

## **Civic Education and Democratic Backsliding in the Wake of Kenya's Post- 2007 Election Violence**

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## **Abstract**

This article examines two unexplored questions concerning the impact of civic education programs in emerging democracies: (1) whether such programs have longer-term effects and (2) whether civic education can be effective under conditions of democratic “backsliding.” We investigate these questions in the context of a large-scale civic education program in Kenya just before the disputed 2007 election that sparked a wave of ethnic clashes and brought the country to the brink of civil war. Analysis of a survey of 1800 “treatment” and 1800 “control” individuals shows that the program had significant long-term effects on variables related to civic competence and engagement, with less consistent effects on democratic values. However, participants who subsequently were affected by the violence were less likely to adopt negative beliefs about Kenya’s political system, less likely to support the use of ethnic or political violence, and more likely to forgive those responsible for the post-election violence.

Keywords: civic education, Kenya, political culture, democratic learning, program evaluation

It is a truism that democracy works better when citizens possess attitudes and values that are conducive to multiparty politics and where citizens are engaged in the political process. Belief in this truism among international donors has inspired a wide range of policies and programs designed to further mass democratic culture around the world. Civic education programs, in particular, have emerged as a core component of efforts by the United States and other Western countries to aid the consolidation of democratic practices in emerging democracies. While little data is available on the extent of such efforts, one recent study estimated that the main U.S. donor, the United States Agency for International Development (USAID), alone spent between 35 and 50 million dollars per year on civic education worldwide from 1990 to 2005 (Finkel and Smith 2011).

Can donor-sponsored civic education programs work in emerging democracies? Recent evaluations among both school-age children and adults in a variety of new democratic settings offer some evidence that such programs can be effective (Bratton et al. 1999; Finkel 2002, 2003; Finkel and Smith 2011; Torney-Purta et al. 2001). Yet important questions remain. In this article, we present the findings of an evaluation of a national civic education program implemented in Kenya between 2006 and 2007.<sup>1</sup> We seek to contribute to the literature in two specific ways. First, we ask whether civic education has longer-term effects. Most prior research has relied on surveys that were conducted shortly after the programs ended, making it impossible to determine whether the observed effects were transitory or more enduring. For reasons explained below, we were forced to delay our examination of Kenya's civic education program until nearly a year after it had ended. While this created a number of logistical challenges, it also provided an opportunity to examine the longer-term effects of the program. Second, this study seeks to understand whether civic education programs can work in countries experiencing democratic reversals. Nearly all existing studies of civic education programs have been conducted in countries that were on a positive trajectory, such as the Dominican Republic, Zambia, Poland, and South Africa in the mid-to-late 1990s, or Kenya at the time of its democratic breakthrough election of 2002. It may be that civic education has greater effects in countries that are on the road to consolidation, where citizens may be particularly receptive to civic education interventions. Less is known about the potential contribution of civic education in settings like Kenya during the elections of 2007, where the consolidation of democracy was challenged by intense ethnic political mobilization, widespread violence, and electoral fraud.

We explore these questions through an examination of the Second Kenya National Civic Education Programme (NCEP II-Uraia), a large-scale civic education program carried out in Kenya during the run-up to the 2007 elections. Our data come from a 2008 survey of 1,800 individuals who attended at least one NCEP II-Uraia civic education event and 1,800 similar nonattendees. Our methods, as will be discussed in detail below, control in a variety of ways for possible biases stemming from the self-selected nature of NCEP II-Uraia civic education exposure.

Three main findings emerge from this study. First, in line with previous research, we find that the program had the most consistent long-term effects on a set of variables related to "civic competence and engagement"—e.g., political knowledge, rights awareness, efficacy, and

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<sup>1</sup> A supplemental appendix for this article is available at <http://www.journals.cambridge.org/jop>. Data and replication files necessary to reproduce the results in the article are available at [http://www.stevenfinkel.info/Civic\\_Education\\_Evaluation\\_Research.html](http://www.stevenfinkel.info/Civic_Education_Evaluation_Research.html).

participation. It was, however, significantly less effective on influencing most core democratic values and orientations. Second, the program nevertheless had positive effects on some variables related to ethnic tolerance and on support for the peaceful resolution of ethnic and political conflict, a key concern in Kenya's multiethnic democracy and especially important in the aftermath of the widespread violence that followed the 2007 elections. Third, we find that the program played some role in mitigating the negative impacts of the post-election violence itself. The direct experience of post-election violence did much to undermine individuals' faith in Kenyan democracy and their role in it, but these negative effects were attenuated among those who had been exposed to NCEP II-Uraia civic education. To this extent, the program was successful in preventing even more disillusionment and democratic backsliding as a result of the events that occurred in Kenya after the program had formally concluded.

### **Civic Education in Emerging Democracies: Unanswered Questions**

Donor-sponsored civic education programs in emerging democracies are typically designed to promote a bundle of norms, values, and behaviors thought to be conducive to democratic politics. While the goals and methods vary across countries, civic education programs generally seek to provide citizens with knowledge about how the political process works, to encourage active participation, and to instill values such as tolerance and support for individual liberties. Civic education programs typically work through some mix of school-based initiatives geared toward students, community-based activities (workshops, meetings, village theater, etc.) aimed at adults, and in some case, media campaigns as well.

While few quantitative studies have been conducted, the consensus within the existing literature is that civic education can be effective, especially on variables related to knowledge, participation, and civic engagement. On participation, for example, Finkel (2003) found that programs implemented in three disparate settings—the Dominican Republic, Poland, and South Africa—increased citizen participation in local politics by a significant margin. Existing studies, however, have found that civic education programs generally have more limited effects on citizen orientations, such as political tolerance or institutional trust, though some modest effects have been documented (Bratton et al. 1999; Finkel 2003; Finkel and Smith 2011).

