Do informal transfers induce lower efforts? Evidence from lab-in-the-field experiments in rural Mexico*

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Abstract

How do informal transfers affect work incentives? The question matters in developing countries, where labor markets are intertwined with transfer networks. The tax-and-subsidy component of transfers would dilute work incentives, but their pro-social element could encourage people to work harder. Such crosscurrents are hard to disentangle because participation in informal networks is likely endogenous. We tackle this problem with a lab-in-the-field experiment that uses a real-effort task. Our main finding is that participants do not reduce their effort in the presence of transfers. This suggests that the impact of informal transfers may extend beyond just the sharing of risk.

Keywords: informal transfers, effort, moral hazard, warm glow, altruism
JEL codes: D64, C91, O12

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1 Introduction

Informal transfers are widespread in most developing countries.[1] While such transfers allow the sharing of risk for any given income distribution,[2] they may also affect the income distribution itself by distorting the incentives to undertake productive efforts or save in the first instance. Any relationship between transfers and efforts may be quite important in economies in which informal transfers are common: if transfers have a detrimental effect on effort, they may hamper economic development; if they have a positive effect on effort, they help promote economic development above and beyond the benefits of risk sharing. To evaluate the overall effects of informal transfers on welfare, it is therefore crucial to understand whether or not, and how, they affect the incentives to undertake productive efforts.

Several recent empirical studies conducted in Africa suggest that "kin taxes" have a detrimental incentive effect. Thus, Azam and Gubert (2005) and Baland et al. (2016) find that being on the receiving end of transfers (from emigrated relatives in the former study, from older siblings in the latter study) induces reduced work efforts. Di Falco and Bulte (2013) show that the presence of relatives induce lower levels of self-protection. The empirical analyses of Alby, Auriol, and Nguimkeu (2013), Grimm, Hartwig, and Lay (2016), and Squires (2016) indicate that kin taxes may be a significant barrier to entrepreneurship. Finally, a number of empirical and experimental studies, including Baland, Guirkinger, and Mali (2011), Di Falco and Bulte (2011), Jakiela and Ozier (2016), and Boltz, Marazyan, and Villar (2016), suggest that some people appear to undertake strategies to avoid having to share with kin.[3]

Such disincentive effects are exactly what would be expected if individuals are selfish and transfers are driven by a desire to comply with a social norm. However, if transfers are instead driven by an intrinsic desire to share (e.g., altruism or warm glow), they may in theory have a positive effect on productive efforts (Alger and Weibull, 2008, 2010). In this paper we report the results of a novel experiment designed specifically to provide insights on how the anticipation of making or receiving a transfer affects the incentives to undertake productive efforts. Compared to the literature, our experimental design introduces two novel features: it imposes the size and direction of transfers, and it relies on a real effort task. These features present two advantages. First, since subjects do not choose the transfers, the design allows to separate effort choice from transfer choice, a separation which cannot be obtained with empirical data, for which both effort and transfer decisions are endogenous. Second, since subjects choose a real effort instead of an investment from an endowment, the design makes it possible to obtain a measure of the motivation to exert effort in the presence of transfers, which is not tainted by the marginal utility of money. Our experiment is also novel in that it studies the effects of both giving and receiving transfers, whereas the literature has focused on the effects of giving transfers only. We further contribute to the literature on forced solidarity and incentives by conducting the experiment on a population outside Africa. By contrast to the findings cited above, we find that on average the anticipation of making and of receiving a transfer does not have a negative impact on effort.

In reality, informal transfers are often embedded in long-term, repeated interactions, in which the willingness to provide effort may be driven by a strategic repeated-interaction effect (Ligon, Thomas, and Worrall, 2002),[4]

[1] For a cross-country analysis, see Cox, Galasso and Jimenez (2006). See also Cox and Fafchamps (2008) for a survey of the literature on extended kinship networks, and Fafchamps (2011) for a survey of the literature on inter-household transfers.

[2] For evidence on such sharing of risk, see Rosenzweig (1988), Udry (1990), Townsend (1994), Ligon, Thomas and Worall (2002), Fafchamps and Lund (2003), Dercon and Krishnan (2003), Dubois, Jullien, and Magnac (2008), Mobarak and Rosenzweig (2013), and Attanasio, Meghir, and Mommaerts (2015).

[3] The field experiment conducted by Di Falco, Feri, Pin, and Vollenweider (2017) on how farmers allocate relatives' labor inputs in response to the productivity of their maize variety is also relevant in this context.

[4] See also Kimball (1988), Coate and Ravallion (1993), and Kocherlakota (1996).

or by a desire to signal pro-sociality (Bénabou and Tirole, 2006). Furthermore, participation in such interactions is often voluntary. Hence, in reality an individual's effort choice is likely affected by a complex set of factors, which, moreover, may interact. In order to understand the overall effects of these factors and how they may interact, a necessary first step is to study each of them in isolation. Thus, the decision to participate in risk-sharing groups has been studied by Barr and Genicot (2008) and Attanasio et al. (2012). We study another question, namely, does the potential pro-social element of transfers, which would encourage people to work harder, outweigh the tax-and-subsidy component of transfers, which dilutes work incentives?

To study this question, we propose an experimental design that imposes the composition of the group within which transfers take place, and that shuts down both the repeated-interaction and the signaling channel. Thus, we disregard limited commitment, by examining behavior in one-shot interactions, and we impose the direction as well as the size of any transfer, so that the only choice variable for subjects is the productive effort. Specifically, in this experiment each subject played four games. In each game the subject received an endowment and was given an opportunity to perform a real effort. The effort level chosen by the subject determined the probability of success, where success meant that additional income was generated on top of the initial endowment. In autarky games, the subject was simply able to keep any such additional income. In transfer games, subjects allocated to a donor role had to make a transfer (of fixed size) to someone else in case of success. Subjects allocated to a recipient role received a transfer (of fixed size) from someone else in case of failure. In any case, individuals who were potentially affected by these transfers were passive, so there was no strategic interaction. Reciprocity and signaling motives were ruled out, as much as possible, by ensuring that effort choices as well as payoffs were private information, and by matching subjects anonymously.

In the absence of other confounders, the comparison of effort choices in autarky games with those in transfer games provides a clean measure of how the mere anticipation of making or receiving a transfer affects effort. However, the choice of a real effort task for our experiment arises two potential concerns: ability and learning in performing the task. We control for the baseline ability with individual fixed effects in all of our estimations. Controlling for learning is particularly relevant in our design because the autarky games were always played before the transfer games in each treatment to avoid priming. We account for learning with two alternative approaches: first, by using observed effort as a dependent variable and including round dummies in our main estimations; and second, by estimating a linear learning model using the first two autarky games in one of our treatments, predicting effort in subsequent rounds for all observations, and then using the difference between predicted and observed effort as a dependent variable.

Importantly, in the autarky and transfer games played by any given subject, effort had the same effect on the expected amount paid by us to the community. Hence, if subjects made a greater effort in the presence of a transfer, this cannot be explained by a desire to extract more money from the experimenters. Instead, an effort increase can only be explained by a positive utility associated with giving a transfer when in a donor role and with avoiding receiving a transfer when in a recipient role.

In economics many classes of pro-social preferences have been studied, including altruism (Becker, 1974), warm glow (Andreoni, 1990), a concern for fairness or inequity aversion (Rabin, 1993; Fehr and Schmidt, 1999), caring about social efficiency (Charness and Rabin, 2003), and moral concerns (Alger and Weibull, 2013). It is beyond the scope of this paper to make a full analysis of all these preference classes. Instead, we limit our attention to the following three: selfishness, altruism, and warm glow. The payoff structures in the experiment allow us to infer whether the observed behaviors are more consistent with selfishness, altruism, or warm glow preferences. The focus on altruism and warm glow is natural in the context of transfers, because these preferences would result in different responses in the event that public transfers were introduced: indeed, public transfers would crowd out individual pro-social behaviors less if these are driven by warm glow than if they are driven by altruism.

The experiment was conducted in 16 small villages in rural Mexico (where informal transfers are common),[5] with a total of 536 participants. Within each village, the participants were randomly split into two groups, one of which was allocated to a donor treatment with transfers going to passive recipients, and the other to a recipient treatment with transfers coming from passive donors. Across villages, we varied the transfer size: in half of the villages the transfer represented 33% of the additional income, while in the other half the transfer represented 100%. While the former is more realistic, the latter allows to draw strong conclusions. Indeed, even in the presence of potential learning confounders, if subjects make some effort at all when the transfer is 100% of the additional income, it must be that they derive utility from the transfer.

Our main findings are as follows. First, we find that transfers do not decrease effort. Most of our preferred estimates of the effect of transfers, i.e., those based on the learning model correction, are positive, although only a few are marginally significant. Second, since this is true even in treatments where the transfer represents 100% of the potential additional income, and since the expected amount of money extracted from the experimenter is held constant across the games played by any subject, we can safely conclude that, on average, subjects derive positive utility from transfers.[6] In particular, the hypothesis that selfishness (or worse, spite) is the main motivation in the sample can be ruled out. Moreover, while in most cases we cannot conclude whether the behaviors lend more support to warm glow or to altruistic preferences, our finding that recipients do not change their effort significantly in response to a transfer, compared to autarky, is more in line with warm glow than with altruism, for no level of altruism could explain this response, as shown in the theoretical analysis.

We also provide suggestive evidence on whether the effort exerted in the transfer games varies with the identity of the passive party affected by the transfer. Specifically, we find positive, larger and slightly more significant effects when the transfer affects another individual drawn at random from a group of passive participants, than when it affects the whole pool of passive participants. However, the hypothesis that both effects are equal cannot be rejected. Additional results on heterogeneous effects suggest that the effect of transfers on effort increases, and thus it is more likely to be positive, with the religious homogeneity of the village.[7]

Our results are consistent with observations of pro-social behaviors in laboratory experiments.[8] In particular, they are qualitatively in line with findings in anonymous dictator games, where positive transfers suggest that the subjects derive a benefit from giving in such situations (Blanco, Engelmann, and Normann, 2011).[9] Such

[5]There is a large literature on behaviors in small villages in Mexico (e.g., Angelucci, De Giorgi, Rangel, and Rasul, 2009, 2010).

[6]This finding further allows us to rule out the possibility that subjects seek to earn a target income (Camerer et al., 1997): indeed, if this motivation was driving their behavior, they should make no effort in treatments where the transfer represents 100% of the potential additional income. Moreover, note that since effort generates the same expected additional income for the subject pool as a whole across the games, a higher effort in the presence of transfers would be inconsistent with full *ex post* sharing within the subject pool.

[7]Even though our subjects do not know the individual identity of the donor or recipient with whom they are being paired, in villages that are more homogeneous in terms of religious denomination the likelihood of being paired with another individual who shares the subjects' religious beliefs is higher, and this can have an effect on sharing behavior. Relatedly, Barr, Dekker and Fafchamps (2012) find that individuals belonging to the same religious group are more likely to share risk when agreements are enforced through social sanctioning than when they are so by the experimenter. Regarding other economic outcomes, Fisman, Paravisini and Vig (2012) show that shared religious beliefs between borrowers and lenders affect outcomes like loan amounts and repayment. Finally, these results are in line with Leider et al. (2009), who find that helping behaviors tend to be different towards friends than towards strangers, and the idea that an individual's behavior may depend on her sense of identity, which may depend on political, religious, educational, cultural, gender, or other identifying factors (Akerlof and Kranton, 2000; see also Kranton, 2016, and Kranton et al. 2016).

[8]For a recent survey see DellaVigna (2009).

[9]If given the possibility to pay a price to avoid giving, however, some subjects use this option (see, e.g., Dana, Weber, and

findings have also been made in experiments in developing countries, with variations on the anonymity of the recipient and the origin of the income to be shared.[10] Furthermore, the fact that our results lend stronger support to warm glow than to altruism is in line with other (different) experiments that have sought to discriminate between these two motives, including Palfrey and Prisbrey (1996), Erkal, Gangadharan, and Nikiforakis (2011), and Gangadharan et al. (2016).[11]

If externally valid, our findings suggest that informal transfers in our participating villages may allow risk to be shared without having a negative impact on productive efforts. However, as is well known external validity is by no means a trivial matter (Levitt and List, 2007). Three caveats may be particularly relevant here. First, in our experiment subjects undertake effort for a short amount of time, while in reality productive efforts take place over extended periods of time. Second, in reality individuals may use productive efforts and transfers as substitutes in order to assist others: hence, even if in reality it was possible to disentangle effort and transfer choices, it is not clear that transfers would be positively correlated with productive efforts. Third, the population we sample is exposed to the communal land system known as *ejido*. It is possible that this has a positive impact on pro-sociality.[12]

The experiment is described in the next section, with the theoretical model presented and analyzed in Section 3. The empirical specification is described in Section 4, the results are presented in Section 5, followed by robustness checks in Section 6. Section 7 concludes.

2 The effort-and-transfer experiment

2.1 Experimental design and session structures

Each subject played a series of four games, one of which was randomly drawn at the end of the experimental session to determine the payoffs; hence, the games were seen as independent by the participants. Each subject also received a payoff from being a passive party for other subjects, but since (s)he had no influence on this payoff, (s)he was informed about it only at the moment (s)he was paid. In each game the subject was presented with the same initial endowment and the same real effort task. The subject's effort level determined the probability that additional income was generated. All subjects played both *autarky* and *transfer* games: in autarky games, nothing else happened besides the effort and the ensuing potential generation of additional income; in transfer games, a transfer sometimes occurred between the subject and another, passive, party. Importantly, in all the transfer games the transfer direction and size were exogenously imposed. Hence, the

Kuang, 2007, DellaVigna, List, and Malmendier, 2012, and Lazear, Malmendier, and Weber, 2012). Our experiment did not include such an option.

[10] Thus, Ligon and Schechter (2012) conclude that the largest proportion of observed transfers in anonymous dictator games among villagers in rural Paraguay can be attributed to preference-related motives. Jakiela (2015) compares the behavior of U.S. students to Kenyans to study whether the willingness to share is different when the endowment in the dictator game derives from luck and when it is the result of a risk-free effort. In a comparative experimental study in which subjects could give own wealth, or steal or destroy others' wealth, Fafchamps and Vargas-Hill (2015) found that some gave and some stole, and that Kenyan and Ugandan subjects behaved more pro-socially than British subjects.

[11] There is also a non-experimental empirical literature that seeks to tease out the motivations behind transfers. See, e.g., Lucas and Stark (1985), Altonji, Hayashi, and Kotlikoff (1992), Cox, Eser, and Jimenez (1998), Foster and Rosenzweig (2001), De la Brière et al. (2002), and De Weerdt and Fafchamps (2011). However, the results reported in this literature are not directly comparable to ours, since empirical work relying on real-life transfers cannot shut down the reciprocity motive.

[12] To wit, Carpenter and Seki (2011) find that fishermen exposed to teamwork act more prosocially in public goods games than those who are not exposed to teamwork.

subject's only decision consisted in the level of effort to exert. Across the subjects we varied the role (*donor* vs *recipient*), as well as the transfer size, which was either *partial* (33% of the additional income) or *full* (100% of the additional income). While a partial transfer may be closer to real-life settings, the full transfer allows us to single out how a transfer affects effort when the effort has no effect on own wealth. This two-by-two design thus resulted in four treatments: *Donor-Partial* (DP), *Recipient-Partial* (RP), *Donor-Full* (DF), and *Recipient-Full* (RF). In the two *Donor* treatments, a subject who had successfully generated additional income had to make a transfer to a passive party. In the two *Recipient* treatments, a subject who had failed to generate additional income received a transfer from a passive party.

The experiment was conducted in 16 villages, each of which was allocated randomly either to the *Partial* or to the *Full* treatments. In each village the subject pool was divided randomly into two groups of equal size (usually 20), one of which was allocated to the *Donor* treatment, and the other to the *Recipient* treatment. The *Donor* and the *Recipient* treatments were run one after the other. Whichever group of individuals was not actively playing, was used (unbeknownst to these individuals) as passive players in some of the games played by the individuals in the active group. In sum, then, any subject in a *Donor* treatment was an active transfer donor and a passive transfer recipient, while any subject in a *Recipient* treatment was an active transfer recipient and a passive transfer donor.

The first four of the six boxes in Figure 1 show the games played in the four treatments (the *Partial* treatments are in the left column and the *Full* treatments in the right column), and the payoff consequences of success (S) and failure (F) in each game, both to the active subject and to the associated passive party (the first number is the initial endowment, an italicized number the additional income, and a bold number a transfer).[13] The following set of three games was qualitatively similar across the four treatments: the *Autarky* game, the *One-to-One* game, and the *Pool* game. In the *One-to-One* game each active subject was matched anonymously with one passive individual (and each passive individual was matched only with this active subject); in the *Pool* game the passive party was the pool of passive individuals, who were all equally affected by any transfer to/from an active subject (in Figure 1, n denotes the total number of passive subjects, n_s the number of other active subjects who succeeded, and n_f the number of other active players who failed). Note that in each treatment the subject's initial endowment as well as the additional income was the same in these three "core" games; furthermore, the transfer amount as well as the initial endowment to any passive player was the same in both of these transfer games, and the active subject was always at least as rich as any passive transfer recipient.

On top of the three core games, which are used for the main analysis, two additional variations were introduced. First, subjects in the *Partial* treatments played an *Autarky Low* game. This game had the same net effect on the active player's payoff as the transfer games, and thus allows to compare two payoff-equivalent games, one with and one without a transfer. Second, subjects in the *Full* treatments played a *Public* transfer game, where the transfer source/destination was the health center of the village, i.e., outside of the experimental setting. The *Public* game will be used for a robustness test, because it differs from the core transfer games in that the transfer no longer affects the participants in the experiment directly, but the village to which they belong. Thus, the *Public* game can be used to determine to what extent in the core games subjects are driven by a desire to extract money from us to the subjects.

