

Civil war violence and political trust: microlevel evidence from Nepal

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Abstract

Exposure to violence can shape people's political and social perceptions. War-time effects on trust in state institutions are particularly relevant for political stability in the aftermath of violent conflict. If people distrust the state, they are less likely to endorse reform plans, will be less inclined to comply with state rules and regulations, and may uphold support for challengers of state authority. Our paper contributes to the understanding of the role of violence for trust in the national government. We use high-quality, geo-referenced survey data, joined with village-level information on civil war casualties, to estimate the effects of exposure to violence on political trust in Nepal. We find that exposure to violence matters for reducing trust in the national government. This association seems to be mainly driven by effects of violence at the outbreak of the conflict as well as at the end of the civil war period under investigation. These findings shed new light on the complex associations between exposure to violence and political trust.

Keywords

Civil war, Nepal, political trust, violence

Introduction

Postwar countries are economically weaker, display lower levels of socio-economic development and have higher risks of renewed violent conflict than other countries (Chen et al., 2008). The material causes are well known: civil wars destroy infrastructure, weaken institutions and absorb scarce financial resources (Collier et al., 2008; Hoeffler and Reynal-Querol, 2003). The immaterial impact of violence has received considerably less attention. Exposure to political violence can affect collective norms, as well as individual social and political

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attitudes. Notably, it may shape how people perceive the state and its institutions. Such consequences of civil war are harder to capture and analyze empirically. However, they can be crucial for the prospects of stabilization in the direct aftermath of violence, as well as for long-term economic, social and political development of postwar countries.

In this paper we add to the understanding of such legacies of violence. Particularly, we are interested in the impact of civil war violence on political trust. Whether people trust in the state is not only shaped by pre-war allegiances or by socio-economic characteristics of the postwar context. The war itself shapes people's perceptions. Varying experiences with violence in war-time might have an impact on how people perceive the state. We argue that exposure to violence has important effects on trust in the national government. Violent conflict is a blatant sign of the government's inability to uphold its monopoly over the use of violence and to protect citizens from physical harm. Moreover, government action in war zones often leads to direct and collateral physical and human losses among the local population, affecting their perception of the state. We expect these dynamics to negatively impact people's trust in the national government and we believe that these effects are particularly pronounced in the direct aftermath of civil wars, before any meaningful political reforms and peace-building activities are underway.

Recently, various studies have been investigating the effects of violence and victimization on individual political and social perceptions and behavior (Bellows and Miguel, 2009; Cassar et al., 2011). However, these analyses feature a couple of shortcomings with respect to the research interest of this study: first, most research focuses on issues of social capital, as well as generalized or inter-personal trust, neglecting the political dimension of trust; second, the majority of the studies rely on surveys and social games that have been undertaken years after the end of the war. Thus, patterns of political trust in the most volatile, direct aftermath of civil wars remain under-investigated.

Our paper fills some of these gaps. We analyze the effects of exposure to civil war violence on political trust in Nepal. We use data that have, to the best of our knowledge, not been used for comparable analyses before: the World Health Survey (WHS) was carried out during a brief period of peace in 2003 following the peak of violence of the Nepalese civil war. It contains information on political attitudes as well as the exact location of more than 8500 respondents. This allows us to match the data with information on war-time violence on Nepal's smallest administrative level and to compute levels of exposure to civil war violence for all respondents. To strengthen inference, we consider a comprehensive list of individual- and village-level covariates and district-level fixed effects in our estimations. Furthermore, we implement an instrumental variables analysis that leverages the geographic location of respondents as a source of exogenous variation in the total exposure to violence. Our analysis, in contrast to some previous studies, finds that exposure to violence reduces political trust.

The remainder of the paper is organized as follows. Section 2 discusses existing work on political trust. In Section 3 we develop an argument about the effects of exposure to violence. Section 4 provides context on the Nepalese civil war. We present our empirical research design and data in Section 5 and main findings in Section 6. Section 7 concludes and discusses implications for further research.

Political trust and why it matters

The aftermath of violent conflict is marked by volatility, transition and uncertainty. The warring parties' motives and strategies are unknown, the reliability of the government's promises of

economic and political reform is hard to assess, and contradictory information and assessments by various political actors cannot be verified. At the same time a great deal is at stake for the population: people might have high expectations with regard to the improvement of their living conditions, worry about potential political and economic disadvantages and fear for their physical security in case armed conflict recurs. How people behave in such situations of uncertainty will, among other things, be shaped by their perceptions of the political actors involved.

Thus, political trust can be considered particularly relevant in postwar societies. People's trust in the state reflects their judgment as to whether the state and its institutions and actors are motivated to deliver on their promises and to act in the interest of the people (Levi and Sacks, 2009; Sacks and Larizza, 2012). In the words of Miller and Listhaug (1990), political trust is the "judgment of the citizenry that the system and the political incumbents are responsive and will do what is right even in the absence of constant scrutiny".¹ Pathologically low levels of political trust among large parts of the population may undermine the implementation of peace agreements as well as the implementation of essential reforms; it may reduce citizens' compliance with laws and regulations and increase the risk of violent conflict recurrence.

Reforms lead to political change that can produce winners and losers. People will resist governmental reform plans if they do not believe that they will profit adequately in absolute or relative (compared with other groups) terms. Their appraisal of reforms will strongly be influenced by their level of trust in the institutions and actors that propose these reforms (Chanley et al., 2000). The more people distrust the state and incumbents, the more they will fear to lose from any reform and the more likely they will be to reject plans for necessary economic, social or political change.

Furthermore, empirical research indicates that people are less likely to comply with government regulations if they mistrust state institutions (Levi and Stoker, 2000). It seems plausible that people will be reluctant to bear the costs of duties as citizens when they feel that the government cannot be trusted (Levi et al., 2009). Studies demonstrate that a lack of confidence in state institutions can undermine peoples' willingness to pay taxes (Fjeldstad, 2004). Low trust levels may also compel people to avert other forms of rules and regulations: from allocation of resources, over rulings of the judiciary, to the state's claims over the monopoly of force.

People have supported violence in the first place because of specific motives—may it be grievances stemming from discrimination, hope for economic gains or out of ideological resistance against specific policies. In many cases at least some of these motives will still be present when violence ends. If it does not seem believable to the people that the state is committed to act on their grievances and to consider their political, social or economic claims, they will be more likely to be willing to support renewed violence against the state (Hutchison and Johnson, 2011). Thus, lack of political trust may motivate (renewed) violent action against a state that is not perceived to act in one's own interests. This dynamic is especially relevant in the context of immediate postwar bargains in which trust plays an essential role for the successful implementation of peace agreements or the re-establishment of the monopoly of violence by the state.

