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Tsimane' Earnings

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Chapter 7

Tsimane' Earnings

Summary: *Aims*: *i]* describe levels and trends in nominal and inflation-adjusted (real) cash earnings from wage labor and sales, *ii]* identify predictors of wage labor and retail, *iii]* assess links between earnings from wage labor and sales, and *iv]* estimate the effect of the foreign currency exchange rate on earnings. *Methods*: 2000-10 cross-sectional and panel surveys are used for the week and the 2 weeks before the interview. Reported earnings had last-digit rounding errors. Forward telescoping bias appeared in retail but not in wage labor data. *Findings-levels*: Wage labor was done by men of prime working age, retail by women and men of almost all ages. Rice, plantains, and wildlife ranked at the top of articles sold in frequency and value. From sales, a woman or man earned $0.8-2.1/day while from paid work men earned $4.7-5.8/day. The estimates put the average income earner at or above the border of the international poverty line. Most men worked for one employer and most people sold one good. Between seasons people did not switch employers but changed the goods sold. *Findings-trends*: *Wages*. During 2000-10 hiring by cattle ranchers, Tsimane', and the government grew while hiring by other employers fell (e.g., logging firms). The share of people without wage earnings rose by 2.0-3.5%/year while the chance of having no wage earnings rose by ~2 percentage points/year. Real wage earnings and daily wages rose by 8% and 3.9%/year. The rise in real earnings came from a rise in the number of days worked more than from a rise in daily wages. Fewer men joined the labor force but those who joined worked and earned more. Body weight, schooling, and survey year predicted joining the labor force and real earnings. Schooling had a larger impact on wage earnings for a man in a village with higher levels of schooling. *Sales*. The share of people who did not sell rose by 3.3-5.3%/year while the number of goods sold declined by 2.3-2.6%. The share of people selling during both weeks before the interview also fell, as did the array of articles sold. Retail real earnings fell by 0.7-6.4%/year. *Wages and sales*. Wage labor and retail functioned without much overlap. Increasing wage earnings by 100% resulted in only 3.1-6.2% lower retail earnings. *Currency exchange rate*. A currency depreciation last year raised current retail earnings but did not change wage earnings.

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In backland economies, as in industrial economies, cash income tells us about success. Putting aside the worth of goods produced for a person's own consumption, cash income tells us where people stand in the scale from the extreme of misery, squalor, and lean penury to its antipode of opulence, and -- when measured in time -- can show if living conditions are improving. But more so than in industrial economies, cash income in encased rural economies also speaks to the strength of ties to the mercantile world. No cash, no ties, most likely. Beyond the obvious point that cash income can point to the number of days worked or to the traffic of goods sold, or that it can show what people can buy, cash income is the keyhole to eye how national happenings in the metropolis bestride choices of those in economic exile. One thinks amiss that because people forage in a sequestered corner of world, marry each other, keep outsiders at bay, and craft what they consume, they must be *ipso facto* autarkic. As we shall see, cash income dismantles the belief -- not just by establishing the pedestrian statistics of days worked for wages or the menagerie of goods sold, but also by showing how cash income swings along with the country's foreign currency exchange rate, defined as the amount of Bolivian currency (*bolivianos*) needed to buy a USA dollar. The finding is intriguing and unsurprising. It is intriguing because Tsimane' have one of the most veiled economies in the world, at least when measured by the calories bought in the market (Henrich et al., 2010). How could events in a country's maw reach people far away in so sheltered an economy? And it is unsurprising because Tsimane', having dealt with foreigners for centuries (Chapter 3), have trodden paths to the outside world many times before.

In this chapter I deal with income in the form of cash earnings from wage labor and from the sale of crops, of domesticated animals, and of wildlife. Earnings has near neighbors I put aside because I treat them in other chapters. For now, I leave aside cash from government programs that pay the aged, school children, or expectant mothers. I do so because I have little to say about the programs, the programs having landed late, mid-way, or at the end of our study[i]. Tsimane' forge bonds with outsiders not just through paid work and retail[ii], but also through barter, a subject also dealt with in a later chapter. Of little value by itself or when saved, cash income matters when invested or when spent to buy the useful and the *outré* baubles of the market. As with barter, I examine cash expenditures later in the book. For now, I care about how people get the cash they will eventually spend, save, invest, or waste.

The goal of the chapter is modest: to limn in detail levels and trends in the number of days worked for wages, in the type of goods sold, and in the cash earned from wage labor and retail. At the end, I detour briefly to probe how Bolivia's foreign currency exchange rate affects cash earnings.

Methods of data collection on earnings

As with most topics broached during interviews, surveyors addressed questions about earnings to people 16 years of age or older, but they also surveyed the young if the young headed a household. We sometimes see in the clean data the unwed young saying they had earned cash. These were children or youth who told surveyors about selling a few eggs, a chicken, a duck they owned, or a handful of fruits to a travelling trader who showed up in the village, their answers finding their way into the screened data. I drop these cases from the analysis because they were rare[iii], because wage earners are adults, and because the young, relying upon -- yet helping -- their parents, are more likely to work in the fields and go to school than to labor for wages or to sell goods. Wife and husband keep earnings and much else apart, so we asked each one

separately about the number of days worked for cash, and about earnings, from wage labor and from the sale of crops, wildlife, domesticated animals, and animal products.

Since most yearly surveys happened during the rainless season (June-September), and we asked about cash income for the two weeks before the interview, most of what we know about cash income refers to the same slim slice of a year. To gather information on earnings we asked about earnings for the past seven days, and, having finished with those questions, we asked about earnings for the past 8-14 days. Over many years and with a large sample, mean earnings for the previous seven days and mean earnings for the previous 8-14 days should be roughly the same, much in the same way the mean number of teaspoons of sugar used with coffee yesterday and the day before yesterday should be about the same, again when measured over-and-over again with many people. By splitting the recall periods in two, we can pool answers to get blanket measures of earnings for the past two weeks, but we can also compare answers about two abutting weeks. The comparison allows us to see blots from omissions and from telescoping bias -- the penchant to move events that took place farther back in time into the more recent past (forward telescoping bias) or the penchant to push recent events farther back in time (backward telescoping bias)[iv]. When tallied over many years, mean earnings, mean days worked, or the mean number of goods sold should be (almost) the same for the week before and for the two weeks before – unless respondents forgot or misremembered when things happened. Depending on their form, depth, and ubiquity, the faults stain the data and analysis.

Other than restricting the recall period, we did not set bounds when asking about employers or sales. We did this to catch the diapason of answers about jobs held and wares sold. If employment or sales happened during the 14 days before the interview, people could acknowledge as many employers and goods as they remembered or as they wished to share with us. The event jotted was the employer in one case, the good sold in the other. Thus, a person could have two or more records in the module on wages if he -- wage earners were usually men, as we shall see -- had more than two employers; for each employer we wrote the number of days the man had worked and his total earnings. So too with sales. A person retailing three different goods during the past seven days, and another three goods during the past 8-14 days would have six events recorded in the sales module, three for each week. With wage earnings, as with sales, we have instances mentioned in Chapter 4 of how access to raw data allows analyses undoable with clean data since the clean data clubs many events to one blunt yearly statistic for each person. For instance, in the clean data, the sale of six different goods -- three sold during the seven days before the interview, and three sold during the 8-14 days before the interviewed -- would be averaged to two total sums: one sum for each of the two previous weeks. Yearly totals hide the number, type, and earnings from each sale. Helpful when examining some time trends, abridged yearly statistics nevertheless hinder our deftness at seeing the filigree beneath changes in yearly totals.

One admonition. Among Tsimane', as among people in other rural societies, workers and sellers get paid with cash or with goods. In our surveys, the module on sales tells us about the amount of cash a retailer received from the transaction. A deal in which a Tsimane' supplied goods and got paid with goods went into the module on barter, examined in a later chapter, not in the module on sales, examined here. The module on sales tells us about the amount of cash coming in, which is a way of engaging with the market. Regrettably, not to so with the module on wages. When asking about payment received from wage labor, we were remiss by neglecting to ask if rewards came in cash or in wares. The cavalier approach is not a problem if workers can swap with ease good for cash in a village, but this is doubtful in backwoods settlements

where one cannot do much with cash. Thus, using our measure of wage earnings as a telltale of engagement with the market is partly misleading, partly accurate.

Raw data sets: Wage earnings and sales

As before, when probing earnings or sales I rely on raw and burnished data from the yearly longitudinal study (2002-2010) and from the baseline (2008) of the randomized-controlled trial. In both cases we retrieved information from people in households. Unlike previous chapters, in this one I also use two so far untapped raw data sets: *(i)* three early yearly household-level surveys from 2000, 2001, and 2002 and *(ii)* a five-quarter panel study (May 2002-August 2003) done as part of the yearly longitudinal study of individuals (2002-2010). By raw I mean that the information lies unfiltered by demographic criteria, such as the respondent's age, or by outlying values. In the analysis with raw data I include very young or very old people if they made money from wage labor or from sales, and I include outlying values if they seem realistic[v]. Unlike the clean data for public use, raw data allows one to pinpoint the type of employer and the type of good sold.

The household survey of 2000 was part of a stand-alone cross-sectional study in 58 villages. The module on wage earnings and sales included about nine households in each village[vi], with households chosen by their availability and willingness to take part in the study. The yearly surveys of 2001 and 2002 were part of a randomized-controlled trial in 37 of the 58 villages of the 2000 household-level study; data from the 2001 survey served as a baseline and data from the 2002 survey served as an end-line to judge change. To answer survey questions about the 2001-2002 trial, we picked at random about 10 households in each village (SD=3.7), and in each household we picked at random the wife or the husband to answer survey questions on behalf of the household. Differences in the sampling approach for the 2000 and for the 2001-2002 study means that the two samples did not fully overlap, so I treat them as independent.

In unpublished analysis we found that the 2001-2002 trial did not change cash income, wildlife conservation, or anthropometric measures. Since the trial did not change outcomes, the trial's baseline and end-line data are included in this chapter. This decision contrasts with the decision of how to use data from the randomized-controlled trial of 2008-2009. Recall from Chapter 4 that the interventions in the 2008-2009 randomized-controlled trial changed outcomes by the end of the trial (2009) (Bauchet et al., 2019; Undurraga et al., 2016). For this chapter it makes sense to drop end-line data from the 2008-2009 trial because keeping it would add noise.

The yearly household surveys of 2000, 2001, and 2002 bring value because they lengthen the time to spot changes in the Tsimane' economy, and the quarterly surveys from May 2002 until August 2003 bring value because they allow us to see how employment and retail changed between quarters and seasons. In earlier chapters we saw information from the quarterly surveys, but that information came packaged as two (yearly) averages for each adult, with each average put into the longitudinal corpus as yearly data for an adult for 2002 or for 2003. In this chapter I unbundle the yearly average for 2002 and 2003 to see wage earnings and sales during each quarter and season.

Table 7.1 has a summary of the surveys used to study earnings from wage labor (part A) and sales (part B).

Insert Table 7.1

Raw yearly data on wage earnings: Scope and quality.

[i] Sample sizes. In Table 7.1, section A, I provide summary statistics of the samples to study wage earnings. For brevity, I sometimes use the word entity to cover both households and individuals, as in "the number of entities surveyed during 2000-2010 was such and such".

In the early years (2000-2002) of the study we gathered aggregate data for households about days worked, earnings, and type of employment, but we asked about the topics in an inconsistent way. In these three yearly surveys, we asked one of the household heads to reckon the number of days people in their household had worked and the earnings from wage labor from that work, but instructions on who to include in the tally differed. In the 2000 survey we asked a household head to tell us about the total number of days worked, wage earnings, and sales for all people dwelling in the household at the time of the survey, whereas in the household surveys of the randomized-controlled trial of 2001-2002 we asked the household head to tell us about the total number of days worked and wage earnings for the household heads, not for other wage earners in the household, whether children or adults. Changes between 2000 and 2001-2002 in how we asked about wage labor could explain some of the differences between years seen later.

In 2002 we started asking each adult in the household about days worked and wage earnings, rather than asking one household head to tally a total for the household. The sample size of households or people of Table 7.1, section A, includes the total number of entities in the module on wages earnings even if the household or person had earned no cash from wage labor. These number reflect the upper bound of the sample. For finer-grained ratable outcomes about days worked, earnings, or employment, the sample sizes are smaller or different from the upper bound owing either to missing information for some outcomes (smaller samples) or owing to valid repeated measures for other outcomes (larger samples).

An example should clear up why sample sizes differ for different topics in the study of wage labor (Figure 7.1). In 2000 we interviewed 508 households about wage labor (Table 7.1, section A), even if they had no earnings from wage labor. Of the 508 households, 488 had complete information on jobs, days worked, and wage earnings – even if some of the outcomes had values of zero (Table 7.6, column [4]). For instance, a household without wage earners would have values of zero for job type, days worked, and wage earnings, but would have been part of the sample of 488 households since the respondent would have answered all the questions about wage labor. The difference between the sample of all households interviewed about wage labor (508) and the sample of households with complete information on all aspects of wage labor (488) leaves us with 20 households with missing information on some topics. Of the 488 households with full information on wage earnings – even if the information had nothing but values of zero for a household -- only 190 households had worked for wages and had information on both: *(i)* employment type, days worked, and earnings for the seven days before the interview and *(ii)* employment type, days worked, and earnings for the 8-14 days before the interview (Table 7.6, column [4]). Of the 508 households interviewed about wage labor, 216 households had information on the jobs people had done (Table 7.3); the 216 households could have missing information on days worked or earnings, so they would not be part of the 190 households with complete information on wage earnings.

Insert Figure 7.1

[ii] Repeats. During 2000-2002 we asked the wife or the husband about earnings and jobs of all in the household for the seven days before the interview and, again, for the 8-14 days before the interview. The information went into one row or record for each household. During 2002-2010 we asked adults to tell us about their own wage earnings for the same two recall periods and put the information in one row. Thus, the yearly modules on wage labor had one row or record for each entity (household or adult) surveyed. Nevertheless, only some answers could be funneled into one row, and these were answers with but one possible value, such as "how many days did you work during the past seven days?" (Chapter 4).

When a question sparked myriad answers, with the number of answers varying from one entity to the next, the one-row-fits-all approach breaks down because each answer deserved its own row (Chapter 4). For these questions, an entity could have as many rows as answers it had tendered, giving the impression of an inflated sample next to the sample of households or adults surveyed. For example, when asking about employment, a person could have had two different jobs: one during the seven days before the interview, and another during the 8-14 days before the interview. The person worked for a logger one week and for a smallholder the next, for instance. Or the person farmed for one smallholder during the previous seven days and did more of the same for another smallholder during the previous 8-14 days. In both cases, in the data set, the person would have two records for employment. This explains why in Table 7.3 and elsewhere I report the number of observations and the number of unique entities undergirding results. To fix ideas even further, return to Table 7.3 and Figure 7.1. In 2000 we surveyed 508 households about wage labor (Table 7.1). Of the 508 households, 216 households had data on the jobs people had done (Table 7.3); in Table 7.3 I call these 216 households the “number of unique entities”. The 216 households with information on jobs had 329 records of employers or jobs because a household could have had more than one of each during the previous fortnight.

[iii] Outcomes. Among outcomes we asked about days worked, cash earnings from wage labor, and type of employment, all coded for the seven days before the interview, and jotted again (but separately) for the 8-14 days before the interview.

[iv] Selection criteria of sample to analyze levels and trends in days worked, wage earnings, and employment type. To analyze levels and trends in the number of days worked and in earnings from wage labor, I tidy the data using three criteria. Entities had to provide information on *(a)* number of days worked during the previous seven days and during the previous 8-14 days, *(b)* cash earnings from wage labor for each of these two recall periods, and *(c)* the type of work or job they had done to earn cash. If households or people had no information for any of these topics, or if they only gave answers about one week but not about the other week, I dropped them from the analysis. The filtering criteria yields a smaller, a more sensible, and a more trustworthy sample to see time trends in the share of people working for wages and -- for those working for wages – to also see time trends in the number of days worked and earnings from wage labor.

To pinpoint the gamut of paid jobs Tsimane’ did, I analyze data on employment type, even if households or adults did not provide information about the number of days worked or wages. We are more likely to trust a person who remembers the employer but not the amount earned, or the number of days worked, than a person who remembers the money earned or days worked, but not the job done. One omission is more ignorable, more believable, and less threatening than the other.

[v] Quality: Technical. We never asked if those who were not working for a wage were searching for a job, making the omission the most glaring shortcoming of the study on

wage labor. The elision makes it hopeless to assess whether people without wage earnings were wageless because they could not find a job (unemployed) or because they were not looking for one (out of the labor force). I use the term *wageless* to encompass both. The information had other flaws. Sometimes, the value of outcomes changed jarringly between years. We see coding inconsistencies and rounding mistakes, but, on the upside, we see almost no telescoping bias. I revisit the flaws as they become relevant in the chapter.

[vi] Quality: Substantive. Like many studies with full or part-time foragers, ours tried to understand ecumenically how things worked by looking inside Tsimane' society from the bottom up, an insider's view. For some topics the approach will do, but for others it will not. Wage earnings and sales bring to the fore the shortcoming of the approach. In all the years studying Tsimane' we never surveyed employers, the much-maligned loggers, cattle ranchers, riverine traders, highland colonizers, or even Tsimane' who hired other Tsimane'. We never bothered to survey buyers, either. We know nothing about the type of workers bosses wanted, the criteria bosses used when choosing workers, how they decided on the daily wage, and why some employers gave workers lunch besides cash while others only paid with cash. We know much about employees, nothing about employers, or buyers. In our approach we resemble psychiatrists querying clients about how clients feel, without bothering to query those outside the room who shaped clients into who they are. Somewhat solid, but partial, very partial our story is.

Raw yearly data on sales: Scope and quality.

[i] Sample sizes. The remarks about sample sizes for the analysis of wage labor apply *grosso modo* to the remarks about sample sizes for the analysis of sales. With earnings from sale, as with earnings from wage labor, we asked one person in the household during the early years of the study (2000-2002) about sales by the entire household and, beginning in 2002, we asked adults about their own sales. The sample size of villages, households, and people for the module on wage labor was the same as the sample size of villages, households, and people for the module on sales (Table 7.1); almost every household and person queried about wage labor was also queried about sales[vii]. Few chose to sell, and fewer still to sell a lot. Of all entities surveyed in a year, about half had sold something during the 14 days before the interview, and, of those who sold, an even smaller share (mean=27.93%; median=26.19%) had sold goods in each of the two weeks before the interview[viii]. I call the latter hard retailers. Owing to people's choices, the sample size gets smaller for topics that require information for both weeks. Telescoping provides an example of such a topic.

[ii] Repeats. At a minimum, each entity in the sales module had two rows of data: a row with information on sales for the seven days before the interview and another one with information for the 8-14 days before the interview. Rows had two fields germane to this chapter, one with the name (or type) of the item sold, and another with the total monetary value of the sale. We did not ask about the number of units sold or about the unit price of the item.

An entity selling nothing during the previous 14 days would have two rows full of zeros – one row for each week -- with zeros for item type and for item earnings. A person selling rice on Monday of one week and rice on Tuesday of the next week would have two rows, with information on rice sales for each day of the two weeks entered in a separate row. If the person had sold only rice one Monday of one week but nothing the next week, the person would still have two records, one full of positive numbers for rice sales for the early week, and another row full of zeros for the next week sans sales. If a person had sold four goods one week and four goods the next week, the person would have eight rows, even if the goods sold each week were

the same. Someone selling rice two days in a week would have two rows for that week. I dwell on these tiresome particulars to clear up that by repeats I mean a sale – a row or record of data – defined by the day of the week in which each good was sold, and by the earnings from that sale. A row of null values lets me tally the share of entities that did not sell, or that sold only in one week.

During a yearly survey, all respondents sold an average of 626 items (median=580) (Table 7.17). The number includes repeats and captures the total number of sale transactions, the sales traffic. With a larger yearly sample of entities surveyed, of course, the number of sales increases. For example, in Table 7.17 we see that in 2000, 298 households sold 978 items; next year, only 161 households sold goods, and the number of items sold, or the number of sales, understandably dropped, from 978 to 699. From the household-level surveys of 2000-2001 until the individual-level survey of 2002 the total number of items sold by all entities rose from 699-978 to 1367 because the yearly average for 2002 comes from three sizeable quarterly surveys; the more often we surveyed entities in a year, the larger the number of sales. Someone surveyed quarterly for a year will appear as retailing more wares than someone surveyed during one quarter.

What matters, to me, is not the array of unique items sold or the number of sales in the yearly sample, but the number of unique items sold by an entity in a year. For some things I care if a person sold rice twice during the past 14 days, but I care more, as a sign of specialization, whether that person sold only one thing during the previous fortnight. For this reason, later I show trends over time in the number of distinct goods sold by households and individuals.

[iii] Outcomes. Surveyors wrote in Tsimane' or in Spanish the name of the good sold and, either during the interview or later, they or a data screener identified the good as an animal, a plant, an artifact fashioned from local material, or an industrial article. Surely, there were ambiguities and mistakes in classification and in my grouping of the data. For instance, sometimes a plant appears with the (presumably correct) Tsimane' name but without the Spanish synonym; an outsider like myself cannot know by the name alone whether the plant is wild or farmed, so I used my admittedly crude judgment to classify the plant as a sylvan or as a domesticated crop[ix]. In the uncouth data, tables, and figures we sometimes see goods called "unidentified". These were faceless goods sold, the earnings from the sale noted, but the type of good left blank for reasons we will never know.

To marshal the data, I lumped goods into what I thought looked like salient, noteworthy categories: leading farm crops, minor farm crops, tree crops, domesticated animals, animal products, wildlife, commercial goods, and artifacts made from local materials. In the notes to Table 7.17 I give examples of the goods under the categories and in Appendix A I provide a count of the distinct goods in a category, such as the total number of wild plants and wild animals gathered from the forest for sale. To assess their importance in earnings, I tally categories by frequency and value. I want to know not only how often people sold domesticated animals, but I also want to know the share of those sales in all sales. And the same with earnings from animal sales; I want to know how much Tsimane' earned from selling domesticated animals and the share of those earnings in their total retail earnings. Frequency and value carry weight. Goods might be sold often but account for a small share of earnings. We will not know this unless we examine both the frequency and the value of sales.

[iv] Selection criteria of sample to analyze levels and trends in sales. The filters to choose people for the analysis of sales mirror the filters used to analyze wage labor. To be included in the analysis of retail, people had to provide information on each item sold and the

earnings from each sale, and do so for each of the two weeks before the interview. I dropped records with missing information on the type of article sold, or on the earnings from the sale of the article but kept people who sold nothing during either or both weeks. In the tables and discussion, I try to remove unwitting equivocations by clarifying if the sample includes all entities, entities selling during either week, or entities selling both weeks.

[v] Quality: Technical. With data on sales we see breaches of inerrancy. We see harsh changes between years in the value of some outcomes, changes which I discuss later. We see, too, rounding around multiples of five when reporting earnings (Table 7.19), and forward telescoping when reporting the number of goods sold (Table 7.18).

[vi] Quality: Substantive. Adequate but incomplete, sales data allows us to know the gross value of selling an article, but not the number of articles sold or the price of the article. We know nothing about buyers, about the venue of sales, and, more importantly, about how sellers and customers picked the price to give or take. All we know is what people sold and the gross earnings from those sales, and even that information comes blemished.

Why do Tsimane' need cash?

Before describing how Tsimane' earn cash, we need to step back and ask: Why do they need it? The question is simple, and the answer – to buy what they cannot produce – trite, and perhaps for these reasons we never asked it, yet a side trip to find out why helps to set the stage for the rest of the chapter.

In 2011-2012 we did a randomized-controlled trial to assess the effects of cash savings on cash holdings, expenditures, and the ability to cope with unforeseen harm. We also wanted to find out if one could increase savings by curbing impulsivity and did so by having two treatments. In one treatment, households chosen at random got a small wooden savings box (like a piggy bank), with a key and a slit to insert coins or bills. In the second treatment, households got the same box, but the key was kept by the organization carrying out the trial in the town of San Borja[x]. If a household assigned to the second treatment wished to withdraw money from the box, it had to travel to the office of the organization in San Borja. In each household receiving a box, we chose the winner at random, either the wife or the husband. Thus, each household had one box and one winner. Through the trial we wanted to find out if having any savings box increased cash holdings, and, by having two treatments (with and without a key), we wanted to find out whether withdrawing the enticement to open the box by having someone else keep the key led to more savings than having the common savings box, which winners could open at any time to satisfy fleeting whims. Before giving the boxes, we asked winners what they wanted to save for, and it is their answers to this question that helps us fathom their perceived need for cash and, indirectly, and more fitting for this chapter, their drive to earn it. Table 7.2 has their answers.

Insert Table 7.2

Most winners (37.68%) said they wanted to save to buy clothing. In olden days, Tsimane' made and wore long homespun cotton tunics, but tunics went out of fashion, replaced by manufactured apparel[xi]. After garments, people said they wanted to save to buy transport goods (e.g., bicycles) (12.23%), tools (11.26%), medicines (10.11%), kitchen utensils (9.30%), and food (7.50). Few mentioned wanting to buy luxuries (3.75%), domesticated animals (3.59%), or

construction materials (2.28%), or to invest in real estate or to begin a business (2.28%). All this people said, but what they did differed. Having finished the trial, we found that people had used cash savings to buy liquor. Nobody mentioned wanting to buy liquor, arguably for fear we would not give them a savings box for such a censurable want.

Wage earnings

Piece rate and wage earnings. It baffles one why people in an immured economy with plenty of forests and farmlands would toil for wages when they could work with the freedom of young Marx selling goods from their farm or forest. Not, that is, until we acknowledge the arrangements between employer and worker, buyer and seller.

Tsimane' workers do not get hired for a daily wage or for a salary. True, researchers, missionaries, and the government pay laborers in cash for time spent working, but most employers pay Tsimane' for finishing a task irrespective of the time spent on the task. Known as piece rate, the arrangement, Karl Marx said, is the one "most in harmony with the capitalist mode of production" (1915 [orig. 1867]; Vol. 1, chapter 21, p. 391), arguably because it halts workers from shirking on their duties. Common tasks in piece-rate covenants include clearing a plot of forest, weeding a patch of land, or portaging logs, all mostly done by men. The contractual arrangement for earning money with piece rate and for earning money from the sale of wares differ. With piece-rate payments, workers know they will get paid for each task they finish or for each unit they put out; with sales, workers have no warranty they will find buyers, or the right price for their wares. With piece-rate payments, workers face deeds and earnings with fastened boundaries, the employer pledging to give a worker a set amount of money or goods for, say, clearing one patch of forest and nothing more. Contrastingly, when workers sell, they have a say on how much to deliver and maybe earn, but almost no say on whether they will find buyers.

The two arrangements for earning cash – wage labor and retail –resemble each other. Consider the frieze of traders travelling upriver, stopping at villages along the way telling villagers they will return in two weeks on their way down to buy roofing panels from thatch palms for five *bolivianos* a panel[xii]. Sparked by the pledge, some villagers will begin making roofing panels. As with a canonical piece-rate arrangement for a task like preparing a hectare of grazing land, in this example a buyer-*cum*-employer defines a chore, with the worker straining to make roofing panels to get paid. In the data, money income from delivering roofing panels gets labeled as sale, whereas money income from preparing grazing land gets labeled as wage income. In both jobs workers get paid by their output, but one job – preparing grazing land – we (researchers) arbitrarily tagged as piece rate and the earnings from this transaction we labeled wage income, whereas the other job – putting out roofing panels – we tagged as sales or retail. The two arrangements differ, naturally. When selling roofing panels, workers enjoy leeway in how many panels to deliver; with orthodox piece-rate payments like preparing grazing land, workers have no say on how much land to raze or how much to make. The two compacts push workers to give up different freedoms. When making roofing panels as part of a piece-rate covenant, workers fit the work around their other daily chores. In villages with accessible thatch palms, a bevy of people during the indolent heat of the day will sit under shade trees, ribbing, leisurely chatting and making roofing panels for traders who promised to return. Work without drudgery. In much of these backlands, the distinct constructs of work and leisure, retail and piece rate blend into each other. Of course, when making roofing panels for outsiders Tsimane' face annoyances, like taking time away from other doings, yet they keep the freedom to work

from home. Less so with canonical piece-rate payment like establishing pasturelands for cattle ranchers. For those jobs, workers need to resettle or take daily trips to ranches. Orthodox piece-rate compacts cut deeper into laborer's freedom, turning them into rural migrants and commuters, but never do the compacts produce the Marxian strains and alienation found in nineteenth-century Europe – at least not so far.

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Employers: Yearly data. In Table 7.3 I list the entities employing Tsimane' for the week or for the two weeks before the survey. Entities included smallholders, other Tsimane', traders, and formal institutions such as the Tsimane' Council, logging enterprises, cooperatives, government agencies, and missionaries.

Insert Table 7.3

Data quality on employers. In the data we see big changes in the share of households working for different types of employers. For instance, in only two years, from 2000 until 2002, the share of household working for smallholders fell from 20.9% to 5.8%, possible but unlikely. The change could reflect the way we gathered data in 2000 and 2002. In 2000 we asked household heads to report any employment in the household during the previous 14 days, whereas in 2002 we restricted the question to employment of the household heads for the same recall period. Another reason for the difference could come from the samples. The sample of household for the survey of 2000 and the sample of households for the surveys of the randomized-controlled trial of 2001-2002 came from two different draws. Thus, the large change in the share of households working for smallholders could show not mistakes, but a truthful shift in employment from one fresh yearly sample to another.

But large changes in employment statistics do not just come from the way we culled information for the household surveys during 2000-2002; large changes also show up in other years, and for other reasons. We see jarring changes as well with longitudinal information from individuals during 2002-2010. The share of Tsimane' working for cattle ranchers, for instance, doubled from 2003 to 2005, from 10.5% to 23.0%. Again, such a hefty change is thinkable but unlikely with repeated measures from the same people over such a brief time.

Besides large changes in the share of people working for the same type of employer, we see at least one other blemish with employment data: *ad hoc* coding of jobs or employers. For instance, in the household surveys of 2001 and 2002 we did not code as employers the Tsimane' Council or Protestant missionaries. If they hired Tsimane' during 2001-2002, these two types of employers got lumped and put in the catchall bin of "Other" employer. In all other years, we split, coded, and acknowledged the Tsimane' Council and Protestant missionaries as distinct employers, but not in 2001-2002. Failure to code some employers does not bespeak carelessness on our part, for they sometimes convey meaning. For instance, oil firms prospected for oil and natural gas in the Maniqui basin during the early years of the study, but then left[xiii]. During the early years of the study, producer cooperatives worked briefly in the study area. Because these employers left, we acknowledge and code them as employers during the early years of the study if they did employ, but not afterwards when they show up as missing observations. In 2010 we coded a person working for the government as a public employee, but we did not code them as such in 2000 because the government barely hired Tsimane' when the study began; in 2000, we lumped people hired by the government as workers hired by "Other" employers.

Snapshot and trends of employers. The right-most column of Table 7.3 (section A) shows that – leaving aside whether information refers to households or to people -- loggers, cattle ranchers, the Bolivian government, other Tsimane' (not the Tsimane' Council), and highland smallholders hired most Tsimane'. By the share of people hired from 2000 until 2010, loggers and cattle ranchers topped the list of employers, accounting for 23.4% and 19.3% of all hires. Next came the Bolivian government, other Tsimane', and highland smallholder, accounting for 13.7%, 11.8%, and 9.8% of employment.

Table 7.3 (section B) shows that most entities worked for one employer during the 1-2 weeks before the interview. Across all the years of the study, 94.7% of households or people had found work with one employer during the fortnight before the interview, with 4.8% and 0.4% saying they had worked for more than one employer. The year 2002 sticks out. In 2002, 76.1% of people had worked for one employer, with 19.5% of people saying they had worked for two employers and 4.3% saying they had worked for three employers. I cannot explain the sudden change.

I next examine changes in employment by doing two analyses. First, I compare the share of households working for different employers in 2000 with the share of people working for different employers in 2010, the last year of the longitudinal study. The comparison of endpoints is flawed because statistics refer to different levels – households in one case (2000), people in the other (2010) – and because some types of employers were coded in one year but not in the other year. These warnings aside and focusing on employers coded in 2000 and 2010, we find that in 2000 the largest employers were loggers (26.4%), smallholders (20.9%), traders (17.0%), and cattle ranchers (10.0%). By 2010, all had lost ground, except for cattle ranchers. The share of entities hired by cattle ranchers rose from 10% in 2000 to 27.7% in 2010. By 2010, other Tsimane' and the government had taken the lead as employers. In 2000, only 0.6% of households reported working for another Tsimane'; by 2010, the share had risen to 18.2%. Hardly noticeable in 2000, the share of people hired by the Bolivian government reached 17.5% in 2010.

The second analysis is tighter because I constrain it to working people, to the longitudinal study, and to jobs coded in each of the nine years of the longitudinal study. The restrictions allow me to tally yearly growth rates of employment by job type and to smooth over large breaks that show up from one year to the next, but which become less noticeable when standing afar over a broader swath of time. Table 7.4 shows the results. Three findings stand out. First, the share of Tsimane' hired by highland smallholders remained unchanged, falling by a trifling yearly rate of 0.99% from 2002 until 2010. The backbone of continuous employment, highland smallholders seem always to be there to hire their lowland peers. Second, fewer Tsimane' got hired by logging firms, traders, or by schools. The share of people hired by traders and hired as school teachers fell by a yearly rate of 8.13% (traders) and 8.67% (teachers), while the share of people hired by logging firms fell by a yearly rate of 14.68%. The third notable finding is the crescent hiring of Tsimane' by cattle ranchers and by other Tsimane'. During 2002-2010, each year saw a 12.38% rise in the share of Tsimane' hired by cattle ranchers and a 6.85% rise in the share of Tsimane' hired by Tsimane', a result consistent with the earlier coarser analysis.

Insert Table 7.4

I next interpret the results. Stable employment. The stable share of Tsimane' hired by highland smallholders is puzzling because highlanders have been flooding the lowlands since the

1970s (Jones, 1995), and their farms, sown with cash crops, have been spreading as well (Paneque-Gálvez et al., 2013). In line with the growth of migrants and farmlands, one would have thought that highland smallholders would be hiring more Tsimane', but we do not see the two trends yoked to each other. The unvarying share of Tsimane' hired by highland smallholders could reflect practical adhesions between a Tsimane' worker and a (highland) ethnic peer. Real and imagined kinship ties between employer and employed, along with deep personal fealties that such ties bring, might lock the couple into a skein sluggish to unravel with changes in the labor market. Declining employment. Shifts in the regional economy have reduced the share of jobs with loggers and traders. Logging enterprises have left the area owing to the depletion of the best hardwoods and to new laws that narrow down where logging firms can search for timber (Paneque-Gálvez et al., 2013). Shrinking employment by outside traders reflects, I think, the replacement of outside traders by Tsimane' traders. In the past, traders from the towns of San Borja or Yucumo would have hired a Tsimane' to riffle through the countryside to buy, store, and bring goods to town. When asking the agents of traders about who had hired them, they would have said that a town merchant had. Today, better-off Tsimane' have become traders, unseating town merchants as employers. The drop in the share of workers hired as school teachers comes from changes in the educational policies of the Bolivian government and from changes in the language tastes of Tsimane'. Until 2006, Protestant missionaries hired and trained Tsimane' village school teachers in Tsimane', but in 2006 the Ministry of Education took over the stewardship of Tsimane' schools. Once in charge, the Ministry of Education raised requirements for hiring village school teachers, requirements that disqualified older Tsimane' from keeping their teaching job (Reyes-García et al., 2010). At the same time came a blooming demand by Tsimane' parents to have their children lettered by native Spanish speakers. Parents increasingly view their children's fluency in spoken Spanish as their children's linguistic passport into the rest of the country[xiv]. Changes in the language taste of parents likely weakened further the demand for Tsimane' school teachers. Increasing employment. The growing share of Tsimane' hired by Tsimane' shows that Tsimane' traders are replacing outside traders. The trend could also reflect a growing share of well-to-do but busy Tsimane' compelled to hire compeers to help them with farm chores (Chapter 6). Last, we see cattle ranchers hiring more Tsimane', a somewhat predictable trend because cattle ranching has been expanding for more than a half century in the Bolivian lowlands (Jones, 1995). Features of ranching have modernized while hiring practices have not. Large ranches buy better livestock and new pastures, and improve health practices for their herds, but they still rely on rural workers like Tsimane' to ready grazing lands. The nearness of cattle ranches to Tsimane' villages down river makes it easy for these villagers to glide in and out of ranches for work.

