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Violence and social capital: Evidence of a microeconomic vicious circle*

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Abstract

We test with a randomized experiment in the slums of Nairobi whether violence suffered during the 2007 political outbreaks affects trustworthiness learning when participants live group experiences and face opportunism and free riding in common pool resource games (CPRGs) between two subsequent trust games (TGs). Our findings document that participants move toward balanced reciprocity after the CPRG, with the exception of those who have experienced directly or indirectly physical violence and/or forced relocation who exhibit significantly less trustworthiness in the second TG round. Results are robust to several robustness checks controlling for selection into victimization.

Keywords: trust games, public good games, randomized experiment, social capital, socioeconomic instability and development.
JEL Classification: O12, C93, Z13.

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1. Introduction

The literature of cross-country growth regressions documents the negative relationship among sociopolitical instability, violence and growth illustrating how social conflicts and civil wars may hamper savings, investment, income and conditional convergence (see, among others, Barro, 1991; Alesina, Ozler, Roubini, and Swagel, 1996; Alesina and Perotti, 1996 and Svensson, 1998). Given that endogeneity problems in this kind of literature cannot be fully solved it is important to find new ways to understand what drives the correlation between the two groups of variables.[1]

In this respect an important direction of inquiry looks at micro data and quasi-experimental evidence. Among the very few papers working in this direction, Bellows and Miguel (2006 and 2009) find with panel data a positive relationship between victimization and later individual political mobilization, on the one side, and participation in local collective action, on the other side, in Sierra Leone. They argue that the positive reaction of victims is a psychological legacy of what they suffered.[2] Akresh, Bundervoet and Verwimp (2009) document the negative effect on height of the experience of child soldiering in Burundi, while Blattman and Annan (2010) provide evidence on the psychological distress generated on children by the civil war experience in Uganda. By commenting these findings in their survey Blattman and Miguel (2010) emphasize that more evidence at micro level is needed to solve econometric identification problems and to understand why in some cases recovery is faster than in others.[3]

In this respect, one of the most explored intermediate channels through which political instability and civil wars may impact upon economic development is *by affecting the law of motion of social capital*. As it is well known social capital is getting increasing attention among economists since its

[1] As it is well known the problem in this case is not just reverse causality (low economic development may well be the cause and not the consequence of socio-political instability due to its persistent high order autocorrelation) but also an omitted factor which may drive the correlation between the two observed variables.

[2] The authors accurately control for omitted variable bias with a series of checks including village fixed effects in order to rule out the possibility that the association they find is explained by violence targeting toward existing or potential village leaders.

[3] *"Viewed through the lens of economic growth theory, however, there remain more gaps than solid conclusions in our understanding of postwar recovery. Both theory and evidence are weakest in assessing the impact of civil war on the fundamental drivers of long-run economic performance—institutions, technology, and culture— even though these may govern whether a society recovers, stagnates, or plunges back into war"* (Blattman and Miguel, 2010, p.8).

importance in promoting wellbeing and growth has been acknowledged in many theoretical and empirical contributions[4]: social capital may help to sustain cooperation, foster institution shaping, reduce market failures as well as negative externalities and conflicts of interests. Several theoretical and empirical contributions conclude that trust and trustworthiness (two of the most important dimensions of social capital), are "lubricants" (Arrow, 1974) of the socioeconomic system, substitutes of formal contracts (Becchetti and Conzo, 2010) and factors which significantly reduce transaction costs in social and economic interactions by helping to "enforce cooperative agreements in bilateral sequential exchanges" (Bohnet and Greig, 2008).

Blattman and Miguel (2010) emphasize both the importance of the social capital channel in the relationship between socio-political instability and growth and the scarcity of empirical analysis and evidence on this point by observing that "*A sizable literature has sought to identify the specific institutional factors that matter most for economic growth — including property rights (Acemoglu, Johnson and Robinson, 2001), social capital and cohesion (Knack and Keefer, 1997), rational bureaucracies, and work ethics, to name a few – but which of these are affected by civil war (not to mention how much and under what circumstances) remains a matter of speculation. The social and institutional legacies of conflict are arguably the most important but least understood of all war impacts*" (p. 42).

The goal of our paper is to contribute to this literature with a randomized field experiment[5] run in the Nairobi slum of Kibera by looking at "how much and under what circumstances" the effects of

[4] The level of trust in a given country is reported to have positive effects on economic growth (Knack and Keefer, 1997 and Zak and Knack, 2001) and on institutions (Putnam, 1993 La Porta et al. 1999). According to Brown and Ashman (1996) different forms of social capital are crucial to solve development problems through cooperation. Becchetti and Pace (2006) and Fullenkamp and Chami (2002) document how trust and trustworthiness may impact positively on firm productivity. At aggregate level, it has been shown that lack of trust and trustworthiness prevents the development of economic relationships among individuals belonging to different ethnic groups and is therefore one of the microeconomic causes of poor economic performance (see, among others, Alesina, Baqir and Easterly, 1999; Gradstein and Justman, 2002; Gradstein 2002 and Montalvo and Reynal-Querol, 2005).

[5] As it will be made clear from what follows the experiment is randomized with respect to participation to the PGG/placebo treatment. It cannot obviously be randomized with respect to the violence experience and it is not in terms of random placement of people having experienced ex ante a given type of violence to avoid bias in participant behavior. We tackle the problem related to this point in the ex post randomness check (section 3) and in the econometric estimates and sensitivity analysis which follow (sections 5 and 6).

the 2007 post-election violence affect a specific form of social capital (trustworthiness) of victimized slum-dwellers.

We choose to run our experiment in the Kibera slum of Nairobi, a place which can be considered among one of those in the world where social capital is scarcest and its development most important to reduce transaction costs in presence of informal or weakly enforceable formal contracts.[6] Cassar and Wydick (2010) run a microfinance game experiment in five poor areas in Armenia, Philippines, India, Kenya (in the same slum of Kibera) and Guatemala at end 2006-beginning 2007 and show that the average players' contribution rate to public goods is dramatically lower in Kenya and roughly half of that in all the other four areas. This striking difference is confirmed by the duration of the microfinance game in which the possibility of participating to subsequent rounds, with potential economic gains for participants, is subordinated to cooperation in the previous ones. The authors observe that in Nairobi 70 percent of groups end their game in the first round while this occurs only for 10 percent or less in the other four countries. These findings are not in contrast with those of Greig and Bohnet (2008) documenting in a one shot trust game experiment run in 2004 that Nairobi slum dwellers adhere to norms of *balanced reciprocity* and not of *conditional reciprocity,* with the former generating less social capital than the latter.[7]

Causes of poverty of social capital in Nairobi slums are both structural and related to the civil unrest following the December 2007 elections. Circular migration patterns which weaken ties among slum dwellers are likely to contribute to the structural element. Beguy, Bocquier and Zulu (2010) document that the majority of them spend less than 3 years on average in the area and that a quarter of them stays for less than one year. Newcomers are attracted by the opportunities offered by the

[6] In such context the effect of the observed scarcity of social capital is expected to be especially severe in those local economic interactions which are not always subject to formal contracts and regulation such as land property and recovery of non performing loans from cosigners or guarantors in microfinance lending schemes.

[7] Following Greig and Bohnet (2008) "*Reciprocity is an internalized norm, inducing people to respond to kindness with kindness and to unkindness with unkindness, even if it is not in a person's material self-interest to do so. It differs fundamentally from cooperation in repeated games where reputational concerns can enforce "cooperation" (e.g., Kreps et al., 1982; Fudenberg and Maskin, 1986).*" In a standard trust game the trustee is regarded as behaving according to a norm of *conditional reciprocity* when trustworthiness increases with trust (Camerer and Fehr, 2003; Greig and Bohnet, 2008), while following a norm of *balanced reciprocity* if trustworthiness does not vary with trust. For theoretical references on reciprocity see among others Rabin (1993), Fehr and Schmidt (2001), Bolton and Ockenfels (2000), Charness and Rabin (2002), Dufwenberg and Kirchsteiger (2004) and Falk and Fischbacher (2006).

city in which around one fifth of the population lives on western standards. This creates a dramatic demographic pressure in a limited space. Due to these specific characteristics, when compared with Dakar and Johannesburg slums, Nairobi ones have relatively high employment and education levels of dwellers but much worse infrastructural endowment and more limited space (Gulyani, Talukdar and Jack, 2010).

The specific event whose consequences are object of our inquiry is the Kenya's outbreak of violence occurred between the end of 2007 and the beginning of 2008, immediately after political presidential election pools were made public. Mwai Kibaki, the incumbent president of Kenya representing Kikuyu tribal interest in the nation, overtook his opponent Raila Odinga, leader of a political party based on a tribal alliance among Kalenjin, Luhyas and Luos. Odinga and his supporters alleged manipulations during the election process in favor of Kibaki, subsequently confirmed by national and international observers. In few months, the country experienced unprecedented crisis whose social, economic and humanitarian impact is still burdening Kenyan society (Roberts, 2009).[8]

Based on these events, we argue that one of the microeconomic links which may explain the impact of violence and civil conflicts on economic development and illustrate the legacy of civil wars is the effect of the memory of violence on social capital which is, in turn, a fundamental driver of economic growth. Our experiment run in the Kibera slum of Nairobi is aimed to address this issue by wondering whether violence suffered during the 2007 political outbreaks affects two years later changes in trustworthiness when participants experience opportunism and free riding in modified public good games (Common Pool Resource games) between two subsequent trust games (TGs). The underlying assumption is that, as it is well known in the literature, in a framework of asymmetric information and incomplete contracts, trust games mimic conditions which reinforce not only social but also business relationships which are at the root of economic growth. In our

[8] The official reported consequences of the 59 day conflict were: (i) 1500 dead, (ii) 3000 women raped, (iii) damage to physical assets, (iv) the displacement of about 300,000 people; (v) the loss of confidence among investors and tourists and (vi) damage to social capital. In addition, in areas such as Kibera living conditions suddenly deteriorated due to disruption in the delivery of HIV/AIDS treatment by local and international NGOs.

experiment the CPRG treatment we devise aims to mimic the frequent practice of community provision of local public goods (*harambee*) in order to investigate whether such practice affects the law of motion of social capital when individuals suffered consequences of a civil war.[9] Our findings document a move toward more social capital (from *less than* balanced toward *balanced* reciprocity) among non victimised participants after the CPRG experience, while not among those who experienced directly or indirectly physical violence and/or forced relocation; in fact, victims of this kind of violence tend to reduce their trustworthiness levels.

The paper is divided into seven sections (introduction and conclusions included). In the second section we describe our experiment design. In the third and the fourth we present and comment results of non parametric hypothesis testing. In the fifth and sixth sections we illustrate our econometric findings and robustness checks respectively. The seventh section concludes.

2. The Experiment design

Our field experiment was implemented during July-August 2010 in Kibera, the second largest slum of the world located in Nairobi (Kenya). We randomly selected 404 slum-dwellers and ask them to participate to three experimental sessions plus a demographic survey[10].

The experiment is based on two identical *individual sessions* where participants play the game only with the instructors, and on an in-between *group session* where participants interact face-to-face with their peers in groups of four members each. The sequence of the sessions is i) *Trust Game 1* (TG1) aimed at measuring ex-ante trust and trustworthiness levels; ii) *Modified Public Good Game*

[9] *Harambee* means in Swahili "let's pull together" (Miguel and Gugerty, 2005) and denominates the well known practice of bottom-up collective effort for providing public goods in the area in which we operate (Greig and Bohnet, 2009). This tradition is at the root of community fundraising and/or gratuitous labour supply for building and maintaining schools, clinics and wells (Miguel and Gugerty 2005; Huges and Mwiria 1990).

[10] To incentivize participation and effort, participants received a show-up fee (150KSh) and were informed that, depending on their performance in the experimental sessions, they might win up to 800 KSh. Individuals have been randomly recruited using a map of the informal buildings in the Kianda area of the slum of Kibera (Nairobi). When finding two households in the same building, local experimenters tossed a coin and selected one; in case they found three, they randomly extracted one token out of the four representing directions (North, South, West, East). They selected individuals aged more than 18, alternating the gender for each household. Finally, a list of all the individuals identified in the area willing to participate was created and 404 participants were randomly shortlisted.

(CPRG) aimed at observing cooperation dynamics over five rounds; iii) *Trust Game 2* (TG2) in which participants repeat the TG1; iv) demographic survey.

In order to reduce potential biases caused by the presence of foreign researchers, we trained local staff to carry out the experiments and the survey (even though we were on the field to direct the experiment). Field assistants were informed about the details of the games only after the end of the selection period in order to limit word-of-mouth effects and prevent local staff from revealing projects' characteristics beforehand. We also made sure local experimenters alternate each other in each session so to exclude confounders deriving from i) previous relationships between experimenters and players ii) social pressure participants may feel in playing the two rounds of the trust game with the same experimenter iii) reputational dynamics deriving from the interaction of the participant with the same experimenter in both trust games. The games are explained in detail in the following subsections.

2.1 Trust Game

The Trust Game we implement has the same structure of the standard two-player Investment Game (Berg, Dickhaut and McCabe, 1995). Participants in our game are told they are matched with a counterpart whose identity is not revealed. If the player is chosen to be a trustor, she must decide how much of her endowment to send to the anonymous counterpart (trustee), knowing that this amount is tripled and that the trustee chooses if and how much of it to return (Figure 1). If the player is chosen to be a trustee, she has to decide how much she will return over a set of ten potential amounts an anonymous trustor may send (*strategy method*). We adopt the strategy method (instead of the alternative of communicating the actual trustor contribution and asking the trustee to respond) because it allows us to match trustees with trustors in a non-simultaneous framework and without a prior knowledge of the trustor choice. In addition, eliciting the full trustee's potential responses to trustor's actions provides us with more detailed insights on her strategies.

