Ethnic Riots and Prosocial Behavior: Evidence from Kyrgyzstan

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Abstract

Do ethnic riots affect prosocial behavior? A common view among scholars of ethnic violence is that riots increase cooperation within the warring groups, while cooperation across groups is reduced. We revisit this hypothesis by studying the aftermath of the 2010 Osh riot in Kyrgyzstan, which saw Kyrgyz from outside the city kill over 400 Uzbeks. We implement a representative survey, which includes unobtrusive experimental measures of prosocial behavior. Our causal identification strategy exploits variation in the distance of neighborhoods to armored military vehicles, which were instrumental in orchestrating the riot. We find that victimized neighborhoods show substantially lower levels of prosocial behavior. Importantly, we demonstrate that the reduction is similarly stark both within and across groups. Using qualitative interviews, we parse out two mechanisms that help explain the surprising reduction in ingroup prosociality: Victimized Uzbeks felt abandoned by their coethnics, and variation in victimization created a feeling of suspicion.

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1 Introduction

Ethnic riots occur with grim regularity around the globe. Since 2010, Africa and Southern Asia alone saw 1,131 fatal riots (Raleigh et al. 2010). In Southern Asia, riots are the predominant form of violence—far more common than military confrontations or state violence against civilians (Kishi, Raleigh, and Linke 2016). Much existing scholarship discusses the causes of riots (e.g., Varshney 2002; S. Wilkinson 2004; Kopstein and Wittenberg 2018). Studies using riots as the independent variable, by contrast, remain relatively rare (e.g., Aidt and Leon 2016). There is particularly little evidence regarding the effect of riots on prosocial behavior.

At first glance, the effect of riots on prosocial behavior is Janus-faced. Riots seemingly lower cooperation across the warring ethnic groups, while ingroup solidarity is strengthened. This is the line of argument in Horowitz's seminal book *The Deadly Ethnic Riot*. Drawing on evidence from over 250 riots, the author hypothesizes that riots widen intergroup, but narrow ingroup cleavages (Horowitz 2001, 445). Robust empirical evidence that explores Horowitz's hypotheses, however, is currently amiss. What is more, the mechanisms that link riots to cooperation have received scant empirical scrutiny.

A related literature on the effect of wartime violence on cooperation has yielded conflicting findings. A number of studies finds civil war victims to show increased prosocial behavior with ingroup members (see Bauer et al. 2016). A different set of studies, however, questions this finding, linking wartime violence to lower levels of trust and prosociality towards both outgroup and ingroup members (Rohner, Thoenig, and Zilibotti 2013; Kijewski and Freitag 2018). Evidence that systematically explores outcomes within as well as across groups remains scarce, however, as do studies that explore mechanisms that could explain these conflicting findings.

add to the existing literature by revisiting the relation between ethnic riots and prosocial behavior. To do so, we draw on micro-level evidence from Kyrgyzstan. In June 2010, the city of Osh saw an outburst of violence pitting Kyrgyz assailants from surrounding villages against Uzbek residents. The riot lasted four days, left 427 dead and another 1,100 injured. Importantly, the Osh riot marks a "typical case": a majority group (Kyrgyz) perpetrated violence against a minority (Uzbeks), leading to large-scale destruction, while security forces remained passive. Osh thus marks a highly relevant case to study within- and between-group cooperation in the aftermath of a brutal riot.

To study the effect of the riot on community cooperation, we fielded a survey to 1,100 Osh residents from August to September of 2017—seven years after the riot. Behavioral experiments serve as a robust measure of ingroup and intergroup prosocial behavior. Comparing Uzbek respondents living in affected areas to those not immediately affected, we find stark differences in prosocial behavior: Victimized Uzbeks are significantly less likely to cooperate in a prisoner's dilemma game, and allocate less money in a dictator game compared to Uzbeks who had been spared. Our benchmark model estimates a 0.33 standard deviation reduction on a comprehensive prosociality index. The finding is robust to the inclusion of potential confounders, the use of a matching procedure as well as an instrumental variable strategy, which exploits variation in exposure of Uzbek neighborhoods to the military barracks from which assailants stole armored vehicles. Importantly and against Horowitz's hypothesis, the reduction is virtually the same within and across groups: Uzbeks in affected areas are less likely to act prosocially no matter whether they engage with a Kyrgyz or an Uzbek individual.

Why did Uzbeks reduce their cooperation with other fellow Uzbeks? To explain this finding—which sets our study apart from most studies on ethnic violence and cooperation—we draw on qualitative evidence. Using interviews and local sources, we put forth two mechanisms. First, Uzbek victims were disappointed that their coethnics failed to support them during the riot. Even the Uzbek government was quick to send Uzbek refugees back to Kyrgyzstan. Victimization, thus, created a feeling of having been let down by other co-ethnics and an urge to punish, if even by small gestures, this perceived betrayal. Second, Uzbek victims expressed suspicion that they, not other Uzbek residents, had been specifically targeted. Victimization thus eroded trust within the Uzbek community, leading to reduced cooperation that continues to this day.

Our paper makes three contributions to the study of ethnic violence. First, we add to a literature on ethnic riots. While empirically and theoretically rich, many of the insights in this literature rely on qualitative findings only. We contribute a rare large-N study with reliable measurement and stringent causal identification. Second, we contribute to the debate on violence and prosociality. Recent work from wartime contexts tends to highlight positive effects of violent conflict on communities. Our work leads us to a more sobering conclusion, highlighting the scarring consequences of at least one type of violence: ethnic riots. Third, we point out two novel mechanisms that may help explain why riots reduce cooperation within the victimized group: riots make coordinated defense difficult, which leads to feelings of "being let down," and the relative arbitrariness of targeting leads to suspicion within the victimized group.

2 Motivation

Do ethnic riots affect prosocial behavior? And, if so, do they differently affect cooperation within the victimized group as compared to cooperation between victims and perpetrators? In line with Horowitz, we define an ethnic riot as an "intense, sudden, though not necessarily wholly unplanned, lethal attack by civilian members of one ethnic group on civilian members of another ethnic group, the victims chosen because of their group membership" (2001, 1). A rich literature examines the causes of riots. Most commonly, scholars argue that riots are triggered by ethnically framed political competition (S. Wilkinson 2004; Kopstein and Wittenberg 2018) and economic grievances (Bohlken and Sergenti 2010; Mitra and Ray 2014). Other cited causes include resentment related to status reversal (Petersen 2002) and deep-seated inter-ethnic hostility (Horowitz 2001). Studies using riots as the independent variable, however, are relatively rare, particularly those studying effects on prosocial behavior.

There is little doubt that riots have deleterious effects on lives and property. Their effect on community relations, however, is often seen as more ambiguous. Horowitz (2001) hypothesizes that riots will widen the gap between the conflicting groups, but will increase cohesion within the victimized group. In line with Horowitz's first hypothesis, Beber, Roessler, and Scacco (2014) find that the 2005 ethnic riot in Sudan hardened negative out-group attitudes, increasing victims' support for separation between the North and the South of the county. Similarly, Dercon and Gutiérrez-Romero (2012) find that the 2007 post-election riot in Kenya lowered trust across ethnic groups (see also Iyer and Shrivastava 2018).

However, there is scant evidence to support the second hypothesis from Horowitz, namely, that riots increase ingroup cohesion. Dercon and Gutiérrez-Romero (2012) show that trust in co-ethnics is *not* higher among victims of the aformentioned 2007 post-election riot in Kenya. In fact, Becchetti, Conzo, and Romeo (2014) show that victims of the same riot are more likely to exhibit *un*trustworthy behavior in experimental games, regardless of whether they play with coethnic or non-coethnic partners.

Evidence from the more developed literature on war and prosociality is likewise mixed. Unsurprisingly, most research investigating intergroup relations finds them to be negatively affected by war (Rohner, Thoenig, and Zilibotti 2013; Bauer et al. 2014; Hadzic, Carlson, and Tavits 2017), although effect sizes are often less pronounced than might be expected (Dyrstad 2012; Whitt and Wilson 2007).

With regard to ingroup cohesion, a significant body of research finds positive effects of exposure to wartime violence on measures of the capacity for collective action and egalitarian behavior (e.g., Bellows and Miguel 2009; Blattman 2009; Voors et al. 2012;

Gilligan, Pasquale, and Samii 2014; Bauer et al. 2016). These effects are sometimes explained in terms of increased investments in social capital rather than physical capital (Gilligan, Pasquale, and Samii 2014). Others have interpreted them as evidence for deep-reaching changes in preferences towards increased egalitarianism and prosociality (Voors et al. 2012; Bauer et al. 2014).[1] The changed preferences can be parochial in nature (Choi and Bowles 2007; Mironova and Whitt 2016). According to this idea—resonating with Horowitz's hypotheses regarding the consequences of riots—positive effects of war on ingroup cohesion are coupled with increased hostility toward the outgroup (Rohner, Thoenig, and Zilibotti 2013; Bauer et al. 2014).

Contrasting with these relatively sanguine findings, other scholars have also collected evidence on cases where war had outright *negative* effects—even on ingroup cohesion. For example, Cassar, Grosjean, and Whitt (2013) document how civil war undermined trust among villagers in Tajikistan. They explain their findings with the social geography of their study site. Conflicting groups settled closely intermixed, making it difficult for locals to differentiate between friend and foe. Similar findings of reduced prosociality from the Balkans and Uganda have been linked to post-traumatic stress disorder, a common psychological consequence of exposure to violence (Kijewski and Freitag 2018; Cecchi and Duchoslav 2018; Ruttan, McDonnell, and Nordgren 2015).

1. Such preference changes have been linked to a phenomenon psychologists have labeled "post-traumatic growth" (Tedeschi and Calhoun 2004)—the observation that the experience of violence can leave individuals more able and willing to reach out to others.

In sum, the literature offers mixed evidence on the consequences of ethnic violence for prosocia1ity—be it in the context of wars or riots. What explains this muddled link? In the case of wars, scholars have tried to explain the contradictory findings by distinguishing between legacies of different types of warfare (Krakowski 2018) and different victimization strategies (Arjona and Chacón 2018). Such distinctions, however, do not apply in the case of riots.

What is more, in the case of riots there is compelling evidence that the causal arrow points in the opposite direction. That is to say, prosociality affects riots. Notably, Varshney (2002, 10), in a detailed study of Hindu–Muslim riots in India, maintains that "[a]ssociations that would suffer losses from a communal split fight for their turf, making not only their members aware of the dangers of communal violence, but also the public at large." As such, any correlation between riots and prosocial behavior may simply be a product of reverse causality (see also Gupte et al. 2014). Add to that the problem of endogeneity: perpetrators do not choose communities at random, compromising any simple comparisons between affected and non-affected areas.

The present study sets out to clarify the link between ethnic riots and prosocial behavior using micro-level evidence from Kyrgyzstan. Given the intuition provided by Horowitz and the weight of evidence from wartime contexts, we originally hypothesized that the 2010 Osh riot increased prosociality within groups, while cooperation across groups was lowered. What we found squarely rejects the first hypothesis. This unexpected finding spurred us to closely examine the evidence at hand, and to formulate two new mechanisms linking riots to prosocial behavior—the disappointment and suspicion channels—which we elaborate upon below.

3 The 2010 Osh riot

A Brief History

Osh is the second largest and oldest city in Kyrgyzstan. The city lies in the south, a few miles from the Uzbek border (Figure 1). Having previously been ruled by various local clans and khanates, Osh was annexed by the Russian empire in 1876. In 1936, Osh was made part of the Soviet Union as part of the Kirghiz Soviet Socialist Republic (Kyrgyz SSR). As a predominantly Uzbek town, Osh's placement within the Kyrgyz SSR (rather than the Uzbek SSR) was a seeming anomaly—arguably, a deliberate strategy to "divide and rule" (Allworth 2013).

It was not until the 1960s that ethnic Kyrgyz began to settle in Osh. Spurred by rapid industrialization, rural Kyrgyz dwellers sought new employment opportunities in the rising urban centers of the Soviet Union (Liu, 2012, 22). In the following years, the former Uzbek city slowly turned into a multi-ethnic economic center. The two dominant groups—Uzbeks and Kyrgyz—peacefully coexisted throughout the 1970s and 80s. Peaceful relations were partly facilitated by the radically inclusive nationality policy promoted by the Soviet Union (Dumitru and Johnson 2011).

Figure 1: Location of Kyrgyzstan and the city of Osh

Kyrgyzstan's independence from the Soviet Union marked the beginning of increased ethnic tensions. Neither Kyrgyzstan nor Uzbekistan had a modern history of statehood. To develop a national identity, the new nations of Central Asia drew on ethnic narratives (Huskey 2003, 34). Both states set up border posts, which cut through long-established trading routes. Uzbeks in Osh were thus separated from their "ethnic homeland." To make matters worse, Kyrgyz authorities continued to encourage the settlement of ethnic Kyrgyz in Uzbek areas, seeking to solidify control by means of "demographic engineering" (Weiner and Teitelbaum 2001). On top of this, administrative borders were drawn to ensure electoral majorities for Kyrgyz voters. In Osh, for instance, heavily populated Uzbek neighborhoods such as Kyzyl Kyshtak, Dikan Kyshtak and Padavan were excluded from the city of Osh despite their proximity to the city center (see Figure A.1 of the Online Appendix), while Kyrgyz villages were made part of the city (Megoran 2013).

As a result of these policies, in 1990 Uzbeks in Osh and several surrounding villages attempted to break away from Kyrgyzstan, formally asking the USSR Supreme Soviet for the establishment of an autonomous region. Their separatist ambitions were never fulfilled, however. Instead, they triggered a counter-reaction in the form of a first ethnic riot. The violence left over 318 people dead (Huskey 2003, 34). The conflict reemerged 20 years later. In April 2010, the toppling of Kyrgyz president Kurmanbek Bakiyev created a power vacuum. Fierce political competition ensued. Ethnic Kyrgyz in Kyrgyzstan's South continued to support Bakiyev, who came from the region. Having suffered discrimination under Bakyiev, however, Osh's Uzbek population was notably cooler. The interim government in Bishkek therefore relied on Uzbek supporters to increase its political influence in the country's South (Solvang and Neistat 2010, 21). The new president specifically appealed to Uzbeks, condemning their political exclusion and discrimination under the Bakiyev regime. Local media and politicians reacted by reinforcing local Kyrgyz's fears. Above all, they successfully mischaracterized the political instability as an attempt by Uzbeks to secede (KIC, 2011, 22-2).

On June 10, 2010, the situation escalated into full-fledged violence. A gambling-related argument in Osh's central casino provoked a violent confrontation between an Uzbek mob of 1,500 people and 30 Kyrgyz police officers. Stones thrown during the clash inadvertently hit the local university's female dormitory. Rumors spread that Uzbek residents had raped Kyrgyz girls. Motivated by what interviewed perpetrators would later call a 'patriotic impulse to fend off Uzbek separatism,' and an 'urge to defend their relatives,' several thousand Kyrgyz from neighboring villages started to make their way to Osh (KIC, 2011, 27-9; International Crisis Group, 2012, 2).

Before reaching Osh on June 11, the Kyrgyz villagers had armed themselves heavily. Weapons were seized from poorly defended border posts near Osh (KIC, 2011, 29). The perpetrators stole grenades, AK-47 automatic weapons, sniper rifles, and bayonets (NHC, 2012, 187-9). Once in Osh, the Kyrgyz perpetrators also managed to take over armored personnel carriers (APC). These happened to be located on two opposite sides of Osh—at the Podgornaya crossroad in western Osh, and near the Furkhat roundabout in eastern Osh—explaining a clear left–right pattern in the ensuing riot (see Figure 2). Supported by these vehicles, two Kyrgyz crowds subsequently marched toward the city center. Any Uzbek residents or property on their way came under attack.

Rampant disagreement among the perpetrators and the presence of improvised weaponry meant that the assault was often chaotic, with assailants plundering at will and hurling Molotov cocktails at buildings that rioters believed were inhabited by Uzbeks (see KIC, 2011, 30; Solvang et al., 2010, 4, 23, 33; NHC, 2012, 67, 69). The riot lasted for four days, during which virtually all Uzbeks of Osh had to fear for their lives.[2] To defend themselves, Uzbeks set up barricades throughout the city. They were instrumental in blocking access to their neighborhoods (known as *mahallas*). There was, however, little defending against the APCs captured by the Kyrgyz rioters, which could easily break through the barricades.

2. Notably, Figure A.2 of the Online Appendix demonstrates that SOS signals were sent across the entire city—not simply in areas that were ultimately attacked.

When the riot came to an end on June 13, 427 people had been killed. 74 percent of casualties were ethnic Uzbeks. 2,843 properties were entirely destroyed (KIC, 2011, 44). During and immediately after the riot, many Uzbek residents tried to flee across the border to Uzbekistan. However, the Uzbek authorities quickly closed the borders and urged those who had already entered to return. The refugees themselves were concerned about losing their Kyrgyz citizenship and property. As a result, almost all of them returned to their former neighborhoods in Osh in the following weeks (KIC, 2011, 46).

One noteworthy feature of the riot was the relative passiveness of the national Kyrgyz security apparatus. The Kyrgyz military only intervened after four days of heavy fighting. Reports from independent sources note that the late intervention was partly a product of the impaired capacity of the central government, which had to quell protests in several parts of the country at the same time (NHC, 2012, 50). Osh's local security authorities were slightly more involved. The NHC report (2012, 85), for instance, mentions various episodes in which local security forces tried to intervene between the warring groups. That said, local efforts to stop the rioters were widely described as feeble and lacking courage.

Given the dramatic, chaotic nature of the riot, the importance of APCs and the swift return of the victims, the 2010 Osh riot presents a suitable case to study the link between ethnic riots and prosocial behavior. The extent of the violence means that any effects on prosociality likely persisted for years. The fact that violence was haphazard—fueled by the availability of APCs—allows us to construct a credible causal identification strategy.

In addition, the fact that Uzbeks returned to Osh ameliorates concerns about nonignorable attrition.

The Osh Riot in Comparative Perspective

How does the Osh riot compare to other ethnic riots around the globe? To address this question, we compiled a list of all riots discussed in Horowitz (2001; see Online Appendix A.1). We characterized them along five key dimensions for which Horowitz reports variation. Table A.1 of the Online Appendix shows that Osh (2010) is fairly typical with regards to i) the minority status of the Uzbek victims, ii) the perpetrator elite support, iii) the scale of destruction, and iv) the conflict history. What distinguishes Osh from the majority of riots is the Uzbek's relative lack of political influence, though it is noteworthy that this only holds in 58 percent of all riots. Taken together, we thus interpret Osh to be a rather typical case. It is comparable to cases such as the anti-Chinese riots in Kuala Lumpur (1969), the anti-Luba riots in Luluabourg (1959), and the anti-Indian riots in Durban (1949-53).

4 Design

Population

Our population of interest are the inhabitants of the city of Osh. According to the 2009 census, Osh has 258,111 inhabitants, 44 percent of which identify as Uzbek, while 47 percent identify as Kyrgyz (see Figure A.6 of the Online Appendix). The remainder comprises a variety of ethnicities including Russians and Tajiks. Geographically, we focus on the historic city center, which we define as the 2.5km radius around Osh's central bazaar. As in many Central Asian countries, the bazaar is the cultural, political and social center of the city, with a trading history stretching over 2,000 years (Welter, Smallbone, and Isakova 2006, 103).

Exposure to violence

To capture whether respondents were exposed to violence during the 2010 riots, we rely on data collected shortly after the riot by the American Academy for the Advancement of Science (AAAS n.d.). Figure 2 shows areas of the city that were destroyed or severely damaged during the riot. We use this data to construct a binary destruction dummy that indicates whether a given primary sampling unit (PSU) was affected. Destruction was severe, making it highly likely that residents of PSUs at the time were directly affected by the riot. This is shown in satellite images from 2010 (see Figure 3). Still, to capture violence exposure at the individual-level, our survey also included an individual-level victimization measure, which we discuss below.

Sampling

To gain a representative sample of Osh's city center, we employed a multi-stage random sampling method, which we detail in Online Appendix A.3. We randomly sampled 880

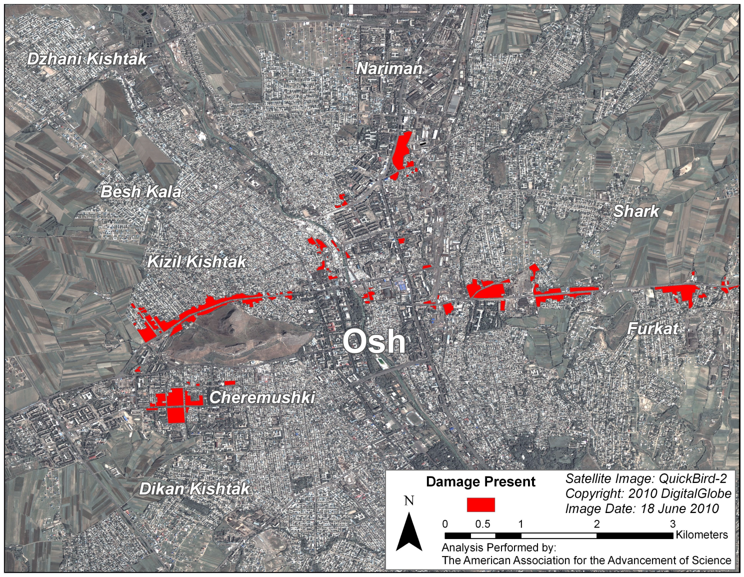

Figure 2: Destruction during the 2010 Osh riot

Notes: The Figure provides an overview of destroyed areas in Osh as of June 2010 (AAAS n.d.).

Figure 3: Destruction during the 2010 Osh riot (Detail)

Notes: The Figure provides a satellite image of one exemplary neighborhood. (Human Rights Watch 2010).

