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**Oskar Nupia**
Universidad de Los Andes
**Andrés Álvarez**
ca.alvarez967@uniandes.edu.co
Universidad de Los Andes

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# The Effects of Multi-Grievance Mass Protests on Individual Attitudes

Oskar Nupia* and Andrés Álvarez**

Department of Economics - Universidad de Los Andes - Bogota

**Abstract**

Recent mass protests around the world share a common characteristic: they encompass multiple grievances. This complexity can make it challenging for the protesters' demands to effectively influence people's beliefs. We study the impact of the massive protests in 2019 in Colombia on individual attitudes closely related to the protesters' demands. A range of grievances drove these protests. Using a unique panel dataset, our findings reveal that protests only change individuals' beliefs regarding some core issues. Specifically, the protests only affect more immediate issues that receive substantial media attention (human-rights support), as well as political attitudes (party sympathy) that are typically influenced by protests. However, they do not affect attitudes related to long-standing structural issues highlighted by the protesters (income distribution and clientelism) nor do they influence structural political attitudes (appreciation for democracy and ideology).

**Keywords:** Protests, social movements, multi-grievance protests, political attitudes, political behavior, ideology, human rights, income distribution, clientelism.

**JEL Codes:** D72, D73, D74, D91, K38.

*Contact: onupia@uniandes.edu.co

**Contact: ca.alvarez967@uniandes.edu.co

# 1. Introduction

Mass protests have multiplied across different political and economic settings, from the Arab Spring to large-scale mobilizations in middle-income countries in Latin America, the United States, and Europe. A key characteristic of recent protests is widespread dissatisfaction with political failures, economic systems, and civil rights. As a result, these protests often address multiple grievances from different groups at the same time. In their analysis of 2,809 global protests that occurred between 2006 and 2020, Ortiz et al. (2022) found that most of these protests involved more than one of these grievances.

Classical theories on protest argue that protests aim to influence public opinion and shift the beliefs of citizens, extending beyond immediate effects on vote choice (Giugni, 1998; Kuran, 1989; Lohmann, 1994; Ellman and Wantchekon, 2000). Recent literature indicates that the impact of protest on individual attitudes may depend on various factors: whether the demonstrations are peaceful or violent (Ortiz et al., 2022; Setter and Nepstad, 2022; Lohmann, 1994), the type of information conveyed by the media (McLeod, 2007; Harlow et al., 2020; Proust and Saldaña, 2022), and the salience of the issues being addressed (Gethin and Pons, 2024).

Despite their significance and recent wide-spreading, the impact of multi-grievance mass protests on individual attitudes is still not well understood. In this context, at least two hypotheses may emerge. First, the variety of demands may lead individuals to struggle in identifying their specific issues, making it difficult for them to receive a clear signal that could alter their beliefs. Second, protests may only influence individual beliefs in certain domains. People might focus only on salient issues due to the context or media coverage. This study aims to enhance our understanding of how multi-grievance mass protests affect individual attitudes.

To do this, we analyze one of Colombia's most significant protest cycles in the twenty-first century: the *N21* movement (shorthand for November 21, 2019), the nationwide onset of demonstrations. N21 was a broad, multi-grievance mobilization with diffuse leadership. The central grievances echoed long-standing tensions in Colombia's social contract—dissatisfaction with political elites, human-rights violations, inequality, and corruption.

The protest began on November 21 and continued to intensify over the course of a month. Most events were peaceful, and national media coverage was extensive, plausibly amplifying exposure beyond protest localities. Using information from the same group of individuals, collected both in the years leading up to the protests and during the days surrounding them, we aim to answer two questions: To what extent did exposure to N21 shift individual attitudes? In which domains are these changes most likely to occur?

We focus on three outcome domains directly related to the grievances of the protesters: *political attitudes* (support for democracy, sympathy for political parties, and ideology), *human-rights–related attitudes* (approval of breaking the law for justice and own hand justice), and *political-economy attitudes* (preferences for redistribution and clientelism). Considering all these outcomes for the same group of respondents allows us to assess selective effects across domains that are rarely analyzed jointly.

Our empirical strategy exploits a quasi-experimental timing: the N21 protest coincided with the fieldwork calendar of a nationally representative survey that collected data between October and December 2019. Additionally, this survey allows us to use panel information from its earlier waves conducted in 2013 and 2016.

We prioritize temporal exogenous exposure to protests (surveys conducted before and after November 21, 2019) and complement it with spatial exposure to protests measures at the municipal level. Our baseline econometric strategy consists of regressing each attitude in 2019 against exposure to protests while controlling for their respective attitude in 2016 and relevant controls. We also use our panel data to explore alternative econometric approaches, such as Differences-in-Differences estimates and pertinent placebo tests. Additionally, we conduct several robustness checks.

Our findings indicate that the N21 protests only affect more immediate issues that receive substantial media attention (human-rights support), as well as political attitudes (party sympathy) that are typically influenced by protests. However, they do not affect attitudes related to long-standing structural issues highlighted by the protesters (income distribution and clientelism) nor do they influence structural political attitudes (appreciation for democracy and ideology).

Our study connects to a growing literature on the effects of protests. Studies of move-

ments with a clear single grievance document changes in opinion or behavior.[1] However, a large body of research shows that even in single-grievance protests, consequences depend on whether protests are perceived as peaceful or violent (Enos et al., 2019; Wasow, 2020; Wang and Wong, 2021; Ketchley and El-Rayyes, 2021; Caprettini et al., 2024; Joly et al., 2025).

Other studies use data from several protests and typically find effects on support for incumbents and vote shares. Gethin and Pons (2024) show that most of fourteen recent U.S. movements have very limited effects on public opinion and electoral outcomes. These studies do not simultaneously capture how such mobilizations shape different attitudes within the same group of individuals.

Unlike these studies, we focus on a single multi-grievance mass protest rather than examining protests with single grievances or multiple protests with specific demands. This enables us to examine changes in a variety of attitudes among the same group of individuals.

There is still little quantitative evidence on how protests affect attitudes towards human rights, redistributive preferences and clientelism. Some studies of protest waves following police violence focus mainly on attitudes towards political violence and policing (Setter and Nepstad, 2022). In a recent paper, Martorano et al. (2025) test whether exposure to protests against inequality changes citizens' beliefs, redistributive preferences, and political participation, combining panel survey data with a social-media information experiment. Finally, although corruption and clientelism are central motives behind many recent protests (Ortiz et al., 2022), we are not aware of causal studies of how protest exposure shapes attitudes towards clientelistic exchanges.

Hence, our study contributes along four fronts. First, we analyze a multi-grievance protest extending a literature heavily focused on single-issue movements. Second, we jointly study political, human-rights, and political-economy attitudes in the same panel of respondents,

[1]Protests focused on a single message show effects on individual preferences on: migration policies (Branton et al., 2015), climate change (Brehm and Gruhl, 2024), and political ideology (Madestam et al., 2013; Valentim, 2024). Other works link #MeToo and Black Lives Matter to changes in reporting of sexual assaults, racial attitudes and support for policing reforms (Levy and Mattsson, 2022; Mazumder, 2018; Dunivin et al., 2022).

enabling a unified view of selective movement across domains. Third, our econometric design uses quasi-experimental timing and a panel structure to strengthen causal interpretation and examine robustness. Finally, we explore new outcomes that have been overlooked in previous literature, such as attitudes toward redistribution and clientelism.

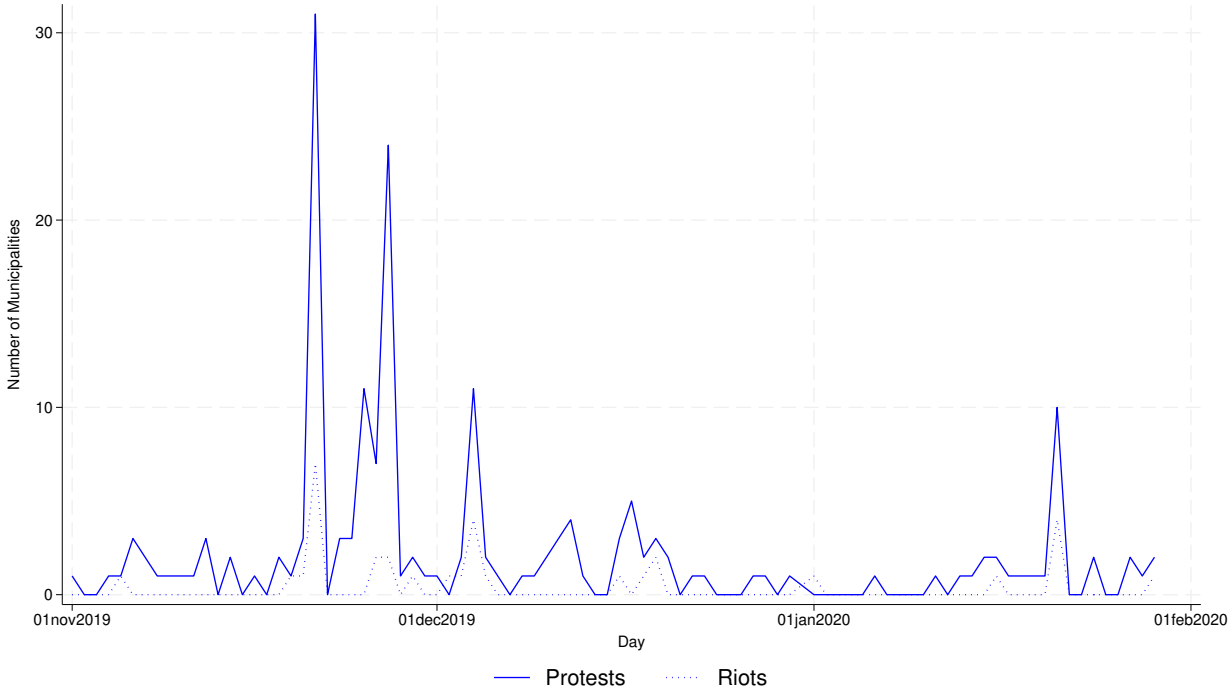
## 2. The 2019 Colombian protests

On November 21, 2019, one of the longest, largest, and most significant social protests in Colombia's recent history began: the so-called N21 movement. This wave of demonstrations occurred between November 21, 2019, and January 21, 2021. Labor unions called the first rally due to their disagreement with the national government's announcements about some labor reforms they considered to affect workers' rights. Due to the widespread social unrest in the country, various groups of university students, defenders of the peace agreement between the state and the Revolutionary Armed Forces of Colombia, anti-corruption activists, peasants, indigenous communities, and other social organizations joined this call, each one with their demands. University students and young people would become the main protagonists of the protests during the following days.

