Introduction. The reviewers point to three broad categories of shortcomings in our initial application that we have addressed in this revision: **(1)** design of the experiment, **(2)** model and data, and **(3)** protection of human subjects. In sections A-E of this proposal we underline changes that we have made in response to the reviewers' comments.

(1) DESIGN OF THE EXPERIMENT. The experiment had three interventions, all geared to 50% of the households selected at random in the bottom half of the income distribution: (**a)** a lottery with rice prizes worth $170/household, (**b)** a lottery with a higher prize ($342) to detect non-linear income effects, and (**c)** purchase of handicrafts worth $170/household, which would allow us to compare income from a windfall **(a-b)** with earned income **(c)**. The interventions would have changed the level of individual income and the distribution of income in villages. The reviewers had three concerns: **(1.1)** untangling the effect of changes in the level of income from changes in income inequality, **(1.2)** small sample size of villages and **(1.3)** short follow-up of only 6 months.

(1.1) Untangling the effect of changes in the level of income from changes in income inequality. The reviewers suggested increasing the level of household income in all households in villages by the same percentage in one intervention, thus leaving the distribution of income in villages unchanged. To include this possibility, we have dropped intervention **(c)**, and replaced it with an intervention in which we raise by the same percentage the income of all households in the village. We will determine the percentage increase in income required so the total amount of income transferred to the village equals the amount of income they would have received with (**a)**. Another reason to drop **(c)** and replace it with the alternative suggested by the reviewers has to do with the absence of data on input costs, which would have made it difficult to compare the results of **(c)** with **(a);** income under **(c)** is gross because it does not take into account the input costs to produce the handicrafts, whereas income under **(a)** is net.

(1.2) Small sample size of villages. To increase the sample size, we have reduced the prize in intervention **(b)** from $342 to $255/household, which allows us to increase the sample size by four villages, from 36 to 40 villages. We will allocate the four additional villages equally among the three treatment groups and the control group. We also propose an estimation strategy that enables us to combine the estimates from the three treatment and the control groups to obtain more precise estimates (**D.5**)

(1.3) Short follow-up of only six months, which "does not allow much time for the effects of the intervention to develop". In the previous proposal we had one post-intervention survey that took place only ~6 months after the interventions. In response to the concerns of reviewers, we have changed the schedule and the number of surveys after the intervention. The baseline survey will occur during 7/2006-9/2006 and the intervention will occur immediately after the baseline survey (10/2006-11/2006). To estimate the effects of the intervention, we will conduct one survey in 1/2007-3/2007, three months after the intervention, and a second survey during 10/2007-12/2007, a year after the intervention. A follow-up survey 12 months after the intervention has the advantage of keeping the seasonal effects the same. Doing two surveys 3 and 12 months after the intervention should enhance the likelihood of capturing short-run effects among children or adults; among children because children's health status responds fast to changes in parental socioeconomic status[1,2], and among adults because changes in income inequality have an almost immediate effect on perceptions of fairness[3], with potential effects on blood pressure or immune function. We cannot extend the follow-up surveys more than a year after the intervention because R21 awards last a maximum of 24 months; since we use the first three months of the study (4/2006-6/2006) to obtain informed consent and explain the experiment to villagers (Section 3) and the last three months (1/2008-3/2008) to clean the data while in the field to explore with villagers their perceptions of fairness after the intervention and to undertake preliminary analysis, we only have a total of 18 months to do the baseline survey, carry out the intervention, and do the two post-intervention surveys.

In sum, the new experimental design consists of three interventions in 30 villages: two lotteries with rice prizes for 50% of households in the bottom half of the village income distribution in 20 villages and an income transfer to all households in an additional 10 villages. Intervention #1 will occur in 10 villages and will include a prize in rice worth $170/household. Intervention #2 will occur in another 10 villages and will include a prize in rice worth $250/household. We set the prize in Intervention #2 higher than in Intervention #1 to detect non-linear income effects. Intervention #3 will occur in another 10 villages; in Intervention #3 we will increase the income of all households (not just those in the bottom half of the income distribution) by the same percentage, leaving the village income distribution unchanged. Intervention #3 allows us to untangle the effect of changes

in overall village income levels from changes in the distribution of income in the village. Besides the 30 villages receiving treatments, 10 villages will serve as controls.

(2) MODEL AND DATA.

(2.1) Lack of specificity of the analytical method and model. We now further develop the explicit model that will be estimated and show why it will lead to unbiased estimates of the impact on child health of household income, mean community income, and within-community income distribution (Section D.5). An important advantage of the proposed estimation procedure is that it will permit obtaining more precision through combining observations from all three treatment samples and the controls samples rather than limiting the analysis to pair-wise comparisons among the four samples.

(2.2) "The lack of specification of alternative social capital measures (a major path argued through which income inequality affects health and, as currently measured, with almost no variation across households)". Researchers have measured social capital in at least four ways: norms (e.g., self-reported trust in others), observed pro-social behavior (e.g., generosity to non-kin), presence of formal institutions (e.g., courts), and participation in formal institutions (e.g., labor unions)[4]. We had proposed to measure social capital only through observed pro-social behavior because it provides a more objective and reliable measure of social capital than norms [5] and because small-scale pre-industrial societies lack formal institutions. Furthermore, we included pro-social behavior because of its ubiquity, salience, and variability. In response to the comments of reviewers, we have expanded the definition of social capital to include people's perceptions of trust in others[6,7]. The reviewers suggested that observed pro-social behavior might not contain sufficient variation. We tested the hypothesis using the latest (2004) wave of panel data from 574 adults (20-65 years of age) and found much variation in pro-social behavior across households. The coefficient of variation (CV; sd/mean) of gifts given to other villagers (excluding close kin) was 5.44 (mean=0.12, sd=0.66, min=0, max=10) and the CV of labor help offered to other villagers (including communal work) was 3.78 (mean=0.23, sd=0.87, min=0, max=10). The measures of social capital contain more variation than other variables of interest for many analyses, such as personal income (CV=2.24) or the body-mass index (CV=0.11), an anthropometric measure of short-run nutritional status (BMI=kg/mt^2).

(2.3) Neglect of objective measure of stress. We will pilot test the collection of cortisol as an objective biomarker of stress with a sample of 100 adults and 100 children, evenly split between females and males across the three treatment and control groups. Data will be collected at baseline and in the first post-intervention survey; during each survey, we will collect cortisol on two different days for each participant. We limit the collection of cortisol to only 200 participants, to only two days/participant during each survey, and to only the first post-intervention survey because of the additional cost ($60/participant/survey with cortisol taken on two days) and because we are unsure about the feasibility of collecting such data in our research setting. On days when we collect cortisol, we will collect three samples/participant at the following intervals: **(1)** immediately upon awakening, **(2)** 30 minutes post-awakening, and **(3)** in the evening before going to sleep. From this series we will estimate three outcomes: response to awakening, area under the curve, and pattern of diurnal rhythm. Section **9.2.4** contains a discussion of how we will take salivary samples to measure cortisol.

(2.4) Problems with using the Gini coefficient with samples of small size. We have been aware of the limitations of the Gini coefficient of income in past work[8-11], and so have used other measures, such as the coefficient of variation and the standard deviation of the logarithm of income. Each measure has advantages and disadvantages [12-14]. In the proposal and in the empirical analysis we use the Gini coefficient of income as a starting point because it is the most widely used measure in studies of income inequality [13], but we are mindful of the need to use other measures of income inequality both because of the lack of precision when using the Gini coefficient with a small sample and because results may vary by the measure of inequality used [12]. Furthermore, to ensure robustness in results, we will compute indices of village income inequality that draw on total household income, household income/person, and household income/adult equivalent.

(2.5) "Questionable health outcomes for children in 6 months given the high prevalence of hookworm and, in terms of increasing access to medical services, prior research shows that much of the study population self-medicates". In part in response to this comment, we have modified the second follow-up survey to occur 12 months after the intervention (Section 1.3). Not only does this increase the time period for effects to develop, but it also controls better for seasonality. Furthermore, some of our health outcomes respond to the high prevalence of hookworm. In research in progress we find that children 2-4 years of age below the median on skinfolds thickness, and who have elevated CRP at baseline (indicating an acute phase

response, likely due to infection), grow significantly less in height over the subsequent three months (9mm difference in height gain between elevated CRP vs. non-elevated CRP).

(3) PROTECTION OF HUMAN SUBJECTS. As the proposal makes clear, we have been working with the Tsimane' since 1999 and have built excellent working relations with them. Since the core of our work centers on the construction of a long-term panel, we have a long-term personal and professional commitment to the people and area that we would not like to jeopardize in addition to our ethical concerns. To redress possible risks to human subjects we will take five steps beyond the ones already outlined in the initial application. **(1)** We will make it clear from the outset we will not exclude males from the lottery to increase the perceived fairness of the lottery. Qualitative evidence from PROGRESA, a program in Mexico which gave income transfers only to women, suggests that the program produced some intra-household strife because men felt excluded. Our approach represents an improvement over PROGRESA because we do not select one sex for special treatment [15]. **(2)** To avoid feelings of exclusion, under Intervention #3 discussed earlier we will include all households in the village. Through focus groups during the last three months of the study we will compare measured feelings of inclusion between this and the other two treatment groups. **(3)** We will hold four small public lotteries at the village level and two small private lotteries at the household level during the month before the interventions to increase people's understanding of lotteries and to assess whether lotteries might produce strife. For the public lotteries we will assemble all villagers in an open yard, assign each household a different chip, put the chips in a bag, have a child (rather than outsiders or authority figures) select some chips at random, and award three fishhooks to winning households. After the lottery, we will discuss why some households won to ensure villagers understand the concept of randomness. We will stress that we do not have enough resources to award a prize to all households; hence the need for a lottery. We will repeat the lottery on three different days in front of all villagers. By holding a total of four public lotteries before the intervention and by having a discussion after each lottery, we will enhance people's understanding of lotteries and the randomness of winning. More importantly, by sequencing the four lotteries over a month, we can use lotteries 2-4 to discuss what happened to the winning or losing households of the previous lotteries, and explore with villagers if lotteries produced strife. We will then hold two additional lotteries in the privacy of each household to give husbands and wives a more private opportunity to ask questions about the lottery. With both wife and husband present, we would assign each one of them a chip of a different color, put the two chips in a bag, and have a child draw one of the two chips at random, with the winner taking three fishhooks. On a different day we would repeat the lottery and ask how the couple felt about the previous lottery. During the last private and public lottery we will ask participants whether they had lingering doubts and whether they felt comfortable about participating in the rice lotteries that form the core of our intervention. **(4)** During the discussions after the six trial lotteries, we will ask participants what steps we should take if people feel uncomfortable with the ex-post inequality from the lotteries. Answers to the question will provide us with a grass-root solution to the possible adverse effects of the lottery for villages that decide to take part in the study. **(5)** We have requested IRB approval from Northwestern and will notify NIH as soon as we hear from them.

