ORIGINAL PAPER

Trust and trustworthiness after a land restitution program: lab-in-the-field evidence from Colombia

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Abstract

We assess the impact on trust and trustworthiness of a governmental program to compensate victims of forced displacement. All our subjects were eligible to apply for restitution of their land in accordance with the 2011 "Bill of Victims" issued by the Colombian government. The key independent variable of our analysis is whether a subject had obtained land within this or similar programs. Our dependent variables are a subject's trust and trustworthiness to unknown others, as measured by an experimental Trust Game. We focus on interpersonal trust and trustworthiness because of their well-documented positive effect on economic development. Our design also included a treatment in which subjects voted on their most preferred outcomes in the game, as it has been shown that consultative democracy can increase mutual trust in other settings. We find that land restitution is significantly correlated with higher trustworthiness, while there is no correlation with trust. This is consistent with the idea that trust and trustworthiness tap into different aspects of individual motivations. Voting is not correlated with trust but is associated with higher trustworthiness in one of the measures, although there is no differential impact for those who were granted land. We compare the effect of having been granted land with rural and urban samples who were not victims of displacement. Overall, our results suggest that land restitution empowers victims reestablishing social capital. The evidence on the impact of political participation and its interaction with land restitution is inconclusive.

1 Introduction

Since the end of the 1950s, Colombia has been hit by a long and violent internal conflict, involving the army, politically motivated guerrilla forces and paramilitary troops. More than eight million civilians were recognized as victims by the government and around 5.5 million were forcefully displaced, generating one of the world's largest population of internally displaced people (IDPs), second only to the Syrian population. IDPs lost possession of an amount of land that has been estimated to be as large as 5.5 million hectares. At the beginning of the peace negotiations, the Colombian government approved in 2011 the Bill of Victims (*Ley de Víctimas*, Bill N. 1448/2011). This legislation served to formally recognize the country's internal conflict and lay the foundation for a long-term commitment to compensating victims. Such a commitment included land restitution for those who had lost possession of their land or who were forced to abandon their property out of security threats. Given the size of the population affected by the civil conflict (around 15% of the total population), a plan of this scale is unprecedented (Sikkink et al., 2014), except for the South African case.

Land restitution is the hallmark of the international judiciary principles to address land dispossession. It is aimed not only at reparation, but also at the reestablishment of perceived legitimacy of the institutional system and of political agency of the victims. Little is known whether this works.

The goal of the paper is to evaluate the effect of this land restitution program on inter-personal trust and trustworthiness, measured through an experimental Trust Game (TG) (Berg et al., 1995). Additionally, we want to assess the impact of consultative democracy on trust and trustworthiness. Since victims of internal conflicts are disenfranchised and disempowered, restoring trust should be a priority, and promoting political agency is a natural mechanism to achieve this goal.

To answer these research questions, we performed a lab-in-the-field experiment in the North of Colombia. We engaged different communities whose members were either in the process of claiming their land or were already recipients of land restitution through the program. Our sample comprises more than a hundred people from rural areas of Colombia, who belonged to victimized households. Their status as victim of the conflict has been officially recognized by the Colombian government. Although covering different communities, this is still a convenience sample. Our key methodological choice to investigate the effects of land restitution is to compare the behavior according to the status of the participant with respect to having been granted land as a part of this program, after balancing on observables. About 20% of our sample had been granted land at the time of

our study, thus we can compare propensities to trust and be trustworthy between people who have been granted land and those who have not.

The second methodological choice of our experiment is to introduce a treatment whereby participants vote on the preferred course of behavior. Voting is considered a fair procedure to participate since it gives equal access to everyone, and a fair procedure to make a decision, since every participant holds the same weight. Since procedures are important in determining the legitimacy of decisions and of organizations (Lind & Tayler, 1988; Olken, 2010), we conjecture that this may promote trust and trustworthiness within the community. This manipulation had been proven effective in communities not exposed to violence in Bogliacino et al., (2018a) (BJG hereafter). We thus wanted to test its effectiveness in communities heavily exposed to violence. To better understand the underlying theoretical mechanism, namely whether voting establishes a social norm or rather acts as a simple coordination device, we added an additional *signal* treatment where we read the results of votes performed elsewhere with the same procedures. Finally, we compare results from this sample with other samples who participated in TGs with the same incentive structures. These samples come from rural areas and urban areas not exposed to warfare and whose inhabitants are not IDPs. In this way we can contrast the effect of land restitution against other relevant samples to understand in relative terms what is the size of the effects that we are estimating.

We find that land restitution is significantly correlated with higher trustworthiness, while there is no correlation with trust. When we look at treatment effects, voting is not correlated with trust but is associated with higher trustworthiness in one of the measures, although there is no differential impact for those who were granted land. When we compare the effect of having been granted land with rural and urban samples who were not victims of displacement, we found that restituted victims catch up with the level of trust of rural households who were not exposed to violence.

This article makes several contributions. While most of existing studies investigating the effects of compensation programs on conflict victims have focused on variables such as employment, wealth, or education (De Greiff, 2006b; Hall, 2004, 2010; Lid, 2010), and it is reasonable to expect differences in consumption, access to credit, and investment (Ibáñez & Moya, 2010; Curtin, 2006; De Soto, 2000; Demsetz, 1967; Besley et al., 1992), we focus on interpersonal trust and trustworthiness because of their well-documented positive effect on economic development (Knack & Keefer, 1997). All studies that want to investigate the effects of compensation programs on victims of conflict are faced with the problem of finding an appropriate sample for the comparison. This is a problem of often difficult solution, because comparing populations exposed to violence with those not exposed—or little exposed—to violence introduce confounds in the analysis. The fact that our study has been run while the program was still under way, enables us to circumvent this problem, because beneficiaries and non-beneficiaries had been exposed to similar amounts of violence and had gone through similar experiences. In spite of some limitations, that will be discussed at length in the paper, we believe that our design contains enough elements of random variation to make it a valuable tool to analyze the causal impact of the intervention (land restitution) on the variable of interest (trust

and trustworthiness). Our identification strategy relies on the selection on observables as well as a control for omitted variable bias, as in Oster (2017). To the best of our knowledge, our lab-in-the-field experiment in rural Colombia is the first assessing the impact of land restitution on individual dispositions to trust and reciprocate.

We also contribute to the literature on institution-building in poor or victimized communities. To the extent that reparation overcomes grievance, affected communities may be more willing to engage in bottom-up institution building and be more sensitive to procedural fairness. Whereas procedural fairness as a tool to establish legitimacy has been studied in several contexts (Tyler, 2006), the case of victims of land dispossession has been overlooked.

Our final contribution is to further test the hypothesis that exposure to warfare may induce higher cooperation and trust, as found in other studies (Bauer et al., 2016; Bauer et al., 2016; Becchetti et al., 2014; Bellows & Miguel, 2009; Blattman, 2009; Gneezy & Fessler, 2012; Vélez et al., 2016; Voors et al., 2012).

The paper proceeds as follows: Sect. 2 discusses the relevant literature and the ethnographic and institutional context and formulate the theoretical hypotheses. Section 3 presents the design. Section 4 reports the results. Section 5 presents discussions and conclusions. An English version of the protocol, materials, and additional analysis are in the Supplementary Online Materials (SOM).