As it is well known from the literature the most common interpretations for trustors' deviation from the Nash Equilibrium are generally strategic altruism, pure altruism and inequity aversion, while those for trustee's deviations pure altruism, inequity aversion and reciprocity (see, among others, Fehr, 2009).

Participants play the trust game at the beginning of the session and after a (modified) Public Goods Game (the Common Pool Resource Game) (Figure 2). They are informed about neither the sequence of the games nor the payoff from the first trust game until the end of the whole experimental session in order to avoid confounding reputation effects. [11]

As explained above we are interested in evaluating whether the experience of the group dynamic interaction in the CPRG modifies players' TG behaviour (measured as contribution in TG2 minus contribution in TG1). However, confounding effects (i.e., first TG history and TG learning effects even in absence of revealed first round players payoffs) not related to the CPRG experience may explain the participants' change in trustworthiness. For this reason we design a specific "placebo-treatment" in which 100 subjects do not play the CPRG between the two trust games (Figure 2).

2.2 The Common Pool Resource game

Public good games are designed to investigate how people behave when facing a conflict between individual and group benefits (Camerer and Fehr 2003). More specifically, we implement a variant of the PGG called the *Common-Pool Resource game* (hereon also CPRG) (Henrich and Smith, 2004)[12]. The CPRG treatment is motivated in our experiment by the attempt of reproducing the

[11] We told participants they would be paid at the end of the whole experimental session and just for one randomly chosen TG. Their initial endowment in each TG was 50 KSh.

[12] Camerer and Fehr (2003) summarize common results in the literature of Public Good Games. In one-period PGG, participants contribute an average of a half of their endowment, but the distribution is typically bimodal with most subjects contributing either everything or nothing. Higher values of the private return lead to higher contributions. When the PGG is repeated for a finite number of periods, irrespective of the group composition from period to period, subjects initially contribute as much as they do in one-period games, but contributions decline substantially over time. In addition, 60-80 percent of all subjects contribute nothing in the final period and the rest contribute little. Another important fact is that about half the subjects are "conditional co-operators" - that is, they contribute more when others are expected to do the same and actually do it (Fischbacher, Gachter and Fehr, 2001). Contributions decline over time because, according to the authors, subjects are willing to cooperate if the others do the same. However, in presence of selfish subjects who never contribute, reciprocal subjects gradually notice that their action encounters free-riding

commonly observed practice of local community provision of local public goods (*harambee*) (Greig and Bohnet, 2009) (see footnote 9 for details).

Each group is randomly composed by four individuals who sit in a circle around a pile of 600 Kenyan Shillings (KSh) corresponding to € 6.18. Participants are told that they can withdraw any amount between zero and 150 KSh from the pile and keep it. After it, the money left in the pile is doubled and distributed equally among players. To play the game anonymously and simultaneously, each player writes down on a sheet how many KShs she wants to withdraw. After it experimenters make the calculations and write down the payoffs accordingly[13].

In the version of the game implemented by Henrich and Smith (2004), only two rounds are played to control for public approval/fear of punishment, the first with restricted information, while the second with extended information. The main difference between the two rounds is that, in the first, players do not see how much the other members of the group receive, while, in the second, they publicly announce how much they withdraw (the payoffs are then publicly distributed). In this framework, however, it may be difficult to isolate the effect of the public approval in the full information round from a mere game-learning effect that starts from the first round.

In order to avoid this confounding effect, we adopt a *between subject* design (each subject participates to just one of two CPRG treatments). Specifically, we design two treatments in which participants play the game in its restricted information version (CPRG-RI) *or* in its public/full information one (CPRG-FI).[14] Each of the two treatments is composed by five rounds but the

responses. When punishment is introduced the pattern is reversed since the punishment causes a sharp jump in cooperation and a steady increase until almost all subjects contribute their whole endowment (Gachter and Fehr, 2003).

[13] Experimenters explain the game with few examples highlighting different potential scenarios. We do not believe that such examples may enhance strategic learning among players since the latter do not know other's player strategy but simply the potential payoff distributions in some of the potential scenarios. Moreover, since the game may sound too unfamiliar, we deem important to help participants in achieving a fair level of comprehension necessary to participate in the game.

[14] The restricted/full information split helps us to understand whether peer pressure in PGG games plays a role in trustworthiness changes between the first and the second TG. The restricted/full information factor will be controlled for in sections 5 and 6.

number of rounds is known only to experimenters in order to reduce endgame behaviour effects[15]. In the CPRG-RI, after the player's sheet reporting the withdrawal decision is handed to the experimenter, payoffs are distributed in envelopes so that players do not know how much other members win. In the CPRG-FI, before calculating and distributing the payoffs, each player has to announce to the group members the amount she withdraws.[16] At the end of each round, payoffs are distributed by marking (on the envelope in the case of CPRG-RI or on a clip in the case of CPRG-FI) the number of the corresponding round. At the end of the game, each player extracts from a black bag a number from 1 to 5 identifying the round for which the player's payoff is eventually converted in real money at the end of the experimental session.

In terms of relevance of the amount of money at stake consider that in the game it is possible to win up to 800 Kenyan shillings (the average weekly wage in the area). The sum is important also when we consider that half of all households in Nairobi slums can be categorized as "*food insecure with both adult and child hunger*" with 70 percent of people living below the poverty rate (Baschieri et al. 2010) and a 2007 survey of the cost of basic needs in Nairobi indicates that such cost amounts to 100 shillings per day per member (Phares Kirii, 2007).

The above described TG-CPRG-TG "sandwich" design is devised in order to gather full information on the strategies of trustees together with the impact of an (restricted or full information) group activity which resembles the well known *harambee* practice in Kenyan slums. Consider in fact that a simple multistage dynamic TG in which round specific payoffs are revealed at end of each stage (hence without strategy method) would have made impossible the verification of the net effect of the group activity and opportunistic behavior from the CPRG game on trust and trustworthiness of TG participants. This is because in the second round TG behaviour would have

[15] Players are informed at the beginning of the game that they will be paid for just one randomly-chosen round and do not know the exact number of rounds. We take this decision in order to mimic the effect of ongoing (non terminating) PGG-like activities on social capital formation.

[16] In this case payoffs are then distributed without envelopes so that all players see how much each person has withdrawn and her payoff (these instructions are given at the beginning of the round). To make sure all participants come to know this information, the experimenter also announces each member's payoffs at the end of each round.

been affected not just by the impact of the CPRG game experience but also by the first round TG outcomes revealed to the players.

3.1 Description of violence and randomness check

As explained in the experiment design section we do not use the ex ante question on the violence suffered in the political riots of 2007 to discriminate between treatment and control sample since we do not want that participants to the experiment understand that we are looking at that specific effect. We are however confident that we will have a sufficient number of individuals being affected by those events by recruiting our participants in the Kianda area.[17] More specifically we consider in our questions five types of consequences: i) personal injuries; ii) loss of relatives; iii) material losses (home destroyed or damage to personal property); iv) job losses; v) forced relocation from original living area.

These five types of consequences may be classified in three more general groups: a) (direct or indirect) *physical violence* (hereafter also DIPV) which includes i) and ii); b) *economic losses*, which include iii) and iv) and c) *forced relocation* (hereafter also FR). We further cluster them in just two groups where the first includes a) and c) and the second b). We define the former as DIPVFR - direct/indirect physical violence and forced relocation - and the latter as EL - economic losses. The rationale for this final taxonomy which follows, is that events a) and c) are those with the strongest impact on social capital. We consider forced relocation as having on social capital stronger impact than economic losses and assimilated to direct or indirect experience of physical

[17] In Nairobi on average, 10% of the households experienced damages to personal properties or loss of job while 18% were affected by closure or destruction of their own business and 21% have been evicted from their homes. In addition, 5% of the households have seen one of their family members to die as direct consequence of the fights/riots. However within Nairobi urban area, post election violence occurred mainly in the slums. As it is well know, Kibera, the most densely and poor populated slum in Nairobi, had to bear the consequences of outbreaks of violence. Hence, it is reasonable to assume that the highest number of households hit by forced migration, job losses or physical violence is concentrated in this area.

violence since individuals who experience it are relocated against their will from the environment in which have built their social ties to a new area in which they presumably do not have them.

By considering the above mentioned groups we find that 54.41 percent of our respondents experienced economic losses (house destroyed, job loss or business loss or destruction), 27.9 physical violence (directly or indirectly due to the death of relatives) and 17.6 percent forced relocation. We also see that: i) the share of participants who did experience neither DIPV and/or FR nor economic losses is small (around 16 percent); ii) there is a wide overlap between the DIPVFR and EL groups (those who belong to both are around 37 percent); iii) the number of those belonging to the DIPVFR group without economic losses is very small (around 8 percent) but iv) there is a large number of participants experiencing economic losses who do not belong to the DIPVFR group (around 57 percent). The breakdown for trustees only is extremely close to the above described one with differences not larger than 2 percent points.

To verify whether the impossibility of randomizing ex ante the violence experience is compensated by ex post randomization conditional on observed variables we look at affected/unaffected balancing properties in terms of socio-demographic characteristics (age, gender, marital status, number of house members, ethnic group, number of friends, education, daily food expenditure, employment status)[18]. More in detail, by using our threefold taxonomy (DIPVFR, EL and unaffected groups) we compare each of the three groups with the rest of the sample or with only one of the two remaining groups (Table 2). We find that affiliation to the DIPVFR group *vis à vis* the rest of the sample is non random with regard to age, education, unemployed, and married status, with affected individuals being on average older, less educated, more likely to be married and less likely to be unemployed (18 against 33 percent).[19] This last finding may be in accordance with the Blattman and Miguel (2010) argument that those experiencing violence react strongly and positively to the shock. Note as well that the balancing property is less frequently met when

[18] See Table 1 for variable legend and Table A1 in Appendix for descriptive statistics.
[19] Note however that some of these differences are very small in magnitude (three years for age, one year for education).

comparing other subgroups. Hence, the best experimental conditions are obtained when comparing the DIPVFR with a control group which includes also those suffering economic losses.

Our second randomisation check looks at balancing properties along the CPRG/placebo divide for trustees. In this case the experiment design is devised to satisfy randomness ex ante. We find that the property is met for all the considered variables at 1 percent significance level (Table 3).

3.2 The CPRG behaviour

The dynamic behaviour of all subjects in the CPRG does not exhibit the standard pronounced decline in cooperation in the fifth and last round consistently with the fact that in our design we do not communicate the number of rounds to players in order to avoid end game effects (see footnote 12). In our sample, cooperation slightly decreases over rounds, with cooperation being measured in each round in terms of players' withdrawal ratio ($ withdraw/150) at individual level and left-in-the-pot ratio ($ left by the group/600) at group level (Figure 3).[20] Participants seem to observe the behaviour of group members and react strategically to it - if one or more than one defect in a round, others also do in the following round. With regard to violence victims we find that victimised tend to be less cooperative than non victimised even though the difference is not significant at 5 percent level (Table 4).[21]

4. Hypothesis testing on trustworthiness learning

In order to check whether involvement in 2007 events affects trust and trustworthiness learning we perform non parametric tests to compare the three violence groups in terms of changes in trustworthiness between the first and second TG. As a preliminary result consider that trustworthiness learning is not significantly different between placebo and CPRG treated (the t-stat of the parametric test for the difference in means is -.014 and the z-stat for the non parametric rank

[20] In this section we look for simplicity at trustees only since we are interested in the effect of our experiment on trustworthiness. Results on trustors are available upon request.

[21] A deeper analysis of what happens in the CPRG is developed in a companion paper and available upon request.

test 0.54). Our main finding is that, for those who experienced violence, the change in (mean) trustee contribution is significantly lower among participants subject to the CPRG treatment (-5.12 for the DIPVFR group against .86 for the rest of the sample and 3.52 for non-involved in any kind of violence) but not among those subject to the placebo treatment (Table 5.1). These results suggest that the reciprocity response to the CPRG experience is affected by the past violence experience.[22]

Consider as well that, given the strategy method adopted to identify the overall trustee strategy what we measure above is the average conditional contribution while the proper original measure to look at is each single trustee conditional response. We therefore repeat our test by looking at trustees' responses conditional to each of the 10 possible trustor contributions (Table 5.2) for CPRG participants. What we find is that the significant difference in trustworthiness applies for trustees' responses conditional to a trustor contribution between 20 and 45 shillings (again when comparing participants to the DIPVFR group with the rest of the sample or with non-involved). The finding is significant among participants subject to the CPRG treatment, while it is not if we consider placebo treated. Since the effect occurs only for trustees (and not for trustors) the candidate explanation is not strategic altruism but, more likely, reciprocity. In other words, individuals who experienced physical violence or forced relocation after the 2007 civil unrest are more likely to reciprocate less after a *harambee*-like practice. Our main result finds correspondence with survey evidence from Dercon et al. (2010). The authors document that the 2007 events modify respondents' opinion on whether violence should be reciprocated or not,[23] with those suffering the violence who exhibit a significant positive change in declared negative reciprocity.

