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Overview of the Tsimane' Amazonian Panel Study (TAPS), 2002-2010

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This paper is a product of the Center for Global Development and Sustainability's work on globalization and indigenous communities.

Overview of the Tsimane' Amazonian Panel Study (TAPS), 2002-2010[i]

Summary: The question of how trade and markets impress on people beyond imperial or national borders goes back to ancient Greco-Roman times, but because the question had been answered haphazardly, as a sideline to worthier topics, the Tsimane' Amazonian Panel Study (TAPS) aimed to improve on prior answers. Every year during nine consecutive years (2002-2010) the TAPS team gleaned information from the Tsimane', a native Amazonian society of horticulturalists-foragers in the early stages of incessant exposure to the market economy. The baseline sample (2002) included all 1,357 people living in 13 villages along the Maniqui River, department of Beni, Bolivia. Villages varied in their nearness to market towns. In the annual surveys the TAPS team asked about or measured: *(1)* demography, *(2)* anthropometrics, *(3)* horticultural inputs and outputs, *(4)* uses of natural resources, *(5)* current wealth in physical assets and recent monetary earnings, *(6)* conviviality, *(7)* perceived health, pregnancy, and lactation, and *(8)* substance use. On average, the share of people who had left the panel since the previous year, who refused to participate, or who could not take part in the annual survey owing to poor health or to physical impairments reached 6.25%/year. During 2005 and 2006 we surveyed attriters who had moved to a village outside of the Maniqui basin. The team also did three additional studies in villages beyond the TAPS sample, including one in 2008-2009 in 40 villages which helps to assess the external validity of findings from the panel. Weaknesses of the panel included a surfeit of questions and variables, light re-training of field staff after the initial phase, too many interventions, and no control group. On the positive side, TAPS is one of a few panel studies in an out-of-the-way, small-scale rural society undergoing lifestyle changes from unabating contact with the market economy and the outside world. Soon after cleaning the data, the team made it freely available to the public. Repeated measures from the same people and households make it possible to estimate associations unattainable with data gathered only at one time.

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The Tsimane' Amazonian Panel Study (TAPS) is one of a few panel or longitudinal studies in an out-of-the-way, small-scale rural society undergoing lifestyle changes from unabating contact with the market economy. Every year during nine consecutive years (2002-2010) the TAPS team gleaned information from the Tsimane', a native Amazonian society of horticulturalists-foragers in the department of Beni, Bolivia. The team asked adults an endless stream of questions about an endless stream of topics about themselves, their children, their households, and their villages. Soon after cleaning the data, the team made it freely available to the public. More than 100 international researchers unaffiliated with TAPS have asked for the data and received it to study matters beyond the ones we studied, and many articles by the TAPS team have come out of the panel (Appendix A)[ii]. As the study unfurled, the TAPS team field trained 46 PhD students in methods of data collection[iii]. At least 14 PhD students, 22 MA students, and eight undergraduate students have used TAPS data for their theses. In this chapter I limn the motivation and origins of the study, the topics covered in the annual surveys, sampling, the survey discharge, and the flaws and strengths of the data. I also go over general methods of data gathering, leaving for other chapters a discussion of methods to gather data on specialized topics.

Motivation, definitions, and long panel studies by (or with) anthropologists

Motivation: The roads not taken. The question of how trade and markets impress on people beyond imperial or national borders goes back to ancient Greco-Roman times (Chapter 2), but because the question had been answered haphazardly, as a sideline to worthier topics, the Tsimane' Amazonian Panel Study (TAPS) aimed to improve on the answers. At the outset we faced several paths. When the study started during the mid-1990s, we thought briefly about using a randomized controlled trial, but never designed the trial (Godoy, 2001). An ideal trial mimicking reality would have given jobs to some self-sufficient rural people chosen at random to work in public endeavors, such as building latrines or maintaining roads. If done well, the trial would have allowed one to measure the impact of monetary earnings on the broad assortment of outcomes which great thinkers have said markets change. Another approach would have relied on a natural experiment (Diamond & Robinson, 2011): comparing changes in outcomes between a remote group opened to trade with a nearby remote twin group, but unexposed to trade, with changes measured in each group before and after trade opening. In Chapter 3 I used a natural experimental to understand why and how the Tsimane' got to be who they are, but, as we saw in that chapter, and as others have pointed out (Rosenzweig & Wolpin, 2000), natural experiments have too much noise, so that approach would have fallen short of answering well the question. Last, we could have tried to find so-called instrumental variables, which would have allowed for a sharper estimate of how the market economy affect well-being, but those variables are hard to find, and, even when found, raise as many doubts as they answer (Deaton, 2010).

Stuck, we went back to basics, and agreed to start small by first retrieving ethnographic and quantitative information from a few villages as a bridge to a larger and longer panel, the silent, unspoken ultimate goal. To answer the question, albeit without clinical precision, would require many years to see, feel, and measure trade and markets as they enveloped cloistered people. More than a half a century ago, physical anthropologist and physician Albert Damon (1965) remarked that failure to track the same people over time made it hopeless to unyoke life-cycle effects from cohort effects, to split changes growing out of the normal life cycle of a person from broader changes in society. For instance, when comparing the height of the aged

with the height of children one cannot tell if one is seeing the normal effect of aging on height, whether one is seeing the effects of living conditions when the young and the aged were growing up, or both: aging and the living conditions of each cohort. A panel breaks the impasse.

Definitions: Well-being and panel. By gathering panel data -- repeated measures from the same people, households, and villages -- we wanted to find out why and how trade with the outside world shaped the well-being of people near autarky. We defined well-being capaciously to include monetary income, asset wealth, conviviality, consumption, production, health (both physical and psychological), and human capital, a term encompassing formal schooling or educational attainment, academic skills learned in school (e.g., reading, math), and traditional or folk knowledge of local plants, animals, parasites, or weather (Zorondo-Rodríguez et al., 2016).

To qualify as a panel, the information garnered over time had to meet four criteria. First, the information had to cover many entities, such as individuals, households, or villages. Tracking only one entity would yield a time series, not a panel, and would curb one's ability to generalize from the study. Second, locked to the baseline sample, we had to follow it with loyalty and patience even when nothing changed. We had affairs with other samples, but practiced sample monogamy to produce a useful panel. Third, we had to follow the same steps to glean information, and we had to code answers numerically to make it easier for any person to use the data. Using the same prompts to collect open-ended narratives from many people over many years would have ended in a panel -- of sorts -- but would have yielded an unserviceable panel since users would have had to wade through unwieldy responses to spot trends. Many ethnographic panels that took years of hard toil to hew remain unused for failure to code answers numerically, or to document their data for public use. Fourth, data collection had to take place between well-defined intervals, in our case the dry season, roughly from May until August. Syncopated panels make it irksome to estimate year-to-year changes.

Defined this way, a panel allows users to estimate several types of associations. With a panel one can estimate the standard link between the level of two variables, such as the link between the amount of monetary earnings and good health. But with a panel one can move beyond contemporaneous links between events and assess links between growth rates of variables, or the link between the level of a variable in the past with the level of the variable in the future. For instance, one can estimate the association between the height of children during early childhood and the height of the same children during puberty, or the annual growth rate in height between early childhood and puberty. By allowing one to see how past happenings link with later happenings, panels allow one a chance to get a better handle at cause-and-effect relations, at how some of what one sees and measures today eventuated from the past. For many topics, panels provide adjuvant and new windows of understanding, unreachable with snapshots.

Long panel studies by (or with) anthropologists. Cultural anthropologists have done many praiseworthy long-term ethnographic studies (Firth, 1990; Foster et al., 1979; Kemper & Royce, 2002; Vogt, 1994), but -- with one exception, discussed in the next paragraph – these studies do not qualify *sensu stricto* as panels (Gravlee et al., 2009). To compare TAPS with other studies, in Table 4.1 I summarize quantitative studies headed by anthropologists, or done with anthropologists, lasting at least five years. In their book Long-term field research in social anthropology, Foster et al. (1979, pp. 1-13, 323-348) said that long-term ethnographic studies needed about a decade to dredge up meaningful changes. As the frequency and intensity of

transactions in the market increase, the shortest duration to detect changes will shrink, but a cachet of 5-10 years seems like a sensible lower bound to filter extant studies.

Insert Table 4.1

The Gwembe study among the Tonga of southern Zambia is the only long-term ethnographic study known to me to have also collected repeated quantitative measures from the same people, households, and communities. During 1956-1961, Clark et al. (1995) gathered demographic information from four communities on six occasions. For each of the six periods of data collection, they merged the information from the four communities into one average for each time period, and then estimated demographic trends over time for the mean of the entire sample, turning a panel of four communities into a time-series analysis of one ethnic group.

Besides the Gwembe study I found no other long-term panel study headed and done only by cultural anthropologists, but I found one panel study in which a cultural anthropologist formed part of a multidisciplinary team. In Ecuador, Trostle and his co-workers assessed the consequences of a new road on diarrhea among rural and town dwellers (Eisenberg et al., 2006; Markovitz et al., 2012). Using about 24 settlements varying in town propinquity, researchers gathered information from all study participants about diarrhea, sanitation, education, social relations, occupation, anthropometry, and migration, and canvassed participants weekly during 176 consecutive weeks (February 2004-July 2007). Every 1-2 years during 2003 -2013 they also collected data on household attributes.

Beyond cultural anthropology, I found three panel studies in biological anthropology that assessed the impact of modernization on biological and medical outcomes. During two decades (1966-1986) a team of physicians, epidemiologists, and (mainly biological) anthropologists measured on several occasions a large quilt of biological and medical outcomes among eight ethnic groups in the Solomon Island and Papua New Guinea (Damon, 1974). Although they gleaned biological and medical data in different years, they did not gather social, economic, or cultural data over time. Instead, researchers designed an index of modernization or acculturation to Western societies at the start of the study, with each of the eight ethnic groups assigned a scored and placed along a continuum of modernization. They then estimated the association between the baseline index of modernization and biological and medical outcomes (Friedlaender, 1990, 2009, 1987; Page et al., 1977). When treating their data as a panel of people, they estimated the association between biological or medical outcomes at baseline with biological or medical outcomes from the same people at a later time.

In 2002, Piperata et al. (2011) took anthropometric measures from 204 people in the Brazilian Amazon and gathered socio-economic data about their households (n=49). In 2009 they re-measured the same people. As part of their study, Piperata et al. (2011a) measured dietary changes among 20 adult women. Because of the small sample, they only described dietary changes and, for each of the two years (2002 and 2009), they assessed the link between diet and the amount of manioc cultivation.

The third long-term panel study in biological anthropology comes from the work of Shephard and Rode (1996). Every ten years (1969-1970, 1979-1980, 1989-1990) they measured anthropometric and biological outcomes among Inuit adults in a Canadian community. Additionally, twice a year during 1981-1989 they took anthropometric measures of children and assessed their handgrip strength. Shephard and Rode created a panel of long duration, but they did not use their data as a panel. Rather, they used their data as a batch of stand-alone surveys

gathered at different times to estimate long-term trends in biological and medical outcomes[iv]. One cannot tell if their analysis includes only people interviewed in all surveys, or if it also includes people who joined the study after it started.

