SOCIAL DISTANCING AND COVID-19 UNDER VIOLENCE: EVIDENCE FROM COLOMBIA*

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ABSTRACT. Did violence increase social distancing and decrease COVID-19 cases? We investigated the effects of massacres on social distancing and subsequent impacts on COVID-19 cases in Colombia. Using an augmented synthetic control method model, we find that massacres reduced human mobility toward parks by six percentage points compared to unaffected areas. However, we did not find significant changes in workplace mobility. Moreover, alterations in social interactions caused by the violence had minimal effects on the spread of COVID-19. Following the occurrence of the first massacre, there was a decrease in 35 new cases per 100,000 inhabitants in the subsequent four months, with no evidence of changes in COVID-19 deaths. By leveraging an exogenous shock unrelated to the fear of the disease or its previous levels, we demonstrate the effect of social distancing and offer insights into social dynamics and public health.

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1. Introduction

The COVID-19 pandemic brought social distancing to the forefront, a public health measure that, although not new to epidemiology, became a pivotal aspect of daily life and a vital tool in the global effort to mitigate the spread of the virus (Moosa, 2020). Many countries implemented stay-at-home measures to curb virus transmission (Nivette et al., 2021). A considerable body of literature has investigated the impacts of mobility restrictions (Alfano and Ercolano, 2020; Allen, 2022; Arias et al., 2023). However, evaluating these policies faces challenges, as the areas where authorities implemented social distancing measures also had acute fears of the disease and its spread. The correlation between disease patterns and policy implementation poses challenges to identifying unbiased effects using standard evaluation methodologies. To address these constraints, we use an exogenous shock unrelated to the disease, which prompted a reduction in mobility in public spaces, enabling us to identify the effects of restricted mobility. Specifically, we examine the heightened levels of violence targeted at civilians in Colombia and explore how the resulting reduction in social activities impacted the transmission levels of COVID-19.

Furthermore, millions of individuals reside in regions governed by criminal groups in the developing world, and even in certain areas of the US and UK (Lessing, 2021). Despite a growing body of literature, our understanding of the impact of violence on social distancing and virus spread remains limited. For example, elected or legitimate governments often fail to enforce movement restrictions effectively, leaving room for *de facto* criminal groups to participate in their enforcement (Cavgias, Bruce, and Meloni, 2023). Along these lines, we examine the effects of massacres perpetrated by Colombian criminal groups on social distancing and COVID-19 related outcomes. Urban and rural Colombian areas have experienced governance by illegal armed groups (Aponte González, Hirschel-Burns, and Uribe, 2023; Blattman et al., 2021), providing an ideal environment to study the intricate relationship between public health and the presence of criminal groups. This study contributes to the public health literature by demonstrating that policies aimed at mitigating viruses and preventing diseases in areas under unlawful control must consider the complex power dynamics exerted by these illicit groups.

We argue that the escalation of violence targeting civilians in regions where this type of violence is unexpected provides a suitable setting to assess how the unforeseen rise in violence affects mobility and the spread of viruses. We hypothesize that such events can potentially affect the behavior of residents in these regions. Therefore, our study examines the impact of the first massacre from March 24^{th}, 2020, coinciding with Colombia's first national lockdowns, to March 31^{st}, 2021, just before widespread protests erupted in the country.[1] Additionally, we concentrate on areas with low coca suitability, characterized by minimal presence of illegal economies, and thus, were not accustomed to this form of violence.

Similar to other Latin American nations, Colombia reported its first positive COVID-19 case in early March 2020 and implemented strict quarantine measures, with limited exceptions for economic sectors, for approximately four months (Prada, Garcia-Garcia, and Guzman, 2022).[2] To assess the influence of violence perpetrated by illegal groups on social distancing and the virus's spread, we used data on human movement from Google Community Mobility, COVID-19 infections and fatalities from official sources, and violence incidents from non-profit organizations, universities, and governmental sources. We aggregated the data at the weekly and provincial levels, a unit of analysis between municipalities and departments, to mitigate biases arising from measurement errors associated with imprecise reporting of the location and timing of violent events and COVID-19 cases.

[1]In 2021, Latin America experienced widespread protests marked by diverse causes such as inequality, corruption, and deficient public service (OCHA, 2022).
[2]Colombia had few COVID-19 cases and deaths under initial lockdowns in the first months of 2020, but a rapid increase in infected people aided by the relaxation of curfews and the lack of vaccines by mid-2020 (Benítez et al., 2020).

Identifying the causal effect of violence on social distancing, cases, and deaths from COVID-19 requires careful estimation, as different areas are treated in different weeks. We used an augmented synthetic control model (ASCM) to build a weighted average of non-affected units that match the pretreatment outcomes of areas with massacres. This empirical strategy compared outcomes between treated units and the weighted untreated units to simulate what would have happened without the treatment. The ASCM method ensures that the synthetic controls accurately represent critical predictive variables for the outcomes of the treated unit (Abadie, Diamond, and Hainmueller, 2015).

The ASCM estimated the average treated effect on the treated under three key assumptions. Firstly, the treatment solely impacts treated units. To validate this assumption, we calculated the population-weighted averages of social mobility and COVID-19 outcomes at the province level. Secondly, there is no anticipated treatment effect, a plausible assumption supported by the literature suggesting that civilians are unlikely to predict massacres (Ibáñez and Vélez, 2008; Steele, 2018). Lastly, the assignment of treatment is random, conditioned on characteristics and pre-intervention outcomes. We assessed the plausibility of this assumption by estimating a discrete-time hazard model. In alignment with the assumptions, we found that neither previous COVID-19 cases nor past human mobility predicted the impact of massacres on the current stage of outcomes. Instead, variables such as share of women, population size, rural share, and share of land abandoned explain an increase in the probability of observing a massacre.

We did not find a discernible difference in outcomes between synthetic controls and treated units before the first massacre, but significant changes occurred following this violent event. Our preferred ASMC model demonstrated a statistically significant decrease in mobility toward parks while not significantly affecting movement to workplaces. Human mobility at parks declined by six percentage points more in treated areas compared to synthetic control units. This decline in mobility gradually and slightly translated into a reduction in COVID-19 cases. Four months after the first massacre, treated units experienced a drop of 35 cases per 100,000 inhabitants compared to synthetic control units. However, this decrease was only statistically significant for the population aged between 15 and 44 years old. We observed no statistically significant effect of massacres on COVID-19-related deaths.

Our findings are consistent across various alternative hypotheses. Specifically, we demonstrated that massacres did not induce changes in behavioral patterns in regions characterized by illegal economies such as high coca suitability production. Furthermore, even amidst social protests and presidential elections in 2022, the occurrence of massacres did not impact social distancing, COVID-19 cases, or related fatalities. These results indicate that the unexpected surge in violence affected mobility behavior and subsequently the transmission dynamics of COVID-19. Additionally, we offered evidence refuting the hypothesis that violence reduced the detection efforts of COVID-19 cases instead of the actual occurrence of cases. We found that the likelihood of detecting a new case was consistent across all Colombian provinces, regardless of the occurrence of massacres. Our results suggest that geographic characteristics accounted for the variation in testing among provinces rather than the incidence of massacres.

