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Inequality, Relative Income, and Development: Field-Experimental Evidence from the Bolivian Amazon*

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Abstract

Much macroeconomic research hypothesizes income distribution to play an important role for economic growth, as inequality shapes the process of development through its interaction with credit market imperfections or the political economy. However, the nature of this research remains conjectural, and all hitherto available evidence comes from correlations in observational data. As a consequence, little is known empirically about the causal relationship between inequality and growth, its determinants, and its direction. To obtain evidence on the causal link from inequality to growth, this paper presents an analysis of a randomized controlled trial in 40 villages of an Amazonian foraging-farming society. In the experiment, we randomly allocated substantial income transfers in the form of rice, altered their associated degree of village income inequality, and measured the short-run effects on important individual-level determinants of development. We find that human capital investments (in the form of studying Spanish) and modern-asset wealth, both of which increase the villagers' expected future earnings through exposure to the outside labor market, are driven by relative social comparisons. Our results establish a causal link between inequality and economic growth, and – exploiting the village-level variation in our experiment – suggest a U-shaped relationship.

JEL classification: D31, E20, O10, O15

Keywords: human capital investment, social comparison, forager-farmers, income distribution, randomized controlled trial

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1 Introduction

This paper presents causal evidence on the long-standing question of how inequality affects economic growth, which generates new insights into underlying microfoundations of development within community contexts. In a field experiment in 40 villages of an Amazonian foraging-farming society, the Tsimane', we conducted a randomized controlled trial to assess the effects of income and community income inequality on human capital investment and asset holdings.

The literature on the relationship between inequality and economic growth, surveyed by Benabou (1996) and – more recently – Galor (2011), is not conclusive. Most cross-country studies report a negative correlation between income inequality and GDP growth (e.g., Alesina and Rodrik (1994) and Persson and Tabellini (1994)). Theories supporting a negative causal link either emphasize the absence of well-functioning credit markets (see, for instance, Banerjee and Newman (1991) and Galor and Zeira (1993)), or introduce a political process underlying the level of redistribution in the economy (e.g., Alesina and Rodrik (1994), Persson and Tabellini (1994), and Benabou (2000)). More related to our setting in the Bolivian Amazon, where fluency in Spanish is identified as one of the strongest drivers of individual economic success in market-related activities, Perotti (1996) provides evidence that inequality is associated with a lower level of investment in human capital, which is in turn associated with lower economic growth.

The use of panel-data techniques has brought a revision of findings, leading to evidence suggesting different forms of the relationship between inequality and economic growth. Barro (2000) finds that the effect of income inequality on growth depends on the level of economic development: the correlation is negative for countries with low per-capita GDP and positive for the rest. Forbes (2000) also challenges the traditional view of a negative relationship between income inequality and growth, and reports a positive correlation in the short and medium terms.

In all of the above-cited studies, the direction of causality is not clear. To advance understanding of possible causal effects of inequality on economic growth, we conducted an experiment in which we exogenously varied income inequality in a relatively pure environment – where premises of typical models, such as political-economy channels through formal institutions, are absent or play only subordinate roles – to investigate the immediate impact of community income inequality on important growth-enhancing activities. The data that we collected include clean measures of changes in human and physical capital, in a setting in which there is a clear notion of what kinds of investments support growth, namely studying Spanish for use in market-related activities rather than foraging-farming (the default activity of the Tsimane'). In the experiment, we transferred edible rice as a positive one-time income shock. In Treatment 1, 13 villages received 782 kg of edible rice, which we divided equally among all households in each village. In a control treatment, we transferred 5.9 kg of modern rice seeds to each household in 14 villages. Last, in Treatment 2, we allocated the same total amount of edible rice as in Treatment 1 (782 kg) to another 13 villages, but only to the poorest 20% of households in each village; as a consolation, the remaining 80% of households in each village received 5.9 kg of modern rice seeds. The latter treatment allows us to contrast between absolute income effects, as expected under all treatments, and relative income effects due to social comparisons, which have been found to play a critical role for well-being (Luttmer (2005)) and for effort exertion in workplace environments (Cohn, Fehr, Herrmann, and Schneider (2011)).

To motivate our interpretation of our results, consider the cross-country correlation between

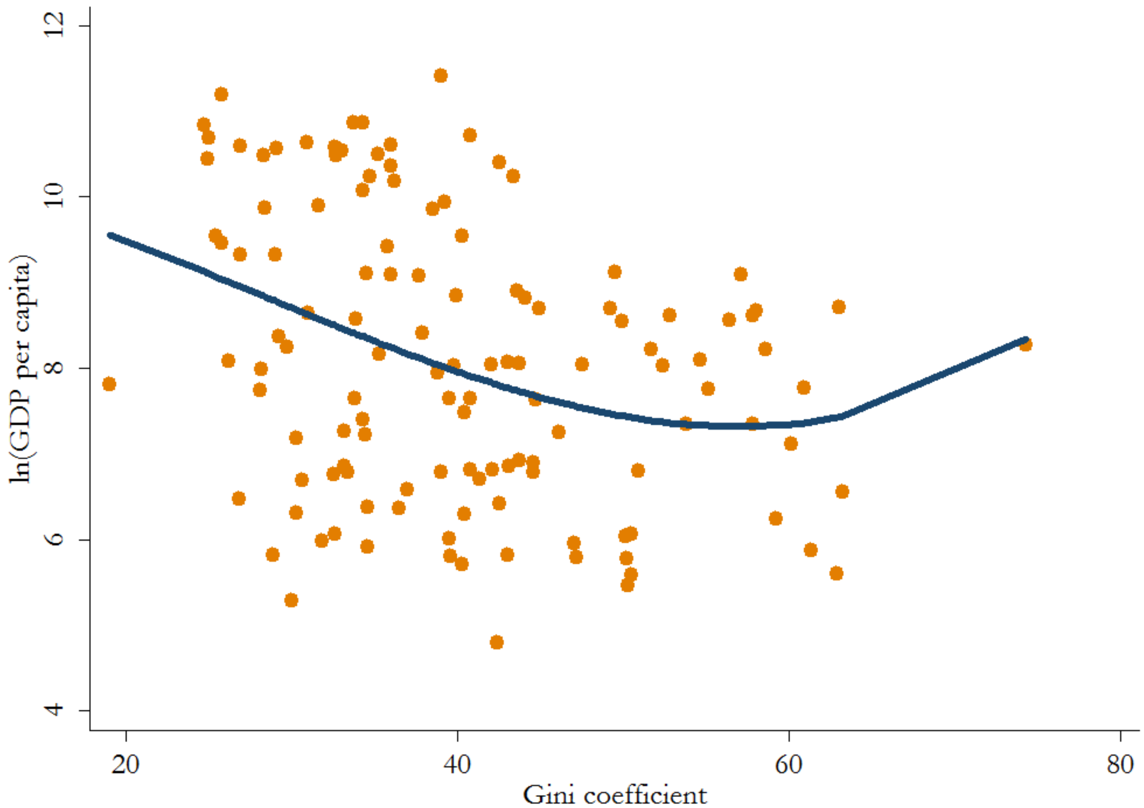

Figure 1: GDP vs. Gini (126 countries in 2006, GDP per capita in 2011 USD)
Data source: World Bank World Development Indicators

GDP per capita and the Gini coefficient, and its polynomial fit, which is U-shaped (or L-shaped, cf. Figure 1). A potential explanation for this shape that is consistent with our interpretation of the experimental findings is the role of social comparisons. The idea is twofold: first, we show that the bottom 20% invested heavily in market-related human capital through improving Spanish fluency when they received rice, as long as the transfer was not associated with a reduction in income inequality. That is, we compare the behavior of the bottom 20%, while controlling for the actual amount received by each individual, under all treatments, and find that the bottom 20% invested in human capital in Treatment 1, but significantly less so in Treatment 2. This suggests that a reduction in income inequality evokes a negative income effect due to social comparisons: when the absolute distance to the top 80% was reduced, the bottom 20% derived utility from this reduction, and did not invest more to increase their future income. In contrast, in the case of income-distribution stability (as in Treatment 1 and the control group) receiving income in the form of rice freed up resources that were used for income-enhancing activities, such as studying Spanish.

On the other hand, we find that the top 80% increased their Spanish fluency in all treatments, i.e., even when the absolute distance in terms of income was reduced under Treatment 2. We find similar evidence for physical-asset portfolios: the top 80% shifted their physical capital from traditional to modern assets in Treatment 2, whereas the bottom 20% did not increase either type of assets. Spanish fluency and the acquisition of modern assets are intertwined in that both activities are conducive to increase exposure to the outside labor market. They can also be considered complements, as modern assets such as radios support investment in Spanish skills through the transmission of educational programs.

Furthermore, we use comparative statics based on ex-ante distributions of both consumption and income in the villages to show that – in line with the relationship implied in Figure 1 – the negative impact of increased income equality is conditional on a relatively unequal ex-ante distribution. Conversely, the positive impact of increased income equality is stronger in villages with more equal ex-ante distributions. Put differently, the bottom 20% invested less (income effect due to social comparisons) the further away their position was from that of the top 80% in the initial distribution because Treatment 2 then constituted a greater relative relief. The top 80% invested more under Treatment 2 the closer their position in the initial distribution was to that of the bottom 20% because the latter then exogenously caught up more. These comparative statics lend support to the idea that comparison-concave utility is the driving force behind our results.

Our findings provide evidence that individual investments that increase future income – and, thus, economic growth in the aggregate – are driven importantly by social comparisons. In particular, our experimental design allows us to disentangle absolute income effects from relative income effects due to social comparisons. This proves useful in interpreting other social experiments. Most notably, the Negative Income Tax (NIT) experiments in urban areas (five cities) in New Jersey and Pennsylvania (1968-1972), rural areas (three counties) in North Carolina and Iowa (1970-1972), Seattle and Denver (1970-1976), and Gary (1971-1974) led to an extensive discussion about the effect on labor supply. Leaving aside statistical and other model issues, the majority view is that the NIT treatment caused a reduction in work hours (e.g., Moffitt (1979) and Moffitt (1981)). Our results (namely for the bottom 20% under Treatment 2) would explain that income effect as stemming chiefly from the increased income equality under the NIT treatment, primarily for those who were poorer to start with. This is strikingly consistent with analyses such as Robins (1985) who showed, for example, that across these experiments young people reduced labor supply more than adults.

The remainder of this paper proceeds as follows. In Section 2, we discuss the context and the experimental design of our study. Section 3 provides some theoretical background on the testable mechanism of social comparison underlying the experimental outcome variables. Section 4 presents the results alongside robustness checks, and Section 5 concludes.