Like cultural anthropologists, biological anthropologists have taken part in multidisciplinary panel studies. These studies have dealt with human growth, nutritional changes between generations, food insecurity, and diet changes. None of the studies directly deals with the impacts of trade and markets. Some of the better-known studies have taken place in the Philippines (Adair et al., 2011), Brazil (Béhague, Goncalves, Gigante, & Kirkwood, 2012; Béhague, Goncalves, & Victora, 2008; Piperata et al., 2011a), Ethiopia (Belachew et al., 2013), South Africa (Radin & Cameron, 2012; Richter et al., 2007), and China (Gordon-Larsen et al., 2014).

In one neighboring discipline, economics, we find that researchers have gathered their own panel data or used public panel data from low-income nations (Harpham et al., 2003), such as Ethiopia (Caeyers & Dercon, 2013), India (Badiani & Dercon, 2007; Townsend, 1994), and Thailand (Townsend et al., 2013). Although they do not focus on very remote rural populations or on human-biological outcomes, economists have focused on the impact of market economies on villagers' well-being, such as the ability to cushion consumption from misfortunes (Townsend, 1994).

Origins of the Tsimane' Amazonian Panel Study (TAPS)

TAPS has roots in a panel study among the Tawahka, a Native American society in eastern Honduras. Centered on two nearby villages along the Patuca River varying in their proximity to a small rural town downriver, the study produced a short panel of six consecutive quarters (June 1994 - December1995). Carried out with a vegetation ecologist (Nicholas Brokaw), a zoologist (David Wilkie), and four data-grubbing students, the study dealt with the footprint of market exposure on people's use of natural resources and on the abundance of those resources in the wild. Using methods of data collection from human and plant ecology, such as weigh days, scans, and censuses of forest plants and animals, students recorded all goods people brought into the villages on selected days, and assessed the imprint of extraction on the abundance of resources in the forests surrounding the two villages (Demmer & Overman, 2001)[v]. The study ended in 1995, when the Bolivia study started. Research in Honduras helped us burnish the methods of data collection we would later use in Bolivia, and produced nearly flawless data, but had two shortcomings which we tried to overcome in Bolivia: a small sample of observations and little variation in contact with the market. With only two villages and with a total of 31 households, we could not generalize from the study. Furthermore, the two villages laid 16.5 km apart from each other, and both laid only about 32 km from the nearest market town. Cramped in space, most households ended up having roughly the same amount of engagement with the outside world, so the variable of most interest -- contact with the market -- ended up showing modest variation.

During 1995-1999 we started looking for lowland Native American societies in Bolivia to identify big groups with large variation in contact with the market. The explorations were done with two PhD students in cultural anthropology from the University of Florida, Tomás Huanca and Josh McDaniel, and a Bolivian undergraduate student in cultural anthropology from the Universidad Mayor de San Andrés (La Paz), Mario Alvarado. The University of Florida played a pivot role in the origins of TAPS. One of its founders, William R. Leonard, and the first two

PhDs from the study (Elizabeth Byron and Victoria Reyes-García) who helped built the TAPS cellar, all came from the University of Florida.

In the summer of 1995, with the help of Alvarado, we reconnoitred the department of Beni in search of the ideal group. During the late 1990s, Huanca (1999) spent two years doing solo fieldwork on ethnobotanical knowledge and horticulture among the Tsimane' of the remote Sécure River while McDaniel (2000) did solo ethnographic research among the Chiquitano. As part of the reconnaissance we also surveyed villagers in two other ethnic groups: Yuracaré and Mojeño (Godoy & Cárdenas, 2000; Godoy & Contreras, 2001; Godoy & Jacobson, 1999; Godoy, Kirby, & Wilkie, 2001).

From the search we found that some groups had weak links to the market economy, but also had few people, while other groups had many people, but were too embedded in the market economy. The Tsimane' met our criteria. Although imprecise, estimates of their population during the late 20th century varied between 5,000 and 8,000 adults (Ellis & Aráuz, 1998, p. 1; Ringhofer, 2010, p. 73; Santamaría, 2005, p. 36). Tsimane' also differed in openness to the outside world, from bilingual speakers fluent in Spanish and Tsimane', who were savvy in Western ways and who valued dealing with Westerners, to cocooned monolinguals of Tsimane' dwelling several days away from madding towns. As we saw in Chapter 3, the Tsimane' were not an ethnographic blank slate. European and Latin American anthropologists, missionaries, and travelers had lived with the Tsimane' or sojourned at their lands before we came, and had written about their experience (Chicchón, 1992; Daillant, 1994; R. Ellis, 1996; Hissink, 1955; Nordenskiöld, 1979 [orig. 1924], 2001 [orig 1924]; Pérez Diez, 1983; Riester, 1978).

To begin the intensive phase of research we asked for and received approval from the Tsimane' Council, the governing body for most Tsimane'[vi], and wrote a code of ethics for TAPS (Appendix B). During 18 months between May1999 and November 2000 we started doing ethnographic and quantitative studies in two villages along the Maniqui River that differed in their nearness to the market town of San Borja[vii]: San Antonio and Yaranda (Figure 4.1). In selecting the two villages far apart we tried to capture the footmarks of contact with the market through space, as we had tried -- but failed to do -- in Honduras. Lilian Apaza, a Bolivian undergraduate majoring in biology and specializing in botany at the Universidad Mayor de San Andrés and Elizabeth Byron, a PhD student in cultural anthropology at the University of Florida, lived in the village of San Antonio, about 30 minutes by motorcycle from the town of San Borja. Eddy Pérez, a Bolivian undergraduate majoring in biology specializing in zoology, also at the Universidad Mayor de San Andrés, Victoria Reyes-García, another PhD student in cultural anthropology from the University of Florida, and Vincent Vadez, an agronomist and former post-doctoral student at the University of Florida, worked in the more remote village of Yaranda, about 1-2 days up-river by motorized canoe from the town of San Borja[viii] (Apaza et al., 2003; Byron, 2003; Pérez, 2001; Reyes-Garcia, 2001).

Insert Figure 4.1

Researchers gleaned theses data to assess the effects of contact with the market on animal wildlife (Apaza), anthropometric indicators of nutritional status and perceived health (Byron), local ecological knowledge (Reyes-García), and fishing (Pérez). They spent the first stage of fieldwork (May-August 1999) developing and testing ways of garnering data and ensuring that they all collected and coded data in the same way. The teams met periodically to solve coding and logistical troubles. Some of the methods for gathering data had been used in Honduras, but

needed reshaping for Bolivia. Once the teams agreed on methods and logistics, they collected ethnographic information and five waves of quarterly quantitative data from all households in the villages of San Antonio and Yaranda (September 1999-November 2000). While the panel unfolded, the two teams met in either of the two villages to solve new problems. Building on the lessons learned from the comparative study of the two villages, during July-November 2000 the four researchers aided by undergraduates from Northwestern University and Susan Tanner, then a PhD student in biological anthropology at the University of Michigan, did a one-time survey of 58 Tsimane' villages to assess if findings from the study of the two villages reappeared in a larger sample of villages (Foster et al., 2005; Reyes-Garcia et al., 2003).

As the comparative study of the two villages took off, Vadez started to assess the feasibility of introducing new cover crops into Tsimane' horticulture (Vadez et al., 2004). Tsimane' struggle every year cutting and clearing forests to till the soil, so a cover crop that replenished soil nutrients would lower the need for annual forest clearing and free up time for other work or for leisure. Building on our preliminary work, and on the finding that the growing of pigeon peas (*Cajanus cajan L.*) would fit nicely in a vacant window of time in the Tsimane' horticultural cycle, the idea arose that the likelihood of adopting pigeon peas would increase if -- at the same time as we acquainted the Tsimane' with pigeon peas – we also empowered them. To test the idea, we designed a randomized controlled trial which we carried out in 36 villages. The treatment consisted of giving villages selected at random the following: *(i)* pigeon peas seeds, *(ii)* workshops on agriculture, *(iii)* workshops on cultural and economic empowerment, and *(iv)* training in the growing of pigeon peas. Workshops on empowerment centered on hygiene, nutrition, math for ordinary transactions in the market, and the uses of time when deciding between foraging and horticulture. After a baseline survey we chose at random 18 villages to receive the treatment, and we used another 18 villages as controls. Control villages received pigeon peas plus a short verbal introduction on how to grow pigeon peas, but they did not receive training in the uses of pigeon peas. To avoid begrudgement among villagers excluded from the treatment, we gave all the workshops to people in the control villages, but after the study ended. The trial allowed us to deepen our grasp of the Tsimane' way of life, and build trust with villagers who would later participate in the panel study.

Like other panel studies, TAPS did not start out as one[ix]. Rather, the idea jelled piecemeal during the early years of work as we became aware that the footprint of exposure to trade and markets on native Amazonians would be clearer if we tracked over time people scattered in space. The five-quarter panel study in the villages of San Antonio and Yaranda had groomed us for what we needed to fashion a longer panel. Scaffolding on our previous work, and taking into account concerns of safety and costs, we picked 13 villages along the Maniqui River for the panel study. The villages extended from Campo Bello, downriver, near the market town of San Borja, up to the village of Yaranda. The 13 villages had been part of both the study with 58 villages to test the universality of the comparative study of the two villages and of the experiment with pigeon peas. When the panel started we had a sturdy ethnographic understanding of Tsimane' culture, and quantitative information on the people and households in the 13 villages.

We financed the panel from several stand-alone research projects which happened at different times, each with a different end, and most funded by the Program of Cultural Anthropology of the National Science Foundation (USA). We obtained funding for specific projects, such as one that examined the links between local plant knowledge and health, or another one that examined the association between contact with the market and psychological

well-being. These studies built on each other, and drew on how we had defined and measured variables in earlier studies, but since each study also covered new topics, later studies needed additional questions or protocols to collect new information. The assortment of studies tied to different grants explains why the final panel contains a core set of variables that we measured annually (e.g., earnings) because we needed them for any of the stand-alone research projects and for monitoring the effects of trade and markets on well-being, but it also explains the glut of other variables tied to different studies.

Topics covered

Every year surveyors used the same methods to gather information about the following topics: *(1)* demography, *(2)* anthropometrics, *(3)* horticultural inputs and outputs, *(4)* uses of natural resources, *(5)* current wealth in physical assets and recent monetary earnings, *(6)* conviviality, *(7)* perceived health, pregnancy, and lactation, and *(8)* substance use. In Tables 4.2-4.3 I list the topics covered either in most annual surveys (Table 4.2) or in only some of the annual surveys (Table 4.3).

