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ECORE DISCUSSION PAPER

2010/17

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Does Conflict affect Preferences?
Results from Field Experiments in Burundi

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Abstract: We use experimental data from 35 randomly selected communities in Burundi to examine the impact of exposure to conflict on social-, risk- and time preferences. These types of preferences are important as they determine people's propensity to invest and their ability to overcome social dilemmas, so that changes therein foster or hinder economic growth. We find that conflict affects preferences. Individuals that have been exposed to greater levels of violence display more altruistic behavior towards their neighbors, are more risk seeking, and have higher discount rates. Adverse, but temporary, shocks can thus alter savings and investments decisions, and potentially have long-run consequences.

Keywords: civil war, preferences, field experiments, Africa

Acknowledgments: We would like to thank seminar participants in Oscarsborg, Göttingen and Zurich for helpful comments and suggestions. This research was funded by grants from MICROCON (www.microconflict.eu), Groningen University and the Dutch Organisation for Scientific Research (N.W.O.), grant nr. 452-04-333. Philip Verwimp gratefully acknowledges financial support from the F.W.O. travel grant program. Daan van Soest gratefully acknowledges financial support as part of the N.W.O. Program on Evolution and Behavior.

1. Introduction

Civil wars are associated with the destruction of capital, the breakdown of social norms and a reduced ability of people to "cope" with adverse conditions (e.g. Collier 2003, Fearon and Latin 2003). While the speed of recovery following conflict is subject of debate, the conventional view is that wars are "development in reverse." This message emerges from both cross-country studies (e.g. Chen *et al*. 2008, Cerra and Saxena 2008) as well as from much of the micro level research (e.g., Deininger 2003, Verwimp 2005, Barron *et al*. 2009, Do and Iyer 2009). This comes as no surprise—deaths and injuries, destroyed infrastructure and assets, displaced populations, and deterioration of institutions all seem to work together to push households into poverty. However, recent evidence suggests this is only part of the picture.

A number of African countries have experienced remarkable post-war recovery after civil war. Examples include Mozambique, Angola, Rwanda, and Uganda. Undoubtedly, this is partly due to generous aid flows that typically follow the cessation of violence. But other mechanisms may be at play too. Heterodox social scientists have long argued that violence can carry the seed of societal reform, spurring the expansion of capitalism and promoting economic growth. For example, Cramer (2006) points to historical events to support his claim that violence can "produce institutional changes, amendments to the rule of the game. In retrospect, many changes that come to be seen as progressive have their origins in social conflicts that have taken a violent turn. Herein lies a paradox of violence and war: violence destroys but is also often associated with social creativity." Interestingly, a small literature is now emerging that appears consistent with this perspective. Blattman (2009) shows that political participation in Uganda is greater, and that community networks are more extensive, in areas that have experienced violence. Bellows and Miguel (2006, 2009) find similar results in Sierra Leone.

One possible interpretation consistent with this evidence is that exposure to conflict induces a shift in preferences. This explanation challenges economic theory as economists regard preferences as exogenous and fixed in their straw man model of *Homo economicus* (at least in the short-term—see below). The notion of endogenous, or context-dependent, preferences gnaws at the foundations of standard welfare theory. Yet, some evidence suggests that preferences can change in the short term. For example, Robson (2002) discusses adaptive utility in the context of relative consumption – the appreciation of a consumption bundle depends on consumption levels relative to those of peers. In this study we aim to explore the issue of endogenous preferences, allowing for the possibility that exposure to conflict has shaped preferences.

Interestingly, there is little opposition against the concept of "malleable preferences" in other social sciences. Indeed, in psychology it is widely accepted that (temporary) shocks can permanently affect somebody's outlook on life, as well as his subjective valuation of goods and services (Hobfoll 1989, Tedeschi *et al*. 1998). Shocks may not only have an impact on political motivations and social preferences, but also on other preferences, including those pertaining to evaluating risk and discounting the future. Since such preferences are fundamental determinants of consumption, saving and investment behavior — the drivers of economic growth — it appears as if the notion of endogenous preferences can have far-reaching consequences for how we think about development. For example, a shock that makes people prefer the present rather than the future will discourage saving, and thereby lower long-term income. Shocks that attenuate preferences for social interaction and collaboration may erode social capital and make communities less resilient against future shocks (undermining joint insurance) or hamper the provision of growth-enhancing public goods. If so, dynamic trajectories of development need to be rethought. Particularly, the scope for vicious and virtuous development cycles will be radically altered—if adverse

shocks invite anti-social preferences or discourage savings, then temporary shocks can condemn communities to trajectories into poverty trap type of outcomes.

The main objective of this paper is to examine the causal effect of exposure to violence on behavior in economic experiments in which payoffs vary between choices across three dimensions: timing, riskiness, and social consequences. Do victims of conflict behave more pro-socially, do they have a higher propensity to invest in the future, and are they more prone to taking risks? We try to answer this important question by pulling together survey and new experimental data from Burundi. First, we collected detailed information on the (local) history of violence in a set of Burundian communities, and on a range of household and community variables. We then conducted a series of artefactual field experiments, playing games believed to gauge risk -, time -, and social preferences in an incentive–compatible fashion. While such preferences have been measured in a variety of contexts (see Carpenter and Cardenas 2008 for a review), this study is the first to apply experimental methods in a post-conflict environment to gauge the effect of violence on human decision-making. We are aware of potential concerns about the endogeneity of exposure to violence, omitted variables and non-random attrition, and probe the robustness of our findings using various additional analyses, including an instrumental variable approach.

Our results strongly suggest that exposure to violence affects behavior – possibly via altering preferences. We find that individuals who have either experienced violence themselves, or who live in communities that have been violently attacked, display more altruistic behavior, are more risk seeking, and act less patiently. Our results are robust across several specifications, and are obtained for both experimental and observational data. We believe they shed important new light on post-war recovery processes by speaking against overly pessimistic views on the destructive long-term consequences of civil war.

This paper is organized as follows. In section 2 we sketch two perspectives on the malleability of human preferences. In section 3 we discuss our data and experiments, and outline our identification strategy (including the way in which we address endogeneity issues). In section 4 we present our main experimental results and aim to interpret them in the context of conventional economic assumptions with respect to behavior. Section 5 concludes.

2. Preferences and Behavior

The canonical model of utility maximization in economics assumes that preferences are given. But preferences are not written in stone – a rich economic literature has developed that tries to capture the evolutionary processes behind the long-term selection of time preferences (e.g., Rogers 1994, Robson and Samuelson 2007, Netzer 2009), risk preferences (Netzer 2009) and social preferences (Bowles 2009).[1] Evolutionary models of preference selection provide an equilibrium match between behavior and the environment within which people reside (for overview work, see Robson 2001, 2002). Given the long-term nature of evolutionary processes, though, the notion of endogenous preferences thus defined does not really challenge the mainstream economics paradigm. Specifically, preferences may still be treated as hard-wired and fixed for individuals.

There is also research that documents systematic differences across communities that may be explained by changes taking place at much shorter time scales, such as changes in physical environments or technologies. For example, cross-section experimental work by Henrich *et al*. (2001, 2004) documents behavioral variation across societies, arguing that this variation can be reasonably well explained by the geographical and social context – including economic variables such as the degree to which individuals are integrated into markets. However, correlation between variables like market integration on the one hand and social or

[1] For other work on the evolution of preferences via selection, see Hanson and Stuart (1990) and Eaton and Eswaran (2003). For a critical approach towards the conventional economic model of stable preferences, see Ariely et al. (2003).

risk preferences on the other hand need not capture a causal effect. Self selection into (market) activities suggests that the reverse may be true as well – certain "types" of individuals are more likely to engage in market exchange than others. Such endogeneity concerns imply that it is not straightforward to challenge the canonical model of fixed preferences.

Psychologists sometimes have difficulty grasping the economist's perspective on behavior. Indeed, their standard framework does not contain the equivalent of exogenous preferences. Rather, individuals are believed to have a flexible repertoire of states and traits, enabling them to formulate context-specific coping strategies. The distinction between personality traits (characteristics)[2] and states (context-dependent behavior, see McCrae and Costa, 1997) appears to reflect the economist's distinction between preferences and behavior, but this is only superficially so. While traits are relatively stable over time and across situations, they can profoundly change in response to traumatic events (as do coping strategies and states). The consensus view among psychologists is that a shock, such as civil war, may result in either temporal ("non-chronic") or permanent changes ("chronic" or "personality" changes or disorders) in behavior (McCrae 2006). This is true even if the experience was not first hand (Yehunda 2002). Whether a trauma develops into a personality change, or not, depends on various factors (de Jong 2002).[3] Yet, if people overcome adversity they may be propelled to a higher level of functioning than prior to the event (Linley and Joseph 2004). Such posttraumatic "growth" has been observed under victims of a wide range of tragedies such as rape, cancer, heart attacks, disasters, combat and the Holocaust (e.g., Tedeschi *et al.* 1998, Carmil and Breznitz 1991, Punamaki et al. 1997).