Lastly, we demonstrate that our estimation method does not influence our results. We arrived at similar conclusions by employing recent advancements in estimating unbiased effects in staggered adoption settings, where the shock occurred at different moments for different units. Utilizing the staggered difference-in-differences (DiD) estimators developed by Borusyak, Jaravel, and Spiess (2024); Callaway and Sant'Anna (2021); Dube et al. (2023), we observed comparable patterns in our outcomes following the first massacre as those derived from the ASMC model.

This article contributes to the extensive literature highlighting the significance of social distancing measures in mitigating COVID-19 cases. The evidence indicated that the effectiveness of lockdowns in reducing COVID-19 cases varies based on their timing and stringency (Alfano and Ercolano, 2020; Prakash et al., 2022). A longitudinal panel study involving 202 countries revealed that lockdowns led to an average decrease of 73-220 new COVID-19 cases. The decline in infections typically began ten days after the implementation of curfews and continued for one to two weeks (Alfano and Ercolano, 2020). Lockdowns in Europe and the United States have been linked to reductions in COVID-19 mortality ranging from 2% to 11% (Herby, Jonung, and Hanke, 2023). However, voluntary actions by individuals have played a more substantial role than government-imposed measures in slowing the pandemic's growth (Herby, 2021; Herby, Jonung, and Hanke, 2023). In the context of developing countries, our study demonstrates that voluntary actions enforced by violent events led to a more modest reduction in new COVID-19 cases and had no significant effect on mortality related to the virus. The fear of illegal violence primarily deterred individuals from engaging in leisure activities, rather than from attending workplaces. Particularly in societies with limited economic support and high levels of poverty, the imperative to attend workplaces outweighed concerns about violence, leading to continued workplace attendance despite the prevailing security concerns. Notably, parks represented a lower-risk environment for COVID-19 transmission due to their characteristics, such as interactions in open spaces with ample ventilation.

Our research expands on the understanding of non-state violence effects during the COVID-19 pandemic. Media and NGOs identified several cases where organized crime groups (OCG) provided resources and imposed or deleted stay-home measures from the government in Brazil, Colombia, Italy, Japan, Kenya, Mexico, and South Africa (Aziani et al., 2023). In Brazil, for example, paramilitary groups disincentivized the mandatory lockdowns imposed by the government, while drug trafficking organizations did not challenge the legal curfews (Cavgias, Bruce, and Meloni, 2023). In Colombia, illegal armies responded to COVID-19 heterogeneously across the territory. Some illegal groups permitted governmental and civil organizations to distribute food in certain areas and enforced lockdown restrictions through threats, patrolling, and random street temperature tests. Others did not follow any governmental curfew since they were skeptical of COVID-19's existence (Sampaio, 2021a). Lastly, some urban gangs neither enforced lockdowns nor provided services during curfews (Blattman et al., 2020). Despite the diverse strategies employed by illegal groups in Colombia, our study uncovered the influence of violence on COVID-19 related outcomes during the pandemic's initial year. Our findings suggest that violence may align strategically with the groups' objectives. These criminal groups might not have deliberately targeted economic activities to extract rents and secure financial sustainability.

Finally, we contribute to the literature investigating how illegal armies use violence as an optimal strategy (Blattman, 2022; Fearon and Laitin, 2003; Kalyvas, 2006). Several papers have studied the trade-off between little force leading to loose authority and too much violence pushing civilians to leave the territory or join the enemy for revenge. High violence could reflect that people do not have a critical role in the criminal business or illegal organization without the power to control the population with other means (Kocher, Pepinsky, and Kalyvas, 2011; Lyall, 2009; Schwartz and Straus, 2018). In Colombia, massacres have multiple motivations, such as electoral incentives, illegal revenues of disputes, and population alignment (Alesina, Piccolo, and Pinotti, 2018; Prem et al., 2022; Robinson and Torvik, 2009). Our findings illustrate that violence can affect behavioral responses in local populations that extend beyond its initial intended goal, thereby indirectly impacting the returns of the groups performing the violent acts. These findings highlight the importance of considering the broader effects that illegal groups may have in mind when assessing their decisions to engage in violent acts. Researchers analyzing violence must acknowledge the potential ripple effects on societal behavior and outcomes beyond the immediate goals of the perpetrators.

The remainder of the article is as follows: Section 2 describes Colombia's COVID-19 situation and civil war background. Section 3 describes the different data sources and descriptive statistics of the COVID-19 evolution and massacres in Colombia. Section 4 shows the synthetic control method and assumption to create a valid control group. Section 5 shows the results for human movement and COVID-19 cases. Section 6 presents robustness tests including the staggered difference-in-differences regression estimators. Finally, section 7 highlights the implications of the findings for policymakers.

2. Background

2.1. **The Civil War in Colombia.** The Colombian conflict is a multi-party conflict that has lasted more than 60 years. Alongside state forces, there are several left-wing guerrilla groups in dispute, such as the Revolutionary Armed Forces of Colombia (FARC) and the National Liberation Army (ELN). These organizations have a strong presence in remote rural areas and have financed their activities using illegal activities and taxes on legal activities (Arango, 2020). Moreover, there are right-wing paramilitary criminal groups in dispute. These illegal groups emerged as a counterinsurgency strategy, with some factions enjoying tacit support from certain segments of the military establishment (Arjona, 2016).

The Colombian civil war reached its peak during the 1990s when most groups were involved in drug cultivation and trafficking (Oslender, 2007). During this period, several criminal groups used massacres of civilians as a strategy to consolidate their power. Specifically, paramilitary groups that unified under the United Self-Defense Forces of Colombia (AUC) used massacres as a strategy to contain local support for left-wing guerrillas (Aranguren, 2001). By 2002, Colombia was experiencing more than one hundred massacres per quarter (Restrepo, Spagat, and Vargas, 2004). However, massacres decreased after the AUC's peace process in 2006. Although Colombia still has several paramilitary groups that splintered from the leading organization, the accord succeeded in reducing the occurrence of massacres (Holmes et al., 2021).[3]

After years of negotiation, the Colombian government signed a Peace Agreement with the FARC in 2016, aiming to reduce violence levels nationwide. However, the government faced the formidable task of establishing control in territories historically under FARC governance (Parada, 2022). Other illegal armed groups asserted their dominance in former FARC-controlled areas following clashes with rival factions and the perpetration of violence against civilians. Before the pandemic, massacres were less frequent in Colombia. In the years leading up to the onset of the pandemic in March 2020, Colombia recorded 47 and 45 massacres in 2018 and 2019, respectively.

