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Civil War and Political Participation: Evidence from Uganda

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1. Introduction

Over the past decade, the body of literature on the legacy of civil war has grown to a respectable size. A prominent place is taken by microempirical research that analyzes the impact of armed conflict on human capital outcomes, measured by health or education, and to a lesser extent physical capital outcomes, measured by investment, or broad welfare measures such as income and consumption (see, e.g., Deininger 2003; Blattman and Annan 2010; Serneels and Verpoorten 2015). Although these studies are valuable, our understanding of the postwar recovery process remains limited as long as little is known about the impact of armed conflict on less tangible factors, such as social capital, political participation, and collective action. The salience of the topic has been highlighted by Blattman and Miguel (2010, 42), who, in their literature review paper, argue that “the social and institutional legacies of conflict are arguably the most important but the least understood of all war impacts.”

This article aims at increasing the understanding of the institutional legacies of civil war by analyzing the impact of the armed conflict afflicting Uganda in 1996–2007 on civic and political participation, measured by the frequency of political discussion, meeting attendance, and electoral turnout. If these factors are affected by civil war, this may have important implications for postwar economic recovery. For instance, active citizenship may translate into more successful collective action in the provision of public goods and services, increase complementarities by coordinating individual decision making, and reduce negative externalities through stipulating rules or fostering social norms. In the new growth literature, it is argued that these outcomes have the potential to lift the economy to a higher growth path (Weil 2009).

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The small number of existing studies on the institutional legacies of civil war have yielded mixed results.¹ On the one hand, early contributions in this debate argue that civil war may destroy the social fabric and the local institutions, endangering future political stability and economic recovery (Colletta and Cullen 2000; Collier et al. 2003). On the other hand, more recently, careful microlevel studies suggest that exposure to violence is not necessarily detrimental to economic development in the long run, as it may stimulate political participation and collective action. Studying the aftermath of the 1991–2002 civil war in Sierra Leone, Bellows and Miguel (2006, 2009) show that victimization during the war increased political participation. In particular, individuals directly experiencing displacement or more intense violence were more likely to attend community meetings, join political and community groups, and vote. Similar findings have been presented by Blattman (2009), who investigates the impact of rebel conscription by abduction in Uganda on postwar social and political participation. His analysis suggests that the level of violence witnessed during the war as an abductee leads to more participation in political life. This more recent evidence may appear counterintuitive but is compatible with a number of studies on posttraumatic behavior that suggest that individual tragedies may lead to personal growth and political activation (Carmil and Breznitz 1991; Tedeschi, Park, and Calhoun 1998; Tedeschi and Calhoun 2004).

A major caveat of these existing studies is the lack of preconflict data on the outcome under study. Lacking a baseline observation is problematic if the incidence of conflict or individual-level conflict experience depends in some way on individual or location-based characteristics that are related to the outcome under study. For instance, if politically active individuals are more likely to experience violence during civil war, then the causality between civil war experience and political participation may be reversed. Bellows and Miguel (2006, 2009) and Blattman (2009) have addressed this concern by arguing that much of the violence taking place was exogenous. Although their arguments are credible, it remains the case that it is difficult to put the exogeneity assumption to a test. Hence, it is valuable to complement these studies with an analysis that relies on an alternative identification strategy.

This brings us to the main contribution of the current article. In order to identify the impact of civil war on political participation, we exploit four rounds of survey data. The data, collected in Uganda by the Afrobarometer

¹ The literature on the impact of civil war on social capital also reports mixed findings (Cassar, Grosjean, and Whitt 2011; Voors et al. 2012; Rohner, Thoenig, and Zilibotti 2013; Gilligan, Pasquale, and Samii 2014).

(AB) project, are repeated cross-sections that are nationally representative and geographically stratified, including about 2,400 individuals in each round. Most important, the first round of data collection took place before the violence escalated in the north of the country, the second round was implemented when civil war was ongoing, while the third and the fourth rounds took place after peace was restored. This unique data sequence allows us to estimate a difference-in-differences model of conditional means at the district level, studying the change in individual-level political and civic participation upon a continuous treatment equal to the number of district-level conflict event days. Apart from this main distinctive feature, our research differs from previous studies in one smaller aspect: exploiting the timing of the surveys, we provide suggestive evidence as regards the duration of the impact.

We measure conflict on the basis of district-level event days taken from ACLED (Armed Conflict Location and Event Data; Raleigh et al. 2010). This implies that, rather than analyzing within-district differences across individuals with different conflict experiences, we analyze individual-level differences in civic and political participation across districts with a different number of conflict event days. This feature makes our study complementary to Blattman (2009) in the sense that the aggregate district-level impact of conflict on civic and political participation may be negative or zero, even though the within-district differences across individuals reveal a higher political participation of heavily affected individuals relative to less affected individuals.

Our findings indicate that, during the conflict, political discussion increases substantially in heavily affected districts. Looking at the period immediately after the end of massive violence, we find that both political discussion and public meeting attendance increase. Interestingly, this positive effect persists in the years after the end of the violence. Unlike previous studies, however, we do not find any positive effect of the war on formal electoral participation; that is, when comparing the voter turnout in the prewar and postwar elections, we find no relationship with the spatial pattern of the conflict.

A possible reading of our findings is that communities experiencing civil war may mobilize to address urgent local needs but that this mobilization remains local and does not translate into increased formal political participation in presidential elections. Alternatively, individuals in affected regions may have more free time (e.g., as a result of displacement in refugee camps). To explain the lack of evidence of a positive impact on turnout in 2006, one could argue that even though conflict may increase the willingness to vote, it also creates a number of constraints regarding the actual voting, for example, insecurity around the poll sites or infrastructure collapse in the war-affected areas. This reasoning, however, fails to account for the lack of a positive effect identified in

the 2011 elections. One explanation that is consistent with our findings would be that exposure to the civil war reduced the willingness to vote, perhaps because of a loss of faith of citizens in the functioning of their country's formal institutions. A robustness check run with official electoral data relative to the 1996, 2006, and 2011 elections confirms the persistent lack of an impact on voting, while additional data from the AB show a decline in the perception of free and fair elections.

The difference-in-differences estimation method, along with the inclusion of several controls at the district and individual levels, provides a solid base for our empirical analysis. Yet, two potential issues remain in establishing a causal effect of conflict on political and civic participation.

First, our results may be prone to selection bias if violence caused the migration of politically passive individuals, thereby changing the population sampled in follow-up surveys. We perform several tests to assess the role of migration. First, directly controlling for the number of migrants due to insecurity produces qualitatively identical results. Second, our results also remain when we artificially introduce the same selection bias in the prevalence sample. For instance, we exclude from the most heavily affected districts in the 2000 sample 15% of respondents, from the lower half of the distribution of political participation, but find that our results hold in a qualitative sense. We therefore conclude that attrition would have to have been improbably high and selective to cancel out our results.