A. SPECIFIC AIMS

A growing concern in the health sciences centers on possible the harmful effects of income inequality on health[16-20]. Income inequality is alleged to erode social capital – trust, safety nets, and norms of reciprocity that enable people to act collectively to protect their health [6,21-24]. Growth in income inequality and the breakdown of social capital is alleged to increase jealousy and stress, and so harm health [17,17,25-28]. Pitted against this view is a second one that emphasizes the direct protective role of own income on health that works independent of income inequality or social capital[29-32]. Of course, both income inequality and the direct effect of own income may matter, so the question may be what are the relative effects of income inequality versus own income. To date most research on the effects of income inequality on health has relied on secondary, observational data from adults in large geographic units of developed nations[33]. We know little about how income inequality, social capital, and stress versus own income shape the health of children in poor villages of developing nations[34,35].

Here a team of biological (Leonard, McDade, Tanner: Northwestern) and economic anthropologists (Godoy, Huanca: Brandeis), an economist (Behrman: U Pennsylvania), and a social epidemiologist/pediatrician (Goodman: Brandeis) propose to pilot test the use of an experimental research design that changes parental income and village income inequality to compare the effects of: **a)** changes in village income inequality on family social capital, perceived and objective stress, and indices of general health of children <13 years of age and of their parents and **b)** changes in parental income on indices of general health of children and adults in a foraging-farming society of Amazonian Indians in Bolivia (Tsimane'). We will do the study in 40 villages (30 treatments; 10 controls) over 24 months (4/2006-3/2008). We propose three interventions to increase parental income and change village income inequality differentially, with one control group. The interventions are designed to permit investigation of the impact of possibly non-linear effects of changes in household income and of changing versus maintaining constant income inequality (D.3). In each household receiving the treatments, we will select at random between the female or the male household head to receive the prize. The prizes of US$170-255 are significant given that average annual income/household member among Tsimane' is US$342. We will assess the effects of the interventions on social capital and adult and child health. Our research builds on knowledge and experience gained in a panel study in progress among the Tsimane' since 1999 but will be done in different villages. The study will allow us to evaluate many issues before submitting an RO1. The specific aims of this R21 include:

Specific aim 1: *Assess the logistical feasibility of doing the experiment on a larger scale,* including the transport of rice to distant villages and assessing villagers' acceptance of the experiment and possible unanticipated effects.

Specific aim 2: *Preliminary (because of limited precision due to small sample sizes) exploration of hypotheses that are to be examined in greater depth in the RO1:*

H1. Lowering income inequality by raising income of the poor will produce ambiguous effects on social capital:

- **H1.a.** If generosity flows from the rich to the poor regardless of kinship, raising income of the poor to lower income inequality will lower social capital since it will lower incentives by the rich to help the poor.
- **H1.b.** If generosity follows kinship lines, income transfers to the poor will strengthen social capital because recipients will share it with others irrespective of the economic status of the giver or receiver.

H2. Income transfers to the poor will:

- **H2.a.** Improve the average health of villagers primarily through the direct protective effect of income rather than through the protective effect of social capital against income inequality.
- **H2.b.** Have stronger average effects on health in poorer households owing to the convexity of the health production function.
- **H2.c.** Improve health by improving food intake and use of medical services and by reducing energy demand from illness, thus improving energy balance.
- **H2.d**. Produce weaker effects in more remote villages because social capital in the form of sharing and gift-giving will induce people to share their income more with others, weakening the protective role that own income might have had on health.
- **H2.e.** In a household with both a female and a male head, transfers will have a stronger protective effect on child health when the transfers go to the female rather than to the male head.

Specific aim 3: *Assess causes, magnitudes, biases, and steps to reduce attrition.* Because we plan to do the study in a semi-mobile population of foragers and shifting cultivators, our sample may suffer from high attrition[36], which could bias results. The pilot study will allow us to identify the causes and magnitudes of attrition. Understanding the causes of attrition will help us identify variables by which to weigh the sample of a

larger-scale study to ensure sufficient statistical power. The pilot study will also allow us to assess the costs of tracking people who do not move too far and of those who move far away, test whether the two groups differ, and decide how many in each group to track for the RO1[37].

Specific aim 4: *Validate methods to collect data on perceived and objective stress.* Theories of inequality and health give a prominent role to perceived stress. Methods to measure stress were developed and are widely used in schooled populations of middle-class, white European-Americans[38-40], but they remain relatively untried in developing nations. The pilot study will allow us to adapt and validate some of these methods. Besides collecting data on perceived stress, we will also collect data on cortisol, an objective biomarker of stress.

Specific aim 5: *Explore collaboration with Bolivian counterparts.* The preparation of the R21 enabled us to undertake the initial stages of preliminary planning together with Bolivian researchers at the Universidad Mayor de San Andrés and at the Instituto Boliviano de Biología de Altura (La Paz) who are doing research on rural health. Undertaking the work proposed in the R21 will enable us to strengthen ties with Bolivian collaborators; we need to develop stronger ties with Bolivian counterparts to develop and implement an R01.

B. BACKGROUND & SIGNIFICANCE. Researchers long had noted that income inequality might harm population health[34,41-43], but it was not until the 1990s that the idea captured the attention of policy-makers and academics owing to the work of Wilkinson[17,18]. He found a positive correlation between income inequality and morbidity in developed nations. The finding galvanized the search for more stringent tests, encouraged further empirical studies, and produced research to identify the paths by which income inequality might affect health.

B.1. More Stringent Tests of Links Between Income Inequality and Health. Early researchers correlated the average health of a community with the income inequality of a community. Critics noted that the positive correlation between income inequality and poor average population health could result purely from the convexity of health production functions, and from aggregating data from individuals to communities [30,44-47]. More stringent tests include the use of individual (rather than community) measures of health as dependent variables and different measures of income inequality because results could vary by the measure of inequality[12-14,19,48]. More stringent tests also include controls for own income (perhaps with non-linearities) because, as noted above, income might have a protective role of its own independent of income inequality[33].

B.2. Empirical Studies. Empirical studies of income inequality and health fall into two chronological stages. Earlier studies used large geographic units (e.g., states) as the unit of observation and analysis, cross-sectional data, and objective indices of average community health as dependent variables[33]. To overcome the problems raised in B.1, later studies relied on individual rather than community measures of health as dependent variables, controlled for individual income, put more stress on self-perceived indicators of health, and often drew on panel data. Panel data allows control for the time elapsed between changes in income inequality and visible effects on health. Studies have produced mixed results[20,49]. Earlier studies found a positive correlation between income inequality and health[6,46,50-54,54,55], but later studies found mixed support[30,47,56,57 58-60 61-64]. The apparent adverse effect of income inequality on individual health weakens after controlling for community attributes, such as racial/ethnic heterogeneity[29,58,59].

B.3. Paths by Which Income Inequality Affects Health. Income inequality is posited to affect health through two paths. (1) Income inequality may make it harder to agree on the provision of public goods that enhance health[65,66]. (2) Income inequality may erode social capital and raise cultural dissonance, each of which raises stress, a proximate determinant of health. Income inequality may erode social capital--social networks, institutions, and norms of sharing and reciprocity that protect health[6,21-24]. The weakening of social capital may create resentment and stress-related behaviors (e.g., smoking)[17,25], which harm health. Income inequality may increases cultural dissonance, the gap between shared cultural norms and peoples' objective socioeconomic status. Cultural dissonance may produce stress and correlate with worse objective health outcomes[67-71].

B.4. Income and Intra-Household Health. Empirical studies for developing nations suggest that own income protects adult and child health[16,32,72]. Rising income protects health, but at a diminishing rate[33]. Studies using an experimental research design to assess the effect of exogenous changes in income on health are rare[73]. The effect of income is modulated by who controls the income. Income held by the female or male household head might have different effects on child health[74-76]. Observational studies suggest that income in the hands of female household heads has stronger protective effects on child health and nutritional status than income in the hands of male household heads[77-79], and that parents might bias investments in favor of children of one sex[74,75,80-86]. Behrman[87,88] discusses the unreliability of drawing inferences from observational studies about the differential effects of parental investments on child health; the unreliability stems from the role of unobserved heterogeneity in child endowments, parental preferences, and household attributes.

B.5. Exogenous Income Windfalls. The measured effects of lotteries vary by the size and duration of the prize, and are mainly positive. In Sweden winning a lottery increased self-employment because it reduced liquidity constraints[89]. A study of lottery winners in the USA that tracked them six years after winning shows that winners worked less, saved more, and spent more on cars and homes if the prize >~$80,000 and came in annual installments; effects increased over time[90]. Among the unemployed, even a small one-time prize increased labor supply[90]. People spend windfall income more readily and share it more widely than they do earned income[91,92], perhaps because they view the two types of income differently[93]. Despite stereotypes[94], winning a lottery does not produce adverse outcomes[95]. Lottery winners in Norway worried about envy from others[96], but a panel study in the UK suggests that two years after winning a lottery, winners were happier than people who had not won[97]. We know of no study that estimates the effects of lotteries on health. A comparative study of the UK, USA, and Taiwan suggests that income inequality of young cohorts grows over time as cohorts age, so when in the life cycle one receives an income shock may matter for health[98]. Studies of lottery winners could provide more accurate estimates of the effects of income on well-being than observational studies, but still yield biased estimates because they cannot control for self-selection into lotteries.

B.6. Summary. Research on the effects of income inequality on health primarily has relied on secondary or observational data from adults in developed nations and used large geographic areas as the unit of analysis. The use of an experimental research design to obtain primary data to test competing hypotheses about the effects of income inequality and parental income on adult and child health can contribute to the debate on income inequality and health in developed nations in the following ways: ***(1)*** Owing to the endogeneity of income and social capital, it is very difficult to obtain unbiased estimates of the effect of own income on health or of the effect of income inequality on social capital and health from observational studies. Income inequality might erode social capital, but a community's social capital might limit the amount of income inequality it tolerates. Social capital might protect health, but people with poor health might not have the resources to invest in social capital[5]. The use of an experimental research design overcomes biases of observational studies. We avoid biases that arise from self-selection into lotteries by assigning prizes at random in our sample population[73,90]. ***(2)*** The study of income inequality, social capital, and health in small-scale, rural societies of developing nations makes it easier to discern patterns harder to spot in developed nations such as the USA. For instance, income inequality in developed nations may mask ethnic/racial heterogeneity, biasing estimates of the direct effects of income inequality on health. Small-scale pre-industrial rural societies lack these confounding variables. ***(3)*** The study of the effects of income inequality on child health might provide sharper results than the study of the effects of income inequality on adult health because children are more sensitive to their environment[1,2]. ***(4)*** Reliance on secondary data makes it hard to test competing hypotheses or to examine the paths by which income inequality affects health because some of the paths (e.g., cultural dissonance) require information typically unavailable in secondary data sets. ***(5)*** Findings from the proposed study will complement the small literature from developed nations on the effects of windfall income on health.