Uzbek and 220 Kyrgyz respondents, drawing an equal split from affected and non-affected areas. We estimated the share of Kyrgyz and Uzbek individuals in a given PSU using data from the Kyrgyz census, which we combined with information on the prevalent housing type inhabited by members of each group. The survey took place between August and September 2017. The period thus coincided with the temporary return of labor migrants from Russia, which minimizes concerns about attrition (more in Online Appendix A.9). The descriptive statistics of the Uzbek and Kyrgyz samples are provided in Online Appendix A.5. Table A.4 demonstrates that destruction cannot be predicted on the basis of individual-level covariates (R-squared of 0.025). Few variables are significant predictors of victimization, and coefficients are small. We discuss ethical considerations about conducting a survey in a riot-ridden neighborhood in Online Appendix A.4.

Measurement

Following Eisenberg and Mussen (1989, 3), we define prosocial behavior as "voluntary actions that are intended to help or benefit another individual or group of individuals." These actions can take various forms. We focus on two common types: cooperation and altruism, which we discuss in turn. We discuss the validity of our measurement in Online Appendix A.7.

Cooperation Our main measure of prosocial behavior is a two-player (here referred to as 'respondent' and 'partner') prisoner's dilemma (PD) game. The PD captures the tension between individual profit and collective benefit and is a widely used measure of cooperation (Axelrod 1985). In our variant of the PD, respondents were given the choice between two options, 'plus' (the cooperative option) and 'minus' (the non-cooperative option). Their payoffs depended on their own choice and that of their partner. The individually profit-maximizing combination was for the respondent to choose 'minus' while the partner chose 'plus', while collective profit was maximized if both players chose 'plus'. The payoffs were explained with a simple graphic, which we reproduce in Figure 4. As can be seen, respondents could earn between 20 KGS (0.30 USD) and 100 KGS (1.45 USD) in the PD. Given that the average daily income in Kyrgyzstan in 2017 was 4.70 USD, the stakes were thus rather high.

You		Partner	
-	60	60	-
+	80	80	+
+	20	100	-
-	100	20	+

Figure 4: Payoff illustration in the PD

Notes: The Figure reproduces the graphic used to illustrate payoffs for respondents (You) and their partner (Partner) during the prisoner's dilemma game. Amounts are indicated in Kyrgyz Som (KGS).

Before respondents made their choices, we informed them that they would play the game twice, with two different residents of Osh. Then, before each game, we told respondents that the partner was either Kyrgyz or Uzbek, in random order.[3] To determine payoffs, participants' choices were randomly matched with the choices of other Osh residents who had participated in the pilot. We informed our participants that their choices would be communicated to their partners via text message, guaranteeing minimal visibility of their actions.[4] Participants were then handed over the tablet-PC used for data collection and took their decisions in private.

Altruism Our second measure of prosocial behavior is the degree to which individuals are willing to act altruistically. We measure altruism using a non-strategic dictator game (DG). Participants were given 50 KGS (0.70 USD) and were asked to decide how much, if any, of this amount they would share with another resident of Osh. To ease implementation, respondents were allowed to choose any share divisible by five. Since there is no sanctioning mechanism in the DG, any positive amount shared with another person is interpreted as an indication of altruistic behavior. Once again, before participants made their choices, they were informed that they would play the game twice. Then, before each game, respondents were told that the partner was either Kyrgyz or Uzbek (in random order). Participants were told that the money shared with other players would be transferred in mobile phone credit, but that no information about the sender would be revealed. All survey respondents who gave us their phone numbers were considered as potential recipients of DG transfers.

3. In Figure A.20 of the Online Appendix, we demonstrate that all our empirical results hold when using the first decision only as the dependent variable.

4. The script with the exact wording of the PD is reproduced in Online Appendix A.6.

Construct validity Do the primes "Kyrgyz" and "Uzbek" truly capture in- and outgroup categories? Given that the riot occurred along ethnic cleavages, it is relatively uncontroversial to assume that Uzbek individuals perceive Kyrgyz counterparts as the outgroup. But, do they also consider co-ethnics as in-group members? Uzbeks' strong sense of ethnic bonding is reflected in similar appearance, customs, gestures, language, clothing, tastes, and habits (Liu 2012, 12). As Horowitz (2001, 47-8) argues, the above cues of similarity confer a special bonding power to ethnic identities. We confirm this conjecture by drawing on a small (N=144) telephone survey that we fielded in Osh in September 2018 (see Online Appendix A.14). The data shows that 60 percent of surveyed Uzbeks would not allow their daughters to marry a Muslim man from another ethnic group, be he Kyrgyz, Russian or Tajik—even though there are no religious rules forbidding such unions. Respondents' donation behavior in the dictator game in the non-victimized areas can serve as another indicator for the strength of ingroup identification. Here, Uzbeks gave 33% of their endowment to other Uzbeks. This is comparable to levels of giving recorded in other studies, where dictators usually pass on 20-30% (Camerer 2003), including to ingroup members (Whitt and Wilson 2007). The amount passed on is therefore on the higher side, testifying to a comparable degree of ingroup solidarity. A final piece of evidence for strong ingroup bonding among Uzbeks can be gathered from the *Life in Kyrgyzstan* survey (implemented in 2010; Brück et al. 2014). While the sample only includes 71 Osh Uzbeks unaffected by the riot, 89 percent of them state that they trust other co-ethnics, with 63 percent expressing "a lot of trust."

5 Results

Simple comparison

Does exposure to ethnic riots affect prosocial behavior? To answer this question, we begin by simply comparing Uzbek respondents in affected and non-affected PSUs. Figure 5 plots coefficients and confidence intervals from an OLS regression of our outcomes on the destruction dummy. The Figure demonstrates that Uzbek respondents residing in damaged neighborhoods show substantially lower levels of prosocial behavior. Affected Uzbeks are 0.16 SD (8 percentage points) less likely to cooperate with a Kyrgyz individual in the PD. And they allocate 0.47 SD (14 out of 100 Som) less to Kyrgyz individuals in the DG. Importantly, the reduction in cooperation is also visible within the Uzbek community. Affected Uzbeks are 0.23 SD less likely to cooperate with an Uzbek individual in the PD, and contribute 0.46 SD less to other Uzbeks in the DG. Taken together, we thus find support for Horowitz's first hypothesis (reduced inter-group cooperation). We do not, however, find support for Horowitz's second hypothesis (increased ingroup cooperation): Uzbeks in affected areas are less likely to act prosocially no matter whether they engage with a Kyrgyz or an Uzbek individual.[5]

5. We highlight a noteworthy similarity between effect sizes across the in- and outgroup measures. One interpretation of this finding is that Uzbek victims leveled equal blame to co-ethnics and perpetrators. Uzbek victims may have had high (and thus easy to disappoint) expectations about cooperation from other co-ethnics, while they were understandably appalled by the Kyrgyz perpetrators. This resonates with Becchetti, Conzo, and Romeo's (2014) work, which shows that victims of post-election violence in Kenya exhibit a comparable reduction in trustworthiness in experimental games played with in and outgroup members.

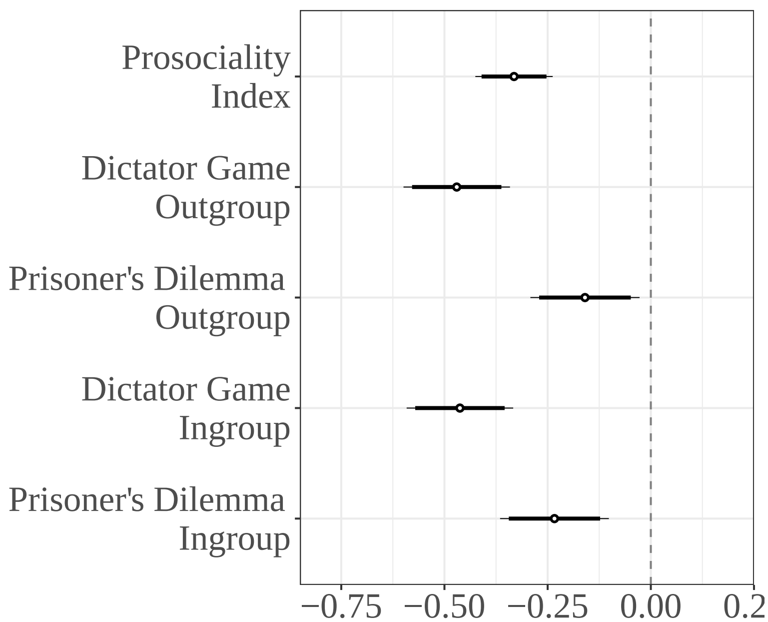

Figure 5: Effect of Riot on Prosocial Behavior (OLS)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of OLS regressions of the indicated outcomes on the destruction dummy. All outcomes are standardized. (DoF = 876).

Controlling for confounders

Though there is evidence that the riot erupted unexpectedly and that target selection was haphazard, riots are not random. A variety of social, economic and political forces may explain why some areas, but not others, are exposed to violence (see S. Wilkinson 2004; Kopstein and Wittenberg 2018). The simple regression is thus likely subject to confounding forces that determine both victimization as well as prosocial behavior. Based on a review of the qualitative literature covering the Osh riot and drawing on interviews with local experts, we distilled four plausible confounders: wealth, state capacity, community policing, and accessibility. To save space, we discuss our measurement of these variables in Online Appendix A.8. In Table 1, we show our results to be robust to the inclusion of the potential confounders as well as the vote share garnered by the *Ata-Jurt* (AJ) party during the 2010 general elections, which we use as a measure of support for the overthrown Kyrgyz president Bakiyev.

Attrition

Even if confounders are appropriately addressed, our research design runs the risk of suffering from nonignorable attrition. It could be that the riot led cooperative people to leave affected areas. Any differences between affected and non-affected areas would then not be due to victimization, but due to selective migration patterns. In Online Appendix A.9 we present four reasons that boost our confidence that attrition is of minor concern. First,

	Cooperation in Prisoner's Dilemma Ingroup (1)	Investment in Dictator Game Ingroup (2)	Cooperation in Prisoner's Dilemma Outgroup (3)	Investment in Dictator Game Outgroup (4)	Prosociality Index (5)
Destruction	−0.387***	−0.575***	−0.267***	−0.537***	−0.442***
	(0.074)	(0.073)	(0.075)	(0.073)	(0.053)
Wealth index	0.433	0.503	0.801*	0.586	0.581*
	(0.328)	(0.319)	(0.330)	(0.320)	(0.232)
State capacity index	0.177	0.679***	0.138	0.677***	0.418***
	(0.146)	(0.142)	(0.147)	(0.142)	(0.103)
Community policing index	0.076*	−0.081*	0.048	−0.081*	−0.009
	(0.033)	(0.032)	(0.033)	(0.032)	(0.023)
Accessibility index	0.362	−0.500	−0.120	−0.526	−0.196
	(0.308)	(0.300)	(0.310)	(0.300)	(0.218)
AJ %	0.767***	0.488*	0.488*	0.083	0.457**
	(0.225)	(0.219)	(0.226)	(0.219)	(0.159)
N	878	878	878	878	878
Adjusted R^2	0.034	0.083	0.019	0.081	0.080

Table 1: Effect of Destruction on Prosocial Behavior (controlling for confounders and mobilization)

Notes: The Table reports point estimates and standard errors of linear regressions of the indicated prosocial behavior outcome on the destruction dummy, controlling for the four indicated confounders plus the vote share obtained by the *Ata-Jurt* (AJ) party in the 2010 general elections. All outcomes are standardized. $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

we show that 97 percent of respondents have never lived elsewhere. Second, respondents state that few individuals have migrated since 2009. Third, self-reported migration is similar across victimized and non-victimized areas. Fourth, we estimate bounds for the estimated share of attritors and show our results to be robust.

Robustness tests

In the Appendix, we confirm the robustness of our key findings in four additional ways. We show that all estimates remain strongly significant when i) aggregating the individual-level data at the PSU-level, ii) when adjusting standard errors for spatial autocorrelation using three independent connectivity matrices, iii) when estimating spatial lag models (Online Appendix A.10), and iv) when matching PSUs on pre-treatment covariates using a rather strict caliper of 0.05 (Online Appendix A.11).

Instrumental variable

Even when controlling for confounders, one might worry that unobserved variables explain post-riot differences across damaged and non-damaged PSUs. To address this concern, this section introduces a pre-registered instrumental variable strategy. We exploit the fact that the riot was inflicted by Kyrgyz villagers from outside of Osh who relied on APCs to break through the barricades set up by Uzbek residents. This finding is documented in three independent reports by the Kyrgyzstan Inquiry Commission, the Norwegian Helsinki Committee, and Human Rights Watch.

Most Uzbeks in Osh, as in other Central Asian cities, live in so-called *mahallas*. These typically include 20-50 houses, which are clustered around a mosque or central square. Mahallas are tightly knit communities "controlled and monitored by the local community [and] [...] accessible only to members of the community" (Kutmanaliev 2015, 457). Despite the construction of multi-story apartment buildings during the Soviet Era, Osh's Uzbek population has largely remained in the historic mahallas (Liu 2012).

Crucially, the mahallas marked an effective hindrance to the Kyrgyz perpetrators. The mahallas allowed Uzbeks to set up barricades and road blocks, which "played an important role in saving some of the neighborhoods from violence" (Kutmanaliev 2015, 453). Without the use of armored vehicles, Kyrgyz perpetrators could not have accessed the mahallas. Human Rights Watch writes:

> *The attacks on Osh Uzbek neighborhoods [...] show a consistent pattern. In many accounts, individuals in camouflage uniforms on armored military vehicles entered the neighborhoods first, removing the makeshift barricades that Uzbek residents had erected. They were followed by armed men who shot and chased away any remaining residents, and cleared the way for the looters.* (Solvang and Neistat 2010, 4)

The ability of the Kyrgyz perpetrators to enter and destroy Uzbek neighborhoods was thus largely a function of whether military vehicles were at their disposal. Indeed, on the night of June 10, Kyrgyz gangs had been unable to break into Uzbek mahallas. It was not until the rioters had managed to capture military vehicles on June 11 that they successfully entered the Uzbek mahallas.

Areas where APCs were not available witnessed little to no destruction. The map in Figure A.6 of the Online Appendix helps explain this logic. It demonstrates that many neighborhoods of Osh were plausible targets for the rioters. Large parts of Osh are predominantly Uzbek. The few areas that did see destruction lie on the direct path to Osh from locations from which the Kyrgyz military sent APCs to quell the riot. To break into the mahallas, Kyrgyz perpetrators managed to steal some of the APCs. In western Osh, they stole them directly from a military barrack. In the east, they appropriated the APCs while on their way to the city center—at the Furkhat roundabout.[6] The precise locations are shown in Figure 7. The following account illustrates how perpetrators took hold of the APCs:

> *[T]hree army APCs [...] were stopped by a crowd of 4000 people [...] Officers and soldiers were dragged out of the APC and beaten up. A mechanic/driver managed to put one APC out of use [...]. The second APC was seized by the crowd. The third managed to get out of the crowd and reached the command center by sideroads. Similar descriptions of the events are found in the report of Ismail Isakov [the Special Representative of the Provisional*

6. The APCs stolen near the Furkhat roundabout had been sent to Osh from the Jalal-Abad province (Mayli-Suu) because Osh's security forces lacked APCs (they had lost several APCs during clashes with demonstrators in the Talas province a few weeks prior to the Osh riot).

Government for Southern Kyrgyzstan].

(NHC, 2012, 131)

All this is not to say that distance from APCs is the sole driver of victimization. While destruction on the direct path of the APCs toward the city center was nearly universal, certain individuals undoubtedly managed to select out of violence. Distance to the APCs thus only captures the *intent-to-treat* effect. Put differently, the IV setup estimates the effect of victimization on prosocial behavior by focussing on the plausibly exogenous assignment process, namely, distance to the APCs. All PSUs on this path are then coded as victimized—even if some may not have complied with the "treatment." As such, the IV setup is rather conservative. If we simply used victimization as the independent variable, the effect would be larger, though biased.

IV assumptions

To use the location of the military barracks as an instrumental variable, we must invoke five assumptions. We will only briefly discuss the assumptions in the main text and refer readers to the SI for a more detailed discussion (see Online Appendix A.12). There, we make five points. First, we show that distance to the barracks is strongly correlated with the destruction dummy (F-Stat of 271.9). Second, we rule out defiers, i.e., individuals selecting to be victimized despite not being "assigned." Third, we present a falsification test which corroborates that the instrument is unrelated to prosocial behavior in a sample of 136 nearby villages, thus underlining the exclusion restriction. Fourth, we address SUTVA concerns by estimating spatial error models. Fifth, we argue that the location of the APCs is likely exogenous and support this assumption by showing that distance is not predicted by the aforementioned confounders.

What, however, if the Kyrgyz military strategically positioned the APCs so as to victimize some Uzbek mahallas, while sparing Uzbeks they deemed loyal? This logic does not apply to the barrack in western Osh, which, as noted, had been present for decades. But, the second APC location, the Furkhat roundabout, could theoretically be plagued by such confounding. Three reasons make this selection pattern unlikely. First, the APCs stolen at the roundabout were sent from the province of Jalal-Abad, taking the most direct path toward Osh. There is thus no evidence that the APCs were strategically placed. Second, we collected precinct-level voting data from elections right after the riot. Based on this data, we can rule out that non-victimized areas were more likely to vote in favor of the local government (see Online Appendix A.17; note, however, that this analysis is post-treatment). Third, in Table A.6 of the Online Appendix we show that respondents in victimized PSUs in eastern Osh were *not* more likely to agree with the statement "people like me have no say in what government does." This, then, suggests that loyalist Uzbeks were unlikely to be underrepresented in victimized areas, implying that they were not systematically protected from local authorities. Taken together, there is thus no evidence that the second APC location was "selected." Even so, to rule out any remaining concerns, in Figure A.12 of the Online Appendix we restrict our IV analysis (more below) to western

Osh—the area where selection could not have taken place—and find treatment effects, if anything, to be larger.

IV results

Having briefly discussed the core IV assumptions, we estimate the following two-equations:

$$Y_i = \beta_0 + \beta_1 \mathrm{X}_i + \varepsilon_i \tag{1}$$

$$X_i = \beta_0 + \beta_1 \mathrm{Z}_i + \varepsilon_i \tag{2}$$

where Y_i are our prosociality outcomes for individual i, X_i depicts victimization, and Z_i is individual i's distance to the nearest location where APCs were stolen. We estimate the equations using two-stage least squares, as well as by simply instrumenting X with Z

In Figure 6, we plot the coefficients and confidence intervals of three separate IV-models. Focusing on the two-stage least-square model (squares), we estimate that destruction during the riot—instrumented using distance to the barracks—reduces prosocial behavior by 0.55 SD. Again, the finding holds for both in- and outgroup prosociality. Importantly, the finding is similar when substituting the destruction dummy with the closeness instrument (triangle) and also detectable when aggregating the data at the PSU-level and adjusting for spatial autocorrelation (flipped triangle).

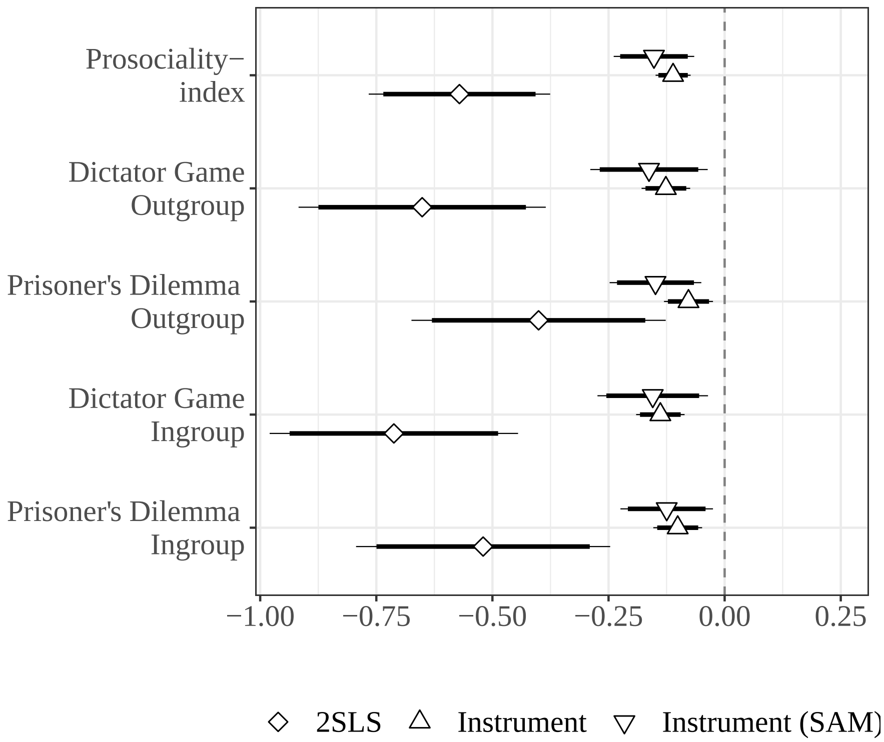

Figure 6: Effect of Riot Destruction on Prosocial Behavior (IV)

Notes: The Figure plots point estimates (marker) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the distance to the closest barrack measure (instrument) or the destruction dummy instrumented with the distance measure (2SLS). SAM refers to a model in which standard errors are adjusting for spatial autocorrelation using the travel time connectivity matrix (see Online Appendix A.10). All outcomes are standardized. All models draw on 876 DoF, except for the SAM models, which are aggregated at the PSU-level (194 DoF).

Randomization inference

In a final step, we develop a randomization inference test to further probe the robustness of our findings. Our goal is to assess how likely it is to observe an effect of the size reported in Figure 6 if the location of the barracks had been chosen randomly. Put differently, we want to gauge how likely it is that our result could arise by chance. To answer this question, we simulate pseudo starting locations for the APCs. To do so, we map the two actual locations from which APCs were stolen (red stars), and then simulate pseudo street-locations of hypothetical APC starting points within a band, which we laid around the city center, stretching from the closer barrack to the farther (see Figure 7). We simulate a total of 10,000 locations, 5,000 to the east and 5,000 to the west of the river Ak-Buura, which splits the city from north to south. We then re-estimate the reduced-form instrumental variable regression of the prosociality index on each pseudo-distance and store its estimate. For each estimation, we 10,000 times draw two pseudo-locations, one from the eastern, and one from the western sample. We determine the closest location, and use the distance between this location and the interview locations to calculate our reduced-form estimates.