The legacy of war—effects of violence on perceptions and behavior

If political trust can play such an important role for stability after violent conflicts, it is important to understand what factors increase or reduce it. Previous research has found that

various economic, political and socio-economic factors can impact people's political trust (Askvik, 2008; Catterberg and Moreno, 2006; Gilley, 2006; McAllister, 1999; Zerfu et al., 2008). Whereas we will consider some of these factors in our own analysis, we want to focus on a variable that can be considered particularly relevant in postwar countries: exposure to violence.

Various studies investigate how individual victimization affects people's political and social behavior. For example, analyses by Bellows and Miguel (2009) in Sierra Leone show that individuals whose households directly experienced more intense war violence are more likely to attend community meetings and join local political and community groups. Similarly, Blattman (2009) finds that war-time witnessing of violence increases community leadership among ex-combatants in Uganda. Using results from social games and a survey undertaken in Tajikistan, Cassar et al. (2011) find that self-reported victimization is negatively associated with trust in people within the same village and positively with trust in citizens from distant regions. In their analysis of the effects of war-time experience on social and economic behavior in postwar Burundi, Voors et al. (2012) find consistent evidence that violence exposure increases altruistic behavior and risk-seeking and decreases patience with respect to inter-temporal choices (see Callen et al., 2014, for other results on violence exposure and risk preference). De Luca and Verpoorten (2012) analyze the effects of civil war violence on generalized trust and associational membership. Their results indicate a decrease in social capital in districts exposed to battle events. Using similar data but a different empirical strategy, Rohner et al. (2013) also find negative effects of violence on generalized trust. Gilligan et al. (2014) find that members of communities with greater exposure during Nepal's civil war are more likely to contribute to public goods and are significantly more trusting. Using survey data from northern Afghanistan, Weidmann and Zuercher (2013) do not find evidence for the hypothesis that wartime violence leads to an increase in social cohesion.

Social capital as well as interpersonal and generalized trust are essential for political and social recovery. As argued in the previous section, we believe, however, that people's perceptions of national level institutions—their trust in the state—is equally important but has received much less scholarly attention. Results of the few studies that consider the effects of violence on political attitudes and political participation yield contradictory results: Blattmann (2009) as well as Bellows and Miguel (2009) find that exposure to violence increases voting. In their analyses of the determinants of political trust in Sierra Leone, Sacks and Larizza demonstrate that respondents who live in areas that were particularly affected by the war are more likely to view their local government councilors as trustworthy (Sacks and Larizza, 2012). Performing similar analyses but focusing on trust in central state institutions, Bakke et al. (2012) do not find any significant association between victimization and trust in president and parliament in Abkhazia. Hutchison and Johnson (2011) analyze how violent conflict affects country-level political trust levels. They find significant detrimental effects of political violence on political trust. Similarly, a recent study by Gates and Justesen (2013) finds that political violence by Tuaregs against the Malian state had negative effects on people's trust in the national president.

Diverging findings of these studies may be attributed to differences across cases and methodological approaches. We argue, however, that a specific issue is of particular relevance beyond individual empirical strategies. Surveys and behavioral games in Abkhazia (Bakke et al., 2012) and Tajikistan (Cassar et al., 2011) were done more than 10 years after the end of the respective armed conflicts. The studies on Sierra Leone use data that were collected two to three years after the end of the civil war. Hutchison and Johnson investigate effects of

violent conflicts on political trust in the following year. Gates and Justesen (2013) investigate the direct attitudinal effects of a single attack in an ongoing long-term conflict rather than of actual exposure to political violence over a specific period of time. Thus, these studies differ substantially with respect to the specific postwar or post-violence phase in which the respective analyses take place.

Associations between exposure to violence and individual perceptions may, however, change over time: thus, the stage of postwar recovery at which trust is measured may play a pivotal role for the respective empirical findings. More specifically, associations between violence and trust may be affected by peace-building activities after violent conflicts. The end of a civil war opens up space for processes that support coping with previous experiences of violence—through formal mechanisms of truth and reconciliation, community-based interventions for social healing or individual psycho-social support programs. Moreover, while violence may be a defining element of state–society interactions during war-time, other aspects, such as direct or indirect political participation or access to public basic services, may develop to become more essential during peace-time. The more time has passed since the end of a civil war the more previous experiences of violence may lose in importance as compared with more recent postwar state–society interactions. De Luca and Verpoorten (2012) lend some support to such a time-dependency—they find evidence for short-term negative associations between violence and social capital followed by processes of recovery after the end of violence.

With regard to timing, most previous studies on the effects of violence on trust focus on the longer-term effects of violence, rather than on perceptions in the immediate aftermath of civil wars. Doubtlessly, postwar social, economic and political recovery is a long-term process. Many countries relapse into civil strife many years after previous civil war had been ended. Thus, studying the long-term effects of civil wars is of high relevance for understanding prerequisites for successful peace-building. As argued above, we believe, however, that associations between exposure to violence and political trust are equally important in immediate postwar situations, where political fragility and volatility are particularly high and where essential but delicate political and institutional change is to be initiated—for example, elections, political reform and reconciliation. Contrary to longer-term effects of violence, the role of violence for political trust in the aftermath of violence has barely been touched upon.

Our paper aims at filling some of this gap of previous research: we investigate the role of civil war violence on political trust using information on village-level intensity of violence and survey data that were collected during a brief cessation of fighting in the Nepalese civil war. This allows us to investigate associations between the exposure to civil-war violence and political trust in the direct aftermath of a ceasefire.

Our main argument is intuitive but in contradiction to previous studies that find positive associations between exposure to violence and trust. We argue that observing civil war violence and the state's inability to end it exerts negative effects on people's trust in the state. The inability of the government to uphold the monopoly of violence or alternatively address underlying grievances that drive opposition to the state communicates low competence. Spectators will downgrade their assessment of the government. Living in areas affected by substantial violence increases the risk that people perceive the state not only as unable to provide security, but also as an actual security threat in itself. The local population suffers from unintended consequences of counter-insurgency campaigns, such as collateral damage to local infrastructure or physical injuries and death resulting from military encounters of state actors and rebel forces. Moreover, in many civil wars state agents commit massive

human rights abuses themselves—as a means to enforce local support, extract information or deter support of rebel movements (Kalyvas, 2006). Again, we believe that exposure to such state action will reduce people's trust in the government. Measuring levels of trust directly after a ceasefire allows us to investigate such associations before postwar social and political processes begin to change the relationship between exposure to violence and political trust. Thus, we expect a negative association between exposure to violence and trust in the national government, leading us to our main hypothesis:

Hypothesis: Exposure to civil war violence decreases political trust in the aftermath of civil war.

