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Seasonal Scarcity and Sharing Norms

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Vojtěch Bartoš*

Abstract

How does scarcity affect individual willingness to share and willingness to enforce sharing from others? Sharing in poor communities gains importance as an insurance mechanism during adverse shocks, yet shocks make it costlier to share. I conducted repeated economic experiments in both a lean and a relatively plentiful post-harvest season with the same group of Afghan subsistence farmers experiencing annual seasonal scarcities. I separate altruistic motives from enforcement effects using dictator and third party punishment games. While altruistic sharing remains temporally stable, the enforcement of sharing weakens substantially in times of scarcity. Temporal norms fluctuations seem to drive the results.

Keywords: Afghanistan, Scarcity, Seasonality, Sharing, Social norms

JEL: C93, D63, I32, Z13

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1 Introduction

The poor face excessive risks in their everyday lives. They frequently rely on informal risk sharing, a well documented source of informal insurance in places lacking formal insurance (Townsend, 1994; Morduch, 1995). When formal enforcement mechanisms are missing, such sharing schemes are sustained by reciprocity, altruism, and informal enforcement (Foster & Rosenzweig, 2001; Ligon et al., 2002; Fafchamps & Lund, 2003). Informal risk sharing gains uppermost importance during periods of scarcity, when it also becomes most costly for those with resources to share. Even shorter periods of scarcity affect sharing adversely, as reciprocal exchanges decrease,[1] and extreme periods of scarcity may lead to complete break downs of sharing (Turnbull, 1972; Dirks, 1980). For example, a common Bengali word for famine translates literally as "alms are scarce". Should reciprocity weaken, the negative effects of scarcity on sharing can be counteracted by individual altruism and altruistic enforcement of sharing within village communities.[2]

Economic literature typically operates on the premise that preferences, as well as norms and their enforcement, are stable economic primitives. However, emerging literature reveals that human behavior may be influenced by scarcity beyond the role of liquidity constraints (Bertrand et al., 2004; Mullainathan & Shafir, 2013; Schilbach et al., 2016). Scarcity seems to make people less patient and more risk averse (Haushofer & Fehr, 2014), and limits their cognitive capacity (Mani et al., 2013). But does scarcity affect human behavior beyond the domain of preferences shaping individual choices, and does it extend to the interpersonal domain of social preferences? If so, does the behavioral effect contribute to increased informal risk sharing and consumption smoothing, or does it hinder it? Answers to these questions can contribute to our limited understanding of the malleability of prosociality and social norms.

In this paper I examine whether scarcity affects altruistic sharing, a crucial component of informal sharing behavior, and altruistic enforcement of sharing, a tool that helps sustain sharing behavior in many social interactions.[3] I study this question among a highly relevant

[1] Anthropological literature provides evidence for weakened reciprocal ties during periods of scarcity, as there are not enough resources necessary to support reciprocal exchanges (Moser, 1996; Devereux, 1999; González de la Rocha, 2001). Coate & Ravallion (1993) show the reduction of reciprocity under scarcity theoretically.

[2] Under enforcement, even non-altruistic individuals may be disciplined to share, thus increasing prosociality (Fehr & Gächter, 2000; Boyd et al., 2003; Fehr & Fischbacher, 2004a).

[3] Willingness to engage in costly third-party punishment in which materially unaffected individuals are willing to forego gains to punish unfair behavior has been documented in economic experiments (Fehr & Fischbacher, 2004b; Bernhard et al., 2006) and correlates positively with the level of altruistic sharing in a society (Henrich et al., 2006). The forms of punishment may range from physical attacks on non-cooperators, through gossip, all the way to ostracism of non-cooperators (Maier-Rigaud et al., 2010). All these are well documented in

population of small-scale farmers in rural Afghanistan. This remote rural society is exposed to dramatic aggregate and idiosyncratic seasonal shocks to consumption (NRVA, 2008). Such seasonal cycles affect a large share of the one billion subsistence farmers dependent on highly volatile harvests in Asia and Sub-Saharan Africa. The cyclical nature of agricultural production, together with limited insurance, credit and savings markets, and low quality storage technologies (Basu & Wong, 2015) exposes many to seasonal scarcities (Devereux et al., 2008; Khandker, 2012). Apart from seasonal migration (Bryan et al., 2014), sharing with others remains one of the few available coping strategies.[4]

By following the same group of farmers over time, I observe their sharing and enforcement behavior in a controlled experimental setting during a seasonal cycle of scarcity and one of relative abundance.[5] Understanding how altruistic sharing unfolds at different points of the agricultural cycle is critical for our knowledge of informal risk sharing dynamics. Similarly, willingness to engage in enforcement of sharing is essential in periods of adverse shocks when the probability of group survival decreases, such as during wars, famines, disasters, and periods of scarcity. But our understanding of whether and how sharing enforcement may change over even shorter periods of time is limited. This knowledge can be informative for designing effective social safety nets in poor communities both in developing and developed countries.

A major challenge in examining the role of altruism and enforcement in sharing behavior under scarcity is that income effects and reciprocity related issues such as the role of kinship, reputational concerns, and fear of retribution all act as potential confounds. Beyond these, using observational data or narrative evidence, it is virtually impossible to distinguish between reputation-driven third-party punishment driven by selfish motives from that driven by altruistic goals.

Incentivized experimental games are useful and well-established tools to measure social preferences. To test whether scarcity affects sharing and willingness to enforce sharing, I conducted a controlled lab-in-the-field experiment using a one-shot dictator game (an idea originally used

anthropology (Cronk et al., 2000), ethnography (Fessler & Navarrete, 2004) and economic history (Greif, 1993). Societies with punishment mechanisms are, from an evolutionary perspective, more competitive than societies in which punishment mechanisms are lacking (Gürerk et al., 2006).

[4]While food sharing is common in hunter-gatherer small-scale societies, informal sharing in more advanced communities may operate through provision of informal loans on flexible interest rates with flexible repayment dates (Collins et al., 2009).

[5]To answer different questions, others have examined behavioral changes by following the same individuals over seasonal cycles. Mani et al. (2013) document an adverse effect of seasonal scarcity on cognitive abilities in a population of Indian sugarcane farmers. Similarly, Behrman (1988) studies temporal dynamics in parental preferences over nutritional allocation between sons and daughters with exposure to seasonal scarcities using a different sample of rural Indians.

in Kahneman et al., 1986b) and a one-shot dictator game with a third party punishment option (Fehr & Fischbacher, 2004b). This allows me to examine the temporal stability of sharing behavior and of sharing enforcement among 207 subsistence farmers from 10 villages in northern Afghanistan. To overcome selection issues, I conducted two rounds of experiments with the same participants: the first during a lean season and the second during a post-harvest season. The panel structure of the data provides a unique opportunity to inspect within-subject behavioral changes in altruistic willingness to share and in willingness to engage in enforcement of sharing when exposed to a sizeable economic shock over which the farmers have little individual control. After the games, I administered a survey. Beyond demographic characteristics, the survey documents seasonal changes in financial, nutritional, and health outcomes of the participants, and it examines real life examples of sharing and risk-coping strategies.

It is not clear, ex ante, whether and how scarcity should affect altruistic sharing and sharing enforcement. On the one hand, assuming concavity of the utility function over income and consumption, the increasing donor cost of sharing under scarcity depresses sharing (Andreoni & Miller, 2002). On the other hand, scarcity increases neediness on the side of the receiver, resulting in increased willingness to share (Engel, 2011). Moreover, scarcity is associated with individuals' cognitive capacity being consumed by concerns. Such reduced cognitive capacity makes it more costly to deliberate when making decisions (Mullainathan & Shafir, 2013; Schilbach et al., 2016). So far, there is no consensus on whether prosociality, including sharing, is stronger when deliberated upon or when it must be acted upon spontaneously (Rand et al., 2012; Tinghög et al., 2013; Kessler & Meier, 2014). The arguments for enforcement of sharing behavior are analogous to the case of sharing, hence also potentially go in either direction.

Although Ostrom et al. (1999) plausibly argue that scarcity forces societies to organize more efficiently and to employ enforcement mechanisms facilitating sustainable resource use, a large body of literature documents the exact opposite. Scarcity of common pool resources leads to more free-riding in ground water usage (Varghese et al., 2013) and in fisheries extraction rates (Maldonado et al., 2009), and to increased anti-social behavior (Prediger et al., 2014). Periods of extreme food scarcities, such as famines or wars, are accompanied by break downs of cooperation (Dirks, 1980; Turnbull, 1972). Observational studies suggest that extreme shocks such as rainfall shortages and maritime disasters result in general acceptance of loosened ethical behavior (Oster, 2004; Miguel, 2005; Elinder & Erixson, 2012). This indicates that social norms may respond to temporal changes in the environment. Less dramatic but equally important for the present study, Wutich (2009) shows that social network activities drop off during dry seasons. Changes in sharing under scarcity occur even in parental treatment of their own children (Behrman, 1988). Although the papers above offer suggestive evidence of changes in sharing behavior with scarcity, they cannot distinguish which effects are driving their findings. In this paper I focus solely on the respective roles of altruistic sharing and of altruistic sharing enforcement, while I eliminate the potential confounds of alternative components of informal sharing studied extensively in earlier literature.

I find that, despite substantial changes in income, consumption, health, and perceptions of stress within individuals across the lean and post-harvest seasons, sharing, measured by the amounts passed in the dictator and the third party punishment games, remains unchanged at the aggregate level and fairly stable at the individual level, suggesting stability of altruistic sharing. In contrast, enforcement of sharing, measured by the willingness and the intensity of costly punishment of unfair allocations by monetarily uninterested third parties, is significantly weakened during the lean season. I provide evidence that the drop in punishment of non-desirable behavior is consistent with a change in village-level social norms rather than a shift in state-dependent individual preferences for engaging in enforcement, as the change in punishment behavior correlates with village-level, rather than individual-level, shock intensity.

To rule out potential confounds, I show that the study period shows stability in terms of local- and national-level political situation, incidence of natural disasters, and levels of local-level violence. I also performed the following robustness checks. First, to rule out the potential effect of the order in which the games were played, I recruited an additional sample of 288 subjects who played the games only once, either in the lean season or in the post-harvest season only. The results are quantitatively similar for this group. Second, the results also hold when I experimentally manipulate payoffs in the games to reflect seasonal changes in market prices. Third, the change in punishment behavior can be attributed either to pure income effects, increased uncertainty about the intentions of others, increased grievances suppressing the expression of altruistic punishment, or to increased leniency in the use of punishment. I rule out the role of income effects, argue against the increased leniency, and provide limited evidence favoring the role of grievances. Fourth, the observed results are also reflected in the beliefs of others. Fifth, the results are quantitatively similar for the two ethnically different groups represented in the study—one made up of predominantly Sunni Tajiks and the other of predominantly Shia Hazaras—allowing for more generalizable statements about the findings presented. Lastly, behavior in the experiments correlates with real-life charitable giving.

This paper is related to several streams of literature. First, it contributes to the literature on endogeneity of social preferences. A closely related study is that of Fisman et al. (2015), who report increased selfishness in cohorts of UC Berkeley students after the Great Recession and in a laboratory experiment simulating an economic downturn. Their study focuses mainly on altruistic sharing and not on the enforcement side. Moreover, the environment they study is very different from mine, as the consequences of the shock are less severe, because a formal social safety net exists in the US context. Another related study is the paper by Prediger et al. (2014), who document higher anti-social behavior among Namibian villagers in an area with greater resource scarcity. They argue that these preferences have formed over long periods of time. Unrelated to scarcity, others also study long term developments of preferences, either through individual lives (Fehr et al., 2008; Almås et al., 2010), or across generations, leading to marked differences across cultures (Henrich et al., 2010). In contrast, my paper analyzes possible dynamics over short-term periods of scarcity. In terms of shorter-term temporal stability of social preferences, Carlsson et al. (2014) and Volk et al. (2012) show that cooperation is stable over a five year and over a five month period, respectively. Preferences have also been shown to be relatively temporarily stable in the domains of risk (Andersen et al., 2008) and time preferences (Meier & Sprenger, 2015).[6] All these studies, however, examine preference stability in stable environments. The exception is an emerging literature on the effects of conflict on social preferences (Bauer et al., 2016). With conflict, the threat to society is an external group against which increased social cohesion and parochialism serve as a defense strategy. Contrary to that, my paper examines an environment exposed to substantial, yet to some extent expected, environmental shocks, where the shock comes from within the society and is caused by nature. It shows remarkable stability of altruistic sharing preferences.

Next, this paper speaks to the sparse literature examining temporal dynamics of social norms and norm enforcement using economic experiments. Only Bursztyn et al. (2017) document short-term changes of social norms. Literature is similarly limited in documenting the dynamics of the willingness to enforce sharing. Gneezy & Fessler (2012) examine changes in enforcement of cooperation during wartime. They show that second-party enforcement of cooperation intensified during the Israeli-Hezbollah war compared to a prior period or in the

[6] Chuang & Schechter (2015) offer a comprehensive review of all studies examining stability of risk, time, and social preferences.

immediate aftermath. Similarly to the literature on conflict and parochial prosociality, the threat to the community in their setting came from an identifiable external threat. Further, second-party enforcement combines altruistic punishment of improper conduct, which I study here, with selfish retaliatory behavior. The results of this paper are novel in that they show that seasonal scarcity reduces willingness to engage in enforcement of sharing. This effect seems to be driven by temporal norms fluctuation.

The rest of the paper is organized as follows. Section 2 describes the sample selection, setting, experimental design, and procedures. Section 3 presents the main results, while Section 4 provides a discussion, documents the robustness of the main results, and rules out alternative explanations. Section 5 concludes.