The results from this procedure are given in Figure 8. The distribution of effect sizes shows that most pseudo-distances to hypothetical barracks do not yield a sizable negative correlation with the prosociality index. Only 421 simulations result in a more negative effect size than observed in reality, corresponding to a p-value of 0.042 (one-sided). Our estimated effect is thus unlikely a product of chance. Interestingly, the analysis does demonstrates that, on average, closeness to hypothetical barracks is associated with a

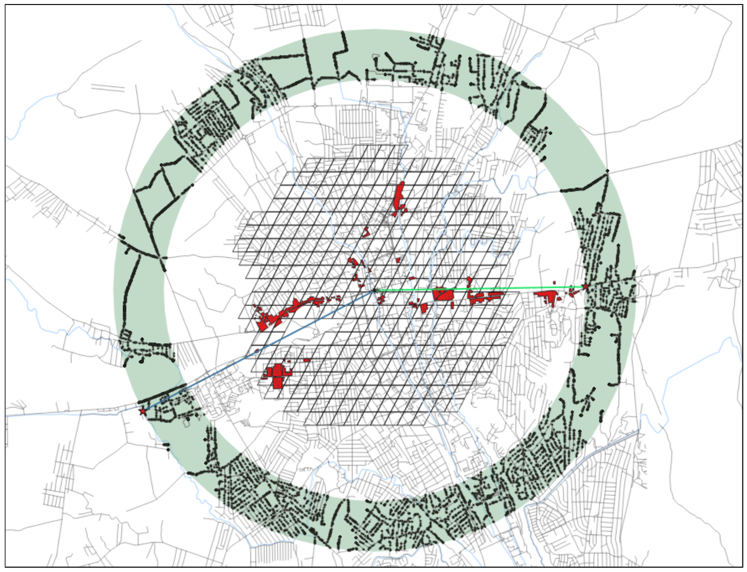

Figure 7: APC locations

Notes: The Figure plots the locations where the APCs where captured (red stars) and 10,000 random pseudo starting locations. The band from which to draw locations was centered around Osh's bazar such that it included the two observed locations.

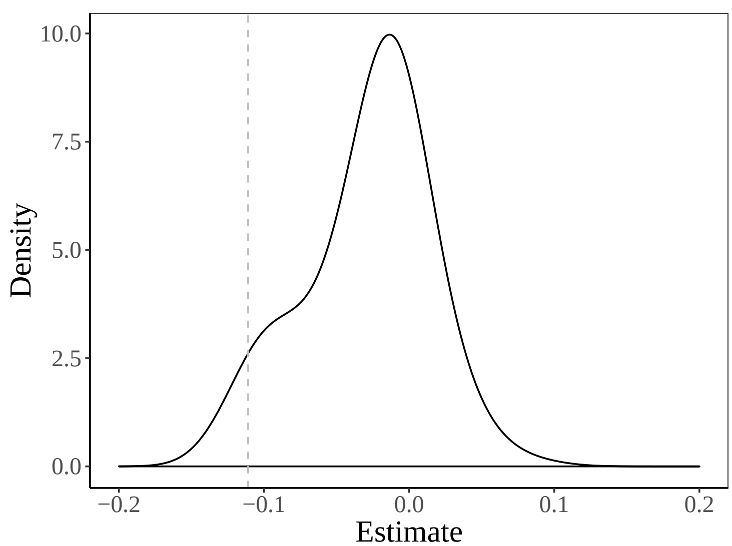

Figure 8: Realized estimates

Notes: Estimated effect sizes of a regression of the prosociality index on the barrack distance instrument, drawing 10,000 pseudo-locations randomly. The grey dotted line plots the observed effect when using the actual barrack location.

slight drop in prosocial behavior: the distribution's mean is -0.03. Most likely, this is due to the fact that individuals residing closer to major roads (who are, hence, closer to the hypothetical barracks) show lower levels of prosocial behavior. This conjecture, however, is not confirmed when visually inspecting prosociality patterns in Osh. A heat map of prosociality in Osh (Figure A.13 of the Online Appendix) shows that prosocial behavior does not cluster along major roads. Rather, there are no distinct geographic patterns. Moreover, the virtue of randomization inference is that it controls—or, rather, "breaks"—any geospatial clustering. It takes such data patterns into account by shifting the distribution against which the effect must survive to the left. Therefore, our observed effect of minus 0.111 stands out even in the face of a general pattern of negative prosociality along locations close to the barracks. And given that randomization inference makes no distributional assumptions (it is fully nonparametric), it, to our minds, is the most convincing piece of evidence that our finding is causal and not a product of chance.

6 Mechanisms

We have demonstrated a robust negative correlation between riot destruction and prosocial behavior. Is this finding a product of victimization? And, if so, how did victimization succeed in destroying bonds even within the Uzbek community? To answer these questions, we first scrutinize whether the main causal channel is, indeed, victimization. To do so, we draw on two pieces of evidence.

Victimization

First, we use a survey item on victimization, namely, property losses. We asked the following question: "During the past 10 years, did you lose any of the following things due to some misfortune? Please mark all that apply." Due to the sensitivity of the topic, we chose this item over straightforward questions about physical harm or losses during the riot. Respondents were given the following options: car, TV, house, money, business, and other. Given that some of these losses plausibly took place as a result of the riot, we can construct an individual-level victimization measure. Reassuringly, Figure 9 confirms that residents in affected areas were significantly more likely to suffer from the loss of a house, business, money or TV—all items that were damaged or stolen during the riot. What is more, the effect sizes are pronounced, which underlines that the destruction dummy does indeed capture the victimized areas of Osh.

We also check if this individual-level measure affects prosocial behavior. Such an analysis is undoubtedly subject to confounding (property as well as property losses are not exogenous). To address this concern, we again instrument the individual victimization measure with distance to APCs. Figure A.14 of the Online Appendix confirms a strong negative correlation between the index and prosocial behavior. The finding thus supports the intuition that Uzbeks became less prosocial because they had been victimized.

Second, we make use of the Kyrgyz sample. As was laid out above, Kyrgyz individuals also lived in areas affected by the riot. They were not victimized, however. Rather, they happened to live in neighborhoods with a large Uzbek population that were targeted by the

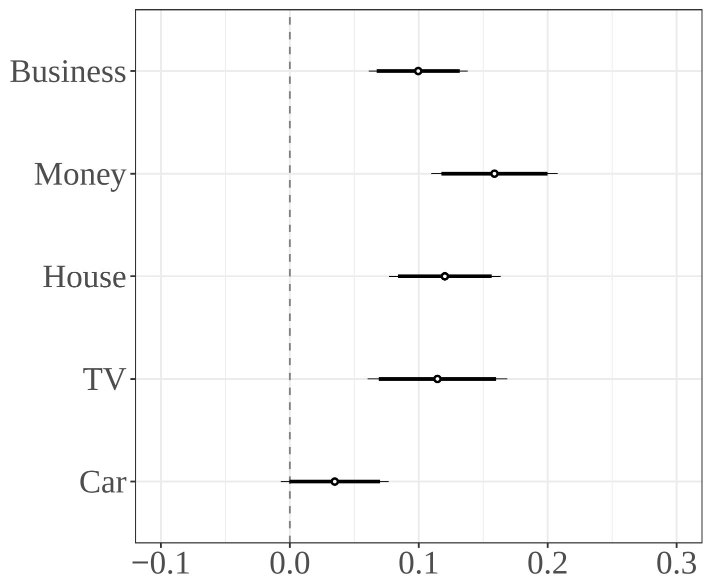

Figure 9: Effect of Riot Destruction on Losses (OLS)

Notes: The Figure plots point estimates (marker) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of OLS regressions of the indicated losses on the destruction dummy. All outcomes are binary. (DoF = 876).

Kyrgyz mob. They are thus a suitable comparison group to assess whether victimization, not a general destruction channel, drives the reduction in prosocial behavior. In Figure A.17 of the Online Appendix, we demonstrate that the prosociality index is essentially unmoved among Kyrgyz individuals. The index is roughly 0.05 SD lower in destructed areas. The accompanying standard error is large (0.11), however.

Given the widespread loss of property during the riots, one may ask whether economic hardships could be responsible for the reduced levels of prosociality we are observing. Property destruction during the riot might have made victims poorer, and thus less able to afford prosocial behavior towards their ingroup. Alternatively, victimization might have increased levels of inequality, fuelling resentment.[7] The available evidence does not support these ideas, however. Current income levels are not significantly correlated with victimization (see Table A.4 of the Online Appendix). In fact, in 2017, affected households were marginally wealthier than non-affected ones (with a monthly income of 317 USD versus 299 USD, respectively). While victimized households certainly suffered economic hardships immediately after the riot, there is no evidence that they still do so seven years later. If anything, we see that inequality is lower in victimized areas, with a Gini coefficient of 0.28, compared to 0.32 in non-victimized neighborhoods.

To further test the economic channel, we investigate heterogeneous treatment effects conditional on pre-riot levels of wealth (measured at the PSU-level). If affected households became less prosocial due to financial hardship, we should observe a greater reduction in

7. We are grateful to one anonymous reviewer for suggesting these mechanisms.

prosociality in areas that were poorer before the riot and thus—arguably—more severely affected by property losses. We do not find evidence on this point either. In fact, the reduction in prosociality is uniform across neighborhoods, no matter their economic situation before the riot (see Figure A.15 of the Online Appendix). Economic hardship, therefore, does not seem to explain why the riot undermined prosocial behavior among Uzbek victims.

Why the Reduction in Ingroup Bonding?

How, then, can we explain the novel finding that victimization led to a reduction of prosocial behavior within the Uzbek community? In our pre-analysis plan we had hypothesized that exposure to violence would *increase* ingroup solidarity, and we had formulated mechanisms that could explain this finding (laid out in Online Appendix A.15). These were i) a decline in risk preferences, ii) increased identification with the ingroup, and iii) investment in community protection due to fear of future conflict. Unsurprisingly, our empirical analysis shows no support for these mechanisms (see Figure A.21 of the Online Appendix). This is reassuring inasmuch as the mechanisms should not be affected, given that we do not find an increase in ingroup prosociality. To explain the surprising reduction in ingroup prosociality, we therefore draw on qualitative interviews conducted during three months of fieldwork. Our sources included local Uzbek and Kyrgyz residents and students, NGO workers, academics as well as representatives of the survey firm. From our interviews, we condensed two channels: a disappointment and a suspicion channel.

Disappointment

The first channel might best be described as a "disappointment" mechanism. Uzbek victims repeatedly complained that other Uzbeks had "let them down" during the riot. For example, many Uzbeks refused to provide shelter to other Uzbeks during the riot. One of Kutmanaliev's (2017) interviewees, for instance, noted that Uzbek leaders in Nariman (a village in northern Osh) instructed their community not to host Uzbek refugees. The leader is cited as follows: "We won't let anyone enter our village. Don't host refugees in your houses. Let them go and pass elsewhere, wherever they want to go" (Kutmanaliev 2017, 213). One of our own interviewees who tried to escape from Osh during the riot told us that his car was stopped by co-ethnics from another mahalla. As a result, he could not leave the city. The fact that the Uzbek government sent Uzbek refugees back to Kyrgyzstan was another example Uzbeks letting down their own ethnic group.

The feeling of being let down also extended to Uzbek community leaders. While local leaders negotiated with both law enforcement authorities and Kyrgyz elders, their efforts turned out to be fruitless (KIC, 2011, 31). To many Uzbeks the failure to settle peace signified a lack of leadership and an inability to safeguard the well-being of the Uzbek community. It therefore came as no surprise that Uzbeks in victimized mahallas began viewing community leadership as a 'meaningless institution.' In the victimized Karajygach mahalla, for example, Uzbeks did not even protest when long-standing leaders were replaced by local bureaucrats after the riot (Ismailbekova 2013, 115). One of the former leaders, Muhtar *aka*, confessed that even before being replaced he had struggled to deal with physical assaults and sexual harassments in his neighborhood. He initially advised community members to report these events to the police, but, according to his own words, residents refused to follow his advice (Ismailbekova 2013, 116).

Uzbek victims also complained that other Uzbeks, including key leaders, did not participate in the relief operation in the late summer of 2010. One local NGO representative, for instance, noted that the reconstruction of destroyed mahallas was almost exclusively financed by international NGOs and the victimized families. This is confirmed in our small follow-up telephone survey, which shows that none of 59 re-interviewed victimized households received any financial help from co-ethnics during the year of the riot. Uzbeks that escaped the violence proved reluctant to contribute to the reconstruction of the destroyed mahallas, exacerbating the feeling of being let down.

Following this disappointment channel, one can conceptualize the reduction in prosociality as "ethnic punishment," whereby Uzbek victims penalized other Uzbeks for the lack of support during and after the riot. This interpretation links the mechanism to the wider literature on the evolution of cooperation (Axelrod 1985; Bowles and Gintis 2004; Smirnov et al. 2010). This literature highlights how punishment is central to maintaining cooperative relations in the long run, especially in cases where the cooperation partner defected—which clearly was the case within the Uzbek community in Osh.[8] Punishment

8. Interpreting reduced prosociality as a punishment may also help explain why we record opposite effects from those expected under "post-traumatic growth" theory. In this theory, growth stems partly from the experience of help by others (cp. Tedeschi and Calhoun 2004). What Uzbeks in Osh experienced was the outright *lack* of support from their co-ethnics, undermining the potential for post-traumatic growth.

here serves both a retaliative function—communicating that exploitation is unacceptable—and a restorative function: through punishment the partner should be convinced to return to cooperative behavior (Ostrom, Gardner, and Walker 1994). Findings from the literature on war and cooperation are in line with this reasoning. For example, Gneezy and Fessler (2012) find that non-cooperation was more severely punished during the Israel–Hezbollah war than during peace time. And Becchetti, Conzo, and Romeo (2014) show that victims of the Kenyan post-election violence react with starkly reduced prosociality when their trust is misused in a previous round of behavioral games.

Suspicion

A second channel relayed to us in interviews may be described as a feeling of "suspicion." Given the haphazard nature of the riot, victimized Uzbeks began to ask why they, not other Uzbeks, had been targeted. During qualitative interviews, victims would frequently ask "why me?" Many victims did not want to accept that, at the local level, no particular individual was to blame for the riot (KIC, 2011, 71-6). The search for answers fueled suspicion. Rumors spread among victimized families that non-victimized Uzbeks had brokered deals with the Kyrgyz assailants—despite a lack of concrete evidence.

One infamous example of suspicion concerned a restaurant in one of the attacked mahallas. While the mahalla was partly destroyed by the attackers, the restaurant in question was spared. Shortly after the riot, the restaurant was renamed after a Kyrgyz hero. One interviewee noted that Uzbeks from the mahalla had grown increasingly suspicious of the restaurant's owners—suspecting them of having secretly collaborated with Kyrgyz rioters. For some, the new name of the restaurant was definitive proof of treason.[9]

In other cases, victims expressed suspicion towards the Uzbek community at large. In interviews conducted by the Norwegian Helsinki Committee (2012, 39), for example, Uzbek victims stated that during the riot they had heard stories that "unknown [Uzbek] men in black T-shirts interrupted the attempts to enter into negotiations with the police and provoked the crowd to action." Although none of these accounts could be confirmed, as the report concludes, the victims became convinced of a large-scale 'conspiracy' behind the riot, which had supposedly been put together by other Uzbeks. Our own interviewees made similar comments. One interviewee from the Oshski Rayon, for instance, suspected the Uzbeki government to be behind the riot so as to justify a military intervention in Kyrgyzstan (which did not materialize).

Feelings of suspicion were also fueled by the perceived unfairness of being victimized. One common line of thinking among victimized Uzbeks was that other Uzbeks were responsible for the riot because they had protested against the Bakiyev government in April 2010 (an event believed to have sparked the riot). This sentiment was relayed to us in several interviews conducted in the area of Furkhat. Victimized Uzbeks portrayed the protesters as "provocateurs" who had brought destruction and death to many innocent

9. When we visited the restaurant, the owners explained to us that the new name was meant to attract Kyrgyz customers and minimize the risk of harassment from the Kyrgyz-dominated police. Shortly after the riot, the police extorted many Uzbek businesses, a fact confirmed by Ismailbekova (2013). The name change, if anything, thus resembles a peace signal, and demonstrates that local-level suspicion was unwarranted.

Uzbeks. Meanwhile, the protesters themselves, it was argued, had managed to stay out of trouble (see also Kutmanaliev 2017). The fact that Kadyrzhan Batyrov—a nationalist Uzbek leader widely believed to have stirred ethnic tensions in April 2010—escaped and found refuge in Sweden was a frequent example of the perceived injustice brought up during our interviews.

The suspicion channel resonates with findings of reduced levels of trust in the aftermath of large-scale violence (Rohner, Thoenig, and Zilibotti 2013; Cassar, Grosjean, and Whitt 2013). A classic explanation of this pattern comes from a study on the slave trade in seventeenth-century Africa by Nunn and Wantchekon (2011). The authors note that a hallmark of the slave trade was that individuals could partly protect themselves by turning against others within their community. Many sold neighbors to the traders in exchange for captives and arms, which undermined trust in affected localities. However, in other cases in which conflict destroyed trust, the betrayal was not so obvious. The mere suspicion was enough to harm community relations (Krakowski 2018). In Osh, this suspicion seemingly affected many cooperative behaviors, such as lending money for weddings or participation in traditional voluntary communal aid (*hashar*) (Liu 2012, 130). It also made more personalized forms of prosociality, targeting the closest family, more prominent (Ismailbekova 2013, 120).

Testing the mechanisms

Can our survey evidence yield quantitative support for our qualitative evidence? Two suggestive pieces of evidence support the claim that the riot created disappointment and suspicion within the Uzbek community. The first piece of evidence comes from the Prisoner's Dilemma game. After each decision in the PD, we asked respondents what they believed their partner would do. In Table A.13 of the Online Appendix, we relate these expectations to actual behavior. The Table demonstrates that the share of respondents expecting non-cooperation from their co-ethnic partners, and who consequently did not cooperate themselves, is higher in victimized than in non-victimized areas (19 vs. 14 percent)—an indication for continued suspicion. Interestingly, we also observe that the share of those that did not cooperate, even though they believed that their counterpart would cooperate, is also higher in victimized areas (24 vs. 18 percent). While this behavior could be seen as mere selfishness, we believe it is better interpreted as an intentional act of punishment for previous non-cooperation.

The second piece of suggestive evidence comes from a supplementary analysis in which we interact our instrument with a survey item that captures respondents' fear of future conflict (see Online Appendix A.16). In line with our mechanisms, Figure A.16 of the Online Appendix shows that a reduction in ingroup prosociality is *not* observed among Uzbek victims who expect other riots to happen in Osh. These Uzbeks cannot afford to let their suspicion drive them to punish coethnics or withdraw from cooperative relations within their community. They are aware that they may need their co-ethnics' help if another riot erupts. Therefore, they continue to invest in positive community relations, hoping to secure future support.

7 Discussion

This paper has explored whether ethnic riots affect prosocial behavior. Drawing on evidence from the 2010 Osh riot, we found that victimized neighborhoods see lower levels of cooperation and altruism. Victimized Uzbeks not only cooperated less with Kyrgyz counterparts—the group of the perpetrators—but also less with other Uzbeks. We argued that the reduction in prosocial behavior toward the ingroup is due to two channels: a feeling of being let down by one's coethnics, and suspicion towards non-victimized neighbors.

Under what conditions might one expect ethnic riots to reduce ingroup cohesion, as was the case in Osh? Put differently, can we spell out scope conditions that allow us to speak to the generalizability of our findings? The considerations that follow are necessarily tentative. That said, two interrelated conditions likely exacerbated the disappointment and suspicion channels. First, is the widespread perception that the riot was unexpected and chaotic. Second is the fact that the riot was a rare, one-off event. We discuss both conditions in turn.

Suspicion within a victimized community is particularly likely to arise when ethnic violence erupts unexpectedly and unravels in a chaotic manner. The "intense" and "sudden" nature of riots, to cite Horowitz (2001, 1), leaves victims clueless as to who the fighting actors are, what motivates them, and why some community members, but not others, are targeted. By most accounts, the Osh riot was disorganized and lacked clear leadership. Kyrgyz elders were unable to stop their co-ethnics from attacking Uzbek mahallas (KIC, 2011, 37-8). To neutral observers the riot thus resembled what Horowitz (2001) called a collective "amok"—a frenzy of goalless killing. For the Uzbek victims, the uncertainty surrounding the violence led to a desperate search for answers, fueling suspicion toward co-ethnics that had managed to remain unscathed. Put differently, had the riot been the result of careful planning on behalf of the Kyrgyz perpetrators, unfolding along clear front lines, the Uzbek community could plausibly have mobilized more effectively. And suspicion and mistrust would likely have been less pronounced.

Disappointment with one's fellow coethnics is particularly likely to materialize if ethnic violence is as a rare, one-off event. By contrast, if riots are a repeating event—as is the case in Lucknow, India, for instance—individuals are less likely to get disappointed by their coethnics for two main reasons: First, the repeated occurrence of violence means that individuals are accustomed to collectively resist attacks and can establish precise codes of conduct. Second, repeated rioting gives individuals an incentive to rush to their neighbors' defense. After all, the next riot is already on the horizon. Cooperation is thus made viable by the need for protection from future violence. In an apparently unique event like the Osh riot, however, defection in the form of non-help is the dominant strategy—disappointing the hopes of those in need of support.