The civil war in Nepal 1996–2003

Nepal serves as a case study for our empirical analysis. More specifically, we focus on the period from the beginning of armed conflict in 1996 until the cease fire in 2003. The truce was accompanied by negotiations between Maoist rebels and the Nepalese government. Even though the dialogue eventually broke down and violence resumed, the brief period of peace may be understood as a critical postwar stage, where political trust plays a particularly important role in the sense described above. Thus, we are interested in how the previous period of violence shaped people's perception of the Nepalese government in the seven month period of peace and negotiations. Before we proceed with the description of the data we use and the analysis we perform, we will briefly describe the context of the conflict and the warring parties. As all our analyses focus on the period from 1996 to 2003, accounts will be confined to this phase of the civil war.

Various economic, political and social aspects have been emphasized as causal factors that lead to the civil war. Nepal's economic performance was marked by widespread poverty and pronounced regional economic disparities. Some of the areas where the insurgency set off had been excluded from development activities and ranked among the poorest across the country. This has led some observers to emphasize the role of absolute and relative deprivation for the political violence that raged through the country (Graham, 2007; Murshed and Gates, 2005; Riaz and Basu, 2007). Others have highlighted the role of political dynamics. In the early 1990s widespread popular protest made the government accept the establishment of a constitutional monarchy. Ensuing reforms, however, failed to bring about substantial political change. The first elections in 1991 did not fulfill expectations for the new democracy with respect to redistribution of power. Moreover, the newly established system has proven incapable of balancing diverging interests and demands by peaceful means. Evolving Maoist political activities were suppressed, fueling the creation of the insurgency (Gersony et al., 2003; Kumar, 2005). Finally, the ethnic dimension of the conflict has been stressed: the Maoists have emphasized their aim of fighting against political and economic inequalities along religious, ethnic and caste differences. This narrative has contributed to mobilization in the light of inequalities between minority groups and the dominant high-caste Hindus (Kumar, 2005; Murshed and Gates, 2005). As is the case in many other violent conflicts, it was most probably a complex interplay of various of these factors that led to the outbreak of civil war in 1996.

The civil war has its roots in a rural area in the northwestern part of the country, namely the Rolpa and Rukum districts. From this heartland, the conflict diffused throughout the country (Gersony et al., 2003). Violence began in 1996 with attacks by the Maoists on police

posts in rural areas. This is a strategy that was the focus of Maoist activists until 2001, a period during which the rebels were able to install themselves across most parts of the country. Peace talks that aimed at ending the conflict broke down towards the end of 2001, when the Maoists unilaterally withdrew from the talks and launched attacks on police and army posts. As a consequence, a national state of emergency was declared, the national army began a massive counter-insurgency campaign and the conflict rapidly increased in intensity (Pettigrew, 2004). By 2002 the conflict had reached the highest level since its beginning in 1996.

The increase in conflict intensity was accompanied by ongoing political instability of the national government. In 2002 the parliament was dissolved along with all elected bodies at the local level, a move that may be traced back to political competition internal to the governing Congress Party (International Crisis Group, 2003). Elections were originally scheduled for the end of 2002. However, the Maoists declared that they would boycott the polls and call for a national strike on election day. More generally, elections were virtually impossible to organize in many regions of the war-ravaged country. Eventually, in October 2002 the king dismissed the government and assumed executive rights by himself (Gersony et al., 2003; International Crisis Group, 2003). He swiftly began a peaceful dialogue with the Maoists that led to the declaration of a mutual ceasefire in January 2003.

Even before the actual outbreak of the civil war, the state was marked by fragility and virtual absence in various sectors and regions of the country (Riaz and Basu, 2007). The national army had long been a well-respected institution; however, it was also rather ill-equipped and trained. More importantly, it was responsible for a large number of human rights abuses. Numerous incidents of extra-judicial detainment and killings have been reported. The majority of the deaths during the conflict were a result of government counter-insurgency activities. State agents have often been perceived as acting randomly. In many areas government troops were feared even more than the Maoist rebels (International Crisis Group, 2003; Pettigrew and Adhikari, 2009). Nonetheless, from 2002 on, despite its brutality and resulting alienation of many civilians, the army was also perceived by many as being able to shift the battle with the Maoists in its own favor (Gersony et al., 2003).

The following analysis aims at contributing to our understanding of how people's exposure to violence has shaped their perceptions toward the national government.

Research design, data and model specification

Data and operationalization

To test our hypothesis we rely on high-quality survey and administrative data from Nepal. Our dependent variable of interest is political trust in the national government. Operationalization and measurement are based on data from the WHS by the World Health Organization. The WHS consists of a series of similar nationally representative surveys that were implemented across 70 countries. The respective questionnaires focus on health issues and aim at compiling comprehensive information on the health of populations and the performance of health systems. However, they also comprise items related to respondents' economic situation and political perceptions. Most importantly for our purposes, the survey also contains information on the exact geo-location of the respondents. The WHS generally targets all members of the population aged 18 years or older living in private households. Households are then selected based on a random, stratified sampling procedure. Finally,

one adult member of each household is selected randomly for the interview—based on a Kish table method. All interviews are conducted face-to-face.

The WHS in Nepal was conducted between February and August 2003. The individual level response rate was 98%. The final dataset comprises interviews of 8822 households and covers 62 out of 75 Nepalese districts. Some districts have been omitted owing to inaccessibility or conflict. This selection implies that we do not have any respondents from regions with the most extreme levels of exposure to civil war violence. Nonetheless, the survey covered many areas exposed to intermediate levels of violence. Assessing the raw data on casualties across districts covered by the WHS survey, the total casualty count ranges from a minimum of zero to a maximum of 22 per year, with a mean of 2.2 and standard deviation of 3.45. For the full sample of village development committees (VDCs), including the worst affected districts excluded from the WHS sampling, the violence count ranges to a maximum of 55, with a mean of 1.53 and a standard deviation of 3.4. In other words, while the WHS did not sample from districts with the most extreme exposure to violence, it also did not sample some of the least affected districts either. Overall, we believe that the WHS sample should inform us in a meaningful way on the effects of violence on trust for large parts of the Nepalese population. At worst, excluding the most affected regions will bias us against finding strong effects for violence because we are losing exactly the respondents who suffered the most during the civil war.

