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Inequality, Relative Income, and Human Capital Investment*

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Abstract

Do investment responses to income transfers depend on the implied level of redistribution because of social comparisons? In a field experiment in 53 villages of an Amazonian foraging-farming society, we allocated substantial in-kind transfers, varied their associated degree of village income inequality, and measured the short-run effects on individual-level determinants of development. We find that the poorest households invested significantly less in human capital and engaged less with the labor market under an inequality-reducing treatment than under income-distribution neutrality. Our evidence suggests that inequality shapes the development process through social comparisons, and has implications for the effectiveness of transfer programs.

JEL classification: D31, O10, O15

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

1 Introduction

Transfer programs in developing countries are important attempts at targeting those in need and reducing poverty within the population. When deciding on the exact implementation of a given program, however, it is crucial to focus not only on the direct effects that such transfers can achieve in terms of poverty alleviation, but also on indirect effects which might be the result of changed incentives. More specifically, most transfers have an effect on inequality within an area, as only part of a population receives a given transfer. Hence, when evaluating a policy, these inequality-reducing effects also need to be taken into account, as individuals might react not just to the transfers themselves, but to the change in inequality as well.

Such an additional effect could operate, for example, through social comparisons, which have been found to play a critical role for well-being (Luttmer (2005)) and for effort exertion in the workplace (Cohn, Fehr, Herrmann, and Schneider (2014)). To explore this channel, we conducted a randomized controlled trial in 53 villages of a foraging-farming society in the Bolivian Amazon, where the Tsimane' reside, and assessed the impacts of both income transfers and their implied village-level income inequality on human capital investment.

To the best of our knowledge, this is the first paper that specifically aims at distilling an additional effect of village-level inequality associated with income transfers. The experiment draws its main strengths from its design in conjunction with unique features of the Tsimane' society. By providing in-kind transfers that were not necessarily distributed equally across different parts of the income distribution, we were able to achieve randomization of income inequality at the village level. To determine the income position of households in their respective villages, we use their area of forest cleared, which we show to be a persistent determinant of the bulk of income earned by adult household members. Furthermore, it is correlated with general expenditures, animal wealth and (self-reported) credit constraints, but not with schooling or other measures of education.

We implemented three treatments in order to contrast between absolute income effects and relative income effects due to social comparisons. In doing so, we transferred rice as a

positive one-time income shock. Besides a control group of 13 villages, we implemented one treatment in which 13 villages each received 782 kg of edible rice, which we divided equally among all households in the village. In another treatment, we transferred 5.9 kg of rice seeds to each household in 14 villages. Finally, in another 13 villages, we combined the first two treatments by allocating the same total amount of edible rice as in the first treatment (782 kg), but only to the poorest 20% of households in each village; the remaining 80% of households received 5.9 kg of rice seeds, which were worth less than the transfers received by the bottom 20%. This set of three treatments enables us to disentangle a potential relative income effect, while keeping absolute income effects from edible-rice and rice-seed transfers constant for the bottom 20% and top 80%, respectively.

The identification of these effects and their economic interpretation are facilitated by the structure of the Tsimane' economy: this is a simple village economy in which income inequality is easily measured, and there are only a few well-defined avenues for behavioral responses.[1] Naturally, these positives need to be balanced against any shortcomings in terms of the external validity of our findings. A drawback of our approach is that lessons from a village economy do not imply symmetric effects in more advanced economies. Yet, to the extent that even the simple Tsimane' economy allows for a trade-off between investment, savings, and consumption, we believe that our experiment offers valuable insights.

For the Tsimane', the only alternative to foraging and farming, their traditional activities, is learning Spanish to connect with the outside labor market. This form of human capital investment offers the highest return in terms of future income. The data we collected include information on measures of human and physical capital. Therefore, instead of focusing on consumption responses (see, among others, Angelucci and De Giorgi (2009), Haushofer and Shapiro (2013), and Bazzi, Sumarto, and Suryahadi (2014)), we investigate investments in main drivers of growth, namely learning Spanish for use in market-related activities.[2]

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[1] More than that, in this relatively pure environment, premises of related theoretical models, such as political-economy channels of redistribution (e.g., Alesina and Rodrik (1994), Persson and Tabellini (1994), and Bénabou (2000)) or assumptions about credit markets (see, for instance, Banerjee and Newman (1991) and Galor and Zeira (1993)), are absent or play only subordinate roles.

[2] Gertler, Martinez, and Rubio-Codina (2012) also extend their analysis of the effects of income transfers beyond consumption to include investment responses.

Our main finding is that the bottom 20% invested in market-related human capital through improving Spanish fluency when they received edible rice (a positive absolute income effect), but significantly less so if the transfer was associated with a reduction in village-level inequality, as was the case in only one of our treatments. This suggests that a reduction in inequality evokes a negative relative 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 (have to) invest more to increase their future income. In contrast, in the case of income-distribution neutrality (in the remaining treatments), receiving the in-kind transfer freed resources that were used for income-enhancing activities, such as studying Spanish. We conduct a similar exercise for the top 80%, but find no differential effect on their human capital investment under the inequality-reducing treatment.

As successful human capital investment is associated with higher participation in wage labor (i.e., working for outsiders, such as loggers, farmers, or cattle ranchers) and, eventually, out-migration by the Tsimane', we also scrutinize treatment effects on wage-labor participation. Mirroring our results for human capital investment, we find that the bottom 20% worked significantly less for outsiders in the inequality-reducing treatment.

These results lend support to the idea that individual investments that increase future income may be driven importantly by the changed village-level inequality due to our in-kind transfers. In order to provide further evidence in favor of our proposed channel of social comparisons, we proceed in multiple steps. First, we show that our documented negative relative income effect on human capital investment is not due to foraging and farming becoming more attractive for the bottom 20% households under the inequality-reducing treatment. The latter would imply a simple substitution of human capital investment by foraging-farming activities, rather than reduced overall effort by the bottom 20% households. Second, we investigate whether our findings could be explained by factors that change with village-level inequality and that could have differentially affected the bottom 20% vs. the top 80% households. To this end, we consider the development of village-level prices and risk-sharing arrangements, but fail to find any differential effects across our three treatments.

In summary, our field experiment was designed to estimate the impact of in-kind transfers associated with income-distribution neutrality and income-inequality reduction. We find an adverse effect on human capital investment by the main beneficiaries of inequality reduction, and provide evidence of social comparisons as the driving force underlying this effect. While this explanation is consistent with higher utility of the inequality-reducing treatment for the poorest households, our findings point to a hidden cost in the form of reduced human capital investment. If proven to persist, this hidden cost can undermine the purpose of redistributional policies by disincentivizing the poor from directing their efforts to income-enhancing activities that are necessary to improve their position in the medium run.

2   Context and Experimental Design

Next, we provide some background on the Tsimane'. We also present our field-experimental design, discuss its implications in terms of economic significance for the villagers, and explain how we determined their position in the income distribution prior to our treatments.

2.1   Background on the Tsimane'

The Tsimane' are a highly autarkic, endogamous, small-scale society in the Bolivian Amazon (Department of Beni). They only recently opened up to regular contact with Westerners, largely initiated through 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 and fishing, 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[3] allows them to interact more closely with loggers, farmers, cattle ranchers, and other outsiders who may offer employment opportunities.

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[3] Local teachers in charge of Tsimane' education were trained and paid by missionaries (from 1954 until 1985) or by the Bolivian government (since 1985).
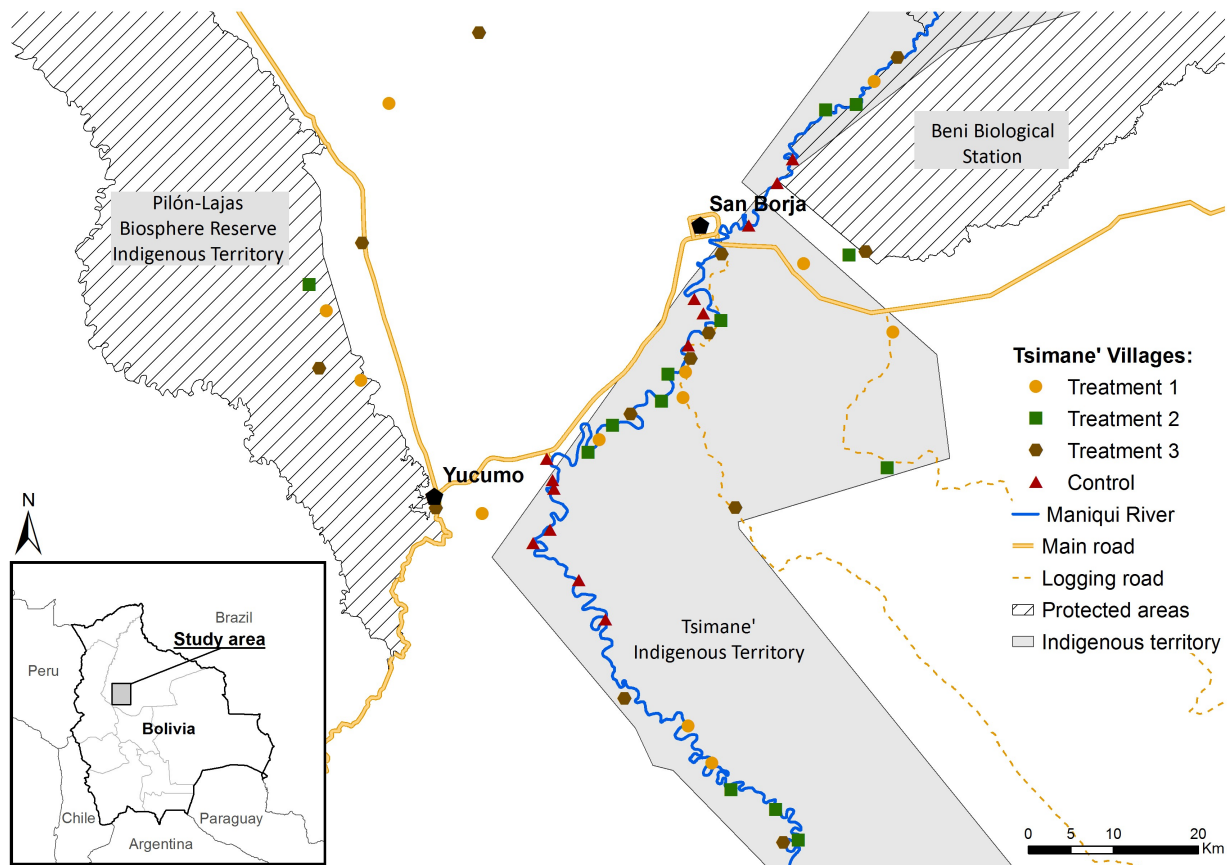
Figure 1: **Map of Study Area.** Two villages could not be located via GPS, namely Puerto Salas (around San Borja) in Treatment 1 and Motacusal (around Yucumo) in Treatment 3.