Insert Tables 4.2-4.3

Although we used the same methods every year to retrieve information, we used appurtenant questions to capture changes in socio-economic conditions, to improve the accuracy of measures, or to study new topics. For instance, at the outset of the study few Tsimane' received monetary income from the government, so we restricted questions about monetary income to earnings from wage labor and from the sale of crops. Toward the end of the study, the government of Bolivia started paying pregnant women for pre-natal visits and families for primary school attendance of children, so we added questions to capture these new sources of monetary income, but kept the original questions about own earnings.

With the panel study underway, we did three randomized controlled trials with additional villages (Table 4.4). Two of the three of the trials included TAPS villages, and all relied on lessons learned from the panel and on our ethnographic knowledge of the Tsimane'. I next turn briefly to the randomized controlled trial of 2008-2009 in 40 villages outsides of the TAPS sample because I will use it often to assess if findings from TAPS extend beyond the villages of the panel.

Insert Table 4.4

The randomized controlled trial of income inequality and health (2008-2009) to assess the external validity of TAPS

Of the three randomized controlled trials, one deserves mention because I will use it to assess if findings from the 13 TAPS villages apply beyond these villages. During 2008-2009, Jere R. Behrman, an economist (University of Pennsylvania), William R. Leonard, who by then had moved to Northwestern University, and I spearheaded a study financed by the National Institute of Health to find out how village income inequality and individual income might mold individual health. For the trial we culled 40 fresh villages for two experimental treatments (Figure 4.2). In the first treatment we picked at random 13 of the 40 villages and gave each

village 782kg of edible rice as in-kind income. We split equally the 782kg of rice between all the households in the village. For the second treatment we chose at random another 13 villages, and gave the same amount of rice to each village, but all the rice went only to the poorest 20% of the households in the village, with each of the poorest households receiving the same amount of rice. All household in the remaining 14 villages and households in the top 80% of the village income distribution in the second treatment acted as controls, and received 6kg of high-yielding, improved rice seeds. We did the baseline survey during February-May 2008, we gave the rice during October 2008-January 2009, and we did the end-line survey during February-May 2009.

Figure 4.2

Since the treatments changed perceived mental health, anthropometric indicators of nutritional status, work effort, Spanish fluency, and village out-migration (Saidi et al., 2013; Undurraga et al., 2013; Undurraga et al., 2016), one cannot use data after the treatments to assess the universality of findings from the TAPS study, but one can use data before the treatments for such an end. And for some topics, information collected after the treatments is fitting. For instance, the psychological bias of Tsimane' to round ages to multiples of five or ten is unlikely to have changed from the treatments (Chapter 5). So for these two reasons -- validating TAPS findings and enlarging the TAPS sample -- I will use data from the trial, but will often show 2008 and 2009 data for full disclosure, aware that 2008 data is more suitable for some topics.

Survey discharge

To collect data, we put together teams comprised of a senior university-trained Bolivian researcher from the highlands and a Tsimane'. Besides Huanca, the senior Bolivian researchers included Milenca Aguilera, Esther Conde, Robin Mamani, and Juan Pablo Ticona. Of the senior Bolivian researchers, two -- Huanca and Mamani -- had trained in cultural anthropology. Aguilera, Conde, and Ticona knew each other as undergraduate biology majors at the Universidad Mayor de San Andrés. During most surveys, two teams worked in the field, one team headed by Huanca and the other team headed by Conde, the latter often aided by another senior Bolivian researcher. Each team relied on at least one Tsimane' translator whom we had trained in the execution of the survey. There was turnover among the Tsimane' hired as translators, but two translators -- Paulino Pache and Manuel Roca – remained steadfast and helped train new translators throughout the duration of the study. Since the same teams tended to visit the same villages each year, villagers and surveyors got to know each other. To present a united front when dealing with the public, Huanca in 2006 formed a non-government organization headquartered in the town of San Borja. As head of the Centro Boliviano de Investigación Socio-Integral (CBIDSI), Huanca kept Bolivians updated of TAPS's work and oversaw the discharge of the annual surveys.

The teams did the survey during the dry season (May-August). Each year the survey team asked village authorities to list all the households living in the village, even if some or all members a household were absent. Having the list, the team visited each household to find the household heads. As we shall see in Chapter 5, most Tsimane' households are nuclear and had two heads, a wife and a husband[x]. By about 16 years of age, Tsimane' couples set up their own household and are regarded as an independent unit. Most nuclear households live under one roof, eat and sleep together, and jointly work fields, but people own and manage their own physical

assets. We studied only villagers willing to participate in the study. Survey teams remained in a village for 7-10 consecutive days canvassing all households. Interviews were done in people's home and lasted a total of about two hours. If a villager knew Spanish, the senior Bolivian researcher spoke in Spanish with the villager, otherwise a Tsimane' translator stepped in to help. During the 7-10 days in a village, surveyors set aside 1-2 days to take anthropometric measures in the village school. While in a village, the teams tried to find missing participants by re-visiting their homes after the first attempt to interview them, but – except for 2005 and 2006 -- they did not try to find people who were still missing after the second attempt to contact them. During the study we learned that permanent out-migrants from the villages of Cara Cara and Alta Gracia in the panel had moved to the village of Undumo, in a region known as Ixiamas, in the province of Abel Iturralde, department of La Paz. During 2005 and 2006 we went to the village of Undumo to interview these attriter and find out if they differed from the more sedentary sample in the 13 villages along the Maniqui River.

To thank them for their goodwill and time, we gave individuals, households, villages, and the Tsimane' Council goods and services. *Individuals*: During the annual surveys we gave different gifts to the female and to the male head of the household. The composition and monetary value of gifts changed during the study, but the total value amounted to at least a half day's wage. During the last years of the panel study a woman and man received the equivalent of 31 *bolivianos* and 42 *bolivianos* in gifts, a fair reward since a rural unskilled worker in the area at that time got paid a daily wage of 50 *bolivianos* (1 US dollar ≈ 7 *bolivianos*). Children received pencils, erasers, and notebooks, and all participants received cookies and soft drinks after we took their anthropometric measures. *Households*: During the annual surveys we did not requite the entire household, but during 2001-2002 and 2005-2007 we gave households pigeon peas and chicks. *Villages*: Villages received two soccer balls. Other ways of thanking villagers included giving them a map of their village (Reyes-García et al., 2012), a book on local knowledge of local plants (Reyes-García, Nate, & Ista, 2001), and booklets on how to grow pigeon peas (Huanca, Reyes-García, & Vadez, 2002, 2003). *Tsimane' Council*: We did not give donations to the Tsimane' Council every year, but we helped them prepare official documents and funding proposals, and in some years we gave them computers, printers, and maps. Beyond these gestures, we helped the Tsimane' in other ways. Tsimane' came to our office in the town of San Borja to visit, sell goods, or to ask for help after misfortunes struck. Because most of the senior Bolivian researchers lived rather permanently in San Borja and because they knew the Tsimane' in the study, they were the ones Tsimane' first approached when in distress. Typically well-acquainted with the villager asking for help, senior Bolivian researchers could assess the merits of the request and a fair response.

During the study we saw more research and projects by foreigners and Bolivians in the Maniqui basin. The rise strained the gate-keeping ability of the Tsimane' Council to monitor outsiders, and made them establish formal procedures when granting initial permission to work with the Tsimane', or when renewing permissions. Through the procedures the Tsimane' Council hoped to enhance the accountability of people working in their territory and leverage more resources for the Tsimane' Council and the Tsimane'. Churning in the leadership of the Tsimane' Council made it necessary for groups working in the area to periodically communicate the purpose and results of their work to the Tsimane' Council. Over time, the Tsimane' Council has asked for more clarity and resources from foreign researchers before granting them new permits or renewing old permits.

The raw and the clean data

Having removed personal identifiers, we make available data from TAPS and from the randomized controlled trial of 2008-2009 in two forms. First, we provide the clean data in Stata 14. We have cleaned, notated, merged, and appended the data to the best of our abilities, but acknowledge that it might still have mistakes. Second, we provide the raw data in Access 2007. I next explain the value of the raw data. I use the raw data from TAPS as an example, but the points I raise also apply to the data from the 2008-2009 randomized controlled trial.

When making the TAPS panel available to the public we had to decide whether to package the data at the level of the individual, the household, or the village. For each year of the panel we could have averaged data from individuals in households or in villages and presented household-level or village-level averages, but doing so would have erased the richness of the data collected from individuals. Instead, we decided to display all the information for each person for each survey year, but merged with the information about the person's household and village for the year of measurement. Our choice has the advantage of presenting the data in its finest-grained form, allowing users to compute village-level and household-level statistics from information about individuals in the village or in the household. Our decision works for outcomes about a person such as body weight or age. As long as a measure or an answer to a question takes only one value for each person at the time of the survey, using the individual as the unit of observation and analysis is best.

But suppose one asked people to list all their expenditures, all their sources of income, or all their illnesses during the seven days before the day of the interview, as in section [A] of Figure 4.3. Since TAPS remained faithful to the way people answered queries, some people might have had many entries or rows (e.g., persons 1-2 in section [A] of Figure 4.3), but others might have had only one row because they had only one or no episodes to report (e.g., person 3 in section [A]). One cannot include separate columns or variables for each answer in the final annual panel because the total number of columns for these questions would have varied between and within years for different people. To harmonize responses from participants to questions where the unit of measure (e.g., all the various symptoms of illness of a person) differed from the unit or entity of analysis (e.g., the person) so that answers to all responses by a person would fit into one row each survey year, with a fixed number of columns capturing responses to the same questions for all people, we had to make an arbitrary decision. Returning to the example in Figure 4.3, since we could not list all the ailments of a person during the past seven days in a panel with the individual as the entity of observation -- one ailment in each separate column -- we had to create a summary annual measure, such as the “total number of illness symptoms" or the "total number of person-days afflicted by symptoms", which is what we have in the TAPS clean data for public use (section [B] of Figure 4.3). We compressed the raw data spanning many rows for each person into one row per person and a fixed number of columns so that the information could fit into an annual panel with the individual as the building block.

Insert Figure 4.3

The solution comes at a price. One has pigeonholed all answers to a question into a summary measure that allows analysts to use the new variable in the annual individual panel with the individual as the unit of analysis. But users who want to track the incidence of, say, self-reported respiratory illness, or who want to identify the particular goods people acquire in

barter or market transactions, or the monetary expenditures in particular foods (Rosinger et al., 2013), will be stymied with the clean data since they will see a summary measure that skips over the specifics.