Can endogeneity explain what we found ? Bellows and Miguel (2009) suspect that violence targeting at actual or potential village leaders might be the omitted driver of the observed

[22] Note as well that before the PGG game victimized individuals exhibit higher trust in both PGG and placebo groups thereby non contradicting Blattman (2010) results on the relationship between violence and social capital. Beyond it we find that the violence experience does not make any difference on levels of trustworthiness and changes in trust. Results are omitted for reasons of space and available upon request.

[23] The pre (2 week ahead) and post election surveys on a nationally and regionally representative sample of 1,207 Kenyans commented by the authors show that the outbreaks of violence significantly contributed to the deterioration of social capital. More specifically, after the election violence victims are 20 percent more likely to support actions outside the law while 40 percent are more likely to resort to violence.

correlation between the violence experience and the higher observed social capital. In our case this hypothesis seems implausible since the violence targeting assumption should generate a correlation in a direction which is opposite with respect to what we find if living victimised are social leaders who also excel in social capital learning. [24]

To sum up, the above mentioned endogeneity argument applies to a correlation between a performance variable (social capital) and a non random treatment (violence). In our case we have a relationship between a change in performance variable (change in trustworthiness) and the product of a variable which is random by experiment design (the exposure to the CPRG treatment) and a variable which cannot be random by experiment design, but is random ex post conditionally on most observables (the experience of a certain kind of violence). Endogeneity is hard to conceive in this case. Even in the extreme case under which we might conceive a variable (i.e. an psychological trait) which both correlates with the change in the performance variable (independently from the treatment) and affects violence targeting, it is hard to explain why the correlation is observed only when the violence interacts with the random CPRG treatment.

To have such a situation we should model a driver which correlates with the trustworthiness reaction to the CPRG and victimisation. We will deal with this extreme hypothesis in the sections which follows (and especially in the sensitivity analysis run in section 6).

5. Econometric findings on the determinants of trustworthiness dynamics across TG rounds

Non parametric tests are generally considered sufficient to illustrate experiment findings by experimentalists when the randomization process is rigorous and verified ex post as being successful. In our case we deem important to perform econometric estimates around our main findings for several reasons. First, we want to evaluate the magnitude of our effect, net of the impact of other controls. Second, we want to evaluate the net effect of different types of violence by

[24] We control for potential observable and unobservable heterogeneity in the sample in the econometric estimates in sections 5 and 6 respectively. The rich set of controls we add in our regressions allows to reduce the heterogeneous effects of DIPVFR on the change in trustworthiness.

controlling for economic losses. Third, we want to eliminate the suspicion that our findings are driven by confounding effects (due to selection) in robustness checks by using inverse propensity score weights and by looking at sensitivity to omitted variable bias. Fourth, following Greig and Bohnet (2008), we want to test whether the observed change in trustworthiness implies a change in (or from) *balanced* or *conditional reciprocity*. Fifth, we need to control for the restricted/full information feature of the CPRG. Sixth and more general, we want to correct for the heterogeneity in the experience of the treatment since our experiment is not a medical trial in which exactly the same substance is administered to all patients in the treatment group. More specifically, we must control whether qualifying variables which describe the story of the CPRG game may have impacted differently on those who suffered violence and those who did not, thereby making the causality link described in the previous section a spurious finding. It may be for instance that the observed effect is due to the fact that participants belonging to the physical violence/forced relocation group experienced higher gender or ethnic fragmentation[25] in the CPRG groups to which they took part, higher mean group withdrawal ratios or were affected by the restricted/full information feature of the CPRG game.

As a first step in our econometric analysis we want to frame the violence/trustworthiness effect into a broader context in which we compare changes in giving of all players in the game (trustors and trustees, participants or not to the CPRG).

We estimate the following specification with OLS:

$$\Delta TG_i = \alpha_0 + \alpha_1 DIPVFR_i + \alpha_2 EL_i + \alpha_3 Trustee_i + \\ + \alpha_4 Trustee * DIPVFR_i + \alpha_5 TG1_i + \sum_j \beta_j X_{ji} + \varepsilon_i \qquad (1a)$$

where the dependent variable $\Delta TG_i = TG2_i - TG1_i$ is the difference in player's giving (if trustor) or returning (if trustee) between the second and first TG.[26] Regressors include a dummy which takes value of one if the individual is part of the DIPVFR group, a dummy which takes value of one if she

[25] On the role of ethnic fragmentation on social capital see, among others, Alesina and La Ferrara (2005).
[26] Trustees contributions are obviously average contributions elicited with the strategy method (we will analyse single contributions in detail in the empirical analysis that follows).

is part of the EL group, a dummy which takes value of one if the individual plays as a trustee in the two TGs, a slope dummy for trustees who witnessed DIPVFR, the amount sent or returned by all players in the first trust game (TG1) and a set of X-controls which include socio-demographic characteristics (see footnote 23). When excluding the sample of individuals in the placebo treatment, the set of X-controls contains also factors which capture the heterogeneity of the CPRG experience such as group ethnic and gender fragmentation, the payoff from the round of the CPRG randomly selected for payment, the average respondent's and group mean withdrawal ratios in the CPRG, the number of friends known by name in the CPRG group, the restricted/full information CPRG treatment, etc. (for a definition of all included variables see Table 1).[27]

Results are reported in Table 6a. In the column 1 we show the estimates of equation (1a) on the full sample in which we add the placebo-treatment dummy. In columns 2-3 we estimate equation (1a) on the sample of participants in the CPRG only and add the above-mentioned CPRG-game controls. In column 4-6 we report regression results when we repeat all the estimates of columns 1-3 excluding demographic controls.

Results document a positive effect of the trustees' dummy as well as a positive effect of violence (DIPVFR) in the whole sample; however and more importantly this effect is reversed for victimised trustees highlighting a negative effect of victimization on trustworthiness learning. The coefficient of the placebo treatment indicator is negative but not significant at 5% and the first TG contribution level is negative too indicating a sort of convergence effect (Table 6a, column 1).

When in columns 2-3 we restrict our focus to the sample of CPRG treated we introduce factors measuring what happens in the CPRG games (ethnic and gender fragmentation, player's and mean group's withdrawal ratios, player's CPRG payoffs). The victimisation-trustworthiness effect (i.e.

[27] A richer specification is generally preferred to the one with a reduced number of regressors since the problem of omitted variable bias is generally considered more serious than that of adding irrelevant variables. We therefore control in the estimate for all the variables recorded in our survey which a priori may be conceived as being potentially relevant. We further control with the Variance Inflation Factor (Marquardt, 1970) that multicollinearity problems do not affect the magnitude of the estimated coefficients (the VIF is below 5). As it is well known the VIF (variance inflation factor) is equal to 1/1-R(x) with R(x) being the R squared of an estimate in which one of the independent variable is regressed on all other independent variables. When R(x) is low (tends to zero) the VIF test is low (equal to one). A VIF value below 10 (or, more restrictively, five) is considered acceptable by rules of thumb generally adopted in the literature.

the variable *Trustee*DIPVFR*) remains strongly significant together with the convergence effect and a negative and significant impact of gender fragmentation (Table 6a, columns 2-3)[28].

Finally, all these findings are robust to the exclusion of socio-demographic controls (Table 6a, columns 4-6). Consistently with our hypotheses and the non-parametric tests from the previous section, the main result from this preliminary regression analysis is that victimization (DIPVFR) implies a significant decrease in trustworthiness relative to its initial levels.

Since trustees' contributions captured by TG1 or TG2 variables in the previous regression are actually an average of the ten conditional trustees' responses in the strategy method we concentrate on them now and further restrict the focus to the sample of trustees participating to the CPRG. The specification we test is therefore:

$$[\Delta TG_i|Tr(x)_j]=\alpha_0 + \alpha_1 DIPVFR_i + \alpha_2 EL_i + \sum_j \beta_j X_{ji} + \varepsilon_i \qquad (1b)$$

where the dependent variable $[\Delta TG_i|Tr(x)_j] = [TG(2)_i|Tr(x)_j] - [TG(1)_i|Tr(x)_j]$ is the difference in trustee's contribution between second and first TG game conditional to a given trustor contribution (x=5,10,..,50). Remember that, given the adoption of the strategy method, trustees cannot observe neither first nor second round trustors behavior in the TG before their second TG choice. Their only experience of the consequences of the behavior of the other players in the experiment is therefore the CPRG game if they are not in the placebo treatment.

Estimates findings show that the victimization effect (variable DIPVFR) is not significant at the extremes and tends to be high in the middle upper part of potential trustor contributions (Table 6b). In terms of magnitude the largest significant effect is that conditional to a contribution of 40 shillings from the trustors for which trustees belonging to the DIPVFR group give around 10 KSh less in the TG2 relative to the TG1 (Table 6b, column 8). Since average trustee's contribution in the first TG round for CPRG treated is equal to 32.89 this implies a maximum reduction in

[28] The negative and significant coefficient of gender fragmentation highlights the negative impact of gender heterogeneity in PGG-groups on the change in contribution in the second TG, with members of more gender-diversified groups reducing their ex-post trust/trustworthiness levels. This result confirms the gendered characteristic of the balanced reciprocity norm tested by Bohnet and Greig (2008). See table 1 for details on how the index is constructed.

trustworthiness which is close to 30 percent. [29] Note also that material or economic losses introduced as additional controls are not significant and placebo treated do not contribute significantly differently from CPRG treated when the effect of the violence experience is controlled for. [30]

6. Conditional/balanced reciprocity and robustness checks

In order to evaluate in the metric of balanced/conditional reciprocity the observed change in trustworthiness of DIPVFR participants subject to the CPRG treatment we follow Greig and Bohnet (2008) whose base model is specified as follows:

$$[Te_{(TG)}/Tr_{(TG)}]=\alpha_0 + \alpha_1 Tr_{(TG)} + \sum_j \beta_j X_{ji} + \varepsilon_i , \qquad (2)$$

where $Te_{(TG)}$ and $Tr_{(TG)}$ are respectively trustee and trustor contributions in the TG game, $\alpha_1=0$ implies *balanced reciprocity*, while $\alpha_1>0$ *conditional reciprocity*. In Table 7 we test separately the Greig and Bohnet (2008) model on the level of the trustee/trustor contribution ratio for (victimised and non victimised) CPRG participants in the two different rounds. In the first round the trustor contribution level is negative and significant and close in magnitude for victimised and non victimised indicating less than balanced reciprocity (Table 7, columns 1-2). In the second round we observe a stronger reduction of the level coefficient for non victimised *vis à vis* victimized (Table 7, columns 3-4). If we control for the first round return ratio (to reduce the effect of first round

[29] Note that an alternative interpretation of our findings on the effects of the violence experience on trustworthiness learning could be that trustees who experience violence are not affected by the PGG game but reveal their true identity only at the end of the game to avoid negative reputation effects. This end game behavior interpretation can however be hardly supported due to the structure of our experiment where also first TG round trustee contributions are revealed at the end of the game.

[30] We re-estimate eq. (1b) restricting the sample to PGG participants only and controlling for players' payoffs, ethnic and gender fragmentation as well as (individual and group) mean withdrawal ratios in the PGG (see Table 1 for details on these variables). Consistently with results in Table 6b, we find that the negative effect of DIPVFR on the change in trustworthiness is confirmed and remains high in the middle upper part of potential trustor contributions. Furthermore, we do not find any significant impact of ethnic or gender fragmentation. Eq. (1b) has also been estimated excluding demographic controls without substantial changes in the main findings. Finally, we also check whether the trustee behaviour is driven by what they expect that trustors expect from them (second order beliefs) regressing their contribution on the latter. We find that this is not the case. The results of all these checks are omitted for reason of space but available upon request.

heterogeneity) we clearly observe that non victimised improve in reciprocity, while the effect for victimised is not significant (Table 7, columns 5-6). [31]

Our results from Tables 6 and 7 do not change when we estimate the same models with a WLS regression where the weights are the inverse of the average propensity score for DIPVFR and Economic Losses (Table 8).[32]

Finally, using the propensity score matching estimator we evaluate in a final robustness check the Average Treatment Effect on the Treated (ATT) of victimization (DIPVFR) on the probability of observing a TE's contribution in the TG1 greater than in the TG2[33]. Our results are consistent with those from the previous sections since the probability that a trustee sends on average more in the TG1 than in the TG2 is around 20% higher if she belongs to the DIPVFR group (*ATT = 0.190, t = 2.422*). When restricting the computation of the ATT just to the placebo treatment we find that such probability declines and is no longer significant (*ATT = 0.118; t = 0.356*). Conversely, the ATT of victimization for the trustees who played the CPRG is 0.212 and remains significant. This result confirms our core finding, that is, individuals who witnessed the 2007 post-election violence tend to reciprocate less after they have face-to-face interacted in the CPRG group experience.

The validity of the matching estimator heavily relies on the assumption of conditional independence of potential outcomes and treatment assignment given observables. In other terms, conditioning on observed covariates, the treatment assignment is independent of potential outcomes (Conditional Independence Assumption, CIA). In order to assess whether and to what extent the estimated ATT is robust to possible deviations from the CIA we carry out the sensitivity analysis proposed by

[31] In order to run regressions with more degrees of freedom, models in Table 7 have been also re-estimated excluding demographic controls but we do not find any significant change in the main results. Furthermore, columns 3 and 4 in Table 7 have been re-estimated including also player and group mean withdrawal ratios. We find that the coefficients are very similar to the ones reported in columns 3 and 4. Results of all these checks are omitted for reason of space but are available upon request.

