How Political Violence Shapes Trust in the State*

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Abstract

How does exposure to violent events shape civilian attitudes towards the government? In this paper we measure the short-term effects of state political violence on civilian attitudes. We create an original subnational dataset of more than 10,000 violent events and more than 65,000 household survey responses. These events and interviews span 16 Sub-Saharan African countries and three waves of the Afrobarometer (46 nationally-representative surveys from 2002-2009). To measure the causal effect of political violence we introduce a novel identification strategy that exploits the exogenous timing of the Afrobarometer's survey dates to compare individuals who were interviewed just after violent events (*treated*) to those individuals interviewed just before violent events (*control*), within the same district. We find that individuals interviewed shortly following the occurrence of violent events in their district are substantially *more* likely to report trust in their head of state, parliament, local government officials, and police forces – an effect that increases in magnitude as the timing of the violence event(s) near the interview date. These findings provide a logic for why leaders pursue strategies of violence both against opposition groups as well as against civilians, a finding with implications for the study of electoral violence and of 'routine' state repression. Finally we show that, while exposure to violence does raise reported levels of *fear*, civilian reports of higher trust for the government does not reflect fear of state repression, punishment or self-censorship but rather support for enhancing local political order.

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1 Introduction

Despite the marked decrease in the incidence of civil war in Africa, political violence remain pervasive. Much of this political violence is directed (or tacitly allowed) by ruling regimes and their allies, by opposition political parties, and by loosely organized groups of ordinary individuals – both at times of electoral competition but also in patterns of puzzlingly 'routine' everyday violence. Since at least Niccolò Machiavelli's *The Prince*, political observers have questioned and theorized how and why leaders instrumentally use force, and how civilians respond: exhibiting either greater allegiance or increasing hostility towards their leaders.

How do individuals respond to events of political violence near their homes? Do citizens in areas exposed to political violence exhibit differential support for their elected leaders? And if so, what are the mechanisms by which exposure to political violence shape attitudes towards the government? In this study we grapple with these challenging questions to better understand the short-term effects of political violence, and why utilizing such strategies may be in the interests of state actors. We focus particularly on state-directed violence, common both during electoral competition and as a feature of 'routine' political competition outside electoral contexts.

Yet to date there has been little systematic analysis of the individual effects of political violence. This gap makes it difficult to understand why states do use violence outside of civil or interstate conflicts, why leaders promote (or allow) small-scale violence and intimidation by police, security forces, parties, mobs, vigilantes, and so on. In a recent survey of the literature on state repression, Christian Davenport asks, "what are the 'benefits' of repression? Why do authorities believe that repressive action will lead them to their objectives, and does repression actually produce intended benefits?" (2007: 17). Davenport continues, "[t]he answers are not clear. One explanation for state repression is that authorities use it to stay in power, but the literature contains not one systematic investigation of this proposition" (ibid).

While scholars (e.g. Horowitz 1985) have long been aware that leaders use violence instrumentally for political ends, we provide cross-national yet micro-evidence across Sub-Saharan African countries of a link between violent events and support for state

actors. While short of mass violence or civil war, police and military repression as well as intimidation by government agents can serve to demobilize opponents, mobilize a regime's base, or nudge unaligned civilians that the government is the actor most able to provide security. We find in fact that civilians exhibit high trust and support for government actors when these actors are perceived to improve order through the use of force. While civilians do fear local violence, these same residents support the use of military force by the government and in its presence exhibit little anxiety or fear of punishment by those government actors.

We contrast our findings of the effects of military repression with an analysis of how individuals respond to the recent presence of riots and protests. When exposed to this form of political conflict, we find individuals are *less* likely to support state actors. An alternative mechanism to this argument may be that individuals are simply fearful of government repression when the state pursues military action but not fearful when witnessing riots or protests. However we provide evidence from multiple measures that respondents when exposed to military repression in their district are fearful of violence generally, but not afraid of state actors and if anything appear less fearful of the state when such government initiated conflict events occur.

Our paper makes three contributions. First, we provide robust empirical evidence showing how exposure to political violence, particularly state-perpetuated violence, increases civilian support for government leaders and institutions. We show individuals report higher support for state actors, not due to fear of the state, but presumably because they desire state actors impose political order. Second, we create a precise yet broad data set of violent events and political attitudes, with geographical variation at the district unit and daily temporal variation across 46 nationally-representative surveys. Third, we develop a novel empirical strategy for identifying causal effects of violence with household surveys by comparing individuals whose districts were affected by violence just prior to or just following their interview.

Our strategy for identifying the causal effects of political violence exploits plausibly exogenous variation generated by the timing of the nationally-representative Afrobarometer household surveys relative to the timing of violent events. Since Miguel, Satyanath and Sergenti's remarkable study of rainfall, economic shocks, and civil war

onset there have been several new studies that examine well-identified causes of how commodity prices, aid or institutions may shape political violence and war (Bazzi and Blattman 2011, Besley and Persson 2012, Burke et al. 2009, Dube and Vargas forthcoming, Miguel and Satyanath 2011, Nunn and Qian 2012). By contrast, we know of no other cross-national study that identifies the causal effects of within-state exposure to political violence.[1]

We identify the causal effects of an area's exposure to violent conflict by explicitly matching surveyed households whose districts experienced exposure to violent events in a 30-day, 15-day, 7-day and 5-day 'window' *prior* to the interview, with similar individuals whose districts also experienced violent events in the same 'window' *after* the interview. We believe this to be the only strategy with external-validity across countries that identifies the causal effects of violent conflict on individuals.[2]

We focus on the short term effects of violence to most precisely measure the the impact of such events, but also as we believe these short term effects are central to the decisions of state and non-state actors who perpetrate such violence. By matching citizen's survey responses with violent events (both sub-nationally and within narrow temporal windows), our analysis is unlikely to be confounded by omitted variables – as any such bias would require unobserved variables that co-varied both within roughly the same week-period of the violent events and in the sub-national districts in which the events took place.