In total, there are three sources of monetary income (for more detail, see Godoy et al. (2005) and Saidi (2012)): sale of forest and farm goods, wage labor (for which knowing Spanish beyond the most rudimentary level is required), and barter trade. Out of the three, wage labor pays the most. To the extent that knowing Spanish opens the door to the outside labor market, it can be considered a major driver of development, at least from an income-growth perspective.

The Tsimane' live in 95 villages. Our control group consists of 13 villages with which we have many years of field experience as part of a panel study conducted from 2002 to 2010. To select the sample for our experimental treatments among the remaining villages, 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, in addition to 13 villages in the control group (see Figure 1). During the surveys, we collected demographic and anthropometric information

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 total sample includes 2,736 individuals in 690 households (in 53 villages), of which 152 belonged to Treatment 1 (13 villages), 170 to Treatment 2 (13 villages), 164 to Treatment 3 (14 villages), and 204 to the control group (13 villages).

2.2   Study Design

In presenting the study design, we discuss two items in particular: the experimental treatments and, as it was our intention to reduce village-level income inequality in one of them, our way of determining a household's position in the village income distribution.

2.2.1   Experimental Treatments

As this society relies substantially on own production and bartering, 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 rice seeds to households, we selected at random either the female or the male household head to receive the transfer. 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.

The majority of household heads were present during the transfer. 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.[4] We next 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. Transferring the same amount of rice to each village ensured that each village was affected by the same positive aggregate in-kind-income shock. However, 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 59.31 kg (std. dev. = 23.38 kg; range from 30 to 130 kg).

Treatment 2. The total amount of edible rice received by each of the 13 villages in Treatment 2 was the same (782 kg) as in Treatment 1. This amount was distributed equally among the poorest 20% of households in each village. We used the area of forest cleared by households during the dry season before the pre-treatment year to identify the poorest 20% of households in each village. We will discuss this further below. Besides the 782 kg of edible rice, we also transferred 5.9 kg of rice seeds to each household in the top 80% of the deforestation distribution.

Since the transfers of edible rice in Treatment 2 went only to households in the bottom 20% of the deforestation 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 deforestation distribution in Treatment 2 was 175.17 kg (std. dev. = 79.36 kg; range from 98 to 391 kg). Note that the average amount of rice for Treatment 2 is not five times the one for Treatment 1, because the distributions of village sizes for the two treatments are not identical. Another reason is that given the discrete nature of the deforestation distributions, we may capture the bottom 25-35% rather than the bottom 20%, because the groups below 20% in the distribution at times add up to only slightly less than 20% and there is a large mass point at 20%.

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[4] These alternative estimates are available upon request.

Treatment 3. Treatment 3 consisted of another 14 villages wherein each household received 5.9 kg of 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 0.63 hectares of old-growth forest (median = 0.5, std. dev. = 0.81), 0.52 hectares of fallow forest (median = 0.4, std. dev. = 0.66), and 1.15 hectares of total forest (sum of old-growth and fallow forest; median = 1, std. dev. = 0.87).

Households have usufruct rights to the plots that 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, households have to clear old-growth forest and/or fallow forest each year to plant rice, the main cash crop and the form of in-kind income chosen for our experiment. Second, people consume all the output from cleared forest that they do not sell. These two points suggest that the area of cleared forest captures both an important 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.[5] Thus, reported area of cleared forest has relatively 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 that are more likely to depend on foraging than on farming, which tend to be concentrated in more remote villages.

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[5] See Alatas et al. (2012) for a more general discussion of the difficulties in identifying poor households.

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 transfers using 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$14.30/person in Treatment 1 and US$42.71/person for the bottom 20% in Treatment 2. For a family living at a daily poverty measure of $1/person, the transfers would amount to income earned over 14.3 days (Treatment 1) or 42.7 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 23.1 (Treatment 1) or 68.9 days (Treatment 2).

In order to estimate the monetary value of the 5.9 kg of rice seeds transferred to the top 80% in Treatment 2 and to all households in Treatment 3, 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$2.11/person. The perceived value of the rice seeds might have been lower than this, however, because there is no market for rice seeds in the study area. Tsimane' buy local seeds in local towns, whereas 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.

Most households reported to have planted rather than sold the rice seeds, so the latter can also be understood as a deferred benefit. According to our field experience, planting 5.9

kg of 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 $15 \cdot 40$ bolivianos (daily wage) $=$ US\$85.71 in addition to transportation cost of 420 bolivianos $=$ US\$60. Given an average household size of six, the rice-seed transfer translates into an income transfer of (US\$241 – US\$85.71 – US\$60)/$6 =$ US\$15.88/person, not accounting for any discounting (in addition to any spoilage of seeds, or loss of crops from theft, pests, and diseases). Depending on whether households decided to plant the rice seeds or not, our rice-seed transfers might actually have been a deferred benefit greater in amount than the transfer in Treatment 1 (see above, US\$14.30/person) but still significantly lower than that in Treatment 2 (US\$42.71/person).

Note that the main effects of our transfers are observed among the bottom 20% households of the deforestation distribution. As we shall see in the next section, a household's position in the deforestation distribution is correlated with its position, and that of its members, in the income distribution. This further strengthens the economic significance of our treatments.

2.3   Descriptive Statistics

In Table 1, we present summary statistics for the adult population in our regression sample (with two available observations for the pre- and post-treatment years) in the pre-treatment year for various income percentiles. This also serves to characterize to what extent the village distribution of area of forest cleared captures features of the village income distribution.

We focus on the following percentiles: the top 40%, the next 40% (summing up to the top 80%), the bottom 20%, and the bottom 10%. Clustering standard errors at the village level, we also provide p-values for the difference between the top 40% compared to the bottom 10%. In the upper panel of Table 1, we consider household-level variables. In the second row, we note that the area of forest cleared is highly correlated with consumption. The third and fourth rows reveal two additional correlates of area of forest cleared, namely household size and whether a household used chainsaws to clear forest. The fact that the number of

household members correlates with the area of forest cleared indicates that the corresponding village distribution has a relatively constant character and is, thus, unlikely to be related to our outcome variables of interest (most notably human capital investment). Still, it may be possible that households suffer idiosyncratic shocks, such as household members unable to help with clearing forest due to illness during the dry season. Conversely, the possession of chainsaws reflects a household's openness to outsiders and the market economy outside the villages, which may capture a household's propensity to learn and/or speak Spanish. Therefore, we also consider in our estimates the subsample of households without chainsaws as a robustness check, in an attempt to make the village distribution of area of forest cleared more independent of our outcome variables of interest.

We move to the individual level in the lower panel of Table 1. Interestingly, not many other characteristics vary across the deforestation distribution. Those that do, however, are all the more important to show that the area of forest cleared by a household maps to an individual household member's position in the income distribution, above and beyond the relationship of deforestation with household size. First, individuals in households at the bottom of the deforestation distribution spend less on goods. The latter category, however, includes food expenditures, which we incorporate in a proxy for income from foraging and farming. We define it as income from the sale of forest and farm goods plus the average value of individual food consumption minus any food expenditures over one week. As can be seen in Table 1, individuals in households at the bottom of the deforestation distribution earn less from foraging and farming. We can also see this by distinguishing between income from the sale of forest and farm goods as opposed to income from wage labor and barter. Among our three income measures, area of forest cleared is closely related only to income generated from the sale of forest and farm goods, rather than wage labor (even when measured as the number of days worked for an outsider over one week) or barter. Altogether, we infer from these statistics that while larger *households* do clear larger areas of forest, *individuals* therein also earn more from foraging and farming in general.

Similarly, animal wealth correlates strongly with area of forest cleared, whereas tra-

ditional and more modern assets – i.e., those that are acquired through interaction with outsiders – do not. That is to say, the area of forest cleared, on average, captures individual-level foraging-farming income and animal wealth, but it is not a measure of an individual's propensity to engage with the outside economy. This is also reflected in the baseline differences in Spanish fluency (both in speaking and in reading, which we measure individually as scores from 0 to 2). In both instances, the bottom 10% do just as well as the top 40%. The same holds for mathematics scores and years of schooling. While area of forest cleared correlates with what constitutes the bulk of people's income, namely foraging and farming, and their respective assets, the relative importance of human capital, our primary outcome measure, is similar across deforestation groups.

