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Civil War, Social Capital and Market Development: Experimental and Survey Evidence on the Negative Consequences of Violence

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Abstract

Recent studies have reported surprising increases in pro-social behavior following exposure to conflict. However, our research provides cautionary evidence of some important detrimental effects of conflict hidden within an overall trend toward increasing certain pro-social preferences. We draw our inferences from experimental and survey evidence we collected from a random sample in post-war Tajikistan. More than a decade after the civil war, which was characterized by insurgency and community infighting, exposure to conflict has opened a significant gap between norms people apply to others in their local communities compared to distant others. Our results show how conflict exposure undermines trust and fairness *within* local communities, decreases the willingness to engage in impersonal exchange, and reinforces kinship-based norms of morality. The robustness of the results to the use of pre-war controls, village fixed effects and alternative samples suggests that selection into victimization is unlikely to be the factor driving the results.

Key Words: Civil war, trust game, dictator game, market institutions, experimental methods, Central Asia

JEL Classification: C93, D03, O53, P30

1. Introduction

What are the long-term consequences of war and conflict-related violence on a society's prospects for development? A large amount of economic research on war focuses on macroeconomic outcomes and finds conflicting results. A significant body of literature puts civil war as a forefront underdevelopment trap (Collier et al. 2003; Collier and Hoeffler 2004) and highlights the economic, social and political disintegration that has followed many conflicts, particularly in the developing world. At the same time, a long tradition in economic and political history has characterized wars and inter-group competition as preconditions for nation building, state formation, and market development (Tilly and Ardant 1975; Tilly 1985; Greif 2006).

In just the past few years, scholars have begun turning to microeconomic and behavioral outcomes associated with conflict, and the results have yielded remarkable insights. These recent studies show that, among other things, exposure to conflict often intensifies positive prosocial elements within societies. (Bauer, Cassar, Chytilova and Henrich 2011; Bellows and Miguel 2009; Blattman 2009; Voors et al., forth.). This has led to some speculation against overly pessimistic views of the effect of conflict on development (Voors et al., forth.).

Our work contributes to this micro-behavioral literature by showing that some prosocial elements are indeed heightened by exposure to conflict-related violence, but we caution that such result should not be taken necessarily as a positive element for market development and growth. We consider the effects that conflict exposure might exert on behavioral preferences for trust, reciprocity, fairness, egalitarianism and then how those preferences in turn affect individual participation in markets, support for market institutions, collective action and political orientation. In contrast to other studies, our data show the emergence of a very different picture when the survey results are combined with the experimental evidence. The increase in collective action that we find is actually associated with the erosion of local trust and a reinforcement of kinship ties. This result emerges when we compare relative levels of victimization in a post-war setting. If one allows that conflict has a direct impact (in different degrees) on a vast number of civilians, then especially brutal wars could unleash behavioral elements which are devastating for market development and growth.

We draw our inferences from recent evidence gathered in post-war Tajikistan. We chose Tajikistan in part because, as a Soviet creation, market development was prohibited prior to

the onset of the conflict thus making it a compelling case for studying market foundations. We report here the results of a series of experiments and a survey designed to investigate whether the 1992-1997 Tajikistan civil war has left any effect on social and economic preferences more than 10 years after the end of the conflict. We focus on preferences and social norms that are thought to sustain the development of impersonal exchange and state building, namely trust and norms of fairness.

To assess norms, we turn to behavioral experiments utilizing a simplified version of the trust game and the dictator game under two treatment conditions: Same Village, in which the anonymous second player is someone who lives in the same village as the first player, and Distant Village, in which the second player might come from anywhere in Tajikistan, therefore naturally a more abstract concept. The experiments are then followed by an in-depth survey. We carried out our study in 17 randomly selected villages within four regions of Tajikistan (Dushanbe, Khatlon, Gharm and Pamir) where within each village, our subjects were randomly selected via the random route technique, resulting in a sample of 426 individuals.

The Tajik civil war was characterized by insurgency and community infighting. Our findings unequivocally point to negative and persistent effects of such violence on the norms that support impersonal exchange, in particular on trust within a village. Victimization during the civil war is associated with a roughly 40% decrease in trust (the amount sent by the first mover in the trust game) when respondents are matched to another individual from the same village. We corroborate our experimental evidence with survey data on actual behavior and stated preferences. Consistently with the decrease in local trust in the experimental games, former victims are both less likely and less willing to participate in local markets, in particular when they do not have a personal connection with the trader they are dealing with. Our results also indicate that experiences of victimization are associated with reinforced kinship-based norms of morality and behavior, at the expense of the rule of formal law.

What we find particularly interesting is that, at the same time, our data confirm previous findings that victimization is associated with more active participation in groups (e.g. Bellows and Miguel 2009; Blattman 2009). Our results nevertheless suggest that such collective action cannot be taken as a form of inclusive social capital. Indeed, for those who have experienced

violent conflict directly, participation in local groups is associated with further erosion of local trust.

In contrast with the negative implications of our results for local social cohesion, when we elicit the behavioral measures under the Distance Village treatment, we find surprising evidence that victims actually display more trust (by about 20%), generosity (by nearly 35%) and egalitarianism (by 18%) towards a distant other living far away from the subject. This increase in prosociality towards an abstract fellow Tajik citizen, someone who is not identifiable as member of the local community, is consistent with an increasing body of evidence on the so-called "post-traumatic growth" and surprisingly altruistic behavior in post-disaster environments (Tedeschi and Calhoun 2004; Solnit 2009; Voors et al. forth.).

Because of the regional nature of the conflict, all specifications include regional fixed effects. Yet, victims of violence may be different from non-victims in observable and unobservable ways and so any comparison of victims and non-victims will conflate the impacts of war with preexisting differences that led some people to be victimized. This is especially problematic if the characteristics associated with victimization are also those associated with the outcomes that we want to observe. As a Soviet creation, Tajikistan had no experience with a market economy and democratic self-governance prior to the onset of violence. This helps alleviate the (statistical) problem that such characteristics may predict selection into war victimization. Still, some concerns remain for the identification of causal effects of victimization and we employ several strategies to deal with the potential selection bias. First, we use a selection on observable strategy by investigating the determinants of victimization and controlling for such characteristics in the analysis. Second, we check that all results are robust to the inclusion of village fixed effects. Village fixed effects enable us to isolate the variation in violence experienced across neighbors within the same village. Third, in order to address the concern that selection into victimization was based upon unobservable characteristics, we follow Altonji, Elder and Taber (2005) and gauge how much the importance of unobservable variables would need to be, relative to observable factors, in order to explain away all the effects of war violence on post-war outcomes. Last but not least, we focus our analysis on different sub-samples. We restrict our attention to individuals who were too young to be systematically targeted – those who were 12 or younger at the beginning of the conflict. We also consider the sub-sample of people who have never moved

in order to rule out that our results are due to selective migration. Taking all the evidence together, our analysis indicates that selection into victimization is unlikely to have been the factor driving our results.

The next section reviews the relevant literature. Section 3 discusses our main hypotheses. Section 4 presents the experimental design and the survey methods. Section 5 describes the empirical strategy. Section 6 discusses the results and Section 7 concludes. More information on the Tajik civil war and additional results can be found in the Supplementary Appendix.

2. Relevant Literature

This paper contributes to two main strands of the literature. First, it contributes to the literature on the origins of prosocial preferences and on the formation of political attitudes, which is becoming increasingly important in several fields of economics. Second, we contribute to the literature on the social and institutional legacy of conflicts. While the long-term impacts of wars have been primarily studied in terms of economic activity, industrial recovery and their effects on physical and human capital, the impact of conflicts through their impact on preferences has only recently started to be experimentally studied (e.g. Bauer et al. 2011; Voors et al., forth.).

For a long time contemporary economists have assumed individual preferences to be exogenously determined and fixed (Stigler and Becker, 1977) or, at the very least, a topic to be studied by other social scientists. As a stark departure, in the past couple of decades experimental and behavioral economists have started to identify a number of predictable determinants of preferences and sources of preference change (e.g. Loewenstein and Angner 2003). The question is important for many fields of economics and for development in particular, since preferences such as trust and fairness have been associated with positive development outcomes (for a survey see Cardenas and Carpenter 2008). So far, the empirical evidence in support of the hypothesis that such preferences would be born with the individual is quite weak. Quite interestingly, a significant set of experimental results show that there are fundamental behavioral differences in fairness and cooperation between people who are WEIRD (Western, educated, industrialized, rich and living in democracies) and people who are not (Henrich, Heine, and Norenzayan 2010). In particular, greater level of fairness and punishment have been found to positively covariate with market integration and community size, providing significant evidence that preferences would not be uniquely

exogenously determined, but might have been evolved over the course of human history jointly with norms and institutions (Friedman 2008). Given this evidence of different behavioral preferences across groups, what we want to address in our research is the issue of whether current conditions and past experiences can affect preferences in a persistent and systematic way[1].

In this paper we focus on prosocial preferences such as trust and fairness, because they have been found vital to solve cooperation and coordination problems in modern societies and therefore crucial for economic and social development. Individual preferences towards others (such as trust, reciprocity, altruism, egalitarianism, parochialism, fairness) are key components of many economic decisions and are often associated with social capital and considered necessary for growth and development. Societal trust and preferences for fairness have been positively associated with growth and the level of market development (e.g. Knack and Keefer, 1997; Knack and Zack, 1999; Henrich et al. 2010). Recent studies have shown how other-regarding preferences are decisive for the human ability to cooperate in large groups (Bowles 2006; Boyd and Richerson 2005), to participate in public life and politics (Bowles and Gintis 2006) and how they amplify reputational incentives in strategic interactions (Rockenbach and Milinski 2006). The role of impersonal social trust in sustaining economic exchange is the object of an ever-growing literature. A prerequisite for the successful development of market economies is to depart from closed group interactions and to enlarge exchanges to anonymous others (Fafchamps 2006; Algan and Cahuc 2010). In this regard, generalized trust appears as a keystone for successful market development and it is often included in the various definitions of "social capital" as one of its main elements. Understanding the effect of social capital on economic decision-making has been the subject of a broad literature too. This literature has pointed to the positive effects of social capital on economic growth (Knack and Keefer 1997), reducing corruption (LaPorta et. al. 1997), community governance (Bowles and Gintis 2006), preventing crime (Case and Katz 1991), curtailing moral hazard in the workplace (Ichino and Maggi 2000), and financial development (Guiso, Sapienza and Zingales 2004). Generosity, egalitarianism and a sense of fairness, instead of spitefulness, may also help sustain trade, cooperation and development

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[1] Some related evidence in this regard is presented by Cassar et al. (2011) who find that Thai subjects affected by the 2004 tsunami are, four and a half year after the event, significantly more trusting (as well as more risk averse, and, modestly, more trustworthy and impatient).

especially in countries when institutional contracts enforcement is weak by letting individual engage in profitable trades that are beneficial to self and others and by preventing the violation of contracts. Even in countries with well functioning institutions, a sense of fairness and trust may support trade, given the necessarily incomplete nature of contracts. Inside societies in which generosity and fairness are anticipated, more individuals may be willing to participate in impersonal trade, while the opposite definitely may work as a trade deterrent (Fehr, Hoff and Kshetramade 2008).