The final dataset contains information on respondents from 329 out of Nepal's 3857 villages. The item nonresponse rate varies greatly across various sections of the survey—they are highest for items on household expenditures ($> 30\%$) and health insurance ($> 20\%$), but well below 10% for most sections of the survey and $< 1\%$ for the items on political trust that this paper is particularly interested in.

We use the following survey item as an as indicator for trust in the national government: "How much of the time do you think you can trust the NATIONAL government to do what is right?"² Figure 1 displays the variation across the five possible answers.

The distribution of answers indicates that more than half of all respondents in the survey (4964) express never or hardly ever trusting the national government, illustrating the precarious support that national institutions had in Nepal during the ceasefire. For our analysis we rely on an inverted version of the Likert-scaled survey item, which assigns higher numerical values to higher levels of trust in the national government.

Our main independent variable is the local exposure to civil war violence. We use count data on the number of total killings per VDC. Information is taken from the Informal Sector Services Center (INSEC), a Nepalese non-profit human rights organization. These data contain information on the total number of killings by Maoists and government forces in the years 1996–2003 for each VDC. Note that the number of killings includes fatalities of the two factions' troops, as well as civilian killings. We focus on the absolute number of killings from 1997 to 2002, because we believe that this fits more closely with the theoretical argument.³ The effects of violence on political trust in national-level institutions is driven not so much by differences in relative levels, but by the absolute level of violence witnessed by the local population. For additional robustness checks though, we also use the total number of killings in each VDC, normalized by VDC population size.

To estimate a respondent's direct exposure to the level of killings per capita, we use the longitude and latitude information contained in the WHS for each respondent and match them to the appropriate districts and VDCs. Figure 2 shows the distribution of the exposure to violence across respondents. The majority of respondents is from local communities that

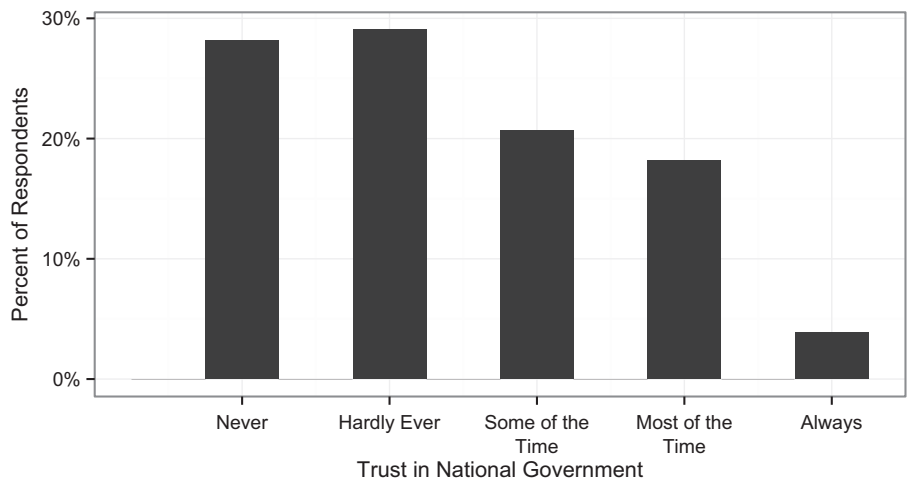


Figure 1. Levels of political trust in the WHS survey.

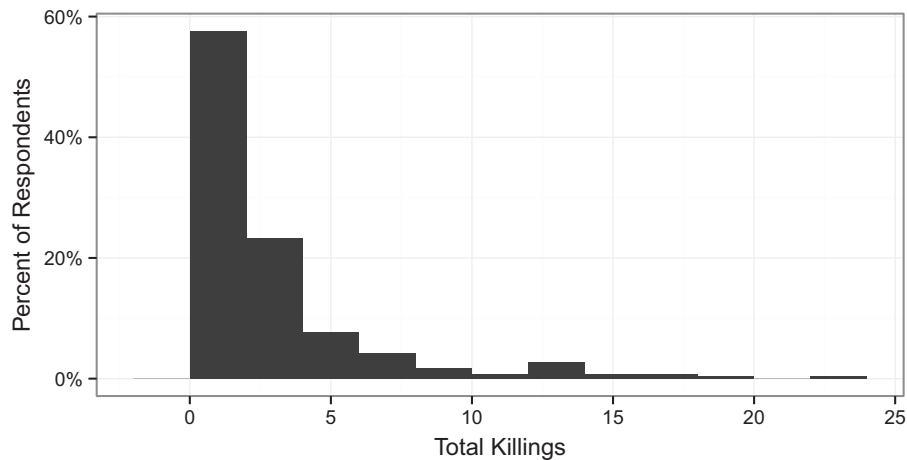


Figure 2. Exposure of WHS respondents to violence.

did not actually experience violence directly, but more than 40% were exposed to some form of civil war violence in the 1996–2002 time period.

Figure 3 further illustrates the distribution of violence and respondents across the territory of Nepal. It shows the broad coverage of the WHS across Nepal and how killings were concentrated within particular VDCs.

Alternative explanations and control variables

To identify our relevant control variables, we draw on previous research that has found various political, economic and socio-economic factors to matter for people’s perception of state

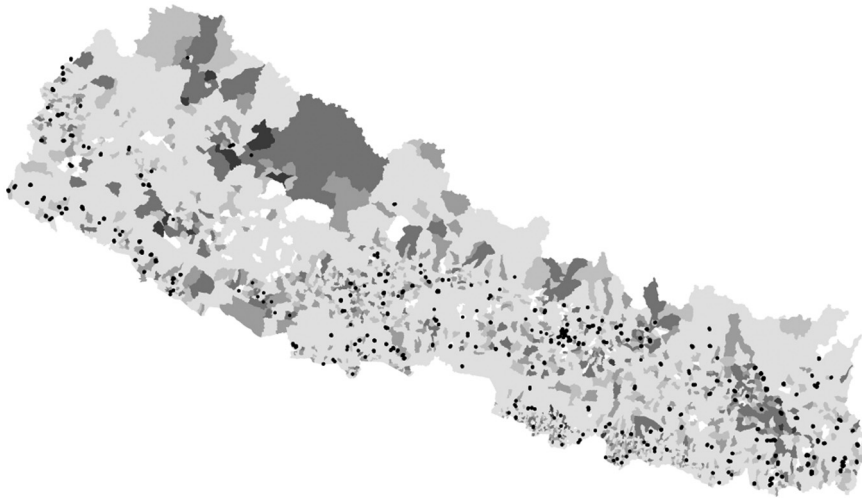


Figure 3. Total killings 1996–2002; darker shades show higher levels of killings and WHS respondent locations are shown in black.

institutions and actors. Even though we are not able to control for all factors that have been emphasized in the past, we will consider aspects that seem particularly relevant to us and that pose the danger of omitted variable bias.