[2] Five personality traits are commonly identified: openness to experience, agreeableness, conscientiousness, extraversion and neuroticism.

[3] Such factors include (i) the magnitude and intensity of the event, (ii) demographic characteristics of the individual (age, gender, socio-economic status, education, preparedness, religion and ethnicity), (iii) the subjective appraisal of the event (whether a person perceives the event as positive or negative and whether a person feels capable of mastering the impact of the event), (iv) the resources available to the person and the social support networks around a person, and (v) the coping behavior of an individual, both problem oriented and emotional effort to deal with the event's impact.

Exposure to traumatic events, including attacks on the community, can augment the value individuals place on people around them. Collins *et al.* (1990) show that the "recognition of one's vulnerability" may enhance the value of social networks. Also, psychologists have pointed out that the disruption of support networks is a key damaging aspect of traumatic events (de Jong 2002). If violence disrupts communities, through permanent displacement or ethnic tensions, the breakdown of social support networks may occur.

What about risk preferences? The psychological literature also offers some clues on how risk preferences might be affected by shocks. The core idea is that human emotions and behavior are affected by traumatic shocks (Cutchin *et al*. 2008) even when the experience is not first hand (Weinstein 1989). These emotions in turn affect people's risk evaluations in situations subsequent and unrelated to the traumatic event (Lerner and Keltner 2001), yielding ambiguous predictions with respect to experimental play. Specifically, when trauma has induced feelings of anger, respondents are more likely to make optimistic risk evaluations and are more prone to choosing risky options (Lerner and Keltner 2001). In contrast, when the trauma has induced feelings of fear, respondents are more likely to avoid risky options. It is an open question which effect dominates.

Time preferences may also be affected by exposure to conflict. Traumatic events may raise the subjective appreciation of the risk of being exposed to trauma again in the future (Lerner and Keltner 2001), and experiencing a shock often induces self-protective behavior in survivors (Weinstein 1989). Hence, exposure to conflict may induce hazard preparedness in people, skewing asset portfolios towards assets that can easily be hidden or moved. If so, exposure to violence should make our respondents seek immediate returns over future ones, raising discount rates as measured in experimental play.

However, the difficulty in identifying the causal effect of conflict on preferences is evident. As mentioned, one key concern is potential endogeneity of exposure to conflict with respect to preferences due to (self) selection effects. Moreover, it is difficult to distinguish true preference shifts from information effects—which would be perfectly compatible with the mainstream model of fixed preferences. If people care more about their fellow survivors than before the conflict, preferences have shifted. However, if people learn during conflict that certain modes of behavior are more functional than others, this may induce them to adopt new heuristics—possibly more cooperative ones. Conflict may teach community members that altruistic behavior pays (or does not pay) because fellow villagers (fail to) reciprocate in times of need. Alternatively, conflict may help people discover what their true preferences are, as it forces them to reflect on the essence of living. That is, preferences may be a stable but fuzzy and multidimensional construct, so that identifying them takes effort (Bateman et al. 2008). We return to this in section 4.2.

3. Experimental Design

3.1 Data

We conducted our series of experiments in a random and stratified sample of 300 households in 35 communities. These communities and households were drawn from a set of 100 communities that we visited in 2007 to collect data on local conflict, social capital and a range of household and community variables.[4] The data gathering effort was part of a larger project, initiated by the World Bank, who collected the first wave of data halfway through the war (in 1998). For a total of 1400 households in 100 communities we have panel data regarding many important household characteristics as well as information on the

[4] Burundi Priority Survey 2007. The data collection was a collaborative effort between the Institut de Statistiques et d'Etudes Economiques du Burundi (ISTEEBU), Antwerp University, Brussels University and Wageningen University, and was implemented under the flag of MICROCON – an EU funded project focusing on household analysis of violent conflict in various regions of the world.

development and consequences of armed conflict. We randomly selected 35 communities and revisited all respondents of the earlier surveys, inviting them to participate in a series of experiments. Of the 35 communities, 24 experienced violence in the period 1993-2003[5], and 11 were not exposed to violence. Table 1 summarizes our main data.

Our starting point is the assumption that there are no systematic differences between our subjects before the onset of conflict. That is, we assume our 300 participants are drawn from the same distribution of preferences in the Burundi population. Next, we test whether exposure to violence is random – the odds of an individual being subjected to violence are independent of variables like wealth, education, etc. Following anecdotal evidence (HRW 1998, Krueger and Krueger 2008, Voors and Bulte 2008), our null hypothesis is that violence is not systematically related to individual characteristics. Our data do not allow us to reject this null—see below. Hence, we can think about exposure to violence during the 1993-2003 conflict as a quasi-experiment, enabling identification of the effects of the "conflict treatment" — systematic differences in experimental play between communities can be attributed to exposure to conflict.

We measure civil war shocks at both the household and community level. Since independence Burundi has been the stage of nearly three decades of civil war between the country's two main ethnic groups; Hutu and Tutsi. At the outbreak of the most recent episode of violence in 1993, following the assassination of the country's first Hutu president, Melchior Ndadaye, Hutu groups targeted Tutsi in retaliation throughout the country, killing 30,000-50,000 Tutsi within weeks. In turn, the Tutsi-dominated army responded with indiscriminate and large-scale attacks on Hutu. In the years that followed, confrontations between rebel groups and the army ravaged communities throughout the country. Nevertheless, most of the violence concentrated around the nation's capital, Bujumbura, as

[5] The end of the conflict was officially sealed with a peace agreement signed in 2005. Yet, the intensity of conflict in the last 2 years was negligible as compared to the intensity in the first 11 years. When constructing our variables on conflict and victimization, we focus on the incidents in the 1993-2003 period.

both rebels and the army fought over its control. Also, violence was less present in remote places such as communities at greater altitudes (Figure 1). Burundi has only recently started to recover from this violence, which left over 300,000 Burundians dead and displaced 1.2 million people (Ngaruko and Nkurinziza 2000). We recorded the number of confrontations between army and rebels as well as bouts of one-sided violence hitting communities without regard for the characteristics of their victims over the 1993-2003 period. In our sample, about two thirds of the communities experienced such attacks resulting in the death of up to 15% of the communities' inhabitants.

3.2 Experimental Games

For our series of experiments we use adapted versions of well-established experimental game protocols. We implement a social orientation experiment, a risk experiment and a time preference experiment, to be implemented in this order. In the risk experiments subjects face the possibility to make losses – as we elicit decisions in both the win and loss domain. To avoid subjects making losses in the session (i.e., over all three games), we pay them a show-up fee of 2000 FBU.[6]

To measure social preferences we adapted the social value orientation experiment devised by Liebrand (1984). In this experiment, subjects (denoted by *i*,*j*) are anonymously matched to another participant from their community (their 'partners'), and make six choices between two own-other payoff combinations; A and B. The pairs of allocations lie on a circle in the positive quadrant, where the amount of money the decision maker allocates to himself (S_i) is measured along the horizontal axis and the amount of money allocated to the other participant is measured along the vertical axis (O_i).[7] The radius of the circle is 250 FBU, so

[6] USD 1 = 1,210 FBU (20 May 2009), which is roughly equal to a full day's wage rate for unskilled labor.

[7] Originating in the social psychology literature this experiment is now frequently applied in the economics literature as well (see Offerman et al. 1996, Vyrastekova and van Soest 2007). In these versions subjects are offered 24 pair wise allocations, covering all four quadrants of the circle. Pretesting revealed that the cognitive

that $S^2+O^2 = (250)^2$. As a result, the total amount of money to be allocated (S_i+O_i) is not constant across combinations.[8] Subject *i*'s earnings are equal to the amount of money allocated to himself (the sum of S_i's over the six choices), plus the amount of money allocated to him by his partner *j* in the experiment (the sum of O_j over the six choices). If we take the ratio of the total amount of money a subject allocated to the partner (the sum of all O_i's) and the total amount allocated to himself (the sum of S_i's), we obtain a measure of this subject's social orientation. This ranges from totally selfish (if the subject always chose the allocation with the highest payment for himself) to totally altruistic (if he always chose the option with the highest payment for his partner). We rescale the results such that social orientation is measured on a scale from 0 to 100, with 0 denoting purely selfish preferences, 100 identifying the subject to be maximizing his partner's payoff, and 50 identifying the social optimum (i.e., choosing allocations to maximize joint payoffs). On average we find a value of 27, indicating that most subjects tend to be fairly individualistic (Table 1). The subjects were not informed about their earnings in this experiment (the sum of S_i+O_j over all 6 choices) until after the other two experiments had been completed.

