Protests and Trust in the State: Evidence from African Countries

Marc Sangnier
Yanos Zylberberg

University of
BRISTOL

Protests and trust in the state:
Evidence from African countries[*]

Marc Sangnier[a]   Yanos Zylberberg[b]

May 2017

Abstract

This paper provides empirical evidence that, after protests, citizens substantially revise their views on the current leader, but also their trust in the country's institutions. The empirical strategy exploits variation in the timing of an individual level survey and the proximity to social protests in 13 African countries. First, we find that trust in political leaders strongly and abruptly decreases after protests. Second, trust in the country monitoring institutions plunges as well. Both effects are much stronger when protests are repressed by the government. As no signs of distrust are recorded even a couple of days before the social conflicts, protests can be interpreted as sudden signals sent on a leaders' actions from which citizens extract information on their country fundamentals.

KEYWORDS: Protests, trust, institutions, leaders.

JEL CODES: D74, D83, H41, O17.

[a]Aix-Marseille University, CNRS, EHESS, Centrale Marseille, Aix-Marseille School of Economics. Corresponding address: Aix-Marseille School of Economics, Chemin du Château Lafarge, 13290 Les Milles, France. Phone: +33 (0)4 42 93 59 85. Email: marc.sangnier@univ-amu.fr.

[b]University of Bristol. Corresponding address: University of Bristol, Priory Road Complex, Priory Road, BS8 1TU Bristol, United Kingdom. Phone: +44 (0)117 331 0706. Email: yanos.zylberberg@bristol.ac.uk.

1   Introduction

As pointed out by Kuran (1989, 1991a,b), an apparently minor signal may drive beliefs in leaders' behavior and in the quality of institutions far from their previous levels. Trust in the state may not only be a capital which slowly accumulates over decades, as frequently argued in the literature, but may also be based on imprecise priors, subject to large volatility.

One recent example of belief volatility is the Thai military overthrow of May 2014 which concluded six months of social convulsions. At the origin of the crisis, there was a reprehensible decision—an amnesty for all political crimes—, followed by protests informing the whole country of the close relationship between the electoral commission, the high-level courts, and the conservative traditional ruling class. After these initial protests, the movement grew to such an extent that the final overthrow was supported by a large part of the population and there were discussions on future changes in the constitution. These events show that it is important to analyze how citizens update their beliefs and understand the dynamics of trust in the state. We contribute to the empirical investigation of this dynamics.

In this paper, we study the evolution of trust toward the head of state and monitoring institutions in the immediate aftermath of social protests. We posit that protests inform citizens about the state of the world: citizens may learn (i) that their leader is dishonest, and (ii) that monitoring institutions are insufficient to prevent leaders from misbehaving. We test these hypotheses thanks to a careful match between two rounds of the Afrobarometer survey between 2005 and 2009, and a database on local conflicts and protests in Africa—the Armed Conflict Location and Event Dataset. Our empirical strategy takes advantage of the precise timing and localization of protests and interviews. The identification comes from the interaction of a spatial treatment, comparing respondents in the immediate surroundings of protests to their regional peers interviewed in the same wave, and a time treatment, comparing individuals interviewed less than 60 days after the occurrence of a protest to those interviewed less than 60 days before. The difference—between recent and future protests—in these within-region differences in beliefs cleans for a possible selection bias arising from non-random variations in protest occurrences within a region.

Our findings indicate large movements in opinions regarding leaders in charge. Importantly, protests also affect beliefs in institutions with a monitoring role such as the electoral commission. The occurrence of a single protest during the two previous months within a radius of 20 kilometers reduces the probability for respondents to trust the president by half of the standard deviation across regions—a raw proxy for long-term differences across space. A similar amplitude is recorded for trust in institutions that supposedly exert some monitoring power over leaders in charge.

We also exploit some declarations of intent related to prospective civic involvement in the Afrobarometer survey, and show that the revisions in priors about leaders and institutions affect the nature and intensity of civic engagement. Protests negatively affect the willingness to vote for the party in power, especially so after a repressed protest. Protests also increase the propensity to consider abstaining in the future election or voting for another party than the ruling one. While the willingness to raise issues in a non-confrontational setting decreases, the declared intention to attend protest rallies increases. These findings may indicate a change in the nature of civic participation, with citizens favoring street protests (even when they are more likely to be repressed) to the silent protest in the ballot box.[1]

There exist competing interpretations to these findings and we undertake a number of empirical robustness checks that support our preferred interpretation. First, we do not find evidence of anticipation effects few weeks before future protests. Instead, we uncover evidence of a discontinuity around protest occurrences: agents update their beliefs abruptly and immediately after the observation of a protest, and they declare much lower trust in leaders and institutions than respondents that are interviewed just before.

Second, the mere existence of protests may signal the inability of leaders and institutions to prevent them: security and public order are public goods that are arguably guaranteed by leaders and institutions such as the police or courts. In addition, a policy may be desirable for the majority of citizens and protests may be the doing of a small minority. We do not find support for this interpretation as the occurrence of a protest lowers the declared support for nipping minority's opinions in the bud. In order to further reduce concern about this alternative interpretation, we show a placebo check using acts of violence against civilians (including violent demonstrations of football fans or ethnic violence) and battles between armed groups instead of protests. We also present additional results in which we distinguish protests motivated by public policy issues from those motivated by international shocks (as for example World food prices fluctuations or US foreign policy) or directed at private actors and we show that trust in the executive power only drops after the former.[2]

Third, it is possible that respondents are afraid to reveal their true sentiment because they perceive the interviewer as being sent by the government. We test this explanation by analyzing how respondents perceive the relationship between the surveyor and the government, and how it affects their responses. Along the same lines and in order to alleviate potential sampling biases, we test the sensitivity of our findings to excluding interviews just

---

[1]These measures of future behaviors may express a short-lived frustration and may not translate into actual changes in civic involvement. However, we do not find evidence supporting a return to the pre-protest levels for trust variables few months after the protest.

[2]Note also that revisions in beliefs are stronger when the government does repress the protest while it would be weaker according to this alternative interpretation.

before or just after the protest and we investigate possible changes in the interview protocol around the protest occurrence.

Fourth, we may capture the strong reaction of an informed or biased minority, and we find, indeed, that trust in the leader and institutions sharply decreases for the few individuals having demonstrated. However, we show that trust also decreases within the sample of non-participants. While we do uncover heterogeneity in the citizens' response along ethnic identity, such heterogeneity is not consistent with the usual interpretation of ethnic minorities being repressed by the main ethnic group. We find instead that the major revision in beliefs is recorded among the ethnic majority.

Our analysis also contributes to the literature in several ways. First, our results show that beliefs in the functioning of institutions are volatile and sensitive to social crises. This volatility may contrast with the common view of the literature, which depicts a very high persistence of social capital or trust.[3] We show that one single protest decreases trust in the head of state and institutions by about 50% of the gap that exists between cities and rural areas. We also uncover evidence of changing patterns in prospective civic engagement. The closest paper to ours in this respect is Chong et al. (2011) which show that voters withdraw from the political process and revise downward their beliefs in institutions after learning about corruption cases. Another recent contribution by Grosjean, Ricka and Senik (2013) also reports in a completely different context that beliefs and trust in institutions are sensitive to the economic cycle.

Our empirical investigation contributes to the literature on the impact of conflicts. The focus on protests rather than on "traditional" violent events such as wars or killings, is an original feature of this paper that makes it distinct from recent studies of changes in individuals' attitudes following civil wars or comparably violent events.[4] There is a recent and small literature on the impact of social protests and mass demonstrations but it remains mostly focused on the United States (Collins and Margo 2004, 2007, Madestam and Yanagizawa-Drott 2012, Madestam, Shoag and Yanagizawa-Drott 2013).

One important contribution of our paper is to use disaggregated data on social unrest and to capture a localized response across time and space. It echoes the call by Blattman and Miguel (2010) for local investigation and identification of the consequences of conflicts. Each protest is precisely located and matched with respondents of the survey to isolate

---

[3]On this issue, Nunn (2008) and Nunn and Wantchekon (2011) link today's development failure and distrust in Africa to historical slave trade intensity. Besley and Reynal-Querol (2014) also report long-term effects on trust of historical conflicts in Africa.

[4]See Bellows and Miguel (2009), Blattman (2009), Jaeger et al. (2012), Voors et al. (2012), Cassar, Grosjean and Whitt (2013), Gilligan, Pasquale and Samii (2014), Rohner, Thoenig and Zilibotti (2013), and Becchetti, Conzo and Romeo (2014) among others.

its impact on the local sentiment toward institutions and leaders. Our empirical strategy relies on a regression-discontinuity argument. Indeed, we identify the effect of protests by studying how the difference in priors between individuals interviewed in the surroundings of a protest and their regional peers differs just before and just after the protest occurrence. While the identification assumption—that the interview protocol (including the selection of respondents) is the same just before and just after a protest—is weaker than in a macro-level analysis, the estimate is local and we cannot analyze the spatial diffusion of social discontent over time.

Many theoretical contributions model protests as being informative about the underlying state of the world, and thus indirectly about potential leaders' misbehavior. We provide empirical support that protests may transmit some informational "content" to citizens about their institutions. However, our analysis does not really shed light on the decision to organize a protest or repress it as in Lohmann (1993), Ellman and Wantchekon (2000), Laussel and van Ypersele (2012), and Passarelli and Tabellini (2013), and the role of the media in this process as modeled by Guriev and Treisman (2015) for example.

Finally, a large literature describes the demise of a state: when citizens have low priors on their institutions, they refuse to invest in the public technology and the country falls into a state disband trap (see Besley and Persson 2010, 2011*a,b* among others). We do not provide a test for this mechanism because we are not able to analyze the further response of the state in terms of public investment. By contrast, we do provide empirical evidence that beliefs in institutions and the willingness of citizens to contribute in the state effort or participate in the political process may be undermined by (the information revealed through) protest marches.

The remainder of the paper is organized as follows. In section 2, we briefly discusses the role of protests in the literature and makes our working hypotheses explicit. We then discuss the empirical strategy and findings in section 3. Finally, section 4 concludes.