In sum, we see changes in employment away from logging, teaching, and reliance on outside traders toward more reliance on cattle ranchers and on other Tsimane'. From whatever leisure village life provides, Tsimane' workers come and go to cattle ranches or to the field of other Tsimane'. The type of paid jobs Tsimane' do likely shapes the type of human capital they value most. If, for work, Tsimane' can commute from their sheltered villages to the demesne of cattle ranchers or to the fields of friends and kin – not to factories or towns -- they will not feel a need to learn Spanish or take up the cultural trappings to deal with the wights beyond the village. Foreshadowing that these days will come to an end, adult nonetheless seem to see things differently for their children, choosing to have them taught by native Spanish speakers.

Employers: Quarterly and seasonal data. Table 7.5 has a breakdown of employers by quarter and by season, from May 2002 until June 2003.

Insert Table 7.5

Sample size. We broke up the year into the following quarters: May-July (I), August-October (II), November-January (III), and February-April (IV). Since the quarterly study took place during parts of 2002 and 2003, the third quarter (November-January) overlaps between the years 2002 and 2003, something to remember when assessing the sample size of people surveyed each quarter. During the first, second, and third quarter of 2002 we surveyed 140, 148, and 110 people[xv]. The sample size of people surveyed during the first quarter (May-July) and fourth quarter (February-April) of 2003 were 134 and 113. Table 7.5 shows that we surveyed four people during the second quarter of 2003, all of them in August. In July 2003 we did the last formal quarterly survey, so the four people surveyed during August 2003 were likely attriters tracked down after the formal quarterly surveys ended. In allegiance to the way we gathered data, and to make the yearly totals of Table 7.5 match the yearly totals of person-level data from 2002 and 2003 of earlier tables, I do two things. First, I date surveys by the month and day when surveyors did the interviews. Second, I do not merge data from January 2003 with data from the third quarter of 2002 (November-December) to create one block of data for the third quarter of 2002, which would have stretched over two years (November-December 2002 + January 2003). If I drop the four people surveyed during the third quarter of 2003 (August-October) and combine the 11 people surveyed during January 2003 with the 99 people surveyed during November-December 2002 to have one total for the third quarter of 2002 (November-December 2002 + January 2003), I find that each quarter we surveyed an average of 129 people (minimum=110; maximum=148).

Intra-annual and inter-annual levels and changes in employment. I begin by comparing employment between the first quarter (May-July) of 2002 with the first quarter of 2003 because those quarters have large samples and because I want to see how trends from the shorter study compare with trends from the longer study. Table 7.4 has the comparisons.

When comparing employment during May-July 2002 with employment during the same months of 2003 (Table 7.5, section A), we see that employment in the homestead of highland smallholders and employment with researchers dropped from 10.4% to 5.0% (smallholders) and from 27.7% to 10.1% (researchers). Between May-July 2002 and May-July 2003, employment in logging camps rose from 12.5% to 28.7% while employment in the farm of Tsimane' rose from 6.7% to 21.4%. These statistics show large changes in job types between the same months of two back-to-back years. Between May-July 2002 and May-July 2003, hires by other employers like cattle ranchers or traders did not change.

Some of the findings from the analysis of quarterly data buttress the earlier analyses of long-term trends summarized in Table 7.4, but some findings do not. Both analyses show that Tsimane' are hiring more, but the quarterly data from Table 7.5 shows negligible changes in hires by traders, cattle ranchers, or by schools. In contrast, the information on longer-term trends shows that traders and schools are hiring fewer Tsimane' while cattle ranchers are hiring more Tsimane'. Quarterly information shows less employment in the homestead of highland smallholders dwelling in the lowlands, whereas longer-term information shows almost no change in the share of Tsimane' hired by highland smallholders. The comparison of the two studies highlights the patent pitfall of relying on a short study to learn about lasting changes, but it also underscores how a long study can blur weighty uncovered in a short study.

The rainy and the dry season change health and diet (Brabec et al., 2018), and could change employment. To see if this is so, I conjoined all quarterly data from the two years of the quarterly study into one value for the dry season and one value for the rainy season, with the abridged values placed in the two right-most columns of Table 7.5. The dry season covers quarterly data from May until October and the rainy season covers quarterly data from November until April.

Table 7.5 shows meek changes between the rainy and the dry season in the type or in number of jobs Tsimane' had. From the rainy to the dry season we see a rise of one percentage point in the share of Tsimane' hired by highland smallholders (from 7.7% to 8.7%) and a rise of 5.1 percentage points in the share of hires by other Tsimane' (from 6.9% to 12.0%). The share of Tsimane' hired by cattle ranchers fell by 4.6 percentage points from the rainy to the dry season, from 16.0% to 11.4%. Since we -- the researchers -- came during the summer months of the northern hemisphere (June-September) or the dry season in the study site, it makes sense that the share of Tsimane' hired by researchers would rise from the rainy to the dry season, as it in fact did, from 6.6% to 16.5%. Beyond employment by researchers, the only other unmistakable change in employment between the seasons happened with loggers. From the wet to the dry season, the share of Tsimane' working for loggers fell from 30.7% to 22.4%.

The number of paid jobs held by a worker did not change between seasons. Table 7.5 (section B) shows that 73% of workers in either the rainy or the rainless season had one employer, 19-20% had two employers, with the balance having 3-4 employers, again with roughly the same shares in each season. Blunt as they might be, these estimates show that Tsimane' have the same number of jobs in each season. They do not diversify employment between seasons, at least not through the number of jobs they hold. The finding has at least two explanations, one having to do with methods, one with substance. On method: our surveys trolled information about employment for the fortnight before the interview. Such a short span confines the range of answers one could possibly get. In truth, how many jobs could you have in two weeks? To get a better feel for seasonal changes in employment we should have lengthened the recall period. On substance: in the homeland of Tsimane' the seasons do not break the year into a barren time of want and a teaming time of plenty. Instead, the steady availability of wild animals and perennial crops (feral and cultivated), along with hardy annual tubers, always available for the taking, soothes whatever hardships the seasons bring, erasing the need to moonlight in menial jobs to safeguard the flow of food or income into the household.

What we cannot tell from quarterly data, regrettably, is how far employment reflects the demand for workers versus how far it reflects workers' willingness to find jobs. Some jobs spring up and vanish with the seasons. Jobs churn, not workers. But the slight peaks of unskilled rural work for highland smallholders or for Tsimane' during the lulling dry months reflects an upsurge in the pool of idle workers with free time for wage labor.

The wageless, days worked, and wage earnings. I next discuss levels and trends in the share of Tsimane' out of the labor force or unemployed and -- for those who worked for wages -- I review the number of days worked and their earnings (Table 7.6). To assess telescoping bias, I compare employment for the previous seven days with employment for the previous 8-14 days (Table 7.9). Without telescoping bias, the number of days worked and the amount of earnings should be alike for the two periods. Table 7.7 has yearly growth rates for the number of days worked and for total earnings. In Table 7.7, I adjust wage earnings for inflation.

With the analysis of the wageless and of workers, data can refer to households or to individuals. For some outcomes, households will have lower values than individuals. For a

household to be tagged as wageless, none of its (presumably) adult dwellers should have worked for a wage. If only one person in the household had earned cash from wage labor, the household was classified as having earned cash. When the focus shifted from gathering data at the household level to gathering data at the individual level --- from and about individuals in a household -- we had a better chance of finding wageless entities. Suppose we had two households, A and B, both with four adults, the first household surveyed in 2000 and the second surveyed in 2010. In household A, surveyors asked a household head to provide information about all the people in the household. Of the four adults in household A, one adult had earned income from a job. In our way of reckoning events we would have classified this household in 2000 as having earned income from wage labor. From the way we retrieved and aggregated data, we lost information about individuals in the household. Next consider household B, canvassed in 2010, also with only one wage worker. With household B we jotted information from and about each adult, so we would have classified as wageless 75% of all the entities in this households. The mismatch in how we gathered and conveyed data about earnings from wage labor would lead one to the mistaken belief that the share of wageless entities had risen from zero in 2000 (n=1 household) to 75% in 2010 (n=4 adults). No easy answer comes to mind. We cannot go back in time and re-survey people to find their employment history. For later survey, I could have squeezed information about individuals in a household into one average for the household; besides shrinking the sample size, I dislike the approach because it would have erased the information each person brought to the study.

As the example shows, changes in the share of wageless entities between the early years of the study (2000-2002) when we asked villagers to tally and report summary values for their household and later years when we asked villagers to tell us about their own employment history reflect how we aggregated and conveyed information. Faced with no easy way to convey trends, I follow two approaches (Table 7.7). First, I pool the sample of surveys referring to households (2000-2002) with the sample of surveys referring to individuals (2002-2010). This defective step helps to spot long but coarse trends, coarse for the reasons just belabored. Second, I use data from individuals (2002-2010) to get more accurate values of trends and to sidestep the noise that comes from changes in methods of data gathering that took place during the study. As we shall see, the two approaches with their different samples yield, as expected, different trends, yet both lead to the same conclusion. With both approaches to estimate trends, summarized in Table 7.7, I leave out baseline data from the 2008 randomized-controlled trial. I prefer 2008 data from the long study (2002-2010) of the same people to ensure all surveys come from the same villages. Useful for some ends, 2008 data from the baseline of the randomized-controlled trial would have increased noise because it came from villages beyond the villages of the longitudinal study.

Data quality on the wageless. I begin by assessing if one can draw lessons about the wageless from the yearly surveys of the long study, extending from 2000 until 2010, and if those lessons extend to Tsimane' elsewhere (Table 7.6, column 5). I do so by comparing the share of wageless entities in the long study with the share of wageless people from the baseline of the randomized-controlled trial (2008). Since we used one large sample of 40 villages for the baseline of the randomized-controlled trial and a different, smaller, sample of villages for the long study, we can compare findings from the two samples to judge how far one can generalize from the smaller sample of the long study to other Tsimane'.

Insert Table 7.6

The last row of Table 7.6 (column 5) shows that during 2000-2010, 76.8% of the pooled sample of households and people in the long study (excluding the randomized-controlled trial) had no wage earnings. Column 5 shows that the share of people in the baseline of the randomized-controlled trial (2008) without wage earnings reached 83.1%. The two shares -- 76.8% and 83.1% -- are almost the same. If we restrict the comparison to 2008 -- the only year in common between the randomized-controlled trial and the long study -- we find that in 2008 the long study had a higher share of people without wage earnings (90.0%) than the randomized-controlled trial (83.1%). Again, the two shares are in the same neighborhood. If one believes that the share of people without wage earnings from the randomized-controlled trial (83.1%) reflects the true population mean, then the estimates of wageless people from the long study are not too far off; these estimates range from 76.8% if one includes all years to 90.0% if one includes only the year 2008 from the long study. In short, estimates of the share of people who did not work for a wage in the long study mirror the share of wageless people in the rest of the Tsimane' population.

Although the long study has synchronic external validity, it also has shortcomings. Repeated cross-sectional surveys from households during 2000, 2001, and 2002 and longitudinal surveys from people (2002-2010) show riddling swings in employment. In only one year -- from 2001 to 2002 -- the share of households without wage income rose from 58.7% to 74.0% (Table 7.6, column 5). A jarring change of 15.3 percentage points in the share of the wageless population is unlikely to have happened in two years but could have happened if, for whatever reason, households with jobs left the sample. Column 4 of Table 7.6 shows that the sample of households surveyed shrank from 378 in 2001 to 328 in 2002 while the sample of households with complete data on wage earnings fell by 53.8% during 2001-2002, from 156 to 84 households. Startling changes also happened in the long study of individuals. Longitudinal data of people shows a quick rise of seven percentage points from 83.0% in 2007 to 90.0% in 2008 in the share of people without wage earnings, a rise erased by a commensurate quick fall of 7.1 percentage points in the next two years (2009-2010), from 91.0% to 83.9%. The sudden spike in the share of people without wage earnings from 83.0% during 2007 to 90-91% during 2008-2009 could have come from a brief distempered labor market, from noisy samples, and from mistakes in measurements.

Snapshot and trends in the share of the sample that was wageless. The share of Tsimane' who did not work for a wage varied by the level at which we measured employment and by the sample. If we join yearly household-level and individual-level figures, the grand yearly mean of entities without wage earnings reached 79.0% (Table 7.6, column 5). If we review the yearly sample of individuals (2004-2010) followed over time, the share of people in a yearly survey who did not work for a wage averaged 81.8% and went from a low of 73.6% in 2004 to a high of 91.0% in 2009. During the early years of the study (2000-2002), an average of 64.5% of households had no wage income.

In sum, during 2000-2010, an average of 80% of households or people surveyed had not worked for cash, with the share ranging and rising from a low of 58.7% in 2001 to a high of 90-91% in 2008-2009. Some of the rise reflects how we posed questions, whether surveyors asked a household head to provide information about all in the household, or whether they asked each adult to tell us about their own earnings from wage labor.

To move beyond stills, I next assess yearly trends in the share of the sample that had no cash from wage labor (Table 7.7). For the analysis I rely on two sets of yearly values from Table

7.6. In one sample, I use all 11 yearly values from 2000 until 2010, and in the second sample I use values from individuals spanning the years 2002-2010 (inclusive)[xvi].

Insert Table 7.7

Pace those who view the leviathan market swallowing people into paid jobs (Chapter 2), one sees the opposite: Tsimane' leaving wage labor. Table 7.7 (row a, column 1) shows that from 2000 until 2010, the mean share of all households and people without wage earnings rose by a yearly rate of 3.56%. If I confine the analysis to people surveyed each year during 2002-2010, the share of people without wage earnings rose by 2.07%/year (Table 7.7, row a, column 2), smaller than the rough first estimate of households and people mixed together, but still positive.

In an industrial country one might read the growing share of people without wage earnings as a troubling sign of fewer jobs in the economy, or as an intimation that people scant of money shied away from job searches. Brought to Tsimane' society, this way of thinking would mean that employers offered jobs, but that Tsimane' did not take them because they chose to do something else with their time, like fish, hunt, socialize, or tend to their parcels (Chapter 6). The rising share of wageless entities could mean Tsimane' find it harder to swap leisure or household chores for paid work. Not having searched and lost, discouraged workers we cannot call them, but reluctant ones we can. Why might this be so?

We can find a likely answer in Table 7.8, which shows trends in the yearly ratio of the village daily real wage to its undertow, the village selling real price of rice. A rise in the real price of rice – the leading cash crop – relative to the village daily real wage tells us, in textbook fashion, that, all else the same, rice husbandry pulls people to earn cash more strongly than wage labor. Table 7.8 shows that, depending on how we measure wages, the real value of wages fell relative to the real price of rice by 5-6%/year, suggesting that Tsimane' could make more by growing rice than by working for a wage. In its shorthand way, the market is saying that for profits and for material gains, cash cropping beats wage labor, a message not lost to Tsimane' drifting away from paid jobs.

Insert Table 7.8

Days worked and earnings from wage labor. Here I focus on people who had worked for wages during the 14 days before the interview.

Data quality on days worked and wage earnings. Puzzling harsh changes resurface in the data, this time in the last years of the longitudinal study of individuals. Columns 6-7 of Table 7.6 show that, of those who worked for wages, the mean and median number of days worked during the 14 days before the interview doubled from 7.0-7.8 days in 2007 to 12.2-14.0 days during 2008-2009, only to drop again to 7.0-7.5 days in 2010. Not shown in Table 7.6, the raw information for 2008 had 63 people with full information on earnings, of whom 73.02% said they had worked all seven days during the week before the interview and 71.43% who said they had worked every day during the 8-14 days before the interview. If true, the figures would suggest a furibund rural economy with full employment. Unlikely. Columns 9-10 of Table 7.6 show that median and mean nominal wage earnings for the last fortnight before the interview doubled from 175-274 *bolivianos* in 2007 to 490-539 *bolivianos* in 2008. Earnings remained high until the end of the study in 2010.

Besides odd changes in the number of days worked and in wage earnings, the data has one other unusual feature, but also a worthy one. On the downside, we see rounding mistakes in reported earnings (Table 7.6, columns 12-14). The bottom rows of columns 12-14 show that over all the years of the study, 75% of households and people earning wage income said they had earned amounts ending in the digit zero (e.g., 40 *bolivianos*) and 15% said they had earned amounts ending in the digit five (e.g., 15 *bolivianos*). It did not matter whether information referred to households or to individuals, whether it came from quarterly or from yearly surveys. Rounding the last digit of reported earnings around multiples of five always showed up to besmirch the data.

On the upside, there is little evidence of telescoping bias. In Table 7.9 I show the results of tests for telescoping bias of reported days worked and earnings from wage labor. For each of the two, I compare answers about the seven days before the interview with answers about the 8-14 days before the interview. After pooling surveys from all years, I find the same average values for the two recall periods. In each of the two weeks before the survey, entities said they had worked an average of 3.6 days and earned 116 *bolivianos*. Perhaps because of more uncertainty and guessing when remembering older events, entities varied more in their answers about how many days they had worked or earned two weeks ago. For example, the standard deviation of cash earned two weeks ago was 162 *bolivianos*, 10.95% higher than the standard deviation of cash earned a good ago (146 *bolivianos*).

Insert Table 7.9

Snapshot and trends in days worked. During the 14 days before the interview, wage earners worked a median and a mean of 7.0 and 7.2 days (Table 7.6, columns 6-7). Tsimane' rest on Sundays, so working 7.0-7.2 days during the past two weeks amounts to working for a wage every other day. When they worked, they worked part-time.

The number of days worked showed variation within and between years. In a year, some people in the labor force worked many more or many fewer days than the average. I use the coefficient of variation – standard deviation divided by the mean – to summarize and describe variation in the number of days worked in a yearly or in a quarterly sample. The bottom row of column 8, Table 7.6, shows that the standard deviation for the pooled sample sans the baseline of the randomized-controlled trial was 4.7 days. With a mean of 7.2 days worked in the previous fortnight, the coefficient of variation for the full sample was 0.65.

The number of days worked during the fortnight before the interview varied notably, not only in a yearly or in a quarterly sample, but also between quarters and between years. The median and mean number of days worked ranged from a low of three to 5.6 days during the second and third quarter of 2002 to a high of 14.0 and 12.2 days during 2008-2009 in the longitudinal study of individuals. To make easier the reading of trends, in Figure 7.2 I plot the coefficient of variation of days worked for each yearly and quarterly sample. With three exceptions, the coefficient of variation was high and remained high, within a narrow range of 0.6 to 0.8. Three values stand out. The unusual values happened during 2008-2009, two coming from the longitudinal study of people and one coming from the baseline (2008) of the randomized-controlled trial. The decline in the two coefficients of variation in 2008 and 2009 of the longitudinal study came from the odd spike in the mean number of days worked in those two years, from an average of 7.4 days during 2004-2007 to 12.2-13.1 days during 2008-2009. The spikes caused the coefficient of variation to drop to 0.2 in 2008 and to 0.1 in 2009. The

coefficient of variation of 0.5 in the baseline (2008) of the randomized-controlled trial is informative because it rests on a large sample of 229 wage earners – higher than any of the other samples – but cannot be compared with the other coefficients of variation because in the randomized-controlled trial we asked about employment during the previous week instead of employment during the previous two weeks. The three unexpected values from 2008-2009 aside, variation in days worked was high but unchanging.

Insert Figure 7.2

During all the years of the study, those who worked, worked more each year. Table 7.7 (row b, column 1) shows that, *in toto*, during 2000-2010 each year saw a 5.47% and a 7.28% rise in the mean and median number of days worked by households and people. Using information from individuals of the longitudinal study (2002-2010), column 2 shows an even higher growth rate in the mean and median number of days worked, 8.39% for the mean and 11.74% for the median.

We saw that a growing share of Tsimane' forgo earning income from wage labor, so why do some Tsimane' leave the labor force while others work more? The answers could tell us about where Tsimane' society is headed and – or -- how households run. If most of those who work for wages and those who do not come from different households, the finding could be telling us about the rending of livelihoods. We could be witnessing the breakup of Tsimane' society into paid workers and woodsmen, into a shrinking group who joins the labor force to toil longer for a wage and a swelling muster of neighbors who, instead, give up working for a wage to farm more. If the trend continues, it could contravene traditional Tsimane' society, segregating it by occupation into proto-proletarians and plantsman. But the trend could tell another story. If those who work for wages and those who do not come from the same households, the trend could be telling us about how households hedge against woeful states. Households writhing in pain from floods and from the loss of crops could endure because some people in those households worked in cattle ranches and brought home income. And when cattle ranches stopped hiring, the same households could endure the hardships because some in the household farmed and -- though they brought no cash -- nonetheless could bring home stop-press food from village commons and farms. Farming could act as a pendant to wage labor, and vice versa. The division of labor within the "self-provisioning" household between farmers and wage earners could also be saying something about how households match skills to tasks[xvii]. Perhaps employers like to hire schooled, bilingual men who, once hired, turn into work fiends, while the unlettered, monolingual churl of the household, silently barred from wage labor by employers and themselves, farm and forage more in the environs of the village. Without sundering Tsimane' society into occupational moities each household has some of both.

Snapshot and trends in cash earnings from wage labor and daily wages. I now examine wage earnings and daily wages, the goal and obverse of days worked. Wage earnings can tell us about people toiling more to make up for falling wages, treading in work to stay in the same place, or about people toiling more to get ahead. Earnings people determine by how much they work. Enacted by the market over which they have no say, daily wages workers must accept.

I estimate daily cash wages in two ways. First, I rely on village surveys in which we asked a village leader, such as a teacher, to tell us about the average current daily cash wage in the village. Some employers paid only with cash while others paid with cash but added lunch

and, so, paid less. We asked about both forms of payments (Table 7.10). The average village daily wage gleaned every year from village leaders was fitting but wanting because it swept aside differences in wages earned by people with different skills, for the same or for different jobs in a village. For this reason, I also computed an implicit daily wage for each worker by dividing the total amount of nominal cash they earned from wage labor by the number of days they had worked, but only for those who had earned money from wage labor. However measured, trends in nominal earnings or in nominal daily wages can be misleading because they include inflation. To redress the flaw, I use Bolivia's consumer price index (CPI) to take out inflation from nominal earnings and from nominal daily wages and arrive at real values for wage earnings and daily wages[xviii].

Insert Table 7.10

Data quality on village daily wages. Four shortcomings stand out about the quality of the data on village daily wages: small samples, missing observations, outliers, and poor phrasing of the question about the going village daily wage.

In most years we surveyed 13 villages, so we need a chary reading of findings as a handful of outliers in such a small sample could bollix up findings (Table 7.10, column 2). Columns 3 and 7 of Table 7.10 show that during 2000-2002, 10-30% of villages had no data on village wages. We cannot tell if missing data means villages had no employers who paid with cash, or if village leaders could not remember or figure out the current daily wage. As before, we see outliers, a menace with small sample. For example, the mean and median foodless cash wage jumped from 31.5-40 *bolivianos* in 2008 (columns 4-5, 8-9) to about 50 *bolivianos* in 2009-2010. The daily wage for 2010 is problematic because of missing observations and outliers. In 2010, one of the 13 villages (7% of the sample) lacked a cash wage and four villages (30% of the sample) had no information about daily wages with food. Furthermore, in 2010 one village leader said that the average daily village cash wage without food was 130 *bolivianos*, unlikely since the median daily nominal cash wage without food for all the years in the study (other than 2010) reached 30 *bolivianos*. Last, we could have done better when asking about the daily cash wage. The question burdened village leaders with figuring out the daily cash wage paid by different employers. To overcome the rub, we should have asked not one person but many people about the most recent wages paid for different jobs by different employers. We did not. In any case, guesses, mistaken averages, and faulty recall becloud the estimates of the village average daily cash wage that we got by leaning on one respondent.

Levels and trends in village daily wages. The bottom row of Table 7.10 (columns 4-5) shows that during 2000-2010 the nominal daily cash wage in a village averaged 30-31 *bolivianos* if employers paid only with cash. The bottom row of columns 8-9 and 11 show that if employers added lunch, the daily nominal cash wage in a village went down by 20% to 25 *bolivianos*.

Figure 7.3 shows the coefficient of variation of the village nominal and real daily wage, with and without lunch[xix]. The two forms of payment stayed near each other, with coefficients of variation ranging between 0.1 and 0.3. The oddity comes in 2010 when the coefficient of variation in the foodless daily wage jumped to 0.7. The jump came from a drop in the sample size and from a high daily wage in one village (130 *bolivianos*); this outlier in a small sample swayed and decentered the trend.

Insert Figure 7.3

Figures 7.4a-7.4b show that the daily village nominal cash wage without lunch and with lunch rose by an average yearly rate of 5% and 6%. The growth rates overstate improvement in wages because they include inflation. Figures 7.4c-7.4d replicate Figures 7.4a-7.4b without inflation. Taking out inflation lowers growth rates. During 2000-2010, the yearly real growth rate of the village daily wage without food barely moved, changing by merely 0.27% (Figure 7.4c), while the yearly growth rate of the village daily wage with food rose by 1.48% (Figure 7.4d).

Insert Figures 7.4a-7.4d

Levels and trends in total earnings and implicit daily wages based on aggregate statistics from Table 7.6. If I divide total nominal earnings from column b of Table 7.6 by the number of days worked in column a (Table 7.6), adjust for inflation, and then use the local exchange rate I find that Tsimane' workers made USA$4.75 to USA$5.87 per day, considerably higher than the daily amount of USA$1 to USA$2 per person commonly used to define global monetary poverty (Bauchet et al., 2018)[xx].

Section [c] of Table 7.7 shows that median and mean nominal earnings rose by a yearly rate of 15.16% to 21.34%, with higher growth rates for median than for mean values and with higher rates during 2002-2010 than during 2000-2010. After removing inflation, the yearly mean and median growth rates dropped by six percentage points, but remain high and positive, ranging from a mean of 9.94% during 2000-2010 to a median of 15.22% during 2002-2010 (Table 7.7, section [d]). In section [e] of Table 7.7 I show the growth rates of the implicit daily wage, defined as earnings divided by the number of days worked. During 2000-2010, the mean and median implicit daily nominal wage grew by a yearly rate that ranged between 8.86% and 10.41% (Table 7.7, section [e1]). Real wages had humbler yearly growth rates than nominal wages. The yearly growth rate of the average real wage reached 4.29% during 2002-2010 and 4.47% during 2000-2010, while the median real wage grew by 3.48% during 2002-2010 and by 3.64% during 2000-2010. Average and median real wages both grew during 2000-2010, but median real wages grew by much less than average real wages.

Section [d] of Table 7.7 shows a bald yearly rise between 10.91% and 15.22% in the median value of total real earnings from wage labor for the 14 days before the interview. What accounts for the rise? The data allows us to pinpoint how far wages and effort contributed to the gain, with findings shown in section [f] of Table 7.7. Since median real wages grew by a yearly rate of 3.64% or 3.48%, they accounted for 33.36% and 22.86% of the rise in total real earnings. The rest of the rise sprang from elsewhere. Indeed, most of the rise in total real earnings came from Tsimane' working more, rather than, or more so, than from the rise in real wages. The median number of days worked rose by a yearly rate of 7.28% (2000-2010) or 11.74% (2002-2010). These rates show that effort accounted for 66.72% and 77.13% of the growth in total real earnings. If, instead of using the yearly growth rate of implicit median wages from section [e2] of Table 7.7, which hovered about 3.5%, I used the yearly growth rate of daily real mean wages from village surveys in Figures 7.4c-7.4d, which hovered between 0.27% and 1.48%, we would conclude that almost all the rise in real earnings came from more exertion rather than from better wages.

If culture welds peoples' wants, then, as real wages rose, the binding wants of culture would have made Tsimane' spend less time working for wages, not more. By spending more time in wage labor, Tsimane' are saying they want to buy more in the market, either to quench new wants or to buy things they can no longer fetch from their land. Or perhaps both. Or perhaps neither. In Table 7.2 I tabulated the self-reported reasons for wanting cash. There we might find an answer to why Tsimane' work longer. The need to have cash to buy food -- arguably to make up for shrinking food supplies from their land -- was, frankly, meek, as was their wish to have cash to buy luxuries. Only 7.50% of respondents said they wanted cash to buy food while an even lower percent (3.75%) said they wanted cash to buy luxuries. Instead, we see a hint of wanting cash to buy useful things, the scaffold from which to make life easier and better. Clothing, transport, metal tools, and modern medicines fall under the bundle of the useful, a bundle that upgrades traditional culture without wrecking it. True, Tsimane' could make their own clothing. True, they could travel by foot or by dugout canoe. True, they could fashion wooden tools. True, they could use wild plants to salve illness. But for all these changelings they can find replacements in the market, replacements cheaper and sturdier than traditional wares. At least for now, they work to pay for cultural renovations more than to build a new cultural home.

Levels and trends in days worked, total earnings, and implicit daily wages based on disaggregated household-level and individual-level statistics. In the taut tale so far, I downsized into a quarterly or into a yearly whole the pastiche of answers given by household heads about their household (2000-2001) or by adults about their earnings (2002-2010). Useful in boiling down the welter of answers to spot blunt trends, abridged statistics, by winnowing the sample size, weaken the trust we can place in findings. For this reason, here I turn to the raw information with its larger sample. I assess in a more orderly way the external validity of the longitudinal study and the growth rate of earnings, this time also stressing the statistical confidence in results.

In Table 7.11, part I, I summarize the attributes of the surveys used. Column A has details about all the household-level and individual-level surveys we did during 2000-2010. Column [1] shows that the sample size alternates between 1,343 and 1,114 observations, with more observations if we use a recall period of one week and fewer observations if we use a recall period of two weeks. In column B[i] I summarize information from people followed through nine successive yearly surveys, from 2002 until 2010. Columns B[ii] and C go together. In column B[ii] I include information for the yearly survey of 2008 of the longitudinal study because I want to weigh how far one can generalize from the results of the longitudinal study to other Tsimane'. I compare results from column B[ii] with the results from column C. In column C, I summarize information from the baseline (2008) of the randomized-controlled trial. Coming from a larger sample of 40 villages beyond the 13 villages of the longitudinal study, the baseline sample of the randomized-controlled trial shows what the rest of the Tsimane' population might look like. In column 10 I show the gap in the mean values for 2008 between the sample from the randomized-controlled trial and the sample from the longitudinal study, the gap standing for the external validity of the longitudinal study, with smaller gaps signaling that results from the longitudinal study apply to other Tsimane'. A caveat about the comparison of results for the year 2008 between the two studies. In 2008 the sample from the randomized-controlled trial had 229 people whereas the sample from the longitudinal study had 63 people. The small size of the sample in the longitudinal study renders imprecise -- though still informative -- the comparison of mean values between the two studies. Last, in column D, I join the sample of people from the

2002-2010 longitudinal study (column B[i]) with the sample of people from the baseline of the randomized-controlled trial (column C). The advantage of the sample in column D over the sample in column A is that in column D I do not mix information collected at the level of households for the entire households, with information collected from adults for themselves alone. The sample in column A is best because it covers a longer time.

Insert Table 7.11

The values from Table 7.11 and Table 7.6 mirror each other, roughly, with dissimilarities partly stemming from the samples. For instance, in Table 7.6 I included all quarterly information for 2002 (three quarters) and 2003 (four quarters), whereas in Table 7.11 I selectively picked quarterly information only for May-October (quarters 1-2) and went further by averaging all the observations for a person for this period into one yearly value[xxi]. The values in Table 7.11 are slightly higher than the values in Table 7.6. For instance, in Table 7.11 people said they worked an average of 8.2 days (SD=4.8) and earned 275 nominal *bolivianos* (SD=308) during the two weeks before the interview. For the same recall period, the aggregate statistics of Table 7.6 show that people worked 7.2 days (SD=4.7) and earned 233 nominal *bolivianos* (SD=279).

People in the longitudinal study earned more than other Tsimane' (Table 7.11). A review of the values in column 10 shows that -- compared with people in the randomized-controlled trial, the yardstick to judge others -- people in the longitudinal study enjoyed about eight more *bolivianos* in nominal or in real daily wages, and 88.6-93.9 more *bolivianos* in nominal or in real earnings. They also worked more. During the seven days before the interview, people in the longitudinal study worked an average of 1.6 more days than people in the baseline of the randomized-controlled trial. In the longitudinal study -- if one trusts what people said -- people reported working an average of 6.1 days (SD=1.7) during the week before the interview. The average is high and the median (not shown in Table 7.11), 7 days, is even higher that the average. Recall from the earlier remarks that 73.02% of the 63 people surveyed during 2008 in the longitudinal study said they had worked all seven days during the week before the interview. If one believes the statistic, we have here a bustling backland rural economy with the semblance of full employment and overtime work for some. In the quieter settings outside the river Maniqui and the longitudinal study, those employed worked 4.4 days (SD=2.2) during the week before the interview, more reasonable than the outlandish figures from the longitudinal study.

The yearly growth rates in Table 7.12 buttress the earlier analysis of a rise in yearly real wages and in real earnings but lend weaker support to an undisputable rise in the length of employment. Yearly growth rates for the longer study (2000-2010) were smaller -- sometimes insubstantially -- than for the shorter study (2002-2010) because the longer study included noisier measures from household heads who ciphered amounts for their entire household. The difference between the two studies was starkest with the yearly growth rate in the number of days worked. The yearly growth rate in the number of days worked with data from the longer study ranged from 3.1% to 5.1% whereas the yearly growth rate with data from the better-measured, more trustworthy, but shorter study ranged from 8.8% to 9.7%. Irrespective of the study or recall period, yearly real daily wages and real earnings grew: wages by 2.7%-4.0% and earnings by 5.9%-13.8%, always with trustful results.

Insert Table 7.12

Table 7.13 shows the contribution of the yearly growth rate of real wages and days worked to the yearly growth rate in total real earnings. As before, we see that real earnings grew from the surge in the number of days worked more than from a rise in real wages. Both helped, but the growth in the number of days worked accounted for 52.54% to 70.29% of the growth in real earnings, while the yearly growth in real wages helped far less: 28.99% to 45.76%.

Insert Table 7.13

Who works for wages and what contributes to wage earnings?

We still do not know why Tsimane' work for a wage. What makes some Tsimane' find jobs or avoid them could have to do with traits of the person, household, or village. Culprit movers could be age, sex, or schooling. But traits of a household, like the ratio of dependent consumers to workers, could sway decisions to become a drudge. And one could even envision village markers, like nearness to town, population size, and wages playing a role. Villages with loggers, cattle ranchers, and highland smallholders dwelling at the village gate would make it easier for Tsimane' to join or leave the workforce.