Lastly, we consider individuals' self-reported ability to receive credit to fathom whether they are financially constrained. We characterize individuals as financially constrained if they reported to be unable to readily borrow 100 bolivianos from anyone. As one can see in the last row of Table 1, individuals at the bottom of the deforestation distribution are more likely to have reported to be unable to borrow 100 bolivianos at the time of the interview. While the difference between individuals in top 40% and bottom 10% households is not statistically significant, the (unreported) difference between the top 80% and the bottom 20% is marginally significant at the 13% level. This provides further evidence that our measure of area of forest cleared approximates characteristics that are related to one's position in the income distribution.

2.4 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 improved Spanish fluency. We now demonstrate that Spanish fluency is an important covariate of personal income. This enables us to interpret changes in Spanish ability as a response to our treatments in terms of changes in expected future income. Using the baseline survey in the pre-treatment year, we estimate the impact of rated Spanish-speaking and Spanish-reading abilities on the

sum of earnings from the sale of forest and farm goods and from wage labor.[6]

We expect Spanish fluency to be associated with labor-market success and, thus, higher income. The results in Table 2 support this hypothesis. The first column suggests that there exists a positive and economically meaningful correlation between Spanish ability and total income: an improvement in the Spanish score, which ranges from 0 to 4, by one unit corresponds to an income increment of US$2.88 dollars per week, which is approximately one-sixth of the average income (US$17.63 per week) in the pre-treatment year. This estimate holds up to including village fixed effects in the second column, as well as personal and household characteristics in the third column. Lastly, we include total assets (i.e., the sum of animal wealth, traditional assets, and modern assets) alongside years of schooling and mathematics scores (also ranging from 0 to 4) as alternative measures of human capital accumulation in the fourth column. The impact of Spanish ability is as high as in the first column, and remains significant at the 4% level.

It is important to note that besides its empirical impact on generated income, Spanish fluency is also a proxy for increased interaction with outsiders and the market economy, which helps to sustain higher income beyond the short run. For instance, the most successful human capital investments lead to employment with outsiders (and, eventually, out-migration), a sign of integration into the market economy. Against this background, we will, along with human capital investment, also consider wage-labor participation as an outcome variable.

3   Results

We now turn to the results of our field experiment. We will proceed in three steps. First, at the village level, we scrutinize whether inequality was reduced under Treatment 2, compared to all other treatments. Second, we present our main findings for individual human capital investment in the form of improved Spanish fluency, along with other outcome variables that could explain any differential human-capital-investment behavior by the main beneficiaries

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[6] Our results are robust to including gains from barter trade.

of inequality reduction. Finally, we consider to what extent our individual-level findings may be driven by other factors related to treatment-induced variation in village-level inequality, rather than by social comparisons between the bottom 20% and the remaining villagers.

3.1 Relationship between Treatments, Inequality, and Human Capital Investment

As our aim was to vary inequality 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) inequality. To establish whether our treatments affected inequality in the first place, we compute the ratios of household consumption between the 60th and the 10th percentile of the deforestation distribution, and consider the difference before vs. after each treatment at the village level.

Ratios of Household Consumption Between 60th and 10th Percentile of Deforestation Distribution Before and After Treatments

	Treatment 1	Treatment 2	Treatment 3	Control
HH consumption ratio before	1.542	1.846	2.098	1.716
HH consumption ratio after	1.954	1.343	2.108	1.527
No. of villages	13	13	14	13

We know from the second row in Table 1 that consumption varies significantly with de-forestation, and that the ratio under consideration was both economically and statistically significant in the pre-treatment year. The above table shows that this variation in consumption at the household level – regardless of its origin, e.g., household size – was reduced most strongly under Treatment 2.[7]

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[7] This also relates to how households decided to use our transfers of edible rice and rice seeds. During the follow-up survey, we asked the household heads about their actual use of the transfers, according to which most households did not sell or barter the transfers received. The bottom 20% households in Treatment 2 and households in Treatment 1 mainly consumed the edible rice received, while the top 80% households in Treatment 2 and households in Treatment 3 mainly planted the rice seeds.

Thus, Treatment 2 was not just designed to, but actually did, reduce inequality by differentially treating the bottom 20% and the top 80% households. In conjunction with Treatments 1 and 3, we yield two tests that can disentangle any treatment effect of mere transfers from an additional effect associated with the change in village-level inequality. We summarize them in the following table:

Empirical Strategy for Identifying Relative Income Effects

	Bottom 20%	Top 80%
Treatment 1	Absolute income effect	
	Income-distribution neutrality	
Treatment 2	Relative income effect	5.9 kg of rice seeds
	Reduction in income inequality	
Treatment 3		5.9 kg of rice seeds
		Income-distribution neutrality

For the bottom 20%, Treatment 1 implies income-distribution neutrality and, therefore, gives an estimate of an absolute income effect of the edible-rice transfers. Conversely, Treatment 2 implies a reduction in income inequality, as just witnessed. For the bottom 20%, Treatment 2 can be compared to Treatment 1 insofar as both treatments should, in principle, imply the same absolute income effect of the edible-rice transfers, but Treatment 2 yields an additional relative income effect due to the implied reduction in village-level inequality. Therefore, the focus for the bottom 20% is on testing whether the respective group of treated individuals responded differently under Treatment 2 compared to Treatment 1.

Similarly, for the top 80%, Treatment 2 also implies reduced income inequality while transferring rice seeds to the top 80%, which is precisely what takes place also under Treatment 3 without said reduction in income inequality. Therefore, for the top 80%, we test whether the respective group of treated individuals responded differently under Treatment 2 compared to Treatment 3, thereby differencing out the absolute income effect from the rice seeds and distilling a relative income effect. Note that our rice-seed transfers, while

theoretically at least as valuable as those under Treatment 1, are not directly comparable to the edible-rice transfers, as the realized value of the former depends on the respective households' decision to plant the rice seeds, which we will scrutinize separately.

Impact on human capital investment. An important concern is the interpretation of what we dub a relative income effect. In its purest form, a relative income effect reflects the existence of social comparisons underlying the villagers' utility. That is to say, in the absence of additional channels through which reduced village-level income inequality differentially affects the bottom 20% vs. the top 80% in Treatment 2, villagers react to their new relative positions in the income distribution, in addition to the value of the rice transfers.

In order to infer such social comparisons governing the villagers' utility, we look at treatment effects on an investment decision that reveals the treated individuals' preferences over future income. As pointed out in Table 2, human capital investment is a major determinant of income, especially through its direct impact on one's access to the outside labor market and wage labor. To this end, before discussing our results in detail, we preview the treatment effects on Spanish fluency. Figures 2 and 3 plot the change in the sum of the rated speaking and reading abilities (which are each valued between 0 and 2, so the sum ranges from 0 to 4) before and after each treatment, separately for the bottom 20% and the top 80%.

We make the following general observations. First, given the magnitude of the positive income effect under Treatment 3, it appears that the fact that rice seeds – if planted – are deferred rather than immediate benefits did not differentially affect the short-run income effect. Second, comparing the changes in Spanish fluency to those in the control villages, only the bottom 20% invested more in human capital compared to the control group, whereas the top 80% do not seem to have been constrained in the absence of any transfers. That is to say, we observe an positive *absolute* income effect only among the bottom 20%, for whom the rice transfers alleviated constraints that would otherwise have kept them from investing in human capital.[8]

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[8] This is in line with our above-mentioned observation based on the last row of Table 1 that the bottom 20% reported to be more financially constrained than the top 80%.

Figure 2: **Average Spanish-rating (Speaking and Reading, 0 – 4) Improvement by Treatment for Individuals in Bottom 20% Households.** Error bars indicate 95% confidence intervals.

