Online Appendix for

"Ethnic Riots and Prosocial Behavior"

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A Additional Information

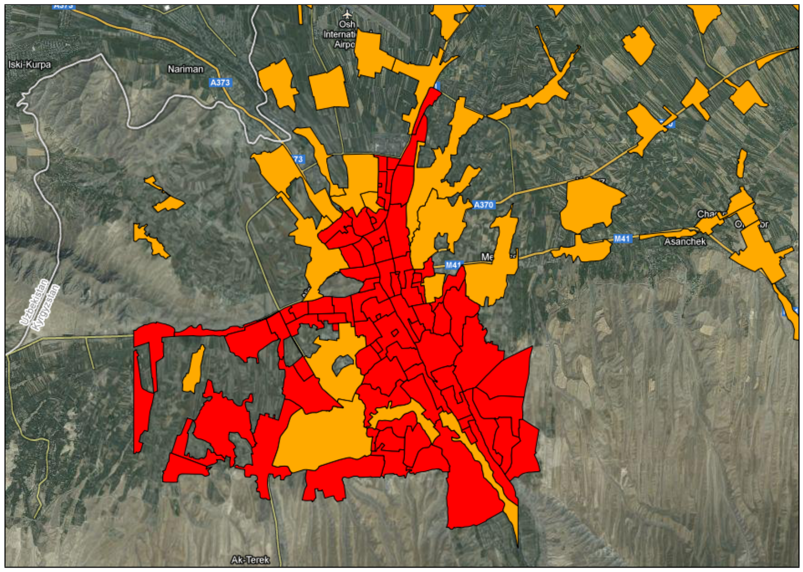

Figure A.1: Electoral precincts in Osh and around

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).

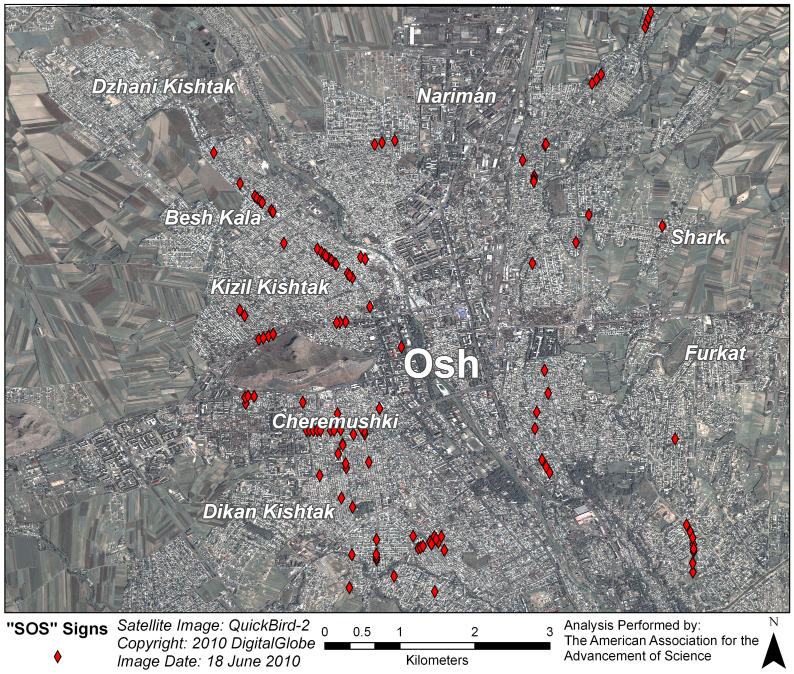

Figure A.2: SOS signs during the 2010 Osh riot

Notes: The Figure shows SOS signs sent during the 2010 Riot (AAAS 2013).

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).

	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.

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

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. 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.

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.

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.

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

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.