Once we understand why hirelings join the workforce, we need to understand what determines their effort, wage, and earnings. To get neat answers, we need to make sure that some of the things that thrusts workers to look for a job do not also determine their earnings. In rich countries, the stage in the lifecycle of a woman determines whether she will look for a job. Textbooks teach that mothers with offspring sidestep the workforce to care for their children, returning to work after their children grow up (Wooldridge, 2002, pp. 564-566). Once in the workforce, not her brood, but her skills, schooling, beauty, report, mind, mien shape how much employers pay her. One can then estimate the effects of such things as schooling on earnings, conditional on the chances of having joined the workforce in the first place, with those chances determined by her offspring's age. At least in theory. Among Tsimane', I find nothing matching what works in rich countries, nothing, that is, that predicts whether people will join the workforce, but not their effort, wage, and earnings. Because we have nothing that forecasts the first step, looking for a job, but not the second, earnings, we are left with the shopworn trope of finding coincident links between happenings but no unimpeachable explanation for the links.

To find out what propels adults to join the labor force and -- once in it -- what determines their earnings and implicit wage, I use clean data from the 2002-2010 longitudinal study of individuals and from the baseline of the randomized-controlled trial, but not without purging it further.

First, I drop people under 16 years of age because Tsimane' marry, set up independent households, and join the workforce at about this age (Chapter 5). I also drop people over 65 years of age because they do not work for wages. We do not know when Tsimane' stop working for a wage. Self-reported answers from older people about their age teem with mistakes, either because they were clueless and guessed, or because they fudged their birthdate to access government pensions and, over time, believed they had the age they mentioned (Chapter 5). Figures 7.5a-7.5c show that that the share of people without wage earnings rose with age, peaking at 65 years of age in the longitudinal study (Figure 7.5a), at 50-55 years of age in the baseline of the randomized-controlled trial (Figure 7.5b), and at 65 years of age in the pooled sample of the two studies (Figure 7.5c). Only 18 wage earners were older than 65 years of age;

they accounted for two percent of the sample of able-bodied adult (men), so dropping them will not muddle findings.

Insert Figures 7.5a-7.5c

Second, to dilate the sample size I cabin the analysis to answers about the seven days before the interview. Again, the restriction does not upset findings. The randomized-controlled trial only had information for the seven days before the interview, so including the two recall periods (1-7 and 8-14 days before the interview) would have shut out from the analysis people who took part in the randomized-controlled trial. The longitudinal study had information about the two recall periods, so we can turn to it to assess the correlation of answers between the two weeks when assessing the dangers of binding the analysis to the past seven days. In the longitudinal study, the number of days worked during the seven days before the interview and the number of days worked during the 8-14 days before the interview were positively correlated -- as one went up, so did the other -- and the same was true of wage earnings for the two time blocks[xxii], suggesting that one can use, untroubled, information from either week because they mirrored each other. Last, I circumscribe the analysis to men because few women worked for wages[xxiii].

In Table 7.14 I pinpoint why some Tsimane' earned no cash from wage labor. Among predictors, I have markers of the person like their age and years of schooling, and three telltale signs of nutrition: standing height, body weight, and mid-arm muscle area. I also include the number of girls, boys, adult women, and adult men in the household at the time of the survey. For the head count, I defined an adult as someone 16 years of age or older. In some of the analysis I also include the village daily wage.

Insert Table 7.14

I have good reasons for choosing these predictors. One would think that the chances of dropping out of the labor force would increase with age and decrease with schooling. During the study, older people got help from kin and -- during the last years of the study –they also got help from government pensions sluggishly reaching the study area. The two forms of help -- kin and pensions -- lower the need of elder Tsimane' to seek paid work. Figures 7.5a-7.5c buttress the interpretation, showing that the chances of being wageless rose with age. Schooling ought to make it easier to find employment outside the village. Skills tangentially learned in school like Spanish, and those most likely learned in school like writing, reading, and math, however badly learned, bring comfort to the job seeker and send cues to the employer about the applicant's quality. Three anthropometric measures -- height, weight, musculature -- speak to good health, so I would expect them to increase the chances of joining the labor force since the drudge chores laborers will have to do once hired, such as clearing forests, portaging wooden planks to rivers, hacking weeds and bramble with blunt cutlasses, all need sinew. In industrial countries, young offspring weigh in on a woman's decision to join the workforce, but we do not know if the same happens among Tsimane' men, so I bring in the number of girls, women, boys, and men in the household at the time of the survey to find out if demography matters. We saw earlier that wages played a role in the resolve to join the workforce and, coming in two forms, I include them both: the village current daily nominal wages, with and without lunch.

Besides these predictors, I put others to increase trust in the findings. I added a variable for the type of study and for survey year, the first, naturally, to control for the study (observational or randomized) and the second to see trends over time in the likelihood of being wageless, a topic discussed before but readdressed here with more rigor. Seen and unseen, measurable and nondescript abstruse features of a village or of a year could befog findings. Besides wages, steadfast traits of a village, such as nearness to employers, latitude, altitude, or the price of crops relative to wages, could push people to join the workforce, or pull them away from it. Some years had outlandish values for wage earnings and other years had aberrant weather, too rainy or too dry to reach the workplace. To rein in the unwanted role of seen and unseen but steady attributes of a year or a village I use a commonplace statistical approach, outlined in the notes to Table 7.14.

Table 7.14 show that two things raised the chances of having no wage earnings, and two things lowered it, or raised the chances of joining the workforce. An additional decade of age increased by two percentage points the chances of having no wage earnings. The finding, congruous with Figures 7.5a-7.5c, nevertheless points to a small real-life effect. After a decade of normal aging, the chances of dropping out of the workforce would rise by a trifling amount.[xxiv] As before, we see that later surveys raised the chances of having no wage earnings. Once we control for age and for unyielding features of the year and village, we find that a survey done a year later would raise by two percentage points the chances that a man would earn no income from wage labor. A rise of two percentage points in the chances of dropping out of the labor force is noteworthy. All things the same other than time, a survey done 10 years later would increase the chances of having no wage earnings by 20 percentage points, a large amount by any yardstick. Schooling and good nutrition made it less likely that a man would have no earnings from wage labor. For each additional school grade finished, a man had a 2.5 percentage-point lower chance of having no wage earnings, while one more kilogram of body weight -- controlling for standing height and musculature -- went along with a 0.4 percentage-point lower chance of having no earnings from wage labor. Some things did not matter. The demographic makeup of the household at the time of the survey did not matter much, nor did the village daily wage[xxv], nor did the type of study, nor did height, nor did musculature. In sum, aging and later surveys raised the chances that a man would have no wage earnings, while schooling and body weight did the opposite.

Having seen why men joined the workforce, I next assess why some earned more. Table 7.15 shows how a handful of predictors affected the amount of real earnings from wage labor of working men (columns 1-2) and their real daily wage (column 3). Heavier men with more schooling living along the river Maniqui earned more than thinner men with less schooling living elsewhere.

Insert Table 7.15

I start by discussing the results from the first two columns. Schooling determined not only whether a man would join the workforce, as just seen, but also how much they made, and the effect had bite. One more year of schooling raised wage earnings by 5.5% to 6.0%, with the spread of estimates reflecting the approach taken. A man finishing five years of primary school would earn 25% to 30% more than an unlettered groundling in the workforce. Over a working lifetime, the profits from schooling would add up.

Nutrition helped, but not as much, it seems. An additional kilogram of body weight raised earnings from wage labor by 0.9%. To vivify the finding, I assess what would happen to the earnings of an average man if he gained the weight implied in the first two columns (0.9%/kg). The average man in the sample of Table 7.15 weighed 63.3 kilograms (median=62.9 kilograms; SD=7.8 kilograms). If the body weight of such a man increased by one standard deviation (7.8 kilograms) -- an admittedly large amount for an adult -- his earnings from wage labor would rise by 7.02%. Under normal conditions how long would it take the average man to gain 7.8 kilograms? The average man's body weight increased by 0.29 kilograms each year, so he would need to wait 26 years to become 7.8 kilograms heavier and profit from the 7.02% rise in real wage earnings[xxvi]. Together, these numbers show that in real life weight gain would not boost earnings by much.

As before, we see higher and higher real earnings from wage labor over time. From one survey year to the next, real wages rose by 8.0%, in the middle of the blunter earlier estimates of yearly growth, which ranged from 5.9% to 12.9% (Table 7.12, section D). Unlike earlier findings, the findings in Table 7.15 show that it matters where the study took place. Workers who took part in the longitudinal study along the river Maniqui earned 38.8% to 112% more than workers who took part in the baseline of the randomized-controlled trial living elsewhere. Higher earnings along the river Maniqui could spring from higher wages reflecting the struggles of cattle ranchers, logging enterprises, and highland smallholders vying to recruit employees by offering them better pay, a subject addressed below.

In column (3) I show what drives the implicit daily real wage. All the worthy drivers of earnings resurface as worthy drivers of implicit wages. Men with more schooling and body weight enjoyed higher daily real wages than thinner men with less schooling. One more year of schooling and one more kilogram of body weight increased daily real wages by 1.4% and 0.6%. Men living along the river Maniqui earned a 26.4% higher daily real wage than their peers elsewhere, a finding that explains why men along the river Maniqui earned more from wage labor, as noted above. Differences in daily wages in abutting areas of the lowlands for doing roughly the same type of jobs would hint that employees find it hard to perambulate between workplaces in search of the best pay. Perhaps workers get forever tethered to one employer and one place, perhaps jobs seethe too much to spur workers and make it worthwhile for them to travel in search of higher rewards. One last point about the results of Table 7.15 (column [3]): daily real wages got better through time, rising by 3.9% each year, in the upper range of the earlier estimates, which went from 2.7% to 4.0% per year (section B, Table 7.12).

Faced with 54 villages dissimilar in their nearness to towns, populace size, crop prices, wages, altitude, and much else, one should ask whether the effects of the strongest drivers of joining the labor force, of real earnings, and of daily real wages varied helter-skelter between villages, or whether their effects remained unchanged across villages. Two reasons motivate the question. First substance. Villages could temper effects. Villages hold households, and households people. With workers encrusted in households and households in villages, the moat of a household or a village could discomfit a man's plan to step into the workforce or his wage earnings, however well qualified the man. For instance, schooling raised the chances that a man would join the workforce and that he would also make more money from wage labor, but this could happen only if he lived in a village near employers. In a cocooned village far from bustling enterprises, a schooled man with grit would not profit from schooling, pleasures of learning aside. The village setting might allow traits of a worker, like schooling or good health, to influence his choice of joining the workforce and, once in the workforce, his choice of how

much to work. Until now, I supposed that the effects of a trait on wage labor would be the same from one village to the next, a strong assumption. Unless we examine the possibility that some of the effects of a trait on wage labor could be the same in all villages while -- in addition -- some of the effects of the same trait could change haphazardly between villages, our appraisal of the trait's effect could be wrong, partial, or both.

Among all the features that drove a man to join the workforce, only schooling had a significant effect, an effect made up of a part that remained the same for all villages and a part that varied at random between villages (Appendix B). In Table 7.16 I show the effects of finishing primary school on the (i) the chance that a man would sidestep the workforce and, for those who entered the workforce, (ii) on his earnings from wage labor. I split the effects of schooling into those that remained fixed for all villages and those that varied at random between villages. First, the unchanging effects. In column [1] of Table 7.14 we see that with one more year of schooling a man would be two percentage points less likely to be wageless. Finishing five years of primary school would make him 10.9 percentage points more likely to join the workforce (column 1, Table 7.16). But the fixed estimates varied by the amount of schooling in a village. Next to the fixed, average effect of 10.9 percentage points, a man in a village with a less schooled workforce was 12.4 percentage points less likely to be wageless while a man in a village with a more schooled workforce was only 9.3 percentage points less likely to be wageless. Villages changed how schooling affected the chances of being wageless. And for men who joined the workforce, villages also changed their wage earnings. Table 7.16, column (1), shows that a man who finished five years of primary school earned 23.72% more than a villager without schooling. Depending on the schooling in the rest of the village, the effects ranged from 19.3% to 28.0% more earnings. The schooling level of neighbors changed men's wage earnings.

Insert Table 7.16

Summary of trends in earnings from wage labor. From 2000 until 2010 cattle ranchers, Tsimane', and the government took the lead as employers. Other than a rise in the share of workers hired by loggers during the dry season, the share of workers hired by other employers did not change much between seasons. Most Tsimane' worked for one employer during the two weeks before the interview, a finding intimating that employment is risky, but that also lays bare the shortcoming of having asked workers to remember their employment history for the immediate past. I did not expect to see a drop in the share of Tsimane' working for a wage and – among those who did – I did not expect to see them toiling more. I would have thought that with economic development one would see a growing share of people working for wages. Apparently not. Real earnings from wage labor rose, more from the increase in the number of days people worked than from the meek growth in real wages. What to make of an increasing share of people leaving the pool of paid laborers while others worked and earned more, I cannot altogether tell. I hazarded a couple of conjectures perhaps worth testing in the future. Those who worked for pay were men, young, and schooled. Almost no woman worked for pay. The chances of being wageless rose with age and surveys done in later years, and declined with schooling and body weight, while the amount of earnings, adjusted for inflation, rose with schooling, body weight, and later surveys. Schooling, age, and body weight predicted the chances of employment and the level of earnings. The noteworthy impact of a man's schooling on his propensity to work for a wage or on his earnings varied by the amount of schooling of

those around him. Having lettered neighbors increased the chances that a man would work for a wage, and, as well, the amount of cash that he would earn from the job.

Sales

To fill in the landscape of the Tsimane' economy I turn to sales, the other way Tsimane' engage with the market economy. Those surveyed during 2000-2010 sold 147 different types of goods (Appendix A). The array hides the truth that most sales came from just two goods, rice and plantains, laced with a hefty dash of forest wildlife. Reliance on the sale of a few goods, like reliance on a few jobs, tells us that Tsimane' have found a snug place in the regional economy, supplying crops they have husbanded for centuries, and working in jobs demanding their ancient skills, deep knowledge of place, and brawn.

Goods sold: Yearly data. In Table 7.17 I lump goods sold during the week and the two weeks before the survey into nine bundles: four crops (rice, manioc, bananas-plantains, maize), other farm crops, tree crops, domesticated animals, animal products, artisanal goods, forest wildlife, commercial goods, and unidentified, nameless items. For each bundle in each year, I compute the bundle's share in all sale transactions and its share in the value of all sales for the year. To illustrate: of all sale transactions in 2000, 24.54% of the transactions came from selling rice; these sales accounted for 43.81% of all sale earnings in 2000 (Table 7.17, sections A-B).

Insert Table 7.17

Data quality on goods sold. I assess the quality of sales data through four lenses: share of unidentified items, large changes in values between years, forward telescoping, and digit heaping.

Except for 2000 when surveyors could not identify three percent of the goods sold[xxvii], in other years unidentified items accounted for less than 0.1% of the wares sold (Table 7.17). The presence of unidentified goods during the first year of the study points to surveyors' shyness and lack of savviness as likely culprits; confronted with bizarre name for goods, surveyors did not press subjects for details. Things changed by the end of the study. During the last five years of the study (2006-2010), the team politely wormed its way to identify almost all the articles mentioned by subjects.

To spot jolting changes in values between years, I compare yearly statistics from the same study: (i) the household surveys of 2001 and 2002 or (ii) the yearly longitudinal surveys of individuals during 2004-2010. Nothing awkward jumps out from the comparison of 2001 and 2002, but the yearly longitudinal survey of individuals shows some oddities. For instance, between 2006 and 2008 the number of rice sales as a share of all sale transactions plummeted from 27.19% to 11.65% (Table 7.17, section A) while the correlative share of rice earnings in the total worth of sales dropped from 50.48% to 19.97% (Table 7.17, section B). These large changes need not bespeak mistakes for they could reflect large changes in rice prices.

Table 7.18 shows the ubiquity of forward telescoping in sales data. In the last column I show the probability of not selling anything during the 8-14 days before the interview compared with the seven days before the interview. Respondents were 12.78-14.72 percentage points more likely to say they had sold nothing two weeks ago than one week ago[xxviii]. To prod the topic further, I restrict the analysis to people who sold goods in both weeks and compare the number of items and earnings of goods sold each week. The sample size shrinks, as will be discussed later, because most people only sold in one of the two weeks. Irrespective of the year, the mean

number of items sold during the past week was always higher than the mean number of items sold two weeks ago. The grand mean and the grand median number of goods sold during the previous week were 1.86 and 1.59, compared with the grand mean and the grand median of 1.59 and 1.32 for the number of goods sold two weeks ago. In the amount of cash earned from sales we again see tumid measurements for the more recent past, but we cannot tell how much of the bloating comes from having sold more wares in the immediate past or from debasing values from the sale of older wares. Perhaps one pound of rice sold last week appears as having more value than one pound of rice sold two weeks ago -- even if rice prices did not change in a fortnight -- because people assigned a higher price to the pound of rice sold in the immediate past than to the pound of rice sold further back in time. Although we cannot pull apart the two effects (of quantity, of price), we see that the grand mean and the grand median earnings of goods sold in the past week were higher than the grand mean and the grand median earnings of goods sold two weeks ago (past week: mean=131 and median=136 *bolivianos*; two weeks ago: mean=113; median=103 *bolivianos*)[xxix]. Among hard retailers, the average number of goods sold in a year, and the average earnings from retail were higher for the past week than for two weeks ago, but one cannot trust the findings owing partly to the small yearly samples.

Insert Table 7.18.

I wrap-up the section on data quality by turning to digit heaping. As we saw in Chapter 5-6 and as we saw again with wage labor in this chapter, Tsimane' like to round the last digit of estimates to multiples of five. The same happens with sales. Restricted to those who sold, Table 7.19 shows that, in a year, 55% to 57% of the value of sales reported ended in zero and 19% to 20% of the values ended in five. Thus, only a quarter of the yearly records had exact information on retail earnings.

Insert Table 7.19

Snapshot and trends of sales.

Those who do not sell. Half of all households and people (51.03% to 52.53%) did not sell, either one or two weeks ago (Table 7.17). The share went from a low of 28.28% of households in 2002 to a high of 61.81% of people in 2009. Depending on the sample, the share of entities that did not sell rose by 3.33% to 5.32% each year (Table 7.20, section [a]). This is odd. We saw that a growing share of Tsimane' did not work for pay. One would think that they would have turned into sellers to make up for the loss of cash. Instead, Tsimane' are withdrawing from both paid jobs and from retail at the same time.

Insert Table 7.20

Those who sell. When averaged over all the years of the study, slightly less than half the yearly sample sold articles during the 14 days before the interview (mean=46.44%; median=44.42%)[xxx]; most sold articles during one of the two previous weeks, not during both. On average, in a typical year, a small share of households or people sold goods during each of the two weeks before the survey (mean=15.92%; median=12.10%). Depending on the sample used to estimate trends, the share of entities selling declined by a yearly rate of

3.47% to 4.99%, while the share of hard retailers selling during both weeks fell by 3.28% to 11.58% (Table 7.20, section [a]).

Goods sold. Based on sections A-B of Table 7.17, Figure 7.6 shows that, whether ranked by frequency of sales or by earnings, rice, bananas-plantains, and forest wildlife topped the list of items sold. During an average year, in frequency of sales, bananas-plantains, rice, and forest wildlife accounted for 30.52%, 22.28%, and 13.90% of transactions while in earnings, rice, forest wildlife, and bananas-plantains ranked at the top, accounting for 32.51%, 25.89%, and 19.22% of earnings[xxxi]. Four crops -- rice, manioc, bananas-plantains, and maize -- accounted for most sale transactions (70.21%) and for most sale earnings (62.75%)[xxxii].

Insert Figure 7.6

After these goods, in frequency of transactions, came manioc (11.58%), maize (5.91%), and artisanal artifacts (5.78%), while in share of earnings came manioc (6.25%), barnyard animals (5.00%), and maize (4.77%). Figure 7.6 shows that other goods -- tree crops, other farm crops, animal products, commercial merchandise, and unidentified items -- all these accounted for a negligible share of sale transactions and sale earnings. I was struck by regraters of commercial merchandise, Tsimane' who bought industrial wares like flashlight batteries in town for resale to villagers in the backlands. With a faint presence, accounting for a small share of sale transactions (0.69%) and an even smaller share of total retail earnings (0.42%), they nevertheless could be heralding the embryonic outline of a Tsimane' merchant class (Table 7.17, sections A-B). From all the evidence I conclude that retail rested on selling four crops, sylvan goods, and, to a lesser degree, selling domesticated animals and goods fashioned from local materials.

From 2000 until 2010, sellers sold an average of 1.46 distinct goods during the two weeks before the interview (SD=0.71; median=1.38) (Table 7.17, section C). This statistic meshes with the narrow range of goods described earlier. Tsimane' sell a handful of goods, and if one asks them about their sales in the recent past, they would say they had sold just one article.

To analyze time trends, I start with Figures 7.7a-7.7b, which display the year-to-year share of sale transactions (Figure 7.7a) and sale earnings (Figure 7.7b), by categories of goods sold. As a share of all sale transactions, the share of sale transactions for many goods and bundle of goods did not change from one year to the next (Figure 7.7a). For instance, Figure 7.7a shows that the share of sale transactions of manioc, of bananas-plantains, and of forest goods did not change noticeably between years. Rice differed. The share of sale transactions from rice reached nadirs in 2002 and 2008 supervened by a pinnacle immediately after, in 2003, and by new highs in 2009-22010. From 2002 until 2003, sale transactions of rice as a share of all sale transactions doubled, from 14.48% to 30.35% and, in the longitudinal study of individuals from 2008 to 2009-2010, the share rose from 11.65% to about 19%.

Insert Figures 7.7a-7.7b

Unlike yearly trends in the share of sale transactions for bundles of different goods, which did not change much, other than rice, trends in the share of sale earnings for different bundles varied more from one year to the next. Except for the last two years of the study (2009-2010), rice and forest wildlife ruled sale earnings, but in an undulating, complementary fashion; when the share of one went up, the share of the other went down, each following its own

sinusoidal-like wave. The share of earnings from the sale of bananas-plantains remained the same until 2009-2010, when it jumped by ten percentage points, from a yearly average of 28.94% during 2000-2008 to 39.19% during 2009-2010. One other finding stands out. The share of earnings from the sale of manioc doubled from the early years of the study (2000-2005), when it accounted for 4.09% of all earnings from retail, to the later years of the study (2006-2010), when it accounted for 8.75% of all earnings from retail.

Table 7.20 (section e) shows yearly trends in the share of sale earnings for the goods of Table 7.17. I estimated the trends using three samples, with results shown in three columns of Table 7.20: *[i]* all entities (households and individuals), *[ii]* individuals surveyed from 2002 until 2010, including individuals surveyed in the baseline (2008) of the randomized-controlled trial, and *[iii]* individuals surveyed in the longitudinal study of 2002-2010 without those surveyed in the baseline of the randomized-controlled trial.

Depending on the sample used, we see a rising share of three goods in total earnings: manioc by 1.86% to 4.63%/year, bananas-plantains by 2.70% to 3.79%/year, and tree crops by 0.99% to 6.49%/year. The share of sale earnings of four goods shrunk in all samples: maize (range: 6.38% to 18.67%/year), other farm crops (range: 5.90% to 25.38%/year), domesticated animals (range: 4.05% to 11.68%/year), and animal products (range: 10.96% to 16.47%/year). It is harder to see trends in the yearly sale of other goods because trends rose or fell depending on the sample used.

We said earlier that most household sold 1.46 goods during the fortnight before the interview (SD=0.71). Figure 7.8 and Table 7.20 (sections b1-b2) show a yearly decline in the mean number of articles sold and a milder yearly decline in the variation of articles sold. The mean number of articles sold fell by 2.38% to 2.66%/year while the coefficient of variation in the number of articles sold fell by 1.51%/year to 0.11%/year. The yearly decline in the coefficient of variation of distinct goods sold, though true, is deceptive because it stems from the high and the low value at the start and at the end of the series; dismissing the weighty end values, Figure 7.8 shows that the coefficient of variation remained fixed and flat at 0.5 for most of the study.

Insert Figure 7.8

Hard retailers selling during each of the two weeks before the interview sold, on average, 3.46 goods each time we surveyed them (median=2.89) (Table 7.18). Figure 7.9 and Table 7.20 (sections b3-b4) show that the number of items sold by hard retailers fell by 5.95% to 7.07%/year while the coefficient of variation declined, as well, by 1.14% to 4.93%/year. Thus, we see over time fewer goods sold, less variation in the types of goods sold, and, among staunch retailers, we see sharper declines in the amount and variety of goods put out for sale.

Insert Figure 7.9

Sellers' earnings. Table 7.19 shows yearly earnings from sales by people who sold anything during the previous seven or 14 days. In nominal values, sellers earned a total average of 181 *bolivianos* and a median of 80 *bolivianos* during the previous fortnight (Table 7.19, columns [4]-[5]). Tolerable as a first scan, these results fall short because they do not adjust for inflation or for the number of days selling. To delve further, in Table 7.21 I build on the results from Table 7.19 to show mean and median inflation-adjusted total earnings during the previous fortnight (columns [4]-[5]), and the mean and median inflation-adjusted earnings from

sales in a day (columns [6]-[7]). To tighten the analysis, I restrict the results of Table 7.21 to data garnered from individuals during 2002-2010, with growth rates computed with and without information from the baseline (2018) of the randomized-controlled trial.

Insert Table 7.21

During the previous 14 days, a seller earned a mean and a median of 196 and 88 *bolivianos*, adjusted for inflation (Table 7.21, columns [4]-[5]). Averaged over the recall period from which these values were computed, a seller earned an average and a median of 16 and 6 *bolivianos* per day, again expressed in real values (Table 7.21, columns [6]-[7]). Put in USA dollars at the current exchange rate in the regional towns, a retailer, just from selling wares and from nothing else -- no wage labor, no rental income, no transfers received -- would have earned an average cash income of USA$2.14 per day and a median cash income of USA$0.80 per day[xxxiii]. These amounts would put many sellers below the threshold of extreme poverty, defined as earning less than USA$1-1.25/person each day, and would put most of them below the line separating the poor from the better-off, defined more generously as earnings less than USA$2.50/person each day (Bauchet et al., 2018, p. 3). The spread of daily earnings from retail -- from USA$0.80 to USA$2.14 -- falls far below the daily earnings from wage labor for a man, which ranged from US$4.75 to USA$5.87.

Table 7.20 (section d) shows that mean real earnings from total sales declined each year by amounts ranging from 0.70% to 7.77%. Analysis restricted to yearly data of individuals from 2002 until 2010 shows that median real values rose by 3.25%/year. But linear estimates befog visual evidence. Here graphs partially controvert numbers. Figures 10a-10b show trends in total real earnings and in daily real earnings. The graphs show that real earnings declined and then rose. Figure 7.10a shows that the trend in total real earnings looks like a U, falling until 2004, and then rising, with a spike in 2008, a low point in 2009, and a return to the upward trend line by 2010. In Figure 7.10b I redraw Figure 7.10a to express values as daily real earnings from sales. The same pattern comes back. Mean and median daily real earnings from sales declined until 2004, rose thereafter, peaked in 2008, dropped in 2009, and again converged to the upward trend line that had started back in 2004.

Insert Figures 10a-10b

Table 7.20 (section d3) shows that the coefficient of variation of the real value of total sales declined by 10.01% to 11.72%/year, but Figure 7.11shows no smooth trend in the variability of real earnings. The coefficient of variation moved up and down with sharp turns, peaking in 2003, then syncopated down until 2010.

Insert Figure 7.11

In sum, Tsimane' come across not as penny capitalists, but as unenterprising merchants. Half retailed nothing during the fortnight before the interview and, when they sold, they sold in one of the two weeks, not in both. From 2000 until 2010 the share of hard and soft retailers dwindled every year. When they sold, they sold few goods (at most one or two), the number and variety shrinking through the years. Against this backdrop, rice, bananas-plantains, and forest wildlife provided unalloyed success, outweighing other goods in frequency of sales and in

revenues. Cash from selling rice and bananas-plantains complemented each other, for when one went up, the other went down. Retailers did not earn much each day -- USA$0.80 to USA$2.14 -- placing them inside or around the liminal boundary of the globe's daily poverty line for a person.

Goods sold: Quarterly and seasonal data. In this section I probe how sales changed between quarters and between seasons from May 2002 until April 2003, the only time when we repeatedly measured sales in a year. I want to find out if the amount of sales and what people sold changed with the seasons, or if sales provided a steady trickle of cash. What I have to say and the graphs of this section rely on Table 7.22.

Insert Table 7.22

Because of the small samples during the second and third quarter of 2003, I drop these samples when computing shares for the graphs and when discussing results. The 2002-2003 survey centered on individuals, asking, once each quarter, about their sales during each of the two weeks before the interview. Done this way, a person had as many observations as quarterly surveys they completed. Half the sample surveyed in a quarter did not sell goods during the 14 days before the interview (mean=57%; median=56%). A slightly higher share of people retailed during the rainy season (59.26%) than during the dry one (56.31%). The gap remained small and did not change after dropping people surveyed during the second and third quarters of 2003; the new shares were 60.51% for the rainy season and 55.17% for the dry one. Thus, seasons do not determine the chances of selling[xxxiv].

In Figures 7.12a-7.12b I compare seasonal changes in what was sold and in the value of those sales. I want to know, for instance, what share of sale transactions came from selling rice during the rainy season and what share during the dry season (Figure 7.12a), and I also want to know -- in the total value of sales in a season -- what share came from selling rice (Figure 7.12b)

Insert Figures 7.12a-7.12b

Figure 7.12a shows that people were equally likely to sell most types of goods during either season. The share of transactions from the sale of goods such as domesticated animals, forest wildlife, or tree crops did not differ between seasons. However, the share of transactions from the sale of rice, manioc, bananas-plantains, and maize differed between seasons. People sold rice and manioc during the dry seasons more than during the rainy season, and they were more likely to sell bananas-plantains and maize during the rainy season. In traffic, rice and manioc dominated sales during the dry season while maize with bananas-plantains dominated sales during the rainy season; sale of other goods showed no appreciable difference between seasons. Figure 12b adds depth to these conclusions. In the share of value from selling farm crops, one can see that rice and manioc continued to dominate sales during the dry season, and maize with bananas-plantains continued to dominate sales during the rainy season. Surprisingly, the sale of forest wildlife during the rainy season towered above all else in importance, accounting for more than half (52%) of the value of goods sold -- and this happened even though the frequency of selling forest wildlife was the same in each season. Amphibious between farms and forests, Tsimane' make money by selling crops during the dry season and wildlife during the wet one. Section C of Table 7.22 shows that whether in the dry season or in the rainy one, Tsimane' tethered themselves to selling one crop.

I press further to see if the average amount of money a seller made each quarter remained constant between seasons, and I find that it did. Drawing on Table 7.22, I divide the total value of items sold by the number of retailers in each seasons, but I edit the amount by the number of quarterly surveys -- three for the dry season and two for the rainy one -- so I arrive at an amount of earnings from sales for a retailer during an average quarter in the rainy season or in the dry season. A seller in the dry season earned 40 *bolivianos* while in the rainy season they earned 44 *boliviano*s, or 10.18% more[xxxv]. By swiveling what they sold -- wildlife in the rainy season, crops in the dry season -- sellers kept steady the dribble of cash coming into their household.

Who sells and what contributes to earnings from sales?

We saw that paid jobs were exclusionary, shutting out all women and any man too young or too old for manual work. Only men in their prime working age applied for jobs or got them. Retail differed. It was embracive, opened to any woman or man of any age. First, consider age. For understandable reasons having to do with strength, men stopped doing wage labor once they reached their 60s. Less draining than wage labor, retail faithfully accompanied people into their old age. Figures 7.13a-7.13c present the share of people, by age cohorts, who did not sell. The figures show a flurry of dots after 60-65 years of age; some of the aged eschewed retail – shown in the horizontal line at the upper right-hand corner of the figures – but many older people continued to rely on retail – shown as a downward scatter of dots from the northwest to the southeast in the bottom right-hand corner of the figures. Figure 14 shows the age distribution of any person who sold anything during the seven days before the interview. Since we restricted questions about retail to people 16 years of age or older, the tail of the graph to the left of age 16 is thin from the way we asked about retail but notice that there is no decline in retail after people reached 60-65 years of age. Next, consider the ubiquity of women in retail. Figure 7.14 shows all retailers; women accounted for 47.41% of them. In charge of child rearing and the household, women doubtlessly found it easier to earn cash by selling wares when merchants stopped in the village, or when neighbors went to town to sell. Without upsetting their daily chores, women could even consign articles to neighbors for sale in town.

Insert Figures 7.13a-7.13c and Figure 14

Beyond these two features (sex and age), I use the same approach to predict cash earnings from retail as I used to predict cash earnings from wage labor. To predict retail earnings, I bring back women whom I had dropped in the analysis of wage labor, and I lift the age cap to include any person 16 years of age or older.

As in the analysis of wage labor, in the analysis of retail I first predict who does not sell (Table 7.23) and -- having become a retailer -- I then predict how much they earned (Table 7.24). Women partook in retail, but men partook more (Table 7.23). Depending on how one assesses chances, men were 4.5 or 9.5 percentage-points more likely to sell than women of the same age, body type, and years of schooling, living in households with the same number of grown-ups and children (Table 7.23, columns 2-3). A decade of aging lowered the chances of not selling by 2-3 percentage points, implying that the old relied on retail more than the young. Unsurprisingly, year of schooling raised the chances of avoiding retail. An additional year of schooling increased by 1.8 percentage points the chances of shunning retail. I say unsurprisingly because schooling foretold the chances of working for wages and since a person cannot be in two places at the same time, what predicts doing one most likely predicts not doing the other. As the

number of adult men in the household increased, so did the chances that an adult in a household would sidestep retail; the presence of an adult man increased by 3.3-4.9 percentage points the chances of not selling. The number of children or women had not visible impact on the likelihood of selling. A survey done ten years later increased by 5-6 percentage points the chances that an adult would sell, and this effect happened aside from a person getting older. Other than musculature, anthropometrics did not matter, probably because human biology has indubitable bearing on the chances of doing strenuous manual work, but it has doubtful bearing on the chances of doing a sedentary job like retail. People along the river Maniqui who took part in the longitudinal study (TAPS) were 6.5-7.4 more likely to avoid retail than Tsimane' elsewhere (columns 1-2). I take away four messages from the dispositive evidence of Table 7.23: getting older and later surveys raised the chances of retailing while the amount of schooling and the number of adult men in a household lowered it.

Insert Table 7.23

Once in retail, a person's sex had a strong effect on their earnings (Table 7.24). Men earned 56.2% to 70.7% more than women. Among the demographic variables of a household, boys and women helped with earnings, but not men or girls. An additional boy in a household raised retail earnings by 3.6%-4.8% while an additional woman raised it by 6.4%-13.4%. A Tsimane' household has 1.2 adult women and 1.8 boys (Chapter 5; Table 5.4), so adding one more of each would be a conceivable change in the demographic makeup of a household. Two other findings leap out. A survey done a year later increased not only the chances of selling (Table 7.23), but also real earnings. A person surveyed a year later earned 6.5% to 8.6% more cash, even after controlling for aging and for unchanging traits of the year, as in columns 3-5. The study site mattered for retail earnings. Depending on how one tallies the statistics, people in villages along the river Maniqui earned 41% less than other Tsimane' (Table 7.24, columns 1-2).

Insert Table 7.24

An obvious question is whether earnings from sales and earnings from wage labor replace or complement each other. Does an increase in earnings from one bespeak a decrease in earnings from the other, or do they trend together? The question is important because it lets us assess if Tsimane' face trade-offs when engaging with the market, or if they have more leeway and can do both at the same time. To explore the topic, I first show graphs of the two variables – earnings from wage labor and earnings from sales for the seven days before the interview – and then I show statistical analysis that conditions for factors that could muddle what we see in the graphs.

