

## **Supplemental Appendix for Finkel, Horowitz, and Rojo-Mendoza. “Civic Education and Democratic “Backsliding” in the Wake of Kenya’s Post-2007 Election Violence,” Journal of Politics (Forthcoming 2012).**

This document provides: further information on the study’s sampling and respondent selection procedures (Section I); estimates of the degree of imbalance between the treatment and control groups on a number of covariates before and after the CEM matching procedure (Section II); results of additional robustness checks for the findings reported in Table 1 (Section III); results of the Rosenbaum bounds sensitivity analysis for assessing possible “hidden bias” due to unobserved confounders (Section IV); and an alternative presentation of the results of the exposure to civic education/experience of post-election violence interaction model reported in Table 2 (Section V).

### **I. Sampling and Respondent Selection Procedures**

We began the sampling by randomly selecting 90 activities conducted by each of the four NCEP-II consortia for inclusion in the study. We then conducted a detailed coding of a random sample of approximately 2000 Form D activity sheets for each Consortia.<sup>1</sup> We decided to limit the sampled activities to those conducted during 2007; activities from 2006 made up less than 20% of the total and were judged to have taken place so long before the data collection (December 2008) that accurate respondent recall would be very difficult. This coding contained information on where a given activity was conducted, what kind of activity it was, which civil society organization undertook the activity, how many people attended, and other aspects related to the nature of the particular Uraia civic education event. We used this data base to determine the mix of civic education activities, e.g., workshops, poetry/drama, or informal gatherings, that were undertaken by each Consortia, and we then allocated a proportional number of each Consortia’s 90 total activities to each kind of civic education event. So, for example, if 20% of CRECO (*Constitution and Reform Education Consortium*) activities were workshops, we allocated 20% of the 90 total CRECO activities (or 18 activities) for inclusion in the study to be workshops. In this way the 90 selected activities for each Consortia represent an accurate reflection of the kinds of civic education activities that each undertook as part of the Uraia program.

We next selected the specific Uraia activities for inclusion in the study, using the quota of 90 activities per Consortium discussed above. Within each Consortium, we then calculated the appropriate number of workshops, poetry-drama, and informal meetings that, given a total sample of 90 activities per Consortium, would accurately reflect that Consortium’s overall mix of activities. We then drew the appropriate number of workshops, poetry-drama, and informal meetings at random from the activity data base. This procedure resulted in 111 targeted workshops, 51 targeted poetry-drama activities, and 198 targeted informal meetings, for a total of 360 targeted activities.<sup>2</sup>

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

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<sup>1</sup> It would have taken months to code all of the 79,040 activities available to us and would have been prohibitively expensive as well.

<sup>2</sup> It was also necessary to oversample the number of poetry-drama events to yield enough sampling points for reliable analyses. The resultant sample was then reweighted in the analysis phase to reflect the true proportion of each kind of activity conducted by each of the program’s four participating Consortia.

following the random route procedures outlined in the next subsection. If, after one full day of interviewing, the survey team was unable to locate any respondents who had participated in *any* Uraia activities, the sampling site was abandoned and replaced with—whenever possible—another of the same type of activity for that Consortium drawn from the activities data base. This occurred in approximately 18 instances. We show in Table SA-1 the targeted number of activities of each type for each Consortium and the total number of each activity that is contained in the final sample. Given the lack of participant lists, and given the other uncertainties involved in these procedures, we consider the sampling for the study to have been extremely successful. Table SA-2 shows that the final sample of activities accurately reflects the regional distribution of Uraia activities as well.

Survey enumerators were given extensive training in the procedures required to select “treatment group” respondents (individuals exposed to NCEP II-Uraia civic education) and “control group” respondents (similar individuals not exposed to NCEP II-Uraia activities). Interview teams started at the exact venue where each of the 360 sampled NCEP II-Uraia activities took place. Interviewers then followed a random walk in a predetermined direction (depending on the day of the week), proceeded at least 200 meters, and began screening either the first or second household that they encountered (again depending on the day of the week). 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>3</sup> Given the random selection of the program’s activities from the activity data base, and the random route respondent selection procedures implemented in the field, we consider the treatment group sample to be a relatively accurate representation of the population of Kenyan citizens who were “treated” or exposed to NCEP-II Uraia civic education activities.