This violence was largely confined to specific areas, primarily those linked to economies dependent on coca cultivation and disputes over profits from these illicit activities (Arango, 2020; Marín Llanes et al., 2020; Prem et al., 2022). However, during the pandemic, there was an increase in violence of several types. For instance, Castro et al. (2020) observed a sharp rise in the number of assassinations targeted at social leaders and vulnerable populations. In the case of the massacres, the situation was not different. Illegal groups capitalized on the state's reduced presence in certain areas due to challenges in pandemic containment, leading to increasing violence levels. This surge in violence constitutes the shock we are interested in analyzing: an unexpected increase in violence that altered population behavior and potentially influenced the course of the pandemic by affecting COVID-19 transmission levels.

2.2. **COVID-19 and lockdowns.** Like many countries around the world, Colombia was heavily affected by the spread of the COVID-19 virus. Beginning with the first confirmed case on March 6^{th}, 2020, the virus rapidly spread across most of Colombia.

[3] Figuire A.I in the Online Appendix shows the historical evolution of massacres in Colombia before 2014.

As of September 2022, Colombia had recorded more than six million confirmed COVID-19 cases with a fatality rate of 2.25% (INS, 2022). Although Colombia's death rate per 100,000 people was lower than that of other regional countries like Peru, Brazil, Chile, and Argentina (Sullivan, 2020), managing the spread of the disease posed challenges in many areas.[4]

The Colombian government implemented various strategies to mitigate the pandemic's outbreak. On March 24^{th}, 2020, the government announced a nationwide lockdown for 19 days. However, due to increasing transmission levels, the government progressively extended the lockdown, incorporating exceptions to reduce the economic impact of the curfews. On May 4^{th}, 2020, the government began an opening plan with the manufacturing and construction sectors. These lockdowns ended on September 1^{st}, 2020, when the government lifted all mobility restrictions (Arregocés, Rojano, and Restrepo, 2021).

Our analysis period extends until March 2021, preceding the surge of national unrest and changes in movement patterns, social distancing behaviors, and the spread of the disease. We argue that the increase in massacres in certain areas incentivized mobility restrictions due to the fear they induced in the population. Since these events are unrelated to the progression of the disease, they provide an ideal scenario to study how mobility restrictions helped reduce virus transmission.[5]

3. Data

3.1. **Province definition.** The presence of spillovers is a challenge when analyzing the impacts of policies to contain infectious diseases like COVID-19. An administrative unit such as a county or a city does not retain the virus, and measures taken in a

[4]In the Online Appendix, Figure A.II shows that Colombia had four COVID-19 deaths peaks in August 2020, February 2021, June 2021, and March 2022.

[5]While there is evidence suggesting that local illegal groups orchestrated some massacres to enforce adherence to social distancing measures (Turkewitz, 2020), we were unable to provide conclusive evidence that this was the case in the majority of instances. In any case, we demonstrate that the occurrence of massacres is unrelated to the previous progression of the disease (see Table 1).

municipality affect virus transmission in neighboring localities. We used as analysis unit the province to overcome the spillover effects. We grouped 1123 municipalities into 154 sub-regions using the definition proposed by Ramírez and De Aguas (2022), who defined a sub-region as a set of places with similar environmental characteristics and proximity to the closest urban center.[6] We believe aggregating municipalities at the providence level takes into account spillover.

3.2. **Massacres.** We used the Armed Conflict Location and Event Data Project (ACLED). This project collects information about political violence events in the world. This project publishes weekly information about events of political violence worldwide collected in real-time through trained individual researchers, partnerships with local organizations, and conflict observatories that provide information on hard-to-access contexts. Reviewers revise the initial data collection in three stages: First, it is revised by a coding reviewer, then cross-checked, and then the third and final reviewer scans event details (ACLED, 2017). This results in a database including the type, agent, location, date, and other factors describing various political violence events, demonstration events, and other non-violent events (ACLED, 2019).

The ACLED data is widely used to study conflict in several contexts.[7] For example, Bloem and Salemi (2021) use the ACLED data to conclude that inter-group violent conflict events had a short decline during the initial months of the pandemic. However, like other datasets on media reporting (e.g., BAAD, GTD, SCAD, UCDP, and OPIE),[8]

[6]Ramírez and De Aguas (2022) do not divide departments such as Arauca, Caquetá, Casanare, Guainiía, Guaviare, Putumayo, Vaupeés and Vichada. A sub-region is neither a political nor an administrative division in Colombia.

[7]The following papers use the ACLED dataset: Del Prete, Di Maio, and Rahman (2023) in Libya, Lu and Yamazaki (2023) in Indonesia, Dorff, Adcox, and Konet (2023) in the United States, Dawkins (2021) in South Sudan, Ekhator-Mobayode et al. (2022) and Nwokolo (2022) in Nigeria.

[8]The acronyms stand for Big, Allied, and Dangerous, Global Terrorism Database (BAAD, Asal, Victor H. and Rethemeyer, R. Karl (2015)), Global Terrorism Dataset (GTD, START (2022)), Social Conflict Analysis Database (SCAD, Salehyan, Idean and Hendrix, Cullen S. and Hamner, Jesse and Case, Christina and Linebarger, Christopher and Stull, Emily and Williams, Jennifer (2012)), Uppsala Conflict Data Program (UCDP, 2022), and Online Political Influence Efforts Database (OPIE, Martin, Shapiro, and Ilhardt (2023)).

ACLED data tells us about some unknowable combination of actual activity and journalistic coverage. Miller et al. (2022) highlighted that ACLED data could suffer from omission bias when specific conflicts, regions, or periods are insufficiently covered, inflation bias or over-reporting of events, and misrepresentation bias when events are inaccurately portrayed due to the lack of information. The measurement error in the ACLED might be country-specific. In the South Sudanese civil war, Dawkins (2021) argued that numerical precision is often unattainable in violent contexts and suggests that qualitative, categorical assessments may provide a clearer understanding of the scale and nature of conflict-related violence.[9] On the contrary, In Afghanistan, Weidmann (2015) compared the ACLED with first-hand accounts from military databases and concluded the accuracy of media reporting increases with the number of potential observers, and events in remote locations are surrounded by more uncertainty in reported information. Thus, the accuracy of media reports is sufficient for analyses at the district level.

For the Colombian case, the ACLED data has been available since January 2018 and came from a wide range of national and local media with the help of local NGOs and community networks.[10] One crucial source of information about massacres is the Institute of Studies for Development and Peace (*Indepaz* acronym in Spanish), a Colombian NGO established in 1984 that monitors the conflict in Colombia.[11] The data comes from different sources such as news, the police, the army, the Ministry of Defense, the Office of the Attorney General of the Nation, the Office of the Ombudsman, local

[9]Dawkins (2021) built her restuls from 40 hours of interviews with 32 human rights advocates, humanitarian workers, and journalists who are the sources for the ACLED and UCDP-GED datasets.

[10]Rojas and Walther (2022) use ACLED data to document the spatial diffusion of the ELN from Colombia to Venezuela after the 2016 peace process.