2 Information transmission through protests

In this section, we review how the theoretical literature models the relationship between a protest, its potential repression and information transmission.

The recent events during the Arab Spring have shown that protests may not only act as coordination devices for groups or unions, or as signals sent to the government. Protests, as salient manifestation of discontent from some groups against the government, may also inform the public about their institutions and their current head of state. A potential

repression by the government may itself carry additional informational content about the institutional environment.

In the empirical framework, we will be able to observe two actions from two different players: the mobilization of a street protest by citizens or political parties and its repression by the government.

There exists a theoretical literature which looks at the role of protests as costly devices to transmit private information to other voters. In Lohmann (1994a) for instance, protests inform the rest of the society about the nature of the regime. Citizens are ready to incur the potential costs because protests could lead to informational cascades and to a change in regime (Kuran 1989, 1991a, Lohmann 1994a,b, Kricheli, Livne and Magaloni 2011, Guriev and Treisman 2015) or a change in policies (Lohmann 1993). In this literature, protests inform other voters about the state of the world, itself often being related to an action of the leader. When there is imperfect information on the leader's type and the existing monitoring institutions, a misbehavior would be sanctioned by a protest organized by the affected or informed citizens. In response, all citizens would aggregate information and revise their beliefs about the leader and the institutional environment.

Protests may also convey information on protesters themselves, when citizens may have perfect information on the regime in power but imperfect signals on all other voters' preferences. Interest groups, by protesting, transmit information on their own preferences to coordinate with other potential protesters, overcome the likely collective action problem and ultimately influence the decision makers (Battaglini and Bénabou, 2003) or generate overthrow through informational cascades (Kuran 1989, Lohmann 1994a, Kricheli, Livne and Magaloni 2011, Ellis and Fender 2011). In these models, a single political action has large consequences because there exists decreasing returns to scale in protesting costs, and voters' preferences are correlated.[5] After a protest, beliefs in future civic engagement would be expected to increase while trust in the state or the head of state would be unchanged.

Most of these models are designed to explain the collective action problem that citizens may be facing in grassroots movements and ignore the existence of structured political parties. In our empirical framework, some protests are not spontaneous and may be at the initiative of a political party. In Ellman and Wantchekon (2000) for instance, political parties are tempted to cause political unrest when the other party is in power and the electoral competition interacts with the capacities of political parties to cause political unrest. In their framework, protests are disruptive threats and citizens would sanction the instigator of the plot and reward the party in power. Repression would be seen as welfare-improving.

---

[5]The theoretical framework of these models hinges on informational cascades or herd behaviors (Banerjee 1992, Chamley 1999, Edmond 2013, Lu, Suen and Chen 2013).

The above mentioned literature examines the problem of voters facing a potentially corrupt or inadequate leader, and deciding to organize a street protest. Another strand of the literature rather considers the decision of a leader facing the threat of revolution, social unrest or intense lobbying from political groups, and deciding whether or not to repress them. Two main aspects of protests pervade this literature. Protests may be well grounded in which case repression is a tool for dominant regimes to impose their political agenda. A repression would then reinforce the negative update in beliefs that follows the protest. Social unrest can also be a disruption from lobbying groups or extremists, and denying nascent opposition would be interpreted in a positive manner by voters.

Among others, Acemoglu and Robinson (2000) and Guriev and Treisman (2015) describes the strategic use of repression for an elite with private objectives conflicting with voters' preferences. In response to street protests, the elite may be willing to incur the costs of repression or nip it in the bud. In Guriev and Treisman (2015), the elite obstruction can either be the ex-post repression of any uprising by the police but the preferred strategy is ex-ante state propaganda or media censoring to avoid protests in the first place.[6] One working hypothesis behind the empirical strategy is that imperfect information transmission generates differential responses by locals and people distant from the protest. However, we will be unable to identify whether such information transmission reflects a political obstruction as in Guriev and Treisman (2015) or whether it derives from exogenous constraints.

Repression can also be a commitment device for benevolent governments to implement public policies. In Passarelli and Tabellini (2013), some citizens or minority groups are always tempted to generate social unrest and prevent reforms from a benevolent government. Leaders and state institutions would be expected to prevent civil disturbances from occurring. A protest would signal that the state is unable to contain the anger of citizens and would highlight a failure of the leader in that respect: (i) security and public order are public goods supposed to be provided by the state, and (ii) some policies may be desirable but blocked by a minority. In this context, and in stark contrast with the previous interpretation, repression would not be sanctioned but rewarded by citizens. Laussel and van Ypersele (2012) and Acemoglu, Egorov and Sonin (2015) incorporate these two aspects of repressions to model their strategic use by governments. Bad and extremists governments are likely to repress all protests, while good or moderate governments may also repress social unrest, but only when organized by lobby groups or extremists.

---

[6]In our dataset, we can observe a repression but not propaganda or media censoring, and non-repressed protests may still be associated with obstructing regimes. However, conditional on social unrest not being avoided, repression by the police would lead to more distrust by citizens in these models and one aspect of our analysis consists in capturing such response.

Empirical evidence presented in this paper allow us to shed some light on these different mechanisms. We observe the evolution of trust in many potential actors (for example, head of state, opposing party, police, etc.) in the aftermath of social unrest, and we provide a suggestive characterization of protests and identify repressed protests or protests that do not engage the government responsibility.

Our empirical strategy is based on two main working assumptions. First, we posit that protests carry information on nationwide institutions or policies. A narrative approach analyzing the protests in our sample reveals that they mostly concern government policy, and we treat separately unrest directed at other actors in an extension.[7]

Second, we assume that the local response (for instance, within a range of 20 kilometers around the protest) is stronger than the average response (for instance, the regional response). While this prediction is not directly discussed in the theoretical literature, it could derive from imperfect transmission of information. If all individuals in a given region react independently of their proximity to a protest, we will not be able to identify the direction of the response in beliefs in our baseline strategy. We will provide some evidence that the response to protests fades away with distance indicating that we may under-estimate the belief update using immediate neighbors as the treated group and regional peers as the comparison group, as the latter seem to react.

3 Empirical evidence

In this section, we present empirical evidence on the evolution of beliefs in the state using respondents interviewed in the Afrobarometer survey.

In many models of civil uprising discussed in the previous section, protests are signals of discontent sent by whistle-blowers who potentially respond to an unobserved fact and reveal some information to citizens about their institutional environment. Our main empirical analysis will quantify the drop in specific indicators of trust in leaders and institutions following a protest. We also present additional results that incorporate two important elements to the analysis: the motive behind the protest and the role of repression by the state. Finally, we explore changes in prospective civic participation.

---

[7]Note that, for most countries in our sample, sub-national governments are accountable to national governments and not to citizens through elections such that local leaders are less likely to be the source of discontent.

3.1 Data and methodology

In this sub-section, we first describe the data sources, document the construction of the local exposure to protests and present the construction of proxies for trust in leader and monitoring institutions. We then present descriptive statistics on the average respondent in the Afrobarometer survey. Finally, we describe the identification strategy, in which we precisely locate respondents of the survey and compare individuals affected differently within the same region. Identification will rely on the variation in exposure to protests coming both from the geographic distance to events, and from the interview timing relative to the protest date.

3.1.1 Data construction

The Afrobarometer is a qualitative survey conducted in 20 African countries. We use rounds 3 and 4 of this survey, conducted between 2005 and 2009, in which we can identify the date of interview and the precise location of respondents. By pooling both rounds, we end up with about 50,000 individuals living in 2,300 districts and 220 administrative regions. Information about districts and localities allow us to locate households precisely inside each region, particularly when we are able, as in the vast majority of cases, to identify the town or village in which interviewees live. The geolocalization process follows Nunn and Wantchekon (2011) and uses websites that associate geographical coordinates to localities throughout the World. A detailed description of the method is presented in the Online Appendix.

The Armed Conflict Location and Event Dataset (ACLED) provides detailed information about conflicts in African countries from 1997 onwards. Available information include the precise geographical coordinates of the event, the identity of actors, the type of event, and the outcome of the conflict. Over the 1997–2009 period, more than 40,000 events are documented and classified along rough categories, i.e., riots and protests, battles, lobbying, peace agreements, violence against civilians, etc. In this paper, we are interested in riots and protests. We refer to these events as *protests* in what follows and interpret them as signals of social unrest against the executive power. Information on potential repression and the motives behind the protest is also available.

We match ACLED protests data and Afrobarometer surveys as follows. We first draw a 20 kilometers radius circle around each interview location and select events which occur inside this area within 120-day period centered on the interview date.[8] This provides us with around 400 protests. We then restrict the sample of interviewees to those living in administrative regions that contain at least one event satisfying the above criterion, which

---

[8]In a robustness check, we associate protests to respondents using a pre-defined grid over the continent.

leaves us with around 11, 200 individual observations from 500 districts, 60 administrative regions and 13 countries.[9] Together with the precise dates of events and interviews, this matching procedure allows us to construct the exposure to both future and recent protests at the individual level.

Except if differently specified, the term "protests" refers to riots and protests occurring within 20 kilometers from the exact interview location in a window of 120 days around the interview, and the term "recent protests" refers to those events occurring during the 60 days before the interview. We also measure local occurrences of other events such as violence against and between civilians. In order to control for the propensity to be affected by protests, we construct past exposure to protest (and past exposure to other types of events reported in ACLED) at the local level as the number of events inside the same area between 2000 and 2003 for respondents interviewed in Afrobarometer round 3, and between 2003 and 2006 for those interviewed in round 4. The precise localization of respondents further allows us to follow Nunn and Wantchekon (2011) and to construct the distance to the coast as control variable for long-run determinants of trust. In addition, we retrieve from the Global Rural-Urban Mapping Project the population within 20 kilometers around each interviewee to account for the fact that attitudes may differ in more populated places and that cities may also be more prone to protests.