The first variable we control for is village population size. Cross-national research has shown that larger populations are more prone to experiencing violence (Fearon and Laitin, 2003), that is, VDC population size might affect the level of exposure to violence.

Our second control relates to basic services delivery. Ensuring access to basic services is among the essential welfare functions of the state (Call, 2011). The state's inability or unwillingness to provide equitable access will be directly felt by the people and thus shape their attitudes and actions toward the state (Askvik et al., 2011; Sacks and Larizza, 2012). We use a measure from the WHS survey that records whether the household has access to piped water or a public standpipe to proxy for government services provision. We also include a survey item asking about access to electricity.⁴

Several studies emphasize the role of economic factors such as poverty and inequality for trust (e.g. Finkel et al., 1989; Haggard, 1990). The underlying assumption is straightforward: people will rather trust in the state if they are well off economically (Catterberg and Moreno, 2006; McAllister, 1999). In our empirical estimations we include two different variables. First, we include the VDC level of income polarization, based on income data from the 1996 Nepal Living Standard Survey and expressed as an Esteban-Ray (1994) polarization index, calculated by Nepal et al. (2011), which has also been found to correlate with violence during the civil war. Second, we include a VDC poverty measure, the share of the local population below the poverty line, also provided by Nepal et al. (2011).

People might rather trust in other people and institutions that belong to their own identity group. Zerfu et al. (2008) find that people who strongly identify with their own ethnic groups display lower levels of generalized trust. This in turn might reduce people's trust in institutions that are controlled by members of other ethnic groups. Similarly, an analysis of

post Apartheid South Africa (Askvik, 2008) finds that identities, notably peoples' race, matter for institutional trust. To account for the potential effect of ethnicity and a rift between the local population and the national elite, we include the percentage of Nepali speakers and the percentage of the population belonging to a higher caste at the VDC level as controls.⁵

Research on the determinants of legitimacy and political trust has emphasized the role of political participation. Respective analyses are based on the assumption that people will be more likely to trust in the state if they are able to influence its policy (e.g. through elections) (Carter and Castillo, 2011; Gilley, 2006; Mishler and Rose, 2001). To capture these effects we include an additional dummy variable from the WHS survey on whether a respondent voted in the 1999 national election. Importantly, this variable also serves as an indirect indicator of prior levels of support for the national government because the Maoists actively discouraged participation in the elections.

Finally, we consider conventional socio-economic indicators that might have an impact on political trust. Notably, we use individual information from the WHS to account for respondents' age, gender and level of education.

There are other potential confounders that might impact rebel violence and levels of political trust: more fine-grained aspects of government services provision, baseline levels of social capital, human development outcomes like infant mortality or other relevant characteristics. Short of detailed measures, we use district-level fixed effects in our analysis. Including fixed effects for each of the 75 districts allows us to account for any unobserved and time-invariant factors at the district level that might confound the relationship between violence and trust in the national government.

Model specification

Our dependent variable is a typical Likert-scaled measure of trust in the national government for each respondent. Trust y_{ijk} of each respondent i in VDC j , nested in district k , is modeled as a linear function of covariates in the following way:

$$y_{ijk} = \alpha + \beta v_{jk} + \mathbf{x}'\theta + u_k + \epsilon_{ijk}$$

The parameter β estimates the effects of direct exposure to violence at the VDC level (v_{jk}). The vector \mathbf{x} contains all the VDC and individual-level controls discussed in the previous section. The parameter u_k represents district fixed effects, while ϵ_{ijk} is the independently and identically distributed error term. The inclusion of district fixed effects and the encompassing list of controls provides increased confidence that we can consistently estimate the effects of the exposure to civil war violence. All models are estimated with standard ordinary least squares (OLS) and we cluster standard errors at the district level to account for heteroskedasticity and arbitrary serial correlation within clusters.

Results

Note that our research design constitutes a difficult test for our hypothesis. Individual-level survey data are characteristically very noisy and since the WHS survey did not sample from regions with the highest levels of violence, it will be very difficult to precisely single out any effects of political violence at the village level on political trust. Table 1 presents the results for the main regression model. The model shows that of the control variables, in particular

Table 1. Trust in the national government

	(1) FE-OLS, clustered SE
<i>log(Population)</i>	−0.0417 ⁺ (0.0222)
<i>Income polarization</i>	1.971 (1.220)
<i>Poverty</i>	−0.336 (0.213)
<i>Percentage of Nepali speakers</i>	−0.112 (0.139)
<i>Percentage of higher caste</i>	−0.113 (0.188)
<i>Age</i>	0.00305* (0.00140)
<i>Gender</i>	0.0217 (0.0300)
<i>Education</i>	−0.0772*** (0.0177)
<i>Voted</i>	0.0269 (0.0388)
<i>Piped water</i>	−0.0154 (0.0429)
<i>Electricity</i>	−0.0807* (0.0330)
<i>Total killings</i>	−0.00897* (0.00451)
<i>Constant</i>	2.761*** (0.320)
Observations	8348
Adjusted R ²	0.017
F	14.01

Clustered standard errors in parentheses. FE, fixed effects; SE, standard error.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

VDC population, respondent's age, respondent's education level and access to electricity, are relevant predictors of political trust. As expected, higher population counts decrease trust in the national government (statistically significant below the 10% level). Older respondents show higher levels of trust in the national government, statistically significant below the 5% level. More educated individuals trust the government less (statistically significant below the 0.1% level). Higher levels of education are likely to increase demands on government services and performance, while at the same time increasing awareness about relative deprivation and governmental failure. All three effects are in line with existing research and confirm the basic validity of the model. Surprisingly, access to electricity has a negative effect on trust, but this might be due to the fact that households with electricity also have higher socio-economic status, with access to radio or television, increasing their demands on government services and exposure to critical evaluations of government activity. Importantly though, none of the other control variables attains statistical significance.⁶

Now, turning to the test of our main hypothesis, the total number of killings in the VDC has the expected sign. Exposure to civil war violence from 1997 to 2002 decreases trust in the national government. The effect is statistically significant below the 5% level and indicates support for the notion that citizens punish a disturbance of the monopoly of violence in their own localities via local levels of trust in the national government. The substantive size of the effect is also non-negligible: a 1 standard deviation increase in civil war violence is associated with levels of trust in the national government approximately lower by 0.03 points on the five-point scale. A respondent from a VDC with the lowest level of violence is expected to have a level of trust into the national government about 0.2 points higher than a respondent from a VDC with the highest level of violence—an 8% difference given the average level of support.