C. PRELIMINARY STUDIES. The latest Bolivian census puts the Tsimane' population at 8,000; 93% live in the department of Beni[99]. The Tsimane' are a typical native Amazonian population. People live in small villages along river banks and logging roads. Subsistence centers on hunting, fishing, and slash-and-burn horticulture[100-102]. They practice cross-cousin marriage (males marry mothers' brothers' daughters), a preferential marriage system that creates a thick and wide web of affinal and blood kin across and within villages. Residence is matrilocal shortly after marriage, followed by neolocal residence[101,103]. Tsimane' remained relatively isolated from contact with outsiders until the 1970s[103]. During the 1970s, the opening of roads brought loggers, ranchers, oil firms, and colonist farmers into the Tsimane' territory. Contact with the outside centers on the sale of rice and forest goods and on work as unskilled laborers[104]. The latest wave of panel data suggests that 44% of Tsimane' households had interacted with some type of encroacher during the last month. The most important market good Tsimane' buy or acquire is food (e.g., canned meat) followed by clothing[105]. Modern medicines and goods related to hygiene (e.g., soap) account for <7% of the value of cash expenditures plus goods received in barter. Despite contact with the outside, Tsimane' are highly autarkic (**C.2.4**). Social capital in the form of gift giving and labor help, which we will measure in this study, is widespread and salient (**C.2.8**). Despite widespread displays of generosity, villages have considerable income inequality (**C.2.6**). When ill, Tsimane' self-medicate with local plants, but some seek modern medical treatments in towns or from traveling traders[106]. Two dissertations from our work contain further ethnographic details of the Tsimane'[100,106]. **C.1** describes data collected and **C.2** discusses results from the project. Godoy's web site[107] has data before 2002 and their documentation plus PDF files of project publications.

C.1. Description of Data Collected. Our research among Amerindians in the Bolivian Amazon started in 1995 [108]. This R21 builds on a panel study started in 1999 among the Tsimane' to assess the effects of markets on well-being. The panel includes 1,541 subjects, 279 households, and 13 villages along the Maniqui River. *This study builds on the panel because we draw on lessons learned since 1999, but it will take place in other villages to ensure that the proposed interventions do not affect the panel study*. Since 2004 we have used the panel villages as an NSF summer training site in methods of data collection for PhD students in anthropology.

C.1.1. Five-Quarter Panel (1999-2000). We collected 5-quarters of panel data from 325 subjects in 60 households of two villages, one remote and close to a market town. Through surveys, physical measures, and direct observations, we collected data on anthropometric indices, perceived health, earnings, use of commercial alcohol and cigarettes, Geographic Positioning Systems (GPS) coordinates, weather, and private rates of time preference (patience). We measured patience by asking subjects to make 8 real (not hypothetical) choices between receiving a small immediate monetary or food reward now or waiting for a larger reward in the future[109,110]; based on responses we estimated a hyperbolic and an exponential delay-discount rate of patience. At the start of the study, we tested subjects to assess their academic skills, and their Spanish fluency. We trained villagers to take daily minimum and maximum temperature, measure rainfall with a pluviometer, and record cloud cover. One student wrote a PhD on the covariates of ethnobotanical knowledge[100], a popular book on ethnobotanical knowledge with a Tsimane'[111], and articles on the measure of ethnobotanical knowledge[112]. Another student wrote a PhD on the effects of markets on perceived illness and nutrition[106]. The research allowed us to asses the best recall periods to lower errors from faulty recall.

C.1.2. Cross-Sectional (2000). We designed this study to test whether results from **C.1.1** applied to villages in other habitats. The study benefited from **C.1.1** because we learned how best to probe and ask questions on many topics. With the help of four undergraduates from Northwestern, two PhD students from U Florida, and a PhD candidate in biological anthropology from U Michigan, we surveyed 1100 subjects in 511 households of 58 villages. In each village we randomly selected ~10 households, and in each household we randomly selected the female or male household head and one child (<13 years) for the interview. We found that the typical village has 18 households (median=12; sd=13.8), with a third having fewer than 10 households. The typical household has six people, evenly split between people above and below 13 years of age, females and males. We used the survey to estimate the effect of market exposure on the loss of ethnobotanical knowledge[113], the role of wild animals in traditional medicines[114], and the effect of cash cropping on deforestation[104]. We found that rice ranks among the top crops in consumption, value, and area planted.

C.1.3. Two-Year Panel (2001-2002). This was an experimental study at the village level. We used data from **C.1.2** to select 36 villages and randomly assign the intervention, which consisted of introducing an edible cover crop and training on how to improve farm productivity and human hygiene. Outcomes included health, nutrition, farm productivity, and earnings. We trained 10 Tsimane' to help implement the intervention. We selected randomly ~10 households/village to monitor outcomes, which we measured once before and once after the intervention. We recently started to analyze the data. Between 2001 and 2002 the sample shrunk by 17.7%, from 378 to 311 households. Owing to shortage of funds, we did not track attriters who moved outside of the region. Using the baseline survey, we did not find significant difference in socioeconomic or demographic attributes between subjects who remained in the sample and subjects who left. Since we had not anticipated such high rates of attrition, we did not use the baseline survey to ask about variables that might have caused attrition. Since then, we have done ethnographic interviews to explore the reasons for permanent attrition and have identified perceived lack of wild game and educational and health facilities in the village. The study sensitized us to some of the substantive and logistical problems that surface when using an experimental research design. For instance, subjects serving as controls wanted training in health and hygiene but were willing to wait until the end of the study.

C.1.4. 5-Quarter Panel (2002-present). The goal is to estimate the effect of markets, income inequalities, and objective health and nutritional status on subjective happiness. The study includes all subjects (n=1541) from all households (n=279) in 13 of the 36 villages from **C.1.3.** The study resembles **C.1.3**, but goes beyond it by including data on objective indicators of health: **(a)** hemoglobin levels (Hgb), **(b)** C-reactive protein (CRP), and **(c**) antibodies against the Epstein-Barr virus (EBV). CRP is the prototypical protein of the acute phase response, a non-specific, systemic response that provides the body's first line of defense against pathogens. CRP increases in response to many pathogenic agents; elevated CRP concentrations have been associated

with detrimental child growth outcomes, poor iron status, and sub-clinical infectious morbidity[115-120]. EBV antibodies provide an indirect, functional measure of cell-mediated immune activity, such that higher levels of antibodies indicate poorer cell-mediated immunity[121,122]. Research has associated increased EBV antibody level with psychological stress, under-nutrition, and infection[69,118,123]. We found that the indicators are easily measured. We have run workshops in villages to explain the results of blood analysis. In the 2004 survey we added a module on expenditures to assess whether female and male household heads differ in the goods they buy. Ethnographic evidence suggests that male household heads are more likely to buy alcohol, cigarettes, and status frills, but we have not yet tested the idea. 56.3% of children <13 years of age had vaccination cards.

C.1.5. Summary of Data Collected. We have collected panel and cross-sectional data on many socioeconomic, health, and demographic variables. Research has allowed us to identify the best wording and sequencing of questions and the best recall period for different questions, write a co-authored book on local plants with villagers and win their trust, train a local nurse, use an experimental research design, use the research site to train students, and identify rice as one of the most important subsistence and cash crops. Two practical issues that need addressing in future work are: **(a)** identifying causes of attrition and a strategy to reduce it and **(b)** reducing the time/error level when transcribing data from paper surveys into the computer. It took us 3 months to put a survey wave of 13 villages in the computer, and another 3 months to clean it.

C.2. Preliminary Results.

C.2.1. Child Subjective and Objective Health. Analysis of three surveys (1999-2002) in which we asked parents whether their children had been ill last week indicate that 57-61% of children had been ill, with similar shares for boys and girls. We started collecting objective measures of health in 5/2002 and have analyzed samples for concentration of CRP and hemoglobin. Concentrations of CRP are high in this population, particularly in young children. 23.3% of children of age 2-3 years had CRP> 5mg/L; 15.2% of 4-5 year-olds had CPR>5mg/L, and ~10% of children of ages 6-15 had CRP> 5mg/L[124]. In addition, we have associated elevated CRP at baseline with reduced linear growth over the subsequent 3 months, suggesting that infection may contribute to growth faltering[125]. Rates of anemia are among the highest ever reported for a native South American group, with 59% of children <10 years of age (227 boys; 193 girls) having low hemoglobin using WHO standards[126]. *In sum, Tsimane' children suffer from poor perceived and objective health.*

C.2.2. Child Nutritional Status. We used data from **C.1.2** to estimate the share of stunted, low-weight, and wasted children <13 years of age. To evaluate nutritional status we compared z scores for children's height-for-age (HAZ), weight-for-age (WAZ), and weight-for-height (WHZ) using USA norms from the National Center for Health Statistics (NCHS)[127]. Children with HAZ, WAZ, or WHZ more than 2 standard deviations (sd) < US median are nutritionally at risk. Stunting (HAZ<-2.0) reflects chronic, long-term mild/moderate under-nutrition. Wasting (WHZ<-2.0) reflects current acute under-nutrition. Low WAZ<-2.0 also indicates severe, acute under-nutrition. ~33% of children were stunted. If we include children 13-16 years old, the share of stunting rises to 40%, similar to other lowland Amerindian groups[128-131]. ~5% of children were low-weight (boys=4.8%; girls=6%) and <1% were wasted (boys=0.3%; girls=1%). The poor height growth of Tsimane' children is established early in life. Stunted children had significantly higher CRP levels than other children[130,132]. Anthropometric indices correlated with hemoglobin levels. Among boys, Hgb levels correlated positively and significantly with HAZ, WAZ, and age and sex-standardized Z scores of sum of triceps and subscapular skinfolds (ZAM), whereas girls showed significant associations with WAZ and ZAM[126]. Daily individual energy and protein intakes average 2912 kcal and 86.3 grams, suggesting the Tsimane' diet is sufficient to meet daily energy and protein requirements. Hence, stunting most likely reflects the role of other factors (e.g., parasites; dietary quality). *In sum, we find substantial stunting, which likely has long-run effects on cognitive skills and schooling[133-136]. Conditional on stature, we do not find much wasting, but we find correlations between anthropometric and biochemical measures of nutritional status.*

C.2.3. Parasites. Using two waves of panel data from the dry and rainy seasons (**C.1.4:** 7/2003-8/2003 and 12/2003-1/2004) from 193 subjects, we found that 85% had helminth or protozoa infection (73% had hookworm (*Necator americanus/ Ancylostoma duodenale*), 15% *Ascaris lumbricoides*, 7-9% *Trichuris trichiura*, and 5-8% *Strongyloides stercoralis)*. The pattern of helminth infection differs from patterns of other rural or poor urban South American populations because the Tsimane' have a lower prevalence of *Ascaris lumbricoides* and *Trichuris trichiura* but a high prevalence of hookworm. It is consistent with the high rates of intestinal parasitic infection commonly encountered among South American Indians[137,138]. The elderly and girls

6-10 years of age were all infected with some form of intestinal parasite; 95% of girls were positive for hookworm. Excluding infants, hookworm infection rates ranged from 60% positive in male children to 100% in the elderly. Infection rates among Tsimane' increase when children begin to walk, peak in late childhood and adolescence, and remain high throughout old age. Infection rates remain relatively uniform from dry to rainy seasons. Chronic helminth infection has been associated with malnutrition, iron-deficiency anemia, and poor cognitive function and development[139,140]. *In sum, we find widespread parasitic infections, which may explain the high levels of anemia and child stunting.*