[32] Specifically, for each individual, the weights are computed as:

$$\left[\left(\frac{DIPVFR}{\widehat{pscore}_{(DIPVFR)}} + \frac{1-DIPVFR}{1-\widehat{pscore}_{(DIPVFR)}}\right) + \left(\frac{EL}{\widehat{pscore}_{(EL)}} + \frac{1-EL}{1-\widehat{pscore}_{(EL)}}\right)\right]/2,$$

where *pscore* is a non-parametric estimate of the propensity score. For details on this strategy see, among others, Blattman and Annan (2010) and Hirano, Imbens and Ridder (2003).

[33] We use the radius matching and control for all the demographic and game regressors used in Table 6b specifications.

Ichino, Mealli and Nannincini (2006)[34]. Despite we are quite convinced of our arguments at end of section 4, let us suppose the CIA is not satisfied in our study and tackle the problem by modelling an unobservable additional binary variable (confounder). In order to do that, we make assumptions on the effects of such a variable on our data and use it as an additional covariate in the matching regression. In such a way, we are able to assess to what extent our baseline ATT is robust to the exclusion of a potential confounder that might have different characteristics.

We start by defining the distribution of the confounder U on the basis of four choice-parameters:

$$p_{ij} = Pr(U = 1|T = i, Y = j, W) = Pr(U = 1|T = i, Y = j) \quad (3)$$

with $i, j = \{0, 1\}$, T and W being the treatment indicator and the observable set of covariates respectively. Equation (3) gives the probability that $U = 1$ in each of the four groups defined by the treatment status and the outcome value.

We can conceive our potential confounder as a socio-psychological trait that makes individuals more likely to be victims (T=1) and, at the same time, more likely to react less reciprocally after the interaction in a group project (Y=1). An individual specific trait of this kind can be for example having a scarce enterprising mood (hereon U=0 if the individual is not enterprising). For less enterprising individuals our baseline ATT is expected to be biased if we do not account for such an unobservable trait. Let us define our outcome variable as *PUNISH*, which is equal to one whenever the amount sent in the second trust game is less than the one sent in the first. Hence, one way to model the distribution of the confounder is by setting:

i) $p_{11} < p_{10}$, so that $Pr(PUNISH = 1| DIPVFR = 1, U = 1) < Pr(PUNISH = 1| DIPVFR = 1, U = 0)$
   - that is, among the victims, those who are less enterprising tend to reciprocate less in the TG2. In other words, enterprising ability reduce punishing behaviour in TG2;

ii) $p_{01} = p_{00}$, so that $Pr(PUNISH = 1| DIPVFR = 0, U = 1) = Pr(PUNISH = 1| DIPVFR = 0, U = 0)$
   - that is, among non-victims, those who are more enterprising reciprocate in the TG2 in the same way as those who are less enterprising;

[34] See also Blatmann and Annan (2010), Rosenbaum and Rubin (1983) and Imbens (2003).

iii) $p_{1.} < p_{0.}$ so that $Pr(DIPVFR = 1|U = 1) < Pr(DIPVFR = 1|U = 0)$, that is, individuals who are less enterprising are more likely to be victims. In other words, the confounder has a negative effect on treatment assignment.

Following Ichino et al. (2006), we define $d_1 = p_{11} - p_{10}$, $d_0 = p_{01} - p_{00}$ and $s = p_{1.} - p_{0.}$ in order to characterize the sign of the bias when estimating the baseline ATT (i.e. computed when U is not in the matching set). In our framework we look at cases in which $d_1 < 0$ and $d_0 = 0$ (*negative effect of U on treated outcome and no effect of U on the untreated one*) and $s < 0$ (*negative effect of U on selection*). In this way we identify the levels of d_1 and s that produce an estimated ATT substantially different from the baseline one and discuss to what extent the existence of a "killer" confounder with these characteristics is plausible.

We report in Table 9 the results from the sensitivity analysis. In panel (a), when $d_0 = 0$, the size of the bias may rise up to a maximum of 11%.

We now relax assumption ii) by reasonably assuming that, also among non victims, less enterprising individuals are more likely to reciprocate less in the TG2 (this implies a negative d_0). When d_0 = - 0.30, the maximum potential bias rises to 37%, to 35% when d_0 = -50 and to 27% when d_0 = -70 as reported in panel (b), (c) and (d) respectively.[35] Under these distributional hypotheses, we conclude that – in the worst case - by excluding the confounder we may overestimate the effect of victimization by a maximum of 37%.

We repeat the sensitivity analysis maintaining the above hypotheses on the outcome effect but assuming a positive selection effect (i.e. more enterprising individuals are more likely to be victimized, that is $s > 0$). Panels (e)-(h) show that by excluding the confounder from the baseline ATT we get a smaller ATT in most of the cases. In the worse scenario, our baseline ATT is 50% lower then the ATT with the confounder. Such a result does not worry us very much since i)

[35] Notice that, when computing the ATT with confounder at different levels of d_0 and in order to give boundaries to our sensitivity analysis, we exclude cases in which $|d_0| > |d_1|$. Such a condition would have required assuming the lack of enterprising mood to have a greater impact on punishing for non-victims than for victims. This assumption seems not reasonable since the violence experience is expected to exacerbate the punishing attitude of participants to the experiment.

excluding the confounder we are just underestimating the true ATT and ii) the largest bias is associated with a highly implausible outcome effect (odd ratio = 18.55) [36].

In addition, notice that the maximum size of the bias under both types of selection hypothesis is evaluated assuming very unlikely probability values for the distribution of the confounder. More realistic values are instead the ones reported in Table 9, where the distributional hypotheses of the confounder (and their correlation with selection into treatment and performance) are simulated from the distribution of the observed binary regressors used so far. With these more realistic probabilities the bias turns out to be very negligible (Table 10).

In conclusion, results from the sensitivity analysis support the robustness of our estimations in most of the cases and lead us to support the main idea of the paper also when the CIA is removed, that is victimization reduces reciprocity after the interaction in a group project.

7. Conclusions

The literature on the relationship between socioeconomic instability and growth identifies cross-country correlations which need further inquiry at micro level for a proper identification of causality links. Our paper contributes to this literature with a randomized experiment aimed at measuring the effect of political violence on learning in trusworthiness. The maintained assumption is that social capital is a crucial driver of institutional shaping and economic development and that the memory of violence suffered during sociopolitical conflicts crucially affects it.

We locate our experiment in the Kianda village of the Kibera slum (Nairobi) in which most inhabitants have been victimized in the 2007 political conflicts. To evaluate the impact on trustworthiness of cooperation and opportunism in public good game-like activities we devise a "sandwich" experiment in which a modified Public Good Game (Common Pool Resource game) is played between two Trust Game (TG) rounds. Trustees' contributions are elicited with the strategy

[36] To have a bias of about 50%, the lack of enterprising ability should increase the relative probability of reciprocating less in the TG2 by a factor greater than 18.

method to avoid the confounding effect of the first TG outcome on the impact of the CPRG experience on the change in trustworthiness between rounds.

As to the external consistency and the policy relevance of our experiment there are several reasons why our CPRG treatment mimics features which are important for the local socioeconomic environment. First, roads, water and sanitation are often provided in these slums by infrastructure community development projects in CPRG-like situations in which people from local community donate labor in order to build the public good (see the *harambee* tradition in Kenya described by Greig and Bohnet, 2009). Second, all individuals in Kenya experience widespread political corruption which may be viewed as an extreme of the opportunistic behaviour experienced by participants in the CPRG game. Last but not least, many informal finance institutions (self help groups, merry go round, microfinance institutions) which are widespread in the area require high levels of social capital in order to work properly.

The main finding of the paper is that the experience of direct or indirect physical violence and/or forced relocation significantly reduces trustworthiness learning. More specifically we find that the violence experience generates negative effects on trustworthiness learning for CPRG participants preventing their move toward balanced reciprocity. The economic relevance of the effect is not negligible since the maximum impact accounts for around 30 percent of the corresponding average trustees' contribution in the game. We document robustness of our findings based on the experiment design, ex post randomness checks and sensitivity analysis.

Our interpretation is that the memory of what suffered in the past significantly and negatively affects a component of social capital such as trustworthiness. In this sense our paper provides a microeconomic and psychological explanation for the observed vicious circles between sociopolitical instability and economic recovery documenting that the memory of the violence experience refreshed by CPRG events weakens reciprocity.

This has important consequences on business opportunities if, as it is commonly considered in the literature, trust game characteristics mimic standard features of socioeconomic life in which quality

of business relationship crucially depends on trust and trustworthiness in presence of asymmetric information and incomplete contracts. In this sense our experiment identifies a microeconomic rationale of the vicious circle among political violence, corruption and low production of social capital.

Note that what we find affects the redistributive (trustworthiness) and not the multiplicative (trust) part of the trust game. If however trustors incorporate in their expectations the violence effect on trustworthiness and are strategically altruist (are affected by their beliefs on trustees' contributions), their trustworthiness effect will impact also on the multiplicative side of the game in the future. A similar link between trustworthiness and trust is modelled and empirically tested by Guiso et al. (2008) through intergenerational transmissions of priors. We can therefore conclude that we do not find first order (direct effect of violence on changes in trust) but, potentially, second order effects (changes in trustworthiness that can likely impact on future changes in trust) on creation of economic value.