Afrobarometer interviewees are asked a series of specific questions regarding politics, religion, and social issues. In particular, the survey includes a series of questions on the respondent's opinions regarding the head of state and institutions, which we use to proxy for beliefs in the state. The common phrasing of these questions is: "How much do you trust each of the following: The president?". Answers are 0 for "not at all", 1 for "a little bit", 2 for "a lot", and 3 for "a very great deal". In order to capture beliefs in the head of state, we use questions related to the "president" and the "ruling party". We use the "opposition party" as a proxy for competing leaders. Beliefs in the quality of monitoring institutions are captured by questions about the "police", the "electoral commission", the "parliament", and "courts of law". We use "local government" to capture the effect on local leaders, and shed light on the extent to which the local effect captured in the empirical strategy may also reflect a response to local politics.

---

[9]The following countries are included in the sample used in this paper: Benin, Ghana, Kenya, Madagascar, Malawi, Mozambique, Namibia, Nigeria, Senegal, Tanzania, Uganda, Zambia, and Zimbabwe. Figure A1, presented in the Online Appendix, maps ACLED protests and Afrobarometer's interview location prior to sample selection.

3.1.2 Descriptive statistics

We first provide some descriptive statistics about the variation in the treatment, i.e., the geographic and temporal distance to protests. The median physical distance between two interviewees of the same administrative region is 110 kilometers, and, in our baseline, we exploit the differential response of individuals in the immediate neighbourhood of a protest (20 kilometers) with their regional peers. Consequently, only 6 to 10% of the territory of a region would be considered as treated on average after one single protest. By contrast, the median time gap between two interviews within the same region is about 3 days (with a standard deviation of 5 days). In the vast majority of instances, individuals of the same region are either all interviewed after the protest, or all interviewed before the protest, and there is little time variation in their interview dates.

Table 1 presents some descriptive statistics about the sample of Afrobarometer interviewees. Their profile is representative of the surveyed countries with a majority of rural and uneducated residents. The right part of Table 1 focuses on interviewees in the immediate proximity of a protest (within 20 kilometers) during the 120-day window centered on the interview date and compares respondents' characteristics depending on whether the protest occurs *before* or *after* the interview. This comparison gives some indications about our findings: places in which protests occur before or after an interview are comparable along observable characteristics, but indicators of trust in leaders and in institutions tend to be lower among the sample of interviewees whose interview occurs just after a protest.[10]

Figure 1 illustrates the differential response along the timing of interviews by plotting the average trust in the president reported by respondents before and after a protest. On the left part of the figure, each dot represents the average trust in the president as reported by respondents interviewed $t = 1, \ldots, 60$ days before a protest, conditional of no protest having taken place within a 20 kilometers radius during the 60 days preceding the interview. On the right part of the figure, each dot represents the average trust in the president as reported by respondents interviewed $t = 1, \ldots, 60$ days after a protest has taken place within a 20 kilometers radius. A sharp discontinuity appears at the interview date. Trust in the president is significantly lower after a protest than just before, and there are no obvious signs of anticipation by respondents.[11] This approach provides suggestive evidence of the

---

[10]Note, however, that respondents living in the proximity to a recent protest march are different than the average sample respondent within the same region: protests are more likely to occur in urban areas and/or where people are more educated. Such places are populated by individuals who are likely to have intrinsically different opinions towards politics, and indeed, reported trust in leaders and institutions appears to be slightly lower in these places than on average (see column 4 compared to column 1).

[11]In addition to the discontinuity, we can notice that the dispersion of responses decreases as if the protest had helped respondents to refine their posteriors compared to their priors. Analyzing this phenomenon is however beyond the scope of this paper.

Table 1: Descriptive statistics: full sample, and comparison between interviewees before/after a protest.

	Full sample		Interviewed after a protest	Interviewed before a protest	P-value of difference
	Mean	(Std dev.)			
Trust in the president	1.66	(1.15)	1.29	1.59	0.00
Trust in ruling party	1.43	(1.15)	1.10	1.33	0.08
Trust in opposition party	1.20	(1.05)	1.16	1.10	0.42
Trust in police	1.38	(1.15)	1.22	1.29	0.11
Trust in electoral commission	1.41	(1.13)	1.09	1.27	0.11
Trust in parliament	1.54	(1.08)	1.28	1.39	0.16
Trust in local government	1.45	(1.09)	1.21	1.31	0.45
Age	34.31	(13.54)	32.32	33.09	0.20
Male	0.50	(0.50)	0.50	0.50	0.35
Household head	0.50	(0.50)	0.42	0.45	0.07
White	0.00	(0.06)	0.00	0.02	0.18
Mixed	0.01	(0.09)	0.02	0.03	0.16
Islam	0.14	(0.34)	0.18	0.21	0.87
Catholic / Protestant	0.80	(0.40)	0.77	0.76	0.73
Traditional religion	0.01	(0.11)	0.01	0.02	0.08
Rural housing	0.56	(0.50)	0.19	0.33	0.57
Primary school	0.35	(0.48)	0.22	0.24	0.96
Secondary school	0.40	(0.49)	0.51	0.44	0.74
Post-secondary school	0.14	(0.35)	0.22	0.20	0.35
Unemployed	0.31	(0.46)	0.36	0.26	0.12
Part-time	0.17	(0.38)	0.16	0.22	0.26
Full-time	0.22	(0.42)	0.24	0.29	0.87
Main ethnic group in region	0.58	(0.49)	0.52	0.48	0.05
Main ethnic group in country	0.37	(0.48)	0.35	0.32	0.01
Distant past protests	5.60	(13.14)	17.16	11.89	0.45
Distance to the coast	5.55	(1.51)	5.16	4.87	0.91
Local population	10.04	(5.43)	13.18	12.72	0.17
Observations	11,200		2,461	1,032	

The *full sample* is made of all respondents from Afrobarometer rounds 3 and 4 who live in an administrative region where at least one protest occurred within 60 days to or from an interview conducted in this region. Respondents *interviewed after a protest* are those interviewed at most 60 days since a protest occurred in the 20-km radius circle from their location. Respondents *interviewed before a protest* are those interviewed at most 60 days before a protest occurred in the 20-km radius circle from their location and for which no protest occurred over the 60 days before the interview in the 20-km radius circle from their location. See the text for the definitions of the different variables. Except *trust in ...* variables, *age*, *past protests*, *distance to the coast* (log), and *local population* (log), all variables are dummy variables. The reference category for *white*, and *mixed* is "black". The reference category for education levels is "no formal education". The reference category for employment status is "inactive". The reference category for religion is "none".

negative and sudden impact of protests on trust in leaders, and the empirical strategy will build on this discontinuity.

Figure 1: Average trust in the president as a function of time between interview and protest.
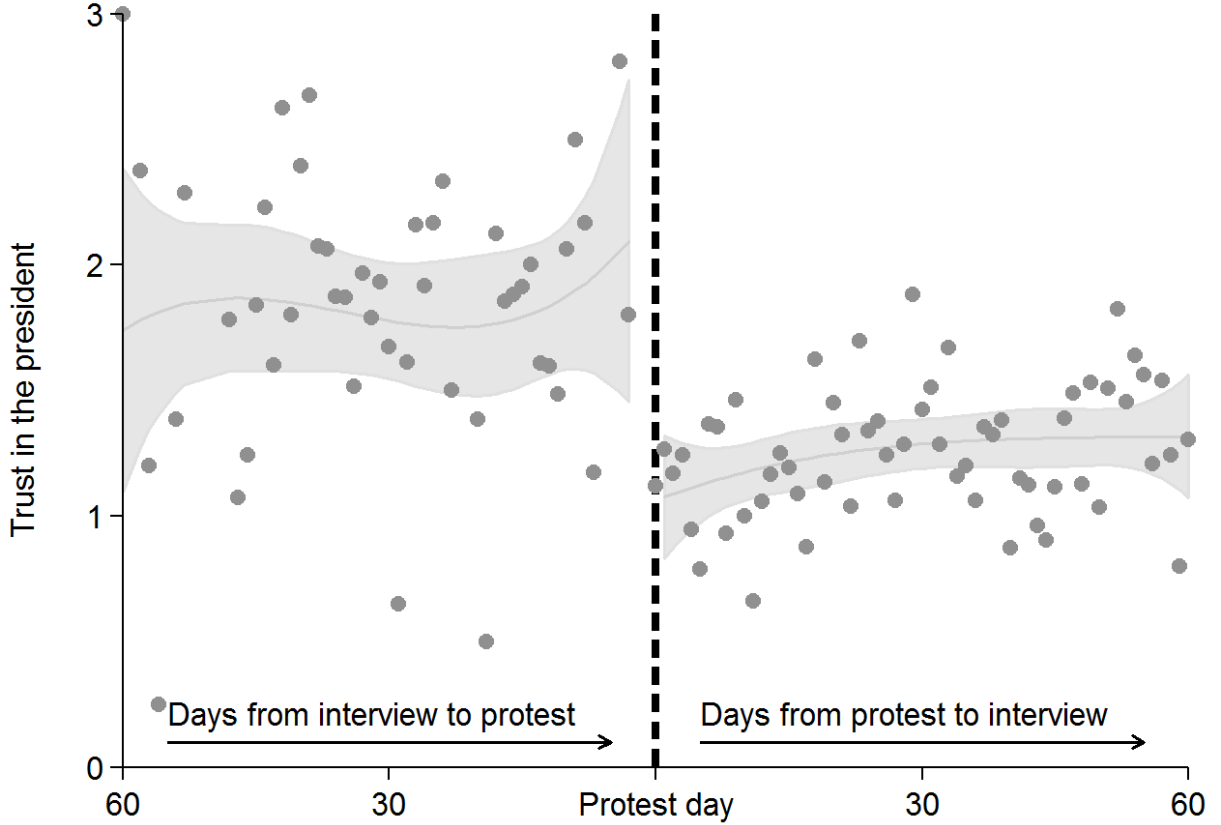
Sources: Afrobarometer and ACLED. On the left part of the figure, each dot represents the average trust in the president for respondents living in places where a protest will take place $t = 1, \ldots, 60$ days after the interview, conditional on no protest having taken place in the 60 days preceding the interview. On the right part of the figure, each dot represents average trust in the president reported by respondents living in places where a protest has taken place $t = 1, \ldots, 60$ days before the interview. Lines and grey areas are estimates and 95% confidence intervals of cubic splines.