These finer-grained distinctions – the specifics that make up the average -- matter because many of the debates in anthropology, history, public health, and development center on the particulars of how people and cultures change as trade and markets punctured them. For instance, in the ethnohistory of colonial North America one debate has centered on whether when trading with Europeans, Native Americans wanted beads of particular colors and shape because those beads fitted best with their traditional sense of aesthetic, or whether -- pragmatists that they were -- Native Americans simply wanted metal tools to lessen toil (Axtell, 2000; Carlos & Lewis, 2010; Krech, 1999). In medical anthropology and in public health, researchers want to know the specific ailments that matter most as people become exposed to the market economy, gain more years of schooling, change their work habits, or earn more cash (Bago D'uva et al., 2008; Sen, 2002; Subramanian et al., 2009). In cultural anthropology, researchers want to know how different aspects of traditional knowledge change in response to more schooling, fluency in the national language, or wage labor (Reyes-Garcia et al., 2013). One cannot use the clean data from TAPS to answer these nuanced queries, but must turn to the raw data.

Because I move amphibiously throughout the book between *(1)* the clean TAPS data, *(2)* the raw TAPS data, and *(3)* the raw or clean data from studies done beyond the 13 villages of the panel study, I make it clear which data I am using so others can replicate or go beyond the analysis I provide. Each chapter has a guide showing how I constructed tables and figures in the chapter (Appendix D).

Composition of TAPS sample of participants: Overview

I next describe the TAPS sample of people, leaving for Chapter 5 the description of the sample of villages and households. In Table 4.5 I partition the annual sample of people into categories, such as people who entered or left the panel, or people who changed household membership within the panel. Because some categories had few people, I use median values and growth rates to describe the sample. To make it easier to link the text with the tables, throughout the book I color in red those numbers in the tables discussed in the text.

Insert Table 4.5

The panel has some oddities and shortcomings that deserve highlighting before describing the panel. For example, we find no deaths in 2004, a sharp drop in the number of refusals from an annual median of eight people during 2002-2006 to two people during 2007-2010, and no temporary attrition or impaired people during 2008-2009. Second, we were not consistent in the way we gathered some of the information. Information for the years 2004-2010 came from annual surveys during the dry season, but information for 2002-2003 came from a five-quarter panel study bracketing two years, with one slice of information from the five-quarter panel placed under the year 2002 and another slice placed under the year 2003. As part of the five-quarter panel, we surveyed people three times in 2002 and two times in 2003. The use of the five-quarter panel to build the information that would eventually form part of the self-contained data for the years 2002 and for 2003 affects the attriter count in each of those years. During a regular annual surveys, an attriter was someone who was not present during the survey,

or someone who could not answer survey questions owing to illness or to a disability, or someone who, though able to take part in the survey, turned down the request for an interview. In 2002 and 2003 a person had more chances of being counted as an attriter since in each of those years surveyors visited the person more than once. This is why in the first year of the panel (2002) we already find attriters and people in other categories who technically should not have shown up until 2003, such as people who had changed their household membership since the last survey.

The baseline sample of 1,357 people included all those living in the 13 TAPS villages at the start of the panel in 2002[xi]. As noted earlier (p.on page 6), estimates of the adult Tsimane' population in the late 20th century varied between 5,000 and 8,000 adults. The baseline sample of 1,357 therefore captured 7-11% of the adult Tsimane' population[xii]. Most people in the sample self-identified as Tsimane'. During 2002, only 0.92% of the sample belonged to another ethnic group, and by 2004 the share had barely moved, reaching only 1.04%[xiii]. Although the sample consisted of a congeries of 13 villages, during 2005 and 2006 the sample grew to 14 villages because in those two years we surveyed people who had migrated to the village of Undumo, outside of the Maniqui basin.

The median gross total number of people we intended to interview each year reached 1,661 and rose by an annual rate of 0.99%. The gross total captures the sample size of people we hoped to survey before adjusting for attriters or people who had left since the previous annual survey. Under attriters we included people who had died since the last survey, people who had migrated temporarily or permanently out of the villages since the last survey, or people who were present when surveyors arrived, but who did not want to be interviewed. On average, at the time of an annual survey 6.00% of the sample from the previous year had attrited. The median net annual sample -- gross minus attrition -- reached 1,488 people and grew by an annual rate of 0.51%. On average, only 0.52% of an annual sample refused to take part in the study, and only 0.17% of the annual sample could not participate owing to old age, disability, or to illness.

We never formally asked villagers why they did not want to take part in the survey. Some studies suggest that participants in panel studies get tired from having to answer the same questions time after time, and that repeated exposure to surveyors shapes expectations and answers (Halpern-Manner & Warren, 2012; Warrren & Halpern-Manners, 2012; Zwane et al., 2011). Fatigue might explain some of the refusals in the sample. In addition, anecdotal evidence suggests that over the years some Tsimane' wanted gifts of greater value in exchange for taking part in the study[xiv]. Irrespective of the reasons, refusal rates were negligible.

Measure of individual, household, and village variables that changed over time

The clean panel for public use has a total of 830 variables about individuals (e.g., body weight), 218 variables about households (e.g., area of forest cleared), and 163 variables about villages (e.g., village-to-town travel time). Some variables (e.g., sex) were measured only once because they did not change and many variables were not measured each year because TAPS researchers only needed them in some years (Table 4.3).

We addressed most survey questions to adults, defined as people ≥ 16 years of age, or younger than 16 years if they headed a household. We chose 16 years of age as a cut-off because Tsimane' form new households at about that age (Chapter 5). Some questions about health required that we collect information from people less than 16 years of age. In such cases, we addressed questions to the dependent's caretaker. We directed questions about household food

consumption to the female head of the household, and we directed questions about horticulture to the male head of the household; if the target subject was absent, we asked the other household head. Questions about the village were directed at a village authority. To give an overview of the data, I next discuss variables about individuals, households, and villages measured during all or most years.

Individuals

Credit, monetary income, and barter (adults). When asking about the amount of credit received or given out, the amount of cash earned, or the monetary value of goods in barter transactions we instructed adults to tell us separately the amount for the past seven days and the amount for the past 8-14 days. Breaking up the recall period into two discrete slots of time allows one to test for recall bias. In particular, one can test for telescoping bias -- the tendency to bring into the more recent period events that happened farther back in time (forward telescoping bias) or the tendency to push farther back in time events that happened closer to the interview date (backward telescoping bias). Questions about monetary income centered on earnings from wage labor and on earnings from the sale of farm crops and feral forest goods. Questions about barter centered on the items and value of goods received, and questions about credit referred to the amount of money borrowed from any source or the amount of money lent to others. For these variables the clean data provides summary measures, such as total earnings for the past seven or 8-14 days; users interested in the goods sold or in the type of wage labor done to earn monetary income should turn to the raw data.

Wealth in physical assets (adults and households). At the outset of the study we identified a set of physical assets that captured the spectrum of physical assets owned by most households. The set included assets which only wealthier villagers owned (e.g., cattle) down to physical assets which most villagers owned (e.g., woven bags). The assets included industrial goods (e.g., metal knives), goods made from local materials (e.g., canoes), and domesticated animals (e.g., chickens). During the surveys we asked adults to report the number of assets they owned from the set. Because children owned some assets (e.g., chickens), we also collected data on assets for the entire household, but we only asked questions about assets for the entire household after having asked adults about their own assets. Asset data is reported for individuals and for the entire household, in the raw units of the asset (e.g., number of chickens owned by the individual and, separately, by the entire household), but without distinguishing between the size, age, or quality of the asset. We report the monetary value of the asset in *bolivianos*, with village selling prices or town buying prices used to impute values. For example, if a person owned three chickens and each chicken sold in the village for 25 *bolivianos*, then we imputed a value of 75 *bolivianos* for the three chickens owned by the person, without taking into account the age or the condition of the chickens.

Perceived health (all), pregnancy (adult women), and lactation (adult women). Either directly or through a proxy respondent, we asked everyone about illnesses, symptoms of illnesses, and about the number of days confined to bed owing to illness. Again, we asked these questions for the seven days before the day of the interview, and for the 8-14 days before the day of the interview. We also asked about their perceived health at the time of the interview compared with their health a year before the interview. In addition, we asked women whether they were pregnant or lactating, and, if lactating, we asked about the number of months they had been breastfeeding.

Commercial alcohol , cigarettes, and coca leaves (adults). We asked adults how much and how often they had drank commercial alcohol, chewed coca leaves, and smoked commercial cigarettes during the seven days before the day of the interview. We emphasize the word commercial to describe the consumption of these substances to distinguish it from the consumption of alcoholic beverages traditionally made by households from local crops, or from the ritual smoking of tobacco harvested from their own fields.

Conviviality (adults). We measured conviviality by the number of gifts and the amount of labor help given to others during the seven days before the day of the interview. To gauge the social support available to an adult in case of a mishap, we asked them if they would have access to 100 *bolivianos* in an emergency. Traditionally, many native Amazonians including the Tsimane' (Godoy et al., 2010), display conviviality by drinking a potation fermented at home called *chicha*. For this reason, to capture sociability we also asked how many days people had consumed *chicha* in the past seven days.

Anthropometrics (all). Following the standard protocol of Lohman et al. (1988) we measured standing height, body weight, skinfold (triceps, biceps, subscapular, and suprailiac), hip and waist circumference, and body fat. From the raw data we computed and include in the clean data sex-age specific Z score reference values recommended for international comparisons (De Onis et al., 2007; Frisancho, 2008; WHO, 2006). The clean data includes both raw anthropometric measures taken each year, and the Z scores.

Households

Horticulture. We asked the male household head to report the area of old-growth forest and fallow forest cleared for horticulture the previous year, the number of plots cleared, the area planted with plantains and manioc, and the provenience of the rice and maize seeds they had used.

Food consumption. We asked the female household head to tell us how much food the household had consumed during the seven days before the day of the interview. We limited questions about food consumption to the most common foods. The questions centered on commercial foods (e.g., cooking oil, refined sugar), local crops (e.g., maize, manioc), meat and other products from domesticated animals (e.g., eggs), and wildlife (e.g., fish). For each food item, we included the units (e.g., kg), the total quantity consumed by the household, and the monetary value in *bolivianos* of the total quantity of the food item, with village or with town buying prices used to impute monetary values.

Villages

Attributes. Village traits included the number of households and healers in the village, transport costs and travel time to the nearest road or town, and some industrial assets owned by the village. Travel time and costs varied over the years owing to changes in the volume of cars, trucks, and canoes. Industrial goods owned by the village included ham radios and outboard motors for canoes. During the early years of the panel, outboard motors for canoes and ham radios were likely to be owned by the village rather than by individuals, but during the later years of the panel some people bought small outboard motors though none, to my knowledge, every bought a ham radio. Cell telephones have superseded ham radios as a way of communicating between villages or between villages and towns, but only for villages close to telephone towers.

Prices. We asked about the village selling or buying price of crops, foods, and assets. We used village selling prices to calculate the value of wealth in physical assets, and we used the village (or town) buying prices to calculate the value of household food consumption. If a village lacked a price for a good, we imputed the price from nearby villages. If the price was still missing, we imputed the price from a nearby town. These different prices are included in the clean data so users do not have to be bound by our imputation technique.