The second issue relates to reversed causality, that is, violence may be the consequence rather than the cause of increased civic and political participation, for instance, because rebel recruitment may be easier in areas where political or civic mobilization is on the rise. Although, as argued by Blattman (2009), this is farfetched in the Ugandan context, where a large share of the Lord's Resistance Army was composed by abducted youths, we instrument for conflict intensity using distance measures. The results obtained confirm a persistent positive impact of violence on meeting attendance and political discussion and no effect on turnout.

In the next section we present the data and provide relevant background information on the armed conflict in Uganda. In Section III, we lay out our empirical strategy, before presenting the main results and the robustness checks in Section IV. In Section V we discuss our findings before concluding.

II. Data and Background

A. Political Participation

The data on political participation are taken from the AB, an independent, nonpartisan research project that measures the social, political, and economic

environment in Africa. We make use of four rounds of AB survey data, implemented in Uganda in 2000, 2005, 2008, and 2012.² Each of these surveys includes information on approximately 2,400 individuals of voting age (older than 18). The samples are nationally representative and geographically stratified and include both urban and rural areas. Table 1 gives details of the observations per region and survey year, showing that the northern region, where most battle events took place, is well represented in all 4 survey years. The AB data include questions on two distinct dimensions of political participation: electoral participation and more informal active citizenship. Regarding the latter, there are two questions that are broadly comparable across the 4 survey years, the first of which informs about the respondent's attendance to community meetings, while the second question inquires about how often the respondent discusses politics with friends, family, or neighbors. The formulation of these two questions is the same for 2005, 2008, and 2012 but slightly different for 2000 (for the exact formulation of the questions refer to table B1, available online). The answer categories also differ slightly. In 2000, the respondents could choose between four possible answers, which are the same for both questions: never, only once, sometimes, and often. In 2005, 2008, and 2012, the answer possibilities for the question on attending meetings were as follows: no, would never do this; no, but would if I had the chance; yes, once or twice; yes, several times; and often. For the question on the discussion of politics, the 2005, 2008, and 2012 answer possibilities were: never, occasionally, and frequently.

In order to obtain variables that are comparable across the 4 survey years, we code the variable "attend meetings" as follows: 0 = never/no, would never do this/no, but would if I had the chance; 1 = only once/ yes, once or twice; 2 = sometimes/ yes, several times; 3 = often. And we code the variable "discuss politics" in the following way: 0 = never/only once; 1 = sometimes/occasionally; 2 = often/frequently.

The question on electoral behavior, included in the 2000, 2008, and 2012 surveys, refers to the 1996, 2006, and 2011 presidential elections, respectively.³ Again, the formulation and answer categories are different but broadly comparable as shown in table B1. In the 2000 survey, respondents are asked about their voting decision with a simple yes/no answer. In 2008 and 2012, the answer categories are more detailed, including "You were not registered,"

² We do not use the AB survey implemented in 2002, as the districts particularly affected by the civil war were not included in the survey (for details on the survey instruments, we refer to <http://www.afrobarometer.org>).

³ In the 2005 survey, the electoral questions concerned local government elections instead of presidential elections. We therefore did not consider it.

TABLE 1
AFROBAROMETER SAMPLES: NUMBER OF INDIVIDUALS

	2000	2005	2008	2012
Central	751	656	672	632
East	559	584	600	640
North	369	544	535	544
West	595	616	624	584
Total	2,274	2,400	2,431	2,400

Note. Samples nationally representative for each survey.

“You voted in the elections,” “You decided not to vote,” “You could not find the polling station,” “You were prevented from voting,” “You did not have time to vote,” and “Did not vote for some other reason.” We reduced these various answer choices to a simple 0/1 coding indicating whether the respondent voted or not.

In the empirical specification, the different formulations and answer categories across years are accounted for by survey year dummies. In addition, we check the robustness of our results against different ways of coding the answers. A summary of the different coding is provided in table 2. In our robustness

TABLE 2
DESCRIPTION OF DEPENDENT VARIABLES

	Main Coding	Coding Robustness Check	Responses across Categories (%)			
			2000	2005	2008	2012
Attend meeting:						
Never	0	0	19	...	...	...
No, would never do this	0	0	...	2	7	5
No, but would do if had chance	0	0	...	20	22	22
Only once	1	0	5	...	...	...
Yes, once or twice	1	1	...	21	27	28
Sometimes	2	1	41	...	...	...
Yes, several times	2	1	...	41	34	29
Often	3	1	35	16	10	15
Discuss politics:						
Never	0	0	18	17	27	26
Only once	0	0	4	...	...	...
Sometimes/occasionally	1	0	41	54	54	57
Often/frequently	2	1	37	29	19	18
Voted:						
No	0		21	...	30	17
Yes	1		79	...	70	83

Note. Table B1 reports the exact formulation of the questions.

checks, we also use official electoral data provided by the Electoral Commission of Uganda to test for changes in electoral turnout.⁴

B. The Conflict

The conflict intensity data are taken from the ACLED, version 4, which is based on the screening of news reports and provides georeferenced information on approximately 4,506 violent events in Uganda between 1962 and 2012 (Raleigh et al. 2010).⁵ The violent events include both battles between armed groups (2,878) and attacks on civilians (1,628).

After a period of relative peace after the power seizure by Museveni in 1986, the number of event days started to rise in 1995. In northern Uganda, the Lord's Resistance Army (LRA), an armed opposition group founded in 1987 by Joseph Kony, was able to intensify its activities in 1995 mainly because of support by the Sudanese government (Dolan 2009).⁶

The bulk of LRA violence, however, fully unraveled in 2002–5 after a military operation in southern Sudan by the Ugandan army, as indicated in figure 1. This operation, called Iron Fist, intended to destroy the LRA supply bases (Dolan 2009). These bases were indeed destroyed and many rebels killed. The mission was, however, considered a failure (Allen 2006; Dolan 2009). In fact, LRA forces managed to outflank the Ugandan army and attacked further south in Ugandan districts until then relatively untouched by the conflict (e.g., Apac and Lira).

Starting from 2006, LRA activities first moved out of Uganda into southern Sudan and into the Democratic Republic of Congo (2006–8) and then further west reaching the Central African Republic after 2008 (Drew 2010). This released the pressure on civilians and opened the way to peace in northern Uganda.

C. Linking Political Participation to the Conflict

We merged the AB data with the ACLED data at the district level, which is the smallest administrative unit they have in common (the ACLED data are

⁴ All data are publicly available at <http://www.ec.or.ug/>.