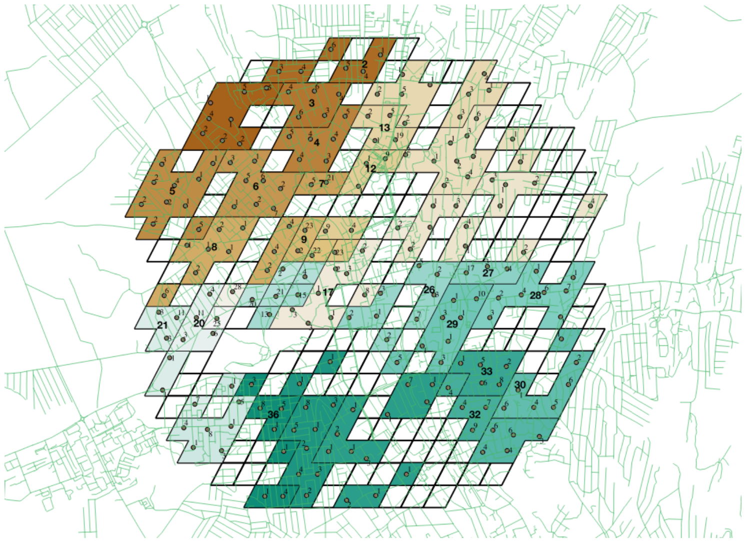

Figure A.3: Uzbek Sample

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

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.

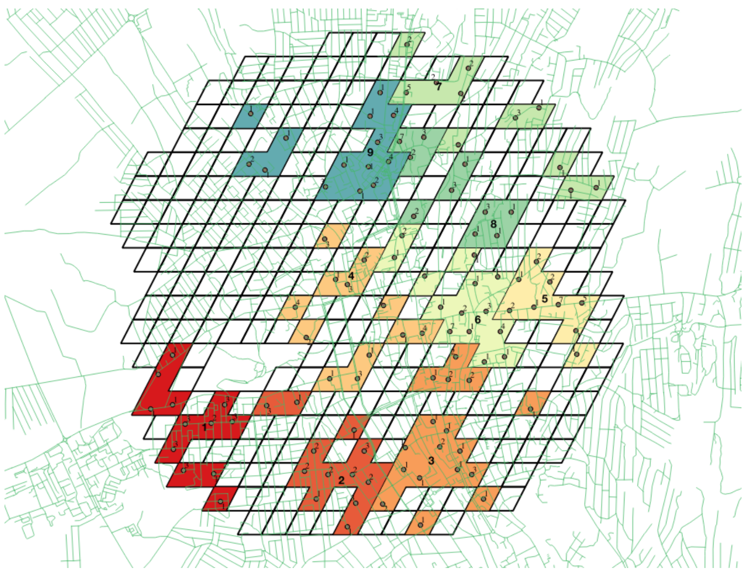

Figure A.4: Kyrgyz Sample

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.

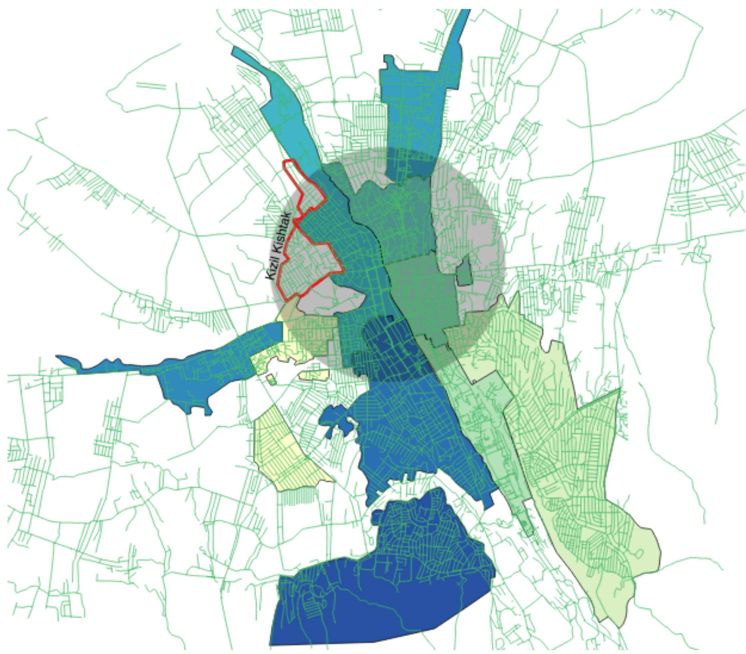

Figure A.5: Administrative districts of Os

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.