Upon completing the games and the ability tests (see more on these below), and while the subjects were still in the lab, all subjects were asked post-experimental questions. First, subjects were asked questions pertaining

[13] All the numbers are expressed in *points*, the experimental unit, worth MXN 0.50 each (when the experiment was conducted, the exchange rate was approximately MXN 13 = USD 1). The overall average payoff turned out to be MXN 92 (it was MXN 59.55.across the *Donor-Full* treatments, MXN 122.02 across the *Donor-Partial* treatments, MXN 62.30 across the *Recipient-Full* treatments, and MXN 122.43 across the *Recipient-Partial* treatments).

to the *One-to-one* game. In the *Donor-Partial* treatment we asked subjects if they would have liked to give more than 25, in the *Donor-Full* treatment we asked if they would have liked to give less than 75, and how much more/less in case of a positive answer. This allows us to check whether the results are driven by the transfer size being imposed by us. Second, we asked each subject in the *Recipient* treatments if (s)he would reveal his/her effort level to the individual with whom (s)he was matched in the *One-to-one* game if given the opportunity to do so. The answers to this question will allow to check if the results were driven by the non-observability of effort.

At the very end of the session, subjects filled out a questionnaire with questions pertaining to standard individual characteristics and transfer behaviours in the past year.

The two boxes at the bottom of Figure 1 summarize the session structures. There were two alternative orders for the *Partial* treatments and two alternative orders for the *Full* treatments.

2.2 The nuts-and-bolts effort task

Given our focus on effort choices, we propose a real effort instead of a "chosen effort" (Bruggen and Strobel, 2007) task. We do so for two reasons: because it removes income effects, and because the population that was sampled for the experiment might be unaccustomed to abstract reasoning. The effort task consisted of threading nuts onto bolts.[14] For each fully threaded nut (all the nut-bolt pairs were identical), the subject increased the probability of generating additional income by 0.1, a probability which was zero for zero threaded nuts.[15] Ten fully threaded nuts thus guaranteed success. In each game the subject had one minute to thread nuts onto bolts.[16] Curtains ensured that subjects could not observe each other's effort choices or communicate. Furthermore, to minimize the experimental pressure to exert effort, each subject had on his/her table a fresh newspaper to look at during the imparted time.

Clearly, threading nuts onto bolts in a short period of time is a task that requires ability. To get measures of ability, we let each subject take two incentivized ability tests, one at the beginning and one at the end of the experimental session. Each ability test consisted in rewarding the participants with one point for each properly threaded nut in one minute. The first ability test gives us information on baseline ability in performing the task. In Appendix B.2, we show that the baseline ability distribution, as measured with this first test, does not differ significantly across treatments and roles. Furthermore, in our main specifications, we include individual fixed effects to control for baseline ability and for any other individual-specific factors that might affect effort

[14]A variety of real effort tasks have been used in the literature, such as solving anagrams (Charness and Villeval, 2009), stuffing envelopes (Carpenter et al., 2010), counting zeros (Abeler et al., 2011), and moving sliders (Gill and Prowse, 2012). We chose a manual task that did not require the use of computers, and which allowed us to use the same materials in all the sessions without experiencing damage.

[15]Concretely, for each fully threaded nut, the subject earned one ball. Thus, upon completing the four games, each subject had earned a certain number (between 0 and 10) of balls in each game. Once the game that would be used to calculate the payoffs had been picked, a traditional style bingo cage with balls numbered from one to ten was used to draw one ball. Any subject who had threaded a number of nuts equal to or greater than the number on the ball drawn in the relevant game generated additional income, whereas those who had threaded a smaller number did not generate additional income.

[16]In the theory section below, we assume that the cost of effort is strictly convex in effort. This allows us to focus on interior solutions. In the experiment the probability of success is linearly increasing in the number of threaded nuts and it may therefore appear that the cost of effort is linear. However, since subjects were time-constrained effort really consisted in the speed at which nuts were threaded, it is sensible to believe that marginal cost of effort was indeed increasing in effort. Furthermore, the fact that out of 2,144 observations (536 participants who played four games each) there are 23 observations where zero nuts were threaded, and 209 where ten nuts were threaded, indicates that our focus on interior solutions is sensible. It may also be that the subjects enjoyed performing the task. This concern is irrelevant for our main results, which rely on within-subject comparisons of efforts.

and that remained constant throughout the session.

Given that the effort task is performed repeatedly, learning may also arise in our experiment. This is of particular concern in our case because we always let the subjects play the transfer games after the autarky game(s) to avoid the priming that the introduction of a transfer, and its subsequent removal, could induce and because of the pedagogical advantage of starting with the simplest game in a population unaccustomed to abstract reasoning (see Figure 1). Thus, subjects might have accumulated more learning when they play the transfer games than when they play the autarky game(s). Since our empirical strategy consists in comparing the effort in the transfer games to the effort in the autarky game(s), learning is a potential confounding variable that must be adequately controlled for.

We adopt two alternative approaches to control for learning in our estimations that compare effort between the transfer and autarky games. First, we include round dummies in the regressions. As mentioned, the autarky games were always played before the transfer games, and the order between the transfer games (*One-to-One* versus *Pool*) was varied. As a result, this approach accounts for the fact that the games being compared are played in different rounds for different session orders, letting the dummies absorb the difference arising from additional rounds of practice. Second, as detailed next, we estimate a simple learning model and then use the results of this model to purge observed effort from potential learning.

To get an estimate of learning, we use the effort observed in the two autarky rounds played in the *Partial* treatments.[17] These two games are comparable: they both involve risk, the same initial endowment, and no transfers. Their similarity is crucial to attribute most of the difference in effort between these two rounds in the *Partial* treatments to learning, i.e., to the effect of an additional round of practice on the number of nuts threaded.[18] The only difference between them is that the expected return to effort is lower in the *Autarky Low* game, compared to the *Autarky* game. However, since the mean difference in effort between these two games is not statistically significant (see Appendix B.3), it is arguably sensible to pool them in our learning estimation.[19]

Given that the two autarky games in the *Partial* treatments were played right after the first ability test, our learning estimate would reflect the learning about the effort task that occurs relatively early in the session. Furthermore, by using these two autarky games, which were played before the introduction of any transfers, we ensure that our estimated learning does not incorporate any of their effect on effort.

Let e_{ir} denote effort of individual i in the autarky game played in round r in a partial treatment, where $r = 1, 2$. We estimate the average change in effort between those two rounds, which we attribute mostly to learning, with the following regression:

$$e_{i2} = \eta * e_{i1}. \tag{1}$$

We then use the estimated value $\hat{\eta}$ to calculate, for each individual i, the predicted effort in each round $r = 2, 3, 4$ of the experiment as follows:

$$\hat{e}_{ir} = \hat{\eta}^{(r-1)} * e_{i1}, \tag{2}$$

[17]We thank the Associate Editor, who remained anonymous, for suggesting this method.

[18]We refrain from using the first ability test for estimating learning because the points per threaded nut in the practice rounds and the autarky rounds were different; the payoff was deterministic in the ability test rounds but stochastic in the autarky rounds; the subjects were sure that they would get the payoff earned in the practice rounds but there was only a 25% chance that any given autarky round would be used as basis for payment; and effort was not bounded above in the practice rounds as it was in the autarky and transfer rounds.

[19]As complementary evidence, we also perform a difference-in-differences (DD) estimation using the fact that, by design, the first two experimental games in the *Partial* treatment are autarkies, whereas the first and second games in the *Full* treatment are an autarky game and a transfer game, respectively. The details and results of this alternative, suggested anonymously by the Associate Editor, are in Appendix B.4.

where e_{i1} is i's effort in the first of the four games after the initial ability test, which is always an autarky game. This approach assumes that subjects learn at a constant rate in each subsequent round, equal to the estimated rate between the first two games. We use the effort in the first game ($r = 1$) to calculate the effort in all subsequent rounds, instead of the effort in the corresponding previous round, to avoid any contamination of the predicted effort due to presence of transfers in the later rounds.

As a final step in this approach, we estimate our main specifications (see Section 4) using observed effort for round $r = 1$ and the following measure of "clean" effort, i.e., effort purged from learning effects, for rounds $r = 2, 3, 4$:

$$e^c{}_{ir} = e_{ir} - \hat{e}_{ir}. \tag{3}$$

The results of the learning estimation and a comparison between raw and "predicted" efforts are presented in detail in Appendix B.3. In that appendix, for robustness we also present the results of a learning estimation using a linear specification with a constant, and a and a log-linear specification with a constant. In general, the results of these robustness checks are consistent with those obtained with the learning specification described in this section.

2.3 Procedures

In each of the 16 villages where a session took place, subjects were recruited by way of public announcements supported by the village authorities, a day before as well as on the day of the session.[20] Applying a "first come, first served" rule for men and women separately, in each village we sought to obtain a total of 40 subjects, gender-balanced whenever possible. At most one representative per household could participate; furthermore, although we selected small localities (see Table C1), the questionnaire data suggest that only a small share of the subjects were first-degree relatives (see Table 1). Upon arrival, each participant picked a card from an opaque bag. Each card had a number that allowed us to track the subject anonymously, and a symbol that was used to assign the participant to one of the two groups. Half of the subjects were thus led to the lab field, while the other half were shown to another room; the latter group got a snack while waiting for their turn to be led to the field lab. In the *One-to-one* games, the active subjects were randomly and anonymously matched with the passive subjects, so that each active and each passive subject was matched with exactly one individual. In the *Pool* games any transfer affected all the passive individuals equally.

Almost all the sessions were conducted in the afternoon, after the end of the agricultural labour day, in the village's own school building.[21] These buildings all had at least two independent rooms, one of which was used as the field lab and another one as a waiting room for the passive subjects. The lab rooms were large enough to contain twenty tables and chairs arranged in four columns; all subjects faced the experiment director and could properly see her and hear the instructions. We installed opaque curtains between the columns to provide privacy for the subjects (they could not see above the backs of those sitting in front of them, and they were not allowed to turn around to look at those sitting behind them). The experiment director was the same in all the sessions. No written instructions were provided, the protocol was read out loud (and clarified in case of need) and all materials were visual and text-free due to the low literacy levels of the participants. Protocols and experimental materials can be found in the Appendix. Translation of the instructions by a native Mayan speaker was provided in those villages where some subjects did not understand Spanish.

[20] Public announcements consisted of loudspeaker announcements and written ads, and by the village authorities spreading the information within the village.

[21] All took place in a primary school, except two, that took place in a high school and in an *ejido* meeting hall, respectively.

In the lab room, each subject sat in front of the table assigned to his or her number. On the table there was one bowl with nuts and one with bolts, as well as a fresh newspaper. Participants were explicitly instructed to not touch any material before receiving the instruction to do so from the experiment director. When the first group had completed the ability tests and the games, and had answered the question(s) (see Figure 1), the two groups would quietly change rooms without interacting. The first group then got a snack and completed the post-experimental questionnaire in the auxiliary room; the second group filled out this questionnaire, also in the auxiliary room, upon having completed its session in the field lab room. Illiterate participants received help by a native Mexican assistant or Mayan native speaker to fill out the questionnaire.

Once both groups had performed their experimental session, a child randomly picked a colored card (from an opaque bag) in front of everybody; the card determined which of the four games would be used to calculate the payoffs (if it was the one-to-one game, then it was the one-to-one game both for donors and for recipients, etc). Finally, to ensure transparency of the payments calculation, the lottery was performed in front of all the participants. The average payment was 92 pesos, close to the minimum wage of an agricultural worker.[22]

Each group spent between 30 and 45 minutes in the field lab room, depending on the need for translation. The complete session, from the arrival to the the departure of the subjects, usually lasted about three hours.

2.4 Field setting and locality selection

The experimental sessions were conducted in July 2014 in the state of Campeche, Mexico. Although the predominant economic activity in Campeche is oil and natural gas extraction (84% of the state GDP according to the National Economic Census 2014), its inhabitants are poor: 43.6% of the state's population is in poverty situation and 11% in extreme poverty (source: CONEVAL 2015).[23] Agricultural activities represent 0.5% of the state GDP in 2014 (source: National Economic Census 2014). Campeche's soils are poorly suited for agriculture, and an important proportion of the agriculture is developed by small and self-subsistence farmers; water is more abundant in the South than in the North of the state. Land ownership is governed by a special system of social land tenure called the *ejido*. An ejido's members (the *ejidatarios*) have collective rights over land.[24] In Campeche, 52% of the total agricultural land area is still cultivated collectively by 60,207 farmers (this represents 7.3% of the state population; of the ejidatarios, 19.5% are women and 80.5% men) in 384 different ejidos (source: National Agrarian Registry Office, RAN). In 2010 a quarter of the population resided in rural localities with less than 2,500 inhabitants, and about 12% of the state population spoke an indigenous language (source: National Population and Housing Census 2010).[25]

The experimental sessions were conducted in rural localities with less than 1,000 inhabitants and a high proportion of subsistence farmers. All of the localities were organized as ejidos, so the institutional and organizational set-up was homogeneous across localities. We excluded seven municipalities due to increasing violence

[22] Although in Mexico there is no legal minimum wage for agricultural workers, we took as reference the general minimum wage in Campeche, which was 63.7 pesos per day, while the minimum wage for an operator of agricultural machinery was 93.6 pesos per day (http://www.conasami.gob.mx).

[23] Extreme poverty refers to persons who cannot afford the cost of a minimum food basket that allows them to carry out a minimal level of physical activity and who, in addition, show between three to six social deficiencies.

[24] Land can be collectively or individually cultivated, depending on the land use decisions taken (by majority rule) in the ejido assembly. Traditionally, ejido plots that were cultivated by individuals could not be sold, rented or put forward as collateral for credit, and no labour outside of the ejidatarios's family could be hired to work on this land. In 1992, the government implemented a reform to give ejidatarios full property rights over their plots and to loosen some other constraints imposed on ejido land (Sanderson, 1984.)

[25] Available online through the National Institute of Statistics and Geography (www.inegi.org.mx).

(Candelaria, Carmen and Escarcega), scarcity of agricultural activity and/or closeness to major developed areas (Calakmul, Campeche, Palizada and Tenabo), and important recent immigration from Central America. We selected sixteen villages from the remaining four municipalities (Calkini, Hecelchakan, Hopelchen and Campoton), eight of which are located in the North and eight in the South. Half of each set of villages was randomly assigned to a *Partial* treatment and half to a *Full* treatment. Figure 2 shows a map with the localities as well as the treatment allocation.

Tables C1 and C2 show that northern and southern localities are similar in some mean socio-economic characteristics, like average fertility and years of schooling, among others. However, these localities also differ along several dimensions that, as we explore later, might affect giving patterns. For instance, the share of common use land is much higher in the north (82%) compared to the south (32%). Northern localities also have a much higher shares of indigenous population (almost 93% versus 9% in southern localities), which reflects a cultural difference that might affect giving patterns, and seem to be poorer than southern localities (see Table C2).[26] The North seems to be less religiously diverse, with 82% of people declaring themselves Catholic, compared to the South, where 32% of people profess a faith other than Catholicism. This could affect the willingness to share with others in the community due to greater community homogeneity and/or cohesion in the north.

The post-experiment questionnaire data (see Table 1) reveals subjects were much more likely to have given a transfer during the year prior to the experimental session in the south than in the north (64.5% vs 45.7%), whereas the share of subjects who had received a transfer was about the same (39.4% vs 38.6%).[27] This suggests that within-village transfers may be less common in the northern villages. Relatedly, southern localities have, on average, higher travel times to the next big town, a feature which may explain why people need to rely more on the community in the South than in the North.

In comparison to the population in the 2010 Mexican Census, Table C3 shows that in our sample there is over-representation of women (especially young women), of individuals with smaller dwellings, and of people born in the state. The first two are not very surprising: although we tried hard to achieve gender balance, it was often harder to recruit men due to their work; moreover, poorer people may have a smaller opportunity cost of time. It is also important to keep in mind that the Census was collected in 2010 while our survey was carried out in 2014, so some differences are to be expected.

Finally, Tables C4 and C5 show that most of the variables just discussed are balanced across the treatments, implying that the correlation between individual characteristics and treatment is virtually non-existent.

3 Theoretical predictions

Let consumption utility from final wealth w be $u(w)$, where u is increasing and strictly concave, and the cost of effort $e \in [0, 1]$ be $c(e)$, where c is strictly convex. The *material payoff* is consumption utility minus the cost of effort, $u(w) - c(e)$. Low effort levels may be enjoyable, but effort is costly at the margin when it is high enough. Formally, there exists some $0 \leq \hat{e} < 1$ such that $c'(\hat{e}) = 0$, where $\hat{e}$ is the *voluntary effort*. Let the

[26] As measures of wealth, we constructed indexes of dwelling characteristics and household assets by taking the first component in a principal components analysis (Filmer and Pritchett, 2001). The variables used for the dwelling index are dummy variables for whether the participant or her family owns their home, availability of running water, toilet, electricity and dirt floor, the number of rooms and light bulbs. For the asset index, we used dummy variables for whether the household owns other real estate properties or land, vehicles, TV, radio, cellphone, gas stove, small kitchen appliances, refrigerator, washing machine, books; productive assets like agricultural machinery or animals; and small livestock.

[27] To the best of our knowledge, no external data on village-level transfers is available for the state of Campeche.

probability of success be e, let $y > 0$ denote the active player's initial endowment, $z > 0$ the additional income generated in case of success, and t a transfer. We limit the analysis to parameter specifications as well as results that are relevant for the experiment, and assume that there is a unique interior solution.

In the *Autarky* game, the expected utility of the active individual simply coincides with the expected material utility, which equals

$$e \cdot u(y+z) + (1-e) \cdot u(y) - c\,(e)\,. \tag{4}$$

The effort, e_A, is implicitly defined by the necessary first-order condition:

$$c'\,(e_A) = u\,(y+z) - u\,(y)\,. \tag{5}$$

We analyze how this effort compares to that in the one-to-one and in the pool games in turn.