If circumstances and experiences can affect prosocial preferences, can they be shaped in a predictable manner by wars and civil conflicts? Very recent literature is finally focusing on these behavioral legacies and offers surprisingly consistent evidence of increased prosocial actions among those more affected by conflict, leading to possibly positive interpretations of some of the effects of wars for social capital building. In particular, Bellows and Miguel (2009) find a significant increase in collective actions among the individuals more affected by the war in Sierra Leone. Blattman (2010) reports higher voting and political action in Uganda. More related to our work, Voors et al. (*forth.*) conducted an experiment in Burundi to examine the impact of exposure to conflict on social, risk and time preferences and find that individuals that have been exposed to greater levels of violence during the war display more altruistic behavior towards their neighbors, are more risk seeking, and have higher discount rates. Becchetti et al. (2011) report higher trustworthiness among the most affected individuals in Kenya. Finally, Bauer et al. (2011) provide evidence of higher egalitarianism and parochialism among victimized children in the Republic of Georgia after the war with Russia. A less positive result on the interplay of trust with violence has been found by Nunn and Wantchekon (*forth.*) who show that violence and a history of violence, even going as far back as the slave trade in Africa, impact contemporaneous trust negatively and strongly. Their hypothesis is that the negative legacy of slave trade on general trust is mainly due to the destruction of social ties through inter-ethnic slave raiding.

In conclusion, given the importance of trust and fairness preferences for development, an important channel through which conflict can affect development and growth is the effects it may exert on such pro-social preferences. Anticipating our results, our study casts a pessimistic view: violent conflict, especially large-scale civil wars characterized by insurgency

and infighting within communities, could also have highly detrimental effects on trust within a society.

3. Research Hypothesis

We consider our research question in the context of a devastating civil war in the former Soviet republic of Tajikistan. When the Soviet Union collapsed, Tajikistan collapsed with it, and regional rivalries, many of which were explicitly developed and exploited during the Soviet era, gave way to a brutal power struggle, which lasted from 1992 until a negotiated settlement brought a tenuous peace in 1997. We test whether over a decade later (or nearly two decades from the start of the conflict) some of the effects of violence are enduring or not (see the Supplementary Appendix for detailed background information on Tajikistan and the civil conflict). One possibility, the null hypothesis, is that pro-social and pro-market economic preferences are not systematically different between individuals that were heavily affected and those who were less so (since everyone is presumably affected, to varying degrees, by a civil conflict that lasts years). An alternative hypothesis is that, at the individual level, the more direct and personal the experience of violence, the more dramatic the effects in altering trust and sense of fairness.

From a theoretical perspective, an important foundation for our work comes from the culture/gene evolutionary approaches to understanding human cooperation. A fascinating hypothesis since Darwin is that frequent lethal inter-groups conflicts are at the very origin of human altruism and prosocial behavior (Darwin, 1873). Such violent conflicts would select as winners groups abounding of altruistic and prosocial types, providing a solution to the evolutionary puzzle of the sustainability of altruism and prosociality in large groups of genetically unrelated strangers (Bowles 2006; 2008; 2009; Choi and Bowles 2007; Boyd and Richerson 2005). In facts, wars and evolutionary pressures would open a gap between insiders and outsiders, and this gap (known as parochialism) would favor cooperative institutions among insiders. To test whether inter-group conflict increases prosocial behavior within the members of one's own group, Bauer et al. (2011) conducted experiments with over 600 children in Georgia six months after the August 2008 armed conflict with Russia. The subjects were children age 4-11 years from locations that were hit by the conflict to various degrees. The results show that experiencing an inter-group conflict does lead to a stronger application of norms of parochial altruism and egalitarianism.

It is not clear, a priori, whether a preferential treatment towards members of one's own group is beneficial for trade and development since, on the one hand, markets require impartiality, but, on the other, they require trust and fairness. In sum, if a market has to be developed within the boundaries of one's group, then it is reasonable to expect parochialism to increase local trust and fairness and therefore to be positive for market development and growth. But while it is a reasonable hypothesis for inter-group conflict, it is not immediately evident what the predictions would be in case of a civil war. We hypothesize that the final effect depends on the type of civil war. If the civil conflict involves village against village or the people in the country against a dictator, the results for the inter-group conflict might still hold. But if, on the contrary, the civil war would be one that turns neighbors against neighbors we would expect the opposite: less trust and fairness between individuals living in the same village.

We think that the case of Tajikistan fits in this second category. What makes the Tajik civil conflict interesting is the complex networks of rivalries that emerged within local communities during the fighting. It was often difficult to make simple shorthand predictions about who was fighting whom in the midst of the chaos of the conflict zone. The various warring factions were not readily identifiable. Among combatants, the Russians and Uzbeks are the only ones who really faced the problem of being readily identifiable by physical appearance and language. Eastern Pamiris (Gorno-Badakhshan) were better capable of blending in and transitioning between Tajik and their Pamiri dialect. There are many examples of the "not readily identifiable" aspect of the conflict. It was widely reported that government soldiers in Dushanbe and elsewhere would stop people at random demanding identity papers, where those with Pamiri names or born in the Gharm region were arrested or simply shot on sight (Jawad and Tadjbaksh, 1995; Hiro, 1995). The opposition applied similar tactics in the capital and when dealing with southern Kulyabis in the Kurgan Teppe region. There were also instances of regionally mixed villages, ethnic/regional inter-marriages, and intra-regional violence that further complicated identification (Tuncer-Kilavuz 2009). In many cases, factions of the same groups were even fighting among themselves within their local communities (Tuncer-Kilavuz 2009, 2011).

We believe that the inability to distinguish friend from foe in conflict may have profound effects on social norms, especially at the local level, by creating long-term concerns about

trusting people close by. Normally, local communities are considered to be safe havens for trust, even in times of violence as long as enemies are readily identifiable and front lines can be drawn. In the Tajik civil war, this was not the case. The local environment was extremely dangerous and unpredictable. In contrast to the usual logic of trust (declining as the network of people expands to include more distant strangers) here trust is conditioned by the probability of others taking advantage of you or doing you harm. In this case, people in the village are in the most likely position to take advantage or harm others. The conflict provides a framework for "common knowledge" about the uncertainty of others close by. Because local environments provide many of the foundations for political and economic communities, we argue that the depletion of prosocial norms in local communities will have profound effects on the ability to develop reliable and credible institutions, decreasing the support for market and democratic reforms.

4. Experimental Design and Survey
4.1. Experimental Protocols

To elicit individual preferences we had subjects participate in three games: the dictator game, the ultimatum game and the trust game. We always run them in this order (given the natural increase of complexity) without disclosing to the first player the decision taken by the second player until the very end (and only for the game randomly selected for payment) to prevent dependency between games. For economy of space, in this paper we do not discuss the results for the ultimatum game since the data are qualitatively similar to the dictator game results but provide little additional added value to our conclusions[2]. For comparison purposes, our instructions were based on the ones used for the *The Roots of Human Sociality*.[3] The original protocol was modified to include our Same Village / Distant Village treatments, to preserve anonymity and to fit the Tajik environment. In each session the second movers in the games were randomly assigned to be someone either from the same village as the subject or from somewhere else in Tajikistan (see the treatment description below).

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[2] Interested readers can find the ultimatum game analysis available upon request.
[3] An Ethno-Experimental Exploration of the Foundations of Economic Norms in 16 Small-Scale Societies. Principal investigators: Jean Ensminger, Joseph Heinrich. Instructions and other information available at: http://www.hss.caltech.edu/~jensming/roots-of-sociality

The dictator game is a 2-player game in which Person 1 has to decide how to allocate a certain sum of money between self and an anonymous other. In our adaptation, subjects had to choose how to allocate 40 Somoni in increments of 10 Somoni (1USD = 4.43 Somoni so approximately $9 in increments of $2.25). Subjects were not given real money, but instead made their allocation decisions by checking a box on a form.[4] To ensure anonymity, each subject was given a big cardboard box to do his or her writing in. Person 1 was then asked to decide how to divide these 40 Somoni (0, 10, 20, 30 40) between him or herself and an anonymous Person 2. In the first part of this game we had every subject participate as Person 1. When subjects played as Person 2 they had nothing to do, still we didn't reveal any Person 1 choices to them until the very end and only if this was the game selected for payment.

The trust game was based on the classic Berg, Dickaut and McCabe (1995) protocol. A first mover has to decide how much of an initial amount I to send to a second mover. The amount sent (X, with $0 \leq X \leq I$) is then multiplied by 3 before reaching the second mover. The second mover receives $3X$ and has to decide how much of that sum (Y, with $0 \leq Y \leq 3X$) he/she wants to return to the first mover. X can then be interpreted as an indication of trust while Y as a measure of trustworthiness. In our adaptation, we gave each first mover 20 Somoni (again, only on a form) and there were only 5 options for dividing the money (0, 5, 10, 15, 20)—this was to simplify the game and to have the same focal points as other studies. In the first part of the game, we had all of our subjects play as first mover, in the second part all played as second mover, using the strategy methods and without revealing what the first mover had actually sent to them.

To avoid issues of correlations across games, we paid subjects only according to one of these game/roles. We announced at the very beginning that at the very end we would ask one of the subjects to throw a 6-faced die and the number this would determine the game/role according to which they would be paid. It is important for our protocol that no information was revealed to Player 2 in the various games until the very end and only if the game/role was selected for payment. To not undermine dominance and introduce other motivations

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[4] The advantage of using forms is that it was easier for subjects to conceal their decisions in a group setting compared to using real money.