Figures 7.15a-7.15c display the relation between earnings from sale and from wage labor. The percent change in sale earnings from a one-percent change in wage earnings was positive but meager, ranging from 0.25 in the yearly longitudinal study of people (2002-2010; TAPS) to 0.21 for people who took part in the baseline of the randomized-controlled trial (2008), to 0.20 for the two studies combined[xxxvi]. The three figures show that more cash income from wage labor went with more cash income from sales. In Table 7.25 I re-do the analysis but control for predictors of retail earnings. Now we can see that monetary earnings from the two livelihoods go in opposite directions. A one-percent increase in earnings from wage labor lowered sale earnings by 0.03% to 0.06% (columns 2-3). Though negative and more trustworthy than the

graphs, the amounts are small, very small. The numbers imply that working much harder to increase wage earnings by100% would depress retail earnings by no more than 6.2% (column 3). A large stimulus for a small response. I conclude that the two ways of earning cash go side by side, without harming or helping each other.

Insert Figure 7.15a-7.15c and Table 7.25

Summary of trends in earnings from sales. In Chapter 6 we saw that Tsimane' grew few crops, so it follows that they would sell few crops. Rice, bananas-plantains, and forest wildlife topped the list of articles sold, both in frequency and in worth, but half the sample never sold anything. The seasons did not change the amount of retail earnings but did change what Tsimane' sold. The rainy season saw an upsurge in the sale of forest wildlife while the dry season saw an upsurge in the sale of a handful of farm crops. Leaving aside how to standardize the value of the local currency over time and countries, my rough reckoning suggests that sellers made between USA$0.80 and USA$2.14 each day, in the boundary of the international poverty line, and further down of what a man could make from wage labor in a day, about USA$4.75-USA$5.87. Depending on how we aggregate information, some trends in retail were clear, some fuzzy. The share of adults who sold dwindled every year, as did the number of distinct goods sold. This much was unmistakable. However, trends in real earnings from retail were murkier. Graphs of aggregates show total real earnings from retail falling, then rising, as in Figure 7.10a, but the statistical analysis of Table 7.24 shows that a survey done a year later brought about a 6.5% to 8.6% increase in real earnings and an increase in the chance of selling. More inclusive than wage labor, which was confined to men in their prime working age, retail embraced females and males of all ages. The chance of retailing rose with age and later surveys and declined with the level of schooling and the number of adult men in the household. Democratic as it was, retail nevertheless allowed men to earn more than women. Earning cash from retail and from wage labor travelled side by side, neither eroding or complementing each other.

Earnings and Bolivia's foreign currency exchange rate: How autarkic are Tsimane'?

Before finishing the chapter, I turn to a neglected but promising topic in studies of human foragers: the effect of a country's currency exchange rate on foragers' earnings. We anthropologists like to portray foragers as autarkic and spend much of our time praising or wailing the effects of markets on their lifestyle. The belief leads one to the measurement of engagement with the market. Straightforward ways include measuring the share of wares and services bought in the market, cash debts, or the amount of cash earned from sale, work, or rental, as in this chapter. Oblique, less fitting ways include indicators of Westernization, like years of schooling or fluency in the national language. I say oblique because Westernization and markets need not overlay tightly and well. Yet another, unequivocal way is to examine if economic choices of foragers change with changes in the foreign currency exchange rate of a country. *A priori*, one would think that changes in a country's foreign currency exchange rate – in our case, the *bolivianos* needed to buy one USA dollar -- would not affect choices of people in ultramontane economies, but if foragers engage with the market, however gingerly their engagement, then changes in the foreign currency exchange rate could affect their economic choices. Economic choices could include cash earnings, cash expenditures, and the compost of

industrial and traditional assets to store wealth or save. All these could chance with changes in the country's foreign currency exchange rate. Almost unattainable with cross-sectional data, the task becomes more doable with longitudinal data[xxxvii].

Besides providing another way of measuring engagement with the market, the foreign currency exchange rate might have other desirable features. The relative price between the domestic and the foreign currency is random to the person, something over which they have no say, giving us a cleaner way of assessing how the market economy could affect foragers' economic choices. Like almost any price anywhere, the relative price of the two currencies lies beyond the control of the individual. If the foreign currency exchange rate predicts cash earnings, cash expenditure, or asset mix, then it might serve as a handle to improve estimates of how signatures of the market like cash or commercial assets, in fact, determine lofty aspects of well-being untouched by the exchange rate[xxxviii]. The purpose here is not to extend the chapter into these uncharted places, but to take a first step in exploring how the local foreign currency exchange rate might affect cash earnings from wage labor and from sales.

Among Tsimane', changes in Bolivia's foreign currency exchange rate should have stronger effects on retail earnings than on wage earnings because Tsimane' produce rice, bananas-plantains, maize, thatch palm, rare hardwoods, all commodities that could -- and in fact, do -- enter international markets. The harvest of these farm and forest crops should be vulnerable to changes in the foreign currency exchange rate. As the number of *bolivianos* to buy a USA dollar rises -- as the local currency losses value or depreciates relative to the USA dollar -- earnings from sales should rise as well. With a rise in the foreign currency exchange, Tsimane' receive more *bolivianos* for every USA dollar worth of tradeable goods they sell; some of these good eventually leave Bolivia. Earnings from wage labor are clunkier for payments are pinioned to personal contracts so even with a rise in the foreign currency exchange rate, earnings from wage labor would not likely change as fast or as much as earnings from the sale of tradeable goods, goods which, in theory and in practice, cross country borders.

Having collected during the yearly surveys the unofficial, local foreign currency exchange rate in the two towns of the study area, San Borja and Yucumo, we can try to estimate the effects of the foreign currency exchange rate, adjusted for inflation, on monetary earnings from retail and from wage labor (Appendix C)[xxxix]. Because information about the foreign currency exchange rate was sometimes collected after the surveys of villages, households, and people, I push back or lag information on the exchange rate by one year. Through this step we can be sure that the measure of the exchange rate happened before the measure of cash earnings. But we now face a new problem: very few measures of the exchange rate. We started with nine yearly measures of the exchange rate (2002-2010, inclusive) but had to take away one measure because I lagged the exchange rate by one year. This leaves us eight measures of the exchange rate for the analysis. Given so few observations, we cannot rein in the influence of other features of a year besides the exchange rate that could affect cash earnings. Weather, interest rates, political unrest, and so much else happening in Bolivia in one year could affect cash earnings and the exchange rate. Our admittedly crude estimate of the correlation between the foreign currency exchange rate and cash earnings picks up not just the effect of the exchange rate, but much else happening in a year[xl].

Table 7.26 shows the results. Columns 1-3 show that a one-percent depreciation of the local foreign currency real exchange rate a year ago increased retail earnings a year later by 1.6% to 1.9%, a strong finding in line with what one would expect from retailing tradeable wares. However, the same change in the local exchange rate had the opposite effect on wage earnings; a

one-percent depreciation of the local foreign currency real exchange rate during the previous year lowered today's wage earnings by 0.5%, an amount indistinguishable from zero (columns 4-6). Thus, we see some indicative evidence that, despite their autarky, some aspect of the Tsimane' economy seem to respond to changes in Bolivia's foreign exchange rate, while other aspects of engagement with the market – namely wage labor – do not.

Insert Table 7.26

Discussion and conclusion

Methods. As in earlier chapters, in this one we see once more shortcomings in procedures and measurements. Among flaws in measurements, we have jarring values, rounding errors, and telescoping bias. *Procedures*. Our biggest flaw is not having asked adults without a job if they were looking for one. The failure hinders us from tallying and distinguishing the share of adults who were unemployed but searching for a job from pococurante searchers, those who had lost hope of finding a job or who did not need one. We jumped to surveys without ethnography. We should have done ethnographic research among buyers, sellers, employers, and workers to understand why they decided to buy, hire, or sell and their correlative thinking of what price to offer or accept. Even when asking about paid work, we did not press workers to tell us if they got paid in cash, in kind, or both. Total payment is all we have. *Disconcerting values*. As usual, we find disconcerting values that we should have caught and fixed in the field but did not. Could people have worked almost every day during the previous fortnight in 2008 when in other years they only worked for seven days (Table 7.6)? How could the share of transactions from rice sales double in only two years, from 11% in 2008 to 20% in 2009-2010 (Table 7.17)? These sorts of oddities in small samples like ours upset trends and summary statistics. *Digit heaping*. Wired to round numbers when reporting figures, Tsimane' liked to say they had earned amounts that ended in multiples of five. We saw the leaning many times before, in demography, in farming, and now we see it again in earnings from wage labor (Table 7.6) and sales (Table 7.19). *Telescoping*. In the number of articles sold and in the value of earnings from retail (Table 7.18) the near past had more weight than the distant past. Retailers believed they sold more goods and made more money last week than two weeks ago. We found no telescoping bias in the number of days worked or in earnings from wage labor (Table 7.9). The presence of telescoping bias when reporting one type of cash earnings but not the other poses a puzzle I cannot answer well. Except when done for a villager, wage labor requires that workers commute, change residence, or speak in a foreign language. When combined with the drudgery of work, the work experience leaves an imprint on a worker's mind, making it less likely that a worker would shift around the amount of earnings or the number of days worked that took place in two back-to-back weeks. In contrast, retailing different amounts of motley goods is less likely to leave a deep chasm in the mind. Owing to memory lapses, people are more likely to make mistakes when remembering when, what, and how much they sold. But this explanation is wanting. Cognitive salience explains why we do not find telescoping bias with wage labor, but it does not explain why, when there is telescoping bias, respondents shifted events to the recent past.

The motivation to earn cash. It puzzles one why people in an enclosed economy would need cash. One believes that nearly autarkic people need cash to buy things they can no longer make, or that they can make but not as well as factories or outsiders. Among Tsimane', these

goods would include cigarette lighters for cooking, machine-made apparel, metal tools, salt -- all goods for which they could make substitutes with local materials or, as with salt, which they could find in the wild. It is a straightforward task to assess how people spend cash and, from expenditures, infer the hierarchy of needs for different types of commercial goods. The problem with this approach is the uncomfortable finding from the randomized-controlled trial mentioned earlier. Recall from the start of this chapter that in the trial we randomly assigned saving boxes to the female or to the male head of a household. Box recipients had more cash holdings by the end of the trial, but they had spent it in commercial alcohol, not in meritorious goods. Many things come in between cash at hand and how one spends it. For this reason, I say we do not know well why they need cash. Is it to buy alcohol, to show off status, to make up for what they can no longer fashion, or to improve the basics of household well-being? All we have shown in this chapter is what Tsimane' do to earn cash, not why they need it.

Levels of cash earnings. Since the publication of the essay "Notes on the original affluent society" by cultural anthropologist Marshall Sahlins (1968), anthropologists have been besotted with the well-being of human foragers. Arguments have centered on the plentiful leisure, fair life expectancy, and healthy diet enjoyed by people near autarky. To the list we might add cash earnings.

My back-of-the-envelope calculations show that from sales -- chiefly of rice, bananas-plantains, and forest wildlife -- a Tsimane' woman or man made, on average, between USA$0.80 to USA$2.14 each day and, from paid work, men made each day between USA$4.75 and USA$5.87. These estimates would put the average seller at the border of the international poverty line and the average male worker above it. Of course, these averages are too high because they leave out those who did not work (about 76-90% of the sample) or did not retail (about half the sample). If we bring back the excluded into the calculations, mean daily earnings from sales and from wage labor drop. Using clean data from the longitudinal study of individuals I ciphered out that the mean daily nominal wage earnings for any adult, irrespective of whether they had worked for wages, reached USA$4.67 while the mean daily nominal earnings from sales reached USA$0.57, both lower than the previous estimates.

These estimates allow one to begin assessing monetary poverty among Tsimane'. A household with one male wage laborer, one female seller, and two dependents would have a total cash income of about USA$5, which, split into four people, would yield a daily cash income of USA$1.25 per person, in the international threshold of extreme poverty (about USA$1 per person). The estimate of cash income per person captures a lower bound in the income continuum due to the well-rehearsed reasons that the estimate excludes the value of transfers from other people or the government into the household, and the value of farm crops consumed from their plots, domesticated animals consumed from their pens, and wildlife consumed from rivers and forests.

Irrespective of how one judges monetary poverty among Tsimane', one thing that becomes clear is the unpretentious diversification practiced by Tsimane' when surveyed. Most men worked for one employer and most grownups sold one good during the fortnight before the interview. Locked into a few employers and crops, people did not switch employers or changed the range of goods they sold between seasons. Between one year and the next, they changed what they sold or who they worked for, but in any year, they sold one good and worked for one employer.

On the surface, retail disrupts life less and embraces more people than wage labor. Anyone can sell, but only some can work in a demanding billet. Children and the aged can both

sell, but they cannot work easily in strenuous jobs. Retail allows people to build on what they have, to sell the surplus of what they harvest, or to dispose of what they want not. Paid work for others, especially for strangers, requires some fluency in spoken Spanish, commuting, physical strength, and a hard edge or insouciance against the snobbery of (white) employers. If I am right in this reading, then two things follow. First, the gap in daily earnings for individuals between wage labor (USA$4.67) and sales (USA$0.57) could be partially seen as a reward for having to put up with the annoyances of wage labor. One could read the wedge in other ways, of course, but my reading seems sensible and supple enough to accommodate other interpretations. Second, retail might be the Trojan horse through which markets begin to upset autarkic economies. Because it is easier to buy, sell, and exchange than to work for wages, retail probably precedes wage labor when engaging with the market economy. Retail changes tastes and fecundates new wants, quenched by more retail and, later, by wage labor. Among Tsimane', so far, the two ways of engaging with the market seem to be moving side by side.

Trends and predictors.

Wage earnings. During the 11 years of observation (2000-2010) the labor market changed. Hiring by cattle ranchers, Tsimane', and the government grew while hiring by logging enterprises, schools, and town traders weakened. The share of people without wage earnings rose each year by 2.07% or 3.56% (Table 7.7) while the chances of having no wage earnings among adult men rose by 2.2 to 2.6 percentage points each year (Table 7.14). Together, these statistics show the shrinking importance of wage labor to earn cash. Men between 16 and 65 years of age, heavier and more schooled than other men joined the labor force to work, but only to work part-time, or the equivalent of every other day in a fortnight. Each year laborers increased by 5.47% or 8.39% the number of days worked (Table 7.7), growth rates that hint at a slow shift to full-time work.

Adjusted for inflation, total wage earnings and daily wages rose each year, earnings by 8% and daily wages by 3.9% (Table 7.15). The increase in median real earnings came from a rise in the number of days worked more than from a rise in real daily wages (Table 7.7). We could be witnessing the sorting of male workers into tracks: fewer and fewer men become wage laborers to work and earn more, while the rest do something else or earn cash by retailing. It's too early to tell if sorting will shatter Tsimane' society into occupational silos, or if sorting is something all households do, to some degree, to shield themselves against unwanted happenings; each household has some workers and some sellers just in case things sour with sales or with jobs.

The features that predicted joining the labor force also predicted the amount that workers earned. Heavier men with more schooling surveyed in later years enjoyed higher real earnings than less lettered, thinner men canvassed early in the study. Villages tempered the effects of a person's schooling on wage labor. For the same schooling level, a man surrounded by more schooled villagers was more likely to join the labor force and earn more from wage labor than his doublet surrounded by less schooled neighbors in another village.

Sales. If Tsimane' are stepping out of wage labor, they might have taken up retail to make up for the loss of wage earnings, but this does not seem to be happening. Each year from 2000 until 2010, the share of the sample that shunned sales rose by 3.33% to 5.32% while the number of goods sold declined by 2.38% to 2.66% (Table 7.20). The share of hard retailers selling during both weeks before the interview also dropped, as did the array of articles they sold. Mean and median real earnings from retail declined by 0.70% to 6.48%/year (Tables 7.20-7.21). Except for the sale of manioc and bananas-plantains, the sale of most other goods declined each year (Table 7.20). Some of these conclusions are shaky for they depend upon the sample or the

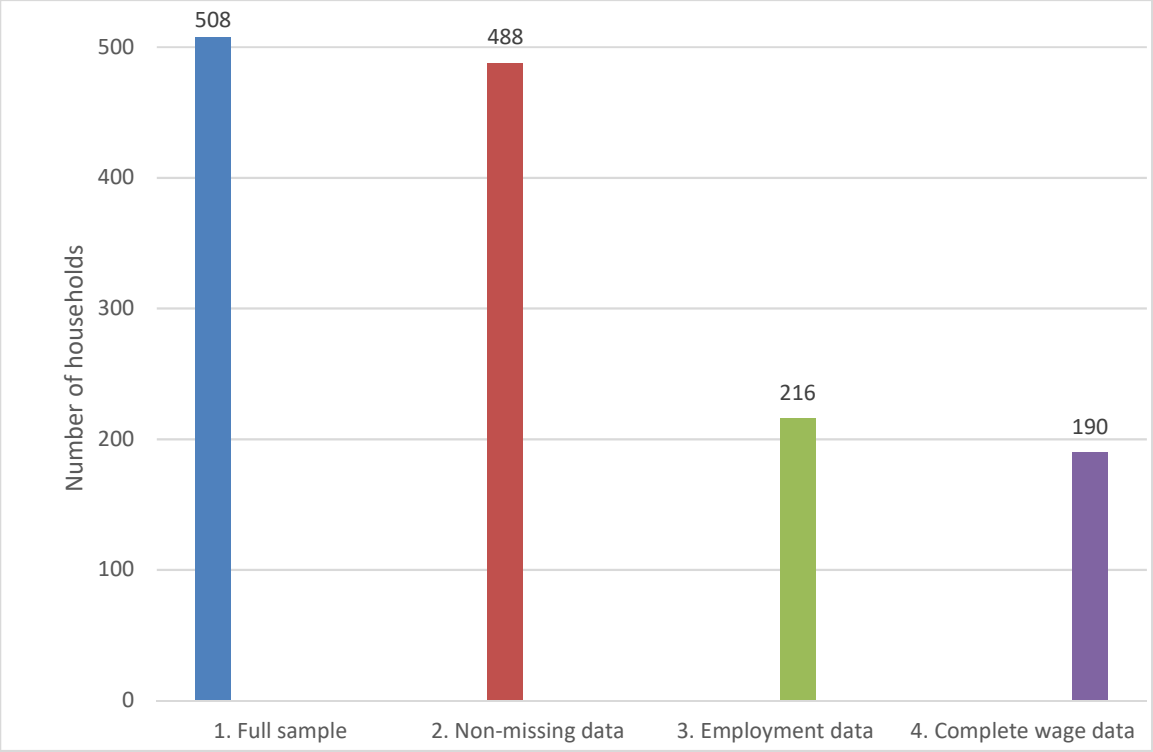
ciphering used. For example, the estimates of Table 7.23 show that each year the chances of selling rose by 0.6 percentage points, a small, barely significant result meaning that calendrical time had no real-world effect (Table 7.23) but Table 7.24 shows that real earnings rose each year by a hefty 8% (Table 7.24). The simpler statistics of Table 7.20 show a general yearly decline in average and in median real earnings, except when using information from the longitudinal study of 2002-2010; in that sample, median real earnings rose by 3.25% each year.

The chances of selling rose with aging and later surveys, and declined with years of schooling, perhaps because schooling improved the chances of working for wages, at least among men. The demographic makeup of a household affected retail. One more grown-up man in the household raised the chances of avoiding sales by 3.3-4.9 percentage points (Table 7.23). One more grown-up woman or boy in the household had the opposite effect on retail earnings; one more adult woman raised retail real earnings by 6.4% to 13.4% while one more boy raised it by 3.6% to 4.8% (Table 7.24). Democratic and all-embracing as it might be, retail nevertheless allowed men to earn 56.2% to 70.7% more than women (Table 7.24).

Wages and sales. We noted that Tsimane' engaged with the market in two sequestered ways: wage labor and retail. In fact, however, Table 7.25 shows that as wage earnings rose, retail earnings declined, but slightly. Raising wage earnings by 100%, an unfrivolous amount, by, for example, doubling the number of days worked and turning part-time workers into full-time workers, would reduce retail earnings by no more than 3.1% to 6.2%. Two reasons come to mind for the small response. First, most Tsimane' worked part time, so increasing the number of days worked would still leave them with days off for retail. Even if people worked Monday through Friday or Monday through Saturday, they would still have free time to sell on the weekend or on Sunday. Second, even full-time workers who were away from the village during the past fortnight could say they had earned money from retail if someone else in the household sold on their behalf. The chances of simultaneously earning cash from retail and from wage labor would be greater if I had done the analysis at the level of the household rather than at the level of the individual. A household-level analysis would allow one to see more clearly if households relied on different people in the household to sell and work at the same time.

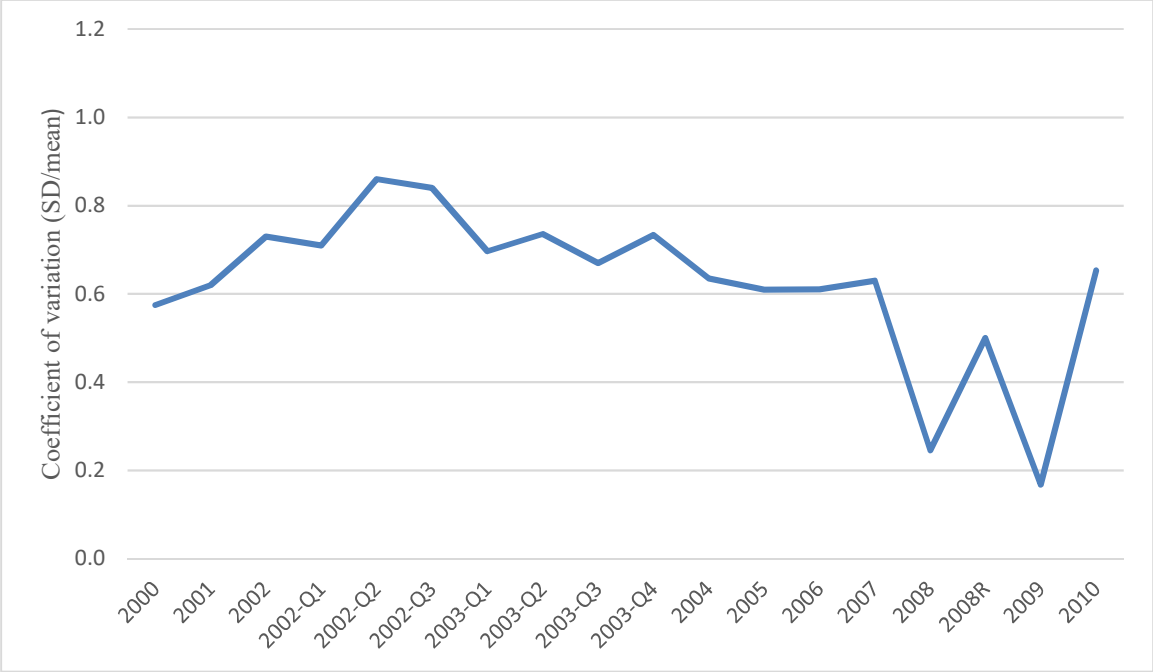
Foreign currency exchange rate. Anthropologists could try using a county's foreign currency exchange rate as another way of gauging foragers' autarky. The approach will not work among foragers devoid of dealings with outsiders, or among foragers producing non-tradable goods, but could work with foragers harvesting tradable goods and with spatial and temporal variation in the local foreign currency exchange rate. Having so few measures of the local exchange rate (n=8) from two towns, we cannot weigh in on the hypothesis with much confidence. Nevertheless, we found some evidence matching expectations that a currency depreciation the past year changed retail earnings next year.

Fig. 7.1. Sample sizes by outcomes in module on wage labor: Example using the household-level survey of 2000



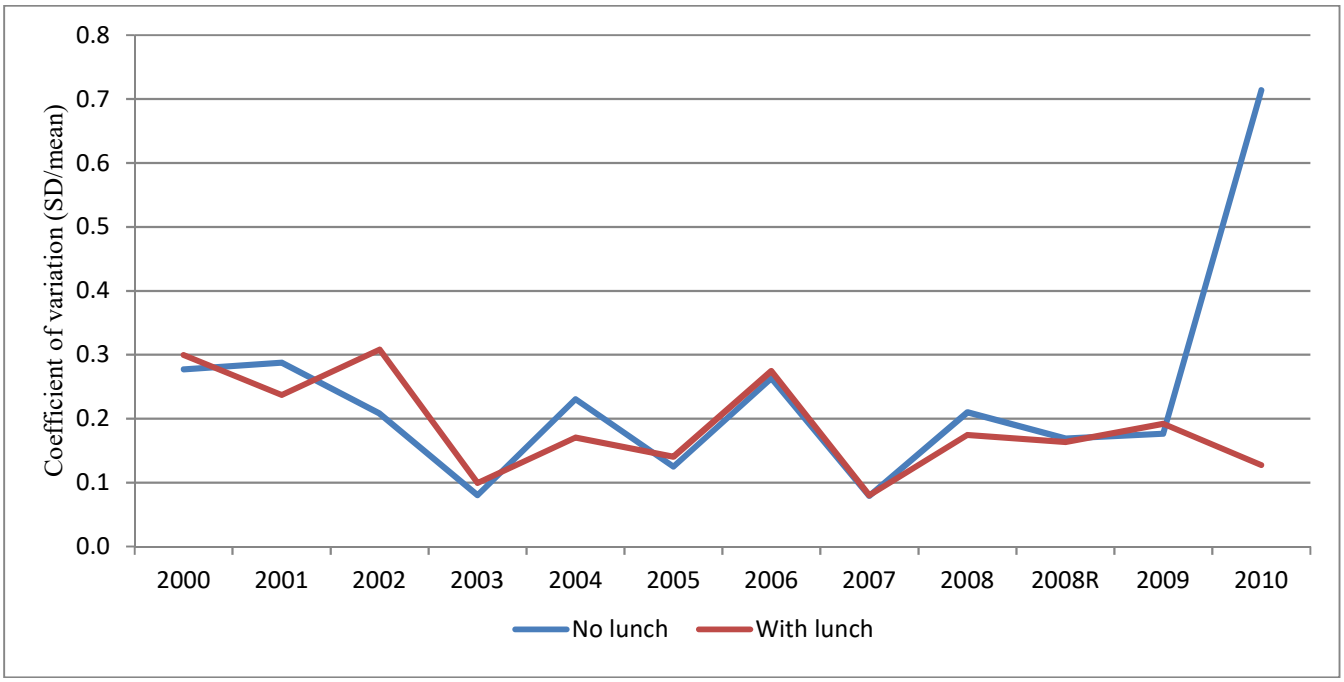
Notes: 1. Full sample of all households in module on wage labor; these households could have missing data for some topics (e.g., days worked). 2. Sample of households with no missing information in module on wage labor for each of the two weeks before the day of the interview; these households might not have worked and have values of zero for all the outcomes in the module on wage labor (e.g., employer or job, days worked, and wage earnings) but they had no missing data. 3. These are households that had indicated the type of employment they had but could have had missing data for some of the other outcomes in the module on wage labor. 4. This is the sample of households that had taken part in wage labor and had complete information on all the outcomes in the module on wage labor, for both the week and for the two weeks before the interview. I compare the sample in column 4 with the sample in column 2 to assess the share of the population that engaged in wage labor and I use the sample from column [4] to estimate days worked and earnings.

Fig. 7.2. Coefficient of variation (standard deviation [SD]/mean) of days worked during the 14 days before the interview, 2000-2010



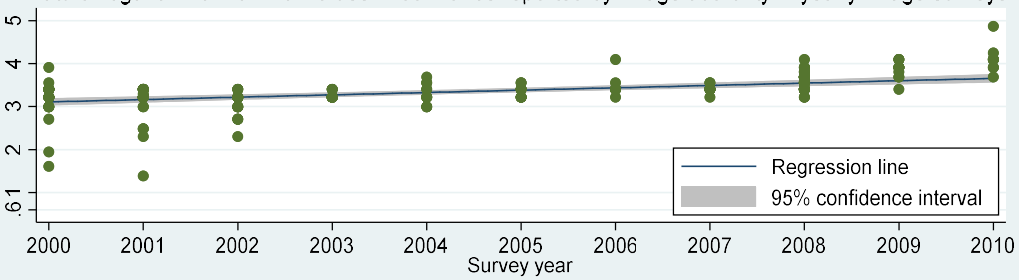
Notes: Information about standard deviations and means come from Table 7.6. For the baseline of the randomized-controlled trial, mean and standard deviation cover values for the seven days before the interview. 2008R=baseline of randomized-controlled trial. In years 2002-2003, the suffix Q stands for the quarter in which the survey took place.

Fig. 7.3. Coefficient of variation (standard deviation [SD]/mean) of the village nominal or real daily wages, with and without lunch given by employer, 2000-2010



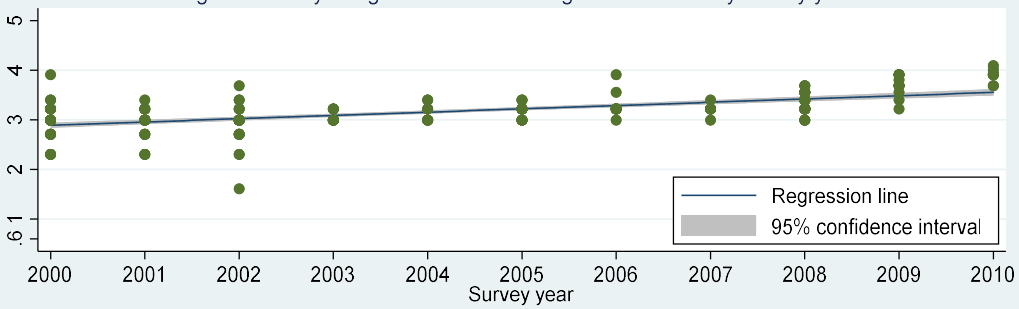
Notes: Information comes from survey of village leader asked to report the current daily cash wage, with and without lunch. 2008R=baseline of randomized-controlled trial. The coefficient of variation of nominal values is tantamount to the coefficient of variation in real values since all yearly values in the sample are transformed by the same constant value of the consumer price index.

Fig. 7.4a. Daily village nominal cash wage WITHOUT lunch by survey year:
Natural logarithm of nominal values in bolivianos reported by village authority in yearly village surveys



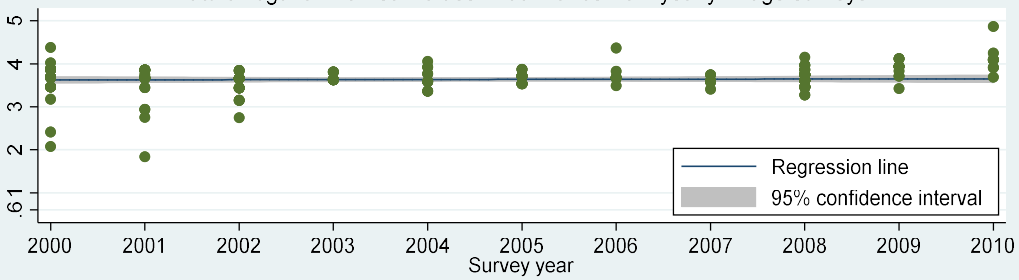
Slope from OLS regression with robust standard errors and village clustering=0.05, p=0.001. N=239

Fig. 7.4b. Daily village nominal cash wage WITH lunch by survey year:



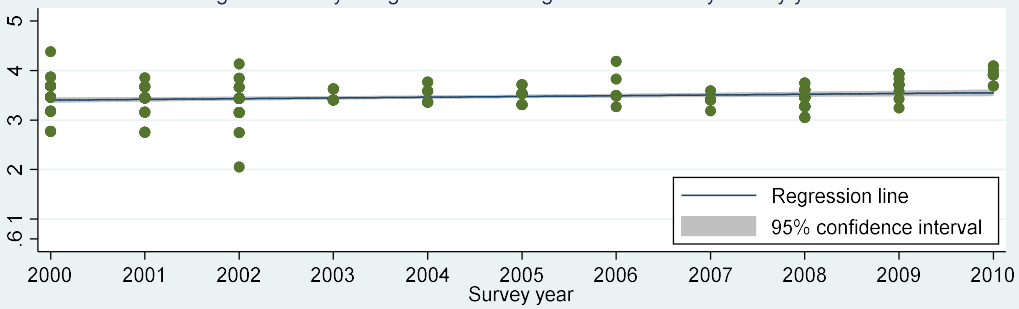
Slope from OLS regression with robust standard errors and village clustering=0.06, p=0.001. N=249

Fig. 7.4c. Daily village real cash wage WITHOUT lunch by survey year:
Natural logarithm of real values in bolivianos from yearly village surveys



Slope from OLS regression with robust standard errors and village clustering=0.002, p=0.81. N=239

Fig. 7.4d. Daily village real cash wage WITH lunch by survey year:

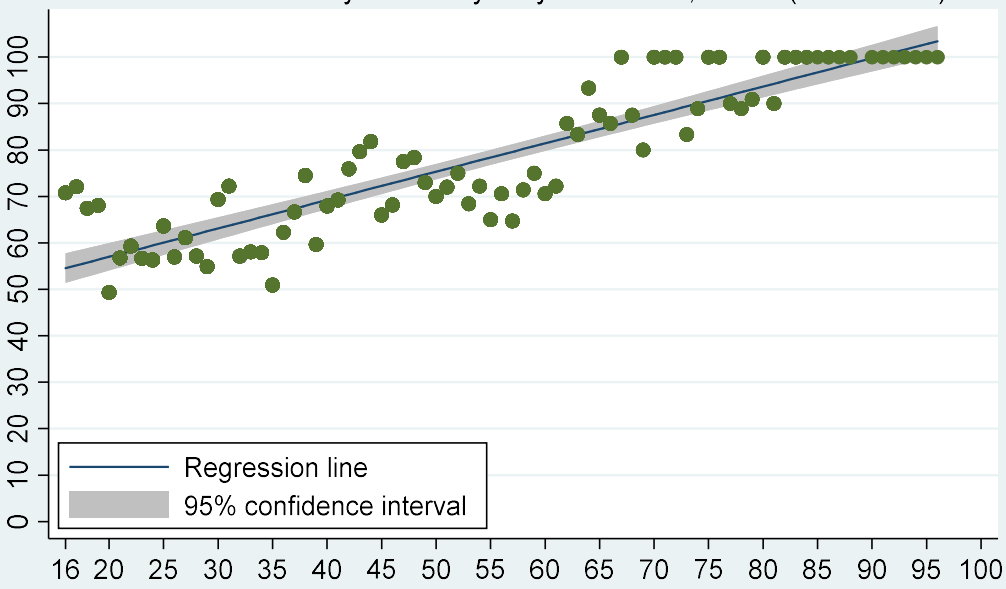


Slope from OLS regression with robust standard errors and village clustering=0.01, p=0.007. N=249

Fig. 4a-4d – continued

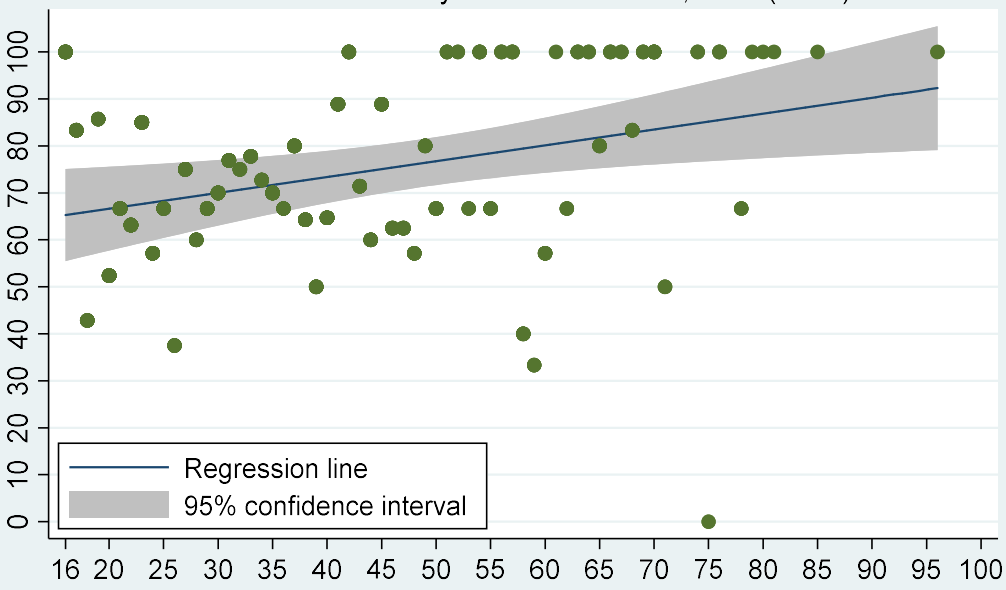
Notes: Bolivia's general consumer price index (CPI) used to change nominal values from Fig. 7.4.a-7.4b to 7.4c-7.4d. See text and endnotes for source of CPI. The unit of measure and analysis is the village wage observed during each survey year.