3.1 The *One-to-one* games

We study the effort choice of an active player under three alternative preferences: selfishness, altruism, and warm glow. Let $\pi\,(y, z, t, e)$ denote the individual's own expected material payoff, and $\pi_M\,(y_M, t, e)$ that of the (passive) individual with whom the active individual is matched. Then the utility that the active player derives is $\pi\,(y, z, t, e)$ if (s)he is selfish,

$$\pi\,(y, z, t, e) + \alpha \cdot \pi_M\,(y_M, t, e) \tag{6}$$

if (s)he is altruistic, where $\alpha \in [0, 1]$ is the degree of altruism towards the passive player,[28] and

$$\pi\,(y, z, t, e) + e \cdot [(1+t)^{\gamma} - 1] \tag{7}$$

if (s)he has warm glow preferences, where $(1+t)^{\gamma} - 1$ is the individual's intrinsic benefit from making a transfer in a donor situation, or from avoiding the shame of receiving a transfer in a recipient situation, for some $\gamma \geq 0$ (note that $(1+t)^{\gamma} - 1 = 0$ for $\gamma = 0$, and that $(1+t)^{\gamma} - 1 > 0$ for any $\gamma > 0$, since $t > 0$).

In the *One-to-one donor* game, the active player makes a transfer $0 < t \leq z$ to a passive player if success occurs; if failure occurs, nothing happens. Hence:

$$\pi\,(y, z, t, e) = e \cdot u(y+z-t) + (1-e) \cdot u(y) - c\,(e)\,, \tag{8}$$

and

$$\pi_M\,(y_M, t, e) = e \cdot u(y_M + t) + (1-e) \cdot u(y_M). \tag{9}$$

Like in the experiment, assume that the player who potentially makes a transfer (here, the active player) is always better off: $y \geq y_M$ and $y + z - t \geq y_M + t$. Decreasing marginal utility of consumption ($u'' < 0$) then implies

$$u(y_M + t) - u(y_M) > u(y+z) - u(y+z-t). \tag{10}$$

Selfishness: For a selfish individual, a transfer has an unambiguously negative effect on the effort compared to the autarky game (formally, this follows immediately from the first term in (8) being strictly smaller than that in (4), and c being strictly convex).

[28] It should be noted that pure altruism is often taken to mean that the individual attaches some weight to the other's utility, and not to the other's consumption utility as is here assumed. This is particularly important if the altruistic inclination is mutual. However, it is well known that as long as each individual attaches a weight less than one to the other's utility, with one weight being strictly smaller than one, the utilities may be written as we do here (see, e.g., Lindbeck and Weibull, 1988).

Altruism: Although a transfer has a disincentive effect by reducing own net wealth in case of success, an altruist playing a *One-to-one donor* game also cares about the transfer recipient's consumption utility and therefore also perceives a benefit from making the transfer. To see that this can lead to a higher effort than under autarky, note that the effort, e_D^{α}, which is implicitly defined by the necessary first-order condition

$$c'\left(e_D^{\alpha}\right) = u\left(y+z-t\right) - u\left(y\right) + \alpha \cdot \left[u\left(y_M+t\right) - u\left(y_M\right)\right], \tag{11}$$

exceeds the autarky effort e_A in the extreme case in which $\alpha = 1$ (by virtue of (10) and strict convexity of c), even if the transfer represents 100% of the additional income ($t = z$). This result arises because the donor is always better off than the recipient, so that the transfer has a greater effect on the recipient's than on the donor's consumption utility.[29] Since at the other extreme ($\alpha = 0$) one obtains $e_D^{\alpha} < e_A$, by continuity we can conclude that there exists $\hat{\alpha} \in (0,1)$ such that $e_D^{\alpha} > e_A$ if $\alpha > \hat{\alpha}$, $e_D^{\alpha} < e_A$ if $\alpha < \hat{\alpha}$, and $e_D^{\alpha} = e_A$ if $\alpha = \hat{\alpha}$.

Warm glow: The warm glow produced by the transfer on the individual may outweigh the disincentive effect that the transfer has through its reduction of own net wealth; as with altruism, this may be true even in the extreme case where $t = z$. To see this, compare the first-order condition for an interior solution,

$$c'\left(e_D^{\gamma}\right) = u\left(y+z-t\right) - u\left(y\right) + \left(1+t\right)^{\gamma} - 1, \tag{12}$$

with (5). We can thus conclude that there exists $\hat{\gamma} > 0$ such that $e_D^{\gamma} > e_A$ if $\gamma > \hat{\gamma}$, $e_D^{\gamma} < e_A$ if $\gamma < \hat{\gamma}$, and $e_D^{\alpha} = e_A$ if $\gamma = \hat{\gamma}$.

A final remark on the *One-to-one* games in the *Donor* treatments: in the *Donor-Partial* treatment the transfer is smaller and the endowment of the passive player is larger than in the *Donor-Full* treatment (see Figure 1). A selfish active player would therefore choose a strictly higher effort in the *Donor-Partial* than in the *Donor-Full* treatment. By contrast, an active player with altruistic or warm glow preferences would either make a higher or a lower effort in the *Donor-Partial* than in the *Donor-Full* treatment (this is easy to verify by way of examples).

Next we study the *One-to-one* game in the *Recipient* treatments. The active player receives a transfer $0 < t \leq z$ from the passive player if failure occurs; if success occurs, nothing happens. Hence:

$$\pi\left(y,z,t,e\right) = e \cdot u(y+z) + (1-e) \cdot u(y+t) - c\left(e\right), \tag{13}$$

and

$$\pi_M\left(y_M,t,e\right) = e \cdot u(y_M) + (1-e) \cdot u(y_M - t). \tag{14}$$

Again, assume that the player who potentially makes a transfer (here, the passive player) is always better off: $y_M \geq y + z$ and $y_M - t \geq y + t$. Decreasing marginal utility of consumption ($u'' < 0$) then implies

$$u(y+t) - u(y) > u(y_M) - u(y_M - t). \tag{15}$$

Selfishness: The transfer has an unambiguous disincentive effect (this follows from the second term in (13) being strictly larger than that in (4), and c being strictly convex).

Altruism: An altruist playing a *One-to-one* recipient game suffers a utility loss from seeing the transfer affect the donor's consumption utility. However, the desire to avoid this loss is never strong enough to induce a higher effort than under autarky. To see this, note that the effort, e_R^{α}, which is implictly defined by the necessary first-order condition

$$c'\left(e_R^{\alpha}\right) = u\left(y+z\right) - u\left(y+t\right) + \alpha \cdot \left[u\left(y_M\right) - u\left(y_M - t\right)\right], \tag{16}$$

[29] Because of this, it is straightforward to verify that if the individual instead were inequity averse (i.e., who attaches a negative weight to the difference in the individuals' final wealth), the predictions would be qualitatively similar.

does not exceed the autarky effort e_A even when $\alpha = 1$ (by virtue of (15)). This is because the active player is always worse off than the passive donor, so that the effect on the consumption utility of the passive individual is always smaller than that on the active player. We thus conclude that $e_R^\alpha < e_A$.

Warm glow: Since the shame of receiving a transfer is independent of the actual effect of the transfer on the passive donor, the benefit from avoiding the shame of receiving a transfer may outweigh the disincentive effect of the transfer on own net wealth; this may be true even in the extreme case where $t = z$. To see this, compare the first-order condition for an interior solution,

$$c'\left(e_R^\gamma\right) = u\left(y+z\right) - u\left(y+t\right) + \left(1+t\right)^\gamma - 1, \tag{17}$$

with (5). In sum: there exists $\tilde{\gamma} > 0$ such that $e_R^\gamma > e_A$ if $\gamma > \tilde{\gamma}$, $e_R^\gamma < e_A$ if $\gamma < \tilde{\gamma}$, and $e_R^\gamma = e_A$ if $\gamma = \tilde{\gamma}$.

We conclude the theoretical analysis of the *One-to-one* games by making observations to be used to assess the results in between-subjects comparisons. First, a glance at the payoffs in the *Partial* treatments (see Figure 1) reveals that if an individual were to play the *One-to-one* game in the *Donor* treatment and the *One-to-one* game in the *Recipient* treatment, (s)he should make less effort in the latter than in the former, whether (s)he is selfish, altruistic or has warm glow preferences. Second, in the *One-to-one* games in the *Full* treatments, effort has no impact on own payoff, and the subject's payoff as well as the transfer size are the same. Since both selfishness and warm glow preferences render its carrier insensitive to the actual impact of their behavior on others, if an individual with such preferences were to play the *One-to-one* game in the *Donor* treatment and the *One-to-one* game in the *Recipient* treatment, (s)he should make the same effort in both. By contrast, if the individual is altruistic, (s)he should make less effort in the latter, because (s)he is poorer than the passive individual in the *Recipient* treatment while the opposite is true in the *Donor* treatment.

3.2 The *Pool* games

In the two *Pool* games there are n active players and n passive players. In the *Pool donor* game the initial endowment is y for each active player and $y_D < y$ for each passive player. A transfer t is taken from each successful active player and divided equally among the n passive players. In the *Pool recipient* game the initial endowment is y for each active player and $y_R > y$ for each passive player. A transfer t is given to each active player who does not succeed, a transfer to which each passive player contributes t/n. In both *Pool* games t is such that a player who makes a transfer is at least as well off as a player who receives a transfer. Formally, in the *Pool donor* game, $y - t \geq y_D + n_s \cdot (t/n)$, where n_s is the number of successful active players; likewise, in the *Pool recipient* game, $y + t \leq y_R - n_f \cdot (t/n)$, where n_f is the number of active players who fail.

Selfishness, warm glow: Starting with individuals with either selfish or warm glow preferences, since the size of the transfer matters but its destination or source does not, the following prediction is immediate. An active player who is selfish or has warm glow preferences makes the same effort in a *One-to-one donor* game and in a *Pool donor* game in which the endowment and the transfer amounts are the same. The same prediction obtains for a *One-to-one recipient* game and a *Pool recipient* game in which the endowment and the transfer amounts are the same.

Altruism: Turning now to altruism, an altruistic player cares not only about the effect of the transfer she makes on her own material well-being, but also on its effect on the n passive individuals. She may also care about the other active individuals but this is irrelevant since her effort has no effect on these; hence we disregard this possibility when writing the utility. An unsophisticated altruist would choose a higher effort than in the *One-to-one donor* game, on the account that the impact of a transfer t/n to n individuals, each with endowment y_M, exceeds the impact of a transfer t to one recipient with endowment y_M:

$$c'\left(e_D^{\alpha}\right) = u\left(y + z - t\right) - u\left(y\right) + n \cdot \alpha \cdot \left[u\left(y_M + \frac{t}{n}\right) - u\left(y_M\right)\right]. \tag{18}$$

However, such reasoning disregards the possibility that the recipients receive transfers from other active players. For sophisticated players there is thus a countervailing force. In Appendix A, we show that on balance the equilibrium effort in a game between sophisticated and equally altruistic players may be higher or lower than in the *One-to-one donor* game.

Figure 3 summarizes the predicted effort comparisons that we will refer to in the empirical sections.

4 Empirical specification

To estimate whether effort is affected by (1) the expectation of receiving or giving a transfer, and (2) the source/destination of the transfer, we first perform a within-subject analysis. We compare the effort of a given participant in the three core games, namely, *Autarky*, *One-to-one*, and *Pool* (see Figure 1). Specifically, we estimate the following OLS regression, for each of the four treatments:

$$e_{ig} = \alpha + \beta_1(\text{One-to-one}_{ig}) + \beta_2(\text{Pool}_{ig}) + \delta_i + u_{ig} \tag{19}$$

where e_{ig} is the number of threaded nuts of participant i in game g, One-to-one_{ig} and Pool_{ig} are indicator variables equal to 1 if the effort corresponds to the transfer game at hand, and zero otherwise (the reference is thus the *Autarky* game), δ_i are individual fixed effects, and u_{ig} is an error term. The coefficients of interest are β_1 and β_2, which measure the effect of a transfer from/to another anonymous participant, and from/to a pool of participants, on effort compared to the autarky situation. With this specification we can directly test for two hypotheses: $\beta_1 = 0$, $\beta_2 = 0$, and $\beta_1 = \beta_2$.[30]

Our 2x2 experimental design also allows us to make two between-subjects comparisons to see how the size of the transfer and the role (donor/recipient) affect effort.

First, to determine whether the effect of a transfer on effort depends on the size of the transfer, we compare the differential effort change of participants due to the introduction of a transfer between the *Donor-Full* and *Donor-Partial* treatments. Note that we can conduct this type of analysis only for donors, since for recipients both the transfer and the endowment were different between the *Partial* and the *Full* treatments. Specifically, the estimating equation is given by:

$$\begin{aligned} e_{ig} = \mu &+ \gamma_1(\text{One-to-One}_{ig}) + \gamma_2(\text{Pool}_{ig}) \\ &+ \gamma_3(\text{One-to-One}_{ig}) * (\text{Full}_i) + \gamma_4(\text{Pool}_{ig}) * (\text{Full}_i) + \delta_i + u_{ig} \end{aligned} \tag{20}$$

where Full_i is a dummy equal to 1 if participant i was assigned to the *Donor-Full* treatment and zero if she was assigned to the *Donor-Partial* treatment, and the other variables are defined as before.[31] In this equation, the coefficients of interest are γ_3 and γ_4, which measure the effect of each transfer game, relative to the autarky game, in the *Donor-Full* treatment after differentiating out the same effect for the *Donor-Partial* treatment.

The estimating equation that we use to analyze whether a transfer has a differential effect for donors and recipients has a parallel structure to equation (20). The coefficients of interest are the interactions of the

[30] See for example Charness, Gneezy, and Kuhn (2012) on within-subjects comparisons and their benefits on experimental designs. Estimating similar regressions for pairwise combinations of autarky and transfer games yields similar results. These results are not shown, but are available upon request.

[31] Note that this specification includes individual fixed effects and hence we cannot include a dummy for the *Full* treatment.

transfer game dummies with a dummy variable equal to 1 if the participant was randomly assigned to a *Donor* treatment, and zero if she was assigned to a *Recipient* treatment.

As explained in section 2.2, in all our specifications described above we need to account for both baseline ability and learning in performing the effort task. We use individual fixed effects to control for baseline ability, and for any individual-specific factors affecting effort that remained constant during the session. To control for learning, we follow two approaches: (*i*) we use raw effort as the dependent variable and add round dummies, which account for the effect of playing the transfer games in different rounds, depending on the game ordering of the session (see Appendix B.1 for a detailed description of the game orders); (*ii*) we use a measure of "clean" effort, purged from the effect of learning and obtained as described in section 2.2, as the dependent variable. In the next section, we present results from these two approaches in our main estimations.

In all of our reported estimations, we cluster the standard errors at the locality level to account for any correlation of errors within villages. Given that we have a relatively small number of clusters (16 villages in total), to test whether the main coefficients are statistically significant we use a t-distribution with $(G-1)$ degrees of freedom, where G is the number of clusters in a given specification. However, in the Appendix (Table C6), we show that our main results are robust to using the cluster wild bootstrap procedure to obtain p-values for this test, which is also recommended when the number of clusters is relatively low (Cameron, Gelbach and Miller, 2008).

5 Results

5.1 Descriptive evidence on raw effort levels

Figure 4 shows the average raw efforts (i.e., the average number of fully threaded nuts) in the ability tests and the four games, in the four treatments, and for the two different orders used in each treatment (see Figure 1). As discussed before, in our design these raw averages could potentially incorporate both the effects of learning and incentives. However, the noticeable differences in the patterns followed by mean effort across rounds for the different orders suggests that learning is not the only factor driving these patterns. For instance, even though the largest increase in mean effort is observed between the first ability test and the first game played for some orders, this is not the case for three out of the four possible order-role combinations in the *Partial* treatments. Analogous differences are also observed when comparing the mean effort in the first and second games played after the first ability test. Additionally, mean effort tends to level off for some rounds during the session in some game orderings, whereas it follows a weakly concave shape, increasing slightly round after round, in others.

Recall that one of the approaches we use to control for learning is to estimate the learning rate by using the difference in the efforts between the first two autarky rounds played in the *Partial* treatments. Then, we use this estimated learning rate to predict effort in subsequent rounds, assuming that, on average, subjects improve their ability in the the nuts-and-bolts task at the same rate throughout the experiment (see equations (1)-(3)). As can be seen in Figure 4, in most cases the difference in average effort between the first two autarky rounds played in the *Partial* treatments is at least as large than the difference between later rounds in the same treatment-role-ordering combination. In addition, the differences in average effort between the first two autarky rounds played in the *Partial* treatments seem to be at least as large than the corresponding differences between the first two games in the *Full* treatments in most cases. Thus, by estimating and predicting learning as explained above we arguably remove a sizable part from observed effort.[32]

[32]Figures C2 and C3 in Appendix B.3 show both the raw and the predicted efforts.

5.2 Do transfers affect effort exerted by donors?

We present the results for the participants randomly allocated to the donor role first. Table 2 presents the results from estimating equation (19), which compares the effort exerted in the *Autarky* game versus the effort exerted in the *One-to-one* and *Pool* transfer games. These estimations, as explained in the previous section, aim at identifying the effect that giving a transfer has on effort, and also the differential effect that the destination and the size of the transfer might have on donors' effort choices. The left panel shows the results for the *Donor-Partial* treatment and the right panel shows those for the *Donor-Full* treatment. In each panel, the first column presents the results obtained using raw effort and including round dummies to control for learning, whereas the second column presents the results when using effort discounted according to the learning model presented in Section 2.3 as a dependent variable.

In all columns of Table 2, the estimates for the indicator variables for the *One-to-one* and *Pool* games are positive. Their magnitude and statistical significance depends on the approach we use to control for learning. Specifically, the *One-to-one* variable is significant at 1% when we use round dummies to control for learning (columns (1) and (3)), whereas it is either significant at 10% or not at all when using "clean" effort as a dependent variable and smaller (columns (2) and (4)). The *Pool* variable is statistically significant only in the *Partial* treatment when learning is controlled for using round dummies (column (1)). These results suggest that the expectation of giving a transfer does not decrease the effort exerted by donors relative to autarky, when subjects get to keep any additional income. In addition, we cannot reject the null hypothesis that the transfer game dummies are equal, as shown by the F-test at the bottom of the table. This suggests that the non-negative effect of giving a transfer on effort is the same when the transfer affects one single individual or the pool of passive individuals.