(like simply having fun while participating), we never called these activities games or refer to it as play, we used more neutral terms like task, decision making, make a choice etc…

In addition to their earnings, all participants received a "show-up" fee of approximately $3 in local currency. Total earnings ranged from 0 to 60 Somoni (0-13.50 USD) with an average of 24 Somoni with a standard deviation of 10.9 (approximately $5.40, SD = $2.46) excluding the show-up fee. Fieldwork started on June 1 and sessions ran from July 1 to July 24, 2010. In total, 426 subjects completed the study. Additional sample characteristics are listed in the analysis below.

4.2. Treatments

In order to test our hypothesis we implemented 2 treatments: "Same Village" (SV) and "Distant Village" (DV). In the SV treatments we explained to the subjects that for each game the second mover was selected among people from the same village, while under DV we explained that the second mover was selected from people from a distant village. We described "Same" and "Distant" villages by showing the subjects a map of their country and pointing to where their village was ("Yeah, that's right, that's your village"). If the session was a SV treatment, we explain that if we were to pay according to player 1 decisions, we would send the money anonymously to another person that lived in the same village and who would participate in a future session. If we were to pay according to player 2 decisions, player 1 offers came from an anonymous other who participated in a previous session in the same village.

For the DV treatment, we draw on the map a large circle around their village and we explained that the distant village could be anywhere outside that circle ("Yeah, that's right, that's your village, and those are all different villages very distant from here"), otherwise we explained the payoffs for first players and second players in a similar manner to the SV sessions. For the DV sessions we used the offers from the first movers from previous locations in which we did DV (using pilot data for the very first one).

4.3. Subject Recruitment and Sampling Frame

The subjects were selected using a multi-stage sampling method. 426 individuals were surveyed and administered the games in 17 villages in 4 regions: Dushanbe, Khatlon, Gharm and Pamir. In Dushanbe, Pamir and Gharm selection of villages (the first sampling stage)

was made at random based on census data with probability of selection proportional to population size. Villages in Gharm were chosen at random within the sub-region of the Rasht Valley, an opposition stronghold where fighting was especially intense during the civil war. Within each village, respondents were selected randomly. On arriving at the sampling point, each enumerator was randomly assigned a starting point within the town or village. For the selection of households, each enumerator followed the standard "random route" technique, starting with 5th numbered apartment building or house selecting every 5th entrance. Individual respondents (1 per household) were chosen using a random selection key (a 12-face die) where every adult member of the household had an equal probability of being selected. For each sampling point, all recruitment of subjects and data collection was conducted on the same day using a team of enumerators and administrators to conduct the survey and to run the experiments. In Dushanbe, Gharm, and Khatlon, the team consisted of the same group of five ethnic Tajik enumerators and one ethnic Uzbek enumerator. In Pamir, we used a different team of four ethnic Pamiri enumerators and a Pamiri administrator. The local teams were trained by two graduate students and by one of the authors of this paper who was always on site to supervise data collection.

To address issues of framing either the experiment or the survey, we conducted some sessions in which experiment came before the survey and others in which the order was reversed. For the survey, most of the subjects were interviewed in their home one-on-one by local enumerators. In cases where the home environment was not sufficiently private or accommodating, subjects were interviewed outdoors or at another location. Once subjects completed the survey, they were taken by their enumerator to a common location in the town or village to participate in the experiments. Most experiments were conducted in schoolrooms, where each person had their own desk and chair to work on. In villages without schools, experiments were conducted in the largest common space, typically a mosque or a meeting hall. The sessions were conducted in groups of 10-20 subjects, depending on the size of the room available. Subjects were not allowed to talk with one another during the sessions and this rule was generally well abided. No significant disturbances or interruptions occurred during the experimental sessions. Each experimental session was conducted by a local administrator and an assistant. The administrator read instructions from a standard script. All survey and experimental instructions, forms, and

materials were translated into Tajik, Russian, and Pamiri and back-translated into English for accuracy.

4.4. Survey

War victimization is captured through survey questions. The survey asks about injury, loss of life of any household member, loss of property and forced displacement as a result of a conflict. Respondents were also asked whether they witnessed or took any direct participation in fighting not only during the conflict but also since the 1997 Peace Agreement.

The survey probes about economic, social and political attitudes. Several attitudes are of noteworthy interest. First, our aim is to provide, through survey questions, a validation of our experimental measures of preferences. We are particularly interested in the implications of the trust game behavior with regards to impersonal exchange. The survey therefore investigates stated preferences towards participating in impersonal exchange and towards market liberalization. In order to measure respondents' actual participation in markets, we follow Heinrich et al. (2010) and ask respondents to report the share of their weekly consumption of food purchased through markets as opposed to self-produced, bartered or exchanged as gifts. Second, we aim at capturing norms of generalized morality and respect for the rule of law as opposed to kinship-based morality. The contribution of generalized norms of morality in solving problems of cooperation and conflict and the contribution of the latter to the development of impersonal exchange and markets has been noted in the literature before, namely by Greif (2006). The survey inquires about procedures of conflict resolution, particularly related to conflicts emerging during market exchange. Last, the survey includes several measures of participation in groups, collective action and political participation. The purpose of these questions is to test whether previous findings of the positive effects of conflict on group membership and local collective action are replicated in the Tajik context.

5. Empirical Strategy and Identification

We investigate how war experience affects individual preferences, values and beliefs. The analysis compares individuals who suffered from different degrees of violence during the conflict. The general form of the estimation equation is as follows:

$$Y_{ij} = \beta_0 + \beta_1 W_{ij} + \beta_2 X_{ij} + \beta_3 C_j + \varepsilon_{ij} \tag{1}$$

where our outcome variable Y_{ij} includes different measures of elicited social preferences, market orientation and economic and political preferences of respondent i in region or village j; W_{ij} is a measure of the intensity of individual exposure to civil war violence, X_{ij} is a set of individual and household controls, and C_j is a set of region or village fixed effects. We use two measures of individual exposure to civil war violence. The first (*Injured or Killed*) is a dummy variable taking value 1 if either the respondent was injured or one of his or her household member was injured or killed during the civil war. The second (*Injured and Killed*) is a dummy variable taking value 1 if the respondent reports both injury and loss of life in the household during the civil war. This second measure thus indicates higher degree of severity of exposure to conflict. In all regressions using experimental data we additionally include controls for the different experimental treatments. Standard errors are clustered at the village level in all specifications.

Because of the regional nature of the conflict, all specifications include regional fixed effects. With regional fixed effects, identification of causal effects of conflict requires victimization within a region to be –close to- random. Such an assumption might be too strong. Victims of violence may be different from non-victims in observable and unobservable ways and so any comparison of victims and non-victims will conflate the impacts of war with preexisting differences that led some people to be victimized. This is especially problematic if the characteristics associated with victimization are also those associated with the outcomes that we want to observe. If, for example, more pro-social or more market oriented individuals, or villages with higher proportion of such individuals, were systematically targeted, this would result in an estimation bias of any effect of the civil war on social preferences and market orientation.

The specific situation of Tajikistan somewhat helps us deal with this issue. As previously noted, Tajikistan had no experience with a market economy, which helps alleviate the –

statistical- problem that such characteristics may predict selection into war victimization. Still, some concerns remain for the identification of causal effects of victimization and we employ several strategies to deal with the potential selection bias.

First, we employ a selection on observables strategy and check that our results are robust to the inclusion of a large number of individual and household controls. Of particular concern are variables that may be related both to post-war outcomes and to victimization. We focus on pre-1992 characteristics, since such characteristics cannot have been affected by victimization. We also empirically investigate what characteristics are associated with victimization and include them as controls in the rest of our analysis.

Second, we check that all results are robust to the inclusion of village fixed effects. Different villages may have been targeted as a function of characteristics that are not observable to the econometrician, for example the support that local clan leaders gave to different fighting factions. With village fixed effects, identification now only requires that violence is -close to- random within villages, conditional on household and individual characteristics.

Third, in order to address the concern that selection into victimization was based upon unobservable characteristics, we follow Altonji, Elder and Taber (2005) and gauge how much the importance of unobservable variables would need to be, relative to observable factors, in order to explain away all the effects of war violence on post-war outcomes. Obtained statistics (see Section 6.2.3.) make it unlikely that the omitted variable bias could account for the full effect of civil war on our main outcomes of interest.

Our last strategy to deal with potential selection bias is to focus our analysis on different subsamples. We first restrict our attention to individuals who were too young to be systematically targeted – those who were 12 or younger at the beginning of the conflict, or at most 31 years old today.[5] This is about a third of our sample. There is another rationale behind focusing on this subsample. The psychology literature stresses that traumatic events have a stronger impact on younger individuals, particularly in their late childhood or early teenage years. The effects of victimization are thus expected to be of a larger magnitude on this subsample of the population, who were at most 18 at the end of the conflict. A remaining issue is that the results could be driven by selective migration of individuals who

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[5] Specifications for the egalitarian split in the dictator game use the sub-sample of those at least 16 or 18 years at the onset of conflict, since 12 or younger leads to too small a sample for this specific outcome.

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experienced violence. Our results would be biased if, for example, war victims systematically migrated to areas where formal institutions are weak and markets poorly developed. In order to deal with this issue, we re-run the analysis on the subsample of people who have never moved and still live in the village where they were born.

6. Results

6.1 Determinants of Victimization

As can be seen in Table 1, the incidence of war victimization in our sample is very high. On average, 21% of respondents declare that they have been personally injured or that a member of their household has been injured or killed as a result of the conflict. 13% of respondents report both injury and loss of life as a result of the conflict. There is a lot of regional variation (see Figure 1 and Table A2 in Supplementary Appendix). Victimization is particularly high in Gharm, one of the regions most affected by civil war violence. We purposefully surveyed respondents in the Rasht Valley, an opposition stronghold where fighting was intense. 40% of respondents in this region report loss of life in their household. Table 2 displays the results of regressions where our victimization indicators are regressed on a number of individual characteristics, controlling for regional and village fixed effects. The results do not support fears that selection into victimization is a major concern. The region where the respondent lived in 1992 is the strongest and most robust predictor of violence. Ethnicity is also found to be a strong predictor of victimization. Members of the –easily identifiable- Uzbek minority are less likely to have been victims of violence. As expected, victimization is positively associated with age, although the relationship is statistically weak. Education is also positively and significantly associated with victimization. To explore in more details the relationship between education and victimization, we restrict the sample to the subset of individuals who were 25 and older in 1992 (Columns 2, 4, 6 and 8), as their education levels were then predetermined and could not have been affected by the conflict. Results on this subsample confirm that higher levels of education are positively associated with victimization[6]. This positive relationship between education and victimization might

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[6] This result could be explained by "guns or butter" models of conflict as a choice between production and appropriation, which suggest that the probability of victimization is linked to the resources of potential victims (Haavelmo 1954; Grossman and King 1995). If more educated people had more resources to be expropriated or were the object of envy, they might have been targeted during the conflict. In contradiction with this explanation however, the relationship between income and victimization is not robust and if anything, is

lead to an upward endogeneity bias in our results. If more pro-market and pro-social individuals were systematically targeted (see footnote 6), this will bias upward the relationship between victimization and trust, fairness and pro-market behavior and preferences. However, our main results point to a negative relationship between local pro-sociality, market orientation and victimization. Absent such correlation between education and victimization, one may thus expect the main relationship discussed in this paper to be even stronger. Other covariates that may have been expected to be correlated with victimization, such as having a family member that was member of the communist party, or having been displaced by the communist regime are positive but not significant predictors of violence. In all regressions, education and communist party membership of household members are controlled for. We also control for all characteristics that are unlikely to have changed as a result of the war, such as age, ethnicity, gender and the region where the respondent lived at the onset of conflict.