While the existing literature provides some evidence that civic education programs of various types can be effective, a number of questions remain. In this study we focus on two issues. First, we ask whether civic education has longer-term effects or whether its effects are more ephemeral. There is good reason to suspect that civic education might have minimal longer-term impact. Much of the existing literature on political culture suggests that attitudes and dispositions ought to change very slowly and mainly in response to large-scale trends in countries' economic, political, and social structures (e.g., Almond and Verba 1963; Inglehart and Wenzel 2005). If these accounts are correct, we should expect that while civic education might have positive short-term effects, such programs will be less likely to produce deeper effects that will be sustained over time. Moreover, nearly all existing studies of civic education look for effects relatively shortly after the programs have ended. Less is known about whether effects endure or whether citizens return to their prior predispositions and orientations over time.

A second question relates to the potential importance of contextual factors. While civic education programs have been implemented in many different types of settings, nearly all existing evaluations come from countries that were on a positive trajectory at the time of the research. Finkel and Smith (2011), for example, characterized Kenya in 2002 as a context where "there was real hope that true democratic change in the country was possible," so that the civic

education that took place at that time “had at least some chance of building successfully on recent democratic developments.” It may be hypothesized that citizens are most willing to adopt new attitudes and behaviors in settings where opportunities for political engagement are expanding and where the messages from civic education reinforce positive experiences with democratic change. On the other hand, the earliest studies of civic education found that greater effects among students were seen when civic education messages were “nonredundant” to the democratic messages that young adults may have heard in their family and friendship circles (Langton and Jennings 1968). To this extent, it may be the case that civic education is even *more* effective in less democratic contexts or in countries experiencing violence or other kinds of democratic reversals. Without evidence from civic education evaluations in more authoritarian, or “backsliding” contexts, we simply do not know which of these processes is more prevalent.

### **The Second Kenya National Civic Education Program (NCEP II-Uraia)**

We examine these issues through an evaluation of the second round of Kenya’s National Civic Education Program (NCEP II), which ran from April 2006 to September 2007. NCEP II, which was funded by a basket of European donors, sought to build on the lessons of a prior civic education program (NCEP I), which was conducted in Kenya from 2001 to 2002 (Finkel and Smith 2011). The program took the name “Uraia,” meaning “citizenship” in Kiswahili. The program was implemented by 43 Kenyan civil society and religious institutions organized into four larger consortia. These organizations conducted a wide variety of activities, including workshops, village theater performances, informal meetings in churches and mosques, cultural gatherings, and other public events. Records collected by the Program (which we later used for sampling purposes) show that approximately 79,000 unique events were held. The program also funded a media campaign that included television, radio, and newspaper. While it is not possible to determine the number of people reached by the program with certainty, we estimate that the program reached between 4.5 and 5.5 million individuals through its face-to-face components (roughly 21% to 26% of the adult population).<sup>2</sup>

The program was designed around five key themes: (1) nation building, (2) democracy, (3) good governance, (4) constitutionalism, and (5) human rights. The activities conducted under the NCEP II rubric sought to increase individuals’ level of awareness and knowledge of these five thematic areas, as well as to cultivate the skills, values, and dispositions that would facilitate effective citizen participation, that would serve to hold political elites accountable, and that would enable individuals to assert and defend their political and human rights against possible encroachment by the state or other members of Kenyan society.<sup>3</sup> NCEP II also addressed many Kenya-specific issues, such as the on-going constitutional review process, ethnic and religious tension, and three “cross-cutting issues”—HIV/AIDS, gender, and environmental concerns.

NCEP II was implemented in the context of a highly polarized political system. The country’s president, Mwai Kibaki, had come to power in 2002 in an election that was widely

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<sup>2</sup> This estimate is based on the total figure of 10 million individuals trained, as reported in program documents, discounted by the level of multiple exposures to program events reported by participants in our survey. The United Nations estimated that in 2006 there were approximately 39 million Kenyans and that 54% of these were adults (16+). Source: World Bank Development Indicators.

<sup>3</sup> These goals are explicitly laid out in the program document *Kenya’s National Civic Education Programme: The Uraia Manual*, available at <http://www.uraia.or.ke/english/>.

viewed as a major step toward the consolidation of democracy (Ndegwa 2003). The election marked the first transfer of power from one party to another in the country's history. Prior to the 2002 election, the country's dominant party, KANU, had maintained its rule through a mix of heavy-handed tactics, electoral fraud, and ethnic mobilization. Kibaki came to power at the head of a highly diverse, multiethnic coalition, promising to bring an end to the corruption, incompetence, and ethnic favoritism that had become endemic to Kenyan politics. Yet, within a short time of taking office, Kibaki's administration became embroiled in a series of major scandals that raised doubts about the president's commitment to reform. Moreover, Kibaki's reliance on a close circle of advisors drawn mainly from his home ethnic region led many to conclude that ethnic favoritism remained deeply entrenched in his administration (Barkan 2008). Tensions between different factions in the Kibaki government came to a head in a 2005 referendum on constitutional change. Many of Kibaki's 2002 coalition allies openly campaigned against the referendum, which was supported by the president. The campaigns devolved into a hostile contest in which ethnic communities lined up on opposing sides of the issue, and leaders employed divisive ethnic messages to rally their supporters. The referendum, which failed by a wide margin, left the country deeply polarized along partisan and ethnic lines. Given this context, one of the core goals of NCEP II-Uraia was to encourage ethnic tolerance during the 2007 election.

