódico El Tiempo is the main newspaper. Note that the number of observations is lower in these specifications, as information is not available for all municipalities.

Table 6
Disaster impact and agricultural production.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
Victims × Post2010	0.209**	0.215**	0.189*	0.175	0.211**	0.216**	0.190*	0.177*
	(0.103)	(0.103)	(0.106)	(0.107)	(0.0990)	(0.0990)	(0.102)	(0.103)
Planted × Post2010	0.135	0.126	0.128	0.156				
	(0.113)	(0.115)	(0.114)	(0.118)				
Planted × Victims × Post2010	−0.351	−0.351	−0.318	−0.340				
	(0.548)	(0.548)	(0.551)	(0.552)				
Harvested × Post2010					0.156	0.145	0.147	0.176
					(0.130)	(0.132)	(0.131)	(0.135)
Harvested × Victims × Post2010					−0.437	−0.435	−0.394	−0.418
					(0.604)	(0.604)	(0.607)	(0.608)
Constant	0.119***	0.116***	0.0421	0.0209	0.119***	0.116***	0.0425	0.0220
	(0.0211)	(0.0219)	(0.0714)	(0.0775)	(0.0212)	(0.0220)	(0.0718)	(0.0782)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2050	2050	2050	2042	2050	2050	2050	2042

Notes: Standard errors clustered at the municipality level are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Victims* is the number of victims per capita. *Planted* is the proportion of planted area in the municipality in 2005. *Harvested* is the proportion of harvested area in the municipality in 2005. *Post*2010 equals 1 for 2011 observations and 0 otherwise. *Literacy* is the pre-disaster average number of years of education. OLS Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

Table 7
Disaster impact and civil conflict.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection	Reelection
Victims × Post2010	0.227***	0.214***	0.164*	0.152*	0.239***	0.226***	0.175**	0.162*
	(0.0766)	(0.0769)	(0.0838)	(0.0880)	(0.0788)	(0.0791)	(0.0867)	(0.0911)
FARC × Post2010	47.04	30.15	44.51	57.69	182.7	147.8	59.33	52.27
	(353.2)	(353.2)	(354.5)	(370.2)	(527.3)	(528.2)	(536.4)	(548.3)
Paras × Post2010	−2436.4	−2405.3	−2478.4	−2497.6	−392.2	−357.9	−518.6	−555.6
	(2375.9)	(2380.3)	(2350.9)	(2385.4)	(2714.9)	(2724.9)	(2729.5)	(2822.6)
FARC × Victims × Post2010					−796.4	−694.8	−115.7	1.010
					(2395.2)	(2401.9)	(2446.4)	(2512.1)
Paras × Victims × Post2010					−10667.4	−10680.4	−10205.8	−9917.2
					(8639.2)	(8690.0)	(8605.9)	(8783.9)
Constant	0.139***	0.146***	0.0582	0.0592	0.137***	0.144***	0.0573	0.0587
	(0.0141)	(0.0153)	(0.0675)	(0.0719)	(0.0144)	(0.0156)	(0.0678)	(0.0722)
Pop. and Density	N	Y	Y	Y	N	Y	Y	Y
Temperature	N	N	Y	Y	N	N	Y	Y
Poverty	N	N	N	Y	N	N	N	Y
Fixed Effects	Y	Y	Y	Y	Y	Y	Y	Y
Time Effects	Y	Y	Y	Y	Y	Y	Y	Y
N	2114	2110	2058	2050	2114	2110	2058	2050
N	2114	2110	2058	2050	2114	2110	2058	2050

Notes: Standard errors clustered at the municipality level are shown in parentheses. Election years in the sample are 2007 and 2011. *Reelection* is a dummy variable that equals 1 if the incumbent party wins the election. *Victims* is the number of victims per capita. *FARC* is the 2009 rate of FARC attacks. *Paras* is the 2009 rate of paramilitary attacks. *Post*2010 equals 1 for 2011 observations and 0 otherwise. *Literacy* is the average number of years of education. OLS Fixed Effects estimation is used in every specification. * is significant at the 10% level, ** is significant at the 5% level, *** is significant at the 1% level.

natural to assume that these shocks would downwardly bias the estimates presented so far. To account for this potential confounder, I use two measures of municipal agricultural dependence: the total extensions of land that are planted and harvested, previous to the disaster, as a proportion of the total area of the municipality.[25] If the income-shocks hypothesis is true, differential impacts of the disaster should be observed across municipalities that are more or less dependent on agriculture.

