

For Whom the Bell Tolls: The Political Legacy of China's Cultural Revolution

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Abstract

Does the use of repression undermine authoritarian rulers' legitimacy? I argue that state repression can make individuals internalize a strategy (trust or distrust) as a heuristic in situations where information acquisition is either costly or imperfect, so repression has a long-lasting negative effect on people's trust of authoritarian rulers. I test this proposition by studying one of the most tragic episodes of authoritarian repression in the modern era—state terror during China's Cultural Revolution. I show that individuals who were exposed to more state-sponsored violence in the late 1960s are less trusting of their political leaders at all levels today. Leveraging a variety of identification strategies, including an instrumental variable approach, I am able to make a stronger causal claim. I also explicitly test the norm-based mechanism and rule out an institutional mechanism. My findings contribute to our understanding of how repression affects the long-term legitimacy of authoritarian regimes.

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What is the long-term effect of state-sponsored violence on authoritarian rulers' legitimacy? Autocrats use repression to maintain political order, promote economic development, and lengthen their term in office (Haggard 1986; Davenport 1995; Bueno de Mesquita and Smith 2011; Svobik 2012; Bhasin and Gandhi 2013; Greitens 2016; Hassan 2016). Many autocracies remain robust facing societal challenges thanks to coercion (Bellin 2004; Levitsky and Way 2012). Scholars consider legitimation, repression, and co-optation to be the three pillars of stability in autocratic regimes (Gerschewski 2013; Dickson 2016b). But few empirical studies investigate how these strategies—legitimation and repression, in particular—affect each other.¹ Does repression *legitimize* authoritarian rule by preserving social order or *undermine* authoritarian rule by ruining citizens' trust?²

The impact of state repression on mass attitudes and behavior has received explicit attention recently, but most studies examine the short-term effect of repression in democracies (Anderson, Regan and Ostergard 2002; Davis 2007; Gibson 2008).³ A distinguished line of research starts to investigate the legacies of authoritarian repression on elections after authoritarian collapse. For example, Lupu and Peisakhin (Forthcoming) find that the descendants of individuals who suffered from the deportation of Crimean Tatars in 1944 more intensely identify with their ethnic group, support more strongly the Crimean Tatar political leadership, hold more hostile attitudes toward Russia, and participate more in politics. Similarly, Rozenas, Schutte and Zhukov (Forthcoming) show that Soviet state violence in western Ukraine has made affected communities less likely to vote for 'pro-Russian' parties. These studies examine the legacies of authoritarian repression *after* regime collapse; we still know little about whether repression legitimizes or delegitimizes authoritarian rulers *while they are still in power*.

Studying whether repression has a long-term effect in authoritarian regimes is worthwhile for

¹One notable exception is Wedeen (1999), who argues that Asad's propaganda and repression generated a politics of public dissimulation in which citizens act as if they revered their leader.

²In this article, I use repression, state repression, state-sponsored violence, and coercion interchangeably to indicate state use of violence against civilians.

³For a review of the recent literature, please see Davenport and Inman (2012).

two reasons. First, different from democracies that have frequent government turnovers, government change is less often in authoritarian regimes. In single-party regimes, one party rules for decades (Geddes 1999). In these regimes, people are more likely to associate their trust of the current leaders with their trust of previous leaders in the same party. Second, while democracies have a strong incentive to update voters on new leaders for electoral motivations, citizens in autocracies often have imperfect (insufficient or inaccurate) information about every new leader. With imperfect information, citizens' trust in new leaders are more likely to be influenced by traumatic events in the remote past. Studies in psychology and cultural anthropology demonstrate that past traumatic experience can make individuals *internalize* a strategy (trust or distrust) as a heuristic or rules of thumb in situations where information acquisition is either costly or imperfect (Tversky and Kahneman 1974; Boyd and Richerson 2005). Without full information about the new leadership, most citizens use heuristics or "information shortcuts" (Lupia 1994) to understand politics. These heuristics are also persistent, because over time a heuristic that works well for a citizen is more likely to be used again, while one that does not is more likely to be discarded (Axelrod 1986). I therefore expect that state-sponsored violence toward civilians has a long-lasting negative effect on the legitimacy of authoritarian rulers.

In this article, I study one of the most tragic episodes of authoritarian repression in the modern era: state terror during China's Cultural Revolution (1966-1976). Initiated by Mao Zedong in 1966, the Cultural Revolution caused 1.1 to 1.6 million deaths and subjected 22 to 30 million to some form of political persecution. The vast majority of casualties were due to *state repression*, not the actions of insurgents (Walder 2014, 534). This extraordinary toll of human suffering is greater than some of the modern era's worst incidents of politically induced mortality, such as the Soviet "Great Terror" of 1937-38, the Rwanda genocide of 1994, and the Indonesian coup and massacres of suspected communists in 1965-66.

The traumatic events during the Chinese Cultural Revolution present an unusual research opportunity. China has not experienced a regime change, so the same party has been in power since

1949. And the party has achieved economic success, which is believed to have promoted the regime's "performance legitimacy" (Zhao 2009). It is then interesting to examine whether state repression in the remote past still affects people's trust in current leaders and whether the party's recent success helps citizens forgive its past wrongdoings. The large variation in political violence at the local level provides enough heterogeneity, while many confounders, such as political institutions, can be held constant at the sub-national level. The findings from China also have the potential to be generalizable to other durable single-party regimes.

Analyzing a prefectural-level dataset of violence during the Cultural Revolution and a nationally representative survey conducted in 2008, I find that the Cultural Revolution has a persistent effect on political trust almost a half century later. Respondents who grew up in areas that experienced more violence during 1966-1971 are less trusting of their political leaders at all levels today. One more death per 1,000 people in 1966-1971 leads to, relative to the sample mean, 5.6% less trust in village/community leaders, 8.8% less trust in county/city leaders, 7.1% less trust in provincial leaders, and 7.1% less trust in central leaders. Exposure to violence has also influenced people across the board, regardless of whether they were victims, perpetrators, or bystanders. I am able to make a stronger causal claim, exploiting a variety of identification strategies, including an instrumental variable (IV) approach.

My preferred causal mechanism is that the individuals who were exposed to state-sponsored violence during the Cultural Revolution have internalized norms of distrust under imperfect information about new leaders, and the norms of distrust have been handed down from generation to generation. I explicitly test this mechanism by showing that 1) the effects of the Cultural Revolution also exist among the younger generation who were born after the Cultural Revolution; and 2) individuals who have frequent access to political news are able to update their evaluations on current leaders and, therefore, are less influenced by the Cultural Revolution. I also examine an alternative institutional mechanism that the chaos during the Cultural Revolution might have resulted in a long-term deterioration of political institutions that adversely affected political trust. To

rule out this possibility, I restrict my sample to respondents who moved to their current localities as adults. If weakened political institutions are a mechanism, the effect of violence (in their current localities) should exist among these new residents who were exposed to the institutions but not to the violence (they were exposed to somewhere else's violence). I, however, find that levels of violence have no effect on new residents' political trust.

My findings contribute to the literature on authoritarian politics. While previous literature focuses on how authoritarian rulers combine different strategies to stay in power (Magaloni and Kricheli 2010; Svolik 2012; Gerschewski 2013; Dickson 2016b), I show that these strategies undermine each other. A coercive regime might promote economic development and maintain political stability (Haggard 1986; Bellin 2004); the use of coercion, however, can delegitimize authoritarian rule by ruining citizens' trust in the leaders (who are not even the repressors).

This study also relates to the literature on the consequences of violence. Most empirical work on violence focuses on war's short-term effects on political attitudes and behavior, and the recent literature is inconclusive as to whether violence's effect is positive or negative. While some studies find a positive effect on political participation (Blattman 2009; Bellows and Miguel 2009; Gilligan, Pasquale and Samii 2014), others find a negative effect on political attitudes (Beber, Roessler and Scacco 2014; Grossman, Manekin and Miodownik 2015; De Juan and Pierskalla 2016). But violence during interstate or civil wars is different from state-sponsored violence. As Besley and Persson (2011) point out, a key feature of civil war is *two-sided* violence between an insurgent and the government, while many citizens suffer consequences of *one-sided* political violence, due to government repression. Amid two-sided violence, both sides could be activated by mutual aggression (Blattman 2009), whereas during one-sided state repression, the citizens are often passively suppressed by the repressor. My article is the first of its kind to quantitatively examine the long-term effect of state-sponsored violence on trust in authoritarian leaders.