2 Violence, land restitution and trust

2.1 Institutional and ethnographic background

Colombia was marked by a long and violent conflict since the end of the 1950s. The main contenders have been the government and left-wing guerrilla groups, but paramilitary groups and crime syndicates have also been involved. According to the Colombia's National Centre for Historical Memory, 220,000 people have died in the conflict, 177,307 of whom were civilians. We briefly reconstruct the political and economic causes of the conflict in the SOM: Section S1. In this section we focus on the legislative measures that have been implemented by the government in the attempt to repair the consequences of the conflict.

Colombian governments have repeatedly tried to establish the rights of IDPs, the first attempt being carried out under the First National Front government in 1958–1962 (Karl, 2017). More recently, former president Uribe approved many legislative acts to implement a system of transitional justice deemed to restore victims' rights (975/2005, 1421/2010, and 1424/2010), the most important of which was known as the *Bill of Justice and Peace* (975/2005). The *Bill of Victims* (Bill 1448/2011) was passed in June 2011 under President Santos and is considered the most ambitious plan in the country's history to repair the multiple victims of internal conflict (Summers, 2012). The Bill officially recognized internal conflict and the government granted status of counterpart in peace talks to the most representative guerrilla group—the FARC. In doing this, the government took a different stance from the predominant governmental position in the Uribe era, which defined and fought illegal groups as terrorists (Robinson, 2013; Rojas, 2015).

The Bill is directed towards all victims, taking 1985 January 1st as the starting date for the eligibility of the claims to any reparation. The status of victim is acquired independently of the identification, apprehension, prosecution or condemnation of the authors of expropriation. The Bill establishes that the dispossessed have a right to restitution (if they have been dispossessed after 1991), through a process which consists of two phases: an administrative one, managed by the *Land Restitution Agency*, the main agency in charge, and a judicial one, where there is the presumption of good faith by the victim and the burden of proof is assigned to the current owner. If restitution is not possible, an alternative property should be transferred. In cases where neither of the two is available, some monetary compensation will be paid out to the victim. Ethnic minorities are granted specific procedures, regulated by additional decrees. The Bill also establishes both the nullity of any administrative act involving legalization of the property without acknowledging the right of the victim, and the invalidity of contracts celebrated on the property without good faith.

Article 66 of the Bill establishes the general rules of the victims' reacquisition of their land, or relocation to other areas if proper conditions for return are lacking. The main principles to be respected are willingness, dignity and safety. However, the Bill only granted restitution to victims whose land was stripped after 1991.[1]

To understand the qualitative change introduced by this Law in the history of Colombian conflict, the first Law that obliged the state to bring attention and care to displaced and dispossessed was approved only in 1997 (Law 387), although it lacked specific measures (Summers, 2012). The Justice and Peace Law was more stringent, but victims should report the crime and the perpetuator in order to ask for damages and restitution, which prevented effective implementation due to fear of retaliation and lack of safety for the victims. By reversing the burden of proof and by establishing a specific judicial regime outside from standard adversarial penal system, the new law lowered down the cost of access to justice for all victims.

In practice, land restitution was effectively established with the new law. To benefit from the law, one should possess the status, which means that the situation should not been resolved with previous interventions. Nevertheless, we cannot trace exactly the judicial story of each victim, as we should guarantee confidentiality with regards to sensitive information.

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[1] A couple of clarifications are in order. First, since 2011, a new Register has been created (Unique Register of Victims; RUV per the acronym in Spanish); according to the most recent available data, more than eight million victims have been officially recognized as affected by the internal conflict. Second, the amount of dispossessed land is unclear, essentially because the legal status of most rural properties is not formalized. Ibáñez et al. (2006), in an initial estimate, placed this number at 1.2 million hectares, but there are estimates as high as 10 million (Sánchez León, 2017). The 5.5 million estimate is validated by the largest survey of victims (Contraloría, 2014), and it is the figure on which the legislative agenda has been based.

2.2 Trust and trustworthiness

The importance of trust for economic development has been extensively documented over the last decades. Since Arrow's (1974) insight that inter-personal generalized trust acts as a lubricant for economic transactions, evidence has accumulated pointing out the beneficial effects of trust on economic performance (La Porta et al., 1999; Zak & Knack, 2001). By *inter-personal* trust we mean trust directed towards other individuals, rather than to the government or public or private institutions. Inter-personal trust manifests its beneficial effects through a broad variety of channels (see Knack & Keefer, 1997, for a review). The positive effects of trust are not limited to the economic sphere but extend to social cohesion and individual well-being (Layard, 2005). In a trusting environment, people can save resources on insurance and on transaction costs to write contracts, and organizations can save the costs necessary to build up enforcing mechanisms. Entrepreneurial activities are stimulated when people trust each other, both for the higher likelihood to set up enterprises and for the increased scale of productive activities. Other channels whereby trust exerts beneficial impact pertain to the social sphere—as inter-personal trust spreads norms of reciprocity and civic norms that bolster cooperation—and the political sphere—as governmental and legal action are more likely to be successful in a trusting society.

For all these reasons, the focus of this paper is on inter-personal trust, and on trustworthiness. Trustworthiness is a natural complement of trust. Simply stated, without trustworthiness, trust would be futile. If trusting actions were not repaid by trustworthy *re*-actions, it would not be difficult to imagine that trust would be wiped out. More specifically, we model trust as the propensity to rely on other agents by willing to put material or immaterial resources at their disposal without a legal or binding constraint for these resources to be returned. A typical characteristic of a trust relationship is the situation of vulnerability to which the trusting agent exposes herself (Kolloch, 1999). Trustworthiness is the propensity not to take advantage of the resources that have been transferred by the trusting agent, but rather to pay back the trusting agent with a share of resources that may be considered fair. Although beliefs are often deemed to be a constituent part of this definition, we are mainly interested in the behavioral aspect of trust (Coleman, 1988; Fehr, 2009).

In experimental economics, trust and trustworthiness are measured through the TG developed by Berg et al. (1995). TG experiments have been conducted in many labs and field environments (see Cardenas & Carpenter, 2008 and Johnson & Mislin, 2011, for surveys). It has been shown that many different motives lead to higher trust, from reciprocity to altruism, from efficiency to self-interest (Cox, 2004). There are both individual level determinants of trust, e.g., age (Belli et al., 2012) and risk attitudes (Karlan, 2005), and institutional settings that promote trust. As discussed by BJG, sanctions, voice, communication, promises and reputation can, to varying degrees, shape the level of trust in the interaction between two anonymous parties. Lab-in-the-field experiments with the trust game have been a common instrument to assess social cohesion in presence of exposure to violence (Becchetti et al., 2014; Gilligan et al, 2014; Vélez et al., 2016).

2.3 Hypotheses

According to the Bill 1448/2011, victims of land dispossession and forced abandonment after 1991 are entitled to have their property rights restored. It is reasonable to expect that victims of land dispossession will feel excluded and perceive the current system of property rights as unjust (Athuahene, 2010; Sánchez León, 2017), reducing their willingness to participate economically and politically.

We conjecture that land restitution has a positive effect on both trust and trustworthiness:

(H1). Land restitution increases trust and trustworthiness.