Fig. 7.5a. Share of men 16y≤age≤96y without wage earnings, by age:
Data refers to 7 days before yearly interviews, TAPS (2002-2010)



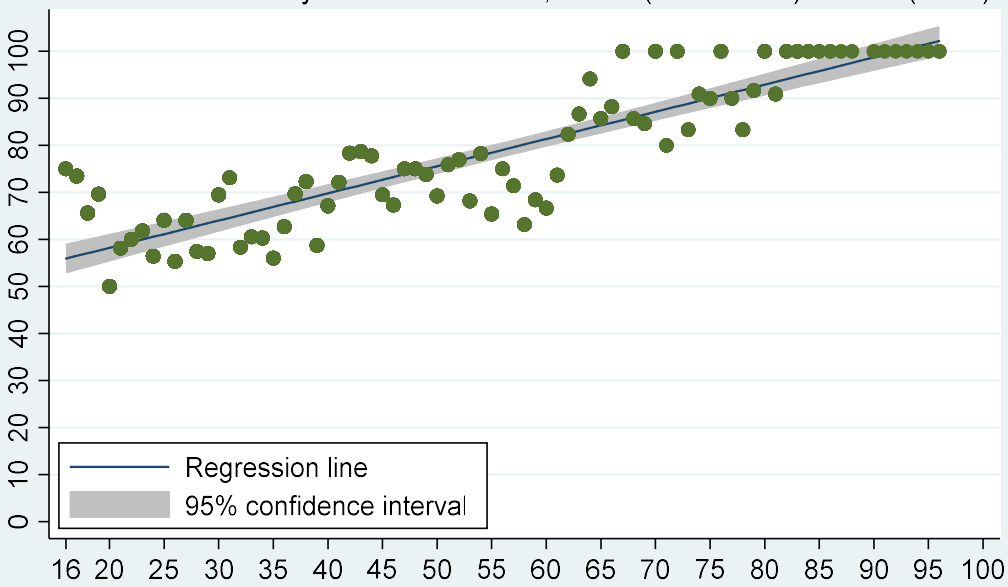
Slope from OLS regression with robust standard errors=0.61, p=0.001, N=80

Fig. 7.5b. Share of men 16y≤age≤96y without wage earnings, by age:
Data refers to 7 days before interview, RCT (2008)



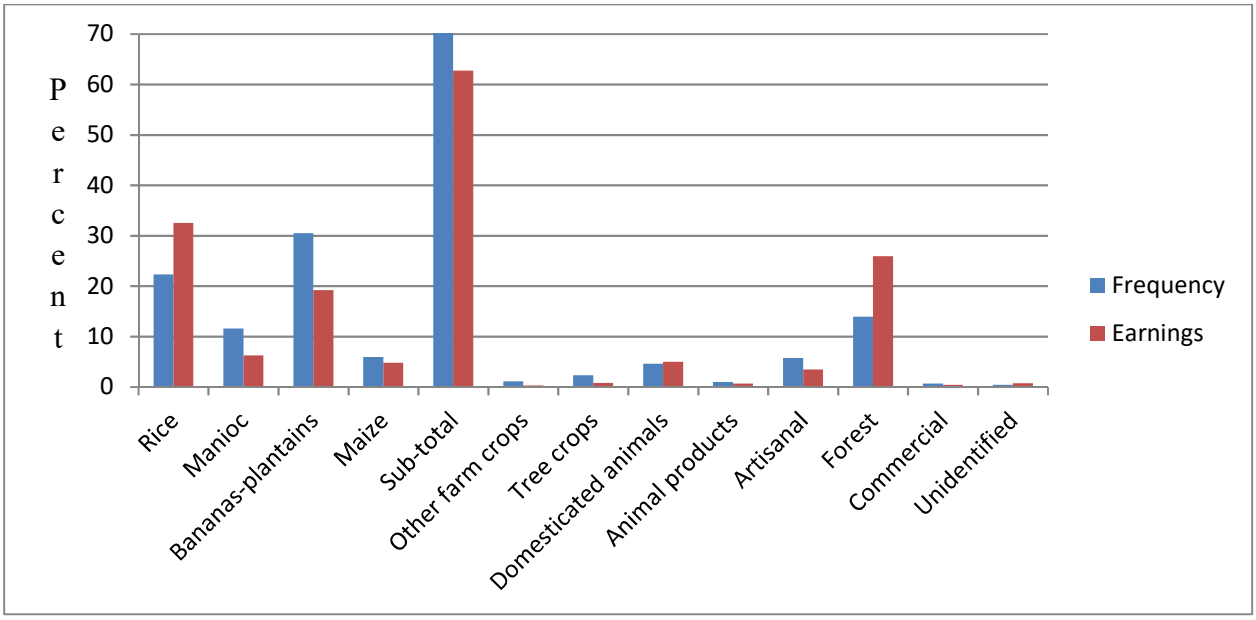
Slope from OLS regression with robust standard errors=0.33, p=01, N=65

Fig. 7.5c. Share of men 16y≤age≤96y without wage earnings, by age:
Data refers to 7 days before interview, TAPS (2002-2010) & RCT (2008)



Slope from OLS regression with robust standard errors=0.57, p=0.001, N=80

Fig. 7.6. Percent of goods sold by frequency of sales and by the value of earnings: Grand mean for 2000-2010



Notes: Graph comes from the yearly grand mean statistics of the last column of sections A-B, Table 7.17. "Sub-total" = sum of the share of rice, manioc, bananas-plantains, and maize.

Fig. 7.7a. Yearly share of *frequency* of sales by type of goods sold: 2000-2010

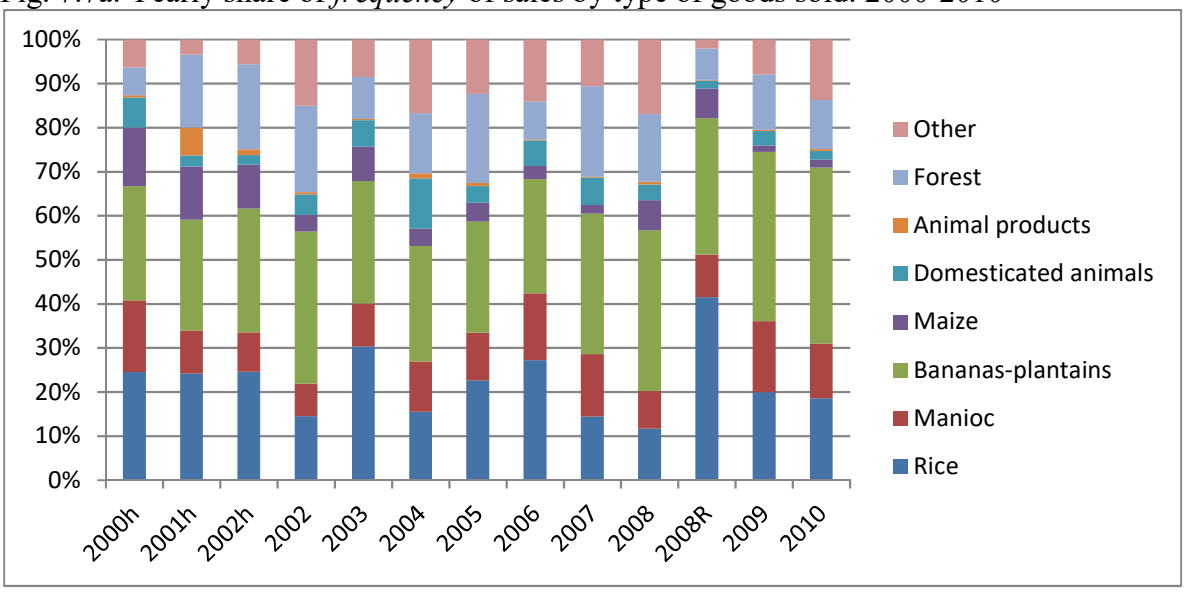
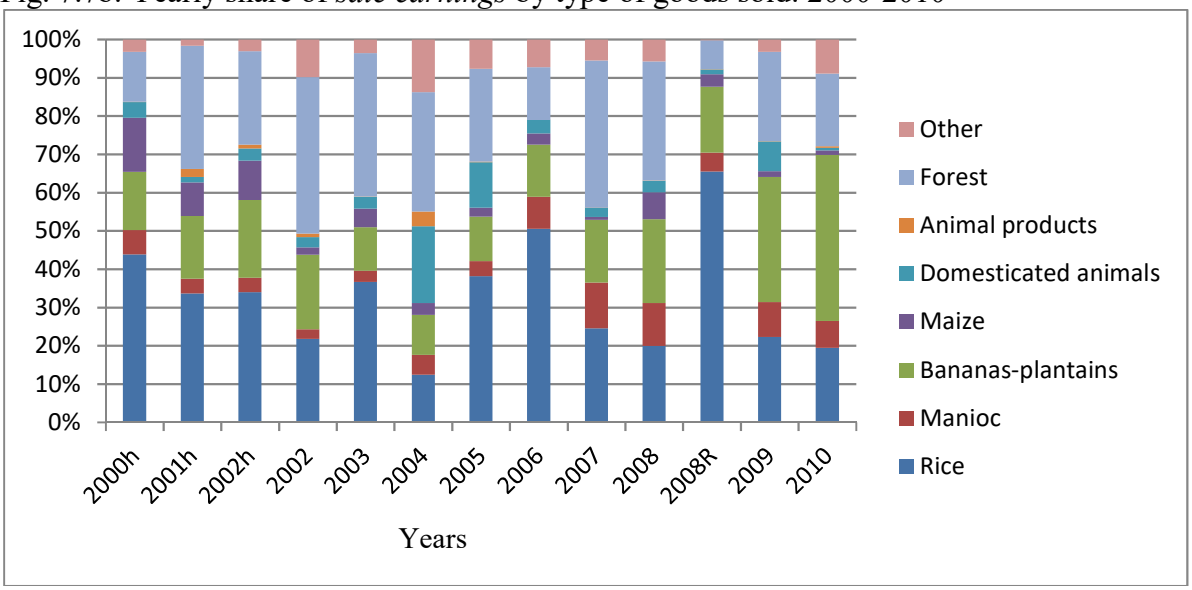
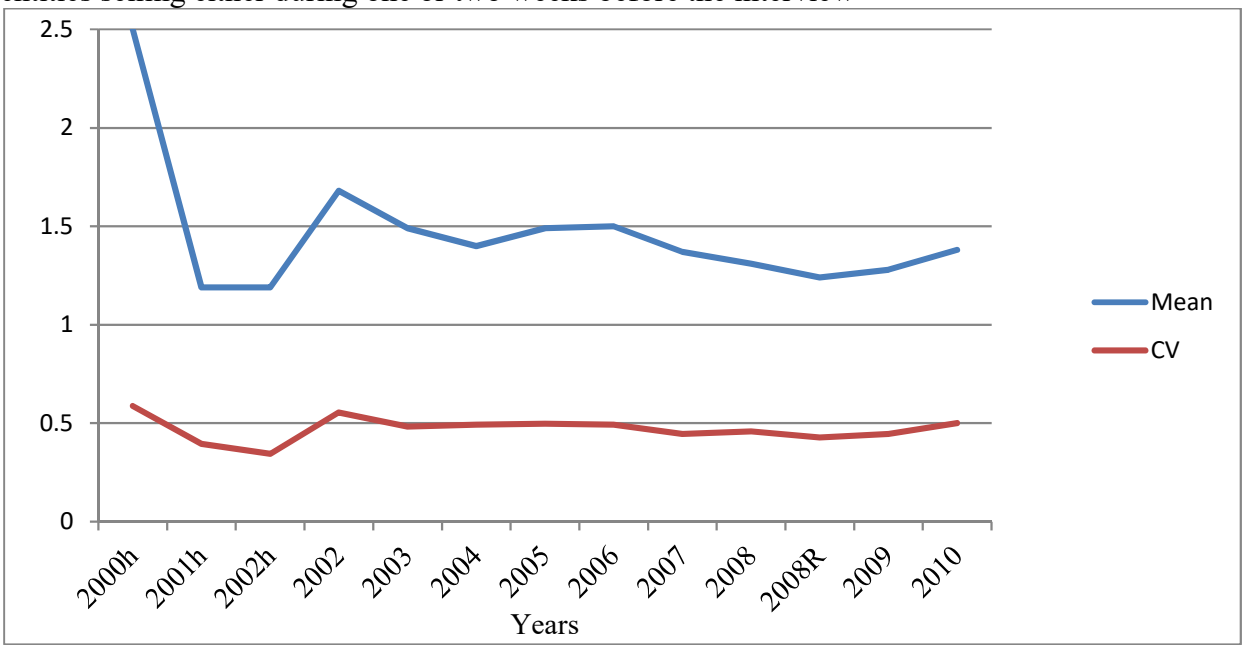


Fig. 7.7b. Yearly share of *sale earnings* by type of goods sold: 2000-2010



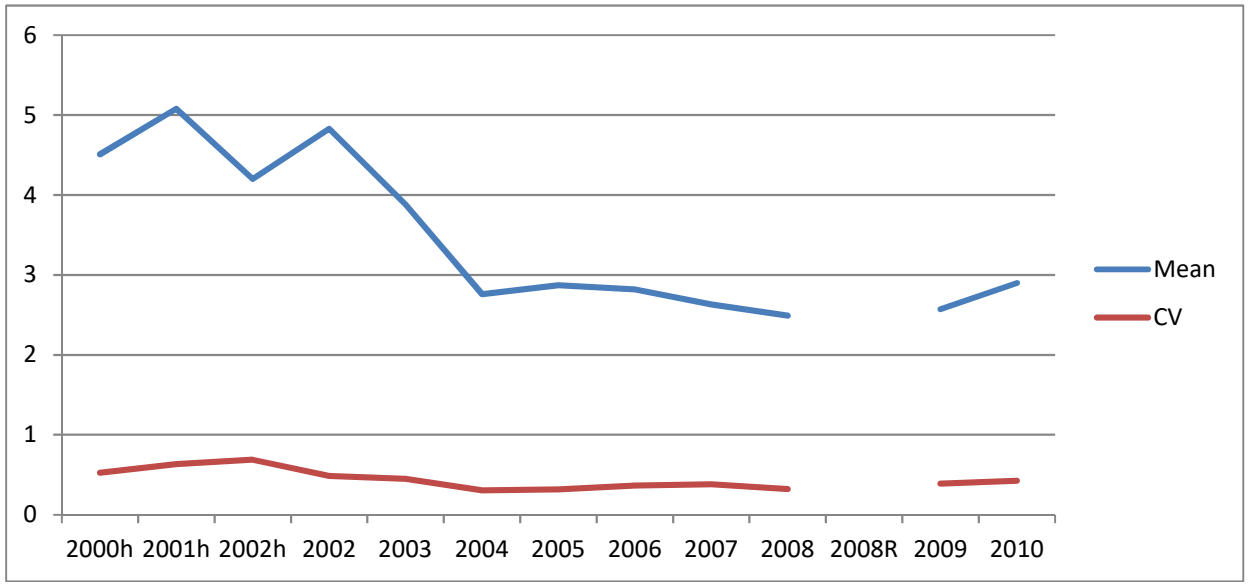
Note: The "Other" category includes tree crops, other farm crops, artisanal artifacts, commercial articles, and unidentified items. The suffix 'h' stands for households and the suffix R stands for the baseline (2008) of the randomized-controlled trial. Source for Figure 7.7a is Table 7.17 (section A) and source for Figure 7.7b is Table 7.17 (section B).

Fig. 7.8. Mean and coefficient of variation (CV; standard deviation [SD]/mean) of *distinct* identified and unidentified items sold by entities selling at least one item, 2000-2010 for all entities selling either during one or two weeks before the interview



Note: Data comes from section C, Table 7.17. The suffix 'h' stands for households and the suffix R stands for the baseline (2008) of the randomized-controlled trial. CV=coefficient of variation (standard deviation/mean).

Fig. 7.9. Mean and coefficient of variation (CV; standard deviation [SD]/mean) of *all* identified and unidentified sold by entity selling at least one item in *both* weeks before the interview, 2000-2010



Notes: Data on mean and standard deviation to compute the coefficient of variation comes from Table 7.18, section C. The suffix 'h' stands for households and the suffix R stands for the baseline (2008) of the randomized-controlled trial. Data for the baseline survey of the randomized controlled trial (2008R) is missing because in that survey we only asked subjects to recall sales over the seven days before the interview.

Fig. 7.10a. Yearly mean and median inflation-adjusted (real) value in *bolivianos* of *total* earnings from sales during the 14 days before the interview, 2000-2010

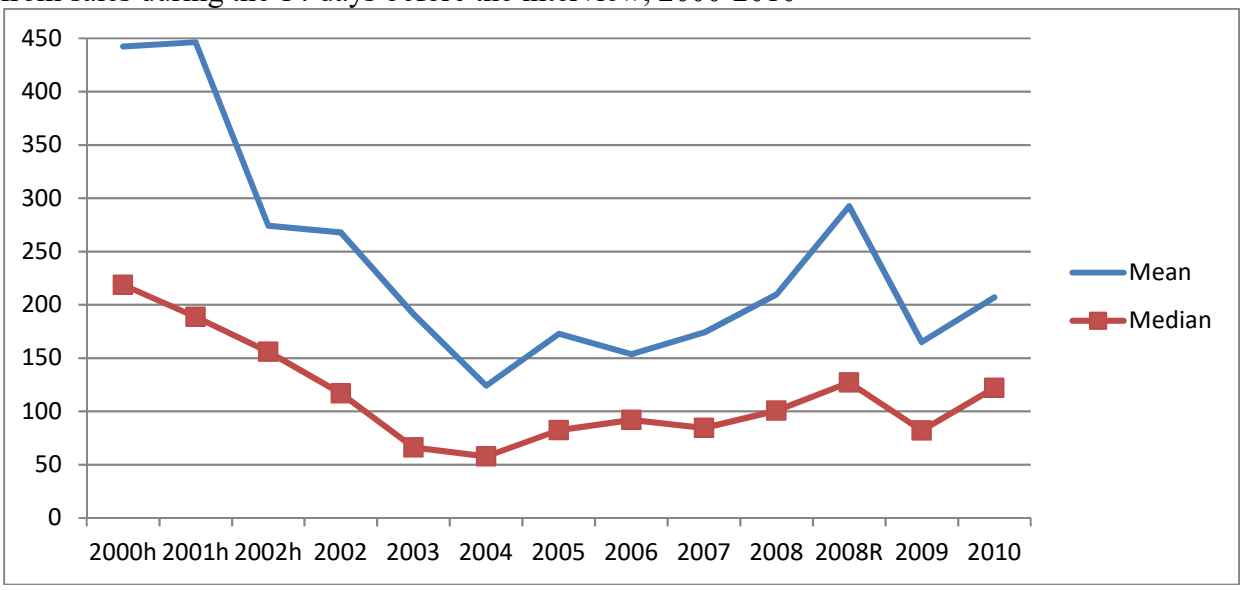
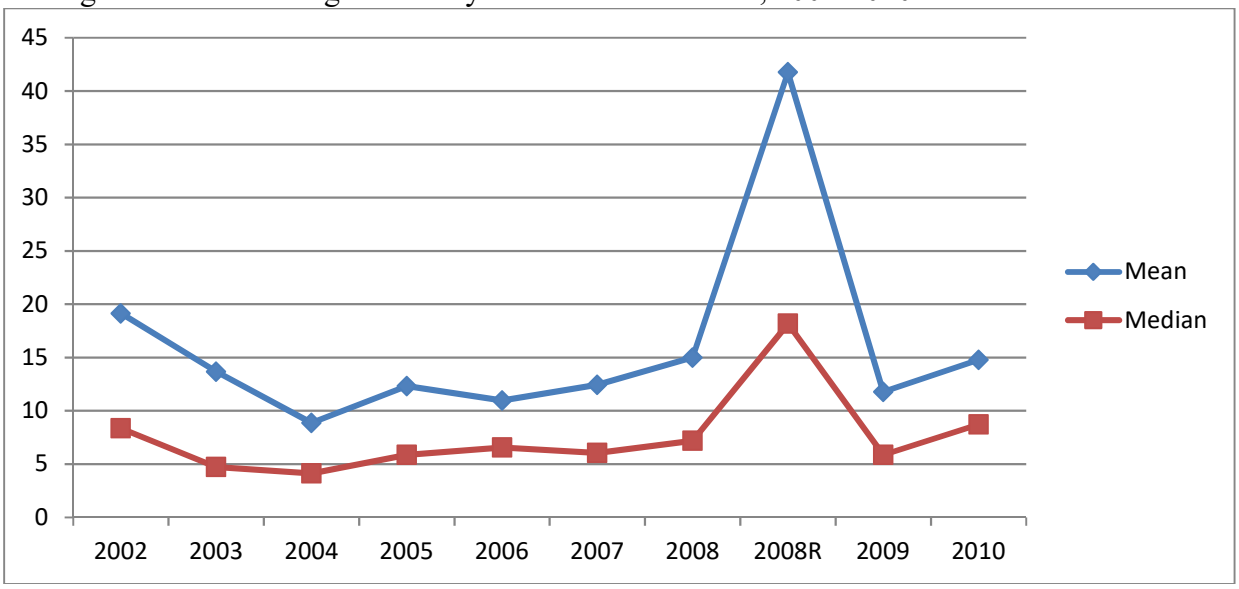
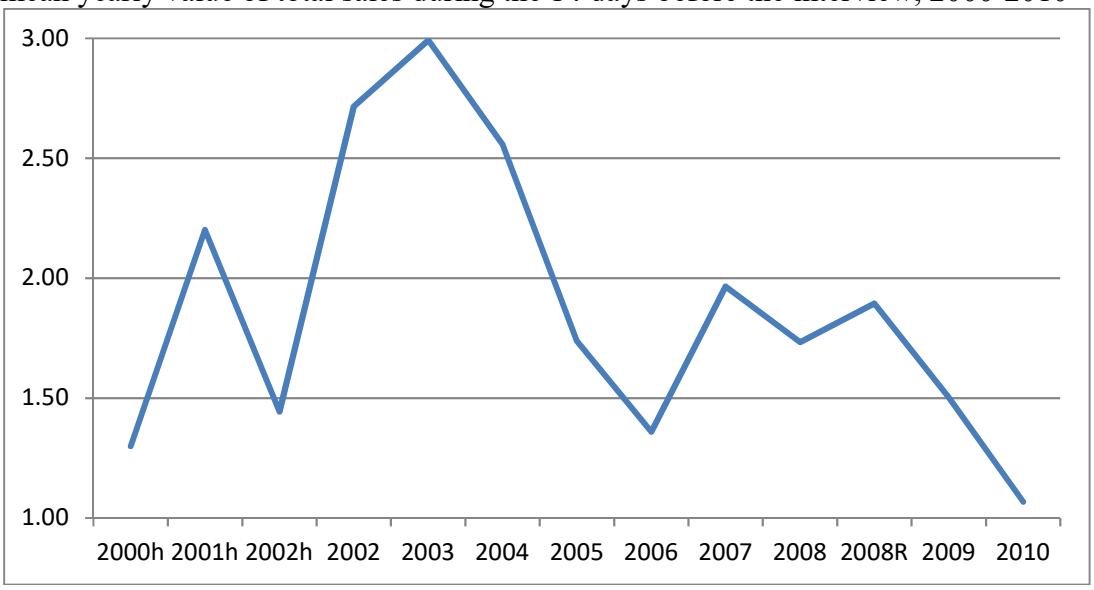


Fig. 7.10b. Yearly mean and median inflation-adjusted (real) value in *bolivianos* of *daily* earnings from sales during the 14 days before the interview, 2002-2010



Note: The suffix 'h' stands for household-level surveys and the suffix R stands for the baseline (2008) of the randomized-controlled trial. Data for Figure 10a comes from yearly values of columns 4-5 (Table 7.19) converted to real values using Bolivia's CPI index (see notes to Table 7.20). Data for Figure 10b comes from columns 6-7, Table 7.21.

Fig. 7.11. Coefficient of variation (CV; standard deviation [SD]/mean) of inflation-adjusted (real) mean yearly value of total sales during the 14 days before the interview, 2000-2010



Note: The suffix 'h' with years stands for households and the suffix R stands for the baseline (2008) of the randomized-controlled trial. Raw data for mean and standard deviation of nominal values comes from columns 4 and 6 of yearly values of Table 7.19. To convert nominal values to real values I used the CPI index, noted in Table 7.20.

Fig. 7.12a. *Frequency* of sales (%) by type of goods sold and season, May 2002-April 2003

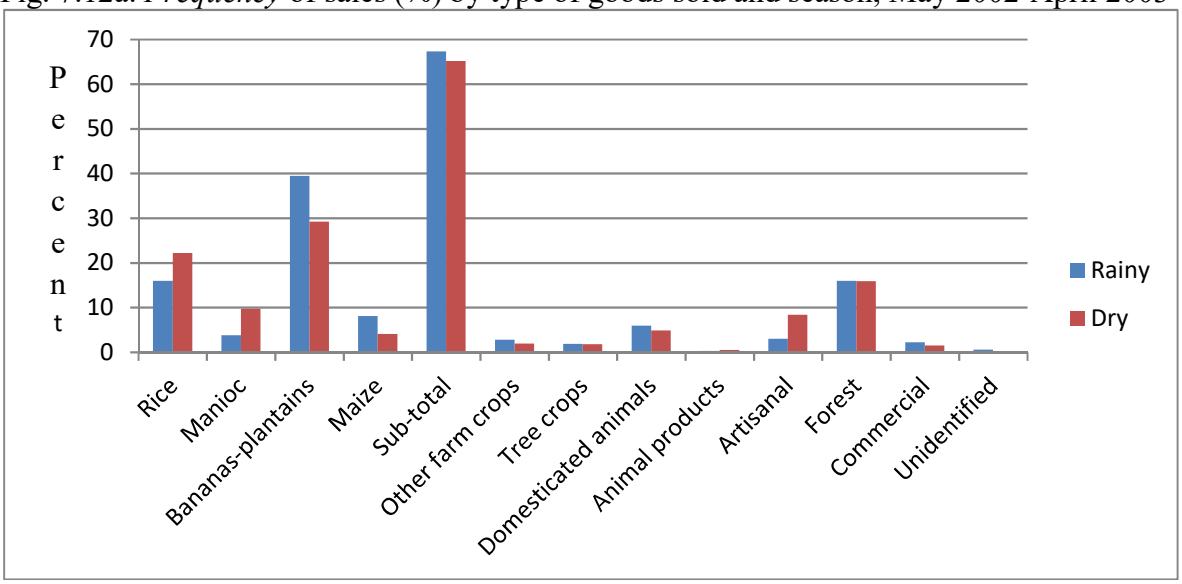
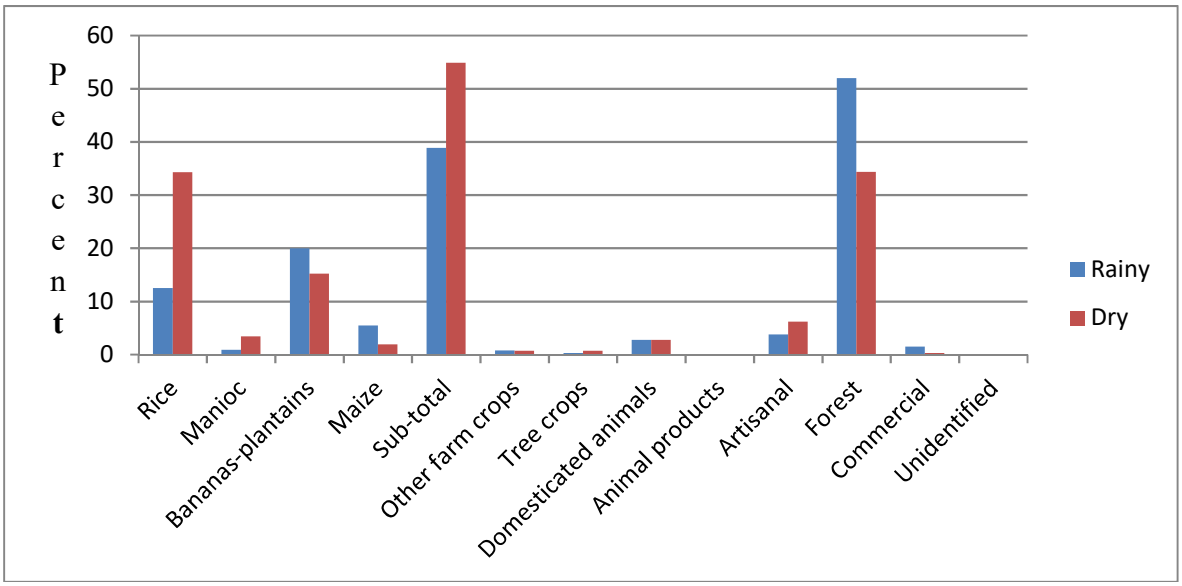
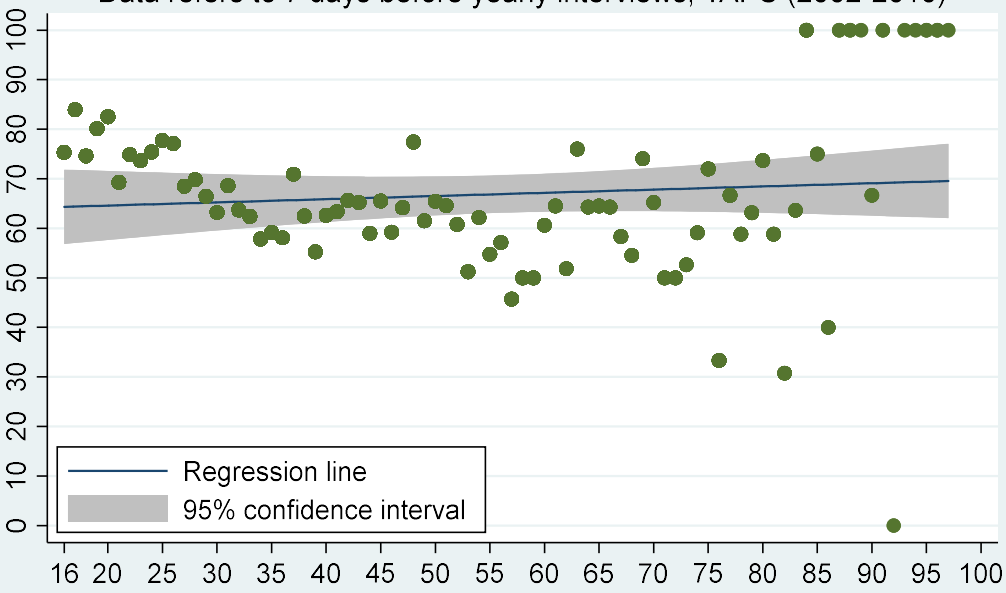


Fig. 7.12b. *Value* of sale earnings (%) by types of goods sold and season, May 2002-April 2003



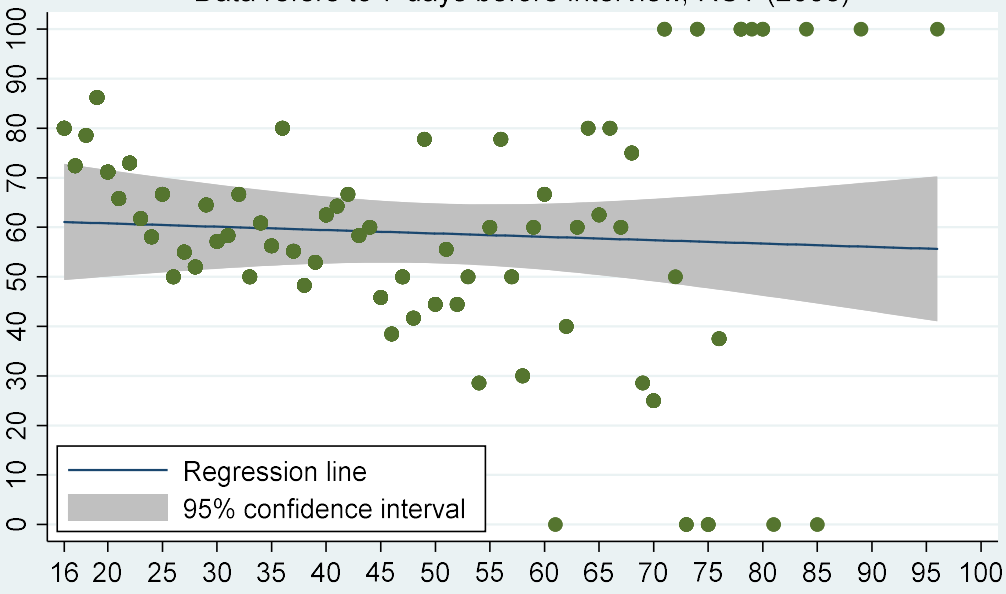
Note: Information for Figure 7.12A and Figure 7.12B come from sections A and B of Table 7.22. To compute the shares, I excluded the second and third quarter of 2003 because they had few observations. The rainy season includes the third quarter of 2002 and the fourth quarter of 2003; the dry season includes the first and second quarters of 2002 and the first quarter of 2003. "Sub-total" refers to the sum of the share of rice, manioc, bananas-plantains, and maize. The rainy season extends from November until April and the dry season extends from May until October. The shares are computed for a season.