2 Experimental Design

2.1 Sample Selection

The participants were recruited for the experiments in 10 randomly selected villages in the Marghzar and Amrakh areas of Zari district in Balkh province, northern Afghanistan, a remote area at high elevation. With more than 60 percent of the population living below the poverty line, Balkh is one of the poorest provinces in Afghanistan (NRVA, 2008). The vast majority of the local population subsists on agricultural production or agricultural labor. All land-owning farmers, a maximum of one adult person per household was allowed, were invited. The head of the household—the main bread winner—was strongly preferred. Due to cultural constraints, only males were invited.

To answer the question whether sharing and enforcement of sharing vary with exposure to resource scarcity I exploit the fact that farmers in this area face annual seasonal food shortages. I conducted 20 experimental sessions in 10 villages with 291 adult male farmers in the lean season of April 2013 and an additional 20 sessions in the same villages with 207 participants who the mobilization team managed to contact also in the post-harvest season in October 2013.

Table 1: Descriptive Statistics Including the "Single-Round" Subjects

	Both seasons		Lean season only		Post-harvest season only		T-test (1)-(3)		T-test (1)-(5)	
	Mean	SD	Mean	SD	Mean	SD	Difference	t-value	Difference	t-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age	38.83	(15.49)	37.25	(15.51)	33.50	(16.00)	-1.58	(-0.79)	-5.32***	(-3.43)
Schooling (completed years)	2.97	(3.82)	2.19	(3.16)	3.14	(4.14)	-0.78	(-1.65)	0.18	(0.45)
Can read a letter (d)	0.58	(0.49)	0.54	(0.50)	0.44	(0.50)	-0.04	(-0.66)	-0.14***	(-2.93)
Number of household members	9.66	(4.69)	9.20	(4.20)	8.60	(3.90)	-0.46	(-0.78)	-1.06**	(-2.50)
Household head (d)	0.83	(0.38)	0.77	(0.42)	0.61	(0.49)	-0.06	(-1.13)	-0.22***	(-5.19)
Not married (d)	0.11	(0.32)	0.13	(0.34)	0.33	(0.47)	0.02	(0.48)	0.23***	(5.77)
Married to a single wife (d)	0.71	(0.45)	0.69	(0.47)	0.61	(0.49)	-0.02	(-0.33)	-0.21***	(-4.82)
Married to multiple wives (d)	0.18	(0.38)	0.18	(0.39)	0.05	(0.22)	0.00	(0.00)	-0.02	(-0.99)
Daughters below 15[a]	1.93	(1.66)	1.95	(1.39)	1.54	(1.51)	0.02	(0.07)	-0.2	(-1.04)
Sons below 15[a]	2.13	(1.60)	1.93	(1.21)	1.82	(1.67)	-0.20	(-0.85)	0.03	(0.18)
Years living in village	36.98	(16.59)	34.95	(16.38)	32.01	(16.56)	-2.03	-(0.95)	4.25	(0.90)
Sunni (d)	0.51	(0.50)	0.51	(0.50)	0.49	(0.50)	0.00	(0.07)	-0.02	(-0.34)
Irrigated land (in jiribs)	4.47	(7.36)	3.58	(3.79)	3.74	(5.54)	-0.89	(-1.05)	-0.73	(-1.13)
Rainfed land (in jiribs)	10.81	(18.68)	9.67	(14.36)	9.76	(22.06)	-1.14	(-0.50)	-1.05	(-0.52)
Observations	207		84		204		291		411	

Notes: Means reported in Columns 1, 3, and 5. Standard deviations in parentheses in Columns 2, 4, and 6. Column 7 reports the difference between the means of the respective characteristics for the sample of participants in both seasons and the sample of participants in the lean season only. Column 9 reports the difference between the means of the respective characteristics for the sample of participants in both seasons and for the sample of participants in the post-harvest season only. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. Columns 8 and 10 report t-values of a two-sided t-test. [a]Questions asked to the subsample of N=139 Players A and C in both periods.

In the post-harvest season I also recruited an additional 212 new participants to substitute for the 84 participants who dropped out and to provide a sample of "single-round" participants who participated only in the second, post-harvest round to control for potential order effects. The selection procedure was the same as in the lean season round, and the differences between the samples presented in Table 1 are discussed in Subsection 4.5. Despite some differences between the respective samples in observable characteristics, I show that the behavior in games does not differ across samples and importantly, in the main analysis I only focus on the behavior of those who participated in both lean and post-harvest season rounds. Each session was conducted with 12 or 15 participants. The participation in each round was voluntary and the participants could leave at any time. All participants decided to complete all tasks within each round.

Demographic characteristics for the sample of the 207 participants participating in both rounds are presented in Columns 1 and 2 of Table 1. Half of the sample is composed of Sunni Muslims (51 percent) mainly of Tajik ethnic origin and the other half of Shia Muslims of predominantly Hazara ethnic origin, living in almost perfectly segregated areas.[7]

It is important to note that 84 subjects who participated in the first, lean season round did not participate in the second, post-harvest round. Out of them 62 (74 percent) migrated either to Iran, to Mazar-e-Sharif, the provincial capital, Kabul, the capital city, or to another village for work. Only the remaining 22 (26 percent) did not show up either because of working elsewhere at the time of the experiment, being sick, or attending a wedding at the time of the assigned experimental session. Reassuringly, no one declined to participate due to reasons related to the experiment. Note that selective attrition would systematically bias the results only if it was correlated with the stability of sharing and with willingness to engage in third-party sharing enforcement.

2.2 Seasonal Effects and Other Events During the Study Period

There is vast evidence that farmers in developing countries are exposed to substantial fluctuations in incomes and consumption over the year (Devereux et al., 2008; Khandker & Mahmud, 2012). Table 2 presents the seasonal differences in observable characteristics among the sample of participants in both seasons. The data show that seasonality does indeed matter. The participants' average monetary income—reported as per OECD equivalence scaled household

[7] I do not control for religion in the analysis because individual religious affiliation is almost perfectly correlated with village affiliation (perfectly in the case of the sample used for the main analysis). I use village fixed effects in regressions that thus control for possible effects of religion too.

member—in the previous month in the lean season is only 69 percent of the post-harvest season income. Also, 59 percent of participants reported having no monetary income in the lean season compared to 38 percent of participants in the post-harvest season. Smoothing consumptions with own income across seasons is unlikely due to almost non-existent monetary savings in the area. Storage technologies cannot be relied upon either, as less than 2 percent of the subjects use a modern cold storage technologies, while over 90 percent store their produce inside their houses or in holes dug in the earth.

Meat is consumed less frequently during the lean season. The share of people in debt increases from 70 percent in the post-harvest season, already high, to 86 percent in the lean season. The share of subjects lending money to others decreases from 39 percent in the post-harvest season to 29 percent in the lean season.[8] Further aggravating the severity of the lean season, the participants report being much more likely to be unable to work due to injury or illness, they feel generally more stressed, and are affected by shocks such as crop pests and diseases, livestock diseases, as well as human diseases. Irrespective of the season, 25 percent of the participants report that someone from their household has been working outside of the village.

Figure A1 shows that the participants are well aware of the seasonal swings over the year. Responding to a question to select three months of a year that are generally most difficult for them to cope with and three months of a year that are generally least difficult for them to cope with, most participants perceive the winter and the spring months (the lean season) as the most difficult to live through and the summer and the autumn months (the harvest and the post-harvest season) as the best months of a year.

[8] Many people in developing countries are lenders and borrowers at the same time (Collins et al., 2009).

Table 2: Seasonal Effects—Individual Time-Variant Characteristics

	Lean season		Post-harvest season		T-test	
	Mean	SD	Mean	SD	Difference	t-value
	(1)	(2)	(3)	(4)	(5)	(6)
Cash earned in past 30 days (ths AFN)[a, b]	0.35	(0.79)	0.51	(0.62)	-0.16*	(-1.93)
Cash earned in past 30 days: selling food (ths AFN)[a, b]	0.15	(0.66)	0.31	(0.54)	-0.16*	(-2.18)
Cash earned in past 30 days: day labor (ths AFN)[a, b]	0.10	(0.26)	0.08	(0.28)	0.01	(0.38)
Perceived income situation[b]	-0.40	(0.67)	-0.03	(0.61)	-0.37***	(-5.89)
Meat eaten in past 7 days (times)[a]	0.73	(1.04)	0.98	(1.00)	-0.25*	(-2.05)
Currently saves money (d)[a]	0.07	(0.26)	0.04	(0.20)	0.03	(1.02)
Currently in debt (d)[a]	0.86	(0.34)	0.70	(0.46)	0.16***	(3.38)
Currently providing loan (d)[a]	0.29	(0.45)	0.39	(0.49)	-0.10*	(-1.79)
Unable to work in past 30 days (days)	7.85	(10.09)	2.25	(6.83)	5.59***	(6.61)
Perceived stress score[c]	5.40	(1.99)	3.97	(1.15)	1.43***	(8.96)
Unusually high level of crop pests & diseases (d)	0.11	(0.32)	0.02	(0.14)	0.09***	(3.84)
Unusually high level of livestock diseases (d)	0.28	(0.45)	0.11	(0.32)	0.17***	(4.43)
Unusually high level of human disease (d)	0.50	(0.50)	0.20	(0.40)	0.30***	(6.70)
Participated in a dispute in past 30 days (d)	0.14	(0.35)	0.08	(0.27)	0.07**	(2.20)
Participated in a voluntary activity in past 30 days (d)	0.51	(0.50)	0.65	(0.48)	-0.14***	(-2.81)
Member of any village association now (d)	0.31	(0.46)	0.17	(0.38)	0.14***	(3.25)
Some household member migrated for work (d)[a]	0.25	(0.44)	0.24	(0.43)	0.01	(0.17)
Observations	207		207		414	

Notes: Means reported in Columns 1 and 3. Standard deviations in parentheses in Columns 2 and 4. Column 5 reports the difference between the means of respective characteristics in the post-harvest season and the lean season. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. Column 6 reports t-values of a two-sided t-test. [a]Questions asked of the subsample of N=139 Players A and C. [b]Cash earned by household head per OECD equivalence scaled household member. [c]Indicating whether the individual perceives his current income to be much worse (-2), worse (-1), same (0), better (+1), or much better (+2) relative to other fellow villagers. [d]A short version of the Cohen et al. (1983) Perceived Stress Scale used: scale ranges from 0 to 8, 8 indicated the highest level of perceived stress.

As political events, natural disasters, and violent acts have all been shown to affect human behavior and could hence act as confounds to scarcity studied here, I examine differences on these domains across the two seasons. First, no elections on the national or local level happened at or around the time when the experiment was implemented. Second, there was a single incidence of a major natural disaster in the area. OCHA Field Offices and IOM Afghanistan Humanitarian Assistance Database reports a flash flood that hit neighboring districts of Kishindih and Sholgara on April 23, 2013, the last day of the lean season experiments, did not cause any material, let alone human losses in the area studied. Third, I use declassified precisely geolocated and timestamped violence data from the International Security Assistance Forces (ISAF) Combined Information Data Network Exchange (CIDNE) database, a most comprehensive source of violent incidents in Afghanistan. I examine the incidences of main categories of violence within a radius of 40 km from the center of either of the villages studied (see map in Appendix Figure A3). There were exactly three instances of direct combat and two instances of improvised explosive device explosions in the period of six months prior to the end of each respective experiment round. The closest incident from March 14, 2013 was reported 7km away from the nearest study village, while a second nearest incident from September 29, 2013 was reported 15km away from the nearest study village.[9] To overcome possible "calendar effects", I conducted the experiments outside of major Islamic holidays, harvest time, or bazaar days.

2.3 Experimental Tasks

Each experimental session consisted of two tasks. A one-shot dictator game (DG; Idea originally used in Kahneman et al., 1986a) and a one-shot dictator game with a third party punishment option, the third-party punishment game (TPPG; Fehr & Fischbacher, 2004b; Bernhard et al., 2006). To control for order effects I randomly manipulated the order of tasks on the session level. The participants were rematched after each task and across lean and post-harvest season rounds in order to avoid strategic behavior and possible reciprocal concerns. After the experiment each participant was surveyed.

The DG allows me to examine the temporal stability of individual sharing behavior in the absence of confounds of kinship, reciprocity, reputation building or the fear of social sanctioning

[9] Several papers use the same ISAF dataset (e.g., Beath et al., 2012; Callen et al., 2014). All violent events lie outside of the 5km radius, beyond which the correlation between violence and risk preferences reported in Callen et al. (2014) breaks down. The effect of a large-scale development program on reducing violent acts in non-eastern districts in Afghanistan finds strongest long-term effect within a radius of 9km (Beath et al., 2012). All effects disappear beyond a 10km radius.

for non-desirable behavior. In this quasi-game a dictator, Person A (PA), divides a given endowment (10 experimental currency units, ECUs) between himself and a passive receiver, Person B (PB). PB is also one of the participants in the same experimental session as PA, but he receives no endowment and only learns the final allocation of money. The game allows for 11 strategies, as only whole units can be passed. The allocation depends entirely on PA's own willingness for unconditional sharing under the veil of anonymity, as his identity is never revealed to PB. Thus, the individual is motivated to reveal his true sharing preferences. For simplicity, the ECUs in the game are represented by money slips evoking 20 AFN banknotes, not by real money. The conversion rate is 1 ECU = 20 AFN $\approx$ 0.4 USD.[10]

In order to test the temporal stability of sharing enforcement, I administer a TPPG. The game allows a monetarily unaffected third party—Person C (PC)—to observe the sharing behavior of a dictator—PA—in a DG where both PA and PB are aware of PC's presence. First, PA decides how much of the 10 ECUs of his endowment to pass to PB who has no endowment as in the DG described earlier. PB only learns PA's final decision and has no control over it. Second, PC may decide to punish the dictator for his behavior but only at a cost to himself. Each PC is endowed with 5 ECUs and he can either refrain from punishment or pay 1 or 2 ECUs to subtract 3 or 6 ECUs of PAs payoff, respectively. This distribution ensures that in a situation when 1) PA behaves as an egalitarian and 2) PC decides not to punish such behavior, all players leave the experiment with 5 ECUs (as in Fehr & Fischbacher, 2004b). However, PCs do not observe PAs' actual behavior. Rather, I elicit their reaction to all possible behaviors of PA using the strategy method.[11] PC's willingness to pay to punish provides me with a direct measure of willingness to engage in altruistic enforcement of sharing, typically attributed to enforcement of sharing norms in the literature. The variable of interest is the minimum acceptable PA offer to PB that is not punished by PC, which I denote as minimum acceptable offer (MAO; as in Henrich et al., 2006).