There is also a distinguished line of research on political trust in China. A consensus from survey research is that Chinese citizens exhibit a high level of trust in the national government,

which many scholars attribute to the high economic growth in the past 40 years, policy successes, the media, or Chinese traditional culture (Shi 2001; Chen and Shi 2001; Chen 2004; Chen and Dickson 2010; Tang 2005, 2016; Wang 2005; Yang and Tang 2010; Stockmann and Gallagher 2011; Lü 2014; Lewis-Beck, Tang and Martini 2014; Dickson 2016a). Other studies have shown a low level of trust in local government (Li 2004), due to its ineffectiveness in implementing central policies (O'Brien and Li 2006) and various examples of government misconduct at the local level (Cui et al. 2015).⁴ The high level of trust in the central government and the low level of trust in local government constitute what Li (2016) terms “hierarchical trust,” which Lü (2014) associates with the decentralized political system and biased media reporting that he argues induce citizens to credit the central government for good policy outcomes. Although my study does not speak to “hierarchical trust” directly, my focus on a historical event distinguishes this study from the extant literature’s focus on proximate causes.

Background and Theory

In this section, I will introduce the Cultural Revolution’s historical background with a focus on the causes and patterns of violence. I will then discuss the conceptual framework that helps us understand how state repression affects people’s long-term political attitudes.

Historical Background

The Cultural Revolution, as MacFarquhar and Schoenhals (2006, 1) contend, was a “watershed” in Chinese modern history and “the defining decade of half a century of Communist rule in China.” It started in 1966 and ended with Mao’s death in 1976. Many scholars believe that the origins of the Cultural Revolution should be understood through the lens of Mao’s personal goals to change the succession and to discipline the huge bureaucracies (MacFarquhar 1997; Lieberthal 2004).

⁴A notable exception is Manion (2006), who shows that democratic village elections have increased citizens’ trust in local authorities.

The early and most chaotic period was from 1966 to 1969. In August 1966, Mao encouraged urban middle school and college students to form Red Guard groups to attack “class enemies” and the party. Millions of teenagers whose schools were closed formed Red Guard groups based on their class backgrounds, geographic locations, and personal ties and quickly launched a reign of terror in most cities (Chan, Rosen and Unger 1980).

Because of the Red Guards’ visibility and large number, earlier works on the Cultural Revolution often explain the violence as a result of group conflict. Many scholars describe this period in the language of mass insurgencies in which various groups organized to press their interests and make demands against party authorities (Lee 1978; Chan, Rosen and Unger 1980). As some recent studies show, however, the public officials themselves were a major player in causing the chaos and violence as they were in widespread rebellion against their superiors (Walder 2009, 2016). For example, beginning in January 1967, lower-ranked officials, with the help of the People’s Liberation Army (PLA), Red Guard groups, and urban workers, started to seize power by sweeping aside party and government leaders to form “revolutionary committees (革委会)” in various cities to exercise authority (Walder 2016).

In May 1968, Mao started the “Cleansing the Class Ranks Campaign (清理阶级队伍运动)” and made new revolutionary committees carry it out. The campaign was “a purge designed to eliminate any and all real and imagined enemies” and “provided whoever happened to be in power with an opportunity to get rid of opponents” (MacFarquhar and Schoenhals 2006, 253). Although it originally had a well-defined target, that target became blurred and the process became uncertain. As MacFarquhar and Schoenhals (2006, 256) observe, “Local officials invariably broadened its scope and used it as an excuse to intensify the level of organized violence in general.”

According to Walder’s (2014) estimate based on local gazetteers, of the deaths that can be linked to specific events during 1966-1971, the vast majority were due to the actions of authorities, and most deaths were caused by the “Cleansing the Class Ranks Campaign,” conducted by revolutionary committees.

The violence during the Cultural Revolution started to fade after 1969 when Mao ordered the PLA to reestablish order and to send the Red Guards to remote rural areas. In 1971, after the death of the radical military leader Lin Biao, China started to recover from the political chaos and economic stagnation. The Cultural Revolution completely ended in 1976 when Mao died and the movement's radical leaders—the “Gang of Four”—were arrested.

Conceptual Framework

State repression “involves the actual or threatened use of physical sanctions against an individual or organization, within the territorial jurisdiction of the state, for the purpose of imposing a cost on the target as well as deterring specific activities and/or beliefs perceived to be challenging to government personnel, practices or institutions” (Goldstein 1978, xxvii). Political trust, on the other hand, is a belief that political leaders will not harm citizens. But political leaders' repressive actions are uncertain, especially in authoritarian regimes. Citizens must assess the probability that leaders will act in certain ways, but they usually lack full information.

According to Boyd and Richerson (2005), when information acquisition is either costly or imperfect, it can be optimal for individuals to develop heuristics or rules of thumb to inform their decision making. By believing what is the “right” thing to do in different situations, individuals save the costs of obtaining the information necessary to always behave optimally. Similarly, Tversky and Kahneman (1974) argue that people rely on a limited number of heuristic principles, which reduces the complex task of assessing probabilities. One principle is *availability*: people evaluate the probability of an event according to the ease with which instances or occurrences can be brought to mind. For example, one may assess the risk of heart attack among middle-aged people by recalling such occurrence among one's acquaintances. Tversky and Kahneman (1974, 1127) further show that the more retrievable and more salient the instances are, the more likely they will be brought to mind. For instance, the impact of seeing a house burning is greater than the effect of reading about a fire in the local paper.

State repression that causes the deaths of family members, friends, neighbors, and other acquaintances is a highly salient event that is similar to “seeing a house burning,” which can put a deep mark on one’s memory. These events can be easily retrieved when one needs to assess what political leaders will do. Experiencing these traumatic events, communities would develop norms of distrust that they use as rules of thumb to understand their relationship with the authorities. And norms of distrust work well for individuals in conflict-prone communities, because they prepare individuals to think how much worse the leaders could be. Over time, these norms will persist because different behavioral rules evolve through a process of natural selection determined by the relative payoffs of different strategies (Axelrod 1986).

I hypothesize that communities that were exposed to more violence during the Cultural Revolution developed a norm of distrust toward the political authorities. I expect this norm to persist even after a change in leadership, because obtaining information about every new leader is costly. A national survey in China shows that over a quarter of the respondents in 2008 cannot correctly name the General Secretary of the Chinese Communist Party (CCP), and that almost 40% cannot correctly name the Prime Minister.⁵ So although it is not optimal to rely on rules of thumb developed in the 1960s to understand leaders in the 2000s, individuals can save the costs of seeking the information necessary to always behave optimally. And because the Cultural Revolution was initiated by a central leader (Mao), sustained by radical leaders in the national government (e.g., Lin Biao and the “Gang of Four”), and carried out by local leaders, its negative effect should be reflected in people’s trust in leaders at all levels.

Some recent studies show that violence affects the victims as well as the perpetrators. Grossman, Manekin and Miodownik (2015), examining the political attitudes of Israeli ex-combatants, argue that combat involves conflictual and threatening intergroup contact, which is associated with prejudice, ethnocentrism, and hostility. Qualitative evidence based on oral histories shows that the perpetrators during the Cultural Revolution have feelings of guilt and regret, and have consequently

⁵The survey was conducted by the Research Center for Contemporary China at Peking University, and the data is publicly available at Peking University’s dataverse. Please see <https://goo.gl/jiKF7J> (Accessed February 13, 2017).

changed their political attitudes (Feng 1996; Dong 2009). I hence expect the negative impact of violence to be significant regardless of an individual's standing during the Cultural Revolution.