6.2. Experimental Results: Trust and Dictator Games

6.2.1. Trust Game

The main hypothesis we want to test in this paper is that civil war related violence hampers trust and, in particular, opens a gap between individual trust towards different groups. It is already apparent from the descriptive statistics displayed in Figure 2 that war victimization has a strong differential effect across the two treatments. Victimization sharply reduces the amount sent to someone in the same village, while the effect is, if anything, positive towards someone living in a different village. Regression results controlling for regional differences and individual characteristics confirm this. Panel (a) of Table 3 displays the results for those who report injury or loss of life as a result of the conflict, panel (b) for those who report both injury and loss of life. Specifications control alternatively for region and village dummies. Columns 1 and 2 investigate the main effect of victimization on trust game donations. Columns 3 to 5 include an interaction term between victimization and the treatment Same Village in order to test for differential effects of victimization on trust within

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negative. Another explanation for the relationship between education and victimization has to do with theories of political participation. Higher levels of education generate expectations, which, if unmet, can induce participation in demonstrations. These ideas have been popularized as the J-Curve theory (Davies 1974). In the context of the Tajik civil war, more educated people were probably more likely to join (or be suspected of joining) the protests that ignited clashes and retaliation by government forces.

and across villages. Columns 6 to 8 report results of regressions performed on the different treatment subsamples. Columns 10 and 11 report results of regressions performed only on the subsample of those how were 12 or younger at the onset of the war, a group for which self-selection into victimization is unlikely.

The picture emerging from the regressions is clear: war victimization destroys local trust. The coefficient on the interaction between the Same Village treatment and whether the respondent reports injury and/or loss of life is always negative and significant at the 5% to 10% level. This is confirmed in Columns 8 and 9 of Table 3b: war victims give substantially less when they play in the same village treatment, and the effect is significant at the 5% level. These effects are robust to the inclusion of village fixed effects. The effect of victimization on local trust is not only statistically but also economically significant. Injury or loss of life during the civil war is associated with between 2.8 and 3.3 Somoni average decrease in donations in the trust game to people of the same village. The average donation being 9.74, this represents a decrease within the same village by more than 30%. The effect is even stronger for those who suffered to an even greater degree during the war. People who report both injury and loss of life give about 47% less to people from the same village. The effects are robust when village fixed effects are included and actually become larger. The effect of victimization on local trust far outweighs the influence of any other individual characteristics such as age, gender, education or communist party membership, none of which has a robust effect, either on its own or interacted with the same village treatment. Only ethnicity has a larger effect, with Pamiri being more trusting. Additional results available upon request (not included for space economy) show that the effect is robust to controlling for additional individual characteristics such as income, working status, marital status, and family size and composition. The effect is also robust to ordinal logit or probit specifications.

By contrast, and somewhat surprisingly, war victims tend to give more when they play with an anonymous partner from a different village (Columns 6 and 7 and main effect in Columns 4 and 5). Victimization is associated with a 17% to 20% increase in trust to an anonymous partner from a distant village, depending on our proxy of victimization.

Taken together, these results indicate that being more directly affected by war-related violence is associated with a lasting negative effect on trust in those most likely to have been direct opponents in the conflict or those that could have provided immediate help in case of

need but didn't (here fellow villagers). At the same time, such experience is associated with an increase in pro-sociality towards a distant other that either did no direct harm or could have not helped anyway. This last result is not surprising when related to that large body of psychological literature on post traumatic growth we surveyed above (Tedeschi and Calhoun 2004; Solnit 2009). This positive effect of trauma towards an abstract other fellow national (distant enough that could have not have harmed the subject as in some civil conflicts or close enough to have potentially helped when the perpetrator came from outside the village as in international conflicts) might help explain the findings of Bellows and Miguel (2009) and Blattman (2009) and has also been found in other post-war societies such as Georgia and Sierra Leone[7] (Bauer et al. 2011). Another possible interpretation of this result has to do with the nature of the -upward- endogeneity bias discussed in Section 6.1. above. The nature of the conflict and the study of the determinants of victimization indicate that more pro-social individuals may have been more likely to be targeted during the conflict, which may explain this overall positive effect. Nevertheless, we do not believe that such selection effect may fully explain the result. We provide more formal evidence for this in Section 6.2.3. Also, when we restrict our attention to the sub-sample of youth, who were too young at the onset of the conflict to be systematically targeted, this effect does not attenuate, quite to the contrary. In any case, this makes our main result, which has to do with the *decrease* in local trust, even stronger.

The effect of war victimization on sending in the trust game remains robust and actually acquires more significant both statistically and economically when we focus our attention to the subsamples of non movers (Supplementary Appendix table A3) and those younger than 12 at the onset of conflict (Columns 10 and 11 of Tables 3a and 3b). In both sub-samples, either proxy of victimization is associated with a statistically significant decrease in sending in the same village treatment and a statistically significant increase in the different village treatment, albeit by a much smaller amount. The sample of youth is the most interesting sample in order to test additional predictions from the psychological literature concerning the malleability of preferences at different ages. Indeed, in this sample, either proxy of war victimization is associated with a roughly 50% decrease in amount sent by the first mover to a partner from the same village and an average 40% increase in amount sent towards

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[7] Where in both cases the authors argue that the enemy came from outside the subject's village, so that the results of a positive increase in egalitarianism is towards fellow villagers and not outsiders.

anonymous partners from a different village. This corroborates the idea that traumatic events leave a larger imprint on preferences if experienced during late childhood or teenage years.

The results for trustworthiness, the amount returned by the second player in the trust game, are not significant even if the sign on the victimization and the Same Village treatment interaction term is still negative in most of the regressions. We display such regressions in Table 4 for the interested reader, but, given their insignificance, we don't discuss them any further.

6.2.2. Fairness and Generosity

In addition to trust, we are interested in whether civil war related violence exerts a lasting impact on norms of fairness. We have two measures of fairness: generosity, which is measured by contributions in the dictator game and egalitarianism, which is measured by offering an equal split. We proceed in the same way as before by first exploring the main effect of conflict victimization and then interacting the victimization variable with the same village treatment to test for differential effects of victimization towards different groups. All regressions control for the full set of controls discussed in Section 5.

Table 5 reports regression results where the dependent variable consists of contributions sent by the first mover in the dictator game. Having been a victim of conflict is associated with an overall increase in generosity. The effect is substantial. Injury or loss of a household member during the conflict is associated with a 25% (within village, Column 2) to 30% (within region, Column 1) increase in dictator game giving. However, mirroring the results of the trust game, this is mainly due to an increase in generosity towards a distant other. Again, the effect is much larger for those who experienced such traumatic events in their childhood or early teens. Columns 10 and 11 report the results for the sub-sample of those 18 or younger at the end of the war. In this group, averaged across specifications and victimization proxies, victims give 55% more towards an anonymous partner from a different village but 80% less to someone from the same village (column 10, Table 5a and 5b). Such effects nevertheless fall short of standard levels of significance when village fixed effects are included.

A similar picture is obtained for preferences for egalitarianism. Regression results are reported in Table 6. Preferences for an egalitarian split among victims increase by about 18% towards respondents from a different village but decrease by a superior amount (20% for our first victimization proxy, 40% for the second) towards someone from the same village. The results are robust to the inclusion of village fixed effects and to using alternative subsamples of non-movers (see Table A4 in Supplementary Appendix) as well as different specifications (logit or OLS). In the subsample of those who were 18 or younger at the end of the conflict, again, the results are of the same sign and larger in magnitude.

6.2.3. Additional Robustness to Selection on Unobservables

Following Altonji, Elder, and Taber (2005) and Nunn and Wantchekon (*forth.*), ratios are computed that reflect how much greater the influence of unobservable factors would need to be, relative to observable factors, to explain away the full positive relationship between war victimization and individual behaviors in game. This test is based on the ratio of coefficients of regressions including full or restricted sets of control variables.[8] The intuition is that the smaller the difference between the two coefficients, the less the estimate is affected by selection on observables so that the larger the selection on unobservables needs to be, relative to observables, in order to explain away the entire effect of the variables of interest. Obtained ratios, displayed in Table7, make it unlikely that unobserved heterogeneity could explain away the relationship between victimization and trust game behavior in the same village treatment. The ratios obtained for the dictator game and egalitarian split results yield similar conclusions.

A frequent objection to experimental evidence however is that behavior in games may poorly reflect actual behavior. We therefore turn in the next subsection to more direct survey evidence on respondents' stated preferences and actual behavior. Such evidence largely corroborates the conclusions drawn from our experimental evidence.

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[8] The first coefficient $\hat{\beta}^R$ is obtained when only the victimization variables are controlled for. The second, $\hat{\beta}^F$ is obtained when the full set of observable characteristics are controlled for. The ratio is calculated as:

$$\hat{\beta}^F / \left(\hat{\beta}^R - \hat{\beta}^F \right)$$

6.3. Survey Results: Market Integration, Economic and Political Preferences

We first investigate directly respondents' stated and revealed preferences on participation in impersonal exchange. Second, we investigate the strength of formal institutions vs. kinship-based informal institutions, particularly in what relates to conflict adjudication. An important literature and in particular Greif (2000) has stressed the importance of conflict adjudication mechanisms in enforcing economic exchange. Historically, the evolution of such institutions from a kinship and interpersonal basis to an open and impersonal basis has been associated with the "birth of impersonal exchange" (Greif 2006). Third, we investigate civil war violence as a determinant of collective action and group participation.