On top of the actual games, I also administered incentivized belief questions about the behavior of other players in order to understand whether the behavior in games is also reflected in beliefs of others.[12] Each of the three belief questions yielded an extra ECU for a correct

[10] According to the World Bank, the price level ratio of PPP conversion factor (GDP) to market exchange rate is equal to 0.3 for the period when the experiment was run.

[11] Brandts & Charness (2011) survey 29 studies that directly compare the strategy method to direct-response elicitation. While in the majority of cases no difference between the two methods is found, the only exception is games with punishment. Out of four studies including a punishment option surveyed, three observed lower levels of punishment when the strategy method was used. Reassuringly, in all cases the treatment effects were detected using both methods and the effects were in the same direction.

[12] Specifically, PBs and PCs were asked how much they think the PA matched with them sent in the DG and

answer.

2.4 Procedures

The experiments were announced one day in advance. The villagers were informed that a "task" requiring a commitment of four hours of their time will be conducted in their village for which they will earn at least 100 AFN (approximately 2 USD) as a show-up fee, but possibly more.[13] All interested farmers were gathered in a community center (a guesthouse, mosque, or a village leader's house) the morning just before the first session. The location within a village was the same across the two seasons. If more villagers showed up for an experimental session than could be accommodated, the mobilizing team either invited them for another session if there was one conducted in the same village or ran a lottery in which the participants were selected by chance. Consequently, the actual participants randomly picked an ID number, which determined their role in the experiment. The numbers of participants by role, village, and round are reported in the Appendix Table A2.

As is common in economic experiments carried out with low-literacy subjects, the instructions were first explained in a group using practical examples and visual aids (See Appendix Figure A4), and only then were the actual experiments carried out with the subjects individually (See Appendix Figure A5).[14] Before making their actual decisions, all participants were shown several examples, were allowed to practice several scenarios themselves, and were then asked to answer several control questions. The research assistants explained the task until the participants fully understood and the experiments were carried out only after participants' full comprehension. Only one participant failed to pass the comprehension test due to hearing problems, not the inability to comprehend the task.

in TPPG, and whether they believe that the majority of PAs would be punished for a transfer of 0 ECUs. The PAs were asked about the modal DG and TPPG transfer of all PAs within a particular session, and whether they believe that their actual TPPG transfers would not be punished by a PC they are matched with.

[13]An average daily wage of a casual laborer is 150 AFN, but it is not possible to find work every day in the area. During the off-season work is particularly scarce. Importantly for my study, the size of the initial endowment does not seem to influence the relative transfers in dictator games (Engel, 2011) or punishment games (Kocher et al., 2008) to the extent that might invalidate the results of the present study. In order to validate this claim, I conducted several experimental sessions with stakes increased by 50 percent in the 2013 lean season only to find that the main results do not differ from those for games with the original endowment size (See Appendix Table A1). The 50 percent increase reflected the reported 50 percent increase in prices of most common consumption goods during the lean season compared to the post-harvest season.

[14]The instructions and procedures I used are inspired by Bernhard et al. (2006) and by Henrich et al. (2006). Instructions are available in the Appendix C. The instructions were presented orally in the local language, Dari, and were back-translated to English.

Table 3: Seasonal Effects—Experimental Outcomes

	Lean season		Post-harvest season		T-test	
	Mean (1)	SD (2)	Mean (3)	SD (4)	Difference (5)	t-value (6)
Player A (Dictator)						
DG transfer (ECU)	3.03	(1.74)	3.22	(1.85)	-0.19	(-0.62)
TPPG transfer (ECU)	2.87	(1.74)	3.10	(1.82)	-0.24	(-0.77)
Belief: DG transfer in session, mode (ECU)	2.94	(1.84)	3.04	(1.60)	-0.11	(-0.35)
Belief: TPPG transfer in session, mode (ECU)	2.93	(1.63)	3.06	(1.67)	-0.13	(-0.44)
Belief: my TPPG transfer will not be punished (d)	0.72	(0.45)	0.71	(0.46)	0.01	(0.13)
Observations	68		68		136	
Player B (Receiver)						
Belief: DG transfer, matched PA (ECU)	3.18	(2.03)	3.63	(1.61)	-0.46	(-1.45)
Belief: TPPG transfer, matched PA (ECU)	3.66	(1.84)	3.68	(1.41)	-0.02	(-0.07)
Belief: most PCs punish zero TPPG transfer (d)	0.68	(0.47)	0.78	(0.42)	-0.10	(-1.35)
Observations	68		68		136	
Player C (Punisher)						
MAO (consistent responses; ECU)[a]	1.35	(1.51)	3.03	(1.87)	-1.68***	(-5.48)
Punish zero TPPG transfer (consistent responses; d)[a]	0.62	(0.49)	0.94	(0.25)	-0.32***	(-4.61)
Belief: TPPG transfer, matched PA (ECU)	3.15	(1.71)	3.41	(1.56)	-0.26	(-0.91)
Belief: most PCs punish zero TPPG transfer (d)	0.65	(0.48)	0.79	(0.41)	-0.14*	(-1.88)
Observations	71		71		142	

Notes: Means reported in Columns 1 and 3. Standard deviations in parentheses in Columns 2 and 4. Column 5 reports the difference between the means of respective characteristics in the post-harvest season and the lean season. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. Column 6 reports t-values of a two-sided t-test. DG stands for the dictator game, TPPG stands for the third party punishment game, MAO stands for TPPG minimum acceptable offer. [a]Values reported for a subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent MAO.

Communication in all rounds of experiments was not allowed and all tasks were strictly anonymous. Only one task was randomly selected for the payment to avoid strategic play across experiments. This procedure was revealed to the participants in the instructions.

Although the participants received their payments at the end of each experimental session they did not receive any feedback on their actions and the actions of other players. Average earnings were about 190 AFN including the show-up fee (100 AFN). In order to prevent post-play retaliation, all payments were carried out in private and this was communicated to the subjects before the play.

3 Results

In this section I first discuss both aggregate and individual-level temporal stability of sharing behavior. Then I present the behavioral change in willingness to enforce sharing over time. In the main results discussed in this section I restrict the sample to the panel of farmers who participated in both lean and post-harvest season rounds.

3.1 Temporal Stability of Sharing Behavior

First, I present the aggregate results of sharing behavior. Second, since the design of the experiment allows me to observe the sharing behavior within the same individual across seasons, I present the results on the within-subject stability of sharing.

Does the aggregate sharing behavior differ across seasons? Columns 1 and 3 in Table 3 show that in the DG the PAs transferred on average 3.03 ECUs to PBs in the lean season compared to 3.22 ECUs in the post-harvest season, the difference being statistically insignificant (Wilcoxon matched-pairs signed-ranks test, WSRT: p=0.28, n=68). Similarly for the TPPG, I find an average transfer of 2.87 ECUs in the lean season and 3.10 ECUs in the post-harvest season, the difference being again statistically insignificant (WSRT: p=0.40, n=68).

I test the temporal stability of sharing behavior using the following regression model:

$$T_{it}^{g} = \beta LS_{it} + \gamma X_{it} + \sum_{v=1}^{10} \delta_v D_{iv} + \varepsilon_{it} \tag{1}$$

where T_{it} is the amount passed by the individual i in the experimental game $g = \{DG,\ TPPG\}$ in the period t, which is either the lean season or the post-harvest season. LS_i is the treatment variable equal to 1 in the lean season, X_{it} is a set of individual characteristics. Village level

Table 4: Effect of Seasonality on DG and TPPG Transfers, and on TPPG MAO

Dependent variable	DG transfer	TPPG transfer	TPPG MAO
	(1)	(2)	(3)
Lean season	-0.19	-0.24	-1.70***
	(0.23)	(0.28)	(0.32)
Observations	136	136	123
R-squared	0.82	0.79	0.71
Bonferroni-adjusted *Lean season* p-value	1.00	1.00	0.00

Notes: OLS coefficients. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Column 3 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). This column shows results for a subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent MAO. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity.

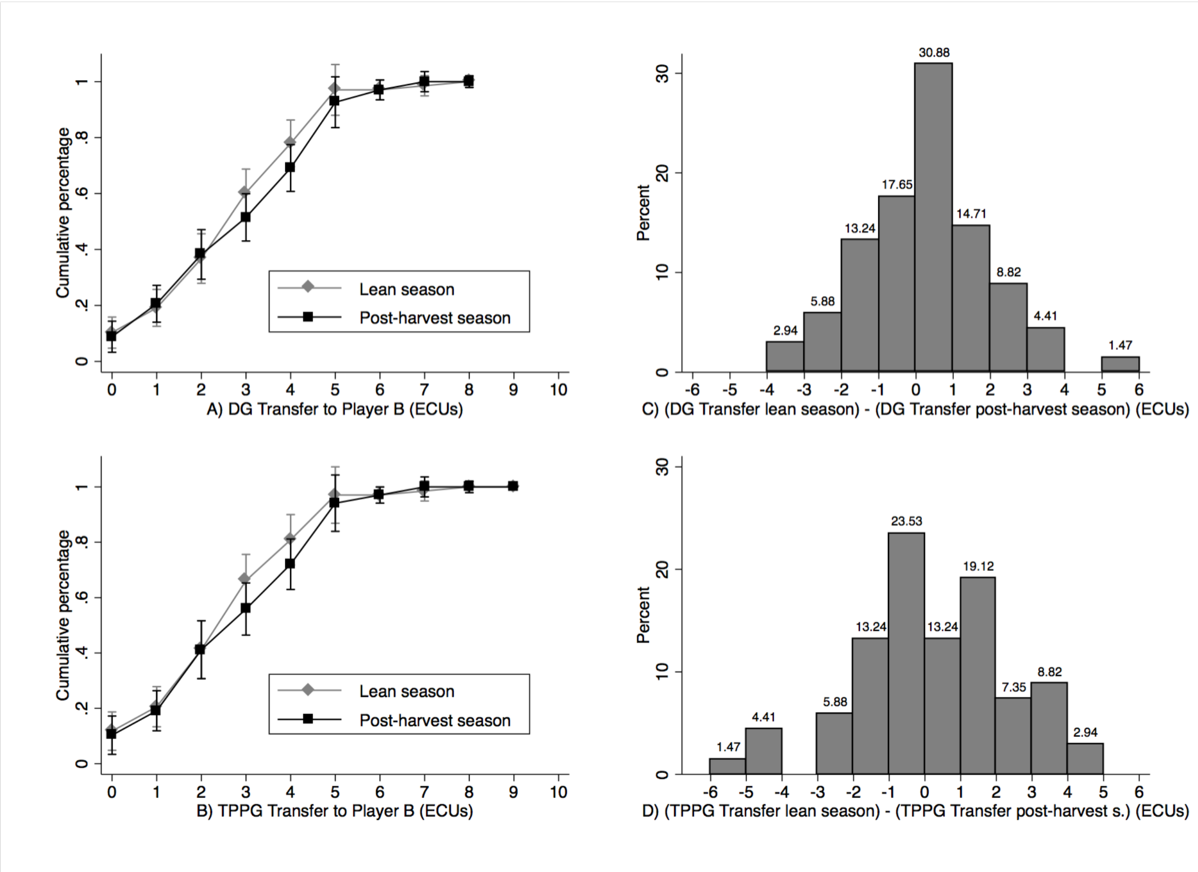
fixed effects are controlled for using a full set of village dummies D_{vi}, and ε_{it} is the error term. All regressions control for village-specific effects, as the village fixed effects explain about 16 percent or 13 percent of the variance in the DG or the TPPG transfers, respectively (See Table A3). Constant is excluded to avoid perfect multicollinearity. Standard errors are clustered at the individual level.

Columns 1 and 2 in Table 4 show that the behavior across seasons remains stable both in the DG and the TPPG when using a regression framework. The variable *lean season* is not statistically significantly different from zero implying that the sharing behavior does not change across seasons for either the DG or the TPPG.[15]

Figure 1 examines the cumulative distributions of respective amounts transferred in the DG (Panel A) and the TPPG (Panel B) across the lean and the post-harvest season rounds. Apart from the difference in the frequency of PAs sending 3 ECUs both in the DG (difference in frequencies across rounds borderline significantly different from zero, p=0.09) and the TPPG (marginally insignificant, p=0.13), the distributions are identical, a necessary condition for stability of preferences. The Epps-Singleton Two-Sample Empirical Characteristic Function (ESCF) test cannot reject the equality of distributions for neither the DG (p=0.22), nor the

[15]The results are robust to using ordered probit, which takes into account the discrete nature of the dependent variables (See Appendix Tables A4 and A5), and to controlling for different composition of participants in sessions across rounds due to attrition (See Appendix Table A6).

Figure 1: Distributions of DG and TPPG Transfers Across Seasons



Notes: In Panels A and B, the figure shows the cumulative distribution of transfers from Player A (dictator) to Player B (passive receiver) in ECUs (allowed between 0 and 10) in A) the dictator game (DG) and B) the third party punishment game (TPPG) across the PAs participating in both rounds (n=68). The cumulative distribution of lean season transfers is depicted in grey, the cumulative distribution of post-harvest season transfers is depicted in black. The error bars represent 95 percent confidence intervals. In Panels C and D, the figure shows the distributions of differences between the transfers in the lean season and the post-harvest season in C) the DG and D) the TPPG within a participant. Transfer differences are in ECUs (the possible range is from -10 to 10).