Once a treatment group respondent interview had been successfully completed, the interviewer recorded that person’s demographic information in terms of:

- Gender
- Age
- Education (highest level of schooling)
- Number of Secondary Group Memberships (number of groups to which the person belongs, including church or religious organization, youth or sports group, trade union, women’s group, cultural or school organization, burial society, tribal or clan association, business or professional association, political party, or other group).

Interviewers were then instructed to find control group individuals who had similar demographic characteristics as the given treatment-group individual but who had not attended Uraia civic education activities before the 2007 election. Interviewers were told to skip at least four houses and then begin contacting households, looking for individuals with the same gender as the treatment group respondent, whose age was within 10 years of the treatment-group

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<sup>3</sup> The exact 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.”

respondent, who was in the same general category of education (Primary, Secondary, Post-Secondary), and who was in the same general category of group memberships (0, 1-2, 3 or more). 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 and Scales**

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 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 highly polarized conditions following the intercommunal violence that occurred after Kenya's 2007 election. Nearly all of the questions represent either standard measures of the respective items in the political science literature or adaptations of recent surveys that were done in Kenya, in particular the NCEP I Impact Evaluation. The questionnaire was translated into Kiswahili by members of the Research International staff and back-translated by a professional translator in Nairobi.<sup>4</sup>

#### *Civic Competence and Engagement*

Respondents were asked a series of questions regarding their knowledge of how the Kenyan government works, ways to protect their basic rights, and their knowledge about the constitution and its reform. Respondents were also asked about their perceived political efficacy, or ability to influence government and politics. In addition, the survey asked respondents about the extent to which they participate in local and national-level politics.

*Political Knowledge.* Respondents were asked four questions about knowledge of political leaders and institutions in Kenya: whether they knew the title of the person who chairs the Kenyan parliament, who appoints members of the Kenyan High Court, how the constitution can be amended, and who is responsible for deciding how money from the Constituency Development Fund (CDF) is used. We summed respondents' correct answers to create a *Political Knowledge* scale ranging from 0 to 4. The reliability of the scale (coefficient alpha) was .69.

*Political Efficacy.* We measured the individual's sense of perceived influence in politics, or political efficacy, by asking individuals whether they agreed with the statement: "I feel well prepared for participating in political life." Responses were scored as "1" for "strongly disagree" to "4" for "strongly agree."

*Political Participation.* The survey instrument included questions on whether the respondent had done any of the following in the past year: (1) discussed political issues with friends, family, or coworkers; (2) worked for a political party or candidate; (3) participated in an organized effort to solve a neighborhood or community problem; (4) attended a meeting of the local town council or with other government officials; (5) contacted a local official, like a local councilor or an official who works for a government agency; (6) lodged a complaint with a government body or a civil society organization about unfair treatment or a violation of your

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<sup>4</sup> Sixty-seven percent of the interviews were conducted in Kiswahili, 26% were conducted in English, and 7% were carried out in one of Kenya's other languages.

rights; (7) contacted a national elected official; (8) taken part in a protest, march, or demonstration on some national or local issue; and (9) contacted a local chief or traditional leader about a problem. For each item, responses were scored on a three-point continuum, with “1” for “not done,” “2” for “once,” and “3” for “several times.” A factor analysis showed that the items tended to “load” on two different dimensions, one corresponding to *Local Political Participation* (items 1, 3, 4, 5, and 9) and one corresponding to *National Political Participation* (items 2, 6, 7, and 8). We created scales associated with each dimension by averaging the responses for the respective items. The reliability of the *Local Participation* scale was .66, and the reliability of the *National Participation* scale was .69.

*Informed about How to Protect Rights.* We asked whether individuals felt informed about what they could do to defend their rights if the police or some other group tried to stop them from exercising basic political and social rights, with responses coded as “1” for “not very informed,” “2” for “somewhat informed,” and “3” for “very informed.”