I also expect that the norm of distrust will be handed down over generations, generating downstream effects of the violence. In the literature on the transmission of norms, several studies show that the family is the primary locus of norms transmission, and that parents act consciously to socialize their children to particular cultural traits (Bisin and Verdier 2001; Jennings and Niemi 2014). For instance, Lupu and Peisakhin (Forthcoming) argue that victims of violence transmit norms to their offspring. Using a multigenerational survey, they show that political attitudes are correlated across generations within each family. As a result, cultural behavior proves remarkably persistent and tends to change slowly (Inglehart 1997).

An alternative mechanism is that the violence during the 1960s caused a long-term deterioration of political institutions, which in turn leads to mistrust. Tabellini (2008) argues that there are strategic complementarities between culture and institutions. Putnam (1993, 121-162) shows that the legacies of self-government in northern Italy has a long-term effect on political and social trust. This institutional mechanism would imply that the governments that experienced more violent factional fights during the Cultural Revolution might have retained more radicals as bureaucrats, and many rules and norms have been destroyed. However, Deng Xiaoping's personnel reform in the 1980s ruled out this scenario. After Deng took power, he gradually pushed for the retirement and exit of many of the Cultural Revolution radicals and replaced them with young and professional bureaucrats, so the government personnel have been almost completely reshuffled since the Cultural Revolution (Manion 1993). By examining a subset of the sample who moved to their current localities as adults (and therefore are exposed to the institutions), I show that Cultural Revolution violence has no effect on these new residents' political trust.

Empirics

In this section, I will introduce the dataset and present the main empirical results, which are robust to a range of checks. Using several identification strategies, I am able to make a stronger causal claim by showing that omitted variable bias is not substantial. In addition, I will show evidence to support my proposed mechanisms that norms are handed down over generations, and citizens without updated information about new leaders are more subject to the influence of past state repression. Last, I will rule out the alternative mechanism that the violence caused distrust by weakening political institutions.

Data

I use a dataset compiled by [Walder \(2014\)](#) to measure local variation in violence during the Cultural Revolution. Based on local annals published in the reform era, [Walder \(2014\)](#) codes the number of deaths from June 1966 to December 1971 for 2,213 jurisdictions (prefectures, cities, and counties). [Walder \(2014\)](#) hires teams of trained coders (double-coding) to read the annals and record the number of “unnatural deaths” during this period. The 2,213 jurisdictions include 89 county-level cities and 2,040 out of the 2,050 counties that existed in 1966, so the coverage is comprehensive. [Walder \(2014\)](#) reconciles boundary changes by examining materials in the annals and tracing the history of boundary changes in the national register of jurisdictions, so the 1966 administrative units can be merged with current units.

There are two potential measurement errors with the dataset. First, rules for counting deaths were conservative: local governments might have an incentive to underreport the number of casualties to cover up the dark history. This error, however, will only create a downward bias for my estimates and make me less likely to find the results. I will also use an IV approach to deal with the potential measurement error and show that my results are similar. Second, the annals’ publication was coordinated at the provincial level, so the between-province variation in deaths is accounted for not only by the actual death tolls but also by the format and reporting efforts of local

annals. In all of my analyses I hence control for provincial fixed effects, so any estimate is the *within-province* effect of violence. I also control for the number of words that each annal devoted to the Cultural Revolution in all analyses to account for the variation in reporting efforts.

The key independent variable is *Number of Deaths/1,000* measured at the prefectural level. As [Walder \(2014\)](#) shows, the vast majority of the deaths are due to state repression. One might instead argue that lots of deaths were caused by intra-community violence, such as children betraying parents, and even spouses denouncing and attacking each other, and so forth. But as [Walder \(2014\)](#) shows, very few deaths were caused by citizens attacking each other. To test this, I regress people's trust in family and relatives on *Number of Deaths/1,000* and find a significantly positive effect, suggesting that people who lived in violent localities relied on their family for support and rescue during the chaos (Table 2.2 in the Web Appendix).

I aggregate the number of deaths at the county level to the prefectural level, because data on some of the covariates are available only at the prefectural level. But I will show in the robustness checks that using county-level data will yield the same results. The resulting dataset includes 277 prefectures (94.5% of all prefectures), and most of the missing prefectures are in Tibet, where local annals were less systematically published. *Number of Deaths/1,000* ranges from zero to 22.57 with a mean of 0.51.⁶ Figure 1 shows the regional distribution of violence during 1966-1971. A glance shows that the violence was concentrated in the Northwest, such as Inner Mongolia and Shaanxi, and the Southwest, such as Guangxi. But these patterns can not be overgeneralized because the between-province variation is largely caused by different reporting rules in local annals. We should instead pay attention to the within-province variation, which is the focus of the empirical analysis. The map also shows spatial clustering of violence, indicating a possible spatial spillover effect of violence (neighboring cities' violence affects trust). I will later show that my results are the same when including a spatial lag to account for the spatial effect.

[INSERT FIGURE 1 HERE]

⁶The sample distribution, which includes only surveyed prefectures, is different, ranging from 0 to 5.02 with a mean of 0.57.

To measure political trust, I use data from the China Survey—a national probability sample survey that was designed by a group of leading survey researchers, coordinated by Texas A&M University, and implemented by the Research Center for Contemporary China (RCCC) at Peking University in 2008. The China Survey used a spatial sampling technique (Landry and Shen 2005) to randomly draw a sample of 3,989 adults across China’s 59 prefectures.⁷ The survey asked four questions about people’s trust in political leaders at various levels: 1) How much do you trust village/community leaders? 2) How much do you trust county/city leaders? 3) How much do you trust provincial leaders? and 4) How much do you trust central leaders? The respondents choose between four possible answers: do not trust at all, do not trust very much, trust somewhat, and trust very much. Table 1.1 in the Web Appendix reports the distribution of responses for each question. Consistent with previous works that show a hierarchy of trust (Li 2004, 2016; Lü 2014), respondents exhibit more distrust in leaders at lower levels: the proportions of respondents who chose “do not trust at all” or “do not trust very much” are 35.02% for village/community leaders, 34.83% for city/county leaders, 24.38% for provincial leaders, and 13.91% for central leaders. Since there are some missing values created by “don’t know” or item non-response, I use listwise deletion in the main analyses and will use multiple imputation in the robustness checks to show that the results are similar.

The main dependent variables are the levels of trust in leaders at various levels by assigning the value of 1, 2, 3, or 4 to “do not trust at all,” “do not trust very much,” “trust somewhat,” and “trust very much,” respectively. An alternative strategy is to maintain the ordinal nature of the answers and instead estimate an ordered logit model. As I show in the robustness checks, the estimates are similar.

Chinese respondents might over-report their political trust due to preference falsification (Kuran 1991; Jiang and Yang 2016). But a recent experimental study finds that political desirability bias is very small among Chinese respondents (Tang 2016, 134-151). I argue that political fear

⁷For more information about the China Survey, please see Section I in the Web Appendix and <http://thechinasurvey.tamu.edu> (Accessed April 29, 2016).

should not contaminate my findings for two reasons. First, the survey was conducted by a university rather than a governmental organization. RCCC, “the most competent academic survey research agency on the mainland” (Manion 2010, 190), took several measures to ease respondents’ concerns about political sensitivity. For example, in the preface of the survey that was read to every respondent prior to its administration, respondents were guaranteed the confidentiality of their identifying information, including their names, addresses, and contact information. In addition, every respondent was informed of the right to skip a question if he or she did not feel like answering it. Second, over-reporting one’s political trust due to political fear will only make me less likely to find a negative effect of violence on political trust, because people who were exposed to more political violence, and hence are more fearful of government repression, are more likely to over-report trust. Nevertheless, I still find these people to be less trusting of political leaders.

I also consider several covariates. At the individual level, I code *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father’s Education*, and *Mother’s Education*. The China Survey also asked respondents their families’ *Class* backgrounds, as defined by the Chinese government in the early 1950s. Following Deng and Treiman (1997), I code *Good Class* to include hired peasants, poor peasants, lower-middle peasants, urban poor, and workers, *Middle Class* to include middle peasants, upper-middle peasants, clerks, and petty merchants, and *Bad Class* to include rich peasants, landlords, and capitalists.