2 Context and Experimental Design

2.1 Background on the Tsimane'

In this paper, we present results from a randomized controlled trial conducted in a foraging-farming society in the Department of Beni, where the Tsimane' of Amazonian Bolivia reside. This can be considered a highly autarkic environment in that the Tsimane' only recently opened up to regular contact with Westerners, initiated by exposure to Protestant missionaries in the early 1950s. The market exposure of the Tsimane' is very limited, even compared to other small-scale foraging-horticultural societies, as reported by Henrich et al. (2010). Besides hunting game and fish (and selling these and other goods to outsiders), the Tsimane' practice slash-and-burn agriculture. Since the Protestant missionaries started offering training, the Tsimane' are also aware of the returns to (voluntary) schooling: studying Spanish with a local teacher[1] and gaining a rudimentary command of the language allows the Tsimane' to interact more closely with loggers, farmers, cattle ranchers, and other outsiders who may offer employment opportunities. The nature of the economy (for more detail, see, for instance, Godoy et al. (2005) and Saidi (2012)) determines the three sources of monetary income: sale of forest and farm goods, wage labor (for which studying Spanish beyond the most rudimentary level is required), and barter trade.

In general, the Tsimane' are an attractive society for our experiment both because we have many years of field experience with them as part of a panel study ($2002-2010$) and because such a setting allows us to control for confounders that plague studies in industrial societies. For example, community or state income inequality in industrial nations picks up the effects of community ethnic-racial heterogeneity and of inequality in access to opportunities (Deaton (2003)). A highly autarkic, endogamous, small-scale society such as the Tsimane' community has much less of such confounders.

The Tsimane' live in over 100 villages. To select the sample, we eliminated villages that took part in any other studies, were too costly to reach, too small or unsafe, or that contained other ethnic groups. This left 65 villages of which we randomly selected the final sample of 40 villages. During the surveys, we collected demographic and anthropometric information and data on reported health from all members in a household, but we limited data collection on most other variables to people ≥ 16 years of age (or younger if they headed a household). We selected 16 years of age as the cut-off because Tsimane' typically set up independent households by that age. The sample includes 563 households comprising $3,449$ individuals. Roughly one-fifth (713 people) of the baseline sample left by follow-up, and another 127 people were present during the two surveys but absent when the transfers took place between the two surveys. We exclude these individuals from the main analysis, but examine attrition below. Finally, we are left with 495 households, of which 155 belonged to Treatment 1, 175 to Treatment 2, and 165 to the control group. We have data on all the variables used in our regressions for 987 adults.

2.2 Study Design

2.2.1 Experimental Treatments

We did not use cash for our transfers because this society is not very monetized, but relies instead substantially on own production and bartering. Thus, we opted to transfer rice as a form of in-kind income: rice is among the most fungible commodities, it is the cash crop of choice, and rice trade is one of the dominant market activities of the Tsimane'. When transferring either edible rice or modern rice seeds to households, we selected at random either the female or the male head of the household to receive the transfer (note that 4% of households had only one head). We conducted the baseline survey from February to May 2008, and the follow-up survey from February to May 2009. The treatments took place between October 2008 and January 2009.

Among households in Treatments 1 and 2, 62% of the household heads who received transfers were present during the transfers to directly receive the rice from us, but among households in

[1] Local teachers in charge of Tsimane' education were trained and paid by missionaries (from 1954 until 1985) or by the Bolivian government (since 1985).

the control group only 52% of the household heads selected were present during the transfer (the difference in means is significant at the 5% level). If the household head selected was missing at the time of the transfer, we gave the edible rice or the rice seeds to a third party, such as the other spouse, another adult of the household who was not a spouse, or to a village authority (e.g., a teacher). We asked the third party to give the edible rice or the rice seeds to the absent household head who had been randomly selected to receive the rice when that head returned. The regression results in this paper are robust to excluding third-party transfers from our sample (these alternative estimates are available on request).

At the time of the transfer, we asked the recipients an open-ended question about their plans for the use of the transfers. Among household heads in Treatments 1 and 2 who received the edible rice directly from us, most said they planned to eat the rice, but 10% hinted at the possibility that they might use the edible rice as a form of payment to recruit workers to help them in farm chores. Among household heads in the control group who answered the question, 98% reported they planned to plant the rice seeds, and 2% (4 household heads) said they planned to eat them.

During the follow-up survey, we asked the household heads about their actual use of the transfers. Households in Treatments 1 and 2 (if among the bottom 20%) reported eating 76.5% of the edible rice received and giving away 11.1% as gifts. They did not sell any of the rice, and used only 2.8% of the edible rice to barter. People reported that 1.4% of the edible rice had been stolen, and 8.2% of the edible rice received was in storage.

Of the rice seeds received by the control group, households planted 81.4%, ate 4.6%, gave away 2.5%, and sold 0.6%. By the time of the follow-up survey, 6.7% of the rice seeds had spoiled, 3.8% were in storage, and 0.4% had been stolen. Households did not use the rice seeds to barter for other goods.

In sum, most households did not sell or barter the transfers received; households in the two treatment groups mainly consumed the edible rice received, and households in the control group mainly planted the rice seeds, consistent with their anticipated use of the transfers, as reported by participants at the time that the transfers were made. We now discuss the treatments in greater detail.

Treatment 1. Each of the 13 villages in Treatment 1 received a total of 782 kg of edible rice, which we divided equally among all the households in the village on the day of the transfer. Transferring the same amount of rice to each village ensured that each village was affected by the same aggregate positive income shock. Because villages differed in the number of households they contained, transferring the same amount of rice to each village introduced variation across villages in the amount of edible rice received by each household that is inversely related to village population. The average amount of edible rice received by households in Treatment 1 was 53.0 kg (std. dev. = 23.7 kg; range from 5.8 to 138 kg).

Treatment 2. In order to keep any aggregate effects constant across the two treatments, the total amount of edible rice received by each of the 13 villages in Treatment 2 was the same (782 kg) as that received by each of the 13 villages in Treatment 1. We used the area of forest cleared by households in the pre-treatment year 2008 to identify the poorest 20% of households in each village. We shall discuss this further below. Besides the 782 kg of edible rice, we also transferred 5.9 kg of modern rice seeds to each household in the top 80%.

Since the transfers of edible rice in villages receiving Treatment 2 went only to households in the bottom 20% of the village distribution, the amount of edible rice each treated household received in Treatment 2 surpassed substantially the amount of rice received by treated households in Treatment 1: the average amount of edible rice received by households in the bottom 20% of the village distribution in Treatment 2 was 143.6 kg (std. dev. = 74.3 kg; range from 28.8 to 414 kg). Note that if the village-population distributions were the same for villages in Treatments 1 and 2, then the average amount of rice should be five times the one for Treatment 1. This is, however, not the case because the distributions of population sizes for the two treatments are not identical, which may not be surprising given the relatively small number of villages for the randomization. Another reason is that given the discrete nature of the cleared-forest distributions, most of the time we capture the bottom 25-35% rather than the bottom 20% because the groups below 20% in the distribution may add up to only slightly less than 20%.

Control group. The control group consisted of 165 households in another 14 villages. Each household serving as a control received 5.9 kg of modern rice seeds.

Forest area cleared as proxy for income. Each year during the dry season (June to September), households clear old-growth and fallow forests from the village commons to plant annual and perennial crops for the coming year. The main annual crops include rice, maize, and manioc. These main crops are planted with plantains and with a wide range of perennials and other plants that the Tsimane' use for house construction, crafts, and medicines. During 2007 (the year before the baseline study), households cleared an average of 0.7 hectares of old-growth forest (median = 0.5, std. dev. = 0.9), 0.5 hectares of fallow forest (median = 0.2, std. dev. = 0.7), and 1.1 hectares of total forest (sum of old-growth and fallow forest; median = 1.0, std. dev. = 0.9).

Households have usufruct rights to the plots they clear from the forest, but they cannot sell the plots because land is communally owned by the Tsimane'. Forest area cleared for farming is a reasonable proxy for income for several reasons. First, area of cleared forest is an annual flow variable because households have to clear old-growth forest and/or fallow forest each year to plant. Second, people clear forest to plant rice, the main cash crop and the form of in-kind income chosen for our experiment. Third, people consume all the output from cleared forest that they do not sell. The last two points matter because they suggest that the area of cleared forest captures both the main source of monetary income and income flowing into the household through consumption from the latter's own farm production. Besides these reasons, area of cleared forest has another advantage. In previous work, Vadez et al. (2003) have found that reported area of cleared forest matches well the area of cleared forest as measured on the ground by our research team. Thus, reported area of cleared forest has low measurement error. This said, the measure also has shortcomings. For instance, it does not capture income from wage labor or income from the sale of non-timber forest goods (e.g., thatch palm), and it underestimates income of households in more remote villages, which are more likely to depend on foraging than on farming.

2.2.2 Economic Significance of Transfers

To assess the economic significance of the rice transfers in Treatments 1 and 2, we consider the monetary value of the rice transfers using current village rice prices. In 2009, the selling price of local rice in the main regional towns of the study area was 8 bolivianos (BS) per kilogram. Using the average amounts of edible rice per household member (dividing the household transfers by the number of people in the respective households), at the going exchange rate in 2009 (7 BS / US$1) observed during fieldwork in the two main market towns of the region (San Borja and Yucumo), the average transfers of edible rice amounted to US$12.62/person in Treatment 1 and US$34.57/person for the bottom 20% in villages in Treatment 2. For a family living at a daily poverty measure of $1/person, the transfers would amount to income earned over 12.6 days (Treatment 1) or 34.6 days (Treatment 2). The economic significance of the transfers might be even higher than suggested by these figures because according to the Government of Bolivia and the World Bank (2005), indigenous people in the Department of Beni are among the poorest in Bolivia. If we use the daily per-capita income of the extremely poor used by the Government of Bolivia (US$0.62), then the transfers would amount to income earned over 20.3 (Treatment 1) or 55.8 days (Treatment 2).

In order to estimate the monetary value of the 5.9 kg of modern rice seeds transferred to the top 80% in Treatment 2 and to the control group, we start with the price paid for the seeds (10 BS/kg) in the city of Santa Cruz, the closest major city to the study area selling this type of seed, and add the transport cost to the town of San Borja in the study area (2 BS/kg). Proceeding in the same fashion as above, the monetary value of the rice-seed transfers was US$2.17/person. The perceived value of the rice seeds might have been even lower than this because there is no market for modern rice seeds in the study area. Tsimane' buy local seeds in local towns. For example, 12% of the 303 households surveyed annually during 2004-2007 as part of our panel study reported buying rice seeds. However, the rice seeds transferred to households were new to them for they were an improved variety. Being unfamiliar with the use of this type of seeds, the Tsimane' may not have valued them as much as traditional local seeds.

Given that most households reported to have planted rather than sold them, the rice seeds can also be understood as a deferred benefit. According to our field experience, planting 5.9 kg of modern rice seeds – requiring 15 person days for clearing, planting, weeding, and harvesting – yields approximately $1,687$ bolivianos = US$241 worth of edible rice, and it takes four to five months from planting to harvest. The labor cost amounts to 15×40 bolivianos (daily wage) = US$86 in addition to transportation cost of 420 bolivianos = US$60. Assuming an average household size of six (cf. second row of Table 2), the rice-seed transfer can thus translate to an income transfer of (US$241 − US$146)/6 = US$15.83/person, not accounting for any discounting. Depending on whether households decided to plant the rice seeds or not, our consolation prize might actually have been a deferred benefit greater in amount than the transfer in Treatment 1 (see above, US$12.62/person) but still significantly lower than that in Treatment 2 (US$34.57/person). While in most regressions we will treat 1 kg of rice seeds and edible rice as equivalent quantities, we will, in Section 4.4, explicitly account for whether villagers who received the seeds planted them and, thus, harvested rice later.