Given our sample of households is determined by these two factors, our sample consists of countries we describe as *intermediate regimes*. Regimes that are neither completely peaceful and stable (consolidated democracies such as Botswana or Ghana) nor completely unstable, such as failed states or countries affected by severe civil war (such as the Democratic Republic of Congo or Somalia). Rather than a select sample of cases, these regimes are broadly representative of most countries in Sub-Saharan Africa, both beneficiaries of democratization in the past two decades and of economic growth

[1]Perhaps the closest such study is Miguel, Seiegh and Satyanath's unique study of how the extent of civil conflict in a given European professional football player's home country "affects his propensity to behave violently on the soccer field" (2011:59).

[2]We discuss below several papers we learned of after completing our analysis that studies public health outcomes in Chicago with a similar survey-timing method.

more recently – but also victims of moderate levels of political repression and internal violence.

Finally, our analysis of the short term effects of political violence is of particular interest for the study of electoral violence. New research has shown electoral violence in Africa to be (a) common, (b) generally perpetrated by the incumbent, and (c) more frequent prior elections than afterwards (Straus and Taylor 2012). Our empirical results provide causal evidence to explain these stylized facts and help adjudicate between rival hypotheses that might predict electoral violence.

2 Existing Literature

Under what conditions will exposure to political violence shape civilian attitudes towards state actors? According to Christopher Blattman and Edward Miguel, "[t]he social and institutional legacies of conflict are arguably the most important but least understood of all war impacts" (2010: 42). In a review on the study of government repression, Christian Davenport, also asks: "[w]hat are the 'benefits' of repression? Why do authorities believe that repressive action will lead them to their objectives, and does repression actually produce intended benefits? The answers are not clear" (Davenport 2007:17). In the past few years, several sets of important new studies have begun to analyze how exposure to political violence affects the social, political and psychological behavior of former combatants and civilian. In this article we make three contributions to this literature: (1) we focus on civilian attitudes towards government actors (rather than community members or internalized attitudes such as generalized trust), (2) we focus on the short term effects of violence, which we argue is relevant for understanding the incentives actors face who might engage in violence or repression, and (3) we measure effects that are cross-country yet also individual, allowing for a set of results that are high in both internal and external validity.

Several studies have offered surprising findings that individuals exposed to political violence are more likely to engage in political behavior after the conflict has ended. Bellows and Miguel find in Sierra Leone that war victimization increased self-reported political mobilization and participation in local collective action, which they interpret as

a psychological legacy of exposure to political violence (2009). Blattman finds in Uganda that former child soldiers were, after demobilization and rehabilitation, more likely to vote and become local leaders (2009). We believe we can complement this growing literature by studying civilians indirectly exposed to political violence – a numerically large category of individuals that to some extent includes (to a greater or lesser degree) every individual living in a country that experiences, or recently experienced, a serious conflict.

Political scientists and economists have also used behavioral economic games to understand social behavior in field-laboratory settings. In Burundi, Voors et al. (2012) find that individuals indirectly exposed to civil war violence were more altruistic while Gilligan et al. studying post-war Nepal found indirect exposure to violence led to greater within-community trust, cooperation, and altruism (2013). Studying fairness, also with a behavioral measurement (the dictator game), Whitt and Wilson find lower than expected out-group (ethnic) bias in post-war Bosnia (2007). However not all such post-war studies using lab experimental methods find pro-sociality – Cassar et al. find that exposure to conflict in post-war Tajikistan has lowered trust and fairness within local communities, decreased likelihood of impersonal exchange, and increased norms of within-group morality (2011). We feel this research complements this experimental literature with a new empirical result and argument about how conflict-exposed individuals react towards external, and specifically government actors.

Other studies on the legacies of violence, military repression and human subjugation generally find a negative legacy for individual trust or perceptions of freedom within society. García-Ponce and Wantchekon find a persistent legacy of colonial repression in Madagascar where individuals living in districts exposed to colonial-era repression have lower perceptions of freedom of expression in society today (2012). One final paper related is Nunn and Wantchekon's study of the long-term effects of the slave trade. They find individuals whose ancestors were heavily raided during the slave trade are less trusting, arguing this legacy was driven by changing cultural norms, beliefs, and values (2011).[3] Finally this research adds to this literature by showing that in some cases

[3]In a separate study, Wantchekon and Garcia-Ponce, find that the nature of political conflict during the colonial period in Africa affects present day political regimes – that those countries that experienced

individuals exposed to conflict may have heighten fear of violence but also desire the state take an aggressive stance against disorder and violence – additionally we discuss how this very human reaction to political insecurity may have negative longer-term consequences.

2.1 Theoretical Mechanisms

In this section we discuss two theoretical mechanisms that explain why indirect exposure to political violence, even when perpetrated by state actors, will *increase* exposed civilians reported trust and support for the government. In the first mechanism, military action by governments is perceived as a signal of a willingness to impose order. In highly insecure political environments, civilians may prefer government actions that signal a commitment to repressing opposition parties, criminal groups, and other non-state armed groups. In this conceptualization, civilians do fear local violence, but support the use of military force with the idea that this force is legitimate and will lead to greater order and security in the medium to long term.

- **Mechanism 1**: Violent conflict events signal relative state strength or weakness depending on the organizer of the event(s).

By contrast when exposed individuals are exposed to riots and protests individuals are *less* likely to support state actors. According to this mechanism the type of conflict event – either (a) instigated by the military or (b) instigated by protesters or rioters – signals either state capacity as the military subdues opposition or anti-state groups, or state weakness as rioters/protesters signal collective opposition to the government regime.

- **Mechanism 2**: Violent conflict events cause high civilian fear of government repression, incentives to hide their true preferences.

Alternatively, a second mechanism explains higher support for government leaders as a result of indirect exposure to conflict as a reflection of civilian fear of government

anti-colonial rural insurgencies are more likely to be autocratic, while those countries that experienced urban insurgencies were more likely to become democratic (2013).

repression. In the fact of military action but not when witnessing riots or protests these individuals may believe they will be the target of state forces. When exposed to such military force civilians will have an incentive to hide their true attitudes towards state actors while when observing riots or protests such individuals will have no such incentive to hide any opposition to the government.