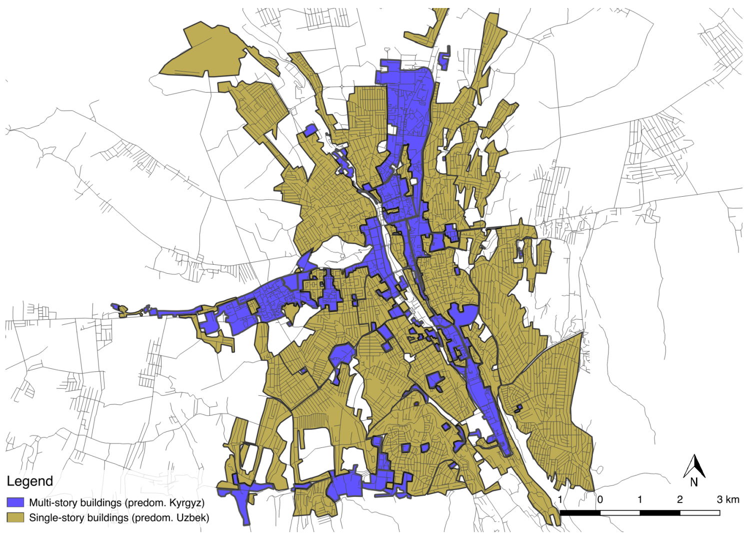

Figure A.6: Ethnicity in Osh

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

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.

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]

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.

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

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.

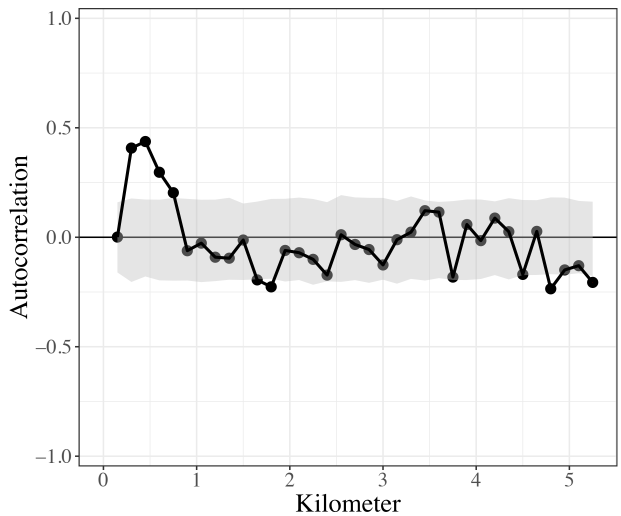

Figure A.7: Spatial autocorrelation: Geodesic distance (km)

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.

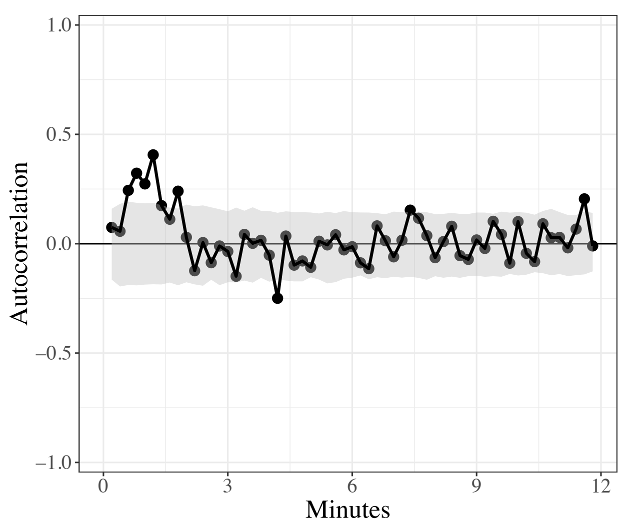

Figure A.8: Spatial autocorrelation: Travel time (minutes)

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.

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 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.

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 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.

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).