To test for a potential *relative* income effect due to social comparisons among the bottom 20%, we compare the treatment effects under Treatments 1 and 2. As can be seen in Figure 2, human capital investment is lower under Treatment 2 than under Treatment 1. This holds true irrespective of whether we consider absolute or relative changes in the Spanish scores, which range from 0 to 4. Thus, while the edible-rice transfers freed resources for the bottom 20% that were used for income-enhancing activities, such as studying Spanish, under both treatments (absolute income effect), the total treatment effect on human capital investment is smaller under Treatment 2. Given that the main difference between Treatment 1 and Treatment 2 consists of the change in village-level inequality benefitting the bottom 20%, we interpret this as evidence of social comparisons as an underlying mechanism. That is, the bottom 20% exhibit a negative relative income effect, as they were made relatively better off and, thus, derived utility from the reduction in income inequality. This led them to invest less in future-revenue-increasing activities such as learning Spanish, as some of that investment would enter utility through increased future income and social comparison.
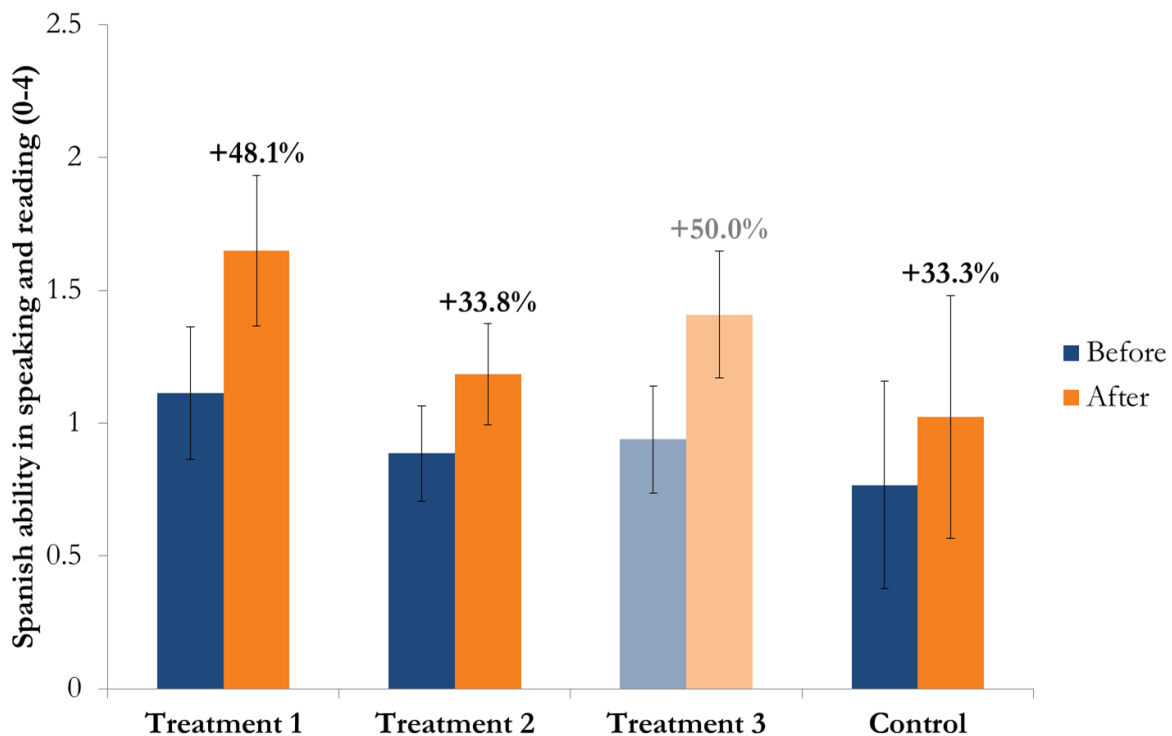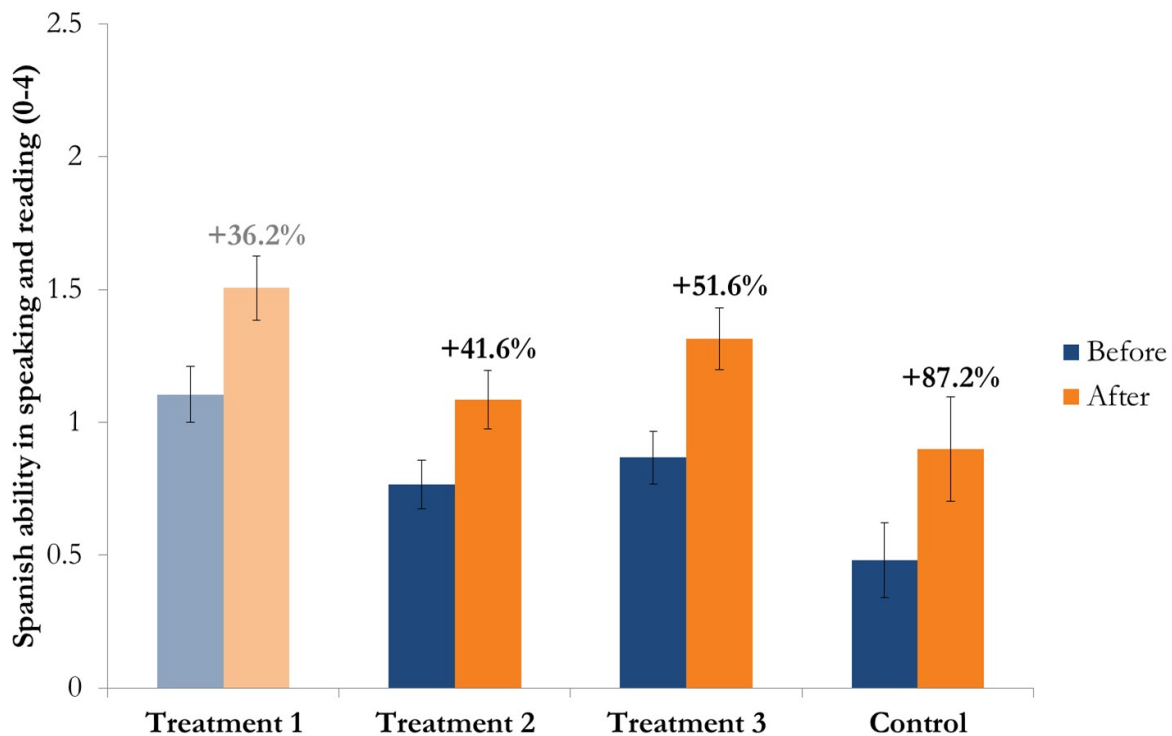
Figure 3: **Average Spanish-rating (Speaking and Reading, 0 – 4) Improvement by Treatment for Individuals in Top 80% Households.** Error bars indicate 95% confidence intervals.

Note that strictly speaking, the bottom 20% households in Treatments 1 and 2, on average, did not receive the exact same amount of edible rice. This is a consequence of our keeping the aggregate amount of edible rice (782 kg) transferred to the respective villages constant. This, however, comes at the cost of introducing some variation in amounts received at the household level. In addition, this gives rise to a simple alternative explanation for our negative relative income effect: leisure could be a normal good, so that given the higher transfer amounts in Treatment 2 compared to Treatment 1, the bottom 20% optimally studied less. To rule out this possibility, one can exploit the overlap in transfer amounts between Treatments 1 and 2 due to the variation in village size and, thus, transfer amounts received by households within these treatments. This enables us to focus on smaller villages in Treatment 1, where each household received relatively more edible rice, and larger villages in Treatment 2, where each household received relatively less 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 once we compare

the bottom 20% in smaller villages under Treatment 1 to the bottom 20% in larger villages under Treatment 2. We will show this more explicitly in our regression analysis.

For the top 80%, when we compare the effects under Treatments 2 and 3 in Figure 3, human capital investment is rather similar across the two treatments. While there is, again, the tendency for human capital investment to be somewhat lower under Treatment 2, the difference is neither economically nor – as we will see in the regression analysis below – statistically significant. This may be related to the deferred-benefit nature of the rice seeds, requiring their planting for full value realization. To explore this possibility, we will also investigate if, and show that, the top 80% households planted equal proportions of their rice seeds in Treatments 2 and 3.

Taken together, Figures 2 and 3 seem to suggest that human capital investment was lower in the aggregate in the inequality-reducing Treatment 2. While such aggregate statements are not the focus of this paper, we interpret this as evidence cautiously suggesting that our treatment-induced reduction in inequality had an adverse impact on aggregate human capital investment. As we have just seen, this effect is driven primarily by the bottom 20% households, possibly operating through a negative relative income effect.

Theoretical mechanism. Before moving to the regression analysis, we sketch a potential theoretical mechanism underlying the negative relative income effect for the bottom 20% households. In doing so, we attempt to relate inequality to human capital investment through social comparisons.

Each villager i, out of n individuals living in the same village in total, has some base income y_{i0} before the treatment, and receives a transfer t_i. Given our characterization of the rather simple Tsimane' economy, adult villagers generally face a decision of allocating their time or effort (say one unit) between two activities: foraging-farming and learning Spanish. While the former is an easy-to-observe activity the idiosyncratic risk of which can be shared within the village, it pays less in expectation than wage labor, which is the positive return realization from studying Spanish. That is, foraging and farming is less risky and pays less, whereas human capital investment is associated with higher expected income but is riskier.

For instance, it is not certain how quickly one can gain employment with an outsider upon human capital investment. In addition, due to the reduced observability of returns to human capital investment, it is presumably more difficult to share associated idiosyncratic risk with other villagers.

We are interested in how different kinds of transfers $\{t_i\}_{i=1}^{n}$ affect the decision to invest $e_i \in [0, 1]$ in the activity with the larger income upside, namely studying Spanish. In particular, our treatments in the field experiment varied the extent to which our transfers affected village-level inequality. We did this by linking transfers to individuals' positions in the income distribution, which are implied by $\{y_{i0}\}_{i=1}^{n}$.

In a model in which village-level inequality bears no relationship with the individual decision e_i through prices (i.e., the value of consumption), social comparisons (or alternative interpretations of other-regarding preferences, as in Fehr and Schmidt (1999)) implied by the villagers' utility functions are the only way in which inequality may affect investment decisions. Empirically, we will attempt to rule out alternative channels through which altered inequality may affect human capital investment other than through social comparisons. This comprises any impacts on prices and risk-sharing arrangements at the village level that could differentially affect poor vs. richer households.

In the context of our findings, what is a plausible interpretation of social comparisons adversely impacting human capital investment in the inequality-reducing treatment? An important property of our findings is that we find a negative relative income effect only for the main beneficiaries in Treatment 2, the bottom 20% households. For them, if their utility is linked to their relative position in the village income distribution, the return to human capital investment is twofold: besides, or because of, the chance of higher income in the future, it constitutes a valuable option to change one's position in the income distribution by opening up a new avenue towards wage labor and, eventually, out-migration. Studying Spanish is the only way out of the community, typically initiated by intensified contact with outsiders (such as loggers, farmers, or cattle ranchers) in the form of wage labor. While this option is not necessarily dominant, its desirability is increasing in one's preference for

"reshuffling the cards."