Naming convention of variables to ease the use of the clean data

We followed four rules when naming variables in the clean data set for public use.

[1] The first letter of a variable shown in lower-case (*i*, *h*, *v*) indicates whether the variable refers to *i*ndividuals, *h*ouseholds, or *v*illages. For instance, we might have collected data from *i*ndividuals about their height, or from *h*ouseholds about the number of forest plots they had cleared for horticulture, or from village leaders about *v*illage prices. The only exception to the rule of variables starting with *i*, *h*, or *v* is the set of variables that start with a majuscule *(V)* instead of the lower-case *v*. These variables capture *v*illage prices, but were created by imputing prices from neighboring villages or towns to villages that lacked prices (p. 16).

[2] The next 2-3 letters after the first letter indicate the topic covered by the variable. For example, a variable beginning with the prefix *hag* shows that information about the variable came from answers by a *h*ousehold head (first letter of the variable); the answer and data about the topic -- *ag*riculture (*ag*) -- refers to the entire household (*h*). In this example, the topic, *ag*riculture, is abbreviated in the second and third letter of the variable.

[3] The next few letters capture sub-topics. For instances, the variable *hagareaforest* shows that we asked a *h*ousehold head to tell us about the *area* of *forest* cleared for *ag*riculture by the entire *h*ousehold.

[4] The suffixes of variables start with upper-case *Y*, which stands for *y*ear, followed by all the last digits for the years in which we measured the variable. For instance, the variable *ianbmiY234567890* shows that we measured the variable *ianbmi* every year from 2002 until 2010, inclusive, whereas the variable *ianlactatemonY23467* shows that we measured the variable in 2002-2004 (inclusive) and 2006-2007 (inclusive), but not during 2005 or during 2008-2010.

The variables that uniquely identify individuals, households, villages, and survey years are: *idssnY234567890* (*i*ndividuals), *hhidY234567890* (*h*ouseholds), *vidY234567890* (*v*illages), and *yearY234567890* (*y*ear). These variables are useful when computing averages for households or village, and when doing statistical analysis[xv].

The naming convention should make it easy for users to identify the entity captured by the variable (e.g., individual, household), the topic covered by the variable, and the years we measured the variable. The variables in Stata contains terse definitions, and some have notes with adscititious remarks, amplifying definitions or alerting users of problems with the variable.

,

Three types of transformed variables

Most of the clean variables have a twin in the raw data, but three types of clean variables stand for raw variables that we changed heavily. The first type includes variables for which we had to work out a summary value, as discussed earlier (Figure 4.3). These variables had to do with events such as illness episodes, goods sold, or types of wage labor. The second type of variables include age-sex anthropometric Z scores. Using raw measures of different body dimensions (e.g., weight) and drawing on reference groups from internationally representative well-nourished populations, we estimate a variety of age-sex anthropometric Z scores, which allow user to assess how Tsimane' compare to international norms. We shall have more to say about anthropometric Z scores in later chapters. The third type of variable has to do with reported age, maximum school grade attained, and birth dates.

We found that some people reported age, education, and birth date erratically over time (Chapter 5). Because people could acquire more education through time, it made sense to ask every year about the highest school grade attained at the time of the interview (particularly for children), but it did not make sense to ask people every year about their age, except for new entrants into the panel. Having once asked about a person's age, we could have computed the person's age in later surveys by using the elapsed time between surveys. We did not do this, and instead asked people every year to report their age and the age of their dependents. The oversight paid off because through repeated answers about perceived age we found that people guessed when reporting their age. We found instability over time not only in reported age, but also in reported educational attainment and in birth date. For example, some people reported growing older by more than two years between two consecutive annual surveys while other people reported becoming younger between two consecutive annual surveys. The same noise happened when people reported the maximum school grade attained or birth date. Some people reported having fewer years of completed education in later surveys and gave inconsistent answers about their birth date. The shakiness in answers to these foundational variables made us transform the raw measures of age, education, and birth date into new variables so that they were consistent across years, aware that consistency does not imply accuracy (Appendix C). We have left the raw measures of age, education, and birth date in the clean data because users might want to use the raw measures to analyze reporting biases, or to correct the raw measures in other ways.

Assessment of TAPS data: Weaknesses and strengths

Weaknesses

First, the panel had a surfeit of questions and variables, too often poorly measured. TAPS would have profited from a sharper focus on fewer matters. *Second*, we took a lax approach to the training of field staff. For instance, after first training field staff to take anthropometric measures and do surveys, TAPS did not have periodic refresher courses for its surveyors or translators. TAPS lacked a metric to ensure surveyors and translators understood

well the tasks before sending them to the field. We never developed or systematically used measures to assess if different surveyors coded answers in the same way. *Third*, TAPS was remiss on attrition. We noted who left the panel, but except for 2005 and 2006, we did not pursue flinty attriters who left the 13 TAPS villages. *Fourth*, our stay most likely changed behavior. For reasons discussed earlier, we offered goods and services to villagers during annual surveys. In addition, some of the randomized controlled trials intentionally transferred goods or services to villages, households, and individuals. Since some of the randomized controlled trials took place shortly before or at baseline, growth rates for outcomes might be biased by our interventions. *Last*, the TAPS panel lacked a control group, so we cannot tell whether changes observed during 2002-2010 only apply to the TAPS sample, or whether the changes also apply to the world beyond our sample.

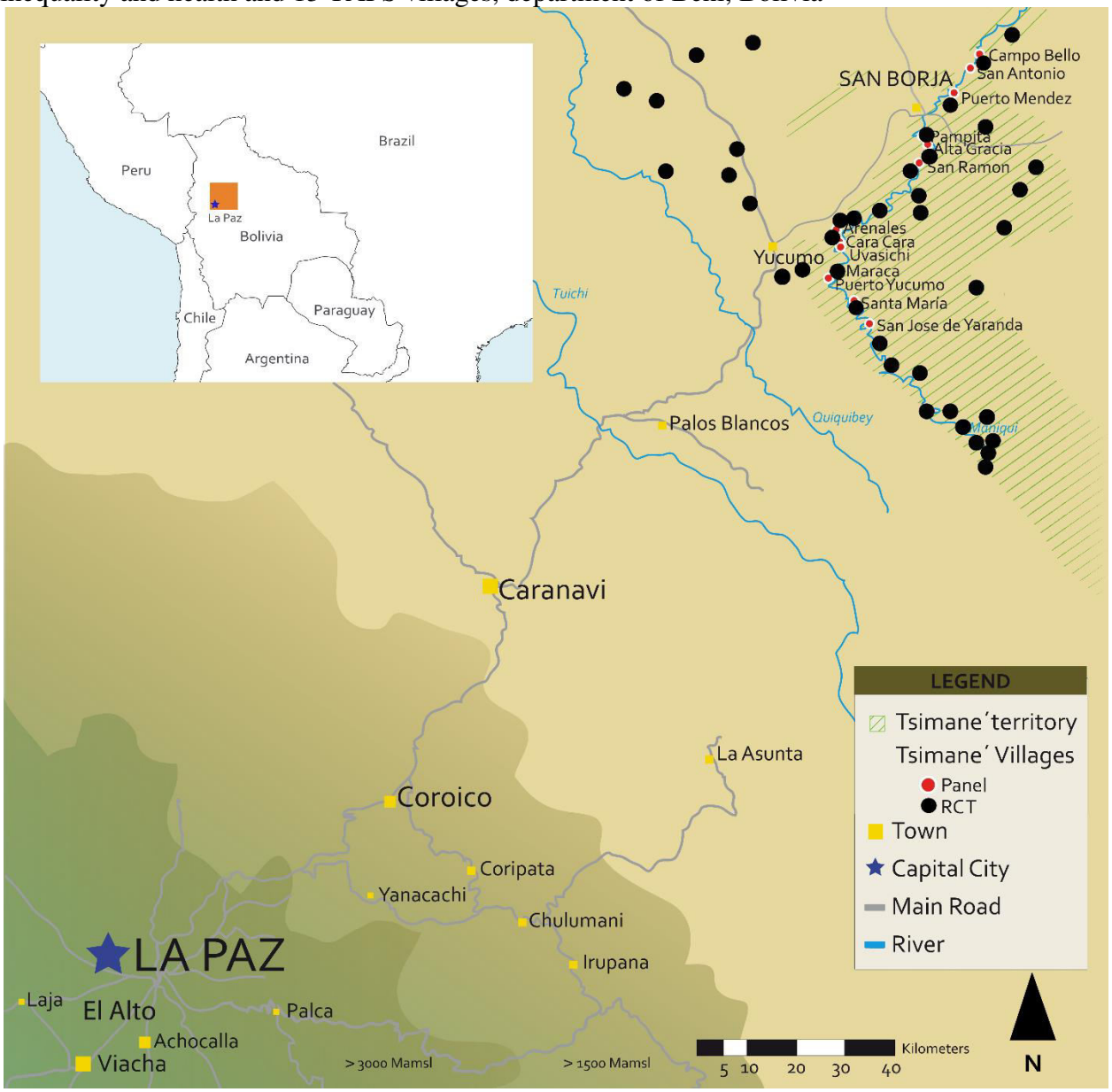
Strengths

First, we made the panel data available to the public as soon as we had cleaned it. After signing a short release form, researchers worldwide have had free access to the data from the outset. Our approach to data contrasts with the approach of other longitudinal studies in anthropology. Data from many longitudinal studies in anthropology can no longer be found, or are so poorly documented as to make them unusable (Foster et al., 1979). *Second*, the panel has social, economic, and anthropometric data that changed over time. The variables allow users to examine the two-way relation between changes in these variables. Our panel allows users to move beyond the common approach in past anthropological panel studies of viewing changes in human biological outcomes as a function of only baseline conditions. *Third*, the study had a footing in ethnographic understanding of Tsimane' culture and changes in Tsimane' society. *Fourth*, data from the 2008-2009 randomized controlled trial allows users to assess if findings from TAPS extend to other Tsimane' villages. *Last*, and most importantly, one can use TAPS data as a nine-year panel to measure changes taking place while the study took place, but one can also use it as one mega baseline with many observations against which to compare outcomes in the future, and thus assess changes beyond the life of the panel. The largest dividends of TAPS will likely accrue to future generations.

Figure 4.1. Map showing 13 TAPS villages, department of Beni, Bolivia

Notes: The shading of the territory denotes elevation (mamsl = meters above sea level). The square symbols and letters in each town are approximately proportional in size to the population of the town. Tsimane' villages include the 13 villages of the annual (2002-2010) Tsimane' Amazonian Panel Study (TAPS). The Tsimane' territory is an administrative division that does not reflect all the lands inhabited by the Tsimane'[xvi]. The unmarked name "El Alto" underneath La Paz refers to a sprawling urban settlement abutting La Paz.

Figure 4.2. Map showing 40 villages of the randomized controlled trial (RCT) of village income inequality and health and 13 TAPS villages, department of Beni, Bolivia



Notes: Same as Figure 4.1.