The second experiment of the session was the risk preferences experiment. Here we used a game (based on Harbaugh et al. 2002) [9] where subjects could choose between playing a simple gamble and receiving a specific amount of money with certainty. Participants were presented with 6 choice cards, each of which presented them the choice between A: receiving (or losing) an amount of money with certainty (*y*, that varied between the 6 choice cards), and B: participating in a game where they may either gain (lose) 2000 FBU with probability 0.3,

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burden of making all 24 choices was too large for our (largely illiterate) subject pool. We decided to just offer subjects pair-wise choices in the "first quadrant" of the social orientation circle—corresponding to positive amounts for both the giver and receiver. We thus reduced the cognitive burden imposed on our illiterate subjects at the cost of reduced precision with which pro-social preferences can be measured.

[8] See Appendix for example of record sheet and full choice pair table.

[9] We used an adapted version of a simple design for children by Harbaugh et al. (2002) which is well suited for illiterate people as it uses a simple trade-off and clear visual instruction. Our use of the experiment differs slightly from Harbaugh et al. as we specifically use information from questions where the certainty equivalent is different from the expected value of the gamble.

or gain (lose) nothing with probability 0.7. Hence, the expected absolute value of the gamble was always the same (600 FBU, which was an expected gain for three cards and an expected loss for the other three cards), whereas we varied the amount of money to be received with certainty (y). For both gains and losses the certain bid (y) was lower, equal to, and higher than the expected value of the gamble. As the certain payoff (y) in A increases, the gamble in B becomes less attractive. The point at which a subject switches from the risky to the safe alternative allows us to determine her degree of risk aversion.[10]

The probabilities of the gamble (0.3 of winning/losing 2000 FBU, and 0.7 of receiving nothing) were represented visually using three black and seven white balls. To illustrate the chances of winning/losing money, the ten balls were put into a bag in the presence of all participants in the session, and subsequently stirred. Next, we drew one ball from the bag about ten times – with replacement – to show the participants that the likelihood of drawing a black ball (implying winning/losing money) was less than half the likelihood of drawing a white ball (resulting in zero payoffs). The choice cards displayed the options both numerically and graphically with each change in money stock represented by an equivalent number of banknotes. Payoffs for this second experiment were not determined until after the third experiment had been completed. Then, payoffs were determined by first selecting which of the six cards was to be implemented. Six numbered balls were put into a bag to randomly select one card to be played for payment. Those subjects who had chosen the safe option A were informed about the amount of money y, as stated on that card, they were to receive (or had to pay). For those who had chosen the gamble, option B, the seven white balls and the three black balls were put into the bag to determine whether they would receive (have to pay) 2000 FBU (when any of the three black balls were drawn), or whether they received nothing

[10] See Appendix for record sheet and full choice pair table.

(if one of the seven white balls was drawn). Note that we made sure that at the end of the experiment all subjects had non-negative earnings because of the 2000 FBU show-up fee.

The third and final experiment aimed to gauge time preferences. We presented subjects with a set of nine simple pair wise choices between two options: receiving an amount of money at some date in the near future, and receiving a larger sum at a later time. The amounts of money were to be delivered by a trustworthy local NGO, Ligue ITEKA. However future money is always less certain than instantaneous money.[11] Consequently, we provided subjects with a choice between two future options – receiving money tomorrow, or in 15 days – rather than one "instant" versus one future income option (see Harrison et al. 2002). The two options to choose between were A: receive 1000 BFU in one day, and B: receive 1000(1 + *d*) FBU in two weeks plus one day, with *d* equal to 0.00, 0.01, 0.02, 0.05, 0.10, 0.40, 0.70, and 1.00.[12] Subsequently, at the highest interest rate subjects earned an additional 1000 FBU by waiting two weeks. In the experiment subjects were asked to identify their switching point from preferring A to preferring B. Increasing the interest rate *d* over the nine decisions allows us to observe the point at which a subject switches from preferring 1000 FBU tomorrow to preferring 1000(1 + *d*) FBU in two weeks plus one day. The switching point serves as a measure of the subject's discount rate; the earlier people switch from A to B the more patient they are.

After subjects completed all questions (and after having determined the payoffs for the social orientation game and the risk preference game), we randomly determined which pair-wise choice was to be paid in the time preference experiment. To do so, we put 9 numbered balls into a bag, and picked one randomly. The option chosen for that question (i.e. A or B) then determined how much money was delivered, and when. Then the participants were informed about their revenues in the social orientation game, the payments for the risk

[11] Ligue ITEKA is a Burundian non-governmental organization with a solid reputation of being trustworthy amongst Burundians. It has a long history of operation as well as nationwide coverage in Burundi.
[12] See Appendix example of record sheets and full choice pair table.

preference game were determined using the procedure explained above, and they received the associated earnings, plus their show-up fee. The pay-off of the time preference game was placed in a sealed envelope and handed over to a representative of the regional office of the local non-governmental organization, while all participants received a receipt stating the amount of money they were entitled to. At the relevant date (either the next day, or 15 days later), the representative went back to the community to deliver envelopes to the respective participants, in return for the their receipts. To ascertain that the money envelopes were indeed delivered by our local organization, we checked whether all receipts were collected – which was indeed the case. The participants were informed about this procedure in advance.

3.3 Implementation

The experiments were conducted during March-April 2009. Following an extensive training of our local experimenters[13], we ran several pilot tests to ensure that our typical participant was able to understand our experiments without much effort. As many of our participants had received little or no education, we followed a relatively simple design and our experimenters used clear and visual instructions to make it easier for illiterate subjects to understand the consequences of any decisions they made in the games. We tested comprehension by asking test questions before the start of each experiment.[14] A day before the experiments, research coordinators contacted local government officials in each research site, and asked them to invite the household heads of the 2007 survey participants. The experiments started at approximately 9 A.M. the next day, and lasted about three hours. Each session started with a general introduction in which the participants were informed, among other things, that upon completion of the session they would receive a show-up fee of 2000 FBU plus or minus the amount of money they would gain or lose as a result of their decisions during the session.

[13] The experiments were conducted in the local language Kirundi.
[14] To enhance understanding we also limited the group size to 10 participants and if needed experiments were conduced in two groups. Also, instructors we went though the experiments question by question.

The games were implemented by three teams, each with one instructor and two research assistants. Subjects who had difficulty completing record sheets by themselves were helped by research assistants who carefully avoided giving specific instructions about how to answer. The average experimental earnings for three games were about 6000 FBU (roughly 5 days wages for unskilled labor), including the 2000 FBU show-up fee.

3.4 Exogeneity and selection bias

The assumption underlying our empirical approach is that violence across and within communities was exogenous with respect to individual preferences. However, any co-variation of preferences and exposure to violence may be due to (i) non-random violence or (ii) non-random attrition in the sample.

Regarding the first, there is ample evidence highlighting the brutal and indiscriminate nature of the Burundi conflict (HRW 1998, Krueger and Krueger 2008; for a more detailed examination of the nature of violence in Burundi, refer to Voors and Bulte 2008). Some communities were severely attacked, for example when the army raided whole communities in search of weapons, rebels and loyalists (HRW 1998, Krueger and Krueger 2007). Similarly, rebels indiscriminately attacked individuals and communities in search of supplies. We collected data on exposure to violence both at the household and community level (Table 1). For example, the community survey elicited information on the intensity of violence resulting from confrontations between the army and rebel groups as well as one-sided violence by either group. We recorded the date and severity of the attacks, including the number of civilians killed or injured over the period 1993-2003.

To statistically examine whether 'selection into violence' might bias our results we assess whether violence experienced by communities is associated with pre-war community characteristics; see Table 2. Violence is measured as the number of attacks during 1993-2003

(column 1), and as the number of people dying in attacks in that period, expressed as a share in the total population (column 2). In these two columns we find no significant correlation between our measure of violence and a broad range of pre-war community characteristics including, among others, average wealth[15], the fraction of votes for the assassinated president, and ethnic homogeneity. Only two (exogenous) variables are correlated with the share of villagers killed – distance to Bujumbura, the nation's capital, and altitude, a measure of geography (see also Figure 1). We employ these variables as instruments for conflict intensity to attenuate any remaining endogeneity concerns and measurement error. Below we will demonstrate that the instruments are not correlated with the error term of the second stage regressions.