Fig. 7.13a. Share of people 16y≤age≤97y without sale earnings, by age:
Data refers to 7 days before yearly interviews, TAPS (2002-2010)



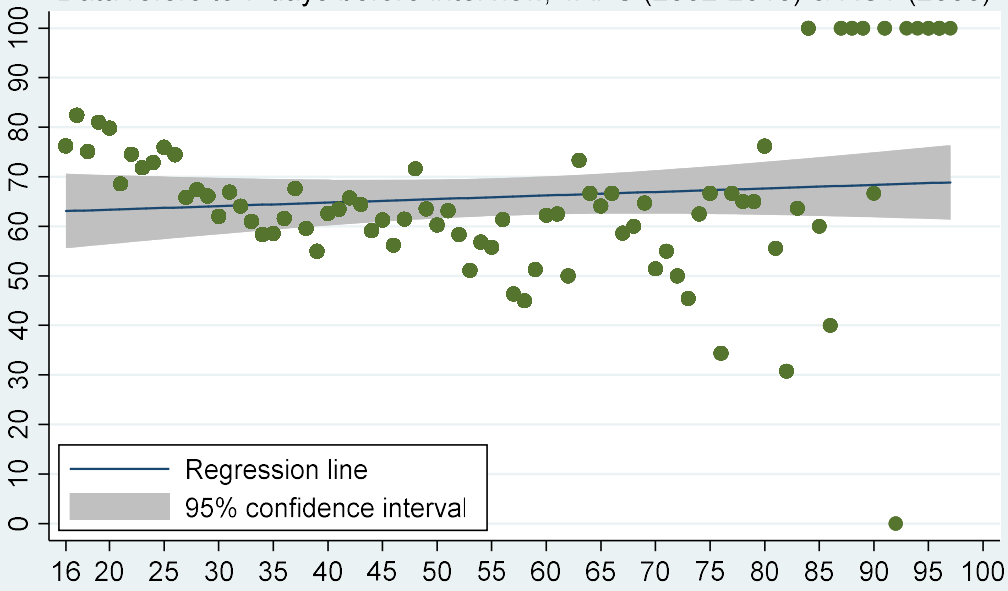
Slope from OLS regression with robust standard errors=0.06, p=0.54, N=82

Fig. 7.13b. Share of people 16y≤age≤97y without sale earnings, by age:
Data refers to 7 days before interview, RCT (2008)



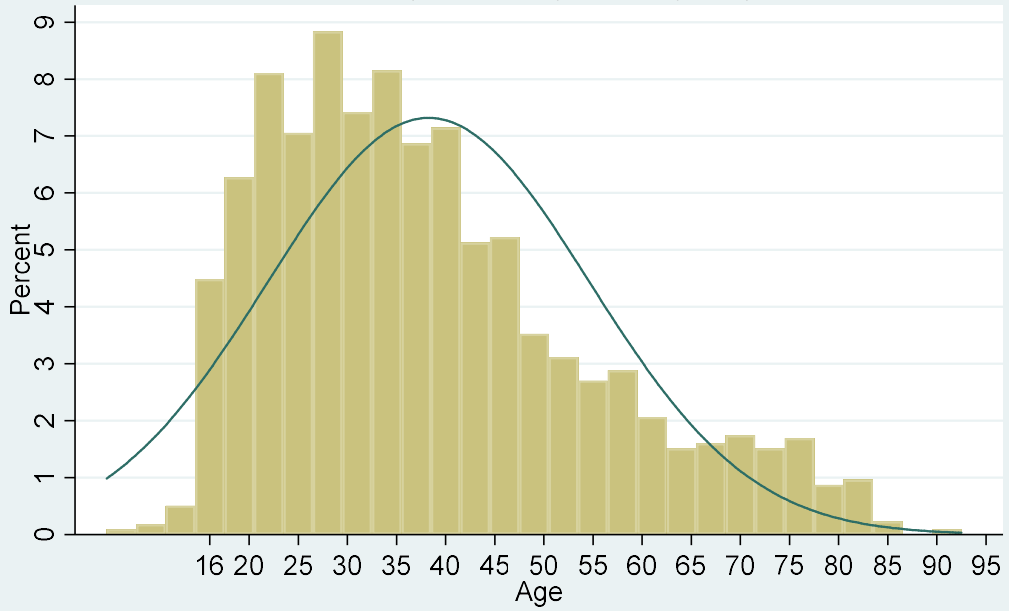
Slope from OLS regression with robust standard errors=-0.06, p=0.72, N=69

Fig. 7.13c. Share of people 16y≤age≤97y without sale earnings, by age:
Data refers to 7 days before interview, TAPS (2002-2010) & RCT (2008)



Slope from OLS regression with robust standard errors=0.07, p=0.503, N=82

Fig 7.14. Age of any person selling during the 7 days before the interview: TAPS (2002-2010) & RCT (2008)

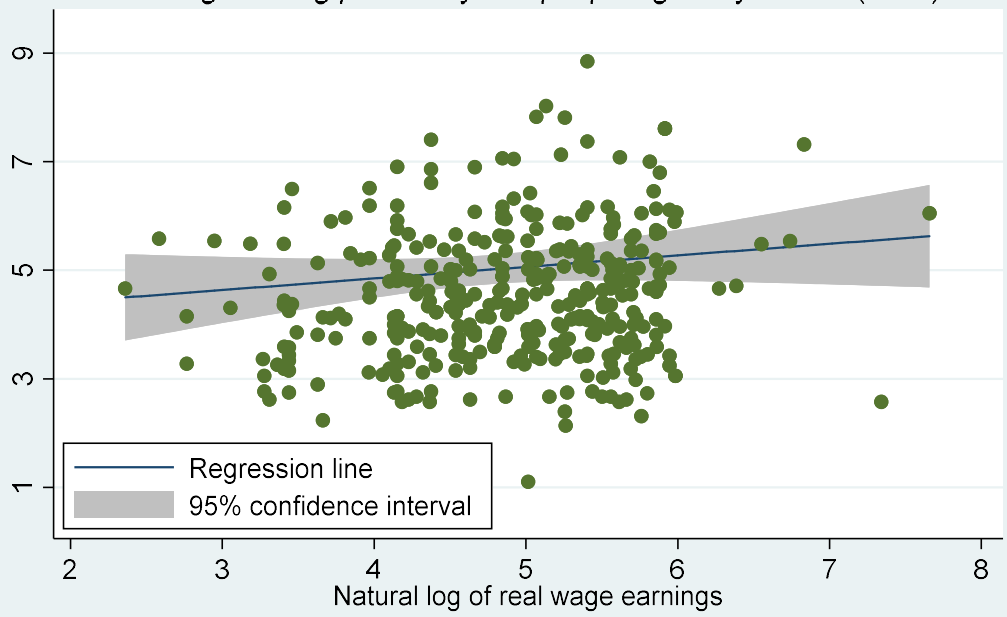


Observations (obs)=2,183. Age: min=7, max=92. Age<16y=1.4%; age>65y=8.7% of obs.

Fig. 7.15a. Elasticity of real sale earnings with respect to real wage earnings:
Earnings during past 7 days for people age≥16y in TAPS (2002-2010)

Elasticity from OLS regression with robust standard errors=0.25, p=0.005, N=267

Fig. 7.15b. Elasticity of sale earnings with respect to wage earnings:
Earnings during past 7 days for people age≥16y in RCT (2008)



Elasticity from OLS regression with robust standard errors=0.21, p=0.16, N=92

Fig. 7.15c. Elasticity of sale earnings with respect to wage earnings:

Earnings during past 7 days for people age≥16y in TAPS (2002-2010) & RCT (2008)

Elasticity from OLS regression with robust standard errors=0.20, p=0.01, N=359

Table 7.1. Summary of surveys on earnings from wage labor and sales used in the chapter: Samples include households or people even if they reported earning no cash from wage labor or sales, or had missing information

	Survey year:												
				TAPS									RCT[8]
	2000	2001	2002	2002	2003	2004	2005	2006	2007	2008	2009	2010	2008
Survey frequency/year	1	1	1	3[1]	2[1]	1	1	1	1	1	1	1	1
Type of data	Cross section	Longitudinal (RCT)[7-8]		Longitudinal									Cross section
Entity surveyed	Household[2]	Household[3]		People[4]									
Number of entities surveyed/village	9/village[5]	~10/village[6]		All									
[A] Data on wage labor. Unique [without repeats] sample size of[7]:													
Villages	58	37	37	13	13	13	14	14	13	13	13	13	40
Households	508	378	330	266	252	236	252	262	250	261	253	268	562
People[4]	NM	NM	NM	693	623	574	678	679	608	632	597	659	1362
[B] Data on sales. Unique [without repeats] sample size of[7]:													
Villages	58	37	37	13	13	13	14	14	13	13	13	13	40
Households	508	378	330	265	251	236	252	262	250	261	253	268	563
People[4]	NM	NM	NM	693	623	574	678	679	608	632	597	657	1365

Table 7.1. Summary of surveys on earnings from wage labor and sales used in the chapter: Samples include households or people even if they reported earning no cash from wage labor or sales, or had missing information - continued

Notes: NM=not measured.
[1]During May-December 2002, we did three consecutive quarterly surveys, and during January-August 2003 we did two consecutive quarterly surveys. See Table 7.5 and 7.22 for a breakdown of sample by quarters and seasons, and the discussion around those tables for the number of quarterly surveys per calendar year.
[2] Information came from one household head and referred to the entire household. The income of all people in the household members irrespective of their age was summed for each household.
[3]One household head chosen at random to answer all questions about the household. For questions about earnings from wage labor and sales, surveyors asked respondents to provide commingled information for themselves and their spouse, and the surveyor entered the total amount; we do not have information for each spouse.
[4]In the raw data, any person reporting wage income is included, even if the person was younger than 16 years of age or was not a member of the household but was living in the household at the time of the survey.
[5]We chose the sample based on convenience; we surveyed the household head who was willing to answer survey questions when the research team arrived.
[6]We chose at random ~10 households in a village. Sample size of households per village for sales module in 2002 is slightly smaller at 8.9 households (SD=3.4).
[7]Sample sizes of villages, households, and people come from the modules on wage labor or sales. The sample of households includes all households, even if they had no or only some information on wages or sales.
[8]RCT=randomized-controlled trial. See Chapter 4 for a description of the interventions for each of the trials.

Table 7.2. Self-reported answers at baseline (2011) about expected end uses of monetary savings by household heads who received a savings box in a two-year (2011-2012) randomized-controlled trial

Category of item:			*Examples*	*Comment*
Type	*N*	*%*		
Clothing	231	37.68	Any clothing, shoes, pants	Includes 3 sewing machines
Transport	75	12.23	Motorcycle, bicycle, outboard motor	
Tools	69	11.26	Machete, rifles, shovels	Includes bullets for rifles or shotguns
Medicines and hygiene	62	10.11	Generic medicines	Includes soap and mosquito nets
Kitchen utensils	57	9.30	Pots, plates	
Food	46	7.50	Noodles, sugar, meat	
Luxuries	23	3.75	Radio, TV, gas stove	
Domesticated animals	22	3.59	Cattle, pigs	
Construction materials	14	2.28	Tin roof, barb wire	
Other	14	2.28	House in town, land, festivity	Only one subject mentioned wanting to start a business
Total	613	100.00		

Notes: The trial assigned a small wooden saving boxes to households selected at random. In a household, we chose at random between the female or the male household head to receive the box. We asked winners what they intended to use the cash. The question was open ended, so a household head could mention many expected end-uses. In making the table, I considered the first end use mentioned. I left out some household heads from the tabulation because they did not know what they were going to save for, provided muddled answers, or did not answer the question.

Table 7.3. Employers of Tsimane' during the week or two weeks before the interview, 2000-2010

	Year:													Yearly grand mean[15]
	2000	2001	2002	2002	2003	2004	2005	2006	2007	2008	2008R[1]	2009	2010	
Survey done once each:	Year	Year	Year	Quarter	Quarter	Year	Year	Year	Year	Year	Year	Year	Year	
Entity surveyed:	Household	Household[2]	Household[2]	Person[3]	Person[4]	Person	Person	Person	Person	Person	Person	Person	Person	
Observation of entities[5]	329	213	119	549	387	217	243	242	159	197	235	183	148	
Number of unique entities	216	156	86	251	202	151	172	166	74	131	234	131	106	
Recall period[6]	2 weeks										1 week	2 weeks		
	[A] Employer (% of row titled "observation of entities" above):													
Smallholder	20.9	9.8	5.8	11.4	3.8	11.9	12.3	7.4	6.2	5.0	14.0	8.2	10.1	9.8
Logger	26.4	50.2	27.7	19.8	33.8	32.2	31.6	23.1	13.8	10.6	15.3	6.5	15.5	23.4
Trader	17.0	10.8	18.4	4.3	5.4	1.8	0.4	2.0	1.8	2.0	7.2	1.6	2.0	5.7
Tsimane'	0.6	10.3	7.5	6.9	14.4	17.5	9.0	10.7	10.6	12.6	15.7	19.1	18.2	11.8
Cattle rancher	10.0	3.7	9.2	15.1	10.5	14.7	23.0	24.7	27.6	27.4	22.1	34.9	27.7	19.3
Tsimane' Council	1.2	NC	NC	0.3	0.7	0.9	1.6	0	0	0.5	0	0	0	0.5
Cooperative	0.6	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	0.6
Missionaries[7]	0.9	NC	NC	1.2	0	0	1.2	0	0	0	0.4	0	0	0.3
Oil firm	2.1	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	NC	2.1
Teacher	1.5	11.2	25.2	2.5	2.5	1.3	1.2	4.1	3.7	2.5	1.7	0.5	0	4.5
Stores[8]	NC	NC	NC	0.1	1.8	3.6	0.4	0	7.5	1.0	0	0	0	1.5
Researchers[9]	NC	NC	NC	15.8	8.2	3.6	3.7	4.5	5.6	9.1	0	6.5	3.3	6.1
Local doctor[10]	NC	NC	NC	3.4	0.5	0	0	0	0.6	0	0	0	0	0.5
Government[11]	NC	NC	NC	16.5	17.0	11.5	10.2	17.3	12.5	10.6	8.9	14.7	17.5	13.7
Town dweller[12]	NC	NC	NC	1.4	0	0.9	2.4	4.9	7.5	14.7	8.9	4.3	2.7	4.8
Municipality	NC	NC	NC	NC	NC	NC	NC	0	0	0	2.9[13]	2.1	0	0.8
Other	19.7	3.7	5.8	0.5	0.7	3.2	2.4	0.8	1.8	3.5	2.5	1.0	2.7	3.7
	[B] % of entities hired by 1, 2, or 3 employers in past two weeks (none had more than three employers)[14]:													
Number of employers:														
1	91.6	93.5	96.5	76.1	91.5	97.3	97.6	96.9	100	92.3	100	98.4	100	94.7
2	8.3	6.4	3.4	19.5	7.4	2.6	2.3	3.0	0	7.6	0	1.5	0	4.8
3	0	0	0	4.3	0.9	0	0	0	0	0	0	0	0	0.4

Table 7.3. Employers of Tsimane' during the week or two weeks before the interview, 2000-2010 – continued

Notes: NC = category not coded in survey. Sample comes from raw data in the module on wage labor. The sample generally refers to people age≥16years, but includes any person found in the module on wage labor. Younger people could be in the module if they had worked for wages.
[1]Baseline of randomized-controlled trial. We only collected data on wage earnings for the seven days before the day of the interview.
[2]Total for wife and husband combined as reported by one household head chosen at random.
[3-4]During May-December 2002, we did three consecutive quarterly surveys, and during January-October 2003 we did four consecutive quarterly surveys. See Table 7.5 for breakdown by quarter of the surveys done in 2002 and 2003.
[5]Sample size of observations, where observations indicate the type of employer (event). Since information was usually collected separately for the 7 days before the interview and for the 8-14 days before the interview, the information could contain valid double counting. For example, a rancher who hired a Tsimane' for 14 consecutive days before the interview would appear as two employers.
[6]One week = only 7 days before the interview; 2 weeks=7 days before the interview plus 8-14 days before the interview.
[7]Work done for Protestant missionaries in villages or in the headquarters of the missionaries, on the outskirt of the town of San Borja.
[8]Stores in the town of San Borja.
[9]Working mainly for our team, but also for other research teams in the area.
[10]A rural healer who lives in a place known as Embocada, a riparian settlement about an hour away from the town of San Borja.
[11]This broad category includes park rangers and people working for conservation organizations, some with close ties to the government.
[12]Work for residents of the town of San Borja; some of this works probably overlaps with the category of "Cattle rancher" above since some cattle ranchers live in the town of San Borja.
[13]Includes 2.5% working for municipal government and 0.4 working for the town (*corregimiento*).
[14]Total number of employers does not distinguish between employers. For example, a Tsimane' working for one cattle rancher one week and for another cattle rancher the next week, would have two employers, but so would a Tsimane' who had worked for the same cattle rancher during each of the two weeks before the interview.
[15]Average for all years in a row.

Table 7.4. Yearly growth rates of employment from longitudinal surveys of individuals, 2002-2010

Employer:	Yearly growth rates (%Δ/year):		
	Stable	Decline	Increase
Smallholder	-0.99		
Trader		-8.13	
Teacher		-8.67	
Logger		-14.68	
Cattle rancher			+12.38
Tsimane'			+6.85

Notes: Growth rates come from ordinary least squares (OLS) regression of the natural log of shares from Table 7.3 (section A) used as an outcome against a continuous variable for survey year (explanatory variable).

Table 7.5. Employers of Tsimane' during the two weeks before the interview, quarterly and seasonal data, 2002-2003

	Year							Season:	
	2002			2003				Rainy	Dry
Quarter:									
Quarter	First	Second	Third	First	Second	Third	Fourth		
Month	May-Jul	Aug-Oct	Nov-Dec	May-Jul	Aug-Oct	Jan	Feb-Apr	Nov-Apr	May-Oct
Total # months in data	3	3	2	3	1 (Aug)	1	3	6	10
Total observations:									
Observation of entities	191	215	143	198	5	19	165	361	575
Number of unique people	140	148	99	134	4	11	113	199	256
	[A] Employer (% of row titled "observation of entities"):								
Smallholder	10.4	12.0	11.8	5.0	0	0	3.0	7.7	8.7
Logger	12.5	23.2	24.4	28.7	20.0	73.6	35.7	30.7	22.4
Trader	4.7	4.6	3.5	3.5	0	5.2	7.8	5.8	4.1
Tsimane'	6.7	6.0	8.3	21.4	0	5.2	7.2	6.9	12.0
Cattle rancher	9.4	19.0	16.7	12.1	20.0	5.2	9.0	16.0	11.4
Tsimane' Council	0	0.9	0	1.5	0	0	0	0.5	0.5
Cooperative	NC	NC	NC	NC	NC	NC	NC	NC	NC
Missionaries	0.5	2.7	0	0	0	0	0	0	1.2
Oil firm	NC	NC	NC	NC	NC	NC	NC	NC	NC
Teacher	1.5	3.7	2.1	0.5	0	0	5.4	3.3	2.0
Stores	0	0	0.7	0	0	0	4.2	2.2	0
Researchers	27.7	10.2	8.3	10.1	20.0	0	6.6	6.6	16.5
Local doctor	0	7.9	1.4	0.5	0	0	0.6	0.8	3.1
Government	23.0	7.9	20.9	15.6	40.0	10.5	18.7	18.0	16.0
Town dweller	2.6	1.4	0	0	0	0	0	0	1.3
Municipality	NC	NC	NC	NC	NC	NC	NC	NC	NC
Other	0.5	0	1.4	0.5	0	0	1.2	1.1	0.3
Number of employers:	[B] % of people hired by 1, 2, 3, or 4 employers[1]:								
1	78.5	82.2	96.8	84.3	75.0	57.1	94.7	73.4	73.3
2	16.4	13.9	3.13	13.4	25.0	42.8	5.2	20.3	19.1
3	5.0	3.8	0	2.2	0	0	0	4.6	5.5
4	0	0	0	0	0	0	0	1.5	2.0

Table 7.5. Employers of Tsimane' during the two weeks before the interview, quarterly and seasonal data, 2002-2003 - continued

Notes: Same notes as in Table 7.3.
[1]In section B the total number of employers for the dry season and for the rainy season reaches four employers in a few cases owing to the way I pooled observations between years and computed the statistic. A Tsimane' working for two different employers (a and b) in the dry season of 2002 and two different employers (c and d) in the dry season of 2003 would appear as having four different employers in the tabulation of seasonal employment above. For each quarter or smaller window of time, the worker would have only two employers, consistent with the figures on the total number of employers of Table 7.3 and the quarterly information of this table.

Table 7.6. Number of days worked and wage earnings in past two weeks, 2000-2010 (except for 2008 of the randomized-controlled trial, which includes data for the past seven days [row 2008R], shown in *italics*).

Survey Year	Survey quarter and season (D=dry; R=rainy)	Survey data refers to:	[a] Number of days worked					[b] Total earnings in nominal *bolivianos* for workers					
			N[1]/n[2]	% 0[3]	Mean	Median	SD	Mean	Median	SD	% values ending in[4]:		
											0	5	F[5]
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]
					People working > 0 days, earnings>0 *bolivianos*, & reporting employment type								
2000	May-Oct (D)	Household	488/190	61.0	7.9	7.0	4.6	164	136	131	86	6	1
2001			378/156	58.7	6.1	6.0	3.8	144	110	143	84	10	0
2002			328/84	74.0	6.0	5.0	4.3	128	95	109	78	8	0
2002	1.May-Jul (D)	People	591/111	81.2	5.2	5.0	3.7	127	100	106	76	15	1
	2.Aug-Oct (D)		424/86	79.7	4.9	3.0	4.2	122	62	121	77	15	0
	3.Nov-Dec I		352/64	81.8	5.6	3.5	4.7	168	100	238	75	9	3
2003	1.May-Jun (D)		427/111	74.0	6.6	5.0	4.6	167	100	136	63	23	4
	2. Aug (D)		16/4	75.0	7.2	7.0	5.3	230	235	163	60	40	0
	3. Jan I		32/10	68.7	4.6	4.5	3.1	106	107	70	66	33	0
	4.Feb-April I		512/101	80.2	6.0	4.5	4.4	157	100	141	67	17	4
2004	May-Oct (D)		574/151	73.6	6.4	6.0	4.0	209	125	293	80	13	0
2005			678/170	75.6	7.7	7.0	4.7	204	173	172	65	24	1
2006			678/164	75.8	7.7	7.0	4.7	240	200	188	71	18	1
2007			608/104	83.0	7.8	7.0	4.9	274	175	267	66	20	1
2008			632/63	90.0	12.2	14.0	3.0	539	490	408	77	11	0
2008R[6]			*1362/229*	*83.1*	*4.4*	*5.0*	*2.2*	*166*	*122*	*171*	*72*	*20*	*1*
2009			597/52	91.0	13.1	14.0	2.2	737	664	414	55	19	0
2010			659/106	83.9	7.5	7.0	4.9	439	328	520	80	5	1
Grand mean[7]			9336/1956	79.0/76.8	7.2	7.0	4.7	233	150	279	75	15	1

Table 7.6. Number of days worked and wage earnings in the past two weeks, 2000-2010 (except for 2008 of the randomized-controlled trial, which includes data for the past seven days [row 2008R], shown in *italics*) - continued

Notes: SD=Standard deviation.
[1]N=total sample size of adults in raw module on wage labor without missing information on number of days worked, wage earnings, or type of employment.
[2]n=sample of households or adults meeting and reporting values for all three of the following criteria: [a] worked for wage, [b] earned cash from wage labor, and [c] was able to identify the type of employment.
[3]% of entities reporting not having worked for wages who, in addition, also had no missing information on employer and earnings for all recall periods.
[4]Last digit of earnings ending in zero or five for people reporting wage earnings >0.
[5]F=fractional figure for earnings; for example, I would place an observation in this column if surveyors reported earnings with one decimal point, such as 17.2 *bolivianos*.
[6]Baseline (2008) of the randomized-controlled trial.
[7]Columns 6-11 exclude row 2008R because in the baseline of the randomized-controlled trial we asked about earnings and days worked only for the seven days before the interview. Data from row 2008R is included in the computation of the last row of columns 12-14. For the last row of column 4, the first number is the total sample and the second number is the sample of entities that worked for a wage. For the last row of column 5, the first number is the share of entities that did not work for a wage using data from all rows, while the second number is the share excluding the randomized-controlled trial.

Table 7.7. Yearly growth rates (% Δ/year)[1] for *(i)* share of households or people earning no cash from wage labor and *(ii)* days worked, cash earnings, and wages for employed entities. Growth rates are based on aggregate statistics from Table 7.6

Outcomes:	Sample covers[2]:	
	2000-2010	2002-2010
	[1]	[2]
[a] Share of entities without cash earnings from wage labor	3.56	2.07
[b] Days worked in past 14 days		
Mean	5.47	8.39
Median	7.28	11.74
[c] Nominal earnings in past 14 days		
Mean	15.16	18.80
Median	16.14	21.34
[d] Inflation-adjusted (real) earnings in past 14 days[3]		
Mean	9.94	12.68
Median	10.91	15.22
[e] Implicit daily wage in past 14 days[4]:		
[1] Nominal		
Mean	9.69	10.41
Median	8.86	9.60
[2] Real		
Mean	4.47	4.29
Median	3.64	3.48
[f] Contribution of growth rates of real median wages and median days worked to growth rate of real median earnings		
Wages[5]	33.36%	22.86%
Days worked[6]	66.72%	77.13%

Notes:
[1]The growth rates is the slope of the outcome expressed in natural logarithm against a continuous variable for survey year.
[2]Data excludes 2008 baseline survey of the randomized-controlled trial. To estimate growth rates, the quarterly values for 2002 and 2003 from Table 7.6 were changed to yearly averages. For column [1], n=11; for column [2], n=9.
[3]Real (inflation-adjusted) earnings = nominal earnings/(CPI/100). CPI = consumer price index. CPI values retrieved from World Bank on July 8 2018, https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=BO.
[4]Implicit daily wage=total earnings in *bolivianos* in past 14 days/total days worked in past 14 days.
[5]Median values of section [e2] divided by median values of section [d].
[6]Median values of section [b] divided by median values of section [d].

Table 7.8. Yearly trends in daily village real[1] wage (P_{wage}=*bolivianos*/day) relative to village selling real[1] price of rice (P_{rice}=*bolivianos*/11.5kg): Regression results using village-level yearly data 2002-2010 of relative prices (P_{wage}/P_{rice}) used as an outcome against survey year (explanatory variable)

Yearly rate of change	Daily village real wage			
	Without food (n^2=113)		With food (n^2=112)	
	P_{wage}/P_{rice}	Natural log (P_{wage}/P_{rice})	P_{wage}/P_{rice}	Natural log (P_{wage}/P_{rice})
	[1]	[2]	[3]	[4]
Coefficient (%Δ/year)	-0.06	-0.06	-0.04*	-0.05*
Standard error	(0.03)	(0.04)	(0.02)	(0.02)
Constant	127.6*	134.4	97.8**	102.9*
Standard error	(70.74)	(81.76)	(44.61)	(50.38)
R-squared	0.02	0.05	0.02	0.05

Notes: Ordinary least square regressions with robust standard errors and clustering by village. *** p<0.01, ** p<0.05, * p<0.10.

[1]See notes to Table 7.7 for sources of the consumer price index (CPI) to compute values adjusted for inflation.

[2]The unit of measurement and analysis is the daily wage or the selling price of rice in the village at the time of the survey. Rice price is *bolivianos* per *arroba* (11.5 kg) of rice with hull. Since employers sometimes offer lunch to workers, daily wages vary, being higher if workers got cash without lunch. I compute relative real prices using the two forms of payment. Sample size of villages=13, except during 2005-2006 when the sample size reached 14. In 2005-2006 we included an additional village where attriters from the longitudinal study had gone.

Table 7.9. Test of telescoping bias in reported days worked and earnings during the seven days and during the 8-14 days before the interview (except for 2008 of the randomized-controlled trial, which only includes data for the past seven days [row 2008R], shown in *italics*), 2000-2010.

Survey Year	Survey quarter and season (D=dry; R=rainy)	[a] Number of days worked before interview					[b] Total earnings in nominal *bolivianos* for workers before interview				
		Past 7 days		Past 8-14 days		Difference[1]	Past 7 days		Past 8-14 days		Difference[1]
		Mean	SD	Mean	SD	Mean	Mean	SD	Mean	SD	Mean
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
		Values refer to households									
2000	Dry (Q1-Q2)	3.6	3.0	4.2	2.9	-0.5*	78	80	85	77	-7.7
2001		3.0	2.6	3.1	2.9	-0.1	67	78	76	101	-8.4
2002		2.6	2.6	3.3	2.9	-0.6	61	66	66	70	-5.0
		Values refer to individuals									
2002	Q1 (D)	2.8	2.6	2.4	2.7	0.3	70	76	56	68	13.4
	Q2 (D)	2.5	2.4	2.4	2.6	0.1	63	69	59	73	3.9
	Q3 I	2.8	2.7	2.7	2.7	0.1	97	192	70	78	26
2003	Q1 (D)	3.4	2.6	3.1	3.0	0.2	86	76	80	83	6.6
	Q2 (D)	5.5	3.0	1.7	3.5	3.7	176	102	53	106	123.7
	Q3 I	2.0	2.6	2.6	1.8	-0.6	45	50	60	44	-15.0
	Q4 I	3.2	2.6	2.7	2.9	0.4	87	81	70	85	16.4*
2004	Dry (Q1-Q2)	3.1	2.5	3.3	2.8	-0.2	103	168	106	149	-2.1
2005		3.7	2.9	3.5	3.1	0.1	106	96	98	105	7.4
2006		3.9	2.8	3.7	3.1	0.2	127	128	112	109	14.0
2007		4.0	2.8	3.8	3.0	0.2	134	128	139	186	-5.4
2008		6.1	1.7	6.1	1.6	-0.1	255	158	284	287	-28.9
2008R		*4.4*	*2.2*	*Not measured*[2]			*166*	*171*	*Note measured*[2]		
2009		6.8	0.8	6.2	1.6	0.5**	374	203	362	218	11.1
2010		3.8	3.0	3.6	3.1	0.1	218	268	221	312	-3.0
Grand mean[3]		3.6	2.8	3.6	3.0	0.1	116	146	116	162	0.7

Notes: Restrictions for sample size and definitions are the same as those in Table 7.6. SD=Standard deviation.
[1]Two-tailed t-test for difference in mean between the seven days and the 8-14 days before the interview. The sample size for the tests of the equality of mean differences is n in column 4 of Table 7.6.
[2]In the baseline of the randomized-controlled trial we only asked about wage earnings for the seven days before the interview.
[3]Grand mean excludes row 2008R. * and ** significant at ≤5% and ≤1%.

Table 7.10. Village daily nominal cash wages in *bolivianos*, without lunch and with lunch offered by employers as reported by a village leader: Village surveys, 2000-2010

Survey year	N	Without lunch				With lunch				Difference in means (n)[1]
		% missing	Mean	Median	SD	% missing	Mean	Median	SD	
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
2000	58	30	26.0	25.0	7.2	17	20.7	20.0	6.2	5.5 (38)
2001	38	28	24.0	25.0	6.9	23	19.4	20.0	4.6	4.7 (27)
2002[2]	37	21	23.1	25.0	4.8	10	19.8	20.0	6.1	3.9 (29)
2003[2]	13	0	26.1	25.0	2.1	0	21.1	20.0	2.1	5.0 (13)
2004	13	0	27.3	25.0	6.3	7	22.9	20.0	3.9	5.0 (12)
2005	13	0	28.8	30.0	3.6	0	24.2	25.0	3.4	4.6 (13)
2006[3]	14	7	32.3	30.0	8.5	7	27.3	25.0	7.5	5.0 (13)
2007	13	0	30.3	30.0	2.4	0	25.0	25.0	2.0	5.3 (13)
2008	13	0	40.0	40.0	8.4	0	31.5	35.0	5.5	8.4 (13)
2008R	40	0	35.5	35.0	6.0	0	28.1	30.0	4.6	7.3 (40)
2009	13	0	48.8	50.0	8.6	0	41.1	40.0	7.9	7.6 (13)
2010	13	7	49.3	55.0	35.2	30	49.4	50.0	6.3	15.0 (9)
Grand total or mean	278	14	31.0	30.0	12.4	10	25.2	25.0	8.9	6.0 (233)**

Notes: % missing = percent of observations with missing values.
[1]Sample size used in comparison is in parenthesis. Difference is between mean cash wage without lunch minus mean wage with lunch. Except for the last row, in the values of the last column (11) I do not report t-tests of statistical significance because sample sizes are small.
[2]2002 data come from household surveys while 2003 data comes from the quarterly surveys of 2003. For 2003 wages, I only include the quarterly surveys from May until October so that the dates of data collection for 2003 match the dates of data collection for the other years.
[3]To track attriters, we included an extra village in 2006 where attriters went. * and ** significant at ≤5% and ≤1%.

Table 7.11. Descriptive statistics of days worked and earnings during the seven days and during the 8-14 days before the interview (except for 2008 of the randomized-controlled trial (RCT), which only includes data for the past seven days). Results for different samples, 2000-2010

Item	Surveys											
	A. All			B. Individual longitudinal (TAPS)				C. Baseline (2008) of RCT (n=229)		Test of equality of means, Bii=C[1]	D. Bi + C (1097)	
				[i] All (n=868)		[ii] Only 2008 (n=63)						
	N	Mean	SD	Mean	SD	Mean	SD	Mean	SD		Mean	SD
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]
	I. Attributes of survey											
Entities surveyed	Household & individual			Individual		Individual		Individual		Individual	Individual	
Years	2000-2010			2002-2010		2008		2008		2008	2002-10	
Exclude[2]	None			2000-01 & RCT		2000-07, RCT & 2009-10		2000-01 & TAPS (Bi)		2000-07 & 2009-10	2000-01	
	II. Summary statistics											
Days[3]:												
1W	1343	4.7	2.2	4.7	2.2	6.1	1.7	4.4	2.2	-1.6**	4.7	2.2
2w	1114	8.2	4.8	8,2	4.8							
Implicit daily wage:												
Nominal	1343	32.5	23.6	34.0	23.5	44.2	30.1	36.4	24.1	-7.8*	34.5	23.6
Real	1343	41.2	27.5	43.2	25.8	46.8	31.9	38.5	25.6	-8.3*	42.2	25.8
Earnings: nominal												
1W	1343	157.6	156.1	170.5	165.4	255.5	158.9	166.8	171.0	-88.6**	169.8	166.5
2w	1114	275.3	308.6	303.6	335.3							
Earnings: real												
1W	1343	196.9	176.6	211.5	186.7	270.6	168.4	176.7	181.2	-93.9**	204.2	186.1
2w	1114	352.3	348.5	373.3	371.9							

Notes: The restrictions of Table 7.6 apply to this table.

[1]t-test for the equality of means between B[ii] and C. Column [10] shows difference between column [8] and column [6]. * and ** significant ≤5% and ≤1%.

[2]Excluded from the computations

[3]Total number of days worked during the seven days before the interview (1 week, 1W) or during the 14 days before the interview (2 weeks, 2W).

Table 7.12. Yearly growth rate (%Δ/year) of days worked and earnings for different samples

Explanatory variable[3]	Sample:					
	[A] 2000-2010[1]			[B] 2002-2010[2]		
	N	Coef.	SE	N	Coef.	SE
A. Days worked						
1w (past 7 days)	1343	3.1	2.0	1097	8.8**	2.4
2w (past 14 days)	1114	5.1	3.0	868	9.7*	3.4
B. Implicit wage[4]						
Nominal-1 week	1343	8.0**	1.0	1097	9.9**	1.3
Real-1 week	1343	2.7*	1.1	1097	4.0**	1.1
Real-2 weeks	1114	4.0**	0.7			
C. Earnings: nominal						
1w (past 7 days)	1343	11.3**	2.5	1097	18.8**	2.1
2w (past 14 days)	1114	14.1**	3.4	868	19.8**	3.2
D. Earnings: real						
1w (past 7 days)	1343	5.9*	2.4	1097	12.9**	2.0
2w (past 14 days)	1343	9.2*	3.0	868	13.8**	3.1

Notes: Regressions are ordinary least squares (OLS) and include the natural logarithm of the outcome against a continuous variable for survey year. * and ** indicate statistical significance at ≤5% and ≤1%.

[1]Sample corresponds to column A of Table 7.11.

[2]Sample corresponds to column D of Table 7.11. For rows with 1w, the regressions include as a covariate a binary variable for the study: TAPS=1 if sample comes from the villages of the longitudinal study or earlier studies and TAPS=0 if sample comes from the baseline of the randomized-controlled trial (2008). I drop the binary variable for TAPS when estimating growth rates for rows with 2w because in the randomized-controlled trial we only asked about earnings for the week before the interview.

[3]See Table 7.11 for definition of explanatory variables.

[4]Wage=total earnings in *bolivianos* divided by total number of days worked, either during the seven or 14 days before the interview. For the randomized-controlled trial, the recall period covers the seven days before the interview, but for the other surveys the recall period covers the 14 days before the interview.