3 Context

To date there is little systematic, cross-country empirical research on the effects of violence in Sub-Saharan Africa, somewhat surprising given the large number of countries and cases of political violence. In a recent review on the study of riots Steven Wilkinson details many important findings from and new challenges for studies on the causes of violence, but only as he concludes the article rightly states "we also ought to do more studies that use riots as an independent variable" (2009:341). In a new empirical study and data set, Straus and Taylor present a cross-national dataset of instances of electoral violence in Africa, providing descriptive evidence that such violence is (a) common–occurring in nearly half of such elections, (b) perpetrated by state actors, and (c) generally occurs prior elections rather than after (2012: 15-38).

In this paper we study violence against civilians – specifically, how state directed and non-state directed political conflict affects individual trust of government actors in Sub-Saharan Africa. We focus on two types of political conflict – state directed military action and protests and riots – which are not, at least explicitly, affiliated with the state. Additionally we focus on short-term effects of such violence on civilian attitudes and suggest these findings have important implications for understanding leader incentives to engage in electoral violence or other 'routine' repression.

Political violence in Sub-Saharan Africa takes multiple forms. Such armed conflict may be perpetrated by states, political parties, police and security forces, opposition groups, armed militia and paramilitary, insurgent movements, and so on. These groups generally vary according to their level of organization/formalization as well as their relationship vis-á-vis the ruling government. Roughly we can then categorize political violence in Africa as either (1) perpetrated by state, political party or opposition parties

against civilians, (2) insurgency or counter-insurgency, or (3) protests and riots. To better understand several contexts of political violence in our sample of countries and time periods, we briefly describe the dynamics of political violence below in Kenya, Uganda and Zimbabwe. These descriptions are not exhaustive of political conflict in these countries nor of political violence in the countries in our data but provide a starting point for understanding the complexities and patterns of conflict.

3.1 Kenya

Kenya, normally thought to be a large, stable and relatively peaceful African nation, made international news in 2007-2008 due to unexpectedly violent post-election conflict. After announcing the re-election of incumbent president Mwai Kibaki in December 2007, opposition Orange Democratic Movement (ODM) supporters of candidate Raila Odinga began "protest demonstrations that soon degenerated into rape, looting, and indiscriminate murder by machete, actions aimed against the Kikuyu, Kibaki's ethnic group" (Chege 2008:125). Post-election protests and clashes continued for three weeks as the Kenyan police attempted to put down the violence and were accused of shooting innocent demonstrators in ODM strongholds – Kikuyu retaliatory attacks then took place in January in and around Nairobi targeting ethnic groups believed to support the ODM – Luo, Luhya and Kalenjin (ibid). In the end, Two months of bloodshed left over 1,000 dead and up to 500,000 internally displaced (Human Rights Watch 2008).

Quite aside from electoral violence from 2008, Kenya has been affected by multiple forms of political violence over the past several decades, both during the rule of Daniel arap Moi and since he left power in 2002. The *Mungiki* armed movement affected both the 1992 and 1997 elections. The movement, in some ways reflecting the earlier *Mau Mau* insurgency against the British, arose amongst individuals associated with the Kikuyu ethnic identity in the early 1990s (Kagwanja 2002). Violence has not been limited to electoral periods but has also grown in urban as a result of vigilante, extortionist and other criminal activity, especially in Nairobi (Anderson 2002). More recently Human Rights Watch documented torture, rape, and other human rights against the local civilian population in 2008 in the Mandera Triangle in northeastern Kenya by state

security forces, leaving more than 1,200 injured (2009). Border areas are also dangerous, as while Kenya has provided asylum to more than 325,000 Somali refugees in the past few decades, authorities have been increasingly hostile to this population, 'demonizing' this group "80 percent of whom are women and children – as a national security threat [that has] made them vulnerable victims of Kenya's notoriously corrupt and abusive police force" (Human Rights Watch 2010).

3.2 Uganda

Uganda's President Yoweri Museveni initially rose to prominence as a rebel leader of the National Resistance Army who seized power from dictator Milton Obote in 1986. Thousands of Achioli soldiers, previously loyal to Milton Obote, fled just before Museveni's victory home to the North fearing the NRA. Alice Auma, a young woman from Gulu in Acholi claimed to be possessed by a Christian spirit, Lakwena, and formed the Holy Spirit Mobile Forces (HSMF) emerged, aiming to combat witches, "impure soldiers," and the Ugandan Government, now led by Museveni. Following the defeat of Lakwena's group in October 1987, Josef Kony's Lord's Resistance Army (LRA) emerged as the dominant Northern rebel group. Unlike the HSMF which focused on conventional military tactics the LRA initially used guerrilla tactics in and around Kitgum district, before spreading through Achioli regions of northern Uganda.

Throughout the 1990s the LRA attacked trading posts, schools and villages in northern Uganda as well as sending small groups to fight Sudan's People Liberation Army (SPLA/M) in Southern Sudan. The group was notorious for using violence against civilians, for abducting the majority of its fighting force, composed almost entirely of youths (Blattman 2009). Throughout the 1990s the LRA received weapons and training from the Sudanese government (Khartoum), set up camps and bases in Southern Sudan and maintained a public presence in the South Sudan urban center of Juba.[4] Since the inception of the LRA the Ugandan civil war has led to the displacement of over two million civilians and the deaths of as many as 500,000 people (Project Ploughshares). Thousands of women and children were forcibly recruited as fighters, porters and sex

[4]Juba is now the capital of the nascent Southern Sudan state but was then firmly controlled by Khartoum.

slaves. According to estimates by Amnesty International, 80% of the LRA's fighters were abducted children (2001: 1).

Numerous attempts to end the war in Uganda have failed. In 1994 the Ugandan government and LRA agreed to a ceasefire but resulting negotiations failed, a side-effect of this brief agreement was the movement of the LRA to Southern Sudan and the beginning of direct support from the Khartoum government. The Sudanese and Ugandan governments signed a peace agreement in 1999 which included provisions barring the Ugandan government from supporting the SPLM/A in Sudan and barring the Sudanese government from supporting the LRA. A further protocol between these governments in 2002 allowed Ugandan troops to deploy and destroy LRA bases in Southern Sudan. Still the LRA survived and Khartoum did not end its support of the rebel group. Khartoum did eventually drop its level of support of the LRA in 2005, shortly after it signed the Comprehensive Peace Accords with the SPLM/A, and the LRA moved its operations to the border region between the DRC and Sudan.