Second, we analyze whether households are selected into violence, or not. In the third and fourth column of Table 2 we repeat the above analysis, but now at the household level. We have two proxies for household exposure to violence: (i) whether a physical attack happened to a household member and (ii) whether household members have been exposed to non-physical attacks (including theft, forced labor, etc.). In columns (3) and (4) we demonstrate that neither variable is correlated with a range of household characteristics (income, gender, education, etc.). The same applies to survey-based data that are related to the preferences we are interested in: (i) perceived trust levels (a measure of social capital as a proxy for social preferences), (ii) crop choice (proxy for risk preferences), and (iii) expenditures on farm improvements (proxy for time preferences--see section 4.3).[16] Taken together, the regression results in columns (1)-(4) of Table 2 support the anecdotal evidence that violence in the Burundi war committed by both soldiers and rebels was random.

Next, to address sample attrition we analyze whether a non-random subset of the population was murdered or moved out in response to the threat of violence (and did not

[15] Unfortunately our data lack pre-war income levels, instead we proxy wealth by average livestock ownership.
[16] These household data are not available for the pre-war period. Instead we rely on early war data from our first survey wave, conducted in 1998.

return) between 1998 and 2009.[17] Some 14% of the households surveyed in 1998 were 'missing' in 2009, which represents a modest attrition rate given the circumstances. In column (5) of Table 2 we analyze whether these drop-out households differ statistically from the re-interviewed household in 2009. We follow the approach taken by Fitzgerald et al. (1998) and estimate a probit model of 1998-2009 attrition on a range of 1998 household characteristics. All but one variable enter non-significantly. The one exception is gender of the household head. According to our data, households headed by a male were less likely to be present in the 2009 survey than households headed by a female (the share of male-headed households dropped from 80% in 1998 to 60% in 2009). We return to this issue in section 4.

4. Conflict, Behavior and Preferences

The descriptive statistics in Table 1 suggest considerable heterogeneity in experimental behavior. In this section we investigate whether experimental behavior varies with exposure to conflict, and regress decisions made in the social, risk and time preference experiments on our measures of violence. We include several household and community characteristics as controls, and also include regional fixed effects.[18] We focus primarily on the relationship between *community* exposure to violence and *individual* preferences. The reason is that even if only a sub-set of individuals directly experienced acts of violence, the consequences may be felt throughout the community (Yehuba 2002). Our main measure of violence comprises the total number of dead during 1993-2003 relative to population size in the community.

[17] Unfortunately, we do not have data to explore this same issue for the entire duration of the war; from 1993 until 2003. Households that migrate in anticipation of violence may have different risk and social preferences than those households which stayed behind. We use data from an alternative study to probe into this. The Enquête Socio-Démographique, Santé et Reproduction (ESD-SR) was collected by a UN institution, and we use it to analyze whether migration during the early war period (1993-1998) is explained by community or household characteristics. In this sample, 20% of households were not present in their village in 1998, and according to this sample both gender, literacy and age mattered for migration decisions. However, for these villagers we do not know whether their village of origin indeed experienced conflict, and it is therefore not evident that migration of households affects the composition of conflict-ridden villages relative to those who escaped conflict.

[18] See appendix for variable definitions.

However, in some models we also include an index of individual level exposure to violence. Throughout we cluster standard errors at the community level to account for intra-community correlation.

4.1 Conflict and Behavior

We explore the relationship between conflict and behavior in the experimental games in Tables 3, 4 and 5.[19] In Table 3 we report the results for our measure for pro-social behavior. Across all OLS specifications we record a statistically significant and positive correlation between conflict intensity and altruistic behavior at the community level (column 1) as well as at the household level (columns 2-5). This is in line with earlier empirical survey work by Bellows and Miguel (2006, 2009) and Blattman (2009) who report an increase in social cohesion and political participation in response to violence. As discussed above, our findings also resonate with psychological studies into the effect of traumatic experiences on people's attitudes.

Our results are robust to the inclusion of ethnicity fixed effects, as well as a series of other household and community controls and regional fixed effects (Table 3, columns 3 and 4 respectively).[20] For example, social preferences are higher when respondents are literate, male, and own more land. We find weak evidence that social preferences are declining in age. Turning to the community level controls, social preferences are positively associated with ethnic homogeneity, and negatively associated with distance to the market and ongoing conflicts within the community over land. The effect of market integration is consistent with Henrich et al. (2004), and the land conflict and ethnicity outcomes makes intuitive sense. We

[19] All regressions in Table (3)-(5) use OLS. As our dependent variables take only a limited number of values, we also estimate the models in columns (1)-(5) using an ordered probit specification. The results are qualitatively similar and available on request.

[20] In addition the coefficient on violence increases if controls are included. Following Bellows and Miguell (2009) this suggest that it is unlikely that omitted variable bias explains away the conflict effect (see also Altonji et al. 2005).

find that population density, average community income and land distribution are not correlated with social preferences.

Next, we employ a household level conflict variable, rather than a community-based measure. For this purpose we have constructed a household victimization index[21] (Table 3, column 5). We again find a positive correlation with social preferences, indicating that both individual and community level violence are associated with altruistic behavior. This is consistent with the existing psychological evidence.

OLS regression results for risk preferences are presented in Table 4. Throughout we document a positive correlation between community level conflict intensity and risk seeking. This result is robust to including common controls and fixed effects (columns 2–5). In column (7) the dependent variable measures preferences over losses. One key insight from Kahneman and Tversky's (1979) seminal paper is that people value changes in gains and losses differently. In line with their work we find that people respond differently to changes in gains and losses: while conflict induces risk seeking over gains it does not affect attitudes towards losses. Though work by economists on shocks and risk preferences has so far been limited (Dercon 2008), this result suggests it may be a viable area for future research. The pattern of responses provided in Table 4 is consistent with the psychologist's model in case exposure to violence has invited sentiments of anger. It is striking to observe that risk preferences are not (robustly) associated with any of the household or community-level controls.

Finally, in Table 5 we summarize the impact of conflict on inter-temporal choices. The models suggest that exposure to conflict causes an increase in discount rates. While the evidence seems more mixed, violence appears to make people less patient. Again, this is not inconsistent with the psychologist's model of interpreting traumatic events. It is interesting

[21] Our victimization index is an additive scale of exposure to a range of violence measures (rape, theft, forced labor, torture, ambushes). Since the absolute scale of this variable is arbitrary it is normalized to have a mean of zero and standard variation of one.

to note that time preferences are not associated with any of the household-level variables (including individual exposure to violence, when controlling for community attacks). In contrast, several of the community variables enter significantly, communities with higher levels of ethnic homogeneity display lower discount rates. Yet, contrary to intuition, we find that discount rates are lower in communities with an unequal distribution of income and those with ongoing conflicts over land.

To attenuate potential endogeneity and omitted variables concerns in Tables 3-5 we re-run our analysis using a 2SLS specification, where we use distance to Bujumbura and altitude as instruments for our conflict measures. Results are reported in columns (6) in all three Tables. Predicted violence is significant at the 1% level, and the value of the coefficient is larger than before.[22] Our identifying assumption is that these geography variables only affect the allocation of violence and have no direct impact on preferences. If, however, distance to the capital is analogous to distance to markets then preferences are likely dependent on distances to Bujumbura (see Henrich et al. 2001). Yet, direct effects of proximity to the capital are likely minimal as most farmers operate at subsistence level and sell goods at local markets, which often act as an intermediary for goods making their way to the capital as well. These markets are close by in all communities in our sample (never over a 2 hour walk away), reducing concerns about a correlation between geography and preferences. Also, export crops production such as coffee is usually sold to local intermediates or washing stations. Statistically, this is conformed by the test statistics indicating that our excluded instruments are correlated with the endogenous variable (see the high partial F values), and also that these instruments are correctly excluded from our second stage regression (the p-value of the Hansen J statistic is well above 0.10).

[22] The 2SLS point estimate is somewhat larger than the OLS estimate. This is a common finding in cross-country studies, usually attributed to measurement error (biasing the OLS estimates towards zero).

Finally, to assess the magnitude of these effects, we report the coefficients of all significant variables in each of the Tables 3-5 after having standardized the explanatory variables such that they have a mean of zero and a standard deviation of one. These so-called beta coefficients are reported in column (7) in Tables 3 and 5, and in column (8) of Table 4. Clearly the impact of conflict dominates all other impacts. For example, the beta coefficient of violence in the social preference regression is 0.46, about three times as large as the beta coefficients of the other variables in the same column.