Based on these two conditions, one can make an informed guess about the generalizability of our key finding. If inter-ethnic fighting is chaotic and one-off, it can reduce cooperation within the victimized group. According to our own quantification of the riots documented by Horowitz (2001), the above description fits a number of riots in post-Soviet countries, Europe and the United States. This includes the riots in Novy Uzen, Kazakhstan (1989), Nottingham, England (1958), and Beaumont, Texas (1943), to name a few. Such riots emerge at the time of critical junctures, such as the collapse of empires, unexpected transitions of power, the introduction of revolutionary policies, or rapid demographic changes. What sets them apart from other riots is the unexpected, novel exposure to violence, the disorganized nature of fighting, and the lack of leadership. Tentatively, one might therefore also expect our finding to apply in some types of war, especially guerrilla war. Here, violence is often directed against civilians and perpetrated by combatants in civilian disguise. Individuals exposed to such violence have been shown to grow increasingly suspicious of their neighbors (Krakowski 2018). This in turn may overshadow their incentives to invest in community relations.

Our evidence also helps address two questions raised by S. I. Wilkinson (2009) in his review of the riot literature. First, we have shown the effect of riots on prosocial behavior to be lasting. We observed a stark reduction in prosociality seven years after the riot took place. Riots, despite being short, have long-term consequences. Second, our evidence helps answer the question of causal order. In discussing Varshney's (2002) *Ethnic conflict and civic life*, S. I. Wilkinson (2009) asks whether peace is the cause or consequence of interethnic associational life. Our evidence points towards peace as a cause more than a consequence.

What we find leaves little in the way of optimism. The effect of the riot appears to have been nefarious throughout. Our findings can therefore resonate with earlier research on war, which generally highlights the destructive effects of war on physical, human and social capital (Collier 2003). Partly in response to this literature, the international community started to focus on community building and reconciliation programs following war. While these are discussed controversially, there is some evidence that such programs have positive effects (Fearon, Humphreys, and Weinstein 2009). Our evidence suggests that focusing solely on inter-ethnic animosities may be shortsighted. Instead, our findings suggest that reconciliation programs will need to address communal rifts both across and within groups.

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A Online Appendix

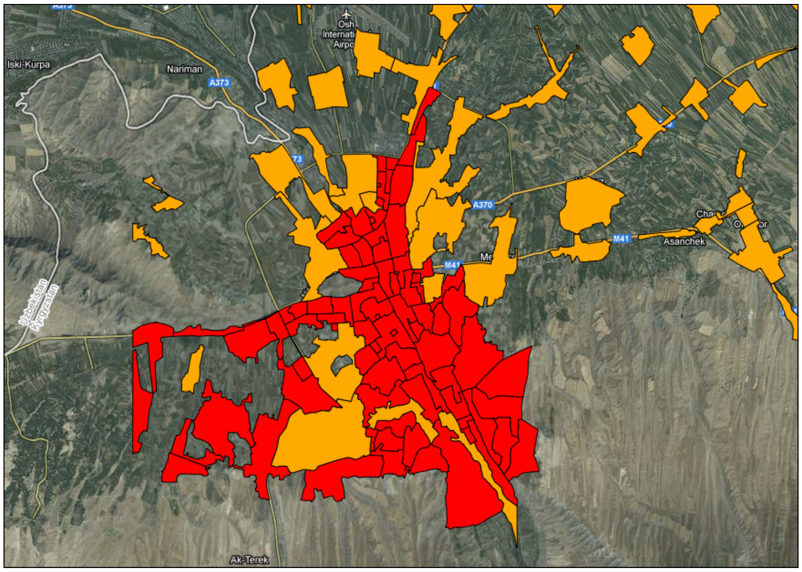

Figure A.1: Electoral precincts in Osh and around

Notes: The Figure shows the 2010 electoral precincts in Osh and surroundings. Orange areas are precincts that are administratively not part of Osh. Many are predominantly Uzbek-inhabited. Source: Hamm (2012).

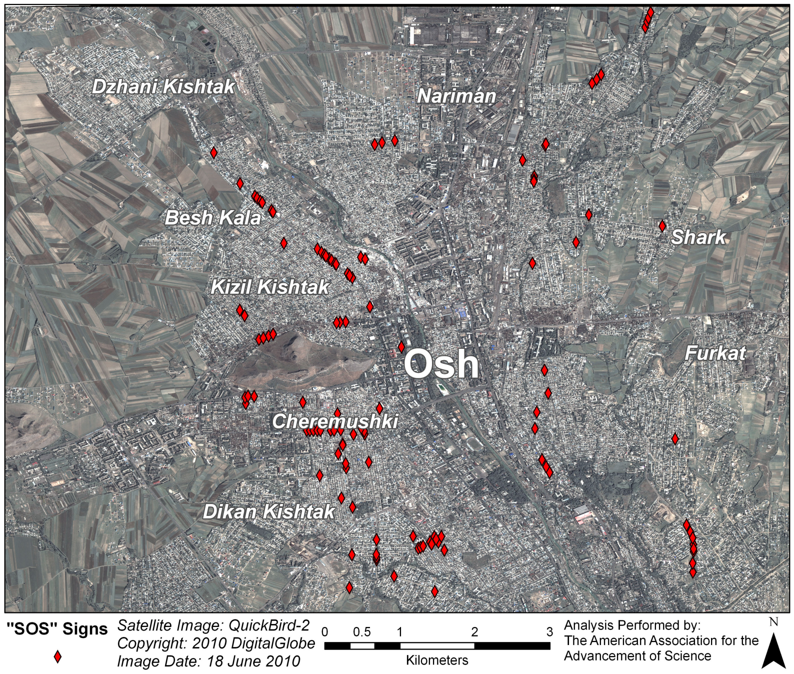

Figure A.2: SOS signs during the 2010 Osh riot

Notes: The Figure shows SOS signs sent during the 2010 Riot (AAAS n.d.).

A.1 The Osh Riot in Comparative Perspective

How does the Osh riot compare to other ethnic riots around the globe? Riots vary on a number of dimensions. The target group can be a local minority (e.g., the 1990 Baku riot) or a local majority (e.g., the 1957 Penang riot).[10] It can be a politically marginalized group (e.g., the 1972 Ferozabad riot), or hold significant influence in government (e.g., the 1953 Kano riot). There is also significant variation in the degree to which state authorities intervene (Wilkinson 2004). Authorities can be quick to contain violence (e.g., the 1968 Baltimore riot) or let it unfold (e.g., the 1920s Jerusalem riots). The scale of destruction is also highly variable. It varies from moderate (e.g., the 1990 Tirgu Mures riot) to extreme (e.g., the 1983 Nellie riot). Finally, some riots are one-shot events (e.g., the 1989 Fergana riot), while others occur repeatedly in the same locations (e.g., the 1980s Karachi riots).

Where does the 2010 Osh riot fit on this map? Is Osh a typical case? To address these questions, we compiled a list of all riots discussed in Horowitz (2001).[11] We characterized them along five key dimensions for which Horowitz reports considerable variation. Table A.1 shows that Osh (2010) is fairly typical with regards to i) the minority status of the Uzbek victims, ii) the perpetrator elite support, iii) the scale of destruction, and iv) the conflict history. What distinguishes Osh from the majority of riots is Uzbek's relative lack of political influence, though it is noteworthy that this only holds in 58% of all riots.

10. All examples that follow are from Donald L Horowitz (2001).

11. Our point of departure was a list generously provided by David Laitin (2001). Online Appendix A.2 describes our coding protocol. Although there are more recent datasets on riots (e.g., Salehyan et al. 2012), we use Horowitz (2001) given its global coverage and unparalleled level of detail.

	Victim local minority	Victim politically influential	Perpetrator elite support	Destructive (>10 casualties & property destruction)	Repeated riot
Global average	64%	58%	76%	75%	52%
2010 Osh riot	Yes	No	Yes	Yes	Yes
N	132	128	115	146	265

Table A.1: The Osh Riot in Comparative Perspective

Notes: The Table categorizes all riots presented in Donald L Horowitz 2001 along the indicated five dimensions, reporting the average incidence. The number of observations varies across columns due to incomplete information for some variables. The second row indicates whether the 2010 Osh riot is in line with the majority of cases or not.

Also, as explained above, the riot happened at a time when the Uzbeks' political influence was on on the rise. Taken together, we thus interpret Osh to be a rather typical case. It is comparable to cases such as the anti-Chinese riots in Kuala Lumpur (1969), the anti-Luba riots in Luluabourg (1959), and the anti-Indian riots in Durban (1949-53). Based on these figures, we are therefore tempted to conclude that the 2010 Osh riot affords a moderate degree of external validity.

A.2 Riots data: Coding protocol

The above global dataset of ethnic riots (see Table A.1) refers to the list of riots discussed in Horowitz (2001) and compiled by Laitin (2001). We coded five variables to describe in detail each riot in the data set. We relied exclusively on information from Horowitz's (2001) *The Deadly Ethnic Riot*. This ensures comparability across cases at the expense of some missing information. The variables were coded according to the following protocol:

Victim minority status

- Coded as "Yes" if, at the time of the riot, the victim group was clearly inferior in numbers to the perpetrator group in the region where the riot took place. Lamentably, given the lack of data on the exact ethnic composition, we cannot use an unambiguous cutoff point of, say, 20 or 30 percent. Instead, we use Horowitz's (2001) statements such as "the victim group was outnumbered by the perpetrator group" as an indication of clear inferiority in numbers.

- Coded as "No" if, at the time of the riot, the victim group was not clearly inferior in numbers to the perpetrator group in the region where the riot took place. In some cases, the perpetrator was even outnumbered by the victim.

Victim politically influential

- Coded as “Yes” if, at the time of the riot, the victim group was represented in the government and/or benefited from targeted government policies (e.g., the recognition of victim’s language in the administration or the education system).

- Coded as “No” if, at the time of the riot, the victim group was not represented in the government, nor benefited from targeted government policies. Note that this does not imply that the victim posed no political threat to the perpetrator. In many cases it did, for example, through participation in strikes, demonstrations, or when electoral competition is tight.

Perpetrator elite support

- Coded as “Yes” if violence against the victim group was actively supported or passively tolerated by major political parties, religious leaders, the government, or law enforcement authorities.

- Coded as “No” if violence against the victim group was not actively supported, nor passively tolerated by any political parties, religious leaders, the government, nor law enforcement authorities.

Destructive riot

- Coded as “Yes” if the riot provoked more than ten causalities and led to major property damage (e.g., shops and/or houses were burned).

- Coded as “No” if the riot provoked less than ten causalities and led to minor property damage (e.g., shops and/or houses were plundered, but not burned).

Repeated riot

- Coded as “Yes” if there were previous riots in a locality between the same ethnic groups.

- Coded as “No” if there were no previous riots in a locality between the same ethnic groups. Note that if other riots took place in the locality but along different ethnic cleavages, a subsequent riot is not considered as repeated.

A.3 Sampling

In order to gain a representative sample of Osh's city center, we employed a multi-stage random sampling method. Our primary sampling units (PSUs) are 250m x 250m grid cells constructed from the GHS population grid (Freire and Pesaresi 2015). We superimposed these PSU unto a map of our sampling area (see Figure A.5). Given our interest in victimization during riot and given that most victims were ethnic Uzbeks (72 percent of identified victims; International Crisis Group 2010), we oversampled i) Uzbek respondents and ii) damaged areas. We decided to sample 880 Uzbek and 220 Kyrgyz[12] and to draw 50% of PSUs from damaged areas. We estimated the share of Kyrgyz and Uzbek individuals in a given PSU using data from the Kyrgyz census, which we combined with information on the prevalent housing type inhabited by members of each group. We marked as 'damaged' all PSUs that suffered from destruction according to the AAAS satellite imagery (Figure 2; 64 PSUS). Undamaged PSUs (N=276) served as the recruitment area for 'non-damaged' observations. We then determined sample sizes for each PSU by randomly drawing with replacement from this pool of PSUs, weighted by the number of inhabitants within them. This procedure left us with 227 PSUs to recruit our sample from, 57 'damaged,' and 170 'undamaged.' Samples sizes were determined independently for Uzbek and Kyrgyz subjects, resulting in the two samples shown in Figures A.3 and A.4.

12. This figure includes 100 observations from a pre-test. The reason for surveying one-fifth Kyrgyz individuals is twofold. First, the survey firm was uneasy with the prospect of conducting an exclusively Uzbek survey. Second, we included Kyrgyz in order to assess whether potential treatment effects can also be seen among members of the group of the perpetrators.

Importantly, enumerators selected households within PSUs following a random walk procedure. They started recruiting respondents at a randomly chosen location, contacting every third household in the designated area until all interviews were completed. Enumerators were allowed to interview only one person per household—randomly chosen from household members present at the time of the interview. Recruited respondents were interviewed in their native language—Kyrgyz or Uzbek—by coethnic enumerators. All interviews took place between August and September 2017, a period coinciding with a temporary return of labor migrants from Russia.[13] The descriptive statistics of the two samples are provided in Online Appendix A.5. We discuss ethical considerations about conducting a survey in a riot-ridden neighborhood in Online Appendix A.4.

13. Scheduling all interviews during the summertime allowed us to reach those residents of Osh who do not permanently live in the city. This was important, given that some residents migrated after the riot, but continue visiting Osh in the summertime.

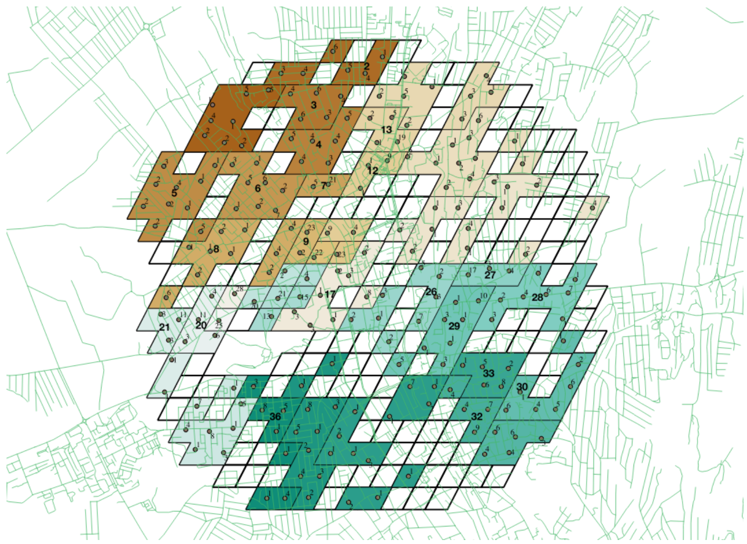

Figure A.3: Uzbek Sample

Notes: The Figure plots the randomly drawn PSUs for the Uzbek sample. The numbers indicate the sample size.

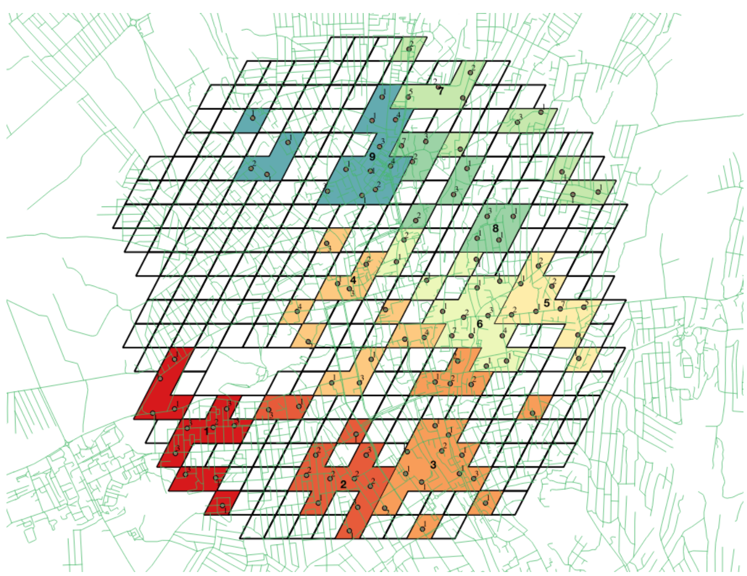

Figure A.4: Kyrgyz Sample

Notes: The Figure plots the randomly drawn PSUs for the Kyrgyz sample. The numbers indicate the sample size.

A.4 Ethical concerns

A survey on intergroup relations in an ethnically-divided city poses risks to subjects and enumerators. We took these risks seriously and devised seven steps to mitigate them. First, before commencing the survey, we conducted qualitative interviews with over 30 residents of Osh, including local students, university lecturers, community leaders, and employees of the survey firm. We also consulted local experts about the appropriateness of the research (including Joldon Kutmanaliev, Ruslan Umaraliev, among others). Following their feedback, we modified several survey items. Notably, we eliminated almost all ethnic references from the survey. We could not eliminate ethnic references from the behavioral games devised to measure cooperation. However, our subjects understood them as providing an equal opportunity to both groups' members. Apart from the games, we included ethnic references in two further items regarding the ethnicity of one's employer and the use of different languages.

Second, we used a professional and reputable local survey firm, which has extensive experience with conducting surveys in Osh and throughout Kyrgyzstan.

Third, we exclusively recruited local enumerators from Osh who had excellent knowledge of the area and sustained experience in conducting surveys. Our enumerators included local school teachers, university lecturers and two students. They were all considered esteemed members of the respective communities, and were trusted by the participants. The recruited enumerators came from both Uzbek and Kyrgyz communities and were instructed to interview members of their own ethnic group only. This explains the high response rate of 78 percent.

Fourth, all relevant Kyrgyz authorities were informed about the study and the survey firm obtained the appropriate research permits. Moreover, all enumerators were given a certificate confirming their authorization to conduct a survey, which they showed to respondents when obtaining informed consent.

Fifth, we invested a significant amount of time in the training of the enumerators, discussing the security situation, the rights of research subjects, and protocol to follow in case of unexpected problems. Although the enumerators were experienced in the conduct of human subjects research, we administered these additional training sessions in one of Osh's city hall offices, underlining that city officials supported the project.

Sixth, to further ensure the appropriateness of the research, we conducted an extensive pre-test. During the pre-test we realized that some respondents felt uncomfortable about questions surrounding violence during the riot. We discovered this after completing five interviews and immediately interrupted data collection (which was restarted with a modified questionnaire without any item on violence exposure).

Seventh, one of the authors was present during the entirety of the data collection. We also put in place a number of security measures. First, we personally met enumerators twice a week to discuss any issues and to hand out cash to pay for incentives. Second, we established a constant response system, asking enumerators to report back continuously. Third, we instructed enumerators to abort surveys as soon as the subject or enumerator was experiencing the slightest feeling of unease. Even so, there were seven instances in which enumerators were approached by people who were not meant to be interviewed but still demanded to be interviewed. We let enumerators interview these subjects but discarded the data later on.

Taken together, the security measures worked very well, providing reliable evidence on the consequences of ethnic riots, while maintaining the security of all persons involved. Despite these security measures, one of the authors was contacted by law enforcement in Osh after the conclusion of the survey, only days before a small follow-up survey in Aravan and Nariman was scheduled to take place (as laid out in the pre-analysis plan). The authorities asked that we abandon the data collection due to the upcoming presidential elections and possible tensions related to this. We complied with this request.

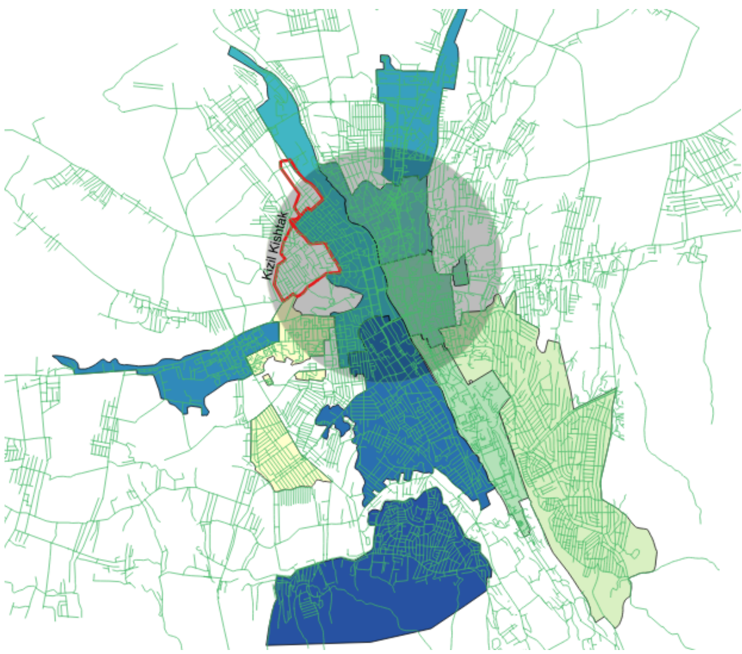

Figure A.5: Administrative districts of Os

Notes: The map plots the administrative districts of Osh. The dark circle indicates the historic center and sampling area. The area outlined in red, Kyzyl-Kyshtak, does not formally belong to Osh, but historically and culturally is part of the city. It is therefore included in our sampling frame.

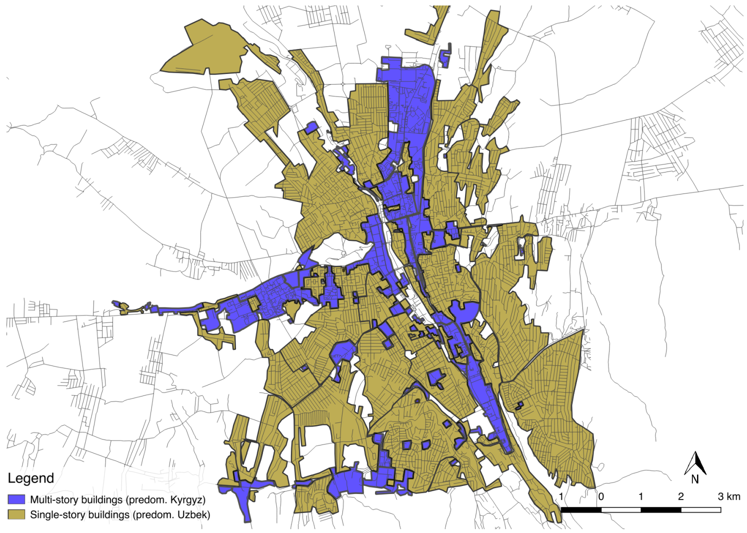

Figure A.6: Ethnicity in Osh

Notes: The map characterizes the buildup in Osh. Multi-story houses are predominantly inhabited by ethnic Kyrgyz, while single-story houses are predominantly inhabited by ethnic Uzbek. The exact ethnic competition is estimated in combination with data from the 2009 census.