Then, a simple explanation for the reduced human capital investment by the bottom 20% households in Treatment 2 is that we exogenously reduced the value of such social-comparison-motivated migration. The behavior of the top 80% households lends further support to our interpretation of the results. First, the top 80% did not invest more in human capital because of the in-kind transfers: they exhibit no positive absolute income effect. This is due to the fact that the top 80% households are less constrained in their choice between foraging-farming and human capital investment, and their decision to switch from foraging-farming to human capital investment was not altered by our intervention. Conversely, the bottom 20% were more constrained in their choice between foraging-farming and human capital investment, and the in-kind transfers relaxed those constraints, leading to more human capital investment by the bottom 20% than in the control group.

As a consequence of the absence of a positive (absolute) income effect among the top 80%, we detect an additional relative income effect only for the bottom 20%. This asymmetry attests to the idea that social comparisons (or another form of other-regarding preferences) drive our results. If the change in inequality under Treatment 2 affected the value of consumption through prices, we should observe changes in foraging-farming activity, as well as some differential reaction – if not in terms of human capital investment, then in terms of foraging and farming – by the top 80% under Treatment 2 (compared to Treatment 3). As we will see in the empirical analysis, to which we turn next, this is not the case.

3.2   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 OLS regression specification is constant across Table 3 and Tables 5 to 8. We restrict the sample to individuals with two available

observations (pre- and post-treatment years), and include individual fixed effects:

$$outcome_{it} = \beta_1 After_t + \beta_2 Treatment\,1_i \cdot After_t$$
$$+\beta_3 Treatment\,2_i \cdot After_t + \beta_4 Treatment\,3_i \cdot After_t$$
$$+\mu_i + E_{it}, \tag{1}$$

where $After_t$ is an indicator for the post-treatment year and we, for convenience, abbreviate the interaction effects by the treatment number in our tables.

As already discussed, our across-treatment comparisons are different for the bottom 20% and top 80% households, so we will run regressions separately for these two groups. Furthermore, in line with our previous discussion, we will use as omitted category (captured by the coefficient on $After_t$) Treatment 1 for the bottom 20% and Treatment 3 for the top 80%, so that the coefficient on $Treatment\,2_i \cdot After_t$ estimates the relative income effect. The underlying rationale is to difference out the pure edible-rice (for the bottom 20%) and rice-seed transfers (for the top 80%) to distill any reaction to the associated change in inequality under Treatment 2.

Human capital investment. The first three columns of Table 3 estimate regression specification (1) for individuals in bottom 20% households (BOT20). The first column reflects the graphical evidence from Figure 2 that the bottom 20% households invested significantly less in Spanish in Treatment 2 than in Treatment 1, namely by a score of 0.237. Note that while our results hold also for percentage improvements in Spanish scores, we do not take a stance on whether improving one's Spanish at a high or low level should be given different weight and, thus, use the absolute scores as dependent variable.

Next, we attempt to address the concern that the edible-rice amounts received by each household varied not just by village size but also by treatment. The endogeneity of edible-rice amounts received by households to village size should not bias the negative relative income effect for the bottom 20%, as long as village size did not differentially affect the impact of edible-rice transfers on human capital investment under Treatments 1 and 2. However, our

interpretation of the negative relative income effect rests on the ability to difference out the absolute income effect, stemming from the edible-rice transfer. Hence, it is important to keep the rice amounts constant for bottom 20% households across Treatments 1 and 2.

This is a valid concern because the bottom 20% households in Treatment 2 received more edible rice than those in Treatment 1. To make the rice amounts more comparable, in the second column, we limit the sample to the four smallest and four largest villages (out of 13) in Treatments 1 and 2, respectively. The resulting average rice amount received by a household is 98.77 kg (std. dev. = 19.51 kg; range from 78 to 130 kg) in Treatment 1 and 116.4 kg (std. dev. = 24.48 kg; range from 98 to 156 kg) in Treatment 2. As can be seen in the second column, the negative relative income effect when comparing Treatment 2 to Treatment 1 becomes more pronounced, both in absolute and in relative terms (now diminishing two-thirds of the Spanish improvement in absolute scores achieved under Treatment 1). From this, we infer that it is unlikely that the negative relative income effect on the part of the bottom 20% was simply a reaction to the higher transfer amounts in Treatment 2 in conjunction with leisure being a normal good.

In the third column, we test for heterogenous treatment effects depending on the gender of the intended recipient.[9] Interestingly, we find no differential treatment effect by gender of the intended recipient (typically one of the household heads).

Furthermore, we note that across the first three columns, individuals in bottom 20% households invested significantly more in Spanish in Treatment 1 than in the control group. This is reflective of the fact that the bottom 20% were constrained in their choice between foraging-farming and human capital investment, and our intervention facilitated their human capital investment by freeing resources. As argued in our discussion on a theoretical mechanism in Section 3.1, if social comparisons are a driving force underlying the negative relative income effect, then in the absence of any absolute income effect, we would not expect to find any differential effect for the top 80% households (TOP80) in Treatment 2 compared to Treatment 3. In the fourth column, we indeed see that the top 80% neither invested more

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[9] Note that although the gender of the intended recipient was randomized, we dropped households for which this information was not recorded.

in human capital under Treatment 3 than in the control group (no absolute income effect), nor was their human capital investment differentially affected by Treatment 2 compared to Treatment 3 (no relative income effect). In the fifth column, we run an analogous specification to the one in the third column by testing for heterogenous effects by gender of the intended recipient, and – again – find no differential effects.

The fact that the top 80% do not seem to have reacted to the variation in village-level inequality between Treatments 2 and 3, whereas the bottom 20% do, may be due to the different nature of rice transfers accruing to the top 80% in Treatments 2 and 3. In particular, the realized value of said transfers depends on whether the top 80% households planted the seeds. In Table 4, we shed light on whether the top 80% planted different proportions of the rice seeds in Treatment 2 compared to Treatment 3, and use as dependent variable said proportion (between 0 and 1) with only one observation per household, as the variable is defined only in the post-treatment year for the household head that received the transfer and was, thus, responsible for it. The first column indicates that 70% of the rice seeds were planted, and that this amount did not differ significantly under Treatment 2 compared to Treatment 3 (the omitted category). In the second column, we include a dummy variable for the bottom 20%, and find that the latter planted roughly the same proportion of seeds in Treatment 3 as the top 80%. This is invariant to the inclusion of further controls in the last two columns, in which we, again, fail to find any heterogenous effects by the gender of the intended recipient.

Wage-labor participation. As the largest returns to human capital investment for the Tsimane' are realized by working for an outsider (such as a logger, farmer, or cattle rancher), we re-run the regressions from Table 3, and use as dependent variable an indicator variable for non-zero income from wage labor, which characterizes the extensive margin of wage labor. The results are in Table 5, and line up with our findings for human capital investment. Focusing on the bottom 20%, we find a negative relative income effect that persists across the first three columns, and is invariant to the gender of the intended recipient in the household.

On the other hand, the zero findings from Table 3 for the top 80% carry over to Table 5

(cf. last two columns), including the insignificance of the interaction terms with the gender of the intended recipient. Furthermore, as in Table 3, the top 80% households were not more likely to participate in wage labor under Treatment 3 than in the control group. Conversely, the coefficient for the control group is negative (and significantly so in the second column) only for the bottom 20%, indicating that the bottom 20% were more likely to engage in wage labor under Treatment 1 than in the control group.

In Table 6, we scrutinize the intensive margin of wage labor. For this purpose, we run Poisson regressions in which the dependent variable is the number of days worked for an outsider over the period of one week. As can be seen in the first column, the bottom 20% worked significantly less following Treatment 2 than under Treatment 1: the implied negative relative income effect amounts to an economically significant reduction in working days by two thirds. Conversely, in the third column, we learn that the top 80% worked less under all three treatments compared to the control group, but not differentially so between Treatment 2 and Treatment 3. This is the opposite of what one would expect if the top 80% had been constrained to work for outsiders prior to receiving the transfers, which seems to hold only for the bottom 20%, as in the case of human capital investment, although we note that the respective coefficient on the control group in the first column is only marginally significant at the 19% level. The reduced precision of the estimate is partly a consequence of the relatively small sample size.

We yield similar results when running OLS regressions with an indicator for whether an individual generated income from wage labor only, conditional on non-zero income in both periods and non-zero wage-labor income in the pre-treatment year.[10] This intensive margin of wage labor, thus, captures any changes in the income composition of active workers. In the second column, we learn that the bottom 20% reduced their likelihood of being pure wage earners by 50 percentage points under Treatment 2, whereas they exhibit no change under Treatment 1. This difference is significant at the 10% level, and chimes with our

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[10] Our results are qualitatively robust to not truncating all wage-labor-income proportions below one. This is due to the fact that the actual proportions of wage-labor income over total income are either 0 or 1 for 77% and 67% of all observations in the second and fourth column of Table 6, respectively.

previous findings. Conversely, we find no effect for the top 80% in the fourth column, except that they – as already witnessed in the third column – reduced their activity in wage labor under all three treatments, whereas there was no change in the control group (the sum of the coefficients on $After_t$ and $Control_i \cdot After_t$ is not significantly different from zero).

In summary, we confirm a negative relative income effect for the bottom 20%, in that they reduced their exposure to outsiders through wage labor at the intensive margin. This reduction even surpasses that under Treatment 3, as the coefficient on $Treatment\ 2_i \cdot After_t$ is even more negative than that on $Treatment\ 3_i \cdot After_t$ in the first two columns. This is all the more striking, as the necessity of planting seeds for their full value realization in Treatment 3 constituted a direct incentive to stay on the farm and, thus, to not engage in wage labor, relative to our edible-rice transfers in Treatment 2. Lastly, we wish to point out a caveat attached to these findings insofar as our income data, unlike those on Spanish fluency, are flow data, and reflect income generated over one week sometime during the post-treatment period. That is to say, while any improvement in Spanish fluency that we measured reflects human capital accumulation over the entire period from before to after our treatments, income generated over one week after our treatments may not necessarily be representative of the entire post-treatment period.