Given the payoff structure, this non-negative effect of giving a transfer on effort can only be driven by a positive utility associated with making a transfer.[33] Specifically, according to the predictions in the theory section (see panel A of Figure 3), this result is consistent with both altruistic and warm glow preferences for strong enough altruism and warm glow, but not with selfish preferences. Furthermore, the fact that the additional effort exerted in the transfer games is the same whether the transfer affects a single person or a pool of individuals, is consistent both with warm glow and with altruistic preferences.

In Table 3, we analyze whether the positive effect of giving a transfer on effort varies with transfer size. Recall that we do this by interacting the dummies of the transfer games with an indicator for the *Full* treatment in a between-individuals estimation (see equation (20)). Table 3 shows that the main coefficients for the transfer games are all positive, as in the previous table, and significant for the *One-to-one* game only. Furthermore, the interactions of these with the *Full* indicator are small and not statistically significant. This implies that the effect of a transfer on effort does not vary significantly with the size of the transfer, a result which is in line with both altruistic and warm glow preferences.

5.3 Do transfers affect effort exerted by recipients?

Turning now to subjects allocated to the recipient role, Table 4 shows the results of estimating equation (19) for efforts observed in the *Recipient* treatments. The estimates for the transfer game dummies are all positive,

[33] In particular, it cannot be driven by a desire to extract more money from us for the community, since the additional income z is the same across the three games used in the regression. Not anticipating that we would obtain such a strong result, in the *Partial* treatments we also included the *Autarky Low* game, which gave the subject the same effect on own material payoff as the transfer games (see Figure 1). Table C7 reports the results of estimating equation (19) with the *Autarky Low* game instead of the *Autarky* game as the reference game. The results are largely consistent with our main estimates.

except for one. As in the estimations for donors, they are statistically significant when learning is controlled for using round dummies, but not when we use the learning model to purge effort. The F-test shows that we cannot reject the hypothesis that the *One-to-one* and the *Pool* transfers have the same effects on efforts.

Thus, the results for recipients suggest that the anticipation of receiving a transfer in case of failure does not decrease effort. This lends further support in favor of non-selfish preferences.[34] In particular, it is consistent with participants experiencing a warm glow from avoiding receiving a transfer rather than altruism towards the passive individual(s)—see panel B of Figure 3.

5.4 Donors vs recipients

When comparing the results for donors and for recipients (see Tables 2 and 4), we see that the estimated effects of transfers appear to be smaller for recipients than for donors. To analyze whether the effort increase is different for donors and recipients, we conduct between-subjects comparisons, separately for the *Partial* and the *Full* treatments, using an equation similar to (20), as described at the end of Section 4.

The first panel of Table 5 shows the results for the *Partial* treatments. We see first that the coefficient for the *One-to-one* game dummy is positive and marginally significant, in line with the results reported above. Second, the coefficient for the interaction between the *One-to-one* game dummy and the *Donor* dummy is positive, suggesting a larger effect for donors as mentioned above, but not statistically significant. Nevertheless, its sign is in line with the theoretical prediction that, holding preferences fixed, the effort in the *One-to-one* game should be higher in the *Donor-Partial* than in the *Recipient-Partial* treatment. The second panel of Table 5 shows that the results for the *Full* treatments are qualitatively similar. Together with the results from the within-subject estimations, these results allow us to rule out that subjects are (on average) selfish. The lack of a significant difference between donors and recipients in the *Full* treatment is more in line with warm glow than with altruistic preferences (see panel C of Figure 3). However, we must acknowledge that we do not have the statistical power to reject the null hypothesis in this particular case.[35]

5.5 Heterogeneous effects of transfers on effort with respect to individual and locality characteristics

In this section, we use the data from the post-experiment questionnaire and the Mexican census to estimate heterogeneous effects of transfers on effort, according to selected individual and/or locality characteristics. For all the estimations in this section, we use our corrected effort variable, obtained by subtracting the potential effect of learning from raw effort and a modified version of equation (19) that includes the interaction of each of these locality or individual characteristics with the *One-to-one* and *Pool* dummies, and individual fixed effects. Specifically, we use the data to see if the subjects' effort choices vary with (1) their real-life transfer patterns, (2) their individual sociodemographic characteristics, (3) poverty indicators, and (4) the homogeneity of their community in terms of religion and indigenous background. Given that these variables are constant throughout the session, by including individual fixed effects we are not able to estimate their main effect but only their interaction with the transfer game dummies. All the results discussed below are reported in the Appendix.

[34] This is true even though the introduction of the transfer reduces the effect of effort on own material payoff. Table C7, which reports the results of estimating equation (19) with the *Autarky Low* game instead of the *Autarky* game, shows that the *One-to-One* game and the *Pool* game coefficients are both positive and significant, which is consistent with our main estimates.

[35] We calculate that, given our sample sizes, and the mean and standard deviation of effort for the treatment (donors) group in the *One-to-One* transfer games, the minimum detectable effect using a two-sided hypothesis test with a power=80% and significance level=5% is 0.63 nuts for the *Partial* treatment and 0.54 nuts for *Full* treatment. Both are larger than the effects we obtain.

In Table C8 and Table C9 we interact the transfer dummies with indicators of whether the individual or the household had given or received any aid in the year preceding the experiment, as they reported in the post-experimental questionnaire. This estimation hints at two interesting patterns. Donors that have given in real life make relatively less effort, while recipients that have given in real life make more effort. This evidence is merely suggestive given that statistical significance is weak overall. For receiving patterns, the evidence is mixed depending on whether the recipient was the individual or the household.

Regarding individual characteristics, Table C10 and Table C11 show that effort exerted by donors and recipients does not vary significantly with wealth, measured by the dwelling and asset indexes, or with age, gender, education or involvement in community festivities, with a few exceptions that are marginally significant. In the *Donor-Partial* treatment, being female has a positive effect on the effort exerted in both transfer games, and secondary education has a negative effect for the *One-to-one* game. In the *Recipient-Partial* treatment, age has a small but positive effect on the effort exerted in the *One-to-one* game, and attending community festivities has a positive effect for the *Pool* game.

Table C12 shows that locality characteristics have no significant interactions with the transfer dummies in the *Full* treatment. However, for the *Partial* treatment the locality-level marginality index has a positive effect on the effort exerted in both transfer games for donors and a negative effect for recipients in *Pool* game. All of these effects are significant at 1 percent. Regarding community homogeneity, Table C12 reveals no heterogeneous effects by the share of indigenous population in the locality, but some effects by religious homogeneity for the *Partial* treatment. For instance, the share of Catholics has a positive effect on the effort exerted by donors in the *Pool* game, significant at 10%, and by recipients in the *One-to-one game*, significant at 1%. In addition, a religious homogeneity index has a positive and significant effect on the effort of recipients in the *One-to-one game*.[36] This suggests that recipients may have preferences that depend on the identity of the passive player.

6 Robustness checks

6.1 Are results driven by a desire to maximize the payments obtained from the experimenter?

One possible interpretation of the willingness of subjects to make productive efforts in the three core games, is that they as a group simply wanted to maximize the sum of the payments received from us, and that they would share the sum between themselves after the experiment. To further study this possibility, in the *Full* treatments we added the *Public* game, in which the transfers affected the local health center (a local public good) instead of the passive participants in the other room (see Figure 1). Thus, we replace the indicator variable for *One-to-one* game with an indicator for the *Public* game in equations (19), and control for learning with the two approaches already discussed.

Table 6 shows that participants exert a higher effort in the transfer games than in the *Autarky* game. The positive estimates of the *Public* and *Pool* game dummies are larger and mostly statistically significant when including round dummies, and not statistically significant when corrected effort is the dependent variable. Thus, we conclude that transfers do not seem to reduce effort. For each estimation, the positive estimates are larger for the *Public* game than for the *Pool* game. We take this as suggestive evidence against the hypothesis that subjects simply want to maximize the resources extracted from the experimenter and shared among participants, even though the F-tests at the bottom of the table allow us to reject the null that those estimates are equal

[36]The religious homogeneity index is the sum of squares of Catholic share, other Christian share, and non-religious share.

only in one case. If subjects had been driven by a desire to extract the sum of payments from us to the group of participants, they should have done *less* effort in the *Public* game than in the *Pool* game.

6.2 Are some effects driven by the fact that transfer size is fixed?

By contrast to many experiments (e.g., Ligon and Schechter, 2012 and Jakiela, 2015) where subjects are asked to choose the size of a transfer (and typically the size can be zero), in our effort-and-transfer experiment the transfers are forced and of fixed size: the participant's choice is on whether to perform effort, but in case of success of this effort (or failure for the case of the recipients), transfers of a given size are automatic. To control for the fact that these transfers may be smaller or larger than the individuals would have chosen themselves, we use data from the post-experimental questions about this in the *Donor* treatments.

First, at the very end of each experimental session (see Figure 1) in a *Donor* treatment, we asked two questions.[37] One question was related to the transfer size, and the other to fact that the recipient was passive. The question related to transfer size was different in the *Partial* and the *Full* treatments. To subjects in the *Donor-Partial* treatments we asked: "Would you have liked to give more?", and "If so, how much more?". It turns out that 40.6% of the subjects would have liked to give more. To subjects in the *Donor-Full* treatments we asked: "Would you have liked to give less?", and "If so, how much less?". We found that 52.7% of the subjects would have liked to give less. This is consistent with the conclusion that we drew from the effort choices, namely, that on average subjects derive positive utility from making a transfer.

We use this information to study whether the effects of transfers on effort may be due to the fact that transfer size was imposed in the games. We do so by checking: (1) for subjects in the *Donor-Partial* treatments whether those who would like to give more respond differently to a transfer compared to autarky than the others; (2) for subjects in the *Donor-Full* treatments whether those who would like to give less respond differently to a transfer compared to autarky than the others. Columns (1) and (2) of Table C13 show that it is not the case: the interaction between the answer to the question and the treatment variable is not statistically significant for both transfer games.

Turning now to the question related to the fact that the recipient was passive, we asked each subject whether his/her answer to the question mentioned above would change if the recipient had been able to do an effort. The answer was positive for 42.5% of the donors in the *Full* treatment and 25.8% of the donors in the *Partial* treatment. This suggests that there is some willingness to reward effort, which in real situations could play a role in effort decisions. In our experiment, however, the way subjects answered the question at hand does not have a significant effect on the differential effort between autarky and transfer games (see columns (3) and (4) of Table C13).

6.3 Does the fact that effort is unobservable matter?

In the experiment a subject's efforts are his or her private information. This allows us to focus on pure preference effects and to avoid signaling effects. In reality, however, effort is often observable, and hence our results may underestimate the effects that transfers have on efforts compared to real-life situations. Although we did not expose subjects to differential observational treatments, we asked a question related to effort observability to

[37] Concretely, we handed out a sheet of paper and a pencil to each subject. The sheet reminded the participants about the transfer size in the *One-to-one* game and asked a question; to ensure that all subjects understood, we also showed them the corresponding visual material and read out the text on the sheet aloud. In both cases the subjects answered this question before knowing which game would be used to calculate their payoffs and before knowing whether they would be successful or not.

all subjects who were assigned to *Recipient* treatments. More precisely, at the very end of each experimental session (see Figure 1) in a *Recipient* treatment, we handed out a sheet of paper and a pencil to each subject. On this sheet we reminded the participant about the *One-to-one* game, and about the fact that his (her) effort would not be revealed to the passive subject with whom (s)he was matched, and then we asked: "If we gave you the opportunity to reveal your effort in the *One-to-one* game to the individual to whom you were matched, would you do so?". The answer turned out to be positive for 39.3 % of the subjects in the *Full* treatments and for 29.07% of the subjects in the *Partial* treatments (numbers which are significantly different at the 7% level). When interacted with the treatment variable of interest, however, the answer to the question has no significant impact on the increase in effort from the *Autarky* game to the *One-to-one* game (see columns 5 and 6 of Table C13).

Summing up, the data collected through questions lend no support to the hypotheses that the effects of transfers on efforts are driven by the fact that transfers are forced and of predetermined size, or the fact that effort is unobservable.

7 Conclusions

We present the results of a novel lab-in-the-field experiment designed to test whether transfers to or from others affect effort choices. The experimental design aims at capturing the effects that transfers have on productive efforts. Hence, in our design the composition of the group within which transfers take place is given, interactions are not repeated, effort is unobservable, and the direction as well as the size of any transfer is imposed by us. We conducted the experiment in 16 small rural villages in Southeast Mexico. After controlling for learning about the effort task that might have occurred during the experiment, our data reveals two main findings.

First, on average subjects do not decrease their effort in the presence of a transfer, not even when such transfer represents 100% of their potential additional income. In a few selected cases, we get a positive, but marginally significant, effect of transfers on the effort exerted by donors. Furthermore, since the amount of money extracted from us is the same whether or not there is a transfer, this non-negative effect cannot be due to a desire to extract more money from us for the community. If externally valid, these effects imply that informal transfers within these villages may increase material welfare because they allow to share risk without inducing the reduction in productive efforts commonly associated with selfish preferences. Furthermore, this suggests that productive efforts might be lower under formal insurance schemes, public or private, than under informal ones, because formal insurance would only involve the tax-and-subsidy component of transfers, and not the pro-social component involved in informal transfers. If this were the case, then formal insurance would not be superior to informal insurance in this respect.

Second, the payoff structures used in the experiment allow us to infer whether behaviors are consistent with selfish, altruistic, or warm glow preferences. On average, the behaviors are inconsistent with selfishness, and they are more consistent with warm glow than with altruistic preferences.

Although our results are in line with findings on pro-social behaviors in other experiments, they do contrast quite starkly with some results reported elsewhere in the literature, which indicate that informal transfers have perverse incentive effects: Jakiela and Ozier (2015) find that individuals are willing to forego money to hide income from others, and Azam and Gubert (2005) find that recipients of remittances have an abnormally low productivity. It remains to be seen how these differences can be explained. To what extent does it matter that in our experiment subjects are asked to engage in productive efforts rather than manipulate sums of money? Would our findings be different if the recipients of transfers had been sampled from a poorer population than the donors, an asymmetry that may help explain the findings of Azam and Gubert (2005)? One may also wonder whether there are differences between Mexico and the other countries studied in the literature that matter for how people respond to transfers, and if so, how and why?[38] These questions must be left for future research.

Our experiment allows to isolate the net effect of the tax-and-subsidy component and the intrinsic pro-social component involved in transfers. Arguably the results indicate that in populations where informal transfers are common it is restrictive to assume that the pre-transfer income distribution is independent of the ensuing transfers. Our study, however, disregards many others factors that may impinge on the productive efforts in this context, such as strategic repeated-interaction effects, a desire to signal pro-sociality, and the decision to participate in informal transfer networks in the first place. Understanding the overall effects of informal transfers on productive efforts will thus eventually require understanding not only the effect of each factor taken in isolation, but also how the factors interact. Future research will be necessary to achieve this goal.

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A Appendix: Theoretical predictions for the *Pool* games

In the main text we analyzed the donor pool game assuming that players are unsophisticated. In this section, we analyze this game assuming that each player is sophisticated in the sense that he understands that the marginal consumption utility that his potential transfer would bestow on the recipients depends on the other active players' effort choices.

To be in line with our experimental design, suppose that shocks are correlated in the following way. Indexing the efforts according to their rank among the n efforts, so that $0 \leq e_1 < e_2 < ... < e_n \leq 1$, suppose that success is determined by the realization of a random variable $\tilde{s}$, which is taken to be uniformly distributed on $[0, 1]$. For any realized value s, let the individuals whose effort was at least s succeed, while the others fail. Under these assumptions, n individuals succeed if $s \leq e_1$, $n-1$ succeed if $e_1 < s \leq e_2$, etc. Hence, the probability that all individuals succeed is e_1, the probability that exactly $n-1$ succeed is $(e_2 - e_1)$, and so forth, and the probability that nobody succeeds is $1 - e_n$.

An altruistic individual i who has degree of altruism α_i towards each of the passive individuals, who makes effort e_i and who faces $n-1$ other active individuals who make the efforts summarized by the vector $\boldsymbol{e}_{-i}$, has the following expected utility in the donor pool game:

$$\begin{aligned} U^{\alpha_i}\left(e_i, \boldsymbol{e}_{-i}\right) &= e_i \cdot u(y+z-t) + (1-e_i) \cdot u(y) - c\left(e_i\right) \\ &\quad + \alpha_i \cdot n \cdot e_1 \cdot u\left(y_D + t\right) \\ &\quad + \alpha_i \cdot n \cdot \sum_{k=2}^{n} \left(e_k - e_{k-1}\right) \cdot u\left(y_D + \frac{(n+1-k)\,t}{n}\right) \\ &\quad + \alpha_i \cdot n \cdot (1-e_n) \cdot u(y_D). \end{aligned} \tag{21}$$

The first line is the individual's own consumption utility net of the effort cost. The second line corresponds to the event that everybody succeeds and the last line to the event that nobody succeeds. Finally, the third line summarizes the remaining events. The effort e_i is one of the efforts of the vector $(e_1, ..., e_n)$, depending on how it compares to the other individuals' effort levels. Clearly, then, the marginal effect of i's effort on the passive individuals' consumption utility depends on how her effort compares to the others' efforts. The general problem is complex and beyond the scope of this analysis. Here we limit our attention to the case where all active individuals have the same degree of altruism, $\alpha \in [0, 1]$.