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Figure 1. The trust game

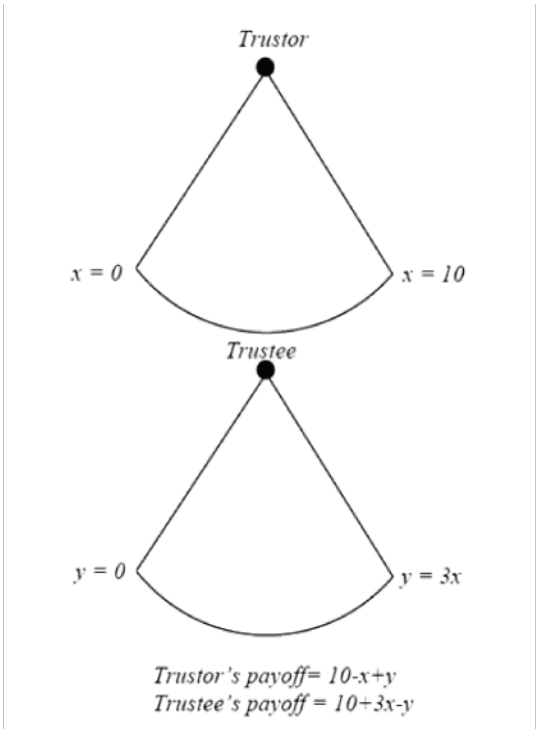

Figure 2. The experiment design

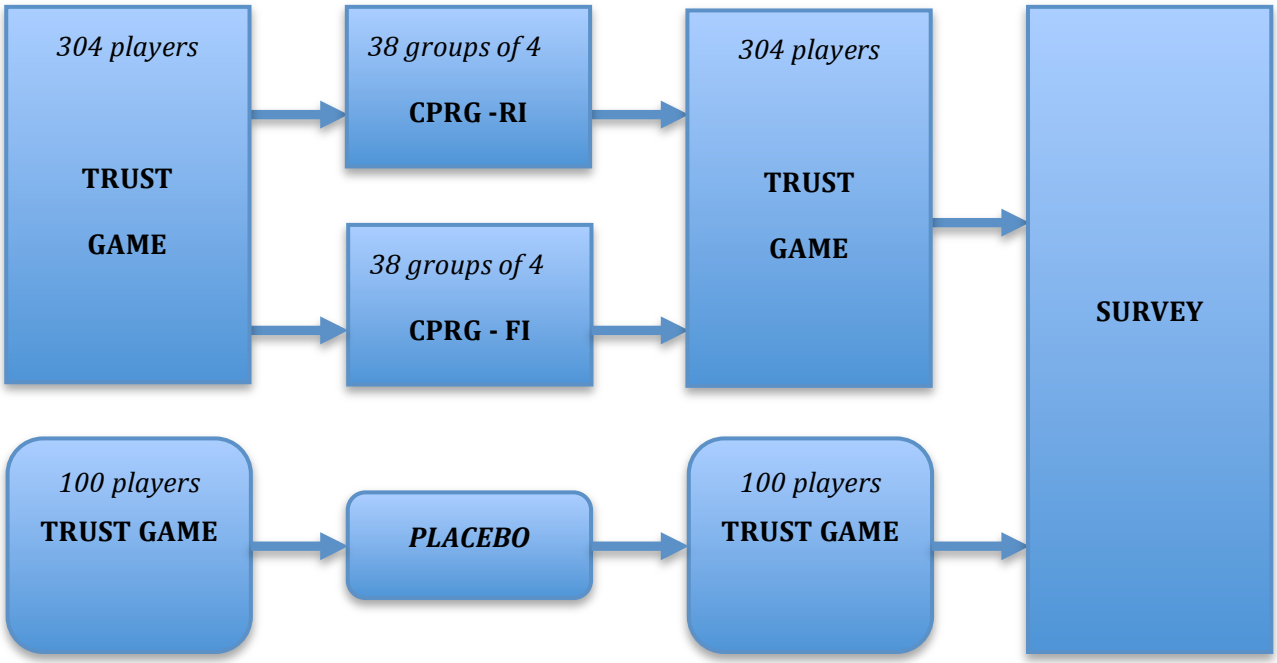

Table 1. Variable legend

ΔTG=TG2-TG1	change in contributions between the TGs	*playermeanwithdrawalratio*	Mean individual's withdrawal ratio over all CPRG rounds.
TG1	player's contribution in the I round of trust game	*groupmeanwithdrawalratio*	Mean group's withdrawal ratio over all CPRG rounds.
TG2	player's contribution in the II round of trust game	*relatives_death*	= 1 if the respondent has lost a relative after the 2007 post-election conflict.
withdrawalratio	= amount withdrew by the participant in the CPRG/maximum the individual can withdraw (150 KSh).	*personal_injury*	= 1 if the respondent has suffered a personal injury after the 2007 post-election conflict.
age	Respondent's age	*home_distruction*	= 1 if the respondent has witnessed the distruction of the house after the 2007 post-election conflict.
female	=1 if the respondent is female	*property_damaged*	= 1 if the respondent has witnessed damages to personal property after the 2007 post-election conflict.
married	=1 if the respondent is married	*job_loss*	= 1 if the respondent has lost the job after the 2007 post-election conflict.
widowed	=1 if the respondent is widowed	*business_distruction*	= 1 if the respondent has witnessed destruction or closure of a business the 2007 post-election conflict.
separated	=1 if the respondent is separted	*eviction*	= 1 if the respondent has suffered a personal injury after the 2007 post-election conflict.
n_house_members	n. of house components	*moved_in*	= 1 if the respondent moved with relatives or others in the area after the 2007 post-election conflict.
kikuyo	=1 if the respondent is from the ethnic group "Kikuyo"	*relocated_other_rural_area*	= 1 if the respondent was relocated to another rural area in Kenya after the 2007 post-election conflict.
luo	=1 if the respondent is from the ethnic group "Luo"	*relocated_other_part_of_town*	= 1 if the respondent was relocated to another part of town after the 2007 post-election conflict.
lubian	=1 if the respondent is from the ethnic group "Lubian"	*relocated_other_town_in_kenya*	= 1 if the respondent was relocated to another town in Kenya after the 2007 post-election conflict.
luhya	=1 if the respondent is from the ethnic group "Luhya"	*Direct/Indirect Physical Violence (DIPV)*	= 1 if relatives_death &/or personal_injury =1
muslim	=1 if the respondent is Muslim.	*economiclosses (EL)*	= 1 home_distruction=1 &/or property_damaged=1 &/or job_loss=1 &/or business_distruction =1 &/or eviction=1
years_schooling	Respondent's years of schooling	*Forced Relocation (FR)*	= 1 if moved_in =1 &/or relocated_other_rural_area =1 &/or relocated_other_partoftown =1 &/or relocated_other_town =1 &/or relocated_other_country =1
food_expenditure_day	daily food expenditure for the respondent's family.	*DIPVFR (Direct/Indirect Phisical Violence and/or Forced Relocation)*	= 1 if DIPV=1 and/or FR=1
unemployed	= 1 if the respondent is unemployed	*Non Involved*	= 1 if DIPVFR =0 & EL= 0
CPRG_FI	= 1 if the respondent participates in the CPRG full information treatment.	*CPRG_payoff*	Payoff of the randomly selected round of the CPRG for payment.
mfi_now	= 1 if the respondent is member of a microfinance	*ethnicfragmentation*	Ethnic fragmentation index in CPRG groups measures the likelihood that four randomly drawn members belong to different ethnic groups = 1-Σ(fraction of members belonging to each of the ethnic groups)^2. NB: if =0, fully ethnic-homogeneous group; if =1, fully ethnic-heterogeneous group.
volunteer	= 1 if the respondent volunteers more than once a month.	*genderfragmentation*	Gender fragmentation index in CPRG groups measures the likelihood that four randomly drawn members belong to different gender groups = *1-Σ(fraction of members belonging to each of the two gender groups)*^{2}. NB: if =0, fully gender-homogeneous group; if =0.50, fully gender-heterogeneous group.
riskaverse	= 1 if the respondent is risk averse (has chosen lotteries with the payoffs at closer distance - see questionnaire in the appendix)	*trustee*	= 1 if the respondent played as trustee in the TG.
betrayalaverse	= 1 if the respondent is betrayal averse ("strongly agrees" or "agrees" on two statements about revenge - see questionnaire in the appendix)	*n_friends*	n. of people known by name in the CPRG.
impatient	= 1 if the respondent is highly impatient (has chosen the lottery with payoffs at higher distance, i.e. need higher payoff in the future to be willing to wait - see questionnaire in the appendix)	*placebo*	= 1 if the respondent has not played the CPRG between the two TGs.

Table 2. Robustness check: violence groups and socio-demographic variables (trustees only)

Type of Test:		DIPVFR (1) vs. rest of the sample (0)			Economic Losses (1) vs. rest of the sample (0)			Non-Involved (1) vs. rest of the sample (0)			DIPVFR (1) vs. non-involved (0)			Economic Losses (1) vs. non-involved (0)		
Variable		*Obs.*	*Mean*	*z, p*	*Obs.*	*Mean*	*z, p*	*Obs.*	*Mean*	*z, p*	*Obs.*	*Mean*	*z, p*	*Obs.*	*Mean*	*z, p*
age	0	119	26.178	-3.065	54	24.172	-4.251	157	28.594	4.304	45	23.674	-4.333	45	23.674	-4.382
	1	83	29.389	0.002	148	28.711	0.000	45	23.674	0.000	83	29.389	0.000	148	28.711	0.000
female	0	119	0.563	0.798	54	0.574	0.592	157	0.516	-1.258	45	0.622	1.256	45	0.622	1.121
	1	83	0.506	0.425	148	0.527	0.554	45	0.622	0.208	83	0.506	0.209	148	0.527	0.262
married	0	119	0.269	-2.263	54	0.259	-1.317	157	0.357	1.406	45	0.244	-1.988	45	0.244	-1.415
	1	83	0.422	0.024	148	0.358	0.188	45	0.244	0.160	83	0.422	0.047	148	0.358	0.157
separated	0	119	0.050	-1.262	54	0.037	-1.088	157	0.083	1.407	45	0.022	-1.561	45	0.022	-1.376
	1	83	0.096	0.207	148	0.081	0.277	45	0.022	0.159	83	0.096	0.119	148	0.081	0.169
widowed	0	119	0.025	-1.253	54	0	-1.739	157	0.051	1.541	45	0	-1.673	45	0	-1.589
	1	83	0.060	0.210	148	0.054	0.082	45	0	0.123	83	0.060	0.094	148	0.054	0.112
n_house_members	0	119	4.504	0.083	54	4.148	-1.105	157	4.584	0.391	45	4.311	-0.234	45	4.311	-0.584
	1	83	4.550	0.934	148	4.660	0.269	45	4.311	0.696	83	4.550	0.815	148	4.660	0.559
kikuyo	0	119	0.076	-0.521	54	0.074	-0.311	157	0.083	-0.129	45	0.089	-0.138	45	0.089	0.022
	1	83	0.096	0.602	148	0.088	0.756	45	0.089	0.897	83	0.096	0.890	148	0.088	0.983
luo	0	119	0.462	1.423	54	0.444	0.410	157	0.420	-0.022	45	0.422	0.673	45	0.422	0.120
	1	83	0.361	0.155	148	0.412	0.682	45	0.422	0.982	83	0.361	0.501	148	0.412	0.905
lubian	0	119	0.134	0.821	54	0.148	0.776	157	0.108	-0.862	45	0.134	0.990	45	0.148	0.858
	1	83	0.096	0.412	148	0.108	0.437	45	0.156	0.389	83	0.096	0.322	148	0.108	0.391
luhya	0	119	0.210	0.514	54	0.204	0.122	157	0.204	0.386	45	0.178	-0.041	45	0.178	-0.270
	1	83	0.181	0.607	148	0.196	0.903	45	0.178	0.700	83	0.181	0.967	148	0.196	0.787
muslim	0	119	0.202	0.370	54	0.222	0.633	157	0.178	-0.988	45	0.244	0.852	45	0.244	0.914
	1	83	0.181	0.711	148	0.182	0.527	45	0.244	0.323	83	0.181	0.394	148	0.182	0.361
years_schooling	0	119	11.714	2.404	54	11.907	1.738	157	11.006	-1.979	45	12.089	2.428	45	12.089	1.948
	1	83	10.578	0.016	148	11.007	0.082	45	12.089	0.048	83	10.578	0.015	148	11.007	0.051
food_expenditure_day	0	119	259.237	1.849	54	257.547	1.140	157	248.631	-1.007	45	259.237	1.571	45	257.547	1.074
	1	83	238.494	0.064	148	248.209	0.254	45	257.955	0.314	83	238.494	0.116	148	248.209	0.283
unemployed	0	119	0.328	2.317	54	0.426	3.069	157	0.204	-3.800	45	0.489	3.658	45	0.489	3.668
	1	83	0.181	0.021	148	0.209	0.002	45	0.489	0.000	83	0.181	0.000	148	0.209	0.000
mfi_now	0	119	0.420	-2.207	54	0.352	-2.284	157	0.522	1.968	45	0.356	-2.397	45	0.356	-2.089
	1	83	0.578	0.027	148	0.534	0.022	45	0.356	0.049	83	0.578	0.017	148	0.534	0.037
volunteer	0	119	0.420	-0.021	54	0.370	-0.875	157	0.452	1.686	45	0.311	-1.224	45	0.311	-1.526
	1	83	0.422	0.983	148	0.439	0.382	45	0.311	0.092	83	0.422	0.221	148	0.439	0.127
riskaverse	0	119	0.513	0.260	54	0.481	-0.402	157	0.516	0.581	45	0.467	-0.294	45	0.467	-0.549
	1	83	0.494	0.795	148	0.514	0.688	45	0.467	0.561	83	0.494	0.769	148	0.514	0.583
betrayalaverse	0	119	0.176	0.370	54	0.130	-0.886	157	0.172	0.259	45	0.156	-0.016	45	0.156	-0.413
	1	83	0.157	0.711	148	0.182	0.376	45	0.156	0.796	83	0.157	0.987	148	0.182	0.679
impatient	0	119	0.487	1.089	54	0.407	-0.826	157	0.459	0.168	45	0.444	0.379	45	0.444	-0.335
	1	83	0.410	0.276	148	0.473	0.409	45	0.444	0.867	83	0.410	0.705	148	0.473	0.738

DIPVFR: direct or indirect physical violence and/or forced relocation

Table 3. Randomness check: CPRG characteristics & socio-demographic variables (trustees only)

CPRG (0) vs. placebo (1)					
Variable	*Group*	*Obs.*	*Mean*	*Std. Dev.*	*z, p*
Age	1	50	25.4124	5.6377	-1.7571
	0	152	28.1838	8.1737	0.0789
Female	1	50	0.5000	0.5051	-0.6461
	0	152	0.5526	0.4989	0.5182
Married	1	50	0.3200	0.4712	-0.2018
	0	152	0.3355	0.4737	0.8401
Separated	1	50	0.0400	0.1979	-0.9383
	0	152	0.0789	0.2705	0.3481
Widowed	1	50	0.0600	0.2399	0.8504
	0	152	0.0329	0.1790	0.3951
Kikuyo	1	50	0.0800	0.2740	-0.1218
	0	152	0.0855	0.2806	0.9031
Luo	1	50	0.4200	0.4986	-0.0130
	0	152	0.4211	0.4954	0.9896
Lubian	1	50	0.1400	0.3505	0.5325
	0	152	0.1118	0.3162	0.5944
Luhya	1	50	0.2400	0.4314	0.8566
	0	152	0.1842	0.3889	0.3917
muslim	1	50	0.1800	0.3881	-0.2692
	0	152	0.1974	0.3993	0.7877
years_schooling	1	50	11.9000	3.0656	2.0583
	0	152	11.0329	2.9553	0.0396
food_expenditure_day	1	50	254.9000	150.4296	0.0745
	0	151	249.2715	127.2693	0.9406
mfi_now	1	50	0.5800	0.4986	1.5432
	0	152	0.4539	0.4995	0.1228
volunteer	1	50	0.3400	0.4785	-1.3307
	0	152	0.4474	0.4989	0.1833
DIPVFR	1	50	0.4200	0.4986	0.1505
	0	152	0.4079	0.4931	0.8803
Economic losses	1	50	0.6800	0.4712	-0.9678
	0	152	0.7500	0.4344	0.3332
Non involved	1	50	0.2039	0.4535	-1.118
	0	152	0.28	0.4042	0.2634

Fig.3 Average cooperation dynamics in the CPRG

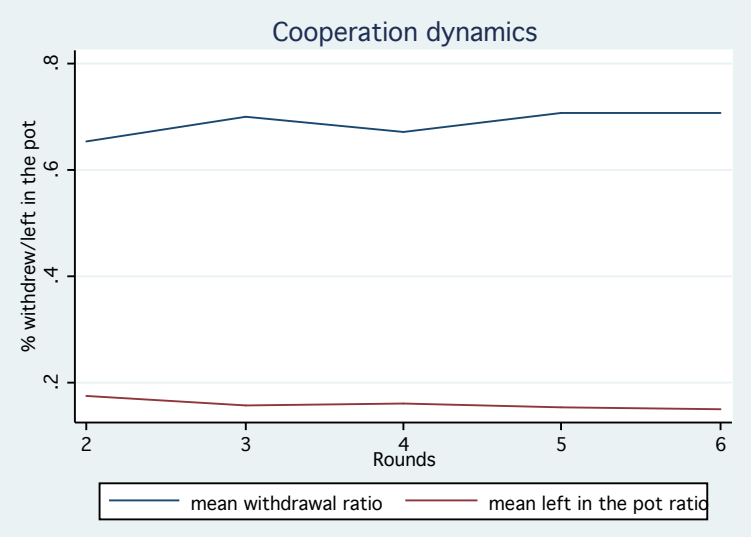

Table 4. Violence experience and CPRG behaviour.