6.3.1. Market integration and participation

Table 8 presents regression results where we use several dependent variables in order to measure willingness to participate in impersonal exchange and preferences for free markets through survey questions. Consistently with the observed decrease in the offers in the trust game, victims of civil war violence have a significantly lower willingness to engage in anonymous exchange. We measure such willingness by the following survey question: "When you go to the market, how important is it to buy from a seller that you know personally?", with a 4 points scale answer from "not important at all" to "essential". The effect of conflict is positive, statistically significant and robust to the inclusion of village fixed effects, signaling a decreased willingness to participate in exchange with an anonymous trader.

We also measure actual participation in markets by asking questions about the proportion of different food items purchased through market exchange, versus self produced or procured through donations. Those who suffered injury or loss of life in the civil war are less integrated into markets according to this measure (see Table A5 in Supplementary Appendix).

War victimization is also associated with a significant decrease in preferences for a free market and for market liberalization, which we measure through two survey questions described in Table 1. Support for free markets and democracy is roughly 20% lower among victims of civil war compared to other individuals from the same village. Preferences for a democratic system are also significantly lower among civil war victims.

6.3.2. Kinship vs. rule of law

Table 9 present regressions results where the dependent variables consist of answers to different questions aimed at capturing the strength of clannishness and kinship ties. We included a question that measures to what extent, when facing a conflict situation, respondents turn to legal and formal institutions – the police or village leader - or to their kin in order to solve the conflict. We are particularly interested in conflicts that may be associated with market transactions, and we probe about recourses in three potential conflict situations: (i) the respondent "lent money to someone who does not repay", (ii) he/she "sold a good to someone who refuses to pay, or (iii) "someone knowingly sold him/her a defective good". We build an index that reflects the number of times the respondent would turn to his/her relatives, as opposed to the police or village leader, in order to solve such a conflict. We then regress this index on war experiences. Results are displayed in Columns 1 and 2 of Table 9. Our first measure of victimization, *Injured or Killed*, is positively and significantly associated with a reinforcement of kinship ties. Accordingly, civil war victims are also less likely to support the statement that "If someone has information that may help justice be done, generally he or she should report it to the police" (Columns 3 and 4).

The third variable that we use to measure the strength of kinship ties is opinions about the freedom to marry. As stressed by Greif (2006), restricted and consanguineous marriages have historically provided one means of creating and maintaining kinship groups. We ask in the survey whether the respondent supports freedom to marry or rather thinks best for parents to choose a spouse for their children. We regress a dummy variable taking value 1 if respondents support the freedom to marry on war experiences. Results are displayed in Columns 13 and 14 of Table 9. War experience is associated with a decrease in the support for free marriage, even when we control for whether the respondent herself married freely.

6.3.3. Participation in groups

Several survey questions aim at capturing participation in groups and association. First, we ask respondents whether they participated in any community meetings during the week preceding our team's visit. Second, we build an index variable that sums the number of groups and associations the respondents belong to. We ask about a variety of groups, such as mosque and religious organization, NGOs, neighborhood groups, labor unions, fraternal

groups and youth associations. This index takes values from 0 to 5. Group participation is low on average in our sample, which is consistent with the literature documenting evidence of low levels of civil society development in post-Soviet Republics (Howard 2003). The mean of the group participation index is 0.79. 40% of respondents do not participate in any group. However, civil war experience is significantly and positively associated with group participation. Regression results are displayed in Columns 3 to 8 of Table 10. War victims are also more likely to have attended community meetings (Columns 1 and 2 of Table 10). This mirrors the result found by an emerging literature that finds a link between civil war and local collective action, namely by Bellows and Miguel (2009) in the case of Sierra Leone.

Group membership and civic participation have been widely used in the literature as measures of social capital and, as such, associated with positive development outcomes (for a recent review, see Guiso, Sapienza and Zingales 2010). However, this acceptation of social capital may also have negative connotation if it leads to the exclusion of outsiders (Bourdieu 1985, Portes, 1998). Our results may just highlight such potential negative implication since group participation among war victims is actually associated with a *decrease* in trust as measured by the trust game. Results are displayed in Table 11. The first variable of interest is an interaction between the same village treatment and a dummy that reflects whether people participate in groups or association. The positive and statistically significant coefficient on this variable reflects the usual result of the literature that group participation is correlated with higher levels of trust. The second variable of interest is an interaction between the same village treatment, a dummy that indicates group participation and one of our victimization proxy (*Injured and Killed*). The coefficient of this interaction term is negative and statistically significant, indicating that those who participate in groups but are victims of the civil war send less to their fellow villagers in the trust game. The effect holds whether we control for region or village fixed effects. Such evidence is consistent with our previous results that civil war victimization is associated with a reinforcement of clannishness and kinship ties. Participation in groups in this case may not be taken as an indication of inclusive social capital but rather as a sign of victims folding back towards exclusive groups (or "bonding" social capital rather than "bridging" social capital).

We also investigate which particular group and association war victims are more likely to join. It is mainly religious groups and, to a lesser extent, labor unions that receive a boost in

membership among war victims. The effect is not significant for any other group. In Tajikistan, participation in religious groups may be perceived as a form of opposition to the government. As a matter of fact, both war veterans and, more worryingly, those who participated in fighting since the Peace Agreement are also significantly more likely to be members of a Mosque and religious groups (see Table A6 for full results).

7. Conclusions

This paper discusses the results of a study designed to investigate the effects of civil war related violence on cooperative, market-oriented social norms and preferences. Much of the literature on trust and social capital highlights that social preferences characterized by norms of fairness and trust can solve for the cooperation and coordination problems implied by interpersonal exchange (Fafchamps 2004; Fehr et al. 2008). When it comes to understanding the effect that war has on institutions and the economy in general, the literature points to a complex scenario of negative as well as surprisingly positive effects. Among the positive, high collective action and voting might increase social capital in the form of group and association participation. Alternatively, wars could be potentially devastating because they undermine both the institutional framework of the state as well as the social fabric for cooperation.

To contribute to this literature, we collected experimental evidence and survey data on prosocial and economic preferences for 426 randomly selected subjects in different regions of Tajikistan. Our results show that, 13 years after the civil war ended, inflicted violence has undermined social trust at the village level, eroded support for market liberalization and democratic reform and reinforced reliance on kinship groups. At the same time, victims participate more in community meetings, associations and religious groups, but we find this increase in collective action associated with a further erosion of local trust, indicating that the kind of social capital that gets enhanced in the aftermath of a civil war might not be the inclusive type which is capable of supporting market development, but the kin-, network-based one that may actually hinder the emergence of efficient, impersonal markets.

A key insight of our paper, taken together with the emerging literature on the behavioral legacy of conflict in other contexts, is that the long-term effect of conflict will likely depend on its specificity. If violence is of a nature that exacerbates the risks and uncertainties of social exchange, the consequences for market development may be dire. Our results,

together with other studies, find some evidence of positive pro-social elements among victims. However, in the case of the Tajik civil conflict, such positive elements pale in face of the disruption of trust and fairness at the local level. We conjecture that the nature of fighting in the Tajik civil war, characterized by localized intra-group conflict and the inability to apply basic cues to identify friends or foe has led to long lasting disruption of norms and preferences that sustain impersonal exchange and has created long-term challenges to institution building. In this context, civil conflict increases the likelihood that Tajikistan will remain "lost in transition", failing to make significant progress either on democratization or market and economic development, which in turn may increase the likelihood of recurrent conflict.

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Table 1: Summary Statistics

Variables	Description	Obs	Mean	S.d.	Min	Max
Victimization						
Injured or Killed	Respondent or household member injured during war	426	0.21	0.41	0	1
Injured & Killed	Household member killed during war	426	0.13	0.34	0	1
Experimental data:						
Offer Trust Game	Amount sent trust game (0, 5, 10, 15, 20)	426	9.74	6.97	0	20
Offer Dictator Game	Amount sent dictator game (0, 10, 20, 30, 40)	426	10.26	9.69	0	40
Egalitarian split	1 if offer=20 in dictator game	426	0.23	0.42	0	1
Same village	same village treatment	426	0.46	0.5	0	1
Survey data:						
Importance knowing trader personally	Scale: not important at all (0) to very important/essential (4)	424	1.82	1.03	1	4
Freedom in economy	1 if agree or strongly agree to: "We are more likely to have a healthy economy if the government allows more freedom for individuals to do as they wish"	407	0.55	0.5	0	1
Favor market economy	1 if "market economy is preferable to any other form of economic system"	421	0.55	0.5	0	1
Favor democracy	1 if "democracy is preferable to any other form of political system"	419	0.6	0.49	0	1
Community meeting	1 if attended community meetings last month	412	0.37	0.48	0	1
Part. groups and assoc.	Sum of dummies=1 if respondent member of: mosque/religious group, NGO, neighborhood group, fraternal group and youth association	410	0.79	0.92	0	5
Member mosque	1 if member mosque/religious group	344	0.33	0.47	0	1
Turn to relatives if cheated in markets	1 if turn to relatives first in either situation: not repaid for loan, sold a good and was not paid, was sold a defective good	426	0.14	0.39	0	2
Should report info to police	1 if agree or strongly agree to: "If someone has information that may help justice be done, generally he or she should report it to the police"	404	0.47	0.5	0	1
Support freedom to marry	1 if favors personal freedom to marry rather than parents choosing spouse for their children	399	0.81	0.39	0	1
Controls						
Age		419	39.84	13.5	17	77
Gender	1 if male	422	0.29	0.46	0	1
Any CP	1 if either respondent, her mother, father or other HH member member of Communist party	426	0.09	0.29	0	1
Comp. education	Highest education level: compulsory education	422	0.68	0.47	0	1
Second education	Highest education level: secondary education	422	0.12	0.32	0	1
Higher education	Highest education level: higher education	422	0.14	0.35	0	1
Pamiri, Shugnani	Pamiri, Shugnani ethnicity	426	0.19	0.39	0	1
Uzbek	Uzbek ethnicity	426	0.05	0.21	0	1
Region lived in 1992:						
Pamir		426	0.19	0.39	0	1
Dushanbe		426	0.23	0.42	0	1
Gharm		426	0.2	0.4	0	1
Khatlon		426	0.38	0.49	0	1