Formally, then, we have a symmetric n-player simultaneous game in which each player has strategy set $[0, 1]$ and payoff given by the expression in (21) for $\alpha_i = \alpha$. It can be shown that this game has no symmetric Nash equilibrium in pure strategies, the reason being that if all the other players choose the same effort, there is a discontinuity in the player's best response function at that effort. To illustrate this, Figure A1 shows the expected utility as a function of the individual's own effort if the other players make effort $\bar{e} = 0.2$ (here, $y = 10$, $z = 5$, $t = 3$, $x_D = 5$, $n = 5$, $\alpha = 0.5$, $u(w) = \ln(w)$, and $c(e_i) = e_i^2$).

The non-existence issue does not arise if the strategy set is finite. To see this, suppose that the strategy set is $\{0, 0.1, ..., 0.9, 1\}$, employ the same parameter values as in Figure A1, and set $\bar{e} = 0.2$. Then, as can be seen in the figure, the individual prefers $e_i = 0.2$ to both $e_i = 0.1$ and $e_i = 0.3$, and *a fortiori* to any other effort in the set $\{0, 0.1, ..., 0.9, 1\}$, so that $\bar{e} = 0.2$ is indeed a symmetric equilibrium effort.

A question of particular interest is how an equilibrium effort in the *Pool donor* game compares to the effort in the *One-to-one donor* game, for a given degree of altruism α. While intuition may suggest that the possibility to free ride on other active players will lead to a lower effort, this intuition does not always apply to altruistic individuals. A numerical example suffices to show that an equilibrium effort in the *Pool donor* game may be either higher or lower than in the *One-to-one donor* game. Suppose first that $y = 10$, $x = 5$, $z = 5$, $t = 3$,

$n = 20$, $u(w) = \ln(w)$, and $c(e_i) = e_i^2$. Then, the effort in the *One-to-one donor* game (with a strategy set $[0,1]$) is $e_D^\alpha \simeq 0.215$ if $\alpha = 0.5$ and $e_D^\alpha \simeq 0.198$ if $\alpha = 0.4$. In the *Pool donor* game, with a strategy set $\{0, 0.1, ..., 0.9, 1\}$, the equilibrium effort is $\bar{e}_D^\alpha = 0.2 < e_D^\alpha$ if $\alpha = 0.5$, and $\bar{e}_D^\alpha = 0.2 > e_D^\alpha$ if $\alpha = 0.4$. Identical reasoning can be shown to apply to the *Pool recipient* game. In sum: an active player who is altruistic may make more, less or the same effort in a *One-to-one donor* game and in a *Pool donor* game in which endowment and the transfer amounts are the same. The same prediction obtains for the effort of an altruistic player in a *One-to-one recipient* game and a *Pool recipient* game in which endowment and the transfer amounts are the same.

B Appendix: Ability and Learning

In this appendix we present a detailed description of the game orders used in the experiment and a descriptive analysis of our baseline ability measure. We also present the results of the learning estimation described in equations (1)-(3), as well as robustness checks using a linear and a log-linear specification, both with a constant term.

B.1 Game orders

Table B1 shows the different orders of games that were used in the *Partial* and the *Full* treatments, respectively (the same orders were used for *Donors* and *Recipients*). Table B2 shows the number of sessions as well as the number of subjects who were allocated to each order. We have four sessions per order, and between 24.44 and 26.12 percent of the sample for each order.[39]

One of the approaches we use to control for learning in our main estimations is controlling for round dummies when using raw effort as a dependent variable. Table B3 summarizes the number of rounds between all game pairs. This table reveals that for some game pairs we have variation in the order only (e.g., there are always two rounds between *Autarky* and *Pool*), while for other pairs we have also variation in the number of rounds between the games (e.g., in some sessions there are three rounds between *Autarky Low* and *Pool* whereas in others there is only one round). The inclusion of round dummies exploits both sources of variation to control for learning.

B.2 Baseline ability

All subjects performed two incentivized ability tests: the first one before the experimental games, and the second one after them. In each ability test, the subject was rewarded with one point for each properly threaded nut in one minute, with no upper bound on the number of nuts that could be threaded. The first ability test thus provides a measure of baseline ability to perform the real effort task, in number of nuts threaded. Figure 8 shows the distributions of the number of threaded nuts in the first ability test, for different treatments and roles, and for the different orders.

Table B4 confirms the similarity on the distribution of ability across roles, protocols and order shown in Figure 8. It shows that mean baseline ability did not differ significantly between roles, or between orders in the

[39] Our original intention was to use two different orders per treatment, but by mistake we ended up with three different orders for the *Full* treatment sessions. The order C was only used in one session, and we exclude this session in our main analysis. Results are robust to the inclusion of that session. The goal in the *Full* treatment was to be consistent with the information given to the participants through the session, and hence the *Public* game—with transfers to or from non-participants—was always played last.

Full treatments, while there is a significant difference between orders in the *Partial* treatments. As mentioned in the main text, we control for baseline ability and other individual characteristics affecting effort that remained constant during the session by including individual fixed effects in all our estimations.

B.3 Estimating learning

As explained in the main text, we follow two alternative approaches to control for learning. First, we use observed effort and include round dummies in our main estimations to account for the fact that the same transfer game was played in different positions by different players. Second, we use the two autarky games played by subjects in the *Partial* treatments to estimate learning, and then "correct" the raw effort in rounds 2-4 by subtracting the effort predicted based on the estimated learning (see equations (1)-(3)). Recall that the validity of this approach to get an estimate of learning depends on the similarity between those two games. Given our design, the only difference between those two games, apart from the round in which they were played depending on the specific game order, is that in the *Autarky* game the expected return to effort is higher than in the *Autarky Low* game. Thus, we need to check first that this difference in returns had no effect on effort to be able to pool all the observations from those two games to estimate learning. To verify this, we estimate the following equation using only observations from the first two experimental games in the *Partial* treatments:

$$e_{ir} = \alpha_1 + \beta_1(\text{Round2}_{ir}) + \beta_2(\text{Aut. Low}_{ir}) + \beta_3(\text{Round2*Aut. Low}_{ir}) + u_{ir} \tag{22}$$

where e_{ir} represents the effort of individual i in game r, $Round2_i$ is a dummy equal to 1 if the observation corresponds to the second game after the first ability test (Round 0), Aut. Low_{ir} is a dummy equal to one for that particular game. Panel A in Table C5 show the results of this estimation. We see that the dummy for the second round is positive and statistically significant in columns (1) to (3). This is robust to the inclusion of individual fixed effects (columns (4) to (6)). In contrast, the estimate for Aut. Low_{ir} is not statistically significant, either by itself or interacted with the dummy for the second round, in any of the specifications.

Consequently, we pool observations from the two autarky games to estimate learning according to equation (1) in the main text. As a robustness check, we also estimate learning using all those observations and the following two specifications:

$$e_{i2} = \alpha + \mu * e_{i1} \tag{23}$$

$$\ln e_{i2} = \alpha + \mu * \ln e_{i1}. \tag{24}$$

Equation (23) adds a constant term to the linear specification in equation (1), whereas equation (24) uses a log-linear specification.

Table B6 presents the results of our main learning estimation (column (1)), of equation (23) (column (2)), and of equation (24) (column (3)). In column (1), the estimation of the main learning equation (1) shows that on average an additional nut threaded in the first autarky game leads to an increase of 1.034 nuts (3.4%) threaded in the second autarky game, presumably due to learning. This estimated effect would imply a 10.5% accumulated increase in effort between the fourth and the first experimental game. For comparison, the percentage difference in the mean observed effort in the *Partial* and *Full* treatments between those same rounds is 17% and 14%, respectively.

As described in the main text, we use the estimate from column (1) of Table B6 to predict effort in rounds 2, 3 and 4. Figures 7 and 8 show the predicted and the raw effort for the different roles for the *Partial* and *Full* treatments, respectively. In general, predicted effort is lower than raw effort for most cases, and the mean difference between these two measures varies notably across orders and roles, suggesting that not all the effort changes across games can be attributed to learning as we chose to model it.

Finally, recall that in our main estimations we use "clean" effort, i.e. the difference between raw and predicted effort to control for learning. When doing so, we find no evidence of a negative and significant effect of transfers on effort. In Table B7 we show that this conclusion is robust to the alternative learning models in columns (2) and (3) of Table B6, because we get some positive but not significant estimates, as before. Even though we also obtain several negative estimates, particularly for the linear specification with a constant term, none of them is statistically significant either.

B.4 Difference-in-Differences Estimation

To provide complementary evidence about whether our results are confounded by learning, we also conduct a difference-in-differences (DD) estimation.[40] For this, we once again exploit the fact that by design the first two experimental games in the *Partial* treatments are autarkies, whereas the first and second games in the *Full* treatments are an autarky game (*Autarky*) and a transfer game (*One-to-One* or *Pool*, depending on the game order), respectively. Thus, to estimate the effect of transfers on effort, our DD estimation compares the raw effort exerted by subjects in the *Full* treatment (the "treatment" group) with that exerted by subjects in the *Partial* treatment (the "control" group), before (first game) and after the introduction of transfers (second game). Specifically, we estimate the following equation:

$$e_{ir} = \alpha_1 + \beta_1(\text{Round2}_{ir}) + \beta_2(\text{Full}_i) + \beta_3(\text{Round2*Full}_{ir}) + u_{ir} \tag{25}$$

where $Full_i$ is a dummy equal to 1 if the subject was assigned the *Full* treatment, and the rest of the terms are defined as above. Table B8 show the results of this DD estimation. We see that the dummy for the second round is positive and statistically significant in columns (1) to (3). This is robust to the inclusion of individual fixed effects (columns (4) to (6)). The estimate for $Full_i$ is also positive, but not statistically significant in columns (1) to (3). Finally, the DD estimate of interest, i.e. that for the interaction of $Full_i$ and $Round2_{ir}$, is negative in all columns, but marginally significant only for recipients (columns (3) and (6)). In addition to being not significant, the DD estimate for donors is small in magnitude (columns (2) and (4)). Overall, these results provide some support to the lack of a negative and significant effect of transfers on effort in all of our main specifications.

[40] We thank the Associate Editor, who remained anonymous, for this suggestion.

Tables

Table 1: Network and participants' giving and receiving patterns

	Tightness of network			Giving and Receiving patterns						Obs.
	% of session participants that			Giving		Receiving		Reason		
	Knows	Family	Meets	Ind.	HH	Ind.	HH	voluntary	reciprocity	
Chunhuas	0.80	0.15	0.20	0.16	0.44	0.13	0.41	0.87	0.12	36
Chunkanan	0.51	0.06	0.15	0.27	0.31	0.41	0.43	1.00	0.00	31
Concepcion	0.78	0.02	0.03	0.34	0.68	0.15	0.43	0.75	0.25	32
Dzitnup	0.82	0.05	0.18	0.48	0.49	0.22	0.47	0.35	0.35	37
Katab	0.86	0.13	0.05	0.16	0.55	0.31	0.60	0.90	0.10	22
Pucnachen	0.81	0.03	0.07	0.38	0.54	0.15	0.39	0.76	0.15	26
San Nicolas	0.83	0.11	0.19	0.37	0.44	0.37	0.40	0.80	0.20	27
Xculoc	0.89	0.02	0.07	0.26	0.30	0.11	0.22	0.87	0.25	34
Average north villages	0.79	0.07	0.12	0.30	0.47	0.23	0.42	0.79	0.18	
Canasayab	1 .00	0.11	0.11	0.50	0.71	0.32	0.57	0.91	0.08	34
General Ortiz Avila	0.91	0.03	0.05	0.51	0.64	0.30	0.43	0.96	0.04	39
Lic. Adolfo Lopez Mateos	0.89	0.05	0.00	0.40	0.51	0.20	0.31	0.94	0.05	35
Moquel	0.87	0.00	0.02	0.50	0.70	0.30	0.43	0.96	0.03	40
San Antonio yacasay	1.00	0.17	0.17	0.48	0.60	0.18	0.32	0.95	0.04	37
San Jose Carpizo Numero	0.86	0.10	0.15	0.41	0.72	0.16	0.38	0.84	0.11	36
Villamar	0.94	0.00	0.21	0.40	0.62	0.13	0.32	0.95	0.08	37
Xkeulil	0.82	0.05	0.10	0.30	0.48	0.21	0.46	0.81	0.18	33
Average south villages	0.91	0.06	0.10	0.44	0.62	0.23	0.40	0.92	0.08	
Difference N-S	-0.13***	0.01**	0.02***	-0.15***	-0.14***	-0.03	-0.01	-0.08**	0.08**	

Source: Post-experimental questionnaire. t-tests: *** p<0.01, ** p<0.05, * p<0.1
Tightness of the network: The post-experimental survey questions were: (1) *Do you know the people that came to the activity?*, with closed answers "all - almost all - a few - none". We present here the summary of a dummy variable taking value one for the answers "all - almost all". (2) *How many of them are members of your direct family?*, with closed answers "all - many - half- a few". We present here a dummy variable that takes value one for the answers "all - many". (3) *With how many of them do you share daily activities like visits to the market or workplace?*, with same answers as question (2).
Giving and receiving patterns: The post-experimental survey questions were: *During the last year, did someone in your household give/receive any help in the form of clothes, food, money, ...?*, and *If yes, who did give/receive this help?* with closed answer "you - someone else in the household". After the giving question, the participants were asked *Do you think that this help was given..?* with multiple choice: "voluntarily - due to community pressure - family pressure - reciprocity - needed to receive help in the future".

Table 2: Within-subjects estimations of the effects of transfers on efforts, *Donors* treatments

	Partial		*Full*	
	(1)	(2)	(3)	(4)
One-to-one (β_1)	1.1719***	0.4497*	0.6507***	0.3708+
	(0.164)	(0.216)	(0.126)	(0.216)
Pool (β_2)	0.9460***	0.2195	0.4374+	0.1676
	(0.200)	(0.169)	(0.241)	(0.269)
p-value $\beta_1 = \beta_2$	0.3196	0.3113	0.3338	0.3924
Observations	393	393	357	357
R^2	0.107	0.027	0.081	0.022
Number of subjects	131	131	119	119
Individual FE	Yes	Yes	Yes	Yes
Learning Controls	round	model	round	model

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of equation (19) are reported, with *Autarky* being the reference game. In Columns (1) and (3) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Columns (2) and (4) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table 3: Between-subjects estimations of the effects of transfer size on efforts, *Donors* treatments

	(1)	(2)
One-to-one	0.6480*	0.4497**
	(0.337)	(0.209)
Pool	0.4221	0.2195
	(0.280)	(0.164)
One to One * Full	0.0027	-0.0789
	(0.385)	(0.294)
Pool * Full	0.0153	-0.0519
	(0.373)	(0.305)
Observations	750	750
R^2	0.091	0.025
Number of subjects	250	250
Individual FE	Yes	Yes
Learning Controls	round	model

Notes: Robust standard errors (cluster session) in parentheses: *** p<0.01, ** p<0.05, * p<0.1.
OLS estimations of equation (20) are reported, with *Autarky* being the reference game. In Column (1) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Column (2) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table 4: Within-subjects estimations of the effects of transfers on efforts, *Recipients* treatments

	Partial		*Full*	
	(1)	(2)	(3)	(4)
One-to-one (β_1)	1.0857**	0.1792	0.2174*	0.0616
	(0.366)	(0.235)	(0.092)	(0.091)
Pool (β_2)	0.8503**	-0.0538	0.2717*	0.1059
	(0.343)	(0.256)	(0.135)	(0.129)
p-value $\beta_1 = \beta_2$	0.2744	0.2693	0.4672	0.6549
Observations	429	429	363	363
R^2	0.072	0.008	0.063	0.002
Number of subjects	143	143	121	121
Individual FE	Yes	Yes	Yes	Yes
Learning Controls	round	model	round	model

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.
OLS estimations of equation (19) are reported, with *Autarky* being the reference game. In Columns (1) and (3) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Columns (2) and (4) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table 5: Between-subjects estimations of the effects of role (*Donors* vs *Recipients*) on efforts

	Partial		*Full*	
	(1)	(2)	(3)	(4)
One-to-one	0.4849*	0.1792	0.2780*	0.0616
	(0.244)	(0.235)	(0.120)	(0.091)
Pool	0.2497	-0.0538	0.3189*	0.1059
	(0.298)	(0.256)	(0.154)	(0.129)
One*Donor	0.2448	0.2705	0.3155	0.3092
	(0.327)	(0.330)	(0.246)	(0.247)
Pool*Donor	0.2538	0.2734	0.0676	0.0617
	(0.260)	(0.258)	(0.225)	(0.226)
Observations	822	822	720	720
R^2	0.072	0.017	0.071	0.014
Number of subjects	274	274	240	240
Individual FE	Yes	Yes	Yes	Yes
Learning Controls	round	model	round	model

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (20) (replacing the Full dummy by a Donor dummy) are reported, with *Autarky* being the reference game. In Columns (1) and (3) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Columns (2) and (4) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table 6: Within-subjects estimations of the effects of the destination/source of transfer (*Public* vs *Pool* game) on efforts, in the *Full* treatments

	Donors		Recipients	
	(1)	(2)	(3)	(4)
Public (β_1)	0.9412***	0.2886	0.8512***	0.2112+
	(0.211)	(0.209)	(0.105)	(0.118)
Pool (β_2)	0.5063	0.1676	0.5483***	0.1059
	(0.455)	(0.269)	(0.128)	(0.129)
Constant	6.2185***	0.0000	6.0992***	0.0000
	(0.150)	(0.149)	(0.069)	(0.073)
p-value $\beta_1 = \beta_2$	0.2186	0.5307	0.0340	0.3925
Observations	357	357	363	363
R^2	0.121	0.012	0.114	0.007
Number of subjects	119	119	121	121
Individual FE	Yes	Yes	Yes	Yes
Learning Controls	round	linear	round	linear

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (19) are reported; *Autarky* is the reference game, and the two other games included in the regression are the *Public* game and the *Pool* game. In Columns (1) and (3) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Columns (2) and (4) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table B1: Game orders used in the experiment

	First game	Second game	Third game	Fourth game
Partial order A	Autarky	Autarky Low	Pool	One-to-one
Partial order B	Autarky Low	Autarky	One-to-one	Pool
Full order A	Autarky	Pool	One-to-one	Public
Full order B	Autarky	One-to-one	Pool	Public
Full order C	Autarky	One-to-one	Public	Pool

Table B2: Number of sessions and subjects for each order used

	Number of sessions	Number of participants	Share of participants
Partial order A	4	134	25%
Partial order B	4	140	26.12%
Full order A	3	109	20.34%
Full order B	4	131	24.44%
Full order C	1	22	4.1%

Table B3: Number of rounds between games across orders and treatments.