Figure 1 shows the daily number of municipalities with protests (peaceful demonstrations) and riots in Colombia between November 1st, 2019, and January 31st, 2020. The N21 movement had 154 peaceful protests and 29 riots and involved 66 municipalities in the country. This indicates that the protests were not limited to the country's main cities but also occurred in numerous medium and small towns. The demonstrations concentrated between November 21st and mid-December, with a pick on January 21st, 2020. On that day, protesters commemorated the start of the N21 movement. The number of protests each day was always more significant than the number of riots, the latter being less than a fifth of the former. Therefore, the N21 movement was mainly peaceful.

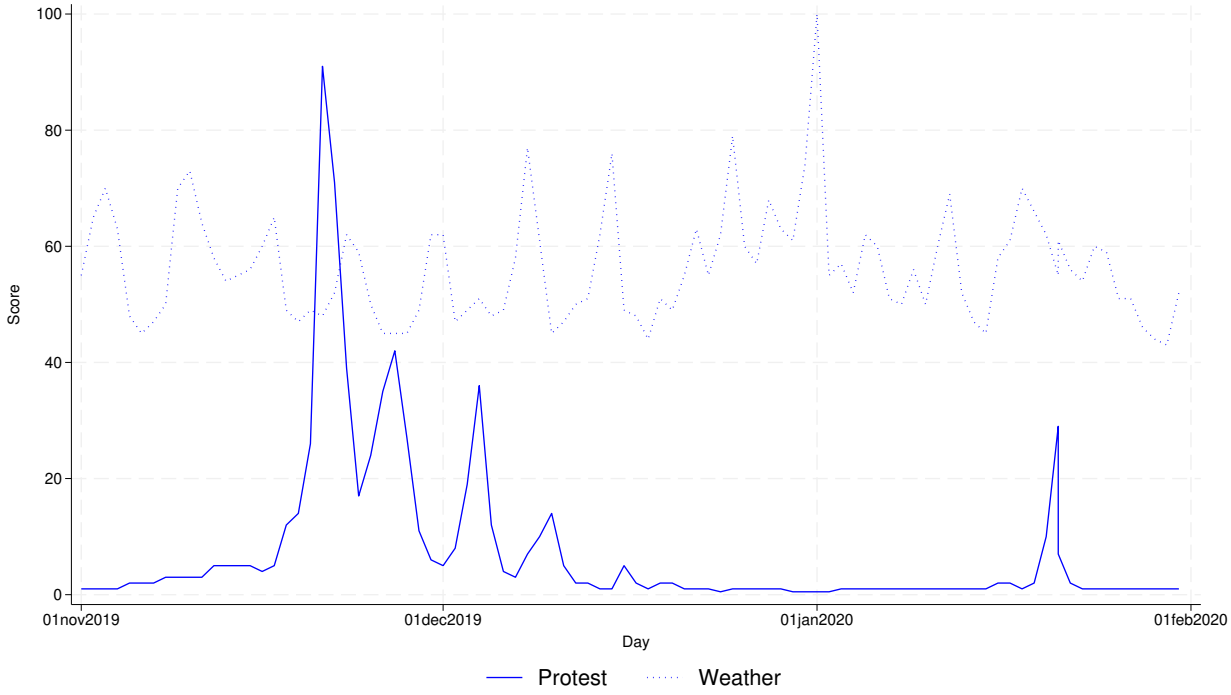
Due to the magnitude of the protests and the extensive coverage by both national and local media, demonstrations became the leading news story and focus of public interest at the national level. Figure 2 compares the search frequency of the term 'Paro' (the most common way to refer to protest in Colombia) with one of the most frequently searched words on

**Figure 1: Number of Municipalities with Protests and Riots (November 1st, 2019 - January 31st, 2020)**




Source: own computation with data from the Armed Conflict Location & Event Data Project (ACLED).

**Figure 2: Google Trends Search Score (Colombia): Protests and Weather (November 1st, 2019 - January 31st, 2020)**




Source: own computation with data from https://trends.google.com.co/.

Google from IP addresses in the country, the term 'Clima' (weather in Spanish)[2] between November 1st, 2019 and January 31st, 2020. 'Paro' maintains high search levels during the periods that coincide with the largest protests in Figure 1. Furthermore, the day the protest began, the term 'Paro' surpassed 'Clima' in search frequency. This national pattern is also evident when conducting the same analysis for each state in the country.

While the N21 movement brought together various groups with varying interests, its primary demands can still be clearly identified. Camacho-Muñoz et al. (2023) identify the ten principal demands of the most visible organizations participating in the N21 movement. Among these demands are: (1) complaints about high levels of corruption (shared by 7% of organizations); (2) dissatisfaction with the actions of the public force in matters of human rights (18% of organizations); (3) dissatisfaction with violence (14% of organizations); and, (4) dissatisfaction with tax collection, misallocation of these resources, and demands for higher government investment and redistribution (20% of organizations).[3] The individuals' attitudes we analyze here directly relate to these demands.

Although the media in the country extensively covered the N21 protests, there is evidence that traditional outlets focus more on certain issues. García-Perdomo et al. (2023) explore the coverage of the 2019 protests in Colombia and Chile by TV and newspapers and claim that traditional media and preeminent television news networks emphasized images depicting unrest and destruction during the protests. Additionally, the media addressed the disproportionate response of the police. The death of a student, resulting from police actions on the second day of the protests, became a major national news story.

# 3. Protests and attitudes

Protests are costly signals of information, conveying private insights from protesters to a large public, as highlighted by Giugni (1998). People may receive information about several issues from protests. For instance, the performance of rulers (Kuran, 1989), interests

[2]'Clima' is a term that, in Google Trends, is only surpassed by 'Colombia' when it comes to searches from computers located in the country.

[3]The lack of labor rights (18% of organizations) was also part of these main demands.

of protesters or interest groups (Lohmann, 1994), and topics relevant to opposition parties (Ellman and Wantchekon, 2000). All this information is likely to shape individuals' attitudes and preferences.

Nevertheless, the connection between protests and individuals' attitudes is not only mediated by the informational content of the claims but also by the public's perception of how these movements unfold. For instance, even if citizens resonate with the grievances articulated in the protests, witnessing associated violence can lead them to fear radicalization and avoid taking extreme positions (Lohmann, 1994; Setter and Nepstad, 2022). Non-violent actions in the U.S. civil rights movement era increased support for civil rights and the Democratic Party, whereas riots and violent episodes in the United States, Hong Kong and the Arab world often generated backlash, strengthened "law and order" positions and eroded trust in institutions (Enos et al., 2019; Wasow, 2020; Wang and Wong, 2021; Ketchley and El-Rayyes, 2021; Caprettini et al., 2024; Joly et al., 2025).

The source through which people received information on protests could also play a key role in how they update their beliefs. A large set of previous studies has shown that traditional and social media play a significant role in shaping public perception of protests, the latter often diverging from traditional media narratives (McLeod, 2007; Harlow et al., 2020; Proust and Saldaña, 2022).

Finally, recent literature has claimed that the spontaneous nature of the new wave of protests, along with the lack of visible leadership and organizational structure, raises doubts about their ability to influence people's beliefs. According to Gethin and Pons (2024), recent protests in the USA have had little impact on these beliefs, except regarding specific contextual issues that are particularly salient.

Using this framework, what can we expect regarding the impact of multi-grievance mass protests on individuals' attitudes? As previously mentioned, the N21 movement was characterized predominantly by peaceful demonstrations. One potential hypothesis is that the N21 protests may have influenced citizens' attitudes, making them more sympathetic to the movement's demands. Nevertheless, given the multi-grievance nature of this movement, should we take that expectation further?

We hypothesize that in a multi-grievance mass protest, it is unlikely that the demands

of protesters will significantly alter individual beliefs. Furthermore, if any change occurs, we anticipate that it will relate to more prominent issues—perhaps amplified by media coverage—that do not necessarily encompass all the long-standing structural problems highlighted by the protesters.

The first hypothesis arises from the fact that the variety of demands within a protest may cause individuals to struggle in identifying their specific issues. This can hinder their ability to receive a clear signal capable of shifting their beliefs. The second hypothesis stems from the first: if clear signals from protesters are difficult to discern, then salient issues, especially those emphasized by the media, may serve as intermediaries that help filter these demands, thereby influencing individuals' beliefs.

In applying these hypotheses to our variables of interest and the context of the N21 movement, we expect that it should have impacted individuals' attitudes toward human rights. As already said, Media coverage highlighted the disorder during the protests, particularly focusing on some violent incidents. However, in doing so, the media also showcased the police's reactions, which could increase individuals' repudiation of the human rights violations that the protesters were addressing.

Regarding the effect of the protests on political outcomes, we remain more agnostic. Some analysts in the country argue that the recent wave of protests (the N21 movement in 2019, and and its continuation in 2022) influenced the outcome of the 2022 presidential election, which resulted in the election of a left-wing candidate for the first time. While the movement was clearly associated with a center-left ideological stance, a characteristic emphasized by the media, we are unsure about how much the protests could influence deeply held political beliefs, such as support for democracy and ideology. However, protests have historically shown the potential to punish those in power, and it is possible that they have influenced sympathy towards political parties.