At the prefectural level, I collect data on pre-Cultural Revolution socioeconomic indicators, including *Male-to-Female Ratio* and *Urban Population Percentage* from the 1964 Census to indicate demographic structure and urbanization, *Longitude* and *Latitude* to consider geography, *Natural Resource* (any of the following: oil field, gas field, coal mine, or ore reserve)⁸, *Colony* (ceded territories in Qing) for colonial legacy, *Suitability for Wetland Rice* for cropping patterns, *Distance to Beijing* to proxy for the geographic reach of the national government, and *Length of Rivers* to

⁸Geo-spatial data for oilfields, gas fields, coal mines, and ore deposits are incorporated from the database of the United States Geological Survey (Karlsen et al. 2001).

consider water transportation.⁹

In every regression, I include *Account Length (log)* in the local annals to control for reporting efforts, and provincial fixed effects to account for provincial-level differences in annal reporting and unobservable historical, cultural, and leadership factors, so the estimates reflect the within-province effects of Cultural Revolution violence on political trust. Table 2.1 in the Web Appendix presents all variables' sources and summary statistics.

Results

I hypothesize that exposure to violence during the Cultural Revolution will decrease people's current trust in political leaders at all levels, holding everything else constant. Because my theory focuses on people's exposure to the Cultural Revolution, I only include the subset (61.64%) of the sample that *grew up in the localities* where they took the survey and exclude those who moved to their current area after they turned 18. In my main analysis, I use ordinary least squares (OLS) to fit Equation (1) to a cross-section data file that mixes prefectural- and individual-level variables, and cluster standard errors at the prefectural level to avoid overstating the precision of my estimation. I also use hierarchical linear modeling (HLM) as an alternative estimation strategy in the robustness checks, and show that the results are similar. My baseline estimation equation is:

$$Political\ Trust_{ij} = \alpha_{ij} + \beta Number\ of\ Deaths/1,000_j + \mathbf{X}_{ij}'\mathbf{\Gamma} + \mathbf{X}_j'\mathbf{\Omega} + \epsilon_{ij}, \quad (1)$$

where i indexes individuals and j prefectures. $Political\ Trust_{ij}$ denotes one of the four measures of trust in political leaders at the individual level. $Number\ of\ Deaths/1,000_j$ is a measure of the number of "unnatural deaths" per 1,000 inhabitants during 1966-1971 at the prefectural level. β is the quantity of interest measuring the effect of violence on political trust. The vector \mathbf{X}_{ij} denotes a set of individual-level covariates, including *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father's Education*,

⁹The dataset on rivers is from the China Historical GIS Project that compiles river data in the late Qing period.

Mother's Education, *Good Class*, and *Middle Class* (leaving *Bad Class* as a reference group). And the vector X_j denotes a set of prefectural-level covariates, including *Male-to-Female Ratio (1964)*, *Urban Population Percentage (1964)*, *Longitude*, *Latitude*, *Natural Resource*, *Colony*, *Suitability for Wetland Rice*, *Distance to Beijing*, and *Length of Rivers*. Every regression also controls for *Account Length (log)* and provincial fixed effects.

[INSERT TABLE 1 HERE]

To avoid any biases introduced by covariates, I will first exclude all prefectural- and individual-level covariates and add them later. Table 1 reports my baseline estimates of the effects of Cultural Revolution violence on political trust in village/community leaders (Column (1)), county/city leaders (Column (3)), provincial leaders (Column (5)), and central leaders (Column (7)) with only *Account Length (log)* and provincial fixed effects. The remaining columns show the estimates with both individual and prefectural controls. To focus on my main quantity of interest, I omit the coefficients of these controls in Table 1 but present the full results in Table 3.1 in the Web Appendix.

Exposure to violence during the Cultural Revolution has a consistently negative effect on current trust in political leaders at all levels. And adding prefectural- and individual-level covariates makes the estimates stronger, indicating that post-treatment bias is not substantial. Considering the estimates with controls, relative to the sample mean, one more death per 1,000 people in 1966-1971 leads to 5.6% less trust in village/community leaders, 8.8% less trust in county/city leaders, 7.1% less trust in provincial leaders, and 7.1% less trust in central leaders.

Violence also negatively affects political trust regardless of whether someone was a victim, perpetrator, or bystander. Using a respondent's family class background as a proxy for his or her standing during the Cultural Revolution, Table 2 includes an interaction term between *Number of Deaths/1,000* and *Class* (higher values indicate "bad" class).¹⁰ If victims from "bad" class backgrounds were more susceptible to violence during the Cultural Revolution, then the coefficient on

¹⁰Table 3.2 in the Web Appendix presents the full results.

the interaction term should be significantly negative. But none of the interaction terms is significant, and three out of four estimates are positive. The magnitude of the effects is also small. This indicates that everyone becomes a *de facto* victim regardless of whether he or she was personally subjected to violence.

[INSERT TABLE 2 HERE]

I conduct nine robustness checks: I exclude the people who were sent down to the countryside (and were thus not only affected by the violence in their hometowns), run the analyses using a county-level dataset, employ HLM, use ordered logit rather than OLS, drop one prefecture at a time, use multiple imputation to deal with missing values, transform the independent variable into a dichotomous variable ($Number\ of\ Deaths/1,000 > 0$) or a natural log-transformed variable ($\log((Number\ of\ Deaths/1,000)+1)$) to tackle its skewness, consider survey design effects, and include a spatial lag to consider the spatial spillover effect of violence. None of these checks change my original results (Section IV in the Web Appendix).

Identification Strategies

Although I have established a strong, negative correlation between exposure to violence and political trust, the relationship is not causal. The biggest challenge for identification is omitted variable bias—i.e., that some factors before the Cultural Revolution affected both the violence and the decline in political trust. And because the data is historical, there might be measurement errors. In the following analyses, I use three approaches to show that omitted variables and measurement errors are unlikely to bias my estimates.

The first strategy is to control for an extra set of observable characteristics of prefectures that may be correlated with Cultural Revolution violence and subsequent trust. In the analysis omitted here but presented in the Web Appendix (Table 5.1), I control for the frequency of mass rebellions in the Qing Dynasty (1644-1911) as a proxy for historical state-society relations, per capita

GDP (provincial level) during 1956-1966, population density in 1964, the number of natural disasters during 1956-1966, Great Famine severity, and Communist Party density. Adding these extra controls not only fails to change the original results, but makes the results stronger.

My second strategy is to calculate the “Altonji ratio” to use selection on observables to assess the potential bias from unobservables (Altonji, Elder and Taber 2008). This ratio measures how much stronger selection on unobservables, relative to selection on observables, must be to explain away the full estimated effect. All of the Altonji ratios are negative, indicating that selection on unobservables should not be a concern because adding more covariates only makes the effect stronger (Table 5.2 in the Web Appendix).

My last strategy is to use an IV. An ideal instrument should be a strong, exogenous predictor of *Number of Deaths/1,000*. To meet the exclusion restriction, the instrument should also affect political trust only through its effect on Cultural Revolution violence. I must find an exogenous variable (specific to the Cultural Revolution) that affected violence. I use the average distance between a prefecture’s center and the nearest sulfur mines as the instrument for Cultural Revolution violence. Below I will demonstrate that this measure is a strong and exogenous predictor of *Number of Deaths/1,000*, and that it affects contemporary political trust only through its effect on Cultural Revolution violence.

The rationale for the instrument is based on the qualitative evidence that, in the early stages of the Cultural Revolution, the PLA dispatched troops to guard important military installments, especially arsenals (兵工厂), to prevent civilians from seizing weapons. The PLA must maintain security and some semblance of law and order to insulate these localities near arsenals from the factional fights. In January 1967, the Military Affairs Commission—the highest military leadership—issued an order endorsed by Mao, and “its general thrust was in the direction of imposing law and order. It explicitly forbade all attempts to ‘assault’ key military installations” (MacFarquhar and Schoenhals 2006, 176). In addition, as dictated by a Central Document (中发[1967] 288) issued on 5 September 1967, “All of People’s Liberation Army’s weapons, equipment, and supplies must

not be seized. People's Liberation Army's buildings are forbidden to be entered. All proletarian revolutionaries, all revolutionary Red Guards, all the revolutionary masses, and all patriotic people must strictly adhere to this order." Mao issued these orders to insulate "the PLA from the disruption among the civilian population; after all, the PLA was his institutional base too" (MacFarquhar and Schoenhals 2006, 177).