C.2.4. Earnings. Tsimane' are highly autarkic and have low income. In **C.1.1** we measured all goods brought into the household (7am-6pm) on days chosen at random and found that market goods accounted for only 2.7% of total household consumption value. Among goods sold or bartered, rice ranked among the highest in value; it is a commodity that people sell or barter with ease, even in remote villages. Mean annual own income from the sum of earnings and the imputed value of consumption was US$342, a third of the national average ($980) [141]. Only 12.4% of people over the age of 16 in **C.1.4** reported having access to credit, suggesting that the population faces credit constraints. Adult women earned 19.7% of the cash income of their husbands, obtained in credit 18.6% of the amount of credit obtained by their husbands, owned 20.4% of the wealth owned by their husbands, and owned only 16.1% of the total value of household assets[142]. Although spouses pool food consumption, they keep separate physical assets and cash. Since 1999 we have tried to find instrumental variables for income and earnings to estimate reduced-form health demand functions. At various times we have used rainfall and/or temperature variability in the village[143,144], exogenous shocks to income (e.g., theft), or parental human capital, but have found unsatisfactory results because the instruments also correlated with the outcome. *In sum, data from the Tsimane' support other findings about the poverty of Bolivian Amerindians*[145-147]*. Poverty, credit constraints, and higher wages for men than women might encourage parental investments in favor of boys over girls* [82,87,148]*. The difficulty of finding reliable instrumental variables for income has induced us to use an experimental research design to introduce random variation in income across and within households. Female and male household heads keep assets and earnings separate, so selecting at random which head in a household (the female or the male) will receive the lottery prize might produce different patterns of expenditures and consumption, with implications for own and child health.*

C.2.5. Girl-Boy Differences in Nutritional Status. Since resource constraints induce parents to skew investments in favor of children of one sex[82], we might expect Tsimane' parents to invest more in sons than daughters since sons will receive higher earnings. Comparisons of age and sex-standardized anthropometric indices of short-run nutritional status between girls and boys reveal no statistically significant difference[128,142]. A possible reason for the absence of significant difference in girl-boy nutritional status is the custom of eating from a common pot, but another might have to do with parental investments to equalize health outcomes among children with different endowments. *In sum, we do not find differences in girl-boy nutritional status.*

C.2.6. Income Inequality. Data from **C.1.2** suggests considerable variation in income inequality across villages[8]. The mean village Gini coefficient of household cash earnings was 0.59 (sd=0.12; min=0.32; max=0.84). Owing to the difficulties of measuring income in highly autarkic settings, we used the following variables to proxy for income: **(i)** adult cash earnings from wage labor and the sale of goods, **(ii)** value of consumption of goods/person from own production, and **(iii)** value of goods/person received in barter. Since Gini coefficients are imprecise with a small sample size, we also used the coefficient of variation and the standard deviation of the log of income to ensure robustness in results, and found similar results as when using the Gini coefficient. In past work we have used total household income, household income/person, and household income/adult equivalent to estimate village income inequality. We tested several hypotheses about the determinants of village income inequality and found no evidence that the level of village income, population size, encroachment by outsiders, or distance from village to town correlated with village income inequality. *In sum, we find that villages differ in income inequality, but have not yet found the reasons for the differences.*

C.2.7. Correlates of Income Inequality. We correlated income inequality with indices of well-being, and found results that ran counter to findings from developed nations. For example, using data from **C.1.3** we regressed the annual amount of deforestation by households against standard covariates of deforestation plus village income inequality and found a strong negative correlation between village income inequality and household deforestation; results held up when using other measures of inequality besides the Gini coefficient [149]. A one-point increase in the Gini coefficient of village income inequality correlated with 9.8% less forest clearance by households. We hypothesize that in villages with more unequal income distribution the rich might be able to

impose their norms of conservation on other villagers, or perhaps offer employment and help the poor so the poor do not need to rely as much on the forest. Data from **C.1.2** and **C.1.4** suggest that adults in villages with greater income inequality had better anthropometric indices of short-run nutritional status even after controlling for social capital, schooling, age, sex, and proximity to town[10]. Contrary to expectations we find that village income inequality correlates with more individual displays of social capital (e.g., gift giving) and that displays of social capital correlate with worse anthropometric indicators. In **C.1.1** we asked whether people had suffered any unanticipated shocks (e.g., illness, crop loss), and, for each shock, we asked how people had coped. We found that 82% of households weathered the shock on their own, without help from others. Sharing and reciprocity permeate everyday life among Tsimane', but they might not necessarily become activated to protect people's health against large unanticipated individual or covariant shocks. Data from **C.1.4** suggests that adults in villages with greater income inequality reported more anger and sadness[150], with potential visible effects on blood pressure or immune function. The result dovetails with a recent experiment among non-human primates showing that researcher's manipulation of rewards produced an almost immediate increase in negative emotions [3]. *In sum, we find anomalies in the relation between income inequality, social capital, and health deserving further explorations. We have yet to collect data on self-perceived and objective stress, one of the paths by which income inequality presumably affects health.*

C.2.8. Social Capital. Researchers still disagree on how best to measure social capital [4,151-156]. In pre-literate, highly autarkic rural societies, social capital takes the form of gifts and services given to others or received from others[108,157,158]. We have only measured goods and services given to others, and find that social capital is widespread. Data from **C.1.3** suggests that only 4.5% of households did not make either any gifts or offer any help to other households during the previous week. Despite its ubiquity, social capital varies widely across households. We view social capital as an individual investment[5] and find that, as in the USA, people with higher income invest more in social capital and that investments peak at ~40 years of age. Unlike the USA, among Tsimane' we find strong group-level effects; village-level measures of social capital correlated positively and significantly with individual investments in social capital. *In sum, social capital is widespread, but we have yet to estimate: **(i)** how health status affects private investment in social capita, **(ii)** whether generosity flows from the rich to the poor, whether it follows kinship lines irrespective of income, or whether it takes into account both kinship and income, and **(iii)** whether results would hold up using other definitions of social capital, such as trust in other villagers unrelated by blood or marriage.*

D. RESEARCH DESIGN & METHODS

D.1. Background of Researchers. Our interdisciplinary team consists of: ***[1] Three biological anthropologists*** with experience collecting data on anthropometrics (**Leonard**, Professor and Chairman, Anthropology, Northwestern), biological markers (**McDade**, Assistant Professor, Anthropology, Northwestern), and intestinal parasites (**Tanner**, post-doctoral fellow, Anthropology, Northwestern) to assess the health of populations undergoing cultural and economic transition. Leonard, the PI, is an investigator whose prior funding has come largely from NSF. Since 1999 he has studied the effect of markets on nutritional status among the Tsimane'. He has done research on the health of Siberian herders, the influence of economic status on childhood growth among smallholders in Ecuador, and biological adaptation of Aymara Indians to high-altitude in Peru. He and Godoy organized an NSF-sponsored interdisciplinary workshop in 2003 on best practices for collecting panel data and in 2004 started an NSF summer training program for PhD students in anthropology on methods for collecting field data. McDade has received four NSF awards, including a PECASE award. He has developed and validated minimally invasive methods to assess health in populations undergoing cultural transitions. The PECASE award provides laboratory and field-training opportunities for graduate, undergraduate, and high-school students. Tanner did fieldwork among the Tsimane' during 6/2002-8/2002 and spent 12 months during 2003-2004 collecting data for her PhD on parasite infections and their links to anthropometric indices of nutritional status (expected PhD 12/2005). ***[2] Two ecological anthropologists*** who specialize on how markets affect the quality of life of tropical rain forest Amerindians (**Godoy**, Professor, Heller School, Brandeis) and on how lowland Amerindians use traditional plant knowledge to manage natural resources (**Huanca**, Research Associate, Northwestern). Godoy has worked with Amerindians in Bolivia since 1978, lowland Amerindians in Central America since 1992, and with lowland Bolivian Amerindians since 1995. Funded by several NSF grants, he has used the same methods to collect data across cultures to ease comparisons and generalizations. Huanca, a Bolivian Aymara, did his PhD among the Tsimane' and showed

they used traditional plant knowledge to manage fallow forest to increase its productivity. He has written widely on traditional healing among the Aymara. ***[3] An economist*** (**Behrman**, Kenan Professor of Economics and Director, Population Studies Center, U Pennsylvania) with experience collecting and analyzing panel household data from developing nations. Supported by NIH and NSF, Behrman has conducted considerable research on a range of topics related to human capital in developing countries, with particular emphasis on obtaining better empirical estimates of causal effects in the presence of endogenous behavior and imperfect data through a combination of improved data and econometric methods. Recent examples include using marginal matching methods to assess the impact of pre-school programs in Bolivia and using experimental data to evaluate the impact of scholarships on schooling and of nutritional supplements on infant and child growth in rural Mexico. ***[4] A pediatrician/social epidemiologist*** (**Goodman**, Professor, Heller School, Brandeis) with multidisciplinary training whose research has been funded by both NIH and private foundations. A board-certified specialist in Adolescent Medicine, Goodman, M.D., acquired multidisciplinary training as a post-doctoral student, first as a Robert Wood Johnson Clinical Scholar and then as a fellow in the Joint Program in Society and Health. Her early work focused on the gap between HIV-related knowledge, risk behaviors, and use of HIV testing by adolescents. Her more recent work, funded by NIH, focuses on: **(a)** the social determinants of child and adolescent health, obesity, and adolescent health risk behavior, with emphasis on the role of socioeconomic inequalities and **(b)** the development of theories to explain how class and gender affect adolescent's perceptions of health. Her publications rely on analyses of large data sets and on the ability to work as part of a large multidisciplinary team. With Leonard and Godoy, she used NHANES data to identify trends and determinants of variability in anthropometric indicators in the US during 1971-2002[11]. Leonard, Godoy, Huanca, McDade, and Tanner have years of collaborative research experience among lowland Amerindians. Their work has focused on the effect of markets on: **(a)** subjective and objective health[126,130,159], **(b)** nutritional status[128], **(c)** social capital[160], **(d)** economic inequality[8], **(e)** rates of private time preference[109,110,161], **(f)** happiness[162], and **(g)** parasite infections[163]. They have trained 5 PhD students in the collection and analysis of cross-sectional and panel data with environmental, socioeconomic, and public-health components. Besides being used by PhD students for dissertations, the field sites has been used by one masters and four undergraduates to write theses, by two MD students to do clinical rotations, and by an additional two undergraduates and three PhD students for summer training in field methods. *In sum, our skills and expertise complement each other. We have worked in Bolivia on health and other dimensions of well-being. We have studied social determinants of health in both developed and developing nations. We have been part of multidisciplinary teams and trained students in the field on methods of data collection. In this proposal we will bring our collective experience to the design of methods of data collection/analysis.*