3 Theoretical Framework: Social Comparison and Individual Effort

In this section, we present an analysis to characterize the income effects in our two main treatments, and discuss the comparative statics that we test. Consider a villager i with base income y_{i0} and transfer t_i who, at a marginal cost $c_i > 0$, exerts effort $e_i \in [0,1]$ which determines the probability of receiving a high additional income X^H as opposed to a low additional income $X^L < X^H$. This simple setting captures the main features of the villagers' actual decision problem, namely that they invest their time either foraging-farming or studying Spanish and subsequently earning a wage. To keep the setting as simple as possible, we set up a one-period model and – for now – neglect the potentially risky nature of human capital investments.

We incorporate social comparisons by allowing the villager to put non-zero weight on the sum of income comparisons with all other villagers (akin to Yitzhaki (1979)'s index of relative deprivation). That is, the villager puts some weight $\theta \in [0,1]$ on linear utility from his direct income, and additionally extracts utility through her relative position, modeled as the average utility from relative comparisons.

The base income y_{i0} characterizes villager i's position in the income distribution, and affects her ability to generate additional income. The induced correlation between initial income and the ability to generate additional income can be understood as signifying that one's current position in the income distribution has a persistent nature. To this end, we use a quadratic cost function for e_i that decreases in $y_{i0}+t_i$, namely $\frac{1}{2}c_i e_i^2 = \frac{1}{2}\frac{c}{\delta(y_{i0}+t_i)}e_i^2$ with $c, \delta \in \mathbb{R}^+$. Finally, we have the following decision problem:

$$\max_{e_i}\left\{\theta\left(y_{i0}+t_i+e_iX^H+(1-e_i)X^L\right)+(1-\theta)\frac{1}{n-1}\sum_{j\neq i}v\left(z_i-z_j\right)-\frac{1}{2}\frac{c}{\delta\left(y_{i0}+t_i\right)}e_i^2\right\}, \quad (1)$$

where $v(\cdot)$ is a concave function, defined on the interval $\left[-y_{i0}-t_i-X^H, y_{i0}+t_i+X^H\right]$, $v'\left(-y_{i0}-t_i-X^H\right) < \infty$, $z_i \equiv y_{i0}+t_i+e_iX^H+(1-e_i)X^L$, and n is the number of all individuals (including i) in i's proximity/village.

Just like in Clark and Oswald (1998), if v is linear (or if $\theta = 1$), comparison income does not impact i's choice. This is, however, not the case with comparison-concave utility $v(\cdot)$. To see this, consider the case of $v(x) = \alpha x - \beta x^2$. Then, the first-order condition with respect to e_i, villager i's work-effort choice, is:

$$\Delta X\left[\theta+(1-\theta)\left(\alpha-\frac{2\beta}{n-1}\sum_{j\neq i}\left((y_{i0}+t_i+e_i\Delta X)-(y_{j0}+t_j+e_j\Delta X)\right)\right)\right]-\frac{c}{\delta\left(y_{i0}+t_i\right)}e_i=0, \quad (2)$$

where $\Delta X \equiv X^H - X^L$.

In our experiment, we varied the income distribution under Treatment 2, which can be captured analytically by *fluctuations* in $z_i - z_j$. The following proposition, the proof of which can be found in the Appendix, characterizes the direction of the relative income effect resulting from an increase in income equality.

Proposition 1 *The equilibrium level of effort e_i^* of villager i with comparison-concave utility derived from $n-1$ comparisons decreases (increases) if – given a base-income distribution $\{y_{i0}\}_{i=1}^n$ – income equality increases, i.e., $\sum_{j\neq i}(t_i-t_j) > 0$ if i has less base income than the average villager (excluding i), or $\sum_{j\neq i}(t_i-t_j) < 0$ if i has more base income than the average vil-*

lager (excluding i). The magnitude of the (negative or positive) relative income effect increases in the total number of villagers n.

So far, we have characterized additional income as a certain outcome of effort choice. In the context of the Tsimane', however, it seems more appropriate to model the villagers' decision problem as the allocation of time e_i across two activities, namely foraging-farming and studying Spanish. As noted in Saidi (2012), studying Spanish and subsequently working for a colonist farmer, cattle rancher, or other outsiders pays more than the sale of forest and farm goods, but is also riskier, as the empirical likelihood of zero income is lower (by more than 15%) for foraging-farming than for wages upon schooling. This can be interpreted as indicating that human capital investment in the form of Spanish improvement pays a high wage X^H with some probability p and 0 otherwise, whereas foraging-farming is relatively riskless and, thus, pays a steady income $X^L < pX^H$. The introduction of uncertainty affects the equilibrium level of e_i due to comparison-concave utility. The next proposition, with its corresponding proof in the Appendix, states that the results from Proposition 1 go through even in the presence of uncertainty over wage-labor income.

Proposition 2 *With uncertainty over wage-labor income, the equilibrium allocation e_i^* of villager i with comparison-concave utility derived from $n-1$ comparisons decreases (increases) if income equality increases and i has less (more) base income than the average villager (excluding i), but e_i^* is lower than in the case without uncertainty (Proposition 1). The magnitude of the (negative or positive) relative income effect increases in the total number of villagers n, and as long as $X^L > \frac{n}{n-1}\Delta X$, both the magnitude of the relative income effect and its sensitivity to n are dampened compared to the case without uncertainty.*

We mention one more comparative static, namely the sensitivity of the relative income effects with respect to ex-ante income inequality. For reasons of tractability, we chose the functional form $v(x) = \alpha x - \beta x^2$. However, for a general concave function $v(\cdot)$ with $v'' < 0$, we can infer from the proofs of the above propositions that the relative income effect $\frac{\partial e_i^*}{\partial\left[\frac{1}{n-1}\sum_{j\neq i}((y_{i0}+t_i)-(y_{j0}+t_j))\right]}$ would also depend on the shape of the base-income distribution which, conditional on villager i's position in it, is characterized by $\frac{1}{n-1}\sum_{j\neq i}(y_{i0} - y_{j0})$. In this manner, the shape of the ex-ante (i.e., before our transfers) income distribution can be related to the magnitude of the relative income effects.

First, if i is a member of a household among the poorest 20%, then $\frac{1}{n-1}\sum_{j\neq i}(y_{i0} - y_{j0})$ is assumed to be small (negative) and the uniform reduction in income inequality by a fixed amount – as under Treatment 2 – generates the largest (negative) income effect for a distribution that minimizes $\sum_{j\neq i}(y_{i0} - y_{j0}) < 0$, i.e., a relatively unequal income distribution. Second, if i is a member of a household among the remaining 80%, then $\frac{1}{n-1}\sum_{j\neq i}(y_{i0} - y_{j0})$ is assumed to be large (positive) and the largest (positive) income effect under Treatment 2 will be observed for a relatively equal ex-ante income distribution, namely one with the smallest possible sum $\sum_{j\neq i}(y_{i0} - y_{j0}) > 0$. To test these comparative statics, we can exploit the village-level variation in our experiment.

Finally, note that while we have also modeled a positive absolute income effect (which is relevant for all treatments) through the cost function $\frac{c}{\delta(y_{i0}+t_i)}$, the primary purpose of our model is to derive hypotheses for the impact of reduced income inequality in Treatment 2, resulting in a relative inome effect. The main points of our theoretical discussion can be summarized as follows:

Implication 1 *Increasing income equality through asymmetric transfers leads to a negative (positive) relative income effect for the poorest 20% (richest 80%) of households in Treatment 2.*

Implication 2 *The negative (positive) relative income effect for the poorest 20% (richest 80%) is stronger in villages with lower (higher) ex-ante income equality.*

In the empirical analysis, we use the above-noted comparative statics to test for the presence of comparison-concave utility by estimating a potential relative income effect under Treatment 2 on variables that are associated with individual effort, and other investments that increase future income.

4 Results

4.1 Relationship between Treatments and Inequality: Descriptive Statistics

As our aim was to vary income distribution across villages and to measure its impact on individual-level outcome variables (e.g., human capital investment), we first have to clarify how our treatments correspond to different states of income (in)equality. To establish whether our treatments affected inequality in the first place, we compute the coefficient of variation in consumption per household member, where consumption is defined as the monetary value of one week's total consumption of game, fish, eggs, maize, manioc, rice, oil, and bread, and consider the difference before and after each treatment at the village level. Indeed, inequality in consumption was significantly reduced only under Treatment 2. The respective drop in the coefficient of variation also reflects the economic importance of the transfers to the bottom 20%.

Coefficient of variation in consumption per household member before and after treatment

	Treatment 1	Treatment 2	Control group
Before	0.63	0.91	0.69
After	0.78	0.63	0.70
p-value	0.164	0.003	0.919
No. of villages	13	13	14

In Tables 1 and 2, we present the summary statistics for the entire adult population (including attriters) in the pre-treatment year by treatment and income quantile, respectively. The treatment groups are similar along many dimensions. Table 2 helps characterize the income quantiles. While we have defined the bottom 20% and top 80% groups by their total area of cleared forest, it appears that the bottom 20% of the respective distribution in the pre-treatment year are, if anything, better at reading and speaking Spanish. They do not consume significantly less (and, thus, share similar health characteristics with the top 80%). Yet, as hypothesized in Section 2.2.1, they earn less in terms of income from the sale of forest and farm goods. While the foraging-farming income gap between the bottom 20% and the top 80% is not significant in Table 2, the foraging-farming income gaps between the bottom 10% and the top 90%, the top 80%, and the bottom 10-60% are all significant at least at the 6% level. Spearman's rank correlation coefficient between household-level foraging-farming income and area of cleared forest is 0.239 (significant at the 1% level) or 0.264 when considering the average village-level correlation coefficient. In contrast, note that wage-labor income is not significantly different between the two groups (and does not become significant for the bottom 10% compared to the top 90%). That is, any Spanish-fluency gaps do not translate into wage-income gaps. While our proxy for income correlates with what constitutes the bulk of people's income, namely foraging and farming, the relative importance of human capital, our primary outcome measure, is similar for both income groups. Lastly, the bottom 20% have fewer assets and borrow much more money than the top 80%. Thus, our income proxy, area of cleared forest, also captures some wealth characteristics.