#### *Democratic Values, Rights, and Responsibilities*

We asked a series of questions concerning the respondent’s support for various democratic norms values and institutions. These questions addressed support for democracy as a form of government; support for the political values that are inherent in democratic governance; support for certain social values that tend to support democratic politics; and the perceived rights and responsibilities of citizenship.

*Democracy is Best.* We asked respondents, “Sometimes democracy does not work. When this happens, some people say that we need a strong leader that does not have to bother with elections. Others say that even when things do not work, democracy is always best. What do you think?” Responses were scored as “0” for those who say that the country needs a strong leader, and “1” for those who say that democracy is always best.

*Support for Rule of Law.* We asked respondents whether “it is sometimes necessary to ignore the rule of law and solve problems using other means”. The question was coded as “1” for “strongly agree” to “4” for “strongly disagree.”

*Vote Buying.* We asked two questions about whether respondents thought it was wrong for “a candidate or party official to offer money in return for a vote” and for “a voter to accept money in return for his or her vote.” Responses were coded as “1” for “not wrong at all,” “2” for “wrong but understandable,” and “3” for “wrong and punishable.” The questions were correlated at .59, and we created a *Vote Buying Wrong* variable by averaging the two items.

*Political Violence.* We included a question that asked whether respondents thought the use of violence was morally justifiable “to achieve an important political goal.” Responses were scored from “1” for “strongly agree” to “4” for “strongly disagree.”

*Rights Consciousness.* We probed respondents about their support for eight basic political and human rights: (1) “the right of individuals to criticize the government,” (2) “the right to form groups that push for political changes,” (3) “the right to obtain information about how government funds are spent,” (4) “the right to be free from unlawful arrest or prosecution by the government,” (5) “the right of anyone to run for elected office, regardless of ethnicity or political viewpoint,” (6) “the right to travel and work anywhere in the country,” (7) “the right of all political parties to campaign for people’s votes in all parts of the country, regardless of which ethnic group is the majority,” and (8) “the right to own land anywhere in Kenya.” For each item, responses were scored as “3” if the respondent thought that the right “should always be maintained,” “2” for “it depends on the situation,” and “1” for “almost never be maintained.” Factor analysis showed that all eight items loaded on a single dimension, and therefore a *Rights*

*Consciousness* scale was created by averaging the responses to the eight questions. The reliability of this scale was .73.

*Women's Rights.* We asked individuals three questions about women's role in Kenyan society, (1) whether "women and men should both be allowed to inherit land," (2) whether "there should be a certain number of parliamentary seats and cabinet positions reserved for women," and (3) whether "women should have the same right as men to serve as religious leaders, that is, as priests, pastors, or imams." All three questions were coded from "1" for "strongly disagree" to "4" for "strongly agree," and the three scores were averaged to create a general *Women's Rights* variable. The reliability of the scale was .58.

#### *Identity and Ethnic Group Relations*

In light of the interethnic violence that occurred after the 2007 election, we included several questions on the strength of ethnic and national identification, tolerance of other ethnic communities, and beliefs about ethnic group rights.

*National versus Tribal Identity.* We asked individuals to provide an assessment of their identity as a Kenyan versus that of a particular tribe or ethnic group. We first asked individuals, "What is your tribe," and then asked them: "Let us suppose that you had to choose between being a Kenyan and being a (INSERT TRIBE). Which of the following statements best expresses your feelings: "I feel only Kenyan," "I feel more Kenyan than (TRIBE)," "I feel equally Kenyan and (TRIBE)," I feel more (TRIBE) than Kenyan," and "I feel only (TRIBE)."'