Asset wealth and income from foraging-farming. We have previously hypothesized that the treatment-induced reduction in inequality rendered human capital investment less attractive for the bottom 20% households due to social comparisons and the fact that the latter group was exogenously made relatively better off in Treatment 2. For this to be a valid interpretation, overall effort should have decreased, rather than a mere substitution of human capital investment by foraging-farming activities. Such a substitution would be plausible, however, if foraging and farming became more attractive under Treatment 2. To explore this possibility, we next investigate whether the bottom 20% shifted their asset portfolios in any meaningful way, and whether they earned significantly more from foraging and farming after the treatment.

In Table 7, we re-run the same regressions as in Tables 3 and 5, and detect no differen-

tial impact of our treatments on changes in the value of total assets for any group. Most noticeably, however, both the bottom 20% and the top 80% held significantly more assets after our treatments than in the control group. While our transfers did not lead to any absolute income effect for human capital investment or wage-labor participation among the top 80%, indicating that our intervention was not pivotal for their decision to switch away from foraging-farming, the treatments did matter for the ability of the top 80% to invest in assets. Yet, this effect is not different between Treatments 2 and 3, further supporting that the top 80% exhibit no relative income effect whatsoever, unlike the bottom 20%.

Similarly, in Table 8, we find no differential treatment effects on income from foraging and farming, which we measure as an individual's income from the sale of forest and farm goods plus the average value of individual food consumption minus any food expenditures over one week. This definition serves to incorporate foraging-farming income in the form of consumption from own-farm production.

The evidence in Tables 7 and 8 does not suggest that foraging-farming became any more attractive for the bottom 20% under Treatment 2. Instead, it appears that this group reduced their investment in human capital while leaving their foraging-farming efforts unaltered.

3.3 Alternative Explanations and Robustness

To complete our analysis, we next discuss the robustness of the interpretation of our findings. We argue that the observed behavior of the bottom 20% households under Treatment 2 is due to social comparisons. 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. While we have attempted to tackle the identification issue of disentangling a relative income effect (due to social comparisons) from an absolute income effect (stemming from the mere transfer), it remains a challenge to properly characterize the social-comparisons channel. In particular, this challenge is due to the fact that asymmetric transfers, targeted at different parts of the

income distribution, are required to change the aggregate state of inequality in a village. Therefore, we need to make sure that the bottom 20% households indeed reacted to the actual transfers associated with reduced inequality, rather than the change in village-level inequality differentially affecting the bottom 20% vs. the top 80% households for reasons that were unrelated to the transfers.

We make a twofold argument as to why we deem this alternative to be unlikely. First, village-level differential impacts for the bottom 20% vs. the top 80% households would have to be centered on their being different along relevant dimensions. However, as we have seen in Table 1, the position in the deforestation distribution is, first and foremost, related to household size, but not to ability or other characteristics that would lead us to believe that these two groups should be differentially affected by village-level changes due to the treatment-induced reduction in inequality. Second, we can explicitly test whether our findings might be driven by any differential aggregate effects, e.g., price effects. Note, however, that such a channel should, in principle, not be relevant for the effect on human capital investment, because the price of schooling is zero, as the Bolivian government pays for the teachers.

For other village-level prices, in Table 9, we test whether they behaved differently across the three treatments, using the same specification as before at the village level (i.e., including village fixed effects). In the first two columns, we scrutinize the village selling prices of manioc and rice. Interestingly, we find that the selling price of rice increased in all three treatments compared to the control group; in fact, the price seems to have dropped in the control group, whereas it remained unchanged in the treatment villages. This demonstrates that our intervention may have had an impact on the price-setting behavior of the Tsimane' towards outsiders who purchase farm goods from them. Most importantly, we find no differential treatment effects. That is to say, none of the differences between any two treatments is statistically significant. In the last two columns, we turn to village buying prices for labor, namely average wages offered to the Tsimane' by outsiders. While wages have increased across all three treatments and the control villages, we, once again, find no differential

treatment effects. Thus, the opportunity cost of not studying Spanish and not working for outsiders increased, but not differentially so in our treatment villages. Therefore, village-level impacts on prices cannot explain the previously documented negative relative income effect in Treatment 2.

Another possibility that we consider is that pre-existing risk-sharing arrangements, possibly between the bottom 20% and top 80% households within the villages, may have been differentially affected by the treatments. To test whether the insurability of idiosyncratic shocks varied at the household level, we run risk-sharing regressions in the spirit of Cochrane (1991), Mace (1991), and Townsend (1994):

$$
\begin{aligned}
\Delta \ln \bar{c}_{hh} - \Delta \ln \bar{c}_v \;=\; & \beta_1 \Delta \ln \overline{Income}_{hh} + \beta_2 \Delta \ln \overline{Income}_{hh} \cdot Treatment\ 1_v \\
& + \beta_3 \Delta \ln \overline{Income}_{hh} \cdot Treatment\ 2_v \\
& + \beta_4 \Delta \ln \overline{Income}_{hh} \cdot Treatment\ 3_v + \mu_v + E_{hh},
\end{aligned}
\tag{2}
$$

where $\bar{c}_{hh}$ and $\bar{c}_v$ denote the average value of one week's consumption per household member at the household level and at the average village level (excluding household hh), respectively, and $Income_{hh}$ is the average sum of earnings from the sale of forest and farm goods, wage labor, and barter trade earned over one week by all earners in household hh. Furthermore, we implicitly assume a unit coefficient of aggregate consumption (see left-hand side) to avoid a bias of the coefficient on aggregate consumption due to a possible correlation with the error term (Mace (1991)).

The results are in Table 10. Risk sharing implies a zero coefficient on $\Delta \ln \overline{Income}_{hh}$, which is what we find in the first column. This is unaltered after including village fixed effects in the second column. In the third column, we include interactions with all three treatments, none of which is significant. Most importantly, none of the differences between any two treatments is statistically significant, implying that risk-sharing arrangements did not differ across the three treatments. There are, however, at least two caveats attached to these conclusions. First, our inability to reject perfect risk sharing should not be overstated,

as it is subject to the nature of the consumption data, which may be noisy. Second, we have only two time periods and, thus, cannot test for any development of the quality of risk sharing within villages.

In the last column of Table 10, we consider a final robustness check. Namely, we limit the sample to households that did not use chainsaws to clear forest so as to make the village distribution of area of forest cleared more independent of any factors that may be correlated with a household's integration with the outside economy (where chainsaws could be acquired). Our (non-)findings from the third column carry over.

Finally, we re-run the main regression specifications from Tables 3, 5, 7, and 8 for the bottom 20% and top 80% households in Tables A.1 and A.2, respectively. All previous findings for human capital investment, wage-labor participation,[11] total assets, and foraging-farming income are robust to limiting the sample to households without chainsaws.

4   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 some of these claims by varying the distribution of income shocks in 53 villages of a foraging-farming society, the Tsimane' of the Bolivian Amazon. We measure the impacts on individual-level outcomes, most notably human capital investment (in the form of learning Spanish), and present evidence that is suggestive of social comparisons driving our results.

We find that poorer households invested less in human capital when transfers were associated with a reduction in income inequality than when they were associated with income-distribution neutrality. Conversely, we find no significant effect for the remaining, richer households that were not the main beneficiaries of the inequality-reducing treatment. This

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[11] The only difference that we wish to note is that, most likely due to the drop in sample size, the coefficient on $Treatment\ 2_i \cdot After_t$ in the second column of Table A.1 is only significant at the 19% level.

is all the more striking, as learning Spanish is the entry ticket to the outside labor market and, thus, the only viable alternative to foraging and farming that offers the prospect of significantly higher future income and, eventually, out-migration. Our experimental findings attest to the extent to which social-comparison dynamics elucidate such investment decisions even among normatively egalitarian villagers. In this regard, our paper potentially offers valuable insights for the debate on labor-supply effects and growth implications of income redistribution through transfer programs, by suggesting that simple income transfers to all households may be more conducive to encourage human capital investment among the poor than redistributional policies.

This paper constitutes a first attempt at relating village-level inequality to individual-level drivers of development, such as human capital investment, in a field experiment. Our experimental design is confined to only one type of inequality, namely that of (edible-rice/in-kind-income) transfers (see Bandiera et al. (2013) for other types of transfers, e.g., transfers of skills and assets). This leaves open the important question of whether our results hold for other kinds of inequality as well, e.g., inequality of opportunities, which motivates the development of new experimental designs to enable corresponding tests in future research.