This positive effect occurs mainly through two channels: the first one is through preferences, the second is through beliefs. According to the preferences channel, trusting, norm complying, and showing other regarding concerns is a normal good (Eckel & Gintis, 2010), as a result, its "demand" is boosted by a positive wealth shock such as land restitution. To understand the belief channel, we make reference to the theory of transitional justice (ONU, 2015). Athuahene (2007: 1431) introduces the concept of property-induced invisibility, that is "*the confiscation or destruction of real property with no payment of just compensation, executed such that dehumanization occurs*". When property-induced invisibility occurs, it is argued that the victim is essentially excluded from the social contract. In other words, the victim perceives the current system of property rights and institutions as unjust. Restoring property rights to the dispossessed makes the current system of property rights more legitimate, increasing political stability and procedural legitimacy, and indirectly injecting trust and trustworthiness in the community. This is achieved by stabilizing citizens' expectations, using material reparations as a means of restoring belief in the trustworthiness of the institutions; in turn, through these channels, trusting behavior is occasioned: "Former victims of abuse are given a material manifestation of the fact that they are now living among a group of fellow citizens and under institutions that aspire to be trustworthy" (De Greiff, 2006a: 464). This would also be consistent with the evidence from land *titling*, according to which beliefs in the legitimacy of the system change (Di Tella et al., 2007; Galiani & Schargrodsky, 2010). Evidence from resettled community is not consistent with this hypothesis (Barr, 1999, 2003; Barreto et al., 2016), but to the best of our knowledge, we are the first to test this mechanism with land restitution.

Our second conjecture is that the positive effect of voting on trust and trustworthiness found in BJG would replicate to a context characterized by high exposure to violence:

(H2) Voting increases trust and trustworthiness.

In the rural environments object of our study, consultative democracy is the most widely used institutional mechanism for community deliberation and empowerment. As non-binding, the vote was tantamount to "cheap talk" (Crawford & Sobel, 1982) and would not affect the theoretical prediction of the Subgame Perfect Nash Equilibrium. Conversely, it may induce a commitment effect (similar to the case of promises, as in Vanberg, 2008), spur the formation of normative expectations (Duffy et al., 2013) or transmit signals on empirical expectations (Bicchieri & Xiao, 2009). If it works, we also want to understand whether it is through coordinating decisions

(as a sort of focal point), or through establishing a social norm. Our experimental design improves on BJG, by allowing this distinction. Since BJG only included a Voting condition but not an Announcement condition, we also wanted to disentangle the effect of the announcement of a course of action from the effect of voting on trust and trustworthiness.

Additionally, we expect the effect of voting to be stronger on land restituted than displaced:

(H3) The positive effect of voting on trust and trustworthiness is larger for restituted than for displaced participants.

It is reasonable to expect that as soon as victims receive reparations, they reach a more advanced stage of healing. As a result, the participation to fair procedure of deliberation should make them more trusting and trustworthy as procedural fairness induce increase the perception of legitimacy of communities and organizations (Tyler, 2006).

Finally, we offer a comparison of results from TGs that we implemented in rural Colombia (BJG) and in Bogotá (Bogliacino, Gómez and Grimalda, 2020). We conjecture the following hypothesis:

(H4) Land restitution makes participants converge to the level of trust and trustworthiness of rural but not urban samples.

3 Experimental design

3.1 Experimental design: the main task

Our version of the TG is similar to Charness et al. (2011) and Ermisch et al. (2009). The extended form of the game is illustrated in Fig. 1, with payoffs given in tokens. Both senders and receivers were assigned two tokens at the beginning of the interaction. The sender then decided whether to transfer one, two, or no tokens to the receiver. We refer to this variable as 'Amount Sent' (AS henceforth). The amount sent was tripled by the researcher. The receiver then was faced with two dichotomous choices over whether to keep all the tokens in her possession or to share them in a way that guaranteed payoff equalization between sender and receiver.[2] These "Keep" or "Share-equally" options were found to be easy to understand and intuitive for a population with low educational levels (Table 1). We recorded receiver's decisions through the strategy method, asking first the receiver's decision if the sender would send one token and then the receiver's decision if the sender would send two tokens. We refer to these variables, respectively, as 'Amount Returned if AS = 1' and 'Amount Returned if AS = 2'. For simplicity of notation, we label these two variables 'AR1' and 'AR2', respectively. In most of our subsequent analysis we will use the *total* Amount Returned by the receiver over the two decisions. We label

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[2] Note that the "Share equally" option prescribes the receiver to return 2 tokens when AS equals one, and four tokens when AS equals two.
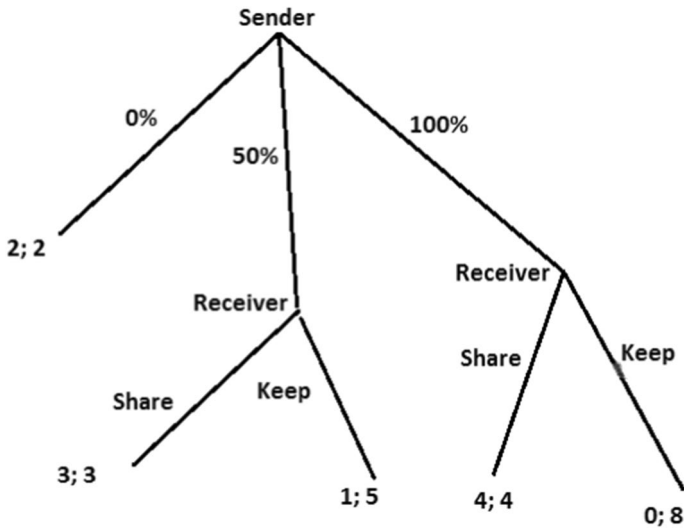
Fig. 1 The trust game in extended form

Table 1 Descriptive statistics of demographic characteristics

Variable	Distribution
Observations	111
Gender	Male: 67%
Age	18–24 (10.20%)
	25–30 (12.25%)
	31–40 (19.03%)
	41–55 (27.55%)
	56–83 (30.61%)
Socio-economic-status (SES)	1 or absent: 90.42%
	2 or 3: 5.31%
Education	No education: 32.38%
	Primary: 37.14%
	Secondary or more: 30.48%
Occupation	Farmer: 74%
	Retired/unemployed: 12%
Land-granted	20.72%

SES measures the quality of a resident's dwelling, where quality increases from tier one to tier six. The quality assignment is the result of a government assessment (*estrato* in Spanish) that is conducted nationwide. People are normally aware of their dwellings' tier because the payment of utility bills is based on it. Easiness of verification ensures truthful self-reporting. The tier is highly correlated with income. Absent is typically related with dwellings in illegal settlement, or too dangerous, thus is similar to SES one

this variable as 'AR', where AR is a weighted average of the two decisions (weights being how much is returned in both cases).

Each token was worth 4000 Colombian Pesos (COP), which were worth 2.14 USD at the time the research was conducted. Considering that the hourly minimum wage was slightly above 2000 COP and that displaced people are mostly poor (83.9% of them are in monetary poverty, according to Contraloría, 2014), stakes appear to have been substantial for participants.

Each participant made one decision as sender and one as receiver. Participants in a session were randomly and evenly split between senders and receivers for the first decision. Participants were re-matched with a different partner for the second decision, with their role being swapped. We chose a "stranger matching" to avoid that reciprocity considerations affected decisions. In this way, we consider the first and second decisions as independent. Participants were aware they would make two decisions, but at the time they made their first decision they were not informed they would have made another decision in the alternative role. After participants passed a comprehension check, they made their experimental decisions.