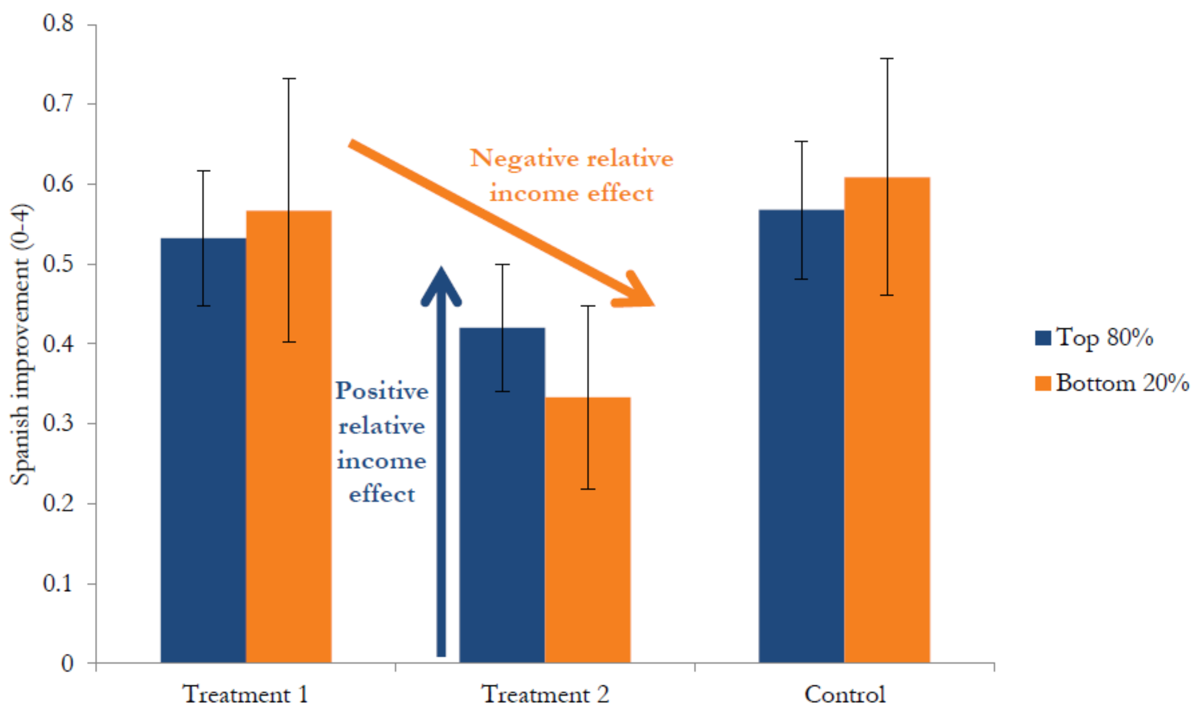

Figure 2: Average Spanish-rating (reading and speaking, 0-4) improvement by treatment with 95% confidence intervals

Before discussing the results, we preview the treatment effects on Spanish fluency, as it enables entry to the labor market and helps sell forest and farm goods to outsiders. Figure 2 plots the change in the sum of the rated reading and speaking abilities (which are each valued between 0 and 2, cf. Tables 1 and 2) between 2008 and 2009 by treatment.

We make the following observations. Most noticeably, the positive income effect derived from edible-rice and rice-seed transfers in Treatment 1 and the control treatment is stable across all villagers. Given the magnitude of the positive income effect in the control group, it appears that the fact that rice seeds are deferred rather than immediate benefits did not differentially affect the short-run income effect. It is useful to compare the changes in Spanish fluency in our experiment with the observational data from our panel study in 40 different Tsimane' villages: there the average Spanish-rating (reading and speaking, from 0 to 4) improvement over the course of one year is 0.167 from 2003 to 2006, and is 0.155 in the most recent year (2006). Thus, our effects in Treatment 1 and the control treatment are sizable.

On the other hand, Spanish improvement rates are lower in Treatment 2. The reason for this is twofold, and reflects the alleged underlying mechanism of social comparison. First, the bottom 20% exhibit a negative relative income effect, as they derive utility from the reduction in income inequality, leading them to invest less in future-revenue-increasing activities such as studying Spanish. A simple alternative explanation would be that leisure could be a normal good, so that given the higher transfer amounts in Treatment 2 compared to Treatment 1, the bottom 20% optimally study less. To rule out this possibility, one can exploit the overlap in transfer amounts between Treatments 1 and 2 (cf. discussion in Section 2.2.1) by focusing on smaller villages in Treatment 1, where each household would receive relatively more edible rice. In this manner, one makes the two treatments more comparable in terms of transfers accruing to the bottom 20%. It turns out that the gap in human capital investment is not reduced and, if anything, widens once we compare the bottom 20% in smaller villages under Treatment 1 to the bottom 20% under Treatement 2. For example, if one considers only the 50% (30%) highest transfer amounts in Treatment 1, then the increase in total Spanish fluency (i.e., reading and speaking, as in Figure 2) becomes 0.67 (0.83) in Treatment 1 compared to 0.30 (0.30) in Treatment 2 (the differences are significant at least at the 2% level). Therefore, it is unlikely that the negative relative income effect on the part of the bottom 20% is simply a reaction to the higher transfer amounts in Treatment 2 in conjunction with leisure being a normal good.

Second, the top 80% improved their Spanish more than the bottom 20% in Treatment 2, despite having received even less rice seeds per person than in the control group (due to varying household sizes). This positive income effect could be explained by a preference for rank preservation of the top 80%, induced by comparison-concave utility, but could also be an ordinary income effect (stemming from the rice-seed transfers) as the one in Treatment 1 and the control group. Our empirical analysis in Section 4.3 and, in particular, our robustness checks in Section 4.4 disentangle the two explanations, and show that the positive effect can be interpreted as a relative income effect due to social comparisons.

4.2 Baseline Determinants of Income

In the main analysis, we will estimate the impact of the rice transfers on various individual outcomes, most notably human capital investment in the form of Spanish fluency and asset holdings. We now demonstrate that these measures are important determinants of personal income, so that we can interpret changes in these measures as a response to our treatments in terms of changes in expected future income. Using the baseline survey in 2008, we first estimate the impact of individual wealth in modern and traditional assets, as well as rated Spanish-speaking and Spanish-reading abilities on total income, i.e., the sum of earnings from the sale of forest and farm goods, wage labor, and barter. While traditional assets are physical assets made from local materials, modern assets comprise physical assets acquired in the market, which, besides their intrinsic asset value, indicate increased interaction with outsiders and the market economy evolving around the Tsimane'. Similarly, we expect, in particular, spoken Spanish fluency to be associated with labor market success and, thus, higher income.

The results in Table 3 support these hypotheses. Increasing one's spoken Spanish fluency by one category leads to an increase in one week's total income well above 25 bolivianos (the respective effect is still significant at the 11% level in the third column). The effect of Spanish-reading ability, albeit positive, is imprecisely estimated while controlling for spoken Spanish fluency, which attests to the fact that spoken Spanish is the more important skill in the labor market. While wealth in traditional assets has virtually no impact on earnings, we find a significantly positive impact of modern-asset wealth. The effects of both Spanish fluency and modern-asset wealth become insignificant only after including household fixed effects as well as individual characteristics. Comparing the third and fifth columns with the remainder of Table 3, males and household heads speak better Spanish and possess more modern assets (the respective mean differences are significant at the 1% level), which explains the drop in the respective coefficients after controlling for gender.

As wage labor is the most lucrative source of income, we also estimate the impact of the above-mentioned variables on whether an individual villager earns any money in the outside labor market. The estimates in Table 4 are in line with the ones in Table 3: spoken Spanish fluency and wealth in modern assets are important determinants of entry into wage labor. For instance, improving one's spoken Spanish ability by one category increases the likelihood of landing employment by up to 10%. Again, there is a strong impact of gender in the cross-section, which is, however, not relevant for our conclusion that under our experimental treatments, increases in Spanish fluency and in modern rather than traditional assets should increase income.

4.3 Effects of Rice Transfers

We now turn to the estimation of the effects of the different rice transfers on individual outcomes. We start by discussing the regression specification for our analysis.

Fixed-effects regression specification. The main regression specification is, unless otherwise noted, constant across the remainder of the tables. We restrict the sample to non-attriters, and always include individual fixed effects to control for unobservable heterogeneity. Whenever we do not use treatment indicator variables, we replace them by transfer-amount-dependent variables. $Rice_{it}$ and $Seeds_{it}$ are measures of how much rice an individual i received in treatment t, i.e., the household allocation of edible rice (under Treatment 1 and for the bottom 20% under Treatment 2) and rice seeds (for the top 80 % under Treatment 2 and for the control group), respectively, divided by the number of household members in 2008. The variables are equal to 0 in 2008, and depend on the treatment condition in 2009. Note that for Treatment 1, Treatment 2, and the control group there are per-capita variations because of differences in household demographics. In addition, for Treatment 1 and for the bottom 20% under Treatment 2 there are cross-village variations because of different numbers of households (while the aggregate amount of edible rice received by each of the 26 villages is constant). We will discuss potential biases of our estimates that may result from this.

To capture effects of relative income and social comparison, we define $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ to be non-zero only for villagers under Treatment 2, where $\overline{Rice}_{vt}$ denotes the average amount of edible rice per household member of all other villagers who are not in i's treatment group. That is, if i belongs to a treated household (bottom 20%) under Treatment 2, then $\left|Rice_{it} - \overline{Rice}_{vt}\right| = Rice_{it}$. If i does not belong to a treated household (top 80%) under Treatment 2, then $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ is equal to the average amount of edible rice received by individuals from households belonging to the bottom 20%. Hence, for the bottom 20% and the top 80%, the amount of edible rice received under Treatment 1 is captured by the coefficient on $Rice_{it}$, whereas for the bottom 20%, the effect of rice received under Treatment 2 is captured by the sum of the coefficients on $Rice_{it}$ and $\left|Rice_{it} - \overline{Rice}_{vt}\right|$. Thus, the effect for the bottom 20% is separated into a pure income and a relative income effect. For the top 80%, the impact of rice received by the bottom 20% under Treatment 2 is captured solely by the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ (because the top 80% do not receive any edible rice under Treatment 2).

Lastly, we disentangle the effect of the rice-seed transfers accruing to the top 80% under Treatment 2 from that in the control treatment by including the respective interaction effects with $Seeds_{it}$, the coefficients of which will naturally be much larger than for the edible-rice variables given the fixed amount of 5.9 kg of rice seeds per household.

Human capital investment. The regressions in Tables 5 and 6 estimate the impact of the rice transfers under the different treatments on the sum of rated Spanish-speaking and Spanish-reading abilities between 2008 and 2009. To control for study predispositions not directly related to Spanish knowledge, we also include the lagged math score (0-4) on the right-hand side. In Table 5, we estimate the average treatment effect in the first and third columns. While for the top 80% (in the third column) all three treatments had positive impacts, which are not statistically different from one another, the bottom 20% (in the first column) improved their Spanish significantly more under Treatment 1 and the control treatment than under Treatment 2 (both differences are significant at the 6% level). We interpret the findings for the bottom 20% as indicating that greater income equality in Treatment 2 led to less human capital investment compared to the other treatments where the income distribution remained stable. Accordingly, we would interpret the positive effect for the top 80% in Treatment 2 as a reaction to the increased income equality stemming from social comparisons. However, note that the top 80% also received rice seeds as a consolation prize which, as evidenced by the positive effect of the control treatment on Spanish fluency, might also have led to human capital investment under Treatment 2. To disentangle the two explanations – social comparisons and the positive income effect due to the deferred benefit from planting the rice seeds – we run the regressions with rice-amount-dependent explanatory variables in the second and fourth columns of Table 5.