The 2007 election proved to be a highly contentious race that brought long-standing communal grievances over economic and political inequalities to the fore. As in the 2005 referendum, the 2007 election was filled with divisive ethnic appeals that played on and exacerbated resentments and hostilities between ethnic communities (Horowitz 2011). Disputes over the outcome of the election, which was widely believed to have been stolen by the incumbent party, sparked a wave of ethnic violence in which more than 1,000 people were killed and at least 300,000 more were displaced from their homes (Anderson and Lochery 2008). The post-election violence, which engulfed large sections of Kenya's Rift Valley and the capital city Nairobi, lasted over two months until a power-sharing agreement was reached in late February 2008 by the main political parties. Our survey, as well as national survey data collected by the Afrobarometer in late-2008, showed that at least a quarter of the Kenyan population was directly affected in one way or another by the conflict.<sup>4</sup> The post-election violence revealed both the tenuousness of Kenya's progress toward democratic consolidation and the depth of ethnic and partisan antipathies within Kenyan society (Barkan 2008; Chege 2008).

The post-election violence raised important questions regarding the effects of the pre-election civic education program. The first concern was that the violence might have erased whatever positive gains were produced by NCEP II-Uraia. As noted, the program sought to promote intercommunal tolerance and to encourage the use of democratic means rather than violence to resolve disputes. We speculated, therefore, that the divisive effects of the conflict could have undermined precisely the attitudes and behaviors targeted by the civic education initiative. Second, it may have been the case that those who had participated in NCEP might have reacted differently to the violence itself than those who did not. Although the program did not anticipate the conflict, one measure of the program's success would be that participants who

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<sup>4</sup> The Afrobarometer Round 4 survey (n=1,104), which was conducted in October—November 2008, found that approximately 24% of respondents had been directly affected through loss of personal property, destruction of a home or business, personal injury, or the death of a family member.

were directly affected by the violence would be less likely to “backslide” away from supportive democratic attitudes and values than those affected by the violence in the control group. We speculated, in other words, that the program might have “inoculated” participants in important ways against the negative or antidemocratic effects of Kenya’s post-election political violence.

## **Data, Measurement, and Statistical Methods**

We explore the impact of the NCEP II-Uraia program through an analysis of a survey of 3,600 individuals conducted across the country between December 2008 and January 2009. The survey was conducted as part of an overall evaluation of the NCEP II-Uraia program that was commissioned by the United States Agency for International Development in September 2008.<sup>5</sup> 1,800 individuals who had been exposed to NCEP II-Uraia face-to-face activities were interviewed by survey teams from the Nairobi-based firm Research International as the “treatment group” sample, along with 1,800 “control group” individuals who were similar to the treatment group but who had no NCEP II-Uraia face-to-face exposure. Treatment group individuals were selected based on a two-stage, random-sampling process (that are described more fully in the supplemental appendix available at <http://journals.cambridge.org/jop>): a random sample of 360 NCEP II-Uraia activities was first selected based on so-called “Form D” cover sheets that facilitators were required to complete after each activity and file with the NCEP II-Uraia central offices; five specific treatment-group respondents were then interviewed based on a random sampling of households in the areas where the selected activities took place. Finally, a detailed procedure was implemented to interview five control-group individuals from those same areas, and who were matched to the treatment group on a series of demographic characteristics, including education, age, gender, and membership in civil society organizations. These procedures, along with additional statistical matching of the treatment and control groups that we introduce at the analysis phase, give us as much confidence as possible—given the inherent limitations of the data that we will discuss below—that the observed differences between the groups can be attributed to NCEP II-Uraia exposure.

Several methodological issues should be noted at the outset. First, in contrast to the previous NCEP I assessment (Finkel and Smith 2011), the current study does not contain a “pretest” component. That is, we do not have baseline data on individuals’ democratic orientations before the NCEP II-Uraia activities took place. This means that, with only cross-sectional data at our disposal, we face even greater difficulties in ruling out the possibility that individuals in the treatment group were already different on democratic outcomes, or different on factors relevant to changes in democratic outcomes, before their exposure to NCEP II-Uraia civic education. Second, the events following the 2007 election prevented the study from being implemented until over a year after the end of the program itself. As noted above, the study was only formally commissioned in September 2008, some nine months after the election and the violence that took place in the election’s aftermath. This means that the study represents an assessment only of the *longer-term impact* of the NCEP II-Uraia program on individuals, as we were unable to observe individuals at or around the time that the NCEP II-Uraia interventions took place. Third, the study by necessity relies on individuals’ recollection of activities that took place many months before they were interviewed; moreover, we had neither names nor contact

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<sup>5</sup> The evaluation was implemented through the Washington D.C.-based firm Management Systems International.

information for specific individuals who had attended. This necessitated the institution of relatively elaborate procedures during the sampling stage to verify the status (treatment versus control) of individuals who were contacted by Research International. We believe the procedures were implemented successfully by the survey teams, but nevertheless there is likely to have been some error in the respondent's recall of participation in NCEP II-Uraia activities.

### **Sampling and Respondent Selection Procedures**

A detailed description of the sampling and respondent selection procedures can be found in the supplemental appendix. We summarize the procedures here.

We sampled at random 90 activities conducted in 2007 by each of the four NCEP-II consortia for inclusion in the study. These activities comprised targeted workshops, poetry-drama events, and informal meetings, with the numbers of each drawn in proportion to the total activities of each type undertaken by the particular Consortium. Research International then sent survey teams to the exact venue (such as a school or marketplace) where each of the 360 sampled activities took place and searched for respondents following random route procedures.

Interview teams started at the exact venue where each of the 360 sampled NCEP II-Uraia activities took place and contacted households following a random walk procedure. A detailed set of screening questions was asked of all potential respondents, and if a given respondent reported attending at least one civic education activity before the 2007 election, the individual was selected for inclusion in the "treatment group."<sup>6</sup> Once a treatment-group respondent interview had been successfully completed, the interviewer recorded that person's demographic information in terms of gender, age, education, and the number of secondary group memberships to which the person belonged. Interviewers were then instructed to find control-group individuals, using random-route procedures starting some distance from the treatment-group respondent's location, who had similar demographic characteristics as the given treatment-group individual but who had *not* attended Uraia civic education activities before the 2007 election. This set of procedures was repeated until five treatment-group respondents and five matching control-group respondents were interviewed from each of the 360 sampling points, resulting in 1,800 treatment-group respondents and 1,800 matched control-group respondents.