[11]Indepaz (2021) defines massacre, according to the Office of the Ombudsman, as three or more people murdered in a state of defenselessness, regardless of the quality of the victim.

agencies protecting the citizens' rights, and other human rights platforms (Indepaz, 2021).[12]

Following the literature (Dawkins, 2021; Miller et al., 2022; Weidmann, 2015), we made several decisions to ensure the ACLED data helps study conflict in Colombia. First, we aggregated data at the province level, which is bigger than the district level in countries such as Afghanistan and counties in the US. As Weidmann (2015) suggested, media reporting data performs better when studying violent events at a highly georeferenced level. Second, we aggregate data at the weekly level to reduce possible bias from not having the precise day when a violent event occurred. Dawkins (2021) argued that aggregate events in time rule out the reporting bias. Third, we study massacres (not clashes), which have been severely analyzed in Colombia.[13] Specialized NGOs such as *Indepaz* and Citizens Rights organization have made an effort to cover most remote and rural areas in Colombia.[14]

We focused our analysis on massacres, defined as the murder of the civilian population or unarmed individuals. These acts are typically motivated by hatred or the intention to instill fear in the population beyond the direct victims. This concept is generally complex and not explicitly codified in Colombia's legal statutes or international criminal law. For our purposes, we define a massacre as an event where three or more unarmed civilians are killed in a single attack perpetrated by non-state actors, excluding military or police forces. This definition aligns with those used in Colombia by NGOs dedicated to identifying such events, such as *Indepaz*.[15] Between January

[12]Many studies have used news media and NGOs to analyze the Colombian conflict. See Acemoglu et al. (2020); Bernal et al. (2024); Prem et al. (2022) for examples.

[13]See Ibáñez and Vélez (2008); Ide, Kristensen, and Bartusevičius (2021); Martinez (2017); Moya (2018); Steele (2018) for examples.

[14]Despite all our efforts, the final database might continue suffering from omission bias, inflation bias, and misrepresentation bias (see Miller et al. (2022) for details about ACLED measurement error). We recommend understanding our results from a compilation of information on discovered violent events, not as findings from the entire universe of such activity.

[15]*Indepaz* and is one of the sources of ACLED (2019) data.

2018 and March 2022, only 62 municipalities in 36 sub-regions experienced 94 massacres, resulting in 375 civilian casualties. This accounts for approximately 5% of the total municipalities in Colombia. Approximately 50% of these sub-regions witnessed only one massacre, while 28% experienced two or three, and the remaining 22% had four or more massacres.

Figure 1 shows the quarterly evolution of massacres since 2018. Before the COVID-19 outbreak in March 2020, there were ten massacres per semester, on average. After the beginning of the pandemic, the massacres increased to around 20 massacres per semester. In the third quarter of 2020, when the government completely lifted the curfews, massacres reached their maximum of 30 events, with 128 victims in a single quarter. We also found that this increasing trend comes from non-coca growing areas. In the Online appendix, Figure A.III shows that massacres in municipalities with high coca suitability remained relatively stable, with around seven massacres per quarter between 2018 and 2022. Massacres in places with low coca suitability experienced a boom after the second quarter of 2020, from 10 to 20 massacres.[16]

Figure 2 shows the time distributions of massacres, victims, and provinces with a massacre in our sample of low coca suitability regions. By March 31st, 2021, around 75% of massacres happened in regions with coca suitability index below the national median. Massacres, victims, and provinces with massacres increased after the Colombian government eliminated all the lockdowns. About 30% of the massacres from 2019 to 2022 occurred ten days after the total release of curfews. Overall, the raw data aligns with the hypothesis that the increase in massacres was not associated with the dispute over illegal activities. Masscres targeted civilians in public spaces rather than people linked to illicit economies. Furthermore, it's worth noting that the occurrences

[16]We did not see an increase in massacres in municipalities receiving government funding to reduce coca cultivation (called PNIS in Spanish, see Figure A.IV in the Online Appendix).

of massacres were relatively evenly distributed over time, with no significant concentration of massacres within the same week. This suggests that they were not influenced by an external shock that could potentially bias our results.

3.3. **Google community mobility.** We hypothesized that massacres affected COVID-19 evolution through changes in mobility patterns that the violence produced on local communities. We used Google Community Mobility reports (GCM) to address this hypothesis (LLC Google, 2021). Based on users that turned on the location history settings, Google could measure the number of visits to different types of locations and compare it to movement trends before the COVID-19 outbreak.[17] We focused on movements to parks and workplaces that represent the primary behaviour of mobility patterns in small cities (Duranton, 2016).[18]

Google data is available for only 321 municipalities and certain days per week. To overcome these issues of data availability, we averaged the available days per week and built weights from the municipality population to aggregate the data at the providence level. When there is no information for a particular week, we interpolate or extrapolate the data to complete the missing observations. The final database recovered the mobility index at workplaces for 119 providences and parks for 122 sub-regions or what it is the same for 79% of Colombia's total number of provinces.[19]

[17]We do not have the raw number of visits to a place in a specific week. We observe, for example, -34% in week seven of 2020, which means a decline of 34% in trips to a particular place, comparing the median number of trips between the first six of the year and the median trips in week seven. The baseline date is the median number of visits to a specific place from January 3^{rd} to February 6^{th} in 2020 (LLC Google, 2021).

[18]The GCM also includes mobility to grocery stores, pharmacies, parks, transport stations, workplaces, entertainment places, and residential areas. Google derived from users who activated location, but the GCM shared, aggregated, and anonymized without personally identifiable information about people's location, contacts, or movements. We neither found evidence of the Colombian government using Google information to track people who did not follow the lockdowns nor evidence that people deactivated the location track before violating legal lockdowns (LLC Google, 2021). Yilmazkuday (2021) have used Mobility Google data to track human mobility during COVID-19 in 130 countries.

[19]For reference, we recovered mobility in places such as supermarkets or recreation places for less than half of the provinces. Our final database recovered the mobility index for retail and residence in 78 and 68 sub-regions, respectively.

After the first lockdown on March 24^{th}, 2020, human mobility declined by about 60% compared with the median trips in the first six weeks of 2022. Although trips outside the home gradually recovered over time, movements did not reach the levels before the pandemic (see Figure 3). Before the total lift of lockdowns, human mobility decreased more in areas with than without massacres. However, one month after the complete release of curfews, the movement trend changed in small sub-regions with massacres. Trips to parks, for example, are below in areas with massacres than without violence. The movement trends did not change in big provinces, regardless of the level of these human rights violations.

3.4. **COVID-19.** Colombian Institute of Health (INS) centralizes the information about the universe of COVID-19 events at the national level. The INS collects information regarding the patient's symptoms, location, and test result dates (INS, 2022). We used only the evolution of symptomatic cases to study the pandemic's evolution.[20] We defined a new case using the self-reported date of the first symptoms instead of the date of diagnosis of the test. This date captures more precisely the growth and circulation of the virus. Even more, considering that COVID-19 tests are not widely available in all the regions, the timing for getting the results depends on the region's connectivity (INS, 2022).