⁵ The ACLED are compiled by screening news articles with language recognition software, which on average yields good results. However, in some cases press accounts may be biased, because very insecure areas may be difficult to access or because computer news screening may be sensitive to the language in which events are reported (Restrepo, Spagat, and Vargas 2006; Verpoorten 2012).

⁶ Kony's movement gathered armed groups reluctant to settle with Kampala's new government and was initially called the Lord's Salvation Army, then the United Democratic Christian Forces, and eventually, from 1994 onward, the Lord's Resistance Army (Doom and Vlassenroot 1999; Allen 2006). The LRA received assistance from Sudan in retaliation for Ugandan support of a rebel group operating in southern Sudan.

Figure 1. Lord's Resistance Army violence in Uganda

geocoded, but the AB data are not until 2008). Doing so yields a data set of approximately 9,502 individual-level observations across 33 districts. In what follows, we use the event days reported in the ACLED data as a treatment to study the change in political participation upon conflict. Before doing so, two issues need to be discussed.

First, although LRA violence escalated after our baseline year, a non-negligible number of event days took place before 2000. About 85% of this pre-2000 LRA activity was confined to one particular geographic region, Acholiland (Gulu and Kitgum Districts; see fig. 2), while LRA violence outside Acholiland only took off after 2000. Hence, while the estimated LRA treatment in Acholiland should be interpreted as the impact of continued and escalating violence on social capital, a cleaner treatment effect can be discerned when focusing on LRA activities outside Acholiland. Therefore, we also run a specification of our empirical model when omitting the Acholi districts (in Sec. IV).

Second, that the AB survey was conducted in times of violence as well as amid a refugee crisis after violence raises the issue of sample selection bias (Nannyonjo 2005; Allen 2006; Joireman, Sawyer, and Wilhoit 2010). Because of insecurity, the surveys may have excluded the most affected individuals in certain districts. In order to verify this, we consulted the local AB team that conducted the survey. We learned that, when an enumeration area within a

Figure 2. Administrative map of Uganda. A color version of this figure is available online

district was highly insecure, the enumeration area was replaced by a more secure area within the same district. The substitution always followed the composition of the original sample in terms of language and ethnicity as well as the direction of displacement of the individuals in the original sample, often ending up with a sample of within-district internally displaced people (IDP; correspondence with Francis Kibirige, 2011). This approach was facilitated by the duplication of local administrative structures in the IDP camps and the relocation of the IDP within their own district.

Another potential consequence of the refugee crisis is a changed composition of the districts' population across the survey rounds due to direct violence or out-migration. If, for instance, the likelihood of migrating is (negatively) correlated with political participation, the changes in political participation observed in the area affected by the violence may be entirely driven by selection.⁷ In Section IV we run several tests to show that sample selection has to be implausibly large to cancel out our results.

III. Empirical Strategy

To identify the impact of event days on political participation, we use a difference-in-difference estimation that exploits variation in event days across districts and through time. We study the impact on political participation both when the conflict is ongoing, using the 2005 survey, and when the conflict has ended, using the 2008 and 2012 surveys. In each of these cases, the 2000 survey is the baseline. The treatment is the number of the event days taking place between the AB survey years 2000–2005, 2000–2008, and 2000–2012, respectively. When we estimate the effect on turnout, we consider the events taking place in 1996–2006 and 1996–2010, since the question on turnout included in the surveys referred to the 1996, 2006, and 2011 presidential elections. The treated group are the households located in the districts where these events took place. In other words, the empirical identification strategy relies on the comparison of the change in political participation in 2000–2005, 2000–2008 (1996–2006 for turnout), and 2000–2012 (1996–2011 for turnout) across areas with low violence intensity and areas with high violence intensity.

Formally, our empirical equations can be written as follows:

$$\begin{aligned} P_{i,t,d} &= \alpha_1 \text{year}_{2005} + \alpha_2 (B_{2000-2005,d} \times \text{year}_{2005}) + X'_{i,t,d} \Psi + \eta_d + \varepsilon_{i,t,d}, \\ P_{i,t,d} &= \beta_1 \text{year}_{2008} + \beta_2 (B_{2000-2008,d} \times \text{year}_{2008}) + X'_{i,t,d} \Psi' + \eta'_d + \varepsilon'_{i,t,d}, \\ P_{i,t,d} &= \gamma_1 \text{year}_{2012} + \gamma_2 (B_{2000-2012,d} \times \text{year}_{2012}) + X'_{i,t,d} \Psi'' + \eta''_d + \varepsilon''_{i,t,d}, \\ V_{i,t,d} &= \delta_1 \text{year}_{2008} + \delta_2 (B_{1996-2006,d} \times \text{year}_{2008}) + X'_{i,t,d} \Psi''' + \eta'''_d + \varepsilon'''_{i,t,d}, \\ V_{i,t,d} &= \mu_1 \text{year}_{2012} + \mu_2 (B_{1996-2012,d} \times \text{year}_{2012}) + X'_{i,t,d} \Psi'''' + \eta''''_d + \varepsilon''''_{i,t,d}, \end{aligned}$$

where i indexes individuals, d districts, and t survey years. The variable $P_{i,t,d}$ denotes one of our two informal political participation measures (meeting attendance, political discussion), and $V_{i,t,d}$ denotes electoral turnout. Variable B denotes the (logged) district-level event days involving LRA, which occurred

⁷ Gilligan et al. (2014) find in Nepal evidence of this *purging mechanism*, by which less social persons disproportionately leave communities plagued by war.

in the various periods; year_{2005} , year_{2008} , and year_{2012} are indicator variables taking 1 for respondents in the 2005, 2008, 2012 surveys, respectively. The vector $X_{i,t,d}$ denotes a set of individual-level covariates reported in the AB, which include the respondent's age, age^2 , educational level, a gender indicator variable, and an indicator variable that equals 1 if the respondent lives in an urban location, and 10 fixed effects for the respondent's ethnicity. Finally, η_d denotes district fixed effects, which are included to capture district-specific unobserved factors that may affect the level of political participation.

It is noteworthy that the period under consideration was characterized by an important change in the macropolitical environment, namely, the 2005 constitutional reform that transformed Uganda from a one-party to a multiparty political system. If the reform affected incentives for political participation differently across the country, this event may affect our results. For instance, it is not unlikely that the impact of the reform on political participation varies with the support for the prereform one-party regime and that this support in turn correlates with conflict intensity. We address this potential omitted variable by allowing the change in political participation to vary with the district-level support for the prereform one-party regime, measured by the share of votes gained by Museveni in the 1996 elections. More specifically, we add to the controls an interaction term of the posttreatment year with the district-level share of the population who voted for Museveni in 1996, as reported by the Electoral Commission of Uganda (<http://www.ec.or.ug>). We also allow the change in political participation to vary with the district-level intensity of other violent events recorded by the ACLED but unrelated with the LRA's operations, by adding to our model an interaction term of the posttreatment year with the number of non-LRA event days recorded in the district during the period considered.