D.2. Hypotheses. By running a lottery among households with income per household member below the village mean with rice as the prize, Interventions #1-2 will raise the income of households in the bottom half of the village income distribution and probably reduce village income inequality. Introducing random variation in household income and village income inequality will allow us to explore their effects on social capital and, through social capital, on the health of parents and their children – as well as through own-income on the health of household members in the households that win the lottery. Intervention #3 allows us to separate the effect of changes in income from the effect of changes in village income inequality. In contrast to what one might expect in industrial nations, in highly autarkic, low-income rural societies of developing nations the effects of income inequality on social capital are ambiguous. If generosity flows from the rich to the poor, then income transfers to the poor could reduce incentives by the rich to give to the poor, reducing the average level of social capital in the village (**Aim2: H1.a**). If generosity follows kinship lines irrespective of income, then income transfers to the poor could increase the average level of social capital in the village (**Aim 2: H1.b**). We are skeptical about the direct protective role of social capital on health for reasons discussed in **C.2.7**. This leads us to hypothesize that the level of income will likely protect health more than social capital (**Aim 2: H2.b**). At low levels of income, such as found among Tsimane' in the bottom half of the income distribution, income transfers should protect parental and child health (**Aim 2: H2.a**) because it will likely raise food intake and access to medical services, reduce energy demand from illness, thus improving energy balance (**Aim 2: H2.c**). Income transfers will have a greater protective effect on health among poorer households due to the convexity of the health production function (**Aim 2: H2.b**). Across villages with different levels of exposure to the market but with similar average levels of income, income transfers will have a weaker protective effect in remote villages because in those villages norms of sharing/reciprocity will cause people to share their income more

with other villagers, thus attenuating the protective role that own income might have had on own health (**Aim 2: H2.d**). We do not expect income transfers to the female versus male household head chosen at random to skew investments toward children of one sex because we do not find girl-boy discrimination (**C.2.5**), but we expect that income in the hands of female household heads will have stronger protective effects on child health because of the greater tendency of adult men to acquire commercial alcohol and cigarettes (**Aim 2: H2.e**).

D.3. Interventions. The three interventions will occur in 30 villages, with an additional 10 control villages. Villages will resemble each other in child stunting (<2 sd NCHS norms), a reliable index of general health. In 10/2006-11/2006 we will hold rice lotteries in 20 treatment villages among 50% of the households in the bottom half of the income distribution. Intervention #1: In 10 of the 20 villages, the households that win will receive a one-time prize in rice equivalent to US$170. Intervention #2: In another 10 of the 20 villages the households that win will receive a prize 50% higher (US$255). Interventions #1-2 will allow us to identify non-linearities in income responses, but they do not allow us to disentangle the effect of changes in income from changes in income inequality; Intervention #3 fills that gap. Intervention #3: In the remaining 10 of the 30 villages that receive treatments, we will increase the income of all households by the same percentage, leaving the village income distribution unchanged. We will set the percentage increase so the total transfer/village equals the transfer that would have occurred under Intervention #1. In each household receiving an intervention, we will select at random either the female or the male household head to receive the prize. The proposed prize should produce visible effects on social capital and health because it is large relative to current own income and also should allow us to detect non-linearities in responses to income. To do lotteries for Interventions #1-2, we will select at random chips with household names inscribed in them from a bowl, which will contain the names of all households in the bottom half of the income distribution. We will select at random half of the chips in the bowl, and, for each household chosen, we will toss a coin to decide whether the female or the male household head receives the prize. To increase transparency we will hold rice lotteries in public so all villagers know who won and how much. Since villages are small (mean= ~18 households) and ~45% of houses lack complete wall enclosures, most people will find out who won the lottery even if we were to communicate only with the winner; doing the rice lottery in public probably will not likely affect social pressure to share the prize. We use rice instead of cash because rice is the most fungible commodity in the area and stores well for over a year. Since our study lasts 24 months, we can measure only short-run effects. We will do two surveys after the intervention: one in 1/2007-3/2007 three months after the intervention to measure very short-run effects, and one during 10/2007-12/2007, a year after the intervention, to capture longer-run effects. The prizes are large relative to average household income. For instance, the lower of the two prizes, $170/household, represents half the annual income of the average Tsimane' and ~10% of annual household income.

D.4. Control Samples. The control sample includes 10 villages with similar levels of child stunting as the treatment villages. At the end of the study all households in control villages and all households in treatment villages that did not win prizes will receive workshops on hygiene and health and 12 fishhooks/household; women and men of all ages and children use fishhooks.

D.5. Modeling and Analysis. The first step in the analysis will be to verify the experimental design by testing for differences in distributions of measured variables between poor treated households (that receive the income transfer) and poor control households (that do not receive the income transfer) before the lotteries. The next step will be to test for impacts of income transfers on health by using difference-in-difference estimators. In this step we also will control for observed attributes in case the assignment of treatment is not completely random. We also will re-weight observations to compensate for sample attrition (i.e., we will predict a latent attrition propensity on the basis of the baseline characteristics and knowledge from subsequent rounds about who leaves the sample and then use this propensity to re-weight observations to account for attrition[164,165]). To be explicit, consider the linear approximation of the reduced-form relation for the determination of child health (parallel considerations hold for the path variables in D.9.2) in which the subscripts "h" and "c" refer to the hth household in the cth community. H_{hc} is a child health outcome, Y_{hc} is household income level, Y_c is the mean community income level, G_c is a measure of community income inequality (e.g., Gini coefficient), X_{hc} is a vector of other household characteristics, X_c is a vector of other community characteristics, and e_{hc} is a stochastic shock (independent of other righ-side variables) that affects child health:

$$(1)\ H_{hc} = aY_{hc} + bY_c + cG_c + dX_{hc} + eX_c + e_{hc}$$

Standard estimation of relation (1) is likely to lead to biased estimates of the coefficients of interest because there are likely to be unobserved components of X_{hc} (e.g., parental abilities, parental concern about their

children's health) and of X_c (e.g., community health environment, weather, prices) that are correlated with household and community incomes. But if there are randomly assigned changes in incomes through the lottery, these are independent of not only unobserved fixed household and community characteristics but also unobserved time-varying household and community characteristics, so consistent estimates of the parameters *a, b* and *c* can be obtained by estimating:

(2) $\Delta H_{hc} = a\Delta Y_{hc} + b\Delta Y_c + c\Delta G_c + d\Delta X_{hc} + e\Delta X_c + \Delta e_{hc}$,

where Δ refers to the change from the time period right before the lotteries to a survey after the lotteries (with separate estimates for 3 and 12 months after) and with ΔY_{hc} the change in household income due to the lotteries, ΔY_c the change in community mean income due to the lotteries, and ΔG_c the change in community income distribution due to the lotteries (alternatively, the changes due to the lotteries can be used to instrument the actual measured changes). In light of this relation, simple comparisons of changes in child health among the three control and the treatment groups permit some inferences: Comparing Intervention 1 or 2 with the control gives an estimate of the combined impact of the individual household, mean community income and income distribution changes, comparing Intervention 3 with the control gives an estimate of the combined impact of the individual household and mean community income changes, comparing Interventions 1 and 3 gives an estimate of the impact of focusing on poor households and changes in income distribution for the same mean community income change. But more precise estimates can be obtained, conditional on the model in relations (1) and (2), by combing the data from the three treatment groups and the control group to estimate directly relation (2), with the parameters *a* and *b* being dependent on the treatment received and the baseline income to test whether there are nonlinear income effects[209-210].

D.6. Implementing the Intervention. We have a list of all Tsimane' villages with their location and population size. Using the list, we will exclude two types of villages: **(a)** those that have been part of previous studies or that are part of study in progress and **(b)** those that have fewer than 8-10 households because measures of village income inequality will be of less interest with so few households. From the remaining villages we will select a random sample of ~45 villages. During 7-9/2006 we will do a baseline survey among all subjects in the 45 villages to measure health, social capital, and, for subjects over 13 years of age, income using definitions ***i-iii*** of income in **C.2.6**. During the baseline we will survey five additional villages beyond the 40 needed for the study; we do so as a backup should some villages refuse to take part in the study. Among the sample of 30 treatment villages we will identify households in the bottom half of the income distribution. During 10-11/2006 we will carry out the three interventions. Since villagers take collective, unanimous decisions, we expect villagers, as a group, to either agree or decline to take part in the study. If villagers decline to participate, we will identify their reasons, try to incorporate their concerns, and search for other villages with similar levels of child stunting. We will not provide incentives to villages that refuse because incentives might create problems of perceived fairness. **E.3.1** has more details on the recruitment strategy. Even though results of lottery studies suggest that windfalls do not produce adverse effects (**B.4**), we will monitor unanticipated effects (**D.9.5**)

D.7. Sampling.

D.7.1. Reasons for Selecting the Tsimane'. (**a)** The Tsimane' are representative of other Amazonian Bolivian Amerindian societies in subsistence and social organization [108]. **(b)** They vary widely in income and income inequality[166]. **(c)** The area is safe for researchers. **(d)** We have 10 yearss of continuous research experience (and research in progress) with them and know how to deal with a wide range of logistical problems (**C.1-C.2**).

D.7.2. Criteria for Selecting Villages. See **D.6**.

D.7.3. Sample Size. The sample consists of 40 villages, split between 30 villages that will receive treatments and 10 that will serve as controls. In 10 of the 30 treatment villages we will offer a prize of $170/household, in another 10 we will offer a prize of $255/household, and in the last 10 villages we will increase the income of all households by the same percentage. Since a typical village has ~18 households and we give the intervention to **(a)** a random sample of 50% of the households in the bottom half of the village income distribution in 20 villages (Interventions #1-2) and **(b)** to all households in an additional 10 villages (Intervention #3), our final sample will contain a total of ~270 households (50%*(18/2)*20+18*10) that will receive a treatment and ~450 households that will either be in treatment villages but do not win (n=270) or that are in the 10 control villages (n=180). Since the average Tsimane' household contains six people, evenly split between children (<13) and adults (13≥), women and men, the sample will contain 1620 subjects in treatment households (405 subjects for each of the following demographic groups: women, men, girls, boys) and 2700 subjects in control households

(675 subjects for each of the following demographic groups: women, men, girls, boys) yielding a total of 4320 subjects who will be surveyed three times. A sample of 40 villages may be too small to estimate the main effect of village-level variables on individual health with precision, though it likely yields sufficient variation to estimate with some precision interaction effects with individual characteristics (e.g., sex, age) and household characteristics (e.g., schooling of household heads, pre-experiment income levels). We will explore such interactions in our estimates of relation (2). The sample is also large enough to estimate with some precision the effect of own household income on household members' health. We limit the analysis to 40 villages because of the budgetary constraints of an R21 and because at this stage we only want to carry out preliminary exploration of hypotheses. Data from **C.1.2** suggests that 21 of the 511 households (4%) had only one head (men=13; women=8), so randomizing the prize within each household between the female or the male head should yield roughly equal numbers of female and male winners.