We should therefore expect the prefectures with PLA arsenals to experience fewer deaths. Although the locations of PLA arsenals are classified, it is intuitive to imagine that the PLA located the arsenals close to raw materials. To make gunpowder—an important component of all ammunition—three ingredients are needed: saltpeter, charcoal, and sulfur. Saltpeter and charcoal can be manufactured; sulfur must be extracted from natural minerals. Sulfur primarily exists in three forms: native sulfur, iron sulfide associated minerals, and iron sulfide. I calculate *Average Distance to Sulfur Mines (log)*, which is the natural log transformed average distance (km) from a prefecture's center to its nearest native sulfur mine, iron sulfide associated minerals mine, and iron sulfide mine, and use it as an instrument.¹¹ Although I do not have systematic data on the locations of PLA arsenals to offer direct evidence, there is qualitative evidence confirming that arsenals were located near sulfur mines. For example, according to the Liaoning Provincial Gazetteer, Fengtian (奉天) Arsenal (which later became Northeastern Arsenal), the largest ammunition factory in northeast China, was located close to iron sulfide mines to take advantage of low transportation costs (Liaoning 1999). Peng Dehuai, one of the founders of the PLA, explicitly suggested to Mao Zedong in 1939 that the PLA should take advantage of the rich reservoir of sulfur in Southeast Shanxi to establish arsenals.¹² The Huangyadong (黄崖洞) Arsenal, the biggest PLA arsenal during the war era, was later established in Changzhi County in Shanxi Province. Figure 5.1 in the Web Appendix shows the geographic locations of sulfur mines.

Average Distance to Sulfur Mines (log) has strong first-stage qualities. As shown in Figure 5.2

¹¹The locations of sulfur mines are from <http://goo.gl/3aLwtX> (Accessed May 3, 2016) and the distances are calculated using QGIS.

¹²Please see <http://goo.gl/rkuugA> (Accessed September 7, 2016).

in the Web Appendix, *Average Distance to Sulfur Mines (log)* is a strong and positive predictor of *Number of Deaths/1,000*. In addition, as the bottom panel in Table 3 confirms, *Average Distance to Sulfur Mines (log)* is a strong instrument: the first stage yields large F statistics ranging from 16.68 to 19.81, which exceeds the standard critical value of 10 required to avoid weak instrument bias.

[INSERT TABLE 3 HERE]

To satisfy the exclusion restriction, *Average Distance to Sulfur Mines (log)* should affect current political trust only through its effect on Cultural Revolution violence. Because seizing weapons from the PLA was a phenomenon that was specific to the Cultural Revolution and no longer occurs, we should not expect the locations of arsenals to influence current political trust through other channels. One possible violation is that the localities that are near arsenals are subject to more contemporary repression given their proximity to weapons. This is unlikely, given the development of modern transportation that distributes the access to weapons. To test this, I collect data on the number of public security organizations from Baidu's points of interest database to proxy for government repressiveness and test whether the prefectures that are closer to sulfur mines have more public security organizations per capita or per square kilometer. As Table 5.3 in the Web Appendix shows, the effect of *Average Distance to Sulfur Mines (log)* on the density of public security is indistinguishable from zero. Another potential violation of the exclusion restriction is that the localities that are close to arsenals have experienced a different development path focusing on certain industries, and therefore have a lower or higher quality of government. To test this possibility, I collect data on a range of variables that measure the quality of government across prefectures, including per capita GDP, perceived corruption, experienced corruption, bureaucratic efficiency, prevalence of bribery, quality of legal institutions, size of government, and average schooling, from a variety of data sources, such as the China Survey, the World Bank's World Business Environment Survey, and government statistics. Table 5.4 in the Web Appendix provides details about these measures and the estimates. As shown, prefectures that are closer to sulfur

mines do not differ from other prefectures on any of these quality-of-government measures.

The top panel in Table 3 shows the second-stage results.¹³ *Number of Deaths/1,000*, after instrumented by *Average Distance to Sulfur Mines (log)*, has a significantly negative effect on political trust at all levels, and the magnitudes of the IV estimates are remarkably similar to the OLS estimates (Table 5.1 in the Web Appendix). In fact, in all specifications, the Durbin-Wu-Hauman test cannot reject the null hypothesis of the consistency of the OLS estimates at the 0.1 level. These results suggest that selection on unobservables is not strongly biasing the OLS estimates. This is consistent with the findings from previous tests.

In sum, although I cannot (and will not) randomize violence to Chinese localities, a variety of tests show that omitted variables are unlikely to bias my estimates.

Causal Mechanisms

So far, my analysis has been based on the assumed mechanism that exposure to violence has made people internalize their distrust in political leaders, which persists until today because 1) parents transmit the norms of distrust to their children, and 2) the costs of obtaining every new leader's information are high. Below, I will explicitly test these two propositions.

To examine inter-generational transmission of norms, I divide the sample into three generations using 1949 (founding year of the People's Republic) and 1976 (last year of the Cultural Revolution) as cutoff points. For simplicity, I call the respondents who were born between 1916 and 1949 "grandparents," respondents born between 1950 and 1976 "parents," and those born between 1977 and 1990 "children." The "grandparents" and "parents" directly experienced the Cultural Revolution, while the "children" are expected to be influenced by the older generation. Figure 2 plots the coefficients (and their confidence intervals) of *Number of Deaths/1,000* across the three generations. As is shown, violence decreases trust for all three generations. The negative effects of Cultural Revolution violence are significant at the 90% level for both the "parents" and the "chil-

¹³Table 5.5 in the Web Appendix shows the full results.

dren,” suggesting that violence has a direct effect on people who experienced it and an indirect effect on the younger generation who heard about it from the older generation.

[INSERT FIGURE 2 HERE]

As another testable implication, I hope to show that informed individuals who overcome the costs should update their evaluations of the new leaders and hence be less susceptible to past events. To test this implication, I examine individuals’ access to political news using the number of days per week they watch political news on TV (*Days Watching TV News*), drawing on data from the China Survey. I then interact *Days Watching TV News* with *Number of Deaths/1,000* and expect the marginal effect of Cultural Revolution violence on political trust to be smaller for people who have more access to political news. Because how informed a respondent is about politics is endogenous to his or her exposure to the Cultural Revolution, the interpretation of the estimates should proceed with caution.

Figure 3 presents the marginal effects of *Number of Deaths/1,000* on measures of political trust at different levels of political information. As shown, the effect of violence is strongest for individuals who watch little political news, but becomes smaller and even insignificant when people increase their news consumption. Especially for trust in local leaders, if they watch political news more than three or four days a week, their levels of trust in village/community and county/city leaders are no longer influenced by their exposure to Cultural Revolution violence. But they need more updated information to dilute violence’s impact on trust in higher-level leaders. This is reasonable, because people have more opportunities to interact with their local leaders than with higher-level leaders, so it is more difficult to change their heuristic about higher-level leaders.¹⁴

[INSERT FIGURE 3 HERE]

¹⁴Before estimating the multiplicative interaction models, I conduct diagnostics recommended by Hainmueller, Mummolo and Xu (2016) to ascertain that my data meets the linear interaction effect assumption and has sufficient common support. For these diagnostics, please see Section VI in the Web Appendix. Table 6.2 in the Web Appendix shows the full results of the multiplicative interaction models.