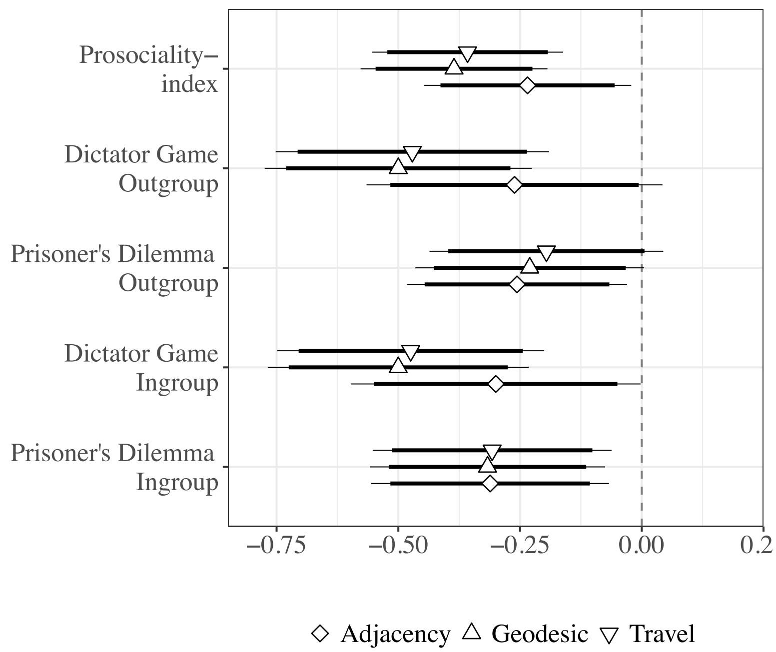

Figure A.9: Effect of Riot Destruction on Prosocial Behavior (autocorrelation adjusted)

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.

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:

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

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.

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.

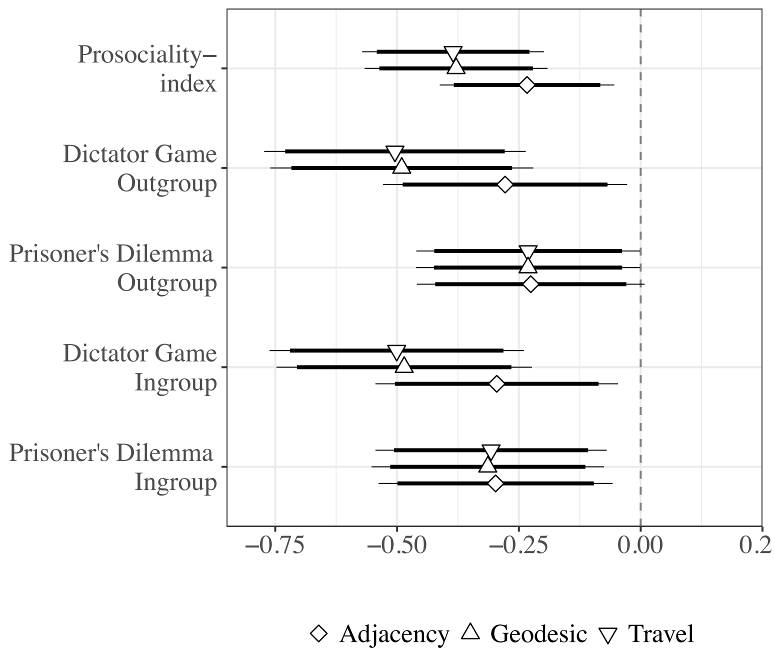

Figure A.10: Effect of Riot Destruction on Prosocial Behavior (spatial lags)

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.

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.

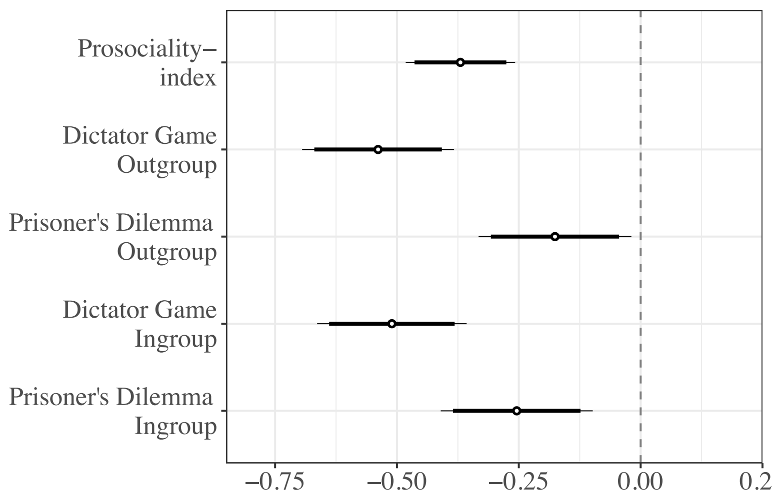

Figure A.11: Effect of Riot Destruction on Prosocial Behavior (matching)

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.

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.

	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.