3.1.3   Estimation strategy

The ideal experiment would identify the impact of protests from variation in interview dates in the same location before and after the occurrence of a protest. However, as explained above, the survey design does not allow for this strategy, and the exact same location cannot be observed just before *and* just after a protest. Our empirical strategy will instead consist in interacting time and spatial variations to isolate a causal impact across different locations.

We first define a spatial treatment by assigning a protest to respondents who live within a 20 kilometer radius, and are interviewed in a 120-day window centered on the protest. The control group will be respondents of the same region who are interviewed in the same time

bracket but live outside the 20 kilometer radius.[12] The time treatment will be defined as being interviewed in the 60-day window *after* the protest march.

The identification comes from the interaction of the spatial treatment and the time treatment. Indeed, locations in the proximity to protests are likely to be selected on unobserved characteristics. For instance, protests are likely to be a by-product of a long-run deleterious environment with low trust in the state and institutions. In order to correct for such selection, we will also use respondents interviewed just before the occurrence of a protest and compare those within to those outside a 20 kilometer radius from the future protest. Under the assumption that the interview protocol is orthogonal to the occurrence of a protest, the difference—between recent and future protests—in these within-region differences in beliefs can be interpreted as the causal (local) effect of a protest.[13]

In order to estimate how reported priors in leaders and institutions evolve in the aftermath of protests, we estimate the following difference-in-differences model using ordinary least squares:

$$y_{it} = \alpha + \beta_r \mathbb{R}_{jt} + \beta_p \mathbb{P}_{jt} + \sum_{k=1}^{n} \gamma_k \mathbf{x}_i^k + \sum_{k=1}^{m} \phi_k \mathbf{X}_j^k + \mathbb{I}_r + \varepsilon_i, \tag{1}$$

where individual i lives in location j inside administrative region r and is interviewed on day t. In the baseline specification, the variable y denotes the answer to one of the trust questions presented above. $\mathbb{R}_{jt}$ is the number of protest within 20 km of place j during 60 days immediately preceding the interview conducted at date t. $\mathbb{P}_{jt}$ is the number of protest within 20 km of place j during the 120 days around the interview conducted at date t. Vector $\mathbf{x}$ is a set of observable characteristics of individual i. Vector $\mathbf{X}$ is a set of observable characteristics of place j such as riots and protests in the past three years, distance to the coast, population density, etc. $\mathbb{I}_r$ is a region×round fixed effect for each first-level administrative country subdivision and each of the two survey rounds of the Afrobarometer, and ε is the error term. We estimate equation (1) using ordinary least squares and correct standard errors for spatial correlation following Conley (1999, 2008), Hsiang (2010) and Berman et al. (2017) by using a diffusion parameter of 100 km, which is the median distance between respondents within administrative regions. Parameter β_p of equation (1) captures how attitudes differ within a region depending on the proximity to future protests. Parameter β_r is the causal effect of a recent protest.

---

[12]The addition of this control group in the empirical sample will allow us to control for region×survey wave fixed-effects and include a large set of control variables.

[13]Note, however, that this effect is the local difference in trust associated with the protest occurrence. If control units are not affected by the protest rally, it can be interpreted as the average effect of a protest on the treated population. If, instead, control units are affected to a lesser extent by the treatment as we will show in a robustness check, the estimate will be a lower bound for the average effect of protests on the treated population.

Figure 2: Daily empirical probability of a protest before and after the date of interview.
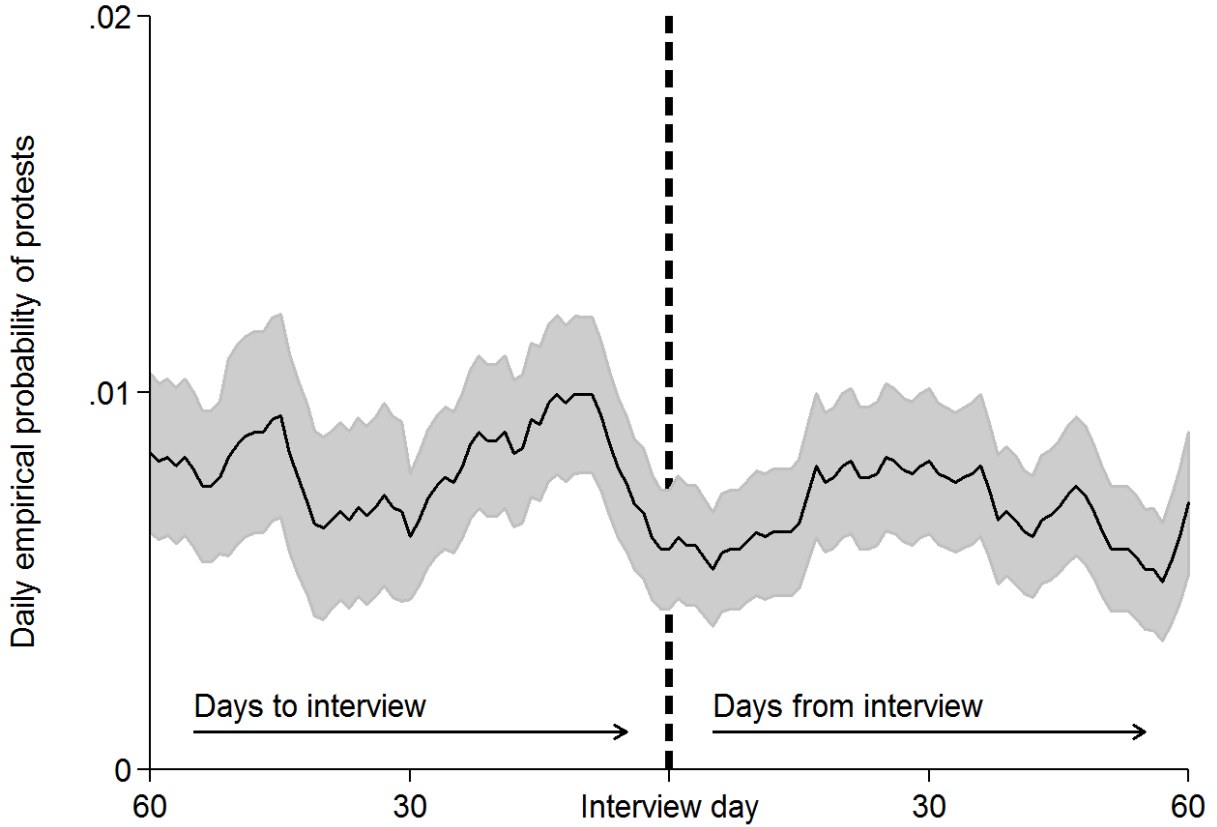
Our empirical strategy crucially relies on the assumption of orthogonality between the interview protocol and the protest occurrences. We have shown in Table 1 the equivalent of a balance test, displaying no differences in permanent characteristics between the respondents in the immediate neighborhoods of future protests and recent protests. We now provide additional tests supporting this hypothesis.

A first threat to identification would be that the decision to conduct interviews is affected by current protests or by unobserved characteristics correlated with the occurrence of protests. Concretely, (i) scheduled interviews may be postponed or canceled after or just before mass protest rallies, and (ii) interviewees may be differently selected before and after the protest. In order to test whether there is a differential propensity to conduct interviews before or after a protest, Figure 2 displays the daily empirical probability of a protest during

the 120-day window centered on the interview. The right-hand side of the threshold depicts protests occurring after the interview. The likelihood of a protest seems to be quite independent of the date of interview.[14] If anything, there is a hump just before the interview showing that more respondents are interviewed just after protest rallies. We will provide evidence that our main empirical results are robust to the exclusion of interviews conducted a week before or after protests.

Another threat to identification would be that protests modify the way interviews are run. To show that the interview protocol does not differ before and after a protest, we rely on two distinct strategies. While respondents interviewed just before and after a protest have similar observed characteristics, they may be subject to different interview protocols. We thus estimate specification (1) with left-hand term variables that characterize the interview protocol and the selection of interviewees, and report the results in Online Appendix Table A1. In column 1, we use a dummy equal to 1 if the respondent has admittedly participated in a demonstration or protest march during the past year.[15] In column 2, we generate a dummy if the respondent thinks that the government sent the interviewer.[16] In columns 3 to 5, we use the number of calls needed to ask households to be interviewed, a dummy capturing whether the household refused the interview during the first call (less than 5%), and whether the field supervisor back-checked the interview (one eighth of interviews are supposed to be back-checked by a field supervisor to ensure that interviewers do follow the survey protocol). We find no differences in the household willingness to answer (columns 3 and 4), in quality standards (column 5), or in the perceived role of the surveyor (column 2). Unsurprisingly, recent protests increase the likelihood for having demonstrated during the past year, but the effect is quantitatively small. All in all, these findings reduce concerns about systematic biases in interview selection just after protests.

3.2   Results

This sub-section presents the main empirical evidence. We first focus on the evolution of trust in leaders in power and in monitoring institutions. Next, we discuss threats to identi-

---

[14]This observation is supported by formal statistical tests such as local tests of equality of the means.

[15]Respondents are asked whether they "[personally have] attended a demonstration or protest march [during the past year]". We create a dummy variable equal to 1 if the respondent answered positively and 0 otherwise. Note that this observation remains imperfect as we have no way to assess whether those who declare that they attended a protest actually attended the most recent local event.

[16]This question reads as follows: "Just one more question: who do you think send us to do this interview?". This is an open question to which respondents can answer anything. Answers vary from "no one" to "god or a religious organization" and include the "government", "newspapers", or a "non-governmental organization". We create a dummy variable that is equal to 1 if the respondent declares that the interviewer was sent by any organized body linked to the country's government, and 0 otherwise.

fication and alternative interpretations. We then provide two sets of results exploiting the heterogeneity in the treatment: one based on a narrative approach to characterize protests, and one exploiting possible repression from the state. Finally, we explore some questions indicative of future civic behaviors.

3.2.1 Main results

In what follows, the estimated model is always given by equation (1) and only the dependent variable varies.