D.8. Potential Biases: (a)Attrition. We expect some attrition because Tsimane' move in search of better foraging and farming grounds (**C.1.3**)(**Aim 3**). We will compare baseline characteristics of migrants and non-migrants and, as noted in **D.5**, re-weigh the sample to reflect any selective migration. **(b)Respondent burden.** Since we survey people only three times, we do not expect many refusals from respondent burden[167]. **(c)Hawthorne effects.** Parents might change their investments in child health as a result of the attention given by researchers. To assess possible Hawthorne effects[136], we will collect data on the importance subjects attach to being part of the study. Such questions will also allow us to proxy for civic mindedness and likelihood of attrition. The sample size of villages in this proposal is too small to include another intervention to directly assess Hawthorne effects. **(d)Other measurement errors.** To reduce measurement errors, after the first survey we will not change data collection formats or rotate surveyors across villagers, and will follow the same hierarchy of proxy respondents for children in all surveys **(D.9)**. Using palm pilots to collect field data should lower errors when transcribing data from paper surveys to computers (**D.10**).

D.9. Methods to Collect Data. We have used most of the methods proposed here, but in **D.9.1.3** we discuss how we will validate methods. **Appendix A** contains a survey draft. The interventions will take place during 10/2006-11/2006. We will do three surveys: **(a)** a baseline survey (7/2006-9/2006) before the intervention, **(b)** a follow-up survey (1/2007-3/2007) three months after the intervention to measure short-run effects, and **(c)** a final survey (10/2007-12/2007) a year after the intervention to assess longer-run effects. Tsimane' whom we have trained will assist in the field. We will interview people directly, but for young children who cannot answer, we will use the following hierarchy of proxy respondents: mother, proxy respondent in previous round, any previous proxy respondent, and first time proxy.

D.9.1. Dependent Variables: Perceived and Objective Health Indices. We measure subjective and objective health because income inequality might affect both (**B.2**). We measure perceived and objective health in different ways to ensure robustness in results.

D.9.1.1. Perceived Health. We will adapt the Child Health Questionnaire because it is valid for children and has been translated for and tested in many cultures[168].

D.9.1.1.1. In the first survey we will use a short version of the well-child checkup questionnaire[169] that includes three questions for the child and her/his principal parental caretaker: **i)** How would you rate your child's health? **ii)** Has the child ever had an illness that required him/her to seek treatment outside the village and, if so, what type of illness, for how long, and does the child still suffer from that illness? **iii)** How often has the child broken her/his bones or had a serious cut or burn? For question **i** we use the standard self-rated 5-point scale: excellent=5, very good=4, good=3, fair=2, poor=1. For question **ii** we will start with a code list of 152 ailments we have developed in the research site over the years and that we include at the end of **Appendix A**. We code question **iii** by the number of times subjects had a serious cut, burn, or broken bone. We measure a child's subjective view of health and how parents view the child's health because the two views likely correlate less than perfectly[170-172]. In the first interview we will ask parents about the child's vaccination history and ask to see written vaccination records.

D.9.1.1.2. In the first survey we will ask all subjects to answer for the seven previous days: **i)** How many days did you feel ill? and **ii)** How many days were you confined to bed? Question **i** lets us obtain a subjective view of illness. Question **ii** measures the severity of illness and allows us to assess subjective views of severity. We will ask subjects 13+ years of age the frequency they consumed commercial alcohol and the number of cigarettes consumed during the seven days before the interview.

D.9.1.1.3. After the first survey, when we take blood, we will ask the questions in **D.9.1.1.2** plus: **i)** On a 1-5 scale (1=poor; 2=fair, 3=good, 4=very good, 5=excellent), how has your (or your child's) health been during the last three (first survey) or six months (second survey)? **ii)** On a 1-3 scale, how concerned are you about your child's health, behavior, or learning? (1=not; 2=some; 3=very); we ask question **ii** only of children's caretakers.

D.9.1.2. Objective Health: Biomarkers and Anthropometric Indices of Nutritional Status.

D.9.1.2.1. Biomarkers of Health Status. We will take systolic and diastolic blood pressure with a Lohmeier B-606 self-inflating digital monitor, which has done well in validation studies [173]. Blood pressure will be measured three times while the subject is seated and relaxed. The first measure will likely be elevated because of the novelty of blood pressure monitoring in this population, and our analyses will focus on the average of the final two readings. We will take blood pressure from children during the first survey to assess whether to take blood pressure in later rounds. We will use a finger prick to collect 3-4 drops of whole blood, a relatively painless procedure we already have used. The 1st drop will be used to assess hemoglobin with a portable HemoCue instrument. Remaining blood drops will be collected on standardized filter paper to diffuse and dry whole blood at a constant, uniform rate. Dried blood spot samples remain stable for several weeks, and will be shipped to Northwestern where they will be assayed for CRP using previously validated blood spot protocols[123,174]. A nurse we have trained will collect blood samples and take blood pressure. Section **D.9.1.2.1** of Appendix A describes how we will take blood samples and blood pressure.

D.9.1.2.2. Anthropometric Indices of Nutritional Status. We will take the following anthropometric measures to assess nutritional status: stature (cm), body weight (kg), mid-arm circumference (cm), and skinfolds (triceps, biceps, subscapular, suprailiac; mm). We will measure people in light clothing without shoes/hat[175]. We will use a portable stadiometer to take linear growth measures, a standing scale for weight, plastic tape measures to measure mid-arm circumference, and Lange callipers to measure skinfold thickness. From raw anthropometric measures, we will derive the following indices: **(1)** body mass index (BMI), **(2)** upper-arm muscle area (UMA), and **(3)** percent body fat. BMI is calculated as weight (kg)/height (m^2). UMA (cm^2) is estimated from mid-arm circumference and triceps skinfold measures using the equation in Frisancho[176]. Percent body fat in adults is estimated from the sum of four skinfolds using the predictive equations of Durnin and Womersley[177]. For children, height-for-age, weight-for-age, and weight-for-height measures will be standardized as z scores relative to the norms of the USA NCHS[127,178]. Among adults (18+), BMI and body fatness will be used as indices of short-run nutritional status. Z scores of arm-muscle area and sum of triceps and subscapular skinfold for all subjects will be estimated relative to USA norms of Frisancho[176].

D.9.1.3. Validating Perceived and Objective Health Measures. To validate perceived and objective health measures, we will explore: **(a)** the total range of variation in the various measures, **(b)** within and between village variation, and **(c)** correlation of perceived and objective measures. For **(c)** we envision three types of households: those with consistent high/good or low/poor measures, and those with mixed measures. Since variance in outcome is as important as the average, for subjects and households we will create composite indices for average health, variance in measure, and concordance of different measures. This will involve converting all health measures into standardized z scores relative to age- and sex-specific medians for the entire populations. Overall health status will be reflected by the mean z score of the indices, whereas the relative concordance of the health measures (i.e., are the measures consistently good or bad, or do they vary much) will be reflected in the variance around the mean value.

D.9.2. Path Variables. Changes in village income inequality due to the lotteries might affect health through three paths: **(a)** social capital, **(b)** cultural dissonance, and **(c)** the provision of public goods that enhance health. **(a)-(b)** affect health through stress **(d)**; **(c)** affects health directly. Furthermore, changes in own income due to the lotteries affect health of the individual through physical activity **(e),** expenditures other than on food **(f),** and food consumption **(g)** (and of other household members through **(f)** and **(g)**).

D.9.2.1. Social Capital. We will measure social capital in several ways. (1) We will ask subjects about the frequency of gifts and labor help given and received, including communal labor and participation in community meetings, during the week before the day of the interview. The measure builds on our work in progress but goes beyond it by asking about the goods and services received and by asking subjects to identify the person who gave him/her the gift/help or who received the gift/help from the subject. Asking about the identity of the person who received or offered the gift/help allows us to assess how much of social capital follows kinship line

or takes place among unrelated people with different incomes. (2) To facilitate cross-cultural comparison, and to address the concerns of a reviewer about excessive reliance on only one definition of social capital, we add questions from the General Social Survey(GSS) of the USA that researchers have used to assess social capital[5-7]. We will add two questions from the GSS about people's perceptions of lack of fairness and social mistrust: (a) "Do you think most people would try to take advantage of you if they got a chance or would they try to be fair" (perceived lack of fairness) and (b) "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" (social mistrust). (3) Other measures of social capital include: (a) attendance at village meetings, (b) number of people spouses could leave their children if spouses had to leave the village for 2-3 days, and (c) frequency and amount of goods borrowed from neighbors. We already collect data on (c) but need to pilot test (a-b) and the questions of the GSS.

D.9.2.2. Cultural Dissonance. A measure of cultural dissonance will be constructed in three stages. (1) Focus groups comprised of Tsimane' participating in the current panel study, but not in the proposed study, will be asked to list the attributes that define a successful or well-respected person in their community. Based on previous work in other cultures[68,69,71,123,179], and our previous experience with the Tsimane', we anticipate that the list will include items such as the following: ownership of tools, equipment, and clothing, consumption of game meat, generosity, and adequate farmlands. (2) We will recruit 40 new individuals (20 men, 20 women, 20 from remote villages, 20 from less remote villages; also from the previous and current panel study, but not from the proposed study). These individuals will be presented with the list of items, and asked to rate on a five-point scale how important each item is to defining success in their community. The results will be analyzed with Anthropac software, using a modified factor analysis protocol that provides a quantitative assessment of the degree of agreement or consensus among the Tsimane' with respect to the attributes that define success in their culture. (3) We will generate a survey instrument based on these results and administer it in the proposed survey. This will allow us to rate the degree to which each subject over the age of 13 attains the local culturally defined ideal for success. Low scores on the scale indicate a higher degree of cultural dissonance, which we anticipate may mediate the association between income inequality and stress.

D.9.2.3. Provision of Public Goods. We will ask adults how often in the previous month outside encroachers, such as loggers, ranchers, or colonist farmers entered their lands, and what, if anything, subjects did to evict them. Recall that 44% of households had interacted with an encroacher during the previous month, so a recall period of one month will likely produce enough variance. Answers will be coded as: **(1)** did nothing, **(2)** called the police or town authorities, **(3)** organized a delegation from the village to try to persuade encroachers to leave, and **(4)** organized villagers and used force to evict them. We have used the question since 1996 to assess encroachment and the effectiveness of villagers in curbing encroachment[180].