4.2. True preference shifts versus changes in beliefs?

Our results shed light on the consequences of conflict for growth. Conflict induces people to make more pro-social choices and take more risky decisions in our experiments, but are also found to be more impatient. What matters for development is behavior, and not the underlying preferences. Nevertheless, it is interesting to consider whether the behavioral change that we document is due to a (true) preference shift. Most of the literature ignores this issue, and simply interprets experimental play as reflecting underlying preferences (e.g., Henrich et al 2001, 2004, Carpenter and Cardenas 2008, Tanaka et al 2009, Croson and Gneezy 2009). The results in Tables 3-5 are consistent with the notion that exposure to violence has altered preferences. However, the observed change in behavior may also be due to the fact that victims and non-victims having been exposed to different opportunities for learning about their own latent preferences, or about heuristics regarding behavior in specific situations. This is akin to questions about the effect of propaganda and advertisement – do they create new preferences, or do they appeal to latent preferences, or invite updating of preferences via the provision of new information? While we cannot analyze this issue in detail, we explore it further by delving a little deeper into differences in means and variances between the victims and non-victims.

In the top panel of Table 6 we compare sample means for communities that have and have not been exposed to violence. Consistent with Tables 3-5 we find systematic differences. We believe the systematic nature of the differences speaks against the interpretation that respondents have learned about their true preferences. There is no reason to assume that *a-priori* (i.e. without exposure to conflict) respondents' uninformed guesses about their true preferences would be systematically biased towards one direction or another. Hence, albeit tentatively, the systematic nature of the response supports the idea of a preference shift rather than the alternative explanation of preference discovering.

Next, we tentatively explore whether the pattern of responses is consistent with the other alternative hypothesis – the idea that exposure to conflict induced learning about optimal behavior (adjusting heuristics), so that people "converge" towards a certain behavioral pattern. In the second panel of Table 6 we run *t*-tests on the differences in variance between the two groups. We find no significant difference in the variance of our preference parameters for social and time preference, which is at odds with the idea of learning about 'best behavior'. However, for risk preference we cannot reject the hypothesis of unequal variances.

4.3 Robustness

In this section we report the outcomes of a series of robustness analyses. First, we explore whether the differences in experimental play translate into systematic differences in behavior in real life. We estimate several alternative models and summarize our findings in Tables 7 and 8. In Table 7 we replace our experimental variables with survey-based social-, risk- and time preference proxies. In column (1) the dependent variable is a social capital index in the spirit of Narayan and Pritchett (1999). This index comprises a weighted (and normalized) scale of respondents' participation in community organizations and the degree of

membership. Consistent with our experimental variables we find a positive correlation with exposure to violence. In column (2) we proxy for risk attitudes by using a common measure of crop choice. If conflict alters risk preferences we would expect an effect on investment and asset portfolio choice—skewing resources to more risky and profitable activities. We use our 1998-2007 panel to estimate the effect of conflict on the share of cash crops to total production, and find that households in regions exposed to greater levels of violence cultivate relatively more cash crops. (Recall that earlier we demonstrated that cash crops did not invite conflict – see Table 2 columns 3-5). Again this result is consistent with the experimental evidence. Lastly, in column (3) we use a measure of long-term investments – the share of expenditures on farm building improvements – as our dependent variable. The assumption is that a greater share of durable investments reflects greater patience. Again we find our experimental results reflected in the survey data: households affected by greater levels of conflict invest less in farm buildings.

To further assess the robustness of our findings we return to the possible bias introduced by selection in to our sample. Table 2, column (5) showed that men may be underrepresented in our sample (some 40% of households were female headed in 2009). Hence, correlation between gender and preferences may bias our estimates in Tables 3-5. In columns (4)-(6) of Table 7 we re-estimate our 2SLS models on a restricted sample of male respondents who were present in both 1998 and 2009, and find that for social and risk preferences the results go through as before, with only minimal differences in the coefficients.

In Table 8 we assess whether other types of shocks such as natural disasters (drought and excess rainfall) and plant diseases affect preferences as well. Again we estimate the models explaining experimental play. Interestingly, we find no consistent set of significant effects of such shocks on preferences. Natural disasters and diseases do not produce the same traumatic responses as exposure to conflict—attenuating the risk that we are picking up

omitted variable effects. The one exception is a correlation between a major plant diseases and social preferences.

5. Conclusions

The literature on the consequences of civil wars has often emphasized its detrimental effects on households' ability to cope. According to this view, such civil wars may invite poverty traps. However, this pessimistic view on development has come under new scrutiny from a few recent careful micro level studies suggesting that exposure to conflict is not necessarily detrimental for development and may contribute to social capital (see Bellows and Miguell 2006, 2009, Blattman 2009). Yet, social preferences are but one of a set of preferences likely affected by conflict and of interest to development economists and practitioners. We extend upon earlier work by (i) including risk and time preferences in our analysis, and (ii) gauging such preferences with a series of incentive-compatible field experiments (rather than via a survey approach).

In this paper we set out to investigate the impact of conflict on social, risks and time preferences and use data from a series of economic experiments using 300 respondents in 35 randomly selected communities in Burundi. We find that conflict is robustly correlated with preferences. Econometric analysis reveals that individuals in communities which were exposed to greater levels of violence display more altruistic behavior to their neighbors, are more risk seeking and have higher discount rates.

The evidence documented in the various tables is consistent with the idea that preferences of people are endogenous and respond to experiences or the context. While we cannot rule out that behavioral differences in experimental play are due to learning effects invited by exposure to conflict – learning about own preferences or those of others, and about consequences of behaviors over a range of contexts – we believe that the behavioral patterns

(including higher order terms such as the variance) do not generally support such an interpretation. Of course our natural experiment approach implies imperfect control, and cleanly separating preference shifts from learning effects is not possible. The benefit of our approach is that we are able to analyze the response to an event of first-order salience. Analysts routinely trade-off control versus the relevance of the context when conducting experiments (e.g. List 2007).

A key finding of this paper is that temporary shocks have long-term consequences: civil war violence that occurred between 1993 and 2003 has a clear impact on individual behavior in 2009. These consequences may even prove to be permanent if they invite preference shifts. Our evidence for Burundi suggests that the net effect on development is unclear. While exposure to violence causes pro-social behavior and encourages risk taking, arguably positive features for development at least within certain bounds, it also seems to trigger impatience. As impatience discourages savings it could drag down investment levels also in the presence of imperfect capital markets (as obviously prevailing in Burundi). If so, the net effect on the ability of communities to rebound after conflict is ambiguous. Nevertheless, the results may partially explain the pattern of recovery observed in many post-conflict settings, and thereby provide new evidence against pessimistic views on the destructive legacies of civil war.

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Table 1 Descriptives

Variable	obs household level	obs community level	mean	std. dev.	min	max
Panel A: Preferences						
Social preferences [0-100]	298	35	27.34	28.10	0	100
Risk preferences Gains	271	35	4.11	2.08	1	6
Risk preferences Losses	259	35	4.83	1.91	1	6
Discount rate (%)	285	35	39.67	41.26	0	100
Panel B: Conflict variables						
Relative number of dead in attacks (%)		35	2.35	4.48	0	15.63
Individual victimization index	299	35	0	1	-0.50	3.86
Panel C: Household variables						
Respondent is literate	296	35	0.52	0.50	0	1.00
Respondent age	298	35	44.91	15.76	14.00	90.00
Respondent is male	298	35	0.61	0.49	0	1.00
Total land holdings per capita (ha^2)	292	35	5.43	5.08	0.10	34.25
Respondent is livestock farmer (1993)	300	35	0.40	0.49	0	1
Perceived trust level (1998)	300	35	4.69	2.16	1	10
Social capital index (2007)	299	35	0	1	-0.47	4.49
Respondent is cash crop farmer (1998)	300	35	0.60	0.49	0	1
Respondent is cash crop farmer (2007)	300	35	0.35	0.48	0	1
Investments farm buildings (FBU, 1998)	295	35	398.20	375.16	0	5000
Investments farm buildings (FBU, 2007)	300	35	221.86	1001.42	0	12155.87
Panel D: Community variables						
Land Gini coefficient		35	0.30	0.20	0	0.54
Distance to market		35	2.85	0.72	1	6
Conflict over land (% yes)		35	0.25	0.15	0	0.60
Ethnic homogeneity		35	0.88	0.15	0.30	1.00
Population density 1990 (log)		35	5.47	0.48	4.20	6.11
Population density 2008 (log)		35	5.79	0.44	4.50	6.49
Per capita total expenditure 1998 (log)		35	8.57	0.70	5.17	10.70
Per capita total expenditure 2007 (log)		35	9.24	0.47	8.09	10.40
Severe draught (% yes)		35	0.63	0.48	0	1
Access rain (% yes)		35	0.63	0.48	0	1
Manioc disease (% yes)		35	0.90	0.29	0	1
Upcoming ceremony (% yes)		35	0.40	0.49	0	1
Distance to Bujumbura (km)		35	93.17	34.53	39.5	167
Altitude (m)		35	1662.06	158.39	1260	2200

.