All explanatory variables are summarized in table 3. The coefficients of interest are α_2 , β_2 , γ_2 , δ_2 , and μ_2 , which capture the treatment effect of the event days on political participation.

A crucial assumption behind the difference-in-difference estimator is that districts affected by violence would have followed a similar trend as those unaffected, had they not been exposed to violence. Ideally, we should compare the trends in our political participation variables across districts before the conflict. Only one AB survey, however, was collected in Uganda before LRA violence escalated. As a second-best approach, we use previous electoral data to test whether before violence started, violent and nonviolent districts were following a similar trend in electoral turnout.⁸ The two Ugandan presidential

⁸ We thank an anonymous referee for this useful suggestion.

TABLE 3
DESCRIPTIVE STATISTICS

	2000	2005	2008	2012
Dependent variable:				
Attend meeting	1.9	1.5	1.2	1.3
Discuss politics	1.2	1.1	.9	.9
Voted	.8	. . .	.7	.8
Explanatory variable:				
LRA event days 2001–5	15.4	18.7		
LRA event days 2001–8	17.5		30.7	
LRA event days 2001–12	17.5			21.8
LRA event days 1996–2006	20.3		35.9	
LRA event days 1996–2012	22.5			22.9
Other event days 2001–5	5.8	6.5		
Other event days 2001–8	9.6		10.7	
Other event days 2001–12	12.2			13.3
Other event days 1996–2006	16.6		17.1	
Other event days 1996–2012	22.5			22.9
Age	33.4	32.6	33.7	35.2
Age ²	1,275	1,207	1,287	1,402
Male	.50	.50	.61	.50
Urban	.14	.30	.20	.14
Education level	3.14	3.35	3.37	3.28
Museveni support	.72	.68	.69	.71
Ethnicity (%):				
Ateso	4.76	7.25	6.75	7.3
Luganda	25.36	18.63	21.6	20.5
Lugbara	2.91	4.92	3.09	3.9
Lumasada	5.15	4	4.9	4.2
Luo (includes Acholi)	12.86	12.92	15.1	11.3
Lusoga	9.91	9.88	10.2	10.1
Rukiga	6.43	6.25	4.6	5.6
Runyankole	2.95	11.04	11.6	12.7
Rutooro	11.93	3.63	2.9	2.0
Other	17.75	21.5	19.4	20.6

Note. Education level is a categorical variable taking values between 0 and 6, with 0 denoting no schooling and 6 postuniversity education; the Acholi ethnic group is a subgroup of the broader category of Luo. LRA = Lord's Resistance Army.

elections preceding the conflict considered by this study took place in 1980 and 1996. The 1980 electoral results, however, have been heavily manipulated by the government (Nohlen, Thibaut, and Krennerich 1999). Moreover, in 1980 the Ugandan political system was profoundly different than in 1996, making the comparison less meaningful. We therefore use 1996 and 2001 elections to test the plausibility of the difference-in-difference assumption. Even though some violence had already occurred by 2001, the actual escalation started in 2002 (see fig. 1). The results of this test confirm that violent and nonviolent districts were not following different trends in electoral turnout

before 2001, suggesting that political participation followed similar trends across violent and nonviolent districts before the conflict erupted in the north.⁹

As respondents' answers to the political participation questions are categorical, two different estimation strategies are possible. We convert the categorical answers into a variable assigning a number to each response and to estimate our empirical model by ordinary least squares (OLS). This yields a measure of political discussion that takes the value 0, 1, or 2 and a measure of meeting attendance that takes on the value 0, 1, 2 or 3, where higher numbers indicate more frequent political discussion and meeting attendance, respectively (see also table 2). OLS has the advantage of being traceable and easy to interpret. Besides, Angrist and Pischke (2008) argue that OLS consistently estimates the linear conditional expectation function and minimizes mean² error, and they recommend linear probability models over limited dependent variable models like probit or logit. In any case, we also estimate an ordered-probit (o-probit) model maintaining the categorical nature of the response.

IV. Results

A. Baseline Estimates

Tables 4 and 5 show the OLS and o-probit results, respectively. Since standard errors are likely to be correlated both within district and within urban and rural strata, we report standard errors clustered at the district level across urban and rural strata. The findings indicate that individuals living in districts heavily affected by LRA violence experienced a positive and significant change in political discussion since 2000, relative to other districts. This finding applies both to 2000–2005, when the LRA fighting was still ongoing, and to the postviolence period. Meeting attendance was also positively affected, but only after the violence ceased. The quantitative impact is rather large. According to the results reported in table 4 column 2, doubling the number of LRA event days experienced in 2000–2005 (100% increase) raises political discussion by 0.087, a considerable increase given its average of approximately 1.2 across the survey years 2000 and 2005. The marginal effect of the o-probit estimate in table 5 column 2 indicates that a 1% increase in violence raises the probability of discussing politics frequently by roughly 0.05 percentage points.

Moving to formal political participation, we do not find significantly different changes between 1996 and 2006 in formal electoral participation across district-level conflict intensity (cols. 5). For the 2011 elections, however, districts affected by LRA violence experienced a significant decrease in turnout

⁹ The results of this test are reported in table A2.

TABLE 4
ORDINARY LEAST SQUARES RESULTS

	2000–2005			2000–2008			2000–2012		
	Attend Meeting (1)	Discuss Politics (2)		Attend Meeting (3)	Discuss Politics (4)	Voted (5)	Attend Meeting (6)	Discuss Politics (7)	Voted (8)
Event days 2001–5 × year 2005	–.021 (.039)	.087*** (.025)							
Event days 2001–8 × year 2008				.101** (.047)	.056** (.025)				
Event days 1996–2006 × year 2008						.002 (.012)			
Event days 2001–12 × year 2012							.145*** (.045)	.079** (.033)	
Event days 1996–2012 × year 2012									–.032** (.015)
Year 2005	–.266 (.254)	–.501*** (.127)							
Year 2008				–1.064*** (.260)	–.372** (.149)	–.040 (.069)			
Year 2012							–.857*** (.280)	–.557*** (.193)	.064 (.089)
Other interacted controls	✓	✓		✓	✓	✓	✓	✓	✓
Individual controls	✓	✓		✓	✓	✓	✓	✓	✓
District fixed effects	✓	✓		✓	✓	✓	✓	✓	✓
Observations	4,517	4,512		4,543	4,541	4,547	4,417	4,428	4,442
R ²	.175	.105		.194	.118	.211	.218	.134	.192

Note. Unit of observation is an individual. Standard errors clustered at the district level across urban and rural strata in parentheses. For the meaning of the individual controls, see table 3.