3.2 Experimental design: treatments

Participants were assigned to three experimental conditions: Control, Vote and Announcement. In the Control condition, participants interacted in the TG as illustrated above. In the Vote condition, experimental decisions in the TG were preceded by a vote. Each participant was asked to indicate on a sheet which actions she considered the most "opportune" for participants to perform.[3] Participants expressed a vote for each of the three decisions in the TG—namely, the sender's AS and the two receiver's AR1 and AR2. The actions that received most votes were then publicly announced. In all sessions the actions that were voted as most "opportune" by the majority were for the sender to send two tokens and for the receiver to share evenly. Hence, the same set of actions was announced in all sessions.

In the Announcement condition, there was no vote, but the researcher read out the set of actions that was voted as most "opportune" by the (simple) majority of participants in a Vote condition previously held. This was, once again, for the sender to send two tokens and for the receiver to share evenly.[4] In both the Vote

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[3] The text of instructions to the sender was: "Please, indicate how many tokens you would consider opportune for the sender to send to the receiver". The text for the receiver was: "For each situation, indicate whether you believe that it was opportune or not that the receiver transferred a part of her tokens." The options for the receiver further specified: "The receiver does not have to transfer anything" and "The receiver has to transfer a share of his or her tokens". The final allocation of tokens in each situation was then specified. The adjective used in the original Spanish was "*oportuno*", a word that conveys a sense of obligation, albeit without a strong moral connotation, as in its English equivalent. See the SOM: Section S2.1 for the full version of the instructions.

[4] The text of the relative instructions was as follows: "According to sessions previously performed in some Colombian villages, the majority of the participants considered opportune for the sender to send two tokens to the receiver. According to the same sessions, the majority considered opportune for the receiver to share when the sender sent one token. According to the same sessions, the majority considered opportune for the receiver to share when the sender sent two tokens." See the SOM: Section S2 for the full version of the instructions.

and Announcement conditions, the announcement was given publicly, emphasizing that it was altogether not binding for future decisions. Of the nine sessions performed, three sessions were assigned randomly to each condition. Due to differences in attendance, the distribution of participants across treatments was slightly unbalanced. An English version of the full protocol is included in the SOM: Section S2.1.

3.3 Experimental design: procedures

Our lab-in-the-field study was conducted in Pava, in the region of Bolívar (subregion *Montes de María*), in Northern Colombia. The area was the theatre of heavy fights between leftwing guerrilla and paramilitary groups. The latter were responsible for 42 different massacres and massive displacement (GMH, 2013, 2009; see SOM: Sect. 1).

Community leaders made possible participants' recruitment and acted as "gatekeepers" for the study, so that researchers never got to know participants' identity. The lead experimenter held different meetings with community leaders prior to the study and provided a generic description of the aim of the research. No mention was made the main hypotheses of the study. We made sure that people from different communities took part in the same session to make sure that we are measuring generalized trust and not in group trust.

The key strategy to identify the effect of land restitution is to divide our sample of IDPs into two subgroups. We assign participants to the group having benefitted from land restitution from a governmental program as those who answered affirmatively to at least one of the following three statements in our post-experiment questionnaire: "I have benefited from the restitution under the Bill of Victims"; "I have benefited from other process of land titles assignment"; "I have benefited from a collective process of land titling". (see SOM: Section S2.2: Question 10). We assign all other participants to the residual group of people who had not benefitted from land restitution. The dichotomous variable 'Land-Granted' assigns value of one to participants who declared to have been granted land according to the abovementioned criterion and assigns value of zero to all other participants. Land-granted people represented around 20% of the sample, while 52.58% declared not to have received any measure of compensation (neither monetary nor in kind). Lack of access to governmental data and the need to protect participants' anonymity prevented us from cross-checking whether answers to these questions were truthful.

3.4 Experimental design: other experiments used as a comparison

In this section we compare results from the current experiment with those coming from previous experiments we conducted in rural areas not affected by conflict and in the urban areas of Bogota. We used the same TG in all these settings with identical incentives, the same leading experimenter, and the same assistants, thus ensuring comparability of our results. The first additional sample comprises 91 observations from BJG. Participants were rural farmers from the region of Cundinamarca, Colombia, who took part in the TG described above under a baseline and a Vote

condition. Both baseline and the Vote conditions had the same structure as the one of the present experiment. The only difference in BJG was that participants made two choices—instead of one—, before and after taking part in the voting. For this reason, we rely on the first round of data for the purposes of the present study, which represents the baseline and is more comparable.

The second sample comprises 80 observations from the Colombian capital Bogotá that used a TG with a baseline condition and a third-party punishment treatment (Bogliacino et al., 2020). In the original study we had 224 observation, of which 80 without TPP, which are the ones used in this comparison. Participants in these case where younger, from 18 to 30 years old, but they were from all over the city and not recruited among the students population.

4 Results

4.1 The effect of land restitution

Table 1 reports descriptive statistics for the demographic characteristics of our sample. We collected 111 observations in the TG, but the regression analysis includes fewer observations due to missing values in the post-experimental questionnaire. Two thirds of participants were males, 58% were older than 40 years and two-thirds of the sample had at most primary education. Our sample predominantly comprised people of low Socio-Economic Status (SES), where SES is "proxied" by the government assessment of the quality of one's dwelling (Bogliacino et al., 2018b).

Of the nine sessions performed, three sessions were assigned randomly to each condition. In the end, we have 24.32% of the sample under the control condition, 34.23% in the signal treatment, and the remaining 41.44% in the vote treatment. Among the reasons for displacement (SOM, Sect. 2.2, Question 6) the main reported cause is threats (47.17%) followed by direct violence (16.85%). Twelve persons reported not having abandoned their land (SOM, Sect. 2.2, Question 5), which means that they belong to the second or third generation of IDPs, as recorded in the law. Among the respondents, 74.04% reported being officially registered as victims, while 20.19% did not know, and 5.77% stated not to be officially registered (SOM: Section S2.2, Question 9).

Table 2 shows that randomization was successful in ensuring exogeneity of the treatment conditions with respect to demographic characteristics. Regarding the Land-Granted variable, socio-demographic characteristics are balanced across conditions, with the notable exception of SES ($\chi^2 = 11.98$, $p = 0.04$). In principle, this may just be a false positive: we are checking a difference across six different variables and this makes likely that at least one is significant, by multiple hypotheses testing. However, this may suggest that either people with higher SES had higher success in getting land, or that obtaining land increased their SES. If the former is true, the fact that our econometric analysis controls for SES and other demographic characteristics increases internal validity. If the latter is true, then SES is causally

Table 2 Test of exogeneity of treatment condition and grant of land status

	By treatment	By land-grant status
Gender	$\chi^2=.01$	$\chi^2=.10$
Marital status	$\chi^2=1.22$	$\chi^2=.53$
Age	$\chi^2=118.65$	$\chi^2=52.35$
HH size	$\chi^2=4.56$	$\chi^2=.00$
SES	$\chi^2=10.21$	$\chi^2=11.98*$
Time of displacement	$\chi^2=57.19$	$\chi^2=32.38$

Table reports the Chi-square statistics of Pearson's χ^2 tests over the null hypothesis that the distribution of demographic characteristics mentioned in the first column is the same along sub-groups of the sample identified by the treatment condition (second column), i.e. Vote/Announcement/Control, or one's status with respect to having been granted land or not (as measured by the Land-Grant variable; third column). $*p<0.1$; $**p<0.05$; $***p<0.01$

affected by the treatment (i.e. restitution) and adding it as a control will filter out part of the effect, providing us with a lower bound of the effect.