TPPG (p=0.34).[16]

Finding 1: *On the aggregate level sharing behavior in the DG and the TPPG does not vary with short term exposure to scarcity.*

The panel structure of the data allows me to inspect the within-subject stability of sharing. The main advantage is that this substantially improves precision of results through increased statistical power. In total, 68 PAs were successfully tracked. These participants were exposed to the same experimental procedure in both the lean season and in the post-harvest season, six months later. I examine the correlations in sharing behavior across seasons and individual changes in sharing behavior. First, I describe the stability of sharing behavior in the DG and then I comment on the stability of behavior in the TPPG.

Panel C of Figure 1 presents the histogram of *changes* in individual behavior in the DG, specified as a difference between the lean and the post-harvest season transfers. It reveals that more than 30 percent of individual decisions in the DG remained constant across both seasons. Moreover, almost 65 percent of decisions remained within a change of one ECU or 10 percent of the PAs endowment. The correlation between DG transfers in the lean season and in the post-harvest season is ρ=0.52 (p<0.01). Such stability is relatively high compared to other studies examining temporal stability of preferences.[17] The data allow me to detect a minimum detectable effect of 0.37 ECUs (or an effect equivalent to 12 percent of a lean season DG transfer average).[18]

It is possible that the result presented here as a proof of temporally stable sharing behavior could arise as a confound, and would arise even if the DG choices were drawn randomly. I can

[16]The distribution of DG transfers fits between the classifications of the developing country and an indigenous society subject pool classification used in the DG meta study by Engel (2011). The Afghan PAs are much more likely to pass positive amounts to PBs than the Western subjects (91 percent versus 67 percent in the Western societies, 81 percent in the developing countries and 95 percent in the primitive societies), slightly less likely to pass equal share (21 percent versus 20 percent Western, 27 percent developing and 28 percent primitive societies), but no one in this sample passes the entire pie unlike 5 percent of the Western subjects and 1 percent both in developing countries and in primitive societies. Similar comparison for TPPG transfers is not possible, since the game has not been used so extensively and no effort to conduct a meta-analysis has been made.

[17]Literature in psychology examines the stability of preferences in much more detail than economics does. Surveys examining stability of single cross-situational measures usually report temporal stability in a range between 0.2 to 0.3 (see e.g. Block, 1983; Jessor, 1983) and perceives such correlations as indicating relatively stable preferences, while within this interval. Similarly to my findings, Meier & Sprenger (2015) report a correlation of 0.5 in individual time preference choices in an experiment repeated twice over a year with the same set of subjects and label such correlation as high.

[18]I assume a two sample two-sided mean comparison test with equal proportion of subjects across the two periods, $\alpha = 0.1$, $1 - \beta = 0.8$. To account for the within-subject nature of the design, I adjust the population size by dividing the number of participants required in a between subject design by $2/(1 - \rho)$ as in Maxwell & Delaney (2004, p.561). This reduces the required sample size substantially.

rule out this possibility, as each choice from the entire set of possible transfers would have to be represented uniformly, which is clearly not the case without any need for statistical testing. On the other hand it is well plausible that due to the limited choice space observed in the cumulative distribution of choices in Figure 1 with the majority (75 percent) of PAs transferring between 2 and 5 units, it could be that the temporal stability of the sharing behavior is an artefact of the experiment. In order to rule out this possibility, I conduct an exercise in which I randomly assign choices from the set of all realized transfers in the post-harvest season to PAs. After reshuffling the PA choices 10,000 times, the average number of equal choices across both seasons is around 15.6 percent, and 42.5 percent of decisions remain within a change of one unit, much lower than the actually observed values.

Next, I discuss the within-subject stability of TPPG results. Although statistically significant, the correlation of individual behavior in the TPPG across seasons is much lower than the correlation discussed in case of the DG, ρ=0.22 (p=0.07). Yet even such correlation would be generally accepted as fairly stable over time in the psychological literature (see footnote 17). Panel D in Figure 1 shows that only 13 percent of individuals sent equal amounts in both seasons, even though the share of individuals with changes within a margin of one ECU reaches over 55 percent. Given the weaker correlation, the data only allow me to detect a minimum detectable effect of 0.48 ECUs (or an effect equivalent to 17 percent of a lean season TPPG transfer average).

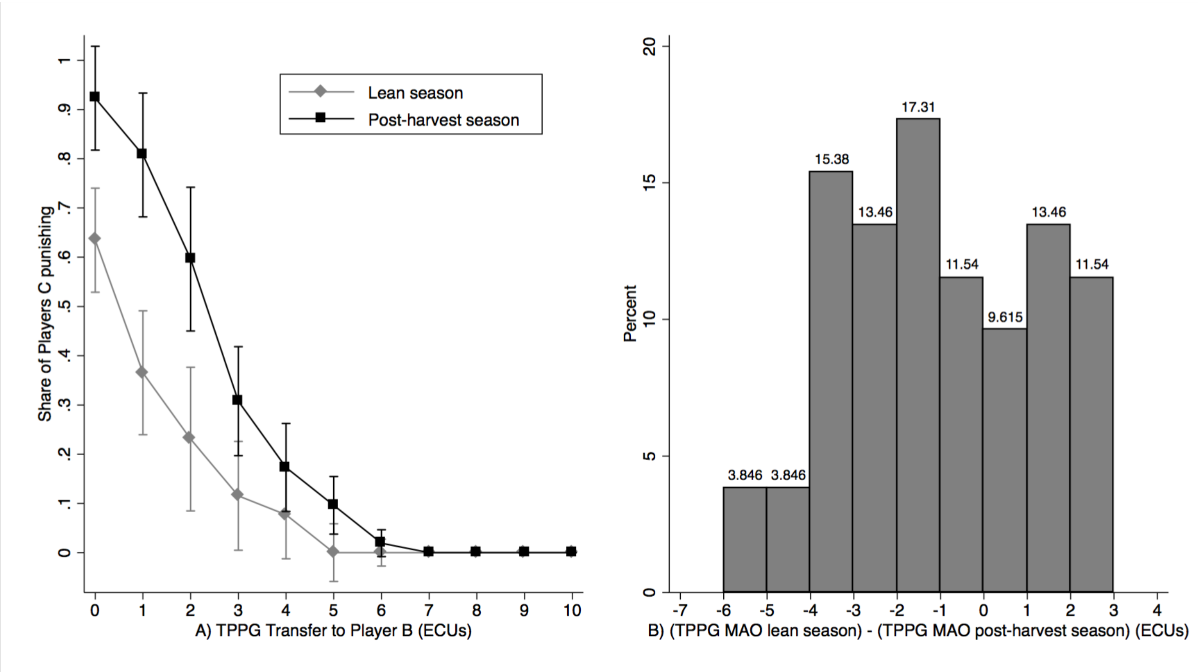
In a similar exercise as presented for the DG, I simulate what would have happened had the distribution of TPPG transfer choices been randomly drawn from the distribution of choices in the post-harvest season to see how many individuals would have sent equal split in such hypothetical case. The average share of participants sending equal amounts in both seasons after random reshuffling in 10,000 repetitions is over 16 percent. This implies that the results I obtain in my experimental data could have arisen due to random chance. More reassuringly, conducting the same exercise for the variable indicating a transfer difference within a margin of one ECU, the share of participants transferring within this extended margin is about 43 percent, indicating some degree of individual stability.

Finding 2: *Transfers in the DG are temporally stable within individuals, suggesting stability of sharing. To a lesser extent I also observe within individual temporal stability in TPPG.*

3.2 Temporal Stability of Sharing Enforcement

Now I analyse the behavior of PCs in the TPPG in order to understand the dynamics of sharing enforcement with exposure to scarcity. I first discuss the aggregate punishment results using the sample of farmers who participated in both the lean and the post-harvest season rounds, and afterwards I examine the within-subject results.

Figure 2: Distributions of TPPG MAO Across Seasons



Notes: Panel A shows the distribution of Player C (punisher; PC) minimum acceptable offers sent by Player A to Player B in the third party punishment game (TPPG MAO). I use data for the 52 PCs for whom TPPG MAO could be recovered in both rounds. The distribution of lean season TPPG MAO is depicted in grey, the distribution of post-harvest season TPPG MAO is depicted in black. The error bars represent 95 percent confidence intervals. Panel B shows the distribution of within-individual changes in Player C (punisher; PC) minimum acceptable offers sent by Player A to Player B in the third party punishment game (TPPG MAO) between the lean and the post-harvest season. I use data for the 52 PCs for whom TPPG MAO could be recovered in both rounds. Positive numbers represent higher TPPG MAO in the post-harvest season compared to the lean season.

Panel A in Figure 2 shows the distributions of PCs' minimum acceptable offers in the TPPG (MAO) in both the lean and the post-harvest seasons. MAO is the lowest PA's transfer to PB that a PC would accept.[19] For example, if a PC decided to engage in either type of punishment of the PA for sending anything less than or equal to 2 ECUs to PB, then the MAO for this PC

[19]Panel A of Appendix Figure A2 shows that the results are even stronger when accounting for punishment intensity.

is equal to 3 ECUs. The lowest value for MAO is 0 ECU if PC decides not to punish any kind of PA's behavior. I was able to elicit MAO for 60 out of 71 PCs in the lean season (85 percent) and for 63 out of 71 PCs in the post-harvest season (87 percent).[20] The subjects for whom I am unable to construct MAO behaved in an inconsistent way, punishing transfers largely at random without any systematic pattern. In the analysis below I use the 123 valid observations.

Panel A in Figure 2 shows that the Afghan participants in the role of PCs were willing to engage in costly punishment of PAs who were not willing to share enough. Regardless of season, the probability of punishing PAs increases with PAs' transfers approaching zero.[21]

Unlike in the case of PAs' transfers, the punishing behavior of PCs is not temporally stable. Panel A in Figure 2 shows that there is a significant decrease in the willingness to punish low offers from the post-harvest to the lean season. Speaking about magnitudes, PCs in the post-harvest season were on average not punishing offers equal to 3.03 ECUs and higher, reaching the levels of average transfers in the DG and TPPG, while in the lean season the average MAO dropped significantly to 1.35 ECUs (Columns 1 and 3 in Table 3). The difference in MAO across rounds is highly statistically significant (WSRT: $p<0.01$, $n=52$). I can also reject the equality of MAO distributions over time (Epps-Singleton, $p<0.01$).

Column 3 in Table 4 shows that the decrease in willingness to punish remains highly significant and of a similar magnitude even in a regression framework. Again, I use the model specified in Equation 1 where T^g_{it} now stands for the MAO by individual i in time t. Testing three hypotheses might increase a chance of false discovery of statistical significance. I report adjusted p-values using a Bonferroni correction at the bottom of Table 4 for all models. Despite this being the most conservative correction, the effect on punishment remains highly significant.

Importantly, the behavior of PCs is also reflected in beliefs of others. Regarding punishment, I asked the participants whether they believe that most PCs in the current experimental session would punish a PA who decides to transfer zero ECUs. The results are presented in Table 3. Although insignificant, the change in beliefs of PBs across seasons (lean season 68 percent vs. post-harvest season 78 percent; WSRT: $p=0.16$, $n=68$) matches the direction of the change in actual punishment behavior of PCs and are of similar magnitude as beliefs of PCs about other PCs' willingness to punish zero transfers in their experimental session (lean season 65 percent

[20] In terms of the task comprehension, this makes my sample comparable to that of Henrich et al. (2006), who were able to assign MAO to 92 percent of their sample. The seasonal difference in sanctioning rates survives even when including inconsistent choices.

[21] Such a pattern emerges even if I include the inconsistent punishers (See Panel B of Appendix Figure A2).

vs. post-harvest season 79 percent; WSRT: p=0.08, n=71). This suggests that the behavioral change across seasons is more generally considered in the population and is not just an artefact of the experiment among the group of PCs. I did not ask this belief question to PAs. Rather, the belief question for PAs asked whether they expect to remain unpunished for their transfer to PB. Regardless of season, slightly above 70 percent of PAs expect not to be punished (WSRT: p=0.85, n=67).[22]

As in previous studies (Fehr & Fischbacher, 2004b; Bernhard et al., 2006; Henrich et al., 2006), the Afghan farmers are willing to engage in costly altruistic punishment for which they have to give up 20 percent or 40 percent of their endowment to punish non-desirable behavior. In terms of daily incomes, the amounts are equal to giving up 13 to 26 percent of average daily incomes to discipline others, a substantial amount given the tight budgets of the population studied. Overall, 93 percent of the PCs for whom I am able to construct the MAO are willing to punish a PA who decides to keep everything in the post-harvest season, a number comparable to the most punishing societies in the study of Henrich et al. (2006), the Kenyan Gusii and Maragoli tribes. This share drops to 62 percent in the post-harvest season, similar to the average punishment choice frequency for zero transfers in the 15 small-scale societies studied in Henrich et al. (2006) (WSRT: p<0.01, n=52; Columns 1 and 3 in Table 3).

Finding 3: *Afghan farmers substantially decrease intensity of sharing enforcement during the lean season.*

As in the case of the sharing behavior, the experimental design also allows me to examine punishing behavior across seasons within an individual. There were 52 PCs for whom I could construct the MAO in both rounds. The remaining 19 PCs behaved inconsistently in either of the seasons, but never in both. In the lean season 11 PCs behaved inconsistently compared to 8 PCs in the post-harvest season. 65.4 percent of PCs decreased the level of punishment in terms of MAO between the post-harvest and the lean seasons, 9.6 percent of PCs punished exactly the same across both seasons, and 25 percent increased the level of punishment. Panel B in Figure 2 presents a histogram of individual changes in MAO across seasons.