Table 7.13. Percentage contribution of yearly growth in real wages and days worked to yearly growth of total real earnings from wage labor

[A] Sample			[B] Growth rate		[C] Percentage contribution to earnings[1]
Entities	Recall period: number of days before interview	Years	Item	%Δ/year	
Households and individuals	1 week (7 days)	2000-10	Wage	2.7	45.76
			Days worked	3.1	52.54
			Earnings	5.9	100.00
Individuals	1 week (7 days)	2002-10	Wage	4.0	31.01
			Days worked	8.8	68.22
			Earnings	12.9	100.00
Households and individuals	2 weeks (14 days)	2000-10	Wage	2.7[2]	29.35
			Days worked	5.1	55.43
			Earnings	9.2	84.8
Individuals	2 weeks (14 days)	2002-10	Wage	4.0	28.99
			Days worked	9.7	70.29
			Earnings	13.8	100.00

Notes: The yearly growth rates in this table come from Table 7.12
[1]Shares do not add up to 100 because of rounding.
[2]The wage shown in Table 7.12 is the average of all wages for the two recall periods. The yearly growth rate for wages for the recall period of the past two weeks is 4%

Table 7.14. Individual, household, and village-level predictors of having no wage earnings during the seven days before the interview among men 16y≤age<65y. Regression results with longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable=1 if person had no wage earnings, and zero otherwise

Main type of explanatory variables:

Explanatory variables	Individual	Individual & household	Individual household & wage	Individual & household
	(1)	(2)	(3)	(4)
Age in years	0.002**	0.002**	0.002**	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)
Maximum school grade	-0.025***	-0.025***	-0.025***	-0.025***
	(0.005)	(0.004)	(0.004)	(0.005)
Anthropometrics:				
Standing height (cm)	0.001	0.001	0.001	0.001
	(0.003)	(0.003)	(0.003)	(0.002)
Body weight (kg)	-0.004**	-0.004**	-0.004**	-0.004**
	(0.002)	(0.002)	(0.002)	(0.002)
Mid-arm muscle area (cm^2)	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Demography-Household composition:				
# of women age≥16y in annual survey		0.005	0.004	0.019
		(0.020)	(0.020)	(0.017)
# of men age≥16y in annual survey		0.033**	0.031**	0.019
		(0.015)	(0.015)	(0.014)
# of girls age<16y in annual survey		0.002	0.003	0.004
		(0.008)	(0.008)	(0.008)
# of boys age<16y in annual survey		-0.008	-0.008	-0.008
		(0.008)	(0.008)	(0.007)
Village wage *(*nominal *bolivianos)**:***				
Daily village wage, no lunch			-0.001*	
			(0.001)	
Daily village wage, with lunch			-0.000	
			(0.001)	
Other:				
Survey year (2002-2010)	0.022***	0.023***	0.026***	0.022***
	(0.004)	(0.004)	(0.006)	(0.005)
TAPS	0.013	0.023	0.033	-0.526***
	(0.027)	(0.028)	(0.029)	(0.057)
Constant	-44.097***	-46.149***	-51.504***	-42.168***
	(8.519)	(8.454)	(11.316)	(9.918)

Table 7.14. Individual, household, and village-level predictors of having no wage earnings during the seven days before the interview among men 16y≤age<65y. Regression results with longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable=1 if person had no wage earnings, and zero otherwise - continued.

	Main type of explanatory variables:			
Explanatory variables	Individual	Individual & household	Individual, household & wage	Individual & household
	(1)	(2)	(3)	(4)
Observations	2,692	2,692	2,638	2,692
R-squared	0.051	0.055	0.056	0.135
Village & year fixed effects	No	No	No	Yes
p>F of test for joint effects of variables re:				
Anthropometrics	0.02	0.03	0.03	0.03
Demography		0.06	0.10	0.12
Village wage			0.10	

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. The unit of analysis is the person measured during a survey year. TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial

Table 7.15. Predictors of real wage earnings and daily real wages during the seven days before the interview among men 16y≤age<65y. Regression results with longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable for columns (1)-(2) = natural logarithm of inflation-adjusted (real) *bolivianos* earned among men with earnings>0. (n=902)

Explanatory variables:	Earnings		Wage
	(1)	(2)	(3)
Age	-0.002	0.000	0.002
	(0.003)	(0.002)	(0.001)
Maximum school grade	0.055***	0.060***	0.014***
	(0.008)	(0.009)	(0.005)
Anthropometrics:			
Standing height (cm)	-0.016**	-0.010	-0.000
	(0.007)	(0.006)	(0.003)
Body weight (kg)	0.009**	0.009**	0.006**
	(0.004)	(0.005)	(0.003)
Mid-arm muscle area (cm^2)	0.001	0.001	0.000
	(0.004)	(0.003)	(0.002)
Other:			
Survey year	0.081***	0.084***	0.039***
	(0.012)	(0.015)	(0.006)
TAPS	0.388***	1.122**	0.265***
	(0.082)	(0.524)	(0.096)
Constant	-156.384***	-163.730***	-75.841***
	(24.624)	(29.450)	(12.939)
R-squared	0.166	0.285	0.251
Village & year fixed effects	No	Yes	Yes
p>F of test for joint effect of variables re:			
Anthropometrics	0.05	0.15	0.04

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. Regression in columns (2)-(3) includes full set of binary variables for each survey year and for each village. The unit of analysis is the person measured during a survey year. TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial. Outcome variable for columns (1)-(2) = natural logarithm of inflation-adjusted (real) *bolivianos* earned among men with earnings>0. Outcome variable for column (3) = natural logarithm of real earnings during the seven days before the interview divided by total number of hours worked during the seven days before the interview.

Table 7.16. Total fixed and random effects of completing five years of schooling on the likelihood of joining the workforce (no wage earnings) and on the amount of earnings for a man in a village with one standard deviation (SD) lower or higher average village schooling

Outcome	Units of effect	Effect is:		Total effect (fixed + random) for village with one standard deviation lower or higher average village schooling	
		Fixed	Random[3]	Lower	Higher
		(1)	(2)	(3)	(4)
Workforce	PP[4]	-0.109[1]	0.015	-0.124	-0.093
Earnings	%	0.237[2]	0.043	+0.193	+0.280

Notes:
[1]Value = coefficient of completed years of schooling comes from column (1), Appendix B, Table B.7.3, times five years of primary schooling.
[2]Same as note 1, but with coefficient of completed years of schooling from column (3), Appendix B, Table B.7.3.
[3]Square root of the variance for the variable with random effects from the section titled, "Random-effect parameters", Appendix B, Table B.7.3.
[4]PP = percentage points.

Table 7.17. Items sold by Tsimane' during the week or the two weeks before the interview, 2000-2010

	Year:													Yearly grand mean/median[9]
	2000	2001	2002	2002	2003	2004	2005	2006	2007	2008	2008R	2009	2010	
Survey done once each:	Year	Year	Year	Quarter	Quarter	Year	Year	Year	Year	Year	Year	Year	Year	
Recall period	2 weeks										1 week	2 weeks		
Entity surveyed:	Household	Household	Household	Person	Person	Person	Person	Person	Person	Person	Person	Person	Person	
Number of unique entities[1]	507	273	244	688	610	574	678	679	608	632	1365	597	657	624/610
# entities that sold[2]	298	161	175	410	304	227	342	271	260	300	522	228	270	290/271
# entities selling both weeks[3]	176	82	62	132	52	46	85	70	73	77	NA	49	65	81/71
% entities without sales	(41.22)	(41.02)	(28.28)	(40.41)	(50.16)	(60.45)	(49.56)	(60.09)	(57.24)	(52.53)	(61.76)	(61.81)	(58.90)	(51.03/52.53)
Total # of sale records[4]	1581	1192	1018	3771	2364	1245	1559	1517	1328	1374	1570	1279	1469	1636/1469
Total # of items sold[5]:	978	699	602	1367	682	355	580	467	423	455	727	341	458	626/580
# items unidentified[6]	(30)	(1)	(1)	(1)	(2)	(3)	(1)	0	0	(2)	0	0	(1)	(3)/(1)
# identified items	948	698	601	1366)	680	352	579	467	423	453	727	341	457	561/523
Total sales value of all items sold[7]	82.61	47.41	29.34	70.66	38.81	19.61	43.18	31.90	37.53	59.48	144.18	36.77	55.92	53.65/43.18
Total # of records no sale	603	493	416	2404	1682	890	979	1050	905	919	843	938	1011	1010/919
	[A] Breakdown of sales (%) by type of item sold (percent computed from the total # of items sold [identified and unidentified])[8]:													
Main crops:														
Rice	24.54	24.18	24.58	14.48	30.35	15.65	22.59	27.19	14.42	11.65	41.54	19.94	18.56	22.28/22.59
Manioc	16.26	9.73	8.97	7.39	9.68	11.55	10.86	15.20	14.18	8.57	9.63	16.13	12.45	11.58/10.86
Bananas-plantains	25.87	25.18	28.07	34.53	27.86	26.48	25.17	25.91	31.91	36.48	30.95	38.42	39.96	30.52/28.07
Maize	13.29	12.02	9.97	3.73	7.77	3.94	4.31	3.00	1.99	6.81	6.74	1.47	1.75	5.91/4.31
Sub-total	(79.96)	(71.10)	(71.59)	(60.13)	(75.66)	(56.62)	(62.93)	(71.31)	(62.41)	(63.52)	(88.86)	(75.95)	(72.71)	(70.21)/(65.83)
Other farm crops	0.41	1.14	1.83	2.63	1.76	1.69	0.34	1.71	0.24	0.88	0.14	0.59	1.09	1.11/1.09
Tree crops	0.82	1.72	2.82	1.54	2.35	3.10	0.34	2.14	1.42	8.57	0.96	1.47	2.62	2.30/1.72
Domesticated animals	6.85	2.58	2.16	4.68	6.01	11.55	3.79	5.78	6.15	3.52	1.79	3.23	1.97	4.62/3.79
Animal products	0.51	6.29	1.33	0.51	0.29	1.13	0.69	0.21	0.24	0.66	0.14	0.29	0.44	0.98/051
Artisanal	1.94	0.43	0.50	8.49	3.67	9.30	11.03	9.85	7.33	6.59	0.55	5.57	9.83	5.78/6.59
Forest	6.34	16.60	19.27	19.60	9.53	13.80	20.17	8.57	20.57	15.38	7.15	12.61	11.14	13.90/13.80
Commercial	0.10	0	0.33	2.34	0.44	1.97	0.52	0.43	1.65	0.44	0.41	0.29	0	0.69/043
Unidentified	3.07	0.14	0.17	0.07	0.29	0.85	0.17	0	0	0.44	0	0	0.22	0.42/0.17
	[B] Breakdown of sales value (%) by type of item sold (percentages computed from total sales value of all items sold)[3]:													
Main crops:														
Rice	43.81	33.61	33.95	21.96	36.61	12.39	38.14	50.48	24.50	19.97	65.45	22.25	19.47	32.51/33.61
Manioc	6.34	3.90	3.76	2.54	2.98	5.21	3.95	8.42	11.94	11.17	4.90	9.16	6.96	6.25/5.21
Bananas-plantains	15.23	16.34	20.33	19.57	11.30	10.43	11.55	13.57	16.39	21.91	17.24	32.66	43.37	19.22/16.39
Maize	14.06	8.79	10.24	1.98	4.87	3.09	2.40	2.91	0.80	6.96	3.26	1.50	1.20	4.77/3.09
Sub-total	(79.44)	(62.64)	(68.28)	(46.05)	(55.76)	(31.12)	(56.04)	(75.38)	(53.63)	(60.01)	(90.85)	(65.57)	(71.00)	(62.75)/(58.30)
Other farm crops	0.16	0.60	0.54	1.07	0.21	0.49	0.05	0.25	0.10	0.10	0.01	0.04	0.25	0.30/0.21
Tree crops	0.24	0.95	1.12	0.63	0.47	0.69	0.20	0.64	0.53	3.11	0.13	0.87	0.69	0.79/0.64
Domesticated animals	4.12	1.41	3.23	2.72	3.13	20.06	11.79	3.54	2.38	3.06	1.25	7.70	0.60	5.00/3.13
Animal products	0.06	2.06	0.98	0.89	0.01	3.86	0.24	0.03	0.01	0.06	0.05	0.08	0.48	0.68/0.08
Artisanal	1.03	0.07	0.56	7.54	2.27	6.55	5.93	6.28	3.09	2.20	0.09	2.32	6.75	3.44/2.32
Forest	13.09	32.22	24.36	41.19	37.43	31.15	24.17	13.72	38.39	31.09	7.53	23.35	18.92	25.89/24.36
Commercial	0.04	0	0.03	0.67	0.64	1.19	0.81	0.12	1.81	0.16	0.03	0.01	0	0.42/0.12
Unidentified	1.75	0.006	0.85	0.007	0.02	4.83	0.69	0	0	0.15	0	0	1.25	0.73/0.02

Table 7.17. Items sold by Tsimane' during the week or the two weeks before the interview, 2000-2010 - continued

	Year:													Yearly grand mean/median[9]
	2000	2001	2002	2002	2003	2004	2005	2006	2007	2008	2008R	2009	2010	
	[C] Number of distinct identified and unidentified items sold by entity selling at least one item (sample comes from row, "Entities that sold" above)													
Mean	2.50	1.19	1.19	1.68	1.49	1.40	1.49	1.50	1.37	1.31	1.24	1.28	1.38	1.46/1.38
Median	2.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	
SD	1.47	0.47	0.41	0.93	0.72	0.69	0.74	0.74	0.61	0.60	0.53	0.57	0.69	0.71/0.69
Coefficient of variation[10]	0.59	0.39	0.34	0.55	0.48	0.49	0.50	0.49	0.45	0.46	0.43	0.45	0.50	0.47/0.48

Notes: NA=not applicable

[1]Entities with complete information on all aspects of sales, such as the type of item sold and value of the item sold.

[2] Entities that sold *either* the week before the interview (1-7 days) or the two weeks before the interview (8-14 days).

[3] Entities that sold in *both* weeks before the interview.

[4]This includes all records of items sold and all records of no sales. An entity selling the same crop on two different days of the previous seven days would have two sale events with positive values for earnings and type of goods sold; if the household sold nothing during the 8-14 days before the interview, it would have one sales record for that recall period, but no values for the type of item sold or for earnings from sales.

[5]The number includes repeats. For example, a household selling the same crop on three different occasions during the past 14 days would be recorded as having sold three items even if the items were the same. A household selling three different goods during the past 14 days would also be recorded as having sold three items.

[6]The good sold could not be identified, but its sales value was noted.

[7]Total value in 1000 *bolivianos* of all sales of identified and unidentified items by all entities in the sample

[8]Besides the four main crops, items in the other categories include some of the following items (shown as examples):

Other farm crops: Sugar cane, sweet potatoes, yams (*ahipa*), chili peppers
Tree crops: lemon, cacao, oranges, papaya
Domesticated animals: Poultry, pigs, ducks, cattle
Animal products: Eggs, milk, sun-dried and salted meat, meat
Artisanal: bow and arrow, necklace, mat, woven bag, hat, axe handle
Forest: honey, firewood, palms for roofing (*jatata*)
Commercial: flour, sugar, flashlight batteries

[9]Average or median for all years in a row; average values are in the numerator and median values are in the denominator

[10]Coefficient of variation=standard deviation/mean

Table 7.18. Number of items sold, and total value of items sold *both* during the seven and during the 8-14 days before the interview, 2000-2010 (except for 2008 of the randomized-controlled trial, which only includes data for the past seven days [row 2008R], shown in *italics*)

Survey Year	[A] Past seven days				[B] Past 8-14 days				[C] Total: past 14 days				[D] Prob[1]
	Number		Value		Number		Value		Number		Value		
	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	Mean	SD	
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]	[12]	[13]	[14]
	Values refer to households												
2000	2.40	1.38	176	217	2.05***	1.24	188	288	4.51	2.36	365	403	10.1***
2001	2.60	1.69	171	303	2.47	1.73	174	418	5.08	3.22	345	696	3.61
2002	2.20	1.68	111	206	2.00	1.39	108	194	4.20	2.89	219	319	20.1***
	Values refer to individuals												
2002	2.72	1.77	107	164	2.10***	1.21	69*	87	4.83	2.35	176	223	13.40***
2003	2.07	1.06	150	352	1.80	1.08	129	389	3.88	1.74	280	727	10.31***
2004	1.43	0.58	61	134	1.32	0.47	53	109	2.76	0.84	114	189	2.64
2005	1.60	0.69	118	227	1.27***	0.56	99	195	2.87	0.91	217	309	14.62***
2006	1.51	0.73	73	73	1.31*	0.57	82	127	2.82	1.03	155	163	12.93***
2007	1.39	0.66	164	428	1.23*	0.48	74*	107	2.63	1.00	238	440	10.63***
2008	1.37	0.64	122	163	1.11***	0.36	128	249	2.49	0.80	251	339	22.20***
2008R	*Not comparable*[2]				*Note measured*[2]								
2009	1.44	0.86	157	181	1.12**	0.38	90**	89	2.57	1.00	247	240	11.18***
2010	1.58	0.89	167	153	1.32**	0.56	166	165	2.90	1.23	333	257	21.69***
Grand mean	1.86		131		1.59		113		3.46		245		
Grand median	1.59		136		1.32		103		2.89		242		

Notes: Sample size = row titled "# entities selling both weeks" in Table 7.17. *** p<0.01, ** p<0.05, * p<0.10 in t-test comparing equality of means between the two recall periods.

[1]=Marginal probability of reporting no sale for the 8-14 days before the interview compared with the seven days before the interview. Results are from OLS regressions with robust standard errors clustered by entity (household or person). Outcome=1 if sale event was zero (i.e., no sale) and 0 if the entity sold a good. The sample size for these regressions is the row titled "Total # of sale records" in Table 7.17. Units are percentage points.

[2]In the baseline of the randomized-controlled trial we only asked about sales for the seven days before the interview. Since the values for the past seven days in the rest of the table come from people who had sold during both weeks, the values from the trial would not be comparable, so they have been excluded.

Table 7. 19. Earnings in *bolivianos* from sale of goods in past two weeks, 2000-2010 (except for 2008 of the randomized-controlled trial, which includes data only for the past seven days [row 2008R], shown in *italics*).

Survey Year	Survey quarter and season (D=dry; R=rainy)	N[1]	Total earnings for entities selling>0 nominal *bolivianos* in goods					
			Mean	Median	SD	% values ending in[2]:		
						0	5	F[3]
[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]
		Values refer to households						
2000	May-Oct (D)	298	277	137	360	48	25	1
2001		161	284	120	625	45	26	1
2002		175	176	100	254	50	26	2
		Values refer to individuals						
2002	1.May-Jul (D)	221	115	45	179	52	16	1
	2.Aug-Oct (D)	213	136	50	202	43	23	1
	3.Nov-Dec I	192	78	37	128	43	23	0
	Yearly average	410	172	75	467	42	18	1
2003	1.May-Jun (D)	216	102	40	209	55	21	1
	2. Aug (D)	3	21	20	2	66	0	0
	3. Jan I	9	34	28	25	55	11	0
	4.Feb-April I	157	110	30	366	62	17	1
	Yearly average	304	127	44	380	54	19	2
2004	May-Oct (D)	227	86	40	220	55	21	1
2005		342	126	60	219	47	19	1
2006		271	117	70	159	52	13	1
2007		260	144	70	283	63	20	1
2008		300	198	95	343	73	18	1
2008R[6]		*522*	*276*	*120*	*523*	*73*	*21*	*1*
2009		228	161	80	242	76	15	1
2010		270	207	122	221	72	21	0
Grand[4]: Mean			181			57	19	
Median				80		55	20	

Notes: SD=Standard deviation.
[1]N = values in row titled '# entities that sold' from Table 7.17
[2]Last digit of earnings ending in zero or five for people reporting sale earnings >0.
[3]F=fractional figure for earnings; for example, I placed an observation in this column if surveyors reported earnings with one decimal point, such as 17.2 *bolivianos*.
[4]In columns 4-5 I exclude quarterly values; the grand mean and median is computed from all yearly values, including 2008R.

Table 7.20. Yearly growth rates (% Δ/year)[1] for aspects of sales based on aggregate statistics from Table 7.17 and Table 7.19

	Years covered in sample:		
	2000-2010	2000-2010	2002-2010 (TAPS)
	[1]	[2]	[3]
Baseline (2008) of RCT included?	Yes	No	No
Outcomes:			
[a] Share of entities that[2]			
[1] Earned cash from sales	-4.99	-4.78	-3.47
[2] Did not earn cash from sales	5.32	5.17	3.33
[3] Sold both weeks	-11.58	-11.58	-3.28
[b] Articles sold by entity[3]			
Distinct items sold by *all* sellers			
[1] Number	-2.66	-2.38	-2.43
[2] Coefficient of variation	-0.32	-0.11	-1.51
All items sold by hard seller *both* weeks			
[3] Number	-7.07		-5.95
[4] Coefficient of variation	-4.93		-1.14
[c] Nominal value of sales[4]			
[1] Mean	-1.09	-2.50	5.42
[2] Median	0.42	-0.60	9.37
[d] Inflation-adjusted (real) sales value			
[1] Mean	-6.48	-7.77	-0.70
[2] Median	-4.97	-5.86	3.25
[3] Coefficient of variation	-10.01	-11.72	-11.46

Table 7.20. Yearly growth rates (% Δ/year)[1] for aspects of sales based on aggregate statistics from Table 7.17 and 7.19 - continued

	Years covered in sample:		
	2000-2010	2000-2010	2002-2010 (TAPS)
	[1]	[2]	[3]
Baseline (2008) of RCT included?	Yes	No	No
[e] Share of sale earnings by categories[6]			
Main crops:			
Rice	-1.70	-3.71	1.97
Manioc	1.86	2.44	4.63
Bananas-plantains	3.57	3.79	2.70
Maize	-16.33	-18.67	-6.38
Sub-total	*0.35*	*-0.29*	*2.73*
Other farm crops	-9.93	-5.90	-25.38
Tree crops	4.47	6.49	0.99
Domesticated animals	-5.78	-4.05	-11.68
Animal products	-16.47	-14.15	-10.96
Artisanal	15.10	21.53	-14.13
Forest	-0.72	0.85	-3.98
Commercial	3.13	6.33	-17.35
Unidentified	-4.40	-4.40	21.91

Notes:
[1]The growth rates is the slope from a regression of the outcome expressed in natural logarithm against a continuous variable for survey year.
[2]I computed the shares by dividing the number of entities that sold, did not sell, or that sold in both weeks by the total number of entities surveyed (Table 7.17).
[3]The growth rates are for the mean number of distinct identified and unidentified items sold by the entity selling at least one item. The averages come from section [C] of Table 7.17 for the entire sample and from Table 7.18 for entities selling during both weeks. I did not compute the growth rate of median values because most of the values were one. The growth rate for the coefficient of variation also comes from section [C] of Table 7.17 for all sellers and from Table 7.18 for entities selling both weeks.
[4]From yearly values of Table 7.19.
[5]Real (inflation-adjusted) earnings = nominal earnings/(CPI/100). CPI = consumer price index. CPI values retrieved from World Bank on July 8 2018, https://data.worldbank.org/indicator/FP.CPI.TOTL?locations=BO.
[6]From section B of Table 7.17.

Table 7.21. Mean and median nominal and inflation-adjusted (real) earnings in *bolivianos* for people who sold goods during the week or two weeks before the interview, data from individuals, 2002-2010

Year	[A] Total nominal sales[1]		[B] Total real sales[2]		[C] Daily real sales[3]	
	Mean	Median	Mean	Median	Mean	Median
[1]	[2]	[3]	[4]	[5]	[6]	[7]
2002	172	75	268	117	19	8
2003	127	44	191	66	14	5
2004	86	40	124	58	9	4
2005	126	60	173	82	12	6
2006	117	70	154	92	11	7
2007	144	70	174	85	12	6
2008	198	95	210	101	15	7
2008R	*276*	*120*	*292*	*127*	*42*	*18*
2009	161	80	165	82	12	6
2010	207	122	207	122	15	9
	Grand (includes 2008R):					
Mean	181		196		16	
Median		80		88		6
	Yearly growth rates (%Δ/year)					
Full sample	0.94	0.85	0.63	0.54	1.56	1.47
Without 2008R	5.42	9.37	-0.70	3.25	-0.70	3.25

Notes:
[1]From columns [4]-[5], Table 7.19.
[2]Values from columns [2]-[3] adjusted by Bolivia's CPI; see notes to Table 7.20 for source of CPI deflator.
[3]Values from column [B] divided by 14 days, except for baseline of randomized-controlled trial, which was divided by seven days.

Table 7.22. Items sold by Tsimane' individuals during the two weeks before the interview, quarterly and seasonal data, 2002-2003

	Year							Season:	
	2002			2003				Rainy	Dry
Quarter									
Quarter	First	Second	Third	First	Second	Third	Fourth		
Months	May-Jul	Aug-Oct	Nov-Dec	May-Jul	Aug-Oct	Jan	Feb-Apr	Nov-Apr	May-Oct
Total # months in data	3	3	2	3	1 (Aug)	1	3	6	10
Number of unique people[1]	508	455	428	489	16	24	460	615/977[8]	701/1403[8]
# people who sold[2]	221	213	192	216	3	9	157	398[9]	613[9]
% of people without sales	(56.50)	(53.19)	(55.14)	(55.83)	(81.25)	(62.50)	(65.87)	(59.26)[9]	(56.31)[9]
Total # of sale records[3]	1461	1263	1047	1223	32	60	1049	2360	3775
Total # of items sold[4]:	562	474	331	428	4	13	237	684	1365
# items unidentified[5]	0	0	(1)	0	0	0	(2)	(3)	0
# identified items	562	474	330	428	4	13	235	681	1365
Total sales value of all items sold[6]	27.03	28.44	15.18	22.47	0.064	0.40	15.88	36.09	73.39
Total # of records no sale	899	789	716	795	28	47	812	1676	2410
	[A] Breakdown of sales (%) by type of item sold (percent computed from the total # of items sold [identified and unidentified)[7]:								
Main crops:									
Rice	28.65	6.12	2.42	31.78	0	7.69	29.54	12.43	23.44
Manioc	9.61	6.96	4.23	12.62	75.00	7.69	3.38	4.09	10.18
Bananas-plantains	27.40	33.54	48.04	26.64	0	23.08	30.80	38.89	29.01
Maize	5.52	2.32	2.72	4.44	0	15.38	13.50	6.58	4.32
Sub-total	(71.17)	(48.95)	(57.40)	(75.47)	(75.00)	(53.85)	(77.22)	(61.99)	(66.96)
Other farm crops	1.75	2.74	3.93	1.40	0	15.38	1.69	2.92	2.05
Tree crops	2.49	0.63	1.21	2.34	0	0	2.53	1.46	1.98
Domesticated animals	4.98	4.64	4.23	4.91	0	15.38	7.59	5.70	4.84
Animal products	0.18	0.84	0.60	0.47	0	0	0	0.29	0.51
Artisanal	6.23	14.35	3.93	4.44	25.00	0	2.11	5.26	7.69
Forest	12.81	24.05	24.77	10.75	0	15.38	7.17	19.44	14.65
Commercial	0.36	3.80	3.63	0.23	0	0	0.84	2.49	1.32
Unidentified	0	0	0.30	0	0	0	0.84	0.44	0

Table 7.22. Items sold by Tsimane' individuals during the two weeks before the interview, quarterly and seasonal data, 2002-2003 - continued

	Year							Season:	
	2002			2003				Rainy	Dry
Quarter									
Quarter	First	Second	Third	First	Second	Third	Fourth		
Months	May-Jul	Aug-Oct	Nov-Dec	May-Jul	Aug-Oct	Jan	Feb-Apr	Nov-Apr	May-Oct
Total # months in data	3	3	2	3	1 (Aug)	1	3	6	10
	[B] Breakdown of sales value (%) by type of item sold (percentages computed from total sales value of all items sold) (sample comes from row, "# of people who sold"):								
Main crops:									
Rice	48.57	7.53	1.63	46.71	0	2.49	23.33	11.55	34.83
Manioc	3.97	1.87	1.25	4.49	68.75	2.49	0.59	1.08	3.49
Bananas-plantains	13.83	18.21	32.30	13.51	0	32.41	7.68	21.33	14.33
Maize	2.83	1.07	2.15	1.90	0	18.70	8.75	5.06	1.99
Sub-total	(69.20)	(28.68)	(37.33)	(66.61)	(68.75)	(56.09)	(40.35)	(39.02)	(54.64)
Other farm crops	0.51	1.46	1.34	0.13	0	6.98	0.16	0.76	0.77
Tree crops	1.45	0.14	0.09	0.48	0	0	0.47	0.24	0.73
Domesticated animals	2.94	2.90	1.98	2.50	0	22.44	3.53	2.98	2.82
Animal products	0.003	0.16	0.09	0.02	0	0	0	0.04	0.72
Artisanal	4.24	11.22	6.51	3.13	31.25	0	1.00	5.86	5.58
Forest	21.54	54.69	50.90	26.88	0	14.63	53.09	49.72	35.01
Commercial	0.05	0.71	1.67	0.20	0	0	1.29	1.33	0.32
Unidentified	0	0	0.03	0	0	0	0.05	0.03	0
	[C] Number of distinct identified and unidentified items sold by person selling at least one item (Sample comes from row, "# of people who sold)								
Mean	1.20	1.18	1.15	1.13	1.00	1.11	1.14	1.14	1.17
Median	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
SD	0.48	0.45	0.40	0.39	0	0.33	0.34	0.37	0.45

Table 7.22. Items sold by Tsimane' individuals during the two weeks before the interview, quarterly and seasonal data, 2002-2003 - continued

Notes:
[1]People with complete information on all aspects of sales, such as the type of item sold, and value of the item sold, even if the person did not sell goods.
[2]People who sold *either* during the week before the interview (1-7 days) or during the two weeks before the interview (8-14 days).
[3]This includes all records of items sold and all records of no sales. An entity selling the same crop on two different days of the previous seven days would have two sale events with positive values for earnings and type of goods sold; if the household sold nothing during the 8-14 days before the interview, it would have one sales record for that recall period, but no values for the type of item sold or earnings from sales.
[4]The number includes repeats. For example, a person selling the same crop on three different occasions during the past 14 days would be recorded as having sold three items even if the items were the same. A person selling three different goods during the past 14 days would also be recorded as having sold three items.
[5]The good sold could not be identified, but its sales value was noted.
[6]Total value in 1000 *bolivianos* of all sales of identified and unidentified items by all people in the sample
[7]Besides the four main crops, items in the other categories include the following items, shown as example:

Other farm crops: Sugar cane, sweet potatoes, yams (*ahipa*), chili peppers
Tree crops: lemon, cacao, oranges, papaya
Domesticated animals: Poultry, pigs, duck, cattle
Animal products: Eggs, milk, sun-dried and salted meat, meat
Artisanal: bow and arrow, necklace, mat, woven bag, hat, axe handle
Forest: honey, firewood, palms for roofing (*jatata*), logs
Commercial: flour, sugar, flashlight batteries

[8]In the fraction, the numerator is the number of unique people measured in the rainy season or in the dry season. For example, a person measured in the rainy season of 2002 and 2003 would be counted as one and would be part of the sample of 615. Since the same person could have been measured in the same season in both years, the number of observations with one repeat per person is included in the denominator. The denominator is not exactly twice as large as the numerator because some people were measured during one season in the two years.
[9] Figures are from sample in the denominator of the row titled "Number of unique people".

Table 7.23. Individual, household, and village-level predictors of having no sale earnings during the seven days before the interview among people age≥16y. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable=1 if person had no sales earnings, and zero otherwise (n=5,985).

	Main type of explanatory variables included:		
Explanatory variables:	(1) Individual	(2) Individual & household	(3) Individual & household
Male	-0.027	-0.045*	-0.095***
	(0.026)	(0.026)	(0.025)
Age in years	-0.003***	-0.003***	-0.002***
	(0.001)	(0.001)	(0.000)
Maximum school grade	0.018***	0.018***	0.018***
	(0.003)	(0.003)	(0.003)
Anthropometrics:			
Standing height (cm)	-0.005**	-0.005**	-0.001
	(0.002)	(0.002)	(0.002)
Body weight (kg)	0.001	0.002	0.002
	(0.001)	(0.001)	(0.001)
Mid-arm muscle area (cm^2)	-0.002*	-0.002*	-0.002*
	(0.001)	(0.001)	(0.001)
Demography-Household composition:			
# of women age≥16y in annual survey		-0.030***	-0.015
		(0.011)	(0.010)
# of men age≥16y in annual survey		0.049***	0.033***
		(0.010)	(0.009)
# of girls age<16y in annual survey		-0.005	-0.006
		(0.006)	(0.005)
# of boys age<16y in annual survey		-0.006	-0.002
		(0.005)	(0.005)

Table 7.23. Individual, household, and village-level predictors of having no sale earnings during the seven days before the interview among people age≥16y. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable=1 if person had no sales earnings, and zero otherwise (n=5,985) - continued

	Main type of explanatory variables included:		
Explanatory variables:	(1) Individual	(2) Individual & household	(3) Individual & household
Other:			
Survey year (2002-2010)	-0.006**	-0.005*	-0.006*
	(0.003)	(0.003)	(0.003)
TAPS	0.065***	0.074***	-0.210
	(0.019)	(0.019)	(0.170)
Constant	14.213***	11.658**	12.323*
	(5.239)	(5.281)	(6.703)
R-squared	0.032	0.038	0.121
Village & year fixed effects	No	No	Yes
p>F of test for joint effects of variables re:			
Anthropometrics	0.02	0.02	0.29
Demography		0.001	0.005

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. Regression in column (3) includes full set of binary variables for each survey year and for each village. The unit of analysis is the person measured during a survey year. TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial.

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Table 7.24. Predictors of real earnings from sales among people age≥16y. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable for columns (1)-(4) = natural logarithm of inflation-adjusted (real) *bolivianos* earned from sales during the seven days before the interview for people with sale earnings>0; column (5) includes real earnings from sales during the 14 days before the interview.

Explanatory variables:	(1)	(2)	(3)	(4)	(5)
Male	0.657***	0.707***	0.684***	0.706***	0.562***
	(0.088)	(0.088)	(0.081)	(0.091)	(0.089)
Age in years	0.002	0.002	0.004**	0.003	0.003
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Maximum school grade	0.003	0.005	0.005	0.010	0.001
	(0.013)	(0.013)	(0.013)	(0.015)	(0.015)
Anthropometrics:					
Standing height (cm)	-0.008	-0.011*	-0.008	-0.010	-0.004
	(0.006)	(0.006)	(0.006)	(0.007)	(0.006)
Body weight (kg)	0.005	0.006	0.008	0.004	0.001
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)
Mid-arm muscle area (cm^2)	0.002	0.002	-0.001	0.002	0.006
	(0.004)	(0.004)	(0.004)	(0.005)	(0.004)
Demography-Household composition					
# of women age ≥16y in annual survey		0.064*	0.128***	0.134***	0.113***
		(0.033)	(0.035)	(0.038)	(0.035)
# of men age≥16y in annual survey		-0.011	-0.052	-0.061	-0.036
		(0.037)	(0.036)	(0.042)	(0.036)
# of girls age<16y in annual survey		-0.026	-0.001	-0.001	0.004
		(0.019)	(0.018)	(0.020)	(0.018)
# of boys age<16y in annual survey		0.048**	0.043**	0.041**	0.036**
		(0.020)	(0.019)	(0.021)	(0.018)

Table 7.24. Predictors of real earnings from sales among people age≥16y. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable for columns (1)-(4) = natural logarithm of inflation-adjusted (real) *bolivianos* earned from sales during the seven days before the interview for people with earnings>0; column (5) includes real earnings from sales during the 14 days before the interview

Explanatory variables:	(1)	(2)	(3)	(4)	(5)
Other:					
Survey year (2002-2010)	0.079***	0.082***	0.065***	0.065***	0.086***
	(0.011)	(0.011)	(0.013)	(0.013)	(0.012)
TAPS	-0.419***	-0.416***	0.224		
	(0.071)	(0.073)	(0.189)		
Constant	-154.006***	-158.338***	-125.311***	-125.080***	-169.267***
	(21.669)	(21.961)	(26.667)	(26.703)	(24.451)
Observations	2,021	2,021	2,021	1,627	2,180
R-squared	0.132	0.145	0.250	0.199	0.178
Village & year fixed effects	No	No	Yes	Yes	Yes
Sample	TAPS, RCT	TAPS, RCT	TAPS, RCT	TAPS	TAPS
p>F of test for joint effects of variables about:					
Anthropometrics:	0.41	0.22	0.33	0.43	0.29
Demography		0.005	0.001	0.001	0.001

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. Regression in columns (2)-(3) includes full set of binary variables for each survey year and for each village (n-1). The unit of analysis is the person measured during a survey year. TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial. Outcome variable for columns (1)-(2) = natural logarithm of inflation-adjusted (real) *bolivianos* earned among men with earnings>0. Outcome variable for column (3) = natural logarithm of real earnings in seven days before the interview divided by total number of hours worked during the seven days before the interview.