TE's mean withdrawal ratio in the CPRG			
Physical violence & forced relocation vs. rest of the sample	Param.	*means:*	0.72 vs .71
		Pr [diff != 0]	0.848
	Non Param.	*z:*	0.068
		p:	0.946
Economic Losses vs. rest of the sample	Param.	*means:*	.73 vs .66
		Pr [diff != 0]	0.178
	Non Param.	*z:*	-1.13
		p:	0.258
Non-involved vs. rest of the sample	Param.	*means:*	.644 vs .734
		Pr [diff != 0]	0.075
	Non Param.	*z:*	1.286
		p:	0.198
Physical violence & forced relocation vs. non-involved	Param.	*means:*	.72 vs .64
		Pr [diff != 0]	0.184
	Non Param.	*z:*	-0.893
		p:	0.371
Economic Losses vs. non-involved	Param.	*means:*	.73 vs .64
		Pr [diff != 0]	0.089
	Non Param.	*z:*	-1.276
		p:	0.201

Table 5.1 Non parametric tests on the relationship among violence, CPRG experience and trustworthiness changes for trustees.

Tested Var: *$ TG2 - $ TG1*		**PLACEBO**		**CPRG**	
		0	1	0	1
DIPVFR (1) vs. rest of the sample (0)	*Obs.*	29	21	90	62
	Mean	-0.041	-2.962	0.860	-5.120
	z, p	1.124	0.261	2.618	0.009
Economic Losses (1) vs. rest of the sample (0)	*Obs.*	16	34	38	114
	Mean	-2.569	-0.656	2.047	-2.788
	z, p	-0.303	0.762	0.962	0.336
Non-Involved (1) vs. rest of the sample (0)	*Obs.*	36	14	121	31
	Mean	-0.661	-2.829	-2.888	3.529
	z, p	0.173	0.862	-1.498	0.134
DIPVFR (1) vs. rest of the sample (0)	*Obs.*	14	21	31	62
	Mean	-2.829	-2.962	3.529	-5.120
	z, p	0.388	0.698	2.340	0.019
Economic Losses (1) vs. non-involved (0)	*Obs.*	14	34	31	114
	Mean	-2.829	-0.656	3.529	-2.788
	z, p	-0.216	0.829	1.413	0.158

Table 5.2 Parametric/non parametric tests on the relationship among violence, CPRG experience and trustworthiness changes

		PLACEBO									CPRG								
		DIPVFR (1) vs. rest of the sample (0)			Non-involved (1) vs. rest of the sample (0)			DIPVFR (1) vs. non-involved (0)			DIPVFR (1) vs. rest of the sample (0)			Non-involved (1) vs. rest of the sample (0)			DIPVFR (1) vs. non-involved (0)		
Tr Send		*Obs*	*Mean*	*z, p*	*Obs*	*Mean*	*z, p*	*Obs*	*Mean*	*z, p*	*Obs*	*Mean*	*z, p*	*Obs*	*Mean*	*z, p*	*Obs*	*Mean*	*z, p*
5	*0*	29	-0.086	0.121	36	0.264	1.400	14	-0.786	-0.887	90	-2.533	-0.106	121	-2.607	0.665	31	-2.032	-0.473
	1	21	0.048	0.903	14	-0.786	0.161	21	0.048	0.375	62	-2.427	0.916	31	-2.032	0.506	62	-2.427	0.636
10	*0*	29	-1.103	-1.888	36	-0.278	0.944	14	-0.857	-1.526	90	-0.878	0.827	121	-2.215	-0.650	31	-0.452	0.853
	1	21	0.476	0.059	14	-0.857	0.345	21	0.476	0.127	62	-3.274	0.408	31	-0.452	0.516	62	-3.274	0.394
15	*0*	29	-0.655	-0.461	36	0.014	0.767	14	-1.429	-0.773	90	-0.156	0.298	121	-1.628	-1.048	31	0.806	0.914
	1	21	-0.024	0.644	14	-1.429	0.443	21	-0.024	0.440	62	-2.548	0.766	31	0.806	0.295	62	-2.548	0.361
20	*0*	29	0	1.494	36	-1.028	0.437	14	-2.143	0.526	90	1.061	2.132	121	-1.302	-0.215	31	1.065	1.165
	1	21	-3.190	0.135	14	-2.143	0.662	21	-3.190	0.599	62	-3.548	0.033	31	1.065	0.830	62	-3.548	0.244
25	*0*	29	1.121	1.761	36	-0.278	0.238	14	-1.786	0.686	90	1.006	1.782	121	-2.599	-2.074	31	5	2.301
	1	21	-3.214	0.078	14	-1.786	0.812	21	-3.214	0.492	62	-4.032	0.075	31	5	0.038	62	-4.032	0.021
30	*0*	29	1.103	1.069	36	0.417	0.828	14	-3.071	-0.052	90	1.233	2.152	121	-3.372	-0.868	31	3.742	1.653
	1	21	-2.857	0.285	14	-3.071	0.408	21	-2.857	0.958	62	-6.500	0.031	31	3.742	0.385	62	-6.500	0.098
35	*0*	29	0.983	1.456	36	-0.333	0.662	14	-5.786	0.310	90	0.044	2.061	121	-4.607	-1.026	31	3.226	1.703
	1	21	-5.786	0.145	14	-5.786	0.508	21	-5.786	0.757	62	-7.444	0.039	31	3.226	0.305	62	-7.444	0.089
40	*0*	29	-0.483	0.623	36	-1.806	0.077	14	-4.571	0.273	90	3.967	3.942	121	-3.521	-1.956	31	6.742	3.257
	1	21	-5.476	0.533	14	-4.571	0.938	21	-5.476	0.785	62	-9.258	0.000	31	6.742	0.050	62	-9.258	0.001
45	*0*	29	-3.017	0.178	36	-5.111	-0.207	14	-3.929	0.187	90	3.356	1.843	121	-3.202	-1.811	31	9.290	2.088
	1	21	-7.214	0.858	14	-3.929	0.836	21	-7.214	0.852	62	-6.476	0.065	31	9.290	0.070	62	-6.476	0.037
50	*0*	29	1.724	-0.110	36	1.528	0.585	14	-3.929	-0.505	90	1.500	1.248	121	-3.826	-1.237	31	7.903	1.456
	1	21	-2.381	0.912	14	-3.929	0.558	21	-2.381	0.613	62	-5.694	0.212	31	7.903	0.216	62	-5.694	0.145

The table reports average changes in trustees' contribution between second and first TG game conditional to trustor contribution of x indicated in row (x=5,10,…,,50)

Table 6a. The determinants of changes in contribution in the TG-CPRG-TG experiment (*Whole sample*)

Dep. Var: Δ TG=TG2-TG1	(1)	(2)	(3)	(4)	(5)	(6)
DIPVFR	2.637**	2.372	2.387	2.280*	1.943	1.927
	(1.282)	(1.620)	(1.622)	(1.234)	(1.538)	(1.528)
economiclosses	-1.182	-2.280	-2.241	-0.870	-1.987	-1.920
	(1.200)	(1.429)	(1.427)	(1.113)	(1.363)	(1.361)
TG1	-0.430***	-0.432***	-0.434***	-0.423***	-0.428***	-0.430***
	(0.0671)	(0.0674)	(0.0672)	(0.0675)	(0.0714)	(0.0719)
trustee	7.039***	7.436***	7.501***	6.753***	7.409***	7.433***
	(1.483)	(1.905)	(1.805)	(1.441)	(1.816)	(1.734)
trustee*DIPVFR	-6.160***	-6.993***	-7.207***	-5.669***	-6.107**	-6.265***
	(1.905)	(2.321)	(2.312)	(1.960)	(2.424)	(2.397)
placebo	-2.040*			-1.853*		
	(1.056)			(0.948)		
CPRG_NA		0.460	0.797		-0.363	0.0469
		(1.275)	(1.152)		(1.308)	(1.214)
n_friends		-1.368*	-1.389*		-1.133	-1.178
		(0.784)	(0.756)		(0.816)	(0.791)
ethnicfragmentation		5.424	5.522		4.508	4.837
		(4.682)	(4.605)		(3.724)	(3.645)
genderfragmentation		-13.80***	-14.49***		-12.14***	-12.82***
		(4.614)	(4.587)		(4.471)	(4.388)
playermeanwithdrawalratio		1.036			-0.415	
		(3.980)			(3.888)	
groupmeanwithdrawalratio		-0.255			2.443	
		(4.764)			(4.844)	
CPRG_payoff			0.0171			0.0140
			(0.0123)			(0.0134)
SOCIO-DEMOGRAPHIC CONTROLS	*Yes*	*Yes*	*Yes*	*No*	*No*	*No*
Observations	401	301	301	403	303	303
R-squared	0.338	0.401	0.405	0.306	0.341	0.343

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Socio-demographic controls include respondent's *age, gender, civil status, n. of house members, ethnic group, years of schooling, daily food expenditures, employment status, participation to microfinance groups, social preferences (volunteering, betrayal aversion), risk aversion and discount rates*.
Regression coefficients of these variables are omitted for reasons of space but available upon request.

Table 6b. The determinants of changes in trustworthiness in the TG-CPRG-TG experiment (*trustees' conditional responses for each of the possible trustor contributions*)

DEP VAR: ΔTG =TG2-TG1	(1) TR send 5	(2) TR send 10	(3) TR Send 15	(4) TR send 20	(5) TR send 25	(6) TR send 30	(7) TR send 35	(8) TR send 40	(9) TR send 45	(10) TR send 50
age	0.0562	-0.0952	-0.169	-0.0436	0.0700	-0.0250	0.157	0.0427	0.00310	0.185
	(0.0849)	(0.105)	(0.118)	(0.165)	(0.198)	(0.245)	(0.355)	(0.402)	(0.412)	(0.482)
female	0.366	-0.0712	0.226	-0.00295	1.214	0.00836	1.019	4.581	2.446	1.387
	(1.268)	(1.158)	(1.495)	(1.874)	(1.904)	(2.688)	(3.411)	(3.568)	(3.813)	(4.757)
married	0.145	1.814	-0.289	-0.519	0.0633	0.559	0.902	2.056	2.749	2.922
	(1.131)	(1.365)	(1.391)	(1.813)	(2.030)	(2.480)	(3.825)	(3.967)	(4.217)	(5.240)
separated	0.445	-3.311	-1.997	-3.413	0.266	0.813	10.74	10.50	7.853	9.617
	(1.401)	(3.137)	(2.764)	(3.236)	(4.442)	(5.627)	(7.952)	(7.914)	(8.661)	(11.47)
widowed	0.768	5.388*	2.663	1.710	3.777	1.987	8.237	2.102	9.617	12.29
	(1.990)	(2.834)	(3.051)	(5.007)	(6.297)	(7.642)	(10.91)	(12.88)	(13.67)	(15.02)
n_house_members	-0.515	-0.0344	0.266	0.169	-0.308	-0.0338	0.133	-0.108	-0.214	0.113
	(0.321)	(0.305)	(0.384)	(0.491)	(0.477)	(0.720)	(0.745)	(0.710)	(0.819)	(0.995)
luo	-1.642	-0.345	-0.242	-0.0235	-1.425	1.537	2.291	2.531	3.721	0.754
	(1.364)	(1.835)	(1.952)	(2.363)	(2.537)	(3.358)	(4.029)	(4.140)	(4.762)	(5.920)
lubian	-5.007**	-2.349	-3.634	-4.081	-8.401*	-9.396*	-7.640	-1.415	-6.336	-12.36
	(2.528)	(2.801)	(3.176)	(3.425)	(4.787)	(5.108)	(10.15)	(9.677)	(9.356)	(12.15)
luhya	0.169	-1.526	-2.936*	-2.391	-0.938	-1.882	0.0519	3.888	6.259	1.869
	(1.160)	(1.770)	(1.708)	(2.022)	(2.604)	(2.989)	(4.658)	(4.893)	(5.575)	(6.707)
muslim	-0.327	2.577	2.215	2.334	3.821	6.495	4.960	1.109	10.62	11.27
	(1.518)	(2.892)	(3.259)	(2.936)	(3.762)	(4.161)	(8.849)	(8.776)	(8.729)	(11.70)
years_schooling	-0.0622	-0.0828	-0.471*	-0.495	-0.338	-0.515	-0.00632	0.302	0.223	0.465
	(0.158)	(0.259)	(0.272)	(0.390)	(0.452)	(0.592)	(0.747)	(0.806)	(0.884)	(1.103)
food_expenditure_day	0.00281	-0.00241	0.00269	0.00887	0.00666	0.00318	0.0204	0.00999	0.00466	-0.00422
	(0.00354	(0.00474)	(0.00476)	(0.00669)	(0.00838)	(0.0105)	(0.0148)	(0.0153)	(0.0174)	(0.0194)
unemployed	1.399	0.465	-0.282	0.831	3.954*	0.622	2.676	5.511	1.325	1.856
	(1.413)	(1.529)	(1.770)	(2.115)	(2.334)	(3.021)	(4.155)	(4.462)	(4.779)	(5.563)
mfi_now	1.144	2.242	3.138*	1.383	1.188	1.113	0.0513	-2.891	-6.425*	-6.815
	(1.341)	(1.578)	(1.752)	(2.013)	(1.768)	(2.642)	(3.414)	(3.323)	(3.844)	(4.705)
volunteer	0.495	-0.622	0.503	1.912	2.673	2.580	6.283*	5.429	5.230	6.025
	(0.943)	(1.236)	(1.342)	(1.866)	(2.167)	(2.858)	(3.391)	(3.645)	(3.838)	(5.061)
riskaverse	-1.409	-3.682***	-0.862	-0.0770	-1.436	-1.036	-1.089	-2.385	-8.599*	-9.330*
	(1.132)	(1.347)	(1.525)	(1.991)	(2.252)	(3.067)	(3.980)	(4.251)	(4.416)	(5.506)
betrayalaverse	-1.541	0.179	-2.397	-3.865	-3.785	-8.247*	-4.637	-5.440	-5.311	-5.494
	(1.614)	(1.573)	(2.113)	(2.853)	(3.375)	(4.392)	(5.589)	(6.112)	(6.972)	(7.936)
impatient	-0.00567	1.436	0.486	0.0455	0.384	-0.00220	1.520	0.127	-3.682	-5.146
	(1.001)	(1.293)	(1.385)	(1.717)	(1.957)	(2.535)	(3.286)	(3.397)	(3.701)	(4.536)
DIPVFR	-0.466	-1.898	-2.097	-5.02***	-5.025**	-8.01***	-7.307*	-10.16**	-6.636	-7.211
	(1.131)	(1.404)	(1.370)	(1.731)	(2.161)	(2.635)	(4.084)	(4.114)	(4.184)	(5.370)
economiclosses	-0.266	0.0386	-0.240	1.153	-2.016	-0.351	-2.667	-0.0938	-4.949	-2.296
	(1.047)	(1.306)	(1.544)	(1.953)	(2.313)	(3.032)	(4.126)	(4.152)	(4.663)	(6.111)
placebo	1.479	-0.389	1.508	0.488	-0.0601	2.119	2.020	-2.328	-6.930	-2.670
	(1.248)	(1.534)	(1.595)	(2.000)	(2.417)	(3.013)	(4.381)	(4.807)	(5.059)	(6.144)
CPRG_NA	-0.199	0.114	1.192	-0.0683	-0.211	-1.799	-1.267	-2.116	-0.501	-4.573
	(1.391)	(1.646)	(1.744)	(2.216)	(2.429)	(3.264)	(4.427)	(4.268)	(4.623)	(5.668)
Observations	201	201	201	201	201	201	201	201	201	201
R-squared	0.120	0.105	0.085	0.089	0.097	0.091	0.103	0.121	0.119	0.079

*Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1*

Table 7. Violence experience and conditional/balanced reciprocity

OLS	(1) *Tg1 (CPRG - DPVFR)*	(2) *Tg1 (CPRG - NO DPVFR)*	(3) *Tg2 (CPRG - DPVFR)*	(4) *Tg2 (CPRG - NO DPVFR)*	(5) *Tg2 (CPRG - DPVFR)*	(6) *Tg2 (CPRG - NO DPVFR)*
age	0.00221	-0.0329*	-0.0121	-0.0215	-0.0148*	-0.0147
	(0.0113)	(0.0175)	(0.00850)	(0.0148)	(0.00813)	(0.0107)
female	0.311	-0.0324	0.130	0.0603	0.0829	0.00411
	(0.203)	(0.166)	(0.156)	(0.144)	(0.138)	(0.114)
married	-0.312*	0.324	0.0334	0.241	0.108	0.169
	(0.175)	(0.200)	(0.155)	(0.196)	(0.137)	(0.155)
separated	-0.593**	0.403	0.138	0.129	0.256	0.0732
	(0.243)	(0.495)	(0.242)	(0.310)	(0.226)	(0.232)
widowed	-0.914**	0.488	0.300	0.357	0.576**	0.320
	(0.351)	(0.377)	(0.237)	(0.319)	(0.240)	(0.287)
n_house_members	0.0576	-0.00818	-0.00162	-0.0246	0.00680	-0.0310
	(0.0349)	(0.0357)	(0.0235)	(0.0335)	(0.0209)	(0.0316)
luo	-0.127	0.460**	-0.245	0.241	-0.174	0.0809
	(0.172)	(0.182)	(0.159)	(0.171)	(0.153)	(0.159)
lubian	0.322	0.170	-0.607***	0.0580	-0.667***	0.0344
	(0.404)	(0.434)	(0.211)	(0.381)	(0.173)	(0.333)
luhya	-0.158	0.244	0.0379	0.0118	0.116	-0.0728
	(0.283)	(0.222)	(0.246)	(0.208)	(0.199)	(0.153)
muslim	-0.281	0.0937	0.380**	-0.0917	0.419***	-0.135
	(0.236)	(0.413)	(0.173)	(0.352)	(0.153)	(0.311)
years_schooling	-0.0103	0.0207	0.00120	-0.00849	0.00140	-0.00488
	(0.0189)	(0.0361)	(0.0152)	(0.0363)	(0.0147)	(0.0290)
food_expenditure_day	0.00148*	7.72e-05	0.00103*	0.000458	0.000847*	0.000336
	(0.000768)	(0.000589)	(0.000585)	(0.000559)	(0.000452)	(0.000475)
unemployed	-0.458*	0.191	0.356*	0.125	0.408**	0.0617
	(0.254)	(0.146)	(0.201)	(0.145)	(0.173)	(0.128)
mfi_now	-0.0902	0.121	0.00817	0.216	0.00417	0.202
	(0.177)	(0.160)	(0.146)	(0.165)	(0.125)	(0.154)
volunteer	0.296**	-0.337*	0.143	-0.138	0.116	-0.122
	(0.133)	(0.177)	(0.129)	(0.173)	(0.108)	(0.141)
riskaverse	0.292*	-0.145	0.0743	-0.238	-0.00168	-0.221
	(0.165)	(0.190)	(0.135)	(0.172)	(0.112)	(0.136)
betrayalaverse	0.715***	-0.172	-0.165	-0.290	-0.272*	-0.335*
	(0.250)	(0.273)	(0.131)	(0.217)	(0.143)	(0.193)
impatient	0.0284	0.214	0.0163	0.165	-0.00164	0.132
	(0.139)	(0.137)	(0.115)	(0.137)	(0.0974)	(0.123)
economiclosses	-0.227	0.335*	-0.205	0.129	-0.169	0.0576
	(0.209)	(0.201)	(0.166)	(0.202)	(0.139)	(0.164)
CPRG_NA			-0.0275	0.0125	-0.0593	0.144
			(0.113)	(0.139)	(0.0977)	(0.124)
ethnicfragmentation					0.533	-0.373
					(0.379)	(0.276)
genderfragmentation					-0.148	0.230
					(0.283)	(0.321)
playermeanwithdrawalratio					-0.414	0.0543
					(0.265)	(0.342)
groupmeanwithdrawalratio					0.0593	-0.565
					(0.356)	(0.581)
TR Send	-0.0413***	-0.0550***	-0.0241***	-0.0185**	0.0152	0.0491**
	(0.0119)	(0.0126)	(0.00855)	(0.00709)	(0.0166)	(0.0191)
TR Send^2	0.000618***	0.000803***	0.000398***	0.000278***	-0.000122	-0.000615**
	(0.000169)	(0.000187)	(0.000131)	(9.71e-05)	(0.000231)	(0.000263)
TG1returnratio					0.0462***	0.0850***
					(0.0162)	(0.0240)
Observations	620	890	620	890	620	890
N. Clusters	62	89	62	89	62	89
R-squared	0.238	0.166	0.202	0.103	0.287	0.221

*Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1. For model specification see eq. (2) section 6.*

Table 8. Robustness checks

Panel A - The determinants of changes in trustworthiness (changes in *TE's conditional responses for each possible trustor contribution*) - WLS estimates

WLS. DEP VAR: ΔTG TG2-TG1	(1) TR send 5	(2) TR send 10	(3) TR Send 15	(4) TR send 20	(5) TR send 25	(6) TR send 30	(7) TR send 35	(8) TR send 40	(9) TR send 45	(10) TR send 50
DIPVFR	-0.795	-1.784	-2.340*	-5.297***	-5.923***	-8.691***	-7.230*	-10.46***	-7.363*	-8.141
	(1.021)	(1.355)	(1.262)	(1.637)	(2.102)	(2.630)	(3.939)	(3.883)	(3.925)	(5.412)
economiclosses	-0.274	-0.182	0.191	1.731	0.453	1.067	-1.168	2.095	-1.107	1.720
	(0.889)	(1.193)	(1.279)	(1.595)	(1.868)	(2.450)	(3.475)	(3.454)	(3.845)	(5.501)
Observations	201	201	201	201	201	201	201	201	201	201
R-squared	0.134	0.127	0.144	0.181	0.198	0.189	0.171	0.251	0.281	0.263

*Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Models estimated are the same as in Table 6. Results on regression controls are omitted and available upon request.*

Panel B. Violence experience and conditional/balanced reciprocity (WLS estimates)

WLS *dep var:*	(1) TG1 (CPRG, DPVFR)	(2) TG1 (CPRG, NO DPVFR)	(3) TG2 (CPRG, DPVFR)	(4) TG2 (CPRG, NO DPVFR)	(5) TG2 (CPRG, DPVFR)	(6) TG2 (CPRG, NO DPVFR)
TR Send	-0.0321***	-0.0535***	-0.0225***	-0.0183**	0.0150	0.0491**
	(0.0118)	(0.0128)	(0.00716)	(0.00728)	(0.0138)	(0.0195)
TR Send^2	0.000501***	0.000780***	0.000389***	0.000274***	-0.000107	-0.000616**
	(0.000176)	(0.000190)	(0.000127)	(9.89e-05)	(0.000193)	(0.000268)
TG1 returnratio					0.0407***	0.0841***
					(0.0138)	(0.0241)
Obs.	620	890	620	890	620	890
Clusters	62	89	62	89	62	89
R^2	0.285	0.168	0.16875	0.109	0.326	0.220

Models estimated are the same as in Table 7. Coefficients of the rest of controls are omitted and available upon request. WLS have been built by separately calculating the score and the weight for the DIPVFR and economic losses variables separately and then averaging the two weights for each individual following the approach of Blattman and Annan (2010) and Hirano, Imbens and Ridder (2003) - see footnote 31.