As discussed, punishment decisions were elicited using the strategy method. Footnote 11 shows that earlier studies have found differences in punishment behavior when comparing the strategy method and direct-response elicitation. It might thus be assumed that actual pun-

[22] One PA did not respond to the belief question in the lean season.

ishment decisions could be higher than those presented here. The reason for this is impulsive behavior when *hot*, directly elicited decisions are made. Yet, reassuringly, the treatment differences in earlier studies have been directionally the same, regardless of the method used. There is a concern that the differences between the strategy and direct elicitation procedures might be different across seasons. Mani et al. (2013) show that cognitive abilities decrease with exposure to scarcity during a lean season. It is plausible that lower cognitive ability is linked to more impulsive behavior (Kahneman, 2011). Hence, it might be expected to observe more impulsive behavior leading to increased punishment in the lean season even in the strategy method decisions. But it is the opposite that I observe.

What characteristics explain the behavioral change? Appendix Table A9 shows that regressing the difference in MAO between the post-harvest and the lean season on a set of regressors that include participant's age, years of schooling, number of household members, individual income in either of the seasons, and the poverty index in either season[23] does not provide us with any explanation for the observed change in behavior.

4 Discussion

In this section I provide some evidence that the drop in the willingness to punish in the lean season can either be attributed to higher uncertainty about the intentions of others or due to higher wealth inequality present at that period. Also, I show that the drop in punishment is not determined by individual severity of the seasonal shock, but rather by the severity of the aggregate shock on the community-level. This suggests that a village-level social norm is driving the behavioral change. I also show some evidence that speaks for the generalizability of the observed behavior and I show that there is a positive correlation between sharing and real-life charitable giving. Besides that, I rule out several possible caveats such as the role of order effects or the effect of changing marginal utility of wealth across seasons as possible explanations for the behavior observed.

[23] The poverty index at a given point of time is estimated using the principal component analysis. The 1^{st} principal component of each poverty measure for a given season is constructed using animals owned, assets owned, variability of food consumed, meat eaten in a given week, days unable to work due to illness or injury in the previous month, a short version of the perceived stress score (Cohen et al., 1983), and dummy variables representing unusual health shocks to humans, animals, and plants.

4.1 Determinants of Seasonal Changes in Enforcement Behavior

In Section 3.2 I show that there is a substantial drop in punishment behavior in the lean season compared to the post-harvest season. What factor is driving the difference? Several possible explanations can be put forth:

First, punishment might be perceived as a normal good, demand for which increases with increasing income. Examining the correlation between MAO and individual income (adding income variable to the Column 3 specification in Table 4), I actually find an opposite: a negative correlation (β=-0.43, p=0.09). This effect may be driven by the fact that the wealthier individuals are in general less likely to engage in altruistic punishment. The nature of the data also allows me to examine the *change* in income within an individual across seasons. Comparing the MAO for those PCs whose reported income was higher in the post-harvest season compared to the lean season (n=21) and those whose income did not increase in the post-harvest season (n=31), I find that MAO is not significantly statistically different across these groups (Mann-Whitney U-test, MWT: p=0.42, n=52).[24] Specifically, the *change* in MAO for those whose income did not increase between the post-harvest and the lean season is equal to -1.74, while the change in MAO for those whose income increased is -1.14. Monetary income might, however, not be the best proxy of wealth in agrarian communities. For this reason, I also conduct a similar analysis for the seasonal difference in the comprehensive poverty index, which yields a similar result. Importantly, only for ten PCs the poverty index was lower in the lean season compared to the post-harvest season, while for the remaining 42 PCs the poverty index increased. Income effects thus do not plausibly explain the observed drop in sharing enforcement in the lean season.

Second, Grechenig et al. (2010), Xiao & Kunreuther (2015), and Bornstein & Weisel (2010) suggest that the punishment level drops with rising uncertainty about PA's intentions. It is plausible that increasing uncertainty about the PA's financial situation might cause the lower punishment levels observed in the lean season. In other words, the PC in the lean season cannot differentiate between a selfish and a needy PA, which is the reason why he rather abstains from getting involved in the judgment and possible later regret if he decided to punish a needy individual. This uncertainty is generally higher in the lean season. Not only is income level

[24]The number of observations in this analysis is 52. This is the number of subjects for whom I was able to construct the MAO in both rounds. The income of 14 PCs remained constant across seasons and for 17 PCs it increased in the lean season compared to the post-harvest season. However, while median income was 2500 AFN higher in the lean season for the group of PCs whose income increased, the median drop in income for the group of PCs whose income decreased in the lean season was 4500 AFN.

generally lower, leaving more people below the subsistence threshold,[25] it is also much more variable. The Gini coefficient for the entire sample reaches 0.47 in the lean season and drops down to 0.33 in the post-harvest season. Table 2 (Columns 2 and 4) shows that the standard deviation for individual income is significantly higher in the lean season (Variance ratio (VR) test: $p<0.01$, n=278). Similarly, the standard deviation of the comprehensive poverty index is also significantly higher in the lean period (VR test: $p<0.01$, n=278). However, the predictive power of a model regressing the seasonal change in MAO (both individual and village-average) in willingness to punish on the average village-level seasonal change in income inequality is not significant (See Appendix Table A7). Nevertheless, the small sample size of only ten villages does not allow me to rule out the proposed hypothesis.

Further, it can be argued that the PCs might expect the PAs to overcome uncertainty about the neediness of PBs by keeping the money from the experiment and sharing it afterwards with some needy person in their village. However, none of the participants reported willingness to share the money with anyone outside of his family in a post-experiment survey. Nearly 90 percent of participants in the lean season and over 96 percent of the participants in the post-harvest season reported that they plan to spend the money from the experiments on food or other household expenses.

Third, increased inequality during periods of scarcity has also been shown to predict the rise of grievances, which is one explanation for the rise in conflicts during scarcity (Hidalgo et al., 2010; Hsiang et al., 2013). It is possible that increased acceptance of violence in solving problems can be associated with the observed decrease in willingness to punish non-cooperative behavior during the period of scarcity. In my sample I observe an increased number of individuals who were engaged in disputes[26] during the lean season when compared to the post-harvest season (14.5 percent versus 7.7 percent; WSRT, p=0.02, n=207).

Appendix Table A8 presents supportive evidence for the role of increased grievances in explaining the drop of punishment. The regressions show a negative correlation between the change in the average village-level share of individuals engaged in a dispute between the lean season and the post-harvest season and the change in MAO between the lean season and the post-harvest season. The first three models use average village level change in MAO as a dependent variable. Despite the small number of observations—ten villages—the effect is highly

[25]NRVA (2008) reports that the food consumption of 48 percent of rural Afghans is below a poverty line during the lean season, compared to 21 percent in the post-harvest season.

[26]Individuals were asked a question whether they were "engaged in a dispute in the previous four weeks".

significant in all three regression specifications that use different analytic weights. Although significance is lower, models 4 to 6 show effects of similar magnitude using individual level changes in MAO as a dependent variable. A simple back of the envelope calculation, with the average change in the share of individuals engaged in disputes being less than 7 percentage points, suggests that the estimate explains around 30 percent of the observed change in punishing behavior.[27] It is important to note that this effect cannot be interpreted causally. Despite that, the link between relatively higher engagement in disputes and relatively lower punishment behavior in the lean season on a village level is telling.

The design of the experiment does not allow to separate the second and third explanations. One way or the other, Fehr & Fischbacher (2004a) have provided strong evidence replicated in numerous experiments that without enforcement mechanisms groups gradually dwindle to a non-cooperative equilibrium. Boyd et al. (2003) provide a theoretical model showing that third party punishment helps societies to maintain cooperative equilibria even in larger groups and its absence leads to a collapse of cooperation, as selfish individuals invade the population and their behavior provides them with higher payoffs compared to the payoffs of cooperators. A cross-cultural study shows evidence of positive correlation between altruistic sharing and sharing enforcement (Henrich et al., 2006). Thus, regardless of PCs' motivations, the drop in sharing enforcement in the lean season increases the likelihood of a drop in sharing.[28]

Lastly, the population may become more lenient towards minor transgressions during lean season. Ostrom (1990, p. 75) presents several historical cases of increased leniency in punishing violations of water sharing agreements under periods of scarcity. Such leniency did not result in reduced of cooperation. This explanation would jointly explain the drop in punishment and the stability of sharing I observe. For the argument to be valid, we would expect a highest drop in punishment with mildest transgressions. This does not seem to be the case within the choice-space in the experiment. While punishment intensity for TPPG transfers of 3 ECUs (the mode) all the way to equal transfers of 5 ECUs dropped on average by 16 p.p., the intensity of punishment of TPPG transfers below 3 ECUs dropped by 38 p.p.

Similarly to Wutich (2009) who documents that weakening of social networks is only temporary for the duration of a dry season and returns to original levels with the end of the dry

[27]Conducting a similar analysis on an individual level changes in dispute engagement does not yield significant estimates.

[28]As in my case, Gneezy & Fessler (2012) do not observe a change in behavior of PAs in the ultimatum game from peacetime to wartime played only once in each period, despite the observed increase in punishment behavior during wartime.

season, Afghan farmers maintain some stabilizing mechanisms that prevent them from plunging into non-prosocial equilibria. However, it seems that they lack mechanisms preventing the collapse of cooperation in times of prolonged scarcity or of unexpected shocks. This might explain the dynamics of collapse of cooperation during famines (Turnbull, 1972; Dirks, 1980; Ravallion, 1997). As my results suggest, the drop in prosocial behavior observed in this literature does not necessarily stem from changes in individual preferences, but rather from weaker social enforcement of prosocial behavior. The aim of the next section is to show that the change in altruistic punishment can actually be attributed to changing social norms, rather than to mere individual preferences responding to changing individual conditions.

4.2 Individual Preferences or Social Norms as Determinants of Punishment Behavior

Although resorting to punishment in the TPPG is generally understood as an expression of willingness to sustain social norms (Henrich & Boyd, 2001; Boyd et al., 2003; Fehr & Fischbacher, 2003; Henrich et al., 2006), the behavior could also be driven by state-dependent individual other-regarding preferences. Such behavior would be consistent with models of inequality aversion (Fehr & Schmidt, 1999) or the theory of reciprocity (Falk & Fischbacher, 2006), in which an unkind act of a PA towards a PB has a negative effect on PC's utility. Note that individuals' wealth and his expectations of the wealth of others outside of the experiment also have to be incorporated into the model's parameters unless strict narrow bracketing is assumed. The act of punishment in such models would have two effects: first by the effect of deterrence preventing PAs to engage in unkind behavior in the first place and second by the moderation of selfish PAs' advantageous inequality by reducing their payoff relative to that of other players.

Understanding this distinction is important. If social norms guided the observed behavior, moral authorities in the society could have their say in affecting individual behavior. On the other hand, individual level interventions would hardly make any change, at least on a short term horizon. The opposite argument can be made if individual preferences were driving the observed behavior. My data speak in favor of the explanation based on social norms.

The regression models including variables representing individual exposure to scarcity—income and the comprehensive poverty index in either season—cannot fully explain behavioral change in punishment behavior (See Appendix Table A9). Splitting the results by whether a particular PC perceived himself as relatively richer or poorer compared to his fellow villagers in the given season also does not predict the behavioral change. On the other hand, examining the average seasonal change in exposure to scarcity within a village is linked to the change in the TPPG MAO between the lean and the post-harvest season in a way that would support the norms-based explanation: the more severe the shock in the average village-level poverty, the larger the drop in MAO. Appendix Table A10 summarizes the results both using the average village-level change in MAO as a dependent variable (models 1 to 3) as well as the invididual-level change in MAO as a dependent variable (models 4 to 6).[29] It thus seems more plausible that the observed drop in punishment during the lean season is driven by changes in village-specific social norms.

4.3 Generalizability

Even though more research needs to be done in understanding whether the presented results can be generalized to other populations, it is important to point out that the results are valid for two very different groups. As shown in Table 1, half of the sample in my experiment are ethnic Tajiks and the other half are ethnic Hazaras, the second and third largest ethnic groups in Afghanistan respectively. While the former are Sunni muslims, the latter are Shia muslims, a minority in Afghanistan.

Tajiks are of Persian origin. They are, after Pashtuns, the second largest ethnic group in Afghanistan with around 32 percent of the population. In the Balkh province where the experiments have been conducted Tajiks are the predominant ethnic group, with around 44 percent of the population (DHS, 2010). The governor of the province is a Tajik himself. Hazaras, people probably of Mongolian descent, constitute around 9 percent of the population of Afghanistan and around 10 percent of the population of Balkh province (DHS, 2010)[30]. They have historically been a marginalized group in Afghanistan with very different origins from the other ethnic groups in Afghanistan.[31] As stated earlier, although the two groups live in close proximity and

[29] A similar regression explaining seasonal changes in MAO by changes in average village-level income does not yield a significant result, though.

[30] The remaining ethnic groups in Balkh province are Pashtuns (12 percent), Uzbeks (11 percent), Turkmen (9 percent), and Balochis (2 percent). The remaining 12 percent did not report their ethnicity. *Source:* Demographic and Health Survey Afghanistan (2010). Indian Institute for Health Management Research (IIHMR), available online at https://dhsprogram.com/data/dataset/Afghanistan_Special_2010.cfm. I rely on DHS data, since the last census was conducted in 1979.

[31] Hazaras faced social, economic and political discrimination, often resulting in atrocities against members of the group. The massacres of Hazaras in 1880s during the reign of Abdur Rahman Khan, and later in 1994 in Kabul and in 1997 in Mazar-e-Sharif during the reign of the Taliban "irreparably damaged the fabric of the country's national and religious soul" (Rashid, 2001, p. 83). Hazaras were sidelined from mainstream Afghan politics when the 1964 constitution ruled that all state officials have to be Sunni (Hanafi) muslims. Although the new constitution does not continue to discriminate against Hazaras and there are many high ranking Hazara

they share the same language, their villages are almost perfectly ethnically segregated and there are very few economic interactions between the two areas.