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5 Tables

Table 1: **Summary Statistics by Income Groups (in Pre-treatment Year)**

	Mean	Difference to next-highest category				
	TOP40	BOT60	BOT20	BOT10	N	p-value (T40 = B10)
Cleared forest (in ha) per HH	1.828***	-0.897***	-0.333***	-0.337***	690	0.000
	(0.107)	(0.084)	(0.055)	(0.051)		
HH consumption in $ in 1 week	57.711***	-15.507***	9.374	-12.145	690	0.000
	(4.773)	(4.031)	(7.985)	(7.925)		
Household size	7.063***	-1.291***	-0.321	-0.367	690	0.000
	(0.177)	(0.247)	(0.317)	(0.390)		
HH used chainsaw $\in \{0, 1\}$	0.224***	-0.114***	-0.045	-0.052*	690	0.000
	(0.037)	(0.031)	(0.034)	(0.026)		
Expenditures for all goods in $	10.130***	-3.295**	-1.191	0.019	1,547	0.005
	(1.502)	(1.399)	(1.080)	(1.657)		
Foraging-farming income in $	23.710***	-5.195*	-4.230	1.726	1,542	0.038
	(2.841)	(2.641)	(3.453)	(3.879)		
Sales income in $	18.129***	-5.515**	-6.087**	1.728	1,546	0.006
	(2.790)	(2.522)	(2.416)	(2.958)		
Wage-labor income in $	4.369***	-0.954	0.150	0.254	1,546	0.594
	(0.689)	(0.669)	(0.905)	(1.081)		
Number of days worked	0.723***	-0.033	-0.015	0.203	1,546	0.402
	(0.097)	(0.095)	(0.160)	(0.207)		
Barter income in $	2.353***	-0.369	0.256	0.132	1,539	0.981
	(0.350)	(0.449)	(0.783)	(0.580)		
Animal wealth in in $100	0.576***	-0.230**	-0.116	-0.098	1,552	0.000
	(0.112)	(0.113)	(0.110)	(0.085)		
Traditional assets in $100	0.590***	-0.070	0.034	-0.029	1,552	0.328
	(0.065)	(0.047)	(0.078)	(0.090)		
Modern assets in $100	1.971***	-0.103	0.169	-0.307	1,552	0.252
	(0.152)	(0.194)	(0.204)	(0.285)		
Spanish-speaking ability (0 – 2)	0.576***	0.012	0.041	0.036	2,047	0.257
	(0.069)	(0.037)	(0.079)	(0.086)		
Spanish-reading ability (0 – 2)	0.318***	-0.073**	0.054	-0.015	2,047	0.556
	(0.042)	(0.033)	(0.054)	(0.070)		
Math score (0 – 4)	0.999***	-0.073	0.074	-0.011	2,007	0.935
	(0.097)	(0.070)	(0.124)	(0.151)		
Years of schooling	1.692***	-0.069	0.311*	-0.259	2,939	0.922
	(0.126)	(0.094)	(0.172)	(0.210)		
Financially constrained $\in \{0, 1\}$	0.287***	0.009	0.032	0.027	1,524	0.183
	(0.031)	(0.031)	(0.038)	(0.054)		

Notes: The sample is limited to individuals (non-attriters) in the pre-treatment year. Summary statistics are shown as outputs from regressions of the variable in question on three income-percentile indicator variables (BOT60, BOT20, and BOT10), so the estimated constant corresponds to individuals in TOP40 households (abbreviated as HHs). The last column indicates the p-values from two-sided difference-in-means tests between TOP40 and BOT10 households by testing whether the sum of the coefficients on BOT60, BOT20, and BOT10 is different from zero. The upper panel presents summary statistics for household-level variables, and the lower panel for individual-level variables. Expenditures and all income variables are measured over one week. Foraging-farming income is defined as income from the sale of forest and farm goods plus the average value of individual food consumption minus any food expenditures over one week. 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. Individuals are classified as financially constrained if they reported to be unable to borrow 100 bolivianos at the time of the interview. Robust standard errors (clustered at the village level) are in parentheses.

Table 2: **Determinants of Income (in Pre-treatment Year)**

	Total income in $ in 1 week			
Spanish ability in speaking and reading (0 – 4)	2.878***	4.046***	1.817**	2.586**
	(0.806)	(0.927)	(0.888)	(1.193)
Male			16.567***	14.009***
			(2.443)	(2.431)
Household head			6.946***	4.618*
			(2.574)	(2.433)
Age			0.123*	0.056
			(0.068)	(0.074)
Household size			0.395	0.229
			(0.308)	(0.280)
Total assets in $100				1.546**
				(0.649)
Math score (0 – 4)				-1.605*
				(0.829)
Years of schooling				0.112
				(0.518)
Constant	12.986***			
	(1.802)			
Village FE	N	Y	Y	Y
N	1,382	1,382	1,382	1,382

Notes: The sample is limited to individuals in the pre-treatment year. The dependent variable is the sum of earnings from the sale of forest and farm goods and from wage labor earned over one week by individuals in the pre-treatment year. Spanish ability is measured as the sum of the two scores for speaking and reading skills, each of which ranges from 0 to 2, giving a total range from 0 to 4. Total assets is the sum of an individual's animal wealth, traditional assets, and modern assets. Total income and assets are winsorized at the 1st and 99th percentiles. Robust standard errors (clustered at the village level) are in parentheses.

Sample	Spanish ability in speaking and reading $(0-4)$				
	BOT20	BOT20, similar amounts	BOT20	TOP80	TOP80
After	0.536***	0.611***	0.571***	0.448***	0.468***
	(0.086)	(0.164)	(0.086)	(0.052)	(0.049)
Treatment 1				-0.047	-0.019
				(0.066)	(0.062)
Treatment 2	-0.237*	-0.402**	-0.277**	-0.129	-0.132
	(0.123)	(0.169)	(0.121)	(0.081)	(0.090)
Treatment 3	-0.067	-0.142	-0.118		
	(0.123)	(0.186)	(0.146)		
Control	-0.280**	-0.355*	-0.316**	-0.029	-0.049
	(0.126)	(0.188)	(0.127)	(0.091)	(0.089)
After · Male rec.			-0.062		-0.032
			(0.167)		(0.039)
Treatment 1 · Male rec.					-0.056
					(0.060)
Treatment 2 · Male rec.			0.092		-0.001
			(0.208)		(0.078)
Treatment 3 · Male rec.			0.099		
			(0.226)		
Individual FE	Y	Y	Y	Y	Y
N	844	524	826	3,272	3,246

Notes: All regressions include individual fixed effects. The sample is limited to individuals with two available observations (pre- and post-treatment years). The dependent variable is Spanish ability measured as the sum of the two scores for speaking and reading skills, each of which ranges from 0 to 2, giving a total range from 0 to 4. Whenever available, Male recipient is an indicator for whether the intended recipient in a treated household was male. The sample in the second column is limited to the four smallest and four largest villages (out of 13) in Treatments 1 and 2, respectively, yielding 35 (instead of 53) clusters. Robust standard errors (clustered at the village level) are in parentheses.

Table 4: **Rice Seeds Planted in Treatments 2 and 3**

Sample	Proportion of rice seeds planted ∈ [0, 1]			
	TOP80	TOP80	TOP80	TOP80
Treatment 2	-0.119	-0.111	-0.098	-0.097
	(0.101)	(0.101)	(0.100)	(0.100)
BOT20		0.039	0.048	0.050
		(0.059)	(0.102)	(0.100)
Treatment 2 · Male rec.			-0.021	-0.028
			(0.061)	(0.058)
BOT 20 · Male rec.			0.019	0.018
			(0.172)	(0.164)
Male rec.			0.039	0.092
			(0.056)	(0.058)
Age at baseline			-0.000	0.000
			(0.002)	(0.002)
Household size at baseline			0.014*	0.015*
			(0.007)	(0.007)
Total assets in $100 at baseline				-0.017**
				(0.008)
Years of schooling at baseline				0.000
				(0.016)
Constant	0.700***	0.692***	0.593***	0.585***
	(0.047)	(0.048)	(0.088)	(0.107)
N	252	252	252	252

Notes: The sample is limited to post-treatment observations for individuals in Treatments 2 and 3 with responsibility over the usage of the rice seeds, namely female or male household heads, where the gender of the recipient of the rice seeds was random. The dependent variable is the proportion of rice seeds that the respective individual reported to have planted, rather than stored, sold, or given away. Total assets is the sum of an individual's animal wealth, traditional assets, and modern assets. Robust standard errors (clustered at the village level) are in parentheses.

Table 5: **Impact on Working for Outsiders – Extensive Margin**

Sample	Any income from wage labor $\in \{0, 1\}$				
	BOT20	BOT20, similar amounts	BOT20	TOP80	TOP80
After	0.098	0.357*	0.120	-0.023	-0.069
	(0.084)	(0.186)	(0.082)	(0.030)	(0.048)
Treatment 1				-0.043	-0.016
				(0.050)	(0.071)
Treatment 2	-0.161*	-0.357*	-0.164*	-0.041	-0.020
	(0.090)	(0.188)	(0.094)	(0.039)	(0.059)
Treatment 3	-0.125	-0.384*	-0.120		
	(0.103)	(0.195)	(0.113)		
Control	-0.107	-0.366*	-0.128	0.023	0.069
	(0.089)	(0.188)	(0.088)	(0.034)	(0.051)
After · Male rec.			-0.037		0.089
			(0.159)		(0.056)
Treatment 1 · Male rec.					-0.052
					(0.069)
Treatment 2 · Male rec.			-0.008		-0.034
			(0.172)		(0.067)
Treatment 3 · Male rec.			-0.030		
			(0.198)		
Individual FE	Y	Y	Y	Y	Y
N	702	524	690	2,392	2,376

Notes: All regressions include individual fixed effects. The sample is limited to individuals with two available observations (pre- and post-treatment years). The dependent variable is an indicator variable for whether an individual received *any* income from wage labor, i.e., from working for outsiders. Whenever available, Male recipient is an indicator for whether the intended recipient in a treated household was male. The sample in the second column is limited to the four smallest and four largest villages (out of 13) in Treatments 1 and 2, respectively, yielding 35 (instead of 53) clusters. Robust standard errors (clustered at the village level) are in parentheses.