Statistical analysis fails to reject the null hypothesis of absence of ordering effects (SOM: Section S3.1).

Histograms of participants' choices are plotted in the SOM, Sect. 3: Fig. 1. AS was higher than similar research (Cárdenas et al., 2013), as only 12% of the sample sent zero tokens, 45% sent one token and 43% sent two tokens. This is probably due to the restricted action space of the sender, with only three possible choices, but also reflects differences between urban and rural environment. AR1 and AR2 were also relatively high, as 63% and 64% of receivers, respectively, decided to share the money evenly. 48% of receivers shared evenly for both AR1 and AR2. The null hypothesis that the distribution of decisions is the same when participants acted first as senders and then as receivers (no ordering effect) is not rejected (SOM: Section S3.1).

Figure 2 reports AS and AR broken down by Land-Granted. No significant difference appears with respect to AS, which is confirmed by a Mann Whitney Wilcoxon rank sum test (MWW, $z=-0.47$, $p=0.63$). AR appears higher in the Land-Granted group than in the other group. A non-parametric test indeed rejects the null hypothesis of equal distribution in the Land-Granted and non-Land-Granted groups (MWW, $z=-2.10$, $p=0.03$, if we look separately at the two return decisions, using χ^2 test given that outcome is dichotomic, we get Peason's chi2$=2.87$, $p=0.09$ for AR1 and chi2$=2.57$, $p=0.10$ for AR2).

Table 3 reports econometric analysis using Ordinary Least Squares (OLS) regressions for both AS and AR. First, we assess if there is a significant increase of AS and AR in the displaced population whose land rights have been restored. We use a selection on observables strategy. Since we could not collect a baseline ex ante, to avoid bad control (Angrist and Pischke, 2009) we use only socio-demographic controls. Bad control occurs when the controls are causally affected by the treatment, biasing the results of the estimation. No significant impact of Land-Granted on AS
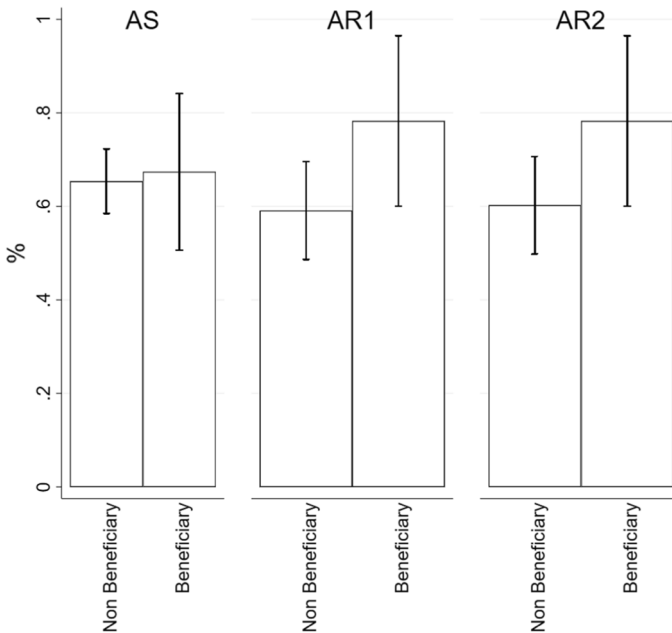
Fig. 2 Trust and trustworthiness in Land-Granted and non-Land-Granted groups. *Note*: See notes to Table 1 for definition of Land-Granted. AS is amount sent as a share of the maximum. AR1 is the likelihood to share when one token is sent, AR2 is the likelihood to share when two tokens are sent

Table 3 Trust and trustworthiness in land-granted and non-land-granted populations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AS	AS	AS	AS	AR	AR	AR	AR
Land-Granted	0.0236	0.0402	0.0540	0.0694	0.176**	0.233**	0.253**	0.236*
	(0.0871)	(0.0947)	(0.103)	(0.116)	(0.0886)	(0.0993)	(0.112)	(0.123)
Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics		Yes	Yes	Yes		Yes	Yes	Yes
Session FE			Yes	Yes			Yes	Yes
Socio economic				Yes				Yes
Constant	0.682***	0.645***	0.717***	0.552***	0.554***	0.750***	0.834***	0.688***
	(0.0619)	(0.121)	(0.173)	(0.201)	(0.0691)	(0.120)	(0.184)	(0.250)
Observations	111	98	98	82	111	98	98	82
R-squared	0.003	0.027	0.068	0.127	0.047	0.081	0.110	0.164

We fit an OLS model with robust standard errors in parentheses. See notes to Table 1 for definition of Land-Granted. Treatments are the dummies for the Announcement and the Vote conditions. Demographics include age and gender (F). Socio economic include Education, SES (see notes to Table 1), a dummy for unemployed, and a dummy for farmers. The full estimation is in the SOM, Section S3.2, Table 1

***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Table 4 Trust and trustworthiness in Land-Granted and Not-Land-Granted groups: Robustness checks

	(1) AS	(2) AS	(3) AS	(4) AS	(5) AR	(6) AR	(7) AR	(8) AR
Other reparation	−0.0133	−0.0559			−0.0662	−0.0953		
	(0.0677)	(0.0961)			(0.0779)	(0.116)		
Land-Granted			0.0743	0.0555			0.255*	0.298**
			(0.137)	(0.122)			(0.147)	(0.112)
Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics		Yes	Yes	Yes		Yes	Yes	Yes
Socio economic		Yes	Yes	Yes		Yes	Yes	Yes
Session FE		Yes	Yes	Yes		Yes	Yes	Yes
Excluding Unregistered			Yes				Yes	
Time				Yes				Yes
Constant	0.691***	0.563***	0.453	0.582**	0.607***	0.707***	0.929***	0.705**
	(0.0648)	(0.207)	(0.293)	(0.220)	(0.0733)	(0.245)	(0.318)	(0.266)
Observations	111	82	58	78	111	82	58	78
R-squared	0.003	0.125	0.116	0.120	0.022	0.122	0.129	0.216

OLS Regression with robust standard errors in parentheses, ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. See notes to Tables 1 and 3 for definition of variables. Time refers to the time (in years) living in the current house (question 7 in the SOM). Unregistered means that the respondent is not included in the Victim Registry (RUV) or does not know (SOM, Sect. 2.2, Q9). The full estimation is in the SOM, Section S3.2 Table 2

can be detected, while we find a significant effect on AR. This effect is robust to controlling for SES, occupation (with two dummies for participants being farmers or retired), and for session effects.

Table 4 provides further robustness checks. We first substitute the variable 'Other Reparation' for 'Land-Granted' in the regression. 'Other Reparation' is a dichotomous variable that identifies the receipt of some form of reparation different from land, typically money. 'Other Reparation' has no effect on AR. This shows that being the recipient of Land is the key aspect in increasing AR. In Columns (2) and (5), we exclude those that declared not to have abandoned their land (see Sect. 2.3), yet the effect of Land-Granted remains. Finally, in Columns (3) and (6), we control for the time spent in the current residence (SOM: Section S2.2, Question 7). Again, the result on AR holds. Table 5 reports separate OLS regressions for AR1 and AR2. The results do not change, although the effect seems to be stronger for AR2 than AR1.