During the first months of the pandemic, COVID-19 cases followed a similar pattern in small sub-regions with and without massacres. However, after the government completely lifted the lockdowns, small provinces with massacres reported a lower infection rate than places without massacres (see Figure 4 panel A). Big sub-regions with and without massacres have a similar trend in COVID-19 events before and after the lift of the curfews (see Figure 4 panel B). In cases resulting in deaths, the institute also centralized information concerning fatal outcomes, including the date of death. Here,

[20]Only Bogota, the Colombian capital, constantly reported asymptomatic patients. Asymptomatic cases in Bogota represent more than 90% of COVID-19 patients with non-symptoms in Colombia.

we observe similar patterns to those we detect in disease transmission. Figure A.V in the Online Appendix illustrates that smaller provinces experiencing massacres have fewer deaths attributed to the pandemic compared to areas without massacres.

Figure 5 displays the spatial distribution of massacres and COVID-19 cases in Colombia, illustrating several key points. Firstly, the majority of massacres occurred in regions with low coca suitability. Secondly, upon closer examination of areas with massacres, it becomes apparent that these regions typically have lower cumulative COVID-19 cases, particularly when compared to neighboring areas.

3.5. **Other data.** We use a large set of predetermined municipal characteristics such as the degree of rurality, population, area, altitude over the sea, distance to the departmental capital, population density, total municipality income, suitability for coca production, gold exploration, electoral risk, and justice inefficiency index. We aggregate these characteristics at the province level. For altitude, distance to the capital, and coca suitability, we aggregate the measure weighting by the total population or the total size of each of the municipalities that belong to the province. The source of this data is mainly *Centro de Estudios sobre Desarrollo Económico* (CEDE) at Universidad de los Andes and the Colombian Census Bureau (DANE). In the Online Appendix, table A.I shows in detail the definition of each variable and the source and table A.II presents the summary the descriptive statistics in our sample.

3.6. **Final sample.** We study the number of massacres from March 24^{th}, 2020, when the Colombian government started lockdowns to control the pandemic, to March 31^{st}, 2021, one month before generalized protests and riots (Uwishema et al., 2022). We were concerned that including events after the protests would have captured a different range of motivation and pattern behaviors of the local population. Our COVID-19 measures include data until September 30^{th}, 2021. That is around 30 weeks after the events we were analyzing. The maximum period that we think the behavior change would affect the evolution of the infection.

We also excluded provinces that are highly suitable for coca cultivation. We determined it using Mejía and Restrepo (2015)'s coca suitability index, which identified 597 municipalities (53%) as highly suitable for coca cultivation, where 95% of the area supports coca growth. A municipality reaching 95% of its area suitable for coca production corresponds to the 75th percentile in the distribution of the province area suitable for coca cultivation. The authors estimated coca leaf yields based on geographic characteristics such as elevation, soil erosion, soil nutrients, mineral content, topography, and rainfall index. We aggregated the index at the province level, weighting it by the municipality area, and calculated the distribution for each province.

Finally, we excluded Bogotá from our final sample since this city had a different tracing COVID-19 strategy than other parts of the country. The city conducted an active search in local communities of cases among positive patients and their contacts. Even more, the city is mainly urban, and its mobility patterns differ from all the other cities and towns of Colombia. We think that this exclusion allows us to perform a better comparison within similar groups.

4. Empirical strategy

Our objective is to evaluate the effect of a massacre on community mobility and COVID-19 transmission rates in sub-regions. We were interested in comparing our outcomes ($Y_{pT}(1)$) on each period after a massacre T with a contrafactual cases as if there had been no massacres ($\hat{Y}_{pT}(0)$). We used an augmented synthetic control method (ASCM) to estimate a version of province p treated ($p \in W_p = 1$) that performed statistically equal before the first massacres ($T_0 < T$).

The seminal method synthetic control method (SCM) uses a weighted combination of untreated units ($W_p = 0$) to build a synthetic unit, such the behavior of the outcome resembles the original treated-unit before the treatment (Abadie, Diamond, and Hainmueller, 2015, 2010; Abadie and Gardeazabal, 2003). This method constructs weights ($\omega_p^{scm} \in [0,1]$) to minimize the difference in pre-intervention trends between the treated and the synthetic control. Once the weights are estimated, they are used to approximate the potential outcome $\hat{Y}_{p,T}^{syn}(0)$ of the treated unit in the post-intervention period. Formally, the estimated synthetic outcome at time T is:

$$\hat{Y}_{pT}^{syn}(0) = \sum_{W_p=0} \omega_p^{scm} Y_{pT} \tag{4.1}$$

However, this method does not guarantee a perfect balance in all the characteristics.[21] To overcome this issue, we corrected the bias on estimations when the pre-treatment fit was not perfect, following Ben-Michael, Feller, and Rothstein (2021a). Formally we estimated the synthetic level of the outcomes of the treated units using the following model:

$$\hat{Y}_{pT}^{aug}(0) = \sum_{W_p=0} \omega_p^{ascm} Y_{pT} + \left(\hat{m}_{pT}(X_p) - \sum_{W_p=0} \omega_p^{ascm} \hat{m}_{pT}(X_P) \right) \tag{4.2}$$

Where $\hat{m}$ is the outcome model that can be seen as an estimate of the bias due to imbalance. The model we choose to de-bias the original SCM estimate is a ridge-regularized linear regression that increases the pre-treatment fit using the variables set X_p. This set included a series of pre-treatment outcomes and a set of fixed province characteristics.[22] The method's cost is to employ negative weights to improve the pre-treatment fits when negative weights are generally more sparse and less interpretable (see Ben-Michael, Feller, and Rothstein, 2021a, sec 4.1).

[21] Appendix Table A.III shows some differences between provinces without and with massacres. A particular concern is that massacres occurred in a place with more share of gold exploitation, more presence of coca substitution programs, provinces at lower altitudes and with higher density, and further away from important cities.

[22] The characteristics are total population, area, the share of the rural population, women, coca suitability, municipalities with governmental financial support to reduce the cultivation of illegal crops, gold exploration area, population density, average altitude, total income and expenditures per capita and distance to the capital.

4.1. **Build the average treatment effect on the treated (ATT).** Our goal is to identify the average effect of massacres on mobility index, COVID-19 cases, and related deaths. Since we have multiple treated units (i.e., sub-regions with massacres), we needed to aggregate the estimated effects for each unit to calculate the ATT. This aggregation is particularly challenging since we have several treated provinces in different weeks. Thus, the weight estimation that minimizes the imbalance before the treatment must consider two forces. The first is the imbalance for each treated unit separately, and the second is the imbalance for the whole average of the treated units.