Now I examine whether the main results differ by ethnicity. I do not make any predictions and take this as a purely empirical exercise. Appendix Table A11 shows that all the main results are valid for both the Tajiks (Columns 1 to 3) as well as for the Hazaras (Columns 4 to 6) in my sample. That is, the transfers in both the DG and TPPG remain stable over time, and that the enforcement of sharing weakens substantially during the lean season.

4.4 Behavior in games and in the real life

After demonstrating that the presented results are similar across distinct ethnic groups, I need to establish that the behavior in experiments reflects the actual behavior outside of the field laboratory. Numerous earlier experiments have shown that sharing and punishment beahvior in experiments correlates with real-life giving (e.g., Benz & Meier, 2008; Ligon & Schechter, 2012) and punishment behavior (e.g., Kosfeld & Rustagi, 2015). Real-life sharing also correlates with individual behavior in the sample studied here. In every round the participants responded to a question whether they gave money as charity in the previous four weeks. Appendix Table A12 shows that the level of sharing in the DG is higher among those participants who also reported to have recently shared money with others. Although the coefficient is insignificant for the TPPG transfers, the effect goes in the same direction. When examining the willingness to enforce sharing, it is again the more charitable individuals who are more likely to have engaged in costly punishment in the experiment. Interestingly, the effects are mainly driven by the sharing and punishment behavior in the post-harvest period and the standard errors for the lean season coefficients are relatively higher, suggesting increased uncertainty in the lean season.

4.5 Potential Confounds

The experiment was conducted over two periods, the lean season first and the post-harvest season second. What if the order of the experiments alone influences the results? Two findings speak against such a claim.

First, it might be argued that the stability of sharing behavior I observe can be attributed to anchoring one's own behavior in the first, lean season round. For this to be the case,

officials in the government, the ethnic division is still present. The terrorist attack on a Hazara demonstration in Kabul on July 23, 2016 that killed 80 and was claimed by a group called Islamic State is the latest reminder of the vulnerability of this group.

the PAs would have to remember their behavior in the previous round of experiments. When asked during the post-harvest round post-experimental survey—in an unincentivised question—about how much they transferred in the DG in the previous round, the PAs guesses were correlated more with the actual transfers in the post-harvest round (ρ=0.61, p <0.01), than with the transfers in the lean season round (ρ=0.48, p <0.01). I asked this question only of PAs. Moreover, only about 32 percent of the participants (22 out of 68) correctly guessed their own transfer in the lean season round. Twelve of these 22 participants decided to choose the same amounts in both rounds. When conducting the same analysis as in Columns 1 and 2 of Table 4 on a subsample of 46 PAs who did not remember their DG transfers from the previous round correctly, I obtain results that are qualitatively very similar to the results obtained for the full sample of 68 PAs, with no statistically significant differences in DG or TPPG transfers across seasons (See Appendix Table A13).

Second, to examine the possible role of order effects on punishment behavior, I compare the subjects who participated in both seasons and are thus susceptible to being influenced by the order of the rounds to the "single-round" population of farmers participating in one round only—either in the lean season or in the post-harvest season. Reassuringly, the personal characteristics of farmers who participated in both seasons and those who participated in the lean season do not differ (Column 7 of Table 1), but the sample of participants recruited for the first time in the post-harvest period is significantly younger and less educated despite the same sampling procedure (Column 9 of Table 1). Appendix Table A14 shows that the punishment behavior of PCs who participated in both periods is not statistically significantly different from the "single-round" subjects in the respective seasons (lean season: F(1,182) = 0.65, p=0.42; post-harvest season: F(1,182) = 0.56, p=0.46). Also, the difference between the sanctioning behavior of "single-round" PCs in the post-harvest season and in the lean season exhibits a very similar declining pattern as I observe among the participants in both periods in Column 3 of Table 4 (F(1,182)=6.43, p=0.01).

Another confound that might explain the results presented here is that of seasonal changes in marginal utility of wealth. It is plausible that, due to diminishing marginal utility of wealth, an additional ECU in the experiment has a different value in different seasons. This issue gains importance in the context of dramatic seasonal income fluctuations. As the marginal utility of an additional ECU is highest in the lean season on average, it might be expected that the participants put a higher value on their own payoffs in the lean season, *ceteris paribus*. If that was the case, it would be possible to attribute the observed lower willingness to engage in punishment in the lean season to the diminishing marginal utility of wealth. I provide three arguments against this explanation.

First, as discussed in footnote 13 and shown in Appendix Table A1, I do not observe any differences in punishment behavior regardless of the randomly assigned size of stakes in the experiment. Second, it is not plausible that the changing marginal utility of wealth would result in a dramatic decline in punishment behavior, but not in the decline in sharing behavior during the lean season. This would imply a disproportionately lower elasticity of willingness to share with respect to wealth compared to the elasticity of willingness to punish with respect to wealth. Since willingness to share is positively correlated with willingness to punish (Henrich et al., 2006), such a conclusion is unlikely.[32] Lastly, as discussed in Section 4.1, individual-level changes in income and poverty in general cannot explain the differences in behavior across seasons. It is thus rather inconceivable that changing marginal value of money across seasons is driving the observed behavioral change.[33]

5 Concluding Remarks

A large portion of the world's population is repeatedly exposed to periods of scarcity. Informal sharing, one of the few coping strategies available in many regions, weakens during such periods. In this paper I ask whether altruistic sharing and enforcement of sharing are affected by seasonal scarcity. As reciprocity, another ingredient of informal sharing, weakens, it is natural to ask whether altruism and enforcement are stable traits or are also malleable, and whether they help or hinder in sustaining informal sharing during difficult times. Specifically, I experimentally examine the dynamics of individual sharing behavior using a dictator game, and of the willingness of third parties to engage in the enforcement of sharing, using a third party punishment game among Afghan subsistence farmers. I visited the area twice in one year—during the lean season and six months later during a post-harvest season, the period of relative plenty—and conducted the same experiment repeatedly with the same participants.

[32] My data on the village level also support a positive correlation between the willingness to share in a DG and the willingness to punish. Interestingly, this correlation between village-level DG transfers and TPPG MAO reaches significance only for the post-harvest season (ρ=0.63, p=0.05), while it is insignificant in the lean season (ρ=0.27, p=0.45).

[33] Similarly, if the participants were concerned about seasonal changes in marginal utility of wealth of their matched partners rather than their own, observation of the differential treatment in the sharing and in the sanctioning conditions would be equally unlikely.

Although the sharing behavior measured by the dictators' transfers in a standard dictator game remains stable over time on both the aggregate level, and to a large extent, on the individual level, the enforcement mechanisms that help to sustain cooperative outcomes—as measured by the intensity of third parties' willingness to punish non-desirable behavior—are significantly weakened during the period of scarcity. My results suggest that rather than individual preferences, it is a social norm with respect to enforcement behavior that is changing on the community level. The results also speak against increased leniency in enforcement behavior. I replicate the results on a different sample of participants who took part in either the lean season or the post-harvest season only.

Even though the population studied seems to be able to sustain altruistic sharing over the period of temporary resource scarcities during the lean season, it is not implausible that prosociality might deteriorate if the population experiences a larger shock or if it is exposed to scarcity over a longer period of time than expected. Since sharing preferences are predictive of trusting and cooperative behavior, the results might have important implications for the functioning of markets and the ability of communities to mobilize and engage in collective action during periods of scarcity. Taken together, this would be consistent with the decline in cooperation over time when enforcement mechanisms are not available observed in laboratory experiments (Fehr & Fischbacher, 2004a). One can also speculate that with weakened enforcement behavior, perverse behavior, such as anti-social punishment (Herrmann et al., 2008; Prediger et al., 2014) or counter-punishment (Nikiforakis, 2008), might gain prominence.[34]

The present study provides initial evidence that even temporary periods of resource scarcity substantially weaken the enforcement of prosociality. The erosion of enforcement observed in this study might be one of the explanatory factors for the increased prevalence and even acceptance of behavior that would typically not be tolerated (Oster, 2004; Miguel, 2005; Sekhri & Storeygard, 2014).

Many solutions to mitigate seasonal scarcities and scarcities in general are available: the introduction of safety net programs, the provision of or assistance with finding off-season employment, the provision of formal insurance, the provision of microcredit, and the introduction of reliable savings products. While providers of these solutions usually promote the impact of

[34]Despite the fact that social norms in most cases promote efficiency, some equity promoting norms may be perceived as harmful. One such example are traditional sharing norms in kinship networks. These norms define obligations of transfers from more to less successful relatives. Such obligations motivate many individuals to undertake costly measures in order to signal liquidity constraints (Di Falco & Bulte, 2011; Jakiela & Ozier, 2015), resulting in lower growth potential of households subject to such norms.

these policies on individuals, they often fall short of stressing their possible effect on preventing negative outcomes on a wider community level. For example, an interesting unintended side-effect of a large-scale public employment program is that it reduces the risk of communal conflicts (Fetzer, 2014). Moreover, since scarcity is shown here to be associated with looser enforcement of prosociality, concerns that the introduction of such policies would crowd out existing informal institutions and moral intentions (e.g., Dupas & Robinson, 2013) seem less plausible.

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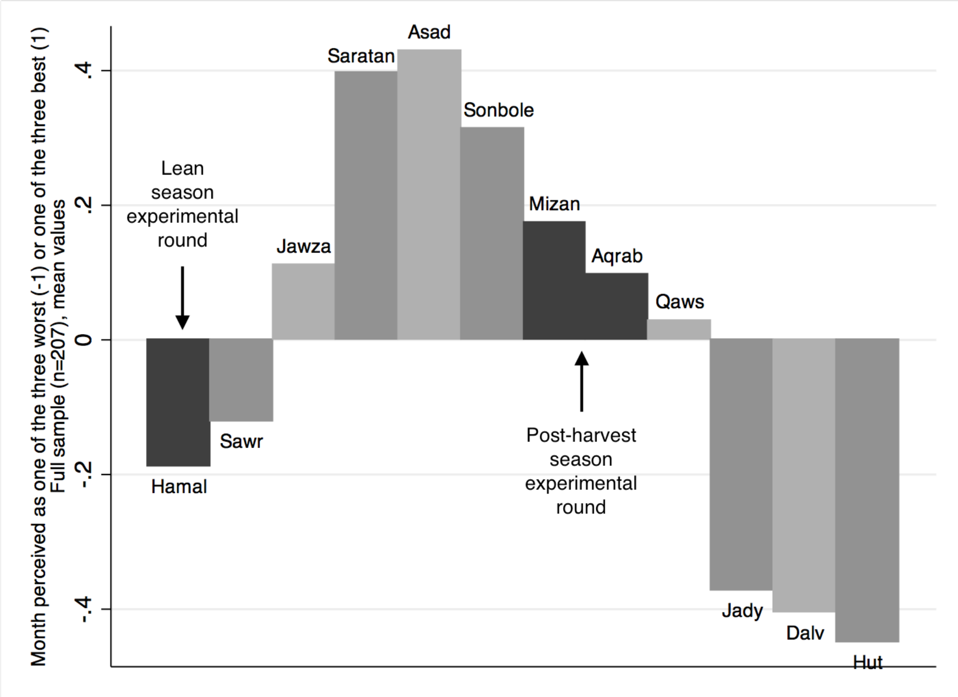
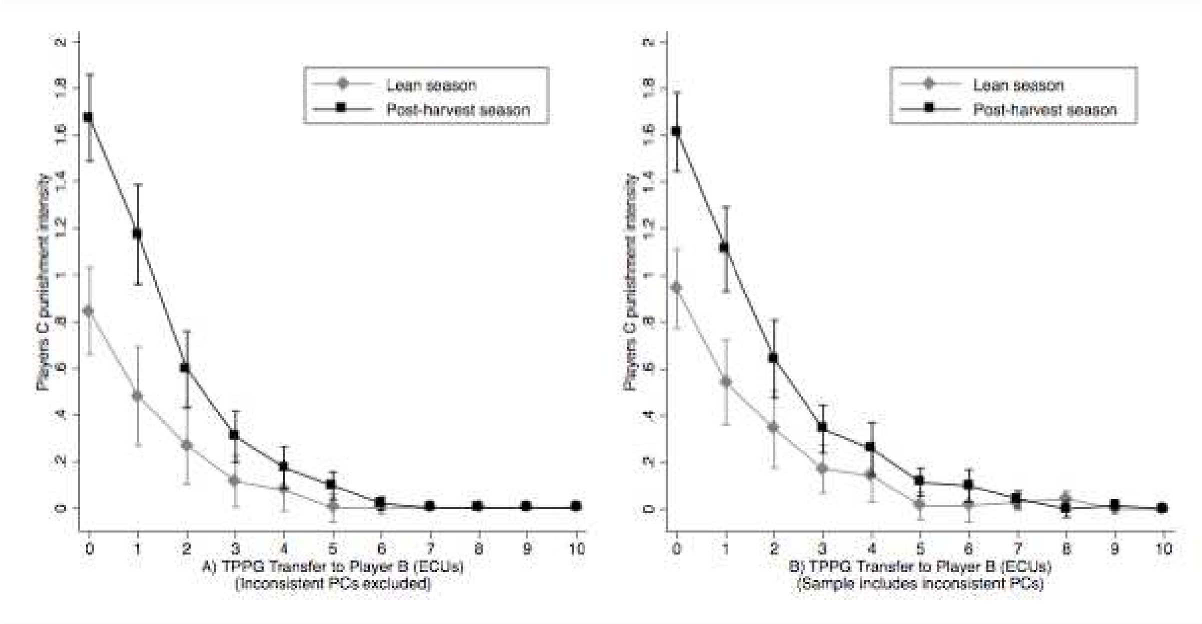
Figure A1: Subjective Perceptions of Living Quality Throughout the Year