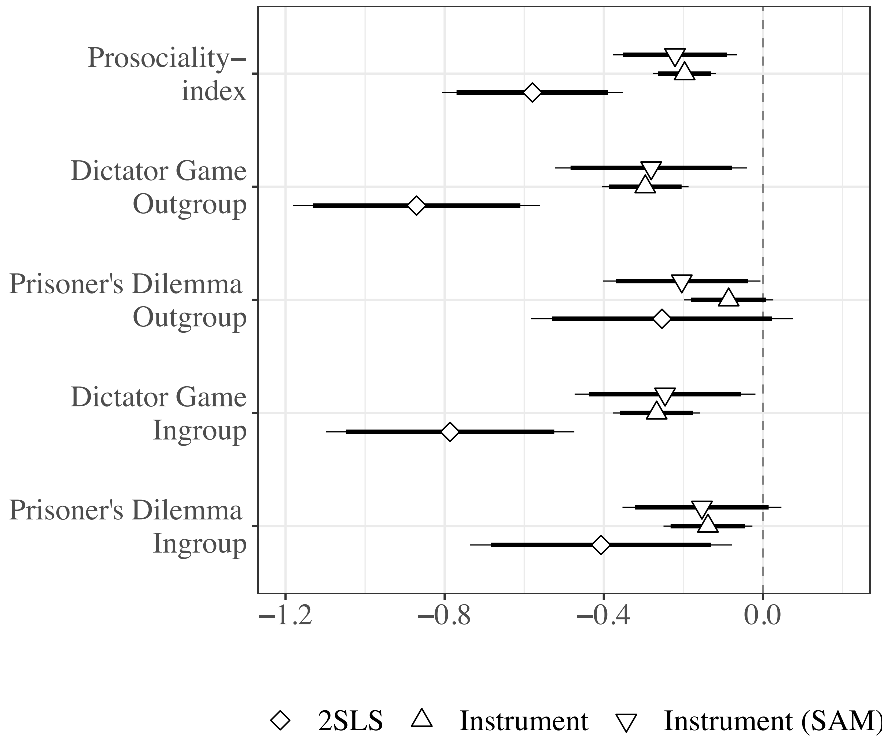

Figure A.12: Effect of Riot on Prosocial Behavior (IV; western Osh)

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).