In addition to the norm-based mechanism, an alternative institutional mechanism might be at work: the chaos and violence during the Cultural Revolution have caused a long-term deterioration of political institutions. As I discuss earlier, Deng Xiaoping's personnel reform in the 1980s ruled out this scenario (Manion 1993). To empirically test this alternative mechanism, I focus on people who moved to their current localities as adults. The locals who grew up in the current localities were exposed to both the violence and the institutions, so it is impossible to distinguish between these two mechanisms by examining them. The new residents, however, are exposed to the institutions but not the violence. To see this, imagine Mr. Zhang moved from prefecture A to prefecture B when he was 20. Since Mr. Zhang's family is not from prefecture B, he should not be affected by B's violence during the Cultural Revolution, *if violence affects people through norms*. By contrast, since Mr. Zhang is now exposed to B's institutions, he should be affected by B's violence during the Cultural Revolution, *if violence affects people through institutions*.

[INSERT TABLE 4 HERE]

As Table 4 shows, violence during the Cultural Revolution has an insignificant effect on new residents' political trust, and the point estimates of these effects are close to zero.¹⁵ This is strong evidence against the institutional mechanism.

Conclusion

Political trust has important political consequences: it enables government to function by making citizens comply with government demands and regulations (Levi 1997; Tyler 1990); it generates the interpersonal trust that promotes a productive economy, a more peaceful and cooperative society, and a democratic government (Putnam 1993); and it encourages citizens to participate in conventional political activities, such as voting, while distrust spurs participation in unconventional activities, such as protest (Tarrow 2000).

¹⁵Table 6.3 in the Web Appendix shows the full results.

There is a popular argument that authoritarian rulers can erase the adverse effect of repression and gain public support through successful policies. For example, many believe that the successful economic reform in post-Mao China has gained legitimacy for the CCP, even though many of Mao's policies were disastrous. [Yang and Zhao \(2015, 64-65\)](#), for instance, argue that Chinese leaders' public support lies in "the state's capacity to make a policy shift" to avoid "making the disastrous mistakes that the Chinese state repeatedly made during Mao's time." I, however, show that the "scars" created by state repression are durable: state repression has a long-lasting negative effect on people's trust in authoritarian leaders. By doing so, I demonstrate that the various strategies that help authoritarian rulers stay in power could undercut each other and have divergent implications for the stability of authoritarian regimes.

Although the study covers over one-fifth of the world's population, I advise against over- or under-generalizing the results to other contexts. On the one hand, China is unique in the sense that it has remained a single-party regime for over 60 years, and that the Cultural Revolution is one of the greatest tragedies of modern history, which makes it difficult to compare the China case with other countries. In democratic countries where government turnovers are frequent and political organizations or the media manage to reduce the costs of information acquisition for political purposes, we should expect the long-term impact of repression on political attitudes to vanish. On the other hand, the logic of the argument can be applied to contexts where the acquisition of information is costly. Recent studies have shown that attitudes toward certain groups of people are highly persistent. For example, [Nunn and Wantchekon \(2011\)](#) show that individuals whose ancestors were heavily raided during the slave trade in Africa are less trusting of other people today. In a similar vein, [Acharya, Blackwell and Sen \(2016\)](#) demonstrate that American whites who currently live in Southern counties that had high shares of slaves in 1860 are more likely to express racial resentment and colder feelings toward blacks. In such contexts, where people either are reluctant or find it costly to update information, past traumatic experience should have a long-lasting impact.

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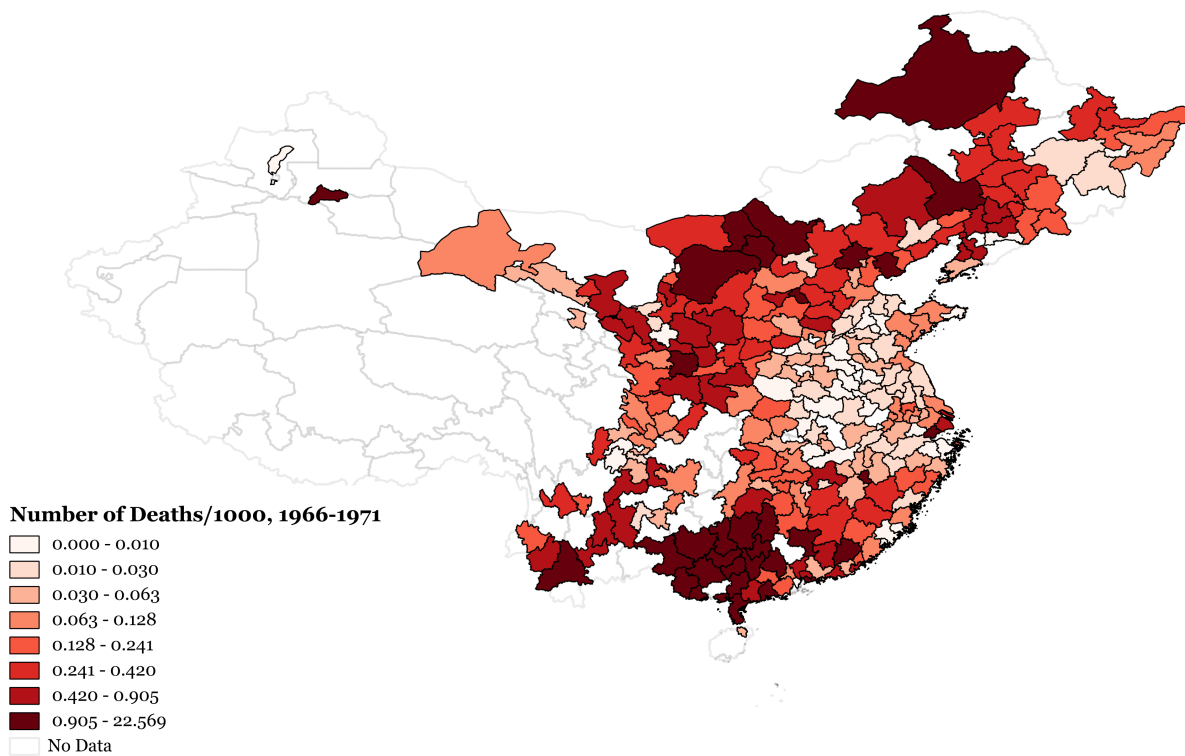


Figure 1: The Number of Deaths Per 1,000 People Across Chinese Prefectures (1966-1971)

Notes: The map shows the regional distribution of violence during the first half of the Cultural Revolution measured by the number of deaths per 1,000 people. The number of deaths is from [Walder \(2014\)](#) and population data is from the 1964 Census.