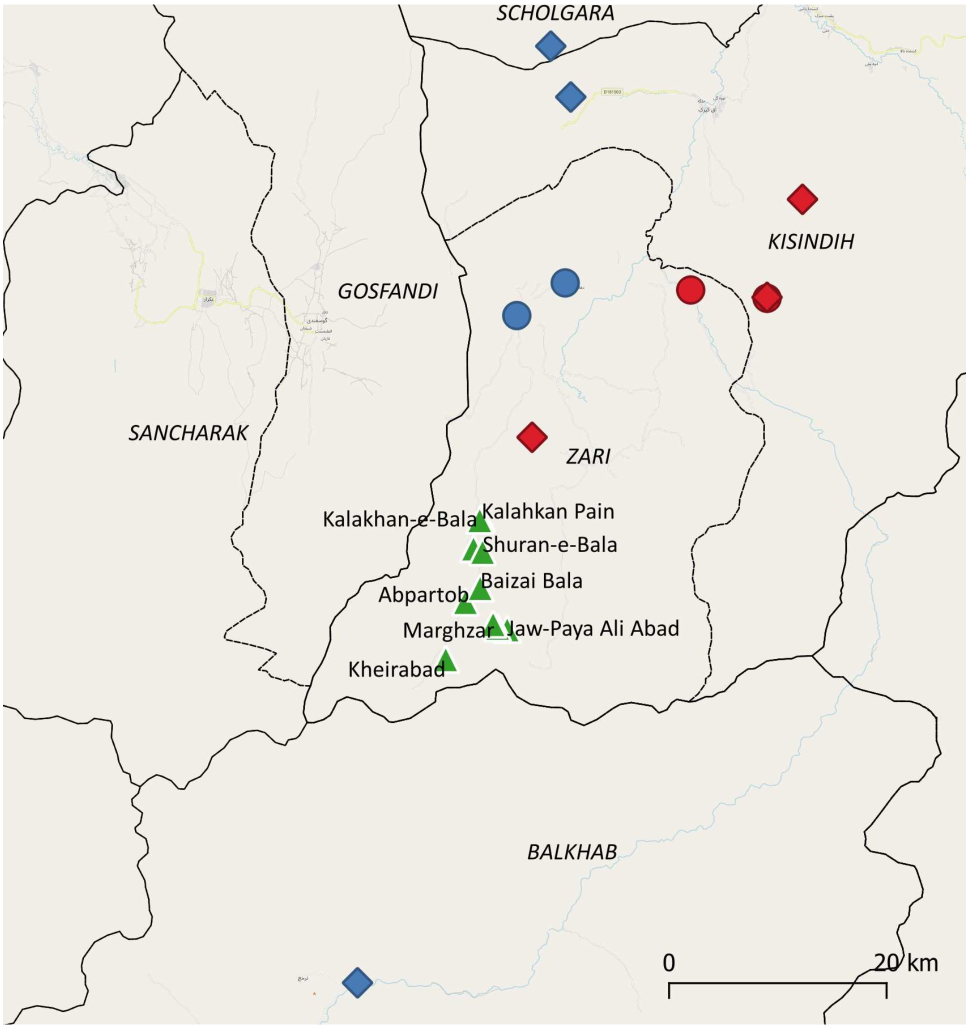
Notes: The figure depicts the average participants' rating of quality of life during each month in the year. The participants rated the month as one of the best three months (+1) or as one of the worst three months by answering the question: "Which three months are usually the [best /most difficult] in terms of food for you?". Months not mentioned are treated as 0. The question was asked during the lean season round. Afghanistan uses the Persian version of the Solar Hijri calendar. Persian month names are presented here, because the conversion to Gregorian calendar would be confusing. The experiments were carried out in the months of Hamal 1392 (March to April 2013, lean season) and Mizan and Aqrab 1392 (October 2013, post-harvest season) represented in the darkest color.

Figure A2: Distributions of TPPG MAO Across Seasons: Punishment Intensity



Notes: Panel A shows the distribution of Player C (punisher; PC) punishment points (0 / 1 / 2) conditional on amounts sent by Player A to Player B in the third party punishment game. I use data for the 52 PCs for whom TPPG MAO could be recovered in both rounds. The distribution of lean season punishment points is depicted in grey, the distribution of post-harvest season punishment points is depicted in black. The error bars represent 95 percent confidence intervals. Panel B shows the same distribution as in Panel A but it also includes the observations from individuals for whom TPPG MAO could not be recovered in either of the rounds.

Figure A3: Map Indicating Violent Incidents in the Surrounding Area



Notes: All incidents of direct fighting (diamonds) and improvised explosive device explosions (circles) within a radius of 40km from the center of either study village (green triangles) in a period of six months prior to the lean season round (red) and a period of six months prior to the post harvest round (blue). Data on violent events are from the International Security Assistance Forces (ISAF) Combined Information Data Network Exchange (CIDNE) database.

Table A1: Effect of Payoff Size on DG and TPPG Transfers and on TPPG MAO in the Lean Season

Dependent variable	DG transfer	TPPG transfer	TPPG MAO
	(1)	(2)	(3)
Payoff high	-0.70	-0.20	0.51
	(0.54)	(0.50)	(0.64)
Observations	68	68	60
R-squared	0.82	0.80	0.53

Notes: OLS coefficients. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Column 3 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). This column shows results for a subsample of N=60 with consistent MAO. Lean season observations only. *Payoff high* is equal to one if 1 ECU equals to 30 AFN instead of 20 AFN used in all other sessions. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity.

Table A2: Number of Observations by Village, Role, Including “Single-Round” Subjects

Participating in	Both seasons			Lean season only			Post-harvest season only		
	Player A (1)	Player B (2)	Player C (3)	Player A (4)	Player B (5)	Player C (6)	Player A (7)	Player B (8)	Player C (9)
Abpartob	3	4	4	2	1	1	7	6	6
Baizai Bala	8	4	8	1	5	1	6	10	6
Jaw-Paya Ali Abad	4	7	6	6	3	4	10	7	8
Kalahkan Pain	8	8	6	2	2	4	7	7	9
Kalakhan-e-Bala	7	7	8	3	3	2	8	8	7
Kheirabad	3	2	2	2	3	3	7	8	8
Koche Aghaz	14	13	14	1	2	1	6	7	6
Marghzar	8	9	10	5	4	3	8	8	7
Quala-e-Noorak	8	7	8	2	3	2	7	8	7
Shuran-e-Bala	5	7	5	5	3	5	5	3	5
Total	68	68	71	29	29	26	71	72	69

Table A3: Village Level Effects

Dependent variable	DG transfer (1)	TPPG transfer (2)	TPPG MAO (3)
Marghzar	0.40	0.59	-0.29
	(0.78)	(0.76)	(0.86)
Koche Aghaz	-1.32*	-0.64	-1.05
	(0.71)	(0.71)	(0.65)
Jaw-Paya Ali Abad	-0.29	0.34	-1.09
	(0.77)	(0.86)	(0.74)
Baizai Bala	0.40	0.90	-0.36
	(0.77)	(0.73)	(0.80)
Abpartob	1.21	1.55*	-0.14
	(0.81)	(0.85)	(0.79)
Kheirabad	1.05	1.38	0.54
	(0.94)	(0.98)	(1.15)
Quala-e-Noorak	0.09	0.34	-1.21*
	(0.76)	(0.70)	(0.65)
Shuran-e-Bala	-0.39	0.21	-0.31
	(0.82)	(0.76)	(0.75)
Kalahkan Pain	-0.41	-0.41	0.29
	(0.81)	(0.75)	(0.90)
Constant	3.29***	2.79***	2.71***
	(0.65)	(0.62)	(0.56)
Observations	136	136	123
R-squared	0.16	0.13	0.08
	F-test H_0: joint significance of village dummies		
F-test p-values	0.90	0.46	0.50

Notes: OLS coefficients. The constant represents the omitted village, Kalakhan-e-Bala. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Column 3 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). This column shows results for a subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent TPPG MAO.

Table A4: Effect of Seasonality on DG and TPPG Transfers (Ordered Probit)

Dependent variable	DG transfer of...						TPPG transfer...					
	... 0	... 1	... 2	... 3	... 4	... 5	... 0	... 1	... 2	... 3	... 4	... 5
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Lean season	0.02	0.02	0.02	0.00	-0.01	-0.03	0.02	0.02	0.02	-0.00	-0.01	-0.03
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.04)	(0.03)	(0.02)	(0.02)	(0.00)	(0.02)	(0.04)
Observations	136	136	136	136	136	136	136	136	136	136	136	136

Notes: Ordered probit. Average marginal effects on the probability of respective DG (columns 1-6) and TPPG (columns 7-12) transfers reported. Excluding marginal effects for infrequent transfers over 5 ECU. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Columns 1 to 6 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Columns 7 to 12 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). All regressions include controls for age, schooling, number of household members, and village fixed effects.

Table A5: Effect of Seasonality on TPPG MAO (Ordered Probit)

Dependent variable	TPPG Minimum Acceptable Offer of...					
	... 0	... 1	... 2	... 3	... 4	... 5
	(1)	(2)	(3)	(4)	(5)	(6)
Lean season	0.29***	0.14***	-0.01	-0.13***	-0.11***	-0.11***
	(0.06)	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)
Observations	123	123	123	123	123	123

Notes: Ordered probit. Average marginal effects on the probability of respective TPPG MAO reported. Excluding marginal effects for infrequent TPPG MAO over 5. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. The dependent variable in all models is the third party punishment game (TPPG) minimum acceptable offer (MAO). Subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent MAO. All regressions include controls for age, schooling, number of household members, and village fixed effects.

Table A6: Effect of Seasonality on DG and TPPG Transfers, and on TPPG MAO (Session-Specific Controls)

Dependent variable	DG transfer	TPPG transfer	TPPG MAO
	(1)	(2)	(3)
Lean season	-0.27	-036	-1.67***
	(0.22)	(0.28)	(0.31)
Average age of session participants	0.11	0.78	0.21
	(0.61)	(0.60)	(0.61)
Average schooling (completed years) of session participants	-0.18	-0.15	0.13
	(0.27)	(0.27)	(0.27)
Average number of household members of session participants	0.39	0.50**	0.26
	(0.24)	(0.21)	(0.20)
Share of participants found in post-harvest season	2.71	4.69**	-2.47
	(2.11)	(1.98)	(1.97)
Observations	136	136	123
R-squared	0.82	0.81	0.73

Notes: OLS coefficients. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Column 3 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). This column shows results for a subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent MAO. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity.

Table A7: Average Changes in TPPG MAO and Changes in Village-level Inequality

Dependent variable	Village average of			Individual		
	TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$			TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Gini[a]	-1.39	-1.12	-1.84	-1.15	-0.98	-1.78
	(2.33)	(2.33)	(2.42)	(1.79)	(1.82)	(1.71)
Constant	-1.54***	-1.44**	-1.62**	-1.50***	-1.43***	-1.62***
	(0.42)	(0.43)	(0.49)	(0.33)	(0.33)	(0.34)
Observations	10	10	10	52	52	52
R-squared	0.04	0.03	0.06	0.01	0.01	0.02
Weight used	No weight	Sample population	Village population	No weight	Sample population	Village population

Notes: OLS coefficients. Columns 2, 3, 5, and 6 report weighted data using analytic weights. Weights used are the sample population size and the reported population size of the entire village based on interviews with community leaders for Columns 2 and 5, and 3 and 6 respectively. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. The dependent variable in models 1 to 3 is the difference in village-level average Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. The dependent variable in models 4 to 6 is the difference in individual Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. Observations in models 1 to 3 represent villages.

[a]The lean season minus post-harvest season change in village level Gini coefficient measured using the self-reported income data within the full sample of experimental subjects. Income is measured as cash earned in past 30 days per equivalence scaled HH member by HH head's in thousands of AFA.

Table A8: Average Changes in TPPG MAO and in Village-Level Disputes Engagement

Dependent variable	Village average of TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$			Individual TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Village-level "Engaged in disputes (d)"[a]	-7.93***	-7.98***	-7.51**	-6.69*	-6.83*	-6.47*
	(2.14)	(2.15)	(2.46)	(3.63)	(3.78)	(3.61)
Constant	-0.84*	-0.80	-0.98	-0.98**	-0.95**	-1.10***
	(0.44)	(0.44)	(0.57)	(0.37)	(0.38)	(0.39)
Observations	10	10	10	52	52	52
R-squared	0.44	0.41	0.34	0.07	0.07	0.06
Weight used	No weight	Sample population	Village population	No weight	Sample population	Village population

Notes: OLS coefficients. Columns 2, 3, 5, and 6 report weighted data using analytic weights. Weights used are the sample population size and the reported population size of the entire village based on interviews with community leaders for Columns 2 and 5, and 3 and 6 respectively. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. The dependent variable in models 1 to 3 is the difference in village-level average Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. The dependent variable in models 4 to 6 is the difference in individual Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. Observations in models 1 to 3 represent villages. [a]The lean season minus post-harvest season change in average village level engagement of individuals in disputes. Individuals asked if they participated in a dispute in the previous four weeks (binary variable).