	(1)	(2)	(3)	(4)	(5)	(6)
			Partial game comparisons			
	Aut. Low Autarky	Aut. Low. One-to-one	Aut. Low Pool	Autarky One-to-one	Autarky Pool	One-to-one Pool
Order A	-1	2	1	3	2	-1
Order B	1	2	3	1	2	1
			Full game comparisons			
	Autarky One-to-one	Autarky Pool	Autarky Public	One-to-one Pool	One-to-one Public	Pool Public
Order A	2	1	3	-1	1	2
Order B	1	2	3	1	2	1
Order C	1	3	2	2	1	-1

Table B4: Tests of differences in means in the first ability test

	Partial		
	Order A	Order B	t-statistic (p-value)
Ability test 1	5.0223 (std. 0.177)	5.4428 (std. 0.150)	-1.8096 (0.0715)
	Full		
	Order A	Order B	t-statistic (p-value)
Ability test 1	5.1832 (std. 0.170)	5.1926 (std. 0.188)	-0.0372 (0.9704)
	Donor	*Recipient*	t-statistic (p-value)
Ability test 1	5.1441 (std. 0.120)	5.2803 (std. 0.121)	0.4267 (0.4267)
	Full	*Partial*	t-statistic (p-value)
Ability test 1	5.1875 (std. 0.126)	5.2372 (std. 0.116)	0.2895 (0.7723)

Table B5: Comparison of *Autarky* and *Autarky Low* in the *Partial* treatments

	(1) all	(2) donors	(3) recipients	(4) all	(5) donors	(6) recipients
Round 2	0.6125*	0.5760	0.6468**	0.6068***	0.6170***	0.5978***
	(0.344)	(0.528)	(0.254)	(0.092)	(0.166)	(0.139)
Low Autarky	0.0482	0.2327	-0.1204	0.0425	0.2737+	-0.1693
	(0.346)	(0.508)	(0.303)	(0.092)	(0.166)	(0.139)
Round 2* Low Autarky	-0.0114	0.0819	-0.0978			
	(0.701)	(1.000)	(0.523)			
Observations	548	262	286	548	262	286
Individual FE	No	No	No	Yes	Yes	Yes
Number of subjects	274	131	143	274	131	143

Notes: Standard errors in parentheses (cluster session).*** p<0.01, ** p<0.05, * p<0.1, + p<0.15

Table B6: Learning modeling under different functional assumptions

	(1)	(2)	(3)
Dependent var:	*Effort round 2*		*ln Effort round 2*
Effort round 1	1.0338***	0.5404***	
	(0.020)	(0.047)	
ln Effort round 1			0.5092***
			(0.044)
Constant		3.2409***	0.9437***
		(0.291)	(0.078)
Observations	274	274	262
R^2	0.904	0.324	0.338

Notes: Standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1, + p<0.15
Column (1) shows the result of the linear specification in equation (1), and Column (2) adds a constant to this specification (see equation (23)). Column (3) shows the results of the log-linear specification in equation (24).

Table B7: Robustness of main results to alternative learning specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Linear learning (w/constant)				**Log-linear learning (w/ constant)**			
	Donors		Recipients		Donors		Recipients	
	Partial	Full	Partial	Full	Partial	Full	Partial	Full
One-to-one	0.0092	0.2138	-0.0333	-0.1777	0.1836	0.4054*	0.1261	0.0152
	(0.315)	(0.210)	(0.196)	(0.205)	(0.293)	(0.208)	(0.222)	(0.208)
Pool	-0.2011	-0.0102	-0.2755	-0.1411	0.0320	0.1784	-0.0912	0.0471
	(0.190)	(0.260)	(0.219)	(0.213)	(0.212)	(0.263)	(0.213)	(0.217)
Observations	393	357	429	363	381	357	414	363
R^2	0.007	0.011	0.011	0.007	0.005	0.028	0.006	0.000
Number of subjects	131	119	143	121	127	119	138	121
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15. OLS estimations of equation (19) are reported, with *Autarky* being the reference game. In the left panel the dependent variable is the effort purged of learning, using the linear specification in equation (23), and in the right panel the dependent variable is the effort purged of learning, using the log-linear specification in equation (24).

Table B8: DD estimation using the first two rounds of Partial and Full treatments

	(1)	(2)	(3)	(4)	(5)	(6)
	all	donors	recipients	all	donors	recipients
Round 2	0.606***	0.611***	0.601***	0.606***	0.611***	0.601***
	(0.0900)	(0.194)	(0.147)	(0.0900)	(0.194)	(0.147)
Full	0.425	0.631	0.232			
	(0.337)	(0.395)	(0.303)			
Round 2 * Full	-0.214	-0.0729	-0.353*	-0.214	-0.0729	-0.353*
	(0.150)	(0.262)	(0.181)	(0.150)	(0.262)	(0.181)
Observations	1,028	500	528	1,028	500	528
Individual FE	No	No	No	Yes	Yes	Yes
Number of subjects	514	250	264	514	250	264

Notes: Standard errors in parentheses (cluster session).*** p<0.01, ** p<0.05, * p<0.1, + p<0.15

Table C1: Sociodemographic Characteristics at Locality level

Location	Total population (persons)	Number of inhabited dwellings	Sex ratio women/men, adults over 18	Fertility [1]	Average Schooling years, population over 15	Share of illiterate population over 15	Share of married population over 12
Chunhuas	401	84	0.99	3.04	6.15	17.81	62.83
Chunkanan	885	197	0.94	2.90	5.68	21.55	63.80
Concepción	351	95	0.86	3.53	6.44	18.25	54.93
Dzitnup	891	185	0.98	2.40	7.83	10.38	57.90
Katab	405	70	0.99	2.75	5.63	18.64	60.00
Pucnachen	865	201	1.06	2.56	6.80	19.42	62.90
San Nicolás	369	88	0.86	2.69	6.80	1.65	61.25
Xculoc	469	84	1.02	2.68	6.22	17.43	52.94
Average North villages	579.50	125.50	0.96	2.82	6.44	15.64	59.57
Canasayab	256	62	0.97	3.10	7.01	9.09	62.50
General Ortiz Avila	507	123	1.01	3.01	6.01	12.69	52.35
Lic. Adolfo López Mateos	420	97	0.93	2.82	6.35	12.04	62.75
Moquel	695	187	1.11	2.82	6.56	8.62	66.91
San Antonio Yacasay	473	108	0.95	3.38	5.72	17.65	60.11
San José Carpizo 2	294	83	0.99	3.24	4.90	20.10	60.00
Villamar	460	115	1.05	3.01	6.39	14.09	63.26
Xkeulil	991	229	0.94	2.38	7.23	4.51	65.19
Average South villages	512.00	125.50	0.99	2.97	6.27	12.35	61.64
Difference N-S	**67.50**	**0**	**-0.03**	**-0.15**	**0.17**	**3.29**	**-2.07**

Source: National Institute of Geography and Statistics (INEGI by its acronym in Spanish), National Census 2010.
t-tests: *** p<0.01, ** p<0.05, * p<0.1
[1] Fertility rate is the average number of living born children for women over 12 (excluding women who did not report this).

Location	Share of Indigenous population over 5	Share of Indigenous population over 5 that do not speak Spanish	Proportion of Catholics	Proportion of non-Catholics, Christian	Religious homogeneity [2] (Index)
Chunhuas	97.71	9.17	62.34	23.94	0.46
Chunkanan	95.28	9.57	58.87	26.78	0.43
Concepción	89.78	7.43	92.02	3.99	0.85
Dzitnup	80.33	2.16	85.41	12.12	0.74
Katab	97.51	13.26	98.27	1.48	0.97
Pucnachen	94.34	10.04	81.16	9.36	0.68
San Nicolás	91.44	5.20	99.73	0.00	0.99
Xculoc	95.73	9.48	75.69	23.03	0.63
Average North villages	92.77	8.29	81.69	12.59	0.72
Canasayab	2.63	0.00	68.36	25.78	0.54
General Ortiz Avila	1.33	0.00	32.74	32.54	0.33
Lic. Adolfo López Mateos	32.80	0.00	37.62	36.67	0.34
Moquel	1.11	0.00	89.93	2.88	0.81
San Antonio Yacasay	8.90	0.00	61.10	23.26	0.45
San José Carpizo 2	15.19	0.00	30.61	46.94	0.36
Villamar	1.73	0.00	39.57	40.00	0.35
Xkeulil	8.50	0.00	37.03	50.76	0.40
Average South villages	9.02	0.00	49.62	32.35	0.45
Difference N-S	**83.74*** **	**8.29*** **	**32.07*** **	**-19.76*** **	**0.30*** **

Source: National Institute of Geography and Statistics (INEGI by its acronym in Spanish), National Census 2010.
t-tests: *** p<0.01, ** p<0.05, * p<0.1
[2] Religious homogeneity index calculated as sum of squares of Catholic share, other Christian share, and non-religious share.

Table C2: Geographic and Productive Characteristics of the Selected Localities

Location	Average size of farms with Procampo support [1]	Share of Procampo beneficiaries beneficiaries in population over 18	Proportion of ejidatarios in population over 18	Share of ejido land with communal use
Chunhuas	2.01	24.54	27.51	97.69
Chunkanan	1.11	19.48	15.95	98.16
Concepción	1.76	28.87	38.03	96.38
Dzitnup	1.51	19.65	32.79	84.52
Katab	5.57	65.45	24.36	94.81
Pucnachen	1.23	16.14	39.82	99.77
San Nicolás	0.90	24.35	27.00	82.00
Xculoc	1.26	51.47	23.53	97.36
Average North villages	1.9	31.2	28.6	93.8
Canasayab	5.20	17.93	26.09	99.29
General Ortiz Avila	2.65	22.44	31.02	8.66
Lic. Adolfo López Mateos	3.45	19.80	25.17	15.14
Moquel	3.82	17.56	16.27	20.86
San Antonio Yacasay	3.39	28.53	23.82	18.02
San José Carpizo 2	3.46	56.52	33.91	32.18
Villamar	4.97	5.43	21.41	1.33
Xkeulil	2.89	15.71	15.97	98.51
Average South villages	3.73	22.99	24.21	36.75
Difference N-S	**-1.81****	**8.25**	**4.42**	**57.09*****

Source: National Institute of Geography and Statistics (INEGI), National Census 2010 and National Agrarian Registry Office.
t-tests: *** p<0.01, ** p<0.05, * p<0.1
[1] For the Program for Direct Income Support to Farmers (PROCAMPO), this indicator represents the average plot size for farmers in the locality.

Location	Distance to main city (km)	Commuting to main city (minutes)	Social Backwardness Index (SBI) [2]	Level of Social Backwardness	Asset index, Experimental data	Asset index
Chunhuas	23.4	39.00	0.14	Medium	-0.69	-2.27
Chunkanan	22.0	33.00	-0.13	Medium	-0.16	-1.33
Concepción	20.6	35.00	-0.24	Medium	-0.54	-0.45
Dzitnup	3.5	8.00	-0.67	Low	0.48	-1.25
Katab	21.7	22.00	-0.08	Medium	-0.81	-1.76
Pucnachen	26.5	32.00	-0.24	Medium	-0.22	-1.08
San Nicolás	25.2	29.00	0.04	Medium	-0.71	-3.57
Xculoc	64.2	60.00	0.25	Medium	-0.62	-1.91
Average North villages	25.9	32.3	-0.12		-0.4	-1.7
Canasayab	26.6	36.00	-0.99	Very low	0.03	0.83
General Ortiz Avila	141.0	104.00	-0.67	Very low	-0.12	0.21
Lic. Adolfo López Mateos	41.0	46.00	0.38	Medium	0.08	-1.16
Moquel	9.5	17.00	-1.23	Very Low	0.75	4.31
San Antonio Yacasay	52.0	69.00	-0.53	Low	0.06	0.46
San José Carpizo 2	27.6	39.00	-0.64	Low	0.31	1.01
Villamar	69.5	54.00	-0.93	Very low	0.83	1.97
Xkeulil	40.8	39.00	-0.88	Low	0.59	3.56
Average South villages	51.0	50.50	-0.69		0.32	1.40
Difference N-S	**-25.11**	**-18.25**	**0.57**		**-3.10*****	**-0.73*****

Source: National Institute of Geography and Statistics (INEGI), National Census 2010 and National Agrarian Registry Office.
T-tests: *** p<0.01, ** p<0.05, * p<0.1
[2] The National Council for Evaluation of Social Development Policy uses the *social backwardness index* (SBI) to rank the lack of social opportunities and the absence of capabilities, the privations, and/or the inaccessibility to goods and services that provide social welfare. This index is based on 15 categories of exclusion, in four dimensions: education, access to health services, basic services and quality of dwelling. The SBI is calculated using principal components analysis. The SBI ranges between -2 (the lowest level of backwardness) and +3 (the highest level of backwardness). The *level of social backwardness* is based on five categories of social backwardness: very low, low, medium, high, and very high. The *asset index* was calculated using the principal factor analysis method that considers the following assets: real state properties, vehicles, TV, radio, cellphone, gas stove, small kitchen appliances, refrigerator, washing machine, books; productive assets like agricultural machinery or animals; and small livestock.

Table C3: Summary statistics - Locality vs. subject characteristics

Part A: South Localities

	Subjects		CENSUS information		
Variable	Mean	SE	Mean	SE	P-value
Proportion women/men, older than 18	1.4648	0.0233	1.0150	0.0026	0.0000
Fertility, children per woman	3.2152	0.1155	3.0882	0.0086	0.2727
Proportion of 18-24 years old women in total population	0.0628	0.0007	0.1156	0.0059	0.0000
Proportion of 15-49 years old women in total population	0.2619	0.0007	0.4170	0.0039	0.0000
Proportion of singles in total population	0.1166	0.0031	0.3489	0.0024	0.0000
Proportion of married in total population	0.7758	0.0027	0.6750	0.0021	0.0000
Proportion of divorced, separated and widows in total population	0.1031	0.0022	0.0865	0.0018	0.0000
Proportion of population that speaks only Indigenous language	0.0045	0.0005	0.0000	0.0000	0.0000
Proportion of population that speaks indigenous language and Spanish	0.0673	0.0019	0.0486	0.0024	0.0000
Proportion of population older than 15 with no formal education	0.1566	0.0026	0.1211	0.0038	0.0000
Proportion of population older than 15 with high school education	0.1885	0.0066	0.2315	0.0025	0.0000
Proportion of people born in State	0.2025	0.0119	0.6168	0.0044	0.0000
Proportion of one-room dwellings	0.2108	0.0052	0.1406	0.0029	0.0000
Proportion of two-rooms dwellings	0.4888	0.0039	0.2617	0.0046	0.0000
Proportion of dwellings with more than three rooms	0.3004	0.0048	0.5958	0.0049	0.0000
Proportion of dwellings with piped water in property	0.8610	0.0037	0.9186	0.0026	0.0000
Proportion of dwellings with electricity	0.9042	0.0028	0.9486	0.0013	0.0000

Part B: North Localities

	Subjects		CENSUS information		
Variable	Mean	SE	Mean	SE	P-value
Proportion women/men, older than 18	1.5605	0.0293	0.9564	0.0023	0.0000
Fertility, children per woman	3.2364	0.0975	2.7768	0.0131	0.0000
Proportion of 18-24 years old women in total population	0.0705	0.0016	0.1770	0.0067	0.0000
Proportion of 15-49 years old women in total population	0.2603	0.0009	0.4538	0.0055	0.0000
Proportion of singles in total population	0.1166	0.0031	0.3489	0.0024	0.0000
Proportion of married in total population	0.8307	0.0034	0.6637	0.0018	0.0000
Proportion of divorced, separated and widows in total population	0.0479	0.0017	0.0480	0.0008	0.9407
Proportion of population that speaks only Indigenous language	0.0607	0.0020	0.0626	0.0017	0.2276
Proportion of population that speaks indigenous language and Spanish	0.6070	0.0070	0.7017	0.0108	0.0000
Proportion of population older than 15 with no formal education	0.1059	0.0018	0.1022	0.0036	0.2368
Proportion of population older than 15 with high school education	0.4036	0.0054	0.2993	0.0031	0.0000
Proportion of people born in State	0.7618	0.0135	0.9822	0.0009	0.0000
Proportion of one-room dwellings	0.3003	0.0051	0.2000	0.0056	0.0000
Proportion of two-rooms dwellings	0.4473	0.0021	0.3864	0.0035	0.0000
Proportion of dwellings with more than three rooms	0.2524	0.0058	0.4100	0.0054	0.0000
Proportion of dwellings with piped water in property	0.9042	0.0031	0.8500	0.0113	0.0000
Proportion of dwellings with electricity	0.9462	0.0012	0.9675	0.0009	0.0000

Source: Own estimations using t-tests on the equality of means for locality sample vs. Census sample.
Note: We compare data collected from our subjects through the post-experimental questionnaire, to Census data for the municipalities involved in the experiment.