A second ceasefire and intermittent negotiations between the LRA and Ugandan government followed in 2006, however these negotiations were complicated by the indictment of LRA leaders by the International Criminal Court (ICC). These indictments served, at least rhetorically, as an obstacle to a final peace agreement according to the LRA leadership. Still, even with an offer of amnesty from Museveni and protection from deportation to the ICC, Kony and the LRA refused in the end to an agreement and regular violent conflict resumed by 2007. Lastly, a joint offensive by the governments of Uganda and DRC in 2008, with intelligence and funding from the United States, aimed to defeat the LRA decisively but they were again unsuccessful in what was considered a logistical and military failure. While Ugandan troops withdrew in March 2009, the LRA has continued to attack villages in parts of the Central African Republic, DRC and Southern Sudan. As of this writing the LRA is still present in the border region of Central African Republic, South Sudan and the Democratic Republic of Congo.

3.3 Zimbabwe

Zimbabwe has been witness to multiple forms of violence over the past several decades, generally instigated by the brutal regime of Robert Mugabe. After the Food Riots of 1998 the government responded with organized violence and torture. The government used military operations in response to rebellions in Matabeleland South and Midlands in 2000 and invaded/expropriated farms in the name of land reform in 2000 and 2001. Catherine Boone has argued that this expropriation, in threat or reality, has provided a material base for exclusionary nationalism (2009:183).

The Zimbabwe Human Rights NGO has also noted human rights abuses against teachers in 2002, and incidents torture through 2012. In May 2005 the government began *Operation Murambatsvina* (in English: "Operation Drive Out Rubbish") a widespread effort to forcibly clear slums in urban areas of the country – areas believed to house much of the internal opposition to the Mugabe regime. The United Nations estimated that at least 700,000 people were directly affected through loss of their home or livelihood and as many as 2.4 million people indirectly affected (2005). Scholars of this context have identified political violence as a new way of mobilizing people – as well as a method, through violence and exclusionary rhetoric, of demobilizing people. In this way *Operation Murambatsvina* served as 'redistricting by other means.' Boone has argued that political violence in Zimbabwe has been used instrumentally to win the support of key electoral constituencies (2009:183).

Zimbabwe has also experienced electoral violence preceding parliamentary elections in 2000, in addition to physical assaults and intimidation of voters suspected of supporting the opposition MDC following the election (Makumbe: 2002:89). Opposition parties were close to winning majority of parliamentary seats in 2000. Since then, Mugabe's government has used vote-rigging, intimidation, fraud and violence in the 2002, 2005, 2008 elections. In Makumbe's words, "the regime deployed tactics who se sheer brutality and underhandedness were without precedent even in the troubled post-independence history of this southern African republic" (2002:87). In response protests and riots did erupt after the 2008 elections which were marked by fraud and party-instigated violence. Since a 2008 power-sharing deal between Mugabe and Morgan Tsvangirai the

government has appeared to more tentatively towards more peaceful political competition. While protest in reaction to the 2008 elections may have led to a compromise, these patterns of violence do appear to have important consequences – Adrienne LeBas has argued that polarization has resulted from the short-run strategies of political elites in this context (2006: 420).

4 Data Construction

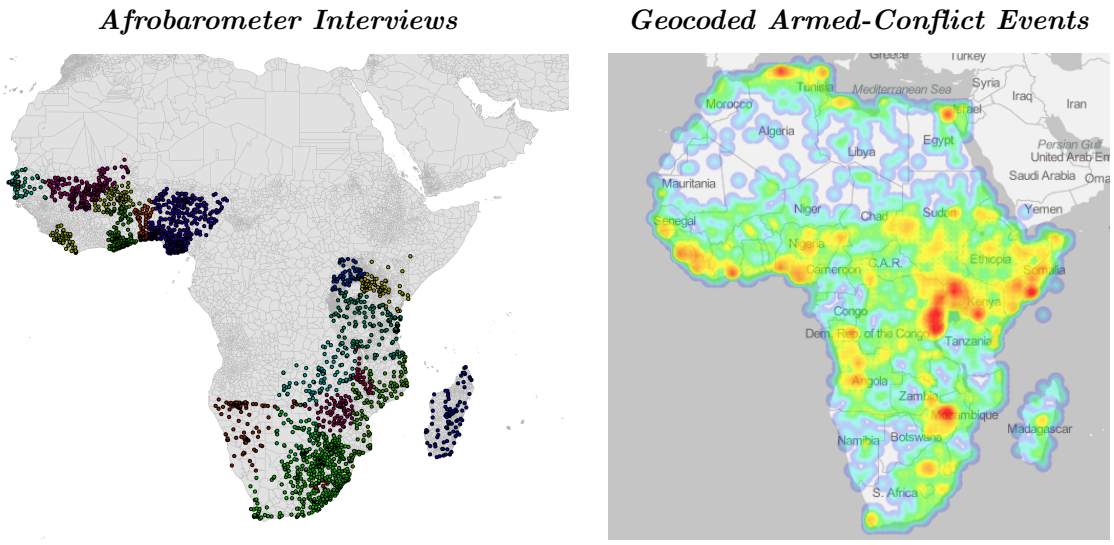
We introduce a new dataset that we believe is unique in the study of political violence and civilian attitudes with regard to the datasets combination of coverage and precision. The dataset provides daily temporal and district spatial variation through the merging of conflict event datasets with public opinion survey datasets. The best available dataset on conflict events across multiple countries is the Armed Conflict Location Event Dataset (ACLED). We combine this dataset with three waves of Afrobarometer public opinion surveys (2000, 2005 and 2008). However as we explain below, this merging was not straightforward.

In order to analyze how political conflict events affected Afrobarometer respondents we required a method to map events to respondents. First we combined all Afrobaroemter surveys and waves, and located geographic coordinates (longitude and latitude) for each district of each respondent in these surveys. We were able to successfully locate more than 96% of the district names from the Afrobarometer surveys. We then spatially merged all Afrobarometer respondents (by district) and ACLED conflict events (through the coordinates included in each ACLED country dataset) to administrative boundaries. These administrative boundaries provided a common geographic reference for each Sub-Saharan African country for which there is both Afrobaroemter and ACLED data available.