### **Survey Instrument**

A detailed list of all variables used, their exact question wordings and response categories, and, where appropriate, scale reliability coefficients, can be found in the supplemental appendix. The survey instrument included questions relating to the general themes of the Uraia program: good governance, human rights, democracy, constitutionalism, and nation building. For some of these dimensions, the questions relate to individuals' *awareness, involvement, or perceived competence* regarding an issue or theme, and we categorize these items under the general rubric of *Civic Competence and Engagement*. For other dimensions, the

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<sup>6</sup> The exact English wording to define "civic education activities" for the respondent in the screening questions was: "As you may know, there are some programs going on in Kenya that try to engage people about democracy and human rights and about how to solve community problems. Sometimes they are sponsored by community organizations or religious organizations, and they can involve workshops, public barazas, theatre or drama presentations, town meetings, or other kinds of public discussions in churches or mosques about citizens' rights and responsibilities. We call these kinds of activities "civic education."

questions related to individuals' preferences or values about politics, the rights of citizens, leaders, institutions, or the overall political system, and we categorize these items under the general rubric of *Democratic Values, Rights, and Responsibilities*. Finally, the survey included a range of questions on *Ethnic Social and Political relations*, taking into account the polarized conditions following the intercommunal violence that occurred after Kenya's 2007 election.<sup>7</sup>

### **Statistical Procedures**

The main obstacle in estimating the casual effect of exposure to the NCEP II-Uraia program, as is the case in nearly all observational assessments of development programs, is the potential for selection bias. Because Kenyan citizens freely chose whether to participate in the program (as opposed to being randomly assigned to treatment conditions in experimental research), it is likely that many differences—both observable and unobservable—exist between participants and nonparticipants that may also be associated with differences on political knowledge, participation, and other dependent variables. To address these concerns, we employ a fourfold approach. First, as described in the previous section, we developed a matching protocol for the sampling stage in which each respondent who reported having participated in NCEP II-Uraia was carefully matched on place of residence, age, gender, education, and group memberships with a respondent who had not participated. This ensured that the control and treatment groups were well balanced on several important demographic variables.<sup>8</sup>

Second, we supplemented this sampling-based matching with additional balancing of the treatment and control groups before the estimation stage, using the CEM (Coarsened Exact Matching) methods developed by Ho et al. (2007) and Iacus, King, and Porro (2011).<sup>9</sup> We matched on a range of factors (age, income, church attendance, group membership, and whether respondents had ever served as a leader of any group or association to which they belonged) that might be related to both NCEP II-Uraia participation and democratic orientations.<sup>10</sup> The CEM matching procedure produces different “bins” or strata consisting of the combinations of characteristics in the specified variables (e.g., leaders with high church attendance, medium income, young age) and then balances the treatment and control groups as completely as possible by eliminating treatment group cases that have no corresponding control-group member in their “bins,” and eliminating control-group cases that have no corresponding treatment group member

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<sup>7</sup> The questionnaire was translated into Kiswahili by members of the Research International staff and back-translated by a professional translator in Nairobi: 67% of the interviews were conducted in Kiswahili, 26% were conducted in English, and 7% were carried out in one of Kenya's other languages.

<sup>8</sup> Because treatment and control individuals were sampled from the same communities, the possibility of spillover effects is a concern. While we cannot eliminate this issue in our statistical tests, we note that spillover would likely bias our tests toward the null hypothesis, making it more difficult to find statistically significant program effects.

<sup>9</sup> As opposed to other matching techniques, CEM balances treatment and control groups *before* (parametric) estimation and, therefore, bounds the degree to which results from subsequent estimation are dependent on modeling assumptions and specifications (Ho et al. 2007).

<sup>10</sup> We further eliminated any respondent who reported attending civic education activities *after* the 2007 elections.

in their “bins” as well. In our case, this procedure eliminated 611 individuals in the original treatment group, and 197 members of the original control group.

Table SA-3 in the supplemental appendix reports the degree of imbalance between the treatment and control groups on a number of covariates before and after the CEM matching. The table shows that, despite the rough matching procedures used at the sampling stage, statistically significant differences remained on some variables, including age and income. And relatively large differences between the groups exist in the pre-CEM columns for group leadership, membership in parties or civic groups, and in the motivational and media exposure factors. Once the CEM balancing procedures were implemented, the groups were nearly perfectly balanced (i.e., had identical mean values) on all the demographic covariates, including many (such as marital and employment status) that were not explicitly entered into CEM. These procedures do much to ensure that observed differences between the treatment and control groups are not the result of pretreatment differences between the groups on demographic factors measured in the study.

Nevertheless, it can be seen that even after implementing the CEM matching procedure, significant differences still exist between the CEM-balanced treatment and control groups on motivational factors such as political interest and discussion, on general media exposure and exposure to specific NCEP II-Uraia media presentations, and on membership in political parties and civic organizations. This presents a difficult choice for assessing the causal impacts of face-to-face NCEP II-Uraia activities. To the extent that civic education exposure led individuals, for example, to become more interested in politics, or to attend to media (or NCEP II-Uraia media) messages more frequently, or to join a civic group, then controlling for, or balancing, these “posttreatment” variables will result in an attenuation of the estimated causal effects of interest. On the other hand, it is likely that at least part of the differences between treatment and control individuals on these variables reflects preexisting differences in motivation and chronic exposure to political information between the two groups. Variables such as interest, discussion and media exposure may further serve as partial proxies for unobservable factors such as an intrinsic “taste” for politics or more democratic personality characteristics that may differ between the treatment and control groups and thus confound the causal inference process as well. On balance (no pun intended), we decided as a third strategy to include these motivational and media exposure variables, as well as reported membership in civic organizations and political parties, as additional controls in CEM-weighted regression models predicting each democratic orientation along with treatment/control-group status. This procedure may result in our underestimating the impact of NCEP II-Uraia exposure. Nevertheless, we include these variables in a conservative attempt to capture some of the political and motivational differences between the treatment and control groups that may produce spurious causal estimates.<sup>11</sup>