Again, the estimates indicate that all villagers invested in Spanish upon receiving rice under Treatment 1, and this also holds true for the rice-seed transfers accruing to the control group. However, this effect is almost eradicated under Treatment 2 for the bottom 20% (e.g., in the second column of Table 5, the sum of the coefficients on $Rice_{it}$ and $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ is 0.065, compared to a coefficient of 0.298 on $Rice_{it}$. While the negative relative income effect might be overstated because it partly reflects the fact that the bottom 20%, on average, received more rice under Treatment 2, the absolute magnitude of the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ clearly exceeds the fraction of the effect under Treatment 1 (namely $\approx 0.298 - \frac{0.298}{2.7} = 0.188$) that would simply adjust for differences in rice-transfer amounts (cf. second row of Table 1). This affirms the average village-level finding in the first column, namely that the bottom 20% in villages under Treatment 2 invested less in Spanish than in any other treatment.

Conversely, and in line with Implication 1 (cf. Section 3), the top 80% improved their Spanish competence even when they received considerably less than the bottom 20%, i.e., when the rice transfer was associated with greater income equality, and this positive effect is primarily due to the comparison effect captured by $\left|Rice_{it} - \overline{Rice}_{vt}\right|$, and not due to the rice-seed consolation prize. Yet, this positive relative income effect did not outweigh the negative relative income effect for the bottom 20%, so the total effect under Treatment 2 yielded less investment in human capital than under Treatment 1.

More than that, we also find a robust positive effect of the rice-seed transfers in the control treatment on Spanish fluency. Given that individuals in the control group received a bit less than 1 kg of rice seeds, the coefficient is rather large. However, as planting 1 kg of rice seeds eventually yields an edible-rice amount that clearly exceeds 1 kg, we interpret the qualitative equivalence of the effects of transferring edible rice in Treatment 1 and rice seeds as evidence that the positive income effect derived from deferred benefits (rice seeds) may be just as large as the one derived from immediate benefits (edible rice).

Note that the edible-rice amounts received by the households in Treatments 1 and 2 are, by design, endogenous to household and, more importantly, village sizes. To assess in what way having the village sizes enter the treatment variable biases the estimates, we estimate the relationship between human capital investment and village size. It turns out that village size (in 2008) is significantly positively correlated with changes in rated Spanish ability. How does this bias our estimates? For Spanish improvement, we find that the rice transfers in Treatment 1 had a positive impact. Thus, the effect of $Rice_{it}$ is probably underestimated – there is, in general, more human capital investment in larger villages, so for small values of $Rice_{it}$ we find upward-biased effects on human capital investment, thereby dampening the overall positive coefficient on $Rice_{it}$. In particular, this implies that the positive relative income effect we find for the top 80% in Treatment 2 is likely an underestimate, too. The negative relative income effect for the bottom 20% in Treatment 2 should be unaffected by this kind of endogeneity, as long as village size did not differentially affect the impact of edible-rice transfers on human capital investment under Treatment 1 and Treatment 2.

Table 6 conducts the comparative-statics exercise laid out in Implication 2 (in Section 3). We test whether the negative impact of increased income equality (as captured by the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$) for the bottom 20% is conditional on an unequal ex-ante distribution, whereas the positive impact of increased income equality for the top 80% is stronger in villages with more equal ex-ante distributions. We find evidence of the latter effect for the top 80% irrespective of whether we use the initial equality of the consumption or the income distribution (cf. third vs. fourth column in both panels of Table 6). For the bottom 20%, the impact of rice transfers under Treatment 2 is more negative only when conditioning on less income-distribution equality in the pre-treatment year (cf. second vs. first column of the bottom panel of Table 6, whereas the sum of the coefficients on $Rice_{it}$ and $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ is not significantly different from zero only in the first but not in the second column of the top panel of Table 6).

Asset holdings. Tables 7 to 10 use the same logic as above to show that people in the top 80% of the village income distribution shifted their physical capital from traditional to modern assets under Treatment 2, while the bottom 20% did not increase either asset base. More specifically, the last column of Table 7 demonstrates that the top 80% increased their wealth in modern assets as a response to the edible-rice transfers accruing to the bottom 20%

in Treatment 2.[2] In addition, Table 8 suggests that this positive impact is more pronounced if the initial consumption or income distribution was more equal (cf. third vs. fourth column in both panels of Table 8) – in line with our prediction for a relative income effect due to social comparisons. The opposite holds true qualitatively for the traditional-asset holdings of the top 80% (cf. third column of Table 9). However, it appears that effect is primarily driven by the rice-seed transfers accruing to the top 80%, rather than by social comparisons stemming from the edible-rice transfers to the bottom 20%. On the other hand, the bottom 20% increased their traditional-asset holdings when given the rice seeds under the control treatment (see, for instance, first two columns of Table 9).

Furthermore, the positive impact of Treatment 1 on the stock of modern assets in both groups (cf. first and third columns of Table 7) partly explains – in conjunction with the before-mentioned complementarity between modern-asset wealth and Spanish skills – the magnitude of the effect of our transfers on Spanish fluency in less than one year. Namely, while Spanish fluency enables interaction with the markets outside the villages and, thus, facilitates the acquisition of modern assets, the latter positively feed back to the Spanish-learning process, e.g., radios transmit educational programs, and generally increase exposure to Spanish.

Migration. So far, we have gathered evidence that rice transfers associated with a reduction in income inequality (under Treatment 2) induced the bottom 20%, who were the primary beneficiaries of the transfers, neither to alter their asset base nor to improve their Spanish any more (actually significantly less) than under income-distribution stability (Treatment 1). In contrast, the top 80% improved their Spanish, and shifted their asset portfolios from traditional to modern assets under both treatments, i.e., even in Treatment 2 when they did not receive any edible rice (whereas the bottom 20% did). The effects and the comparative statics that we have found are in line with a theory of social comparison implying that expected income follows a U-shaped curve as a function of inequality, where changes in expected future income are represented by estimated changes in the above variables (cf. Tables 3 and 4).

To further establish that the observed changes in Spanish fluency and the asset base imply increased interaction with non-Tsimane' and the labor market outside the villages, we directly test the impact of the treatments on migration (attrition) patterns. To this end, we run regressions with an indicator for attrition (i.e., for having left the village by the time of the follow-up survey) as dependent variable, because an important (albeit not the unique) reason for leaving is employment in the towns, logging operations, or on cattle ranches. Table 11 presents the results for the bottom 20% and top 80% separately: while the first and third columns use only treatment dummy variables, the second and fourth columns include the rice variables used before to capture amount-dependent effects. For the bottom 20%, we find that villagers were less likely to leave their communities under Treatment 2 than under Treatment 1 (the difference between the two coefficients is significant at the 1% level; also note that the omitted category is the control treatment, because we use only the 2008 data). There are, however, no significant amount-dependent effects in the second column of Table 11. Conversely, the top 80% were more likely to migrate under Treatment 2 than under Treatment 1 (the difference is significant at the 1% level) – but not more so than in the control group, which renders possible that the positive effect on migration in Treatment 2 might be driven by an income effect from the

[2] Similar to the case of human capital investment, the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ for the top 80% is endogenous to village size. Indeed, in regressions not presented in this paper, we find that village size (in 2008) is significantly positively correlated with changes in modern-asset wealth. Following the same logic as for human capital investment, this implies that the positive relative income effect might, if anything, be underestimated.

rice-seed transfers. The fourth column of Table 11, by using the actual rice-transfer amounts, clarifies that the decision to leave the village among the top 80% was primarily due to the relative income effect in Treatment 2 (as captured by the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$), and not due to the rice-seed transfers. In summary, we indeed find migration (attrition) patterns to be consistent with our interpretation that the responses in our experiment have aggregate implications for economic development that are driven by social comparisons.

4.4 Robustness

To complete our analysis, we next discuss the robustness of the interpretation of our findings. In this paper, we argue that the observed behavior under Treatment 2 is due to social comparisons, and present evidence that the magnitudes of the relative income effects across different village distributions are in line with the comparative statics of comparison-concave utility (cf. Section 3). Our identification builds on the idea that particularly for the bottom 20%, the only difference, after controlling for the amount received, between Treatment 1 and Treatment 2 lies in the distribution of the edible-rice shock. Given that the villages in both treatments received the same total amount of edible rice, we can rule out that our findings are driven by any differential aggregate effects, e.g., price effects, which would not even be relevant for the effect on human capital investment because the price of schooling is zero, as the Bolivian government pays for the teachers.

As our results for human capital investment are central to our conclusions regarding the impact of income distribution on economic growth in the aggregate, our findings in Tables 5 and 6 warrant further discussion. Most noticeably, all villagers improved their Spanish upon receipt of the rice seeds in the control treatment. This gives rise to the interpretation that our consolation prize also helped generate a positive income effect, at least with respect to human capital investment. As planting 5.9 kg of rice seeds (per household) yields much more edible rice within four to five months (cf. discussion in Section 2.2.2), this could explain our finding, and would also suggest that the villagers' revealed time preferences do not imply a high discount rate. However, if this is true, then we have a competing explanation for the positive relative income effect for the top 80% in Treatment 2, as they also received rice seeds as a consolation prize. Yet, the regressions for the top 80% in Tables 5 and 6 control for the amount of rice seeds received in Treatment 2, and we still yield a sizable relative income effect, which stems from the edible-rice transfers accruing to the bottom 20% (as reflected by the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$).

To provide further evidence that the positive relative income effect for the top 80% is not an artifact of a positive absolute income effect derived from the rice-seed transfers, we decompose the latter effect. The competing explanation implies that the edible-rice transfers to the bottom 20% immediately reduce income inequality, but that effect might be offset by the fact that after planting the rice seeds, the top 80% would also have more rice, which renders Treatment 2 qualitatively similar to the other treatments by neutralizing the perceived impact on the income distribution. As the rice-seed transfers are deferred benefits, the villagers who derive a positive income effect from them are more likely to be patient, i.e., they have a relatively low discount rate, as witnessed in the control group. However, if the discount rate of the top 80% is low, then the deferred benefit might actually be more relevant than the comparison with the immediate benefit accruing to the bottom 20%, thus dampening (or even eradicating) the inequality-reducing impact of Treatment 2.

In Table 12, we re-run the regressions for human capital investment, interact $Rice_{it}$ with measures that reflect the villagers' actually experiencing the deferred benefits, and show that the relative income effect captured by $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ is robust. To approximate discount rates, we use a revealed-preference argument: we have data on the proportion of the rice-seed transfers that were actually planted by households ($Planted \in [0, 1]$),[3] interact this variable with $Seeds_{it}$ × Treatment 2, and hypothesize the triple interaction effect to be positive. In the first column of Table 12, the interaction effect is indeed positive, albeit insignificant, but we maintain the economic and statistical significance of the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$. In the second column, we extend the interaction to villagers in the top 40%, as approximated by their area of cleared forest, to account for the possibility that the success of planting rice seeds may also depend on such resources as the forest area available. The respective interaction effect in the second column is negative and not significant at conventional levels.

How can we interpret the persistence of the (positive) relative income effect for the top 80%? In the absence of income-shock inequality (i.e., in the control group), the deferred benefit of planting rice seeds indeed leads to human capital investment, but this positive income effect is outweighed by the salient social comparison in Treatment 2, so that the potentially positive impact of the rice-seed transfers is dominated by the relative income effect (although both would have the same, positive direction). That is, the deferred benefit becomes even less attractive or ultimately irrelevant to the top 80% because of the immediate reduction in income inequality through the edible-rice transfers. While such a perspective is certainly subject to further debate and research, it exemplifies potential interactions between different behavioral effects, here: between social comparisons and hyperbolic discounting.