Table 2: Determinants of Victimization

OLS regression

Dep var:	1	2	3	4	5	6	7	8
	Injured or Killed				Injured and Killed			
Sample:	whole sample		>25 at onset of conflict		whole sample		>25 at onset of conflict	
age	0.002	0.001	**0.008**	0.007	0.001	0.000	0.007	0.005
	[0.227]	[0.377]	[0.088]	[0.161]	[0.352]	[0.685]	[0.144]	[0.258]
gender	0.002	-0.001	-0.060	-0.053	0.065	0.066	-0.012	-0.022
	[0.967]	[0.989]	[0.324]	[0.473]	[0.295]	[0.325]	[0.821]	[0.685]
Pamiri		0.868		0.582		-0.059		**-0.300**
		[0.000]		[0.000]		[0.448]		[0.000]
Uzbek	**-0.143**	**-0.122**	-0.096	-0.023	**-0.096**	**-0.113**	**-0.090**	-0.060
	[0.026]	[0.000]	[0.269]	[0.538]	[0.050]	[0.003]	[0.088]	[0.101]
Dushanbe	**0.891**	**0.927**	**0.893**	**0.853**	-0.009	0.021	-0.036	-0.013
	[0.000]	[0.000]	[0.000]	[0.000]	[0.849]	[0.707]	[0.421]	[0.769]
Gharm	**1.272**	**1.310**	**1.269**	**1.338**	**0.207**	**0.318**	0.197	**0.489**
	[0.000]	[0.000]	[0.000]	[0.000]	[0.045]	[0.000]	[0.127]	[0.000]
Khatlon	0.072	**0.907**	0.109	**0.563**	0.079	-0.048	0.084	**-0.266**
	[0.279]	[0.000]	[0.296]	[0.000]	[0.128]	[0.579]	[0.187]	[0.002]
any member CP	0.012	0.003	**0.245**	0.267	-0.029	-0.031	**-0.045**	-0.027
	[0.862]	[0.963]	[0.091]	[0.109]	[0.440]	[0.348]	[0.098]	[0.333]
displaced Comm reg	0.142	0.172	0.170	0.151	0.089	0.099	0.008	-0.028
	[0.343]	[0.295]	[0.155]	[0.222]	[0.456]	[0.468]	[0.906]	[0.706]
urban	-0.037	0.021	**-0.177**	**-0.177**	0.048	0.056	0.060	-0.060
	[0.364]	[0.729]	[0.007]	[0.001]	[0.242]	[0.254]	[0.232]	[0.041]
comp edu	0.166	**0.238**	**0.303**	**0.370**	0.068	0.121	**0.208**	**0.234**
	[0.122]	[0.051]	[0.025]	[0.034]	[0.447]	[0.210]	[0.044]	[0.018]
second edu	**0.256**	**0.293**	**0.426**	**0.479**	0.077	0.092	**0.299**	**0.321**
	[0.020]	[0.016]	[0.020]	[0.024]	[0.282]	[0.270]	[0.052]	[0.047]
higher edu	0.101	0.178	**0.259**	**0.319**	0.022	0.078	0.099	0.124
	[0.441]	[0.198]	[0.076]	[0.070]	[0.855]	[0.550]	[0.335]	[0.249]
mid income	-0.036	-0.045	-0.032	-0.043	-0.031	-0.038	-0.046	**-0.105**
	[0.474]	[0.344]	[0.592]	[0.443]	[0.492]	[0.388]	[0.557]	[0.080]
rich	-0.042	-0.035	-0.103	-0.057	-0.053	-0.039	-0.051	-0.051
	[0.327]	[0.481]	[0.123]	[0.278]	[0.267]	[0.432]	[0.502]	[0.339]
FE	region	psu	region	psu	region	psu	region	psu
Observations	377	377	141	141	377	377	141	141
R-squared	0.17	0.23	0.25	0.38	0.15	0.24	0.21	0.39
Mean Dep var	0.21		0.20		0.13		0.12	

Notes to Table 2: robust standard errors clustered at the village level. All regressions with a constant. P-values in brackets. For a description of the variables, see Table 1. Columns 4 and 8: sample restricted to respondents 25 or older in 1992 (43 and older today). Excluded ethnicity is Tajik, excluded 1992 region isPamir, ecluded education is: compulsory education not completed, excluded income is poor (lower third of the income distribution).

Table 3: Trust Regression Results

OLS regression

Dependent variable: Amount sent by first mover in the trust game

Panel a.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Mean dep. var.			9.742			9.035		10.556		10.076	
Injured or Killed	0.654	0.688	1.454	**1.902**	**2.049**	**1.972**	**1.561**	-1.332	-1.313	**3.383**	**4.007**
	[0.366]	[0.382]	[0.133]	[0.044]	[0.036]	[0.041]	[0.095]	[0.301]	[0.350]	[0.080]	[0.078]
Same Village	1.401	1.345	**2.552**	2.001	2.142					2.901	3.049
	[0.220]	[0.394]	[0.174]	[0.121]	[0.218]					[0.125]	[0.221]
Same Vill. * Inj. or Kill.			-2.886	-2.901	-3.261					-4.816	-4.766
			[0.097]	[0.072]	[0.057]					[0.035]	[0.042]
Age	0.009	0.015		0.010	0.016	0.016	0.020	0.014	0.016	0.071	0.117
	[0.744]	[0.614]		[0.734]	[0.600]	[0.662]	[0.542]	[0.806]	[0.793]	[0.639]	[0.510]
Gender	-0.318	-0.653		-0.218	-0.590	-2.616	-2.842	1.759	1.233	0.250	-0.132
	[0.545]	[0.274]		[0.668]	[0.329]	[0.003]	[0.003]	[0.093]	[0.310]	[0.847]	[0.929]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.057	0.114	0.098	0.064	0.123	0.118	0.183	0.071	0.224	0.137	0.258
Panel b.						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Injured & Killed	-0.376	-0.017	1.372	1.433	**1.863**	1.699	1.513	**-3.433**	**-3.463**	3.536	**4.500**
	[0.696]	[0.986]	[0.244]	[0.201]	[0.071]	[0.150]	[0.166]	[0.029]	[0.046]	[0.102]	[0.074]
Same Village	1.361	1.314	**2.623**	1.912	2.121					2.556	2.587
	[0.244]	[0.412]	[0.165]	[0.129]	[0.231]					[0.177]	[0.314]
Same Vill. * Inj. & Kill.			-4.720	-4.352	-4.776					-5.251	-5.141
			[0.037]	[0.031]	[0.037]					[0.037]	[0.083]
Age	0.011	0.016		0.008	0.013	0.013	0.018	0.012	0.012	0.080	0.119
	[0.690]	[0.583]		[0.792]	[0.677]	[0.720]	[0.599]	[0.830]	[0.846]	[0.593]	[0.497]
Gender	-0.305	-0.663		-0.198	-0.605	-2.733	-2.951	1.965	1.323	0.182	-0.372
	[0.559]	[0.254]		[0.702]	[0.314]	[0.002]	[0.002]	[0.065]	[0.294]	[0.893]	[0.811]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.057	0.113	0.103	0.067	0.125	0.113	0.181	0.087	0.237	0.135	0.256

Notes: P-values in brakets (robust standard errors clustered by sampling village). All regressions include a constant.

Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Table 4: Trustworthiness Regression Results

OLS regression

Dependent variable: Mean amount returned by second mover in the trust game

Panel a.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Mean dep. var.			13.496			13.386		13.621		13.358	
Injured or Killed	0.640	-0.281	0.527	1.336	0.481	0.284	-0.302	0.432	-0.693	0.649	-0.074
	[0.665]	[0.847]	[0.731]	[0.471]	[0.795]	[0.878]	[0.876]	[0.842]	[0.746]	[0.781]	[0.975]
Same Village	0.069	-0.836	0.397	0.403	-0.390					0.170	0.509
	[0.942]	[0.509]	[0.763]	[0.670]	[0.749]					[0.912]	[0.795]
Same Vill. * Inj. or Kill.			-1.803	-1.619	-1.824					-0.261	-0.537
			[0.467]	[0.502]	[0.493]					[0.928]	[0.872]
Age	-0.035	-0.045		-0.035	-0.045	-0.055	-0.060	-0.015	-0.043	-0.105	-0.076
	[0.353]	[0.230]		[0.354]	[0.233]	[0.282]	[0.271]	[0.778]	[0.404]	[0.513]	[0.697]
Gender	0.806	0.867		0.862	0.902	0.704	0.990	1.502	0.537	1.081	0.399
	[0.374]	[0.373]		[0.370]	[0.367]	[0.589]	[0.500]	[0.246]	[0.697]	[0.549]	[0.858]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.107	0.177	0.140	0.108	0.179	0.117	0.202	0.135	0.247	0.143	0.274

Panel b.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Injured & Killed	1.302	0.216	1.518	2.640	1.805	1.671	1.181	0.149	-1.720	-0.877	-1.422
	[0.300]	[0.825]	[0.429]	[0.178]	[0.322]	[0.352]	[0.498]	[0.936]	[0.125]	[0.748]	[0.637]
Same Village	0.075	-0.820	0.619	0.483	-0.138					-0.071	0.342
	[0.939]	[0.522]	[0.603]	[0.584]	[0.898]					[0.963]	[0.859]
Same Vill. * Inj. & Kill.			-3.656	-3.221	-4.038					0.931	0.371
			[0.187]	[0.237]	[0.171]					[0.742]	[0.917]
Age	-0.036	-0.046		-0.038	-0.049	-0.059	-0.062	-0.014	-0.045	-0.106	-0.076
	[0.334]	[0.218]		[0.293]	[0.181]	[0.244]	[0.256]	[0.791]	[0.373]	[0.514]	[0.694]
Gender	0.746	0.861		0.826	0.910	0.645	0.966	1.514	0.583	1.157	0.512
	[0.403]	[0.371]		[0.388]	[0.369]	[0.614]	[0.503]	[0.248]	[0.677]	[0.519]	[0.819]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.108	0.177	0.143	0.112	0.183	0.121	0.203	0.134	0.249	0.143	0.275

Notes: P-values in brakets (robust standard errors clustered by sampling village). All regressions include a constant.

Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Table 5: Dictator Game Regression Results

OLS regression

Dependent variable: Amount sent by first mover in the dictator game

Panel a.	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Mean dep. var.			10.258			10.307		10.202		10.758	
Injured or Killed	**3.065**	2.631	**2.753**	**3.733**	**2.893**	**2.950**	**2.199**	2.128	1.367	**6.258**	4.699
	[0.039]	[0.105]	[0.077]	[0.008]	[0.048]	[0.018]	[0.041]	[0.367]	[0.590]	[0.072]	[0.256]
Same Village	0.029	**-2.300**	-1.645	0.350	**-2.146**					-1.510	-3.807
	[0.979]	[0.058]	[0.365]	[0.762]	[0.108]					[0.631]	[0.407]
Same Vill. * Inj. or Kill.			-0.460	-1.553	-0.627					**-8.866**	-7.269
			[0.815]	[0.352]	[0.743]					[0.037]	[0.169]
Age	-0.046	**-0.054**		-0.045	**-0.054**	-0.110	-0.107	-0.001	-0.021	0.107	0.175
	[0.126]	[0.084]		[0.128]	[0.085]	[0.069]	[0.082]	[0.986]	[0.702]	[0.615]	[0.443]
Gender	-1.196	-0.657		-1.143	-0.645	-0.984	-0.613	-0.355	-0.621	1.430	2.209
	[0.350]	[0.645]		[0.374]	[0.653]	[0.614]	[0.758]	[0.807]	[0.724]	[0.486]	[0.324]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.147	0.204	0.175	0.148	0.204	0.156	0.240	0.231	0.294	0.323	0.397
Panel b.						Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 12 or Younger 1992	
Injured & Killed	**3.033**	**2.508**	**2.892**	**4.292**	**3.257**	**3.656**	**2.832**	1.602	0.509	**7.506**	5.725
	[0.018]	[0.067]	[0.072]	[0.001]	[0.003]	[0.012]	[0.065]	[0.552]	[0.852]	[0.015]	[0.124]
Same Village	-0.027	**-2.377**	-1.580	0.356	-2.055					-2.058	-4.403
	[0.981]	[0.056]	[0.383]	[0.773]	[0.121]					[0.465]	[0.284]
Same Vill. * Inj. & Kill.			-1.825	-3.028	-1.903					**-10.199**	-7.557
			[0.493]	[0.200]	[0.407]					[0.009]	[0.130]
Age	-0.043	-0.052		-0.046	**-0.054**	-0.117	-0.112	0.005	-0.018	0.126	0.173
	[0.154]	[0.101]		[0.137]	[0.100]	[0.058]	[0.072]	[0.915]	[0.750]	[0.543]	[0.443]
Gender	-1.346	-0.806		-1.271	-0.783	-1.192	-0.790	-0.368	-0.651	1.216	1.875
	[0.306]	[0.580]		[0.339]	[0.594]	[0.553]	[0.700]	[0.811]	[0.714]	[0.550]	[0.393]
Extended Controls											
Region lived in 1992	yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE	region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations	416	416	426	416	416	224	224	192	192	131	131
R-squared	0.143	0.201	0.171	0.146	0.202	0.158	0.241	0.227	0.292	0.324	0.396

Notes: P-values in brakets (robust standard errors clustered by sampling village). All regressions include a constant.

Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Table 6: Egalitarian Regression Results

Probit regression

Dependent variable: 1 if first mover in the dictator game divides equally the endowment

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
							Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 16 or Younger in W:	
Mean dep. var.				0.228			0.219		0.237		0.259	
Injured or Killed	coefficient	0.323	0.318	**0.649**	**0.690**	**0.651**	**0.544**	0.487	-0.144	-0.061	**1.128**	**0.986**
	p-val	[0.101]	[0.156]	[0.006]	[0.011]	[0.015]	[0.049]	[0.110]	[0.723]	[0.891]	[0.023]	[0.080]
	marg. effect	0.09	0.08	0.16	0.17	0.16	0.13	0.12	-0.04	-0.02	0.29	0.25
Same Village	coefficient	0.120	-0.115	0.070	**0.301**	0.085					0.040	-0.164
	p-val	[0.392]	[0.570]	[0.701]	[0.006]	[0.612]					[0.851]	[0.531]
	marg. effect	0.03	-0.03	0.02	0.07	0.02					0.01	-0.04
Same Vill. * Inj. or Kill.	coefficient			**-0.798**	**-0.904**	**-0.864**					**-1.791**	**-1.524**
	p-val			[0.092]	[0.053]	[0.080]					[0.027]	[0.051]
	marg. effect			-0.19	-0.23	-0.21					-0.46	-0.39
Age	coefficient	-0.003	-0.006		-0.003	-0.005	-0.006	-0.006	0.003	-0.003	-0.024	-0.022
	p-val of coeff	[0.468]	[0.159]		[0.492]	[0.192]	[0.331]	[0.398]	[0.645]	[0.623]	[0.317]	[0.426]
	marg effect	-0.00	-0.00		-0.00	-0.00	-0.00	-0.00	0.00	-0.00	-0.01	-0.01
Gender	coefficient	-0.091	-0.032		-0.061	-0.019	-0.218	-0.237	0.246	0.244	-0.285	-0.129
	p-val of coeff	[0.483]	[0.842]		[0.634]	[0.905]	[0.351]	[0.369]	[0.129]	[0.239]	[0.344]	[0.693]
	marg effect	-0.02	-0.01		-0.02	-0.00	-0.05	-0.06	0.07	0.07	-0.07	-0.03
Extended controls		yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE		region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations		416	416	426	416	416	224	224	192	192	138	138
							Sub-sample Distant Village		Sub-sample Same Village		Sub-sample 18 or Younger in W:	
Injured or Killed	coefficient	**0.327**	0.282	**0.747**	**0.823**	**0.742**	**0.698**	**0.598**	-0.668	-0.626	**0.979**	**0.883**
	p-val	[0.078]	[0.224]	[0.001]	[0.000]	[0.003]	[0.001]	[0.025]	[0.238]	[0.382]	[0.004]	[0.038]
	marg. effect	0.09	0.07	0.19	0.21	0.18	0.17	0.17	-0.19	-0.17	0.28	0.23
Same Village	coefficient	0.117	-0.122	0.078	0.275	0.089					-0.033	-0.168
	p-val	[0.417]	[0.550]	[0.659]	[0.011]	[0.590]					[0.856]	[0.590]
	marg. effect	0.03	-0.03	0.02	0.07	0.02					-0.01	-0.04
Same Vill. * Inj. or Kill.	coefficient			**-1.526**	**-1.572**	**-1.606**					**-1.254**	**-1.189**
	p-val			[0.011]	[0.006]	[0.011]					[0.028]	[0.069]
	marg. effect			-0.38	-0.41	-0.40					-0.36	-0.31
Age	coefficient	-0.003	-0.006		-0.004	-0.006	-0.008	-0.008	0.003	-0.003	-0.015	-0.019
	p-val of coeff	[0.522]	[0.189]		[0.357]	[0.129]	[0.254]	[0.315]	[0.682]	[0.663]	[0.450]	[0.384]
	marg effect	-0.00	-0.00		-0.00	-0.00	-0.00	-0.00	0.00	-0.00	-0.00	-0.01
Gender	coefficient	-0.110	-0.052		-0.075	-0.039	-0.258	-0.269	0.264	0.232	-0.321	-0.258
	p-val of coeff	[0.407]	[0.746]		[0.566]	[0.806]	[0.285]	[0.320]	[0.098]	[0.253]	[0.217]	[0.378]
	marg effect	-0.03	-0.01		-0.02	-0.01	-0.06	-0.07	0.08	0.06	-0.09	-0.07
Individual controls		yes	yes	no	yes	yes	yes	yes	yes	yes	yes	yes
FE		region	psu	psu	region	psu	region	psu	region	psu	region	psu
Observations		416	416	426	416	416	224	224	192	192	180	180

Notes: Robust standard errors clustered by experimental village. All regressions include a constant.

Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Table 7: Assessing the Bias due to Selection on Unobservables

		Trust Game							
		Injured or Killed		Inj. or Kill*SameVillage		Injured & Killed		Inj. & Kill*SameVillage	
		region FE	psu FE	region FE	psu FE	region FE	psu FE	region FE	psu FE
Controls, full set	none	-1.55	-1.89	-9.27	-8.23	0.92	0.02	-32.62	-60.00
Extended set of controls	Restricted set (age, gender, region lived in 92, ethnicity)	-2.34	-2.83	-8.42	-13.16	1.43	0.04	-35.33	53.33
Restricted set (age, gender, region lived in 92, ethnicity)	none	-2.60	-3.67	81.67	-20.27	4.31	1.53	-412.00	-28.76

		Dictator Game							
		Injured or Killed		Inj. or Kill*SameVillage		Injured & Killed		Inj. & Kill*SameVillage	
		region FE	psu FE	region FE	psu FE	region FE	psu FE	region FE	psu FE
Controls, full set	none	-301.00	-258.00	3.71	-5.60	-19.19	-7.64	4.69	-61.67
Extended set of controls	Restricted set (age, gender, region lived in 92, ethnicity)	13.68	10.32	-20.14	-2.07	51.17	126.00	11.19	12.33
Restricted set (age, gender, region lived in 92, ethnicity)	none	-14.04	-10.88	2.98	1.71	-14.23	-7.26	8.81	-11.11

		Egalitarian Split							
		Injured or Killed		Inj. or Kill*SameVillage		Injured & Killed		Inj. & Kill*SameVillage	
		region FE	psu FE	region FE	psu FE	region FE	psu FE	region FE	psu FE
Controls, full set	none	4.00	4.00	23.00	-21.00	8.00	na	35.00	-33.00
Extended set of controls	Restricted set (age, gender, region lived in 92, ethnicity)	na	8.00	-23.00	-10.50	na	na	na	na
Restricted set (age, gender, region lived in 92, ethnicity)	none	4.00	9.00	11.00	19.00	8.00	na	35.00	-33.00

Table 8: Market participation and Economic and Political Preferences

OLS regression

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Importance of knowing trader		Freedom in economy		Favor market economy		Favor democracy	
Injured or Killed	0.588 [0.001]		-0.247 [0.015]		-0.191 [0.008]		-0.170 [0.008]	
Injured and Killed		0.442 [0.038]		-0.310 [0.002]		-0.094 [0.132]		-0.177 [0.012]
Extended controls	yes	yes	yes	yes	yes	yes	yes	yes
Village FE	yes	yes	yes	yes	yes	yes	psu	psu
Observations	416	416	400	400	414	414	412	412
R-squared	0.184	0.158	0.259	0.263	0.203	0.188	0.303	0.299
Mean dep. var.	1.80		0.55		0.55		0.60	

Notes: Robust standard errors clustered by experimental village. All regressions include a constant.
Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Table 9: Conflict resolution and kinship

OLS regression

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Turn to relatives if		Should report		Support freedom to	
Injured or Killed	**0.120**		**-0.377**		**-0.095**	
	[0.077]		[0.001]		[0.027]	
Injured and Killed		0.060		**-0.344**		-0.070
		[0.334]		[0.001]		[0.208]
Extended controls	yes	yes	yes	yes	yes	yes
Village FE	yes	yes	yes	yes	yes	yes
Observations	416	416	397	397	344	344
R-squared	0.081	0.068	0.121	0.092	0.547	0.543
Mean dep. var.	0.13		0.46		0.81	

Notes: Robust std. err. clustered by exp. village. All regressions include a constant.
Extended Controls: former comm. in HH, edu., ethnicity, region where lived in 1992.
Col. 5, 6 include a contr. for whether respondent were free him/herself to marry.