To assess robustness of results, we also performed a sensitivity analysis of the effect of land on outcome variables, inferring the potential impact of omitted variable bias from the stability of the coefficients of interests when further controls are added (Oster, 2017). Based on the key (unverifiable) assumption that the selection on observables is the same as the selection on unobservables, after adjusting for differences in the variance of these distributions, we can calculate the bias and estimate the value of the coefficient after correcting for it. The formula (based on OLS regressions) for this coefficient is:

Table 5 Trustworthiness in displaced and restored population: Analysis of the strategy method

	(1) AR1	(2) AR2	(3) AR1	(4) AR2	(5) AR1	(6) AR2	(7) AR1	(8) AR2	(9) AR1	(10) AR2	(11) AR1	(12) AR2
Land-Granted	0.202**	0.163	0.208*	0.245**	0.204*	0.277**	0.192	0.258*	0.254**	0.320**	0.272*	0.386**
	(0.100)	(0.100)	(0.109)	(0.105)	(0.111)	(0.123)	(0.120)	(0.139)	(0.108)	(0.128)	(0.142)	(0.163)
Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographics			Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Session FE					Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Socio economic							Yes	Yes	Yes	Yes	Yes	Yes
Time									Yes	Yes	Yes	Yes
Excluding unregistered											Yes	Yes
Constant	0.674***	0.494***	0.830***	0.710***	1.181***	0.661***	1.088***	0.488	0.965***	0.575*	1.153***	0.941**
	(0.090)	(0.099)	(0.183)	(0.161)	(0.215)	(0.211)	(0.267)	(0.304)	(0.291)	(0.308)	(0.330)	(0.385)
Observations	111	111	98	98	98	98	82	82	78	78	54	54
R-squared	0.036	0.054	0.050	0.072	0.179	0.117	0.304	0.170	0.343	0.220	0.390	0.204

OLS Regression with robust standard errors in parentheses, $*** p < 0.01$, $** p < 0.05$, $* p < 0.1$. See notes to Tables 1 and 3 for definition of variables. The full estimation is in the SOM, Section S3.2 Table 3

$$\bar{\bar{\alpha}} = \hat{\alpha}^* - \left(\widehat{\alpha^0} - \hat{\alpha}^*\right) * \frac{R_{max} - R^*}{R^* - R^o} \tag{1}$$

where $\hat{\alpha}^*$ and R^* are the coefficient estimate and R squared from the regression using observable covariates, respectively, and α^0 and R^o are the coefficient and R squared from the uncontrolled regression, respectively. The key to understanding this procedure is R_{max}: this is the R-squared when y is regressed against both observable and unobservable controls, which is clearly unknowable and represents a degree of freedom. In our investigation, we followed a procedure similar to that of Gonzales and Miguel (2015), calculating four different scenarios: (1) a conservative scenario wherein $R_{max} = 1$, which would be the case given zero measurement error; (2) a scenario wherein $R_{max} = 2R^* - R^o$, which corresponds to the assumption that the relationship between the treatment and the observables is the same as the relationship between the treatment and the unobservables (Bellows & Miguel, 2009); (3) Oster's (2017) proposal of $R_{max} = Min\{2.2R^*, 1\}$; and, finally, (4) a rule of thumb $R_{max} = 0.5$, which corresponds to a measurement error of 50%.

This sensitivity analysis confirms the robustness of the results to the correction for omitted variable bias (SOM: Section S3.2, Table 10 for the complete output). The reason is that the magnitude of the coefficient actually increases as we move from uncontrolled OLS regression to controlled OLS regression. In other words, there is no attenuation bias in our sample. This is the case for all three experimental choices—AS, AR1 and AR2. Since by (2), the correction for omitted variable bias has the same sign as the difference between the uncontrolled coefficient and the controlled coefficient, in this case we infer a larger impact of the land restitution. This suggests that data are consistent with our H1, at least for trustworthiness. SOM, Sect. 3.2, Tables 5, 6 and 7 report Ordered Logit regressions confirming the results.

4.2 The effect of the voting mechanism

BJG found that a consultative voting treatment was able to improve trust and trustworthiness in a rural environment characterized by no exposure to warfare and no presence of IDPs. We investigate whether this mechanism is also effective in communities exposed to warfare.

Figure 3 reports the impact of both Vote and Announcement treatments on experimental choices. Only in one of the behavioral variables (AR2) there is an effect of vote on behavior, while in the other measures, differences are not statistically significant. Regressions in SOM, Section S3.2: Tables 1, 2 and 3 show that this result is not robust. This suggests that consultative voting did not work, in the aggregate, in violence-exposed population in the same way as with non-exposed population (BJG; Sect. 3). In other words, data do not support our H2.

The results of the vote are interesting: 52.63% voted to send two tokens and 63.16% and 65.79% voted to share evenly for AR1 and AR2, respectively. This means that consultations indicated coordination on efficient behavior as the most opportune behavior. Moreover, most of the participants stick to the behavior they voted for (around 65% in all three cases). In fact, if we compare the behavior and the vote for those in the vote condition, the two distributions are not independent, as if

Table 6 Heterogeneous effect of the voting mechanism on the restored population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	AS	AS	AS	AS	AR	AR	AR	AR
Vote	0.00302	−0.0199	0.194	0.0642	0.0903	0.149	0.159	0.220
	(0.0834)	(0.0936)	(0.175)	(0.172)	(0.102)	(0.116)	(0.227)	(0.253)
Announcement	−0.0475	−0.0414	−0.126	−0.136	0.0154	0.0550	0.0308	−0.00254
	(0.0833)	(0.0921)	(0.190)	(0.156)	(0.0973)	(0.102)	(0.172)	(0.204)
Land Granted	0.113	0.0679	0.0682	0.135	0.155	0.253	0.340**	0.293*
	(0.120)	(0.138)	(0.153)	(0.155)	(0.131)	(0.160)	(0.165)	(0.173)
Vote#Land Granted	−0.194	−0.0574	−0.0299	−0.144	0.0462	−0.0423	−0.143	−0.126
	(0.172)	(0.188)	(0.206)	(0.223)	(0.176)	(0.204)	(0.233)	(0.254)
Constant	0.668***	0.647***	0.717***	0.512**	0.557***	0.752***	0.612**	0.653**
	(0.0632)	(0.121)	(0.174)	(0.206)	(0.0709)	(0.121)	(0.235)	(0.263)
Observations	111	98	98	82	111	98	95	82
R-squared	0.017	0.028	0.069	0.133	0.048	0.082	0.148	0.168
Demographics	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Session FE	No	No	Yes	Yes	No	No	Yes	Yes
Socio economic	No	No	No	Yes	No	No	No	Yes

OLS Regression with robust standard errors in parentheses. See notes to Table 1 and Table 3 for definition of variables. ***$p<0.01$, **$p<0.05$, *$p<0.1$ The full estimation is in the SOM, Section S3.2 Table 4

voting indicates a commitment effect (Vanberg, 2008). The null hypothesis of independence of the distribution of voting and AS is rejected (χ^2=14.95, $p<0.01$), and, weakly, for AR1 (χ^2=2.89, $p=0.08$), but not for AR2 (χ^2=0.36, $p=0.54$). Nevertheless, the two treatments do not appear to have any significant impact on behavior.