Robustness checks

Although we control for a number of important covariates and include district fixed effects, we also consider several additional checks to ascertain the robustness of our finding (the Online Appendix provides detailed regression tables). First, we check the sensitivity of our findings to changes in model specification. We repeat all our analyses using a measure of exposure to violence that normalizes the level of killings by local VDC population size. Doing so produces substantively identical results. We also alternatively estimate random effects models, instead of fixed effects, without any impact on our main finding. Similarly, controlling for media exposure by using information on access to a radio does not alter our findings.

Second, we assess whether relying on a single survey item affects our main results. Relying on a single survey question can produce spurious findings, because the risk of idiosyncratic wording having an effect on our results is higher. The WHS does not contain an extensive battery of political questions, but offers a few other questions that we can exploit for robustness checks. Specifically, the survey also includes items eliciting levels of trust in local government, an assessment of government responsiveness and levels of government repression. We believe that the question on trust in local government, while aimed at a different level of government, can act as a useful alternative measure for the same concept. In fact, the questions on trust in national and local government correlate at the 0.68 level, whereas the two other measures ask about different concepts and show hardly any correlation with the trust items. Repeating our main analysis with levels of trust in local government as our dependent variable produces qualitatively similar results. For the main regression using total killings until 2002, the effect is still estimated to be negative, but fails to attain standard levels of statistical significance. When we disaggregate the level of killings temporally though (see below), we find very strong and statistically significant and negative effects for the initial levels of violence on trust in local government.

Third, we address potential endogeneity issues. While it is unlikely that simple reverse causality affects the link between violence in the pre-ceasefire period and levels of trust measured in 2003, it could be the case that, for example, the Maoist rebels applied violence selectively in villages with higher baseline levels of government trust. Conversely, most fighting might have taken place in regions with high support for the Maoists and little trust in the national government. We believe, however, that these concerns are largely unfounded. We included a comprehensive list of individual-level and village-level controls that can act as

confounding variables. Furthermore, all our models include district-level fixed effects. Hence, any unobserved factors at the district-level (e.g. services provision by the district government, distance to the capital, baseline Maoist support, etc.) are accounted for and do not interfere in our analysis of exposure to violence. Moreover, several pieces of evidence suggest that selective use of violence across villages within districts did not occur. Our measure of total killings contains killings of civilians by Maoists, but more importantly also fatalities from encounters between rebels and government troops. The location of skirmishes between insurgency and counter-insurgency forces within districts is unlikely to be tightly linked to baseline levels of governmental trust, but is rather driven by tactical or other unrelated reasons. In fact, Gilligan et al. (2011) point out that violence in the Nepalese civil war can be characterized by a near-random and haphazard pattern. Owing to the difficult geography in many of Nepal's districts and the conflict strategy of the rebels, killings by Maoists reach high levels in some locations, but remain low in many similar villages, solely for idiosyncratic reasons. This implies that the degree of exposure to direct violence is in large parts driven by factors unrelated to political trust. Investigating associations between indicators of the local population's baseline support for the government and subsequent levels of killings, our own data lend support to this argument. In our sample we observe no correlation between total killings in the village and individual participation in the 1999 election, which is an indirect indicator of prior government support. We also do not see any significant correlation between killings in the village and turnout in the 1999 election measured at the district level, a point that is also confirmed for district-level violence by Do and Iyer (2010).

To further probe the robustness of our fixed effects estimate, we implement a sensitivity analysis following Bellows and Miguel (2008). They suggest a simple method that compares the "full" regression estimates to a "sparse" model to identify a bound on the size of any remaining omitted variable bias needed to invalidate our findings. To estimate the size of the theoretical bias we have to compare our estimates for the effect of the violence across different sets of regression models. Specifically, we take the estimate from our "full" specification (all control variables and district fixed effects) and compare it with the estimate from a "sparse" or "restricted" model, that only includes our violence variable and district fixed effects. The ratio

$$\frac{\widehat{\beta}_{\text{full}}}{\widehat{\beta}_{\text{sparse}} - \widehat{\beta}_{\text{full}}}$$

increases in the size of the estimated regression coefficient for the full model, which is the conservative estimate of the effect, and decreases in the differences between regression coefficients between the conservative and more permissive model, that is, the degree to which observable factors change the estimate. The higher the ratio, the larger the selection on unobservables must be to explain the estimated effect. We find that selection on unobservables would have to be 89% of the variation explained by selection on observables to invalidate our findings. This is close to the suggested rule-of-thumb threshold of 100% suggested by Bellows and Miguel.

We also employ as a robustness check an instrumental variable strategy that exploits an exogenous source of variation in the total level of killings. We identify two instruments to explain variation in the total number of killings in each VDC. First, we use the minimum distance from the respondent's district to the closest of the districts in which the civil war

violence initially started. Second, we use the level of elevation. The reasoning for the first instrument is that districts closer to the outbreak of the Maoist rebellion had longer exposure to civil war violence as the rebellion spread through parts of the country. Geographic distance here proxies for the length of time regions were exposed to fighting between Maoist forces and the government, which correlates with a higher casualty count.⁷ Similarly, villages at lower levels of elevation were more accessible to government and rebel troops and thus more likely to feature as locations of military incidents. In total, respondents close to the initial outbreak of violence and in more accessible villages are more likely to have experienced higher levels of killings, as compared with respondents further away in remote locations. At the same time, pure geographic distance to one of the initial districts and remoteness are unlikely to have any effects on levels of political trust, other than through the exposure to civil war violence.