Table 2 Exogeneity

Dependent variable:	(1) Attack during 1993-2003 Probit	(2) Rel. number of dead in attacks 1993-2003 OLS	(3) Physical attack on family member Probit	(4) Non-physical attack on family member Probit	(5) Interviewed in 1998 and 2009 Probit
Community variables					
Average literacy household head	-0.593	-5.943			
	(1.148)	(3.479)			
Average age household head	-0.002	0.0823			
	(0.043)	(0.144)			
Percent male	-0.368	1.268			
	(1.723)	(6.927)			
Livestock farmer (1993)	2.350	5.636			
	(1.908)	(5.608)			
Density in 1990 (log)	0.185	0.761			
	(0.573)	(1.866)			
Ethnic homogeneity (1993)	0.015	0.026			
	(0.020)	(0.060)			
Fraction of votes for Ndadaye (1993)	-0.011	0.025			
	(0.020)	(0.061)			
Distance to Bujumbura (km, log)		-5.749*			
		(3.160)			
Altitude (m, log)		-17.360*			
		(8.889)			
Relative number of dead in attacks 1993-2003					0.0255
					(0.0189)
Household variables					
Literacy			-0.041	-0.0283	0.312
			(0.192)	(0.156)	(0.270)
Age			0.006	-0.0008	-0.00239
			(0.007)	(0.0060)	(0.00660)
Gender			0.125	-0.176	-0.794***
			(0.263)	(0.222)	(0.239)
Livestock farmer (1993)			0.225	0.0748	0.0660
			(0.216)	(0.181)	(0.252)
Ethnicity			0.0684	0.0453	
			(0.167)	(0.142)	
Total expenditures (1998)			0.253	0.160	-0.193
			(0.223)	(0.186)	(0.164)
Perceived trust level (1998)			-0.0052	-0.0109	
			(0.0431)	(0.0378)	
Share of cash crops in total production (1998)			-0.366	0.170	-0.0973
			(0.417)	(0.307)	(0.412)
Expenditures farm improvement (1998)			0.0843	0.0356	0.0296
			(0.0902)	(0.0803)	(0.0605)
Constant	0.302	145.7*	0.457	1.686	2.295
	(4.755)	(75.45)	(0.390)	(1.452)	(1.448)
FE	no	no	yes	yes	yes
N	35	35	239	239	279
adj. R^2		0.13			

Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Dependent variable in column (3) is a dummy, 1 if respondent was interviewed in both 1998 and 2007, zero else; in column (4) is a dummy, 1 if respondent was interviewed in both 2007 and 2009, zero else; in column (5) is a dummy, 1 if respondent was in village during 1998, zero else.

Table 3 Conflict and social preferences

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) 2SLS	(7) 2SLS
Relative number of dead in attacks (%)	0.143*	0.136*	0.241***	0.255***	0.259***	0.422***	0.464***
	(0.0792)	(0.0687)	(0.0676)	(0.0738)	(0.072)	(0.115)	
Individual victimization Index					0.499***		
					(0.254)		
Household variables							
Respondent is literate			1.338**	1.330**	1.430**	1.436***	0.174***
			(0.533)	(0.527)	(0.523)	(0.521)	
Respondent age			-0.0301*	-0.0291*	-0.031*	-0.0315*	-0.119*
			(0.0157)	(0.016)	(0.016)	(0.0161)	
Respondent is male			0.882*	0.914*	0.866*	1.047**	0.123**
			(0.503)	(0.500)	(0.481)	(0.450)	
Total land holdings per Capita			0.110***	0.095**	0.099**	0.108**	0.130**
			(0.0389)	(0.0381)	(0.038)	(0.0497)	
Community variables							
Land Gini coefficient			-1.379	-1.170	-0.542	-2.097	-0.101
			(1.415)	(1.291)	(1.264)	(1.366)	
Distance to market			-0.858**	-0.853**	-0.768*	-1.06***	-0.181***
			(0.387)	(0.398)	(0.405)	(0.370)	
Conflict over land			-3.556**	-3.127**	-3.152**	-4.45***	-0.167***
			(1.667)	(1.444)	(1.385)	(1.668)	
Ethnic homogeneity			0.037**	0.040**	0.034**	0.029*	0.106*
			(0.017)	(0.017)	(0.0160)	(0.016)	
Population density			0.893	0.996	1.087	1.399**	0.150*
			(0.687)	(0.866)	(0.850)	(0.706)	
Per capita total expenditure			0.101	0.103	-0.0347	-0.143	-0.018
			(0.484)	(0.595)	(0.600)	(0.560)	
Constant	41.19***	41.23***	34.59***	34.11***	35.24***	35.89***	
	(0.356)	(0.338)	(6.804)	(6.828)	(6.826)	(5.860)	
Regional FE	no	no	no	yes	yes	yes	yes
N	35	298	288	288	288	288	289
adj. R^2	0.063	0.019	0.138	0.136	0.197	0.115	
First stage instruments							
Distance to Bujumbura (log)						-4.996***	
						(0.695)	
Altitude (log)						-22.41***	
						(3.401)	
Hansen J, p-value						0.35	
Partial F						44.01	

Dependent variable: Degree of altruism scale 0-100. Robust standard errors in parentheses clustered at community level. Column (1): dependent variable is community average. Column (6): excluded instruments of first stage reported only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (7) contains beta coefficients.

Table 4 Conflict and risk preferences

	(1) gains OLS	(2) gains OLS	(3) gains OLS	(4) gains OLS	(5) gains OLS	(6) gains 2SLS	(7) losses OLS	(8) gains 2SLS
Relative number of	0.11***	0.10***	0.0614*	0.0659*	0.0656*	0.114*	0.0261	0.247*
dead in attacks (%)	(0.042)	(0.033)	(0.0328)	(0.0378)	(0.0377)	(0.0699)	(0.0366)	
Individual					-0.0914			
victimization Index					(0.0999)			
Household variable								
Respondent literate			-0.404	-0.398	-0.407	-0.351	-0.191	-0.086
			(0.253)	(0.256)	(0.258)	(0.269)	(0.308)	
Respondent age			0.0105	0.0107	0.0114	0.00945	-0.0085	0.073
			(0.0066)	(0.0065)	(0.0068)	(0.0074)	(0.0105)	
Respondent is male			-0.288	-0.257	-0.256	-0.208	-0.0456	-0.050
			(0.268)	(0.267)	(0.268)	(0.267)	(0.231)	
Total land holdings			-0.0309	-0.0384	-0.0395	-0.0347	-0.0298	-0.086
Per Capita			(0.0273)	(0.0285)	(0.0280)	(0.0250)	(0.0288)	
Community variable								
Land Gini			-1.153	-1.020	-1.111	-1.310*	0.125	-0.125*
Coefficient			(0.769)	(0.886)	(0.901)	(0.758)	(0.806)	
Distance to market			0.237	0.254	0.237	0.189	0.207	0.066
			(0.190)	(0.178)	(0.176)	(0.209)	(0.225)	
Conflict over land			0.132	0.359	0.358	-0.0068	1.153	0.000
			(1.243)	(1.288)	(1.299)	(1.059)	(1.210)	
Ethnic homogeneity			0.0122	0.0133	0.0143	0.0101	0.0001	0.075
			(0.0086)	(0.0086)	(0.0086)	(0.0114)	(0.0143)	
Population density			0.505*	0.480	0.473	0.579	0.685	0.125
			(0.291)	(0.367)	(0.363)	(0.426)	(0.453)	
Per capita total			-0.370*	-0.340	-0.319	-0.413	0.0433	-0.096
Expenditure			(0.197)	(0.218)	(0.215)	(0.284)	(0.355)	
Constant	3.81***	3.88***	2.974	2.933	2.728	3.581	0.351	
	(0.189)	(0.201)	(2.596)	(2.591)	(2.603)	(3.169)	(3.776)	
Regional FE	no	no	no	yes	yes	yes	yes	yes
N	35	271	261	261	261	261	251	262
adj. R^2	0.16	0.042	0.118	0.115	0.113	0.109	0.09	
First stage instruments								
Distance to Bujumbura (log)						-4.996***		
						(0.695)		
Altitude (log)						-22.41***		
						(3.401)		
Hansen J, p-value						0.82		
Partial F						39.31		

Dependent variable ranges 1 (risk averse) - 6 (risk loving). Robust standard errors in parentheses clustered at community level. Excluded instruments of first stage reported only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1): dependent variable is community average. Column (8) contains beta coefficients.