Table 6 reports the results of DID models that include these two measures. In this case, we are interested in the triple interactions between these measures of agricultural dependence, exposure to the disaster, and the post-disaster time dummy. Columns 1–4 show that there are no heterogeneous effects when we use the proportion of land planted, while columns 5–8 report an equivalent result if we instead use

[25] $Planted_i$ corresponds to the 2005 proportion of planted land. Similarly, $Harvested_i$ is the proportion of harvested land.

the proportion of harvested land. In any case, places that depend more on agriculture do not exhibit lower (or higher) effects of the disaster on reelection. Moreover, in general, the overall impact of this event remains unchanged even when we control for these measures.

6.6.3. Civil conflict dynamics

The final alternative mechanism that I explore in this study deals with civil conflict dynamics, which has been proven to affect electoral outcomes in Colombia. Illegal armed organizations, whether right-wing paramilitary groups or left-wing guerrilla movements, might respond strategically to allocations of aid, as has occurred in other contexts. Additionally, the disaster could represent a negative shock on state capacity, as government forces might get distracted from their security provision duties while they focus on the catastrophe. In any case, if illegal armed groups respond accordingly and their actions affect electoral results, violence might explain the association between disaster exposure, aid allocation, and reelection outcomes.

To test the role of conflict, Table 7 presents the results of models that incorporate the 2009 rates of FARC and paramilitary forces attacks,[26] the main active illegal armed groups in Colombia during the period of analysis for this study. Columns 1–4 show that the impacts of the disaster on reelection remain unchanged once we control for these attacks. Similar results are found if we use the other measures of exposure. section A6 in the Online Appendix shows that this effect also remains unchanged once we control for these measures both in OLS FE models with extreme rainfall used as the exposure measure, and in the IV specifications that use surface runoff as an instrument. Moreover, columns 5–8 show that these effects do not vary as a function of the level of exposure to these armed groups. Hence, it is not true that in places more affected by conflict the effect of the disaster on reelection is higher (or lower). Similar results are found in models using relief allocation (results available upon request): the effect of relief on reelection is robust to the inclusion of these conflict measures and no heterogeneous effects at this level are found.

7. Conclusion

I have shown that natural disasters might be beneficial for political survival. However, in contrast with other studies that find similar results (Healy and Malhotra, 2009; Cole et al., 2012), I focus on a mechanism that has not been fully explored in the literature: the clientelistic allocation of disaster aid. This study represents a contribution to the literature for at least three reasons. First, my results are robust to several specifications and tests, which show that the positive relationship between disaster exposure and reelection is quite strong. Second, tentative evidence suggests that places receiving more relief are more likely to reelect their incumbent parties, while the same is not true for reconstruction aid. This result represents a contribution to the literature in itself, since it shows that the distinction between preparedness and relief spending (Healy and Malhotra, 2009) is not the only one that matters.

Third, I show that traditional theories, which differ from the clientelism story, are not satisfactory explanations in this case. Using official evidence of the (in)efficient allocation of aid by local governments, measures of state capacity, media penetration, and education, I show that it is not necessarily true that in Colombia incumbent parties get reelected more in places more affected by the disaster, because in such places governments respond more and voters reward them accordingly. Moreover, economic shocks, caused by agricultural losses after the disaster, do not seem to play a mediating role in these effects. Finally, even though it is possible that illegal armed groups respond strategically to disaster aid allocation, these responses are not confounding with the effect of the catastrophe on reelection outcomes. In sum, I present evidence in favor of the clientelism mechanism and against other plausible explanations.

Naturally, the conclusions of this paper do not imply that aid should not be allocated after a disaster takes place because politicians will use it strategically to buy votes. The final message is that policies aimed to diminish clientelism will also reduce the incentives to strategically manipulate the utilization of disaster relief. In the meanwhile, alternative mechanisms can prevent this behavior if aid is managed and distributed by truly independent agencies and not by political organizations with clear electoral goals. In the context of climate change and global warming, in which extreme climatological events are expected to become more frequent, it is important to understand what the political consequences of natural disasters are and what can be done to ameliorate the pervasive consequences of these events.

[26] Results are similar if we use attacks during any other pre-treatment year.

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Appendix A. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.electstud.2018.08.001.

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