We estimate two models, the first in which we include the log of the minimum distance and log elevation levels as instruments, and the second in which we also add their interaction term. In both models we include our standard set of covariates and district fixed effects—which is important to block alternative channels from the instrument to our outcome variable. We also cluster standard errors at the district level. Table 2 reports our results. First note that, in both models, the Kleibergen–Paap rk LM statistics lie below the rule of thumb threshold of 10, that is, our estimates might suffer from weak instrument bias (Stock et al., 2002). At the same time, the values of the Hansen *J* statistic indicate that we cannot reject the null hypothesis of exogenous instruments, which lends support to our assumptions about the validity of the instrument. In model 1 of Table 2 the estimated coefficient for total violence is negative and statistically significant below the 10% level. In model 2 of Table 2, in which we add the interaction term between minimum distance and elevation, statistical significance is below the 5% level. Note that the size of the coefficient for violence is larger than in the initial fixed effects models, suggesting that we were underestimating the magnitude of the effect of violence on trust. Both results confirm our initial findings and lend support to our main hypothesis. Even when leveraging exogenous variation in the total exposure to violence, we find a negative association between civil war violence and trust in the national government.

Our fourth and last robustness check aims at shedding some light on the divergence of results of our analysis and previous studies. While we find a clear negative effect of political violence on trust in the government, others have found a positive association. As argued above, we believe that timing may account for some of this seeming contradiction. We have used survey data collected in the immediate aftermath of a ceasefire, whereas previous studies have used data from surveys undertaken several years after the end of violence. We have argued above that such a time-gap between the end of a civil war and the date of the opinion survey may affect empirical findings, as processes of peace- and state-building can shape associations between exposure to violence and political attitudes.

Importantly though, while our study focuses on levels of trust in the immediate post-ceasefire period, we have so far not accounted for the timing of violence before the ceasefire. Some respondents might have been exposed to violence early on, while others only experience clashes between government forces and Maoist rebels right before the ceasefire. Victimization that happened long ago may produce different effects of violence on trust through a longer-term process of personal recounting of and coping with war-time experiences. Effects of violence might, however, be different when memories of violence are still fresh. Hence, there might be two important processes of timing at work in shaping the

Table 2. Trust in the national government, IV

	(1) 2SLS-FE	(2) 2SLS-FE, clustered SE
<i>log(Population)</i>	0.0913 (0.0801)	0.107 (0.0766)
<i>Income polarization</i>	1.979 (1.807)	1.969 (1.901)
<i>Poverty</i>	-0.240 (0.272)	-0.228 (0.282)
<i>Percentage Nepali speakers</i>	0.0426 (0.151)	0.0617 (0.164)
<i>Percentage higher caste</i>	-0.196 (0.202)	-0.208 (0.214)
<i>Age</i>	0.00314* (0.00140)	0.00315* (0.00141)
<i>Gender</i>	0.0244 (0.0307)	0.0244 (0.0309)
<i>Education</i>	-0.0710*** (0.0172)	-0.0705*** (0.0174)
<i>Voted</i>	0.0154 (0.0402)	0.0143 (0.0405)
<i>Piped water</i>	-0.0255 (0.0453)	-0.0268 (0.0461)
<i>Electricity</i>	-0.0902* (0.0357)	-0.0902* (0.0364)
<i>Total killings</i>	-0.0753 ⁺ (0.0416)	-0.0830* (0.0403)
Kleibergen–Paap rk LM statistic	3.41	3.67
Hansen <i>J</i> statistic	1.143	1.49
Observations	8198	8198
Adjusted <i>R</i> ²	-0.008	-0.012
<i>F</i>	12.12	11.89

Clustered standard errors in parentheses. 2SLS, Two-stage least squares.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

relationship between exposure to violence and political trust: the number of “peace years” and associated peace-building and the time lapsed since the last exposure to violence. While our study by design is able to identify effects of violence on trust without the influence of postwar reconstruction and peace-building efforts, we can only start to disentangle to potentially varying effects of the level of temporal closeness between exposure to violence and measurement of trust.

As a first step we make use of the high level of temporal disaggregation of the INSEC fatalities data. As the data report killings for each year separately, we are able to distinguish between the effects of early civil war violence—more than six years before the survey was undertaken—and the most recent exposure in the year preceding the survey. Table 3 reports the estimated coefficients and standard errors for our main model, but temporally disaggregates levels of violence.

We find evidence for temporal differences in the exposure to violence. Coefficients and errors vary substantially across the models. Models 1 and 6 in Table 3 indicate that

Table 3. Trust in the national government

	(1) FE-OLS, clustered SE	(2) FE-OLS, clustered SE	(3) FE-OLS, clustered SE	(4) FE-OLS, clustered SE	(5) FE-OLS, clustered SE	(6) FE-OLS, clustered SE	(7) FE-OLS, clustered SE
<i>log(Population)</i>	-0.0581** (0.0200)	-0.0604** (0.0200)	-0.0590** (0.0197)	-0.0641** (0.0193)	-0.0590* (0.0260)	-0.0342 (0.0222)	-0.0400 (0.0241)
<i>Income polarization</i>	1.871 (1.211)	2.022* (1.149)	1.914 (1.206)	1.987* (1.152)	1.987 (1.203)	1.932 (1.224)	1.792 (1.174)
<i>Poverty</i>	-0.362* (0.212)	-0.341 (0.219)	-0.364* (0.207)	-0.344 (0.214)	-0.348 (0.216)	-0.317 (0.215)	-0.330 (0.215)
<i>Percentage Nepali speakers</i>	-0.142 (0.138)	-0.143 (0.144)	-0.117 (0.134)	-0.169 (0.144)	-0.134 (0.137)	-0.120 (0.138)	-0.170 (0.156)
<i>Percentage higher caste</i>	-0.0846 (0.190)	-0.0947 (0.190)	-0.110 (0.188)	-0.0665 (0.198)	-0.0990 (0.188)	-0.103 (0.190)	-0.0526 (0.209)
<i>Age</i>	0.00303* (0.00139)	0.00302* (0.00141)	0.00305* (0.00140)	0.00304* (0.00140)	0.00303* (0.00140)	0.00305* (0.00140)	0.00304* (0.00139)
<i>Gender</i>	0.0214 (0.0299)	0.0212 (0.0300)	0.0217 (0.0299)	0.0217 (0.0299)	0.0216 (0.0299)	0.0217 (0.0300)	0.0212 (0.0299)
<i>Education</i>	-0.0776*** (0.0176)	-0.0780*** (0.0180)	-0.0778*** (0.0176)	-0.0779*** (0.0178)	-0.0777*** (0.0177)	-0.0771*** (0.0177)	-0.0779*** (0.0181)
<i>Voted</i>	0.0280 (0.0384)	0.0283 (0.0385)	0.0279 (0.0384)	0.0284 (0.0385)	0.0280 (0.0385)	0.0259 (0.0387)	0.0264 (0.0386)
<i>Piped water</i>	-0.0137 (0.0429)	-0.0137 (0.0429)	-0.0137 (0.0431)	-0.0122 (0.0424)	-0.0140 (0.0428)	-0.0149 (0.0426)	-0.0116 (0.0421)
<i>Electricity</i>	-0.0809* (0.0329)	-0.0799* (0.0326)	-0.0810* (0.0327)	-0.0799* (0.0328)	-0.0807* (0.0328)	-0.0797* (0.0330)	-0.0775* (0.0329)
<i>Killings 1997</i>	-0.179** (0.0549)						-0.193* (0.0858)
<i>Killings 1998</i>		0.0270 (0.0663)					0.0491 (0.0809)
<i>Killings 1999</i>			-0.0742 (0.0772)				-0.0656 (0.0873)