In Table 6 we report the results on heterogeneous effect between land-granted and non-land-granted. The test is clearly underpowered, so this is exploratory evidence. As can be seen, the interaction is not significant, rejecting our H3.

4.3 Comparative evidence

We report the levels of AS and AR in Fig. 4. The first two bars plot mean values for Non-Land-Granted and Land-Granted from the present experiment. The third bar, labelled "Rural", reports mean values for rural areas not exposed to warfare as in the present sample from BJG. The last bar, labelled "Urban", reports mean values for the urban sample from Bogliacino et al., (2020).

Overall, the highest levels of AS are found in the rural sample not exposed to warfare and the lowest in the urban sample, with both Non-Land-Granted and Land-Granted lying in between. A one-way analysis of variance rejects the null hypothesis of absence of difference in AS between groups (F=8.04, $p<0.01$). Pairwise comparisons using Bonferroni corrections show differences between Non-Land-Granted and Rural ($p=0.07$), between Non-Land-Granted and Urban ($p=0.09$), and between Urban

Table 7 Trust and trustworthiness: Non Land Granted, Land-Granted, Rural and Urban samples

	(1) AS	(2) AS	(3) AS	(4) AR	(5) AR	(6) AR
Land-Granted	0.0205	0.0395	0.0271	0.184**	0.239**	0.203**
	(0.0868)	(0.0926)	(0.0982)	(0.0876)	(0.0957)	(0.102)
Rural	0.121***	0.0976*	0.0427	0.189***	0.149**	0.114*
	(0.0456)	(0.0514)	(0.0544)	(0.0574)	(0.0631)	(0.0656)
Urban	− 0.122**	− 0.127*	− 0.192***	0.147**	0.0685	0.0394
	(0.0519)	(0.0679)	(0.0685)	(0.0582)	(0.0761)	(0.0796)
Sex (F)		− 0.0626	− 0.0532		− 0.0355	− 0.0333
		(0.0416)	(0.0419)		(0.0481)	(0.0502)
Age		0.00197	0.000983		− 0.000774	− 0.000767
		(0.00148)	(0.00147)		(0.00179)	(0.00186)
SES			0.0191			0.00809
			(0.0203)			(0.0224)
Education		0.0333*	0.0259		0.0278	0.0226
		(0.0171)	(0.0177)		(0.0198)	(0.0209)
Constant	0.653***	0.535***	0.592***	0.598***	0.613***	0.636***
	(0.0347)	(0.0850)	(0.0892)	(0.0432)	(0.0975)	(0.109)
Land-Granted = Rural (*p* value)	1.41 (0.23)	0.39 (0.53)	0.02 (0.87)	0.00 (0.99)	0.87 (0.35)	0.75 (0.38)
Land-Granted = Urban (*p* value)	2.59 (0.10)	2.52 (0.11)	3.85 (0.05)	0.18 (0.66)	2.44 (0.11)	1.94 (0.16)
Rural = Urban (*p* value)	24.99 (0.00)	14.39 (0.00)	15.82 (0.00)	0.59 (0.44)	1.61 (0.20)	1.30 (0.25)
Observations	281	261	245	282	262	246
R-squared	0.080	0.099	0.096	0.046	0.058	0.040

OLS Regression with robust standard errors in parentheses, See notes to Tables 1 and 3 for definition of variables. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$. *Rural* is equal to one if the observation belongs to the sample of Bogliacino et al. (2018a), while *Urban* is equal to one if the observations belong to the Bogotá sample (Bogliacino et al., 2020)

and Rural ($p < 0.01$). The low level of trust in the urban environment is in line with comparative evidence and previous studies on Bogotá (Cárdenas et al., 2013). Several studies found a positive effect of exposure to warfare on pro-social dispositions (Bauer et al., 2016). However, this result does not replicate in our sample. One possible explanation is the following. In our rural environment (BJG) transfers were within each community, where trust is higher, while in this article, sessions were mixing participants coming from different communities, where trust is naturally lower. Additionally, the meta review by Bauer et al. (2016) claim that the effect of violence on cooperation and trust is strongest within each community (in group bias). In other words, when we compare this sample with BJG's sample, there are two factors at play, exposure to violence may be driving the trust upwards, but the effect is small, as in group bias is minimized
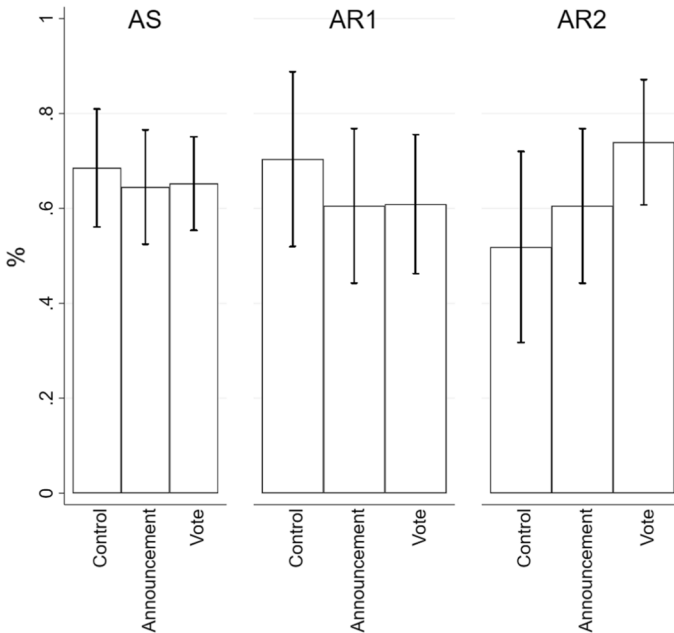
Fig. 3 AS, AR1, and AR2 per experimental condition. *Note:* Vote is the subsample under the vote treatment; Announcement is the subsample under the Announcement treatment

here, and the presence of mixed communities may be driving the trust downward with respect to BJG. The net effect is negative.

As for AR, it is noteworthy from Fig. 4 that mean levels in the Land-Granted group are on a par with those from Rural, thus confirming our H4, while the Non-Land-Granted group has levels of AR even inferior to Urban. The null hypothesis of absence of difference between groups is rejected (4.46, $p<0.01$). The level of AR in the Non-Land-Granted group is significantly different from both Rural ($p<0.01$) and Urban ($p=0.06$). Hence, the intervention of land restitution appears to be associated with a large increase in trustworthiness.