Our fourth strategy for dealing with the potential problem of selection bias is to conduct a sensitivity analysis for “hidden bias” in the estimates due to the potential effects of unobservable factors that may differentiate the treatment and control groups. This method, developed by Rosenbaum (2002), calculates bounds on the magnitude of an effect that an unobserved covariate would have to have on the selection process in order to overturn the inferences about treatment effects that we report. This effect can then be compared to the magnitude of effects that the *observed* variables actually have on selection, in order to assess the plausibility that treatment

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<sup>11</sup> A further robustness check incorporates these variables into the CEM matching procedure itself, as we discuss below and in supplemental appendix Table SA-4.

effects are due to unobservable factors. Recent political science applications of the method can be found in Ladd and Lenz (2009) and Stein and Vonnahme (2008).

To estimate treatment effects, we ran separate OLS regressions for each dependent variable described in the previous section. All models use the post-CEM balance data and included whether the respondent had attended an NCEP II-Uraia face-to-face activity, along with the following controls: age group, gender, education level, income level, frequency of church attendance, a measure of group membership, whether the respondent belongs to a political party or a civic organization, whether respondent has ever been a leader of any group to which he/she belongs, media consumption, political interest, a measure of political participation, and exposure to the NCEP II-Uraia media campaign. We also include a dummy variable or fixed effect for each of the 360 sampling points in the study and cluster standard errors by the specific matched treatment-control respondent pairs within each sampling point.

## Basic Results

Table 1 presents our core findings from regression models predicting each of the dependent variables described above. To facilitate comparison of the magnitude of effects across these variables, we also show “Y-standardized” coefficients, which were calculated by dividing the regression coefficient for each dependent variable by the standard deviation of the variable.

[Table 1 here]

The results show that NCEP II-Uraia had the most consistent effects on Civic Competence and Engagement variables and more limited effects on democratic values and orientations or identity and ethnic group relations. Program participants scored higher both on objective measures of political knowledge (*general knowledge*) and perceived knowledge of Kenya’s political system (*perceived knowledge of the constitution*) and their basic rights as citizens (*informed about protecting rights*). Participants also expressed greater confidence in their ability to influence political outcomes (*internal efficacy*). These effects were moderate in substantive terms, with standardized effects in some cases in the .15-.20 range. Given the long time period that had elapsed since individuals experienced the NCEP II-Uraia activities, this is relatively impressive evidence regarding the durability of civic education’s impact, at least on this cluster of democratic orientations. The program had mixed effects on participation, leading to higher levels of participation at the local level but not at the national level. We suspect that the difference can be attributed to the program’s greater focus on local-level politics and perhaps also the nature of the Kenyan political system, which offers few avenues for citizens to engage national-level institutions.

On the cluster of variables related to Democratic Values, Rights, and Responsibilities, we observe only scattered effects. The program had no effect on basic orientations toward democracy or support for the rule of law (*democracy is best, support for rule of law*). Effects were seen, however, on two variables related to more quotidian aspects of electoral politics in the Kenyan context, *rejection of vote buying* and *opposition to the use of violence*. We suspect that in emerging democracies like Kenya, where citizens often have good reason to be skeptical about national-level political institutions, civic education programs face greater difficulty in influencing citizen orientations toward broad concepts like democracy or the rule of law, relative to the more immediate issues that have greater bearing on the citizens’ interaction with the political process. On human rights, we find that the program had a small positive effect on

support for a bundle of basic human and political rights (*rights consciousness*), but was not effective in regard to women’s rights (*support for women’s rights*).

Similarly, we find mixed results on variables related to Identity and Ethnic Group Relations. The program led to significant increases in the amount of social tolerance that individuals are willing to extend to their “most disliked group” and led to significant increases in the perception that violence is not an appropriate means for ethnic groups to defend themselves if they feel threatened. The program was less effective regarding questions of national belonging or ethnic political tolerance. Given the centrality of these outcomes to the program’s goals, we conclude that the modest observed effects are indicative of a mixed record—while some positive changes were observed, no movement was seen on other key variables. It should be noted, moreover, that none of the observed effects for democratic values or ethnic orientations were large in terms of their substantive magnitude. The standardized effects for these variables were all below .10, well below the values registered for the variables in the civic competence and engagement cluster. All of this suggests that NCEP-II Uraia was a relatively effective long-term agent of political empowerment, but a much less effective long-term agent for value change.

We report a series of additional robustness and sensitivity checks on these findings in the supplemental appendix. Table SA-4 shows that the results are robust to several alternative procedures for matching treatment and control-group respondents: one that adds possible posttreatment outcomes such as political interest, media exposure, and political discussion into the set of matching variables in the CEM procedure, and another that uses propensity score matching techniques, with the entire set of control variables (both pretreatment and possible posttreatment covariates) as predictors of the likelihood of NCEP II-Uraia civic education exposure. In both tests the effects of all five of the competence and engagement variables remain significant and of comparable (and in some cases larger) magnitude as is reported in Table 1. Two of the “democratic values and identity” variables—rejection of vote buying and opposition to violence as an ethnic defense—also retain their strength and significance, while only the smaller effects observed in Table 1 for rights consciousness, opposition to political violence and ethnic social tolerance are more sensitive to the exact matching method and specification of the selection model.