We followed Ben-Michael, Feller, and Rothstein (2021b) and calculated ω^{ascm} by minimizing the two sources of imbalance in the average effect instead of calculating the mean individual effects for each treated unit. The method is a partially pooled SCM that weights the combination of these two measures. We allow the algorithm to choose a combination of the two factors based on how well separate synthetic controls balance the overall average. Formally, the parameter that governs the relative weight is ν. $\nu = 0$ is equivalent to estimating separate SCM fits for each province, then estimating the ATT by averaging those estimates. $\nu = 1$ is equivalent to finding the weights that minimize the ATT's root mean squared placebo estimate.

4.2. **Assumptions.** This method correctly estimates unbiased ATT in the presence of three assumptions. First, the treatment is stable across units, or what is the same, that massacre only affected the treated areas. An analysis at the municipality level violates this assumption. Colombian illegal armies do not operate in isolated municipalities, and massacres send signals to different municipalities beyond the location where they happened. We overcome this issue by aggregating the data at the province level. These are units bigger than municipalities and represent neighboring places with physical connections. Yet, they are smaller than departments where the sign of a massacres could get lost over large territorial extensions.[23]

Second, massacres had no effects before their occurrence, with no anticipation of such events. Given the unexpected nature of a massacre, we think this is a realistic assumption. According to the literature, the population cannot fully predict massacres and therefore change their behavior before a massacre (Ibáñez and Vélez, 2008; Steele, 2018).

Third, the assignment of treatment was random, conditioned on observable covariates and the pre-intervention path of outcomes. This assumption is valid in our setting. That is, the previous levels of our outcomes (COVID-19 cases and mobility indices) did not affect the hazard ratio of observing a massacre. We tested the assumption by estimating a discrete-time hazard model using the method described by Jenkins (1995). We modeled the probability of having the first massacre at a given week as a function of province fixed characteristics and time-varying covariates in a duration dependence equation. We employed the following specification:

$$h_{pt} = \exp(\beta' X_p + \gamma' L_{pt} + c_t) \tag{4.3}$$

Where h_{pt} is the hazard rate for having at least one massacre between February 15^{th}, 2020, and March 31^{st}, 2021. X_p includes time-invariant characteristics of each province. L_{pt} is a set of time-variable aspects measuring past COVID-19 levels and community mobility. c_t are week dummies that control for duration dependence.

5. Results

5.1. **Addressing the randomized treatment assumption.** Before presenting the ASMC model results, we provided evidence that past COVID-19 case levels did not predict future massacres. By estimating Equation 4.3, Table 1, Columns 1 and 2 show

[23]The aggregation of massacres also help us to reduce the possible measurment bias from using the ACLED data as Dawkins (2021) suggested.

that positive cases from one to four weeks ago did not explain the likelihood of having a massacre. The coefficients of previous infection levels was non-significant and close to zero.

An alternative hypothesis was that illegal groups acted when they perceived a rise in human movements. We tested this explanation by introducing average mobility levels at workplaces and parks before the first massacre. Table 1, Column 3 and 4 show that the likelihood of massacres did not increase more in places with high levels of commutes to workplaces than in areas with low trips to work. Similarly, we did not see a variation in the probability of having a massacre depending on lag trips to parks (Columns 5 and 6 in Table 1).

We also tested the relation of massacres with other province-fixed characteristics, socioeconomic characteristics, illegal sources of production, and violence variables. We found that population size, share of men, and share of the rural population increased the probability of having at least one massacre. Public income, public expenditure, the number of institutions, and the justice efficiency index did not explain massacres. Furthermore, the presence of illicit activities (e.g., coca cultivation, trafficking routes, or gold exploitation) and previous levels of victimization did not predict current massacres (see Table 1).[24] Overall, these results reject the possibility that we are capturing the incidence of unintended consequences of disputes for illegal economies. These findings also show that massacres observed were not a common practice before COVID, and the increase was not the result of groups performing regular activities before the pandemic.

These findings indicate that illegal groups did not increase violence in response to high or low levels of COVID-19 transmission. The treatment was random, conditional on the donor pool, observable covariates, and the pre-massacre path of the outcome. Both pre-levels of infections and mobility were unrelated to massacres. The evidence shows that the randomized treatment assumption is valid in this setting.

[24]Only the share of land that was abandoned due to past violence levels increases the probability of having a massacre in the present.

5.2. **Results on community mobility.** Our initial hypothesis posited that massacres would reduce mobility among community members. By estimating the ASMC model in Equation 4.2, Figure 6 shows the difference in percentage change of human movements compared to the first weeks of 2020 between treated provinces and synthetic control pre- and post- the first massacre in provinces with low coca suitability.[25] As evidence that the ASMC model created synthetic controls similar to the treated unit, we do not find any difference in human movement between control and treated sub-regions before the first massacre. The left panel in Figure 6 shows a three percentage point decrease in trips to work when comparing places with and without massacres, but the difference is not statistically significant. For travels to parks, we found a statistically significant decrease of six percentage points one week after the first massacre. Park trip reduction continued in the following weeks by about six percentage points (see the right Panel in Figure 6).

We found a different story when we replicated the same exercise using a sample of highly suitable coca provinces. Figure A.VI in the Online Appendix shows a non-significant increase in human movement compared with and without massacres. Our interpretation of the opposite effects in low and high coca suitability areas is that criminal groups balance violence and keep the earnings from illegal activities such as coca cultivation. People became more critical in non-coca-growing areas when coca leaves were not a reliable source of income. This maximization behavior is not unusual in the Latin American region. Brazilian criminal groups, for example, encouraged some businesses to remain open since they needed them as a source of revenue while forbidding social events (Miagusko and Da Motta, 2021; Sampaio, 2021b).

In terms of magnitude, the reduction in mobility at parks is considerable. Comparing it to the increase in park mobility after the total lifting of lockdown measures in September, which saw a rise of around 15 percentage points in large provinces and

[25]We built standard errors and confidence intervals using a Jackknife method (see Ben-Michael, Feller, and Rothstein, 2021b, sec 5.3).

20 percentage points in small provinces, our observed effects amount to approximately one-third of these measures. In other words, the massacres induced fear in people, leading them to reduce their mobility, which amounted to approximately one-third of the effect of the mandatory government mobility restrictions.

5.3. **Results on COVID-19.** We evaluated whether the reduction in human movement translated into a decline in COVID-19 transmission. Figure 7 presents the estimated average difference in new COVID-19 cases per 100,000 inhabitants between provinces with massacres (treated) and synthetic control areas. To confirm that the control units from the ASMC model matched the treated provinces, Figure 7 shows similar COVID-19 trends in both treated and synthetic units before the first massacre.