Table C4: Summary statistics on Experimental Subjects: Mean Difference *Full* vs *Partial* treatments

	Donor			Recipient		
	Full	**Partial**	**Difference**	**Full**	**Partial**	**Difference**
Age	37.68	42.27	-4.598**	38.70	40.34	-1.639
Married	0.592	0.695	-0.102	0.720	0.748	-0.029
Number of siblings	6.008	5.344	0.664	5.553	5.671	-0.118
Fertility, children per woman	3.246	3.405	-0.158	3.447	2.846	0.601*
Proportion of people born in the municipality	0.646	0.649	-0.003	0.674	0.727	-0.053
Indigenous that do not speak Spanish	0.031	0.038	-0.007	0.061	0.021	0.040
Indigenous that speak Spanish	0.346	0.412	-0.066	0.333	0.434	-0.100
Has at least High School	0.462	0.481	-0.019	0.439	0.469	-0.029
Agriculture is main income source of household	0.723	0.580	0.143 *	0.652	0.741	-0.090
Dwelling Index	-0.108	0.201	-0.309*	-0.195	0.144	-0.340*
Assets Index	0.157	0.038	0.118	-0.144	0.134	-0.277
Organization enrollment	1.377	1.344	0.033	1.621	1.622	-0.001
Number of festivities attended	2.331	2.626	-0.295	2.629	2.657	-0.029
How actively she participated in festivities	1.408	1.481	-0.073	1.545	1.510	0.035
Individual perception of how cooperative is the community	1.308	1.351	-0.043	1.432	1.336	0.096
Average perception of how cooperative is the community	1.367	1.380	-0.012	1.323	1.361	-0.038
Believes people are just	0.185	0.198	-0.014	0.205	0.217	-0.012
Believes people would take advantage if given the chance	0.477	0.466	0.011	0.424	0.524	-0.100
Believes effort is rewarded	1.562	1.634	-0.072	1.348	1.608	-0.260
Observations	**130**	**131**	**261**	**132**	**143**	**275**

Source: Post-experimental questionnaire
t-tests: *** p<0.01, ** p<0.05, * p<0.1

Table C5: Summary statistics on Experimental Subjects: Mean Difference *Recipient* vs *Donor* treatments

	Full			Partial		
	Recipient	**Donor**	**Difference**	**Recipient**	**Donor**	**Difference**
Age	38.70	37.68	1.020	40.34	42.27	-1.939
Married	0.720	0.592	0.127*	0.748	0.695	0.054
Number of siblings	5.553	6.008	-0.455	5.671	5.344	0.328
Fertility, children per woman	3.447	3.246	0.201	2.846	3.405	-0.558*
Proportion of people born in the municipality	0.674	0.646	0.028	0.727	0.649	0.078
Indigenous that do not speak Spanish	0.061	0.031	0.030	0.021	0.038	-0.017
Indigenous that speak Spanish	0.333	0.346	-0.013	0.434	0.412	0.021
Has at least High School	0.439	0.462	-0.022	0.469	0.481	-0.012
Agriculture is main income source of household	0.652	0.723	-0.072	0.741	0.580	0.161**
Dwelling Index	-0.195	-0.108	-0.088	0.144	0.201	-0.057
Assets Index	-0.144	0.157	-0.300	0.134	0.038	0.095
Organization enrollment	1.621	1.377	0.244	1.622	1.344	0.279*
Number of festivities attended	2.629	2.331	0.298	2.657	2.626	0.031
How actively she participated in festivities	1.545	1.408	0.138	1.510	1.481	0.030
Individual perception of how cooperative is the community	1.432	1.308	0.124	1.336	1.351	-0.015
Average perception of how cooperative is the community	1.323	1.367	-0.045	1.361	1.380	-0.019
Believes people are just	0.205	0.185	0.020	0.217	0.198	0.018
Believes people would take advantage if given the chance	0.424	0.477	-0.053	0.524	0.466	0.059
Believes effort is rewarded	1.348	1.562	-0.213	1.608	1.634	-0.025
Observations	**132**	**130**	**262**	**143**	**131**	**274**

Source: Post-experimental questionnaire
t-tests: *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table C6: Comparison of p-values obtained with T(G-1) distribution and cluster wild boostrap

	(1)	(2)	(3)	(4)	(5)
	Coefficient	T-statistic	T(G-1) p-value	WB p-value	N of clusters (G)
A. Table 2					
A1. Donors Partial					
One-to-one	0.45	1.703	0.132	0.108	8
Pool	0.22	1.061	0.324	0.224	8
A2. Donors Full					
One-to-one	0.371	1.405	0.210	0.124	7
Pool	0.168	0.508	0.629	0.512	7
B. Table 4					
B1. Recipients Partial					
One-to-one	0.179	0.623	0.553	0.506	8
Pool	-0.054	-0.171	0.869	0.870	8
B2. Recipients Full					
One-to-one	0.062	0.553	0.600	0.524	7
Pool	0.106	0.668	0.529	0.432	7
C. Table 3					
One-to-one	0.45	1.757	0.101	0.096	15
One-to-one*Full	-0.079	-0.219	0.830	0.862	15
Pool	0.22	1.095	0.292	0.214	15
Pool*Full	-0.052	-0.139	0.892	0.912	15

P-values in Columns (3) and (4) correspond to a two-sided test of the null that the reported coefficient is equal to zero. Column (3) presents the p-value that corresponds to Stata small sample correction for standard errors and the use of a T distribution with (G-1) degrees of freedom, where G is the total number of clusters in the estimation. Column (4) reports the p-value obtained with the cluster wild bootstrap proposed by CGM (2008) for the case of few clusters. For G>=15 we use Rademacher weights, as suggested by CGM (2008). For G<=8, we use the 6-point weights proposed by Webb (2013). In all cases, the number of bootstrap replications is 999.

Table C7: Within-subjects estimations of the effects of transfers on efforts, using *Autarky Low* as the reference game (in the *Partial* treatments)

	Donors		Recipients	
	(1)	(2)	(3)	(4)
One-to-one (β_1)	0.9119***	0.3158*	0.8705***	0.2691
	(0.128)	(0.141)	(0.207)	(0.211)
Pool (β_2)	0.6860***	0.0856	0.6351*	0.0360
p-value $\beta_1 = \beta_2$	0.3190	0.3110	0.2739	0.2691
Observations	524	524	572	572
R^2	0.100	0.012	0.085	0.008
Number of subjects	131	131	143	143
Individual FE	Yes	Yes	Yes	Yes
Learning Controls	round	model	round	model

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (19) are reported; instead of using the *Autarky* game as reference, here we use *Autarky Low*. In Columns (1) and (3) the dependent variable is the raw effort, and learning is controlled for using round dummies. In Columns (2) and (4) the dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3).

Table C8: Interaction with individual and household giving patterns

	Donors		Recipients		Donors		Recipients	
	Partial	Full	Partial	Full	Partial	Full	Partial	Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
One-to-one	0.290+	0.328*	0.0924	-0.105	0.238	0.265	-0.0502	-0.0266
	(0.153)	(0.167)	(0.239)	(0.136)	(0.176)	(0.178)	(0.147)	(0.110)
Pool	0.148	0.117	-0.149	-0.0115	0.148	0.449*	-0.525+	-0.0465
	(0.126)	(0.214)	(0.336)	(0.207)	(0.142)	(0.206)	(0.322)	(0.165)
	Individual giving				Household giving			
Giving* One to One	0.0553	-0.358*	0.364+	0.0675	0.131	-0.109	0.520**	-0.0830
	(0.297)	(0.153)	(0.210)	(0.217)	(0.241)	(0.170)	(0.215)	(0.171)
Giving* Pool	-0.154	-0.315	0.391	-0.0620	-0.0986	-0.810*	0.946*	0.0159
	(0.288)	(0.248)	(0.411)	(0.192)	(0.187)	(0.413)	(0.405)	(0.103)
Observations	512	436	556	468	512	436	556	468
R^2	0.012	0.013	0.012	0.002	0.012	0.032	0.031	0.001
Number of subjects	128	109	139	117	128	109	139	117
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by a Giving dummy. The value of the Giving dummy depends on the subject's answer to the following post-experimental survey questions: *During the last year, did someone in your household give any help in the form of clothes, food, money, ...?*, and *If yes, who did give this help?*

For the "Individual giving" results, the Giving dummy equals 1 if the subject answered "I did".

For the "Household giving" results, the Giving dummy equals 1 if the subject answered "Someone else in the household".

Table C9: Interaction with individual and household receiving patterns

	Donors		Recipients		Donors		Recipients	
	Partial	Full	Partial	Full	Partial	Full	Partial	Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
One-to-one	0.328+	0.258	0.246	-0.0180	0.206	0.335	0.224	0.0216
	(0.177)	(0.177)	(0.210)	(0.0209)	(0.159)	(0.203)	(0.207)	(0.102)
Pool	0.0892	-0.0556	0.0756	0.0117	0.0547	0.0668	-0.0950	0.115
	(0.186)	(0.205)	(0.205)	(0.101)	(0.207)	(0.293)	(0.176)	(0.207)
	Individual receiving				Household receiving			
Receiving* One to One	-0.0479	-0.209	0.0953	-0.131	0.302	-0.349+	0.108	-0.221
	(0.276)	(0.263)	(0.220)	(0.354)	(0.244)	(0.199)	(0.273)	(0.206)
Receiving* Pool	-0.0140	0.521**	-0.162	-0.0573	0.0499	-0.176	0.310	-0.343
	(0.227)	(0.208)	(0.394)	(0.359)	(0.255)	(0.340)	(0.285)	(0.274)
Observations	524	476	572	484	512	464	568	468
R^2	0.012	0.016	0.009	0.001	0.015	0.012	0.010	0.007
Number of subjects	131	119	143	121	128	116	142	117
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by a Receiving dummy. The value of the Receiving dummy depends on the subject's answer to the following post-experimental survey questions: *During the last year, did someone in your household receive any help in the form of clothes, food, money, ...?*, and *If yes, who did receive this help?*

For the "Individual receiving" results, the Receiving dummy equals 1 if the subject answered "I did".

For the "Household receiving" results, the Receiving dummy equals 1 if the subject answered "Someone else in the household".

Table C10: Donors - Interaction with individual characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
Donor Full						
One-to-one	0.419	0.373	0.277	0.228	0.236	0.246
	(0.276)	(0.360)	(0.170)	(0.181)	(0.186)	(0.262)
Pool	0.105	0.0535	0.179	0.0157	0.0411	0.0656
	(0.429)	(0.387)	(0.227)	(0.191)	(0.200)	(0.195)
Ind. characteristic	sex	age	secondary	dwelling	assets	celebrations
Charact* One to One	-0.293	-0.00393	-0.104	0.0342	-0.0414	-0.0351
	(0.343)	(0.00632)	(0.218)	(0.0771)	(0.0592)	(0.305)
Charact* Pool	-0.105	-0.000809	-0.319	-0.129	-0.0797	-0.0776
	(0.421)	(0.0130)	(0.231)	(0.0911)	(0.0855)	(0.343)
Observations	444	476	476	476	476	476
R^2	0.013	0.008	0.012	0.013	0.011	0.008
Number of subjects	111	119	119	119	119	119
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
Donor Partial						
One-to-one	-0.0740	0.684	0.601**	0.308*	0.315*	0.355
	(0.220)	(0.690)	(0.199)	(0.138)	(0.138)	(0.302)
Pool	-0.265	-0.144	0.235	0.0665	0.0857	0.119
	(0.210)	(0.438)	(0.166)	(0.153)	(0.154)	(0.177)
Ind. characteristic	sex	age	secondary	dwelling	assets	celebrations
Charact* One to One	0.595*	-0.00871	-0.593*	0.0397	0.0322	-0.0849
	(0.296)	(0.0144)	(0.261)	(0.0341)	(0.0597)	(0.428)
Charact* Pool	0.593*	0.00543	-0.310	0.0950	-0.00139	-0.0737
	(0.276)	(0.00815)	(0.241)	(0.110)	(0.0679)	(0.261)
Observations	496	524	524	524	524	524
R^2	0.025	0.015	0.023	0.014	0.012	0.012
Number of subjects	124	131	131	131	131	131
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by an Individual Characteristics variable (based on data collected through the post-experimental questionnaire), as follows:

Sex: a dummy variable which equals 1 if the individual is a woman. **Age**: the individual's age in years. **Secondary**: a dummy variable which equals 1 if the individual has completed secondary education. **Dwelling**: *dwelling index*, calculated using the principal components analysis on dummy variables for whether the participant or her family owns their home, availability of running water, toilet, electricity and dirt floor, the number of rooms and light bulbs. **Assets**: *asset index*, calculated using the principal components analysis using dummy variables for whether the household owns other real estate properties or land, vehicles, TV, radio, cellphone, gas stove, small kitchen appliances, refrigerator, washing machine, books; productive assets like agricultural machinery or animals; and small livestock. **Celebrations**: a dummy variable which equals 1 the individual reports participating in community celebrations.

Table C11: Recipients - Interaction with individual characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
			Recipient Full			
One-to-one	0.0337	-0.0921	-0.0415	-0.0356	-0.0482	-0.180
	(0.0496)	(0.445)	(0.0772)	(0.0693)	(0.0614)	(0.160)
Pool	0.101	-0.260	-0.112	0.00781	0.000595	0.0759
	(0.0974)	(0.459)	(0.152)	(0.104)	(0.108)	(0.210)
Ind. characteristic	sex	age	secondary	dwelling	assets	celebrations
Charact* One to One	-0.146	0.00122	-0.00551	0.0479	-0.0510	0.366
	(0.149)	(0.0108)	(0.256)	(0.0946)	(0.0521)	(0.347)
Charact* Pool	-0.199	0.00661	0.246	0.0429	0.00350	-0.203
	(0.240)	(0.0107)	(0.243)	(0.0583)	(0.0371)	(0.331)
Observations	464	484	484	484	484	484
R^2	0.002	0.002	0.003	0.001	0.002	0.010
Number of subjects	116	121	121	121	121	121
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

			Recipient Partial			
One-to-one	0.424	-0.307	0.311	0.246	0.278	0.264
	(0.319)	(0.329)	(0.233)	(0.198)	(0.221)	(0.238)
Pool	0.0271	-0.0819	-0.129	0.0309	0.0531	-0.213
	(0.456)	(0.312)	(0.282)	(0.196)	(0.206)	(0.268)
Ind. characteristic	sex	age	secondary	dwelling	assets	celebrations
Charact* One to One	-0.245	0.0143*	-0.0901	0.159	-0.0640	0.0120
	(0.255)	(0.00636)	(0.267)	(0.100)	(0.0613)	(0.268)
Charact* Pool	0.0106	0.00292	0.352	0.0353	-0.128+	0.540*
	(0.485)	(0.0116)	(0.290)	(0.0771)	(0.0708)	(0.279)
Observations	556	572	572	572	572	572
R^2	0.010	0.012	0.013	0.012	0.013	0.017
Number of subjects	139	143	143	143	143	143
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.

OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by an Individual Characteristics variable (based on data collected through the post-experimental questionnaire), as follows:

Sex: a dummy variable which equals 1 if the individual is a woman. **Age**: the individual's age in years. **Secondary**: a dummy variable which equals 1 if the individual has completed secondary education. **Dwelling**: *dwelling index*, calculated using the principal components analysis on dummy variables for whether the participant or her family owns their home, availability of running water, toilet, electricity and dirt floor, the number of rooms and light bulbs. **Assets**: *asset index*, calculated using the principal components analysis using dummy variables for whether the household owns other real estate properties or land, vehicles, TV, radio, cellphone, gas stove, small kitchen appliances, refrigerator, washing machine, books; productive assets like agricultural machinery or animals; and small livestock. **Celebrations**: a dummy variable which equals 1 the individual reports participating in community celebrations.

Table C12: Interaction with locality characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Donors							
	Partial				Full			
One-to-one	0.123	0.597***	0.174	0.313	0.491*	-1.391	0.615	0.587
	(0.218)	(0.0594)	(0.505)	(0.807)	(0.216)	(1.046)	(0.472)	(0.446)
Pool	-0.134	0.302**	-1.138+	-0.928	0.200	0.238	0.394	0.229
	(0.252)	(0.0941)	(0.597)	(0.966)	(0.186)	(0.981)	(0.304)	(0.402)
Location characteristic	Share Indigenous	Marginality index	Share Catholic	Relig. homoegeneity index	Share Indigenous	Marginality index	Share Catholic	Relig. homoegeneity index
Locality* One to one	0.286	1.726***	0.0915	-0.180	-0.376+	-6.111	-0.452	-0.443
	(0.344)	(0.446)	(1.036)	(1.899)	(0.223)	(4.059)	(0.574)	(0.576)
Locality * Pool	0.434	1.298**	2.007*	1.984	-0.246	0.544	-0.464	-0.234
	(0.333)	(0.527)	(1.014)	(1.911)	(0.305)	(4.034)	(0.363)	(0.488)
Observations	476	476	476	476	524	524	524	524
R^2	0.015	0.020	0.025	0.015	0.017	0.014	0.015	0.013
Number of subjects	119	119	119	119	131	131	131	131
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
	Recipients							
	Partial				Full			
One-to-one	-0.0836	-0.0213	-0.454***	-0.539***	0.192	2.269	-0.457	-0.458
	(0.0974)	(0.0261)	(0.0653)	(0.0928)	(0.263)	(3.216)	(0.468)	(0.509)
Pool	0.0696	-0.244***	-0.117	0.109	0.0502	0.971	0.00623	-0.0157
	(0.136)	(0.0304)	(0.247)	(0.356)	(0.0871)	(3.788)	(0.344)	(0.451)
Location characteristic	Share Indigenous	Marginality index	Share Catholic	Relig. homoegeneity index	Share Indigenous	Marginality index	Share Catholic	Relig. homoegeneity index
Locality* One to One	0.0905	0.107	0.690***	0.999***	0.167	7.163	1.110	1.205
	(0.107)	(0.196)	(0.139)	(0.229)	(0.422)	(11.34)	(0.718)	(0.900)
Locality* Pool	-0.158	-1.155***	0.198	-0.219	-0.0307	3.348	0.0455	0.0858
	(0.210)	(0.263)	(0.509)	(0.779)	(0.412)	(13.18)	(0.673)	(0.862)
Observations	484	484	484	484	572	572	572	572
R^2	0.002	0.006	0.003	0.003	0.009	0.010	0.018	0.018
Number of subjects	121	121	121	121	143	143	143	143
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors (cluster session) in parentheses. *** p<0.01, ** p<0.05, * p<0.1, + p<0.15.
OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by a Locality Characteristic variable (based on Census data at the municipality level), where Marginality is the Social Backwardness index (SBI) (see Table B2), and the Religious homogeneity index is defined in Table B1.