Table A9: Explaining Within-Individual Changes in TPPG MAO Across Seasons

Dependent variable	TPPG MAO Difference		
	(1)	(2)	(3)
Age (in years / 10)	-0.04	-0.04	-0.02
	(0.28)	(0.28)	(0.28)
Schooling (completed year)	0.10	0.08	0.08
	(0.10)	(0.09)	(0.11)
Number of household members	0.02	0.00	-0.01
	(0.09)	(0.10)	(0.09)
Cash earned in past 30 days per equivalence scaled HH member by HH head's (ths AFA) - Lean season[a]	-0.24	-0.21	
	(0.55)	(0.45)	
Cash earned in past 30 days per equivalence scaled HH member by HH head's (ths AFA) - Post-harvest season[a]	0.38		0.26
	(1.04)		(0.88)
Poverty index (z-score) - Lean season	-0.20	-0.11	
	(0.43)	(0.39)	
Poverty index (z-score) - Post-harvest season	0.30		0.21
	(0.55)		(0.49)
Village fixed effects	Yes	Yes	Yes
Constant	-2.43	-2.12	-2.23
	(1.83)	(1.53)	(1.87)
Observations	52	52	52
R-squared	0.22	0.21	0.21

Notes: OLS coefficients. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. The dependent variable in all models is the within-subject third party punishment game (TPPG) minimum acceptable offer (MAO) difference between MAO in the lean season and MAO in the post-harvest season. I control for village fixed effects in all models. Subsample of N=52 observations in each season with MAO consistent in both seasons. [a]Cash earned by household head per OECD equivalence scaled household member.

Table A10: Average Changes in TPPG MAO and Village-Level Intensity of Scarcity

Dependent variable	Village average of			Individual		
	TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$			TPPG MAO_{lean}-TPPG $MAO_{post-harvest}$		
	(1)	(2)	(3)	(4)	(5)	(6)
Δ Poverty z-score[a]	-1.09***	-1.16***	-1.13***	-1.03**	-1.11**	-1.10**
	(0.22)	(0.31)	(0.29)	(0.41)	(0.52)	(0.52)
Constant	-1.42***	-1.36***	-1.48**	-1.50***	-1.45***	-1.55***
	(0.37)	(0.37)	(0.45)	(0.32)	(0.33)	(0.34)
Observations	10	10	10	52	52	52
R-squared	0.33	0.27	0.28	0.04	0.03	0.04
Weight used	No weight	Sample population	Village population	No weight	Sample population	Village population

Notes: OLS coefficients. Columns 2, 3, 5, and 6 report weighted data using analytic weights. Weights used are the sample population and the reported population of the entire village based on interviews with community leaders for Columns 2 and 5, and 3 and 6 respectively. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. The dependent variable in models 1 to 3 is the difference in village-level average Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. The dependent variable in models 4 to 6 is the difference in individual Third Party Punishment Game (TPPG) Minimum Acceptable Offer (MAO) in the lean season minus the post-harvest season TPPG MAO. Observations in models 1 to 3 represent villages. [a]The lean season minus post-harvest season change in average village level normalized poverty index, in other words the intensity of a seasonal shock on a village level. See footnote 23 in the main text for description of how poverty index is constructed.

Table A11: Effect of Seasonality on DG Transfers, TPPG Transfers, and TPPG MAO (by Ethnic Group)

Sample	Tajik			Hazara		
Dependent variable	DG transfer (1)	TPPG transfer (2)	TPPG MAO (3)	DG transfer (4)	TPPG transfer (5)	TPPG MAO (6)
Lean season	-0.17	0.06	-1.89***	-0.22	-0.56	-1.51***
	(0.40)	(0.38)	(0.41)	(0.36)	(0.43)	(0.45)
Observations	72	72	63	64	64	60
R-squared	0.82	0.81	0.76	0.85	0.79	0.67
	F-test H$_0$: $\beta_{Lean,\ Tajik} = \beta_{Lean,\ Hazara}$					
$\beta_{Lean,\ Tajik} - \beta_{Lean,\ Hazara}$	-0.05	0.62	-0.38			
F-test p-values	(0.91)	(0.26)	(0.54)			

Notes: OLS coefficients. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Columns 1 and 4 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Columns 2 and 5 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Columns 3 and 6 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity. The last two rows compare the coefficients on *Lean season* from both *Tajik* and *Hazara* regressions, using an F-test.

Table A12: Giving Money as Charity and DG Transfers, TPPG Transfers, and TPPG MAO

Dependent variable	DG transfer		TPPG transfer		TPPG MAO	
	(1)	(2)	(3)	(4)	(5)	(6)
Lean season		0.02		-0.18		-1.31***
		(0.26)		(0.31)		(0.32)
Given money as charity[a]	0.55*	1.01**	0.10	0.23	1.39**	1.53**
	(0.30)	(0.43)	(0.35)	(0.59)	(0.57)	(0.60)
Lean season * Given money as charity		-0.87		-0.23		-1.34
		(0.78)		(0.87)		(0.85)
Observations	136	136	136	136	123	123
R-squared	0.82	0.82	0.79	0.79	0.66	0.74

Notes: OLS coefficients. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Columns 1 and 2 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Columns 3 and 4 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Columns 5 and 6 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). These columns show results for a subsample of N=123 observations (60 lean season, 63 post-harvest season) with consistent MAO. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity.

Table A13: Effect of Seasonality on DG and TPPG Transfers (Subsample of PAs Who Do Not Recall Their Own Previous Round DG Transfer)

Dependent variable	DG transfer (1)	TPPG transfer (2)
Lean season	-0.13 (0.31)	-0.15 (0.38)
Observations	92	92
R-squared	0.82	0.79

Notes: OLS coefficients. Clustered standard errors in parentheses. Clustering at individual level. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). Subsample of 46 PAs who did not recall their DG transfers from the previous, lean season round. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity.

Table A14: Differences Between Subjects Participating in Both Rounds and in One Round Only

Dependent variable	DG transfer (1)	TPPG transfer (2)	TPPG MAO (3)
Lean season ("single-round")	4.67***	3.59***	2.24***
	(0.59)	(0.55)	(0.77)
Lean season (both seasons)	4.30***	3.18***	1.93***
	(0.57)	(0.59)	(0.65)
Post-harvest season (both seasons)	4.49***	3.42***	3.60***
	(0.54)	(0.53)	(0.69)
Post-harvest season ("single-round")	5.18***	3.87***	3.86***
	(0.51)	(0.53)	(0.62)
Observations	235	235	200
R-squared	0.84	0.82	0.72
	F-test H_0: "both seasons" equals "single-round"		
Lean season p-value	0.33	0.24	0.48
Post-harvest season p-value	0.02	0.13	0.47

Notes: OLS coefficients. Regression without a constant. Robust standard errors in parentheses. *** denotes significance at 1 percent level, ** at 5 percent level and * at 10 percent level. In Column 1 the dependent variable is the dictator game (DG) transfer in ECUs (range from 0 to 10). In Column 2 the dependent variable is the third party punishment game (TPPG) transfer in ECUs (range from 0 to 10). In Column 3 the dependent variable is the third party punishment game (TPPG) minimum acceptable offer (MAO). Subsample of N=200 observations in Column 3 (23 lean season "single-round", 60 lean season participating in both seasons, 63 post-harvest season participating in both seasons, and 57 post-harvest season "single-round") with consistent MAO. All regressions include controls for age, schooling, number of household members, and village fixed effects. Constant is dropped to avoid perfect multicollinearity. The F-test compares the "both season" and "single-round" participant coefficients.

B Image Documentation

Figure A4: Explaining Instructions in a Group

(a) Experimental Subjects (b) Explaining Instructions in a Group

Figure A5: Individual Player Experimental Sessions

C Experiment Instructions

C.1 Group General Instructions

Before we begin I want to tell you about what we are doing here today and explain the rules that we must follow. We will be making a task in which you can get some money. Whatever money you will get in the task will be yours to keep and take home.

Maybe you won't get any money from the task, but if you decide to stay with us today, I will pass out 100 AFN to each of you to thank you for coming today. This money is not part of the task, it will be yours to keep. You will also get some snack and tea when you finish the task.

You should understand that this is not our own money. A University gave this money to us for research. This payment will not be regularly repeated in the future. It is not assistance, you will get the money for the task you will do here for us. It is not even a survey that you may have experienced before.

Please, also understand that there is no relation between our University and the organization People in Need delivering assistance in this area for a long period. I will not tell the organization about what you did here. Also, nothing you do here today will affect how the organization treats you or your community.

You should understand that there are no "right" or "wrong" answers in this task. Also, let me stress something that is very important. You were invited here without understanding what we are planning to do today. If you find that this is something that you do not wish to participate in, you can leave anytime.

Now, I will explain the task to you in the group. Later one after the other will come with me to carry out the task. It is important that you listen as carefully as possible, because only people who understand the task will actually be invited to participate. We will run through some examples here while we are all together.

You cannot ask questions or talk while we are here in the group. This is very important. Please be sure that you obey this rule, because it is possible for one person to spoil the task for everyone. If one person talks about the task while sitting in the group, we will not be able to carry out the task today. But do not worry if you do not completely understand the task as I show you the examples here in the group. Each of you will have time to ask questions when we sit alone together to be sure that you understand what you have to do. Now I will explain you what we are going to do during the task.

C.2 Group Games Instructions: Dictator Game

In one part of the task there will be two persons - Person A, and Person B. Both persons come from this village. None of you will know exactly with whom you are interacting. Only I know who will interact with whom and I will never tell anyone else.

Here are 200 AFN in 20 AFN bills that I will give to a Person A. Person A must decide how much of these 200 AFN he wants to give to Person B and how much he wants to keep for himself. I will not give any money to Person B. Person B takes home whatever Person A gives to him.

Here are some examples:

1. Suppose Person A gives 100 AFN to Person B, and keeps 100 AFN for himself. Person A goes home with 100 AFN (From the 200 AFN he had given 100 AFN to Person B and had kept 100 AFN for himself). Person B goes home with the 100 AFN from Person A.

2. Here is another example. Suppose Person A gives 0 AFN to Person B and keeps 200 AFN for himself. In this case, Person A goes home with 200 AFN. Person B doesn't have anything.
3. Here is another example. Suppose Person A gives 200 AFN to Person B and keeps 0 AFN for himself. In this case, Person A goes home with 0 AFN. Person B goes home with the 200 AFN from Person A.
4. Here is another example. This time suppose Person A gives 60 AFN to Person B and keeps 140 AFN for himself. In this case, Person A goes home with 140 AFN. Person B goes home with the 60 AFN from Person A.

Note again, there are no "right" or "wrong" answers in this task.

C.3 Group Games Instructions: Third Party Punishment Game

In another part of the task, there will be three persons - Person A, Person B, and Person C. All three persons come from this village. None of you will know exactly with whom you are interacting, but it will definitely not be the person with which you interacted in the previous part of the task. Only I know who will interact with whom and I will never tell anyone else.

Here is another 200 AFN. Person A must decide how much of these 200 AFN he wants to give to Person B and how much he wants to keep for himself. Person B takes home whatever Person A gives to him, but Person A has to wait until Person C has made a decision before finding out what he is going to take home. Person C is given 100 AFN. Person C can make three things with his 100 AFN.

1. He can pay 20 AFN to subtract 60 AFN of Person A's money, which Person A wanted to keep for himself. This money will be taken away; none of the Persons will get it. Person C will keep the remaining 80 AFN.
2. He can pay 40 AFN to subtract 120 AFN of Person A's money, which Person A wanted to keep for himself. This money will be taken away; none of the Persons will get it. Person C will keep the remaining 60 AFN.
3. He can pay nothing, keep all of the 100 AFN for himself and leave the money Person A wanted to keep for himself untouched.

Before hearing how much Person A has given to Person B, Person C has to decide what he wants to do for each of the possible amounts that Person A can give to Person B. This is 0 AFN, 20 AFN, 40 AFN, 60 AFN, 80 AFN, 100 AFN, 120 AFN, 140 AFN, 160 AFN, 180 AFN, or 200 AFN.

Here are some examples (All examples are shown with 20 AFN banknotes):

1. Suppose Person A gives 200 AFN to Person B and keeps 0 AFN for himself. Person C states that he would "do nothing" if Person A does this. In this case, Person A goes home with 0 AFN. Person B goes home with the 200 AFN from Person A, and Person C goes home with 100 AFN.
2. Here is another example. Suppose Person A gives 60 AFN to Person B and keeps 140 AFN for himself. Person C states that he would "do nothing" if Person A does this. In this case, Person A goes home with 140 AFN (He had kept 140 AFN for himself and Person C didn't decide to subtract money from him). Person B goes home with the 60 AFN from Person A. And Person C goes home with 100 AFN.
3. Here is another example. As before, Person A gives 60 AFN to Person B and keeps 140 AFN for himself. But now, Person C states that he would pay 20 AFN to subtract 60 AFN from Person A's money. In this

case, Person A goes home with 80 AFN (He had kept 140 AFN for himself minus the 60 AFN equals 80 AFN). Person B goes home with the 60 AFN from Person A. And Person C goes home with 80 AFN.

4. And a last example: Suppose Person A gives 120 AFN to Person B and keeps 80 AFN for himself. Person C states that he would pay 20 AFN to subtract 60 AFN from Person A's money. In this case, Person A goes home with 20 AFN (He had kept 80 AFN for himself minus the 60 AFN equals 20 AFN). Person B goes home with the 120 AFN from Person A. And Person C goes home with 80 AFN (100 AFN minus 20 AFN equals 80 AFN).

Again, there are no "right" or "wrong" answers in this task.

We will then call each of you in turn to make the task, starting with the person who picked number 1. In case you cannot read numbers, we will assist you.

When you finish the task, you have to wait until everybody has finished. Then I will call you in one by one again and I will tell you whether you have gained something. If yes, I will pay you that amount plus you will get the 100 AFN I promised you at the beginning.

We will not pay you for both tasks. At the end of the session you will have to pick a ball from a pouch to decide for which of the tasks you will get the payment. We will then give you the payment according to what color of the ball you picked. Please, take both tasks as if there was no other task before or after. Do you understand this?

Remember that you are not allowed to talk to the people still waiting to carry out the task. If you do talk to other people, the Assistant 3 will tell you to leave and not come back even if you may have earned some money.