Four months after the first massacre, treated subregions reported 35 fewer cases per 100,000 per week than control units—a result of considerable magnitude. This result was considerable in magnitude. On average, 5 cases per 100,000 inhabitants per day is around half of the rate of infection observed in small provinces during the first months of the pandemic and one-quarter of the infection rate in big provinces in a period with a low transmission rate.[26]

The nature of COVID-19 transmission accounts for the gradual reduction in cases after the first massacre and the delayed observation of a decrease at aggregate levels. The decrease in positive tests stemmed from reduced visits to parks, which are areas with a low transmission risk (Althouse et al., 2020; Tenforde, Fisher, and Patel, 2021). The restriction of human mobility to low-risk contagion areas slowly translated to a drop in the total number of cases.

[26]The construction of the synthetic units seems reasonable based on qualitative assessment. Appendix Table A.V illustrates the weights of donors we used in constructing the synthetic units, focusing on those with a weight of 5% or higher. Notably, for the 27 treated provinces, we incorporated more than 35 unique provinces to create the synthetic controls. Additionally, most synthetic controls utilized more than two untreated provinces in their composition, averaging 5 per treated unit. Moreover, many of these donor provinces are located within the same department or neighboring departments within the regions of the affected province.

5.3.1. *Effects by age.* As heterogeneous effects, we tested the model for different age groups. Younger people were more likely to transmit COVID-19, while older people were more likely to die from the virus (Davies et al., 2020). In Figure 8, we estimated the difference in COVID-19 cases between sub-regions with massacres and synthetic control units by age. As a test that the ASCM mode built synthetic controls that correctly emulate treated provinces, we found no statistically significant effect in the six categories before the first massacre.

Overall, we found that the reduction in COVID-19 events after a massacre comes from a statistically significant decline in cases of people aged 15 to 29 and 30 to 34 (on average, 30 cases and 50 cases per 100.000 inhabitants, respectively). We did not find a significant decrease in children from 0 to 14 years old and adults from 45 years old or older (see Figure 8). This result is congruent with our previous finding, which showed reduced travel to parks since people aged 15 to 34 generally have more social activities and networks.

5.3.2. *Effect on deaths.* We examined whether the decrease in positive tests corresponded to a reduction in COVID-19-related deaths. Figure 9 displays the difference in fatalities between synthetic control and treated provinces. Only 20 weeks following the initial massacre, we observed a decrease in the daily rate, approximately 0.4 cases per 100,000 inhabitants, but these differences were not statistically significant. We investigated the varying impacts of age on mortality rates. Similarly, we found no significant impact on COVID-19 deaths.[27] However, we observed a slight decrease in deaths among individuals aged 45 years or older (see Figure A.VII in the Online Appendix). These results suggest that reducing virus transmission levels did not affect COVID-19 mortality rates. Massacres affected the behavior of the younger population, who are less susceptible to the virus's fatal consequences.

[27]Estimation for younger groups was not informative, given the low mortality levels among young individuals.

6. Robustness checks

6.1. **Staggered difference-in-differences.** Another option for assessing the effect of the first massacre is to leverage the timing and location of the event by estimating the following staggered difference-in-differences specification:

$$Y_{pt} = \alpha_t + \alpha_p + \sum_{l=-T, l\neq -1}^{T} \theta_l D^l_{mt} + \epsilon_{tp} \tag{6.1}$$

Here, α_p and α_t represent the province and calendar week fixed effects, respectively. D^l_{mt} is a dummy variable for provinces that experience a massacre l weeks after the first massacre. The coefficients of interest θ_l encompass all leads and lags and evaluate the change in outcome Y_{pt} in those provinces with a massacre relative to the week before the event.

The primary basis for interpreting these coefficients as causal hinges on the assumption that treated and untreated provinces would follow similar trends in the absence of massacres (Bertrand, Duflo, and Mullainathan, 2004). However, recent studies suggest that this approach could produce biased results (Roth et al., 2023). The problem arises from the irregular timing of massacres and their varying impacts over time and across different areas. Consequently, the coefficient obtained might not accurately reflect the true average treatment effect on those provinces directly affected by the massacres (ATT).

Forbidden comparisons can introduce bias into the estimates in the context of a two-way fixed effects (TWFE) model (Baker, Larcker, and Wang, 2022; Callaway and Sant'Anna, 2021). When estimations employ early units as a control for units treated later, the model 6.1 lacks validity in identifying the impact of the massacre on mobility and COVID-19 transmission. With sixteen cohorts of provinces treated at various points throughout the year, the probability of heterogeneous effects is significant, thereby increasing the potential for bias. Consequently, we adopted recently developed estimators robust to biases inherent in conventional TWFE models.

We followed Callaway and Sant'Anna (2021)'s procedure that identifies specific ATT for each cohort g and week t to aggregate them in ways to present like "event study" figures. This procedure allows treatment effect heterogeneity, avoiding incorrect comparisons. CS is suitable for our model because it calculates ATTs per group before and after the event. We estimate the model using inverse-probability weighting, wherein the treated provinces are compared with never-treated provinces with the same probability of experiencing a massacre. Estimating these probabilities relies on geographical and socioeconomic characteristics, as defined in Table 1.[28]

In the Appendix, Figure A.VIII displayed the evolution of coefficients after the first massacre relative to the period before treatment. The pre-treatment coefficients are averaging consecutive weeks before the first massacre, that is, short-term comparisons (see Roth (2024) for details). The coefficients fluctuated around zero before the first massacre, exhibiting no discernible differential pre-treatment trend in workplace and park mobility or COVID-19 transmission. These findings provide evidence that the primary assumption of this methodology, namely, the evolution in potential outcomes post-treatment (without a massacre), is likely similar across treated cohorts and untreated units.

The analysis also revealed no statistically significant reduction in mobility at workplaces following the first massacre, but we observed a notable decline in parks and recreational areas. The magnitude of the estimate closely resembles our main findings, with approximately a six percentage point reduction after a massacre. For COVID-19 transmission, the CS showed that massacres led to a decrease of roughly 35 cases per 10,000 inhabitants per week. The reduction in transmission levels started after the 15th week and became statistically significant after the 20th week.

[28]We did not include all variables due to the lack of common support, which impedes statistical inference. We excluded the density variable from consideration for the same reason.

In summary, our findings from the CS model remain consistent with the results from the ASMC model. We concluded that the massacre prompted changes in mobility behavior, particularly a reduction in non-essential locations such as parks. The estimates of mobility scores diverge from the synthetic control in two respects. Firstly, there is an additional decrease in mobility at parks, with the reduction reaching ten percentage points after ten weeks. Secondly, although the decline in workplace mobility is not statistically significant, its magnitude surpasses the results levels from the ASMC model. The decline in human mobility resulted in a decrease in COVID-19 transmission levels. The reduction took several weeks to materialize and was relatively modest, likely due to the nature of interactions at parks, which are open spaces associated with lower transmission risk.