*Ethnic Tolerance.* We included a battery of questions to test respondents' willingness to extend basic political and social rights to members of other ethnic communities, particularly groups that the respondents disliked. For this, we used a multistep procedure. First, we asked respondents about their feelings toward each of Kenya's five largest ethnic communities (Kalenjins, Luos, Kikuyus, Kamba, and Luhyas). Respondents were asked whether they liked or disliked each group, and responses were recorded on a 5-point continuum with "1" indicating "like the group very much" and "5" indicating "dislike the group very much." Next, we asked respondents whether there was one group among the five that they liked the *least*. We considered this the respondent's "Most Disliked Group" (MDG).<sup>5</sup> Third, after having identified a Most Disliked Group, respondents were asked whether members of that group should be allowed to "form their own political party" and "speak in your community even if they say things that you do not think are right." Responses were scored from "1" for "strongly disagree" to "5" for "strongly agree." These two items were moderately correlated at .24 and were averaged to create a *Political Tolerance of Most Disliked Group* variable. Next, we asked two questions about interethnic social relations. Respondents were asked whether they would want their son or daughter to marry a member of the MDG and whether they would support an organized effort to keep members of the MDG from living in their area. Again, responses were from "1" as "strongly disagree" to "5" as "strongly agree." The two items were averaged to create a *Social Tolerance of Most Disliked Group* variable, with an interitem correlation of .33.

*Oppose Ethnic Violence.* Respondents were asked, "If one ethnic or religious group feels threatened by other ethnic or religious groups, the use of violence to defend itself is morally justifiable." Responses were scored from "1" for "strongly agree" to "4" for "strongly disagree."

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<sup>5</sup> In the event that a respondent refused to identify any group as her MDG, we used the group with the lowest score on the like/dislike questions as that respondent's MDG. In the case of a tie, we randomly selected one of the five largest groups as the respondent's MDG.

**Table SA-1. Targeted and Sampled Activities by Uraia Consortium**

Consortium	Targeted Activities			Sampled Activities		
	Workshops	Poetry-Drama	Informal Meetings	Workshops	Poetry-Drama	Informal Meetings
CRECO	47	23	20	47	23	20
CEDMAC	18	12	60	18	8	64
NAMCEC	14	1	75	15	0	75
ECEP	32	15	43	33	11	46
<b>Total</b>	<b>111</b>	<b>51</b>	<b>198</b>	<b>113</b>	<b>42</b>	<b>205</b>

**Table SA-2. Distribution of Sampled Activities by Province**

Province	Number of Targeted Activities	Number of Activities in Final Samples	Percent of Sampled Activities	Percent of Total Uraia Activities
Nairobi	16	11	3.1%	4.5%
Central	46	39	10.8	12.7
Coast	36	45	12.5	10.0
Rift Valley	80	71	19.7	22.3
Western	31	32	8.9	8.6
Nyanza	35	38	10.6	9.6
Eastern	89	95	26.3	24.7
North Eastern	27	29	8.1	7.5
<b>Total</b>	<b>360</b>	<b>360</b>	<b>100%</b>	<b>100%</b>

## II. Imbalance between Treatment and Control Groups

**Table SA-3. Covariate Imbalance, Pre and Post CEM balancing**

Variable	Prematching Means		Postmatching Means		Scale
	Control	Treatment	Control	Treatment	
Sex	0.60	0.60	0.57	0.58	0 = female 1 = male
Education	4.18	4.23	4.18	4.08	0 = no schooling 9 = university graduate
Age	<b>33.52</b>	<b>34.73</b>	33.67	33.86	17-86 years old
Income	<b>1.90</b>	<b>1.99</b>	1.92	1.92	1 = 5,000 or less 4 = 15,000 or more
Church Attendance	<b>4.12</b>	<b>4.18</b>	4.14	4.16	1 = less than once/year 5 = more than once/week
Employment Status	<b>0.21</b>	<b>0.18</b>	0.19	0.19	0 = employed 1 = unemployed
Marital Status	<b>0.66</b>	<b>0.70</b>	0.67	0.68	0 = not married 1 = married
Group Memberships (Average)	<b>0.43</b>	<b>0.47</b>	0.42	0.42	0 = no membership 2 = extremely active
Ever Group Leader	<b>0.30</b>	<b>0.41</b>	0.35	0.35	0 = no 1 = yes
Member of Political Party	<b>0.30</b>	<b>0.36</b>	0.28	0.27	0 = not a member 1 = yes, but not active 2 = yes, active member
Member of Civic Group	<b>0.19</b>	<b>0.31</b>	<b>0.18</b>	<b>0.23</b>	0 = not a member 1 = yes, but not active 2 = yes, active member
Political Interest	<b>2.44</b>	<b>2.50</b>	<b>2.42</b>	<b>2.48</b>	1 = very little interest 4 = great deal of interest
Discuss Politics	<b>1.54</b>	<b>1.62</b>	<b>1.57</b>	<b>1.63</b>	0 = never 1 = once 2 = several times
Uraia Media Exposure	<b>0.41</b>	<b>0.63</b>	<b>0.42</b>	<b>0.58</b>	0 = no 1 = yes
N of Cases	1800	1800	1603	1189	