A.5 Descriptive statistics

The descriptive statistics of our sample are given in Table A.2. The Table splits the sample along the Uzbek and Kyrgyz subsamples for the damaged and non-damaged neighborhoods. 59 percent of the sample self-identify as women (*Female*) and the average age is 40 (*Age*). Respondents, on average, have three children (*Children*) and earn 304 USD per month (*Income*). 26 percent of the sample have lived abroad (*Lived abroad*), a result of significant migration to neighboring Uzbekistan and Russia. Respondents' households have, on average, six members (*HH size*). 66 percent of respondents live in apartments (*Apartment*), while the remaining 34 percent live in houses (*House*). 61 percent of the sample own their dwelling (*Owner*). Respondents' education level is as follows: 15 percent have completed primary education (*Primary*), 53 percent have completed secondary edcuation (*Secondary*) and the remaining 32 percent have completed tertiary education (*Tertiary*). 75 percent of the sample are married (*Married*), 13 percent are in a relationship (*Relationship*), while 12 percent have another marital status (*Other*)—being widowed, divorced or without a steady partner. Regarding employment, 24 percent of the sample are employees (*Employee*), 28 percent consider themselves housewives (*Housewife*), 15 percent are retired (*Retired*) and 14 percent are self-employed (*Self-employed*).

The Table also allows one to assess post-treatment differences across the two affected and unaffected samples. Interestingly, we find almost no noticeable differences. Within the Uzbek sample, the only variable with a noteworthy difference is respondents' housing type. Respondents residing in affected areas are 7 percentage points more likely to live in apartments. All other variables, however, are near identically distributed. Within the Kyrgyz sample, there is a similar noteworthy imbalance in housing type as well as gender. We sampled 14 percentage points more women in affected areas. We should point out, however, that the sample size for the Kyrgyz sample is small—given that our prime focus is to study the victimized Uzbek group. Differences in this sample may thus stem from sampling variability. Moreover, all of these differences are post-treatment and must therefore be interpreted with caution.

Finally, the Table allows us to characterize the Kyrgyz and Uzbek groups in greater detail. Uzbeks, on average, are slightly richer and have greater households. They are also more likely to reside in apartments. Their education-level, by contrast, is lower compared to the Kyrgyz sample. These differences corroborate our own qualitative evidence gathered in the field. Uzbeks in Osh are widely portrayed as active businesspeople, engaged in trade, construction and carpentry. They tend to have larger families and place less of an emphasis on education. By contrast, Kyrgyz individuals live in smaller families and prize education to a greater degree.

	Full Sample		Uzbek				Kyrgyz			
			Affected		Unaffected		Affected		Unaffected	
	N	*Mean*	*N*	*Mean*	*N*	*Mean*	*N*	*Mean*	*N*	*Mean*
Female	1100	59.4	409	57.9	469	60.3	83	68.7	139	54.7
Age (#)	1100	40.2	409	39.6	469	42.3	83	38.9	139	35.9
Children (#)	1100	2.7	409	2.7	469	2.8	83	2.9	139	2.1
Income (USD)	1100	304.3	409	317.3	469	299.3	83	291.3	139	290.4
Lived abroad	1001	26.1	395	26.3	404	25.0	75	26.7	127	28.3
Migrants	1100	1.2	409	0.4	469	0.6	83	3.4	139	3.9
Household										
HH size (#)	1100	5.9	409	6.2	469	6.2	83	5.3	139	4.5
Apartment	1100	66.0	409	79.5	469	72.5	83	44.6	139	17.3
House	1100	33.8	409	20.3	469	27.3	83	55.4	139	82.7
Owner	1100	61.2	409	58.9	469	56.9	83	72.3	139	75.5
Education										
Primary	1100	14.8	409	16.4	469	19.4	83	1.2	139	2.9
Secondary	1100	53.0	409	58.9	469	57.4	83	39.8	139	28.8
Tertiary	1100	32.2	409	24.7	469	23.2	83	59.0	139	68.3
Marital status										
Married	1100	75.0	409	76.0	469	75.9	83	74.7	139	69.1
Single	1100	13.1	409	12.2	469	10.4	83	15.7	139	23.0
Other	1100	11.9	409	11.7	469	13.6	83	9.6	139	7.9
Employment										
Employee	1100	24.0	409	22.7	469	19.8	83	36.1	139	34.5
Self-Employed	1100	13.5	409	12.2	469	12.6	83	15.7	139	19.4
Retired	1100	15.3	409	13.9	469	19.0	83	10.8	139	9.4
Housewife	1100	28.4	409	32.8	469	29.0	83	25.3	139	15.1
Student	1100	5.5	409	3.2	469	5.5	83	4.8	139	12.9
Unemployed	1100	7.5	409	6.4	469	9.2	83	3.6	139	7.9
Other	1100	5.7	409	8.8	469	4.9	83	3.6	139	0.7

Table A.2: Descriptive statistics

Notes: The Table shows the descriptive statistics of the full sample as well as for the affected and un-affected Uzbek and Kyrgyz subsamples, respectively. We report the sample size (N) and the average (Mean). All variables are in percent, unless stated otherwise.

	Uzbek Sample		Uzbek Affected		Uzbek Unaffected	
	N	*Mean*	*N*	*Mean*	*N*	*Mean*
Outcomes						
PD ingroup	878	62.8	409	56.7	469	68.0
PD outgroup	878	55.6	409	51.3	469	59.3
DG ingroup (Soms)	878	26.5	409	18.8	469	33.2
DG outgroup (Soms)	878	24.1	409	16.4	469	30.7
Confounders (2009)						
Nighttime lights (0-61)	878	53.1	409	54.1	469	52.2
Historic wealth (1-5)	878	3.7	409	3.8	469	3.6
Police station	878	20.5	409	25.7	469	16.0
Hospital distance (km)	878	2.0	409	1.7	469	2.2
Leadership (1-5)	878	3.2	409	3.3	469	3.0
Floor area ratio	878	85.1	409	84.8	469	85.3
Street width (2-18)	878	5.2	409	5.6	469	4.9

Table A.3: Measurement

Notes: The Table shows the descriptive statistics of the outcome and mechanism measures as well as the potential pre-treatment confounders. We report the sample size (N) and the average (Mean) for the full sample as well as for the affected and un-affected Uzbek samples, respectively. All variables are in percent, unless stated otherwise.

	Affected
Female	0.038
	(0.050)
Age	−0.004
	(0.002)
Children	0.010
	(0.012)
Income	0.0001
	(0.0001)
Lived abroad	−0.005
	(0.045)
Migrants	−0.029
	(0.020)
HH size	−0.014
	(0.008)
Apartment	0.123**
	(0.046)
Owner	0.081*
	(0.040)
Education primary	−0.111
	(0.060)
Education secondary	−0.008
	(0.045)
Marital married	−0.012
	(0.060)
Marital single	0.125
	(0.099)
Employment employee	−0.093
	(0.078)
Employment self employed	−0.138
	(0.085)
Employment retired	−0.127
	(0.098)
Employment housewife	−0.101
	(0.088)
Employment student	−0.428***
	(0.124)
Employment unemployed	−0.225*
	(0.092)
N	799
Adjusted R^2	0.025

Table A.4: Regression of destruction on individual-level covariates

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the destruction dumy on the indicated individual-level covariates. *p<0.05; **p<0.01; ***p<0.001.

A.6 Scripts for experimental games

Prisoner's dilemma (PD) introduction I will now ask you to do four tasks, for which you will be paid at the end of the interview. This money is your compensation for taking part in the interview. How much **exactly** you earn will depend on your decisions, so please pay close attention to the instructions that I'm going to read.

At the end of the interview we will only see the **sum** of what you have earned for all tasks, so I won't know what you have decided in each task. Only the researchers responsible for this project will know your decisions in each task.

An important point is that there are no right or wrong decisions in these tasks. Please just decide whatever you think is best for you.

This first task is about deciding whether to play PLUS or MINUS in a game together with a partner. Before coming here, other enumerators asked Kyrgyz and Uzbek participants of this study from other neighborhoods in Osh to make the same decision you will shortly make. One of these persons will be your partner for the first task. The computer will choose who exactly.

For this task, both you and your partner receive 60 Som from us to play in the game. Depending on what you decide, you can earn different amounts of money:

1. If you and your partner both decide to play MINUS, each of you just keeps the 60 Som that you received from us.

2. If you both decide to play PLUS, we add some money, and each of you receives 80 Som.

However, it is also possible that you and your partner will make different decisions, so there are two more possible options:

3. If you decide to play PLUS, but your partner plays MINUS, you receive 20 Som, while your partner receives 100 Som.
4. On the other hand, if you play MINUS, but your partner plays PLUS, you receive 100 Som, while your partner receives 20 Som.

Are these choices clear?

PD first choice Let's move on to your decision. The computer has just chosen that your partner for this task is a Kyrgyz/Uzbek [randomized] participant of this study.

Your partner already made his/her decision previously and the computer knows what he/she decided. He/she only knew that you would be a Kyrgyz/Uzbek [participant's ethnicity] participant of the study. I did not interview your partner, so I cannot tell you what he/she decided.

The computer will show us how much you have earned only at the end of the interview. I will pay you in cash then. We will also send a SMS to your partner informing him/her about your decision. We will pay him/her in phone credit. We will not tell your partner anything about you.

I will now hand over the tablet to you for you to make your decision.

Make your decision and swipe the screen.

Do you want to play PLUS or MINUS in the game with your Kyrgyz/Uzbek [randomized] partner?

Before we move on to the second decision, I would like you to guess what your partner chose.

I guess my partner chose: PLUS/MINUS

PD second choice Now, we would like you to play exactly the same game with another partner. This time the computer has chosen that your partner is a Uzbek/Kyrgyz [randomized, different from first choice] participant of our study. Again, the computer already knows your new partner decided. Your partner only knew that you would be a Uzbek/Kyrgyz participant of the study.

Once you have made your decision, we will send a SMS to your new partner informing him/her about your decision and paying him/her in phone credit. We will not tell your partner anything about you. Please decide if you want to play PLUS or MINUS in the game with your new partner.

Do you want to play PLUS or MINUS in the game with your Uzbek/Kyrgyz [randomized, different from first choice] partner?

Again, I would like you to guess what your partner chose.

I guess my partner chose: PLUS/MINUS

Dictator game (DG) introduction We would now like to ask you to do two other simple tasks. I will pay you 100 Som in cash for doing each of these tasks. Actually, this money is already yours.

You can keep all this money for yourself, or give some part of it to other persons who participated in our survey before.

In the first task, you can give some part of your money to a Kyrgyz/Uzbek [randomized] participant. In the second task, you can give some part of your money to a Uzbek/Kyrgyz [randomized, different from first choice] participant.

If you decide to give anything to these persons, we will send the money to their phone credit, but we will not tell them anything about you.

DG first choice For the first task, you are given 100 Som. Please decide how much of this amount you want to give to a Kyrgyz/Uzbek [randomized] participant of the study, if any, by tapping on the respective amount.

I choose to give the following amount to the Kyrgyz/Uzbek [randomized] participant: [0/5/10/15/20/25/30/35/40/45/50 KGS]

DG second choice For the second task, you are given another 100 Som. Please decide how much of this amount you want to give to a Uzbek/Kyrgyz [randomized, different from first choice] participant of the study, if any, by tapping on the respective amount.

I choose to give the following amount to the Uzbek/Kyrgyz [randomized, different from first choice] participant:

I choose to give the following amount to the Uzbek/Kyrgyz [randomized, different from first choice] participant: [0/5/10/15/20/25/30/35/40/45/50 KGS]

A.7 Measurement validity

Do the experimental games capture real-life behavior? A long literature has shown experimental behavior to correlate with behavior outside the lab. Notably, Karlan (2005) finds that individuals who play cooperatively in a trust game are also more likely to repay loans in a Peruvian microcredit program. Benz and Meier (2008) show that donations in games correlate with naturally occurring decisions on charitable giving. In comparison to attitudinal measures of prosociality, or recalled acts of charity and cooperation, the advantages of using behavioral games are twofold. For one, they measure actual behavior with real monetary consequences rather than relying on self-reported behavior, which is easy to misrepresent. For another, using game behavior makes results comparable to previous research as the same game has been played in a large number of different contexts (this is particularly true for the dictator game; cp. Henrich et al. 2001). For this reason, nine out of 23 studies that investigate the link between violence and prosociality reviewed by Bauer et al. (2016) use experimental measures of prosocial behavior. Still, to corroborate that the experimental behavior captures real behavior, we draw on one self-reported behavioral item in the survey. We asked individuals: "When you interact with Uzbek/Kyrgyz/Russian citizens of Osh, do you try to use words in the Uzbek/Kyrgyz/Russian language?" Reassuringly, this self-reported measure of cooperation correlates strongly with our out-group prosociality index (T-Value of 64.9; see Table A.5).

	Use of others' language
Outgroup prosociality index	0.331***
	(0.063)
N	877
F Statistic	27.732

Table A.5: Correlation between experimental measures and self-reported behavior

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the indicated self-reported cooperative behavior measure on a out-group prosocial behavior index.

	People like me have no say in what the government does
Destruction	−0.055
	(0.102)
N	434
F Statistic	0.287

Table A.6: Correlation between destruction and government support in eastern Osh

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the indicated self-reported survey item on the destruction dummy. The sample is restricted to eastern Osh in order to address the concern that the local APC was positioned to victimize illoyal Uzbeks.

A.8 Controlling for confounders

Though there is evidence that the riot erupted unexpectedly and that target selection was haphazard, riots are not random. A variety of social, economic and political forces may explain why some areas, but not others, are exposed to violence. The simple regression is thus likely subject to confounding, i.e., causal forces that determine both victimization as well as prosocial behavior. Based on a review of the qualitative literature on the Osh riot and drawing on interviews with local experts, we distilled four plausible confounders, which we discuss in turn.

First, rioters might have chosen Uzbek neighborhoods that are more wealthy, given that this increases rioters' incentives to loot (McPhail and Wohlstein 1983; Rosenfeld 1997; Collier 2000). At the same time, a neighborhoods' level of wealth might positively affect its level of prosocial behavior (Cardenas 2003; Stark 2004). To measure wealth before the riot, we use two variables, which we report in Table A.3. First, we use nighttime lights, a relatively well-established measure of wealth (Weidmann and Schutte 2017). The average PSU nighttime light density in 2009, scored on a scale from 0 to 61, was 53.1 (*Nighttime lights*). Second, we administered a retrospective survey item, asking respondents about their respective PSU's wealth before the riot. The question was as follows: "How would you describe the economic situation of people from this neighborhood in 2009?" Answer choices, scored on a five-point scale, ranged from *very badly off* to *very well off* (*Historic wealth*; mean of 3.7). The question was prefaced with the following script in order to ensure an accurate historic recollection: "I'd like you to think back to the year 2009. In that year the last census was conducted, and we want to compare the situation then with the situation now." We combine both variables to a standardized historic wealth index.

Second, rioters may have chosen Uzbek neighborhoods with lower levels of state capacity, given that this lowers the risk of detainment (Kalyvas and Kocher 2007; Richani 2010; Gennaioli and Voth 2015). At the same time, low state capacity may affect cooperation since the state is unable to effectively enforce contracts (Banerjee and Somanathan 2007; Besley and Persson 2010). To measure state capacity before the riot, we use two variables. First, we administered a retrospective survey item that inquired about the presence of police stations in a given neighborhood before the riot. We asked: "Which of the following places were present in your immediate neighborhood in 2009?" We then recorded what percentage of respondents mentioned a police station (*Police station*; mean of 20.5). Second, we measure a given PSU's distance to the nearest hospital (Hendrix 2010). The assumption here is that hospitals are unlikely to move as a result of riots (*Hospital distance*; mean of 2.0 km). Both variables are combined to a standardized state capacity index.

Third, rioters may have chosen neighborhoods with lower levels of community policing, given that this reduces the likelihood of effective local defense (Sampson and Groves 1989; Sampson et al. 2005). At the same time, low community policing might also lower cooperation by making it impossible to punish defectors (Fearon and Laitin 1996; Hipp and Perrin 2006). To measure community policing before the riot, we use a retrospective survey item. We inquired about the power of local leaders before the riot: "How powerful were your local leaders then (in 2009)?" The answer choices were scored on a five-point scale, which ranged from *not powerful at all* to *very powerful* (*Leadership*, mean of 3.2).[14]

Fourth and last, rioters may have chosen neighborhoods that are more easily accessible, so as to minimize the risk of an ambush (Adams 1972; Watts 2010; Schutte 2015). At the same time, accessibility may also spur prosocial behavior by making interactions more feasible. To measure accessibility before the riot, we use two variables based on 2009 satellite data. First, we calculate the density of houses within a PSU in terms of its floor area ratio (*Floor area ratio*; mean of 0.15). Second, we measure the width of roads in a given PSU (*Street width*; mean of 5.2). Both variables are combined to a standardized accessibility index.

In Table A.7, we report the main regression including control variables for the four potential confounders. The Table shows that the coefficients survive virtually unchanged. The destruction coefficient continues to be significantly lower among Uzbek respondents living in damaged neighborhoods. The aggregate prosociality index (Model 5) is 0.39 standard deviations lower with a small standard error of 0.05. And again, cooperation is lower both within and across groups. The riot, seemingly, led to a breakdown of cooperation between Uzbeks and Kyrgyz as well as within the Uzbek community.[15]

14. For the construction of the indices, several missing values had to be imputed to avoid dropping outcome measures and thus compromising the representativeness of our sample. The imputation procedure is explained in the SI. We note that our results are fully robust to dropping observations for which variables are missing, as demonstrated in Figure A.19.

15. In Table 1, we re-estimate the same model controlling for a history of political mobilization, captured using the vote share of the AJ party in the 2010 election (see Online Appendix A.17). Doing so does

not change the results. This builds trust that the riot was not targeted toward Uzbek areas opposed to the government.

	Cooperation in Prisoner's Dilemma Ingroup	Investment in Dictator Game Ingroup	Cooperation in Prisoner's Dilemma Outgroup	Investment in Dictator Game Outgroup	Prosociality Index
	(1)	(2)	(3)	(4)	(5)
Destruction	-0.294^{***}	-0.516^{***}	-0.208^{**}	-0.526^{***}	-0.386^{***}
	(0.070)	(0.068)	(0.070)	(0.068)	(0.049)
Wealth index	0.460	0.521	0.818^{*}	0.589	0.597^{*}
	(0.330)	(0.320)	(0.331)	(0.320)	(0.233)
State capacity index	0.172	0.676^{***}	0.135	0.677^{***}	0.415^{***}
	(0.147)	(0.142)	(0.147)	(0.142)	(0.104)
Community policing	0.076^{*}	-0.081^{*}	0.049	-0.080^{*}	-0.009
	(0.033)	(0.032)	(0.033)	(0.032)	(0.023)
Accessibility index	0.290	-0.546	-0.166	-0.533	-0.239
	(0.309)	(0.300)	(0.310)	(0.299)	(0.218)
Constant	-0.658^{*}	0.010	-0.716^{*}	-0.047	-0.353
	(0.280)	(0.272)	(0.281)	(0.271)	(0.198)
N	878	878	878	878	878
Adjusted R^2	0.023	0.079	0.015	0.081	0.072

Table A.7: Effect of Riot Destruction on Prosocial Behavior (controlling for confounders)

Notes: The Table reports point estimates and standard errors of linear regressions of the indicated prosocial behavior outcome on the destruction dummy, controlling for the four indicated confounders. All outcomes are standardized. Models without imputing the community policing item are reported in Figure A.18, which confirms the robustness of our finding (it increases the effect sizes slightly). *p<0.05; **p<0.01; ***p<0.001.

A.9 Attrition

Even if confounders are appropriately addressed, our research design runs the risk of suffering from nonignorable attrition. Perhaps, the riot led cooperative people to leave affected areas. Any differences between affected and non-affected areas would then not be due to victimization, but due to selective migration patterns. Our study did *not* coincide with the yearly labor migration to and from Russia. Typically, local residents go to Russia in the spring and in the fall and work mostly in the construction sector. They return in the winter and summer months when many construction works are suspended due to low / high temperatures. For this reason, we conducted the survey during the summer. Four other reasons support our confidence that attrition is of minor concern.

First, we asked respondents: "Have you always lived in this house within this this mahalla / rayon or did you move between houses?" 96.0 percent of Uzbek respondents said they have never moved houses. This number is similar in destructed areas (97.6 percent). These high numbers confirm our own qualitative interviews. Despite witnessing traumatic destruction and violence, Uzbeks moved back into their old neighborhoods. They rebuilt their houses and put their lives back on track. Current satellite images, too, confirm that the destructed houses have since been rebuilt.

Second, we asked our respondents to estimate how many residents had migrated into or out of their neighborhood. We asked "Since 2009, how many people you know of have moved into or out of your apartment block / your street in your mahalla?" On average, Uzbek respondents estimated that 1.7 people had migrated into the neighborhood, while

3.0 had migrated out of the neighborhood. Within affected neighborhoods, this number was, if anything, smaller. Here, Uzbek respondents stated that 1.4 individuals had moved into the neighborhood, while 3.0 had left it. The low number thus showcases the absence of systematic migration. This evidence is in line with qualitative interviews with victims of the Osh riot conducted by other scholars. Ismailbekova (2013, 12), for instance, writes that "*Uzbeks have proved reluctant to leave the Osh area. Uzbeks have a long history of living in the region of Osh; strong emotional and historical sentiments bind them to the region and its graveyards and sacred sites*."