Table 9. Sensitivity Analysis: *Identifing "Killer" Confounders.*

Assumptions	p_{11}	p_{10}	p_{01}	p_{00}	$p_{1.}$	$p_{0.}$	s	d_0	d_1	Bias %	ATE	Selection effect (Odds)	Outcome Effect (Odds)	WSE	BSE
	0.10	0.70	0.70	0.70	0.35	0.70	-0.35	0.00	-0.60	0.065	0.198	0.181	1.353	0.089	0.025
	0.10	0.60	0.60	0.60	0.31	0.60	-0.29	0.00	-0.50	0.077	0.195	0.264	1.293	0.089	0.028
	0.10	0.50	0.50	0.50	0.27	0.50	-0.23	0.00	-0.40	0.069	0.197	0.342	1.267	0.089	0.023
	0.10	0.40	0.40	0.40	0.23	0.40	-0.17	0.00	-0.30	0.061	0.199	0.413	1.253	0.089	0.018
	0.10	0.30	0.30	0.30	0.18	0.30	-0.12	0.00	-0.20	0.043	0.203	0.516	1.323	0.088	0.018
	0.10	0.20	0.20	0.20	0.14	0.20	-0.06	0.00	-0.10	0.038	0.204	0.752	1.657	0.088	0.014
a) s<0 ; d0=0	0.10	0.80	0.80	0.80	0.39	0.80	-0.41	0.00	-0.70	0.113	0.188	0.121	1.630	0.090	0.032
	0.10	0.70	0.80	0.80	0.35	0.80	-0.45	0.00	-0.60	0.073	0.196	0.100	1.419	0.090	0.031
	0.10	0.60	0.80	0.80	0.31	0.80	-0.49	0.00	-0.50	0.070	0.197	0.082	1.475	0.090	0.033
	0.10	0.50	0.80	0.80	0.27	0.80	-0.53	0.00	-0.40	0.033	0.205	0.055	1.540	0.093	0.035
	0.10	0.40	0.80	0.80	0.23	0.80	-0.57	0.00	-0.30	0.027	0.206	0.045	1.545	0.094	0.041
	0.10	0.30	0.80	0.80	0.18	0.80	-0.62	0.00	-0.20	0.032	0.205	0.032	1.672	0.099	0.062
	0.10	0.20	0.80	0.80	0.14	0.80	-0.66	0.00	-0.10	0.007	0.210	0.020	1.829	0.108	0.076
	0.10	0.10	0.80	0.80	0.10	0.80	-0.70	0.00	0.00	-0.048	0.222	0.014	1.603	0.110	0.068
	0.10	0.80	0.50	0.80	0.39	0.68	-0.29	-0.30	-0.70	0.259	0.157	0.247	0.226	0.088	0.022
b) s<0 ; d0=-0.30	0.10	0.70	0.50	0.80	0.35	0.68	-0.33	-0.30	-0.60	0.273	0.154	0.199	0.215	0.089	0.026
	0.10	0.60	0.50	0.80	0.31	0.68	-0.37	-0.30	-0.50	0.309	0.146	0.163	0.196	0.089	0.028
	0.10	0.50	0.50	0.80	0.27	0.68	-0.41	-0.30	-0.40	0.306	0.147	0.139	0.204	0.090	0.033
	0.10	0.40	0.50	0.80	0.23	0.68	-0.45	-0.30	-0.30	0.370	0.133	0.089	0.184	0.092	0.044
	0.10	0.80	0.30	0.80	0.39	0.60	-0.21	-0.50	-0.70	0.299	0.148	0.339	0.066	0.087	0.021
c) s<0; d0=-0.50	0.10	0.70	0.30	0.80	0.35	0.60	-0.25	-0.50	-0.60	0.331	0.142	0.302	0.050	0.088	0.025
	0.10	0.60	0.30	0.80	0.31	0.60	-0.29	-0.50	-0.50	0.358	0.136	0.232	0.049	0.088	0.023
d) s<0; d0=-0.70	0.10	0.80	0.10	0.80	0.39	0.52	-0.13	-0.70	-0.70	0.279	0.153	0.474	0.008	0.087	0.025
	0.80	0.80	0.10	0.10	0.80	0.10	0.70	0.00	0.00	-0.059	0.224	456.833	2.100	0.120	0.096
	0.70	0.80	0.10	0.10	0.74	0.10	0.64	0.00	-0.10	-0.073	0.227	128.944	1.709	0.107	0.058
	0.60	0.80	0.10	0.10	0.68	0.10	0.58	0.00	-0.20	-0.028	0.217	62.871	2.003	0.097	0.050
	0.50	0.80	0.10	0.10	0.63	0.10	0.53	0.00	-0.30	-0.022	0.216	41.944	2.456	0.093	0.039
	0.40	0.80	0.10	0.10	0.57	0.10	0.47	0.00	-0.40	-0.047	0.222	29.167	2.097	0.091	0.029
	0.30	0.80	0.10	0.10	0.51	0.10	0.41	0.00	-0.50	-0.058	0.224	24.003	1.902	0.089	0.029
	0.20	0.80	0.10	0.10	0.45	0.10	0.35	0.00	-0.60	-0.093	0.231	16.014	2.008	0.089	0.043
e) s>0; d0=0	0.10	0.80	0.10	0.10	0.39	0.10	0.29	0.00	-0.70	-0.090	0.231	12.057	2.254	0.088	0.038
	0.10	0.70	0.10	0.10	0.35	0.10	0.25	0.00	-0.60	-0.091	0.231	9.617	2.526	0.088	0.034
	0.10	0.60	0.10	0.10	0.31	0.10	0.21	0.00	-0.50	-0.065	0.225	7.238	2.243	0.087	0.028
	0.10	0.50	0.10	0.10	0.27	0.10	0.17	0.00	-0.40	-0.059	0.224	6.701	2.309	0.088	0.023
	0.10	0.40	0.10	0.10	0.23	0.10	0.13	0.00	-0.30	-0.052	0.223	3.950	1.880	0.088	0.022
	0.10	0.30	0.10	0.10	0.18	0.10	0.08	0.00	-0.20	-0.013	0.214	3.819	2.182	0.088	0.017
	0.10	0.20	0.10	0.10	0.14	0.10	0.04	0.00	-0.10	0.024	0.207	1.964	2.109	0.088	0.012
	0.10	0.10	0.10	0.10	0.10	0.10	0.00	0.00	0.00	0.012	0.209	1.140	3.364	0.088	0.012
	0.80	0.80	0.10	0.40	0.80	0.28	0.52	-0.30	0.00	-0.492	0.316	18.547	0.136	0.095	0.053
f) s>0; d0=-0.30	0.70	0.80	0.10	0.40	0.74	0.28	0.46	-0.30	-0.10	-0.350	0.286	10.636	0.126	0.092	0.043
	0.60	0.80	0.10	0.40	0.68	0.28	0.40	-0.30	-0.20	-0.281	0.271	8.584	0.100	0.090	0.032
	0.50	0.80	0.10	0.40	0.63	0.28	0.35	-0.30	-0.30	-0.208	0.256	6.073	0.133	0.089	0.029
	0.80	0.80	0.10	0.60	0.80	0.40	0.40	-0.50	0.00	-0.482	0.314	9.150	0.048	0.091	0.052
	0.70	0.80	0.10	0.60	0.74	0.40	0.34	-0.50	-0.10	-0.395	0.295	5.824	0.035	0.090	0.040
g) s>0; d0=-0.50	0.60	0.80	0.10	0.60	0.68	0.40	0.28	-0.50	-0.20	-0.319	0.279	4.642	0.034	0.089	0.033
	0.50	0.80	0.10	0.60	0.63	0.40	0.23	-0.50	-0.30	-0.268	0.268	4.084	0.034	0.089	0.032
	0.40	0.80	0.10	0.60	0.57	0.40	0.17	-0.50	-0.40	-0.197	0.253	2.891	0.041	0.088	0.029
	0.30	0.80	0.10	0.60	0.51	0.40	0.11	-0.50	-0.50	-0.139	0.241	2.142	0.039	0.088	0.023
	0.80	0.80	0.10	0.80	0.80	0.52	0.28	-0.70	0.00	-0.096	0.232	1.753	0.043	0.087	0.022
	0.70	0.80	0.10	0.80	0.74	0.52	0.22	-0.70	-0.10	-0.431	0.303	4.336	0.007	0.088	0.054
h) s>0; d0=-0.70	0.60	0.80	0.10	0.80	0.68	0.52	0.16	-0.70	-0.20	-0.303	0.276	2.987	0.006	0.088	0.040
	0.50	0.80	0.10	0.80	0.63	0.52	0.11	-0.70	-0.30	-0.175	0.249	2.012	0.007	0.088	0.033
	0.40	0.80	0.10	0.80	0.57	0.52	0.05	-0.70	-0.40	-0.108	0.234	1.525	0.006	0.088	0.027

Bias % = (ATE baseline-ATE)/ATE baseline - NB: Baseline ATE (no confounders) - Only Trustee / no placebo = 0.212. $d_1 = p_{11} - p_{10}$ *(outcome effect of U for the treated);* $d_0 = p_{01} - p_{00}$ *(outcome effect of U for the controls);* $s = p_1 - p_0$ *(effect of U on the selection into treatment).*

Selection effect (odds) = $\frac{Pr(T=1|U=1,W)}{Pr(T=0|U=1,W)} / \frac{Pr(T=1|U=0,W)}{Pr(T=0|U=0,W)}$*; Outcome Effect (odds) =* $\frac{Pr(Y=1|T=0,U=1,W)}{Pr(Y=0|T=0,U=1,W)} / \frac{Pr(Y=1|T=0,U=0,W)}{Pr(Y=0|T=0,U=0,W)}$. WSE = "within-imputation standard errors"; BSE = "between-imputation standard errors". For further details see Ichino et al., 2006.

Table 10. Sensitivity Analysis: *Simulating Unobservables Using Observables*.

Variable	p_{11}	p_{10}	p_{01}	p_{00}	$p_{1.}$	$p_{0.}$	s	d_0	d_1	Bias %	ATE	Selection effect (Odds)	Outcome Effect (Odds)	WSE	BSE
Volunteer	0.47	0.50	0.31	0.50	0.48	0.42	0.06	-0.19	-0.03	0.019	0.208	1.320	0.433	0.087	0.013
Female	0.47	0.62	0.50	0.61	0.53	0.57	-0.03	-0.11	-0.15	0.039	0.203	0.879	0.694	0.088	0.017
RiskAverse	0.69	0.46	0.61	0.59	0.59	0.60	0.00	0.02	0.23	0.031	0.205	1.086	1.347	0.088	0.012
BetrayalAverse	0.14	0.08	0.19	0.11	0.11	0.14	-0.03	0.08	0.06	-0.001	0.212	0.782	5.223	0.088	0.014
Unemployed	0.08	0.15	0.33	0.24	0.11	0.28	-0.17	0.09	-0.07	-0.030	0.218	0.356	2.590	0.089	0.026
Muslim	0.14	0.31	0.17	0.20	0.21	0.19	0.02	-0.03	-0.17	0.017	0.208	1.515	1.050	0.088	0.013
Married	0.44	0.38	0.19	0.33	0.41	0.27	0.14	-0.14	0.06	-0.014	0.215	2.361	0.480	0.088	0.018
Widowed	0.00	0.08	0.00	0.06	0.03	0.04	0.00	-0.06	-0.08	0.019	0.208	1.193		0.088	0.015
Separated	0.11	0.15	0.06	0.04	0.13	0.05	0.08	0.02	-0.04	0.006	0.210	6.795	5.459	0.088	0.019
Mfi_now	0.53	0.65	0.28	0.43	0.58	0.37	0.21	-0.15	-0.12	-0.049	0.222	3.143	0.491	0.088	0.022
Kikuyo	0.14	0.04	0.06	0.09	0.10	0.08	0.02	-0.03	0.10	0.038	0.204	2.027	1.088	0.088	0.014
Luo	0.33	0.31	0.44	0.52	0.32	0.49	-0.17	-0.08	0.02	0.062	0.199	0.500	0.839	0.088	0.020
Lubian	0.11	0.12	0.06	0.15	0.11	0.11	0.00	-0.09	-0.01	0.026	0.206	1.357	0.522	0.088	0.011
Luhya	0.08	0.27	0.31	0.13	0.16	0.20	-0.04	0.18	-0.19	0.002	0.211	0.896	6.774	0.088	0.013

Bias % = (ATE baseline-ATE)/ATE baseline - NB: Baseline ATE (no confounders) - Only Trustee / no placebo = 0.212.

$d_1 = p_{11} - p_{10}$ (outcome effect of U for the treated); $d_0 = p_{01} - p_{00}$ (outcome effect of U for the controls); $s = p_1 - p_0$ (effect of U on the selection into treatment).

Selection effect (odds) = $\frac{Pr(T=1|U=1,W)}{Pr(T=0|U=1,W)} / \frac{Pr(T=1|U=0,W)}{Pr(T=0|U=0,W)}$ *; Outcome Effect (odds) =* $\frac{Pr(Y=1|T=0,U=1,W)}{Pr(Y=0|T=0,U=1,W)} / \frac{Pr(Y=1|T=0,U=0,W)}{Pr(Y=0|T=0,U=0,W)}$.

WSE = "within-imputation standard errors"; BSE = "between-imputation standard errors". For further details see Ichino et al., 2006.

Appendix 1

Table A1. Descriptive statistics for the variables used in the empirical analysis

Variable	Mean	Median	Max	Min	Std. Err.
Age	27.84448	25	60	18	.4077708
Female	.5173267	1	1	0	.0248918
Married	.3341584	0	1	0	.0234968
Separated	.049505	0	1	0	.0108055
Widowed	.0445545	0	1	0	.0102777
n_house_members	4.531386	4	23	0	.1202529
Kikuyo	.0915842	0	1	0	.0143681
Luo	.4034653	0	1	0	.0244382
Lubian	.1534653	0	1	0	.0179546
Luhya	.1856436	0	1	0	.0193684
Muslim	.220297	0	1	0	.0206451
years_schooling	11.32754	12	18	0	.1517923
food_expenditure_day	268.933	250	1000	50	7.033822
unemployed	.2549505	0	1	0	.0217104
mfi_now	.519802	1	1	0	.0248872
volunteer	.4059406	0	1	0	.0244621
riskaverse	.4554455	0	1	0	.0248077
betrayalaverse	.220297	0	1	0	.0206451
impatient	.4480198	0	1	0	.0247718
placebo	.2475248	0	1	0	.0214982
CPRG_NA	.3811881	0	1	0	.0241934
n_friends	.2945545	0	3	0	.0313434
TG2-TG1	-.3435484	0	45.5	-78	.5686677
TG1	32.09468	29.35	111	5	.8042153
TG2	31.78114	30	108	5	.7168801
genderfragmentation	.4046053	.375	.5	0	.0064722
ethnicfragmentation	.5476974	.625	.75	0	.0088693
groupmeanwithdrawalratio	.6863203	.7133333	.969697	.2272727	.0095502
playermeanwithdrawalratio	.6884239	.7555556	1	0	.0144033
business_distruction	.5310174	1	1	0	.0248897
job_loss	.5569307	1	1	0	.0247448
personal_injury	.1960298	0	1	0	.0198001
property_damaged	.4851485	0	1	0	.0248958
home_distruction	.3325062	0	1	0	.0234969
eviction	.2945545	0	1	0	.0227071
relatives_death	.0792079	0	1	0	.0134528
same_area	.6203474	1	1	0	.0242046
moved_in	.1513648	0	1	0	.0178756
relocated_other_rural_area	.0693069	0	1	0	.0126514
relocated_other_part_of_town	.2009926	0	1	0	.0199872
relocated_other_town_in_kenya	.0420792	0	1	0	.0100011
DIPV	.240099	0	1	0	.0212775
Forced Relocation	.3762376	0	1	0	.0241317
Economic Losses	.7326733	1	1	0	.0220457
DIPVFR	.4207921	0	1	0	.4207921
Non Involved	.220297	0	1	0	.4149608