The top panel of Table 2 presents estimates of the relationship between protests and trust in the current head of state (president and ruling party), institutions with some monitoring power (police, electoral commission, parliament and courts of law) and local leaders (local government) conditioning only for region×round fixed effects. The bottom panel presents mirroring estimates cleaned for the observable characteristics presented in Table 1.[17]

While most estimated coefficients keep a comparable order of magnitude, introducing these individual- and location-level covariates captures some differences across neighborhoods along their general exposure to protests. In particular, conditional on covariates, respondents in the proximity to future protests are not significantly different than their regional peers. The selection bias—captured by β_p, the coefficient in front of *Protests*—is not significantly different from 0.

The causal impact of recent protests, β_r, is estimated to be negative and statistically significant for trust in the president, the ruling party, the police, the electoral commission, the parliament and courts. The only effect that is positive (although not statistically different from 0) is the one obtained when the dependent variable is trust in the opposition party (column 3), which could be a potential alternative to incumbent leaders.

To summarize, trust in current head of state, as captured by the president and the ruling party, deteriorates if a protest occurs in a respondent's neighborhood during the two months that precede the interview. Evidence also speak in favor of a negative revision of beliefs in the quality of monitoring institutions such as the police, the electoral commission and the parliament once a signal of social unrest has been observed.[18] By contrast, trust in the local government appear to be stable following protests.

---

[17]Estimated coefficients of covariates are presented in the Online Appendix Table A2.

[18]Interpreting these estimates as proxies for trust in underlying institutions (independently of the leader identity) is not straightforward. Indeed, respondents may distrust the current parliament members but not the role of the parliament as an institution. Furthermore, it is difficult to untangle in our results what comes from the incapacity of institutions to monitor the leader (a passive role) and what comes from their active role in the repression. The police is indeed likely to be one of the actors of the repression and courts can punish the leader but also be used by the executive power to punish protesters.

Table 2: Relationship between recent protests and trust in leaders and institutions.

Baseline regressions without covariates

Trust in ...	(1) Presid.	(2) Ruling party	(3) Opposit. party	(4) Police	(5) Elect. comm.	(6) Parl.	(7) Courts of law	(8) Local govern.
Recent protests	-0.087*** (0.029)	-0.070*** (0.020)	0.013 (0.022)	-0.083*** (0.019)	-0.067*** (0.024)	-0.056** (0.027)	-0.059** (0.024)	-0.032 (0.020)
Protests	0.017 (0.013)	0.010 (0.012)	0.001 (0.011)	0.029** (0.012)	0.005 (0.012)	0.011 (0.014)	0.017* (0.009)	-0.005 (0.015)
Observations	10,868	10,781	10,541	10,990	10,437	10,655	10,720	10,630

Baseline regressions with covariates

Trust in ...	(9) Presid.	(10) Ruling party	(11) Opposit. party	(12) Police	(13) Elect. comm.	(14) Parl.	(15) Courts of law	(16) Local govern.
Recent protests	-0.082*** (0.028)	-0.060*** (0.021)	0.013 (0.022)	-0.051** (0.020)	-0.056** (0.024)	-0.054** (0.026)	-0.050** (0.021)	-0.019 (0.020)
Protests	0.009 (0.017)	0.005 (0.015)	0.004 (0.012)	0.019 (0.013)	-0.007 (0.014)	0.011 (0.015)	0.020* (0.010)	-0.008 (0.013)
Observations	10,043	9,999	9,803	10,159	9,688	9,899	9,937	9,837

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in columns heading. Standard errors computed for a 100-km spatial adjustment following Conley (1999) are in parentheses. Each column displays the estimate of a separate OLS regression. All regressions include region × round fixed effects and a constant term. The following covariates are included in columns 9–16: age, gender, a dummy equal to 1 if the respondent is household head, dummies for race and religion, a dummy equal to 1 if housing is rural, dummies for education level and employment status, dummies equal to 1 if the respondent belong to the main or the second ethnic group in country or region, distant past violent events, protests and repressed protests in a 20-km radius, the (log of) the distance to the coast, and the (log of) population in a 20-km radius. See Table A2 in the Online Appendix for covariates' estimated coefficients. *Recent protests* is the number of protests in a 20-km radius over the 60 days preceding the interview. *Protests* is the number of protests in a 20-km radius over the 60 days following and preceding the interview. See the text for the definition of dependent variables.

As regards the amplitude of estimated effects, one more protest during the two months preceding the interview reduces trust in the president by 12% of the gap in trust in the president between places that never experience protests and those that do. The effect of one protest is equivalent to 70% of the gap of trust in the parliament between rural and urban areas. The immediate impact of protests on trust in leaders and institutions is sizable and of the same order of magnitude as long-run heterogeneity across locations or individuals with different characteristics (e.g., the gap in reported trust due to gender). In other words, while beliefs are likely to be heavily rooted, these findings show that they are also frequently updated and strongly affected by sudden signals. Beliefs are not only static, they have a substantial dynamic component.

3.2.2 Robustness checks

In this subsection, we discuss the sensitivity of our analysis (i) to varying sample choices and definitions for treated and control units, (ii) to heterogeneous responses across interviewees (e.g., to explore the role of information and behavioral responses), (iii) to alternative spatial correlation corrections and additional controls capturing time-varying confounding factors (e.g., seasonal effects).

In a first step, we focus on sample choices and definitions for treated and control units. In Table 3, we explore the robustness of our estimates to alternative sample choices. In the first panel, we include all administrative regions irrespectively of the possible occurrence of a protest in a 120-day window (but conditional on at least one protest being observed in the country in the 120-day window). In the second panel, we only keep respondents in the proximity to at least one protest between 2000 and 2003 (resp. 2003 and 2006) for those interviewed in 2005 (resp. 2008). In the third panel, we exclude respondents interviewed within 7 days of a protest, as they may be more susceptible to be differently selected by surveyors. The results are qualitatively similar to the baseline specification in the three cases. However, standard errors are larger in the second and third robustness checks, possibly due to the lower number of observations.[19]

In the fourth panel of Table 3, we provide a sensitivity analysis to the chosen time bracket for the treatment assignment and the sample choice. We replicate our empirical strategy, but replace the 120-day window, which defines the sample of affected regions and treated respondents, by a 60-day window. As in the previous cases, the coefficient for trust in leaders remain qualitatively similar with a smaller time window, even though less precisely estimated. There is one difference with Table 2 which is worth mentioning: the estimates for monitoring institutions are smaller, possibly indicating a lag in the belief revision. One tentative interpretation could be that monitoring institutions are also evaluated on the basis of their actions in the aftermath of protests, and these actions require some time to be correctly identified by citizens.

In the baseline specification, we stratify the sample by administrative regions, i.e., we select regions with at least one protest in a 120-day window, and assign treatment within each region on the basis of the individual physical distance to protest occurrences. There are two possible concerns with this approach. First, the spatial clustering of treatment may change over time. Two locations simultaneously assigned to treatment after one protest may not be grouped together after another protest. Second, administrative borders may not be

---

[19]The sensitivity analysis reported in the second panel of Table 3 illustrates the tension underlying our empirical strategy: the inclusion of respondents within regions affected by protests but not in their proximity helps refine the estimation of region×round fixed effects.

Table 3: Sensitivity analysis with different samples, time windows and spatial grouping.

Using all available observations

Trust in ...	(1) Presid.	(2) Ruling party	(3) Opposit. party	(4) Police	(5) Elect. comm.	(6) Parl.	(7) Courts of law	(8) Local govern.
Recent protests	-0.063**	-0.056***	0.008	-0.068***	-0.069***	-0.057**	-0.066***	-0.032
	(0.026)	(0.021)	(0.022)	(0.019)	(0.021)	(0.025)	(0.025)	(0.021)
Observations	26,985	26,683	26,084	27,123	25,732	26,351	26,458	26,325

Only locations with protests in the distant past

Trust in ...	(9) Presid.	(10) Ruling party	(11) Opposit. party	(12) Police	(13) Elect. comm.	(14) Parl.	(15) Courts of law	(16) Local govern.
Recent protests	-0.064*	-0.052*	-0.010	-0.074***	-0.059**	-0.072*	-0.029	-0.020
	(0.033)	(0.028)	(0.023)	(0.020)	(0.028)	(0.038)	(0.022)	(0.027)
Observations	4,934	4,907	4,817	4,999	4,786	4,871	4,907	4,795

Excluding interviews within +/- 7 days of protest

Trust in ...	(17) Presid.	(18) Ruling party	(19) Opposit. party	(20) Police	(21) Elect. comm.	(22) Parl.	(23) Courts of law	(24) Local govern.
Recent protests	-0.067*	-0.054**	-0.007	-0.038	-0.065**	-0.052*	-0.030	-0.021
	(0.036)	(0.027)	(0.022)	(0.025)	(0.025)	(0.031)	(0.023)	(0.025)
Observations	9,163	9,123	8,934	9,262	8,836	9,029	9,056	8,995

Using a 60-day window

Trust in ...	(25) Presid.	(26) Ruling party	(27) Opposit. party	(28) Police	(29) Elect. comm.	(30) Parl.	(31) Courts of law	(32) Local govern.
Recent protests	-0.076**	-0.064**	0.043***	-0.012	-0.043	0.005	-0.008	-0.019
	(0.030)	(0.032)	(0.016)	(0.020)	(0.035)	(0.023)	(0.021)	(0.027)
Observations	6,813	6,780	6,640	6,886	6,583	6,720	6,755	6,647

Using rectangular grids to define treatment

Trust in ...	(33) Presid.	(34) Ruling party	(35) Opposit. party	(36) Police	(37) Elect. comm.	(38) Parl.	(39) Courts of law	(40) Local govern.
Recent protests	-0.107***	-0.066**	0.033	-0.035	-0.099***	-0.037	-0.038	-0.054*
	(0.041)	(0.033)	(0.041)	(0.038)	(0.028)	(0.034)	(0.031)	(0.032)
Observations	6,372	6,299	6,226	6,401	6,052	6,212	6,254	6,207