Third, as stated, we fielded an additional small-N telephone survey in 2018 to further explore the potential for non-ignorable attrition. To do so, we asked respondents whether they, themselves, had lived outside of Osh during the year 2010. Reassuringly, self-reported migration is highly similar across Uzbeks from victimized and non-victimized areas (29.4 and 30.5 percent, respectively). And, we also asked victimized individuals how many household members had left the city in 2010. The average is a rather low 0.45, of which an average of 0.31 returned. These numbers thus buttress the qualitative impression that victims promptly returned to Osh.

Fourth, in order to assess the degree to which our evidence is sensitive to the inclusion of "outsiders" (i.e., the 2-3 percent of respondents not born in the sampled neighborhoods), we estimate our preferred model using extreme value bounds. One might argue that prosocial individuals migrated to safer areas of Osh, while anti-social individuals migrated into the destructed areas. We have no evidence, whatsoever, that this took place. Still, in Table A.8, we re-estimate our benchmark model (Table A.7) assigning migrants in destructed areas the highest possible outcome for the four cooperation items, and migrants in non-destructed areas the lowest possible outcome. The results show that the findings are robust to this harsh imputation strategy.

A.10 Adjusting for spatial autocorrelation

Riots typically exhibit pronounced spatial clustering (Field et al. 2008). In the statistical models presented in the main body, we have assumed that individuals or PSUs are independent. The map in Figure 2, however, demonstrates that victimization was spatially clustered. If spatial autocorrelation is present and not adjusted, it leads to incorrect estimates of coefficients and standard errors. There are several reasons why the chance of one Uzbek house being victimized depends on its neighboring houses' level of victimization. Most visibly, rioters set houses on fire, which caught on to neighboring houses. More generally, a host of social variables that might have attracted rioters (discussed above) could be spatially clustered. This includes, i.a., individuals' ethnic status—one key determinant of victimization.

To formally estimate the degree of spatial autocorrelation in our sample, we draw on common practices in geostatistics. In a first step, we estimate the degree of spatial correlation between neighboring units and prosocial behavior. To do this, we must define a spatial connectivity matrix for the PSUs. To ensure a robust measurement of connectedness, we rely on two connectivity measures. First, we use geodesic distance in kilometers. Second, we use the travel time between PSUs in minutes, estimated using Open Street Maps. Using these two measures, we can calculate the correlation of prosocial behavior between a given PSU and its spatially lagged neighbors. "Neighbors," here, are defined as PSUs that lie in a specific meter- or second-band. We vary these bands from the smallest computationally feasible band to the largest possible band (i.e., the entire city).[16] Put differently, we use an iterative procedure to determine spatial autocorrelation.

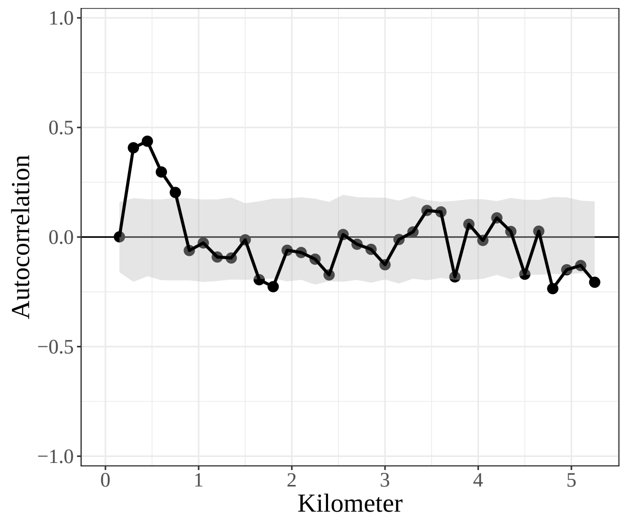

Figure A.7: Spatial autocorrelation: Geodesic distance (km)

Notes: The Figure plots spatial autocorrelation (dots) between neighboring PSUs for the prosociality index in the Uzbek sample for the indicated meter-bands. The smallest possible band is 150 meters, while the largest possible band is 5.5 km. The grey line plots 95 percent confidence intervals estimated using Monte Carlo simulations.

We assess spatial autocorrelation in Figures A.7 and A.8. The two Figures show that spatial autocorrelation, across most bands, is low. The correlation between neighboring units typically hovers around 0. Yet, for small bands between 0 and 500 meters or 0 and 2 minutes, we detect noticeable positive autocorrelation. This, as was argued above, is not surprising. But, it underlines the need to adjust our models for interdependence between neighboring PSUs. Importantly, having uncovered a salient autocorrelation threshold, we

16. For the geodesic distance measure the bands range from 150 meters to 5.5 km. For the travel distance measure the bands range from 0.2 minutes to 12 minutes.

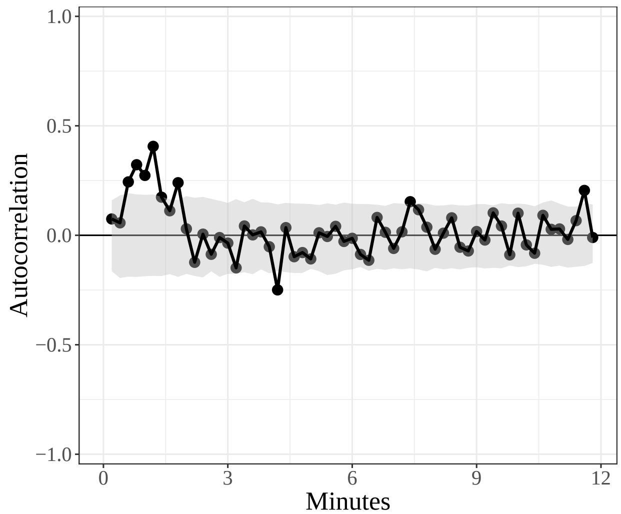

Figure A.8: Spatial autocorrelation: Travel time (minutes)

Notes: The Figure plots spatial autocorrelation (dots) between neighboring PSUs for the prosociality index in the Uzbek sample for the indicated meter-bands. The smallest possible band is 20 seconds, while the largest possible band is 12 minutes. The grey line plots 95 percent confidence intervals estimated using Monte Carlo simulations.

are in a better position to choose the appropriate level at which to cluster standard errors (more below; Conley 1999; Ward and Gleditsch 2002).

To more formally estimate the degree of spatial autocorrelation in our sample, we use Moran's I (Moran 1950). It is defined as

$$I = \frac{\sum_i \sum_j \mathbf{w}_i j (y_i - \mu)(y_j - \mu)}{\sum_i (y_i - \mu)^2} \tag{3}$$

where y is the aggregate prosociality index and μ is the average of y in our sample. Estimating Moran's I, again, requires that we define a connectivity matrix $\mathbf{w}$, which denotes the degree to which PSUs are connected. In the pre-registration document, we laid out two measures. First, we use PSU adjacency. We construct a matrix in which we code

PSUs adjacent to a target PSU as 1, and others as 0. Second, we use the average travel time in minutes between the PSUs, which we calculated using Open Street Maps. This second measure more appropriately captures day-to-day connectedness (Gilardi 2015), while the first—while simple—imposes arbitrary cut-offs. As a third additional measure, we use the aforementioned geodesic distance between PSUs in km.

When estimating Moran's I, we detect significant spatial autocorrelation for two of the three adjacency matrixes. Geodesic distance is associated with an insignificant p-value of 0.113. The travel distance matrix, by contrast, yields a p-value of 0.002. The rather simplistic adjacency matrix yields a p-value below 0.000. The latter result thus falls in line with the evidence presented in Figures A.7 and A.8. Our sample is spatially correlated within rather small clusters within the city of Osh.

Having demonstrated that there is spatial autocorrelation, we proceed to estimate a pre-registered and widely used "spatial model error" model (see Anselin 1988). We model prosocial behavior with the following linear equation:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 \text{Destruction}_i + \beta_2 \text{Wealth}_i + \beta_3 \text{State Capacity}_i \\ + \beta_4 \text{Community Policing}_i + \beta_5 \text{Accessibility}_i + \varepsilon_i + \lambda \mathbf{w}_i \varepsilon \end{aligned} \tag{4}$$

The variables are the same as in equation 2, while w captures our three connectivity matrices. In doing so, we must point out that the above model is very punishing. It aggregates all observations at the PSU level, which reduces the N from 878 to 196.[17] And, we adjust standard errors for spatial autocorrelation using three separate measures of connectedness.

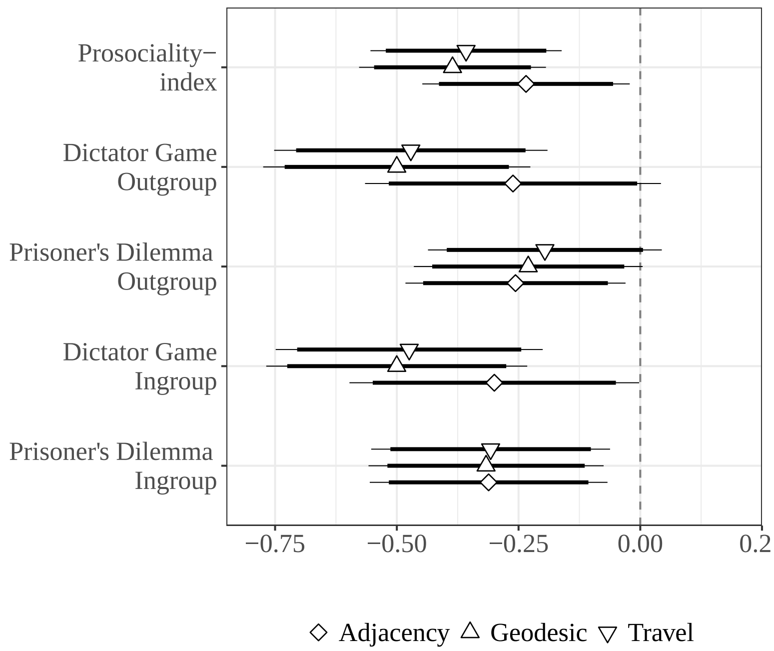

Figure A.9: Effect of Riot Destruction on Prosocial Behavior (autocorrelation adjusted)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy, adjusting standard errors using the indicated geographic connectedness matrix and aggregating individual-level outcomes at the PSU-level. All outcomes are standardized. All control variables are included. The models draw on 190 degrees of freedom.

We report the results from this model using a coefficient plot in Figure A.9. The plot demonstrates that adjusting standard errors for spatial autocorrelation increases the variance considerably. Nevertheless, we continue to see a strong and statistically significant

17. This number is based on imputing the community policing item (see Footnote 8). Without imputation, the number reduces to 133. Below, we also show that all results are robust to non-imputation (see Figure A.19).

reduction in prosocial behavior. The aggregate index is roughly 0.4 standard deviations lower, with a standard error of 0.17. The effect is remarkably consistent regardless of the connectivity matrix used. The most punishing (and most crude) adjustment is the adjacency matching. The least punishing matrix is the geodesic distance matrix. As a whole, the spatially adjusted models thus confirm our headline finding that prosocial behavior is noticeably lower in riot-ridden neighborhoods.

In an additional robustness check, we estimated equation 2 using spatial autocorrelation robust standard errors (Conley 1999).[18] We use a threshold of 1km—motivated by visible autocorrelation within the 1km band when using geodesic distance (see Figures A.7 and A.8)—within which standard errors are assumed to be correlated. Using this estimation procedure increases the standard error of the destruction dummy from 0.049 (column 5, Table A.7) to 0.141. Thus, while the variance increases, its estimates remain statistically significant.

In a final robustness check, we address spatial autocorrelation by modeling prosocial behavior as affected by nearby PSUs. Doing so addresses the critique that the spatial error model merely adjusts standard errors, but not point estimates. We use a "spatial autoregressive" model, which allows spatially connected units to affect the outcome of unit i (see, Beck, Gleditsch, and Beardsley 2006). The model is as follows:

18. This analysis as well as the next were not pre-registered. But, we believe they represent sensible additional checks to buttress the robustness of our findings.

$$
\begin{aligned}
Y_i = \beta_0 + \beta_1 \text{Destruction}_i + \beta_2 \text{Wealth}_i + \beta_3 \text{State Capacity}_i \\
+ \beta_4 \text{Community Policing}_i + \beta_5 \text{Accessibility}_i + \varepsilon_i + \kappa \mathbf{w_i y}
\end{aligned} \tag{5}
$$

We report the results from this regression in Figure A.10. It demonstrates that fitting a spatial lag model does not alter our substantive conclusions—regardless of the connectivity matrix used. We continue to see a statistically significant reduction in prosocial behavior with modest accompanying standard errors.

A.11 Matching

We further improve the causal inferences we aim to draw using matching. To do so, we estimate a logistic regression of the binary destruction indicator on the four pre-treatment confounders. We then use the estimates from the logit model as the probability of a given individual of being exposed to the treatment. We match individuals with different treatment statuses, but highly similar propensity scores. We then simply estimate the difference in means across the two samples using a simple linear regression.

We report the results from matching in Figure A.11. Our results discussed thus far are confirmed. Matching further reduces the variance surrounding our key estimates. The aggregate prosociality index is 0.36 standard deviations lower among individuals residing in damaged PSUs (standard error of 0.04). Cooperation in the prisoner's dilemma and the investment in the dictator game, too, are significantly lower among victimized respondents.

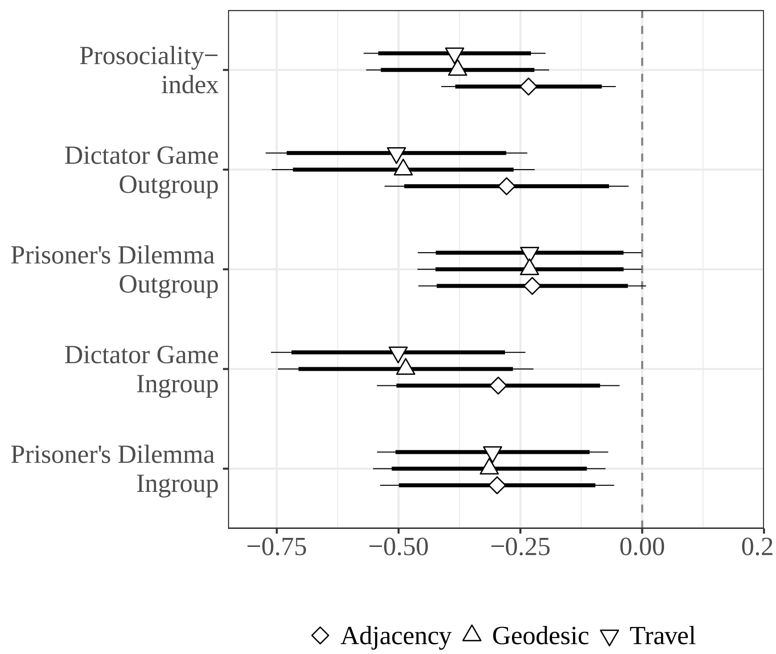

Figure A.10: Effect of Riot Destruction on Prosocial Behavior (spatial lags)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy, including spatial lags, using the indicated geographic connectedness matrix. All outcomes are standardized. All control variables are included (imputing missing values for the community policing variable). The models draw on 128 degrees of freedom.

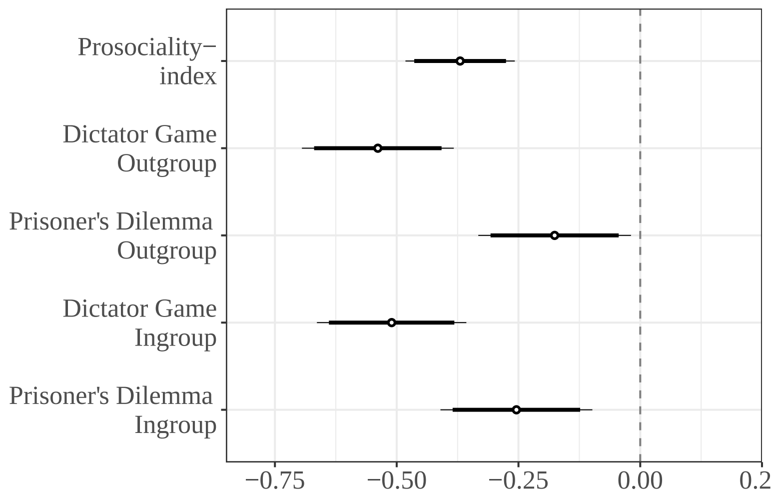

Figure A.11: Effect of Riot Destruction on Prosocial Behavior (matching)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy with the propensity score matched sample. All outcomes are standardized. All models draw on 620 degrees of freedom, using a caliper of 0.05.

	Cooperation in Prisoner's Dilemma Ingroup	Investment in Dictator Game Ingroup	Cooperation in Prisoner's Dilemma Outgroup	Investment in Dictator Game Outgroup	Prosociality Index
	(1)	(2)	(3)	(4)	(5)
Destruction	-0.197^{**}	-0.408^{***}	-0.107	-0.405^{***}	-0.279^{***}
	(0.070)	(0.068)	(0.070)	(0.068)	(0.051)
Wealth index	0.270	0.206	0.416	0.264	0.289
	(0.331)	(0.323)	(0.332)	(0.323)	(0.241)
State capacity index	0.254	0.737^{***}	0.219	0.749^{***}	0.490^{***}
	(0.145)	(0.142)	(0.146)	(0.142)	(0.106)
Community policing index	0.071^{*}	-0.076^{*}	0.048	-0.077^{*}	-0.008
	(0.033)	(0.032)	(0.033)	(0.032)	(0.024)
Constant	-0.452	-0.032	-0.529^{*}	-0.082	-0.274
	(0.262)	(0.256)	(0.263)	(0.256)	(0.191)
N	878	878	878	878	878
Adjusted R^2	0.013	0.059	0.005	0.059	0.043

Table A.8: Regression of prosocial behavior on destruction controlling for confounders (Manski bound imputation for migrants)

Notes: The Table reports point estimates and standard errors of linear regressions of the indicated prosocial behavior outcome on the destruction dummy, controlling for the four pre-registered confounders. All outcomes are standardized. Self-described migrants in affected areas are imputed with the highest possible cooperative outcome, while migrants in non-affected areas are imputed with the lowest possible cooperative outcome. $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$.

A.12 Instrumental variable

IV assumptions

To use the location of the military barracks as an instrumental variable, we must invoke five assumptions. We can only briefly discuss the assumptions in the main text and refer readers to the SI for a more detailed discussion.

First, we must assume that the location of the barracks is orthogonal to the unmeasured causes of the outcome. Above, we laid out four variables that may jointly have affected riot intensity and prosocial behavior (Online Appendix A.8). Importantly, we see no plausible reason to believe that a) wealth, b) state incapacity, c) low levels of social capital or d) the accessibility of houses should lead a government to prefer one military location over another. Rather, the location of military barracks (which, in this case, were set up by the USSR decades ago) likely follow logistical rationals such as sufficient space and distance to nearby neighborhoods. To quantitatively buttress the assumption, we assess whether the potential pre-treatment confounders jointly predict our instrument. We provide the results from this model in Table A.10. It shows that all but one estimate are insignificant. Moreover, the coefficients are not consistently in line with our theoretical considerations.

What, however, if the Kyrgyz military strategically positioned the APCs so as to victimize some Uzbek mahallas, while sparing Uzbeks they deemed loyal? This logic does not apply to the barrack in western Osh, which, as noted, had been present for decades. But, the second APC location, the Furkhat roundabout, could theoretically be plagued by such confounding. Three reasons make this selection pattern unlikely. First, the APCs stolen at the roundabout were sent from the province of Jalal-Abad, taking the most direct path toward Osh. There is thus no evidence that the APCs were strategically placed. Second, we collected precinct-level voting data from elections right after the riot. Based on this data, we can rule out that non-victimized areas were more likely to vote in favor of the local government (see Online Appendix A.17; note, however, that this analysis is post-treatment). Third, in Table A.6 we show that respondents in eastern Osh in victimized PSUs were *not* more likely to agree with the statement "people like me have no say in what government does." This, then, suggests that loyalist Uzbeks were unlikely to be underrepresented in victimized areas, implying that they were not systematically protected from local authorities. Taken together, there is thus no evidence that the second APC location was "selected." Even so, to rule out any remaining concerns, in Figure A.12 we restrict our IV analysis to western Osh—the area where selection could not have taken place—and find treatment effects, if anything, to be larger.

Second, we must assume that the APCs' locations have no direct effect on the outcome other than through the channel of victimization. Again, we see no compelling theoretical reasons why closeness to military barracks should affect intra- and inter-ethnic prosociality. Both barracks are outside the city. By all accounts, the exchange between the barracks and the city is low. The military—as the riot showed—takes a distinctly passive role in Osh. While this assumption is untestable, we try to buttress our reasoning below by constructing a falsification test. We show that distance to the barracks does not predict prosocial behavior in a sample of 136 nearby villages and towns (see Online Appendix A.12 and Table A.11). The null finding also holds when restricting the sample to villages within a mere 10 kilometer radius around the city of Osh (see Table A.12). Moreover, to explore whether the military interacts with Osh residents, we asked respondents whether they know why and when the barracks were set up (see Online Appendix A.14). 77 percent of respondents are not aware that there are any barrack in their vicinity. Even in interviews with a former employee of the Kyrgyzstan Emergency Situations Ministry, we were unable to determine when the barracks were set up (besides obtaining vague information that they are from the Soviet era). This confirms the passive role of the military in Osh and its detachment from residents' daily life.