The final result is are a set data which can be disaggregated for analysis as precisely as the district-day but also can be aggregated for instance at the level of country-year (or anything in between). Figure 1 displays the location of Afrobarometer interviews and armed-conflict events from ACLED (1997-2009). Table 1 presents a summary of descriptive statistics from the merged dataset. These data include more than 65,000

interviews across 16 countries in from 2002-2009, merged to more than 13,000 conflict events. In this paper we focused on relatively narrow windows around each interview date: this allows us to measure the effect of indirect exposure to political violence for small daily intervals around each interview date. For instance we can measure whether a survey respondent was exposed to conflict event(s) in their district 30 days prior to the interview, 15 days prior, 7 days prior, and so on. We hope this broader dataset will be of use to researchers who study development political change and conflict in Sub-Saharan Africa and expect to enlarge the dataset with subsequent releases of Afrobarometer survey waves.

Figure 1: Afrobarometer Surveys (2002-2009) & ACLED Conflict Events (1997-2009)



Notes. The map on the left shows the location of Afrobarometer interviews. The dots represent districts' centroids, using a different color for each country in the sample. The heat map on the right displays the location of armed-conflict events, which were mapped using the coordinates included the ACLED data set. Warmer colors denote higher levels of fatalities.

Table 1: Summary of Descriptive Statistics

Variable	Mean	Std. Dev.	N
Balance Variables			
Enough Food	0.270	0.305	64971
Enough Water	0.257	0.332	64999
Medicine	0.300	0.320	64793
Electricity	0.321	0.377	61128
Education	0.333	0.215	60497
Female	0.500	0.500	65123
Age	26.925	14.32	60453
Living Cond.	1.837	1.036	62663
Treatment Variables			
Violence, 5 days	0.845	0.362	9063
Violence, 7 days	0.891	0.311	10163
Violence, 10 days	0.890	0.314	10626
Violence, 15 days	0.901	0.298	11141
Violence, 30 days	0.898	0.303	8210
Mil. Force, 5 days	0.813	0.390	7440
Mil. Force, 7 days	0.837	0.370	8474
Mil. Force, 10 days	0.837	0.369	8688
Mil. Force, 15 days	0.818	0.386	9014
Mil. Force, 30 days	0.932	0.252	6325
Protest, 5 days	0.481	0.500	5553
Protest, 7 days	0.545	0.498	6454
Protest, 10 days	0.620	0.485	7634
Protest, 15 days	0.782	0.413	8601
Protest, 30 days	0.929	0.257	7189
Outcomes and Mechanisms			
Trust Parliament	1.621	1.033	60476
Trust Local Officials	1.619	1.081	41368
Trust Police	1.533	1.084	63255
Trust President	1.837	1.077	62303
Trust People	2.341	0.902	43123
Fear of Violence	1.147	1.170	21410
Careful	1.722	1.118	62009
Punished	1.145	1.087	19802

5 Empirical Strategy

The "ideal" test to assess the causal effect of political violence on attitudes towards the government would imply to experimentally –and unethically– manipulate individual exposure to violent events. Ideally, one would aim to compare virtually identical groups of people which vary only in their degree of exposure to violence. However, this is neither possible nor desirable in real-life settings. Since individuals are not randomly exposed to violent events, estimates of the effect of political violence will be biased if unobserved factors jointly predict exposure to violence and the outcome of interest. To address this omitted variable bias concern, we employ a novel identification strategy that exploits plausibly exogenous variation in the timing of local violent events relative to the timing of survey interviews among individuals living within the same district.

Our identification strategy approximates as-if random exposure to violent events by comparing respondents who were interviewed right before a violent event took place in their community to those who were interviewed right after the event was perpetrated. The underlying assumption of our identification strategy is that, within a given district, the timing of violent events in relation to the timing of survey interviews produces exogenous variation in the recency of local violence. This exogenous variation serves as a the basis for causal inference. To better approximate as-if random exposure to violence, we focus on individuals in districts that were exposed to violent events within a narrow temporal window of their interview (5, 7, 10, 15, or 30 days). After completing our analysis we learned of two other studies, related to crime and public health in Chicago, that use a similar method to ours here (Sharkey 2010; Sharkey, Tirado-Strayer, Papachristos, and Raver 2012). However we believe this study to be the first to use such a methodology in political science, to examine the effects of political violence, and the first application in a cross-country setting.

We believe this is a credible identification strategy for two main reasons. First, within a very narrow temporal window, whether a given individual is interviewed before or after a violent event is defined almost by chance, specially if households are selected using a systematic random sampling method. Second, violent events that take place during the conduction of a survey are usually unexpected events. Unsafe areas are

generally not included in the sample, or avoided by survey enumerators. Moreover, interview dates and survey routes are usually planned well ahead on time. Therefore, if a violent event is perpetrated in a particular district during the conduction of a survey, the likelihood of moving to a neighboring district to complete the remaining interviews is very low.

Table 2: Balance Statistics

	Mean pre-event	Std. dev. pre-event	Mean post-event	Std. dev. post-event	Difference Std. Error
Viol. Against Civ.					
Enough Food	0.312	0.329	0.401	0.341	-0.089 (0.020)***
Enough Water	0.293	0.355	0.320	0.348	-0.027 (0.021)
Medicine	0.322	0.366	0.469	0.368	-0.147 (0.022)***
Electricity	0.338	0.357	0.335	0.337	0.003 (0.021)
Education	0.461	0.162	0.442	0.174	0.019 (0.011)*
Female	0.499	0.501	0.506	0.500	-0.007 (0.030)
Age	25.678	14.019	25.798	14.039	-0.119 (0.844)
Military Force					
Enough Food	0.404	0.342	0.433	0.335	-0.029 (0.025)
Enough Water	0.308	0.338	0.344	0.357	-0.036 (0.026)
Medicine	0.449	0.371	0.497	0.362	-0.048 (0.027)*
Electricity	0.385	0.348	0.366	0.341	0.019 (0.026)
Education	0.469	0.173	0.424	0.169	0.045 (0.013)***
Female	0.515	0.501	0.500	0.500	0.015 (0.037)
Age	26.425	13.277	25.783	14.430	0.642 (1.050)
Protests					
Enough Food	0.363	0.321	0.347	0.316	0.016 (0.026)
Enough Water	0.309	0.339	0.282	0.323	0.026 (0.026)
Medicine	0.380	0.351	0.425	0.353	-0.046 (0.029)
Electricity	0.263	0.268	0.334	0.309	-0.072 (0.026)***
Education	0.436	0.184	0.458	0.172	-0.022 (0.015)
Female	0.498	0.501	0.509	0.500	-0.011 (0.040)
Age	25.972	13.885	23.407	12.847	2.565 (1.062)**