Lastly, while issues of corruption and distribution were significant demands of the protesters, their salience may have been overshadowed by other events. Therefore, we do not expect these beliefs to be significantly affected by the protests.

# 4. Data

The information for our analysis is taken from three different sources. The first one is the Longitudinal Colombian Survey (ELCO, for its acronym in Spanish), currently conducted by the Colombian National Statistic Service (DANE, for its acronym in Spanish). The first three waves of this survey (2010, 2013, and 2016), the former Longitudinal Colombian Survey (ELCA), were conducted by the University of Los Andes. The ELCO is a large-scale household panel survey unique in Colombia. Its baseline began in 2010 with 10,000 households representative of the urban and rural regions of the country. The ELCO's information has been widely used in prior studies, including those analyzing clientelism (Fergusson et al., 2022).

In 2013, a politics module was included in the ELCO. This module contains questions about attitudes toward different issues broadly related to politics. The module is usually administered to a single household member—the household head or his/her partner—with random assignment when both are available.[4] It should be noted that data collection for the 2013 and 2016 waves occurred during the first semester of the respective year, while in 2019, it occurred during the second semester. We have information on 4,878 individuals. However, due to our econometric design, which will be explained later, we mainly use data for 4,001 individuals in the same number of households.[5]

We consider three dimensions of individual attitudes closely related to the protests' demands: political, human rights, and political economy attitudes. The political attitudes dimension includes four variables. The first is based on a question that asks respondents

[4]In the 2019 survey, all individuals over 18 who were in the household during the module survey days were included. Since our econometric design takes advantage of the individuals' panel information, we focus on those surveyed during the three waves of the political module.

[5]In 2013, the politics module received responses from 8,329 individuals. Therefore, due to attrition, we lose 3,451 individuals, which represents 41.4% of the initial sample. In Table A.7 of the Appendix, we show that, while there are some differences in a few individual attitudes and socioeconomic characteristics between our sample and the initial sample, these differences are small.

to rate the importance of rulers being elected by popular vote. We recode the original responses into integer values from 0 to 3, where 0 means strongly disagree, and 3 means strongly agree.[6] We apply this same recode to all variables that will be introduced later and have the same coding scheme. The second variable captures whether individuals sympathize with a political party. The variable takes a value of 1 if the answer is Yes and 0 if it is No. The third variable is a dummy variable that takes a value of 1 if the individual reports an ideology on the left-right scale and 0 otherwise. We interpret that individuals who declare an ideology on this scale show more interest in political issues, regardless of whether they sympathize with a political party. Finally, we consider individuals' political ideology on the left-right scale. This variable takes integer values from 1 to 5, where 1 represents left, and 5 means right. This coding scheme was utilized in all ELCOs until 2016. In the 2019 survey, political ideology was rated on a scale of 1 to 10, with 1 indicating left and 10 indicating right. For this particular wave, we recoded values 1 and 2 as 1, values 3 and 4 as 2, and so on.

The human rights dimension includes two variables. The first variable is derived from a question asking respondents to assess the statement: "To capture criminals, it is acceptable for authorities to sometimes break the law." The second variable comes from a question asking respondents to rate the statement: "When criminals do not receive legally established punishments, it is justifiable for people to take justice into their own hands." Both variables are coded on a scale from 0 to 3, where 0 indicates strong disagreement, and 3 indicates strong agreement with the statement.

Finally, the political economy dimension considers two variables. The first one is related to the support for distributive policies. It measures the degree of agreement with the statement "Strong policies must be implemented to reduce inequality between rich and poor". The range of this variable is from 0 to 3, where 0 means strongly disagree and 3 means strongly agree. The second variable is clientelism. It takes a value of 1 if individuals have considered benefits, gifts, or jobs a candidate offers in exchange for their votes and 0 if they have not.

[6]The original variable in the survey ranges from 1 to 4, with 1 meaning strongly agree and 4 meaning strongly disagree.

It is important to note that any effect of the protests on clientelism must be properly interpreted. No elections were held in the country during the N21 movement protests. Thus, it is unlikely that individuals who had previously received private benefits in exchange for their vote would have rejected such an offer due to the protests. Instead, this effect, if any, must be interpreted as dismissing this type of electoral misconduct.

Table A.1 in the Appendix presents descriptive statistics for all individual attitudes described above for each of the three ELCO waves we utilized.

We also use the individuals' information reported in the ELCO, such as their socioeconomic characteristics and the sources of information they use to find out about the situation in the country. Table A.2 in the appendix displays these characteristics for the three waves of the ELCO's political module (2013, 2016, and 2019).

Half of the individuals live in urban areas, 60% of the individuals are women, and the average age is 53 years. The majority of individuals (57%) have incomplete or basic education and live in a household with low socioeconomic strata.[7] Television is the most frequently used source of information, with more than 80% of individuals using it to stay informed. Of the three primary sources of information (television, radio, and newspapers), 93% of individuals in 2019 stated that they had used at least one. Therefore, it is quite likely that individuals in our sample had received information not only on the protests happening in their regions but also in the cities where the most significant protests were concentrated.

The second source of information is the Armed Conflict Location & Event Data Project (ACLED). From this, we extract the daily number of municipalities with protests in Colombia between November 21, 2019, and January 21, 2020. This data lets us determine whether at least one protest occurred in each individual's state or municipality, and the exact date of each event during the N21 movement.

Finally, we use relevant municipal information for each individual. This is taken from the CEDE municipal data panel (the Center for Development Economics Studies, Universidad

[7]Strata is a classification system that assigns a value from 1 to 6 to each household based on their socioeconomic characteristics, where 1 represents the lowest conditions, and 6 represents the highest.

de Los Andes)[8]. We capture differences in development across municipalities using: the Multidimensional Poverty Index, the per capita tax revenues as a proxy of the municipal GDP, the rurality index (the percentage of the population living in rural areas), the linear distance to the state capital city, and the average score of all high schools in the municipality on national standardized tests. Finally, to measure exposure to violence affecting social leaders, the number of violent acts resulting in three or more deaths per year in each municipality is used. Statistics on these variables are also reported in Table A.2.

# 5. Methodology

To identify the effects of the protests on people's attitudes, we use individuals' exogenous *temporal exposure* to protests as our treatment measure in the baseline econometric model. As previously claimed, the N21 movement was a nation-wide significant event that received extensive media coverage. Additionally, our data indicates that individuals in our sample heavily rely on the media for information. Therefore, their temporal exogenous exposure to protests should be sufficient for identifying the causal effect of these events on attitudes. In section 7.3, we will revisit this definition of exposure to protests by incorporating not only *temporal exposure* but also geographical information about the demonstrations, i.e., *spatial exposure*.

Having information on individuals surveyed before and after the N21 movement's protests provides a precise exogenous temporal exposure to them. This enables us to identify the protests' causal effects on individuals' attitudes. The interview protocol for the survey was established several months before the protests began and was not affected by them. As evidence of this, only 14 surveys were conducted outside of the designated data collection period. This indicates that the interview protocol is uncorrelated to the occurrence of the protests and is an exogenous source of variation in the exposure to them. Additionally, surveys were conducted before and after the beginning of the protests in all the country's states, ensuring that regional data collection is also orthogonal to the protests.

Therefore, we compute the variable exposure to protest ($Exposure$) as follows: It takes

[8]Available online at: https://datoscede.uniandes.edu.co/ (accessed 15-05-2024).

the value of 1 if the survey of individual $i$ began on or after November 22nd (*temporal exposure to protest*); otherwise, it takes the value of 0 if the individual $i$ completed the survey on or before November 21.

Of the 4,878 panel respondents, 2,751 completed their interview on or before November 21 (non-exposed), 1,250 were first interviewed on or after November 22 (exposed), and 877 began before November 21 but finished on or after November 22. We exclude this last group from our baseline analysis because we cannot determine whether they were exposed to the protest while answering the political module; even if completion occurred after the onset, respondents may not yet have been aware of the event. In Section 7.3, we integrate these individuals to our analysis. The analysis sample therefore includes 4,001 individuals (2,751 non-exposed and 1,250 exposed). We assess the inclusion of the excluded group in the robustness section. Using this measure, 31% of individuals in our sample were exposed to protests.

Table 1 shows the mean of socioeconomic characteristics for the exposed and non-exposed groups as well as the p-value for the difference between these means. Some differences between the two groups, closely related to the survey protocol, are observed: before the protests, the survey was conducted more extensively in rural than urban areas. As a result, the exposed group has more people living in urban areas who tend to have higher levels of education, belong to higher economic strata, and are generally better informed than those in the non-exposed group. While there are statistical differences between the two groups regarding these variables, it is important to note that these differences are relatively small when considering the scale of each variable. Nonetheless, we will take into account these differences in our estimates and revisit the urban-rural balance.