** $p < .05$.

*** $p < .01$.

TABLE 5
ORDERED-PROBIT RESULTS

	2000–2005			2000–2008			2000–2012		
	Attend Meeting (1)	Discuss Politics (2)		Attend Meeting (3)	Discuss Politics (4)	Voted (5)	Attend Meeting (6)	Discuss Politics (7)	Voted (8)
Event days 2001–5 × year 2005	–.015 (.044)	.145*** (.041)							
Event days 2001–8 × year 2008				.127** (.054)	.090** (.041)				
Event days 1996–2006 × year 2008						–.016 (.045)			
Event days 2001–12 × year 2012							.181*** (.055)	.132** (.055)	
Event days 1996–2012 × year 2012									–.088 (.073)
Year 2005	–.427 (.281)	–.836*** (.212)							
Year 2008				–1.289*** (.306)	–.597** (.243)	–.098 (.270)			
Year 2012							–1.075*** (.343)	–.927*** (.322)	.108 (.454)
Other interacted controls	✓	✓		✓	✓	✓	✓	✓	✓
Individual controls	✓	✓		✓	✓	✓	✓	✓	✓
District fixed effects	✓	✓		✓	✓	✓	✓	✓	✓
Observations	4,517	4,512		4,543	4,541	4,547	4,417	4,428	4,442
Pseudo-R ²	.076	.053		.083	.060	.199	.090	.068	.203

Note. Unit of observation is an individual. Standard errors clustered at the district level across urban and rural strata in parentheses. For the meaning of the individual controls, see table 3.

** $p < .05$.

*** $p < .01$.

(compared to 1996), relative to other districts, although the coefficient is insignificant in the o-probit estimation (cols. 8).

B. Robustness Checks

We implement a number of additional tests to check the robustness of our results. We first restrict the analysis to districts that were relatively unaffected by violence before 2000. Next, we thoroughly address the potential bias due to conflict-induced migration. Third, we test the robustness of our finding relating to formal political participation using official data on electoral turnout. Next, we check the robustness of our results to alternative estimation methods or variables' codification. Finally, to tackle the potential endogeneity of conflict distribution, we instrument for conflict intensity using distance measures. All results are condensed in table 6, in which we report only the coefficients for the interaction terms of interest.

When presenting figure 1 we pointed out that, although violence escalated after our baseline year, a nonnegligible number of events took place before 2000. To account for the timing of the violence, we drop the Acholi districts, where LRA violence erupted before 2000. The results of this exercise, reported in the first row of table 6, are qualitatively similar to our main specification presented in table 4.

Another potential caveat relates to sample selection, which may drive our results if the population sampled dramatically changed across the different survey rounds. In particular, the refugee crisis unraveling during the years studied might invalidate our findings if attrition (due to out-migration, abduction by the LRA, or killings) correlates with our political participation variables, for instance, if individuals with lower political participation in the 2000 survey round dropped out in the 2005, 2008, and 2012 rounds. The test reported in table 6, in which Acholiland, where the refugees crisis reached dramatic proportions, is dropped from our sample, constitutes a first piece of evidence that displacement is not the driving factor behind our findings. Had it been the main driver, we would expect our effects to substantially change.

Two alternative strategies are adopted to check whether selective migration drives our results. First, we directly control for district out-migration due to insecurity. Since the AB survey does not contain information on migration, we turn to Ugandan Living Standards Measurement Study (LSMS) data (Uganda Bureau of Statistics 2006). In the 2005 LSMS sample, during the peak of violence, 10.9% of individuals moved because of insecurity. About 70% of these individuals originated from the most affected districts Gulu, Kitgum, Apac, or Lira. Amending our model controlling for out-migration

TABLE 6
ROBUSTNESS CHECKS

	2000–2005			2000–2008			2000–2012		
	Attend Meeting (1)	Discuss Politics (2)		Attend Meeting (3)	Discuss Politics (4)	Voted (5)	Attend Meeting (6)	Discuss Politics (7)	Voted (8)
Drop Acholiland	−.014 (.044)	.089*** (.028)		.093* (.052)	.070** (.029)	.007 (.014)	.159*** (.049)	.070** (.052)	−.026 (.017)
Control for migration	−.010 (.047)	.071*** (.027)		.125*** (.047)	.048* (.026)	.008 (.013)	.160*** (.048)	.068** (.034)	−.028* (.017)
Drop bottom 15% from 2000 north	−.054 (.040)	.070*** (.024)		.070 (.047)	.046* (.025)	−.010 (.013)	.120*** (.045)	.066** (.033)	−.042*** (.016)
Drop bottom 50% <24 from 2000 north	−.031 (.040)	.079*** (.025)		.089* (.047)	.051** (.025)	−.006 (.013)	.136*** (.045)	.071** (.033)	−.039** (.016)
Drop Luo outside Acholiland	.013 (.034)	.101*** (.024)		.108** (.048)	.050* (.026)	−.001 (.012)	.146*** (.047)	.076** (.036)	−.017 (.016)
Drop Luo outside Acholiland and bottom 50% <24 from 2000 north	.008 (.036)	.090*** (.024)		.103** (.048)	.043 (.027)	−.007 (.013)	.141*** (.049)	.069** (.035)	−.023 (.017)
Official electoral data ^a						−.043** (.019)			−.072** (.031)
Alternative coding of answers	−.016 (.060)	.102** (.042)		.139** (.057)	.112** (.053)		.217*** (.055)	.042 (.089)	
Logged events instead of logged event days	−.024 (.032)	.072*** (.025)		.073* (.041)	.045* (.023)	.001 (.010)	.126*** (.043)	.065** (.030)	−.036*** (.014)
IV second stage ^b	.027 (.051)	.134*** (.035)		.127* (.067)	.083** (.033)	.024 (.020)	.184*** (.064)	.111** (.048)	−.023 (.025)

Note. Only interaction term of interest is reported: event days 2001–5 × year 2005 for cols. 1 and 2; event days 2001–8 × year 2008 for cols. 3 and 4; event days 1996–2006 × year 2008 for col. 5; event days 2001–10 × year 2012 for cols. 6 and 7; and event days 1996–2010 × year 2012 for col. 8. Unit of observation is an individual. Standard errors clustered at the district level across urban and rural strata in parentheses. Included control variables are as in table 4.

^a Unit of observation is a district. Robust standard errors in parentheses.

^b Hausman test rejects the null hypothesis of exogeneity in none of the regressions.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

due to insecurity produces qualitatively identical results (second row of table 6).