Column 5 reports two-sided t-tests. * $p < .1$, ** $p < .05$, *** $p < .01$

Table 2 reports covariate balance statistics comparing treated and control units. As it is shown, very few covariates are significantly imbalanced between individuals exposed before and after violent events, which is consistent with the initial intuition of our empirical strategy. We show at the end of the paper that all of our main results are statistically and substantively unchanged when we add unbalanced covariates.

We estimate the causal effect of indirect effect of exposure to political violence on various measures of trust for state institutions with the following specification:

$$Trust^{w}_{idt} = \beta^{w}_{0} + \beta^{w}_{1} Violence_{idt} + \alpha^{w}_{p} + \tau^{w}_{m} + \gamma^{w}_{y} + \mathbf{X}'_{idt}\phi^{w} + \varepsilon^{w}_{idt} \quad (1)$$

where $Trust^{w}_{idt}$ is the reported level of trust for individual i in district d, and on day t. In this specification, β_0 and β_1 are coefficients to be estimated, where β_1 measures the effect of exposure to political violence on trust. $Violence_{id}$ is an indicator which is equal to 1 if individual i was interviewed after a violent event in her district d within a particular temporal window, and 0 if the interview took place before the violent event within the same temporal window. The superscript w refers to the fact that we consider different 'window' (treatment) definitions, based on 5-day, 7-day, 10-day, 15-day, or 30-day windows. We include province fixed effects, α_p, as well as month and year fixed effects, τ_m and γ_y, respectively. $\mathbf{X}'_{idt}$ is a vector of control variables that varies across specifications. The individual level error term is denoted ε_{idt}. Robust standard errors are clustered at the level of district to account for spatial correlation in patterns of violence.

6 Empirical Results

6.1 The Effect of Violence on Trust for the President

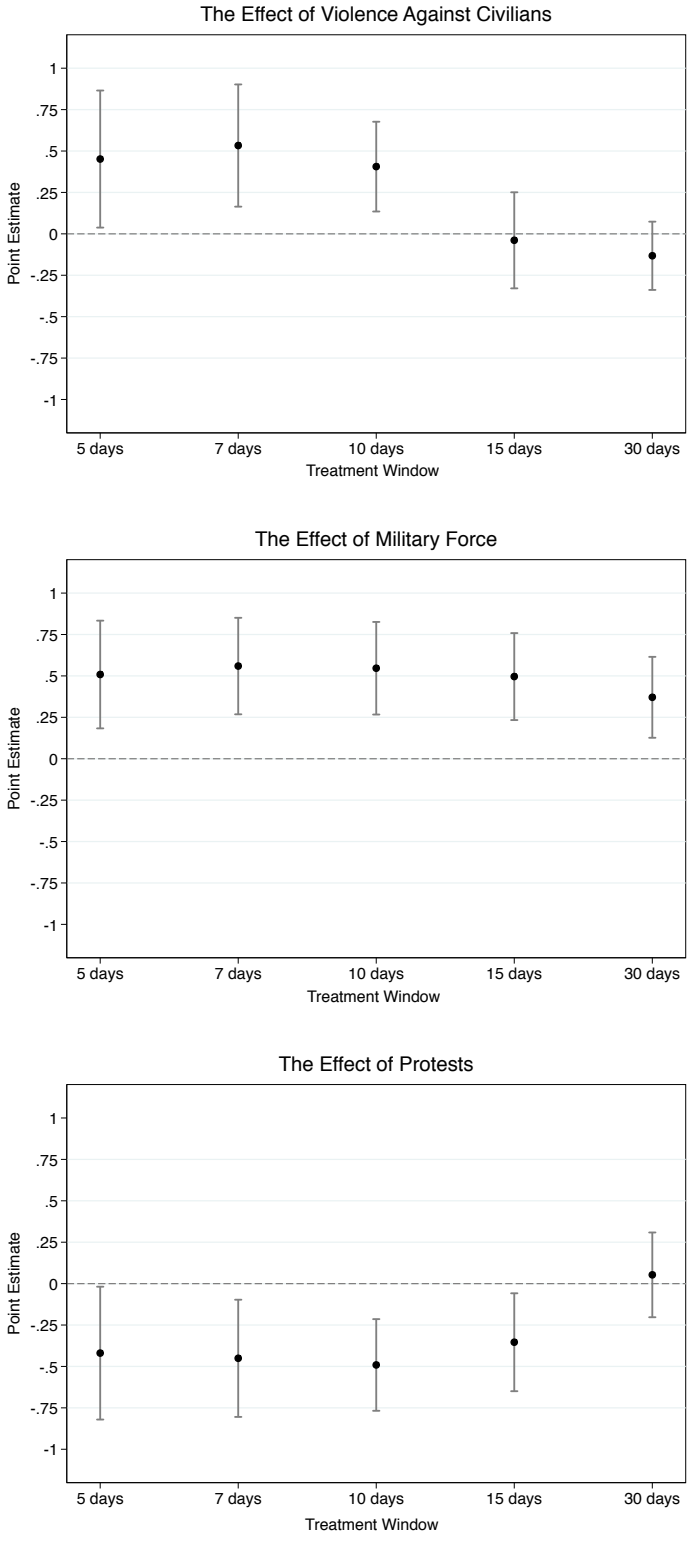
We start by estimating OLS regressions of trust in the president on different treatment definitions of violence against civilians, which are based on 5-day, 7-day, 10-day, 15-day, and 30-day windows. The results shown in Figure 2 show that recent exposure to political violence leads to higher levels of trust for the president. The effect is statistically significant at the conventional levels for the 5, 7, and 10-day treatments (regression results presented in tabular format are shown in the appendix: Tables 5, 6, and 7 respectively). As for the 15-day and 30-day treatments, the coefficient is negative, but not significantly different from zero. This evidence is indicative of an effect that tends to fade away over time.