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in columns heading. Standard errors computed for a 100-km spatial adjustment following Conley (1999) are in parentheses. Each column displays the estimate of a separate OLS regression. All regressions include region × round fixed effects, a constant term, *protests* as defined in Table 2 and all covariates included in the bottom panel of Table 2. *Recent protests* is the number of protests in a 20-km radius over the 60 days preceding the interview. *Protests* is the number of protests in a 20-km radius over the 60 days following the interview. In columns 25–32, the sample construction procedure uses a 60-day window around the interview. In columns 33–40, the sample construction procedure described in the text has been repeated using pre-defined 0.4375-degree cells to match respondents and protests, and 1.75-degree arbitrary cells in lieu of administrative regions. In this panel only, standard errors are clustered at the 0.4375-degree cell level.

relevant to define control groups, or a respondent may be in the proximity of a protest but in another administrative region, and thus not in-sample. In the last panel of Table 3, we tackle both issues, and replace (i) administrative regions by cells of a rectangular grid over the continent and (ii) treatment units by cells of a smaller grid. We choose large cells of 1.75-degree to match the average region size in the sample, and smaller cells of 0.4375-degree such that there are 4×4 small areas within each newly-defined region. We then replicate specification (1) with three changes. First, protests occurring within a small rectangular area will be assigned to all respondents living in this area. Second, region fixed effects are replaced by dummies for the larger grid and we restrict the analysis to the synthetic regions that have at least one protest within +/- 60 days of an interview. Third, standard errors are clustered at the smaller grid level. As apparent in the bottom panel of Table 3, estimates are qualitatively similar to our baseline specification.[20]

The baseline strategy cleans for the observed differences with regional peers outside of a 20 kilometer range from the protest. However, these respondents may be affected even though our baseline estimate shows that they respond less than treated individuals. In Online Appendix Table A3, we provide evidence that the response to protests is stronger close to the focal point and fades away with distance. We consider additional treatments for being within a ring bounded by 20 and 40 kilometers radius circles (about 15% of a region) and within a ring bounded by 40 and 80 kilometers radius circles (about 45% of a region). Coefficients are smaller for these outer circles, suggesting that our baseline estimate is a lower bound for the belief revision incurred by individuals exposed to a protest. More generally, two interpretations could arise behind this imperfect propagation of belief revision. First, the access to information may restrict the reach of a signal which should be of interest for any citizen in the country—national grief, local reach. Second, the information is spread across the country but is mostly relevant for certain locations—local grief, national reach.

In a second step, we explore possible sources of heterogeneity across respondents. In the Online Appendix Table A4, we estimate the coefficient of interest on different samples to check whether interviewees' responses vary along education, location (rural, urban), information or ethnic groups. While we observe the same qualitative results across groups, the response is larger among uneducated respondents, urban residents and members of the main ethnic group in the country . This observation contradicts the interpretation that social conflicts reflect ethnic disagreements with leaders favoring systematically one of the party as

---

[20]The number of observations is markedly lower in the grid exercise because we stratify the treatment using different units than the survey protocol. Indeed, assume that there is a protest in an administrative region overlapping with two large rectangular grids, and suppose that neighboring regions are not surveyed at the same time by Afrobarometer or not surveyed at all. In such case, only the respondents in one of the two grids will be included in the sample versus all regional respondents in the benchmark.

leaders are mostly from the dominant group. We do not see relevant differences depending on whether the respondent is politically informed or not—proxied by whether she can name her local Member of Parliament or not.

In Table 4, we further analyze the heterogeneity across respondents along their perception of the survey motives or proximity to whistle-blowers. People may strategically conceal their true beliefs in a context of social protests. In the Afrobarometer survey, two questions allow us to identify (i) individuals susceptible to tone down their criticisms of government policy, and (ii) possible participants in the protest. In columns 1–8 of Table 4, we show that the revision of beliefs after a protest is not attenuated for respondents mistakenly associating Afrobarometer surveyors with the government.[21] In columns 9–16 of Table 4, we test whether the drop in trust is circumscribed to the minority of protesters. While recent protesters do revise their priors more abruptly, those who did not attend any protest during the past year also express lower trust toward leaders and institutions in the aftermath of protest and the revision of active participants is only responsible for a small share of the average drop.

In a third step, we further explore the robustness of our estimates to alternative corrections for spatial correlation and to controls capturing time-varying confounding factors (e.g., seasonal effects). In the Online Appendix Table A5, we show the sensitivity of the results to the exact specification of the spatial-autocorrelation correction. We use 20, 50, 150 and 250 kilometers as the parameter for spatial diffusion. Our results remain statistically significant even in the most stringent specification, and the correction for 100 kilometers adopted in the baseline is quite conservative. In the Online Appendix Table A6, we use more precise controls for the timing of the interview and we account for seasonal or cyclical effects. In the most stringent specification (last panel), we control for region/month fixed effects and thus clean for possible differences in interview timing within the same region, in particular between treated individuals and the control group. The estimates are left unchanged, which confirms that the (small) existing variation in the length of the survey protocol across regions does not correlate with the occurrence of a protest.

An important robustness check would consist in considering placebo protests or, more generally, investigating whether respondents react to protests which directly question government policy. We turn to the heterogeneous response along the type of protests in the next section.

---

[21]We have shown in Table A1 that there is no correlation between the exposure to a protest and the likelihood to declare that the interviewer was sent by the government. In unreported specifications, we also investigate whether respondents were more or less likely to refuse to answer this (and other) question(s) following protests but do not uncover any meaningful correlations.

Table 4: Relationship between recent protests and trust in leaders and institutions: perception of interviewer's principal and participation in protests.

Trust in ...	(1) Presid.	(2) Ruling party	(3) Opposit. party	(4) Police	(5) Elect. comm.	(6) Parl.	(7) Courts of law	(8) Local govern.
Recent protests	-0.096***	-0.053**	0.019	-0.062***	-0.060**	-0.060**	-0.054**	-0.032
	(0.033)	(0.025)	(0.023)	(0.020)	(0.025)	(0.026)	(0.023)	(0.022)
Int. perception	0.005	0.076***	-0.001	0.026	0.030	0.030	0.062**	0.041
	(0.025)	(0.023)	(0.026)	(0.027)	(0.025)	(0.023)	(0.025)	(0.030)
Recent protests	0.020	-0.012	-0.010	0.015**	0.006	0.007	0.004	0.017
× Int. perception	(0.015)	(0.014)	(0.019)	(0.006)	(0.008)	(0.009)	(0.009)	(0.012)

Trust in ...	(9) Presid.	(10) Ruling party	(11) Opposit. party	(12) Police	(13) Elect. comm.	(14) Parl.	(15) Courts of law	(16) Local govern.
Recent protests	-0.075***	-0.047**	-0.003	-0.042**	-0.052**	-0.049*	-0.047**	-0.017
	(0.029)	(0.023)	(0.021)	(0.021)	(0.023)	(0.029)	(0.021)	(0.020)
Demonstrated	-0.096**	0.055	0.070*	0.097**	-0.002	-0.000	0.023	0.048
	(0.043)	(0.043)	(0.036)	(0.038)	(0.046)	(0.041)	(0.035)	(0.046)
Recent protests	-0.007	-0.043**	0.024**	-0.041***	-0.035**	-0.005	-0.016	-0.006
× Demonstrated	(0.013)	(0.020)	(0.011)	(0.011)	(0.015)	(0.014)	(0.010)	(0.021)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in columns heading. Standard errors computed for a 100-km spatial adjustment following Conley (1999) are in parentheses. Each column displays the estimate of a separate OLS regression. All regressions include region × round fixed effects, a constant term, *protests* as defined in Table 2 and all covariates included in the bottom panel of Table 2. *Recent protests* is the number of protests in a 20-km radius over the 60 days preceding the interview. *Int. perception* is a dummy variable equal to one if the respondent thinks that the interviewer has been sent by any governmental body. *Demonstrated* is a dummy variable equal to one if the respondent declares having participated in a demonstration over the past year. See the text for the definition of dependent variables. For each regression, the number of observations is the same as for the corresponding dependent variable's regression displayed in the bottom panel of Table 2.

3.2.3 Placebo events, motives behind protests and repression

We first study the response of citizens to violent events indirectly related to national government policy (e.g., battles between non-governmental bodies, cases of ethnic violence, football fans protests) and which are not classified as protests by ACLED. We then focus on political protests and propose a narrative investigation of the motives behind protests. More specifically, we use qualitative information available in ACLED and separate protests that are directed at leaders' actions from those unrelated to public policy. Finally, we distinguish protests repressed by authorities from non-repressed protests.

In the first row of Table 5, we replicate our empirical strategy with *violent events* instead of protests. We use the ACLED categories "violence against civilians" and "battles" to

construct a measure of exposure to violent events. An event is classified as "violence against civilians" in ACLED when it involves the attack of unharmed citizens by a third party (for example, an attack of a civilian bus by a rebel group, ethnic violence between citizens or violence between football fans). These outbursts of violence should not convey the same information as political protests and Table 5 does confirm this insight. These *violent events* do not lead to the same systematic revision of beliefs in leaders or institutions.[22]

In the second panel of Table 5, we isolate political protests which question national policy from riots related to international shocks, private actors or commemorations. To achieve this, we classify a protest as *rooted* if the ACLED description of the event or the original media source includes information about any grievance against leaders in charge (e.g., protests initiated by pensioners against the non-payment of pensions). *Non-rooted* protests are those directed at "international" economic or political shocks (e.g., rise in World food prices, protests against the US foreign policy), at private entities (e.g., closure of foreign-owned plants) or commemorating historic events.[23] Interviewees should learn little from the latter about national leaders and institutions. As shown in the second row of Table 5, our main findings are mostly driven by rooted protests. If anything, non-rooted protests are followed by a slight improvement in the citizens' priors about their government and institutions.

In the last panel of Table 5, we focus on protests *repressed* by the government, the ruling party, the official army, or the police. If leaders are more likely to repress a protest when they expect to face severe consequences, the revision in reported trust should be larger when citizens observe repression by the authorities. Consistent with this interpretation, Table 5 shows that the drop of trust in leaders is much larger when protests are repressed by the government than when they are not. Similarly, trust in institutions with a monitoring power over leaders should decrease more in the aftermath of repressed protests. When we focus on such protests, the response of trust in all monitoring institutions is one order of magnitude stronger.