As another robustness check, we note that as the decision to plant the rice seeds reflects a lower discount rate, the latter might also be correlated with other, unobserved characteristics driving the estimates of the interaction effects with $Seeds_{it}$ × Treatment 2. We used the planting decision as an indicator for actually experiencing the income (rice) benefits at a later point in time, rather than selling the rice seeds immediately. To capture the full experience of the deferred benefit without the endogeneity due to lower discount rates, we re-run the regressions from the first two columns of Table 12 on the subsample of household members in the top 80% who are at most 20 years old, as they are least likely to have taken the decision to plant the rice seeds but are still exposed to the consequences. The results in the last two columns of Table 12 are very similar, and reveal an even stronger relative income effect. The findings are robust to using a higher age cut-off, e.g., 25 years. Across all four estimations, the coefficient on $\left|Rice_{it} - \overline{Rice}_{vt}\right|$ is at least significant at the 3% level. To summarize, although both treatments involve rice-seed transfers, the income effect for the top 80% in Treatment 2 is not analogous to the one in the control group and, thus, can be considered a relative income effect due to social comparisons.

Having gained further validation for the existence of comparison-concave utility, we consider one more potentially confounding explanation in Table 13. We argued that the reduction in income inequality under Treatment 2 motivates the top 80% to improve their Spanish because they have a preference for rank preservation, induced by comparison-concave utility. While we have seen above that this effect is unlikely to be explained by the absolute income effect stemming from the rice-seed transfers, there exists another explanation that would render the income effect for the top 80% in Treatment 2 equivalent to the one in Treatment 1 and in the control group. As shown in Saidi (2012), the Tsimane' engage in risk sharing. Thus, our

[3] Unfortunately, we do not have this information for all households.

inequality-reducing intervention in Treatment 2 might have led to a reduction in risk-sharing obligations on the part of the top 80%, freeing up resources that could be used for other, income-enhancing activities. This would constitute an absolute income effect as observed under Treatment 1 and the control treatment, and would be independent of social comparisons. To test whether the risk-sharing obligations of the top 80% were reduced as a consequence of our Treatment 2, we analyze changes in household consumption in the first two columns of Table 13. Although one would have hypothesized that the top 80% consumed relatively more under Treatment 2, we find no effect at all, and the differences between all three treatment groups in the first column are not significant.

Additionally, Saidi (2012) provides evidence that the Tsimane' borrow and lend money among themselves, and that the form of repayment resembles an equity-like or insurance contract, which contributes to the overall level of risk sharing in the villages. Against this background, we estimate the treatment effects on the total amount of loans given out by individuals in the last two columns of Table 13. Again, we find no evidence that risk-sharing obligations were differentially impacted by our interventions (the differences between all three treatment groups in the third column are not significant). In regressions unreported in this paper, these (non-)findings also extend to gift-giving behavior of the top 80%. Despite considering multiple potential channels for risk sharing, we have failed to find evidence that the relative income effect for the top 80% was not due to social comparisons.

5 Concluding Remarks

There are many claims based on associations that social comparisons of relative income affect subjective well-being (e.g., Clark and Oswald (1996), Easterlin (2001), Ferrer-i Carbonell (2005), and Luttmer (2005)). In our field experiment, we test these claims by exogenously varying the distribution of income shocks in 40 villages of a foraging-farming society, the Tsimane' of the Bolivian Amazon. We measure the impacts on individual-level outcomes, namely human capital investment (in the form of studying Spanish) and the composition of asset portfolios, and find evidence that is suggestive of social comparisons driving our results.

From an identification point of view, the Tsimane' society is a particularly well-suited setting for our endeavor because it facilitates the measurement of growth-enhancing activities by defining the latter as the substitution of time spent foraging-farming by time spent on human capital investment, which is the entry ticket to the outside labor market. Our experimental findings attest to the extent to which social-comparison dynamics elucidate such investment decisions even among normatively egalitarian villagers. Besides encouraging similar studies in other rural societies, these observations give rise to a general theory – akin in spirit to Cole, Mailath, and Postlewaite (1992) – of how processes of social comparison form a channel through which inequality of various kinds translates into growth.

Finally, our paper contributes to the debate on labor-supply effects and growth implications of income redistribution, and suggests that simple income transfers to all households may be more conducive to encourage human capital investment among the poor than redistributional policies. This way, our paper lines up with, and complements, analyses based on observational data in different contexts, such as Chetty, Friedman, and Saez (2012) who analyze the Earned Income Tax Credit (EITC) in the US, and estimate the impact of this inequality-reducing intervention on individual earnings.

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6 Omitted Proofs

Proof of Proposition 1: First, rewrite the first-order condition (2) as follows:

$$\begin{aligned}&\Delta X\left[\theta+(1-\theta)\left(\alpha-2\beta\frac{n}{n-1}\left(y_{i0}+t_i+e_i\Delta X\right)+\frac{2\beta}{n-1}\sum_{j=1}^{n}\left(y_{j0}+t_j+e_j\Delta X\right)\right)\right]\\ =\ &\frac{c}{\delta\left(y_{i0}+t_i\right)}e_i.\end{aligned} \tag{3}$$

Summation yields

$$\sum_{j=1}^{n}e_j=\frac{n\Delta X\left(\theta+(1-\theta)\alpha\right)}{\frac{c}{\delta(y_{i0}+t_i)}},$$

which can be reinserted in (3), so that:

$$e_i^*=\frac{\Delta X\left[\theta+(1-\theta)\left(\begin{array}{c}\alpha+\frac{2\beta n(\Delta X)^2}{(n-1)\frac{c}{\delta(y_{i0}+t_i)}}\left(\theta+(1-\theta)\alpha\right)\\ -\frac{2\beta}{n-1}\sum\limits_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right)\end{array}\right)\right]}{\frac{c}{\delta(y_{i0}+t_i)}+(1-\theta)2\beta\frac{n}{n-1}\left(\Delta X\right)^2}.$$

This implies:

$$\frac{\partial e_i^*}{\partial\left[\frac{1}{n-1}\sum\limits_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right)\right]}=-\frac{\Delta X(1-\theta)2\beta}{\frac{c}{\delta(y_{i0}+t_i)}+(1-\theta)2\beta\frac{n}{n-1}\left(\Delta X\right)^2}<0.$$

That is, given a base-income distribution $\{y_{i0}\}_{i=1}^{n}$, a reduction in income inequality by means of transfers s.t. $\sum\limits_{j\neq i}(t_i-t_j)>0$ if i is among the poorest villagers $\left(\frac{1}{n-1}\sum\limits_{j\neq i}(y_{i0}-y_{j0})<0\right)$ and $\sum\limits_{j\neq i}(t_i-t_j)<0$ if i is among the wealthiest villagers $\left(\frac{1}{n-1}\sum\limits_{j\neq i}(y_{i0}-y_{j0})>0\right)$ leads to, respectively, a reduction and an increase in equilibrium effort.

Finally, we have that:

$$\frac{\partial^2 e_i^*}{\partial\left[\frac{1}{n-1}\sum\limits_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right)\right]\partial n}=-\Delta X\left(\frac{\Delta X(1-\theta)2\beta}{(n-1)\left(\frac{c}{\delta(y_{i0}+t_i)}+(1-\theta)2\beta\frac{n}{n-1}\left(\Delta X\right)^2\right)}\right)^2<0,$$

so the aforementioned effects are amplified for higher values of n. ■

Proof of Proposition 2: The first-order condition is:

$$\begin{aligned}&\Delta\widetilde{X}\left[\theta+(1-\theta)\left(\alpha-2\beta\frac{n}{n-1}\left(y_{i0}+t_i+e_i\Delta X\right)+\frac{2\beta}{n-1}\sum_{j=1}^{n}\left(y_{j0}+t_j+e_j\Delta X\right)\right)\right]\\ =\ &\left(\frac{c}{\delta\left(y_{i0}+t_i\right)}+(1-\theta)2\beta(1-p)X^HX^L\right)e_i,\end{aligned} \tag{4}$$

where $\Delta\widetilde{X} \equiv pX^H - X^L$.

Summing up yields:

$$\sum_{j=1}^{n} e_j = \frac{n\Delta\widetilde{X}\left(\theta + (1-\theta)\alpha\right)}{\frac{c}{\delta(y_{i0}+t_i)} + (1-\theta)2\beta(1-p)X^H X^L},$$

which can be reinserted in (4), so that:

$$e_i^* = \frac{\Delta\widetilde{X}\left[\theta + (1-\theta)\left(\begin{array}{c} \alpha + \frac{2\beta n\Delta X\Delta\widetilde{X}(\theta+(1-\theta)\alpha)}{(n-1)\left(\frac{c}{\delta(y_{i0}+t_i)}+(1-\theta)2\beta(1-p)X^H X^L\right)} \\ -\frac{2\beta}{n-1}\sum_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right) \end{array}\right)\right]}{\frac{c}{\delta(y_{i0}+t_i)} + (1-\theta)2\beta\left((1-p)X^H X^L + \frac{n}{n-1}\Delta X\Delta\widetilde{X}\right)},$$

which is less than the equilibrium choice e_i^* in Proposition 1 because $\Delta\widetilde{X} < \Delta X$.

Furthermore:

$$\frac{\partial e_i^*}{\partial\left[\frac{1}{n-1}\sum_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right)\right]} = -\frac{\Delta\widetilde{X}(1-\theta)2\beta}{\frac{c}{\delta(y_{i0}+t_i)} + (1-\theta)2\beta\left((1-p)X^H X^L + \frac{n}{n-1}\Delta X\Delta\widetilde{X}\right)} < 0.$$

Finally:

$$\begin{aligned} & \frac{\partial^2 e_i^*}{\partial\left[\frac{1}{n-1}\sum_{j\neq i}\left((y_{i0}+t_i)-(y_{j0}+t_j)\right)\right]\partial n} \\ = \quad & -\Delta X\left(\frac{\Delta\widetilde{X}(1-\theta)2\beta}{(n-1)\left(\frac{c}{\delta(y_{i0}+t_i)} + (1-\theta)2\beta\left((1-p)X^H X^L + \frac{n}{n-1}\Delta X\Delta\widetilde{X}\right)\right)}\right)^2 < 0. \end{aligned}$$

Both derivatives are less negative than the corresponding sensitivities in Proposition 1 if $(1-p)X^H X^L + \frac{n}{n-1}\Delta X\Delta\widetilde{X} > \frac{n}{n-1}(\Delta X)^2 \iff X^L > \frac{n}{n-1}\Delta X$. ■