Figure 4.3. Example of two data sets where the unit of measure or observation differs from the unit of analysis: [A] raw data, [B] clean data for public use. Data captures answers to the question: "What illness symptoms did you experience during the past 7 days?"

[A] Unit of measure: person-symptom

Person	Symptom	# days
1	Fever	2
1	Vomit	3
1	Pain	1
2	Rash	2
2	Cough	1
3	None	0

[B] Unit of analysis: person [as in TAPS data for public use]

Person	Total # symptoms	Total person-days with symptoms
1	3	6
2	2	3
3	0	0

Table 4.1. Long panel studies conceived and executed by anthropologists in remote, small-scale rural societies

Study name & Main reference	*Country*	*Societies*	*Cohort age*	*Years & frequency (t)*	*Baseline sample*	*Attrition*	*Outcome*	*Time-varying covariates*	*Data for public use & (# of requests)*
I	***II***	***III***	***IV***	***V***	***VI***	***VII***	***VIII***	***IX***	***X***
Cultural									
[1] Gwembe	Zambia	Tonga	All	1956/7, 1962/3, 1972/3, 1981/2, 1978/8, 1992 (6)	10000i, 4c	NA	Demography[a]	Survey date[b]	Demography; (0)
[2] Environmental change and diarrheal disease: A natural experiment	Ecuador	Smallholders, town dwellers, & Chachis	All	2003-13; 1 observation/village; 2/18/2004-7/4/07, weekly (~3; 176 weeks)[a]	4196i, 21c	23%[c]	Diarrhea & three marker pathogens	Household network, exposure, hygiene, wealth, demographics[a]	Some[b]; (2)
[3] TAPS	Bolivia	Tsimane'	All	2002-10; annual (9)	1453i, 13c	Table 5 & text	Many bio-social	Wide range	All; (>100)
Biological									
[1] Solomon Islands (SI)	PNG, SI	Nasioi[b], Nagovisi, Aita, Lau, Baegu, Kwaio, Ulawa, Onton Java	0-19 y	1966-72 & 1985-86 (2)	1710	34%	Lipids	Anthropometrics[a]	See note below; (2)
[1a]				1966-72, 1978-80, 1985-86 (3)	294[a2a1]	45%	BMI	Anthropometrics[a]	
[1b]							Blood pressure	Age, muscle, adiposity, height	
[2] Ribeirinhos Longitudinal Study	Brazilian Amazon	Mixed ethnicity river dwellers	0-77	2002 & 2009 (2)	469	56%	Δ in HAZ, WHZ, BMIZ, ZTSF, ZUMA	Δ in cash income, schooling, farming,	See note below; (0)
[2a]			16-59		30	33%	Δ in diet	Manioc cultivation	
[3] IBP-HAP	Canada	Inuit	Adults, 20-69	1969-70, 1979-80, 1989-90 (3)	118i 1c	NA[a]	Many biological outcomes	Age,	Unavailable
[3a]			School age 5-17	1981-89 (2/year)	547i 1c		Height, BMI, skinfold, handgrip	Age	

Notes: i=individuals, c=communities. X=number of requests by third parties unaffiliated with the study.

Table 4.1 – continued

Cultural

[1] Clark et al. (1995). a**=**Demography=dates of birth, marriage, death, divorce, and migration. b=Wealth and education are coded, but still not available to the public at the time of this writing (January 2016).
[2] Eisenberg et al.(2006) and Markovitz et al. (2012). a=Diarrhea data collected weekly, but covariates collected every 1-2 years. b=Data collection finished in 2013; the complete data set is not yet available to the public at the time of this writing (January 2016). c = Because the study started with 21 villages, but 5 villages were dropped in 2009, I estimated the gross attrition rate at 23%. During 2004-2008 they conducted 11 surveys with a total of 4196 individuals. Of these, 13% were surveyed only once, 27% were surveyed 2-4 times, and 46% were surveyed seven or more times (James Trostle, personal communication).

Biological

[1] Weitz et al. (2014). a=Baseline height-for-age and BMI-for-age Z scores and subscapular skinfolds. b=The baseline surveys of the eight societies differed; two were done in each of the following years: 1966, 1968, 1970, and 1972, making it an unbalanced panel. The data is available to qualified users via Professor Jonathan Friedlaender, department of anthropology, Temple University, Philadelphia.
[1a] Weitz et al. (2012). a=Baseline height-for-age Z score.
[1b] Page and Friedlaender (1987).
[2] Piperata et al. (2011). Data is not available for public use at the time of this writing (January 2016) but might be made available to qualified users in the future.
[2a] Piperata et al. (2011a)
[3] Shephard and Rode (1994; 1996). International Biological Program Human Adaptability Project. Upper age bracket varies by the outcome measured. The main outcomes were related to physical fitness and included such things as skinfold, BMI, handgrip and knee extension force, and hemoglobin. a=The authors do not report attrition, but they note that only 50% of the adults (usually the more physically fit) volunteered.
[3a] Shephard and Rode (1995). The authors do not report the initial sample size or the attrition rate.

Table 4.2. Summary of core topics and variables measured annually during most years of the 2002-2010 panel

Level or entity & topic	*Approximate # of variables*	*Examples of variable*
Individual		
Credit[1]	7	Money owed to others
Addiction	5	Commercial alcohol/cigarette consumption last week
Anthropometrics	27	Standing height, body weight
Demography[2]	35	Age, birth place, travel frequency to towns
Expenditures & barter	~70	All monetary expenditures in last 14 days and year; value of goods received in barter
Human capital	5	Tests of reading and math; assessment of Spanish spoken fluency; schooling level
Perceived health	4	All symptoms and ailments experienced in last 14 days
Emotions	18	Frequency of emotions (e.g. anger) last week
Monetary income	18	Monetary earnings from sales and wage labor, and remittances received
Shocks	14	Mishaps experienced in last year & ways of coping with mishaps
Conviviality	19	Gifts given/received last week
Assets	48	# of bows or cooking pots owned by person
Household		
Horticulture	30	Forest area cleared during previous year
Food consumed	31	Basket of food items consumed in last 7 days
Assets	48	Total bows or chickens owned by entire household (adults + children)
Village		
Prices	23	Market prices for assets and foods
Attributes	11	# households, teachers; village-town travel time

Notes: [1]Data for topics in italics were collected only from adults. [2]Some of these variables were only measured once (e.g., birth place, sex).

Table 4.3. Summary of topics measured in only some years of the 2002-2010 panel

Level or entity	*Topic*	*Years measured*
Individual	Local ecological knowledge of:	
	Plants	2002, 2003, 2005, 2006, 2007
	Parasites	2007
	Fecal samples, parasite detection and treatment[1]	2003, 2007
	Cultural orientation	2002, 2007
	Land encroachment by non-Tsimane'	2002
	Vaccination history	2002
	Time allocation via scans or spot observations[2]	10/2002-8/2003
	Pigeon pea cultivation	2003-2007
	Household decision making	2004
	Correlates of happiness/sadness	2004
	Perceived parental height	2005
	Status, trust	2005
	Regret	2005-2006
	Perceived beauty	2006
	Siblings, mortality, marriage, residence	2007
	Blood pressure and pulse rate	2007
	Aspirations for offspring	2010
Household	Pigeon pea cultivation	2003-2007
	House construction	2002
	House position (GPS)[2]	2007

Notes: [1]Children only. [2]Not included in the clean panel available to the public.

Table 4.4. Randomized controlled trials among the Tsimane' by the TAPS team

Years	*Outcome*	*Intervention or treatment*	*Villages included:*		*Data availability*
			TAPS	*Total*	
2007-09[1]	Map village lands	Participatory mapping of village lands	Yes	32	Yes; UAB
2008-09[2]	Child & adult health	Unconditional food transfers to all households in a village or to the poorest 20% of households in a village	No	40	Yes; Brandeis
2010-11[3]	Savings	Locked boxes with and without keys	Yes	59	Pending

Notes: [1]The trial is described by Reyes-García et al. (2012). As of January 2016, the data is available at: http://icta.uab.cat/Etnoecologia/proyecto.php?Id_proyecto=69. [2]Undurraga et al. (2016) describe the trial. [3]At the time of this writing (August 2016) the analysis has not yet been completed. Currently the data is housed at Innovations for Poverty Action (IPA, Yale University)

Table 4.5. Composition of TAPS sample of study participants by categories, 2002-2010

Category - Compared with previous year, participant experienced:		Survey year									Annual	
		2002[1]	2003[1]	2004	2005[2]	2006[2]	2007	2008	2009	2010	Median	%Δ
0 No change	n	1275	1,484	1,171	1,274	1,355	1,285	1,339	1,257	1,295	1,285	-0.27
	%	83.12	88.28	83.52	76.70	76.73	83.06	87.23	74.69	74.86	83.06	-1.26
1 Births	n	na	34	43	73	144	72	47	39	92	59.50	5.32
	%	na	2.02	3.07	4.39	8.15	4.65	3.06	2.32	5.32	3.73	4.43
2 Deaths	n	na	8	0	7	9	5	14	10	7	7.50	2.23
	%	na	0.48	0	0.42	0.51	0.32	0.91	0.59	0.40	0.45	2.35
3 Enters panel	n	58	63	38	50	57	40	33	15	37	40	-11.01
	%	3.78	3.75	2.71	3.01	3.23	2.59	2.15	0.89	2.14	2.71	-12.01
4 Leaves panel	n	169	63	21	73	68	9	30	255	32	63	-6.40
	%	11.02	3.75	1.50	4.39	3.85	0.58	1.95	15.15	1.85	3.75	-7.20
5 Person changes household in panel	n	19	26	83	34	38	33	28	21	26	28	-2.65
	%	1.24	1.55	5.92	2.05	2.15	2.13	1.82	1.25	1.50	1.82	-3.64
6 Other	n	5	0	14	41	7	3	2	21	23	7	4.06
	%	0.33	0	1.00	2.47	0.40	0.19	0.13	1.25	1.33	0.40	2.58
7 Refusals	n	8	3	8	14	21	1	3	1	13	8	-9.92
	%	0.52	0.18	0.57	0.84	1.19	0.06	0.20	0.06	0.75	0.52	-10.92
8 Becomes an adult (> 16 years of age)	n	na	na	20	42	28	24	23	18	28	24	-3.15
	%			1.43	2.53	1.59	1.55	1.50	1.07	1.62	1.55	-5.00
9 Leaves panel temporarily	n	na	na	na	37	8	70	0	0	113	22.50	34.86
	%				2.23	0.45	4.52	0	0	6.53	1.34	34.43
10 Impaired[3]	n	na	na	4	4	6	2	0	0	3	3	-7.61
	%			0.29	0.24	0.34	0.13	0	0	0.17	0.17	-9.91
11 Returns to panel	n	na	na	na	12	25	3	16	46	61	20.5	33.24
	%				0.72	1.42	0.19	1.04	2.73	3.53	1.23	33.09
Gross total with attriters	n	1,534	1,681	1,402	1,661	1,766	1,547	1,535	1,683	1,730	1,661	0.99
	%	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00	100.00		
Attriters (categories 2+4+7+9)	n	177[1]	74	29	131	106	85	47	266	165	106	6.82
	%	11.54	4.40	2.07	7.89	6.00	5.49	3.06	15.81	9.54	6.00	5.83
Net total without attriters of which	n	1,357	1,607	1,373	1,530	1,660	1,462	1,488	1,417	1,565	1,488	0.51
Non-Tsimane' represent about[4]	%	0.92	0.95	1.04	na	na	na	na	na	na		

Notes: na=not available. [1]See section entitled "Caveats about the demographic information" in Chapter 5 for how 2002 and 2003 data sets were constructed and see this chapter (p. 13) for the reason we have attriters and people in some of the other categories in the baseline year 2002. [2]2005-2006 includes attriters from the villages of Cara Cara and Alta Gracia of the panel who moved to the village of Undumo (outside of the Maniqui River) where we surveyed them. [3]Impaired includes people not interviewed because they were physically impaired, sick, or aged. [4]The % refers mostly to adults.