Table 3: The Effect of Conflict Exposure on Trust for ...

	(1) *President*	(2) *Parliament*	(3) *Local Gov.*	(4) *Police*	(5) *People*
Violence, 10 days	0.41***	0.29**	0.47***	0.47***	0.02
	(0.14)	(0.12)	(0.13)	(0.14)	(0.12)
Observations	1112	1132	867	1205	664
R^2	0.123	0.109	0.109	0.098	0.113
Mil. Force, 10 days	0.55***	0.28**	0.34***	0.48***	-0.00
	(0.14)	(0.12)	(0.12)	(0.13)	(0.00)
Observations	745	749	482	798	276
R^2	0.116	0.104	0.049	0.073	0.035
Protest, 10 days	-0.49***	-0.47***	-0.36**	-0.64***	-0.18
	(0.14)	(0.12)	(0.14)	(0.11)	(0.16)
Observations	688	698	444	731	253
R^2	0.126	0.086	0.042	0.130	0.092

OLS regressions with province, month and year fixed effects.
Robust standard errors clustered by district in parentheses.
* p<.1, ** p<.05, *** p<.01 (p-values are for two-sided tests.)

Figure 2: Timing of Conflict Exposure and Trust for President



Notes: Dots indicate point estimates per Equation 1. Vertical bars indicate 95% CI.

Examining the effect of military force on civilian attitudes we find even stronger effects. Generally, conflict events perpetrated by the government, our measure of 'military force' in this case, represent a subset of the 'violence against civilians' conflict events. These effects are substantial and precisely estimated across the 5 to 30 day windows, and results are generally consist with a slower attenuation of the effect in the 15 and 30-day windows. By contrast to the preceding results, we find indirect exposure to protests to have a negative effect on trust for government actors. We interpret this difference as representing a signaling logic. Exposure to protests suggest a weakness of the state government– one that, *ceterius peribus*, cannot maintain political order. By contrast the use of military force in the countries in our sample leads to greater support for the government, which we interpret as reflecting support for the state's imposition of political order.

6.2 The Effect of Violence on Trust for Government Actors

We now move past looking at the effect of measures of conflict exposure on the head of state to examine attitudes towards a range of government actors. The top panel in Table 3 shows that exposure to violence is also positively correlated with trust in the parliament, local government officials, and the police. Interestingly, the effect on interpersonal trust –a placebo variable in our empirical analysis– is null, which further confirms the hypothesis of recents exposure to violence leading to increased support in the state. The regressions estimated in the middle panel of Table 3 focus on the effect of violence perpetrated by military forces or state-security actors. This is a more restrictive treatment definition intended to capture the effect of violence directly inflicted by the state. The data show a strong positive correlation between the treatment and trust for the president. Note that the coefficient of interest becomes smaller as the treatment window widens

In order to better understand how individuals respond to different types of political violence, we contrast how the impact of state-perpetrated violence differs from that of protests. In the bottom panel of Table 3 we find the impact of protests on trust for the president to negative, statistically significant and sizable. The magnitude of this effect

decreases in size in the 15-day window and becomes insignificant in the 30-day window showing a clear *short* term effect of protests on the attitudes of civilians. In Table 3 we examine how trust for government leaders more broadly shifts as a result of exposure to protests and also find a negative relationship. We interpret these findings as consistent with a mechanism whereby the type of violence signals the capacity and performance of the government – in this case the presence of protests signals the *weakness* of national government leaders.

6.3 Mechanisms Linking Violence to Support of the State

In order to pin down the mechanism(s) moderating the impact of political violence on trust towards government leaders we analyze additional survey questions. We focus on three questions asked of survey respondents: (1) do you fear political violence? (2) are you careful talking about politics in public? (3) do you fear punishment if you speak out against the state? Taken together, these questions help us understand the survey responses above.

Specifically, whether civilians are more trusting of the government due to fear of the state – a sort of response bias – or they legitimately support government military action in order to create order. We find support for the latter, as while respondents do clearly fear violence (see Panel A of Table 4) they are neither careful when speaking publicly about violence (Panel B) nor fear punishment if they if they choose to speak out against the government. The results with regards to 'careful taking about politics' are particularly striking. In short temporal periods after being exposed to action by military forces individuals report being statistically and substantively *less* careful in this way.

Table 4: The Effect of Military Force on...

	(1)	(2)	(3)	(4)	(5)
			Panel A: *Fear of Violence*		
Mil. Force, 5 days	0.87***				
	(0.28)				
Mil. Force, 7 days		0.87***			
		(0.28)			
Mil. Force, 10 days			0.88***		
			(0.28)		
Mil. Force, 15 days				0.54*	
				(0.30)	
Mil. Force, 30 days					0.16
					(0.17)
Observations	142	198	274	372	394
			Panel B: *Careful Talking About Poltiics?*		
Mil. Force, 5 days	-0.11				
	(0.12)				
Mil. Force, 7 days		-0.17			
		(0.11)			
Mil. Force, 10 days			-0.23**		
			(0.11)		
Mil. Force, 15 days				-0.20**	
				(0.10)	
Mil. Force, 30 days					-0.23***
					(0.09)
Observations	634	713	787	964	1094
			Panel C: *Punished if Speak Against State?*		
Mil. Force, 5 days	-0.39				
	(0.55)				
Mil. Force, 7 days		-0.39			
		(0.54)			
Mil. Force, 10 days			-0.39		
			(0.54)		
Mil. Force, 15 days				-0.31	
				(0.46)	
Mil. Force, 30 days					-0.06
					(0.28)
Observations	133	182	254	348	370

OLS regressions with province, month and year fixed effects.
Robust standard errors clustered by district in parentheses.
* p<.1, ** p<.05, *** p<.01 (p-values are for two-sided tests.)