An alternative strategy to gauge the potential impact of selective migration consists in excluding respondents with low political participation from our 2000 sample in areas later affected by the violence. Introducing in the 2000 survey the same selective conflict-induced migration that may occur in the later surveys allows us to assess to what extent such a selection may affect our results. If our results remain largely unaffected, the potential bias of selective migration is low. If results are turned upside down, we need to be very cautious. Excluding 15% of respondents from the bottom half of the distribution of civic participation only marginally affects our qualitative results (third row of table 6). In other words, even when assuming a rather large level of attrition and strong negative correlation between political participation and migration, our findings remain. The alternative of dropping 50% of respondents with the lowest political participation only among youth (less than 24 years old), which are more directly affected by LRA operations and likely to be most mobile, also yields similar results (fourth row of table 6).

Selection may also affect our analysis if relatively safe districts (our control group) are net recipients of low political participation migrants during the period considered. The average political participation in control districts would artificially decrease in 2005 (2008 and 2012), creating a differential trend in affected versus unaffected districts purely due to change in the population composition. We design a test to assess the importance of this bias. About 90% of the violence was concentrated in districts of Luo ethnolinguistic origin.¹⁰ Migration due to insecurity from violent districts to relatively safe ones is therefore almost exclusively of Luo individuals. Since AB does not register information on migration, we instead remove all Luo respondents surveyed outside the districts affected by the violence. In the north, Luo account for about 60% of the population, while their share is only around 1% in the other regions. By removing all Luo individuals outside the northern region, we minimize the risk of bias due to migration of Luo individuals to safe districts. The results of this exercise are reported in the fifth row of table 6. Finally, we combine the latter (dropping all Luo outside north) with the previous one (dropping from the 2000 sample 50% of youth with the lowest political participation in the north) in the sixth row of table 6. Both tests only marginally affect our results, strongly suggesting that selection bias cannot entirely explain our findings. Turnout in 2011 remains negative but becomes, however, insignificant.

¹⁰ Acholi represent a subgroup of Luo.

Information concerning voting behavior may suffer from reporting bias. To further investigate the impact of violence on electoral turnout, we therefore use the official electoral data of the three presidential elections in 1996, 2006, and 2011. We replicate our difference-in-differences estimate using official turnout as the dependent variable and control for district fixed effects as in our main specification. The interaction terms of interest, capturing the impact of violence on turnout, are reported in row 7 of table 6. In spite of the entirely different sources of the data and the dramatic drop in the number of observations (from about 4,500 to 66), the results are consistent with our survey result on voting. If anything, experiencing violence reduced electoral participation.

We also estimate a probit model with an alternative binary coding for our dependent variables (the coding is given in table 2). The results are reported in row 8 of table 6. Our results are qualitatively identical, with one exception: the estimated impact on political discussion in 2012 becomes insignificant.¹¹

Next, we repeat our main specification when measuring conflict intensity by the logged number of conflict events per district, instead of logged conflict days. The results displayed in row 9 of table 6 are very similar.

The positive correlation between additional event days and active citizenship that is documented so far is consistent with the hypothesis that conflict, in the form of additional battle events, increases active citizenship but does not stimulate electoral participation. However, this correlation does not necessarily imply causality. The increase of political participation itself may be a determinant of conflict (reversed causality). For instance, rebel leaders may find it easier to recruit in districts in which civic participation and the receptivity to political indoctrination is on the rise or in which voters are losing faith in the functioning of formal democratic institutions. If increased political activism fed the existing armed conflict, our OLS coefficients are biased upward. In Section III, we argued that violent and nonviolent districts were not following different trends in electoral turnout before 2001, validating our empirical strategy. Additionally, we directly regress our political participation variables in 2000 on later conflict intensity and find no significant correlation for meeting attendance and electoral turnout and a negative correlation with political discussion.¹² These patterns suggest that it is much more plausible that conflict caused participation rather than the other way round.

As a final robustness check, we turn to an instrumental variable (IV) strategy to address the potential endogeneity in conflict distribution. In addition to the

¹¹ Notice that voting is excluded from this robustness check, as this variable is already coded binary.

¹² The results of this exercise are reported in table A3.

potential reverse causality, which is unlikely as argued above, conflict events may be measured with error. For instance, events in very insecure areas may have received little news coverage, leading to an underestimation of violence in the most affected areas (Verpoorten 2012). An IV strategy may partly correct for this.

As an excluded instrument for LRA violence, we use the distance to Sudan. The support of the Sudanese government to the LRA, which provided the rebels with logistics and bases on the Sudanese territory, represents a milestone in the conflict in Uganda. It determined the geographical distribution of the violence that concentrated in locations from where the rebels could quickly cross the border after their raids. Hence, we expect the distance to Sudan to predict conflict intensity. In order for this distance measure to be a valid instrument, it should not have an effect on the change in political participation, apart from its effect via violence. This may be a strong assumption, as regionalism may take a prominent place in Ugandan politics. However, we believe that, conditional on controlling for the support for Museveni, district fixed effects, and ethnicity of respondents, the exclusion restriction is likely to hold.

The first-stage results indicate that the instrument is relevant since the estimated coefficient on the instrument is significantly different from 0. In all cases, the values for the *F*-test largely exceed the rule-of-thumb value of 10 for significance in the first stage. The relevance of our instruments is also confirmed by the Anderson canonical correlations likelihood-ratio test (see table A1).

The interaction terms of interest of the second stage, reported in the last row of table 6, are similar to the OLS results. The point estimates remain remarkably similar, but the negative impact on turnout in 2012 elections is no longer significant. Thus, according to these results, experiencing the violence of a civil war would increase civic participation but leave turnout unaffected.

V. Discussion

We found conflict to have a positive and significant impact on meeting attendance, not in 2005 at the peak of the conflict but in 2008—immediately after the end of massive violence—as well as in 2012. Our analysis of political discussion yielded positive coefficients across all years and across all robustness checks, and these coefficients were significantly different from zero in most cases. Taken together, these findings indicate that, if anything, violence had a positive impact on informal political participation. Our results on formal electoral participation paint a very different picture, with all estimated coefficients virtually zero or negative—at times significantly so.

Can we come up with an explanation that is consistent with these findings? At the risk of being too speculative, we make an attempt.