We validated the robustness of our results by employing alternative estimation methods that address potential issues associated with the TWFE model. Appendix Figure A.IX displays the staggered difference-in-differences using Borusyak, Jaravel, and Spiess (2024)'s estimator.[29] Following Roth (2024), we presented two graphs for each outcome. Whereas panels A, C, and E depict the coefficients of each week before the massacre compared to the earliest possible week, panels B, D, and F illustrate the coefficients corresponding to the effects of the week after the massacre compared to the average levels before the event.[30] As in the ASCM and Callaway and Sant'Anna (2021) estimators, the results illustrated that outcomes exhibited behavior similar to our main results following the first massacre.

[29]Due to the nature of our data, which comprises few units compared to a lengthy time series, we cannot incorporate time-invariant controls to interact with period dummies in this model. Consequently, we omit the inclusion of any controls to estimate the BJS estimator.

[30]Borusyak, Jaravel, and Spiess (2024) employ an imputation approach for post-treatment effects: (i) fit a TWFE model using only untreated units; (ii) form individual treatment effect estimates by calculating the difference between the actual outcome and the estimated outcome derived from predictions made by the TWFE model; (iii) average these estimates across units. The authors utilize a dynamic TWFE specification for pre-treatment effects, employing untreated units with dynamic indicators for the number of periods until the treatment. The earliest pre-treatment period is normalized to zero.

While the estimates are smaller, the patterns remain consistent in estimating effects on mobility levels. Following a massacre, there was a slight temporal decrease and no statistically significant workplace mobility. However, there was a significant permanent reduction in mobility at parks of approximately 4.5 percentage points. This change in mobility correlated with a decrease in COVID-19 transmission levels, averaging a reduction of around 35 cases per 100,000 inhabitants after week 15.

Finally, we utilized Dube et al. (2023)'s estimator, which uses local Jordan projections. Using the long time series in our data, we incorporated controls for previous outcome levels in the model, and the same time-invariant controls used in the synthetic control estimation interacted with quarterly dummies. Appendix Figure A.X again exhibits similar patterns and levels to our main results. We are confident that our conclusions remain unaffected by the estimation method and are indeed robust. The occurrence of a massacre reduced mobility at parks, translating into a modest reduction in COVID-19 levels.

6.2. **Additional exercises.** Our results remained robust to some additional tests. First, we evaluated our decision to include only the massacres occurring from the beginning of the pandemic until the first quarter of 2021. We argue that due to the social unrest beginning in April 2021, the occurrence of a massacre had a different impact from that date forward. Moreover, often, the occurrence of a massacre might relate to the presence of protesters in the streets, affecting both mobility and infection data.[31] To examine the post-protest period, we replicated the analysis of massacre effects post-2022—two years after the first COVID-19 case in March 2020 and over a year after lifting all pandemic measures in September. Figure A.XI in the Appendix shows that social movement patterns did not change after a massacre. Indeed, following the first massacre, there was a negligible increase in park mobility and no change

[31]It is essential to mention that this period includes the general election campaign, historically marked by elevated levels of violence and, consequently, an increased occurrence of massacres.

in workplace mobility. The figure indicates no significant shifts in COVID-19 cases following the massacres in 2022. These findings affirm the validity of our decisions, suggesting that massacres following social unrest do not significantly influence mobility or subsequent COVID-19 transmission levels. Furthermore, this implies that the 2020 transmission level changes were likely due to alterations in mobility patterns rather than other unobservable factors.

Secondly, we assessed whether massacres impacted the tracking of COVID-19 cases. If this were the case, the observed decrease in COVID-19 cases in our primary findings might not be due to a reduction in disease levels but rather a decline in testing for the illness. To explore this alternative explanation, we evaluate different possibilities. As shown in Figure A.XII in the Appendix, detecting a new COVID-19 case did not differ significantly between areas with and without massacres. Furthermore, Figure A.XIII illustrates no significant changes in the number of tests conducted at the department level in areas with massacres. It's worth noting that Colombia did not provide detailed data on the daily number of tests conducted at the municipality level. Instead, the National Institute of Health (INS) sporadically updated data with municipal cumulative tests. We utilized data on daily tests performed and available from May 9th, 2021. Finally, Table A.IV demonstrates no correlation between COVID-19 testing, massacres, and victims of massacres. These results suggest that massacres did not affect access to COVID-19 tests, and our results are not the product of increased difficulties of the health system to perform tests due to increased security concerns.

Thirdly, we validated if a subsample selection is driving our results. We estimated our main results using only the sample of areas where human mobility data is available. Like the main results, COVID-19 cases decreased more in provinces with massacres than those without (refer to Figure A.XIV in the Online Appendix). These findings mirror those of Figure 7 for all non-coca Colombian provinces, indicating that we are not concerned that sample selection and the lack of complete information on mobility trends biased our results.

7. Conclusion

During the COVID-19 pandemic, societies worldwide grappled with implementing measures to enforce social distancing and combated the spread of the virus. Countries faced the challenges of allocating resources to maintain public safety and address the socio-economic consequences of stringent restrictions. Amid this complex landscape, organized crime groups had heteregeneous responses to the pandemic. Colombia, for example, witnessed an escalation in violence during this period, particularly in areas historically characterized by low levels of criminal activity.

We analyzed the impact of massacres occurring between March 24^{th}, 2020, the onset of the first Colombian lockdown, and March 31^{st}, 2021, before widespread protests and unrest erupted across the country, on human mobility and COVID-19 cases, and deaths related to the virus. Unlike previous studies that evaluated the impact of mandatory restrictions, which were often influenced by high transmission levels, our analysis benefited from a scenario where mobility reductions were not correlated with prior infection rates or fear of the disease. We took advantage of the unexpected surge in violence, arguing that these massacres induced fear among the local population, ultimately leading to reduced mobility.

Utilizing the Augmented Synthetic Control Method (ASCM), we constructed a synthetic control unit to match pre-existing levels of cases and mobility in sub-regions affected by massacres. Our results indicated that the first massacres led to a decrease in mobility to parks but not workplaces. These findings highlight the prioritization of economic necessity over safety concerns and the significant impact of violence on individual behavior. We also found a modest decline in COVID-19 cases, particularly among individuals aged 15-44 who engage in more social activities. This reduction in mobility did not translate into a significant decrease in COVID-19 deaths.

Our findings have significant implications. They suggest that voluntary measures to reduce social interaction were effective in curbing COVID-19 transmission. However, given their voluntary nature, their impact was limited, particularly in reducing mortality rates among the most vulnerable populations. This paper sheds light on the debate surrounding the efficacy of mandatory restrictions in public spaces to contain airborne diseases, considering the associated economic costs, especially in disadvantaged settings.

As COVID-19 shifts from being a pandemic to becoming endemic, it has worsened poverty and vulnerability in numerous global regions. Restrictions on outdoor mobility have added to the already existing mental health difficulties. Our analysis indicates that the costs of these measures outweigh their benefits, remarkably, as they failed notably to decreased mortality rates among older individuals but have placed significant burdens on the broader population. In uncertain times such as a pandemic, ongoing assessment of policy impacts is crucial to minimize costs and ensure that policies stay aligned with their original objectives.

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