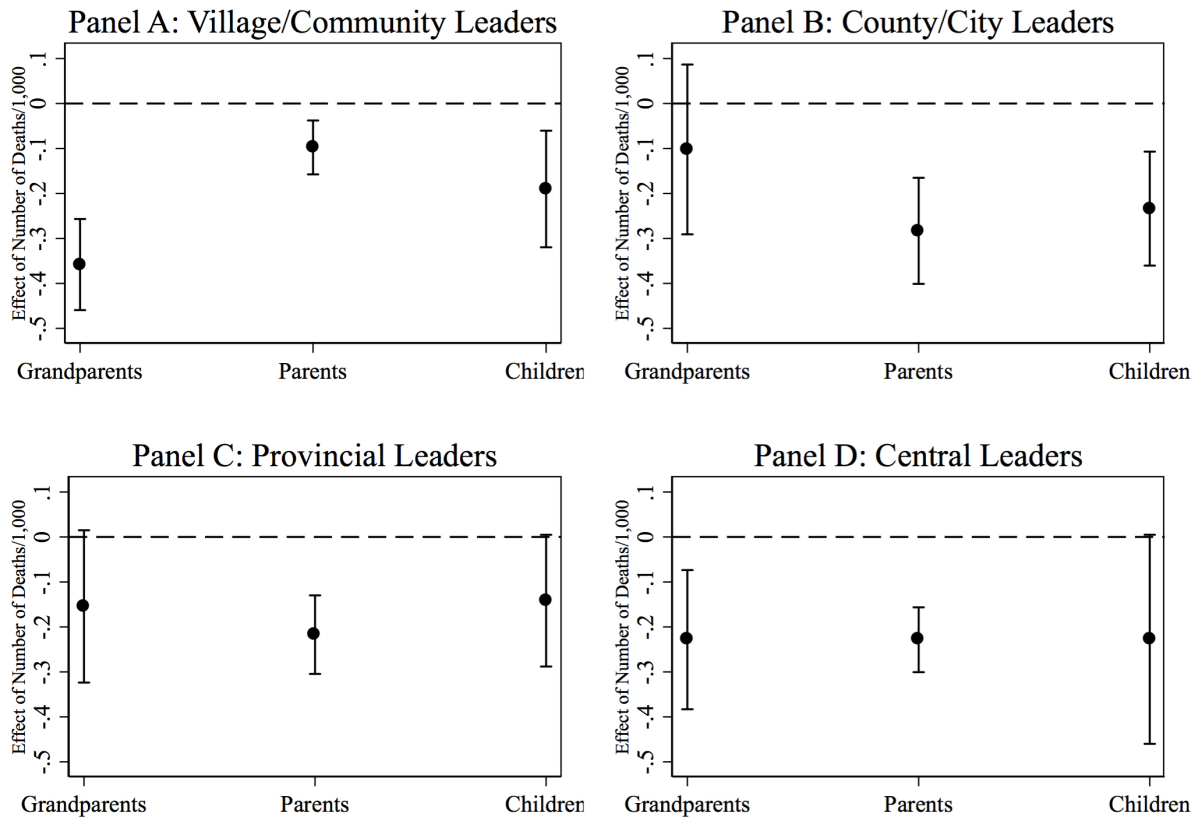


Figure 2: The Effects of Cultural Revolution Violence Across Three Generations

Notes: The plots present the OLS estimates of the effects of *Number of Deaths/1,000* across three generations. For simplicity, the respondents born between 1916 and 1949 are called “grandparents,” the respondents born between 1950 and 1976 “parents,” and the respondents born between 1977 and 1990 “children.” Panel A uses trust in community/village leaders as the dependent variable, Panel B trust in county/city leaders, Panel C trust in provincial leaders, and Panel D trust in central leaders. The black dots indicate the point estimates, and the bars the 90% confidence intervals. Table 6.1 in the Web Appendix shows the estimates.

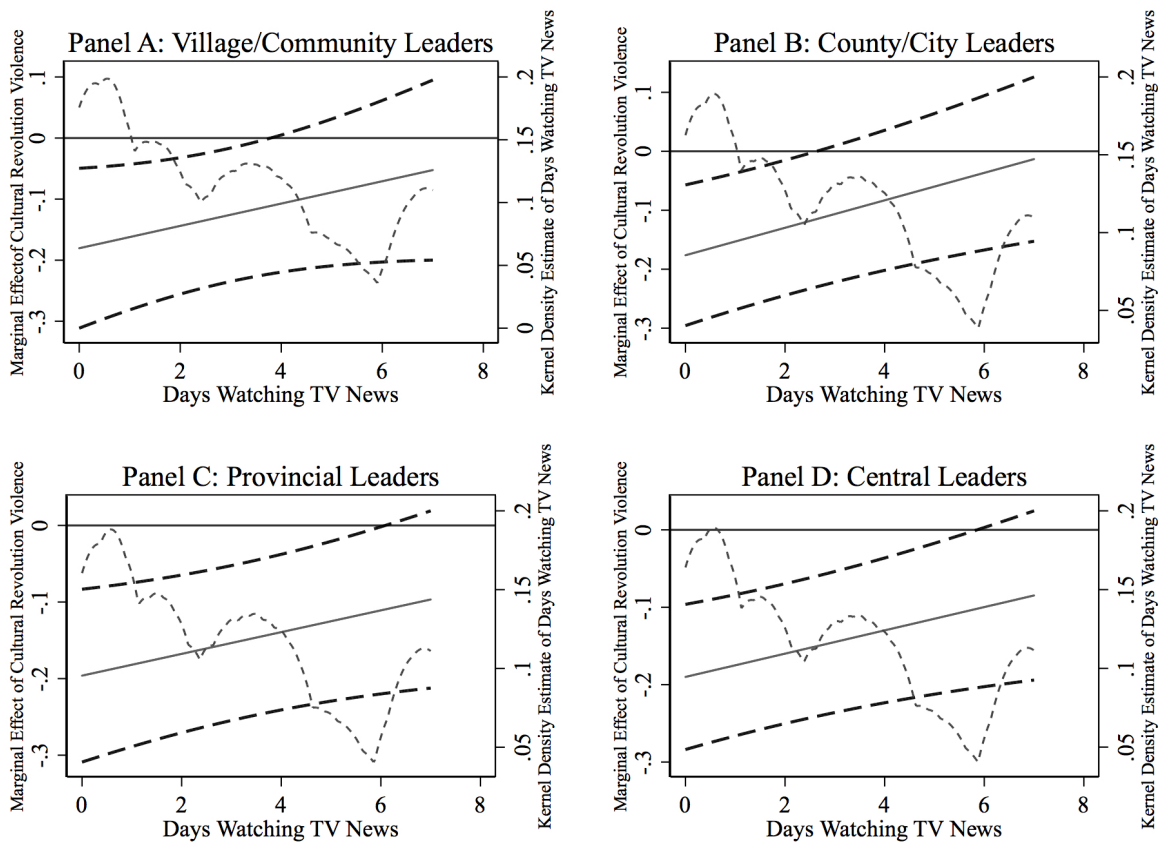


Figure 3: Marginal Effects of Cultural Revolution Violence

Notes: The plots present the OLS estimates of the marginal effects of *Number of Deaths/1,000* conditional on *Days Watching TV News*. *Days Watching TV News* is measured by questions in the China Survey asking how many days the respondent spends in a week on watching political news in the central and local television stations. Panel A uses trust in community/village leaders as the dependent variable, Panel B trust in county/city leaders, Panel C trust in provincial leaders, and Panel D trust in central leaders. All regressions include prefectural controls, *Account Length (log)*, and provincial fixed effects. 95% confidence intervals (dashed lines) are based on standard errors clustered at the prefectural level. Table 6.2 in the Web Appendix shows the estimates.

Table 1: OLS Estimates of the Effects of Cultural Revolution Violence on Political Trust

Variable	Village/Community Leaders		County/City Leaders		Provincial Leaders		Central Leaders	
	(1) Coefficient (Clustered S.E.)	(2) Coefficient (Clustered S.E.)	(3) Coefficient (Clustered S.E.)	(4) Coefficient (Clustered S.E.)	(5) Coefficient (Clustered S.E.)	(6) Coefficient (Clustered S.E.)	(7) Coefficient (Clustered S.E.)	(8) Coefficient (Clustered S.E.)
Number of Deaths/1,000, 1966-1971	-0.077*** (0.026)	-0.152*** (0.040)	-0.135*** (0.026)	-0.240*** (0.068)	-0.133*** (0.016)	-0.209*** (0.043)	-0.093*** (0.033)	-0.232*** (0.033)
Prefectural Controls	X	✓	X	✓	X	✓	X	✓
Individual Controls	X	✓	X	✓	X	✓	X	✓
Account Length (log)	✓	✓	✓	✓	✓	✓	✓	✓
Provincial F.E.	✓	✓	✓	✓	✓	✓	✓	✓
Number of Observations	2,185	1,404	1,885	1,210	1,798	1,157	1,895	1,210
Number of Prefectures	57	50	57	50	57	50	57	50
R ²	0.036	0.073	0.064	0.105	0.081	0.107	0.088	0.128

Notes: This table presents the estimated effects of Cultural Revolution violence on political trust using OLS regressions. The dependent variables are levels of trust in *Village/Community Leaders*, *County/City Leaders*, *Provincial Leaders*, and *Central Leaders*. The data is from the China Survey, and the sample is restricted to respondents who grew up in their current localities. *Number of Deaths/1,000* is a continuous variable measuring the number of “unnatural deaths” per 1,000 people during 1966-1971. Prefectural controls include *Male-to-Female Ratio (1964)*, *Urban Population Percentage (1964)*, *Longitude*, *Latitude*, *Natural Resource*, *Colony*, *Suitability for Wetland Rice*, *Distance to Beijing*, and *Length of Rivers*. Individual controls include *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father’s Education*, *Mother’s Education*, *Good Class*, and *Middle Class* (leaving *Bad Class* as a reference group). Columns (1), (3), (5), (7) present results without prefectural and individual controls, and the remaining columns present results with prefectural and individual controls. All specifications include *Account Length (log)* and provincial fixed effects. Standard errors clustered at the prefectural level are presented in the parentheses. Table 3.1 in the Web Appendix presents the full results including coefficients and standard errors of all of the covariates. *p*-values are based on a two-tailed test: **p* < 0.1, ***p* < 0.05, ****p* < 0.01.