Table 5 Conflict and time preferences

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) 2SLS	(7) 2SLS
Relative number of	0.572	0.760	1.468**	1.359**	1.366**	3.42***	0.383***
dead in attacks (%)	(0.591)	(0.474)	(0.618)	(0.566)	(0.571)	(1.249)	
Individual					3.185		
victimization Index					(2.143)		
Household variables							
Respondent literate			-4.301	-3.982	-3.791	-2.709	-0.033
			(5.349)	(5.332)	(5.345)	(5.387)	
Respondent age			-0.143	-0.146	-0.174	-0.185	-0.070
			(0.168)	(0.166)	(0.171)	(0.172)	
Respondent is male			-2.649	-3.185	-3.320	-1.589	-0.019
			(4.758)	(4.595)	(4.569)	(5.291)	
Total land holdings			0.206	0.417	0.474	0.572	0.071
Per capita			(0.454)	(0.446)	(0.447)	(0.502)	
Community variables							
Land Gini coefficient			-56.5***	-62.0***	-59.2***	-73.9***	-0.358***
			(15.32)	(15.55)	(15.84)	(14.41)	
Distance to market			2.500	2.686	2.976	0.0537	0.001
			(3.539)	(3.991)	(4.008)	(4.166)	
Conflict over land			-29.24	-37.04**	-37.43**	-54.5***	-0.204***
			(19.43)	(17.66)	(17.27)	(18.73)	
Ethnic homogeneity			-0.554**	-0.626***	-0.659***	-0.77***	-0.280***
			(0.226)	(0.187)	(0.181)	(0.189)	
Population density			7.699	8.285	8.978	13.50*	0.145*
			(6.509)	(8.106)	(8.150)	(7.284)	
Per capita total			10.37	9.685	9.036	6.198	0.071
expenditure			(7.011)	(5.882)	(5.897)	(5.283)	
Constant	39.32***	37.83***	-26.48	-25.45	-20.80	1.119	
	(4.280)	(3.967)	(77.03)	(72.39)	(73.12)	(59.86)	
Regional FE	no	no	no	yes	Yes	yes	yes
N	35	285	276	276	276	276	276
adj. R^2	0.01	0.004	0.104	0.115	0.117	0.082	
First stage instruments							
Distance to Bujumbura (log)						-4.996***	-4.996***
						(0.695)	(0.695)
Altitude (log)						-22.41***	-22.41***
						(3.401)	(3.401)
Hansen J, p-value						0.44	0.44
Partial F						48.65	48.65

Dependent variable: discount rate. Robust standard errors in parentheses clustered at community level. Excluded instruments of first stage reported only. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Column (1): dependent variable is community average. Column (7) contains beta coefficients.

Table 6 Student t-test on means and variances of preferences

	social	risk	time
Community mean			
Below mean violence	25.38 (1.51)	3.82 (0.12)	37.27 (2.35)
Above mean violence	35.59 (3.69)	4.97 (0.21)	48.30 (4.65)
Difference	-10.21	-1.15	-11.45
p-value	0.00	0.00	0.04
Variance around community mean			
Below mean violence	1.75	1.04	5.80
Above mean violence	1.86	0.60	5.94
Difference	-0.11	0.44	-0.15
p-value	0.64	0.00	0.79

Table 7 Conflict and behavior

	(1) social capital	(2) fraction of cash crops in total production	(3) expenditures on farm improvements	(4) social subsample 1998 male households	(5) risk subsample 1998 male households	(6) time subsample 1998 male households
	OLS	Tobit	OLS	2SLS	2SLS	2SLS
Relative number of dead in attacks (%)	0.0377**	0.00783*	-21.28**	0.567***	0.328**	0.575
	(0.0155)	(0.00474)	(10.39)	(0.198)	(0.144)	(1.956)
Household variables						
Respondent is literate	0.376**	0.0500	119.1	2.244***	0.107	-9.137
	(0.145)	(0.0377)	(90.63)	(0.752)	(0.422)	(7.105)
Respondent age	-0.00162	0.00174	0.323	-0.0465**	0.00247	-0.0487
	(0.00403)	(0.00143)	(3.271)	(0.0219)	(0.0133)	(0.236)
Respondent is male	-0.139	0.0609	-30.83			
	(0.140)	(0.0415)	(72.38)			
Total land holdings per capita	0.0270*	0.0126***	-5.933	0.110*	-0.0147	0.355
	(0.0142)	(0.00460)	(9.504)	(0.0646)	(0.0338)	(0.567)
Community variables						
Land Gini coefficient	-0.0157	0.0940*	128.3	0.135	-1.098	-89.39***
	(0.330)	(0.0541)	(200.1)	(1.871)	(1.198)	(18.56)
Distance to market	-0.0913	-0.492***	74.36	-1.334**	-0.251	7.122
	(0.0996)	(0.138)	(51.83)	(0.539)	(0.329)	(4.964)
Conflict over land	-0.988**	0.00289	519.8	-4.596**	-0.236	-82.32***
	(0.479)	(0.0411)	(353.9)	(2.260)	(1.397)	(23.33)
Ethnic homogeneity	-0.00182	-0.620***	-5.222*	0.0155	-0.00612	-0.805***
	(0.00656)	(0.206)	(2.850)	(0.0211)	(0.0161)	(0.227)
Population density	0.0870	0.00219	-89.96	0.919	0.601	14.14*
	(0.188)	(0.00211)	(139.4)	(0.892)	(0.520)	(7.956)
Per capita total expenditure	-0.266*	0.295***	132.1*	0.0986	-0.452	4.742
	(0.144)	(0.101)	(69.95)	(0.790)	(0.414)	(6.359)
Constant	2.252	0.255***	-564.5	39.04***	5.979	2.785
	(1.864)	(0.0287)	(1059.4)	(7.981)	(4.601)	(72.82)
Fixed effects	yes	yes	yes	yes	yes	yes
N	283	288	289	169	148	161
adj. R^2	0.029		0.005	0.023	-0.153	0.186
Hansen J, p-value				0.30	0.83	0.55
Partial F				17.88	9.22	17.09

Robust standard errors in parentheses. * p < 0.10, ** p < 0.05, *** p < 0.01. First stage instruments distance and altitude, results not shown. Column (2) is unconditional tobit regression with fixed effects, potentially inducing some bias in our estimate; regression using random effects is qualitatively similar.

Table 8 Other type shocks and preferences

	(1) social	(2) risk	(3) Time
Severe draught	-1.290	-0.689	-0.334
	(1.453)	(0.597)	(0.676)
Access rain	0.0936	-0.223	0.0516
	(1.057)	(0.439)	(0.435)
Manioc crop disease	-2.142**	-0.400	-0.861
	(1.011)	(0.552)	(0.992)
Upcoming ceremony			-0.768
			(0.795)
N	288	261	276

Table summarizes coefficients of separate regressions including controls and fixed effects. Robust standard errors in parentheses, clustered at community level. * p < 0.10, ** p < 0.05, *** p < 0.01

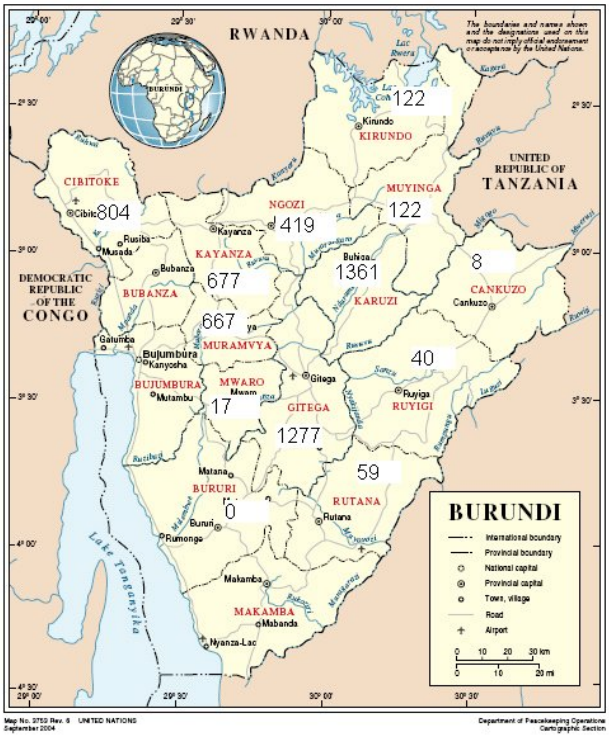


Figure 1. Number of casualties in surveyed communities as a result of attacks by army or rebels by province over 1993-2003 period (BCS data)