Finally, we show in Table SA-5 that the results are robust to reasonable amounts of possible “hidden bias” from unobserved variables that may influence both selection into treatment and the outcomes reported in Table 1. Following the Rosenbaum bounds method (DiPrete and Gangl 2004; Rosenbaum 2002), we estimate that, in order for an unobserved factor to alter the inferences about treatment effects we have made, it would have to be of sufficient magnitude to have increased the odds of receiving treatment for one person in a pair of otherwise (propensity-score) matched individuals by factors ranging from a high of 2.40 (in the case of *informed about protecting rights*) to a low of 1.13 (for *political efficacy*). Six of the eight variables register factors of 1.30 or above, indicating that the hidden bias would need to be associated with at least a 30% increase in the odds of treatment for one member of an otherwise equally (propensity-score) matched pair of individuals. Hidden bias of these magnitudes cannot be ruled out entirely, though it seems implausible here, given the size of selection effects we find for observed resource and motivational factors such as income, interest, media attentiveness, and political discussion. For example, belonging to the highest versus lowest category of interest, income, media attentiveness, and political discussion increased the odds of receiving treatment by factors of 1.16, 1.17, 1.20, and 1.22, respectively (see Table SA-6). This means that an unobserved covariate would need to have influenced selection into treatment *over and above*

*these observed factors*, and with equal or greater magnitudes in nearly all cases to call our results into question. Even the most powerful variables in our selection model, belonging to a civic group and being a “leader” of a secondary association, increase the odds of receiving treatment by factors of 1.40 and 1.57, respectively; an unobserved covariate would need to be uncorrelated with, and have nearly equal or more powerful effects than these variables in influencing treatment in half of our eight cases to overturn our findings.<sup>12</sup>

## **The Impact of Post-Election Violence**

In this section we explore the ways that the violence and dislocations that occurred in Kenya following the disputed 2007 elections may have influenced the effects of NCEP II-Uraia civic education. Because the fieldwork for the evaluation took place after the post-election violence had ended, we were able to inquire about respondents’ personal experiences with the post-election violence and disruptions and to determine whether and how such experiences may have mitigated or otherwise influenced the impacts of pre-election civic education. Such an exploration, as discussed above, provides perhaps the first window into the effects of civic education in an inhospitable political context characterized by violence, acute ethnic conflict, and democratic “backsliding.”

One possibility is that the intercommunal violence that followed the 2007 election might have attenuated, or even erased, the positive effects of NCEP II-Uraia civic education. If this were the case, we would observe weaker effects in our sample than would have been the case had we conducted the evaluation before the violence occurred. Of course, we cannot directly test for this, but we may provide some indirect evidence for this claim by demonstrating smaller effects of NCEP II-Uraia impact among individuals who were personally affected by the violence than among those who were not. Another, perhaps more positive possibility, however, is that exposure to NCEP II-Uraia civic education served in some ways to “inoculate” individuals against some of the more deleterious effects of the violence itself. That is, individuals who experienced violence *and* who were exposed to civic education may have been less likely to adopt negative attitudes towards ethnic outgroups, lose faith in democratic institutions and processes, or withdraw politically than individuals who experienced violence in the control group.

To test for these possibilities, we asked individuals whether “you or your family was affected by the violence that occurred after the 2007 election.” Of CEM-balanced treatment group, 27.2% and 26.6% of the (CEM-balanced) control group reported that they had been affected. We entered this variable (*Violence*) into a series of regression models predicting core democratic and other ethnic-related orientations, along with the interaction between *Violence* and NCEP II-Uraia treatment exposure. The signs and magnitudes of the various terms in the model allow us to assess how civic education and the experience of post-election violence may have influenced attitudes about democracy. In addition to the core democratic variables from the

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<sup>12</sup> It should also be noted that the sensitivity analysis provides a relatively conservative test of possible hidden bias. The unobserved covariates are assumed not only to influence the odds of receiving treatment for one person in an otherwise propensity-score matched pair of individuals with the magnitude described above, but also to be related to the dependent variables so strongly as to predict *perfectly* which of the two individuals ranks higher than the other on the outcomes (DiPrete and Gangl 2004). The method will thus find bias even if the unobservables’ effects on the dependent variable are weaker than assumed in the test.

previous section's analyses, we include two additional ethnic or violence-related measures (neither of which were significant in simple treatment effects models): whether the individual believes that "it is sometimes necessary to use violence to avenge past wrongs committed against your family or ethnic community"; and the extent to which the individual has "forgiven those responsible for the post-election ethnic violence."

[Table 2 here]

Table 2 shows the results of the analysis. We present in the first column the estimated effect for treatment exposure, which, in the context of these models, indicates the impact of NCEP II-Uraia exposure among individuals who did *not* experience post-election violence. The second column shows the estimated effect of *Violence*, which indicates the impact of post-election violence experiences among individuals who were *not* exposed to NCEP II-Uraia activities. The third column presents the estimated interaction effect: the additional impact of NCEP II-Uraia exposure for those who experienced post-election violence, or, alternatively, the additional impact of the experience of post-election violence among individuals in the treatment group.

We see little support for the hypothesis that the violence reduced the impact of NCEP II-Uraia activities among those individuals who participated. For the Civic Competence and Engagement variables, we see that the treatment effect remains strong and significant, with very little influence of political violence on the process. Individuals who experienced violence were not significantly different on these dimensions than those who did not, and the interaction terms, aside from that seen in the internal efficacy model, are very close to zero.

For Democratic Values and Ethnic Orientations, it is also the case that the experience of post-election violence did not reduce the impact of NCEP II-Uraia exposure. In no case is the interaction effect between *Violence* and *Treatment* negative, indicating that the effect of treatment is never *less* for those who experienced violence than for those who did not. In fact, on several key variables, there are *positive* interactions between *Violence* and *Treatment*, such that the effects of NCEP II-Uraia exposure are stronger among treatment-group respondents who were directly affected by the violence compared with treatment-group respondents who were not.