One might argue that this result comes from the fact that repressed events occur in more central locations than those that are not repressed. While we cannot measure the relative importance of repressed protests versus non-repressed ones, one observation helps us reduce concerns about this interpretation. Relying on the premise that urbanity or population

---

[22]Distinguishing between battles and violence against civilians, we find that the former are not at all correlated with reported trust variables, while the latter have a small negative effect on trust in leaders and the police. This would be consistent with violence against civilians being interpreted as signals that incumbent leaders are not able to protect civilians, which would be less related to the efficiency of monitoring institutions.

[23]See the Online Appendix for a description of the narrative approach to classify protests into *rooted* and *non-rooted* ones.

Table 5: Relationship between (i) violent events, (ii) protests questioning government policy, (iii) repressed and non-repressed protests, and trust in leaders and institutions.

Trust in …	(1) Presid.	(2) Ruling party	(3) Opposit. party	(4) Police	(5) Elect. comm.	(6) Parl.	(7) Courts of law	(8) Local govern.
Violent events	-0.038	-0.049	0.043*	-0.046*	-0.034	-0.006	0.007	-0.022
	(0.061)	(0.033)	(0.023)	(0.024)	(0.042)	(0.030)	(0.044)	(0.038)

Trust in …	(9) Presid.	(10) Ruling party	(11) Opposit. party	(12) Police	(13) Elect. comm.	(14) Parl.	(15) Courts of law	(16) Local govern.
Rooted protests	-0.090**	-0.051**	-0.008	-0.026	-0.053**	-0.059	-0.027	-0.056**
	(0.041)	(0.023)	(0.032)	(0.031)	(0.026)	(0.043)	(0.049)	(0.024)
Non-rooted protests	0.035	0.056***	-0.041**	-0.020	0.077***	0.010	0.008	0.023
	(0.028)	(0.021)	(0.021)	(0.023)	(0.025)	(0.024)	(0.030)	(0.022)

Trust in …	(17) Presid.	(18) Ruling party	(19) Opposit. party	(20) Police	(21) Elect. comm.	(22) Parl.	(23) Courts of law	(24) Local govern.
Repressed protests	-0.390***	-0.392***	0.140	-0.238***	-0.408***	-0.162**	-0.243***	-0.256***
	(0.116)	(0.102)	(0.087)	(0.070)	(0.107)	(0.076)	(0.084)	(0.056)
Non-repressed protests	-0.058*	-0.038	0.001	-0.069***	-0.034	-0.045	-0.042*	-0.010
	(0.034)	(0.024)	(0.019)	(0.021)	(0.023)	(0.031)	(0.022)	(0.023)

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in columns heading. Standard errors computed for a 100-km spatial adjustment following Conley (1999) are in parentheses. Each column displays the estimate of a separate OLS regression. All regressions include region $\times$ round fixed effects, a constant term, *protests* as defined in Table 2 and all covariates included in the bottom panel of Table 2. *Violent events* is the number of events classified by ACLED as "violence against civilians" or "battles" in a 20-km radius over the 60 days preceding the interview. *Rooted protests* is the number of protests directed against leaders in charge in a 20-km radius over the 60 days preceding the interview. *Non-rooted protests* is the number of protests not directed against leaders in charge in a 20-km radius over the 60 days preceding the interview. See the online Appendix for more details about rooted and non-rooted protests. *Repressed protests* is the number of protests that have been repressed by the authorities in a 20-km radius over the 60 days preceding the interview. *Non-repressed protests* is the number of protests that have not been repressed by the authorities in a 20-km radius over the 60 days preceding the interview. See the text for the definition of dependent variables. For each regression, the number of observations is the same as for the corresponding dependent variable's regression displayed in bottom panel of Table 2.

density partly reflect the potential size of an event, we find that the probability of an event being repressed is unrelated to the local population density of the location.

The large revision in beliefs after a repressed protest supports the interpretation of repression being used as a punishing tool by an elite whose private objectives conflict with voters' preferences. It does not fit with the interpretation of repression as a commitment device for a government willing to implement desirable policy. In particular, repression is unambiguously punished by all citizens irrespectively of their education or ethnic group.

In the next section, we briefly analyze the consequences of protests on prospective civic participation.

3.2.4 Civic involvement and perceived cost of repression

In this subsection, we study the impact of protests on future civic involvement. In particular, we evaluate how respondents intend to vote in a prospective election, and whether they report to be prone to participate in a protest march. While the latter indirectly captures the perceived costs of collective action through reported preferences, the Afrobarometer questionnaire also directly asks about the perceived likelihood of future repression and the interviewee's opinion about the strategic use of repression as a way to silence minorities.

To study possible changes in civic involvement, we use declarations of intent in the Afrobarometer survey. These declarations are not the ideal outcome, but our empirical strategy—heavily relying on the survey timing—does not really allow for the observation of actual patterns in future citizens' behaviors especially when they would be measured at a distant date in the future. In order to capture future voting patterns, we exploit a question asking "If a presidential election were held tomorrow, which party's candidate would you vote for?" and we replace the indicators of trust in the baseline equation (1) with the likelihood to vote in favor of the ruling party, the opposition party and the likelihood to abstain, including uncertain voters. In order to capture alternative methods of civic participation, we exploit once again the question "[Have you] attended a demonstration or protest march" during the past year, and generate a dummy equal to 1 when they declare that they "would do this if [they] had the chance".[24] We exploit a similar question asking interviewees if they "Got together with others to raise an issue" during the past year, and create a dummy equal to 1 if they have not but "would do this if [they] had the chance". In order to capture the perceived cost or benefit of collective action, we exploit two questions. We first explore the revision in expectations about future repression from the government, and the answers to the question "How likely [is it] that people can be punished by government officials if they make complaints about poor quality services or misuse of funds?" (from 0 "Not likely" to 3 "very likely"). The second question relates to the role of repression as a way to limit the power of small lobbying groups. In Passarelli and Tabellini (2013), Laussel and van Ypersele (2012) or Acemoglu, Egorov and Sonin (2015), some citizens or minority groups may be tempted to generate social unrest and prevent reforms from a benevolent government, and the majority of citizens would like the government to repress this movement. We generate a dummy equal

---

[24]Note that this is conditional on not having attended one in the past year, and we restrict the sample to the vast majority of individuals having answered that they have not attended any demonstration during the past year.

Table 6: Relationship between recent protests, and prospective civic behaviors (and views on potential repression).

	(1) Vote for ruling	(2) Vote for other party	(3) Abstention	(4) Would demonstrate	(5) Would raise issue	(6) Likelihood repression	(7) Encourage repression
Recent protests	-0.037***	0.002	0.035***	0.014*	-0.021	0.032**	-0.004
	(0.008)	(0.010)	(0.008)	(0.008)	(0.016)	(0.016)	(0.010)
Observations	10,321	10,321	10,321	8,797	4,901	4,395	10,329

	(8) Vote for ruling	(9) Vote for other party	(10) Abstention	(11) Would demonstrate	(12) Would raise issue	(13) Likelihood repression	(14) Encourage repression
Repressed protests	-0.080**	0.038	0.042	0.048*	-0.116***	0.116***	-0.076**
	(0.040)	(0.024)	(0.030)	(0.027)	(0.033)	(0.013)	(0.033)
Non-repressed protests	-0.034***	-0.001	0.035***	0.011	-0.015	0.029*	0.002
	(0.009)	(0.010)	(0.009)	(0.008)	(0.017)	(0.016)	(0.008)
Observations	10,321	10,321	10,321	8,797	4,901	4,395	10,329

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. Dependent variables in columns heading. Standard errors computed for a 100-km spatial adjustment following Conley (1999) are in parentheses. Each column displays the estimate of a separate OLS regression. All regressions include region × round fixed effects, a constant term, *protests* as defined in Table 2 and all covariates included in the bottom panel of Table 2. *Recent protests* is the number of protests in a 20-km radius over the 60 days preceding the interview. *Repressed protests* is the number of protests that have been repressed by the authorities in a 20-km radius over the 60 days preceding the interview. *Non-repressed protests* is the number of protests that have not been repressed by the authorities in a 20-km radius over the 60 days preceding the interview. In columns 1–3 and 8–10, the dependent variable is a dummy variable equal to 1 if the respondent mentioned the ruling party, another party, or abstention/uncertain vote, when she was asked the following question: "if a presidential election were held tomorrow, which party's candidate would you vote for?" In columns 4 and 11 (5 and 12), the sample is restricted to respondents who declared they never "attended a demonstration or protest march" (never "got together with others to raise an issue"). In these columns, the dependent variable is a dummy equal to 1 if the respondent answered "no, but would do if had a chance" rather than "no, would never do this." In columns 6 and 13, the dependent variable is a dummy equal to 1 if the interviewee finds it likely that "people can be punished by government officials if they make complaints". In columns 7 and 14, the dependent variable is a dummy variable equalt to 1 if the respondent prefers "Statement 1: Government should not allow the expression of political views that are fundamentally different from the views of the majority" over "Statement 2: People should be able to speak their minds about politics free of government influence, no matter how unpopular their views may be".

to 1 if the respondent prefers "Statement 1: Government should not allow the expression of political views that are fundamentally different from the views of the majority" over "Statement 2: People should be able to speak their minds about politics free of government influence, no matter how unpopular their views may be".

As shown in columns 1–3 and 8–10 of Table 6, recent protests are associated with transfers of votes from the ruling party to the opposition party or potential abstention. One additional repressed protest reduces the intent to vote for the ruling party by 8 percentage points, which are shared between the opposition party or potential abstention. The same pattern can be seen after non-repressed protest, and in general, we do not find strong qualitative

differences between repressed and non-repressed protests in this subsection but the revision after repressed protests is one order of magnitude larger.

As regards future civic involvement through protests or grassroots movements, we find that the declared likelihood to demonstrate is slightly higher after a protest, and even more so after a repressed protest (columns 4 and 11). Surprisingly, repressed protests may help coordinate and motivate individuals, and solve the collective action problem. By contrast, respondents would be less likely to assemble and raise an issue in a less confrontational setting (columns 5 and 12). Finally, we find that the occurrence of protests increases the perceived likelihood of future punishment by the government, and decreases the support for such punishment (columns 6–7 and 13–14). One additional repressed protest reduces the probability for the respondent to support the repression of minority's political views by 8 percentage points.