Appendix A

Bibliography of journal publications in English on the Tsimane' by the TAPS team, including results from exploratory studies (1997-2001) as of August 2016

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Appendix B

Code of ethics for TAPS fieldworkers

During the early years of the study we developed a code of ethics for researchers, students, or employees doing fieldwork under the auspices of TAPS. As new problems emerged we added items to the code. The training mentioned in items 9 and 11 below refers to the 2004-2011 summer field school in methods of data collection for PhD students in cultural anthropology implemented by TAPS staff. By 2011, the code included the following items:

1. No sex with people from the area, either Tsimane' or non-Tsimane'
2. No drinking of alcoholic beverages in town with indigenous people. The only drinking allowed is: (a) in the privacy of your home, (b) by yourselves in town, or (c) in public in the village when it is part of a social event. Under no circumstance should researchers buy Tsimane' an alcoholic drink.
3. No use of any drug that is illegal in Bolivia.
4. Researchers should refrain from providing credit to villagers, and never for more than six USA dollars at a time to the same household (~b$50 *bolivianos*).
5. Should someone in the village need emergency medical evacuation (even if unrelated to the project) the project should pay for it. We will request that the patient repay the costs incurred in the medical evacuation, but should the patient or her/his family refuse or be unable to repay, we will cover the costs.
6. Policies on stealing by subjects: ignore it unless the item stolen is very valuable; do not hold a grudge.
7. Policy on distribution of cigarettes and coca leaves to locals: do not distribute them to subjects.
8. Unless there is an emergency or medical condition, you must always remain with the group.
9. If for some reason you find that you want to leave the training program, you must return to the USA; you cannot hang around the town of San Borja or the region.
10. No jokes or teasing about another student's sexual, religious, or political orientation
11. If you break any of the above rules, the project reserves the right to dismiss you from the training at no cost to the project.

Appendix C

Fixing inconsistencies in reported age, education, and birth dates for clean TAPS data

by Rebecca Zhang

[I] Age

- only used when birth date year is unavailable -

The original age variables: Every year TAPS surveyors asked people ≥ 16 years of age (or younger if they headed a household) to report their age and the age of their dependents. If people admitted not knowing their age, we added 900 as a prefix. For example, if a parent said her child was 7 years old but the parent admitted not being sure of the child's age, then this child would have been coded as having age 907 . The name of the raw variable with a prefix for uncertainty is *idageY234567890* (or *idageY)*. The clean and the raw data contain another variable resembling *idageY234567890*, except that it excludes the prefix for uncertainty. The name of the variable without the prefix for uncertainty is *idageaproxY234567890*, and is the one used to construct the age variable consistent across years.

The age variable consistent across years: The age variable consistent across years is called *idage_beckyY234567890* (based on *idageaproxY234567890*). The rest of section I describes the construction of the variable *idage_beckyY234567890.*

Challenges for estimating age: Many people did not know their exact age. Below are some issues and examples with self-reported ages and years:

- Ages are not consecutive: first year (2003) John is 12 years old; second year (2004) John is 15 years old.
- Ages are not strictly increasing: first year John is 12 years old; second year John is 10 years old, and the third year John is 10 years old again.
- Lapse in years: John is surveyed in 2003, but John is not surveyed in 2004 or 2005. John's information returns in 2006 and continues to the most recent year.
- When a year is skipped, age does not necessarily skip by the same amount: In 2003 John is 12 years old. There is no information for John for 2004, but in 2005 John is not 14 years old as he should be. This problem is similar to the problem of non-consecutive age above.

Definition of terms to clarify steps in method section below:

Self-reported age – what the adult or caretaker reports or estimates for self or dependent. The name of this variable with the raw information is *idageaproxY234567890.*

Observed number of years – the number of years available in the data set, excluding skipped years. Example: If John has self-reported age in 2003, 2006, and 2007, then the observed number of years is 3.

Actual number of years – actual number of years between the most recently observed year and the earliest observed year, with the most recent and the earliest years included in the count. Example: if John has self-reported age in 2003, 2006, and 2007, then the actual number of years is 5.

Method

1. Take the average of self-reported age using observed number of years. For example, assume that John's information is as follow:

Year	Self-reported age
2003	19
2006	22
2007	23

   Observed number of years = 3
   Average of self-reported age = (19+22+23)/3 = 21.33
2. Find the median of the actual span of years. Example: John's actual span of years is 2003, 2004, 2005, 2006, 2007. Median of actual span of years: 2005
3. Assign the rounded average age to the median year. Rounding is done to the nearest integer, with halfway values rounded up. Example: 21.33 is rounded to 21. If the value was 21.72, it would be rounded to 22. In the example from the previous step (2), John is estimated to be 21 years old in 2005.
4. With even number of observations there would be no single median year. In these cases we round down the average reported age and assign it to the lower of the two median years. Example:

Year	Self-reported age
2003	19
2005	21
2006	22

   Observed number of years = 3
   Actual number of years = 4
   Average age = (19+21+22)/3 = 20.67
   Actual span of year (even number of observations): 2003, 2004, 2005, 2006
   Median year: 2004 and 2005 (median is usually estimated as the mean of the two years, so, 2004.5)
   Average reported age (rounded down): 20
   Assign round down (or floor) average to the lower of the two median years. Thus, for 2004, John's age is estimated to be 20, and in 2005, John's age is estimated to be 21.

More examples

Example I: Ages are reported correctly and consistent with year.

Year	*idageaproxY* (reported age)	Average age (15+17+18+19)/4	Age_bday_becky (estimated age)
2003	15	17.25	15
2005 (median)	17	17.25	17
2006	18	17.25	18
2007	19	17.25	19

Example II: Ages are not reported correctly, and therefore are not consistent with year (median is a whole number).

Year	IdageaproxY (reported age)	Average age (15+20+19+21)/4	Age_bday_becky (estimated age)
2003	15	18.75	17
2005 (median)	20	18.75	19
2006	19	18.75	20
2007	21	18.75	21

Example III: Ages are not reported correctly, and therefore are not consistent with year (median is not a whole number).

Year	IdageaproxY (reported age)	Average age (37+37+40+46)/4	Age_bday_becky (estimated age)
2003	37	40	38
2006 (above median)	37	40	41
2007	40	40	42
2008	46	40	43

Other anomalies: The rules just described apply to most cases, but some anomalies remain. For instance, a person reported the following:

Year	Reported age
2002	30
2003	31
2004	32
2005	33
2006	14

In this example we applied the same rule. However, the age reported in 2006 could have been a mistake of the person entering the data, and not a mistake of the person reporting the age.

Name of new variable: *idage_beckyY234567890.*

[II] Education

The original education variable: Every year TAPS asked people ≥ 16 years of age (or younger if they headed a household) to report the maximum years of schooling they and their dependents had completed. The name of the raw variable is *ihceduY234567890*.

The education variable consistent across years: The education variable consistent across years is called *ihcedu1Y* or *ihcedu2Y*. *ihcedu1Y* uses the peak method – for each individual, the highest value before any decrease in reported education years is used for all the years starting with the decrease. *ihcedu2Y* uses the smoothing method and only corrects education years for individuals 16 years of age or under. Unlike the peak method, the smoothing method takes into account the reported education years both before and after any decrease. The rest of section II describes the construction of the variable *ihcedu2Y*.

Steps before correction:

1. Replace all values <1 with 1. Fractional values were originally assigned to people who had not yet completed the first grade.
2. Manually replaced the following *ihceduY234567890* for people with the following unique identification numbers (*idssnY234567890)* because the values were too troubling and we used our best judgment: 1, 38, 107, 137, 257, 1150, 1396, 1534, 2160, 2164, 2167.
   An example of subject *idssnY234567890*=38:

year	ihceduY
2002	5
2003	5
2004	0
2005	0
2006	0
2007	6
2008	13
2009	12
2010	12

Corrected to =>

year	ihceduY
2002	5
2003	5
2004	5
2005	5
2006	5
2007	6
2008	6
2009	6
2010	6

   Two things to note about this type of correction. First, the number of troubling cases is small. Second, there are other possible interpretations. In the above example, it is possible that the person had five years of schooling in 2002 and increased schooling by one grade each year, so that in fact the person had 12 years of schooling by 2009.
3. Keep all observations that had a decrease in *ihceduY234567890*, but this rule was applied only for people younger than 16 years of age.

Correction:

Step 1. Oddity in one year + the value after the odd year does not increase enough from the values before the odd year according to the time span.

Example I:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	**0**	4
2005	4	4
2006	5	5

Example II:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	**0**	5
2005	6	6
2006	7	7

Example III:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	**0**	4
2008	6	6
2009	7	7

Example IV:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2006	**0**	5
2008	6	6
2009	7	7

All of the above cases would be evaluated using the average between the year before and the year after, weighted by how many years are apart. Then the result is rounded.

Step 2. Oddity in one year + the value after the odd observation increases too much from the value before the odd year according to the time span

Example I:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	0	5
2005	7	7
2006	8	8

Example II:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2006	0	7
2008	12	12
2009	13	13

The above cases would be corrected by following the previous years with appropriate addition due to increase of years.

Step 3. Oddity in one year + the values before and after the odd observation are the same

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	10	4
2005	4	4
2006	5	5

The above case is included in step 1, but because Stata works down the row, this case would re-emerge after the first two steps.

Step 4. Last observation decreases

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	5	5
2005	6	6
2006	0	6

If the last observation decreased, it would be corrected by taking the value of the previous observation.

Step 5. The first observation is larger.

Year	Original ihceduY	corrected ihcedu2Y
2002	10	4
2003	4	4
2004	5	5
2005	6	6
2006	7	7

If the first observation is larger than the second, it would take on the value of the next observation.