For each of our outcomes of interest, we estimate the following model:

$$y_i^{2019} = \beta * Exposure_i + \alpha * y_i^{2016} + \gamma' * X_i + \epsilon_i \tag{1}$$

Where $y_i^t$ is an individual's attitudes in year $t$. $X_i$ is a vector that contains state dummy variables, individual socioeconomic variables (sex, age, a dummy for urban areas, education

**Table 1: Balance: Socioeconomic Characteristics and Sources of Information**

| | | Non-exposed to protests | Exposed to protests | Difference | p-value |
|---|---|---|---|---|---|
| Women | Mean | 0.60 | 0.61 | 0.01 | 0.51 |
| | SD | 0.49 | 0.49 | | |
| | Observations | 2,751 | 1,250 | | |
| Age | Mean | 53.68 | 52.93 | -0.75 | 0.06 |
| | SD | 11.57 | 11.82 | | |
| | Observations | 2,751 | 1,250 | | |
| Urban | Mean | 0.46 | 0.62 | 0.16 | 0.00 |
| | SD | 0.50 | 0.49 | | |
| | Observations | 2,751 | 1,250 | | |
| Education level | Mean | 1.65 | 1.82 | 0.17 | 0.00 |
| | SD | 1.37 | 1.50 | | |
| | Observations | 2,711 | 1,232 | | |
| Strata | Mean | 1.66 | 1.77 | 0.11 | 0.00 |
| | SD | 0.77 | 0.82 | | |
| | Observations | 2,694 | 1,222 | | |
| TV | Mean | 0.88 | 0.90 | 0.03 | 0.02 |
| | SD | 0.33 | 0.30 | | |
| | Observations | 2,751 | 1,250 | | |
| Newspapers | Mean | 0.12 | 0.14 | 0.02 | 0.05 |
| | SD | 0.32 | 0.35 | | |
| | Observations | 2,751 | 1,250 | | |
| Radio | Mean | 0.45 | 0.45 | 0.00 | 0.92 |
| | SD | 0.50 | 0.50 | | |
| | Observations | 2,751 | 1,250 | | |
| Internet | Mean | 0.10 | 0.15 | 0.05 | 0.00 |
| | SD | 0.30 | 0.36 | | |
| | Observations | 2,751 | 1,250 | | |
| Internet social networks | Mean | 0.17 | 0.27 | 0.10 | 0.00 |
| | SD | 0.38 | 0.44 | | |
| | Observations | 2,751 | 1,250 | | |

Education level takes on the following values: 0 for individuals with no education or preschool education, 1 for basic education, 2 for middle or secondary education, 3 for incomplete technical education, 4 for completed technical education, 5 for incomplete university education, 6 for completed university education, 7 for incomplete graduate education, and 8 for completed graduate education. The number of observations for the Education level is 1,074 for the exposed group and 2,869 for the non-exposed group. The number of observations for Strata is 1,071 for the exposed group and 2,845 for the non-exposed group.

level, and strata),[9] a dummy variable for each source of information used by individuals (TV, newspapers, radio, internet and social networks), and municipality characteristics where individuals reside for 2019 (per-capita GDP proxy, poverty index, rurality index, distance to the state capital, average score of students in the national education test, homicide rate, and number of massacres). Finally, the error term is $\epsilon_i$.

In Equation 1, the parameter $\beta$ captures the effect of the exposure to protests on the respective individual's attitude. Introducing the attitude of individuals in 2016 ($y_i^{2016}$) as a control in this equation allows us to avoid potential problems with relevant omitted variables and to consider previous individuals' conditions. Controlling for individual characteristics is pertinent because, as Table 1 shows, the balance of these traits was affected by the earlier implementation of the survey in rural areas. Not accounting for this might cause biases in the estimated effect of protests on individuals' attitudes. Finally, controlling for both state effects and aggregate municipal-level measures enables us to consider relevant contextual factors influencing individuals' attitudes.

To facilitate the presentation of results, we report Least Square (LS) estimates of Equation 1. We also estimate this equation using logit, probit, ordered logit and ordered probit estimates based on the nature of the dependent variables. The results obtained from these estimations are qualitatively the same as the ones reported here (these results are available upon request).

Equation 1 is estimated using standard errors clustered at the municipal level to account for possible correlations between errors and local characteristics. It is important to note that whenever we find statistical significance of an estimated $\beta$ using this clustering, we obtain the same results using non-robust errors, robust errors, and errors clustered at the department level (these results are also available upon request).

# 6. Baseline Results

Figure 1 displays the average responses of individuals towards each attitude in 2016 and 2019 for individuals who were exposed and not exposed to the 2019 protests. Since these

[9]With strata and education measured as indicated in the notes at the bottom of Table 1.

averages do not account for the differences in the observable characteristics of both groups of individuals, the differences observed in Figure 1 must be considered as a preliminary result.

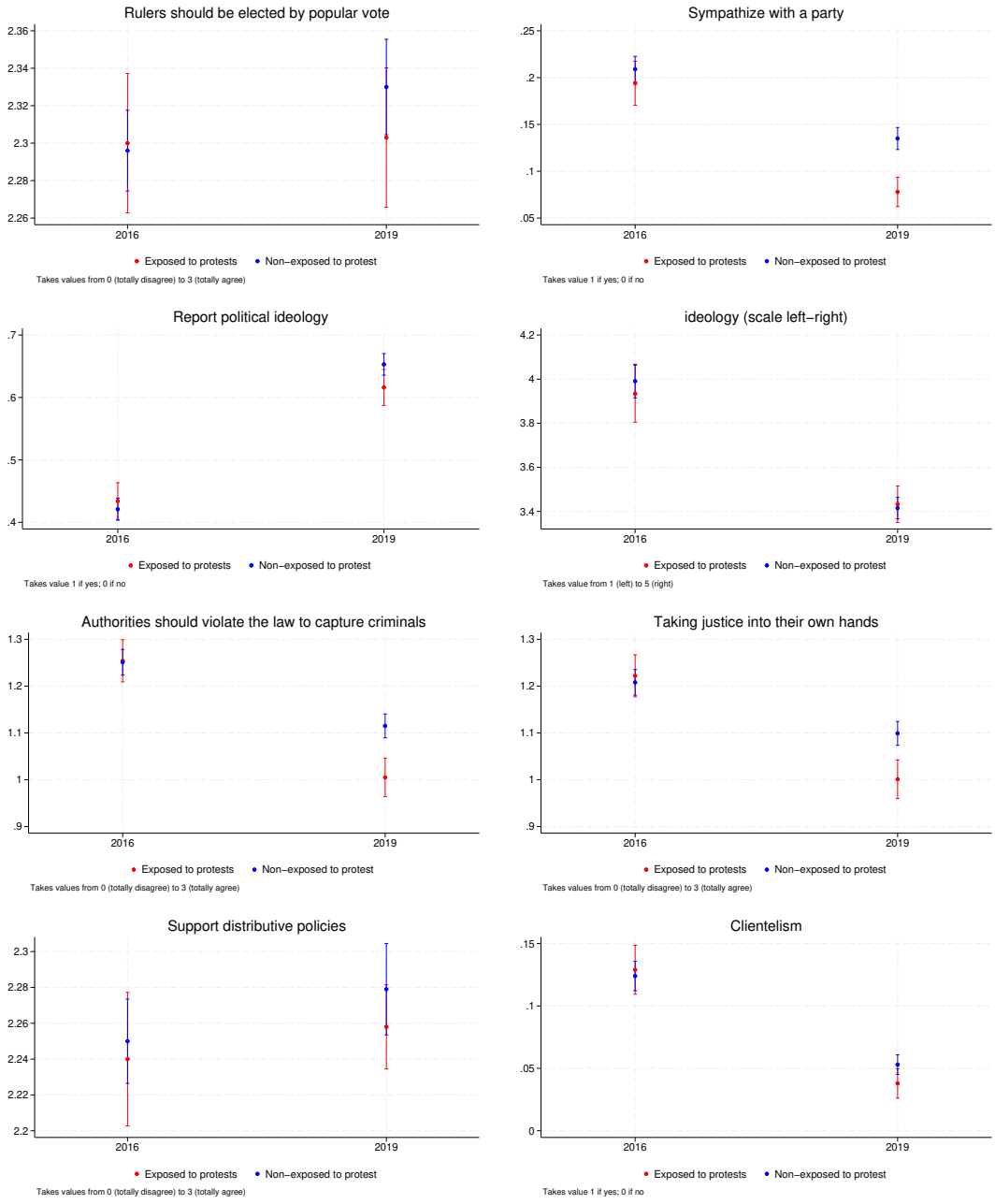
In 2016, no differences were observed between the two groups regarding attitude. However, in 2019, differences were observed regarding sympathy towards a political party, reporting political ideology and human rights attitudes. In the rest of the section, we test for these preliminary findings by estimating Equation 1.

The effects of protests on individuals' attitudes are reported in Tables 1 to 3. The odd-numbered columns report these effects using the respective attitude in 2016 as the only control, while the even-numbered columns report these by adding all controls in $X$. We do so to show the impact of controlling for individual characteristics on estimating these effects. As will be seen, in general, including these controls does not qualitatively affect the results except for one case (see Table 1, column 5 and 6). Our analysis below focuses on the parameters reported in the even-numbered columns.

Table 1 reports the effects of protests on political attitudes. The main finding is that while protests did not affect individuals' appreciation for using the popular vote to elect rulers or their ideological position, they did cause disenchantment with political parties and the probability of reporting political ideology. Between 2016 and 2019, both groups' probability of sympathizing with a political party decreases (see Figure 1). Nevertheless, individuals exposed to protests exhibited a further decrease in their sympathy than those not. Being exposed to protests negatively affects the probability of sympathizing with a political party by almost 7 percentage points, representing 0.21 standard deviations of this measure.

Figure 1 indicates a general increase in the probability of individuals reporting a political ideology between 2016 and 2019, regardless of whether they were exposed to protests. However, according to the estimates in Table 1, the increment was lower for individuals exposed to the demonstrations. The protests negatively affected this probability by 8 percentage points, equivalent to 0.17 standard deviations of this measure. It is worth noting that the shift between 2016 and 2019 in the ideological position of individuals towards the center of the political spectrum observed in Figure 1 cannot be attributed to the protests. Thus, rather than impacting ideology, protests discourage people from identifying with a particular position in this spectrum.

## Figure 1: Average Individual Attitudes: Exposed and Non-exposed to Protests




95% confidence intervals.

**Table 1: Effect of Exposure to Protest on Political Attitudes**

| | Rulers elected by popular vote | | Sympathize with a party | | Report political ideology | | Ideology (scale left-right) | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| *Exposure* | -0.032 | -0.055 | -0.061*** | -0.068*** | -0.054 | -0.083*** | -0.049 | 0.083 |
| | (0.038) | (0.035) | (0.017) | (0.016) | (0.031) | (0.022) | (0.100) | (0.089) |
| Attitude in 2016 | 0.016 | 0.012 | 0.162*** | 0.131*** | 0.095*** | 0.056*** | 0.162*** | 0.155*** |
| | (0.020) | (0.017) | (0.026) | (0.022) | (0.020) | (0.017) | (0.027) | (0.023) |
| Controls ($X$) | NO | YES | NO | YES | NO | YES | NO | YES |
| Observations | 4,001 | 3,859 | 3,865 | 3,727 | 4,001 | 3,859 | 1,186 | 1,149 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. Controls ($X$): state dummy variables, sex, age, a dummy for urban areas, education level, strata, source of information (TV, newspapers, radio, internet and social networks), per-capita GDP proxy, poverty index, rurality index, distance to the state capital, average score of students in the national education test, homicide rate, and number of massacres.