Table 6: **Impact on Working for Outsiders – Intensive Margin**

Sample	Number of days worked (Poisson) BOT20	Wage labor only $\in \{0, 1\}$ BOT20, non-zero income	Number of days worked (Poisson) TOP80	Wage labor only $\in \{0, 1\}$ TOP80, non-zero income
After	0.700	-0.000	-0.331**	-0.314**
	(0.509)	(0.254)	(0.164)	(0.120)
Treatment 1			-0.238	-0.033
			(0.336)	(0.154)
Treatment 2	-1.098*	-0.500*	-0.022	0.024
	(0.604)	(0.289)	(0.324)	(0.135)
Treatment 3	-1.083*	-0.143		
	(0.605)	(0.270)		
Control	-0.716	-0.222	0.446**	0.160
	(0.547)	(0.291)	(0.201)	(0.170)
Individual FE	Y	Y	Y	Y
N	172	112	538	380

Notes: All regressions include individual fixed effects. The samples in the first and third columns are limited to individuals with two available observations (pre- and post-treatment years), at most one of which is zero (Poisson regression). The samples in the second and fourth columns are limited to individuals with non-zero total income from wage labor and foraging-farming in both pre- and post-treatment years, and non-zero income from wage labor in the pre-treatment year. The dependent variable in the first and third columns is the number of days worked for an outsider over one week. The dependent variable in the second and fourth columns is an indicator variable for whether an individual's total income comprised *only* income from wage labor, i.e., from working for outsiders. Robust standard errors (clustered at the village level) are in parentheses.

Table 7: **Impact on Total Asset Wealth**

Sample	BOT20	BOT20, similar amounts	BOT20	TOP80	TOP80
		ln(1+Total assets)			
After	0.198	-0.019	0.274*	0.212*	0.220*
	(0.191)	(0.144)	(0.137)	(0.120)	(0.120)
Treatment 1				0.123	0.253
				(0.168)	(0.278)
Treatment 2	0.017	0.257	0.043	-0.079	-0.120
	(0.219)	(0.200)	(0.179)	(0.148)	(0.144)
Treatment 3	0.142	0.359	-0.099		
	(0.313)	(0.288)	(0.357)		
Control	-0.514**	-0.297	-0.590***	-0.439***	-0.447***
	(0.253)	(0.220)	(0.215)	(0.154)	(0.154)
After · Male rec.			-0.128		-0.016
			(0.296)		(0.203)
Treatment 1 · Male rec.					-0.248
					(0.346)
Treatment 2 · Male rec.			-0.114		0.081
			(0.328)		(0.217)
Treatment 3 · Male rec.			0.542		
			(0.490)		
Individual FE	Y	Y	Y	Y	Y
N	702	524	690	2,391	2,375

Notes: All regressions include individual fixed effects. The sample is limited to individuals with two available observations (pre- and post-treatment years). The dependent variable is the log of the sum of an individual's animal wealth, traditional assets, and modern assets. Whenever available, Male recipient is an indicator for whether the intended recipient in a treated household was male. The sample in the second column is limited to the four smallest and four largest villages (out of 13) in Treatments 1 and 2, respectively, yielding 35 (instead of 53) clusters. Robust standard errors (clustered at the village level) are in parentheses.

Sample	ln(1+Foraging-farming income)				
	BOT20	BOT20, similar amounts	BOT20	TOP80	TOP80
After	0.130	0.598**	0.139	-0.114	-0.229
	(0.199)	(0.257)	(0.397)	(0.165)	(0.160)
Treatment 1				-0.108	-0.074
				(0.218)	(0.225)
Treatment 2	0.033	-0.208	-0.309	-0.194	-0.015
	(0.325)	(0.472)	(0.491)	(0.268)	(0.255)
Treatment 3	-0.620**	-1.088***	-0.374		
	(0.262)	(0.309)	(0.430)		
Control	-0.407	-0.875***	-0.415	-0.369	-0.255
	(0.256)	(0.304)	(0.428)	(0.227)	(0.223)
After · Male rec.			-0.015		0.249
			(0.516)		(0.208)
Treatment 1 · Male rec.					-0.097
					(0.375)
Treatment 2 · Male rec.			0.469		-0.400
			(0.601)		(0.321)
Treatment 3 · Male rec.			-0.635		
			(0.637)		
Individual FE	Y	Y	Y	Y	Y
N	700	522	688	2,384	2,368

Notes: All regressions include individual fixed effects. The sample is limited to individuals with two available observations (pre- and post-treatment years). The dependent variable is the log of an individual's income from the sale of forest and farm goods plus the average value of individual food consumption minus any food expenditures over one week. Whenever available, Male recipient is an indicator for whether the intended recipient in a treated household was male. The sample in the second column is limited to the four smallest and four largest villages (out of 13) in Treatments 1 and 2, respectively, yielding 35 (instead of 53) clusters. Robust standard errors (clustered at the village level) are in parentheses.

Impact on (Village-level) Macro Outcomes

	ln(Price of manioc)	ln(Price of rice)	ln(Wage)	ln(Wage incl. lunch)
After	-0.126	-0.612***	0.208***	0.269***
	(0.10)	(0.09)	(0.07)	(0.09)
Treatment 1	0.604*	0.797***	0.109	0.143
	(0.31)	(0.21)	(0.09)	(0.10)
Treatment 2	0.190	0.499***	0.082	0.085
	(0.25)	(0.16)	(0.11)	(0.11)
Treatment 3	0.222	0.449**	0.085	0.018
	(0.18)	(0.21)	(0.12)	(0.11)
Village FE	Y	Y	Y	Y
N	84	94	92	92

Notes: All regressions include village fixed effects. In each column, the sample is limited to villages with two available observations (pre- and post-treatment years). Prices in the first two columns refer to village selling prices. Wages in the last two columns are for one day. Robust standard errors (clustered at the village level) are in parentheses.

Table 10: **Impact on Village-wide Risk Sharing**

Sample	$\Delta \ln \bar{c}_{hh} - \Delta \ln \bar{c}_v$			
	All HHs	All HHs	All HHs	No chainsaw
$\Delta \ln \overline{Income}_{hh}$	0.013	0.021	0.047	0.056
	(0.022)	(0.026)	(0.065)	(0.073)
$\Delta \ln \overline{Income}_{hh} \cdot$ Treatment 1			-0.073	-0.088
			(0.081)	(0.102)
$\Delta \ln \overline{Income}_{hh} \cdot$ Treatment 2			-0.072	-0.083
			(0.083)	(0.092)
$\Delta \ln \overline{Income}_{hh} \cdot$ Treatment 3			0.004	0.015
			(0.074)	(0.080)
Constant	0.028			
	(0.025)			
Village FE	N	Y	Y	Y
N	420	420	420	356

Notes: Observations are at the household level (one observation per household), and the sample is limited to households with two non-zero consumption *and* income observations, enabling us to take differences between post- and pre-treatment years. $\bar{c}_{hh}$ and $\bar{c}_v$ denote the average value of one week's consumption per household member at the household level and at the average village level (excluding household hh), respectively. $Income_{hh}$ is the average sum of earnings from the sale of forest and farm goods, wage labor, and barter trade earned over one week by all earners in household hh. Robust standard errors (clustered at the village level) are in parentheses.

Supplementary Appendix (Not for Publication)

A   Supplementary Tables

Table A.1: **Robustness – BOT20 Households without Chainsaws**

	Spanish $(0-4)$	Wage labor $\in \{0,1\}$	$\ln(1+\text{Assets})$	$\ln(1+\text{FF income})$
Sample	BOT20	BOT20	BOT20	BOT20
After	0.565***	0.052	0.188	0.123
	(0.080)	(0.072)	(0.195)	(0.208)
Treatment 2	-0.265**	-0.106	0.064	0.085
	(0.121)	(0.080)	(0.230)	(0.370)
Treatment 3	-0.099	-0.080	0.137	-0.676**
	(0.130)	(0.095)	(0.325)	(0.285)
Control	-0.309**	-0.060	-0.506*	-0.418
	(0.123)	(0.079)	(0.253)	(0.268)
Individual FE	Y	Y	Y	Y
N	806	672	674	670

Notes: All regressions include individual fixed effects. The sample is limited to individuals in BOT20 households with two available observations (pre- and post-treatment years). Furthermore, the sample is limited to households that did not use any chainsaws to clear forest before the pre-treatment year, which is when we determined the income groups according to the area of forest cleared. The four columns correspond to the regressions from the first column of Tables 3, 5, 7, and 8, respectively. Robust standard errors (clustered at the village level) are in parentheses.

Table A.2: **Robustness – TOP80 Households without Chainsaws**

Sample	Spanish $(0-4)$ TOP80	Wage labor $\in \{0,1\}$ TOP80	ln(1+Assets) TOP80	ln(1+FF income) TOP80
After	0.403***	-0.008	0.237*	-0.246
	(0.055)	(0.026)	(0.129)	(0.169)
Treatment 1	-0.020	-0.044	0.151	0.047
	(0.076)	(0.036)	(0.217)	(0.218)
Treatment 2	-0.079	-0.059	-0.126	-0.046
	(0.089)	(0.037)	(0.156)	(0.302)
Control	-0.080	0.008	-0.462***	-0.175
	(0.097)	(0.033)	(0.163)	(0.238)
Individual FE	Y	Y	Y	Y
N	2,568	1,946	1,950	1,938

Notes: All regressions include individual fixed effects. The sample is limited to individuals in TOP80 households with two available observations (pre- and post-treatment years). Furthermore, the sample is limited to households that did not use any chainsaws to clear forest before the pre-treatment year, which is when we determined the income groups according to the area of forest cleared. The four columns correspond to the regressions from the fourth column of Tables 3, 5, 7, and 8, respectively. Robust standard errors (clustered at the village level) are in parentheses.