The impact of NCEP II-Uraia exposure was, for example, nearly zero on attitudes towards the rule of law, opposition to political violence, and forgiveness of those who perpetrated violence if treated individuals did not personally experience post-election violence, while the effects are in the .13 to .15 range (and statistically significant) among treated individuals who experienced the upheavals directly.<sup>13</sup> For ethnic group social tolerance, the effect of NCEP II-exposure is more than double in size for directly-affected treatment-group respondents (.18 (.08+.10) compared to .08). This is an intriguing pattern, suggesting that NCEP II-Uraia activities had longer-lasting impact on many ethnic and violence-related attitudes among those individuals who personally experienced the upheavals following the 2007 election.

An alternative way to interpret this pattern is to note that the experience of violence among individuals in the control group had *negative* effects on nearly all of the ethnic and

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<sup>13</sup> These effects are the sum of the coefficients in the "treatment" and "interaction" columns of the table; the treatment column provides the treatment effect for individuals not experiencing violence, with the interaction column providing the additional increment for treated individuals who did experience post-election violence.

violence-related orientations, with several of these effects reaching statistical significance. Among the control group, the personal experience of post-election violence is significantly associated with *less* opposition to political violence, *less* opposition to the use of violence for ethnic defense, and *less* likelihood of having forgiven the perpetrators of the 2007–08 violence and ethnic conflict. Negative (though insignificant) effects for violence among the control are also observed for support for the rule of law and opposition to violence in order to “avenge past wrongs.” In all but one of these cases, the effects of personal experience with violence are essentially reduced to zero (and statistical insignificance) among individuals in the treatment group.<sup>14</sup> This lends relatively consistent support to the “inoculation” hypothesis put forward above, as exposure to NCEP II-Uraia civic education activities in effect blunted the deleterious negative impacts that the individual’s personal experience with post-election violence had on ethnic-related political and democratic attitudes.

This interpretation is further strengthened when we examine responses to the final set of questions we asked concerning the traumatic events following the 2007 election. We asked respondents to “Please think about the time right before the December 2007 elections, that is, before all the violence and dislocations that occurred in Kenya,” and then asked if they agreed or disagreed with the following statements:

- At that time I thought that democracy was a better system of government than I do now.
- At that time I thought I could influence the political process more than I do now.
- At that time I was more willing to consider the views of people from other ethnic groups than I am now.
- At that time I was more optimistic about building a true democracy in Kenya than I am now.

Positive responses on these items mean that the impact of the post-election upheavals caused individuals to become less optimistic about Kenyan democracy, their roles in it, and their consideration of other ethnic groups’ points of view. The overall responses to these questions are revealing, in that between 61% and 70% of all respondents “agree” or “strongly agree” with each of the statements.

### [Table 3 here]

We show in Table 3 the effect of the direct experience of post-election violence on each of these orientations, among individuals who were treated in NCEP II-Uraia civic education activities and individuals who were not. It can be seen that on three of these dimensions, Perceived Influence, Consider Other Ethnic Views, and Optimism about Kenyan Democracy, the effect of personal experience with violence had strongly positive effects on these variables *only among the control group*. That is, people in the control group who directly experienced post-election violence became more pessimistic about their Kenyan democracy and their role in it, and less willing to consider the views of other ethnic groups, while *no such negative impact occurred among the treatment group*. Thus we conclude that the NCEP II-Uraia program had some role in mitigating the negative impacts of the traumatic events of the post-election period and in mitigating some of the democratic “backsliding” that occurred in the election’s aftermath.

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<sup>14</sup> We show a full table with the effects of personal experience of violence for the control group and the treatment group separately, along with conditional standard errors, as Table SA-7 in the supplemental appendix.

## Conclusions

We explored two previously unexamined questions related to civic education programs: whether such programs have long-term effects and whether they can be effective in the context of democratic backsliding. Our results provide evidence that large-scale civic education programs *can* produce relatively long-term effects, adding to a growing number of evaluations from other settings that report similar findings. As in previous research, however, the strongest evidence of long-term impact was found on variables related to civic competence and engagement, with only sporadic and relatively small effects found on most variables related to democratic values. This supports an emerging pattern in civic education evaluation research, that donor-sponsored education programs can be relatively effective agents of political *empowerment*, but are typically much less effective agents of value change.

One important exception to this generalization is perhaps the most surprising finding that emerges from the analysis. We found that NCEP II-Uraia exposure had some “inoculation effects” related to the post-election violence that erupted in Kenya shortly after the program ended. Specifically, we found that participants in the program who subsequently were affected by the post-election violence were less likely (relative to nonparticipants who were affected by the violence) to adopt negative views on a range of important variables related to ethnic relations, tolerance, and conflict resolution (see also Paluck and Green 2009). Participants were less likely to express support for the use of political and ethnic violence, more likely to have forgiven those responsible for the post-election violence, and more likely to develop generalized support for the rule of law. These intriguing results imply that civic education has the potential to reduce at least some of the negative effects of democratic backsliding and that the negative impact of the post-2007 election aftermath in Kenya would have been greater in the absence of the NCEP II-Uraia program.

The findings have additional implications for the future implementation and evaluation of donor-sponsored civic education programs. First, the limits of civic education in influencing core democratic values need to be taken more seriously in the design and implementation phases of future programs. The results here confirm that mere exposure to civic education is typically not enough to produce substantial gains in tolerance, support for the rule of law, or other important democratic values in either the long or the short term. But this is not to say that influencing these orientations is impossible. Rather, we support the findings from previous research (e.g., Campbell 2008; Finkel 2003) that changing core democratic values requires frequent, focused training with active, participatory teaching methodologies and with high-quality instructors. These are issues that need to be built into program design, yet all too often are not. NCEP II-Uraia, for example, adopted a highly diffuse strategy, seeking to affect a very wide range of attitudes, beliefs, and behaviors rather than a more limited but focused strategy. Moreover, the vast majority of NCEP II-Uraia activities were relatively short in duration, with some being only a few hours or less, and this was simply not sufficient time to impart the kind of information needed to bring about meaningful value change. A more intensive approach focused on a narrower range of outcomes would have been substantially more effective, and we urge future programs to adopt these design features to maximize individual-level impact.