Third, we must assume that the location of barracks is, indeed, correlated with destruction. We test this assumption in Table A.9. The first column reports the individual-level data set, while the second column aggregates observations at the PSU-level. Both regressions confirm a strong correlation between distance to the nearest barrack and the destruction indicator. The F-Stat is 271.9 and 47.9, respectively. The magnitude of the F-Stat makes it exceedingly difficult to believe that the correlation is purely coincidental. As such, it corroborates the qualitative evidence cited above, which ascribes the APCs a central role in facilitating the riot.

Fourth, we must assume that there are no defiers. Given our "treatment," it is highly unlikely that neighborhoods are destroyed only if *not* assigned to be attacked. The treatment, in other words, is not available to those not assigned to be attacked.

Last, we must assume that any given observation is unaffected by treatments assigned or received by other units. We have addressed this issue by estimating spatial error and spatial lag models (Online Appendix A.10). We therefore corroborate the IV estimates using a spatial error model. In addition, we provide evidence from a randomization inference test (more below).

	Destruction	
	Individual-level	*PSU-level*
Closeness to nearest barrack	0.194***	0.152***
	(0.012)	(0.022)
Constant	1.244***	0.898***
	(0.049)	(0.096)
N	878	196
Adjusted R^2	0.236	0.194
F Statistic	271.856***	47.894***

Table A.9: First stage regression of destruction on barrack distance

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the destruction dumy on the distance indicator to the nearest barrack. *p<0.05; **p<0.01; ***p<0.001.

	Distance to nearest barrack
Nighttime lights	−0.001
	(0.014)
Historic wealth	−0.208
	(0.119)
Hospital distance	0.829***
	(0.065)
Police station	0.036
	(0.234)
Leadership	0.105
	(0.121)
Floor area ratio	0.771
	(2.004)
Street width	0.036
	(0.061)
Female	0.323
	(0.234)
Age	0.003
	(0.007)
Father's education	0.016
	(0.058)
East of main river	−0.389
	(0.216)
Constant	3.535**
	(1.120)
N	157
R^2	0.562
F Statistic	16.9

Table A.10: Regression of the instrument on pre-treatment covariates

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the instrument on the indicated pre-treatment covariates. *p<0.05; **p<0.01; ***p<0.001.

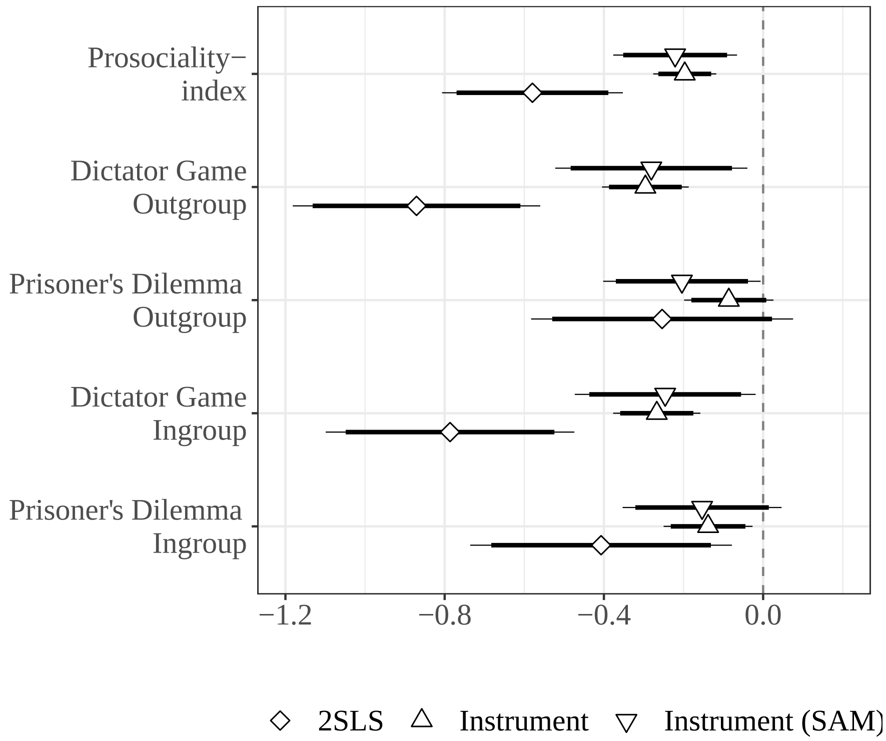

Figure A.12: Effect of Riot on Prosocial Behavior (IV; western Osh)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the distance to the closest barrack measure (instrument) or the destruction dummy instrumented with the distance measure (2SLS). SAM refers to a model in which standard errors are adjusting for spatial autocorrelation using the travel time connectivity matrix (see Online Appendix A.10). All outcomes are standardized. The sample is restricted to western Osh. All models draw on 503 degrees of freedom, except for the SAM models, which are aggregated at the PSU-level (89 DoF).

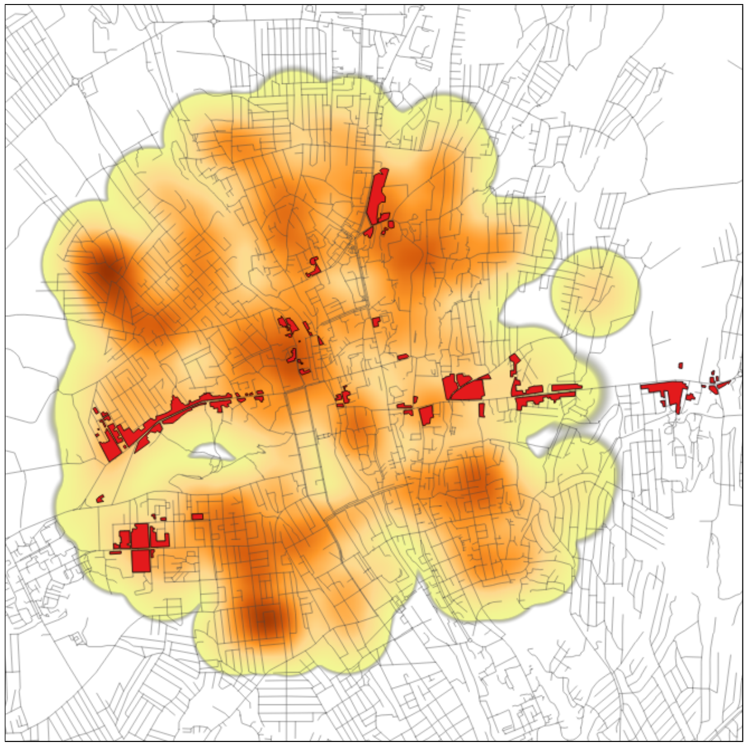

Figure A.13: Heat map of prosociality

Notes: The Figure plots the average of the standardized prosociality index for 300 meter buffer areas around each interview location.

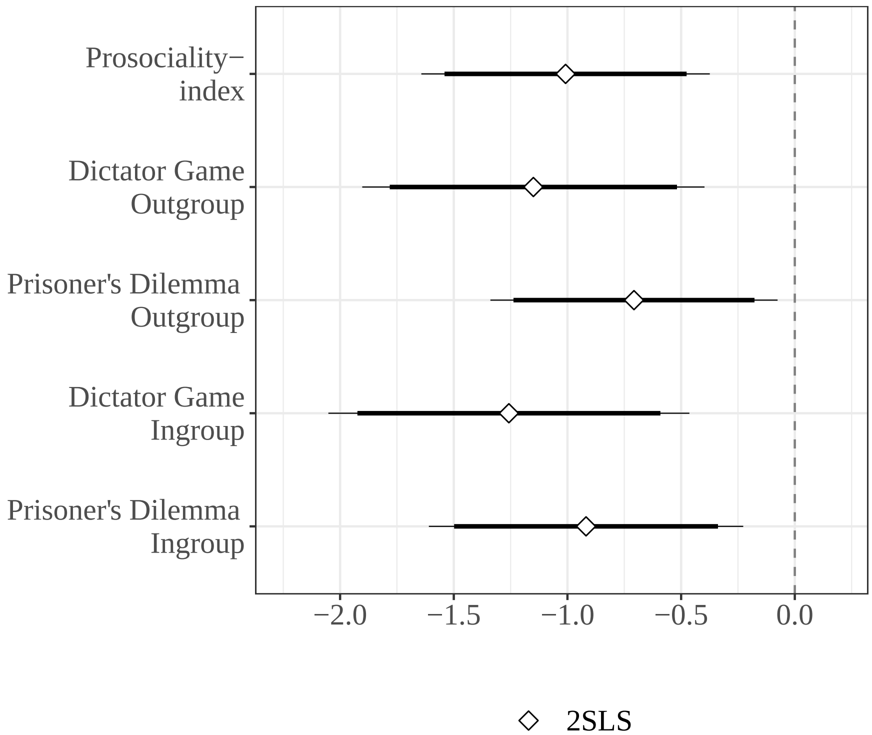

Figure A.14: Effect of Individual Victimization on Prosocial Behavior (IV)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the property loss index instrumented with the distance measure (2SLS). All outcomes are standardized. All models draw on 876 degrees of freedom.

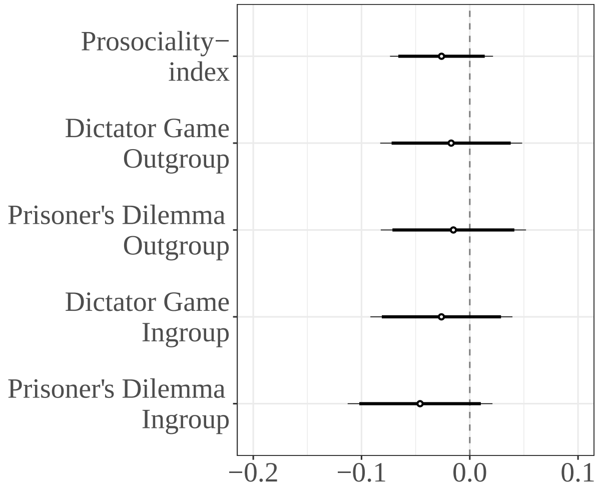

Figure A.15: Interaction effect of Riot Destruction × Historic Wealth on Prosocial Behavior (OLS)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of OLS regressions of the indicated outcomes on the destruction dummy interacted with historic wealth. All variables are standardized. All models draw on 876 degrees of freedom.

Falsification test

Our IV strategy rests on two pivotal assumptions. Namely, that victimization is the only channel through which distance to the nearest barrack affects prosociality (excludability). And, that no unobserved variables affect our instrument and our outcome (independence). These assumptions are untestable. Yet, one can construct a falsification test to bolster the assumption empirically.

To do so, we test whether the instrument predicts prosociality in a comparable sample outside of Osh. To our mind, the most plausible comparison group are the villages and towns nearby Osh that were unaffected by the riot. If distance to military barracks does correlate with prosocial behavior—e.g., rowdy soldiers might deteriorate communal trust; or the military might drive up prices by being the local monopsonist—such a correlation may also explain altered levels of prosocial behavior in nearby Osh. If, by contrast, we find no meaningful correlation, this corroborates the excludability assumption.

To construct such a test, we gained access to data from the *Social Cohesion Through Community-Based Development* research project (Esenaliev et al. 2018), which administered an individual-level survey about community cooperation to a random sample of 136 villages in the surrounding areas of Osh. We use this data set to test whether closeness to the two military barracks predicts prosocial behavior outside of Osh. We measured the distance of each sampled village to the closest barrack. We then regressed two prosocial behavior items on the distance measure, clustering standard errors at the village-level, and controlling for population size and elevation.

Our preferred survey measure from the SIPRI survey, akin to donation in the dictator game, is the following question: "To how many people did you give any financial help during the last 12 months?" Our second preferred survey measure, akin to cooperation in a prisoner's dilemma, is the following question: "If you were asked to cooperate with other people in your community / neighborhood for social purposes, e.g., charity or fundraising, how likely is it that you would cooperate?" The latter question was scored on a four-point scale ranging from *very unlikely* to *very likely*.

The results from this test are given in Table A.11. Put simply, we virtually estimate a null. Distance to the barracks, in a random sample of nearby villages and towns, does not predict prosocial behavior to any meaningful degree. The R^2 for both models is below 0.01. The null finding thus bolsters the excludability assumption. The null finding also holds when restricting the sample to villages within a mere 10 kilometer radius around the city of Osh (see Table A.12).

	"To how many people did you give any financial help during the last 12 months?"	*"If you were asked to cooperate with other people in your community / neighborhood for social purposes, how likely is it that you would cooperate? cooperate?"*
Distance to nearest barrack	0.000	-0.000
	(0.002)	(0.001)
Population	0.000	0.000
	(0.000)	(0.000)
Elevation (meters)	0.000	0.000
	(0.000)	(0.000)
Constant	0.357	2.367***
	(0.405)	(0.165)
N	6,297	6,298
R^2	0.004	0.005
F Statistic	1.26	1.05

Table A.11: Falsification test

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the indicated prosocial behavior measures on the distance measure, controlling for population size and elevation. Standard errors are clustered at the village-level (136).

	"*To how many people did you give any financial help during the last 12 months?*"	"*If you were asked to cooperate with other people in your community / neighborhood for social purposes, how likely is it that you would cooperate? cooperate?*"
Distance to nearest barrack	0.053	0.093
	(0.059)	(0.055)
Population	-0.000	-0.000
	(0.000)	(0.000)
Elevation (meters)	-0.003	-0.002
	(0.002)	(0.002)
Constant	3.632	4.013*
	(1.723)	(2.064)
N	541	541
R^2	0.061	0.069
F Statistic	0.25	0.15

Table A.12: Falsification test (radius of 10 kilometers)

Notes: The Table reports point estimates and standard errors of a linear regression (OLS) of the indicated prosocial behavior measures on the distance measure, controlling for population size and elevation. Standard errors are clustered at the village-level (9).

Decision	*Expectation*			
	Non-victimized areas		Victimized areas	
	Cooperation	Defection	Cooperation	Defection
Cooperated	54.8%	13.2%	47.4%	9.3%
Defected	17.9%	14.1%	24.2%	19.1%

Table A.13: Decisions and expectations in the PD played with the ingroup

Notes: The Table shows the distribution of respondents' decisions in the PD game in relation to what they expected their co-ethnic partners to do. We can observe a shift towards more defections in response to expected defections by the partner, and towards defections in response to expected cooperation. The two distributions are different at P=0.005, Fisher's exact test, N=878.

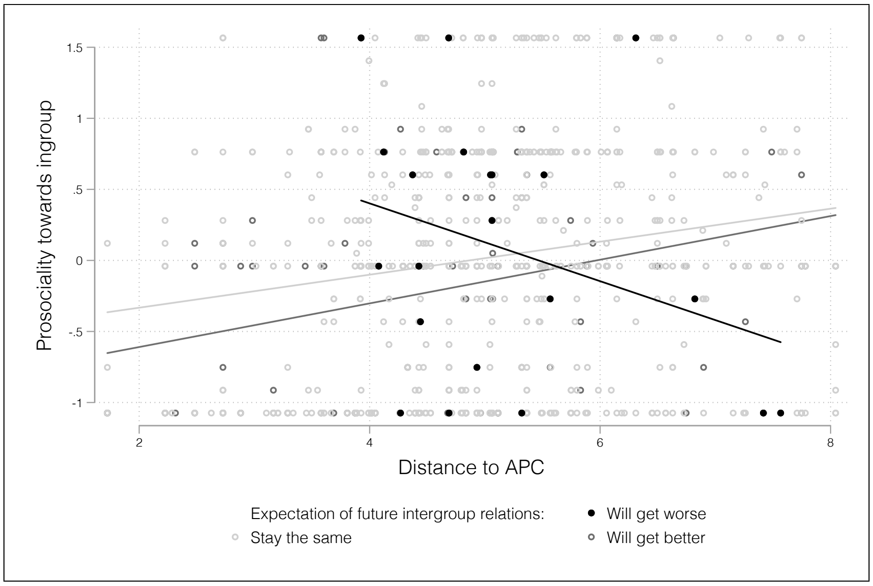

Figure A.16: Correlations between prosociality and instrument conditional on expectations

Notes: The Figure shows the correlation between the index for prosociality towards the ingroup (on the y-axis) and the instrument (distance to the nearest APC, on the x-axis), conditional on the respondents' expectations about future intergroup relations

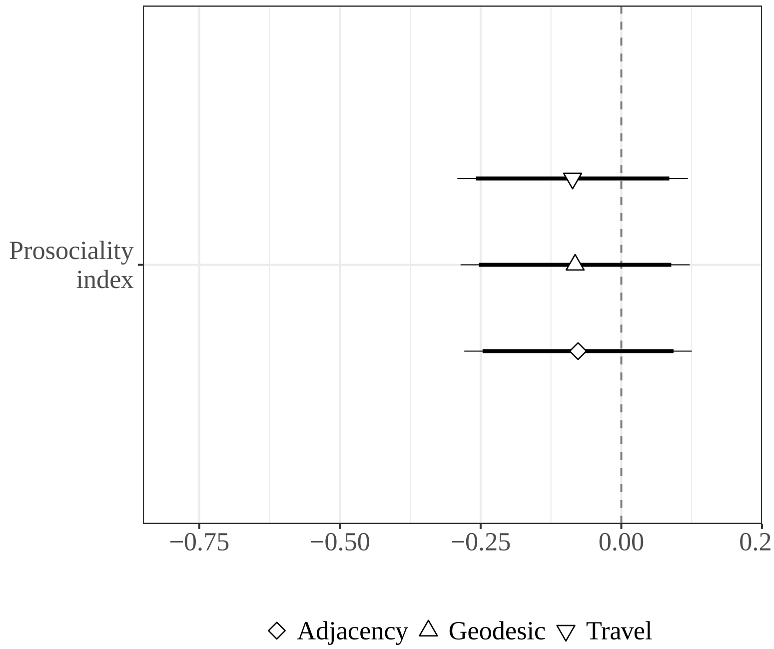

Figure A.17: Effect of Riot Destruction on Prosocial Behavior (Kyrgyz sample)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy, including spatial lags, using the indicated geographic connectedness matrix. All outcomes are standardized. All control variables are included, imputing the community policing variable. The models draw on 84 degrees of freedom.

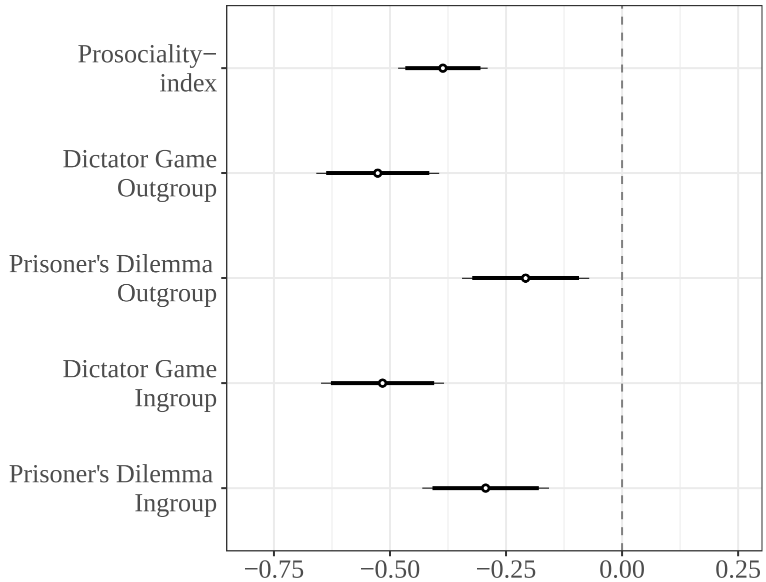

Figure A.18: Effect of Riot on prosocial Behavior (no imputation)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy, controlling for all confounders but without imputation for the community policing variable. All outcomes are standardized. All controll variables are included. The models draw on 769 degrees of freedom.

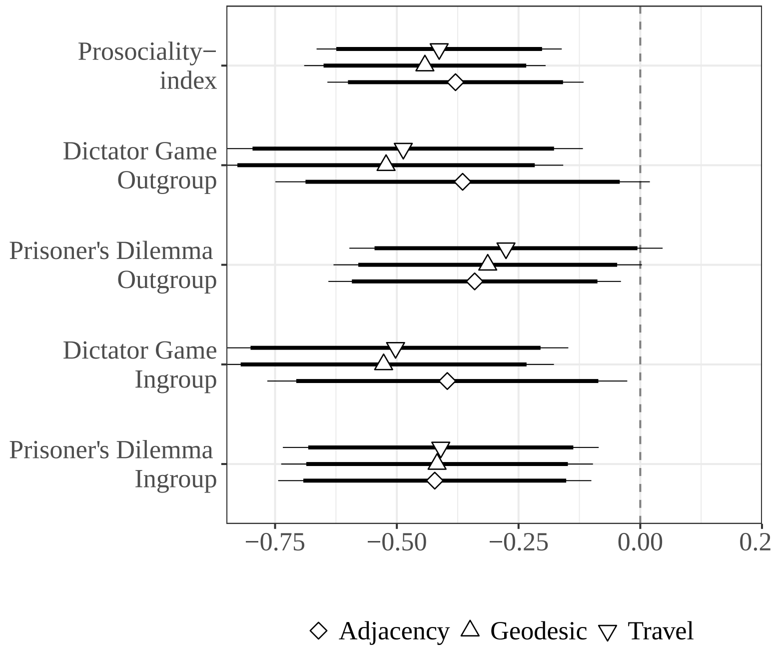

Figure A.19: Effect of Riot Destruction on Prosocial Behavior (autocorrelation adjusted, no imputation)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated outcomes on the destruction dummy, adjusting standard errors using the indicated geographic connectedness matrix. All outcomes are standardized. All controll variables are included but without imputation for the community policing variable. The models draw on 128 degrees of freedom.