Note: Boldfaced comparisons indicate significant differences at .10 level between treatment and control groups.

### **III. Robustness Checks for Results in Table 1**

We present in Table SA-4 estimates of treatment effects using alternative methods for matching treatment and control-group respondents on the observed covariates. The first column presents the baseline estimates with no matching whatsoever, and the second column shows the effects from the CEM-weighted regression as reported in Table 1 of the main text. In column 3, we impose a more stringent CEM matching procedure, whereby the possible posttreatment outcomes of political interest, media exposure, and political discussion are included in the matching procedure itself. In column 4, we use standard propensity-score matching procedures (Gaussian kernel matching with the imposition of common support) to estimate treatment effects with the entire set of covariates (both pretreatment and possible posttreatment) included as predictors of the likelihood of NCEP II-Uraia civic education exposure. As can be seen the results are highly robust to these alternative specifications, especially, as noted in the main text, in the case of *Civic Competence and Engagement* variables.

**Table SA-4. Effects of NCEP II on Knowledge, Participation, Values, and Orientations:  
Additional Robustness Tests**

	No Matching	Table 1 Results	Alternative CEM I <sup>1</sup>	PSCORE Matching <sup>2</sup>
<b>I. Civic Competence and Engagement</b>				
Political Knowledge	.15**	.19**	.18**	.17**
Perceived Knowledge of Constitution	.09**	.13**	.16**	.12**
Informed about Protecting Rights	.10**	.10**	.08**	.12**
Internal Efficacy	.13**	.12**	.17**	.17**
Local Political Participation	.09**	.10**	.11*	.18**
National Political Participation	.00	.03	.06*	.03
<b>II. Democratic Values, Rights, and Responsibilities</b>				
Democracy is Best	.03**	.02	.01	.00
Support for Rule of Law	.03	.04	.04	.04
Rejection of Vote Buying	.05**	.05**	.06**	.06**
Opposition to the Use of Violence	.04	.05*	.06	.06*
Rights Consciousness	.01	.02**	.02	.01
Support for Women's Rights	.04	.04	.03	.06*
<b>III. Identity and Ethnic Group Relations</b>				
National versus Ethnic Identity	.03	.03	-.01	.05
Most-Disliked Ethnic Group Political Tolerance	-.05	-.01	.02	.01
Most-Disliked Ethnic Group Social Tolerance	.11**	.11**	.09	.06
Opposition to Violence as Ethnic Defense	.10**	.08**	.11**	.13**

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

<sup>1</sup> In addition to the original matching variables, the CEM algorithm includes *political interest*, *political discussion*, and *exposure to political news (radio)*.

<sup>2</sup> Results are average treatment effects on the treated (ATT) after propensity score matching using Gaussian kernel matching and imposing common support.