D.9.2.4. Perceived and Objective Stress (Salivary Cortisol Sampling). Perceived stress will be measured using the Perceived Stress Scale, a global measure of stress which assesses frequency of stressful situations during the past month as well as perceptions of stress, and has been shown to be reliable and valid[39]. The measure has been used in studies of socioeconomic status' influence on host resistance and the relationship between psychological stress and susceptibility to upper respiratory infections in the US[181,182]. The questions will be translated and assessed for internal consistency by administering the survey to ~20 subjects twice, one week apart. We will collect salivary cortisol samples from 100 adults and 100 children, evenly split between females and males across the three treatment groups and the control group. Data will be collected at baseline and in the first post-intervention survey; during each survey, we will collect cortisol on two different days selected at random for each participant. We limit the collection of cortisol to only 200 participants, to only two days/participant during each survey, and to only the first post-intervention survey because of the additional cost ($60/participant/survey with cortisol taken on two days) and because we are unsure about the feasibility of collecting such data in a small-scale, pre-literate rural society. On days when we collect cortisol, we will ask participants to expel their saliva through a straw into a 1.8 ml cryogenic vial pre-treated with sodium azide as a preservative; only 50uL of saliva are required, but participants typically provide much more than that amount. No stimulants or absorption material will be used, and participants will be instructed not to eat, drink, or brush their teeth in the 30 minutes before obtaining each sample. Salivary cortisol levels are unaffected by the motion and temperature variations typically associated with sample storage and shipment[183]. Participants will be told that the timing of the salivary cortisol samples is critical. They will be asked to place the collection kit by their bed and to take the first sample in the morning immediately upon awakening, the second sample 30 minutes

later, and the third immediately prior to going to bed. Participants will be given a small mechanical 30-minute timer to set immediately after taking the wake-up sample to ensure they take the second sample on time. Because cortisol values tend to be low and stable in the evening, the timing of the bedtime sample is less critical. Samples will be analyzed in the Laboratory for Human Biology Research at Northwestern University using a previously validated EIA protocol (Salimetrics, State College, PA). Salivary cortisol sampling has been implemented with success in prior community-based studies, both in the USA and overseas, and is intended to provide the maximal amount of data on HPA axis functioning with the simplest possible collection protocol[184,185]. From the 3 samples we will be able to estimate three HPA axis parameters: slope of the diurnal cortisol curve, average cortisol level across the waking day, and the cortisol response to awakening. Each of these basal cortisol parameters is predicted by stress exposure and has been previously related to mental and/or physical health outcomes[186,187 188,189].

D.9.2.5. Physical Activity. Daily activity patterns and energy expenditure will be assessed using a modified version of Bouchard's protocol[190]. During each survey, we will ask subjects >10 years of age about their activities during the previous 24 hours and the duration of each activity. We limit the sample to subjects >10 years of age because questionnaires of daily activities are unreliable for children ≤10[191]. The instrument has been validated with children[192], including Ameridian children[193]. Individual activities will be converted to metabolic equivalents (METs=kcal/kg body weight/hr) based on Ainsworth's compendium of physical activities[194]. Daily energy expenditure (kcal/day) is determined by summing activity-specific METs for the entire day. Leonard has successfully used the method to quantify energy expenditure in traditional rural populations of Ecuador[195] and herding populations of Siberia[196]. In both populations estimates of daily energy expenditure from this protocol correlated strongly with those from the flex-heart rate method[195,196]. As in adults, heart rate monitoring and accelerometers can effectively quantify energy expenditure in children[197].

D.9.3. Categorizing Variable: Income. Since income comes from earnings and the value of goods consumed from people's own farms/forests, we will measure the value of earnings and consumption and add them to obtain an estimate of annual income. First, we will measure consumption through weigh-days and surveys of daily extraction, the first being more accurate but time consuming than the latter. On two days chosen at random/semester (irrespective of the day of the week, but excluding feast days), we will identify, weigh, and value all non-market goods entering households (7am-6pm) and ask subjects bringing the good about the provenience of the good, time spent extracting the good, and mode of procureremen[198]. We have developed reliable methods to value non-traded goods[199]. As a check on weigh days, we will use surveys. Through surveys at the end of the day we can query people about all the forest and farm goods extracted that day. Second, we will ask about the sources and amount of cash earned during the month before the interview, days worked, length of working days, daily wage received, and remunerations besides cash (e.g., food). If earnings came from the sale of goods, we will ask about the type, quantity, and price of goods sold and the place and total value of transactions. We will use measured income to generate indices of village income inequality expressed as household income/person, household income/adult equivalent, and total household income.

D.9.4. Control and Instrumental Variables. Controls include age, sex, birth order, schooling, measures of modern and traditional human-capital skills[113,200,201], village price of selected food and health inputs, and village-to-town distance measured with a GPS and travel time. Drawing on methods from **C.1.1**, we will collect daily data on rainfall, temperature, and cloud cover to control for the effects of weather shocks on income and health and for use as instruments when we explore whether there are interaction effects with pre-experiment income. The basic instrument for income changes after the baseline, as noted in section **D.5**, is the receipt of lottery income.

D.9.5. Variables to Monitor Effects of Lottery. We will ask subjects about any conflict between their household and other household and within their household. We will also ask them about conflicts they may know about in other households. Asking subjects about the behavior of third parties yields reliable data on events when subjects are unwilling to tell the truth about their own behavior[202,203].

D.10. Data: Entry, Quality Assurance, and Access. To reduce measurement errors when transcribing data from paper surveys to the computer, we would like to test the use of the palm-pilot to collect field data. We have found mixed evidence about the usefulness of the palm pilot[204,205], and would like to test how it might work in the humid tropics. After the study ends and we have cleaned the data, we will put it in ACCESS plus the data dictionary in English and Spanish, and other supporting documentation on the web site of

Northwestern, with links to U Penn and Brandeis. Supporting documentation will include codebooks, history of the project, instructions for how to link different files, and PDF files of background papers. In **E.3.2** we discuss steps to guarantee subject confidentiality. We will spend the last three months (1/2008-3/2008) cleaning and documenting the data so it is ready for analysis before the end of the project. We will use the last three months to do focus groups about feelings of fairness and exclusion from the treatment.

D.11. Allocation of Tasks. All researchers will be in the field for a week at the start of the first survey to pilot test the modules falling under their expertise: Leonard (anthropometrics, food consumption), Goodman (perceived health), McDade (biomarkers, cultural dissonance), Tanner (physical activity), Behrman (income, expenditures), Godoy (social capital, attrition, social effects), Huanca (control and instrumental variables). Leonard, Godoy, McDade, and Huanca will take part in the trial lotteries to introduce the idea of randomization to villagers to obtain informed consent. Huanca, Tanner, Leonard, and Godoy will supervise the collection of data. Tanner and Huanca will rotate their stay in the field site so at least one of them is in the field for the entire duration of the study. After the first survey, all researchers will report on preliminary findings, comment on improvements in the survey design, and discuss unanticipated logistical and substantive issues that may have arisen during the study. During their annual visits to Bolivia, Leonard, Huanca, McDade, and Godoy will arrange for meetings with medical researchers in Bolivia to discuss the feasibility of submitting an RO1 (**Aim 5**). In addition, we will have one nurse who has already worked with us take blood samples and blood pressure. Leonard and Godoy will be in the field three months/year to supervise the collection of data; Leonard, Godoy, and Huanca/Tanner will have a telephone conference call once/month to monitor the project.

D.12. Analytic Methods. Data will be analyzed in three stages. We will first compute indices of income inequality using different indices of inequality because the choice of index may matter in the analysis[19,48]. Second, we will do descriptive, bivariate analysis of the relation between income, income inequality, path variables, and outcome variables. In the third stage we will assess the impact of the intervention by: **(a)** comparing changes in mean health outcomes between treatment and control groups, before and after the intervention and **(b)** estimating regressions for relation (2). Weighted multi-level regressions will be used since people are nested in households and households in villages.

D.13. Limitations. (a) The use of the village as the geographic unit to compute indices of income inequality may be too small; inequality may matter but only when computed over a larger area[19]. **(b)** We measure effects 3 and 12 months after the intervention and so may not allow enough time to elapse to observe the effects of changes in income inequality on path variables and adult health, though we hypothesize that the effects on child health should be visible in the short run[206]. **(c)** Because Tsimane reside in small villages where most activities are public knowledge and because we hold the lottery in public, we will only be able to assess the effects of lotteries or income transfers when transfers are public knowledge; people might use windfall income differently if neighbors do not know who won the lottery. **(d)** Since people appear to put income in different "mental accounts" and are more likely to share windfall gains than earned income, we cannot tell from our experiment whether earned income might have had the same effects on health as windfall income (**B.5**).

D.14. Summary. The study of income inequality and health has taken the center stage of many policy and academic debates in developed nations. Previous studies have produced inconclusive results owing to biases from reliance on secondary, observational data not designed to estimate the links between income inequality, social capital, own income, and own health. In this pilot study we propose to use an experimental research design in a low-income, rural population of indigenous peoples in the Bolivian Amazon. We will manipulate the level of individual income and income distribution in villages and assess their effects on social capital and individual health. The use of children and data from a small-scale, pre-industrial, highly autarkic society should make it easier to estimate the effects of income inequality and individual income on health. *In sum, with this R21 we hope to evaluate the following issues before submitting an RO1:* ***a)*** *estimate non-linearities in income responses that may affect the desired magnitude of the transfer for the larger study that we hope to undertake,* ***b)*** *ascertain how successfully migrants can be followed up and estimate the magnitude of attrition,* ***c)*** *do preliminary exploration of hypotheses that are to be examined in the RO1 to rule out variables and hypotheses with low explanatory power,* ***d)*** *identify adverse effects of the intervention, and* ***e)*** *pilot test methods to measure subjective and objective stress and social capital, and the use of palm pilots to collect field data.*

E. HUMAN SUBJECTS RESEARCH. Past work and work in progress in Bolivia funded by NSF have received approval from the Institutional Review Boards of Northwestern and Brandeis (Northwestern IRB approvals: **[1]**

1053-001, "Markets integration and health among the Tsimane' of lowland Bolivia"; **[2]** 0732-007, "Modernization and it discontents". Brandeis IRB approval: **[1]** 01-03-007: "Effects of economic development in the clearance of old-growth forests among Tsimane' Amerindians, Bolivia"; **[2]** 04-02-017, "Summer field-training program in methods of data collection, Bolivia and Zambia". At this time, IRB approval from Northwestern for the proposed project is pending.

E.1. Women and Minority Inclusion in Clinical Research.

E.1.1. Inclusion of Women Plan. We will survey all villagers in treatment and control groups, so the share of female and male subjects should be similar.

E.1.2. Inclusion of Minorities Plan. The research proposal focuses on an understudied population – indigenous Amerindians. Indigenous peoples account for 62-81% of Bolivia's population[207,208]. We will study one ethnic minority group – Tsimane' -- who make up ~2% of the indigenous population of the Department of Beni[99].

E.1.3. Inclusion of Children's Plan. Much of this proposal concerns children. We will study all children (defined as people below 13 years of age) in the 40 villages of our sample. This should ensure that our sample has roughly equal shares of girls and boys. A pediatrician in our research team, Goodman, has professional expertise dealing with children of this age group. The sample size of children, 2160, is large and accounts for half of all subjects.

E.2. Risks to the Subjects.

E.2.1. Human Subject Involvement and Characteristics. The two adjoining NIH tables entitled "Targeted/Planned Enrollment Format Page" and "Inclusion Enrollment Report Format Page" contain information on the broad sample characteristics for the population. The target study population consists of 4320 Tsimane' participants from 40 villages in the Department of Beni, Bolivia. All subjects (girls, boys, women, men) in each village irrespective of whether the village is a treatment or a control, will be surveyed. We include children because **Hypothesis 2.e** (**Aim 2**) focuses on children<13 years of age. We include all adults irrespective of sex because many hypotheses deal with adults.