Appendix

Table A1 Choice Pairs Task 1

	A		B	
Question	Self	Other	Self	Other
1	0	250	60	240
2	60	240	120	220
3	120	220	180	180
4	180	180	220	120
5	220	120	240	60
6	240	60	250	0

Table A2 Choice Pairs Task 2

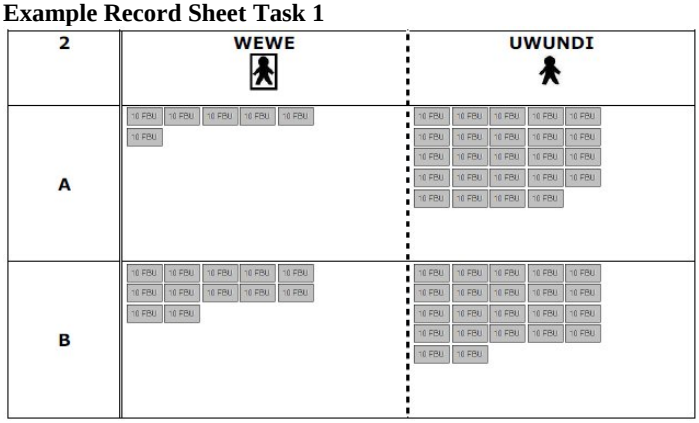
	Certain	gamble	
Question	gain/loss	*p*	gain/loss
1	500	0.3	2000
2	600	0.3	2000
3	700	0.3	2000
4	-500	0.3	-2000
5	-600	0.3	-2000
6	-700	0.3	-2000

Table A3 Choice Pairs Task 3

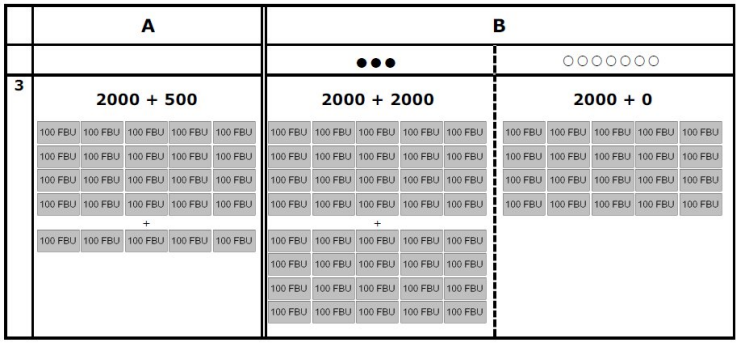
Question	Tomorrow	In two weeks and one day	d
1	1000	1000	0
2	1000	1010	0.01
3	1000	1020	0.02
4	1000	1050	0.05
5	1000	1100	0.1
6	1000	1400	0.4
7	1000	1700	0.7
8	1000	2000	1

Participants were not shown (d).

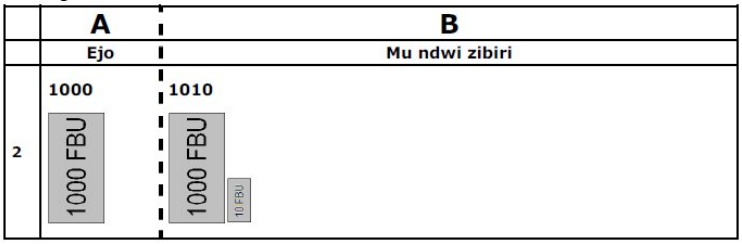
Example Record Sheet Task 1



Example Record Sheet Task 2



Example Record Sheet Task 3



Data Appendix

Violence variables

- Number of dead in community attacks relative tot population size Reports number of dead on *colline* as a consequence of confrontations between rebels and army as well as one sided violence between 1993-2003 divided by population size, as stated by local administrators (BCS).
- Household level victimization index is an additive index of the exposure of any household member to either rape, theft, forced labor, torture, ambushed. Since the absolute scale of this variable is arbitrary it is normalized to have a mean of zero and standard variation of one (BPHS).

Household and community variables

- Age: *Age of head of household*, measured in years (BPHS and ESD-SR) .
- Respondent is male: *Gender of head of household*. In household level regressions variables is dummy variable taking unity if head of household is male, zero else. In community level regressions, variable household dummy's are averaged over number of households per community (BPHS and ESD-SR).
- Respondent is literate: *Literacy of head of household*. In household level regressions variables is dummy variable taking unity if head of household is literate, zero else. In community level regressions, variable household dummy's are averaged over number of households per community (BPHS and ESD-SR).
- Land size per capita (m^2): Total land size of household i in square meters, divided by number of adult equivalents present in household i (BPHS).
- Distance to market: Distance to main agricultural market where food and non-food items are traded, measured in time intervals of 15 minutes, where $t = 1,...,5$ (BCS).
- Prewar income (1993): In household level regressions variables is i-th household indication of perceived level of wealth in 1993 on a scale, ranging 1 (very poor)-6 (very rich). In community level regression, variable for j-th community is created by averaging over all community households (BPHS).
- Perceived trust level 1998 and 2007. Perceived level of trust in community members, rated on a 10 point scale 1 (very low)-10 (very high) (BPHS).
- Livestock farmer 1993: In household level regressions variables is dummy taking unity if household owned livestock in 1993, zero else. In community level regression, variable for j-th community is created by averaging over all community households (BEES and ESD-SR).
- Cash crops relative to total production. Share of cash crops (coffee, tea, tobacco and cotton) produced relative to total production (BPHS).
- Land Gini coefficient: Variable based on household land holdings. Community level Gini coefficient is created by $G = 1 - 2\int_0^1 L(X)dX$ (BPHS).
- Population density (1990 and 2008): Number of people in community per square kilometer (MPDRN).
- Vote in favor of president Ndadaye: Percentage of votes in favor of Ndadaye at the commune level during the presidential elections in 1993.
- Distance to Bujumbura. Distance of j-th community to capital in kilometers.
- Altitude. Average altitude of j-th community (MPDRN).
- Land conflicts: Dummy taking unity if there are land conflicts in community (BPHS).
- Ethnic homogeneity: Percentage of Hutu population in community (BEES).

- Community income: Variable is the aggregated income of per capita expenditure for all goods purchased over a 15 day period valued at local market prices and divided by the adult equivalents of household i (BPHS).
- Severe draught: Dummy taking unity if household was exposed to severe draught in past three years (2007-2009) (BEES).
- Access rain: Dummy taking unity if household was exposed to access rain in past three years (2007-2009) (BEES).
- Manioc crop disease: Dummy taking unity if household was exposed to cassava crop disease in past three years (2007-2009) (BEES).
- Upcoming ceremony: Dummy taking unity if household was expecting a ceremony in the near future (BEES).

Experimental variables

- Social preferences: Degree of altruism resulting from 6 choices between participant (S) and randomly chosen community member (O), for each question holds $S^2+O^2 = (250)^2$, the resulting degree of altruism is $\alpha = \tan(O/S)$, with $37.5 < \alpha < 52.5$. Item rescaled to $0 < \alpha < 100$. In community level regression, variable for j -th community is created by averaging over all community households.
- Risk preferences: Switch point [0 (risk averse), … , 6 (risk loving)] between risky gain (or loss) with a probability p = 0.3, and certain (y) gain (or loss). In community level regression, variable for j -th community is created by averaging over all community households.
- Time preferences: Discount rate is switch point between receiving 1000 FBU in one day and receiving (1+ d)1000 FBU in two weeks and one day, d = [0, … , 100]. In community level regression, variable for j -th community is created by averaging over all community households.

Data sources

BPHS. Burundi Priority Household Survey 2007
BCS. Burundi Community Survey 2007
BEES. Burundi Experiments Exit Survey 2009
MPDRN. Monographies Communales Burundi. Ministre de la Planification du Developpement et de la Reconstruction Nationale, Bujumbura, 2006
ESD-SR. Enquête Socio-Démographique, Santé et Reproduction 2002