Table 10: Participation in groups

OLS regression

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)
	Community		Participation in		Member Mosque or	
Injured or Killed	**0.324**		**0.551**		**0.460**	
	[0.004]		[0.000]		[0.000]	
Injured and Killed		**0.220**		**0.406**		**0.399**
		[0.028]		[0.000]		[0.002]
Extended controls	yes	yes	yes	yes	yes	yes
Village FE	yes	yes	yes	yes	yes	yes
Observations	405	405	402	402	337	337
R-squared	0.227	0.190	0.269	0.242	0.290	0.228
Mean dep. var.	0.38		0.79		0.33	

Notes: Robust std. err. clustered by exp. village. All regressions include a constant.
Extended Controls: former comm. in HH, edu., ethnicity, region where lived in 1992.

Table 11: Participation in Groups, War Experience and Trust

OLS regression

Dependent variable: Amount sent by first mover in the trust game

	(1)	(2)	(3)	(4)	(5)	(6)
Same Village	1.242	1.271	1.041	1.079	1.403	1.207
	[0.243]	[0.315]	[0.409]	[0.450]	[0.380]	[0.446]
Participation in groups and assoc.	-0.165	-0.380	-0.523	-0.131	-0.372	-0.504
	[0.635]	[0.293]	[0.149]	[0.701]	[0.351]	[0.204]
Injured and Killed	-0.256	1.513	**-7.528**	**0.076**	**1.940**	**-5.344**
	[0.791]	[0.180]	[0.000]	[0.936]	[0.052]	[0.000]
Same Vill. *Part. Groups		0.726	**1.022**		0.810	**1.079**
		[0.197]	[0.051]		[0.253]	[0.099]
Same Vill. * Inj. and Kill.		**-4.340**	8.926		**-4.800**	5.640
		[0.028]	[0.083]		[0.033]	[0.256]
Part. Groups * Inj. and Kill.			9.878			7.937
			[0.000]			[0.000]
Same Vill. * Part. Groups * Inj. and Kill.			-15.171			-11.926
			[0.010]			[0.045]
Extended Controls	yes	yes	yes	yes	yes	yes
FE	region	psu	region	psu	region	psu
Observations	402	402	402	402	402	402
R-squared	0.058	0.071	0.089	0.112	0.126	0.137

Notes: Robust standard errors clustered by experimental village. All regressions include a constant.

Extended Controls include: former communist in HH, education, ethnicity, region where lived in 1992.

Figure 1: Map of victimization and surveyed villages. Intensity of civil war violence: Proportion of respondents in our sample affected by conflict (Injured or Killed).
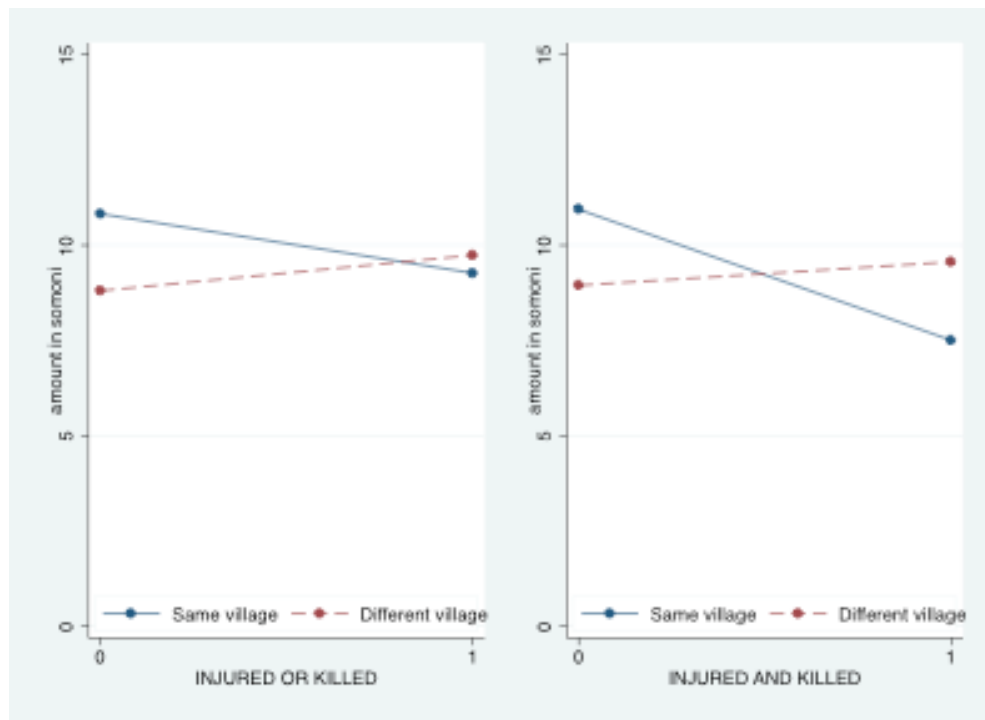
Figure 2: Trust Game: Amount Sent in the Trust Game

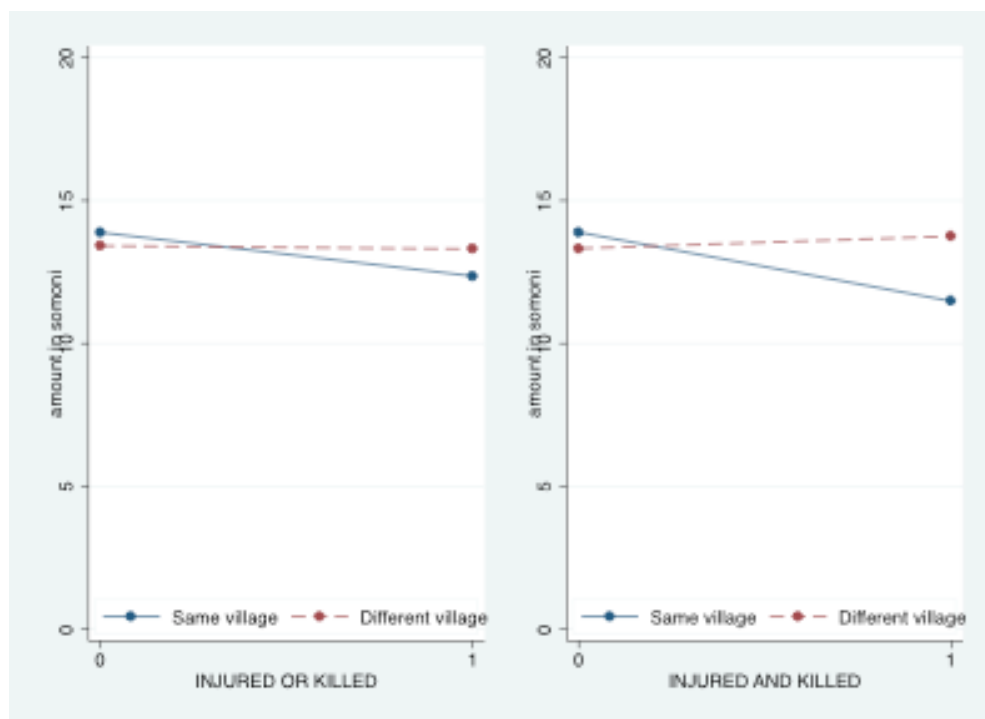**Figure 3: Trustworthiness: Amount Returned in the Trust Game**
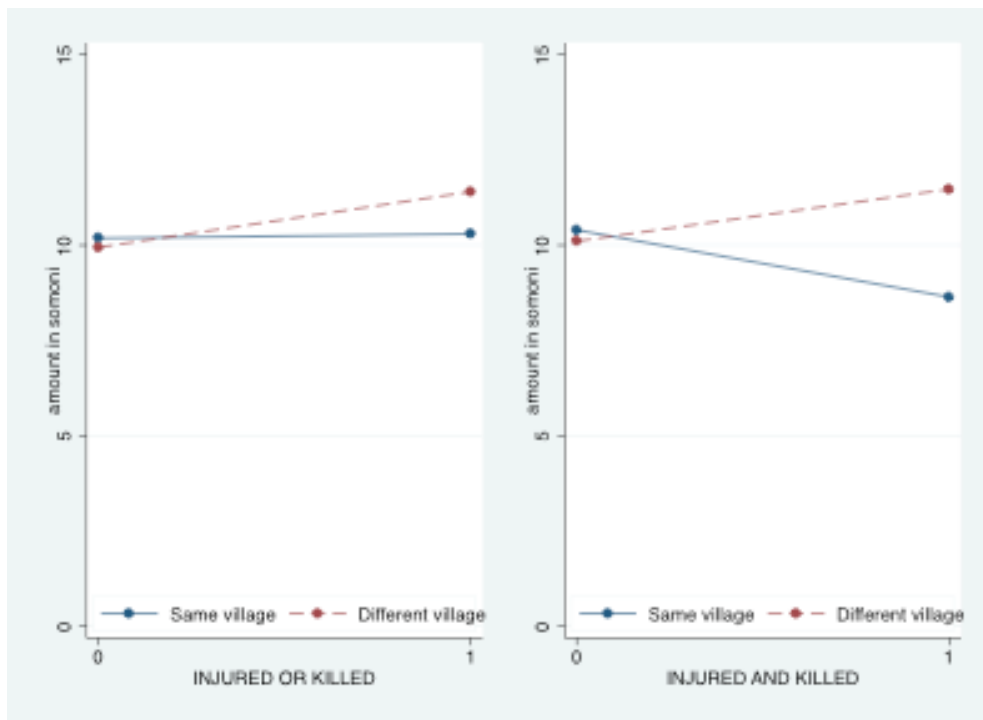
Figure 4: Dictator Game: Amount Sent

Figure 5: Egalitarianism: Offer of an Egalitarian Split in the Dictator Game