7 Tables

Table 1: **Summary Statistics by Treatment (in Pre-treatment Year 2008)**

Variable	Treatment 1 Mean (Std. dev.)	Treatment 1 N	Treatment 2 Mean (Std. dev.)	Treatment 2 N	Control group Mean (Std. dev.)	Control group N
Households per village	13.154	13	15.385	13	13.714	14
	(5.38)		(6.08)		(7.50)	
Edible rice in kg per treated household	52.970	13	143.630	13	/	14
	(23.71)		(74.32)		/	
Cleared forest (in ha) per household	1.271	169	1.124	199	1.139	191
	(1.19)		(0.82)		(0.84)	
Currently in school	0.349	982	0.384	1,106	0.416	1,113
	(0.48)		(0.49)		(0.49)	
Spanish speaking (0-2)	0.992	918	0.698	1,005	0.777	972
	(0.83)		(0.74)		(0.74)	
Spanish reading (0-2)	0.515	833	0.385	938	0.488	886
	(0.77)		(0.66)		(0.74)	
Math score (0-4)	1.080	836	0.820	940	1.027	882
	(1.53)		(1.33)		(1.44)	
Consumption per household	245.074	171	243.247	199	275.556	192
	(168.31)		(200.80)		(189.11)	
Total income (in 1 week)	166.400	405	150.483	473	139.749	474
	(441.70)		(424.78)		(257.93)	
Income from sale of goods (in 1 week)	110.143	407	112.128	480	95.663	476
	(407.33)		(394.44)		(230.07)	
Income from wage labor (in 1 week)	41.029	407	19.578	480	27.857	477
	(135.90)		(66.77)		(75.74)	
Total assets (in bolivianos)	1490.550	409	1564.218	483	1858.504	479
	(1953.00)		(1956.79)		(2350.97)	
Modern assets (in bolivianos)	842.060	409	1017.981	483	1269.052	479
	(890.10)		(1405.18)		(1485.99)	
Traditional assets (in bolivianos)	309.493	409	377.852	483	347.490	479
	(560.52)		(581.10)		(512.41)	
Credit (in bolivianos in a week)	19.993	407	9.436	479	23.270	474
	(147.77)		(77.40)		(324.28)	
Pulse rate (in bpm)	75.754	372	75.249	448	75.809	436
	(10.14)		(10.44)		(10.96)	
Diastolic blood pressure (in mmHg)	70.762	373	68.326	448	70.274	436
	(8.29)		(8.47)		(8.17)	
Systolic blood pressure (in mmHg)	114.402	373	113.355	448	115.225	436
	(11.77)		(12.10)		(11.51)	
BMI (in kg/m^2)	20.284	865	19.937	1,002	19.997	1,004
	(4.33)		(4.04)		(4.00)	
Age	18.456	1,047	17.506	1,204	18.669	1,198
	(17.54)		(17.32)		(18.53)	

Notes (Tables 1 and 2): Household consumption corresponds to one week's consumption of game, fish, eggs, maize, manioc, rice, oil, and bread (in bolivianos). Total income is equal to earnings from the sale of goods, wage labor, and barter.

Table 2: **Summary Statistics by Income Quintile (in Pre-treatment Year 2008)**

Variable	Bottom 20% Mean (Std. dev.)	N	Top 80% Mean (Std. dev.)	N	p-value (two-sided)
Cleared forest (in ha) per household	0.534 (0.58)	161	1.433 (0.95)	398	0.00
# of household members	5.509 (2.49)	161	6.364 (2.91)	398	0.00
Currently in school	0.355 (0.48)	828	0.395 (0.49)	2,373	0.04
Spanish speaking (0-2)	0.881 (0.77)	749	0.795 (0.79)	2,146	0.01
Spanish reading (0-2)	0.481 (0.72)	684	0.453 (0.73)	1,973	0.38
Math score (0-4)	0.990 (1.43)	676	0.964 (1.44)	1,982	0.69
Consumption per household	243.356 (159.90)	161	259.452 (197.71)	401	0.36
Total income (in 1 week)	137.483 (333.04)	356	156.494 (396.14)	996	0.42
Income from sale of goods (in 1 week)	88.128 (314.88)	358	112.075 (361.99)	1,005	0.27
Income from wage labor (in 1 week)	31.715 (85.96)	358	27.863 (98.88)	1,006	0.51
Total assets (in bolivianos)	1473.322 (1708.74)	359	1705.982 (2228.05)	1,012	0.07
Modern assets (in bolivianos)	993.914 (1244.90)	359	1074.257 (1339.91)	1,012	0.32
Traditional assets (in bolivianos)	308.628 (477.67)	359	360.410 (575.71)	1,012	0.13
Credit (in bolivianos in a week)	33.254 (382.19)	355	11.823 (98.23)	1,005	0.10
Pulse rate (in bpm)	76.478 (10.56)	329	75.279 (10.51)	927	0.08
Diastolic blood pressure (in mmHg)	69.789 (8.49)	329	69.702 (8.34)	928	0.87
Systolic blood pressure (in mmHg)	114.008 (12.03)	329	114.423 (11.75)	928	0.59
BMI (in kg/m^2)	19.992 (4.08)	727	20.087 (4.13)	2,144	0.59
Age	18.074 (18.29)	887	18.241 (17.65)	2,562	0.81

Table 3: **Determinants of Income (in Pre-treatment Year 2008)**

	Total income (in bolivianos in one week)				
Modern assets	5.975***	6.639***	4.401***	6.207**	2.772
	(1.23)	(1.48)	(1.52)	(2.44)	(2.42)
Traditional assets	0.871	2.279	1.293	3.946	2.037
	(2.50)	(2.64)	(2.73)	(3.51)	(3.74)
Spanish speaking	50.564***	40.079**	27.639	30.308	5.124
	(14.76)	(17.30)	(17.34)	(33.33)	(34.31)
Spanish reading	1.642	14.566	15.186	37.773	35.582
	(17.71)	(19.26)	(24.05)	(28.34)	(40.76)
Male			81.282***		103.701***
			(21.43)		(37.38)
Household head			43.214**		75.020
			(20.38)		(55.27)
Age			0.968		1.258
			(0.81)		(1.54)
Constant	30.392*	-110.911***	-165.092***	4.602	-118.518
	(17.57)	(37.89)	(55.97)	(29.25)	(113.79)
Fixed effects	No	Villages	Villages	Villages, Households	Villages, Households
N	1,308	1,308	1,308	1,308	1,308

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. In the OLS regressions, standard errors are clustered at the household level. Total income is equal to earnings from the sale of goods, wage labor, and barter. Modern and traditional assets are in 100 bolivianos. Spanish speaking and reading skills are measured in three categories (0, 1, and 2), indicating no competence, some knowledge, and a good command of the Spanish language, respectively.

Table 4: **Determinants of Wage Labor as Source of Income (in Pre-treatment Year 2008)**

	Non-zero income from wage labor $\in \{0, 1\}$				
Modern assets	0.003***	0.003***	-0.002	0.005***	-0.002
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Traditional assets	-0.001	-0.000	-0.001	-0.002	-0.003
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Spanish speaking	0.070***	0.084***	0.039*	0.098***	0.023
	(0.02)	(0.02)	(0.02)	(0.04)	(0.03)
Spanish reading	0.018	0.039**	0.002	0.056**	0.002
	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Male			0.252***		0.267***
			(0.03)		(0.04)
Household head			0.016		0.029
			(0.03)		(0.04)
Age			-0.001		0.000
			(0.00)		(0.00)
Constant	0.055***	-0.194***	-0.137***	-0.189***	-0.174**
	(0.02)	(0.03)	(0.04)	(0.03)	(0.07)
Fixed effects	No	Villages	Villages	Villages, Households	Villages, Households
N	1,315	1,315	1,315	1,315	1,315

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. In the OLS regressions, standard errors are clustered at the household level. Modern and traditional assets are in 100 bolivianos. Spanish speaking and reading skills are measured in three categories (0, 1, and 2), indicating no competence, some knowledge, and a good command of the Spanish language, respectively.

Table 5: **Human Capital Outcomes Across Treatments and Income Quantiles**

	Spanish ability in speaking and reading (0-4)			
Treatment 1	0.559***		0.470***	
	(0.11)		(0.06)	
Treatment 2	0.322***		0.371***	
	(0.04)		(0.08)	
Control	0.576***		0.498***	
	(0.13)		(0.07)	
$Rice_{it}$		0.298***		0.358***
		(0.06)		(0.05)
$\left\|Rice_{it} - \overline{Rice}_{vt}\right\|$		-0.233***		0.082**
		(0.07)		(0.04)
$Seeds_{it}$ × Control		4.153***		4.105***
		(1.11)		(0.75)
$Seeds_{it}$ × Treatment 2				0.541
				(1.13)
$Math\ score_{i,t-1}$	0.018	0.086**	0.077**	0.121***
	(0.04)	(0.04)	(0.03)	(0.03)
Constant	1.115***	1.173***	0.958***	1.006***
	(0.03)	(0.02)	(0.02)	(0.02)
Population	BOT20	BOT20	TOP80	TOP80
N	646	646	1,926	1,926

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member. $Math\ score_{i,t-1}$ is i's score (from 0-4) on a math test in the pre-treatment year 2008.

Table 6: **Human Capital Outcomes Across Treatments and Initial Distributions**

	Spanish ability in speaking and reading (0-4)			
$Rice_{it}$	0.432***	0.234***	0.356***	0.363***
	(0.09)	(0.06)	(0.07)	(0.09)
$\lvert Rice_{it} - \overline{Rice}_{vt}\rvert$	-0.327**	-0.184**	0.205*	0.059**
	(0.12)	(0.07)	(0.12)	(0.03)
$Seeds_{it}$ × Control	4.907***	2.833***	4.777***	3.088***
	(1.61)	(0.86)	(1.04)	(0.56)
$Seeds_{it}$ × Treatment 2			-3.031	0.886
			(2.87)	(1.04)
$Math\ score_{i,t-1}$	0.027	0.141**	0.112**	0.130***
	(0.05)	(0.06)	(0.05)	(0.03)
Constant	1.368***	1.000***	1.203***	0.859***
	(0.04)	(0.02)	(0.03)	(0.02)
Consumption-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	294	352	824	1,102

	Spanish ability in speaking and reading (0-4)			
$Rice_{it}$	0.149***	0.389***	0.338***	0.377***
	(0.04)	(0.10)	(0.02)	(0.08)
$\lvert Rice_{it} - \overline{Rice}_{vt}\rvert$	-0.083*	-0.319***	0.104**	0.037
	(0.05)	(0.10)	(0.05)	(0.05)
$Seeds_{it}$ × Control	5.298***	3.075**	5.679***	3.264***
	(1.59)	(1.14)	(1.42)	(0.55)
$Seeds_{it}$ × Treatment 2			-0.551	1.975
			(1.21)	(1.41)
$Math\ score_{i,t-1}$	0.060	0.092*	0.141**	0.105***
	(0.06)	(0.05)	(0.06)	(0.03)
Constant	1.060***	1.261***	0.895***	1.087***
	(0.04)	(0.03)	(0.03)	(0.03)
Income-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	290	356	832	1,094

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member. $Math\ score_{i,t-1}$ is i's score (from 0-4) on a math test in the pre-treatment year 2008. Villages with more (less) consumption-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in weekly consumption of game, fish, eggs, maize, manioc, rice, oil, and bread (in bolivianos). Villages with more (less) income-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in total income, which is equal to weekly earnings from the sale of goods, wage labor, and barter.