In Table 7, we report the results of OLS regressions on the pooled data from the three samples. The effect of land restitution on AR appears robust. We run the test to compare the coefficient: the most important result is the catch up by beneficiary of the restitution with respect to level of trust and trustworthiness of rural and not exposed to violence sample. The difference between rural and urban participant is interesting per se and confirm the evidence from the literature. In SOM, Sect. 3.2, Table 9, we report also Ordered Logit Regression that confirm these results.
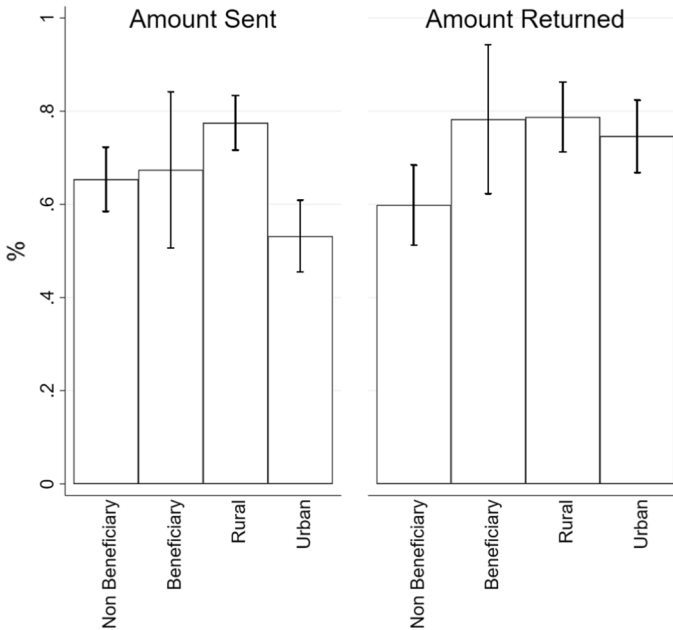
Fig. 4 Trust and trustworthiness among displaced and restored participants, compared with rural and urban Colombia. *Note*: See notes to Tables 1 and 3 for definition of variables. *Rural* is the sample in BJG, while *Urban* is a sample from Bogotá

5 Discussions and conclusions

The goal of this paper was to assess the impact of land restitution on interpersonal trust and trustworthiness in rural communities that had been displaced because of civil conflict in Colombia. We also assessed to what extent communities exposed to violence, both repaired with land restitution and non-repaired, changed their level of trust and trustworthiness in presence of consultative voting, as it happens with rural non-victimized communities.

We used TG to measure trust and trustworthiness with a lab-in-the-field experiment. We introduced two treatments, one in which participants vote on their preferred course of action, and one in which they are informed of the results of a similar vote elsewhere, to disentangle the social norms effect of voting from the coordination effect of an announcement with a specific action. To assess the effect of land restitution we measured self-reported land granted status, and we used selection on observables and control for omitted variable bias as research designs.

The main result of the paper is that land restitution is indeed positively correlated with trustworthiness. Trust is instead unaffected by land restitution. It is noteworthy that this result does not hold when we consider other generic compensation from the government that substitutes land restitution—for instance monetary compensation. Hence, the restitution of land seems to be in itself essential to recover trustworthiness. The effect on trustworthiness appears large, as people who have been

granted land display levels of trustworthiness on a par with those of other rural non-displaced populations, while people who have not been granted land have levels of trustworthiness even inferior to those from an urban sample from Bogotá.

As mentioned in the introduction, our design has the advantage that the group who benefitted from the intervention—i.e. Land-Granted people—and those who did not—i.e. non-Land-granted people—are comparable in terms of life experiences—in particular for having been displaced—and also demographic characteristics (Table 2). Our design addressed one of the typical problems of studying the effect of an intervention on war victims, that is, that of finding an appropriate comparable sample to the one who benefitted from the intervention.

A possible explanation of the differential effect on trust and trustworthiness is that an important difference exists between the two notions. Unlike the decision as to whether to repay trust or not, the decision as to whether to trust or not involves significant uncertainty over the receiver's behavior, and thus over one's payoffs. In fact, it has been shown that risk aversion is a key component of trust in TGs, although other factors play a role (Bohnet et al., 2008). Conversely, the decision of the receiver of a TG is devoid of any risk because this is the final decision of the game. It has been argued elsewhere that trust and trustworthiness tap into different aspects of a person's motivations (Bicchieri et al., 2011).

With regards to assessing whether introducing a consultative democratic process in the TG would further increase trust and trustworthiness this was only partially the case, as one measure of trustworthiness was affected. There was no differential impact between beneficiary and non-beneficiary of the Law.

A possible conjecture to explain this overall failure to replicate our previous finding is that the real-life process of the organization of victims to which our sample has participated already shares many of the characteristics that the experimental manipulation was meant to convey. The same can be stated for many aspects of the Bill of Victims in which these participants have been involved. This process is democratic in character because it was based on extensive public consultation and was implemented by a democratically elected government. It removes unnecessary administrative burdens to displaced people—in particular shifting the onus of proof away from the claimant and onto the judiciary—thus improving the sense of inclusion. It is then possible to conjecture that the experimental manipulation became less effective and somehow redundant in a sample who have been going through a process with strong democratic content in real life. Involving people in process of consultative democracy is a broadly used instrument of social policy and of empowerment in developing communities, hence this result warrants further investigation.

We have to lay out the limitations of our approach. The status of having been granted land or not may not be deemed as fully exogenous, as it was obviously not randomly assigned as part of the experiment. Moreover, this variable is self-reported and could not be independently verified for the need to protect participants' anonymity. For these reasons, we are cautious in claiming a causal relationship between having obtained land and trustworthiness, because we cannot claim that acquiring land is an entirely exogenous process. Nonetheless, we are also confident that selection bias cannot fully account for our results, for three reasons. (1) It is indisputable that the process of review and evaluation of applications has some implicit

randomness, which could cause applications to have different durations. In other words, if two claimants with similar cases had started their application for land restitution at the same point in time, it is likely that they would receive the outcome from their application at different points in time due to exogenous randomness in the process—e.g. the two applications are evaluated by two different judges (assignment is random, indeed) who differ in their processing speed. (2) We get similar results using two research designs: selection over observables and sensitivity analysis for omitted variable bias. (3) The institutional context of the law, i.e. the presence of an administrative and a judicial phase, and the socio-demographic characteristics of the participants (victims in state of poverty with limited access to lawyers or other instruments to speed up the process) makes very unlikely that there is some selection with respect to individual preferences.

Due to the limited sample size, we had to include together those that were directly victimized and those that belonged to victimized households. Although this does not correspond to a different legal status, there may be different effects if the exposure to violence is direct or indirect. Similarly, it would be ideal to look for heterogeneous effects across the length of exposure to land restitution and separate those that will benefit at a certain point in time from those that will not access to land restitution. Finally, self-reported data are not administrative data. Further research with a larger sample and more accurate measurement of exposure to the Bill is certainly needed to draw more robust conclusions.

As mentioned in the introduction, several studies found a positive effect of exposure to violence on various measures of pro-sociality (Bauer et al., 2016). We have not replicated this result in our sample with respect to trust and trustworthiness. One reason might be that the experience of displacement and of exposure to violence happened two decades before our study. It is possible that the positive effects of exposure to war fades away with time. Another possibility is that displacement during an ongoing conflict is different from having experienced displacement and having survived an ended conflict. Displacement may have a negative effect on trust, because it breaks down the social network in which the individual was embedded before the relocation experience. This was also the result found by Barr (2003). The result may simply be explained by the measurement: in the rural environment transfer were within the same community, while here sessions were mixed.

In spite of the limitations of our study, we believe we have provided valuable and novel evidence on a broadly diffused policy intervention. Land restitution is a widely used economic reform in a variety of contexts, ranging from conflict resolution to economic reforms. We have been the first to examine the effects of this process while this is still ongoing, thus providing fresh evidence on its effects of policy-relevant individual characteristics such as trust and trustworthiness. These results are also policy relevant as they suggest that the slow pace of implementation of the process should be accelerated. The findings also suggest that restitution should be coupled with further measures to increase empowerment and political agency by victims, as per our inconclusive results of consultative voting.

Supplementary Information The online version contains supplementary material available at https://doi.org/10.1007/s10602-021-09339-5.

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