The estimated effects of protests on human rights attitudes are reported in Table 2. Appreciation for human rights increased between 2016 and 2019 for individuals exposed and not exposed to protests (see Figure 1)—that is, the approval for the two anti-human rights behaviors considered here decreased. However, as hypothesized, the extent of this increase was not the same among both groups. The protests negatively affected the appreciation for breaking the law to capture criminals (by 0.15 points, representing 0.21 standard deviations of this measure) and taking justice into own hands (by 0.12 points, representing 0.16 standard deviations of this measure).

Finally, the estimated effects of protests on political economy attitudes are reported in Table 3. Findings indicate that the protests did not influence support for distributive policies or clientelism. Nonetheless, both issues are long-run structural problems in Colombia and in many other developing countries, and were among the salient grievances of the N21 movement.

In sum, the baseline results indicate that the protests did not diminish faith in democracy or the electoral process, did not influence individuals' political ideologies, and did not impact attitudes toward redistribution and clientelism. Instead, they resulted in disillusionment with political parties and a reluctance to identify with any particular ideology. Additionally, the protests positively affected appreciation for human rights. These results show that protests effectively conveyed the general dissatisfaction with the political class and some of their

**Table 2: Effect of Exposure to Protest on Human Rights Attitudes**

| | Breaking the law to capture criminals | | Taking justice into own hands | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Exposure* | -0.105*** | -0.147*** | -0.081** | -0.115*** |
| | (0.037) | (0.033) | (0.040) | (0.041) |
| Attitude in 2016 | 0.063*** | 0.065*** | 0.112*** | 0.111*** |
| | (0.017) | (0.016) | (0.020) | (0.018) |
| Controls ($X$) | NO | YES | NO | YES |
| Observations | 4,001 | 3,859 | 4,001 | 3,859 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. Controls ($X$): state dummy variables, sex, age, a dummy for urban areas, education level, strata, source of information (TV, newspapers, radio, internet and social networks), per-capita GDP proxy, poverty index, rurality index, distance to the state capital, average score of students in the national education test, homicide rate, and number of massacres.

**Table 3: Effect of Exposure to Protest on Political Economy Attitudes**

| | Support distributive policies | | Clientelism | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Exposure* | -0.026 | -0.038 | -0.015 | -0.008 |
| | (0.038) | (0.030) | (0.012) | (0.005) |
| Attitude in 2016 | 0.056*** | 0.043*** | 0.069*** | 0.040*** |
| | (0.018) | (0.016) | (0.014) | (0.013) |
| Controls ($X$) | NO | YES | NO | YES |
| Observations | 4,001 | 3,859 | 3,846 | 3,709 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. Controls ($X$): state dummy variables, sex, age, a dummy for urban areas, education level, strata, source of information (TV, newspapers, radio, internet and social networks), per-capita GDP proxy, poverty index, rurality index, distance to the state capital, average score of students in the national education test, homicide rate, and number of massacres.

demands to the public and managed to affect people's beliefs. However, they did not affect the individuals' beliefs towards "structural" issues (like democracy and ideology) or problems (like distribution and clientelism).

# 7. Robustness checks

In this section, we conduct four robustness tests to validate our baseline results. These tests include: a placebo test, a Difference-in-Difference (DID) estimate that utilizes our panel data structure in a different manner, alternative measures of exposure to protests based on individuals' spatial proximity, and an analysis of whether the estimated effects of protests on individuals' attitudes are influenced by other significant events, particularly the regional elections of October 2019.

The main conclusion obtained from this analysis is that the baseline estimates reported in section 6 are generally robust. However, we find evidence contradicting the negative effect of protest on the probability of reporting a political ideology, along with some non-robust evidence suggesting that protests may have a negative impact on clientelism.

## 7.1. 2016 as a placebo test

Our panel data structure enables us to conduct a placebo test to examine whether the 2019 protests affected individuals' attitudes in 2016. If no effect is found, it validates that the reported effects of protest on individuals' attitudes in section 6 are not due to pre-existing differences between individuals exposed and not exposed to protests—as already suggested by Figure 1.

We estimate Equation 1 by using $y_i^{2016}$ as the dependent variable and $Exposure$, $y_i^{2013}$, along with controls for 2016, as independent variables. We then test whether $Exposure$ has a statistically significant effect on individuals' attitudes. Table 1 reports these results. As expected, we found no effect of $Exposure$ on individuals' attitudes in 2016.[10]

[10]Another placebo test would be conducted by running $y_i^{2016}$ against $Exposure$, but defined with survey dates in 2016. However, as mentioned earlier, the 2016 survey data collection did not occur during the same months as the 2019 survey.

**Table 1: Effect of 2019 Exposure to Protest on 2016 Individual Attitudes**

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.001 | -0.004 | -0.001 | 0.081 | 0.018 | 0.033 | -0.022 | 0.004 |
| | (0.022) | (0.016) | (0.017) | (0.107) | (0.029) | (0.027) | (0.020) | (0.012) |
| Attitude in 2013 | 0.051** | 0.247*** | 0.162*** | 0.371*** | 0.159*** | 0.207*** | 0.068*** | 0.111*** |
| | (0.022) | (0.025) | (0.016) | (0.041) | (0.015) | (0.019) | (0.018) | (0.024) |
| Controls ($X$) | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 3,988 | 3,912 | 3,988 | 914 | 3,988 | 3,988 | 3,988 | 2,421 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$.

## 7.2. Differences-in-Differences

An alternative method to exploit our panel dataset structure is implementing a Differences-in-Differences estimation (DID). This approach helps us consider unobserved individual characteristics that remain constant over time and could generate potential omitted variable biases. Furthermore, it allows us to verify the existence of parallel trends in individuals' attitudes between 2013 and 2016.

Using information for the three waves of the ELCO's political module and defining $Post_t$ as a dummy variable that takes a value of 0 in 2013 and 2016 and 1 in 2019, we estimate the following Equation:

$$y_{it} = \beta * (Exposure_i * Post_t) + \alpha_i + \gamma' * X_{it} + \lambda_t + \epsilon_{it} \tag{2}$$

where $\alpha_i$ stands for individual fixed-effects, $\lambda_t$ for time effects, and $\epsilon_{it}$ is the error term. Parameter $\beta$ captures the average treatment effect of the treated (ATT). Since fixed-effects already account for unobserved characteristics, Equation 2 excludes the lag on the respective attitude from the control variables. Furthermore, it allows us to use the complete time dimension information and avoid well-known endogeneity problems when lags are included in these estimates.

Table 2 reports the findings from estimating Equation 2. First of all, we cannot reject the parallel trend hypothesis for all attitudes, except for the case of support for distributive policies. These parallel trend tests are presented in Figure A.1 and Table A.3 in the

**Table 2: DID: Effect of Exposure to Protest on Individuals' Attitudes**

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| $Exposure * Post$ | -0.058 | -0.050** | -0.047 | 0.069 | -0.108** | -0.098 | -0.051 | -0.014 |
| | (0.040) | (0.020) | (0.030) | (0.125) | (0.050) | (0.055) | (0.048) | (0.013) |
| Controls ($X$) | YES | YES | YES | YES | YES | YES | YES | YES |
| Parallel trends | YES | YES | YES | YES | YES | YES | NO | YES |
| Observations | 11,912 | 11,736 | 11,912 | 5,837 | 11,912 | 11,912 | 11,912 | 10,251 |
| Individuals | 3,995 | 3,995 | 3,995 | 3,321 | 3,995 | 3,995 | 3,995 | 3,993 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$.

Appendix.

Second, the effect of protests on individual attitudes is largely consistent with the findings presented in Tables 1 to 3, except for two cases. Firstly, the effect of the protests on the probability of reporting a political ideology is not statistically significant in this analysis. Secondly, although the effect of protests on taking justice into own hands is not statistically significant at the traditional 95% level, it remains statistically different from zero with a 90% probability.

## 7.3. Other measures of exposure to protests

In this section, we explore two alternative ways for measuring exposure to protests. The first measure considers temporal and spatial exposure to protests at the municipal level. We compute the variable $Exposure * Mun$ as follows. It takes the value of 1 if the survey of the individual $i$ began on or after November 22nd (*temporal exposure*), and there was at least one protest in the individual's municipality before the end of his/her survey (*spatial exposure*). Otherwise, it takes the value of 0 if the survey of individual $i$ was completed on or before November 21, or it began on or after November 22nd and there was no protest in the individual's municipality before the end of his/her survey.

Out of the 66 municipalities where protests occurred in the country, only 32 have information from people surveyed by the ELCO. This considerably reduces the percentage of exposed individuals to protests from 31.2% to 12.5%.

The results of this exercise are presented in Table 3 using the complete set of controls.

**Table 3: Effect of Temporal and Municipal Spatial Exposure to Protest on Individuals' Attitudes**

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| $Exposure * Mun$ | -0.006 | -0.072*** | -0.076*** | 0.018 | -0.250*** | -0.181*** | -0.045 | -0.019** |
| | (0.066) | (0.023) | (0.029) | (0.138) | (0.038) | (0.059) | (0.043) | (0.008) |
| Attitude in 2016 | 0.012 | 0.130*** | 0.056*** | 0.154*** | 0.065*** | 0.111*** | 0.044*** | 0.040*** |
| | (0.017) | (0.022) | (0.017) | (0.023) | (0.017) | (0.018) | (0.016) | (0.012) |
| Control ($X$) | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 3,859 | 3,727 | 3,859 | 1,149 | 3,859 | 3,859 | 3,859 | 3,709 |

Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$.