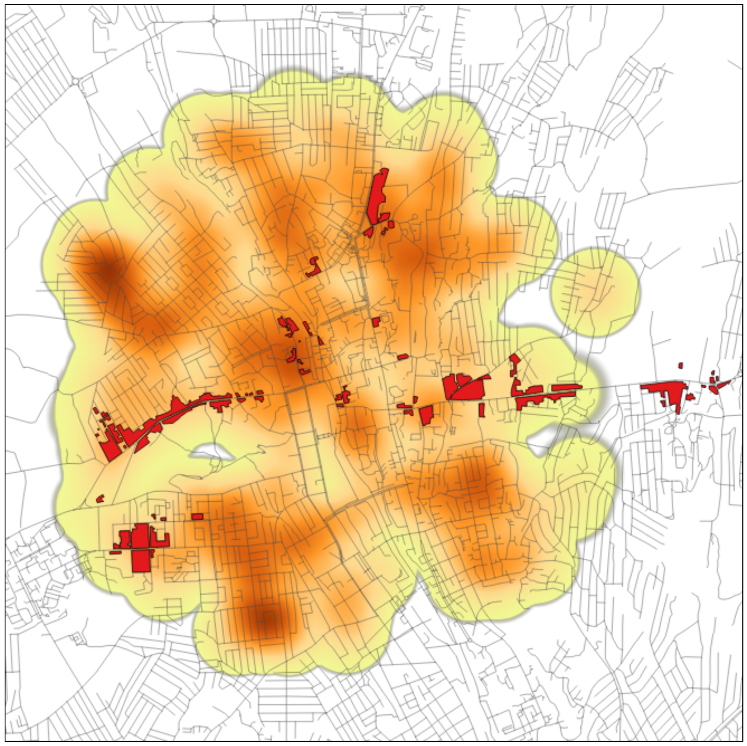

Figure A.13: Heat map of prosociality

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

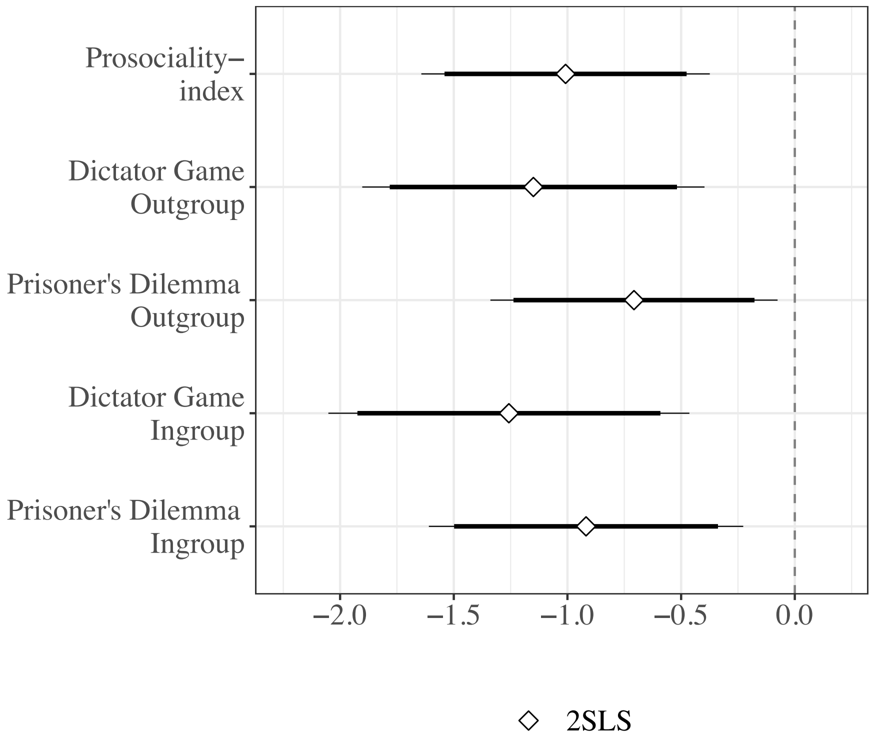

Figure A.14: Effect of Individual Victimization on Prosocial Behavior (IV)

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.

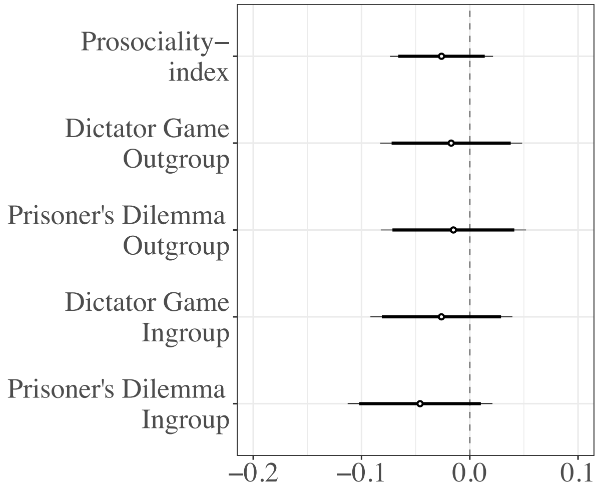

Figure A.15: Interaction effect of Riot Destruction × Historic Wealth on Prosocial Behavior (OLS)

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.

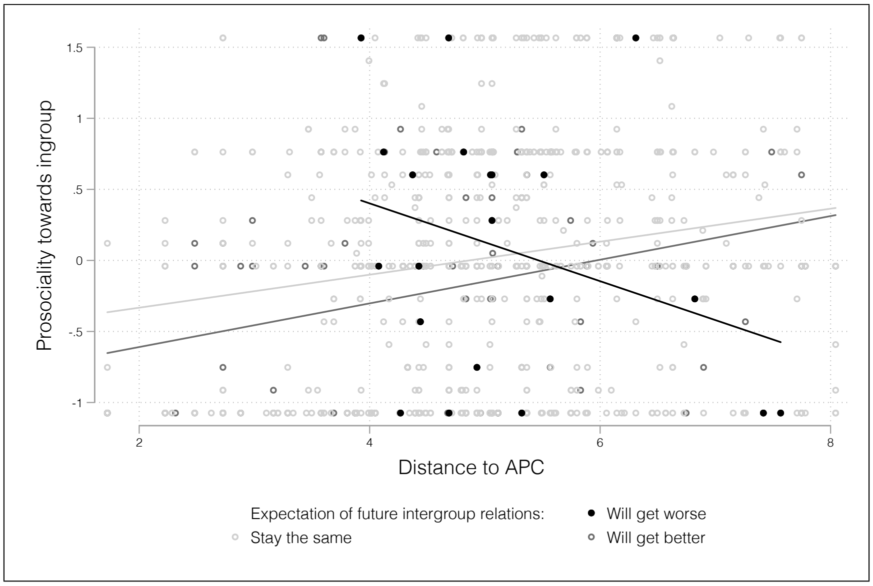

Figure A.16: Correlations between prosociality and instrument conditional on expectations