Targeted/Planned Enrollment Table
This report format should NOT be used for data collection from study participants.

Study Title: Inequality, social capital and health: Bolivia

Total Planned Enrollment: 4,320

TARGETED/PLANNED ENROLLMENT: Number of Subjects			
Ethnic Category	**Sex/Gender**		
	Females	**Males**	**Total**
Hispanic or Latino	2160	2160	4320
Not Hispanic or Latino			
Ethnic Category Total of All Subjects*	2160	2160	4320
Racial Categories			
American Indian/Alaska Native	2160	2160	4320
Asian			
Native Hawaiian or Other Pacific Islander			
Black or African American			
White			
Racial Categories: Total of All Subjects *	2160	2160	4320

*The "Ethnic Category Total of All Subjects" must be equal to the "Racial Categories Total of All Subjects."

Inclusion Enrollment Report Table

This report format should NOT be used for data collection from study participants.

Study Title: Inequality, social capital and health: Bolivia

Total Enrollment: 4320 **Protocol Number: _____**

Grant Number: _____

PART A. TOTAL ENROLLMENT REPORT: Number of Subjects Enrolled to Date (Cumulative) by Ethnicity and Race				
Ethnic Category	**Sex/Gender**			
	Females	**Males**	**Unknown or Not Reported**	**Total**
Hispanic or Latino	2160	2160		4320
Not Hispanic or Latino				
Unknown (Individuals not reporting ethnicity)				
Ethnic Category: Total of All Subjects*	2160	2160		4320 *
Racial Categories				
American Indian/Alaska Native	2160	2160		4320
Asian				
Native Hawaiian or Other Pacific Islander				
Black or African American				
White				
More than one race				
Unknown or not reported				
Racial Categories: Total of All Subjects*	2160	2160		4320 *

PART B. HISPANIC ENROLLMENT REPORT: Number of Hispanics or Latinos Enrolled to Date (Cumulative)				
Racial Categories	**Females**	**Males**	**Unknown or Not Reported**	**Total**
American Indian or Alaska Native	2160	2160		4320
Asian				
Native Hawaiian or Other Pacific Islander				
Black or African American				
White				
More Than One Race				
Unknown or not reported				
Racial Categories: Total of Hispanics or Latinos**	2160	2160		4320 **

* These totals must agree.

** These totals must agree.

Provided subjects are Tsimane', no subject will be excluded because of gender, pregnancy status, disability, or ethnicity. This study does not involve fetuses, prisoners, or institutionalized individuals. *In sum, the study involves only American Indians, both genders, and children and adults. The sample includes equal representation of females and males, children and adults.*

E.2.2. Sources of Material. Research material will include primary data from interviews, anthropometric measures, blood pressure, and blood and saliva specimens collected from children and adults.

E.2.3. Potential Risks. The empirical literature reviewed in **B.5** suggests that lotteries do not produce adverse effects. We will nevertheless proceed cautiously through the following steps to ensure the experiment does not cause discord. ***1)*** Before the baseline, we will explore with villagers if they want to be part of the study. We will try to improve the intervention based on their feedback. ***2)*** For villages that show an interest in participating, we will hold a total of six trial lotteries before the intervention to assess whether prizes cause discord and to ensure villagers understand well the meaning of randomness (see next paragraph).If a village declines to participate, we will search for another village with similar levels of child stunting. ***3)*** If villagers accept the study, we will carry out the experiment but monitor possible adverse effects (**D.9.5**). Other risks are minimal since we only want to collect socioeconomic and anthropometric data and to collect a few drops of blood and saliva. We have trained a local nurse to draw drops of blood and have found the procedure is relatively painless. We have taken over 3,000 samples of blood, and have not encountered a single case of resistance, fainting, infection, or any other adverse effect. Most of the questions we plan to ask we have asked before in many surveys and in many ways, so we do not anticipate the questions will cause discomfort or embarrassment. We stress in our village meetings and in our one-on-one interviews that the interviews are voluntary. The project will not involve any cost to participants except for the time they have to give up to answer the interview questions twice a year.

As the proposal makes clear, we have been working with the Tsimane' since 1999 and have built excellent working relations with them. Since the core of our work centers on the construction of a long-term panel data set, we have a long-term personal and professional commitment to the people and area that we would not like to jeopardize in addition to our ethical concerns. To redress possible risks to human subjects we will take five steps beyond the ones already outlined in the initial application. **(1)** We will make it clear from the outset we will not exclude males from the lottery to increase the perceived fairness of the lottery. Qualitative evidence from PROGRESA, a program in Mexico which gave income transfers *only* to women, suggests that the program produced some intra-household strife because men felt excluded. Our approach represents an improvement over PROGRESA because we do not select one sex for special treatment [15]. **(2)** To avoid feelings of exclusion, under Intervention #3 discussed earlier we will include all households in the village. Through focus groups during the last three months of the study we will compare measured feelings of inclusion between this and the other two treatment groups. **(3)** We will hold four small public lotteries at the village level and two small private lotteries at the household level during the month before the interventions to increase people's understanding of lotteries and to assess whether lotteries might produce strife. For the public lotteries we will assemble all villagers in an open yard, assign each household a different chip, put the chips in a bag, have a child (rather than outsiders or authority figures) select some chips at random, and award three fishhooks to winning households. After the lottery, we will discuss why some households won to ensure villagers understand the concept of randomness. We will stress that we do not have enough resources to award a prize to all households; hence the need for a lottery. We will repeat the lottery on three different days in front of all villagers. By holding a total of four public lotteries before the intervention and by having a discussion after each lottery, we will enhance people's understanding of lotteries and the randomness of winning. More importantly, by sequencing the four lotteries over a month, we can use lotteries 2-4 to discuss what happened to the winning or losing households of the previous lotteries, and explore with villagers if lotteries produced strife. We will then hold two additional lotteries in the privacy of each household to give husbands and wives a more private opportunity to ask questions about the lottery. With both wife and husband present, we would assign each one of them a chip of a different color, put the two chips in a bag, and have a child draw one of the two chips at random, with the winner taking three fishhooks. On a different day we would repeat the lottery and ask how the couple felt about the previous lottery. During the last private and public lottery we will ask participants whether they had lingering doubts and whether they felt comfortable about participating in the rice lotteries that form the core of our intervention. **(4)** During the discussions after the six trial lotteries, we will ask participants what steps we should take if people feel uncomfortable with the ex-post inequality from the lotteries. Answers

to the question will provide us with a grass-root solution to the possible adverse effects of the lottery for villages that decide to take part in the study. **(5)** We have requested IRB approval from Northwestern and will notify NIH as soon as we hear.

E.3. Adequacy of Protection Against Risks.

E.3.1. Recruitment and Informed Consent. In **D.6** we describe the steps to recruit villagers. Here we elaborate. We will first speak with the Gran Consejo Tsimane', the umbrella organization for all Tsimane' with whom we have worked since 1995, to explore their reactions. We will then explain the study to villagers -- at first in group meetings and then one on one with each household. Since the intervention affects the income distribution of the village, we will ask for informed consent from all household heads. Village decisions typically rest on widespread consensus. In the past, we have found only two villages that have refused to take part in our studies, and in both cases we respected their decisions. The small sample of refusals is unlikely to lead to biased results.

To collect data on individuals, we will ask adults for consent to interview them, draw their blood, take their blood pressure, and take salivary samples. For children, we will ask both parents to agree on our obtaining data from the child. Because 77% of Tsimane' adults are illiterate, we cannot use written consent for most subjects. Instead, our practice has been to explain in village and individual meetings the purpose of the study. We will have written consent forms for parents who are literate, but we have found that it is better to explain the project orally than through print. People have told us whether they want to take part in studies and we have respected the decision. Recruitment is voluntary. For example, in study **C.1.1**, 9.0% of households initially refused to take part in the study (but later joined).

E.3.2. Protection Against Risk. We do not expect the intervention to have adverse effects on subjects. Through six practice lotteries, we will make sure that villagers understand lotteries and probe their reaction to the transfer. Six trial lotteries should allow ample opportunities for people to voice their concerns. To protect the confidentiality of subjects, we will keep the raw data in a locked file cabinet at Northwestern, accessible only to researchers whose names appear on this grant. We will not share the critical data related to confidentiality with anybody outside of the research team. Before making the data available to the public we will remove the names of people and villages and GPS references of villages, top and bottom-code variables, and remove any other identifier that would allow a user to identify a subject by the data. Any publication from participation in the study will not identify subjects by name. From the start of the study in 1999 onwards we have been successful at protecting the confidentiality of subjects. We have done so by restricting access to the data only to people in our research team; we have removed all identifiers on information we have placed on the web. We have developed a formal set of rules or code of ethics that we have applied to all researchers, including undergraduate and graduate students, since 1999 that has proven effective. The rules include:

1. No sex with people from the area.
2. No drinking of alcoholic beverages in the towns with indigenous peoples. Only drinking allowed is: **(i)** in the privacy of your home, **(ii)** by yourselves in town, or **(iii)** in public in the community when it is part of a communal event. Under no circumstance should researchers buy subjects an alcoholic drink.
3. Researchers should refrain from providing credit to subjects and never for more than b$50 at a time (<6 US $). Debts (whether of money or of fuel) should all be repaid in cash or in commodities within one month. If debts are not repaid, no further loans to that household will be allowed. We will restrict credit to medical or other important emergencies.
4. Should someone in the community require 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 to repay or be unable to repay, we will cover the costs incurred.
5. Any request by an outside organization to do extra work must be approved by the PI and co-principal investigators.
6. Policies on stealing by subjects: ignore it unless the item stolen is very valuable; do not hold a grudge.
7. Distribution of cigarettes and coca: do not distribute them to subjects.

E.4. Potential Benefits of the Proposed Research To the Subjects and Others. There is one potential benefit of this project beside the intellectual contribution we make to the study of income inequality and health. At the end of the study we will be able to notify people who are malnourished, have high blood pressure, or show symptoms of other illnesses as revealed by blood analyses or anthropometric measures. We will have the nurse arrange to refer respondents to city hospitals if they request such services, and coordinate with doctors for follow-up treatments. Life-threatening, urgent ailments detected during the study will be referred to the local hospitals in the towns of San Borja or Yucumo; depending on the type of treatment and costs, the project might cover the medical costs of these cases.

E.5. Importance of the Knowledge to be Gained. See section **A**.

F. VERTEBRATE ANIMALS AND BIOHAZARDS. Not applicable.

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H. CONSORTIUM AGREEMENTS. None.

I. CONSULTANTS.

Behrman is an economist with wide-ranging experience collecting and analyzing household data in developing nations (**D.1**). He will consult for three days/year during each of the two years. His letter of support is attached.

Goodman is a board-certified pediatrician with extensive experience collecting data on child and adolescent health and stress (**D.1**). She will consult for 10 days/year during each of the two years. Her letter of support is attached.

Huanca is a Bolivian cultural anthropologist with extensive research experience with the Tsimane' and other indigenous groups (**D.1**). He will serve as a consultant and the Field Research Director of the project for both years. His letter of support is attached.