### **III. Rosenbaum Bounds Sensitivity Analysis**

Matching and propensity-score models control for possible biases that result from *observed* differences between the treatment and control groups on demographic, attitudinal and motivational factors that may also be related to democratic outcomes. Yet the possibility remains (as in all nonexperimental research of this kind) that some unobserved factor or factors may also lead individuals to select into the treatment, and these “hidden biases” may be responsible for the civic education effects that we have reported in the paper thus far. Rosenbaum (2002) proposes a sensitivity analysis for estimating the magnitude that any hidden bias would have to have on the selection process in order to overturn the inferences about treatment effects that have been made (see also Diprete and Gangl 2004 for additional details). The procedure involves the calculation of a series of Wilcoxon signed-rank tests of treatment effects for each dependent variable for propensity-score matched pairs of individuals under different assumptions about the likelihood of each individual in a given pair being “treated”. The initial test is conducted under the assumption of equal likelihood of each individual in the pair being in the treatment group, which is associated with an odds-ratio (or “gamma”  $\Gamma$ ) of 1. The method then simulates odds ratios or gammas of increasingly greater values due to the effects of some unobserved factor(s), such that the odds of one person in a matched pair being treated would be, say, 10% higher ( $\Gamma = 1.1$ ) due to the unobservables, then 20% higher ( $\Gamma = 1.2$ ), then 30% higher ( $\Gamma = 1.3$ ), etc. The Wilcoxon signed-rank test under each of these different scenarios is calculated until the null hypothesis of “no treatment effect” can no longer be rejected at the .05 level. The  $\Gamma$  at that point represents the magnitude of the effect that the hidden bias would have to have on selection in order to alter the inference about the treatment effect.

**Table SA-5. Sensitivity Analysis for Dependent Variables**

	$\Gamma$	Ratio of Gamma Associated with Unobserved Confounder(s) to Gammas (Selection Effect Odds-Ratios) Associated with:					
		High Income	Low-Mid Education	High Media	High Discussion	Civic Group Member	Leader of Group
Opposition to Violence as Ethnic Defense	<b>2.40</b>	2.05	1.95	2.00	1.98	1.73	1.52
Opposition to the Use of Violence	<b>2.12</b>	1.81	1.70	1.83	1.74	1.53	1.35
Informed about Protecting Rights	<b>2.11</b>	1.80	1.69	1.76	1.73	1.52	1.34
Rejection of Vote Buying	<b>1.41</b>	1.21	1.13	1.18	1.16	1.01	0.90
Political Knowledge	<b>1.35</b>	1.15	1.08	1.13	1.11	0.96	0.86
Local Political Participation	<b>1.30</b>	1.11	1.04	1.08	1.07	0.93	0.83
Perceived Knowledge of Constitution	<b>1.15</b>	0.98	0.93	0.98	0.91	0.81	0.74
Internal Efficacy	<b>1.13</b>	0.97	0.90	0.92	0.90	0.81	0.72

Gamma ( $\Gamma$ ) indicates the level at which the effect of the treatment becomes sensitive to hidden bias (upper bound p-value  $> .05$ ).

Cell entries represent the ratio between the levels of hidden bias needed to change our inferences relative to the effects of the observed covariates (obtained from the odds-ratios of the associated effects in the last column of Table SA-6 below).

The results of the Rosenbaum Bounds method, applied to the democratic outcome variables where significant treatment effects are reported in the main text, are shown in table SA-5. It can be seen that the unobserved factor(s) would have to change the odds of being treated for a given individual in an otherwise matched pair hidden bias by between 13% (for internal efficacy) to 240% (for *Opposition to Ethnic Violence*) in order to overturn the inferences that were made. Six of the eight variables register  $\Gamma$  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.

These magnitudes can then be compared to the effects that known *observed* covariates actually have on the selection process, in order to assess the plausibility that hidden biases were responsible for the pattern of treatment effects that were observed.<sup>6</sup> We show the results of the

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<sup>6</sup> It should also be noted that the sensitivity analysis provides a relatively conservative test of possible hidden bias. The unobserved covariates are assumed to be related to the

selection model predicting NCEP II-Uraia civic education exposure in Table SA-6; it can be seen, for example, that belonging to the highest versus lowest category of interest, income, media attentiveness, political discussion increased the odds of receiving treatment by factors of 1.16, 1.17, 1.20, and 1.22, respectively. This means that, for hidden bias to alter the inference we made in the case of, for example, political knowledge ( $\Gamma = 1.35$ ), the effect of the unobserved factor(s) would have to be larger than *all* of these observed factors, and exert effects on selection *over and above* the effects of these observed factors as well. This appears implausible, given that these observed covariates are almost certainly proxying to some extent for unobserved confounders related to personality, prior democratic tendencies, and the like. Even in the case of the two variables with the weakest associated  $\Gamma$  (*Internal Efficacy*,  $\Gamma = 1.13$ , *Perceived Knowledge of Constitution*,  $\Gamma = 1.15$ ), the magnitude of the hidden bias on selection would have to be almost as large as being in the highest category of interest and income and nearly as large as being in the high media and discussion groups, again with these hidden biases operating over and above the effects of the observed covariates. And 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; 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. We conclude that the results we reported are robust to reasonable levels of potential bias caused by unobserved factors related to individuals' (self-) selection into the NCEP-II Uraia treatment.