Table C13: Interaction with the answer to post-experimental questions

	(1)	(2)	(3)	(4)	(5)	(6)
	Donors				Recipients	
	Partial	Full	Partial	Full	Partial	Full
One-to-one	0.338**	0.219	0.277+	0.0585	0.226	-0.0408
	(0.123)	(0.164)	(0.171)	(0.215)	(0.257)	(0.0614)
Pool	0.182	0.0814	0.0328	-0.132	0.219	0.0400
	(0.233)	(0.213)	(0.142)	(0.305)	(0.214)	(0.141)
Question	give more/less		change opinion		reveal effort	
Question * One to One	0.00739	0.00789	0.228	0.296+	0.0814	-0.00952
	(0.288)	(0.156)	(0.237)	(0.161)	(0.276)	(0.121)
Question * Pool	-0.218	-0.133	0.215	0.291	-0.659	-0.175
	(0.395)	(0.174)	(0.385)	(0.420)	(0.447)	(0.128)
Observations	512	472	496	464	564	464
R^2	0.015	0.009	0.015	0.010	0.019	0.002
Number of subjects	128	118	124	116	141	116
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors (cluster session) in parentheses: *** p<0.01, ** p<0.05, * p<0.1.
OLS estimations of a variant of equation (20), are reported, with *Autarky* being the reference game. The dependent variable is the effort purged of learning, using the linear specification in equations (1)-(3). In equation (20) the Full dummy is replaced by an Answer dummy (based on questions asked after all the games were played), as follows:
In Column (1) the Answer dummy equals 1 if the subject answered "yes" to the question: "Would you have liked to give more?"
In Column (2) the Answer dummy equals 1 if the subject answered "yes" to the question: "Would you have liked to give less?".
In Columns (3) and (4) the Answer dummy equals 1 if the subject answered "yes" to the question: "Would your answer to the previous question [see the two previous question] change if the recipient had been able to make an effort?".
In Columns (5) and (6) the Answer dummy equals 1 if the subject answered "yes" to the question: "If we gave you the opportunity to reveal your effort in the *One-to-one* game to the individual to whom you were matched, would you do so?"

Figures

Payoffs in the *Partial* treatments

Game	State	Payoff to Active	Payoff to Passive
DP Autarky	S	100 + *75*	
	F	100	
DP One-to-One	S	100 + *75* - **25**	50 + **25**
	F	100	50
DP Pool	S	100 + *75* - **25**	50 + n_s(25/n)+ **25/n**
	F	100	50 + n_s(25/n)
DP Aut Low	S	100 + *50*	
	F	100	
RP Autarky	S	100 + *75*	
	F	100	
RP One-to-One	S	100 + *75*	200
	F	100 + **25**	200 - **25**
RP Pool	S	100 + *75*	200 - n_f(25/n)
	F	100 + **25**	200 - n_f(25/n) - **25/n**
RP Aut Low	S	100 + *50*	
	F	100	

Payoffs in the *Full* treatments

Game	State	Payoff to Active	Payoff to Passive
DF Autarky	S	100 + *75*	
	F	100	
DF One-to-One	S	100 + *75* - **75**	25 + **75**
	F	100	25
DF Pool	S	100 + *75* - **75**	25 + n_s(75/n)+ **75/n**
	F	100	25 + n_s(75/n)
DF Public	S	100 + *75* - **75**	25 + n_s(75/n)+ **75/n**
	F	100	25 + n_s(75/n)
RF Autarky	S	25 + *75*	
	F	25	
RF One-to-One	S	25 + *75*	175
	F	25 + **75**	175 - **75**
RF Pool	S	25 + *75*	175
	F	25 + **75**	175 - n_f(75/n) - **75/n**
RF Public	S	25 + *75*	175
	F	25 + **75**	175 - n_f(75/n) - **75/n**

Game orders used in the *Partial* treatments

Order A	Order B
Incentivized ability test 1	
Autarky	Autarky low
Autarky Low	Autarky
Pool	One-to-One
One-to-One	Pool
Incentivized ability test 2	
Post-experimental questions	
Questionnaire	

Game orders used in the *Full* treatments

Order A	Order B
Incentivized ability test 1	
Autarky	Autarky
Pool	One-to-One
One-to-One	Pool
Public	Public
Incentivized ability test 2	
Post-experimental questions	
Questionnaire	

Figure 1: The top four boxes show the payoff consequences of the active subject's effort in the games played by subjects in the *Donor Partial* (DP) games, the *Donor Full* (DF) games, the *Recipient Partial* (RP) games, and the *Recipient Full* (RF) games. In each cell, the first number is the initial endowment, an italicized number the additional income, and a bold number a transfer. S denotes success, F failure, n the number of passive subjects, n_s the number of active subjects other than the subject at hand who succeeded, and n_f the number of active subjects other than the subject at hand who succeeded. The bottom two boxes show the session structures. Two alternative game orders were used in the *Partial* treatments (left column) and two alternative game orders were used in the *Full* treatments (right column). The order highlighted in dark grey is Order A, and the one highlighted in lighter grey is Order B.

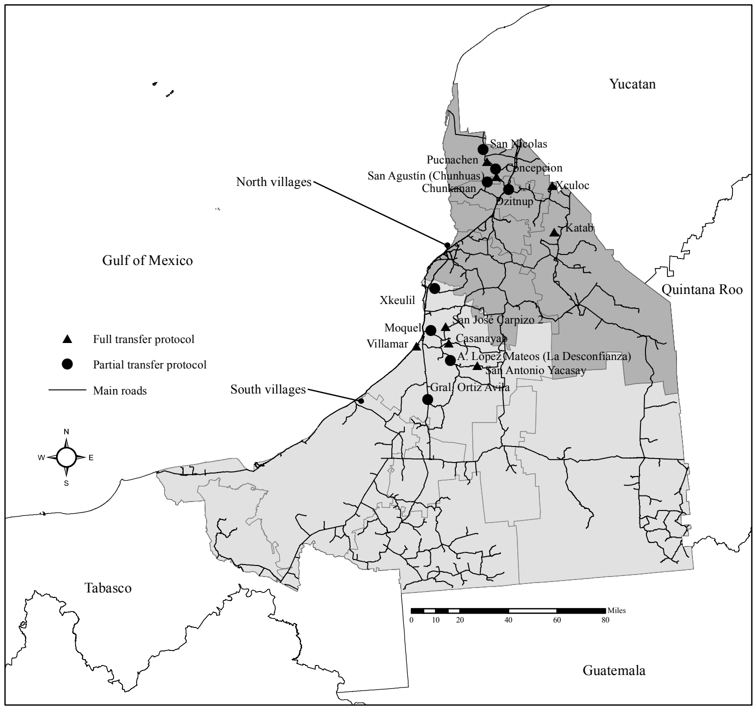

Figure 2: Map of selected localities and treatment allocation.

	Panel A : Donors		
	Selfish	*Altruist*	*Warm glow*
One-to-One	$e^{o}_{1,D} < e_A$	$e^{\alpha}_{1,D} \gtrless e_A$	$e^{\gamma}_{1,D} \gtrless e_A$
Pool	$e^{o}_{n,D} = e^{o}_{1,D}$	$e^{\alpha}_{n,D} \gtrless e^{\alpha}_{1,D}$	$e^{\gamma}_{n,D} = e^{\gamma}_{1,D}$
	Panel B : Recipients		
	Selfish	*Altruist*	*Warm glow*
One-to-One	$e^{o}_{1,R} < e_A$	$e^{\alpha}_{1,R} < e_A$	$e^{\gamma}_{1,R} \gtrless e_A$
Pool	$e^{o}_{n,R} = e^{o}_{1,R}$	$e^{\alpha}_{n,R} \gtrless e^{\alpha}_{1,R}$	$e^{\gamma}_{n,R} = e^{\gamma}_{1,R}$
	Panel C : Donors vs. Recipients		
	Selfish	*Altruist*	*Warm glow*
One-to-One			
Partial	$e^{o}_{1,D(P)} > e^{o}_{1,R(P)}$	$e^{\alpha}_{1,D(P)} > e^{\alpha}_{1,R(P)}$	$e^{\gamma}_{1,D(P)} > e^{\gamma}_{1,R(P)}$
Full	$e^{o}_{1,D(F)} = e^{o}_{1,R(F)}$	$e^{\alpha}_{1,D(F)} > e^{\alpha}_{1,R(F)}$	$e^{\gamma}_{1,D(P)} = e^{\gamma}_{1,R(P)}$

Note: $e^{\text{Preferences}}_{\text{Game,Treatment}}$ denotes effort of an individual whose preferences are Selfish (o), Altruistic (α), or Warm Glow (γ), in the game *Autarky* (A), *One-to-One* (1), or *Pool* (n), in the treatment *Donor* (D), or *Recipient* (R). When information about *Partial* (P) and *Full* (F) is absent, it means that the result applies to both.

Figure 3: Theoretical predictions.

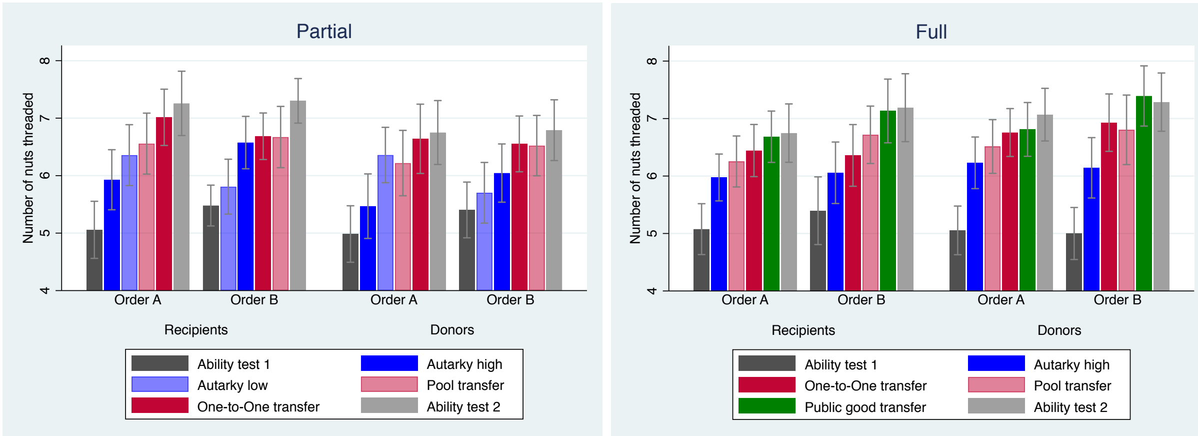

Figure 4: Raw levels of effort in the ability tests and the experimental games.

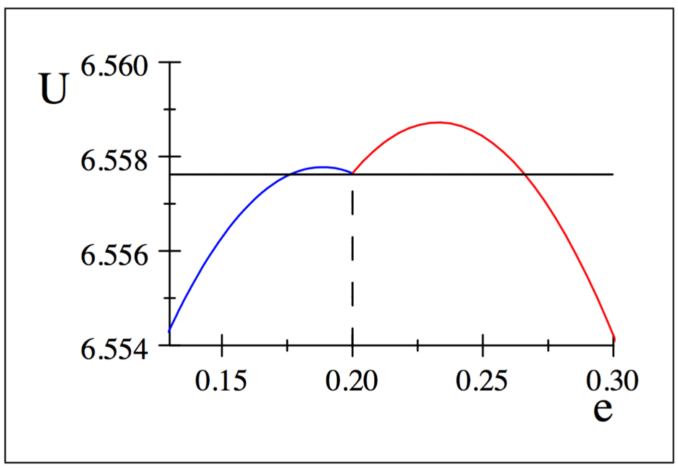

Figure 5: $U(e_i, \bar{e})$ for $\bar{e} = 0.2$

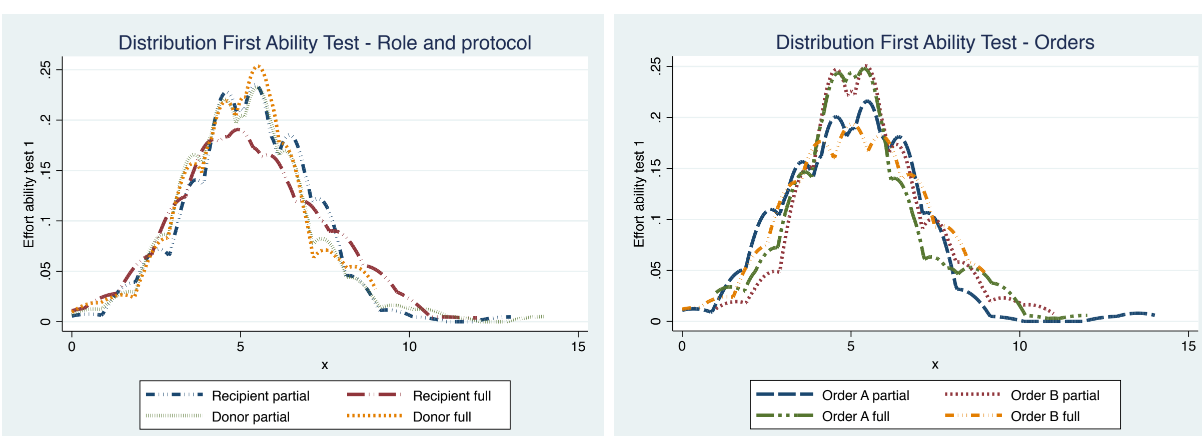

Figure 6: Effort distributions in the first ability test, for different treatments and roles, and for the different orders.

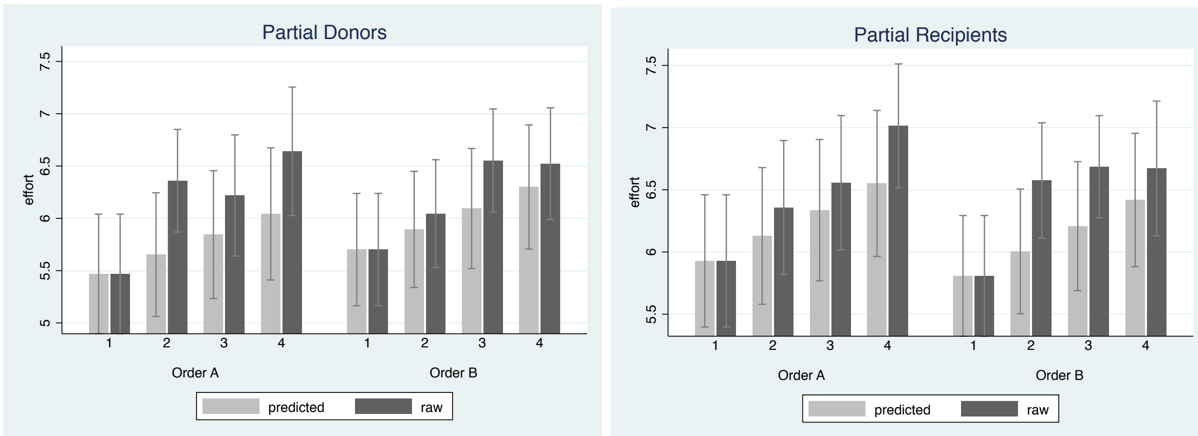

Figure 7: The figure shows the raw and the predicted levels of effort (as per equation (2)) across roles (donor and recipients) for the autarky and transfer games in the *Partial* treatments. The orders are: (A) *Autarky*, *Autarky Low*, *Pool*, *One-to-one*, and (B) *Autarky Low*, *Autarky*, *One-to-one*, *Pool*. Predicted and raw effort coincide in the first round by construction.

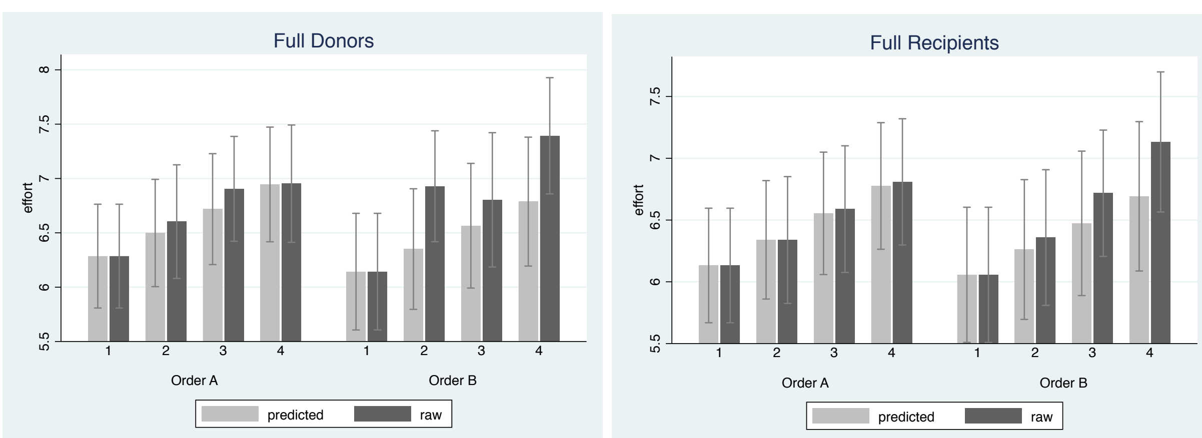

Figure 8: The figure shows the raw and the predicted levels of effort (as per equation (2)) across roles (donor and recipients) for the autarky and transfer games in the *Full* treatments. The orders are: (A) *Autarky*, *Pool*, *One-to-one*, *Public* and (B) *Autarky*, *One-to-one*, *Pool*, *Public*. Predicted and raw effort coincide in the first round by construction.