Table 7: **Changes in Modern-Asset Holdings Across Treatments and Income Quantiles**

	Wealth in modern assets (in bolivianos)			
Treatment 1	615.144***		658.542***	
	(179.39)		(125.21)	
Treatment 2	384.566**		253.772*	
	(175.03)		(137.96)	
Control	204.064		301.076	
	(230.53)		(178.74)	
$Rice_{it}$		180.188		523.327***
		(125.76)		(81.17)
$\left\|Rice_{it} - \overline{Rice}_{vt}\right\|$		-103.934		125.565**
		(145.68)		(47.83)
$Seeds_{it}$ × Control		917.677		2443.520**
		(1225.87)		(1163.54)
$Seeds_{it}$ × Treatment 2				-1109.708
				(1352.04)
Constant	1059.097***	1173.258***	1087.642***	1119.995***
	(57.03)	(48.57)	(44.29)	(32.88)
Population	BOT20	BOT20	TOP80	TOP80
N	583	583	1,656	1,656

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member.

Table 8: **Changes in Modern-Asset Holdings Across Treatments and Initial Distributions**

	Wealth in modern assets (in bolivianos)			
$Rice_{it}$	314.603**	87.870	352.796***	643.617***
	(144.78)	(134.47)	(102.98)	(111.97)
$\|Rice_{it} - \overline{Rice}_{vt}\|$	-115.639	-40.819	297.329***	81.198*
	(147.12)	(161.27)	(51.65)	(41.85)
$Seeds_{it}$ × Control	-56.089	2376.967*	3297.828*	1127.874**
	(1793.40)	(1362.03)	(1673.67)	(491.63)
$Seeds_{it}$ × Treatment 2			-1474.201	-888.517
			(1464.41)	(1438.15)
Constant	1105.357***	1218.582***	1040.737***	1178.682***
	(55.32)	(68.52)	(48.95)	(35.65)
Consumption-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	280	303	706	950
	Wealth in modern assets (in bolivianos)			
$Rice_{it}$	228.926	138.218	557.920***	500.700***
	(187.41)	(164.03)	(99.78)	(117.61)
$\|Rice_{it} - \overline{Rice}_{vt}\|$	-66.643	-179.208	150.388**	66.454
	(189.56)	(216.20)	(68.29)	(79.97)
$Seeds_{it}$ × Control	155.211	1490.489	3633.745*	1233.294**
	(2528.40)	(955.60)	(1915.57)	(470.85)
$Seeds_{it}$ × Treatment 2			-1708.099	-10.408
			(2432.46)	(1211.53)
Constant	998.065***	1315.648***	1055.156***	1175.553***
	(66.70)	(63.48)	(58.60)	(27.54)
Income-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	255	328	730	926

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member. Villages with more (less) consumption-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in weekly consumption of game, fish, eggs, maize, manioc, rice, oil, and bread (in bolivianos). Villages with more (less) income-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in total income, which is equal to weekly earnings from the sale of goods, wage labor, and barter.

Table 9: **Changes in Traditional-Asset Holdings Across Treatments and Income Quantiles**

	Wealth in traditional assets (in bolivianos)			
Treatment 1	-10.567		-74.149	
	(44.34)		(46.06)	
Treatment 2	-91.480		-114.603**	
	(58.40)		(52.82)	
Control	124.628***		47.676	
	(44.91)		(55.25)	
$Rice_{it}$		-10.449		-47.036
		(14.57)		(29.50)
$\lvert Rice_{it} - \overline{Rice}_{vt} \rvert$		-23.701		-10.771
		(18.95)		(17.57)
$Seeds_{it}$ × Control		671.204**		407.296
		(321.69)		(345.22)
$Seeds_{it}$ × Treatment 2				-584.312**
				(287.80)
Constant	329.586***	337.329***	380.986***	372.032***
	(14.48)	(8.93)	(15.27)	(11.33)
Population	BOT20	BOT20	TOP80	TOP80
N	583	583	1,656	1,656

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member.

Table 10: **Changes in Traditional-Asset Holdings Across Treatments and Initial Distributions**

	Wealth in traditional assets (in bolivianos)			
$Rice_{it}$	21.103*	-32.119	-15.068	-69.585
	(10.45)	(20.33)	(21.41)	(46.03)
$\lvert Rice_{it} - \overline{Rice}_{vt}\rvert$	-77.917**	3.363	-27.817	-3.421
	(31.89)	(24.20)	(30.39)	(16.24)
$Seeds_{it}$ × Control	495.541**	934.452	29.392	989.273*
	(226.67)	(622.69)	(341.43)	(480.24)
$Seeds_{it}$ × Treatment 2			-1366.674**	-532.049*
			(527.01)	(283.53)
Constant	247.071***	418.514***	298.157***	426.741***
	(8.86)	(13.58)	(12.59)	(13.88)
Consumption-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	280	303	706	950
	Wealth in traditional assets (in bolivianos)			
$Rice_{it}$	-8.686	-11.968	-35.022	-54.893
	(20.72)	(21.17)	(44.64)	(38.77)
$\lvert Rice_{it} - \overline{Rice}_{vt}\rvert$	-30.454	-15.382	-1.204	-34.982
	(28.06)	(25.00)	(19.42)	(28.87)
$Seeds_{it}$ × Control	444.038	841.865*	111.170	708.398
	(305.88)	(480.14)	(383.89)	(482.60)
$Seeds_{it}$ × Treatment 2			-768.837**	-190.656
			(352.23)	(196.16)
Constant	317.072***	353.517***	324.278***	409.880***
	(13.46)	(11.03)	(16.74)	(14.95)
Income-distribution equality (in pre-treatment year 2008)	More	Less	More	Less
Population	BOT20	BOT20	TOP80	TOP80
N	255	328	730	926

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member. Villages with more (less) consumption-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in weekly consumption of game, fish, eggs, maize, manioc, rice, oil, and bread (in bolivianos). Villages with more (less) income-distribution equality are defined as the 20 villages with the lowest (highest) coefficient of variation in total income, which is equal to weekly earnings from the sale of goods, wage labor, and barter.

Table 11: **Determinants of Attrition (Leaving the Village)**

	Indicator for attrition			
Treatment 1	-0.299***		-0.270***	
	(0.02)		(0.01)	
Treatment 2	-0.800***		-0.011	
	(0.02)		(0.01)	
$Rice_{it}$		-0.061		-0.003
		(0.05)		(0.04)
$\lvert Rice_{it} - \overline{Rice}_{vt} \rvert$		0.080		0.057***
		(0.05)		(0.01)
$Seeds_{it}$ × Control		-0.356		-0.110
		(0.65)		(0.40)
$Seeds_{it}$ × Treatment 2				-3.365***
				(0.56)
Male	0.050*	0.049*	0.017	0.020
	(0.03)	(0.03)	(0.02)	(0.02)
Household head	-0.153***	-0.148***	-0.178***	-0.129**
	(0.05)	(0.05)	(0.04)	(0.05)
Age	0.003*	0.003*	0.004***	0.003***
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.784***	0.053	0.279***	0.280***
	(0.02)	(0.09)	(0.01)	(0.03)
Fixed effects	Villages	Villages	Villages	Villages
Population	BOT20	BOT20	TOP80	TOP80
N	877	877	2,445	2,445

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. In the OLS regressions, standard errors are clustered at the household level. Attrition is an indicator variable for whether an individual left the village after the pre-treatment year 2008. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member.

Table 12: **Human Capital Outcomes Across Treatments – Robustness**

	Spanish ability in speaking and reading (0-4)			
$Rice_{it}$	0.355***	0.355***	0.337***	0.338***
	(0.06)	(0.06)	(0.07)	(0.07)
$\lvert Rice_{it} - \overline{Rice}_{vt} \rvert$	0.102**	0.106**	0.140**	0.145**
	(0.04)	(0.05)	(0.06)	(0.06)
$Seeds_{it}$ × Treatment 2	-0.721	-1.141	-1.371	-1.829
	(0.71)	(0.93)	(0.87)	(1.16)
$Seeds_{it}$ × Treatment 2 × Planted	1.370	2.263*	1.199	2.058
	(1.46)	(1.20)	(2.59)	(2.25)
$Seeds_{it}$ × Treatment 2 × TOP40		0.645		0.605
		(0.86)		(1.31)
$Seeds_{it}$ × Treatment 2 × Planted × TOP40		-2.089		-1.794
		(1.40)		(2.23)
$Seeds_{it}$ × Control	4.060***	4.058***	3.882***	3.887***
	(0.75)	(0.75)	(0.85)	(0.85)
$Math\ score_{i,t-1}$	0.127***	0.127***	0.160***	0.159***
	(0.03)	(0.03)	(0.04)	(0.04)
Constant	0.999***	0.999***	0.848***	0.848***
	(0.02)	(0.02)	(0.02)	(0.02)
Population	TOP80	TOP80	TOP80, Age $\leq$ 20	TOP80, Age $\leq$ 20
N	1,882	1,882	1,144	1,144

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household member, and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household member of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member. $Planted \in [0, 1]$ is the proportion of the rice-seed transfer (in Treatment 2 and the control group) that was planted, and is measured at the household level. $TOP40$ is an indicator variable for households among the richest 40% in terms of area of cleared forest (cf. Section 2.2.1). $Math\ score_{i,t-1}$ is i's score (from 0-4) on a math test in the pre-treatment year 2008.

Table 13: **Alternative Checks**

	Consumption		Credit given out	
Treatment 1	20.193		-31.386	
	(38.53)		(21.77)	
Treatment 2	26.238		-3.747	
	(33.85)		(3.95)	
Control	-4.035		-1.401	
	(21.02)		(6.82)	
$Rice_{it}$		5.838		-32.088
		(6.09)		(28.28)
$\lvert Rice_{it} - \overline{Rice}_{vt}\rvert$		0.636		-1.390
		(2.03)		(2.27)
$Seeds_{it}$ × Control		-6.838		-25.716
		(35.65)		(36.61)
$Seeds_{it}$ × Treatment 2		18.516		-14.454
		(298.78)		(38.67)
Constant	261.191***	261.899***	20.866***	21.195***
	(9.13)	(7.79)	(3.54)	(4.08)
Population	TOP80	TOP80	TOP80	TOP80
N	685	685	1,651	1,651

Notes: */**/*** denote significance at the 10%/5%/1% level, respectively. All OLS regressions include individual fixed effects, and standard errors are clustered at the village level. The unit of observation is the household in the first two and the individual level in the last two columns. Household consumption corresponds to one week's consumption of game, fish, eggs, maize, manioc, rice, oil, and bread (in bolivianos). Credit given out is measured in bolivianos, and represents the total amount of loans provided by an individual in the last two months before the interview. $Rice_{it}$ denotes the amount of edible rice in 10 kg per household (member), and $\overline{Rice}_{vt}$ is the average amount of edible rice in 10 kg per household (member) of all other villagers (not in i's treatment group) in village v. $Rice_{it} - \overline{Rice}_{vt}$ is defined to be non-zero only in Treatment 2, and is then positive for villagers in the bottom 20% (BOT20) and negative for all remaining villagers (TOP80). $Seeds_{it}$ denotes the amount of rice seeds in 10 kg per household member.