7 Final Thoughts

We have provided robust evidence of causality between political violence and trust in the state. The main conclusion is that exposure to political violence, particularly state-perpetuated violence, increases civilian support for government leaders and institutions. We believe these findings have important implications for the understanding of why state leaders may pursue strategies of violence both against opposition groups as well as against civilians. Our interpretation is that the entrenchment of violence against civilians creates new daily insecurities, which are likely to translate in greater support for the state to enhance local political order. Additional empirical tests are to be conducted to unpack the causal mechanisms, and to explore potential heterogenous effects.

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Additional Tables for Online Appendix

Table 5: Violence Against Civilians and Trust for the President

	(1)	(2)	(3)	(4)	(5)
Violence, 5 days	0.45**				
	(0.21)				
Violence, 7 days		0.53***			
		(0.19)			
Violence, 10 days			0.41***		
			(0.14)		
Violence, 15 days				-0.04	
				(0.15)	
Violence, 30 days					-0.13
					(0.11)
Observations	838	961	1112	1381	2095
R^2	0.133	0.120	0.123	0.121	0.150

OLS regressions with province, month and year fixed effects.

Robust standard errors clustered by district in parentheses.

* p<.1, ** p<.05, *** p<.01 (p-values are for two-sided tests.)

Table 6: The Effect of Military Force on Trust for the President

	(1)	(2)	(3)	(4)	(5)
Mil. Force, 5 days	0.51***				
	(0.17)				
Mil. Force, 7 days		0.56***			
		(0.15)			
Mil. Force, 10 days			0.55***		
			(0.14)		
Mil. Force, 15 days				0.50***	
				(0.13)	
Mil. Force, 30 days					0.37***
					(0.12)
Observations	608	678	745	915	1046
R^2	0.129	0.128	0.116	0.096	0.125

OLS regressions with province, month and year fixed effects.

Robust standard errors clustered by district in parentheses.

* p<.1, ** p<.05, *** p<.01 (p-values are for two-sided tests.)

Table 7: The Effect of Protests on Trust for the President

	(1)	(2)	(3)	(4)	(5)
Protest, 5 days	-0.42**				
	(0.20)				
Protest, 7 days		-0.45**			
		(0.18)			
Protest, 10 days			-0.49***		
			(0.14)		
Protest, 15 days				-0.35**	
				(0.15)	
Protest, 30 days					0.05
					(0.13)
Observations	375	556	688	850	1697
R^2	0.085	0.097	0.126	0.128	0.143

OLS regressions with province, month and year fixed effects.
Robust standard errors clustered by district in parentheses.
* p<.1, ** p<.05, *** p<.01 (p-values are for two-sided tests.)

Table 8: The Effect of Violence Against Civilians on Trust for the President

	(1)	(2)	(3)	(4)	(5)
Violence, 5 days	0.42*				
	(0.22)				
Violence, 7 days		0.59***			
		(0.20)			
Violence, 10 days			0.47***		
			(0.14)		
Violence, 15 days				0.02	
				(0.16)	
Violence, 30 days					-0.09
					(0.11)
Unbalanced Covariates	✓	✓	✓	✓	✓
Observations	750	864	993	1228	1844
R^2	0.218	0.207	0.205	0.183	0.189

OLS with province, month and year fixed effects. * p<.1, ** p<.05, *** p<.01
Robust standard errors clustered by district. (p-values are for two-sided tests.)

Table 9: The Effect of Military Force on Trust for the President

	(1)	(2)	(3)	(4)	(5)
Mil. Force, 5 days	0.43***				
	(0.15)				
Mil. Force, 7 days		0.51***			
		(0.14)			
Mil. Force, 10 days			0.48***		
			(0.14)		
Mil. Force, 15 days				0.44***	
				(0.13)	
Mil. Force, 30 days					0.36***
					(0.12)
Unbalanced Covariates	✓	✓	✓	✓	✓
Observations	546	608	667	819	938
R^2	0.196	0.191	0.177	0.169	0.194

OLS with province, month and year fixed effects. * p<.1, ** p<.05, *** p<.01
Robust standard errors clustered by district. (p-values are for two-sided tests.)

Table 10: The Effect of Exposure to Conflict on Trust for... (with controls)

	(1) *President*	(2) *Parliament*	(3) *Local Gov*	(4) *Police*	(5) *People*
			Panel A		
Violence, 10 days	0.47***	0.24*	0.40***	0.42***	-0.01
	(0.14)	(0.14)	(0.15)	(0.15)	(0.13)
Observations	667	668	437	716	248
R^2	0.177	0.143	0.120	0.140	0.040
			Panel B		
Mil. Force, 10 days	0.48***	0.18	0.18	0.42***	0.01
	(0.14)	(0.12)	(0.12)	(0.13)	(0.03)
Observations	993	1005	774	1070	586
R^2	0.205	0.143	0.164	0.172	0.160
Unbalanced Covariates	✓	✓	✓	✓	✓

OLS with province, month and year fixed effects. * p<.1, ** p<.05, *** p<.01

Robust standard errors clustered by district. (p-values are for two-sided tests.)

Table 11: Military Force and Non-Response for *Careful*

	(1)	(2)	(3)	(4)	(5)
Mil. Force, 5 days	0.03				
	(0.03)				
Mil. Force, 7 days		0.03			
		(0.02)			
Mil. Force, 10 days			0.03		
			(0.02)		
Mil. Force, 15 days				0.03	
				(0.02)	
Mil. Force, 30 days					0.04*
					(0.02)
Observations	664	744	820	1003	1140
R^2	0.018	0.012	0.010	0.010	0.010

Table 12: Military Force and Non-Response for *Punish*

	(1)	(2)	(3)	(4)	(5)
Mil. Force, 5 days	-0.01				
	(0.01)				
Mil. Force, 7 days		-0.01			
		(0.01)			
Mil. Force, 10 days			-0.01		
			(0.01)		
Mil. Force, 15 days				-0.01	
				(0.01)	
Mil. Force, 30 days					-0.01
					(0.01)
Observations	664	744	820	1003	1140
R^2	0.063	0.084	0.061	0.060	0.058