Step 6. Steps 1-5 are run again to fix missed entries.

Step 7. Skipping two years but should follow the year before.
Example I:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2004	0	5
2005	0	0
2006	7	7
2007	8	8

Example II:

Year	Original ihceduY	corrected ihcedu2Y
2002	3	3
2003	4	4
2006	0	9
2008	0	0
2009	13	13
2010	14	14

If two consecutive years appear to need correction, the average of the values before and after the two observations are taken and weighted by how many years they are apart, then rounded. Only the first observation would have a change because Stata works row by row.

Step 8. Re-run Steps 1-5. This is meant to target observations left from Step 7.

Step 9. Last manual adjustments: the age of the following were manually adjusted. They could not be correct by the program due to various reasons including missing values in a few years. Extreme cases, often hard to correct even manually, were changed using our best judgment. The subjects (*idssnY234567890*) included: 241, 271, 284, 402, 689, 1008, 1306, 1460, 1688, 1725, 1793, 1809, 1818, 1876, 1982, 2027, and 2230.

Step 10. Last, we checked to see if any persons' starting age for school conflicted with how many years of education they had. For example, someone 8 years old cannot have 7 years of

education. The following subjects were corrected for such problems: *idssnY234567890*=2033, 2113, 2232.

Note:

- The examples given are simplified. Data are usually a combination of multiple scenarios listed above. That is why steps 1-5 are repeated.
- Although *idschoolageY* (age when first started school) was considered, it was not used extensively because it contains many missing values, and data are not reliable. For example, someone will report he started school at the age of 6 years, then next year it becomes 10, then 7 the following year. It is possible that these inconsistencies capture the fact that the person started school at different ages because the person repeated the first grade many times. Unfortunately, we do not have information to resolve this impasse.
- This program is meant to eliminate any decrease in *ihceduY234567890*. Cases where subjects report more than 1 year of education increase are not fixed. For example, a subject reports 5 years of education, then in the next year he reports 9.

Name of new variables: *ihcedu1Y* (peak) or *ihcedu2Y* (smoothing).

[III] Birth date

Problems: Study participants have three birth date variables, each populated to a different degree, and within each variable there is discrepancy for a person.

- Birth date variables: idbirthY234567890, idbirth_par_selfY2345678, idbirth_TAPSY8

Goal: To create a new variable, idbirth_beckyY234567890, that is consistent across years for each subject and that is consistent with the clean age (see above): idage_beckyY234567890.

Method:

1. Create a new birth year variable by taking the difference between survey year (yearY234567890) and idage_beckyY234567890
2. Create a new birth month and day variable (BMD = month*100 + day) using idbirthY234567890; if missing idbirthY234567890, idbirth_par_selfY2345678 and idbirth_TAPSY8 are used in that order.
3. If multiple BMD's are available for each person, the earliest BMD is taken and applied to the person for all years.
4. If no BMD is available, idseasonY7890 is used to approximate BMD. If one is born in the dry season (May-July), BMD is assumed to be July 1; if born in rainy season (August-April), BMD is assumed to be August 1.
5. If idseasonY7890 and BMD are both not available, June 1 is assigned to the person.
6. Finally, idbirth_beckyY234567890 is created using the birth year created in step 1 and the birth month and day (BMD) created in steps 2-5.

Appendix D

Guide to tables and figures of Chapter 4

Table	Figure	Endnotes or other	Stata do file	Comments
4.2			None. Instead use TAPS clean data: TAPS_2002-2010_July_13_2016.dta	Manually explore the initial 2-4 letters of the name of variables to identify the level or entity to which the variable refers and the topic covered by the variable. See section entitled "Naming convention of variables to ease the use of the clean data"
4.3			None. Instead use TAPS clean data: TAPS_2002-2010_July_13_2016.dta	Manually explore the initial 2-4 letters of the name of variable to identify the level or entity to which the variable refers and the topic covered by the variable. See section entitled "Naming convention of variables to ease the use of the clean data". In addition, examine the suffix starting with Y to identify the years in which we measured the variable.
4.5			Do_Demography_10_age_pyramid	The construction of Table 4.5 was cumbersome and had to be done manually for each year, often using additional textual information from the column entitled "Notes" in the raw Access file, and cross-checking information between variables. I have tried to clarify some of the steps in the do file, particularly how the 2002 and 2003 annual data were created from the five-quarter panel. Interested readers should explore the nine annual Access files (tbl_demography), one for each year of the survey, which contain all the basic demographic information used in Chapter 4-5. The variable in the raw filed with a prefix "ReasonCh-[years]" (reason for change) or "Change" indicates the change in the person's status from the previous year (e.g., left panel, became an adult,. The construction of Table 4.5 differs from the construction of other tables in the book which can be easily replicated with the indicated data files and do files.

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[i] This chapter builds on an early draft written with William R. Leonard, Victoria Reyes-García, Asher Rosinger, Alan Schultz, Susan Tanner, Vincent Vadez, and Rebecca Zhang. A vignette of the chapter appeared in Leonard et al. (2015). I would like to thank Reyes-García for having read and commented on a later version of the draft.

[ii] The bibliography includes mostly studies by the TAPS team using the 13 villages of the panel as their core. TAPS researchers have done studies with larger sample of villages (Table 4.4), but those studies are generally excluded from Appendix A unless they include data from the TAPS panel.

[iii] During 2004-2011 TAPS ran a summer field school on methods of data collection for PhD students in cultural anthropology. The Program of Cultural Anthropology of the USA National Science Foundation financed the training program.

[iv] They used regressions to estimate secular (long-term) trends of outcomes. A person's anthropometric or biological measure was used as an outcome, and a person's birth period (e.g., decade) and age were used as explanatory variables. A cross section refers to information gathered at one time, or to information treated as though it had been gathered at one time.

[v] Weigh days is a method used by cultural anthropologists to measure consumption in hunting-gathering societies. During weigh days, the researcher sits unobtrusively in the camp or courtyard and identifies, weighs, and measures any good brought in, noting the time when the good was brought, the person bringing the good, and the place from which the extractor got the good. Scans or spot observations consists of walking a settlement at a constant pace on a pre-determined day and time, and noting what people are doing at the moment when researchers first see the person (Bernard, 2012; Johnson & Sackett, 1998). Done repeatedly over a long time, scans are ideal for estimating the allocation of time to quotidian events, at least in flocculated settlements where researchers can see what people are doing.

[vi] During the 1980s Protestant missionaries nurtured the establishment of the Tsimane' Council to strengthen the defense of the Tsimane' homeland against encroachment from highlanders and logging firms (Chicchón, 1992, pp. 66-68). The Tsimane' Council has jurisdiction only over the Tsimane' living in an area known as Territorio Indígena Chimán along the Apere and Maniqui rivers in the department of Beni. In the early 20th century this area includes 65% of Tsimane' villages. The Tsimane' also live in three other places beyond the reach of the Tsimane' Council: *(1)* Pilón Lajas Biosphere Reserve (26%), *(2)* Territorio Indígena Multiétnico (5%), and *(3)* Territorio Indígena Parque Nacional Isiboro-Sécure (3%) (Reyes-Garcia, 2001, pp. 30-31).

[vii] According to the 2001 Bolivian census, the "sección" of San Borja in the Province of José Ballivián, department of Beni, had a total population of 34,363, split between 19,363 in the urban center and 15,000 in the countryside, and had experienced an annual growth rate of 3.76% during 1992-2001. A sección is the smallest political and administrative sub-division in Bolivia, forming part of a province, which in turn forms part of a department. Information downloaded on July 10, 2016 from http://www.ine.gob.bo/indice/visualizador.aspx?ah=pc20102.htm

[viii] Yaranda (15'16.369 S, 66,50.838W) and San Antonio (14'48.698 S, 66'39.761W) lie 50 km and 10 km from the town of San Borja in a straight line.

[ix] The Wisconsin Longitudinal Study (Sewell et al., 2001) provides an apt example of a serendipitous beginning of a panel study.

[x] As we shall see in Chapter 5, the Tsimane' do not have a marriage ceremony, so I avoid using the terms wife and husband, but occasionally I use the terms as a short hand.

[xi] In 2010 the village of Arenales broke up into two villages: Arenales proper and, about 20-30 minutes away walking, the village of Las Minas. In the surveys we treated the two communities as though they shared the same attributes (e.g., distance to the town of San Borja). For studies done after 2010, we treated the village of Las Minas as a separate entity from the village of Arenales.

[xii] From the baseline (2002) sample of 1,357 people, 568 (41.87%) were older than 16 years of age. The estimate of 568 comes from multiplying the mean number of adults in each household (2.46) times the total number of households in the 13 villages of the panel during the first year of the study (231). The estimates for the mean number of adults per household and for the total number of households surveyed in 2002 come from Table 5.4.

[xiii] We only coded for ethnicity during the first three years of TAPS (2002-2004, inclusive). The last row of Table 4.5 suggests almost no growth in the share of non-Tsimane' living in TAPS villages, from 0.92% to 1.04%. The 2008-2009 randomized control trial suggests a higher share of non-Tsimane' dwelling in Tsimane' villages. During 2008 and 2009, 6.68% and 7.60% of the people in the 40 villages of the randomized controlled trial did not self-identify as Tsimane'. We cannot tell whether the large difference in the shares between the two studies -- about 1% versus 7% -- mirrors actual differences between the two samples, or whether they mirror changes in the ethnic composition of Tsimane' villages in the region from 2002-2004 until 2008-2009.

[xiv] Although we compensated people generously for taking part in the annual surveys, some people might have felt that the compensation did not keep up with the rise in the monetary value of time. The TAPS clean data for public use suggests that during 2002-2010 the median village monetary wage (adjusted for inflation) without adding the value of a lunch lagniappe, as is often done in the area, rose by an annual rate of 5.65% or 2.72 *bolivianos*.

Year	CPI[1]	Nominal daily wage without food[2]	Real wage[3]
[a]	[b]	[c]	[d]
2002	64.20	25	38.94
2003	66.40	25	37.65
2004	69.30	25	36.07
2005	73.00	30	41.09
2006	76.20	30	39.37
2007	82.80	30	36.23
2008	94.40	40	42.37
2009	97.60	60	61.47
2010	100.00	60	60
Annual Δ in %		11.78	5.65
Annual Δ in *bolivianos*		4.58	2.72

Notes: [1]Retrieved from World Bank on January 24, 2016. http://data.worldbank.org/ indicator/ FP.CPI.TOTL?page=1\. [2]Name of variable in clean TAPS data for public use (2002-2010): *wpbwageY234567890*. [3]Real (inflation-adjusted) wage = nominal daily wage/(CPI/100). CPI = Consumer Price Index.

[xv] I have in mind the use of individual, household, or village fixed-effect regressions.

[xvi] Thanks to Eduardo A. Undurraga and Camila García for preparing the map, which first appeared in Undurraga et al. (2016).