A number of potential factors may account for the increased participation in political discussions and public meetings. First, the exceptionally unsafe environment may have raised the frequency of interaction to coordinate action to protect the villages from LRA raids and children from abduction. Second, the concentration of the population in IDP camps in the affected areas may have involved new administrative procedures such as “compulsory” meetings for the organization of daily life in the camps (Dolan 2009). A third potential mediating factor between violence and increased informal political participation is the presence of nongovernmental organizations (NGOs), whose activities increased in response to the humanitarian needs caused by the conflict. Typically, NGOs encourage the population to engage in participatory meetings to take advantage of their services. Finally, our results on increased informal political participation fit well the influential resource model of political participation proposed by Brady, Verba, and Schlozman (1995), who explain variation in political participation in terms of political interest but also the endowments of the resources needed to engage in different political activities. According to this theory, one of the key determinants for engaging in activities like meeting attendance is the availability of free time. The dramatic refugee crisis in northern Uganda substantially disrupted the economic life of the local population, leaving them with more time available for engaging in local political activities.

We turn to the data to tentatively assess the plausibility of the various mechanisms. First, we collect figures of the IDP population by districts as reported by the Internal Displacement Monitoring Centre for the years in which AB surveys were implemented (<http://www.internal-displacement.org/countries/Uganda>). Second, we collect data on the presence of NGOs at the district level from the Uganda National NGO Directory, a database initiated by the Uganda National NGO Forum in November 2009 to serve national and international development partners (<http://www.ugandangodirectory.org/>). Unfortunately, the database does not claim to be exhaustive, nor does it record NGOs activities over time. We therefore consider the figures reported in the directory to roughly capture the distribution of NGOs in more recent years (2009–13). We create two measures of district-level NGOs presence: one reporting the total number of NGOs operating in the district, the second focusing only on community-based NGOs.

In table 7 we show the results of our tests. As in table 6, we only report the coefficient of the interaction term between violence and the relevant year dummy. In the first row, we control for the share of IDP population over

TABLE 7
TESTING POSSIBLE MECHANISMS

	2000–2005		2000–2008			2000–2012		
	Attend Meeting (1)	Discuss Politics (2)	Attend Meeting (3)	Discuss Politics (4)	Voted (5)	Attend Meeting (6)	Discuss Politics (7)	Voted (8)
Control for IDP population	–.008 (.050)	.078** (.031)	.088* (.053)	.071** (.029)	.005 (.014)	.150*** (.057)	.066* (.039)	–.039** (.018)
Control for NGOs presence			.172*** (.057)	.079** (.032)	.001 (.014)	.147** (.060)	.076* (.042)	–.036** (.018)
Control for community-based NGOs presence			.108* (.057)	.055* (.030)	.001 (.012)	.121** (.053)	.069* (.038)	–.026 (.016)

Note. Only interaction term of interest is reported: event days 2001–5 × year 2005 for cols. 1 and 2; event days 2001–8 × year 2008 for cols. 3 and 4; event days 1996–2006 × year 2008 for col. 5; event days 2001–12 × year 2012 for cols. 6 and 7; and event days 1996–2012 × year 2012 for col. 8. Unit of observation is an individual. Standard errors clustered at the district level across urban and rural strata in parentheses. Included control variables are as in table 4. IDP = internally displaced people; NGOs = nongovernmental organizations.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

total population by district. Our results remain qualitatively identical, and the magnitude is very similar, suggesting that the concentration of population in IDP camps cannot explain the increase in informal political participation observed. In the next two rows we control for the number of NGOs and community-based NGOs operating in the district, respectively. Given the time span covered by the NGOs data, we restrict the test only to the specification of the model focusing on the impact of violence on political participation in 2008 and 2012. Our original results are barely affected by the inclusion of the NGO variables. Even though the quality of the data does not allow us to dismiss this potential channel, the results in table 7 suggest that the increased informal political participation observed in violent districts cannot be entirely explained by NGOs' mobilization.

The free time factor may be a more likely mediator of our results, because the special wartime circumstances cannot account for the persisting effect of conflict on informal political participation in 2012. Data constraints do not allow for a direct test of this mechanism.¹³ Secondary sources provide, however,

¹³ The AB question about employment status was not asked consistently across the different survey years included in the analysis.

some evidence for a strong negative impact of the violence on working time. Lehrer (2010), for instance, argues that a culture of male idleness developed in IDP camps in northern Uganda and that the length of the residence in a IDP camp has a strong negative impact on the time spent working. Hence, the lack of work opportunities may have left refugees with more time available for engaging in local political activities.

Whatever the nature of the impulses given by the conflict to informal political participation, its effects were still operating after the emergency had ceased. In contrast, we find no positive impact of violence on electoral turnout of individuals in affected districts compared to those in unaffected districts. The results based on actual electoral data even suggest a decrease in turnout in violent districts. This does not necessarily contradict the findings from previous studies that documented increased voter turnout among individuals exposed to violence compared to individuals who were less exposed to violence in the same district (Blattman 2009). Why may district-level formal political participation on average not be affected by violence?

A first possibility is that, while violence may stimulate interest in politics and result in more free time, it also creates a more challenging environment to exercise the voting right. In particular, the collapse of local infrastructure, population displacement, and safety concerns at the time of elections may constrain individuals willing to vote. For instance, although the LRA regularly announced unilateral cease-fire around election days, their credibility may have been questionable, while at the same time the large presence of the government regular army may have discouraged voters in traditionally pro-opposition districts. These constraints may be individual specific, resulting in reduced participation for some and increased participation of others, for example, those who are more fearless and able to face the above-described challenges.

One may also conjecture that local violence simply did not raise the genuine interest for national-level politics or at least national elections. In other words, while conflict may have affected informal political participation at the local level where the emergencies were mostly perceived, it may have failed to affect the general attitude with respect to nationwide politics. On the basis of the persistence of the nonresult on voting, even several years after the end of violence, the second explanation is more plausible than the first. After all, if the lack of increase in turnout in 2005 was a result of the exceptionally dangerous environment in the districts affected by LRA activities, we should observe an increase in turnout in the 2011 elections, when the risks incurred by voters were substantially lower.

The lack of any effect of violence on the 2011 turnout indicates that the violent environment was not the only element constraining electoral partici-

pation and suggests instead that after a long exposure to the civil war, some individuals may have lost faith or interest in the functioning of their country's democratic institutions.

To further investigate whether the latter interpretation is plausible, we turn back to the AB data, studying the answers to two questions that capture the level of support of and faith in the democratic institutions. The first question inquires about the respondent's support for democracy. The second one asks the respondent to rate the freeness and fairness of the last presidential election.