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

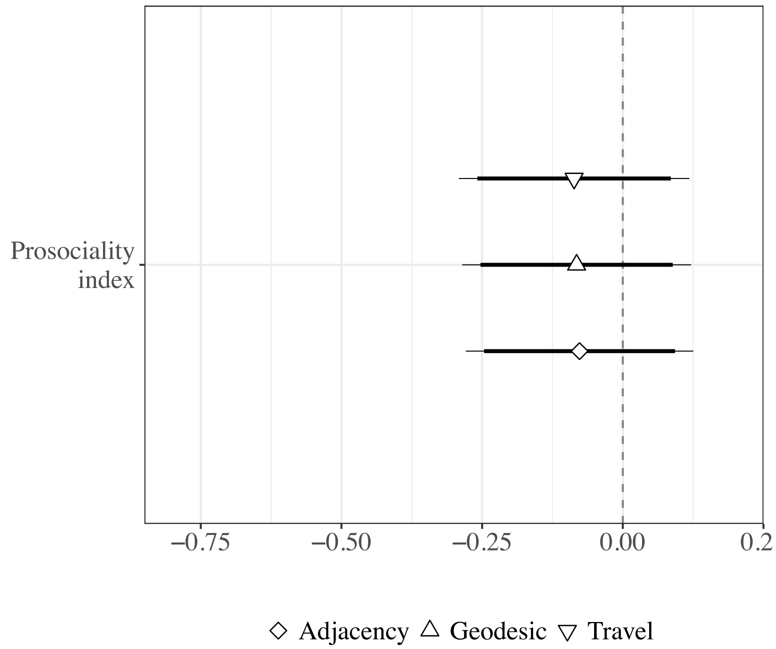

Figure A.17: Effect of Riot Destruction on Prosocial Behavior (Kyrgyz sample)

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.

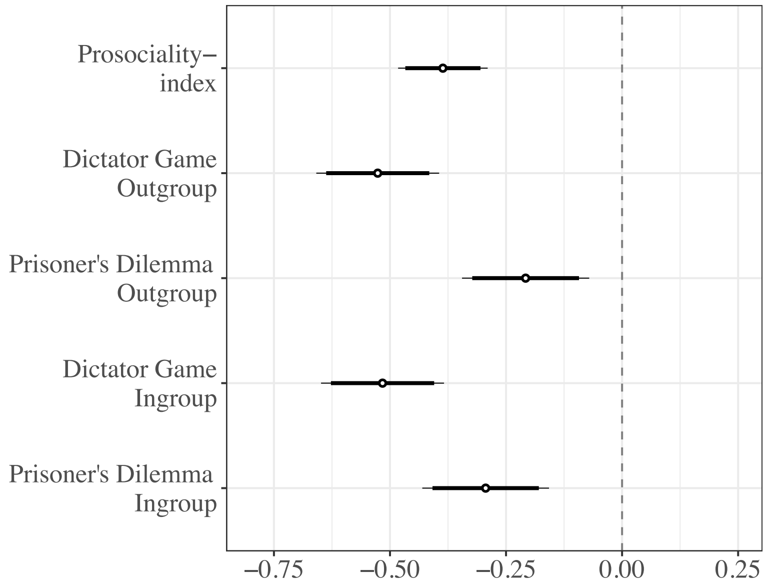

Figure A.18: Effect of Riot on prosocial Behavior (no imputation)

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.