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dependent variables so strongly as to predict perfectly which of the two matched individuals ranks higher on the outcomes (See DiPrete, Thomas A. and Markus Gangl. 2004. "Assessing Bias in the Estimation of Causal Effects: Rosenbaum Bounds on Matching Estimators and Instrumental Variables Estimation with Imperfect Instruments." *Sociological Methodology* 34(1): 271–310). The method will therefore find bias even if the unobservables' effects on the dependent variable are weaker than assumed in the test. For a recent application of the method that follows closely our own procedures, see Wermink, Hilde, Arjan Blokland, Paul Nieuwbeerta, Daniel Nagin, and Nikolaj Tollenaar. 2010. "Comparing the Effects of Community Service and Short-Term Imprisonment on Recidivism: A Matched Samples Approach." *Journal of Experimental Criminology* 6 (3): 325–49.

**Table SA-6 Estimating the Propensity of NCEP II-Uraia Civic Education Exposure**

	Coefficient	Odds-Ratio
Middle Income	.02 (.10)	1.02
High Income	.16* (.09)	1.17
Low-Middle Education	.22** (.10)	1.25
Middle Media	.10 (.08)	1.11
High Media	.18 (.13)	1.20
High Discussion	.20** (.08)	1.22
Civic Group Member	.34** (.11)	1.40
Leader of a Group	.45** (.08)	1.57
High Churchgoing	.11 (.08)	1.12
Middle Group Membership	-.16 (.23)	0.85
High Group Membership	-.41* (.24)	0.66
Middle Interest	.11 (.14)	1.12
High Interest	.15 (.12)	1.16
Male	-.07 (.08)	0.93
Political Party Member	-.02 (.10)	0.98
Age (25-34)	.04 (.10)	1.04
Age (35-49)	.06 (.11)	1.06
Age (50-86)	.13 (.14)	1.14
Constant	-.83** (.29)	

Results from logistic regression with standard errors in parentheses.

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

#### IV. Alternative Presentation of Table 2

See main text, p.61 and footnote 14 for explanation.

**Table SA-7. The Effects of Post-Election Violence on Civic Competence, Democratic Values, and Ethnic Orientations 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	
	Coefficient	Standardized Coefficient	Coefficient	Standardized Coefficient
<b>Civic Competence and Engagement</b>				
Political Knowledge	.01 (.09)	.01	.02 (.08)	.02
Internal Efficacy	-.09 (.07)	-.08	.01 (.08)	.01
Local Political Participation	.11 (.08)	.08	.09 (.09)	.06
Informed about How to Protect Rights	-.05 (.04)	-.07	-.04 (.05)	-.06
<b>Democratic Values and Ethnic Orientations</b>				
Democracy is Best	-.04 (.03)	-.10	.00 (.03)	.01
Support for Rule of Law	-.12 (.08)	-.12	.02 (.08)	.02
Opposition to the Use of Violence	-.12* (.06)	-.13*	.01 (.06)	.01
Opposition to Violence as Ethnic Defense	-.15** (.06)	-.16**	-.14** (.06)	-.15**
Opposition to Violence to Avenge Past Wrongs	-.08 (.06)	-.09	-.01 (.06)	-.01
Most-Disliked Ethnic Group Political Tolerance	.10 (.08)	.08	.12 (.08)	.10
Most-Disliked Ethnic Group Social Tolerance	.01 (.07)	.01	.10 (.08)	.09
Forgive those who Perpetrated Violence?	-.10* (.05)	-.13*	-.03 (.05)	-.04

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

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