Table 2: OLS Estimates of the Effects of Cultural Revolution Violence on Political Trust by Class Background

Variable	Village/Community Leaders	County/City Leaders	Provincial Leaders	Central Leaders
	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)
Number of Deaths/1,000	-0.158*** (0.055)	-0.222** (0.083)	-0.252*** (0.057)	-0.271*** (0.057)
Class	0.003 (0.071)	-0.093 (0.066)	-0.117* (0.066)	-0.050 (0.052)
Number of Deaths/1,000 × Class	0.004 (0.030)	-0.013 (0.041)	0.034 (0.033)	0.031 (0.036)
Prefectural Controls	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Account Length (log)	✓	✓	✓	✓
Provincial F.E.	✓	✓	✓	✓
Number of Observations	1,404	1,210	1,157	1,210
Number of Prefectures	50	50	50	50
R ²	0.073	0.104	0.105	0.126

Notes: This table presents the estimated effects of Cultural Revolution violence interacting with an individual’s class background on political trust using OLS regressions. The dependent variables are levels of trust in *Village/Community Leaders*, *County/City Leaders*, *Provincial Leaders*, and *Central Leaders*. The data is from the China Survey, and the sample is restricted to respondents who grew up in their current localities. *Number of Deaths/1,000* is a continuous variable measuring the number of “unnatural deaths” per 1,000 people during 1966-1971. *Class* measures the class background of the respondent’s family, which was defined by the Chinese government in the 1950s. There are three values of *Class*: 3=*Bad Class*, including rich peasants, landlords, and capitalists; 2=*Middle Class*, including middle peasants, upper-middle peasants, clerks, and petty merchants; and 1=*Good Class*, including hired peasants, poor peasants, lower-middle peasants, urban poor, and workers. Prefectural controls include *Male-to-Female Ratio (1964)*, *Urban Population Percentage (1964)*, *Longitude*, *Latitude*, *Natural Resource*, *Colony*, *Suitability for Wetland Rice*, *Distance to Beijing*, and *Length of Rivers*. Individual controls include *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father’s Education*, and *Mother’s Education*. All specifications include *Account Length (log)* and provincial fixed effects. Standard errors (clustered at the prefectural level) are presented in the parentheses. Table 3.5 in the Web Appendix presents the full results including coefficients and standard errors of all of the covariates. *p*-values are based on a two-tailed test: **p* < 0.1, ***p* < 0.5, ****p* < 0.01.

Table 3: IV Estimates of the Effects of Cultural Revolution Violence on Political Trust

Variable	Village/Community Leaders	County/City Leaders	Provincial Leaders	Central Leaders
	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)
Second Stage: Dependent Variable is Political Trust				
Number of Deaths/1,000, 1966-1971	-0.260** (0.103)	-0.248*** (0.082)	-0.309*** (0.058)	-0.327*** (0.057)
Durbin-Wu-Hauman Test (<i>p</i> -value)	0.231	0.764	0.647	0.660
<i>R</i> ²	0.071	0.110	0.111	0.133
First Stage: Dependent Variable is Number of Deaths/1,000				
Average Distance to Sulfur Mines (log)	2.746*** (0.611)	2.647*** (0.624)	2.610*** (0.634)	2.676*** (0.612)
Frequency of Mass Rebellions, 1644-1911	✓	✓	✓	✓
Population Density, 1964	✓	✓	✓	✓
Prefectural Controls	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Account Length (log)	✓	✓	✓	✓
Provincial F.E.	✓	✓	✓	✓
Number of Observations	1,404	1,210	1,157	1,210
Number of Prefectures	50	50	50	50
<i>F</i> -Stat of Excluded Instrument	19.81	17.70	16.68	18.78
Adjusted <i>R</i> ²	0.940	0.941	0.945	0.943

Notes: This table presents the two-stage least squares estimates of the effects of Cultural Revolution violence on political trust. The upper panel presents the second-stage results, while the bottom panel presents first-stage results. The dependent variables are levels of trust in *Village/Community Leaders*, *County/City Leaders*, *Provincial Leaders*, and *Central Leaders*. The data is from the China Survey, and the sample is restricted to respondents who grew up in their current localities. *Number of Deaths/1,000* is a continuous variable measuring the number of “unnatural deaths” per 1,000 people during 1966-1971. *Average Distance to Sulfur Mines (log)* is the excluded instrument that measures the natural log transformed average distance between a prefecture and its nearest native sulfur mine, iron sulfide associated minerals mine, or iron sulfide mine. *Frequency of Mass Rebellions* is the number of mass rebellions in each prefecture during the Qing Dynasty (1644-1911). *Population Density* is the number of people per square kilometer based on the 1964 Census. Prefectural controls include *Male-to-Female Ratio (1964)*, *Urban Population Percentage (1964)*, *Longitude*, *Latitude*, *Natural Resource*, *Colony*, *Suitability for Wetland Rice*, *Distance to Beijing*, and *Length of Rivers*. Individual controls include *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father’s Education*, *Mother’s Education*, *Good Class*, and *Middle Class* (leaving *Bad Class* as a reference group). All specifications include *Account Length (log)* and provincial fixed effects. Standard errors (clustered at the prefectural level) are presented in the parentheses. Table 5.5 in the Web Appendix presents the full results including coefficients and standard errors of all of the covariates. *p*-values are based on a two-tailed test: **p* < 0.1, ***p* < 0.5, ****p* < 0.01.

Table 4: Testing the Quality of Political Institutions as an Alternative Mechanism

Variable	Village/Community Leaders	County/City Leaders	Provincial Leaders	Central Leaders
	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)	Coefficient (Clustered S.E.)
Number of Deaths/1,000, 1966-1971	0.025 (0.082)	-0.031 (0.067)	0.029 (0.077)	0.035 (0.065)
Prefectural Controls	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Account Length (log)	✓	✓	✓	✓
Provincial F.E.	✓	✓	✓	✓
Number of Observations	759	709	663	706
Number of Prefectures	51	51	51	51
R^2	0.101	0.120	0.123	0.146

Notes: This table presents the estimated effects of Cultural Revolution violence on political trust among new residents using OLS regressions. The dependent variables are levels of trust in *Village/Community Leaders*, *County/City Leaders*, *Provincial Leaders*, and *Central Leaders*. The data is from the China Survey, and the sample is restricted to respondents who moved to their current localities after they reached adulthood. *Number of Deaths/1,000* is a continuous variable measuring the number of “unnatural deaths” per 1,000 people during 1966-1971. Prefectural controls include *Male-to-Female Ratio (1964)*, *Urban Population Percentage (1964)*, *Longitude*, *Latitude*, *Natural Resource*, *Colony*, *Suitability for Wetland Rice*, *Distance to Beijing*, and *Length of Rivers*. Individual controls include *Male*, *Age*, *Age Squared*, *Year of Education*, *Ethnic Han*, *Urban Family Registration (hukou)*, *Per Capita Family Income (log)*, *Father’s Education*, *Mother’s Education*, *Good Class*, and *Middle Class* (leaving *Bad Class* as a reference group). All specifications include *Account Length (log)* and provincial fixed effects. Standard errors (clustered at the prefectural level) are presented in the parentheses. Table 6.3 in the Web Appendix presents the full results including coefficients and standard errors of all of the covariates. p -values are based on a two-tailed test: * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.