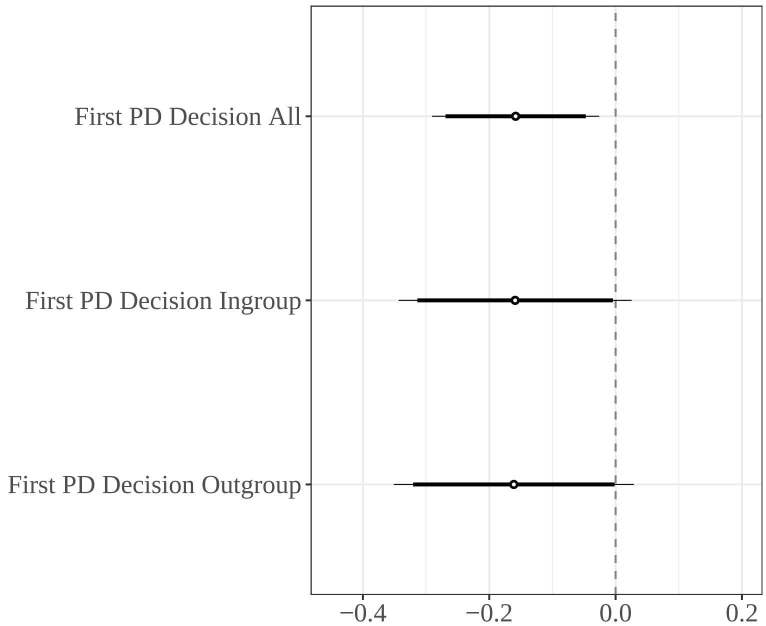

Figure A.20: Effect of Riot Destruction on Prosocial Behavior (first game decision only)

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the first decision in the behavioral games: the prisoner's dilemma (PD) played with either an in- or an outgroup member. The uppermost coefficient is for all first decisions by Uzbek respondents together (N=878). The second and third coefficient are for the split samples of respondents matched first with an ingroup member (i.e., Uzbek, N=451), and respondents matched first with an outgroup member (i.e., Kyrgyz, N=427), respectively. All outcomes are standardized.

A.13 Imputation of missing values for indices

Several of the variables that our indices are based on suffered from missing variables. A major source of missingness is the fact that some variables were added after we concluded the pilot study (n=99). Other missing values are due to non-responses. The variables that needed imputation, and the respective methods used, are the following:

Variable	**N missing values**	**Imputation method**
Ethnic identification	99	Average value from non-missing values
Coethnic hero	99	Average value from non-missing values
Historic wealth	78	Linear regression model with non-missing values of the historic wealth variable as the dependent variable, and the nightlight index and distance to hospitals as independent variables.
Strength of neighborhood leader	117	Linear regression model with non-missing values of the leadership variable as the dependent variable, and the nightlight index and indicators for the presence or absence of various state institutions as independent variables.
Future conflict	93	Average value from non-missing values
Cooperative signals	1	Average value from non-missing values

A.14 Follow-up survey

In September 2018, we fielded a small follow-up telephone survey in Osh. The reason for conducting a telephone survey was the security situation in Osh, which very volatile. The purpose of the survey was to clarify some technical details and obtain further insights into the mechanisms discovered in our qualitative interviews. We contacted all respondents who gave us their phone numbers in the original survey. In total, we contacted 596 individuals, including 421 Uzbeks. We managed to re-interview 144 Uzbek respondents (response rate of 34.2 percent). According to the survey firm, the rather low response rate is not unusual. Due to the security situation on the ground, we were not in a position to ask direct questions on victimization, or ethnic identification, or 'real-life' cooperation measures that make explicit distinctions between cooperation with Uzbek or Kyrgyz people.

That said, we obtained useful evidence on along four dimensions. First, we inquired about the location of military barracks in the vicinity of Osh. Second, we fielded a question on inter-ethnic marriages within Osh's Uzbek community. Third, we asked victims about their sources of financial support in the year covering the period of the riot. Fourth, we included items that provide a more nuanced picture of labor migrations to and from Russia. As presented in the main text, these items allow us to confirm the 'ingroupness' of the Uzbek community in Osh and the breakdown of its cooperative norms in the aftermath of the riot. The new items also alleviate possible concerns about non-random attrition and the endogeneity of the instrument.

Below we present the exact wording of the new questions:

1. “Is there a military barrack close to your place of residence?” [Yes, there is / No, there isn’t / Don’t know]

2. “Would you agree to a marriage if your daughter wanted to marry a Muslim from another ethnicity?” [Definitely yes / Rather yes / Rather no / Definitely no]

3. “Talking about your life in 2010, have you received any help in 2010 from the following sources? Mark all applicable responses.” [Family / Neighbors / Government / International NGO / Other]

4. “During the year 2010 did you live outside the city of Osh?” [Yes / No / Don’t remember]

5. “During the same year (2010), did your family members leave the city?” [Yes / No / Don’t remember]

If yes: “How many members of your family left the city?”

If > 0: “Since then, how many of them have returned?”

	Uzbek		Uzbek			
	Sample		Affected		Unaffected	
	N	*Mean*	*N*	*Mean*	*N*	*Mean*
Aware of barracks in vicinity	144	0.23	59	0.29	85	0.19
Non-coethnic Muslim spouse (1-4)	136	2.82	54	2.72	82	2.88
Financially helped by coethnics	144	0	59	0	85	0
Fled from Osh	144	0.30	59	0.31	85	0.29
Family member fled (#)	142	0.49	58	0.45	84	0.52
Family member returned (#)	138	0.35	55	0.31	83	0.37

Table A.14: Descriptive statistics (follow-up)

Notes: The Table shows the descriptive statistics of the indicated outcomes from the telephone follow-up survey. We report the sample size (N) and the average (Mean) for the full sample as well as for the affected and un-affected Uzbek samples, respectively. All variables are in percent, unless stated otherwise.

A.15 Pre-registered mechanisms

In the following, we lay out four pre-registered mechanisms, we had included in the pre-analysis document. We include them to transparently report all hypotheses and findings. However, given our finding of *reduced* ingroup cooperation, the mechanisms have no explanatory power. Moreover, the measurement is solely based on survey questions, and thus subject to common concerns regarding social desirability or demand effects.

Risk-preferences

The first potential causal channel linking victimization to prosocial behavior are risk preferences. Conflict exposure has been shown to increase individuals' appetite for risk. Risk preferences, in turn, are associated with cooperative behavior (Karlan 2005; Schechter 2007; Dohmen et al. 2011). Voors et al. (2012), for instance, find an increased willingness to take risks among victims of war violence in Burundi. They argue that this result can be explained in terms of "personal growth" (Tedeschi and Calhoun 2004). Following this view, violence exposure may lead individuals to change how they scale relative risks. The risk of exploitation in cooperative exchange will appear small if weighted against the risk of losing one's home or beloved ones. Relatedly, experiencing large-scale violence may also enable individuals to better handle the relatively minor misfortunes of day-to-day cooperation. This may make them relatively less afraid of self-exposure to the possibility of exploitation in cooperative exchange.[19] Taken together, an increased appetite for risk should therefore increase prosocial behavior both toward the ingroup as well as the outgroup.

Ethnicization

The second potential causal channel linking victimization to prosocial behavior is a more ethnicized social landscape (Choi and Bowles 2007). In reaction to ethnic violence, existing social divisions based on class, caste or religion will lose importance. Ethnic identities, by contrast, get reinforced (Donald L. Horowitz 1985). After all, one hallmark of ethnic riots is that violence is highly targeted toward a specific ethnic group. Perpetrators carefully screen potential victims for ethnic markers to avoid assailing members of the "wrong" group. In Osh, attackers showed no mercy towards Uzbeks, but systematically spared their Russian neighbors (Kyrgyzstan Inquiry Commission, later KIC, 2011, 30). Indeed, during the riot, non-Uzbek residents of apartment buildings marked their houses as "KG" (Kyrgyz) or "RUSSKIE" (Russian) to avoid assault. Potential victims had their ethnicity literally forced upon them, no matter the degree to which they self-identified as Uzbeks or Russians before the riot. Higher levels of ethnicization might thus lead to increased levels of prosocial behavior toward the ingroup (Yamagishi and Kiyonari 2000).

The reverse side of this identification process is a growing hostility toward members of the outgroup (Miguel, Saiegh, and Satyanath 2011). Psychologists show how the

19. We should point out that the risk-cooperation link is disputed by an alternate set of studies, which find no relationship between risk preferences and cooperative behavior (Eckel and Wilson 2004).

distress caused by experiencing violence and the fear of future aggression directly translate into negative outgroup attitudes (Canetti-Nisim et al. 2009). More broadly, riots sow seeds of distrust between ethnic groups (Donald L Horowitz 2001; Beber, Roessler, and Scacco 2014; Rohner, Thoenig, and Zilibotti 2013). They also lead to increased residential segregation (Donald L Horowitz 2001). In search of safety, members of the victimized group isolate themselves from members of the perpetrator group. This reduces the opportunity for positive interethnic contact that could otherwise soothen heightened ethnicization (Enos and Gidron 2016). As a result, ethnicization should therefore lead to reduced prosocial behavior toward the outgroup.

Expectation of future conflict

A third potential causal channel linking victimization to prosocial behavior are changed expectations about the likelihood of future conflict (Pearson 2001). Riots make visible the high cost and likelihood of intergroup conflict. This holds particularly true for those directly affected. In Osh, victims are constantly reminded about the possibility of renewed escalation. Some areas of the city still bear signs of destruction, and during our fieldwork, a monument dedicated to the victims of the riot was vandalized. Theoretically, the fear of future conflict may trigger different reactions. Realizing the necessity of good intergroup rapport may lead victims to invest into intergroup relations to signal their good intentions (Schaub 2017). Such patterns of prosociality–even in the face of traumatic hostility—are well known from classic anthropology (Mauss 1925). Expectations about future conflict may thus spur prosocial behavior toward the outgroup.

At the same time, a heightened expectation of future conflict may increase ingroup prosociality as individuals seek protection from members of their ethnic group. In light of widespread destruction of property, investment in social insurance may be perceived as more secure than investment in physical capital (Bauer et al. 2016). One way of guaranteeing this support is to make obligations among neighbors through unilaterally cooperative behavior. This type of cooperation is associated with short-term losses incurred for implicit promise of future reciprocation in terms of protection and help when in need. As a result, riots may increase ingroup cooperation by altering expectations about future conflict.

Economic interdependence

The fourth potential causal channel linking victimization to prosocial behavior is economic interdependence. Whether economic interdependence rises or falls as a result of riots, however, is unclear. On the one hand, the Osh riot led to a reshuffling of economic activity. Long-standing ethnic networks were undercut. Members of the victimized group who lost their businesses were driven to seek employment with employers of the other ethnic group (Ismailbekova 2013). Perplexingly so, the riot thus forced Uzbeks to cooperate more with Kyrgyz, and vice versa. On the other hand, riots destroy properties and livelihoods. Victimized families may therefore choose to send members of their households to work abroad, thus reducing economic interdependence (Ismailbekova 2013). In our own interviews, residents reported that members of victimized households had migrated to Russia after the riot in order to raise funds for the reconstruction of destroyed houses. Thus, while there can be little doubt that economic interdependence should increase outgroup cooperation (Jha 2013), it remains unclear *whether* riots increase or decrease such interdependence.

Taken together, the four channels demonstrate that the link between riots and prosocial behavior is muddy. Several conflicting hypotheses exist. While, as a whole, they point to increases in prosocial behavior, particularly toward the ingroup, significant uncertainty prevails. Our goal is to provide causally credible evidence about the effects of riots on prosociality within and across groups. And, in so doing, we also measure the aforementioned mechanisms in order to unpack the complex causal process from riots to prosocial behavior (see Online Appendix A.16).

A.16 Mechanism measurement

In addition to estimating the reduced-form link between the riot and prosocial behavior, we also collected measures for the aforementioned four potential mechanisms. While we cannot make a causal argument about mediation, we can nonetheless explore whether the riot affected the potential causal channels.

We measure **risk preferences** using a hypothetical lottery instrument (Eckel and Wilson 2004). Respondents were provided with four scenarios in which they could receive a smaller amount of money with certainty or a larger amount with greater uncertainty. The questions read as follows "What would you prefer? A 50% chance of receiving 1000 Som and a 50% chance of receiving nothing, or a sure payment of [50/100/200/500] Som?" We scale these items to an additive risk index (*Risk preferences*; mean of 1.6).

We measure **ethnicization** using two survey items. First, we asked respondents to choose two of their most important identities out of a list of five (Eifert, Miguel, and Posner 2010). These included gender, language (Kyrgyz or Uzbek), class, religion or the so-called *intelligentsia* (well educated). We then recorded whenever respondents mentioned language (*Ethnic identification*; mean of 39.4 percent). Second, we asked respondents to choose their two most significant historical or mythical heroes out of a list of five. They included: Amir Temur, Alisher Navoi, Manas, Prophet Muhammad, and Jesus of Nazareth. We then recorded whenever Uzbek respondents mentioned Amir Temur (*Coethnic hero*; mean of 34.5 percent). We combine both variables to a standardized ethnicization index.

We measure the **expectation of future conflict** using two survey items. First, we asked respondents "Thinking about the relations between citizens of different nationalities in Osh, how do you think the relationship will develop in the future?" The variable was scored on a three-point scale ranging from *may get better* to *may get worse* (*Future conflict*; mean of 2.8). Second, to measure the extent of cooperativeness signaling so as to avoid conflict, we asked "When you interact with Uzbek / Kyrgyz citizens of Osh, do you try to use words in the Uzbek / Kyrgyz language?" All Uzbek subjects were asked about their use of the Kyrgyz language when interacting with Kyrgyz. The variable, scored on a five-point scale, ranged from "never" to "all the time" (*Cooperative signals*; mean of 3.2). We combine both variables to a standardized future conflict index.

We measure **economic interdependence** using two survey items. First, respondents were asked "What percentage of your household income comes from money transfers from relatives who work abroad or in another Kyrgyz city?" (*Remittances*; mean of 14.1 percent). The variable captures economic disengagement, which should reduce interdependence. Second, respondents were asked "What is the ethnicity of your employer?" We recorded whenever an Uzbek mentioned a Kyrgyz employer (*Interethnic employment*; mean of 15.5 percent). We combine both variables to a standardized interdependence index.

Pre-registered mechanisms: Results

In Figure A.21, we report a coefficient plot from a regression of the indicated mechanism outcomes on the destruction dummy. We estimate our preferred and most punishing model. We include all pre-registered confounders, aggregate the data at the PSU-level and adjust standard errors for autocorrelation using the indicated spatial weight matrices. The Figure shows that the riot did not meaningfully affect the hypothesized mechanisms. The first potential channel, risk preferences, is slightly higher in damaged areas, but the estimate is noisy. The same holds true for the second causal channel, ethnicization. Here, too, we do not find significant differences between affected and unaffected areas of Osh. The third channel—expectations about future conflict—also seems unaffected by the riot. Individuals in affected areas are no less likely to expect future conflict. Finally, the fourth channel, economic interdependence is shown to be slightly higher in damaged neighborhoods. But the uncertainty around the estimates is too large to draw firm conclusions.

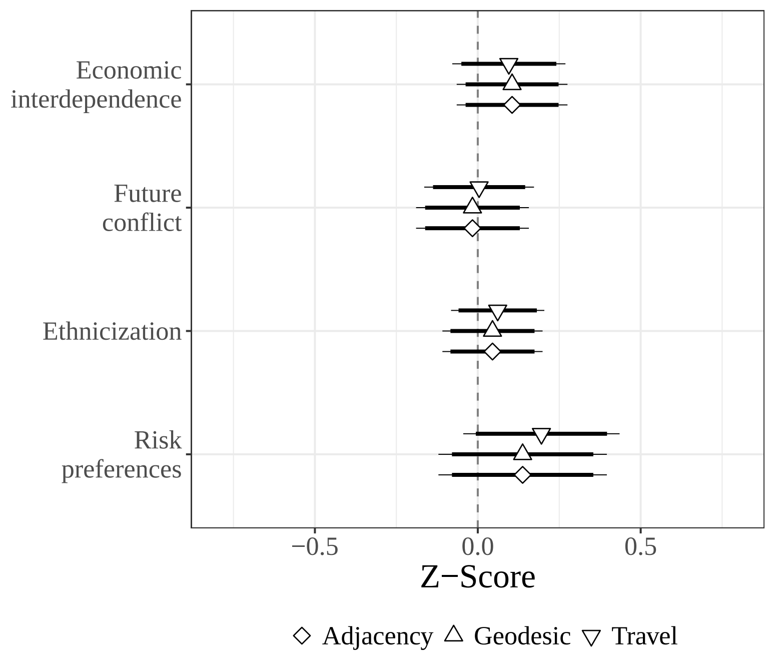

Figure A.21: Effect of riot on pre-registered mechanism outcomes

Notes: The Figure plots point estimates (dot) and 90 / 95 percent confidence intervals (thin and thick lines, respectively) of regressions of the indicated mechanism outcomes on the destruction dummy, adjusting standard errors using the indicated geographic connectedness matrix (see Online Appendix A.10) and controlling for all possible confounders (Online Appendix A.8). All outcomes are standardized. The models draw on 190 degrees of freedom.

A.17 2010 Kyrgyz parliamentary election

To explore the possibility that APCs were strategically placed, we draw on electoral data from the 2010 Kyrgyz parliamentary election. This election took place a few months after the riot (in October 2010). To our knowledge, this represents the best measure to capture political attitudes of victimized and non-victimized Uzbeks at the time of the riot.[20] If local authorities channeled violence toward "non-loyalist" Uzbeks, one would expect that Uzbeks in unaffected neighborhoods were more supportive of the pro-Bakiyev party (*Ata-Jurt*, AJ) during the 2010 election compared to Uzbeks from affected areas.

To test this hypothesis, we use electoral data from 34 polling stations, which fall within our sampling area. We match this data with our satellite victimization indicator using the addresses of polling stations indicated in official election results.[21] These sheets include information on the boundaries of specific voting precincts. Unfortunately, due to ambiguous street naming conventions and inconsistencies across maps, we are unable to reconstruct precincts using information about street-level boundaries. Instead, we approximate the precinct outlines using Voronoi diagrams. The Voronoi approximation identifies for each polling station the area that is closer to this polling station than to any other polling station. Using this procedure, we determined the location and extent of the electoral precincts. 16 of the 34 precincts are located in victimized areas.

20. We note, however, that the data are post-treatment and might therefore have been influenced by the riot.

21. See, cec.shailoo.gov.kg; the data were collected by Nathan Hamm, but are no longer available online. They are available from the authors upon request.

The election results are provided in Table A.15. Did unaffected areas express greater support for the old regime? To answer this question, we assess the vote shares across victimized and non-victimized areas (Panel A) for the AJ. The AJ supported Bakiyev and may thus serve as a measure of loyalism to the old regime. If anything, however, the AJ vote share is *higher* in victimized areas. There is thus no evidence that the riot was targeted toward Uzbek areas illoyal to the old regime

We must caution, however, that precincts are ethnically mixed, while we are interested in the Uzbek vote. A rough solution to this ecological inference problem is to focus on districts that are mainly inhabited by Uzbeks. We do so in Panel B of Table A.15. In our sample, 12 precincts have population shares of Uzbeks larger than 50%—six of which are located in areas affected by the riot. Here, too, we confirm that support for the AJ, while lower, is virtually identical across victimized and non-victimized areas. Even in the aftermath of the riot, support for the old regime is therefore not stronger in victimized as compared to non-victimized areas.

To test the robustness of this result, we adopt an approach to ecological inference promoted by King and colleagues (1997; 2008), who also applied it to voting data. The method uses data from all precincts. The basic idea is that while we do not know the voting behavior of individuals from different ethnic groups, we can establish estimates from the marginal shares obtained by each party in the different precincts. In each precinct, we know the maximum number of votes a party could have obtained from Uzbek voters—notably the number of votes cast for a given party. Working across groups, columns and rows, the

Party	A (Simple comparison)		B (Uzbek majority)		C (Ecological inference)			
	NV	V	NV	V	NV		V	
	%	%	%	%	%	SE	%	SE
AN	34.4	29.3	45.9	41.3	46.6	12.5	54.0	0.8
AJ	26.6	30.5	19.6	19.5	22.2	3.4	19.9	9.9
SDPK	6.3	6.2	6.3	5.5	7.8	3.2	0.2	0.3
AM	3.1	3.2	2.1	3.6	2.4	1.8	5.2	1.6
Res	7.6	9.7	6.6	9.9	6.5	1.2	6.7	4.4
BK	4.6	4.4	3.3	2.5	2.5	1.2	0.2	0.2
AS	1.9	1.7	1.8	1.4	1.7	0.3	1.5	0.9
Other	15.5	15.4	14.4	16.8	18.0	6.5	20.4	6.3

Table A.15: Vote shares in 2010 elections in victimized vs. non-victimized areas

Notes: The table shows the votes shares obtained by different parties during the October 2010 parliarmentary elections in victimized (V) as compared to non-victimized (NV) areas in our sampling area. We report vote shares for Ar-Namys (AN), Ata-Jurt (AJ), Social Democratic Party of Kyrgyzstan (SDPK), Ata-Meken (AM), Respublika (Res), Butun Kyrgyzstan (BK) and Ak-Shumkar (AS). Panel A shows a simple comparison, Panel B focuses on 12 majority Uzbek districts only, Panel C uses the ecological inference approach promoted by King and colleagues (1997; 2008). The electoral data was generously made available to us by Nathan Hamm.

algorithm sets bounds on the vote shares plausibly cast for a given party by a given group. We implement the procedure using the R *eiCompare* package by Collingwood et al. (2016). We run the algorithm separately for the affected and the unaffected precincts. Results are presented in Panel C. Reassuringly, the vote shares are similar to those presented in Panel B. Electoral support was strongest for the AN (Ar-Namys), followed by support for the AJ. Support for both parties is at very similar levels in both victimized an non-victimized areas. Importantly, the ecological inference approach also provides a standard error for the point estimates. Taking this uncertainty into consideration, vote shares are statistically indistinguishable across victimized and non-victimized areas for all major parties.

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