Overall, these results suggest potential changes in agents' behaviors and that respondents are willing to use different tools to influence leader's actions. Voting intentions decrease for the ruling party, and go towards the opposition party but also abstention. However, civic involvement does not decrease as a whole: respondents are more likely to participate in a protest march, and less likely to support the repression of demonstrations. These results are in line with the findings of Chong et al. (2011) and may complement them: revelations of leaders' misbehavior decrease voter turnout but encourage a more direct form of civic involvement, which relies less on existing institutions and more on trials of strength in order to guarantee the balance of power between voters and political leaders.

4   Conclusion

This paper studies the aftermath of protests and identifies large variations of beliefs both in the head of state and in institutions. Our interpretation is that protests reflect misbehaviors of leaders and spark a revision in priors about the state. Even though leaders might change, a shock may drive down beliefs in fundamentals—institutions—and consequently affect future civic involvement with citizens shying away from traditional instruments to influence public policy. These findings may support the existence of different regimes in social coordination with sudden switches and may explain unexpected overthrows.

We can identify two limits to our work. First, we consider as an interesting feature of our empirical framework that it relies on local responses and small time windows. The interaction of a local spatial treatment and a discontinuous time treatment provides a cleaner identification than a macro-analysis. However, our findings are silent about the possible dynamics of discontent and protests over time and regions. In particular, an interesting

extension may consist in studying when a social conflict generates a sudden outburst of anger across the country, and when it remains confined to a city and a short period. With the data sources in this paper, such analysis is hardly implementable: surveys are not conducted frequently enough. Second, the empirical observations presented in this paper do not include possible economic counterparts in addition to changes in beliefs. A possible extension could analyze the actual changes in voters' behaviors, actual civic participation and the associated implications on public policy in the longer run.

References

Acemoglu, Daron, and James A. Robinson. 2000. "Why Did The West Extend The Franchise? Democracy, Inequality, And Growth In Historical Perspective." *The Quarterly Journal of Economics*, 115(4): 1167–1199.

Acemoglu, Daron, Georgy Egorov, and Konstantin Sonin. 2015. "Political Economy in a Changing World." *Journal of Political Economy*, 123(5): 1038–1086.

Banerjee, Abhijit V. 1992. "A Simple Model of Herd Behavior." *The Quarterly Journal of Economics*, 107(3): 797–817.

Battaglini, Marco, and Roland Bénabou. 2003. "Trust, Coordination, and the Industrial Organization of Political Activism." *Journal of the European Economic Association*, 1(4): 851–889.

Becchetti, Leonardo, Pierluigi Conzo, and Alessandro Romeo. 2014. "Violence, trust, and trustworthiness: Evidence from a Nairobi slum." *Oxford Economic Papers*, 66(1): 283–305.

Bellows, John, and Edward Miguel. 2009. "War and local collective action in Sierra Leone." *Journal of Public Economics*, 93(11-12): 1144–1157.

Berman, Nicolas, Mathieu Couttenier, Dominic Rohner, and Mathias Thoenig. 2017. "This Mine is Mine! How Minerals Fuel Conflicts in Africa." Forthcoming in *American Economic Review*.

Besley, Timothy, and Marta Reynal-Querol. 2014. "The legacy of historical conflict: Evidence from Africa." *American Political Science Review*, 108(02): 319–336.

Besley, Timothy, and Torsten Persson. 2010. "State Capacity, Conflict, and Development." *Econometrica*, 78(1): 1–34.

Besley, Timothy, and Torsten Persson. 2011a. "Fragile States And Development Policy." *Journal of the European Economic Association*, 9(3): 371–398.

Besley, Timothy, and Torsten Persson. 2011b. *Pillars of Prosperity: The Political Economics of Development Clusters.* Princeton University Press.

Blattman, Christopher. 2009. "From Violence to Voting: War and Political Participation in Uganda." *American Political Science Review*, 103: 231–247.

Blattman, Christopher, and Edward Miguel. 2010. "Civil War." *Journal of Economic Literature*, 48(1): 3–57.

Cassar, Alessandra, Pauline Grosjean, and Sam Whitt. 2013. "Legacies of violence: Trust and market development." *Journal of Economic Growth*, 18(3): 285–318.

Chamley, Christophe. 1999. "Coordinating Regime Switches." *The Quarterly Journal of Economics*, 114(3): 869–905.

Chong, Alberto, Ana L. De La O, Dean Karlan, and Leonard Wantchekon. 2011. "Looking Beyond the Incumbent: The Effects of Exposing Corruption on Electoral Outcomes." National Bureau of Economic Research Working Paper 17679.

Collins, William J., and Robert A. Margo. 2004. "The Labor Market Effects of the 1960s Riots." In *Brookings-Wharton Papers on Urban Affairs.* , ed. William G. Gale and Janet Rothenberg Pack. Washington, DC: Brookings Institution.

Collins, William J., and Robert A. Margo. 2007. "The Economic Aftermath of the 1960s Riots in American Cities: Evidence from Property Values." *The Journal of Economic History*, 67(04): 849–883.

Conley, Timothy G. 1999. "GMM Estimation with Cross Sectional Dependence." *Journal of Econometrics*, 92(1): 1–45.

Conley, Timothy G. 2008. "Spatial Econometrics." In *The New Palgrave Dictionary of Economics.* , ed. Steven N. Durlauf and Lawrence E. Blume, 741–747. Palgrave Macmillan.

Edmond, Chris. 2013. "Information Manipulation, Coordination, and Regime Change." *Review of Economic Studies*, 80(4): 1422–1458.

Ellis, Christopher J., and John Fender. 2011. "Information Cascades and Revolutionary Regime Transitions." *Economic Journal*, 121(553): 763–792.

Ellman, Matthew, and Leonard Wantchekon. 2000. "Electoral Competition Under The Threat Of Political Unrest." *The Quarterly Journal of Economics*, 115(2): 499–531.

Gilligan, Michael J., Benjamin J. Pasquale, and Cyrus Samii. 2014. "Civil War and Social Cohesion: Lab-in-the-Field Evidence from Nepal." *American Journal of Political Science*, 58(3): 604–619.

Grosjean, Pauline, Frantisek Ricka, and Claudia Senik. 2013. "Learning, political attitudes and crises: Lessons from transition countries." *Journal of Comparative Economics*, 41(2): 490–505.

Guriev, Sergei, and Daniel Treisman. 2015. "How Modern Dictators Survive: An Informational Theory of the New Authoritarianism." National Bureau of Economic Research, Inc NBER Working Papers 21136.

Hsiang, Solomon M. 2010. "Temperatures and Cyclones Strongly Associated with Economic Production in the Caribbean and Central America." *Proceedings of the National Academy of Sciences*, 107(35): 15367–15372.

Jaeger, David A., Esteban F. Klor, Sami H. Miaari, and M. Daniele Paserman. 2012. "The struggle for Palestinian hearts and minds: Violence and public opinion in the Second Intifada." *Journal of Public Economics*, 96(3-4): 354 – 368.

Kricheli, Ruth, Yair Livne, and Beatriz Magaloni. 2011. "Taking to the Streets: Theory and Evidence on Protests under Authoritarianism."

Kuran, Timur. 1989. "Sparks and prairie fires: A theory of unanticipated political revolution." *Public Choice*, 61(1): 41–74.

Kuran, Timur. 1991*a*. "The East European Revolution of 1989: Is It Surprising That We Were Surprised?" *American Economic Review*, 81(2): 121–25.

Kuran, Timur. 1991*b*. "Now Out of Never: The Element of Surprise in the East European Revolution of 1989." *World Politics*, 44(1): pp. 7–48.

Laussel, Didier, and Tanguy van Ypersele. 2012. "When the squeakiest wheel gets the most oil: Exploiting one's nuisance power." *European Economic Review*, 56(8): 1593–1606.

Lohmann, Susanne. 1993. "A Signaling Model of Informative and Manipulative Political Action." *The American Political Science Review*, 87(2): 319–333.

Lohmann, Susanne. 1994*a*. "Information Aggregation Through Costly Political Action." *The American Economic Review*, 84(3): pp. 518–530.

Lohmann, Susanne. 1994*b*. "The Dynamics of Informational Cascades: The Monday Demonstrations in Leipzig, East Germany, 1989-91." *World Politics*, 47(1): pp. 42–101.

Lu, Yang, Wing Suen, and Heng Chen. 2013. "The Power of Whispers: A Theory of Rumor, Communication and Revolution." Society for Economic Dynamics 2013 Meeting Papers 411.

Madestam, Andreas, and David Yanagizawa-Drott. 2012. "Shaping of the Nation: The Effect of Fourth of July on Political Preferences and Behavior in the United States." Harvard University, John F. Kennedy School of Government Working Paper Series rwp12-034.

Madestam, Andreas, Daniel Shoag, and David Yanagizawa-Drott. 2013. "Do Political Protests Matter? Evidence from the Tea Party Movement." *The Quarterly Journal of Economics*, 128(4): 1633–1685.

Nunn, Nathan. 2008. "The Long-Term Effects of Africa's Slave Trades." *The Quarterly Journal of Economics*, 123(1): 139–176.

Nunn, Nathan, and Leonard Wantchekon. 2011. "The Slave Trade and the Origins of Mistrust in Africa." *American Economic Review*, 101(7): 3221–52.

Passarelli, Francesco, and Guido Tabellini. 2013. "Emotions and Political Unrest." C.E.P.R. Discussion Papers CEPR Discussion Papers 9446.

Rohner, Dominic, Mathias Thoenig, and Fabrizio Zilibotti. 2013. "Seeds of distrust: conflict in Uganda." *Journal of Economic Growth*, 18(3): 217–252.

Voors, Maarten J., Eleonora E. M. Nillesen, Philip Verwimp, Erwin H. Bulte, Robert Lensink, and Daan P. Van Soest. 2012. "Violent Conflict and Behavior: A Field Experiment in Burundi." *American Economic Review*, 102(2): 941–64.