Table 7.25. Elasticity of earnings from sales with respect to wage earnings among Tsimane' age≥16y. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable = natural logarithm of real earnings from sales during the seven days before the interview (n=5,982)

Explanatory variable	(1)	(2)	(3)
Natural log of wage earnings	-0.027	-0.031*	-0.062***
	(0.018)	(0.018)	(0.018)
Constant	-115.441***	-102.446***	-97.664***
	(27.112)	(27.348)	(35.342)
R-squared	0.043	0.050	0.140
Village & year fixed effects	No	No	Yes
Demographic variables	No	Yes	Yes

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. Outcome variable is the natural logarithm of cash earnings during the seven days before the interview; raw values of sale and wage earnings were transformed with an inverse hyperbolic sine function before taking natural logarithms to avoid losing people without earnings. Regression in column (3) includes full set of binary variables for each survey year and for each village; regression in column (2) includes four household demographic variables (number of women and men age≥16y, number of girls and boy age<16y). The unit of analysis is the person measured during a survey year. Shown in the table is the main explanatory variable: the natural log of real wage earnings during the seven days before the interview. Explanatory variables not shown are the same as those in Table 7.24 and include: sex, age, schooling, height, weight, musculature, survey year, and a binary variable for the study (TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial).

Table 7.26. Effects of the one-year lagged local foreign currency real exchange rate (in natural logarithms) on cash earnings. Regression results with yearly longitudinal data from TAPS (2002-2010) and from the baseline of the randomized-controlled trial (2008). Outcome variable = natural logarithm of real earnings from sales (columns 1-3) or from wage labor (columns 4-6) during the seven days before the interview.

Explanatory variable:	(1)	(2)	(3)	(4)	(5)	(6)
Lagged exchange rate	1.698***	1.695***	1.991***	-0.595	-0.580	-0.509
	(0.467)	(0.465)	(0.465)	(0.446)	(0.446)	(0.449)
Constant	-418.843***	-406.736***	-469.834***	254.441***	257.138***	238.008***
	(77.226)	(77.252)	(77.403)	(77.497)	(77.522)	(78.554)
Observations	5,458	5,458	5,458	5,068	5,068	5,068
R-squared	0.044	0.050	0.134	0.148	0.150	0.186
Village fixed effects	No	No	Yes	No	No	Yes
Demography included	No	Yes	Yes	No	Yes	Yes
Earnings outcome from	Sales	Sales	Sales	Wages	Wages	Wages

Notes: Regressions are ordinary least squares (OLS) with robust standard errors in parentheses and clustering by individual. *** p<0.01, ** p<0.05, * p<0.1. Outcome variable is the natural logarithm of cash real earnings during the seven days before the interview, either from sales (columns 1-3) or from wage labor (columns 4-6); raw values of sale and wage earnings were transformed with an inverse hyperbolic sine function before taking natural logarithms to avoid losing observations with values of zero. Regressions in columns (3 and 6) include a full set of binary variables for each village, but not for each survey year owing to multicollinearity with the yearly local currency exchange rate. Regressions in columns (2-3; 5-6) include four household demographic variables: number of women and men age≥16y, number of girls and boys age<16y. The unit of analysis is the person measured during a survey year. Shown in the table is the main explanatory variable: the natural logarithm of the mean real local foreign currency exchange rate in the towns of San Borja and Yucumo, measured during the yearly surveys, but lagged by one year. Explanatory variables not shown are the same as those in Table 7.24 for columns 1-3, and include: sex, age, schooling, height, weight, musculature, survey year, and a binary variable for the study (TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if village was part of the longitudinal study along the river Maniqui, and 0 if village was part of the baseline of the randomized-controlled trial). Explanatory variables for wage earnings are the same as those in Table 7.15 for wage earnings, except that here we also include the four household demographic variables; as in Table 7.15, regressions in columns 4-6 are restricted to men 16y≤age≤65y. Real foreign currency exchange rate = average nominal foreign currency exchange rate (*bolivianos*/USA dollar) between the two towns (San Borja and Yucumo), divided by Bolivia's CPI index. Appendix C has the nominal exchange rates.

Appendix A
Number of distinct goods sold by category: 2000-2010

In the column "Number" I include a count of the distinct goods sold under the category. For examples of the types of items under a category see notes to Table 7.17.

Table A.7.1. Number of distinct goods sold by category: 2000-2010

Category	Number	% of total	Rank (1=most; 9=least)
Main crops			
Rice	1	0.68	9
Manioc	1	0.68	9
Bananas	1	0.68	9
Maize	1	0.68	9
(Sub-total)	*(4)*	*(2.72)*	*(9)*
Other farm crops	16	10.88	3
Tree crops	13	8.84	5
Domesticated animals	5	3.40	7
Animal products	5	3.40	7
Artisanal	15	10.20	4
Forest	60	40.82	1
Commercial	21	14.29	2
Unidentified	8	5.44	6
Total	**147**	**100.00**	

Appendix B
Predictors of wage labor: Estimates from hierarchical linear regressions

Besides the straightforward point that Tsimane' men live in households embedded in villages, I can think of another reason for using hierarchical linear regressions to judge how a man's personal traits affect his likelihood of joining the labor force, his earnings from wage labor, and his wage. We noted in the chapter that features of a village like prices, wages, or nearness to employers could impress decisions about wage labor. I control for some of these effects by using binary variables for villages, but the approach is problematic because it assumes that the effect of a man's personal trait on wage labor is the same for all men in all villages. We cannot tell from Tables 7.14 and 7.15 if the effects of a significant predictor like a man's schooling fluctuate between villages. With 54 villages we have enough clusters to assess if the village setting could change the impact of a man's traits on decisions about wage labor.

The analysis moves in five steps, with end results shown in Table B.7.3 of this appendix and Table 7.17 of the chapter.

Step 1. I focus on the plain tale of men encrusted in villages, leaving aside the Whole -- the whole more cumbersome tale of men encrusted in households and households in villages, with individual and household predictors acting upon each other repeatedly during the study. I use Tables 7.14-7.15 as a scaffold to screen what to study further, and what to study further seems to be the spare tale of how some features of a man like his schooling, age, and weight affect his wage labor. The demographic composition of a household did not foretell who joined the workforce, as seen in column (2)-(4) of Table 7.14, and in my view played no part in earnings, so I leave aside households, the middle rung in the hierarchy between individuals and villages.

Step 2. I first assess the intra-class (village) correlations for the three outcomes and find that villages explained 5-8% of the variation in outcomes (Table B.7.1).

Table B.7.1. Conditional and unconditional intra-class (village) correlations (ICC) for three outcomes of wage labor: (i) the propensity to join the labor force, (ii) real wage earnings, and (iii) the implicit daily real wage

	Outcomes:					
	[A] Join labor force (no wage earnings)		[B] Real wage earnings		[C] Implicit daily real wage	
Outcome defined in	Table 7.14		Table 7.15		Table 7.15	
	ICC:					
Type	Unconditional	Conditional	Unconditional	Conditional	Unconditional	Conditional
Value	0.07	0.08	0.06	0.07	0.07	0.05
	Sample:					
Observations	2665		884		902	
Number of groups	54		45		46	
Observations/group:						
Minimum	2		1		1	
Average	49.4		19.6		19.6	
Maximum	281		101		101	
Covariates in conditional ICC	Not applicable	Age, schooling, height, weight, musculature, girls, boys, women, men, survey year, TAPS[1], village wage without lunch	Not applicable	Age, schooling, height, weight, musculature, survey year, TAPS[1], village wage without lunch	Not applicable	Age, schooling, height, weight, musculature, survey year, TAPS[1]

Note:
[1]TAPS=Tsimane' Amazonian Panel Study; the variable took the value of 1 for villages in the TAPS study and zero for villages in the baseline of the randomized-controlled trial.

Step 3. I assess if age, body weight, and schooling -- the three significant predictors of the outcomes in Tables 7.14-7.15 -- varied randomly between villages (Table B.7.2). I tested for four covariance structures for the random-effect regressions and chose unstructured covariances because the produced among the lowest values for the Akaike's Information Criteria (AIC). I used a likelihood ratio test to compare two regressions: (a) one in which all effects are fixed between villages and (b) one in which one of the three significant predictors varies randomly between villages. The covariates for the regressions of Table B.7.2 are the same as the covariates for the conditional regressions of Table B.7.1

Table B.7.2. AIC values and results of likelihood ratio (LR) test comparing models in which a significant personal predictor of the outcome varies randomly between villages

	Outcome:									
	[A] Join labor force (no wage earnings)				[B] Real wage earnings			[C] Implicit daily real wage		
			LR[3]							
Predictor	Covariance[1]	AIC[2]	Li[4]	Lo[4]	Covariance[1]	AIC[2]	LR[3]	Covariance[1]	AIC[2]	LR[3]
Age	Unstructured	3452.12	0.261	0.200	Not applicable since age was not a significant predictor					
Maximum school grade	Unstructured	3445.40	0.009	0.0005	Unstructured	2021.14	0.004	Unstructured	1010.41	0.008
Body weight in kilograms	Unstructured	3444.11	0.004	0.003	Unstructured	2029.48	0.263[5]	Unstructured	1017.48	0.286[5]

Notes:
[1]I tested four covariance structures for random effects (*sensu* Stata 15): independent, exchangeable, identity, and unstructured.
[2]AIC=Akaike Information Criteria. The values shown are for the variance structure with the lowest or one of the lowest AIC.
[3]LR=Likelihood ratio test, p>chi squared. Regressions for computing likelihood ratio tests are mixed linear regressions with restricted maximum likelihood, but for estimates of joining the labor force (column [A]) I also used a logistic regression, both as a check on the results of the linear regression and because the outcome is binary.
[4]Li=Linear mixed regression; Lo= mixed-effect with logistic regression.
[5] The AIC values for the regressions with unstructured and identity covariance were almost the same. For column [B], the regression with unstructured covariance had an AIC of 2029.4 and the regression with an identity covariance had an AIC of 2028.9. For column [C], the regressions with an unstructured covariance and with an identity covariance had almost identical AIC values: 1017.3 for identity covariance and 1017.4 for unstructured covariance. All the values for the likelihood ratio tests in Table B.7.2 come from regressions with unstructured covariance.

Step 4. Based on the results from Table B.7.2, I used mixed linear regressions allowing the effects of schooling to vary randomly between villages for each of the three outcomes, and for the effects of body weight to vary randomly between villages in determining who was less likely to join the workforce. Table B.7.3 contains those results.

Step 5. Once I allow for the effect of schooling or body weight to vary randomly between villages I find that the fixed effect of body weight is no longer a significant predictor of having no wage earnings (column (2), Table B.7.3) (body weight) and the fixed effect of

schooling is no longer a significant predictor of wages (column (4), Table B.7.3). This leaves me with two regressions, shown in columns (1) and (3), in which the fixed effect and the random effect of the predictor are both significant in each regression. The fixed effect and the random effect of schooling both predict well who fails to join the work force (column 1) and the amount of earnings (column 3). I use the results of these two regressions to assess the total effect -- fixed and random -- of schooling on the likelihood that a man does no join the labor force and on his earnings. Table 7.16 in the main body of the chapter distills those results from the values of Table B.7.3.

Table B.7.3. Effect of predictors of wage labor allowing the effects of education and body weight to vary randomly between villages

Explanatory variables:	Join labor force (no wage earnings)		Earnings	Wage
	(1)	(2)	(3)	(4)
Age in years	0.002***	0.002***	-0.001	0.002
	(0.001)	(0.007)	(0.002)	(0.001)
Maximum school grade	-0.022***	-0.022***	0.047***	0.011
	(0.005)	(0.003)	(0.012)	(0.008)
Anthropometrics:				
Standing height (cm)	0.000	0.001	-0.013**	-0.001
	(0.002)	(0.002)	(0.006)	(0.003)
Body weight (kg)	-0.004**	-0.003	0.012***	0.007***
	(0.002)	(0.002)	(0.005)	(0.003)
Mid-arm muscle area (cm^2)	-0.001	-0.001	0.003	0.000
	(0.001)	(0.001)	(0.004)	(0.002)
Demography-Household composition:				
# of women age≥16y in household in annual survey	0.020	0.020		
	(0.013)	(0.013)		
# of men age16≥y in household in annual survey	0.019	0.018		
	(0.012)	(0.012)		
# of girls age<16y in household in annual survey	0.002	0.003		
	(0.006)	(0.006)		
# of boys age<16y in household in annual survey	-0.007	-0.006		
	(0.005)	(0.005)		
Daily village wage:				
Daily village wage, no lunch (*bolivianos*)	0.001	0.001	0.003	
	(0.001)	(0.001)	(0.002)	
Other:				
Survey year (2002-2010)	0.020***	0.020***	0.077***	0.036***
	(0.004)	(0.004)	(0.012)	(0.006)
TAPS	-0.002	0.003	0.376***	0.239***
	(0.052)	(0.052)	(0.104)	(0.055)
Constant	-40.437***	-39.299***	-148.48***	-68.94***
	(8.453)	(8.452)	(24.500)	(12.088)
Observations	2,665	2,665	884	902
Number of groups	54	54	45	46

Table B.7.3. Effect of predictors of wage labor allowing the effects of education and body weight to vary randomly between villages - continued

Explanatory variables:	Join labor force (no wage earnings)		Earnings	Wage
	(1)	(2)	(3)	(4)
Variable with random effects:	Education	Weight	Education	Education
Random-effect parameters:				
Variance for variable with random effects	0.0002	0.00004	0.001	0.001
	(0.0001)	(0.00002)	(0.001)	(0.0007)
Variance of constant	0.027	0.255	0.099	0.027
	(0.01)	(0.132)	(0.48)	(0.019)
Covariance (parameter with RE, constant)	-0.001	-0.003	-0.01	-0.004
	(0.001)	(0.001)	(0.006)	(0.003)
Variance of residual	0.196	0.196	0.492	0.153
	(0.005)	(0.005)	(0.024)	(0.007)
Likelihood ratio test (p>chi squared)	0.009	0.004	0.004	0.008

Notes. Regressions are restricted maximum likelihood with unstructured covariances. Standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1. TAPS=Tsimane' Amazonian Panel Study; TAPS=1 if the village was part of the longitudinal study along the river Maniqui, and TAPS=0 if the village was part of the baseline of the randomized-controlled trial. Outcome variable for columns (1)-(2) = 1 if a man had no earnings from wage labor during the seven days before the interview, and zero if he had any. Outcomes for columns (3)-(4) are the natural logarithm of inflation-adjusted (real) *bolivianos* earned by men who had some wage earnings (>0) (column 3) or the natural logarithm of the implicit daily real wage (column 4), again only for men who had worked for wages during the seven days before the interview. Implicit daily wage=natural logarithm of real earnings during the seven days before the interview divided by the total number of days worked during the seven days before the interview. Likelihood ratio tests assesses if the effects of body weight or schooling varied randomly between villages; see also Table B.7.2.

Appendix C
Local and official exchange rates (*bolivianos* per USA dollar)

Table C.7.1 shows the local and the official exchange rate for Bolivia, and the World Bank line for extreme poverty for Bolivia. By local exchange rate we mean the unofficial exchange rate in the towns of Yucumo and San Borja. Information on the local exchange rate was collected during the yearly surveys from one or more stores in these towns; these stores were the ones that also provided information on prices of commercial goods. Data on Bolivia's official exchange rate comes from the web site of Bolivia's Central Bank.

Table C.7.1. Local and official exchange rate in Bolivia (*bolivianos* per USA dollar) and World Bank line for extreme poverty

Year	Exchange rate			Poverty (PPP) Per person/day (2005)
	Local	Official		
		Sell	Buy	
	[1]	[2]	[3]	[4]
2002	7.29	7.18	7.16	1.00
2003	7.60	7.67	7.65	1.00
2004	7.92	7.95	7.93	1.00
2005	7.98	8.09	8.05	1.25
2006	7.91	8.06	7.96	1.25
2007	7.57	7.90	7.80	1.25
2008	7.04	7.29	7.19	1.25
2009	7.00	7.07	6.97	1.25
2010	6.96	7.07	6.97	1.25
Mean	7.47	7.58	7.52	
Median	7.57	7.67	7.65	
SD	0.42	0.43	0.44	

Notes on sources:

[1] Exchange rate in retail shops in the towns of San Borja and Yucumo. Data collected during yearly surveys.
[2]-[3] The yearly average official exchange rate from Bolivia's Central Bank. Information downloaded on November 3 2018 from:
https://www.bcb.gob.bo/tiposDeCambioHistorico/index.php
[4] Word Bank Extreme poverty line downloaded on November 3 2018 from:
https://datahelpdesk.worldbank.org/knowledgebase/articles/193310-how-is-the-global-poverty-line-derived-how-is-it. Currency is Purchasing Power Parity (PPP) in 2005.

Appendix D
Guide to tables and figures for Chapter 7

Introduction: Some of the tables in this chapter were manually created from the Stata do files; the do file produces the statistic, but I extract and paste selected values in many of the tables. Thus, to reconstruct some tables one executes an entire do file, making year-by-year stops to extract the data. I tried to clarify in the do file what numbers I extracted for the tables, and the table number related to the Stata output. For some tables, I copied the raw data from the Word document of this chapter into an Excel file; I did this to compute some summary growth rates. For example, I copy Table 7.3 from this chapter into Excel file Employers_2000_2010 to produce the growth rates of Table 7.4. When this is done, I indicate the table in Word from which the Excel spreadsheet was constructed.

Table	***Figure***	***Discussion in end notes or text***	***1st line: Folder 2nd line: Name of Stata do file or Excel file***	***Comment; in most cases search for table or figure # in Stata do file or in Excel file***
		% of people earning cash in last 2 weeks who were age<16 in text & end notes	Work➔Employers DoWork_Employer_V7	Search for "% of sample in TAPS & RCT"
7.1		# of people surveyed in 2000 study	Work➔Employers DoWork_Employer_V7	Search for "To find out # of hh/village surveyed in 2000 study"
7.1			Work➔Employers DoWork_Employer_V7	Work: Search for table numbers
			Sales➔Items_Sold Items_Sold_V17	Sales: Search for "assessing # of households"
	7.1			Figure created in Excel within the Word document; click inside figure. #s come from different tables, as explained in text
7.2				Computed manually from the ACCESS data set; see Chapter 4 for the availability of this data set.
7.3			Work➔Employers DoWork_Employer_V7	Search for Table 7.3. Grand means computed in Excel file, Employers_2000_2010. See next row
7.4			Drafts➔Figures_Graphs➔Employers Employers_2000_2010	Excel file: Employers_2000_2010
7.5			Work➔Employers DoWork_Employer_V7	Search for Table 7.5
7.6			Work➔Days_Worked_Earnings DoDays_Worked_Earnings_V12	Search for Table 7.6
		Daily earnings of USA$4.75-5.87/worker	Drafts➔Figures_Graphs➔ Days_Worked_Earnings_V3	In Excel file Excel file: Days_Worked_Earnings_V3 search for "Real daily earnings"
7.7				Excel file: Days_Worked_Earnings_V3. Table 7.7 copied into Excel; growth rates computed in Excel
7.8			Work➔Do_Relative_Prices Do_Relative_Prices	Search for Table 7.8
7.9			Work➔Days_Worked_Earnings DoDays_Worked_Earnings_V12	Search for Table 7.9. Grand means computed in Excel, Days_Worked_Earnings_V3 (search for Table 7.9)
	7.2			Excel graph created in Word document with values from Table 7.6
7.10			Work➔Wages Village_wages_V4	Scroll through do file: statistics generated year by year. CV in Excel: Daily_Wages_Earnings_V2; #s from Table 7.10
	7.3		Drafts➔Figures_Graphs Daily_Wages_Earnings_V2	Excel file: Daily_Wage_Earnings_V2; the #s come from Table 7.10
	7.4a-7.4d		Work➔Wages Village_wages_V4	Search for "Fig. 7.4*" where *=letter, a-d
7.11			Work➔Days_Worked_Earnings DoDays_Worked_Earnings_V12	Search for Table 7.11
7.12				Search for Table 7.12
7.13				#s come from Table 7.12

	7.5a-7.5c		Work➔HLM Do_HLM_class_12_GMLM_binominal _Poisson_ultinomial_ordinal_Chapter_7_v20	Search for Fig. 5.a., b or c
		Correlation of answers between two recall periods		Search for "assessing correlation of days worked"
		% of women who work		Search for "Tabulating the share of women and men in/out of the labor force"
7.14				Search for Table 7.14
7.15				Search for Table 7.15
7.16				Search for Table 7.16 and Appendix B
		Appendix B		Search for Appendix B
7.17			Sales➔Items_Sold Do file: Items_Sold_V17	Year-by-bear search for Table 7.17
7.18				Year-by-bear search for Table 7.17, t test or reg
7.19				Search for "digit heaping" or Table 7.19
7.20			Figures_Graphs➔Sales Excel file Sales_V6 [sheet Annual]	Based on Tables 7.17 and 7.19. Excel file: Sales_V6. Search for Table 7.20
		Share of entities selling or not selling		Under sheet 'Annual' look for "share of sample that …"
	7.6			Based on Table 7.17; see notes to Figure 7.6
	7.7a-b			Based on Table 7.17; see notes to these figures
	7.8			Based on Table 7.17; see notes to Figure 7.8
	7.9			Based on Table 7.18; see notes to Figure 7.9
7.21				Based on Table 7.19; see notes to Table 7.21
	7.10a-b			Based on Tables 7.20-7.21; see notes to these figures
	7.11			Based on Table 7.19; see notes to Figure 7.11
7.22			Sales➔Items_Sold Items_Sold_V15_Quarterly_2002-2003 Figures_Graphs➔Sales Excel file Sales_V6 [sheet Annual]	Search for Table 7.22. Summary statistics described for the quarterly sample can be replicated using Excel file "Sales_V6_[sheet Quarterly_Data]
	12a-b		Figures_Graphs➔Sales Excel file Sales_V6 [sheet Quarterly_Data]	Search for Figures 12a-12b
	13a-c		Sales➔HLM Do_HLM_class_12_GMLM_binominal _Poisson_ultinomial_ordinal_Chapter_7_v7	Search for Fig. 7.13a, b, or c or Fig. 7.14
	14			
7.23				Search for Table 7.23
7.24				Search for Table 7.24
	15a-15c			Search for Figures 15a-15c
7.25				Search for Table 7.25
7.26			Sales➔HLM Do_HLM_class_12_GMLM_binominal _Poisson_ultinomial_ordinal_Chapter_7_v8	Search for Table 7.26
		Correlation between official and local domestic currency exchange rate		Look for: reg ln_off_xrate_real ln_ler_real if x==1
		Daily earnings in nominal USA dollars, 2002-2010	TAPS clean data: TAPS_2002-2010_July_13_2016.dta.	g daily_income_wages=(iiLwagetotY234567890/iiLdaywY234567890)/7.47 sum daily_income_age, d g daily_income_sales=(iisSaletotY234567890/14)/7.47 sum daily_income_sales,d

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[i] Beginning in 2002, the Bolivian government rolled out three programs, most available to any Bolivian citizen. In 2002, the government started an old-age pension (Bono Sol) for any person 65 years of age or older. The program was renamed Renta Dignidad in 2008 and the minimum age lowered to 60 years. In 2006 the government began a cash transfer program known as Bono Juancito Pinto for children in public primary schools who attended at least 80% of school days in a year (Bauchet et al., 2018). And in 2009 the government started a program known as Bono Juana Azurduy for pregnant mothers and mothers with children under two years of age. The government restricts Bono Juana Azurduy to mothers and children without health insurance who meet program requirement, such showing up for pre-natal checkups. At the time of this writing (May 2018), the government transferred yearly USA$ 340 to a pensioner (Renta Dignidad), USA$28 to a school-age child who missed less than 20% of school days (Bono Juancito Pinto), and about USA$260 to a woman who fulfilled the requirements of the program Bono Juana Azurduy for herself (if pregnant) and her child.

[ii] I use the word retail to cover a sale to a final consumer or to a trader because out data does not allow us to distinguish between the two types of buyers.

[iii] The table below shows summary statistics for people I excluded from the clean data sets for most of the analysis of this chapter. I dropped these people because they were under 16 years of age. The two data sets include TAPS (nine-year yearly longitudinal study of individuals, 2002-2010) and the baseline (2008) survey of the randomized-controlled trial (RCT)

	Data set:	
	TAPS	Baseline (2008) of RCT
N of people excluded/a/	65	2
Excluded people as % of total in the data set	2.0%	0.4%
Age of excluded people		
Mean	13.6	10
Median	14.0	10
SD	2.1	0
Minimum	7	10
Maximum	15	10
Cash from sales and wages in *bolivianos*		
Mean	147.0	86.0
Median	75.0	86.0
SD	163.6	36.7
Minimum	10	60
Maximum	725	112

/a/ People dropped in an early year because they did not meet the age cut off would still be included in later surveys after they turned 16 years of age.

[iv] I mention telescoping bias and omission bias in the same sentence because the two biases overlap. For example, if people say they earned more income in the past seven days than in the past 8-14 days, one cannot tell whether this comes from forgetting what happened farther back in

time (omission), or whether this comes from people mentally shifting income earned in the past 8-14 days into the previous seven days (forward telescoping bias).

[v] I screened information for gross mistakes, such as people reporting they had worked for more than seven or 14 days during the past seven or 8-14 days before the interview.

[vi] Standard deviation [SD] = 2.7 households/village for wage earnings and 2.4 households/village for sales.

[vii] For the years 2002 and 2003, the longitudinal study of individuals had one more household in the module on wage labor than in the module on sales. The sales module of 2010 had two fewer people than the module on wage labor. The randomized-controlled trial had one extra person and two extra households in the module on sales than in the module on wage labor.

[viii] Using information from the last column of Table 7.17 and selected rows, I find that 44.42%-46.47% of the sample sold goods during one or both weeks. I arrive at these estimates by dividing the grand mean or the grand median in the row titled, *# entities that sold*, by the grand mean or grand median in the row titled, *number of unique entities*. I computed the shares of entities selling in both weeks by dividing (a) the yearly grand mean or median of the sample size of *# entities selling both weeks* by (b) the yearly grand mean or median *# of entities that sold*.

[ix] The 2010 ACCESS files called *tlk_Plantas* and *Tlk_Animales* have the most complete list of the animals and plants mentioned by respondents when answering questions in any module (not just on sales). Those files have the name of the plant or animal in Tsimane' and, where possible, in Spanish.

[x] Mentioned in Chapter 4, the non-profit organization was the Centro Boliviano de Investigación y Desarrollo Socio-Integral (CBIDSI).

[xi] One can find pictures of Tsimane' dressed in traditional cotton tunics in the book by Karin Hahn-Hissink and Albert Hahn (1989). The couple took the pictures during the mid-twentieth century. Tsimane' also made clothing from bast fabric known as *corocho* (Lieberman, 2000).

[xii] See Ringhofer (2010, pp. 107-108) for a description of weaving thatch palm for roofing panels. Añez (1992) and Rioja (1992) discuss trade in roofing panels made from thatch palm.

[xiii] Firms still prospect for oil and natural gas in the region (Reyes-García et al., 2014).

[xiv] In the 2002 and 2003 surveys of the longitudinal study, 98.4% of parents with children said it was important for their children to learn Spanish. The statistic comes from tabulating the variable *ihcspanishkidY23* in the clean data set: *TAPS_2002-2010_July_13_2016.dta*. In 2010, the last year of the longitudinal study of individuals, we asked parents the following question: "When your youngest child is an adult, what language will she or he speak at home?" Over a quarter of parents (27.6%) said Spanish. The statistic comes from tabulating the variable *iaspiration2Y0* in the clean data set: *TAPS_2002-2010_July_13_2016.dta*

[xv] 110 = 99 people from November-December 2002 + 11 people from January 2003.

[xvi] In 2002 we did two independent surveys. In one survey we gathered information at the level of the household and in the other survey we gathered information at the level of the individual. With both analyses of trends, I drop the 2002 household-level survey. To compute yearly averages for 2002 and 2003 I use individual-level quarterly surveys from 2002 and 2003.

[xvii] The neologism "self-provisioning" household comes from historian Richard Lyman Bushman (2018).

[xviii] The notes to Table 7.7 contain the source for the consumer price index used.

[xix] The nominal and real coefficient of variation in a year are the same because nominal values in a year are changed into real values using the same constant value from the consumer price index.

[xx] The statistics build on Table 6, transposed into the Excel file Days_Worked_Earnings_V3. See Appendix B, and search for Table 7.6 for exact address.

[xxi] During 2002-2003 we did three quarterly surveys in 2002 and four quarterly surveys in 2003. In deciding how to integrate 2002-2003 quarterly surveys with the rest of the yearly surveys, I chose to use information only from the quarterly surveys of the dry season -- quarters 1-2, May-October -- so that the dates of data collection for 2002 and 2003 would match the dates of data collection of the other yearly surveys. Except for 2002-2003 when surveys took place 3-4 times a year, all other surveys took place once a year, during the dry season.

[xxii] The correlation coefficients for the two recall periods for women and men combined were 0.66 for days worked ($p=0.001$) and 0.62 for earnings ($p=0.001$) ($n=5,394$). The coefficients adjust for multiple comparisons and used the combined sample of women and men. The correlation coefficients for the two recall periods just for men were 0.61 for days worked and 0.60 for earnings, with both results significant at the <1% ($n=2,654$). The results in Table 7.9 support the conclusion that the values from the two recall periods can act as substitutes for each other because they look alike. During 2000-2010, the average number of days worked during the seven days before the interview and during the 8-14 days before the interview were almost identical, and the same was true for earnings. The only difference is that the older recall period had more variation.

[xxiii] In the sample of people over 16 years of age in the longitudinal study ($n=5,394$), in the randomized-controlled trial ($n=1,055$), and in the two studies combined ($n=6,449$) only 2.41% of women were in the workforce in the longitudinal study, 5.26% of women were in the workforce in the baseline of the randomized-controlled trial, and 2.89% of women were in the workforce in the two studies combined.

[xxiv] A decade is a long time in a worker's productive life. For a man laboring from 16 until 65 years of age, a decade represents 20% of a working lifetime.

[xxv] I re-did the analysis of Table 7.14, column [3], with wages adjusted for inflation, and the main results of column [3] remained unchanged.

[xxvi] The yearly increase in weight of 0.29 kilograms comes from an ordinary least squares (OLS regression of body weight used as an outcome against a continuous variable for survey year (2002-2010) used as an explanatory variable; 0.29 is the slope or weight gain per year (p=0.006). The waiting period of 26 years comes from dividing a gain in weight of one standard deviation (7.8 kilograms) by the weight gain in a year (0.29 kilograms). If I control for all the explanatory variables in Table 7.15, the yearly gain in body weight drops to 0.12 kilograms (p=0.16). The percent increase in weight mentioned in the text, 7.02%, come from multiplying the coefficients for the variable weight in Table 7.15 (columns 1-2; coefficient = 0.009) times 7.8 kilograms, a one standard deviation in measured body weight.

[xxvii] Using the numbers in Table 7.17, 3%=30/978

[xxviii] Using information from the last column, I computed two averages. In one I included all the values and in the other I left out the two statistically insignificant results. The first estimate yielded a probability of 12.78 percentage points, the second yielded a probability of 14.72 percentage points.

[xxix] We cannot unravel the two effects because when asking about an item sold, we did not ask about the number of units sold or about the unit price of the item. All we have is the name of the article and the gross cash income from selling the article.

[xxx] These averages come from dividing the values in the last column of Table 7.17: row titled # *entities that sold*/*number of unique entities*. The values are slightly lower than they should be (mean=48.97%; median=47.47%) to add up to 100% to match the share of entities not selling. The discrepancy comes from rounding errors. The percentages mentioned in this and in the neighboring paragraphs that follow can be found in the Excel spreadsheet called “Sale_V6”.

[xxxi] The use of the yearly grand median yielded the same ranking of goods as the use of the yearly grand mean.

[xxxii] Among forest wildlife, thatch palm for roofing and timber likely accounted for most of the sales. I do not provide sub-totals for these goods because I cannot tell from the name of the plants which plants were used for timber and which for something else.

[xxxiii] Appendix C has the local foreign currency exchange rates used to convert real *bolivianos* to USA dollars. For simplicity, I used the mean unofficial exchange rate in the towns of San Borja and Yucumo. During 2002-2010, the foreign currency exchange rate averaged 7.47 *bolivianos* per USA dollar (median=7.57; SD=0.42).

[xxxiv] The summary statistics mentioned for quarters in this and in other paragraphs of this section can be found in Excel file "Sale_V6", sheet "Quarterly_Data"

[xxxv] After dropping the second and third quarters of 2003 from Table 7.22 for the computations that follow, we have the following:

Item	Season[1]		Comments
	Rainy	Dry	
[a]. Gross sales value/ person	0.09	0.12	In 1000s of *bolivianos*. Row 'total sales value of all items sold/# of people who sold'
[b] Gross sales value/ person	89	120	In regular *bolivianos*
[c] # of quarters	2	3	
[d] Sales earnings/seller/quarter	44	40	Row [b]/row[c]

Note:
[1]Rainy: third quarter of 2002 and fourth quarter of 2008. Dry: First and second quarter of 2002 and first quarter of 2003. The computations can be found in Excl file Sales_V6, search for "Sales value/person: Gross" (Figures_Graphs➔Sales➔Sales_V6)

[xxxvi] Elasticity = %Δ in an outcome/1% Δ in an explanatory variable.

[xxxvii] It might be possible to estimate the effect of the currency exchange rate on cash earnings with cross-sectional data. For instance, if one had a cross-sectional sample over a large area with many towns in the sample, in which there was little correlation between the unofficial currency exchange rate of different towns, one could, in principle, assess the effects of the currency exchange rate on cash earnings. In such a setting, we would have spatial variation in the currency exchange rate.

[xxxviii] I am referring to the possibility of using the foreign currency exchange rate as an instrumental variable for cash earnings when estimating the effect of cash earnings on outcomes that have little or nothing to do in a direct way with the country's foreign currency exchange rate.

[xxxix] The local foreign real currency exchange rate tracked the official currency real exchange rate. The elasticity of the official currency exchange rate with respect to the local currency exchange rate was 0.84.

[xl] One cannot polish the estimates of Table 7.26 by adding binary variables for each survey year. This would allow one to disentangle the coincident effects of the foreign currency exchange rate from other happenings in a year upon cash earnings. The limitation issues from flawless collinearity between the foreign currency exchange rate and binary variables for each survey year.