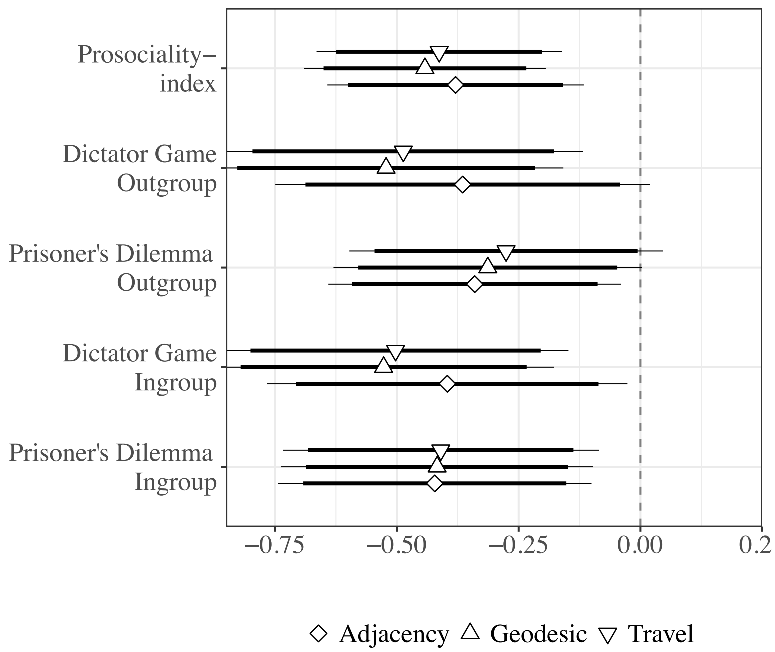

Figure A.19: Effect of Riot Destruction on Prosocial Behavior (autocorrelation adjusted, no imputation)

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.

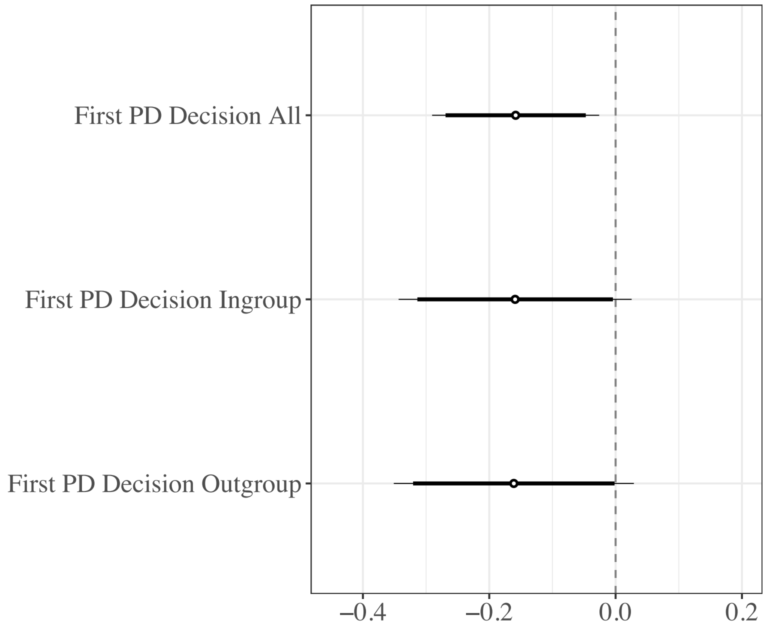

Figure A.20: Effect of Riot Destruction on Prosocial Behavior (first game decision only)

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 Sample		**Uzbek** Affected		**Uzbek** 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.

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).

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 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.

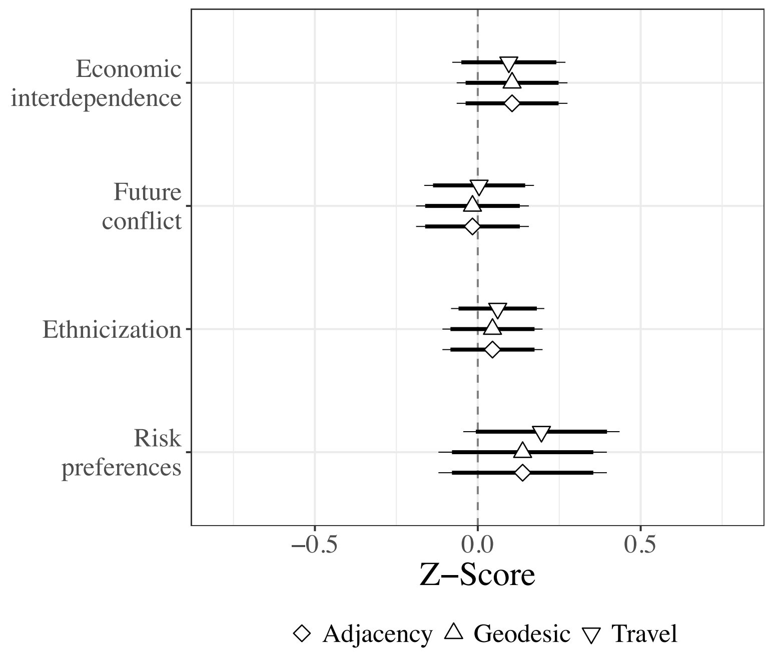

Figure A.21: Effect of riot on pre-registered mechanism outcomes

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.

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

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.

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.

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.

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 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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