(continued)

Table 3. Continued

	(1) FE-OLS, clustered SE	(2) FE-OLS, clustered SE	(3) FE-OLS, clustered SE	(4) FE-OLS, clustered SE	(5) FE-OLS, clustered SE	(6) FE-OLS, clustered SE	(7) FE-OLS, clustered SE
Killings 2000				0.0368 (0.0317)			0.0408 (0.0336)
Killings 2001					-0.00108 (0.0231)		0.00727 (0.0291)
Killings 2002						-0.0204* (0.00958)	-0.0224+ (0.0132)
Constant	2.924*** (0.289)	2.915*** (0.288)	2.924*** (0.291)	2.953*** (0.287)	2.908*** (0.363)	2.702*** (0.301)	2.784*** (0.349)
Observations	8348	8348	8348	8348	8348	8348	8348
Adjusted R ²	0.017	0.017	0.017	0.017	0.017	0.018	0.018
F	35.39	13.26	11.88	11.76	11.96	12.84	68.83

Clustered standard errors in parentheses.

+ $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

especially the initial and—to a lesser extent—the most recent experiences matter most for levels of trust in the national government in the expected way. Killings in the years 1998–2001 are not found to have any statistically significant impact on political trust. Model 7 in Table 3 includes all violence variables simultaneously and confirms this pattern.⁸ We also estimate models 1–6 in Table 3 using our set of instruments. Doing so, we find even stronger evidence for the negative effects of violence at the beginning of the conflict, as well as the most recent year.⁹ While overall lending clear support for a negative association between exposure to violence and political trust in an immediate postwar setting, this set of findings also suggests a more nuanced role of violence for the formation of political attitudes. Future research will have to investigate further the temporal effects of exposure to violence and entertain more specific explanations for this pattern. For now, however, our final robustness check lends some support to our assumption that varying levels of temporal closeness of exposure to violence and measurement of trust alone cannot explain the difference between our findings and results of previous studies.

Conclusion

Civil wars can shape social and political perceptions. Our analysis of the effects of violence on political trust in Nepal speaks to previous findings on the role of victimization for social capital and cohesion: the more people are exposed to violence, the more probable are lower levels of trust in the government when the war ends. The more people experience the material and human costs of violence, the more negatively they will evaluate the government's failure to curb rebellion, as well as the consequences of violent government action itself.

These findings differ from results of previous studies finding positive associations between exposure to violence and trust. We believe that this seeming contradiction can at least partly be explained by the fact that research has thus far mainly concentrated on long-term effects of civil-war violence, while we have explicitly concentrated on the role of violence in trust in the immediate aftermath of a ceasefire. More specifically, we have argued that processes of peace- and state-building can affect associations between war-time experiences and postwar political attitudes and empirical attention to the immediate postwar context is warranted. Furthermore, our study explicitly focuses on trust in the national government, while many other studies instead analyze interpersonal or general measures of trust or social capital.

Future analyses will have to provide additional evidence on the effects of exposure to political violence. First, analyses based on multiple waves of opinion surveys may help shed light on the exact role of timing for associations between violence and trust. Our analysis relies on a specific type of violence, notably exposure to aggregate levels of killing. Future studies may perform comparable analyses on the effects of exposure to governmental or rebel violence. Second, our findings are based on analysis of one single case study. The civil war in Nepal may display some specific characteristics not found in other contexts. This can in turn reduce the possibility of generalizing our findings to other cases. Finally, our analysis is based on a survey item that asks for people's trust in national government. Operationalizations of political trust that differentiate between various state institutions or elements of trust such as confidence in policies or capacities may contribute to understanding associations between violence and political trust.

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Notes

1. Political trust is not the same as legitimacy. However, both concepts are intertwined. The less people trust in various political institutions, the higher the chance is of them perceiving the state to be illegitimate (Hutchison and Johnson, 2011; Newton, 2006; Sacks and Larizza, 2012). More specifically, political trust has been conceived as an antecedent normative condition of value-based legitimacy: the more people believe that the government is able and willing to deliver on its promises, the more likely they are to willingly obey the government's rules and regulations (Levi et al., 2009).
2. The exact formulation of the respective survey item reads as follows: "How much of the time do you think you can trust the NATIONAL government to do what is right? Every person will have a definition of what is right. For some it may be passing an abortion law, while for others it will be passing an anti-abortion one. Respondents may think about how the government deals with violence, corruption, drugs, crime, as well how permissive it is and how much it defends the interest of the citizens."
3. Violence in 1996 is actually taking place in VDCs not in our sample, owing to WHS respondents' locations.
4. Since these variables are measured after the first part of the civil war, they are post-treatment measures and considered "bad controls" (Angrist and Pischke, 2009, p. 66). Importantly though, none of our findings depend on the inclusion or exclusion of these measures.
5. Ideally, we would like to include additional measures of caste status at the individual level, but the WHS survey codebook does not let us identify high status castes from the numeric codes.
6. The estimated negative sign for the percentage of Nepali speakers and higher castes goes against expectations, but is far from standard levels of statistical significance, that is, not distinguishable from zero.
7. The Online Appendix provides maps for each year from 1997 to 2002, showing the geographic distribution of violence. Available at: <https://www.dropbox.com/s/7ggk0pf3139zj6h/NepalTrustAppendix.pdf>
8. The substantive effect size is similar. Although the coefficients differ dramatically, the difference in the level of killings between 1997 and 1997–2002 compensates proportionally.
9. Interestingly, in the 2SLS-IV models we also find a positive and statistically significant effect for violence in 2001, which coincides with initial attempts for a peace agreement.

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