Second, our findings suggest a complex relationship between political context and civic education impact that future research needs to explore more fully. The results here indicate that, perhaps contrary to expectations, civic education can be a useful approach in countries experiencing democratic reversals, and not simply in countries that are making steady progress toward the consolidation of democracy. Indeed, our results suggest that civic education actually

was able to counteract some of the negative effects of violence, instability, and institutional failure in the Kenyan context. But there are likely to be limits to these processes. Despite the “backsliding” that Kenya experienced around the time of the 2007 election and thereafter, the country was still in far better democratic shape than it had been only a decade or so before and was in far better shape than many autocracies and nondemocracies around the world as well. The impact of civic education in more chronically inhospitable contexts is still very much an open question. As more evaluations of civic education (and other donor-sponsored democracy assistance) programs are conducted in more varied political and social contexts, the interactions between the political environment and the microlevel effectiveness of these democratic interventions will come into greater focus.

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**Table 1. Effects of NCEP II on Knowledge, Participation, Values, and Orientations**

	<b>Coefficient</b>	<b>Standardized Coefficient</b>	<b>Adj-R<sup>2</sup></b>
<b>I. Civic Competence and Engagement</b>			
Political Knowledge	.19** (.05)	.14**	.45
Perceived Knowledge of Constitution	.13** (.02)	.20**	.31
Informed about Protecting Rights	.10** (.02)	.15**	.28
Internal Efficacy	.12** (.04)	.11**	.31
Local Political Participation	.10** (.04)	.07**	.42
National Political Participation	.03 (.02)	.04	.36
<b>II. Democratic Values, Rights, and Responsibilities</b>			
Democracy is Best	.02 (.02)	.04	.20
Support for Rule of Law	.04 (.04)	.04	.27
Rejection of Vote Buying	.05** (.02)	.08**	.34
Opposition to the Use of Violence	.05* (.03)	.06*	.34
Rights Consciousness	.02** (.01)	.07**	.38
Support for Women's Rights	.04 (.03)	.05	.36
<b>III. Identity and Ethnic Group Relations</b>			
National versus Ethnic Identity	.03 (.03)	.04	.26
Most-Disliked Ethnic Group Political Tolerance	-.01 (.04)	-.01	.39
Most-Disliked Ethnic Group Social Tolerance	.11** (.04)	.09**	.37
Opposition to Violence as Ethnic Defense	.08** (.03)	.09**	.33

Results from CEM-weighted regressions with robust standard errors in parentheses.

*N* ranges from 2670 to 2783.

\*\* $p < .05$  (two-tailed); \*  $p < .10$  (two-tailed)

**Table 2. The Effects of Post-Election Violence and NCEP II-Uraia Exposure on Civic Competence, Democratic Values, and Ethnic Orientations**

	<b>Treatment</b>	<b>Violence</b>	<b>Interaction</b>	<b>Adj-R<sup>2</sup></b>
<b>Civic Competence and Engagement</b>				
Political Knowledge	.18** (.05)	.01 (.09)	.01 (.10)	.45
Internal Efficacy	.10** (.05)	-.09 (.07)	.10 (.09)	.31
Local Political Participation	.11** (.05)	.11 (.08)	-.02 (.11)	.42
Informed about How to Protect Rights	.09** (.03)	-.05 (.04)	.00 (.06)	.28
<b>Democratic Values and Ethnic Orientations</b>				
Democracy is Best	.00 (.02)	-.04 (.03)	.04 (.04)	.20
Support for Rule of Law	.00 (.05)	-.12 (.08)	.14 (.10)	.27
Opposition to the Use of Violence	.02 (.04)	-.12* (.06)	.13* (.08)	.34
Opposition to Violence as Ethnic Defense	.08** (.04)	-.15** (.06)	.01 (.08)	.33
Opposition to Violence to Avenge Past Wrongs	.00 (.04)	-.08 (.06)	.07 (.07)	.33
Most-Disliked Ethnic Group Political Tolerance	-.02 (.04)	.10 (.08)	.02 (.10)	.39
Most-Disliked Ethnic Group Social Tolerance	.08* (.05)	.01 (.07)	.10 (.10)	.37
Forgive those who Perpetrated Violence?	.01 (.03)	-.10* (.05)	.07 (.06)	.31

Results from CEM-weighted regressions with robust standard errors in parentheses.

N ranges from 2676 to 2778.

\*\*p<.05 (two-tailed); \*p<.10 (two-tailed)

**Table 3. The Effects of Post-Election Violence on Disillusionment with Kenyan Democracy among NCEP II-Uraia Participants and Control Group**

	Estimated Effect of Experience with Violence among the Control Group		Estimated Effect of Experience with Violence among the Treatment Group		Adj-R <sup>2</sup>
	Coefficient	Standardized Coefficient	Coefficient	Standardized Coefficient	
I thought Democracy was a Better Form of Government	.07 (.08)	.06	.14* (.08)	.12*	.39
I thought I could Influence Politics More Than Than Now	.24** (.09)	.19**	.09 (.09)	.07	.29
I was More Willing to Consider the Views of Other Ethnic Groups	.16* (.08)	.12**	.02 (.09)	.01	.34
I was More Optimistic about Kenyan Democracy	.16** (.07)	.12**	.06 (.08)	.04	.37

Results from CEM regressions with robust standard errors in parentheses.

*N* is 2777 for the first reported regression and 2778 for the rest.

\*\**p*<.05 (two-tailed); \**p*<.10 (two-tailed)