Table 8 reports the results of running our estimation using as dependent variables "support for democracy" and "elections' freeness and fairness." The support for democracy does not show any differential pattern in war-torn districts (cols. 1, 2, and 4). Similarly, individuals living in areas affected by the violence did not rate differently 2006 elections with respect to the rest of the country (col. 3). We do find, however, a more pronounced decrease in the perceived freeness and fairness of 2011 elections in areas more heavily hit

TABLE 8
IMPACT OF VIOLENCE ON INTEREST AND FAITH IN DEMOCRACY

	2000–2005 Democracy Support (1)	2000–2008		2000–2012	
		Democracy Support (2)	Free and Fair Elections (3)	Democracy Support (4)	Free and Fair Elections (5)
Event days 1996–2006 × year 2005	.012 (.016)				
Event days 1996–2006 × year 2008		–.001 (.014)	.010 (.016)		
Event days 1996–2012 × year 2012				.014 (.014)	–.047*** (.017)
Year 2005	–.150* (.080)				
Year 2008		–.009 (.074)	–.181** (.085)		
Year 2012				–.062 (.074)	.352*** (.091)
Other interacted controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
District fixed effects	✓	✓	✓	✓	✓
Observations	3,542	4,097	4,117	4,035	4,021
R ²	.052	.047	.271	.119	.212

Note. Unit of observation is an individual. Standard errors clustered at the district level across urban and rural strata in parentheses. For the meaning of the individual controls, see table 3.

* $p < .1$.

** $p < .05$.

*** $p < .01$.

by the violence (col. 5). In general, the results of this exercise suggest that the experience of violence in northern Uganda did not increase the interest and faith in democratic institutions.

Once more, our results are consistent with the resource model of political participation (Brady et al. 1995). The key determinant explaining the decision to vote in the resource model is political interest, which in the case of Uganda seems to have remained virtually unaffected by the violence.

To place our result in the context of the existing studies, which suggest a positive impact of violence on electoral turnout (Bellows and Miguel 2006, 2009), we highlight an important contextual factor. Namely, during the immediate postviolence period in Uganda, the Acholi ethnic group, the core group of and affected by the LRA, remained largely excluded from any power-sharing arrangement in the country (Lindemann 2011). This may have negatively affected the interest of Acholi individuals for national politics, even after the conflict's termination.

VI. Conclusion

This article adds to the fast emerging literature that seeks to understand the social and institutional legacies of civil war. We analyzed the impact of the armed conflict disrupting northern Uganda in 1996–2007 on informal and formal political participation, measured by the frequency of political discussion, meeting attendance, and voter turnout.

This study differs from the handful of existing microlevel studies in two main ways. First, relying on a survey sequence that started before the spiral of violence fully unraveled, we perform difference-in-differences estimation. Second, exploiting the geographic variation as well as the timing of the surveys, we provide suggestive evidence as regards the time path of the impact. We find that experiencing violence substantially stimulates political discussion and local meeting attendance in the immediate postwar years. While part of the local mobilization may be a by-product of the postwar humanitarian activities, the impact persists even half a decade after the violence ceased.

We fail to detect any positive effect of violence on formal electoral participation in the elections held during the civil war, which stands in contrast to previous studies that compared individual-level outcomes within affected districts, and find that individuals who directly experienced violence were voting more than less affected individuals (Bellows and Miguel [2009] for Sierra Leone; Blattman [2009] for Uganda). We put forward two possible explanations for our finding. First, while violence may stimulate interest in politics, it also creates a challenging environment to effectively express this interest by means of voting participation. For instance, the collapse of local

infrastructure, population displacement, and safety concerns may constrain individuals willing to vote, and thus account for the discrepancy between informal political participation and turnout. Second, on a more pessimistic note, conflict may lead to an increase in informal political participation at the local level, not because of a larger genuine interest in the macropolitical environment and institutional development but merely in response to pressing needs.

The latter explanation better aligns with our finding relative to the 2011 elections, the first held after the end of the violence. If the challenging environment was constraining electoral participation in 2006, we would expect to observe an increase in turnout in the LRA-ravaged districts by 2011. We detect, however, no positive impact on turnout in the 2011 elections. Instead of fostering active involvement in the political life of the country, we find that the exposure to violence in northern Uganda seems to have affected the confidence in the functioning of the country's democratic institutions. Even though this result may not be generalized to other cases, it certainly tones down the excitement sparked by the recent studies on the impact of violence on political participation.

Appendix A

TABLE A1
FIRST-STAGE RESULTS

	Event Days 2000–2005 × Year 2005 (1)	Event Days 2000–2008 × Year 2008 (2)	Event Days 1996–2006 × Year 2008 (3)	Event Days 2000–2012 × Year 2012 (4)	Event Days 1996–2012 × Year 2012 (5)
Distance to Sudan × year 2005	−.007*** (.002)				
Distance to Sudan × year 2008		−.008*** (.002)	−.007*** (.001)		
Distance to Sudan × year 2012				−.008*** (.002)	−.008*** (.001)
Year 2005	4.733*** (.347)				
Year 2008		4.992*** (.342)	5.141*** (.334)		
Year 2012				5.003*** (.359)	5.104*** (.354)
Other interacted controls	✓	✓	✓	✓	✓
Individual controls	✓	✓	✓	✓	✓
District fixed effects	✓	✓	✓	✓	✓
Observations	4,539	4,583	4,583	4,439	4,439
R ²	.941	.951	.956	.939	.945

TABLE A1 (Continued)

	Event Days 2000–2005 × Year 2005 (1)	Event Days 2000–2008 × Year 2008 (2)	Event Days 1996–2006 × Year 2008 (3)	Event Days 2000–2012 × Year 2012 (4)	Event Days 1996–2012 × Year 2012 (5)
<i>F</i> -test on excluded instruments	2,010.03***	2,322.84***	2,363.85***	2,678.56***	2,654.37***
Partial R^2 on excluded instruments	.4214	.4578	.4319	.4575	.4409
Anderson canonical correlations statistic	2,471.26***	2,781.19***	2,571.47***	2,684.3***	2,565.93***

Note. Standard errors clustered at the district level across urban and rural strata in parentheses. For the meaning of the individual controls, see table 3.

*** $p < .01$.

TABLE A2
TESTING PRECONFLICT TRENDS

	Voted
Event days 2000–2012 × year 2001	–.019 (.014)
Year 2001	.069 (.077)
Other interacted controls	✓
Individual controls	✓
District fixed effects	✓
Observations	65
R^2	.888

Note. Standard errors in parentheses. For the meaning of the individual controls, see table 3. Replacing event days 2000–2012 with event days 2000–2005 generates identical results.

TABLE A3
TESTING PRECONFLICT CORRELATION

	Attend Meeting in 2000 (1)	Discuss Politics in 2000 (2)	Voted in 1996 (3)
Event days 2001–5	–.013 (.037)	–.042** (.021)	.018 (.011)
Observations	2,157	2,156	2,154
R^2	.122	.104	.285

Note. Standard errors clustered at the district level across urban and rural strata in parentheses.

** $p < .05$.

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