Except for clientelism, which is now statistically significant, the estimates for all attitudes are qualitatively similar to those found in our baseline results. Since the coefficients for clientelism return to being statistically insignificant when we use these definitions of exposure in the DID regression (these results are not reported here but available upon request), we approach this finding with caution due to its lack of robustness. These findings support our assertion that the protests were a national event, impacting individuals regardless of their geographical proximity to the demonstrations.

The second variable of exposure considers only the finalization date of the survey. We define $Exposure2$ as follows: it takes the value of 1 if the survey of the individual $i$ was completed on or after November 22nd; otherwise, it takes the value of 0. As mentioned earlier, one issue with this measure is that it considers as treated individuals who likely were not exposed to the protests. This could be because they completed the political module before the protests began, or even if they completed the module after the protests started, they may not have been fully aware of them.

Using this definition, we can include information from the 4,878 individuals in our panel. However, due to missing observations for some control variables, the estimates reported below are based on a maximum of 4,688 individuals. According to this definition, 43% of individuals were exposed to the protest.

The results using this definition of exposure are reported in Table 4. Once again, these results are qualitatively similar to our baseline findings. However, as expected from our

**Table 4: Effect of *Exposure*2 to Protest on Individuals' Attitudes**

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| *Exposure*2 | -0.009 | -0.040*** | -0.071*** | 0.142 | -0.088*** | -0.078** | 0.012 | -0.002 |
| | (0.027) | (0.013) | (0.014) | (0.074) | (0.031) | (0.037) | (0.025) | (0.005) |
| Attitude in 2016 | 0.005 | 0.131*** | 0.058*** | 0.152*** | 0.061*** | 0.109*** | 0.047*** | 0.040*** |
| | (0.015) | (0.021) | (0.016) | (0.021) | (0.017) | (0.018) | (0.015) | (0.010) |
| All Controls | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 4,688 | 4,511 | 4,688 | 1,372 | 4,688 | 4,688 | 4,688 | 4,483 |

Standard errors clustered at the municipal level. *** $p < 0.01$, ** $p < 0.05$.

discussion regarding the appropriateness of this measure, all point estimates are smaller than those in the baseline.

## 7.4. Other significant events

During the 2019 ELCO data collection, in addition to the protests, another significant national event was the regional elections (governors, mayors, and regional councils) held on October 27 of the same year. These elections could have influenced most of the individual attitudes we are studying, particularly the political attitudes and the attitude toward clientelism. People may have paid more attention to factors such as ideology and partisan sympathies and heard more about vote-buying offers during that period. If it happened, the baseline effects of protests on these attitudes reported in section 6 could be contaminated by the political campaign during these elections, potentially leading to up-biased results.

To address this issue, we estimate Equation 1 by excluding all individuals surveyed before the regional elections, i.e., individuals surveyed before October 28. This reduces the number of observations by 33%, resulting in an increase in the percentage of individuals exposed to protests to protests (42.2%).

The results are reported in Table 5. They indicate that most of our baseline results are robust to this exclusion of information. However, the point estimates are now smaller than the baseline results, and the statistical significance of taking justice into own hands disappears. Despite the later, our other measure of appreciation for human rights still

**Table 5: Effect of Exposure to Protest on Individual Attitudes Excluding Information Before 2019 Regional Elections**

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.061 | -0.039** | -0.050** | 0.127 | -0.105*** | -0.056 | -0.034 | -0.005 |
| | (0.043) | (0.016) | (0.023) | (0.104) | (0.029) | (0.037) | (0.033) | (0.006) |
| Attitude in 2016 | 0.012 | 0.104*** | 0.039* | 0.122*** | 0.049** | 0.120*** | 0.015 | 0.050*** |
| | (0.023) | (0.021) | (0.022) | (0.035) | (0.019) | (0.021) | (0.017) | (0.013) |
| Control ($X$) | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 2,586 | 2,504 | 2,586 | 712 | 2,586 | 2,586 | 2,586 | 2,505 |

Standard errors clustered at the individual level. *** $p < 0.01$, ** $p < 0.05$.

demonstrates strong statistical significance.

After conducting the previous robustness checks, we conclude that protest has a robust negative effect on the probability of sympathizing with a political party and a robust positive effect on people's support for human rights. Furthermore, protests have a robust no impact on people's appreciation for popular elections, ideology, and redistributive policies. Finally, there is less consistent evidence that protests negatively affect the probability of reporting a political ideology and accepting clientelistic offers.

# 8. Heterogeneous effects

People with different traits might adjust their initial beliefs differently after receiving the same information. Furthermore, individuals may receive varying information based on the sources they use. As mentioned in section 3, these sources can filter details about the protesters' demands or highlight certain issues more prominently. In this section, we explore whether people's characteristics and the sources of the information they receive influence the impact of protests on individual attitudes.

To do this, we estimate Equation 1 for each individual attitude by adding the interaction between *Exposure* and each of the following variables $x_i$={women, age, education, strata, TV, newspapers, radio, internet, internet social networks}. The complete results of these estimates are reported in Tables A.4, A.5, and A.6 in the Appendix. In this section, we focus on attitudes that exhibit a robust impact from protests, specifically the probability of

**Table 1: Effect of Exposure to Protests and Heterogeneous Effects (Main Results)**

| | Sympathize with a party | | Breaking the law to capture criminals | | Taking justice into own hands |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| *Exposure* | 0.025 | -0.101*** | 0.012 | -0.117*** | -0.088** |
| | (0.049) | (0.026) | (0.076) | (0.030) | (0.037) |
| *Exposure**Age | -0.002** | | | | |
| | (0.001) | | | | |
| *Exposure**Education | | 0.019** | | | |
| | | (0.009) | | | |
| *Exposure**TV | | | -0.177** | | |
| | | | (0.081) | | |
| *Exposure**Newspaper | | | | -0.224** | -0.206** |
| | | | | (0.099) | (0.093) |
| Control ($X$) | YES | YES | YES | YES | YES |
| Observations | 3,727 | 3,727 | 3,859 | 3,859 | 3,859 |

Standard errors clustered at the individual level. *** $p < 0.01$, ** $p < 0.05$.

sympathizing with a political party and the appreciation of human rights. The main findings regarding these attitudes are presented in Table 1.

The effect of protests on the probability of sympathizing with a political party is partially but not totally driven by two characteristics: age and education. While the effect of protests on this attitude is consistently negative and significant across all age groups in our sample, it is more pronounced among older individuals. Conversely, the negative impact of protest on this probability diminishes as individuals' education increases, eventually becoming negligible among very highly educated individuals (remember that our education variable runs from 0 to 8). In sum, this effect is predominantly driven by older and less educated individuals, although it remains negative and significant across nearly all groups.

The effect of protest on attitudes toward human rights is mainly driven by the use of TV and radio as sources of information. The strongest evidence for this is that being exposed to the protests and using TV as a source of information accounts for the entire effect of protests on approval for breaking the law in order to capture criminals.

As mentioned earlier, national media coverage emphasized isolated violent episodes and forceful police responses. On the second day, a student was struck by a police-fired riot-control projectile and died two days later, prompting widespread condemnation. This near-contemporaneous incident complicates the interpretation of our findings. The shifts in atti-

tudes toward human rights could indicate either a rejection of broader human rights violations denounced by the protesters, a disapproval of police conduct during the protests, or a combination of both. Because the incident occurred essentially at the onset of the protest cycle, our data lack the statistical power and temporal separation needed to isolate its effect from the overall mobilization. Nevertheless, our evidence suggests that the salience of this issue during the protests likely influenced changes in attitudes toward human rights.

# 9. Conclusions

This paper contributes to understanding how massive multi-grievance protests transmit information and selectively shape public opinion. By exploiting panel data and quasi-experimental exposure, and by examining heterogeneous information environments, we show that protest effects arise in specific attitudinal domains rather than producing broad ideological shifts. Our analysis shows that the influence of the protests was concentrated rather than broad. Exposure weakened attachment to political parties and reduced tolerance for practices that contravene human-rights principles. These shifts stand in contrast with the stability observed in views about democracy, ideological self-placement, and redistribution. Attitudes toward clientelistic practices also exhibit some negative associations with exposure, although these patterns are not consistently identified across specifications that rely on alternative exposure measures.

Our evidence suggests that large, heterogeneous protest cycles do not uniformly alter beliefs associated with longstanding structural concerns. Instead, changes emerge in domains that became highly visible during the mobilization period—particularly those amplified by media coverage—while other dimensions of political and distributive attitudes remain largely unaffected.

We likewise do not find systematic adjustments in individuals' ideological positioning or their valuation of electoral procedures. This stability implies that, despite generating pronounced reactions in some areas, the protests did not translate into broader shifts in democratic commitments or heightened ideological polarization.

From a broader point of view, our paper illustrates how protest episodes can transmit

information unevenly, with discernible consequences for specific attitudinal outcomes. Our contribution highlights the importance of studying how some salient pieces of information linked to protests, but not necessary related with the main grievances of the protesters, can reshape public opinion.

# Author contributions

- Oskar Nupia: Conceptualization (equal); Methodology (lead); Formal analysis (lead); Investigation (equal); Writing – original draft (equal); Writing – review and editing (equal).
- Andrés Alvarez: Conceptualization (equal); Methodology (supporting); Formal analysis (supporting); Investigation (equal); Writing – original draft (equal); Writing – review and editing (equal).

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# A. Online Appendix

**Table A.1: Statistics: Political, Human Rights and political economy attitudes**

| | | 2013 | 2016 | 2019 |
|---|---|---|---|---|
| | | Political Attitudes | | |
| Rulers should be elected by popular vote | Mean | 2.33 | 2.30 | 2.32 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.59 | 0.60 | 0.66 |
| | Observations | 4,001 | 4,001 | 4,001 |
| Sympathize with a party | Mean | 0.18 | 0.21 | 0.12 |
| 0 no, 1 yes | SD | 0.38 | 0.40 | 0.33 |
| | Observations | 3,962 | 3,963 | 3,901 |
| Report political ideology | Mean | 0.41 | 0.43 | 0.64 |
| 0 no, 1 yes | SD | 0.49 | 0.49 | 0.48 |
| | Observations | 4,001 | 4,001 | 4,001 |
| Ideology (scale left-right) | Mean | 4.07 | 3.98 | 3.42 |
| From 0 (left) to 5 (right) | SD | 1.39 | 1.40 | 1.08 |
| | Observations | 1,623 | 1,700 | 2,573 |
| | | Human Rights Attitudes | | |
| Breaking the law to capture criminals | Mean | 1.25 | 1.25 | 1.09 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.77 | 0.75 | 0.68 |
| Taking justice into own hands. | Mean | 1.18 | 1.21 | 1.07 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.76 | 0.76 | 0.70 |
| | Observations | 4,001 | 4,001 | 4,001 |
| | | political economy attitudes | | |
| Support distributive policies | Mean | 2.32 | 2.25 | 2.27 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.62 | 0.64 | 0.68 |
| | Observations | 4,001 | 4,001 | 4,001 |
| Clientelism | Mean | 0.18 | 0.13 | 0.05 |
| 0 no, 1 yes | SD | 0.38 | 0.33 | 0.22 |
| | Observations | 2,488 | 3,902 | 3,942 |

## Table A.2: Socioeconomic Characteristics

| Variable | Statistic | 2013 | 2016 | 2019 |
|---|---|---|---|---|
| | Characteristics of Individuals | | | |
| Urban | Mean | 0.48 | 0.50 | 0.51 |
| (1 urban; 0 rural) | SD | 0.50 | 0.50 | 0.50 |
| Female | Mean | | | 0.60 |
| (1 female; 0 male ) | SD | | | 0.49 |
| Age | Mean | 46.97 | 49.98 | 53.45 |
| (Years) | SD | 11.61 | 11.62 | 11.65 |
| Participate in protests | Mean | NA | NA | 0.03 |
| (0 no; 1 yes) | SD | | | 0.16 |
| | Observations | 4,001 | 4,001 | 4,001 |
| Education level | None or preschool | 7.1% | 6.8% | 7.6% |
| | Basic | 50.2% | 49.6% | 49.6% |
| | Secondary or middle | 30.4% | 30.3% | 31.1% |
| | Technical or university (Education levels 3 to 8) | 12.3% | 13.3% | 11.6% |
| | Observations | 3,981 | 3,988 | 3,943 |
| Strata | 1 | NA | NA | 48.3% |
| | 2 | NA | NA | 36.8% |
| | 3 | NA | NA | 12.8% |
| | 4 and 5 | NA | NA | 2.2% |
| | Observations | | | 3,916 |
| | Individuals' Source of information | | | |
| Tv | Mean | 0.83 | 0.86 | 0.89 |
| (1 yes; 0 no) | SD | 0.38 | 0.35 | 0.32 |
| News papers | Mean | 0.20 | 0.15 | 0.13 |
| (1 yes; 0 no) | SD | 0.40 | 0.36 | 0.33 |
| Radio | Mean | 0.52 | 0.49 | 0.45 |
| (1 yes; 0 no) | SD | 0.50 | 0.50 | 0.50 |
| Internet | Mean | 0.10 | 0.14 | 0.12 |
| (1 yes; 0 no ) | SD | 0.30 | 0.35 | 0.32 |
| Social networks | Mean | NA | NA | 0.20 |
| (1 yes; 0 no ) | SD | | | 0.40 |
| | Observations | 4,001 | 4,001 | 4,001 |
| | Municipal Characteristics | | | |
| Per capita GDP | Mean | 0.15 | 0.19 | 0.25 |
| (thousand pesos) | SD | 0.19 | 0.24 | 0.30 |
| Poverty Multidimensional Index | Mean | 29.97 | 28.85 | 29.53 |
| (%) | SD | 14.83 | 14.84 | 14.79 |
| Rurality index | Mean | 35.38 | 34.66 | 35.57 |
| (%) | SD | 24.47 | 26.35 | 27.23 |
| Distance to the state capital | Mean | 46.16 | 45.97 | 44.84 |
| (Km) | SD | 46.19 | 46.07 | 46.26 |
| Average last year education score | Mean | 49.03 | 49.67 | 49.16 |
| | SD | 2.32 | 3.18 | 3.09 |
| Homicide rate | Mean | 0.25 | 0.21 | 0.16 |
| | SD | 0.24 | 0.19 | 0.17 |
| Number of massacres | Mean | 0.33 | 0.11 | 0.18 |
| | SD | 0.81 | 0.40 | 0.43 |
| | Observations | 4,001 | 4,001 | 4,001 |

## Table A.3: Test for Parallel Trends (2013-2016)

| | Rulers elected by popular vote (1) | Sympathize with a party (2) | Report political ideology (3) | Ideology (scale left-right) (4) | Breaking the law to capture criminals (5) | Taking justice into own hands (6) | Support distributive policies (7) | Clientelism (8) |
|---|---|---|---|---|---|---|---|---|
| $\theta$ (Ho: parallel trends) | 0.03 | 0.02 | -0.01 | -0.13 | -0.02 | -0.04 | 0.08** | 0.02 |
| | (0.04) | (0.02) | (0.04) | (0.08) | (0.04) | (0.04) | (0.04) | (0.03) |
| Control ($X$) | YES | YES | YES | YES | YES | YES | YES | YES |
| Observations | 7,969 | 7,892 | 7,969 | 3,307 | 7,969 | 7,969 | 7,969 | 6,367 |

We report the parameter $\theta$ obtained by estimating the equation $y_{it} = \beta * (Exposure_i * Post_t) + \gamma' * X_{it} + \lambda_t + \theta * Exposure_i * Post_t * DummyYear2013 + \epsilon_{it}$ using observations for 2013 and 2016.

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$.

## Table A.4: Heterogeneous Effects: Political Attitudes

Panel A: Rulers elected by popular vote

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| $Exposure$ | -0.063 | -0.104 | -0.058 | -0.115 | -0.107 | -0.050 | -0.072** | -0.050 | -0.042 |
| | (0.044) | (0.111) | (0.041) | (0.066) | (0.071) | (0.039) | (0.031) | (0.033) | (0.032) |
| $Exposure * x$ | 0.012 | 0.001 | 0.001 | 0.034 | 0.058 | -0.038 | 0.037 | -0.043 | -0.060 |
| | (0.040) | (0.002) | (0.016) | (0.029) | (0.082) | (0.108) | (0.064) | (0.090) | (0.071) |
| Observations | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 |

Panel B: Sympathize with a party

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| $Exposure$ | -0.078*** | 0.025 | -0.101*** | -0.062** | -0.042 | -0.075*** | -0.074*** | -0.071*** | -0.069*** |
| | (0.020) | (0.049) | (0.026) | (0.026) | (0.048) | (0.015) | (0.016) | (0.018) | (0.018) |
| $Exposure * x$ | 0.016 | -0.002** | 0.019** | -0.004 | -0.029 | 0.057 | 0.013 | 0.024 | 0.004 |
| | (0.019) | (0.001) | (0.009) | (0.015) | (0.045) | (0.055) | (0.023) | (0.030) | (0.031) |
| Observations | 3,727 | 3,727 | 3,727 | 3,727 | 3,727 | 3,727 | 3,727 | 3,727 | 3,727 |

Panel C: Report political ideology

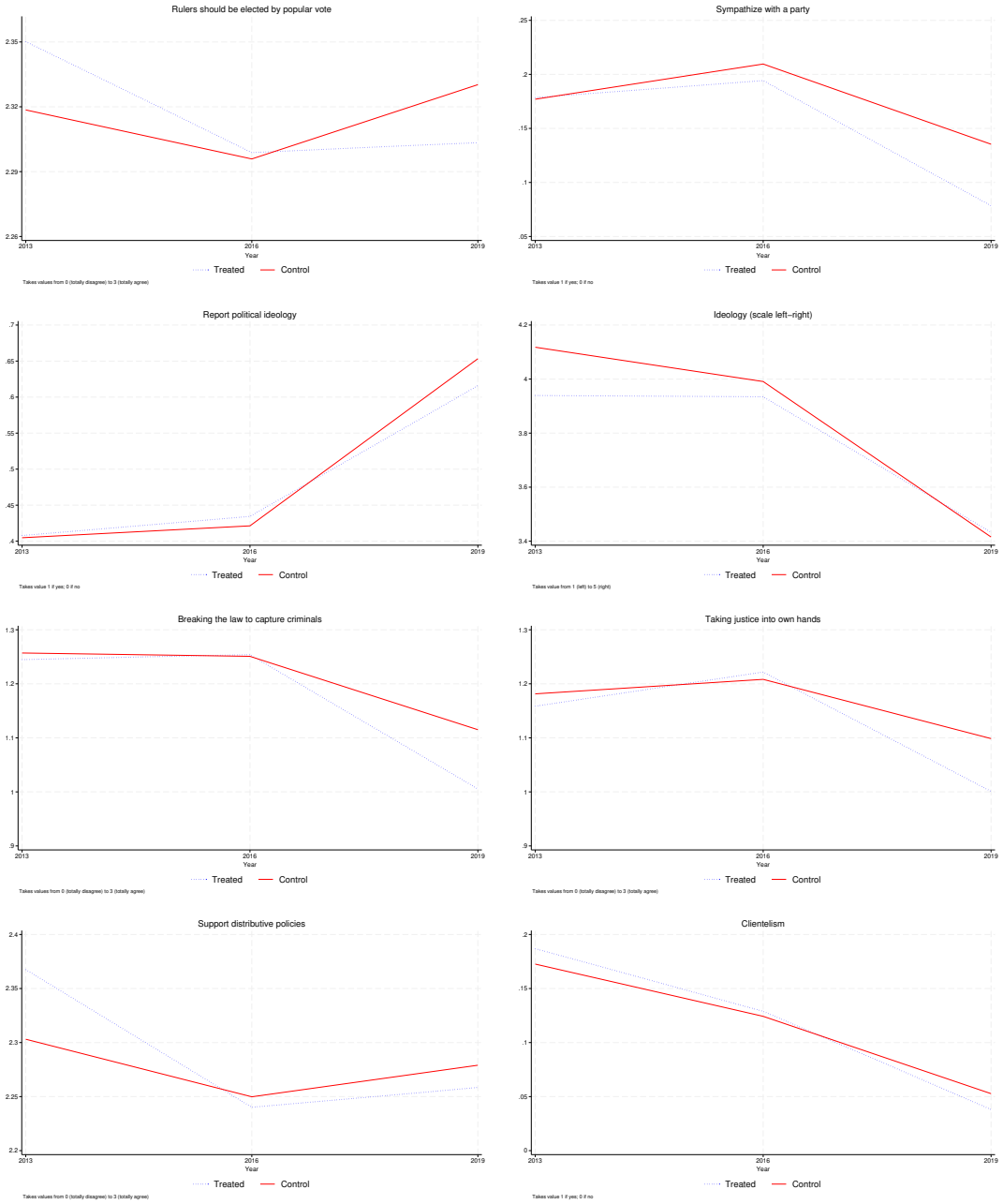
| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| $Exposure$ | -0.059** | -0.198*** | -0.093*** | -0.075 | -0.192*** | -0.103*** | -0.101*** | -0.079*** | -0.077*** |
| | (0.028) | (0.068) | (0.034) | (0.047) | (0.058) | (0.024) | (0.027) | (0.024) | (0.027) |
| $Exposure * x$ | -0.040 | 0.002 | 0.006 | -0.005 | 0.121** | 0.144** | 0.039 | -0.030 | -0.028 |
| | (0.028) | (0.001) | (0.011) | (0.021) | (0.057) | (0.057) | (0.034) | (0.052) | (0.043) |
| Observations | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 |

Panel D: Ideology (scale left-right)

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| $Exposure$ | 0.008 | 0.257 | 0.124 | 0.510*** | 0.223 | 0.101 | 0.115 | 0.105 | 0.166 |
| | (0.117) | (0.284) | (0.116) | (0.145) | (0.184) | (0.097) | (0.131) | (0.114) | (0.109) |
| $Exposure * x$ | 0.158 | -0.003 | -0.019 | -0.245*** | -0.155 | -0.093 | -0.069 | -0.107 | -0.288 |
| | (0.144) | (0.005) | (0.033) | (0.059) | (0.167) | (0.170) | (0.220) | (0.226) | (0.162) |
| Observations | 1,149 | 1,149 | 1,149 | 1,149 | 1,149 | 1,149 | 1,149 | 1,149 | 1,149 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. All estimates include the lag of the dependent variable and all controls in $X$.

## Figure A.1: Parallel Trends: Exposed and Non-exposed to Protests



## Table A.5: Heterogeneous Effects: Human Rights Attitudes

Panel A: Breaking the law to capture criminals

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.170*** | -0.214 | -0.139*** | -0.125 | 0.012 | -0.117*** | -0.141*** | -0.154*** | -0.140*** |
| | (0.037) | (0.140) | (0.037) | (0.064) | (0.076) | (0.030) | (0.043) | (0.035) | (0.034) |
| *Exposure**$x$ | 0.038 | 0.001 | -0.005 | -0.013 | -0.177** | -0.224** | -0.012 | 0.052 | -0.032 |
| | (0.045) | (0.002) | (0.017) | (0.030) | (0.081) | (0.099) | (0.052) | (0.072) | (0.056) |
| Observations | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 |

Panel B: Taking justice into own hands

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.126** | -0.099 | -0.104** | -0.104 | -0.072 | -0.088** | -0.102** | -0.129*** | -0.118*** |
| | (0.049) | (0.105) | (0.043) | (0.073) | (0.073) | (0.037) | (0.046) | (0.042) | (0.041) |
| *Exposure**$x$ | 0.017 | -0.000 | -0.006 | -0.006 | -0.048 | -0.206** | -0.027 | 0.108 | 0.014 |
| | (0.061) | (0.002) | (0.015) | (0.044) | (0.070) | (0.093) | (0.056) | (0.075) | (0.055) |
| Observations | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. All estimates include the lag of the dependent variable and all controls in $X$.

## Table A.6: Heterogeneous Effects: political economy attitudes

Panel A: Support distributive policies

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.037 | -0.213 | -0.015 | 0.061 | -0.075 | -0.018 | -0.049 | -0.033 | -0.026 |
| | (0.038) | (0.120) | (0.043) | (0.071) | (0.090) | (0.032) | (0.035) | (0.030) | (0.032) |
| *Exposure**$x$ | -0.003 | 0.003 | -0.013 | -0.057 | 0.041 | -0.155 | 0.023 | -0.041 | -0.054 |
| | (0.043) | (0.002) | (0.017) | (0.034) | (0.087) | (0.104) | (0.052) | (0.072) | (0.082) |
| Observations | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 | 3,859 |

Panel B: Clientelism

| | $x$=women | $x$=age | $x$=education | $x$=strata | $x$=TV | $x$=newspapers | $x$=radio | $x$=internet | $x$=internet social networks |
|---|---|---|---|---|---|---|---|---|---|
| *Exposure* | -0.009 | -0.087*** | -0.005 | -0.007 | -0.067*** | -0.009 | -0.023** | -0.009 | -0.006 |
| | (0.008) | (0.033) | (0.008) | (0.011) | (0.022) | (0.006) | (0.009) | (0.005) | (0.005) |
| *Exposure**$x$ | 0.001 | 0.001** | -0.002 | -0.001 | 0.065** | 0.002 | 0.032 | 0.005 | -0.013 |
| | (0.011) | (0.001) | (0.004) | (0.006) | (0.025) | (0.024) | (0.017) | (0.022) | (0.018) |
| Observations | 3,709 | 3,709 | 3,709 | 3,709 | 3,709 | 3,709 | 3,709 | 3,709 | 3,709 |

Standard errors clustered at the municipality level. *** $p < 0.01$, ** $p < 0.05$. All estimates include the lag of the dependent variable and all controls in $X$.

## Table A.7: Differences in 2013 between our sub-sample and the sample in the first ELCO's political module

| | | Statistics in 2013 | | Boostrap (1,000 replications) | |
|---|---|---|---|---|---|
| | | 4,001 individual Baseline estimates | 8,329 individuals First wave political module | Difference | p-value |
| Rulers should be elected by popular vote | Mean | 2.33 | 2.33 | -0.01 | 0.39 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.59 | 0.60 | | |
| | Observations | 4,001 | 8,329 | | |
| Sympathize with a party | Mean | 0.18 | 0.16 | 0.02 | 0.00 |
| 0 no, 1 yes | SD | 0.38 | 0.37 | | |
| | Observations | 3,962 | 8,249 | | |
| Report political ideology | Mean | 0.41 | 0.37 | 0.03 | 0.00 |
| 0 no, 1 yes | SD | 0.49 | 0.48 | | |
| | Observations | 4,001 | 8,329 | | |
| Ideology (scale left-right) | Mean | 4.07 | 4.05 | 0.02 | 0.36 |
| From 0 (lef) to 5 (right) | SD | 1.39 | 1.39 | | |
| | Observations | 1,623 | 3,112 | | |
| Breaking the law to capture criminals | Mean | 1.25 | 1.24 | 0.01 | 0.17 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.77 | 0.77 | | |
| | Observations | 4,001 | 8,329 | | |
| Taking justice into own hands | Mean | 1.18 | 1.17 | 0.00 | 0.64 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.76 | 0.77 | | |
| | Observations | 4,001 | 8,329 | | |
| Support distributive policies | Mean | 2.32 | 2.33 | -0.01 | 0.06 |
| From 0 (totally disagree) to 3 (totally agree) | SD | 0.62 | 0.63 | | |
| | Observations | 4,001 | 8,329 | | |
| Clientelism | Mean | 0.18 | 0.18 | 0.00 | 0.45 |
| 0 no, 1 yes | SD | 0.38 | 0.39 | | |
| | Observations | 2,488 | 5,045 | | |
| Urban | Mean | 0.48 | 0.52 | -0.04 | 0.00 |
| 1 urban; 0 rural | SD | 0.50 | 0.50 | | |
| | Observations | 4,001 | 8,329 | | |
| Female | Mean | 0.60 | 0.59 | 0.01 | 0.16 |
| 1 female; 0 male | SD | 0.49 | 0.49 | | |
| | Observations | 4,001 | 8,329 | | |
| Age | Mean | 46.97 | 46.24 | 0.73 | 0.00 |
| | SD | 11.61 | 12.59 | | |
| | Observations | 4,001 | 8,329 | | |
| Education level | Mean | 1.71 | 1.74 | -0.03 | 0.12 |
| | SD | 1.40 | 1.41 | | |
| | Observations | 3,981 | 8,254 | | |
| Tv | Mean | 0.83 | 0.82 | 0.01 | 0.01 |
| 1 yes; 0 no | SD | 0.38 | 0.39 | | |
| | Observations | 4,001 | 8,329 | | |
| News papers | Mean | 0.20 | 0.21 | 0.00 | 0.69 |
| 1 yes; 0 no | SD | 0.40 | 0.40 | | |
| | Observations | 4,001 | 8,329 | | |
| Radio | Mean | 0.52 | 0.48 | 0.03 | 0.00 |
| 1 yes; 0 no | SD | 0.50 | 0.50 | | |
| | Observations | 4,001 | 8,329 | | |
| Internet | Mean | 0.10 | 0.11 | -0.01 | 0.11 |
| 1 yes; 0 no | SD | 0.30 | 0.31 | | |
| | Observations | 4,001 | 8,329 | | |